# COMMENTS ON THE GTAP LAND FUNCTION

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Recent work on the GTAP model has both introduced cropland pasture into the land use categories for U.S. land, and increased its conversion responsiveness based on evidence that farmers are more willing to substitute between corn and soybeans than previously estimated, and that this willingness to substitute should imply the same willingness to substitute between crops and pasture. The net effect of this change is to further reduce the quantity of forest converted as a result of biofuels and to reduce ILUC emissions. It is impossible to evaluate this change properly without evaluating the underlying validity of how the GTAP model land expansion function works. GTAP uses the same function to estimate both how much land expansion will occur as prices rise and what types of land will be converted, e.g., forest or pasture. Rather than addressing all elements of the new GTAP model, my comments therefore address the question of whether there is genuine evidentiary support for the GTAP model's estimation method for land expansion.

## Role of GTAP Land Function

Unlike other global land use models, GTAP uses economics to estimate not only how much cropland will expand as crop demands increase and prices rise, but also whether the lands are converted from forest or pasture. For the second question, other models have used historical experience and assumed that types of land converted in the future will track types of land converted in the past. The model's results largely depend on the model's land supply function, which in turn are ultimately derived from a low estimate of the land supply elasticity (0.045) (Ahmed 2008), and the assumption of a far higher responsiveness of yield to price (0.25). The GTAP cropland expansion function leads to both a low total range of land use change and a low rate of forest conversion. GTAP predicts a lower ILUC than some other models because it predicts both less land expansion, and less expansion into forests (Plevin 2010).[1] Other models, including EPA's model, in one way or another extrapolated

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[1] The GTAP analysis now reflected in CARB rules estimates that 19% of net conversion is from forest and 81% from pasture (Plevin 2010). That increased to 25% in the Tyner April 2010 paper for the 2001 database and to 35% using the 2006 database (Tyner 2010).

from prior conversion history and projected future conversion would follow the same patterns.

The basic land expansion function in the GTAP model is relatively simple. The function that the same coefficient, sigma (0.2) dictates the same level of land expansion in response to a change in rental rates for (1) each land use type and (2) in every region of the world. The formula for the expansion of each land use type (cropland, pasture, or forest) in response to a 1% change in price is 0.2 * (1-R), where R is the rental share of that land. For example, if cropland within a region receives 50% of the total rents for all land uses, then the elasticity is 0.2 * (1-0.5), which equals 0.1. The same function and the same constant are applied to pasture and forest, with the only change lying in their share of total rents ("R").

This function means that single factor that dictates changes in the elasticity from region to region is the share of rents in one region received by each land use type compared to the shares of rent for those types in another. In other words, if all the rents received by cropland in a region are 90% of all the rents for all land uses, the ratio of change (the elasticity) is 5 times higher than a region where cropland rents are only 50% of the total (0.02 compared to 0.1). This simple land expansion function has great consequences for the ultimate GTAP result, and the question is what actual evidence there is to support it.

## Lack of Evidentiary Support and Other Flaws with the Land Use Function

### A. The GTAP model attempts to estimate land use change around the world based entirely on a study of changes in U.S. land.

#### 1. *The responsiveness of land to crop price changes is a peculiarly local response.*

The GTAP land function does not in any way attempt to define a function that reflects the rational behavior of landowners. It does not, for example, attempt to estimate whether land use change makes economic sense based on the revenues and costs of converting one piece of land to another. It claims instead that a change in cropland rents results in a certain percentage increase in cropland for whatever reasons. Lacking any particular economic rationale (microeconomic foundation), such a prediction is only as compelling as the evidence to support it. In this case, these direct observations are based on a single study of U.S. land use behavior from 1982 to 1997 described in two related papers (Lubowski 2006, 2007).

Unfortunately, the factors in the real world that will determine how much cropland expands in response to rents (and in turn to price) are peculiarly area-specific. They should include at least the following:

- The relative productivity for crops of pasture and forest in any particular region, in particular, not the average productivity but the productivity of the best potential conversion sites, which means heterogeneity important;
- The costs of converting non-cropland to cropland in one region rather than another, and any differential costs in farming the converted land;
- The transportation access to non-cropland versus cropland;
- Legal restrictions;
- Other physical restrictions;
- Ownership patterns and restrictions;
- Farmer access to capital.

The GTAP model does not attempt to take account of any of these factors and varies expansion rates entirely based on the share of rents.

To highlight one obvious distinction, in the U.S., total cropland actually declined by 9.5% over the period of the study used (from 1982 to 1997, Lubowski (2008), Table 1.) In regions such as Latin America and Southeast Asia, cropland was expanding. At best, the model assumes that land use change worldwide responds to changes in rent in precisely the same ways as the U.S., which is implausible.

### 2. *The response of cropland expansion in the U.S. is a poor model for estimating world land use change in general and particularly for the period studied.*

The U.S. is a highly established agricultural country, without any expanding frontier, and has long had extraordinarily stable agricultural land areas. Moreover, until 1996, cropland was heavily influenced by government policy, with set aside programs and the CRP. In fact, the Lubowski study extends from 1982 to 1997, just prior to the abandonment of cropland area restrictions and set-aside programs in the 1996 Farm Bill. Much of what Lubowski was therefore measuring was the effect of government policy.

Lubowski's analysis also occurred during the period of CRP enrollments. To avoid ascribing land use change to the CRP program, when calculating net change elasticities for use in the GTAP model, Lubowski took CRP acreage as fixed and examined only changes among land uses that did not involve CRP (Lubowski, personal communication, 2011).[2] That was appropriate, but CRP inevitably had the effect of dampening other land use changes. In the period after its establishment from 1985, which corresponds to Lubowski's first five year period of study, those seeking to reduce their cropland due to the low prices of the period could enroll their land in CRP, but such changes were not counted by Lubowski's analysis. In the last five year period, CRP acreage was fixed by ten year contracts and so many of the acres most likely to come in and out of production would not be among the acres able to change. None of this is a criticism of the Lubowski analysis of this time period, but it all combines to help explain why the estimated elasticity of conversion for cropland due to price was low, and why these figures are therefore poor judges of the responsiveness of land use change worldwide.

In short, even if the GTAP model correctly generated an excellent analysis of land use change in response to price in the U.S., the evidentiary support for applying such a low responsiveness to other countries would be minimal.

### B. GTAP uses a short-term elasticity that is one fourth the 30 year elasticity estimated by the Lubowski study.

The GTAP model derived its land expansion factors from Lubowski, but chose to use the elasticity of cropland expansion estimated for five year periods of roughly 0.045. (In Lubowski et al. 2006, the elasticity is presented only in a chart, not a table, so the number presented here is the best that can inferred from the chart.) This elasticity is used to derive the constant sigma of 0.2 used by the GTAP model as described above and further discussed below. Lubowski estimated the 30 year elasticity at almost 0.2, a four-fold increase. These longer term elasticities are as inherent in the structure of the model used by Lubowski as the shorter term elasticities.

CARB has decided to examine the emissions from land use change and the saved fossil fuel emissions over a 30 year period. Even if the U.S. were an appropriate model for the rest of the world, CARB's focus means that the 30 year elasticity should be used and should result in substantially more land expansion.

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[2] In Lubowski (2007), the paper provides elasticities for gross land use changes due to price, as a percentage of the changes not due to price, for changes from particular land uses to another, e.g., pasture to forest. In that context, Lubowski looked at transitions of land into and out of CRP. For use of the GTAP model as reflected in Ahmed (2008), Lubowski calculated net own price elasticities for area changes in each land use, and for that analysis, he ignored changes in and out of CRP.

## C. The revenue shares of each land use type in each region have a large effect on both the amount of estimated expansion and the type of land converted but lacks evidentiary support.

### *1. There is no evidence to support the critical role of revenue shares in the GTAP land function.*

The Lubowski analysis estimated the elasticity of land use change using a "Logit" model that is implicitly influenced by the total share of land in the region occupied by different land uses as measured by area, not by their share of rent. Changes in the *per acre* net return (per acre rent ) of the land then cause certain changes in area. Nothing in the Lubowski model provides that the responsiveness of land use changes to a change in the per acre return of land is influenced by the share of total rent of a particular land use. The share of cropland in land area and the share of cropland in rent are, of course, quite different. The share of land is the percentage of land that is in one use, e.g., cropland. The share of rent is the percentage of the total rent received for all land of one category. For example, in Lubowski (2007), cropland represents 44% of the percentage of total cropland, pasture and forest but 77% of the total rent to those land uses.

Although rent shares have no role in the Lubowski analysis, they play a critical role in the GTAP model as the elasticity of land expansion depends on the rent share. For example, if cropland occupies 10% of the land and receives 50% of the total rent, its elasticity of conversion is .1, but if it that same quantity of land receives 90% of the rent, its elasticity of conversion is only .02, and the amount of cropland expansion will be only one fifth as high. The share of rent by grassland and forest also plays a critical role in determining whether cropland expansion occurs at the expense of grassland or forest as the same function determines the response of each of these land uses to a change in price and therefore the cross-price elasticities.

### *2. The revenue share function contradicts the Lubowski analysis.*

The Lubowski model not only has no role for revenue shares but its findings contradict the GTAP function. Using the share of rents in the U.S. presented by data in Lubowski (2007), it is possible to calculate the elasticity of each land use type in response to a change in its average rent for cropland, pasture and forest for both five years and 30 years as predicted by the GTAP function. That predicted elasticity can then be compared to the actual elasticities estimated by Lubowski. As shown in Appendix 1, the GTAP function is off by as much as a factor of 10 compared to the elasticities estimated by Lubowski. If Lubowski is correct, and that is the only actual economic evidence to back up the GTAP model, then the rent share function cannot be correct, which means it does not properly predict the response of different kinds of land to rent increases.

## D. The role for revenue share also violates common sense.

### 1. *The revenue share model makes the availability of unmanaged land of no consequences to the estimate of cropland or pasture expansion.*

Much of the focus of the land use inquiry is the potential for expansion of agricultural land into carbon-rich, unmanaged lands. The role of revenue shares means that the availability of such lands has no effect on the estimate of cropland or pasture expansion. For example, if cropland receives 80% of a region's rent, the amount of expansion GTAP will predict will be the same if there is no unmanaged land in the region, and if unmanaged land occupies 90% of the region's land. According to Gollb (2008) Figure 1, unmanaged forest is not only the majority of forest worldwide, but a dominant share of total forest land in Latin America, North America, Oceania, and the former Soviet Union.

Indeed, the result would largely be the same if unmanaged land were arbitrarily assigned some low rent, as apparently some "accessible" forest land has been treated in parts of the GTAP database. The regionally varying factors that determine the absolute quantity of cropland expansion are solely its revenue share and the quantity of cropland. If unmanaged land has a modest rent, it will only minimally affect the revenue share of cropland and therefore only minimally affect the extent of cropland expansion.[3]

The lack of consideration of unmanaged land also plays another role in reducing net forest conversion. Because the demand curve for forest products does not change, in the GTAP model a reduction in forest supply in one location due to cropland expansion results in part in an increase in forest land in another location (and also creates a pushback on forest conversion in the first region). In the real world, a substantial portion of that replacement forest supply is likely to result from increasing the harvest of unmanaged land, but that is not possible under GTAP. The result of leaving out unmanaged land as a source of timber products is that the GTAP model predicts substantial forest reversion and the net decline in forest area is substantially less than the gross decline.

GTAP's use of a broad definition of grazing land gives added importance to this failure to incorporate unmanaged forest. The Lubowski paper provided different elasticity

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[3] This fact may be obscured somewhat by the fact that once GTAP estimates an area of forest expansion, the emissions for forest expansion, which depend on the type of forest converted, have been estimated based on historic factors and includes unmanaged forest. That twist does not alter the fact that the quantity of both total land conversion and the percentage of that land conversion from forest estimated by GTAP are unaffected by the availability of unmanaged forest.

estimates for rangeland and pastureland, and rangeland was far less elastic. At 129 million acres, pastureland was also a small fraction of the 411 million acres of rangeland (Lubowski 2007, Table 1). However, GTAP applies its elasticities derived from Lubowski to one category of grazing land worldwide, including rangeland. Rangeland differs from pasture land in the U.S. in large part based on how wet it is, with rangeland typically being too dry for cropping. In effect, while GTAP is underestimating forest by leaving out managed forest, it appears to be estimating all grazing land as equivalently suitable for cropping as pasture.

To their credit, the GTAP authors have identified the conventional model's inability to address managed land and have made stabs at developing model elements that would attempt to do so (Golub 2008). However, that is not this version of the model (and I offer no comments on the viability of the approach pursued in that paper).

Others commentators have previously observed that the lack of a role for unmanaged forest is a problem for GTAP. Perhaps because those comments have not explained the land expansion function, it may not have been obvious how the availability of unmanaged land has no effect on the model's estimate of the expansion of agricultural land. Because the GTAP model does not account for the availability of unmanaged land as agricultural lands, nor permits forest product demand to be met by an increase in timber from unmanaged forests, the GTAP model cannot rightfully be considered an appropriate model for estimating ILUC.

### *2. The revenue share model implies that lower economic returns to forest and pasture result in less cropland conversion not more.*

The GTAP model projects that as the share of cropland rent increases, the elasticity of cropland expansion decreases. A guiding intuition is that as cropland occupies more and more of a region, less other land remains to convert, so cropland will expand at a slower rate. But that is not the actual GTAP approach. It turns not on the share of land area, but on the share of land rent. In general, cropland rents are higher than rents for pasture and forest, but the higher the rents for pasture and forest, the lower the share of cropland rent. As a result, according to GTAP, the higher the rents (and therefore economic returns) of pasture and forest, the lower is the level of cropland expansion.

That proposition not only has no supporting evidence, but contradicts general economic principles. By first order economic thinking, the higher the rent for pasture and forest, the greater their economic return, and the less likely that cropland will displace them. Conversely, the lower the relative rents, the higher the expected level of conversion. In the original GTAP formulation, there was a rationale for this approach because low rents of pasture and forest were assumed to reflect a proportionately lower potential cropland yield.

The lower the rent to pasture, for example, the lower its assumed productivity for cropland and the less likely it would be converted. But all the versions of GTAP used for CARB have properly abandoned that assumption. Although the potential productivity of pasture and forest for cropland is undoubtedly a factor that influences whether or not it is converted, so long as the land remains as pasture or forest, its rents should reflect the economic returns to using such lands for pasture and forest.

Indeed, the assumption that rents of pasture and forest reflect their economic returns is critical to the use by GTAP of the Lubowski estimates. Lubowski actually estimated the response of land to changes in the net returns for that land, including the net returns to pasture and forestry. GTAP treated net returns as equivalent to rent. If pasture and forest rents are not based on their net returns, then GTAP cannot use the Lubowski results at all as it has used them.

Some strange results reported by the GTAP model probably reflect this flawed land function. In the April 2010 (2001 base) versions of the model, the modelers estimated that while two thirds of the cropland expansion in Brazil would come from forest, in the rest of South America not only would all of the cropland expansion come from pasture, but forest expansion would be more than three fourths as extensive as cropland expansion (Tyner 2010, Table C1). This expansion of forest in the rest of South America eliminated the consequences of roughly two thirds of the forest conversion in Brazil. The directional effects were the same but less substantial in the version using the 2006 database.

### D. The inclusion of cropland pasture only adds assumptions that reduce ILUC without an evidentiary basis.

New versions of the GTAP model have incorporated cropland pasture, and as I understand it, cropland pasture is treated as cropland for purposes of estimating the likelihood that cropland pasture will supply new cropland. Under GTAP, switching land devoted to different crops among the same cropland is more elastic than switching from pasture or forest to crops. The effect of treating cropland pasture as a crop is to estimate more conversion of low carbon land (cropland pasture), particularly in the U.S., and less conversion of higher carbon land worldwide, such as forests. In the yet newest version of the model, as I understand it, GTAP has also increased the responsiveness of cropland conversion generally, based on an analysis of increasing responsiveness to price of decisions by farmers in the U.S. to plant corn or soybeans. The likely combined effect of the two decisions is to increase even more the conversion of cropland pasture in the model and therefore to decrease the conversion of higher carbon lands.

Cropland pasture is one land use and it obviously makes sense to include it in some way in any modeling analysis. However, that does not say anything about the actual evidence for the elasticity of conversion of cropland pasture versus any other land use. Once again, the model is working without evidence. The use of the corn/soybean relationship seems particularly inappropriate. Corn and soybeans are typically grown in the same rotation. The fact that farmers, in response to relative prices, may be able to "cheat" their rotations relatively easily for a year or two in response to price differences of corn and soybeans implies virtually nothing about the conversion of cropland pasture to other crop uses.

On a more detailed level, Appendix B includes a memorandum from Ralph Heimlich analyzing even the analysis of the corn/soybean relationship estimated by the GTAP authors. The GTAP authors have apparently used pure OLS regression of price to land area, rather than using exogenous instruments. Using this method too Heimlich shows that the statistical relationship is a shaky one, with the use of a ten-year analysis rather than nine years dramatically weakening the statistical fit (R squared). His analysis also shows the strong trend fit matching corn and soybean production, casting doubt even on the long-term sustainability of a change from one crop to the other.

One of the many problems with just assuming that cropland pasture shifts to other crops in a similar manner as corn and soybeans shift to each other is that GTAP then assumes that the yields will be the same. Cropland pasture is land that has been used as cropland and that farmers are instead using for pasture. In light of the different economic returns to cropping and pasture, that must generally occur because this land has lower yields, and probably substantially lower yields. The intuition therefore is that cropland pasture is marginal cropland that may come back into production with high crop prices. While that is plausible, that does not tell us how high the prices have to be, or what the likely yields are. In fact, for many years, areas of cropland pasture were remarkably stable. They began to decline after 1997 probably in part because of the elimination of set-aside programs, but it is unclear how price fluctuations have affected their acreage without a more a properly constructed economic analysis.

In addition, to the extent cropland pasture exists because it is marginal cropland, cropped in year of high prices, structural shifts in demand such as those caused by biofuels, may have no bearing on its cropping level. After a structural shift in demand, prices will continue to fluctuate year to year, which will cause some shift in land in and out of production. To the extent cropland pasture provides some of that frictional cropland, the quantity of cropland pasture will fluctuate around a new long-term equilibrium, and there is no reason to believe that price will therefore permanently increase cropland at the expense of cropland pasture.

The fundamental problem is that in the absence of real evidence, the proper treatment of cropland pasture is speculation. Its proper treatment needs to reflect an underlying evidentiary basis for itself and for the overall land function in which it is placed. All that can now be said is that the treatment of cropland pasture adds yet another an element to the model's overall land function that lacks evidence.

## A model is not "better than nothing" if key elements lack a meaningful basis in evidence.

Some evidence is better than no evidence, and there is a tendency to rely on model results as though they were automatically evidence and therefore "better than nothing." But a model is not evidence by itself. It is a way of using working economic evidence together, and it can be no better than the evidence on which it is based. If GTAP starts with an assumed elasticity and function for the yield response to price without evidence, and an assumed elasticity and function for the land response to price, the further incorporation of those functions into a more complex model only introduces yet more uncertainties, and therefore only makes the problem of insufficient evidence "worse."

The use of sensitivity analysis for key parameter does not improve the problem. Not only is there no evidence for any particular parameters, but that means there is no evidence for any particular function that the parameter applies to, and no proof that there even is a function that explains the demand or supply response. For example, there is no reason to believe that a single land supply function explains the land supply response to price across a range of price increases, a range of baseline conditions, a range of biofuel levels, or a range of regions.

GTAP represents a substantial work of economic craftsmanship. There are undoubtedly situations in which it is useful to help explore possible relationships in world land use that should then also be studied and explored using direct economic methods. As GTAP authors have previously described it, GTAP can offer useful thought experiments. Despite the craft, policymakers need to be keenly aware about what does and does not have an evidentiary basis. The land use function in this GTAP model now does not have such a basis. It is obviously critical to the analysis of ILUC, and its lack of evidence requires an alternative approach.

## Summary

The critical ILUC question is how much land will be converted and of what type in response to a diversion of crops. That depends ultimately not on the absolute responsiveness of demand and supply factors but on their strength relative to each other. The supply response consists of yield intensification and land extensification. GTAP authors have apparently now acknowledged that the yield intensification factor they have been using lacks evidence. For the reasons presented here I believe the land supply function equally lacks evidence:

- Land responses in different regions depend on peculiarly local factors, which makes extrapolation for a single area peculiarly inappropriate.

- The extrapolation entirely from one study of U.S. experience is particularly problematic because the U.S. has long had a settled agricultural frontier and because the short-term responsiveness of U.S. cropland to prices in the 1982 to 1997 period was probably held down by U.S. agricultural programs that are not followed elsewhere.

- The prominent role for rent shares in determining the elasticity of supply lacks an evidentiary basis and contradicts the U.S. study relied upon. The formula also implies counter-intuitively that the higher the economic return to grazing and forest land uses, the greater the likely expansion of cropland.

- The GTAP modelers chose to use a short-term estimate of land response from the one underlying U.S. study rather than the 30 year estimate from the same study that was four times larger even though CARB is focused on land use change over 30 years.

- Most importantly, the availability of unmanaged land, including unmanaged forest, has no consequence for the amount of land conversion predicted by the model. As that is a primary focus of ILUC, the model results cannot be considered credible for estimating ILUC.

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## Appendix A: Analysis of How GTAP Land Function’s Use of Rent Shares is Consistent with U.S. Estimated Elasticities

Under the GTAP land function, the elasticity of expansion for cropland, pasture, and forest is all determined by a constant, “sigma,” multiplied by one minus the share of rents of that land use. For example, if sigma is .2 and if the cropland receives 80% (.8) of the total rents in a region, then the elasticity equals 0.2 * (1-.8) = .04.

To derive the sigma, the GTAP authors first used the same relationship and ran the equation backwards using the Lubowski elasticity estimates (Ahmed 2008, Figure 2). In other words, Lubowski’s five-year own price elasticity for cropland was 0.45, and using the share of land rents in Lubowski, that would yield a sigma of 0.2. However the authors did not stop there. For functional reasons, they wished to use the same sigma for all land uses, including pasture and forest. They therefore derived the sigma for each land use and then generated an average sigma weighted by the share of rents for each land use. Using the data we can derive from the Lubowski paper that generates a sigma of 0.169, still close to 2. It is close to the cropland sigma because cropland receives three quarters of the rent and therefore dominates the weighted average.

One can then use this derived sigma to derive the elasticities using the same Lubowski data. Not surprisingly, the elasticities for cropland derived from this average sigma are closely related to Lubowski’s estimated elasticity because of the heavy weight of cropland in deriving the average sigma. However, pasture and forest elasticities derived in this way from the GTAP function differ dramatically from those estimated by Lubowski. In short, the GTAP land function is inconsistent with the Lubowski elasticities, which are the only evidence offered to justify that land function. Table A1 shows these estimates using the data provided directly in Ahmed and Lubowski (2008). Ahmed states that the sigmas were derived using the revenue shares derived for the U.S. from the GTAP database, a different database than that used by Lubowski. Table A2 shows the same calculations using the slightly different rental rate data used by GTAP, with roughly the same overall results.

Table A1—Comparison of originally estimated own-return elasticities of land use by use to derived elasticities for 5-year and 30-year periods, United States

| | Use | | | | | |
|---|---|---|---|---|---|---|
| Variable | Crop | Pasture | Forest | Total | Simple average | Revenue-weighted average |
| Rent per acre ($/acre)[1] | $71 | $13 | $17 | | | |
| Acres (1,000)[2] | 416,000 | 129,000 | 399,000 | 944,000 | | |
| Revenue ($1,000)[3] | $29,536,000 | $1,677,000 | $6,783,000 | $37,996,000 | | |
| Rent share | 77.7% | 4.4% | 17.9% | 100.0% | | |
| Original 5 year elasticity[4] | 0.045 | 0.230 | 0.005 | | | |
| Derived Sigma[5] | 0.202 | 0.241 | 0.006 | 0.449 | 0.150 | *0.169* |
| Derived 5 year elasticity[6] | 0.038 | 0.161 | 0.139 | | | |
| Ratio of 5 year elasticitiy derived from GTAP function to 30 year Lubowski elasticity[7] | 0.8 | 0.7 | 27.7 | | | |
| Original 30 year elasticity[4] | 0.195 | 0.395 | 0.015 | | | |
| Derived Sigma[5] | 0.876 | 0.413 | 0.018 | 1.307 | 0.436 | 0.724 |
| GTAP formula derived 30 year elasticity[6] | 0.161 | 0.692 | 0.595 | | | |
| Ratio of 30 year elasticity derived from GTAP to 30 year Lubowski elasticty[7] | 0.8 | 1.8 | 39.6 | | | |
| Ratio of 30 year to 5 year elasticity[8] | 4.3 | 1.8 | 1849.0 | | | |

[1] Lubowski, et al, 2008, Table 2, p. 33, values are for 1988-92 period. These prices were used to estimate the land use change and elasticities between 1992 and 1997, which were the elasticities used by GTAP an dreported in Ahmed (2008), figure 2.

[2] Lubowski, et al, 2008, Table 1, p. 32, values are for 1982.

[3] Product of rent (row 1) times acres (row 2).

[4] Estimated from graph in Figure 2, page 7 of Ahmed, et al, 2008.

[5] Calculated as the elasticity over 1 minus the rent share, derived from the equation used in GTAP for the elasticity.

[6] Calculated as 0.2 times 1 minus the rent share, used to calculate elastiticies in the GTAP model. See Ahmed, et al, 2008.

[7] Derived elasticity divided by original elasticity. This indicates the degree to which using values derived with an average Sigma deviates from estimated elasticities for each use.

[8] Derived 5 year elasticity divided by original 30 year elasticity. This indicates the degree to which using 5 year values derived with an average Sigma deviates from estimated 30 year elasticities by use.

Table A2—Comparison of originally estimated own-return elasticities of land use by use to derived elasticities for 5-year and 30-year periods, United States [using Ahmed et al revenue shares]

| | Use | | | | | |
|---|---|---|---|---|---|---|
| Variable | Crop | Pasture | Forest | Total | Simple average | Revenue-weighted average |
| Rent share | 74.9% | 9.8% | 10.2% | 94.9% | | |
| Original 5 year elasticity[1] | 0.045 | 0.230 | 0.005 | | | |
| Derived Sigma[2] | 0.179 | 0.255 | 0.006 | 0.440 | 0.147 | 0.160 |
| Derived 5 year elasticity[3] | 0.042 | 0.152 | 0.152 | | | |
| Ratio of 5 year elasticities[4] | 0.9 | 0.7 | 30.3 | | | |
| Original 30 year elasticity[1] | 0.195 | 0.395 | 0.015 | | | |
| Derived Sigma[2] | 0.777 | 0.438 | 0.017 | 1.231 | 0.410 | 0.640 |
| Derived 30 year elasticity[3] | 0.182 | 0.653 | 0.650 | | | |
| Ratio of 30 year elasticities[4] | 0.9 | 1.7 | 43.3 | | | |
| Ratio of 5 year to 30 year elasticity[5] | 4.8 | 1.7 | 2,020.6 | | | |

[1] Estimated from graph in Figure 2, page 7 of Ahmed, et al, 2008.

[2] Calculated as the elasticity over 1 minus the rent share, derived from the equation used in GTAP for the elasticity.

[3] Calculated as 0.2 times 1 minus the rent share, used to calculate elastiticies in the GTAP model. See Ahmed, et al, 2008.

[4] Derived elasticity divided by original elasticity. This indicates the degree to which using values derived with an average Sigma deviates from estimated elasticities for each use.

[5] Derived 5 year elasticity divided by original 30 year elasticity. This indicates the degree to which using 5 year values derived with an average Sigma deviates from estimated 30 year elasticities by use.

# Appendix B. Reltionships Between Corn and Soybeans

## By Ralph Heimlich

In a paper discussing changes to the GTAP model for biofuels, Tyner and others added greater flexibility in switching between crops in response to price changes based on the following regression for the time period of 2000-2009:

Δ Harvested corn area (acres) = 0.263 + 0.122 Δ Corn revenue/acre(t-1) – 0.187 Δ Soybean revenue/acre(t-1)

They stated that

> "The independent variable *t* values are 4.8 and 5.0 respectively, and the adjusted $R^2$ is 0.73. Clearly, for the 2000-2009 period, changes in corn and soybean revenues were a major driver of changes in corn acres. We did the same regressions for prior periods and found no significant relationship. As the literature suggests, in prior periods, government policy was a major driver, and now it is commodity prices and revenue. For these reasons, we increased the transformation elasticity that helps govern the response in acreage share to changes in commodity prices from 0.5 to 2.5. "

I reran Tyner's regression using NASS data for 1949-2010 on corn harvested acreage (in acres, not thousands of acres), and the revenue per acre of corn and soybeans. I actually got a better fit with unlagged than lagged data for the revenue, but the signs were reversed. That is, the change in harvested corn acreage (say 1949 to 1950) was explained by the change in corn revenue per acre (1950-1949) and the change in soybean revenue per acre (1950-1949):

2000-2010
SUMMARY OUTPUT

| *Regression Statistics* | |
|---|---|
| Multiple R | 0.899265 |
| R Square | 0.808677 |
| Adjusted R Square | 0.760846 |
| Standard Error | 2929727 |
| Observations | 11 |

ANOVA

| | *df* | *SS* | *MS* | *F* | *Significance F* |
|---|---|---|---|---|---|
| Regression | 2 | 2.9E+14 | 1.45E+14 | 16.90704 | 0.00134 |
| Residual | 8 | 6.87E+13 | 8.58E+12 | | |
| Total | 10 | 3.59E+14 | | | |

| | *Coefficients* | *Standard Error* | *t Stat* | *P-value* | *Lower 95%* | *Upper 95%* |
|---|---|---|---|---|---|---|
| Intercept | -1,714,731 | 1065663 | -1.60907 | 0.146267 | -4172156 | 742692.7 |
| Change in production value per acre, corn | -43,656.7 | 15882.31 | -2.74876 | 0.025107 | -80281.4 | -7032.06 |
| Change in soybean production value per harvested acre | 161,888.7 | 30197.85 | 5.360934 | 0.000677 | 92252.31 | 231525 |

So my equation is:

$\Delta$ Corn acres harvested = -1,714,731 - 43,656.7 ($\Delta$ Corn revenue per acre) +161,888.7 ($\Delta$ Soybean revenue per acre)

Adj. $R^2$ = .76 and both revenue terms significant at the 95% level, although the t-statistics for the variables are lower than Tyner reports.

When you lag the revenue variables one year (so 2001 minus 2000 revenue per acre explains 2001 harvested corn acreage), the signs agree with Wally's analysis, but the $R^2$ drops to .36 and the significance of all the variables declines:

2000-2010, lagged
SUMMARY OUTPUT

| *Regression Statistics* | |
|---|---|
| Multiple R | 0.701936 |
| R Square | 0.492714 |
| Adjusted R Square | 0.365892 |
| Standard Error | 4770568 |
| Observations | 11 |

ANOVA

| | *df* | *SS* | *MS* | *F* | *Significance F* |
|---|---|---|---|---|---|
| Regression | 2 | 1.77E+14 | 8.84E+13 | 3.885092 | 0.066224 |
| Residual | 8 | 1.82E+14 | 2.28E+13 | | |
| Total | 10 | 3.59E+14 | | | |

| | *Coefficients* | *Standard Error* | *t Stat* | *P-value* | *Lower 95%* | *Upper 95%* |
|---|---|---|---|---|---|---|
| Intercept | 1,287,268 | 1592908 | 0.808125 | 0.442384 | -2385984 | 4960520 |
| Change in production value per acre, corn | 81,953.37 | 31873.5 | 2.571207 | 0.033065 | 8452.942 | 155453.8 |
| change in soybean production value per harvested acre | -130,898 | 49259.05 | -2.65734 | 0.028925 | -244490 | -17306.4 |

So

$\Delta$ Corn acres harvested = -1,287,268 + 81,953.4 ($\Delta$ Corn revenue per acre) – 130.898 ($\Delta$ Soybean revenue per acre)

Adj. $R^2$ = .36 and both revenue terms are still significant at the 95% level.

It is troubling that adding another year of data (2000-2010 vs. 2000-2009) changes the explanatory power of the regression so much, indicating that the relationship may not be very robust to relatively minor changes in the variables.

The analogous regression for the earlier period is:

1950-1999, lagged
SUMMARY OUTPUT

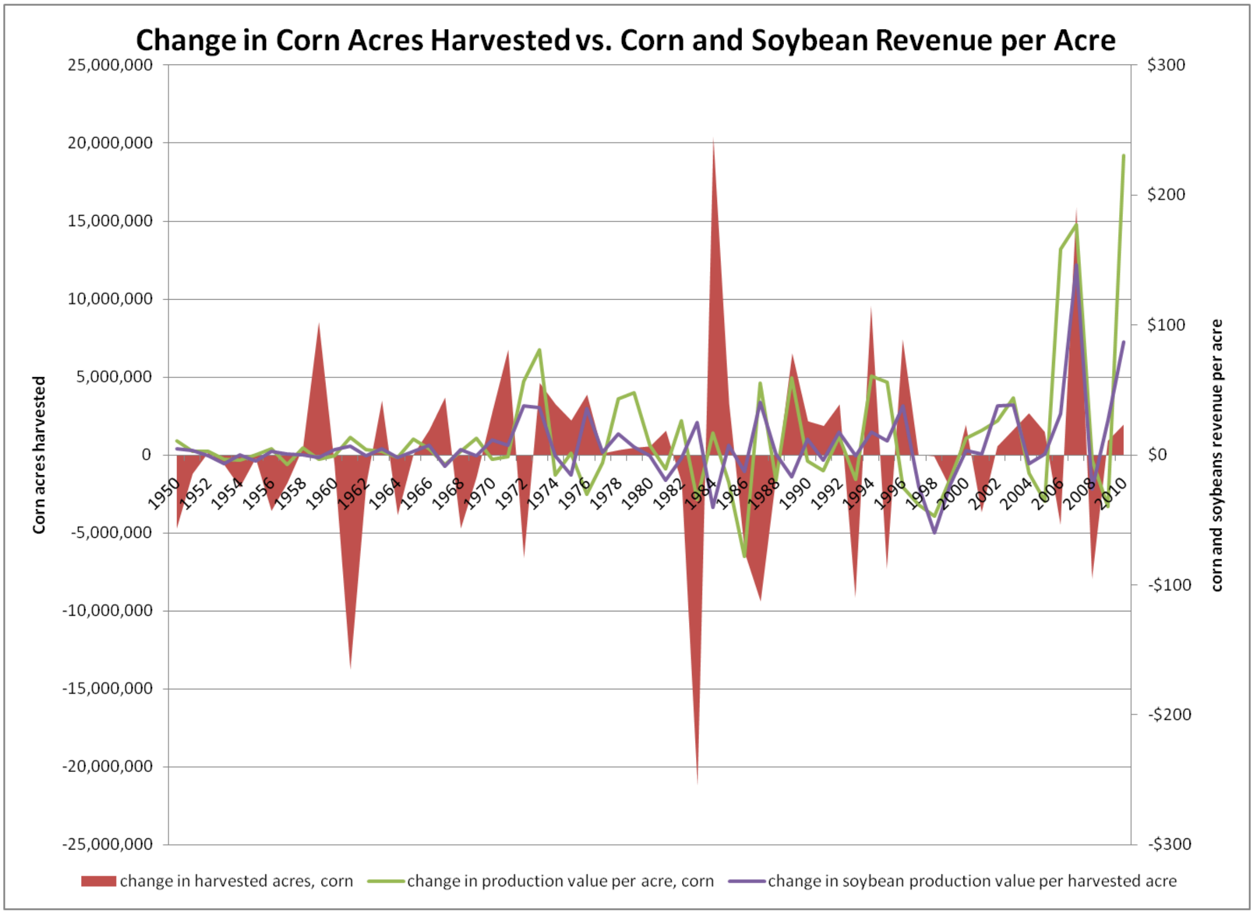
| *Regression Statistics* | |
|---|---|
| Multiple R | 0.133663 |
| R Square | 0.017866 |
| Adjusted R Square | -0.02484 |
| Standard Error | 6321138 |
| Observations | 49 |

ANOVA

| | *df* | *SS* | *MS* | *F* | *Significance F* |
|---|---|---|---|---|---|
| Regression | 2 | 3.34E+13 | 1.67E+13 | 0.418391 | 0.660584 |
| Residual | 46 | 1.84E+15 | 4E+13 | | |
| Total | 48 | 1.87E+15 | | | |

| | *Coefficients* | *Standard Error* | *t Stat* | *P-value* | *Lower 95%* | *Upper 95%* |
|---|---|---|---|---|---|---|
| Intercept | -131,549 | 918538 | -0.14322 | 0.886746 | -1980470 | 1717372 |
| Change in production value per acre, corn | -12,063.9 | 33147.63 | -0.36395 | 0.717566 | -78786.7 | 54658.79 |
| change in soybean production value per harvested acre | 49,607.86 | 54234.93 | 0.914685 | 0.365127 | -59561.4 | 158777.1 |

But the $R^2$ is only .02 and none of the coefficients are significant at the 95% level. So this confirms, as Tyner says, that the relationship he posits for 2000-2009 is not statistically significant for the earlier period. A plot of the variables over time is:



Regardless of whether the 2000-2009 or 2000-2010 results are used, I do not understand how an insignificant relationship between revenue and harvested area in prior decades can be used to estimate how much the GTAP parameter should change. Effectively, the own-revenue parameter for corn harvested acreage change in 1950-99 is zero, as is the soybean revenue parameter. It is not 5 times larger, but infinitely larger (i.e., 81,953/0) but making the calculation is really not helpful as a way of estimating the elasticity for GTAP.

An alternative way to approach this is to look at the mean, lower and upper estimates for regressions for 2000-2010, 1950-1999, and 1950-2010. This is made difficult by the lack of the robustness of the relationship over time, in which sometimes the coefficient for corn per acre revenue is positive and sometimes negative. Comparing a negative coefficient to a positive one doesn't provide much meaningful information.

| Regression | Mean | Lower 95% | Upper 95% |
|---|---:|---:|---:|
| 2000-2010 | 81,953 | 8,452 | 155,453 |
| 1950-1999 | -12,064 | -78,786 | 54,658 |

| | | | |
|---|---|---|---|
| Ratios of coefficients | meaningless | meaningless | 2.8 |
| 1950-2010 | 17,440 | -32,705 | 67,584 |
| Ratios of coefficients | 4.7 | meaningless | 2.3 |

The comparison between the mean 2000-2010 and mean 1950-2010 (which is not statistically significant) gives a ratio of 4.7, which may be where the multiplier of 5 came from. The comparison between the upper 95% level of both the 2000-2010 and 1950-1999 and 1950-2010 regressions both give a ratio of 2.3 to 2.8, which is about half the one Tyner used. Again, neither of the 1950-99 and 1950-2010 coefficients is statistically significant.

**Direct Ratio of Corn to Soybeans**

To more directly get at the relationship between crop choice and revenue prospects, I estimated some regressions using the ratio of corn to soybean acres harvested as the dependent variable and the ratio of corn and soybean revenue per acre as the independent variable. Over the whole 1949-2010 period, the explanatory power of the relationship is weak, but it falls completely apart after 2000.

1949-2010:

Ratio of corn to soybean harvested acreage = 6.21 – 3.42 (corn to soybean per acre revenue, lagged)

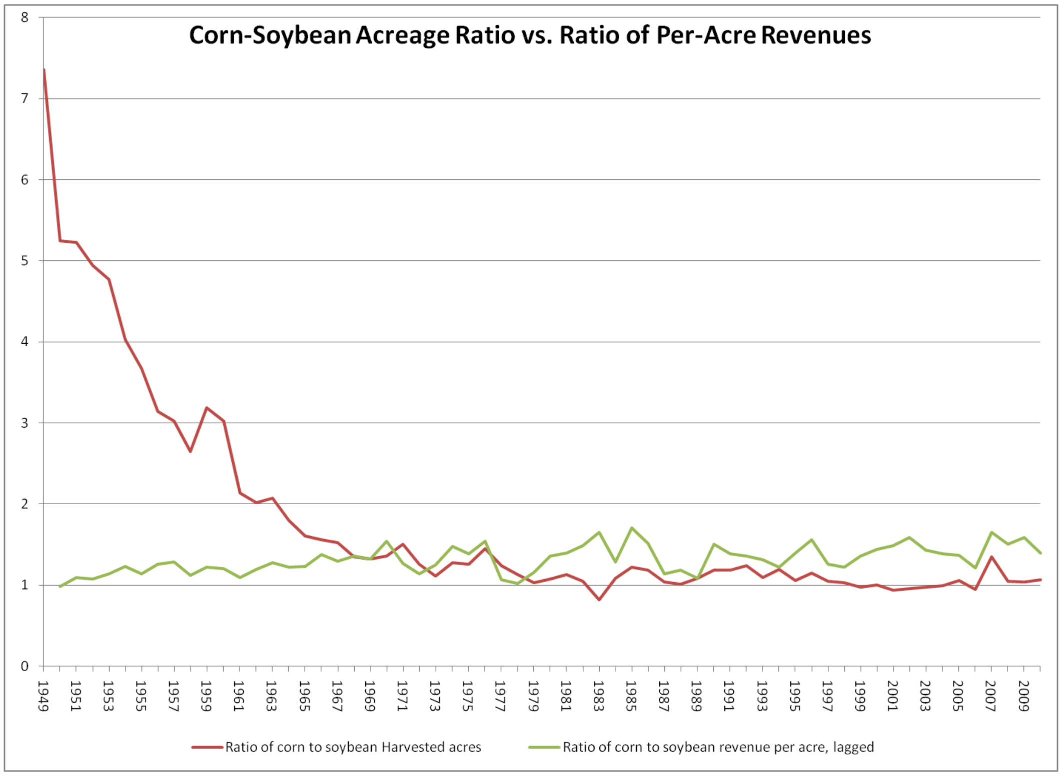
Adjusted $R^2$ = .26 and both intercept and revenue variable are significant at the 95% level.

2000-2010:

Ratio of corn to soybean harvested acreage = 0.329 + 0.482 (corn to soybean per acre revenue, lagged)

Adjusted $R^2$ = .195 and both intercept and revenue variable are not significant at the 95% level.

If anything, this evidence argues that the role of relative prices in determining crop selection between corn and soybeans has gotten weaker over time, rather than strengthening. The plot of the variables is:



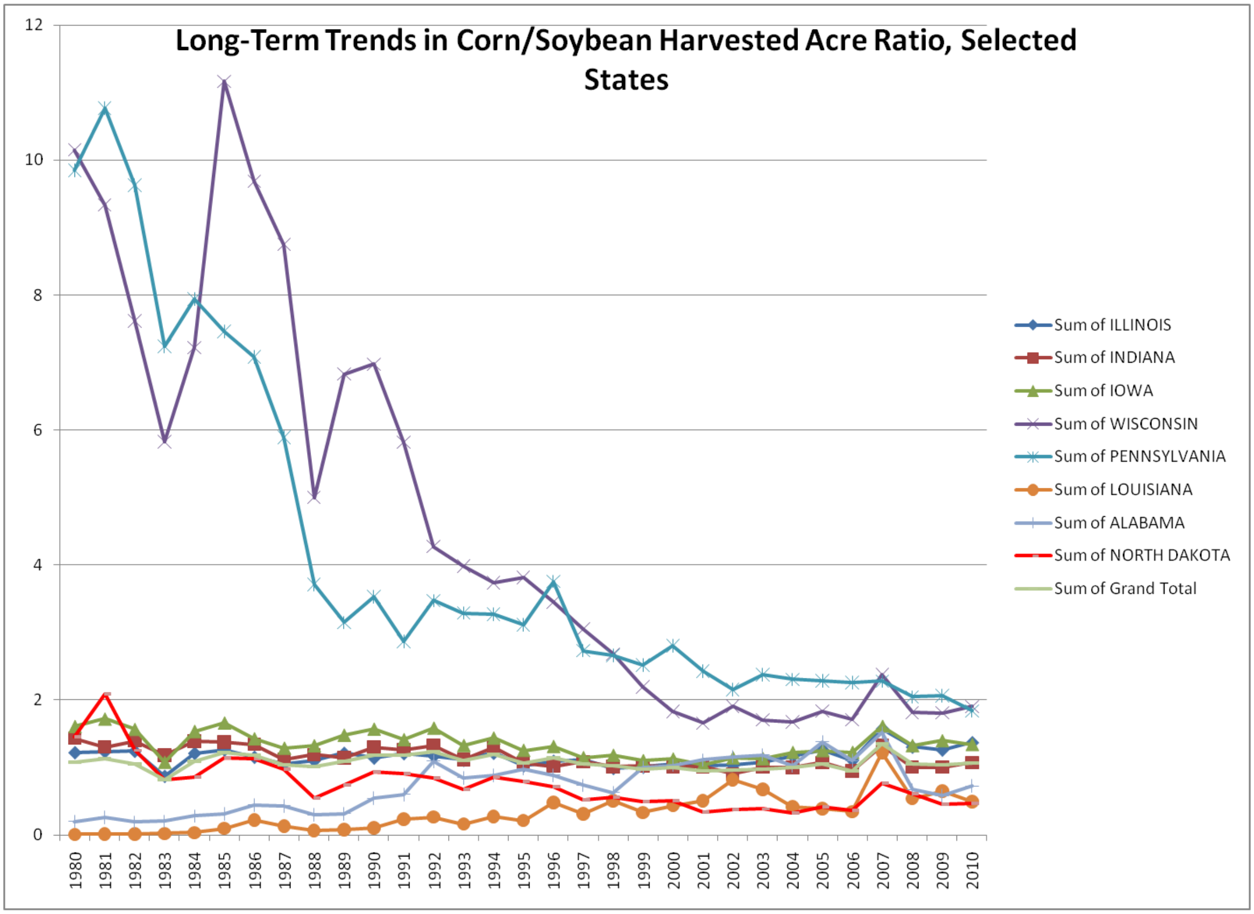
The falling ratio from 1949 to 1969 just reflects the establishment of soybeans as a major U.S. crop, and the subsequent relationship shows the price ratio leading the acreage ratio somewhat.

Tyner's hypothesis is that the influence of government programs on corn and soybean crop choice prior to the mid-1990s overwhelmed any response to relative prices. Increased planting flexibility after the 1996 Farm Bill supposedly let relative price responsiveness reassert itself as a factor in corn-soybean crop choice. However, the data above do not support such an increased hypothesis.

An alternative hypothesis is that, once soybeans were fully integrated into American cropping patterns after about 1970, the fundamental rotation between corn and soybeans became steady over time, with price spikes causing distortions from that rotation for a year one way or the other. To test this idea, I calculated the ratio of corn to soybean harvested acreage by state and averaged that ratio over 2000-2010, 1990-2010, and 1980-2010. I also calculated the variance in the ratio for each of these periods. States with high ratios of corn to soybeans (Texas, Florida, Pennsylvania, Wisconsin, Nebraska) have traditionally had dairy or livestock on feed and needed more corn than soybeans, which could often be purchased more cheaply as meal. The Cornbelt states of Iowa, Illinois, and Indiana maintain a near constant ratio around 1.00 with very little variance over time. States in the south (Alabama, Arkansas, Mississippi, Tennessee, Missouri) where soybeans were first introduced have gone the other way, reducing soybean acreage in favor of corn over time as more pest-resistant varieties and Roundup Ready corn made that cultivation with soybeans preferable over cotton-soybean rotations.

At least since 1980, the long-term rotations between corn and soybeans do not appear to be more heavily influenced by relative prices, but appear to be very stable or undergoing long-term secular trends related to other factors. These trends are graphed for selected states below.

| Ratios of corn to soybean harvested acreage over time | | | | | | |
|---|---|---|---|---|---|---|
| State | Average 2000-2010 | Average 1990-2010 | Average 1980-2010 | Variance 2000-2010 | Variance 1990-2010 | Variance 1980-2010 |
| TEXAS | 9.80 | 8.43 | 7.06 | 17.36 | 12.65 | 14.17 |
| FLORIDA | 2.69 | 2.36 | 1.84 | 2.31 | 1.44 | 1.57 |
| PENNSYLVANIA | 2.26 | 2.67 | 4.15 | 0.06 | 0.30 | 6.87 |
| WISCONSIN | 1.84 | 2.87 | 4.57 | 0.04 | 2.17 | 8.95 |
| NEBRASKA | 1.76 | 2.22 | 2.46 | 0.05 | 0.35 | 0.42 |
| GEORGIA | 1.35 | 1.26 | 1.04 | 0.17 | 0.12 | 0.19 |
| IOWA | 1.25 | 1.29 | 1.35 | 0.03 | 0.03 | 0.04 |
| ILLINOIS | 1.20 | 1.16 | 1.16 | 0.03 | 0.02 | 0.02 |
| KANSAS | 1.12 | 1.06 | 0.94 | 0.02 | 0.02 | 0.05 |
| ALABAMA | 1.04 | 0.94 | 0.73 | 0.08 | 0.07 | 0.14 |
| **Total U.S.** | **1.03** | **1.07** | **1.07** | **0.01** | **0.01** | **0.01** |
| INDIANA | 1.03 | 1.09 | 1.15 | 0.01 | 0.02 | 0.02 |
| MICHIGAN | 1.03 | 1.21 | 1.52 | 0.01 | 0.07 | 0.33 |
| MINNESOTA | 0.99 | 1.04 | 1.09 | 0.01 | 0.02 | 0.03 |
| SOUTH DAKOTA | 0.98 | 1.12 | 1.50 | 0.03 | 0.07 | 0.51 |
| KENTUCKY | 0.91 | 0.97 | 0.99 | 0.02 | 0.02 | 0.02 |
| MARYLAND | 0.90 | 0.87 | 1.03 | 0.01 | 0.01 | 0.10 |
| DELAWARE | 0.88 | 0.81 | 0.75 | 0.02 | 0.02 | 0.02 |
| OKLAHOMA | 0.87 | 0.72 | 0.56 | 0.05 | 0.06 | 0.09 |
| NEW JERSEY | 0.76 | 0.71 | 0.72 | 0.01 | 0.01 | 0.01 |
| OHIO | 0.71 | 0.76 | 0.82 | 0.00 | 0.01 | 0.02 |
| VIRGINIA | 0.67 | 0.66 | 0.69 | 0.00 | 0.00 | 0.01 |
| SOUTH CAROLINA | 0.64 | 0.59 | 0.52 | 0.01 | 0.01 | 0.02 |
| LOUISIANA | 0.59 | 0.45 | 0.32 | 0.06 | 0.06 | 0.08 |
| MISSOURI | 0.57 | 0.54 | 0.50 | 0.00 | 0.01 | 0.01 |
| TENNESSEE | 0.52 | 0.53 | 0.48 | 0.01 | 0.01 | 0.01 |
| NORTH CAROLINA | 0.52 | 0.59 | 0.67 | 0.01 | 0.01 | 0.03 |
| NORTH DAKOTA | 0.45 | 0.58 | 0.75 | 0.02 | 0.04 | 0.14 |
| MISSISSIPPI | 0.34 | 0.26 | 0.19 | 0.01 | 0.02 | 0.02 |
| ARKANSAS | 0.10 | 0.07 | 0.05 | 0.00 | 0.00 | 0.00 |


Long-Term Trends in Corn/Soybean Harvested Acre Ratio, Selected States
12
10
8
6
4
2
0
1980
1981
1982
1983
1984
1985
1986
1987
1988
1989
1990
1991
1992
1993
1994
1995
1996
1997
1998
1999
2000
2001
2002
2003
2004
2005
2006
2007
2008
2009
2010
Sum of ILLINOIS
Sum of INDIANA
Sum of IOWA
Sum of WISCONSIN
Sum of PENNSYLVANIA
Sum of LOUISIANA
Sum of ALABAMA
Sum of NORTH DAKOTA
Sum of Grand Total