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Distributed Lag Fixed Effects and Non-Additive Response Surface Inference for Intergovernmental Fiscal Transfers: Evidence on Food Security

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Abstract This study examines how intergovernmental fiscal transfers relate to food security when effects may be delayed and instruments may operate non-additively. Using panel data on 435 Indonesian districts/cities in 2018–2023 (N = 2,610), we estimate fixed-effects distributed-lag models for DAK-Irrigation, DAK-Agriculture, and the Village Fund, then fit post-2020 interaction models. To keep interactions interpretable, we evaluate marginal effects over observed transfer combinations and complement the baseline with checks for common shocks, extreme values, and placebo threats. Three findings emerge. First, irrigation associations are timing-sensitive: near-term coefficients are negative, whereas the two-year lag is more favorable. Second, Village Fund associations are most consistent with a one-cycle $(t-1)$ pattern, although they remain sensitive to aggregate time variation. Third, agriculture terms are weak. The post-2020 response surface suggests flattening at high joint transfer intensity, but implied FSI shifts are small, marginal effects remain indistinguishable from zero, and placebo diagnostics do not support a causal interaction block. Because 2020–2023 coincides with the COVID-19 crisis and recovery, any non-additivity may reflect crisis-era coordination stress rather than a normal-state relationship. Overall, the paper offers a framework for evaluating lagged fiscal programs in short panels and interprets estimates as within-district associations rather than causal effects.

Keywords Distributed Lags, Fixed Effects, Marginal Effects, Non-Additivity, Intergovernmental Fiscal Transfers, Food Security

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1. Introduction

1.1. Background

Food security faces overlapping shocks, including COVID-19, commodity price volatility, climate extremes, and geopolitical disruptions. These pressures have renewed concern about the resilience of local food systems [1, 2, 3, 4]. In decentralized settings, intergovernmental fiscal transfers are expected to ease these pressures. They finance irrigation networks, support agricultural production, and strengthen local services that shape food access and nutrition. However, larger transfer envelopes do not automatically produce proportional gains. Effects may arrive late, vary across places, or appear adverse in the short run when implementation and coordination constraints arise.

This study addresses a clear applied-statistics challenge. It examines delayed effects and joint policy effects in panel data while accounting for fixed differences across districts and shocks that affect all districts simultaneously.

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Indonesia provides a useful case for examining this issue. The country is the world's largest archipelagic state and faces marked spatial disparities. Geographic and infrastructure constraints make food distribution and access more difficult, especially for remote and small-island communities [5]. These constraints intersect with poverty, child health burdens, and uneven local economic capacity. Together, they increase exposure to supply disruptions and price changes [6, 7].

Indonesia's decentralization framework uses several place-based transfers to strengthen local development and service delivery. Two streams are especially important for food-system investment. The first is the Special Allocation Fund (Dana Alokasi Khusus/DAK), which includes earmarked allocations for irrigation and agriculture. The second is the Village Fund (Dana Desa), which finances village priorities such as basic infrastructure and community services. These funds move through different administrative channels and planning cycles. In practice, districts can receive several food-related transfers at the same time. Even so, coordination across sectors and levels of government is not automatic. This pattern reflects broader concerns about alignment and governance in food policy [8, 9].

1.2. Related Literature and Remaining Gaps

Much of the empirical literature on fiscal transfers and welfare outcomes reports positive average associations. However, research focused specifically on food security remains limited. Many studies still present the issue in a static and additive way ("more spending $\rightarrow$ better outcomes"). Recent applied-statistics work has also examined food security through statistical learning and clustering. For example, one study uses Multi-Arm Bandits and Partitioning Around Medoids to classify provincial food security in Sumatera, illustrating the value of quantitative methods for food-security assessment [10]. Related work uses spatially varying-coefficient models, such as geographically weighted regression (GWR) and multiscale geographically weighted regression (MS–GWR), to examine geographic differences in nutrition outcomes, including stunting [11]. These findings show that key food-system correlates can vary sharply across space even within a single province.

Most studies still analyze one instrument at a time, which limits the analysis of dynamic adjustment or combined effects across policies [12, 13, 14, 15]. In Indonesia, research on fiscal decentralization and regional development typically emphasizes the direct effects of revenues and transfers or their distributional consequences [16, 17]. Institutional and spatial perspectives offer useful insights into governance quality. Even so, they rarely examine how multiple fiscal instruments work together over time [18, 19]. Taken together, the literature suggests three broad limitations: limited attention to timing, limited attention to how several instruments operate together, and little guidance on how to interpret higher-order interactions when multiple policy inputs change together.

Two methodological gaps motivate this study. The first concerns timing, because many studies do not model it explicitly. Irrigation and agricultural programs pass through procurement, construction, and learning-by-doing phases. As a result, measurable benefits may appear only after projects become operational. At the same time, reallocation toward capital spending and the disruptions that accompany implementation may coincide with short-run declines [20, 21]. Distributed-lag models are therefore increasingly used to identify delayed effects in panel settings [22, 23].

The second gap concerns how instruments work together. The literature has given limited attention to whether sectoral DAK and the Village Fund complement or substitute for each other [18, 19, 24]. It also rarely asks whether scaling several streams at the same time creates coordination problems that reduce effectiveness, as suggested in work on polycentric governance and cross-sector resource coordination [24]. In practice, districts may face coordination overload when they coordinate several earmarked streams with different administrative channels, reporting requirements, procurement timelines, and implementing units. Under these conditions, high joint intensity may reflect not only greater resources but also heavier coordination demands, fragmented planning, and uneven local capacity to use funds effectively.

These gaps point to a broader statistical concern. Several policy inputs can move together because they are correlated. When that happens, purely additive models may mislead. Higher-order interactions are also difficult to interpret without careful reporting of marginal effects over the range of values actually observed in the data. For that reason, this study estimates and explains dynamic, non-linear relationships in a short district panel. It does not rely only on a static average-effect approach.

This motivation is especially important in Indonesia. Districts often manage several funding streams at once, but they do so under unequal administrative capacity and uneven local conditions. These features make Indonesia a substantively important setting and a demanding statistical case. They also help explain why a simple average-effect model may miss the timing and joint-pattern issues that matter for food security.

Against this background, we ask two questions: how do Indonesia's main intergovernmental fiscal transfers, DAK for irrigation, DAK for agriculture, and the Village Fund, associate with district-level food security over time, and do these associations change with the joint policy mix? To answer these questions, we use a short-panel design in applied statistics. We estimate district fixed-effects models with distributed lags to capture timing. We also estimate interaction-response surface specifications to assess possible non-additivity in the policy mix.

Because higher-order interaction coefficients are difficult to interpret on their own, the analysis reports marginal effects over the observed range of transfer intensities rather than relying on raw interaction terms alone. It also reports statistical checks for common shocks, cross-sectional dependence, and extreme values. In addition, the analysis explicitly assesses identification threats through placebo-based tests motivated by the possibility that transfers respond to worsening local conditions. Throughout, we interpret the estimates as within-district associations rather than definitive causal effects. Formal model definitions and inference details are provided in Section 2.

1.3. Preview of Results

The results suggest three main patterns. First, the irrigation estimates are lag-dependent, with weaker near-term coefficients and more favorable longer-lag terms. This pattern is consistent with implementation delays. Second, the Village Fund is most consistent with a near-term, or one-cycle, association, although the magnitude remains sensitive to common time shocks. For that reason, we also report alternative time controls in this short panel.

Third, the policy-mix models suggest possible non-additivity, although the interaction evidence remains limited and must be interpreted cautiously. In the fitted response surface, marginal effects vary with the joint levels of transfers and may flatten at high joint intensity, although these local patterns are estimated with limited precision. We summarize this pattern in two ways: statistically, and in substantive terms through predicted changes in the Food Security Index (FSI) across empirically relevant transfer combinations drawn from the observed distribution.

The interaction models are estimated on the post-2020 window. We therefore assess whether these patterns may be specific to the pandemic and recovery period rather than broader features of intergovernmental coordination. Across specifications, poverty and stunting remain negatively associated with the FSI, while gross regional domestic product (GRDP) shows a positive association.

1.4. Contributions

This paper makes three main contributions. First, it contributes empirically by examining how several fiscal instruments relate to district food security in Indonesia, rather than treating each instrument in isolation, while also speaking to broader debates on how intergovernmental transfers and policy mixes shape food security under decentralized governance. Second, it contributes methodologically by combining distributed lags with interaction-response surface specifications in a short panel and by interpreting these results through marginal effects evaluated over the observed support rather than through raw interaction terms alone. Third, it contributes substantively by showing why the post-2020 context matters for interpretation, especially when subnational governments must coordinate several funding streams under uneven implementation capacity.

More specifically, the analysis provides dynamic estimates and exploratory evidence of possible non-additivity in the policy mix. We use district fixed effects with distributed lags to capture implementation timing $(t, t-1, t-2)$, and we use two-way and three-way interactions to examine potential nonlinearity in the policy mix. Because higher-order interaction terms are not informative on their own, we present marginal effects evaluated over the observed support as well as contrasts that can be interpreted substantively across the observed joint distribution of transfers.

The paper also places the post-2020 evidence in its crisis-era context and explains possible coordination problems through concrete administrative constraints, such as fragmented planning, uneven implementation capacity, and overlapping reporting and implementation demands. Sensitivity checks and statistical diagnostics support these interpretations. Even so, the main conclusions are framed cautiously rather than as automatic evidence of stable causal relationships.

2. Data and Methods

2.1. Conceptual Framework and Hypotheses

We examine food security through four dimensions: availability, access, utilization, and stability [25]. Intergovernmental fiscal transfers can affect these dimensions through sector-specific investment and local service delivery. Effects may appear slowly because programs differ in procurement routines, implementation lags, and coordination requirements [24, 26]. We focus on three transfer streams that are relevant to food security.

1. **DAK for Irrigation** is expected to support food availability and stability. It can improve water-supply reliability and reduce production risk. Irrigation projects usually move through planning, procurement, construction or rehabilitation, and commissioning. Benefits may therefore appear with a delay. Short-run associations may be weak or negative if implementation disrupts production. They may also be weak if funds are concentrated in higher-risk districts [20].
2. **DAK for Agriculture** may affect availability and access. It supports inputs, extension, post-harvest facilities, and related programs. Its effects may depend on complementary infrastructure and local institutional capacity [27, 28].
3. **The Village Fund** may affect access and utilization. It supports village-level basic infrastructure, local services, and community interventions. Effects may appear within a budget cycle when local implementation capacity and participation are strong [29, 30, 31].

We also consider coordination problems at high joint transfer intensity. These problems may reflect administrative capacity and fragmentation. Districts may receive several earmarked and village-level streams at the same time. Local officials may then face overlapping planning calendars, procurement and reporting requirements, sectoral silos, and limited staff capacity. These conditions can make coordination harder across agencies and tiers of government. In that setting, the marginal effect of an additional stream may weaken even when the policy goals are complementary [24, 26].

This framework leads to six hypotheses. We examine these hypotheses through lag structure and policy-mix specifications. H1 (Irrigation timing): DAK for Irrigation improves district food security, with stronger effects after implementation lags. H2 (Agriculture effect): DAK for Agriculture improves district food security. H3 (Village Fund timing): Village Fund allocations improve district food security, with effects plausibly appearing at short to medium lags. H4 (Complementarity): DAK for Irrigation and DAK for Agriculture are complementary. H5 (Cross-program interaction): interactions between sectoral DAK and the Village Fund may be complementary or substitutive, depending on implementation capacity. H6 (Coordination friction): scaling multiple transfer streams at the same time can reduce effectiveness because coordination demands increase, and this possibility is captured with a three-way interaction.

2.2. Data, Study Sample, and Variables

We use a balanced district/city panel of 435 districts/cities observed annually over 2018–2023. The panel covers six years and 2,610 observations. This structure matches the descriptive statistics and the effective sample sizes reported in the Results and Discussion.

The outcome is the district/city Food Security Index (FSI; 0–100), a composite indicator developed by Indonesia's National Food Agency. The indicator summarizes food availability, access, and utilization at the local level. The main policy variables are district/city receipts from DAK for Irrigation (DAK-Irrigation), DAK for Agriculture (DAK-Agriculture), and the Village Fund. We include the Village Fund because part of it is relevant to food security. These fiscal variables are strongly right-skewed, so the baseline specifications use natural logarithms. This transformation reduces the influence of extreme allocations and supports semi-elasticity interpretations.

Some district-year observations are zero in the raw data. We therefore replace zero values with a small positive placeholder before taking logs. In the baseline specification, the placeholder is 0.001 for DAK–Irrigation and 0.0001 for DAK–Agriculture and Village Fund (The Appendix Table A0). The Appendix Table A1 reports the proportion of zero observations for each transfer stream by year. The Appendix Table A2 also re-estimates the post-2020 full interaction model using two alternative placeholders. The first is one-half of the minimum positive observed value in the relevant transfer series. The second is a value one order of magnitude smaller than the baseline placeholder. We keep the log specification as the baseline because its semi-elasticity interpretation is direct and matches the main tables. We also report an Inverse Hyperbolic Sine (IHS) transformation as a robustness check. IHS can handle zero values without requiring an arbitrary additive constant, and it is widely recommended for skewed monetary variables with zeros [32].

The baseline control set includes GRDP, the poverty rate (%), and stunting prevalence (%). These controls capture time-varying local capacity and vulnerability. GRDP is strictly positive in the sample, so it does not require a placeholder adjustment. In Models 1-3, we keep GRDP in levels. This choice preserves the original scale of within-district annual changes in local economic size over the full 2018–2023 window. In the post-2020 specifications, including the baseline post-2020 model and the full interaction model summarized in Table 3, we use log GRDP. This block combines strongly right-skewed fiscal variables with a right-skewed income control, so a common scale is useful. The Appendix Table A8 re-estimates the key specifications using $\ln(\mathrm{GRDP})$ consistently. This check assesses whether the GRDP transformation materially changes the estimates. Poverty reflects purchasing constraints and barriers to food access [33]. Stunting reflects nutritional utilization and chronic deprivation [34, 35].

We draw the data from official sources, including Statistics Indonesia (BPS), the Ministry of Finance, and the National Food Agency. Fiscal transfer variables are recorded in rupiah. Socioeconomic controls come from official district-level statistics. The descriptive tables report monetary variables in million rupiah (Million Rp) for readability. The regressions use the transformations described above.

2.3. Empirical Strategy

We examine whether district food security varies with changes in transfer receipts over time. We also examine whether these associations differ across lags and across the joint distribution of transfers. We begin with a distributed-lag district fixed-effects specification:

$$FSI_{it} = \alpha_i + \sum_{k=0}^{2} \beta_k \ln(\mathrm{DAK\_Irr})_{i,t-k} + \sum_{k=0}^{2} \theta_k \ln(\mathrm{DAK\_Agri})_{i,t-k} + \sum_{k=0}^{2} \eta_k \ln(\mathrm{VF})_{i,t-k} + \gamma' Z_{it} + \varepsilon_{it} \quad (1)$$

Here, i indexes districts/cities and t indexes years. The district fixed effect α_i absorbs time-invariant district differences. DAK_Irr is DAK for irrigation, DAK_Agri is DAK for Agriculture, and VF is Village Fund. Z_{it} contains time-varying controls: GRDP, poverty, and stunting. The distributed-lag structure captures implementation lags and delayed payoffs that are plausible for infrastructure and program spending [22, 23]. We estimate this timing structure step by step in Models 1-3. Model 1 uses t only. Model 2 adds $t-1$. Model 3 emphasizes $t-1$ and $t-2$. The estimation window changes to match the lag structure. We also estimate a two-way fixed-effects specification as a robustness variant. This model helps account for common time shocks, such as pandemic-era disruptions:

$$FSI_{it} = \alpha_i + \lambda_t + \sum_{k=0}^{2} \beta_k \ln(\mathrm{DAK\_Irr})_{i,t-k} + \sum_{k=0}^{2} \theta_k \ln(\mathrm{DAK\_Agri})_{i,t-k} + \sum_{k=0}^{2} \eta_k \ln(\mathrm{VF})_{i,t-k} + \gamma' Z_{it} + \varepsilon_{it} \quad (1a)$$

Here, λ_t denotes year fixed effects.

The effective number of observations after lag placement is 2,610 for Model 1 (2018–2023), 2,175 for Model 2 (2019–2023), and 1,740 for Model 3 (2020–2023). The post-2020 specifications reported in Table 3 and the aligned appendix diagnostics also use 1,740 observations. The tables and notes explicitly report these effective sample sizes. This reporting makes the consequences of lag placement clear. The interaction models are estimated on the 2020–2023 effective window, which coincides with the COVID-19 crisis and recovery period. We therefore treat the post-2020 interaction specification as potentially context-specific. We assess it using year controls and placebo diagnostics. We also interpret its generalizability beyond crisis-era conditions with caution.

We also examine whether transfer streams operate additively or exhibit non-linear, combined patterns. For this purpose, we estimate interaction specifications that describe how FSI changes across the joint space of transfer intensities. The core interaction model is:

$$FSI_{it} = \alpha_i + b'X_{it} + X'_{it}BX_{it} + \kappa[\ln(\text{DAK\_Irr}) \times \ln(\text{DAK\_Agri}) \times \ln(\text{VF})]_{it} + \gamma' Z_{it} + \varepsilon_{it} \tag{2}$$

Here, X_{it} includes the transfer terms. The lag placement follows the timing logic reported in the Results, for example, $\ln(\text{DAK\_Irr})_{t-2}$, $\ln(\text{VF})_{t-1}$, and contemporaneous $\ln(\text{DAK\_Agri})_t$. $X'_{it}BX_{it}$ collects all two-way interaction terms. B is a symmetric matrix of pairwise interaction coefficients. We use the quadratic-form notation only as a compact way to represent the full set of two-way interactions. A two-way fixed-effects variant adds year indicators (λ_t) to absorb nationwide shocks and common time movements. Consistent with the Results, we treat this specification as part of the supplementary robustness diagnostics rather than as one of the two main post-2020 models emphasized in Table 3.

Higher-order interaction coefficients are not directly interpretable on their own. We therefore focus on marginal effects evaluated over the observed support of transfer intensities. For example, the marginal effect of ln(DAK-Agriculture) on FSI in the three-way model is:

$$\frac{\partial FSI_{it}}{\partial \ln(\text{DAK\_Agri})_{it}} = b_A + B_{AI} \ln(\text{DAK\_Irr})_{i,t-2} + B_{AV} \ln(\text{VF})_{i,t-1} + \kappa \ln(\text{DAK\_Irr})_{i,t-2} \cdot \ln(\text{VF})_{i,t-1} \tag{3}$$

We evaluate equation (3) at empirically relevant values. Specifically, we use selected sample quantiles, such as p25, p50, and p75, of ln(DAK–Irrigation) and ln(Village Fund). This choice avoids extrapolation beyond the data's support. We report the resulting marginal effects together with their uncertainty estimates. We also express selected changes in the fitted surface as predicted changes in FSI, measured in index points. We do this for substantively meaningful movements across the observed-support grid. One example is moving from the 25th to the 75th percentile of joint transfer intensity while keeping the evaluation profile within the data's empirical support. This approach expresses any flattening of the fitted surface in real-world units rather than only as changes in local slopes.

With logged fiscal regressors, coefficients and marginal effects can be read as semi-elasticities. A 10% change in a transfer stream corresponds to 0.1 × (marginal effect) change in FSI, holding other included factors constant

2.4. Identification Assumptions and Threats

We treat identification in this design as conditional and associational rather than causal. The key identifying assumption concerns within-district changes in transfers. Conditional on district fixed effects, year fixed effects when included, and the observed time-varying covariates, these changes are assumed to be uncorrelated with time-varying un observables that also affect food security. This assumption is strong, and it may be violated if transfers are targeted to deteriorating local conditions or to other shocks that are not fully captured by the controls. We therefore interpret the estimates as structured within-district associations rather than definitive causal effects. Accordingly, we interpret any policy implications conditionally rather than causally.

The Appendix Table A3 reports a placebo lead test to probe this threat. This test re-estimates the main models using one-year-ahead FSI as the outcome, with transfers at t, $t-1$, and $t-2$ as regressors. The placebo approach is intended to show whether the main lag structure is consistent with simple reverse causality or anticipatory targeting. If contemporaneous transfer changes only proxy unobserved deterioration that mechanically predicts later food security, the placebo specification should also detect that pattern. We therefore treat the placebo results as a robustness check against targeting-related endogeneity rather than as a standalone causal test. Stronger designs, such as instrumental variables or a policy-shock difference-in-differences approach, would require exogenous institutional variation that is not available in the present district-year panel.

2.5. Estimation and Inference

We estimate all models using the within (fixed-effects) estimator. This estimator is equivalent to least squares with district indicators and, where specified, year indicators [36]. Identification comes from within-district variation over time. In the main results tables, inference relies on district-clustered standard errors. This choice allows for arbitrary serial correlation and heteroskedasticity within districts. The time dimension is short (T = 6). Even so, clustering remains a sensible default because precision is driven mainly by the cross-sectional dimension (N = 435), and the asymptotic approximation is usually applied in N. In practical terms, district-level clustering helps avoid overstating precision when shocks persist within districts across years. We therefore treat clustered standard errors as the primary reporting standard. We also interpret statistical significance with appropriate caution under short-panel conditions.

We complement clustered inference with dependence-robust checks (Driscoll–Kraay). These checks serve as a conservative diagnostic because T is small and nationwide shocks can induce cross-sectional co-movement. Driscoll–Kraay standard errors are robust to broad forms of cross-sectional dependence. With very small T, however, they may be conservative, and their finite-sample behavior may be less stable. We therefore use them to assess whether the conclusions are sensitive to dependence assumptions rather than as the primary basis for inference.

Because the panel is short (T = 6), we do not estimate a model with a lagged dependent variable. As a result, classic Nickell bias from dynamic fixed-effects models does not arise directly in the present specification [37]. Even so, short T can still limit precision for distributed-lag regressors and higher-order interactions. We therefore keep the lag structure parsimonious, report effective sample sizes explicitly, and rely on conservative uncertainty reporting and robustness checks.

For interaction specifications, we report statistical evidence mainly through uncertainty around marginal effects evaluated at empirically relevant points, such as quantiles of the interacting variables. In the revised presentation, we report exact p-values and 95% confidence intervals for the interaction coefficients rather than relying only on significance stars. Unless otherwise noted, we use $\alpha = 0.05$ as the conventional threshold for statistical significance. For the marginal-effects figures, we report 95% pointwise confidence bands. We also state explicitly which evaluated marginal effects are statistically distinguishable from zero at that threshold. The analysis involves multiple interaction and marginal-effect tests. We therefore treat these results as partly exploratory and report a Benjamini–Hochberg false discovery rate adjustment [38] in the Appendix Table A5 as a sensitivity check. This approach keeps interpretation aligned with the estimand in Section 2.3 and supports clearer interpretation of higher-order interactions.

The baseline specifications use natural logarithms for the fiscal transfer variables. This transformation reduces the influence of extreme allocations. Some transfer observations are zero in the raw data, so the log specifications use the baseline placeholder noted in Section 2.2 before transformation. The Appendix Table A1 reports the year-specific share of zero observations for each transfer stream. The Appendix also reports the sensitivity of the results to the two alternative placeholder choices defined in Section 2.2. As an additional functional-form robustness check, we re-estimate the main specifications using the inverse hyperbolic sine (IHS) transformation [39]. The dependent variable (FSI) is bounded at 0–100. We use fixed-effects least squares as a transparent baseline for within-unit change. We then use robustness checks to assess whether the conclusions are driven by outliers, transformation choice, or a single variance assumption.

Estimation follows four steps. First, we estimate the baseline and interaction specifications using the fixed-effects transformation, with district indicators and, where relevant, year indicators. Second, we store the estimated coefficient vector and the corresponding variance–covariance matrix under the selected inference choice. District-clustered standard errors serve as the primary standard, and dependence-robust estimates serve as a diagnostic. Third, for interaction models, we compute marginal effects implied by the fitted surface at empirically relevant points, typically sample quantiles of the interacting transfer variables, such as p25, p50, and p75. This keeps evaluation within the observed support. Fourth, we attach uncertainty to these marginal effects using the model-based variance–covariance matrix and delta-method propagation. We then report marginal effects and confidence intervals alongside the corresponding evaluation points.

We rely on within-district variation in this design. Each district acts as its own benchmark as transfer receipts rise or fall over time. This feature helps reduce bias from unobserved, time-invariant district characteristics. A key remaining concern is policy targeting and simultaneity. For example, transfers may increase when local food security deteriorates. To reduce this risk, we (i) use distributed lags to align funding with implementation cycles and to separate contemporaneous funding changes from later outcomes, and (ii) use policy-mix tests to assess whether outcomes vary with combinations of funds rather than with simple additive scaling alone.

2.6. Robustness and Transparency Checks

We estimate non-additive response surfaces under short-panel conditions and run ten robustness and transparency checks. Specifically, we use: (1) two-way fixed effects (district FE + year FE) to absorb nationwide trends and common shocks; (2) dependence-robust uncertainty (Driscoll–Kraay) as a conservative diagnostic for cross-sectional dependence, with lag selection necessarily limited by short T; (3) outlier sensitivity by winsorizing the top 1% of transfer variables; (4) lag sensitivity using alternative lag structures that remain plausible given implementation windows; (5) functional-form sensitivity using the two alternative zero-value placeholders defined in Section 2.2 and the inverse hyperbolic sine (IHS) transformation for fiscal variables [35]; (6) GRDP-specification sensitivity using a common transformation across models as an additional robustness check; (7) a placebo lead test that regresses one-year-ahead FSI on current and lagged transfers to probe reverse-causality and targeting concerns; (8) a permutation-based placebo test for the three-way interaction, implemented by shuffling transfer streams across districts within years and re-estimating the model repeatedly to compare the observed coefficient with its null distribution; (9) multiple-testing sensitivity using a Benjamini–Hochberg false discovery rate adjustment [38] for focal interaction and marginal-effect tests; and (10) interaction transparency by reporting marginal effects over the observed support with 95% pointwise confidence intervals, exact p-values, and selected predicted FSI-point changes over substantively meaningful shifts in joint transfer intensity rather than relying on raw interaction coefficients alone.

3. Results and Discussion

3.1. Identification Assumptions and Interpretive Scope

This section estimates within-district associations over time. The key identifying assumption is straightforward. After accounting for district fixed effects, year fixed effects when included, and the observed time-varying covariates, changes in transfers are uncorrelated with unobserved time-varying factors that also affect district food security. This assumption is strong and may fail if transfers are targeted toward districts whose food security is already deteriorating. We therefore interpret the coefficients as within-district associations rather than causal effects. A stronger causal claim would require exogenous policy variation, such as a credible discontinuity, allocation rule, or instrument, but such variation is not available in the present specification. The Appendix Table A4 therefore reports placebo checks based on a lead outcome and within-year permutation of the transfer streams.

3.2. Descriptive Statistics and Baseline Patterns

Table 1 reports descriptive statistics for the balanced district/city panel for 2018–2023. The panel includes 435 districts/cities observed over six years, for a total of 2,610 observations. The outcome is the Food Security Index (FSI). The main regressors are annual district/city transfers: DAK-Irrigation, DAK-Agriculture, and the Village Fund. GRDP (Produk Domestik Regional Bruto/PDRB), poverty, and stunting serve as the main controls for local economic capacity and vulnerability.

Table 1 shows wide dispersion in FSI. Poverty and stunting also vary strongly across districts. These patterns suggest persistent differences in baseline conditions. We therefore use district fixed effects and focus on within-district changes over time rather than on level comparisons that may mainly reflect time-invariant structural factors. Fiscal transfers and GRDP are also strongly right-skewed, which means that a small number of districts account for very large values. The baseline specification therefore transforms fiscal variables to reduce leverage from extremes

Table 1. Descriptive statistics (balanced panel; pre-log variables)

Variable	Unit	Mean	Median	SD	Min	Max
FSI (Food Security Index)	Index	73.2624	73.4100	5.8490	47.5600	93.6500
DAK–Irrigation	Million Rp	18,019.61	10,462.20	25,046.97	0.00	338,100.00
DAK–Agriculture	Million Rp	12,455.36	8,000.00	13,337.00	0.00	176,000.00
Village Fund	Million Rp	139,765.60	117,396.00	73,637.33	0.00	665,400.00
GRDP (PDRB)	Million Rp	18,891,468.48	8,483,000.00	28,653,900.00	220,000.00	260,200,000.00
Poverty rate	%	10.9445	9.4850	6.7106	1.5100	41.7300
Stunting	%	20.4171	19.9000	8.5205	0.0000	53.3000

Source: Authors' work.
Notes: Descriptive statistics use the original (pre-log) values; regressions use natural logs for fiscal variables in the baseline specification. Monetary figures are scaled for readability. Some observations in all three transfer series are zero in raw form, so small positive placeholders are added before taking logs (0.001 for DAK-Irrigation; 0.0001 for DAK-Agriculture and Village Fund). Appendix Table A1 reports the proportion of zero observations by year. The stunting series includes 0-coded entries for some district-year cells (min = 0.00), which likely reflects non-reporting in those cells; the descriptives therefore summarize the data as coded.

and to improve numerical stability. Because zeros are present in the raw series, we interpret the fiscal terms together with the transformation-sensitivity checks in Appendix Table A2. In those checks, the three-way term remains non-significant across alternative placeholders and the inverse hyperbolic sine transformation, which shows that the interaction results are sensitive to how zeros are handled [32].

The joint variation in poverty, stunting, and FSI is consistent with a broad literature linking food insecurity to socioeconomic vulnerability and child undernutrition, which supports the use of poverty and stunting as core controls [39, 40, 41, 42, 43]. Even so, the food insecurity-stunting relationship is not uniform. A recent cohort-based meta-analysis shows that pooled effects can weaken under high heterogeneity, which suggests that food insecurity is only one contributor among several [44]. Related evidence also highlights sanitation and infection pathways, including environmental enteric dysfunction, that can sustain stunting even when dietary intake improves [45, 46]. Measurement choices also matter because food security indicators differ in sensitivity and level [47], and careful selection and validation of access measures can reduce reliance on a single metric [48].

3.3. Distributed Lag Fixed-Effects Results

We begin with district fixed-effects models that include progressively longer lag structures so the estimates can reflect possible implementation delays. As lags are added, the usable estimation window shrinks. The effective samples are 2,610 observations for Model (1), 2,175 for Model (2), and 1,740 for Model (3). Table 2 reports these three models with district fixed effects and district-clustered standard errors.

Table 2 shows that the transfer-FSI relationship changes over time, although these patterns should still be read as within-district associations rather than causal lag profiles. For irrigation, coefficients are negative at t and $t-1$, then move toward zero or a more favorable sign at $t-2$. This pattern is consistent with delayed implementation effects, but it does not prove them [23]. In the water sector, such delays are plausible because benefits depend on institutions, infrastructure, and operating conditions that may take time to align [20, 24, 49]. For the Village Fund, the contemporaneous term is slightly negative, whereas the $t-1$ term becomes positive once lags are included. This pattern is consistent with village-level implementation cycles [50, 51]. DAK-Agriculture shows a mixed lag profile within this horizon, including a negative $t-2$ term. This pattern is one reason we examine the policy mix rather than rely only on main effects [28]. With logged regressors, coefficients are semi-elasticities. For example, in Model (1), a 10% increase in DAK-Irrigation corresponds to $0.1 \times (-0.0284) \approx -0.0028$ FSI points, holding other included factors constant.

The controls move in expected directions. GRDP is positively associated with FSI, whereas poverty and stunting are negatively associated with it. These patterns align with the broader empirical literature, but they should still be read as conditional associations in a short panel rather than as structural effects [43, 46, 52, 53, 54].

Table 2. Distributed-lag fixed-effects estimates of transfers and FSI (Models 1–3; district FE; district-clustered SE)

Variable	**Model (1) 2018–2023**	**Model (2) 2019–2023**	**Model (3) 2020–2023**
$\ln$(DAK–Irrigation)	-0.0284 (0.0101)	-0.0214 (0.0096)	-0.0019 (0.0109)
$\ln$(DAK–Agriculture)	0.0095 (0.0104)	0.0099 (0.0104)	0.0028 (0.0112)
$\ln$(Village Fund)	-0.0115 (0.0114)	-0.0096 (0.0128)	-0.0161 (0.0129)
$\ln$(DAK–Irrigation) $(t-1)$	–	-0.0213 (0.0107)	-0.0182 (0.0131)
$\ln$(DAK–Agriculture) $(t-1)$	–	-0.0015 (0.0093)	-0.0019 (0.0103)
$\ln$(Village Fund) $(t-1)$	–	0.0166 (0.0224)	0.0094 (0.0237)
$\ln$(DAK–Irrigation) $(t-2)$	–	–	0.0067 (0.0149)
$\ln$(DAK–Agriculture) $(t-2)$	–	–	-0.0312 (0.0152)
$\ln$(Village Fund) $(t-2)$	–	–	-0.0086 (0.0234)
GRDP (PDRB; level)	1.14e-07 (3.14e-08)	1.24e-07 (4.06e-08)	1.49e-07 (3.61e-08)
Poverty rate	-1.4737 (0.1554)	-0.9311 (0.2330)	-0.9062 (0.2372)
Stunting	-0.0233 (0.0053)	-0.0132 (0.0051)	-0.0087 (0.0057)

Source: Authors' work.
Notes: FSI is the dependent variable. All models include district fixed effects, and standard errors are clustered at the district level (in parentheses). Fiscal variables are entered in natural logs. The distributed-lag specification places transfer streams at t, $t-1$, and $t-2$. GRDP (gross regional domestic product; PDRB in Indonesian) is measured at levels, so coefficients are small by construction. Poverty rate and stunting are included as time-varying controls.

3.4. *Non-Additive Response Surface: Interaction Results And Marginal Effects*

We next turn to the post-2020 policy-mix specifications. These models use the 2020–2023 effective window, which yields 1,740 observations. Table 3 focuses on the baseline post-2020 model and the full interaction model. The lag placement follows the main design: irrigation at $t-2$, Village Fund at $t-1$, and agriculture contemporaneous. Additional interaction diagnostics are reported in the Appendix Table A8.

Table 3. Post-2020 Policy-Mix Specifications: Baseline and Full Interaction Model (District FE; District-Clustered SE)

Variable	**Baseline post-2020 model**	**Full interaction model**
$\ln$(DAK-Irrigation) $(t-2)$	0.0818 (0.0367)	0.0818 (0.0367)
$\ln$(DAK-Agriculture)	-0.0019 (0.0405)	-0.0019 (0.0405)
$\ln$(Village Fund) $(t-1)$	0.0570 (0.0343)	0.0570 (0.0343)
$\ln$(GRDP)	3.4591 (4.6627)	3.4591 (4.6627)
Poverty rate	-0.9290 (0.2300)	-0.9290 (0.2300)
Stunting	-0.0022 (0.0078)	-0.0022 (0.0078)
Three-way interaction	–	4.50e-05 (8.63e-05)
Agriculture $\times$ Irrigation	–	-0.001263 (0.001850)
Irrigation $\times$ Village Fund	–	-0.003582 (0.001654)
Agriculture $\times$ Village Fund	–	0.000509 (0.001900)

Source: Authors' work.
Notes: FSI is the dependent variable. All models include district fixed effects, and standard errors are clustered at the district level (in parentheses). Fiscal variables are entered in natural logs. Lag placement follows the timing structure from Table 3: irrigation at $t-2$, Village Fund at $t-1$, and agriculture contemporaneous. GRDP is entered in logs in both post-2020 specifications. Sample size: $N = 1,740$. Additional diagnostics for exact p-values, multiple-testing adjustment, placebo tests, and marginal-effect uncertainty are reported in the Appendix A.

The full interaction model does not provide strong evidence of a three-way interaction. The three-way coefficient is positive but very small (4.50e-05), and it is not statistically distinguishable from zero (p = 0.602). Among the pairwise terms, only the Irrigation × Village Fund interaction falls below 0.05 before correction (p = 0.030), but it does not survive Benjamini–Hochberg adjustment (q = 0.121). We therefore treat the interaction terms as exploratory and read the post-2020 response surface as suggestive rather than as firm evidence of non-additive effects.

We summarize the policy mix through marginal effects evaluated over the observed support of transfer intensities rather than at arbitrary mean profiles. We use a data-supported grid, illustratively p25/p50/p75 of each interacting transfer stream. This choice keeps interpretation anchored to combinations that actually occur in the data. It also avoids extrapolation into sparse regions. The grid makes comparisons across regimes easier. It shows where marginal returns become flatter as other streams scale up. This approach follows current guidance for interaction models [55, 56, 57]. We use coefficients to derive quantities of interest, such as slopes, contrasts, and predicted changes. We then report uncertainty for those quantities rather than for raw parameters alone [58, 59, 60, 61].

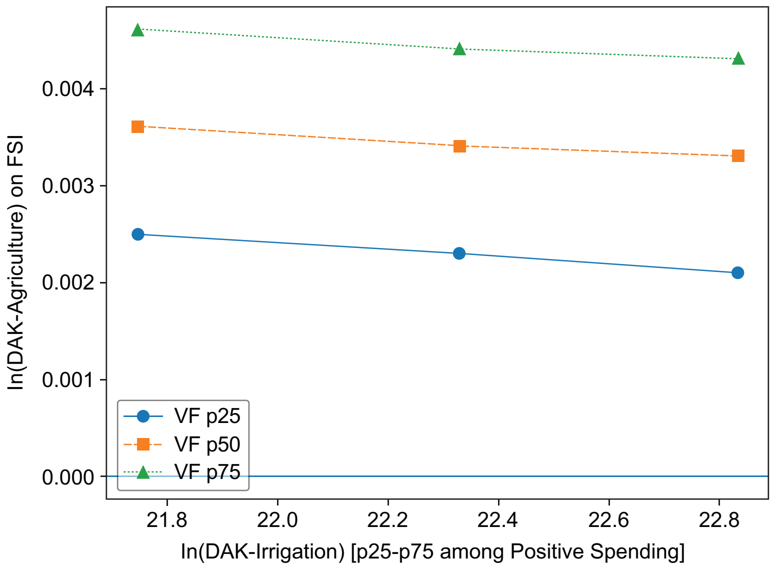

Figure 1. Marginal effect of ln(DAK-Agriculture) on FSI Across ln(DAK-Irrigation), Evaluated at Village Fund p25/p50/p75.

In Figures 1–2, we plot the implied marginal effects from the fitted interaction model. We restrict interpretation to the observed support of transfer intensities, that is, combinations of irrigation, agriculture, and Village Fund levels that actually appear in the sample. The figures show how the fitted slope changes across the policy-mix space covered by the data. The Appendix Table A7 provides 95% pointwise confidence intervals and shows that the marginal effect of ln(DAK–Agriculture) is not statistically distinguishable from zero at any point on the p25/p50/p75 grid. The figures should therefore be read as summaries of an estimated surface rather than as evidence of precise local effects.

The estimated effects are small in magnitude across the observed-support grid. In the synchronized file, the marginal effect of agriculture ranges from about 0.0021 to 0.0046 FSI points across the p25/p50/p75 combinations. A 10% increase in DAK–Agriculture, therefore, corresponds to only about 0.0002 to 0.0005 FSI points across the displayed regimes. All 95% confidence intervals include zero. Consistent with that pattern, moving from lower-joint-intensity to higher-joint-intensity combinations within the p25-to-p75 observed-support grid implies only very small differences in predicted FSI. This reinforces the substantive interpretation that the estimated flattening is modest in index-point terms.

Even though the interaction estimates are imprecise, the fitted surface can still be compared with two strands of recent evidence. One strand suggests that complementarities can arise when agricultural investments are paired with coordinated targeting, complementary inputs, or financing [62, 63, 64]. Micro-level evidence also suggests that irrigation gains can be larger when they are combined with complementary production inputs and support [65]. The other strand points to coordination and absorption limits. Under these conditions, multiple funding streams scaling up simultaneously can reduce returns, especially when administrative capacity is limited, and planning is

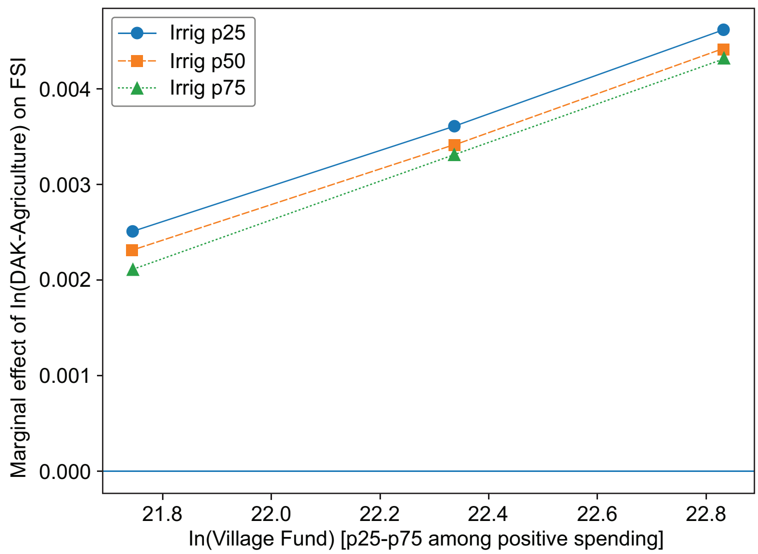

Figure 2. Marginal effect of ln(DAK–Agriculture) on FSI Across ln(Village Fund), Evaluated at Irrigation p25/p50/p75.

fragmented [65, 66]. Regime-switching evidence also suggests that the relationship between public finance and food security can vary across economic contexts [27]. Because these interaction models are estimated only on the 2020–2023 window, they should also be read in light of the pandemic and recovery period. In that period, emergency reallocations, implementation bottlenecks, and common shocks were unusually strong [67]. The fitted surface may therefore reflect crisis-era coordination stress at least in part, rather than a relationship that would necessarily generalize outside that period [55, 57, 58, 59, 68].

3.5. Robustness, dependence, and sensitivity checks

Before turning to mechanisms and implications, we assess how sensitive the main findings are to reasonable changes in specification and inference. This step is especially important in a short panel. Aggregate time variation and cross-sectional dependence can affect both point estimates and precision. Tables 4-5 summarize the main robustness checks. The Appendix A reports additional diagnostics for transformation sensitivity, placebo tests, multiple-testing correction, and marginal-effect uncertainty.

Tables 4-5 distinguish between checks that assess overall specification stability and checks that evaluate the interaction terms directly. The broader robustness exercises suggest that the timing pattern for irrigation is reasonably stable in direction. By contrast, the post-2020 interaction results are much more fragile. The interaction evidence is sensitive to how zeros are handled. It weakens after multiple-testing adjustment. It does not reappear in the lead-outcome placebo. It is also not unusual relative to the within-year permutation distribution.

Taken together, these diagnostics support a cautious interpretation. The post-2020 interaction model is informative as a descriptive summary of possible policy-mix structure, but it does not provide strong evidence of precisely estimated non-additive effects. The figures are therefore best read as summaries of the fitted surface over observed support, not as evidence of robust local interaction effects.

Overall, the Results speak to the six hypotheses set out in the Methods, but with varying degrees of support. The distributed-lag evidence remains broadly consistent with the timing hypotheses for irrigation (H1) and the Village Fund (H3). It also indicates that DAK-Agriculture has weak and sign-varying standalone associations over this short horizon (H2), which motivates a policy-mix reading. In the post-2020 interaction model, the evidence

Table 4. Robustness and Transparency Checks (Overview)

Robustness Check	What It Addresses	Main Takeaway
Add year fixed effects (district FE + year FE)	Nationwide shocks and macro-trends	Timing patterns should be interpreted with caution in a short panel.
Driscoll–Kraay SE (DK)	Cross-sectional dependence and serial correlation	Precision becomes more conservative; the qualitative timing pattern remains similar.
Winsorization (p99)	Outliers and extreme allocations	The interaction block is not driven only by extreme observations.
Alternative placeholders and IHS	Sensitivity to zero handling	The interaction surface is sensitive to transformation choice.
Lead-outcome placebo	Reverse causality and forward-looking spurious correlation	The interaction block is not significant in the placebo specification.
Within-year permutation placebo	Spurious interaction structure	The observed three-way term is not in the extreme tail of the null distribution.
Benjamini–Hochberg correction	Multiple related interaction tests	No interaction term survives multiple-testing adjustment.
Consistent $\ln(\text{GRDP})$ specification	Functional-form consistency across model blocks	A consistent GRDP transformation does not materially strengthen the interaction evidence.

Source: Authors' work.

Table 5. Key Diagnostics for The Post-2020 Interaction Model (2020–2023)

Diagnostic	Key result	Interpretation
Three-way interaction (full model)	4.50e-05, $p = 0.602$	Not statistically distinguishable from zero.
Irrigation $\times$ Village Fund	-0.003582, $p = 0.030$, $q = 0.121$	Below 0.05 before correction, but not after BH adjustment.
Alternative placeholders and IHS	Three-way term remains non-significant across all variants	Interaction structure is sensitive to zero handling.
Lead-outcome placebo	Three-way term $p = 0.928$	No evidence of a significant interaction block in the placebo model.
Permutation placebo	Permutation $p = 0.536$	Observed three-way term is well inside the null distribution.
Marginal effects on p25/p50/p75 grid	All 95% CIs include zero	Local slopes are imprecisely estimated across the observed-support grid.
Consistent $\ln(\text{GRDP})$ specification	Three-way term $p = 0.602$	Functional-form harmonization does not materially change the interaction evidence.

Source: Authors' work.

bearing on complementarity (H4), cross-program interaction (H5), and coordination friction (H6) remains weak once transformation sensitivity, exact p-values, multiple-testing adjustment, placebo diagnostics, and marginal-effect uncertainty are taken into account. These interaction results are therefore best read as exploratory rather than conclusive.

3.6. *Mechanisms: Timing, Complementarities, and Coordination Capacity*

The estimates suggest two broad explanations. The first concerns timing, as the irrigation coefficients are negative in the short run and become more favorable at longer lags. One explanation is delayed adjustment, because benefits may appear only after institutions, infrastructure, and operational readiness are in place. Distributed-lag models are designed to capture this timing profile [23]. Related evidence also shows that irrigation and water-management impacts often depend on complementary institutions and infrastructure and may appear only over multi-year windows [20, 21, 24, 49]. Another explanation is targeting and policy responsiveness, under which earmarked funds are directed toward districts facing weaker baseline conditions or emerging food-security stress [18].

For the Village Fund, the positive one-year lag is consistent with village-level implementation cycles, since planning, procurement, and community execution can shift realized effects into the following year [50, 51]. Reform efforts that emphasize networks and coordination also suggest that local institutional arrangements can influence how policy improvements translate into nutrition-related outcomes [69]. For DAK–Agriculture, the muted standalone coefficients within the observed horizon fit the view that agricultural spending often does more when it is paired with complementary infrastructure and institutions, especially when constraints bind. These terms are therefore more informative within a policy-mix reading than as main effects alone [28, 64].

The interaction results are consistent with two competing interpretations. Coordinated policy bundles may generate complementary impacts when targeting and implementation are aligned [62, 63]. At the same time, scaling multiple streams simultaneously may run into absorption and coordination limits. A likely administrative explanation is that districts must coordinate earmarked sectoral transfers with village-level implementation, procurement calendars, reporting requirements, and sector-specific oversight. In that setting, additional funds do not automatically translate into greater implementation capacity. Existing studies on Village Fund governance and food-policy coordination point to fragmentation, uneven local capacity, and underinvestment in coordination functions as practical constraints. Polycentric water-governance work also shows that alignment problems can persist even when resources increase [50, 69, 70]. The COVID-19 period likely intensified these strains by forcing reallocations and faster budget adjustments under crisis conditions [67]. Taken together, Sections 3.2–3.4 suggest that timing and coordination are likely to matter, but the current interaction estimates are too imprecise to identify robust synergies or coordination frictions with confidence.

3.7. *Implications for evaluation and program design*

Three practical implications follow from these patterns, although causal interpretation remains limited. If these associations reflect policy effects, three implications are especially relevant. First, irrigation investments may be better judged on multi-year horizons. Performance windows should be aligned to implementation and operationalization rather than annual disbursement alone. This reading fits the estimated timing profile and the broader evidence that irrigation impacts depend strongly on institutions and infrastructure, so short evaluation windows can miss benefits that arrive later [20, 24, 49].

Second, the Village Fund results are best interpreted against the backdrop of aggregate time variation, and any policy reading should remain conditional on the associational nature of the estimates. In a short panel, the fact that some short-run terms change once time controls are added is a reminder that nationwide shocks can shape inference. Evaluation designs should therefore account for year effects. They should also use dependence-aware inference as a diagnostic when districts may co-move through spillovers or shared shocks [71, 72, 73, 74]. This caution sits alongside the programmatic reality that Village Fund impacts can reflect village-level implementation cycles and administrative processes and can vary with local governance and complementary sector performance [18, 50, 51, 69].

Third, the response surface is consistent with the importance of sequencing and alignment, although the interaction evidence remains imprecise and should be read conditionally. Complementarities are plausible when targeting and implementation are coordinated [65]. However, simultaneous scale-up can also run into absorption and coordination constraints that reduce returns at high joint intensity [68, 72, 75]. Because the interaction models are estimated on the 2020–2023 crisis and recovery window, this pattern should be interpreted cautiously. It may reflect crisis-era coordination stress as much as a broader structural relationship. For that reason, integrated planning and staggered rollout are more appropriately evaluated as policy packages using marginal effects over observed support, rather than additive main-effect logic, with uncertainty reported for the resulting quantities of interest [56, 58, 61].

3.8. Limitations

This study has several limitations. The time dimension is short (T = 6), which limits precision, shortens the lag horizon, and reduces power for higher-order interactions. Model (2) uses 2,175 observations, and Models (3)–(7) use 1,740. These samples are adequate for estimation, but they do not remove the difficulty of learning dynamic patterns from a short panel. More generally, finite-T panel settings require caution because dynamic patterns can be sensitive to persistence and specification choices, even when fixed effects are appropriate for time-invariant heterogeneity [22, 36].

More importantly, district fixed effects do not remove time-varying confounding, policy targeting, or simultaneity. Transfers may respond to worsening food-security conditions, which would bias within-district associations upward or downward depending on the targeting rule [18, 67]. We therefore do not claim causal effects. The present study also does not implement an instrumental-variables design or a difference-in-differences design because a credible source of exogenous policy variation is not available in the current specification.

A simulation-based assessment of estimator performance under alternative persistence structures would also be a useful extension in a short-panel setting. We do not implement that exercise here because its conclusions would depend on additional assumptions about regressor persistence, shock structure, and targeting dynamics that are not directly identified in the present application. Instead, we take a more conservative approach by reporting effective sample sizes after lag placement, adding placebo-based diagnostics, and interpreting the estimates as within-district associations rather than causal effects.

Finally, the results are sensitive not only to time controls and dependence assumptions, but also to transformation choices when zeros are present in the fiscal series. For that reason, the manuscript reports year-specific zero shares, the exact placeholder values used before log transformation, sensitivity to alternative placeholders, and an inverse hyperbolic sine robustness check [32]. Taken together, these diagnostics support a cautious reading of the interaction results. The paper therefore identifies structured within-district associations and a suggestive response surface rather than definitive causal magnitudes.

4. Conclusion

This study examines how intergovernmental fiscal transfers are associated with district food security in Indonesia by using district fixed-effects distributed-lag models and post-2020 interaction models. The results show a lag-dependent pattern for DAK–Irrigation, with less favorable near-term estimates and a more favorable two-period lag estimate. The Village Fund shows its most favorable association at the one-period lag, whereas DAK–Agriculture remains weak and changes sign across specifications. The placebo checks also support a cautious reading, as the lead-outcome and within-year permutation tests do not support a strong claim that the post-2020 interaction block captures a stable causal relationship.

The interaction results also require caution. The fitted results suggest flatter marginal associations at high joint transfer intensity, but the three-way interaction is imprecisely estimated and becomes weaker across robustness checks, including multiple-testing and placebo diagnostics. In substantive terms, the predicted FSI changes across the observed-support grid are small, and the estimated marginal effects are not statistically distinguishable from zero on the evaluated p25/p50/p75 combinations. Taken together, these results support a cautious reading of within-district associations rather than a strong claim of causal or clearly non-additive effects. The policy-mix pattern is therefore best treated as exploratory.

The post-2020 interaction results should also be read in light of the COVID-19 crisis and recovery period, when common shocks, emergency reallocations, and implementation bottlenecks were unusually strong. Any flattening at high joint transfer intensity may therefore reflect crisis-era coordination pressure rather than a normal policy relationship. One practical explanation is that districts receiving several transfer streams at the same time must manage overlapping planning calendars, reporting requirements, procurement routines, and uneven implementation capacity. Under these conditions, additional funds may not produce equally large gains even when the policy goals are complementary.

Overall, the findings suggest that the food-security associations of fiscal transfers vary with timing, implementation cycles, and the way transfer streams are combined. If these associations reflect underlying program effects, their implications depend on timing, implementation cycles, and the way transfer streams are combined. The present panel does not provide credible exogenous variation for an instrumental-variables or difference-in-differences design, so the estimates should be interpreted as conditional within-district associations rather than causal effects. This study provides an empirical approach for examining lagged and potentially non-additive transfer effects in short panels, while also pointing to the need for longer panels, stronger identification strategies, and further evidence on whether the post-2020 coordination pattern persists beyond the crisis period.

A. Appendix

Structural Zeros and Log-Transformation Handling of Fiscal Transfer Variables

Table A0. Zero observations by year and exact placeholder values

Transfer stream	Stored placeholder used before log
DAK–Irrigation	0.001
DAK–Agriculture	0.0001
Village Fund	0.0001

Values below are treated as structural zeros in the raw transfer series.

Table A1. Zero observations by year and transfer stream

Year	DAK–Irrigation: zero n (%)	DAK–Agriculture: zero n (%)	Village Fund: zero n (%)
2018	42 (9.7%)	17 (3.9%)	47 (10.8%)
2019	69 (15.9%)	12 (2.8%)	27 (6.2%)
2020	139 (32.0%)	76 (17.5%)	39 (9.0%)
2021	94 (21.6%)	123 (28.3%)	18 (4.1%)
2022	223 (51.3%)	166 (38.2%)	18 (4.1%)
2023	298 (68.5%)	258 (59.3%)	157 (36.1%)

The zero shares are substantial and increase sharply in later years, especially for DAK–Irrigation and DAK–Agriculture. This confirms that zero handling is a substantive modeling issue rather than a negligible preprocessing detail.

Sensitivity to alternative placeholders and the inverse hyperbolic sine transformation

Table A2. Transformation sensitivity for the post-2020 full interaction model (Model-7 family)

Term	Baseline coef (p)	Alternative 1 coef (p)	Alternative 2 coef (p)	IHS coef (p)
Three-way interaction	4.50e-05 (0.602)	0.002509 (0.448)	0.001353 (0.473)	0.000117 (0.564)
Agriculture × Irrigation	−0.001263 (0.495)	−0.080579 (0.260)	−0.042450 (0.296)	−0.003219 (0.467)
Irrigation × Village Fund	−0.003582 (0.030)	−0.072104 (0.287)	−0.043277 (0.254)	−0.006725 (0.082)
Agriculture × Village Fund	0.000509 (0.789)	−0.041923 (0.556)	−0.020086 (0.619)	0.000436 (0.922)
Irrigation $(t-2)$ main term	0.081799 (0.026)	2.075450 (0.160)	1.187357 (0.151)	0.160872 (0.066)
Village Fund $(t-1)$ main term	0.056952 (0.097)	1.350570 (0.352)	0.782014 (0.331)	0.108580 (0.176)

The post-2020 interaction structure is sensitive to transformation choice. In the synchronized analysis file, the three-way interaction is not statistically distinguishable from zero in any of the four transformation variants. The irrigation × Village Fund interaction is negative in all variants, but it is only below 0.05 in the baseline placeholder specification and weakens under alternative placeholders and the IHS transformation. These results support a cautious, exploratory reading of the interaction block.

Lead-outcome placebo test

Table A3. Lead-outcome placebo for the post-2020 full interaction model

Term	Coefficient	SE	p-value
Irrigation $(t-2)$	-0.048635	0.048404	0.315
Agriculture (t)	0.045650	0.065793	0.488
Village Fund $(t-1)$	-0.000256	0.059417	0.997
Three-way interaction	1.25e-05	1.37e-04	0.928
Agriculture × Irrigation	$-2.20e-05$	0.002798	0.994
Irrigation × Village Fund	0.001377	0.002338	0.556
Agriculture × Village Fund	-0.002522	0.003164	0.425

The lead-placebo does not show evidence that the interaction block strongly predicts the one-year-ahead outcome under the same post-2020 structure. This does not establish causal identification, but it weakens the claim that the fitted interaction pattern is entirely driven by simple forward-looking spurious correlation.

Permutation-based placebo for the three-way interaction

Table A4. Permutation-placebo summary for the three-way interaction

Statistic	Value
Actual three-way coefficient	4.50e-05
Mean of permutation distribution	2.35e-06
SD of permutation distribution	7.69e-05
2.5th percentile	$-1.58e-04$
Median	1.19e-06
97.5th percentile	1.52e-04
Two-sided permutation p-value	0.536

The observed three-way coefficient is not located in the extreme tail of the permutation-based null distribution. In the synchronized analysis file, this means the three-way interaction should not be presented as strong standalone evidence. It is more appropriate to describe it as suggestive fitted surface structure that requires cautious interpretation.

Exact p-values and multiple-testing correction

Table A5. Exact p-values and Benjamini–Hochberg correction for Model 7 interactions

Interaction term	Coefficient	Exact p-value	BH-adjusted q-value
Three-way interaction	4.50e-05	0.602	0.789
Agriculture × Irrigation	-0.001263	0.495	0.789
Irrigation × Village Fund	-0.003582	0.030	0.121
Agriculture × Village Fund	0.000509	0.789	0.789

Only the irrigation $\times$ Village Fund interaction is below 0.05 before correction, but it does not survive Benjamini–Hochberg correction for the family of interaction tests. Accordingly, the interaction block should be reported as exploratory and hypothesis-generating in the synchronized analysis file.

Marginal effects with 95% pointwise confidence intervals

For the post-2020 full interaction model, the marginal effect of $\ln(\text{DAK–Agriculture})$ is evaluated as:

$$\begin{aligned}\text{ME}_{\text{Agriculture}} = \beta_{\text{Agriculture}} &+ \beta_{\text{Agriculture}\times\text{Irrigation}} \times \ln(\text{DAK–Irrigation})_{(t-2)} \\ &+ \beta_{\text{Agriculture}\times\text{Village Fund}} \times \ln(\text{Village Fund})_{(t-1)} \\ &+ \beta_{\text{three-way}} \times \ln(\text{DAK–Irrigation})_{(t-2)} \times \ln(\text{Village Fund})_{(t-1)}\end{aligned}$$

The evaluation grid uses the observed-support quantiles from the positive-spending distributions in the synchronized post-2020 sample.

Table A6. Quantile grid used for marginal-effects evaluation (positive-spending observations)

Variable	**p25**	**p50**	**p75**
$\ln(\text{DAK–Irrigation})$ $(t-2)$	21.746	22.329	22.834
$\ln(\text{Village Fund})$ $(t-1)$	21.381	22.147	22.789
$\ln(\text{DAK–Agriculture})$	21.065	21.735	22.489

Table A7. Marginal effect of $\ln(\text{DAK–Agriculture})$ across the observed-support grid

Irrigation	**Village Fund**	**Marginal effect**	**SE**	**95% CI**	**Distinguishable from zero?**
p25	p25	0.0025	0.0095	$[-0.0161, 0.0210]$	No
p25	p50	0.0036	0.0095	$[-0.0150, 0.0222]$	No
p25	p75	0.0046	0.0096	$[-0.0143, 0.0234]$	No
p50	p25	0.0023	0.0095	$[-0.0163, 0.0209]$	No
p50	p50	0.0034	0.0095	$[-0.0152, 0.0221]$	No
p50	p75	0.0044	0.0096	$[-0.0145, 0.0233]$	No
p75	p25	0.0021	0.0095	$[-0.0166, 0.0208]$	No
p75	p50	0.0033	0.0096	$[-0.0154, 0.0221]$	No
p75	p75	0.0043	0.0097	$[-0.0146, 0.0232]$	No

Across the p25/p50/p75 grid, the marginal effect of agriculture is not statistically distinguishable from zero at conventional levels in the synchronized analysis file. This means the fitted response surface may still be useful descriptively, but it should not be over-read as evidence of precisely estimated local effects.

Consistent transformation of GRDP

Table A8. Robustness to using $\ln(\text{GRDP})$ consistently across models

Term	**Coefficient**	**SE**	**p-value**
Panel A. Model (1) with $\ln$(GRDP)			
$\ln$(DAK–Irrigation)	-0.031957	0.011654	0.006
$\ln$(DAK–Agriculture)	0.009126	0.011059	0.409
$\ln$(Village Fund)	-0.011945	0.011621	0.304
$\ln$(GRDP)	-0.060669	4.553528	0.989
Poverty rate	-1.478964	0.200612	< 0.001
Stunting	-0.023292	0.005284	< 0.001
Panel B. Post-2020 full interaction model with $\ln$(GRDP)			
Irrigation $(t-2)$	0.081799	0.036732	0.026
Agriculture (t)	-0.001877	0.040531	0.963
Village Fund $(t-1)$	0.056952	0.034324	0.097
Three-way interaction	4.50e-05	8.63e-05	0.602
Agriculture $\times$ Irrigation	-0.001263	0.001850	0.495
Irrigation $\times$ Village Fund	-0.003582	0.001654	0.030
Agriculture $\times$ Village Fund	0.000509	0.001900	0.789
$\ln$(GRDP)	3.459129	4.662735	0.458
Poverty rate	-0.928978	0.230035	< 0.001
Stunting	-0.002209	0.007757	0.776

Using $\ln(\text{GRDP})$ consistently across specification blocks does not materially strengthen the case for the three-way interaction in the synchronized analysis file. The interaction block remains weak, and the main substantive message remains one of caution rather than decisive non-additive evidence.

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