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Daniel O. Gilligan
International Food Policy Research Institute
d.gilligan@cgiar.org

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HOW ACCURATE ARE REPORTS OF CREDIT CONSTRAINTS? RECONCILING THEORY WITH RESPONDENTS' CLAIMS IN BUKIDNON, PHILIPPINES

Daniel O. Gilligan[a,b], Sarah Harrower[c], and Agnes Quisumbing[a]

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Abstract:

We test the validity of self-reported credit constraint status from household surveys in the Philippines against alternative specifications of theoretical models of consumption and production behavior. We construct two indicators of credit constraints based on reports from agricultural production modules on excess demand for credit and self-imposed credit rationing due to default risk. Both indicators yield plausible results in estimates of the determinants of the probability of being credit constrained. However, in a switching regression model of consumption smoothing, credit constrained households showed no significant difference from unconstrained households in sensitivity of consumption to income changes. In a model of farm labor demand, we reject the separability of production and consumption decisions for credit constrained households and not for unconstrained households, demonstrating that reported credit constraints lead to rationing of labor hired on farm. Including risk rationing in credit constraints improves test results for credit constraints hypothesis. However, failure to reject equality of estimated effects of household structure on labor demand across credit constrained regimes weakens these results. Results show that the context in which direct elicitation methods of measuring credit constraints are collected and applied affect the support for credit constraints and estimates of their implications. They also demonstrate the lack of fungibility of credit across uses within the household for this Philippines sample.

JEL Classifications: D12, O16, Q12
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[a] International Food Policy Research Institute.
[b] Corresponding author: d.gilligan@cgiar.org; IFPRI, 2033 K Street NW, Washington, DC 20006.
[c] University of British Columbia.

1. Introduction

Credit constraints have a number of serious consequences for production and consumption in the short run, and for asset accumulation, poverty reduction, and the evolution of well-being in the long run.[1] Credit constraints reduce capacity to smooth consumption in the face of income shocks (Zeldes, 1989); lead to adoption of low return-low risk crop and asset portfolios (Rosenzweig and Binswanger, 1993); can obviate the supervision cost advantage of small farmers (Kevane, 1996); and hinder investment in children's health and education (Becker and Tomes 1986; Behrman, Pollak, and Taubman 1992; Foster 1995). Credit constraints may also result in other behavioral adaptations, such as fragmentation of fields, migration, gift-giving, and formation of patron-client relationships (Townsend 1995; Rosenzweig and Stark 1989; Fafchamps 1992). Despite these significant costs, there is no consensus on an empirical approach to determining which households are credit constrained. Knowing this would have clear benefits for policy. Governments could improve economic efficiency through targeted programs to reduce the incidence of credit constraints or their effects.[2] A rigorous approach to identifying credit constrained households would also benefit research by improving estimates of behavioral parameters in models involving credit constraints. In this paper, we investigate the empirical and theoretical support for an increasingly popular method for identifying credit constraints, using self-reported credit constraint status from surveys, in panel data from the rural Philippines.

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[1] By credit constraints, we mean restrictions on access to credit at market rates. These can arise from quantity rationing due to weak enforcement mechanisms or information asymmetries in credit contracts (Stiglitz and Weiss, 1981), transaction costs in obtaining credit, or self-imposed risk rationing by borrowers unwilling to lose their collateral (Boucher and Carter, 2001).

[2] To the extent that poorer households are more likely to be credit constrained (Townsend, 1994) and that lenders exhibit a wealth bias in assessing a borrower's credit risk (Barham, Boucher and Carter, 1996; Carter and Olinto, 2003), knowing which households are credit constrained could increase the effectiveness of credit programs for the poor.

Many approaches to identifying credit constrained households fall into one of two categories: indirect methods based on tests of a theoretical model involving credit constraints and direct methods using responses to qualitative questions about credit constraint status collected in surveys.[3] Indirect tests for the presence of credit constraints often involve a comparison of parameter estimates for the outcomes of interest across constrained and unconstrained groups (eg, Hu and Schiantarelli, 1998), or tests of exclusion restrictions on key parameters for the unconstrained group that arise from the non-binding credit constraint (eg, Zeldes, 1989; Jacoby, 1994). For these tests, the sample is divided into groups of potentially constrained and unconstrained households using arbitrary criteria, such as a threshold level of the asset to income ratio, or based on observed credit demand. In many formulations, these ad hoc approaches can lead to biased and imprecise estimates of the effects of credit constraints. Moreover, they can only identify the presence of credit constraints on average in a sub-sample of the data.[4]

Methods for identifying credit constrained households based on direct elicitation of credit constraint status from survey questions about restrictions on credit (Jappelli, 1990; Feder et al, 1990) have several attractive features. The necessary information can be gathered directly in surveys, providing a simple, unambiguous method for identifying credit constraints for each household. Moreover, questions can be designed to capture all sources of constraints on credit access, including self-imposed rationing due to high default risk, which provides a more comprehensive measure of constraints than methods

[3] Other methods include: (i) estimating excess demand for credit by measuring both household level supply and latent household demand (eg, Kochar, 1997; Diagne, Zeller and Sharma, 2000), and (ii) comparing the marginal return from investments to market interest rates (Becker and Tomes, 1986). These approaches have demanding data requirements and so are less common.
[4] Cowing (1978) developed an approach that could be adapted to obtain estimates of the Lagrange multiplier on the credit constraint for each observation. Zeldes (1989) applied a variant of this approach, but one that only tests for a non-binding credit constraint for sample sub-groups.

relying on observed credit use or rejected loan applications (Jappelli, 1990; Jappelli, Pischke and Souleles, 1998). If self-reported credit constraint status is accurate, it provides significantly more information than most theoretical approaches. However, little is known about the quality of credit constraint indicators based on direct elicitation, about how methods of elicitation affect their accuracy, and about the consistency of these indicators with theoretical models.[5]

This paper investigates the accuracy of credit constraint indicators based on direct elicitation and explores their applicability by validating the indicators against alternative theoretical models of credit constrained behavior using panel data collected four times in 1984/85 on rural households in Bukidnon province, Philippines. In the farm production modules of the survey, respondents were asked about their access to credit, including whether they would have used more credit if it had been available to them for farming activities. Using alternative indicators of credit constraint status based on this information, we test whether a simple model of the probability of being credit constrained can identify differences in credit constraint determinants across types of constraints. Using this analysis to control for selection by credit constraint status, we test the predictions of models of consumption smoothing and farm labor demand to see if this division of the sample is consistent with theory.

In the consumption smoothing model, households whose consumption covaries with income changes are deemed to be credit constrained, since they are unable to insure their consumption against income shocks through borrowing (Hayashi, 1985; Zeldes,

[5] See Barham, Boucher, and Carter (1996) for a discussion on the design of survey questions to capture credit constraints. Japelli, Pischke and Souleles (1988) explore the effects of using a more limited direct elicitation approach than the one used here on parameter estimates in Zeldes' (1989) model of consumption smoothing.

1989). We test the accuracy of self-reported credit constraint status by testing that households with (without) self-reported credit constraints have a positive (zero) response of consumption changes to income changes. We also test that the sensitivity of consumption changes to income changes is greater for the cohort reporting constraints than for the cohort reporting no constraints in alternative models with increasing power to identify credit effects. In the model of farm labor demand, household demographic variables such as household size affect on-farm labor demand only if there is a failure of the separability of household production and consumption decisions, as would occur in the presence of credit constraints (Feder et al, 1990; Benjamin, 1992). Tests for differences in effects of household structure on labor demand by reported credit constraint status provide greater power in identifying the role of credit constraints against alternative explanations for the failure of separability. If the credit constrained cohort does not demonstrate the behavior predicted by theory in both the consumption smoothing and labor demand models, this suggests either that credit is not perfectly fungible across household activities or that the consumption and production models have different power for identifying the effects of credit constraints. We investigate the plausibility of these alternative explanations.

We find informal support for reported credit constraints in the form of plausible parameter estimates in alternative models of credit constraints determinants that use an indicator capturing quantity rationing and transaction costs and another one that also incorporates risk rationing. In the formal tests of credit constraints using the theoretical models, we find no evidence of credit constraints in any specification of the consumption smoothing model. In the labor demand model, unlike Benjamin (1992) we reject the hypothesis of separability of production and consumption decisions, with strong evidence that the failure of separability is caused by credit constraints, as reported in the survey.

Section 2 describes the direct elicitation approach to classifying households as credit constrained, introduces the models of consumption smoothing and labor demand, and outlines the empirical switching regression model used throughout this analysis. Section 3 presents the data and credit constraint indicators. A multivariate analysis of credit constraint status and the results of tests of the theoretical models are presented in Section 4. Section 5 concludes.

2. Methodologies for Identifying Credit Constraints

2.1 Direct elicitation of credit constraints in surveys

Direct elicitation methods use responses to qualitative survey questions on the perception of constraints, the history of households' access to credit, and current credit demand to identify households facing credit constraints. In an early application of this approach, Jappelli (1990), classified households in the US 1983 Survey of Consumer Finances as credit constrained if they had a loan application rejected or did not apply for a loan because they believed they faced a high probability of rejection (labeling the latter "discouraged borrowers"). Feder et al. (1990) classified households from a household survey in China as credit constrained if they stated they would have used more credit at current interest rates if it were made available to them. Diagne, Zeller and Sharma (2000) classified a household as credit constrained if it had reached its perceived credit limit from any loan source or if its members stated that they could not obtain credit. Other examples of the direct elicitation approach include Zeller (1994) and Barham, Boucher, and Carter (1996).

Each of these approaches relies on a set of survey questions designed to identify whether the household's demand for credit exceeds supply available to it at current prices. Following Jappelli, a simple representation of credit constraints can be made by considering the household's budget constraint in the presence of credit markets. A household finances consumption in the current period, C_t, out of income, Y_t, and the returns on investments, A_t, at a rate of return, r. If borrowing is possible, the amount by which consumption can exceed income and asset returns is restricted to be no greater than the cost of borrowing at the household's credit limit, R_t,

$$C_t - Y_t - (1+r)A_t \leq (1+i+s)R_t \qquad (1)$$

where i is the interest rate on loans and s is the transaction cost for obtaining credit as a share of the amount borrowed. If at optimal levels of consumption, income and asset holdings demand for credit on the left-hand side exceeds supply of credit on the right-hand side, the credit constraint in (1) will bind. This expression shows the ways in which credit constraints can arise. Quantity rationing occurs when lenders set a low R_t relative to household credit demand, possibly because of moral hazard or concerns about the reliability of household investments. Alternatively, high transaction costs are another form of market imperfection that restricts the supply of credit to the household. Risk rationing occurs when a household accepts little or no credit because uncertainty about returns on its investments causes the household to choose a low risk, low return investment strategy.[6]

Diagne, Zeller and Sharma collected information on all components of credit demand and supply in equation (1) in household surveys in Bangladesh and Malawi in order to identify the presence and severity of credit constraints. However, respondents' reports of perceived credit limits, in particular, may be unreliable. The Bukidnon data set used here employs a more straightforward approach similar to Feder et al. intended to capture only whether the household was credit constrained in each period.[7] This approach treats excess demand for credit, $ED_t = D_t - S_t$, as a latent variable. For each household *h* in each period *t*, only an indicator for whether the household is credit constrained is observed,

$$
\text{(2)} \qquad \begin{aligned} k_{ht} &= 1 \quad \text{if} \quad ED^{*}_{ht} = X'_{ht}\alpha + u_{ht} \geq 0 \\ k_{ht} &= 0 \quad \text{if} \quad ED^{*}_{ht} = X'_{ht}\alpha + u_{ht} < 0 \end{aligned}
$$

where X_{ht} includes household and farm characteristics that determine credit demand as well as characteristics of the household and the local credit market that determine credit supply. The error term u_{ht} is a mean-zero random variable capturing stochastic factors affecting both demand and supply. We begin our investigation of the reliability of self-reported credit constraint status in Section 4 by estimating the determinants of self-reported credit constraints in equation (2).

[6] Risk rationing can arise if the rate of return on investments includes a random component with variance σ^2. If $r'(\sigma^2) > 0$, risk averse households may adopt low-risk investment strategies to avoid defaulting on a loan or to reduce demand for credit as *ex post* insurance.
[7] The survey questions used to capture credit constraints in the Bukidnon data are described in detail in Section 3.

2.2 Testing for credit constraints in consumption smoothing models

Consumption smoothing models describe the relationship between the intertemporal path of consumption and changes in income. These models predict that households use credit and insurance to mitigate the effects of income risk by "smoothing" their consumption over time in response to income shocks when financial markets are complete. The stark predictions of these models provide the framework for empirical tests of their assumptions and implications, including the assumption of complete credit and insurance markets. Empirical models of this sort designed to test the implications of the Rational Expectations-Permanent Income Hypothesis have been developed by Hall and Mishkin (1982), Altonji and Siow (1987), and Zeldes (1989) among others. Conditions for efficient risk-sharing in a coinsurance community from this type of model were derived by Wilson (1968) and Diamond (1967) and tested by Mace (1991), Cochrane (1991), and Townsend (1994). A central implication of these models is that when credit and insurance markets are complete, and controlling for changes in preferences, the growth rate in household consumption over time is affected only by changes in aggregate or covariate risk and not by idiosyncratic shocks to income.

Taking villages as coinsurance communities, an empirical specification of the consumption smoothing model (see Ravallion and Chaudhuri, 1997, and Jacoby and Skoufias, 1997) is given by

(3) $$\Delta \ln c_{hvt} = \sum_{vt} \beta_{0vt} \left(VD_{vt} \right) + \beta_1 \Delta \ln y_{hvt} + X'_{hvt} \beta_2 + \Delta \varepsilon_{hvt}$$

where $\Delta \ln c_{hvt}$ is the growth rate in total consumption per capita of household *h*, in village *v*, from period *t-1* to *t*, $\Delta \ln y_{hvt}$ is the growth rate of income, X_{hvt} is a vector of household characteristics capturing differences in preferences, and VD_{vt} is a set of binary variables identifying each *barangay* (roughly, village) in the Bukidnon sample by survey round.[8] The parameters to be estimated include β_1, the $v \times t$ scalars, β_{0vt}, and the vector β_2. The term $\Delta\varepsilon_{hvt}$ is a household-specific error term that represents unobservable changes in the household preferences over time.

The *barangay*-round indicators, VD_{vt}, remove the effects of aggregate or covariate income shocks experienced by all households in a community by survey round. These covariate shocks can include the common component of drought effects, differences in inflation rates across communities, and changes in local interest rates, which affect the cost of capital.[9] With these controls for aggregate shocks in place, $\Delta \ln y_{hvt}$ captures only the effects of household-specific or idiosyncratic income shocks. The central implication of the model is that these idiosyncratic shocks have no effect on changes in consumption under perfect financial markets. Therefore, a test of the null hypothesis $H_0 : \beta_1 = 0$ is a test that households face perfect financial markets, including unconstrained access to credit.

As a test of credit constraints, this approach has relatively weak power because failure to reject consumption smoothing could arise when credit constraints are binding if (i) insurance markets are complete or (ii) households use precautionary savings or less risky investments to protect against their restricted access to liquidity (Morduch, 1995).

[8] The *barangay* is the smallest political unit in the Philippine government.
[9] Including the community/round interaction dummies is equivalent to deviating all variables from their respective community/round mean. For more detailed discussion of this equivalence see Deaton (1997).

Jacoby and Skoufias (1997) suggest an alternative formulation of equation (3) that addresses the first of these concerns by separating the role of insurance markets from that of credit constraints. They argue that if credit constraints are absent but insurance markets are incomplete, unanticipated shocks to income affect welfare, and so will affect the change in log consumption. Using a sample from the ICRISAT survey in India, they decompose the effects of unanticipated shocks from anticipated shocks to income at the household level by regressing the change in log income against a vector of farm characteristics, X_{vht}, and these same characteristics interacted with rainfall shocks measured as deviations of seasonal rainfall from long run averages, DR_{vht},

$$(4) \qquad \Delta \ln y_{hvt} = \sum_{vt} (VD_{vt}) \delta_{0vt} + X'_{hvt}\delta_1 + \left(X_{hvt} \otimes DR_{vt} \right)' \delta_2 + \Delta\varepsilon_{hvt} .$$

Interacting rainfall surprises with farm characteristics identifies the household-specific unanticipated shock, since these shocks affect each household differently.

Estimates of anticipated and unanticipated idiosyncratic income changes, respectively, can be calculated as

$$(5.a) \qquad \Delta \ln \hat{y}^a_{hvt} = X'_{hvt}\hat{\delta}_1,$$

$$(5.b) \qquad \Delta \ln \hat{y}^u_{hvt} = \left(X_{hvt} \otimes DR_{vt} \right)' \hat{\delta}_2.$$

Using these results, it is possible to modify equation (3) to capture separate effects of credit and insurance markets

(6) $$\Delta \ln c_{hvt} = \sum_{vt} \beta_{0vt} \left(VD_{vt}\right) + \beta_{1a} \Delta \ln \hat{y}^{a}_{hvt} + \beta_{1u} \Delta \ln \hat{y}^{u}_{hvt} + \Delta \varepsilon_{hvt}$$

In this specification, the null hypothesis $\hat{\beta}_{1a} = 0$ tests for imperfect intra-village credit markets and $\hat{\beta}_{1u} = 0$ tests for incomplete intra-village insurance markets.

One shortcoming of the test of credit constraints in equation (6) is its symmetric treatment of positive and negative shocks (see Jacoby and Skoufias, 1997). The factors that determine whether one can deal with positive shocks (including access to safe assets and savings instruments) compared to dealing with negative shocks (selling assets, receiving transfers, or obtaining credit) may be quite different in general and between households. Although the proposed direction of the effect of positive and negative income shocks on consumption changes is the same, the magnitude of these changes may be quite different, which muddies the estimates of $\hat{\beta}_{1a}$. Moreover, only the effect of negative income shocks should truly identify credit constraints. These concerns can be addressed by allowing for separate effects of positive and negative income shocks,

(7) $$\Delta \ln c_{hvt} = \sum_{vt} \beta_{0vt} \left(VD_{vt}\right) + \beta^{+}_{1a} d^{+}_{hvt} \Delta \ln \hat{y}^{a}_{hvt} + \beta^{-}_{1a} \left(1 - d^{+}_{hvt}\right) \Delta \ln \hat{y}^{a}_{hvt} + \beta_{1u} \Delta \ln \hat{y}^{u}_{hvt} + \Delta \varepsilon_{hvt},$$

where $d^{+}_{hvt} = 1$ if $\Delta \ln \hat{y}^{a}_{hvt} > 0$, and $d^{+}_{hvt} = 0$ otherwise.

2.3 Testing for credit constraints in a model of farm labor demand

Finally, we draw from the literature on household separability to test credit constraints in a model of farm labor demand. A central result of agricultural household models is that when markets and information are complete, household production and consumption decisions are separable (Singh, Squire and Strauss, 1986). Benjamin (1992) tested the separation hypothesis by testing that farm labor demand is independent of household structure, specifically household size and the gender and age composition of household members. A test of exclusion restrictions on these variables is also a test of perfect labor and credit markets. If a farmer has access to as much labor as needed at market clearing wage rates and can obtain the credit needed to hire the workers, optimal labor demand on farm will be unaffected by the number of potential farm workers in the household. Using a farm sample from Indonesia, Benjamin was unable to reject the separation hypothesis in most specifications of his model, finding no evidence against perfect labor and credit markets.[10]

In applying Benjamin's test to the Bukidnon sample, we estimate a labor demand equation for household *h* in period *t* of the form

$$L_{ht} = \delta_0 + \delta_1 w_{vt} + \delta_2 A_{ht} + \delta_3 n_{ht} + \delta_4 X_{ht} + \omega_{ht}, \tag{8}$$

where L_{ht} is the total number of labor days on farm by hired workers and household members, w_{vt} is the *barangay* average of male wage rates paid by respondent households for activities between planting and harvest, A_{ht} is a vector of land variables capturing area planted and quality, n_{ht} is a vector of household structure variables, X_{ht} is a vector of household variables controlling for managerial ability and farm assets, and ω_{ht} is a

[10] However, Fafchamps and Quisumbing (1999) reject separability using a data set from rural Pakistan, with very different labor market conditions.

random error term. In estimates shown below, the land variables in A_{ht} are area operated by round and its square. Variables included in X_{ht} control for managerial ability (age and education of the household head), access to labor-saving technologies (the value of carabao and of plows and harrows owned in round 1), and distance to labor and credit markets (distance to the municipality seat). We represent household structure, n_{ht}, by household size and by the share of prime age (16-65) adult males and the share of prime age adult females in the household.

The test for separation of production and consumption decisions in equation (8) is a joint test that $\delta_3 = 0$. The appropriateness of this test for our purposes depends on the interpretation of a rejection of separation. It can be argued that a rejection of the separation hypothesis in the labor demand model most directly indicates imperfect labor markets. Indeed, Benjamin interprets results of alternate versions of this model as tests of a surplus labor model and of a model with restrictions on hiring in workers. However, this model has power as a test of credit constraints as well if farmers commonly hire workers for cash wages and use credit to finance the transactions, or if they would like to in the absence of credit constraints.[11] Moreover, testing for credit constraints in a labor demand model provides a more direct test than could be constructed from an output supply model like that used in Feder et al. (1990) because the effect of credit access on output supply operates through demand for factors such as labor and fertilizer. Finally, we improve the power of this test against the alternative of credit constraints by

[11] In the Bukidnon sample, hired labor use is common, with an average of nearly 80 percent of farmers hiring some labor in each round. Sugar producers (33.8% of the sample) rely heavily on credit to finance labor demand: 68.3 percent of sugar farmers took out cash loans by round and 82.8 percent of these loans were for labor. Although only 36.8 percent of corn producers (61.2% of the sample) use credit by round, they reported that 88.9 percent of these loans were used to finance labor. These figures understate demand for credit to finance labor transactions if access to credit is constrained.

estimating the labor demand model as a switching regression in which the parameter estimates are allowed to vary depending on whether the household reports being credit constrained. If the estimated effects of household structure on labor demand are greater for the credit constrained cohort, we can argue credibly that the difference is driven by credit constraints. The same argument applies to tests for credit constraints in switching regression estimates of the consumption smoothing model. We briefly introduce the switching regression approach to estimating the consumption smoothing and labor demand models presently.

2.4 An empirical switching regression model

We use the empirical models of consumption smoothing (equations (3), (6) and (7)) and labor demand (equation (8)) to test both the accuracy of self-reported credit constraints and the relative power of these models to identify the constraints by allowing parameter estimates in each model to differ in credit constrained and credit unconstrained regimes (as described in Section 3 below). Using equation (2) as the criterion function for whether the household is credit constrained and Y_{ht} to represent the dependent variable ($\Delta \ln c_{hvt}$ or L_{ht}, respectively) we estimate an endogenous switching regression (see Maddala, 1986) of the form

(9.a) $$Y_{1ht} = W_{1ht}'\phi_1 + v_{1ht} \quad \text{if} \quad k_{ht} = 1,$$

(9.b) $$Y_{2ht} = W_{2ht}'\phi_2 + v_{2ht} \quad \text{if} \quad k_{ht} = 0,$$

where, using equation (3) as the model of consumption smoothing, for example,

$\phi_i = [\beta_{i0vt}\ \beta_{i1}\ \beta_{i2}]'$, $W'_{ihvt} = [VD_{ivt}\ \Delta \ln y_{ihvt}\ X'_{ihvt}]'$, and $v_{iht} = \Delta\varepsilon_{ihvt}$ for i =1 if constrained and i=2 if unconstrained. This model is an endogenous switching regression because we allow correlation between the error terms in the credit constraint criterion function, (2), and the equations of interest, (9.a) and (9.b). That is, we assume $(v_{1hvt}, v_{2hvt}, u_{hvt})'$ is jointly normally distributed with mean zero and covariance matrix,

$$\sum = \begin{bmatrix} \sigma_{11} & \sigma_{12} & \sigma_{1u} \\ \sigma_{12} & \sigma_{22} & \sigma_{2u} \\ \sigma_{1u} & \sigma_{2u} & \sigma_{uu} \end{bmatrix}.$$

The switching regression model accounts for the fact that each household has a non-zero probability of being credit constrained in each period, that this probability varies depending on household characteristics, and that only one realization of these probabilities is observed in each period. Consistent estimates of parameters ϕ_1, ϕ_2, and α can be obtained (up to a proportional constant for α) by estimating the credit constraint equation in (2) as a probit and estimating (9.a) and (9.b) separately accounting for the selection into constrained and unconstrained regimes by inserting the appropriate inverse Mills ratio from equation (2).

3. Credit Constraint Indicators and Data Summary

3.1 The Bukidnon survey and credit constraint indicators

The data used in this analysis draws from a survey of households conducted by the International Food Policy Research Institute (IFPRI) and the Research Institute for Mindanao Culture, Xavier University (RIMCU) in southern Bukidnon Province on the island of Mindanao in the Philippines. The survey was designed to investigate the effects of agricultural commercialization on nutrition and household welfare. In 1977, the Bukidnon Sugar Company (BUSCO) began operating a sugar mill in the area, which had previously been dominated by subsistence corn production. The presence of the mill gave farmers the opportunity to adopt this cash crop, depending on their proximity to the mill. The survey was fielded in four rounds at four-month intervals from August 1984 to December 1985, so that rounds 1 and 4 cover the same season. The survey contains information on food and non-food consumption expenditure, agricultural production, income, asset ownership, credit use, anthropometry and morbidity, education and 24-hour food consumption recall. The sample was drawn from 30 *barangays* and was stratified by (i) agricultural production activities, particularly sugar (the cash crop) and corn (the food crop), (ii) proximity to the sugar mill (as a proxy for access to the new crop), and (iii) access to land, including ownership, tenancy and landlessness. The initial sample included 510 households, and 448 households were interviewed in all four rounds. These 448 households make up the sample for this study. Bouis and Haddad (1990) provide a detailed description of the sample design and survey area.

The data on credit use includes principal amount borrowed, source of loan and repayment conditions by round (only in rounds 2-4) in each of four crop-specific agricultural production modules: one each for sugar, corn, rice and other crops. Data on borrowing was also collected for non-production loans in survey modules on agricultural wage labor (for loans paid back through labor) and other income sources. Also, in the food consumption expenditure module, respondents were asked whether the foods listed were purchased with credit, but not the amount borrowed.[12] These data show substantial involvement in the credit market by sample households. Seventy three percent of households reported some borrowing in the agricultural production and non-production loans modules. Another 13.6 percent of remaining respondents indicated using credit to finance food purchases in at least one round of the survey. Despite this frequency of activity in the credit market, debt levels are moderate, with median amount borrowed during rounds 2-4 at 1085 pesos, or 6.3% of median income for borrowing households.

In rounds 2-4, farmers were also asked about credit constraints in the agricultural production modules. For each crop produced, they were asked:

1. "If more production credit had been available to you for [crop] production in the past four months, would you have used it?"
2. "If yes, how would you have used it?"
3. "If no, why not?"[13]

This method of direct elicitation of credit constraints could capture most sources of credit constraints described earlier, including quantity rationing, transaction costs, and

[12] Because credit data were collected by activity, it was not always possible to determine whether the same loans were being referenced in different modules of the survey. In most cases, it was possible to differentiate loans by principal amount borrowed, date borrowed, and repayment terms. When distinct loans could not be identified, we assumed each reported incidence of credit use represented a new loan.
[13] Question 3 was asked only in survey rounds 3 and 4.

discouraged or risk-rationed borrowing, subject to several important caveats. In principle, households facing quantity rationing should answer yes to question 1. Households with zero borrowing because of high transaction costs for obtaining a loan should also answer yes to question 1, since the wording of the question suggests that transaction costs would be reduced to make the loan available. Less clear is how effective this question would be at capturing credit constraints due to a moderate level of transaction costs that leaves the household with positive borrowing. Pre-coded responses to question 3 included fear of losing collateral, so this question should be able to identify risk rationed or discouraged borrowers.

There are some shortcomings in using these three questions to capture credit constraints. The terms of the hypothetical loan that would be made available to the household are not clearly specified in question 1. Feder et al (1990) and Barham, Boucher and Carter (1996) add a phrase like "at going rates of interest" to this question. The omission of such a phrase is a limitation of the Bukidnon data. However, even when similar phrases are included, it is unclear how the respondent chooses the loan characteristics (interest rates, length of repayment, collateral requirements) on which to judge his desire for more credit. From these questions, we do not know if the respondent considers the average terms of loans recently taken or the likely terms of his next best, or marginal, source of credit, which would be less favorable. For respondents with little or no recent experience in the credit market, errors in judging the probable terms of this hypothetical loan may be great. Moreover, the hypothetical nature of the question may lead to inflated reports of credit constraints because respondents are not immediately faced with the burden of paying back the hypothetical loan. Finally, the context in which these questions were asked in the Bukidnon survey (loans for production of specific crops) suggests that some households that were credit constrained for consumption or other purposes were inaccurately classified as unconstrained. Tests of the theoretical models conducted below will help to assess the robustness of this methodology.

Based on responses to the three questions listed above, we developed two indicators of credit constraint status. In the first, households answering “yes” to question 1 in any of the four crop modules were classified as credit constrained for that round of the survey. In the second approach, we add risk-rationed borrowers identified by question 3 to the list of constrained households. In the first classification, 245 households were credit constrained in at least one round and 130 households were never credit constrained. The remaining 73 households were not agricultural producers. These households did not have the opportunity to answer the credit constraint questions because they did not produce any crops. By round, 36.4% of those responding reported being credit constrained for at least one crop. However, many of these (43.2%) reported being unconstrained in credit access for at least one other crop. This combination of responses may arise if the household prioritizes financing for its primary crop, and does not expect to require financing on secondary crops. In some cases, these secondary crops require few purchased inputs. Indeed, only 6.5% of respondents indicated being credit constrained for more than one crop.

The second classification, which includes risk-rationed borrowers in the credit constrained cohort, can only be constructed for rounds 3 and 4, when question 3 was included. For those households responding in rounds 3-4, 59.7 percent of completed household-round observations were constrained by the measure including risk-rationed borrowers, compared to 41.3 percent constrained by the first classification in these two rounds. This high frequency of risk-rationed borrowers suggests that elicitation methods based only on rejected loan applications may be missing a potentially large group of constrained borrowers.

These credit constraint indicators based on questions asked in the agricultural production modules of the survey may reflect credit constraints only for this activity. Credit for agricultural production represents only 41.4 percent of total borrowing on average for borrowing households. Households may face a different probability of being credit constrained in other activities if credit is not perfectly fungible across uses. Some evidence on the fungibility of production loans for consumption purposes is provided by question 2. For example, among corn farmers that indicated wanting more credit, 8.9% said they would use the additional credit for household expenses. However, 87.2% said they would use the money for fertilizer or other farm expenses. This suggests a fairly low level of fungibility of production-based loans for consumption uses, and that the indicators used here may have limited relevance for consumption smoothing behavior.[14] We return to this issue in Section 4.

3.2 Summary of the data

Table 1 provides descriptive statistics for the sample by reported credit constraint status using the first classification of credit constraints because it was measured in three of the four survey rounds. The summary is taken over all households that completed the

[14] The fungibility of different credit sources across uses in the household depends on the timing of credit availability, the length of repayment, and the form of credit (whether cash or in-kind). For example, many of the consumption loans in the Bukidnon data are short duration loans that would not be suitable for financing agricultural production.

production modules of the survey for rounds 2-4, so that a household may move between the credit constrained and unconstrained cohorts across rounds. This is consistent with the idea that each household has a positive probability of being credit constrained, and we observe realizations of this probability in each round. The last column of Table 1 presents a t-test for equality of means of each variable across the two sub-samples.[15]

Table 1 includes some interesting evidence about the characteristics of households by credit constraint status. For example, the average value of loans taken by credit constrained households during the round is roughly twice as large as for credit unconstrained households, and this difference is significant. Also, the probability of having taken some loans is significantly more likely for credit constrained households (65%) than for unconstrained households (50%). Apparently, most credit constrained households are not kept out of the credit market entirely. They are likely to have positive borrowing, but would like to borrow more. Credit constrained households also have significantly lower education attainment.

The data also show that households headed by someone who is of Cebuano ethnicity, not Catholic, and not born in Misamis Oriental are significantly more likely to face credit constraints. Credit constrained households are larger on average and have more dependents. In addition, household heads in credit constrained households are significantly shorter than their unconstrained counterparts, which could arise from a height bias in lending similar to the one observed in many labor markets. This difference could also occur if taller people have greater earning capacity in farming activities

[15] This t-test for equality of the means assumes homogenous variance across the two distributions for each variable. These hypothesis tests are not valid if this assumption is violated. This is the Behrens-Fisher problem. Because the comparisons in Table 1 are mostly exploratory, we do not investigate the assumption of homogenous variance here.

requiring hard physical labor. Distance to the *poblacion* or administrative seat of the municipality government is positively associated with being credit constrained, an indication that proximity to lenders reduces transaction costs of borrowing. Not surprisingly, credit constraints are much more common in rounds 3 and 4 of the survey, during the peak season of agricultural production.[16] Finally, we show the distribution of credit constrained households across wealth quintiles measured by value of asset holdings and total household expenditure in round 1.[17] The profile of asset holdings is much steeper for credit constrained households than for the unconstrained, which suggests a positive relationship between wealth and the probability of being credit constrained. However, the profile based on expenditure is much flatter for credit constrained households.

4. Results

4.1 The probability of being credit constrained

The first step in assessing the validity of self-reported credit constrained status involves estimating the model in equations (1) and (2) to identify factors associated with the probability of being credit constrained. Because credit constraints arise from restrictions to latent excess demand, both demand- and supply-side factors enter the model. When

[16] Data from round 1 and round 4 were collected one year apart during the same season, but round 1 data is omitted from this analysis because it did not include information on credit constraints.

[17] Data on asset holdings were only collected in rounds 1 and 4 of the survey, so only round 1 holdings are presented here, as a indication of wealth levels at the start of the survey. Expenditure quintiles are also presented from round 1 in order to be directly comparable with the asset variables.

these two effects are opposing, it is usually not possible to determine which effect will dominate.

The explanatory variables include characteristics of the household head, the household, and the farm. Table 2 lists these variables and the expected direction of their effect on household demand for credit, the household's supply of credit or credit limit, and the probability that the household is credit constrained. Typical patterns of economic activity over the life cycle suggest that demand for credit is increasing in household head age until late in life. Supply of credit is also positively associated with age if lenders use age as a proxy for experience. As a result, the effect of age on the probability of being credit constrained is ambiguous. The education level of the household head could have competing effects on demand for credit: a positive effect if education improves managerial ability and a negative effect if education is associated with financial prudence and better savings behavior. On the supply side, education should increase credit limits set by lenders, so the net effect on the credit constraint probabilities is ambiguous. The height of the household head will increase credit supply if lenders exhibit a height bias similar to that found in the labor market. Height may also be a legitimate indicator of agricultural productivity (not indicated in the table) and may also reflect greater success in other labor markets, if attained height is positively correlated with other forms of human capital such as schooling, but these effects on supply and demand for credit may be nearly offsetting. This suggests a negative effect of height on credit constraints probabilities overall. The indicator variables for Cebuano ethnicity, Misamis Oriental as region of origin, and Catholic religion may have either a positive or negative effect on credit limits if lenders exhibit bias, or if these indicators serve lenders as reliable proxies for unobserved characteristics such as social capital or access to liquidity from other sources in the case of default.

The number of adults in the household increases demand for credit for consumption purposes, but reduces demand for labor on farm if factor markets are imperfect. We include separate counts of male and female adults to allow the magnitude of these effects to vary by gender. Having more adults in the household may increase access to credit if lenders prefer households with multiple income earners, so we posit the credit supply effect is either positive or zero. The overall effect of the number of adult household members is ambiguous. However, the number of children in the household increases the probability of being credit constrained, as they increase demand for credit and have no effect on credit limits. Distance to the *poblacion* also increases the probability of being credit constrained by raising transaction costs of obtaining loans. The association between the indicator for having any borrowing and the probability of being credit constrained is ambiguous. If the relationship is negative, it suggests that having credit makes it easier to obtain more credit, a possible indication of barriers to entry into credit borrowing. A positive relationship indicates that obtaining sufficient credit is more difficult than initial access to credit, i.e., that households are quantity rationed. The indicator variables for round 1 asset ownership quintiles capture wealth effects on the probability of being credit constrained. Wealthier households have higher demand for credit, but also higher credit limits, so the net effect of wealth on the probability of being credit constrained is ambiguous.

Three land variables are included in an attempt to decompose the effects of land on the probability of being credit constrained into demand effects associated with the scale of the farming operation and supply effects arising from the use of land as collateral. Land area operated per capita and its square are included as measures of demand for credit arising from demand for other factors such as labor, fertilizer and other variable inputs. The value of land owned in round 1 is also included as a more proximate indicator of the value of land as collateral. Although both land area operated per capita and first-round value of land owned should increase demand and supply for credit, land area should have a larger effect on credit demand (controlling for land quality through land values) and value of owned land should have a larger effect on credit supply (controlling for operated area). If so, land area will be positively associated with the probability of being credit constrained and value of owned land will have a negative association. Area devoted to sugar, corn, and rice is included as separate control variables for crop choice, with other crops as the residual category. These variables are intended to capture any favorable access to credit for farmers growing the commercial sugar crop in particular. Finally, credit constraints should be more likely in rounds 3 and 4 when demand for credit for agricultural production is at its peak.

Table 3 presents estimates of the probability of being credit constrained in rounds 2-4. Column 1 presents probit estimates with household random effects using an indicator for the first classification of credit constraints as the dependent variable, where a household is credit constrained in a survey round if it indicated wanting more credit than was available.[18]

[18] The random effects probit model was estimated using the *xtprobit* procedure in Stata (_____, *Stata Reference Manuals*, 2003). This procedure uses Gauss-Hermite quadrature to obtain parameter estimates. The approximations involved become less accurate as the panel size or intra-household correlation coefficient, ρ, increase. Although this is a short panel, we re-estimated the model using 8, 12, and 16 quadrature points. The difference in parameter estimates between all three models was very small. Results presented in Column 1 of Table 3 are those using 12 quadrature points.

The parameter estimates of the random effects probit model have the expected sign for all variables for which the direction of the effect was unambiguous. Household head age and education and the number of adult males and females have no significant association with the probability of being credit constrained, which is consistent with offsetting credit demand and supply effects for these variables. The negative effect of height of the household head on the probability of being credit constrained suggests lender bias in favor of taller applicants, although this effect is not significant (p-value = 0.127). Being Catholic, from Misamis Oriental, and not Cebuano all significantly reduce the probability of being credit constrained, a further sign of lender bias or that these characteristics are useful proxies to lenders for unobservables. Being Cebuano probably indicates being a migrant to the area and possibly lack of accumulated capital in the destination area and lower levels of social capital, which may limit access to lenders. The number of children in the household raises the probability of being credit constrained by increasing demand for liquidity as expected. The estimates also confirm that distance to the *poblacion* increases credit constraint probabilities through higher transaction costs for obtaining credit. Households with credit are significantly more likely to report being credit constrained, which is evidence that quantity rationing is another source of credit market imperfections. Obtaining the desired amount of credit appears to be a larger problem than entry into the credit market as a borrower. We successfully decomposed the competing effects of land into a source of demand for liquidity (measured by area operated per capita) and a source of collateral (measured by the value of land owned). Households with larger cultivated area per household member are significantly more likely to be credit constrained, but having more valuable land makes credit more available.[19] Sugar producers are less likely to be credit constrained than other farmers, but not significantly so.

Differentiating households by quintiles of asset holdings, the second lowest quintile has a markedly higher probability of being credit constrained, though the difference is not significant. These results provide some evidence that the poor (asset quintile 2) are more likely to face credit constraints than wealthier households (21.3% more likely than the highest quintile), but that the extremely poor (asset quintile 1, the omitted category) are not, probably because of fewer opportunities to use credit productively. As a further test of this result, we replaced asset holding quintiles in the regression with per capita expenditure quintiles.[20] In this regression, the poorest quintile had the highest probability of being credit constrained. Being in the wealthiest per capita expenditure quintile was associated with a statistically significant 45.1% reduction in the probability of being credit constrained compared to the poorest quintile.

Column 2 of Table 3 presents estimates of the probability of being credit constrained using the second credit constraint classification, which adds to the credit constrained cohort risk rationed households that did not want more credit because of fear of default. This broader measure of credit constraints is only available in rounds 3-4 of the survey, which included question 3 on why more credit was not desired. Including risk rationed households substantially expands the set of credit constrained households in rounds 3-4. Using the first classification, 41.3 percent of household-rounds were credit constrained in these rounds. After adding risk rationing, the credit constrained share

[19] Area of land cultivated per capita may be subject to simultaneity bias because credit constraints make it more difficult to finance land rentals. However, this bias has a downward effect on the parameter estimate, suggesting that the true effect is at least as large as the estimate in Table 1. Because the value of land owned in round 1 is predetermined, it is less likely to be subject to this bias.
[20] The results of this regression are available from the authors upon request.

jumps to 59.7 percent. This suggests that households claiming risk rationing in the credit market may have very different characteristics than those facing quantity rationing or high transaction costs. Risk rationing operates primarily by depressing demand for credit, and is not as closely tied to local availability of credit. As a result, supply side variables such as distance to the *poblacion*, father's height and the value of land as collateral should not have as large an effect on credit constraint probabilities. This prediction is born out in Table 3. Distance to the *poblacion* changed sign and is no longer significant, and the coefficients on father's height and the value of owned land are much smaller in absolute value in Column 2 than in Column 1. Also, other characteristics affect demand for credit differently in risk rationed households. For example, an increase in the number of children in the household may have a muted effect on credit demand if the household is risk rationed, as demonstrated by the smaller coefficient and insignificance of this variable in Column 2. Grouping households with different sources of credit constraints into one indicator also leads to less precisely estimated coefficients. Of course, part of the increase in standard errors in moving from Column 1 to Column 2 is due to fewer observations.

The inclusion of risk rationing in the constrained group also changes the wealth profile of the constrained. Households in asset holdings quintiles 2-4 are all significantly more likely to report being credit constrained than the poorest quintile when risk rationing is included. This difference is largest for quintile 2, but the next largest difference is for quintile 4.[21] This suggests that although wealthier households are less likely to be quantity rationed in the credit market, they are at least as likely to withdraw from taking advantage of additional credit opportunities due to concerns over risk. Crop

[21] The difference in coefficients between quintiles 2-4 is not significant.

choice in round 1 also plays a larger role when risk rationing is included in the definition of credit constraints. Sugar farmers are significantly less likely—and rice farmers are significantly more likely—to be credit constrained than other farmers in Column 2. This probably reflects the relative risk associated with growing these crops because wealth and liquidity differences between these farmers are mostly controlled for by other variables.

A final concern about model specification is that the estimates presented in Table 3 may be subject to selection bias because the questions used to elicit credit constraints were only asked to households actively engaged in farming. In order to test for selection based on agricultural production, we estimated a maximum likelihood probit model similar to that in Column 1 together with a selection equation for the farming activity. The p-value of the Wald test of independence of the credit constraints and selection equations was 0.323, suggesting that selection for farming activity does not lead to biased parameter estimates for the models presented in Table 3.

4.2 Credit constraints in the consumption smoothing models

As a further test of the validity of self-reported credit constraint status, we estimate the consumption smoothing models from Section 2. First, we estimate the model in equation (3) for survey rounds 2-4. The dependent variable is the change in log total household consumption from period *t-1* to period *t*, deflated by the consumer price index, where survey rounds are the period of observation. Treating survey rounds as distinct periods of observation is justified because they correspond fairly closely to agricultural seasons.[22] Control variables in X_{hvt} include age of the household head in round 1, number of

[22] The sugarcane crop is typically harvested only once per year. However, within the sample there was considerable variation in the timing of these harvests across survey rounds. Thus, the probability of being credit constrained for sugar farmers in the sample varies across rounds.

household members in the previous period, change in number of household members since the last period, lagged years of formal education of the household head and its square. In addition, we include a full set of *barangay*-round dummy variables, VD_{vt}. These remove the effects of covariate shocks within the *barangay* and control for differences in conditions in local credit markets over time, including differences in interest rates. The variable used to test for consumption smoothing is the change in log total household income from period *t-1* to period *t*, deflated by the consumer price index. In the Bukidnon data, 9.7 percent of observations have non-positive income values. These must either be dropped from the analysis or transformed because income changes are in (natural) logarithms. We transformed non-positive income values by $-\ln\left(abs\left(Y\right)\right)$ as a way of keeping them in the sample because dropping these observations led to substantial bias in estimates of β_1.[23]

Table 4 presents estimates of the consumption smoothing model in equation (3). Column 1 presents least squares estimates using rounds 2-4 for the full sample as a test of financial market imperfections in the full data. The results show that consumption is not sensitive to idiosyncratic income changes, suggesting initial support for the Permanent Income Hypothesis.[24] However, this least squares estimate may be subject to several sources of bias. First, income changes may be simultaneously determined with changes

[23] A strength of this arbitrary transformation is that more negative income values are transformed into smaller fractions. We also tried replacing non-positive income values with a small positive constant (alternately, 0.1 and 0.001) before taking logarithms and calculating changes in income. These approaches did not lead to substantially different results than those presented here.

[24] The estimate of β_1 in Column 1 of Table 4 is much smaller than estimates on other samples in the literature. For example, using a somewhat different methodology and using changes in food expenditure as the dependent variable, Altonji and Siow (1987) obtain a least squares estimate of $\hat{\beta}_1$ =.076 for a sample from the Panel of Income Dynamics in the United States. One reason for this difference is our transformation of negative income values before calculating changes in log income. When negative income values are omitted from this regression, the estimate we obtain is $\hat{\beta}_1$ =.019.

in consumption. Other sources of bias include measurement error in the income variable and imputation errors in the calculation of consumption of food and other goods produced at home. Measurement error could be a major source of bias because the income variable is an aggregation of income derived from a variety of sources across several survey modules. Even in the US, where salary and wage income dominates and is easier to measure, Altonji and Siow (1987) found substantial attenuation bias caused by measurement error in income. In Bukidnon, not only do agriculture and non-farm business activities comprise a much larger share of income on average, but using local prices to value consumption of food produced at home may lead to correlation between imputed components of the dependent variable and measurement error in income, leading to a positive bias in estimated income effects (Deaton, 1997). Given that the net effect of these sources of bias cannot be signed in advance it is prudent to make an effort to control for them using instrumental variables.

Column 2 of Table 4 presents instrumental variables estimates of the model in equation (3) for the full sample. The estimate of the sensitivity of consumption to income changes is more than three times larger than the least squares estimate in Column 1, although the parameter estimate is still insignificant. The jump in estimated income sensitivity probably indicates substantial measurement error in the construction of the income variable. In addition to the variables reported in Table 1 and the *barangay*-round indicator variables (the ‘included’ instruments), the ‘excluded’ instruments for changes in income include interactions of round 1 values of farm assets (carabao, plows and harrowers) with deviations in rainfall levels from long-run means, plus the squares of these variables.[25] We also include the lagged value of land owned and lagged area operated, both interacted with rainfall deviations, and their squares. In this agricultural setting, the interaction of past values of these productive assets with rainfall shocks are good candidate instruments; they should be correlated with income changes, but are unlikely to affect consumption changes including labor supply because their effects are mostly unanticipated. Other instruments include whether the husband's parents were landowners, indicators for negative shocks to income from rats and weather, the total duration of illnesses of all household members, the area of titled and untitled land owned in 1977, and the distance from the farm to the local market. Results of the first stage regression are reported in Table A1 in Appendix A. An F-test of these excluded instruments rejected that they have zero joint effects on changes in log income with a p-value of 0.001. An F-test for all instruments yielded a p-value of 0.000. Also, the Hansen J statistic test of overidentifying restrictions fails to reject this set of instruments. However, the Durban-Wu-Hausman test cannot reject equality of the coefficients between these IV estimates and least squares, suggesting that the least squares model in Column 1 is preferred because it is more efficient.

Another potential source of specification error in the model in equation (3) is correlation in the error term across observations within the same household. We estimated both random and fixed effects models to address this concern and in each case rejected the change in model specification in favor of OLS. This suggests that this model

[25] Rainfall levels by round were constructed using monthly rainfall data from the government's PAGASA weather station at Central Mindanao University in Musuan, Bukidnon, the only rainfall station in the sample area. This measure of rainfall is less accurate for villages further from Musuan. However, we expect rainfall deviations at this site to be highly correlated with actual rainfall deviations in all sample *barangays*, so interacting this measure of rainfall shocks with farm assets should provide good instruments for changes in income.

does not provide substantial variation in the estimated probability of being credit constrained within households over the three rounds. The models presented in Table 4 implicitly assume that household specific effects are uncorrelated with the probability of being credit constrained.

We now consider the effect of splitting the sample according to predicted credit constraint status generated from reported credit constraints for the model in equation (3), adapted to the switching regression model in equations (9.a) and (9.b). Because an indicator of credit constraint status is observed, consistent estimation of the switching regression is possible using separate Heckman selectivity models for the change in log consumption conditional on the probability of being credit constrained or unconstrained, respectively.[26] Columns 3 and 4 of Table 4 present estimates of the consumption smoothing model conditional on the household being predicted credit constrained and unconstrained, respectively.[27] The estimated effect of income changes on consumption changes is higher for the constrained group $\left(\hat{\beta}_{11} = .0028\right)$ than for the unconstrained group $\left(\hat{\beta}_{21} = .0017\right)$, as expected. However, neither parameter estimate is significantly different from zero, and a chi-square test of equality of these parameters across the two samples fails to reject the null hypothesis with a p-value of .775. These results show no evidence of credit constraints in either sample. As a result, they provide no support for

[26] Parameter estimates reported are based on maximum-likelihood estimates of the Heckman selectivity model, rather than the less efficient two-step estimator.

[27] The total number of observations is considerably smaller in the switching regressions models in columns 3 and 4 than in estimates for the full sample in columns 1 and 2 because the questions on credit constraints were only asked of farmers who were actively cultivating. Estimates of the credit constraint selection equation for the switching regression model are not presented in Table 4. These estimates differ inconsequentially from results presented in Table 3 due to a reduction in sample size from missing values for variables in the consumption smoothing model. For all switching regression models in Table 4, the dependent variable in the credit constraint selection equation is the indicator variable for whether the household had excess demand for credit. Estimates based on the credit constraint indicator that includes risk rationed households in the constrained group are generally similar and are available upon request.

the validity of this sample split. One possible interpretation of these results is that the predicted credit constrained sample is indeed constrained, but they can insure against negative income shocks so there is no measured sensitivity of consumption to income changes. This provides a motivation for turning to the models used by Jacoby and Skoufias (1997) in equations (6) and (7) that separate the effects of credit from insurance and differentiate between the effects of positive and negative income shocks.

The first step in estimating these models is to estimate the ancillary regression in equation (4) to decompose changes in income into estimates of anticipated and unanticipated idiosyncratic income changes. The explanatory variables in this model that capture anticipated income changes include household characteristics known at time *t-1*[28] and farm characteristics known at time *t-1*.[29] Unanticipated income changes are captured using these farm characteristics interacted with deviations from mean rainfall and the squares of these interactions.[30] F-tests showed that the variables capturing anticipated income changes are jointly significant (p-value=.001) as are the variables for unanticipated income changes (p-value=.000). Using the parameter estimates from this model, we calculate estimates of anticipated and unanticipated income changes as in equations (5.a) and (5.b).

We first estimate the model in equation (6) using the switching regression structure in equations (9.a) and (9.b) to allow the sensitivity of consumption to vary by predicted credit constraint status. Columns 5 and 6 of Table 4 present the estimates of

[28] The household characteristics include lagged household head education, lagged number of adult males, lagged number of adult females, lagged number of children, travel time to a doctor, and distance to the *poblacion*.

[29] The farm characteristics include the value of carabao owned in round 1, value of animal plows and harrows in round 1, lagged percent of hectare-months devoted to sugar, lagged percent of hectare-months devoted to corn, lagged percent of hectare-months devoted to rice, lagged value of land owned, and lagged area planted.

[30] The estimates of equation (4) are not included here, but are available upon request.

this model for the predicted constrained and unconstrained samples, respectively. A t-test for significance of the estimate on unanticipated income changes tests for the presence of complete insurance markets, and a t-test on the anticipated income change tests for credit constraints. We are unable to reject the hypothesis of complete insurance markets in either the predicted-credit-constrained or -unconstrained samples. For the sample predicted to be unconstrained, the estimated effect of anticipated income changes is negative, but small and insignificant, suggesting no evidence of credit constraints in that sample. For the sample predicted to be constrained, the model shows a large positive effect of anticipated income changes on consumption changes. The parameter estimate, $\hat{\beta}_{1a} = .034$, is quite large. However, we cannot reject that this estimate is statistically different from zero at standard significance levels, so again we find no conclusive evidence of credit constraints in the data, and only weak support for the sample split created using predicted probabilities of reported credit constraint status.

The restriction that positive and negative anticipated income changes have the same effect may contribute to these weak results for equation (6). The model in equation (7) relaxes this restriction, recognizing that it is easier to adjust consumption to positive income changes, through saving, than to negative income changes, which require availability of credit. The estimates of equation (9) for the predicted credit constrained sample are consistent with this argument. The estimated sensitivity of consumption to negative income changes, $\hat{\beta}_{1a}^{-} = .054$, is much larger than the effect of positive income changes, $\hat{\beta}_{1a}^{+} = .002$. However, the difference between these effects is not significant (Prob>chi2 = .260). In the predicted unconstrained sample, consumption sensitivity to negative income changes is much smaller than in the constrained sample, but the difference in these measured effects is also not significant (Prob>chi2 = .156). A surprising result is that the estimated effect of positive income changes for the unconstrained sample is negative and significant. A reduction of consumption in response to a positive anticipated income change is inconsistent with the theory.

4.3 Credit constraints in the farm labor demand model

Table 5 presents estimates of various specifications of the labor demand model in equation (8). The first specifications are reduced form models of labor demand. The dependent variable is total labor days worked on farm by round. In addition to the household demographic variables and other control variables, we include linear and quadratic terms for wages and area operated in order to introduce some flexibility in the specification. We resist any structural interpretation of the parameters of the reduced form models. However, testing exclusion restrictions on household demographic variables as a test of separability of production and consumption decisions, and of credit constraints, is still valid. Reduced form estimates of labor demand should be independent of household structure if production and consumption are separable. In later specifications, we estimate a structural labor demand model consistent with a restricted profit function using a Cobb-Douglas functional form.

Column 1 of Table 5 presents estimates of the reduced form model of labor demand for the full sample of farmers in rounds 2-4. The results show that household size has a positive and significant effect on farm labor demand. A one-person increase in household size increases total labor days on farm by 7.3 days (7.0%) in a four-month period. The prime male fraction has a negative effect and prime female fraction has a positive effect, but neither is significant.[31] An F-test of joint significance of the three household demographic variables rejects the null hypothesis at the one percent level of significance.[32] This is a strong rejection of separability for the farming portion of the sample. Based on the evidence of constraints to credit access in the sample presented in Section 3, it is possible that credit constraints contribute to this failure of the separability hypothesis. However, labor market imperfections are another prominent possible explanation. In order to strengthen the case for credit constraints, we estimate the labor demand model in (8) as a switching regression model as in equations (9.a) and (9.b), splitting the sample based on predicted, reported credit constraint status.

Columns 2 and 3 present estimates of the switching regression model for the credit constrained and credit unconstrained households, respectively.[33] In this model, the effect of household size is significant in both regimes, but is larger in the credit constrained regime. The effect of prime male fraction is also larger in the constrained regime, but is only significant at the ten percent level. Tests for the joint significance of the household demographic variables reject the null hypothesis of no effect at the one percent level for the constrained regime and at the five percent level for the unconstrained regime. These results support a failure of separation of production and consumption decisions in both credit constrained and unconstrained households, although with

[31] The wage rate has a negative effect on labor demand, as expected, though only the quadratic term is significant. Area operated has a positive and significant effect on labor demand. The effects of all other variables are insignificant in Column 1.

[32] Estimates in Column 1 are pooled OLS estimates of the model that includes multiple observations on each household. In order to account for the panel structure of the data, we also estimated the reduced form model using random effects. The results were very similar. A joint test of signficance of the household demographic variables for this model rejected the null with a p-value of .0002.

[33] The switching regression models presented in Table 5 are estimated (consistently) using a Heckman selection model to obtain estimates in each regime. Maximum likelihood estimates of this model did not converge in the unconstrained regime in many specifications, a fairly common outcome for this type of model. As a result, we estimated the model using Heckman's two-step method and adjusted the standard errors accordingly. Estimates of the first stage credit constraint probit are not shown here, but are only slightly different than those in Table 2 due to differences in sample size

stronger support for constrained households. With the measured effect of household size on labor demand twice as large in the credit constrained households as in unconstrained households, there is some evidence that the credit constraints are a cause of the failure of separation. To test whether this difference in effects is significant, we estimated the switching regression again using the two-stage approach described in Maddala (1983, p. 227), which creates a simple test for equality of the coefficients across regimes. We were unable to reject the equality of the household demographic variables, individually or in a joint test, using this model.[34]

These results provide only weak support for the reported credit constraint status of the households in the sample. Because the effect of the failure of separation is not significantly larger in the credit constrained households, we cannot reliably attribute the difference in effects to credit constraints. This result indicates the limited power of these tests. Although the credit constraint questions in the survey were asked in the context of production, it is difficult to separate the contribution of credit market imperfections from labor market imperfections in the tests for separability.

In Columns 4 and 5, we estimate the switching regression including a full set of *barangay*-round interaction terms. These terms control for wage and price differences and remove the effects of any omitted *barangay*-round level variables that may be correlated with household size. With these *barangay* and period effects removed, the estimated effect of household size is less different between credit constrained and unconstrained households. The effect of prime male fraction increases in both regimes, but is only significant for credit constrained households. In joint tests, the set of three

[34] Results of the estimates of this model are available upon request. Results of the joint test are presented in the last row of Table 5.

household structure variables remain significantly different from zero in both regimes. However, we still cannot reject equality of the effects of the household structure variables for constrained and unconstrained households. Other notable results are that household head education and distance to the municipality seat both have significant negative effects on labor demand for credit constrained households.

In Columns 6 and 7, we estimate the switching regression model with the labor demand equations in log-log form. We transform the dependent variable on labor days on farm and control variables on household size, the wage rate, and area operated by taking natural logarithms. This specification is that of a restricted labor demand function corresponding to a Cobb-Douglas production function. We omit the barangay-round dummy variables from this specification in order to estimate the effect of wages with this functional form. The results show that household size is positive and significant for both credit constrained and unconstrained households, but the elasticity of labor demand to household size in the credit constrained group (.601) is more than double that in the unconstrained group (.237). Moreover, the joint test of significance of the household demographic variables rejects the null hypothesis only for credit constrained households. This suggests stronger support for reported credit constraints from the survey than in previous models because the failure of separation is identified only for the credit constrained households. However, we are still unable to reject equality of the coefficients across these two sets of households, a small caveat to the support for the validity of respondents' claims of credit constraints from this model.

Finally, we estimate the log-log model again (see Columns 8 and 9) after expanding the definition of credit constrained households in the criterion function to include risk rationed households, or those that would not take more credit if it were available due to fear of default risk. Expanding the definition of credit constraints reduces the elasticity of labor demand to household size in both regimes. However, household size is only significant in the credit constrained regime, and the joint test for significance of the household demographic variables rejects the null only for the constrained regime. Also, for credit constrained households, the wage rate has a negative and, for the first time, significant effect on labor demand. As in the last model, distance to the municipality seat is positive and significant in the credit constrained regime. Also, the value of plows and harrows has a positive, significant effect on labor demand for credit constrained households and a negative, significant effect for the unconstrained. Farm equipment may induce greater demand for hired labor by constrained households, but may act as labor-saving technologies for unconstrained households. As expected, moving risk rationed households from the unconstrained to the constrained regime has made the results more consistent with theory. Still, we cannot reject equality of the effects of household structure across the two regimes in this specification either.

5. Conclusion

An unresolved issue in the enormous literature on the effects of credit constraints on welfare and productivity is the appropriate method for identifying credit constrained households. Jappelli, Pischke and Souleles (1998) have shown that the methods used to divide the sample by credit constraint status can substantially change estimated effects of credit constraints on outcomes. Given the current emphasis on credit programs as a catalyst for economic development, improving estimates of the costs of credit constraints could have significant policy and welfare implications.

Direct elicitation methods that identify credit constrained households in surveys by asking about unmet demand for credit have been suggested as an improvement over ad hoc methods, methods based purely on theory, or methods that attempt to measure household specific excess credit demand. This paper investigates the reliability of an approach to direct elicitation in which respondents are asked if more credit would have been used if it were made available. We provide several indirect tests of the validity of responses to this question using data on rural farm households from Bukidnon, the Philippines. The results indicate that both the activities for which credit constraint information is collected and the design of the questions capturing credit constraints affect the relevance and effectiveness of the resulting credit constraint indicators.

Using reported credit constraint status from farm production modules of the Bukidnon survey, we estimate the probability of being credit constrained and find that the model was generally consistent with expectations. We then tested the credit constraint classification in both consumption- and production-oriented models. In a model of consumption smoothing, we checked whether relying on reported credit constraint status to divide the sample led to expected changes in estimates of the sensitivity of consumption to income changes. Although the sensitivity of consumption changes to income generally grew in magnitude as we isolated credit constrained households and separated credit market imperfections from the role of insurance, we found no statistically significant evidence of credit constraints for those predicted to be constrained based on reported credit constraints. This weak support for the sample split suggested by reported credit constraint status could be due to the relatively weak power of the tests performed. These limitations may also be due to the production context of the credit constraints information. If credit is not entirely fungible across uses, even accurately reported credit constraints for production activities may not reliably be used to explain consumption behavior.

When we split the sample using reported credit constraints in a model of on-farm labor demand, results are more consistent with the theory. In the preferred log-log specification, household structure only had a significant effect on labor demand for credit constrained households. This was true whether risk rationed households were included among credit constrained households or not. However, the labor demand model did not provide complete support to the respondents' claims. The increase in measured effects of household structure on labor demand in credit constrained households over those in unconstrained households was not significant in any of the models estimated. As a result, we cannot state conclusively that the observed failure of separation of consumption and production in these models was due to credit constraints. Although the production model should have had greater power in this context, it was not able to fully validate reported credit constraints.

These results demonstrate that households can face differing degrees of credit constraints across different activities. This suggest that the context in which credit constraints indicators are collected and then applied can affect the conclusions from theory concerning the degree of support for credit constraints and their implications. Tests for the presence or welfare implications of credit constraints will be improved by using credit constraint indicators that are directly relevant to the theoretical model being tested. In addition, the evidence of limited fungibility of credit within the household suggests an alternative mechanism through which credit market imperfections, lender conditionalities or restrictions on the timing and form of credit can contribute to the failure of separation of production and consumption decisions and reduce welfare.

Table 1: Households Characteristics by Credit Constraint Status, Rounds 2-4

	Credit Constrained: Yes, would use more credit on some crops if available (N=387)		Credit Unconstrained: No, would not use more credit on any crop if available (N=677)		Equality of means t-test[1]
	Mean	Std Dev	Mean	Std Dev	
Value of loans taken in 84 ('000 pesos)	2.94	12.62	1.46	6.83	**
Age of household head in round 1	37.12	8.38	36.63	8.52	
Years of formal education of household head	5.49	2.90	5.93	3.12	**
Cebuano ethnicity of household head, %	52.71	49.99	45.35	49.82	**
Household head born in Misamis Oriental, %	9.30	29.08	13.15	33.82	*
Household head is catholic, %	91.21	28.35	94.39	23.03	**
Household size	7.66	2.80	7.22	2.62	***
Dependency ratio[2]	1.72	0.87	1.63	0.82	*
No. male household members age >15	1.54	0.84	1.46	0.88	
No. female household members age >15	1.39	0.76	1.36	0.66	
No. household members age <=15	4.35	1.79	4.06	1.77	***
Percent total hectare-months to sugar	14.94	26.65	13.77	26.55	
Percent total hectare-months to corn	33.66	27.77	34.23	28.74	
Percent total hectare-months to rice	6.50	13.74	4.76	12.95	**
Average height of father, rounds 1-4	160.74	5.96	161.57	5.47	**
Distance to poblacion (kilometers)	5.85	3.36	5.19	3.47	***
Travel time to nearest doctor (minutes)	44.08	30.33	40.22	25.29	**
Value of owned land in round 1 ('000 pesos)	3.75	5.42	4.12	5.83	
Area cultivated per capita (hectares)	0.50	0.48	0.48	0.50	
1 if household has any debt	64.08	48.04	50.37	50.04	***
Value of carabao owned, round 1 ('000 pesos)	5.82	12.34	6.45	15.93	
Value of plows, harrows owned, round 1 ('000 pesos)	1.00	1.53	1.03	2.69	
Quintile # 1 of round 1 value of assets, %	10.59	30.82	13.59	34.29	
Quintile # 2 of round 1 value of assets, %	19.90	39.97	17.73	38.22	
Quintile # 3 of round 1 value of assets, %	21.71	41.28	22.01	41.46	
Quintile # 4 of round 1 value of assets, %	22.48	41.80	23.63	42.51	
Quintile # 5 of round 1 value of assets, %	25.32	43.54	23.04	42.14	
Quintile # 1 of round 1 total expend quintile, %	20.67	40.55	17.43	37.96	
Quintile # 2 of round 1 total expend quintile, %	18.60	38.96	19.65	39.76	
Quintile # 3 of round 1 total expend quintile, %	20.16	40.17	18.61	38.95	
Quintile # 4 of round 1 total expend quintile, %	20.93	40.73	20.83	40.64	
Quintile # 5 of round 1 total expend quintile, %	19.64	39.78	23.49	42.42	
Dummy variable for round 2, %	23.26	42.30	37.67	48.49	
Dummy variable for round 3, %	39.28	48.90	30.58	46.11	
Dummy variable for round 4, %	37.47	48.47	31.76	46.59	

[1]Difference in means: *** significant at 1%; ** significant at 5%; * significant at 10%.
[2]Dependency ratio = (no. of children (<16) + no. of elderly (>65)) / no. of adults (16-65)).
All variables are by round for rounds 2-4, unless otherwise noted.

Table 2: The Proposed Effect of Household and Farm Characteristics on the Probability of Being Credit Constrained

Key: + positive ++ positive and large – negative +/– ambiguous 0 no effect	Household demand for credit D_t	Supply of credit to the household S_t	Probability of being credit constrained $Pr(D_t – S_t > 0)$
Household Head Characteristics			
Ln age of household head in round 1	+	+	+/–
Years of formal education of household head	+/–	+	+/–
Average height of father, rounds 1-4	0	+	–
Cebuano ethnicity of household head	0	+/–	+/–
Household head born in Misamis Oriental	0	+/–	+/–
Household head is Catholic	0	+/–	+/–
Household Characteristics			
No. male household members age >15	+/–	+/0	+/–
No. female household members age >15	+/–	+/0	+/–
No. household members age <=15	+	0	+
Distance to *poblacion*	0	–	+
1 if household has any debt	+/–	+/–	+/–
Quintile # 1 of round 1 value of assets	–	–	+/–
Quintile # 2 of round 1 value of assets	–	–	+/–
Quintile # 3 of round 1 value of assets	+/–	+/–	+/–
Quintile # 4 of round 1 value of assets	+	+	+/–
Quintile # 5 of round 1 value of assets	+	+	+/–
Farm Characteristics			
Area cultivated per capita	++	+	+
Area cultivated per capita squared	–	–	+/–
Value of owned land in round 1	+	++	–
Percent total hectare-months to sugar	+	+	+/–
Percent total hectare-months to corn	+	+	+/–
Percent total hectare-months to rice	+	+	+/–
Dummy variable for round 3	+	0	+
Dummy variable for round 4	+	0	+

Table 3: The Probability of Being Credit Constrained

Dependent variable:	Credit constrained if desired more credit	Credit constrained if desired more credit, or if avoiding default risk
	(1)	(2)
Household Head Characteristics		
Ln age of household head in round 1	0.015	-0.240
	(0.288)	(0.324)
Years of formal education of household head	-0.020	-0.027
	(0.019)	(0.021)
Ln average height of father, rounds 1-4	-2.235	-1.257
	(1.465)	(1.655)
Cebuano ethnicity of household head	0.224	0.229
	(0.107)**	(0.122)*
Household head born in Misamis Oriental	-0.308	-0.062
	(0.169)*	(0.180)
Household head is Catholic	-0.413	-0.176
	(0.202)**	(0.234)
Household Characteristics		
No. male household members age >15	0.071	0.049
	(0.068)	(0.077)
No. female household members age >15	-0.042	0.059
	(0.079)	(0.092)
No. household members age <=15	0.055	0.012
	(0.031)*	(0.034)
Distance to *poblacion*	0.038	-0.004
	(0.016)**	(0.018)
1 if household has any debt	0.273	0.169
	(0.098)***	(0.115)
Quintile # 2 of round 1 value of assets	0.256	0.522
	(0.201)	(0.216)**
Quintile # 3 of round 1 value of assets	0.091	0.364
	(0.200)	(0.209)*
Quintile # 4 of round 1 value of assets	0.085	0.436
	(0.212)	(0.226)*
Quintile # 5 of round 1 value of assets	0.043	0.252
	(0.257)	(0.277)
Farm Characteristics		
Area cultivated per capita	0.676	0.843
	(0.308)**	(0.328)**
Area cultivated per capita squared	-0.223	-0.229
	(0.112)**	(0.110)**
Value of owned land in round 1	-0.014	-0.005

	(0.011)	(0.012)
Percent total hectare-months to sugar	-0.112	-0.518
	(0.223)	(0.240)**
Percent total hectare-months to corn	0.144	0.270
	(0.220)	(0.241)
Percent total hectare-months to rice	0.350	0.790
	(0.367)	(0.427)*
Dummy variable for round 3	0.477	0.067
	(0.109)***	(0.104)
Dummy variable for round 4[1]	0.389	
	(0.109)***	
Constant	10.069	7.043
	(7.650)	(8.651)
Observations	1007	706
Number of households	347	353
P-value: Wald chi2(23)	0.000	0.042
Proportion of variance explained by household-level error component, ρ	0.167	0.119
LR p-value for H_0: $\hat{\rho} = 0$	0.002	0.105

*** significant at 1%; ** significant at 5%; * significant at 10%.
Standard errors in parentheses, are Huber/White robust standard errors.
[1]The Round 4 dummy variable is omitted from estimates in Column 2 due to collinearity with other explanatory variables.

Table 4: Estimates of Change in Ln Consumption

Dependent Variable: Change in ln real expenditure from last period	Equation (3) Full Sample OLS	Equation (3) Full Sample IV	Equation (3) Predicted constrained	Equation (3) Predicted un-constrained	Equation (6) Predicted constrained	Equation (6) Predicted un-constrained	Equation (7) Predicted constrained	Equation (7) Predicted un-constrained
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Change in ln real income from last period	0.003	0.009	0.003	0.002				
	(0.002)	(0.013)	(0.003)	(0.003)				
Unanticipated change in ln real income from last period					0.003	-0.004	0.002	-0.003
					(0.019)	(0.010)	(0.019)	(0.010)
Anticipated change in ln real income from last period					0.034	-0.008		
					(0.022)	(0.013)		
Anticipated positive change in ln real income from last period							0.002	-0.067
							(0.045)	(0.029)**
Anticipated negative change in ln real income from last period							0.054	0.010
							(0.036)	(0.012)
Age of household head in round 1	0.000	0.000	0.000	0.001				
	(0.002)	(0.001)	(0.003)	(0.002)				
Household size lagged one period	-0.007	-0.007	-0.007	-0.008				
	(0.005)	(0.005)	(0.010)	(0.007)				
Difference in household size from last period	0.044	0.042	0.029	0.049				
	(0.020)**	(0.020)**	(0.041)	(0.026)*				
Household head education lagged one period	-0.013	-0.013	0.000	-0.024				
	(0.010)	(0.010)	(0.024)	(0.014)				
Household head education lagged one period, squared	0.001	0.001	-0.000	0.001				
	(0.001)	(0.001)	(0.002)	(0.001)				
Observations	1254	1254	924	924	921	921	921	921
Uncensored (included) observations	–	–	324	600				
R^2	0.224							
F-test of excluded instruments, p-value		0.001						
Durbin-Wu-Hausman test, chi-sq(1) p-value		0.557						
Hansen J statistic test of overidentifying restrictions, chi-sq(14) p-value		0.622						

*** significant at 1%; ** significant at 5%; * significant at 10%. Standard errors in parentheses are Huber/White robust standard errors, except in the test of the Hansen J statistic. All regressions included a full set of *barangay*-round dummy variables (estimates not shown).

Table 5: Estimates of On-farm Labor Demand`

Dependent Variable: Total labor days on farm (hired or household sources), by round[b]	Full Sample OLS	Credit constrained	Credit un-constrained	Credit constrained, village-round effects[a]	Credit unconstrained village-round effects[a]	Cobb-Douglas, Credit constrained	Cobb-Douglas, Credit un-constrained	Cobb-Douglas, Credit constrained[c]	Cobb-Douglas, Credit un-constrained[c]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Household size[b]	7.313	7.995	3.965	6.180	4.829	0.601	0.237	0.535	0.198
	(2.221)***	(3.302)**	(2.004)**	(3.211)*	(2.033)**	(0.197)***	(0.136)*	(0.147)***	(0.166)
Prime male fraction	-39.699	-131.838	-13.063	-143.318	-32.898	0.019	0.201	0.459	-0.138
	(35.861)	(68.631)*	(43.155)	(69.647)**	(41.585)	(0.564)	(0.421)	(0.437)	(0.509)
Prime female fraction	13.540	84.701	-62.307	82.333	-56.286	0.510	-0.305	0.066	-0.326
	(51.362)	(79.517)	(49.341)	(77.361)	(47.284)	(0.666)	(0.491)	(0.503)	(0.627)
Village average real male wage for planting-harvesting tasks[b]	-1.073	-2.530	-3.399			-0.153	-0.074	-0.252	0.095
	(0.837)	(1.792)	(3.310)			(0.137)	(0.111)	(0.113)**	(0.133)
Village average real male wage for planting-harvesting tasks squared	0.020	0.029	0.087						
	(0.010)**	(0.025)	(0.075)						
Area operated[b]	9.072	0.839	17.926	2.780	18.378	0.513	0.671	0.575	0.676
	(4.540)**	(4.038)	(2.586)***	(3.973)	(2.676)***	(0.071)***	(0.047)***	(0.050)***	(0.055)***
Area operated squared	0.440	1.091	-0.242	1.046	-0.385				
	(0.306)	(0.165)***	(0.121)**	(0.166)***	(0.122)***				
Ln age of household head	-3.355	12.992	-6.722	23.844	-5.920	-0.101	-0.010	-0.014	-0.063
	(14.234)	(31.586)	(18.394)	(32.067)	(17.983)	(0.260)	(0.176)	(0.190)	(0.222)
Household head education	1.208	-0.711	1.836	-4.533	1.431	0.015	0.001	0.011	-0.004
	(0.964)	(2.055)	(1.258)	(2.087)**	(1.295)	(0.017)	(0.012)	(0.012)	(0.014)
Value of carabao owned round 1 (‘000 pesos)	-0.002	0.616	0.361	0.327	0.127	0.003	0.005	0.003	0.006
	(0.431)	(0.504)	(0.265)	(0.484)	(0.251)	(0.004)	(0.003)**	(0.003)	(0.003)**
Value of plows and harrows owned round 1 (‘000 pesos)	4.604	-4.521	3.536	-8.382	2.616	0.063	-0.018	0.052	-0.056
	(4.964)	(5.194)	(1.616)**	(5.246)	(1.529)*	(0.040)	(0.015)	(0.021)**	(0.019)***
Distance to the municipality seat	1.657	2.705	0.155	-10.783	-2.007	0.035	-0.005	0.026	-0.006
	(1.245)	(1.916)	(1.239)	(4.301)**	(2.255)	(0.015)**	(0.012)	(0.012)**	(0.014)
Constant	0.585	39.169	36.713	58.666	38.996	3.053	3.163	2.898	3.206
	(52.540)	(105.523)	(70.439)	(141.334)	(99.529)	(0.898)***	(0.648)***	(0.671)***	(0.805)***

Observations	992	992	992	992	992	990	988	989	989
R^2	0.454								
Joint test ($F(3,n\text{-}k)$ or $\chi^2(3)$) of household demographic variables, p-value	0.000	0.009	0.046	0.029	0.008	0.007	0.245	0.001	0.338
Joint *F* test of difference in effects of household demographic variables between credit-constrained and -unconstrained regimes, p-value		0.451		0.727		0.885		0.653	

*** significant at 1%; ** significant at 5%; * significant at 10%.

[a] Model includes full set of *barangay*-round interaction terms. Estimates for these terms are not shown.

[b] In log form for Cobb-Douglas labor demands in Columns (6)-(9).

[c] Definition of credit constrained household for this model includes those desiring more credit and those who did not seek credit to avoid default risk.

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Table A1: First-Stage Estimates of Change in Ln Income for IV Consumption Smoothing Model

Dependent variable:[1]	Change in Ln Real Household Income from previous period
Age of household head in round 1	0.007
	(0.024)
Household size in previous round	0.000
	(0.082)
Change in Household size between rounds	0.301
	(0.253)
Lag of years of formal education of household head	0.044
	(0.148)
Lag of years of formal education of household head squared	-0.001
	(0.010)
Value of carabao owned in round 1 times deviations of rainfall from LR mean	0.000
	(0.001)
Value of animal plows and harrows in round 1 times deviations of rainfall from LR mean	-0.024
	(0.008)***
Lagged value of land owned times deviations of rainfall from LR mean	-0.002
	(0.004)
Lagged area operated times deviations of rainfall from LR mean	0.007
	(0.006)
Square of the value of round 1 carabao times deviations of rainfall from LR mean	-0.000
	(0.000)
Square of round 1 plow and harrow values times deviations of rainfall from LR mean	0.001
	(0.000)***
Square of lagged value of land owned times deviations of rainfall from LR mean	0.000
	(0.000)
Square of area operated times deviations of rainfall from LR mean	-0.000
	(0.000)**
1 if husband's parents were land owners	0.037
	(0.297)
1 if household experienced rat infestation in fields during production this round	-0.821
	(0.599)
1 if household experienced a severe rainfall shortage this round	-0.051
	(0.555)
Total duration of illnesses of all household members	-0.004
	(0.011)
Area of land household owned with title in 1977, hectares	0.068
	(0.115)
Area of land household owned without title in 1977, hectares	0.064
	(0.152)

Distance to market, km	0.044
	(0.085)
Constant	-0.936
	(1.161)
Observations	1254
R^2	0.154
F(106,1147), p-value	0.000

*** significant at 1%; ** significant at 5%; * significant at 10%.
Standard errors in parentheses, are Huber/White robust standard errors.
[1]Parameter estimates for full set of *barangay*-round interaction terms are omitted.