

Mainstream Economics is not Scientific

I. Introduction.

Precision¹ and consistency are two “sins” of which economics, unfortunately, is not guilty. Science requires, *inter alia*, precision and consistency with respect to its fundamental concepts. One problem of long standing that sets economics apart from the natural sciences is the equivocal and inconsistent use by economists of expressions intended to convey a technical meaning. This, at least ideally, is capable of being changed, and this paper in part is dedicated to helping start up that process. This issue is addressed in section II of our paper. There is also a second problem, at least if “becoming a science” means becoming *empirically* oriented. And that is because the type of economics we advocate, Austrian economics, explicitly rejects the empirical model of such hard sciences as physics, chemistry and biology. This is thus an intractable problem, if we are obligated to remain under the baleful influence

¹This refers to the true precision of scientific language in contradistinction to the spurious precision of incorrectly used mathematics and statistics.

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of the methodological worldview of the natural sciences. But there is no reason we should be. In section III we made the case for an alternative methodological perspective. We conclude in section IV.

II. Consistency and Precision.

Consider how a physicist, P, might use the expressions “energy” and “momentum.” First, when not acting *qua* physicist, he might use them non-technically. For example, in discussing the campaign of some supplicant, S, for political office, P might say, “S’s campaign has a lot of energy,” or “S’s campaign has momentum,” intending the same meaning in both cases. Both statements would convey P’s intended meaning, within the limits of the natural imprecision necessarily involved in all such matters. The non-technical and interchangeable use of these terms would be taken as just that – “energy” and “momentum” would not be understood as being used in a scientific sense, each with a specific technical meaning, and they would be taken to intend the same ordinary-language message.²

Second, consider P when acting *qua*

²Block (2002, fn. 48) refers to “... a perfectly rational concept in ordinary language, but not in technical economics.” For more on this point see Block (1980, 1999).

physicist. For example during a lecture on classical mechanics in an introductory level class, he might discuss the results of an experiment in terms of the (kinetic) energy and (linear) momentum of a body under particular conditions. Certainly, in such a case P would use the expressions “energy” and “momentum” in their technical senses: energy, a scalar with the SI unit,³ joule (= kilogram•meter²/second²), would be given by half the product of the mass and the square of the speed of the body; and the momentum, a vector with SI unit, newton•second (= kilogram•meter/second), would be given by the product of the mass and the velocity. P would use these expressions precisely and consistently, and, as they refer to very different concepts, he would not use them interchangeably. Moreover, neither would the specific technical meanings intended by P when using “energy” and “momentum,” nor their precise and consistent use by P, distinguish him from any other competent physicist. Every competent physicist, acting qua physicist, would intend the identical meaning for “energy” and also for “momentum” as does P. And, as would P, they would use these expressions precisely and consistently.

Now consider how an economist, acting qua economist, might use the term “monopoly,” for example, during a lecture on microeconomics in an intermediate level class, assuming he wishes to be consistent with the assigned textbook.⁴ Depending upon the book used (see Table 1), students are presented with different concepts of “monopoly.”

³For more on the SI units go to <http://physics.nist.gov/cuu/Units/index.html>.

⁴We choose textbooks to illustrate these problems, rather than articles in peer-reviewed journals, since the former, not the latter, are the repository of knowledge in a given field.

Table 1 — Uses of the term “Monopoly”

Frank (1991), p. 370	The case of a market served by a single seller with no close substitutes.
Mansfield and Yohe (2004), p. 356	A monopoly exists whenever there is a single source of supply.
Nicholson (2005), p. 651	An industry in which there is only a single seller of a good.
Varian (2006), p. 12	A situation where a market is dominated by a single seller of a product.
Varian (2006), p. 423	An industry structure where there is only <i>one</i> firm in the industry.

These definitions are not identical in meaning. The definition of Mansfield and Yohe ignores the issue of substitutes, whether close or not, that is essential to Frank’s. And, unless the term “good” implies that there is no close substitute for any specific good, Nicholson’s definition is not the same as Frank’s. Then, Varian’s first definition differs from his second. Moreover, his first clashes with Frank’s, Mansfield and Yohe’s, and Nicholson’s, because a market “dominated” by one firm implies the possibility that other, non-dominant firm’s exist in the market, contrary to the others’ claim of a “single” seller/source-of-supply/product.

Furthermore, the concept of “close substitute” is, to say the least, highly imprecise. These definitions are obviously inconsistent and imprecise, characteristics

that are, to say the least, undesirable for scientific work.

Or consider how the same economist, acting qua economist, might use the term “investment,” for example, during a lecture on macroeconomics in an intermediate level class, again assuming he wishes to be consistent with the assigned textbook (see Table 2). Again, depending upon the book used, students are presented with different concepts of the relevant term, and again, these definitions are not identical in meaning. Blanchard conflicts with Abel and Bernanke in that he excludes firms’ holdings of inventories. DeLong and Olney include government creation of infrastructure (“sometimes”), which Abel and Bernanke and also Blanchard do not include. And then there is Mankiw who apparently includes *all* durable goods, including consumer durables, as it is indubitable that they are “bought for future use.” Moreover, Mankiw’s second definition conflicts with his first, unless individuals’ stocks of consumers’ goods are considered to be part of their stocks of capital goods, an unusual position for mainstream macroeconomics,⁵ and one which is belied by his definition of capital: “The stock of equipment and structures used in production” (Mankiw, 1992, 504). That these definitions are inconsistent and imprecise is clearly evident. Again, consistency and precision, hallmarks of scientific work, are absent.

Such examples are but the tip of the iceberg. Mankiw (1992, 504), again, provides two different definitions of the same term: “*Capital*: 1. The stock of equipment

⁵The authors take the position that investment may be classified as either investment in new capital goods (i.e., means of production) and/or investment in new consumers’ durable goods.

Table 2 — Uses of the term “Investment”

Abel and Bernanke (1992), p. 733	Spending for new capital goods, called fixed investment, and increases in firms’ inventory holdings, called inventory investment. (Elsewhere [p. 34] they state that residential construction is included in fixed investment along with business structures and equipment.)
Blanchard (2006), p. G-5	Purchases of new houses and apartments by people, and purchases of new capital goods (machines and plants) by firms.
DeLong and Olney (2006), p. 528	The buildings and goods (both machines and inventories) purchased to add to the economy’s stock of capital, plus (sometimes) government creation of infrastructure, plus residential construction.
Mankiw (1992), p. 26	Investment consists of goods bought for future use.
Mankiw (1992), p. 507	Goods purchased by individuals and firms to add to their stock [<i>sic</i>] of capital.

and structures used in production. 2. The funds to finance the accumulation of equipment and structures.” Moreover, the latter is not exactly a paragon of clarity as, when used in finance, “funds” refers to money whereas capital is used to refer not only to money, but also to various other financial assets (e.g. stocks and bonds).

Any competent economist can, in short order, think of a substantial number of other examples of such inconsistent and imprecise definitions of terms important for scientific work in economics. Until and unless a protracted effort is made by the entire profession to root out such practices, the honorific “scientific” will continue to elude us, and rightly so. Nor is this an ideological point we are making. We here only insist on consistency and precision. This is not itself a substantive issue; rather, it is the precondition for all of our scientific endeavors.

III. Methodology.

Economics is not a science, at least not on the model of the natural sciences, for other reasons than the imprecision of language.⁶ In addition, it is because while physics, chemistry, biology, etc. are indeed empirical sciences, in sharp contrast, economics, at least as practiced by the mainstream, mistakenly attempts to copy them in this regard.^{7,8}

⁶Lest there be any misunderstanding, we do *not* claim that Austrian economics is free of inconsistency and imprecision of language.

⁷Here is a statement written by the second author of this paper: “Gary Becker was my dissertation advisor at Columbia University. I was awarded the Ph.D. degree in 1972. My thesis topic was rent control. I was attempting to demonstrate that this law was associated with various indices of housing malfunction (abandonment, poor quality, etc.), holding constant variables such as income, wealth, unemployment, weather, etc. Most of the time I could show the proper signs on my rent control variable (my observations were cities, and this variable was based on the number of years a city had controlled rents), and often with significant *t* values. However, every once in a while, playing around with different combinations of dependent and

independent variables, I would generate the *wrong* sign for my rent control variable, and, even more embarrassing, sometimes it was statistically significant. Did Gary say ‘Hey, I’ve got this genius student Walter Block who will now overturn everything we economists think we know about rent control?’ He did not. Instead, he said something to me that sounded like ‘Block, you moron, run these regressions again until you get it right.’ (Actually he was always far more polite, but that is the way his criticisms sounded to me at the time.) So, what was ‘testing’ what, in this exercise? Were my equations testing the traditional microeconomic analysis of rent control, according to which demand exceeds supply, creating shortages? Of course not. It was *entirely* the other way around: we *knew* in advance what proper econometric results would look like. Theory, instead, was testing my statistical acuity.”

⁸In addition to rent control, the minimum wage serves as evidence that while highly competent neoclassical economists may “talk” logical positivism, their “walk” is praxeological. To put this in other words, if you scratch a good mainstream dismal scientist, you will find a praxeologist. When two high profile practitioners claimed that the minimum wage law did not create additional unemployment amongst the unskilled (Krueger, 1993; Card and Krueger, 1994) what any self-respecting logical positivist *should have said* was something along the lines of “Well, maybe *sometimes* this is true,” or “Well, *maybe* economic law works differently in New Jersey and Pennsylvania” (Card and Krueger used data from these jurisdictions). After all, for the logical positivist, the evidence is the dog, and the theory is only the tail. If, for example, 95% of empirical research suggests that this type of legislation costs jobs for the unskilled, and 5% does not, well, then we must conclude that this is true of 95% of the cases, and false for 5% of them. Instead, when *mainstream* economists (Becker, 1995; Burkhauser, Couch and Wittenburg, 1996; Deere, Murphy and Welch, 1995; Adie and Gallaway, 1995; Sowell,

However, economics is really a branch of logic, along with mathematics and symbolic logic. We are here defending the philosophy of methodological dualism: the claim that different methods are appropriate for the physical sciences, on the one hand, and for economics, on the other.⁹

Why should there be such a disparity between the two different kinds of science? The primordial reason is that human beings, the subject of economics, have free will, while molecules, cells, and particles, the subjects of the physical sciences, do not.¹⁰ There are thus no constants in the former, while the latter is replete with them.

The mainstream neoclassical tradition in economics is squarely based on logical positivism.¹¹ This means that only mean-

ingful (that is, falsifiable) hypotheses can be entertained by the dismal science, and their truth can only be tentative. In this perspective, there is no such thing as apodictic knowledge in economics. If a proposition *can* be known with absolute certainty, then it is a tautology which tells, merely, how we have chosen to use words, and offers no insights about the real world. For example, “bachelors are unmarried males” or “ $1 + 1 = 2$.” On the other hand, if a claim is to be non-tautological, that is, having to do with reality, then it can be accepted only provisionally, for as long as evidence in its behalf can be adduced.

Perhaps the most accomplished (or at least most famous) advocate of logical positivism in relatively recent times has been Friedman (1953).¹² On empirical evidence, states Friedman: “... my judgment ... is itself ... to be accepted or rejected on the basis of empirical evidence” (p. 5). And again: “Empirical evidence is vital at two different, though closely related, stages: in constructing hypotheses and in testing their validity” (p. 11). On falsifiability: “Given that the hypothesis is consistent with the evidence at hand, its further testing involves deducing from it new facts capable of being observed but not previously known and checking these deduced facts against additional empirical evidence. For this test to be relevant, the deduced facts must be about the class of phenomena the hypothesis is designed to explain; and they must be well enough

1995) reacted to Krueger (1993) and Card and Krueger (1994), they were *highly* critical. Not for a moment did these commentators even *entertain* the notion that Card and Krueger could possibly be correct in their analysis. So, again, what, precisely, is testing what?

⁹True, in *both* cases, economics and physics, there is a precise technical use of words, and then, also, an ordinary language use. But this fact should not mask the fact that these are two very different disciplines, methodologically speaking.

¹⁰The material in this section is derived from Barnett (unpublished), Block (1973, 1980, 1999), Batemarco (1985), Fox (1992), Hoppe (1989, 1991, 1992, 1995), Hulsmann (1999), Mises (1969, 1998), Rizzo (1979), Rothbard (1951, 1957, 1971, 1973, 1976, 1997a, 1997b, 1997c, 1997d, 1993), Selgin (1988).

¹¹See also Popper (1968) and Quine (1960). For a comparison of Popper and Friedman, see Frazer and Boland (1983).

¹²For critiques of Friedman (1953) see Ebeling (2001), Herbener (1991), Hulsmann (1999), Long (2006), Rappaport (1986) and Rothbard (1989). For support of Friedman in his critique of the Austrians on this ground, see Blaug (1997), Caldwell (1998), Hutchison (1935, 1938) and Spiegel (1991). See also Boettke (1998) and Leeson and Boettke (2006).

defined so that observation can show them to be wrong” (pp. 12-13).

But this viewpoint has not gone uncriticized by its Austrian praxeological opponents. Here is Murray Rothbard’s view: “What Milton Friedman (1953) did was to import into economics the doctrine that had dominated philosophy for over a decade, namely logical positivism. Ironically, Friedman imported logical positivism at just about the time when its iron control over the philosophical profession in the United States had already passed its peak. For three decades, we have had to endure the smug insistence on the vital importance of empirical testing of deductions from hypotheses as a justification for the prevalence of econometric models and forecasting” (Rothbard, 1989, p. 54).

States Hulsmann (1999, p. 3): “For more than forty years, economists have routinely rejected the postulate that economic theory should be realistic. Ever since Milton Friedman (1953) sketchily outlined a positivistic methodology for economics, most students of our science have come to endorse Friedman’s view and have claimed that the only quality standard of economic reasoning was its predictive power. Good theories yield fairly correct predictions whereas bad theories yield wrong predictions. Today, the utter failure of this program is patent. Positivism has not improved economic forecasting. It has encouraged the preoccupation with purely formal problems in mathematical economics and game theory, and at the same time the multiplication of applied studies proving, in the words of Frank H. Knight, that ‘water runs downhill’.”

And in the view of Herbener (1991, pp. 44-45): “This criticism extends with

equal force to modern neoclassical theory, since it is built upon positivism (Friedman 1974). Milton Friedman tells us that all proper economic theory must be testable and subject to falsification; that economic propositions, like those in physics, are hypothetical, tentative, and forever subject to testing and potential rejection. Yet what basic principles of economics have neoclassical economists rejected for failing tests of statistical significance? The laws of supply and demand? The principle of diminishing marginal utility? The concept of opportunity cost? The idea that exchange leads to mutual benefit? Such basic principles are either non-testable, and thus, not positivist economic theories at all, or routinely rejected in econometric tests. Yet all *economic* defenses of the free market are built from basic principles. Friedman and other neoclassical economists say that economic theory must be empirical but they do economic theory deductively, although not as well as Mises.”

Hoppe (1998) offers the following as examples of synthetic *a priori* statements. These *do* apply to the real world, but are *not* falsifiable, in the sense that empirical evidence is even relevant to them, let alone can possibly count against their truth:

Whenever two people A and B engage in a voluntary exchange, they must both expect to profit from it. And they must have reverse preference orders for the goods and services exchanged so that A values what he receives from B more highly than what he gives to him, and B must evaluate the same things the other way around. Or consider this: Whenever an exchange is not voluntary but coerced, one party profits at the expense of the other. Or the law of marginal utility: Whenever the supply of a good increases by one additional unit, provided each unit is

regarded as of equal serviceability by a person, the value attached to this unit must decrease. For this additional unit can only be employed as a means for the attainment of a goal that is considered less valuable than the least valued goal satisfied by a unit of such good if the supply were one unit shorter. Or take the Ricardian law of association: Of two producers, if A is more productive in the production of two types of goods than is B, they can still engage in a mutually beneficial division of labor. This is because overall physical productivity is higher if A specializes in producing one good which he can produce most efficiently, rather than both A and B producing both goods separately and autonomously. Or as another example: Whenever minimum wage laws are enforced that require wages to be higher than existing market wages, involuntary unemployment will result. Or as a final example: Whenever the quantity of money is increased while the demand for money to be held as cash reserve on hand is unchanged, the purchasing power of money will fall.

Let us consider only the first of these in more detail. Why would two people, A and B, engage in a voluntary exchange, if they did not both expect to profit from it? It is logically inconceivable that they would do any such thing. Suppose that both parties to the trade swore a solemn oath that they were entering into the transaction *not* in order to improve their situation. Would we believe them? Certainly not, for actions speak louder than words. Now, it is entirely possible that one or both does not value that which he is to receive in the commercial arrangement more than that which he must give up. If A is giving B an apple in return for B's banana, that is, it is no self-contradiction to suppose that A really prefers this particular apple more than the banana, and that B, were he free to choose, would rank his own banana higher than A's apple. Both, that is, might be moti-

vated by entirely different considerations. For example, A and B might barter these products with each other in order to please C. However, there must be *something* about the swap that improves the welfare position of both (e.g., pleasing C, putting D's nose out of joint) otherwise the embarrassing question¹³ would arise: why in bloody blue blazes are they both consenting to it?

In addition to Hoppe's examples, there are numerous others that can be generated via "tendency" considerations. For example, there is a *tendency* for profits to come into equality with each other in different industries, when due allowance is made for different levels of risk; there is a *tendency* for profits to fall to zero; there is a *tendency* for wages to come into equality with discounted marginal revenue product; there is a *tendency* for supply and demand to be equated; there is a *tendency* for the offerings of businessmen to match the desires of consumers, etc. None of these can be possibly refuted. If there is still a divergence between any of these pairs of economic variables, that does not gainsay that there is a *tendency* from them to come into alignment with one another. Nor are these "mere" tautologies, indicating, only, how we have chosen to use words. They have great explanatory power, in that they shed the light of economics on phenomena that would otherwise be well nigh incomprehensible.¹⁴

¹³Embarrassing for neoclassical logical positivist theory, that is.

¹⁴Austrians are sometimes accused of opposing the use of econometric equations per se. Not so. We object, only, with some interpretations of them; to wit, when they are supposedly "testing" axiomatic truths, as couched in synthetic *a priori* statements. But there is more to the dismal science than

But Milton Friedman is having none of this. A staunch, indeed bitter opponent of praxeology, he claims that the Austrian school of economics is untenable. Without empirical evidence to settle disputes, they can only, in a very unseemly manner for academics, engage in “fighting.”¹⁵ Let us allow him to speak for himself, as he criticizes Mises in this regard:

... his fundamental idea was that we knew things about “human action” (the title of his famous book) because we are human beings. As a result, he argued, we have absolutely certain knowledge of the motivations [*sic*] of human action and he maintained that we can derive substantive conclusions from that basic knowledge. Facts, statistical or other evidence cannot, he argued, be used to test those conclusions ... That philosophy converts an asserted body of substantive conclusions into a religion Suppose two people who share von Mises’s praxeological view come to contradictory conclusions about anything. How can they reconcile their difference? The only way they can do so is by a purely logical argument. One has to say to the other, “You made a mistake in reasoning.” And the other has to say, “No, you made a mistake in reasoning.” Suppose neither believes he has made a mistake in reasoning. There’s only one thing left to do: fight (Friedman, 1991, p. 18).¹⁶

praxeological reasoning. It is not a matter of apodictics, for example, as to how large is a demand or supply elasticity.

¹⁵We would like to thank the following people for helping us uncover Milton Friedman’s statements about Austrians “fighting” each other, and critically commenting on them: Peter J. Boettke, Rafe Champion, Brian Doherty, Richard Ebeling, Roger Garrison, Richard O. Hammer, Ludwig van den Hauve, Peter Klein, Don Lloyd, Roderick Long, Mateusz Machaj and Shawn R. Ritenour.

¹⁶Friedman seems inordinately fond of mak-

Raico’s (1995) rejoinder, however, is definitive:

How such an argument could emanate from such a distinguished source is quite simply baffling. Among other problems with it: Friedman’s theory would predict the occurrence of incessant bloody brawling among mathematicians and logicians; the non-occurrence of such brawling thus falsifies that theory in Friedman’s own positivist terms. Moreover, Friedman’s position entails that no religious person who felt certain about his religious beliefs could have any principled reason to respect the conflicting religious beliefs of others, which is an absurdity. Finally, his “explanation” of Mises’s alleged personal “intolerance” fails to account for the personal tolerance of other practitioners of apriorism in economics.

Klein (2007) offers a powerful *reductio ad absurdum* of this position:

You have to admire Friedman’schutzpah. As is painfully obvious from reviewing the mainstream literature in almost any field of economics, there are assuredly more disagreements among Friedmanite positivists about the interpretation of empirical data than among praxeologists about the conclusions of deductive reasoning. One could even say the following: “Suppose two people who share Friedman’s methodological views come to contradictory conclusions about anything. How can they reconcile their difference? The only way they can do so is by appealing to the econometric evidence. One has to say to the other, ‘You made a mistake in your empirical analysis.’ And the other has to say, ‘No, you made a mistake in your empirical analysis.’ Suppose neither believes he has

ing this spurious charge, as he does it again in Hammond (1992), and once more in Ebenstein (2001), p. 273; see also Doherty (2007), pp. 467-68.

made a mistake in his empirical analysis. There's only one thing left to do: fight."

Ebeling's (2001) assessment is no less devastating to Friedman's viewpoint:

... I must say that I have been around Austrian economists since the first Austrian economics conference in South Royalton, Vermont, in June 1974, and I have never seen two Austrians put on the boxing gloves to resolve disputes over either theory or historical applications of their theories. Have they argued? Have they sometimes had falling-outs and not spoken to each other for periods of time? Yes, but I've heard that even experimental physicists comparing laboratory results have shouted, cursed, verbally attacked each other, and sometimes refused to be in the same room with their scientific colleagues. Unfortunately, these are failings attributable to being human. The "scientific method" has not changed this aspect of human nature. I have even witnessed Milton Friedman get "cranky" at a professional meeting.

Here is an alternative version of Friedman's attack on the praxeological approach: "[It] ... tends to make people intolerant. If you and I are both praxeologists, and we disagree about whether some proposition or statement is correct, how do we resolve that disagreement? We can yell, we can argue, we can try to find a logical flaw in one another's thing, but in the end we have no way to resolve it except by fighting, by saying you're wrong and I'm right" (quoted in Ebenstein, 2001, p. 273). Here is Long's devastating rejoinder:

Friedman obviously thinks that in *a priori* reasoning, as opposed to empirical science, there is no objective way of resolving disagreements. But why does he believe this? Why is he so confident that trying to "to find a logical flaw in one

another's thing," as Friedman puts it, is unlikely to resolve the matter? I can only conjecture that Friedman thinks of *a priori* reasoning as a *subjective* process of consulting the inner contents of one's own mind, heeding the deliverances of some essentially private inner voice that no second person can check on. The empirical method, by contrast, appeals to *publicly* available evidence and so allows for objectivity. But to think about *a priori* reasoning in this way is precisely to confuse the psychological with the logical.

Let's take a less controversial case of an *a priori* discipline: mathematics. If two mathematicians disagree about the results of a calculation, they don't come to blows; nor do they consult a private source of revelation. Instead they "try to find a logical flaw in one another's thing," and presumably one of them will succeed—because *logical relations are at least as "public" as empirical ones* In advocating methodological apriorism, Mises was not advocating reliance on private psychological experiences. After all, it was Mises who wrote: "There is no rational means available for either endorsing or rejecting a doctrine suggested by an inner voice" Instead he was advocating reliance on the *publicly accessible* standards of logical reasoning. For Mises it is apriorism that resolves the intractable debates among empiricists, and not vice versa, since one cannot choose among competing interpretations of data without appealing to abstract theory Friedman is of course free to dispute the *content* of Mises's aprioristic arguments; but the very fact that he can do so shows that Friedman's criticism of their form is misguided. In treating praxeology as a *subjective*, publicly untestable method, Friedman commits the fallacy of *psychologism*: conflating logical relations with psychological ones (Long, 2006, pp. 19-20).

There are yet additional drawbacks of the position articulated by Friedman. One

implication is that Austrians would never change their minds on any issue. Yet, this school of thought, like any other, has had its share of just this phenomenon. Just as in the case of mathematics, logic, and other sciences that feature non-empirical aspects.

Perhaps the most damaging refutation of Friedman's logical positivist thesis is that it is logically incoherent. According to this philosophy, all scientific statements fall into one of two categories. One, they are truisms, and tell us nothing about the real world. Two, they are empirical claims that are indeed relevant to reality, but they purchase their relevance at the cost of certainty. Now, consider for a moment that specific "two category" claim. What, pray tell, is *its* logical status? If it is a truism, then we know it as a certainty. But, then, unfortunately for Friedman, it is totally divorced from reality. If it is an empirical proposition, then where, pray tell, is the *evidence* in its support? Where are the double-blind experiments on the basis of which logical positivists offer it? And, how can we explain the *certainty* with which they make it? This school of thought wishes to have its cake and eat it too: to make an unshakeable "two categories" claim, that somehow, contradicting their basic axioms, nevertheless applies to the real world. It cannot be done, according to their own views.

IV. Conclusion.

We take section II of our paper to be non-controversial. All reaches of the profession of economics, from Marxists to Keynesians to Monetarists; from Institutionalists to Chicagoans to representatives of the Cambridge School (both of them), to Austrians, ought to be able to come

together in support of consistency and precision. This is, after all, nothing but the *sine qua non* of scientific endeavor. Section III offers a very different message. Here, we cannot expect widespread support within the profession. But controversial as are our views in this section, we maintain that the ideas discussed herein are just as important for placing the dismal science on a truly scientific basis.

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