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REJOINDER TO CAPLAN ON BAYESIAN ECONOMICS

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TO THE BEST OF my knowledge, there were three *Methodenstreits* (or debates in the literature over the proper method to use in economics) and a handful of "mini-*Methodenstreits*" in which Austrians took part.

The first *Methodenstreit* was the most famous, and established the Austrian School of economics. It took place in the latter two decades of the nineteenth century, with the historicists Gustav von Schmoller, Lujo Brentano, Adolf Wagner, and Werner Sombart on one side, and the Austrians Carl Menger, Eugen von Böhm-Bawerk, Friedrich von Wieser, and Ludwig von Mises on the other.[1]

The second started in the mid-twentieth century with the publication of Friedman's classic article on methodology, followed by a plethora of others reacting to it. But here, unlike the first, where the two sides were evenly matched at least in terms of numbers, only a few Austrians participated.[2]

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[1]See Ludwig von Mises, *The Historical Setting of the Austrian School of Economics* (Auburn, Ala.: Ludwig von Mises Institute, 1984); "Methodenstreit," www. wikipedia.org; and Walter Block, "Reply to Caplan on Austrian Economic Methodology" (unpublished manuscript; department of economics, Loyola University).

[2]Milton Friedman, "The Methodology of Positive Economics," in *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953), pp. 3–43. A conference on this seminal article took place in 2003, but no Austrians were invited. See http://www2.eur.nl/fw/philecon/Friedman53.html for a list of participants. The Austrian perspective included Glenn Fox, "Economics as Prediction," Chap. 5 in *Reason and Reality in the Methodologies of Economics: An Introduction* (Cheltenham, U.K.: Edward Elgar, 1997); and Samuel Bostaph, "Epistemological Foundations of Methodological Conflict in Economics: The Case of the Nineteenth Century *Methodenstreit*" (Ph.D. diss., Southern Illinois University at Carbondale, 1976), pp. 119–39.

There were also what might be regarded as several mini-*Methodenstreits*, consisting of only two or three entries; a neoclassical argument and an Austrian reply with no follow up, or an exchange between only two participants.[3]

We are now in the midst of the third such *Methodenstreit*, which began with an article by Bryan Caplan, and has subsequently included articles by William Barnett, Walter Block, Guido Hülsmann, and Frank van Dun, as well as replies by Caplan.[4] Note that this *Methodenstreit* is almost the opposite of the second in terms of the proportion of participation of Austrians and neoclassicals. That one, launched by Friedman, included only two Austrians, along

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[3]One such example involved Robert Nozick and Walter Block. See Robert Nozick, "On Austrian Methodology," *Synthese* 36 (1977): 353–92; and Walter Block, "On Robert Nozick's 'On Austrian Methodology,'" *Inquiry* 23, no. 4 (Fall 1980): 397–444. For a second example, see G.J. Schuller, Review of "*Human Action* by Ludwig von Mises," *American Economic Review* 40, no. 3 (June 1950); Murray N. Rothbard, "Mises's 'Human Action': Comment," *American Economic Review* 41, no. 1 (March 1951); G.J. Schuller, "Rejoinder," *American Economic Review* 41, no. 1 (March 1951); and Murray N. Rothbard, "Praxeology: Reply to Mr. Schuller," *American Economic Review* 41, no. 4 (December 1951).

For more on the debates between Austrians and their intellectual opponents, see Walter Block, Christopher Westley, and Alex Padilla, "The Case for Refereed Journals Devoted to Austrian Economics Revisited (Are there any objective criteria for determining truth in economics?)," (unpublished manuscript; department of economics, Loyola University).

[4]Bryan Caplan, "The Austrian Search for Realistic Foundations," *Southern Economic Journal* 65, no. 4 (April 1999): 823–38.

Austrian responses to Caplan include William Barnett, II, "Contra Caplan" (unpublished manuscript; department of economics, Loyola University New Orleans); Walter Block, "Austrian Theorizing, Recalling the Foundations: Reply to Caplan," *Quarterly Journal of Austrian Economics* 2, no. 4 (Winter 1999): 21–39; Walter Block, "Realism: Austrian vs. Neoclassical Economics, Reply to Caplan," *Quarterly Journal of Austrian Economics* 6, no. 3 (Fall 2003): 63–76; Block, "Reply to Caplan on Austrian Economic Methodology"; Jörg Guido Hülsmann, "Economic Science and Neoclassicism," *Quarterly Journal of Austrian Economics* 2, no. 4 (Winter 1999): 3–20; and Frank van Dun, "Bayesianism and Austrian Apriorism," http://allserv.rug.ac.be/-frvandun/Texts /Articles/Bayesianism-Austrian.pdf.

Caplan, in turn, has offered replies to his critics. See Bryan Caplan, "Probability, Common Sense, and Realism: A Reply to Hülsmann and Block," *Quarterly Journal of Austrian Economics* 2, no. 4 (Summer 2001): 69–86; and Bryan Caplan, "Probability and the Synthetic *A Priori*: A Reply to Block," *Quarterly Journal of Austrian Economics* 6, no. 3 (Fall 2003): 77–83.

with a myriad of neoclassicals. The present one consists of Caplan, the only neoclassical, and four Austrians.

In the present article, it is my goal to critically comment on Caplan's most recent argument. The next section is devoted to Probability and Science, followed by a section on Synthetic *a priori* and Common Sense, and then a section on Caplan's Bayesian Cure.

PROBABILITY AND SCIENCE

I fear that a serious misunderstanding has sprung up between Caplan and myself on the relationship between probability and the synthetic *a priori*. I relish the opportunity to try to set matters straight.

Throughout his most recent article, Caplan focuses upon my supposed "concession" regarding synthetic *a priori* statements and probabilities. He writes: "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." Caplan also labors under the false assumption that I maintain that empirical claims are "unscientific."[5]

In my view, the *only* sense in which the truth of synthetic *a priori* statements can be uncertain is in the *psychological*, not the *logical*, sense.[6] Caplan, unfortunately, interprets me in precisely the wrong way on this matter.

Previously, I said that we could be less certain of synthetic *a priori* statements that involved long chains of reasoning, or convoluted deductions, and more sure of those that involved more direct logic, or simpler mathematical operations. For example, we can in this sense be more sure that the square root of 9 is 3 than that the square root of 2,187,441 is 1,479. Or, to use my example, the claim that a triangle has 3 sides is relatively simple, that it has 180 degrees in all three of its angles is moderately difficult to understand, and the Pythagorean Theorem is more complex yet.[7] But here, we are limited to discussing *psychological states* of people being confronted with these various truths. In terms of their *pure logic*, they are *all* of a piece; that is, to deny *any one* of these contentions is to commit an internal contradiction, a strict no-no in logic.

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[5]Caplan, "Probability and the Synthetic *A Priori*," p. 77.

[6]See Block, "Realism."

[7]Ibid., pp. 71, 72. To be clear, I assume that the base of these numbers is ten, and I posit Euclidian, not Riemannian, Lobachevskian, or some other type of geometry.

These claims are not testable; they are not falsifiable. There is no possible state of the world that would render them false. Yet, Caplan claims that since some of the more complex statements in this category can be *psychologically* confusing, their *logical* status has somehow been converted into mere empirical claims. Surely, this is a category mistake on his part.

On the other hand, based on my "concession" that, in the empirical world, too, there can be more or less certain claims with high and low probability of being correct, he concludes that we can be more confident of highly secure contingent claims than about difficult to see but necessary ones, for example, that we can rely more heavily on the statement "Elephants are heavy creatures" than on the Pythagorean theorem. However, say what you will about the former, its denial involves no self-contradiction, while to assert the contrary of the latter most certainly does. This is a category mistake on stilts.

I am tempted to say that I regret my Table 2 since it has sown so much confusion.[8] But, upon reconsideration, I do not, for it opens up avenues of exploration that otherwise would not have arisen. Using it shows that Caplan does not understand methodology.[9] Testing the Pythagorean theorem indeed. Why did I construct this table? Solely as an attempt to bridge what I saw as a yawning gap between Caplan's argument and my own. Caplan continually brought probability into play,[10] a concept I saw as beside the point.

Very well, I decided, if Caplan cannot argue in the absence of probability, let us by all means discuss it, but in a way that shows its irrelevance to the issues under contention. This is what I meant when I wrote "we can accommodate his concerns" in my presentation of Table 2.[11] In other words, I was utilizing the "arguendo" method in an attempt at a *reductio ad absurdum*. I was saying, in effect, that the entire notion of probability leads us astray from the discussion involving the distinction between contingent and necessary truths. However, I tried to make clear, if you must discuss it, here is a way to do so that need not deflect us from our main debate.

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[8]Block, "Realism," p. 71. In Caplan, "Probability and the Synthetic *A Priori*," all page number citations to Block, "Realism," are incorrect, apparently due to an editing error. In the present article, whenever I cite Caplan's article with reference to my prior article, I substitute the correct pagination.

[9]Van Dun, "Bayesianism and Austrian *A Priorism*," has similar misunderstandings over this table.

[10]See Caplan, "Probability, Common Sense, and Realism."

[11]Block, "Realism," p. 71.

Rothbard uses the arguendo gambit against Blum and Kalven, who write: "An equitable apportioning of sacrifice requires inflicting equal hurt on each taxpayer."[12] Rothbard responds:

> To a thoroughgoing individualist and libertarian, this basic goal of the sacrifice theorists reveals the utter absurdity of their position. Instead of worrying about what constitutes "equal hurt," why inflict any hurt at all? Why tolerate an institution that represents only pain and injury, and then try to find some sort of "equitable means" of spreading it around? The entire concept of "just and equal" in suffering is an absurdity. *Setting aside this point for the moment. . .*[13]

Rothbard then continues with his analysis of the writings of these two authors, *ignoring* the devastating criticism he has just leveled against them. Does this mean Rothbard has changed his mind, and now *accepts* the validity of equal hurt? Does this mean, to utilize Caplan's terminology, that Rothbard has "conceded" to Blum and Kalven the propriety of hurting people, so long as the hurt is spread around equally, or equitably? If you believe this, then you are ready to seriously entertain the notion that I "conceded" to Caplan that we can be *logically* more secure in believing well established empirical findings in economics than we can regarding less certain synthetic *a priori* statements, "less certain" *only* because they involve long chains of reasoning. This is *psychological, not logical,* uncertainty.

I claim that Gary Becker and other supposed neoclassicals are really praxeologists under the skin, based on their reaction to the Card-Krueger "finding" that minimum wage legislation has very different effects than previously supposed.[14] Caplan explains the Becker et al. reaction not in terms of praxeology, but on the basis of Bayesian probability theory.[15] I reject this analysis on the ground that it is based on yet another confusion between logic and psychology. Yes, for all I know, there may be any number of economists such as Becker, livid with the Card-Krueger article, and determined to

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[12]Walter J. Blum, and Harry J. Kalven, Jr., "The Uneasy Case for Progressive Taxation," *University of Chicago Law Review* 19 (1952): 417–520.

[13]Murray N. Rothbard, "The Uneasy Case for Degressive Taxation," *Quarterly Journal of Austrian Economics* 4, no. 1 (Spring 2001): 52; emphasis added.

[14]See David Card and Alan B. Krueger, "Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania," *American Economic Review* 84, no. 4 (September 1994): 772–93; cf. Gary Becker, "It's Simple: Hike the Minimum Wage, and You Put People Out of Work," *Business Week Magazine* (March 6, 1995).

[15]Caplan, "Probability and the Synthetic *A Priori.*"

show its flaws, who are motivated by something along the lines of Caplan's Bayesian analysis. But that is mere *psychology*. I am making an entirely different point. As I see it, I am not motivation-mongering. I am claiming, instead, that the critics, supposedly neoclassical, of the Card-Krueger argument *logically* could not take the stance they did without a healthy dose of praxeological insight.

On the Bayesian explanation, in contrast, the fact that minimum wage legislation leads to unemployment for unskilled workers is so well established that neoclassicists approach any deviation from this finding with great skepticism. But how, on this interpretation, did any such knowledge get established *initially* on the Bayesian account? It logically could not have been so established based on this system, because it requires a previous body of knowledge on the basis of which to examine new claims. The response, "it's turtles all the way down" seems to be appropriate here.

With this introduction to Caplanian methodology, we are ready to consider chapter and verse. What follows is an analysis of Caplan's statements. I shall quote him extensively, and then respond. I am led to this mode of presentation to forestall future misunderstandings between us. My hope is that if we cannot in the end agree, then we should at least achieve real disagreement, as opposed to presently passing each other as ships in the night.

Caplan writes:

> 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.[16]

It is no "concession" at all. It is merely evidence of an attempt to try to communicate more clearly with Caplan. Yes, "Elephants are heavy" has a high probability of being correct at any given time, and we are more likely to make mistakes in long involved sets of mathematical proofs, but these statements occur in two entirely separate universes of discourse, the former empirical, the latter *a priori*. Caplan is wrong to draw from this the implication he does. The "low probability" attached to a specific synthetic *a priori* statement is not with regard to its truth or its being correct, but rather relevant to the odds of arriving at it; e.g., it is true that, modulo ten, the square root of 2,187,441 is 1,479, but the odds that most people could figure this out on their own is, I suspect, small, especially given the large number of innumerate people in the world. There are certain truths for which the probability is small that people *will ferret them out*; nevertheless they are *indubitable* truths.

Caplan writes:

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[16]Ibid., p. 77.

> [I]f synthetic *a priori* claims, like empirical ones, vary in probability, then Block has no reason to single out the latter as unscientific.[17]

But I do no such thing. Indeed, a word search of the electronic version of my article does not reveal that I even used the word "unscientific,"[18] let alone asserted that empirical claims cannot pass muster under this criterion. Indeed, my view is the very opposite. As I see it, empirical claims most certainly *can* be scientific, in the physical sciences. It is only in fields like economics, mathematics, and logic, but not in the physical sciences, that we can do better, e.g., attain synthetic *a priori* knowledge.

Caplan writes:

> 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?[19]

There are more mistakes in this short quotation than you can shake a stick at. I do not at all label the claim that socialists would be offended by my book *Defending the Undefendable* "scientifically empty." Rather, this empirical claim seems very likely. It is not "scientifically" empty; rather, it is *praxeologically* empty, since its denial involves no self-contradiction. This claim about the sensibilities of socialists is a high-probability empirical statement. We cannot properly say it is scientifically inferior to a "low-probability" (again, only in the psychological sense) synthetic *a priori* claim. If the purpose of science is to discern truth, then every synthetic *a priori* statement is superior to every empirical statement. But, "you can't add apples and oranges." A synthetic *a priori* statement is true (if not, then it is not truly a synthetic *a priori* statement); however, an empirical statement is never true in this sense. Therefore, one really can't compare them regarding scientific value.

Caplan writes:

> [Block] has already foreclosed one route: equating "scientific knowledge" with certainty. After all, he admits that many synthetic

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[17]Ibid., p. 77.

[18]It would help to achieve real disagreement, and thereby promote rational discussion, if Caplan would explicitly cite me and quote me when he attributes specific claims to me.

[19]Caplan, "Probability and the Synthetic *A Priori*," p. 78, discussing Walter Block, *Defending the Undefendable* (New York: Fox and Wilkes, 1991); emphasis in the original.

> *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.[20]

But the lack of certainty between empirical claims such as those regarding my book, and synthetic *a priori* ones concerning the minimum wage, occupy entirely different philosophical categories. The former are uncertain due to our lack of information about the world. We think it likely that most socialists would be shocked and offended by *Defending the Undefendable*, but heck, there might be a few of them out there with a sense of humor, or who may even enjoy a brutal attack on their position.

The uncertainty concerning the minimum wage or the Pythagorean theorem is entirely of a different type. Here, we are not concerned only with a state of the world, but also with *necessary* conditions which cannot be denied but on pain of self-contradiction.

The ways we *prove* these two contentions are also very different. We can examine socialists and their feelings, or elephants and their weight, but we can never establish knowledge that is certain in the logical sense. After all, the next socialist down the pike, or the next elephant, can be an exception to the rule. Not so for synthetic *a priori* statements, not at all.

Caplan writes:

> 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.[21]

Not a bit of it. I fear I shall have to continue to wallow in my "weakness." Yes, "elephants are heavy" is an empirical claim that most would accept. In that sense, it is a very likely empirical claim. On the other hand, never in a million years would the average person be able to prove Gödel's Theorem. In the psychological sense, this insight is unlikely, or improbable. But this is truly comparing apples and oranges. It is as if Caplan is saying that the best weight lifter on the planet can bench press triple his own weight; therefore, such a person should be a great golfer or chess player. The one has nothing to do with the other.

Caplan writes:

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[20]Caplan, "Probability and the Synthetic *A Priori*," p. 79; emphasis in the original.

[21]Ibid.

> Many of Block's examples of the synthetic *a priori* are poorly chosen. Demand curves . . . do not necessarily slope downward. Rothbard's own discussion of backward-bending curves admits as much.[22]

Unhappily, Caplan leaves out an important part of my statement, which reads as follows: "Demand curves are downward sloping (e.g., when forced to give up one unit of a stock of a good, the economic actor will give up the least valuable of them)." Would Caplan be so rash as to claim that when forced to give up one unit of a stock of a good, the economic actor will give up other than the least valuable of them? If not, he then necessarily acquiesces with my contention. Of course, if income is allowed to vary as we move along a demand curve, it will not necessarily be downward sloping throughout its entire length. We all know about backward-bending demand curves and Giffen goods. This dispute is not about that. Rather, it concerns creating straw men and then attacking them.

SYNTHETIC *A PRIORI* AND COMMON SENSE

In a continuation of his claim that my synthetic *a priori* examples are "poorly chosen," Caplan writes:

> Nor is it necessarily true that raising the minimum wage reduces employment, *ceteris paribus*. It does not have to do so under monopsony.[23]

This ought to put paid to the notion, if ever entertained, that Caplan is some sort of subterranean Austrian, at least of the Rothbardian variety. After all, the concepts of monopsony and monopoly, when applied to the free-enterprise system, are dead from the neck up. The only legitimate denotations of these words are when the government grants an exclusive privilege, whether to a marketing board buyer (monopsony) or to a seller (monopoly). This is not the time nor place to go into the neoclassical fallacies concerning these concepts. Suffice it to say that no one has ever succeeded in demonstrating a perfectly competitive price or quantity with which to contrast the so-called market monopoly or monopsony price or quantity. Indeed, the entire concept of free-market monopoly or

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[22]Ibid. Caplan's refusal to directly quote me is apparently not personal; here, he fails to cite Rothbard. I always tell my students to document anything about which any reasonable person might take issue. Evidently, rules of documentation are not uniform. Why should the reader have to go through the trouble of finding Rothbard's actual discussion of this issue?

[23]Ibid., p. 79.

monopsony is a veritable contradiction in terms. The entire model is logically incoherent.[24]

Caplan, further continuing his claim that my cases in point of synthetic *a priori* are misconceived, writes:

> 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.[25]

Unfortunately for Caplan, that last bit, "at least until bankruptcy set in," gives away the entire point: Such craziness cannot last in the long run, *even given* these rather heroic assumptions. The piper must be paid: *Whenever* the minimum wage rises, *no matter what anyone thinks*, employment will eventually have to be reduced. But no one ever claimed any more for the minimum wage than that. Certainly, no competent economist ever asserted that this legislation would *immediately* create unemployment. In the very next microsecond, right after a rise in the mandated level but before human action can possibly react to the change, employment would not fall at all. It takes purposive choice on the part of entrepreneurs for any such result to ensue. Nor are we interested, praxeologically, in *how long* this market process will take, only that it *necessarily* must occur. It would appear that Caplan, despite his own best efforts to the contrary, agrees with this Austrian insight. He, in effect, demonstrates this with his "at least until bankruptcy set in" concession.

Caplan "quibbles" with two other of my synthetic *a priori* examples. He states:

> 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)."[26]

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[24]For a critique of neoclassical monopoly theory, see William Anderson, Walter Block, Thomas J. DiLorenzo, Ilana Mercer, Leon Snyman, and Christopher Westley, "The Microsoft Corporation in Collision with Antitrust Law," *Journal of Social, Political, and Economic Studies* 26, no. 1 (Winter 2001): 287–302; Thomas J. DiLorenzo, "The Myth of Natural Monopoly," *Review of Austrian Economics* 9, no. 2 (1997): 43–58; and Murray N. Rothbard, *Man, Economy, and State* (Los Angeles: Nash, 1970).

[25]Caplan, "Probability and the Synthetic *A Priori*," p. 79.

[26]Ibid., p. 79.

I don't call these "quibbles" at all. Rather, my intellectual opponent is re-arguing the issue of indifference, without citing, let alone mentioning, my previous objections to his views. So let me again ask my learned friend: Why would anyone bestir himself to act if he only expected to make a non-loss; e.g., not improve his subsequent position, after the act, even in the *ex ante* sense? Why, pray tell, would man act (to create a world as *much* to his liking in the future as one which would arise but for his action)? Why not do something else? Why not just turn over and go back to sleep, rather than waste energy achieving a state of affairs that is *no better* than the one he *already has*? Not only does praxeology cry out for an answer to these questions, so does common sense.

Caplan writes:

> [O]nce you admit, as Block does, that synthetic *a priori* claims vary in probability, then even the synthetic *a priori* can be empirically tested. . . . 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.[27]

There is so much wrong here it is hard to know where to start. First of all, there is no such thing as a "less solid" synthetic *a priori* proposition, and a "more solid" synthetic *a priori* proposition. All synthetic *a priori* propositions are *equally "solid"* from a logical perspective. From a *psychological* viewpoint, there are some that are harder to grasp, and others that are easier, but, to the extent that a synthetic *a priori* proposition is a synthetic *a priori* proposition, its "solidity" from a logical point of view is *equal*. It may be harder to understand that 1,479 is the square root of 2,187,441 than that the square root of 9 is 3, but these two claims are equally valid or "solid."

Second, not only is it not "fruitful" to empirically test "less solid" synthetic *a priori* propositions, it is not even possible. Indeed, it is logically incoherent. Austrians do not reject empirical testing *per se*. Econometrics has a role to play with regard to economic statements that are not axiomatic, for example, concerning the magnitude of a particular demand elasticity. Nor, even, is it denied among most praxeologists that regression analysis can be properly used with regard to axioms, i.e., synthetic *a priori* statements. Here, though, the role is not to use empirical research to test the axioms; rather, if anything, it is to have axioms test the empirical research. Or, better yet, econometric analysis can *illustrate* the underlying economic realities, as with, for example, minimum wages or rent

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[27]Ibid., p. 80.

controls. Thus, I am claiming that Caplan has failed to fully reckon with Mises's elaboration upon these distinctions.[28]

Caplan, however, offers perhaps the strangest analysis ever made in the history of methodology:

> Imagine that we were uncertain about the Pythagorean Theorem. We could spend more time checking out 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. . . . 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.[29]

Let us consider the errors here one by one. First, Caplan suggests that we measure a wide variety of triangles to verify that $c^2 = a^2 + b^2$. Suppose we find a triangle for which this is not true. But this means it is not a right triangle at all (obviously, the triangle does not contain a 90° angle)! The point is, we already have a criterion for a shape being a right triangle: namely, that $c^2 = a^2 + b^2$. Let us make it even simpler. Triangles have three (straight) sides, and the internal angles add up to 180 degrees. How do we "test" this, pray tell? I can just see Caplan sending his graduate students out into "the field" to capture some wild triangles, corral them, see if they have three straight sides, and measure their internal angles. But shapes will be picked on the basis of having these characteristics in the first place! If this is a test, it is a fudged "test."

Second, what, precisely, can be meant by the phrase "indubitable empirics"? History, according to some wag, is "just one damn thing after another." So is it with empirical research. This discipline can never establish cause and effect, only, at best, constant conjunction or correlation. There was a lot of economic freedom (e.g., very little government regulation of the economy) in the U.S. in the eighteenth century, and the people were relatively poor. There is much less economic freedom in the U.S. in the twenty-first century, and the people are far wealthier, at least in terms of material goods. This, I suppose, is an "indubitable" empirical finding, if anything is, but it hardly establishes that government regulation

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[28]See Ludwig von Mises, *Theory and History* (New Haven, Conn.: Yale University Press, 1957).

[29]Caplan, "Probability and the Synthetic *A Priori*," p. 80; emphasis in original.

enriches a country. This is because empirical economics, or, better yet, "indubitable empirics," cannot distinguish whether we are now richer *because of or despite* government interference with economic liberty.

Third, there being no such thing as "indubitable empirics" in economics, and no such thing as anything less than "an absolutely certain synthetic *a priori* claim," at least in the logical albeit not in the psychological sense, there can never be a case for rejecting *any* synthetic *a priori* proposition in favor of an empirical one, no matter how well established the latter, and complicated the former. Perhaps the real problem is with some specific synthetic *a priori* statements. If Caplan thinks he knows of some that are refuted by empirical evidence, then he should be able to point out the logical misstep(s) or faulty premise(s) that misled praxeologists into thinking such statements to be synthetic *a priori* statements when in fact they involved logical contradictions. This he has not done. This he has not even *tried* to do. This he has not recognized the *need* for, if his viewpoint is to even approach coherency.

BAYESIAN CURE

How, according to Caplan, are we best to explain the outraged reaction of the neoclassicals to the Card-Krueger claim concerning the employment effects of minimum wages? We are to utilize Bayesian insights. Caplan writes:

> 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 [sic] in.[30]

But this appears to be a very poor explanation on its very face. Even Caplan concedes: "Why then, as Block insightfully asks, do economists react so *violently* to empirical evidence against the conventional view of the minimum wage's effect?"[31] If they were really Bayesians, e.g., acting according to this model, these economists would marginally shift their perspective on the minimum wage law. They might be slightly surprised—this was just one new "data point"—but they would certainly not be *angry*. However, economists were *affronted*, as well they should be, by the Card-Krueger "findings," as even Caplan admits.

Caplan writes: "Becker . . . had a 'strong prior': he was almost, but not quite, sure of the standard conclusion."[32] Caplan makes it

[30]Ibid., p. 81.

[31]Ibid., p. 80; emphasis added.

[32]Ibid., p. 81.

sound as if Becker, who, presumably, had no strong feelings about the elasticity of bananas in Ohio in 1955, but thought it was -1, now confronts a new "data point" indicating it was really -1.1. Does Becker come out of the starting blocks with fire in his eye over this new "data point"? He does not. He calmly, and *very* slightly, adjusts his presumptions on this matter.

But that is not at all what Becker and his colleagues did when these "two Princeton economists" rubbed the profession's collective noses in their nonsensical claim. Rather, they were breathing fire on this matter, again, as Caplan himself acknowledges: "economists react so *violently* to empirical evidence against the conventional view of the minimum wage's effect."[33]

According to Caplan's numerical example, Becker, upon reading Card-Krueger, *reduces* the probability he places on the typical economists' view of minimum wage employment effects from 98 percent to 95.5 percent. However, Becker reading Card-Krueger was more like waving a red flag in front of the bull, as even Caplan notes.[34] It is very difficult to reconcile this with a slight movement *in favor of* that pernicious and fallacious position.

Caplan writes: "Bayes's Rule shows that we should automatically be more skeptical of empirical evidence in favor of an improbable conclusion."[35] However, if this were all there were to the matter, how would we ever *initially* establish truth in economics? The point is, to say the best possible for Bayesianism, we need some independent criterion of truth *before* it sets in, and this the system itself cannot furnish.

Caplan lists three possible alternative explanations for the ferocity of the reaction to the Card-Krueger analysis on the part of the neoclassicals:

> Explanation #1: Neoclassical economists are covert praxeologists, though they will not admit it;
>
> Explanation #2: Neoclassical economists are intellectually dishonest dogmatists posing as empirical scientists; [or]
>
> Explanation #3: Neoclassical economists are Bayesians with some strong priors.[36]

Let us focus on 1 and 3, the only two possible explanations that interest me here. Caplan correctly attributes belief in the first to me,

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[33]Ibid., pp. 81, 80; emphasis added.

[34]Ibid., p. 82.

[35]Ibid., p. 82.

[36]Ibid., pp. 80–81.

and in the third to himself. The problem I have with his treatment of these two alternatives is that he believes implicitly that they are incompatible with one another. Caplan's view is that if he can prove explanation 3 correct, he disproves, or at least disparages, explanation 1. However, I cannot see my way clear to agreeing with this. It seems to me that the two alternatives are compatible with one another. Both can be true, together. Neither contradicts the other.

Now, I have given reason just above to indicate why I disagree with explanation 3. But let us assume, arguendo, that I *accept*[37] Caplan's Bayesian explanation. Why does that prove that explanation 1 is false? The reason that explanations 1 and 3 are not necessarily contrary to one another is that they occupy different philosophical categories. Explanation 1 is a claim that neoclassicals are really praxeologists, a logical category. Explanation 3 is a claim that neoclassicals form their beliefs on the basis of Bayesian considerations, and belongs to the realm of psychology. Caplan, in order to undermine explanation 1, will have to do better than merely to successfully defend explanation 3—which he has not done anyway. He will have to come to grips, directly, with explanation 1, something he has not even attempted to do, let alone accomplished.

Basic economic theory tells us, with a single voice, that rent control causes housing shortages. However, several of my econometric regression results failed to bear this out. Caplan offers a Bayesian explanation, according to which these

> 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.[38]

But why? Without economic theory to guide us, it is as likely that rent control causes housing shortages as that it solves them. True, most empirical results support the shortage hypothesis, but truth can never be demonstrated based on majority vote. Moreover, according to Caplan's own analysis, new "data points" are supposed to shift received opinion in their direction.[39] E.g., he is on record as stating that, due to Card-Krueger, Becker's trust in the view that minimum wages cause unemployment shifted from 98 percent to 95.5 percent. Why wasn't my dissertation committee

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[37]Lest there be any misunderstanding here, I reiterate my rejection of explanation 3. I am only assuming this proposition to be correct for the sake of argument in an attempt at a *reductio*. This should not be interpreted as a "concession" to Caplan's Bayesian analysis.

[38]Caplan, "Probability and the Synthetic *A Priori*," p. 83.

[39]Ibid., p. 82.

similarly moved in my direction by my "findings" that rent control solves housing shortages? They should have been, on Caplan's Bayesian account. However, let me assure you, I was there, and this was *not at all* their reaction. Rather than being slightly moved in my direction, they were derisive and, if truth be told, dismissive and even disappointed. That is, the fix was in. We *all* knew the correct results. The incorrect ones were fudged away.

CONCLUSION

Caplan has launched the third Austrian *Methodenstreit*. But, as in the other two cases, he has not succeeded in overturning the Austrian edifice. As they say in boxing, he has not so much as laid a glove on it. His publications in this vein, along with the critiques they have engendered, have instead served to strengthen praxeology. Caplan simplistically confuses logic and psychology, and his Bayesian account is of no account.

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