Decomposing Deforestation after a Food Price Shock: Evidence from Cambodia

Steven W. Wilcox, Ariel Ortiz-Bobea, David R. Just*

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Abstract

Cash crop markets are commonly assumed to be primary drivers of agricultural expansion associated with deforestation. An alternative mechanism that has received scant attention, which may drive cash crop expansion and deforestation, is staple food markets. In this paper, we use the 2008 global staple food price shocks to study high rates of deforestation in Cambodia over 2004-2014. Using a shift-share instrumental variables strategy we estimate elasticities of 2-5 for the effect of local mean rice prices on deforestation; estimates for price standard deviation are 3-4 times smaller. Furthermore, we show that households were likely rice net-buyer dominant, and that household agricultural expansion is positively associated with local deforestation.

Keywords: deforestation, land-use change, staple food prices, agricultural households.

JEL Classification: O13, Q12, Q18.

*Corresponding author: Steven W. Wilcox, sww62@cornell.edu. Author affiliations: all co-authors are affiliated with the Charles H. Dyson School of Applied Economics and Management at Cornell University (Ariel Ortiz Bobea: ao332@cornell.edu; David R. Just: drj3@cornell.edu). We gratefully acknowledge support of the Richard Bradfield Research Award from the College of Agriculture and Life Sciences and the Dyson School Graduate Program at Cornell University. We thank Christopher B. Barrett, Brian Dillon, John F. Hoddinott, Shanjun Li, Cynthia Lin Lawell, Prabhu L. Pingali, Peter Potapov, Sasha Tyukavina, Avralt-Od Purevjav, Tung Dang, Maulik Jagnani, Todd Gerarden and seminar participants at Cornell University, NEUDC, and Utah State University for helpful suggestions and insightful comments. Additional comments, and assistance with field work in Cambodia, were provided by researchers at IRRI and the FAO in Southeast Asia. Mr. Lundy Saint at the National Institute of Statistics of Cambodia answered many questions about CSES data, as did Hans Pettersson and Veronica Wikner at Statistics Sweden. All errors are our own.

1 Introduction

Over 2007-2011 the world experienced historic price spikes for staple foods on a scale not seen since the 1970s. A large economics literature has subsequently studied the causes of the price spikes, policy responses (e.g. de Gorter, Drabik, and Just 2015, Pinstrup-Andersen 2015), and welfare outcomes (Headey and Martin 2016). Although these shocks are comparatively well studied, at least one important dimension of potential impact has been overlooked: land-use change response.

Land use is important in this context because economic theory suggests various means whereby price shocks of the scale observed in recent history may have precipitated widespread land-use change. This notion may have important implications for phenomena like deforestation, which is frequently observed to be associated with agricultural land use, and widely thought to be driven in good measure by international cash crop markets (Henders, Persson, and Kastner 2015, Ordway, Asner, and Lambin 2017, Pendrill et al. 2019, Chaves et al. 2020, zu Ermgassen et al. 2020). It is less commonly appreciated that staple food prices may have significant roles in deforestation, and market-driven land-use change generally, though theoretical support for this mechanism has been around for decades (Barrett 1999).

In this paper, we leverage the recent food price shock era as a natural experiment to study land-use change as a function of a change in the price of staple food. These shocks are informative for this topic not only because they may shed light on less commonly considered drivers of phenomena like global deforestation, but also because of the unique potential impacts from these shocks on households – particularly via welfare impacts from staple food production and consumption. As such, these shocks embody a unique opportunity to study the interplay of aggregate land-use change patterns and micro-level land-use behavior amidst potentially important impacts to basic welfare.

Our research objectives are two-fold. First, we seek to understand whether or not local shocks to the price of staple foods caused aggregate local land-use change ex-post of sufficient scale to comprise overlooked environmental externalities. Second, if aggregate impacts can be substantiated, we wish to know whether or not household-level adaptive responses to the price shocks, expressed through land use,

were an underlying mechanism. For an empirical context, we study these questions in the setting of Cambodia, which experienced some of the highest national rates of deforestation in the world over the 2000-2014 period (Petersen et al. 2015) with a sharp increase within a short lag of the 2008 rice price spike[1].

To our knowledge, no prior empirical work has managed to address these aggregate and household-level questions together in any research setting focused on commodity prices and land-use change. This is unfortunate as answers to these nested questions should offer insights into market-driven patterns of land use. Research progress on these scores should also speak to long-standing theoretical ideas surrounding agricultural responses to price shocks (e.g. Sandmo 1971, Finkelshtain and Chalfant 1991, de Janvry, Fafchamps, and Sadoulet 1991, Fafchamps 1992).

To motivate our empirical analysis, we develop an agricultural household framework covering both separable and non-separable cases. Our focus here is to study how a staple food price change impacts general land-use choice at the extensive margin. Using this framework, we develop several propositions that establish conditions where a staple food price shock not only has direct effects that increase staple land use, but also indirect effects that increase land devoted to non-staple land uses, and the total amount of land put to productive use.

To test the aggregate implications of our theoretical framework, we first test whether local staple food price shocks circa 2007-2011 lead to aggregate land-use change ex-post. For Cambodia this objective permits focus on rice prices and deforestation over 2004-2014. We then test for mechanisms suggested by our models by studying household-level extensive margin staple and non-staple agricultural responses and household-level rice marketable-surplus positions, which our theoretical framework shows may be important for the direction of response.

As a measure of aggregate land-use change we use remotely sensed data on deforestation from Hansen et al. (2013), which we disaggregate by Cambodia's administrative levels and prominent aspects of land-tenure. For rice price data, we use rich, spatially-disaggregated consumer and producer price series from the Cambo-

[1] In their global land cover study of land use ex-post of deforestation, Curtis et al. (2018) attribute most 2001-2015 deforestation in Southeast Asia to "commodity-driven deforestation" ("...conversion ... to a non-forest land use such as agriculture, mining, or energy infrastructure").

dia Socioeconomic Survey (CSES) over 2004 and 2007-2013, which has a continuous, monthly sampling frequency. To study household-level rice marketable-surplus, we use CSES data to estimate rice-focused net benefit ratios (NBR[2], after Deaton 1989), which are short-run welfare measures that encapsulate rice marketable surplus. For measures of household-level land use, we gather season-specific agricultural land use data from the CSES, which we use to construct measures of rice and cash-crop land allocation. To our knowledge, no prior work in this space has leveraged a comparable combination of data to study commodity prices and land-use change – let alone in the context of a historic global price shock.

Our tests focusing on deforestation instrument for local rice price variation at a one-year lag of local deforestation. For rice price variation, we estimate the local mean and standard deviation of consumer and producer rice price series and employ a shift-share[3] instrument comprised of the interaction between centroid road distances to Cambodia's deep sea port and a US-based average international rice price. In our preferred specifications, we estimate large positive elasticities in the range of 2 to 5 for areas dominated by smallholder agriculture, which are statistically significant at the 1% level. We find positive elasticities of roughly half this magnitude when deforestation originating within Cambodia's economic land concessions (ELCs)[4] is the dependent variable. In specifications focused on local price standard deviation, we estimate positive elasticities between 0.5 to 2 that are also statistically significant, with larger magnitude responses also coming from areas dominated by smallholders. Impulse-response estimation also yields some evidence for extended impacts up to four years ex-post of the shock. Our results are robust to alternative specifications, inclusion of other commodity prices, and a battery of tests, including leave-one-out, placebo and permutation tests.

For our descriptive study of NBR distributions, we estimate monthly and annual non-parametric densities. The resulting distributions suggest substantial variation in the relative density of rice net-buyers versus net-sellers over the period of study,

[2]The NBR is the ratio of the value of marketable surplus to income.

[3]Shift-share instruments interact a time-varying "shift" with more time-invariant "shares". A raft of new papers study shift-share research designs (e.g. Goldsmith-Pinkham, Sorkin, and Swift 2020).

[4]ELCs are long-term leases granted to foreign or domestic investors by the Cambodian government for commercial development (e.g. see discussion in Davis et al. 2015).

though net-buyers were likely dominant (see related discussion in Section 5). These findings suggest diverse land-use incentive regimes playing out on the landscape.

For our extensive margin analysis, we estimate reduced-form specifications[5] that incorporate our instrument, lags of local deforestation, and household-level covariates. The dependent variables of interest are household-level, seasonal land allocation shares of rice and cash crops, which we also aggregate within and across seasons. Here we find statistically significant relationships between land allocation shares and our instrument, and increased local deforestation outside of ELCs.

Our work contributes to several additional strands of literature. With respect to the staple food price spikes, there is some empirical work studying agricultural supply responses. Magrini, Balié, and Morales-Opazo 2017 and Nakelse et al. 2018 study staple supply responses for several staple commodities in Africa covering the food price shock period and generally find positive and inelastic to marginally elastic responses. An ex-ante example is Yu and Fan (2011) who use 2004 and 2007 CSES data to estimate own-price production elasticities to simulate[6] Cambodia's potential rice supply response from the rice price shock.

Contributions in the space of trade, commodity prices, land-use change and deforestation are also closely related to our work. Recent trade-focused examples include Alix-Garcia et al. (2018) who finds evidence of specialization induced land-use change following the 1850 Austro-Hungarian trade union, and Abman and Lundberg (2020) who find evidence of regional trade agreements increasing deforestation at the country level. Outside of economics, there is also growing focus on international trade and particularly international demand for commodities that are so-called "deforestation intensive" (e.g., Henders, Persson, and Kastner 2015).

Regarding commodity prices, Busch and Ferretti-Gallon (2017) cite four econometric papers studying commodity prices and deforestation. We encountered seven other papers in this vein (list available upon request) from the 1990s to more recent (e.g., Barbier and Burgess 1996, Bragança 2018). Our view is that the related literature has had great difficulty in linking observed local price variation to local

[5]Corresponding instrumental variables specifications generally reinforce these results, but missing data from the pre-shock period leads us to favor the reduced-form.

[6]They find positive inelastic and marginally elastic short- and long-run responses respectively.

deforestation measures (often resorting to imputed prices), economically significant price shocks have rarely been studied, and use of aggregated price indices or international prices has limited clean tests of underlying commodity-price mechanisms (e.g., Wheeler et al. (2013), Assunção, Gandour, and Rocha 2015, Assunção et al. 2020). As a result, it is challenging to draw generalizable knowledge from the related literature. Other than our work, Lundberg and Abman (2021) is the only known empirical study focused on staple prices and deforestation. Using a panel of Sub-Saharan African countries over 2002-2013, Lundberg and Abman (2021) find increased maize volatility leads to lower subsequent deforestation with no significant effect from maize price levels.

Another related strand of work is literature on biofuels and indirect land-use change (e.g. Searchinger et al. 2008). The relationship stems from evidence that US biofuels policies played a role in propagating the initial 2008 price shock through the bridge these policies established between food and fossil fuel markets (de Gorter, Drabik, and Just 2015). Indirect land-use change typically considers the extra land needed for food production displaced by biofuel crops. Our work suggests an alternative mechanism for indirect land-use change stemming from biofuels policy as Cambodia has only a nascent biofuels sector (e.g. cassava and sugar cane).

The remainder of our paper proceeds as follows. In section 2 we provide background on Cambodia over our study period. Section 3 covers our theoretical models. Section 4 describes our data and our econometric strategy. Section 5 covers summary statistics. Section 6 presents our results and section 7 will conclude.

2 Cambodia, Rice Prices, and Deforestation

Rice is a pillar of the Cambodian economy, a reality clearly demonstrated in figure 1 by the dominance of rice area harvested over 1961-2017. Figure 1 also shows how severely Cambodia's civil war impacted rice production in the 1970s and how long it took to recover pre-war production levels. Beyond increases in rice production, a few other crops also show recent upward trends[7] (e.g. cassava).

[7] Analogous figures in our appendix explore these trends for crop value and total production.

An aggregate approximation of the welfare effects from the rice price shock is Cambodia's trade position in rice. Parsing Cambodia's status in this regard, during our study period, is reportedly made difficult due to significant informal[8] rice trade. Research by Sophal (2011), based on a contemporaneous survey of 2,235 households, suggests most rural households were net-buyers (implying net-negative impacts) and many respondents reported contemporaneous increased costs of living and adoption of coping strategies, including increased use of common pool resources. In March 2008, Cambodia briefly banned rice exports in response to the shock. However, informal trade networks combined with limited storage for the contemporaneous dry season paddy harvest weakened the export ban and it was rescinded in May 2008 (Pandey and Humnanth 2010, Sophal 2011).

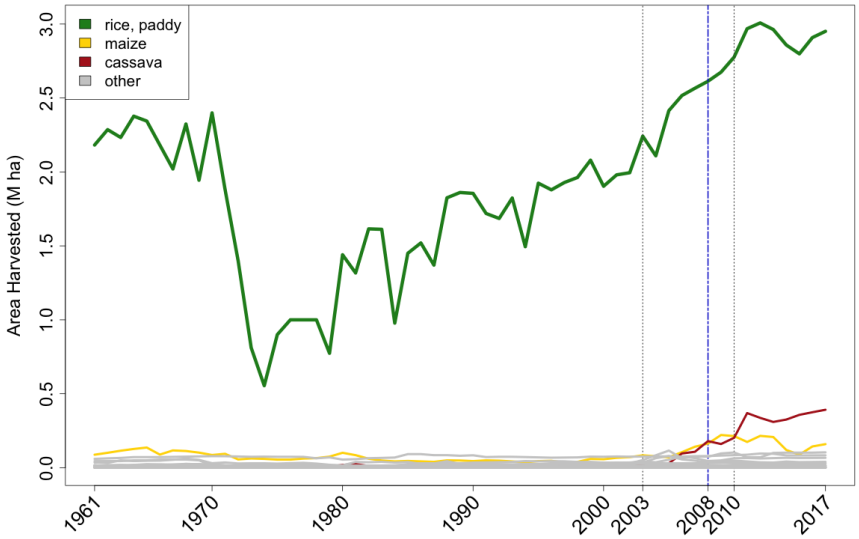

Figure 1: Area harvested 1961-2017 in millions of hectares (FAOSTAT data, authors' calculations). Vertical lines mark: production increases for several crops (2003); the peak rice price shock (2008); and peak deforestation (2010).

Figure 2 presents plots of important rice price variation. The top panel shows the international rice price series we employ in our identification strategy. The bottom panel shows the first two moments of the low-quality rice unit value series

[8]This speaks to larger rice market challenges (Pandey and Humnanth 2010, Sokhorng 2018 cite limited storage and milling capacity). Thailand and Vietnam (the region's largest rice exporters) are Cambodia's primary rice trading partners (Indonesia and The Philippines are the largest importers).

we employ in our econometric models. Our appendix provides several[9] related supporting figures of agricultural commodity prices, including the wet season paddy series we also use in our models.

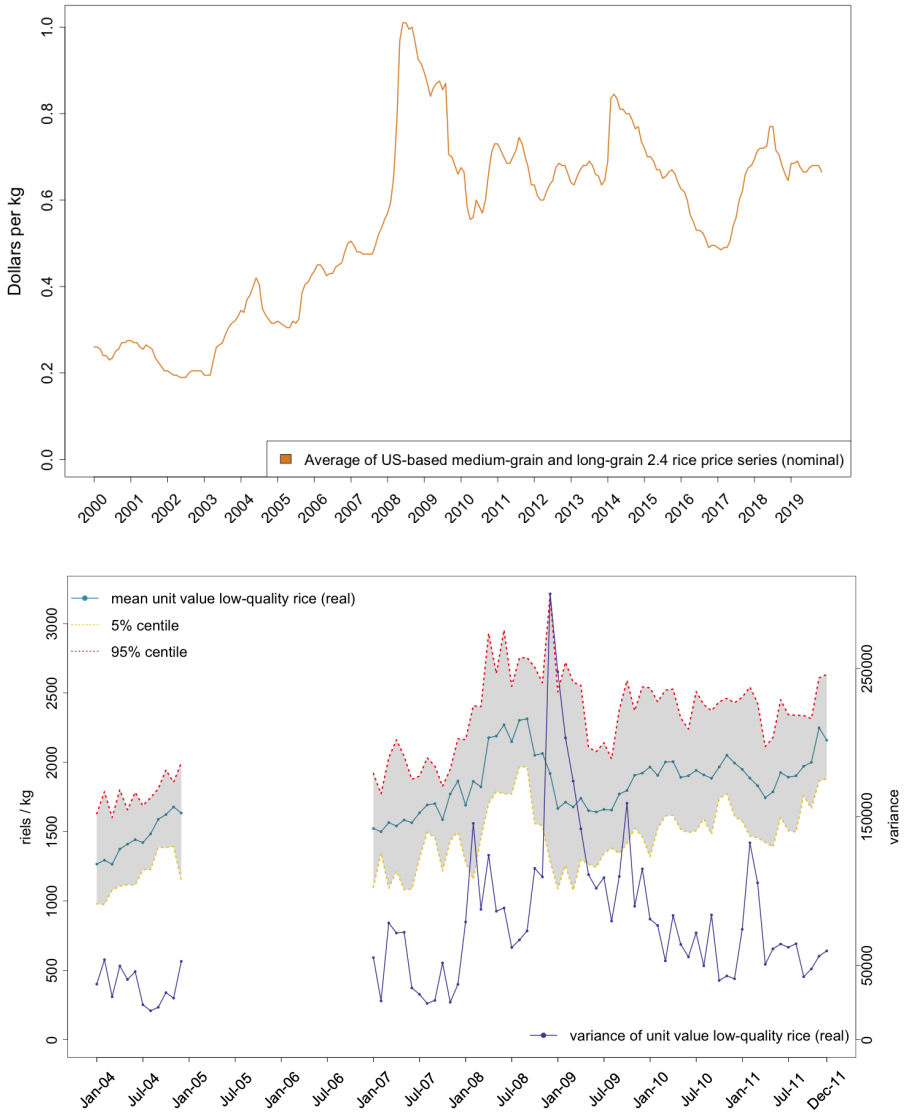

Figure 2: Authors' calculations. Top panel: FAOSTAT data. Bottom panel: Low-quality rice unit value distribution (n = 497,129) deflated using CPI data from Cambodia NIS; centiles above 99% or below 1% are assigned to the mean.

[9]Supporting figures show: FAO producer price data; CSES price data for cash crops, fuelwood and charcoal; CSES wet season paddy price data; and regional wholesale rice prices.

In the top panel of figure 2 we see that international rice prices began rising intermittently in the early 2000s and then rose dramatically in 2007-2008. FAO producer price data for Cambodia (see appendix) shows that rice farmers faced declining then steady prices over 1995-2003 when gentle increases began. In the bottom panel of figure 2 we see that Cambodia's consumer rice prices closely mirror the international price[10]. Price trends for other crops are mixed[11] (see appendix).

Figure 3 shows a steep upward trend in deforestation within a small lag of the initial price shock at both the country- and district-level. Deforestation trends in

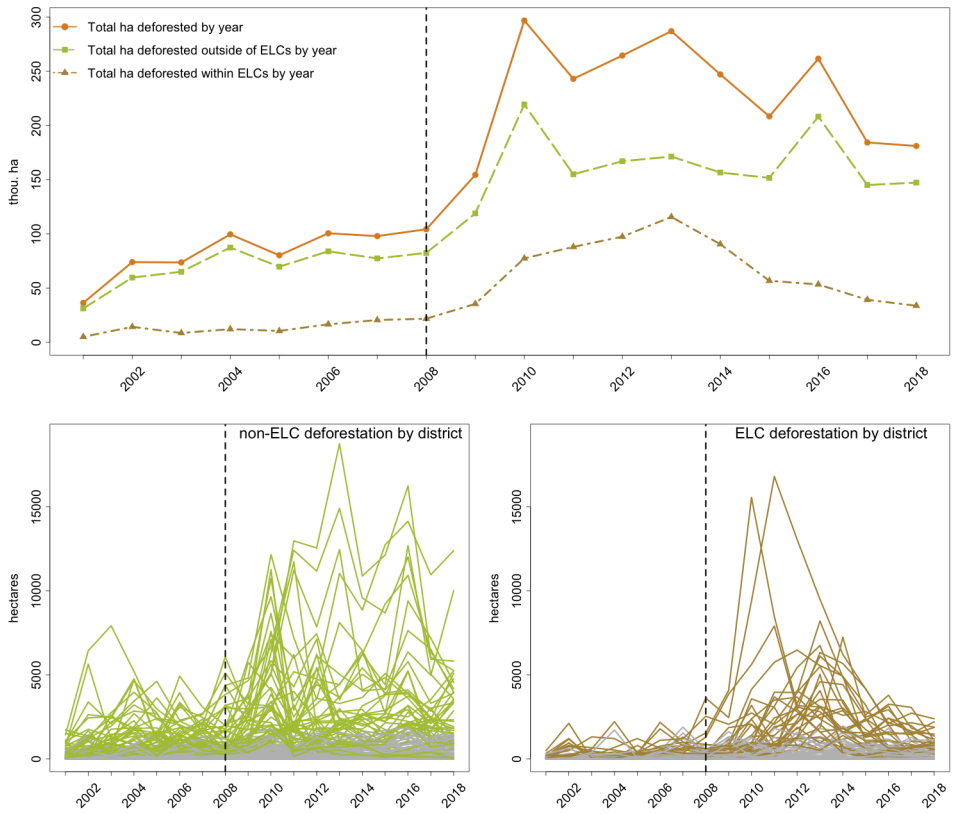

Figure 3: Top panel: deforestation in Cambodia 2001-2018. Bottom left panel: trend lines for each district's non-ELC deforestation. Bottom right panel: trend lines for each district's ELC deforestation. Districts with a maximum under 2000 hectares deforested across all years are plotted in grey. Vertical lines mark 2008.

[10] A companion figure in our appendix shows the wet season paddy series.

[11] Some cash crop prices show upward trends before or following 2007, others have incomplete data, others exhibit erratic (e.g. banana) or conflicting trend behavior (e.g. cassava).

Cambodia and Southeast Asia have received attention outside the economics literature, with claims regarding cash crop prices and Cambodia's ELCs as mechanisms (e.g. Davis et al. 2015, Grogan et al. 2015, Petersen et al. 2015). In figure 3 we observe that deforestation outside of ELCs was much greater than within ELCs and that deforestation trends differ markedly by land tenure. These facts suggest that something else[12] has been driving most of Cambodia's recent land-use change.

3 A Model of Land Use Choice

There are two prior theoretical economic studies of deforestation by agricultural households with particular relevance, Angelsen (1999) and Barrett (1999). Angelsen (1999) studies several deterministic models that range in their assumptions of household market integration and establishes a variety of conditions where changes in the value of output leads to increased deforestation. In each model Angelsen (1999) assumes households do not consume any of their output.

Barrett (1999) is the first known study to formalize a role for staple food price changes in deforestation. Barrett (1999) assumes a stochastic two-period model with missing contingency markets and assumes the household consumes part of their output. For net-buying agricultural households, Barrett (1999) finds that labor allocated to land-clearing is increasing in both the mean and variance of the price of food; an ambiguous result is found for net-sellers.

An important factor for our context that is absent in the aforementioned studies is extension to more general land-use choice[13]. In other words, could a staple food price shock, in addition to increasing staple land use, also lead to an increase in non-staple land uses? If so, to what extent and under what conditions? The question of cross-price land-use impacts from a staple price shock is novel to our knowledge, but also salient given the dominant claim in known literature that Cambodia's recent deforestation was driven by cash crop prices and associated expansion.

[12] Other plausible channels include the rice price shock, technology adoption (e.g. koryon tractors over the traditional water buffalo), infrastructure expansion, ordinance removal, and corruption.

[13] Fafchamps (1992) tangentially addresses such generality by studying crop portfolio choice under income and consumption price risk and finds that small farms reliant on their own staple production are less likely to diversify into non-staple land uses than large farms.

To study these ideas, we develop a simple static[14] agricultural household framework to demonstrate that such behavior is possible and to explore conditions where such behavior may manifest itself. We leave more exhaustive study of these questions to future work. As a starting point, we borrow from the idea of a land partition (e.g. Lichtenberg 1989, Just and Kropp 2013) and suppose there is a total amount of land $\bar{T} \in \mathbb{R}_+$ within reasonable access to the household. Land allocation choice is simplified to staples A_s and other[15] productive land use A_o. We define the total land allocation as $T = A_s + A_o$ by definition (not by imposed constraint) with $T \leq \bar{T}$. In other words, we suppose there is some slack un-allocated non-agricultural land – an important distinction as an increase in agricultural land (e.g. by deforesting) cannot take place if all land is already in production. Using $T = A_s + A_o$ as a definition rather than a constraint also implies staple land allocation does not directly affect the amount of land put to the other use and visa versa[16].

In an agricultural household framework, the question of non-separability[17] arises – a plausible, but to our knowledge, un-tested notion for Cambodia. To remain agnostic, we study both settings. Choice variables are identical across models and reduced to three choices for parsimony: an amount of staple food to purchase F_s, and the aforementioned land allocation choices A_s and A_o. We assume a strictly concave utility function U, additively separable strictly concave production functions, $f(A_s)$ and $g(A_o)$, and a strictly convex cost function $C(A_s,\ A_o)$, assumed to account for option value. Staple output price is assumed to be equal to the consumption price, P_s. To relax the separability assumption we assume binding constraints[18] on staple production (e.g. due to land quality), $\bar{Q}_s$, and staple market purchase, $\bar{S}$.

We are particularly interested in $\frac{d\rho_s}{dP_s}$, $\frac{d\rho_o}{dP_s}$, and $\frac{dT}{dP_s}$ – the effect of a staple food price change dP_s on the total relative share of land allocated to staples and other land

[14]Static models abstract from dynamics (e.g. multi-year land preparation considerations) but remain useful since static optimality will comprise an important subset of dynamic equilibria.

[15]We are agnostic on what A_o is comprised of, though cash crops are a common alternative.

[16]Generalization to n land uses with an equality constraint that shares sum to one is possible, but this produces a mutually exclusive and collectively exhaustive endogenous land partition with non-negligible complication. Two land use choices achieves similar intuition with simpler exposition.

[17]Under non-separability, consumption and production decisions influence eachother. For separable agents, production influences consumption but not visa versa (Singh, Squire, and Strauss 1986).

[18]Labor market constraints are more commonly applied and possibly more realistic, however, our approach produces qualitatively similar results with simpler exposition.

use, and the total area under productive use, respectively, where $\rho_i = \frac{A_i}{T}$. We define $\frac{dT}{dP_s} = \frac{dA_s}{dP_s} + \frac{dA_o}{dP_s}$ as the change in total land in cultivation T with respect to a staple price change; $\frac{d\rho_i}{dP_s} = (\frac{dA_i}{dP_s}T - A_i\frac{dT}{dP_s})/T^2$ as the change in relative share i with respect to a staple price change; and $\eta_{xp_j} = \frac{dx}{x}/\frac{dp_j}{p_j}$ as the elasticity of x with respect to p_j. In the propositions below, we summarize our findings. Full exposition of each respective model and proofs of each proposition are provided in our appendix.

Proposition 1 (Separable Model). If the price of the staple food P_s increases:
(A) the total relative area allocated to staples ($\frac{d\rho_s}{dP_s}$) and other land use ($\frac{d\rho_o}{dP_s}$) will increase, respectively, if and only if the respective own-price and cross-price elasticities, $\eta_{\rho_s P_s}$ and $\eta_{\rho_o P_s}$, are individually positive; and **(B)** the total land allocation ($\frac{dT}{dP_s}$) will increase if and only if the price elasticity of the staple to total cultivated area, η_{TP_s}, is positive.

Proposition 1 shows that under separability a staple food price shock can increase land devoted to staples or other uses; increase the relative share of either land use; and increase the total amount of land in productive use.

Proposition 2 (Non-Separable Model). If the price of the staple food P_s increases, relative and total share responses are as follows:
(A) The total relative area allocated to staples ($\frac{d\rho_s}{dP_s}$) will increase if and only if the household is: (i) a staple net-buyer and staples and other land use are complements in production costs; or (ii) a staple net-buyer (net-seller), staple and other land use are substitutes in production costs, and other-area-weighted second order profit effects of other land use are greater than (less than) staple-area-weighted second order cross-input cost effects (i.e. $|A_o(P_o g_{oo} - C_{oo})| > |A_s C_{so}|$, or visa versa).
(B) The total relative area allocated to other land use ($\frac{d\rho_o}{dP_s}$) will increase if and only if the household is: (i) a staple net-seller and staples and other land use are substitutes in production costs; or (ii) a staple net-buyer, staple and other land use are compliments in production costs, and other-area-weighted second order profit effects of other land use are less than staple-area-weighted second order cross-input cost effects (i.e. $|A_o(P_o g_{oo} - C_{oo})| < |A_s C_{so}|$).
(C) The total amount of land allocated to staples and other land use ($\frac{dT}{dP_s}$) will increase if and only if the household is: (i) a staple net-buyer and staples and other

land-use are complements in production costs; or (ii) a staple net-buyer (net-seller), staple and other land use are substitutes in production costs, and other land use second order profit effects are greater than (less than) second order cross-input cost effects (i.e. $|P_o g_{oo} - C_{oo}| > |C_{so}|$, or visa versa).

Proposition 2 shows that, under non-separability, broad land use effects from a staple food price shock persist and that household staple consumption becomes a determining factor in land use. We will test these relationships in aggregate and at the household level, and study NBR distributions[19] to get a sense of net-buyer versus net-seller status. We will also study response to price volatility since it is of long-standing interest (Sandmo 1971, Barrett 1999, Lundberg and Abman 2021).

4 Data and Econometric Models

4.1 Data

We make use of a variety of data from the CSES, which we also georeference to enable merging with other spatial data. The CSES is a nationally representative repeated cross-sectional survey managed by the National Institute of Statistics (NIS) of Cambodia. It has a monthly probability proportional to size sampling design and provides household and village-level data. The inaugural year of the CSES was 2004, there was a gap in 2005-2006, but it has been continuous since 2007. Every fifth year is a large sample year (1000 households per month; 2004 and 2009 in our data) and intervening periods are small sample years (360 households per month).

Although the CSES is not designed to be representative at any particular administrative unit, prices are largely similar to all people within locations. This reality drives our estimates of the mean and standard deviation of local prices and is aided by the high-frequency nature of the CSES. We analyzed all available CSES rice price series and found wet-season paddy farm gate prices ($n = 18,621$) and

[19] Data used to construct NBRs have a time-stamp ahead of land allocation data, making NBR an inappropriate regressor; as the NBR is also endogenous it would be a troublesome regressor.

low-quality rice unit[20] values ($n = 497,129$) were the most reliable. This is not surprising as these are the most commonly traded forms of rice in Cambodia.

Our deforestation data come from Hansen et al. (2013) who use 30 m^2 resolution Landsat data to measure global deforestation. Hansen et al. (2013) focus on the presence or absence of trees at the pixel scale, where trees are defined as vegetation taller than 5m. Forest stand replacement events are coded per pixel for the year during which all such cover is removed. Conservative definitions of forest can be employed by masking pixels based on year 2000 tree cover percentage. We use the least restrictive definition of forest since our interest is in general land-use change. To disaggregate non-ELC versus ELC deforestation by administrative levels we use administrative unit shapefiles from the World Food Program and a shapefile for ELC areas obtained from Open Development Cambodia (ODC 2019). A map depicting the three primary administrative levels of Cambodia and the boundaries of Cambodia's ELCs is provided in our appendix.

For weather data we use monthly series from Abatzoglou et al. (2018) on precipitation and maximum and minimum temperatures. These data were created using interpolation algorithms that produce monthly data at a resolution of 0.05°, or approximately 4 km^2 from 1958-2018. We studied the long-run record to partition the data into wet and dry season variation (May-Oct and Nov-Apr respectively).

We rely on various data sources for our shift-share variables. For shifts, we rely on international rice price series obtained from the FAO GIEWS FPMA tool (see figure 2). For our share variables, we calculated road distances and travel times using the Google Distance Matrix API, we gathered deep sea port coordinates using Google Earth Pro, and we calculated linear distances and centroid coordinates in R. We also experiment with rice production suitability using data from FAO GAEZ[21], which has produced biophysical agronomic suitability measures for many crops based on a 1961-1990 baseline. To account for variation in Cambodia, we use an average of total production capacity (tons/hectare) for low and intermediate input level rain-fed wetland rice.

[20] With unit values a caveat is unobserved quality (Deaton 1988), which Gibson and Kim (2019) show can induce underestimates of elasticities; though low quality rice elasticities estimated by Gibson and Kim (2019) in Vietnam show little difference from those estimated with observed quality.

[21] Data were retrieved from FAO Global Agro-ecological Zones (GAEZ v3.0).

4.2 Econometric models

For our aggregate tests of $\frac{dT}{dP_s}$, we account for the reality that most land-use change is likely to occur after, not during, the initial price shock. This is likely since annual agricultural calendars force many decisions to be made earlier in the year, which cannot easily be changed within the same year. To address this in our specifications we set aggregate deforestation D (total hectares) in location l (province, district, or commune) and land tenure j (non-ELC or ELC areas) in year $t+k$, to the mean or standard deviation of local rice prices P in location l and year t. The following equations show the first and second stages of the IV panel fixed-effects estimator that we employ,

$$P_{l,t} = \delta_1 Z_{l,t} + X'_{l,t}\delta + \mathscr{L}_l + \mathscr{T}_t + u_{l,t} \tag{1}$$

$$D_{l,j,t+k} = \beta_1 \widehat{P_{l,t}} + X'_{l,t}\beta + \mathscr{L}_l + \mathscr{T}_t + \varepsilon_{l,j,t+k}. \tag{2}$$

$P_{l,t}$ represents the mean or standard deviation of rice prices, with $\widehat{P_{l,t}}$ being the predicted variation from (1). $Z_{l,t}$ is our instrument, $X'_{l,t}$ captures a vector of control variables, and $D_{l,j,t+k}$ is our dependent variable. $\mathscr{L}_l$ and $\mathscr{T}_t$ are location and time fixed effects. Respective first and second stage error terms are $u_{l,t}$ and $\varepsilon_{l,j,t+k}$.

Our objective in equations (1) and (2) is to identify β_1. We focus especially on deforestation in $t+1$ as a short-run test of an effect from the rice price shock. We also estimate models using up to year $t+6$ deforestation to test for longer-run impacts, and back to $t-3$ deforestation for placebo tests. $X'_{l,t}$ includes time trends, location by year trends, lagged ELC deforestation (for non-ELC specifications to account for spill-over effects), and mean seasonal weather variation in t. This approach to weather variation controls for lagged seasonality effects on deforestation and contemporaneous seasonality effects on local rice prices. District-level specifications are preferred as they maximize observations, clusters, and panel balance.

We are concerned with endogeneity arising from omitted variables (e.g. other prices and covariates that affect deforestation). To identify β_1 we rely on $Z_{l,t} = w_l s_t$, where s_t (our shift) is the mean of a US-based average international rice price in t, and w_l (our share) represents road distances from administrative unit l centroids to

Cambodia's deep seaport[22]. Distances are likely to play a large determinative role in exposure to the price shock and are commonly used measures to capture exposure to shocks (e.g. Peri 2012, Tanaka 2020). Both s_t and w_l are highly likely to be exogenous to local rice prices and allow us to account for the global price shock and local variation in shock exposure as a function of exogenous distance-induced transportation costs. Exogeneity of both share and shift variables is advantageous in shift-share research designs as it ensures $\mathbb{E}(w_l s_t \varepsilon_{lt}) = 0$ is satisfied. However, consistency may be maintained if either is endogenous depending on applicable asymptotic theory (e.g. Goldsmith-Pinkham, Sorkin, and Swift 2020).

At the household level our goal is to test for evidence of the role of the price shock and local aggregate deforestation in land-use behaviors of interest. Here, we focus on the following reduced-form[23] equation as an approximation of $\frac{dT}{dP_s}$ and $\frac{dA_j}{dP_s}$,

$$A_{i,c,s,l,t+1} = \delta_1 Z_{l,t} + D'_{l,t-j}\delta_2 + X'_{i,l,t}\delta_3 + W'_{l,t-j}\delta_4 + \mathscr{L}_l + \mathscr{T}_t + \varepsilon_{i,c,s,l,t+1}. \tag{3}$$

$A_{i,c,s,l,t+1}$ captures household i's area-share for crop(s) c in season(s) s, in the l'th location during year $t+1$. $Z_{l,t}$ is the instrument from equation (1), $D'_{l,t-j}$ is a vector of lagged aggregate deforestation (plausibly exogenous to individual households), $X'_{i,l,t}$ is a vector of household covariates, and $W'_{l,t-j}$ captures seasonal weather covariates. $\mathscr{L}_l$ and $\mathscr{T}_t$ are location and time fixed effects and $\varepsilon_{i,c,s,l,t+1}$ is the error term.

Our particular focus is on δ_1 and δ_2. With this focus in mind, we estimate (3) with Z_l at t and $t+1$, since contemporaneous prices may be more important for land allocation choice (hence $Z_{l,t+1}$ may be more relevant). We vary $D'_{l,t-j}$ in construction by land tenure, level of aggregation, and lags and we restrict $X'_{i,l,t}$ to a parsimonious

[22] We also explore other shift-share instruments of similar construction using other international price variation for shifts (e.g. averages or variances of other series) and different share variables (e.g. linear distances or travel times to other regional deep sea ports, and measures of rice suitability).

[23] We also estimate an instrumental variables version of (3) (see our appendix), however, we view this approach as an inferior test since the time-step of available recall-based production data forces one to drop most pre-price-shock observations as local price data is not available at a lagged or contemporaneous time-step for 2004 and 2007 data (i.e. there was no CSES survey in 2003, or 2005-2006). To operationalize this approach we use district-level price data used in equations (1) and (2). The reduced-form, in contrast, permits use of all pre-shock observations and has significant merit as an approximation of relationships of interest that one might study using standard two-stages least squares (Angrist and Pischke 2009).

set of household covariates to avoid bad controls (Angrist and Pischke 2009). $W'_{l,t-j}$ captures similar seasonal weather variation as in equations (1) and (2).

By the nature of $Z_{l,t}$, there seems little reason to suppose our instrument should directly affect our response variables. A bigger concern may be potential instrument correlation with omitted variables that may be driving our dependent variables. One plausible channel in this respect, which we will address, is other agricultural prices. The distributions of most[24] household-level response variables within the unit interval also imply estimation challenges, and our use of linear models does not maintain ideal fidelity to the underlying data generating processes. However, given the limitations of our data, a conservative approach of using linear models to accurately identify signs and assess correlations seems warranted and there is some evidence[25] to support this approach. For the interested reader, our appendix offers a discussion of associated econometric challenges and alternative estimators.

5 Summary Statistics

A table of household level summary statistics is provided in our appendix broken out by the full sample, non-agricultural, and agricultural households. The majority of the sample (65%) is involved in agriculture and non-agricultural households fair slightly better in terms of education (6.2 versus 4.3 years for heads of households), time allocated to fuel wood collection (1.5 versus 4.5 hours per week), dietary diversity (10.6 versus 9.5) and vulnerability (0.77 weeks starving versus 1.08). We also see that agricultural and non-agricultural households both spend about 80% of their monthly budget on food, but that agricultural households spend more on rice (10% versus 28% of their food budget). On average, agricultural households have just under two plots, grow 1.4 crops, have less than 2 hectares of agricultural land, and 0.6 plots with irrigation. Mean rice area allocation accounts for almost 80% and 36% for wet and dry season respectively, while cash crops account for about 1% and 18% on average in respective wet and dry seasons. Crops that comprise

[24] Most shares of interest are bounded by the unit interval with mass at 0 and 1, though aggregates of shares across seasons, or of diverse cropping systems within season, may exceed 1.

[25] Ramalho, Ramalho, and Murteira (2011) show with Monte Carlo and empirical models of fractional data that signs can be correctly estimated via linear models even with model misspecification.

our measure of cash-crop[26] shares are: cassava, cashew, mango, banana, rubber, soybean, mung bean, sweet potato, coconut, groundnut, sesame, and sugar cane.

As our theoretical models highlight, welfare impacts to farmers from the rice price shock are likely to play an important role in land-use change responses. Although a separable household may experience a negative welfare impact if they happen to be net-buyers in the staple, their consumption decisions play no role in their land-use behaviors. For non-separable households, staple consumption does influence production, and a household's marketable surplus position plays a determinative role in signing regimes. To better understand the empirical distribution on this score, we constructed approximate monthly NBRs using a combination of consumption, income, and production data. Figure 4 depicts the distribution of this measure for agricultural households.

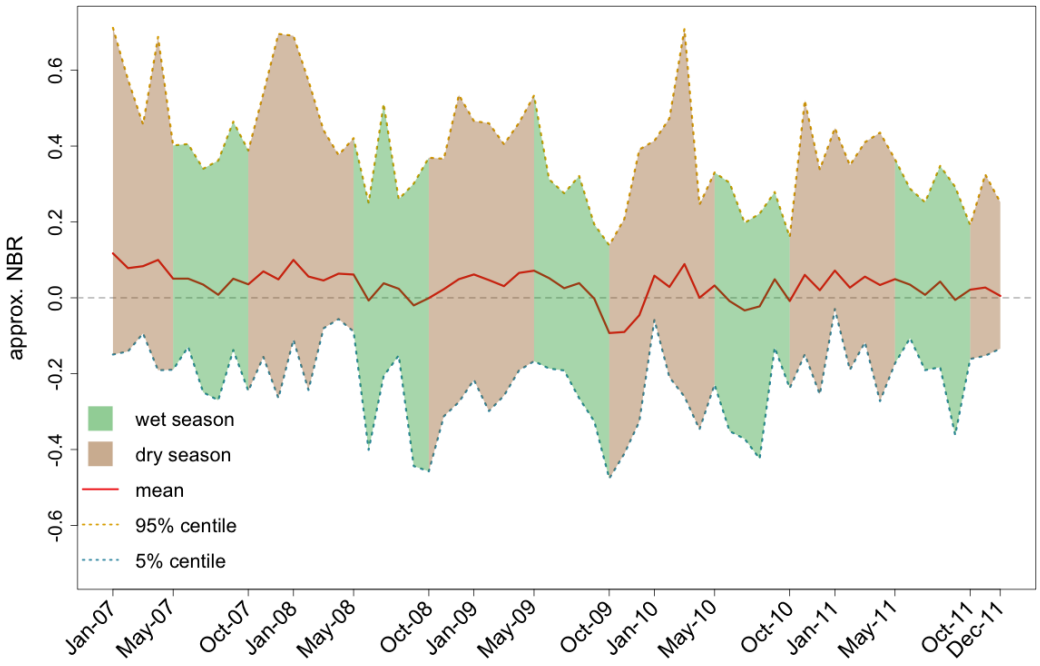

Figure 4: Monthly distribution of approximate NBRs for agricultural households. Negative (positive) values denote net-buyers (net-sellers). The y-axis is set within the $[-1, 1]$ theoretical support of the NBR to better render the observed variation.

[26]This subset arose from analysis of commonly cultivated crops and consumed food groups.

In figure 4 we see that the predominance of mass between net-buyer and net-seller status is dynamic. This can be seen in both the tails and the average trend line, which shows evidence of net-seller status rising on average in the dry season and net-buyer status increasing in the wet season – this conforms with the observation that the lean season coincides with the wet season. A challenge with our measure is that we do not have a clear monthly measure for rice income[27]. As a reasonable alternative we use total monthly agricultural income. Since we know total agricultural income is greater than or equal to total rice income, this approach understates net-buyers. Annual non-parametric densities of this measure (see appendix) suggest higher density on the net-buyer side (particularly since net-buyer status is understated), although mass in each density avoids the extremes of the support at -1 or 1. Our analysis therefore lends some support to the claim that net-buyer status was predominant (as claimed by Sophal 2011), but we also see that this status was not static. This implies welfare gains and losses from the rice price shocks and possibly heterogeneous land use choice regimes as captured in our propositions.

Figure 5 provides and animation of annual spatial variation in deforestation at the district level showing deforestation outside of ELCs and within ELCs. The distribution of non-ELC based deforestation shows distinct increases within a short lag of the initial shock, and the respective plot of ELC-originating deforestation confirms the distinctness of these respective distributions as captured in figure 3. Inspection of these plots also shows the random sampling of the CSES results in imbalanced panels. However, after dropping districts for each respective price series that are only observed once (12 for unit values, 10 for paddy) the resulting panels are strongly balanced: 85% of the sample is observed for ≥ 4 years with low-quality rice unit values ($t = 6$, $n = 786$); 75% of the sample is observed for ≥ 6 years with wet season paddy ($t = 8$, $n = 988$). Additional static spatiotemporal maps in our appendix show variation in rice prices and deforestation at the district level. These maps show widespread transmission of the price spike and concordance in trend between price series.

[27]The CSES has monthly rice consumption data, but rice income is not separated from total agricultural income, hence we approximate Deaton (1989). See our appendix for further details.

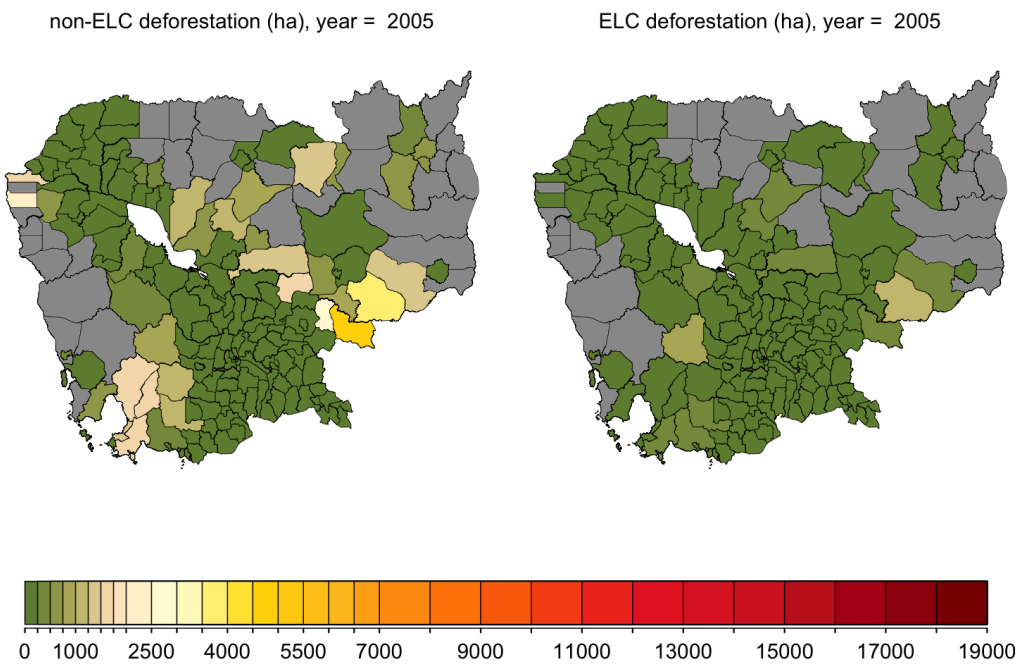

Figure 5: District-level deforestation in hectares (ha), in year $t+1$, both outside of economic land concessions (non-ELC, top-left) and within economic land concessions (ELCs, top-right), for districts that were sampled by the Cambodia Socioeconomic Survey (CSES) in a given year t (districts in grey were not sampled by the CSES in year t). As outlined in section 4.2, our econometric specifications set local deforestation in $t+1$ to local price variation in t. (Note: Adobe Acrobat reader may be needed to run the animation, which can be run to completion, or slowly frame by frame).

Figures 6 and 7 show boxplots of wet and dry season rice and cash-crop shares and sums of these respective shares, defined as: T_d and T_w, the sum of rice and cash crop shares within season; and $T_{dw} = T_d + T_w$, a measure of the overall intensity of land allocation. Each set of boxplots features year-season means and horizontal lines for the respective means at baseline. These figures show that allocations declined between 2003-2008 as exhibited by movements in averages, medians (gray squares) and the tails of interquartile ranges. However, this trend starts to reverse around 2008. By 2013, allocations regain or exceed their baseline means.

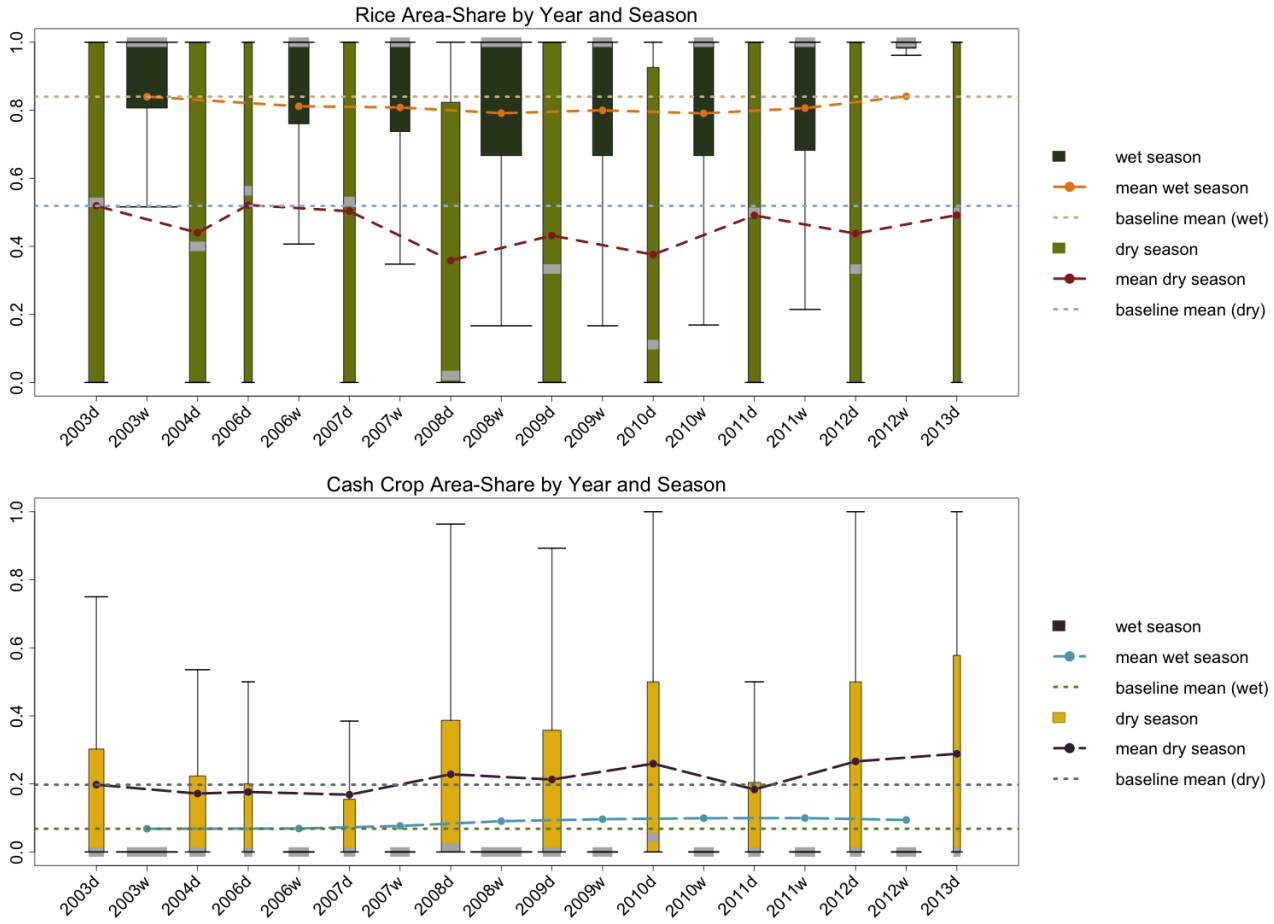

Figure 6: Wet and dry season area shares for rice and cash crops conditional on growing a crop within each respective season. Data used to construct area shares come from CSES agricultural production modules for 2004, 2007-2013.

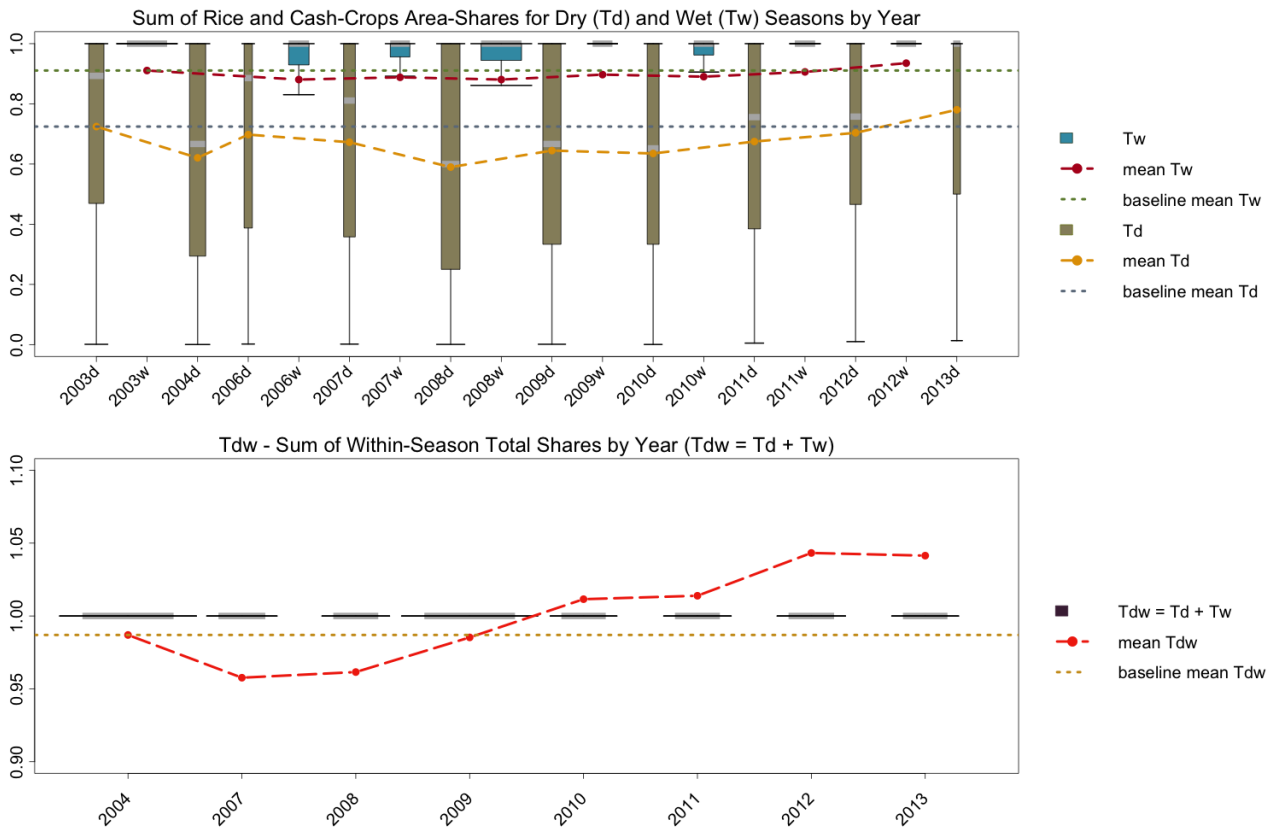

Figure 7: Within-season sum of rice and cash-crop shares (top panel) and sum shares across seasons (bottom panel). Data used to construct area shares come from CSES agricultural production modules for 2004, 2007-2013. The range of the bottom panel y-axis is truncated to enable discernment of the tightly packed variation around 1.

Finally, in our appendix we provide two figures showing point estimates and p-values for instrument Z when regressed on unit values or farm gate prices for major cash crops, firewood, and charcoal. The majority of price series show weak to no correlation with out instrument. Among the unit value regression tests (which generally have larger sample size), only mango and coconut unit values show signs of strong correlation with our instrument; cassava shows no correlation. We further explore these issues in our results. Overall, these findings do not lend support to the idea that our instrument is correlated with other commodity prices a strong and meaningful way.

6 Results and Conclusion

6.1 Aggregate Deforestation Response

Figures 8 and 9 present results for our aggregate test of $\frac{dT}{dP_s}$ as modeled by equations (1) and (2) at the district level. Each figure presents point estimates and confidence intervals for low-quality rice price coefficients in arcsinh-arcsinh specifications ranging from simple OLS to IV fixed effects. Coefficients have an elasticity interpretation since the magnitudes of the averages of the respective variables are large[28]. Figure 8 shows point estimates for mean rice prices when non-ELC-based deforestation (top panel) and ELC-based deforestation (bottom panel) are the dependent variables. Similarly, figure 9 shows point estimates for rice price standard deviation when non-ELC-based deforestation (top panel) and ELC-based deforestation (bottom panel) are the dependent variables. Each figure provides specification details, model summary statistics, first-stage effective F statistics (Montiel Olea and Pflueger 2013) and regression-based Durbin-Wu-Hausman (DWH) statistics (Wooldridge 2010). In our appendix we show companion sets of figures with wet season paddy rice prices as the endogenous variable; results are very similar in magnitude and significance.

[28] Bellemare and Wichman (2020) show that for arcsinh-arcsinh specification, $y = x\beta + \nu$, the elasticity is $\varepsilon_{yx} = \beta \cdot \frac{\sqrt{y^2+1}}{y} \cdot \frac{x}{\sqrt{x^2+1}}$. Evaluated at mean deforestation and rice prices, $\frac{\sqrt{y^2+1}}{y} \cdot \frac{x}{\sqrt{x^2+1}} \approx 1$.

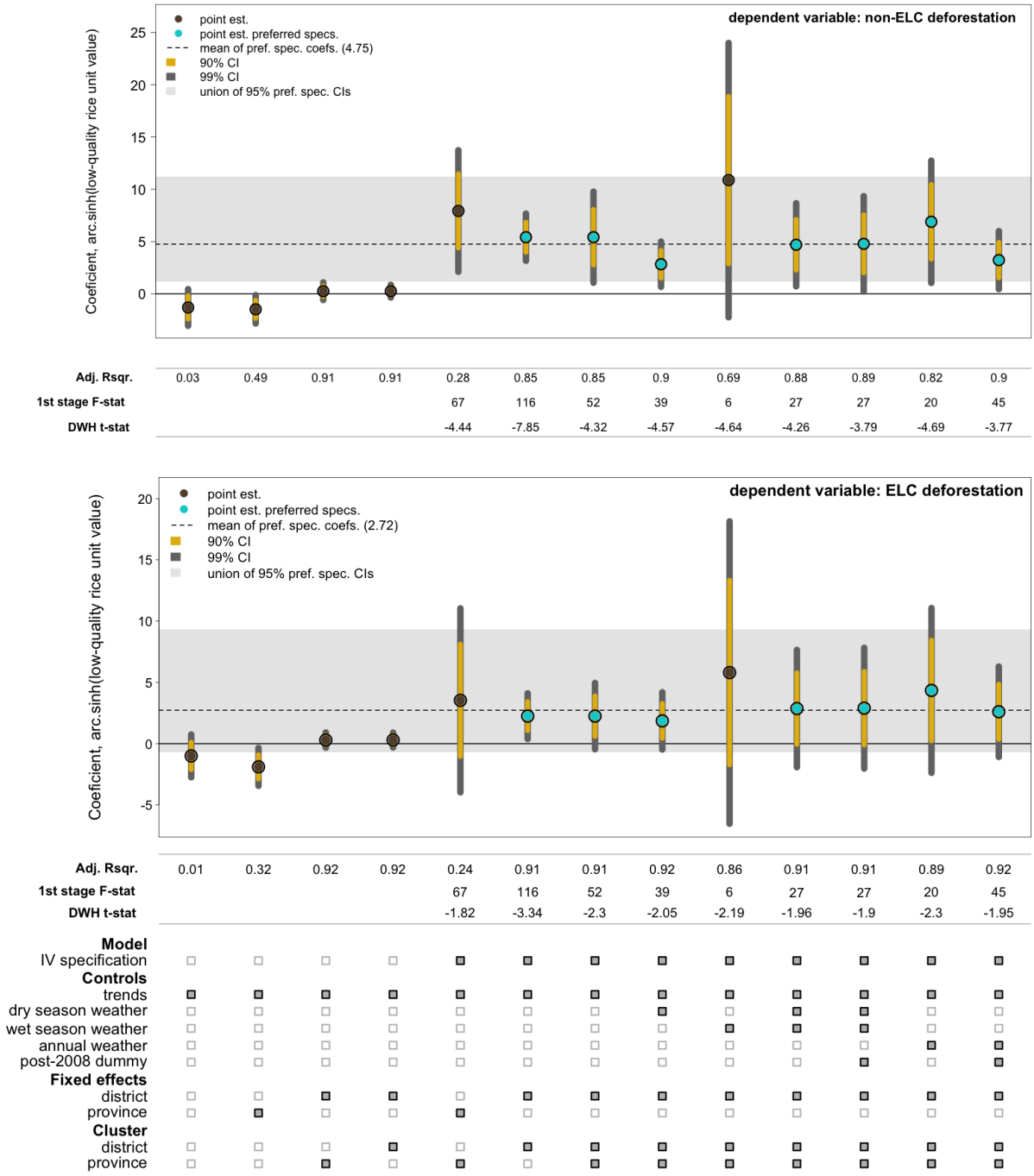

Figure 8: Point estimates of district-level arcsinh(mean low-quality rice unit values) in t. Top panel has arcsinh(non-ELC hectares deforested) in $t+1$ as the dependent variable. Bottom panel has arcsinh(ELC hectares deforested) in $t+1$ as the dependent variable. All specifications have $N = 786$ and $t \in (2004, 2007-2013)$.

The result for the effect of local mean rice prices is striking. Figure 8 shows that in IV specifications mean rice prices produce large positive elasticities, averaging around 4 to 5 in preferred specifications that are broadly significant at the 1% level when non-ELC is the dependent variable. These results point to a large increase in local deforestation in areas dominated by smallholder agriculture. When ELC deforestation is the dependent variable point estimates remain positive, but decline by about half and statistical significance weakens. This result is consistent with the idea that more cash-crop, or corporate focused agents would be less responsive to a staple food price shock, and may also reflect that politics could play a role in ELC-based deforestation.

Figure 9 also shows intriguing results for effect of local price standard deviation. When deforestation outside of ELCs is the dependent variable, point estimates are positive and around one quarter to a third of mean price point estimates. Statistical significance is quite strong, though not as strong as results for mean prices, largely owing to the fact that our instrument is weaker across the specifications considered, especially when weather lags are included. When ELC-based deforestation is the dependent variable, point estimates remain positive but become inelastic and statistical significance weakens.

Additional noteworthy findings (provided in our appendix) include: a positive and significant, inelastic (0.10-0.20) spill-over effect of lagged ELC-based deforestation (with non-ELC deforestation as the dependent variable); negative and significant effects from dry season temperatures in the second stage (logical given the difficult manual labor required to deforest and prepare land); and a variety of interesting relationships between price variation in the first stage and seasonal weather covariates. On the latter subject we offer brief discussion in our appendix regarding the economics of first stage results. Several additional figures and tables in our appendix provide full details on second and first stage findings.

With respect to exogeneity tests of the endogenous variables, DWH statistics strongly reject exogeneity. Reported DWH t-statistics are for the coefficients on the first stage residuals in the second stage regression with the uninstrumented endogenous variable. Exogeneity is rejected if the residuals are significant. First stage F statistics are quite strong and well-above standard thresholds, with some exceptions.

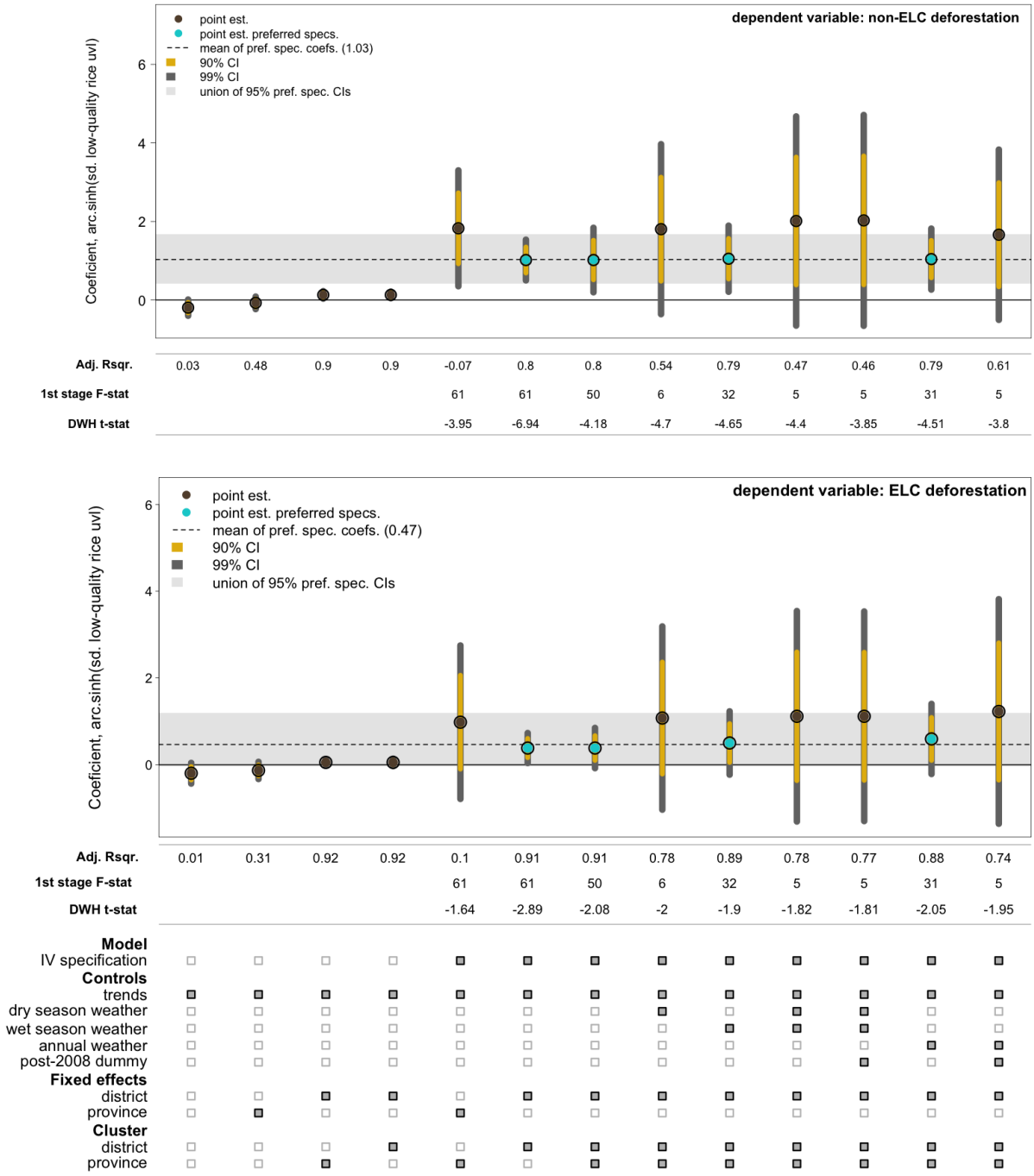

Figure 9: Point estimates of district-level arcsinh(standard deviation low-quality rice unit values) in t. Top panel has arcsinh(non-ELC hectares deforested) in $t+1$ as the dependent variable. Bottom panel has arcsinh(ELC hectares deforested) in $t+1$ as the dependent variable. All specifications have $N = 786$ and $t \in (2004, 2007-2013)$.

Although Stock and Yogo (2005) critical values do not in general apply in non-homoskedastic settings, they are applicable in just identified settings since the respective nonhomoskedastic F statistic retains a non-central chi-square distribution (Andrews, Stock, and Sun 2019). By these standards, weak instruments are not a concern for a majority of our specifications. We also provide tables of the reduced form, which can be used to verify our results via indirect least squares.

Common threads between instances of weak instruments in our results are differential effects of seasonal weather controls in the first stage and year fixed effects. For mean prices, wet season variation, absent dry season controls, seems to soak up some of the strength of the instrument. For price standard deviation, seasonal weather controls have similar effects, but price variance is also not as strongly correlated with our instrument, nor other instruments we have studied. With year fixed effects the issue is that they explain very little[29] deforestation variation, but they remove substantial variation in rice price variables with consequent impacts to model quality. As such, we argue that the data do not permit one to use year fixed effects and still have a well-constructed[30] test as these primarily annually varying price series lose too much information. We do, however, show in figures 8 and 9, that results are robust to including a post-2008 dummy.

For basic robustness checks, we use similar instruments, estimate at province and commune levels, apply different forms (e.g. levels, semi-logs, etc.), and test with cash crop prices. With other similar instruments (see section 4.1 for discussion), we find comparable results that sometimes show superior strength. For example, using price variance as the shift produces comparably strong results for mean prices that seem to withstand including or excluding different weather controls, and point estimates reduce by about 1 point.

Estimations with different functional forms and/or at province and commune levels also produce comparable results, with some caveats. For example, although commune level specifications add more observations, they come with greater panel imbalance, and although province level specifications are almost perfectly balanced

[29] Adjusted R^2 increases by around 0.02 with year fixed effects.

[30] A variety of regression tables and figures in our appendix illustrate what happens in the first and second stages when year fixed effects are included and provide further discussion on these topics.

the sample shrinks dramatically resulting in a small number of clusters. Commune level results are broadly similar but with somewhat lower significance. Province level results are also comparable, but with the caveat that point estimates focused on ELC deforestation start to exceed the corresponding results focused on non-ELC deforestation. We suspect these differences may be functions of the aforementioned factors and expression of the modifiable areal unit problem (MAUP)[31], which is a form of aggregation bias. We suggest these issues lend credence to district-level specifications striking a desirable balance.

For tests that include cash crop prices, we use the unit value series with at-least several hundred observations and we focus on cash crop prices that showed strong correlation with our instrument. When we instrument for rice prices and either coconut or mango unit values our results are unchanged[32]. Additional results focused on other cash crops prices can be provided upon request.

Further extensions and robustness checks that we perform include placebo tests and impulse response estimation (Jordà 2005), leave-one-out estimation, and permutation tests. We summarize methods and results here and refer the reader to our appendix for figures and discussion. For placebo and impulse response estimation, we estimate our panel IV model with district-level lags and leads of deforestation from $t-3$ to $t+6$ as dependent variables using preferred specifications. Results for all price series and deforestation variables of interest are provided for point estimates, confidence intervals, and model statistics. Placebo tests largely perform as expected. The impulse response component is interesting and we encourage the interested reader to compare the respective figures in our appendix with the aggregate deforestation trend lines shown in figure 3; starting at t the arc of point estimates up to $t+6$ bears a notable resemblance (i.e. teeth of a saw for non-ELC deforestation, and a smooth arc for ELC deforestation). In terms of significance, non-ELC models shows some evidence of significant effects at t, $t+1$, and $t+4$, whereas ELC models show a more steady significant impacts from $t+1$ to $t+4$.

Leave-one-out estimation and permutation tests have been recommended by recent studies of IV estimation for guarding against over-leveraged data (Young 2019)

[31] See Avelino, Baylis, and Honey-Rosés (2016) for an exploration of the impacts of the MAUP.

[32] Results are provided in our appendix where we also discuss tests with ≥ 2 endogenous variables.

and spurious time-series correlations (Christian and Barrett 2021). We adapt these recommendations to our setting with a focus on mean prices. The main graphical feature of our respective results are kernel densities of point estimates compared against observed estimates. We implement leave-one-out tests and versions that randomly drop two and four districts ($n = 5000$) and collect resulting second stage coefficients, p-values, and first stage F statistics. More dispersion appears when four districts random are dropped, but all densities remain centered on observed point estimates. To guard against type II error with permutation tests, since our data are spatially contiguous and spatially correlated (unlike the country-panel settings explored by Christian and Barrett 2021), we randomize ($n = 1000$) without replacement within cross-section, between t and $t+1$, and across all years and compare densities against observed second stage coefficients of interest, excluded IV statistics, and DWH t-statistics. Similar results do obtain for second stage statistics under within cross-section tests, as one might expect with spatially correlated data, but resulting DWH tests resoundingly do not reject exogeneity (our observed statistics do) and first stage F statistics weaken. Randomization across t and $t+1$ and across all years show scrambled relationships clearly break-down. These findings lend confidence that our results are not driven by over-leveraged data or spurious correlations.

6.2 Household Land Use Response

Figures 10, 11, and 12 show results for our household-level tests of the comparative statics of interest: total allocation responses $\frac{dT}{dP_s}$ within and across seasons, and relative share responses $\frac{d\rho_i}{dP_s}$ within crops and seasons. Each figure depicts coefficients, confidence intervals, and model summary statistics, respectively, for Z_{t+1}, district-level non-ELC deforestation in $t-1$, and district-level ELC deforestation in $t-1$ from estimation of equation (3)[33] where dependent variables are land allocation shares in $t+1$ shown in figures 6 and 7. We provide similar results for Z_t and

[33] As mentioned in section 4.2, we do estimate an IV specification on 2008 and up data with district level price series. The reduced form is generally of the expected sign and first stage results remain strong. Second stage results do not produce many noteworthy results for price variables. There are, however, strong correlations between production behaviors and deforestation lags. Loss of power and cross-sectional variation explains these findings as sample size drops by about half.

cumulative deforestation over t to $t-1$, as well as an example regression table with output for all covariates in our appendix[34].

The first striking result is that Z_{t+1} is strongly correlated with wet season rice, T_w, T_{dw}, and, to a lesser extent, dry season rice and cash crops. However, coefficient sign only becomes positive for dry season rice; results for Z_t show the same pattern. It is commonly taught that if the reduced form result is either not present or not of the hypothesized sign, then the structural relationship of interest is unlikely to exist (e.g. Angrist and Pischke 2009). We cannot rule out this eventuality, but we have already shown many statistically significant results demonstrating that our instrument is strong, and positive sign in the reduced form. Therefore, we suspect that these results actually reflect the role of the price shock in observed expansion.

In this setting, we posit that simultaneous relationships are biasing point estimates for Z, and causing a so-called "incorrect" sign[35]. A natural and plausible candidate is non-separability between production and consumption. The null of the separation hypothesis in common tests is that household labor endowment has no role in farm labor demand (e.g. Benjamin 1992). There are no prior known tests of the separation hypothesis in the Cambodian context, however, in our appendix regression tables it is clear that several household covariates are strongly correlated with crop allocation (e.g. household size). If the separation hypothesis is rejected, the implication is that household consumption influences production – hence household staple supply and demand are involved in production decisions, thus implying a different underlying structural model that brings in consumption decisions (i.e. we only model the supply side). Another possibility is that the observed signs are "correct", in which case the implication might be one of short-run re-allocation from rice and wet season production into other land investments. If so, the observed findings might reflect more complex behavior taking place at the household level. We leave further study of this issue to future work.

[34] All models include linear trends, commune-specific trends, a parsimonious set of household controls, and district-level deforestation (ELC and non-ELC) in $t-1$, and shift-share variable Z $t+1$. Household covariates include: household size, male share of the household above 15 years old, an indicator for female head of household, years of education of the household head, and the number of plots with irrigated rice.

[35] Various factors may cause "incorrect" signs. See discussion in Kennedy 2005 and An, William, and Zhao 2016 for theory and empirics when estimating "but-for" prices in price-fixing cases.

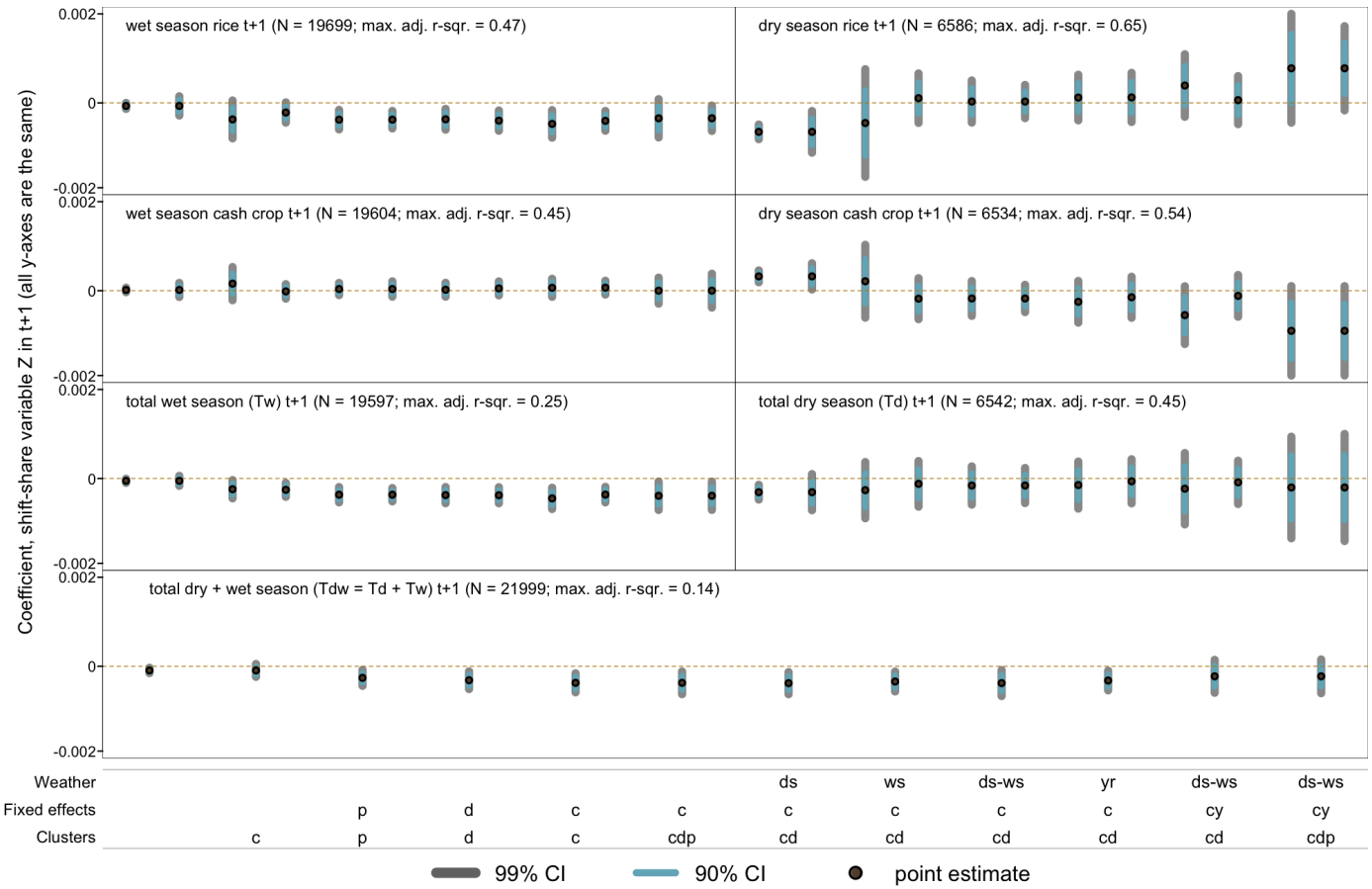

Figure 10: Point estimates of commune-level shift-share variable Z in $t+1$ regressed on household-level area-shares in $t+1$. Wet and dry season allocations in rice, cash crops, and their sum (T_w and T_d) are the dependent variables in the top six panels; total rice and cash crops shares seasons, T_{dw}, is the dependent variable in the bottom panel. Seasonal weather controls in t, fixed effects, and clustered standard errors are denoted in the bottom table: wet season (ws), dry season (ds), annual (yr), wet and dry season (ds-ws), commune (c), district (d), province (p), year (y).

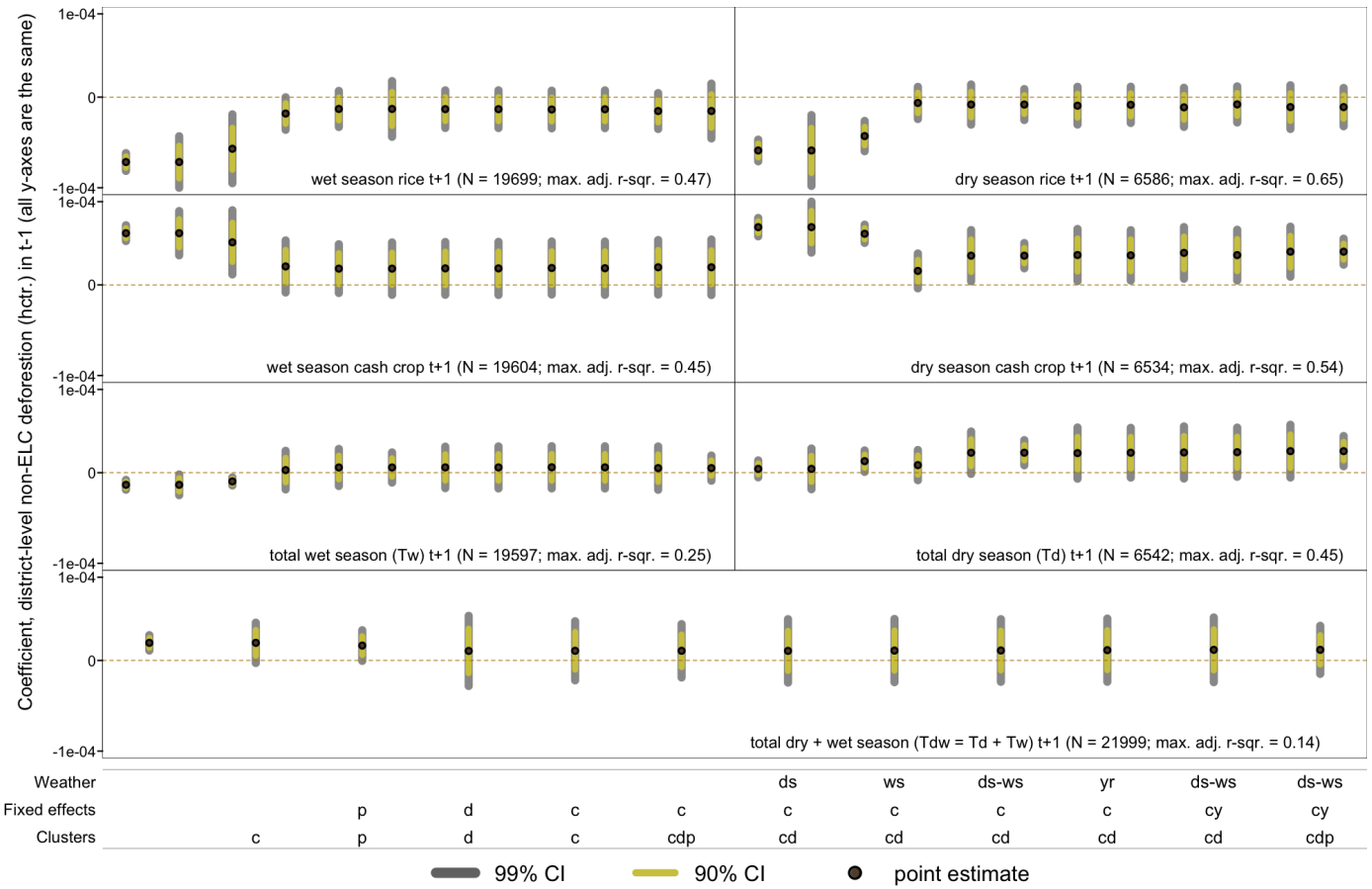

Figure 11: Point estimates of district-level non-ELC deforestation (hectares) in $t-1$ regressed on household-level area-shares in $t+1$. Wet and dry season allocations in rice, cash crops, and their sum (T_w and T_d) are the dependent variables in the top six panels; total rice and cash crops shares seasons, T_{dw}, is the dependent variable in the bottom panel. Seasonal weather controls in t, fixed effects, and clustered standard errors are denoted in the bottom table: wet season (ws), dry season (ds), annual (yr), wet and dry season (ds-ws), commune (c), district (d), province (p), year (y).

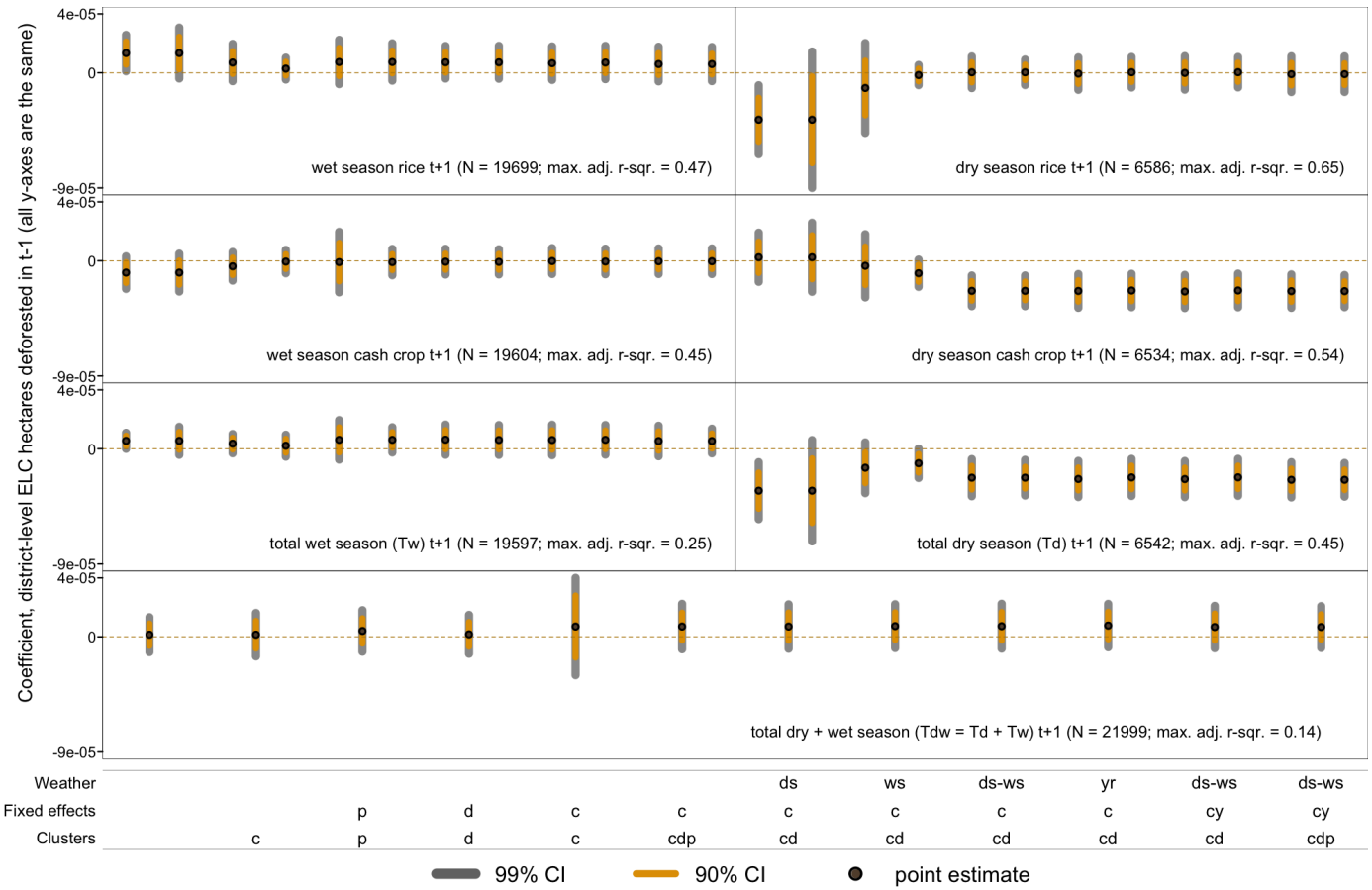

Figure 12: Point estimates of district-level ELC deforestation (hectares) in $t-1$ regressed on household-level area-shares in $t+1$. Wet and dry season allocations in rice, cash crops, and their sum (T_w and T_d) are the dependent variables in the top six panels; total rice and cash crops shares seasons, T_{dw}, is the dependent variable in the bottom panel. Seasonal weather controls in t, fixed effects, and clustered standard errors are denoted in the bottom table: wet season (ws), dry season (ds), annual (yr), wet and dry season (ds-ws), commune (c), district (d), province (p), year (y).

Tests for presence of a relationship with lagged deforestation are more easily understood and consistent in sign with prior results. A notable result is that ELC and non-ELC correlations are often inverse of each other: non-ELC deforestation is positive and statistically significant for cash crops and total dry season allocation, while ELC deforestation is largely negative and statistically significant for these dependent variables. Non-ELC deforestation is also negatively correlated with rice allocation, but positive for all total allocations, though only significant for total dry season allocation. In contrast, all other relationships with lagged ELC deforestation are almost precisely zero or indistinguishable from zero. These relationships are accentuated when cumulative deforestation is the regressor.

We find these reduced form results to be consistent with our aggregate deforestation results. Clearly the magnitudes of coefficients for household results are exceedingly small, but we remind the reader that magnitudes should be interpreted with caution and that our primary objective is to estimate signs in these specifications given the challenges with accurately measuring magnitudes with fractional data (see section 4.2 and our appendix for discussion). We suspect the true magnitude of marginal effects is larger than is apparent in our results.

7 Conclusion

At the nexus of many land and resource challenges around the world, there are market-mitigating forces that affect the balance of development and conservation. In order to make progress on a variety of sustainability objectives, it is important that mechanisms underpinning market-driven patterns of resource use are better understood to develop appropriate policy responses. An important example is the case of deforestation, which is frequently observed to be associated with agricultural land use. In related literature it is commonly assumed that cash crops prices and associated expansion are primary drivers of agricultural-related deforestation. This is not an illogical presumption, particularly when cash crop associated land use is observed to follow a deforestation event. On the other hand, since correlation is not causation, researchers should be cautious is ascribing causal mechanisms based on what comes ex-post.

Although cash crops surely have a major influence on land-use change and deforestation, other plausible agricultural market forces also deserve attention. A case in point are staple foods and staple food markets, which form the pillar of many economies around the world. Over 2007-2011, the world experienced a historic series of price shocks for staple foods on a scale not seen in 40 years. Economists have devoted significant attention to the study of these price shocks, however, potential impacts to land use have been overlooked in the related literature.

In this paper, we leverage the food price shock period circa 2008 as a natural experiment to test the extent to which a change in a staple food price might change land use. In the empirical context of Cambodia over 2004-2014, we find that the region's rice price shock lead to substantial increases in deforestation. Not only that, but we estimate that local deforestation response to local price shocks was greater in magnitude ($\approx$ 2 to 3 times) and statistical significance outside of ELC areas, where smallholder agriculture is dominant. Consistent with these results and our theoretical models, we also find strong evidence for the rice price shock and local deforestation being correlated with extensive margin production expansions at the household level.

These findings substantiate the notion that staple food prices and their markets can drive deforestation and land-use change. Our work not only adds a new empirical dimension to literature on deforestation and land use, but also contributes a new dimension to the study of the food price crisis period. As such, our research continues to shift the narrative surrounding the food price shock era using longer-run perspectives as other recent work has done (e.g. Headey 2016 finds declines in global poverty and Jacoby 2016 finds increased agricultural wages).

Another important contribution from our work comes with our data and methods. To our knowledge, no peer studies have combined spatially disaggregated household, land use, and price data in a study of aggregate natural resource impacts and household-level behavior – let alone in the context of a historic price shock. Our empirical approaches have also not previously been combined, including welfare analysis in the vein of Deaton (1989), extensive margin crop allocation analysis, and study of land use with remote sensing data focused on causal inference.

The only other known empirical study of staple food price changes and land use

is Lundberg and Abman (2021) who use 2002-2013 country-level panel data from sub-Saharan Africa to study maize price volatility and deforestation. Lundberg and Abman (2021) report no effect from maize price levels but offer evidence of declining deforestation one and two years after high maize price volatility. The intuition being that price volatility reduces incentive to exercise the option of developing new land. This perspective has merit, however, existing theory of how smallholders may respond to commodity price changes (e.g. Barrett 2008) in the midst of factor market failures (Dillon and Barrett 2017) raises questions about the micro-level behavior underlying such aggregate findings.

The setting and scale of our work is very different and it offers a counter-narrative. Our evidence suggests that changes in price levels exerted much greater impact on deforestation and associated land-use change than price variance. We estimate positive elasticities for the effect of mean rice prices on deforestation, which are greater in magnitude ($\approx$ 3 to 4 times) and statistical significance than respective point estimates for price standard deviation. Impulse-response estimation shows some evidence for extended impacts up to four years ahead; respective results for price standard deviation are positive and inelastic. These findings provide evidence that response to price levels was much greater than to price volatility.

Our household level results reinforce these findings by showing significant correlation between our shift-share instrument (in the reduced form) with rice shares, dry season cash crops, and total share allocation. We also demonstrate strong positive and significant relationships between lags of local non-ELC deforestation with increased household allocation to cash crops and total dry season shares, thus lending evidence for the role of extensive margin agricultural expansion as a mechanism. These findings show that staple food prices affect land-use change and that the indirect effects of price shocks may be as important as the direct effects.

What accounts for the large land-use response in the Cambodia? Surely many[36] factors are at work. One plausible explanation in line with our theoretical model and empirical results is high prevalence of net-buyer status combined with non-separability. For non-separable households, our model demonstrates that net-buyer

[36]Examples include productivity gaps, value chain challenges (Pandey and Humnanth 2010, Yu and Fan 2011, Nguyen et al. 2018), and increased investment in cassava (Vireak 2019).

status and complementary production costs between land uses provide sufficient conditions for positive extensive margin response. We do not test for presence of complimentary production costs, though we do provide evidence suggesting that net-buyers were likely dominant over this period and our household level results suggest separability should be rejected based test criteria developed by Benjamin 1992 (see appendix). A related mechanism may also come from consumption substitution, and analysis[37] we can provide upon request suggests as much. Indeed, if a net-buyer household faces rice production constraints, and rice purchase price increases, it seems plausible that the household might expand production where possible, produce rice to the extent practicable, and produce substitute consumption goods with cash value (e.g. cassava). We leave these ideas to future work.

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[37] Specifically, we find that reported frequency of consumption in the tuber group (cassava, sweet potato, potato, sugar beet) within 7-day recall consumption data shifted up in rank: from 15th or 12th least frequently reported food group consumed over 2004-2011 to the 1st or 3rd most consumed food group over 2012-2013.

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