

SELECTION BEFORE TREATMENT: IDENTIFYING PROGRAMME EFFECTS ON FIRMS

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ABSTRACT

Firms that join a business support programme differ from firms that do not, and the characteristics that put them in the treated group also produce the outcome the programme is credited with. This study examines that selection problem as the central obstacle to identifying programme effects on firms, and asks what the reviewed evidence establishes about its size and its remedies. The method is a structured conceptual review of research published between 2017 and 2024 on credit guarantees, subsidised lending, digital adoption, tax compliance and enterprise performance, classified by whether take-up was self-selected and how the study addressed it. Four findings follow. Selection is documented directly rather than assumed, with one experimental introduction of a financial instrument finding adopters more educated than non-adopters and more likely already to hold a bank account. The characteristic that predicts take-up is frequently the same characteristic that predicts survival, since prediction studies identify staffing quality, industry experience and marketing capability as the discriminators between surviving and failed firms. Aggregate effects can invert once selection or subgroup structure is examined, with one meta-analysis finding a positive published effect becoming small and statistically insignificant after correction for publication bias. And the strongest designs in the corpus obtain identification from variation the firm did not choose, including guarantee eligibility thresholds and branch density that predicts conservative rather than expanded borrowing. The study proposes reporting the selection margin alongside the effect.

ABSTRAK

Perusahaan yang mengikuti program pendampingan usaha berbeda dari yang tidak, dan ciri yang menempatkan mereka pada kelompok terlakukan itu juga menghasilkan capaian yang lantas dinisbahkan kepada programnya. Kajian ini menelaah persoalan seleksi tersebut sebagai penghalang utama identifikasi dampak program pada perusahaan, dan menanyakan apa yang ditetapkan bukti mengenai besaran serta penawarnya. Metodenya adalah telaah konseptual terstruktur atas riset terbitan 2017 hingga 2024 mengenai penjaminan kredit, kredit bersubsidi, adopsi digital, kepatuhan pajak, dan kinerja usaha, digolongkan menurut apakah keikutsertaannya dipilih sendiri dan bagaimana studinya menanganinya. Empat temuan mengikutinya. Seleksinya terdokumentasi secara langsung, bukan diandaikan, dengan satu pengenalan instrumen keuangan secara eksperimental menemukan pengadopsinya berpendidikan lebih tinggi daripada yang bukan pengadopsi dan lebih mungkin sudah memiliki rekening bank. Ciri yang memprediksi keikutsertaan kerap merupakan ciri yang sama dengan yang memprediksi kelangsungan usaha, sebab studi prediksi mengidentifikasi mutu ketenagakerjaan, pengalaman industri, dan kemampuan pemasaran sebagai

	pembeda antara usaha yang bertahan dan yang gagal. Dampak agregat dapat berbalik begitu seleksi atau struktur antarkelompoknya ditelaah, dengan satu meta-analisis menemukan dampak terbitan yang positif menjadi kecil dan tidak signifikan setelah bias publikasi dikoreksi. Adapun rancangan terkuat dalam korpus ini memperoleh identifikasi dari ragam yang tidak dipilih perusahaannya, termasuk ambang kelayakan penjaminan dan kerapatan kantor cabang yang justru memprediksi pendanaan konservatif alih-alih perluasan utang. Kajian ini mengusulkan pelaporan margin seleksi berdampingan dengan dampaknya. Kata kunci: bias seleksi; evaluasi program; usaha kecil; penjaminan kredit; identifikasi kausal
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1. INTRODUCTION

A support programme is offered. Some firms apply. Those that received it grow faster than those that did not, and the programme is reported to work. The comparison is intuitive and the conclusion does not follow, because the firms that applied were different before they applied, and the difference is the kind that produces growth on its own.

This is the selection problem, and in business support it takes an unusually sharp form. Applying for a guarantee requires financial statements. Adopting a digital tax system requires equipment and a person who can operate it. Joining a cooperative requires the time to attend meetings and enough surplus to contribute. Each requirement is a filter, and each filters on a characteristic that independently predicts firm survival.

The evidence for selection is direct rather than inferred. Batista and Vicente (2020), tracking an experimental introduction of a financial instrument in a rural setting over three years, find adopters — and early adopters especially — more educated than non-adopters and more likely already to hold a bank account, and conclude that this self-selection must be accounted for when such instruments are deployed for inclusion purposes.

The same characteristics appear on the outcome side. Gyimah and Lussier (2021), testing a prediction model on 230 rural businesses of which 120 succeeded and 110 failed, find four variables discriminating at seventy-one per cent accuracy: staffing quality, capital, industry experience and marketing skills. Three of the four are accumulated capabilities, and all four plausibly predict whether a firm applies for support in the first place.

Three questions organise the analysis. First, how large is the selection margin where it has been measured? Second, which studies obtain identification, and from what source of variation? Third, what reporting practice would let a reader judge how much of an estimate is selection?

2. LITERATURE REVIEW

Direct evidence that take-up is selective

Batista and Vicente (2020) supply the clearest case, because the introduction was experimental and the adoption decision was still the firm's. Adopters were more educated and more likely already banked, and the pattern was strongest among early adopters. An instrument justified by the exclusion of the unbanked reached first the people who were least excluded.

Anatan and Nur (2023) find readiness for digital transformation uneven across micro, small and medium enterprises, which specifies the filter for every digitally delivered programme. Rusliana et al. (2023) examine what separates growing from static small firms and identify characteristics that are present before any programme arrives. Chen et al. (2022), across 16,876 rural household observations, find digital financial inclusion reducing poverty vulnerability by 3.53 per cent among low-income households against 0.75 per cent among high-income ones — a subgroup structure that only a deliberate cut reveals.

Yu et al. (2024), using household finance survey data with an instrumental-variable strategy, find digital inclusive finance promoting entrepreneurship through credit constraints, information barriers and risk attitudes, with stronger effects for opportunity-driven than necessity-driven ventures. The distinction is the selection problem stated from the demand side: firms that already identified an opportunity respond more than firms operating from necessity, and the two groups differ in everything else as well.

Credit programmes and the eligibility margin

Brown and Earle (2017) examine finance and growth at firm level through a guaranteed lending programme and find employment and survival effects. Bertoni et al. (2023) examine the long-term effects of loan guarantees and find persistence beyond the guarantee period, which is a stronger result than contemporaneous take-up because persistence is harder to generate by selection alone.

Bachas et al. (2021) shift attention to the lender, examining how a guarantee changes credit supply rather than only borrower access. That reframing matters for identification: if the guarantee moves the lender's decision rule, then variation in eligibility generates variation in credit that the borrower did not choose, which is exactly the source of exogenous variation the problem requires.

Ehrl (2021) finds survival effects of subsidised loans varying with firm characteristics at entry, which is selection appearing as heterogeneity. Crawford et al. (2023) review government finance, loans and guarantees for small and medium enterprises and identify design features determining whether an instrument reaches its intended population. Adha (2023) examines

welfare effects of a national subsidised credit programme on recipients; Aristanto et al. (2020) trace its policy dynamics; Darjana et al. (2022) examine whether credit performance changed after a major shock.

The supply channel is itself non-random. Behr et al. (2017) find public ownership changing the cyclical profile of small-firm lending. Englmaier and Stowasser (2017) find electoral cycles in savings bank lending. Borsuk et al. (2024) find state-owned banks transmitting international shocks differently. Ashraf et al. (2018) find institutional quality moderating political influence in lending. A firm's access therefore depends on the ownership, political calendar and external exposure of whichever institution serves its district — variation the firm did not choose, and therefore usable.

Presence is not access: an identification result in disguise

Gama et al. (2024) find higher bank branch density at municipal level raising the odds that local small firms adopt a conservative, debt-avoiding financing policy, while cooperative bank branches show the inverse relationship, with the effect economically larger for long-term than short-term debt.

Read as a substantive finding, this says presence and borrowing are different variables whose relationship depends on institution type. Read as a methodological finding, it is more useful still. Branch density is not chosen by the firm, so it is a candidate source of exogenous variation in credit access — and the study demonstrates that the variation carries a signal, since the estimated relationship is significant and its sign depends on an observable institutional characteristic.

The study also shows what goes wrong when presence is read as access. An instrument evaluated on branch coverage would report success while the firms in question were becoming more debt-averse. Bel and Sebo (2021), examining whether inter-municipal cooperation reduces delivery costs, reach a structurally similar conclusion: an arrangement's existence does not establish its effect, and the empirical answer is more equivocal than the policy assumption.

What happens to aggregate estimates under scrutiny

Ma et al. (2023) meta-analyse 158 yield estimates from forty-two studies across nineteen developing countries, detect positive publication bias, and find that after correction the effect of cooperative membership on yield is small and statistically insignificant. Meta-regression shows reported effects varying with sample composition, membership ratio, econometric method, effect-size type, product category and climatic zone.

Membership is voluntary, so every study in that meta-analysis faced the selection problem, and the correction that reversed the conclusion was for publication bias rather than for selection. The two filters compound: studies that found effects were more likely published, and within each study the members were more likely to have been the firms that would have done better anyway.

Zou and Wang (2022) show the subgroup version, finding cooperative membership raising household income by roughly nineteen per cent on average while the effect is statistically insignificant at the low-income quantiles. Jamaluddin et al. (2023), reviewing thirty studies of cooperative governance across seventeen countries, find about two-thirds measuring financial rather than social or environmental performance and the governance–performance evidence mixed and inconclusive.

Designs that obtain identification

Three approaches appear in the corpus. The first uses variation in the institutional environment rather than in firm choice: Gama et al. (2024) on branch density and type, Behr et al. (2017) and Englmaier and Stowasser (2017) on ownership and electoral timing, Ashraf et al. (2018) on institutional quality. Each supplies a determinant of access that the firm did not select into.

The second uses instrumental variables. Yu et al. (2024) apply an instrumental-variable strategy to household finance data to separate the effect of inclusion from the characteristics that predict adoption. Wu et al. (2023), in an adjacent setting, use an instrumented ordered probit across 641 villages to estimate the effect of an exposure that households do not choose.

The third uses randomisation or a natural experiment. Bergeron et al. (2024) randomise tax rates and enforcement across nearly thirty-eight thousand property owners; Weigel (2020) randomises collection method across 356 neighbourhoods; Batista and Vicente (2020) randomise the introduction of an instrument while observing who adopts, which is what allows selection to be measured rather than assumed. Caldeira et al. (2023) exploit staggered administrative reassignment across municipalities.

Bellon et al. (2022), Okunogbe and Pouliquen (2022), Kochanova et al. (2020) and Vincent (2023) all study mandated rather than voluntary adoption of digital tax systems, which removes firm choice from the treatment assignment and is therefore the least selection-prone design in the compliance literature. Their common finding — that effects are conditional on complementary capability — is a heterogeneity result rather than a selection result, and the distinction is what makes it credible.

Why the characteristic that selects is the characteristic that matters

Gyimah and Lussier (2021) identify staffing quality, capital, industry experience and marketing skills as discriminating between 120 successful and 110 failed rural firms at seventy-one per cent accuracy. Hong et al. (2023), applying configurational analysis across twenty villages in twelve counties, identify entrepreneurial leadership as the core condition present in both sufficient paths to a successful collective economy. Wang and Wang (2024), through necessary condition analysis on 420 smallholders, find property-rights reform, leadership and social capital necessary for high economic performance.

A necessary condition cannot be compensated for, and a firm that possesses it is both more likely to apply for support and more likely to succeed without it. That is the worst case for identification, because the confounder is not a nuisance variable but the principal cause on both sides.

Kania et al. (2021) supply the practical illustration, finding village enterprises constrained by a shortage of qualified human resources alongside implementation–regulation mismatches. Del Olmo-García et al. (2023) frame rural entrepreneurship as a policy response to depopulation. Rahayu et al. (2024) find social capital confined to a bonding stage without bridging or linking development — again a pre-programme characteristic that determines both participation and result.

RESEARCH GAP

Two gaps follow. The first is that studies of firm support programmes rarely report the selection margin, meaning the observable differences between takers and non-takers, although those differences are typically available in the same data used to estimate the effect.

The second is that the institutional variation which would supply identification is documented but unexploited. Branch ownership and density, electoral timing in lending, and guarantee eligibility thresholds are all established as determinants of firm credit access in the reviewed corpus, and none is used as an identification strategy for evaluating a support programme.

3. METHOD

The study is conceptual and methodological. It advances no estimate; it classifies existing studies by how treatment was assigned and asks what each classification permits.

A corpus was assembled of peer-reviewed research published between 2017 and 2024 on credit guarantees and subsidised lending, digital adoption and tax compliance among firms,

cooperative and collective enterprise, and small-firm performance. Each item was verified against the publisher record, the digital object identifier resolver, or an indexing record, and items in journals discontinued from major indexes or flagged as predatory were excluded regardless of topical fit.

Each study was classified on two dimensions specified in advance: whether the firm chose its treatment status; and where it did, what the study did about it — nothing, controls, instrumental variables, matching, or a design that removed choice. Studies documenting the characteristics of takers were recorded separately as direct evidence on the selection margin.

Two limitations apply. The corpus is not a systematic sample. And the classification records design rather than assessing whether each design's assumptions hold, which requires engagement with each study on its own terms.

4. RESULTS AND DISCUSSION

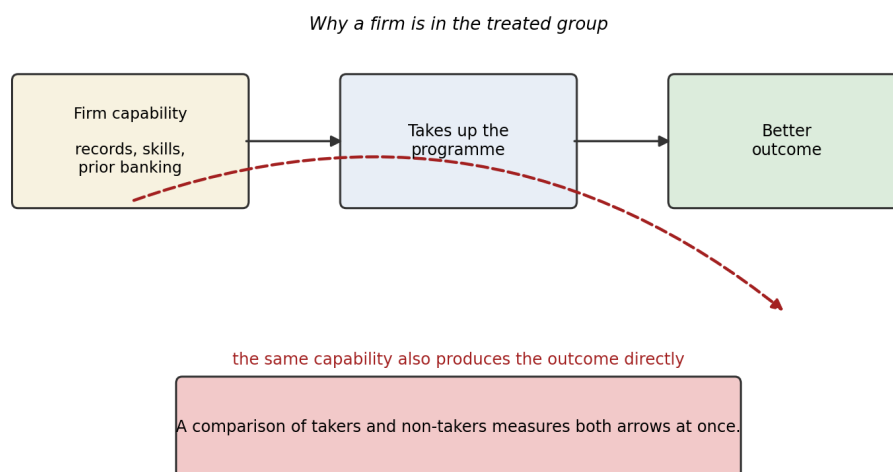


Figure 1. The two paths from firm capability to observed outcome

Finding one: selection is measured, not merely suspected

Batista and Vicente (2020) observe who adopts and report it: adopters more educated, more likely already banked, and the pattern strongest among early adopters. That is the selection margin stated as a finding rather than acknowledged as a caveat, and it is available because the study tracked adoption over three years rather than comparing adopters with non-adopters at a point in time.

Anatan and Nur (2023) specify the filter for digitally delivered programmes by measuring readiness directly. Chen et al. (2022) decompose by income and find a nearly fivefold

difference in effect between the lowest and highest groups. Yu et al. (2024) separate opportunity-driven from necessity-driven ventures and find the effect concentrated in the first.

Each of these is a description of who is in the treated group. None required additional data collection, and each transforms what a reader can conclude from the accompanying effect estimate.

Table 1. Sources of variation in firm treatment status

Source of variation	Chosen by the firm?	Usable for identification?	Example in the corpus
Voluntary application to a programme	Yes	Not without further design	Cooperative membership studies
Digital readiness at baseline	Partly	As a moderator, not an instrument	Anatan & Nur (2023)
Guarantee eligibility threshold	No	Yes, at the threshold	Bachas et al. (2021)
Bank branch density and institution type	No	Yes	Gama et al. (2024)
Electoral cycle in local lending	No	Yes	Englmaier & Stowasser (2017)
Mandated adoption of a digital system	No	Yes	Bellon et al. (2022); Okunogbe & Pouliquen (2022)
Randomised introduction	No	Yes	Batista & Vicente (2020); Bergeron et al. (2024)

Note. Compiled by the author from the reviewed corpus. 'Usable for identification' indicates whether the variation is plausibly independent of the firm characteristics that predict the outcome.

Finding two: the confounder is the principal cause

Ordinary selection bias involves a nuisance variable correlated with treatment. Here the variable doing the selecting is the one the outcome literature identifies as decisive. Gyimah and Lussier (2021) place staffing quality, industry experience and marketing capability among four discriminators of survival. Wang and Wang (2024) establish leadership and social capital as necessary conditions. Hong et al. (2023) find entrepreneurial leadership present in every successful configuration.

A firm with strong leadership and an experienced workforce is more likely to hear about a programme, more likely to complete an application, more likely to be approved and more likely to succeed regardless. Controlling for observable size or sector does not remove this, because the relevant characteristic is capability and capability is not on the application form.

The consequence for interpretation is specific. An estimate from a taker–non-taker comparison in this setting is an upper bound on the programme effect, and the distance

between the estimate and the true effect grows with the strength of the filter. Programmes with demanding application requirements — audited statements, collateral documentation, digital submission — have the strongest filters and therefore the most inflated naive estimates.

Finding three: aggregates reverse under correction

Ma et al. (2023) find a positive published effect becoming small and statistically insignificant after correcting for publication bias across 158 estimates in nineteen countries. Zou and Wang (2022) find a nineteen per cent average effect that is insignificant at the low-income quantiles. Jamaluddin et al. (2023) find the governance–performance evidence mixed and two-thirds of studies measuring only financial performance.

The three findings describe successive filters on the same literature. Selection determines who is in the sample; the reported margin determines which part of the distribution is shown; publication determines which studies appear. A reader of the headline result has passed through all three, and only the third has been corrected anywhere in this corpus.

Bel and Sebo (2021) offer the same caution in a different domain, finding the cost-reduction claim for inter-municipal cooperation more equivocal on examination than the policy assumption. Voorn et al. (2020) find traditional predictors of municipal corporation performance having limited explanatory power, with size, resource access and multiple-principal interaction dominating instead.

Finding four: the exogenous variation exists and is unused

The corpus documents at least four determinants of firm credit access that firms do not choose. Gama et al. (2024) establish branch density and institution type as predictors of financing policy, with the sign depending on type. Behr et al. (2017) establish public ownership as a determinant of lending cyclicity. Englmaier and Stowasser (2017) establish electoral timing in savings bank lending. Borsuk et al. (2024) and Ashraf et al. (2018) establish ownership and institutional quality as determinants of how external shocks and political influence reach lending decisions.

Each is a candidate instrument for the effect of credit access on firm outcomes, and each is documented in a study whose purpose was descriptive. Bachas et al. (2021) come closest to using one, examining how a guarantee changes lender behaviour and thereby identifying a supply-side channel that operates independently of borrower selection.

The digital compliance literature demonstrates the alternative route. Bellon et al. (2022), Okunogbe and Pouliquen (2022), Kochanova et al. (2020) and Vincent (2023) all study

mandated adoption, which removes firm choice from treatment assignment entirely. Their conclusion — that effects depend on complementary capability — is a statement about heterogeneity that a selection-prone design could not have distinguished from a statement about who signed up.

What reporting the selection margin would require

Three steps follow, none requiring new data. First, report the observable characteristics of takers and non-takers before treatment. Batista and Vicente (2020) show how much this alone conveys, and the variables involved — education, prior banking, size, sector, digital readiness — are collected as a matter of course.

Second, state which of the outcome's known determinants the selection is on. Where the filter operates on capability, and the outcome literature identifies capability as decisive, the estimate should be presented as an upper bound rather than as a point estimate of the programme effect.

Third, where institutional variation is available, use it. Branch density, ownership type, electoral timing and eligibility thresholds are documented determinants of access that the firm did not choose, and their use converts a descriptive comparison into an identified estimate. Yu et al. (2024) and Wu et al. (2023) both demonstrate the instrumental-variable route in adjacent settings using survey data of the kind that firm studies already hold.

Limitations

Three limits bound the argument. The corpus is not a systematic sample and over-represents studies that reported selection characteristics. The classification records design rather than assessing assumptions. And the study establishes that naive estimates are upper bounds without estimating how far above the true effect they lie, which would require applying an identified design to a programme previously evaluated naively.

5. CONCLUSION

Firms select into support programmes on the characteristics that determine whether they would have succeeded anyway. That is not an ordinary confounder. The outcome literature names staffing quality, industry experience, marketing capability and entrepreneurial leadership as the discriminators of survival, and one study establishes three of these as necessary conditions that cannot be compensated for. The same attributes determine who applies.

Where selection has been measured rather than assumed, it is substantial: adopters of a financial instrument were more educated and more likely already banked, and the pattern

was strongest among the earliest. Where aggregate results have been corrected, they have moved: a positive published effect across nineteen countries became small and statistically insignificant once publication bias was addressed.

The remedy is available and undeployed. Branch density and institution type, bank ownership, electoral timing and eligibility thresholds are all documented determinants of firm credit access that firms do not choose, and mandated digital adoption removes choice from treatment assignment altogether. Each is reported in this corpus as a descriptive finding by a study that was not trying to identify a programme effect.

Until such designs are used, the modest step is disclosure. Report who took up the programme and how they differed beforehand, and present the resulting estimate as what it is: an upper bound whose distance from the true effect grows with the strictness of the application form.

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Author contributions (CRediT)

Dr. Yeni Indraningtyas: conceptualisation; methodology; formal analysis; writing - original draft; writing - review and editing.

Conflict of interest

The authors declare that they have no competing interests.

Data availability

All data supporting the findings of this study are available within the article and from the publicly accessible sources cited therein.

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Declaration on the use of artificial intelligence

No generative artificial intelligence tool was used in the preparation of this manuscript.

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