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"Just Keep Discounting, But..."

Martin L. Weitzman

We at this workshop have been asked to address a seemingly arcane issue of technical economics that has important real-world consequences. Furthermore, we have been asked to explain this issue, and to explain how we think it should be resolved, in a language and a style that can be understood by an interested general audience.

The seemingly arcane issue concerns how to discount events that come to pass in what I shall call the "deep future"—meaning they will happen many generations, even centuries, from now, long after we and everyone we know have passed away. At first thought it might seem that such projects are of limited practical importance. Maybe that was true once, but increasingly today we are being asked to analyze projects whose effects will be spread out over hundreds of years. The most prominent single example of this is the mother of environmental catastrophes—global warming. Examples other than global climate change include: radioactive waste disposal, biodiversity loss, stratospheric ozone, groundwater pollution, and mineral depletion. Projects or activities with prominent deep-future payoffs are all over the environmental landscape today.

Now, a funny thing happened to us economists on the way to the forum when we tried to apply standard benefit-cost methods to these really long-term effects. It turned out that the deep-future part of deep-future projects didn't much matter. For any reasonable discount rates (above a couple or so percentage points per year), what happens a few centuries from now hardly counts at all. The reason, as I am sure most people here know, is the extraordinary power of compound interest to discount a deep-future dollar's worth of goods and services into near nothingness as measured by today's dollars.

MARTIN L. WEITZMAN is the Ernest E. Monrad Professor of Economics at Harvard University.

What this result means is something like the following. Global warming might cause a rise in sea level, which in turn could cause flooding in low-lying cities if they were not walled off and pumped out. But if and when this event happens two centuries or more into the future, it's not such a big deal because a very modest savings program started now would accumulate enough bricks and metal and so forth that we could easily afford to build the walls and pumps and everything else we would need *if the underlying trend of the real rate of interest remains about the same.* This, I believe, is the key issue in deep-future discounting, and we should not be led astray by the numerous sideshows that impair focusing directly on it.

The remainder of the paper, then, tries to answer the key question: what is our best prediction of the real rate of interest into the deep future?

Well, what drives deep-future real interest rates? For that matter, what determines the underlying trend of the real rate of interest more generally?

For society as a whole, our economy acts like one big bank account. If we decide to curtail our consumption this year by saving an extra dollar's worth of consumption, that's just like depositing an extra dollar's worth of the underlying resources, which have been liberated by our refraining from consuming them, into society's big bank account to be recycled for capital formation. The amount of extra future output that we get from such an act of saving is, essentially, the real interest rate. The value of the real interest rate is just the return on the loans that the bank can make with our savings, otherwise known as the "productivity of investment."

So, the real rate of interest for any time period is determined by the productivity of investment in that period. What drives the deep-future interest rate is then the same force or forces that drive the deep-future productivity of investment.

What the economists on their way to the forum are doing when they apply standard benefit-cost methodology to deep-future projects is something like the following. They assume, implicitly, that the productivity of investment in the deep future will be about the same as it has been in the recent past. Since it is a fair generalization to say that the rate of return on capital has been essentially trendless over past periods for which we have data, it seems like a reasonable procedure to project this trendless real interest rate forward to the deep future. Or is it so reasonable?

An old debate on the so-called "limits to growth"—defined broadly—has been taking place for a long time now. Although the exact content of the debate may differ from decade to decade, its generic form is surprisingly invariant over several centuries.

The "growth pessimist" has a world view of history wherein humanity is condemned to be navigating toward a horizon over which looms stagnation, or worse, because we will run out of one limiting factor or another—like land, labor, minerals, a clean environment, or whatever. Then diminishing returns will set in, choking off growth.

"Diminishing returns" in this context means that the productivity of investment declines to zero in the deep future because so much capital will have been accumulated by then that more of it is no longer effective in substituting for the limiting factor or factors.

By contrast, the typical economist tends to be a "growth optimist," believing that human ingenuity will always rescue the day because, in the words of J. M. Clark, "knowledge is the only instrument of production not subject to diminishing returns."

Thus, the "limits to growth" debate is in essence a debate about the deep-future productivity of capital. Which side is right?

The forces that determine the deep-future productivity of capital are essentially the same forces that determine deep-future productivity more generally. Productivity is just output per unit of inputs.

It turns out that a long line of extremely important economic research has proven beyond any reasonable doubt that, almost no matter how it is measured, long-term productivity has grown over time at a more or less trendless rate. Whether we look at time series of raw tons of outputs over raw tons of inputs, or quality-adjusted tons of outputs over quality-adjusted tons of inputs, or dollars-worth of outputs over dollars-worth of inputs, or almost-anything-weighted outputs over almost-anything-weighted inputs, the ratio just seems to keep growing trendlessly over time.

The reason that we keep on getting ever more units of output per unit of inputs is because of "technical progress," which is just a synonym for human ingenuity or inventiveness. It is technical progress that, by warding off diminishing returns, prevents capital productivity from falling over time. Like a magic cook in a fairy tale, we seem always to be able to conjure up fantastic new recipes for combining inputs to make bigger portions of food out of the same base of raw ingredients.

The chain of reasoning looks like this so far. Deep-future interest rates are driven by essentially the same forces that drive deep-future capital productivity, which is driven by essentially the same forces that motivate and propel human inventiveness in conjuring up new ways to offset diminishing returns. So a projection of real interest rates from the recent past into the deep future is very much the same thing as an extrapolation of the rate of technical change from the recent past into the deep future. Everything, then, comes down to estimating the deep-future effectiveness of human ingenuity to come up with new recipes of production for new circumstances.

Is it possible to conceive of a world where we run out of productive new ideas? Yes, in a way it is. All inventors have times when they just don't seem able to think up creative new ideas, or the potentially new ideas just don't seem to work out. All of us academic researchers have had dry spells where we go through the motions of research, we continue the finger exercises, but we don't really have any good ideas and can't come up with anything truly original. As it is for individuals, so it is for companies, or sectors of an economy, or even whole societies. It seems that some periods are just not very fruitful for human ingenuity or inventiveness. At a very high level of abstraction, then, it is possible to imagine a time of no technical progress because there just happens to have been a streak of bad luck during that time.

But is it possible to imagine a deep-future world in which the state of knowledge is perpetually stagnant? After all, maybe good ideas are like stars in the sky—there are a lot of them out there to be discovered but not an infinite number, and eventually even the stars run out. It might feel very frustrating to live in such a twilight world of no technical progress, where we would be having always the empty sensation that anything we could invent has already been invented. But there is no reason in principle why God made the universe with an infinite supply of good ideas. Or is there?

If your intuition is like mine, something will strike you as intrinsically implausible about a world supposedly in a steady state where it is impossible to think up any fruitful new inventions. In a world like this, the production of new knowledge would be a process akin to the discovery of new oil fields, where even the cleverest geologist some day finds that there is just no more oil to be discovered.

It seems to me that something fundamentally different is involved here. When research effort is applied, new ideas arise out of existing ideas in some kind of cumulative interactive process that intuitively has a very different feel from prospecting for petroleum. To me, the research process has a sort of pattern-fitting or combinatoric feel about it. It seems that ideas build upon each other in such a way that many new ideas are essentially successful reconfigurations of already existing ideas that have not previously been combined with each other. If it is true that many basic ideas can be viewed as combinations of other basic ideas, then there is the potential for knowledge to build upon itself by a kind of self-generating recombinant feedback process where the discovery of new ideas itself generates further new ideas by combinations of the new with the old.

The power of combinatorics becomes overwhelming past a certain point. Most people do not appreciate the fantastically large number of

ways that even familiar everyday objects like cards in a deck or bits on a hard-disk drive can be combined with each other. It has been calculated that the number of different bit strings that can be put on today's average personal computer disk is larger than the number of elementary particles in the universe, while the number of ways to arrange an ordinary deck of playing cards exceeds the number of seconds that have elapsed since the big bang. Imagine, then, what eventually happens to a dynamic process where the number of playing cards in the deck is itself an endogenously determined variable that increases in proportion to the number of untried card arrangements. A growth process whose increments are based on any positive fraction of previously untried combinations may start out growing very slowly. But even for the tiniest proportionality factor, given enough time a combinatoric growth process will always end up overwhelming any growth process based on mere geometric expansion.

If this way of looking at the world is correct, and ideas are fundamentally self generating, I don't think we are going to run out of new inventions. The germ of truth here, which I think is relatively robust, is that if we buy into the idea of combinatoric innovation augmenting a fixed factor, then there is a sense in which we have to bend over backwards *not* to buy into the undoing of diminishing returns as an ultimate retardant of economic growth. If the essence of the creative act consists of cleverly combining and recombining useful existing ideas to fashion useful new ideas, then there is truly a rigorous sense in which human ingenuity can be said to have the potential to overcome diminishing returns.

Coming back to the main strand, I don't see fundamental reasons, or what might be called "reasons of principle," why the productivity of capital in the deep future should be lower than it is today. And following that observation, I don't see fundamental reasons why we should not keep on discounting the deep future at today's best estimates of the rate of return to capital. After all, the very nature of deep-future projects allows us to revisit them after five or ten years with new information, including updated discount rates, if we think something has changed.

It is true that elements of "irreversibility" may be involved—both on the side of environmental damages and on the side of investments to offset those damages. An argument based on "irreversibility" per se can be made to cut either way on the issue of slowing down or speeding up remedial action. If our best benefit-cost analysis is indicating strongly that no action is warranted, then I guess I think our best general response is to do nothing now and revisit the issue in five years. So I guess my advice in this case would be the deep-future equivalent of: "take an aspirin now and call me in the morning."

Incidentally, for the purposes of the present paper I think it is largely beside the point to get enmeshed in a detailed discussion about which interest rate out there in the real world is most appropriate to employ as a starting point for benefit-cost analysis, and I sidestep this aspect here. Unless one is prepared to argue the extraordinarily radical proposition that the relevant discount rate should be "adjusted" almost to zero, the fundamental issues raised by deep-future discounting are not pertinently affected by whether this or that real-world interest rate is being used as an initial approximation in benefit-cost calculations.

Why, then, all this controversy, all this fuss and angst, about deep-future discounting? I'm not really sure, but I think that some of it might have to do with the paradoxical nature of compound interest itself. If the power of compound interest over decades or generations is counterintuitive, then perhaps deep-future compounding represents the ultimate paradox of this type.

Suppose you were to sit a schoolchild down in front of a chess board, lay a penny on the first square, two pennies on the second square, four on the third, eight on the fourth, and then ask what would eventually happen if you were to keep on playing this game of "Doubling the Pennies." The answer is that by the time you get to the sixty-fourth square, the value of the pennies would exceed the total wealth of everything in the world. A youngster will have no comprehension of what this means. Why? Because the long term power of compound interest is very counterintuitive!

The deep-future power of compound interest is the ultimate paradox of this genre. It really looks as if it would not make much difference in the relevant units of today's standard of living whether climate change from global warming comes 200 years from now or 400 years from now.

Up to now, this paper is exactly (word-for-word) the paper that I wrote for the conference in November of 1996. The paper to this point represents what I thought *then*. But something happened since then. The conference itself gave me a motivation and an excuse to reflect upon deep-future discounting. As is evident by the text to this point, it was pretty clear to me that the deep-future productivity of capital is actually highly uncertain. When I got to thinking further about it, something started gnawing at me about the peculiar way in which uncertain interest rates need to be averaged over time, and how that might conceivably force a revision in how we conceptualize the problem for the very long run. Then, about a year after the conference was held, the light bulb that signals the "Eureka" experience finally flashed on in my head. (The details are spelled out in a technical working paper entitled "Why the Far-Distant Future Should Be Discounted at its Lowest Possible Rate," available on request.)

The *But...* part of the present paper's title is this: While there is uncertainty about almost everything in the deep future, perhaps the most fundamental uncertainty of all concerns the discount rate itself. As seen from today, the deep-future interest rate is a true random variable for all of the reasons that make the far-distant productivity of capital uncertain. Well, so what? Why not just take expected or average values of interest rates, like we always do? Here is why not!

The variable that should be probability-averaged over various uncertain states of the world is not discount *rates* like r, but discount *factors* like $1/(1 + r)^t$. And this can make an incredible difference for the deep future, when t is very large. In the limit, the properly averaged certainty-equivalent discount *factor* corresponds to the *minimum* discount *rate* having any positive probability mass. From today's perspective, the only relevant limiting scenario is the one with the lowest interest rate—all of the other states at that far-distant time, by comparison, are relatively much less important now because their expected present value is so severely shrunk by the power of compound interest at a higher rate.

What does this mean for the optimal form of a deep-future project like ameliorating the impact of global warming? In effect, the basic idea should make itself felt by biasing strongly the choice of policy instruments and levels of imposed stringency *as if* toward what is optimal for the low-interest-rate scenario, because that scenario will weigh most heavily in the expected difference between benefits and costs.

Uncertainty about future interest rates provides a strong generic rationale for using certainty-equivalent social interest rates that decline over time from around today's observable market values down to the smallest imaginable rates for the far-distant future. As a policy matter, this effect does not "kick in" until we are out of the range of a near-future period within which we can feel confident projecting forward today's relevant interest rates. However, for deep-future projects the effect of declining interest rates might be very important.

Playing casually with some numerical examples suggests to me a sliding-scale social discounting strategy something like the following. For about the next 25 years from the present, use a "low-normal" real annual interest rate of around 3–4%. For the period from about 25 to about 75 years from the present, use a within-period instantaneous interest rate of around 2%. For the period from about 75 to about 300 years from the present, use a within-period instantaneous interest rate of around 1%. And for more than about 300 years from the present, use a within-period instantaneous interest rate of around 0%.

So, I now think the moral of the story is "just keep discounting, but..." at a declining interest rate for very long-term projects.