PEER REVIEW REPORT

OF

AN ECONOMIC MODEL FOR ROUTINE ANALYSIS OF THE WELFARE EFFECTS OF REGULATORY CHANGES

V. 3.00 April 20, 2005 Draft

JUNE, 2007

USDA Animal and Plant Health Inspection Service

Riverdale, MD

Peer Review Report

Reviewers No. 1 and No. 2

To: Jim Schaub

Re: APHIS's Economic Model for Routine Analysis of the Welfare Effects of Regulatory Changes v 3.00; April 20, 2005

From: Scott Malcolm and Clare Narrod

Under review is documentation that uses a partial equilibrium welfare analysis to measure the effect of regulatory changes on the economy. Specifically, it uses estimates of supply and demand elasticities and current prices and quantities to measure shifts in supply and demand resulting from a regulatory change. From these shifts, changes in welfare can be computed. Presumably, alternative regulatory options could be evaluated under this framework to assess the impact of each. We have some general comments regarding the presentation, and follow these up with specific concerns.

General comments:

For the most part the documentation is straightforward and easy to read. As part of the purpose of the documentation is to explain what is going on to non-economists it would be useful if the notation was explained in more words; perhaps a table would be useful. Similarly it would be useful if examples were given so that non-economists could see how the model described here would capture the impact of potential policies on various sectors. The document includes examples sometimes, but for the most part it does not. On an editorial note, the equation numeration is confusing (equations 1.1 to 5.4); the first digit of the equation number should refer to the "family" of equations, rather than the second digit. It would have also been useful if the spreadsheet had been included so that we could actually see what was programmed, as this is often where there are discrepancies; thus, it is difficult to see if they actually captured what they say they have.

Though in the introduction it states that it cost-effectiveness model or different control strategies…we are not clear that it does this. It would be useful for them to define what they mean by cost-effectiveness as others may use it differently; it seems to us that they are referring to cost-benefit analysis.

It would be useful if some attention was paid to the subject of sensitivity analysis. Clearly, the accuracy of the model is dependent on the estimates for the various elasticities, prices and quantities. Is the spreadsheet set up to facilitate changes of model parameters?

There are a number of minor typographical errors and omissions that a careful editing should take care of.

Specific comments:

Under purpose and potential applications:

We suggest that they alter what is written somewhat. We suggest that paragraph one begins by stating why economic analyses are constructed at the departmental level and relating that to what is required under the OMB guidance as how to do cost-benefit and cost-effective analysis instead of stressing what is of interest by the academic community (Lichtenberg and others page 4 paragraph 2).

Suggest that paragraph two could then discuss the purpose of the various types of models done in a regulatory context (what is listed at on page 4, paragraph 1). Though much is said here it is not clear what they mean by each…in the written part they discuss "relative cost" or cost-effectiveness of different programs, then they go to list benefit costs and the reason to provide methods of analyzing changes in polices and actions. It is not clear to us what they are refereeing to in terms of cost-effectiveness analysis…Intuitively we would think that cost-effectiveness in the context they are describing…i.e., "cost-effectiveness" of different control measures of VS program diseases they would be evaluating the various measures in terms of cost-effectiveness of risk reduction…here it assumes that the control method is effective and they are only measuring it in terms of impacts on society.

Suggest that paragraph three remains as it is discussing the market and equity effects of policy changes or activity captured in the model.

Suggest that paragraph 4 expands on how each of these potential applications may be used in the model. We would actually be interested in knowing if the cost-effectiveness in terms of risk could be captured in a variation to the model to provide policy makers with an understanding of the effectiveness of various policy options they are considering in terms of risk reductions not in terms of economic effects on society.

Under analytical methods applied to the model:

In the discussion of the partial equilibrium welfare model, the authors state that this model expands on methods discussed and applied by Lichtenberg et al, Ebel et al, and Forstyhe et al, but the readers have no idea of what these other authors did and how this work expands from previous work, if it does. We assume it expands on what the others did because there were limitations to the models referenced. It would be useful to list these limitations and how this model addresses them.

Page 6 it would be useful if they described how the subgroups Q_k are defined, what the authors mean by "approximations of small changes for any functional form", and what they mean by (K) regions. It would also be useful if they defined their entire notation under market equations … eg) Q_m, a_m, Q_d, b_d, Q_e, and b_e. Also, will it be clear to a user of this model what is meant by a "small change"? Is there any guidance as to the extent of changes that can be accurately modeled using this approach?

Page 7 -9 This is fairly straightforward, however it would be useful if they put words to some of their notation for those unfamiliar with economics could understand. It would be useful if this was related to an example throughout so the reader could see what how it would work. Suggest that the authors renumber their equations, currently it is very confusing.

Page 10, It is good how they use examples here to convey cross-commodity effects. It would be useful if equations 7-10 were accompanied with verbal explanations, focusing on their intuitive plausibility. Also, explain why the other shifters are set to zero. (Also on page 12). Is this a critical assumption, or just a mathematical convenience.

Page 11, Do the sum of the shares alpha i equal 1?

Page 25-26: It is not clear how the discussion of modeling shifts over time fits in with the static model described in the preceding pages.

Peer Review Report

Reviewer No. 3

Comments on "An Economic Model for Routine Analysis of the Welfare Effects of Regulatory Changes,"USDA, APHIS, September 22, 2005

The model represents a substantial amount of work and appears to be adaptable to welfare analysis in a variety of situations. Positive features included derivation of virtually all equations and comments on how different features of the model could be implemented. It appears relatively straightforward to implement, although some additional explanation may be needed. At times, it was unclear which variables were exogenous and which were endogenous. Of course, this depends upon the purpose of the model and could conceivably change for different uses of the model. Still, the paper could benefit by discussing this issue more explicitly and by describing in more detail the typical policy issues that the model will be used to evaluate. Specific comments on each section are shown below. A caveat should probably be included stating that this model is for policy analysis, not forecasting, and that some variables that will influence actual prices are not included in the model.

Base Model

The base model is clear with domestically produced domestic supply, supply of imports in the domestic market, aggregate domestic demand, and export demand for the domestically produced product, and a model closure equation. Each (inverse) supply and (inverse) demand equation has a shifter, which seems like a straightforward way to handle shocks while allowing other variables that would theoretically enter these equations to remain constant unless affected by the shock. The closure (domestic production of good i + imports of good i - exports of good i = domestic demand for good i) appears logical.

1. I didn't understand the reason for the K domestic supply equations. Are these individual producers whose individual supplies will be summed to make an aggregate market supply function? The introduction states that the model is nonspatial, but the section on market equations states that there are K domestic market regions. Obviously the model will be more parsimonious and easier to implement if there is one equation for the aggregate supply of domestically produced goods. If one equation is inadequate, then the reasons for it should be stated and the additional equations should be explained.

2. I was somewhat concerned that there was only one price variable. In a perfect-substitutes model like this one with an integrated national market, it is reasonable that the supply of domestically produced goods, import supply, and aggregate domestic demand all have the same own-price variable, but taxes or tariffs could separate the demand price from the supply price. If the domestic market consists of K regions, each region could have a different price. Also, the price in the export demand equation could be different. This approach is adequate if there are no major trade barriers and markets are integrated to the extent that the law of one price holds worldwide. For many applications domestic prices will likely differ from foreign prices or world prices, and consideration should be given to adding more price variables to the appropriate equations.

Solving the System

This section was good. The math checked, and the resulting equation 6 appears useful (although if some of the earlier suggestions, such as adding price variables, are taken, the equations will change.)

Modeling cross-commodity effects and the derived demand for inputs

My comments on these sections are combined. My main comment concerns the approach, but first a minor comment is made. The math checked on all equations including no. 10 and no 14, although I wasn't sure why a minus sign appeared in equation 10.2 given that it wasn't in equation 10.1.

The setup includes supply and demand shifters that could be interpreted as any variables other than price or quantity (which are already included) that might appear in the supply and demand equations. My main comment is that this shifter setup is not directly and fully exploited and that there may be no need to derive equations 10 and 14 because all relevant information already appears in equation 6.

For example, the demand shifter could be the price of a substitute product and its associated cross-price elasticity, or the supply shifter could be an input cost shock (together with an elasticity). These would reflect exogenous changes to the model. Suppose you have a demand equation that includes the price of a substitute and a cross-price elasticity. There is some exogenous policy change that affects the price of the substitute product; this is equal to db_d in your setup. The cross-price elasticity, actually the cross-price elasticity divided by the own-price elasticity because this is an inverse demand function, is equal to $\partial P^d / \partial b_d$ in your setup where the superscript indicates that this is a demand price. It appears that what is likely to be most interesting is the change in price of the target good, which is given by equation 6, with which an associated new equilibrium price and quantity could be derived following the policy shock.

My suggestion is calculating dP directly via equation 6 instead of $\beta_d = \partial P / \partial b \, db$ because it seems that dP in response to some exogenous change is exactly what is desired, not the latter expression. The suggestion is also more straightforward. Once dP is known, other relevant variables can easily be calculated.

Similar amounts of information are exogenous in each approach. Equation 10 has expressions for the own-price and cross-price elasticities and a percentage change in the price of the substitute. The suggestion includes the own-price and cross-price elasticities and an absolute change in the price of the substitute. The absolute change could be converted to a percentage change by multiplying the numerator and denominator by the 1/(price of the substitute), or the entire system could be converted to percentage changes, which could be useful, by taking logs and calculating logarithmic derivatives. The suggested change would permit easy calculation of multiple effects on price by simultaneously including expressions for any exogenous factors that might be nonzero in the numerator of equation 6. The individual shifters could also express more complicated shocks.

Trade

The comment in the previous section about the use of equation 6 applies to this section, as well. When reading the base equation, I thought export demand would be a generic export demand for the rest of the world, so I was surprised by comments in the first paragraph page 13 about each country having a rest of the world component to account for trade diversion. If the focus of the model is mainly on the domestic market, just having one foreign component may be adequate (see below). Also, in contrast to the initial setup, a distinct foreign commodity price was specified in the excess demand equation (no. 16), which could be more realistic for many situations.

Obviously the model will have to be adapted to the needs of its users, but it would fit with the base model better if there were just one foreign entity–the rest of the world. It would simplify the model and eliminate the need to deal with trade diversion as all imports would be directly included in the single import supply function.

If multiple countries or a single country and the rest of the world need to be included, it would be preferable to have separate import supply or export demand functions for each one. Expressions for trade diversion (equations 21.1 and 30.1) and trade displacement (30.2) do not fit in well with the base model. These expressions, attributed to Armington, are based on a view of imports of similar goods from different sources as being imperfect substitutes. Thus, the Armington substitution elasticities would have a counterpart in the cross-price elasticities of a demand equation. The Armington approach is usually based on a two-stage budgeting process and the constant elasticity of substitution functional form, which is different from the perfect-substitutes model with linear supply and demand here. Implementation of this approach would also require specification or calibration of the Armington substitution elasticities. The same issues could be addressed in this model framework by including supply equations from different sources that include prices from all markets, so that supply can be shifted as prices change.

Many trade models make the small country assumption when a country's imports of a particular commodity are judged to be too small to influence the world price of that commodity or when its exports are considered too small to influence world price. In these situations, the small country is modeled as facing a perfectly elastic supply of imports and a perfectly elastic demand for exports. While obviously not always appropriate, consideration should be given to applying this assumption where valid because it reduces the number of parameters of the model.

It is common in modeling exercises for parameters to proliferate and to calibrate some parameters within the model. While this approach is practical and sometimes necessary, there is danger when, say, the calibrating equations are based on a single realization of volatile processes. This is a potential problem for calibration or derivation within the model of the export demand and import supply elasticities. Exports and imports, especially from individual countries, are often highly variable. If reliable econometric estimates are not available, it may be desirable to base the calibration of these parameters on several years of data instead of a single year.

Deriving the Surplus Equations

This section was clear. Equation 53 was a good succinct but general purpose statement of the change in producer surplus. Two suggestions for improving readability for non-economists: 1) note that the elasticity (p. 23) is the elasticity for the base case (because the functions are linear, the elasticity is non-constant), 2) note that after a shock that the new equilibrium price and quantity are found by equating supply and demand.

Also, because consumer surplus is an exact measure of welfare change only under special circumstances, the use of equivalent or compensating variation should be considered. Usually one can derive an expression for the Hicksian demand and thus equivalent or compensating variation given a Marshallian demand. If it is decided to continue with the consumer surplus measure, it should be stated that in these circumstances that it is believed that consumer surplus represents a good approximation to consumer welfare changes.

Modeling Shifts over Time

This section seemed generally ok to me, but it was not clear what “perpetual welfare changes”meant or how they are calculated. Are they changes that might result from an infinite horizon model, or is it an instantaneous jump between the initial state and the final state, or something else? Why couldn’t the time effects be compared just by estimating the individual welfare effects for each period and discounting them back to some initial period? Does the model implement these features?

Peer Review Report

Reviewer No. 4

PURDUE
UNIVERSITY

DEPARTMENT OF AGRICULTURAL ECONOMICS

February 16, 2007

Frank Fillo, Ph.D.
Policy and Program Development
Animal and Plant Health Inspection Service, USDA

Dear Frank:

I am writing to report my review for APHIS of the manuscript "An Economic Model for Routine Analysis of the Welfare Effects of Regulatory Changes." I will attempt to follow the guidelines laid out in your letter and in attachments to that letter. Those emphasize that my review should consider whether the approach outlined in this document is economically sound, and whether the documentation is complete. My assessment of the soundness of this approach will be guided by the objectives laid out in its introduction and the extent to which this modeling framework reaches those objectives, relative to other approaches common in our profession. My assessment of completeness will also consider the extent to which issues related to the modeling choices made in implementing this framework have been explained.

In conducting this review I compared modeling choices in this document to the approaches taken in a variety of different modeling efforts. This approach is most like that employed in early versions of the SWOPSIM model developed by Vern Roningen at the Economic Research Service, USDA(Roningen, Sullivan and Dixit). Agricultural policy models at places like Iowa State-Missouri (FAPRI), Texas A&M and Tennessee are also relevant, if more complex and so more complete than this framework. Trade policy modeling is now often done utilizing computable general equilibrium (CGE) models such as GTAP at Purdue and the World Bank's Linkage model. Each of these models addresses a purpose similar to the modeling framework in this document, and exhibits features and problems related to trying to design a "routine" analytical framework. Documentation of those models would likely be a source of information for an analyst employing this framework, and would suggest alternative specifications of a model as well.

My overall assessment is that the methodology in this manuscript faithfully implements basic microeconomic theory. But it would not get an analyst very far in carrying out a specific assessment of APHIS policies or interventions. In terms of the goals outlined for economic analysis, it meets relatively few, and leaves the more difficult parts to be assessed outside this framework. The documentation is incomplete in the sense that there is no discussion of the modeling choices made, nor of alternatives that might be preferable in a particular situation. Analysts who would use this framework are likely to confront often such choices when applying this framework. It is also incomplete in that it explains how specific formulae for spreadsheet cells are arrived at, but does not explain how these are put together to form a "multi-market model" and does not provide insight into the crucial steps of determining just how policies or interventions impact producers, nor how costs of policies are determined. Comparison to existing models used by other government agencies (particularly the Economic Research Service of USDA) suggests there are more modern, more flexible and less restrictive approaches now also implemented in spreadsheets which can more fully accomplish the goals

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stated here. The review below uses specific concerns to show how I arrived at this overall assessment.

The introduction in this manuscript clearly states the purpose and goals of this modeling framework. It is designed "to have and maintain a routine systematic method of analyzing the "relative cost" or "cost-effectiveness" of different strategies or control measures of Veterinary Services program diseases… A key goal for the model is reducing turnaround time for analyses." (Forsythe, page 4) To make such an assessment it indicates there are four components:

(1) The yield or unit cost effects of the changes on affected livestock operations,
(2) How market prices and quantities adjust to these effects,
(3) How consumers and producers are affected by the adjustments, and
(4) Government expenditures required to make the changes. (Forsythe, page 4.)

The framework to accomplish these goals is described as "a multi-market, non-spatial, partial price equilibrium, welfare model." (Forsythe, page 5.)

In my view this framework addresses some but not all of these goals. As described, it appears to be a single market model with calculations of impacts from other markets, imposed as exogenous shifts in the equations of the single market modeled. There may be ways to implement the framework described here as a multi-market model. That would require explaining how the spreadsheet framework and equations handle feedback from the central market under investigation back to the secondary markets, which could in turn mitigate the effects modeled here on the primary market. That is either not done, or not explained. Given the nature of the model described, as analytically deterministic, my guess is that it is not done. The documentation suggests there are several sheets pertaining to several markets, but across sheet linkages are never explained. This is one key aspect in which this documentation is quite incomplete.

Given that shocks from veterinary disease programs enter this model as exogenous shifters, and since there are no policy variables in the model, it captures neither item (1) nor item (4) above. The effects of policies on producers directly are assessed outside this framework, and simply imposed as supply shifters on it. Any cost assessments appear to be done in that step as well, and not within this model. No equations to calculate government costs are reported. This model therefore cannot be used to look at alternative policy instruments, as that is all done outside the model.

What this modeling framework does do is to calculate standard welfare measures and overall market price and quantity changes as a consequence of assumed shifts in regional supply functions. This is also one purpose of similarly designed modeling frameworks, although most alternative models would more explicitly incorporate policy instruments and calculate government costs directly within the model. I was quite surprised at the absence of either domestic or trade policy instruments in the equations reported in this document.

Most of the space in the current document is devoted to derivations of equations to be used in the modeling framework to calculate equilibrium prices, and then quantities, that follow from the externally determined supply shocks. The methods used are basic intermediate microeconomics, and come largely from the textbook referenced in the document (Hirshleifer). In this aspect the documentation is quite complete. Full details of the derivations of the basic equations utilized in the spreadsheet are provided. Since these are well known results, it is not hard to see that the equations arrived at are correct derivations of the relationships stated. The model starts with supply and demand functions, written in an inverse form. It examines how elasticties may be incorporated into linearized versions of those relationships. It then shows how equilibrium market prices may be computed from assumed elasticties and exogenous shocks to the original inverse supply or demand functions. Prices may then be substituted back into relationships that describe supply and demand in normal rather than inverted form. The paper then goes on to show how changes in secondary markets or input markets can be imposed on the inverse forms of the supply and demand functions. It presumes that the shocks in those secondary markets will be known in terms of quantity rather than price (which will sometimes but not always be the case). This is somewhat harder using inverse forms, as shifters due to quantity changes in basic behavioral equations must be computed in their price equivalent form.

Problems with this framework come from the decisions made at the beginning in terms of what it does and how it is implemented, and not from these derivations.

Much of what is done here is unnecessary given recent advances in spreadsheet technology, specifically the capability of solving non-linear simultaneous equation systems. That capability has existed in the mainstream spreadsheet platform, Excel, since the late 1990s and was available earlier in other spreadsheets (e.g. Quattro). This methodology may predate that advance, as the citations to applications at the latest are 1994 (Lichtenberg and others, Ebel and others, and Forsythe and Corso). At that time there was a need to derive analytical solutions to simultaneous equation problems, as is done here, or to impose some external solution method, as was done in some early versions of ERS's SWOPSIM model, for example. With the more modern solution methods, the derivations reported here are unnecessary, the exogenous shocks may be directly imposed on either prices or quantities in the underlying behavioral relationships, and non linear functional forms (such as constant elasticity supply or demand) may be utilized.

While econometricians may debate the merits of direct or inverted supply and demand relationships, since the current model linearizes, moving from one form to another is trivial for the equations utilized here. Consider the following simple re-specification of the problem posed here, which takes advantage of writing these equations in direct rather than inverse form to incorporate cross price, multi-market effects:

Supply:	$Qki = Ski(Pi + \gamma ki , Pj)$	$k = 1,..K$
Imports:	$Qmi = Sm (Pi + Ti)$	
Domestic Demand:	$Qdi = Ddi(Pi + \beta, Pj)$	
Exports:	$Qei = Dei(Pi)$	
Market clearing:	$\Sigma kQki + Qmi = Qd\ i + Qei$	

These are equations (1)-(5) from Forsythe written in a direct rather than inverse form (S and

D are the inverse of their functional forms in (1)-(4)), with a cross price term and exogenous shifters introduced)(Pj, γki and β), including the most basic policy instrument (a tariff Ti) and with i denoting the primary market while j denotes the secondary market. This can be directly written as one equation in one unknown (Pi) and exogenous terms:

$$\text{Excess demand:}\quad Ddi(Pi + \beta, Pj) + Dei(Pi) - \Sigma k\ Ski(Pi + \gamma ki\ , Pj) - Sm(Pi + Ti) = 0$$

The spreadsheet strategy would be to exogenously set in some cells Ti, γki and β (and if comparable to this model, also Pj), to feed those numbers into cells containing the explicit formulae for Dd, De, Sk and Sm. Then with one cell containing Pi – which feeds into those formulae – compute for any given Pi the corresponding excess demand. "Solver", a built in tool of Excel, may then be used to find Pi so that excess demand equals zero, as required by equation 5 (here and in Forsythe).

The multi-market problem can be solved by reproducing equations (1)-(5) for all markets of interest, and simultaneously solving for Pi and Pj (in this two good case, or all of several Pj in multi good cases). That strategy would capture not only the direct impacts from secondary markets, now addressed on pages 10-12, but also any feedbacks from this primary market to that secondary market. This solution strategy was employed in SWOPSIM since the mid to late 1990s.

Since the stated goals of this paper include the ability to rapidly develop an assessment of a policy issue, its simplicity in terms of relying on basic theory, is a virtue. But a key reason for using a simple model is flexibility, and the current solution strategy prevents that. If an analyst wants to deviate from the functional forms or variable inclusions of this framework, the derivations need to be redone, and/or terms changed in the resulting equations utilized in the spreadsheet. With the use of solver, the analyst can substitute other functional forms or add variables determining supply or demand behavior directly. Quantity shifts in secondary markets, and on these markets are also easily incorporated in the more direct, original statement of the model. Two issues in modeling choice implemented here, but not discussed, can illustrate this point. Linearization need not apply, and modern demand systems, such as LES or AIDS can be employed.

The author correctly states in the text that linearization is appropriate for small changes in market outcomes. But sometimes issues on which APHIS has influence (e.g. mad cow disease?) may have a large impact on markets. Any model will have difficulty when such large changes occur, but non linear functional forms, such as constant elasticity functions, may be know to better represent market behavior over some broader range of changes. Linearity is a quite strong, and as it turns out unnecessary, assumption. The derivations reported here likely would not be possible for many modeling choices, yet the 'simultaneous equations' or 'solver based' solution strategy could easily incorporate some non-linearities.

The best example of the use of non linear functional forms commonplace in policy modeling today is the use of theoretically sound demand systems. The analyst starts with a utility function and derives from maximization of that subject to an income constraint demand curves (for all goods, not just the primary market good). The parameters of the utility function can be chosen (or estimated) so that the basic axioms of demand theory are respected. It is quite easy to choose own and cross price elasticties of demand which may violate those axioms. The derivations of demand equations, and constraints on parameters have been worked out for several utility functions. The simplest of these, the linear expenditure system (LES) allows for non homothetic demand (some income elasticties differ from one), which is a problematic assumption of consumer surplus employed here. Use of consumer surplus remains a standard in policy analysis, but better models have mostly incorporated a demand system, like the LES, or a more complicated version, such as the AIDS model, which does a better job of representing cross price effects. A second advantage of this approach is that theoretically superior welfare measures, such as compensating variation or equivalent variation, may then be employed.

Textbook derivations of these models are available (Deaton and Mullbauer, 1980) comparable to the derivations now taken from Hirshleifer. In spite of its name, LES demand equations are non-linear, and AIDS model equations are on linear, as well. It is not uncommon for Taylor series approximations to be used to linearize these demand equations, but that is not necessary if one need not derive analytical solutions of market equilibrium. A practice in the early version of SWOPSIM, which at the time was employing a solution strategy like the one found here, was to utilize such demand systems to impose constraints on choices of demand parameters to insure theoretical consistency. No discussion of this concern is found in this document, but cross price elasticities are used to compute effects from secondary markets.

One advantage of utilizing one of the well known demand systems is that more advanced and accurate welfare measures (compensating or equivalent variation) can then be computed, since a utility function is specified. These have been employed even in partial equilibrium settings, by specifying an “other good” to capture rest of the economy effects. They better represent agricultural products, for which some assumptions behind consumer surplus may be invalid (e.g. homothetic demand).

A more difficult challenging to this modeling effort has been raised in recent work by Paarlberg, Lee and Seitzinger on issues of direct relevance to APHIS concerns. They find that that the type of disease shocks APHIS policy addresses may have altered consumer preferences, shifting demand functions and seriously changing the computation of welfare measures like consumer surplus or equivalent variation. Both of those methods require that the utility function is unchanged before and after the market shock. Paarlberg, Lee and Seitzinger discuss ways of trying to adjust welfare measures. There is no discussion of this important concern in this document.

Incorporation of trade effects is another aspect emphasized in the modeling framework documentation (Forsythe, pages 13-21). But no trade policy instruments are incorporated in any equations. The presumption of the model as written is that the U.S. is a large country exporter and importer in the (primary) market in question, and engages in free trade. There may be cases where that is appropriate, but for lots of goods APHIS could be interested in, some or all of those assumptions may be incorrect. An analyst using this framework would have to seriously alter it to incorporate dairy tariff quotas (TRQs), for example. More problematic is that even more implicit assumptions about international market behavior, and the correct modeling strategy, are invoked without discussion.

A key a parameter of this model, and of agricultural policy generally, is the net export demand elasticity – ηe. This document and the model derivations utilize the method of Bredahl, Meyers and Collins which requires supply and demand elasticities, price transmission elasticities, and market quantity data for all important trading partners. (the number of trading partners allowed for in this framework is not made clear.) At least two alternatives exist to that method of estimating ηe. It is typical in computable general equilibrium models such as GTAP and Linkage to incorporate explicit trade policy instruments or models of trade policy regimes rather than price transmission elasticities. The successor to SWOPSIM at ERS, Hjort's CPPA model, also incorporated direct policy instruments. In domestic policy models, such as those employed at Texas A&M or Tennessee, direct net export demand functions are estimated. This choice of method is controversial. Direct estimation yields lower net export demand elasticities, and explicit policy instrument modeling yields higher elasticties than are found using the price transmission method. My research suggests that the price transmission model captures rather badly the price stabilization regimes it is intended to represent. In the case of the EU variable levy for example, Brehdal, Meyers and Collins suggest the policy regime should correspond with a price transmission elasticity of zero. Estimation of that price transmission elasticity yields a much higher value, well above 0.5, and more decent dynamic models suggests something in between for annual economic models, with lagged adjustment in prices. This implies that the elasticties an analyst must find to implement this method are problematic (supply and demand elasticties for all trading partners are also not readily available without debate). They depend on a correct dynamic specification of price adjustment, and estimation results are not robust over sample periods. Moreover, the dynamics explained at the end of this paper would miss entirely the lagged price adjustments that characterize foreign markets.

In the end, all this or even my model really require is a net export demand elasticity and/or trade policy instruments. The presentation here should have recognized that there are alternative ways of coming up with that parameter, and should have allowed the analyst the flexibility to choose the method that best captured available data and the circumstances surrounding the role of international trade in the market(s) under investigation. The capability of incorporating basic trade policy instruments for the U.S., such as tariffs or quotas, is highly desirable.

Another feature of the trade specification of the model, which is chosen but not discussed, is use of the Armington specification to capture effects of bilateral trade flows. (Discovering these assumptions requires carefully looking at parameter definitions and equation specifications, not reading text.) There are alternatives to the Armington framework that may in some cases be superior choices. In their work on the USDA-IATRC trade embargo study McCalla, Abbott, and Paarlberg found that for the reasonably large market impacts under study, the Armington model characterized trade flows as overly inflexible. The polar extreme assumption, a spatial equilibrium model characterized trade flows as overly flexible – reality lay somewhere between these two modeling choices. The Armington framework was suitable only when trade flows already exist, and only for small market changes. The larger the change, the large should be an Armington substitution elasticity. But modelers have found solution methods for models written as Armington become unstable as those substitution elasticties become larger, so treating this specification choice as a choice of parameter values has not worked well.

This assumption is employed in the large trade models (GTAP and Linkage), but has received a good deal of criticism in recent years, as the problems outlined above appeared in analyses using those frameworks. Most competing modeling frameworks have also resorted to assuming Armington substitution elasticties rather than estimating them, as econometric evidence on their magnitude is weak. Moreover, early econometric evidence (Alston, Carter, Pick and Green) indicated this framework did not fit well agricultural trade data, and its assumptions were routinely violated in practice.

If net trade is all that is required for a problem, and information about bilateral trade flows is not relevant, this feature of the modeling framework need not be used, and Armington parameters are not necessary.

Since I am a trade economist by training, I have focused on the trade specification of this modeling framework, and its lack of trade policy instruments. (Nearly a third of the document was devoted to the trade specification.) In light of the model's focus on impact on a domestic economy, its lack of inclusion of domestic policy instruments is more troubling. Agricultural policy can be complex, and incorporating provisions of farm bills may not be easily accomplished by setting only price based supply or demand shifters. In any case, no such effects are discussed in this document. Once again, writing the model in a more direct form may make addition of policy variables more straightforward and not require redoing the algebraic derivations described there.

Compared to the competing modeling frameworks, this framework is rather incomplete. In analyzing a particular issue many decisions will need to be taken. Markets will need to be defined and an appropriate level of disaggregation chosen. Prices, grades and standards will need to be defined so that data can be collected to implement an actual model. In the other frameworks, most of those decisions have already been taken, although the analysts may change many of these assumptions, including the level of disaggregation in some, and in particular the base data and behavioral parameters. Like this framework, some economic relationships are chosen a priori, however, and cannot be subsequently changed (without rewriting the model).

A great advantage of that prior information is that it gives an analyst a starting point. Some data, like base supply utilization balances, are unlikely to need changing. This framework does not provide the analyst a staring point, and so will be less helpful in meeting its "key goal" of "is reducing turnaround time for analyses" (Forsythe, page 4)

A tradeoff in the design decision taken here is that with less information set a priori, less should have to be changed in the process of modeling. But changing parameters in a spreadsheet is a rather trivial task. Finding appropriate parameter assumptions is a much more demanding task, and no help in that direction is provided here. Rewriting cell equations to accommodate new variables would render this approach ineffective.

While this framework appears to make few theoretical modeling decisions a priori, several are made, and those tend to be buried rather than explicitly discussed. It utilizes basic theory, which is a strength. The analytical structure of the model now brings a degree of inflexibility, however, so that little advantage is gained by having such a structure available. Moreover, the types of shocks to markets modeled here are more easily incorporated in a more direct representation of behavioral equations (they are characterized as quantity shifts here, not price shifts). My guess is that not only would this framework save little time for an APHIS analysts charged with assessing a particular policy, but that analyst would run against constraints to this approach rather quickly, and would want to make different modeling assumptions than those employed here. It would almost always be the case that the derivations described here would need to be redone. That could be avoided by using the solver approach.

In summary, while I do not find fault with the derivations of basic microeconomic theory employed here, I question whether the approach taken to develop this framework will be useful to practical analytical work at APHIS. This document explains what was done to develop the spreadsheets that make up this modeling framework, but not why it was done that way. Avoiding the "why's" leaves unaddressed, and unrecognized, a number of controversies which are implicit in modeling choices now made. The technical strategy for implementing this framework has also fallen behind current spreadsheet capabilities, so that more flexible and complete modeling frameworks are now possible which do not require the tedious derivations presented here.

I doubt that the original goal of this document was achievable. Most analysis of the type AHPHIS must address is not routine. It must be informed by specifics of the problem at hand and by the nature of the narrowly defined markets impacted by the problem. In terms of the work requirements to model such impacts, this framework takes the analyst only a very short distance along the path to informed economic analysis.

Please let me know if I can be of more help with this review. Best personal regards.

Sincerely,

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Professor

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