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Land-use conversion and rural income dynamics in rubber to oil palm transition in Riau Province, Indonesia, using a PSM-DID approach

HEFFI CHRISTYA RAHAYU[1,♥], ETTY PUJI LESTARI[2], AHMAD FATHONI[1], FEBIANA GITYA[3]
[1]Universitas Pasir Pengaraian. Jl. Tuanku Tambusai Kumu, Rambah, Rambah Hilir, Rokan Hulu 28558, Riau, Indonesia.
Tel.: +62-852-6553-0483,[♥]email: heffirahayu@upp.ac.id
[2]Universitas Terbuka. Jl. Cabe Raya, Pondok Cabe, Pamulang, South Tangerang 15437, Banten, Indonesia
[3]Universitas Indonesia. Pondok Cina, Beji, Depok 16424, West Java, Indonesia

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Abstract. *Rahayu HC, Lestari EP, Fathoni A, Gitya F. 2026. Land-use conversion and rural income dynamics in rubber to oil palm transition in Riau Province, Indonesia, using a PSM-DID approach. Asian J Agric 10 (1): g100178. https://doi.org/10.13057/asianjagric/g100178.* The prolonged decline in rubber prices has prompted farmers to shift their land to oil palm cultivation, despite its challenges (e.g., replanting requirements and price volatility). However, there is limited research on smallholder farmers, a vulnerable group that is often overlooked by the law. Therefore, this study attempts to fill the gaps by examining the causal effect of rubber to oil palm land conversion on annual household income among smallholder farmers in Riau Province, Indonesia. We observed and interviewed 206 farmers in Kampar and Rokan Hulu regencies in 2023-2024, and analysed the data using a Propensity Score Matching-Difference-in-Differences (PSM-DID) approach. For the analysis, the study period was divided into two phases: the pre-conversion period (around 2015, before the decline in rubber prices) and the post-conversion period (2021-2022). Propensity score matching was used prior to the DID analysis to ensure that the treatment and control groups were more comparable. The results show that households that converted their land from rubber to oil palm earned significantly higher incomes than comparable households that did not convert. On average, household income increased by approximately 38.5% following conversion. The results indicate that land conversion can bring substantial economic benefits to smallholders. At the same time, policymakers need to consider the environmental costs that may accompany these gains.

Keywords: Difference-in-differences, land-use conversion, oil palm, propensity score matching, rubber

Abbreviations: ATT: Average Treatment Effect on the Treated, CPO: Crude Palm Oil, DID: Difference-in-Differences, FFB: Fresh Fruit Bunches, GRDP: Gross Regional Domestic Product, IDR: Indonesian Rupiah, PSM: Propensity Score Matching, OLS: Ordinary Least Squares, SMD: Standardized Mean Difference

INTRODUCTION

The plantation sector serves as a principal engine of economic growth in developing countries (Retnaningsih et al. 2023). In Indonesia, Riau Province is the largest palm oil-producing region, with oil palm contributing 25.34% to the Gross Regional Domestic Product (GRDP) of the agricultural sector in 2021 (Badan Pusat Statistik (BPS) 2021). Oil palm plantation area expanded from 2.3 million hectares in 2014 to 2.86 million hectares in 2022 (BPS 2023), while rubber plantation area contracted from 460,000 to 320,000 hectares over the same period (BPS 2022). This structural transformation reflects household-level decisions by smallholder farmers responding to changing economic incentives, and constitutes one of the most consequential land-use transitions in the region (Zhang et al. 2023).

Another economically important commodity is rubber. However, the shift from rubber to oil palm can largely be explained by differences in economic returns between the two crops. Between 2015 and 2016, rubber prices in Indonesia fell sharply by nearly 50 percent, from around IDR 10,000 to IDR 5,000 per kilogram. On the other hand, the price of palm oil remained steady over the period. The growing difference in returns between the two commodities encouraged many farmers to convert their rubber land to oil palm. In addition, oil palm trees require less intensive management, reach harvesting maturity within three to four years, and yield fresh fruit bunches on a biweekly cycle (Akbar et al. 2024), compared with the daily tapping labour required by rubber cultivation. This labor efficiency advantage is particularly significant for households managing larger plots (Oosterveer 2015). The advantage of palm oil is not only at the micro level but also at the macroeconomic level. Oil palm expansion stimulates downstream processing, bolsters export revenues, and generates rural employment (Kwatrina et al. 2018; Chen et al. 2020; Hernawan et al. 2021).

Although a growing body of research has examined the relationship between oil palm expansion and rural welfare (Santika et al. 2019), there is still limited causal evidence on how converting rubber land to oil palm affects household income in Riau Province, Indonesia. Identifying the income effect of land conversion is challenging because farmers who choose to convert may differ from those who do not. They often differ in terms of land ownership, access to capital, farming experience, and willingness to take risks, all of which can affect income regardless of whether conversion takes place. As a result, it is difficult to isolate the effect of conversion itself. However, most existing studies do not fully address self-selection, as they rely primarily on cross-sectional comparisons or broad temporal analyses (Chrisendo et al. 2022; Numata et al. 2022). Without a credible counterfactual, that is, what income converting households would have earned had they not converted, income differences between converted and non-converted households cannot be attributed causally to conversion itself. The rapid expansion of oil palm has not occurred without environmental consequences. Forest cover in Riau Province declined by about 4.63 million hectares between 1990 and 2020 (Numata et al. 2022). Previous research has also reported changes in hydrological conditions and biodiversity loss following land conversion (Kwatrina et al. 2018; Fahrig 2019; Powers and Jetz 2019; Johari et al. 2021; Ramlah et al. 2021; Pradana et al. 2024).

This study investigates the causal effect of rubber to oil palm land conversion on annual household income among smallholder farmers in Riau Province, Indonesia. This study uses Propensity Score Matching-Difference-in-Differences (PSM-DID) (Goodman-Bacon 2021) to analyze primary data collected from 206 smallholder households in Kampar and Rokan Hulu Regencies. To our knowledge, it is among the first studies to estimate the causal effect of rubber to oil palm conversion on household income at the micro level in this region. The PSM-DID approach combines matching and difference-in-differences techniques, allowing income changes among converting households to be compared with those of similar non-converting households over time.

The study addresses three research questions: (i) Does conversion from rubber to oil palm significantly increase annual household income among smallholder farmers in Riau? (ii) How large is the causal ATT income effect after controlling for baseline differences through the PSM-DID design? (iii) What are the policy implications for sustainable rural development, considering environmental externalities as reported in the contextual literature? To address these questions, the study makes three main contributions: (i) provision of micro-level causal evidence that addresses the self-selection bias limiting prior cross-sectional studies; (ii) combination of matching and longitudinal differencing to control for both observed covariate imbalance and unobserved time-invariant confounding; and (iii) policy analysis that situates income gains within environmental trade-offs reported in the contextual literature rather than measured in the present study.

MATERIALS AND METHODS

Study area and period

This study takes place in Riau Province, which accounts for around 21.3% of Indonesia's total oil palm plantation area. The data were collected between 2023 and 2024 through structured questionnaires and in-depth interviews with smallholder farmers in Rokan Hulu and Kampar Regencies. These regencies were selected because rubber to oil palm conversion has been widely documented in both areas and because they contain sufficient variation in conversion status among farmers. We selected the villages based on two considerations. The first was the existence of rubber to oil palm conversion since 2015, which we confirmed using agricultural extension records and interviews with local informants. The second was the presence of both converting and non-converting households within the same village or nearby locations, allowing us to compare households facing similar agroecological conditions. Households were included in the survey if they had cultivated rubber or oil palm as their main crop for at least five consecutive years before the survey. Commercial plantations and landless agricultural laborers were not included in the sample.

Data collection

In total, we surveyed 206 farming households with 106 farmers in the treatment group (those who had converted to oil palm) and 100 in the control group (those who had remained with a rubber plantation). Fieldwork was conducted in accordance with established ethical standards for social science research involving human participants. Before the interview, respondents were informed of the study's purpose and provided verbal informed consent. We followed the study protocol approved by the relevant institutional research ethics committee so that no personally identifiable information was recorded.

Annual household income, the primary outcome variable, was defined as net income from all farm and non-farm sources, including plantation crops, livestock, off-farm wage employment, and household transfers. Income was measured in nominal Indonesian Rupiah (IDR) and collected retrospectively for two reference periods. The pre-conversion reference period is fixed at 2015 for all households, corresponding to the year immediately prior to the sharp rubber price decline widely documented as an accelerator of conversion decisions in Riau smallholder communities (Sumarny 2023). Individual conversion years vary across households but cluster in the 2016-2019 window, and 2015 therefore represents a common pre-treatment baseline that predates conversion for all treated units. The post-conversion reference period is fixed at 2021-2022 for all households, by which time converted households had reached stable oil palm yields following the typical three-to-four-year maturation period. Control households were assigned the same reference years, approximately 2015 and 2021-2022, to ensure temporal equivalence. Income data are expressed in nominal IDR and are not deflated, because the DID estimator differences out common time trends, including aggregate price-level changes, under the parallel trend's assumption; both treated and control households are subject to the same macroeconomic price environment in each reference year, so nominal comparisons are internally consistent for identifying the differential income effect of conversion.

The use of retrospective recall over a seven-year period may introduce measurement error. Respondents may not accurately remember their pre-conversion income, and the resulting bias could operate in either direction. If past economic conditions are recalled as being worse than they actually were, the estimated ATT may be overstated. Conversely, if respondents remember their past circumstances more favourably, the estimated effect may be understated. To improve recall accuracy, respondents were asked to relate their answers to a series of easily identifiable local events, including rubber and oil palm harvest seasons, the Eid al-Fitr and Eid al-Adha holidays, and regional administrative elections. These calendar-based reference points were used to help respondents place past events more accurately in time, thereby reducing telescoping and confusion over the reference period. Before fieldwork began, enumerators completed two days of standardised training, and the questionnaire was pilot-tested with 15 households in a non-sample village to improve question wording and the anchoring procedure. Despite these precautions, recall bias remains an inherent limitation of the income measurement strategy and should be considered when interpreting the magnitude of estimated effects. Recall reliability was externally validated against published income benchmarks for rubber-farming households in Kampar Regency (Heriyanto et al. 2017) and internally assessed by comparing responses across subgroups with similar observable characteristics.

Analytical framework: PSM-DID

Voluntary land conversion is not random assignment: households self-select into treatment based on both observable characteristics (land size, capital access, education, farming experience) and unobservable factors (risk preferences, social networks, entrepreneurial ability). To address this, the study employs a PSM-DID estimator that combines propensity score matching, to balance observed pre-conversion covariates between treated and control units, with a difference-in-differences design, to remove time-invariant unobserved heterogeneity, treating conversion as a quasi-natural experiment (Goodman-Bacon 2021).

In the first step, a logit model was estimated to derive the propensity score, defined as the conditional probability of conversion given observed pre-conversion household characteristics: age (years), gender (male = 1, female = 0), education level (six-category scale, 0-6), land size (hectares), farming experience (years), and access to conversion services derived from self-reported survey responses (a three-level ordinal index coded 1-3, where 1 = many institutional barriers and restricted access, and 3 = few institutional barriers and easy access). Although self-reported ordinal measures are inherently more subjective than objective covariates, potential bias was partially addressed through the PSM step, which matches treated and control households on observed characteristics including this index, thereby reducing systematic differences in institutional access between comparison groups.

All covariates were measured or recalled for the pre-conversion period to ensure they are not affected by the conversion itself. Each converting household was then matched to the nearest control household on the propensity score using 1:1 nearest-neighbor matching with replacement, subject to a common support constraint. The 1:1 nearest-neighbor specification was selected over kernel and radius matching because the treated and control pools are of similar size (106 vs. 100), minimizing the risk of poor matches under one-to-one assignment, and because it produces the most transparent and replicable matched sample. Kernel matching was retained as a robustness check given its efficiency advantages in smaller samples. Matching with replacement was employed because the number of treated units (106) slightly exceeds the control pool (100), allowing all treated households to receive a valid match; consequently, some control households are matched to more than one treated unit, and the full analytic sample retains all N = 206 observations. Covariate balance before and after matching is evaluated using Standardized Mean Differences (SMD), with a threshold of 10% indicating adequate balance.

In the second step, the DID estimator was applied to the matched sample using a two-period structure (pre-conversion ≈ 2015; post-conversion ≈ 2021-2022):

$$\ln Y_{it} = \beta_0 + \beta_1 Treated_i + \beta_2 Post_t + \beta_3 (Treated_i \times Post_t) + \gamma X_{it} + \varepsilon_{it} \quad [1]$$

Where, $\ln Y_{it}$ is the natural logarithm of annual household income; $Treated_i$ equals one for converting households; $Post_t$ equals one for the post-conversion period; β_3 captures the Average Treatment Effect on the Treated (ATT) - the net causal income effect of conversion for converting households; and X_{it} is a vector of household-level controls (age, gender, education, land size, farming experience, access to conversion services). Standard errors are clustered at the household level to account for within-unit serial correlation across the two time periods.

Validity of the DID estimator rests on the parallel trends assumption, that in the absence of conversion, treated and control households would have experienced similar income trajectories. This assumption is fundamentally untestable in this study, as only two time periods are available via retrospective recall, precluding pre-trend analysis, event study designs, or placebo tests that would ordinarily provide supporting evidence. The parallel trends assumption therefore represents the most consequential and unverifiable identifying restriction in this study, and all causal interpretations of the ATT are conditional on its validity. It is supported, albeit indirectly, by the near-equality of pre-conversion income means between matched groups and by the statistical insignificance of the main Post and Treated effects in the DID regression. This limitation is revisited explicitly in the Conclusions. Robustness was examined by re-estimating with kernel matching and by varying caliper widths; results are qualitatively unchanged.

RESULTS AND DISCUSSION

Respondents' characteristics

Table 1 presents respondents' characteristics for the full sample. The average respondent is approximately 47 years old with an average education level of 2.8 on the six-category scale (where 0 = no formal schooling and 6 = university). Farms average 3.5 hectares with over 18 years of farming experience. Approximately 51% of the sample constitutes the treated group. Mean log income increased from 14.60 to 14.83 between the pre- and post-conversion reference periods across the full sample; however, this unconditional difference reflects compositional differences that the PSM-DID design is specifically constructed to address.

Determinants of pre-conversion household income

Table 2 presents OLS regression results for pre-conversion income. Education and land size are the strongest predictors: each additional education level is associated with an approximately 20.9% income premium (interpreting 100×0.209 as a semi-elasticity given the log-linear specification; $p<0.01$), and each additional hectare with an approximately 12.8% income gain ($p<0.01$). These findings align with human capital theory and the land-income nexus documented in the smallholder agriculture literature. Crucially, the treatment indicator is positive and significant ($\beta=0.499$, $p<0.01$), confirming that converting farmers already had higher baseline incomes, which validates the necessity of propensity score matching prior to DID estimation to remove this pre-existing difference.

Propensity score estimation and covariate balance

Table 3 presents the logit model for propensity score estimation. Land size ($\beta = -0.169$, $p<0.01$) and access to conversion services ($\beta = -1.627$, $p<0.01$) are the primary drivers of conversion uptake. The negative coefficient on land size indicates that farmers with larger rubber holdings face higher opportunity costs of replanting, and are therefore less likely to convert. Access to conversion services refers specifically to households' access to agricultural credit and financing facilities for conversion, coded 1-3, where higher scores reflect fewer institutional barriers and easier access. The large negative coefficient thus confirms that households with more restricted access (lower scores) are substantially less likely to convert, a direction consistent with both economic reasoning and field observations in Rokan Hulu and Kampar. The model's pseudo R-squared of 0.258 confirms reasonable predictive fit.

Table 1. Descriptive statistics of sample farming households (N = 206)

Variable	Mean	SD	Min	Max
Age	46.57	10.80	20	81
Gender (Male = 1)	0.566	0.496	0	1
Education (level, 0-6)	2.765	1.419	0	6
Land Size (ha)	3.492	3.377	0.5	23
Farming Experience (years)	18.82	9.585	1	50
Access to Conversion (1-3)	2.292	0.914	1	3
Treated (1 = converted)	0.512	0.501	0	1
Income Before (IDR)	4,172,591	7,081,961	600	50,000,000
Log Income (Before)	14.599	1.323	6.40	17.73
Income After (IDR)	4,422,171	4,896,745	800	40,000,000
Log Income (After)	14.828	1.185	6.69	17.50

Note: Education is coded 0-6, representing the highest formal schooling level completed (0: No schooling, 1: Incomplete primary, 2: Primary completed, 3: Junior secondary, 4: Senior secondary, 5: Diploma, 6: University). Access to conversion services is coded 1-3 (1: Many institutional barriers and restricted access, and 3: Few institutional barriers and easy access). Gender is a binary indicator (Male: 1, Female: 0)

Table 2. OLS regression: Determinants of pre-conversion household income

Variable	Coefficient (SE)	p-value
Age	0.0070 (0.0099)	0.481
Gender (Male = 1)	-0.3551* (0.2118)	0.096
Education (level, 0-6)	0.2091*** (0.0614)	0.001
Land Size (ha)	0.1284*** (0.0285)	0.000
Farming Experience (years)	0.0043 (0.0127)	0.732
Access to Conversion (1-3)	0.1932 (0.1736)	0.268
Treated	0.4994*** (0.1742)	0.005
Constant	12.4923*** (0.7289)	0.000
R-squared	0.2203	
N	206	

Note: Dependent variable is log annual household income (pre-conversion period). Coefficients are interpreted as semi-elasticities: a one-unit change in the regressor is associated with an approximate 100×β percent change in household income. Standard errors in parentheses. ***: $p<0.01$, **: $p<0.05$, *: $p<0.10$

Table 3. Propensity score estimation - logit model (dependent variable: treatment status)

Variable	Coefficient (SE)	p-value
Age	0.050* (0.031)	0.100
Gender (Male = 1)	0.486 (0.410)	0.236
Education (level, 0-6)	0.207 (0.152)	0.172
Land Size (ha)	-0.169*** (0.064)	0.008
Farming Experience (years)	0.007 (0.032)	0.830
Access to Conversion (1-3)	-1.627*** (0.403)	0.000
Constant	1.900 (1.551)	0.220
Pseudo R-squared	0.258	
N	206	

Note: Dependent variable is treatment status (1: Converted to oil palm). Access to conversion services is coded 1-3 (1: Few barriers/easy access, 3: Many barriers/restricted access); the negative coefficient indicates that households with easier access (lower score) are more likely to convert, consistent with the expected direction. Standard errors in parentheses. ***: $p<0.01$, **: $p<0.05$, *: $p<0.10$

Table 4 presents covariate balance before and after 1:1 nearest-neighbor matching with replacement. Before matching, access to conversion services showed the largest imbalance (SMD = 86.5%), followed by gender (SMD = 54.3%). After matching, the access-to-conversion SMD was reduced to 3.4%, a 96.1% improvement, and all post-matching p-values exceed 0.20. Two variables, gender (SMD = 21.0%) and farming experience (SMD = -21.2%), remain above the conventional 10% threshold after matching. This residual imbalance indicates that matched treated and control households are not fully comparable on these dimensions, meaning that unobserved differences associated with gender composition and labor experience could partially confound the ATT estimate if they also affect post-conversion income trajectories. To mitigate this, both variables are explicitly retained as controls in the DID regression (equation [1]), which absorbs their direct effects on income and reduces, though cannot fully eliminate, the potential for residual confounding. Readers should interpret the ATT estimate with this caveat in mind. Figure 1 presents the Love plot of SMDs before and after matching, confirming substantial balance improvement across all covariates.

PSM-DID estimates: income effect of land conversion

Table 5 shows that the ATT estimate - captured by the interaction coefficient Post × Treated - is 0.326 (SE = 0.135, p = 0.018), significant at the 5% level. Using the exact exponential transformation - (e°0.326 - 1) × 100 - this implies an annual income gain of approximately 38.5% for converting households relative to comparable non-converting households. The individual main effects of Post and Treated are statistically insignificant, which is consistent with (though not itself proof of) the parallel trends assumption underlying the DID estimator.

Table 4. Covariate balance before and after PSM matching

Variable	Treat. Mean	Control Mean	SMD % (Before→After)	p-val (After)
Age	45.83	44.40	15.6 → 10.3	0.615
Gender (Male=1)	0.63	0.31	54.3 → 21.0†	0.207
Education (level)	2.79	2.67	8.1 → -8.7	0.706
Land Size (ha)	2.77	3.40	-17.7 → 15.6	0.515
Farming Experience	18.58	18.45	1.5 → -21.2†	0.326
Access to Conversion	2.21	2.85	-86.5 → -3.4	0.776

Note: SMD: Standardized Mean Difference (%). Balance is evaluated against an SMD threshold of 10%. †: These variables exceed the 10% threshold post-matching and are retained as regression controls in the DID model to reduce residual bias. p-values refer to t-tests of mean differences in the matched sample

Table 5. PSM-DID estimates: Income effect of land conversion (matched sample)

Variable	Coefficient (SE)	p-value
Post (=1 after conversion)	-0.015 (0.101)	0.885
Treated (=1 if converted)	0.345 (0.251)	0.172
Post × Treated (ATT)	0.326** (0.135)	0.018
Constant	14.575*** (0.215)	0.000
R-squared	0.065	
F-stat	5.854 (p = 0.001)	
N (matched)	206	

Note: Dependent variable is log annual household income. ATT: Average Treatment Effect on the Treated. ***: $p<0.01$, **: $p<0.05$, *: $p<0.10$. The income effect is calculated as (e°0.326 - 1) × 100 ≈ 38.5%

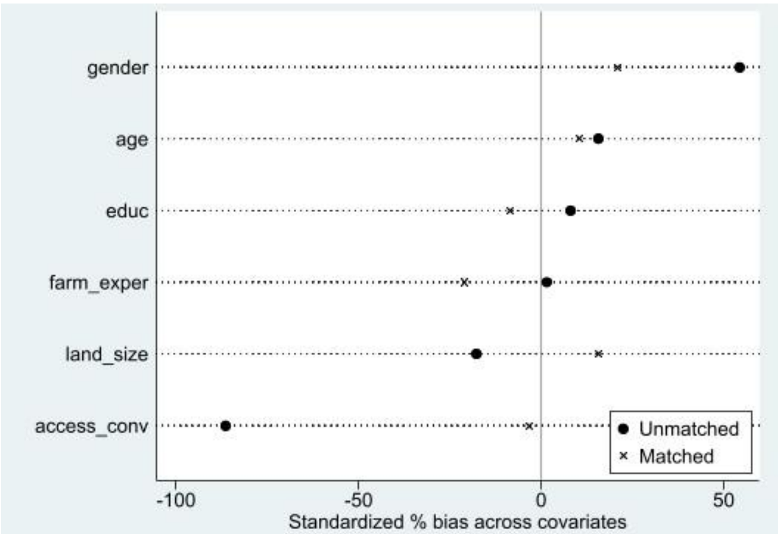

Figure 1. Covariate balance before and after propensity score matching. Standardized Mean Differences (SMD) are shown for each covariate before matching (filled circles) and after 1:1 nearest-neighbor matching (crosses). Variables with post-matching SMD exceeding the 10% threshold (gender, farm_exper) are retained as controls in the DID regression

Discussion

Economic interpretation

The central estimate-a causally identified income gain of approximately 38.5% in nominal terms, corresponding to a real gain of roughly 10-12% once cumulative 2015-2022 consumer price inflation is netted out-is economically substantial yet lies within the range reported by the wider smallholder literature rather than at its extreme. Chrisendo et al. (2022), drawing on nationally representative panel data, find that oil palm adoption raises smallholder living standards and human capital investment, while Krishna and Kubitza (2021) attribute a meaningful share of the gain to the labour released by oil palm's biweekly harvest cycle relative to the daily tapping that rubber cultivation demands. The determinant estimates reported here are consistent with these mechanisms: the negative association between landholding size and conversion uptake (Table 3) indicates that the households most likely to convert are those for whom the labour-saving and price-stability advantages of oil palm are most binding, while the steep education and land-size gradients in baseline income (Table 2) show that the income gains from conversion accrue on top of, rather than as a substitute for, returns to human and physical capital.

Situating the Riau estimate against evidence from other Indonesian contexts clarifies its external validity. In a Kalimantan-wide multidimensional counterfactual assessment, Santika et al. (2019) show that the welfare effect of oil palm development is strongly heterogeneous: it tends to reduce poverty in villages previously oriented toward market agriculture, yet can leave forest- and subsistence-dependent communities no better off, or worse off. The positive treatment effect recovered here is therefore not in tension with that work, because the study villages in Kampar and Rokan Hulu are established market-oriented rubber-farming communities converting an existing cash crop-precisely the setting in which Santika et al. (2019) anticipate net welfare gains. This contrast underscores that the income benefit of conversion is conditional on the pre-existing livelihood structure and should not be extrapolated to frontier or customary-land contexts. The meta-analysis of agricultural land conversion by Zhang et al. (2023) reaches a comparable conclusion, reporting predominantly positive but context-dependent income effects, while Chiarella et al. (2023) caution that the labour-productivity gains that drive results of this kind frequently coincide with declining labour intensity that redistributes, rather than uniformly raises, household welfare.

Two further implications follow for interpretation. First, because institutional access-operationalised here as access to conversion credit and financing-is the dominant predictor of conversion uptake (Table 3) rather than of income conditional on conversion, the binding constraint for households below the conversion threshold appears to be financing rather than the profitability of oil palm itself; this points to credit access, rather than output-price support, as the more effective policy lever. Second, the real-terms magnitude of roughly 10-12% is best read as a lower-bound welfare gain that does not internalise the ecological costs considered in the following section, so the economic case for conversion presented here is deliberately partial.

Consumer prices increased by approximately 26% from 2015 to 2022 (BPS 2023), so that, the estimated 38.5% gain in nominal household income is likely to overestimate the corresponding improvement in real welfare. Deflating income using the BPS consumer price index (2015 = 100) reduces the estimated average treatment effect on the treated (ATT) to approximately 10-12% in real terms. Nevertheless, the effect remains positive and economically meaningful, indicating that the welfare benefits of rubber to oil palm conversion are robust to inflation adjustment.

Environmental trade-offs

While the economic evidence suggests a substantial income benefit from rubber to oil palm conversion, these gains must be interpreted alongside significant environmental externalities documented in the broader literature. These environmental dimensions were not measured in the present household survey and are discussed here as contextual background only, not as empirical findings of this study. Studies indicate that oil palm expansion in Riau Province contributed to a decline of approximately 4.63 million hectares in forest cover between 1990 and 2020 (Numata et al. 2022), with associated habitat fragmentation and threats to endemic species (Gopal et al. 2023). The literature further suggests that substitution of rubber by oil palm monocultures has altered hydrological patterns and increased soil degradation risk (Powers and Jetz 2019; Tiwari et al. 2019). Greenhouse gas emissions from peatland conversion associated with oil palm expansion in Riau represent a significant contributor to Indonesia's national emissions profile (Safitri et al. 2024). These documented trade-offs suggest that income gains may be partially offset by long-term ecological costs, underscoring the relevance of governance frameworks that promote agroforestry, RSPO-aligned certification, and spatially targeted conversion policies. Future research should integrate remote-sensing-derived land cover and peat presence data with household survey evidence to directly quantify plot-level environmental impacts alongside income effects.

In conclusion, this study provides quasi-experimental evidence suggesting that rubber to oil palm conversion in Riau Province is associated with a statistically significant annual household income increase of approximately 38.5%, identified at the 5% significance level through a PSM-DID design. Education and land size are the primary determinants of pre-conversion income, while institutional access to conversion services is the key predictor of conversion uptake. The income gains are consistent with the comparative labor efficiency of oil palm and the relative stability of FFB revenues compared to rubber. Three policy implications follow: (i) programs reducing institutional barriers to conversion can accelerate income gains for households below the conversion threshold; (ii) investments in rural human capital yield income returns independent of land-use decisions; and (iii) given environmental trade-offs documented in the contextual literature, governance frameworks should integrate economic and ecological objectives through agroforestry promotion, RSPO-aligned certification, and spatially targeted conversion policies.

This study is subject to several limitations. First, the retrospective recall design introduces potential recall bias, particularly for income data spanning multiple years; while external and internal validation was conducted, residual measurement error cannot be excluded. The direction of this bias is uncertain: over-reporting of past hardship would inflate the ATT estimate, while telescoping of favorable memories would compress it, and the net effect on the reported 38.5% nominal gain cannot be precisely quantified. Second, the two-period panel structure prevents formal testing of the parallel trends assumption; results should therefore be interpreted as credible conditional estimates rather than point-identified causal effects. Third, the matched sample retains residual SMD above the 10% threshold for gender and farming experience; although these variables are included as regression controls, unobserved heterogeneity correlated with these covariates may still influence estimates. Fourth, environmental outcomes were not directly measured; all environmental claims rely on contextual literature rather than primary data. Fifth, standard errors are clustered at the household level; clustering at a higher geographic level such as the village or district was not possible due to the absence of such identifiers in the dataset, which may understate standard errors if income shocks are correlated within villages. Sixth, robustness checks across alternative matching specifications were not conducted beyond the caliper constraint of 0.01 applied in the baseline specification and represent a direction for future work. Future research should employ objective multi-period panel data, integrate remote sensing evidence on plot-level land cover change, and apply event-study designs with multiple pre-conversion periods to more rigorously test the identifying assumptions of the DID framework.

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