

Bayesianism and Austrian Apriorism

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A Bayesian middle ground?

In the last published round of his debate with Walter Block on economic methodology,¹ Bryan Caplan introduces Bayes' Rule as 'a cure for methodological schizofrenia'. Block had raised the question 'Why do economists react so violently to empirical evidence against the conventional view of the minimum wage's effect?' and answered it with the suggestion that economists do so because they are covert praxeologists. This means that they base most of their economic arguments on conclusions derived from their a priori understanding of human action, although, as methodologists, they prefer to maintain that their arguments are merely appropriately qualified generalisations of empirical observations. Against this, Caplan maintained that neoclassical economists are Bayesians with some strong prior beliefs, which lead them to ascribe low probability to any statement that goes against the strongly held consensus. Presumably, there is such a strongly held consensus with respect to the minimum wage effect. Caplan concluded that '[t]he Bayesian position stakes out a compelling middle ground between atheoretical positivism and praxeology. On the one hand, the Bayesian view emphasizes that few propositions are known with certainty, and that we should adjust our probabilities as new information comes in. On the other hand, the Bayesian view recognizes that the rational view is not an average of past empirical findings, much less a naïve faith in the last prominent study.' (C, p.83)

Caplan's references to Bayes should be considered carefully before we accept that Bayesianism makes for a middle ground—let alone a compelling one—between positivism and praxeology. The image of a middle ground may be soothing, but it is no more than a metaphor. Whether it makes sense in this context, is an altogether different matter.

From Bayes' theorem to Bayesianism

Bayes' theorem belongs to the calculus of probability, which is a branch of mathematics. It allows us to calculate the probability that a hypothesis is true if a particular set of data ('evidence') is available, if we have a priori probabilities for all the competing hypotheses as well as a priori probabilities for the evidence relative to each of the hypotheses. We shall consider the formula later in the text.

Bayesianism builds a methodology on that theorem, presenting it as a rule of rational belief formation—that is, a rule telling people how they should revise their opinions in the face of new evidence or data.² However, sometimes, it also appears

¹ In *The Quarterly Journal of Austrian Economics*, Fall 2003, volume 6, number 3: Walter Block, 'Realism: Austrian vs. Neoclassical Economics, Reply to Caplan', p.63-76; Bryan Caplan, 'Probability and the Synthetic A Priori: A Reply to Block', p. 77-83.

² The generalised formulation of Bayes' Rule requires also cardinal utility assignments to compute the 'expected utility' of each option in a decision set. Then, Bayes' Rule applies to all sorts of decision problems: "Bayes' Rule: An ideally situated decision-maker... ought rationally to pick that

as a descriptive theory of belief formation. Caplan's approach combines the prescriptive and the descriptive aspects of Bayesianism. 'The Bayesian model states that rational people do not revise their whole worldview every time a new data point emerges. Rather, they marginally update their initial views as facts come in—ideally according to Bayes' Rule...' (C, p.81). In short, by applying Bayes' theorem to any evidence or data that are relevant to their previously held beliefs, truly rational people rightly and justifiably come to believe what they do believe.

That sounds good—but is it true? More to the point, does the Bayesian strategy of belief formation obliterate the rationale for a distinctively 'Austrian' style of economic analysis and its emphasis on a priori reasoning?

It should be obvious that in moving from a theorem in the calculus of probability to a prescriptive or descriptive theory of belief formation one has to cross a bridge. Similarly, in moving from a theory of belief formation to a discussion of scientific truth claims, one has to cross another bridge. If the bridges are there and if they are sufficiently solid then in principle there is no problem, but perhaps there are no bridges that guarantee a safe passage. We should not assume simply that there is no problem here. Probability, degree of belief, and truth, after all, are not synonyms.³ It is true that words and expressions such as 'probable', 'likely', 'in all probability', 'maybe', 'perhaps', 'most of the time', 'there is a good chance', and 'almost certainly' appear repeatedly in scientific discourses. However, it would be rash to conclude that they prove that all the statements in which they appear are 'probabilistic'. Yes, indeed, to say that it is only probable that the first child born in the maternity ward of the University Clinic in Ghent on October 17th, 2004, will be a boy is to make a probabilistic statement. But when I say that I am not likely to become a fan of Big Brother, that is not a probabilistic statement at all. While the former statement is logically equivalent to a statement of the form 'the probability of A is p ', the latter is not. At the very least, to render the latter as a statement of that form we need a different notion of probability and different methods for a determination of p .

Probabilities and degrees of belief

Bayesianism, as Caplan discusses it, is not concerned with probabilities but with degrees of belief. Does that matter? Probability is a ratio between a set and another set, the first being a subset of the other. Hence, probability is a value with a maximum of 1 and a minimum of 0. Consequently, the same is true for improbability: high improbability corresponds with low probability. However, is degree of belief a ratio between one set and another? Is there any compelling

option (or one of those options) bearing maximum expected utility." (Isaac Levi, *Gambling with Truth, An Essay on Induction and the Aims of Science*, Alfred A. Knopf, New York, Routledge & Kegan Paul, London, 1967, p. 45).

Caplan alludes to this generalised formulation when he writes that 'neoclassical economists themselves [should] adopt the Bayesian model of belief formation that they routinely apply to everyone else' (C, p.83). Presumably, he means that neoclassical economists assume that everybody else is a Bayesian decision-maker. However, we are then left with a number of questions: Is there such a thing as a neoclassical economist's utility function? If so, does it differ from an Austrian economist's utility function? In what sense does being an economist of whatever school determines any person's utility function? And so on.

³ See e.g. J.L. Mackie, *Truth, Probability and Paradox*, Clarendon Press, Oxford, 1973.

argument for saying that a degree of belief cannot be higher than a particular value (say, 1) or lower than another (say, 0)? Is a degree of belief 'equal to zero' the highest degree of disbelief—or is a degree of disbelief a 'negative' degree of belief?

What one believes is a proposition or statement (or a source of propositions—as in 'I believe you'). Admittedly, in some cases, expressions such as 'statement S is true with probability p ' are meaningful. Suppose the probability of S ('the next time an object is taken at random out of this box, it will be a red ball') is p . Then the probability of not-S ('the next time an object is taken at random out of this box, it will be something else than a red ball') is $1-p$. A rational person will recognise that.

In the general case, however, a rational person may believe neither proposition P nor its negation not-P. Either Mickey is stronger than Minnie or not. It does not follow that if you do not believe that he is stronger than she is then you must believe that Mickey is not stronger than Minnie. Is there a fixed relation between your degrees of belief for those propositions? Suppose your degree of belief for the proposition 'Mickey is stronger than Minnie' is .6. Does it follow that your degree of belief for the proposition 'Mickey is not stronger than Minnie' is .4?

Not believing that P is true is not the same as believing that P is false. That Mr. A does not believe either P or its negation gives us no reason to conclude that he believes that P is true with probability .5. If we say that not believing P indicates that one's degree of belief for P is zero, then Mr. A's degree of belief for not-P also is zero. Whatever the Bayesian case for treating degrees of belief as probabilities may be, it hardly can be compelling.

In many cases, it is relatively easy to calculate the probability of an event of a particular type, either on a priori grounds or because we have a sufficiently long series of data to calculate the 'frequency-probability' of the event. However, it is an altogether different thing to assess, let alone calculate, a person's degree of belief in the occurrence of that event.

It is easy to compare a person's belief that an event will occur with a probability p with the calculated probability p^* of the occurrence of the event. However, that is different from comparing the calculated probability with the degree to which that person believes that the event will occur with probability p . Surely, 'A believes that an event E has a probability p ' is not the same proposition as 'the degree of A's belief that E will occur is p '. A person may believe the proposition that one has a one-in-six chance of getting a total of six eyes by rolling two fair dice, but that piece of information is compatible with the person having a high or a low degree of belief in the proposition. I may strongly believe this is going to be my lucky night in the casino, but does it follow that I do or should believe that tonight the probability of, say, rolling a five with a single dice is anything else than one in six?

Not every instance of 'A believes it is likely that S is true' is truth-functionally equivalent to 'A believes that the probability of S being true is higher than T' (where T is, say, .5). In some instances, it may be the truth-functional equivalent, for example, of 'A believes that S is true but he knows no way of proving that it is true'.

It is not self-evidently true that we can transform the calculus of probability without further ado into a calculus of degrees of belief. So, how much 'further ado' should we expect to be able to make the transformation?

Objective probability, subjective probability and degree of belief

The application of Bayesian inference to the confirmation of scientific theories is far from straightforward. Let us write down Bayes' theorem to point to the essential problem.

$$P(A_i|E) = \frac{P(A_i) \cdot P(E|A_i)}{[P(A_1) \cdot P(E|A_1) + P(A_2) \cdot P(E|A_2) + \dots + P(A_n) \cdot P(E|A_n)]}$$

For example, a box contains 10 balls, each of which is either red or white. We now perform an experiment and shake one ball out of the box. It turns out to be red: $E=1$ red ball. Based on the evidence, what is the probability that the box contains nothing but red balls? Let A_{10} stand for 'Exactly 10 (i.e. all the) balls in the box are red'. We have to calculate $P(A_{10}|E)$. To do that, we need to know $P(A_i)$ for every A_i —that is, for every possible combination (0 red, 10 white), (1 red, 9 white), ..., (10 red, 0 white).

If we knew that our box was chosen at random from a set of eleven boxes, each containing 10 red or white balls, and none containing the same number of red balls as any other, then we could assign $P(A_i)=1/11$. However, since we have no such additional information, there is no way for us to assign probabilities to any A_i . Although after the experiment—which gave us one red ball—we know that $P(A_0)=0$, we cannot assign a probability to any of the other hypotheses A_i ($1 \leq i \leq 10$). Moreover, prior to the experiment we had no ground for assigning a priori a probability to any A_i ($0 \leq i \leq 10$). We should not assume simply that $P(A_i)=1/11$, because not knowing the probability of the elements in a collection is no ground for assuming that all the elements are equiprobable. Therefore, in this case, Bayes' theorem does not permit us to calculate the probability of A_{10} on the evidence E .

In some sciences, we may be able occasionally to define a complete probability space and design an experimental set-up so that we can assign, with a high degree of confidence, a priori probabilities to all A_i that our experimental set-up has not excluded. If we can do so then we properly can use the Bayesian theorem to learn from our experiments which A_i has the higher posterior probability—or, as is sometimes said, 'to learn about the cause from the observation of the effect'. Evidently, we should not assume simply that economics (or any other science of human action) is such a science.

As noted before, not knowing the probability of the elements in a collection is no ground for assuming that all the elements are equiprobable. Of course, everybody is free to believe anything and to substitute his beliefs for the knowledge that he lacks. If we insert the probabilities we believe to be true into the Bayesian formula, we can use it to make a calculation. However, we then no longer calculate the probability of A_i given E . We calculate the probability a person with a given set of prejudices should assign to A_i when confronted with E , assuming that we want to interpret E as a test of his prejudices. Rather than acquiring knowledge about which condition A_i is the true state of the world, we learn something about how evidence may strengthen or weaken prejudices. Surely, those are different things—even if both of them still involve using the probability calculus.

Let us get back to Bayesianism. We now read $P(A_i|E)$ as '[someone's] degree of belief in A_i given E ' instead of 'the probability of A_i given E '. Then, to maintain consistency, we should read every expression of the form $P(\)$ in that way. However, if we do that then we take leave of the probability calculus. Unless we can come up with a compelling argument or proof that degrees of belief behave in exactly the same way as probabilities, we are nowhere.

In an orthodox probabilistic interpretation of the Bayesian Rule, there is no difficulty in calculating the various $P(E|A_i)$ that we need to calculate $P(A_i|E)$. All the information we need is given in the definition of the probability space. For example, E is the outcome '1 red ball' and A_6 is a box containing 6 red and 4 white balls. However, if $P(E|A_i)$ is not a probability but a degree of belief then we obviously cannot calculate it from the definition of a probability space. Indeed, the degree of belief that E is true if A_i is true is a piece of information that is independent of the data we have concerning E or A_i . How do we get that information? More to the point, how do we *measure* degrees of belief? What is an appropriate unit of measurement?

If Bayesianism is supposed to tell us how scientific beliefs [should] change as new evidence becomes available, there is little use for merely recording what people indicate as the degree of their belief. Personal mood swings—'one day I believe anything, the next days I believe nothing'—are the least of our problems. A greater difficulty is that there may be swings even in widely shared beliefs, such as those that constitute the so-called 'consensus of the scientific community'. These changes, after all, may affect beliefs about what constitutes scientific evidence and about appropriate methods of research. This is no small problem. Was the 'Keynesian revolution' a Bayesian slide of beliefs? Were the partial retreat from Keynesianism and the neoclassical upsurge in the sixties and seventies of the twentieth century Bayesian movements, or were they sociological, ideological or cultural phenomena that went far beyond questions of 'theory and evidence'?

In modern society, in its governmental and its private sectors, there is a large number of 'leveraged institutions' where policy decisions made by a few movers and shakers translate into massive more or less co-ordinated responses by a great many other people. We can find similar and often dominant, even quasi-monopolistic, institutions in education and the media, in research and scientific publishing. Therefore, we should allow for the existence of 'systemic bias' in belief formation. What if subsidies and grants are likely to go to people—'scientists' and 'academics'—that are, or at least appear to be, politically correct or otherwise willing to secure the aims and respect the basic philosophy of their paymasters? What if political and ideological forces that prevail at the top of the education establishment begin to 'colour' textbooks and other course material? Pressure on careers, desire for money, prestige or the social comfort that only going with the crowd can bring, intellectual laziness or indifference to anything outside the small coral of one's expertise—all of these and many other factors will inevitably show up in degrees of belief. To what extent, if any, they run parallel to the force of scientific evidence and argument (supposing they do not stifle it altogether) is a good question. However, the answer to it is not going to come forward from another round of registering degrees of belief.

Reasons for not being a Bayesian

An early critic of Bayesianism, Clark Glymour⁴, had questioned already the Bayesian claim that probabilities represent, and that the probability calculus is applicable to, degrees of belief. If that claim cannot be substantiated then the probability calculus and its Bayesian formulas give us no reason to subscribe to the view that they are relevant to questions of changes in the degrees of belief. We have seen that this criticism goes to the heart of the matter.

In addition, Glymour noted that even if the claim is accepted then the most we can infer is that changes in belief are law-like phenomena. What we cannot infer is that those changing beliefs result in an advance of knowledge. Bayesianism establishes no connection between ‘what is inferred’ and ‘what is the case’.

It would be easy to deny the force of the latter criticism, if there were some independent reason to assume that over time new evidence leads people, individually and collectively, to change their beliefs in the direction of objective truth. If that were true then the subjectivity, quirkiness and perhaps plain irrationality of their initial beliefs would not matter. Now this may be true in some sciences, which study phenomena generated by some underlying structure of reality that is fixed and invariable at least on the time-scale of human existence. Those sciences expect ‘stationary’ series of data and by and large find that the data meet their expectations. Over time, their series of data converge, or appear to converge, on some value and thus reveal, or appear to reveal, an increasingly clear picture of the underlying reality. However, it requires a leap of faith to generalise from the few sciences for which that assessment holds, or appears to hold, to all pursuits of knowledge, including economics.

Another reason to dismiss Glymour’s criticism would emerge, if it were true that, in the fields of science, Bayesian changes in the degrees of beliefs really reflect changes in scientific knowledge. However, the risk here is that scientific knowledge gets confused with its sociological proxy, the ‘consensus among the experts that constitute the scientific community’—or even, in at least some cases, the consensus of those who, because of their institutional positions and control of academic appointments and research grants, determine who shall be admitted to that select ‘community’.

Glymour also noted that Bayesianism does not address most of the standards of sound methodology that scientists use in judging the scientific value of research, theoretical work and the confirmation of theories. This raises the question, what, if anything, Bayesianism contributes to the philosophy of science, at least if the latter is conceived as an endeavour to safeguard the integrity of science.

Glymour’s most forceful criticism deserves to be quoted in full.

What we want is an explanation of scientific argument; what the Bayesians give us is a theory of learning, indeed a theory of personal learning. But arguments are more or less impersonal; I make an argument to persuade anyone informed of the premises, and in doing so I am not reporting any bit of autobiography. To ascribe to me degrees of belief that make my slide from my premises to my conclusion a plausible one fails to explain anything not only because the ascription may be arbitrary, but also because, even if it is a correct assignment of my degrees of belief, it does not explain why what I am doing is arguing—why, that is, what I say should have the least influence on others, or why I might hope that it should.

⁴ Clark Glymour, *Theory and Evidence* (1980), p.63-94.

The Bayesian's reply to this criticism is that the theory should only be applied when there is 'probative evidence'. Other types of evidence should not be allowed to alter the prior probabilities or degrees of belief. Obviously, that reply begs all the important questions, most certainly in those sciences where the very nature of what constitutes 'probative evidence' is in dispute. The reply is equally question begging in situations where people must conform to the intellectual fashions of the day to gain access to the hallowed 'community of scientists'.

Are all propositions probabilistic?

Looking for an easy way out of their dispute, Caplan seizes upon Block's statement that synthetic a priori propositions 'have been mischaracterised as being certain. This is not so.' Thus, Block seems to admit that one should 'incorporate probability' into synthetic a priori propositions (B, p.72). Caplan sums up: 'If synthetic a priori claims vary in degree of probability, they can no longer be treated as scientifically superior to empirical claims. Furthermore, while empirically testing absolutely certain a priori claims is pointless, empirically testing uncertain synthetic a priori claims is not' (C,p.83).⁵ Surely, however, what we have here is a conclusion based on a sloppy colloquial expression for which Block should be chided, not praised.

Block carelessly writes that empirical statements are 'intrinsically probabilistic'. They are not. There is nothing probabilistic about statements such as 'My neighbour's cat died last night' or 'Between January and September, the supply of money (M1) decreased by approximately 10%'. We may have doubts about the truth of such statements, but having doubts is different from having grounds—of the a priori or frequency kind—for assigning probabilities to their being true. Whether we have such grounds or not, the statements are empirical in any case. However, since we can make sense of them even if we have no such grounds, they certainly are not intrinsically probabilistic.

Then Block compounds his error by making the claim about the need to 'incorporate probability' into synthetic a priori propositions. His argument is that the more complex such propositions are, 'the greater the opportunity for human error'. Therefore, according to Block, a complex proposition—surely, it does not matter whether it is synthetic a priori or not—is less 'certain' than a simple one.

That is plausible *prima facie*, but it is not true. We can be more certain of a proposition requiring a complex proof (deductive or empirical) that has been checked many times by competent, diligent researchers than of a proposition requiring a relatively simple proof that no one so far has bothered to look at. Which one, then, is the more probable or certain proposition?

⁵ Caplan goes so far as to suggest that an empirical refutation of the Pythagorean theorem would carry as much weight as a mathematical disproof. Whether mathematicians agree with that statement is another thing. Let us take another example: the famous Banach-Tarski theorem. It states that a solid sphere can be divided in many pieces in such a way that it is possible to rearrange those pieces to constitute two solid spheres, each of which has the same volume as the original one. As far as I know, after nearly eighty years, the theorem still stands, although there is no empirical evidence whatsoever for it and every attempt to verify (or even exemplify) it empirically has failed. Banach, S. and Tarski, A. "Sur la décomposition des ensembles de points en parties respectivement congruentes." *Fund. Math.* 6, 244-277, 1924. Stromberg, K. "The Banach-Tarski Paradox." *Amer. Math. Monthly* 86, 3, 1979.

Austrian apriorism does not imply human infallibility. Human fallibility does not imply that all statements are probabilistic statements, let alone probability statements. It does not even imply that there always is a ground for doubting anything whatsoever. It does imply that individual or collective displays of self-confidence or high degrees of belief never by themselves constitute probative evidence.

Block's concession about the probabilistic nature of synthetic a priori claims is unwarranted. Therefore, Caplan's use of it, while understandable in the context of his discussion with Block, has no bearing on the substantial issue of the debate.

Praxeology, Bayesianism and the Minimum Wage Effect

Colloquialisms may get in the way of a proper understanding of praxeology. Block gives the following example of an economic synthetic a priori proposition: 'The minimum wage law causes unemployment of unskilled workers, other things equal' (B, p.67) Caplan then discusses the minimum wage effect in his reply. So, let us take a closer look at it.

We should be aware that, in a discussion between an Austrian praxeologist and a Bayesian neoclassical empiricist, the word 'cause' should not be presumed to have the same meaning for both parties. In fact, I doubt that 'raising the minimum wage causes unemployment' is a praxeological statement at all, although I do not deny that there are sound praxeological reasons for believing it to be true. Such reasons are the statements:

-a) A person (that is, in pure praxeology speech, a purposive agent) looking for a paid employment will not be deterred from looking for one merely because a policy is implemented that mandates a rise of the minimum wage.

-b) A person who is not currently looking for a paid employment may start to do so for no other reason than that a policy is implemented that mandates a rise of the minimum wage.

-c) A person looking to hire an employee may be deterred from doing so for no other reason than that a policy is implemented that mandates a rise of the minimum wage.

-d) A person currently employing people for a wage may want to reduce the number of people he employs for no other reason than that a policy is implemented that mandates a rise of the minimum wage.

It follows that, unless in a particular case for no person a rise of the minimum wage is a reason to change his plans, either one or both of the following will be true. 1) At least one person will seek employment who would not have done so if the minimum wage had not risen. 2) At least one person will cease (to seek) to employ another who would not have done so if the minimum wage had not risen. As part of a praxeologist's argument, 'Raising the minimum wage causes unemployment' is merely a colloquial summary of the above.

Of course, with respect to a particular historical occurrence, the praxeologist cannot exclude on a priori grounds that the unless-clause of the last paragraph is satisfied.⁶ On the other hand, if his experience and knowledge of social and

⁶ To get a proper perspective on the discussion between Block and Caplan, we should keep in mind Mises' distinction between theory and history (Mises, *Theory and History*, Yale University Press, 1957). Block presupposes that distinction, but Caplan apparently does not. Hence, he attacks Block

economic history are like ours, they provide justification for his appraisal that it is very unlikely that no person will revise his plans upon learning that the minimum wage will go up. Hence, in most situations, he is justified in saying that the policy will lead to increased unemployment—a larger gap between labour offered and labour demanded. However, he will not make that statement in situations where the minimum wage is raised only symbolically, for example, to a level still below the prevailing market rate, or where there is no way to enforce it. Nor will he say that the effect is likely to occur in situations where only a few persons would be affected by a raise of the minimum wage. The reason is that, for all he knows, in the latter case, all of those persons may have other reasons for not changing their plans. Nothing in praxeology warrants the statement that, in every sort of situation and in every group (however small), merely because the mandated minimum wage goes up at least one person will begin to look for a paid job or for ways to restrict employment.

The praxeologist will not even attempt to predict, on praxeological grounds, by how much unemployment will rise following a raise of the minimum wage. However, as an observer of social reality, he is as well placed as anybody else to venture an opinion on the quantitative effects of such a raise on a particular occasion.

Where do ‘strong prior beliefs’ come from?

Praxeological reasoning explains why even positivists and empiricists among economists have what Caplan calls ‘strong prior beliefs’. Caplan disputes this (C, p.80-81). He denies the claim made by Block that ‘neoclassical economists are covert praxeologists’. Instead, Caplan asserts that ‘neoclassical economists are Bayesians with some strong prior [beliefs]’. However, this will not do. A pertinent question is this: Where did the neoclassical economists get those strong prior beliefs? One answer is psychologically plausible: Most economists get their strong beliefs from their teachers, professors and textbook authors. Indeed, but where did their teachers, professors and textbook authors get those beliefs? To cut short an infinite regress, we should assume that at some point someone originated those beliefs. However, on what grounds did that person, or those persons, hold those beliefs?

If one refuses to accept praxeological reasoning as the foundation for those beliefs, one might think that they were based on empirical evidence. That, however, is unlikely because the minimum wage ‘theorem’ presumably entered the economics literature long before any (especially systematic or rigorous) attempts to observe the effects of a raise of the minimum wage were made. Perhaps, the strong prior beliefs of neoclassical economists were derived from the analogy of minimum prices in general, of which there were presumably already some well-documented occurrences. Perhaps, but all the evidence points to an origin that had

for not accepting that certain historical / empirical claims are as ‘scientifically’ valuable as certain synthetic a priori claims. However, the Misesian position (which Block presumably shares) is not that all historiography is ‘unscientific’ but that it provides a different kind of knowledge than economic theory does. It implies that historiography must be based on sound theory and cannot be used to arrive at universal laws by some sort of inductive generalisation or Bayesian adaptation of beliefs.

little to do with empirical evidence and much with the flowering of the ‘moral sciences’, economics among them, that relied on a general knowledge of human nature and in particular an appreciation of man’s rationality. Many of the strong initial beliefs of economists can be traced at least to the late-scholastics of the sixteenth century or to a number of eighteenth century French economists, who, if Rothbard’s interpretation⁷ is correct, were pioneers of the praxeological style of economic analysis.

Would the strong initial beliefs of economists on the effects of raising minimum wage have had a different origin? Could they have had a different origin? Was there ever sufficient empirical evidence of those effects to explain a Bayesian slide from a neutral belief to a strong one? For what it’s worth, in my opinion there was not. Moreover, if the Card-Krueger results,⁸ to which both Block and Caplan refer, hardly make a dent in a neoclassical Bayesian economist’s beliefs—as Caplan alleges (p.81-82)—then we may well ask what sort of evidence would make a significant difference in this matter. If an economist can dismiss a priori conclusions based on general axioms (perhaps because the proof of those conclusions would be complex), why can he not dismiss with much better reason any empirical evidence that comes his way? After all, checking even a complex deductive proof is a much easier task, requiring much less specialised skills, than checking the results of an empirical investigation and the methods of data-collection, data-processing, statistical analysis, model building and the like, that it involves. That is true especially if the empirical investigation is not a repeatable experiment. When running the same data through different procedures yields different results—hardly an uncommon occurrence in empirical economics—then the question arises, what, if anything, do the data prove?

Moreover, an application of the Bayesian Rule to ‘degrees of belief’ rather than objectively measurable frequencies or otherwise known probabilities is itself a questionable procedure. As a part of a blackboard illustration of the mathematics of the Bayesian Rule, Caplan’s statement that ‘based on his pre-Card-Krueger knowledge,... Gary Becker’s [prior degree of belief in the proposition] ‘minimum wage reduces employment’ was 98%’ (C,p.81) is unobjectionable. As part of a theory of scientific belief formation, it invites questions about the propriety of assigning cardinal values to, making interpersonal comparisons of, and aggregating, degrees of belief.

Why should we follow Caplan in using [a hypothetical] Gary Becker to illustrate that the Card-Krueger results make no more than a relatively small difference in an economist’s beliefs about the minimum wage effect? Why not ask [the real] Card and Krueger about the significance of their result? Why not ask the editors and the referees who decided to accept and to publish the Card-Krueger piece, rather than, like Block’s dissertation advisors, to return it with the advice ‘to go run these regressions again until you get them right’? Why not ask all the certified economists all over the world who liked the Card-Krueger finding, and wanted to believe it, because of their political and ideological prejudices? One should hope,

⁷ M.N. Rothbard, *Economic Thought Before Adam Smith: An Austrian Perspective on The History of Economic Thought*, Volume I, Edward Elgar, Adershot, 1995.

⁸ David Card & Alan B. Krueger, ‘Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania’, in *American Economic Review*, Volume 84, (4), p. 772-93.

indeed, that Caplan agrees that Gary Becker is only one neoclassical economist and not the oracle whose degrees of beliefs [should] determine those of the ‘community of neoclassical economists’. Perhaps there are reasons for assigning a higher weight to Becker’s degrees of belief than to another economist’s, but on what grounds? Anyway, since when are questions of science to be settled by opinion polls?

The Bayesian Rule is a procedure for processing numbers. Unless we have independent methods for assessing the relevance and validity of those numbers, it is no more than that—a garbage-in-garbage-out procedure.

Neoclassical economics, whether of the straightforward empiricist or of the empiricist-Bayesian variety cannot—and therefore should not try to—do more than establish correlations between one particular set of data and another. It can never explain why the correlation it finds exists. It can never explain what causes what.

Suppose we had accurate and complete data on the minimum wage effect from the dawn of civilisation to the present. Would any neoclassical economist suggest that, as time progresses, we should find that the observed data converge on a single value of the ratio of the correlation between the rate of change of employment and the rate of change in the minimum wage? Or would he suggest that we should expect that value to jump up and down? Would he suggest that we should expect the degrees of belief of economists in a particular value of that ratio to converge? Or would he suggest that economists are only human and that we should expect their beliefs to vary from one person to another, especially when we take into account that what is ‘evidence’ for one economist may be no more than a fluke for another?

Praxeology and the limits of empirical evidence

Praxeology does not seek to prove an immediate causal relationship between raising the minimum wage and rising unemployment. It endeavours to explain why raising the minimum wage gives purposive agents a reason to change their plans and why these adjustments will result in more unemployment—unless, of course, other more or less simultaneous events or conditions give those agents reasons for not making those adjustments. Whether or not one can identify any of those other events or conditions in a particular historical context does not affect the validity of the praxeological argument.

Let there be a praxeological proof that event A causes phenomenon B—‘cause’ having here the praxeological sense that we noted before. Even if it were true that B rarely or never follows an occurrence of A, that proof would hold. Indeed, all that historical or empirical evidence can demonstrate is that, so far, in the circumstances in which A did occur, not enough persons had a sufficient reason to do what they would have to do to make B happen on a scale where it could be detected empirically.⁹

⁹ Thus, Leland B. Yeager concludes a recently published paper with the caveat: ‘The entire discussion serves an analytical purpose only.... It does not claim that the effects described are quantitatively important and detectable amidst all the constantly occurring changes in economic conditions.’ L.B. Yeager, ‘Land, Money, and Capital Formation’, J.G. Backhaus a.o. (eds), *Economic Policy in an Orderly Framework*, Wirtschaft: Forschung und Wissenschaft, Band 5, Lit Verlag, Münster,

For an empiricist, the meaning of ‘A causes B’ is very different. It is merely a colloquial expression for ‘[In most cases,] A is followed by B’. Praxeology gives him no reasons to believe that such a statement is true. His reasons, presumably, are (preferably long) series of data processed according to one procedure or another.

Suppose that, based on the accumulated negative evidence, a Bayesian empiricist’s degree of belief $P(A \text{ causes } B)$ is as close to zero as a degree of belief can be. Then, out of the blue, A occurs and is followed by B. The Bayesian empiricist may register a very small rise of his degree of belief for ‘A causes B’ and perhaps also for his belief ‘Probability rules the universe’. The praxeologist can do more. He can explain why people make B happen when A occurs and he can explain why this particular occurrence of A is exceptional or indicative of a significant change in people’s circumstances.

The praxeological proof holds even if no one has ever observed an occurrence of A: a praxeologist can proceed to explain why, if there are no more pressing concerns, people who face A will bring about B, the praxeological effect. On the other hand, empiricism has nothing to say about such an unobserved A or what it may cause. It would have to wait until a theory was proposed that implied, say, that not every instance of B could be explained unless we assumed the existence of A. Then it would have to look for ways to devise a scientifically relevant empirical test for that theory. Now, where would such a theory come from, if not from praxeology?

Even if we discard the criticisms of the Bayesian paradigm of science, it seems to me that there are gaps between praxeology and empiricism no amount of Bayesianism can close.

2003, 455-469. Surely, the caveat does not qualify the paper as not contributing to scientific economic knowledge. It merely directs any relevant criticism to consider the logic of the discussion.