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**Demand for On-Farm Permanent Hired Labor in  
Family Holdings:  
A Comment**

by

**Ayal Kimhi**

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P.O. Box 12, Rehovot 76100

ת.ד. 12, רחובות 76100

# **Demand for On-Farm Permanent Hired Labor in Family Holdings: A Comment**

by

Ayal Kimhi  
Department of Agricultural Economics and Management  
The Hebrew University, Israel  
[Kimhi@agri.huji.ac.il](mailto:Kimhi@agri.huji.ac.il)

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## Abstract

This comment critically discusses the theoretical and empirical treatment of corner solutions in the analysis of labor decisions on farm households. As more and more labor decisions are analyzed jointly, the more ambitious becomes the theoretical justification of empirical applications. "Cutting corners" in theoretical models puts the validity of empirical conclusions in doubt. In such cases relying on intuitive theoretical justification of empirical modeling is preferred.

In a recent article, Blanc et al. (2008), denoted hereafter as "the authors", offer a theoretical and empirical analysis of labor decisions on French family farms, with a focus on permanent hired labor. This is an important and relevant study, because the world-wide trend of increasing average farm size is often accompanied by family farms are an increase in the importance of hired labor. This trend changes the way in which conceived, and has implications for the classification of farms into family farms on one hand and commercial farms on the other hand.

Unfortunately, the authors fall into the trap of insisting on supporting their empirical analysis with a detailed theoretical model. Indeed, our discipline strongly

encourages this strategy: our cheapest comment as referees is to ask for a detailed theoretical model even if the empirical work can be justified by verbal theoretical arguments and/or references to the existing literature. However, the logical link from the theoretical model to the empirical application should follow either of two routes: (1) the structural approach, where parameters of the theoretical model are directly estimated; and (2) the reduced-form approach, where the theoretical model is used to outline the assumptions that underlie the empirical application, identify the relevant explanatory variables (and, if relevant, exclusion restrictions), and predict signs of marginal effects. In this case, the authors follow the reduced-form approach, but significant gaps exist between the theoretical model and the empirical application. The authors recognize these gaps, but in my opinion the reliance on a detailed theoretical model loses its foundation in such a case. In what follows, I will elaborate my reservations about the theoretical model proposed by the authors and about their empirical methods, focusing especially on the link between them.

To start with the theoretical model, the authors present a time-allocation model in which household time is divided between farm work, off-farm work and leisure, in order to maximize household utility derived from income (a proxy for consumption) and leisure. Hiring of farm labor is also part of household decisions, and either the demand for hired labor or the supply of family off-farm labor (or both) are allowed to be in a corner solution. This naturally leads to four different labor regimes: (1) neither off-farm labor supply nor hired labor demand are positive; (2) off-farm labor supply is positive while hired labor is zero; (3) off-farm labor supply is zero while hired labor demand is positive; and (4) both off-farm labor supply and hired labor are positive. However, the authors assume that family and hired labor are perfect substitutes (up to a scale) on the farm. This leads to the well-known result (DeSilva et al., 2006) that if

family labor and hired labor are perfect substitutes, regime (4) can be observed only if the off-farm wage is equal to the wage of hired labor (again, up to a scale), which, as the authors acknowledge, is an event with zero probability. Actually, regime (4) can occur in the case of complete specialization in which family members do not work on the farm, but this is ruled out in this case, for good reasons. The data show that between 3% and 6% of farms are in regime (4), which may not be too bad for a regime with a probability zero, but they account for 42%-69% of the farms with positive hired farm labor, which are the focus of the research, and this must be a source of concern. In order to explain this, the authors depart from their own theoretical model, by claiming that "...in the real world, an economic agent ... takes into consideration several criteria: wage, career prospect, work environment, etc. (page 500)." They further elaborate that "...the configuration where the household uses hired workers and supplies both on- and off-farm labor can also be due to other reasons that our model does not account for. For instance, the off-farm wage may not be the same for all the household members (footnote 4)."

To be more precise, regime (4) can be due *only* to reasons that are not included in the theoretical model. The reason mentioned by the authors, that different household members have different off-farm wages, could lead to specialization within the household, where household members with relatively lower off-farm wages work on the farm, while those with relatively higher off-farm wages work off the farm. The household aggregate labor supplies will then correspond to regime (4), but the model would then lead to an inflation in the number of corner solutions (including corner solutions related to farm work of household members) that would make the empirical application more complicated than the authors are probably willing to accept. In fact,

limiting the empirical analysis to farm operators and spouses would result in the model estimated by Benjamin and Kimhi (2006) using the same data.

Another possible reason, not mentioned by the authors, is that family labor and hired labor are not perfect substitutes. This possibility gains more sense as farm size increases and as farm production becomes more sophisticated, leading to specialized roles of different farm workers. For example, it is likely that household members specialize in managerial activities while hired workers specialize in non-managerial activities. But, relaxing the perfect substitutability assumption would make hired labor just another farm input and reduce the model to the farm-household time-allocation model suggested by Huffman (1980), with the additional complication that allows for a corner solution for hired labor, justified by arguments based on, say, fixed costs of hiring or supervising labor. In fact, relaxing the perfect substitutability assumption removes the problem with regime (4) having zero probability of occurring, because in this case the marginal farm productivity of family labor becomes endogenous, and will equal the off-farm wage for all households supplying off-farm labor.

It should be noted that conventional farm-household models (e.g., Benjamin, 1992), that maintain the perfect substitutability assumption, allow for corner solutions even when the off-farm wage is equal to the wage of hired labor. To see this, consider figure 1, which depicts a curved line representing restricted farm profit (Lopez, 1984) as a function of farm labor (measured from right to left) and a straight line with a slope that is equal in absolute value to the market wage. The optimal level of farm labor, denoted as  $L^*$ , is determined in this model as a solution to a farm profit maximization problem, at the point  $O$  where the wage line is tangent to the farm profit curve, independently of utility considerations (the separability property – see Benjamin, 1992). Utility considerations then determine who provides farm labor. If

the household indifference curve between income and leisure is tangent to the wage line at  $O$ , farm labor is provided by household members only, and no off-farm labor is supplied, which is equivalent to regime (1). However, it could be that household members work off the farm while hired labor is used to make up for their lost farm labor, because the wages are equal. This is regime (4), although this case can be ruled out for reasons that the current "model does not account for". If the point of tangency is to the left of  $O$ , household members supply off-farm labor, in addition to farm labor. Whether hired labor is also used on the farm is indeterminate, due to a similar argument to the one leading to regime (4) in the previous case. If farm labor is provided by household members only, the situation is equivalent to regime (2). If hired labor is also employed, the situation is equivalent to regime (4). Now consider the case in which the point of tangency is between  $O$  and  $T'$ . In this case farm labor will be provided by both household members and hired workers, which is equivalent to regime (3). Tangency at  $T'$  or to its right implies that farm labor is provided by hired workers only, a regime which is not considered in this paper. These different regimes may all occur under perfect substitutability of family and hired farm labor *and* under uniform wages, whereas in the model proposed by the authors, only regime (4) is supported under these conditions.

The case in which the off-farm wage is lower than the wage of hired labor can be illustrated similarly. In figure 2, the slope of the line that is tangent to the farm profit curve at  $O_1$  is equal to the absolute value of the hired labor wage, while the slope of the line that is tangent at  $O_2$  is equal to the absolute value of the off-farm labor wage of household members. Here, tangency of an indifference curve to the hired labor wage line between  $O_1$  and  $T'$  represents regime (3), tangency to the off-farm labor wage line to the left of  $O_2$  represents regime (2), while cases in between,

where the indifference curve is tangent to the farm profit curve itself, represent regime (1). Note that if the off-farm wage is higher than the wage of hired labor, family members will not supply farm labor. These observations are consistent with the authors' model.

Let me summarize my criticism of the author's theoretical model in two parts. First, the model correctly captures regimes (1)-(3), that are conditional on the off-farm wage to be lower than the hired labor wage. However, the authors do not provide any intuition or, preferably, comparative static results, that motivate the empirical analysis. Secondly, the authors define regime (4) in the case of equal wages, but as has been shown above, equal wages do not necessarily lead to regime (4). Altogether, the value of the theoretical model to the subsequent empirical analysis is very limited. The authors could do no worse by relying on the existing literature or on a graphical analysis as illustrated above.

Finally, the authors' empirical strategy itself may be based on shaky grounds. The use of the Bourguignon et al. (2007) two-stage selectivity correction method based on a multinomial logit selection rule is justified. However, the authors also seem to correct, at the same time, for the endogeneity of family farm labor using 2SLS techniques. It is not clear whether there are theoretical-econometric justifications for this estimation strategy. Bourguignon et al. (2007) make assumptions about the joint distribution of the residuals of the multinomial logit equations and the regression equations. It is not clear whether the 2SLS residuals could satisfy these assumptions. Also, 2SLS estimation is based on complete samples, and one should justify its use in the presence of sample selectivity, even if the latter is adequately treated. It is not surprising that the authors are getting results – the second-stage estimation is simply an OLS and econometric software packages are not able to judge

the underlying assumptions – but I am not aware of a reliable reference for this kind of estimation and the authors do not provide one either. Hence, the empirical results, although making much sense, should be evaluated with caution.

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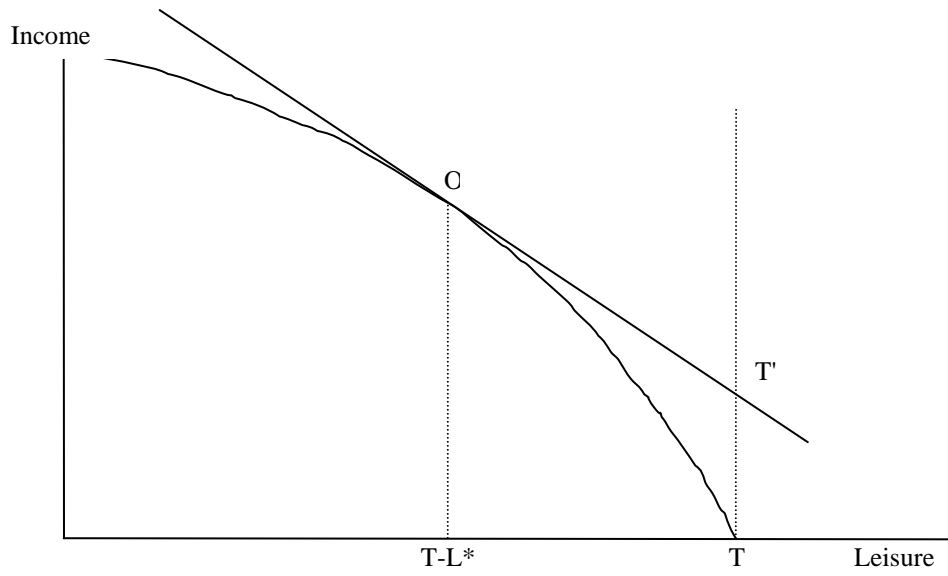


Figure 1. Illustrating the case of equal wages

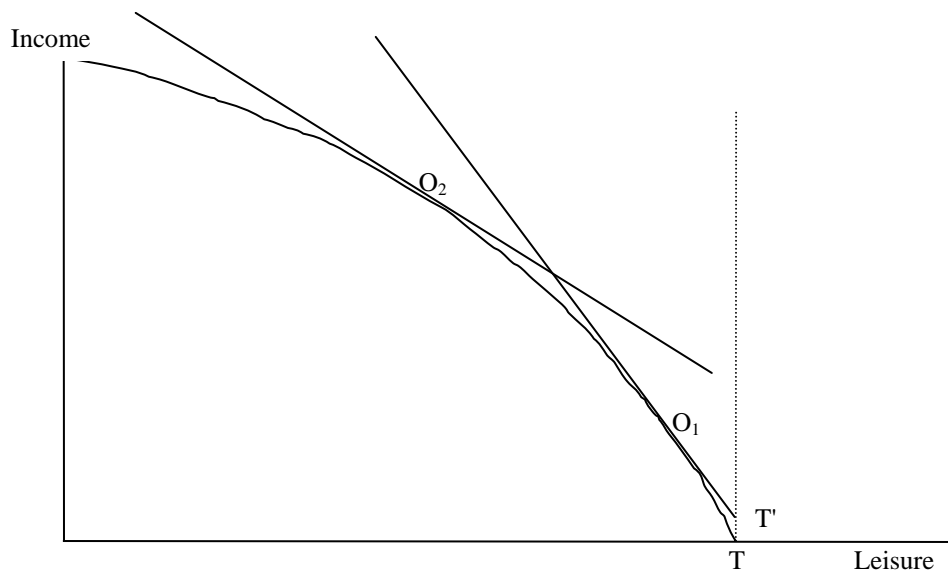


Figure 2. Illustrating the case of unequal wages

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