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Import Dependence and Employment Adjustment during Monetary Easing: Evidence from Indonesian Manufacturing

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Abstract

This paper asks whether Indonesian manufacturing firms' reliance on imported inputs shaped their employment response to the 2019-2021 monetary easing episode, when Bank Indonesia cut its policy rate from 5.00 to 3.50 percent, and bank credit growth swung from a 2020 contraction to a 2021 recovery. Using establishment-level panel data from BPS Indonesia's (2022) Annual Survey of Large and Medium Manufacturing Industries, an event-study specification interacts firm-level import share with year indicators against a 2017 baseline. Import-dependent firms expand employment significantly more than their peers in 2021, a gap roughly three times the small pre-existing drift, robust to a wild cluster bootstrap. The effect concentrates in small, domestically owned, non-exporting firms, the firms with the least alternative financing access, while capital stock shows no differential response. A direct mechanism test on materials purchases does not show the same divergence, and a continuous specification using actual monetary conditions suggests a threshold effect tied to the depth of the easing episode rather than a smooth function of the policy rate. The paper discusses these complications, its principal limitation (a missing 2020 survey wave), and implications for targeting credit policy at trade-exposed manufacturing firms.

Keywords: Monetary Transmission, Credit Channel, Import Dependence, Employment Adjustment, Indonesian Manufacturing

JEL Classification: E52, F14, J23

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Introduction

Standard accounts of monetary policy transmission in emerging markets emphasise two channels operating through the banking system: the interest-rate channel, through which policy rates move the cost of credit facing all borrowers in roughly the same way, and the credit (or bank-lending) channel, through which the same policy change has a more pronounced effect on borrowers who depend more heavily on external, short-term financing (Bernanke & Gertler, 1995; Kashyap & Stein, 2000). A large literature has shown that this heterogeneity matters for real outcomes at the firm level, particularly for smaller and more credit-constrained firms (Gertler & Gilchrist, 1994). A separate literature on trade finance has documented that firms engaged in international transactions face a distinct, additional financing need: imported inputs are typically paid for through letters of credit or other short-term trade-finance instruments denominated in, or linked to, foreign currency, so that credit conditions at home can bind particularly hard on import-dependent firms during periods of financial stress (Amiti & Weinstein, 2011; Chor & Manova, 2012; Manova, 2013; Paravisini et al., 2015).

This paper brings these two literatures together in a single Indonesian manufacturing panel, exploiting the 2019-2021 window as a natural episode of monetary easing. Between the end of 2019 and the end of 2021, Bank Indonesia's policy rate (the BI 7-Day Reverse Repo Rate) fell from 5.00 to 3.50 percent, its lowest level on record, while aggregate bank credit growth swung from a contraction

of 2.41 percent in 2020 to a recovery of 5.20 percent in 2021. If domestic credit conditions matter disproportionately for firms whose input purchases require short-term, often foreign-currency-linked financing, then firms with a higher share of imported materials should show a more pronounced differential improvement in real outcomes, in employment and possibly in investment, once credit conditions ease, relative to firms that source inputs domestically.

The empirical strategy is an event-study design using firm-level import share (IMPS) interacted with year indicators, estimated on establishment-level panel data from BPS Indonesia's Annual Survey of Large and Medium Manufacturing Industries for 2017, 2018, 2019, and 2021 (BPS did not survey establishments in 2020). Because import share is measured for essentially every firm-year rather than being a coarse industry-level classification, the design has far more cross-sectional variation than industry-level measures of financial dependence used in the [Kroszner et al. \(2007\)](#) approach, which this paper's own preliminary industry-level analysis (not reported in the results below) confirmed was too coarse, with only 23 usable industry cells, to detect anything beyond marginal significance.

The results show a clear pattern. Employment growth among import-dependent firms is close to that of less import-dependent firms in 2018 and shows only a small, marginally significant divergence by 2019, but jumps sharply in 2021, a gap roughly three times the size of the pre-existing drift and clearly distinguishable from a simple continuation of the trend. Capital stock shows no such differential pattern in any year, consistent with capital being subject to higher adjustment costs than labour. Splitting the sample further shows that the response concentrates in small, domestically owned, non-exporting firms, precisely the firms with the least alternative access to financing. The paper also documents an important limitation transparently: the average annual rupiah/US dollar exchange rate in 2021 had largely reverted to its 2019 level, because the sharpest depreciation occurred within the unsurveyed 2020 wave, so the persistent, non-reverting element of the 2019-2021 shock in this data is the policy rate and the credit cycle, not the exchange rate level.

The next section reviews the monetary transmission and trade-finance literatures that motivate the empirical design and states the paper's three predictions, followed by a description of the data and empirical strategy, the presentation and discussion of the results, and a concluding section that draws out the policy implications.

Literature Review

Two strands of literature motivate this paper's empirical strategy. The first is the credit-channel literature on monetary transmission. [Bernanke and Gertler \(1995\)](#) distinguish the interest-rate channel, in which monetary policy affects the user cost of capital in roughly the same way across borrowers, from the credit channel, in which the same policy action has an additional, amplified effect on borrowers who are more dependent on bank credit or have weaker balance sheets. [Kashyap and Stein \(2000\)](#) show, using U.S. bank-level data, that the lending of less liquid banks is more sensitive to monetary policy, implying that firms dependent on such banks feel policy changes more strongly. At the firm level, [Gertler and Gilchrist \(1994\)](#) find that small manufacturing firms, plausibly more credit-constrained, contract inventories and short-term debt more sharply than large firms following monetary tightening, and recover more sharply during easing. This paper's design is closest in spirit to this last result: it looks for uneven firm-level responses to a common monetary shock, using a measure of financing need (import dependence) rather than firm size as the source of the underlying heterogeneity.

The second strand is the trade-finance literature, which documents that international transactions carry financing requirements distinct from, and often larger than, domestic transactions. [Amiti and Weinstein \(2011\)](#) show that Japanese exporters' financial health predicts their export performance during the 2008-2009 financial crisis, and argue this reflects a demand for trade finance that is not fully substitutable with ordinary bank credit. [Chor and Manova \(2012\)](#) find that countries and industries with tighter credit conditions during the global financial crisis saw a larger collapse in exports to the United States, and [Manova \(2013\)](#) shows theoretically and empirically that credit constraints shape not only trade volumes but also the extensive margin of which firms trade at all. Using matched bank-firm data from Peru, [Paravisini et al. \(2015\)](#) isolate a causal effect of credit supply on export volumes, and [Bricongne et al. \(2012\)](#) document that French exporters in financially vulnerable sectors cut trade more sharply than domestically oriented firms during the same crisis. None of these papers, however, examines the import side of the trade-finance channel, or a monetary easing episode; most study export financing during a credit contraction. This paper examines whether the same logic runs in

reverse for import-dependent firms during an easing episode: financing that is constrained during a downturn should also become disproportionately more available when domestic credit conditions ease.

A third, smaller literature is directly relevant to Indonesia. [Shokr and Elsayed \(2023\)](#) document that a bank-lending channel of monetary transmission operates in Indonesia, with banks' loan responses to policy-rate changes varying systematically with bank-level characteristics such as income and size, which by extension should transmit unevenly to the firms that borrow from them. On the exchange-rate side, [Forbes \(2002\)](#) finds that large currency depreciations have uneven, sometimes contractionary effects on firm performance in emerging markets through firms' foreign-currency liabilities and input costs, while [Amiti et al. \(2014\)](#) show that firms which are simultaneously large importers and large exporters have much lower exchange-rate pass-through than firms that only import or only export, because movements in the cost of imported inputs partially offset movements in export revenue. This motivates treating the exchange-rate and interest-rate elements of the 2019-2021 shock as conceptually distinct in the empirical discussion below, since the two need not move together, or persist to the same degree, over the sample window used here.

This paper's identification strategy is also designed with an eye toward three alternative explanations the credit-channel interpretation must be distinguished from. A pure demand-recovery story, in which all firms simply hire more as the broader economy recovers from the 2020 contraction regardless of financing conditions, would not predict a differential response concentrated specifically in import-dependent firms; the paper's design tests for exactly this differential rather than for an aggregate employment recovery, and the heterogeneity results reported later, where the response concentrates in small, domestically owned, non-exporting firms rather than appearing uniformly across the sample, speak directly against a generic recovery account. A supply-chain-disruption story, in which firms responded to the availability of imports rather than to domestic financing, would predict a different pattern. The data cannot fully rule this out, but Indonesia's aggregate import volumes recovered broadly in step with the domestic credit cycle rather than ahead of it, making a purely supply-side account less compelling; the paper does not, however, test the two accounts directly. Finally, while the foundational trade-finance literature reviewed above predominantly studies the 2008-2009 global financial crisis and export financing during credit contractions, this paper's own setting, an easing episode's effect on the import side of financing, combined with Indonesian establishment-level data, is to the authors' knowledge not yet examined directly in existing published work, which is the specific gap this paper fills.

Three predictions follow directly from this literature. The first can be called the credit-availability prediction: if domestic credit conditions bind on firms' ability to finance imported input purchases, a monetary easing episode should produce a more pronounced improvement in real outcomes for more import-dependent firms than for less import-dependent firms, relative to a pre-shock baseline, and this improvement should be visible not only in employment but directly in materials-purchase growth, the most immediate margin along which a working-capital constraint would bind. The second is the adjustment-cost prediction: because capital investment is subject to higher adjustment costs and longer planning horizons than employment ([Gertler & Gilchrist, 1994](#)), any differential response to the easing episode should be more clearly visible in employment than in capital stock, at least within the short window available here. The third is the credit-constraint heterogeneity prediction. If the mechanism is a credit constraint rather than a general trade-exposure effect, the response should concentrate among firms with the least alternative financing: smaller firms ([Gertler & Gilchrist, 1994](#)), domestically owned firms without access to a foreign parent's internal capital markets ([Antras et al., 2009](#)), and non-exporters without export earnings or trade-credit relationships. Rather than being spread evenly across the firm-size and ownership distribution, the response should cluster there. On the adjustment-cost prediction specifically, more recent firm-level evidence reinforces the older argument: [Kurt \(2024\)](#) finds that United States firms' employment and sales respond more strongly to monetary policy than their investment does, a difference she attributes to the firm specificity and irreversibility of capital relative to the comparative ease of adjusting a workforce, a mechanism this paper returns to below when comparing the employment and capital results directly.

Research Method

This section explains, in turn, what data the analysis uses, how the 2019-2021 monetary easing episode is measured, how the main event-study regression is specified and why, and how three further checks then extend that main result to probe the credit-channel interpretation more directly.

The data are establishment-level records from BPS Indonesia's Annual Survey of Large and Medium Manufacturing Industries, covering all manufacturing establishments with 20 or more workers, for survey years 2017, 2018, 2019, and 2021 (BPS did not conduct the survey in 2020). The estimation sample pools 121,591 establishment-year observations across 24 two-digit manufacturing industries defined under Indonesia's standard industrial classification (KBLI), after dropping observations with missing employment, import share, or industry code. Employment is total reported workers; capital stock is reported fixed-asset value, which suffers from item non-response for about half of observations (a known limitation of the capital-stock question in this survey) and is therefore treated as a secondary, lower-powered outcome. Import share (IMPS) is the value of imported raw materials as a share of total raw-material expenditure, defined for essentially the full sample. Materials purchases, used later as a mechanism-test outcome, are the total value of raw materials and intermediate inputs used in production. Firm-level controls include exporter status and the share of foreign equity ownership.

Two macroeconomic series characterise the 2019-2021 monetary easing episode. The BI 7-Day Reverse Repo Rate, Bank Indonesia's benchmark policy rate, fell from 5.00 percent at the end of 2019 to 3.50 percent at the end of 2021, its lowest level since the instrument's introduction in 2016 (Bank Indonesia, 2022, rate-decision announcements, year-end values). Aggregate bank credit growth, reported by Otoritas Jasa Keuangan (OJK, 2022), fell from 6.08 percent in 2019 to a contraction of 2.41 percent in 2020 before recovering to 5.20 percent in 2021. A third series, the average annual IDR/USD exchange rate (World Bank, 2022, World Development Indicators), moved from 14,148 in 2019 to a 2020 peak of 14,582 before reverting to 14,308 in 2021, a net change of only 1.1 percent on an annual-average basis, because the sharpest intra-year depreciation occurred during the unsurveyed 2020 wave. Unlike the policy rate and the credit cycle, the exchange rate therefore shows no persistent, non-reverting change into 2021, so it is unlikely to be the primary driver of any divergence detected in the firm-level data below, a point revisited in the limitations discussion at the end of the results.

The main specification is an event-study interacting import share with year indicators, estimated as a pooled cross-section across all four survey waves. A pooled cross-section is used, rather than a firm fixed-effects panel, because the great majority of 2021 establishments cannot be individually linked to their 2017-2019 identity: BPS re-numbers establishment identifiers between the 2019 and 2021 releases, and no official crosswalk exists. The specification below does not require such a linkage.

$$Y_{it} = \alpha + \beta_{2018}(IMPS_{it} \times D2018_t) + \beta_{2019}(IMPS_{it} \times D2019_t) + \beta_{2021}(IMPS_{it} \times D2021_t) + \gamma X_{it} + \theta_j + \varepsilon_{it} \quad (1)$$

In this equation, i indexes establishments and t indexes survey year, and 2017 is the omitted reference year, so that each coefficient measures the difference between year t 's IMPS-outcome relationship and the 2017 baseline, with its own standard error. Industry fixed effects, defined at the two-digit KBLI level, absorb time-invariant differences in level across the 24 manufacturing divisions, and exporter status and foreign-ownership share control for firm characteristics that may independently affect employment growth. Standard errors are clustered by two-digit industry, the level at which the identifying variation in import share exposure is most plausibly correlated across firms.

Reading the coefficients as a simple before/after contrast, however, would overstate the identifying power of the design if the IMPS-outcome relationship were already trending prior to the 2021 shock. The specification is therefore reported in full event-study form, with separate coefficients for 2018 and 2019 shown alongside 2021, so that any pre-existing drift is directly visible and can be compared in magnitude to the post-shock coefficient, rather than assumed away.

The main specification is extended in three directions, corresponding respectively to the three predictions set out above, to probe the credit-channel interpretation more directly rather than relying on the employment result alone, and each extension is designed to rule competing explanations in or out.

The first extension is a mechanism test. It replaces employment with materials-purchase growth as the outcome, estimated with the identical event-study specification. The logic is direct: if the channel is genuinely a working-capital constraint on financing input purchases, the clearest and most immediate

signature should appear in the purchases themselves, not only in downstream employment, so this test speaks directly to the credit-availability prediction.

The second extension is a continuous monetary-stance specification. It replaces the year indicators with the actual level of the BI policy rate and aggregate credit growth in year t , interacted with import share.

$$Y_{it} = \alpha + \beta IMPS_{it} + \gamma_1(IMPS_{it} \times BIRate_t) + \gamma_2(IMPS_{it} \times CreditGrowth_t) + \delta X_{it} + \theta_j + \tau_t + \varepsilon_{it} \quad (2)$$

This specification nests the event-study as a special case but additionally reports an economic magnitude, the change in the outcome per percentage point of policy rate or credit growth, and, by using the continuous macroeconomic series rather than a single 2021 indicator, tests whether the relationship is well described as a smooth, roughly linear function of monetary conditions across the whole 2017-2021 window, or whether it is better understood as concentrated specifically in the deepest phase of the easing episode.

The third extension is a heterogeneity analysis, which speaks to the credit-constraint heterogeneity prediction. The main employment event-study is re-estimated separately for six mutually exclusive subgroups: small versus large firms (split at the within-year median of log output), exporters versus non-exporters, and domestically owned versus foreign-owned firms. If the response is a genuine credit-constraint effect rather than a general trade-exposure effect, it should concentrate among the firms with the least alternative access to financing.

Finally, because the identifying variation in the interaction terms above comes from only 23-24 industry clusters, standard cluster-robust standard errors may be unreliable: Cameron, Gelbach, and Miller (2008) show that with this few clusters, asymptotic cluster-robust inference can over-reject even when the underlying model is correctly specified. As an additional check on the headline 2021 employment coefficient, a wild cluster bootstrap is implemented following their restricted procedure. The model is first re-estimated under the null that the coefficient of interest is zero; residuals from this restricted model are then multiplied by random Rademacher signs (values of plus or minus one) assigned at the cluster level, across 999 replications; the model is re-estimated on each simulated sample; and the bootstrap p-value is the share of replications whose simulated t-statistic exceeds the magnitude of the one actually observed in the data.

Results and Discussion

Descriptive Patterns

Table 1 reports summary statistics for the estimation sample. Import share has a sample mean of 0.16 and is right-skewed, consistent with a substantial share of establishments sourcing materials entirely domestically, so that the identifying variation used throughout the paper comes from a relatively thin but economically meaningful upper tail of import-intensive firms rather than from the sample as a whole. Employment averages 199 workers per establishment, though the median of 55 shows the distribution is heavily skewed toward a large number of smaller establishments, which matters for interpreting the heterogeneity results reported later in this section, which show that small firms drive most of the headline result. Capital stock is reported for just over half the sample, the item non-response already noted above, and materials purchases, used later as a mechanism-test outcome, are reported for essentially the full sample, matching the coverage of employment and import share.

Table 1. Summary Statistics

Variable	Mean	SD	N
IMPS (import share)	0.157	0.249	121,591
Employment (L)	198.600	784.700	121,591
Capital stock, IDR (K)	2.80×10^8	4.82×10^9	61,767
Materials purchases, IDR (M)	1.05×10^8	8.49×10^8	121,591
Exporter (dummy)	0.086	0.280	121,591
Foreign ownership share (%)	7.920	26.060	121,591

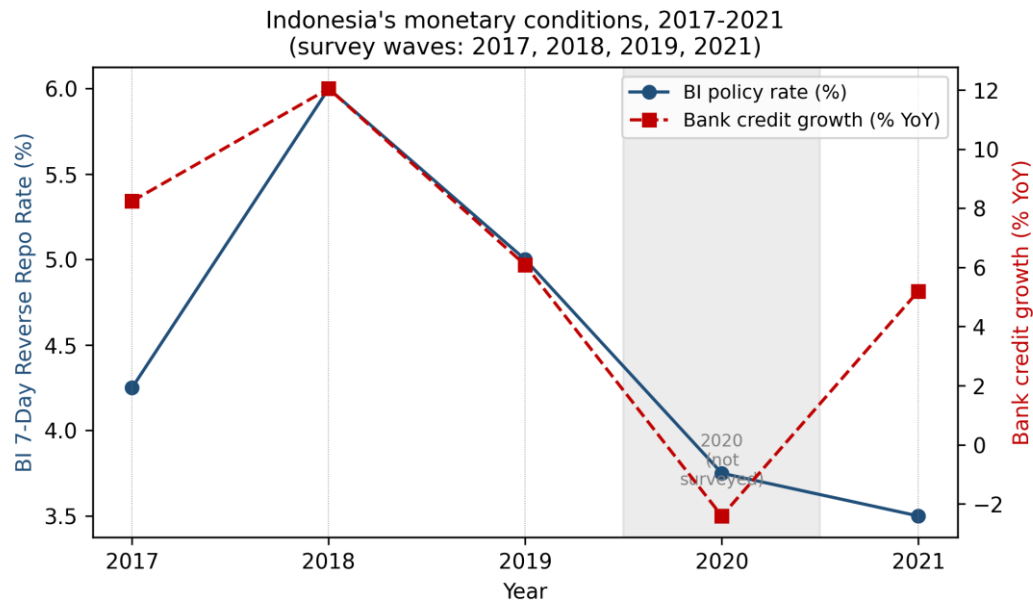


Figure 2. Indonesia's Monetary Conditions, 2017-2021

These cross-sectional patterns say nothing yet about timing, so Figure 2 sets out the macroeconomic backdrop that motivates treating 2019-2021 as a distinct episode rather than an arbitrary choice of end points. The BI policy rate rose from 4.25 to 6.00 percent between 2017 and 2018, a tightening driven largely by external pressure on the rupiah rather than domestic overheating, before falling to 3.50 percent by 2021, while aggregate credit growth traces a similar hump, rising through 2018, collapsing to -2.41 percent in the unsurveyed 2020 wave, and recovering to 5.20 percent in 2021. The two series move together rather than independently across the sample period, a fact that becomes directly relevant later, when the paper asks whether the firm-level response is better described as a smooth function of these series taken individually or as a threshold effect concentrated in the deepest part of the cycle they jointly trace out.

It is worth showing directly how much cross-sectional variation in import dependence there actually is to identify from. Figure 3 shows that import dependence varies widely and systematically across the 24 manufacturing divisions used here, from essentially zero median exposure in food,

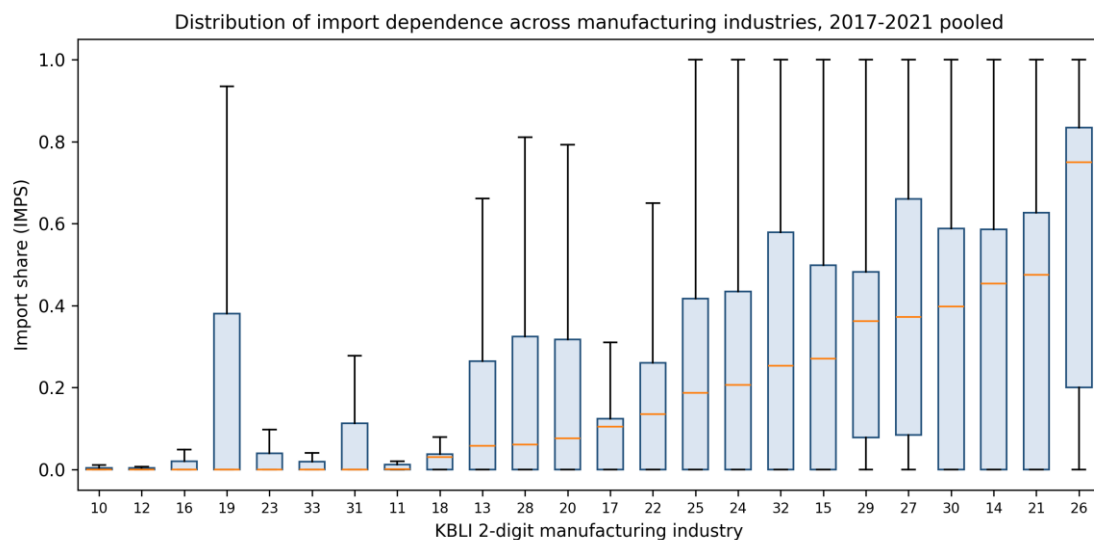


Figure 3. Distribution of Import Dependence across Manufacturing Industries

tobacco, and non-metallic mineral products to a median above 0.7 in computer, electronic, and optical products, with electrical equipment, motor vehicles, and other transport equipment clustered at intermediate levels above 0.4. This is why the preliminary industry-level analysis mentioned in the introduction, based on the [Kroszner et al. \(2007\)](#) approach applied to only 23 industry cells, was judged too coarse to retain as the paper's main specification.

The industries at the high end of this distribution are also the industries whose firms provide most of the identifying variation in the results that follow, since it is variation in exposure across firms and over time, not the average level of exposure itself, that the event-study coefficients reported next are estimated from. The industries at the low end anchor the 2017 baseline against which the later years are compared, and their inclusion is what allows industry fixed effects to absorb level differences across divisions without absorbing the year-specific interaction of interest.

Main Result: Employment and Capital

Figure 1 plots the main event-study coefficients, the differential IMPS-outcome relationship in each year relative to the 2017 baseline, for employment and capital stock, with 95 percent confidence intervals, and provides the clearest single summary of the paper's headline finding before the underlying regression is reported in full in Table 2 below. Plotting both outcomes on the same year-by-year scale also makes the contrast between them easy to see directly, ahead of the formal comparison to the adjustment-cost literature later in this subsection.

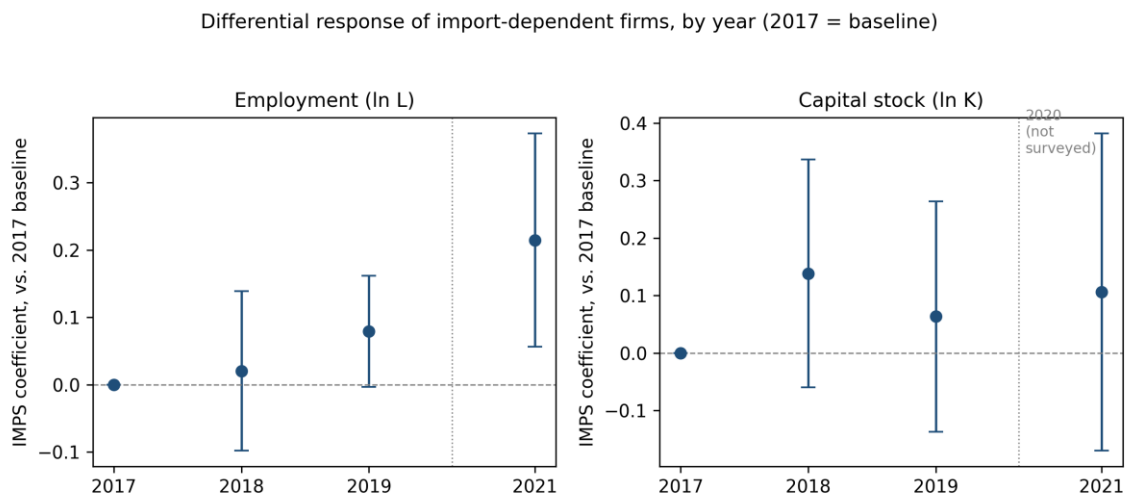


Figure 1. Differential Response of Import-Dependent Firms, by Year (2017 = baseline)

For employment, the 2018 coefficient (0.020, not significant) is close to zero, and the 2019 coefficient (0.079, $p = .06$) is small and only marginally significant, indicating a modest but not clearly significant pre-trend. The 2021 coefficient (0.215, $p = .008$) is roughly three times the size of the 2019 figure and clearly significant, consistent with the credit-availability prediction: import-dependent firms expanded employment significantly more than less import-dependent firms specifically in the year following the deepest phase of the monetary easing cycle, beyond what a linear continuation of the modest pre-shock drift would predict. This pattern echoes [Gertler and Gilchrist's \(1994\)](#) finding that small, more credit-constrained manufacturing firms in the United States respond more sharply than larger firms to monetary policy, recovering faster during easing. The present result extends that logic from firm size to import dependence as the source of financing need, in a very different institutional setting, and is consistent with the bank-lending channel premise that the same policy action can have a materially larger effect on borrowers who depend more heavily on external, short-term financing ([Kashyap & Stein, 2000](#)). Figure 4 shows the same pattern directly in the raw, industry-demeaned data, without relying on the regression's functional form.

Binning establishments by import share, the employment-IMPS relationship in 2021 sits visibly above the 2017-2019 relationship at low-to-moderate import shares, matching the regression

coefficients rather than being an artefact of the linear specification chosen for the main regression. Table 2 reports the full regression underlying Figure 1, together with capital stock as a second outcome, and adds the inference check described in the method section above.

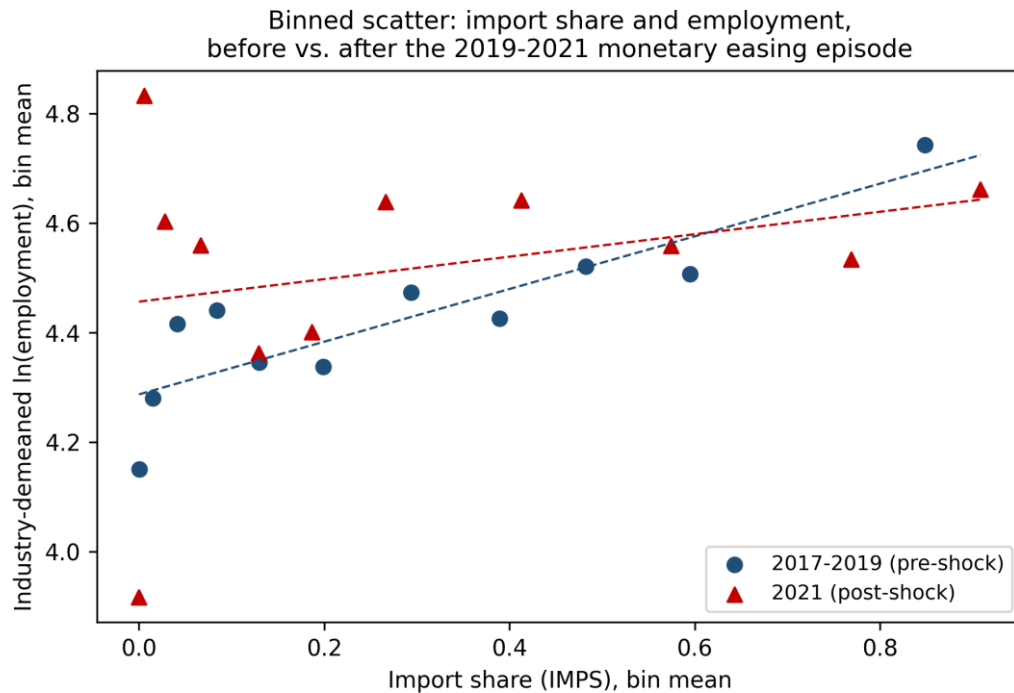


Figure 4. Binned Scatter: Import Share and Employment, Before vs. After the Shock

Table 2. Event-Study Coefficients: IMPS x Year (ref. = 2017), Cluster-Robust SE by Industry

Year (vs. 2017)	Employment (ln L)	Capital (ln K)
2018	0.020 (0.060)	0.139 (0.101)
2019	0.079* (0.042)	0.064 (0.102)
2021	0.215*** (0.081)	0.106 (0.141)

Note: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors clustered by KBLI two-digit industry in parentheses. Wild cluster bootstrap p-value (999 Rademacher replications) for the 2021 coefficient = 0.013. Controls (exporter status, foreign ownership share) and industry fixed effects are included but not shown.

Because the identifying variation comes from only 23-24 industry clusters, Table 2 also reports a wild cluster bootstrap p-value for the 2021 employment coefficient, following [Cameron et al. \(2008\)](#). At 0.013, it remains significant at the 5 percent level even under this more conservative small-cluster inference procedure, essentially confirming the analytic cluster-robust result ($p = .008$), so the headline result does not appear to be an artefact of unreliable small-sample inference. For capital stock, by contrast, none of the year-specific coefficients differs significantly from the 2017 baseline (2018: 0.139, $p = .17$; 2019: 0.064, $p = .53$; 2021: 0.106, $p = .45$), and the confidence intervals are wide throughout, consistent with the adjustment-cost prediction and with the smaller, noisier sample available for this outcome. This employment-capital split matches the more recent, directly comparable evidence in Kurt (2024) discussed above: the two outcomes examined here suggest that the margin of adjustment during the 2019-2021 episode was primarily the workforce rather than fixed capital, not because Indonesian import-dependent firms lacked the financing to expand capacity, but because employment is simply the faster-moving margin on which credit-constrained firms can respond within a short window.

A Direct Mechanism Test: Materials Purchases

If the credit-channel interpretation is correct, the most direct signature should appear in materials-purchase growth itself, since it is precisely the financing of imported input purchases that a working-

capital constraint would bind on; this is also the outcome closest in spirit to the trade-finance literature reviewed above, which typically measures financing effects directly in trade or purchase volumes rather than in downstream employment (Amiti & Weinstein, 2011; Chor & Manova, 2012). Table 3 reports the same event-study specification with materials purchases as the outcome.

The results are less supportive than the employment result. The 2018 coefficient is positive and

Table 3. Mechanism Test: Materials-Purchase Event-Study, IMPS x Year (ref. = 2017)

Year (vs. 2017)	Materials purchases (ln M)
2018	0.508* (0.265)
2019	0.547 (0.396)
2021	0.312 (0.420)

Note: * p<.10, ** p<.05, *** p<.01. Standard errors clustered by KBLI two-digit industry in parentheses. Wild cluster bootstrap p-value for the 2021 coefficient = 0.709.

marginally significant (0.508, p = .06), but neither 2019 (0.547, p = .17) nor, crucially, 2021 (0.312, p = .46; wild-bootstrap p = .71) is statistically distinguishable from the 2017 baseline. This is an important complication for the credit-availability prediction: the employment result does not have a clean, contemporaneous counterpart in the most direct available proxy for the financing mechanism itself, and it sits somewhat awkwardly alongside studies such as Amiti and Weinstein (2011) and Chor and Manova (2012), both of which find that financing conditions move trade and purchase volumes directly and sizeably, albeit during credit contractions rather than the easing episode studied here. A reading consistent with both sets of results is Manova's (2013) point that credit constraints often bind on the extensive margin, whether a firm trades at all, rather than on the intensive margin of how much an already-trading firm purchases. If so, an easing episode might draw new firms into importing, or let existing importers avoid cutting back, without producing a large, clean increase in the rupiah value of materials purchased by firms that were already importing. Employment may also simply respond to firms' expectations about future financing conditions faster than materials purchases respond to realised ones; the data cannot adjudicate between these explanations, and the result should temper how confidently the employment finding is attributed to the working-capital channel specifically rather than to a more general improvement in credit-constrained firms' operating condition.

Is the Response Linear in Monetary Conditions, or a Threshold Effect?

Table 4 reports the continuous monetary-stance specification, which replaces the year indicators with the actual level of the BI policy rate and credit growth in each year, and speaks to a question the event-study alone cannot answer: whether the 2021 result reflects a general, continuously operating sensitivity to monetary conditions of the kind assumed in most of the bank-lending channel literature (Kashyap & Stein, 2000), or something more specific to this particular episode.

Table 4. Continuous Monetary-Stance Specification (IMPS x Actual Policy Rate / Credit Growth)

Variable	Employment (ln L)	Capital (ln K)	Materials (ln M)
IMPS	1.004*** (0.204)	1.083*** (0.306)	1.278*** (0.315)
IMPS x BI policy rate	-0.022 (0.040)	0.018 (0.082)	0.326* (0.183)
IMPS x credit growth	-0.018** (0.009)	0.0005 (0.021)	-0.086 (0.078)

Note: * p<.10, ** p<.05, *** p<.01. Standard errors clustered by KBLI two-digit industry in parentheses. Year and industry fixed effects and controls included but not shown.

For employment, the interaction with the policy rate is small and insignificant (-0.022, p = .59), while the interaction with credit growth is negative and significant (-0.018, p = .03). This is the reverse of the sign a simple linear credit-channel story predicts, and it points the opposite way from the clear positive break documented in the event-study. This apparent contradiction is informative rather than merely puzzling, and it echoes the broader theme in Kurt's (2024) more recent firm-level evidence that

monetary policy can operate asymmetrically across the cycle, so that a single linear coefficient fitted across tightenings and easings alike need not recover the sharper response visible around one specific, unusually deep episode. The continuous specification here fits a single linear relationship across all four survey waves, and is accordingly pulled by the 2017-2019 sub-period, when credit growth was slowing from a 2018 peak while, in those earlier waves, the IMPS-employment relationship was not moving in the same direction. The event-study result, a sharp divergence specific to 2021 rather than a smooth trend, together with the continuous specification's failure to fit a single linear relationship across the full window, suggests a threshold or nonlinear effect. That effect is concentrated in the depth of the 2019-2021 easing episode, the record-low 3.50 percent rate together with the sharp 2020 contraction and 2021 recovery, rather than a smooth function of the policy rate across ordinary cyclical fluctuations. For materials purchases, by contrast, the interaction with the policy rate is positive and marginally significant (0.326, $p = .07$), again not aligned with the employment result, reinforcing the conclusion from the mechanism test above that the two outcomes are not telling a fully consistent quantitative story even where both show statistically suggestive patterns.

Heterogeneity: Who Drives the Result?

Table 5 re-estimates the main employment event-study's 2021 coefficient separately by firm size, exporter status, and ownership, testing the credit-constraint heterogeneity prediction directly rather than inferring it from the pooled coefficient alone. Each row of the table is a separate regression on the indicated subsample, so the comparison is between the significance and magnitude of the 2021 coefficient across subsamples, not between a single interacted coefficient and its components.

Table 5. Heterogeneity: 2021 Employment Coefficient by Subgroup

Subgroup	IMPS x year=2021 (vs. 2017)	N
Small firms	0.422*** (0.050)	60,412
Large firms	0.189*** (0.071)	61,179
Non-exporters	0.270** (0.114)	111,180
Exporters	-0.167 (0.154)	10,411
Domestically owned	0.260** (0.114)	110,525
Foreign-owned	0.099 (0.108)	11,066

Note: * $p < .10$, ** $p < .05$, *** $p < .01$. Each row is a separate regression estimating the full event-study specification on the indicated subsample; standard errors clustered by KBLI two-digit industry in parentheses.

The pattern is coherent across all three dimensions. The effect is roughly twice as large among small firms (0.422, $p < .001$) as among large firms (0.189, $p = .007$), though both remain significant, closely matching the size gradient documented for United States firms by [Gertler and Gilchrist \(1994\)](#) and, more recently, by [Kurt \(2024\)](#). It is significant among non-exporters (0.270, $p = .02$) but not among exporters (-0.167, $p = .28$, a wrong-signed point estimate). This fits [Amiti et al. \(2014\)](#) finding that firms that both import and export have much lower exchange-rate pass-through, since movements in imported-input costs partially offset export revenue; exporters may also have foreign-currency liquidity that reduces their reliance on domestic credit relative to firms that import without exporting. Finally, the effect is significant among domestically owned firms (0.260, $p = .02$) but not among foreign-owned firms (0.099, $p = .36$), consistent with [Antras et al. \(2009\)](#) account of multinational firms drawing on internal capital markets that substitute for local bank credit, so that a domestic monetary easing episode has less to offer a foreign-owned establishment than it does an otherwise similar domestically owned one. In every comparison, the significant response is concentrated in the firms with the least alternative access to financing: smaller firms, firms without a foreign parent's internal capital market, and firms without export earnings or trade-credit relationships of their own. This is exactly the pattern the credit-constraint heterogeneity prediction anticipates if the underlying mechanism is a domestic credit constraint rather than a general trade-exposure effect, and this consistency across three independent dimensions of financial access, each separately grounded in an existing strand of the literature reviewed above, is on its own more supportive of the credit-channel interpretation than the headline pooled coefficient alone.

Robustness: Firm and Cohort Fixed Effects, and a Placebo Check

Two of the main specification's limitations noted above, that import share cannot be linked to the same establishment across waves, and that the identifying variation comes from a small number of industry clusters, are properties of the data rather than choices that a different econometric specification alone can fully resolve. Three additional checks nonetheless probe how much these limitations actually matter for the paper's main conclusion, using the sharpest tools the data structure allows.

The first check exploits an underused feature of the data: although establishment identifiers cannot be linked between the 2019 and 2021 waves, BPS's identifiers are in fact consistent across 2017, 2018, and 2019, where 29,474 establishments are observed in at least two of these three years. Within this linkable window, a true firm fixed-effects version of the event-study (equation 1, restricted to 2018 and 2019 relative to 2017, with establishment fixed effects replacing industry fixed effects) tests directly whether the small pre-shock drift documented in Table 2 survives controlling for time-invariant, establishment-specific heterogeneity. It does: the IMPS x 2019 coefficient, itself only marginally significant in the pooled specification restricted to this same linkable subsample (0.017, $p = .66$), becomes larger and more precisely estimated under firm fixed effects (0.029, $p = .007$). This check cannot be extended to 2021, since that wave's establishment identifiers are not linkable to the earlier ones, so it speaks only to the pre-trend rather than to the headline 2021 result; but it indicates that the modest pre-existing drift is a real pattern in the data rather than an artefact of unobserved establishment heterogeneity, within the window where such heterogeneity can be directly controlled for.

The second check brings a fixed-effects-style control into a specification that does include 2021, using a pseudo-panel rather than individual establishments as the unit tracked over time (Deaton, 1985; Verbeek & Nijman, 1992). Establishments are grouped into 72 cells defined by industry and within-year size tercile, each cell is observed in all four survey waves, and the resulting cell-year panel, weighted by the number of establishments each cell-year average represents, is estimated with cell fixed effects, which absorb time-invariant heterogeneity at the industry-size-tercile level in the same way establishment fixed effects would absorb it at the individual level. The IMPS x 2021 coefficient is not only preserved but larger under this specification (0.533, $p < .001$) than in the main pooled result (0.215, $p = .008$), while the 2018 and 2019 coefficients remain statistically indistinguishable from zero, matching the pattern in Table 2. Because only 72 cells are available, standard errors here use a heteroskedasticity-robust rather than a cluster-robust estimator, itself a limitation of the pseudo-panel approach, so the result should be read as a directional robustness check rather than a direct, statistically equivalent substitute for a true establishment-level panel. Taken together with the first check, it indicates that the main result is not an artefact of pooling observations that happen to differ systematically, and unobservably, by industry-size cell.

Table 6. Robustness: Firm and Cohort Fixed Effects, and a Placebo Interaction

Check	Year (vs. 2017)	Coefficient (SE)	N
(a) Firm FE, 2017-2019 window	2018	0.006 (0.014)	82,520
(a) Firm FE, 2017-2019 window	2019	0.029*** (0.011)	82,520
(b) Pseudo-panel cell FE, all years	2018	-0.136 (0.105)	288
(b) Pseudo-panel cell FE, all years	2019	-0.018 (0.082)	288
(b) Pseudo-panel cell FE, all years	2021	0.533*** (0.118)	288
(c) Placebo: wage ratio x year	2018	-0.052* (0.031)	103,594
(c) Placebo: wage ratio x year	2019	-0.071** (0.033)	103,594
(c) Placebo: wage ratio x year	2021	0.020 (0.035)	103,594

Note: significance levels * 10 percent, ** 5 percent, *** 1 percent. Panel (a) restricts the sample to establishments linkable across 2017-2019 and observed in at least two of those years, with establishment fixed effects; standard errors clustered by industry. Panel (b) is estimated on 72 industry x size-tercile cells observed in all four survey waves, weighted by establishments per cell-year, with cell fixed effects; HC1 robust standard errors (too few cells for reliable cluster-robust inference at this level of aggregation). Panel (c) interacts the production/non-production wage ratio, rather than import share, with year; standard errors clustered by industry.

The third check addresses a different concern directly: that any firm characteristic interacted with the 2021 wave might show a similar pattern simply because the 2021 observations differ from 2017-2019 for reasons unrelated to credit conditions, for instance, a general post-pandemic recovery

common to all firms. If so, the IMPS result would not be specific evidence of a credit channel. As a placebo, the main specification is re-estimated interacting year indicators with the production/non-production wage ratio, a skill-composition measure with no theoretical connection to credit-channel exposure under any of this paper's three predictions. At 2021, the year the paper's own result turns significant, the placebo interaction is small and statistically indistinguishable from zero (0.020, $p = .57$), unlike the IMPS interaction. The placebo is significant in 2018 and 2019 ($p = .10$ and $p = .03$), which we cannot explain and report rather than omit. Because the substantive claim rests on 2021, where the placebo is null, the check remains informative against a generic 2021-wave effect, although it is not uniformly clean across all four years.

Limitations

Two limitations deserve explicit discussion, both of which qualify how the results above should be read rather than undermining the headline finding itself. First, BPS did not survey establishments in 2020, so the panel cannot observe the trough of the credit contraction (-2.41 percent aggregate credit growth) or the sharpest point of currency depreciation directly. The 2021 coefficient therefore captures the combined effect of the full 2019-2021 window, which includes both the 2020 contraction and the 2021 recovery, rather than isolating either phase, and the inability to fit a stable linear relationship across waves documented above may partly reflect this missing observation rather than a genuine threshold in the underlying economic relationship. Second, the annual-average exchange rate had largely reverted by 2021, so while a currency-cost channel cannot be ruled out as a contributor to the 2019 and 2020 dynamics not directly observed here, it is unlikely to explain a level shift that is still present, and indeed larger, in the 2021 wave itself. The policy rate and credit-growth series, both of which show a persistent, non-reverting change over the same window, are accordingly the more plausible drivers of the result, but the design cannot fully separate the interest-rate channel from the credit-availability channel using aggregate, economy-wide monetary series that move together over this short window, a limitation shared with much of the bank-lending channel literature that relies on aggregate rather than bank-specific or firm-specific measures of credit conditions (Kashyap & Stein, 2000).

Conclusion and Recommendations

This paper documents that Indonesian manufacturing establishments with greater reliance on imported inputs expanded employment significantly more than less import-dependent establishments during 2021, following Bank Indonesia's deepest monetary easing cycle on record, a divergence that survives a wild cluster bootstrap and is roughly twice as large among small firms as among large firms, present among domestically owned but not foreign-owned firms, and present among non-exporters but not exporters. This consistency across three independent dimensions of financial access supports a credit-constraint interpretation rather than a general trade-exposure story. Two complications qualify it, however: the materials-purchase test does not show the same divergence found in employment, and the continuous specification cannot fit a stable linear relationship across the full window, suggesting a threshold effect tied to the depth of this episode rather than a general, continuously operating credit channel. Capital stock shows no differential response in any specification, consistent with capital's higher adjustment costs relative to labour.

Two further checks, reported in the Results section, address the main specification's two most consequential data limitations directly: a true establishment fixed-effects estimate over the subset of waves where establishment identity can be tracked (2017-2019) confirms the modest pre-shock drift is not an artefact of unobserved firm heterogeneity, and a pseudo-panel specification using industry-size cohorts rather than individual establishments, which can be extended to include 2021, finds the main result preserved and, if anything, larger. A placebo interaction using an economically unrelated firm characteristic is null specifically in 2021, the year the paper's claim rests on, weakening the alternative explanation that any characteristic interacted with the 2021 wave would show a similar pattern. None of these checks resolves the underlying data limitation that individual establishments cannot be linked to 2021; they instead test, as directly as the available data permit, how much that limitation actually threatens the paper's central conclusion, and on each test the conclusion survives.

The findings carry two policy implications, tempered by the complications above. First, because the differential response concentrates in employment rather than investment, and concentrates

specifically among small, domestically owned, non-exporting firms, credit-easing measures aimed at supporting manufacturing activity should not be expected to translate proportionately into capacity expansion, and are likely to reach the credit-constrained segment of import-dependent firms most directly rather than firms with access to trade credit, export earnings, or a foreign parent's internal capital. Second, because the response appears concentrated in a specific, unusually deep easing episode rather than behaving as a smooth function of ordinary policy-rate movements, policy that relies on this channel to transmit smoothly and predictably at all points of the monetary cycle may be overstating its reach outside comparably large easing episodes.

The paper's main limitation is data availability. The missing 2020 survey wave prevents direct observation of the credit trough and the sharpest currency depreciation, plausibly contributing to the difficulty of fitting a single linear relationship across waves, and the aggregate, economy-wide monetary series used here move together over this short window in a way that limits the ability to separate the interest-rate channel from the credit-availability channel cleanly. The gap between the employment result and the materials-purchase mechanism test is itself a finding that warrants further investigation: future work with a firm-level crosswalk into the 2020 wave, or with bank-firm matched credit data as in [Paravisini et al. \(2015\)](#), could test directly whether hiring in this episode reflected realized financing of input purchases, anticipated future financing conditions, or a broader improvement in credit-constrained firms' operating environment not confined to the import channel specifically.

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