Comments

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American Agricultural Economics Association Annual Meeting
August 2-5, 1998
Salt Lake City, Utah

Session 4

Comments on Bryant and Richards, "Hysteresis and the Shortage of Agricultural Labor"

This paper analyzes whether there is evidence supporting an agricultural labor shortage in the state of Washington, using cross-sectional data on wages. The presumption is that if labor markets clear, there should be an equilibrium relationship between urban and rural wages at one point in time. An estimation procedure adapted from Sexton, Kling and Carman's (1991) study of spot prices in celery is utilized to measure the probability of this equilibrium relationship against the alternative scenarios of relative surplus or shortage in the agricultural market. This is an interesting idea on a critical question of whether seasonal labor supply is insufficient to meet demand. Support for proposed agricultural guest worker programs for seasonal labor hinge on whether the case for excess demand can be supported, and the authors' argument that relative wages are measured more accurately than the numbers of workers available for the agricultural sector is likely to be correct. Nevertheless, I have some questions about how the estimation is conducted.

The first concerns the use of average wages to measure agricultural and nonagricultural wages. Skill requirements for jobs are not identical across the two markets, and relative wages will reflect differences in education, work experience, and job tenure. This problem is compounded by the fact that the agricultural labor market is seasonal, so average agricultural wages will vary as the largely unskilled seasonal workers enter and then exit the market over the year. Wage differentials between urban and rural markets will reflect any changes in skills between these markets, biasing the test procedure against the finding of an equilibrium.

A hint of the potential importance of changing skill composition in biasing the results away from an equilibrium is that state-level estimates are consistent with an equilibrium relationship, but regional estimates are more supportive of the finding of an agricultural labor shortage. Seasonal biases will be more important within a region than in the state as a whole, and smaller samples of urban wages will also be more susceptible to biases associated with small changes in the composition of jobs included in the urban wage responses. This puzzling outcome may be a consequence of the use of averages and not a spatial mismatch as argued by the authors.

Another correctable problem with this approach is that it assumes that labor markets should adjust instantaneously to shocks. A study of the response of the Alaskan labor market to the huge shocks associated with oil pipeline construction (Carrington, JPE, 1996) found that the complete adjustment took 1.5 years and that much of the response was from increased hours of work and labor market entry. Presumably, the seasonal agricultural labor requirements are well known and the shocks over this three year period are relatively small. Nevertheless, the

presumption that a labor market must be in spot price market equilibrium if it is to be functioning properly (as would be the case for commodities such as #2 yellow corn) is not realistic. Most studies of hysteresis make use of time series data to estimate how long it takes the market to adjust. I would recommend that the authors adapt their methodology to account for possible lagged reactions to see how long it takes the labor markets to adjust to shocks.

All of this begs the question of what the true decision process regarding agricultural labor supply will be. Is it sensible to think that urban workers in full-time jobs will quit in order to take a seasonal job in agriculture? Or is the true opportunity cost of working in Washington's agricultural sector going to be some weighted average of expected earnings from low-skilled part-time work, unemployment or welfare benefits, and expected earnings from seasonal labor in Oregon and California? If it is the latter, than the authors should try to incorporate those markets into their analysis.

Comments on Thilmany and Espy, "Farm Labor Demand and Supply: A Meta-analysis of Wage Elasticities"

This paper reviews the underlying causes for differences in estimated farm labor demand elasticities from 29 studies published between 1962 and 1995. There is little discussion of labor supply, so the title could be shortened a bit. The paper uses a regression analysis approach to assess how differences in data sets (timing, level of aggregation, time-series vs. cross section, U.S. vs. elsewhere, hired vs. family) model (static vs. dynamic, single equation vs. system, translog vs. Cobb-Douglas) and estimation method (OLS vs. GLS vs. simultaneous equations). In general, the results seem to make qualitative sense. Long-run elasticities exceed short-run elasticities, labor demand has become more elastic over time, and demand for labor in the U.S. is more elastic than in other countries, and demand for hired labor is more elastic than demand for family labor.

That said, there are some ways that the analysis could be improved. There are 84 estimates used in the analysis from 29 studies. Presumably, multiple estimates from the same study are not independent, nor are estimates using the same data sets. Some effort to correct for this source of heterogeneity could be critical, particularly as 45 percent of the estimates used come from 5 studies. While I did not review all 29 studies, there is at least some overlap in the data sets used for the analysis which could also be incorporated into the analysis.

It would also be useful for some guidance as to the interpretation of the parameters. What do the coefficients mean in terms of the typical elasticity for a time-series study or a typical elasticity for hire labor? With most coefficients being large and some implying upward sloping demand curves, it would be good to report a

baseline elasticity and then an implied marginal elasticity for each factor.

All this begs the question of the use of met analysis in the first place. If we treat each elasticity as estimated with error, we would undoubtedly assign greater reliability weights on some studies and lower weights on others. If there is one study which does everything right, we would use its estimate as the best guess as to the true demand elasticity. Judicious reading of the papers should enable one to weed out the reasonable from the unreasonable in order to report what the range of reasonable estimates would be. This is the method employed by Hamermesh (Labor Demand, 1993). Alternatively, if one wishes to assess the impact of using time series versus cross-sectional data on estimated demand elasticities, simply get an appropriate data set and estimate the difference directly.

Comments on Howard and Swidinsky, "Estimating the Off-Farm Labour Supply in Canada."

This paper estimates a off-farm labor supply equations as a two-step process, one whether to work off-farm and another on the hours of labor supplied off-farm, conditional on choosing to work off-farm. Many of the parameters in the participation equation are opposite in sign to those in the hours equation, making it hard to interpret the results. The two most puzzling results are that higher off-farm wages increase the probability of working off-farm but lower hours worked and that spouse's income lowers off-farm participation, but increases hours worked off-farm.

The hours worked equation has the oddest results. While the estimation was not spelled out, it appears that the estimation has no correction for participation. Because the choice of whether to participate in off-farm work must be made jointly with the decision of how many hours to work off-farm, excluding those who do not work off-farm will bias the coefficients.

A second possible problem is the inclusion of endogenous regressors. Type of operation, farm size, ownership structure, and several income sources are presumably jointly selected with off-farm hours. One could argue that changing type of operation involves a large capital cost, but then the labor supply decisions must be interpreted as short-run decisions subject to exogenous capital. Nevertheless, it is possible for farms to move in or out of cattle, poultry or hogs on an annual basis depending on prices. Farm size can be changed by moving in or out of rental agreements on land. I'd be willing to assume ownership structure is exogenous in the short-run, but I'm not sure why ownership structure should change off-farm hours. Spouse's income is presumably earned off-farm, and if so, must be endogenous. Spouse's occupation may be predetermined, but I have no idea why it should affect off-

farm choices. Farm net income must be endogenous if dependent on current on-farm hours. Government support income is endogenous if it depends on current hours worked on- or off-farm. What impact excluding some of these variables from the estimation will have is not clear, but it should help a bit.

The authors should also investigate using an alternative definition of "farm". Canadian statistics define a farm as having net sales of at least $250. Therefore, there could be many observations in the off-farm sample that treat farming as a leisure activity. Excluding those hobby farmers should yield parameters that are more consistent with the true off-farm decision.

The current paper does not spend much time discussing how these estimates can be used. Elasticities are reported for the conditional hours equation but not for the unconditional off-farm participation equation. The total labor supply derivative with respect to the wage would be H*dP/dW + P*dH/dW where H is off-farm hours, P is number participating in off-farm work, and W is the off-farm wage rate. Total labor supply elasticities should be computed incorporating both equations.

Comments on Huffman and El-Osta, "Off-Farm Work Participation, Off-Farm Labor Supply and Off-Farm Labor Demand of U.S. Farm Operators"

This paper investigates decisions to participate in the U.S. off-farm market and, conditional on participation, how many hours to work off-farm. In what I think is a unique addition, the paper also has estimates of on-farm hours equations. Because the paper is so similar in purpose to the previous paper, it is particularly interesting to read the two papers back-to-back. I would recommend that the authors of the papers agree on a common set of regressors and procedures so that the differences in labor supply decisions between Canadian and U.S. farmers can be highlighted.

I have read and commented on earlier versions of the Huffman and El-Osta paper, so as a discussant, many of my criticisms have been already handled. Let me just say that this version does a good job of justifying the variables used and avoiding simultaneity problems, and the results seem plausible. As in the previous paper, the estimation contains a probit estimate of participation and a separate hours equation which does not correct for selection. However, the paper mentions that selection corrected estimates yielded implausible results, a common problem in Heckman-type estimation when there are weak identifiers for the selection term. The hours and participation equations are quite consistent with one another, suggesting that the problems faced in the Canadian

study are driven more by the endogeneity problems than by the selection bias.

My suggestion for this paper is that it also try to do a better job of combining the hours and participation equations into a single elasticity so that the total effect of the variables can be easily digested. Simulations that show how wage and income changes affect on- and off-farm hours and participation would also be interesting, particularly if they showed how alternative scenarios for future off-farm wage growth would affect farmer time allocations.