

**THE THICK AND THE THIN OF CONTROVERSY:
A CRITIQUE OF BATEMAN ON KEYNES**

By

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ABSTRACT

This paper critically analyses Bateman's interpretation of Keynes's thought as presented in his 1996 book, *Keynes's Uncertain Revolution*. The book has two main aims. One is to present a 'thick' history of the evolution of Keynes's thinking on probability and uncertainty, by which is meant a history that refers to both the internal and external influences on a person's ideas. According to Bateman, this is necessary because an adequate understanding Keynes's ideas on probability and uncertainty is impossible without such a history. The second aim is to advance a particular interpretation of the relationship between Keynes's philosophy and his economics. This interpretation contrasts with previous 'continuity' interpretations by arguing for the existence of major discontinuities in Keynes's philosophical thinking. The paper also comments on some of the methodological issues involved in clashes between different interpretations.

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Controversy is the permanent companion of interpretation, just as it is of economics. We may never reach agreement in either area but, as rational beings, we are at least capable of understanding, or doing our best to understand, different points of view. To do so, however, requires sufficient good will and respect for the tasks of understanding and discussing these other points of view. Clashes between protagonists will be intellectually more productive if they are well-informed and fair.

In this chapter, I discuss some of the general and particular issues that arise in controversies and debates concerning Keynes's philosophy and its relations to his economics. Given the space limitations, I concentrate on two interpretations only, Bradley Bateman's as set out in his 1996 book (*Keynes's Uncertain Revolution*) and my own as set out in my 1989 book (*Keynes: Philosophy, Economics and Politics*).² As well as seeking to contribute to the better understanding of aspects of both interpretations, the discussion will also address issues that arise in Keynes scholarship regardless of the identities of the protagonists.

It may seem self-indulgent to dwell on my own interpretation, but I do so because some commentators evidently still do not understand it adequately, because it is based on all the available evidence, and because I still adhere to its main theses and overall structure. The reasons for choosing Bateman's is because his interpretation is in strong contrast to mine, because his book criticises me more than any other author,³ and because I would like an opportunity to discuss his views and rebut his criticisms. As always, space limitations constrain coverage to a subset of the issues.

Histories

Bateman characterises all prior efforts at exploring the links between Keynes's philosophy and economics as works in the history of economic thought and hence as works of *history* (p.3).⁴ No other possibility is envisaged. Even books discussing Keynes's ideas and analyses are viewed as histories of doctrines or analytical constructs. Having forced them all into the mould of being histories, he then finds much fault with them. They are inadequate because they are only 'thin' histories and not 'thick' histories, they don't explore the origins of Keynes's thinking, and they don't pay enough attention to putting Keynes's ideas into historical context.

¹ I am grateful to Wylie Bradford for comments on an earlier draft.

² See O'Donnell (1989) and Bateman (1996). References will also be made to some other papers by both authors.

³ Carabelli runs second as a target. Most of Bateman's criticisms are not discernible from the sparse index to his book. This only contains 2 references to me and 1 to Carabelli, whereas a more accurate index would record 19 for me and 10 for Carabelli.

⁴ All page numbers on their own refer to Bateman (1996).

The task he sets himself at the beginning of his book is to write a better history, one that will adequately tell 'the story of Maynard Keynes's concern with probability and uncertainty' (p.3). Despite all previous scholarship, he regards this story as remaining untold because previous authors have not addressed 'the question of *where* Keynes's interest in the ideas came from' (p.3). And, instead of the 'thin histories of analytical constructs' produced by other writers, he will aim at presenting a 'thick' history.

Little time is spent in explaining McCloskey's distinction between thin and thick histories. From his brief discussion, it emerges that thin histories are written by historians of economic thought, they focus on 'the pedigrees of specific analytical ideas', and they are primarily concerned with the internal influences on an author's thinking. Thick histories, by contrast, are practised by historians proper, they place economic ideas in social and cultural contexts, and they embrace both external and internal influences on an author's thought. A thin or purely internalist history has some value, but a thick history, one that 'tells a full story of a history of an idea', will almost always be more valuable (pp.3-4).

In relation to its historical side, the main thesis of his book is that it is impossible to understand uncertainty and expectations in the *GT*⁵ without understanding the 'external' influence on Keynes's thought of his debates in the early 1930s with other economists over the role of business confidence in the Great Depression (pp. 4, 10, 124-5, 145). The main reason for this emphasis on thick history is his belief that it is impossible adequately to understand ideas without understanding their historical origins (on which see below).

Some observations on Bateman's introductory remarks are pertinent. Firstly, it seems extraordinary to suggest that other writers still have not told stories of Keynes's concern with probability and uncertainty. Most of the well-known books Bateman refers to have done just that – related stories about how Keynes came to these issues in his early years and how these resurfaced, in full or in part, in the *GT*. One could criticise these efforts for being incomplete or misleading, but to say that nobody has told a story is quite inaccurate. Perhaps he means that earlier writers have only told 'thin' stories and that what is missing is a 'thick' story, or that earlier writers have only told 'a' story whereas he will tell 'the' story. But if this is what is meant, it would help if it were made clear.

Secondly, my book (and possibly those of others) was not written as a historical work, even one in the history of economic thought. Its primary aim was always *analytical* – to explore the philosophy, and the links between this philosophy and the economics and politics, of one of the greatest economists of the 20th century whose thought still has contemporary relevance. Of course, it had historical dimensions which entered the story as required, but it never claimed to be, and was never intended as, a history, thick or thin. Criticising it for not being what it never sought to be is beside the point.

⁵ The abbreviations *GT* and *TP* respectively stand for the *General Theory of Employment, Interest and Money* and the *Treatise on Probability*. The abbreviation *CW* stands for the *Collected Writings of John Maynard Keynes* (Keynes 1971-89); references to the *Collected Writings* take the form of *CW* followed by the volume number and page number(s).

Thirdly, describing all previous works as thin histories and his own as a thick history conveniently massages the odds in his favour. Since thick history almost always outperforms thin history, his book is almost guaranteed to be a superior account.

The Division of Labour

Among the objectives that have motivated writings on Keynes over the last few decades, three seem to me to stand out:

- (1) understanding Keynes's ideas, their changes and their interconnections,
 - (2) investigating the origins of Keynes's ideas and their historical context, and
 - (3) exploring the fruitfulness of Keynes's ideas for contemporary economics.
- These objectives, which might be abbreviated to 'analysis', 'history' and 'relevance', intermingle in various ways.

Most authors, depending on their interests and skills, and the state of current knowledge, focus on one (or possibly two) of the objectives and let the others play second or third fiddle. This sensible practical decision invokes the division of labour. A complete book giving equal and adequate treatment to all three, if such could be written, would be an immense labour.

In my case, given my qualifications in philosophy and economics, my interest in combining economics and philosophy in my doctoral research, and the complete absence, at the time (1978-82), of any systematic studies of the relations between Keynes's philosophical and economic writings, my primary interest was in (1). My secondary interest was (3) since I thought Keynes was far more than just a figure in the history of economic thought but continued to have much contemporary relevance. I did not neglect (2), for nobody can when studying Keynes, but I was content with a broad brush approach. Other reasons reinforced the priority I gave to (1) over (2) and (3). One was my view that understanding the origins and relevance of ideas depends on having a good grasp of these ideas in the first place. Another was that the database of knowledge concerning the historical development of Keynes's writings was much less accessible than it later became. Later writers benefited from all the published documents in the *Collected Writings*, as well as more detailed information concerning unpublished documents in various archives. Criticisms of the works of earlier writers for not concentrating on history not only ignores the benefits of the division of labour, but also neglects the very factor that Bateman stresses so much, understanding the historical context in which authors make their contributions.

In the case of Bateman's book, its primary concern initially appears to be (2), but its ultimate primary concern is, in fact, (1). History, while important, ends up as part of the supporting story for an interpretation of Keynes's ideas. As regards (3), his book keeps it at arm's length and provides little comment. The real conflict between our two books is thus over (1) where we have radically different interpretations, and not over history where the general outline of his account of the debates over business confidence is quite compatible with my account of the relationship between the *TP* and the *GT*. For this reason, the bulk of the following discussion will focus on our differences regarding analytical issues.

Is History Necessary to Adequate Understanding?

Is it possible to obtain an adequate understanding of ideas without knowing their history? Bateman and I answer this question very differently. My answer is yes – the adequate understanding of ideas is not dependent on a knowledge of their history, although such knowledge can broaden and deepen understanding. It follows that productive discussions about ideas are possible without becoming embroiled in the details of their origin and evolution.

Bateman's answer appears to be no – knowing their history, or at least the external factors in their history, is essential to the adequate understanding of ideas. This is the message conveyed by remarks such as the following.

But without considering the external [historical] influences, *it is simply not possible to get Keynes's ideas in clear focus.* (p.4, emphasis added).

Likewise, it is *impossible* to understand Keynes's use of uncertainty in *The General Theory* without understanding his long and protracted battle in the 1930s over the importance of business confidence to the Slump. (p.4, emphasis added).

Our respective answers have influenced the kinds of books we have written. I wrote a book that pursued the analysis and investigation of Keynes's ideas, referred to their history and the historical context as the discussion required, but did not undertake a lengthy or detailed investigation of the historical development of these ideas. I also believe that before one can write a history of ideas, one must have a prior notion of what these ideas are. By contrast, Bateman's book seeks to be a thick history that will make adequate understanding possible by clarifying the evolution of Keynes's ideas. On his account, non-historical analytical works such as mine have merely mired themselves in misunderstanding, confusion and error through their neglect of history.

In fact, however, Bateman's book doesn't actually deliver on its promise of showing the *necessity* of history to understanding. This is because his account of the history of business confidence in Keynes's writings in the 1920s and 30s is basically compatible with a number of interpretations, including my own. So if the same history can be embraced by his (correct) interpretation and my (incorrect) interpretation, how can it be that this history is essential to the adequate understanding of Keynes's ideas?

Does Keynes's Philosophy Drive his Economics?

Bateman suggests that I argue that 'Keynes had an overarching philosophical schema that drove his economics' and that 'changes in his philosophical ideas were a *cause* for the changes in his economic ideas' (p.10&n9). This, however, is a misrepresentation. What my book argues are that there are strong links or continuities between Keynes's philosophy and his economics, such that a knowledge of the former clarifies parts of the latter, and that the way Keynes theorised parts of his economics was *influenced* by his philosophy. It also contends these links do not take the form of linear translations or transferences of one body of thought into another.

In fact, Bateman uses exactly the same *kind* of argument as I used. When Keynes decides, *for whatever reason*, to make uncertainty a central theme of the *General Theory*, he turned to his philosophical beliefs, *whatever these were*, as a way of theorising this theme. What divides us is our views on what these philosophical beliefs were – in the simplest of terms, I say the conceptual framework of the *TP* within which there was an internal shift, while Bateman says Ramsey’s theory of subjective probabilities and Keynes’s intersubjective version of this. If turning back to philosophical ideas, or saying that philosophy influences economics, means that philosophy ‘drives’ economics, then this characterisation is equally applicable to Bateman’s own book.

Bateman also says that a serious flaw in my argument is that ‘it cannot account for Keynes’s changing attitude toward the importance of businessmen’s expectations to macroeconomic performance’ (p.10n9). This is another misunderstanding. It is true to say that my book does not give an account of Keynes’s changing attitude, but there is nothing in the structure of my interpretation which *prevents* it from including such an account. Bateman’s history of this episode is detachable and capable of appropriation by other interpretations.

Changes in Keynes’s Philosophical Ideas

Bateman contends that I argue that ‘Keynes’s philosophical ideas remain unchanged’ between the *TP* and the *GT* (p. 8), and that my story ‘depends on Keynes having one unchanged conception of probability after 1921’ (p.102n3). This is another poor representation because it implies that I think Ramsey had no impact on Keynes and because it confuses two things which my account deliberately distinguishes. My basic argument is that, in philosophy, Keynes’s *conceptual framework* remained unchanged, but that certain of his *ideas* within that framework did change in significant respects as a result of Ramsey’s critique. The outcome is an internal shift within the existing framework away from a heavy emphasis on strong rationality, known probabilities, probabilistic uncertainty and determinate conditions towards a greater recognition of the role of weak rationality, unknown probabilities, radical uncertainty and indeterminate conditions. There are thus some significant changes in Keynes’s ideas about the appropriate mix of concepts to use in the analysis of behaviour under uncertainty.

Bateman does not mention these vital aspects of my interpretation except for a footnote reference to unknown probabilities (p.57n34). This footnote misrepresents my argument and subjects it to misguided criticism. My view is that unknown probabilities are relevant to the *GT* in the context of long term expectations and radical uncertainty because a dearth of relevant information about the long term future means that agents with given mental abilities are unable to perceive logical probability-relations; it is not, as Bateman puts it, that ‘Keynes merely came to realise that more and more probabilities were unknown and fewer and fewer people had the ability to recognise them’. The first of his two criticisms is that Keynes’s use of unknown probabilities was ‘an unsatisfactory move in the first place that betrayed the unsatisfactory nature of his larger theory’. This is not a criticism of my view, however, but a criticism of Keynes’s theory. Serious commentators do not confuse one thing with another – we do not criticise an interpretation of an author’s work on the grounds that the author’s own work was unsatisfactory. His second criticism is that Ramsey’s critique of Keynes’s theory was ‘directly aimed at’ unknown probabilities, and that it was to ‘this part of Ramsey’s

critique' that Keynes referred in his response. Bateman must have different copies of these two documents from mine. Unknown probabilities are not mentioned by Ramsey in his critique of Keynes. He cites a passage from the *TP* which, while related to unknown probabilities, is used to launch a quite different criticism, namely, the presence of a contradiction in Keynes's theory; as Ramsey (1931 p.163) put it, it shows Keynes's 'inability to adhere to [his theory] consistently even in discussing first principles'. However, as argued at length in my book, this criticism is a mistake; Ramsey overlooked the role of limited logical insight in Keynes's theory.⁶ Finally, I would like Bateman to point out to readers the passage in Keynes's response that refers to any argument by Ramsey about unknown probabilities, for I cannot find it.

In relation to the issue of change and continuity in Keynes's philosophical views between the *TP* and *GT*, it is interesting to note how elements in our respective interpretations form supporting matrices. Bateman advances a 'discontinuity' position in which Keynes's philosophical ideas underwent dramatic transformations. In broad terms, three major changes are said to have occurred – Keynes abandoned the *TP* and its logical probabilities for Ramsey's subjective theory, he abandoned Moorism and Platonism, and he switched from the view that people behave rationally to the view that they behave irrationally. Two of the most important documents in his account are Keynes's 1931 response to Ramsey's critique (*CW X* 336-9) and Keynes's 1938 memoir *My Early Beliefs* (*CW X* 433-50). His interpretations of these two documents mutually support each other, with the abandonment of the *TP* (Ramsey's critique) going hand in hand with the abandonment of Moore's Platonism (*My Early Beliefs*), and with both documents being cited in support of the view that rationality is displaced by irrationality.

My account is quite different in that it advances a 'continuity' position.⁷ In my view, Keynes's ideas underwent change but it was change within the continuity of adherence to an existing conceptual framework. Whatever he said or meant in his response to Ramsey, he did not, in fact, throw over the conceptual framework of the *TP*; he reaffirmed, with qualification, his commitment to Moorism in *My Early Beliefs*; and the *GT* typically portrays people as behaving as rationally as they can under the circumstances. My three contentions are also consistent with each other and, while Keynes's response to Ramsey and *My Early Beliefs* are also central to my account, I draw, in addition, on all of Keynes's other writings.

Methodologies of Interpretation

On many key points Bateman's interpretation and mine are worlds apart. How are our divergent, and sometimes contradictory, views to be explained? An important part of the answer seems to derive from the use of different underlying methodologies of interpretation and argument. If so, it will help if these differences are clarified.

⁶ See O'Donnell (1989 pp.64-6), where an appendix is specifically devoted to explaining Ramsey's mistake which Braithwaite repeats in his foreword to the *Collected Writings* edition of the *TP* (*CW VIII xxi*). Bateman (1991 p.109n2) comments on this passage in my book, but his comments completely misunderstand my argument for they just restate Ramsey's points in different words.

⁷ See O'Donnell (1989 pp.138-48), (1990) and (1991 pp.24-7).

My methodology seeks to base itself, in large part at least, on the following principles:

- (i) All the available evidence must be considered, including the whole of Keynes's writings (while not being restricted to these).
- (ii) The interpretation produced must be *consistent* with this evidence, where consistency means the following. The interpretation must (a) agree with a substantial portion of the evidence, (b) not contradict the remaining portion of the evidence, unless (c) reasons are provided for departing from parts of this remaining evidence.
- (iii) Interpretations are based on arguments which are *probabilistic* in nature, and not demonstrative. They lay claim to a probability on all the relevant evidence, rather than complete certainty.

Two things follow from these principles. Firstly, newly discovered or omitted evidence has the capacity to undermine or substantiate interpretations. Secondly, a range of legitimate interpretations is permitted, but not all interpretations.

Whatever their nature, all I request of other interpretations is that they be consistent (in the above sense) with all the available evidence. I take this to be a rational requirement on interpretations but, so far as I am aware, none of the other large scale interpretations of Keynes's thought that have been produced either espouse or conform to this criterion. My typical practice, when writing about Keynes, is to advance a thesis and then support this with reasons and arguments based on all the available evidence. Such theses are only advanced *after* lengthy reflection. The ambiguities or imprecisions that crop up at key points in Keynes's writings are handled using (i) and (ii) above. The outcome, though it may not always be explicitly stated, is an evidence-based probabilistic argument.⁸

It seems to me that Bateman's methodology follows a different path. It doesn't have any systematic basis so far as I can discern. He doesn't invariably cleave to the above three principles but relies instead on a variety of procedures deployed in piecemeal fashion. He is certainly capable of providing an evidence-based case for positions, as his detailed story about confidence indicates (leaving aside the issue of whether it is based on all the evidence). But he doesn't always take this course, and other less satisfactory practices play significant roles in his method of interpretation, two of which are discussed below. The aura with which he surrounds his position is one of *certainty*, this being partly conveyed by using words such as 'unambiguously', 'unmistakeably', 'certainly', 'undoubtedly', 'perfectly clear', 'exactly' and so forth. Bateman does accept that room exists for a variety of interpretations, but he doesn't appear to me always to make a genuine effort to understand the views of those he criticises.⁹

⁸ See also the discussion in O'Donnell (1999).

⁹ As a clear example of this, consider his representation of my view of the proposition that Keynes undertook the study of probability in order to overthrow Moore's argument for rules. He claims that I say this proposition is 'quite false' (Bateman 1991 p.109n1). In fact, this is the *exact opposite* of what I say at a number of points in my book, including my account of *My Early Beliefs*. See O'Donnell (1989) pp.12, 106-7, 109-12, 117-19, 135, 149-51.

The two unsatisfactory practices which I think underpin a number of Bateman's accounts are:

- (a) argument by an implicit claim of superior insight into Keynes's mind, and
- (b) argument by assertion.

These practices are inter-related and act as substitutes for reasoned arguments based on all the evidence. The presence of these practices will be illustrated at various points in what follows, but for the moment an example may be given based on his remarks about how to deal with ambiguity in Keynes's language

Bateman tells us that while he is 'certain' about some things, he is 'not certain that the full story is the one [he has] told here, for there is a *fundamental indeterminacy* in the retelling of that story' (p.12, emphasis added). The main reason given for the existence of this indeterminacy is 'the fact that Keynes found himself by the late 1930s with a set of sentiments and ideas accumulated over a lifetime but with no good way to express these' (p.12). That is, 'Shorn of the certainty of his early beliefs, he was left to express himself imperfectly', and 'found himself prone to lapsing' into his old modes of speech (pp.13-14). As a result, 'scholars will always be faced with the dilemma of trying to discern his actual beliefs from the imperfect approximations he found for his own thoughts' (p.14).

This distinction between Keynes's actual beliefs and their imperfect expressions is both troublesome and revealing. Firstly, it shifts the discussion towards Keynes's mind. His mind was clear early on, but later struggled to express itself adequately. But how do we know this? Bateman doesn't provide any *relevant* textual evidence¹⁰ and, to my knowledge, there are no passages in Keynes's later writings supporting this view in relation to the concepts surrounding probability and uncertainty. So it appears that we only know about it on Bateman's say so. This is either an assertion or it implies that Bateman knows more about Keynes's mind than others do. Secondly, Bateman is quite certain of his interpretation of various key passages in Keynes's writings, even though others have provided reasons for thinking these passages are ambiguous or imprecise. It must therefore be that Bateman can perceive what Keynes is really trying to say, even though Keynes could not find the proper words to say it. Again this implies insight into Keynes's mind. And thirdly, assuming Bateman's distinction does accurately portray the state of Keynes's mind, how are we to decide between competing interpretations when they offer different dividing lines between his actual beliefs and their imperfect approximations? The absence of any criteria seems to be the reason for the fundamental indeterminacy. But if everything is fundamentally indeterminate, why is Bateman's interpretation better than any other? No pointers are given as to the answer, other than an assertion or Bateman's own certainty and insight.

Methodologies of Engagement

Strategies of engagement between protagonists in controversies are many and varied. However, if we seek understanding rather than emotional venting, we need to adopt methods which encourage clarity and accuracy, not fogginess or misrepresentation. Our goal should be intellectual progress through the better understanding of competing points of view.

¹⁰ The quotations he gives from *My Early Beliefs* are irrelevant in my view because they don't provide support for his proposition.

In writing about other interpretations of Keynes, my objective has always been to gain as good an understanding as I can of the other point of view. As best I am able, I try to detach temporarily from my own views, preconceptions and feelings, to undertake a thorough and fair investigation of the alternative account, and to engage in reasoned, well-informed dialogue.¹¹ I find it noteworthy that, although Bateman criticises my work many times in his book, only once does he complain about my rendition of his views, and then only to say that one of my essays has attributed views to him that do ‘not accurately, or completely, represent’ his views (p.136n79). Unfortunately, I cannot return Bateman’s compliment so far as his book is concerned. I find my work frequently misunderstood and misrepresented, even though we have debated issues in the past and my positions on key issues have been set out more than once.

Certain practices facilitate understanding and constructive dialogue while others hinder them. Some of the more obvious instances of the former are listed below:

- (i) Putting an effort into understanding other interpretations.
- (ii) Representing these interpretations as accurately and as fairly as possible.
- (iii) Meeting the arguments of other interpretations head on and not avoiding them.
- (iv) Providing reasons when criticising the work of others.
- (v) Providing page references when discussing other interpretations.¹²
- (vi) Providing reasons when claiming misrepresentation, so that commentators can re-evaluate and learn from their possible mistakes.

These elementary protocols will not reduce the clash of competing interpretations, but will hopefully improve levels of understanding and analytical engagement and reduce distortion, caricature and unnecessary controversy.

Das Maynard Keynes Problem

Bateman re-asserts the alleged existence of a problem or ‘paradox’ in Keynes’s writings analogous to the apparent paradox in Adam Smith’s.¹³ How could the author of the *TP* with its stress on rational individuals and objective probabilities have written the *GT* with its stress on ‘irrational people who base their decisions on social conventions’ (p.7)? He regards this problem as a significant and enduring one which will still provoke books 100 years after his own.

Superficially, it would seem that Bateman’s solution to the alleged problem is the obvious one of saying that Keynes changed his philosophical beliefs between the two books. That is certainly part of his story. But, despite his claim that it is only possible to solve the problem on the basis of his understanding of the evolution of Keynes’s views on probability (pp. 69, 73), by the end of his book the problem remains unresolved. The reason the reader is left in limbo is because nothing like an adequate explanation is given as to why agents in the *GT* are irrational.

¹¹ I leave it to others to judge the extent to which my contributions have lived up to these ideals.

¹² Most (but not all) of Bateman’s references to myself and Carabelli consist of references to our reasonably lengthy books with no page numbers. Thus one has no clue as to which part of one’s book is relevant to his comments.

¹³ He first raised the alleged problem in Bateman (1991).

Bateman says that Ramsey's critique of the *TP* converted Keynes to subjective probabilities, that subjective probabilities underpin uncertainty and expectations in the *GT*, and that the agents in the *GT* are irrational. It is the last of these that creates internal difficulties for his account. Firstly, the subjective theory of probability, whether in Ramsay's or later versions, does not assume agents are irrational but grants them the minimum amount of rationality necessary to support the theory (usually in the form of a consistency requirement). Secondly, when Bateman later discusses the behaviour of agents in the *GT* (pp. 128-39), not once does he characterise them as irrational. They now have (inter)subjective probabilities and follow social conventions, but the question of their rationality or irrationality is never addressed. There is enough in his discussion, however, to indicate that *some* element of rationality is present in their behaviour, but to admit this would destroy *das Problem*. Given his earlier stress on the problem and its enduring nature, and his claim that only his account can supply a solution, it is remarkable that he does not return to it towards the end of his book and satisfy the reader by providing an adequate explanation of why the agents of the *GT* are irrational.

The only hints we are given come at the start of his book. Its first chapter states that, early in life, Keynes took rationality to have 'a very narrow meaning', namely, that 'people always act so as to achieve what is objectively and recognisably *good*', and that, later in life, when people didn't act this way, 'he called them "irrational"' (pp.13-14). The following page then adds a slightly different, but closely related, view, namely, that Keynes's conception of rational behaviour in the *TP* depended on objective probabilities so that, when he threw these over, no basis remained for rational action (p.17). However, these brief, unexpanded arguments sit in a thicket of difficulties. Leaving aside the debatable issue of whether they adequately portray Keynes's early or later conceptions of rationality, they presume that Keynes only ever had one conception of rationality and that, when that was abandoned, he was incapable of making use of any other concepts of rationality. Hence the only alternative was irrationality. Unlike probability where he is portrayed as switching with relative ease from one theory to another, he was unable to make any movements or shifts concerning rationality. There is also no realisation that, if he accepted subjective probabilities, Keynes also accepted the form of rationality required by that theory.¹⁴ And, finally, by assuming there is only one form of rationality in the *TP*, these arguments ignore Keynes's remarks in that work that point to the alternative forms of behaviour which I have labelled 'weak rationality'.¹⁵

By contrast, *das Problem* does not exist in my account. There is no paradox about the author of the *TP* also being the author of the *GT*. My characterisation of the relationship between the two works is the different and more subtle one of saying that, by the time Keynes came to write the *GT*, he had made an internal shift or change of emphasis within the conceptual framework of the *TP*. This allows my account to embrace certain changes in the handling of uncertainty, expectations, probability and conventions, whilst maintaining that both works deal with rational agents operating in different environments. I agree the *TP* emphasises rationality based on objective probabilities, but I add that it also refers to situations where this breaks down and alternative forms of behaviour can be followed by rational agents. However, the representation of the *GT* as populated by irrational individuals is nonsensical in my

¹⁴ For Keynes's recognition of the consistency requirement in his response to Ramsey, see *CW X* 339.

¹⁵ See, for example, O'Donnell (1989) pp.59, 78, 121, 147, 254, and O'Donnell (1991) pp.14-19, 40-5.

account. In my view, its agents behave as rationally as they are individually able in the context of the economic environment in which they are situated. It is the nature of this environment that dictates the responses of rational agents, these responses including subjective expectations and convention-following. On my account, both the *TP* and the *GT* deal with rational agents who try to behave as rationally as they can under the circumstances. The typical circumstances in the *TP* are highly favourable, being rich in information and logical ability, but Keynes also mentions circumstances in which these preconditions are absent and rational agents are forced to follow different procedures. In the *GT*, the circumstances embrace *both* those of adequate information and ability as well as the better known, informationally-deprived circumstances which generate radical uncertainty and the need for alternative strategies such as conventions.

It follows that the Maynard Keynes Problem is non-existent in my account and does not require a solution. My work provides an evidence-based explanation of why the person who wrote the *TP* could also write the *GT*, and why the agents in both books behave rationally. The problem only exists in Bateman's account, and his book does not provide an adequate solution.

My Early Beliefs

This 1938 memoir by Keynes plays a vital role in Bateman's argument for the Maynard Keynes Problem. There is much that could be said here but let me focus on just one of Bateman's key propositions, namely, his contention that the memoir makes it abundantly clear that Keynes rejected his earlier attachment to Platonism which he later came to regard as a 'mistake' and an 'absurdity' (pp.8, 39). While claiming that Keynes 'offered ample evidence of...the changes in his ideas' between the *TP* and *GT* (p.9), he actually provides no quotations from Keynes to support this claim. Passages are quoted from the memoir (pp.13-14, 43, 162) but none of them lend credibility to his view (and one of them even seems to contradict it though this goes unremarked). Given the key role this proposition plays in his interpretation, there is no wonder that he reacts so strongly to my opposing viewpoint and my description of the memoir as possibly the most deceptive document in Keynes's *oeuvre*. He dismisses this as an unpersuasive 'allegation' (p.14n14) and takes no notice of the fact that it is actually the conclusion to a lengthy, reasoned, evidence-based case which draws on *history* to point out that the memoir mixes remarks which are accurate with others that are false.

However, let us turn to the memoir itself. One of the three questions Keynes says he will address is whether 'one still holds by that youthful religion' (*CW X* 435), where religion primarily means Moore's ethics with its Platonist, intuitionist elements. Keynes's answer is pretty clear and direct.

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 that I know, with less irrelevant extraneous matter and nothing to be ashamed of; though it is a comfort today to be able to discard . . . the calculus and the mensuration and the duty to know *exactly* what one means and feels. . . .It is still my religion under the surface. (*CW X* 442)

I see no reason to shift from the fundamental intuitions of *Principia Ethica*; though they are much too few and too narrow to fit actual experience which provides a richer and more various content. (CW X 444)

Would someone who had completely abandoned Platonism have written these passages? I read them as providing solid evidence against Bateman's proposition. If there is other relevant evidence that puts them in a different light and provides support for his proposition, Bateman doesn't reveal it in his book. But if, in fact, there is no other evidence, his argument degenerates into argument by assertion. That is, Keynes rejected Platonism because Bateman says he did.

By contrast, my interpretation says that the memoir reveals *qualified reaffirmation* of Keynes's belief in his early Moorism. When I reflect on all the evidence and seek a consistent interpretation (in the sense previously outlined), I agree that Keynes criticised his earlier beliefs but I contend that these represent immanent criticisms and qualifications rather than wholesale abandonment. The above passages seem to me to support this view and contradict Bateman's.

Classifications of Probability Theory

Bateman takes me to task for not following Hacking's 'very helpful' bipartite classification of probability theories, for 'creating' a tripartite division in which the third category is redundant, for being unable to see that the logical theory of probability is the same as an objective epistemic theory, and for allowing myself 'a false sense of distinction' between Keynes's and Ramsey's theories (pp.49&n19, 51n21).

These criticisms are just plain silly. At the time I wrote my book (which refers to Hacking's volume), I was well aware of Hacking's distinction and where Keynes's theory would fit into his classification. However, I chose not to use it because the tripartite division used by several other philosophers seemed to me to offer more advantages in relation to Keynes. (Note that I borrowed, and did not create, this system). One of the advantages was that it made it very difficult to conflate Keynes's theory with Ramsey's theory, a mistake which Bateman himself recognises other people have made (pp.50-1).

Bateman treats Hacking's bipartite (aleatory/epistemic) classification as if it was obviously the correct system, but the system one uses for classifying probabilities ultimately has a large arbitrary element. Hacking cuts the cake in one way, but a different bipartite division could be made using an objective/subjective criterion which would group Keynes's logical theory and the frequency theory together, leaving the subjective theory on the other side. Or, following Gillies and Ietto-Gillies (1991 pp.393-4), we could change the meaning of objective and divide the terrain using an objective/epistemological split which, despite the non-standard terminology, results in groupings similar to Hacking's. The ultimate arbitrariness is also evident in the fact that Hacking's epistemic probabilities then have to be subdivided into objective ones (Keynes's theory) and subjective ones. So Bateman himself finishes up using a tripartite division, the only difference between his and mine being the route used to get there. And if we both end up with three distinct theories, why is one of them redundant in my account but not in his?

Intersubjective Probabilities

Bateman's view is that, by 1931, Keynes had switched from logical probabilities to subjective probabilities and that, by 1933, he had also embraced intersubjective probabilities. The theory of intersubjective probabilities is borrowed from Gillies and Letto-Gillies (1991). Leaving aside my fundamental disagreement with this interpretation, let me comment on the way Bateman handles intersubjective probabilities in relation to the *GT*.

There is a noteworthy difference between the arguments of Gillies *et al* and Bateman. While Gillies *et al* propose a link between intersubjective probability and the *GT*, their view of this link is cautiously and circumspectly expressed. In relation to long-term expectations, their contention is that 'Keynes is implicitly making use of a concept of probability more or less equivalent to the notion of intersubjective probability', that Keynes's theory 'implicitly involves the notion of intersubjective probability', and that, in analysing these expectations, 'Keynes was moving towards a concept close to our intersubjective probability'.¹⁶ Thus the most that is claimed is that intersubjective probabilities are something *implicit* in the *GT*; there is no claim that Keynes consciously developed them. Bateman, however, sweeps this tentativeness aside and replaces it with confident assertion. Intersubjective probabilities are definitely present in the *GT* because Keynes actually created and introduced them.

When probabilities are formed according to group norms, they are referred to as intersubjective probabilities. After Keynes abandoned his work in *Probability*, he embraced this idea of *intersubjectivity* and made it central to *The General Theory*. (p.50n20)

Suddenly, two years after he had publicly abandoned his objective theory of probability and accepted Ramsey's criticisms, he had a place to use his own newly minted intersubjective probabilities. (p.78)

But there is more to Keynes's achievement than this. ...[He] was, at the same time, the first to introduce an analytical treatment of intersubjective probability. (p.131)

What Gillies *et al* see as implicit, Bateman sees as explicit. We move from something implicit in Keynes's thought, something towards which it was moving, something of which he may or may not have been conscious, to something of which he is definitely aware, something which he had just recently created, and something to which he had given analytical treatment. Is there any independent evidence for Bateman's interpretation? One would think that if Keynes had newly minted a theory of probability (even as an extension of Ramsey's) and was the first to give it analytical treatment, there would be some remark somewhere relating to this, for Keynes was not backward in writing about his intellectual advances and innovations. But, in fact, there is total silence. All things considered, it looks as if we are again faced with either argument by assertion or argument by insight into Keynes's mind.

¹⁶ The quotations are from Gillies *et al* (1991) pp.404, 407, 408 (see also 395).

Confidence and Expectations

Notwithstanding the interest of the historical debates over business confidence, Bateman's analytical treatment of the concept of confidence is loose and incomplete. Instead of regarding confidence and expectations as separate concepts, he reveals a tendency, at key points, to treat them as synonyms. Thus, in discussing the *GT*, he observes: 'Chapter 12 presents Keynes's theory of the state of long-term expectation, or confidence, and its effect on investment' (p.127). Earlier, he comments that 'there was no necessity for Keynes to turn to confidence, or expectations about the future, as the final element' in pulling the *GT* together (p.78). This conflation, however, is at odds with what is said in the *GT*. In chapter 12, Keynes says that the state of long-term expectations depends on *two* separate factors – forecasts (or expectations) and confidence.

The state of long-term expectation, upon which our decisions are based, does not solely depend, therefore, on the most probable forecast we can make. It also depends on the *confidence* with which we make this forecast—on how highly we rate the likelihood of our best forecast turning out quite wrong. (*CW VII* 148)

This section of the *GT* is also notable for being one of two points where Keynes refers directly to the *TP*, in particular, to its chapter on the weight of argument. In the *TP*, the concept of weight can be viewed as underpinning the amount of confidence that rational agents have in probabilities.¹⁷ In my account, the remarks in the *GT* provide evidence of a link between the *TP* and *GT* such that the probability-weight pair of the former has some connection with the expectation-confidence pair of the latter. Keynes's second footnote reference to the *TP* provides further relevant evidence in the context of liquidity preference. The difference between liquidity premium and risk premium is described as corresponding to 'the difference between the best estimates we can make of probabilities and the confidence with which we make them' (*CW VII* 240). The distinction, and its relation to the *TP*, is even more explicit in Keynes's subsequent letter to Townshend in 1938.

I am rather inclined to associate risk premium with probability strictly speaking, and liquidity premium with what in my *Treatise on Probability* I called 'weight'. ...A liquidity premium...is payment...for an increased sense of comfort and confidence during the period. (*CW XXIX* 293-4).

Bateman is aware that Keynes's ideas on liquidity preference have a 'strong dependence on confidence' (p.124), but he remains silent about pursuing Keynes's own leads on the further analysis of confidence.

Bateman does not ignore the *GT*'s footnote reference to the *TP*, but gives it a novel twist which draws on his distinctive approach to interpretation. He maintains that probability and weight in the *TP* are Platonic entities, that when Keynes rejected Platonism he rejected the original meanings of these words, and that when he subsequently continued to use these words it was with new subjective meanings (p.59n38). It is notable that no grounds are given for this view that are independent of

¹⁷ See O'Donnell (1989 p.73).

Bateman's interpretation. Firstly, it rests on the contested proposition that Keynes rejected Platonism. Secondly, it rests on the claim that the *TP* reference was not to the original meaning of a phrase but to a new meaning which Keynes does not reveal. Most authors, including Bateman and Keynes on other occasions, when they refer back to their earlier work without qualification, refer back to its original meaning. But not Keynes on this occasion. Thirdly, nowhere in his writings does Keynes tell us, or even hint, that he is now using a previously distinctive concept with a new meaning. How, then, do we know that Keynes has injected a different meaning into an old phrase and now has a subjective theory of weight? Only on Bateman's say so. Argument by superior insight into Keynes's mind seems to have re-entered the fray.

Conventions

Many recent scholars have recognised conventions as important in the *GT*'s discussion of economic behaviour under uncertainty. In Bateman's account, however, they are elevated to supreme importance. They are treated as the foundation of expectations formation: 'Keynes makes it perfectly clear that agents form their expectations by means of a *social convention*' (p.130). They are also viewed as the foundation of the theory of intersubjective probabilities that Bateman believes Keynes developed out of Ramsey's theory: 'Keynes intended to represent his agents' beliefs via an extension of Ramsey's subjective probabilities as being determined intersubjectively by social conventions' (p.131). And they are central to one of the key themes of Bateman's book, the 'unexpected story of the importance of rules and conventions to Keynes's thought' (p.9), and the 'fine irony' associated with this that someone who started out rejecting general rules of conduct finished up championing rules and social conventions as necessary for the maintenance of civilisation (p.162).

Several features of Bateman's treatment of conventions invite comment and criticism. Firstly, in relation to the *GT*, their scope is not defined. Do they apply to all expectations, both short-term and long-term, and to all agents in all markets? Sometimes it is suggested that they are the unique foundation of expectations formation (p.130), while at other times it is suggested that Keynes's agents calculate expected values using probabilities (p.131-2). Whether the latter is separate from, or reducible to, conventions is left unexplained.

Secondly, Bateman's use of the term is astonishingly elastic. He offers no definition of the concept, which leaves him free to treat it like putty.¹⁸ In the final two chapters of his book, its meaning undergoes extension and metamorphosis. Initially, the term is used uncontroversially to cover the three techniques that Keynes outlined in 1937 (pp.130-1). A few pages later, however, we encounter an extension in which social conventions become general rules or habits *of the human mind*. After quoting a passage from Ramsey's discussion of 'human logic' in which Ramsey says that the 'human mind works essentially according to general rules or habits', Bateman asks: 'What indeed are social conventions if not "general rules or habits"?' (p.139). To which the obvious response is 'But are they general rules and habits of the way the human mind works?'. The next

¹⁸ In my account of Keynes, conventions, and the strategies of weak rationality more generally, are defined as 'practical measures for resolving theoretically undeterminate decision problems'. See O'Donnell (1991 p.17).

chapter then produces a breathtaking metamorphosis. Social conventions become synonymous with economic policy.

[Keynes] came to see that maintaining the proper conventions was crucial to investment, too. Just as a well-established convention of low interest rates might be self-fulfilling, so, too, might be a well-established convention of stable, high levels of investment. (p.152)

Thus the judicious use of public capital expenditure could be made a new convention that would yield more stability in the level of aggregate investment. (p.159)

And 'the three most prominent ways in which Keynes meant to socialise investment by creating new conventions' were given as 'socialising public investment', 'public works and a balanced budget' and 'stabilising private investment' (pp152-9).

In all cases, 'convention' can be replaced with 'policy'. Where standard usage would talk of policies, Bateman substitutes conventions. The explanation for this Humpty Dumpty-like disdain for ordinary meanings is bound up, I think, with his story about the great irony of Keynes's life – the evolution, concerning rules and conventions, from repudiation to acceptance. Since Bateman sees the later Keynes as insisting that conventions and rules are essential to the maintenance of liberal civilisation, then other things that Keynes also advocated to help create a better world, such as certain economic policies, are swept into the same conceptual basket. Conventions and rules then take pride of place as the central organising concept of the thought of the later Keynes. The old cliché is again vindicated – those who start out being radicals finish up being conservatives in their final years.¹⁹

Conclusion

The valuable contribution of Bateman's book is to have advanced a history of the concept of confidence in Keynes's economic thought in the 1920s and 30s. As he rightly suggests, previous works on Keynes had not addressed this question in sufficient historical detail. No doubt, further debate and discussion will test the robustness of his story.

Unfortunately, Bateman has wrapped up his valuable contribution in other analytical arguments, many of which are dubious, controversial and unsupported by all the relevant evidence. The inclusion of these other arguments leave parts of his book open to heavy criticism, and reveal that his agenda is not merely to write a 'thick' history of the development of some of Keynes's ideas but also to advance a particular interpretation of the relationship between the *TP* and the *GT*.

In my view, his book would have been far less controversial if he had not sought to tie its historical and analytical themes so tightly together, but had realised that its historical account of confidence is compatible with the main outlines of other interpretations of the analytical links between Keynes's two works.

¹⁹ In this area, another strong contrast emerges between his account and mine; see, for example, O'Donnell (1989) chs 6, 13, 14.

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