

The Ethical and Scientific Offences of Erhard and Jensen

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Abstract: Erhard and Jensen try to produce a standard for "integrity" without ethical input. It is making bricks without straw. They further suggest that existence theorems and statistical significance will suffice. On all counts they are mistaken.

JEL codes: B41, C01, G05, M20,

The long paper by Werner Erhard and Michael C. Jensen is admirably ambitious. But it makes three related mistakes. The mistakes, I am unhappy to report, add up to bad ethics and bad science.

Bad ethics first. Erhard and Jensen suppose that economics, or any science, not to mention a social science, can arrive at judgments of good or bad in functioning without ethics. They think they are avoiding ethics, getting to the good "with no normative aspects whatsoever."

You can see that there is something strange in such a program. The strangeness is inspired by the ethics and epistemology we get in graduate school, and that some of us never recover from. Erhard and Jensen draw on the vocabulary of "positive" as against "normative," the reduced ethical theory that most economists take as the last word. As they write, "In the current economic mindset 'integrity' automatically occurs as normative, most economists will dismiss it out-of-hand." That's right: the remark is certainly correct as sociology. The dismissal is a Nouvelle Chicago-School/junior-high dogma, enforced with more and more enthusiasm as the 1970s wore on.

But the positive/normative distinction comes out of a (justifiably) obsolete philosophy of ethics and of science. A central dogma in the positivism of the early twentieth century was that "good" and "bad" are merely opinion, "preaching" (with an anti-clerical attitude assumed, that we must not preach). It is called the "hurrah-boo" theory of ethics, or "emotivism." Emotivism was believed by very many 20th-century people, some under the influence of logical positivism, others under the influence of a falling away from religious faith. It is "the doctrine that all evaluative judgments and more specifically all moral judgments are *nothing but* expressions of preference."² Or as Thomas Hobbes, a fount of the view, wrote in 1651, "Good and evil are names that signify our appetites and aversions."³ (Emotivism, observe, taken as a doctrine which

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² MacIntyre 1981, p. 11, his italics.

³ Hobbes, *Leviathan*, 1651, I, Chp. 15, p. 82; and I, Chp. 6, p. 24.

one *should* believe, is of course self-contradictory, since preaching against preaching is preaching. But non-contradictory logic is not the strong point of logical positivism, or of those who have fallen away from religious faith.)

Undergraduates and many of their professors become uneasy and start giggling when an ethical question arises. They regard such questions as having mainly to do with sex – thank *you* fundamentalists of the late 20th century – or with unargued authority, such as the Baltimore Catechism and the nuns to enforce it. The agreement to disagree that ended the wars of religion in Europe can be traced in their unease and in their stock remarks expressing it: “That’s just a matter of opinion.” “Religion should not be mentioned in polite conversation.” “If we disagree about ends it is a case of thy blood against mine.”⁴ “The only methods for reconciling different normative value judgments are political elections or shooting it out at the barricades.”⁵ According to the emotivist theory, to be caught making ethical statements is to be caught in meaningless burbling. Shame on you. That’s why Erhard and Jensen are so proud they have achieved “no normative aspect whatever.”

Yet we cannot in science or business do without ethics, and neither can Erhard and Jensen. Their laboriously axiomatized “model,” therefore, will have to sneak in its ethics unobserved. Of course. You can’t get “good” results, in business or in science itself, both of which Erhard and Jensen amiably seek, without having some idea of goodness.

The way forward is to realize that most scientific issues are both positive and normative. So we should get philosophically serious about the norms. Fact and value are distinct only at a high and mostly useless level. Yes, there are facts of the world, sitting there like stones (although what stones to pick up is a normative issue). And, yes, there are values that people have distinct from the stones (although, as Bart Wilson has argued, many of our values are located out in the language, not in our heads). But most of our lives take place in picking up a stone and, say, hurling it at a leader we do not like, or examining it in some scientific program for its iron content, or placing it pleasantly along the garden path. The Danish physicist Niels Bohr said in 1927, that “It is wrong to think that the task of physics is to find out how nature is. Physics concerns what we can *say* about nature.”⁶ We. Say. With words. About categories involving philosophical and ethical analysis. The German poet Rose Äuslander wrote, “In the beginning/ was the word/and the word was with God/ And God gave us the word/ and we lived in the word./ And the word is our dream/ and the dream is our life.”⁷ We dream of categories, in our metaphors and stories, and with them make our lives, especially our scientific lives. It’s ethical acting.

Consider for example the assertion, believed by economists and by almost no one else, that free trade is good. Erhard, Jensen, and I, for example, all believe it. At a high level of Pareto optimality we can note on a blackboard the efficiency, achieving the

⁴ Robbins 1932, p. 134. Amartya Sen (1987) says that such a view was “quite unfashionable then.” Not I think among the reigning *fashionistas* of 1932.

⁵ Blaug, *Methodology of Economics*, 1980, pp. 132-33.

⁶ Quoted in *Niels Bohr: Reflections on Subject and Object* (2001) by Paul McEvoy, p. 291. The provenance of the remark is a little hazy, but it is very well known. In Danish, the philosopher Hans Siggaard Jensen informs me, it was something like “*Fysik er ikke om hvordan verden er, men om hvad vi kan sige om den.*”

⁷ Am Anfang/war das Wort/und das Wort/war bei Gott/Und Gott gab uns das Wort/und wir wohnten/ im Wort/ Und das Wort ist unser Traum/ und der Traum ist unser Leben.

contract curve. At a high level of fact gathering we can note in the newspaper different prices facing people for the same item, out of Pareto equilibrium. At a high level of ethical philosophy in the style of Harsanyi, Buchanan, Tulloch, and Rawls, we can deny the relevance of actual hurt to losers in trade, or else revert to a Kaldor-Hicks criterion undefended. But to arrive at the assertion that free trade is good, which in practice defines economists as professionals, we need to mix such facts and values, at a lower and less pure level. Of course.

Of course “the prevailing financial economics paradigm requires a transformation.” In particular it requires a dropping of an anti-ethical agency theory – for the adoption of which Michael Jensen personally gets a measure of credit and blame – not a re-enactment of it, as in the present paper. The paper does not refer to Rakesh Khurana’s (2007) careful history of how business schools lost their ethical way, under the fashion for Jensenist “positive” economics. It is distressing to see that Jensen has learned nothing over the decades about what to read and think, after the ethical and economic disasters of “greed-is-good,” derived from his earlier advocacy for agency theory.

Of course integrity is a factor of production. It is certainly so in our own science, as shown at length in the recent *Oxford Handbook on Professional Economic Ethics*, edited by George DeMartino and me. The assumption of scientific and commercial honesty, imperfect though each will be, is essential to any society, complex or simple. It is not “heretofore hidden,” as Erhard and Jensen claim – though hidden I suppose from Erhard and Jensen. Anyone slightly acquainted with the history or sociology of the economy knows that integrity is central. For that matter, anyone who reads novels or plays knows it. Anyone who has lived with a little awareness in an economy knows that ethics and professionalism, bundled into integrity, are central.

Erhard and Jensen say that they “draw on insights from other disciplines.” A good idea, implied in fact by the economist’s doctrine of free trade. Doing so, however, would have required them to actually read in other disciplines. Actually trade. There is not much evidence they have done so. To take a discipline highly relevant for thinking about the good, they have no idea of philosophy, because they have not troubled to read any, at any rate with the humility of students seeking actual learning. Their dependence on part 1 of a dictionary definition of “integrity” as wholeness, for example, is in fact, and despite their naïve claim that it is a value-free datum, a little piece of ethical philosophy (though incompetent as philosophy, and a junior-high-school rhetorical ornament of quoting dictionaries to boot). To understand the actual philosophy in the matter, Erhard and Jensen would have needed to have read Aristotle’s *Nicomachean Ethics* and to have entered as mature students into the gigantic library it generated on *telos* – “end,” “purpose,” that is, “wholeness in performance.” They haven’t. And anyway, even without bothering with tiresome reading assignments, one can see that “good for ones word,” in the phrase they use, involves the word “good,” and therefore, of course, has ethical valence.

I do not know why anyone would think they can talk confidently about ethics without having read any ethical philosophy, or without having thought through life or fiction. Yet many people do.

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Then the two scientific mistakes. They are not peculiar to Erhard and Jensen. But then neither is their mistake in ethical philosophy. If we economists are going to be seriously scientific about ethics we should stop committing the mistakes in philosophy

and in quantitative science. The paper is an extreme example of the ethical and scientific mistakes, which makes it a good proof text.

For one thing, the paper supposes that *qualitative* existence theorems are scientific. Though universally taught and practiced in economics, the supposition is mistaken, because existence theorems are unbounded in number and character, and cannot be tested, precisely because they have no quantitative expression. Unlike the quantitative propositions that characterize physics and geology, there is no way to test whether an effect “exists.” Zero is zero. Unless one has a criterion rooted in the science or policy at stake of how far something is from zero to “exist,” mere existence is scientifically useless.

For another, the paper depends on a supposition – again universally taught and practiced in economics – that, null hypothesis “significance” tests are a meaningful way to do just such testing of existence, zero or not. Since the beginning of modern statistics around 1900 many of the leading voices have explained that “significance” is not inherent in a number itself, and can only be judged in substantive form, within a scientific discussion of magnitudes: Edgeworth, Gossett, Egon Pearson, Jeffreys, Borel, Neyman, Wald, Wolfowitz, Yule, Deming, Yates, Savage, de Finetti, Good, Lindley, Feynman, Lehmann, DeGroot, Chernoff, Raiffa, Kenneth Arrow, Blackwell, Milton Friedman, Mosteller, Kruskal, Mandelbrot, Wallis, Roberts, Clive Granger, Press, Berger, and Arnold Zellner. How big is big is a scientific question, but cannot be answered merely by staring at the numbers. If I ask, “Is it a good day?” and you answer, “Six, which is statistically significant,” we have not got anywhere. We need to decide on a scale (Celsius temperature, say, or a non-interval scale of 1 to 10 in human opinion, or whatever) and decide further whether “six” is sufficient to judge the day good or bad. It is a substantive scientific decision among humans, and cannot be turned over to a table of *t*. The very inventor of the table of *t*, William Sealy Gosset (the “Student” of Student’s *t*) said so, first among the others.

Economists (and a few other scientists, such as, most alarmingly, medical scientists) have ignored such leading voices in statistics, as Erhard and Jensen do. Most other scientists, such as physicists, astronomers, chemists, and historians, do not. They judge daily how big is big, and do not think $p < .05$ is any sort of answer. (If you don’t understand what I am saying here, or think you disagree with it, or are scornfully indignant that anyone would say such things, you need to read the declaration in April 2016 of a committee of the American Statistical Association, which said that tests of significance are silly; and then you need to betake your worried self to reading Ziliak and McCloskey, *The Cult of Statistical Significance* [2008], or the leading voices in statistics since 1900 quoted in the book.)

The program of The Two Mistakes that Erhard and Jensen are innocently following was announced in Samuelson’s modestly entitled PhD dissertation, *The Foundations of Economic Analysis* (1941, 1947) and in Friedman’s “The Methodology of Positive Economics” (1953) – a paper, he told me, that he later regretted. The program was set out most clearly 1957 in Tjalling Koopmans’ self-confident *Three Essays on the State of Economic Science*, which we graduate students of the 1960s took as holy writ. Devise *qualitative* theorems (such as Erhard and Jensen’s statement that perfect performance requires perfections). Then “test” the “hypothesis” (as Erhard and Jensen then propose) with null hypothesis significance inherent in the numbers, without a standard of how big is big.

Most economists, including here Erhard and Jensen, therefore stopped thinking in

1957 about what they were doing. My attempts during the late 1980s and 1990s (1985/1998, 1990, 1994, 1997, and 2002) to get them restarted in thinking had essentially no result. A pity. Some years ago I was alarmed to hear that Economics at Indiana University assigns its graduate students Milton's article as a complete guide to economic research. And all the best graduate programs require the theorem-proving *Microeconomic Theory* by Mas-Colell, Whinston, and Green, which is the sole reason that graduate students need proofs in real analysis, otherwise useless for actual economic science. And then the students do three terms of econometrics with no mention that how big is big is the chief scientific question and that its answer depends on judgment in light of the numbers, not on the numbers stripped of judgment, such as the tests buried in canned regression programs.

Really, let's think it through.

What would be the point of a "purely positive approach . . . with no normative aspects whatsoever"? Positivism has been shown decisively and repeatedly since the 1920s to be lacking in point. Erhard and Jensen quote Thomas Kuhn but do not appear to have understood what he was doing, namely, destroying positivism by actually studying science. They refer to *The Structure of Scientific Revolutions* (1962), not to his more unsettling *The Essential Tension* (1977), which showed how physics actually operates. In the philosophy, history, and sociology of science the positivism that Erhard and Jensen admire was aborted as early as the Duhem Dilemma of 1914: "if the predicted phenomenon is not produced, not only is the questioned proposition put into doubt, but also the whole theoretical scaffolding used by the physicist."⁸ No science ever, it has been shown again and again since 1914, actually follows positivism, or should. And no one could live her personal or scientific life on the positivist ukases against ethics that economists carry about with them on 3" x 5" cards.

Erhard and Jensen write for example, "the state of being whole. . . is a necessary (and sufficient) condition for maximum workability." What would be the scientific point of such a tautology? They think they are articulating a Theorem, with suitably fancy definitions. But theorems, contrary to the Samuelson-Arrow-Koopmans orthodoxy, are not how science works. In an early chapter of his notoriously difficult freshman physics course at Cal Tech, the great physicist Richard Feynman told the kids that they needed to learn some matrix algebra, and might as well see the simple proofs involved. Then he wrote, defensively, "What is [proof-oriented] mathematics doing in a physics lecture?" His rhetorical question – why proof? (he said "how various mathematical facts are demonstrated") – would startle an economist who has learned her math outside the departments of physical science.⁹ Science (by the English definition since the 1850s) works with magnitudes. Math-Department mathematics, as against the application of some of its results to quantitative science such as physics and economics, does not.

The Math Department wants to know whether there *exists* an even number that is not the sum of two primes, and doesn't care at all that calculation up to high powers of ten has not found a single instance of an even number that is *not* the sum of two primes. Math-Department mathematics is, like theology and philosophy and the other humanities, interested in yes/no, exist/not. I yield to no one in admiration for pure math, economic theory, theology, literary criticism, and philosophy, and have even

⁸ Duhem 1914, 281; 1954, 185.

⁹ Feynman 1963, vol. I, p. 22-1.

published items in four of the five. But they need to be recognized as humanities, necessary categorizing *first* steps in a policy science like economics or engineering. They are mere fancies if they lack the further step of quantitative testing.

The proposed quantitative testing by Erhard and Jensen, I have noted, are the statistically “significant” results that have driven modern economics into the ground. Erhard and Jensen put their faith in “formal measurement of the statistically significant increase in performance created by integrity.” The faith does not acknowledge the essential absurdity of tests of significance in the absence of a substantive loss function. They say, “We look forward to the completion of additional formal statistical tests” — when, after all, the business world we are all studying has a straightforward loss function, called “profit” or “market valuation” which is plenty “formal” enough. When I used to eat lunch daily in the 1970s at the Quad Club of the University of Chicago with Merton Miller, Gene Fama, Myron Scholes, and Fischer Black I would hear — without then quite grasping its import — that *The Journal of Business* did not accept tests of statistical significance of an alleged irrationality in the stock market but would instead demand to see the author’s bank account. It’s a good test, with a loss function surely relevant to a business discipline. (Later, when I finally got it, I wrote a book on the theme, called *If You’re So Smart: The Narrative of Economic Expertise* [1990], and had meanwhile started to criticize tests of statistical “significance.”)

Richard Feynman spoke of “cargo-cult science.” It is what Erhard and Jensen are doing. It has some hard math that evokes Science. It claims to deal in numbers, like Science. It proposes Hypotheses, like Science according to positivism. But in truth their paper is like the coconut-and-candle “landing strips” the New Guineans built after World War II to get the big airplanes to come back and deliver more of their intriguing cargo.¹⁰

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The paper, then, is an example of how unscholarly and unscientific are economists. The *direction* of their unscholarliness and unscientificity is worth a little further comment. Erhard and Jensen, with most economists — but in their case with an illuminating simplemindedness worthy of the web site Economics Job Rumors run by grown children — ignore and disdain the humanities.

The humanities deal with the categories of meaning that we humans regard as important, such as business ethics vs. political ethics, corporation vs. partnership, red giants vs. white dwarves, viruses vs. bacteria, citizens vs. illegals, ugly vs. beautiful, dignity vs. pleasure, good vs. bad. Clearly, you need to know the meaning of a category before you can count its members, which is why the humanistic sciences — the Germans call them *die Geisteswissenschaften*, the “spirit sciences” — must always precede the quantitative sciences, whether social or physical. Meaning is scientific, and science cannot be done without human meaning. Piling up “existence” theorems and “significant” results is meaningless.

It is not therefore only technical philosophy among the humanities that can illuminate the business of ordinary life. You can learn from the plays of Henrik Ibsen or Arthur Miller about the meaningful categories in a bourgeois life — such as that a Master Builder fears entry by the young; or that respect must be paid even to the unsuccessful salesman. You can learn from Milton — John, not Friedman — that “evil be thee my good” is a clever fool’s plan for a life, even for an angelic life, as is also an aristocratic or peasant or bourgeois plan such as “he who dies

¹⁰ Feynman 1974.

with the most toys wins,” or “greed is good.” You can learn from linguistics, or from the Dilbert cartoon, that the surface rhetoric of a manager’s declaration can have the opposite pragmatic or illocutionary force. You can learn from the existence-theorems in the sort of mathematics beloved in high-brow economic theory – itself part of the humanities, not of the quantitative sciences – that there *might* exist a category of spillovers in free markets that *might* justify massive intervention by a hypothetically perfect government of benevolent philosopher kings. The categories themselves of spillover (any effect however small?), justified intervention (shooting polluters?), government (carelessly exercising the monopoly of violence?), benevolent (towards whom?), and philosopher (not rhetoricians?) are themselves appropriate subjects for a humanistic inquiry.

The experimental economist Bart Wilson I have mentioned, who coined the term “humanomics,” which is what I am advocating here, recently used the philosopher Ludwig Wittgenstein (1889-1951) to locate the sense of justice not merely in the utility functions of individuals but in the language game they play.¹¹ He is the only economist to use Wittgenstein deeply. I myself have begun to use the philosopher John Searle (1932-) to bring the study of economic institutions up to philosophical and literary speed in the matter of categories to count.¹² Such a tactic pays off scientifically. That is, you can learn the categories of human meaning, the first step in a science, by getting to know, on all the matters which most concern us, “the best which has been thought and said in the world” by a variety of philosophers, from Confucius (Kongzi, Kung the Teacher) to Amartya Sen.

It is therefore a childish mistake to suppose that the central question in the humanities – What *kind* is this or that? – is *unscientific*. The avoiding by Erhard and Jensen of serious engagement with the humanism of ethical philosophy participates enthusiastically in the mistake. The what-kind question occurs prominently in biology, for example, and is central to art history and mathematics and systematic theology. The systematic, scientific humanities are an exploration of kinds. The physical and much of the social sciences are then an exploration of the *amounts* of the kinds. Obviously before you can count you have to know what we humans wish to count (note the word) as a red giant star or a citizen of Zurich. A scientific and humanistic step of human meaning, which establishes what kind we want to count, precedes every scientific job of counting. The disdain that most economists have for humanistic thought is without scientific or philosophical justification.

The ornamental gestures by Erhard and Jensen towards a claim of philosophical literacy merely serve to confirm how uninstructed they are and how little they have thought through the humanistic step of a science. Footnote 8, for example, is supposed to illuminate their so-called “veil of invisibility” (which says merely that people often don’t notice when they are being unethical; all right: “not whole”). One is startled to find references there to Harsanyi and Rawls on the veil of *ignorance*, which Erhard and Jensen proudly declare they are “playing on” (they could have mentioned Buchanan and Tullock and Rawls in the same connection). But their veil has nothing whatever to do with a veil worn in imagination precisely for the establishment of ethical principles. Ethical principles are not on the agenda for Erhard and Jensen.

I could go on. And on. The paper is for example wretchedly written, with gigantic amounts of anticipation and repetition that obscure the point. How many times does one need to repeat *word-for-word* a dictionary definition of “integrity,” part 1, itself uncritically received

¹¹ Wilson 2010.

¹² McCloskey 2015; Arnhart 2015.

and not further analyzed? The answer according to Erhard and Jensen is fifteen times. True, we have grown accustomed in economics and business studies to a level of stylistic barbarity in which such writing is accepted – nay, expected, required, embodied even in editorial suggestions, such as the idiotic table-of-contents paragraph that every paper now has (“This paper is organized as follows”). Still, Erhard and Jensen plumb the depths of bad writing, as though testing the bottom.

You can see, in short, that I do not like their paper or believe any of its conclusions. Neither should you.

Why bother, then? Why not pass over the ethical and scientific, and literary, offenses of Erhard and Jensen in silence? This: I live in hope that my grumpy plain-spokenness will lead even a handful of the younger readers here to question the Received Paradigm, 1957 to the present. I hope they will venture to learn something serious about philosophy, say, or literature, or sociology of science, or economic and business history, or for that matter statistical theory. It would save their intellectual lives from cargo-cult science, such as the essay by Erhard and Jensen.

I will pray for them.

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