Forced Modernization and its Discontents: Policy and Culture in Laos*

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Abstract

This paper evaluates a centrally-planned resettlement policy in rural Laos intended to modernize livelihoods and reduce poverty among ethnic groups traditionally practicing swidden (slash-and-burn) upland rice agriculture, where households cultivate dry-rice fields on forested slopes using rotational fallowing. By regrouping houses away from mountain slopes and into nearby valleys, the policy aimed to steer these societies toward permanent wet-rice paddies, considered technologically more advanced. Using the staggered timing of treatment, I find that the policy reduced village swidden plots by 30–40% five to ten years post-resettlement. However, ethnographic studies reveal that swidden practices are deeply interwoven with kinship ties and ethnic identities, and resistance to new methods reflects more than just unsuitable terrain. Using a triple-difference strategy—comparing traditional swidden-rice and wet-rice societies across regions with varying treatment intensity before and after resettlement—I find evidence of decreased well-being among those impacted: lower schooling attainment, increased child mortality, worse child health outcomes, and reduced household wealth. Exploring mechanisms, I find suggestive evidence that the policy only marginally shifted households into wet-rice cultivation; instead, these farmers reduced fallow duration as a possible coping response to limited swidden land in the new environment.

JEL Codes: I32, O13, O21, P21, R14.
Keywords: Central Planning, Forced Resettlement, Agricultural Reform, Cultural Mismatch, Ethnic Inequality.

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1 Introduction

In *Seeing Like a State*, James C. Scott (1998) offers one of the most influential critiques of central planning. Drawing on a range of historical case studies, he portrays the bureaucrat as a well-intentioned reformer whose efforts to improve society's living conditions are ultimately undermined by a detachment from local, practical knowledge.

While a handful of microeconomic studies have examined the disasters of central planning, their primary focus has been on collectivization—for example, Li and Yang (2005) and Chen and Lan (2017) on China's Great Leap Forward; Naumenko (2021) on Soviet collective farming. This raises a question: were central planning's failures *à la* Scott intrinsic to top-down bureaucratic control, or merely artifacts of collectivization and the abolition of private property? In other words, will central planning yield its intended outcomes if directed toward objectives such as technological adoption, or de-collectivization?[1]

This paper revisits Scott's thesis quantitatively by evaluating the state-sponsored agricultural modernization of traditional swidden ethnic groups in Laos in the past few decades. The aim was to replace swidden agriculture with the practice of wet-rice cultivation, the latter type seen by the state as technologically superior, and also less environmentally degrading. The policy involved the relocation of scattered houses of villages—a feature of swidden societies—from mountain slopes and into nearby valley clusters. By densifying these villages and bringing them closer to streams, the intention was that the villagers would abandon their swidden practice and instead adopt the more modern wet-rice methods of irrigated rice fields.[2]

While the intentions of the state may have been to modernize the economy, a more relevant perspective, however, comes from anthropologists' accounts that reveal layers of complexity intertwined with agriculture (Sprenger, 2006; Baird and Shoemaker, 2007). They argue that swidden cultivation is not merely a form of technology that can be improved by replacement but rather a deeply embedded social and cultural system that structures kinship relations, spiritual practices, land tenure arrangements, and collective identity formation within ethnic communities.[3] Therefore, the transition to new methods

[1] This question deserves asking given how the terms *central planning* and *collectivization* are too often spoken of synonymously.

[2] Theories about the effects of population density on agricultural intensification go back to Boserup (2014). Although attempts by the Government of Laos (GoL) to abolish the practice of swidden cultivation among all its mountain-inhabiting populations are not new, it was only in the late 1990s that it became more explicit in the form of an engineered resettlement policy.

[3] Another influential qualitative study comes from Lansing (2009) who studies how well-intentioned irrigation projects in Bali came apart because of imperfect information on the part of planners.

of agriculture that do not account for these less-easily observed factors may end up undermining welfare (Granovetter, 1985).[4]

To explore this econometrically, I begin by examining the policy's impact on swidden plots.[5] I first construct a dataset by merging satellite data, collected by environmental scientists, with administrative boundaries to create an annual village panel data of swidden plots across three decades (1991-2019). Exploiting the staggered nature of treatment that started in 2000, I find that resettlement reduces village swidden plots by 30-40% about five to ten years post-resettlement. The regression employs the estimator of Callaway and Sant'Anna (2021) to account for biases associated with the traditional TWFE method under staggered settings. At the outset, this result confirms the policy's direct effectiveness in achieving its immediate objective of reducing swidden prevalence.

Next, I investigate whether this policy of resettlement towards the valleys generated the intended improvements in living standards and economic outcomes. To identify these welfare effects, I use individual-level census data and employ a difference-in-differences-in-differences strategy that exploits three sources of variation: ethnicity (traditionally swidden vs. wet-rice groups), geography (districts with high vs. low resettlement intensity), and time (before vs. after policy implementation). Using ethnographic information from various sources, I classify ethnic groups into two broad groups: those traditionally practicing swidden cultivation, and those traditionally practicing wet-rice cultivation. To address the absence of village-level identifiers in these data, I measure treatment intensity as the share of swidden villages resettled within one's own district (or province) between 2000, the start date of the official policy, and 2015, the latest available census year. The specification includes a comprehensive set of interacted fixed effects to account for regional-, ethnic-, and time-specific confounders.

I find that the resettlement policy is associated with significant increases in child mortality. A 10 percentage point increase in the district share of resettled swidden villages is associated with 0.05 additional child deaths per mother, representing a 15-20% increase over baseline mortality rates. The adverse consequences of the policy extend to education outcomes. Swidden groups living in districts with more intensive resettlement face modest reductions in school completion rates at both the primary and secondary school levels.

Additional economic outcomes (using the Multiple Indicator Cluster Survey data from two periods, 2000 and 2011-12) follow a similar negative pattern. Wealth—measured either as scores or relative rankings—declines significantly for swidden communities

[4] A recent review article by Guiso et al. (2024) introduces economists to the idea of embedded culture, where culture is not merely an outcome of economic interactions but is rooted in social relations that shape behavior.

[5] Besides the term swidden, other synonyms include shifting and slash-and-burn.

following resettlement, indicating increased poverty relative to trends that would have occurred otherwise, while health outcomes (proxied by height-for-age, weight-for-height and weight-for-age) also decline by about 0.3 to 0.6 standard deviations for swidden households moving from the bottom to top quartile of the intensity measure. This is again puzzling considering the policy's aim was to alleviate poverty by inducing technological change and ushering in modernization.

Therefore, this raises the question of whether resettlement in fact transformed societies toward the newer form of cultivation. Or is there evidence suggesting inertia of old practices? To assess this, I use data from the Lao Decide Info Portal (2011) that provides village level information on the share of households cultivating swidden rice or wet rice. Given that this is a one-period cross-sectional data, I first restrict the sample to villages under the resettlement plan. Then for each village I use its duration since resettlement as the independent variable and regress on it the village's share of swidden farmers. As an alternate measure of treatment, I use a binary variable that takes value 1 if a village was already resettled as of 2011, and 0 if it was to be resettled in the near future. This OLS specification includes ethnicity and district fixed effects, as well as controls for mean village elevation, ruggedness, area, and pre-policy swidden plot counts.

The negative welfare effects, it appears, are likely driven by the policy's failure to generate the anticipated structural transformation—I observe relatively small impacts on the share of farmers actually adopting the new wet-rice method, with many farm households continuing with the old swidden ways, suggesting the policy merely reduced per-capita land availability by moving farmers away from their old plots without facilitating structural transitions. Already-resettled villages have a 5.4 percentage point (<10%) lower share of swidden farm households compared to villages to-be-resettled as of 2011. Likewise, for every additional year since resettlement, the share of swidden farm households drops by 0.5 percentage points. Against the 30-40% reduction in swidden plots five to ten years post-resettlement, this implies a significant deficit in plots per swidden household.

In line with this hypothesis, I observe behavioral adjustments: farmers shorten fallow periods, reducing the time the soil has to regenerate. To arrive at these results, I first identify all swidden plots cultivated in 2011. I then calculate their fallow duration by identifying the next year those same plots were re-cultivated. I find that already-resettled villages have 0.28 years shorter fallow durations compared to villages to-be-resettled, against a mean of 7.4 years. At the intensive margin of treatment intensity, for every additional year of a village's exposure to the policy, fallow duration drops by 0.1 years. Another outcome I use is a binary variable if a plot cultivated in 2011 was re-cultivated in 2019 (the last year of available swidden plot data) or earlier. Already-

resettled villages are 7.4 percentage points (14%) more likely to have been re-cultivated by 2019 or earlier. And since long fallow times are required for the soil to regenerate nutrients (Chen et al., 2023), the shortened duration likely increases weeding efforts and reduces productivity (Shoemaker and Baird, 2005). Consistent with observations from anthropologists, resistance to agricultural modernization reflects more than geographic constraints and instead stems from the deep cultural and social embeddedness of swidden practices within ethnic community structures.

An alternative explanation is that resettlement disrupted the provision of public services, thereby reducing welfare. However, if anything, the evidence suggests the opposite: villages that have been resettled for a longer period are more likely to have access to public services—roads, medical facilities, and schools. This pattern is intuitive since the original locations on steeper slopes were less accessible, whereas the new sites facilitated connectivity. Therefore, a public service disruption story is unlikely to explain the observed effects.

Lastly, I try to rule out a purely topographical explanation for the slow technological transition, if the concern is that the resettled villages are significantly different from those inhabited by traditional wet-rice societies. To do so, I employ a propensity score matching design that directly compares resettled swidden villages with traditional wet-rice villages that are geographically comparable within the same province. This method pairs villages based on key topographic characteristics—elevation, terrain ruggedness, distance to nearest river/stream, and village area—to create a control group of wet-rice villages that face nearly identical agro-ecological constraints. The results show a persistent 21 to 27 percentage point gap in wet-rice adoption.

These findings contribute to three strands in the literature. First, it informs us about how a policy intended to boost agricultural productivity and rural livelihoods instead produced worse outcomes. In this sense it speaks to a nascent but growing literature on cultural inertia and policy failures. For example, Le Rossignol et al. (2024), in a global sample, show how societies with traditional communal land ownership tend to have less success with land titling projects. McGuirk and Nunn (2024) find that when agricultural projects in Africa are implemented in traditional pastoral societies, they raise the incidence of conflict.[6] Using a randomized controlled trial, Puerto (2024) finds that a greater mismatch between the seeds offered to farmers and their stated preferences substantially reduces the uptake of the new technology. Other closely related studies are

[6]Outside economics, there are mixed findings on the impacts of sedentarizing nomadic societies. For example, Little and Gray (1990) find height increases for children post settlement, while Fratkin et al. (2004) find the opposite.

more qualitative, such as Lansing (2009) on the importance of rituals in agriculture, and how for some societies the two are deeply intertwined. Likewise, Scott (1998) provides multiple historical case studies of large-scale policies across the world that ultimately failed.

Second, this paper helps us better understand theories of technological adoption (Basu and Weil, 1998). Recent work shows the importance of suitability between technology types and the environment they are applied to (Moscona and Sastry, 2025). While in my study the policymakers did recognize the importance of appropriate environments for the new technology, and hence the relocation, they failed to account for the more tacit knowledge associated with traditional agricultural technologies, and how over time they also shape other societal relations beyond agriculture (Nelson and Winter, 1985; Guiso et al., 2024). Thus, simply changing the environment might not facilitate the intended technological change.

Lastly, this paper adds to the literature on the consequences of forced relocation for the migrants themselves. Studies mainly focus on displacement due to war or persecution (Beaman, 2012; Arbatli and Gomtsyan, 2019) or natural disasters (Sacerdote, 2012; Nakamura et al., 2022). A few have nonetheless explored state-sponsored resettlement outside conflict settings. For example, Bazzi et al. (2016) study the voluntary resettlement of Indonesians away from the denser islands and into sparser regions, and find that agro-climactic similarity between old and new settlements predicts rice productivity. In an African case study, Carlitz et al. (2025) find that the Tanzanian government's *Ujamaa* policy strengthened national identity among those resettled in state-created villages.

The remainder of the paper proceeds as follows. Section 2 provides background on the resettlement policy and its historical context. Section 3 describes the multiple data sources and construction of key variables. Section 4 outlines the empirical strategy, including both the staggered difference-in-differences approach for agricultural outcomes and the triple difference specification for welfare outcomes. Section 5 presents the main results on agricultural practices, education, health, and wealth. Section 6 investigates potential mechanisms underlying the welfare effects, examining whether communities successfully transitioned to wet-rice agriculture or simply experienced reduced agricultural productivity.

2 Study Setting

2.1 Brief Historical Account of Southeast Asian Agriculture

The agricultural landscape of Southeast Asia is characterized by a fundamental dichotomy between the lowland valleys and the upland highlands, a division that is as much socio-cultural as it is geographical (Scott, 2009). For centuries, the fertile alluvial plains and river deltas have been the domain of *wet-rice cultivation*. This is an intensive form of irrigated agriculture that supports high population densities and is typically associated with dominant ethnic majorities such as the Lao Loum in Laos and the Kinh in Vietnam. Wet-rice societies often developed centralized political structures, detailed land tenure systems, and social hierarchies organized around the control and maintenance of complex irrigation networks (Lansing, 2009). This agricultural system, with its sedentary nature and high yields, came to be seen by colonial and post-colonial states alike as the epitome of civilization and economic productivity.



(a) A wet-rice field.

(b) A swidden plot burning.

Figure 1: Images of agricultural farms: wet-rice cultivation (left) and swidden agriculture (right).

In contrast, the vast, forested uplands that span the region are home to a diverse array of ethnic minorities who have traditionally practiced *swidden agriculture* (also known as shifting or slash-and-burn cultivation). This system involves clearing a patch of forest, burning the vegetation to release nutrients into the soil, and cultivating it for a few seasons before allowing it to revert to forest for an extended fallow period (Conklin, 1957). Far from being a primitive or haphazard practice, swidden is a highly adapted and complex agro-ecological system (Dove, 2012). It is designed to manage risk, conserve biodiversity, and sustainably cycle nutrients within a fragile ecosystem. Crucially, swidden is more than a mere technique for growing crops; it is a foundational element of social and cultural life.

Kinship ties, spiritual beliefs, ritual calendars, and collective land ownership are deeply embedded within the practice, forming a cohesive socio-cultural system that has sustained these communities for generations (Sprenger, 2006).

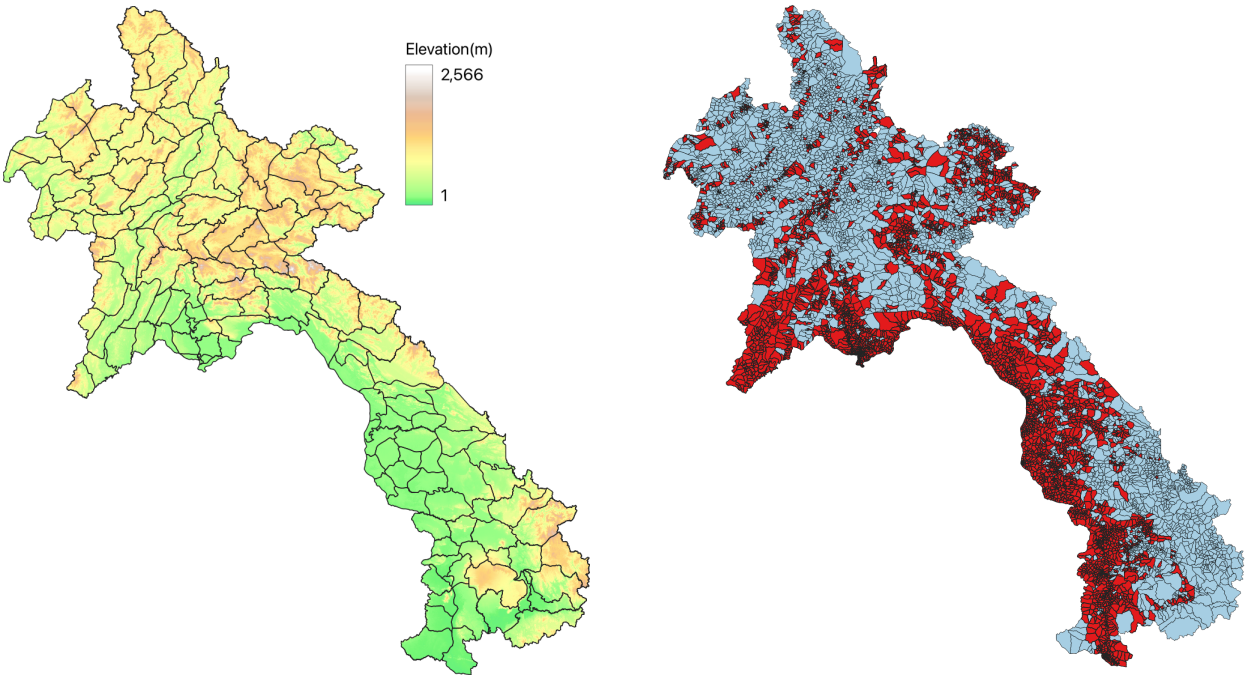
The historical relationship between these two agrarian systems has often been one of tension. From the perspective of lowland states, the uplands and their inhabitants were peripheral, both geographically and culturally. The mobility and perceived independence of swidden societies posed a challenge to state-making projects focused on territorial control, taxation, and legibility (Scott, 2009). This historical context frames the modern-era policies of governments across the region, including Laos, which have sought to sedentarize these populations, integrate them into the national fabric, and replace swidden with what is deemed a more modern and productive form of agriculture.

Against interpreting a strict dichotomy: An important point one should note, however, is that these two agricultural systems should not be interpreted as mutually exclusive categories applying cleanly to entire ethnic groups. In practice, most villages—and often individual households—operate along a continuum between swidden and wet-rice cultivation. For instance, even communities traditionally identified as swidden cultivators frequently maintain small irrigated paddies in valley bottoms, while predominantly wet-rice societies often cultivate upland fields during years of labor shortages, pest shocks, or irregular rainfall. These mixed strategies reflect ecological variability as well as household-level risk diversification. However, what varies is not the presence or absence of either practice but rather the degree of reliance on each system and the institutional arrangements that evolve around them (Mertz et al., 2009).

2.1.1 Swidden as Practiced in Laos

In Laos, swidden agriculture is a finely tuned agro-ecological system synchronized with the tropical monsoon climate. The cycle begins in the dry season (typically February to March) when a household or kin group selects a secondary forest plot based on the quality of the regenerated vegetation, which indicates soil fertility (Ducourtieux et al., 2005). The chosen area is cleared and the dried vegetation is burned towards the end of the dry season, with the resulting ash providing a vital pulse of nutrients for the soil (Mertz et al., 2009). With the arrival of the first rains in April or May, a diverse mix of crops—primarily upland (dryland) rice, but also vegetables, maize, and tubers—is sown. The rice is harvested in the fall, after which the plot is left to lie fallow for a period that can range from 5 to over 15 years, allowing the forest to regenerate and the soil to recover its fertility before the cycle may begin again on that same land (Schmidt-Vogt, 1999).

Panel (b) of Figure 2 plots the spatial distribution of traditional swidden (blue) and



(a) District boundaries of Laos overlaid on an elevation map.

(b) Spatial distribution of traditional swidden (blue) versus wet-rice (red) villages.

Figure 2: Maps of Laos for reference

wet-rice (red) villages.

2.2 Policy Background

Historically, the approaches taken by various Southeast Asian states to modernize their rural populations have varied considerably, but a common theme, as the previous section outlines, has been the drive to transition societies to wet-rice agriculture (Scott, 2009). In the modern era, countries like Vietnam have tackled this by encouraging migration of the majority Viet Kinh ethnic group (who traditionally practice wet-rice) from densely populated lowlands to the highlands inhabited by swidden minority groups, a policy aimed at integrating the minorities into the national fabric while also inducing them, via cultural learning, to adopt a new livelihood method (Hardy, 2005). Laos, on the other hand, has pursued the opposite strategy, relocating highland populations to the valleys to consolidate settlement patterns, agglomerate villagers, and thereby end the practice of swidden agriculture.

Although efforts to regulate swidden cultivation date back to the 1970s and 80s, it was only in 1998-1999 that it became an official policy (Boillat et al., 2015). This large-scale resettlement was a central pillar of the GoL's modernization and poverty reduction agenda

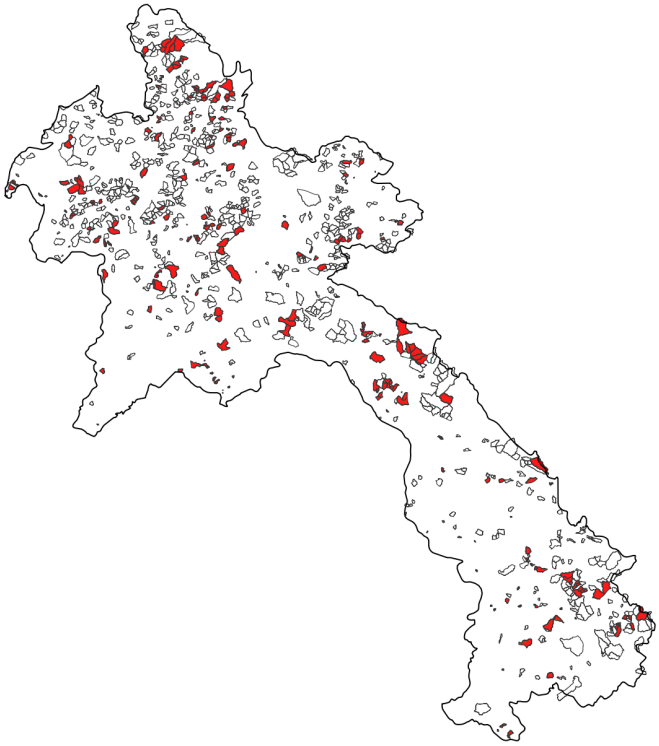


Figure 3: Unshaded polygons are villages resettled between 2000 and 2011. Red (shaded) ones indicate villages planned to be resettled after 2011.

in the early 2000s. The primary objective was to replace what was perceived as an economically inefficient and environmentally destructive form of agriculture with sedentary wet-rice cultivation (Evrard and Goudineau, 2004). Swidden farming—practiced predominantly by ethnic minorities in the uplands—was seen as an obstacle to development, forest conservation, and state integration. The GoL aimed to concentrate scattered highland settlements into clustered villages in valley areas, closer to infrastructure, markets, and state services. During the early stages of crafting the policy, the ambitions were high, with an initial target to completely relocate all swidden villages by 2000 (Baird and Shoemaker, 2007). But with delays and limited state capacity, the deadline was pushed to 2020. Figure 3 gives the spatial distribution of resettled villages.

Figure 4 plots a histogram of the number of villages resettled. The data comes from the Laos Agricultural Census published in 2011. Thus for the years after 2011, the villages in the resettlement program were listed as 'to be resettled.' Between 2000 and 2011, over 750 villages were relocated.

International donors and environmental organizations often supported these efforts, while also sharing the view that swidden cultivation may be associated with deforestation and poverty (EU, 2011). However, critics have consistently raised concerns about inad-

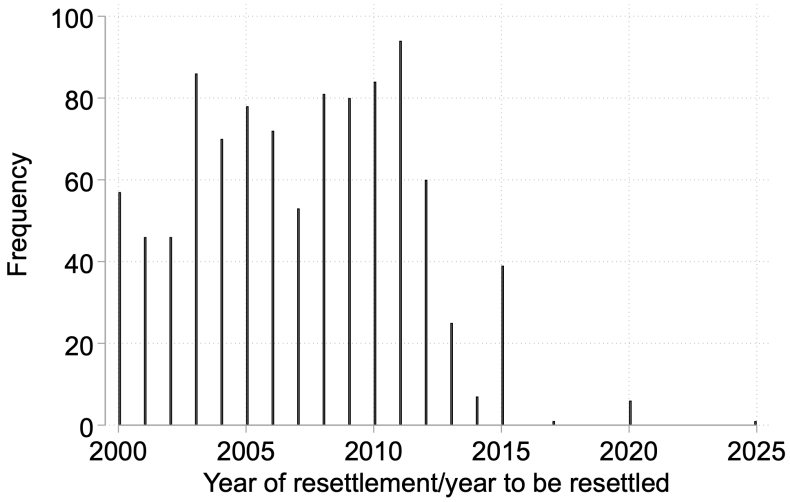


Figure 4: Histogram of the year a village was resettled/to be resettled. Data from the 2011 Laos Agricultural Census.

equate consultation with affected communities, and loss of traditional livelihoods and cultural practices (Petit, 2008). To date, studies evaluating the policy have been descriptive. Nonetheless, the overwhelming consensus is that the policy has been disastrous for the livelihoods of those affected (Lund, 2013).

3 Data

The data used in this study are drawn from a range of sources, among them datasets compiled by environmental scientists. In what follows, I describe each source in detail and also explain how the key variables are constructed.

3.1 Swidden Plot Data

I create annual data at the village level for the number of swidden plots by merging together two geographic data sources. The first one, in raster format, comes from Chen et al. (2023) and Chen et al. (2023). Using Landsat satellite time series data on Google Earth Engine, the authors construct annual maps of swidden cultivation across Laos from 1991 to 2020 at 30m × 30m pixel resolution. Their methodology combines Continuous Change Detection and Classification with Spectral Mixture Analysis (CCDC-SMA) to identify forest disturbances, followed by object-based image analysis and post-disturbance land cover classification to attribute disturbance causes. The approach specifically detects the cyclical patterns characteristic of shifting cultivation: slash-and-burn events (marked

by sudden decreases in vegetation indices), short cropping periods with high seasonality, and fallow periods during vegetation recovery.

The authors focus their analysis on the dry season (February to May) when slash-and-burn activities typically occur in Laos, creating image composites from this period to minimize cloud interference and capture burning events effectively. The final maps classify annual forest disturbances into five categories (shifting cultivation, new plantation, deforestation, severe drought, and subtle disturbance).

To identify administrative boundaries (province, district, village) in Laos, I use data from the Lao Decide Info Portal.[7] Using GIS software, I am able to aggregate the agricultural data to the village level while also preserving detailed identifiers that I merge with other datasets mentioned below.

3.2 Village Level Indicators

The Lao Decide Info Portal also provides additional village-level information I incorporate for my analysis. Collected primarily in 2011, it includes information on the year villages were resettled, or the year a village was planned to be resettled. This allows me to identify treatment start year for my diff-in-diff analysis as well as for measuring district-level exposure to the policy (more details outlined later). The data also includes information from the Agriculture Census 2011, such as share of farm households growing upland/lowland rice.

3.3 Economic Outcomes Data

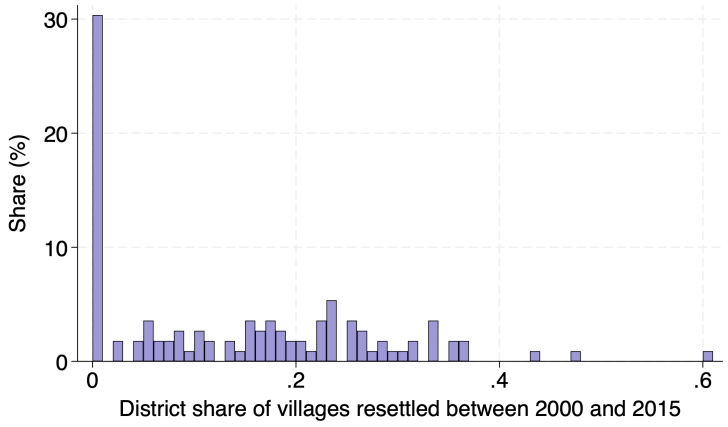
Data for schooling and mortality outcomes are obtained from the Laos Population Census (1995 and 2015) provided by the IPUMS Database (Ruggles et al., 2010). Both census years use 0.1% samples of the full population, providing individual-level data on educational attainment, demographic characteristics, and household composition. The census data includes information on years of schooling completed, primary and secondary school completion rates, age, gender, ethnicity, and geographic location at the district level. The 20-year gap between the two census rounds allows for identification of cohort-specific effects of the resettlement policy, as individuals surveyed in 2015 would have experienced their schooling years either before or after the policy implementation in the 2000s.

For wealth and other health outcomes, I use data from the Multiple Indicator Cluster Surveys (Bolgrien et al., 2024) using the 2000 and 2011-2012 rounds. The MICS surveys are nationally representative household surveys conducted by UNICEF that collect data

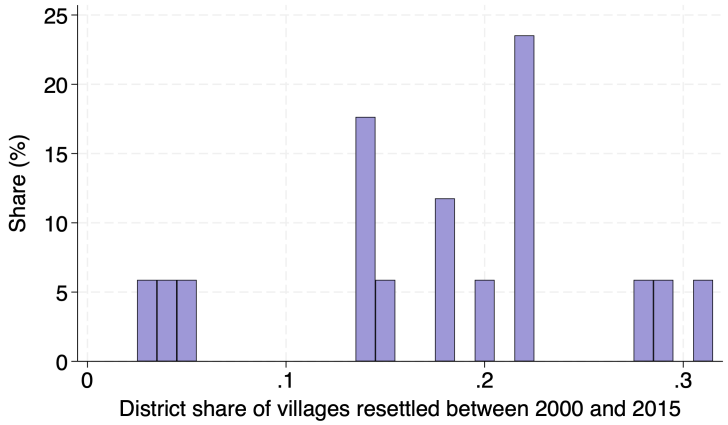
[7] https://en.data.k4d.la.

on child mortality, maternal health, and household wealth indicators. Wealth score: The wealth score is constructed using principal component analysis of household asset ownership, housing characteristics, and access to utilities, with higher scores indicating greater wealth. Wealth index: The wealth index is a standardized composite measure combining asset ownership, housing quality indicators, and access to basic services, normalized to have a mean of zero in the baseline population.

3.4 Treatment Intensity Measure



(a) District level



(b) Province level

Figure 5: Histogram of the share of swidden villages resettled between 2000 and 2015.

For a number of individual- and household-level outcomes I study, I do not have location identifiers at the village level. Therefore, to measure the treatment intensity, I create a variable by using data on the year of resettlement and aggregating them to the

district (or province) level.

I first identify all swidden villages in a given district. Then I calculate the share of swidden villages resettled within that district. The idea here is to more accurately capture the policy's specific focus on swidden traditional livelihoods.

Specifically, the intensity in district d is given by:

$$\text{Intensity}_d = \frac{\text{Number of resettled swidden villages in district } d}{\text{Total number of swidden villages in district } d} \tag{1}$$

This metric provides a continuous measure of the local policy pressure faced by the targeted population. Specifically, it serves as a proxy for the probability that a swidden-practicing individual in a given district was affected by the resettlement policy.[8]

Figure 5(a) shows the distribution of Intensity_d across districts. The distribution is skewed, with approximately 30% of districts showing an intensity value of zero, meaning they did not have any swidden villages resettled.

Analogously, for cases where I only have province-level identifiers, a similar variable is created by aggregating the numerator and denominator to the province level.

Figure 6 presents the distribution of average year of resettlement across districts that experienced at least one village resettlement. Among the 103 districts affected by the policy, the median resettlement year was 2006, with a standard deviation of 2.3 years.

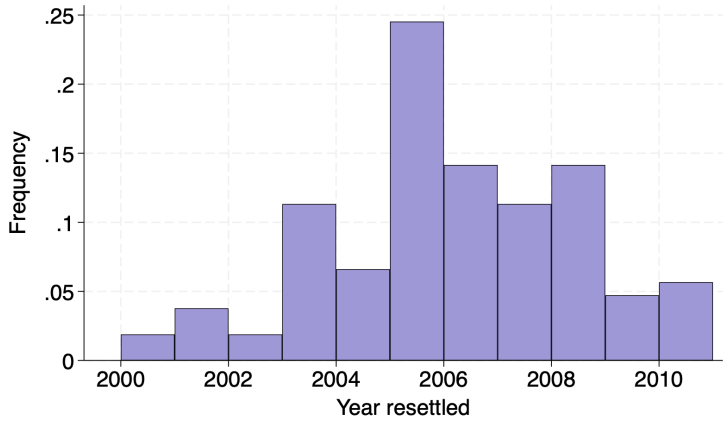


Figure 6: Distribution of year of resettlement at the district level, conditional on having at least one village resettled within the district.

Drawbacks of the Intensity Measure: Constructing the intensity measure at the district level necessitates assigning a uniform exposure score to all individuals within that

[8]Each district in Laos has at least one swidden village, with a mean of 36 (about half a district's share) and a median of 33. Therefore, there is no concern of 0 values in the denominator.

boundary, regardless of their specific resettlement status. This aggregation inherently introduces measurement error by failing to perfectly distinguish between those directly affected by relocation and those who were not. Such imprecision functions as classical measurement error in the independent variable, which effectively dilutes the treatment signal and generates attenuation bias, driving the estimated coefficients toward zero. Therefore, the negative welfare effects reported here likely represent a conservative lower bound of the true impacts on the resettled communities.

3.5 Identifying Traditional Agricultural Practices

In Laos, the distinction between traditional wet-rice and swidden cultivation is well documented and largely corresponds to ethno-linguistic divisions (Stuart-Fox, 1993). Groups belonging to the Kra-Dai (or Lao-Tai) language family are typically wet-rice cultivators, whereas those from the Hmong-Mien, Austro-Asiatic, and Tibeto-Burman families have historically practiced swidden agriculture (Enfield, 2008). Accordingly, to classify ethnic groups into either agricultural category, it suffices to identify their language family. Tables C1 and C2 in the Appendix list the sources used for this linguistic-based classification.

Figure B.4 in the Appendix displays a residual binned scatter plot of the share of farmers practicing swidden cultivation against the author's classification of ethnic groups using the criteria above (conditional on geographic controls and district fixed effects). The relationship is strong and highly significant ($\beta = 0.20$, $se = 0.02$), providing validation for the classification approach and strengthening the claim that linguistic affiliation is a reliable proxy for traditional agricultural practices.

4 Empirical Strategy

4.1 Difference-in-Differences

To study the impact of the policy on swidden cultivation prevalence, I use a difference-in-differences model exploiting the staggered timing of the policy. To correct for biases of the traditional TWFE estimator under such settings, I use the one proposed by Callaway and Sant'Anna (2021) and run the following specification-

$$y_{it} = \sum_{l=-15}^{-2} \beta_l \times \text{Resettled}_{i,t}^{(l)} + \sum_{p=0}^{15} \beta_p \times \text{Resettled}_{i,t}^{(p)} + \alpha_i + \alpha_t + \epsilon_{it} \tag{2}$$

where y_{it} is the logarithm of the number of swidden plots in village i at time t, and $\text{Resettled}_{i,t}^{(k)}$ is an indicator equal to 1 if village i is k periods relative to resettlement at time t, with $k = -1$ as the omitted reference period. α_i and α_t are village and time fixed effects respectively. The coefficients β_l and β_p capture the dynamic treatment effects of the resettlement policy. Standard errors are clustered at the village level.

4.2 Triple Difference Estimation

Next, to estimate the policy's impact on my economic outcomes, I use Census data as well as the MICS survey data. Unfortunately, these sources provide geographic identifiers at levels of aggregation above the village: district and province. Thus, to get causal estimates of the policy, I employ a triple difference (DDD) as follows-

$$y_{iedt} = \beta \left(\text{Swidden}_e \times \text{Intensity}_d \times \text{Post}_t\right) + \alpha_{dt} + \alpha_{et} + \alpha_{de} + \mathbf{X}_{iedt}^{'}\mathbf{\Gamma} + \epsilon_{iedt} \tag{3}$$

where Swidden_e takes value 1 if ethnic group e is a traditional swidden society, and 0 otherwise; Post_t takes value 1 if the census year is 2015, and 0 if 1995; Intensity_d is the share of swidden villages within district d that were resettled between 2000 and 2015 (see Section 3.4 for more details on this variable's construction). Figure 5(a) plots the histogram of this variable. It has a mean of 11% and ranges from 0 to 51%.

The specification includes a comprehensive set of fixed effects to control for unobserved heterogeneity: α_{dt} represents district-by-time fixed effects. These absorb any time-varying characteristics specific to each district, such as local economic shocks, infrastructure development, or policy changes that could independently influence outcomes like education or health. The term α_{et} controls for nationwide trends or shocks that differentially affect each specific ethnic group over time. This accounts for factors such as broad cultural shifts or nationwide policies targeted at specific ethnicities. α_{de} accounts for district-by-ethnicity fixed effects. These absorb time-invariant, district-level factors that differently impact ethnic groups, such as historical settlement patterns, persistent geographic advantages or disadvantages, or long-standing local ethnic dynamics.

The key identification assumption is that swidden and wet-rice societies would have experienced similar differential trends between high and low intensity regions in the absence of the resettlement policy, conditional on district-specific time trends, ethnicity-specific time trends, and time-invariant district-ethnicity interactions. I two-way cluster my standard errors at the ethnicity-district (or ethnicity-province) level.

5 Results

5.1 Policy Targeting

I begin by examining whether the resettlement policy systematically targeted villages traditionally associated with swidden agriculture. Specifically, I estimate a linear probability model where the dependent variable equals one if a village was already resettled or planned to be resettled under the policy, and zero otherwise. The key explanatory variable, Swidden_v, indicates whether the village belongs to an ethnic group traditionally practicing swidden cultivation. The specification includes district fixed effects to account for unobserved geographic heterogeneity, and controls for village characteristics such as area, elevation, ruggedness-

$$\text{Targeted}_{vl} = \beta \times \text{Swidden}_v + \mathbf{X'}_{vl}\mathbf{\Gamma} + \alpha_l + \epsilon_{vl} \tag{4}$$

Targeted_{vl}, equals one if village v in location l {District, Province} was targeted by the resettlement policy, and zero otherwise. The key explanatory variable, Swidden_v, is an indicator for whether the village belongs to an ethnic group traditionally practicing swidden cultivation. The variable $\mathbf{X}_{vl}$ includes a control for village area. Location fixed effect, α_l, absorb unobserved geographic or administrative factors common to all villages within the same district or province, while ϵ_{vl} is an idiosyncratic error term. The coefficient of interest, β, captures the within-district difference in the probability of being targeted for resettlement between villages traditionally associated with swidden cultivation and those that are not, conditional on the included controls.

Table 1 reports the results. Across all specifications, villages belonging to ethnic groups historically practicing swidden cultivation were significantly more likely to be selected for resettlement. In the simplest specification, shown in Column 1, swidden villages were about 15 percentage points more likely to be targeted by the policy. The coefficient remains positive and highly significant after the inclusion of district and province fixed effects, and its magnitude stabilizes around 8–10 percentage points once village-level controls.

These results provide strong evidence that the policy prioritized communities traditionally engaged in swidden cultivation. The pattern of targeting is consistent with the government's modernization agenda, which sought to transform shifting cultivators into sedentary wet-rice farmers through planned relocation.

Table 1: Policy Targeting of Swidden Villages

	(1)	(2)	(3)	(4)	(5)	(6)
	Was the village targeted by the resettlement policy?					
Swidden_v	0.150***	0.146***	0.095***	0.094***	0.115***	0.111***
	(0.012)	(0.012)	(0.010)	(0.010)	(0.011)	(0.011)
Controls		✓		✓		✓
District FE			✓	✓		
Province FE					✓	✓
Observations	8,643	8,643	8,643	8,643	8,643	8,643
R-squared	0.056	0.058	0.121	0.121	0.074	0.076
Mean dep. var.	0.0879	0.0879	0.0879	0.0879	0.0879	0.0879

Notes: Data taken from the Laos Agriculture Census 2011. The unit of observation is a village, and the sample includes all villages in Laos listed in the 2011 Agricultural Census. Swidden_v indicates if village v belongs to an ethnic group traditionally practicing swidden agriculture. The outcome variable indicates if a village belongs to the two types of villages under the policy: already resettled villages between 2000 and 2011, and villages to-be-resettled from 2011 onward. Standard errors are two-way clustered at the district-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

5.2 Resettlement and Swidden Plots (First Stage)

A fundamental question for evaluating the resettlement program concerns whether the policy achieved its stated proximate objective of reducing swidden cultivation. To address this, I estimate the dynamic treatment effects of village resettlement on swidden agricultural land use using high-resolution satellite imagery that identifies shifting cultivation plots through spectral signatures of recently cleared and burned forest areas.

Figure 7 plots the estimated coefficients β_l (leads) and β_p (lags) from equation 2 using the Callaway and Sant'Anna (2021) difference-in-differences estimator, with $log(0.01 + \text{shifting pixels}_t)$ as the dependent variable.[9] These coefficients correspond to group-time average treatment effects on the treated (ATTs) relative to the last pre-treatment period, aggregated across cohorts and years. The log transformation addresses the highly skewed distribution of swidden plot counts while allowing for a proportional interpretation of treatment effects.

The results provide strong evidence that resettlement causally reduced swidden agricultural land use. First, the pre-treatment coefficients remain statistically indistinguishable from zero across all lead periods, lending credibility to the parallel trends assumption underlying the difference-in-differences identification strategy. This pattern indicates that treated and control villages exhibited similar trajectories in swidden cultivation prior to

[9] Appendix Figure A.2 demonstrates the robustness of these results to an alternative specification using $log(0.1 + \text{shifting pixels})$ as the outcome measure.

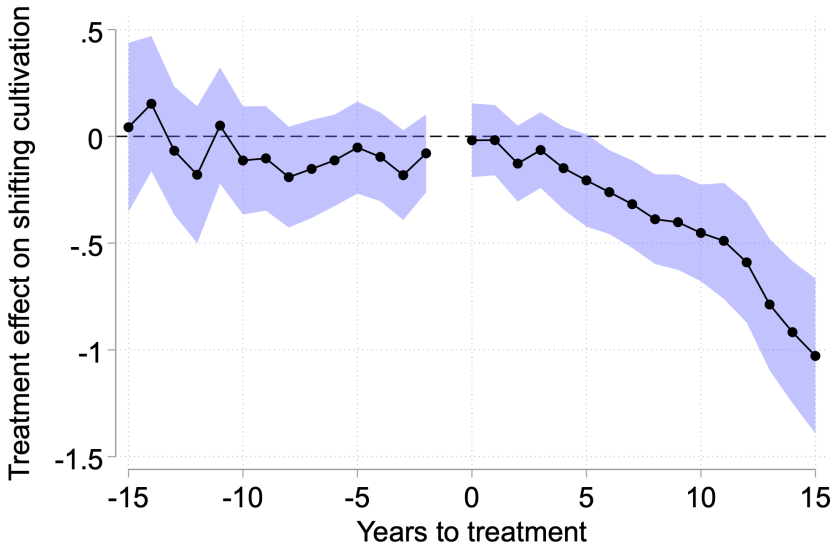


Figure 7: Event study plot of the logarithm of swidden plots. The estimates are based on equation-2 and uses the Callaway and Sant'Anna (2021) estimator.

resettlement, alleviating concerns about differential pre-trends that could confound causal inference.

Following resettlement, swidden land use begins declining approximately 4-5 years post-treatment, with the effect becoming statistically significant and growing in magnitude over time.

The magnitude of the estimated effects is economically substantial and policy-relevant. Between 5-10 years after resettlement, treated villages experience a 30-40% reduction in swidden cultivation area relative to control villages, with the decline persisting and potentially intensifying in subsequent years.

These findings conclusively demonstrate that the resettlement policy succeeded in achieving its proximate objective of reducing traditional swidden agricultural practices among targeted ethnic minority communities. This first-stage result establishes a necessary foundation for interpreting the welfare effects examined in subsequent sections: the policy generated a significant shock to traditional production systems, raising the critical question of whether this disruption facilitated successful transitions to alternative livelihoods or instead imposed welfare costs without generating commensurate benefits.

5.3 Effects on Child Mortality

Having established the policy's direct effects on agriculture, I turn to examine its broader welfare implications, beginning with mortality. Table 2 presents triple difference estimates

for mortality outcomes, comparing swidden and wet-rice ethnic groups across districts with varying resettlement intensity before and after policy implementation. The results provide stark evidence of deteriorating health conditions among swidden communities following resettlement.

Table 2: Triple Difference Estimates: Mortality

	(1)	(2)	(3)	(4)	(5)
	Any deaths in household last year	Number of children who have died			
$\text{Swidden}_e \times \text{Intensity}_d \times \text{Post}_t$	-0.018	0.488**	0.358**	0.338*	0.441**
	(0.036)	(0.205)	(0.171)	(0.180)	(0.193)
Mother born after-	Full sample	Full sample	1965	1960	1955
Controls	✓	✓	✓	✓	✓
District FE × Census Year FE	✓	✓	✓	✓	✓
District FE × Ethnic FE	✓	✓	✓	✓	✓
Ethnic FE × Census Year FE	✓	✓	✓	✓	✓
Observations	126,342	118,067	54,562	75,225	94,615
R-squared	0.031	0.187	0.159	0.167	0.180
Mean dep. var.	0.0408	0.434	0.238	0.293	0.359

Notes: The data is a repeated cross section taken from the 1995 and 2015 Laos Census. Column 1 outcome is a household level question and therefore the sample is restricted to the household head. Outcomes of columns 2 to 5 are available only for women aged 15-49. Columns 3 to 5 are further restricted to women born in later years, so that for the 1995 census I avoid cohort-specific confounders due to the Laotian Civil War (1962-75). All columns control for age. Post equals 1 if the census year is 2015 and 0 if 1995. Swidden indicates if the individual belongs to an ethnic group traditionally practicing swidden agriculture. Intensity (ranging from 0 to 1) is the share of swidden villages resettled in one's district of residence between 2000 and 2015. Standard errors are two-way clustered at the district-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

Column 1 shows no significant effect on whether households experienced any deaths in the past year, suggesting the policy did not affect overall mortality. However, columns 2 to 5 reveal substantial increases in child mortality specifically. For women of childbearing age, each ten percentage point increase in district resettlement intensity leads to 0.049 additional child deaths (significant at the 5% level) among swidden groups relative to wet-rice groups.

Columns 3 to 5 restrict the sample to women born in later cohorts, so as to avoid potential confounders by the Laotian Civil War (1962-75). This historic event would have differentially impacted older mothers, and the set of fixed effects I include may not adequately absorb this shock. This restriction yields a slightly smaller effect though highly significant: for women born after 1965, it amounts to 0.036 additional child deaths per ten percentage point increase of resettlement intensity (significant at the 5% level). Given

the restricted sample mean of 0.24 children who have died, this represents about a 15% increase in baseline child mortality.

These mortality findings demonstrate that the resettlement policy imposed severe costs on child survival rates, thus corroborating other qualitative studies of this particular outcome (Baird and Shoemaker, 2007).

5.4 Impact on Educational Attainment

Table 3 examines the policy's impact on education outcomes using the same empirical framework. The sample is restricted to individuals aged 15-30 at the time of their respective census, ensuring these cohorts experienced their schooling years either entirely before (1995 census) or substantially during (2015 census) the policy period. The results reveal consistently negative effects across all educational measures for swidden groups in high-intensity resettlement districts.

Table 3: Triple Difference Estimates: Schooling

	Primary school	Secondary school	High school	Years of schooling
$\text{Swidden}_e \times \text{Intensity}_d \times \text{Post}_t$	-0.162*	-0.198**	-0.041	-0.852
	(0.085)	(0.077)	(0.049)	(0.647)
Controls	✓	✓	✓	✓
District FE × Census Year FE	✓	✓	✓	✓
District FE × Ethnic FE	✓	✓	✓	✓
Ethnic FE × Census Year FE	✓	✓	✓	✓
Observations	207,755	207,755	207,755	207,755
R-squared	0.328	0.203	0.098	0.371
Mean dep. var.	0.565	0.297	0.0853	5.075

Notes: The sample is restricted to individuals aged 15 to 30 at the time of their respective census (or 0-15 years of age in 2000 for the Post_{2015} census). 'Years of schooling' ranges from 0 to 13 and captures the number of schooling years completed. Post equals 1 if the census year is 2015 and 0 if 1995. Swidden indicates if the individual belongs to an ethnic group traditionally practicing swidden agriculture. Intensity (ranging from 0 to 1) is the share of swidden villages resettled in one's district of residence between 2000 and 2015. Controls include age and gender. Standard errors are two-way clustered at the district-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

The coefficient on the triple interaction term indicates that a ten percentage point increase in district-level resettlement intensity of swidden villages reduces primary school completion rates by 1.62 percentage points for swidden relative to wet-rice groups (significant at the 10% level). Secondary school completion shows a decline of 1.98 percentage points (significant at the 5% level). High school completion rates fall by 0.41 percentage

points, though this estimate lacks statistical significance. In column 4, total years of schooling decrease by 0.085 years (significant at the 10% level) for swidden groups for every 10 percentage points increase in treatment intensity.

5.5 Household Wealth and Child Health

This section draws on data from the Multiple Indicator Cluster Surveys (MICS), specifically the 2000 and 2011–12 rounds. Because this dataset does not consistently report district identifiers, I measure treatment intensity at the province rather than district level, following the same construction as in equation 1.

5.5.1 Wealth

Table 4: Triple Difference Estimates: Wealth

	(1) Wealth score	(2) Wealth index
$\text{Swidden}_e \times \text{Intensity}_p \times \text{Post}_t$	-4.019**	-4.946***
	(1.612)	(1.693)
Controls	✓	✓
Province FE × Year FE	✓	✓
Province FE × Swidden FE	✓	✓
Swidden FE × Year FE	✓	✓
Observations	18,432	18,432
R-squared	0.372	0.357
Mean dep. var.	-0.283	2.544

Notes: The data is a repeated cross section taken from the 2000 and 2011-2012 Multiple Indicator Cluster Survey, and is restricted to household heads. Post equals 1 if the survey year is 2011-12 and 0 if 2000. Swidden indicates if the individual belongs to an ethnic group traditionally practicing swidden agriculture. Intensity (ranging from 0 to 1) is the share of swidden villages resettled in one's province of residence between 2000 and 2011. Standard errors are two-way clustered at the province-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

Table 4 presents results on economic welfare using two measures of household wealth: the first based on the wealth score, a continuous measure constructed from household ownership of assets and housing characteristics; and the second based on the wealth index, which groups households into quintiles—poorest (1), poorer (2), middle (3), richer (4), and richest (5)—according to their relative economic status within each survey round.

Both wealth measures show large, negative, and statistically significant effects. The wealth score decreases by 4.019 points (significant at the 5% level) for each 10 percentage point increase in provincial resettlement intensity among swidden groups. The wealth index similarly falls by 4.946 points (significant at the 1% level).[10]

In terms of interpreting magnitudes, swidden individuals moving from the bottom to the top quartile of the treatment intensity measure experience a reduction in wealth of about 0.70 units, equivalent to about 0.6 standard deviations.

Table 5: Triple Difference Estimates: Child Health

	(1) Height for age (z-score)	(2) Weight for height (z-score)	(3) Weight for age (z-score)
$\text{Swidden}_e \times \text{Intensity}_p \times \text{Post}_t$	-3.340*	-5.980***	-5.277***
	(1.949)	(1.923)	(1.182)
Controls	✓	✓	✓
Province FE × Census Year FE	✓	✓	✓
Province FE × Ethnic FE	✓	✓	✓
Ethnic FE × Census Year FE	✓	✓	✓
Age-group FE	✓	✓	✓
Observations	10,819	10,896	10,975
R-squared	0.160	0.104	0.122
Mean dep. var.	-1.956	-0.527	-1.526

Notes: The data comprises children aged between 6 months to 5 years. The regressions control for gender. Age-group are divided into 5 groups based on child age in months. Post equals 1 if the survey year is 2011-12 and 0 if 2000. Swidden indicates if the individual belongs to an ethnic group traditionally practicing swidden agriculture. Intensity (ranging from 0 to 1) is the share of swidden villages resettled in one's province of residence between 2000 and 2011. Standard errors are two-way clustered at the province-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

5.5.2 Child Health

Next, looking at child health outcomes from the same survey, Table 5 shows that children from swidden groups in provinces with higher resettlement intensity experience substantial declines in anthropometric outcomes measured by height-for-age, weight-for-height, and weight-for-age.

To interpret these magnitudes, among swidden households post-reform, moving from a province in the bottom quartile to one in the top quartile of treatment intensity reduces

[10] See Appendix Figure B.7 for trend changes in wealth by sub-groups.

height-for-age by 0.28 standard deviations, weight-for-height by 0.65 standard deviations, and weight-for-age by 0.67 standard deviations.

These magnitudes are substantial—comparable to effects documented in studies of forced displacement and severe livelihood disruption, such as civil war exposure Bundervoet et al. (2009); Jürges et al. (2022). While our setting differs from conflict zones, the resettlement program produced shocks similar in nature: forced relocation, loss of access to traditional lands and forest resources, and disruption of swidden agricultural practices that had sustained these communities for generations.

Discussion: Taken together, the results in section 5 establish a clear pattern: the resettlement policy achieved its immediate objective of reducing traditional agricultural practices but at considerable cost to human welfare. This pattern sets up the mechanisms analysis in Section 6, by investigating whether these welfare costs reflect successful transitions to modern agriculture, or failed transitions that left communities worse off without generating intended benefits.

6 Possible Mechanisms

As shown earlier in Figure 7, there are large reductions in swidden plots for the resettled villages. But does this reflect an actual decline in households practicing swidden agriculture, or simply a reduction in plots devoted to swidden cultivation that itself stems from the new, unfamiliar and thus unsuitable terrain for continued swidden activities? If the latter is true, then it suggests a possible mechanism for why we observe these drastic falls in living standards: rather than making the transition to wet-rice agriculture, cultural inertia interacting with unfamiliar terrain likely prevents successful adoption of the new practice.

6.1 Structural Change Analysis using the Already Resettled and To-be Resettled Villages Sample

To investigate this statistically, I use village-level data from the Laos Agriculture Census 2011. An outcome of interest is the share of farm households growing upland or lowland rice. I use these two measures as proxy outcomes for swidden and wet-rice cultivations respectively. Because I only have data from one survey round (2011), in order to create comparable groups, I first trim the sample to villages that were already resettled as of 2011, and those that were to be resettled from 2011 onward. Since all villages in this sample were targeted for resettlement, differences between already- and not-yet-resettled villages

are unlikely to reflect permanent selection differences. This produces a source of variation I can exploit: a treated group that takes value 1 for a variable I will call 'Already resettled,' and a not-yet treated group that takes value 0. I then run the following regression.

$$y_{vde} = \beta \times \text{Already resettled}_v + \alpha_e + \alpha_d + \mathbf{X}_\mathbf{v}'\mathbf{\Gamma} + \epsilon_{vde} \tag{5}$$

The dependent variable y_{vde} denotes the outcome of interest for village v in district d and ethnic region e, measured as the share of farm households cultivating upland (swidden) or lowland (wet-rice) rice. The main explanatory variable of interest is Already resettled$_v$, a binary indicator equal to 1 if village v had been resettled by 2011 and 0 otherwise. In addition, I also use 'Years since resettlement' as another explanatory variable at the intensive margin. The specification includes ethnic fixed effects α_e and district fixed effects α_d, which absorb unobserved cultural and geographic heterogeneity across these villages. The vector $\mathbf{X_v}$ contains village-level controls—area, elevation, ruggedness, and pre-policy average swidden plots—to account for observable differences across villages. Standard errors are two-way clustered at the district–ethnicity level to allow for correlated shocks within these dimensions.

Limitations of the data: Nonetheless, the analysis to follow exploiting variation between already-resettled and to-be-resettled villages requires interpretive caution. While restricting to policy-targeted villages addresses some selection concerns, the timing of treatment may not be fully exogenous—villages resettled earlier may systematically differ from those resettled later. Likewise, while the "years since resettlement" variable likely captures treatment duration, it may also partly reflect selection into early versus late resettlement. Moreover, the single cross-section from 2011 precludes difference-in-differences estimation. Consequently, these results should be interpreted as suggestive evidence of limited structural transformation rather than definitive causal estimates.

Results: Column 1 of Table 6 shows that villages already resettled as of 2011 have a 5.4 percentage point ($p < 0.1$) lower share of farm households engaged in swidden activities. Conversely they have a 4 percentage point higher share for wet-rice farm households, though this effect is not statistically significant. At the intensive margin, each additional year since resettlement is associated with a 0.5 percentage point decline in the share of swidden farmers (column 4, not statistically significant) and a 0.5 percentage point increase in the share of wet-rice farmers (column 8, also not statistically significant). This suggests two things: first, the policy appears to have moved societies in the intended direction. Secondly, and more starkly, the sizes are remarkably small relative to their means. This suggests that a vast majority in the resettled villages continue practicing

Table 6: OLS Estimates: Share of Farming Type by Resettlement Status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Share of swidden farmers				Share of wet-rice farmers			
Already resettled$_v$	-0.054*	-0.047			0.058*	0.050		
	(0.032)	(0.031)			(0.034)	(0.033)		
Years since resettled$_v$			-0.005	-0.003			0.006	0.005
			(0.004)	(0.004)			(0.004)	(0.004)
Controls		✓		✓		✓		✓
District FE	✓	✓	✓	✓	✓	✓	✓	✓
Ethnic FE	✓	✓	✓	✓	✓	✓	✓	✓
Observations	765	765	765	765	765	765	765	765
R-squared	0.557	0.575	0.556	0.574	0.517	0.528	0.517	0.527
Mean dep. var.	0.554	0.554	0.554	0.554	0.474	0.474	0.474	0.474

Notes: Data taken from the Laos Agriculture Census 2011. The unit of observation is a village, and the sample is restricted to villages belonging to the two types of villages under the policy: already resettled villages as of 2011, and villages to-be-resettled from 2011 onward. 'Years since resettled' is a variable that ranges from 0 to 12 and captures the duration a village has been under the policy. The outcome is share of farm households practicing a type of cultivation: swidden or wet-rice. Village level controls include area, elevation, ruggedness, and pre-policy average swidden plots. Standard errors are two-way clustered at the district-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

swidden cultivation.

6.2 Change in Fallow Duration

To further elucidate the mechanisms driving the observed declines in wealth and welfare, I investigate changes in agricultural intensity by examining fallow duration. In sustainable swidden systems, long fallow periods are essential for soil regeneration and yield maintenance. If resettled households face land scarcity in the valleys but have not successfully transitioned to wet-rice cultivation—as suggested by the results in the previous section—they would be forced to intensify land use through shortened fallow cycles.

To analyze changes in fallow duration, I run equation 5 using two outcomes. First I identify all plots cultivated in 2011. Then I note the next year each of these 2011 plots is re-cultivated. Using the gap between these two years, I can calculate plot-specific fallow durations. The disadvantage of this measure is that my data is censored at 2019, the last year that data is available. This measure ranges from 2 to 8 for those re-cultivated sometime again between 2012 and 2019. For plots likely re-cultivated beyond 2019 I mechanically assign the value 9. Nonetheless, in light of this concern, as an additional measure I create a binary variable that takes value 1 if the plot cultivated in 2011 was not cultivated even up to 2019. I then aggregate these outcomes to the village level to create

village mean values.[11]

Table 7: OLS Estimates: Impact of Resettlement on Fallow Duration

	(1)	(2)	(3)	(4)
	Fallow duration (years)		$\mathbb{I}$(Left fallow beyond 2019)	
Already resettled$_v$	-0.280***		-0.074***	
	(0.100)		(0.025)	
Years since resettled$_v$		-0.027***		-0.007***
		(0.010)		(0.002)
Controls	✓	✓	✓	✓
District FE	✓	✓	✓	✓
Ethnic FE	✓	✓	✓	✓
Observations	742	742	742	742
R-squared	0.485	0.484	0.485	0.482
Mean dep. var.	7.462	7.462	0.541	0.541

Notes: The unit of observation is a village, and the sample is restricted to villages belonging to the two types of villages under the policy: already resettled villages as of 2011, and villages to-be-resettled from 2011 onward. 'Years since resettled' is a variable that ranges from 0 to 12 and captures the duration a village has been under the policy. The outcome in columns 1 and 2 is 'years left to fallow since 2011,' while for columns 3 and 4 it is a binary variable that takes value 1 if a plot was left to fallow beyond 2019. Village level controls include area, elevation, ruggedness, and pre-policy average swidden plots. Standard errors are two-way clustered at the district-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

Table 7 provides direct evidence that resettlement led to a substantial intensification of land use through shortened fallow cycles. Columns 1 and 2 show that, relative to villages yet to be resettled in 2011, already-resettled villages exhibit markedly shorter fallow periods: the average time until a plot is re-cultivated is roughly 0.28 years shorter, and each additional year under the policy further reduces fallow duration by about 0.027 years. These magnitudes are economically meaningful, given a mean fallow duration of 7.46 years.[12] Columns 3 and 4 reinforce this pattern using the binary measure: resettled villages are 7.4 percentage points less likely to keep 2011 plots fallow all the way through 2019, and each additional year since resettlement reduces this probability by another 0.7 percentage points.

These findings suggests the emergence of an interesting paradox: that the policy succeeded in agricultural intensification, but not by the resettled farmers adopting the new method but rather by intensifying land using the old practice. And as is well documented

[11] See Appendix Figure B.10 for an illustration of fallow duration calculation.

[12] The reduction in sample size between Tables 6 and 7 is driven by missing observations for a subset of villages whose shifting-cultivation plots were not detected in the 2011 satellite imagery, resulting in their exclusion from the fallow-duration analysis.

by environmental scientists (Chen et al., 2023), this shortening of the fallow cycle leads to soil degradation and declining productivity, offering a proximate explanation for the substantial drops in household wealth and consumption observed earlier.

6.3 Alternate Mechanisms: Public Services Disruption?

A possible driver of the paper's main findings is that the resettlement caused a disruption to the flow of existing public goods and services. However, there is an argument that, if anything, the relocation would have improved such access. After all, scattered houses on mountain slopes were relocated to more accessible areas.

6.3.1 Road Connectivity

To explore this I first use two data sources: one collected in 2011 that provides point locations of villages, and the second, a shapefile of roads, collected in 2021.[13] Then for each village, I calculate its distance (in kilometers) to the nearest road. Using this as an outcome, I re-estimate equation 5.

Table 8: OLS Estimates: Distance to Nearest Road

	(1)	(2)	(3)	(4)
	Distance to the nearest road			
Already resettled$_v$	-1.607*	-1.505*		
	(0.895)	(0.834)		
Years since resettled$_v$			-0.250***	-0.222***
			(0.081)	(0.078)
Controls		✓		✓
District FE	✓	✓	✓	✓
Ethnic FE	✓	✓	✓	✓
Observations	648	648	648	648
R-squared	0.351	0.383	0.358	0.388
Mean dep. var.	8.865	8.865	8.865	8.865

Notes: The unit of observation is a village, and the sample is restricted to villages belonging to the two types of villages under the policy: already resettled villages as of 2011, and villages to-be-resettled from 2011 onward. 'Years since resettled' is a variable that ranges from 0 to 12 and captures the duration a village has been under the policy. The outcome is a village's distance to the nearest road. Village level controls include area, elevation, ruggedness, and pre-policy average swidden plots. Standard errors are two-way clustered at the district-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

[13]The roads shapefile data (2021) was obtained from the Open Development Mekong website (link).

As columns 1 and 2 of Table 8 show, already-resettled villages on average lie about 1.6 kilometers closer to a road, though these estimates are not statistically significant at conventional levels. The regression controls for village area, average elevation and ruggedness, and pre-policy average swidden plots. At the intensive margin, columns 3 and 4 suggest that every additional year of head-start in a village's resettlement reduces its distance to a road by 0.25 kilometers.

6.3.2 Schooling and Medical Services

The analysis of medical and schooling services further weakens the public-goods-disruption hypothesis by providing evidence that physical access to services did not worsen, and in some cases improved, post-resettlement. Columns 1 and 2 of Table 9 examine the relationship between resettlement and the distance to the nearest medical center (hospital or dispensary). With an average time of 1.43 hours to a medical center, the coefficients are small and statistically insignificant. Specifically, while already-resettled villages are marginally closer by 0.134 hours, this difference is not robust. Similarly, the intensive margin effect, showing a non-significant reduction of 0.008 hours per year of resettlement duration, suggests that being under the policy does not meaningfully change a village's proximity to healthcare facilities.

Table 9: OLS Estimates: Other Public Services

	(1)	(2)	(3)	(4)
	Hours to medical center		$\mathbb{I}$(Primary school in village)	
Already resettled$_v$	-0.177*		0.090*	
	(0.094)		(0.048)	
Years since resettled$_v$		-0.015*		0.024***
		(0.009)		(0.005)
Controls	✓	✓	✓	✓
District FE	✓	✓	✓	✓
Ethnic FE	✓	✓	✓	✓
Observations	765	765	765	765
R-squared	0.191	0.188	0.328	0.349
Mean dep. var.	1.494	1.494	0.478	0.478

Notes: The unit of observation is a village, and the sample is restricted to villages belonging to the two types of villages under the policy: already resettled villages as of 2011, and villages to-be-resettled from 2011 onward. 'Years since resettled' is a variable that ranges from 0 to 12 and captures the duration a village has been under the policy. Village level controls include area, elevation, ruggedness, and pre-policy average swidden plots. Standard errors are two-way clustered at the district-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

Likewise, the relationship between resettlement and primary school availability is positive and statistically robust, as shown in Columns 3 and 4. Resettled villages are significantly more likely to have a primary school located within the village. The coefficient in Column 3 indicates that villages already resettled are 10.2 percentage points more likely to have an on-site primary school compared to the to-be-resettled group. Furthermore, at the intensive margin (Column 4), every additional year a village is resettled is associated with a 2.4 percentage point increase in the probability of having a primary school.

While exploratory, this evidence, combined with the findings on road connectivity, nonetheless weighs against interpretations based on a public-good disruption story.

6.4 Terrain Characteristics as a Barrier to Adoption?

A potential alternative explanation for the policy's failure is that the resettlement sites themselves were agro-ecologically unsuitable for wet-rice cultivation, rather than any cultural resistance from the communities. To rigorously rule this out as the primary explanation for low wet-rice adoption among resettled communities, I employ a propensity score matching methodology that compares resettled swidden villages with geographically comparable traditional wet-rice villages. This approach addresses the critical counterfactual question: would traditional wet-rice societies, if placed in the same locations as resettled villages, demonstrate similar agricultural patterns?

Table 10: Propensity Score Matching

	(1)	(2)	(3)
	Share of wet-rice farmers		
Resettled$_v$	-0.270***	-0.218***	-0.225***
	(0.049)	(0.042)	(0.042)
District FE		✓	
Province FE			✓
Observations	913	896	913
R-squared	0.115	0.521	0.292
Mean dep. var.	0.574	0.574	0.574

Notes: Resettled$_v$ takes value 1 if the village is a resettled swidden village and 0 if it is the nearest neighbor wet-rice matched village. Matching variables are elevation and ruggedness within 500 meters of a village, village area, and distance of village centroid to nearest river/stream. Standard errors are two-way clustered at the district-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

To construct the matching variables, I first generate a 500-meter buffer around each village point and compute mean elevation and terrain ruggedness within that radius,

along with distance from the village centroid to the nearest river or stream. One practical caveat is that only 551 resettled swidden villages in the spatial dataset can be matched to village-level outcome data on the share of wet-rice farmers, which limits the treated sample available for matching. Each of the 551 resettled swidden villages is matched to one nearest neighbor traditional wet-rice village based on the variables above, with a caliper of 0.1 ensuring that matches differ by no more than this amount in their predicted probability of being resettled, and only villages within the common support of propensity scores are retained.

The empirical results provide compelling evidence against the topographic constraints hypothesis. Across multiple specifications, I find large, statistically significant negative coefficients ranging from 21 to 27 percentage points. These substantial adoption gaps persist even when comparing villages with nearly identical topographic profiles, indicating that geographic factors alone cannot explain the limited transition to wet-rice agriculture. This finding reassures us against the alternative explanation that poor topographic suitability might be explaining the low adoption rates observed among resettled communities.

7 Sensitivity Checks

Alternate diff-in-diff estimators: In Figure A.1 of the Appendix, I check the robustness of the staggered diff-in-diff results of the policy on swidden plots by re-estimating equation 2 using the Sun and Abraham (2021) method and find very similar results.

Robustness of economic outcomes to excluding Vientiane Capital (Municipality) province: In tables A1 to A4, I repeat my analysis after dropping villages within Vientiane Capital province since its large urban share meant it had limited intensity of the policy. The results are very robust to this restriction.

Robustness of economic outcomes to excluding districts without a sufficient share of traditionally swidden or wet-rice populations: Tables A5 and A6 test my results' robustness to dropping those in districts with shares of swidden populations less than 10% or above 90%.

Sample restriction for fallow duration calculation: One concern is that villages resettled after 2011 would have cultivated plots in 2011 that were distant from their future locations and likely to be abandoned post-resettlement, mechanically inflating measured fallow durations. Therefore I run an additional check by restricting the sample to the already-resettled villages as of 2011. By focusing on plots cultivated in 2011 by already-resettled villages, I ensure that these plots reflect post-resettlement agricultural practices in the new locations. Table A7 shows the results of this restricted sample and, furthermore, analyzes

by sub-groups based on pre-policy swidden intensity.

Demographic changes in population: Another concern is that resettled villages experienced more out-migration after the policy. While this does reflect the policy's impact on behavior, it nonetheless changes the interpretation of the mechanisms discussed previously. However, as shown in Table A8 shows, the policy does not correlate with population changes. If anything, the coefficients on the 'Already resettled' and 'Years since resettlement' are positive in magnitude, though not statistically significant at the 10% level.

8 Conclusion

This paper evaluates Laos's recent resettlement policy targeting swidden agricultural communities. The results present a sobering assessment: while the policy achieved its immediate, technical goal of reducing the area under swidden cultivation, it fell short of its ultimate objective of improving human welfare and fostering sustainable development.

The empirical analysis reveals that resettlement led to a sharp reduction in swidden plots, but this success came at considerable cost. Educational attainment declined markedly, child mortality rates rose, child health outcomes deteriorated, and household wealth dropped substantially. The policy's implementation also failed to produce meaningful structural transformation in agricultural practices. Households displaced from upland areas did not transition *en masse* towards what the state perceived as the more productive form of cultivation. Instead the evidence points to a reduction in fallow duration as a means of coping in the new and unsuitable environment from the point-of-view of the swidden farmers.

The policy was a classic high-modernist intervention: well-intentioned and technically plausible on paper, yet ultimately disastrous in practice because it disregarded the intricate social, cultural, and practical knowledge embedded within the communities it sought to reform. Swidden cultivation was not merely a backward or inefficient technology to be abandoned, but a sophisticated adaptive system—deeply intertwined with local ecology, kinship structures, and risk management strategies. By overlooking these dimensions, the state's effort to modernize rural livelihoods produced widespread hardship rather than progress.

In this sense, the policy exemplifies a tension between *techne*, the formal, codified knowledge of planners, and *mētis*, the embodied, experiential know-how of cultivators.[14]

[14]The distinction between *techne* and *mētis* originates in classical Greek thought. *Techne* refers to formal, rule-based, and often abstract knowledge, while *mētis* denotes practical wisdom grounded in experience and adaptability. While the former can be codified, the latter is much more context-specific (Coyne et al., 2025).

Development cannot rest on technical design alone. Durable progress depends on engaging with the social and ecological knowledge that communities already possess—the tacit systems of adaptation, cooperation, and learning that anchor rural life. When policy treats these as obstacles rather than assets, it risks undermining the very foundations of resilience it seeks to build.

Nonetheless, this paper does not study outcomes into the longer run, and perhaps states learn from their mistakes and update their policies accordingly, as demonstrated by earlier studies on the importance of state antiquity (Bockstette et al., 2002; Chanda and Putterman, 2007). Future works may explore these longer-term trends.

The contrast is developed most prominently by Scott (1998).

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A Appendix

A.1 Robustness to alternate estimators: Swidden cultivation event study plot using the Sun and Abraham (2021) estimator

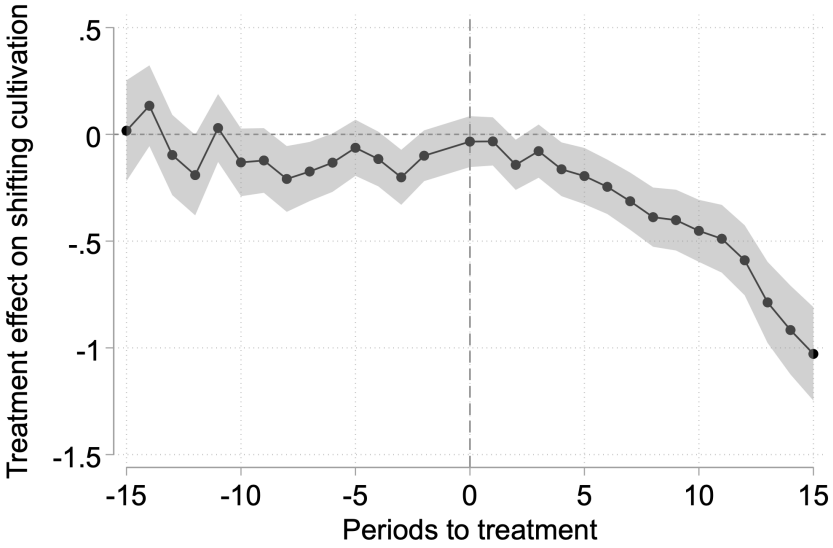


Figure A.1: Event study plot of the logarithm of swidden plots. The estimates are based on equation-2 and uses the Sun and Abraham (2021) estimator.

A.2 Robustness to using $log(0.1 + shifting)$ as the outcome variable: Swidden cultivation event study plot using the Sun and Abraham (2021) estimator

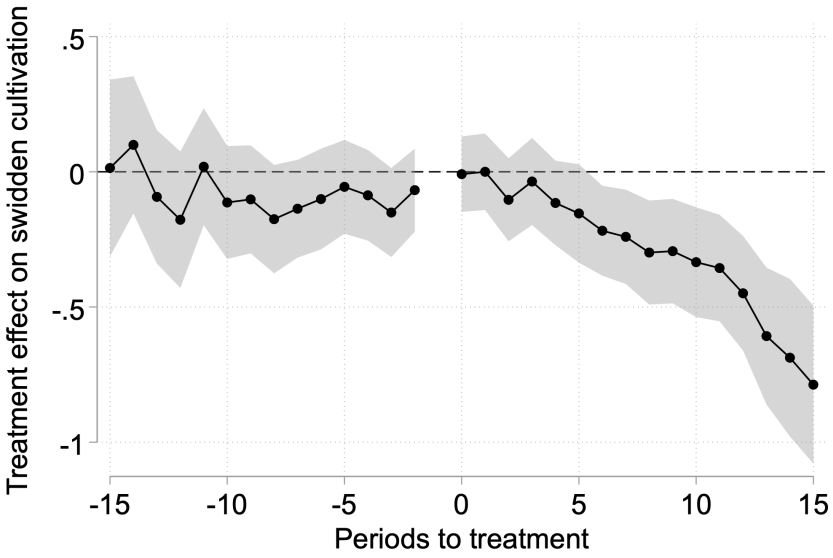


Figure A.2: Event study plot of the logarithm of (0.1+swidden plots). The estimates are based on equation-2 and uses the Sun and Abraham (2021) estimator.

A.3 Robustness: Swidden cultivation event study plot excluding Vientiane capital province

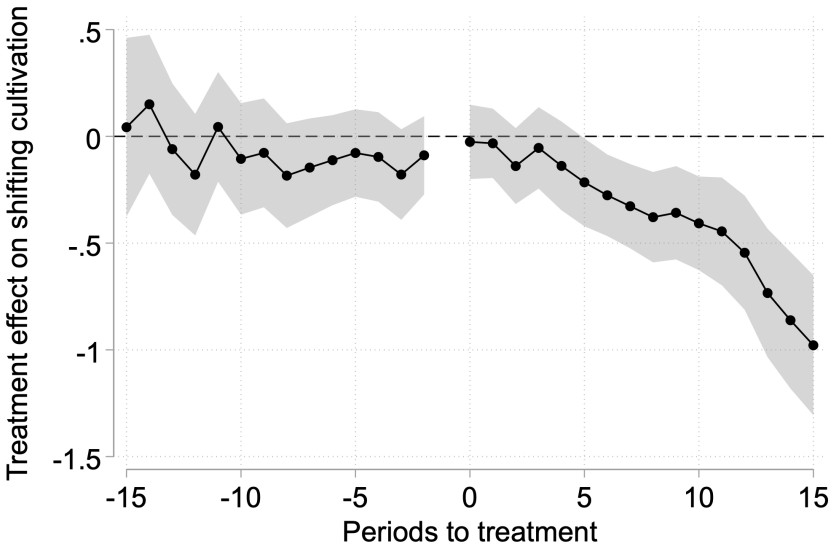


Figure A.3: Event study plot of the logarithm of swidden plots. The estimates are based on equation-2 and uses the Callaway and Sant'Anna (2021) estimator.

A.4 Robustness: Schooling estimates excluding Vientiane capital province

Table A1: Triple Difference Estimates: Schooling

	Primary school	Secondary school	High school	Years of schooling
$\text{Swidden}_e \times \text{Intensity}_d \times \text{Post}_t$	-0.162*	-0.200**	-0.043	-0.858
	(0.085)	(0.078)	(0.050)	(0.649)
Controls	✓	✓	✓	✓
District FE × Census Year FE	✓	✓	✓	✓
District FE × Ethnic FE	✓	✓	✓	✓
Ethnic FE × Census Year FE	✓	✓	✓	✓
Observations	199,967	199,967	199,967	199,967
R-squared	0.328	0.200	0.092	0.370
Mean dep. var.	0.553	0.286	0.0793	4.960

Notes: The sample is restricted to individuals aged 15 to 30 at the time of their respective census (or 0-15 years of age in 2000 for the Post_{2015} census). 'Years of schooling' ranges from 0 to 13 and captures the number of schooling years completed. Post equals 1 if the census year is 2015 and 0 if 1995. Swidden indicates if the individual belongs to an ethnic group traditionally practicing swidden agriculture. Intensity (ranging from 0 to 1) is the share of swidden villages resettled in one's district of residence between 2000 and 2015. Controls include age and gender. Standard errors are two-way clustered at the district-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

A.5 Robustness: Child mortality estimates excluding Vientiane capital province

Table A2: Triple Difference Estimates: Mortality

	(1)	(2)	(3)	(4)	(5)
	Any deaths in household last year	Number of children who have died			
$\text{Swidden}_e \times \text{Intensity}_d \times \text{Post}_t$	-0.020	0.489**	0.326*	0.322*	0.438**
	(0.036)	(0.206)	(0.169)	(0.180)	(0.194)
Mother born after-	Full sample	Full sample	1965	1960	1955
Controls	✓	✓	✓	✓	✓
District FE × Census Year FE	✓	✓	✓	✓	✓
District FE × Ethnic FE	✓	✓	✓	✓	✓
Ethnic FE × Census Year FE	✓	✓	✓	✓	✓
Observations	121,464	113,697	52,671	72,463	91,187
R-squared	0.030	0.186	0.157	0.166	0.179
Mean dep. var.	0.0421	0.449	0.246	0.304	0.371

Notes: The data is a repeated cross section taken from the 1995 and 2015 Laos Census. Column 1 outcome is a household level question and therefore the sample is restricted to the household head. Outcomes of columns 2 and 3 are available only for women aged 15-49. Column 3 is further restricted to women born after 1960, so that for the 1995 census I avoid cohort-specific confounders due to the Laotian Civil War (1962-75). All columns control for age. Post equals 1 if the census year is 2015 and 0 if 1995. Swidden indicates if the individual belongs to an ethnic group traditionally practicing swidden agriculture. Intensity is the share of swidden villages resettled in one's district of residence between 2000 and 2015. Standard errors are two-way clustered at the district-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

A.6 Robustness: Wealth estimates excluding Vientiane capital province

Table A3: Triple Difference Estimates: Wealth

	(1) Wealth score	(2) Wealth index
$\text{Swidden}_e \times \text{Intensity}_p \times \text{Post}_t$	-4.016**	-4.939***
	(1.612)	(1.693)
Controls	✓	✓
Province FE × Year FE	✓	✓
Province FE × Swidden FE	✓	✓
Swidden FE × Year FE	✓	✓
Observations	18,332	18,332
R-squared	0.372	0.358
Mean dep. var.	-0.286	2.540

Notes: The data is a repeated cross section taken from the 2000 and 2011-2012 Multiple Indicator Cluster Survey, and is restricted to household heads. Post equals 1 if the survey year is 2011 and 0 if 2000. Swidden indicates if the individual belongs to an ethnic group traditionally practicing swidden agriculture. Intensity is the share of swidden villages resettled in one's province of residence between 2000 and 2011. Standard errors are two-way clustered at the province-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

A.7 Robustness: Child health outcomes estimates excluding Vientiane capital province

Table A4: Triple Difference Estimates: Child Health

	(1) Height for age (z-score)	(2) Weight for height (z-score)	(3) Weight for age (z-score)
$\text{Swidden}_e \times \text{Intensity}_p \times \text{Post}_t$	-3.354*	-5.974***	-5.280***
	(1.948)	(1.927)	(1.181)
Controls	✓	✓	✓
Province FE × Census Year FE	✓	✓	✓
Province FE × Ethnic FE	✓	✓	✓
Ethnic FE × Census Year FE	✓	✓	✓
Age-group FE	✓	✓	✓
Observations	10,784	10,859	10,938
R-squared	0.159	0.101	0.122
Mean dep. var.	-1.960	-0.522	-1.525

Notes: The sample comprises children aged 6 months to 5 years. The regressions control for gender. Age-group are divided into 5 groups based on child age in months. Post equals 1 if the survey year is 2011-12 and 0 if 2000. Swidden indicates if the individual belongs to an ethnic group traditionally practicing swidden agriculture. Intensity is the share of swidden villages resettled in one's province of residence between 2000 and 2011. Standard errors are two-way clustered at the province-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

A.8 Robustness of schooling outcomes to restricting sample to districts with swidden share of population between 10% and 90%

Table A5: Triple Difference Estimates: Schooling

	Primary school	Secondary school	High school	Years of schooling
$\text{Swidden}_e \times \text{Intensity}_d \times \text{Post}_t$	-0.149*	-0.202**	-0.027	-0.820
	(0.086)	(0.081)	(0.052)	(0.674)
Controls	✓	✓	✓	✓
District FE × Census Year FE	✓	✓	✓	✓
District FE × Ethnic FE	✓	✓	✓	✓
Ethnic FE × Census Year FE	✓	✓	✓	✓
Observations	150,436	150,436	150,436	150,436
R-squared	0.337	0.224	0.103	0.386
Mean dep. var.	0.530	0.277	0.0747	4.779

Notes: The sample is restricted to individuals aged 15 to 30 at the time of their respective census (or 0-15 years of age in 2000 for the Post_{2015} census). 'Years of schooling' ranges from 0 to 13 and captures the number of schooling years completed. Post equals 1 if the census year is 2015 and 0 if 1995. Swidden indicates if the individual belongs to an ethnic group traditionally practicing swidden agriculture. Intensity (ranging from 0 to 1) is the share of swidden villages resettled in one's district of residence between 2000 and 2015. Controls include age and gender. Standard errors are two-way clustered at the district-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

A.9 Robustness of mortality outcomes to restricting sample to districts with swidden share of population between 10% and 90%

Table A6: Triple Difference Estimates: Mortality

	(1)	(2)	(3)	(4)	(5)
	Any deaths in household last year	Number of children who have died			
$\text{Swidden}_e \times \text{Intensity}_d \times \text{Post}_t$	-0.009	0.568***	0.365**	0.396**	0.523***
	(0.037)	(0.209)	(0.176)	(0.182)	(0.198)
Mother born after-	Full sample	Full sample	1965	1960	1955
Controls	✓	✓	✓	✓	✓
District FE × Census Year FE	✓	✓	✓	✓	✓
District FE × Ethnic FE	✓	✓	✓	✓	✓
Ethnic FE × Census Year FE	✓	✓	✓	✓	✓
Observations	91,631	85,899	40,575	55,474	69,435
R-squared	0.029	0.185	0.154	0.165	0.179
Mean dep. var.	0.0437	0.462	0.257	0.316	0.384

Notes: The data is a repeated cross section taken from the 1995 and 2015 Laos Census. Column 1 outcome is a household level question and therefore the sample is restricted to the household head. Outcomes of columns 2 and 3 are available only for women aged 15-49. Column 3 is further restricted to women born after 1960, so that for the 1995 census I avoid cohort-specific confounders due to the Laotian Civil War (1962-75). All columns control for age. Post equals 1 if the census year is 2015 and 0 if 1995. Swidden indicates if the individual belongs to an ethnic group traditionally practicing swidden agriculture. Intensity is the share of swidden villages resettled in one's district of residence between 2000 and 2015. Standard errors are two-way clustered at the district-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

A.10 Robustness: Fallow duration using a restricted sample

Table A7: OLS Estimates: Fallow Duration (Restricted Sample)

	(1)	(2)	(3)	(4)
Panel A: Fallow duration in years				
Years since resettled$_v$	0.013	-0.060***	-0.002	-0.034
	(0.018)	(0.019)	(0.018)	(0.021)
Observations	297	315	272	292
R-squared	0.161	0.095	0.572	0.483
Mean dep. var.	7.447	7.333	7.447	7.333
Panel B: Left to fallow beyond 2019				
Years since resettled$_v$	0.001	-0.014**	-0.002	-0.009*
	(0.005)	(0.006)	(0.004)	(0.005)
Observations	297	315	272	292
R-squared	0.011	0.046	0.533	0.470
Mean dep. var.	0.545	0.501	0.545	0.501
Baseline Swidden Plots $\lessgtr$ Median	Below	Above	Below	Above
Controls	✓	✓	✓	✓
Ethnic FE			✓	✓
District FE			✓	✓

Notes: The unit of observation is a village. The sample is restricted to villages already resettled as of 2011. The outcomes are based on plots cultivated in 2011. **Panel A** uses years left to fallow as its outcome while **Panel B** uses a binary variable that takes value 1 if a plot was left to fallow beyond 2019. 'Years since resettled' ranges from 0 to 12. Controls include total village area. Standard errors are two-way clustered at the district-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

A.11 Robustness: Population changes (2005 to 2015)

Table A8: Robustness: Population changes (2005 to 2015)

	(1)	(2)
	Log population change	
Already resettled$_v$	0.090	
	(0.062)	
Years since resettled$_v$		0.020
		(0.013)
Controls	✓	✓
District FE	✓	✓
Ethnic FE	✓	✓
Observations	400	400
R-squared	0.392	0.393
Mean dep. var.	0.279	0.279

Notes: Data taken from the Laos Agriculture Census 2011. The unit of observation is a village, and the sample is restricted to villages belonging to the two types of villages under the policy: already resettled villages between 2005 and 2011, and villages to-be-resettled from 2011 onward. 'Years since resettled' is a variable that ranges from 0 to 12 and captures the duration a village has been under the policy. The outcome is the difference in the natural log of population between 2005 and 2015. Village level controls include area, elevation, ruggedness, and population in 2005. Standard errors are two-way clustered at the district-ethnicity level. *** p<0.01, ** p<0.05, * p<0.1

B Descriptive statistics

B.1 Borders of villages in Laos

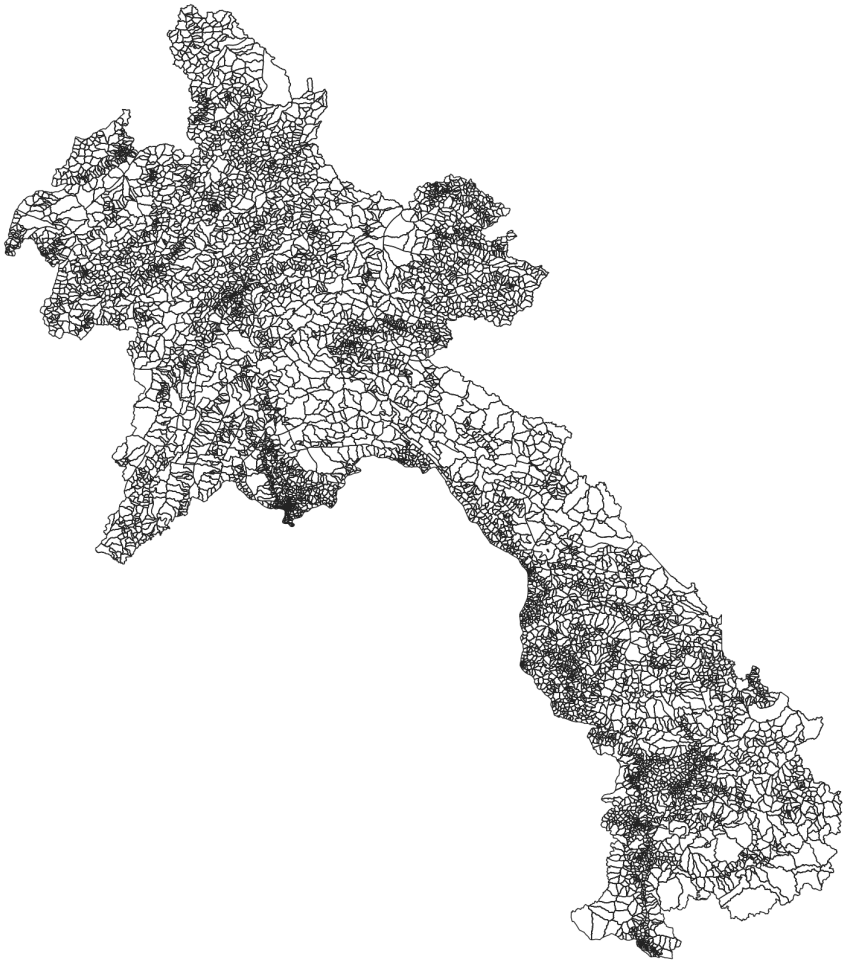


Figure B.1: Polygons of all villages in Laos as of 2011. (Source: Laos Agricultural Census 2011.)

B.2 Histogram of share of swidden villages resettled within a province between 2000 and 2011

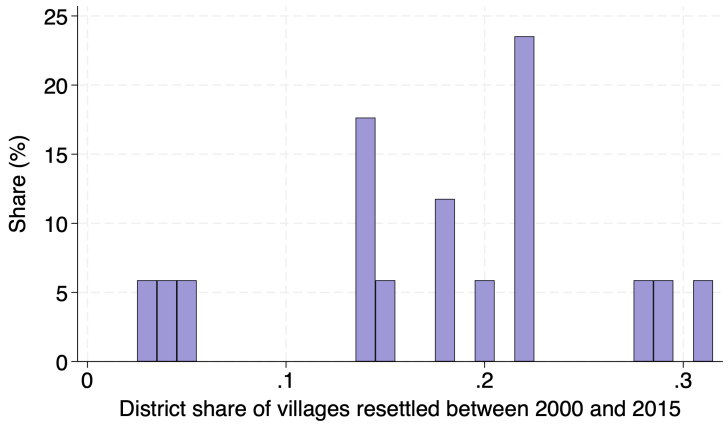
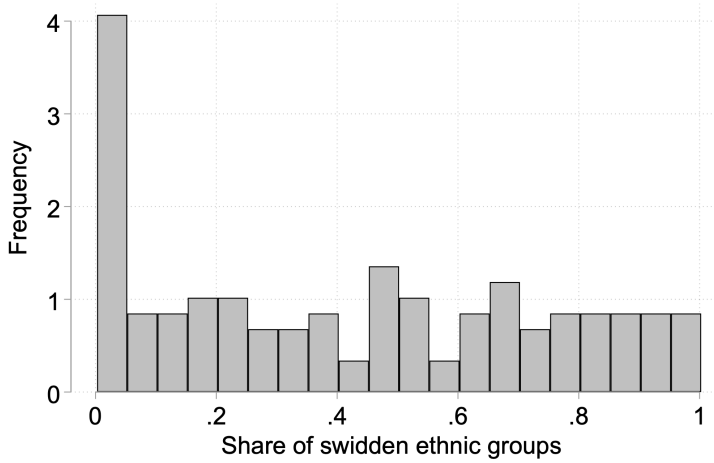
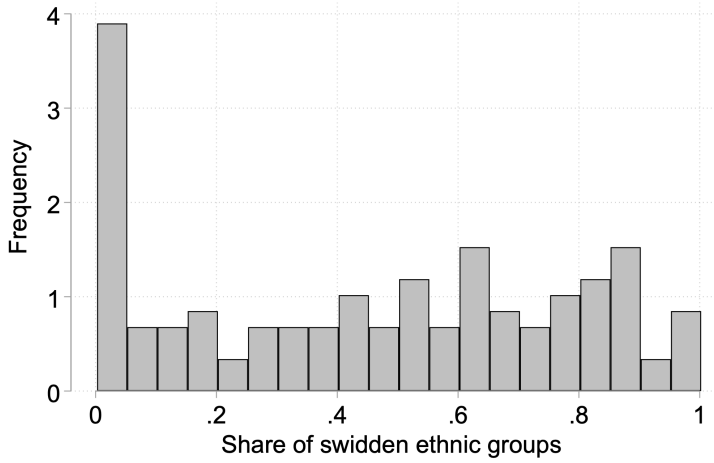


Figure B.2: Histogram of share of swidden villages resettled within a province between 2000 and 2011.

B.3 Histogram of district share of traditional swidden individuals in the census data



(a) 1995 Census



(b) 2015 Census

Figure B.3: Histogram of district share of traditional swidden individuals in the 1995 and 2015 census data. *(Note: Survey weights applied.)*

B.4 Validating author's coding of swidden ethnic groups

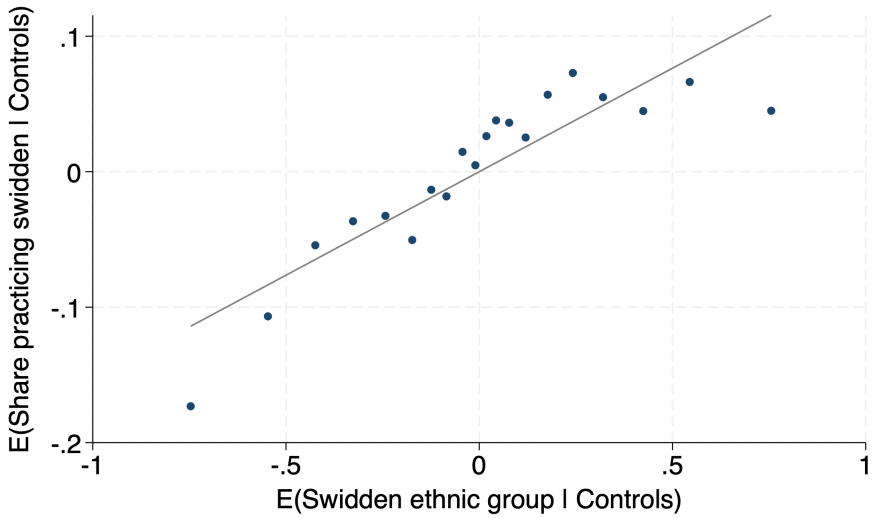
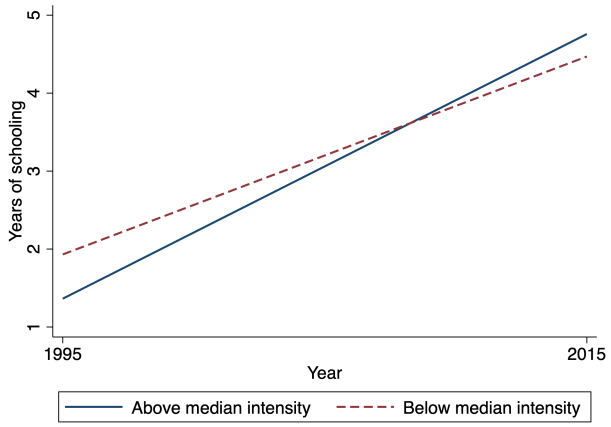
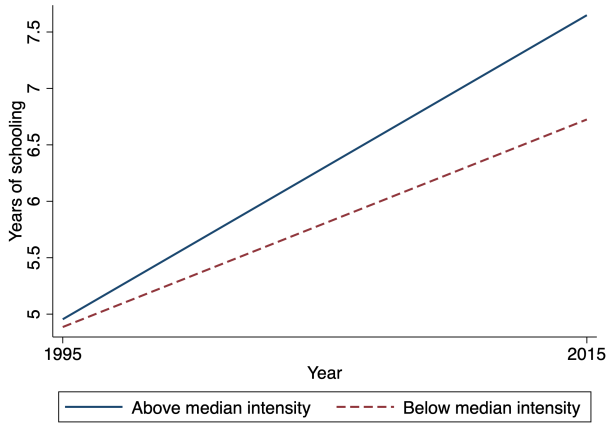


Figure B.4: Residual binned scatter plot of share practicing swidden agriculture against traditional ethnic swidden societies, conditional on controls (village area, elevation, ruggedness, pre-policy average swidden plots).

B.5 Change in schooling by treatment intensity and traditional practice



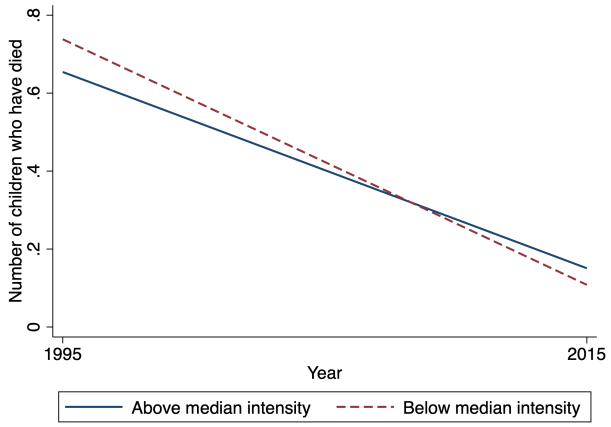
(a) Swidden households



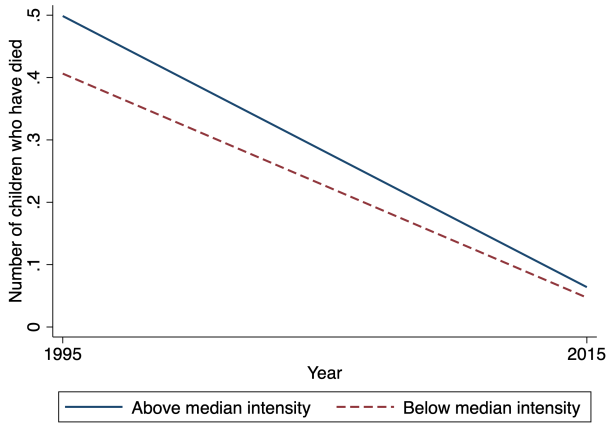
(b) Wet-rice households

Figure B.5: Change in schooling over time (1995 vs 2015) for traditionally (a) swidden households, and (b) wet-rice households. *(Note: Survey weights applied. Sample restricted to rural residents aged 20-30.)*

B.6 Change in mortality by treatment intensity and traditional practice



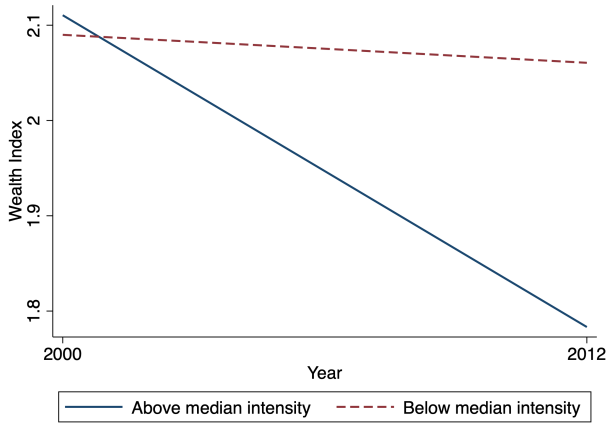
(a) Swidden households



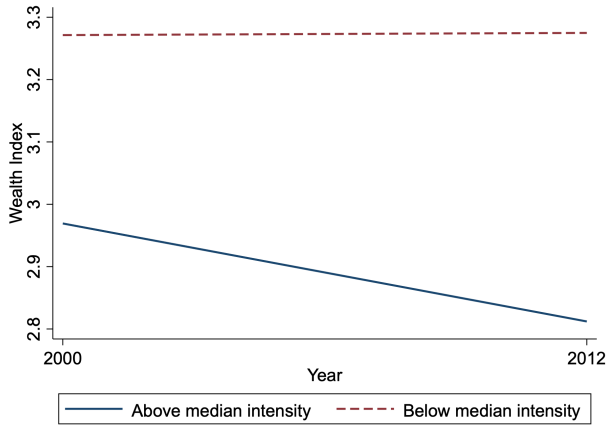
(b) Wet-rice households

Figure B.6: Change in mortality over time (1995 vs 2015) for traditionally (a) swidden households, and (b) wet-rice households. *(Note: Survey weights applied. Sample restricted to rural mothers aged 35 and under.)*

B.7 Change in wealth index by treatment intensity and traditional practice



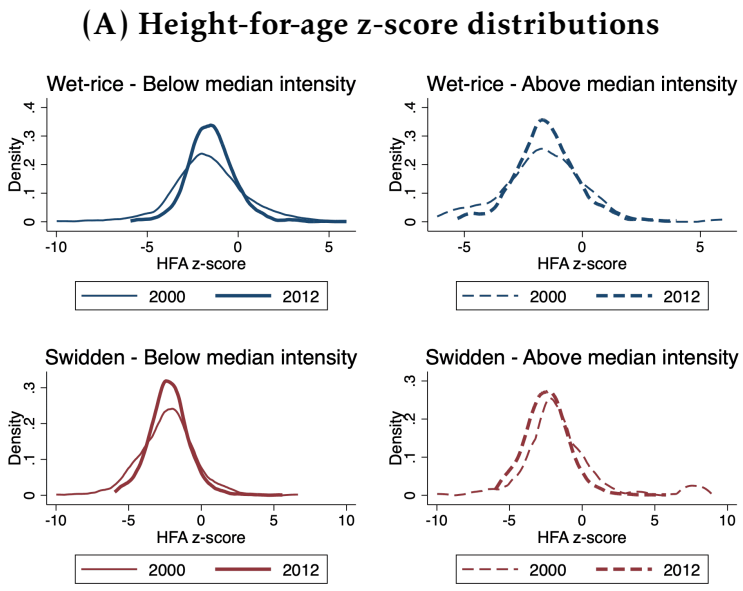
(a) Swidden households



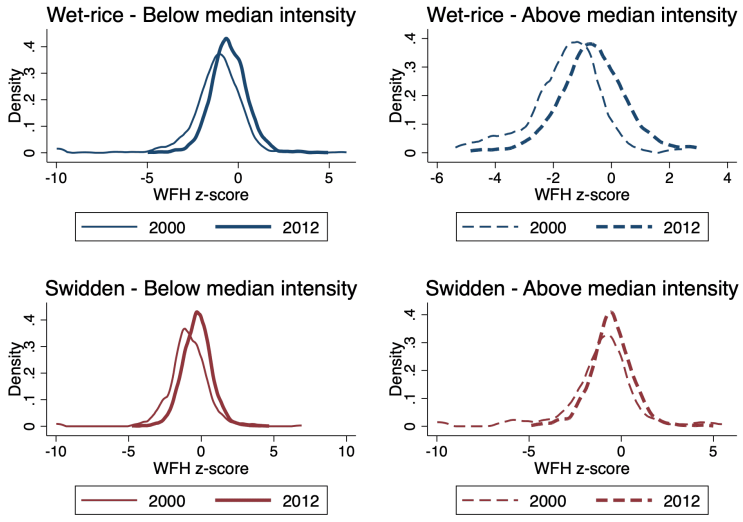
(b) Wet-rice households

Figure B.7: Change in wealth index over time (2000 vs 2012) for traditionally (a) swidden households, and (b) wet-rice households. *(Note: Survey weights applied. Sample restricted to rural household heads.)*

B.8 Change in child health outcomes by treatment intensity and traditional practice



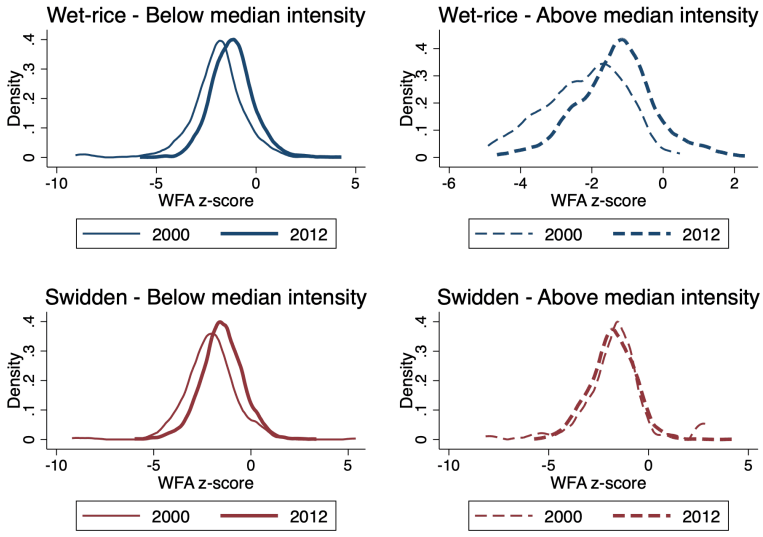
(a) Height-for-age distributions in 2000 vs 2012



(b) Weight-for-height distributions in 2000 vs 2012

Figure B.8: Distribution of child health z-scores in 2000 and 2012 for swidden and wet-rice households. Each panel shows kernel density estimates for four groups: swidden households in above- and below-median settlement intensity areas, and wet-rice households in above- and below-median settlement intensity provinces. Thin lines represent 2000 distributions; thick lines represent 2012 distributions. *(Note: Survey weights applied. Data available only for children 5 years and under.)*

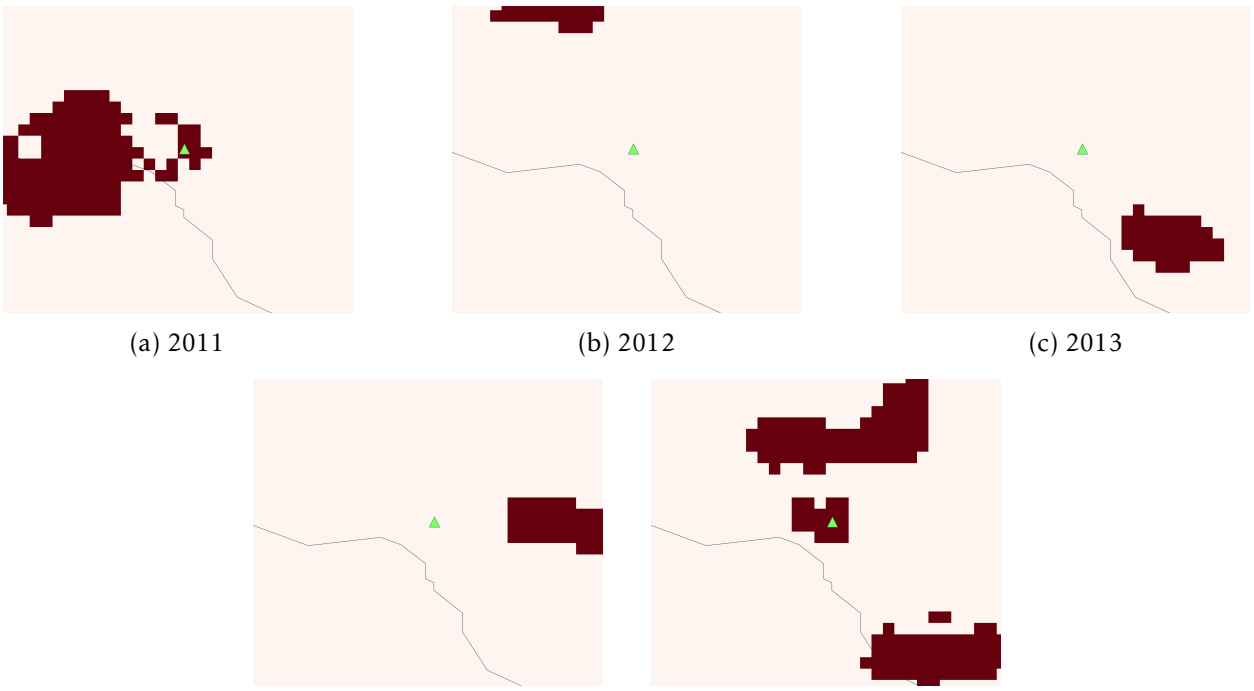
(C) Weight-for-age z-score distributions



(a) Weight-for-age distributions in 2000 vs 2012

Figure B.9: Distribution of child weight-for-age z-scores in 2000 and 2012 for swidden and wet-rice households (continued). This panel shows kernel density estimates for four groups: swidden households in above- and below-median settlement intensity areas, and wet-rice households in above- and below-median settlement intensity provinces. Thin lines represent 2000 distributions; thick lines represent 2012 distributions. *(Note: Survey weights applied. Data available only for children 5 years and under.)*

B.9 Fallow duration calculation

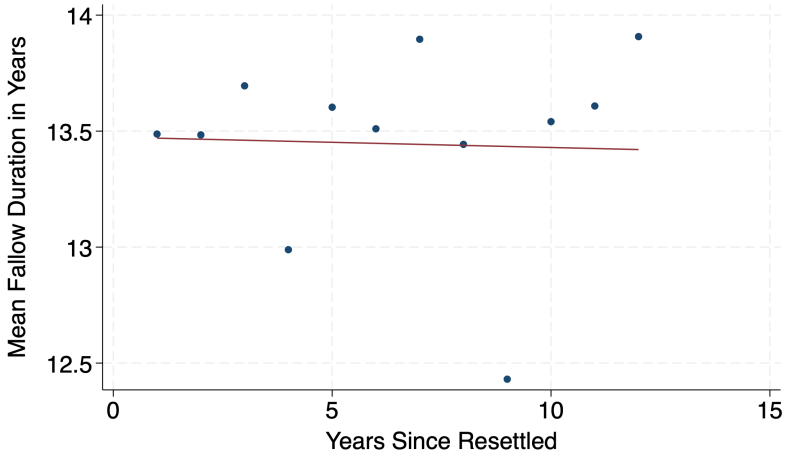


(a) 2011 (b) 2012 (c) 2013

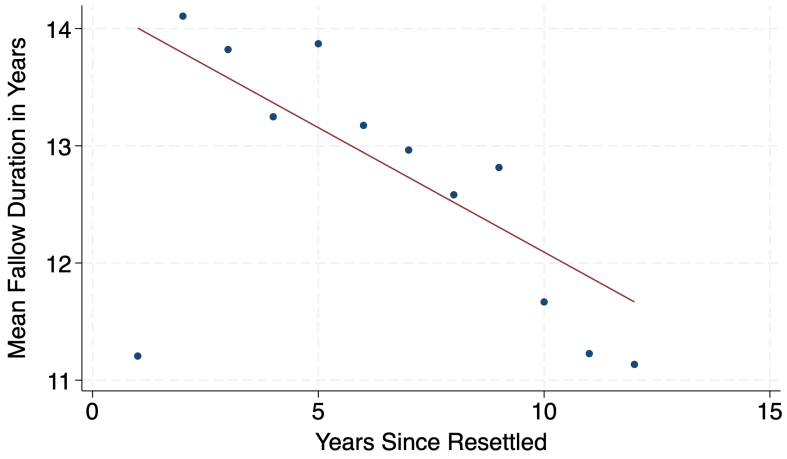
(d) 2014 (e) 2015

Figure B.10: Illustration of fallow duration calculation. Red pixels indicate swidden plots. The pixel indicated by the green triangle, which was cultivated in 2011, was re-cultivated in 2015, and thus gets a fallow duration value of 4.

B.10 Change in fallow duration by baseline fallow plot volume



(a) Below baseline median swidden plots



(b) Above baseline median swidden plots

Figure B.11: Change in fallow duration (2011 to 2019). *Note: Data is censored at 2019.*

B.11 Relationship between swidden and wet-rice shares of farm households

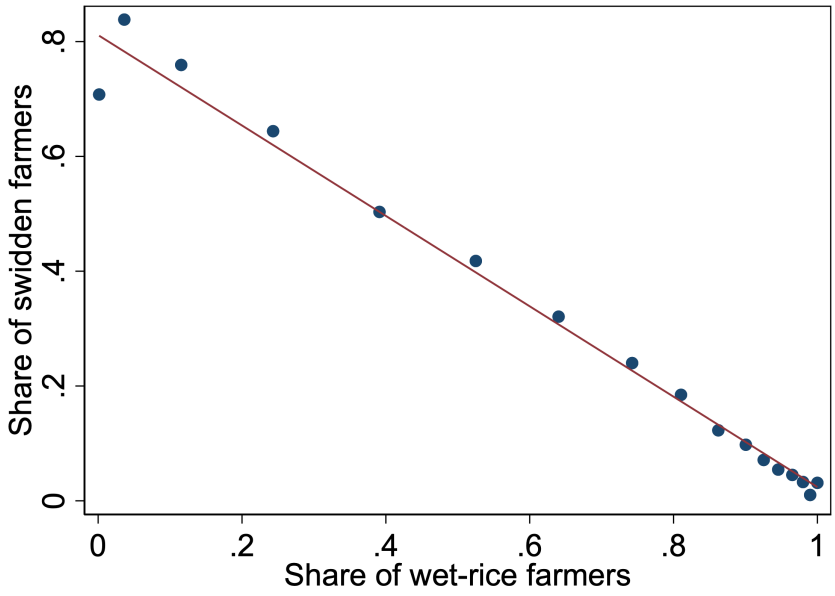
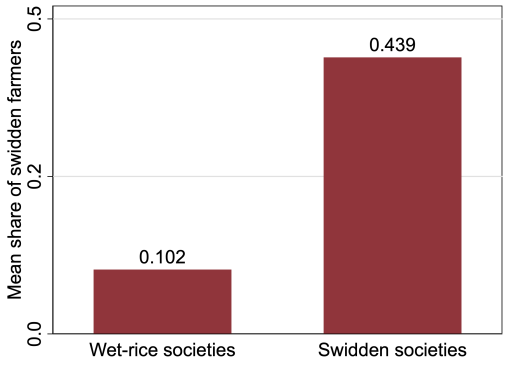
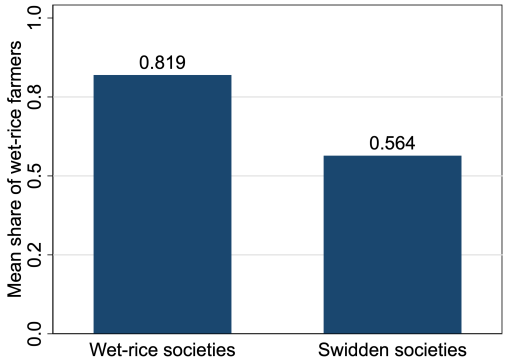


Figure B.12: Binned scatter plot of the village share of two types of practices: swidden versus wet-rice). The higher the share of farm households growing swidden rice, the lower the share growing wet-rice.

B.12 Agricultural practice types based on author's coding of ethnic groups



(a)



(b)

Figure B.13: (a) Share of farm households growing upland (swidden) rice. (b) Share of farm households growing lowland (wet-rice) rice.

C Data construction details

C.1 Sources to code traditional agricultural types

In Laos, the dichotomy between traditional wet-rice and swidden practice is well studied, and broadly falls along ethno-linguistic lines (Stuart-Fox, 1993). Those belonging to the Kra-Dai (or Lao-Tai) families are known to be wet-rice societies while those from the Hmong-Mien, Austro-Asiatic, and Tibeto-Burman families traditionally practice swidden cultivation (Enfield, 2008). Therefore to classify ethnic groups into one of these two agricultural categories I only need to identify to which linguistic groups they belong to. Table C1 provides sources uses for the paper's coding.

Table C1: Ethnic Groups and Agricultural Types in the Laos Census (IPUMS)

Code	Ethnic Group	Language Family	Source
01	Lao	Kra-Dai	(Sidwell and Jenny, 2021, p. 239)
02	Tai	Kra-Dai	(Sidwell and Jenny, 2021, p. 238)
03	Phouthay	Kra-Dai	(Sidwell and Jenny, 2021, p. 239)
04	Lue	Kra-Dai	Phillips and Hanna (2019)
05	Ngoaun	Austro-Asiatic	(Faming, 2019, p. 217)
06	Yang	Kra-Dai	Open Development Cambodia
07	Xaek	Kra-Dai	(Sidwell and Jenny, 2021, p. 227)
08	Thaineau	Kra-Dai	Chantanaroj (2007)
09	Kuemmeu	Austro-Asiatic	(Sidwell and Jenny, 2021, p. 70)
10	Pray	Austro-Asiatic	Malapol (1989)
11	Xingmoun	Austro-Asiatic	Ethnologue
12	Phong	Austro-Asiatic	(Sidwell and Jenny, 2021, p. 191)
13	Thaen	Austro-Asiatic	(Schlemmer, 2017, p. 258)
14	Oedou	Austro-Asiatic	(Schlemmer, 2017, p. 258)
15	Bid	Austro-Asiatic	(Schlemmer, 2017, p. 258)
16	Lamed	Austro-Asiatic	(Schlemmer, 2017, p. 258)
17	Samtao	Austro-Asiatic	(Schlemmer, 2017, p. 258)
18	Katang	Austro-Asiatic	(Schlemmer, 2017, p. 258)
19	Makong	Austro-Asiatic	(Schlemmer, 2017, p. 258)
20	Tri	Austro-Asiatic	(Schlemmer, 2017, p. 258)
21	Yrou	Austro-Asiatic	(Schlemmer, 2017, p. 258)
22	Trieng	Austro-Asiatic	(Schlemmer, 2017, p. 258)

Continued on next page

Table C1 – *Continued from previous page*

Code	Ethnic Group	Language Family	Source
23	Ta-oy	Austro-Asiatic	(Schlemmer, 2017, p. 258)
24	Yae	Austro-Asiatic	(Schlemmer, 2017, p. 258)
25	Brao	Austro-Asiatic	(Schlemmer, 2017, p. 258)
26	Katu	Austro-Asiatic	(Schlemmer, 2017, p. 258)
27	Harak	Austro-Asiatic	(Schlemmer, 2017, p. 258)
28	Oy	Austro-Asiatic	(Schlemmer, 2017, p. 258)
29	Griang	Austro-Asiatic	(Schlemmer, 2017, p. 258)
30	Cheng	Austro-Asiatic	(Schlemmer, 2017, p. 258)
31	Sadang	Austro-Asiatic	(Schlemmer, 2017, p. 258)
32	Xuay	Austro-Asiatic	(Schlemmer, 2017, p. 258)
33	Ngaheun	Austro-Asiatic	(Schlemmer, 2017, p. 258)
34	Lavy	Austro-Asiatic	(Schlemmer, 2017, p. 258)
35	Pacoh	Austro-Asiatic	(Schlemmer, 2017, p. 258)
36	Khmer	Austro-Asiatic	(Schlemmer, 2017, p. 258)
37	Toum	Austro-Asiatic	(Schlemmer, 2017, p. 258)
38	Guan	Not identifiable	–
39	Moy	Austro-Asiatic	(Schlemmer, 2017, p. 258)
40	Kree	Austro-Asiatic	(Schlemmer, 2017, p. 258)
41	Hmong	Hmong-Mien	(Sidwell and Jenny, 2021, p. 139)
42	Ewmien	Not identifiable	–
43	Akha	Tibeto-Burman	(Schlemmer, 2017, p. 258)
44	Pounoy	Tibeto-Burman	(Schlemmer, 2017, p. 258)
45	Lahou	Tibeto-Burman	(Schlemmer, 2017, p. 258)
46	Syla	Tibeto-Burman	(Schlemmer, 2017, p. 258)
47	Hayi	Tibeto-Burman	(Schlemmer, 2017, p. 258)
48	Lolo	Tibeto-Burman	(Schlemmer, 2017, p. 258)
49	Hor	Tibeto-Burman	(Schlemmer, 2017, p. 258)
50	Nhahem	Austro-Asiatic	The Joshua Project
51	Yao	Hmong-Mien	(Sidwell and Jenny, 2021, p. 139)
52	Sida	Austro-Asiatic	Badenoch and Norihiko (2017)
53	Nhuane	Kra-Dai	(Goscha and Ivarsson, 2003, p. 165)
54	Jeng	Austro-Asiatic	The Joshua Project
55	Laven	Austro-Asiatic	Jacq et al. (2001)

Continued on next page

Table C1 – *Continued from previous page*

Code	Ethnic Group	Language Family	Source
56	Kui	Austro-Asiatic	Ethnologue
57	Khir	Tibeto-Burman	Hsiu (2016)
58	Lavae	Austro-Asiatic	Évrard et al. (2016)
59	Ngae	Austro-Asiatic	(Schlemmer, 2017, p. 258)
60	Kor	Austro-Asiatic	The Joshua Project
61	Musir	Not identifiable	–
62	Talieng	Swidden	Ethnologue
63	Other, Lao–Thai	Kra-Dai	
64	Other, Mon–Khmer	Austro-Asiatic	
65	Other, China–Tibet	Tibeto-Burman	
66	Other, not specified	–	
97	No ethnicity	–	
98	Unknown	–	
99	NIU (not in universe)	–	

Notes: Based on the ETHNICLA variable from IPUMS Laos (1995, 2015).

C.2 Ethnicity coding for the MICS data

The two rounds I use from the MICS datasets are the 2000 and 2011/12. For the 2000 round, the data provides detailed ethnic identifiers from the Census categorization. However, for the 2011/12 round, ethnicity is only available at the ethno-linguistic level:

Table C2: Ethnic coding of MICS data

(1) MICS coding	(2) Author's coding
Lao	Kra-Dai
Khmu	Austro-Asiatic
Hmong	Hmong-Mien
Other	Tibeto-Burman

Using the categorization from Section C.1, I first code each ethnic group from the 2000 round into one of the four groups from Table C2. This way I have a way of harmonizing the 2000 round with the 2011/12 round. The ethnic fixed effects of section 5.5.1 are based on this aggregate category.