

## Reply to Caplan on Austrian Economic Methodology

By

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Abstract: A Methodenstreit is a debate in economics concerning the philosophy of social science. It involves the issue of which is the property method to pursue in the dismal science. Although this type of debate had its origins two centuries ago, the present paper is a contribution to a more modern Methodenstreit begun by Caplan (1999). It addresses some of the fundamental issues in economics: is this discipline best to be thought of along the lines of an empirical science, such as physics or chemistry (the view of the logical positivist school), or is it more properly described as a branch of logic or mathematics (the perspective of Austrian economics)? Is the argument synthetic a priori a coherent concept (the praxeological perspective), or a mere trivial tautology? Can empirical work (e.g., econometric regression equations) test economic axioms (are there any such things?), or merely illustrate them? These issues underlay the present debate over such issues as indifference (can there be any such thing in economics?), cardinality (is there room in economics for cardinal numbers, or is only ordinality to be tolerated?), continuity (are neoclassical findings the result of an artificial smooth curve assumption, or do they stem from real elements of the economy?), income and substitution effects (can there be backward bending supply curves and upward sloping demand curves?) and demonstrated preference and welfare economics (can government involvement in the economy possibly improve matters, or is this a logical contradiction in terms?)

JEL categories: B41, B49, B53

## I. Introduction

The first Methodenstreit<sup>1</sup> took place between the Austrians and the historicists during the last two decades of the 19<sup>th</sup> century. Participants from the Historical School included Gustav von Schmoller, Lujo Brentano and Werner Sombart. Their opponents from the Austrian School of Economics included Carl Menger, Eugen von Bohm-Bawerk, Friedrich von Wieser and Ludwig von Mises<sup>2</sup>. So important was this intellectual give and take that it led directly to the formation of the Austrian School of Economics.

The second major economic Methodenstreit was launched by Friedman (1953)<sup>3</sup>, and consists of it and the large literature to which that seminal article gave rise. So important has this publication been in the history of economic thought that 50 years after its publication, a conference was held focusing on this one article alone<sup>4</sup>.

A third and ongoing economic Methodenstreit, to which the present paper is a contribution<sup>5</sup>, began with the publication of Caplan (1999). He claimed that despite the highly touted “realism” of the Austrians, and the supposed unrealistic “pragmatism” of the neoclassicals, it was the latter who were actually more realistic than the former. Round two consisted of Huelsmann (1999) and Block (1999). The former articulated the position that Caplan (1999) did not accurately distinguish between praxeology, the science of human action,

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<sup>1</sup> Intellectual debate over proper method in economics.

<sup>2</sup> <http://en2.wikipedia.org/wiki/Methodenstreit>; <http://www.mises.org/hsofase/ch2sec3.asp>.

<sup>3</sup> Austrians who took part include Fox (1997) and Bostaph (1976). There are numerous other exchanges between Austrians and their intellectual opponents that do not even deserve the appellation “mini Methodenstreit.” For example, Nozick (1977) a neoclassical critique of Austrianism, vs. Block (1980), an Austrian defense against these denigrations.

<sup>4</sup> <http://www.eur.nl/fw/philecon/Friedman53.html>.

<sup>5</sup> Austrians took part, to a large degree, only in the first and third of these Methodenstreits. I mention this at the outset, since the present paper is written from that perspective.

and psychology. The latter took issue with Caplan's claim of realism on the issues of indifference, cardinality, continuity, uncertainty, demonstrated preference and public goods. Round three went to Caplan (2001) who used probability theory, and his views on common sense and ordinary language philosophy, to criticize Huelsmann and Block on the topics at issue: indifference, cardinality, demonstrated preference and welfare economics, and income and substitution effects.

After these interchanges, it becomes more difficult to characterize the debate in terms of discrete rounds. Suffice it to say that Block (2003) focused on the alleged difficulties of Caplan (2001) on probability, and Caplan (2003) returned the favor. As well, Block (unpublished) and Van Dun (unpublished) concentrated on what they saw as errors in Caplan's views on the Bayesian form of probability, while Van Dun (unpublished) also took Block (2003) to task for what he saw as the latter's kid glove treatment of the former, on praxeology. Barnett (unpublished) offers a critical analysis of Caplan (1999, 2001) in their entirety.

The present paper attempts to point out weaknesses in Caplan (2001) with regard to II. Common Sense and III. under the rubric of specific replies: 1. indifference, 2. cardinality, 3. continuity, 4. income and substitution effects and 5. demonstrated preference and welfare economics. It concludes in section IV.

## II. Common sense

Caplan (2001, p. 10) starts off this section by berating me for drawing a sharp distinction between ordinary language and scientific discourse. Yes, I agree, there is certainly a use for the words "indifference" and "envy" in ordinary language, and we all know full well how to apply them, accurately in most cases.

Now comes this (2002, p. 10) leap of logic: “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.’” But the second claim does not at all follow from the first. Even if it is true that Caplan is indifferent between his green and blue sweaters, or that “Socialists envy the rich”<sup>6</sup> (Caplan 2002, p. 2), both of which I am readily willing to accept, why does this logically imply that it matters not one whit whether we are (still) in the realm of ordinary discourse, or have entered the more rarified domains of economic analysis? On the contrary, I maintain, it matters and it matters very much.

For one thing, there is always a matter of attaining truth, even if only for its own sake. Something can be true in the one arena, and not in the other, simply because words are being used differently in the two contexts. For another, on pragmatic grounds, without indifference in the technical sense there can be no such thing as indifference curves. This alone, in one fell swoop, would under or unemploy hundreds of mainstream economists, certainly a matter of practical interest. And with these curves would go such “discoveries” as transitivity, retained not because people cannot change their rank preference orderings (even neoclassicals, if you get them in a mellow mood, might concede this), but because if they are allowed to do so this will play havoc with hypothesis testing, another logical positivist excrescence presently inflicting the dismal science.

Avers Caplan (2001, p. 10): “... it is hard to avoid the impression that my critics label claims as ‘non-praxeological’ in order to somehow exclude them from the discussion.” Not a bit of it. No one in the Austrian camp is excluding ordinary language from discussion. I readily agree to the

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<sup>6</sup> This could well be interpreted as a tautology. We would scarcely allow that a person is a socialist if he didn’t promote envy of the rich, and feel that way himself.

truth of indifference -- but only in ordinary, not technical, non praxeological discourse. As a speaker of the English language, I have been known to employ these words. As a critic of Caplan's views, I (Block, 1999, p. 22) explicitly pointed out that these words have a proper use. What more can I do?

Next, Caplan (2001, p. 10, ft. 11) taxes the Austrians with logical inconsistency: on the one hand we reject his attempt to distinguish between ordinary and technical language, on the other hand we, e.g., Rothbard (1962), Hoppe (1989) "all acknowledge a role for empirical assumptions to supplement pure praxeology, ... such as the value of leisure, human and resource variety..."<sup>7</sup>

However, I fail to see why these two occurrences are related. Yes, broad empirical generalizations such as mentioned above play a role in Austrian analysis. But these are hardly necessary<sup>8</sup>. In any case, why, just because we praxeologists incorporate these broad empirical generalizations into our analysis must we jettison the distinction between scientific and ordinary language? And since when did anyone on the praxeological side of the aisle reject Caplan's attempt to distinguish between ordinary and technical language? Very much to the contrary, this was an essential part of Block (1999), as even Caplan (2001, p. 10) admits.

Another unjustified leap in logic occurs with regard to possible purely semantic disputes. States Caplan (2001, p. 11):

"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

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<sup>7</sup> This reminds me of a joke. A group of friends have for many years sat around telling each other jokes. They know each other's repertoire so well that they have become accustomed not to telling the jokes themselves, but rather just mentioning the numbers that now represent them. For example, one will mention "29," and they all burst out laughing. Another will say, "301," to the same effect. Along came a newcomer to the group who, catching on said "15." But no one laughed. When he enquired as to why he was being treated different than everyone else he was told "it's all in how well you tell the joke."

<sup>8</sup> Although it cannot be denied that without them, the enterprise could still be conducted, but at the loss of numerous interesting avenues of discourse which would have to be foregone.

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.”

But why? Note that Caplan gives no reason for his supposition that “if apparent conflicts between Austrian claims and common sense were purely semantic ... then it would be reasonable to suppose that neoclassical-Austrian disputes were purely semantic as well.” So again I ask “why so?” Let us try this on for size using the example mentioned in Block (1999), the difference between the ordinary language use of the word “work” and that employed by physicists to this end. In the latter case, work equals force times distance. In the former, it pretty much includes anything that brings sweat to the brow. If a person holds two dumbbells weighing 50 pounds each with arms extended at shoulder height, sweat will pretty soon start popping out on his forehead. If this is not “work” in the common sense notion of the word, then nothing is. But for the physicist, since the dumbbells move not a scintilla of an inch, no “work” is being done. So there is a semantic difference in word usage between the ordinary man and what we will call the “Austrian” physicist, to maintain our analogy. But does this mean that somehow such a “praxeological” physicist would find himself enmeshed in a verbal dispute with a physicist of another persuasion, or with anyone at all for that matter? Not at all. It simply does not follow as a matter of logic that having a verbal dispute with ordinary language has any such implication. It is incumbent upon Caplan to give reasons for his assertions; merely making them will not do. We need, therefore, not follow down Caplan’s (2002, p. 12) speculation as to “biting the bullet.” Just as the physicist who defines “work” differently than the common man would not want to get into a debate with him over whether holding dumbbells is work or not, so would the Austrian who (operationally) defines indifference or envy differently than is typically used would eschew any analogous altercation about who is “really” right. That is the whole point of a

verbal dispute: there is no right or wrong answer, since the disputants are not talking about the same thing. Austrians certainly do not quarrel with ordinary language in these cases, since they and the common man occupy two different universes of discourse. Our conflict is with mainstream economists who (I would contend) mistakenly employ the ordinary language notion of envy or indifference to technical issues.

Somehow, Caplan (2001, p. 13) gets himself from verbal disputes between praxeology and ordinary language to “conflicts with common sense.” This is quite a leap. Let it then be said once and for all, loud and clear, there is no conflict between Austrianism and common sense on any of these matters; there is only a verbal dispute, which emanates from investing the same word with different meanings.

Let me take another hack at this, from a slightly different perspective.

In ordinary language, I am willing to concede, Caplan can be indifferent between his green and blue sweaters, even though he chooses the former. As long as he didn't always pick that one when confronted with both, such a statement is unexceptionable. But if once we allowed this “truth” into the sanctified halls of professional economics, havoc would be the result. Why, we would be reduced, horrors!, to the level of mere neoclassical economics. There could no longer be any objection to indifference curves, for one thing! For another, we could no longer infer gains from trade, *ex ante*. There would now be the possibility, nay, “probability,” in Caplanian terms, that one or even perish the thought both of those engaged in a commercial interaction were only doing it out of indifference. Purchases and sales would be rendered into mere charades, with there now being no necessity for purposeful gain on the part of those who engage in them. At one fell swoop the heart and soul of economics, purposeful human action in an attempt to better one's lot in life, would be ripped away from us as an explanatory tool.



If this would be a disaster for economics, it is equally unnecessary, happily, that we accept the truth of his indifference claim. This was precisely the point I made (Block, 1980) against Nozick's (1977) charge that Austrians could not coherently define the concept of a supply curve without indifference. Yes, before a choice has to be made, we may speak loosely of a supply of a good, all elements between which we are indifferent, or of Caplan's two sweaters, which he values equally. But at or during the point of choosing, if a choice is actually made, then indifference, necessarily, can play no role. If the person was truly indifferent between the two choices, how, ever, did he decide to pick one over the other. "Flipping a coin" is no answer, since the economic actor must *accept* the result of the coin toss, and how can he, if he is indifferent?

Is there a "verbal dispute" between Austrians and ordinary language speakers? On the one hand, there is, if we loosely interpret<sup>9</sup> this claim that one can be indifferent between two things and yet choose one of them. The reconciliation between the two consists of pointing out that the word indifference is being used differently in the two contexts: strictly by the praxeologists, and only roughly by the man using "common sense."

On the other hand, there is no verbal dispute if we take seriously the common man's claim, espoused by Caplan, that one can be indifferent between two things and yet choose one of them. Then, the Austrians are correct, and the "common sense" claim incorrect.

This is not at all to the liking of Caplan (2001, p. 13) who states: "In the social sciences... conflicts with common sense are far more suspicious (than they are) in the natural sciences."<sup>10</sup>

Yes, we must of course be "suspicious" of economics when its conclusions deviate from those of

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<sup>9</sup> It is in this regard that I can join with Caplan's (2002, p. 12) depiction of the Austrian view on these common sense claims that they are "colorful metaphors, not literal assertions."

<sup>10</sup> Material in brackets inserted by present author.

the common man. For that matter, we must be “suspicious” of economic findings even when they agree with “folk wisdom,” and under all other circumstances imaginable as well.

“Suspicion” is truly the middle name of the dismal science. But this hardly means that “common sense has priority” (Caplan, 2002, p. 14) over praxeology. First, it is not at all clear that if the Austrian’s insight could somehow be explained to the non-economist, that he would not see the correctness of denying that a person can be indifferent between two things and yet select one of them<sup>11</sup>, even if Caplan cannot see this. Secondly, “common sense” can be shown to be misleading on a whole host of issues, where it deviates from the findings of Austrian economics. If we use public opinion polls as a proxy for “common sense,” then examples include the minimum wage law, rent control, “caps” on energy prices, usury laws, and interferences with free trade, with “profiteering,” and with saving.

Another argument of Caplan (2001, p. 13, ft. 13) is as follows:

“Misesians might be tempted to reply that the action axiom has a probability of 1 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?”

This sounds suspiciously like the “argumentum common sensicum,” a newly discovered informal fallacy. In terms of chart 2 we need not be in category IV (e.g., “the action axiom has a probability of 1”) in order to refute this misunderstanding on the part of Caplan and his “common sense” confreres; section III, with all due respect for our possible

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<sup>11</sup> Certainly, my own introductory students easily grasp this insight (having not been perverted by the type of graduate training recently undergone by Caplan), although for obvious reasons (small biased sample) I would not want to greatly rely upon this as evidence for my contention.

errors in deductive capacities will do very well. The point is, Caplan (2001, p. 2) is here doing precisely that of which he accuses Austrians:

“... 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.”

The Austrians are not guilty of casting doubt for doubt's sake instead of providing argument; rather, Caplan is. Note, his point is merely that when in doubt between the two of them, the presumption is that we trust the common man rather than the economist. This is not an argument, it is an attempt to camouflage its absence. It is a denial of specialization and the division of labor. Would Caplan be willing to extrapolate from this insight? Would he apply it, for example, to physicians? That is, would he maintain that when old wives' tales and medical science deviate (say, on some quack cure) that we should be inclined to follow the former? Not likely. Why then for economics?

I want to be able to both have my cake and eat it too, at least in this context. On the one hand, I want to be able to admit, as an ordinary language speaker, that Caplan can be indifferent between his blue and green sweaters, even though he chose one and not the other, so close were there values to him in his estimation. On the other hand, I want to maintain that as a matter of technical economics, it is simply wrong, untruthful and incorrect to take this stance. When we speak carefully, as social scientists, we must see this claim for the error that it is. In my view, nothing in Caplan (2001) forces me to change my perspective on this.

Caplan (2001, p. 13) states: “The literal existence of indifference ... is not mere hypothesis; we know it is real from introspection.” Yes, yes, I agree. I, too, have felt, or experienced, or understood the concept of indifference. But what good is it to the economic analyst in

attempting to understand and explain economic reality? It provides very little if anything to this, one of the basic goals of our science. This is because it helps us not one whit to ferret out the purposes of other people. As it stands, indifference leads to no human action whatsoever. If not, it cannot be used as a means to shed light on commercial behavior.

### III. Specific replies

#### 1. Indifference

It is not true that “Common sense and neoclassical theory say we can be indifferent; Misesians say we cannot” (Caplan, 2002, p. 14). The first two claims are non controversially correct, but not the third. Misesians, too, allow that we can be indifferent, but only as ordinary language speakers, not qua economists. Why not? Because there is no human economic action that can unambiguously demonstrate indifference. When A gives up an a to B in return for the latter’s b, we can infer that A ranked the two goods b/a and B ranked them inversely, a/b. Economic activity, in other words, reveals preference. But indifference? We have to take Caplan’s word for it; he cannot demonstrate his indifference between his green and blue sweater by anything he does. I challenge Caplan to point to a commercial act that demonstrates<sup>12</sup> indifference. If he cannot, he abandons the role of the economist in explaining human action when he himself is not a party to it.

Nor can I accept Caplan’s (2002, p. 14) claim that “I can imagine being indifferent in a praxeological sense (I prefer a and b equally)...” I join with him as an ordinary language speaker in reporting internal feelings of indifference. But it is a simple misuse of the word “praxeological” to assert that this philosophy is compatible with indifference in the technical sense. If Caplan were correct in this assertion, it would simply leave no room at all for my

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<sup>12</sup> In the sense of this word employed by Rothbard (1997).

position. It would make it totally incoherent, utterly meaningless, not merely erroneous, and I have seen no evidence that this author regards it in that way.

Caplan (2001, p. 14) calls me out (1999, p. 24) for wondering “Why would anyone bestir himself if there were absolutely no gain in it for him?” To the contrary, he (2002, p. 14) maintains, “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?’” The reason I would not act under these circumstances even if there were no cost to me of so doing, is that there is nothing in it for me in engaging in the relevant commercial enterprise.

I see a pair of shoes in the window of a store, selling for \$100. This happens to be the exact valuation I place upon these shoes<sup>13</sup>. In my view, I would not buy them, because there would be no consumer surplus in it for me. Even most neoclassical economists, totally unaware of the niceties of Austrian economics, would go along with me on this. Consult any intermediate or even introductory micro text on this under the topic of consumer’s surplus. Caplan, however, seems to be going out on a limb that very few of his confreres would follow. He is saying, if I understand him correctly, that I would purchase these shoes (provided only that I could do so at absolutely no cost to myself over and above the sale price of \$100). But again, why would I do so? Don’t we have to explain economic activity in terms of some sort of motive or goal or purpose? And isn’t profit seeking, or self-improvement, or welfare increase the explanation which transcends the division of economists into Austrian and neoclassical? The point is, here Caplan is not talking merely in terms of the existence of indifference as some sort of free floating

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<sup>13</sup> This sentence makes sense only in common language. As far as technical economics is concerned, all such expressions must be couched in terms of preference, not equality or indifference. I owe this point to Guido Huelsmann.

abstraction, but rather of actually acting in the face of indifference, an even worse fallacy from my point of view.

There are always costs to acting. At the very least there are alternative costs to all actions, in that whenever a man acts, he foregoes other opportunities. Caplan's phrase "... if there were absolutely no cost to him of doing so..." is a literal impossibility.

Caplan (2001, p. 15, footnote 14) tries to drive a wedge between Rothbard and myself on the issue of Buridan's ass. He contrasts my statement to the effect that "If indifference were his exact mental state, surely he would select neither article of clothing" (Block, 1999, p.22), with Rothbard's, which Caplan maintains, "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 by Caplan.) This author (2002, p. 15) cites Rothbard against me: "Buridan's ass ... is confronted not with two choices but with three, the third being to starve where he is."

I reject this sally of Caplan's not because I wish never to differ from Rothbard on anything<sup>14</sup>, but because there is no divergence between the two of us on this particular point. In my reading of Rothbard, he would agree that if either a person or an ass were truly indifferent between two options, and if there were no third alternative, then he would choose neither; or, alternatively, the fact that a person or an ass picked A/B shows that he preferred the former to the latter, not that he was indifferent between them, again in the complete absence of a third option. Rothbard's contribution on this point was not to deny any of this, but, separately, very much beside this particular issue, to point out that the case of Buridan's ass there was actually a third option

available. It was never any part of my brief to deny this; on the contrary, I (Block, 1999, p. 22) cited Rothbard on this very point, as Caplan (2001, p. 15, ft. 14) acknowledges, although he calls it “strange.” Let us attempt to obviate any strangeness here by confronting a case where this is no necessary third option. For example, an ass faced with some straw which he can either eat or not eat, or Caplan confronted with but one sweater, which he can either wear or not. Rothbard’s insight simply does not apply to these two cases, and we are left with him and myself, united, on one side of this issue, and Caplan on the other. The two of us infer preference for either starvation or life on the part of the ass, depending upon his choice, and either wearing the sweater or not in the other case, while Caplan, in sharp contrast, makes no such inference. Rather, for him, while preference is certainly possible in either of these two cases, so is indifference.

Rothbard, his feet mired deeply in reality and common sense, notes that the ass actually has a choice not only between haystack A and B, but also between either of them and starving to death. This cannot be denied. However, this insight is a red herring insofar as the debate between Caplan and myself is concerned; I fear it distracts my intellectual opponent from the point at issue and confuses him. Let us therefore abstract from Rothbard’s insight, and limit our perspective to the choice only between A and B. We suppose further that the ass chooses A (it may well be that he does this to avoid starvation, not out of any initial preference for A; that is not now our concern). Having chosen A/B, are we (e.g., Caplan and other neo classicals) entitled to conclude that the ass A is indifferent to B? I submit we are not. For the question that undermines any such facile assessment is: If the ass was really indifferent between A and B, why oh why did he choose A? Yes, I am willing to “concede” to Caplan, before the ass had to choose

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<sup>14</sup> There are only two issues upon which I have so far differed with Rothbard in print: voluntary slavery and

between these two bales of hay, we may reasonably surmise (if we step out of our roles as economists and embrace those of the typical ordinary language speaker) that he was indifferent between them. But, now that the ass has to choose, and selects A/B, any such analysis can no longer be considered valid. If the ass chooses A/B, he is not indifferent, he cannot be indifferent, it is no less than a logical contradiction to suppose that he remains indifferent. To repeat the Austrian maxim, only preference, not indifference, allows for human action, let alone is compatible with it.

Caplan (2001, p. 15) finds “unimpressive” my “attempt to show that (his) position is self refuting.” I fear that this is because either he did not read my attempt carefully enough, or because I was not sufficiently clear in my first (1999, p. 24) attempt to demonstrate this. Let me try again. I had stated:

“Even Caplan, in his attempt to deny this primordial reality, is himself snared in its clutches. For this George Mason economist is not indifferent between the Austrian (anti indifference) and the neo Classical (pro indifference) theories. Rather, he prefers the latter to the former, and offers reasons, to the best of his ability, why the still undecided members of the profession should cleave to his own views. From this we can make certain inferences. But what would follow were he truly indifferent to these two viewpoints? Nothing. Caplan could attempt to reply on his own grounds that he is indifference (sic, I am sure he meant to say ‘indifferent’) between the Austrian and neoclassical positions and yet, still wrote his (1999) paper. But this would not be true on technical grounds. This would be a very strange way indeed to demonstrate praxeologically he is indifferent between the two views.”

Caplan (2001, p. 15) rejects this on the ground that his “thesis is not that we are always indifferent, but that we are sometimes indifferent.” But I do not think I said, and certainly did not mean to imply, that we are always indifferent, or, indeed, that Caplan said any such thing. Nothing of the sort could be reconciled with the fact that Caplan has

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abortion. On the former see ; on the latter .



survived to the ripe old age of whatever age he now is. For to live one must choose, and to choose one cannot be indifferent.

Rather, I meant to say, and still maintain, Caplan's objection notwithstanding, that if he somehow became indifferent between the Austrian and neoclassical views on the issues under discussion, he would then find himself in a conundrum: he would have no way to demonstrate his newly found indifference. If he wrote a paper similar to the one he did, it would be an unequivocal attack on Austrianism. If similar to my own (1999) or to Huelsmann's (1999), then the very opposite. Nor could this possibly be done even by an economist undecided about these issues. We may posit that such a person could write a pro and con list for each side, and then at the end of this exercise still be unsure of where his own considered opinions lay. Could not even he be indifferent? Yes, in the ordinary language usage of this word in which Caplan was indifferent between his green and blue sweaters, or in the sense in which most men in the street know nothing of this debate, and, if possible, care even less. But, again, not in the technical sense.

Caplan reveals himself as less than entirely firm on the distinction between utility ex ante and ex post, which even mainstream economists adopt. He (2002, p. 15) states: "So what if one-in-trillion (sic) exchanges strictly raise the utility of neither participant?" First, why "neither?" This seems a bit of overkill. All he needs for his point is that one in a trillion exchanges fail to benefit both parties. Second, and more importantly, I am perfectly willing to stipulate that a small but significant minority of commercial interaction fails to benefit at least one participant; but only in the ex post sense, not the ex ante. Here Caplan fails to perceive the praxeological point that it would be a denial of logic for anyone to take part in a purchase, sale or barter if he

did not at least expect to improve his position; why ever else would he agree to do so if not on the basis of that hope?

## 2. Cardinality

I find Caplan's treatment of this subject most unsatisfactory. Not because of what he said, but for what he didn't say.

The main intellectual or academic purpose of such exchanges as I am now involved in with this author is to push out the envelope of knowledge. It is the hope that out of thesis and antithesis will emerge a synthesis that more closely approaches the truth than before. But in order to attain this goal, it is imperative that each participant recognize all points made by his opposition, not merely those on which he thinks he can score points. I spent roughly two entire journal pages (Block, 1999, pp. 24-26) criticizing Caplan's (1999, p. 827) views on cardinal utility. I dealt with his claim that the neoclassicals eschew cardinality, I charged that the much vaunted mainstream "cost-benefit" analysis is no more than a futile exercise in cardinality mongering, I cast aspersions on his attempt to evade the distinction between cardinal (1,2,3, etc.) and ordinal (1<sup>st</sup>, 2<sup>nd</sup>, 3<sup>rd</sup>, etc.) numbers, and maintained that the typical textbook interpretation of the tangency between the budget line and an indifference curve provides for the invalid division of so called cardinal by ordinal numbers.

Yet to not one of these points did Caplan (2001) reply. I earnestly entreat him that should he choose to take further part in this debate he be more thorough in his future rejoinders.<sup>15</sup>

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<sup>15</sup> Perhaps this assessment is too harsh. After all, there is such a thing as freedom of speech and academic freedom. It is not entirely cricket for one party to a debate to dictate to another what he should have said. Quite possibly, Caplan may wish to call my response to him into question on this ground, although I have tried to be as fully reactive to his arguments as I could. On the other hand, there is surely something to what I have just said. Intellectual progress cannot as easily come about unless there is "full disclosure": full coverage of each other's points.

Instead, he attempts to cudgel Huelsmann (1999) concerning price ratios and identity of units. I again ask that Caplan explain to me precisely how 7<sup>th</sup> or 9<sup>th</sup> or 22<sup>nd</sup> or any other cardinal number can appear in either the numerator or denominator of a fraction. If it cannot, then the tangency point between the budget line and the indifference curve cannot be expressed in equations; e.g., it is invalid, and must therefore be stricken, along with the indifference curves that underlay it.

### 3. Continuity

A similar point can be made about Caplan's (2002) failure to come to grips with my (Block, 1999, pp. 26-28) critique of his earlier (Caplan, 1999) criticism of Austrian theories on continuity. To wit, I found fault with his view that "the unrealism of continuity is only minor" (Block, 1999, p. 26), and showed, through three average cost diagrams, how this concept befouls the professions' view of monopoly as a market failure and the anti trust policy based on it; and I overturned his attempted reductio regarding the monetary unit by utilizing the Austrian theory of equilibrium, all of which is unfortunately ignored<sup>16</sup>.

This author (2002, p. 16) contents himself solely with the following two sentences in response: "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."

This simply is not good<sup>17</sup> enough<sup>18</sup>. Continuity is like a rot, which has infected the entire corpus of mainstream economics. Human action simply does not occur in infinitesimally small steps, as

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<sup>16</sup> I also attempted to set Caplan straight on the issues of supply and demand, based upon what I saw as his misinterpretation of Rothbard's (1962) work, and of the triteness of the neoclassical school. But Caplan has a wonderful "excuse" for ignoring this section of my paper: due to a printer's error, these pages were mistakenly deleted from the published version of it. However, the missing pages are now available in an errata: [http://www.mises.org/journals/qjae/pdf/qjae2\\_4\\_9.pdf](http://www.mises.org/journals/qjae/pdf/qjae2_4_9.pdf).

<sup>17</sup> Caplan does not appear to recognize how devastating a concession this is to his defense of neoclassical economics. The distinction between perfect and imperfect competition – surely a bedrock of mainstream

implied by this assumption. And any practice that so radically misconceives the human condition is almost guaranteed to have negative repercussions. Monopoly theory is but the tip of the iceberg<sup>19</sup>. The market failure literature in general is practically riddled with errors emanating from this source. In addition to those applying to monopoly, there are various market failure dead weight losses, all of which, purportedly, are calculated by use of integration, which also requires the smooth curve or differentiability assumption. Consider as cases in point to begin with only monopsony (which, admittedly, it at least the first cousin of monopoly) and externalities. In the examination of “neighborhood effects,” or external economies, how often have we been treated by mainstreamers to shifting supply or demand curves, which are moved around in these peculiar manners in order to depict the difference between social costs and benefits and their private counterparts, much to the advantage of the former vis a vis the latter. But then, virtually all of supply and demand analysis is vulnerable to this charge, and Caplan can hardly deny that this is a basic building block of virtually all of economics<sup>20</sup>, both Austrian and neoclassical varieties. The idea that equilibrium can be depicted as a point, rather than as an

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economists -- intimately rests on monopolistic competition. Further, in making this concession, Caplan removes himself from the mainstream consensus, and steps not only into the Austrian one, but into the extreme praxeological Rothbardian wing of this movement.

<sup>18</sup> Asks Caplan (2001, p.16, ft. 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.” I am dubious about this. Were it true, then neoclassical economists in some specializations (e.g., trade, labor, etc.) would have adopted Austrian views on continuity; it would be only due to a greater division of labor that the message of these particular practitioners would have failed to percolate out into the profession as a whole. But I ask in vain for Caplan to point out to me any sub speciality of economics where neoclassicals widely adopt discontinuity.

<sup>19</sup> Caplan (2001, p. 16) thinks that I “forget that neoclassical economists have spent a staggering amount of brain power ... on ... continuity.” The exact opposite is the case. I do not forget it at all. Rather, I complain of it.

<sup>20</sup> This would leave out Marxist “economists,” but there is at least a strong question of whether they should be considered members of this discipline in the first place.

inequality, which can emerge from Austrian step function supply and demand curves, is but a further instance of the continuity assumption.<sup>21</sup> Then there is cost benefit analysis, which also relies upon this premise. In fact, it is only a slight exaggeration to say that all neoclassical calculations, apart from very unsophisticated basic ones depend upon smooth curves<sup>22</sup>.

Caplan (2001, p. 16) condemns Huelsmann for making this very point on the ground that “this position is so counter-intuitive that, contrary to Huelsmann, he is virtually the only economist to ever embrace it.” Not so, not so. I hereby enter the lists in behalf of this position. That makes at least the two of us.

This is not to deny that Austrians, too, use smooth curves to illustrate economic variables. I said as much in Block (1999, p. 27). But these are utilized only as heuristic devices. No praxeologist worth his salt would deny that behind these continuous functions lie the more realistic jagged curves, something no neoclassical would admit.

Caplan (2001, p. 16) notes that I “excuse Rothbard’s habitual reversion to intersecting supply-and-demand diagrams” as a heuristic device. He (2002, p. 16) claims that “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.”

But it is not merely a heuristic device for mainstream economists; no, they live and breathe in this context. Please show me a non-praxeologist who stresses the importance of discontinuous functions.

#### 4. Income and substitution effects

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<sup>21</sup> There is an exception in this case, however. Austrians characterize this as a “highly unlikely event” (Rothbard, 1962, p. 114). I thank Guido Huelsmann for pointing this out to me.

<sup>22</sup> There is also the highly regarded indifference curve analysis, which typically but not always is based on continuity, which has been criticized above on different grounds.

Caplan (2001, p. 18, ft. 18) tangles with Huelsmann (1999) over the issue of Mises' views on monopoly. Caplan accuses Huelsmann of overlooking the fact that "In fact, Mises did 'bother' about the shape of supply curves," and then proceeds to quote him to this effect, concerning the monopolistic buyer and seller (Mises, 1966, p. 383).

So far, I have been assuming, with Caplan, that there are two categories of Austrians; one, headed by economists such as Mises, Rothbard and (I have added) Hoppe, call them the praxeologists, the other, with such people as Hayek and Kirzner, call them the moderate Austrians.

It is now time to relax this assumption, at least as far as Mises is concerned, at least on the one issue of monopoly, and hence either for the monopolistic supply curve<sup>23</sup>, or the one facing the monopsonist. The problem with Caplan's attempt to drive a wedge in between Mises and Huelsmann, to embarrass the latter with the quote from the former, is that Mises is not really a "Misesian," if only on this one issue<sup>24</sup>. To put this in other words, while on virtually every topic under the sun Mises full well deserves to be listed amongst the radical or extremist praxeologists, that does not apply in this single case. Thus, Caplan presents no difficulty at all for Huelsmann in showing that the latter and Mises have different perspectives on the supply curve of the monopolist or monopsonist.

Caplan (2001, p. 17) disparages my attempt to rescue Rothbard (1962) from his (Caplan, 1999) criticism of the latter on the grounds that he "contradicts himself by introducing income effects and backward-bending supply curves after purporting to prove that the laws of supply and

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<sup>23</sup> Such that it is; in neoclassical theory, this supply curve for the monopolist is merely one single point. For the radical Austrian, this entire discourse is invalid. See on this Rothbard (1962, ch. 10).

<sup>24</sup> For an explicit critique of Mises and Kirzner as deviating from praxeology on monopoly theory, see Block (1977).

demand are exceptionless theorems.” I (Block, 1999, p. 29) defended Rothbard on the ceteris paribus assumption of no income changes. That is, for example, there cannot be any backward bending part of the supply curve if income is not allowed to change as we move along it, nor can there be any upward sloping demand curve, e.g., based on the Giffen good, under this same assumption. Caplan’s (2002, p. 17) critique of me: “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.” But another equally valid point is that it is always possible to define a theoretical demand (or supply) curve along which income does not change<sup>25</sup>. My claim is that this was Rothbard’s implicit assumption, and it wards off Caplan’s charge that Rothbard (1962, pp. 106, 515) contradicts himself on this issue. Even if we do not interpret Rothbard (1962, pp. 106, 515) in so sympathetic a manner, this it is a relatively minor mistake to overlook the effect of income on the shape of supply and demand curves. It certainly does not warrant Caplan’s (2002, p. 18) rather hysterical condemnation “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.” This, rather, sounds like a “wild exaggeration” (Caplan, 2002, p. 8).

#### 5. Demonstrated preference and welfare economics

In this section, Caplan (2001, p. 18) is back at the same old lemonade stand, beating the drums for envy: “If verstehen works for signing a contract, it works for envy too.” I have given my reasons for rejecting this contention (Block, 1999, pp. 30-32). I will not reiterate them, except to

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<sup>25</sup> Slutsky () and Hicks () provide two ways in which this can be accomplished.

mention that Caplan has not only failed to come to grips with these arguments, he has totally ignored them.

Instead, let me admit to being vanquished. All right, okay already, I concede (at least for the sake of argument) that Caplan is correct. Envy is real. It can be scientifically established. There are even, since as a neoclassical he is so intent upon this and my fondest wish is to accommodate him, units of envy called “envies.”<sup>26</sup> We can contemplate supply and demand curves for envy, with price on the vertical axis and the quantity of “envies” on the horizontal.

What will be the position of economics under this new dispensation?

To begin with, the claim of free enterprise economists<sup>27</sup> to the effect that free trade is socially beneficial goes by the board. For any left wing environmentalist, any trade unionist, any socialist, is now in a position to object to any purchase, sale, hire, rental, in short any commercial or other interaction whatsoever, on the ground that he is envious of the fact that this makes both parties to it better off, and hence wealthier<sup>28</sup>. Before we can reach the conclusion that economic welfare is thus enhanced, we must first ensure that the gains of the beneficiaries are greater than the losses suffered by third parties who object on grounds of envy. Perhaps this could best be dealt with by a U.S. Department of Envy. The Republicans may not inaugurate such an initiative, but we can be sure, based upon their failure to repeal the Department of Education, that when the Democrats institute this new Department, they will not rescind it, either.

Envy is like the Pandora’s Box for the profession of economics. Once allow its cloven hoof in under the tent, and pretty much all of welfare economics is lost. Does Caplan really want that?

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<sup>26</sup> Think “utils.”

<sup>27</sup> I still include Caplan under this rubric despite the foregoing

<sup>28</sup> The egalitarian, no doubt, will object to trade for the reason that not all people engage in the same number of trades, and that this too is unfair.



Presumably not. But if not, then he is forced by the logic of his words into a sort of super skeptical view of even so pedestrian an occurrence as signing a contract. “Penmanship” (Caplan, 1999, p. 833) indeed.

According to Caplan (2001, p. 19): “... more mutually beneficial trades could have happened if conditions (in this case, communication) were somehow better.” I fail to see how the inclusion of, or emphasis on, “at that moment” saves Caplan from his error. To reiterate, Rothbard (1962, p. 768) made the insightful comment “If we may use the term ‘society’ to depict the pattern, the array, of all individual exchanges, then we may say that the free market maximizes social utility, since everyone gains in utility from his free action.” Caplan (1999, p. 833) dismissed this as a “simplistic non sequitur” on the ground that “... it is simply confused to posit latent preferences; if two individuals fail to make an exchange, then this ipso facto demonstrates that at any moment at least one of them would not have benefited from the exchange.” But as I said in Block (1999, p. 31):

“Of course the free enterprise system only permits the implementation of all desired voluntary exchanges, this is a far cry from guaranteeing any such outcome. Caplan appears to assume the latter, however. For it is perfectly possible for there to be a failure to communicate. Just because all trade is mutually beneficial does not mean that ‘if two individuals fail to make an exchange, then this ipso facto demonstrates that at that moment at least one of them would not have benefited from the exchange.’ Both could have benefited, if only they had but known of each other.”

I agree whole-heartedly with Caplan (2001, p. 19) that “... more mutually beneficial trades could have happened if conditions (in this case, communication) were somehow better.” But so what? Just because it is likely that more trades will take place with better communication does not render Rothbard’s insight a “simplistic non sequitur.” This author’s attack on Rothbard, to use terminology employed by Caplan (2001, p. 18) “makes about as much sense” as me

criticizing Caplan's (correct) views on minimum wages because he is a white male, and hence part of the power structure, and therefore not to be trusted in his analysis of this law.

Caplan (2001, p. 20) also puts me on the hook for thinking that there is something "inherent in public goods theory that makes it (quoting from Block, 1999, p. 35) an 'ideological wedge for government intervention.'" In rebuttal, he offers Friedman (1989, pp. 156-159) and his own (2001b)<sup>29</sup>. But I never said or implied that there was anything inherent in public goods theory that of necessity renders it only a criticism of markets. I only said (Block, 1999, p. 35) that this concept "is used" for the purpose to which Caplan ascribes to me. Surely he would not be rash enough to deny this. Caplan (2001, p. 20) himself admits that "Most economists are statist, 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." No truer words were ever said. I would only amend this to say that most neoclassical but not Austrian economists are apologists for dirigisme, and that while there are certainly government failures, I have not yet met one that could be fairly characterized as a "market failure."

#### IV. Conclusion

Caplan (2001, pp. 21-22) accurately quotes me (Block, 1999, p.24) to the effect that in the praxeological perspective, human action, or behavior, is necessary to demonstrate motive or introspection. His (2002, p. 22) objection: "Individuals can know their own motives from introspection alone." No doubt, (Mr. Probability) Caplan would concede that they only know their own motives imperfectly, not for certain. Which of us, after all, has not witnessed a person yelling at the top of his lungs "I am not angry!!!" when it was obvious, at least to everyone else, that he was. Some of us have even done this ourselves, hard as it is to believe. It is only a slight

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<sup>29</sup> He could well have added Lee, 1999. For a rejoinder to that essay, see Block (2002).

exaggeration to say that the entire field of psychology is predicated upon at least the partial falsity of Caplan's assertion. Practitioners in this field attempt to discern hidden motivations, concealed from the person himself. From an economic perspective, even the limited extent to which Caplan's claim is true, is all but irrelevant. For the dismal science is mainly an attempt to explain the actions of others not merely of oneself<sup>30</sup>. And while *verschtehen* gives us a leg up in interpreting and understanding the behavior of other people, this is based on their behavior, sometimes subtle, sometimes not so subtle. In any case, Caplan reckons without taking into account the meaning of the word "demonstrate," in Austrian economics (Rothbard, 1997). It means to publicly demonstrate, or to reveal, choices. An act made solely within the confines of one's own mind is not a human action of the sort with which economics deals. For Austrian economics, there are no free-floating, abstract, separate preferences, existing in a disembodied state, or on a blackboard. They are necessarily part and parcel of human action.

Caplan (2001, p. 22) speculates as to "what could possibly motivate this queer refusal (on the part of Austrians) to recognize the reality of most mental states?" ones presumably disembodied from actual behavior. Has he not yet heard that specialization and the division of labor apply to intellectual pursuits as well as to all others? Economic imperialism is a movement<sup>31</sup> we can all embrace: the taking over to general improvement by economists of questions hitherto the domain of other social sciences. But this is surely going too far. It is not at all the job of the economist to enquire into the truth-value of someone discussing his inner emotional state.

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<sup>30</sup> Of course, there is such a thing as Robinson Crusoe economics. An example of this would be Caplan's own musings about his choice of sweater, and his indifference between two of them.

<sup>31</sup> See on this Becker (1957, 1964, 1976), Radnitzky and Bernholz (1987).

Caplan (2001, p. 22) again accurately quotes me (Block, 1999, p. 35) to the effect that it is a defect of public goods theory that there “exists no unambiguous way to measure the costs of benefits” of government expenditures such as on public education. He (2002, pp. 22-23) denies this: “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.” If this is truly “realism,” then count me amongst those opposed to reality. But it is no such thing. No one on the praxeological side of this debate advocates throwing out the baby with the bathwater: But in this case, there is only the latter. If there is no unambiguous way to measure these things, not least because of the lack of valid interpersonal comparisons of utility, then there simply is no more probable or less probable way to do this either. Who, in any case, is so intent to measure these unmeasurables: interventionists, or free enterprisers?

To match Caplan’s (2002, p. 23) ending on a positive note, I, too, am willing to acknowledge the splendid contributions on the part of neoclassical economists to the study of such things as airline deregulation, price controls, minimum wages, rent control, free trade<sup>32</sup>, etc. On the other hand, I deny there can be any such thing as “tedious repetition of what Mises or Rothbard said” (Caplan, 2002, p. 24, ft. 21). As Gary Becker once said to me in another context, “You can’t have too much of the truth.” Nor can I deny the truth of Caplan’s (2002, p. 24) assertion that “... most neoclassicism is bad economics, but most good economics is still neoclassical.” However, the reason for this is not that complementary to the mainstream; when they outnumber us more than 1000 to 1, it should occasion little surprise that they also do more good work, in total.

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<sup>32</sup> Milton Friedman once said (paraphrase), “Thanks to the efforts of the entire profession of economics, tariffs are probably .01% lower than they would otherwise be. But because of that, we have contributed to GDP an

Caplan (2001, p. 24) invites Austrians to throw down our differences, join the neoclassicals, and “help shift the balance” toward good economics. In return, I suggest he get in touch with his inner economist, and return to the Austrianism of his not at all misspent youth. If he does, the percentage increase of praxeologists he alone embodies will vastly outstrip the loss to the mainstream, again in percentage terms.

Finally, I invite Caplan to reply again to this missive, to “keep the conversation going” as some Austrian non-praxeological commentators are wont to put it. I don’t think we have come anywhere near a meeting of the minds. But since, in my view, this is due in large part to Caplan’s failure to respond to specific criticisms, I encourage him to be more thorough in this regard.

One last note, a disquieting one. Caplan’s (2002, p. 20) treatment of Hoppe’s (1989) argument from argument, or “argumentation ethics” is nothing less than a scandal. It is one thing to criticize a perspective by giving reasons that tend to undermine it. It is quite another to reject solely on the grounds that others have done so. The technical name for such discourse is the informal fallacy of argument from authority. Hoppe can of course deal with critics of his argument, and has done so. It is not for me to get into the specifics on this. But I must protest Caplan’s very un-intellectual attitude, even an anti intellectual one.

Caplan (2001, p. 20), however, does make one substantive point: Even if it is true that this is all that Hoppe accomplishes, what is so wrong with literally undermining the intellectual ground from which statist launch their attacks on economic freedom?

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amount most likely 100 times our total salaries. This certainly made me proud to be an economist, even in tandem with, gulp, neo-classicals.

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