PROBABILITY AND THE SYNTHETIC *A PRIORI*: A REPLY TO BLOCK

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Walter Block's (2003) supposedly "entirely critical" (p. 48) reply greatly narrows the gap between our positions (Caplan 2001, 1999, 1997; Block 1999). It is a major concession for Block to admit that synthetic *a priori* propositions can have a low probability and empirical propositions can have a high probability. The implication, though he does not admit it, is that his initial claims most contrary to common sense are mistaken.

Furthermore, on the general topic of the synthetic *a priori*, Block incorrectly classifies me as a logical positivist. At no point in my critique did I fault Austrians for their belief in the synthetic *a priori*, or dismiss any of their positions for failing to be "verifiable" or "falsifiable." My position is not that their key arguments are methodologically flawed, but that they are mistaken. It may surprise Block, but I fully recognize the existence of synthetic *a priori* truths. However, most of his economic examples do not qualify.

Block's misunderstanding of my methodological position is partially understandable. Though his stereotype does not fit me, it is grounded in fact. Neoclassical methodologists have long subscribed to some version of logical positivism that working economists routinely flout. But there is no need, I shall argue, for neoclassicals to become praxeologists to eliminate the contradiction. All they need to do is jettison logical positivism and become thoroughgoing Bayesians (Howson and Urbach 1989).

My reply is organized as follows. The second section shows that if synthetic *a priori* claims, like empirical ones, vary in probability, then Block has no reason to single out the latter as unscientific. Section three sketches my position on the synthetic *a priori*, and explains the value of empirical testing

of less-than-certain *a priori* propositions. Section four provides a nonpraxeo-logical solution for the methodological schizophrenia that Block correctly diagnoses in mainstream economics. Section five concludes.

ARE HIGH-PROBABILITY EMPIRICAL CLAIMS "SCIENTIFIC"?

Towards the end of his reply, Block provides the reader with a useful table. Its rows distinguish between empirical and synthetic *a priori* statements, its columns between low and high probability statements (p. 55; table 2). He then gives examples of all four logical possibilities; for Block, none of the categories are empty. There are high-probability empirical claims, such as "Elephants are very heavy," and low-probability synthetic *a priori* claims, like the Pythagorean Theorem. How can synthetic *a priori* propositions vary in probability? As Block explains: "the more (and more complicated) the steps [in a logical proof], the greater the opportunity for human error"[1] (p. 55; table 2). For example, it is less probable that the minimum wage leads to unemployment than that I now exist. (Table 2 appears in Block's article in the current issue of this journal.)

Block's table is eminently sensible. Unfortunately, it pulls the rug out from under the methodological position of his earlier reply. In particular, consider the "extreme" claims for which I criticized Block (Caplan 2001, p. 69), such as:

1. The envious "have no way to *prove* they lose out at all."

2. "Scientifically we can know *nothing* about states of mind not demonstrated in action."

3. "How can we, as economists, even know they [states of mind not demonstrated in action] exist?"

Where do claims about states of mind fit in Block's typology? At least a fair number must fall into category II—high-probability empirical claims. Take for example "Most socialists would be offended by *Defending the Undefendable*." It is hardly true *a priori*, yet it is plainly true. How then can Block label such a claim scientifically empty? Indeed, how can he call it scientifically *inferior* to a low-probability synthetic *a priori* claim? His own typology offers no basis for doing so.

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[1]A corollary, which Block does not explicitly state, is that measures to reduce the opportunity for human error in a proof raise its probability. Even a complex theorem becomes virtually certain if thousands of mathematicians have checked and rechecked it for thousands of years. For this reason, the Pythagorean Theorem is a poor example of a low-probability synthetic *a priori* claim. For the sake of convenience, however, I will stick with Block's chosen example.

One can only guess at the rationale Block would offer. He has already foreclosed one route: equating "scientific knowledge" with certainty. After all, he admits that many synthetic *a priori* claims—like empirical ones—fall short of certainty. If "Most socialists would be offended by *Defending the Undefendable*" is unscientific *due to lack of certainty*, so are claims about the minimum wage.

Might Block instead equate "scientific knowledge" with "synthetic *a priori* knowledge"? This would be a peculiar position. No one dubs physics and chemistry "unscientific" due to their empirical character. Why should common-sense claims about people's preferences be held to a higher standard?

To put my point in context: The reason I originally introduced probability was not "as a stick to beat up on the Austrian concept of the synthetic *a priori.*" (Block 2003, p. 48) My goal, rather, was to use probability as a stick to *ward off* Austrians' unwarranted dismissal of common-sense empirical claims about human preferences, indifference, and the like. By admitting that the empirical can be more probable than the synthetic *a priori*, my critic has distanced himself from the weakest parts of his first reply.

A COMMON-SENSE APPROACH TO THE SYNTHETIC *A PRIORI*

Many of Block's examples of the synthetic *a priori* are poorly chosen. Demand curves, as previously explained, do not necessarily slope downward. Rothbard's own discussion of backward-bending curves admits as much. Nor is it necessarily true that raising the minimum wage reduces employment, *ceteris paribus*. It does not have to do so under monopsony.[2] Or to take a more fanciful counterexample, imagine that employers have been hypnotized to believe that the minimum wage declines every time Congress increases it. Then raising the minimum wage would increase employment instead of decreasing it, at least until bankruptcy set in.

In spite of these objections, and contrary to my critic's suspicions, I readily admit that synthetic *a priori* knowledge exists. I have no complaints about Block's noneconomic examples (p. 49). My objections to "There are mutual benefits to trade in the *ex ante* sense," and "Man acts (to create a world more to his liking in the future than one which would arise but for his action)," are only quibbles. To be strictly true, these assertions would have to allow for indifference: "There are mutual nonlosses to trade in the *ex ante* sense," and "Man acts (to create a world as or more to his liking in the future than one which would arise but for his action)." My difference with the Austrians, then, is not on the general question of synthetic *a priori* knowledge, but on

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[2]Block is incorrect (p. 53, n. 19) to state that I would "demolish the monopsonistic argument" in a graduate class. I think that monopsony is a *bona fide* exception. This does not mean, of course, that raising the minimum wage always increases the quantity of labor a monopsonist employs, but it can under certain conditions.

the particular question of what *qualifies* as synthetic *a priori*. My list is more selective, but it is not blank.

Still, if there are only a handful of *bona fide* synthetic *a priori* truths in economics, the relative value of empirical work increases. Moreover, once you admit, as Block does, that synthetic *a priori* claims vary in probability, then even the synthetic *a priori* can be empirically tested. Contradictory? Not at all. It is true, as Austrians have long insisted, that there is no point empirically testing synthetic *a priori* claims *known with certainty*. Empirically testing less solid synthetic *a priori* propositions, in contrast, can be fruitful.

Imagine that we were uncertain about the Pythagorean Theorem. We could spend more time checking the steps of the proof. On the other hand, we might measure a wide variety of triangles to verify that $c^2=a^2+b^2$. *Both efforts would rationally tend to increase our confidence in the theorem, even though neither is foolproof.* If we found a clear empirical counterexample, we would conclude that either the premises or the reasoning behind the theorem were incorrect, despite our inability to pinpoint them. Similarly, if we have doubts about Austrian business cycle theory, we might spend years reviewing the steps of the proof, or we might study economic history and look for counter-examples. If less-than-certain empirical evidence contradicts an absolutely certain synthetic *a priori* claim, then, as the Austrians advise, we should ignore the empirics. The opposite holds if indubitable empirics contradict a less-than-certain synthetic *a priori* claim.

A Bayesian Cure for Methodological Schizophrenia

When Block dissects my lecture notes on the minimum wage, he states: "Would he say it is very *likely* that this would cause unemployment, or that it is undeniable that it would? I very strongly suspect the latter" (p. 53). He suspects incorrectly. I have already stated that demand curves do not necessarily slope downward, so it would be illogical to proclaim that the disemployment effect of the minimum wage holds without exception. The reason for the unambiguous nature of my lecture notes is precisely the one Block prematurely rejects: They were written for an undergraduate class, where it is hard enough to communicate the main point, let alone any subtleties.

Why then, as Block insightfully asks, do economists react so violently to empirical evidence against the conventional view of the minimum wage's effect? Why did Block's dissertation advisor make him run regressions on rent control, then dismiss his anomalous results out of hand? There are three explanations worth considering:

> Explanation #1: Neoclassical economists are covert praxeologists, though they will not admit it.

Explanation #2: Neoclassical economists are intellectually dishonest dog-matists posing as empirical scientists.

Explanation #3: Neoclassical economists are Bayesians with some strong priors.

Explanation #1 is essentially Block's: Neoclassical economists suffer from methodological schizophrenia. When they shut their eyes and close their ears to contrary empirical evidence on the minimum wage, neoclassicals are doing the epistemically correct thing even though it contradicts their official methodology.

Explanation #2 is routinely offered by economists who publicize their counter-intuitive empirical findings. To some extent, they have a point. There are more than a few economists who claim to be empiricists, but never change their minds about anything—even on questions that Mises himself would have seen as empirical.

For the most part, though, I prefer Explanation #3. The Bayesian model of belief formation permeates neoclassical economics. Since it will be unfamiliar to many Austrians, let me elaborate. The Bayesian model states, in essence, 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 comes in—ideally according to Bayes' Rule, one of the most important formu-las in probability theory (Varian 1992, pp. 190-2):

$$P(A \mid B) = \frac{P(B \mid A)^* \ P(A)}{P(B \mid A)^* \ P(A) + P(B) \mid \text{not } A)^* \ P(\text{not } A)}$$

P(A) is the "prior probability" of A—the degree of belief you assigned to A before seeing the evidence, B. P(A|B), read "the probability of A *given* B," is your "posterior probability"—the degree of belief you assign to A *after* seeing the evidence, B.[3]

Now consider the implications of Bayes' Rule for the Card-Krueger mini-mum-wage debate. Suppose, based on his pre-Card-Krueger knowledge, that Gary Becker's P(minimum wage reduces employment) was 98 percent. In Bayesian language, he had a "strong prior": he was almost, but not quite, sure of the standard conclusion. But recognizing the flaws of empirical research, he might have only weak expectations that any one study would confirm his view. Thus, Becker's P(Card-Krueger results| minimum wage reduces employ-ment) is, say, 30 percent. Even if he is right about the minimum wage, there is still a 30 percent chance that a careful study by two Princeton economists will find the opposite. Becker would presumably have similar doubts about

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[3]Note that the basis for both the prior probability and the subsequent evidence could be either synthetic *a priori* or empirical, though admittedly most applications emphasize the latter.

P(Card-Krueger results| minimum wage does not reduce employment). Even if the conventional view were *wrong*, it is hardly certain that the Card-Krueger empirical test would expose it as such, so Becker assigns this conditional probability a mere 70 percent.

Given all this, what probability should Becker assign to his original view after reading Card and Krueger's paper? Using Bayes' Rule:

P(minimum wage reduces employment|the Card-Krueger results)

$$= \frac{.3*.98}{.3*.98 + .7*.02}$$

which simplifies to 95.5 percent. This is of course less than Becker's prior probability of 98 percent. The important fact, however, is how *little* his probability changes. He can rationally remain almost certain that the minimum wage reduces employment, although he must be slightly less certain than before. Given his initial certainty and the imperfections of the test, to change his mind more would be an overreaction.

Now consider Block's assertion that "[I]f the basic supply and demand analysis of rental housing is only 'probable,' then my (erroneous) results should have had as much standing as those of anyone else, including my own 'correct' findings" (p. 54). In a sense this is true—if you become more sure when you find a negative effect of rent control, you must become less sure when you find a positive effect. This does not imply, however, that both findings are *equally* reliable. Indeed, Bayes's Rule shows that we should automatically be more skeptical of empirical evidence in favor of an improbable conclusion.

How so? Compare the P(Block's econometrics were flawed| he found rent control causes shortages) to P(Block's econometrics were flawed| he did not find that rent control causes shortages). Suppose that P(Block's econometrics were flawed) is 30 percent, P(he found rent control causes shortages| Block's econometrics were flawed) is 45 percent, and P(he found rent control causes shortages| Block's econometrics were not flawed) is 80 percent. Then P(Block's econometrics were flawed| he found rent control causes shortages) is:

$$\frac{.45*.3}{.45*.3 + .8*.7} = 19.4\%$$

On the other hand, P(Block's econometrics were flawed| he did not find that rent control causes shortages) is:

$$\frac{.55*.3}{.55*.3 + .2*.7} = 54.1\%$$

On these assumptions, Block's anomalous results were almost three times as likely to be flawed as his regular results. His dissertation committee justifiably greeted the former with extra skepticism.

The Bayesian position thus stakes out a compelling middle ground between atheoretical positivism and praxeology. On the one hand, the Bayesian view emphasizes that few propositions in economics 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 naive faith in the last prominent study.

CONCLUSION

Contrary to Block, the synthetic *a priori* has little to do with our dispute. My critique of the Austrians is not that their methods are "unscientific," but that their most distinctive positions are false or overstated. Yet Block's latest reply does inadvertently make Austrian economics more reasonable. 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 synthetic *a priori* claims is pointless, empirically testing uncertain synthetic *a priori* claims is not.

As is often the case with Austrians, Block is better at criticizing neoclassicals than he is at producing a sound alternative. He is right that most economists do not practice the logical positivism that they preach. He is also correct to maintain that logical positivism is mistaken. However, both of these problems can be resolved if neoclassical economists themselves adopt the Bayesian model of belief formation that they routinely apply to everyone else.

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