DISCUSSION

G. C. ARCHIBALD: I will confine my remarks to Papandreou's paper, which I think provides a very elegant and interesting formalization of theory construction. Friedman pointed out, in his "Methodology of Positive Economics," that the specification of the conditions in which a theory is to hold is a necessary part of the theory, and Papandreou shows clearly the consequences of failure to provide that specification: either the theory is irrefutable or it is far too easily refuted because it is unbounded. Clarification of this point is extremely helpful.

It is interesting to ask if a formalization of this sort can help to resolve any of our current difficulties and controversies. Can we, for example, apply it to the dispute over "testing assumptions" (the subject of Nagel's paper)? I once suggested (*British Journal for the Philosophy of Science,* May, 1959) that the word "assumption" is used very loosely in economics, to include statements of very different sorts (about motivation, overt behavior, conditions in which the theory is to hold, etc.), and that the argument about assumptions might be more easily settled if we distinguished the different sorts of statements. After reading Papandreou's paper, I feel that we can drop the word entirely: we can refer directly to the components of theory without using vague portmanteau words. The main components are the structural relations (which may or may not be deduced from postulates about motivation) and the specification of context. On the other hand, it seems to me that the adequacy of a theory (or the performance of the structure in a given context) is, to some extent at least, a quantitative matter. What I would like to do is see if Papandreou's treatment sheds any light on the problem of quantitative adequacy. Let us start by considering a little further the question of specifying the context in which a theory is to hold.

The specification of the rule s provides the context in which the theory is supposed to hold in generic rather than time-space form; e.g., "in competitive markets in capitalist economies." When the context is so specified, we may say that the theory is "generically anchored." Now, says Papandreou, even when, as is commonly the case, s is inadequately specified,we may formulate a refutable "descriptive" statement (3.0) that the theory holds in some individual space-time, H^*. Here we may say that the theory is "locally anchored." Papandreou's (3.0), however, is really derived from (1) the general theory (2.0) in which the rule $s(v) = \omega^*$ is uninterpreted and (2) the statement of a "local anchorage," that $H^* \varepsilon \omega^*$ which together give the local test statement (3.0).

What we now have, however, is the composite statement "if a PH^* and if $H^* \varepsilon \omega^*$, then $\chi \varepsilon \bigcap_i F_i$." Now where do we stand in the event of a refutation? Plainly it is possible either that the structure $\emptyset$ is wrong or that the local anchorage is wrong. It is the latter possibility that is the source of so many alibis for prima facie refutations: we may maintain our faith in structures by asserting, usually with hindsight, that the local anchorage was inappropriate.

Thus the proper specification of s is required to eliminate this source of alibis. On the other hand, a test with H^* is not valueless: a refutation at least limits the social space to which the theory might be anchored.

To test a theory, however, we always require a local anchorage, a given historical space-time. Thus even if a theory is generically anchored, it is necessary to identify a given H^* as an element of ω^* before a test can be performed. Now s is a rule for mapping observation acts; so if s is fully specified there should be no difficulty. But consider the usual case, discussed by Papandreou, in which s has an uninterpreted component. We may posit a local anchorage to obtain the test statement (4.10) (my remarks about the derivation of (3.0) apply equally to (4.10)), which lies partway between the purely local, (3.0), and the complete statement (1.5). In the case of (4.10) we still have potential alibis of the sort we met with (3.0). Clearly, the best we may usually expect in economics is a test statement of the (4.10) type. Yet much of our theory appears to have a generically specified anchorage; e.g., in market structure. I suggest that our difficulty often lies in identifying a given space-time as an element of the relevant general social space. The perfectly competitive market is an example. The relevant theory is generically anchored, but can we identify a local example? Do we know, in other words, that this social space belongs to the set of actual states of the world (in which case identification is possible) or only to possible states? This is really only another way of saying that we may have specified a generic context without making s operational.

This leads to my quantitative problem. Let us suppose that we have a theory anchored to a "conceptual" social space c (I do not use Papandreou's γ here because it is a component of ω, a subset of actual states of the world). We want to know if c does correspond to states of the world. This is the same question as: can we identify an appropriate H^* (or, nearly enough, can we make s operational)? Now "conceptual" and "real" states of the world are never in exact correspondence. So how do we judge? The attempt to settle the question on its own merits, so to speak, is surely a case of arguing about the realism of assumptions! It seems to me that the correspondence between c and ω, or the appropriateness of the H's selected for test, is very hard to settle except *ex post,* in the light of the results of the test. And the results of a test are, in general, quantitative: the adequacy with which a given structure performs in a given context is normally a question of "how much error." So then the correspondence of c and ω is itself a quantitative matter. This I take to be Friedman's position. And we still have no guidance in deciding "how much" is a failure. All we can say is that failure, however it is judged, does not refute the basic structure, which is, without context, irrefutable. It does, however, limit the range of its applicability. So we either learn to specify the context better, or we can maintain the structure without operational context—if we like ideal forms.

I am so intrigued by Papandreou's formalization that I should like to go on to consider many other questions in its light. We could, for example, formulate precise definitions of complementary and rival theories; or consider Friedman's proposition that different theories may be applied to the same

phenomena for different purposes (the answer, I think, is: only if Papandreou's rule *g*, which provides the index to the filing cabinet of theories, has been defined, which it has not in the example over which Friedman and I are arguing). Unfortunately, time is limited, so I must content myself with one last point. Papandreou asserts, I think correctly, that statements about the properties of the equilibrium set, as well as its derivatives, are meaningful statements. But he says that this is true for models of the (2.0) type. (2.0), however, is completely unanchored since *s* is unspecified. If by meaningful we mean refutable, at least in principle, this is a mistake. Neither statements about equilibrium conditions nor about directions of change are refutable if they are unanchored. Given an anchorage, however, I agree that both types of statement are potentially meaningful. Which we try to test is a matter of expediency.

HERBERT A. SIMON: I find methodological inquiry interesting and instructive to the extent to which it addresses itself to concrete problems of empirical science. Thus, while I find myself in general agreement with almost everything that has been said in the previous papers and by discussants, I should like to pitch my remarks at a level less abstract than theirs.

The Relation of Premises and Conclusions in Economic Theory. Professor Nagel has pointed out that whether a particular proposition is a fundamental assumption of a theory or one of its derived conclusions is relative to the formulation of the theory. If this were the whole story, then asymmetry between assumptions and derivations in Friedman's position—what Professor Samuelson called the F-Twist, and what I like to think of as Friedman's "principle of unreality"—would be entirely arbitrary. Professor Krupp's remarks on composition laws and the relation of microscopic to macroscopic theories suggest, however, that something more is at issue.

Since the prefixes "micro" and "macro" have rather special meanings in economics, let me talk instead of theories of economic actors and theories of economic markets, respectively. In the present context, the relevant theory at the actor level can be approximated by the propositions: *X*—businessmen desire to maximize profits; *Y*—businessmen can and do make the calculations that identify the profit-maximizing course of action. The theory at the market level may be summed up as: *Z*—prices and quantities are observed at those levels which maximize the profits of the firms in the market. (For simplicity, let us assume that we mean the maximum of perfect competition theory.)

Defending the theory consisting of *X*, *Y*, and *Z*, Friedman asserts that it doesn't matter if *X* and *Y* are false, provided *Z* is true. Professors Nagel and Samuelson have already exposed the logical fallacy in using the validity of *Z* to support *X* and *Y*, or to support consequences of *X* and *Y* that do not follow from *Z* alone. But there are other equally serious difficulties in Friedman's position.

That *X* and *Y* are taken as premises and *Z* as a conclusion is not just a matter of taste in formulation of the theory. The formulation fits our common, if implicit, notions of explanation. We explain the macroscopic by the

microscopic (plus some composition laws)—the market by the actors. We do this partly because it satisfies our feeling that individual actors are the simple components of the complex market; hence proper explanatory elements. We do it partly because X and Y, plus the composition laws, allow us to derive other propositions at the market level—say, about shifting of taxes, or other policy matters—which we are not able to test by direct observation.

The logical fallacy in Friedman's principle of unreality has exerted so much fascination—both in this session and elsewhere—that attention has been distracted from its other errors. Most critics have accepted Friedman's assumption that proposition Z is the empirically tested one, while X and Y are not directly observable. This, of course, is nonsense. No one has, in fact, observed whether the actual positions of business firms are the profit-maximizing ones; nor has anyone proposed a method of testing this proposition by direct observation. I cannot imagine what such a test would be, since the tester would be as incapable as business firms are of discovering what the optimal position actually is.

If, under these circumstances, Z is a valid theory, it must be because it follows from empirically valid assumptions about actors together with empirically valid composition laws. Now we do have a considerable body of evidence about X and Y, and the vast weight of evidence with respect to Y, at least, is that it is false. The expressed purpose of Friedman's principle of unreality is to save classical theory in the face of the patent invalidity of Y. (The Alchian survival argument that "only profit-maximizers survive," does not help matters, since it, like Z, cannot be tested by direct observation—we cannot identify the profit-maximizers.)

The remedy for the difficulty is straightforward, although it may involve more empirical work at the level of the individual actors than most conventionally-trained economists find comfortable. Let us make the observations necessary to discover and test true propositions, call them X' and Y', to replace the false X and Y. Then let us construct a new market theory on these firmer foundations. This is not, of course, a novel proposal. The last two decades have seen it carried a long distance toward execution.

Ideal Types and Approximations. My final comment is related to the previous one. There has been much talk at this session of ideal types: perfect vacuums and perfect competition. I am not satisfied with the answers to Friedman's argument that he has as much right as the physicists to make unreal assumptions. Was Galileo also guilty of using the invalid principle of unreality? I think not. I think he was interested in behavior in perfect vacuums not because there aren't any in the real world, but because the real world sometimes sufficiently approximates them to make their postulation interesting.

Let me propose a methodological principle to replace the principle of unreality. I should like to call it the "principle of continuity of approximation." It asserts: if the conditions of the real world approximate sufficiently well the assumptions of an ideal type, the derivations from these assumptions will be approximately correct. Failure to incorporate this principle into his formulation seems to me a major weakness in the interesting approach of Professor

Papandreou's paper. Unreality of premises is not a virtue in scientific theory; it is a necessary evil—a concession to the finite computing capacity of the scientist that is made tolerable by the principle of continuity of approximation.

Working scientists employ the principle of continuity all the time. Unfortunately, it has no place in modern statistical theory. The word "significant" has been appropriated by the statisticians to mean "unlikely to have arisen by chance." Now, in testing extreme hypotheses—ideal types—we do not primarily want to know whether there are deviations of observation from theory which are "significant" in this sense. It is far more important to know whether they are significant in the sense that the approximation of theory to reality is beyond the limits of our tolerance. Until this latter notion of significance has been properly formalized and incorporated in statistical methodology, we are not going to accord proper methodological treatment to extreme hypotheses. The discussion at this session has not provided the solution, but it has identified this problem as one of central methodological importance for economics.

PAUL A. SAMUELSON: When Maxwell's Demon rank orders scientific disciplines by their "fruitfulness" and by their propensity to engage in methodological discussion, he finds a negative correlation and a strong inverse relationship. It is as if a science could lift itself by its own bootstraps: by maintaining a superlative silence on method, a science can become superlatively fruitful and accurate. Like many "as if" statements this is nonsense. It is more correct, albeit not very informative, to say that soft sciences spend time in talking about method because Satan finds tasks for idle hands to do. Nature does abhor a vacuum and hot air fills up more space than cold. When libertines lose the power to shock us, they take up moral pontification to bore us.

But, of course, I jest. Methodological discussion, like calisthenics and spinach, is good for us, and Dr. Nagel deserves our thanks for taking the time away from other sciences to help straighten us economists out. It is the Lord's work, and we are grateful.

As I understand his paper, Nagel comes to save Milton Friedman from himself. Nagel believes that "theory" does have an important role to play in economics and any discipline, but that Friedman's attempt in his essay on positive economics to vindicate the importance of abstract theory involves mistakes which might themselves be wrongly held against theory's establishable role.

I think Nagel's paper is valuable in pointing out certain errors in the stated claims for theory. I think, within the limits imposed by his need for brevity, it performs the constructive function of sketching some valid arguments for the useful role of theories in an empirical science. But Professor Nagel is too polite. He has not, to my mind, vindicated against itself that which was special and distinctive in the Friedman methodology; instead he seems to have jettisoned what might be called the special "Friedman Twist." And rightly so, I am afraid.

Let me first state some valid interpretations of the "as if" character of using theory to help organize our descriptions of empirical reality. Then point out some illegitimate interpretations.

When a writer on positive economics says that hypotheses or theories should be judged on their "consequences"—or their ability to describe well and organize well empirical observations—he is saying something valuable. Valuable, but perhaps not new. Pragmatists have long insisted that a theory's worth is measured by the consequences of believing it rather than something else or nothing else. Scientists and philosophers who never read Peirce, James, Dewey, Mach, Bridgman, or Carnap have enunciated this same view.

Heinrich Hertz said that a belief in Maxwell's theory of light meant nothing more and nothing less than that the observable measurements agreed with the partial differential equations of Maxwell. (With the advent of quantum mechanics and wave theory the situation became one of *reductio non ad absurdum:* physicists didn't know or much care what it was that was waving in Schrodinger's equation, a probability or what not, so long as the facts of refraction and emission could be described well by this mnemonic model.) Poincaré said that the whole content of classical dynamics was summed up in the hypothesis that certain sets of second-order differential equations exhibited solutions that to a good approximation duplicated the behavior of celestial bodies and terrestrial particles. Pascal made generous use of Occam's Razor in his "explanation" of why "nature abhors a vacuum [period or up to 30 inches of mercury and 30 feet of water]" was an inferior theory to one which assumes that there is an equilibrium balance reached between the "weight" of the unseen atmosphere and the seen mercury and water columns. When Newton wrote down his system of the world, he explicitly said what would have to be translated into modern terminology as, "I don't care to speculate why n-bodies behave in accordance with the inverse-square law of gravity and acceleration; I am content to show what are the implications of this law in contrast to the implications of variant hypotheses, and to present my calculations demonstrating agreement with the observations of moons, apples, and planets."

So long as light rays continue to act so as to go from place to place by the paths of least time, except as a figure of speech no one insisted that they exercised conscious, self-conscious, deliberative will. At worst, some scientists who were Deists, or Sunday poets, said that God or Nature acted like a Great Economizer.

None of the above is banal or trite. As against other authorities who insisted on seeking "more ultimate explanations," these writers said what needed to be said and Professor Friedman is a welcome recruit to their camp. But what I and other readers believe is his new twist—which from now on I shall call the "F-Twist," avoiding his name because this may be, and I hope it is, a misinterpretation of his intention—is the following: A theory is vindicable if (some of) its consequences are empirically valid to a useful degree of approximation; the (empirical) unrealism of the theory "itself," or of its "assumptions," is quite irrelevant to its validity and worth.

At points, the F-Twist seems to go even farther and claim: It is a positive merit of a theory that (some of) its content and assumptions be unrealistic since only if it is not tailored closely to one small bit of reality can it give a useful fit to a wide spread of empirical situations. Unless we explain complex reality by something simpler than itself we have accomplished little (period or by theorizing).

The last part of this F-Twist is separable from its basic part. While I believe that this last part is misphrased and that its germ of truth should be stated in other terms, brevity forbids my discussing it here and forces me to concentrate on the basic F-Twist, which is fundamentally wrong in thinking that unrealism in the sense of factual inaccuracy even to a tolerable degree of approximation is anything but a demerit for a theory or hypothesis (or set of hypotheses). Some inaccuracies are worse than others, but that is only to say that some sins against empirical science are worse than others, not that a sin is a merit or that a small sin is equivalent to a zero sin.

To a philosopher or scientist, the F-Twist is of no great moment and its discussion might perhaps be bypassed. To present-day economics—and I daresay to Professor Friedman—its validity would be of considerable moment. For, as Rotwein (*Q.J.E.,* 1959) and others have hinted, the nonpositivistic Milton Friedman has a strong effective demand which a valid F-Twist brand of positivism could supply. The motivation for the F-Twist, critics say, is to help the case for (1) the perfectly competitive laissez faire model of economics, which has been under continuous attack from outside the profession for a century and from within since the monopolistic competition revolution of thirty years past; and (2), but of lesser moment, the "maximization-of-profit" hypothesis, that mixture of truism, truth, and untruth.

If Dr. Friedman tells us this was not so; if his psychoanalyst assures us that his testimony in this case is not vitiated by subconscious motivations; even if Maxwell's Demon and a Jury in Heaven concur—still it would seem a fair use of the F-Twist itself to say: "Our theory about the origin and purpose of the F-Twist may be 'unrealistic' (a euphemism for 'empirically dead wrong'), but what of that. The consequence of our theory agrees with the fact that Chicagoans use the methodology to explain away objections to their assertions."

This, however, is cheap humor. It is hard lines to hoist a man on his own petard, while at the same time arguing that there exists no such valid petard. I must be brief in explaining why the F-Twist lacks validity. Many of these arguments can actually be found in Friedman's essay, as Nagel has noted; but that may only indicate a noble inconsistency rather than invulnerability. Besides, I am discussing the F-Twist, not any person's views, and by any other name, such as the S-Twist, it would be just as bad.

1. Define a "theory" (call it B) as a set of axioms, postulates, or hypotheses that stipulate something about observable reality. (If no conceivable observation can even in principle refute, confirm, or touch or bear upon the axiom system taken as a whole, then B is not economics, astronomy, physics, biology, or anything properly called science. It might be a model of language,

logic, mathematics, mathematical probability or geometry, or game-playing—but that is something different.)

2. A reader of Friedman might be forgiven for lapsing into thinking that the thing called B has consequences (call them C) that somehow come after it or are implied by it and (*sic*) are somehow different from it.

3. That same reader might be forgiven for thinking that just as B has consequences C that come after it, it also has some things which are somehow antecedent to it called its "assumptions" (and which we can label A).

4. The F-Twist says that the empirical realism, at least up to some "tolerable degree of approximation," of C is important. If C is empirically valid (realistic) then B is important even if A—and for that matter B itself—is not empirically valid (is unrealistic in the sense of being empirically at variance with known or knowable facts, at any tolerable level of approximation).

5. If C is the complete set of consequences of B, it is identical with B. B implies itself and all the things that itself implies. There can be no factual correctness of C so defined that is not also enjoyed by B. The minimal set of assumptions that give rise to B are identical with B, and if A is given this interpretation, its realism cannot differ from that of the theory B and consequence C.

6. But now consider a proper subset of C, which contains some but not all of the implications of B and which we may call $C-$. And consider a widened set of assumptions that includes A as a proper subset, so that it implies A (and B and C and $C-$) but is not fully implied by A. Call this A+.

In symbolic notation we can say

$$A+ \supset A \equiv B \equiv C \supset C-$$

7. Now, suppose that C has complete (or satisfactory) empirical validity. Then bully for it. And bully for the theory B and for its assumption A.

8. We cannot say bully for $A+$ in the same sense—unless its full content, which we may call $A+ \equiv B+ \equiv C+$, also have empirical validity. If that part of $C+$ which is not in C is unrealistic in the sense of being impirically false at the required level of approximation, then $A+$ is definitely the worse for it. The invalidity of part of $A+$ is not irrelevant to its worth. If only the A subset of $A+$ is valid, then so much the worse for $(A+) - (A)$ and for $A+$.

If as often happens we do not have evidence on the factual inaccuracy or accuracy for $A+$, we simply reserve judgment about it, and keep saying bully for A. If no evidence can bear on $(A+) - (A)$, then we use Occam's Razor and concentrate on $A \equiv B \equiv C$ above, forgetting $A+$.

9. It should be unnecessary for me to explain why the empirical validity of $C-$ does not, of itself, import any luster to $A \equiv B \equiv C$—unnecessary because this is the same logical case as I have just disposed of.

This completes my demonstration that the F-Twist is fallacious. I shall illustrate briefly with some examples, primarily economic.

Let B be maximizing ordinal utility (satisfying certain regularity conditions) subject to a budget constraint defined by given income and prices.

Let C be the Weak and Strong Axioms of revealed preference, which are

stated in testable form involving $\Sigma P_j Q_j$, price-quantity data. My above arguments will show how misleading it is to think such tests are in any genuine sense "indirect" ones.

It happens that C implies B as well as being implied by it. It is nonsense to think that C could be realistic and B unrealistic, and nonsense to think that the unrealism of B could then arise and be irrelevant.

But suppose the Weak Axiom, $C-$, is valid and the Strong Axiom is definitely not. If the F-Twist means anything, it says, "Never mind that B is unrealistic; its consequence $C-$ is realistic and that is all that counts."

Surely this is nonsense. B has been shown to be empirically false. That $C-$, one of its implications, is valid does not in any way atone for the fact that $(C) - (C-)$ is definitely false. Only that part of B which is $C-$ has been vindicated by the validity of $C-$. That other part $(B) - (B-)$, has been refuted. The only sensible thing to do, I mean the only thing to do, is jettison $(B) - (B-)$ and replace as your theory $B-$. If you say, "But $B-$ is a truncated fragment of the organic whole B, and it is odd to call $B-$ my theory," I simply reply: "How do you define organic wholes, and anyway I'd rather have the valid tail of a theory than have an invalid dog's body attached to that tail. What is required is not Occam's Razor so much as God's Hatchet."

Similar examples could be given where it is a question of maximizing profit and not utility. Let me add as an aside that I should be astonished to find a beast who consistently satisfied the Weak Axiom and consistently violated the Strong Axiom. That beast has a nonintegrable preference field. While I can see why a man with a mind should exercise it consistently, I fail to see why a beast with no mind should satisfy the Weak Axiom or even consistency of demand choices. I am here applying Samuelson's Razor, which, unlike Occam's which is primarily aesthetic, is based on a lifetime of sad experience: All economic regularities that have no common-sense core that you can explain to your wife will soon fail. This cannot be said of all that you can explain to her; so my statement is not an empty one. It is considerations like this which make me think that the Alchian doctrine of survival adds something to the maximization hypothesis.

Almost all the remarks about the $S = \frac{1}{2}gt^2$ law for falling bodies that Friedman thinks support his thesis seem to me misleading. They could as well, or poorly, apply to a purely empirical theory that says: The first terms in a Taylor's expansion for motion of a body at rest released at $S = 0$ are of the form $S = 0 + 0t + \frac{1}{2}gt^2 +$ remainder. Galileo's simple theory, $S''(t) = + g$, has a subset of consequences that is in tolerable agreement with some facts; e.g., for t "small," $S''(t) = + g$. But it, B, is vastly inferior as every parachute jumper, golfer, and schoolboy knows, to B^* which says $S'' = - f(S') + g, f(0) = 0, f'(S') > 0$ and which correctly predicts $S'''(t) \neq 0$ and, $\lim_{t \to \infty} S'(t) =$ a constant; etc., etc.

To reject, as I was taught to do in Chicago, monopolistic competition on

the ground that it is not a "nice, simple, unified" theory like that of perfect competition, is like insisting that $f(S') \equiv 0$ because that is simpler and more manageable. If perfect competition is the best simple theory in town, that is no excuse for saying we should regard it as a good theory if it is not a good theory. To use the F-Twist to minimize its imperfections or irrelevancies is, as I have argued, simply wrong.

We must not impose a regularity—or approximate regularity—in the complex facts which is not there. Good science discerns regularities and simplicities that are there in reality—I almost said "out there." Epicycles are more horrid than perfect circles, but the ancient astronomers were right to abandon perfect circles and not say, "Well, even if wrong or imperfect, they are the best wheels in town."

Post-Copernicans were also wrong to go to the stake for the belief that Keplerian ellipses, B, were a more correct theory than epicycles, B^*. Relativism should have told both sides that this was a nonsense issue. Actually, B^* is merely a representation of B and deductively $B \equiv B^*$. However, to imperfect human minds the B^* formulation "looks" simpler and has the great mnemonic virtues of "economical description" which Mach rightly recognizes as the essence of good science. Mach has few friends today: physicists who confuse the psychological process of arriving at notions with the validity of those notions find him sterile. I should record that my experience with economics led me to notions that seem much like Mach's.

There is a final point, which was perhaps not made explicitly by Nagel, Friedman, or Mach and yet which I feel I share with Einstein and practitioners of harder sciences.

Experience suggests that nature displays a mysterious simplicity if only we can discern it. This is a bonus and need not have been so. And unrealistic, abstract models often prove useful in the hunt for these regularities. (Sometimes they prove misleading to a whole generation of searchers.)

This psychological usefulness should not be confused with empirical validity. Black coffee may be useful to physicists, mathematicians, economists, and artists. But coffee is coffee. Such abstract models are like scaffolding used to build a structure; the structure must stand by itself. If the abstract models contain empirical falsities, we must jettison the models, not gloss over their inadequacies.

The empirical harm done by the F-Twist is this. In practice it leads to Humpty-Dumptiness. Lewis Carroll had Humpty-Dumpty use words any way he wanted to. I have in mind something different: Humpty-Dumpty uses the F-Twist to say, "What I choose to call an admissible amount of unrealism and empirical invalidity is the tolerable amount of unrealism."

The fact that nothing is perfectly accurate should not be an excuse to relax our standards of scrutiny of the empirical validity that the propositions of economics do or do not possess.