PROBABILITY, COMMON SENSE, AND REALISM: A REPLY TO HÜLSMANN AND BLOCK

BRYAN CAPLAN

I n my critique of Austrian economics[1] (Caplan 1999), I carved out a virtually unique position: Despite the Austrians' professed devotion to "realism" against neoclassical pragmatism, the latter approach is in fact far more realistic than the former. My critics (Hülsmann 1999, Block 1999) remain unconvinced. The editor's invitation to respond provides me with an ideal opportunity not only to defend myself, but to spell out the philosophical side of my original thesis in greater depth.

PROBABILITY

Block calls the treatment of probability "the weakest section" (1999, p. 30) of my original paper. By itself, this spurs me to try to strengthen it. But more fundamentally, reading my critics convinces me that the issue of probability permeates every aspect of this discussion. Block, for example, observes that the envious "have no way to *prove* they lose out at all," that "scientifically we can know *nothing* about states of mind not demonstrated in action," and, regarding latent preferences, asks, "How can we, as economists, even know they exist?" (1999, pp. 32, 34, 23; emphasis mine). Hülsmann, similarly, remarks that "the very existence of the innovator prevents *any attempt* to establish regularities of what human beings choose," and that "there is *no way* to judge whether people really want a good, and how much they want of it, other than by looking at their actions" (1999, pp. 13, 16; emphasis mine). On

[1]Throughout this paper, the term "Misesian" is used to specifically refer to the strand of Austrian economics primarily influenced by Mises and Rothbard, and "Austrian" to jointly refer to Misesian and non-Misesian Austrians alike. Both Hülsmann and Block obviously fall on the Misesian side, so most of the comments will be directed accordingly.

topic after topic, Block and Hülsmann make extreme claims about how little people are able to know.

The link to probability theory is fairly simple. Once we rule out all talk about "degrees of probability" for practical questions (as opposed to idealized games of chance), the pettiest doubt can be used to trump the "scientific" merits of an array of mundane observations. Take "Socialists envy the rich," for example (Mises 1972). After refusing to assign a probability to this assertion, it is easy to slide from "There's no way to be certain," to "Not proven," to "No scientific basis," to "No way of knowing." Austrian critiques of neoclassicism often come down to this generic complaint.

Calling an objection "generic," needless to say, hardly shows that it is wrong. In Caplan (1999), my central counter to the full-blown Misesian rejection of quantifiable probability was a reductio ad absurdum. If we cannot quantify the probability of an "individual, unique, and nonrepeatable" (Mises 1966, p. 111) event, then we can never quantify probability at all, because strictly speaking, *all* events are "individual, unique, and nonrepeatable." Naturally, though, this reductio is only persuasive if, ex ante, you acknowledge the absurdity of rejecting all real-world applications of probability theory.[2]

This indirect argument can be complemented, however, with a direct defense: The basic principles of probability are simply self-evident. It is self-evident that one holds beliefs with *some degree* of certainty.[3] It is self-evident that the degree of belief must vary from *impossible* to *certain*. It is self-evident that intermediate degrees of belief can be compared to each other (Bush was more likely to win than Gore) and to any point on the probability spectrum (Bush was more than 50 percent likely to win). These self-evident truths have

_____

[2]Hülsmann is apparently willing to deny that there is anything absurd here: "*even if* [Caplan] were correct, it would be a confirmation rather than a problem for the Austrian approach" (1999, p. 12). Block, responding with greater restraint, states that "at worst it only attacks the minor Austrian point" (1999, p. 29). But then Block goes on to affirm that probability theory can still sometimes be used for real-world problems: "It all depends upon just how 'dishonest' are the roulette wheels. If only slightly so, then class probability would still apply" (1999, p. 29). But if people can keep using class probabilities in spite of "slight" unique aspects, what prevents them from using them (with less and less success) as the unique aspects expand indefinitely? Moreover, calling a deviation "slight" is itself a quantitative judgment about a unique event!

[3]Part of the Misesian complaint about probability theory seems to stem from a conflation between objective and quantifiable probability. Hoppe, for instance, objects to the view that we possess "perfect knowledge of the probability distributions of all future classes of actions" (1997, p. 56). On the subjective-but-quantifiable view of probability, however, "perfect knowledge of probability distributions" is equivalent to "perfect knowledge, upon reflection, of one's degree of confidence in one's own beliefs," not "perfect knowledge of carefully prepared actuarial tables for all possible events."

a wealth of corollaries, like: Lack of full certainty is not equivalent to no knowledge at all.[4]

One probability judgment I make is that flat-footed appeals to self-evidence are unlikely to win over my critics.[5] But perhaps I can slightly raise this likelihood by couching these claims in strict praxeological terms. Mises writes:

> Acting requires and presupposes the category of causality. Only a man who sees the world in the light of causality is fitted to act. In this sense we may say that causality is a category of action. The category *means and ends* presupposes the category *cause and effect*. In a world without causality and regularity of phenomena there would be no field for human reasoning and human action. Such a world would be a chaos in which man would be at a loss to find any orientation and guidance. Man is not even capable of imagining the conditions of such a chaotic universe. (1966, p. 22)

But is causality really implicit in the axiom of action? Is it really the case that "[I]n order to act, man must know the causal relationship between events, processes, or states of affairs" (Mises 1966, p. 23)? Not quite. Mises's claim is too strong; what action really supposes is not *causality* but *probability*.[6] In order to act, one merely needs to believe that one's action produces a *better distribution* of outcomes than any alternative action. There is no need to believe that one's action will succeed, or is even very likely to succeed. For example, my action of submitting a paper to the *American Economic Review* does not depend on the strict *causal* belief that "Submitting papers to the *AER*

---

[4]Hoppe (1997) forcefully asserts this truism several times, but juxtaposes it against various extreme skeptical claims, such as "any progress in [man's ability to predict his own future actions] must be considered systematically impossible" (p. 60).

[5]Even so, as David Gordon astutely observed in another context:

> I have claimed that certain moral judgments are obviously true: but they are certainly not obvious to [L.A. Rollins, a critic of Rothbard's natural rights theory]. Fortunately, it is not an argument against a moral theory's truth that L.A. Rollins fails to believe it. That is a fact of purely biographical significance. (1988, p. 233)

[6]Mises interestingly discusses "statistical laws" in the same section, leaving it somewhat unclear as to whether he acknowledges such a thing as "statistical causality." But Hoppe explicitly endorses a deterministic interpretation of causality:

> [O]ne establishes that phenomena have such causes by following a particular type of investigative procedure, by refusing on principle to allow *any exceptions*, i.e., instances of inconstancy, and by being prepared to deal with them by producing a new causal hypothesis each time any such an apparent inconstancy occurs. (1989, pp. 113–14; emphasis mine)

always causes acceptances." I only need to believe that "Submitting papers to the *AER is*—all things considered—better for me than sending them anyplace else."

In other words, Mises is correct to point out that beliefs about the efficacy of action are implicit in action. But he at best misspeaks when he characterizes this necessary feature of action as knowledge of "causality." Instead, the necessary belief component of action is weaker; we don't need to know—or even believe we know—any exceptionless causal laws. We merely require beliefs about conditional probabilities.

As with other fundamentals of action, doubts about probability are self-refuting. One cannot even argue against probability theory without implicitly holding some belief about the probability that doing so will change listeners' minds. Few Misesians would be naive enough to imagine that attacking probability theory *has to* reduce its number of adherents. Rather, they attack probability theory because, given their beliefs about the probability of changing people's minds, they see it as the most valuable way to use their time. Any attack on probability presupposes it.

But how can these claims about probability be reconciled with realism? Hülsmann (1999, p. 12) makes the fair point that university professors spend far more time explicitly calculating probabilities than businessmen (though, contrary to Hülsmann, even moderately sophisticated businessmen habitually compute expected present discounted values using elementary probability theory). To this, I respond that a probability assessment is exactly analogous to a willingness-to-pay. People may be unable to *articulate*, for example, that "I would be willing to pay $200 per month in additional rent to live in a safer neighborhood." They might even nonsensically assert that "You can't put a price on safety." But in acting, they implicitly make such trade-offs. Similarly, people may be unable to articulate that "I believe the probability of being murdered in my neighborhood is .001 percent per year," and they might evasively respond, "I just don't know." But in acting, they implicitly set probabilities. If they thought the probability of being murdered was 90 percent per year, they would move; conversely, if they thought the probability was 0 percent, they would stop wasting time on ordinary precautions. In short, just as demand theory does not commit us to the view that the typical person explicitly ponders, "How much Gouda cheese would I buy if the price were a penny per pound?" probability theory does not commit us to the view that the typical person explicitly ponders, "What is the probability that I have an evil twin?"

Incidentally, Hoppe (1997) raises two other noteworthy objections to the realism of probability theory. The first is that it implies that "it would be possible to give an exhaustive classification of all possible actions. . . . For without a complete enumeration of all possible types of actions there can be no knowledge of their relative frequencies" (p. 56). There is a simple reply: One

can always complete an enumeration by adding a catch-all "other" category. In assigning probabilities to the party affiliation of the next president of the United States, for example, one need merely break the possibilities down into Democrat, Republican, and "other." Hoppe's second objection points to the reality of systematic error (1997, p. 57). But this conflates a fundamental tenet of neoclassicism—subjective-but-quantifiable probabilities—with a popular subsidiary neoclassical hypothesis—rational expectations (1997, p. 56). Rational expectations is a hypothesis *linking* subjective-but-quantifiable probabilities to objective frequencies. Empirical evidence of systematically mistaken beliefs (Caplan 2001a) counts only against rational expectations, not probability.

If they were to change their positions on probability at all, I bet that Hülsmann and Block would embrace my view, not an intermediate position like Kirzner's. Still, it is worth noting the weakness of Kirzner's account from a praxeological standpoint. Kirzner often speaks of opportunities that have been "utterly overlooked" (e.g., 1997, p. 71). When we bear in mind my observation that every action presupposes probabilities, however, this strongly suggests that Kirznerian "utterly overlooked" possibilities are neither more nor less than events with a perceived probability of 0. They are not counter-examples to the usual neoclassical approach. To act while "utterly overlooking" X is praxeologically equivalent to acting on the belief that X is impossible.[7]

Hülsmann offers two main arguments against my wholesale rejection of Knightian uncertainty. One is the role of ideas in human history:

> [T]he invention of new ideas again and again changes the way human beings act under otherwise equal circumstances. . . . The very existence of the innovator prevents any attempt to establish regularities of what people choose. (1999, p. 13)

But there is no reason the invention of new ideas cannot be assimilated into a standard probabilistic framework. We don't expect big changes to happen every day; or, in other words, we assign low probabilities to big innovations. Moreover, the effects of big innovations are often quite predictable. When the automobile suddenly appears, it doesn't take a genius to realize that people will move further away from their places of employment, that gas stations will spring up, and so on. Even when predictions are highly inaccurate, that is different from *totally* inaccurate. Few futurists foresaw the extent of progress in personal computing, but at least they correctly guessed that computers would sharply reduce the fraction of human labor devoted to rote activities.

---

[7]As I observe in Caplan (1999), this insight raises serious doubts about the empirical relevance of Kirzner's approach. If you really thought that the probability of X was 0, the apparent occurrence of X would not lead you to change your mind about X. Instead, you would have to conclude that you were mistaken or delusional.

Indeed, it is trivial to list an array of "regularities of what people choose" that have persisted unchanged in spite of the entire history of innovation: Most people are selfish most of the time and give a small fraction of their income away to strangers. Most people will eat rather than starve if they have access to food. Most people are heterosexual. Males are, on average, more attracted to younger women. Most people would pay a large amount to escape an otherwise uninhabited island. Sociobiology provides us with a great many less jejune generalizations about human choices (Dawkins 1989). The claim that "the emergence of new ideas *necessarily* overthrows *all* previous patterns of behavior" (Block 1999, p. 31; emphasis mine) is a wild exaggeration.[8]

Hülsmann's second argument, which he attributes to Hoppe, is that "any determinist theory of choice (like the probabilistic approach) implies an inescapable contradiction." Why? It

> presupposes that man *cannot* learn because any adoption of new ideas would change the way he acts. . . . However, presupposing that man cannot learn contradicts the necessary assumption of research activity, namely, that research will make a difference. (1999, p. 13)

In spite of my firm belief in free will, though, this contradiction appears readily "escapable." Suppose, for example, that my determinist theory is that every extra hour of study on the SAT will raise my score by exactly three points. This is a determinist theory of learning, and it hardly contradicts the possibility of learning. The determinist can forsee *that* he will learn, and *how* his learning will improve his performance, without already *knowing* the unlearned material. Students do this all the time.[9]

That aside, classifying probabilism as a "determinist theory of choice" is sleight of hand. Probability theory *explicitly* allows for learning; that is what "Bayesian updating" is. Suppose I believe "There is an 80-percent probability that I get tenure next year. If I get tenure, I will stay where I am; otherwise, I will find a new job someplace else." The next year comes along, I reach "new ideas" when I *learn* whether I get tenure, and depending on the outcome, I change the way I act. Even if Hoppe's argument against determinist choice theories were correct, using it to argue against probabilistic choice theories makes about as much sense as using contradictions in the Bible to argue against Buddhism.

---

[8]Hoppe (1997, pp. 72, 77), in contrast, broadly acknowledges the existence of "regularities of what people choose," though ultimately this is difficult to square with his strictures against extending the scientific method to the study of human beings (1997, pp. 60-61).

[9]Hülsmann's formulation also reflects a sharp departure from subjectivism. Why couldn't a researcher be motivated by idle curiosity, rather than by an instrumentalist desire to "make a difference"?

In sum, then, the basics of probability theory are self-evident and, rightly understood, are highly intuitive. Knightian uncertainty is incoherent. More importantly for Misesians, probability theory deserves to take the place of causality as a fundamental implication of the action axiom. Any attempt to deny probability theory inevitably winds up presupposing it.

COMMON SENSE[10]

Another basic philosophical difference between myself and my critics is the relation we see between common sense and science. Block repeatedly draws this sort of distinction: "*In ordinary language*, a person could be readily understood to be indifferent between wearing a green or a blue sweater." "Sure [latent preferences can exist], in the *ordinary commonsensical* notion" (1999, pp. 22, 36; emphasis mine). Hülsmann remarks that "Caplan's errors seem all to be rooted in his failure to grasp that Austrian economics is a theory of action (praxeology) rather than some kind of applied psychology" (1999, p. 4).

Simple question: Is the "common-sense," "ordinary-language" belief *true or not*? If it is true, then it makes no difference whether the belief is "praxeological." Austrians have rightly lambasted neoclassical economists for their narrowness; why would they want to imitate them? Naturally, one might draw attention to the nuances that distinguish praxeology and, say, "applied psychology." But it is hard to avoid the impression that my critics label claims as "nonpraxeological" in order to somehow exclude them from the discussion. This is particularly distressing when praxeology and "applied psychology" can be joined together to yield new insight. Mises, Rothbard, and even the "extreme rationalist" Hoppe (1989, p. 1) all acknowledge a role for empirical assumptions to supplement pure praxeology.[11] Why might not the same hold for any of the propositions of "applied psychology" I purportedly advance?

Block puts forward the most cogent potential answer. As he explains, "other academic disciplines also utilize ordinary words and invest them with technical meaning. . . . [T]he physicist means one thing by 'work,' . . . while the man in the street means something somewhat different" (1999, p. 22). But this raises a disturbing possibility for Misesians. Could many of their long-standing disputes with neoclassical economics be a trivial product of divergent technical

---

[10]While this section focuses on Misesian departures from common sense, non-Misesian Austrians are at least equally guilty on this count. As Hoppe (1997, p. 52) asserts, "To claim . . . that we are faced with radical uncertainty and that the future is to all of us unknowable is not only self-contradictory but also appears to be a position devoid of common sense."

[11]Rothbard (1962) emphasizes three empirical assumptions: the value of leisure (p. 37, and particularly, p. 437, n. 27), human and resource variety (p. 82), and rate-of-return maximization (pp. 377–78).

definitions? Particularly if neoclassical usage accords with ordinary language, Austrians would have only themselves to blame for any misunderstanding. Take, for example, the Misesian critique of indifference analysis. Suppose common-sense claims about "indifference" were *really* compatible with Rothbard's views on the subject. Then when he accuses neoclassicals of subscribing to "fallacies" on this point, they could fairly respond, "No, the neoclassical concept of indifference is identical to the common-sense one."

My point here is *not* that neoclassical-Austrian disputes *are* purely semantic; rather, my point is that *if* apparent conflicts between Austrian claims and common sense were purely semantic, as Block suggests, *then* it would be reasonable to suppose that neoclassical-Austrian disputes were purely semantic as well. If we deny the latter, we must deny the former, too. The implication is that Austrian claims and common sense often *really are* at odds. Austrians must in consequence say that they are right and common sense is wrong. In spite of disclaimers to the contrary, I believe that this is the real Austrian position.[12] The most charitable stance they can take vis-à-vis incompatible common-sense claims is that the latter are colorful metaphors, not literal assertions.

It is easy to imagine many Austrians biting this bullet: "So common sense is wrong. One important function of science is to correct popular confusions." Or, as Kant scoffed at Thomas Reid and other "common-sense" critics of Hume:

> They found a more convenient method of being defiant without any insight, viz., the appeal to *common sense*. It is indeed a great gift of heaven to possess right or (as they now call it) plain common sense. But this common sense must be shown in deeds by well-considered and reasonable thoughts and words, not by appealing to it as an oracle when no rational justification of oneself can be advanced. . . . Seen in a clear light, it is but an appeal to the opinion of the multitude, of whose applause the philosopher is ashamed, while the popular charlatan glories and confides in it. (1977, p. 851)

To this, I respond that Kant was wrong and Reid (1940, 1872) was right. The whole point of "proofs" is to move from the known to the unknown, from the more probable to the less probable. Fundamental principles of common sense cannot be "proven" *because they are obvious*; there is nothing more probable to derive them *from*. As Reid brilliantly puts it,

> [W]hen we attempt to prove, by direct argument, what is really self-evident, the reasoning will always be inconclusive; for it will either take for granted the thing to be proved, or something not more evident; and so, instead of giving strength to the conclusion, will rather tempt those to doubt of it who never did before. (1872, p. 637)

---

[12]Rothbard amusingly pointed out heterodox movements' tendency to camouflage unpalatable "esoteric" doctrines with crowd-pleasing "exoteric" doctrines (Raimondo 2000, pp. 122–23).

Philosophers who miss this point wind up, like Hume, in abject skepticism, or, like Kant, constructing enormous systems of non sequiturs in a desperate attempt to avoid skepticism.

The only way to challenge common-sense beliefs is to show that they conflict with even more obvious common-sense beliefs.[13] In the natural sciences, careful observation has occasionally shown that "the obvious" is wrong. But this only succeeds by relying on an even more basic principle of common sense: the validity of the senses. In the social sciences, however, conflicts with common sense are far more suspicious. The fundamental principles of the natural sciences are frequently beyond the experience (and comprehension) of ordinary people; but as Austrians have often reiterated, the fundamental principles of the social sciences are omnipresent. As Rothbard observes,

> Whereas in physics, causal relations can only be assumed hypothetically and later approximately verified by referring to precise observable regularities, in praxeology we *know* the causal force at work. This causal force is human action, *motivated*, purposeful behavior, directed at ends. (1962, p. 277)

The literal existence of indifference, for example, is not a mere hypothesis; we *know* it is real from introspection.

My critics implicitly make a distinction between common sense and praxeological truths. If this distinction is simply disciplinary, Austrians ignore common sense at their peril. If the distinction is semantic, there is every reason to think that many apparent conflicts between neoclassical and Austrian economists are semantic as well, rendering decades of Austrian objections quite hollow. And if, as I think, common sense and praxeology literally disagree about basic questions, common sense has priority.

Specific Replies

Building on these foundations of probability and common sense, I now turn to specifics of the Hülsmann and Block critiques. Due to their high (not to say perfect) level of agreement, I mostly respond topic by topic, rather than author by author.

---

[13]Misesians might be tempted to reply that the action axiom has a probability of one and is consequently able to trump common sense. But this claim itself presupposes various common-sense premises about the reliability of one's intellectual faculties. Moreover, even the apparent implications of a perfectly certain axiom must be less than fully certain due to the fallibility of deduction. Suppose that one's deductions about, say, indifference, conflict with common sense. Which is more likely? That an error has slipped into an extended chain of abstract reasoning, or that our every introspective experience of indifference is illusory?

Indifference

Here I have little to add to my original discussion. Common sense and neoclassical theory say we can be indifferent; Misesians say we cannot. Contra Block (1999, p. 24), the dispute is hardly a semantic one between praxeology and psychology. I can imagine being indifferent in a praxeological sense (I have no strict preference between *a* and *b*) while having diverse psychological states. Rather than having no strong feelings about green versus blue sweaters, for example, I might have intense but conflicting feelings about both, which, coincidentally, leave me indifferent. Block wonders, "Why would anyone bestir himself if there were absolutely no gain in it for him" (1999, p. 24)? But one could just as well inquire, "Why would anyone *fail* to bestir himself if there were absolutely no cost to him of doing so?" Normally, moreover, one is only indifferent between some options; I can be indifferent between green and blue sweaters, but strictly prefer one sweater to none. As Rothbard explains, "Buridan's ass . . . is confronted not with *two* choices, but with *three*, the third being to starve where he is" (1962, p. 267).[14]

Block's attempt to show that my position is self-refuting is unimpressive. "For this George Mason economist is not indifferent between the Austrian (anti-indifference) and the neoclassical (pro-indifference) theories" (1999, p. 24). True enough; however, my thesis is not that we are *always* indifferent, but that we are *sometimes* indifferent. Similarly, Block's deduction that "For Caplan, it might be that one of the trading partners, or, even, neither of them, made any gain in utility," is quite true. But that "might" is crucial. So what if one-in-a-trillion exchanges strictly raises the utility of neither participant?

Cardinality

Hülsmann argues that a necessary condition for the existence of a ratio is identity of units: A ratio could only exist if "preference ranks and prices had the same dimension (that is, if they were the same kind of thing)" (1999, p. 8). In general terms, this is simply wrong. A ratio of two lengths (2 feet: 1 foot) can equal a ratio of two timespans (2 centuries: 1 century). The units on both sides of the equality cancel. But for neoclassical utility functions, even this point is superfluous, for the simple reason that utility functions are *unitless*! We can divide an abstract function *f(x)* by an abstract function *g(x)* even though neither has any units "attached." Hülsmann goes on to list two supposedly absurd ratios: "a rabbit divided by a piano concerto . . . a combustion engine divided by a prayer" (1999, p. 9). Amusing as these examples are, there is nothing

---

[14]Block strangely footnotes this passage after telling us, "If indifference were his exact mental state, surely he would select neither article of clothing" (1999, p. 22)! Rothbard, in contrast, correctly infers against Schumpeter that "*Even on the indifferentists' own grounds*, this third choice will be ranked lower than the other two on the actor's value scale. He will *not* choose starvation" (1962, p. 267; first emphasis added).

wrong with them. How do they differ from "miles divided by hours" (a ratio of distance to time) or "pies divided by children," (a ratio of pies to children)? Finally, in noting that "prices are themselves ratios," Hülsmann neglects the standard assumption that both prices are expressed in terms of a numeraire.[15]

Continuity

I agree completely with Block that continuity (and differentiability) assumptions have occasionally been misleading, for example in the theory of monopolistic competition. But this justifies only caution, not wholesale rejection. Block interestingly excuses Rothbard's habitual reversion to intersecting supply-and-demand diagrams as follows: "Surely, after taking great pains to show discontinuities, he may be forgiven for lapsing into the traditional analysis, if only as a heuristic device" (1999, p. 29). Block seems to forget that neoclassical economists have spent a staggering amount of brain power (indeed, in my judgment, far more than the marginal benefits warrant; see, e.g., Hildenbrand and Kirman 1988) spelling out the dependence of standard analysis on the assumption of continuity. If Rothbard's "great pains to show discontinuities" grant him a license to draw intersecting supply-and-demand curves, neoclassical economists deserve the same privilege.[16]

Hülsmann arguably goes further than Block by firmly endorsing the conclusion that "market equilibrium . . . cannot be *represented* as the intersection of supply and demand curves" (1999, p. 10). This position is so counter-intuitive that, contrary to Hülsmann, he is virtually the only economist to ever embrace it. *Human Action* is graphless, but Mises repeatedly refers to "intersections" of supply and demand, admitting, for example, that "It is possible to visualize this interaction by drawing two curves, the demand curve and the supply curve, whose intersection shows the price" (1966, p. 333).[17] The near-impossibility of talking about supply and demand without alluding to their intersection reveals how contrary to common sense Hülsmann's position is.

---

[15]Alternately, as Murphy (2000) astutely notes, we can include the appropriate units on *both* the left- and right-hand sides of the equation: "That's what *marginal utility* means, after all: the increase in utility . . . resulting from an additional quantity of the good; its expression necessarily must contain the *unit* of the good in question" (pp. 4–5).

[16]Why then are neoclassicals admittedly more inclined than Misesians to make continuity-related mistakes? The answer is that the vastly greater number of neoclassical economists leads to greater division of labor, with some economists devoting their lives to the study of continuity while the remainder get down to other business. Continuity-related mistakes largely arise due to the low level of communication between specialists—a sociological rather than a doctrinal failing.

[17]Mises attaches the disparaging proviso that such curves "do not add a whit to our insight," but goes on to note that "such curves may prove expedient in visualizing the problems for undergraduates" (1966, p. 333). At minimum, then, these curves *can* "add a whit to the insight" of undergraduates!

Income and substitution effects

Caplan (1999) maintains that Rothbard contradicts himself by introducing income effects and backward-bending supply curves after purporting to prove that the laws of supply and demand are exceptionless theorems. Block denies the charge; Rothbard is perfectly correct, given the "*ceteris paribus* assumption of no income changes" (1999, p. 29). There is a fundamental problem with Block's reply, though. It hardly makes sense to invoke an "all else equal" condition in cases where all else is *of necessity never equal*! The key neoclassical insight is that price changes ipso facto change income. Income effects do not happen at the same time as price changes by miraculous coincidence. They are inherent in the nature of price changes. Block's defense of Rothbard makes about as much sense as a "theorem" that "no one ever dies of starvation," which is apodictically true "given the *ceteris paribus* assumption that no food is nourishing."

Hülsmann, in contrast, amazingly declines to either (a) defend Rothbard's use of income and substitution effects, or (b) argue that this was an unfortunate neoclassical corruption of Rothbard's thinking. Instead, he remarks only that "Mises did not bother about the shape of supply curves" and that relatively little depends on this.[18] Bear in mind that "shape" here is not just slope, but sign! "The fact that Rothbard occasionally refers to income and substitution effects does not warrant the claim that these effects correspond to anything real" (Hülsmann 1999, p. 11). True enough; but Rothbard did claim to have strictly deduced the conclusions that supply slopes up and demand slopes down from the axiom of action. Why doesn't Hülsmann say that Rothbard's supposed proofs *warrant the rejection* of the income and substitution effects? If he thought Rothbard's proofs were valid, he would. And if Rothbard's proofs of the laws of supply and demand are not valid, the remaining ten chapters of *Man, Economy, and State*–everything from interest-rate determination to monetary economics to the theory of price controls–rest upon error. If these are not "fundamental feature[s] of price formation" (Hülsmann 1999, p. 11), what is?

Demonstrated preference and welfare economics, I: Block

Block misinterprets my paraphrase of Rothbard's utility theory: "[T]he free enterprise system only *permits* the implementation of all desired voluntary

---

[18]In fact, Mises did "bother" about the shape of supply curves. The following quote plainly takes the positive slope of supply curves as a given:

> The monopolistic buyer—whether he is an individual or a group of individuals acting in concert—cannot reap a specific gain corresponding to the monopoly gains of monopolistic sellers. If he restricts demand, he will buy at a lower price. But then the quantity will drop too. (1966, p. 383)

exchanges, this is a far cry from guaranteeing any such outcome. Caplan appears to assume the latter, however" (p. 32). But this ignores a key proviso in my original statement: "*at that moment* at least one of them would not have benefited" (Caplan 1999, p. 833; emphasis added). Yes, as Block points out, two people might fail to trade solely because they don't *know* of each other. But praxeologically speaking, this only means that more mutually beneficial trades could have happened if conditions (in this case, communication) were somehow better.

Block moves into more interesting ground in his discussion of *verstehen*, or understanding. For what is *verstehen* other than what Hülsmann calls "applied psychology"? I wholeheartedly agree with Block's observation that "The economist must *interpret* human action; he must 'get into the mind' of the economic actor" (1999, p. 33). My complaint is, in essence, that Misesians do not take *verstehen* far enough. If *verstehen* works for signing a contract, it works for envy, too. Block's assertion that "[W]e *know* what a contract sign-ing means. But in the case of supposed envy, we really do not" (1999, p. 33) is bizarre. Surely Block will not claim apodictic certainty in the former *ver-stehen*; so why is the lack of apodictic certainty a problem for the latter? What is the difference? Accurate *verstehen* may be harder for envy than contracts, but there is no way for Block to justifiably treat these as radically different cases. The same applies to judgments about public goods, efficiency, and so on. We identify them using "applied psychology" or *verstehen*, hopefully after careful study of the facts. Such judgments are fallible, but the same is true of all *verstehen*.

Finally, contrary to Block, there is nothing *inherent* in public goods theory that makes it an "ideological wedge for government intervention" (1999, p. 36). As Caplan (1999) mentions, public choice economists have used public goods theory to analyze the inefficiencies of democracy. David Friedman (1989, pp. 156-59) bases much of his argument for anarcho-capitalism squarely on public goods theory! In my own research on the economics of irrationality, I use public goods theory to explain why democracy is so ineffi-cient compared to the market (Caplan 2001b). Most economists are statists, so they unsurprisingly tend to put a statist spin on various "failures" that apply just as well—if not more so—to the state itself.

Demonstrated preference and welfare economics, II: Hülsmann

Hülsmann asserts that "'efficiency' refers to the relationship between means and ends. . . . All one can state is that [a policy] is efficient for some persons and inefficient for others" (1999, p. 16). This is, for once, a purely definitional disagreement. Hülsmann addresses *means-ends* efficiency. But I was clearly referring to *Kaldor-Hicks* efficiency, the most common usage; and this sense of the term is not agent-relative (Posner 1998, pp. 12-17; see also Landsburg

1993, pp. 49–105). *Kaldor-Hicks* inefficiency sidesteps the problem of "comparing non-extended entities like value, utility, preference ranks, etc." (Hülsmann 1999, p. 16) by instead comparing willingness to pay (Stringham 2001). As an absolute ethic, I would surely reject this efficiency standard, but it is internally coherent.

Hülsmann may be correct that "the original purpose of public goods theory was to establish a rational criterion for government intervention"[19] (1999, p. 17). But like many other theoretical innovations, the *motive* behind its development is irrelevant to its truth. Even if the "original purpose" of atomic theory was to build better bombs, this neither shows that atomic theory is false, nor that it lacks civilian applications.

Finally, I am well aware of Hoppe's "argumentation ethics" (Hoppe 1989, pp. 127–44). I did not discuss it in my original article because my goal was to critique Austrian economics at its strongest. Like almost everyone who has walked through the steps of Hoppe's attempted proof, I see it as a tissue of error (Friedman 1988, Lomasky 1989). At an absolute maximum, Hoppe shows that arguments in favor of violations of private property *during arguments* are self-refuting.

CONCLUSION

Block aptly sums up much of our disagreement:

> The Austrian takes a position midway between that of the behaviorist and Caplan's neoclassicism. For the behaviorist, there is no such thing as motive, introspection; all is human behavior. For Caplan, all is motive and introspection; no behavior whatsoever is necessary to tie these phenomenon to the real world. In the praxeological perspective, there is both, and the latter is necessary to demonstrate the former. (1999, p. 24)[20]

---

[19]I doubt, however, that economists who do see it as a rational criterion for intervention would find Hülsmann's counter-arguments impressive.

> [E]xternalities are clearly not a feature of a good as such, but depend exclusively on the subjective feelings of those other people. Whenever any person other than its owner takes an interest in a good, it becomes *ipso facto* a public good. As a consequence, there is no means to clearly distinguish between public and private goods. (1999, p. 16)

Hülsmann's foils would presumably reply that the distinction is clear enough and stable enough for practical purposes.

[20]This passage stands in contrast to Rothbard (1962, p. 266): "[I]n so far as praxeology touches on psychology, its principles are the *reverse* of those of behaviorism."

The crux of the disagreement comes in the final clause. Yes, there are innumerable connections between introspection and behavior, but are the latter necessary to demonstrate the former? Nothing could be further from the truth. Individuals can know their own motives from introspection alone. And unless Block is talking about absolute certainty, common sense tells us that we can also know a great deal about the motives of others without ever observing their behavior. By rebuilding economics on the denials of these truisms, Misesians are moving off the path of realism rather than along it.

What could possibly motivate this queer refusal to recognize the reality of most mental states? This is a serious challenge for my *verstehen*, and a dose of humility is in order. Still, my best guess is that it hinges on the Austrian rejection of probability theory. When apodictic proof is unavailable, they have no substitute to fall back on. Orthodox probability theory, in contrast, formalizes the common-sense view that certainty is just the endpoint on a continuum. A person speaks the words, "I envy the rich." No one can *prove* he is telling the truth. For the Misesian, this insight virtually ends discussion. But common sense can still inquire, "*How likely* is the speaker to be telling the truth?" It is not a defect of public goods theory that there "exists no unambiguous way to measure the costs and benefits" (Block 1999, p. 36) of, for example, education. This reflects the inherent uncertainty of the world. The common-sense response is to put forward one's best estimate. By treating all imperfect answers as equally bad on methodological grounds, the Austrians are once again straying from the path of realism.

To end on a positive note: Block's practice of introspection is far better than his theory. Caplan does indeed agree that "[Austrianism] has far more to offer the practitioner of economics than [Marxism]" (Block 1999, p. 37). But the Austrian contribution comes not from the letter of its doctrine, but from the spirit of its approach. In methodological practice—though never in pure theory—neoclassical economists have indeed often been narrow behaviorists. Their firewalls against survey evidence and narrative history have only recently begun to crumble. Rothbard (1960) saw through their cant from the start. Austrians have also long recognized the robustness of the free market: Rothbard anticipated the theory of contestable markets (1962, pp. 573-79, 787-91, 913 n. 72; 1977, pp. 37-80) as well as rational-expectations macroeconomics (1962, pp. 683-87, 864, 875-77; 1963, pp. 43-53), and he seriously undermined the *application* of public goods theory to the defense services industry (1978, pp. 215-41). Finally, few neoclassicals have analyzed democracy with the Austrians' lack of reverence (Rothbard 1977, pp. 16-23, 189-99). Public choice economists frequently try to temper dispassionate analysis of democracy's failings with abiding faith in its sanctity. Austrian critiques of democracy have consequently often been more penetrating.

Block also astutely parses my left-handed compliment to mainstream economics: "this is a bona fide case where neoclassical economists did not merely

tediously formalize the obvious" (Caplan 1999, p. 829). I have no illusions about the quality of most research. Austrians are quite prudent to avoid the modal neoclassical publication.[21] Only a small percentage are worth reading. But one should not jump to the conclusion that the total contribution of neoclassical economics is small. The modern literature is so massive that multiplying it by the tiny percentage of insightful articles yields a large body of valuable knowledge. Yes, most neoclassicism is bad economics, but most good economics is still neoclassical.[22]

There are two paths for Austrian economics to evolve along. The first is obscurantist philosophizing about preferences, probability, uncertainty, and welfare economics. The second is to reinvent itself as a species of neoclassical economics infused by the spirit of the Austrian approach. Yes, there are many neoclassical economists who focus myopically on "measurable behavior," who insist that markets *only work* if we assume "perfect competition, perfect information, and no externalities,"[23] and who turn a blind eye to every failing of democracy. But the problem is not neoclassical economics; it is bad neoclassical economi*sts*. By becoming good neoclassical economists, the Austrians can help shift the balance.

REFERENCES

Block, Walter. 1999. "Austrian Theorizing: Recalling the Foundations." *Quarterly Journal of Austrian Economics* 2:21–39.

Caplan, Bryan. 1997. "Why I Am Not an Austrian Economist." Unpublished Manuscript. Available at: http://www.gmu.edu/departments/economics/bcaplan/whyaust.htm.

———. 1999. "The Austrian Search for Realistic Foundations." *Southern Economic Journal* 65:823–38.

———. 2001a. "Systematically Biased Beliefs About Economics." Unpublished Manuscript. Available at: http://www.gmu.edu/departments/economics/bcaplan/sbbe.doc.

---

[21]Then again, neoclassicals are likewise prudent to avoid the modal Austrian publication. While Austrians are rarely guilty of tedious *formalization*, they regularly engage in tedious *repetition* of what Mises or Rothbard said, or, worse yet, tedious exegesis of what Hayek meant to say.

[22]For a discussion of the main postwar achievements of neoclassical economics, see Caplan (1997).

[23]Contrary to many classroom presentations, these are merely *sufficient*–not necessary–conditions for the Kaldor-Hicks efficiency of laissez-faire. For example: Bertrand competition and contestable monopolies work as well as perfect competition; *symmetric* imperfect information implies no deviation from efficiency; and externalities have no efficiency implications if they are infra-marginal, or if positive and negative externalities balance each other out.

———. 2001b. "The Logic of Collective Belief." Unpublished Manuscript. Available at: http://www.gmu.edu/departments/economics/bcaplan/colbel10.doc.

Dawkins, Richard. 1989. *The Selfish Gene.* New York: Oxford University Press.

Friedman, David. 1988. "On Hoppe." *Liberty* 2(2): 44.

———. 1989. *The Machinery of Freedom: Guide to a Radical Capitalism.* La Salle, Ill.: Open Court.

David Gordon. 1988. "In Defense of Rights." In *Man, Economy, and Liberty: Essays in Honor of Murray N. Rothbard.* Walter Block and Llewellyn H. Rockwell, Jr., eds. Auburn, Ala.: Ludwig von Mises Institute.

Hildenbrand, W., and A.P. Kirman. 1988. *Equilibrium Analysis: Variations on Themes By Edgeworth and Walras.* Amsterdam: North-Holland.

Hoppe, Hans-Hermann. 1989. *A Theory of Socialism and Capitalism.* Boston: Kluwer Academic Publishers.

———. 1997. "On Certainty and Uncertainty, Or: How Rational Can Our Expectations Be?" *Review of Austrian Economics* 10(1): 49–78.

Hülsmann, Jörg Guido. 1999. "Economic Science and Neoclassicism." *Quarterly Journal of Austrian Economics* 2(4): 3–20.

Kant, Immanuel. 1977. "Prolegomena to Any Future Metaphysics." In *Classics of Western Philosophy.* Steven Cahn, ed. Indianapolis, Ind.: Hackett. Pp. 849–974.

Kirzner, Israel. 1997. "Entrepreneurial Discovery and the Competitive Market Process: An Austrian Approach." *Journal of Economic Literature* 35:60–85.

Landsburg, Steven. 1993. *The Armchair Economist.* New York: The Free Press.

Lomasky, Loren. 1989. "The Argument from Mere Argument." *Liberty* 3(1): 55–57.

Mises, Ludwig von. 1966. *Human Action.* Chicago: Contemporary Books.

———. 1972. *The Anti-Capitalistic Mentality.* Grove City, Penn.: Libertarian Press.

Murphy, Robert. 2000. "Utility Functions and Divided Preferences: A Note on Hülsmann." Unpublished Manuscript. Available at http://www.mises.org/journals/scholar/murphy%2EPDF.

Posner, Richard. 1998. *The Economic Analysis of Law.* New York: Aspen Publishers.

Raimondo, Justin. 2000. *An Enemy of the State: The Life of Murray N. Rothbard.* Amherst, N.Y.: Prometheus Books.

Reid, Thomas. 1872. *Essays on the Active Powers of the Human Mind.* Charlottesville, Va.: Lincoln-Rembrandt Publishing.

———. 1940. *Essays on the Intellectual Powers of Man.* Charlottesville, Va.: Lincoln-Rembrandt Publishing.

Rothbard, Murray N. 1960. "The Mantle of Science." In *Scientism and Values.* Helmut Schoeck and James Wiggins, ed. Princeton, N.J.: D. Van Nostrand. Pp.159–80.

———. 1962. *Man, Economy, and State.* Los Angeles: Nash Publishing.

——. 1963. *America's Great Depression*. Kansas City: Sheed and Ward.

——. 1977. *Power and Market*. Kansas City: Sheed Andrews and McMeel.

——. 1978. *For a New Liberty*. New York: Libertarian Review Foundation.

Stringham, Edward. 2001. "Kaldor-Hicks Efficiency and the Problem of Central Planning." *Quarterly Journal of Austrian Economics* 4(2): 43–52.