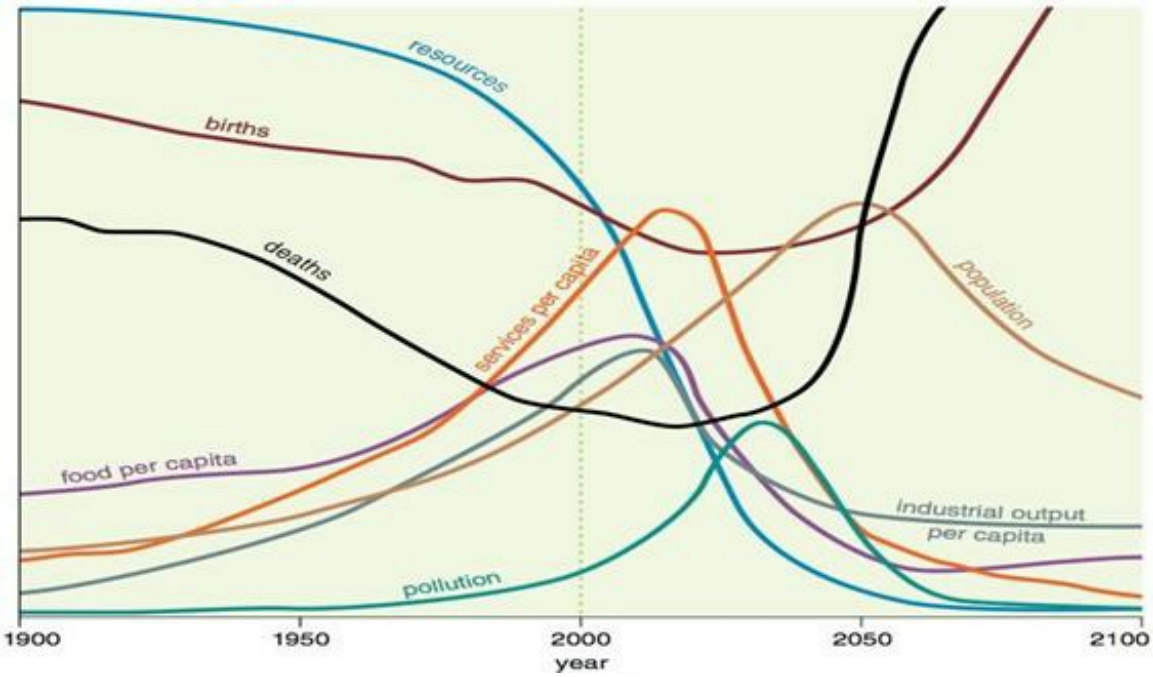
A Few Things You Probably didn't Know about William Nordhaus

Exploring Hydrocarbon Depletion

Ugo Bardi, October 14, 2018

The "base case" scenario of "The Limits to Growth" 1972 report to the Club of Rome. The strong non-linearity of the behavior of complex systems — including the global economy — is nearly impossible to understand for people trained in economics. William Nordhaus, the recent Nobel prize winner in economics, is no exception to the rule. In this post, I'll report how, at the beginning of his career, Nordhaus criticized "The Limits to Growth", showing in the process that he had understood nothing of the way complex systems work.



After having been awarded the Nobel prize in economics of this year, William Nordhaus has been often presented as some sort of an ecologist (see, e.g. this article on Forbes). Surely, Nordhaus' work on climate has merit and he is one of the leading world economists who recognize the importance of the problem and who propose remedies for it. On the other hand, Nordhaus' approach on climate can be criticized: he tends to see the problem in terms of costs and solvable just by means of modest changes.

Nordhaus' approach to climate change mitigation highlights a general problem with how economists tend to tackle complex systems: their training makes them tend to see changes as smooth and gradual. But real-world systems, normally, do what they damn please, including crashing down in what we call the Seneca Effect.

On this point, let me tell you a little story of how Nordhaus started his career at Yale by an all-out attack against system dynamics, the method used to prepare the 1972 study "The Limits to Growth," showing in the process that he had understood nothing on the way complex systems work.

In 1973, Nordhaus published a paper titled "Measurements without Data." It was directed specifically against Jay Forrester, the founder of system dynamics, accusing him of having developed a model able only to describe a world existing only in his (Forrester's) imagination. If you know something about how scientists think, you may understand that this is not just an accusation: it is an insult. And Nordhaus' paper didn't mince words, even getting into direct and personal accusations against Forrester, for instance that he was favorable to the extinction of the human race, that he lacked humility, that he wasn't testing his assumptions, that he ignored the previous literature, that his model was the equivalent of a "widow cruse", and a few more quips.

Nordhaus's attack was one of the first broadsides against world dynamics, possibly the pebble that originated the avalanche of political criticism that gave a bad name to "The Limits to Growth" in the 1980s and 1990s. To get some idea of the adversarial atmosphere of the time, you may like to know that, contrarily to all normal procedures in science, the editor of the journal that published Nordhaus' paper refused to publish Forrester's rebuttal – he was forced to publish it in a much less known journal , where it remained basically unknown.

But what was the substance of Nordhaus's criticism? Nearly half a century after the publication of his paper, it would make little sense to go into the details of its 29 pages, dense with formulas and reasoning. Basically, the paper demonstrates how Nordhaus just didn't understand Forrester's ideas and methods, claiming over and over that standard economics was a better tool to describe the world system. He couldn't understand — just as most modern economists can't — that standard economics doesn't account for the kind of oscillations — including crashes – which are observed in history and that system dynamics describes very well.

This is an especially serious limitation when dealing with the earth's climate, which is a complex system subjected to abrupt changes and tipping points: here the approach of economists is not only wrong but outright dangerous because it leads decision makers to a false sensation of safety and control which, in reality, we don't have.

The whole story is told in some detail in my book “The Limits to Growth Revisited” (Springer 2011. Below, an excerpt dedicated to Nordhaus’s criticism

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From “The Limits to Growth Revisited”
by Ugo Bardi, 2011

We can now examine the work of William Nordhaus, who emerged out of the debate as one of the major critics of the LTG study and, in general, of system dynamics as a method for modeling economic systems. In 1973, Nordhaus published a paper titled “World dynamics: measurements without data” [Nordhaus 1973] taking as a target Forrester’s book [Forrester 1971]. However, it is clear that Nordhaus’s attack broadly included also the LTG work.

Nordhaus’s paper spans 27 pages and contains much material worth discussing, but it would be out of scope to go into all the details here. Forrester himself used 21 full pages in his response that was published in “Policy Sciences” [Forrester et al 1974]. For what we are concerned here, we may summarize Nordhaus’s criticism as pertaining to basically three categories: 1) accusations ad personam, 2) unsubstantiated statements of disbelief and 3) quantifiable criticism.

As for the first category, we can take as an example the accusation of “lack of humility,” made against Forrester. The gist of this accusation is that carrying world simulations all the way to the end of the 21st century is much too ambitious to make sense. This is a legitimate opinion, but not something that can be evaluated on the basis of objective criteria. On this point, however, it is worth noting that Nordhaus himself, later on, committed the same intellectual fault – according to his own definition – with his DICE (Dynamic Integrated Climate Economy) model [Nordhaus 1992, (b)].

The second category of criticism from Nordhaus, “statements of disbelief,” collects alleged shortcomings of world modeling which, however, are not substantiated by actual proof. One such statement, taken as an example, is the following: (p. 1166)

“..we discover dramatic returns to scale of the economy: if we double both the number of blast furnaces and the number of ore fields the output of pig iron quadruples”
But nowhere in his paper does Nordhaus demonstrate that Forrester’s model produces such obviously unrealistic results. In fact, Nordhaus is simply looking at one of the several equations of the model without realizing that the output of each equation will be modified by the interaction with all the other equations and that will insure correct returns to scale. This is the essence of systems thinking: that parts interact.

Let’s now consider the accusation of “measurements without data” which is the most important part of the paper and gives it its title. This is a quantifiable criticism: if it can be shown that Forrester (or the LTG group) were making models which are totally unable to describe the real world, then it is correct to dismiss their work as useless and irrelevant.

In “World Dynamics” (1971) and in “The Limits to Growth” (1972) one thing that can be immediately noticed is that historical world data do not appear in the calculated scenarios. For a reader accustomed to the common approach of “fitting” the data, that gives a bad

impression. Is it possible that the authors of these studies were really so cavalier that they did not care to compare their results to real world's data?

But a more careful examination of the text of both studies shows that the authors do state that their calculations were calibrated on actual historical data. Not showing these data in the figures was a choice made in order to improve clarity. As a choice, it may be criticized, but not ignored.

On this point, note also that, in the "Models of Doom" book [Cole et al 1973] examined before, none of the several authors engaged in the study felt that Forrester's work (or the LTG book) could be criticized in the terms used by Nordhaus. In the chapter by Cole "The Structure of the World Models" [p. 31 of Cole et al 1973] the data used in the models are examined in detail. Some of the approximations utilized are criticized and in some cases it is said that the data are insufficient for the purposes of the model. But it is never stated that the models were "without data".

So, it is clear that the world2 (Forrester's) and world3 (LTG) were calibrated to the historical data – at least within some limits. On this point, although both Forrester and the LTG team made an effort of choosing the parameters of the model on the basis of historical data, they also felt that their models had a heuristic rather than explicitly predictive objective. Therefore, there was no need for their scenarios to use a rigorous data fitting procedure of the type used in physical studies. Again, this is an attitude that can be criticized, but that cannot be ignored.

Forrester himself describes this attitude in his book "World Dynamics"" [Forrester 1971]. On page 14 (2nd edition) he says:

There is nothing new in the use of models to represent social systems. Everyone uses models all the time. Every person in his private life and in his community life uses models for decision making. The mental image of the world around one, carried in each individual's head, is a model. One does not have a family, a business, a city, a government, or a country in his head. He has only selected concepts and relationships that he uses to represent the real system. <..> While none of the computer models of social systems existing today can be considered as more than preliminary, many are now beginning to show the behavioral characteristics of actual systems.
System scientists have a structured approach on this point, as described, for instance, by Sterman [Sterman 2002, p. 523].

… it is important to use proper statistical methods to estimate parameters and assess the ability of the model to replicate historical data when numerical data are available <..> Rigorous defining constructs, attempting to measure them, and using the most appropriate methods to estimate their magnitudes are important antidotes to causal empiricism, muddled formulations and the erroneous conclusions we often draw from our mental models. Ignoring numerical data or failing to use statistical tools when appropriate is sloppy and lazy"
Of course, the very fact that Sterman feels that it is necessary to criticize those modelers

who “fail to use statistical tools” indicates that the problem exists. Modeling socio-economic systems using system dynamics tools is not immune to the biases that are easy to see in the ordinary political debate.

So, taking into account all this, how should we understand Nordhaus’s criticism? If it is intended as meaning that system dynamics models provide only approximations of the historical behavior of the world, then it is a weak criticism that hardly justifies the statement “measurements without data.” This point must have been clear to Nordhaus himself, who tried to substantiate his criticism by the following statement, referred to Forrester’s world2 model (emphasis in the original) :

…..contains 43 variables connected to 22 non-linear (and several linear) relationships. *Not a single relationship or variable is drawn from actual data or empirical studies.*
Let’s analyze this sentence. First of all, Forrester’s model, as all models, contains three elements: the mathematical relationships, or equations, the variables (populations, resources, etc.) and the constants which appear in the equations and which determine the quantitative behavior of the model. Nordhaus speaks here only of two of these elements: variables and relationships, but not of the third; the constants. Clearly, he was aware that Forrester was using constants derived from real world’s data. But, then, what does it mean that “Not a single relationship or variable is drawn from actual data or empirical studies”?

Evidently, Nordhaus thinks that the equations and the variables of the model should have been determined by fitting the experimental data. This is an approach that often goes under the name of “econometrics.” This term does not describe a specific type of model, but it refers to a series of methods and techniques used to fit a set of data, typically a time series, to a model [Franses 2002]. Econometrics can be used to test a model but, in some cases, it is the “best fit” of several models that determines which one is to be chosen. This is a legitimate technique, but one that may easily lead the modeler astray if the physical elements of the system are not sufficiently understood.

In any case, the “best fit procedure” tells you little about the physics of the system being studied. Think of Newton’s law of universal gravitation. The scientists who worked before Newton on planetary motions, from Ptolemy to Johannes Kepler, had basically used a “data fitting” procedure to describe their observations but never could derive the law of universal gravitation using that approach. Instead, Newton devised a law that he thought plausible. Maybe he got the idea watching an apple falling from a tree, but that hardly qualifies as data fitting. Then, he calculated the motion of the planets according to his law. He found that simulated bodies orbiting around the Sun would describe elliptical orbits, just as it was observed for the planets. At this point, he could vary the “g” constant in his law in such a way that it was possible to use the equation to describe the movement of real planets.

So, if Nordhaus’ criticism to Forrester were to be applied to Newton’s gravitation law, then one should criticize it because it is not “drawn from actual data or empirical studies” One could actually criticize Newton for performing “measurements without data.”

Of course, Forrester’s model is much more approximate and tentative than Newton’s law of universal gravitation. Nevertheless, the considerations about the validation of the model remain valid. So, in order to prove his point, “Measurements without data”, Nordhaus needs

to do more. He needs to demonstrate that Forrester's model is totally unable to describe reality.

So, Nordhaus sets up in his paper to "evaluate the specific assumption in the subsectors of World Dynamics." (p. 1160). The examination of the population subsystem is crucial in this analysis. In fig 3 of his article, Nordhaus plots data on the birth rate as a function of the Gross National Product for several countries, together with what he claims to be the results produced by Forrester's model.

Figure 15. Nordhaus model of the population subsector in Forrester's "World Dynamics." From Nordhaus 1973

From this figure, it would seem that Forrester's assumptions are completely wrong and this is, indeed, Nordhaus's conclusion. But what is the curve that Nordhaus calls "Forrester's assumption"? In the article, we read that this curve is "Forrester's assumed response of population to rising per capita non-food consumption when population density, pollution and per capita food consumption is held constant" (emphasis added).

But this is *not* Forrester's assumption. Nordhaus had simply taken one of the equations from Forrester's model and had plotted it keeping constant all parameters except one (the "non-food consumption" that he equates to GNP). But Forrester's model was never meant to work in this way. [..] In the "world3" model all the equations need to be solved together to make the model work as it is supposed to. Nordhaus's obvious mistake was noted and described by Forrester himself [Forrester 1974]:

"The case made by Nordhaus against the population sector of World Dynamics rests on the use of real-world data that he attempts to relate to model assumptions. However, Nordhaus incorrectly compares a single dimensional relationship in world dynamics (between net birth rate and material standard of living) with time series data. He fails to account for the presence of other variables influencing the time series. As a result, he erroneously asserts that the model is inconsistent with the data. In fact, the data Nordhaus present support the validity of the World Dynamics model assumptions."
Subsequently, Forrester runs his complete model and produces the following figure:

Figure 16 – Forrester's response to Nordhaus

In this figure, we see that the behavior of birth rates as a function of GNP produced by Forrester's model is qualitatively consistent with the historical data. Later on, Myrtveit [2005] re-examined the question and arrived at the same conclusion.

It appears clear from this discussion that Nordhaus, in his criticism of Forrester's book, had missed some basic points of the methods and the aims of world modeling by system dynamics. Unfortunately, however, Nordhaus's 1973 paper left a strong imprint in the successive debate, owing in part to Nordhaus' reputation and in part to the fact that Forrester's response [Forrester 1974] wasn't so widely known, mainly because it was published in a scarcely known journal (Policy Sciences) which wasn't even dedicated to economics.

On this issue, it is surprising that the editors of the “Economic Journal,” who published Nordhaus’s paper, did not ask Forrester to reply; as it is a common policy, and even courtesy, in cases such as this one. We have no record that Forrester asked to the “Economics Journal” to publish his rebuttal, but that was the obvious first choice for him if he wanted to reply to Nordhaus; as he did. Consequently, it seems probable that the editors of the “Economic Journal” refused to publish Forrester’s reply and that for this reason he was forced to publish it in another journal. Another indication that the debate about world modeling was especially harsh and that it did not follow the accepted rules for this kind of exchange.

[..]

The debate about world modeling by system dynamics flared again, briefly, in 1992, when three of the authors of the first LTG book (the two Meadows and Jorgen Randers) published a sequel with the title “Beyond the Limits” [Meadows et al. 1992]. In this second book, the authors updated the calculations of the first LTG study, obtaining similar results. The publication of “Beyond the limits” generated a new response from William Nordhaus; this time with the title of “Lethal Models” [Nordhaus 1992]. This new paper took up again some of the earlier arguments put forward by Nordhaus in his 1973 paper, but with considerable differences.

Facing the 43 pages of Nordhaus’ 1992 paper, we immediately see that it does not contain anymore the *ad personam* attacks of his first paper on this subject [Nordhaus 1973]. On the contrary, Nordhaus explicitly thanks the authors of LTG for their comments and their assistance. We also see that this paper does not contain anymore the accusation of “measurements without data” that was the main theme of Nordhaus’s 1973 paper. All that Nordhaus has to say in this respect is (p. 14):

In Limits I, no attempt was made to estimate the behavioral equations econometrically, although some attempt seems to have been made to calibrate some of the equations, such as the population equation, to available data.

It appears that this is not the only point where Nordhaus is backtracking. On page 15, for instance, we read that,

“the dynamic behavior of the enormously complicated LTG was not fully understood (or even understandable) by anyone, either authors or critics”

And we may wonder whether with these “critics” Nordhaus intended also himself.