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Mathiness in the Theory of Economic Growth†

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Politics does not lead to a broadly shared consensus. It has to yield a decision, whether or not a consensus prevails. As a result, political institutions create incentives for participants to exaggerate disagreements between factions. Words that are evocative and ambiguous better serve factional interests than words that are analytical and precise.

Science is a process that does lead to a broadly shared consensus. It is arguably the only social process that does. Consensus forms around theoretical and empirical statements that are true. Tight links between words from natural language and symbols from the formal language of mathematics encourage the use of words that are analytical and precise.

For the last two decades, growth theory has made no scientific progress toward a consensus. The challenge is how to model the scale effects introduced by nonrival ideas. Mobile telephony is the update to the pin factory, the demonstration that scale effects are too important to ignore. To accommodate them, many growth theorists have embraced monopolistic competition, but an influential group of traditionalists continues to support price taking with external increasing returns. The question posed here is why the methods of science have failed to resolve the disagreement between these two groups.

Economists usually stick to science. Robert Solow (1956) was engaged in science when he developed his mathematical theory of growth. But they can get drawn into academic politics. Joan Robinson (1956) was engaged in academic politics when she waged her campaign against capital and the aggregate production function.

Academic politics, like any other type of politics, is better served by words that are evocative and ambiguous, but if an argument is transparently political, economists interested in science will simply ignore it. The style that I am calling mathiness lets academic politics masquerade as science. Like mathematical theory, mathiness uses a mixture of words and symbols, but instead of making tight links, it leaves ample room for slippage between statements in natural versus formal language and between statements with theoretical as opposed to empirical content.

Solow's (1956) mathematical theory of growth mapped the word "capital" onto a variable in his mathematical equations, and onto both data from national income accounts and objects like machines or structures that someone could observe directly. The tight connection between the word and the equations gave the word a precise meaning that facilitated equally tight connections between theoretical and empirical claims. Gary Becker's (1962) mathematical theory of wages gave the words "human capital" the same precision and established the same two types of tight connection—between words and math and between theory and evidence. In this case as well, the relevant evidence ranged from aggregate data to formal microeconomic data to direct observation.

In contrast, McGrattan and Prescott (2010) give a label—location—to their proposed new input in production, but the mathiness that they present does not provide the microeconomic foundation needed to give the label meaning. The authors chose a word that had already been given a precise meaning by mathematical theories of product differentiation and economic geography, but their formal equations are completely different, so neither of those meanings carries over.

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The mathiness in their paper also offers little guidance about the connections between its theoretical and empirical statements. The quantity of location has no unit of measurement. The term does not refer to anything a person could observe. In a striking (but instructive) use of slippage between theoretical and the empirical claims, the authors assert, with no explanation, that the national supply of location is proportional to the number of residents. This raises questions that the equations of the model do not address. If the dependency ratio and population increase, holding the number of working age adults and the supply of labor constant, what mechanism leads to an increase in output?

McGrattan and Prescott (2010) is one of several papers by traditionalists that use mathiness to campaign for price-taking models of growth. The natural inference is that their use of mathiness signals a shift from science to academic politics, presumably because they were losing the scientific debate. If so, the paralysis and polarization in the theory of growth is not sign of a problem with science. It is the expected outcome in politics.

If mathiness were used infrequently to slow convergence to a new scientific consensus, it would do localized, temporary damage. Unfortunately, the market for lemons tells us that as the quantity increases, mathiness could do permanent damage because it takes costly effort to distinguish mathiness from mathematical theory.

The market for mathematical theory can survive a few lemon articles filled with mathiness. Readers will put a small discount on any article with mathematical symbols, but will still find it worth their while to work through and verify that the formal arguments are correct, that the connection between the symbols and the words is tight, and that the theoretical concepts have implications for measurement and observation. But after readers have been disappointed too often by mathiness that wastes their time, they will stop taking seriously any paper that contains mathematical symbols. In response, authors will stop doing the hard work that it takes to supply real mathematical theory. If no one is putting in the work to distinguish between mathiness and mathematical theory, why not cut a few corners and take advantage of the slippage that mathiness allows? The market for mathematical theory will collapse. Only mathiness will be left. It will be worth little, but cheap to produce, so it might survive as entertainment.

Economists have a collective stake in flushing mathiness out into the open. We will make faster scientific progress if we can continue to rely on the clarity and precision that math brings to our shared vocabulary, and if, in our analysis of data and observations, we keep using and refining the powerful abstractions that mathematical theory highlights—abstractions like physical capital, human capital, and nonrivalry.

I. Scale Effects

In 1970, there were zero mobile phones. Today, there are more than 6 billion. This is the kind of development that a theory of growth should help us understand.

Let q stand for individual consumption of mobile phone services. For $a \in [0,1]$, let $p = D(q) = q^{-a}$ be the inverse individual demand curve with all-other-goods as numeraire. Let N denote the number of people in the market. Once the design for a mobile phone exists, let the inverse supply curve for an aggregate quantity $Q = qN$ take the form $p = S(Q) = Q^b$ for $b \in [0, \infty]$.

If the price and quantity of mobile phones are determined by equating $D(q) = m \times S(Nq)$, so that $m \geq 1$ captures any markup of price relative to marginal cost, the surplus S created by the discovery of mobile telephony takes the form

$$S = C(a,b,m) \times N^{\frac{a(1+b)}{a+b}},$$

where $C(a,b,m)$ is a messy algebraic expression. Surplus scales as N to a power between a and 1. If $b = 0$, so that the supply curve for the devices is horizontal, surplus scales linearly in N. If, in addition, $a = \frac{1}{2}$, the expression for surplus simplifies to

$$S = \frac{2m-1}{m^2} N.$$

With these parameters, a tax or a monopoly markup that increases m from 1 to 2 causes S to change by the factor 0.75. An increase in N from something like 10^2 people in a village to 10^{10} people in a connected global market causes S to change by the factor 10^8.

Effects this big tend to focus the mind.

II. The Fork in Growth Theory

The traditional way to include a scale effect was proposed by Marshall (1890). One writes the production of telephone services at each of a large number of firms in an industry as $g(X)f(x)$, where the list x contains the inputs that the firm controls and the list X has inputs for the entire industry. One obvious problem with this approach is that it offers no basis for determining the extent is of the spillover benefits from the term $g(X)$. Do they require face-to-face interaction? Production in the same city, the same country, or anywhere?

If we split $x = (a, z)$ into a nonrival input a and rival inputs z, a standard replication argument implies that f must be homogeneous of degree 1 in the rival inputs z. Euler's theorem then implies that the value of output equals the compensation paid to the rival inputs z. In a full equilibrium analysis, anything that looks like producer surplus or "Marshallian rent" is in fact part of the compensation paid to the rival inputs.

It follows that there can be no nonrival input a that the firm can use yet exclude other firms from using. Production for an individual firm must take the form $Af(z)$ where A is both nonrival and fully nonexcludable, hence a public good.

I started by my work on growth using price taking and external increasing returns, but switched to monopolistic competition because it allows for the possibility that ideas can be at least partially excludable. Partial excludability offers a much more precise way to think about spillovers. Nonrivalry, which is logically independent, is the defining characteristic of an idea and the source of the scale effects that are central to any plausible explanation of recent experience with mobile telephony or more generally, of the broad sweep of human history (Jones and Romer 2010).

In models that allow for partial excludability of nonrival goods, ideas need not be treated as pure public goods. In these models, firms have an incentive to discover a new idea like a mobile phone (Romer 1990) or to encourage international diffusion of such an idea once it exists (Romer 1994). In such models, one can ask why some valuable nonrival ideas diffuse much more slowly than mobile telephony and how policy can influence the rate of diffusion by changing the incentives that firms face.

As many growth theorists followed trade theorists and explored aggregate models with monopolistic competition, the traditionalists who worked on models with a microeconomic foundation maintained their commitment to price taking and adhered to the restriction of 0 percent excludability of ideas required for Marshallian external increasing returns. Perhaps because of unresolved questions about the extent of spillovers, attention turned to models of idea flows that require face-to-face interaction. Because incentives in these models motivate neither discovery nor diffusion, agents exchange ideas in the same way that gas molecules exchange energy—involuntarily, through random encounters. Given the sharp limits imposed by the mathematics of their formal framework, it is no surprise that traditionalists were attracted to the extra degrees of freedom that come from letting the words slip free of the math.

III. Examples of Mathiness

McGrattan and Prescott (2010) establish loose links between a word with no meaning and new mathematical results. The mathiness in "Perfectly Competitive Innovation" (Boldrin and Levine 2008) takes the adjectives from the title of the paper, which have a well established, tight connection to existing mathematical results, and links them to a very different set of mathematical results. In an initial period, the innovator in their model is a monopolist, the sole supplier of a newly developed good. The authors force the monopolist to take a specific price for its own good as given by imposing price taking as an assumption about behavior.

In addition to using words that do not align with their formal model, Boldrin and Levine (2008) make broad verbal claims that are disconnected from any formal analysis. For example, they claim that the argument based on Euler's theorem does not apply because price equals marginal cost only in the absence of capacity constraints. Robert Lucas uses the same kind of untethered verbal claim to dismiss any role for books or blueprints in a model of ideas: "Some knowledge can be 'embodied' in books, blueprints, machines, and other kinds of physical capital, and we know how to introduce capital into a growth model, but we also know that doing so does not by itself provide an engine of sustained growth." (Lucas 2009, p.6). In each case, well-known models show that these verbal claims are false. Any two-sector growth model will show how Marshall's style of partial equilibrium analysis leads Boldrin and Levine astray. Any endogenous growth model with an expanding variety of capital goods or a ladder of capital goods of improving quality serves as a counter-example to the result that Lucas claims that we know.

In Lucas and Moll (2014), the mathiness involves both words that are disconnected from the formal results and a mathematical model that is not well specified. The baseline model in their paper relies on an assumption P that invokes a distribution for the initial stock of knowledge across workers that is unbounded, with a fat Pareto tail. Given this assumption, Lucas and Moll show that the diffusion of knowledge from random encounters between workers generates a growth rate $g[P](t)$ that converges to $\gamma > 0$ as t goes to infinity.

Assumption P is hard to justify because it requires that at time zero, someone is already using every productive technology that will ever be used at any future date. So the authors offer "an alternative interpretation that we argue is observationally equivalent: knowledge at time 0 is bounded but new knowledge arrives at arbitrarily low frequency." (Lucas and Moll 2014, p.11). In this alternative, there is a collection of economies that all start with an assumption B (for bounded initial knowledge.) By itself, this assumption implies that the growth rate goes to zero as everyone learns all there is to know. However, new knowledge, drawn from a distribution with a Pareto tail, is injected at the rate β, so a B economy eventually turns into a P economy. As the arrival rate β gets arbitrarily low, an arbitrarily long period of time has to elapse before the switch from B to P takes place. (See the online Appendix for details.)

For a given value of $\beta > 0$, let $\beta : B \Rightarrow P$ denote a specific economy from this collection. Any observation on the growth rate has to take place at a finite date T. If T is large enough, $g[P](T)$ will be close to γ, but $g[\beta : B \Rightarrow P](T)$ will be arbitrarily close to 0 for an arbitrarily low arrival rate β. This means that any set of observations on growth rates will show that the P economy is observably different from any economy $\beta : B \Rightarrow P$ with a low enough value of β. They are not observationally equivalent in any conventional sense.

The mathiness here involves more than a nonstandard interpretation of the phrase "observationally equivalent." The underlying formal result is that calculating the double limit in one order $\lim_{\beta\to 0}(\lim_{T\to\infty} g[\beta : B \Rightarrow P])$ yields one answer, γ, which is also the limiting growth rate in the P economy. However, calculating it in the other order, $\lim_{T\to\infty}(\lim_{\beta\to 0} g[\beta : B \Rightarrow P])$, gives a different answer, 0. Lucas and Moll (2014) use the first calculation to justify their claim about observational equivalence. An argument that takes the math seriously would note that the double limit does not exist and would caution against trying to give an interpretation to the value calculated using one order or the other.

IV. A New Equilibrium in the Market for Mathematical Economics

As is noted in an addendum, Lucas (2009) contains a flaw in a proof. The proof requires that a fraction $\frac{\alpha}{\gamma}$ be less than 1. The same page has an expression for γ, $\gamma = \alpha\frac{\gamma}{\gamma + \delta}$, and because α, γ, and δ are all positive, it implies that $\frac{\alpha}{\gamma}$ is greater than 1. Anyone who does math knows that it is distressingly easy to make an oversight like this. It is not a sign of mathiness by the author. But the fact that this oversight was not picked up at the working paper stage or in the process leading up to publication may tell us something about the new equilibrium in economics. Neither colleagues who read working papers, nor reviewers, nor journal editors, are paying attention to the math.

After reading their working paper, I told Lucas and Moll about the discontinuity in the limit and the problem it posed for their claim about observational equivalence. They left their limit argument in the paper without noting the discontinuity and the *Journal of Political Economy* published it this way. This may reflect a judgment by the authors and the editors that at least in the theory of growth, we are already in a new equilibrium in which readers expect mathiness and accept it.

One final bit of evidence comes from Piketty and Zucman (2014), who cite a result from a growth model: with a fixed saving rate, when the growth rate falls by one-half, the ratio of wealth to income doubles. They note that their formula $W/Y = s/g$ assumes that national income and the saving rate s are both measured net of depreciation. They observe that the formula has to be modified to $W/Y = s/(g + \delta)$, with a depreciation rate δ, when it is stated in terms of the gross saving rate and gross national income.

From Krusell and Smith (2014), I learned more about this calculation. If the growth rate falls and the *net* saving rate remains constant, the *gross* saving rate has to increase. For example, with a fixed net saving rate of 10 percent and a depreciation rate of 3 percent, a reduction in the growth rate from 3 percent to 1.5 percent implies an increase in the gross saving rate from 18 percent to 25 percent. This means that the expression $s/(g + \delta)$ increases by a factor 1.33 because of the direct effect of the fall in g and by a factor 1.38 because of the induced change in s. A third factor, equal to 1.09, arises because the fall in g also increases the ratio of gross income to net income. These three factors, which when multiplied equal 2, decompose the change in W/Y calculated in net terms into equivalent changes for a model with variables measured in gross terms.

Piketty and Zucman (2014) present their data and empirical analysis with admirable clarity and precision. In choosing to present the theory in less detail, they too may have responded to the expectations in the new equilibrium: empirical work is science; theory is entertainment. Presenting a model is like doing a card trick. Everybody knows that there will be some sleight of hand. There is no intent to deceive because no one takes it seriously. Perhaps our norms will soon be like those in professional magic; it will be impolite, perhaps even an ethical breach, to reveal how someone's trick works.

When I learned mathematical economics, a different equilibrium prevailed. Not universally, but much more so than today, when economic theorists used math to explore abstractions, it was a point of pride to do so with clarity, precision, and rigor. Then too, a faction like Robinson's that risked losing a battle might resort to mathiness as a last-ditch defense, but doing so carried a risk. Reputations suffered.

If we have already reached the lemons market equilibrium where only mathiness is on offer, future generations of economists will suffer. After all, how would Piketty and Zucman have organized their look at history without access to the abstraction we know as capital? Where would we be now if Robert Solow's math had been swamped by Joan Robinson's mathiness?

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