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Evaluating Indonesia's Rp200 Trillion Government Fund Placement: Effects on Microcredit and Labor Absorption

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ABSTRACT

Purpose– To examine whether Indonesia's September 2025 Rp200 trillion government fund placement was associated with changes in microcredit and labor-market outcomes, while assessing limits to causal interpretation.
Design/methodology/approach – The manuscript reports a two-way fixed-effects difference-in-differences design comparing five recipient banks with 20 matched commercial banks over January 2023–March 2026, supplemented by an event-study, a survey of 150 microcredit managers, and provincial unemployment indicators.
Findings – The reported bank-level estimate for log microcredit was 0.0135 ($p = .001$), with a reported interest-rate coefficient of −0.76 percentage points ($p < .001$). The reported coefficient for provincial open unemployment was −0.32 percentage points ($p = .023$). Although statistically significant in the reported specification, this association should not be interpreted as a confirmed employment effect without an established link between bank-level treatment exposure and provincial labor-market outcomes. The original analysis also reports stronger responses for banks with pre-policy loan-to-deposit ratios below 90% and rural areas, and characterizes microcredit as a partial mediator. These latter claims require cautious reading because subgroup and indirect-effect estimates are not separately displayed and the link between bank- and province-level outcomes is unspecified.
Originality/value – The study places a discrete fiscal liquidity intervention within the bank lending-channel literature and identifies the design conditions needed to distinguish credit expansion from labor-market transmission.

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1. Introduction

Government liquidity placement offers an apparently direct response to weak business lending, yet additional deposits need not generate additional productive credit. Banks may use new funding to adjust liquidity buffers, manage risk, or replace other liabilities rather than extend loans (Bernanke & Blinder, 1992; Çolak & Öztekin, 2021). This distinction matters in bank-centered financial systems because a policy's balance-sheet effect can occur before its consequences for borrowers become visible (Soedarmono et al., 2023; Thakor & Yu, 2024). International evidence also suggests that banks respond differently to funding conditions depending on capital, asset size, and portfolio quality (Jiménez-Rodríguez & Koo, 2025; Naiborhu, 2024). Accordingly, a meaningful evaluation must separate changes in banks' lending volumes and prices from broader claims about real economic activity. The Indonesian government's Rp200 trillion placement in September 2025 presents a timely setting for that distinction.

Indonesia's micro, small, and medium enterprises depend on access to formal credit, but the availability of such financing differs across borrowers and locations. Research in developing economies links financing constraints to limited enterprise resilience, investment capacity, and business adjustment during disruptions (Anas et al., 2022; Khan, 2022). Studies of small firms also show that lending standards, bank market structure, and borrower characteristics influence credit access and loan terms (Finaldi Russo et al., 2024; Harrison et al., 2022). Within Indonesia, monetary conditions and bank funding liquidity have been associated with changes in undisbursed loans and the transmission of monetary policy (Handayani & Setiastuti, 2025; Soedarmono et al., 2023). Economic conditions after the pandemic consequently make it relevant to ask whether a public deposit placement reached credit-constrained enterprises rather than remaining within bank balance sheets. Evidence concerning the October 2025 slowdown in bank credit is part of the subsequent policy environment and should not be mistaken for an exclusively pre-intervention baseline (Otoritas Jasa Keuangan, 2025).

The policy was introduced on September 12, 2025, with Rp200 trillion allocated across five state-linked banks. The recorded allocation was Rp55 trillion each at Bank Rakyat Indonesia, Bank Negara Indonesia, and Bank Mandiri, Rp25 trillion at Bank Tabungan Negara, and Rp10 trillion at Bank Syariah Indonesia (Bank Indonesia, 2025). The stated restriction on purchasing government securities was intended to orient the intervention toward productive financing, although the restriction itself is not an experimental contrast. Recipient-bank lending could also change because of contemporaneous interest-rate conditions, borrower demand, risk assessments, and other public programs (Aziz & Maulida, 2024; Naiborhu, 2024). The placement therefore represents an identifiable policy event, not an automatically exogenous shock for every outcome that followed it. Assessment of its effectiveness requires attention to comparison-group selection and the timing of the responses observed.

The policy is also relevant to employment, but the path from additional bank liquidity to new jobs involves several decisions outside banks' direct control. Firms must obtain financing, employ it for productive activity, and choose whether to hire workers rather than purchase equipment or refinance existing liabilities. Evidence from a middle-income economy shows that credit-supply shocks can affect formal employment among smaller firms, while effects

differ according to prior banking relationships and borrower characteristics (Gutierrez et al., 2023). Other studies associate financial access with employment outcomes but rely on measurements and institutional arrangements that are not directly interchangeable with Indonesia's provincial unemployment rate (Bakhtiari & Magnani, 2025; Han et al., 2025). Regional labor-market inequality further complicates the interpretation of national policy outcomes (Sen, 2024). Consequently, a credit response at the bank level and a change in provincial unemployment cannot be treated as a single causal chain without an explicit cross-level identification strategy.

Existing studies give plausible reasons to expect a credit response but also show why the answer cannot be assumed in advance. Evidence on Indonesia's earlier funding-for-lending scheme indicates that participating state-owned banks expanded lending relative to nonparticipants during pandemic-related financial distress (Naiborhu & Ulfa, 2023). A separate Indonesian study links fiscal liquidity placement in the COVID-19 period to changes in credit allocation by government-owned banks rather than uniform expansion across all portfolios (Putra, 2026). Studies from other jurisdictions similarly document heterogeneous lending responses to credit support, macroprudential rules, and funding constraints (Ahn et al., 2025; Ćehajić & Košak, 2022; Guo et al., 2023). The relevant gap is therefore not the complete absence of evidence about public bank funding. Although previous Indonesian studies have examined government-supported bank lending during the COVID-19 period, evidence remains limited on the microcredit response to the September 2025 Rp200 trillion placement. A further unresolved issue concerns whether bank-level credit responses can be meaningfully connected to provincial labor-market outcomes. Addressing these distinctions provides a more specific basis for evaluating the policy's financial transmission and its potential implications for employment.

Methods for evaluating a discrete intervention clarify both the appeal and the limitations of such an inquiry. Difference-in-differences compares changes among exposed and comparison institutions but requires a credible counterfactual trajectory, not merely insignificant pre-intervention coefficients (Callaway & Sant'Anna, 2021; Roth, 2022). Recent work emphasizes careful interpretation of parallel trends, sensitivity to untreated time-varying shocks, and the risk of problematic comparisons when treatment adoption varies (Rambachan & Roth, 2023; Roth et al., 2023). When treatment begins at the same time for the five recipient banks, the central challenge is less staggered adoption than the comparability of recipient and nonrecipient banks and the small number of treated institutions (Goodman-Bacon, 2021; Wing et al., 2024). Matching on pre-policy balance-sheet characteristics may improve observed comparability, but it cannot remove unobserved determinants of post-policy lending. These considerations also constrain any attempt to extend the bank-level treatment design to provincial labor-market indicators.

This study examines the reported association between Indonesia's Rp200 trillion government fund placement and microcredit disbursement, credit pricing, and provincial open unemployment over January 2023–March 2026. It compares five recipient banks with 20 matched commercial banks using a two-way fixed-effects difference-in-differences specification and an event-study representation. It also discusses reported differences

associated with pre-policy loan-to-deposit ratios and provincial rural classification. Conceptually, the study uses the bank lending channel to distinguish the availability of funding from the allocation of productive credit (Bernanke & Gertler, 1995; Kanga et al., 2024). The study contributes to the Indonesian banking literature by examining reported changes in microcredit disbursement and credit pricing following the September 2025 government fund placement. By comparing recipient and nonrecipient banks, it extends the discussion of fiscal liquidity transmission to a distinct policy episode while distinguishing bank-level lending responses from less directly established labor-market outcomes. The policy implication is necessarily conditional: targeting and monitoring recommendations should follow only from transmission channels that the available data can substantiate.

2. Literature Review and Hypothesis Development

The bank lending channel describes how funding and balance-sheet frictions shape loan supply and, potentially, real activity (Bernanke & Gertler, 1995; Stiglitz & Weiss, 1981). For Indonesia, Ariefianto et al. (2022) describe dynamic bank liquidity management, while Soedarmono et al. (2023) show that funding liquidity alters banks' response to policy conditions. Findings by Naiborhu (2024) suggest that the lending consequences of monetary policy and provisioning depend on existing capital and liquidity buffers. Likewise, Çolak and Öztekin (2021), Jiménez-Rodríguez and Koo (2025), and Handayani and Setiastuti (2025) document heterogeneity in bank lending channels across institutions or monetary conditions. The analytical implication is that the amount deposited by government should not be treated as equivalent to the amount of incremental microcredit supplied. H1: Recipient banks are expected to show a more favorable post-placement change in microcredit disbursement than matched nonrecipient banks.

Microcredit access is shaped by the interaction of lender capacity and firm-level demand. Harrison et al. (2022) emphasize the credit-supply constraints faced by small businesses, while Finaldi Russo et al. (2024) report persistent differences in access between microfirms and larger borrowers. Kanga et al. (2024) show that the structure of financial-sector competition affects the microfinance sector's lending to small firms; Tsuruta (2023) additionally identifies differences between mature and young firms during liquidity shortages. Findings concerning macroprudential tightening and firm financing needs indicate that banking policy can affect borrowers unevenly (Akbas et al., 2026; Amin & Motta, 2023; Ćehajić & Košak, 2022). Financial market competition and late-payment risk offer additional explanations for changes in credit availability independent of a particular liquidity placement. These studies motivate reporting the interest-rate outcome alongside lending quantity, while avoiding the assumption that simultaneous quantity and price changes alone identify a unique supply mechanism.

The pathway from microcredit to employment is plausible but not mechanically determined. Gutierrez et al. (2023) identify formal-employment responses to bank credit shocks among smaller firms in Mexico; Han et al. (2025) discuss nonfarm employment and credit access in a different institutional setting. Bakhtiari and Magnani (2025) connect firms' liquidity positions to employment decisions, whereas Kouakou (2026) shows that credit constraints are influenced by firm-level and institutional conditions. Indonesia-specific labor-market

inequality examined by Sen (2024) points to the importance of regional characteristics. Nevertheless, provincial unemployment includes labor-market dynamics beyond the firms receiving credit and does not measure hires by individual borrowers. H2: Microcredit expansion is proposed as a potential mechanism connecting fund placement and unemployment, subject to establishing a valid bank-to-province exposure measure and mediation identification assumptions (Celli, 2022; Imai et al., 2010).

Heterogeneity may reflect unused balance-sheet lending capacity, borrower composition, and institutional structure rather than liquidity shortages alone. A lower pre-policy loan-to-deposit ratio can indicate greater room to expand lending, although it does not directly establish that a bank is liquidity constrained. Studies of macroprudential regulation and funding show that policy responses vary with capitalization, regulation, and risk (Ahn et al., 2025; Husodo et al., 2024; Yudaruddin et al., 2024). Yu et al. (2025) further show that capital support does not invariably raise liquidity creation, and Minoiu et al. (2026) find lending-program spillovers that depend on participating-bank characteristics. Regional differences in financial access may similarly shape where incremental credit is deployed, as suggested by evidence on small-firm loan access and bank relationships (Guo et al., 2023; Kholidah et al., 2025).

H3: The microcredit response to government fund placement is expected to be stronger among banks with pre-policy loan-to-deposit ratios below 90%. Differences between rural and urban areas are considered separately as an exploratory dimension of regional policy transmission.

The empirical framework must distinguish hypotheses from conclusions that its output can establish. A bank-level matched comparison can speak primarily to differences in recipient and comparison banks' lending outcomes, provided treatment and control trends would have remained comparable. Sun and Abraham (2021), Borusyak et al. (2024), and Roth et al. (2023) discuss the interpretation of dynamic treatment effects and the limits of conventional event-study representations. Roth (2022) cautions that nonrejection of pretrend tests does not itself establish the required identifying assumption, while Rambachan and Roth (2023) present sensitivity approaches for possible trend violations. Recent international policy studies also show that spillovers and financing reallocation can complicate simple treated-versus-untreated comparisons (Minoiu et al., 2026; Putra, 2026). For this reason, the manuscript presents its reported results without treating all three hypotheses as equivalently established.

3. Methodology

3.1 Research Design and Setting

This study employed an explanatory quantitative quasi-experimental design using two-way fixed-effects difference-in-differences (DiD) to compare changes around the September 12, 2025 fund placement. The stated observation window was January 2023 through March 2026. The policy recipients were five state-linked banks, and the bank-level comparison group comprised 20 other commercial banks selected from a stated population of approximately 106 banks. The quantitative focus was on the logarithm of microcredit disbursement and changes in microcredit interest rates; provincial open unemployment was also identified as an outcome in the original manuscript. DiD can estimate a treatment contrast under assumptions about untreated potential outcomes, but use of a quasi-experimental label does not itself establish

those assumptions (Callaway & Sant'Anna, 2021; Roth et al., 2023). Because provincial unemployment was not linked to a stated bank-to-province exposure measure, its association with policy participation requires a more cautious interpretation than the bank-level outcomes.

3.2 Data Sources, Sampling, and Measurement

Bank-level and macroeconomic data were compiled from Otoritas Jasa Keuangan (OJK), Bank Indonesia, Badan Pusat Statistik (2025), and the Ministry of Finance, supplemented by a structured survey of 150 microcredit managers at recipient banks. Five recipient institutions were compared with 20 nonrecipient commercial banks selected using propensity score matching based on pre-policy total assets, capital adequacy ratio (CAR), loan-to-deposit ratio (LDR), and the MSME loan share observed in 2024 (Rosenbaum & Rubin, 1983). The reported regional component involved 12 provinces selected through a stratified approach to represent urban and rural conditions. The outcome variables in the source were the natural log of microcredit disbursement, the effective microcredit interest rate, and provincial open unemployment (TPT); bank and macroeconomic covariates were third-party funds (DPK), nonperforming loans (NPL), inflation, and the BI rate. The matching algorithm and balance diagnostics were not set out in the reported specification, and provincial TPT was not defined on the monthly bank panel's unit-time scale. Consequently, the bank and regional components should not be interpreted as an automatically integrated panel.

3.3 Survey Instrument and Measurement Quality

The primary-data component used a closed-ended five-point Likert questionnaire and observation sheets. The authors reported content-validity screening through Aiken's V, with retained items exceeding 0.78, and confirmatory factor analysis meeting CFI > 0.95 and RMSEA < 0.08; internal consistency was described through Cronbach's alpha and McDonald's omega (Aiken, 1980; Cronbach, 1951; Zakariya, 2022). These summary indices should be interpreted as reported instrument-screening criteria, rather than as a presentation of complete item-level validity or reliability results. More importantly, no survey variable is explicitly linked to the reported DiD coefficients, event-study, or mediation calculation. Thus, the survey is treated as an additional descriptive component rather than as evidence of a separately established mechanism.

3.4 Econometric Specification and Interpretation

The bank-level comparison was described by the model $Y_{it} = \alpha_i + \lambda_t + \delta(Treat_i \times Post_t) + \beta' X_{it} + \varepsilon_{it}$, where $Treat_i$ denotes a recipient bank, $Post_t$ denotes observations from September 2025 onward, α_i and λ_t are unit and period effects, and X_{it} is the listed covariate vector. For log microcredit, δ represents a difference in log outcome changes between recipient and comparison banks conditional on the specification; it should not automatically be described as a difference in percentage-point growth rates. The estimation used bank-clustered standard errors and a dynamic event-study with three pre-policy and three post-policy relative periods. The very small number of treated banks makes conventional clustered inference a concern, as does possible selection into government-bank treatment (Conley & Taber, 2011; Wing et al., 2024). Non-significant pre-event coefficients are informative but do not prove parallel trends,

and anticipation, macroeconomic shocks, bank-specific trajectories, and spillovers remain alternative explanations (Rambachan & Roth, 2023; Roth, 2022).

3.5 Mediation and Heterogeneity Analyses

A bootstrap mediation analysis was described with 5,000 replications to examine whether microcredit expansion statistically mediated the relationship between the policy and unemployment (Celli, 2022; Preacher & Hayes, 2008). It also reports comparisons between banks with pre-policy LDR below versus at least 90% and between provinces classified as rural and urban. However, the reported results do not contain indirect-effect estimates, bootstrap confidence limits, subgroup-interaction coefficients, or an exposure definition linking provincial unemployment to recipient-bank activity. Accordingly, the directional summaries are distinguished from fully estimated causal mediation or statistically demonstrated subgroup contrasts. This distinction keeps hypothesis statements separate from the numerical evidence used to assess them. Figure 1 summarizes the analytical sequence, separating the definition of policy exposure, data assembly, bank-level comparison, estimation, and interpretation.

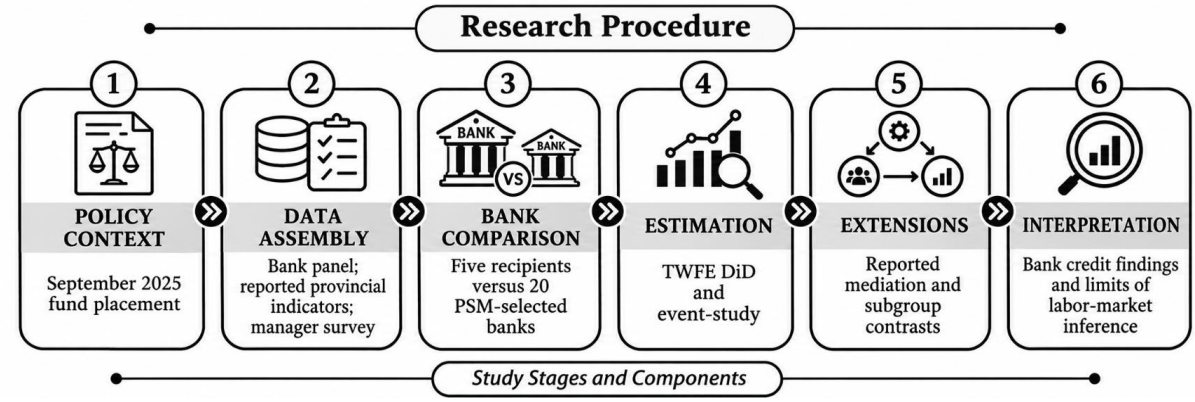


Figure 1. Analytical workflow of the evaluation design

Figure 1 distinguishes policy definition from the measurement stage that follows it. The comparison stage pertains to bank-level treatment, whereas the labor-market outcome is measured at the provincial level. The matched-bank comparison precedes the DiD and event-study results described in the article. Mediation and subgroup extensions are distinct from the bank-level coefficient estimates and should be assessed using their own numerical outputs. The final step therefore emphasizes the limits of what can be inferred from the available results.

4. Results and Discussion

4.1 Results

Table 1 summarizes the reported pre-policy and post-policy descriptive statistics for recipient banks and the comparison group.

Table 1. Descriptive statistics before and after the fund placement

Variable	Treated pre	Treated post	Control pre	Control post
Log microcredit	5.12 ± 0.34	5.26 ± 0.31	4.87 ± 0.42	4.91 ± 0.40
Microcredit interest rate (%)	12.4 ± 1.1	11.6 ± 0.9	12.6 ± 1.2	12.3 ± 1.1
LDR (%)	88.3 ± 4.2	86.1 ± 3.8	91.5 ± 5.0	91.0 ± 4.9
Open unemployment rate (%)	4.82 ± 0.61	4.51 ± 0.58	4.79 ± 0.64	4.68 ± 0.62

Table 1 shows an increase in average log microcredit from 5.12 to 5.26 among recipient banks, compared with a change from 4.87 to 4.91 in the controls. The corresponding unadjusted difference-in-differences in the displayed group means is 0.10 log points. The interest-rate means declined by 0.8 and 0.3 percentage points in the treated and comparison groups, respectively. Unemployment means declined in both groups, producing an unadjusted contrast of −0.20 percentage points from the displayed values. These descriptive comparisons differ from the reported regression coefficients in Table 2 and should not be presented as the same estimate.

The event-study for log microcredit presents coefficients from three relative periods before to three relative periods after the placement. Figure 2 presents the associated event-study estimates and the confidence intervals displayed in the study.

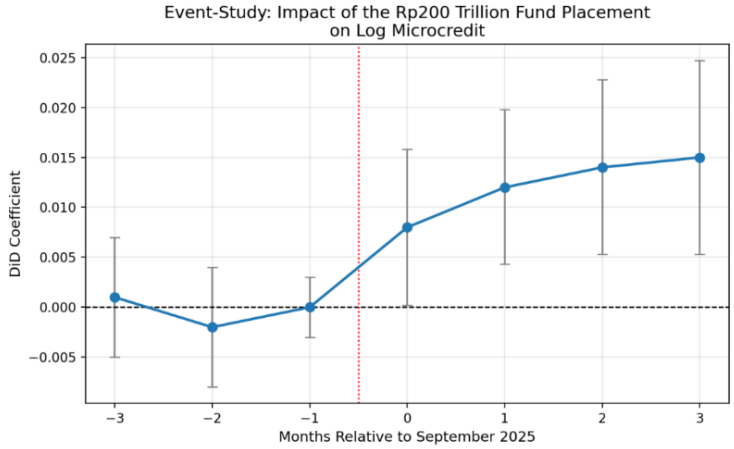


Figure 2. Event-study estimates for log microcredit

Figure 2 reports coefficients close to zero during the three displayed pre-policy periods. Its confidence intervals overlap zero before implementation, but this does not establish that untreated outcomes would have followed parallel trends. The estimates shown after the intervention are positive and increase over the displayed three-period window. The event-study displays increasingly positive coefficients over the three reported post-intervention periods, suggesting that the estimated lending differential developed over time rather than appearing entirely at implementation. However, this pattern does not establish whether the response originated from new lending, portfolio reallocation, or other contemporaneous changes in banking conditions. Its reported dynamic pattern should be interpreted alongside the DiD specification and the limited number of treated banks. Table 2 reports the three coefficients and corresponding p-values as estimated in the study, with decimal notation standardized for English-language reporting.

Table 2. Reported difference-in-differences estimation results

Outcome	Reported DiD coefficient	Reported p-value
Log microcredit	0.0135	0.001
Microcredit interest-rate change (percentage points)	−0.76	< 0.001
Provincial open unemployment rate (percentage points)	−0.32	0.023

Table 2 reports a positive coefficient of 0.0135 for the logarithm of microcredit. On a level interpretation, this corresponds to approximately 1.36% higher microcredit relative to the comparison trajectory, conditional on the fitted model, rather than a 1.35-percentage-point growth-rate effect. The reported interest-rate contrast is −0.76 percentage points, and the unemployment contrast is −0.32 percentage points. Table 2 presents the reported coefficients and associated p-values for microcredit, interest-rate changes, and provincial unemployment. Because standard errors, confidence intervals, and observation counts are not displayed, the precision and comparative reliability of these estimates cannot be fully evaluated from the available results. In particular, the reported unemployment coefficient cannot by itself establish an employment effect without the regional treatment definition and the bank-to-province linkage. Table 3 summarizes the proposed hypotheses, reported assessments, and the corresponding scope of the displayed quantitative evidence.

Table 3. Hypothesis summary and scope of available evidence

Hypothesis	Source assessment	Evidence displayed
H1: Fund placement increases microcredit disbursement	Supported	Bank-level DiD coefficient shown; identification and inference remain conditional
H2: Microcredit mediates the reduction in unemployment	Partially supported	No indirect-effect estimate or bootstrap interval displayed
H3: Stronger microcredit response among banks with pre-policy LDR below 90%	Supported	Reported as supported; subgroup estimates not displayed

Table 3 summarizes the three hypotheses and their reported assessments. The H1 claim is accompanied by a numerical DiD coefficient, but its causal interpretation still depends on the design assumptions. H2 is described as partial mediation without a displayed indirect effect or uncertainty interval. H3 is described as a heterogeneous response without numerical subgroup comparisons. The available evidence provides a numerical bank-level estimate relevant to H1, although its causal interpretation remains conditional on the identification assumptions. In contrast, H2 and H3 cannot be considered conclusively supported by the displayed results because the corresponding indirect-effect estimates, subgroup coefficients, and uncertainty measures are not reported.

4.2 Discussion

The reported positive microcredit coefficient is consistent with the possibility that fiscal funds placed in recipient banks were followed by stronger credit growth than in matched nonrecipient institutions. In the bank lending-channel framework, additional deposit funding can affect lending decisions when liability costs or liquidity constraints matter (Bernanke &

Gertler, 1995). Naiborhu and Ulfa (2023) provide a particularly pertinent Indonesian comparison because their earlier funding-for-lending evaluation used a DiD approach and found higher lending among participant banks. The present case concerns a different policy episode, but the conceptual comparison is stronger than presenting the September 2025 placement as an unprecedented bank-funding experiment. Soedarmono et al. (2023) and Ariefianto et al. (2022) further suggest that changes in funding conditions operate through established bank balance-sheet processes. The resulting interpretation is a policy-associated lending response, not definitive evidence that the entire observed increase was financed directly by the placed funds.

The reported decline in microcredit interest rates offers a second perspective on the adjustment of bank lending after placement. Lower rates alongside a higher loan-volume coefficient are compatible with an easing of funding conditions, but neither observation rules out concurrent shifts in borrower demand, competitive pricing, or portfolio composition. Evidence on Indonesian provisioning and bank lending shows that capitalization and credit risk can alter the sensitivity of loan growth to monetary conditions (Naiborhu, 2024). International studies likewise document the relevance of macroprudential changes and loan-market conditions for small-firm financing terms (Ćehajić & Košak, 2022; Harrison et al., 2022). Minoiu et al. (2026) find lower lending spreads and higher loan volumes in another public credit-support setting, underscoring the relevance of pricing as well as quantity. The interest-rate result is therefore informative, although the stated government-securities restriction cannot be credited as the unique cause without evidence on actual asset substitution.

The difference between the descriptive mean contrasts and the fitted DiD coefficients requires a separate reading of the reported magnitude. Table 1 implies a larger unadjusted change in log microcredit than the 0.0135 coefficient displayed in Table 2, while its simple interest-rate and unemployment contrasts also differ from the regression estimates. Such differences are possible when models include unit effects, period effects, and covariates, but the reported coefficient table does not include the complete model outputs needed to explain them. Research on difference-in-differences repeatedly emphasizes that the estimand depends on the model, comparison structure, and assumptions about untreated trends (Callaway & Sant'Anna, 2021; Roth et al., 2023). Roth (2022) additionally shows that failure to reject pre-intervention trends should not be mistaken for proof that the identifying condition holds. These considerations support interpreting the reported microcredit coefficient as conditional evidence rather than converting it directly into an amount of newly originated credit.

The labor-market coefficient is potentially relevant but carries greater uncertainty about identification than the bank-credit outcomes. Provincial open unemployment can change because of sectoral demand, migration, labor-force participation, and conditions unrelated to the recipient banks. Gutierrez et al. (2023) establish a more direct credit-employment linkage using loans to firms and measured formal employment, while Bakhtiari and Magnani (2025) examine firm liquidity and employment decisions with firm-level data. Those designs highlight the inferential step missing when bank-level treatment is compared with a province-level unemployment rate without a geographic exposure rule. Han et al. (2025) provide further evidence that access to finance and employment can be connected through entrepreneurial

activity, but their context and outcome differ from provincial TPT in Indonesia. Thus, the reported −0.32 coefficient should not be restated as the number of jobs created or as an established effect of fund placement on labor absorption.

The claimed partial mediation through microcredit also requires greater conceptual restraint. Causal mediation entails assumptions about treatment assignment, mediator-outcome confounding, and the temporal ordering of credit and labor-market responses (Celli, 2022; Imai et al., 2010). The methodology specifies 5,000 bootstrap replications but does not display the resulting indirect effect or its confidence interval. Moreover, the observations used for banking and provincial unemployment have different units, and their correspondence is not specified. The reported statistical narrative can therefore be retained as an author-reported proposition, but it cannot be treated as demonstrated causal transmission. The useful contribution at this stage is to articulate a plausible credit-to-employment pathway and its measurement requirements rather than to claim that the pathway has been fully quantified.

The reported stronger response among banks with pre-policy LDR below 90% is compatible with the idea that unused lending headroom conditions policy transmission. A lower LDR does not, by itself, indicate that liquidity was tight; it may signal more deposits relative to outstanding loans and a greater capacity to expand lending. Evidence from Indonesia and other markets points to conditional responses to liquidity and prudential rules, rather than a uniform response across banks (Ahn et al., 2025; Handayani & Setiastuti, 2025; Husodo et al., 2024). Yu et al. (2025) also document circumstances in which capital constraint relief does not translate into greater liquidity creation. Without subgroup coefficients, the reported results do not establish the size or statistical precision of the LDR contrast. Nonetheless, the hypothesis identifies a worthwhile design dimension for future placement programs, provided it is measured consistently.

Regional heterogeneity is also plausible, but rural classification alone cannot establish that a fund placement reached rural microenterprises or generated additional employment there. Kanga et al. (2024) show that the relationship between financial development and microcredit depends on institutional structure, while Kholidah et al. (2025) document financing-related enterprise outcomes in an Indonesian setting distinct from government bank deposits. Akbas et al. (2025) emphasize how discouraged borrowers contribute to unmet financing needs across emerging economies, indicating that a rise in bank liquidity may not automatically reach excluded firms. Moreover, Sen (2024) describes persistent dimensions of labor-market inequality that cannot be reduced to a single rural-urban contrast. The reported rural finding is consequently a useful hypothesis-generating observation rather than proof of targeted regional job creation. More granular branch, borrower, and provincial exposures would be necessary for the claim to carry a causal interpretation.

At the level of public policy, the distinction between new bank funding, incremental credit, and final employment outcomes is the principal lesson of the analysis. The policy may improve financial intermediation when recipient banks have appropriate lending capacity, borrower demand, and incentives, but deposit placement alone does not demonstrate that productive firms were the ultimate beneficiaries. Putra (2026) documents reallocation toward working-

capital lending during an earlier Indonesian fiscal liquidity episode, suggesting that portfolio composition may be as important as total loan growth. Comparative evidence on small-firm constraints and government lending programs supports transparent allocation and monitoring, while also raising the possibility of interbank and borrower substitution (Amin & Motta, 2023; Guo et al., 2023; Minoiu et al., 2026). Relative to earlier Indonesian studies of government-supported bank lending, the September 2025 intervention provides a distinct setting for examining how public liquidity placement relates to microcredit disbursement and lending rates. The reported estimates are consistent with differences in post-placement bank lending responses, while the provincial unemployment findings reveal the limitations of extending these observations to employment outcomes. This distinction contributes to the literature by clarifying the difference between financial intermediation responses and the evidence required to establish real-sector transmission. Future evaluations would be better positioned to inform allocation decisions if they integrated bank-to-borrower credit flows with geographically traceable employment measures.

5. Conclusion and Suggestions

The estimates indicate a positive post-placement bank-level DiD coefficient for log microcredit (0.0135, p = .001), a negative microcredit interest-rate coefficient (−0.76 percentage points, p < .001), and a negative provincial unemployment coefficient (−0.32 percentage points, p = .023). These figures are considered alongside the assumptions necessary to interpret the corresponding contrasts. The lending result is compatible with a strengthening of the bank lending channel after the September 2025 placement, but its causal interpretation depends on the quality of the comparison group, the parallel-trends assumption, and appropriate inference for five treated banks. The unemployment, mediation, and subgroup claims are less fully supported by the material displayed because the geographical exposure structure and detailed corresponding estimates are absent. The findings indicate a positive reported difference-in-differences coefficient for microcredit disbursement and a negative coefficient for microcredit interest rates following the September 2025 government fund placement. These results are consistent with a more favorable credit-market response among recipient banks relative to the comparison group, subject to the assumptions underlying the estimation. However, the available evidence does not establish that the observed banking responses directly generated additional employment or reduced provincial unemployment through microcredit expansion. The study therefore supports a cautious interpretation of financial transmission while identifying the need for more direct evidence on labor-market outcomes.

For policy, transparent monitoring should distinguish changes in recipient-bank deposits from additional microcredit, loan pricing, borrower reach, and the quality of any employment outcomes. Any future allocation strategy based on LDR or regional disadvantage should rely on an established and reproducible comparison of heterogeneous effects, not on descriptive contrasts alone. The more general contribution is to encourage evaluation of fiscal liquidity placement as a sequence of potentially distinct outcomes rather than as a single automatically successful stimulus mechanism.

6. Limitations and Future Research

The post-policy observation window is short relative to the possible timing of loan origination and employment responses. The study compared five treated banks with 20 matched banks, and the reported results do not provide complete PSM diagnostics, bank-clustered standard errors, or confidence intervals for the DiD estimates. Because recipient banks were specifically selected by policy, unobserved differences, time-varying shocks, and spillovers remain material threats to attribution. The provincial unemployment measure was not linked to a documented bank-by-region exposure series, and the survey of 150 microcredit managers was not integrated explicitly into the estimated models. These features restrict both the internal validity of labor-market claims and the generalizability of the results to other fiscal placements.

Future work could combine administrative bank lending data with loan-level and geographic borrower information, enabling more direct examination of financing and employment transmission. A longer observation period would permit evaluation of loan performance, borrower entry, and the persistence of any credit response. Matching diagnostics, treatment-specific trend graphs, and sensitivity or inference procedures tailored to a small treated group would make bank-level estimates more interpretable. Subgroup analysis should display interaction coefficients and uncertainty intervals, while any mediation analysis should state its exposure mapping, estimated indirect effect, and causal assumptions. These steps would build on the policy setting without requiring unsupported conclusions from the present results.

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Declaration of AI and AI-Assisted Technologies in the Writing Process

During preparation and revision of this work, the authors used ChatGPT (OpenAI) for manuscript drafting, language refinement, and content organization. The authors remain responsible for the accuracy, validity, and integrity of the manuscript, including its data, analyses, and references.

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