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The fiction of verifiability in economic "science"

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Abstract
Today's economic theory is unverifiable. The argument justifying the claim is as follows. Economic theory makes predictions about equilibrium positions. To verify such predictions, we need equilibrium data. Since, hitherto, we have no way of knowing if the data we use in empirical work is equilibrium data, all tests that have hitherto been conducted to verify economic theory are non sequitur.

Key words methodology, economic statistics, econometrics

JEL classification B41, C00, C01

I. Economic theory is unverifiable

The central argument of this article can be stated in a few sentences. Let me provide it sharply to begin with, and then add the requisite qualifications. The proposition is "Today's economic theory is unverifiable".[2] The argument justifying the claim is as follows. Economic theory makes predictions about equilibrium positions. To verify such predictions, we need equilibrium data. Since, hitherto, we have no way of knowing if the data we use in empirical work is equilibrium data, all tests that have hitherto been conducted to verify economic theory are non sequitur.

As the argument is short and elementary, let me break it down into its component parts.[3]

- Economic theory makes predictions about equilibrium positions. This is common knowledge and requires only a brief review.
- To verify such equilibrium predictions, we need equilibrium data. This is the central inference I draw from the above. It is both immediate and obvious.
- We have no way of knowing if the data we use in empirical work is equilibrium data. This is a practical point, only to be judged by the looking at the data generating process and the data collecting procedures.
- Hence, all tests that have hitherto been conducted are non sequitur.

There is something indecent in accusing an entire profession of engaging in a global non sequitur. Good manners require that one at least address the question: how did we get into this mess? Here is my best guess. The early empirical studies were based on agriculture, a field where there used to be only one annual major crop. Once the harvest comes in and the size of the crop is accepted, prices reach equilibrium fairly fast and stay predictable for a while. Those who reported the price had enough sense to let the market settle down before

[1] I am grateful for helpful comments to Anis Chowdhury, M G Quibria, and M A Taslim as well as the participants at several conferences: Illinois Economic Association (2014), INEM (2015) and the SEA (2018). All errors are mine.
[2] With the corollary that "economics cannot claim to be a science", in the sense of a systematic study capable of empirical verification and accurate prediction.
[3] I will make many critical comments on the profession, so let me begin by saluting two individuals who were steadfast in their intellectual integrity: Franklin M. Fisher and the late Zvi Griliches. This essay is meant to show my respect for them.

reporting the price, which was thus a plausible equilibrium price. In such a world, it makes sense to think of the market price as the equilibrium price.

Section II goes over the above claims more carefully. Logically, that ends the paper. However, it is intellectually unsatisfactory to note incompleteness in an accepted argument and place a burden upon others without providing some concrete reasons for doubt. Section III addresses this point by examining the law of one price. Section IV suggests that the doubts expressed earlier should give particular qualms to macroeconomists Section V is a light-hearted look at the burden placed upon the "optimising agents" who are supposed to ensure equilibria. Section VI summarises and concludes.[4]

II. The argument amplified

Economic theory provides predictions. Such predictions arise from comparative statics or CS. If the prediction is qualitative, then the confirmation is weak e.g. "Tighter supply leads to higher prices and lower sales". If this is all that can be said, we can join company with every milkman in producing "science". It is a very low bar, and familiarity with historical documents will show that such "science" was known to multitudes of illiterate humans, such as tribal chiefs in Africa and peasants in India. There is a school of economics, the Austrian school, who claim that only patterns can be predicted, (much as in biology) and that knowledge of such patterns suffices to make economics scientific. Hayek is probably the best known proponent of such a view. Arguing about what is *really* science is pointless and needless for my purposes. Mainstream graduate economics is not based on such views of science as patterns – so I will not engage with such a view here.

"Graduate School Science" demands that a prediction be exact. For example, "A 10% decrease in supply causes price to rise by 8%." This claim is obtained through Comparative Statics or CS. Predictions relate to *equilibrium* values. The prediction quoted above, "A 10% decrease in supply causes price to rise by 8%" is loose. A more exact version of the prediction will state that "If the market is initially in equilibrium, and if it reaches a new equilibrium, then a 10% decrease in supply causes price to rise by 8%." The method is called "comparative statics" because it compares positions of equilibrium, so the claim that predictions in economics refer to equilibria should not require elaboration.

To test such a prediction, we need equilibrium data. i.e. *The numbers used for testing **must** be equilibrium values.* If we do not provide assurance that the data used for empirical tests are actually equilibrium values, our tests are sub judice or non sequitur. Are the data currently used for testing at all relevant? Strictly speaking, NO – unless we have devised some tests for data to be equilibrium data, and the data to be used for testing have passed those tests. To claim that economic data have equilibrium properties, we need to show that data collection is based on an understanding of what will constitute "equilibrium" for each type of data collected. No one has engaged directly with this question, though many have expressed unease about the relationship between economists and data. Most data are collected for administrative

[4] The thoughts presented here have been in my mind since 2007 (see footnote 5 below). Thinking I must have missed something essential, I wrote to many economists. Only Ed Leamer was kind enough to acknowledge that I had a point, but he doubted its empirical significance. To my mind, we can only answer the question of empirical significance if the subject is properly studied, rather than being ignored, as at present. By 2013 I decided that no one would give me a direct reply, so I returned to the issue and started presenting my ideas at seminars. I hope this explains the patchwork manner of references given here.

purposes – imports, exports, taxes etc.; perhaps accounting needs would be a more exact characterization of the data collecting process. What bearing do such collections of numbers have on economic equilibrium?[5]

Can we *never* claim our data to be equilibrium values? There is only one case I can think of in which we can avoid the criticism of testing with data of unproven relevance. It consists of the assumption, (or, better yet, demonstration), that *all* data are equilibrium data. Such a move would "save the day" by making any and all data ever assembled to be relevant for *some* prediction. This is the *hara kiri* gambit. Let us apply the perpetual equilibrium argument to the oldest question in economics – demand and supply. Demand and supply curves are determined by given parameters for the exogenous variables.[6] Given such values, demand and supply curves become well-defined, and equilibrium price and quantity are known. Since prices and quantities are endogenously determined, and everyday observation shows us that they are visibly moving, the *hara kiri* gambit entails the claim that exogenous variables are in some form of continual motion. Those who are willing to accept the last characterization may have trouble explaining why one is interested in separating endogenous from exogenous variables.

Strictly speaking, the above is a complete, but negative, argument. The burden of proving that the economic data being used are in fact equilibrium data is not mine, but that of those who use such data. Using the perpetual equilibrium assumption solves the difficulty, but it does so by fiat. Since it is making a claim about real world data, the *hara kiri* gambit denies any virtue to the price system, which has been considered an equilibrating device par excellence for centuries. According to *hara kiri*, there is nothing to find, since equilibrium is always there. On the other hand, if equilibrium is not true everywhere and always, then those who use data are required to demonstrate why the data that they do use has any relevance for testing equilibrium positions.

III. The prevalence of disequilibria

This section provides some reasons for doubting the assumption that our data are equilibrium values. It arose from an inability to verify a simpler version of equilibrium, the law of one price, or LoP hereafter.

- The law of one price states that the same commodity cannot have two prices in the same market.
- The claim sounds trivially true.
- Now look at all the papers that have been written to empirically test LoP – at least a half, perhaps two-thirds, claim that the law is not confirmed.
- In an earlier note[7], I showed how the failure of LoP occurred at several levels of aggregation by tracing the failure of LoP though successive levels of disaggregated models.
- Whether or not the law is "true", in some exact (ontological) sense yet to be determined, is beside the point. Can LoP be verified *with the data available to us*? This is the real question.

[5] Of all the papers I have read, Fischer Black's charmingly iconoclastic presidential address comes closest in spirit to my message here.
[6] Such parameters need not be scalars, as when they represent "taste" or "technology".
[7] Rashid, 2007.

Failure to verify the LoP is sufficient for my claim, which states that the data we have at hand, which we use every day, and which we have been using, do not justify being considered "equilibrium values"[8]. In one of the most important papers written at the start of the modern quantitative revolution in economics, Trygve Haavelmo laid down some guidelines which point directly to my claims[9].

> "The economist… often has to be satisfied with rough and biased measurements. It is often his task to dig out the measurements he needs from data that were collected for some other purpose;… his task being to build models that explain what has been observed. The practical conclusion of the discussion above is… that one should study very carefully the actual series considered and the conditions under which they were produced, before identifying them with the variables of a particular theoretical model."

Once we recognize that the testing of economic theory requires comparative statics, hence *equilibrium* data, Haavelmo's caution about studying "very carefully the actual series considered and the conditions under which they were produced", leads directly to my point about the need to test our data for LoP.

Let me repeat, the responsibility for establishing the equilibrium property of the data is not mine, but of those who use the data for testing equilibrium theory. However, the widespread failure to confirm it is an intellectual curiosity worth some discussion.

Since LoP fails in a multitude of cases, we have to say that at least one of its underlying assumptions fails. But LoP is based on:

1. Greed
2. Homogeneity of goods
3. Speedy move to equilibrium

The literature to date has focused upon the fact that almost all data are aggregated to some extent, hence the culprit is point 2, the homogeneity of goods. A small number of papers have addressed point 1, which consists of showing that small optimization errors can have large effects.[10] I turn now to the questions raised when we consider the speed of convergence.[11]

LoP can fail tests if there is no equilibrium, or if equilibrium is reached in a slow or fluctuating manner. How bad is the potential failure of LoP due to such phenomenon? The difficulty here is that once one turns a skeptical eye to the literature of applied economics, examples of disequilibria that continue through periods of data collection – which is the relevant standard for this question – seem to be all around us. What empirical economists seek are markets where prices converge quickly *and* monotonically to their equilibrium values. Even if prices converge quickly, but do so with violent oscillations, we will have trouble relating the measured value to the equilibrium value. Applied economists acknowledge the importance of disequilibria and of rates of convergence and frame their research to avoid issues created by

[8] Section 3 of Rashid (2007), provides evidence for the claim that the LoP is confirmed only for a very limited class of commodities, even after disaggregating to the maximal extent we appear capable of.
[9] Haavelmo, 1944, p. 7. There are further relevant observations on pp. 15 and 16.
[10] Akerlof & Yellen, 1985.
[11] I once thought of estimating speeds of convergence in individual markets, with finely defined goods, but correspondence with some very helpful BLS staff persuaded me that such an effort would probably be inconclusive. The details are given in Appendix A.

oscillating prices or by disequilibria. But they do not seem to appreciate that their bread and butter tools of numerical evaluation are directly affected by disequilibria in the macro markets whose impact upon the market they are studying cannot be ignored. Consider the three principal prices used in applied economics, say benefit-cost analysis: wages, interest rates and exchange rates.[12]

- Wages have a large "human" component and adjust slowly in many, if not most, cases.[13]
- Exchange rates, also often "managed", can have "perverse" effects, as in the J curve, before reaching new equilibria.[14]
- Interest rates are not primarily market driven, but anchored by the Fed.

The contrariness of price convergence is compounded by looking beyond the standard models and institutions, which do not assume the necessary "stability" of our models or the force of profit maximisation.

- Chaos and catastrophe theory are two alternative ways of looking at the world, both are quite different from the neoclassical view, and each provide so many examples of non-monotonic convergence that one is at a loss to pick a "favorite" example.[15]
- We cannot rely on profit to provide speedy adjustments in those sectors that are not based on profit maximization. Few will argue that Government functions like a competitive enterprise while the Health sector has many profit seeking enterprises but is rife with the problems detailed in the pioneering paper of Arrow (1963) What is the force leading to convergence when profit is not providing the energy? Such sectors, whose combined share of GDP is about 50% in the USA, or half the economy, will confound any claim that our data are equilibrium data. Neither Government nor Health, considered as individual sectors, can be expected to provide the data we expect from profit maximization; furthermore, as these sectors are large, their general equilibrium effects upon the economy can be significant.[16]

The widely observed fact that many investors are infrequently active traders is the theme of Duffie's Presidential address. It leads to the "key implication" that supply or demand shocks must be absorbed on short notice by a limited set of investors. Since shocks have to be absorbed by a limited number of traders, prices move excessively initially. "As a result, the initial price impact is followed by a gradual price reversal".[17] In some markets, such as that for catastrophic insurance, these price reversals can occur over several months. In explaining such inattention Duffie concisely states:[18] "A simple explanation is that trading takes time away from valuable alternative activities." The ambiguity of this statement needs exploring. If

[12] The literature on both wages and exchange rates is vast, and I have given only a few references for illustration.
[13] Jardim et al., 2019. This paper provides a welcome empirical antidote to the literature suggesting rigid wages, but it does not dispute the point needed here, i.e. slow adjustment. Further evidence on this point comes from Grigsby et al., 2019 and Hall & Kudlyak, 2019.
[14] The estimated half-life to convergence, not full convergence but just half the distance, is estimated at over a year (Bergin et al., 2017).
[15] Rosser, 2000.
[16] If William Lazonick's careful arguments about corporate organizational form being a device to increase control by owners is correct, then even the profit maximizing thrust of corporations is put in doubt, and with it the force of profit maximization as validating equilibrium based on market fundamentals (Lazonick, 2017)
[17] Duffie, 2010.
[18] Ibid. p.1238.

the more valuable activities are economic then the neoclassical model holds, but if the more valuable activities are social or cultural, then a substantial concession has been made, especially since a telling example of slow trading occurs when traders took an hour off to listen to Tiger Woods' televised confession of infidelity.[19]

That the problem of quick and monotonic convergence is probably of fundamental import can be gauged by looking at one area where the LoP should be rapidly validated – financial markets. In financial markets prices move rapidly, the goods are homogenous, and traders are clearly motivated to maximise their wealth. Nonetheless, many clearly documented cases of anomalies appear. Since markets reward those who make the most money, one would expect the actions of superior managers to be particularly profitable. Daniel Kahnemann provides some striking evidence to the contrary.[20] One would also expect that profit making opportunities cannot exist much beyond the span of time needed to exploit them; Richard Thaler has been carefully documenting cases where profit seekers appear to lie waiting.[21] In conclusion, note the disturbing fact that some of the failures of speedy market convergence are the result of the very institutions that perform arbitrage and whose actions are supposed to make LoP true.[22]

A significant part of the problem with the usual defense of markets lies in the method by which economists can simply claim "if a profit can be made, someone will make it". We are never told who this someone is or how they will even know that the opportunity exists. In the language of mathematics, all such proofs are not "constructive", i.e., they do not provide a step-by-step account of the process by which profitable opportunities are realized. For a while, market believers claimed that markets "see through" accounting forms, hence different accounting schemas cannot stop market efficiency. However, many of these same market believers lobbied hard, and succeeded, in getting accounting procedures revised, so as to be more amenable to investors. This activity tells us that accounting forms do matter. But if so then need to understand their construction and use as well as in the self-interest of the actors involved. Larry Cunningham and Stephen Penman have been criticizing current accounting practice on just these grounds.[23]

For data to be relevant for tests of equilibrium theory the requirement imposed upon empirical economists is to find data which result from fast *and* monotonic convergence. We not only need to claim that prices move rapidly to equilibrium, but also that the path is rapidly damped. If prices can have large fluctuations even as they get close to equilibrium, then nothing has been solved. The convergence of prices to equilibrium has to be both fast *and* monotonic for the data to be relevant for testing economic theory. Our confidence in the data generating process arises because we believe in the force of profit maximization, in the impact of competition, and effective arbitrage. But even if we believe that competition will move us *towards* an equilibrium, nothing tells us *how fast* we will be so moved. An equilibrium reached in a decade is of minor interest while an adjustment that is quick but very volatile also makes it impossible to use observed values as equilibrium values. Franklin Fisher stated the difficulty beautifully.[24]

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[19] In itself a telling commentary on the cultural homogeneity of this group of profit seekers.
[20] Kahneman, 2011.
[21] Thaler & Ganser, 2015.
[22] Maćkowiak & Smets, 2008, pp. 7-8.
[23] Penman, 2002; Novak, 2008; Cunningham, 2005. I am grateful to Ehsan Feroz for his advice on these issues.
[24] Franklin Fisher, Disequilibrium and Stability, pp. 75-76.

> There are two fairly common mistakes that must be avoided in considering such matters. First, one must not confuse the fact that the economy will move away from positions that are not equilibria with the much deeper and unproven proposition that the economy always converges to equilibrium (let alone the proposition that it spends most of its time near equilibrium). In more specific terms, the fact that agents will seize on profitable arbitrage opportunities means that any situation in which such opportunities appear is subject to change. *It does not follow that profitable arbitrage opportunities disappear or that new opportunities do not continually arise in the process of absorbing old ones.*

In an earlier paper, I claimed that the failure of LoP is of more significance than is generally believed without clearly stating its nihilistic implications for empirical economics. Now I want to refine and strengthen the earlier argument. If LoP does not hold, then it is probable that equilibrium is not reached, and *if equilibrium is not reached, how do we relate our models to data?*

IV. "Testing" macro

Macroeconomic theory is based upon the belief that 1) aggregates can be usefully reasoned upon, and that 2) there are important instances where the properties of an aggregate cannot be deduced from the behavior of its parts. Applications of Macroeconomics to policy implicitly requires the data to be equilibrium values. This last assumption, that Macro data are equilibrium data, is one that has simply not been questioned or tested to my knowledge. The problem is particularly acute for Keynesian macro. We can build models with aggregates such as C, I, G, X and M, and then fit these models to the data, but when we make predictions, are these not about equilibria? Since almost all macro discussion has been conducted with annual data, have we not implicitly claimed that macro variables reach their equilibria within the year and that only one such equilibrium is reached annually. If several different equilibria were attained during the year, which of these should we use as our datum? Implicit in our arguments on relating macro-models to data lies an assumption about the way data is generated and how it is collected.[25]

How is one to understand Macro equilibria in terms of observable data? Macroeconomics deals with many sectors and one wonders if it needs something like uniform convergence across sectors for its empirical claims to be acceptable – what if one sector only reaches half its equilibrium in the data period?[26]

[25] Haavelmo, 1944. I found nothing on the same lines in Frisch or Tinbergen, but it needs pointing out that Haavelmo thanks Frisch for many ideas.

[26] Having taught CGE modelling for many years, I tried to modify the price algorithm we use by slowing down price adjustment in some sectors, by 80% for food and by 90% for agriculture, and then introducing an exogenous policy change. Suppose the lagged model is called B and the original model A. Of course model B took longer to converge than A. To my surprise, the distance from equilibrium prices for B, after letting the model run for the number of iterations needed to reach equilibrium in A, was less than 5%. If the real world is as simple, with equilibria as reliably unique, then speeds of convergence may not matter much. I am grateful to Hadi Esfahani for having introduced me to CGE modelling on Excel.

Services, in particular, raise many questions of delimitation and counting, known to us at least since Hill (1977).[27] Readily available data is often unusable as an equilibrium proxy – sometimes because it is unreliable, but more often because it is an aggregate or index whose "make" is not transparent. The more accurate, basic, data, the source of the index that is publicly provided, is confidential, inaccessible, and perhaps incomprehensible to ordinary economists. The problem is so prevalent that it is non-negligible, even if it may not be systemic.

Many, perhaps most, economic variables are affected by expectations. Why does an announcement by the Fed, or even a casual remark by its Chair, give rise to changes in prices? Similarly, the January announcements of profits by corporations leads to noticeable buying and selling – phenomenon that are to be expected if agents are widely seeking information. Any equilibrium has to be based on a set of expectations and the expectations are derived from projections of data. But since these projections are defensible only as bounds and not as point values, as argued forcefully by Charles Manski in his critique of the widespread but unjustifiable desire for "incredible certitude", it follows that the unique equilibria needed by theorists for comparative statics either do not exist, or, if they do exist, are normally unknowable from the data.

One of the widely agreed upon lessons of the GFC of 2008 is that finance must be integrated within macroeconomics for macro to achieve relevance. A worthwhile goal, but can it be rigorously achieved? Eugene Fama is generally respected as one of the founders of modern finance and Nobel Laureate for 2013. In an interview with Joel Stern in 2016 Fama has some very discouraging observations.[28] After dismissing the literature on anomalies and giving guarded respect to Richard Thaler behavioral critique, Fama goes on to assess the rigor of modern corporate finance. Fama's emphasis upon rigor needs to be understood and his honesty applauded. The entire quote is too long to be provided but Fama tells us he is not allowed to teach corporate finance, "because my view is that what we teach our students has very flimsy theoretical underpinnings". Fama then elaborates

> "Take DCF analysis, for example, which comes out of the perfect certainty world of Irving Fisher. My first objection is that we don't live in a perfect certainty world. But let's push past that problem. We then have the problem of estimation. So, let's suppose the sky opens and a voice tells us that CAPM is the right model, and all we have to do is to estimate beta. The problem here is that, for individual stocks, the estimate of beta is garbage. Even for an industry, there's no hope of estimating beta because it's too dynamic through time. Ken French and I wrote a paper that showed you're no better off using two years of data than ten years of data because there's so much movement. But now let's suppose that the sky opens again and the correct beta comes down. Now, all you have to do is to estimate the market premium. But all we can really say is that it's a number somewhere between 2% and 10%, and we have very little basis for settling on a particular estimate. And yet the number you choose will have a dramatic effect on your results."

A major problem is that Finance must consider asset values and this is a beast yet to be tamed. Fischer Black claimed long ago that asset values depended on several indeterminate

[27] Hill, 1977.
[28] Fama & Stern, 2016.

variables and would perhaps always be subject to estimation with a wide margin of error.[29] Vernon Smith found, to his surprise, that subjects who were able to get close to the equilibrium when participating in goods markets, were prone to produce wide swings and bubbles when dealing with asset values.[30] When we combine both points – Black telling us that even experts work in a fog when estimating asset values and Smith providing experimental evidence about the proneness of humans to miscalculate asset values – it appears that we have little hope of getting asset values "right" in some objective sense.

It is no wonder that new models seem to be continually proposed to explain the observed anomalies in asset and in financial markets.[31] The most empirically relevant is perhaps that of Fostel, Genakopoulos & Phelan,[32] which treats a question of increasing importance at a time of greater global financial integration – cross-border financial flows. They show that such flows can arise when otherwise identical countries differ in their abilities to use assets as collateral to back financial contracts. Due to a resulting gap in collateral values, financially integrated countries can have access to the same set of financial instruments without producing price convergence for assets with identical payoffs. The price divergence will produce financial flows which can amplify asset price volatility in both countries. Unless the countries adopt the same institutions and legal characteristics it is hard to see how collateral value equalization will be attained.

For empirical Macroeconomics to be plausible, we need data which are collected while parameters are stable and after equilibrium is reached. This involves more than one assumption, so let us call the joint assumptions the Fundamental Assumptions of Measurement. Once the implications of these assumptions – that measured values are taken to be the equilibrium values generated by stable systems – are explicitly spelled out, some obvious questions arise about the acceptability of such assumptions:

1. What if parameters change in the time it takes us to reach equilibrium?
2. What if data is collected in time periods too short for equilibrium to be reached?
3. What if the categories being used for data change before data collection is complete or before equilibrium is reached?

Since many useful applications of economics are occurring every day, surely the situation cannot be as chaotic as implied by the above questions about data and equilibrium?[33] Agreed, but this may be because those who engage in policy readily modify or select data to represent reality – just as those who reported crop price 200 years ago waited till the harvest was known and crop prices could be assumed to have settled.[34] This recognition of "dirty detail" and its importance is missing in neoclassical economics. Those who have participated in policy discussions generally attest to the fact that all the interesting debates occur are about institutional details which seem too petty for theory. We manage not only because we know a lot more than our theory tells us, a la Michael Polyani's views on "implicit knowledge",

[29] Black, 1986.
[30] Noussair, 2017.
[31] Guo & Wachter, 2019 consider an economy in which investors believe dividend growth is predictable, when in reality it is not. They show that a wide variety of evidence can be explained with this hypothesis and furthermore that are "rational" when confronted with evidence.
[32] Fostel, Genakopoulos & Phelan, 2019
[33] It is not directly germane to this paper, but in looking at the unexpected regularities of some forms of data, I am driven to the observation that different levels of aggregation may lead to different empirical "laws", meaning regularities which we cannot be theoretically comfortable with, but which are adequate for practical purposes (see note 28 also).
[34] As one observes Working (1925) when he is selecting the data to use for demand studies.

but also because we tacitly allow such practical knowledge to supplement or even override our theoretical framework.

V. "Cognition" for optimisation

Modern *homo economicus* is possessed of "greed" to an extent scarcely recognized in the past. They are optimization machines whose internal functions are unobservable but whose effectiveness is absolute. "Knowledge" for such people requires "knowing" of a peculiar sort. "Knowing" has two parts here. First, the agent has to know all the relevant facts, in the sense of being aware of them and being able to assimilate and store them. Thereafter, the agent has to be able to process and deduce the implications of the knowledge acquired. Thereafter, there is a third step wherein the agents actually execute the optimal plan. This requires self-control and will power that psychologists have shown to be not only demanding but infeasible for most of us. Since two Nobel prizes, to Kahnemann and to Thaler, have made the last point, among many others, very effectively, it is needless to discuss their import.

The finite limits of our cognition is an important point, which has been inadequately stressed. Our agents need to accept, store and assimilate very large amounts of information. Computer scientists can perhaps address the reasonableness of this assumption. Thereafter, "cognition" requires deduction and our rational agents are capable of very large amounts of inference. I once heard Larry Samuelson joke about chess players refusing to play because of Zorn's lemma. It seemed a most effective retort to the idea of perfectly informed agents. Chess players are highly motivated to win – I have read that Grandmasters put in so much effort that they can lose up to 10lbs in a match. If infinite cognition made any sense, why do the chess players not just refer to Zorn's lemma, claim that such deterministic games have a solution, then shake hands and award the prize?

Indeed, the whole history of mathematics has been a waste of energy. Once Euclid wrote the assumptions, all of Geometry was known to such infinitely competent inference machines. Perhaps the rules for manipulating integers were known before Euclid. In which case, all of Number theory was already common knowledge before Euclid. There was never any need to teach mathematics – only to state the axioms. And so on… *ad absurdum.*

Suppose however, in the spirit of nonchalance with which the profession accepts any and all axioms and then looks to their consequences and the observable predictions that follow, we accepted "unbounded cognition" as a research program. This would completely alter valid research questions. Now we would *assume* that everyone always knew all of Geometry, Calculus and Algebra; what needed explaining was why and how they hid this knowledge?

- Was it due to social forces?
- Was it the result of censorship?
- Did people communicate the results by some cryptic code?
- Were there indirect forms of speech which conveyed the message that "polynomials are solvable"?
- Who pressured Gauss into providing three different proofs of the solvability of polynomials?

And the *piece de resistance* of this research program,

- Why did mathematicians pretend to have such a hard time with proofs?

It is undoubtedly a fruitful program. There is much to occupy academics for decades…

VI. Summary and Conclusion

It is plausible that economics slipped into its current difficulty because all earlier theory was framed with agriculture in mind. But we are not in an agricultural world anymore. The number of available products must have expanded a 1000-fold since the 1700s. Unless one looks, it is difficult to grasp the sheer amounts of data that are generated and potentially available – but perhaps impossible to digest because of their magnitude and complexity. Below are two examples, from Trade statistics and from Price indices.

> Customs forms provide us with Trade data, one for each export shipment. There were about 22 million export shipments originating in the U.S. in 2005. This suggests that we have information on some 22 million individual decisions. However, there are 229 countries and 8,867 product codes with active trade, so a shipment can have more than 2 million possible classifications.[35]
>
> Next, consider the Consumer and Producer Price Indexes, the CPI and PPI[36]. The Producer Price Index program collects monthly price data on about 128,000 individual items from about 32,000 establishments. The CPI collects data on about 80,000 individual items. The larger number for the PPI is presumably due to the addition of many intermediate goods in the PPI.

Unless one accepts the *hara-kiri* assumption of perpetual equilibrium, the question of data relevance now revolves around speeds of convergence in each market. However, there are practically no studies of this question – the speed of convergence to equilibrium – for goods or services in *microeconomics*.[37]

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[35] Surprisingly perhaps, even such extensive data show several regularities. (1) Most product-level trade flows across countries are zero; (2) The incidence of non-zero trade flows follows a gravity equation; (3) Only a small fraction of firms' export; (4) Exporters are larger than non-exporters; (5) Most firms export a single product to a single country; (6) Most exports are done by multi-product, multi-destination exporters (Armenter & Koren, 2010).

[36] I am very grateful to Scott Sager, Ken Stewart and Amy Hobby for answering my queries. I have used their replies for this section with only the minimal editing needed for my purposes. The plethora of data obtained from POS transaction records should satisfy the quantitative economist by their volume. But the sheer volume alone does not solve questions of aggregation, or functional form, or endogeneity of explanatory variables. More interestingly, none of our usual procedures recognise how the institutions at work have adapted to their particular circumstances. Economists at the FTC, who have to argue for or against mergers of firms, urge caution in moving from the retail level POS data to inferences about wholesale market elasticities (Hosken et al., 2002, pp. 2, 3-4, 21, 24).

[37] A Google search of 200 plus items under "rates of convergence in economics" produced only one entry on micro. All the others consider growth theory, which does not bear on this issue. The one seeming exception adapts growth theory concepts to micro contexts and fails to address the concerns expressed here, i.e., the rate at which price and quantity converge to equilibrium in each individual market (Fazio & Piacentino, 2011).

Recognising the complexity of the modern world leads me to return to the principal message of this paper in three sentences.

1. Equilibrium economic theories can be tested only by equilibrium data
2. Data being used for tests have no presumption of being equilibrium data.
3. Hence, the data now used for "testing economic theories" provide no tests at all.

Are there any conditions which justify the current practice? Yes, a state of perpetual equilibrium will serve to justify current practice. However, such an assumption creates many subsequent difficulties, some of which have been described above.

Appendix A

To know about rates of convergence to equilibrium, one wants to get direct estimates of the speed with which prices of individual goods adjust to shocks. Unfortunately, the wealth of detail possessed by US agencies does little to help the ordinary inquirer who wishes to behind such well known figures as the Consumer Price Index or CPI. For several reasons, the pursuit of individual goods is not considered feasible, even by many insiders in the BLS. First, the data are almost certainly insufficient for looking at specific items such as eggs. The average sample size for a given item, such as eggs, in a given area such as Cleveland-Akron, is 10 price quotes. Because the CPI reflects consumer shopping patterns, the price data contain considerable heterogeneity. Isolating a single homogeneous item thus leads to extremely small sample sizes. For example, in the relatively homogeneous category of eggs, the most common unique item contained a monthly average of 62 price quotes for all 87 areas. Second, because of the heterogeneity, it would be burdensome for BLS staff to select specific homogeneous items for such a study, which would end up being quite expensive and probably subject to high variance because errors, either in data collection or in BLS selection, could affect the results. Third, many prices change slowly with prices changing on average only once every eight to 11 months. Because BLS samples are rotated either every two years or every four years, only a very small number of price changes in any specific item will be observed before that item is replaced. Fourth, most price changes for durable goods only occur when models change. If one wishes to observe price changes in homogenous goods, durable goods of a given model will frequently show no price changes at all. To follow the dispersion, transmission and adjustment of the prices of individual goods in a world with micro-data is beyond the range of ordinary persons and probably requires a bureaucracy in itself. If we want to model the process by which data are actually generated, a further complication lies in the use of price/revenue algorithms by many supermarket chains. If such algorithms include local idiosyncratic patterns and if there are many such idiosyncratic patterns in the sample area, then interpretations of the data become even more complex.

The fact that the difficulties which are alluded to above are real is both supported by, and hopefully will be addressed with the considerations provided in "Minding your Ps and Qs".[38]

[38] Ehrlich et al., 2019.

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