Notes on "Doomsday Models"

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I would like to state, briefly and bluntly, why I think the various "Doomsday Models" are worthless as science and as guides to public policy. I hope it will not be deduced that I believe the problems of population control, environmental degradation, and resource exhaustion to be unimportant, or that I think an adequate response to them is a vague confidence that something will turn up. They are important problems and, for that very reason, public policy had better be based on sound and careful analysis. I do not think that the global models under discussion provide even the beginnings of that kind of foundation. What follows are some of my reasons.

Structure of the models

The characteristic conclusion of the models in question is that the natural evolution of the world economy is overshoot of its possible equilibrium output, followed by collapse, and that the collapse is likely to happen within the next 30–70 years unless special and drastic changes are made in the behavior of the system. I call this a "conclusion" of the models, but it is actually practically an assumption. That is to say, the step from the assumptions to the conclusion that collapse is imminent is so short and so obvious that it hardly needs "an M.I.T. computer" to do the logic and tell the bad news.

The basic assumption is that the stocks of things like natural resources and the waste-disposal capacity of the environment are finite, that the world economy tends to consume the stocks at an increasing rate, and that there are no mechanisms by which approaching exhaustion tends to turn off consumption gradually. Then, to show how the forecast of overshoot and collapse is resistant against optimism, one is told: Imagine that the stock of natural resources were actually twice as big as we now believe it to be, or imagine that the annual amount of pollution were once halved and then set to growing again, all that would happen is that the date of collapse is postponed by T years, where T is not a large number.

The annual output of any production process, large or small, divided by the annual employment of labor is called the productivity of labor. Symmetrically, though the usage is less common, one could call output per unit of some particular natural resource (or per unit of natural resources in general) the productivity of natural resources. We usually think of the productivity of labor as rising exponentially, say at 2 or 3% per year, because that is the way it has behaved in the past century or so since the statistics began. But all the Doomsday Models are prepared to allow is a once-for-all increase in the productivity of natural resources from one constant level to another. Why shouldn't the productivity of any given natural resource rise steadily through time, like the productivity of labor?

In fact, though the figures have been less studied, Gross National Product (GNP) per unit of natural resources does seem to rise slowly through time. It is easy to understand why labor productivity rises faster. Labor costs amount to some three-quarters of all costs of producing the real GNP; an increase of 1% in the productivity of labor saves 0.75% percent of the cost of producing GNP. Resource costs are a much smaller fraction of GNP (about 5%). Therefore, industry and engineering have a much stronger motive to reduce labor requirements per unit of output by 1% than to reduce resource requirements per unit of output by 1% (assuming, which may or may not be true, that it is intrinsically about as hard to do one as to do the other). But if this is the explanation, then, as the earth's supply of natural resources nears exhaustion and as natural resources become more and more valuable, and as the resource-cost component of GNP rises, the motive to economize resources should become at least as strong as the motive to economize labor. One could imagine that the productivity of resources might begin to increase more rapidly than in the past and to increase fairly steadily.

But then the characteristic assumption–conclusion of the Doomsday Models fails and overshoot–collapse is no longer the inevitable trajectory of the system. Please note that I am not asserting the truth of this counter-story, but only claiming that the overshoot–collapse pattern is built into the models by assumption, very near the surface, and not deduced from any more compelling set of postulates.

Absence of a price system

The most glaring defect of the Forrester–Meadows models is the absence of any sort of functioning price system. The price system is, after all, the main device evolved by capitalist (and, to an increasing extent, even socialist) economies for registering and reacting to relative scarcity. I have already mentioned one way that a price system might radically alter the behavior predicted by the models—by inducing more active search for resource-saving innovations as resource costs bulk larger in total costs and appear to be increasing. There are other, more pedestrian, modes of operation of the market mechanism. Higher and rising prices of exhaustible resources can be expected to lead to the substitution of other, more plentiful, and, therefore, cheaper materials. To the extent that it is impossible to design around or substitute away from expensive natural resources, the prices of commodities containing a lot of them will rise relative to the prices of other goods and services. Consumers will be driven to buy fewer resource-inten- sive goods and more of other things. All these effects work automatically to increase the productivity of natural resources, i.e., to reduce resource requirements per unit of GNP, and steadily, not once-and-for-all—indeed, one might say "exponentially" as first approximation. To forestall misunderstanding, let me say that this is not an argument for laissez-faire. Many markets are "imperfect";
they contain substantial elements of monopoly; information is of different quality and spread unevenly among participants; and private interests, to which prices respond, may be in conflict with the public interest. Since the proper response depends to some extent on what is expected to happen and not on what has already happened, uncoordinated activity may lag too far behind events, and even become perverse. Public agencies can try to shorten the lag by providing the best available information about future supplies and demands. But I don’t see how one can have the slightest confidence in the predictions of models that seem to make no room for everyday market forces.

As a matter of fact, the relative prices of natural resources and resource products have shown no tendency to rise over the past half-century. This means that there have been offsets to any progressive impoverishment of deposits—improvements in extraction technology, savings in use, the availability of cheaper substitutes. The situation could, of course, change. If the expert participants in the market now believed that resource prices would be sharply higher at some time in the future, prices would already be rising.

Suppose you owned one of the few remaining deposits of X and felt reasonably certain that by the year 2000 it would bring the high price of $1000 per ton, because by then X would be very scarce indeed. If the interest rate is now 5% a year and expected to remain near that level (ignoring inflation or deflation of the general price level, for convenience), then the value today of a dollar to be earned in 2000 is only about 25 cents (because 25 cents deposited today in a bank paying 5% annual compound interest and left there for about 28 years will have accumulated to a dollar). That means you would have to get $250 a ton for X now (less something for uncertainty) to induce you to part with any. At any lower price, you would do better to leave it in the ground and borrow, if necessary, to tide you over until the price were higher. Next year you will insist on $262.50 a ton, 5% higher than $250, because next year there will only be 27 years to go until 2000 and a dollar to be delivered in 27 years is worth about 26.25 cents today at 5% interest. In other words, if X is confidently expected to be worth more at some time in the future, then its price will have to rise at an annual rate at least equal to the rate of interest in order to induce owners of the resource to mine any in the meanwhile. The historical steadiness of resource prices thus suggests that participants in the market have not been acting as if they foresaw exhaustion in the absence of substitutes, and therefore sharply higher prices. They may well have been utterly wrong. But the models claim to rely on “experts” for specification of their structure!

**Empirical foundations**

This raises the question of the reliability of the functional relationships that make up the models. Economics has a long history of experience in constructing determinate dynamic models of a national economy, to be used for simple forecasting and for estimating the effects of alternative public policies. These econometric models range in size from a couple of equations to hundreds. They are usually intended for seeing ahead at most six or eight quarters of a year, but there are many models, based on annual data, that are meant to look as much as 5 years ahead, perhaps a bit more. Occasionally, such models are allowed to run for 20 or 30 years with plausible forecasting functions, but these exercises are usually part of an exploration of the nature of the model as a dynamical system and not genuine forecasts.

I think it is the general experience of econometricians that these models positively devour empirical parameters. They require the estimation of very many coefficients describing the response of one economic variable to changes in another. Because economics is not an experimental science, the only source of parameter values is statistical analysis of the historical record of economic behavior. Econometricians invest much of their effort in the statistical exploitation of time series and whatever other relevant data they can find.

So far as one can tell, the Doomsday Models do none of this. There is no trace of detailed statistical estimation of behavior parameters. There is not even any clear standard of goodness of fit, or any reasonably objective test of whether a particular model is any good or not. And indeed, so far as one can tell, some of the important behavior relations that are actually employed appear to be nonsensical, to fly in the face of the facts or to contradict earlier work on the basis of no evidence whatever.

This is a matter of some importance. Econometricians modeling a national economy often have the following experience. Two alternative models—not necessarily radically different, but slightly different in formulation and using slightly different parameter estimates—will fit the observed facts of recent history reasonably well, and about equally well. Moreover, they give forecasts of the immediate future that more or less agree, and which have similar implications for policy. Nevertheless, if you let the models run for many years they will diverge quite considerably, and even may behave in unbelievable ways. One of the reasons for this is that economic quantities are often related to one another with substantial “distributed lags.” These lag relationships are extraordinarily difficult to estimate from time series, yet the long-run characteristics of the model may be fairly sensitive to the lag structure.

Long lags play a crucial role in the Doomsday Models. So far as one can tell, the particular lag structures are essentially invented and then made the basis of long extrapolations. One can perhaps excuse this on the grounds that the underlying configuration of the trajectory has already been imposed by fiat—as discussed above—so the rest is mere detail.

**The Chicken Little syndrome**

I have heard it said that, even if the Doomsday Models are all balderdash, the publicity surrounding them has served an important social purpose in drawing the world’s attention to the possibility that Spaceship Earth is about to abort. Maybe so. It seems to me more likely, however, that the net effect will be a minus. A sound analysis of the dynamics of population growth, resource use, and environmental pollution might provide the basis for public policy. It might that is, suggest a list of things to do that can actually be done, and say what will happen if they are done or if they aren’t. My impression is that the Doomsday Models divert attention from remedial public policy by permitting everyone to blame “the predicament of mankind.” Who could pay attention to a humdrum affair like legislation to tax sulfur emissions when the date of the Apocalypse has just been announced by a computer?