The Marginal Revolution: Rise and Decline, and the Pending AI Revolution

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CHAPTER ONE

What Is Marginalism?

How is it that ideas, and human capabilities, become lost? And how is that new insights come to pass? If eventually the insight seems obvious, why didn't we see it before? Or maybe we did see it before, but didn't really know we were on to something important? Why do new insights arrive suddenly, in a kind of flood? How do new worldviews replace older ones?

And what does all of that have to do with the future of science, the future of research, and the future of economics in particular? Especially when we try to understand how the ongoing artificial intelligence revolution is going to reshape human knowledge, and the all-important question of *what economists should do*.

Those are the motivating questions behind this work, but I will address them in what is initially an indirect fashion. I will start by considering a case study, namely the most important revolution in economics, the Marginal Revolution (to be defined shortly). The Marginal Revolution made modern economics possible. What *was* the Marginal Revolution? How did it start? Why did it take so very long to come to fruition? From those investigations we will get a sense of how economic ideas, and sometimes ideas more generally, develop. And that in turn will help us see where the science, art, and practice of economics is headed today.

I like the Marginal Revolution so much I even named my blog – coauthored with Alex Tabarrok – after it.

Might the history of the Marginal Revolution reflect some broader trends about how our world is changing? I'll get to these questions and more, but let us start by taking a look at how marginalist ideas are in fact more complex – and more interesting – than many onlookers (and also some economists) believe. That will help us see why marginalism is a complex rather than simple matter, and also why it took so long for economists to appreciate it.

We will see that marginalism – one of the most important ideas in economics – becomes trickier the more we look at it, and less intuitive. That will be a continuing thread in this investigation, namely that matters are rarely simple on closer investigation, and that intuitions can mislead or be highly incomplete. I will attempt to unravel the history of marginalist ideas, and in Quincunx-like fashion, we will see that it leads us further and further down the path of science being radically incomplete. Down the path of humans losing their central role in understanding the world, or what they thought was their central role. And down the path of humans waking up and one day realizing that, all along, they never really knew what was going on.

What was the Marginal Revolution? What is the Marginal Revolution?

I'll start with a very simple and indeed too simple definition of marginalism:

"Marginalism is the economic doctrine that we can best understand value by considering the question of how many units of a good or service an individual has, and using that starting point to ask how much an additional – or marginal – unit would be worth in terms of other goods and services."

You can use that as a mental placeholder for the time being, but we will see that marginalism is more like a heuristic for a complex bundle of closely related concepts. There are multiple marginalisms, and that is one of them, and the one you are most likely to find in a textbook.

As for the history of the marginalist idea, the the so-called "Marginal Revolution," dating from 1871, is considered a formative event – maybe *the* formative event – in the history of economic reasoning. In the early 1870s three different economists published similar insights, known today as marginalism, and thus the name Marginal Revolution.

The three pioneering economists were William Stanley Jevons of Britain, Leon Walras of France and Switzerland, and Carl Menger of the Austro-Hungarian Empire. All three addressed a central question in value theory, namely how are prices determined for goods and services in a market economy. As mentioned above, we cannot understand value or price without starting with the question, "How much of the thing does a person actually have?" Or more specifically, do you already have one horse or three horses? The value of an additional horse will depend on the answer to that question, and thus the demands and supplies in the market are based on that information as well.

To make it slightly more technical, supply and demand are core concepts of economics and perhaps the best-known economics ideas with the general public. Those tools rely very directly on the ideas of marginalism. The derivations of supply and demand schedules refer repeatedly to how much of a resource people currently have, or how much they are likely to have once they engage in marketplace activity. Both the quantity individuals will supply at a particular price and the quantity they will demand will depend on which margins they are at. For instance, if you already have two horses on your farm you are less likely to pay a lot of money for a third horse. When making a purchase decision, you compare the marginal value of another horse with the marginal value of the money you might spend on it.

That sounds simple, maybe too simple, but in earlier centuries most economists were not able to stumble upon this highly fruitful analytical starting point, or if they had the insight they did not see its full import and how it could be turned into further actionable scientific advances.

One of the first applications of marginalism was to explain what is called "the diamonds-water paradox." It would seem that water is far more valuable than diamonds, since we need water to live and diamonds are a mere bauble or trinket, albeit sometimes a beautiful one. Yet water is cheap to buy, and diamonds are very expensive. If water is more valuable, why does it have a lower price?

Marginalism comes to the rescue. If someone had to choose between having no water and having enough water to avoid dying of thirst, they would be willing to pay a very high price for water (the exact final price still would depend on how many suppliers were competing to sell you water and the cost of getting you the water, questions which also can be addressed using marginalism). But that is not the choice for most people. Most people have a fair amount of water, and they are paying for *more water*, or you might say water at the margin. And the value of water at the margin just isn't that high. In American restaurants, many people don't even drink the glass of water they get for free.

To consider diamonds, if you could receive plenty more diamonds by turning on your kitchen tap, diamonds probably wouldn't sell for much in the market, just as water doesn't. Of course, that is not the case and real diamonds remain relatively scarce, thus boosting their value. That said, circa 2025 competition from high-quality artificial diamonds, which can be made in the lab, is threatening to further depress diamond prices. Again, that is consistent with marginalism.

The first known person to resolve the diamonds-water paradox was the famous astronomer Galileo, writing in 1632. Galileo put the point simply:

"People … ought to remember that if there were as great a scarcity of soil as of jewels or precious metals, there would not be a prince who would not spend a bushel of diamonds and rubies and a cartload of gold just to have enough earth to plant a jasmine in a little pot, or to sow an orange seed and watch it sprout, grow, and produce its handsome leaves, its fragrant flowers, and fine fruit."[1]

[1] Galileo, (1953 [1632], p. 59).

And this:

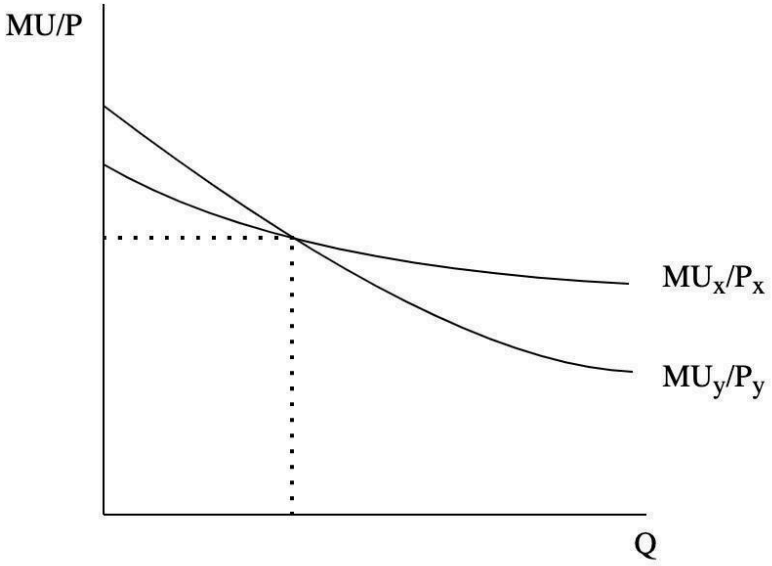
"It is scarcity and plenty that make the vulgar take things to be precious or worthless; they call a diamond very beautiful because it is like pure water, and then would not exchange one for ten barrels of water. Those who so greatly exalt incorruptibility, inalterability, etc. are reduced to talking this way, I believe, by their great desire to go on living, and by the terror they have of death. They do not reflect that if men were immortal, they themselves would never have come into the world."[2]

For Galileo, the water-diamonds issue was part of a broader dispute between Copernican and Ptolemaic cosmologies, and the relative nobility of the earth and the heavens. He probably didn't know he was stating an important principle of economics, though I suspect he was pleased to have presented this kind of philosophic, scholastic point so clearly. In any case, the words of Galileo and other earlier appreciators of marginal utility theory, such as the Salamancan theologians of 17th century Spain, basically were ignored.[3]

As marginalist theory developed, it postulated an equilibrium where marginal utilities, from further expenditures in different areas, are equalized. That meant ratios of marginal utilities to price had to be equalized, so if an extra marginal allocation of a good yielded higher value than for other goods, you would keep on buying that good unless its price were higher in equal proportion. For instance, if you could get more beans at the same price as cheese, and at current margins you enjoyed beans more, you would keep on spending more money on beans, and less money on cheese, until equality of marginal returns would hold.

[2] Galileo, (1953 [1632], p. 59).
[3] On Galileo and marginal utility theory, see Ekelund and Thornton (2011).

Graphically, that looks like this:



As shown in the figure, the ratios of market prices to marginal utility all should be equal.

Or if you prefer to see it in equation form:

$$MU(x)/P(x) = MU(y)/P(y) = MU(z)/P(z)$$

Marginalism also is the starting point for explaining wages, rents, returns to capital, and basically all factor prices. For instance, many economic models write down some conditions under which the wage for a worker equals the marginal product for that worker, marginal product being defined as how much that worker adds to the output of the firm. Few economists believe that wage actually will end up literally *equal* to marginal product, and that difference stems from numerous reasons, including transactions costs, differential bargaining power, various laws, and measurement difficulties. Nonetheless the wage = marginal product model, at least as a rough approximation, is the accepted starting point for figuring out how these other factors are likely to matter.

The purpose of this writing, however, is not to rehash basic economics but to give you a new way to think about it and also its more general relevance. To suggest that what is central in economic reasoning is historically contingent, and not as obvious as we might have once thought. To realize that finding "an intuitively appealing answer" may be less valuable than you had thought. To give you new ways of thinking about progress in science more generally. To give you new ways of thinking about why intellectual progress can be so slow, then extremely fast, and later so evanescent. And yes, as we shall see, to understand how the value of human intuitive thinking can plummet over a short period of time. I also consider why it is so hard to discover new ideas you don't already have a handle on. And to see how many complementary pieces must be in place before a new method moves into its heyday.

I hope to explain why ideas we love, and work with fruitfully, can disappear and leave center stage. You and I may be crying, but scientific life will go on. I've already put down my handkerchief.

I will next show that marginalism is deeper and more multi-faceted than most moderns believe. I will use that presentation to explain why it took so long to properly appreciate and apply marginalist insights. On the more ominous side, I will consider how and why marginalism is already declining in import, and why it is receding from the scientific frontier. And why that might be for the better.

To proceed, I will distinguish between what I call "intuitive marginalism" and "tautological marginalism." Both are useful, and they do not contradict each other, but they have different emphases. It turns out that other marginalisms will enter the fray as well, including engineering marginalism, social marginalism, and marginalism as an organizing category. The world is full of marginalisms, which is perhaps in slight tension with the core insights of marginalism itself. Did the simplest presentations of marginalism actually prepare us for the reality that marginalism is a many-splendored thing?

I'll continue by presenting intuitive marginalism, which is best done by example.

I. Intuitive Marginalism

Why do drivers in China sometimes intentionally kill the pedestrians they hit?

Well, under some compensation schemes it is cheaper – in terms of fines – to kill the person and pay a fine than to have to pay for his continued upkeep. If you as a driver kill a pedestrian, the associated fine – at least circa 2015 – can run in the range of $30,000 to $50,000. But if you have to pay for lifetime care for a disabled surviving victim, that can run into the hundreds of thousands of dollars. At the margin, you lower your penalty if the victim dies. That is why there are some reported cases of hit-and-run drivers returning to the scene of the crime, and running over the victim again, to make sure the "work" is finished off. At the margin, there is no financial penalty for worsening the crime.[4]

For another strange and morbid example, might applying the death penalty ever backfire? Once you have committed a very serious crime and potentially face the death penalty, you may be more willing to shoot at policemen or innocent bystanders in your attempt to get away. (This is true at least for individuals without a conscience, but if you are facing the death penalty anyway, that might be you.) After all, if the system is going to kill you anyway, the punishment can't end up being worse if you commit more serious crimes, even in great numbers.

Many strict punishment schemes face a version of this problem, and it pops up in ordinary life too. One study found "that individuals who hold very strict norms of honesty are more likely to lie to the maximal extent." If lying at all is so, so terrible, once you have crossed the threshold of lying a bit you don't face a "very large marginal conscience cost" from lying all the more.[5]

[4] Sant (2015).
[5] Aycinena, Rentschler, Beranek, and Schulz (2022), 9.

Along related lines, you might be wondering why so many victims of "woke cancellations" are moderate Democratic women, rather than right-wing white men. One reason is that the cancellers may feel that such individuals can more readily be cowed, and furthermore they are setting a precedent that may strike fear in the hearts of others in that same group. At the margin, those individuals really care about getting cancelled and thus their behavior may be more responsive to the threatened punishment. Many right-wing white men, in contrast, already feel rejected by left-wing institutions, and they likely find their support bases elsewhere. At the margin, they may have less to lose from cancellation, and in some cases left-wing cancellation may boost their credibility with their support groups on the right. As I once remarked to a Stanford crowd at a conference on free speech, "They don't cancel Satan!"

In many marriages, one party feels he or she cannot win the approval of the other party no matter what. A husband, for instance, might give up trying if he feels – however incorrectly – that he cannot please his wife. He may not perceive much of a disapprobational incentive at the margin to simply doing as he pleases.

Why might many of the homeless prefer to live in cities with very high rents? Well, high rents imply those cities have nice amenities, such as the good weather enjoyed in many parts of California, or perhaps the attractive scenery of Seattle. Yet the homeless do not pay the associated high rents or home prices, as by definition they do not have formal residences. At the relevant margins faced by the homeless, high real estate prices don't matter, or if anything they are a signal that the amenities are good. So you would expect a disproportionately high number of the homeless to seek out relatively attractive locations. It's not just about the weather – many homeless enjoy public amenities such as parks, libraries, and even museums and public performances in their chosen cities of residence.[6]

[6] Sumner (2022).

Sometimes margins operate through subtle, not so easily visible mechanisms. Why, for instance, is not Mexico a better educated nation, given that it is one of the wealthier middle-income countries? The full explanation is likely complex, but one relevant factor has to do with choice on the margin. If you are crossing the border in Texas to work in a supermarket, you will not be paid more if you have a college or even a high school degree. Furthermore, many Mexicans, knowing they may someday migrate northward, have less reason to invest in an educational credential that has most of its value at home.[7]

In other words, given migration opportunities, which have been extensive for many millions of Mexicans, the marginal returns to more education within Mexico are relatively low.

Those who steal from me also understand marginal principles. Every now and then I see unjustified charges on my credit card statements, presumably resulting from the theft of my card number. If it is for $200, I will contest it, but for $2 maybe not.

Why are you sometimes colder in the morning in a relatively warm climate, and warmer in the morning in a relatively cold climate? David Friedman, son of economist Milton Friedman, once posed that puzzle. He suggested that in the colder climates, such as Boston, you insulate your home better. With better insulation, the *marginal* cost of turning up the thermostat is lower (note you still had to cover the *fixed* cost of insulating). In southern California, the insulation properties of apartments and homes are often quite weak, so if the morning is cool, you are stuck being a bit cold yourself. The generally sunny weather means it is not worth investing in a quality insulation system. I've never seen a general proof of this proposition, but I did once live in New Zealand, in Wellington, which has a relatively temperate climate but not without a fair number of chilly moments. My apartment had no central heating whatsoever, and so often I was cold, and not only in the winter months.[8]

[7] Cowen (2006).
[8] See Friedman (1989).

There are many more examples to be had, but those are some basic illustrations of intuitive marginalist reasoning. They share some common principles, namely that an incentive is present, and such an incentive influences human behavior and helps to explain some state of affairs.

II. Tautological Marginalism

There is an ongoing tension between marginalism as a series of discrete intuitive insights, as presented in the above examples, and marginalism as a universally valid principle, some would say a tautology. In the former deployment, marginalism is a distinct form of reasoning and behaving that not everybody will have mastered. In the latter tautological deployment, marginalism always applies even if it is not recognized as such. All coherent arguments, if examined deeply enough, boil down to claims at the margin and can be expressed in terms of marginalist principles.

The tautological understanding of marginalism can be illustrated by example, for instance by considering cases where people do not seem to think on the margin at all. To put it most simply, if you are deciding to not calculate decisions at the margin, that too can be viewed as a marginal decision. You have decided that further reflection and calculation, at the margin, is not worth your time and effort. You may feel that is a tautology, but you can't say it is flat out wrong, and perhaps it is a useful tautology. Whether that assessment is exactly correct in cost-benefit terms is not the point, as it is indeed a marginal calculation.

In short, any example of a person apparently neglecting marginalism can be reinterpreted as that person using marginalism along some other variables or dimensions. Similarly, when you allocate your funds to leisure versus education, no matter what choice you make we have a way of describing your choice, *ex post*, as if you weighed one set of marginal variables against the other. In that sense you can never violate marginalist principles.

When we teach our students marginalism, which brand are we teaching them? Marginalism as intuitive empirical and possibly counterintuitive insight? That is what the opening examples of this chapter try to do. Or are we teaching them marginalism as tautology? That is what formal economic theory typically tries to do, and that is what you see when you look at mathematical expression of marginalist principles. One approach tries to teach you something new, the other tries to give you a language for what you (supposedly) already knew.

The reality is that we are teaching both approaches. We need the examples to make marginalism stick, to make marginalism vivid, to help people see unexpected marginal conditions, and to give people new ways of arranging their thoughts and to generate new hypotheses. At the same time, those actually studying economics will learn – correctly – that just about any choice can be conceptualized – *ex post* – as individuals equating first-order conditions at the margin.

The very best marginalists are able to deploy both toolboxes – intuitive and tautological marginalism – without collapsing into contradictions. They also will avoid confusing one brand of marginalism with the other. So if you put forward a clever point about how capital punishment incentives might possibly backfire, you have promoted one brand of marginalism but not the other. One way of reconciling the two marginalisms is to view the tautology approach as "the true marginalism," and to view the clever examples as "marginalism plus a complementary empirical hypothesis about what the relevant margin is." So if the claim about how capital punishment may backfire turns out not to be true, marginalism is not refuted. The only intellectual damage would be to the subsidiary empirical hypotheses about how these criminals perceive and respond to the incentives before them.

Why might anyone find tautological marginalism at all interesting? How impressed you are by marginalism may depend on which brand you are thinking about. If you like clever (or supposedly clever) insights, you will prefer the marginalism bundled with some empirical assumptions, that is the potentially falsifiable version. If you are a theorist and obsessed with calculus, first-order conditions,

and the building out of theory, you will prefer the marginalism that can explain each and every choice as equating different values at the margin.

The tautological version of marginalism allows you to generate techniques of empirical estimation of very real power. It is not that the tautology is so powerful *per se*, but if you assume margins (of some sort) are always holding you can model economic systems with greater ease and facility. For instance, you can solve marginal equations for consumption and obtain equations like the following:

A consumption equation:

$$c_t + 1 = \lambda_t c_t + \varepsilon_t + 1/u''(c_t)$$

And then:

$$\lambda t = \left(\frac{1+\delta}{1+r}\right)^{u'(c_t)/c_t u''(c_t)}$$

Don't worry if you don't understand this or how we got there. The general point is that we can move from a simple insight to something that is pretty complex and also useful. That equation can be derived using calculus and first-order conditions, based on marginalist insights. You can then estimate that equation by getting numbers about how people are spending and on what, and you can test whether in fact consumption is a random walk. On the consumption side, for instance, if the Fed and Treasury have estimates of consumer wealth, interest rates, expected inflation, and other numbers, they will have a sense of how consumption will behave in forthcoming periods. In turn that will help them forecast economic conditions and thus help them to set monetary and fiscal policy.[9]

[9] For the full model and derivation, see Hall (1978).

Much of 20th-century econometrics (and 21st-century econometrics) was based on tautological marginalism. Marginalism offered a framework for generating relationships among economic variables, measuring those variables, and then testing hypotheses about those variables.

It is no wonder that marginalism succeeded. You can think of it as a full employment project for economists. Opinions will differ on exactly how good or how useful all of this econometric estimation has been. Still, it is broadly recognized that for most problems and for most of the history of the economics profession better alternatives have not been available.

We now also can start to see some reasons why marginalism was slow to catch on. Until all these econometric possibilities are on the table, marginalism just doesn't seem all that impressive. Intuitive marginalism applies only in some cases, and some of the time the auxiliary hypotheses turn out to be wrong. It can be fun and insightful, but it is not a general mode of analysis. Furthermore, early marginalism was not well-geared to win over the loyalties of economists as it did not, without accompanying econometric advances, give them all that much to do. Marginalism did enable the growth of mathematical economic theory, but most economists are not and were not well-equipped to do interesting pure theory. The growth and spread of marginalism really did require significant progress in empirical methods and applications.

In this setting, both intuitive marginalism and tautological marginalism rose together, often without much explicit differentiation. Marginalism was working! Nonetheless it was tautological marginalism that drove the growth in the doctrine, at least at the research level. For Principles 101 we economists taught intuitive marginalism, and we showed students how to graph marginalist ideas. If you went on to intermediate micro you would start to get more doses of tautological marginalism. If you went to graduate school in economics, you would learn sophisticated empirical techniques derived from tautological marginalism. The marginal revolution seemed complete.

III. Engineering Marginalism

Sometimes marginalists want to improve the world, and then they turn to "engineering marginalism." For instance, congestion pricing is designed to raise the marginal cost of driving at crowded times, discourage trips of marginal value, and thus make the traffic easier to bear. Ideally, what is the best way to set the size of the congestion fee? It should be equal to the marginal cost your car, when it is on the road, imposes on other drivers through their added delays, and ideally based on broader social marginal costs as well, for instance from auto emissions.

When the congestion fee is set at marginal social cost, it can be said that policy has "internalized the externality." That is, the individual driver has to bear the marginal social cost in the form of a fee or toll. If the personal value of your commute, to you, is higher than the social cost imposed by your car, you still will take the trip. Otherwise not.

As a citizen, you don't have to believe that the proper toll, all things considered, has to be exactly the same as the marginal social cost of the individual car trip. You might for instance invoke including ethics and distributional issues to come up with a final congestion fee differing from the estimated marginal social cost. For instance, you might doubt whether poor people should have to pay the same toll as rich people, for reasons of distributional justice. Still, the marginal social cost is used as a starting point for the investigation, to be modified by subsequent considerations. We are still acting as engineering marginalists, even if we end up modifying our marginalism … at the margin.

Singapore is perhaps the place where policymakers think most carefully about marginal incentives. As you might expect there is congestion pricing in Singapore. But instead of using a pre-announced single price or toll, the fee for driving during peak hours is adjusted based on real-time traffic conditions and average speeds. I've been in Singapore numerous times and typically am amazed at how smoothly the traffic flows, even at rush hour.

The Singaporean government also levies stiff taxes on car ownership, reflecting a social marginal cost from congestion and also pollution from cars, plus the need for parking and car storage, which can create land use problems. It is not unusual for those fees to run well above $100,000 for a single car.

In August 2013 in Singapore, I saw a sign that outlined a fine for incorrect behavior in public. The sign stated: "$250 plus GST," noting that GST is a consumption tax similar to a VAT.[10]

Why is this done? Presumably the violators derive consumption pleasure from the fined activity. Paying the fine (stochastically) is, at the margin, equated with the return from other forms of consumption, as standard economics would suggest. Most other forms of consumption are taxed and thus subject to GST. Therefore, the fine-generating activity should be taxed too, so that Singaporeans can equate their values at the margin more readily.

The sign can be read on (at least) two levels. One is a recognition that the Singaporean government is aware of marginal incentives, and it desires to apply taxes evenly and in a just manner. Why tax the purchase of baby formula but not tax an illegal activity? The second level of meaning is that the Singaporean government is aware that its citizens will be thinking at the margin, even the scofflaw citizens who are breaking the laws regulating public order. Is this an *expectation* that such ruffians will be thinking at the margin? A hope? Or perhaps an intended lesson and prescription, even if marginal thinking is not to be expected? The very ambiguity here reflects the richness of marginalist reasoning.

Engineering marginalism can be found all over public policy. The notion of taxing pollution or carbon emissions, for instance, is an attempt to impose some of the social marginal cost of pollution on the polluting enterprises. Economists typically believe we don't do enough of this, and indeed marginalism leads them to that conclusion. Engineering marginalism has produced ideas as unusual

[10] Cowen (2013).

as taxing dangerous bully dogs, taxing noise pollution, and taxing people whose home alarms go off, even when there is no intruder. Who has to pay for the police time to deal with the incident, and should we not internalize that externality?

How about an obesity tax, for individuals who weigh too much, thereby raising future health care costs for broader society? I don't favor that idea, but I can see some first-order benefits from such a proposal, because I understand marginalism. At the margin, some individuals would have a stronger incentive to eat less, possibly saving on health care costs in the longer run.

If the FCC runs an auction to sell off part of the electromagnetic spectrum, marginalist principles are used to determine the best way to run that auction. Many such auctions are run on a "second price" basis, namely the winning bidder pays the amount of the next highest bid, rather than paying what he bid. In a second-price auction there is a general incentive to bid what the object actually is worth to you, rather than to put in a lower bid to save money. Let's say for instance the object is worth $9 to you, and someone else might bid $8 for it, but you are not sure. Should you bid $8.01, seeking to save money, or should you bid up to $9? In the second price auction, in this scenario if you win the item, you pay only the second bid of $8 anyway, you do not pay extra because you bid $8.99. So bid what the item is worth to you. The second price auction thus elicits truthful bids, and that in turn ensures that the auctioned item goes to the parties who place the highest economic value on it. It was marginalism – and the great marginalist economist William Vickrey – who helped figure out exactly how this works.

Sometimes a welfare scheme needs to be made universal rather than selective or need-based, because the latter can imply very high marginal tax rates for those who earn enough to climb out of benefit eligibility. Consider the work disincentives embedded in the U.S. Social Security system. The rules are complex, but in many cases if seniors work more and earn more money, your benefits go down. Economists Auerbach, Kotlikoff, Koehler, and Yu estimated this effect and found the following: "Consider, for example, the implications for those 60–64 of earning $20,000 more for one

year. Among the lowest quintile, 51 percent will lose more than 80 cents of every extra dollar earned, 8 percent will lose between 61 and 80 cents, and 7 percent will lose between 51 and 60 cents. … Among those in the top quintile, 39 percent are in a 61 to 80 percent marginal net tax bracket."[11]

Perhaps that policy should be redesigned so those work disincentives are removed or at least diminished. Work doesn't just contribute to the economy, but people with jobs, including seniors, tend to be happier and more fulfilled.

In the private sector, entrepreneurs and managers try to estimate the marginal benefits and marginal costs of various activities and investments, to better understand the impact on their profit and loss. Accounting data typically record average magnitudes ex post, such as realized costs and revenues. But what would have happened in the counterfactual, had slightly different decisions been made? Making progress on that question is part of the art of management. If a manager has studied some degree of marginalism, or simply has good built-in marginalist intuitions, she will be aware that so much of the data fed to her is not truly marginalist in nature. She also will be aware that she will need to make marginalist inferences from that same data. Can the data be improved? Can the data be massaged to produce marginalist conclusions or at least inferences? Or do we just have to fly blind and guess? Those questions will be paramount in the mind of the marginalist manager.

Not surprisingly, economists love engineering marginalism. It gives us something to do, it makes us feel useful, and often we are paid to do it.

Part of engineering marginalism has been to use economics for predictive purposes. Let's say for instance that the government increases the income tax on labor. There is a substitution effect (the return to working is now lower), and also an income effect (the tax makes workers poorer, so they might work harder to make up part of the gap). The net result of the tax will depend, among other

[11] See Auerbach, Kotlikoff, Koehler, and Yu (2016). Before the recent plagiarism scandals, I might have paraphrased this quotation and cited the authors rather than quoting them. But since those scandals, my incentives have changed at the margin.

things, on the diminishing marginal utility of wealth, namely on how much the new, lower wealth level makes workers worse off. Marginal utility theory again showed its usefulness, as it could help policymakers engineer a better tax system.

The late 19th century and early 20th centuries were times when engineering mindsets were ascendent throughout Europe and the United States. We used fossil fuels and powerful machines, as designed and implemented by engineers, to lay the basic structures of the modern world. So if economics also had a can-do mindset, it is no surprise that engineering marginalism helped contribute to the rise of marginalism more narrowly. In a time of engineering marvels, marginalism really did matter.

The 19th century Frenchman Jules Dupuit was an engineer and although his name is not directly linked to the 1871 marginal revolution, he has as good a claim as anybody to be the father of marginalism. Dupuit published his major work, *On the Measurement of the Utility of Public Works*, in 1844. Dupuit is not so much a precursor of marginalism as a flat-out marginalist early on. He presented the first systematic case for marginal cost pricing, which he applied to public bridges. Bridges are a special case because they cost a lot to build, but once the bridge is in place the marginal cost of an extra crossing is very low, at least outside of periods of congestion. That in general will imply low prices for bridge traffic at the (non-congested) optimum. Dupuit understood the math, he presented the first-order conditions, and he even understood how to work the details of consumer surplus. More generally, he understood that economics should be organized around the ideas of gains from trade and incentives. He helped to birth what Ekelund and Hébert have called the "Econo-Engineer" tradition in 19th century French economic thought.[12]

Through Dupuit, one can see that marginalist engineering was built into marginalism from the very beginning. And yes, he was a "real engineer," as he wrote extensively on groundwater hydraulics,

[12] See Ekelund and Hébert (1999), and also Ekelund (1968). Note, however, that Dupuit was not an advocate of laissez-faire, as inheritance taxes and licensing for quality assurance were two areas where he favored government intervention (1999, 330–336).

helped oversee the building of the Paris sewer system as Chief Engineer of the city, improved waterworks in other parts of France, and also improved road construction. As both a theoretical and practical engineer he was well known in his time. Not surprisingly, Dupuit was a confirmed utilitarian as well – not just a casual person who favored the greater well-being of mankind, rather he argued at length and with passion for utilitarianism as the proper approach to public policy.[13]

If you read the blog Marginal Revolution, you will know I have a co-blogger Alex Tabarrok. Alex and I agree on many issues, but we also have different perspectives and priorities. Alex is more firmly in the camp of engineering marginalism than I am, with my interests being somewhat more philosophical.

IV. Social Marginalism

As a side act to intuitive and tautological and engineering marginalism, there is social marginalism. Social marginalism tries to use marginalist insights to make social judgments about value, thus the label.

Often social marginalism makes some kind of comparison between rich and poor. An extra thousand dollars for a poor person might mean food security, or perhaps their kids can finish high school rather than needing to get a job. An extra thousand dollars for a wealthy person might just be saved, or perhaps they will be spent on a higher quality watch, albeit a watch that tells the time no better than a cheaper watch.

In poorer societies the differences may be more striking yet. In those cases even a small number of dollars might make the difference between life and death.

[13] See Ekelund and Hébert (1999, p. 309, passim). See Hager (2004) on the engineering achievements of Dupuit.

Social marginalism is not committed to dogmatic judgments about poor people valuing money more at the margin. There may be rich people who greatly enjoy earning another thousand dollars, and there may be poor people who practice asceticism and who (at the margin) derive their pleasure from the non-monetary sides of life. Still, once we allow such rough and ready comparisons into the analysis, we will find a lot of cases where the social value of a dollar to poorer people will be higher than for wealthier people.

None of this commits you to a vast redistributionist agenda per se. You might think it is important to leave strong productive incentives for the wealthy in place, or you might think the trickle-down effects from wealthy people holding and investing their wealth will be good for the poor. Again, at some (but probably not all) margins. This is not the forum to debate such questions, rather the key point is simply that judgments about margins, for human beings, have a social dimension and, whether we like it or not, we will end up comparing well-being across different individuals.

Social marginalism does not fall out of contemporary economic reasoning or textbook economics in any direct sense, and indeed you will find many (most?) economists insisting that utility is ordinal only and cannot be compared across individuals in any scientific sense. Nonetheless for most people there is a version of social marginalism that resonates.

Let's say you had two friends, but you had time to drive only one of them to the hospital. One friend has a hangnail and seems vaguely out of sorts, and the other broke their arm and is screaming in pain. Which friend would you take to the hospital?

The answer is obvious. And if the neoclassical economist insists "well that is not a scientific judgment," the biomedical establishment would beg to differ with that assessment. I don't know many economists who would insist they had no idea who was suffering more. The empty claim that "it is not an economic judgment" doesn't really mean much, as the judgment has some scientific and empirical content in any case, whether or not you choose to call it economics.

Or say you had a choice between being a random person born in Denmark, or a random person being born in Myanmar. Most people think you have good grounds for preferring the Denmark "birth lottery." Few would doubt there are some subjective components to such judgments, but they hardly seem to be entirely removed from the realm of factual, science-laden assessments. Are economists not proud of their frequent comparisons between North and South Korea?[14]

A lot of the motivation behind the original marginal revolution was social marginalism, or more concretely marginalism as a means of improving social outcomes, in particular the lot of the poor. The three early marginal revolutionaries – Jevons, Menger, and Walras – all believed in cardinal utility to some extent, and all believed that cardinal utility had social implications.

I'll consider Jevons in the next chapter, but even Menger was not a hardcore ordinalist, even though much of the later Austrian School took that direction. Menger assumed a zero point for marginal utility, and he stated on multiple occasions that ratios of marginal utilities were a meaningful concept. For instance, he wrote that the marginal utility of a horse, under the proper circumstances, could be "three times the value of a cow," which implies at least a partial notion of cardinality. Walras also sent conflicting signals on the issue of the cardinality of utility, but in the words of Walras scholar Donald Walker "he nevertheless believed that it is measurable and therefore that to treat it as such was not contrary to fact." In part for the purposes of social policy, that was a move Walras was willing to accept, despite his hesitations about measuring cardinal utility directly.[15]

Hermann Heinrich Gossen was one of the earliest precursors of marginalism, and he stated the principles of marginal utility clearly in his 1854 German-language *The Laws of Human Relations*. Gossen's goal was most definitely social reform, and he was a confirmed Benthamite. Land reform was of special importance to him. Toward those ends, he developed a theory of diminishing

[14] On some of these points, see Nozick (1977).
[15] See Moscati (2013, p. 398). Moscati cites Menger (1981) [1871], pp. 126–127, 135, 183–186, with p. 184 on the horse and cow. On ambiguities on whether Walras assumed cardinal utility, see Moscati (2013, pp. 399–405) and Walker (2006, pp. 132–133, the quotation from p. 132).

marginal utility, which he applied also to utility comparisons across individuals. He even asserted that "for all men of the same social class and in otherwise similar circumstances, the outlay devoted to the same pleasure is in many instances subject to very small differences among these individuals, these differences being so insignificant that they hardly deserve consideration." And have no doubt – Gossen was an ambitious thinker. He starts the second paragraph of the author's preface with "I believe I have accomplished for the explanation of the relations among humans what a Copernicus was able to accomplish for the explanation of the relations of heavenly bodies." He thought that with his book and with his ideas he was ushering in a new era of human progress through beneficial social change, led by the reasoning of Gossen himself of course.[16]

We also can look after 1871, and social marginalism hardly disappears. Irish economist Francis Ysidro Edgeworth became famous for his analysis of market exchange ("the Edgeworth Exchange box"), first-order conditions, the contracting curve, and he also was a statistical pioneer for developing significance tests. When it came to social utility, he wanted to establish an "exact Utilitarianism" based on a "hedonimeter" that would calculate maximum social pleasure, as would be established through optimum policies. Not everyone agreed, but that was hardly unusual at the time, including amongst the pioneers of modern economic thinking. Edgeworth's 1881 book had the appropriate title *Mathematical Psychics*.[17]

In short, social marginalism has been part of the marginal revolution from the beginning. Starting in the 1970s, some economists such as Amartya Sen has resurrected broader notions of utility, including the possibility of partial utility comparisons across individuals. Due largely to Sen's influence, interpersonal utility comparisons, if only in partial or imperfect forms, have re-entered standard normative economics. So social marginalism is by no means dead or dormant, and arguably since the 1980s it has constituted quite a large part of work on the frontiers of neoclassical welfare economics.

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[16] See Gossen (1983, p. cxlvii) [1854], and see p. 163, passim on interpersonal utility comparisons.
[17] Peart (1996, pp. 165–166) talks about Mill and his opinion on empirical regularities and explaining the gap between the expected and the actual outcome. Also see Edgeworth (1967, pp. 80, 101–102) [1881].

In the broad sweep of history, social marginalism played an important role in the spread of marginal utility theory. It was easy for reformers to see marginalist doctrine as standing on their side. And indeed, many of the important marginalist thinkers of very late 19th century and of the very early 20th century rejected laissez-faire doctrines. Instead, they thought governments should play a critical role in fighting poverty, and given the growing wealth of their societies, they thought such anti-poverty campaigns were increasingly possible. Social marginalism thus also created a lot of employment for economists and economic modes of thinking. If you wanted to make the world a better place, the doctrine of diminishing marginal utility seemed like a good place to start.

Morally Controversial Examples of Social Marginalism

While social marginalism does embody interpersonal utility comparisons, it also does not always imply left-leaning conclusions. One of marginalism's hidden truths is that sometimes it may imply conclusions that some would call reactionary or offensive. I don't mean to prejudge these questions, but once you start seeing marginalism as complex and multi-faceted, you will realize that its political implications may point in varied directions. Some applications of marginalism can raise points that irritate or infuriate many people.

A simple example is that social marginalism may direct our attention to gains from redistributing resources from elderly people to very productive wealthy investors. The wealthy investors might create higher social gains, all things considered, if they have access to those resources. The elderly, in contrast, might have used that money to finance additional increments of their consumption. It doesn't *have to* work out that way, nor do you have to endorse such a redistribution (who amongst us is a pure utilitarian?), but social marginalism puts that possibility on the table.

The general issue is that people have their own moral and ethical points of view about what is right and wrong, about what is socially desirable. Marginalism has built a framework for systematically

introducing other pieces of information about what individual human beings value. Those valuations do not in general match up to the moral and ethical viewpoints that most observers wish to uphold.

Or say there are problems in a region of the country that has been deindustrialized and now has poor social and economic indicators. The standard "normie" response to such problems is to subsidize the affected area, and indeed governments do that around the world. If you are trained in marginalism, you immediately wonder whether we should not tax the stayers to get them to leave and start anew elsewhere. That may or may not be the correct policy, all things considered, including of course fairness norms. But the idea will occur to a marginalist right away, whereas the non-marginalist thinker is often shocked when hearing it.

How about taxing people who attempt to commit suicide? Whether we should do it or not, that too is an idea inspired by marginalism. After all, people who either succeed in committing suicide, or who merely try, do impose significant costs on their loved ones and often on society more generally.

The intuitive disruptions get worse yet. One recent research paper, by Joanna Venator and Jason Fletcher, reported: "We find that a hundred-mile increase in distance to the nearest [abortion] clinic is associated with 25 percent fewer abortions." Also in the abstract, they write: "Our results suggest that even small numbers of clinic closures can result in significant restrictions to abortion access." It is easy to see how those results follow from basic economics. If abortions are harder to get and less convenient to access, fewer women will seek out abortions, as these numbers indicate.[18]

Typically, such results are cited on behalf of the pro-choice side in the abortion debate. Restricting clinic access, as numerous American states have done, is shown to severely limit the opportunities of women seeking abortions. From a pro-choice point of view, that is indeed a terrible outcome. First there is a change in policy, and then there is a notable decrease in the number of abortions. That

[18] See Venator and Fletcher (2019).

implies the policy altered which is the best option for many of these women, namely from "get an abortion" to "don't get an abortion."

If you think in terms of marginalist frameworks, however, you quickly will see that the above cited facts also – at least potentially – might support the pro-life side of the debate. If it is harder to get an abortion, and fewer women get an abortion, that means they decided not to fly, drive, or otherwise travel to another state to get an abortion. Or perhaps they decided not to use those drugs that can induce miscarriages. Using economics language, it could be said that the "abortion elasticity" is relatively high. Or in other words, there are many women who are at the margin when it comes to getting their abortion. If said abortion is less convenient, they will flip to the side of the decision ledger where they do not get the abortion. To the woman (or whoever is serving as the relevant decision-maker, such as the parents or father) in question, the value of that abortion measures as relatively low, not extremely high.

The fact that you might interject and suggest those alternatives are somehow morally outrageous does not refute the argument.

In contrast, consider a woman who is absolutely determined to get an abortion because she feels that having a child will ruin her life. The closure of some clinics induces such a woman to go elsewhere for her absolutely necessary abortion. These empirical results, in essence, are suggesting the number of such women, namely those absolutely determined to get an abortion, is somewhat smaller than we had been expecting.

To be clear, invoking marginalism does not settle the matter. It might be argued that the women considering an abortion cannot afford an extra trip (but they can afford a child?), or that they are too stressed to make a rational decision, or that the need for a discrete, extra trip might expose them to additional social and familial pressures, all of which could militate in favor of just staying home. So the lack of greater adjustment to less clinic access can be given multiple interpretations, and not just

"those women don't value the abortions very much." Still marginalism points our attention to the *possibility* that the marginal value of those foregone abortions is not, from the woman's point of view, extremely high. Even some of these additional factors, such as fear of family pressure, suggest that the woman is somewhat wavering on the margin and that the value of the abortion to her is not absolute.

There are (at least) two possible responses to such results. One is to say that a great deprivation has occurred because many fewer women end up having abortions. Another response is to infer that the marginal value of the potential abortions, to the choosing women that is, could not have been so high to begin with, if the number drops off so readily with a change in the costs.

Many people hate even thinking about the issue in this kind of framework because it brings up the notions of trade-offs and marginal valuations. You might think, as many do, that the abortion debates should be settled by absolute moral values and appeals to rights and ethics, not economic valuations, even if those valuations are taken from the actual decision-makers counted in these debates. That reluctance to consider marginal trade-offs may be true for individuals on both the "pro-choice" and "pro-life" sides of the debate, though different rights judgments are made by those two groups. Still, even if you think rights settle the matter, the marginalist perspective pushes some complications right into our faces. All of a sudden people start hearing about the trade-offs associated with their viewpoint. They do not always want to consider that plural and indeed conflicting values may be relevant. In this setting, marginalism is playing a forcing role by keeping those plural values squarely in the discussion. And this is one reason why marginalism is sometimes unpopular. It is corrosive of moral certainties.

Marginalism, and also social marginalism, will open your eyes to some different understandings of how evidence is deployed in the debates. In common sense understandings, "a big effect" of a policy (i.e., abortions drop a lot with abortion restrictions) can sound just terrible. Armed with marginalism, you can see that "a big effect" of a policy can be interpreted as either a larger tragedy or a smaller tragedy, depending on other assumptions you might choose to make.

Similar issues arise with Obamacare, a less controversial but still controversial topic. As you may know, one central feature of President Obama's Affordable Care Act is high subsidies for lower-income individuals buying insurance on the Obamacare exchanges. It is sometimes debated whether those subsidies should be raised or lowered. Obamacare defenders typically argue that if the subsidies were lowered, many fewer individuals would buy health insurance, even if the law (weakly) requires them to. One reaction is to say the size of that number would be a measure of the human tragedy resulting from the (postulated) price increase on health insurance. Alternatively, you might view the (potentially) large response in quantity demanded as an indication that people do not actually place an extraordinarily high value, ex ante, on having health insurance.

Again, I've found that many people hate it when this point is brought up at all. You'll hear scornful responses like "*well, if you get really sick you'll need that insurance*." Or "*what if that were your child?*" Or claims that health care and health insurance are human rights. Maybe all those responses are relevant. But they don't dent or deny or countermand the new, collateral information introduced by the marginalist perspective. You can think of marginalist perspectives as commentary or "nagging" on a lot of our standard moral intuitions. You don't have to discard the moral intuitions, but the question remains on the table whether you should accept the nagging or dismiss it altogether. The more familiar you are with marginalism, the harder it will be to send the nagging packing.

Perhaps fortunately for the advance of economic science, these and related points were not thought through carefully in the early days of marginalism. The stress, when it came to social issues, was on the higher marginal of money for the poor, and so marginalism seemed very directly egalitarian, as indeed it is in many instances. In those times and still to a considerable extent today, marginalism mostly helped the poor by boosting the rationale for social welfare schemes. [19]

[19] For Jevons on working hours, see Jevons (2002, chapter II) [1887].

But the more marginalism developed, and the more people saw its full implications, it became evident that marginalism also was a potential engine of social discomfort. It has a confrontational side, most of all because many people are not always eager to think in terms of trade-offs, or to ask themselves if their preferred values should take priority at each and every margin. The more you internalize marginal thinking as a method of inquiry, the harder it is to avoid those questions.

Some parts of the world responded by developing caricatures of economists and the economic method. Sometimes critics of the marginalist perspective snap "who ever said the marketplace is the measure of all value?" Or "not everything is about money!" But neither is a claim of marginalism or social marginalism. Marginalism, in its most basic form, directs your attention to facts about valuation, facts about trade-offs, and interpretative possibilities. Those are all features of an issue that otherwise might have been obscured by moralizing without analytic discipline.

Next up is marginalism and risk-aversion, which helped birth the economic field of finance.

V. Marginalism for Understanding Risk and Risk-Aversion

In the twentieth century economists started to realize that the theory of diminishing marginal utility gave a simple and powerful way of thinking about risk and risk-aversion. The basic theory implies that individuals should be risk-averse, for instance they should reject a fair gamble. If, for instance, you are faced with a 50-50 gamble where you either gain or lose one hundred dollars, that gamble is bad for you, at least within the simplest versions of the theory. If you win, what you can buy with the next hundred dollars is less valuable than what you have to give up if you lose. That will follow directly from the assumption of a diminishing marginal utility of wealth.

Risk-aversion rapidly became a core assumption behind theories of insurance and also strategies for consumption smoothing. Economists also were led to understanding the value of portfolio diversification. What matters is not the outcome from a single risk but rather the entirety of risks. If

various risks are negatively correlated, for instance, that means some pay off when others do not, or when others fail altogether. The overall portfolio position can be safe even if the individual is holding a considerable number of individual risky assets.

Once economists started down this path, they developed modern theories of both insurance and the pricing of financial assets, including the identification of expected excess returns. For instance, gold was once a general hedge against bad market conditions (it is less clear that it remains so today). Thus, if gold offers insurance value, that bids up its price and in turn lowers its expected return. Part of the overall net expected return to holding gold includes its insurance value, so in equilibrium gold's pecuniary returns will be somewhat lower. Conversely, a highly cyclical market asset that varies in extreme form with general market conditions offers the equivalent of negative insurance value. Its pecuniary return should be correspondingly higher, to maintain an equilibrium of overall net equal returns across assets. Some parts of this approach you might label "predictive marginalism," since the marginalism is trying to help us predict expected securities returns.

Once again, you can see that the advent of marginalism, and its application to the topics of risk and uncertainty, gave economists a great deal to do. We could go around measuring risk and trying to predict financial market returns. The resulting field of financial economics turned out to be the most lucrative field of economics ever created. That hardly hurt the fortunes of marginal utility theory.

Here is not the place to recap the entirety of financial theory, but it is worth noting that these successes of marginalism have somewhat eroded with time. In the realm of financial economics, diminishing marginal utility theories of risk seem to have lost their ability to predict excess returns. The key paper here came from Eugene F. Fama and Kenneth R. French in 1992, Fama being a Nobel Laureate and one of the original builders of the marginal utility approach to asset pricing and returns. Fama and French showed that the systematic portfolio risk of an asset, as had been understood by standard marginal utility theory, did not seem to predict its subsequent returns, at least not once the full, proper model of asset returns had been specified.

The concept of marginal utility, as applied to risk, also was dented though not destroyed by the advent of behavioral economics. It turns out that there are many different ways of measuring risk, and they induce different responses and thus differing measurements for the marginal utility of wealth. Researchers can measure risk across small gambles, across larger gambles, risks over time, risk for other people, and more. For each method of measuring risk, researchers usually get quite different results for marginal utility. According to one study by Miles Kimball and co-authors: "The estimated median value of the curvature parameter [for marginal utility] ranges from [log] 0.6 to 13.2 depending on the dimension." Those are hardly trivial differences. The curvature is stronger for small gambles than for large gambles, and often the curvature is also more pronounced when altruistic decisions are being made for other people.[20]

By no means do those results destroy the theory of diminishing marginal utility, but the shape of that curve seems to depend a great deal on the context and the details of the risk under consideration. That puts a lot more analytical power in the corner of context-dependence, and takes away some explanatory power from "pure marginal utility theory, considered alone." But at the very least, marginal utility theory was instrumental for getting these discussions off the ground.

VI. Marginalism as Organizing Category

In plenty of cases, marginalism doesn't solve any concrete problem, but it does give us better categories for organizing our thoughts.

For instance, economists are especially interested in cases where individuals or groups do not adjust at the margin, or at least they do not seem to adjust. Such cases are the exceptions, but still they are not uncommon, and we can learn by studying them.

[20] On this, see Kimball, et. al. (2024).

Richard Reeves, in his book *Of Boys and Men*, finds that men sometimes respond much less to social incentives than women do, and sometimes the men do not seem to respond at all. There was one program, from Kalamazoo, Michigan where a donor put up money so high school graduates in the region could go to colleges in the state for free. As a result of the program, the number of women attending and finishing college rose by 45 percent. The number of men finishing college did not go up at all. Other programs as well sometimes have much stronger marginal effects on women than on men, for instance early childhood education.

There do seem to be cases – usually centered around trying to help or "elevate" men of lower socioeconomic status – where many men do not take opportunities offered to them. This phenomenon is not well understood, but the simplest explanation is that they do not see the supposed benefit from a subsidy or program as a benefit at all. They may prefer their lives as they are, as opposed to being "elevated," and in that sense no additional marginal incentive has been applied at all. Whether or not that is the best explanation for a zero-elasticity response, if you are attuned to marginalism you will realize this is an important puzzle to be solved. It may well hold the key to understanding why American upward mobility for some groups seems to have stalled and is resistant to so many policy solutions.

Minimum wage hikes are another area where it is not obvious that the demand for labor falls, or falls by a significant amount. Why is there so small an adjustment at the margin, as has been found in numerous (but by no means all) research papers?[21]

One possibility is that the burden of the wage hike is passed along to landlords. That is, if a restaurant in Seattle now has to pay $20 an hour to its waiters, that restaurant in the longer run will have a lower demand to locate in Seattle, compared to other cities with a lower minimum wage. Rents in Seattle might fall accordingly, making the restaurant owner whole again, at least in the medium- or long-run. The restaurant might, at some margins, substitute capital for labor, but under

[21] For one survey of the literature, covering varying points of view, see Neumark and Shirley (2021).

these assumptions we would not see any noticeable increase in the number of restaurants going out of business. Labor demand might be pretty robust.

Whether (or alternatively *how much*) the burden of minimum wage hikes is passed along to landlords is not fully known at the moment. The import of the point is this: if you see an increase in the marginal cost of labor, and no big adjustment, marginalism tells you to start thinking around about other, more complex hypotheses. We need to find some other marginal adjustment that has occurred, and so it helps you organize your thoughts about the event in question. My former colleague Gordon Tullock used to suggest that when the legal minimum wage was raised, employers turned down the air conditioner accordingly.

To close this chapter, I'll offer one more example of an ambiguity in the word marginal. There is the well-known episode of Donald Trump speaking on the phone with a child and asking the kid if he believed in Santa. He quickly followed up with "because at [age] seven it's marginal, right?"

Just about everyone has their own conception of the margin, Trump included. Don't think marginalism is one single, simple thing.

Chapter two

William Stanley Jevons, Builder and Destroyer of Marginalism

Another way to grasp marginalism is to look at the marginalists themselves, in particular those at the origin. I'll focus on William Stanley Jevons, who is the most legible of the three marginalist founders from the 1870s. Jevons also has the strongest claim of priority. Unlike Carl Menger or Leon Walras, Jevons had formulated and stated his marginalist idea as early as 1862, while Walras presented his breakthrough only in 1873, Menger arguably in 1869. Jevons also placed marginalism at the center of his thought, whereas Walras thought of general equilibrium theory as his most important contribution. Menger is best known today as the father of the Austrian School of Economics, an important tale but not the centerpiece of this story. It was Jevons who carried marginalism into the Anglo-American research community of economists, which dominates the profession to this day.[22]

Another reason why I focus on Jevons is because he helped birth many of the forces that later started to counteract the marginal revolution and which might eventually destroy the marginal revolution. Explanation on that point will be forthcoming, but hold it in the back of your mind.

To start with origins, Jevons came from a well-to-do, intellectual, British Unitarian family. His father was an iron merchant and intellectually curious. His mother was interested in poetry, chemistry, logic, botany, and political economy, although she died when William Stanley was ten years old. Jevons's

[22] On Walras and his contributions and their timing, see Jaffe (1976, pp. 512–513). On Menger and timing, see Kauder (1965, p. 87). On general equilibrium being the most important contribution of Walras, see Walker (2006, p. 13). Howey (1989, chapter four) covers Walras and his father. Kauder (1965) and Howey (1989) offer useful overviews of the marginal revolution as a whole, including both origins and inception. Furthermore, marginalist ideas already were present on the Continent, at least as early as Cournot in 1838 and Dupuit in 1844, noting that Cournot had worked with the father of Leon Walras and that Auguste Walras already had a version of the marginal utility idea. For this contemporary economist, Cournot and Dupuit make for much more pleasant reading than does Walras, and they also stand closer to much of current economic theory.

sisters subsequently contributed to his intellectual development. By the time he was fifteen, Jevons was enrolled in college.[23]

Jevons became a true polymath, tackling such diverse subjects as economics, logic, philosophy, mathematics, statistics, chemistry, metallurgy, weather and cloud formation, maps, music, and data collection. In addition to being one of the top economists of his century, Jevons was one of the top figures in logic, building on the work of George Boole and his Boolean logic, which stressed the formalization of logic through algebra and binary states. Jevons was considered a top logician at the time, but unlike Peirce and Frege he did not advance the logic of relations and quantification theory. So his contributions did not feed into what became the dominant logic school of Bertrand Russell and Alfred Whitehead.[24]

In the 1860s Jevons built a Logical Abacus, sometimes called a logical piano, a kind of early computer that could perform (some kinds of) logical operations faster than humans could. It is held in the Museum of the History of Science at Oxford University, and you can think of its structure and operation as broadly akin to a player piano in music. It was limited in its powers, and geared mainly toward replicating Boolean logic, but extreme in its ultimate ambitions. Jevons understood the potential. In his written presentation of the project, Jevons cites the work of Charles Babbage, and noted that "material machinery is capable, in theory at least, of rivalling the labours of the most practiced mathematicians in all branches of their science. Mind thus seems able to impress some of its highest attributes upon matter, and to create its own rival in the wheels and levers of an insensible machine." Jevons understood that science would be able to tackle some of the most difficult projects, and he wanted to be on as many of those frontiers as possible. He understood that his own work was a mere beginning, and he wanted to press forward as much as possible.[25]

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[23] On the family of Jevons, see Mosselmans (2007, pp. 2–3).
[24] For Jevons on logic, see Mays and Henry (1953) and also Maas (1999) and Keynes (1936, p. 542). For Jevons on music, see Mosselmans (2007, pp. 83–105) and Mosselmans and Mathijs (1999). More generally, see Maas (2005, including p. 35). Jevons's most important work on logic was Jevons (1958 [1874]).
[25] See Jevons (1870, the quotation from p.498), and also Maas (2005, chapter six). For a general background on Boolean logic, see Hailperin (1986).

Jevons also studied molecular motion in liquids and developed the concept of "pedesis," a precursor of what we now call Brownian motion. That said, Jevons thought his pedesis was an electrical phenomenon related to osmosis, and so he turned out to be incorrect in his fundamental hypotheses. Nonetheless, this topic, like the others, showed he was an observant mind and obsessed with developing theories to explain anything and everything. He wasn't just a pedant, rather he made real contribution to a number of scientific fields above and beyond economics.[26]

Jevons also was a "born collector" in the words of Keynes, and an extreme bibliomaniac. He accumulated thousands of books, and he lined the walls of his house and attic with them, and also stored them in piles in the attic, which became a problem for his wife upon his passing.

Keynes described Jevons as "strongly introverted," "not intimate with … anyone," a loner, and "excessively sensitive to noise." The contemporary reader wonders if Jevons was autistic.[27]

Jevons started his career with real world experience, dropping out of college and becoming an assayer at the Sydney Mint in Australia, where he worked for almost five years. His later writings on monetary economics sprang from this early experience. The work as an assayer, constantly measuring quantities, also spurred Jevons's later interest in data collection and quantitative measurement; the job was a good place to start for someone with those interests. During Jevons's time in Australia, he pursued his interests in chemistry, weather, barometers, and measurement in science more generally. [28]

[26] On Jevons on pedesis and Brownian motion, see Brush (1968).
[27] See Keynes (1936, pp. 540–541), and on the personal description of Jevons see pp. 545–547).
[28] See Keynes (1936, p. 518), and on Jevons in Australia more generally see Black and Könekamp (1972, vol. I, p. 35).

The first visible inkling of marginalism in Jevons's thought comes in his early 20s, in 1860 (June 1), when he is writing a letter to his brother Herbert. The letter is worth quoting at length, because it shows Jevons understood marginalism and its import well before his 1871 publication. Here goes:

"During the last session I have worked a good deal at political economy; in the last few months I have fortunately struck out what I have no doubt is *the true Theory of Economy*, so thorough-going and consistent, that I cannot now read other books on the subject without indignation. While the theory is entirely mathematical in principle, I show, at the same time, how the data of calculation are so complicated as to be for the present hopeless. Nevertheless, I obtain from the mathematical principles all the chief laws at which political economist have previously arrived, only arranged in a series of definitions, axioms, and theories almost as rigorous and connected as if they were so many geometrical problems. One of the most important axioms is, that as the quantity of any commodity, for instance, plain food, which a man has to consume, increases, so the utility or benefit derived from the last portion used decreases in degree. The decrease of enjoyment between the beginning and end of a meal may be taken as an example. And I assume that on an average, the *ratio of utility* is some continuous mathematical function of the quantity of commodity. This law of utility has, in fact, always been assumed by political economists under the more complex form and name of the Law of Supply and Demand. But once fairly stated in its simple form, it opens up the whole of the subject. Most of the conclusions are, of course, the old ones stated in a consistent form; but my definition of capital and law of the interest of capital are, as far as I have seen, quite new."[29]

Jevons, by the way, did publish this in his "Notice of a General Mathematical Theory of Political Economy" in 1866, and it was read out loud in 1862 in his absence to a meeting of the British Association in Cambridge.[30]

[29] See Keynes (1936, pp. 531–532) and Jevons (1886 [1860], p. 151).
[30] Keynes (1936, p. 532), and of course Jevons (1866) [1862].

That passage, and the subsequent follow-up, is significant for a few reasons. Jevons was fully self-aware of the import and comprehensive nature of his work, and how it would encompass theory, mathematics, statistics, and real-world observation. He really "got it," and early on. This passage shows that such an extreme awareness was needed to appreciate the import of the marginal revolution. Jevons admitted that the laws of supply and demand already were in place in economic theory and reasonably well understood. It is thus no surprise that most economists of that time could not grasp the import of the marginal revolution. They had a local rather than a global vision, and even the more sympathetic commentators often could not see what kind of big change might be coming to economics. As Jevons wrote, "it [marginalism] opens up the whole of the subject."

Thus, among his many other intellectual virtues, Jevons understood why his ideas might face some tough sledding in the immediate future. He wrote to his brother in September of 1862 about his Cambridge paper: "Although I know pretty well the paper is perhaps worth all the others that will be read there put together, I cannot pretend to say how it will be received – whether it will be read at all, or whether it won't be considered nonsense."[31]

The canonical form of Jevons's theory of marginal utility was outlined in his 1871 *The Theory of Political Economy*. Throughout the book, Jevons knew he was on to something special, and he knew he finally had worked the idea out, albeit not fully. The very introduction to the book starts with his key point about the relevance of utility, and he knew he would be dragging British classical economics away from its obsession with cost and costs of production. On the first page of that introduction, he put it succinctly:

"I show, on the contrary, that we have only to trace out carefully the natural laws of the variation of utility, as depending upon the quantity of commodity in our possession, in order to arrive at a

[31] Keynes (1936, p. 532).

satisfactory theory of exchange, of which the ordinary laws of supply and demand are a necessary consequence."[32]

As for marginal utility more directly, Jevons stated what we would now call the marginal utility principle as follows: "We may state as a general law, *that the degree of utility varies with the quantity of commodity, and ultimately decreases as that quantity increases*."[33]

Jevons then outlines this perspective throughout the book. His key framing is that the allocation of any commodity or money will be performed in a manner to equalize its marginal return in any use. So the marginal value of a unit of barley used to make beer will end up equal to the marginal value of a unit of barley used to feed cattle. Note, however, that marginal and marginalism were not the preferred terms of Jevons, rather he preferred to refer to the final degree of utility.[34]

Jevons understood that from that insight it would be possible to rebuild the entire science of economics. Jevons also presented this point in terms of calculus and derivatives, and furthermore in graphs, not just in words, again pointing to the major impetus the marginal revolution gave to mathematical economics.

Jevons also lays out applications of the basic theory, for instance considering the allocation of labor versus leisure, the judging of probabilities and expected value, and arguing that the equalization of marginal returns should hold for resource decisions through time. He also understood that the ratio of marginal utilities for a good, after-market exchange, would stand in proportion to the ratio of their prices. That was an implication of the equalization of returns at the margin. Jevons's general background in the sciences had prepared him for seeing the broader application of marginalist principles.[35]

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[32] Jevons (1970, p. 77) [1871].
[33] See Jevons (1970, p. 111) [1870].
[34] For the barley example, see Jevons (1970, p. 115) [1871].
[35] On the intertemporal element, see p. 123. On prices and ratios of marginal utilities, see for instance p. 139. On expected value, see p. 183, on labor and leisure see p. 194. Those are all from Jevons (1970) [1871].

In one of his lectures, Jevons put it clearly: "Now the degree of utility of any commodity means the utility of the last portion wh. [sic, a stand-in for "which"] has come into use." The same lecture shows how Jevons thought of declining marginal utility as a schedule that could be graphed in either discrete or continuous space. He applied calculus to the problem and solved for first-order conditions, much like a modern economist.[36]

Jevons understood the radical nature of his marginalist approach. We are told in *Theory of Political Economy* that marginalism will apply also to the determinants of wages and rent. He did not spell out a full marginal productivity theory of distribution, as J.B. Clark and others did later, but he knew where his doctrine was headed. He stressed that the entire mainstream Ricardian apparatus would have to be abandoned, and that the classical economists were wrong about distribution, and their fears that rent would absorb an increasing amount of the social surplus. From now on, the marginalist determinants of rent would be just like the marginalist determinants of the wages of labor. Thus, Jevons brought a methodological unity to the theory of pricing and distribution.[37]

More generally, marginalism is a theme that runs throughout Jevons's work, even before he realized that he had discovered marginalist principles.

His second best-known book (or is it now his best-known book, due to environmentalism?) is *The Coal Question: An Inquiry Concerning the Progress of the Nation and the Probable Exhaustion of Our Coal-Mines*, which expressed a pessimism about Britain's reliance on coal. Circa 1865, Jevons feared that Britain would run out of coal, which would significantly restrict future economic growth. The early chapters present geological arguments which in retrospect turned out to be far too pessimistic. But pessimism about British supply and pessimism about energy more generally are two quite

[36] Jevons (1977, vol. VI) [1875–1876], p. 85. Note the sentence is taken from the lecture notes of one of his students, but the notes are generally well organized, and their reflection of Jevons's actual views is confirmed by many passages. As for the graphing and calculus, see pp. 86–87.
[37] See for instance, the Preface, Jevons (1970, p. 69) [1871] and also the latter book chapters on labor, capital, and rent.

different things. Circa 2024 the world still has plenty of coal, though it is now viewed as a much less propitious energy source because of pollution and climate change.

Jevons's real contribution comes in chapter seven "Of the economy of fuel," where he presents a decidedly marginalist argument, though without recognizing it as such in 1865. Jevons is pessimistic about the ability of conservation to extend the supply of coal indefinitely. Let us say for instance that energy use becomes more efficient. In the short run, the effect is more conservation. But to use modern terminology, the price of energy, coal-based or otherwise, has declined. With energy prices lower at the margin, we will in turn use more energy. Much of the initial savings from conservation will be exhausted by the new, greater demand for energy. This is now called "The Jevons Paradox," and it is a central idea in environmental economics, especially when conservation is discussed.[38]

Jevons put it as follows: "It is wholly a confusion to suppose that the economical use of fuel is equivalent to a diminished consumption. The very contrary is the truth." Whether or not you agree exactly with that proposition as stated (yes, there is some offset, but is it the "very contrary" that holds?), it is similar to the examples of "intuitive marginalism" that open this book. It can be said that Jevons was a marginalist in his thinking before he discovered marginalism in the formal sense of the term.

The Coal Question was in any case a hit in Jevons's own day. It led to him to consultations with Gladstone at Downing Street and at the age of thirty appointment to the Cobden Chair in logic and political economy at Owen's College at Manchester.[39]

The Jevons Paradox had a bit of a heyday in January 2025, when China's artificial intelligence model DeepSeek hit the market. As you may already know, DeepSeek can perform high-quality artificial intelligence calculations using fewer and cheaper chips than was the case for the best extant models.

[38] See Jevons (1865), and for historical background on *The Coal Question* see Klitgaard (2022).
[39] See Maas (2005, p. 33) on the reception of *The Coal Question*.

The share price of Nvidia, supplier of the very best chips, fell more than 10 percent in one day. A furious debate started, most of all on Twitter. Would the Jevons Paradox hold, or more concretely, would the demand for high-quality chips go up or down? On one hand, a kind of conservation was achieved, namely, that a great system could be built with fewer chips. That should, at least in the short run, lower chip demand. On the other hand, this will over time increase the demand for AI services and also the demand for chips. How are the net effects going to fall?

On January 27, 2025, Microsoft CEO Satya Nadella tweeted: "Jevons paradox strikes again! As AI gets more efficient and accessible, we will see its use skyrocket, turning it into a commodity we just can't get enough of," with a link to the Wikipedia article on the Jevons Paradox. (Was he trying to defend his own share price there?) He was hardly the only one. Later in the day, Kevin Roose, the well-known *New York Times* tech reporter, in response to the growing discussion tweeted: "just realized i'm going to hear about Jevons Paradox at every party and work event i attend for the next year."

In other words, the Jevons Effect is not going away, and we can be pleased that Jevons's name remains attached to the concept.

Jevons and Social Marginalism

One reason Jevons is so interesting is because he embodies so many different sides of marginalism, as discussed in chapter one, and that he did so very early on.

In *The Theory of Political Economy* Jevons talks repeatedly of utility as a cardinal magnitude susceptible to measurement. He cites Bentham, and presents utility as an actual magnitude with a real existence in the world. In contrast, later neoclassical approaches such as those of Samuelson (and some of the later Austrians) treat utility as simply the economist's analytical representation of ordinal choice. Not so for Jevons. Jevons does offer conflicting remarks on the interpersonal comparison of utilities, but

the overall thrust of his argument is that utility variables have social meaning, relevant to an understanding of broader social welfare.[40]

Jevons did not shy away from talk of higher- and lower-order wants, following in the footsteps of Mill and many of the other British utilitarian writers. Thirst and hunger, for instance, are among the most fundamental wants, and they are greater in import than say the desire for nice clothing. Furthermore, the desires for food and water must be satisfied before other wants can come into play. In one of his lectures, Jevons noted that such a point was central to the theory of value.[41]

Here we see Jevons showing his allegiance to what I have labeled social value marginalism. If the importance of satisfying wants declines in sequence, once the most fundamental wants are satisfied, we again have reasons to compare the impact of more resources for one group of people (the rich), relative to the other group of people (the poor). Along those lines, Jevons had expressed a hedonistic concept of utility as early as 1856, and later Jevons's son described his approach as stemming from that of Bentham and based on a psychological notion of utility.[42]

Jevons grew up in Liverpool, where the problems of poverty were highly visible, even by the standards of his time, which may account for his interest in social reform. Massive in-migration from Ireland, following the potato famine and its aftermath, made Liverpool especially poor even for its time. Later, Jevons was influenced by the novels of Charles Dickens, and he also took care to walk through poor districts of London, to better observe living conditions at the time. He was curious about almost everything.[43]

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[40] For the passage of skepticism about interpersonal utility comparisons, see Jevons (1970, p. 85) [1871]. For Jevons's broader take on utility, see pp. 92–107.
[41] Jevons, (1977, vol. VI) [1875–1876], p. 14. Note the relevant passages are taken from the lecture notes of one of his students, but the notes are generally well-organized, and their reflection of Jevons's actual views is confirmed by many passages.
[42] Black (1972, vol. I, pp. 27–28), and H.S. Jevons (1936, p. 550).
[43] Black (1972, vol. I, pp. 16–17).

In one of his lectures, Jevons states clearly that proportional taxation is not a proper utilitarian approach, and he cites principles of marginal utility to establish that conclusion. Taking away 10 percent of the income of a poor person, for instance, damages utility much more than taking away 10 percent of the income of a wealthy person. Jevons did not shy away from making such interpersonal utility calculations, based on his premises.[44]

Yet Jevons is ever the sophisticated thinker, and he states that the proper principles of taxation have to consider many factors, not just marginal utilities. He worried for instance that the larger number of poor voters could lead to a situation where the poor keep on voting themselves wealth redistribution, simply because they outnumber the wealthy. Overall, Jevons emphasized that the practical and theoretical sides of these questions could be quite distinct, and that Smith's argument for proportionate taxation could end up being the best available approach, all things considered.[45]

In general, Jevons favored government action to alleviate the problems of poverty, consistent with his views on marginal utility. He was hardly a socialist, but he argued for government intervention in sanitation, housing, recreation, the arts, public education, and restrictions on child labor, to boost human welfare. He placed a special emphasis on individuals cultivating their faculties so they might enjoy the benefits of an increasingly modern Victorian society. As Bert Mosselmans and Ernest Mathijs have pointed out in their study on Jevons on music, even his proposals for public music performance and education were motivated by a desire to cultivate and elevate taste and morals, within the framework of his broader utilitarianism.[46]

If you are wondering about the rest of the original marginalists, Walras, like Jevons, had broad interests. Before writing his major economic works, he worked as a journalist, wrote a novel, worked in a bank, in a railroad office, and then again for banks. Walras developed strong interests in tax

[44] Jevons, (1977, vol. VI) [1875–1876], pp. 135–136.
[45] Jevons, (1977, vol. VI) [1875–1876], pp. 135–136.
[46] See Jevons (1965) [1883], and on those writings Schabas (1990, p. 29). Specifically on music see Mosselmans and Mathijs (1999).

issues, most of all the taxation of land, and he wanted to pursue the idea of land nationalization. He was influenced by French utopian traditions, and he had strong ideas about how to create an ideal society, by policy engineering backed by economic science. Marginalism for Walras, like his general equilibrium model, was a means to that end, and so he too was a social marginalist.[47]

As for Carl Menger, his background was in journalism. He started in Lemberg (now Lviv in Ukraine) and later worked for several newspapers in Vienna. Unsurprisingly, Menger had a strong interest in real world issues, because he started his career with them. After his 1871 *Principles*, which laid out his version of marginal utility theory, Menger also served as the personal tutor to Crown Prince Rudolf of the Austro-Hungarian Empire. The originally assigned subjects were math and statistics, but Menger went further and taught him economics, the principles of free trade, and Menger also made some effort to interest him in journalism.[48]

Jevons Led Both the Marginal Revolution and the Average Revolution

Can we say that for Jevons it was the marginal that ruled in theory, but the average that ruled in measurement and practice? And this is not just a statement about Jevons. His entire historical period saw both the rise of marginalism but also the rise of quantitative and statistical techniques in economics and the other social sciences. "Marginalism" and "averageism" came together, as part and parcel of the same package. The late 19th century, whether in economics or elsewhere, was as much about "the average revolution" as about "the marginal revolution." Note that the statistics of that time, much more than today, was very explicitly about measuring, calculating, and estimating averages.

Lambert Adolphe Jacques Quetelet (1796–1874), 19th century Belgian mathematician, arguably has as good a claim as anybody to be regarded as the founder of econometrics. He was central in

[47] On Walras, see Walker (2006).
[48] On Menger as journalist, and also as tutor, see Yagi (1992).

applying quantitative methods to the social sciences, but his general approach did not embrace marginalism. His vision, as he developed it throughout the 1830s, was to use statistics to develop an account of the "average man," based on quantitative data. He also viewed mankind as following an arc of moral and intellectual progress. There was a notion of the "average man" for each culture, and over time societies will approach some vision of a superior average man overall.[49]

More generally, science historian Victor L. Hilts has stressed that, "The basic tasks of statistics during most of the nineteenth century was the use and interpretation of statistical averages." For all the later statistical advances that moved beyond the concept and measure of the average and closely related concepts, they were mostly not available at this time, and so the emphasis on averages was very closely tied to a loyalty to statistical methods.[50]

When it comes to the era of Jevons, you might wonder, "why the rise of interest in the *marginal* at that point in time?" And that is a good question, to which I will return. But perhaps the more important question is, "why the rise in all those *-isms* at that point in time? Including both marginalism and averageism among many others?" That question will point your attentions in a very different direction. Marginalism was indeed strong, but it was not unique. Along with the rise of marginalism in the late 19th century came concomitant movements, such as the emphasis on the statistical average as a means of measurement.

Jevons's loyalty to the relevance of the average is expressed repeatedly throughout his work. In *The Theory of Political Economy* he emphasizes the difference between the individual and the average. He notes that marginalist theory presumes to investigate the individual mind, but much of the actual action in economics comes at the level of aggregates. What we now call the law of demand, for Jevons, operates at the aggregate level, not the individual level. If the price of sugar goes up, Jevons notes that many individuals will not curtail their sugar consumption. But the market as a whole will:

[49] See Mosselmans (2007, pp. 28–33) for one good account of Quetelet and his relevance here. See also Gigerenzer, et. al. (1989, chapter two), covering also Wilhelm Lexis and Henry Buckle on related issues.
[50] See Hilts (1973, p. 208).

"It would be by examining the average consumption of sugar in a large population that we might detect a continuous variation, connected with the variation of price by a constant law. … The use of an average, or, what is the same, an aggregate result, depends upon the high probability that accidental and disturbing causes will operate, in the long run, as often in one direction as the other, so as to neutralize each other."[51]

Early, pre-marginalist Jevons had read Quetelet, cited him several times, and wondered about his arguments on the properties of averages and aggregates. In particular, Jevons wondered about whether outliers would cancel out, leaving averages as good predictors for systems as a whole.[52]

Jevons sought to minimize the distinction between social and natural sciences, and to put the social sciences within the framework of physiology, which would make them amenable to quantitative and scientistic methods. The laws of human beings suddenly would become more like the laws of mechanics, and mental phenomena would be measurable and also subject to the laws of averages. In the preface of his *The Theory of Political Economy* Jevons spells out this connection with mechanics and the desire to apply mechanics to the social sciences. If you look at the introduction to *The Theory of Political Economy*, the first page is spent outlining the marginalist idea. Immediately thereafter, Jevons spends pages arguing for quantification and the unity of the social and mechanistic sciences. For Jevons, *The Theory of Political Economy* really was a plea for two distinct revolutions, not just what we now call the marginal revolution but a quantitative and statistical revolution as well. Margaret Schabas, a Jevons scholar, described Jevons as the most important figure "shifting the tide" toward mathematical economics.[53]

John Stuart Mill and John Elliott Cairnes had stressed the differences between the social sciences, which deal with data from human beings, and the physical sciences, which deal with the natural

[51] See Jevons (1970, p. 86) [1871].
[52] On the influence of Quetelet on Jevons, see Mosselmans (2007, pp. 34–35).
[53] Again, see Maas (2005, p. 154). For the preface, which is to the second edition, see Jevons (1970, p. 50) [1871]. As for the talk of quantification in the introduction, see Jevons (1970, pp. 78–84). On shifting the tide, see Schabas (1990, p. 4).

world. For both Mill and Cairnes there was an unbridgeable gulf between the two approaches and thus a strong methodological dualism. Not surprisingly, the later modern Austrian economists of the 20th century were much enamored of Mill and Cairnes for these writings. Jevons, in contrast, stood on the opposite side of these debates. As it was expressed by Jevons scholar Haaro Maas: "Jevons's innovation was to move the basis of political economy from the laws of mind to those of physiology."[54]

One also can read Jevons's work on index numbers as engaging with marginalist ideas, but not in every way siding with the overriding relevance of marginalist principles. Jevons was interested in calculating how much the value of gold had fallen over a particular time period. That is on one hand a marginalist interest, namely by how much has the value of money fallen and how much have the prices of goods and services increased? Yet it is also a question about averages, and techniques concerning averages will be needed to address those queries. In his 1863 work *A Serious Fall in the Value of Gold Ascertained, and Its Social Effects Set Forth*, Jevons very carefully calculated what he thought was the best approach to index numbers, and thus the best way of measuring the extent of monetary depreciation. This is still considered one of Jevons's most important contributions to economics, along with marginalism and the Jevons Paradox for coal or energy use.[55]

To evaluate the decline in the value of gold, Jevons's primary approach used a geometric mean rather than an arithmetic mean. Perhaps influenced by Quetelet, Jevons was worried about errors in the data, and the geometric mean does a better job of balancing out measurement errors on both sides. Jevons thus is concerned with finding a true average of sorts, and he resorted to related averaging techniques in some of his work on Australian climate and temperature. It is the average (Jevons hoped) that will have the most predictive power over subsequent economic activity. In any case,

[54] Maas (2005, p. 154).
[55] Reprinted in Jevons (n.d.), original publication of the volume 1884, original publication of the study 1863.

Stephen M. Stigler described this Jevons work on currency values "as careful and as intelligent a piece of empirical work as can be found at the time."[56]

Jevons's subsequent *On the Variation of Prices and the Value of the Currency since 1782* was read before the Statistical Society in 1865. That too was a quintessential marginalist empirical question – how big was an observed change? – but resolved by methods focused on the average. We can see Jevons drawing on his background on a gold assayer to find methods of calculation that would give us a better sense of economic reality, again as expressed through averages.[57]

Jevons's progress on index numbers was significant, causing Keynes to write: "Jevons had to solve the problem of price index-numbers practically from the beginning; and it is scarcely an exaggeration to say that he made as much progress in this brief pamphlet as has been made by all succeeding authors put together. He examines the logical and dialectical problem, the question of weighting, the choice between an arithmetic and a geometric mean, whether articles which have moved abnormally should be excluded, and, generally speaking, what classes of commodities can best be taken as representative."[58]

Index numbers, of course, are averages. That topic was Jevons's first major contribution to science, and it continued to hold his attention throughout his career.

Keep in mind that for Jevons this was not mere statistical work, but rather it was aimed toward improving social outcomes. With the best index numbers in hand, Jevons advocated the move to a tabular standard, namely contracts and prices would be posted in terms of the best constructed index of prices rather than in terms of some weight of gold or silver. The medium of exchange

[56] See Maas (2005, pp. 267–268, and p. 35 on the Australian climate work). On the general import of Jevons in the history of statistics, see Stigler (1999, chapter 3, and pp. 72–73 on geometric vs. harmonic means for Jevons. The Stigler quotation on index numbers is from p. 77). Stigler (1986) covers the surrounding rise of statistical reasoning more generally.
[57] On the 1865 reading, see Keynes (1936, p. 527). For Jevons more broadly on the import of the balance, including in chemistry, see Maas (2005, pp. 259–263).
[58] Keynes (1936, p. 525).

would be banknotes, with the notes convertible into however much gold was required to make good the defined amount of value in terms of the index tabular standard. Jevons saw such a system as checking both inflation and deflation, boosting macroeconomic stability, and improving the fairness of contracts. It wasn't quite "engineering marginalism," but a tongue in cheek "engineering averageism" would do.[59]

Jevons's obsession with the average also showed up in his macroeconomic work on the "sunspots" theory of commercial cycles. Jevons had presented evidence that business cycles occur at a typical regularity of about ten and a half years. At the same time, astronomers had told him that the solar period had a typical regularity of about ten and a half years. He concluded that the two phenomena were related, but never successfully pinned down the evidence or the case for a causal relationship. What he had in the back of his mind was that the right solar patterns led to good crops, which in turn boosted optimism and the positive phase of the business cycle. Whatever you think of this theory (the estimations of the economic and solar cycles and their lengths have not subsequently been confirmed), it is again based on a notion of mathematically measuring and correlating averages, not the operation of marginalist principles in a market setting.[60]

To the extent one is on board with the Jevons program, it is easy to see how the social sciences become more and more mathematical, and indeed that is what happened. And in turn that reliance on statistics brought a greater initial interest in averages. As statistics developed, of course, it became more sophisticated and embraced more techniques that are not so oriented toward measuring average quantities. Current statistical techniques of causal identification, difference-in-differences, seemingly unrelated regressions, time series, machine learning and much more have *something* to do with averages, but they are not obsessed with averages as 19th century statistics was. Averageism leads to quantification and measurement and identification more generally, with both marginalism

[59] The tabular standard idea recurs in Jevons's writings on monetary economics, for the argument most forcefully see Jevons's "An Ideally Perfect System of Currency" (1884), originally authored 1875 but not published then.
[60] See Keynes (1936, pp. 528–531) on this episode. Stigler (1999, pp. 77–78) considers some of the problems, statistical and otherwise, with Jevons's work on sunspots.

and averageism taking a back seat in terms of where the emphasis is applied. The research program of William Stanley Jevons is thus both the birth and (eventual) dwindling of marginalism.

You also can think of this in terms of competitive processes. Once you reach a certain level of microeconomic prowess, it is hard to "understand marginalism better than the other person." So competition moves into other fields of endeavor, including finding new data sets, working with the data sets, building models to fit that data into, doing the best econometrics, and writing the proper programs along the way. To put it simply, that is a lot of work. Marginalist insights lie behind some of that, but so does the air you breathe, and we do not organize economics around the value of free air. Instead, we focus on the grounds of relevant competition across economists, and those are centered on math, programming, and statistics. I'm not trying to argue this is good or bad, it is just how it is. In 1906 Knut Wicksell was a big deal because he applied marginalism better than did his peer economists. It is difficult to make comparable claims about many individual economists today.

We now can see why Jevons's version of the marginal revolution won out over the version of Carl Menger of the Austrian School. Menger's marginalism emphasized the mind of the individual, and Menger himself, like most of the subsequent Austrian School, accepted the Mill-Cairnes distinction between the methods of the natural sciences and the methods of the social sciences. There is no simple path from the approach of Menger to an emphasis on averages and statistics. There is instead a clear path from Menger to an emphasis on the *interpretative* and also the *biographical*, although biography is not exactly the path the later Austrian school took, much less the mainstream of the economics profession. So the Jevons strand of the marginal revolution is the live, active strand, in part because it contained averageism. That also means the Jevons strand of marginalism contained the seeds of its own destruction. And that is why this is a chapter on Jevons, rather than Menger or for that matter Walras.[61]

[61] The marginal revolution trio of economists were never constituted as such or even fully self-aware of the broader developments they were ushering in. Jevons for instance did not even know of Menger's work. Menger in turn thought that the mathematical approach to economic phenomena was flat out wrong. Jevons and Walras conducted a friendly correspondence between 1874 and 1882. The two had obvious shared interests, and they shared the

What Did Jevons Bring That Was New?

Finally, this emphasis on the empirical helps explain why Jevons and not his precursors drove the marginal revolution. Jevons's approach to marginalism did not spring fully from his head in 1871, and he was well aware of his numerous precursors, including in the British literature, not just the French, such as Augustin Cournot's marvelous mathematical and marginalist analyses from 1838.

For instance, Jevons cited the unknown Richard Jennings, a British author who published a political economy tract in 1855. The relevant part of Jennings focused on sensations, namely their quantity and quality and also how they varied with the quantity of a commodity held by an individual. The roots of the marginal utility insight were indeed in those passages by Jennings, just as they were in Galileo. Jevons himself described Jennings as the writer who "most clearly appreciated the nature and importance of the law of [diminishing marginal] utility."[62]

Jennings, like Jevons, had a complex notion of "value," and he was not seeking to reduce marginal utility to simple ordinal rankings, as Paul Samuelson later tried to do. For Jennings, value was a "complex *mental* conception," and it had its static and dynamic elements. To this reader, Jennings was the most muddled of the relevant Jevons precursors, but perhaps it was that imperfect exposition that caught the eye of a man who at the time was himself confused. In any case, we see once again that the origin of marginal utility concepts is rooted in complex rather than simple notions of

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background of being neglected in their home environments. Throughout the correspondence it is hard to avoid the sense that some rivalry was seeping through. Walras chided Jevons for not sufficiently recognizing the import of his work, including of course on marginal utility theory. Jevons wrote Walras and told him that he would create a new book with full references to Walras's work, the writings of Cournot and Gossen (two marginalist precursors) and more. Yet this never happened, and Walras ended up communicating his disappointment to Jevons in a letter. Jevons, however, had credited the ideas of Walras in some of his post-1874 papers and lectures. Nonetheless Walras later accused Jevons of plagiarism. See Black (1972b), p. 374, and see also Black (1992, p. 119 on plagiarism charges). On Jevons not knowing Menger, see Schabas (1990, p. 5). For Menger on the mathematical approach, see Kauder (1965, p. 90).

[62] Jevons (1970, p. 112 [1871]. For more on Jennings, and his relation to Jevons, see White (1994) in addition to Jennings himself (1969 [1855], with some marginal utility passages on pp. 166–192, especially pp. 166–167.

marginalism, and that complex notions of marginal utility were necessary for the idea to take off. The ever-insightful Cairnes, when reviewing Jennings's book in 1857, noted that if the Jennings approach were to win out, emphasizing "laws of mind," political economy would become "a wholly different study." Cairnes was right.[63]

Henry Dunning MacLeod, also from the middle of the 19th century, in this case 1858, understood that value was related to the conditions of supply and demand in the marketplace. He didn't state the concept of the margin explicitly, but his book *The Elements of Political Economy* impelled Jevons to develop his basic point. In a three-page section, MacLeod presents an account of how value in use depends on how much of a commodity an individual holds. He states and resolves the diamonds-water paradox with clarity, and to this day it would be hard to improve on the quality of the presentation. Most of the book concerned banking and credit, however, and MacLeod did not claim to be leading a revolution in value theory. MacLeod has been cited by some more contemporary free banking theorists, and for his credit theory of money. For the most part, however, he has fallen between the cracks, both in the history of economic thought and also in his own day.[64]

When it comes to investigating economics mathematically, and also to fleshing out the broader implications of marginalism at the supply level, Jevons probably was most indebted to Dionysius Lardner and his 1850 treatise *Railway Economy*. This really was a book about trains, written by an Irish engineer and scientific polymath, perhaps with a mind a bit like that of Jevons himself. Parts of the Lardner book presented the ideas of marginal cost and average cost clearly. Lardner also understood the difference between fixed and variable costs, all of those distinctions being explained in the context of managerial decisions faced by railway companies. Lardner understood that for-profit

[63] On the Jennings notion of utility and its complexity, see White (1994, p. 204). On Cairnes on Jennings, see White (1994, p. 223). On some of the precursors of the marginal revolution from psychology, see Moscati (2013).
[64] On relevant precursors, see Robertson (1951). McLeod's presentation is McLeod (1858, pp. 51–53). Some of the precursors neglected by Jevons include John Craig, Samuel Bailey, W.F. Lloyd, and Mountifort Longfield; see Robertson (1951). Jevons's son, H.S. Jevons (1936, p. 550), also cited Nassau Senior as a major influence on his father.

railways were seeking to equalize marginal revenue and marginal cost, though none of this was presented as an economic or even conceptual revolution. Lardner was a well-known engineer, and a public figure due to his promotion of the idea of scientific progress. Nonetheless it is to Jevons's credit that he read the book carefully enough to perceive and take away the marginalism embedded in the basic framework.[65]

Finally, there was Fleeming Jenkin, an early pioneer of supply and demand analysis. Jevons himself admits that the publication of some Fleeming Jenkin papers in 1868 and 1870 led him to hurry up and publish his own work in 1871. Fleeming was a British engineer, important in the development of the underseas telegraph, author of a serious critique of Darwin's *Origin of the Species*. He even had a memoir of his life written by Robert Louis Stevenson. In economics, Jenkin's main essay of import was "The Graphic Representation of the Laws of Supply and Demand, And Their Application to Labour" from 1870.[66]

On the second page of this essay, Jenkin does something Jevons never did, namely he draws a downward-sloping demand curve. He also draws an upward-sloping supply curve and shows that market equilibrium occurs where the two meet. It gets better yet. Jenkin is not using modern terminology, but he has good discussions of market expectations, costs of production, and issues of bargaining power in the context of labor. He was flat out a very good economist, at least on issues of supply and demand. He also understood the difference between the shift of a curve and movement along that same curve. You won't find in Jenkin any explicit presentation of marginalism, but he comes oh so close to the concept and in some regards he leapfrogs it into market analysis. Perhaps for that reason he was not so focused on finding it and stating it properly. In an 1871–2 volume he published a short essay on how his supply and demand analysis could be used to analyze problems

[65] See Lardner (1968, pp. 191–265) [1850]. On Lardner, see Hays (1981).
[66] Cookson and Hempstead (2000) is the biography of Jenkin. Morris (1994) covers Jenkin's critique of Darwin, which was serious and induced Darwin to reassess his views somewhat. Two of the main points were whether random mutations would be mostly cancelled out by having the contributions of two parents, and also whether the Earth was old enough to have supported Darwinian processes. On Jenkin hurrying the publication of Jevons, see Peart (1996, pp. 81–82).

of tax incidence. Margins play a role in the analysis, though he did not explicitly state the concept as Jevons (and McLeod) did.[67]

So what then was the critical contribution of Jevons? McLeod nailed the resolution of the diamonds-water paradox in 1858. Why is Jevons, and say not Dionysus Lardner, the British father of the marginal revolution? Or what about Fleeming Jenkin, who comes closer to the fundamentals of supply and demand theory and also their applications?

Most of all, Jevons prevailed because he was a systematizer, and he communicated the desire to systematize to others. He wanted to systematize economics, he wanted to systematize science, and he wanted to systematize logic and philosophy. None of his marginalist precursors could make comparable claims or anything close. None of them promoted statistical methods for the social sciences, and none of them tried to integrate what I am calling marginalism and averageism. As Keynes put it: "Jevons was the first theoretical economist to survey his material with the prying eyes and fertile, controlled imagination of the natural scientist."[68]

You can think of Jevons's work with the logic machines as of a piece with his economics. He wanted to apprehend – and ultimately steer – all aspects of human life on a rigorous, scientific basis. That vision came along at the right time as Britain was building out an industrial and highly scientistic society. It is thus no surprise that he ended up carrying the marginalist torch forward in British economic thought. In these regards, his precursors simply could not compete.

Sadly, in 1882 Jevons drowned at the age of 46 near Hastings, leaving behind a wife and three children. If you read his obituary in *The Economist*, he is praised first and foremost for his work in logic ("by far the most able and original of our living logicians"), and only secondly for his

[67] For the latter essay, see Jenkin "On the Principles Which Regulate the Incidence of Taxes" (1887) [1871–2].
[68] Keynes (1936, p. 524).

contributions to economic science. His skill as a musician also was mentioned in the first paragraph, as was his work in chemistry and meteorology.[69]

In sum, Jevons was the engineer's economist. He turned value into a rate, built a machine to test inference, and insisted that measurement beats dogma. He got some calls wrong (sunspots), but he made the marginal intellectually irresistible. In the AI era, when efficiency gains seem to promise frugality and deliver expansion, the Jevons Paradox returns as a live policy question. At the margin that is of course.

That brings our story about Jevons to an end, but the broader story of economics still stretches both backwards and forwards in time, and with that we will continue.

[69] See *The Economist*, 19 August 1882, vol. 40, pp. 1032–1033, reproduced in *William Stanley Jevons: Collected Reviews and Obituaries* (2002, vol. 2), pp. 297–299.

CHAPTER THREE

Why Did It Take So Long for the Science of Economics to Develop?

To better understand the Marginal Revolution, we need to ask some fundamental questions about economics as a science. In particular, why did it take so long for economic reasoning to develop? I don't even mean as a full, literal science, replete with advanced econometric methods, but simply as a general conceptual toolbox for intelligent people. The lateness of the Marginal Revolution is part of a broader story about the lateness of economic reasoning more generally.

Going back well before the Marginal Revolution, the most commonly cited founder of modern economics, Adam Smith, did not publish his major work until 1776. And Smith did not have an entirely clear grasp of supply and demand, or for that matter their underlying marginalist foundations. Smith intuitively understood the basic principles of supply and demand, but compared to later economists his treatment seems like a muddle. Arguably James Steuart, a Scot who published his major work in 1767, had a clearer understanding of supply and demand than did Smith.[70]

If you go back a generation or two before Smith, to Richard Cantillon (1734 text, 1755 publication), while you will not find claims at *variance* with supply and demand, you will not find an explicit presentation of supply and demand either. Maybe economists back then just didn't think it was important enough. Looking to the end of the 17th century, perhaps the best economic writing you will find is Dudley North's *Discourses Upon Trade* from 1691. This little gem is a wonderful defense of free trade and the market economy. Still it is noteworthy how little of what we now consider "economic knowledge" North, and other writers of his time, were able to articulate.[71]

[70] See Steuart (2022), *An Inquiry into the Principles of Political Economy* [1767].
[71] See Cantillon (2010) [1755], *An Essay on Economic Theory* [1755] and North (2022) [1691].

When it comes to the fundamentals of marginal utility theory – a building block of economics but not quite the same as *doing* economics – you don't find them in the Greeks or Romans. There are hints in the medieval theologians and finally the idea blossoms fully formed and correctly stated in both Galileo, as presented in chapter one, and in the Spanish Salamancan theologians of the 17th century. Was it really so hard to explain why diamonds are (at the margin!) more valuable than water, even though we must drink water to survive? I guess so. Funny me. When I read about the diamonds-water paradox resolution as a thirteen-year-old, I felt I picked it up in a second. Five seconds later I was bored.[72]

When it comes to particular areas of economics, some are well-developed much earlier than others. If you read a high-level British macro/monetary debate in say 1811, it might be pretty good. It won't have all the bells and whistles of modern macroeconomics, but the arguments are coherent and recognizable, and many of them are correct. You can read about the quantity theory of money, or how some business cycle might be caused by real shocks, or bubbles, or how lower demand has thrown people out of work.

If you are looking for the Coase theorem in economics, you won't find it before Ronald Coase wrote it up in 1960. (For our purposes here, treat the Coase theorem as saying: "if transaction costs are zero, the initial assignment of property rights will not matter for final allocative efficiency.") Coase initially presented the idea to a Chicago economics seminar with Milton Friedman and George Stigler present, and at first they thought he was crazy. They were convinced he was wrong. It was only the next day that they came back, stunned, and after some pondering admitted that Coase was right. You can argue that the Coase Theorem is implicit in Francis Ysidro Edgeworth at the turn of the 20th century, but I think it has to be seen as *very* implicit, otherwise Friedman and Stigler would

[72] On the Salamancans and marginal utility theory, see Grice-Hutchinson (1952).

not have looked at Coase like he was crazy. These days, we teach the Coase Theorem to undergraduates, and I don't mean for their senior-level honors thesis. [73]

Overall, progress in economics was very slow for a long time. I don't think it was slow from about 1890 onwards, as the Marginal Revolution was digested, mathematical tools were applied, and empirical methods were generated, often at extreme speeds. University professorships were increasingly "professionalized" in the world's advanced countries, creating a large body of professional economists to write for each other and then debate. Furthermore, the turmoil of the first half of the 20th century gave economists plenty of problems to tackle, and without those problems you would not have had Keynes, Hayek, or Friedman, among many others. Later on, computers and the internet kicked in to accelerate the possibilities for empirical work and also interchange between researchers, such as co-authorships at great distances.

So I don't think progress in economics has been slow in general. It is right now coming off an incredible 130-year or so run. Progress in economics, however, was glacial from the time of the ancient Greeks to the late 19th century, with a noticeable burst in the 18th century as well, centered around Adam Smith.

Any assessment of "slow," of course, relies on a notion of "slow relative to what." For purposes of contrast, let's consider some other areas for the exercise of human ingenuity.

Philosophy: Plato and Aristotle, who wrote in ancient Athens, centuries before the birth of Christ, are still considered amongst the greatest philosophers. Professional philosophers read them for insight and not just for their historical value. You might think that damns philosophy somewhat, but even if so, Plato and Aristotle are far more advanced than any surviving economic commentary we have from their time. They made a lot of progress on highly complex problems, and Aristotle

[73] On the Coase anecdote, see Davies (2014, pp. 79–80).

arguably was the leading economic thinker of his time, even though most of his discussion had a moral orientation.

Geometry: Euclid's *Elements* is an amazing and still well-regarded book, and it was done circa 300 B.C.

Mathematics more generally: Newton and Leibniz invented the calculus in the mid-17th century, when most economics was a non-rigorous, slapdash form of mercantilism, written in polemic pamphlets with no formalization.

Physics: Again, take Isaac Newton. He was doing amazing work in physics in the 17th century, to this day some of the most important work ever done, when economics still was primitive and basically had no models at all.

Astronomy: Progress in astronomy is a mixed bag, as for instance the insights of Copernicus (or for that matter the earlier ancient world) were slow to disseminate. In other regards progress was halted by the limitations of earlier telescope technologies. Nonetheless the peaks of what had been done were remarkable. Hipparchus, in the 2nd century before Christ, accurately measured the size of the earth and the distance from the earth to the moon. He calculated the length of a solar year and realized that the axis of the Earth was not fixed with respect to the stars.[74]

If you consider Ptolemy, Kepler, Brahe, and Galileo, there were no contemporaneous economists of comparable import or quality. In fact, early economics relied in part on these same astronomers. Copernicus was one of the first people to state the quantity theory of money, and as already presented Galileo provided an early resolution of the diamonds-water paradox. Some of the

[74] On the early astronomy contributions of Hipparchus and others, see Aughton (2004).

astronomers were better economists than the nearly non-existent economists of the same time. What does that tell you?[75]

Theatre and literature: It is plausible to regard Shakespeare as the greatest author of all time, to this day, and as one of the individuals with the most insight into the human condition. His work spanned the 16th and 17th centuries.

Music: Perhaps Johann Sebastian Bach was the greatest and most musically complex composer of all time? He lived from 1685 to 1750, never seeing the publication of Adam Smith's *Wealth of Nations*. Bach was hardly the only first-rate composer of his era, with Händel for instance dying in 1759.

Painting: The 17th century was an incredible time for painting, as it had Rubens, Velazquez, Poussin, Rembrandt, Vermeer, Breughel, and El Greco, among many others. The great Italian Renaissance painters and sculptors were earlier yet. Painting isn't easy, and these creators relied on a deep knowledge of paint, paint mixing, and how to work with materials, all scientific areas in their own right. Techniques of linear perspective – unknown in medieval times – were required as well.

Further below I will consider some sciences and insights that were slower to develop, and what they might have in common with economics. Still, the basic puzzle is clear: why did human knowledge advance so fast in some areas and so slowly in others? I will try to address that question for economics, hoping that it will tell us what economics actually is good for and what economics might do for us in the future.

I believe that economic ideas are fundamentally counterintuitive and hard to grasp. They do not require expensive equipment such as telescopes. Data gathering is important for some

[75] For more on the economic contributions of Copernicus, see Bieda (1973). For more on Galileo's, see Ekelund and Thornton (2011).

macroeconomics, but it should not be a prerequisite for clearly stating supply and demand, or for grasping and communicating many other parts of microeconomics.

But the "hard" of economics is a very special kind of hard. Painting like Vermeer is really hard too, and no one else since Vermeer has been able to do it. Yet plenty of us can do economics, albeit with varying degrees of proficiency.

I think of economic ideas as hanging in a kind of asymmetric corridor when it comes to their conception, a bit like factoring very large numbers. Once you know what the factors are, it is easy to verify that multiplying one by the other will give you the specified very large number. But if you are just staring at the very large number, as for instance you might be staring at an economic system, you don't have a very good sense of what the factors are, or in the case of the economy the underlying principles of operation won't exactly leap out at you.

Looking at an economy and trying to figure it out is – more than most economists realize – like staring at a very large number and trying to factor it. It is not only hard, but you don't know where to start. "Should I try dividing it by 323,477?" Well, maybe, but it won't be obvious that this is the correct way to proceed. Alternatively, you could say, using economic language, that the marginal cost of producing economic insight is very high, but it can be maintained, preserved, and transmitted at relatively low marginal cost. Lower than the marginal cost of learning how to paint like Johannes Vermeer.

There is no "brute force" method for obtaining fundamental economic insight. Rather, you need to peer around a corner and see something that the other people have not already seen. And once you see and grasp it, you cannot easily forget it, again reflecting the asymmetry of this path toward knowledge. So often I have heard economists make proclamations like: "Once you start thinking about the world in economic terms, you can no longer unsee those things."

That is exactly correct, but it is truly hard to see them in the first place. In essence, I think economics was so late to develop because it was so hard to peer around its corners. To see supply and demand in their proper workings. To see the relevance of price theory. To begin to grasp the spontaneous order of the marketplace, as Adam Smith and later Hayek did. To see how an aggregate demand theory of downturns might hang together. And of course to understand the importance of the Marginal Revolution. Maybe it was not so hard to resolve the diamonds-water paradox, as Galileo did fairly early on, but it was very very hard to see why that resolution was important.

Some mathematics fit this pattern as well, even though Euclid was early with his breakthroughs in geometry. For instance, the use of zero as a number was slow to develop, even though it seems quite intuitive to most people who learn it in grammar school, if not earlier. Indian mathematics incorporated zero into its numbering system centuries before the birth of Christ, but Western mathematics was slow to realize the importance of this practice. The concept of zero as a number spread later in the Arabic world but did not come to the West in full-blown form until early in the 13th century, through Leonardo of Pisa, better known as Fibonacci, who was in touch with Arabic mathematics. Eventually it was seen that the use of zero as a number made calculations and record-keeping much easier, and that later enabled Renaissance Italy to lead the spread of double-entry accounting, a fundamental factor behind the rise of capitalism. Part of what made the benefits of "zero as a number" difficult to grasp was that its potential for subsequent improvements in real world practice was so spread out over time.[76]

Or consider logarithms, which are an extremely powerful mathematical tool and calculating device, all the more useful for calculation before powerful computers. Logarithms are just sitting there, waiting to be found, and once you know them you won't forget them. Yet they are not discovered until early in the 17th century.[77]

[76] On zero as a number, see Seife (2000).
[77] See Pierce (1977).

We now can see some pieces of why Jevons had an eventual impact on British economics but his marginalist precursors did not. By the time of Jevons, outsiders can start to see (with some lag) that the Marginal Revolution will boost the "average revolution" as I have called it, or the extensive use of statistics in economics. They can see that the Marginal Revolution will give rise to an amazing burst of development of neoclassical models, led by individuals in a growing number of full-time university professorships. And they can see that economic policy is starting to become a science, debated within governments by a growing number of well-educated, mathematically literate advisors and bureaucrats. None of that was apparent – at all – when Galileo or the Salamancans first proffered solutions to the diamonds-water paradox. Nor was it yet sufficiently apparent to the precursors of Jevons, such as Richard Jennings or Henry MacLeod or others. Jevons, quite simply, was the economist who saw where the whole broad picture was headed, and that is arguably a greater contribution than just stating the principle of marginal utility per se.

The earlier revolution in British economic thought, the very founding of economic reasoning in the 17th century, that too had some preconditions. There was a cluster of publicly engaged individuals in London, debating economic issues very actively. Sustained economic growth in Britain seems to date from the 1620s, so suddenly there was much more to talk about. Due to the onset of joint stock companies and also colonies, there were more trade issues to debate. Due to ongoing political turmoil in 17th century Britain, a lot of issues suddenly were up for grabs. Pamphlet culture and coffeehouse consumption meant that publication was relatively cheap, readers could be reached, and an intellectual culture of back-and-forth argumentation developed. Some merchants, such as Dudley North and Nicholas Barbon, had enough wealth that they were not merely beholden to a court, church, or king, in their economic writings.

Those forces were not enough to generate the Marginal Revolution or even advanced economic reasoning. Nonetheless they allowed economic discourse to move to a new level, one that recognizably looked and felt like economic reasoning and eventually gave birth to Adam Smith and his book *The Wealth of Nations*. But even in this early case we can see how many disparate elements

had to come together to create an economic community that could iterate its way toward some basic economic truths.

I'd now like to consider some other branches of science and creativity that were slow to develop, with the purpose of seeing what they might have in common with economics. I will start with botanical classification, a branch of science that also seems like it should have been around earlier than it actually was.

Botanical Classification as a Laggard Science

The history of botany is a parallel example to that of economics. Some key insights of botany seem fairly intuitive, at least once you understand them, yet they took a long time to develop. Let's consider that story.

The Swede Carl Linnaeus (1707–1778) was a breakthrough figure in the history of botany, as he developed a new classification scheme for plants early in the 18th century. Once Linnaeus did this naming and taxonomy, the field could take off. Subsequent botanists had a framework for organizing their knowledge, and for trading information within a common framework. The work of Linnaeus, and its subsequent propagation, led to an explosion in exploration, classification, and eventually a greater understanding of botany.

You might think "botany is so simple – all you have to do is to look at a bunch of plants and give them names in some coherent system. They should have mastered this in the Dark Ages!" Surely plants are around us all, and observing them does not require complex equipment such as telescopes. But no, taxonomic botany was difficult to figure out. Linnaeus's contribution, which he created most centrally in 1748–1752, was to develop a classification system based on the sexual characteristics of plants (the number of stamens and pistils, with the male organ coming first in the count), and then a more detailed naming scheme. There were 24 broad classes of plants, smaller

groups ("orders") within each class, orders divided into genera, and then the genus divided into species. The important contribution was not a new theory of how plants operate or grow, rather a new series of organizing categories.

More broadly, behind these more detailed classifications there were animals, vegetables, and minerals, three basic categories. Within a kingdom were the qualifiers, later codified into phylum, class, order, genus, and species. Every creature had a Latin name, descriptive in some way, beginning with the genus and followed by the species, for instance Homo Sapiens. It was two words and two words only, consistently.

In essence, Linnaeus created a searchable system for plant names based on what we now would call keywords, with unique names for each and every plant standing in the background. The number of male parts (stamens) determined the class, while the number of female parts (pistils) determined the order, a method which succeeded precisely because it was arbitrary yet uniform. This was simple in its own way, but it had not been done before, nor is it an approach that a random smart person would stumble upon casually. It took a lot of hard thinking, and seeing around corners, finally producing a kind of filing cabinet for information about plants. It also took away the need for the previous systems of name translations and synonyms across different researchers.[78]

Earlier, there had been so many different classificatory schemes, sometimes based on plant shapes, habitats, or perhaps by the medical or agricultural uses of the plants. But those systems were not typically consistent across different nations or environments, for instance as a particular plant would be used differently in varying locales. These earlier classificatory schemes often lacked universality, and they had too many conflicting organizational principles. The Linnaean system, in contrast, focused on sex, stamen, and pistils.

[78] See Koerner (1999, p. 55, among other sources).

John Ray (1627–1705) was one example of where matters stood before Linnaeus came along. Ray was a respected British scientist, an expert anatomist, teaching at Trinity College, Cambridge, a member of the Royal Society, and was considered the leading British botanist of his time. Ray relied on multiple criteria to build a system of classification, and in his 1682 *Methodus* he argued that the classification of plants should consider monocotyledons (which produce a single-seed leaf), dicotyledons (which produce two seed-leaves), and furthermore classification should consider the seeds of the plant, the fruit, the flower, and the stems. On one hand, it is easy to see why a scientist might think all of those features of the problem should matter. Yet the resulting classification scheme was too cumbersome and too difficult to replicate or teach. It was never clear which was the exact rule to apply. Ray did make the important contribution of stressing that a species should be defined in terms of the ability to sexually reproduce, and his empirical work covered many thousands of plants, but he never converted his efforts into a workable and easily replicable taxonomic scheme.[79]

Linnaeus's innovations did not come from nowhere, and in fact they built upon the cataloguing work of John Ray and others, including Linnaeus himself. Scientists needed a large database of plants to think about and then test methods for schematic organization and classification. Furthermore, the ongoing voyages of exploration placed plant and animal classification squarely on the agenda and furnished ongoing sources of new data for any theory. Yet a major theoretical advance was needed as well. It was not until the late 17th century that European naturalists generally recognized that plants engaged in sexual reproduction at all. Without that insight the Linnaean system would not have been possible.[80]

Like most advances in economics, Linnaeus's methods for classification of plants were sufficiently novel that they did not catch on right away, even though he had an army of disciples to promote

[79] On Ray, see Knight (1981, chapter two) and also Shermer (2002, pp. 97–100) and Gribbin (2002, pp. 203–213). For earlier precursors of the Linnaean binomial feature of his taxonomy, see Heller (1983, pp. 41–42).
[80] On sexual reproduction, see Schiebinger (2003, p. 201), with her entire essay being useful on the preconditions of the Linnaean invention.

them. Even 50 years after their presentation, the principles of the Linnaean system still remained controversial. It was difficult for many natural scientists to reorient their conceptual schemes along the new proposed lines, and its full advantages, including those of easy communication and interoperability of results, were not entirely obvious until the system was adopted on a larger scale.[81]

Some commentators objected simply because the scheme was so *sexual*. The *Encyclopaedia Britannica* of the time argued, "A man would not naturally expect to meet with disgusting strokes of obscenity in a system of botany … obscenity is the very basis of the Linnaean system." The Reverend Richard Polwhele, a writer on science at the time, agreed that botany had become far too sexualized. Linnaeus himself did little to counter those fears. His biographer Gunnar Broberg noted that, "Time and time again, Linnaeus wrote about the sensual pleasure or rapture he experienced in studying nature," and refers to his "lively interest in sex."[82]

Economics also faced obstacles in its development because not everybody liked its implications. It was a method of thinking about social reality quite distinct from Christianity and church. It did not always approve of what a King or Queen might be doing. In the case of 18th century Dutch writer Bernard Mandeville, economic modes of reasoning seemed to suggest that the pursuit of vice is OK, or could even be beneficial, by motivating individuals to act in the broader public good. Along those lines, a lot of economics seemed to emphasize looking at the final results of action, rather than merely individual motivations, as some variants of Christianity suggested. It is no accident that many of the top economists, including Adam Smith, David Hume, and Jevons were not especially religious and even may have been downright non-believing and impious. By asking people to buy into economic modes of reasoning, you were asking them to consider a whole new way of looking at the world. It should be no surprise that was not accepted either quickly or automatically.

[81] See Fara (2017, including pp. 39–40).
[82] See Fara (2017, pp. 43–44), and for the quotation Broberg (2023, p. 253, p. 247).

But just as economic modes of reasoning eventually triumphed, Linnaeus and his methods also won out. Not only did classification system made sense, but Linnaeus also put in the work. Linnaeus collected numerous specimens, and in his first publication he gave formal names to 5,900 species of plants. He encouraged his students and co-workers to do the same, and he adopted the clever convention of naming plants after friendly researchers who had done important work in the area. That gave them an ongoing incentive to embrace the Linnaean scheme of classification. Just as the Marginal Revolution gave economists a lot more to do, so did the Linneaus approach to botany.[83]

Linnaeus and his system also benefited from the professionalization of science, a gradual trend at the time but an accelerating one. He held a chair in medicine at Uppsala, attracted many students, and his classification scheme created a more-or-less automatic research program for them. They could then go out and explore the world and report back, having performed the very manageable task of classifying additional plants and animals, including from voyages of exploration. In turn, some of those students received their own academic posts, or were in positions to solicit further patronage, or they were invited on voyages and travels, or tasked with developing a botanical garden. It was easier for a universally consistent system to benefit from this professionalization, whereas the less consistent schemes were able to proliferate and survive in a world of gentleman amateur scientists.[84]

Professionalization benefited economic modes of thinking as well. Adam Smith worked as a full-time professor, as did Jevons, both in times when that was somewhat unusual for economic writers. Linnaeus was appointed to one of the chairs of medicine at Uppsala University in 1741.

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[83] See Shermer (2002, pp. 100–102).

[84] See Knight (1981, p. 73) on Linnaeus, his students, and professionalization. Once in place, much more progress was possible through the Linnaean system. For instance, Joseph Banks (1743–1820) was a major figure in the history of botany and expanding botanical investigation around the entire world. As a boy in England he collected plants and animals, but he did not develop a systematic theoretical framework until he attended lectures by Israel Lyons, the Cambridge botanist and astronomer, and a major British follower of Linnaeus. With the framework of Linnaeus in hand, he (among his other contributions) was a leading proponent of the systematization of botany and the internationalization of scientific knowledge. For background on Banks, see Goodman (2021).

Those university chairs helped them conceive of their intellectual enterprises as building edifices that later could be systematized by other individuals, working, studying, and writing on a full-time basis.[85]

One striking feature of Linneaus's thought is how, in spite of his major advances in botany, he still was a poor economist. Writing in the middle of the 18th century, before Adam Smith, Linnaeus argued for national economic self-sufficiency, and viewed international trade as a zero-sum game. The goal was to keep the trade balance positive, and by the standards of the time Linnaeus would be considered a "Cameralist," a kind of Continental version of mercantilism. In the time of Linnaeus, the development and professionalization of economics simply had not proceeded far enough. Linnaeus was Swedish, and the Swedish mid-century debate over economics, while not as advanced as Smith, did offer entirely coherent and largely correct arguments for the freedom of trade. Linnaeus just couldn't see around that corner, just as so many scientists of the time did not really grasp the import of his contributions.[86]

The New Ideas Behind Geology

Geology, like economics, was relatively slow to develop. Some of the most important ideas became apparent only in the 18th century, even though they were available for clear viewing throughout the entire history of mankind. Furthermore, as late as the 1830s those ideas were contested and revolutionary rather than part of the mainstream, and it took much of the 19th century for them to be digested fully, much like the timing for economics. Similarly, the big advances in geology were not primarily dependent on new scientific equipment, such as good telescopes or microscopes, but they were fundamentally conceptual in nature.

One key shift in thinking was the move from seeing the Earth as young in age and relatively static, to thinking of geologic time as an important matter in its own right, rife with information about the

[85] On the chair appointment process for Linnaeus, see Broberg (2023, pp. 140–142).
[86] See Koerner (1999, pp. 2–3) and also Broberg (2023, pp. 264–270) on the views of Linnaeus on economics and policy. On the better side of 18th-century Swedish economic thought, see for instance Eagly (1969).

development of life as well. The time scale had to be enlarged *greatly*, and to be seen as involving change, lots of information, and full of lessons for the history of mankind on earth.

From today's vantage point that all sounds trivial, and I recall that I, like many other young children, had some of my earliest scientific fascinations with fossils and most of all dinosaurs. I loved going to the Natural History Museum, and reading dinosaur books, and I recall it all seeming so *intuitive* to me. I think I was seven or eight years old, and I grasped the (very basic) fundamentals of geology without much hesitation. My mother even bought me some cheap fossils and I kept them in my room. I didn't ask, "Mom, what the heck is this stuff!? Isn't the earth static and only 6,000 years old?" No, I had a general sense of what I held in my hands, and its ancient nature, and I read and largely understood the science books for children that explained it.

This heightens the puzzle. Like the basic laws of economics, many of the most basic insights from "geology as a whole" just do not seem that difficult. It is strange that it took many centuries for the very smartest humans to figure out those truths.

One critical figure for the birth of modern geological thinking is James Hutton (1726–1797), who like David Hume and Adam Smith was a product of the Scottish Enlightenment. Hutton is sometimes called the father of modern geology, and for good reason. Yet it is striking how ordinary and commonsensical his main contributions seem. He studied his native Scottish landscapes and came to the conclusion that geological features underwent ongoing change for very long periods of time. OK, fair enough, but as with supply and demand my reaction is, "You mean you people didn't already know that!?" Nope, not really. Hutton also argued, contra many Christians of his time, that the earth was quite old and that you could look at present-day rocks and geologic formations and make inferences about its early history.[87]

[87] On the history of Hutton and his ideas, see Baxter (2003).

Hutton also put forward the view of "uniformitarianism," namely that geologic features are the result of slow, broadly uniform changes over long periods of time, rather than sudden disruptions. That is less obviously true, and although it was seconded by Charles Lyall, another founding father of modern geology, in the 1830s many geologists still believed in the contrasting doctrine of catastrophism. Uniformitarianism was not entirely correct, but again it was a relative advance and as a basic idea it is remarkable how late it gained currency and appreciation.[88]

Earlier, Georges Cuvier (1769–1832) founded significant parts of paleontology and noticed that different strata of geologic rock contained different animal fossils, and that those fossils could be used to date the age of the rock; William Smith (1769–1839) of England, another major early figure in geological investigation, was doing the same. Lest you think all of those conceptual advances were easy, the great physicist Lord Kelvin was questioning assumptions about the age of the earth as late as 1860.[89]

In the case of geology, there is at least one obvious reason why these views took so long to assemble and digest, namely Christianity. Both the Bible and the influential churches of the time promulgated the doctrine that the earth was quite young and stemmed from a recent act of God's creation. Christian children learned from a young age that the earth was of recent origin, and that made it harder for them to acquire the background concepts to see the world in other terms.

Scientists with opposing ideas may have feared persecution and kept their mouths shut, or maybe they were discouraged from entering the study of geology in the first place. One striking feature of Hutton's life is simply that he made an independent living as a physician, a farmer, and running a

[88] On uniformitarianism, see for instance Browne (1995, pp. 188–189). For one general survey of parts of these debates, see the very useful Knell and Lewis (2001).
[89] On Werner, see Maddox (2017, pp. 21–22). Another candidate for father of geology is Abraham Gottlob Werner (1749–1817). He was professor of mineralogy in Germany at the Freiburg School of Mines, the first mining school in Europe. While he promoted many false ideas under the guise of his doctrine of "Neptunism," he understood the notion that layers of rock represent a chronological success of events, an idea that would soon prove to be important in interpreting fossil data from rocks. He also understood that the earth was very old. On Kelvin, see Baxter (2003, p. 210).

profitable chemical company, at varying points in his life. He was not at the mercy of the religious establishment for his living or his well-being, and that may have increased his freedom of speech. In this regard he resembles some of the earlier merchants, such as Dudley North and Nicholas Barbon, who wrote economics pamphlets in earlier 17th century London. Such forms of financial independence were much harder to pull off in earlier periods of European history.

It is not surprising that the spread of better geologic ideas coincided with the rise of geology as a distinct profession with independent means of support. Only in late Stuart Britain was geology thought of as its own endeavor, as before that time geological investigations were performed by people called "scholars," "enquirers," and other terms, rather than geologists. Cosmology, natural history, and antiquarianism were all more popular concepts than was geology, and there was no general path for deriving income for studying earth science. As a result, there was no intellectual coherence to the investigations of the topic. Matters improved in the 18th century, but still most of the investigators were either clergy or "gentlemen of landed property," rather than scientists *per se*. The consumer revolution of the British 18th century did create opportunities for people to write books, give tours, and market an interest in science to the wealthy. Still, actual professionalization did not arise until the Geological Survey started employing scientists in geological work in 1835. And indeed, after that time, geological knowledge, and freedom of inquiry, grew rapidly. Shortly thereafter, a systematic series of teaching opportunities arose, including at the School of Mines, and also ad-hoc lecturing opportunities at scientific societies. Geological societies sprang up throughout the first half of the 19th century. By the 1870s, a fair number of British geologists had technical competence and could carry on reasonably reliable research or write a plausible textbook. By 1900, sixteen British colleges and universities were teaching geology as a distinct subject of study.[90]

Again, we can see a connection with the career of Jevons and the more general development of British economic thought. University professorships in economics became common in Britain and also on many parts of the Continent in the late 19th century. Marshall, Edgeworth, Menger,

[90] On all these developments, see Porter (1978) and also O'Connor and Meadows (1976).

Wicksell, Pareto, and others later had university chairs, and since 1866 Jevons himself held a chair at Owens College, Manchester, which he owed to his contribution *The Coal Question*, as mentioned earlier. That gave him a comfortable salary of about 300 British pounds a year.[91]

After that point, once some basic groundwork had been laid, including the insights of marginalism, scientific progress in economics exploded. There was a critical mass of smart people who had relative autonomy to explore new ideas and also to give each other critical feedback.

Charles Lyell, the most important 19th century geologist, said that the three practical rules for doing geology were "travel, travel and travel." That required money, some degree of personal health, the time to travel, and also the infrastructure to get around. All of those had been harder to come by in the Britain of the 17th century than in Lyell's own time, namely 1797–1875. Lyell did follow his own stricture, visiting Paris and the geologically relevant Paris basin in the 1820s, and then central France and the massif central. Next he went to Italy and Rome, finding Rome itself a source of geological insight, and then Sicily and Vesuvius, with geological questions in mind throughout. In Paris Lyell met with Georges Cuvier, arguably the father of paleontology and who drove the major changes in geology in the 1820s. Among his other achievements, Cuvier studied fossils, he understood that they represented species that had disappeared, and he fully grasped that this represented a big puzzle. He postulated a great catastrophe in times past, and more generally his inquiries both paved the way for others and helped people put aside the mindset of a static earth and a static set of species.[92]

When it came to William Smith, the aforementioned English geologist and geological map-maker, Lyell noted that Smith came from a humble family background and largely had to work alone, covering Britain on foot without the aid of fellow workers.[93]

In these regards the history of geology was very much like the history of economics. For people to come closer to the truth, they needed some means of independent support distinct from the church

[91] Keynes (1936, p. 527).
[92] See Maddox (2017, pp. 33–46, and p. 40 for the quotation about travel.
[93] See Maddox (2017, p. 36).

or their own wealth. I've already discussed university chairs, but also important were better developed markets and sometimes also higher stocks of family wealth. Carl Menger and Leon Walras got their starts as journalists, which required middle- and upper-class readers as an economic prerequisite. Edgeworth was appointed at Oxford, and Pareto was at the University of Lausanne in Switzerland, both institutions backed by endowed wealth. One key factor behind the take-off in economic reasoning, analogous to the history of many other arts, letters, and sciences, was simply that growing wealth created many more opportunities for independent thought, creation, and publication.

The early Scottish geologists and early Scottish economists and social scientists seem to have experienced the same basic background liberation from previous dogmas, and that helped generate new hypotheses in many different areas, including also engineering and inventions. Consider John Playfair (1748–1819), another founding father of geology. He was trained as a minister and in his teen years he worked as a philosophy instructor and later in mathematics. He became friends with Adam Smith and Joseph Black (an important figure in Linnaean botany), and he tutored Adam Ferguson, a leading social science light in the Scottish Enlightenment. His younger brother, William Playfair, was in part raised by John, and William in turn wrote on political economy. John was close friends with James Hutton, and wrote a whole book defending his geology, making John himself an early founding father of geology. The point is this: Edinburgh was not a huge place, and once some people started questioning orthodoxy, and independent means of subsistence were possible, the spirit of inquiry spread rapidly and to many fields. Dogmas fell in geology, philosophy, botany, and other areas, just as they did for economics. To give you a sense of Playfair's open-mindedness, he also studied Indian ["Hindu"] mathematics and astronomy in his quest for better ideas.[94]

As usual, you can find anticipatory figures who came before Hutton, Lyell, and their important geologic peers. Yet the scientific world as a whole could not digest the relevance of those observations. People just didn't have the conceptual frameworks to put the whole picture together,

[94] On John Playfair, see *Dictionary of National Biography* (2004) and also Playfair's Wikipedia page.

nor the sustained scientific networks for the better observations to displace the errors and serve as building blocks for further progress. Once again, we see some common factors that retarded the developments of both economics and geology.[95]

Evolution and the Theory of Natural Selection

Theories of evolution and natural selection also are intuitive once you understand them, and they seem virtually inescapable once you are willing to consider them seriously. Yet they are remarkably late in becoming part of general human knowledge, and indeed to this day, according to polls a significant percentage of Americans still do not accept those doctrines.

As a fully fleshed out doctrine, our understanding of natural selection can be dated to the middle of the 19th century, when both Charles Darwin and Alfred Russel Wallace published versions of the idea.

It is striking how many different inspirations Darwin needed. Darwin for instance was well aware of how his grandfather Erasmus Darwin had stated an early version of evolutionary principles in his works, including *Zoonomia*. Lamarck also had emphasized the idea of species changing and evolving over time, even if he did not embrace natural selection as the mechanism. So Darwin was alert to the basic idea of evolution, but still not for a long time able to put the pieces together. When scientist Robert Grant pushed a version of evolutionary ideas on Darwin in the 1820s, Darwin by his own account simply did not get it. That is similar to how Alfred Marshall's early review of Jevons on marginalism also did not see the importance of the idea.[96]

Darwin himself was inspired by geology, and he used geological insights to arrive at his theory of natural selection. On his famous voyage on the *Beagle*, he took a copy of Lyell's *Principles*, and he was

[95] On the earliest geologists, see Knell and Lewis (2001).
[96] On the role of Grant and also Charles Darwin's awareness of both Lamarck and his grandfather, see Browne (1995, pp. 82–84).

continually on the lookout for phenomena of geological interest. His *Beagle* notebooks are full of geological observations. And *The Origin of Species* was dedicated to Lyell. It was Lyell's work that allowed Darwin to see that the history of the earth held important truths about the history of animals and indeed mankind itself. As Darwin's biographer Janet Browne wrote: "Without Lyell there would have been no Darwin: no intellectual journey, no voyage of the Beagle as commonly understood." So just as the development of economics into a full-blown marginal revolution had required the general spread of interest in both mathematics and statistics, evolutionary biology had to wait for a spread of greater interest in geological questions, including about fossils and the age of the earth. [97]

It is not just Darwin but also Alfred Russel Wallace who took much of his inspiration from the new geological learning. Wallace's 1855 essay "On the Law Which Has Regulated the Introduction of New Species" (commonly known as "Sarawak Law") was his primary document for laying out his initial thoughts on evolution. The entire first two-thirds of the document focuses on the new geological learning and his very first article heading is "Geographical Distribution dependent on Geologic Changes." He writes of how the earth has been changing for long periods of time, and that the organic life of the earth has been through corresponding gradual changes which must be explained. The fossil record shows the ongoing extinction and creation of species, and that too must change our thinking about how the organic world before us has come to pass. And the major influence on Wallace here was, as you might expect, Charles Lyell.[98]

It is striking how much Wallace, to arrive at a theory of evolution, put together the same intellectual building blocks as did Darwin. Just as Darwin had his voyage of the *Beagle*, Wallace had trips to the Amazon and to Indonesia, both posing him questions about nature and the latter trip inducing Wallace to ponder the connection between orangutans and human beings. And in addition to Lyell, Wallace took from Malthus the key idea that varying numbers would cause different kinds of beings

[97] On Darwin's interest in geology, see Maddox (2017, chapter 12). For the quotation, see Browne (1995, p. 186).
[98] On the influence of Lyell on Wallace, see Costa (2013).

to become more or less prominent. The final impetus for a theory of evolution came to Wallace during a bout of malaria.[99]

In the 1850s it became known, most of all to British scientist Gideon Mantell, that an age of reptiles had preceded the age of mammals, and that dinosaurs had walked on land and with two feet. That was hardly equivalent to Darwin's theory of natural selection, but still it cried out for explanation of some kind, and for Darwin it helped all of the pieces to fall into place. Mantell's own work relied on the geological insight that fossils were distributed into various strata and layers, corresponding ultimately to different periods of geologic time. This all built on earlier discoveries of dinosaur fossils in the 1820s, by Mary Anning and others.[100]

One of the early statements of evolutionary theory came from James Hutton, mentioned earlier as one of the founding fathers of modern geology in the 18th century. For instance Hutton wrote: "In concerning an infinite variety among the individuals of that species, we must be assured that, on the one hand, those which depart most from the best adapted constitution will be most liable perish while on the other hand, those organized bodies which most approach to the best constitution for the present circumstances will be best adapted to continue in preserving themselves and multiplying the individuals of their race."[101]

That is hardly the entire Darwinian theory, but clearly Hutton was liberated from the mistakes of his predecessors, and he was able to grasp and articulate some of the key ideas of evolution. It is probably no accident that Hutton challenged Christianity both on matters geological and biological. He was a smart observer, but more importantly he was able to throw off the conceptual shackles that had limited earlier theorizing.

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[99] On all this, see Browne (2002, pp. 27–32).
[100] See Maddox (2017, chapters seven and eight).
[101] See Baxter (2003, pp. 110).

Darwin was good friends with Charles Lyell and dedicated his *Origin of Species* to him (and others). He had written: "The great merit of [Lyell's] *Principles* was that it altered the tone of one's mind." He also wrote in 1844 that he felt "as if my books came half out of Lyell's brains." Lyell himself was skeptical about the idea of the mutation of animals, partly for theological reasons, so what influenced Darwin was his geology, not any biological insight per se.[102]

The discovery of human fossils, which provided evidence for the antiquity of humans, was another significant piece of evidence that went into putting together the geological puzzle and the history of speciation. In the 1820s, several naturalists in the south of France found human bones mixed in with the remains of fossil animals, and these were "exotic" animals that no longer walked the earth. That too was a puzzle, and it was hard to avoid the implication that perhaps the time of humans on earth was much longer than what the Bible had postulated. More generally, the previous distinction between a static human history, but changing varieties of animals in the past, no longer seemed so appealing. Maybe both humans and animals were subject to the same principles of change and variation over time. Maybe the evidence here was misleading (were the different human and animal bones somehow mixed together by mistake?), but still the more sophisticated observers were starting to wonder what was up. All of these questions became more pressing in the 1830s when fossils were found of primates that did not match either humans or known apes but rather seemed to occupy an intermediate space between the two.[103]

According to Darwin himself, what finally enabled his breakthrough was reading Malthus. Malthus, in his 1798 *Essay on the Principle of Population*, focused on the notion that the numbers of humans (or animals) would increase or decrease, depending on how well those people did in producing enough food to eat.[104] In retrospect it seems odd for the Malthusian model to have been the missing piece of the puzzle, but it helped Darwin to see that there was a competition to survive, and Darwin then put

[102] For the first quotation, see Baxter (2003, p. 209), for the 1844 quotation, see Costa (2013, p. 236).
[103] See Rudwick (2008, pp. 228–236, 408–409, and 417–421 on the primate fossils).
[104] Malthus (2000) [1826]. First edition was published anonymously in 1798, with Malthus elaborating as himself in 1826.

this insight together with all the other pieces of the natural selection model he had been accumulating in his mind. It was differential death rates that accounted for the balances of nature. After Darwin finished reading Malthus's *Essay* on October 3, 1838, he saw all this rather suddenly. In this sense evolutionary biology was late for literally the same reasons that developing economic ideas of competition and selection were late too.[105]

The theory of natural selection, once it arrived, was hardly obvious to all observers. The very smart William Whewell, one of the leading scientists and philosophers of science in England, declared that the mutability of species was finite and had definite limits. Much of the opposition was politically or religiously motivated, but it also seemed that commentators simply could not conceive how monkeys might be transmuted into human beings. Alfred Russell Wallace himself, as time passed, rejected the notion that natural selection could explain the evolution of humans and he moved increasingly to spiritualist explanations of human intelligence. For all his brilliance and originality, Wallace never quite internalized the rules of the scientific method.[106]

Marginalism, like the Darwinian theory of natural selection, had its delays, its opponents, and simply those who didn't think it was very important. After Jevons published *The Theory of Political Economy* in 1871, British economists did not line up to endorse this new marginal revolution. In 1874, Jevons noted that he didn't think he had persuaded anyone, with the exception of some younger mathematicians and economists, most of all George Darwin (son of the naturalist Charles Darwin).[107]

Worse yet, many argued Jevons was wrong. John Elliott Cairnes, one of the best-known and also best classical economists, wrote a review. He claimed that Jevons misunderstood the classical theory of value and was flat out incorrect in his supposed innovations. There was no rush to defend Jevons

[105] See Browne (1995, pp. 386–389).
[106] See Rudwick (2008, p. 357). On Wallace's spiritualism, see Ruse (2008). On general opposition to Darwin, see Browne (2002, pp. 107–108).
[107] Black and Könekamp (1972, p. 48). On Jevons doubting his success, and mentioning George Darwin, see Keynes (1936, p. 535).

in the British community of economists, yet Jevons responded like a gentleman in his correspondence with Cairnes.[108]

Alfred Marshall, later one of the greatest neoclassical economists, also reviewed the Jevons book, but his response was indifferent. This was Marshall's very first appearance in print, but he didn't see a huge need to be either a careful reader or sympathetic. Marshall went so far as to write: "We may read far into the present book, without finding any important proposition which is new in substance." Keynes described the review as "tepid and grudging." Jevons simply wasn't succeeding at persuading the broader intellectual world. Even later on, Marshall was reluctant to give public credit to Jevons at all. There was, however, one fragment in Marshall's personal journal where he admitted that Jevons was in fact "among the very greatest of economists," but that Jevons's intellectual virtues "impressed me gradually." He nonetheless still regarded his own primary intellectual debts as being to Cournot and von Thünen, not Jevons.[109] Thomas Edward Cliffe Leslie, a purveyor of historical and institutional approaches to economics, also reviewed Jevons, in this case he wrote in 1879 about the second edition. Leslie praised Jevons and noted the import of his work, and requested readers to consider it carefully. Yet Leslie was by no means happy with the thrust of the work. He expressed his skepticism for the deductive and mathematical methods of Jevons, favoring a much more inductive and historical approach. Jevons, it seemed, couldn't quite make anybody sufficiently happy.[110]

Worse yet, an untitled review from the 1871 *Saturday Review* charged that Jevons had done little that was new, that his mathematization brought us nowhere, and that Jevons was denying the laws of supply and demand. That review is a good example of how most people at the time simply didn't understand the new contribution.[111]

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[108] On this episode, see Black (1960), and of course Cairnes (1981 [1872]. For a collection of an extensive series of reviews of Jevons's treatise, see *W. Stanley Jevons: Collected Reviews and Obituaries* (2002, vol. I,), pp. 187–294.
[109] On the Marshall review, see Marshall (1981 [1872], and Keynes (1936, pp.534–537, the quotation from p. 535, and Marshall's journal entry covered on pp. 536–537).
[110] See Leslie (1981) [1879].
[111] See Untitled and Anonymous Review (1981) [1871].

If you read through the reviews of Jevons from the time, many are positive but very few understood that he had achieved something revolutionary. Highest credit here goes to *The New York Times*, which on 13 May 1872 issued a one paragraph notice of the book, which ran as follows: "This work, though somewhat abstruse in character, will be found both novel and suggestive to all those interested in the study of political economy. The author's final theory appears to be that value depends entirely upon utility, and that economy as a science may be made wholly mathematical and be determined with the utmost exactness by the use of the differential calculus. The work is one of unusual ability and may prove, should the theories advanced in it be substantiated, of inestimable value."[112]

Although that response was the exception, Jevons kept at work, and in his personal journal he noted: "The one thing requisite to me is invincible determination & perseverance – When I think what discouragement I have gone through I feel sure that the greatest of disappointments cannot permanently shake me."[113]

Still, the Jevons book was at least noticed, and its reception was not entirely as negative as Jevons himself seemed to believe. The book was covered in the *Manchester Guardian*, in the *Athenaeum*, the *British Quarterly Review*, and the *Westminster Review*. Simon Newcomb, a prominent American astronomer and also economist, reviewed the book favorably in the *North American Review*. And although the reviews by Cairnes and Marshall largely were negative, simply getting a reviewer of Cairnes's stature at the time was a real achievement, Marshall then still a neophyte. Nonetheless, the revolutionary tenor of Jevons's contributions largely was ignored, both in the reviews and when it came to how economics was being done in the Anglo world.[114]

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[112] For those reviews, see *W. Stanley Jevons: Collected Reviews and Obituaries* (2002, vol. I,), pp. 187-294. The *NYT* review is from p. 261.
[113] Jevons (1972, vol. I, p. 205) [1866].
[114] On these reviews, see Schabas (1990, p. 98).

A look at America shows just how slow the Marginal Revolution was spread. Richard T. Ely's leading American textbook of 1889, *Introduction to Political Economy*, did not present marginalism. The 1893 sequel, *Outlines of Economics*, had some marginalism but hardly as the dominant strand of analysis. In most of the world, the true spread and entrenchment of marginalism was a 20th century phenomenon, accompanied by an ongoing rise of mathematics and statistics.[115]

Conclusions

By studying the slow intellectual development of economics, and contrasting it with other fields of study, we can learn the following:

1. Some insights are very hard to grasp, even if they are apparently simple once they are understood. People need to "see around corners" in the right way to understand these insights and incorporate them into their world views.

2. Economics is one of those fields, and that is why it took intuitive economic reasoning so long to evolve, marginalism included. Those of us who are educators, or who spend time talking to policymakers, should take this point very seriously.

3. Even very, very smart people are likely unaware that these "see around the corner" insights are missing – did Euclid rue that he did not have access to proper supply and demand and tax incidence theory? Probably not.

4. Economics is not the only such field that is hard to grasp, some other examples being segments of botany, geology, and evolutionary biology.

5. Scientific revolutions come about when many complementary pieces are in place, such as financial support, intellectual independence, and networks of like-minded others to talk with. Those

[115] See Howey (1989, p. xxxi).

conditions help people to understand that "seeing around those corners" can bring both high social and professional returns.

Are there major conceptual corners that today still no one can see around? If so, how might we discover what they are? And why are we not working harder on this? Or are we?

CHAPTER FOUR

Why Marginalism Will Dwindle, and What Will Replace It?

The underappreciated news is that marginalism is on the way out. Furthermore, this is old news, though the trend is accelerating.

Most of all it is underdiscussed news. As economics continues to evolve, marginalist insights – probably of all different kinds – will lie ever further from the frontiers of research and knowledge.

I find it easy to imagine that – less than 20 years from now – marginalism will be viewed as a historical curiosity rather than a central analytical engine of economics. No one will quite come out and say that, nor will they present marginalism as false or destructive. Rather it will be seen as of limited relevance, much as we might view parts of the earlier classical economists, such as their expositions of the quantity theory of money. New and different analytical frameworks will replace the ones that have dominated neoclassical economics to date.

The training of Large Language Models (and other forms of AI) will enshrine marginalism into their basic operating concepts, as those models are trained on writings that understand marginalist concepts. Indeed, if you ask current top-tier LLMs questions about marginalism, they will do very well. But in those frameworks, marginalism will be a sliver of a much broader pie, baked into the inputs we fed our tools of artificial intelligence, but eventually invisible. It will not be at the center of the major analytical constructs and debates, or weigh very much on the minds of economists. Marginalism will be like a sacred book that inspired later religions, but is no longer read or debated.

These changes will differ across empirical and theoretical economics, just as empirics and theory in economics already have evolved along different tracks. In empirics, data quality and causal identification are far more important than several decades ago, as are robustness checks. The prevailing mentality used to be, "Do I have any kind of result at all? Can I then publish this?" That was the case even at very good journals and in very good departments. The current mentality is, "Have I done everything possible to make sure this is correct?"

The latter view is going to lead to better research results, though it is harder to live under as a regime. In the "good ol' days" an ideal journal submission was 17–25 pages, with a transparent model and centered around a single (supposedly) clean estimation. These days a submitted paper might be 90 pages or longer, with multiple appendices and discussions of data quality. The paper as a whole, starting with finding and cleaning the data, could take over five years from start to publication time. Robustness checks will multiply to the point where it is hard to tell which are the important ones. Even then, after the piece is submitted to a journal, the referees may well ask for yet further robustness checks. Occasionally a contemporary referee report will be longer than an earlier paper might have been.

One can go back and read major papers from the 1980s, or even the 1990s, and marvel that anyone ever might have believed such results (did they?). Today, you know that for better or worse all stops have been pulled out, at least for papers that are accepted at the top journals for their empirical work.

Another change is that no one demands that these research papers restrict themselves to economics narrowly construed. The National Institutes of Health is now a major funder of research by economists, mostly because economists are doing so much work in the field of public health. Some of this research is accompanied by the traditional "downward sloping demand curve," or by concepts such as externalities, but a lot of it is not. It's just public health work. In similar fashion, top economists are writing plenty of papers about education and educational outcomes. Some of

these are centered around economic questions such as the rate of return to educational expenditures, but again a lot of them are not. They are on questions and topics that you would otherwise expect to find coming out of a Department of Education.

The day before drafting this paragraph, I blogged a paper on confidence gaps between men and women. It was a paper written by economists, published in the prestigious *American Economic Review*, the profession's number one journal. Is this actually sociology, or personality or social psychology, or part of some gender studies field? No one in the economics profession cares to discuss that anymore. It is not that there is a dogmatic attachment to what used to be called "economic imperialism," rather the view is that if the paper is good enough … it is good enough to publish. I also recently read a paper on using cell phone data to estimate how many people actually were attending church. Freakonomics guru Steve Levitt wrote and published well-known papers on the choice of baby names and corruption in Sumo wrestling[116].

The dirty little secret is that what distinguishes economics as a field, right now, is a mix of higher standards, harder work, better math, and higher IQs. That is the real (dare I say marginal?) contribution of "empirical economics today," not marginalism per se, though of course contemporary models typically are consistent with marginalist reasoning.

The confrontational or "social discomfort" side of marginalism, discussed in chapter one, is now hurting marginalism somewhat. It is not the main reason why marginalism as a series of intuitions is dwindling. Yet (at the margin!), as the economics profession has moved to the left, a diminished role for at least some marginalist intuitions is perhaps not entirely unwelcome. Marginalism may thus have somewhat fewer defenders than might otherwise have been the case, if no political motives had intervened. I am not suggesting anyone is being dishonest here, rather, that their politics induce them to champion other methodological causes than marginalism, mostly because those other causes seem, for normative reasons, more important.

[116] See Exley and Nielsen (2024), and on cell phones see Pope (2024).

As for what has risen in the place of marginalist intuitions, most significant and influential economics today is empirical economics. It continues to grow and displace theory as a receipt of professional attention. For instance, by one measure the portion of theoretical papers published in the *American Economic Review*, *Journal of Political Economy*, and *The Quarterly Journal of Economics* – three of the top journals – fell by 31.6 percentage points from 1963 to 2011. This work typically builds on marginalist insights, but it does not throw the confrontational side of marginalism in your face.[117]

Given the evolution of the profession, if you return to the history of economic thought and ask which trend or school of thought has won out, it is not Austrian, marginalist, monetarist, Keynesian, Marxist, or anything along those lines. The dominant strand in economics today is empirical. And transplanted into history of economic thought language, you could start with William Stanley Jevons as a major driver of that trend. We thus can see another major victory for Jevons, who to this day, for all the plaudits he receives, remains an underrated economist.

And it is not just Jevons, as he stemmed from a broader 19th century British empirical tradition. John F.W. Herschel, William Whewell, and John Stuart Mill all deserve significant credit, with Mill's import in this context stemming from his writings on logic and method, not his economics per se. That strand of thought emphasized empirical investigation, but it also recognized that facts were theory laden. That view was expressed best in the writings of Whewell and his circle, centered around Cambridge. They were not what sometimes later was called "crude empiricists" or "crude positivists," but rather they thought very carefully about how theory and empirics should fit together.[118]

[117] See Hamermesh (2013) and also Cherrier (2016).
[118] On Whewell, see for instance Butts (1973). Mill's Essay on Logic, and its import for current economics, I have discussed in detail in GOAT (Cowen 2023). On the group and their milieu, see Blake, Ducasse, and Madden (1960), and there is further background in Yeo (1993). For Whewell himself, on science, see Whewell (1967) [1847]. Do note that like many scientists propounding empirical methods, Whewell is not actually all that fun to read.

Like today's economists, those writers, when they covered philosophy of science, were not overly concerned with the demarcation of different fields or disciplines. Instead, they wanted to uncover the structures of good scientific investigation, which is a very contemporary attitude. They also understood that their campaign against intuitionism in the sciences was part and parcel of a broader view that applied to the social sciences as well and also to ethics and public policy. In particular, social questions should rest on the evidence, and potential political reforms should not be ruled out a priori, but rather they were to be evaluated scientifically.[119]

William Whewell (1794–1866) in particular led a circle of thinkers, spanning different fields of science, mathematics, and also economics. Harvey Becher wrote about Whewell: "the only word that seems appropriate is ubiquitous. As a giant of erudition, a wide-ranging scientist, a historian and philosopher of science, and an educator, Whewell was omnipresent in the scientific milieu of nineteenth-century England, intellectually and politically. Pick up almost any book dealing with English science or scientific culture of his time, and his name will be in the index." The astronomer John Herschel described him thus: "a more wonderful variety and amount of knowledge in almost every department of human inquiry was perhaps never in the same interval of time accumulated by any man." Alfred Lord Tennyson called him "a lion-like man."[120]

Whewell was a legitimate polymath, making contributions to mechanics, physics, geology, astronomy, mathematics, economics, and also poetry. He was an Anglican priest, he translated works by Goethe and Grotius, and he organized a pathbreaking citizen science project to study ocean tides. It was he who persuaded Darwin to become secretary of the Geological Society of London, noting (see chapter three of this work) that geology was a critical input into Darwin's theory of evolution. On science and its philosophy, Whewell's major work was his 1837 multi-volume *History of the Inductive Sciences, from their Earliest to the Present Times*, now largely forgotten outside of academic history of

[119] On the importance of evidence for reform, see Snyder (2006, pp. 204–266). Note that on this inductive point, Malthus and the group had some mutual sympathies, see Snyder (2006, pp. 273–274).

[120] See Becher (1996, p. xi). On Herschel and Tennyson on Whewell, see Snyder (2006, p. 1). Fisch (1991) is one good general introduction to Whewell and his philosophy of science.

science, but massively influential in his time. Whewell also coined the terms scientist (in 1833), physicist, linguistics, consilience, catastrophism, uniformitarianism, and astigmatism, an impressive list. To Michael Faraday he suggested the terms anode, cathode, and ion.[121]

When it came to economics, the Whewell circle was surprisingly advanced. Most of all, they opposed Ricardian economics for its overly theoretical bent, and felt the methods of empiricism and induction refuted it, again a very contemporary attitude. Circle members also made contributions to economic reasoning. Edward Rogers wrote a short pamphlet in 1822 that presented some rudiments of supply and demand theory. Whewell himself put forward an approach to demand elasticity that was imperfect but on the right track; he then applied those ideas to problems of tax incidence. Perhaps most importantly, Dionysus Lardner of *Railway Economics* fame – discussed above in chapter two – was in the group. Of all the precursors to Jevons, he stated the principles of marginal revenue and marginal cost most clearly, and he used mathematics to do so.[122]

Most generally, the group understood that economics was on the verge of radical mathematization, at both the theoretical and empirical levels. For the 1820s, that was a remarkably advanced attitude, though it was to take longer than Whewell and his associates were hoping.[123]

Unknowingly, the group also stumbled upon an approach to marginalism that is remarkably contemporary. There are marginalist and supply and demand insights throughout the writings of the Whewell circle, yet they saw mathematization, empirics, and induction as more important than any specific economic hypotheses. We economists have been living in William Whewell's world (and Jevons's world) for quite some while, we just have not known it.

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[121] On coining those terms, see Snyder (2023).
[122] On Rogers, see Henderson (1996, p. 5). On elasticity and tax incidence, see Henderson (1996, pp. 177–202). On Lardner, see Henderson (1996, pp. 294–295). Interestingly, Whewell favored Christian morality over Benthamite morality, see Henderson (1996, p. 104). For criticisms of Ricardo, from the Whewell circle, see for instance Jones (1964) [1831], and Whewell's own Six Lectures on Political Economy (1862, Lecture V0).
[123] On mathematization, see Henderson (1996, pp. 15–16, passim) and also see Whewell (1971 [1829, 1831, 1850].

One modest sign of all these changes is how many advisors, when speaking to individuals considering economics graduate school, recommend math or even computer science as a possible background undergraduate major. While most are still undergraduate economics majors, if only because that is where their interest in economics came from, no one seems to mind if they are not. These days, a background in mathematics or computer science is at least as useful for the graduate work to come. Once you get to graduate school, you will have to learn plenty of math and programming anyway, so why not start off in those fields? The prevailing attitude is that the economics you can figure out along the way, or for some topics you may not need to know much of it at all. How complicated are all those economic principles anyway? General skills of apprenticeship and plain ol' hard work are growing in importance too, as top graduate programs increasingly want their incoming students to have done a "predoc" with an accomplished researcher somewhere along the way.

That's how it stands with empirics. As for economic theory, that too has moved away from the earlier emphasis on marginalism. One sign of the decay of interest in marginalism is that "price theory" has moved to being a niche interest in economics. To some economists, especially from a few decades ago, that may sound almost contradictory, almost like saying "economics has become a niche field within economics." Well, that is a bit true as well.

If you are wondering what I mean by price theory, I am referring to the view that the basic intuitive economic concepts, as would be taught in intermediate microeconomics, are highly useful and for advanced problems too. The price theory approach suggests that you should think very carefully about basic economic concepts and try to figure out which of those apply to the problem you are working on. Your hypothesis should be intelligible in terms of microeconomic concepts that you can hold in your mind and understand. In most (maybe not all?) cases, you should be able to explain some version of those principles to a well-educated, non-economist onlooker.

The price theory approaches also are relatively skeptical of axiomatization and formal proofs, fearing they will never well represent the most important policy-relevant levers for improving our world. There is also a fear that formal proofs will lead us too far down a path where we as human beings do not even understand the actual propositions we are generating, or have enough knowledge of their relevant background context, to apply them usefully to actual decisions.

Price theory, as an approach, is not identical to marginalism. But so many of the basic economic concepts from intermediate micro use marginalist ideas, so price theory has been a comfortable home in which marginalism has flourished, including marginalism as an active research program.

Sadly, price theory is fading in relevance, and it is taking marginalism down with it. It used to be that some graduate programs favored the axiomatized approach to micro and others (e.g., University of Chicago, UCLA, University of Virginia) favored the price theory approach. These days the axiomatizations have won out pretty much everywhere, except at my own George Mason University. I don't know of any other exceptions to that, but I do keep looking for them. To the extent I see exceptions to the dominance of the axiomatic approach, it is because empiricism is ascendant, not because price theory is making a comeback.

I am struck by the Price Theory Camp run for a week each summer, to date organized by Kevin Murphy at the University of Chicago. It advertises: "The Price Theory Summer Camp, led by UChicago economist Kevin Murphy, was created to introduce PhD students from outside the University of Chicago to price theory, which emphasizes the application of basic economic tools to problems." Kevin himself is one of the greatest exponents and practitioners of price theory ever to have lived, a truly impressive economist and scholar.

I love that. But notice what it is saying. The ad suggests – correctly – that a lot of economics PhD students outside of the University of Chicago do not receive a rigorous grounding price theory. They

are not so aware of price theory as an independent mode of investigation. To date, hundreds of graduate students have attended these programs over the course of 14 years.

For all my enthusiasm for what they are doing, the existence of this program makes me uneasy. This rearguard action is a clear sign that price theory is in obvious retreat in the economics profession. And likely it will retreat much further yet. Consider the trajectory of Kevin Murphy himself. He spent most of his career at the University of Chicago, teaching price theory to graduate students at a top six program, but now he has stepped down from that post and retired (briefly he had a stint at University of Austin). Very likely Murphy will no longer influence the next generation of students, at either the graduate or undergraduate level, and that is another sign of intellectual retreat.

The other leader of the price theory movement, Steve Levitt of *Freakonomics* fame, retired from academia and the University of Chicago at age 57, claiming he was having no impact with his research papers. He has stated flat out, "And I think in the marketplace for ideas, I gotta say that the Chicago price theory really has lost." And "I think it [price theory] is essentially lost to posterity at this point." Levitt notes that Milton Friedman had such a worry as long ago as the 1990s.[124]

Sometimes when I interview job candidates for economics slots, I will ask them price theory questions. I am not saying those questions are easy, but I don't pick the very hardest of questions either. These are typically job candidates from the top 20 rated graduate programs, more often than not from the top 10 rated programs. Well over half the time I find they don't give me really good answers, as they cannot think through a problem in price-theoretic terms on their feet. Maybe they're all just really nervous, but I believe it is more than that. I strongly suspect most of them are not masters of price theory. They can, however, in great detail tell you which estimation methods they have used in their job market paper, and how their robustness checks have gone. These

[124] See Hartley's interview with Levitt, Hartley (2024). For further perspectives on price theory, and its development and decline, see Weyl (2019) and Cutsinger and Salter (2024).

individuals just don't think naturally in terms of economic intuition, and the neglected intuitions of course include marginalism.

In 2024 I saw a tweet that summed up what has happened to economic intuition as of late. The author is Shengwu Li, a tenured economic theorist at Harvard (and of the famed Lee family of Singapore). He wrote:

"'Can you give me an economic intuition for that result?' means 'Can you explain that using math that was introduced to economics >20 years ago?' Corollary: Topkis's theorem counts as economic intuition. Also virtual values. And eigenvalues."[125]

I would sooner say that economic intuition, including that of marginalism, is dwindling. Call me old-fashioned or ill-informed, but I had never heard of Topkis's theorem before I read that tweet. I researched the matter and learned:[126]

Topkis's Theorem

Let X and T be complete lattices. Let $f\colon X\times T \to \mathbb{R}$ be supermodular in x and have increasing differences in (x, t). Assume $f(\cdot, t)$ is upper semicontinuous and the argmax is nonempty for each t. Define

$\Phi(t) = argmax_{\{x\in X\}} f(x, t).$

Then:

1. $\Phi(t)$ is a nonempty complete sublattice of X.
2. $\Phi(\cdot)$ is monotone in the strong set order: if $t' \geq t$, then $min\ \Phi(t) \leq min\ \Phi(t')$ and $min\ \Phi(t) \leq min\ \Phi(t')$ where the min and max are taken with respect to the lattice order on X.

[125] Shengwu Li (@ShengwuLi). "'Can You Give Me an Economic Intuition for That Result?' Means 'Can You Explain That Using Math That Was Introduced to Economics >20 Years Ago?' Corollary: Topkis," Twitter (now X), February 27, 2024, 2:31 p.m., https://x.com/ShengwuLi/status/1762364767438798850.

[126] See for instance Yildiz (2024).

- Equivalently, there exist selections $x^{*}(t) \in \Phi(t)$ and $x^{**}(t) \in \Phi(t)$ for all t such that both $x^{*}(\cdot)$ and $x^{**}(\cdot)$ are monotone increasing.

I do not deny Topkis's theorem (do I even understand it?), but explaining some economic result in terms of Topkis's theorem does not, to me, count as satisfying our economic intuitions. Even after extended thought, as I once applied to the diamonds-water paradox in my youth, I do not think in terms of this theorem. Nor do I know many economists who do. You might call invoking the theorem "supra-intuitional," as it stands beyond our normal intuitions about how the world works.

A further look at the Wikipedia page tells us that Topkis's theorem can be used to analyze problems where we might have otherwise used the implicit function theorem (which I have studied and do know). The Topkis theorem, however, is more general and requires fewer assumptions. You can use it, for instance, to derive the conditions under which more potholes on a road means that the drivers won't speed as much. One of those conditions is:

"$U(.)$ is twice continuously differentiable, concave in s, that the domain over which s is defined is convex, and that it there is a unique maximizer $s^{*}(p)$ for every value of p and that $s^{*}(p)$ is in the interior of the set over which s is defined."

Who can argue with that? After all, without the conditions behind that proof, more potholes in the road *might* lead to more speeding rather than less.

It is interesting to read the Twitter responses to Li's observation. One economic theorist from Carnegie Mellon, Ali Shourideh, wrote: "There is a quote in Minnesota attributed to Leo Hurwicz: "Intuition, Shmintuition, look at the ***ing proof!!!"""[127]

[127] Ali Shourideh (@ShouridehAli). "There is a quote in Minnesota attributed to Leo Horwicz: "Intuition, Shmintuition, look at the ***ing proof!!!" I am sure it is from more than 20 years ago!!!," Twitter, February 28, 2024, 10:53 a.m., https://twitter.com/ShouridehAli/status/1762672365366870021. Note I have corrected a spelling mistake in the original, making "Horwicz" to the proper "Hurwicz."

Christopher Phelan, a theoretically oriented economist from the University of Minnesota, tweeted in response: "If the intuition were correct, it would BE the proof. (Larry Jones)"[128]

Economic intuition, RIP. And marginalism with it.

But for Marginalism It Gets Worse Yet

I am seeing the traditional, intuitive approach to economic reasoning retreating from one field after another. To give one vivid and also important example, machine learning and neural nets are overturning the world of finance.

For a long time I have thought of finance as the most advanced and most successful branch of economics. It works with the highest quality data, has many of the most rigorous models, and the economic assumption of "people really do want money only" seems relatively justified in that sphere of endeavor. If you doubt the successes of finance, just consider how much many of the top financial economists have been paid. Even if you are not famous, if you graduate from MIT with a PhD in finance you may well be earning millions a year, less than 10 years later.

Financial economics also has relatively good error-correction mechanisms. A lot of papers are written from the same data sets, or from broadly comparable data sets. So results can be compared to each other, and newer papers can build upon earlier results in a fairly transparent manner. The same cannot be said of say industrial economics, where many of the data sets are firm-specific or industry-specific, perhaps proprietary as well, and it is hard to compare differing results when they come out of different data sets and for that matter different structural models.

[128] Christopher Phelan (@ChrisPhelanEcon). "If the intuition were correct, it would BE the proof. (Larry Jones)," Twitter, February 28, 2024, 11:14 a.m., https://twitter.com/ChrisPhelanEcon/status/1762677688551068154.

All good for finance! But when you look at who the top hedge funds and trading firms hire, economists are playing a smaller and smaller role in the sector. Those finance PhDs from MIT are still out there, but they play an increasingly small role. They are being outcompeted by individuals with degrees in math, computer science, and physics. The requirements for quantitative skills at the extreme frontier are so great that very few economists – MIT degrees or not – remain competitive. If anyone in the sector needs to learn some economic models, they can do so with a modest investment of time, as the quant skills are the truly essential input, not the economics. And so, the practice of financial economics is moving in very different directions, including those of machine learning and neural nets. The ascension of economic portfolio models, which started in the 1960s, is now very far from the relevant frontiers. To put it very bluntly, at the current state of knowledge those models are failing the market test.

This is especially painful for economics, because the original triumphs of economics in that field were explicitly marginalist in their origins. As discussed in chapter one, one of the first major contributions of economics to finance was CAPM, or the Capital Asset Pricing Model, dating from the work of Sharpe, Treynor, Lintner, Mossin, and others in the 1960s. CAPM builders started with the concept of risk-aversion, or in other words the diminishing marginal utility of money. It assumed investors wanted to avoid gambles, unless those gambles brought them higher expected returns. In the final equilibrium, the assets that brought higher expected returns were precisely those associated with higher on net gambles for investor portfolios, namely assets that had a positive covariance with the market portfolio and thus exacerbated risk rather than limiting it. So for instance, countercyclical gold (it was countercyclical in some earlier times) should yield lower than average returns, and procyclical stocks (some consumer durables companies) should yield higher than average returns. Higher expected return would, in equilibrium, compensate for higher systematic risk. That risk – covariance with the market portfolio – was dubbed "Beta."

That all sounded wonderful, and that core model and its offshoots dominated financial research for decades. The problem, however, was that it wasn't true, or at least it wasn't nearly as true as we had

thought and hoped. When financial economists refined the models with more complete specifications, it turned out Beta didn't predict stock returns much at all. Eugene Fama and Kenneth French delivered one of the final blows to earlier approaches with a 1992 paper that showed Beta didn't have explanatory power over expected returns at all. Since Fama himself was one of the original architects of CAPM-like reasoning, and French also was a renowned finance economist, these revisions to the model were credible. For all its original promise, marginalism, and the concomitant notion of diminishing marginal utility, no longer seemed to help explain asset returns.

Under one plausible account of intellectual history, you can date the decline of marginalism to that 1992 paper. In the most rigorous, data-oriented, and highest-paying field of economics, namely finance, marginalist constructs had every chance to succeed. In fact, they ran the board for several decades. But over time they failed. In the most prestigious field of economics, marginalism has been in full retreat for over 30 years, and it shows no signs of making a comeback.

We already know that financial practice is dominated by the (non-economist) quants. But how about financial economics research, the parts that are still done by economists? What direction is that work moving in?

I was struck by a 2024 paper published in the *Journal of Financial Economics*, one of the two leading journals of financial economics (*Journal of Finance* is the other). The authors are Scott Murray, Yusen Xia, and Houping Xiao, and the title is "Charting by Machines." The core result is pretty simple, and best expressed in the well-written abstract:

"We test the efficient market hypothesis by using machine learning to forecast stock returns from historical performance. These forecasts strongly predict the cross-section of future stock returns. The predictive power holds in most subperiods and is strong among the largest 500 stocks. The forecasting function has important nonlinearities and interactions, is remarkably stable through time, and captures effects distinct from momentum, reversal and extant technical signals. These findings

question the efficient market hypothesis and indicate that technical analysis and charting have merit. We also demonstrate that machine learning models that perform well in optimization continue to perform well out-of-sample."[129]

In other words, the successful approach to predicting returns is giving up on traditional portfolio theory and using the "theory-less" technique of machine learning. Although this is published in the *Journal of Financial Economics*, in some significant sense it is not economic reasoning at all. It is calculation, combined with expertise in math and computer science. The modeling is not economic modeling in a manner that has ties to marginalism or standard intuitive microeconomic theory. And the work is predicting excess returns in a pretty robust and successful way.

Some may respond by noting that those excess returns may disappear with the publication of this article, or more likely yet they have disappeared already as the knowledge spread pre-publication. That may well be true, but such an observation is missing the point. One question is, "for how long will any identification of excess returns last, no matter how those excess returns were identified?" That is a valuable and important research (and practical) issue. But no matter what the answer to that question, it will not dislodge the best answer to, "did we use standard economic theory, or the calculation-intensive methods of machine learning, to achieve these results?" We already know the answer to that query, and it is likely that future finance research on excess returns will move in similar directions. In fact, that 2024 article is just scratching the surface, as broader AI techniques, going above and beyond current machine learning approaches, are likely to grow more powerful.

There is a recent working paper which is perhaps more striking yet, by Antoine Didisheim, Shikun (Barry) Ke, Bryan T. Kelly, and Semyon Malamud. They pick up from Arbitrage Pricing Theory (APT), a well-established idea from financial economics. APT typically looks for "factors" in the

[129] Murray, Xia, and Xiao (2024, p. 1). Or consider the new paper Borri, Chetverikov, Liu, and Tsyvinski (2024). They propose a new non-linear, single-factor asset pricing model. In the abstract: "Most known finance and macro factors become insignificant controlling for our single-factor." Yet you won't find traditional economic variables discussed in this paper, it is all about the math, in particular a representation of the Kolmogorov-Arnold representation theorem.

data which predict excess returns, and a traditional APT model might have found five or six such factors. Are "inflation" or perhaps "the term structure of interest rates" useful factors? Well, that can be debated, but if so, those results sound pretty intuitive. But those intuitions seem to be disappearing. In a paper by these authors, they apply machine learning methods to look for more factors. As we know, machine learning is very good at finding non-obvious relationships in the data. The largest model they built has 360,000 (!) factors, and it reduces pricing errors by 54.8 percent relative to the classic six-factor model from Fama and French. Bravo to the authors, but what kinds of intuitions do you think possibly can be supported by those 360,000 factors?[130]

To sum it all up, finance has fallen. Not to "the enemy," because the enemy is us, namely the economics and finance profession as a whole. Significant parts of financial theory, and the practice of finding excess returns, are moving well outside the ambits of traditional microeconomic and portfolio reasoning.

In related work, two economists use ChatGPT to digest and interpret the news, and find that it too has some power to forecast returns, relative to traditional approaches, especially for smaller stocks and negative news. Again, the point is not to sell you on any particular result or paper (early results in a field typically are revised or improved on), rather to show you which way the wind is blowing.[131]

Finance is one of the first fields to be revolutionized in this manner, in part because the data are so good, and in part because the stakes are so high that it makes sense to put a lot of time, money, and talent into these research lines. But there is no reason to think finance will be the last area of economics to change fundamentally, even though other fields of investigation will require more time and investment and are likely to change more slowly than did finance.

[130] See Didisheim, Ke, Kelly, and Malamud (2024), pp. 2–4 for a summary of their results.
[131] See Lopez-Lira and Tang (2023).

The ties of empirical work in economics to economic theory are evolving, and in particular the explicit ties to intuitive microeconomic reasoning, and marginalist thinking, are being cut. In much of traditional econometrics, the emphasis is on testing pre-existing models, identifying possible causal effects, and finding unbiased estimates of coefficients. That connects reasonably well to the microeconomics we have been teaching since the Marginal Revolution was understood and digested. But in machine learning, we let the algorithm build the "theory" for us, noting it may have tens of millions of variables and thus not count as a theory in a way that can be digested by an individual human mind. What role is intuitive microeconomics supposed to play in such a system? Big data, flexibility of estimation, and out of sample prediction are prioritized, not concordance with what an economist, even a brilliant one, is geared to expect or even able to understand.[132]

Of course, as the internet, tech and AI become more important in everyday life, we can expect the import of big data to grow. That in turn will boost the import of machine learning and related techniques, and it will further diminish the power of human intuitions about economics. AI is likely to push us further yet in these directions, even if we do not currently know the exact forms that AI-driven economics investigation is likely to take.

Some young researchers from MIT and Harvard have developed a new method of doing research, which they describe as "fully automated social science." In that method, they create scenarios based on standard economic problems, for instance a number of individuals bidding at an auction. They then simulate that scenario, with LLMs playing the roles of the seller and the bidders, generating artificial data. That creates a structural estimation of the problem, and those results are fed to an LLM standing outside of the initial process. The researchers then have built an open-source computational system, capable of understanding that economic problem, and in principle many other economic problems as well. The parameters of the problem then can be varied, and the LLM can be queried as to what are the likely comparative static results. As the researchers put it: "Our contribution is to demonstrate that it is possible to create a system without human input at any step

[132] On machine learning in economics most generally, I have found Athey and Imbens (2019) and Athey (2019).

– to simulate the entire social science process." That is all from Benjamin S. Manning, Kehang Zhu, and John J. Horton.[133]

In other words, we now can create controlled experimental simulations "en masse." Researchers could use this method to see how sensitive a given result is to parameter values, for instance. Or we might see when LLMs predict results that differ from those of theory, which could cause us to look at the theory (or the LLM) more closely. We also could prompt the LLMs in different manners in the original simulations, thereby learning the import of different behavioral assumptions.

Note how this differs from simply using LLMs as stand-ins for the humans in experimental economics. Experiments are initially run with LLMs as subjects, but the learnings from those experiments are fed back into the LLM as an input for its further simulations (the researchers show that this multi-step process outperforms simply asking LLMs what is likely to result from a given scenario). As the researchers put it, "There is good reason to believe that LLMs possess latent information about human behavior." Their method is one possible way of uncovering that latent information. As a side observation, this research also shows that using LLMs to uncover underlying truths can involve a great deal of work, and does not follow from posing them simple queries, as many LLM skeptics are wont to do.

It remains to be seen how useful this method will prove. Presumably, if it does make initial progress, the feedback from that progress will itself imply some modifications to the core approach. Still, this approach is important even if it does not triumph in its current form. These researchers have generated an entirely new and coherent way to model of human behavior, and thus this work stands a chance of being one of the most important breakthroughs in economics in recent times. I would describe it as "let LLMs play around, create a structural model from that, and then feed that model

[133] See Manning, Zhu, and Horton (2024). Hewitt, Ashokkumar, Ghezae, and Willer (2024) use less elaborate techniques and find that a straightforward use of LLMs can use simulated responses to predict actual treatment effects with a correlation of 0.85. See also Tranchero, Brenninkmeijer, Murugan, and Nagaraj (2024) for a further investigation, focusing on management problems and research.

back into an LLM." New models of human behavior are not created very often, but now we have another one. You also could say these researchers have created an entirely new method for doing economic research. One amazing side point is that, at least circa early 2026, most economists have yet to notice. Perhaps Jevons would not be surprised?

We do not yet know how much attention this paper, or its possible extensions and spin-offs, will generate. Nonetheless I am ever so slightly reminded of the early days of marginalism. But whether or not this particular approach takes off, I expect other new methods, based on AI, LLMs, machine learning, and neural nets, in varying combinations, will follow in its wake. So while my treatment here may end up obsolete rather rapidly, that is in fact my point. Progress in economic methods will be arriving at flood-like speeds, just as it did once marginalist insights were digested and applied.

And yes, marginalism is embedded in the training set for these AI models, and probably always will be, but likely so are the Four Gospels, among many other texts. It can be said that marginalism will not die, but we will automate it, and in the process drain marginalism away from the human intuitions of most economists.

What about prediction? How is AI doing in that sphere of human activity? I'm not talking about macroeconomic prediction, using for instance time series as a macroeconomist might do. Just plain, flat out prediction, such as whether Israel will attack Iran before a prespecified date, or vice versa, or whether Hamas will lose control of Gaza. It turns out that LLMs as predictors already match the accuracy of human crowds, the "crowd" here being individuals who would participate in a forecasting tournament, compared against a wide variety of LLMs. Furthermore, the predictions of the LLMs can be improved further by exposing them to the predictions of the median human forecaster. That's a start, and the time may come when the LLMs can predict better than the humans. In the meantime, consulting the LLMs is much easier and cheaper than setting up a forecasting tournament. So by at least one measure, we now have a superior method of forecasting

real world events, namely LLMs. All of these results, by the way, I expect will be much further along within a year's time.[134]

And as for that macroeconomic forecasting I just mentioned, there is a recent paper comparing LLM forecasts to ARMA-GARCH models, a standard time series technique. By this point, need I tell you? The LLMs come out ahead.[135]

In another recent paper, outside the realm of economics, LLMs outperform human experts in predicting the results of neuroscience experiments. It works better yet when the LLM is trained on the neuroscience literature. The authors note that their results are not neuroscience specific, and likely can be transferred to many areas. Yet another study created an LLM chemistry agent "to accomplish tasks across organic synthesis, drug discovery, and materials design," with apparent successes in automating some tasks in chemistry research. There is plenty of work applying machine learning to the methods of the physical sciences, and to date the most famous triumph of AI in biology produced protein structure prediction, using a neural network model known as Alphafold.[136]

Jensen Huang of Nvidia is reputed to have said: "Biology has the opportunity to be engineering not science. When something becomes engineering not science it becomes … exponentially improving, it can compound on the benefits of previous years."[137]

Very likely these papers and methods will be obsolete very soon, or at least not the very best examples of AI progress in the sciences. Again, that is exactly the point, as progress along these lines is remarkably rapid at the moment. At the current moment, talk of agent-driven research is the rage, and that will probably help revolutionize the sciences as well.

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[134] See Schoenegger, Tuminauskaite, Park, Bastos, and Tetlock (2023). See also Yu, et. al. (2023).
[135] See Yu, et. al. (2023).
[136] On the neuroscience results, see Luo, et. al. (2024). The chemistry paper is Bran, et. al. (2023), and for machine learning and the physical sciences see Carleo (2019). The Alphafold paper is Jumper (2021). On machine learning as applied to economics, see for instance Belloni, Chernozhukov, and Hansen (2014) and Mullainathan and Spies (2017).
[137] Cited by Eric Topol in his dialogue with Daphne Koller, Topol (2024).

So much for prediction, what about hypothesis generation? Well, there is a new approach to that too, using machine learning. Economists Jens Ludwig and Sendhil Mullainathan have a 134-page working paper titled "Machine Learning as a Tool for Hypothesis Generation." They focus their investigation on a specific problem, namely which features in the faces of defendants influence the sentence given by the judge. (Are you not sure this kind of work should be done by economists? You are way behind the times – those battles are over.) Using 51,751 observations, they find that the pixels alone in the defendant's mug shot account for up to half of the predictable variation in the sentence. But what exactly in the face is doing the predictive work, and can we predict better yet? They find that machine learning generates useful hypotheses that are not already known to experts or the existing body of psychology research. That is, the machine can "see" features of the faces that predict judge behavior, even when humans are not explicitly aware of those features, or cannot articulate them in the form of a hypothesis. It turns out that two variables of particular note are being well-groomed (already obvious to me?), and whether the face is heavy-set in appearance (less obvious to me).[138]

Ludwig and Mullainathan note that their method is general, and that faces of defendants are but a single example of how machine learning might generate novel and interesting hypotheses. In principle, similar methods could be applied to any high-dimensional dataset, such as cell phone data, satellite data, on-line photos and images, online behavior, student work, medical images, corporate filings, and much more, including any form of high-frequency time series data. In other words, they (building on a good deal of previous work in machine learning) have come up with a new way of generating hypotheses (new for economics at least), and some of those hypotheses pan out.

Note there is no simple way to sum up their model, because as the authors note a machine learning model can have tens of millions of parameters. We can work hard on making some particular predictors "interpretable," as Ludwig and Mullainathan do, but at the end of the day the algorithms are doing something that is far from transparent to the researchers and referees, much less to casual observers.

[138] Ludwig and Mullainathan (2023), and see also Batista and Ross (2024).

Another recent paper, a gargantuan study by Chris Lu, Cong Lu, Robert Tjarko Lange, Jakob Foerster, Jeff Clune, and David Ha has the ambitious title "The AI Scientist: Towards Fully Automated Open-Ended Scientific Discovery." The researchers build an autonomous AI scientist which "generates novel research ideas, writes code, executes experiments, visualizes results, describes its finding by writing a full scientific paper, and then runs a simulated review process for evaluation." Their long paper reproduces much of the work of the AI scientist, which yes really does all these things. Are these papers good enough to be published in top journals? Probably not. Are they impressive work nonetheless? Absolutely. This research method is only going to get better with time, and of course it could be focused more specifically on economic questions as well.[139]

The bottom line is this: through machine learning, the social sciences have as of late come up with new means of generating, testing, and evaluating novel hypotheses. We used to view that as an especially "human" activity, but the machines are encroaching on that too.

Of course it would be remiss to not consider LLMs themselves. They can be thought of as a recent attempt to model language and to model verbal reasoning. We used to look to linguists to do this, or perhaps philosophers, or logicians. These days, by far the most successful attempts to model language and also reasoning come through computer science, namely Large Language Models and perhaps their eventual offshoots as well.

This text is not the place to explain how LLMs work, and besides any explanation rapidly would be obsolete, due to the rate of current progress in the field (you might do best by asking an LLM; as a general observation, books need to stop filling pages where an LLM would suffice). Suffice to say, LLM construction has for the most part ignored linguists and philosophers, and that also means ignoring their intuitions. LLM construction also ignored a lot of people in the AI field who insisted neural nets were a dead end. Instead, in a relatively short number of years humans invented new

[139] See Lu, Lu, Lange, Foerster, Clune, and Ha (2024).

ways of modeling language and reasoning through language. That research program has proven wildly successful, as we have *much* better models of language and reasoning than almost anyone had been expecting.

Current LLMs can ace a wide battery of tests, including in medicine, the law, and for that matter economics. Non-crazy people talk of achieving "AGI" – artificial general intelligence – within a year or two. Exact definitions of AGI differ, but typically it refers to a machine that can surpass humans at most tasks of intellect and reasoning, or perhaps a program that can do most non-physical human jobs. If most LLMs do not pass "Turing tests," that time-honored standard for whether an AI device can fool us into thinking it is human, it is because they are too smart and too sophisticated, not because they are too bumbling. LLMs for instance are quite capable of understanding and generating humor and puns, or serving as therapists to humans.

The classic breakthrough paper behind LLMs was a 2017 study titled "Attention is All You Need," where in this context attention is defined by GPT-4 as "a mechanism that learns to focus selectively on parts of an input sequence, giving it 'attention,' while encoding a sentence or piece of information. This allows the model to treat different words or characters with different levels of importance, providing a 'weight' that aids in better understanding and decoding of information." The paper was not titled "More Linguists are All You Need," or for that matter "Marginalism is All You Need." In other works, given some of the most complex human systems, we came up with ways of understanding them that were new. To be clear, neural nets were not new, since the ideas and also the practice (in much weaker form) have been around for decades. High-powered, well-functioning neural nets, however, are new in the contexts of providing excellent results for general linguistic ability and general reasoning.[140]

[140] I accessed the Playground on March 26, 2024 and prompted it: In the famous paper "Attention is All You Need," on AI, what is a simple way of presenting how the concept of attention is defined there?"

By the time you are reading this, the LLMs I am citing likely are themselves irrelevant. But they won't be replaced by older scientific and investigatory methods, rather scientific and intellectual progress is likely to continue further in recent directions.

As for images and video, I will simply refer you to GPT Image, Midjourney, Veo, and the many other innovations coming into the market. We are developing new ways of modeling the image and video creation process.

We even are finding new ways to model the game of chess, and we are doing so without any particular chess understanding. As of 2024, it is possible to produce "Grandmaster-Level Chess Without Search," courtesy of Google DeepMind.[141]

What exactly does that mean? One intuitive way of expressing the result is that AIs can play top-level chess without understanding anything about chess, and without searching through trees of chess moves. A typical top chess engine, such as Stockfish, will search different parts of the decision tree, make an evaluation of different possible positions, and then choose the best move. It is not hard to set up or access different ways of watching to evaluate the most desirable parts of the decision tree. The quality of the chess engine depends on its computing power, the quality of its pruning algorithms, the complex heuristics it has been fed, whether the right "fixes" have been applied to it, and more. Nonetheless in very broad terms it can be said that the engine evaluates sequences of moves, in broad terms, as a skilled human would do. It searches through decision trees.

The new innovation from DeepMind dispenses with all of this. It is a transformer model that was fed 15 billion pieces of data, namely individual chess positions graded by Stockfish as to which player is better or worse and by how much. In AI lingo, it can be said that the large and for the time being unprecedented number of chess data points – 15 billion – represents a major investment in *scaling*. In the vernacular, it could be said that more is being stuffed down the throat of the beast.

[141] See Ruoss, et. al. (2024).

Transformers then do as transformers will do, namely they use those data points to figure out which features of positions are appealing ones, or not, and those "judgments" get coded into neural nets, which then generate the subsequent decisions. In blitz play, this creature can achieve an Elo chess rating of 2895, which makes it competitive with the top humans. It also can solve a significant fraction of difficult chess puzzles. It is not as good as Stockfish 16, which beats the top humans virtually all of the time, but this particular technique is being realized for the first time. If DeepMind decides it merits further investment, presumably later generations of this technique will be stronger yet. As can usually be said about AI, you are currently witnessing the weakest version of the thing you ever will see.[142]

As the creators of this method note, it reminds them of an old quotation from 1920s world chess champion Capablanca: "I see only one move ahead, but it is always the correct one."

Whither human intuition, and also economic intuition?

Contemporary economics was born from the Marginal Revolution, but if we are forecasting its future, right now it is hard to see even one move ahead, much less the correct one.

There is however a slightly scarier version of this story yet. Maybe our intuitions about the world, including the economic world, were never so strong in the first place. Maybe we put so much value on "intuitive" results, in 20th century microeconomics, as a kind of cope and also security blanket, to make up for this deficiency. But our intuitions, even assuming them to be largely correct, always were just a small corner of understanding, swimming in a larger froth of epistemic chaos. And now the illusion has been stripped bare, and the true complexities of economic reasoning are being revealed.

[142] If you are wondering about the contrast with Alpha Zero, Alpha Zero "learns" by playing millions of games against itself. This creation "learns" by receiving a very large number of evaluations from Stockfish 16.

As Arnold Kling would say, "Have a nice day."

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