Comments on ***"Identifying Agricultural Demand and Supply Elasticities: Implications for Food Price Volatility",*** by Steven T. Berry, Michael J. Roberts and Wolfram Schlenker

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Overview of the paper

In their paper, Berry et al. (2012) use several relatively new approaches to empirically identify agricultural and supply demand elasticities for four major food grains (wheat, maize, rice and soybeans) at a global level. They use annual data from the FAO and USDA on production, area harvested, yields, and changes in stocks for the largest grain producing countries, and USDA data on crop and fertilizer prices. By focusing on global production, the authors are able to abstract from the complications of substitutability between grains and trade between countries, to focus more narrowly on global supply and demand elasticities. However, the main innovation in this paper – and in their previous paper (Robert and Schlenker 2009) – is to use past weather shocks to predict shifts in the demand curve in the current period (which are linked via stock adjustments), which in turn allows them to trace out the supply curve. As in their previous paper, the present paper uses deviations in yields from trends as their proxy for weather shocks, but in this paper they also develop a kind of "residual generated regressor" measured of shocks, which allows for serial correlation in the error term. Both techniques, however, yield quite similar estimates of the net supply/demand elasticity: $\frac{1}{(\beta_S - \beta_D)}$

After estimating this key parameter, they then conduct some basic simulations of how the exogenous change in demand for biofuels is predicted to affect equilibrium prices. While they could derive various estimates from their results, their preferred estimate is that the 2009 biofuels mandate has increased equilibrium prices by around 30 percent. They also extend their approach to see whether demand increases lead to supply responses on the extensive margin (land expansion) or the intensive margin (yield expansion, proxied by fertilizer use). In their penultimate section they consider some policy implications of their results. In addition to raising equilibrium prices they argue that the biofuels mandate is also expected to increase price volatility. They argue that one way to mitigate the adverse effects on volatility is to remove the ceiling on the trade of Renewable Identification Numbers (RIN). Like physical grain reserves, trade in RINs could be used to dampen price volatility. The paper also contains a rather separate exploration of how the 2012 drought in the United States may influence grain production this year. However, my main focus in this commentary is on the core of their paper, the identification and estimation of the "net supply/demand elasticity" above, and its application to the biofuels question. I will also tough upon some possible extensions of this approach.

Strengths of the study

If anything, this paper undersells its main strength somewhat, though it could perhaps also do a better job of outlining the limitations of the approach. The main strength of the paper if that it solves the identification problem, given that unobserved shifts in supply and demand (*u* and *v*) influence prices via the equilibrium identity. As the authors note in their previous paper, without correcting for the endogeneity of prices, the supply elasticity would be biased negatively, since unobserved positive supply shifts (*u*) would tend to reduce price, creating a negative correlation between *u* and price. For analogous reasons, the demand elasticity would be positively biased. Solving this endogeneity problem is rather important in the broader context. Previous research on related topics has tended to focus on either time series techniques for identification, or used simulation models (though even these must make use of some empirically estimated elasticities). Thus it is possible – perhaps even likely – that previous estimates of the impacts of biofuels on equilibrium prices have been biased.

Hence I think the paper should make more comparisons between its own results and those reported elsewhere. The authors do cite work by Hausmann, Auffhammer & Berck (Forthcoming) and Carter, Rausser & Smith (2012), but there is much more work out there, including results from various simulation models (e.g. IFPRI, FAO, and so on). As it happens, the two papers cited above seem to predict very similar changes in prices to the present paper, though it is not immediately obvious how similar the simulations are (this model is for four crops, the others seem to be for maize only). Without more in-depth comparisons between these results and other papers the reader is left with the impression that the IV strategy in this paper adds conceptual value, but doesn't really generate different results relative to other approaches. So one recommendation is to engage in a broader comparison of research results on the impact of biofuels on prices, to give readers a better sense of whether this approach really makes much difference.

I also note that in their original paper, Robert and Schlenker (2009) do compare SUR-based estimates (presumably biased by endogeneity) to their IV approach, and show that the IV approach does seem to produce less biased estimates. The present paper does less to motivate its main innovation, and this should be corrected. Similarly, the present paper does not do a great job of motivating its proxy for weather shocks, as opposed to actual weather shocks. Certainly, weather shocks are widely used as instruments in the development economics literature, but data paucity is likely a major issue in some contexts, particularly the global context. Again, Robert and Schlenker (2009) make note of that, but in the present paper a sentence or two justifying the use of a weather proxy would be helpful.

Limitations of the study

What the paper does and does not do

Upon reading the title, a reader might reasonably expect that new estimates of supply and demand elasticities could be used to answer a range of questions on the sources of food price volatility. Actually, though, the title is somewhat misleading because the paper is not about volatility, but about equilibrium prices. Excessive volatility is probably more about "disequilibrium" factors, such as speculation in futures markets (Gilbert 2010, Irwin, Sanders and Merrin 2009, Sanders and Irwin 2010), but also physical hoarding in the forms of precautionary purchases and export restrictions (Headey 2011). Research on these topics more accurately gets at the issue of short term price changes or volatility. This is not so much a criticism of the paper as it is a suggestion to identify the specific scope of the study quite early on (in the introduction), and to perhaps omit volatility from the title.

Making more use of the net supply/demand estimates

As I noted above, the main application of the model is to simulate the impact of exogenous changes in global demand. Because demand for biofuels stems from the energy sector and from government mandates, it can reasonably be thought of as an exogenous demand shock. So the authors' idea of simulating the effect of increased biofuels demand on global prices seems a sensible one. Specifically, they multiply their net supply elasticity of 5.8 by the change in the composition of demand for grains from food purposes to biofuels purposes, which is about 5 percent. This calculation implies an equilibrium price increase of just under 30 percent.

However, it is not clear from the paper where the estimate of a 5 percent point change in composition of demand comes from. In Table 1 I try to replicate this estimate with my own calculations from the FAO food balance sheets for 2000 and 2008. Specifically I look at sources of demand (or use) for cereals.[1] Unfortunately, the FAO data are unclear or where biofuels is classified; specifically on whether biofuels shows up under “processing” or “other utilization”. Table 1 shows that the category of “processing” sees its share of global grains demand increase from 4.4 percent of global demand to 8.1 percent, a shift of 3.6 percentage points, for a predicted price increase of 21 percent (with the net demand elasticity of 5.8). “Other utilization” sees its share increase from 2.7 percent to 4.2 percent for a change of 1.5 points and a predicted price increase of 8.6 percent. Together these two categories account for a 29.6 percent price increase which is very, very similar to the authors’ estimate. However, it is not clear that changes in “processing” and “other utilization” entirely relates to increased biofuels demand. More details on where the authors derived their 5 percent composition change from would be helpful.

Table 1. Changes in the composition of total cereals demand between 2000 and 2008, and predicted price changes

	2000	**2008**	**Change**	**Predicted price change**
Feed	37.3%	36.7%	-0.6%	-3.2%
Seed	3.5%	3.0%	-0.5%	-2.9%
Waste	4.3%	4.2%	-0.2%	-1.0%
Food	47.7%	43.8%	-3.9%	-22.5%
Possible biofuels categories:				
Processing	4.4%	8.1%	3.6%	21.0%
Other Utilization	2.7%	4.2%	1.5%	8.6%
Total of biofuels categories	***7.1%***	***13.3%***	***5.1%***	***29.6%***

Source: FAOSTAT (2012).

Perhaps more importantly, though, why is it the change in the composition of demand for grains that matters for the price calculation? After all, as Table 1 trivially shows, the share of demand allocated to biofuels has gone up, but the share of other demand sources has gone down, so the net effect of all changes in composition of demand on prices is zero (ignoring endogeneity issues of the different demand sources for the moment). I can only suppose that the authors view biofuels demand as an entirely new source of demand, and assume that all other sources of demand are more legitimate food needs (i.e. like a baseline). However, it arguably makes more sense to calculate the price increase from growth rates in biofuels demand, or specifically from share-weighted growth rates. I do this in Table 2 for 2000 and 2008. The data show that the growth rate of demand for processed grain outputs doubled over 2000-2008, the share weighted growth rate was 5.1 percent, which translates into a price increase of 29.4 percent. This is essentially the same as the result derived by the authors of this paper. “Other utilization” may also include biofuels (it is not clear from the FAO website) and this accounts for a 13 percent price increased.

[1] This does not include soybeans, though soybeans are not a major source of biofuels supply.

Together these imply the increase in biofuels demand raised equilibrium prices by 42.4 percent, which is substantially higher than the author's estimate of just under 30 percent. So the number reported on price responses would seem to depend upon the method of calculation: i.e. whether composition of demand is used, or absolute change in demand.

Table 2. Growth rates in absolute levels of various types of cereals demand between 2000 and 2008, and predicted price changes

	Unweighted growth	Weighted growth	Predicted price change
Feed	16.2%	6.0%	35.0%
Seed	1.4%	0.1%	0.3%
Waste	13.3%	0.6%	3.4%
Food	8.4%	4.0%	23.1%
Possible biofuels categories:			
Processing	114.1%	5.1%	29.4%
Other Utilization	82.1%	2.2%	12.9%
Total of biofuels categories	***101.9%***	***6.3%***	***42.3%***
Total of all categories	15.1%	18.0%	104.1%

Source: FAO (2012).

Of course, the predicted price increases in Tables 1 and 2 only make sense if the changes in demand are exogenous, or specifically, if they are independent of prices. I noted before that the application of their model to biolfuels demand is probably reasonable because we think of this demand as exogenous, driven by higher fuel prices, new technologies and government mandates, rather than by cereal prices. But why not also explore how changes in other sources of demand have affected equilibrium prices? After all, trends in food and animal feed demand are also driven by relatively exogenous factors like population growth and income growth. Moreover, factors such as "changing Asian diets" have often been cited as important underlying determinants of the food crisis. Interestingly, Table 2 shows that changes in demand for food over 2000-2008 predict a 23 percent increase in equilibrium prices, while demand for animal feed predicts a 35 percent increase. In other words, growing feed demand accounts for at least as large a price increase as growing biofuels demand. Importantly, these estimates would net of any exogenous drivers of production growth over this period. The net supply/demand elasticity incorporates an average supply response to price increases (β_s) but there is no guarantee that actual supply increased by this amount over the period in question.

Turning back to the "changing diets" hypothesis, in earlier work my colleague and I had expressed some skepticism about the role of increased feed demand in price increases (Headey and Fan 2010). Table 3 gives contrasting evidence on this front. On the one hand the data in the last column suggest that Asia's share of global animal fat consumption increased substantially over this period by 4.6 percentage points. On the other hand Asia's share of global animal feed demand was unchanged, with the only action a decline in the America's share and an increase in Europe's share. This discrepancy could be explained by increased meat imports in Asia. The FAO data suggest that live animal exports from the rest of the world to Asia increased by about 70 percent over 2000-2008. In any event, the authors could go beyond the biofuels issue to also look at other sources of increased demand.

Table 3. Shares of total feed demand and total animal fats demand, 2000 and 2008

	Consumption of animal feed			Consumption of animal fats		
	2000	**2008**	**Change**	**2000**	**2008**	**Change**
Africa	3.3%	3.6%	0.3%	3.4%	3.6%	0.2%
Americas	36.5%	32.3%	-4.2%	18.1%	17.3%	-0.8%
Asia	28.8%	28.8%	0.0%	36.3%	41.0%	4.6%
Europe	30.2%	33.7%	3.5%	41.3%	37.0%	-4.3%
Oceania	1.2%	1.5%	0.4%	1.0%	1.2%	0.2%

Source: FAO (2012).

Are yield shocks a good proxy for weather shocks?

The notion that yield shocks are a good proxy for weather shocks could be tested a bit more, as the whole identification strategy rests on the exogeneity of this proxy. I would expect that in developed countries with relatively stable macroeconomic environments, yield shocks are indeed a good proxy for weather shocks (and surely some previous research has linked the two). But in some developing countries I am not so sure, since macroeconomic policies could drive yield changes. It seems to me that the study is implicitly assuming one world price for outputs and inputs. Over the long run this may hold, but in the past 10 years lots of countries have engaged in policies to prevent full price transmission of both international output prices and input prices. So for some countries yield shocks could represent policy shocks, not weather shocks, such as removing a fertilizer subsidy, re-imposing it, changing it again, and so on.

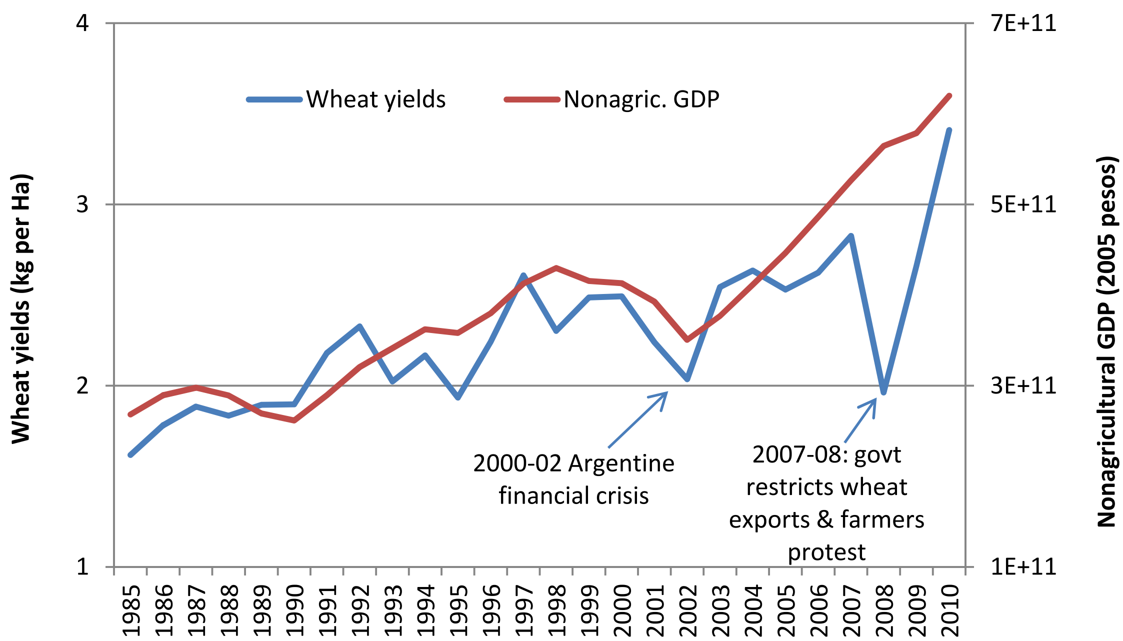
How important could this problem be in practice? The authors use a sample that covers 1961 to the mid to late 2000s, for grain producers that produce at least 1 percent of the world supply of the grain in question. This excludes many small countries, but it does include some developing countries (or country "episodes") with very unstable macroeconomic policies and unstable agricultural policies: hyperinflationary Brazil in the 1980s, Nigeria with its oil booms and busts, Argentina in the past decade with very frequent shifts in agricultural policies under the current regime, and former Soviet Bloc countries that underwent massive changes in both agricultural policies and broader macroeconomic policies. Although weather could well explain yield shocks, I would be somewhat concerned that policy shifts ad macro shocks also account for some amount of the variation in yield shocks. Figure 1 gives an example from Argentina. The figure shows wheat yields, and also nonagricultural GDP trends. Over 2000-02 Argentina experienced a massive financial meltdown, with GDP contracting by 15 percent over those two years. Perhaps non-coincidentally, wheat yields declined by about 20 percent. So macroeconomic shocks seem to matter (of course, I have not controlled for weather fluctuations but the evidence in Figure 1 is still suggestive). In 2007-08 the wheat sector received a different sort of shock in the form of a series of disastrous attempts by the Argentine government to curb domestic inflation via export restrictions. These restrictions are well described in Dollive (Dollive 2008),[2] who also mentions a

[2] The quote below from Dollive (2008) gives some idea of the course of events in 2007 and 2008:
Argentine exporters also faced export restraints and domestic problems, which stymied the export of wheat and other agricultural products. Beginning as early as March 2007, Argentina closed its export registry for wheat, effectively restricting wheat export9 The registry was reopened in November. The exporters rushed to register, expecting the change to be temporary. Based on a law put into effect that month, exporters had to complete their export within 45 days of registration, as opposed to the 365-day window previously in effect. The convergence of these factors caused a large amount of grain to be exported in

weather shock in the form of frost. Perhaps poor weather partly explains the 30 percent decline in yields in 2008, but the interference of government policies is another strong candidate.

So the Argentine case shows that macro shocks are a possible explanation of yield shocks, even in a relative advance net grain exporting country like Argentina.

Figure 1. To what extent are Argentine wheat yields explained by macroeconomic phenomenon?



Source: GDP data are from the UN (UN 2012); Wheat yields are from FAO (FAO 2012).

How important could this problem be in a more general way? To examine this I merged FAO data on annual yield growth with annual growth in nonagricultural GDP. These annual growth rates could also be considered shocks and should give similar results to the more elaborate process of creating shocks by de-trending the various series. Note that I use data for a large sample of countries, but I excluded very small countries (e.g. island states) where weather shocks could simultaneously influence yields and nonagricultural GDP (e.g. tourism). I also split the sample into developed and developing countries. The results are reported in the table below. As expected, I find that nonagricultural GDP growth has no effect on yields in richer countries, although inflation has a negative effect on yield in these more developed countries (though the effect is small and imprecisely identified). In low and middle income countries, however, nonagricultural GDP growth is positively associated with yield growth, and the effect is significant at the 1 percent level. The size of the elasticity is just under 0.10, which is relatively small. Yet

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January of 2008. Later in November, Argentina raised the export taxes on soybeans (35 percent), maize (25 percent), and wheat (28 percent). In December, Argentina closed the registries indefinitely in order to increase domestic grain supply and to assess the crop damage of a recent frost.33 In January, Argentina promised to partially open its wheat export registry by limiting the export of wheat to 400,000 MT per month.34 However, by mid-February, Argentina had once again closed the registries. After several closure extensions, Argentina announced it would end the ban in May. In addition, in June, Argentina allowed 1 million MT of wheat to be exported, half of which went to Brazil. To contest the export taxes and other domestic policies, Argentine farmers led various strikes from March until June 2008. The strikes complicated exports and further tightened the local supply of wheat.

given the possibility of attenuation bias related to substantial measurement error of both GDP and yields in developing countries, it is possible that more accurate data would yield an even larger coefficient. In any event, Table 4 casts doubt on the idea that yield shocks only represent weather shocks. Were I able to measure agricultural policy shocks with cross-country data, we might also find that these too explained yield shocks. So although the idea of using weather shocks to instrument for supply and demand seems sound, it is not obvious that yield shocks are a sufficiently good proxy for weather shocks.

Table 4. Do macroeconomic shocks predict yield shocks?

Sample	Developed countries	Developing countries
Number of obs.	791	4401
Growth in nonagricultural GDP	-0.17	0.08***
Growth in GDP deflator (inflation)	0.02	-0.00
Growth in exchange rate	-0.07*	0.00
Constant	0.00	0.01

Source: UN (2012) and FAO (2012).

Notes: The dependent variable is the annual percent change in yields. All dependent variables are also estimated in annual percent changes. The estimator is the robust regressor (rreg command in Stata), which attaches less weight to unduly influential observations.

Small sample size and other data issues

Like many macro papers that use cross-country data, this paper does not pay much attention to data quality. The main source of data in the study is FAOSTAT. Despite their best efforts, FAO is essentially required to report the data that individual countries give them, and even data that individual countries do not give them! This means that for many countries the quality of the data is very poor. Since this study deals with global data, inaccurate data in relatively small developing countries is less of an issue that data errors in larger developing countries. However, of particular concern, in my view, is the FAO series on stocks, or changes in stocks, which is a key variable for this approach as it is used to trace out the demand curve. And of special concern for that variable are the observations for China, who (to my knowledge) do not report stocks to the FAO or USDA. Hence this estimated series of Chinese stocks could induce a major source of measurement error into global stocks. A related problem raised by Timmer (2009), is that global rice stocks are generally much more difficult to estimate than other grain stocks. This is partly because China is a major holder of rice stocks, and partly because rice stocks are held by millions of Asian smallholders, and their holdings are not accurately reflected in national statistics.

What are the implications of this measurement error? It is difficult to say, because we know little about how accurate or inaccurate FAO and USDA estimates of global stocks are. There are grounds to think that it matters, though. The table below is drawn from my study with Shenggen Fan (Headey and Fan 2010) on the 2008 food crisis. When we were deliberating on the role of stocks in causing the food crisis we argued that separating out China from global trends could indeed by important given the aforementioned data problems for China, and because China is only partially integrated in the global grains trade. The table shows that that China's stock levels are large enough to significantly impact global trends in stocks. It is possible that stock trends in the former USSR were also reported with substantial error. Hence I urge the authors to consider how this measurement error could impact their estimation, and to make readers aware of the potential problems of the data.

Table 4. Trends in stocks to use ratios with and without China

Commodity	Country	Stocks / (consumption + exports) 1990–2000	2005–08	Change
Maize	World	26	14	-12
	World, excluding China	12	12	0
Rice	World	33	17	-16
	World, excluding China	14	13	-1
Wheat	World	27	18	-9
	World, excluding China	19	14	-5

Source: Headey and Fan (2010).

Very much related to the aforementioned issue is the problem of aggregating up to the global level, and aggregating across periods, and across crops. Essentially the paper is deriving net cereal demand elasticities at the global level from data over 1961-2003, and then using these elasticities to predict cereal price responses to surging biofuels demand in the late 2000s. I think it's fair to say that the current global food system does not look very much like the food system of the 1960s and 1970s. For one thing, Russia and China are now much more integrated into the global food system than they there in the 1960s. Indeed, Nixon's détente with the USSR and China in the early 1970s was arguably one of the causes of the 1974 food crisis. That said, China still remains poorly integrated for some crops, particularly rice.

From the objective of predicting a global supply response the grouping together of countries with high degrees of integration in global markets (and hence, presumably, higher supply responses) and countries with low integration (and presumably lower supply responses) does not matter. Though beyond the scope of the present study, I note that we should also be interested in supply response across countries and across crops, because this would improve our understanding of the global food system. For example, in a previous paper, myself and coauthors looked at supply response to the 2008 food price hikes for maize, rice and wheat, by major consumers and net exporters (Headey, Fan and Malaiyandi 2010). We found significant heterogeneity by crop and by the consumer/exporter dichotomy. So while the aggregation to the global level does not induce any biases, it is also not very informative in some ways.

In contrast to spatial aggregation, however, intertemporal aggregation may be an important source of bias. If previous global food regimes are different from today's regime (i.e. if there are structural breaks), then the paper's out-of-sample projections may be inaccurate. Unfortunately, the use of global annual data creates a very restricted sample size for the authors (e.g. 46 observations), meaning that any tests for structural breaks may not be very powerful. Even so, such tests should be conducted.

A related question is whether the arrival of a very new source of demand – biofuels – produced market responses that were somewhat different from the norm. As the authors themselves note, the surge in biofuels demand greatly expanded crop area, which might suggest that supply response was strong relatively to historical experience (a conjecture only). On the other hand, awareness of this huge new source of demand could have induced increased speculation in financial markets, and increased panic and precautionary purchases in spot markets. These market responses pertain more to disequilibrium price volatility than equilibrium prices. Study disequilibrium phenomenon comes with inherent difficulties – small sample sizes for one thing – but this is an area where we need much more research.

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