REALISM: AUSTRIAN VS. NEOCLASSICAL ECONOMICS, REPLY TO CAPLAN

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This Reply is part four of an ongoing debate that first began with Caplan (1999). Part two can be found in Block (1999), and part three in Caplan (2001)[1].

Overall, the debate concerns the issue of whether the Austrian or the neoclassical vision more closely approaches the truth in economics, with regard to such issues as methodology, indifference, envy, *verschtehen*, continuity, demonstrated preference, welfare economics, public goods, and cardinality.[2]

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[1]There is a parallel debate to this one over many of the same issues taking place between Caplan and Hülsmann. It consists of first, Caplan (1999), second, Hülsmann (1999), third Caplan (2001) and fourth, Hülsmann (2002). I shall for the most part be focusing on Caplan's (2001) criticisms of Block (1999), tending to leave those parts of Caplan (2001) that criticize Hülsmann (1999) for the latter author's response. Another part of this debate is Barnett and Block (unpublished ms.)

[2]It is interesting to note that Caplan (1999) appeared in *Southern Economic Journal*. Block (1999) and Hülsmann (1999) were originally submitted to the *SEJ*, as befits the usual practice of such rejoinders being featured in the journal in which the "first shot" was fired. Only when these two papers were rejected by that journal did they appear in the *Quarterly Journal of Austrian Economics*. Even more curious, Caplan (2001) was published not by the *SEJ*, but by the *QJAE*. The debate between Caplan on the one side and myself and Hülsmann on the other is of course between representatives of neoclassical and Austrian economics respectively. The *SEJ* is a vehicle for the former position and the *QJAE*, as its name implies, for the latter. What conclusions can be drawn from this state of affairs? One possibility is that the praxeological school is interested in, and willing to debate issues that separate the two perspectives and the neoclassical (at least as represented by *SEJ*) is not. Another is that from the mainstream point of view, a critique of Austrianism is barely worth mentioning, and certainly not worth continuing, hence the acceptance of Caplan (1999) in *SEJ*, but the rejection by this journal of Block (1999), Hülsmann (1999), and even of Caplan (2001).

The present article is limited to replying to Section 2: "Probability," of Caplan (2002).[3]

Before beginning my substantive reply, which will be entirely critical, let me say at the outset that I very much appreciate Caplan's willingness to engage with Austrians in intellectual debate, and, even more, the quality of his commentary. All too often the response of the neoclassical economists, of which Caplan is a splendid example, is merely to ignore criticism emanating from praxeologists. As is only human nature, I vastly prefer the former to the latter. But I go further. Caplan is unique amongst mainstream members of the profession in that he is actually knowledgeable about that which he criticizes. Further, it is marvelous to be taken to task for what one has actually said, rather than for what is falsely attributed to an author. In Block (1999) I complimented Caplan (1999) by comparing him favorably to Nozick (1977),[4] as a critique of Austrianism extremely well informed about its subject matter. Nothing in Caplan (2002) has induced me to change my assessment in this regard; if anything, it is only strengthened.

PROBABILITY

For Caplan (2002, p. 1) "reading my critics convinces me that the issue of probability permeates every aspect of this discussion." For me, the last refuge of the neoclassical economist lies in probability, at least insofar as concerns the use of it being made by Caplan. For while this author may not realize it, he is using probability as a stick to beat up on the Austrian concept of the synthetic *a priori*–without directly confronting it. He states (2002, p. 2; emphasis added), "On topic after topic, Block and Hülsmann make extreme claims about."[5]

I can see why a non-Austrian would interpret matters this way, but this is not precisely correct. As a "friendly amendment" to his statement, I would substitute "On topic after topic, Block and Hülsmann make apodictic, logically necessary, or synthetic *a priori* claims about."[6]

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[3]For a rejoinder to Caplan (2001) in its entirety, see Barnett (unpublished ms.) and Block (unpublished ms.).

[4]For a rejoinder to that article, see Block (1980).

[5]This sentence ends: "about how little people are able to know." This, in my opinion, is relatively unimportant. The real issue between Caplan and myself does not revolve upon what we claim people are able to know, a lot or a little. Rather, the debate focuses on the certainty, or the probability, on the basis of which this knowledge is held.

[6]This emphasis on the apodictic nature of Misesian (1957, 1998) economics should not be interpreted as some sort of grandiosity, or immodesty. It is not at all the case that praxeologists cannot make mistakes. There are errors of logic, errors of fact, errors of understanding, etc. Apodictic argumentation is no more free of the possibility of error than is mathematical or geometrical reasoning. Friedman (1953) claims that if two practitioners of apodictic discourse disagree, since they have no empirical experiments to help

There is no way that all the talk in the world about "probability" can breach this fortress indirectly, as Caplan attempts to do. If it is to be overturned, it must be done directly, and this neoclassicists, particularly the best of them such as Caplan himself, are congenitally unable to do, since, as I shall attempt to show, to the extent they are competent economists at all of any stripe or variety, they must themselves adhere to these very "extreme" principles.

For an audience of praxeologists, the main readers of a journal such as *QJAE*, there is little more that need be said. But for the sake of those unschooled in this tradition, I need to elaborate[7].

What, then, is a synthetic *a priori* statement?[8] This can best be explained with the aid of Table 1. The column headings indicate ways of learning about the world. We learn *a posteriori* through experience. In contrast, we learn *a priori* through understanding; that is, as soon as we grasp the meanings of the words involved, we immediately seize upon the truth (or falsity) of a statement comprised of them. The row headings indicate the status of sentences; e.g., their "realism," or agreement with physical reality about which Caplan is so concerned. Analytic statements are true by definition, and have no application to the real world; they indicate merely how we choose to use words. For example, bachelors are unmarried men; or "elephants" are big gray creatures with tusks and a funny looking nose; or the bischon frise is a dog. Synthetic claims are specifically about the real world; e.g., I am now wearing a red shirt, water runs downhill. There is no debate concerning the status of three of the four boxes found in this two by two matrix. That is, no one doubts that there is such a thing as A, B, or C can exist.

Table 1

	A posteriori	A priori
Analytic	A	B
Synthetic	C	D

Take *A* first. Although there is some question about this in philosophical circles,[9] one way to characterize the elements in this set is in terms of the ways

them determine who is right, they can only fight, or at best fail to come to any conclusion. But this is precisely the same position occupied by two mathematicians who disagree with one another, or two different theorists of geometry. Needless to say, no such conclusion applies in either of those cases, nor would anyone assert they would. Why only for praxeology?

[7]For further reading on these matters, see Hoppe (1988, 1992, 1997), Rothbard (1957, 1976, 1993, 1997a, 1997b), Mises (1957, 1999), Selgin (1988), Fox (1992), and Rizzo (1979).

[8]In preparing this section, I had the help of William H. Friedman, Michael Levin, and Jan Narveson. The usual caveats apply.

[9]The objection can be put as follows: only an ignoramus would even try to verify an analytically true statement by observation. Someone could actually undertake a survey of the men in his city to determine whether any of its bachelors were married, and if he

in which we come to learn language. For example, "We shall use language in such a way that 'rouge' in French will translate to 'red' in English." We learn this through experience, but to say "Rouge equals red" is an analytic statement. For *A*, the analytic *a posteriori*, we can learn each of the words in this sentence through experience (in an *a posteriori* manner), and yet the meaning of it is tautological.

Now consider *B*. An example is "Bachelors are unmarried men." Another is that the bischon frise is a dog. These analytic *a priori* sentences are necessarily true, once we know the meaning of each of these words; their denial involves one in a self-contradiction. But it says nothing about the real world. Once we grasp the meanings of the words, we understand the statement to be necessarily true, without any further resort to experience.

Likewise, *C* is also unobjectionable: People can learn I am now writing on this paper and wearing a red shirt by observation. Or that water flows downhill. Or that dogs like to chase squirrels. Statements in this category fall in to the scientific realm of empirical studies, and also into the common sense world of observation.[10]

It is only *D*, the synthetic *a priori*, which presents any real controversy. For the claim, here, is that the statement is logically undeniable, and yet does not arise (only) from experience. Not being based on empirical observation, articulations of this sort cannot be *refuted* by appeal to evidence; they are true apodictically, by their very nature, and yet refer, and intimately so, to the real world. Consider some examples of noneconomic statements that are logically necessary, cannot be denied except upon pain of self-contradiction, and yet explain and help us understand the workings of the universe:

- Nothing is uniformly red and uniformly green all over its surface;
- Everything is getting older;
- The angles of a (Euclidian) triangle sum to 180 degrees;
- The Pythagorean theorem: $a^2 + b^2 = c^2$, where a and b are the sides of a rectangular-triangle, and c is the hypotenuse;
- I now exist.

These are very general statements about the universe. Nevertheless it is impossible to even imagine them to be false. Also, it cannot be denied that they are about the real world, and crucial to our understanding of it.

Now take a few cases from the realm of economics:

- Rent control, *ceteris paribus*, leads to less investment in residential rental housing;

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couldn't find any that were then perhaps he could be said to know an analytic truth *a posteriori*. But the question would be: how did he identify the bachelors in the first place without reference to their marital status? I owe this point to Michael Levin.

[10]Http://www.utm.edu/research/iep/a/a priori.htm.

- The minimum wage law causes unemployment of unskilled workers, other things equal;
- There are mutual benefits from trade in the *ex ante* sense;
- Man acts (to create a world more to his liking in the future than the one which would arise but for his action);
- Demand curves are downward sloping (e.g., when forced to give up one unit of a stock of a good, the economic actor will give up the least valuable of them).

With regard to the contention over the status of the synthetic *a priori*, there are two schools of thought. One, the Austrian, based primarily on the writings of Mises, Rothbard, and Hoppe, accepts this insight, and not only with alacrity, but places it at the very basis of economic science. The other, the Vienna School, or logical positivists,[11] from which all neoclassical economists such as Caplan intellectually descend, rejects it, and with equal fervor.[12]

The view of the latter perspective may be summarized by the following, call it *E*: "A claim *either* applies to the real world, in which case it is at least in principle falsifiable on the basis of evidence, *or* if it is not falsifiable on the basis of evidence, then it *cannot* be applicable to the real world." But this perspective is internally self-contradictory.[13] The problem is, where does *E* fit into the categories depicted in Table 1? How can the statement, *E*, itself, be reconciled with its own substance? If *E* is merely an empirical claim, then it can only be known on a probabilistic or tentative basis; and if so, why all the certainty with which it is maintained by mainstream economists? What happened to the "probability" about which Caplan places such great reliance? On the other hand, if *E* is necessarily true, according to those who articulate it, then it *cannot* apply to the real world, and must be dismissed as merely giving us conventions about word usage. Either way, mainstream methodology is embedded in quicksand.

Caplan attempts to short-circuit these logical positivist difficulties with his emphasis on "probability." He cannot be allowed to get away with such a ploy. This way of looking at the world is fatally mired in the tired shop worn dogma of the Vienna School. My claim is that even Caplan himself cannot consistently maintain his own attempt to evade the primordial fact of the synthetic *a priori*. Consider two cases in point.

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[11]Schlick (1985), Carnap (1967), Friedman (1953), Harris and Severens (1970), Pap (1958), Quine (1980), Baillie (1996), Dummett (1996), Stroll (2000).

[12]Caplan (2001, n. 1) is very insightful in distinguishing Misesian Austrians (e.g., quintessentially Mises, Rothbard, and I would add Hoppe) from the non-Misesian ones, and of placing Hülsmann and myself in the former camp. In fact, based upon this one single crucial distinction, there is serious question as to the application of the honorific "Austrian" for the so-called non-Misesian members of this school.

[13]See Hoppe (1988, 1992, 1997).

First, the recent brou-ha-ha about the minimum wage law, created by the publication of Card and Krueger (1994). These two worthies purport to show that contrary to most empirical findings of economists, a raise in the minimum wage, at least insofar as Pennsylvania and New Jersey at the close of the twentieth century were concerned, does not lead to unemployment of unskilled workers but instead to more jobs for them at higher wages. Now if mainstream economists were to have followed their self-announced positivistic methodology, they would have reacted to this "finding" in terms such as these: "Oh, well, isn't it interesting; economic law works differently in Pennsylvania and New Jersey at the close of the twentieth century," or "Oh, well, isn't it interesting; economic law works not at all in Pennsylvania and New Jersey at the close of the twentieth century," or, to utilize Caplan's probabilistic language,

> It is only *probable* that economic law would work in the same manner in Pennsylvania and New Jersey at the close of the 20th century as it does everywhere else and at all other times; Card and Krueger (CK) have shown, however, that what is merely probable did not work out as expected at least in this one instance.

But this is not at all the reaction of many of the best economist critics of CK. Instead, they came out swinging, with real fire in their eyes.[14] They cast aspersions on Card and Krueger's sampling technique, on their empirical work, on their calculations, on just about every aspect of their study. Hell or high water, they were determined to show the error of their ways to CK. In other words, to use Caplanian language, they made "extreme" claims. Nor was this merely a disinterested attempt on the part of these economists to check and see if CK were right or wrong; a fact-finding mission, so to speak, let the chips fall where they may. No. They *knew* that CK were in error, grave error, and they were going to prove it.

I asked Caplan if he had ever written on this topic,[15] and he said that while he had not formally published anything on the minimum wage, there was some material on it at his web page,[16] to which he referred me.

Let us consider some passages from Caplan's classroom teaching notes.[17] Note the definitiveness of Caplan's views on the minimum wage. No "probabilistic" talk here. Specifically, he states:

- At the equilibrium wage, there are neither labor shortages nor surpluses;

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[14]Becker (1995), Burkhauser, Couch, and Wittenburg (1996), Deere, Murphy, and Welch (1995), Gallaway and Adie (1995), Neumark and Wascher (1992).

[15]Via email, in June 2001.

[16]Http://www.gmu.edu/departments/economics/bcaplan/e321/lab2.htm.

[17]Apart from the fact that they are extremely informative, and that I wish I were even half as well organized in my teaching as he.

- unemployment is *voluntary*;
- how is *involuntary* unemployment possible? *Only if the prevailing wage is too high!*
- This is no different from any other surplus good;
- There is no case where workers are *both* "underworked" and "underpaid";
- The general solution to all involuntary unemployment boils down to: reduce the market wage until the surplus disappears;
- Suppose the equilibrium wage is $10/hr. If the government imposes a minimum wage of $15/hr., there will be unemployment;
- Simple question for proponents: Why not $1,000,000/hour?

One possible explanation for the unambiguous[18] nature of his language is that this is an undergraduate course, and it doesn't do too well to introduce all sorts of qualifications[19] in an introductory treatment of the subject. But I do not think this fully accounts for his stance on these issues. Query for Caplan: suppose one of his students took him up on the minimum wage of $1million/hour. Would he say it is very *likely* this would cause unemployment, or that it is undeniable that it would? I very strongly suspect the latter.[20]

Caplan's probabilistic methodology is incompatible with his strong support of free enterprise. There are two different perspectives on the latter, call them principled and empirically based. The former favors free markets as a matter of principle, apart from empirical findings; the latter favors free markets if and only when empirical findings support them. As an economist, Caplan happily seems to belong to the former. As a methodologist, unfortunately, to the latter.

Let me give another example from my own early professional life. I was a graduate student at Columbia University in the late 1960s, working on my Ph.D. dissertation on rent control. I was attempting to empirically demonstrate that this law causally brings about less investment in rental house building, greater vacancies, reduced labor mobility, etc. Most of the time, my econometric results yielded the correct sign for the independent variables (e.g.,

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[18]The one possible counterexample to my claim is Caplan's statement: "This simple application of S&D runs contrary to almost all popular beliefs about labor. But there can be little doubt that it is correct." My interpretation of "little doubt" in this context, however, is that this is a rhetorical flourish, not a serious attempt to bring probability into the analysis.

[19]For example, he does not mention and then demolish the monopsonistic argument in favor of minimum wages, which I am sure he would do in a graduate course. For a critique of this argument, see Block and Barnett (unpublished ms.).

[20]I certainly hope that the latter is true; in my view, Caplan's very status as an economist rides on this issue. I have a moral certainty that were this gifted economist asked about this issue in any other context, he would come through with flying colors, as an "extremist."

indicating the correctness of the usual economic analysis of this legislation). But every once in a while I generated the "wrong" signs; e.g., my equations zigged when they should have zagged. Did my faculty advisors congratulate me on these results? Did they brag about the fact that their young genius student Block was going to turn the world of economics upside down with his great and wondrous findings? That rent control, *mirible dictu*, could actually help poor tenants? Not a bit of it. Instead, they said something to the effect of: "Block, you dummy, get the hell out of my office and go run these regressions again until you get them right." But if the basic supply and demand analysis of rental housing is only "probable," then my (erroneous) results should have had as much standing as those of anyone else, including my own "correct" findings. My faculty advisors were good economists, happily, otherwise they would have accepted my erroneous findings without demur. Or, at the very worst, tolerated them as correct, but "improbable." To say the least, that was not their reaction to them at all.

How does Caplan stand on rent control? Again, quoting from his web page,[21] he gives the standard analysis:

C. The short-run effect: Admittedly, the physical stock of housing doesn't decline much, but quantity demanded rises substantially. The result: rationing.

D. Standard form of rationing: landlords retain their pick of tenants, but tenants get to stay as long as they keep paying the regulated rent.

E. The long-run effect: Housing supply gets much more elastic. New construction falls off, as does maintainence (sic). Shortage gets more and more aggravated over time.

F. Hidden margins: While the physical housing stock takes time to deteriorate, there are numerous ways for landlords to respond more rapidly. General strategy: cut quality. With a shortage of housing, you will still be able to get tenants.

G. Examples:

    1. Eliminate free services.
    2. Charge extra for furniture.
    3. Cut back on safety and maintainence (sic).

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[21]Http://www.gmu.edu/departments/economics/bcaplan/e103/micro5.htm.

Again, we find no probabilistic stipulations. Rather, in a straightforward manner, he depicts the standard analysis of rent control. Thus we see that Caplan, too, since he is a highly competent economist, approaches issues such as the minimum wage and rent control in precisely this synthetic *a priori* manner. But if so, then this would undermine the positivistic claims made in the section of his paper on probability.

One related instance arises in Caplan's (2001, p. 4) unwarranted attack on Mises for maintaining that there *are* "exceptionless causal laws." But there are "exceptionless causal laws" in economics: every synthetic *a priori* statement can serve as an instance. Caplan believes, in contrast, that "We merely require beliefs about conditional probabilities." But is it only "conditionally probable" that minimum wage laws unemploy workers with marginal revenue productivity below the levels mandated in such legislation? That rent control leads to housing disarray? That trade is mutually beneficial in the *ex ante* sense? Even Caplan himself does not really believe this, let alone numerous other neoclassicals who have not allowed their mistaken methodology to get in the way of their more accurate economic practice. Caplan refuses to see economics in any other way than merely probable. As methodologist, he refuses to acknowledge economic law; but as economist, as we have seen, he transcends this.

Let me put this point on probability in an entirely different way. Consider Table 2.

Table 2

	Low probability	High Probability
C (empirical statements)	I	II
D (synthetic *a prioris*)	III	IV

Caplan maintains that all is probability in economics. He taxes Hülsmann (1999) and myself (Block 1999) with ignoring this "insight." Well and good. Let us take him at his word. With the aid of Table 2, we can accommodate his concerns within the Austrian nexus. The columns indicate high and low probability, the rows distinguish between empirical statements and synthetic *a priori* ones. Caplan wants to separate the columns, or to stress the differences lying in this dimension,[22] at the expense of that regarding the rows. The Austrians, in contrast, typically place much more emphasis on the latter than the former, but are perfectly able to accommodate both.

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[22]As depicted, probability admits of only two levels: high and low, for ease of exposition. This could easily be changed into a continuous variable, starting with zero probability on the left and ending with 100 percent certainty on the right.

Let us furnish some examples so as to flesh out this table.

1. An example of a low probability empirical statement is that all elephants wear red suspenders or that the price elasticity of demand for bananas in New Orleans in 1991 was exactly minus two.

2. A high probability empirical statement is that elephants are very heavy, and that the price elasticity of demand for bananas in New Orleans in 1991 was somewhere between minus .001 and minus 100.

It is easy to see how we can incorporate probability into empirical statements: they are intrinsically probabilistic in the first place. Doing so for synthetic *a prioris* will be a bit more of a challenge. But this is only because they have been mischaracterized as being certain.[23] This is not so. Rather, a synthetic *a priori* statement is of a different logical type than an empirical one.

3. and 4. A relatively highly complex and therefore low probability synthetic *a priori* statement is the pythagorean theorem, to the effect that the sum of the squares of the sides of a right triangle equal the square of the hypotenuse. A higher probability and less complex statement is that all triangles have 180 degrees. An even simpler and thus yet higher probability one is that all triangles have three sides. Why the variation in probability? It is because the earlier the sentence of these three, the more steps that must be taken in the logical proof, and the more (and more complicated) the steps, the greater the opportunity for human error; the later, the simpler, and the fewer steps in the argument. We might misunderstand words. We might make an erroneous deduction from one step in the argument to another. The more elementary, short, simple and basic the statement, the less likelihood of such error.

Now consider some economic postulates, all of them synthethic *a priori*, and therefore necessarily true on the assumption we understand and deduce correctly. Some simple high probability short statements are: I exist; I act. Some intermediate level ones are: minimum wages lead to unemployment; rent controls promote housing shortages; and trade is mutually beneficial in the *ex ante* sense. An example of a complex, many stepped (and therefore in the terminology we are currently employing) "less probable" but still apodictic argument is the Austrian business cycle theory or its money regression theorem.[24]

The problem I have with Caplan's (2001) remarks on probability is that he uses them to in effect deny the existence of the rows. But this need not be the

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[23]Praxeologists have been interpreted by critics as intellectually arrogant. For a reply to this charge, see Hoppe (1998).

[24]Lest I be misunderstood, I am saying with regard to the Pythagorean theorem, and Austrian business cycle theory, etc., that since they are comprised of relatively long chains of reasoning, there is more room for human or psychological error in deducing their conclusions. They are thus only less probable in this very restricted sense. I am not saying that they are any less apodictic than less complex synthetic *a priori* chains of reasoning.

case at all. That is, just because there is a valid distinction to be made between high and low probability, on the one had, it does not mean that there is none to be drawn between claims about the world based upon empirical evidence and those based on apodictic reasoning, on the other.

To Caplan on probability I offer the following rejoinder: Say if you will that probability is all-pervasive; that nothing exists without probability. But this will avail you nothing in your attempt to obliterate the primordial distinction between an empirical claim and one that attains the status of the apodictic. For synthetic *a priori* statements, too, if only in the sense mentioned above, can partake of degrees of probability.

Moreover, if it is only "probable" that the minimum wage causes unemployment, and that rent control leads to housing shortages, then when these laws are enacted, *sometimes* they have these effects, and *sometimes* they do not. But this hardly accounts for the reaction of the mainstream economist to Card and Krueger's "discovery" that the minimum wage had no such deleterious effects in their "study." It hardly accounts for the reaction of my dissertation advisors to my detection of the opposite from expected effects of rent control. It hardly accounts for Caplan's own nonprobabilistic language concerning these attempts to fix prices on his own website. No, Caplan cannot have it both ways. If he wishes to stick to his story on probability, then he must jettison his certainty about shortages and surpluses in supply and demand (and much, much more in economics). If he wishes to continue amongst the ranks of economists who see clearly on such subjects, he must renounce his methodological views on probability.

Caplan (2001, p. 2, n. 2) upbraids me for mentioning slightly inaccurate roulette wheels: "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!"

First, these are not unique events; rather, the opposite.[25] Second, and more important, Caplan confuses a difference in degree with a difference in kind. Class probability involves endlessly repeatable non-unique events, such as games of chance, cards, dice, coins, roulette, bingo, etc. With regard to them, we do have probability distributions (we can precisely quantify the likelihood of a seven or snake eyes in dice games, or of five heads in a row in coin flipping). Case probability concerns nonrepeatable unique events, about which we have no information about probability distributions. Indeed, such a concept, when applied to such things as elections, or sales of a good, is logically incoherent.

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[25] I am here implicitly abstracting from phenomena that render this false; e.g., every time the wheel spins, the temperature in the room, and the air pressure, are slightly different.

Now it is true that dice tosses are not *exactly* "endlessly repeatable non-unique events." There is temperature, air pressure, the fact that if the dice are used thousands of times there might be differential wear on their sides, particularly if they were not perfectly balanced at the outset. There is always the possibility of cheating. On the other hand, surely we have some information about future presidential elections or sales.

Because of this lack of perfection in the distinction between these two types of probability, Caplan wishes to deny that there is difference in kind to be drawn. Not so. This can be shown by the fact that it is at least possible to perceive a state of the world where dice and coins are balanced perfectly. If so, then we can deduce probability distributions with mathematical precision. But due to that little inconvenient matter of free will, it is not possible to even contemplate any such analogous situation when confronted with issues based on human choice.

Caplan (2002, p. 6) calls into question Hoppe's (1997, p. 56) claim that it is impossible "to give an exhaustive classification of all possible actions." He offers a "simple reply: one can always complete an enumeration by adding a catch-all 'other' category." But this goes far too quickly. The "other" category does not at all satisfy Hoppe's proper demand for a full enumeration. Rather, it is merely a name for our ignorance. It does not at all contribute to the exhaustiveness, the sum total, or our knowledge. Instead, it concedes our ignorance.

Caplan (2002, p. 8) condemns my statement (Block 1999, p. 30) "the emergence of new ideas necessarily overthrows all previous patterns of behavior." With great forbearance, he castigates this as a "wild exaggeration." I thank him for pointing out this error of mine, but I think it only just to go further than he in his condemnation of me. I do not regard this statement as it appears merely as a "wild exaggeration." It seems to me that this is no less than lunacy.[26]

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[26]Where oh where was Caplan when I needed a copy editor for Block 1999? I apologize for this typographical error.

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