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GTAP-BIO-ADV and Land Use Emissions from Expanded Biofuels Production

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Preface

This paper is written at the request of the California Air Resource Board with the goal of identifying parameters and model structures that are likely to be important determinants of recent estimates of land use CO_2 emissions produced using the GTAP-BIO-ADV model as reported in Tyner et al. (2010). The GTAP effort builds on decades of data development and the researchers involved have deep experience in agricultural economics, modeling, and in evaluating issues related to ethanol production. My own work on economics of ethanol production goes back 25 years or more but some of my early work drew on the then already considerable expertise of Wally Tyner, lead author of the recent Purdue study. My work for the past decade or so utilizes the GTAP data base, and I collaborated with the GTAP group in developing the GTAP land data set which has provided a key resource for analysts concerned about land use change. My interest and support for the GTAP data effort goes back to its inception at Purdue while I was at USDA. The authors adopt for this work, results from the Terrestrial Ecosystem Model (TEM), and that is a component of the modeling system I have used to evaluate land use emissions (Melillo, et al., 2009). I note these relationships in the interest of allowing readers to take them into account should they sway my evaluation.

While the nature of an evaluation is often to be critical, I note that the Tyner et al (2010) study is carefully written with extensive cautions regarding the uncertainties that necessarily exist in any such modeling study. The authors themselves have gone to some length to describe key parameters and sensitivities and have made the model available to others. This openness greatly facilitates evaluation of the work

A note on the nature of models and sensitivity is also in order. Any economic model that solves for market equilibrium across multiple interlinked markets is a complex system where aggregate responses are emergent properties of the model that depend on many different parameters and structures. In computable general equilibrium models literally everything depends on everything else through the input-output structure and the allocation of limited factor inputs across all sectors—hence they are solved as a simultaneous system. Parameterization and model structures directly involved in the process of interest are the obvious place to look for identifying critical parameters but given the interactive nature of CGE models, other parameters and model structures seemingly unrelated to the process of interest may be important. While sensitivity analysis is very useful in testing a model rarely if ever are sensitivity analyses conducted for every one of hundreds or thousands of parameters. Further, sensitivity analysis appropriately tests one parameter at a time varying it a small amount and calculating the change in the output of interest, leaving others parameters unchanged. The approach is

intended to be a numerical approximation of the total derivative that could be analytically derived for simpler mathematical systems. As such sensitivities are a *local response* holding *all else constant*. The sensitivity of a result to one parameter almost certainly depends on the value of other parameters. In addition the sensitivity result is a linear approximation of the response and will therefore be a poor estimate of the sensitivity of the model for large changes in the parameter to the extent the model is non-linear.

As noted already, the authors of the study have already evaluated many of the obvious sensitivities. My focus will be on some of the broader structural issues that may affect the emissions result. My goal is therefore to identify key processes and reflect more broadly on how these are structured in GTAP-BIO-ADV, comparing them generally to some of the other approaches that have been applied to this issue.

Introduction

The GTAP effort to evaluate land use emissions from biofuels expansion leading up to the development of the GTAP-BIO-ADV is probably the largest of its kind but other models have attempted similar calculations. Among them are the MIT-IGSM/EPPA/TEM (Melillo et al., 2009; Gurgel et al., 2007, Antoine, et al., 2008; Gurgel et al., 2010), FAPRI (Babcock, 2010), FASOM (Schneider and McCarl, 2003), and MINICAM (Wise, et al., 2009) in addition to the early analyses that kicked off concern about indirect emissions of Searchinger et al (2008) and Fargione et al (2008). First, what are some of the broad differences and similarities in these models? The economic model in the MIT IGSM framework is, similar to GTAP-BIO-ADV, a computable general equilibrium model of the entire global economy with a full input-output structure of intermediate demands. MINICAM treats total GDP but energy and agriculture are sectoral models that do not interact through a full I-O structure with the entire economy. FASOM and FAPRI are agricultural market models, providing the most detail on agricultural products and markets but no treatment of the rest of the economy.

FASOM, FAPRI, MINICAM, and MIT-IGSM are all dynamic models designed to step through time, period by period whereas GTAP-BIO-ADV is a static model. The time steps of the models vary from 15 years for MINICAM to 1 year for the FAPRI model. In the Tyner, et al. (2010) study the GTAP-BIO-ADV model is exogenously updated from it base year of 2001 to 2006 and into the future by adjusting parameters to match available data through 2006 and by GDP and population and yields to reflect growth and yield trends. In general, the exogenous scaling approach can for most purposes approximate the endogenous dynamics of the recursive dynamic models. Some of the key endogenous dynamics in these models involve functions that scale up GDP by increasing labor productivity, and that increase other factor productivity (e.g., land, energy).

The FASOM model is unique in that investment decisions in one period depend on prices in future periods whereas the other models are recursive dynamic, solving one period at time so that expectations are said to be “myopic”—agents make investment decisions in period t based on period t prices. In forward looking dynamic models like FASOM all periods of the model must be solved simultaneously. Solution of such models is consistent with agent behavior that is forward-looking with perfect foresight—agents know exactly what will happen in all periods. Expectations about the future are

important for investment decisions such as clearing of land and decisions to plant trees and harvest forests. This structural difference could affect the land use emissions response. On the other hand, tests of a recursive dynamic and fully forward solution of the EPPA component of the MIT IGSM model showed little difference in technology choices in the energy sector between the two solution approaches and this would likely carry-over to the agriculture sector (aggregate welfare results did differ because the forward looking behavior allowed another avenue of adjustment through consumption shifting) (Babiker, et al, 2009). This is a perhaps surprising result because if there is a persistent price trend projected in the future, myopic models would tend to lag in adjusting to this trend, being always behind it whereas forward looking models would anticipate future prices. In the Babiker et al. (2009) comparison the key price with a trend was the CO_2 allowance price. In their simulations they applied an allowance banking assumption, solving the model iteratively to find a solution where the allowance price rose at the discount rate consistent with theory (and the result, as expected, in the forward looking model). Without this iterative solution, the allowance price path would have been different and so would the energy results. Similarly Reilly et al. (2010) in modeling forestry and land use in the EPPA model apply theoretical results on dynamic behavior to approximate dynamic behavior in a recursive solution. Thus, it is possible to at least approximate some elements of a forward-looking response in a recursive dynamic solution.

In general, full-forward looking behavior models are preferred by economists for theoretically consistency. In practice they are computationally demanding, limiting the complexity and detail that can be included, and the assumption of perfect foresight, while avoiding time inconsistency problems, is a poor representation of actual behavior because it doesn't reflect uncertainty that has real effects on investment decisions. They also can be problematic in policy simulations where a policy of limited duration is being considered. Solving the model with the limited duration policy will create a horizon effect—agents "know" the horizon of policy and therefore behave differently during the policy period looking ahead and knowing it will expire. If indeed the policy was going to expire at a fixed horizon that would be a benefit of the forward looking approach. However, often the expectation is that the policy measure will be renewed in some form, but policy makers have only specified the level for a certain period. Current period results can then be highly sensitive to what additional policy is specified beyond the policy horizon. Real agents will have some expectation as to whether the policy will be extended and how, and their investment decisions should reflect that expectation. But given that the modeler does not know what agents expect means the forward-looking results will not reflect actual behavior. These aspects of the forward-looking solution mean that many specifications of the distant future—technology, policy, etc.—will affect simulation results for the near term, requiring much speculation and guesswork.

The other element of dynamics that is not captured in the approach GTAP-BIO-ADV uses to step forward is the dynamics of vintaged capital. In the EPPA model production from vintaged capital is Leontief while production from malleable capital is represented as a CES production function. Only some fraction of capital is vintaged, reflecting the possibility of retrofit. The advantage is that the price elasticity response of a sector is an emergent property of the response of vintaged and malleable capital and

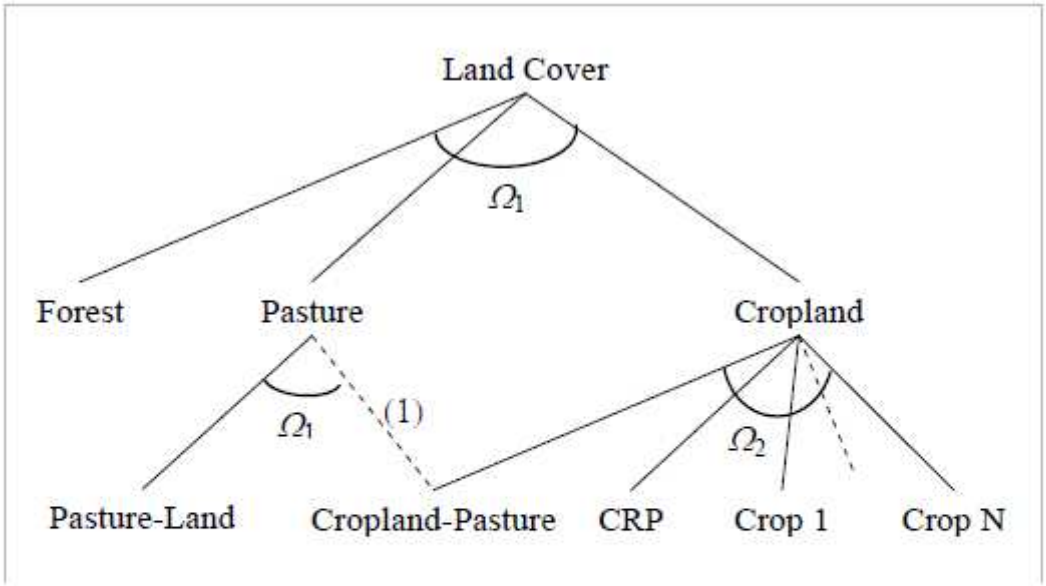
depends how much of each there is which further depends on the level of new investment. Since vintaged capital only gradually depreciates a sector or the economy as a whole will exhibit a short run elasticity response and a long run elasticity response. The short run response reflects the fact that much of the production initially is from vintaged capital that has a zero price elasticity. But, if a new price level remains over time, eventually all of the vintaged capital will be replaced by capital that reflects the substitution possibilities represented by the CES production function. So even if there is no further price increase the model will show continued adjustment over time to the initial price shock. This potentially solves a problem of what to do about elasticity values in a model. If a model without this structure is tuned to behave close to observation in a given year, it will have a short-run elasticity. However, often applications of static models are a comparison of different long run equilibrium states. For this purpose, one would assign long-run values for elasticities but then one would not expect the model to replicate short run behavior. This is a potentially very important concern for land use. Land use change, opening new areas of land, is a slow process. Short-run observations may suggest little response, but if it takes some years to build roads and clear land the short run response may underestimate the ultimate response. With a vintage capital structure as in EPPA, in principle, the model can replicate short term response by adjusting the share of capital that is vintage as well as the long run elasticity in the CES structure.

In following Sections, I address several key issues: (1) How is land supply handled, (2) What is the productivity of new land? (3) How are yields of crops determined? (4) How are the greenhouse gas implications of land conversion addressed? (5) Demand and trade elasticities (6) How might dynamics affect the calculations? (7) Comparing results to actual evidence on land use change. I contrast the GTAP approach with other approaches that are distinctly different.

How is land supply handled?

Many agricultural models have focused more on commodity supply and therefore have focused more on how different agricultural uses compete for exisiting agricultural land. GTAP-BIO-ADV is in that vein but is broader than some models in that it also includes forest and pasture land. The model as revised by Tyner et al. 2010, an ability to use cropland for pasture. The basic approach is, however, a Constant Elasticity of Transformation (CET) function approach with 3 basic land types and a single CET elasticity (Ω_1) governing the tranformation of land from one type to another (Figure 1). This can be contrasted with the approach of Gurgel et al. (2007) and reproduced in Figure 2. The MIT IGSM distinguishes between managed forests (those regularly used for timber harvest) and unmanaged forests as well as pasture/grazing and unmanaged grasslands. It explicitly converts land from one type to another or can abandon production on land to return to a less intense use. In the conversion process inputs are required and so it captures some of the investment issues involved in land conversion, and the investment required is different depending which conversion is being done—Forest to pasture, pasture to crop, etc. This is much more realistic than modeling this as a single elasticity. The MIT-IGSM approach also explicitly considers timber stand on

unmanaged forests. There is a huge difference on the average stock of timber (and thus carbon) between a forest that is in a harvest rotation and a virgin forest. Failure to make these distinctions in the GTAP-BIO-ADV seem like a significant limitation, undermining the carbon accounting. With a single elasticity of substitution governing conversion from forest or pasture to cropland, if there is more demand for cropland for biofuels production, land will be converted from the other two types proportionally to their value



(1) To create this link we introduced an industry into the GTAP framework which uses only cropland-pasture as an input and sells its output (land) to the livestock industry.

Figure 4. Land cover and land use activities in the GTAP-BIO-ADV

Figure 1. Land supply in GTAP-BIO-ADV as reproduced from

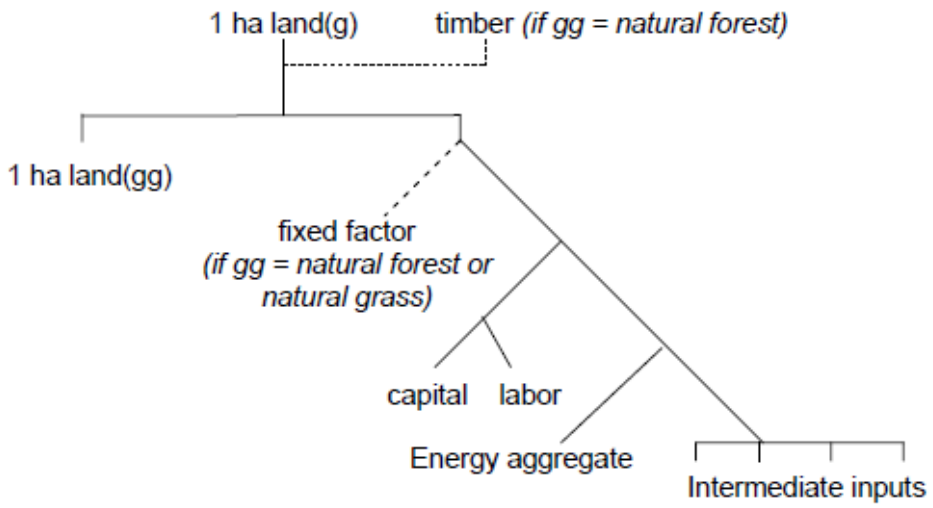


Figure 2. Structure of Land Transformation Functions

Figure 2. Land conversion in the MIT-IGSM, reproduced from Gurgel et al., 2007.

shares. The GTAP-ADV-BIO results tend to confirm this tendency, always generating fairly similar shares of land conversion from forests and pasture. The overly simplistic representation of land supply limits the type of response one might expect, especially over time and with larger diversions of crops to biofuels.

The GTAP-ADV-BIO approach is also not explicitly spatial. It includes land classes by AEZ, where for example, the entire continental US is covered by a half dozen AEZs whereas the MIT IGSM is resolved at 0.5° x 0.5° latitude-longitude resolution or on the order of 4800 separate grids within the US each with unique climate, soils, and land use characteristics. Its hard to know what the implications of these simplifications in the GTAP-ADV-BIO are without further testing and evaluation. Moreover, while in principle the more explicit representation of processes and much greater spatial detail is an apparent advantage in an approach like the MIT IGSM, having that detail means that one needs to accurately project implications of crop diversion for biofuels on land use at this detailed level. So the details may not "improve" predictions. However, if the model structure is fairly inflexible yielding very consistent responses under different conditions that may lead to overconfidence that the results are narrowly bounded by the ranges found in such sensitivities when in fact the apparent robustness is an artifact of the functional forms used in the model.

What is the productivity of new land?

This issue came to the fore in recent revisions of GTAP-ADV-BIO from previous versions of the model. Previous version assumed new land coming in to production was far less productive than existing cropland, applying what would seem to be logical—that farmers would use the best land first and so any still unused land would necessarily be less productive. Of course exactly what that yield penalty might be is critical—is it 5%,

20% or 50%. Almost certainly it should very by region—in regions where agricultural land is fully exploited (India, China) it may be quite reasonable to expect that any unused land is unused for a reason—its not very productive. However, for other regions like Latin America, Africa, and other parts of Southeast Asia the determinants of what land has been converted are often a function of access rather than productivity. The previous GTAP work assumed new land was only about 60% as productive as existing land, and applied this to the whole world. This also seems extreme given that the AEZs are already controlling for climatic factors that would create big productivity differences. Also, one can ask how much natural land characteristics determine productivity and how much is determined by management. The fact that yields of maize have increased 6 times in the US since the 1930's (due to improved technology and management) seems to suggest that management is more important than natural conditions. Moreove, economic incentives would drive expansion to those areas that were more productive, and so the failure to distinguish this factor regionally would be a concern.

In the MIT-IGSM approach, the Terrestrial Ecosystem Model (TEM), is embedded and so the productivity of each parcel being converted is determined for that parcel based on soils, climate and other environmental conditions (as they change from year to year). The GTAP-ADV-BIO adopted results from the TEM model to create a static (unchanging) estimate of the average productivity of new land for each AEZ, and it truncates those estimates by assuming that new land would never be more productive than existing cropland in that AEZ, even though for nearly regions there are at least some grid cells where that was found to be the case, and in several the average of unused land is better than the existing cropland. Thus, even the new GTAP-ADV-BIO approach appears to be very conservative in its assumption about the productivity of new cropland. In general, the new approach based on TEM showed that the yield was only in the 50 to 60% range for a very few AEZs in a very few regions. While it is hard to compute a correctly weighted average given the vastly different areas in different AEZs and different regions, it appears that the yield penalty on average over the world is on the order of 10% rather than 40 to 50% originally assumed, and that includes the truncation effect so that there is never new land that is more productive than existing land. This latter assumption seems extreme because particularly in less developed areas, it is quite clear that road access determines use, not the natural productivity. It also seems more consistent with the fact that large differences in productivity are already controlled for with the AEZ classifcation, and with the fact that management rather than natural conditions (at least within climate boundaries as defined by AEZs) probably are more important in determining yield differences.

The new GTAP-ADV-BIO approach is an improvement. More evidence following other lines of research are needed to investigate this issue. This evidence must be region specific to assure that it takes into account the how intensely land is already utilized in the region. And to be useful to improve the GTAP modeling it must consider the AEZ classifications. Evidence for the US as a whole, where expansion might bring in land in the Great Plains, and show that to be much less productive than average land heavily weighted by productivity corn belt land is simply irrelevant to the GTAP formulation that already controls for those differences via the AEZ classification scheme.

How are yields of crops determined?

There are a couple of issues that are critical in yield assumptions. Then most straightforward is that yields of maize and other crops have increased with some variability but with a clear overall upward trend so that the average US yield has increased more than 6-fold since the 1930's. If that continues, and there is no reason to expect some continued growth of some sort, then as we march ahead it will take every fewer acres to produce a given amount of biofuel because there is a higher crop yield on each acre, and thus proportionally more fuel can be producer per acre. The second important issue is more subtle. That is whether yields are exogenous, unrelated to economic pressure, or wholly or in part endogenous. If the latter, then any diversion of land for biofuels will require new land to make up for the diverted land (except for reduced food demand because of higher prices). If, however, yields respond to economic pressure, then at least some of the crop diverted for biofuels will come from intensification of production on existing land. Here, efforts to statistically relate yields to crop prices typically find a neglible response, and models that are based on separately predictions yield and area have often assumed no endogenous response. The FAPRI model is an example, and it is only recently that they have included a low elasticity< .10. The production function approach of CGE models like MIT-IGSM or GTAP-ADV-BIO has substitution elasticities rather than price elasticities but they are approximately related via after adjusting for the 1-input share. These models often use substitution elasticities of .25 to .3. CARB (2009) has calibrated the substitution elasticities among inputs in the crop production function to match a crop yield elasticity of 0.4 for corn ethanol and 0.25 for sugarcane, based on a literature review of corn yield elasticities by Keeney and Hertel (2008) which found values between 0.22 and 0.76.

If one looks historically, concerns about running out of food and land, have recurred on variable time scale but on roughly periods of 20 or 30 years, with commodity price spikes followed by a huge supply response, collapsing prices, and excess supply. Morever, one can make the argument that if there were no food shortage—a garden eden world where food was there for the taking—there would be no incentive to improve yields—why bother? From that perspective, all yield improvements are driven by economic incentives of some kind—the realization that food is scarce in some sense. Thus, models ought to explain all yield changes as an endogenous process. That does not mean that their are no biological and technical limits to yields but it suggests that our failure to explain yield increase as a fully endogenous process means that we tend to underestimate the potential for intensification in the face of further diversions of crops for biomass.

The evidence, first on the simple story of what are yield trends and what do they mean for the future. There are two schools of thought. The pessimists have pointed to some apparent evidence in yield trends toward slowing yield growth. They see biological limits to yield growth, and believe we are approaching them. Indeed the experience of rapid yield growth is a phenomenon that has only been with us since the 1930's-1940s (Figure 3).

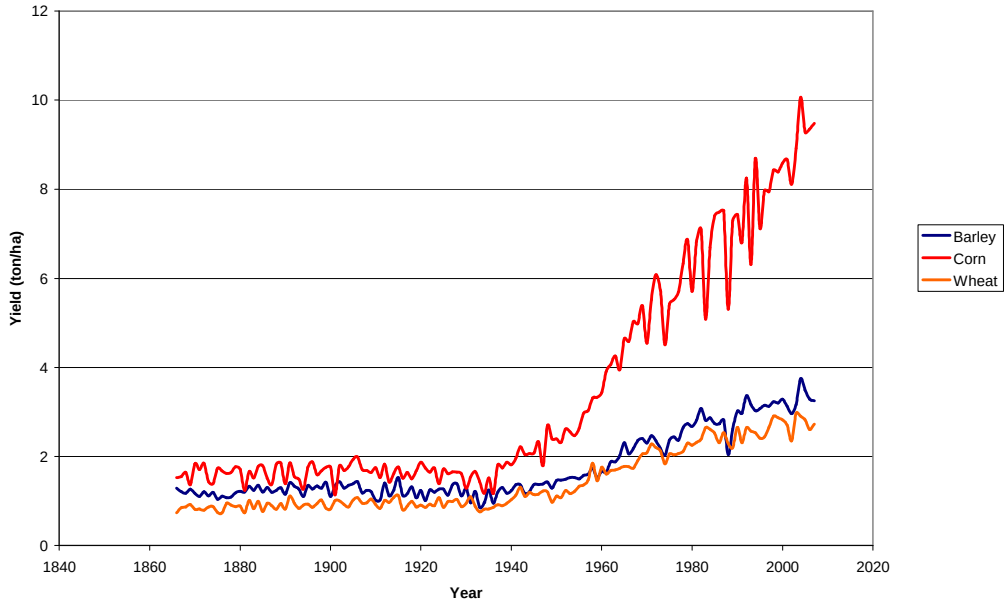


Figure 3: USA corn, wheat and cotton crops yields. Source: 1866-2007 NASS data.

Prior to that period yields were little changes for as far back as the record goes. And observers in the US in the mid-1990s thought they saw a plateau. From figure 3, the period from the late 1980's through 2000 has nearly flat yields for maize, barley, and wheat in the US. However, going past that period we now see with hindsight that yield growth was set to return. Never-the-less yield growth as a relative recent and perhaps temporary phenomenon jumps out in this longer record.

The optimists for yield growht see remaining large differences between best experimental yields, best yields on farms, and average yields as evidence for the fact that management should be able to raise the average much closer to best or experimental yields. And large differences among countries could be eliminated with improved management (with enough economic incentive.) They also point to the still hardly tapped potential of genetic engineering. And, ultimately, a narrow focus on yields of specific crops may be misleading if advancing technology can utilize a wider range of crops with more resilience to a variety of growing conditions that could make feed grain crops obsolete. (The dominance of the corn-soybean cropping pattern is largely a post-war development.) For example, Carolan et al. (2007) and Laser et al. (2009) describe bioprocessing refineries that could produce multiple products including feed grain equivalent livestock feed, ethanol, and other biochemicals from a general biomass stock. Such a process would greatly increase the economic productivity of land by producing valuable feed grain equivalent products, using the entire plant rather than just the grain. It could use a wide range of biomass feed stocks with different climatic tolerances, thus dramatically expanding the climate range suitable for providing bioprocessing feed stocks. The tropics might become major 'feed grain product' (or livestock product) and

fuel suppliers, with corn and soybean production in the United States disappearing and that land used instead to produce grains and other crops for direct human consumption.

In a draft manuscript (Gitiaux, et al., 2010) conduct extensive time series analyses of yields of major crops in all regions of the world. They find that simple time trend observations on yields can often lead to poor future estimates and greater certainty about those estimates than is warranted. The test for a unit root in the time series and conclude that a random walk model better characterizes yield growth. They show that fitting a simple linear model through 1995 and doing out of sample forecasting ahead to 2006, results in estimates that are wrong, and where the error bounds on the estimates do not contain the actual outcome. In contrast the random walk model produces error bounds that contain the observed yield level and different variant of simple time trend models. This is because simple time trend models do not include the possibility of structural breaks that are shown to exist in the data. They develop forecasts ahead to 2010 using all the data through 2006. This shows wide error bounds, and in most cases the error bounds contain simple linear, exponential and logarithmic time trend models. So the conclusion they reach is that we cannot statistically differentiate which of these very different simple models are better predictors even over a period of a decade or so (and the possibility of structure breaks cannot even recommend extending the last few years of yield growth into the near future.)

So literally even the next 5 to 10 years are highly uncertain in terms of yield growth: Neither the pessimists or the optimists can be confidently ruled out. On the question of endogeneity of yields, my sense based on apparent super responsive of agriculture to scarcity fears and price shocks suggests we should model yields as fully endogenous but we don't know what that process is, the time lags involved, nor the ultimate technical and biological limits.

How are the greenhouse gas implications of land conversion addressed?

GTAP-ADV-BIO uses a widely available data set they refer to as the Woods Hole data. The methods of developing these data are documented in a series of papers (Houghton, 1999, 2003; Houghton and Hackler, 2006). These estimates reflect years of painstaking compilation of many inventories and peer review of the methods. However, a limit to this method is the relatively limited spatial detail. They provide only a half dozen or categories of vegetation types and resolve them only for major countries or continental scale areas (Latin America, Africa). Importantly, the data includes information on regrowing areas and carbon uptake on regrowing areas. Thus, they are able to include estimates of foregone carbon sequestration that would have occured on converted land if the forests there had been left undisturbed. The biggest concerns with this approach is (1) its unclear how the GTAP-ADV-BIO marries the ecosystem types in the Woods Hole data with the AEZs in GTAP (2) is the use of averages justfied and (3) the averaging idea does not consider the dynamics of the problem. With regard to (1) there is reason to think that there is some correlation between AEZs and ecosystem types but the correlation is hardly perfect, thus in principle temperate forest in one AEZ may be quite different in terms of carbon stock than a temperate forest in a different AEZ in that region. With regard to (2), lacking spatial detail either in the land projections or in the Woods Hole data one is left will few options but to assume an average. However, one

might argue that either very recently harvested land, or land with significant timber stand so that the forest product value could be recovered would be more than proportionally the target of conversion efforts. Including both actual carbon and foregone sequestration tends to correct for the grossest error this approach would introduce. With regard to (3), since the overall CARB approach takes an antitheoretical approach to dealing with the dynamics of the problem by simple using a 30 year undiscounted horizon, the fact that the forest accounting of dynamics is some average effect with the foregone carbon cut-off at 30 years is at least consistent at some levels with that approach. However, if the average mispredicts the likely stage at which forests will be converted the 30 year cut-off using the average may be inaccurate.

By way of contrast, the MIT-IGSM approach is explicitly spatial and within each grid cell a cohort structure is retained that tracks the unique history of the land going back historically, and tracking the projected land use into the future. Thus, when a particular amount of land in a grid cell is converted the the carbon released from the conversion is based on how long the forest area has been regrowing, and the carbon that might have been sequestered had it not been harversted is estimated in a separate counterfactual run where the conversion did not take place. Moreover the carbon content in each grid cell depends on grid-specific climate and soils as well as the management history. Moreover, both the carbon implications and the productivity of the land are based on the TEM model so that the productivity of the new land is consistent with the carbon dynamics of its conversion. GTAP-ADV-BIO uses completely independent approaches—TEM for productivity, Woods Hole data set for carbon—and so there is no reason to expect consistency. So the MIT-IGSM approach seems far superior conceptually, although as noted previously adding detail adds the burden that the detail needs to be predicted accurately, and there is great uncertainty in any approach that attempts to predict land use at a detailed spatial scale in response to global forcing due to crop diversions for biofuel production.

Demand and trade elasticities

With diversion of cropland for biofuels in a particular region several things can happen. We have already discussed two possibilities: land can be converted to replace the land diverted for biofuel production or higher crop prices can cause farmers to more intensely manage existing land to increase yields. The other response to higher prices is less consumer demand. In addition, a diversion in a particularly country (e.g. the US) may result in changes within country in terms of consumption, land use, and intensification or these shocks can be transmitted abroad where they then affect all these channels of response in other countries. The GTAP group has conducted extensive analysis of demand elasticities and have estimated advance demand systems. My sense is that this is a particular strenght of the GTAP effort, and that it reflects the best available estimates. At the final consumer level, the food demand is a relatively basic demand, and this has been studied in depth. A possible limit however is in the interindustry demands for specific crops. I noted above the potential for eliminating the need for feedgrains. This may be too long term or speculative to consider as realistic or relavant here, however, the long run nature of the climate issue, to which the LCFS is addressed, forces us to

continually think over the longer term, and it becomes hard to escape the implications of changes in the distant future on what we do today. If, for example, this elimination of feedgrains is a possibility at some future where the technology has advanced and commodity price rises from more bifuels have made it economic, then pressure on land created today is only temporary and is eventually relieved once this breakthrough occurs. If it doesn't occur, then the land pressure is permanent.

A more relevant near-term issue are trade elasticities. While there is various econometric evidence for limited willingness to substiture foreign product for domestic, or to change trading partners, I believe those estimates are inherently reflecting short run rigidities. Otherwise, simulating persistently different pressures in different regions of the world (e.g. lots of increase in biofuel production in the US) will tend to create much higher commodity prices in the US then in other parts of the world, and that leaves large opportunities for trading gains. In dynamic models with persistent differences Armington assumptions lead to wide divergences in prices of similar products in different regions. This result seems untenable, and so regardless of the econometric evidence I think one needs to reject the idea of low trade elasticies, and for the long run analysis of climate change and LCFS accept essentially perfect substitutes among similar crops of different origins. This is the approach of the FAPRI model. The MIT IGSM includes Armington agricultural markets but includes perfect substitution in biofuels. From trade theory we know that the existence of some tradeable goods can lead to "factor price equalization" and that is the case in the MIT IGSM with biofuels—the biofuel trade leads to land prices moving in parallel among regions of the world, and this tends to limit price divergences in conventional agricultural markets because trade in biofuels substitutes for trade in traditional commodities. Over 1 to 5 year horizons, lower Armington assumptions probably are more realistic but those horizons are irrelevant for the problem at hand.

How might dynamics affect the calculations?

This is fundamentally a dynamic problem. It begins with the critical assumption in the net carbon accounting, of the time horizon to be considered. Searchinger et al. (2008) introduced the idea of an integration period of 30 years, and that approach has been followed by CARB (CARB, 2009) and EPA (EPA, 2010). CARB assumed that all induced emissions from land use change over the 30 year period should be divided by the carbon savings from biofuels and annualized. They considered two alternative approaches, one discounting emissions by an interest rate to get a net present value of emissions and another which calculates the fuel warming potential of GHG emissions (O'Hare et al., 2009). The legislation uses the 30-year annualized method to define the carbon intensity of alternative fuel pathways. EPA used two approaches with regard to the time horizon issue. One approach evaluates the GHG reductions from biofuels compared to gasoline for a period of 100 years, applying a discount rate of 2% to assess *net present* emissions, and the second applies the 30-year GHG emission reductions rule of the CARB without discounting. The EU uses a 20 year undiscounted time period.

A longer integration period makes biofuels more attractive because there is a longer period to repay the *carbon debt* associated with initial deforestation. However,

discounting reduces the effective quantity the further in the future the savings occur. While discounting is a common approach in economics for comparing across time periods, discounting physical tons has been criticized by many economists. In fact, simple arithmetic shows that it is consistent with economic principles but implies that the price of carbon is constant over time. The standard formula for calculating net present value (NPV) of the value of different quantities (price, P, times quantity at t, Q_t) over time t=0 to T is in the first summation sign, but since P is independent of t by assumption, it can be factored out of the summation and we are left with an expression that "discounts" the physical quantity. If P is going to be established in a LCFS credit market, then you don't need it to make the discount calculation. So if you are willing to assume the price is not changing over time then the EPA calculation is fine.

$$NPV = \sum_{t=0}^{T} \frac{P * Q_t}{(1+r)^t} = P \sum_{t=0}^{T} \frac{Q_t}{(1+r)^t}$$

In the indirect emissions calculation, Q_t is composed of two parts, a land use emission (or uptake) component and a fossil fuel offset component. In the carbon debt characterization of the problem, the land use emissions are a very big negative in period 0 so that the net effect is that $P*Q_0$ is negative. The land emissions are 0 for the rest of the period, and so the positive offset of fossil fuels makes $P*Q_t$ for t>0. Normal discounting gives greater weight to early periods because $(1+r)^t$ is greater one and getting larger with t.

This formulation then raises the question of the appropriate discount rate. Is it a social discount rate reflecting a prescriptive view of intertemporal equity, or descriptive estimate of the rate at which the economy makes such tradeoffs? Some argue that the former should be very low, perhaps zero while the latter is often gauged to be in the range of 3 to 5 percent. Still another view is that this rate should reflect private investor decisions in assets that have some risk. The rate at which companies can finance investment through combined debt-equity financing are often assumed to be on the order of 8-9%.

Other simple assumptions about the time path of P can get other expressions where P factors out of the summation but that have a much different implication (Herzog, et al., 2003). If P grows at the rate of r—as might be expected if a given set of allowances were allocated optimally over time, then the $(1+r)^t$ factor appears in both numerator and denominator and cancels out, P factors out, and you end up with the simple sum over T years. This characterization justifies the simple sum in the CARB approach. However, there is not a particularly compelling reason to choose a particular finite integration period. Some have suggested the period should be the time horizon of the policy, while others have suggested the lifetime of biofuel production facility. But nothing prevents biofuels plant from unexpectedly shutting down because economic conditions change, or of the facility staying open much longer (or a new facility being built to continue to utilize the biomass crop produced on the originally deforested land if the old one is retired). If it is likely that the policy, or something similar, will be extended then the stated policy lifetime may not be relevant. If the biomass facility shuts downs and is not

replaced by another facility, then that pressure on land use goes away, and in principle the land would grow back to a forest. So eventually the carbon debt is repaid by restocking carbon on the land. The CARB approach of not discounting and having a short terminal horizon is inconsistent with this observation. If biofuels production went on forever, then the initial debt would tend toward having no effect on the long run marginal emissions. If the biofuels plant would be abandoned then the forest would return and completely offset the initial carbon debt, and so again it would have no effect. So by this reasoning the indirect emissions should always be zero. With discounting (price not rising at the discount rate) then the fact that things happening in the long term are worth less than those happening closer will mean the flow of emissions savings over time relative to the initial carbon debt.

This is the sense in which Herzog, et al., 2003 referred to value of temporary storage, only in this case it is the cost of having extra CO_2 temporarily in the atmosphere, and during that time biomass energy is offsetting fossil fuel use. They assessed the value of temporary carbon storage, applied to the carbon uptake of ocean, assuming alternative paths for the carbon price and interest rate. Following a similar approach, O'Hare et al. (2009) proposed a cost-benefit accounting to calculate the net present value of biofuels relative to fossil fuel, assuming a linear damage function applied to the cumulative radiative forcing of GHG emissions from fuels. Both approaches follow sound economic construction, similar to those necessary to compare different GHG gases, as shown by Reilly and Richards (1993). However, because these require the controversial choice of a proper discount rate and involve even more complexities and uncertainties that arise in the assessment of how marginal damages would change over time, policy makers have shown little interest in such methods.

A bigger problem with this approach is that even if we could make compelling cases for value of discount rate and marginal damage cost, simply assigning a value is based on a presumption of the continuation of biomass production does nothing to create an incentive to do so. In the Herzog, et al. (2003) case of ocean sequestration, the leakage phenomenon they considered was purely physical, and unaffected by incentives. In land use and biofuels production the decision to continue or not is a choice and if any payment presuming continuation is paid up front it ceases to produce an incentive to carry through once the payment is made.

Reilly et al. (2007) and Reilly and Asadoorian (2007) raised this incentive issue, and described how a calculation like this would produce an asymmetric treatment of biofuels and land use investment decisions compared with an investment in a power plant that face a CO_2 price and where potential investors would need to form their own expectations of the future price. They argued that any CO_2 emitted at the time of its discharge (or uptake) is equal and should face the same CO_2 price, whatever its source. A cap and trade or tax policy which includes emissions and credits for uptake from land-use change, with monitoring of carbon stored in vegetation and soils and enforcement of such policies, would eliminate the need for the integration time accounting. Investors in biofuels and land development would need to develop expectations about the future in the same ways as investors in power plants. Following this logic, Melillo et al. (2009) argued that there should be no integration over time. If large indirect emissions occur in early years those should be fully charged in those years without consideration of potential future offsets of

fuel use. Later production would then not face this charge, and investors would decide if future benefits justified the initial large penalty. This approach would monetize the carbon debt. It avoids the problem of the implementing agency having choose an integration period or a proper discount rate. Those decisions would be left to market, as they are in all other private investment decisions. It would likely mean that initially biofuels would be far worse than gasoline and they would face a penalty in an LCFS, and they would only be used if there were strong expectations that in future periods the penalty would go away because no further expansion of biofuels was occurring and so there need be no further deforestation to accommodate expansion.

In this context, the underling dynamics of the calculation pursued by CARB are hard to justify and so its not clear that correcting the dynamics of the estimation of emissions is an improvment or not. The GTAP Type III calculations that attempt to correct for yield growth move us closer to the right answer for the 2013, 14, or 15, but the relationship of that number to the correct number to be used in the calculation at hand is unclear to me.

Comparing results to actual evidence on land use change

Model simulations offer a weak foundation for the quantitative requirements of the LCFS, yet ignoring indirect emissions completely also seems a mistake. The question arises: Is it possible to ground-truth model-based projections with actual land use changes due to an expansion of biofuels? Or are their data assimilation approaches that optimize model estimates to fit the data, and if so how what would that involve? For emissions involved directly in the production process it is possible, at least in principle, to follow the production chain and measure emissions at each point, although in practice it may be difficult to track all inputs and to make actual measurements. Emissions at the ethanol production plant can be measured but going back further in the process, emissions from transporting the biomass to the plant may be harder to measure directly if the exact origin of the grain is not known, and that would complicate estimating the specific emissions on the farm growing the biomass, etc. However, at least direct measurement of a representative example of each process is possible. For emissions due to indirect land use change, in any year we may observe changes in land use and changes in fertilizer use (and hence N_2O emissions) but many other forces are contributing to those changes. If one could assemble an estimate of the changes in the share of different land uses, and assemble an estimate of the changes in carbon stocks on land could one then attribute, based on the carbon stock of land in different uses, the contribution of each change to carbon emissions (or uptake)? This is the potential promise of inverse methods that would use satellite and in-situ measurements of concentrations of greenhouse gases in the atmosphere and of land use change, and through assimilation of weather data would detect where the gases must have been emitted. The approach needs to be comprehensive and provide further data on the activities contributing to emissions otherwise it would not obviously distinguish between fossil fuel emissions and land use emissions. At present

this is still a far-off possibility since the measurements of concentrations are not accurate enough to resolve well net emissions. If it is to attribute land use emissions to human activity it would also need to identify separately changes due to natural causes.

Other more conventional statistical approaches can be applied. It is unlikely these can be comprehensive, but they can evaluate and attempt to statistically test some assumptions of the models. The testing for effects of expansion of growing area on yields in Brazil as discussed above is one example. Efforts to tune models to replicate observations are an ad hoc way of fitting the model parameters to observation, and such efforts have been made with at least the GTAP and FAPRI models. However, with many parameters there are many ways to adjust the model to match observation. Perhaps the more problematic aspects of use of data are the dynamic aspects of the problem as discussed briefly in Section III. Agricultural commodity markets go through periods of relative slack and tightness. During slack periods, some cropland may be fallow and other used less intensively. Additions of biofuels production during such a period may be possible with little expansion of cropland. But as demand for food recovers and grows the land that was fallow or used less intensively would no longer be available for food production adding pressure for land conversion. If biofuels production can swing in and out, making use of slack resources when they are available, then attributing little emissions to them may be accurate. But if production persists through tight periods then ti could be argued that land conversions in those periods are attributable to previously expanded biofuels production. A simple annual accounting would not capture this dynamic effect. More thought is needed on how such attribution of short term changes are consistent with long term responses. However, at present the only feasible approach appears to be the use models.

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