

Indirect Land Use Thoughts: Bruce Dale March 3, 2008

Dear Colleagues:

I have spent a lot of time the last couple of weeks, including most of a recent 12 hour Tokyo-Detroit flight, trying to think through the indirect land use change (ILUC) issue. I have divided my current point of view into two questions that I am asking myself: 1) are we in fact currently able to estimate these changes with any degree of confidence?, and 2) if we could estimate such changes, would it be a good idea to base policy on those estimates? My current answer to both questions is “no”. Please don’t be put off by my answers—I ask you to consider my reasons. Here they are.

1) Are we able to estimate such changes? These changes are estimated by linking demand for corn with land use decisions and the land use decisions with release of greenhouse gases. Three models must be linked: the economic model for supply and demand, the supply and demand model with the land use decision model and the land use model with the release of greenhouse gases. We actually know a fair amount about the effect of land use changes on release of greenhouse gases. My lab has been working with DAYCENT (an agroecosystem model) for the past seven years to better understand the environmental impacts of agricultural operations, including land use change.

I think it is beyond argument that this agroecosystem model, based on thousands of actual field experiments, actual plant and microbial physiology and actual soil-water physical relationships, is by far the most “scientific” piece of the whole “cause and effect” structure outlined above. If you accept that statement, here is a major conclusion of our DAYCENT work to date. It is not possible to draw broad conclusions across a large geographic region about the effects of a particular land use change on the resulting greenhouse gas emissions. Very different greenhouse gas emissions are caused by differences in local soil types (organic matter content, sand, etc.), local climate (temperature, rainfall, etc.), and especially by different tillage and fertilization practices.

For example, we have studied the effects of a change from continuous corn production to a corn-soybean rotation on resulting greenhouse gas emissions in different parts of the Corn Belt. The resulting emissions vary by more than 10 fold in our studies using DAYCENT. Furthermore, it is possible to change these emission patterns greatly by how the system is managed. So how can I possibly believe a model that says that if an area as vast as the Brazilian *cerrado* is converted to corn, that a specific greenhouse gas emission level will occur? I simply don’t believe it. The reality is a lot more complicated, and much more important, a lot more subject to human intervention and management.

If the most “scientific” part of the overall linkage described in the Fargione and Searchinger papers is in fact highly uncertain and imprecise, how can the results of linking three uncertain models together be anything other than speculation? The time may come when a reasonable degree of certainty can be applied to such analyses, but I can tell you that that time is not now based on the uncertainty surrounding the most scientific part of the overall system, the agroecosystem model. Believe the Searchinger and Fargione results if you wish, but they are not science, they are speculation.

2) If we could predict the effects of such changes, should we base policy decisions on them? I have arranged my reasons from the most specific to the most general.

- The legislation regarding ILUC is couched in life cycle terms. Whether Congress intended to or not, LCA has some formal rules. For example, LCA strives to analyze based on specific knowledge of the environmental impacts of inputs and outputs. For example, if electricity is an input for a product, we strive to be specific about where the product is manufactured, because different areas of the country are served by different electrical grids, and each grid has its own greenhouse gas footprint.
- A farmer who produces corn in one county in Iowa under specific practices will have a particular environmental impact, and a farmer in an adjacent county using different practices will have a different impact. If a responsible corn ethanol producer wants to improve his environmental impact, he will source corn from the environmentally superior corn grower. I believe we should encourage, not discourage, such good behavior. The perversity of the ILUC concept is that both the environmentally conscious corn producer and the irresponsible one are equally linked to environmental changes thousands of miles away over which they have no control.
- One of the tenets of the environmental movement has been “think globally, act locally”. But the ILUC idea stands that tenet on its head. If I act to produce a crop with the very best local knowledge, I am still guilty by a very tenuous and speculative association for the actions of others thousands of miles away over whom I have no control. I believe we are much more likely to make environmental progress by holding people responsible for their behavior, and not that of others. The ILUC idea takes the focus off things an individual can control and shifts them toward things he cannot control. That is the wrong direction.
- The policy dilemmas are obvious. If a corn farmer in the U.S., trying to meet national fuel security objectives and also produce corn in an environmentally responsible way, is deemed to contribute to bad behavior in Brazil, just exactly what is the appropriate U. S. policy response? Abandon or limit corn ethanol, tell the Brazilians to clean up their act (good luck on that one), or something else? We may decide to limit biofuel production to certain classes of land, as Searchinger and Fargione recommend, but as far as I am concerned they have not proven their case...not by a long shot.
- My last reason, which I think is the strongest, may also be the most difficult to explain well. I will try. The Searchinger and Fargione argument at its root is this: corn (and perhaps cellulosic) ethanol is not sustainable because it will divert land use for animal feed (over 70% of corn is fed to animals) to new lands that will release large amounts of greenhouse gases as they are cultivated. But if corn for animal feed production were to be expanded, I am confident that they would come to the same conclusion: that would be an unsustainable practice because of the greenhouse gases that would be released as new lands were opened up for corn cultivation. So they are saying that ethanol production from corn or cellulose is unsustainable by linking it to a practice which by itself is not sustainable. In other words, any attempt to use current corn land to make any fuel is unsustainable because we must have that land to continue another unsustainable practice. They are not really making a comparison between gasoline and ethanol, as they think. In reality, they are making a comparison between ethanol and steak (or milk and cheese), and the analysis is forced to choose

steak. How logical is that? I think most folks are missing this enormous contradiction at the root of ILUC analysis. The only way out is to “reimagine agriculture” as Lee and some of the rest of us have suggested.

A couple of sincere and I hope conciliatory parting remarks to my friends. I am doing what I can to ensure that biofuels live up to their potential for environmental improvements. But please recall that there are three fundamental drivers for biofuels, three reasons why we finally have the political coalition necessary to promote biofuels: 1) national security improvements, 2) environmental benefits and 3) rural economic development. Please recall that we are discussing features of the Energy *Independence and Security* Act of 2007 (emphasis added). In our current focus on environmental issues, please let us not lose sight of the other biofuel drivers. Whatever corn ethanol’s environmental performance (I believe it is pretty good and, most important, that it can be improved), without a doubt corn ethanol displaces lots of petroleum (about 22 to 1 on an energy basis) and contributes very significantly to rural development. I score corn ethanol 2.5 on a scale of 0 to 3.0. Without corn ethanol to clear the way, cellulosic ethanol would have a much more difficult task. In our zeal for the “perfect”, let us not destroy the “pretty darn good”. I realize that many of you have a strong distaste (double *entendre* intended) for corn ethanol.

I am not in favor of shielding corn ethanol or any other biofuel from legitimate, well-founded analysis. I just don’t think the ILUC issue is legitimate and well-founded, and certainly not in its current state of development. When we allow poor analysis to get a pass because the result reinforces our beliefs, I think we set ourselves up to have poor analysis used as a weapon against us. We need to be very clear on this, extremely powerful forces would like to bury biofuels, and they will use any weapon that is handy. Whatever the motivations of the authors, the papers by Searchinger and Fargione are being used as weapons against all biofuels, regardless of the actual merits of the fuels.

I strongly believe that our society will have fuels. The alternative to biofuels is not some perfect fuel, most likely it is coal to liquids, or tar sands oil, or oil shale. I continue to be struck at how much biofuel commentary and analysis fails to make any sort of reasonable comparisons with the alternatives. If David Pimentel had been forced to compare ethanol’s “net energy” with that of gasoline, perhaps that specious net energy issue would never have gotten the hold it has on people’s thinking. So, my friends, what are the direct and indirect effects of making fuels from coal, or oil shale, or the tar sands? Now, that is the question I will be asking loudly, but hopefully with my usual courtesy and decorum. © I invite you to join me in asking that question, while we continue honest and rigorous analysis of biofuels.

I hope you feel somewhat rewarded for reading this far. I appreciate it.

Your contrary friend,
Bruce