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Reply to Taheripour, Delgado and Tyner (2020)

ABSTRACT

The present letter is aimed at providing a step-by-step comment on the criticisms raised by Taheripour, Delgado and Tyner, authors of the Response to Santeramo and Searle (2019). The paper by Santeramo and Searle (2019) analyses the responsiveness of the US soy and palm oil markets to price changes. The authors find positive cross-price elasticities of palm oil import with respect to soy oil price, and vice versa. Hereafter we will refer to the paper by Taheripour, Delgado and Tyner as TDT (2020) and to the paper by Santeramo and Searle as SS (2019).

1. Providing wrong and misleading data

TDT (2020) claim that SS (2019) provides wrong and misleading data. The authors have noted a minor error in translating units when presenting the market statistics in SS (2019), but those typos clearly do not alter the results nor the implications of the study. TDT (2020) also note that the shares in Table 2 do not add up to 100%. As stated in the title, this table reports “statistics of major oils and fats in the US market” and does not claim to be comprehensive. Hence, it should be obvious why the shares in Table 2 do not add up to 100%.

2. Improper theoretical justification

TDT (2020) notes that the theoretical model presented in SS (2019) does not explicitly include a resource constraint in the profit maximization problem and claims that this is necessary. This is not true from a theoretical point of view. While consumer theory defines the utility maximization as a constrained optimization, this is not the case for producer theory and it is not necessary to explicitly include land constraints in profit maximization. The theoretical model SS (2019) includes a matrix (Z) collecting all control factors: an unconstrained optimization using binding factors is equivalent to an optimization problem in which the constraints are explicitly represented. As a result the model in SS (2019), described in section 3.2, is generally sufficient. To be more technical, the profit maximization (cfr. Jehle and Reny, *Advanced Microeconomic Theory,* Prentice Hall, 3rd edition, pag. 146) is as follows:

$$\text{Max } p^*y - w^*x$$

$$\text{s.t. } f(x) \geq y$$

$$(x,y) \geq 0$$

where p stands for output price, y stands for output, w indicates input prices and x stands for inputs. The function $f(x)$ represents the production function. Jehle and Reny state that “*we may replace the inequality in the constraint by an equality,*” and that because $y = f(x)$, subject to having positive values of inputs, the maximization problem may be rewritten as follows:

$$\text{Max } p^*f(x) - w^*x$$

In short, the economic model is correct and it implicitly incorporates the potential limitations of land availability.

3. Data on palm oil

TDT (2020) claim that US palm oil imports are “a tiny amount.” This is not true. US palm oil imports in 2015 were 1.3 million metric tons. The lack of production in the domestic market justifies our assumption that net imports capture the quantity of palm oil supplied in the US market and thus imports are likely influenced by vegetable oils prices.

TDT (2020) also claim that palm oil import prices are affected by the global market, and thus data on imports and prices represent no production behavior. Indeed, all commodities, including other vegetable oils, are influenced by global markets, and yet econometric analyses are still performed and published, recognizing that no analysis can explicitly account for all global effects.

4. Unraveled estimated parameters and their corresponding statistics

TDT (2020) claim that SS (2019) does not report model diagnostics, nor tests the relevance of the instruments. SS (2019) is a short communication in *Energy Policy* and is constrained by length. The criticism that the first stage has been hidden on purpose is meaningless. The first stage suggests that the instruments are relevant. We computed F-tests equation-by-equation as well as on the full system. The results (reported below) shows that the null hypotheses that instruments (time trend, lagged temperature, lagged temperature squared, lagged precipitation and lagged precipitation squared) are equal to zero are rejected.

In addition, TDT (2020) incorrectly describes SS (2019), claiming Table 4 only reports p-values and no model diagnostics. As clearly indicated, Table 4 in fact reports standards errors, not the p-values.

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5. Limited number of observations and lack of variation

TDT (2020) argues that, in SS (2019), 25 observations have been used to estimate 16 parameters. This is incorrect. The careful reader would agree that in SS (2019) there are eight parameters (not sixteen) per equation. Each equation estimates one own-price elasticity, one cross-price elasticity, one constant term, and a few control factors. As further evidence that the results are robust, we estimate the model by excluding the statistically not significant parameters: results (reported below) are unchanged.

6. Source of variation in palm oil and soy oil prices

TDT (2020) argue that the high correlation of domestic and international prices should be taken into account and the links between the soybean market and the soy oil market should be considered.

The statement on the relationships among prices is useless. Economists are well aware that prices are interconnected (a property named "Law of One Price"), but this feature does not violate the fact that domestic production reacts to domestic prices. If the readers take literally the concern expressed in TDT (2020) that supply estimations should include all factors affecting prices, they would conclude that all economic studies that assess demand or supply elasticities without taking into account all prices in the global economic system are useless. Either one or the other is false: either the statement in TDT (2020) is so true to revolutionize decades of economic literature, or their statement is exaggerated and unfounded. Simply put, the approach taken in SS (2019) is standard and legitimate to assess own- and cross-prices elasticities.

The concern on potential lack of variation is clearly unfounded. If there is not enough variability in the data, the estimates would be statistically not significant: this is not the case in SS (2019). The production of soy oil rises by more than 70% in our dataset, with annual variations ranging between −7% and +13%. The palm oil price increases by up to 90% with annual variations ranging between −41% and +71%.

TDT (2019) also argue that we did not account for variation in the soy meal market. While it is possible that the soy meal market affects the soy oil market, Table 1 in SS (2019) shows that the proportions for soy oil production, exports and imports remain very similar across time periods, which suggest no substantial changes in soy crushing patterns.

7. Improper implemented instrumental variables

TDT (2020) argue that the implemented instrumental variable approach is unclear and improper because weather influences the production of soybean but not that of soy oil. The approach is in line with Roberts and Schlenker (2013), who test whether the shocks from derivative products (corn and soy, mainly processed into animal protein) have different influence on the aggregate food price than do primary products (wheat and rice, used directly for food), and found no evidence for concerns about instrumenting derived products with weather variables. In a similar fashion, we instrument the supply of vegetable oils, which are derived from commodities (e.g. soybean), with (lagged) weather variables and time trend. Another concern in Roberts and Schlenker (2013) is potential autocorrelation with the weather variables. We followed their approach to test for this problem and found that weather shocks display little autocorrelation in our analysis (shown in Table 3). Also in line with Roberts and Schlenker (2013), we included current weather shocks in the supply equation.

Table 1
F-tests on instruments.

Equation	χ^2	p-value
Soy	11.56	0.04
Canola	12.51	0.03
Palm	2.99	0.70
Full systems	28.15	0.02

Table 2
Stage two - statistically not significant control factors omitted.

	Soy oil	Palm oil
Price of soy oil	0.233***	0.995***
	(0.037)	(0.325)
Price of palm oil	0.162***	0.076
	(0.024)	(0.240)
Precipitation	−8.061***	*na*
	(0.840)	
Precipitation squared	2.905***	*na*
	(0.344)	
Temperature squared	0.006***	−0.011**
	(0.001)	(0.006)
Time trend	*na*	0.149***
		(0.019)

Standard errors are in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 3
Autocorrelation tests for weather variables (temperature and precipitation).

Lag	Temperature		Precipitation	
	χ^2	p-value	χ^2	p-value
1	4.64	0.03	0.23	0.63
2	4.78	0.09	0.37	0.83
3	5.79	0.12	0.39	0.94

Table 4
Stage two – equation-by -equation.

	Dependent variables	
	Equation 1: Soy oil	Equation 2: Palm oil
Own price	0.278***	0.145
	(0.086)	(0.353)
CF	YES	YES
	Equation 3: Soy oil	Equation 4: Palm oil
Cross-price	0.188***	1.035**
	(0.054)	(0.377)
CF	YES	YES

We included only statistically significant control factors. The equations of soy oil include precipitation squared and temperature squared. The equations of palm oil include temperature squared and time trend. Standard errors are in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1.

8. Correlation between soy oil and palm oil prices

TDT (2020) argue that our analysis could lead to insignificant results because soy and palm oil prices are highly correlated. This is clearly not a concern because our results were statistically significant, both when the model is estimated as a system of equations (*cfr*. Table 4 in SS 2019) and when the equations are estimated separately (*cfr*. Table 4 below).

9. Missing a big part of produced vegetable oils in US

The paper by TDT (2020) argue that the analysis in SS(2019) is limited and "misses a big portion of the market for vegetable oils".

No paper is comprehensive, and no paper will be. Santeramo and Searle (2019) is not an exception. The paper presents a targeted analysis on soy and palm, and does not draw conclusions on the entire set of existing vegetable oils. As such, the criticism in TDT (2020) is not appropriate.

10. Relationship between cross price elasticities, land use change, and US biofuel policy

TDT (2020) argue that SS (2020) cannot draw conclusions about land use change because SS (2020) does not model global land use change. While it is perfectly true that SS (2020) does not model land use change, that study does not overstate its conclusions. SS (2020) states: "The findings presented here suggest that the high land use change emissions associated with palm oil may be under-represented in regulatory analyses of US soy biofuel," referring to the US Environmental Protection Agency's (EPA) analysis for the RFS, which "found that only 3% of gross land expansion resulting from soy biodiesel demand is for new oil palm plantations." The finding in SS (2020) that palm oil supply is much more elastic than soy oil supply to soy oil price is highly relevant to EPA's modeling and thus has important implications for our understanding of the overall greenhouse gas impact of US biofuel policy.

TDT (2020) continue to argue that the land use change impacts of palm oil are small, citing their recent study (Taheripour et al., 2020) finding that "an increase in the US soy biodiesel by 500 million gallons increases the global area of cropland by about 37.3 thousand hectares" and cropland in Malaysia and Indonesia by "about 6.5 thousand hectares," which they argue "is negligible." Those authors know perfectly well that dividing a small number (cropland change in Malaysia and Indonesia) by a small number (the soy biodiesel shock) can still yield a large number. That these authors would emphasize the "small" numerator of a term without noting the similarly small size of the denominator is deliberately misleading. Furthermore, the development of the model used in Taheripour et al. (2020), GTAP-BIO, has been critiqued as being biased in favor of underestimating land use change from biofuel policies (Malins et al., 2020).

This letter responds to all concerns raised by TDT (2020). The criticisms that have been raised are unjustified and generally due to careless reading of the article. The further explanations presented in the present letter allows us to reinforce that the empirical analysis in SS (2019) is valid and robust. The use of instrumental variables allows us to identify supply responses to price changes. As stated in SS (2019), we conclude that US palm oil imports increase with increases in soy oil price.

References

Malins, C., Plevin, R., Edwards, R., 2020. How robust are reductions in modeled estimates from GTAP-BIO of the indirect land use change induced by conventional biofuels? J. Clean. Prod. 120716.

Roberts, M.J., Schlenker, W., 2013. Identifying supply and demand elasticities of agriculturalcommodities: Implications for the US ethanol mandate. Am. Econ. Rev. 103 (6), 2265–2295.

Santeramo, F.G., Searle, S., 2019. Linking soy oil demand from the US Renewable Fuel Standard to palm oil expansion through an analysis on vegetable oil price elasticities. Energy Pol. 127, 19–23.

Taheripour, F., Delgado, M.S., Tyner, W.E., 2020. Response to Santeramo and Searle (2019). Energy Pol. 137, 111159.

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