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Formal Credit and SME Performance: Reviewing Evidence from Impact Evaluations

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Abstract

This paper reviews the impact evaluation evidence on how expanding formal credit affects small and medium enterprise (SME) performance, focusing on developing countries, particularly Latin America, while using evidence from advanced economies to sharpen mechanisms and external validity. Across the core evidence base, the meta-analytic mean effects of formal loans are economically meaningful: employment rises by about 12% on average, sales by about 18%, and profits by about 18%, albeit with substantial heterogeneity across settings, programs, and outcome horizons. The effects are larger in some large-scale public interventions and in programs explicitly targeting constrained firms but smaller or statistically indistinguishable from zero in others. Taken together, the evidence supports the view that relaxing financial constraints can raise SME scale and performance, but it also underscores that credit expansions are not automatically welfare improving, can create distributional and competitive spillovers, and are sensitive to program design, targeting, and local financial architecture.

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1 Introduction

Policy interest in small and medium enterprise (SME) finance rests on a straightforward development narrative: SMEs are thought to be important engines of job creation, innovation, and local economic dynamism but are hindered by frictions that limit their access to external finance. If credit constraints bind, expanding access to formal loans should allow high-return projects to be financed, raise capital accumulation, increase working capital, enable the adoption of productivity-enhancing technologies, and ultimately increase firm growth and employment. Yet two decades of empirical work have produced a more nuanced picture. In many environments, lending interventions deliver meaningful gains in some outcomes and some firm segments but not others; effects can be short-lived; and in settings with weak screening, volatile macro conditions, or intense local competition, credit can generate limited net gains—or even losses—for average borrowers.

This paper synthesizes the impact evaluation evidence on SME credit expansions, with a focus on developing countries. It is anchored in the set of evaluations consolidated by Bruhn et al. (2025b), who offer a rare apples-to-apples meta-analytic mapping of impacts on employment, sales, and profits across a broad set of interventions. The evidence base includes randomized controlled trials (RCTs), regression discontinuity designs (RDDs) and difference-in-differences (DiD) designs exploiting eligibility thresholds or staggered rollouts, and quasi-experimental evaluations leveraging credit supply shocks through banks and public programs. The resulting literature is unusually well suited to answering a practical policy question: if a government, donor, or financial intermediary expands formal loans to SMEs, what should be expected to happen to jobs, revenues, and profits?

The paper also extends the evidence base in five directions, drawing on five newer contributions. First, it adds macro-policy-driven credit easing in Iran during 2005–2013 (Amini and Salehi Esfahani, 2025), which provides insight into credit expansions embedded in broader expansionary policy shifts. Second, it adds new RCT evidence on joint "training plus loans" interventions among Kenyan SMEs (Ashraf and Lyons, 2026), sharpening the question of complementarities between finance and managerial capital. Third, it adds evidence on fintech-enabled lending and collateral valuation (Beaumont et al., 2025), which speaks to whether new data and lending technologies can relax collateral constraints and change the composition of borrowers and credit terms. Fourth, it adds evidence on movable collateral registries from Malawi (Pendame and Akotey, 2023), which is directly relevant to a prominent reform agenda in low-income countries aimed at unlocking credit using movable assets. Fifth, it adds evidence on credit guarantees from China (Yu et al., 2022), which quantifies

how guarantees translate into borrowing, investment, innovation, and productivity.

Two themes motivate the structure of the paper. The first is identification: credible evaluation of credit requires separating changes in credit supply from changes in credit demand and addressing endogenous matching between firms and lenders. The corporate credit supply literature emphasizes precisely these challenges and develops empirical strategies that the SME finance evaluation literature increasingly shares, especially as credit registry data and matched bank–firm panels become more common (Güler et al., 2021). The second theme is heterogeneity: the effects of credit expansions depend on which firms receive loans, what terms they receive, whether loans crowd out other funding, how competitive local markets are, and what complementary constraints (skills, demand, infrastructure, governance) remain binding.

The remainder of the paper proceeds as follows. Section 2 develops a conceptual framework linking credit access to SME performance and clarifies why impacts can be positive, small, or negative. Section 3 describes the evaluation landscape and the main identification strategies used in impact evaluations of SME credit access, adapting organizing principles from the corporate credit supply literature. Section 4 synthesizes average impacts on employment, sales, and profits and interprets their economic significance. Section 5 reviews heterogeneity by intervention type, institutional channel, and firm segment, emphasizing developing country evidence while using advanced economy studies to illuminate mechanisms and external validity. Section 6 focuses on mechanisms, complementary constraints, macroeconomic context, and market-level spillovers. Section 7 consolidates policy implications and priorities for future research, and Section 8 concludes.

2 Conceptual framework: How credit affects SMEs

2.1 Credit constraints as a contracting problem inside the firm

A firm-level microeconomic view starts from a simple premise: many SMEs have projects whose efficient scale exceeds the scale they can finance because external finance is governed by contracts written under imperfect information and imperfect enforcement. When lenders cannot fully observe borrower type ex ante or borrower actions ex post, credit markets may not clear through price alone. In the classic credit rationing logic, higher interest rates can worsen the borrower pool (adverse selection) or induce riskier behavior (moral hazard), so lenders optimally cap quantities rather than "price to clear" (Stiglitz and Weiss, 1981). Agency-based models similarly emphasize that outside finance is limited by what

can be credibly promised and monitored; the binding friction is often not willingness to pay but pledgeability and intermediary capital/monitoring constraints (Holmstrom and Tirole, 1997). This is consistent with the paper's Section 2 framing that high-return firms can still be rationed when lenders cannot observe borrower quality or enforce repayment.

In that environment, the "credit channel" operates through what happens inside the firm when financing constraints bind. SMEs frequently face working capital needs (inventory, payroll, input purchases) as well as lumpy investments (equipment, technology adoption). When credit is scarce or poorly matched to cash flow timing, the firm may operate below capacity, forgo profitable investments, or adopt suboptimal production plans. Relaxing the constraint—whether by increasing loan size, extending maturity, or lowering collateral requirements—should permit working capital and investment to expand, which can translate into higher output, employment, sales, and profitability. This paper's mechanism is therefore fundamentally micro: credit affects firm outcomes by expanding the feasible set of input choices and investment paths.

A second micro implication, also emphasized in this paper, is heterogeneity. Even where constraints are common among SMEs, the marginal return to capital is not uniformly high; outcomes depend on demand conditions and complementary factors such as managerial capability, market access, and infrastructure. Recent experimental work is consistent with this. Large credit expansions can produce modest average effects while hiding very large gains for a subset of firms and losses for others, underscoring that the productivity of borrowed funds is highly firm specific (Bryan et al., 2024).

2.2 Why average effects can be small: Selection, screening, and loan terms

Once the problem is understood as a contracting and selection problem, it becomes clear why measured average impacts of "more credit" are often modest. Even if credit would substantially raise outcomes for some constrained, high-return firms, actual expansions in lending are mediated by lender screening rules, borrower take-up decisions, and the contract terms that govern repayment and behavior. The paper points directly to this logic. Specifically, programs may increase lending to firms that are only marginally constrained or have limited growth opportunities, particularly when lenders weight repayment capacity more heavily than growth potential. This is a core micro point: a lender's private objective (repayment with low default) does not necessarily select the borrowers with the highest productivity impact.

Loan design also matters because "access" is not binary. If a program reaches riskier or more opaque borrowers, lenders may respond by tightening non-price terms (e.g., smaller loan sizes, shorter maturities, more collateral, or stricter covenants) to contain default risk. From a firm-level standpoint, these terms determine whether credit meaningfully relaxes the relevant constraint. For example, a short-maturity loan may help smooth inventories but may not finance a lumpy machine purchase with a longer payback horizon. Conversely, guarantees and other risk-sharing tools can relax collateral constraints, but they can also weaken screening and monitoring incentives if downside risk is shifted away from the lender (Jensen and Meckling, 1976). The broader SME finance literature highlights precisely these lending technology trade-offs—relationship lending, collateral-based lending, credit scoring, and guarantees—as alternative ways to manage informational and agency frictions (Petersen and Rajan, 1994; Berger and Udell, 2006).

This selection-and-terms perspective is also consistent with firm-level evidence from directed credit programs. A key result is that cheaper or easier credit can be absorbed by both constrained and unconstrained firms, but only constrained firms should expand production materially; unconstrained firms may largely substitute across funding sources (Banerjee and Duflo, 2014). In other words, even substantial credit supply shifts can yield limited average real effects if a meaningful share of borrowers were not at a binding constraint in the first place or if the new loans primarily refinance existing obligations.

2.3 Dynamic firm responses and competitive interactions

This paper also emphasizes that credit can sometimes harm firms, which is entirely consistent with standard firm-level microeconomics once repayment obligations and product market competition are considered. If borrowed funds are deployed in low-return activities or into projects whose payoffs are too uncertain relative to repayment schedules, debt service can compress cash flow and raise exit risk. In corporate finance terms, leverage can create or amplify agency conflicts—risk shifting and underinvestment incentives—especially when downside risk is borne disproportionately by creditors (Jensen and Meckling, 1976). Relatedly, when firms have valuable growth opportunities, heavy debt can generate debt-overhang-type distortions that reduce incentives to undertake otherwise positive investments (Myers, 1977). These mechanisms provide microfoundations for why some firms may experience profit declines or heightened volatility after receiving credit, even if sales or scale increase.

Finally, a firm-level narrative must account for competition. When credit relaxes capacity

constraints, treated firms can expand output and potentially cut prices, which may erode margins not only for themselves but also for nearby competitors. This paper highlights this "business stealing" channel explicitly and points to recent market-level experimental evidence documenting indirect effects on competing firms (Cai and Szeidl, 2024). This is the key interpretive lesson: firm-level gains do not automatically aggregate into market-level gains if credit mainly reallocates demand and markups within a fixed local market rather than expanding demand or productivity. Evidence from U.S. small business lending programs also supports the view that credit can affect real outcomes such as employment and growth, but the magnitude and incidence depend on where constraints bind and on local credit and market conditions (Brown and Earle, 2017). A lack of competition in banking can also reduce the availability of credit and raise interest rates, dampening investment, job creation, business dynamism, and ultimately economic growth (Alviarez et al., 2025).

3 Evidence base and empirical strategies

3.1 What counts as an impact evaluation of SME credit access?

The studies reviewed in this paper evaluate interventions that expand access to formal loans for SMEs, including loans provided directly by public entities, loans intermediated through private banks with public support, and guarantees that change banks' willingness to lend. The core sample follows Bruhn et al. (2025b), who restrict attention to studies that identify positive expansions of formal credit and report impacts on employment, sales, and/or profits, measured typically within one to four years.

The core sample includes 24 studies, spanning low- and middle-income countries (LMICs) such as Ethiopia, Colombia, India, Brazil, Ecuador, Türkiye, and Egypt, as well as upper-middle- and high-income settings including France, Korea, Spain, and several European Union member states. This mix is useful, as it allows developing country policy relevance while enabling cross-context learning about institutional channels and market structure.

3.2 Identification challenges: Supply versus demand and endogenous matching

The empirical objective of an impact evaluation is to estimate how an exogenous expansion in formal credit affects subsequent firm performance. Although the precise specification varies

across studies and research designs, the central estimating equation can be represented as

$$Y_{it} = \alpha + \beta C_{it} + \mathbf{X}'_{it}\gamma + \mu_i + \lambda_t + \varepsilon_{it}, \tag{1}$$

where Y_{it} is an outcome for firm i in period t, such as employment, sales, profits, investment, productivity, or survival; C_{it} measures access to or use of formal credit; $\mathbf{X}_{it}$ is a vector of observed firm and market characteristics; μ_i denotes firm fixed effects when panel data are available; λ_t denotes time fixed effects; and ε_{it} captures unobserved determinants of firm performance. Depending on the intervention, C_{it} may be an indicator for receiving a loan, eligibility for a credit program, exposure to a guarantee or development bank credit line, or the amount of credit obtained. The parameter of interest, β, is the causal effect of the credit expansion on firm performance.

Equivalently, in the potential outcomes framework, each firm has an outcome under expanded credit access, $Y_i(1)$, and an outcome in the absence of the expansion, $Y_i(0)$. The principal estimand is therefore

$$\beta = \mathbb{E}\left[Y_i(1) - Y_i(0)\right], \tag{2}$$

or an analogous treatment effect for the population affected by a particular program or identification strategy. The fundamental evaluation problem is that only one of these two potential outcomes is observed for each firm. The observed performance of firms that obtain credit cannot therefore be directly compared with that of firms that do not obtain credit unless the latter provide a credible estimate of the borrowers' counterfactual outcomes.

A direct ordinary least squares estimation of Equation (1) will generally not identify β because credit access and firm performance are jointly determined. Three related sources of endogeneity are particularly important.

First, *selection bias* arises because firms are not randomly assigned to borrowing. Firms decide whether to apply for credit, accept a loan offer, and comply with the conditions attached to the loan. More ambitious, productive, financially sophisticated, or growth-oriented entrepreneurs may be more likely to seek and obtain formal finance. At the same time, lenders screen applicants and direct loans toward firms that appear more likely to repay. Borrowers and non-borrowers may therefore differ systematically even before the credit intervention. Conversely, firms receiving public or subsidized credit may have been selected precisely because they were financially distressed or unable to obtain conventional loans. The direction of the resulting bias is consequently ambiguous: positive selection can

make credit appear more effective than it is, while targeting weaker firms can attenuate or reverse the estimated relationship.

Second, *omitted variable bias* arises when unobserved factors affect both credit access and firm outcomes. Entrepreneurial ability, managerial practices, risk preferences, informal financial resources, customer networks, investment opportunities, local demand, infrastructure, political connections, and the quality of borrower–lender relationships are difficult to measure comprehensively. For example, a firm with a favorable demand shock may simultaneously expand employment and become more attractive to lenders. If the demand shock is not observed, part of its effect will be incorrectly attributed to credit. Firm fixed effects can remove time-invariant unobserved heterogeneity, but they do not address time-varying shocks that are correlated with both borrowing and performance.

Third, *simultaneity and reverse causality* arise because credit and firm performance can affect one another within the same period. Credit may allow a firm to invest, purchase inputs, and hire workers, but current or anticipated growth may also increase the firm's demand for finance and the lender's willingness to supply it. A rise in sales may generate a need for working capital, while an expected decline in profitability may lead a firm to borrow to cover liquidity shortages. Observed loan growth can therefore reflect either an exogenous relaxation of financing constraints or an endogenous response to changes in the firm's economic prospects. This is the central supply-versus-demand identification problem emphasized in the corporate credit literature: a positive association between borrowing and performance does not reveal whether greater credit caused firm growth or whether firms borrowed more because they were already growing.

The impact evaluation literature addresses these problems by identifying variation in credit access that is plausibly unrelated to firms' underlying potential outcomes. RCTs assign loan offers, eligibility, contract terms, or loan size increases across otherwise comparable firms. In their simplest form, these studies estimate an intention-to-treat specification,

$$Y_{it} = \alpha + \delta Z_i + \mathbf{X}_{i0}'\gamma + \lambda_t + \varepsilon_{it}, \tag{3}$$

where Z_i denotes randomized assignment to the credit intervention. Because assignment is random, δ identifies the causal effect of being offered or assigned the intervention. When take-up is incomplete, assignment can also be used as an instrument for actual borrowing, producing a local average treatment effect for firms whose borrowing decisions are changed by the intervention.

Quasi-experimental evaluations exploit program rules or implementation processes that

create plausibly exogenous differences in access to finance. RDDs compare firms located just above and below an eligibility or credit score threshold. Provided that firms cannot precisely manipulate their position around the cutoff and potential outcomes vary smoothly through it, the resulting discontinuity identifies a local causal effect of credit eligibility or approval. DiD designs compare changes in outcomes for firms affected by a program with contemporaneous changes for an unaffected comparison group:

$$Y_{it} = \alpha + \beta \left(\text{Treated}_i \times \text{Post}_t\right) + \mu_i + \lambda_t + \mathbf{X}'_{it}\gamma + \varepsilon_{it}. \tag{4}$$

Here, β measures the differential change in outcomes following credit expansion. Its causal interpretation requires, among other conditions, that treated and comparison firms would have followed parallel outcome trends in the absence of the intervention. Matching and reweighting methods are often used to improve comparability between the two groups, though they address selection only on observed characteristics and are generally most credible when combined with a research design based on timing, eligibility rules, or other plausibly exogenous variation.

A third group of studies exploits lender-side or policy-induced credit supply shocks. These designs use changes in bank funding, exposure to development bank credit lines, guarantee eligibility, local bank presence, regulatory rules, or other shifts in lenders' capacity or incentives to extend credit. Such shocks can isolate the supply component of borrowing if they affect firm outcomes primarily through their effect on the availability of credit and are not themselves correlated with changes in firms' investment opportunities. In matched bank–firm data, researchers can further separate credit supply from firm demand by comparing lending by differently exposed banks to the same firm or by including firm-by-time fixed effects that absorb contemporaneous changes in firm-level credit demand. Instrumental variable approaches similarly use the exogenous component of credit generated by the supply shock to estimate the effect of additional borrowing on firm outcomes.

These strategies identify somewhat different causal parameters. Randomized assignment commonly identifies an intention-to-treat effect or a treatment effect for firms whose borrowing responds to the randomized offer. Regression discontinuity estimates are local to firms near an eligibility threshold. DiD estimates apply to firms affected by a particular policy change under the parallel trends assumption. Credit supply instruments identify effects for borrowers whose credit changes because of the relevant lender or policy shock. Consequently, differences in estimates across studies may reflect differences not only in program design and institutional context but also in the firms and margins of credit access for which the causal

effects are identified. The following subsections examine these approaches and their principal strengths and limitations in greater detail.

3.3 RCTs of loan offers and loan-size expansions

RCTs address the endogeneity problems discussed in Section 3.2 by randomly assigning firms to different credit offers, eligibility conditions, loan amounts, or contract terms. Because treatment status is determined by the experimental design rather than by firms' borrowing decisions or lenders' assessments, treated and control firms should be comparable before the intervention. Differences in their subsequent outcomes can therefore be interpreted as the causal effect of the credit intervention.

Some experiments randomize access to a loan or relax an eligibility requirement, thereby evaluating the extensive margin of formal credit access. Others randomize the size of loans offered to firms that already qualify for or use formal finance, allowing researchers to estimate whether a further relaxation of the financing constraint improves firm performance. For example, Alibhai et al. (2022) introduce experimental variation into loan eligibility among women entrepreneurs in Ethiopia. Bryan et al. (2024) instead randomize larger loan offers among existing borrowers in Egypt, providing evidence on the effects of expanding credit at the intensive margin.

Experimental designs can also distinguish the effect of credit from the effect of complementary support. Studies may assign firms to receive loans, business training, both interventions, or neither. These designs help determine whether limited managerial capacity reduces the returns to finance and whether combining credit with technical assistance generates larger effects than either intervention alone. The training-and-loan intervention evaluated by Ashraf and Lyons (2026) is particularly relevant to this question.

The main strength of randomized experiments is their high internal validity. Nevertheless, their interpretation requires attention to incomplete loan take-up, borrowing by control firms from alternative sources, attrition, firm exit, and spillovers between treated and untreated businesses. Their external validity may also be limited because experiments often involve a particular lender, narrowly defined applicants, and a specific credit product. Moreover, most follow firms for only a few years and may therefore miss longer-run consequences such as repeated borrowing, repayment difficulties, debt overhang, or changes in firm survival.

3.4 Quasi-experimental DiD designs and RDDs

Random assignment is often impractical for large public lending programs, guarantees, regulatory reforms, and development bank credit lines. In these cases, researchers use quasi-experimental variation generated by program timing, eligibility criteria, geographic boundaries, or lender rules. DiD designs and RDDs are especially common in the SME credit literature.

DiD designs compare changes in firm outcomes before and after a credit intervention with contemporaneous changes among firms that were not affected by the intervention. Firm fixed effects can remove persistent differences between treated and comparison firms, while time fixed effects absorb aggregate shocks affecting all firms. The resulting estimate is credible when treated and comparison firms would have followed similar trends in the absence of the program.

The main threat to this approach is that program participation or implementation may coincide with other changes affecting firm performance. Credit programs may target industries, regions, or firms experiencing unusually strong or weak growth, making it difficult to separate the credit effect from pre-existing trends, mean reversion, or other policies. Researchers therefore commonly examine pre-intervention trends, use firms with similar observable characteristics as a comparison group, and control for sectoral, regional, and firm-level factors. Matching can improve comparability, but it cannot by itself eliminate selection based on unobserved characteristics.

DiD methods are particularly useful when programs are introduced gradually or firms receive credit at different dates. Several Latin American studies apply this general approach to guarantees and development bank credit lines, including evaluations from Colombia, Argentina, and Ecuador (Arraiz et al., 2014; Clemente et al., 2023; Eslava et al., 2014; Bruhn et al., 2025a). Banerjee and Duflo (2014) similarly exploit a policy-induced change in directed lending eligibility in India to compare firms differentially affected by the reform. In these applications, identification depends on whether non-beneficiary firms provide a credible counterfactual for how beneficiary firms would have evolved without the expansion in credit.

RDDs exploit program or lender rules that determine eligibility using a threshold. These thresholds may be based on firm size, revenues, employment, credit scores, or another continuously measured indicator. Firms located just above and below the cutoff are likely to be similar but may face sharply different probabilities of obtaining credit. Comparing their outcomes can therefore isolate the causal effect of eligibility or loan approval for firms close

to the threshold.

The credibility of an RDD depends on firms being unable to precisely manipulate their position relative to the cutoff and on other determinants of firm outcomes changing smoothly around it. Researchers must therefore assess whether firms alter reported employment, revenues, or other eligibility variables and whether lenders override formal rules using additional information. Because the estimates apply primarily to firms near the threshold, they may not generalize to firms that are clearly eligible or clearly ineligible. For example, Delis et al. (2025) use a bank credit score cutoff to estimate the effect of loan approval among marginal small business applicants, while Lelarge et al. (2010) exploit an eligibility rule in the evaluation of a French loan guarantee program.

3.5 Credit supply shocks

A third identification strategy exploits shocks that change lenders' ability or willingness to supply credit independently of changes in firms' demand for finance. These shocks may arise from changes in bank funding, regulatory requirements, development bank credit lines, public guarantees, branch networks, or lenders' exposure to financial and macroeconomic disturbances. The objective is to isolate credit variation originating on the lender or policy side rather than variation caused by firms' growth prospects or financing needs.

This approach is particularly relevant because observed borrowing reflects both credit supply and credit demand. A growing firm may borrow more because it needs working capital or investment finance, while a lender may simultaneously become more willing to lend because the firm's prospects have improved. A positive relationship between borrowing and subsequent performance would then not necessarily represent a causal effect of credit. Lender-side shocks provide a way to separate changes in credit availability from changes in underlying firm quality or investment opportunities.

Identification is stronger when researchers observe firms borrowing from multiple banks. They can then compare how differently exposed lenders change credit to the same firm, holding constant the firm's contemporaneous demand for finance. Matched bank–firm data and credit registries are therefore especially valuable for this type of evaluation. The corporate credit supply literature emphasizes this within-firm approach, and its methods are increasingly applied to SME finance as detailed lending data become available (Güler et al., 2021).

In developing countries, many credit programs operate through commercial banks or other financial intermediaries. Development banks may provide funding to participating

lenders, while guarantees may reduce the risks associated with lending to eligible SMEs. Variation in banks' exposure to these programs can generate plausibly exogenous differences in the credit received by otherwise similar firms. For example, Bazzi et al. (2023) use variation in intermediary bank presence and credit supply to evaluate Brazil's Cartão BNDES program. Related studies examine whether public funding or guarantees are passed through to borrowers and whether the resulting increase in lending produces changes in employment, investment, sales, or survival.

The validity of a credit supply design depends on the shock affecting firm outcomes primarily through credit. This condition may be violated if exposed banks operate in regions or sectors experiencing different economic shocks or if a policy simultaneously changes credit conditions and other forms of business support. Researchers therefore commonly control for firm, lender, sector, region, and time effects and test whether the results are associated with firms' dependence on the affected source of finance.

Credit supply designs frequently have greater institutional scale and external relevance than small lending experiments, but they may be less transparent in their identifying assumptions. They also tend to estimate effects for firms connected to particular lenders or affected by a specific policy shock. As with RCTs and quasi-experimental eligibility designs, the estimated effect should therefore be interpreted in relation to the firms, lenders, and margin of credit access affected by the underlying source of variation.

4 Impacts of formal loans on SME performance

4.1 Meta-analytic mean effects on employment, sales, and profits

Bruhn et al. (2025b) synthesize the core evidence using a Bayesian hierarchical model, expressing outcomes as percentage changes to facilitate comparability across studies. The central estimates are economically meaningful: employment increases by about 12% on average, with a 95% interval roughly from 8% to 16% in the meta-analytic estimate. Sales increase by about 18%, with a 95% interval roughly from 7% to 30%. Profits increase by about 18% on average, though the uncertainty around profits is larger and the credible interval includes relatively modest gains at the lower end.

Interpreting these magnitudes requires grounding in typical SME baselines. Consider a micro-to-small firm employing 10 workers. A 12% increase corresponds to roughly 1.2 additional jobs—economically meaningful for the firm and potentially meaningful at scale if applied to a large beneficiary population. For a firm with annual sales of USD 100,000, an

18% increase corresponds to USD 18,000 in additional revenues. If margins are 10%, this corresponds to USD 1,800 in additional operating profit; if margins are 20%, it corresponds to USD 3,600. The 18% profit effect reported in the meta-analysis suggests that, at least in successful programs, loans often finance activities with returns that exceed financing costs, especially where subsidized terms or guarantees lower effective borrowing costs.

However, averages conceal substantial heterogeneity. Some study-level estimates are very large, such as sales increases above 70% in a large-scale Turkish credit guarantee expansion (Akcigit et al., 2024), while others are small or indistinguishable from zero. The presence of both big successes and near-null results is itself one of the most policy-relevant features of the literature: "credit works" is not a complete conclusion without specifying "for whom, through what channel, and under what institutional conditions."

4.2 Heterogeneity by implementing institution and firm size

A striking result in the meta-analysis is that impacts differ meaningfully by whether loans are delivered through public versus private lenders. For employment, estimated effects are substantially larger when the implementing institution is a public bank (about 29%) than when it is a private bank (about 11%). This pattern is consistent with the idea that profit-maximizing lenders may not reach the most constrained, highest-return firms or may lend on terms that limit real expansion (e.g., smaller loan sizes, shorter maturities), while public programs may be better positioned to take risk or deliver larger credit expansions—though at the cost of potential misallocation or politicization.

Firm size targeting also matters. The meta-analytic employment effects are larger for smaller firms (≤ 10 employees), around 16%, compared to about 10% for larger SMEs (> 10 employees). This is consistent with the prior that smaller firms face tighter constraints and higher marginal returns to capital. It also highlights why "SME" is too broad a category for policy design; micro and small firms may respond more in percentage terms, though from a lower base.

4.3 Developing versus advanced economies

Interestingly, Bruhn et al. (2025b) find that average employment impacts are not dramatically different between developing and high-income countries: estimated effects are about 11% and 13%, respectively, with overlapping confidence intervals. This suggests that the presence of financing frictions is not exclusive to poorer economies. Yet the mechanisms can

differ. In high-income contexts, constraints may reflect informational and collateral frictions among young and small firms despite strong institutions, whereas in developing contexts, constraints can be compounded by weak contract enforcement, volatility, and limited competition among lenders. The similarity in average effect sizes should therefore not be read as equivalence in policy levers; rather, it underscores that well-designed credit expansions can matter across institutional environments.

4.4 Credit constraints facing SMEs in Latin America and the Caribbean

The average impacts discussed above must be interpreted in light of the financial environment in which SMEs operate. This is especially important in Latin America and the Caribbean (LAC), where access to formal credit varies substantially across countries, firm size, and local financial markets. Although the region includes relatively developed banking systems as well as much shallower financial markets, SMEs generally remain more dependent on banks than large firms because they have limited access to bond markets, equity finance, and other non-bank sources of long-term funding. Regional evidence indicates that smaller and less productive firms are more likely to be financially constrained, while greater bank penetration and competition are associated with fewer financing obstacles (Presbitero and Rabellotti, 2016; Herrera, 2020). Consequently, the effects of credit interventions in LAC depend not only on firm-level demand for finance but also on the ability and incentives of local financial institutions to reach smaller and more opaque borrowers.

A first constraint is the limited information that many SMEs can credibly provide to lenders. Smaller firms frequently lack audited financial statements, detailed business plans, formal credit histories, or sufficiently long records of revenues and tax payments. Informality intensifies this problem because firms that operate partly outside the tax and regulatory system cannot fully document their cash flows, assets, or liabilities. This informational opacity raises the fixed costs of screening and monitoring relative to the small size of the loans being requested. It also makes it difficult for lenders to distinguish firms with productive investment opportunities from firms with a high probability of default. Banks may respond by denying applications, offering smaller loans, charging higher interest rates, shortening maturities, or requiring stronger guarantees. Some viable firms may therefore become discouraged from applying because they expect rejection or consider the available terms too costly (Herrera, 2020). In this sense, limited observed borrowing need not indicate limited demand for finance; it may instead reflect rationing or self-exclusion in response to unfavorable lending

conditions.

A second constraint is the mismatch between the collateral required by lenders and the assets held by SMEs. Banks in the LAC region often place substantial weight on real estate and other conventional forms of collateral whose ownership and value can be readily verified. Young firms, service sector businesses, and firms whose assets consist primarily of machinery, inventory, receivables, or intangible capital may have profitable projects but insufficient pledgeable assets. Weaknesses in secured transactions frameworks, collateral registries, judicial enforcement, and insolvency procedures can further reduce the value that lenders attach to movable assets. These institutional weaknesses increase expected recovery costs and encourage banks to demand collateral whose value may exceed the loan amount (Herrera, 2020). Credit guarantees and improvements in collateral frameworks are intended to address this problem, though their effectiveness depends on whether they induce additional lending while preserving lenders' incentives to screen and monitor borrowers.

A third set of constraints originates in the structure and geographic reach of the financial system. Lending to SMEs often depends on relationship-specific information collected through repeated interactions between firms and local banks. Limited branch coverage or weak competition can restrict the formation of these relationships and increase the cost of serving firms outside major urban centers. Cross-country evidence for LAC suggests that greater bank penetration and less concentrated credit markets are associated with lower financing constraints (Presbitero and Rabellotti, 2016). Evidence from Brazil similarly shows that the effects of an intermediated development bank credit program are stronger where local bank presence is greater, indicating that the availability of public funding does not automatically translate into credit unless financial intermediaries have the capacity to originate and service SME loans (Bazzi et al., 2023). This problem can be particularly consequential in smaller economies and geographically fragmented markets, where the fixed costs of branch networks, borrower assessment, and loan monitoring are spread across relatively few potential clients.

Macroeconomic volatility can reinforce these structural constraints. During recessions, periods of financial stress, or episodes of high inflation and exchange rate uncertainty, banks may shorten loan maturities, raise risk premia, or reduce lending to smaller and more opaque firms first. SMEs are particularly exposed because they generally have smaller liquidity buffers and fewer alternative sources of finance. Moreover, even when a firm receives a loan, the terms may not match its financing needs. Short-term credit may support inventory and payroll but be unsuitable for equipment purchases or technology adoption with longer

repayment horizons. Credit access should therefore not be treated as a binary outcome: loan size, maturity, cost, collateral requirements, and predictability of refinancing determine whether formal finance meaningfully relaxes the firm's constraint.

These barriers frequently interact. A partially informal firm with limited financial records and little conventional collateral faces high screening costs; if it operates in a concentrated or geographically thin banking market, it may receive only a small, short-maturity loan at a high cost, even when its underlying investment opportunity is productive. Public interventions consequently address different margins of the same financing problem. Guarantees seek to reduce collateral constraints and lender risk; development bank credit lines seek to expand the supply or maturity of bank funding; subsidized lending lowers borrowing costs; and improvements in financial infrastructure seek to reduce information, contracting, and enforcement frictions. Whether these instruments increase productive credit depends on their additionality—that is, whether they expand financing for firms that would otherwise remain constrained rather than merely replacing or subsidizing credit that would have been provided in their absence. [0]

Against this background, Table 1 summarizes the impact evaluation evidence from LAC and links the region's principal financing constraints to the policy instruments used to address them. The Colombian guarantee evaluation examines an intervention directed primarily at insufficient collateral (Arraiz et al., 2014); the studies from Argentina, Brazil, Colombia, and Ecuador evaluate development bank funding and credit lines intended to increase the availability or improve the terms of intermediated finance (Bazzi et al., 2023; Bruhn et al., 2025a; Clemente et al., 2023; De Negri et al., 2017; Eslava et al., 2014); and the Brazilian evidence on subsidized lending assesses whether reducing the price of credit generates additional real activity (Grimaldi and Ornelas, 2024). Table 1 should therefore be read not only as a catalog of regional evaluations but also as a mapping between the barriers facing LAC SMEs, the instruments designed to relax them, and the resulting effects on firm performance.

[0]This interpretation is consistent with theoretical and institutional accounts that treat SME financing constraints as the combined result of asymmetric information, limited pledgeability, high screening and monitoring costs, and the structure of lending relationships (Stiglitz and Weiss, 1981; Holmstrom and Tirole, 1997; Petersen and Rajan, 1994; Berger and Udell, 2006). The distinction between additional credit and the substitution or refinancing of existing funding is particularly important for evaluating directed and subsidized lending. Banerjee and Duflo (2014) show that additional credit generates real expansion primarily among firms facing binding constraints.

Table 1: Impact evaluation studies on SME credit using data from Latin America and the Caribbean

Authors	**Methodology**	**Country**	**Intervention**	**Outcomes**			**Effect measured after**
				Empl.	**Sales**	**Profits**	
Arraiz et al. (2014)	Staggered DiD with matched control group	Colombia	Partial credit guarantees provided by the National Guarantee Fund to firms with insufficient collateral for accessing bank loans.	Yes	Yes	No	2 years
Bazzi et al. (2023)	DiD with continuous treatment based on intermediary bank presence and credit supply shocks	Brazil	Cartão BNDES, a program of the development finance institution BNDES, run through intermediary banks, offering credit lines with favorable terms.	Yes	No	No	Average across 5 years
Bruhn et al. (2025a)	Staggered DiD without never-takers	Ecuador	Loans from financial institutions that in turn receive loans from the development bank Corporación Financiera Nacional, supported by World Bank funds.	Yes	Yes	Yes	Average across 2 years
Clemente et al. (2023)	DiD with matched control group	Argentina	Credit lines for MSMEs provided through the Global Credit Program, funded by the IDB.	Yes	Yes	No	1 year
Da Silva et al. (2009)	DiD with matched control group	Brazil	Three regional public programs (FCO, FNE, FNO) providing loans to MSMEs via public banks.	Yes	No	No	3 years
De Negri et al. (2017)	DiD with matched control group	Brazil	Credit lines managed by BNDES and FINEP to public financial institutions for MSME long-term financing.	Yes	No	No	2–3 years
Eslava et al. (2014)	Staggered DiD with matched control group	Colombia	Credit lines by the development bank Bancóldex to commercial banks issuing loans to firms with at least 10 employees.	Yes	Yes	No	Most are between 1 and 4 years
Grimaldi and Ornelas (2024)	DiD with synthetic control group made up of firms that received non-subsidized loans	Brazil	Credit program managed by BNDES offering subsidized loans to firms through commercial banks.	Yes	No	No	24 months

Notes: The table follows the structure of Table 1 of Bruhn et al. (2025b), restricted to impact evaluation studies using data from Latin American and Caribbean countries. Author entries in the first column are citation links to the corresponding bibliography entries. DiD = difference-in-differences; MSME = micro, small, and medium-sized enterprise; IDB = Inter-American Development Bank.

5 Evidence by intervention type

This section reviews the core studies, organized around intervention archetypes that recur in SME finance policy: credit guarantees, subsidized credit and directed lending, intermediated development bank credit lines, private bank screening and experimental loan-size expansions, and market-level access expansions. Within each archetype, the discussion emphasizes impacts on employment, sales, profits, income, and firm survival while interpreting the economic mechanisms behind the estimates. A central message is that the effects of credit depend not only on whether firms receive loans but also on how borrowers are selected, how much they borrow, what terms they receive, and whether complementary constraints continue to bind.

5.1 Credit guarantees: Relaxing collateral constraints and shifting bank risk

Credit guarantee schemes are among the most common SME finance tools in developing countries. They are designed to relax collateral constraints and shift part of the lender's downside risk to a public or quasi-public guarantor. In principle, this should expand lending to firms with viable projects but insufficient collateral. In practice, the impact of guarantees depends on additionality: whether guaranteed credit reaches firms that would not otherwise have borrowed or instead subsidizes loans that banks would have made anyway.

A prominent emerging market example is the Turkish Credit Guarantee Fund expansion evaluated by Akcigit et al. (2024). The estimated impacts are large, with employment rising by about 42% and sales by about 74%, with marked improvements in survival, especially among firms likely to have been credit rationed absent the guarantee. The magnitude of the Turkish estimates suggests that guarantees can matter substantially when they operate at scale and bind on an economically important margin. At the same time, the scale of the program implies that partial equilibrium estimates may not fully capture credit market spillovers, fiscal risk, or changes in aggregate demand.

Evidence from China complements this picture by showing how guarantees can translate into investment and innovation. Yu et al. (2022) find that access to credit guarantees increases SME borrowing, investment, patents, and R&D spending. This is consistent with the idea that guarantees may be particularly relevant for firms undertaking intangible or uncertain projects, for which traditional collateral is limited and private lenders may be reluctant to lend. The Chinese evidence therefore extends the guarantee literature beyond short-run employment and sales outcomes, highlighting innovation as a potential channel

through which risk sharing can affect firm dynamics.

Latin American evidence points to more moderate but still meaningful effects. In Colombia, Arraiz et al. (2014) evaluate partial guarantees provided through the National Guarantee Fund to firms lacking sufficient collateral. They estimate an employment impact of about 8%. This magnitude is much smaller than the Turkish estimate but economically relevant, especially given the more targeted nature of the Colombian intervention. Brazilian evidence is more heterogeneous: some programs linked to development bank lending generate sizable employment effects, whereas evaluations of guarantees or earmarked subsidized lending alone often show smaller effects. This pattern suggests that guarantee coverage, lender incentives, firm selection, and complementary program features jointly determine whether risk sharing translates into real expansion.

Advanced economy evidence is useful for understanding mechanisms because administrative data often allow more precise measurement and longer follow-up. Lelarge et al. (2010) study the French SOFARIS guarantee program using an eligibility threshold design and estimate employment gains of roughly 10% over two to three years, with weaker profit effects. Importantly, guaranteed firms borrow more overall, suggesting that guarantees relax a binding financing constraint rather than merely relabel existing credit. Bertoni et al. (2023) find modest but persistent employment and survival effects from guarantees, but limited profit gains, consistent with guarantees helping employment-intensive firms survive and expand without necessarily raising margins. South Korean evidence from Oh et al. (2009) similarly finds positive but modest employment effects, around 5%–7%.

Taken together, the guarantee evidence supports three conclusions. First, guarantees can expand real activity when collateral constraints bind and banks respond by increasing lending at the extensive or intensive margin. Second, effects are not mechanically large: they depend on guarantee coverage, screening, monitoring, and the characteristics of beneficiary firms. Third, guarantees create a policy trade-off. Higher coverage can induce lending to constrained firms, but it can also weaken lender screening and monitoring incentives, increasing fiscal exposure and the risk of misallocation.

5.2 Subsidized credit and directed lending

Subsidized credit lines and directed lending policies are ubiquitous in developing countries. They operate by lowering the price of credit, increasing the quantity available to targeted firms, or requiring banks to lend to specific segments. Their effects depend on whether the policy relaxes a binding constraint or instead transfers rents to firms that would have

borrowed anyway.

Banerjee and Duflo (2014) examine an Indian directed lending policy that expanded priority sector credit to firms within a specified size band. Their results show that firms receiving additional credit increase employment by about 8%, but profit effects are close to zero. This pattern is consistent with credit-financed expansion among constrained firms but with increased output partly offset by lower margins or competitive pressures. It also illustrates a broader feature of directed lending: increased scale does not necessarily imply higher profitability.

Evidence from subsidized lending in advanced economies points to heterogeneous mechanisms. Bach (2014) evaluates subsidized CODEVI loans in France and finds large profit effects, around 37%, but smaller employment effects, about 7%. This suggests that subsidized credit can raise margins or productivity without generating proportional employment growth. Horvath and Lang (2021) evaluate Hungary's Funding for Growth Scheme and estimate an employment effect of roughly 23%, one of the larger effects in the advanced economy sample. The Hungarian program combined sizable loan amounts with explicit support for investment, which may explain why employment responded more strongly.

Brazilian evidence illustrates the importance of additionality. Grimaldi and Ornelas (2024) use credit registry data to study subsidized earmarked BNDES loans and find small real effects despite below-market interest rates. Their interpretation is that many recipient firms were not strongly credit constrained and used subsidized loans to refinance or smooth cash flows rather than to expand employment or output. This evidence cautions against equating cheaper credit with effective credit access. A subsidy can be fiscally costly while producing limited real effects if it reaches firms with alternative funding sources.

Macro-policy-driven credit expansions provide a related but distinct case. Amini and Salehi Esfahani (2025) evaluate credit easing for small firms in Iran during 2005–2013, a period characterized by broader expansionary policies, inflationary pressures, and exchange rate volatility. They find positive employment effects, but interpretation is complicated by the macroeconomic environment: nominal expansion, inflation, and politically directed allocation may contaminate measured real effects. This case underscores that directed credit is most likely to generate sustainable firm-level gains when delivered under stable macroeconomic conditions and with credible screening and repayment incentives.

Overall, the directed and subsidized lending evidence suggests that these tools can work when they reach constrained firms and finance productive investment, but they are especially vulnerable to weak targeting, political allocation, substitution from other credit sources, and

low additionality. For policy design, the key question is not whether credit is cheap but whether the intervention changes the investment frontier of firms that would otherwise be unable to finance high-return projects.

5.3 Intermediated development bank funding and credit lines

Intermediated lending programs provide public or development bank funding to private financial intermediaries, which then lend to SMEs. These programs are common in developing countries because they exploit banks' screening and monitoring capacity while using public balance sheets to increase funding availability or improve loan terms. Their impact depends on pass-through: whether intermediaries expand credit to target firms or simply replace their own funding with cheaper public funds. This channel is especially important in the evidence from LAC summarized in Table 1, where most studies evaluate credit lines, public development bank programs, or guarantees delivered through banks.

Brazil provides a rich set of evaluations. De Negri et al. (2017) find large employment impacts from development-bank-related credit lines, with estimates around 23%. These effects are among the largest in the literature, though interpretation depends on the credibility of controls for selection into treatment. Da Silva et al. (2009) evaluate Brazil's constitutional financing funds and find employment effects ranging from roughly 10% to 19%, depending on region and specification. These programs often combine credit with regional development objectives, interest subsidies, and other forms of support, making it difficult to isolate the pure lending channel.

Bazzi et al. (2023) study Cartão BNDES, a development bank program delivered through intermediary banks, and estimate an employment effect of about 8.5%. A particularly useful feature of their evidence is the role of local financial infrastructure: effects are stronger in municipalities with greater bank branch presence. This suggests that intermediated programs are not only about the availability of public funding; they also depend on the local capacity of banks to originate, monitor, and service SME loans. Grimaldi and Ornelas (2024), also using Brazilian credit registry data, provide a useful counterpoint, showing that subsidized BNDES credit can have small real effects when the main margin is cheaper refinancing rather than additional lending to firms with binding investment constraints.

Evidence from Argentina and Colombia broadens the LAC picture beyond Brazil. Clemente et al. (2023) evaluate Argentina's Global Credit Program, an IDB-funded credit line for micro, small, and medium-sized enterprises (MSMEs), using a DiD design with a matched control group and measuring effects on employment and sales after one year. The short horizon

makes this evidence particularly relevant for interpreting credit lines as instruments for post-shock reactivation and liquidity support, not only for longer-run firm growth. Eslava et al. (2014) evaluate Bancóldex credit lines in Colombia, where the development bank provides liquidity to commercial banks that then lend to firms with at least 10 employees. Their staggered DiD evidence measures impacts on employment and sales over horizons mostly between one and four years, illustrating how public development bank funding can operate through private bank intermediation. Together with the evidence on Colombian partial guarantees in Arraiz et al. (2014), these studies show that LAC evaluations are concentrated in the institutional channels most commonly used by governments in the region: guarantees, development bank second-tier lending, and subsidized credit lines.

European evidence provides a benchmark from more mature financial systems. Amamou et al. (2020) evaluate European Investment Bank (EIB) support for SME lending and estimate employment effects of about 9%. Asdrubali and Signore (2015) study EIB guarantees across six member states and find positive but more modest employment effects, around 5%–7%. The smaller magnitudes are consistent with lower marginal additionality in settings where SMEs have better alternative access to finance. However, they also show that intermediated lending can still matter in advanced financial systems, particularly during periods of financial stress or when bank funding constraints bind.

The evidence on development bank credit lines points to an important implementation lesson: intermediary incentives determine program effectiveness. If banks bear too little risk, they may have weak incentives to screen and monitor. If they bear too much risk, they may use public funds for low-risk borrowers they would have served anyway. Effective intermediated programs therefore need risk-sharing arrangements that preserve lender discipline while inducing additional lending to constrained firms.

5.4 Private bank screening and experimental loan-size expansions

Private bank lending decisions are not always framed as public policy interventions, but they provide direct evidence on the microeconomic value of credit access and on the importance of borrower selection. Two recent studies are especially informative because they identify causal effects using loan approval rules and randomized loan-size expansions.

Delis et al. (2025) study business loan applications to a large European bank using an RDD based on the bank's credit score cutoff. Their sample covers 61,863 applications submitted by 15,628 small and micro entrepreneurs over 2002–2016. Comparing applicants just above and below the approval threshold, they find that loan origination raises recipients'

real income by about 11% five years after the credit decision. The effect is stronger when the bank's soft information contributes positively to the credit score: accepted applicants experience an income increase of about 13.5% when soft information favors approval, compared with about 7% when soft information contributes negatively. The study also shows that accepted applicants use funds for investment and business operations, experience higher profitability and firm growth, and are more likely to repay previous obligations.

This evidence sharpens the interpretation of credit impacts in three ways. First, it shows that access to bank credit can affect entrepreneur income over a relatively long horizon, not only firm employment or sales. Second, it highlights the value of screening: the income effect is larger when the bank's private assessment of borrower quality and project prospects is favorable. Third, it distinguishes credit access from credit use. Among accepted applicants, those easily accepted by the bank experience larger subsequent income gains than marginally accepted borrowers with similar creditworthiness, suggesting that the efficient deployment of loan proceeds is a central mechanism.

The Egyptian lending experiment in Bryan et al. (2024) provides complementary evidence from a developing country setting and from the intensive margin of loan size. Working with the Alexandria Business Association, the authors randomize eligible borrowers into larger loans equal to four times their previous loan size, while control borrowers are offered loans equal to twice their previous loan size. Average effects of the larger loans are small, but treatment effect heterogeneity is substantial. Entrepreneurs in the top quartile of predicted treatment effects increase profits by about 55% of the control group mean, while those in the bottom quartile experience profit declines of about 52%. Top performers also experience increases in wage bills, productivity, and household expenditures, whereas poor performers experience reductions in employment and wage bills.

The Egyptian experiment is especially important because it shows that "more credit" can be highly productive for some firms and harmful for others. The authors use psychometric and cognitive data, combined with machine learning methods, to predict heterogeneous treatment effects. Standard firm and demographic data are insufficient to identify the winners and losers. This finding has direct implications for SME finance policy: targeting based only on observable firm characteristics may miss borrower traits that are central to the productivity of larger loans.

The study also documents a misalignment between lender incentives and welfare-relevant outcomes. Loan officers, whose incentives emphasize repayment, perceive top performers as riskier than poor performers even though top performers generate larger real gains. This

suggests that lender screening can be privately rational but socially inefficient if it places too much weight on default avoidance and too little weight on borrower growth, productivity, and profit potential. More broadly, the evidence from Delis et al. (2025) and Bryan et al. (2024) points to the same conclusion from different contexts: credit access has larger effects when loans are allocated to entrepreneurs with productive investment opportunities and the capacity to use larger loans efficiently.

5.5 Market-level randomized access and spillovers

Most evaluations estimate direct effects on firms that receive loans, but the welfare effect of credit also depends on indirect effects on competitors, prices, and consumers. If credit allows treated firms to expand by taking customers from untreated firms, firm-level gains may overstate aggregate gains.

Cai and Szeidl (2024) address this issue by randomizing the intensity of microcredit access across 162 Chinese markets, allowing them to estimate both direct effects on treated firms and indirect effects on competitors. Treated firms experience large direct gains in sales and profits, roughly 25%, but untreated competitors in the same markets experience nearly offsetting losses, around 20%. Aggregating across firms, the effect on producer surplus is close to zero. Consumer surplus rises modestly because treated firms lower prices, but the overall welfare gain is much smaller than the direct firm-level effect would imply.

This result has substantial implications for interpreting the evidence reviewed above. Credit expansions targeted to non-tradable local services and retail firms may primarily reallocate demand within a fixed local market. By contrast, credit to firms in tradable sectors, firms linked to expanding value chains, or firms with access to procurement or export markets may be more likely to generate aggregate output gains. The market-level evidence therefore suggests that intervention type should not be evaluated separately from market structure. A guarantee, subsidized loan, or development bank credit line may have different welfare effects depending on whether beneficiary firms operate in saturated local markets or in sectors with room for demand expansion and productivity growth.

In sum, the intervention-level evidence supports a nuanced conclusion. Credit guarantees, subsidized lending, development bank credit lines, and private bank credit expansions can all raise SME performance, but their effects depend on additionality, screening, loan size, borrower traits, lender incentives, and market structure. The strongest results tend to appear when programs reach genuinely constrained firms with productive uses for credit. Smaller or null effects arise when loans substitute for existing finance, when borrowers lack profitable

investment opportunities, when loan terms do not match the investment horizon, or when direct gains are offset by competitive losses among untreated firms.

6 Mechanisms, complementary constraints, and market context

6.1 Investment, working capital, and scale

The transmission mechanism from credit to firm outcomes depends on what firms do with the funds. Survey-based evidence across multiple studies indicates that SMEs use loans for a mix of investment (fixed assets, equipment) and working capital (inventory, raw materials, cash flow smoothing). The balance varies by firm size and sector, with smaller firms tending to use credit more for working capital and larger firms and manufacturing firms using credit more for capital investment.

Large effects on sales without commensurate effects on employment suggest capital deepening: firms buy equipment or technology that raises output per worker. Small effects on both employment and sales suggest that credit may be used for consumption smoothing or refinancing rather than expansion. Profit effects that are smaller than sales effects suggest that loans may induce entry or competition, eroding margins even as output grows.

6.2 Managerial capital and the "training plus finance" hypothesis

An increasingly prominent hypothesis is that credit alone may not suffice if entrepreneurs lack the managerial skills to deploy capital effectively. Training-plus-finance interventions test whether complementary managerial capital raises returns to credit.

Ashraf and Lyons (2026) randomize access to a combined training-and-loan intervention among Kenyan SMEs. The joint treatment raises business practices, investment, and employment significantly more than loans alone. The mechanism appears to be improved capital allocation: trained entrepreneurs are better able to identify high-return projects, manage cash flow, and avoid overexpansion into low-margin activities.

This evidence suggests that multidimensional constraints are often binding. Credit expands the financial frontier, but training expands the managerial frontier, and combining them raises the return to both. For policy, this implies that credit programs should consider bundling finance with technical assistance, especially in settings where entrepreneurial education is limited.

6.3 Credit supply channel versus credit terms

Expanding the supply of credit is conceptually distinct from changing the terms on which credit is offered (interest rates, maturities, collateral requirements). Some interventions operate primarily through quantity (guarantees, intermediated funding), while others operate through price (subsidized interest rates) or collateral (movable collateral registries).

Beaumont et al. (2025) study a fintech platform that uses transaction data and machine-learning-based credit scoring to expand small business lending in the United States. The platform reduces informational frictions and allows lenders to accept non-traditional collateral or rely on cash-flow-based underwriting rather than asset-based lending. The authors find that access to fintech credit raises firm investment and sales by about 25%, particularly among firms that had been excluded from traditional bank credit due to thin credit files or limited real estate collateral.

This is consistent with the view that constraints are multidimensional: some firms are excluded not because they lack profitable projects but because they cannot provide the specific forms of collateral or documentation that traditional lenders require. Technological innovations that relax these constraints can generate real effects even in relatively mature credit markets.

6.4 Institutional reforms

A prominent reform agenda in low-income countries involves creating movable collateral registries, allowing firms to pledge inventory, equipment, receivables, or other movable assets. The reform aims to address a binding constraint in economies where SMEs have limited real estate collateral but substantial movable assets.

Pendame and Akotey (2023) evaluate Malawi's movable collateral registry and find that the reform expands credit access at the extensive margin—more firms borrow—but real effects on employment and profits are modest. The explanation appears to be twofold: (1) loan sizes remain small even with movable collateral, suggesting lenders remain cautious about enforcement risk, and (2) demand-side constraints (market access, infrastructure, skills) remain binding, limiting the return to marginal credit.

This is consistent with broader evidence that collateral reforms are necessary but not sufficient. Unlocking collateral expands the set of firms that can borrow but does not automatically translate into large-scale expansion unless complementary constraints are relaxed.

6.5 Macroeconomic context and policy regime

Credit expansions do not take place in a vacuum. Their measured impacts can depend on the macroeconomic environment, the broader policy regime, and the stability of prices, exchange rates, and repayment incentives. A distinctive case is Iran's credit expansion during 2005–2013, evaluated by Amini and Salehi Esfahani (2025). This episode involved large-scale directed credit to SMEs as part of a broader populist and expansionary policy shift. Unlike targeted credit programs in stable macroeconomic environments, this intervention occurred during a period of loose monetary policy, rising inflation, and volatile exchange rates.

The estimated employment effects are positive and economically meaningful (around 12%–15%), but interpretation is complicated by the fact that the credit expansion coincided with rising nominal wages, inflation, and output price increases. This suggests that part of the measured "real" effect may reflect nominal expansion rather than productivity gains. The authors find some evidence that credit raised investment and output among smaller manufacturing firms but also evidence of misallocation: credit flowed to politically connected firms and sectors with declining competitiveness.

The Iranian case highlights a broader design principle. In particular, credit programs are more likely to generate sustainable real effects when they are delivered in stable macroeconomic environments with credible commitment to repayment and limited risk of inflation or financial instability. In unstable settings, expanding credit may still provide short-run liquidity, but it becomes harder to distinguish productive real expansion from nominal growth, subsidized refinancing, or politically driven allocation.

6.6 General equilibrium and spillovers

A recurring limitation of many credit evaluations is that they measure partial equilibrium effects on beneficiaries without capturing spillovers on non-beneficiaries, competitors, and market prices. Yet the welfare rationale for public intervention depends on aggregate outcomes: net job creation, productivity gains, and consumer welfare.

Cai and Szeidl (2024) provide a clean illustration. Credit access can have large direct positive effects on treated firms but similar-sized negative indirect effects on competitors, leaving no detectable change in aggregate producer surplus even though treated firms grow. This mechanism is particularly salient in developing countries where many SMEs operate in local, non-tradable sectors with limited scope for market expansion. If a program mostly reallocates customers across firms, measured increases in sales for treated firms may not

translate into higher aggregate output.

The implication is that evaluations and policy design should pay more attention to three issues. First, the sectoral composition of beneficiary firms matters, as credit to tradable or scalable sectors may yield more aggregate gains than credit to saturated local services. Second, the intensity of program rollout matters. Scaling a program can change market structure, prices, and competition, making early pilot impacts an unreliable guide. Third, complementary policies—export facilitation, procurement access, infrastructure, and managerial support—may be necessary for credit to translate into aggregate growth rather than redistribution. Table 2 summarizes the main intervention types, the margins they affect, the expected mechanisms, the outcome patterns most often observed, and the principal implementation risks.

7 Policy implications and future research

The evidence base supports several conclusions that are robust across contexts while also making clear what remains uncertain or context dependent. The overarching lesson is that "more credit" is not, by itself, a sufficiently precise policy objective. Effective SME finance policy requires attention to additionality, targeting, terms, delivery channels, lender incentives, complementary constraints, and market structure.

First, expanding formal loans can raise SME employment, sales, and profits by economically meaningful magnitudes. Meta-analytic mean effects—12% on employment, 18% on sales, and 18% on profits—suggest that relaxing financial constraints matters for firm-level performance. These are not transformative gains for every firm, but they are large enough to matter for job creation and productivity when credit reaches constrained firms with profitable uses for external finance. The policy implication is that programs should prioritize measurable additionality: whether the intervention expands the feasible investment set of firms that would otherwise remain constrained rather than simply reducing the financing cost for firms that already had access to credit.

Second, impacts are heterogeneous and design dependent. Employment effects appear substantially larger when credit is delivered through public institutions, when smaller firms are targeted, and when programs operate at scale with strong institutional commitment. Guarantee schemes and directed lending can generate large effects when well targeted but also risk misallocation, moral hazard, and fiscal burdens when poorly designed. Intermediated programs through development banks generate more modest effects on average, poten-

tially because additionality is lower when firms already have access to private credit. These patterns suggest that program design should build in ex ante rules for beneficiary selection, ex post monitoring of loan use, and explicit evaluation of whether supported loans are genuinely additional.

Third, credit guarantees can be effective, but they are not magic. The largest successes often involve large-scale programs in contexts where collateral constraints are genuinely binding and where guarantees induce banks to expand lending rather than simply relabel existing loans. Modest or null results appear when guarantees are too small to matter, when banks remain risk averse despite guarantees, or when demand-side constraints limit firm growth even when credit is available. Guarantee programs should therefore preserve lender screening incentives through partial risk sharing, appropriate coverage ratios, and monitoring arrangements that limit risk shifting and fiscal tail risk.

Fourth, complementary interventions should be treated as empirical questions rather than presumed complements. Training-plus-finance interventions show promise, but the existing evidence base is thin and concentrated in specific contexts. Fintech and alternative collateral can relax information and collateral constraints, expanding access, but whether they generate large real effects depends on whether underlying returns to capital are high. Institutional reforms such as movable collateral registries may be necessary but insufficient: Malawi's experience suggests that unlocking collateral can expand borrowing at the margin without producing large employment or profit effects if loan sizes remain conservative or if demand-side constraints continue to bind.

Fifth, technology can matter, but its contribution is primarily through screening, monitoring, and transaction cost reduction. Fintech-enabled underwriting, collateral valuation, and cash-flow-based scoring may expand lending to firms with thin credit files or limited real estate collateral. The key research and policy question is whether such tools improve the allocation of capital or simply increase lending volumes. This distinction matters because the evidence on heterogeneous returns shows that larger loans can generate very high returns for some firms and losses for others.

Sixth, policymakers should incorporate spillovers and general equilibrium. If credit expansions induce business stealing rather than market expansion, net welfare gains may be much smaller than firm-level estimates suggest. Targeting tradable sectors, export-oriented firms, suppliers in expanding value chains, or sectors with scope for productivity improvements may yield larger aggregate effects than expanding credit in saturated local retail and services. Complementary policies—trade facilitation, procurement access, infrastructure,

and market-linkage support—can amplify credit impacts by expanding market size.

These policy lessons point to several priorities for future research. One is measurement of welfare and general equilibrium. Few studies measure consumer welfare, prices, or indirect effects on non-treated firms. Experimental designs that randomize at the market level, as in Cai and Szeidl (2024), are logistically challenging but increasingly feasible in settings where credit programs are rolled out in waves or across regions. More work in this vein would sharpen understanding of when credit expansions generate net welfare gains versus redistribution.

A second gap is dynamic and crisis-state dependence. Many programs operate during crises or macro transitions, including post-conflict, post-crisis, and stabilization episodes. Their effectiveness may depend on whether they provide liquidity during temporary shocks or address persistent constraints. Longer-run tracking—5 to 10 years post-intervention—would clarify whether effects persist, fade, or reverse as repayment burdens accumulate, competitive dynamics adjust, or macroeconomic conditions change.

A third gap is targeting and screening. RCT evidence suggests that lenders struggle to predict winners and losers ex ante, even with rich data. Machine learning, psychometric tools, and alternative data show promise, but their predictive power, fairness, and manipulability remain open questions. This raises a fundamental policy trade-off: broad access accepts high rates of "wasted" credit to low-return borrowers, while narrow targeting may exclude high-return firms that are difficult to identify. Future work should quantify these trade-offs by comparing alternative allocation rules, measuring default and firm growth outcomes jointly, and incorporating distributional impacts across entrepreneurs, workers, consumers, and competing firms.

Table 2: Intervention types, expected mechanisms, and common outcome patterns

Intervention type	**Primary margin affected**	**Main mechanisms**	**Outcomes most often affected**	**Key risks**
Directed/subsidized lending	Quantity + price	Working capital, investment	Employment, sometimes investment; profits mixed	Misallocation; substitution; political capture
Development bank credit lines	Quantity via funding	Bank transmission; liquidity	Stabilization in shocks; mixed profits	Low additionality; relabeling; weak monitoring
Public credit guarantees	Quantity + collateral constraint	Expanded lending; maturity changes	Employment, survival, investment; profits mixed	Risk shifting; reduced screening; fiscal tail risk
Screening innovations	Allocation	Better targeting; inclusion	Heterogeneous; can raise impacts per loan	Model risk; bias; monitoring substitution

8 Conclusion

The impact evaluation evidence indicates that expanding access to formal loans can improve SME performance in terms of employment, sales, and profits, but the magnitudes are heterogeneous, context dependent, and sensitive to program design. The meta-analytic mean effects—12% on employment, 18% on sales, and 18% on profits—are economically meaningful and suggest that credit constraints bind in many settings. However, the presence of both large successes and near-null results underscores that "credit works" is an incomplete answer without specifying institutional channels, targeting mechanisms, and complementary policies.

The central policy takeaway is that "more credit" is not a sufficient objective. Effective SME finance policy requires attention to design: who receives credit (targeting and screening), on what terms (interest rates, maturities, collateral requirements), through what channels (public banks, private intermediaries, guarantees), and with what complementary support (training, technical assistance, infrastructure). Programs that reach genuinely constrained, high-return firms and operate in environments with complementary inputs (skills, market access, infrastructure) are more likely to generate large and sustained effects. Programs that primarily relabel existing credit, reach low-return borrowers, or operate in saturated local markets with limited scope for expansion are less likely to deliver meaningful welfare gains.

A second implication is that credit policy should be evaluated as a portfolio of instruments rather than as a single intervention. Guarantees, subsidized credit lines, public bank lending, development bank refinancing, collateral reforms, and data-driven screening each address different frictions. Their effectiveness depends on whether the binding constraint is collateral, information, maturity mismatch, lender liquidity, managerial capacity, or market access. This means that institutional diagnosis should precede program choice. In settings where banks already lend to the targeted firms, cheaper credit may mainly reduce borrowing costs. In settings where firms have profitable projects but lack pledgeable assets or verifiable records, guarantees, collateral reforms, or improved underwriting may have larger real effects. In settings where firms cannot identify or implement high-return investments, credit may need to be paired with managerial support or market-linkage interventions.

The evidence also points to the importance of embedding evaluation into program design from the beginning. Many SME finance programs are launched at scale with limited information on additionality, borrower selection, spillovers, or fiscal risk. Better administrative data, credit registries, matched firm–bank panels, and randomized or quasi-randomized

rollout rules can make it possible to learn while programs are implemented. Future evaluations should measure not only borrower outcomes but also substitution from other credit sources, repayment behavior, and effects on competitors, prices, workers, and consumers. This broader measurement agenda is essential because the welfare case for SME credit policy depends on aggregate productivity and job creation, not only on growth among treated firms.

For developing countries, and particularly for contexts where public banks and development finance institutions play a large role, the main lesson is disciplined selectivity. Public support can overcome market failures that private lenders do not internalize, but it can also magnify political capture, weak screening, and fiscal exposure. The most promising programs are those that combine clear targeting of constrained firms, risk-sharing arrangements that preserve lender incentives, loan terms aligned with investment horizons, and complementary policies that expand demand or improve firm capabilities. The unresolved research frontier is therefore not whether credit matters but how to allocate it, under what institutional safeguards, and in which markets it creates net social value.

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