

# Biofuels Policy and the Empirical Inputs to GTAP Models

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## 1 Introduction

This report evaluates the GTAP models being used by the California Air Resources Board (CARB) to calculate the total carbon effects of biofuel use. It focuses primarily on the quality of the empirical inputs to the model but also considers the important relationship between “carbon accounting” and reduced food consumption.

The GTAP model, a “Computable General Equilibrium” (CGE) model, is a particular version of a long-used method for predicting policy outcomes. The advantages of CGE models include an ability to model a relatively rich array of outcomes in a manner that is consistent with economic theory. The model produces outcomes that are consistent with assumed resource constraints, consumer preferences and production possibilities.

A problem with the GTAP model (and with complex CGE models generally) is that they require the researcher to input a large list of parameters that define the equations of the model. The more detailed are the results desired by policy-makers, the more empirical parameters that must be input. Different parameters will give very different results and, as usual, the principle of “garbage in, garbage out” applies here. If the input parameters do not reflect the real world, then neither will the policy predictions of the model.

In an ideal world, empirical economics research would provide GTAP modelers with convincing estimates of the needed parameters. Unfortunately, the empirical literature on many of the necessary parameters is weak or even non-existent. Worse, though, the GTAP results provided to CARB sometimes rely on a misleading reading of the empirical literature that does exist. These problems with empirical inputs call into question the GTAP results provided to CARB.

The most obvious first-order issue regarding empirical inputs concerns estimates of the yield price elasticity. The current GTAP runs make use of yield elasticity estimates that are very difficult to justify on the basis of the existing scientific literature. Indeed, they appear to be based on an *a priori* belief that yield elasticities should be set to a fairly high level (relative to other elasticities) regardless of the actual findings of the empirical literature.

A review of the literature, together with a simple econometric examination of data on US corn, supports a yield-price elasticity estimate no higher than approximately 0.1. This is much lower than the 0.25 elasticity used in the current GTAP / CARB runs. Government policy should not be made on the basis of elasticity estimates that are contradicted by most of the existing empirical literature. The GTAP results can easily and quickly be re-run using yield elasticity estimates that are supported by the empirical literature.

A second issue with the GTAP / CARB results is that the GTAP model is asked to provide “carbon accounting” without regard to a number of potentially important non-carbon related issues. The most important of these issues is the diversion of human food to energy production. In many CGE model runs, including GTAP runs, a large portion of crops used in biofuels are predicted to be crops diverted from human consumption.

There is perhaps little cause to worry about the effects of “diverted” food on the population of the developed world. To the degree that there is a high food-based demand elasticity for bio-fuel crops, it must be coming largely from poor countries (although there is likely also an effect via the production and consumption of meat.) The diversion of food from the world’s poorest families raises an important social welfare question. Is it proper to treat reduced human food consumption (and the associated decline in carbon “emitted” by human breath and so forth) as a pure social gain? The GTAP models are being asked by CARB to focus only on “carbon accounting,” but an appropriate social accounting would take steps to recognize effects on food-poor consumers in the third world.

In addition to issues of yield elasticity and the social cost of reduced food

consumption, there are important questions about GTAP land-use elasticities, about the economic (vs. biological) yield potential of new land, about the lack of “unmanaged forest” in the GTAP land model, about the out-of-date GTAP “Armington” trade model and about the empirical source for consumption elasticities. Similarly, there are legitimate questions about the role of the “CET” (and other) functional forms used in the GTAP analysis. These are all worthy of considerable further study. However, “quick fixes” do not appear to be available for these problems and so they should likely be treated as questions for on-going evaluation.

It is important to note that both of the major issues discussed – incorrect yield price elasticities and ignoring the social cost of increased food prices – bias downwards the current GTAP / CARB estimates of the indirect land use cost of biofuels. Correcting these problems would increase the estimated carbon cost of biofuels. Several of the other problems discussed (such as the lack of an economic model of land use) point in the same direction, while some other problems that are discussed (*e.g.* Armington elasticities) do not have an obvious direction of bias. Overall, this raises a concern that the current GTAP estimates understate, perhaps substantially, the indirect land use cost of biofuels.

## 2 Economics of Bio-fuel Carbon Policy

Possible carbon gains from bio-fuel use come from some combination of increased production of crops and/or reduced human consumption of crops.<sup>1</sup> The increased production of crops can result from increased land-use (acreage used for agricultural production) and/or increased yields (output per acre.) As noted in Searchinger et al (2008) and other works, the final tally of the carbon accounting depends critically on the mix of sources (demand, yield, land) for the bio-fuel crops

Note that *all* of these effects are *price-mediated* (“indirect”) via the world market for food and fuels. To judge them, we have no choice but to consider “supply and demand” models of world food markets in equilibrium. In the present case, the GTAP model is providing the supply and demand framework. In addition to the underlying logic of economic equilibrium, model results will be driven by [i] pure modeling choices (i.e. the assumed func-

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<sup>1</sup>See Searchinger (2010).

tional forms for various relationships) and by [ii] various assumed measures of the price responsiveness (“price elasticities”) of food demand and supply.

A simple biofuel mandate raises the demand for bio-fuel crops.<sup>2</sup> In the simple undergraduate “supply and demand” framework, the mandate therefore raises the price of relevant crops. Important lessons of basic microeconomics are that

- price goes up more as demand and supply are (each) *less price elastic* and
- how much of the mandate is provided by *more supply* vs. *less demand* is determined by the *relative price elasticities* of demand and supply, with the relatively more elastic side accounting for more of the crops used for bio-fuels.

On the supply side, the overall elasticity of supply is the sum of the price-elasticity of land-use and the price-elasticity of yield. From this, we know that

- the degree to which any supply increase is provided by *increased land-use* versus *increased yield* is determined by the *relative* price-elasticities of farm-land and yield.

An important lesson from economics, then, is that the predicted mix of sources depends critically on the *relative* elasticities. A high yield price-elasticity mixed with a much higher land-use price-elasticity will imply a large incremental use of land, whereas the same yield-elasticity mixed with a low land-use elasticity will imply relatively little incremental land-use.

The simplest carbon-accounting supply and demand model for biofuels therefore requires at least three separate elasticities for demand, land and yield. For example, Roberts and Schlenker (2009) and Roberts and Schlenker (2010) attempt to provide credible empirical estimates of supply and demand elasticities in the world market for “food commodity calories.” Roberts and Schlenker (2010) finds a supply elasticity ranging from 0.08 to 0.13 and a demand elasticity ranging from -0.05 to -0.08. Roughly speaking, then, about one-third of the response to a price increase would come from demand, with the rest coming from supply. Roberts and Schlenker (2010) argue that the

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<sup>2</sup>The proposed CARB policy is much more complicated than this and will interact in complicated ways with other national and international policies.

empirical evidence is not at all consistent with yields that respond greatly to price at the country level (see the discussion below.) Therefore, their predicted supply response comes entirely from land-use change (although predicted world-aggregate yield does change with changes in the cross-country composition of agricultural land use.)

The GTAP approach is much more theoretically sophisticated in that it separately models more crops and crop-locations while also considering intermediate inputs and the explicit effects of international trade. If done correctly, this would allow much more precise economic and carbon accounting. However, there is an important question as to where all of the necessary elasticities come from. Unfortunately, in many cases the GTAP elasticities are not justified by economic research and therefore the apparent increased detail considered by the GTAP model may come at the cost of unrealistic predictions.

## 2.1 How to Learn About Elasticities from Market Data

One of the central insights of 20<sup>th</sup> century econometrics is how to estimate market primitives (like supply and demand) from market-generated data. The literature realizes that price can be high either because demand is high or because supply is low. Quantities and prices are determined “simultaneously” in equilibrium and it is not proper to think of one as “causing” the other.

Similarly, agricultural land-use in some regions or time-period can be high because land is very productive or because there is simply a high demand for agricultural products. Yields may be high because of good weather, or because high expected prices have induced farmers to use more fertilizer. All of the relevant observed outcomes – output, yield, land, price – are simultaneously determined in market equilibrium.

The econometric literature makes clear that in order to estimate demand, we need an observed variable that shifts supply, but not demand. Further, this supply shifter cannot be correlated with the unobserved determinants of demand. That is, it cannot be correlated with the demand “shock” in each period. Such a supply shifter is an example of an “instrumental variable” or “natural experiment” that moves supply and statistically “traces out” demand.<sup>3</sup> The proper instrument has to be both *exogenous* (not deter-

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<sup>3</sup>For an relatively informal discussion, see Angrist and Krueger (2001). For a formal discussion, see any standard econometrics textbook.

mined within the system and plausibly uncorrelated with unobserved demand shocks) and *excluded* from the demand relationship itself.

The classic supply shifting instrumental variable, dating back to the very first application of instrumental variables regression in Wright (1928), is weather, often proxied by yield. Weather is assumed not to effect the demand curve and is quite clearly exogenously determined. Indeed, this is the same supply shifter used in Roberts and Schlenker (2010).<sup>4</sup>

On the other side of the market, to estimate supply we need an observed variable that shifts demand, but not supply (and that is not systematically correlated with the unobserved “supply shock” in each period / region.) In agricultural economics, it has apparently been harder to think of an appropriate demand shifter to trace out supply. Roberts and Schlenker (2010) make use of *past* weather shocks, which create changes in current inventories and therefore change demand for current production (since current demand can also be met from inventories.) This is a clever argument and seems to work well.

Ignoring the instrumental variables (IV) literature and making use of simple correlation (or “least squares” or “linear regression”) techniques leads to incorrect and misleading estimates. Unfortunately, the GTAP model (and CGE literature more generally) is filled with examples of elasticities that are estimated via simple (and incorrect) least squares techniques.

To understand the potential problem, suppose that we use least squares to “fit” quantities to prices. Is the resulting fitted line a “demand curve” or a “supply curve”? Prices in the data are typically responding to a mix of supply and demand effects, so the least squares results are typically a confused mess of demand and supply effects, neither supply nor demand. (See Working (1927).) Similarly, least-squares estimates of yield “fit” to price via least-squares do not reveal the yield price-elasticity.

These problems were first discovered in the context of empirical agricultural studies nearly a century ago (Wright (1915)), leading directly to the creation of the instrumental variables (IV) techniques that can solve them. It is most unfortunate, and potentially quite misleading, when the GTAP models rely on incorrect least-squares results instead of on correctly estimated IV results.

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<sup>4</sup>The use of yield as a proxy for weather is not as obviously correct if yield itself responds to prices. Roberts and Schlenker address this primarily by testing whether yield appears to move with prices (and rejecting the relationship) and by doing a robustness check what makes use of a limited amount of direct data on weather.

Any serious empirical work on the question of estimating production relationships has to begin with the question of how to deal with the problem of simultaneously determined demand and production choices. We need to know: what is the “exogenous” change in demand that potentially traces out supply? Without such an instrumental variable, any empirical result is suspect.

Since Roberts and Schlenker (2009) and (2010) make standard and sensible use of instrumental variables, asking for IV results is not an unreasonable standard.

For an example of a recent paper that makes a particularly bad hash of the difference between demand and supply, see Lywood, Pinkney, and Cockerill (2009). This paper runs bivariate least-squares regressions (for example, yield on price) and gives a naive causal “supply-side” interpretation to the results.

**Long run vs. Short Run** An additional issue concerns *short-run* versus *long-run* elasticities. Typically, it is easier to estimate short-run elasticities, in part because it is easier to make statistical use of variables with a high degree of variation across time. But, since a bio-fuel policy is expected to be in effect for a reasonably long period of time, we would prefer to deal with long-run elasticities when possible.

The long and short-run distinction is particularly important for land-use elasticities. It is costly to transform land from one use to another and so land is likely to be put into a new use only if an economic change is likely to persist. The slow-moving nature of land-use change has been emphasized since the seminal work of Nerlove (1956).

To estimate land-use elasticities, it is likely preferable to look at studies that make use of persistent changes in the economic returns to land-use – for example, differences in transportation cost in a geographical cross-section. These long-run differences are likely to trace out something much closer to the long-run land price-elasticity.

To summarize the lessons of econometrics:

1. Price, quantity, yield and land-use are jointly and *simultaneously* determined in equilibrium.
  - Therefore, correlations or traditional least-squares regression analysis of, e.g., price and yield reveal correlation, *but nothing about causation*.

- Classic “instrumental variables” (IV) solutions to this problem instruct us to study supply via plausibly *exogenous changes in demand* that can trace-out causal supply relationships.
2. Different empirical approaches will identify short vs. long-run elasticities. For bio-fuels, we want the long-run elasticity and this is especially important for land-use elasticities.
  3. Too many studies used as the basis for parameter inputs to the GTAP model ignore simultaneity and many focus only on short-run elasticities

### 3 Empirical Yield Estimates

There is a long tradition in agricultural economics (dating back to Wright (1928) and Nerlove (1956)) that takes as obvious the notion that almost all of the price-elasticity of supply comes from land-use rather than yield. In these cases, changes in yields are treated as determined by technological change (in the long run) and by weather (in the short run.)

However, there is a literature that attempts to estimate the price-elasticity of yield. Unfortunately, most of this literature is quite bad, making use of OLS regression fits and ignoring the classic results of the IV literature just described. The classic “simultaneous supply” bias is in the direction of finding supply that is *too inelastic*, or even having the wrong (negative) sign. Sometimes price is high because of a bad supply shock (bad weather) and this creates a possible (spurious) negative correlation between yields and prices. However, many studies try to get around the joint effect of weather on prices and yields by using an time-lagged “expected price” (often a futures prices measured before planting), and in this case the direction of the bias is not as obvious, especially if there is more than one unmeasured confounding factor. For example: if demand from China happens to grow when technology is improving faster than trend (and vice-versa), we will find a positive yield-price correlation that does not reflect causation from price to yield.

The yield elasticity number used in the current GTAP results is 0.25. This number is quite high relative to the existing empirical literature. Note that it is much higher than the *total supply elasticity* estimated by Roberts and Schlenker using plausibly correct IV methods.

Roberts and Schlenker (2010) employ the following empirical test for a positive yield price-elasticity. They note that crop prices (and crop futures



prices) are highly serially correlated over time. If yields are driven to a noticeable degree by prices, then yields are also likely to be serially correlated. Weather, however, is not noticeably serially correlated. Roberts and Schlenker test the hypothesis that yields are themselves serially correlated. Their statistical test rejects the hypotheses of serial correlation in yields, which is consistent with yields driven by weather but not by price.

**US Time Series Evidence** Turning away from the recent data and relative (i.e. standard) econometric sophistication of Roberts and Schlenker, Keeney and Hertel (2009) provide a summary of least-squares time-series estimates of US corn yield-price elasticities. The Keeney and Hertel (2009) summary provides the cited intellectual basis for the 0.25 yield elasticity estimate used in the recent GTAP models. Unfortunately, this stated intellectual basis is clearly contradicted by the actual written statements of the authors of the cited papers.

Table 1 of this report gives information on the US time-series studies reported in Keeney and Hertel (2009) (and also on one further US study that was reported in working paper versions of the Keeney and Hertel paper.) The studies in the table are Houck and Gallagher (1976), Menz and Pardey (1983), Choi and Helmberger (1993) and Kaufmann and Snell (1997). The table is modeled after the corresponding table in Keeney and Hertel (2009).

The first column of Table 1 gives the authors' names and the second column gives the time period for the data used in the study. The third column gives the Keeney-Hertel ("KH") reported elasticity. The last column summarizes the differences between the Keeney and Hertel discussion and the actual conclusions / comments of the authors in question. Houck and Gallagher (1976) report a range of least-squares elasticity estimates on what is now quite old data. This paper's conclusions are accurately reflected in the KH discussion.

The whole point of the Menz and Pardey (1983) paper is to call Houck and Gallagher's results into question when applied to more recent periods. While they can replicate the Houck and Gallagher findings on older data, they emphasize agronomic evidence that the marginal productivity of fertilizer, at currently employed high levels, is very low and that further yield gains in response to price are not at all likely. That is, Menz and Pardey in fact argue on the basis of agronomic studies and on the analysis of then-recent data that yields do not respond much to price. The discussion in Keeney

and Hertel does not reflect this.

Using more recent data Menz and Pardey actually find a *negative*, although statistically insignificant effect of price on yield.

Choi and Helmberger (1993) are clearly sympathetic to the possibility of a positive yield price-elasticity and seem somewhat frustrated that the idea has little support in the literature. As in other papers, they find that fertilizer use does move with prices. However, the fact is that they *conclude that they have no evidence that yields move with prices*. Their own summary is: “yields are found to be quite insensitive to price.” Indeed, once they control for technological improvement via a time-trend, the yield-price correlation is *negative*. It is not encouraging that Keeney and Hertel record this paper as reporting a 0.27 yield-price elasticity, which is actually reported by the authors themselves as the combined effect of technology and price. It is completely non-standard, actually shocking, to simply assume that *improved technology has no effect on yields*, but that is what Keeney and Hertel do in this case.

Kaufman and Snell (1997) is the most recent paper in Table 1. It was reported in working paper versions of Keeney and Hertel, but dropped from the published version. Kaufman and Snell report an estimated yield elasticity of 0.02 “at the sample mean.”

The US least-squares time-series evidence (such as it is) therefore supports the opposite conclusion from what Keeney and Hertel report. Except for the very old data of Houck and Gallagher, which Menz and Pardey believe reflects a period of lower (but growing) average fertilizer use, the cited papers all point to no evidence of any substantial yield-price elasticity.

**Agronomic Evidence** The Menz and Pardey agronomic argument is supported by other agronomic evidence for the US. Cerrato and Blackmer (1990) is the highest cited paper on Google Scholar for the searches “corn yield response,” “corn yield fertilizer” and several other similar searches. Figure 1 reproduces a table from that paper that fits various functional forms to purely experimental data on the response of corn yields to fertilizer. One point of the paper is that there is plausibly a plateau above which increased fertilizer use has no further effect. Another point is that average US fertilizer use is at or near the plateau level, so that the marginal productivity of fertilizer is likely frequently near zero in actual use. One possibility, consistent with Cerrato and Blackmer’s agronomic evidence, is that the marginal pro-

Table 1: US Time Series / Least Squares Corn Yield Elasticities: Comparison of Keeney-Hertel 2009 Parameter Reports to Authors' Comments

Paper	Time		KH Reported	
	Period	Elasticity	Authors' Report	Comments
Houck and Gallagher	1951-1971	0.24-0.76	same	
Menz and Pardey	1951-1971	0.61	same, replication of Houck Gallagher	
Menz and Pardey	1972-1980	*	statistically insignificant and <i>negative</i>	
Choi and Helmerber	1964-1988	0.27		0.27 is "upper bound" that includes effect of both technology and price; estimate with control for technology is <i>negative</i> ; overall conclusion is that "yields are found to be quite insensitive to price."
Kaufmann and Snell	1969-1987	*		input (not yield) elasticity; range of 0.0002 to 0.65; elasticity is 0.02 "at the sample mean"

\* Not Reported in KH 2009. The KH table also cites one international study.

ductivity of fertilizer is declining quite rapidly near the theoretical optimum level. At such a point, fertilizing a little more has no bad consequence, but fertilizing too little could have large negative consequences. This may lead farmers to error on the side of adding additional fertilizer, with the outcome that changes in fertilizer use have little impact at the margin. It is possible (but not obvious) that similar arguments could apply to other variable inputs, like farm labor.

Figure 1: Cerrato-Blackmer Fits to Yield Experiments

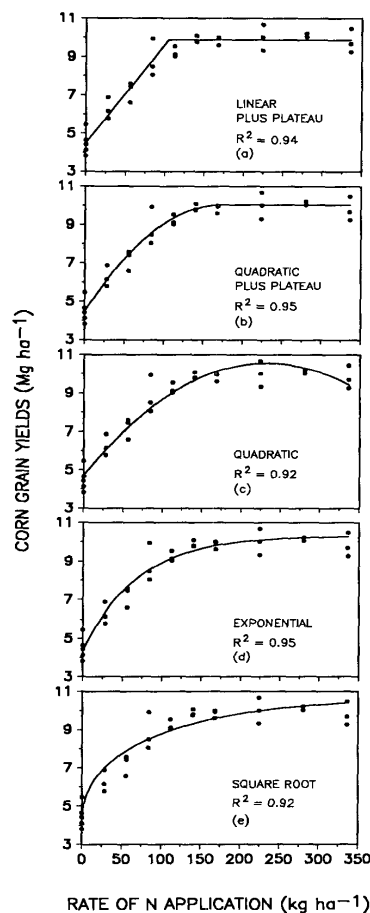


Fig. 1. Example of how each of the five models fits the response data for one site-year (Site 5 in 1986).

**Double Cropping** Arguments about price-induced fertilizer use have a long history. Recently, Babcock and Carriquiry (2010) have raised the issue of price-induced double-cropping. In research “supported in part by the National Biodiesel Board,” the authors do not present formal statistical or econometric evidence about double-cropping, but rather discuss anecdotes about recent double-cropping experience. The number of data points presented is too small to make any robust statistical inference.

While Babcock and Carriquiry do not argue for a large price-induced effect of double-cropping in the US, they argue for a large effect in Brazil. The Brazilian double-cropping data is given in Table 18 of their paper. That table shows the share of Brazilian double-cropped land declining from about 0.19 in 2003 to about 0.15 in 2006. There is then a single dramatic increase, from 0.15 to 0.22 in 2007. In 2008 the share increases slightly and then falls by the same small amount in 2009.

The Babcock and Carriquiry calculation is only sketched (and not fully explained) but it appears to be primarily driven by that very large increase from 2006 to 2007.<sup>5</sup> USDA data shows that the large run-up in soybean prices occurred from the 2006/2007 market year to the 2007/2008 market year, arguably at least partly *after* the large reported (Table 18) increase in double-cropping. At the least, for the Babcock and Carriquiry result to be at all plausible, the authors would need to detail the exact timing of the actual price increases as compared to the timing of planting decisions in Brazil.

In addition, if the profitability of double-cropping increases as prices increase, then this will also have an effect on the demand for farm land in Brazil. New land can be double-cropped as well, which increases the profitability of transforming forest to cropland (for related points see Feng and Babcock (2008).)

More fundamentally, though, the Babcock and Carriquiry anecdote is about one price increase in one country for one crop. It is an anecdote that *suggests* that at least sometimes prices can effect yields. Because it applies to one year for one crop in one country, even Babcock and Carriquiry note that it in no way supports the 0.25 elasticity number applied in all years to all crops in all countries. At best, this is a Brazil specific soybean effect. At worst, it is poorly explained, cherry-picked anecdote.

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<sup>5</sup>There are a number of oddities in the discussion in the paper. For example, the text says that there was a 150% increase in double cropping from the 2004-2006 period to the 2007-2009 period. However, the table shows an increase from about 0.15 to only about 0.24. Overall, it is nearly impossible to make out the details of the argument.

**Farm Level Studies** Turning away from time-series (and anecdotal) evidence, there are yield studies at the level of the individual farm. These studies have the advantage of controlling for otherwise unobserved qualities of the soil, for the talent of the farmer and for variations in local knowledge or practice. Hertel, Stiegert, and Vroomen (1996) summarize these studies as finding little or no response to prices. Hertel, Stiegert, and Vroomen (1996) attempt to “reconcile” the supposedly positive yield elasticity findings of the time-series literature with the lack of such findings at the farm level. However, as we have seen, the time-series literature itself provides little evidence of a positive yield elasticity.

Hertel, Stiegert, and Vroomen (1996) correctly note a role for heterogeneity in farmers productivity and talent. They propose that higher prices may lead to more productive farmers taking over the land of less talented farmers, generated a yield increase even though the “farmer-specific” yield remains constant. This is an interesting theory, not backed up by direct evidence. The overall thrust of the productivity literature is that the marginal firm (on the border of exit and entry) is typically not very productive. An alternative hypothesis is that, on the extensive margin of exit / entry, rising prices draw less productive farmers into the market. The balance of these two possible factors (faster growth by productive farmers on the “intensive” margin versus less productive farmers / farms entering on the extensive margin) is simply not known.

**County Level Panel Data** More recently, Huang and Khanna (2010) use data at an intermediate level of aggregation, between the level of the country (or world) and the level of the individual farm. They use “modern” panel-data techniques to study US county-specific data for 1977-2007. They find a yield-price elasticity of 0.15 for corn, 0.06 for soybeans and 0.43 for wheat. Note that the value for corn is 40% lower than the current GTAP estimate and the value for soybeans is lower yet, while the value for wheat is quite high. The Huang and Khanna estimates are only in working paper form and the “Arellano-Bond” empirical technique that they use is somewhat difficult to relate to standard instrumental variables arguments and implicitly relies on timing assumptions about when productivity shocks are observed and when variable inputs are chosen (see Akerberg, Caves, and Frazer (2006).) It would be easier to evaluate the Huang and Khanna work if in future versions the authors can describe what is the assumed nature of

the unobserved shock to yield and land use – *i.e.* is the unobservable shock just serially uncorrelated weather, or can there be serially correlated shocks from technology or regulation?

If the Huang and Khanna yield result is taken seriously (and it is, frankly, one of the few “modern” studies) then their land elasticities should also be taken seriously. They find own-price land elasticities of 0.51, 0.49 and 0.07 for corn, soybeans and wheat, respectively. However, some of this elasticity comes from cross-crop substitution. The composite land elasticity, with respect to an overall crop-price index, is 0.257. Unfortunately, the GTAP model does not make it easy to compare these straight-forward numbers to the highly implicit land-elasticity assumptions in the GTAP model.

Taken together, the Huang and Khanna results indicate that a large fraction of the price elasticity of corn supply comes from land (not yield) and almost all of the incremental soybean supply comes from land (not yield.) The opposite is found to be true for wheat.

### 3.1 Evidence from Recent Data

Much of the discussion of yield price elasticities is based on quite old data. (Exceptions include Roberts and Schlenker (2010) and Huang and Khanna (2010).) To address this problem, and also to illustrate the issues involved, I downloaded data from the USDA on U.S. corn yields and prices.

Figure 2 shows a long time series of US corn yields, together with a flexible time trend, while Figure 3 displays similar data for inflation-adjusted corn prices. Note that yields trend steadily upward, with sharp downward spikes that are consistent with bad weather in a particular year. There is perhaps a slowdown in yield growth in the middle of the data, that may or may not have recently reversed.

Prices follow a more complicated pattern. Real prices tend to fall over time, with periods of strong upward movement. This is consistent with a causal story in which technology increases yields over time, which in turn reduces prices. Surely, this is the primary story about yield and price.

Figure 4 shows a scatterplot of the log yield and price data. The strongly downward sloping line is the OLS “best fit” line. To the degree that some researchers want to defend OLS estimates of elasticities, they should be prepared to confront this strongly downward sloping line, which if interpreted as a yield-price elasticity would imply a *negative* effect of price on yield (consistent, for example, with Menz and Pardee’s result on their later data period.)

Figure 2: Log US Corn Yield and Trend

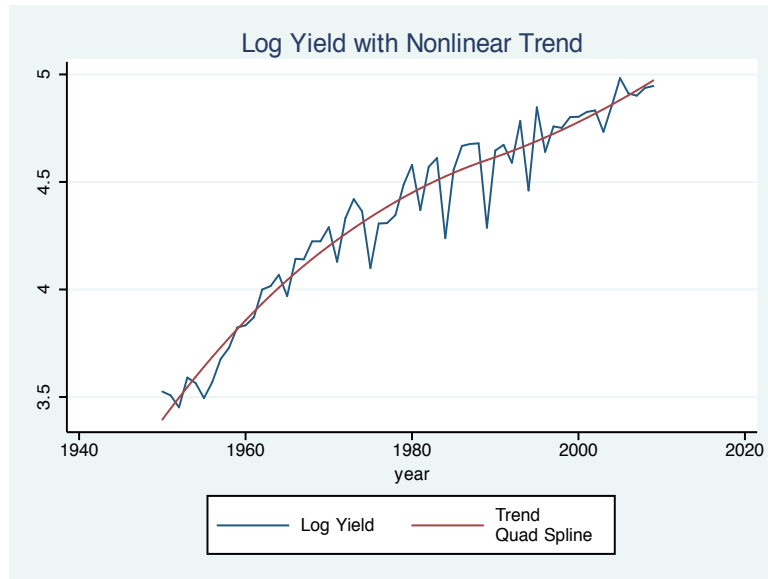
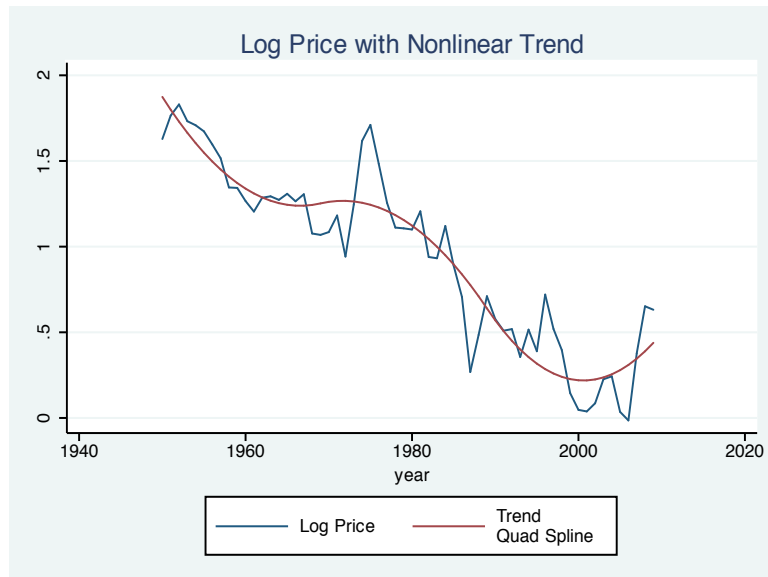


Figure 3: Log Real US Corn Price and Trend





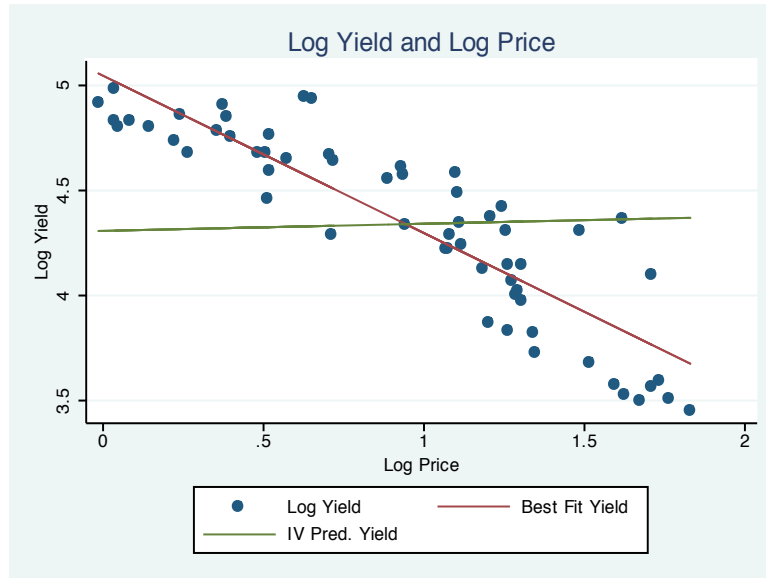
The slope of the relatively flat line represents a simple instrumental variables estimate of the yield-price elasticity. This is from an IV regression of log-yield on log-price, controlling for a linear time trend. The instrument for log-price is just lagged (by one year) log-price, which is a valid instrumental variable if (and only if) the only unobservable source of variation in yields (in addition to price) is a serially uncorrelated shock, like weather.

The estimated elasticity from this IV regression is 0.03, with a standard error of 0.11. This is one standard error lower than the Huang and Khanna value of 0.15 and two standard errors away from 0.25. The hypothesis of a 0.25 yield elasticity is therefore rejected by this model at a 5% significance level.

Allowing for a more complicated non-linear time trend results in a slightly negative yield elasticity estimate.

The result illustrated in Figure 4 is quite simple, but it has the advantage of using recent data and of relying on a very simple identifying assumption (that the unobserved shock to yields is serially uncorrelated, while farm prices themselves are serially correlated.)

Figure 4: Log Yield and Log Price, with Fitted Values from OLS and IV



## 3.2 Conclusion on Yields

In summary, then, the great bulk of empirical evidence points to yield price-elasticities that are close to zero and not close to the values used in recent versions of the GTAP. Further and better research on yields could change this view, but in the meantime it is not responsible to simply assume a high value of for the yield elasticity relative to the assumed values for land elasticities.

Do recall once again that it is *relative* elasticities that matter. Speculations about why yield elasticities might be higher than found in actual empirical work could and should be matched by speculations about why land-use elasticities might be higher. Technological change and directed research can make previously marginal land into prime agricultural acreage. This appears to have happened in Brazil, where much land used for soybeans was previously thought to be uneconomic. The short-run / long-run distinction is even more important for land than for yield. A long-run increase in prices may spur political and social investments that are complementary to land use. As examples, consider highways extended to previously inaccessible areas or consider better enforcement of agricultural property rights.

A focus on speculative reasons for high yield elasticities unaccompanied by speculation on reasons for high land elasticities gives the appearance of a pre-conceived agenda and should be avoided.

Several papers, including those most emphasized by Keeney and Hertel, find *in the words of the reseachers themselves* that the yield-price elasticity cannot be distinguished from zero. This is consistent with a long tradition on agricultural economics, which frequently treats yields as determined by long-run technology and weather, but not by prices. Roberts and Schlenker, in recent high quality work, support a similar view. In results just reported, I find a similar result in a simple model using a long and recently updated time series of USDA data.

However, there is much resistance to a literal value of zero for the yield-price elasticity. There is evidence that farm inputs (such as fertilizer use) respond to prices, which is consistent with some positive value for the yield elasticity. This view has some merit, although it does not support any particular positive value, such as 0.25. Agronomic evidence is consistent with the idea that in the modern developed world, increases in fertilizer use will have (at the margin) relatively small effects on yields.

One of the larger empirically estimated values for the US corn price elasticity, at least in recent literature, is the 0.15 value found by Huang and

Khanna (who find a lower value for soybeans and a higher value for wheat.) This seems to be a reasonable “high” value for the elasticity. A point estimate that is positive but quite close to zero, such as the 0.03 estimate just discussed, is also easy to defend. Averaging 0.03 and 0.15 gives a 0.09 estimate, which (given all the imprecision) is naturally rounded to 0.10.

Based on all the empirical evidence, and taking into account the “input-based” arguments against an elasticity value of zero, I would recommend a yield-price elasticity value of 0.10. Larger values, such as 0.25, have little to no support in the empirical literature. The initial use of 0.25 was based on a reading of the literature that simply ignores the clearly stated conclusions of the researchers being cited. On the other hand, while a value very close to zero could easily be defended, a value of *exactly* zero would conflict with evidence that farmers do respond in some ways to price increases. The value 0.10 avoids this last problem.

## 4 Demand Reduction as Source of Carbon Gain

Roberts and Schlenker (2010) estimate that reduction in human consumption of grains would account for about 1/3 of the biofuel stock. This follows directly from their (relatively credibly) estimated demand and supply elasticities. GTAP and other CGE models often find a similarly large (or even larger) demand reduction. For example, Edwards, Mulligan, and Marelli (2010) use the GTAP model and find that (after adjusting for by-products) every ton of corn used for ethanol is associated with a 0.52 ton reduction in the human consumption of corn. The similar figure for wheat is 0.46 tons.

The standard GTAP accounting takes “carbon credit” for the reduced consumption. Searchinger (2010) notes that this amounts to taking credit for the carbon that humans fail to exhale (and excrete and so forth) because they are eating less food. It treats carbon-rich human breath as a pure “social bad.” Treating human food consumption in this way raises important moral, economic and political concerns.

The reduction in food consumption has a potentially very large social welfare impact. Roberts and Schlenker (2010) find that a simple biofuel mandate that makes use of 5% of world calorie consumption reduces consumer surplus by \$155 billion worldwide.

While the predicted price increases reduce consumer surplus, they also increase producer surplus: farm incomes go up. This of course explains much of the political economy of bio-fuel policy. One traditional economic approach would be to add up the changes in consumer and producer surplus and compare them to the dollar-valued “social benefit” of carbon reduction.

There are several problems with this approach. First, the CARB / GTAP exercise involves carbon accounting in units of carbon, not social welfare accounting in dollars. There is a very difficult question of how to translate “lost consumer surplus from reduced food consumption” into units of carbon.

Another problem is with treating all social gains and losses equally. It will seem reasonable to many observers to treat the food losses to the world’s poorest consumers (who are likely to be, by far, the most elastic consumers of unprocessed food) as much more important than the effects of small increases in the price of processed food eaten by consumers in the developed world.

To take one’s “moral pulse” on this question, one can ask if one would be willing to combat global warming by undertaking direct efforts to reduce the food consumption of the world’s poorest families. Such a policy would have the same beneficial carbon effects as the consumption reductions predicted by the GTAP model. If one would not be willing to take such steps to deprive poor families of food directly, then one should not take “carbon credit” for implementing the same steps indirectly via the market for food.

There are several possible objections to the argument above. The first is that one *would* be willing to combat global warming by taking direct steps to limit the food consumption of the already poor. This is a moral disagreement that is not amenable to empirical resolution.

A second objection is that rising food prices would help the global poor who are also *producers* of food. Some food aid groups have apparently argued that food aid, by reducing prices, hurts poor farmers. According to this story, the urban poor might be hurt by rising food prices, but the rural poor would be helped and the two effects might roughly cancel out.

This second argument is amenable to empirical study and there is some evidence available. First, note that subsistence farmers who grow some food for personal consumption but do not sell food into the market are not aided by rising prices (unless the price rise is sufficient to draw them into the market.) Keeping this in mind, Levinsohn and McMillan (2007) study the effect of food aid (modeled as a reduction in the price of wheat) in Ethiopia. They summarize their findings as follows:

We find that food aid in Ethiopia is "pro-poor." Our results indicate that (i) net buyers of wheat are poorer than net sellers of wheat, (ii) there are more buyers of wheat than sellers of wheat at all levels of income, (iii) the proportion of net sellers is increasing in living standards and (iv) net benefit ratios are higher for poorer households indicating that poorer households benefit proportionately more from a drop in the price of wheat. In light of this evidence, it appears that households at all levels of income benefit from food aid and that - somewhat surprisingly - the benefits go disproportionately to the poorest households.

I know of no empirical evidence to the contrary. This finding refutes the idea that net sellers of food, who would benefit from price increases, are nearly as poor as the net purchasers of food who would benefit from lower prices.

A third counter-argument to the concern about food reductions is to argue that food consumption would not decline at all. First, if this is true the GTAP results are incorrect and need to be changed. One argument that might be made is that food demand is quite inelastic and therefore any reduction in food consumption is implausible. This perhaps confuses absolute and relative elasticities. Since supply elasticities are also low, reductions in food consumption are plausible even when demand is quite inelastic (again, see Roberts and Schlenker (2010).)

Another related argument is that third-world countries will subsidize food consumption to keep it from falling. This is said to have perhaps occurred in response to the food commodity price increases that were observed prior to the recent economic crash. Again, if counter-subsidization is the correct model of demand, then the standard GTAP model is incorrect and needs to be changed to account for this.

If CARB agrees that it is not moral to take "carbon credit" for taking food out of the mouths of the poor, then one can suggest two ways of changing the models and/or the accounting. Hertel, Golub, Jones, M, Plevin, and Kamme (2010) alter the GTAP model to hold food consumption fixed. This is similar to implementing the "counter-subsidization" scenario where governments do not allow food consumption to fall. The equilibrium model is otherwise allowed to adjust, so that further production changes (land and yield increases and so forth) take place in response to the fixed amount of food consumption. This approach may be best if the "counter-subsidization" scenario seems plausible.

Otherwise, one could simply “not count” the carbon benefits of the reduced food consumption. This approach amounts to assuming that the social cost of the reduced food consumption is exactly matched by the social benefit of the carbon reduction. It does not treat the reduced food consumption as on net “bad,” but neither does it treat it (as in the standard GTAP result) as “good.” This approach is apparently under consideration by the European Union’s Joint Research Centre.

## 5 Other Issues

In this section I very briefly discuss other matters that are worthy of further consideration and research. In these areas, I do not have a current recommendation for a short-term fix, but as a multi-year regulatory process moves forward it would be good for these issues to receive further attention.

### 5.1 Yields on New Land

Farmers presumably make use of the best available land first when deciding where to locate production. This economic “selection” effect implies that land newly brought into production will be worse in some way as compared to land that was previously used. It might feature worse yields, have higher input requirements or it might be badly located with respect to transportation or to competing land uses that bid up the price of land.

The GTAP model has to assume something about the yield of new land. Earlier versions picked an entirely *ad-hoc* number. More recently, the GTAP model has been tied to a sophisticated biological model of the land’s ability to grow a standardized reference crop. This is clearly a big improvement over an arbitrary guess.

However, a purely biological model ignores the economic selection effect just mentioned. If there is highly biologically productive land that is not used for farming, why is it not being used already? It must be that it has other disadvantages in terms of transportation, land use regulation, or competition with alternative uses. Perhaps it is in a protected park, or in a war zone or in a region without roads.

The biological model being used sometimes predicts that in some region land not used for agriculture is *more productive on average* than land used for agriculture. This fails a kind of sanity check, not with respect to the

biology but with respect to economics. There must be some problem with the apparently productive land or else it would already be in use.

I understand that current GTAP runs cap the yield of new land at 100% of the yield of old land, even when the model says that new land should be more productive. This is a start at imposing economic logic on the model for yields on new land.

The purely biological model of the yield of new land should be, in the medium run, augmented with a more complete economic model that asks why apparently productive land isn't currently used. In this case, biology cannot substitute for economics.

The effect of ignoring the economic selection of land is to overstate the productivity of the marginal land that is pulled into production by a price increase. The biological model assumes that productive land not currently used is readily available for future production, whereas an economic selection model would take into account the reasons why such productive land is not currently used, and therefore might be unlikely to be used in the future as well.

The effect of using the purely biological model is therefore to understate the indirect land use cost of biofuels.

## 5.2 Land Use Elasticities

The GTAP model has to decide how much land is drawn into production when prices rise, and also where that land is located and exactly what crops are grown on it. This is a very large set of elasticities, and as usual the existing empirical literature is not nearly as helpful as one would hope.

One very important point is that unmanaged forest is missing from the GTAP model because that particular model requires the existence of an observed land-rent. The absence of unmanaged forest seems a very serious problem, since the transformation of unmanaged forest into agricultural land would have very serious carbon consequences.

Gibbs, et al, (2010) indicates the magnitude of the possible problem. The abstract of that paper reads in part:

Across the tropics, we find that between 1980 and 2000 more than 55% of new agricultural land came at the expense of intact forests, and another 28% came from disturbed forests.

It is a very important question whether the GTAP predictions are at all consistent with this result. Gibbs et al (2010) does not attempt to distinguish supply from demand effects and so it is not a definitive statement about the likely pattern of demand-driven land-use change, but the potential carbon issue is clear. There is at least the strong possibility that a large portion of new land will be transformed from intact tropical forests and GTAP is not systematically modeling unmanaged forest.

Other concerns, listed briefly:

- The empirical issues of the simultaneous determination of price and land use apply here. How do the GTAP elasticities compare to elasticities determined by conventional IV methods? Again, Roberts and Schlenker (2010) provide such estimates and Huang and Khanna provide a table of results from their own and other models.
- GTAP models are largely based on US results. Other regions have very different land-use rules, capital markets, agronomic features and so forth.
- It is hard enough to get an empirical estimate of the “aggregate land elasticity.” It is harder still to get all the needed cross-land use elasticities (e.g. elasticity of the use of land for wheat with respect to price of corn.)
- The GTAP CET production function has to impose many of these cross-price effects via functional form governed by a few parameters.
- Other kinds of empirical studies can use richer empirical specifications (For example: Lubowski, Plantinga, et al used relatively flexible logit models.) How different are the implications of different models?

### 5.3 Trade Elasticities

The GTAP model makes use of “Armington” trade elasticities, which were originally proposed as a method for dealing with the observed fact that much consumption seems to be “home biased” even when trade barriers are low or non-existent.

There are two problems with the GTAP’s use of Armington elasticities. The first is that they are (once again) estimated by least squares rather than by credible instrumental variable methods.



The second issue is that modern trade theory has rejected the Armington model as unrealistic. The Armington model keeps trade patterns from changing “very much,” which can be too extreme an assumption. Modern trade theory relies on a combination of transportation costs and product differentiation to explain the apparent home bias in consumption. By far the best cited papers of recent trade theory are Eaton and Kortum (2002) and Melitz (2003), the latter of which extends the Dixit-Stiglitz-Krugman framework. Both of them emphasize product differentiation and transportation costs and neither make any use of Armington elasticities. For a recent Ph.D. paper that criticizes the empirical performance of traditional CGE models and uses an Eaton-Kortum framework, see Caliendo and Parro (2010).

According to recent trade approaches, commodities (like ethanol) that are basically undifferentiated should flow fairly freely in international trade, with price differences across regions reflecting transportation costs but very little additional “home bias.” This prediction is not similar to the Armington model and it has large consequences for the location of production. The Armington model will predict a greater tendency for production to take place in the region of final consumption.

## 6 Conclusions

Because all of the positive gains from bio-fuel use are market mediated (and in that sense “indirect”), some kind of equilibrium economic model is needed to evaluate bio-fuel policy. The GTAP model features relative sophistication on the theoretical side but (in common with other CGE models) its empirical inputs are often weak. This follows partly from the weakness of the underlying empirical literature.

In the case of yield elasticities, the empirical work cited in the GTAP research literature substantially misstates actual empirical results and vastly misrepresents the clearly stated opinions of the authors of cited research. The existing empirical literature generally points to yield price elasticities that are difficult to distinguish from zero. Even when one takes into account arguments that the yield elasticity should be positive and not strictly zero, it is very hard to argue for yield elasticities much in excess of 0.1.

The use of 0.10 as a yield elasticity could be defended on intellectual grounds, whereas the use of 0.25 is without scientific basis.

Speculation about possible reasons for a higher yield elasticity does not

justify a different course of action, as one could equally well speculate about higher land-use elasticities. Since it is the relative elasticities that matter for the carbon accounting, such speculation about yield but not land elasticities would indicate an obvious bias.

The incorrect yield elasticities used in the current GTAP modeling clearly bias the reported finding toward a lower indirect land use cost of biofuels. This bias is easily corrected via the use of a 0.10 value for the yield elasticity.

The use of a purely biological model of the productivity of new land, while a clear intellectual improvement over early purely ad-hoc estimates, also biases downward the indirect land use cost of biofuels, as compared to a more appropriate combined economic / biological model that explains why apparently highly productive land is not currently farmed. However, such a model would be difficult to introduce in the very short run.

It is likely that the GTAP treatment (or lack of treatment) of unmanaged land also biases the indirect land use cost downward, although this is somewhat harder to establish with certainty, at least without actually changing the model and re-running it. Unfortunately, such a change would require dealing with fundamental problems in the GTAP land use model.

Other problems with GTAP inputs may also bias the model, but not clearly in one direction.

Aside from problems with inputs to the model, carbon policy-makers should understand the difficulties with treating reduced consumption of food by humans as a pure social good, as in done in the current baseline GTAP / CARB result. Such a policy stance may be unlikely to find broad moral support. Two possible solutions are to either [i] hold the consumption of food fixed in the GTAP model or [ii] simply decide not to count as a carbon credit the reduced emission of carbon from humans (as in the carbon exhaled by humans.) The GTAP model has already been adapted for the first course and the second course may also be feasible within the current model.

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