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Information Transfer Economics

A working paper exploring the idea that information equilibrium between an information source and an information destination is a general principle for understanding the micro- and macroeconomic allocation problem. [\[Here\]](#) is a first draft.

Tuesday, November 1, 2016

Economics, physics, and data: a response to Blackford

The economist George H. Blackford [responded](#) to my snark-laden criticism of his article in Evconomics. In part, he accused me of not reading his work and links carefully or completely so I have been taking my time with my response. The format below includes quotes that I am responding to the substance of, or that represent markers for a few paragraphs that I am responding to. Just below the quotes, I give the TL;DR summary of my response ("too long; didn't read").



First, let me give the overall TL;DR it's own TL;DR: *Pick another target to criticize in economics. Don't make analogies with physics. Friedman's "as if" methodology is not the issue here, Dude.*

Here's the overall summary ...

TL;DR

Blackford makes a case that Friedman's "as if" methodology is the source of the "bad science" in economics, and I guess via the "bad begets bad" heuristic, much of the bad policy as well as the bad aspects of the economy. However Friedman's methodology isn't the only reason economists use rational agents, there are other potential sources of bad policy that aren't derived from the "as if" methodology (e.g. the EMH, the "Laffer curve"), and there exist rational agent theoretical explanations of stagnation and the financial crisis consistent with Friedman's "as if" methodology.



Macroeconomic data is so uninformative that despite not being a great description of the empirical data, mainstream macroeconomics (mostly New Keynesian DSGE models) hasn't actually been rejected by the data. Mainstream economics is "unscientific" (in my opinion) by being too complex (e.g. too many parameters) for the available data. However none of this has anything to do with Friedman's "as if" methodology.

Friedman's description of e.g. Galileo's law of falling bodies is actually quite excellent from this physicist's perspective for being written in 1953. His "as if" methodology is essentially an argument for [effective theory](#) that physicists wouldn't get to [the 1970s and 80s](#). While Friedman makes a few errors in his descriptions of physics in [analogies](#), there is nothing in physics that would discredit this general approach. Therefore any counter argument invoking Newton, Einstein, or science in general misunderstands or misrepresents them. The arguments against Friedman (which definitely exist!) should be confined to economics and economic data.

I am not defending conservative politics. I think [this quote from Paul Krugman](#) probably characterizes my view here:

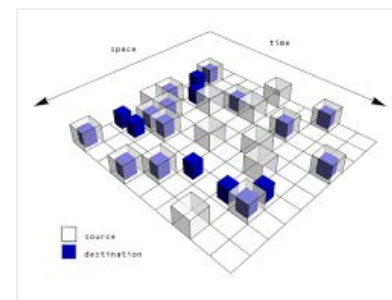


I'm on record declaring Friedman a "great economists' economist". His work aimed primarily at a professional audience — the permanent income theory of consumption, the case for flexible exchange rates, the natural rate (even if it does break down at low inflation), the optimum quantity of money — was often, maybe even usually, brilliant, and will live on.

What isn't living on, however, is Friedman's role as a guiding light for conservative economic policy.

TL;DR

Information equilibrium



Information equilibrium (draft papers)

- [Information equilibrium as an economic principle](#)
- [Utility maximization and entropy maximization](#)
- [Maximum entropy and information theory approaches to economics \(7th BPE conference 2016\)](#)

Predictions from the ITM

- [Aggregated prediction link](#)

Information equilibrium short course

- [Information theory 101](#)
- [Information equilibrium 101](#)
- [Standard economics from information equilibrium](#)
- [Information equilibrium as a framework](#)
- [The economic state space](#)
- [Economics as information theory](#)
- [How money transfers information](#)
- [General information transfer model for physical systems \(Fielitz and Borchardt\)](#)

Featured Post

[The economic state space: a mini-seminar](#)

I thought I'd curate a mini-seminar of information equilibrium blog posts chosen from over the past two years that draw on a particular ...

* * *

"Which side of this argument [about how scientific economics is] are you on?"

TL;DR "As if" = scientific, not comparing to data = unscientific, too complex = unscientific, adding things to theory despite not improving understanding of data = unscientific

I'm not really sure there is a dichotomy of "sides" here. There are a lot of attacks on economics that say it is unscientific, and there are defenses. There are attacks on string theory as unscientific, even by physicists like Lee Smolin. At the edges of understanding -- beyond wikipedia science (where everything has already been figured out) -- any field will have trouble meeting every expectation of being scientific.

In some ways, string theory isn't "scientific". There aren't very many experiments that test it (though there are some that test aspects of it), so it's not grounded empirically. In my post on this question (as an analogy with economics), I show that string theory is a natural extension of very successful theoretical frameworks (and contains them), so it's scientific.

Macroeconomics does not have any successful theoretical frameworks, so the "string theory defense" I make in my post doesn't apply to e.g. DSGE models. In fact, in comparison to string theory, macroeconomics has tons of data that it should be explaining. And that is where the failure to be scientific is. Macroeconomic time series only has a few thousand data points, though, so macroeconomic models need to be simple (have few parameters).

In the end, it's particular aspects of theoretical and empirical approaches that are scientific or unscientific. And my defense of Friedman against Blackford's argument was restricted to Friedman's "as if" methodology, which is identical to effective theory as practiced by physicists and so is pretty scientific.

However the lack of comparison of (mostly macro) economic theory with data -- data that exists -- is unscientific. So are the models that are too complex to be supported by the limited available data. In his Economics article, says that several things should be included (for example: inequality, debt accumulation) without any indication that they improve the theoretical description of the empirical data. I said this was like adding aether to physics. At the time, physicists felt in their bones light waves had to propagate in some medium. Not only was there no empirical evidence for this, but it actually led down the wrong path.

* * *

"My paper is about how Friedman's as if methodology is used in economics."

TL;DR Not all uses of rational agents are a result of Friedman's "as if" methodology.

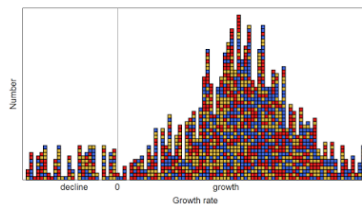
The purpose of Friedman's paper was definitely to defend the "rational agent" and "Chicago school" view of economics. And I agree that the modern perception in economics of Friedman's paper differs from how a physicist would look at it. For example, Noah Smith perceives the billiard player analogy as something very different from how I look at it.

In reading Friedman's paper, he seems to be saying, with regard to assumptions that:

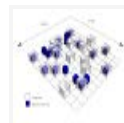
1. Unrealistic assumptions are not a problem if predictions are good
2. Predictions are more important than realistic assumptions
3. More significant theories will have less realistic assumptions
4. Judge assumptions by whether they are good approximations and lead to predictions

With the exception of #3, this is basically effective theory. And my defense of Friedman is restricted to this aspect.

Friedman does use his "as if" methodology to support his free market view of the world and rational agents, and part of that is in #3 -- something that is purely a matter of opinion. I'd say that if very unrealistic assumptions led to accurate predictions, then you probably have stumbled on a deep or emergent result that doesn't have much to do with the assumptions (Friedman appears aware of this possibility when he says "For all I know there may be other sets of assumptions that would yield the same formula." with regard to falling bodies in physics).

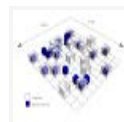


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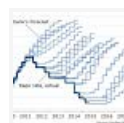
Scoping the Financial Times

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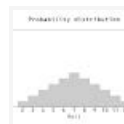
Light to no blogging starting a few days ago

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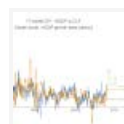
Thought experiment

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Utility maximization, matching and information equilibrium

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10% growthiness

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From my experience, modern economists interpret Friedman's "as if" approach to unrealistic assumptions as a defense of the use of rational agents. However not all uses of rational agents in economics follow from modern economists using the "as if" methodology.

For example, [here is David Andolfatto](#) saying that rational agents (underlying DSGE models) are just a starting point

"We are all scientists trying to understand the world around us. We use our eyes, ears and other senses to collect data, both qualitative and quantitative. We need some way to interpret/explain this data and, for this purpose, we construct theories (or hypotheses, or models, or whatever term you prefer). Mostly, these theories exist in our brains as informal "half-baked" constructs. This is not meant to be a criticism (as long as we recognize the half-baked nature of our ideas and why some humility is always in order). Often it seems we are not even aware of the implicit assumptions that are necessary to render our views valid. Ideally, we may possess a degree of higher-order awareness--e.g., as when we're aware that we may not be aware of all the assumptions we are making. It's a tricky business. Things are not always a simple as they seem. And to help organize our thinking, it is often useful to construct mathematical representations of our theories--not as a substitute, but as a complement to the other tools in our tool kit (like basic intuition). This is a useful exercise if for no other reason than it forces us to make our assumptions explicit, at least, for a particular thought experiment. We want to make the theory transparent (at least, for those who speak the trade language) and therefore easy to criticize. Constructive criticism is the fuel that fires the furnace of new ideas in academia."

Roger Farmer [argues](#) that we need DSGE (and therefore the rational agents underlying it) to understand general equilibrium. Simon Wren-Lewis [simply says](#) the rational agent DSGE models were an improvement over what had come before. DSGE models represent the current "mainstream" approach to macroeconomics, and are the modern incarnation of the rational agent approach. **They do not result from Friedman's "as if" methodology.**

* * *

"As far as I can tell, the basic paradigms of physics are not, for the most part at least, based on demonstrably false assumptions"

TL;DR They are, and the way that is understood is exactly Friedman's "as if" methodology.

General relativity is incompatible with quantum mechanics. Therefore one theory's assumptions are demonstrably "false" (we don't know which one yet, we think GR). The Standard Model does not have neutrino masses/neutrino oscillations, and is therefore demonstrably "false". Calculating the vacuum energy of empty space gives an answer that is off by 120 orders of magnitude, and therefore the assumptions of quantum field theory are demonstrably "false".

However no physicist really thinks of these in terms of true/false, but rather in terms of scope and scale. General relativity and quantum mechanics have different scope that only overlaps at energies higher than we can reach with experiments or near black holes. Neutrino masses are a very small correction to the Standard Model. Quantum field theory is limited to energies well below the Planck scale.

That is to say physicists deal with demonstrably false things by assuming any theory is an effective theory with limited scope. An effective theory is exactly Friedman's "as if" methodology.

* * *

RE: The discussion of Newton's laws

TL;DR At the time, the assumptions involved in Newton's laws had no independent truth value besides the retro- and pre-dictions of the Newton's theory.

Yes, Newton's version is hard to parse, especially using $F = m a$ as a frame (which is not generally true, even ignoring relativistic and quantum effects). The Latin is:

Lex II: Mutationem motus proportionalem esse vi motrici impressae, et fieri secundum lineam rectam qua vis illa imprimitur.

You quote one interpretation:

The alteration of motion is ever proportional to the motive force impressed; and is made in the direction of the right line in which that force is impressed.

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Jason Smith

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I am a physicist who messes around with economic theory. You can read more

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The modern understanding is:

The change of momentum of a body is proportional to the impulse impressed on the body, and happens along the straight line on which that impulse is impressed.

This is best interpreted mathematically as

$$I \equiv \Delta p$$

where I is impulse and p is the momentum vector. The instantaneous force is (by the fundamental theorem of calculus, therefore no assumptions of relationships in the world)

$$I = \int dt F$$

$$F \equiv dp/dt$$

where p is the momentum vector. The alteration of "motion" (i.e. momentum) is Δp (or infinitesimal dp), and the rest of the definition says that the F vector (and impulse vector I) is parallel to the p vector. Newton would have written in his own notes something like $f = \dot{x}$ using his fluxions (i.e. $f = dx/dt$).

As you could just work in terms of momentum p (which is conserved via Newton's 3rd law, although the conservation is actually considered more fundamentally a consequence of the universe having approximate Galilean invariance -- $SO(3)$ spatial symmetry plus velocity shear transformations) and its derivatives, defining force is unnecessary and mostly a notational convenience that helps with calculations.

However, what Newton "really meant" is still debated. And yes this is mostly semantics, but it is relevant background.

Blackford continues:

"... if the assumptions underlying Newton's law had been demonstrably false his law would not have been accepted by physicists in the way in which economists are willing to accept false assumptions in economics."

This misunderstands the scientific method at work here. Newton's laws were accepted because they organized previous work by Galileo, Kepler, and others into a simpler framework. Newton explains tides, orbits, the Earth's oblateness, and Kepler's laws using his three laws plus his gravitational law. It's the results of the assumptions, not the assumptions themselves, that are compared to the empirical data.

At the time, the criticisms of Newton were mostly philosophical -- and included rejection of Newton's use of Galileo's empirical approach that we now call science. We can't really know what would have happened if any of Newton's laws were "false". They mostly aren't except for quantum and relativistic effects, and at the time they were pretty much untestable themselves because they require doing experiments in a vacuum, and no one could test $F = m a$ by applying a force to the Moon.

Basically, in the 1600s and 1700s the assumptions involved in Newton's laws had no independent truth value apart from their results. They were accepted because they explained a lot of things with very little -- which is exactly what Friedman says:


"A hypothesis is important if it 'explains' much by little, that is, if it abstracts the common and crucial elements from the mass of complex and detailed circumstances surrounding the phenomena to be explained and permits valid predictions on the basis of them alone."

* * *

"It seems quite clear to me, even if it is not clear to you, that 'unrealistic' is nothing more than a euphemism for terms such as 'wrong' or 'false' or the expression 'contradicted by empirical evidence' in Friedman's arguments and as used by other economists as well. It's the term economists use when they don't wish to use the more explicit term or expression."

TL;DR If these terms are synonyms, then this leaves no room for approximations or analogies with physics. Additionally, if one understands these terms synonyms, then one will misunderstand Friedman's paper.

I can surely believe this is how some economists and other people use these words (from my

own experience), but if these words are all synonyms then any analogy with physics  they will not make any sense. Sorry, but I have to be a bit pedantic here.


"False" is either half of the Boolean or logical binary or as an adjective where one could use the *pseudo-* prefix. I am not sure "false" is ever really used in physics except in a generic narrative or as an adjective like "false vacuum". Happy to be contradicted. But physicists would never say Galileo's law or Newton's laws (assumptions) are "false" when they mean that Einstein's theories are better approximations when $v \sim c$. Newtonian physics and Galilean invariance are valid approximations under certain conditions.

"Wrong" could be a method that fails. For example, there are wrong arguments that lead to correct results (old quantum theory leading to the Bohr energy level formula, or this argument about spin-statistics). Or it could be that X is wrong because the answer is A and $X - A \neq 0$. This does not say anything about the magnitude of the error. Wrong can mean "way off" or "approximately right". Again, Friedman makes exactly this point.

"Unrealistic" is a relative term. But the unrealistic Einstein solid gives a comparable explanation to more realistic Debye model. Is it unrealistic to assume a surface is frictionless? That depends on how big a role friction plays. Basically, something can be unrealistic and approximately right. And again, Friedman makes exactly this point.

"Contradicted by empirical evidence" is, as the phrase implies, an empirical claim. In physics, this would have some scope attached. Using Newton's laws to calculate something involves assumptions contradicted by all the empirical evidence behind relativity and quantum mechanics (say, the survival of muons at the surface of the Earth produced cosmic rays). But physicists assume e.g. the speed of light is infinity as an approximation all the time.


To bring this back to economics, rational agents are both contradicted by empirical evidence (people fail to optimize the public goods game) and not contradicted by empirical evidence in (the field experiments of List 2004).


Does this mean the rational agent assumption is both right and wrong? Realistic and unrealistic? True and false? In physics, we'd say it is an approximation that works in some cases, but not others. Again, Friedman makes exactly this point. 




If you take these terms as synonyms, then it would not only be impossible to understand physics, but additionally impossible to understand Friedman's paper. Friedman is obviously not using these terms as synonyms because he makes direct statements about the degree of error and how close a prediction is to the empirical evidence.


* * *

"mainstream economists justified deregulating the financial system 'on the basis of an economic theory that assumes speculative bubbles cannot exist'"

TL;DR The EMH is the source of  push for deregulation, not "as if" methodology

I would be fine with this if you added "some" in front of the "mainstream economists"  some mainstream economists study speculative bubbles and financial crises (I offered Reinhart and Rogoff as an example).






However I don't think Friedman's "as if" methodology is the source of the push for deregulation, but rather the efficient markets hypothesis. The EMH is the reason people believed speculative bubbles can't exist (because the best available information is  included in the price). And the EMH was directly based on empirical data (prices following  random walks). However, the push for financial deregulation was all Fama and no Shiller -- Shiller's additional empirical studies noted deviations from the EMH 


Actually,  it seems to be the failure of Friedman's "as if" methodology that lead to this interpretation of the EMH. Instead of saying markets behave "as if" information was included in asset prices even though it might not be, the deregulators treated the EMH as saying that information was definitely included in asset prices.

* * *

"gratuitous accusations and innuendo"

TL;DR These quotes are actually serious points, and Blackford avoids them by essentially calling them *ad hominem* attacks. A few are derived from a section I blocked off that was about heterodox economics in general, not Blackford specifically.

I stand by the ments I made the list of quotes Blackford cites. He points to the 2008 financial crisis as vidence of the failure of "mainstream economics". That literally is a single data point. Mainstream economists Reinhart and Rogoff studied [hundreds of financial crises](#), and they come away with a far more nuanced conclusion that more financial regulation is required, not the complete failure of "mainstream economics"  Keynes. They also don't mention Friedman's "as if" methodology.  

At this point I have no idea what Blackford  means by "mainstream economics". It seems to more closely hew to so-called "freshwater economics", however the [predominant consensus view in academic economic community is closer to saltwater economics](#). In fact, if instead of "mainstream economics" he had said "economic theories supported by Republicans, libertarians, and conservatives", I might have had no problem with his article except for the bit about physics and effective theory. Milton Friedman is a definitely source of "Republican economics" ([but not anymore](#)), and many a conservative ideologue has been unscientific. This confusion is what I mean by the "neoclassical slant Blackford ... conflate[s] with mainstream economics".

But even if we restrict ourselves to "freshwater economics", there are explanations of e.g. stagnation or financial crises that appear consistent with the data to the level of Blackford's own narrative explanations of the data (more on this below). Specifically, since financial crises and recessions are unpredictable in versions of that view, the 2008 financial crisis is not evidence of its failure.

The remaining quotes Blackford cites are derived from a separate section (blocked off with asterisks) that is about alternative approaches to (i.e. heterodox) economics in general. Not everything applies to Blackford.

I used the word "positive" in the sense of "consisting in or characterized by the presence or possession of features or qualities rather than their absence". As in giving an example of a scientific approach, rather than presenting "negative examples" of things that aren't scientific approaches. It was an objective adjective, not a normative one.

I included the term "crackpot" in my description of Carroll's alternative science checklist. I actually refer to myself as a "crackpot" many times, and to Carroll's list as the "crackpot checklist" which I strive to follow (e.g. [here from a few years ago](#)). I think Blackford's interpretation of how science operates is incorrect, and not all uses of rational agents derive from Friedman's methodology. However I don't think Blackford or his economic ideas are "crackpot". I actually agree with most of the social and political implications, and the narrative he tells (in his links) is plausible given the data (more on this below). Again, this is in a separate section directed at heterodox economic in general.

* * *

"If you had bothered to look at these references ... it would have become apparent to you that I have spent an excruciating amount of time and effort in examining data and attempting to develop a theory that explains that data"

TL;DR I did read the references and there is no theory there that explains the data that is relevant to Friedman's "as if" methodology.

Ah, the classic "you didn't read my references". I actually did look at the references, but there appears to be no theoretical model of data at any of the links. There is a graph of some data in [Ideology Versus Reality](#), as well in the links to chapters of his eBook in the Evconomics post, but that is not theory explaining data. Blackford seem to be telling a story that appears to be plausible and/or consistent with the data. However, what are the magnitudes of the effects of deregulation, inequality, and/or debt accumulation? Can I predict anything? We don't know because Blackford's theory is generally not a mathematical theory, and where it is (e.g. [here](#)), it is not compared to empirical data.

It is true that as a physicist, I am biased towards mathematical theory, where most others think of "theory" as representing non-mathematical ideas from "critical theory" to Darwin's theory of evolution (which despite the popular impression, does actually have many quantitative mathematical aspects e.g. [here](#) or [here](#)).

Regardless of one's opinion of mathematical theory in economics, Friedman's "as if" methodology was definitely talking about mathematical theory. He talks about formulae, predictive error, approximations, and comparing the results of formulae the data.

And that's a problem. Blackford is presenting a narrative (non-mathematical theory) as a counterpoint to the mathematical fruits (say, DSGE/RBC models) of Friedman's arguments about mathematical theory. It's a category error. One needs a mathematical theory that does better than the fruits of Friedman's arguments about mathematical theory in order to show the failure of

those fruits. However, most mathematical theories seem to suck (ironically, the EMH based theory of a random AR process does best at forecasting).



But regardless of how much mathematical theories suck, they're still the only thing that can be quantitatively compared to numerical data in economics (interest rates, output and growth). Narrative explanations of economic history, from Keynes to Blackford, from Milton Friedman to Scott Sumner, are generally created by intelligent people and human brains are remarkably adept at creating narratives out of the noisiest data.

And it would be a mistake to assume other narrative theories don't have explanations following from experimentally examining the exact same data set one is using to construct one's own narrative theory. Part of the reason a lot of neoclassical or "freshwater" economics still exists is probably because the narrative versions are completely consistent with the data.

This is why I like math. I mean math. Comparing theoretically calculated quantities to empirically measured ones is concrete in such a way that you can at least tell when something is wildly wrong or approximations fail. Physicists have been doing since Newton, but economists have generally only been doing this since Samuelson so it may take awhile to catch on. Additionally, math lets other people use your theory. I can't use Blackford's theory, but he could easily use mine.



Blackford also claims that his references support his claims about the financial crisis representing the failure of mainstream economics, however at the links there are simply more details about the claims, not supporting evidence. To his credit, Blackford does at least mention Reinhart and Rogoff:

"At the same time the most serious depressions involve financial crises that have at their root the inability to service debt. [Reinhart and Rogoff]"

However this statement of part of Reinhart and Rogoff's thesis is about **sovereign debt crises** (e.g. the European debt crisis, or the Asian debt crisis), and has nothing to do with the US financial crisis, Milton Friedman's "as if" methodology (he's not popular in Europe), or "freshwater economics" (Blackford's "mainstream economics", which is not as prominent outside the US).

* * *

"... you would not have so flippantly assumed that adding government expenditures to an economic model is all you have to do ..."

TL;DR Reading comprehension fail.

I explicitly did not assume that. After quoting Blackford:

"Is it any wonder that this [economic] paradigm ignores the relevance of the essential role of cooperative action through democratic government?"

I said:

"I'm pretty sure that government spending is discussed as part of mainstream economics. ... Is this supposed to be some kind of other role?"

My point was not about government spending being sufficient; I ceded that government spending was one role of government that has been empirically validated. I was asking a question. What role of government besides government spending does Blackford want to add to economic models that isn't studied by mainstream economists, and what improvement does this make in those models' explanation of the empirical data?

There is no answer to this.



* * *

"... you seem to view the economic system as something comparable to a physical system that can be understood and explained by developing equilibrium models that accurately predict economic data. I see economics as much more than this."

TL;DR This is irrelevant to Friedman's "as if" methodology



Let me just rewrite this in a way that more accurately characterizes my view.

The economic system is at times comparable to a physical system that can be understood and explained by developing models that accurately predict economic data, but at other times this understanding just represents a bound where no rational answer exists. I've put this in the

[context](#) of Keynes Book 1 versus Keynes Chapter 12 before.


However it makes no sense to talk about economic theories that aren't about accurate predictions when Friedman's "as if" methodology is entirely about ignoring the assumptions of economic theories when they make accurate predictions.

* * *

"I see economics as the study of a vital part of our social system ..."

I have no problems with anything said in the closing paragraphs. However it is critical to Blackford's view that one is correct about how social and economic systems operate. And in order to determine if a theory is correct, its claims about empirically measurable quantities have to be tested. Blackford's theories make claims about empirically measurable quantities (e.g. unemployment and interest rates).

Posted by [Jason Smith](#) at 6:00 PM

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