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Government Venture Capital and Financial Constraints: Growth and Credit Effects among Swedish SMEs

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Abstract

This paper examines how government venture capital (GVC) affects firm growth and financing in small and medium-sized enterprises. Using matched panel data for Swedish firms over 2008–2020, we evaluate impacts on employment, sales, equity, and debt. GVC-backed firms expand employment almost immediately after investment, while sales growth follows with a lag of two to three years. GVC participation also strengthens firms' equity positions and improves access to bank credit, consistent with certification and balance-sheet channels that alleviate financial constraints. Importantly, the positive impacts are concentrated in metropolitan and innovation-dense regions. Despite receiving a disproportionately large share of GVC funding due to regional allocation rules, rural and peripheral regions display notably weaker effects, suggesting that local ecosystem depth moderates program effectiveness. These findings highlight the dual role of GVC as both growth finance and a mechanism for reducing capital-market imperfections, while raising policy questions about the efficiency of geographically earmarked allocation. These findings demonstrate that GVC generates genuine financial additionality by improving both equity and credit access.

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1. Introduction

Technological innovation is a key driver of economic growth but is often underfunded in early stages due to market failures. While governments support R&D through grants and tax incentives to address knowledge spillovers (Arrow 1962; Jaffe 1998; Hall et al. 2010; Bloom et al. 2019), a second funding gap frequently arises during commercialization. Early-stage firms face high uncertainty and lack collateral, making external financing—whether equity or debt—difficult to obtain. These financing frictions stem from asymmetric information, leading to adverse selection and moral hazard (Akerlof 1970; Stiglitz and Weiss 1981). As a result, private investors and banks hesitate to support young, innovative firms, particularly in capital-intensive or technology-driven sectors (Hubbard 1998; Kaplan and Strömberg 2001).

To address these gaps, many governments operate public loan schemes or government venture capital (GVC) programs targeting early-stage firms. GVC is designed to screen, certify, and support firms overlooked by private investors, thereby easing capital constraints (Lerner 1999). In theory, GVC funds mitigate information asymmetries, act as quality signals, and crowd in private capital by improving firm credibility and governance. However, empirical evidence on GVC effectiveness is mixed. Some studies report positive growth effects (Bertoni et al. 2011; Brander et al. 2015), especially in hybrid public-private co-investments, while others find that GVC alone delivers limited or even negative outcomes compared to private VC (Grilli and Murtinu 2014; Alperovych et al. 2015; Cumming and MacIntosh 2006). These inconsistencies highlight the importance of fund structure, investment strategy, and context.

Two aspects of GVC effectiveness remain underexplored. First, although many programs pursue regional development goals, few studies evaluate whether GVC outcomes differ across geographic areas. Evidence suggests that private venture capital clusters in urban hubs, whereas GVC often targets rural or less-developed regions to address spatial inequities. Yet regional allocation may involve a trade-off between inclusion and efficiency if weaker ecosystems lack quality deal flow (Munari and Toschi 2015). Second, most research on GVC focuses on growth and follow-on equity funding, overlooking whether GVC improves access to debt financing, such as bank loans. If GVC enhances firm net worth and reduces lender uncertainty, it could also ease credit constraints (Mishkin and Serletis 2011).

This paper contributes to the literature on government venture capital (GVC) in three main ways. First, we move beyond firm growth to quantify GVC's effects on external finance along two margins—follow-on private equity and access to bank debt—thereby testing financial

additionality directly rather than inferring it from growth alone. Second, we embed the analysis in Sweden's institutional setting, where European Regional Development Fund (ERDF)-tied regional quotas and "use-it-or-return-it" deadlines constrain investment geography. This design allows us to examine how GVC effectiveness varies with local ecosystem depth and to assess the efficiency–equity trade-off embedded in geographically earmarked public venture capital. Third, using matched panels and staggered-timing estimators (Gormley and Matsa 2011; Callaway and Sant'Anna 2021; Sun and Abraham 2021), we provide robust evidence on dynamic treatment effects for both growth and finance.

Empirically, we evaluate four related questions: (1) whether GVC funding increases firm growth (employment and sales) compared to matched controls; (2) whether these effects differ between metropolitan and rural regions; (3) whether GVC crowds in additional private equity capital; and (4) whether it improves access to bank debt. Our results show that treated firms experience significantly higher growth in both employment and sales, with the strongest effects in metropolitan regions where innovation ecosystems are more developed. Rural regions—despite receiving a disproportionate share of GVC funding due to policy mandates—exhibit weaker outcomes. Moreover, GVC funding alleviates financing constraints by improving access to external capital: GVC-backed firms attract additional private co-investment, consistent with a crowding-in effect, and are more likely to secure bank loans as the equity injection strengthens their balance sheets and reduces information asymmetries.

The remainder of the paper is organized as follows. Section 2 reviews the relevant literature on government venture capital, financial frictions, and regional allocation. Section 3 describes the institutional setting, data, and empirical methods. Section 4 presents the results, including firm performance outcomes, financing effects, and geographic variation. Section 5 concludes with implications for theory, policy, and future research.

2. Literature review

2.1 GVC and firm performance

Government venture capital (GVC) programs aim to address financing gaps for young, innovative firms by targeting early stages of development where private investment is limited. Several studies find that GVC can enhance firm growth, especially when structured as public–private co-investments. Bertoni et al. (2011) show that high-tech start-ups backed by public VC achieve higher employment and sales growth than non-VC-backed peers, while Brander et al.

(2015) find that firms co-financed by both GVC and private VC attract greater investment volumes and have higher exit probabilities than those with only private VC. These outcomes support the notion that GVC complements rather than displaces private capital when designed to share risk and amplify resources.

However, other research highlights the limitations of standalone GVC funding. Grilli and Murtinu (2014) show that private VC significantly increases firm sales, while GVC alone has no such effect. Alperovych et al. (2015) report that Belgian firms backed solely by public VC exhibit lower post-investment productivity than those financed by private VC. Cumming and MacIntosh (2006) attribute these shortcomings to bureaucratic constraints, non-commercial objectives, and weaker selection and monitoring practices. Collectively, these findings suggest that GVC outcomes depend heavily on fund design, investment discretion, and implementation quality. While public VC can catalyze firm growth, its effectiveness is uneven and contingent on complementary mechanisms such as private investor involvement and a robust innovation environment.

2.2 Regional allocation of GVC

Many GVC programs are motivated by regional development goals and are mandated to invest in geographically underrepresented areas. The rationale is to counteract the concentration of private VC in urban innovation hubs by stimulating entrepreneurship in peripheral or lagging regions. Yet, this raises concerns about trade-offs between spatial inclusion and investment efficiency. Rigid geographic quotas may force capital into thin markets with limited deal flow, diluting GVC effectiveness.

Empirical evidence confirms this tension. Brander et al. (2015) warn that geographic mandates can reduce program performance if fund managers are constrained in sourcing high-quality projects. Munari and Toschi (2015) find that GVC has stronger firm-level effects in regions with robust ecosystems—such as dense networks of human capital, universities, and incumbent innovators—while impact is weaker in areas lacking such infrastructure. These findings imply that local context shapes GVC effectiveness and that allocating capital to less-developed regions may require complementary support (e.g., incubators or training) to yield comparable results. Thus, regional equity objectives must be carefully balanced with considerations of investment potential and long-term program sustainability.

2.3 GVC as a catalyst for private investment

One of GVC's primary theoretical functions is to certify venture quality and crowd in private capital. By conducting due diligence and offering early-stage funding, GVCs can reduce information asymmetries and signal firm viability to other investors. Empirical studies support this certification effect. Guerini and Quas (2016) find that GVC-backed European start-ups are more likely to attract private VC and achieve follow-on rounds or exits. Similarly, Bertoni et al. (2011) show that public VC enhances a firm's ability to secure additional financing, both equity and debt. Brander et al. (2015) further confirm that GVC co-investment raises the scale and success rates of total venture funding.

These findings imply that GVC, when implemented with market-oriented practices, can enhance financial access beyond its initial investment. Conversely, programs that behave like passive subsidies or invest without rigorous screening may fail to attract further capital. The structure and credibility of the GVC program are therefore central to its effectiveness in catalyzing private investment.

2.4 GVC and access to debt financing

While most studies focus on equity outcomes, GVC may also improve a firm's ability to access debt financing. Young firms typically face credit constraints due to lack of collateral, high risk, and informational opacity (Stiglitz and Weiss 1981). GVC funding can alleviate these constraints by increasing a firm's equity buffer, improving solvency ratios, and reducing default risk from the lender's perspective. Moreover, the presence of a public investor can signal quality, mitigating adverse selection and building trust with banks.

Although limited, emerging evidence supports this mechanism. Meuleman and De Maeseneire (2012) find that public R&D grants increase small firms' access to long-term credit, suggesting that certification effects extend beyond equity. Bertoni et al. (2011) observe that GVC-backed firms are more likely to attract debt capital in subsequent financing rounds. These findings indicate that GVC not only addresses the equity gap but may also indirectly alleviate credit market frictions, enabling firms to build a more balanced and flexible capital structure.

Taken together, the literature highlights both the potential and the limitations of GVC programs. Their impact on firm growth, capital access, and innovation depends on fund structure, implementation, and contextual factors such as regional ecosystem strength. Yet two areas remain underexplored: (1) the consequences of regional allocation mandates for firm outcomes,

and (2) the role of GVC in facilitating access to debt financing. This study contributes to both areas by examining the effects of a nationwide GVC program in Sweden, focusing on spatial variation and the evolution of firms' capital structure post-investment.

3. Data and econometric methods

Institutional setting and testable mechanisms. Almi Invest's GVC relies on ERDF funds that require one-to-one domestic co-financing, impose investment deadlines ('use-it-or-return-it'), and earmark capital by region. These features create clear mechanisms: screening/certification (signaling quality), balance-sheet strengthening (higher equity buffers), and ecosystem complementarity (crowding-in where private investors and deal flow are dense). From these mechanisms we derive five hypotheses (H1–H5) on employment, sales dynamics, equity crowd-in, debt access, and regional heterogeneity.

3.1. Data on government venture capital and firm variables

The state-owned company Almi AB has offered various forms of business loans and advisory services to SMEs since 1994 and has provided GVC since 2008. Almi manages lending and advisory services through 16 majority-owned regional subsidiaries across Sweden. The wholly owned subsidiary Almi Invest manages GVC investments, focusing exclusively on the seed and start-up phases. The total loan portfolio is approximately SEK 5.5 billion, and the equity capital portfolio is about SEK 1.3 billion. Almi annually provides loans amounting to around SEK 2–2.5 billion and equity capital of about SEK 200–300 million. The loan and equity capital portfolios that Almi works with are revolving, meaning that when a loan is repaid, or when Almi Invest exits a portfolio company, the funds are reused for lending to and investing in new client companies.

According to the Swedish government's ownership directive, Almi is to complement the financing solutions available in the private market. Almi's financing and advisory services should also be available throughout the country and targeted at companies with profitability and growth potential. For its GVC investments, Almi Invest receives capital for a seven- to eight-year period. Half of the funding comes from ERDF, one quarter from the Swedish government, and one quarter from Swedish regional authorities. For ERDF to provide financing, it requires: 1) that its funds are matched 50/50 by Swedish authorities; 2) any funds from ERDF that are not invested by Almi at the end of the period must be returned to ERDF. This requirement gives Almi an incentive to invest all funds even if no suitable portfolio companies are available; and 3) that the funds be earmarked for specific regions in Sweden.

Data on firms that have been financed with GVC by Almi Invest were obtained directly from Almi. The information includes the firms' corporate registration numbers, the year of the financing decision, and the financed amount in Swedish kronor (SEK). Data for GVC from Almi Invest is available for the years 2008–2020, i.e., from the first year this type of financing was offered. The raw dataset consists of 585 unique portfolio firms that have received GVC financing from Almi Invest in the period 2008–2020.

Almi Invest provides GVC funding exclusively to innovative SMEs. GVC from Almi Invest takes the form of share capital or convertible loans. Potential firms must apply for funds, meaning the initiative for funding comes from the firms themselves. When investing, Almi Invest usually requires 50 percent co-financing from the owners of the portfolio firm or a third party on equal terms. By using this requirement, Almi relies on market signals that indicate the applying firm has the potential to grow and survive, in line with Lerner (2009). A firm may receive GVC from Almi Invest in several financing rounds over different years. However, Almi can only invest up to a maximum of SEK 10 million in a portfolio company, aggregated across all rounds. Furthermore, Almi Invest applies milestone investments, meaning that portfolio firms must achieve certain goals to receive further investments from Almi. This rule reduces problems with adverse selection and moral hazard, as recommended by Lerner (2009).

The first year a firm receives GVC is defined as the year of financing for the firms in this study and is thus our event time point. After this year, the firm is referred to as treated.

The Almi database is merged with the Serrano firm database using the firms' corporate registration numbers. The Serrano database is a panel dataset covering firms and years from 1999 to 2022.[1] The database includes all Swedish limited liability companies as well as certain other types of firms (e.g., partnerships and sole proprietorships) and contains all the traditional information found in firms' balance sheets and income statements. The Serrano database is used starting from three years before the first financing in the analysis, i.e., the years 2005–2022 are used in the estimation.

From the Serrano database, information is retrieved for the outcome variables used in this study: the number of employees (*EMP*), net sales (*SALES*), total equity (*EQUITY*) and total debt (*DEBT*), all measured in thousands of SEK.[2] It also includes information on region (county)

[1] The Serrano dataset is based on information from Statistics Sweden (SCB), the Swedish Companies Registration Office (Bolagsverket), and Dun & Bradstreet.
[2] *EQUITY* is defined as the firm's total share capital, while *DEBT* refers to total interest-bearing liabilities.

and industry affiliation. A variety of financial variables are available and used in the study: total assets, equity, liabilities, liquid assets, dividends, and solvency.[3] All variables measured in SEK are adjusted using the Producer Price Index (PPI) from Statistics Sweden (SCB), with 2020 as the base year to account for inflation.

There are also a few other authorities and state-owned companies (Industrifonden, Fouriertransform, and Inlandsinnovation) in Sweden that provided GVC during the period in question. Unfortunately, the study lacks information about such GVC investments. At first glance, such government co-investments could be correlated with Almi's investments and thereby distort the statistical estimates. However, Almi and the other state-owned companies that provide GVC have entirely different profiles. Almi is the only one that exclusively invests in SMEs and focuses on the earliest phases (seed and start-up phases) and small projects (<10 million SEK). The other state-owned GVC investors focus on later stages (growth and expansion phases), investing significantly larger amounts per portfolio firm (SEK >5 million) (Industrifonden, Fouriertransform, and Inlandsinnovation), targeting specific industries (Fouriertransform: automotive industry) or regions (Inlandsinnovation: northern Sweden). Therefore, it is unlikely that Almi's investments are correlated with those of the other state-owned investors.

This focus on the seed and start-up phases allows Almi Invest to address critical financing gaps in the earliest stages of firm development. By concentrating on firms during their most vulnerable periods, Almi Invest ensures that its resources are directed to where they are most needed, laying the foundation for future firm growth and innovation.

A distinctive feature of Almi Invest is that a large share of its capital originates from the European Regional Development Fund (ERDF) and Swedish regional authorities. These funding sources (i) require one-to-one domestic co-financing, (ii) impose "use-it-or-return-it" deadlines, and (iii) earmark funds to specific regions. While these rules ensure broad spatial coverage, they can dilute average project quality in thin markets, creating a testable prediction that GVC effects will be stronger in regions with deeper private investment networks—such as metropolitan areas.

[3] See Section 3.3. about matching.

3.2. Stacked difference-in-difference method with panel data

The empirical analysis examines how government venture capital (GVC) affects firm outcomes such as employment, sales, equity, and debt. The main identification strategy relies on a DiD design with staggered treatment timing, where some firms receive GVC financing in different years while others never receive it.

Following Gormley and Matsa (2011), we estimate a stacked DiD specification that includes both firm-cohort and year-cohort fixed effects. This design captures the specific combination of firms and treatment cohorts while flexibly controlling for time-varying aggregate shocks that may differ across cohorts. The Gormley–Matsa specification is particularly suited for data where the treatment timing varies across groups and where firm-specific and cohort-specific heterogeneity could bias standard two-way fixed-effects estimators.

The baseline equation is specified as:

$$Y_{ft} = \alpha_f + \mu_t + \delta_{c(f)} + \theta_{txc(f)} + \beta\, Treated_{ft} + \varepsilon_{ft} \quad . \qquad [1]$$

where Y_{ft} denotes the outcome variable for firm f in year t (employment, sales, equity, or debt), α_f are firm fixed effects, μ_t are year fixed effects, $\delta_{c(f)}$ are cohort (treatment-year) fixed effects, and $\theta_{txc(f)}$ are year-by-cohort interactions that absorb common shocks within cohorts. The binary variable $Treated_{ft}$ equals 1 for treated firms in years ≥ the first year of GVC financing, and 0 otherwise.

The coefficient β identifies the average treatment effect on the treated (ATT), assuming parallel trends between treated and matched control firms before treatment. This assumption is supported by the design of the propensity-score matching (PSM), which ensures that treated and control firms have comparable observable characteristics—including ex-ante financial constraints—before receiving GVC funding.

In our primary DiD regressions we do *not* include additional time-varying control variables (beyond firm and time fixed-effects) because our matched-sample design ensures treated and control firms are comparable in pre-treatment characteristics and trends. Under the conditional parallel-trends assumption the DiD estimator identifies the ATT even without extra covariates (Angrist and Pischke 2009). Moreover, recent work demonstrates that including arbitrary time-varying covariates may introduce bias unless strong assumptions hold—specifically the "two-way common causal covariates (CCC)" assumption (Karim and Webb 2024). To avoid specification-search bias ("data mining") we thus preference a parsimonious specification,

focusing on matching and event-study diagnostics rather than extensive control-variable inclusion.

We estimate equation (1) both by OLS with firm-clustered standard errors and by Poisson pseudo-maximum likelihood (PPML) to account for heteroskedasticity, a concentration of low values in the outcome variables, and skewness in outcomes measured in SEK.[4]

The PPML estimator also allows for many low-valued observations but not zeros. Therefore, zero values in the dependent variables were replaced by small positive constants—0.1 for employment and 1 (thousand SEK) for sales, debt, and equity—to enable estimation while preserving the relative scale of the data.

The Gormley and Matsa (2011) stacked DiD model serves as our main specification because it directly fits the structure of our matched panel with one treatment episode per firm and balanced pre- and post-treatment observations. The model efficiently exploits within-firm variation while controlling for cohort-specific shocks through firm-cohort and year-cohort fixed effects, providing a transparent and easily interpretable benchmark for the causal effect of GVC.

To test robustness and verify that the results are not driven by the choice of estimator or by heterogeneous treatment effects, we estimate four complementary models: (i) Gormley and Matsa (2011) using OLS; (ii) Gormley and Matsa (2011) using PPML; (iii) Callaway and Sant'Anna (2021); and (iv) Sun and Abraham (2021).

The Callaway and Sant'Anna (2021) approaches allow estimation of dynamic treatment effects using a fixed control group of never-treated firms, which directly matches our design based on propensity-score matching.[5] The Sun and Abraham (2021) estimator provides an additional event-study specification that corrects for potential bias in conventional two-way fixed-effects designs with staggered treatment timing. These two estimators are therefore used as robustness checks to confirm that the dynamic treatment effects identified in the main Gormley-and-Matsa model are not sensitive to functional-form or aggregation assumptions.

Moreover, Wooldridge (2025) shows that our Gormley and Matsa (2011) specification with firm-cohort and year-cohort fixed effects is algebraically equivalent to the extended two-way fixed-effects (two-way Mundlak) model, thereby confirming the theoretical validity of the

[4] An average of 32 percent of the observations has a 0 value for *EMP*, 18 percent for *SALES*, 10 percent for *EQUITY* and 2 percent for *DEBT*, calculated for both treated and control firms.
[5] This estimator is implemented in Stata with the csdid command and wild-bootstrap inference (wboot option) to obtain robust p-values and confidence intervals under potential heteroskedasticity and unbalanced panels.

baseline equation. Hence, the four empirical estimations together ensure that our findings are theoretically consistent and robust to heterogeneous treatment timing and estimator choice.

Formally, the estimand identified by Equation (1) under the conditional parallel-trends assumption and no anticipation can be expressed as the average treatment effect on the treated (ATT):

$$\text{ATT} = \text{E}\left[Y_{ft}(1) - Y_{ft}(0) \,\middle|\, \text{treated firms} \right] . \qquad [2]$$

This parameter measures the average change in outcomes for treated firms after receiving GVC relative to their matched controls.

Finally, dynamic treatment effects are visualized through event-study plots based on Callaway and Sant'Anna (2021) and Sun and Abraham (2021), testing whether pre-treatment coefficients are jointly insignificant (validating the parallel-trends assumption) and whether post-treatment coefficients become significantly positive.

3.3. Matching method to create a control group

The Almi database includes only firms that received government venture capital (GVC) financing. Information about rejected or non-applicant firms is unavailable. To identify the causal effects of GVC support, it is therefore necessary to construct appropriate control groups of comparable non-treated firms. Because Almi's portfolio firms are not randomly selected—typically being small or medium-sized enterprises (SMEs <250 employees) with weaker balance sheets and limited access to external finance—propensity-score matching (PSM) is applied to ensure comparability on observable pre-treatment characteristics (Rosenbaum and Rubin 1983; Caliendo and Kopeinig 2008).

PSM estimates each firm's probability of receiving GVC financing one or two years before the actual investment and then pairs each treated firm with a non-treated firm that exhibits a similar predicted probability (Rosenbaum and Rubin 1983). One-to-one nearest-neighbor matching without replacement is used, and the analysis is restricted to the region of common support to guarantee that each treated firm has an observationally similar control. This procedure removes selection bias stemming from observable differences while maintaining a sample composition representative of the treated firms.

The propensity score is obtained from the following logit model:

$$\Pr(GVC_{f,t} = 1) = \Lambda(\theta_0 + \theta_1 KZ_{f,\bar{t}} + \mathbf{X}'_{f,t-1}\boldsymbol{\gamma}_1 + \mathbf{X}'_{f,t-2}\boldsymbol{\gamma}_2 + \theta_2 X_{f,t-2} + \theta_3 AGE_{ft} + \delta_i + \delta_r + \delta_t + \eta_{f,t}) . \quad [3]$$

where $GVC_{f,t}$ = 1 if firm *f* receives Almi financing in year *t*, and 0 otherwise; $\Lambda(\cdot)$denotes the logistic cumulative-distribution function; $KZ_{f,\bar{t}}$ is the firm's Kaplan–Zingales (KZ) index, averaged over the two pre-treatment years (t–1 and t–2), which measures the firm's degree of financial constraint; $\mathbf{X}_{f,t-1}$ and $\mathbf{X}_{f,t-2}$ are vectors of additional firm-specific covariates lagged one and two years, including indicators of age, size, assets, liquidity, solvency, and performance; δ_i, δ_r, and δ_t are industry, regional, and year fixed effects; and $\eta_{f,t}$ is the residual.

The KZ index is constructed following Kaplan and Zingales (1997, Table IV) and Lamont et al. (2001), using firm-level accounting ratios for cash flow, Tobin's Q proxy, leverage, dividends, and cash holdings, each scaled by total assets. The index is multiplied by –1 so that higher values correspond to stronger financial constraints. All explanatory variables are measured strictly before treatment to avoid endogeneity.

Outcome-specific matching models

Because the causal effects are evaluated for four outcome variables—employment, net sales, external equity, and debt—separate matchings are performed for each outcome. All logit models share a common core of covariates (KZ index, age, industry, region, and year dummies) to maintain a stable identification structure targeting the financial-constraints mechanism. To strengthen comparability in pre-treatment trajectories, each outcome-specific model additionally includes at least one lag—lag (–1) and/or lag (–2)—of the corresponding outcome variable. Cross-lags of other outcomes (for example, lagged sales in the employment model) are included when they improve balance without narrowing the common support excessively. This outcome-specific tailoring ensures that treated and control firms exhibit similar performance paths before treatment while preserving a consistent core specification across outcomes. Concretely, the key specifications are depicted in Table 1.

Table 1. Covariates used in propensity-score matching for each outcome variable

Outcome variable	Lagged outcome variables	Optional cross-lags
EMP	*EMP*(t–1) and/or *EMP*(t–2)	*SALES*(t–1)
SALES	*SALES*(t–1) and/or *SALES*(t–2)	*EMP*(t–1)
EQUITY	*EQUITY*(t–1) and/or *EQUITY*(t–2)	*SALES*(t–1), *EMP*(t–1)
DEBT	*DEBT*(t–1) and/or *DEBT*(t–2)	*SALES*(t–1), *EMP*(t–1)

Note: All models include a common core of covariates: the Kaplan–Zingales (KZ) index (average of t–1 and t–2), firm age, and industry, regional, and year dummies. Additional financial ratios are included when they improve covariate balance: Total assets (*ASSET*, t–1) and solvency (*SOL*, t–1). All variables are measured prior to treatment. Lagged values refer to one and two years before the GVC investment year.

Selection of preferred matchings

For each outcome, roughly 25 alternative matchings were estimated with varying combinations of covariates and lag structures. The preferred specification for each outcome was chosen according to covariate balance and parallel-trends criteria. First, balance tests required a mean standardized bias below 5 %, non-significant t-tests for all covariates, a clear reduction in pseudo-R^2, and an insignificant joint χ^2-test of covariate equality. Second, dynamic pre-treatment coefficients from the corresponding event-study regressions had to be statistically indistinguishable from zero, confirming the parallel-trends assumption. Only matchings that satisfied both sets of criteria were retained for the DiD estimation. If the pre-treatment coefficients were significantly different from zero, the parallel-trends assumption was considered violated, and no causal inference was drawn for that outcome.

This approach ensures that treated and matched control firms are statistically indistinguishable in observable pre-treatment characteristics and follow similar pre-treatment trajectories in the outcome variables.

To provide full transparency regarding model specification, Table A5 in Appendix A lists all variables included in the twelve propensity-score matchings underlying the four outcome analyses (employment, sales, equity, and debt). The table shows which lagged outcomes, cross-lags, and financial ratios enter each logit specification, complementing the representative matching diagnostics reported in Tables A1–A4.

Sample restrictions

Only limited-liability companies are included in the tread and control groups, as the balance-sheet and income-statement variables required to compute the KZ index and other covariates are not consistently available for partnerships or sole proprietorships.

3.4. Descriptive statistics

In the database, we have 585 unique firms that have received equity capital from Almi Invest at least once during the period 2008–2020. When these data are merged with the Serrano database, 584 of the 585 firms can be identified, all being limited liability companies. A requirement for inclusion in the DiD estimations is that firms must be at least two years old at the time of financing, enabling the observation of pre-policy trends in the outcome variables. This criterion excludes 151 firms, leaving 433 treated firms for matching and DiD estimation.

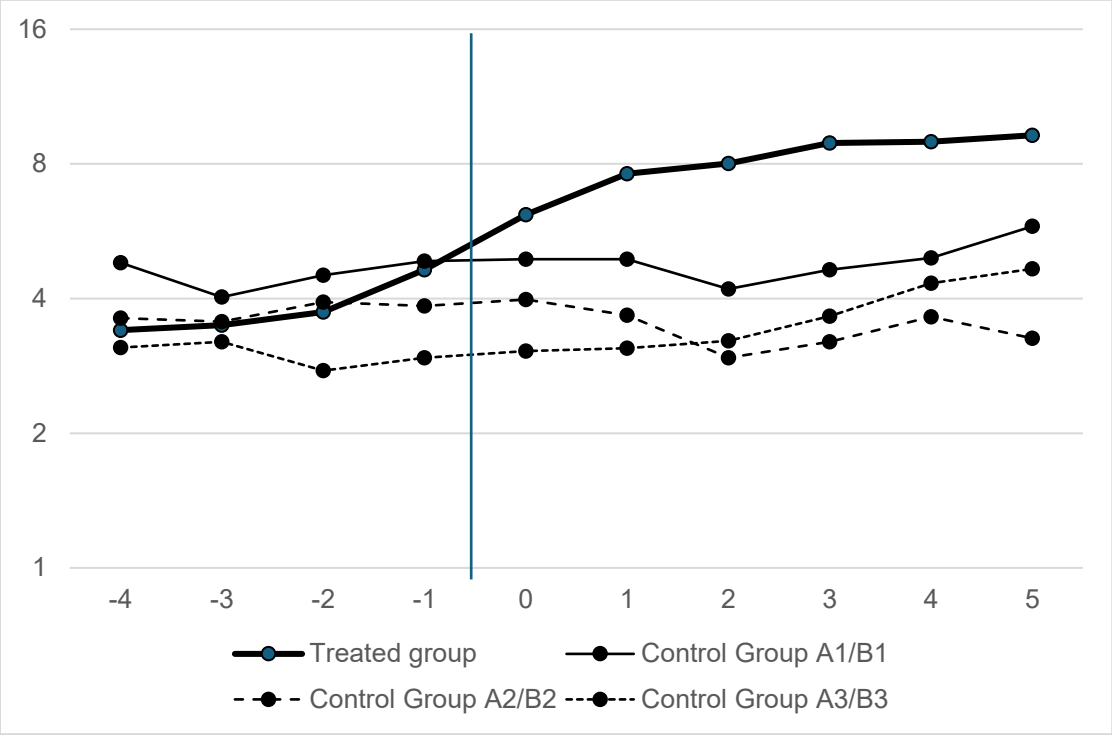
To verify that this age restriction does not distort the representativeness of the treated sample, we compare in Appendix B the development of employment and sales among younger and older portfolio firms. The descriptive figures show broadly similar growth trajectories across age cohorts, although—as expected—the youngest firms experience steeper initial expansion during the first two years after investment. From year t+1 onward, the trends of all age groups converge, indicating that the exclusion of very young firms mainly removes short-term volatility rather than altering long-run patterns.

Among the treated firms included in the DiD analysis (aged ≥ 2 years), average employment was 4.6 full-time equivalents, mean net sales around 4 MSEK (≈ 0.4 M EUR), total equity around 5 MSEK, and total debt around 5 MSEK in the year prior to receiving GVC. The average first-round GVC investment was roughly 2 MSEK (≈ 0.2 M EUR) accompanied by an additional 2 MSEK in required private co-investment. Together, these contributions represent a substantial financial injection relative to firm size.

Because three separate propensity-score matchings are performed for each of the four outcome variables, the analysis is based on twelve distinct matched samples with partly different control groups. Consequently, reporting a single pooled descriptive-statistics table would not be meaningful or comparable across models. Instead, Tables A1–A4 in Appendix A present representative examples—one for each outcome variable—showing the mean values of matching covariates for treated and control firms, standardized mean differences, *t*-tests, and model-fit diagnostics (pseudo-R^2 and χ^2). Across all reported matchings, the differences in covariate means are statistically insignificant, confirming that treated and control firms are well balanced on all relevant pre-treatment characteristics.

The raw descriptive development of employees and sales for treated firms and their matched control groups is shown in Figures 1 and 2.[6] In both diagrams, the treated and control groups follow nearly parallel trajectories during the pre-policy period (years –4 to –1), suggesting that the parallel-trends assumption is reasonable. After the first GVC investment (t=0), treated firms exhibit a visibly steeper growth path, most pronounced for employment. The development of net sales is more gradual, with the divergence from the control group becoming evident after one to two years—consistent with the time required for new investments, personnel, and innovations to generate revenues.

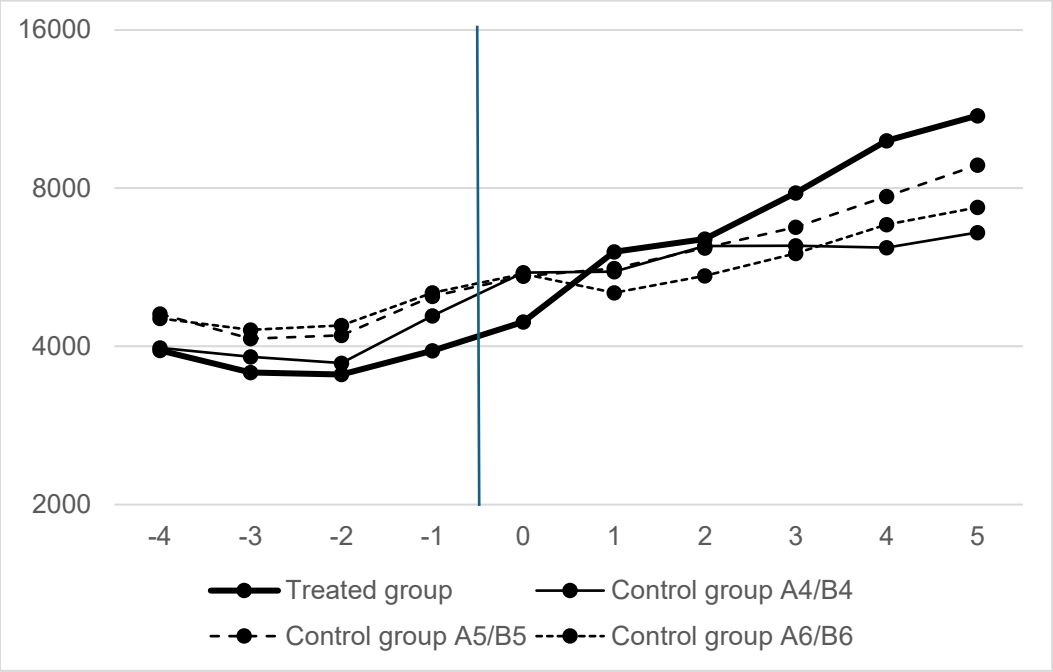
Figure 1. Development of employment in treated and control firms, logarithmic scale.



Note: The figure shows the raw average number of employees by event time for firms receiving GVC funding (treated) and their matched control firms. Each firm's timeline is normalized relative to its first year of GVC investment (t=0), with t<0 representing pre-treatment years and t>0 post-treatment years. Values are expressed as averages within each group.

[6] These control groups are included in the estimations in Section 4.1.

Figure 2. Development of net sales (thousand SEK) in treated and control firms, logarithmic scale.

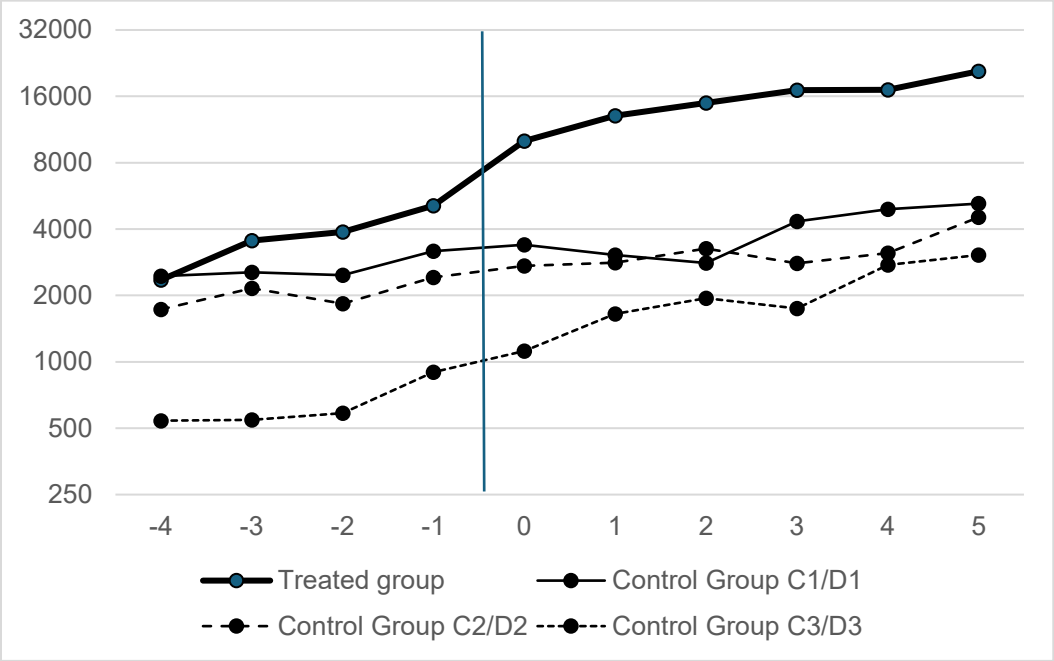


Note: The figure shows the raw average net sales by event time for firms receiving GVC funding (treated) and their matched control firms. Each firm's timeline is normalized relative to its first year of GVC investment (t=0), with t<0 representing pre-treatment years and t>0 post-treatment years. Values are expressed as averages within each group.

Figure 3 presents the development of total equity (*EQUITY*) for the treated group and the control groups in the years before and after the first round of GVC funding. The pre-treatment trajectories are closely aligned, indicating that the treated and control firms had similar levels and growth trends in equity before receiving financing. Following the investment (t=0), the treated firms show a sharp and sustained increase in total equity, reflecting the direct capital injection as well as subsequent private co-investment. This pattern supports the hypothesis that GVC funding enhances firms' ability to attract additional external equity and strengthens their capital structure over time.

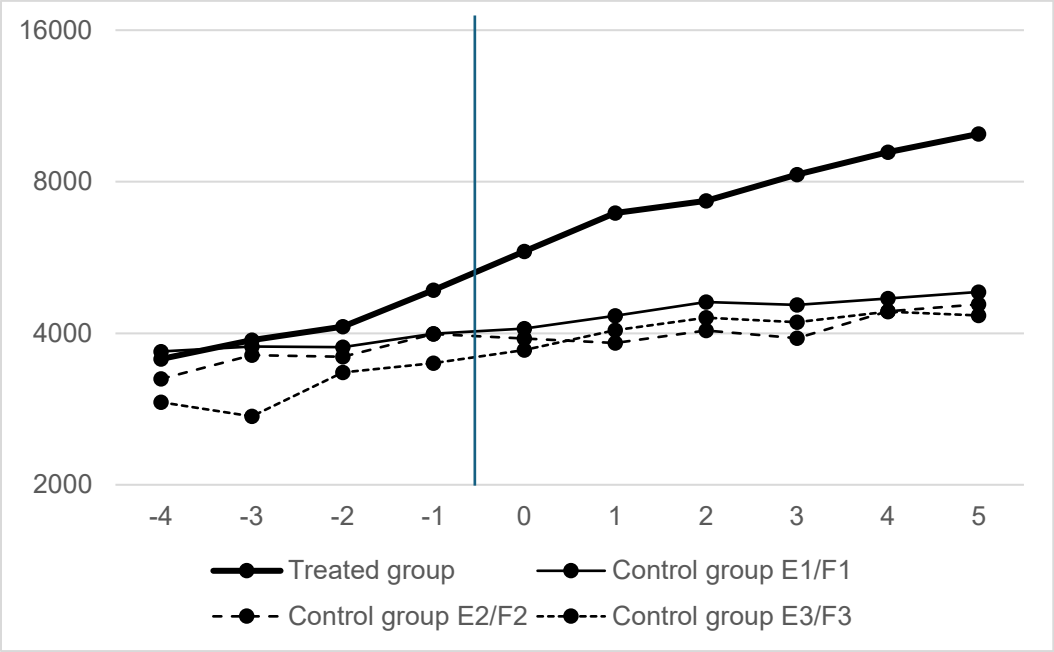
Figure 4 shows the corresponding development of total debt (*DEBT*) for treated and control firms. The pre-treatment period again reveals comparable and parallel trends, confirming the quality of the matching procedure. After treatment, however, a pronounced divergence appears: treated firms accumulate substantially more debt than their controls. This outcome supports the interpretation that GVC participation improves access to external credit markets by signaling firm quality, reducing information asymmetries, and increasing lenders' confidence through stronger equity positions.

Figure 3. Development of total equity (thousand SEK) in treated and control firms,, logarithmic scale.



Note: The figure shows the raw average equity capital by event time for firms receiving GVC funding (treated) and their matched control firms. Each firm's timeline is normalized relative to its first year of GVC investment (t=0), with t<0 representing pre-treatment years and t>0 post-treatment years. Values are expressed as averages within each group.

Figure 4. Development of total debt (thousand SEK) in treated and control firms,, logarithmic scale.



Note: The figure shows the raw average total debt by event time for firms receiving GVC funding (treated) and their matched control firms. Each firm's timeline is normalized relative to its first year of GVC investment (t=0), with t<0 representing pre-treatment years and t>0 post-treatment years. Values are expressed as averages within each group.

3.5. Hypotheses for empirical testing

As discussed in Section 3.4, the GVC investment represents a major financial shock for recipient firms, roughly doubling their available capital through public and matched private equity. Building on this empirical context, this section develops the theoretical mechanisms through which GVC funding is expected to affect firm performance and financing outcomes.

By providing equity capital to young and innovative firms that face severe financing constraints, GVC directly relaxes liquidity constraints and enables firms to expand operations, recruit personnel, and accelerate product development. Such short-term liquidity effects should be materialized quickly after investment as recipient firms hire additional employees and initiate new projects.

At the same time, the impact on revenues is expected to appear with a delay. New staff and R&D activities require time to translate into new products, market entry, and commercial sales. Hence, sales responses are expected to lag behind employment effects as the output of new investments and innovations reaches the market (Hall and Lerner 2010; Kerr and Nanda 2011).

GVC may also influence firms' external financing. By strengthening the balance sheet and signaling credibility to private investors, public venture capital can attract additional equity capital and facilitate access to debt financing from banks and other lenders.

In the equity market, this operates primarily through a certification and crowding-in mechanism. When GVC investors undertake due diligence, take board seats, and monitor portfolio firms, they reduce information asymmetries and transaction costs for private co-investors (Lerner 2002; Brander et al. 2015). The public endorsement and governance oversight associated with GVC participation signal firm quality, increasing investor confidence and lowering perceived risk. Moreover, the requirement of private co-financing in most GVC programs leverages additional private capital, creating a multiplier effect that extends well beyond the initial public investment. Consequently, GVC participation can accelerate firms' progression toward later financing rounds and eventual market exit, strengthening financial resilience and reducing dependence on public support.

In credit markets, high net worth or equity capital mitigates asymmetric-information problems by providing lenders with a financial cushion that reduces default risk (Mishkin & Serletis, 2011). Firms with stronger equity bases can offer collateral and thereby lower lenders' exposure

in case of financial distress. These certifications and signaling mechanisms are well documented in the venture-capital and banking literature and constitute an important rationale for public intervention in early-stage finance.

Finally, GVC effects may differ across regions depending on the density of private-investment networks, availability of skilled labor, and local innovation ecosystems. In metropolitan areas, where private investors, knowledge institutions, and entrepreneurial networks are concentrated, public venture capital can act as a complementary source of finance and generate strong crowding-in effects. In contrast, rural and peripheral regions receive a disproportionately large share of Almi Invest's total funding relative to their firm population. This broader allocation may force regional funds to invest in a wider range of projects, including those of lower quality or higher risk, thereby reducing the average effectiveness of GVC support. Consequently, regions with more developed financial and entrepreneurial infrastructures—such as metropolitan areas—are expected to exhibit stronger responses to GVC investments.

In the Swedish setting, the ERDF-driven earmarking and co-finance requirements sharpen these mechanisms. Screening and board monitoring by Almi Invest, together with mandatory private co-investment, should certify venture quality and crowd-in private equity; the public equity buffer should also relax bank-lending frictions. Because earmarking pushes more capital to peripheral regions, where deal flow and follow-on investors are scarcer, we expect weaker average effects outside metropolitan ecosystems.

The five hypotheses are grounded in the standard theory of credit-market imperfections, where asymmetric information and financing frictions limit entrepreneurial growth (Akerlof 1970; Stiglitz and Weiss 1981). GVC is expected to alleviate these distortions by certifying firm quality and expanding external finance.

Hypothesis 1. GVC funding increases the number of employees in recipient firms immediately after investment.

Hypothesis 2. GVC funding increases firms' net sales, but with a lag of two to three years following the investment.

Hypothesis 3. GVC funding increases firms' ability to attract additional private equity capital.

Hypothesis 4. GVC funding improves firms' access to external debt financing by enhancing creditworthiness and reducing information asymmetries.

Hypothesis 5. The effects of GVC funding vary across regions and are expected to be strongest in metropolitan and innovation-dense areas.

4. Results of the estimations

4.1. Effects of GVC funding on firm growth

This section tests Hypotheses 1 and 2, which predict that GVC funding increases employment immediately and sales with a short lag.

Matching quality

The preferred matchings for both outcomes satisfy all balance and parallel-trend criteria. Tables A1 and A2 report mean standardized biases below 5 %, insignificant t-tests for all main covariates, and substantial reductions in pseudo-R^2 and χ^2 statistics after matching, confirming that treated and control firms are statistically comparable before treatment.

Table 2. Baseline DiD-estimates of GVC effects on employment and net sales, OLS.

Outcome variable	*EMP*			*SALES*		
	Method = DiD OLS with firm-cohort and year-cohort fixed effects					
Explanatory variables	Model A1	Model A2	Model A3	Model A4	Model A5	Model A6
D-post	2.78 ***	3.17 ***	4.43 ***	1,894 ***	1,586 ***	2,442 ***
	(0.49)	(0.52)	(1.33)	(686)	(528)	(670)
No. of clusters	723	731	822	724	814	723
No. of observations	6,212	6,295	7,039	6,043	6,798	6,116
F-value	4.23 ***	3.85 ***	3.86 ***	3.37 ***	4.36 ***	2.33 ***
Adjusted R^2	0.73	0.84	0.49	0.69	0.77	0.73
Statistics from event-study graph						
F-test pre-trend-equality (p-value)	1.94 (0.15)	2.73 * (0.07)	1.50 (0.21)	0.73 (0.53)	2.24 (0.52)	1.12 (0.34)
Statistics from robustness estimation Callaway and Sant'Anna (2021)						
CS pre-average (95 % conf. interval)	0.53 * (0.02 – 1.04) [a)]	0.39 (–0.10 – 0.89)	1.08 * (0.02 – 2.15) [b)]	179 (–286 – 645)	–68 (–496 – 359)	146 (–321 – 613)
CS post-average (95 % conf. interval)	2.24 (0.55 – 3.93)	1.73 ** (0.02 – 3.45)	3.47 ** (0.02 – 6.91)	2,659 ** (550 – 4,768)	2,497 ** (775 – 4,219)	2,780 ** (533 – 5,026)
Statistics from robustness estimation Sun and Abraham (2021)						
Wald-$\chi^2(3)$ pre-trend test (p-value)	7.35 * (0.06)	5.11 (0.16)	5.55 (0.14)	2.82 (0.42)	2.77 (0.43)	3.29 (0.35)

All regressions include firm- and year-cohort fixed effects (Gormley and Matsa 2011), as well as region fixed effects to capture persistent regional differences in economic conditions. This specification controls for unobserved firm heterogeneity, cohort-specific shocks, and spatial variation in treatment intensity. Standard errors are clustered at the firm level. Coefficients represent average treatment effects on the treated (ATT) relative to the pre-treatment year (t = –1). The models are based on different matchings (A1/B1, A2/B2, A3/B3, A4/B4, A5/B5, A6/B6). Robust standard errors are clustered at the firm level. ***, *** and * denote statistical significance at the 1-, 5- and 10-percent levels.

[a)] Although the joint pre-trend test is significant different from zero, all four individual estimates from t–4 to t–1 are insignificant, see Figure 7.

[b)] Although the joint pre-trend test is significant different from zero, three out of four individual estimates from t–4 to t–1 are insignificant.

Baseline results

The DiD estimations based on Gormley and Matsa (2011) using OLS and PPML are presented in Tables 2 and 3. For employment, the OLS models (A1–A3) show increases of 2.7–4.4 employees, significant at the 1 percent level, while the PPML models (B1–B3) indicate proportional increases of 39–62 percent relative to the control group. For sales, the OLS models (A4–A6) show increases of 1.6–2.4 million SEK, and the PPML estimates (B4–B6) imply growth of 45–60 percent, all highly significant.

Table 3. Baseline DiD-estimates of GVC effects on employment and net sales, PPML.

Outcome variable	*EMP*			*SALES*		
	Method = DiD PPML with firm-cohort and year-cohort fixed effects					
Explanatory variables	Model B1	Model B2	Model B3	Model B4	Model B5	Model B6
DiD (D*T)	0.33 ***	0.45 ***	0.48 ***	0.37 ***	0.38 ***	0.47 ***
	(0.06)	(0.07)	(0.06)	(0.10)	(0.09)	(0.10)
No. of clusters	723	731	822	724	814	723
No. of observations	6,212	6,295	7,039	6,073	6,798	6,116
Pseudo R^2	0.69	0.76	0.67	0.86	0.88	0.87
DiD-effect	39.1 %	56.8 %	61.6 %	44.7 %	46.2 %	60.0 %
Statistics from event-study graph						
F-test pre-trend-	0.43	6.76 **	20.31 ***	1.85	1.72	0.72
equality (p-value)	(0.81)	(0.03)	(0.01)	(0.60)	(0.63)	(0.86)

All regressions include firm- and year-cohort fixed effects (Gormley and Matsa 2011), as well as region fixed effects to capture persistent regional differences in economic conditions. This specification controls for unobserved firm heterogeneity, cohort-specific shocks, and spatial variation in treatment intensity. Standard errors are clustered at the firm level. Coefficients represent average treatment effects on the treated (ATT) relative to the pre-treatment year (t = –1). The models are based on different matchings (A1/B1, A2/B2, A3/B3, A4/B4, A5/B5, A6/B6). Robust standard errors are clustered at the firm level. ***, *** and * denote statistical significance at the 1-, 5- and 10-percent levels.

Event-study diagnostics

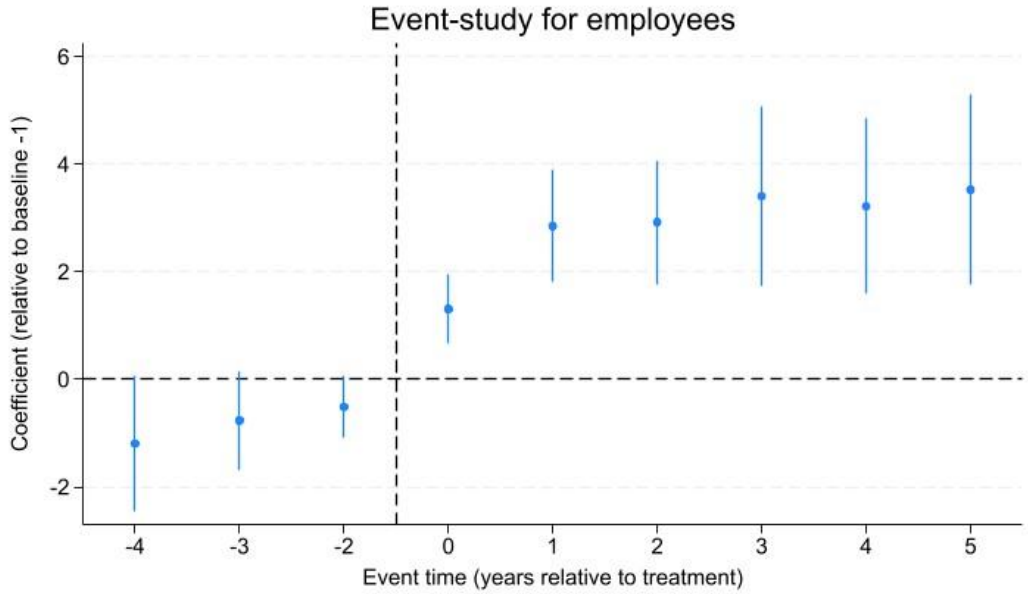
Event-study graphs (Figures 5–12) evaluate dynamic treatment effects and the parallel-trends assumption.

Employment (*EMP*). The pre-treatment coefficients across the four event-study specifications (Figures 5–8 for Models A1 and B1) fluctuate around zero, suggesting broadly similar trends between treated and control firms before GVC funding. Figures 5–7 provide the clearest visual support for parallel pre-treatment paths, while Figure 8 (Sun & Abraham) passes the joint pre-trend test at the 5 percent level. The immediate post-treatment response in year 0 (Figures 5–8)

indicates that firms begin to expand staffing almost at once after receiving GVC support, consistent with a direct liquidity effect of the investment. However, the pre-trend diagnostics are not fully robust across all matchings, as can be seen from the different pre-trend tests (A1–A3, B1–B3), and some specifications (Appendix C, Figures C1–C2) display minor deviations from the parallel-trends requirement. Consequently, while the direction and timing of the employment effects are economically plausible and consistent with expectations, the statistical evidence is somewhat mixed, and the causal interpretation for employment remains less robust than for the other outcome variables (*SALES*, *EQUITY* and *DEBT*).

Sales (*SALES*). The pre-treatment coefficients are consistently insignificant, and both Callaway and Sant'Anna (2021) and Sun and Abraham (2021) tests confirm valid parallel trends (Table 1). Figures 9–12 show that the treatment effect appears only after two to three years, as expected for technology-intensive firms that must first expand production and marketing capacity before realizing higher revenues. The post-treatment effects grow steadily and remain statistically significant throughout the observation window, demonstrating a strong and persistent sales response to GVC funding.

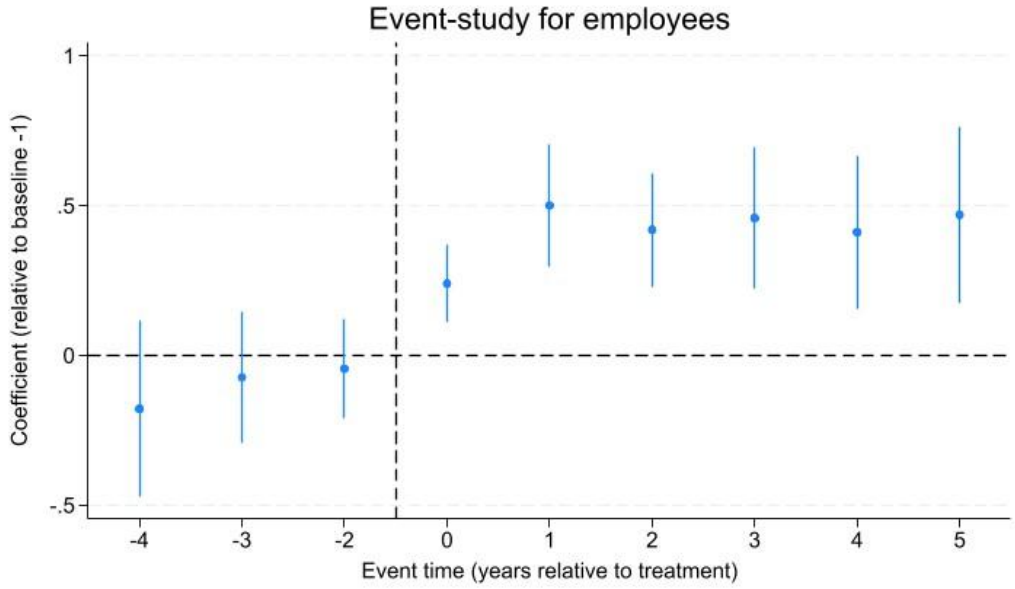
Figure 5. Annual DiD-estimates of the effects of GVC funding on employment (OLS, Model A1).



Note: The figure plots estimated coefficients for each year relative to the treatment year (t=0; baseline = t–1). Vertical bars indicate 95 % confidence intervals clustered at the firm level.

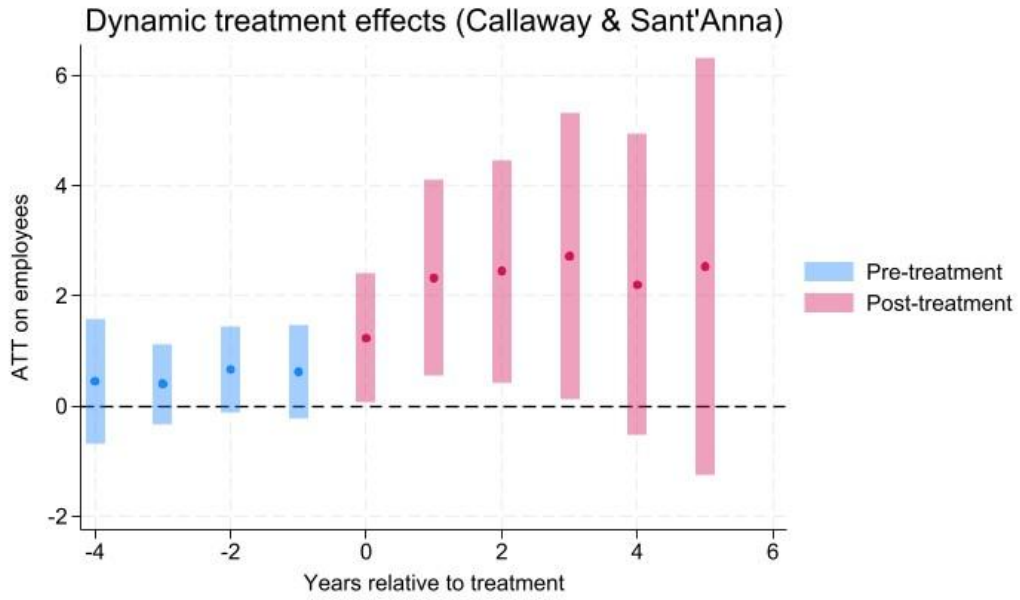
In summary, the estimations provide consistent support for Hypotheses 1 and 2. The employment effects appear immediately after the capital injection, reflecting a rapid relaxation of liquidity and labor constraints, but their magnitude and significance are somewhat sensitive to model specification and matching quality. In contrast, the sales effects emerge only after a two- to three-year lag and are remarkably robust across all estimators—OLS, PPML, Callaway–Sant'Anna, and Sun–Abraham—indicating a durable revenue response. Together, these findings suggest that GVC funding first facilitates short-term capacity expansion and subsequently enables sustained sales growth as firms scale production and commercialize innovations.

Figure 6. Annual DiD-estimates of the effects of GVC funding on employment (PPML, Model B1).



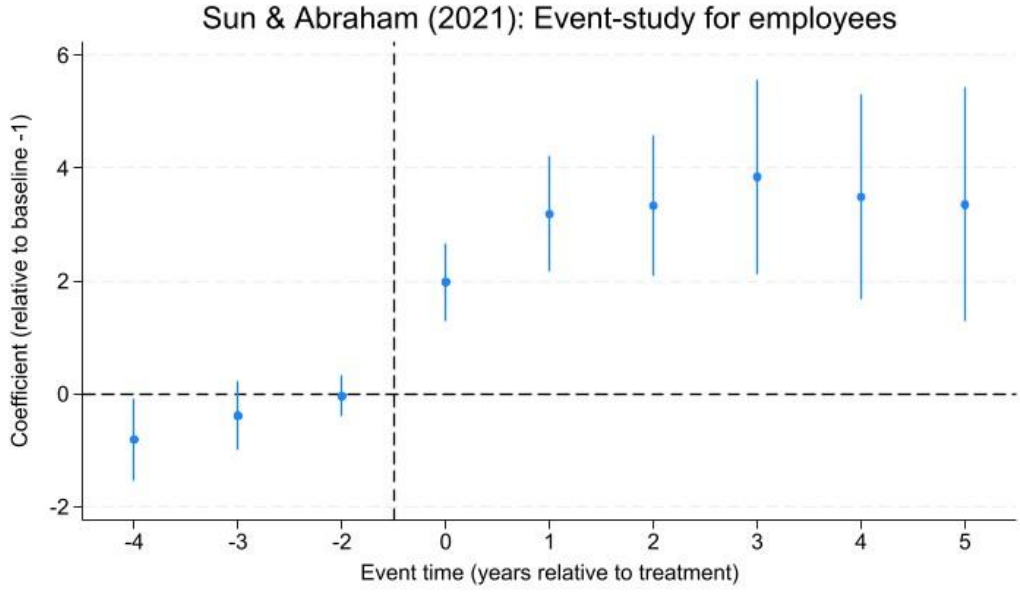
Note: The figure plots estimated coefficients for each year relative to the treatment year (t=0; baseline = t–1). Vertical bars indicate 95 % confidence intervals clustered at the firm level.

Figure 7. Dynamic treatment effects of GVC funding on employment estimated using Callaway and Sant'Anna (2021) (Model A1).



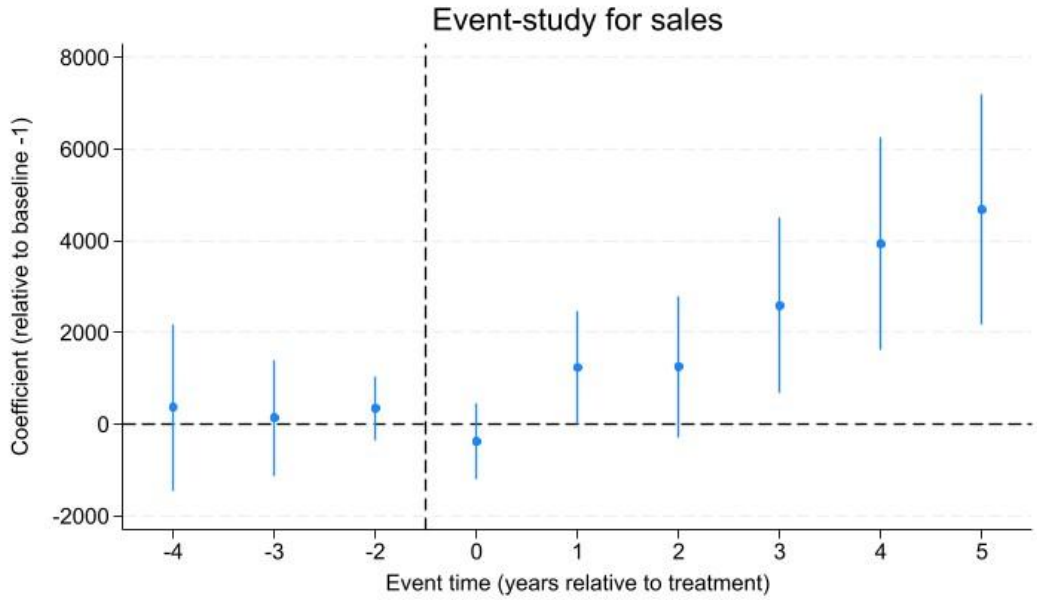
Note: The figure shows average treatment effects on the treated ($ATT(g,t)$) relative to the pre-treatment period. Blue bars denote pre-treatment coefficients, while red bars post-treatment effects, with 95 % confidence intervals based on wild bootstrap inference.

Figure 8. Event-study estimates of dynamic treatment effects on employment following Sun and Abraham (2021) (Model A1).



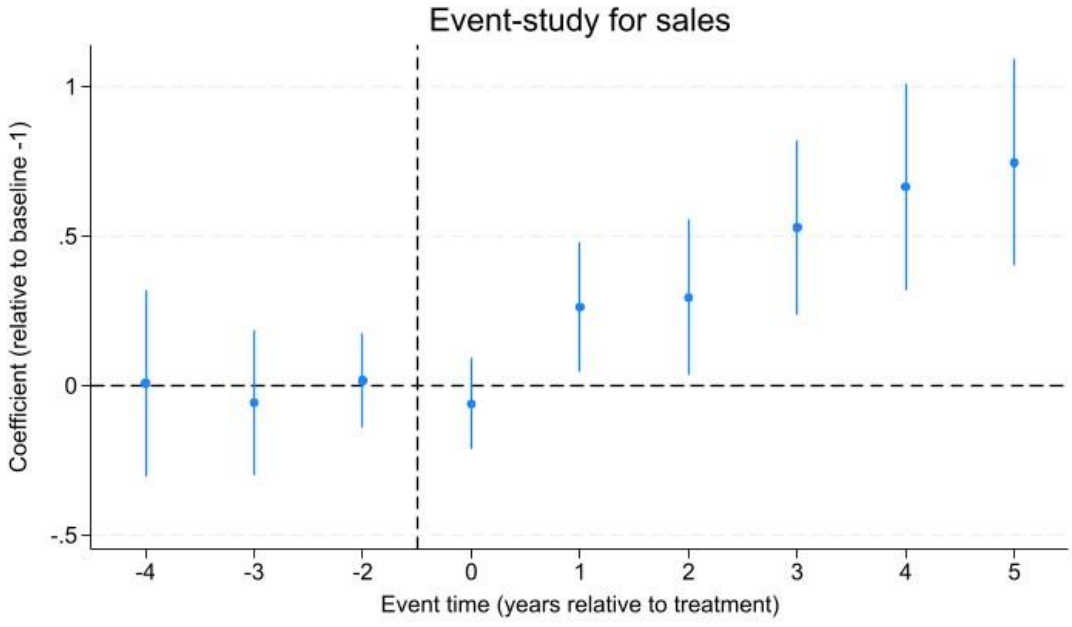
Note: Coefficients are shown relative to the baseline year (t–1), with 95 % confidence intervals. The estimator corrects for potential bias in conventional two-way fixed-effects models with staggered treatment timing.

Figure 9. Annual DiD-estimates of the effects of GVC funding on net sales (thousand SEK) (OLS, Model A4).



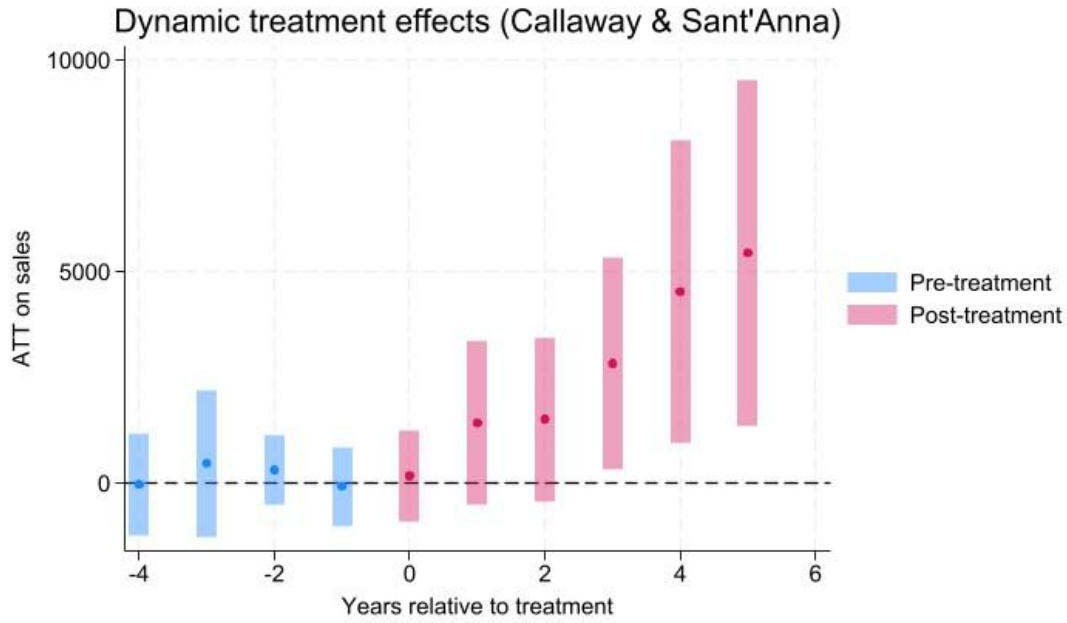
Note: The figure plots estimated coefficients for each year relative to the treatment year (t=0; baseline = t–1). Vertical bars indicate 95 % confidence intervals clustered at the firm level.

Figure 10. Annual DiD-estimates of the effects of GVC funding on net sales (thousand SEK) (PPML, Model B4).



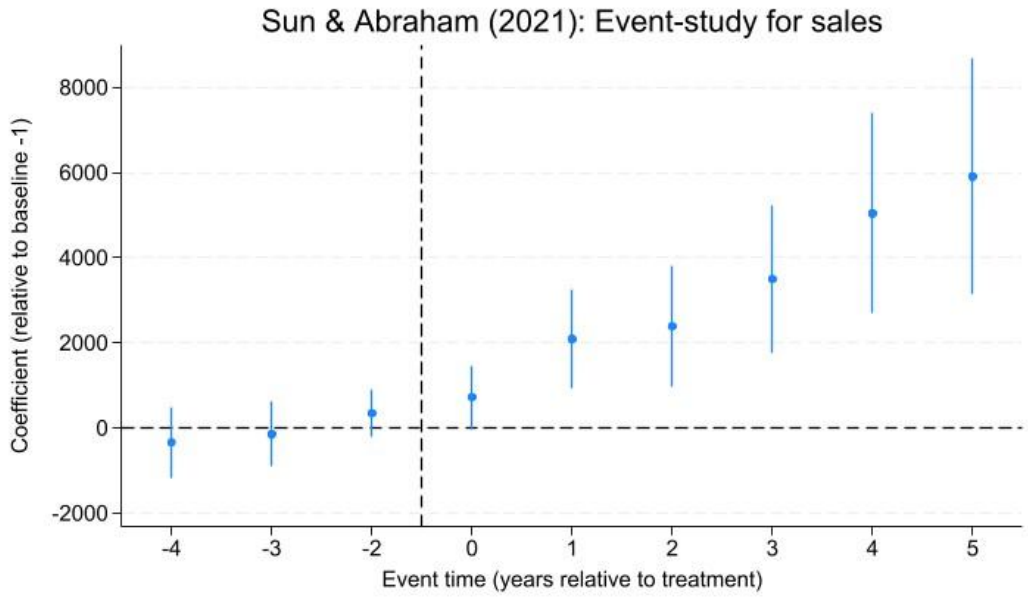
Note: The figure plots estimated coefficients for each year relative to the treatment year (t=0; baseline = t–1). Vertical bars indicate 95 % confidence intervals clustered at the firm level.

Figure 11. Dynamic treatment effects of GVC funding on net sales (thousand SEK) estimated using Callaway and Sant'Anna (2021) (Model A4).



Note: The figure shows average treatment effects on the treated ($ATT(g,t)$) relative to the pre-treatment period. Blue bars denote pre-treatment coefficients, while red bars post-treatment effects, with 95 % confidence intervals based on wild bootstrap inference.

Figure 12. Event-study estimates of dynamic treatment effects on net sales (thousand SEK) following Sun and Abraham (2021) (Model A4).



Note: Coefficients are shown relative to the baseline year (t–1), with 95 % confidence intervals. The estimator corrects for potential bias in conventional two-way fixed-effects models with staggered treatment timing.

4.2. Regional effects

This section tests Hypothesis 5, which predicts that the effects of GVC funding vary across regions and are strongest in metropolitan and innovation-dense areas.

Since there are regional restrictions on the allocation of GVC funding by Almi Invest, we test whether the estimated effects differ across regions. On average, about five percent of applicant firms receive GVC financing. This share is somewhat lower in the metropolitan region of Stockholm and in the densely populated counties of Scania, Västra Götaland, and Uppsala, which we group as "Large cities." Applicants from smaller and more peripheral regions in Southern and Northern Sweden face higher approval rates, reflecting relatively greater Almi resources per firm. As a result, regional funds outside metropolitan areas may have to invest in a broader set of projects, including some of lower quality or higher risk, which could weaken average treatment effects. The 400 treated portfolio companies are fairly evenly distributed across these four regions: Stockholm (90), Large cities (133), Southern Sweden (83), and Northern Sweden (94).

Table 4. Regional heterogeneity in the employment and sales effects of GVC funding (OLS, with interaction terms).

Outcome variable	*EMP*			*SALES*		
	Method = DiD OLS with firm-cohort and year-cohort fixed effects					
DiD-effect compared to other three regions	Model A1	Model A2	Model A3	Model A4	Model A5	Model A6
Stockholm	2.15 **	2.23 **	1.91 *	352	1,029	393
	(1.04)	(1.01)	(0.99)	(1,955)	(2,132)	(1,906)
Large cities	1.48	1.09	1.31	790	640	196
	(1.07)	(1.09)	(1.04)	(2,513)	(2,498)	(2,591)
Southern Sweden	–3.64 ***	–3.57 ***	–3.02 ***	–5,769 ***	–4,721 ***	–5,783 ***
	(0.85)	(0.84)	(0.86)	(2,030)	(2,188)	(2,064)
Northern Sweden	–0.02	0.25	–0.43	4,815	3,072	5,757
	(1.44)	(1.43)	(1.28)	(6,154)	(5,523)	(6,266)

Results are from one regression per outcome including interaction terms between the treatment variable and regional dummies (Treatment × Region). Regional dummies use effects (sum-to-zero) coding, so coefficients indicate each region's deviation from the national average treatment effect rather than from a single reference region. Robust standard errors are clustered at the firm level. ***, *** and * denote statistical significance at the 1-, 5- and 10-percent levels.

To examine regional heterogeneity, we estimate a single regression for each outcome variable that includes interaction terms between the DiD treatment indicator and regional dummies. We use effects (sum-to-zero) coding for regions, so the coefficient on *D-post* represents the national average treatment effect, and each interaction term measures that region's deviation from the average of all other regions. This design allows us to test whether the treatment effect in each

region differs from the average treatment effect across all regions within one unified model, rather than estimating separate regressions.

Tables 4 (OLS) and 5 (PPML) present the estimated deviations for employment (*EMP*) and sales (*SALES*).

From the estimates, GVC funding in Stockholm yields significantly stronger employment effects than the national average, both in the OLS (Table 4) and PPML (Table 5) specifications. In the PPML estimation, the "Large Cities" region also shows a positive and statistically significant deviation for employment, indicating that the scaling of firm size effects is particularly pronounced in these dynamic urban markets. However, no statistically significant deviation is observed regarding sales in either Stockholm or the other regions.

In contrast, Southern Sweden exhibits significantly weaker effects on both employment and sales, irrespective of estimator, suggesting less developed co-investment networks or slower post-investment scaling dynamics. For Northern Sweden, the estimated effects are statistically insignificant and display greater variability across models, suggesting that outcomes in this region are more heterogeneous and possibly influenced by firm-level or sectoral differences rather than regional investment capacity.

Table 5. Regional heterogeneity in the employment and sales effects of GVC funding (PPML, with interaction terms).

Outcome variable	*EMP*			*SALES*		
	Method = DiD PPML with firm-cohort and year-cohort fixed effects					
DiD-effect compared to other three regions	Model B1	Model B2	Model B3	Model B4	Model B5	Model B6
Stockholm	0.23 **	0.25 **	0.19 *	0.26	0.25	0.42 **
	(0.11)	(0.12)	(0.11)	(0.20)	(0.19)	(0.20)
Large cities	0.29 **	0.26 **	0.27 **	0.36	0.38	0.33
	(0.12)	(0.12)	(0.14)	(0.25)	(0.25)	(0.22)
Southern Sweden	–0.62 ***	–0.58 ***	–0.46 ***	–1.03 ***	–0.86 ***	–0.99 ***
	(0.13)	(0.13)	(0.14)	(0.21)	(0.18)	(0.18)
Northern Sweden	–0.04	–0.02	–0.10	0.29	0.15	0.16
	(0.12)	(0.12)	(0.12)	(0.32)	(0.26)	(0.25)

Results are from one regression per outcome including interaction terms between the treatment variable and regional dummies (Treatment × Region). Regional dummies use effects (sum-to-zero) coding, so coefficients indicate each region's deviation from the national average treatment effect rather than from a single reference region. Robust standard errors are clustered at the firm level. ***, *** and * denote statistical significance at the 1-, 5- and 10-percent levels.

4.3. Additional external equity financing

This section tests Hypothesis 3, which predicts that GVC funding increases firms' ability to attract additional external private equity financing.

Matching quality

A separate matching procedure is conducted for this outcome, based on firm characteristics relevant to equity financing. The matching diagnostics presented in Table A3 indicate excellent covariate balance for the preferred models (C1 and D1), with a mean standardized bias below 5 %, insignificant t-tests for all covariates, and a pseudo-R^2 reduction from 0.19 to 0.03. This confirms that treated and control firms are statistically comparable before treatment.

Table 6. Baseline DiD-estimates of GVC effects on equity capital (thousand SEK), OLS and PPML.

Outcome variable	*EQUITY*					
Explanatory variables	Method = DiD with firm-cohort and year-cohort fixed effects					
	OLS			PPML		
	Model C1	Model C2	Model C3	Model D1	Model D2	Model D3
D-post	8,433 *** (3,205)	8,388 *** (2,458)	7,761 *** (851)	0.62 *** (0.18)	0.54 *** (0.12)	0.42 *** (0.09)
No. of clusters	809	809	816	809	809	816
No. of observations	6,746	6,714	6,590	6,746	6,714	6,590
F-value	3.85 ***	2.96 ***	4.77 ***	---	---	---
Adjusted R^2	0.57	0.84	0.66	---	---	---
Pseudo R^2	---	---	---	0.91	0.87	0.86
DiD-effect	---	---	---	85.9 %	71.6 %	52.2 %
Statistics from event-study graph						
F-test pre-trend-equality (p-value)	0.28 (0.84)	1.45 (0.23)	2.20 * (0.09)	1.74 (0.63)	2.24 (0.52)	3.88 (0.28)
Statistics from robustness estimation Callaway and Sant'Anna (2021)						
CS pre-average (95 % conf. interval)	1,801 (–508 – 4,110)	1,080 ** (30 – 2,130) [a]	863 (–28 – 1,756)	---	---	---
CS post-average (95 % conf. interval)	12,775 ** (5,376 – 20,173	10,169 ** (5,073 – 15,265)	9,806 ** (3,375 – 16,238)	---	---	---
Statistics from robustness estimation Sun and Abraham (2021)						
Wald-$\chi^2(3)$ pre-trend test (p-value)	0.30 (0.96)	5.99 (0.11)	6.19 (0.11)	---	---	---

All regressions include firm- and year-cohort fixed effects (Gormley and Matsa 2011), as well as region fixed effects to capture persistent regional differences in economic conditions. This specification controls for unobserved firm heterogeneity, cohort-specific shocks, and spatial variation in treatment intensity. Standard errors are clustered at the firm level. Coefficients represent average treatment effects on the treated (ATT) relative to the pre-treatment year (t = –1). The models are based on different matchings (C1/D1, C2/D2, C3/D3). Robust standard errors are clustered at the firm level. ***, *** and * denote statistical significance at the 1-, 5- and 10-percent levels.

[a)] Although the joint pre-trend test is significant different from zero, all individual estimates from t –4 to t – 1 are insignificant.

Baseline results

As in previous sections (4.1 and 4.2), where we examined the effects on employment and net sales, we apply the DiD method with matching to estimate causal effects. Our primary variable of interest is the company's share capital (*EQUITY*), excluding the government's capital injection, the required 50 % private investor co-financing, and any increases due to retained earnings. This ensures that our measure of equity capital reflects only changes resulting from additional external equity financing sources. A significant increase in share capital after receiving GVC funding would indicate that public venture capital is not crowding out private investment but instead fostering crowding-in effects, encouraging additional private-sector participation.

The results of the baseline OLS and PPML estimations are shown in Table 6. In all specifications, GVC funding has a strongly positive and statistically significant effect on total share capital, regardless of the estimation method. The estimated effect ranges between 5 and 12 million SEK (approximately 0.5–1 million EUR).

The estimated coefficients reflect net crowding-in effects rather than mechanical correlations, because the dependent variable includes only private follow-on equity raised after the GVC round and excludes both the public contribution and the required co-investment capital.

Event-study diagnostics

The event-study estimations also support the baseline results. In most cases, the parallel-trends assumption is not rejected during the pre-policy period (see Figures 13 and 14 for Models C1 and D1). However, this is not always the case.

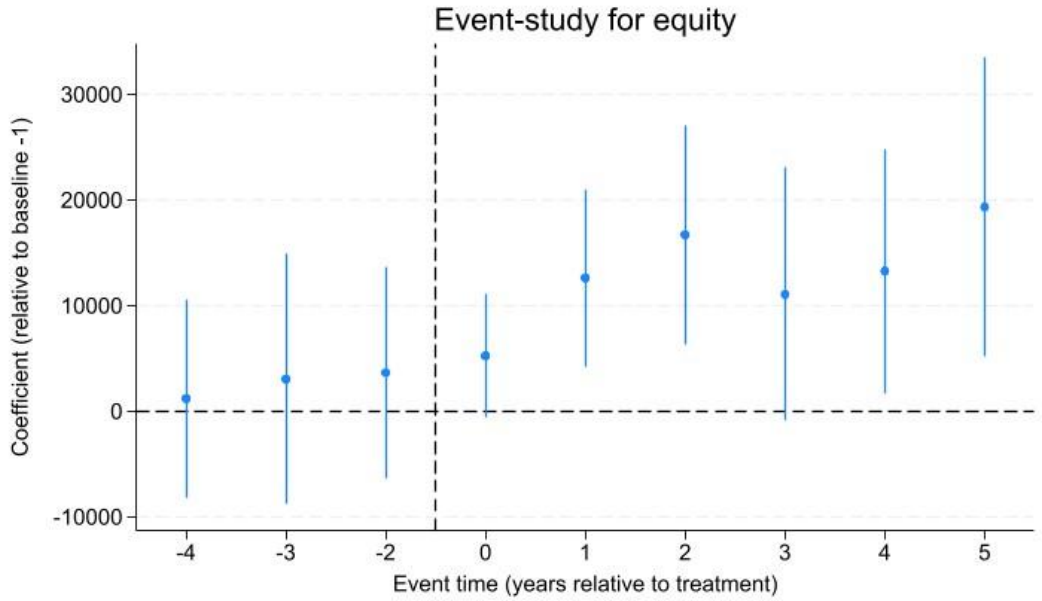
Importantly, when using the Callaway and Sant'Anna (2021) estimator, the pre-treatment coefficients are almost always statistically indistinguishable from zero, indicating much clearer parallel trends than in the Gormley and Matsa (2011) models. This suggests that the Callaway–Sant'Anna approach better captures the dynamic treatment effects for equity, especially given that it allows for heterogeneous responses across treatment cohorts while relying on the fixed control group of never-treated firms. Both the post-treatment coefficients and the post-average ATT estimates are positive and statistically significant, reinforcing the conclusion that GVC financing enhances firms' ability to attract additional private equity.

The Sun and Abraham (2021) event-study estimations in Figure 16 provide further robustness. The Wald–$\chi^2(3)$ pre-trend tests reported in Table 6 show no systematic differences between

treated and control firms prior to receiving GVC financing. The post-treatment effects remain strongly positive, confirming that the increase in equity financing is not driven by pre-existing trends.

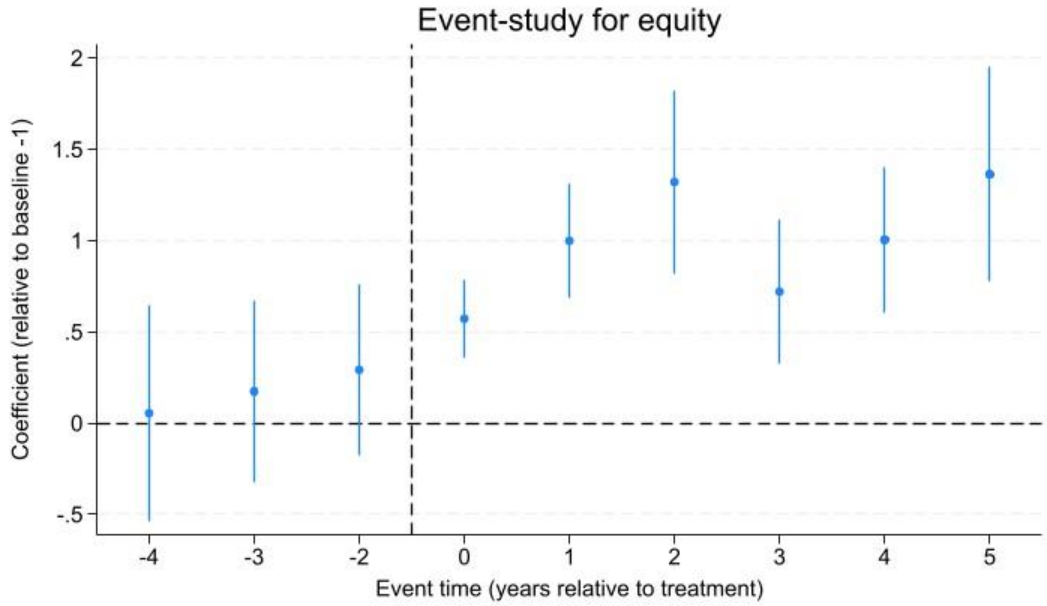
Taken together, the results across the four estimators—Gormley and Matsa (OLS and PPML), Callaway and Sant'Anna, and Sun and Abraham—consistently demonstrate that GVC funding has a strong and persistent crowding-in effect on private equity financing. The superior pre-trend performance of the Callaway and Sant'Anna estimator further strengthens the credibility of these findings.

Figure 13. Annual DiD-estimates of the effects of GVC funding on equity capital (thousand SEK) (OLS, Model C1).



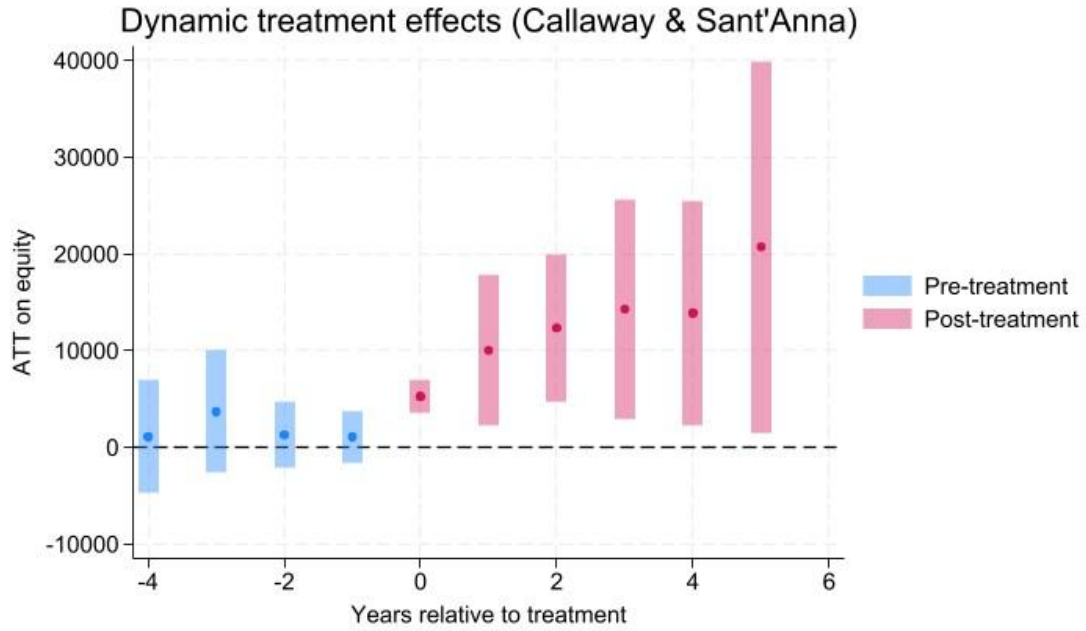
Note: The figure plots estimated coefficients for each year relative to the treatment year (t=0; baseline = t–1). Vertical bars indicate 95 % confidence intervals clustered at the firm level.

Figure 14. Annual DiD-estimates of the effects of GVC funding on equity capital (thousand SEK) (PPML, Model D1).



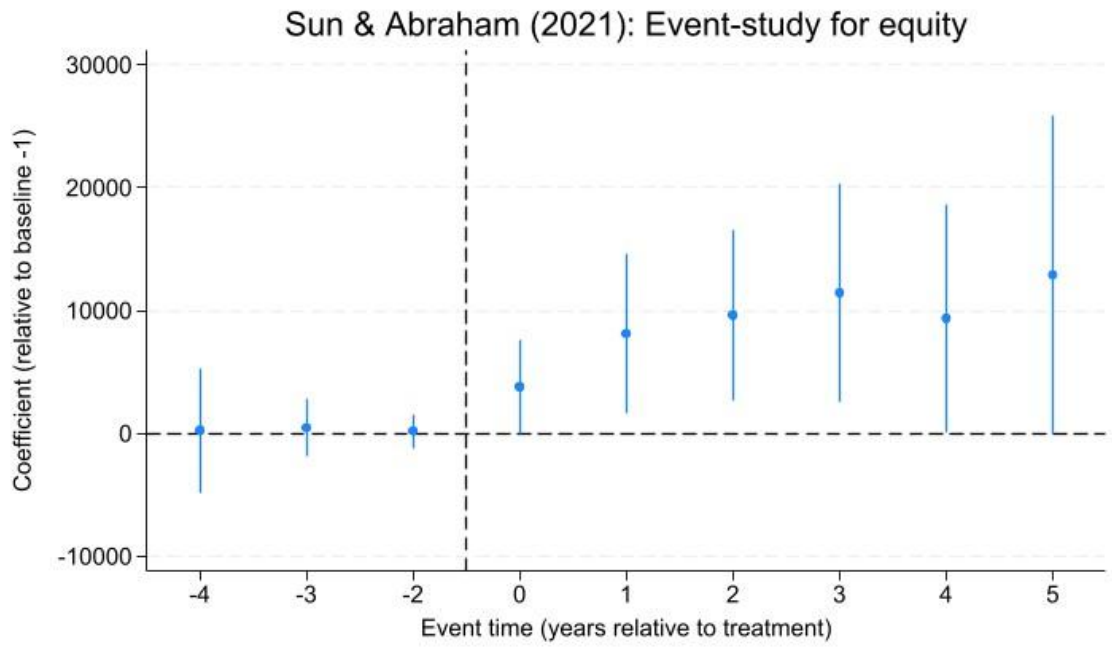
Note: The figure plots estimated coefficients for each year relative to the treatment year (t=0; baseline = t–1). Vertical bars indicate 95 % confidence intervals clustered at the firm level.

Figure 15. Dynamic treatment effects of GVC funding on equity capital (thousand SEK) estimated using Callaway and Sant'Anna (2021) (Model C1).



Note: The figure shows average treatment effects on the treated ($ATT(g,t)$) relative to the pre-treatment period. Blue bars denote pre-treatment coefficients, while red bars post-treatment effects, with 95 % confidence intervals based on wild bootstrap inference.

Figure 16. Event-study estimates of dynamic treatment effects on total equity (thousand SEK) following Sun and Abraham (2021) (Model C1).



Note: Coefficients are shown relative to the baseline year (t–1), with 95 % confidence intervals. The estimator corrects for potential bias in conventional two-way fixed-effects models with staggered treatment timing.

4.4. The impact of GVC on firms' access to loans

This section tests Hypothesis 4, which predicts that GVC funding increases firms' ability to attract additional external debt financing.

Table 7. Baseline DiD-estimates of GVC effects on total debt (thousand SEK), OLS and PPML.

Outcome variable	*DEBT*					
Explanatory variables	Method = DiD with firm-cohort and year-cohort fixed effects					
	OLS			PPML		
	Model E1	Model E2	Model E3	Model F1	Model F2	Model F3
D-post	4,147 *** (1,520)	3,741 *** (1,040)	3,125 *** (931)	0.61 *** (0.19)	0.57 *** (0.14)	0.45 *** (0.15)
No. of clusters	805	809	813	805	809	813
No. of observations	6,661	6,662	6,749	6,661	6,662	6,749
F-value	2.67 ***	6.33 ***	3.56 ***	---	---	---
Adjusted R^2	0.76	0.60	0.64	---	---	---
Wald chi-square	---	---	---	526 ***	425 ***	349 ***
Pseudo R^2	---	---	---	0.91	0.88	0.88
DiD-effect	---	---	---	84.0 %	76.8 %	56.8 %
Statistics from event-study graph						
F-test pre-trend-equality (p-value)	1.37 (0.25)	1.73 (0.16)	0.81 (0.49)	2.52 (0.47)	0.60 (0.90)	3.24 (0.36)
Statistics from robustness estimation Callaway and Sant'Anna (2021)						
CS pre-average (95 % conf. interval)	256 (–190 – 702)	365 (–20 – 751)	321 (–53 – 694)	---	---	---
CS post-average (95 % conf. interval)	2,750 ** (568 – 4,932	2,282 ** (633 – 3,929)	2,252 ** (257 – 4,248)	---	---	---
Statistics from robustness estimation Sun and Abraham (2021)						
Wald-$\chi^2(3)$ pre-trend test (p-value)	3.30 (0.35)	6.63 * (0.09)	5.35 (0.15)	---	---	---

All regressions include firm- and year-cohort fixed effects (Gormley and Matsa 2011), as well as region fixed effects to capture persistent regional differences in economic conditions. This specification controls for unobserved firm heterogeneity, cohort-specific shocks, and spatial variation in treatment intensity. Standard errors are clustered at the firm level. Coefficients represent average treatment effects on the treated (ATT) relative to the pre-treatment year (t = –1). The models are based on different matchings (E1/F1, E2/F2, E3/F3). Robust standard errors are clustered at the firm level. ***, *** and * denote statistical significance at the 1-, 5- and 10-percent levels.

Baseline results

The dependent variable in this section is total debt (*DEBT*). The analysis is based on three separate matched samples derived from distinct propensity-score matchings. Each sample corresponds to a different matching specification of the debt model: Models E1 and F1 (first matching), Models E2 and F2 (second matching), and Models E3 and F3 (third matching). The first matching—used for Models E1 and F1—is reported in Appendix Table A4. Propensity-

score matching ensures comparability between treated and control firms with respect to the KZ-index, pre-treatment debt levels, and other financial covariates. The preferred matching achieved excellent balance (mean standardized bias = 3.2 %, pseudo-R^2 = 0.03, LR χ^2 p = 0.98).

Table 7 reports the baseline Difference-in-Differences (DiD) estimates using both OLS and PPML specifications with firm–cohort and year–cohort fixed effects following Gormley and Matsa (2014). This structure controls for unobserved heterogeneity within each cohort of firms funded in the same year, as well as for time-specific shocks affecting comparable cohorts. The PPML estimator, which naturally accommodates heteroskedasticity and zero values, serves as a robustness check.

Across the OLS models (E1–E3), GVC-funded firms increased their total debt by roughly 3.1 – 4.1 million SEK relative to matched controls. The PPML estimates (F1–F3) confirm these results in percentage terms, indicating that GVC backing raises firms' debt by 57 – 84 %. The F-test of pre-treatment coefficients (reported at the bottom of Table 7) cannot reject the null hypothesis of equal pre-treatment trends ($p > 0.10$), supporting the validity of the parallel-trends assumption.

Event-study diagnostics

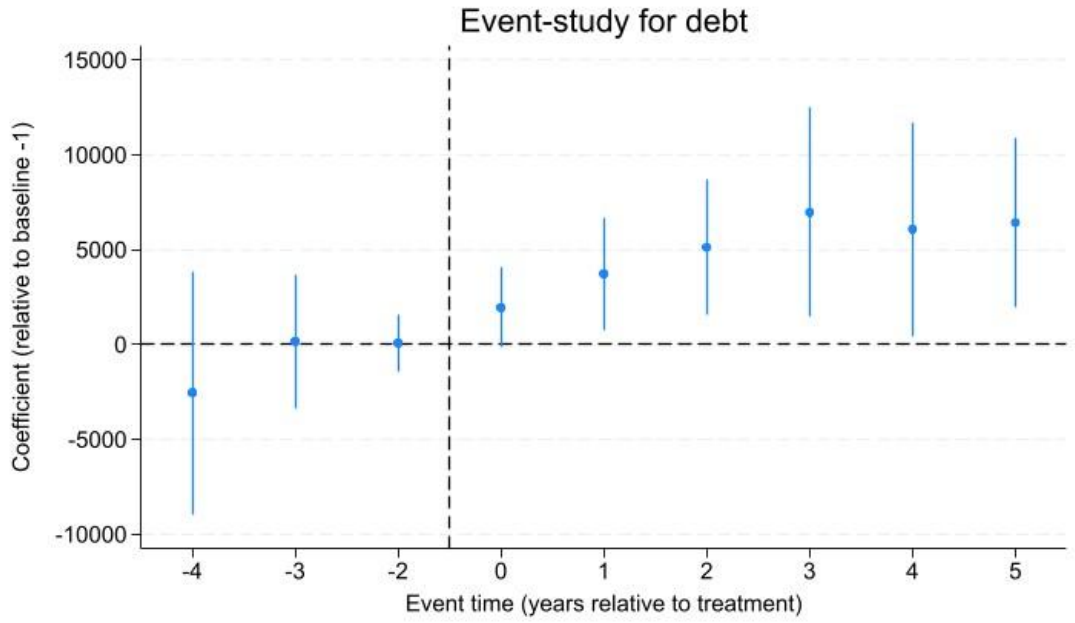
Figures 17–19 display the annual dynamic effects estimated through OLS, PPML, and the Callaway and Sant'Anna (2021) approach. All four methods yield consistent results: the coefficients for pre-treatment years are close to zero, while a sharp and sustained increase in debt occurs from the treatment year onward. The magnitude and persistence of the post-treatment effects indicate that GVC support significantly enhances firms' borrowing capacity and that this improvement persists for at least five years after the investment.

As an additional robustness exercise, we also estimate the model following Sun and Abraham (2021), which corrects for potential bias in conventional event-study designs with staggered treatment timing. The results, presented in Figure 20 and summarized in the last row of Table 7, show that the pre-treatment coefficients are statistically indistinguishable from zero. The Wald-$\chi^2(3)$ tests of equality across pre-treatment coefficients yield p-values above 0.05 in all specifications, confirming that there are no systematic differences between treated and control firms prior to receiving GVC funding. The post-treatment coefficients display a clear and sustained increase, fully consistent with the dynamic patterns observed in the OLS, PPML, and Callaway–Sant'Anna estimations. This additional test therefore strengthens the conclusion that the estimated debt effects reflect genuine causal impacts rather than pre-existing trends.

Consistent with the equity results in Section 4.3, these debt effects suggest that strengthened balance sheets reduce lenders' perceived risk.

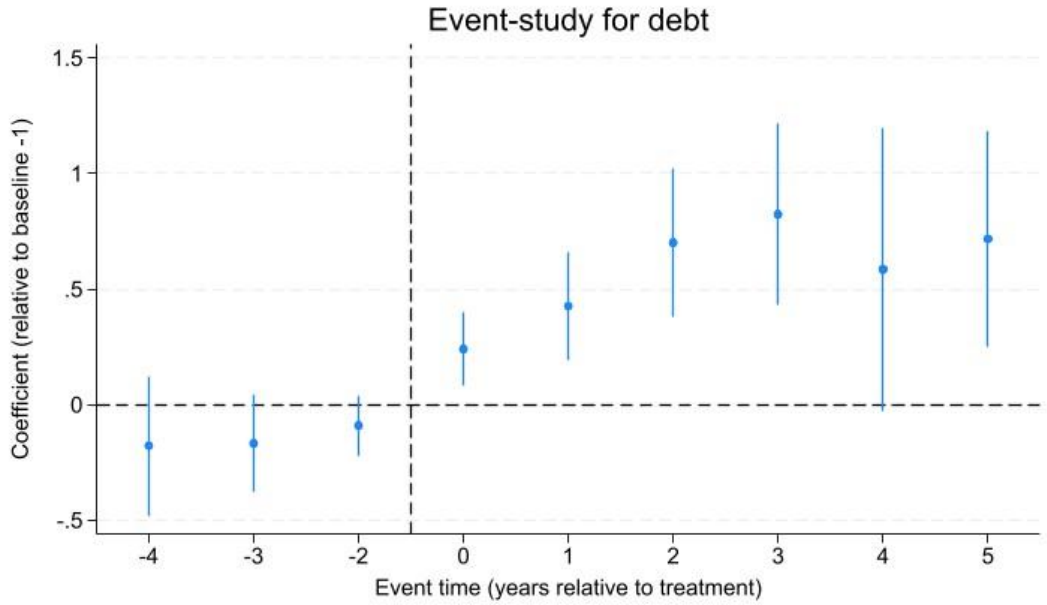
Overall, the debt results reinforce the interpretation that GVC alleviates financial constraints. By strengthening equity positions and signaling firm quality, GVC enables recipient firms to leverage additional credit for expansion. The consistency of results across estimators—OLS, PPML, Callaway–Sant'Anna, and Sun–Abraham—demonstrates the robustness of these findings and highlights the complementarity relationship between public venture capital and private debt markets.

Figure 17. Annual DiD-estimates of the effects of GVC funding on total debt (thousand SEK) (OLS, Model E1).



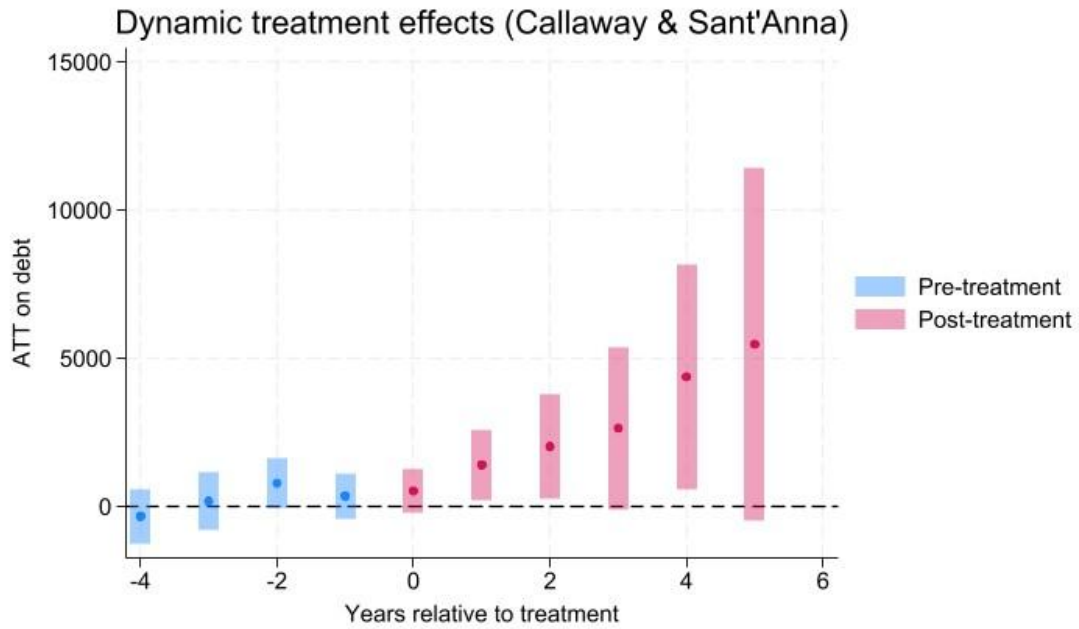
Note: The figure plots estimated coefficients for each year relative to the treatment year (t=0; baseline = t–1). Vertical bars indicate 95 % confidence intervals clustered at the firm level.

Figure 18. Annual DiD-estimates of the effects of GVC funding on total debt (thousand SEK) (PPML, Model F1).



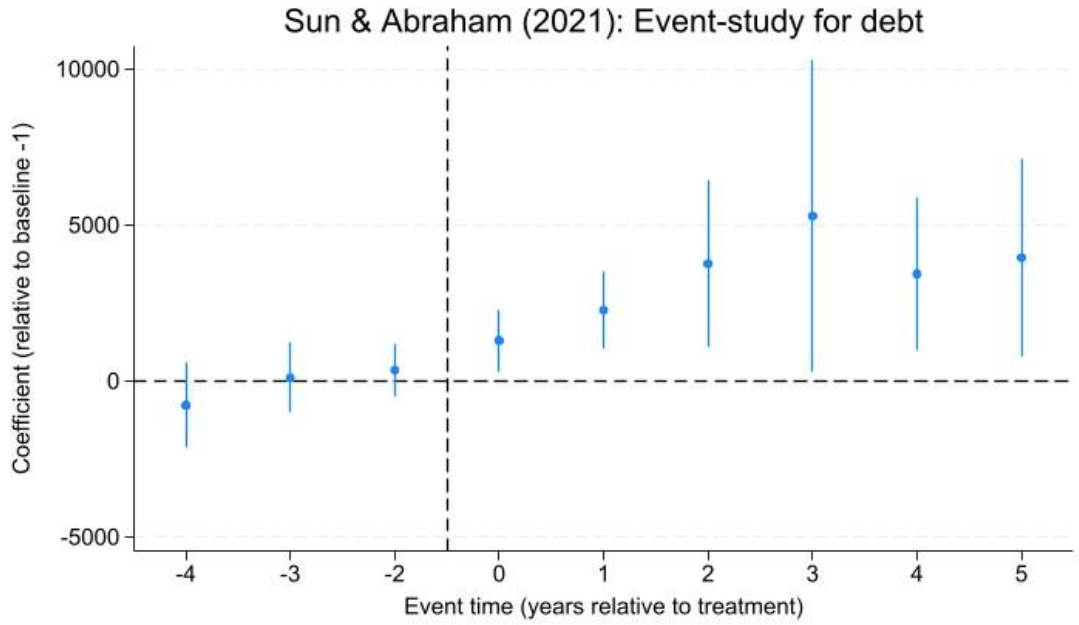
Note: The figure plots estimated coefficients for each year relative to the treatment year (t=0; baseline = t–1). Vertical bars indicate 95 % confidence intervals clustered at the firm level.

Figure 19. Dynamic treatment effects of GVC funding on total debt (thousand SEK) estimated using Callaway and Sant'Anna (2021) (Model E1).



Note: The figure shows average treatment effects on the treated ($ATT_{(g,t)}$) relative to the pre-treatment period. Blue bars denote pre-treatment coefficients, while red bars post-treatment effects, with 95 % confidence intervals based on wild bootstrap inference.

Figure 20. Event-study estimates of dynamic treatment effects on total debt (thousand SEK) following Sun and Abraham (2021) (Model E1).



Note: Coefficients are shown relative to the baseline year (t–1), with 95 % confidence intervals. The estimator corrects for potential bias in conventional two-way fixed-effects models with staggered treatment timing.

5. Conclusions

This study examines the effects of government venture capital (GVC) funding on the performance and financing of young firms, drawing on data from Sweden's Almi Invest between 2008 and 2020. Overall, our results demonstrate that GVC can alleviate financial constraints and stimulate firm growth when properly embedded in a supportive institutional and regional context.

Our findings are threefold. First, GVC funding has a positive impact on firm growth, as evidenced by significant increases in employment and sales among treated firms relative to matched controls. Second, the benefits of GVC are unevenly distributed across regions. Firms located in metropolitan areas with well-developed innovation ecosystems reap substantially greater returns from GVC support, whereas firms in rural or peripheral regions— despite receiving a disproportionate share of funding—show more modest growth effects. Third, GVC eases financing constraints by enhancing access to both equity and debt. Firms that received GVC funding were more successful in attracting private co-investment and experienced increased borrowing from banks, suggesting that GVC improves creditworthiness by reducing information asymmetries and strengthening balance sheets.

These results contribute new insights to the entrepreneurial finance literature. First, we extend prior work on GVC's "certification effect" by showing that it not only facilitates follow-on equity investments but also enables access to bank loans. This finding supports the theoretical view that GVC serves as a reputational signal and reduces credit-market frictions, enhancing firms' overall financial position. Second, we document how regional context shapes the efficacy of public venture capital. In line with previous research, our findings suggest that GVC performs best when deployed in environments with supportive infrastructure—such as networks, talent, and follow-on capital. Conversely, when funding is allocated primarily to less-developed areas, political goals may override efficiency, limiting the growth potential of GVC-backed firms. Together, these results underscore the importance of considering institutional design and regional ecosystem conditions in evaluating the effectiveness of public venture capital.

From a policy perspective, the findings highlight that GVC can be a powerful tool to bridge financing gaps, provided the program design is well-calibrated. Allowing greater concentration of GVC in high-potential regions could increase overall economic returns, while complementary instruments—such as incubators, investor-matching initiatives, or credit guarantees—may be required to enhance effectiveness in thinner markets. Our results also support public–private collaboration: hybrid investment models are likely to enhance firm outcomes by leveraging the discipline and value-added of private partners. Moreover, the fact that GVC facilitates debt financing suggests that coordination between GVC funds and loan or guarantee programs could further reduce financing constraints, particularly for firms seeking to scale beyond the start-up phase.

While our results are robust, several limitations remain. First, the findings are based on one national program, and their generalizability may depend on institutional and market contexts. Second, our analysis focuses on growth and financing outcomes up to a few years post-investment; longer-term impacts on innovation, survival, or profitability merit further study. Third, despite using matching and fixed-effects DiD, unobserved confounders cannot be ruled out. Future research could build on this study by exploring heterogeneous treatment effects across sectors or investor types, or by employing quasi-experimental and experimental designs to reinforce causal inference.

In conclusion, GVC serves not only as a direct source of growth capital but also as a signal and enabler of broader financial access. Its effectiveness, however, hinges on strategic allocation, ecosystem fit, and integration with private actors and complementary policies. By addressing

both the equity and debt constraints of early-stage firms, well-designed GVC programs can play a vital role in fostering sustainable entrepreneurship, innovation, and balanced regional development. Ultimately, this suggests that government venture capital can play a catalytic role in building sustainable innovation ecosystems.

Data availability statement

The firm-level data used in our study originates from Almi and Dun & Bradstreet (Serrano database). Due to data confidentiality agreements, we cannot share the raw data with third parties. However, we are prepared to provide the journal data repository with the code and programs used to generate our results, along with a detailed data description document.

Disclosure of interest

The authors declare that they have no known competing financial interests or personal relationships that could have influenced the research reported in this paper.

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Appendix A.

Matching Diagnostics

This appendix presents illustrative examples of the propensity-score matching (PSM) results used to construct the control groups. Only representative examples are shown; full diagnostics for all twelve matchings are available upon request

Tables A1–A4 display representative matchings—one for each outcome variable (employment, sales, equity, and debt)—out of the twelve total matchings performed in the analysis. Each table reports mean values of the matching variables for treated and control firms, standardized mean differences, t-tests of equality, and model-fit statistics (pseudo-R^2 and χ^2).

Across all examples, the differences in means are statistically insignificant, confirming that the matching procedures achieve satisfactory covariate balance between treated and control firms.

Table A1. Results of matching for Sales (Models A1 and B1, Tables 2 and 3).

Variable	Method = Propensity score matching based on logistic regression					
	Mean treated	Mean control (unmatched)	Mean control (matched)	% bias (matched)	Diff. treated – control	t-stat (p-value)
KZ-index (t–1, t –2 average)	2.42	0.27	2.24	6.3	0.18	0.89 (0.37)
EMP(t–2), (‘000 SEK)	3.69	6.21	4.51	–1.3	–0.82	–0.81 (0.42)
ASSET(t–1), (‘000 SEK)	9,103	71,623	8,924	0.0	179	0.07 (0.95)
SOLID(t–1), share	0.43	0.52	0.41	1.8	0.02	0.70 (0.48)
Age (years)	5.75	14.07	5.67	0.9	0.24	0.11 (0.81)
Overall matching quality statistics			Before matching		After matching	
Pseudo R^2			0.195		0.042	
LR χ^2 (p-value)			1471.3 (0.00)		42.3 (0.99)	
Mean standardized bias (%)			----		4.2	
Median standardized bias (%)			----		3.6	
B Share of covariates with standardized bias >25 %			----		48.1	
R (variance ratio median)			----		0.63	
% variance ratios within [0.5, 2]			----		80	

***, *** and * indicate statistical significance for the t-test on 1-., 5- and 10-percent-level. Industry, region and year dummies are included, but not shown. “B” denotes the percentage of covariates with absolute standardized bias greater than 25%. “R” is the median ratio of treated-to-control variances. Values of $B < 50\ \%$ and $R \in [0.5, 2]$ indicate acceptable balance.

Table A2. Results of matching for Sales (Models A4 and B4, Tables 2 and 3).

Variable	Method = Propensity score matching based on logistic regression					
	Mean treated	Mean control (unmatched)	Mean control (matched)	% bias (matched)	Diff. treated – control	t-stat (p-value)
KZ-index (t–1, t –2 average)	2.41	0.27	2.21	7.0	0.20	1.18 (0.24)
SALES(t–2), (‘000 SEK)	3,715	22,069	3,720	–0.0	–5	–0.01 (0.99)
ASSET(t–1), (‘000 SEK)	9,158	70,586	6,958	2.3	2,200	0.91 (0.36)
SOLID(t–1), share	0.43	0.52	0.41	0.1	0.02	0.85 (0.40)
Age (years)	5.78	14.10	5.74	0.9	0.04	0.11 (0.91)
Overall matching quality statistics			Before matching		After matching	
Pseudo R^2			0.196		0.039	
LR χ^2 (p-value)			1468.2 (0.00)		39.0 (0.99)	
Mean standardized bias (%)			----		4.0	
Median standardized bias (%)			----		3.7	
B Share of covariates with standardized bias >25 %			----		47.0	
R (variance ratio median)			----		0.99	
% variance ratios within [0.5, 2]			----		100	

***, *** and * indicate statistical significance for the t-test on 1-., 5- and 10-percent-level. Industry, region and year dummies are included, but not shown. “B” denotes the percentage of covariates with absolute standardized bias greater than 25%. “R” is the median ratio of treated-to-control variances. Values of $B < 50\ \%$ and $R \in [0.5, 2]$ indicate acceptable balance.

Table A3. Results of matching for Debt (Models C1 and D1, Table 6).

Variable	Method = Propensity score matching based on logistic regression					
	Mean treated	Mean control (unmatched)	Mean control (matched)	% bias (matched)	Diff. treated – control	t-stat (p-value)
KZ-index (t–1, t –2 average)	2.47	0.65	2.19	8.8	0.28	1.21 (0.23)
EQUITY(t–2), (‘000 SEK)	3,250	20,599	9,573	–1.1	–6,323	–1.13 (0.26)
EMP(t–1), number	4.5	6.1	7.4	–4.1	–2,9	–0.93 (0.35)
Age (years)	5.72	14.00	5.88	–2.0	–0.16	–0.51 (0.61)
Overall matching quality statistics			Before matching		After matching	
Pseudo R^2			0.187		0.034	
LR χ^2 (p-value)			1575.4 (0.00)		38.2 (1.00)	
Mean standardized bias (%)			----		3.7	
Median standardized bias (%)			----		3.5	
B Share of covariates with standardized bias >25 %			----		41.3	
R (variance ratio median)			----		0.76	
% variance ratios within [0.5, 2]			----		100	

***, *** and * indicate statistical significance for the t-test on 1-., 5- and 10-percent-level. Industry, region and year dummies are included, but not shown. “B” denotes the percentage of covariates with absolute standardized bias greater than 25%. “R” is the median ratio of treated-to-control variances. Values of $B < 50\ \%$ and $R \in [0.5, 2]$ indicate acceptable balance.

Table A4. Results of matching for Debt (Models E1 and F1, Table 7).

Variable	Method = Propensity score matching based on logistic regression					
	Mean treated	Mean control (unmatched)	Mean control (matched)	% bias (matched)	Diff. treated – control	t-stat (p-value)
KZ-index (t–1, t –2 average)	2.47	0.48	2.69	1.9	–0.22	–0.92 (0.36)
DEBT(t–1), (‘000 SEK)	4,865	40,167	7,451	–0.1	–2,586	–0.86 (0.39)
DEBT(t–2), (‘000 SEK)	4,286	38,920	6,808	–0.1	–2,522	–0.94 (0.35)
Age (years)	5.73	14.01	5.63	1.2	0.10	0.29 (0.77)

Overall matching quality statistics	Before matching	After matching
Pseudo R^2	0.194	0.029
LR χ^2 (p-value)	1628.4 (0.00)	31.8 (1.00)
Mean standardized bias (%)	----	3.2
Median standardized bias (%)	----	2.6
B Share of covariates with standardized bias >25 %	----	39.7
R (variance ratio median)	----	1.28
% variance ratios within [0.5, 2]	----	92

***, *** and * indicate statistical significance for the t-test on 1-., 5- and 10-percent-level. Industry, region and year dummies are included, but not shown. “B” denotes the percentage of covariates with absolute standardized bias greater than 25%. “R” is the median ratio of treated-to-control variances. Values of $B < 50\ \%$ and $R \in [0.5, 2]$ indicate acceptable balance.

Table A5. Variables included in the 12 propensity-score-matchings.

Matching variables	Matchings											
	A1/ B1	A2/ B2	A3/ B3	A4/ B4	A5/ B5	A6/ B6	C1/ D1	C2/ D2	C3/ D3	E1/ F1	E2/ F2	E3/ F3
KZ index	x	x	x	x	x	x	x	x	x	x	x	x
EMP(t–1)		x	x		x	x	x	x			x	
EMP(t–2)	x		x									
SALES(t–1)				x		x			x			x
SALES(t–2)					x	x						
EQUITY(t–1)								x	x			
EQUITY(t–2)							x	x				
DEBT(t–1)										x		
DEBT(t–2)										x	x	x
ASSET(t–1)	x			x	x	x		x	x			x
SOL(t–1)	x			x		x						
Age	x	x	x	x	x	x	x	x	x	x	x	x
Industry dummies	x	x	x	x	x	x	x	x	x	x	x	x
Year dummies	x	x	x	x	x	x	x	x	x	x	x	x
Region dummies	x	x	x	x	x	x	x	x	x	x	x	x

Appendix B

Descriptive outcome trajectories by firm age

Figures B1 and B2 illustrate the development of employment and net sales in treated firms by age cohort at the time of first GVC financing.

Figure B1 presents the number of employees by firm-age group, using a logarithmic scale to allow direct comparison of proportional growth. The trends are broadly similar across age groups, though the youngest firms (aged 0–1 years) display the steepest initial increase during the two years following investment. Only firms aged ≥ 2 years at first financing (solid red line) are included in the DiD estimations.

Figure B2 shows a corresponding comparison for net sales by firm-age group. While younger firms again demonstrate faster initial growth, the trajectories of all cohorts converge from t + 1 onward.

Overall, the descriptive evidence confirms that excluding firms younger than two years removes early-stage volatility but does not bias the overall growth dynamics of the treated sample.

Figure B1. Development of the number of employees in firms financed with equity capital, depending on firm's age, logarithmic scale.

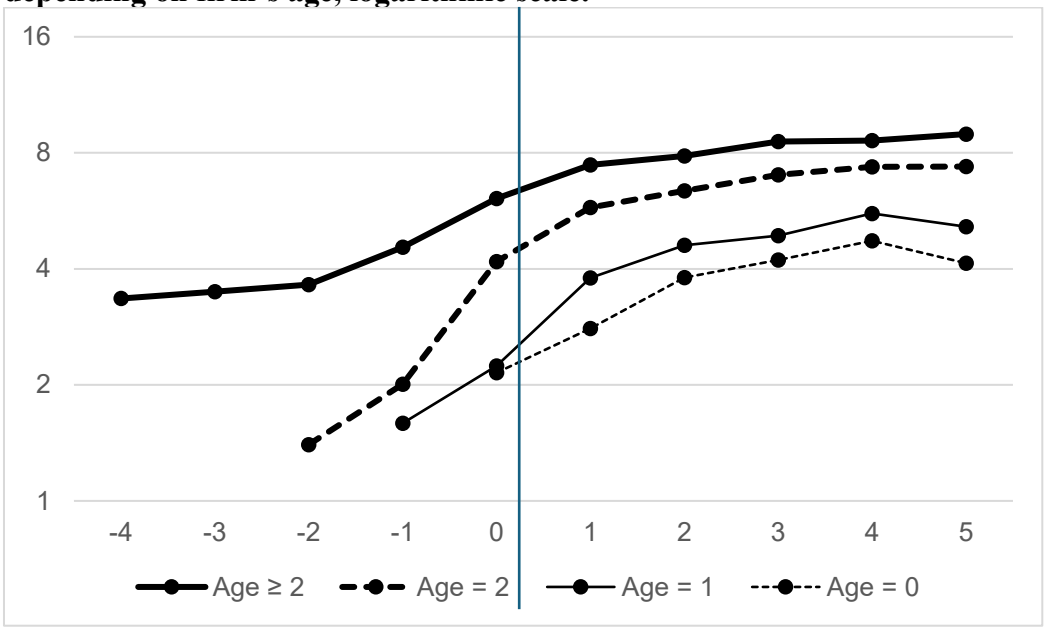
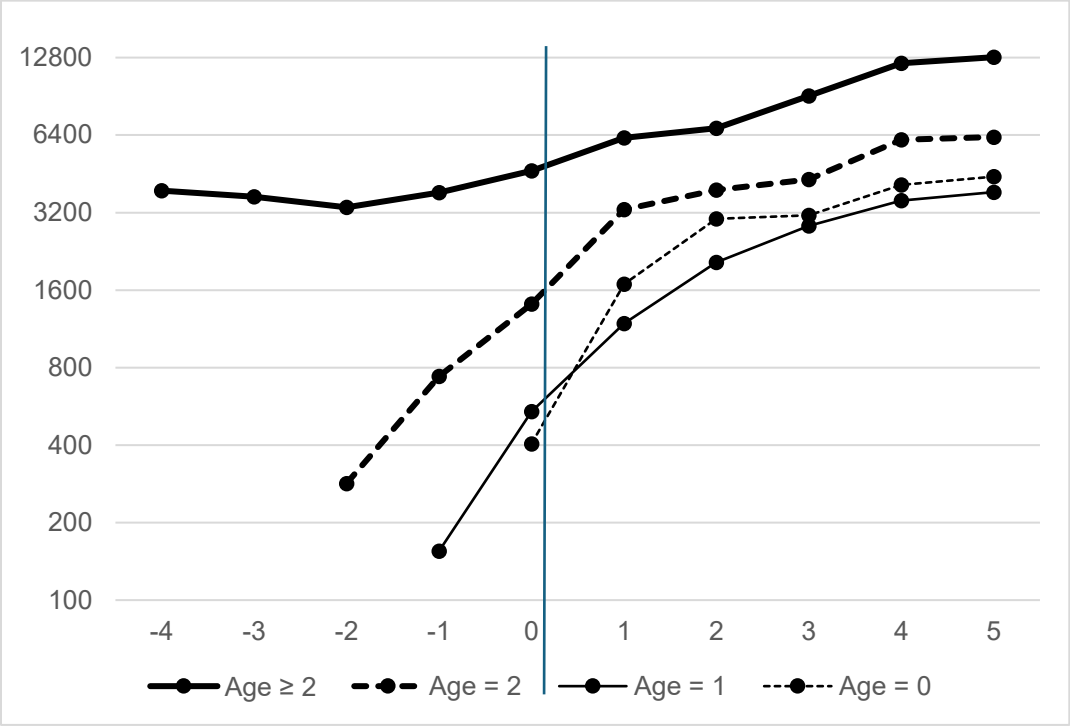


Figure B2. Development of net sales in firms financed with equity capital, depending on firm's age, logarithmic scale.



Appendix C

Figure C1. Annual effects of GVC-funding on the number of employees, OLS.

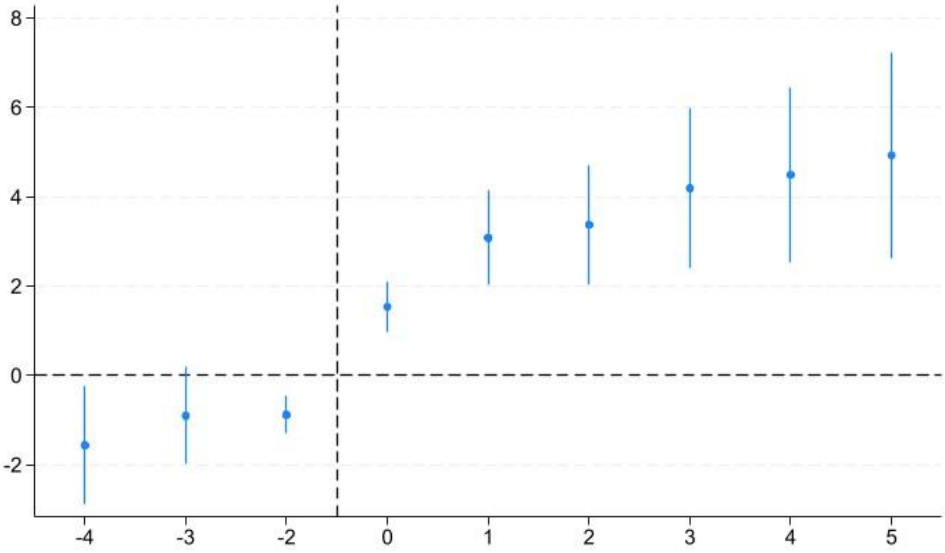
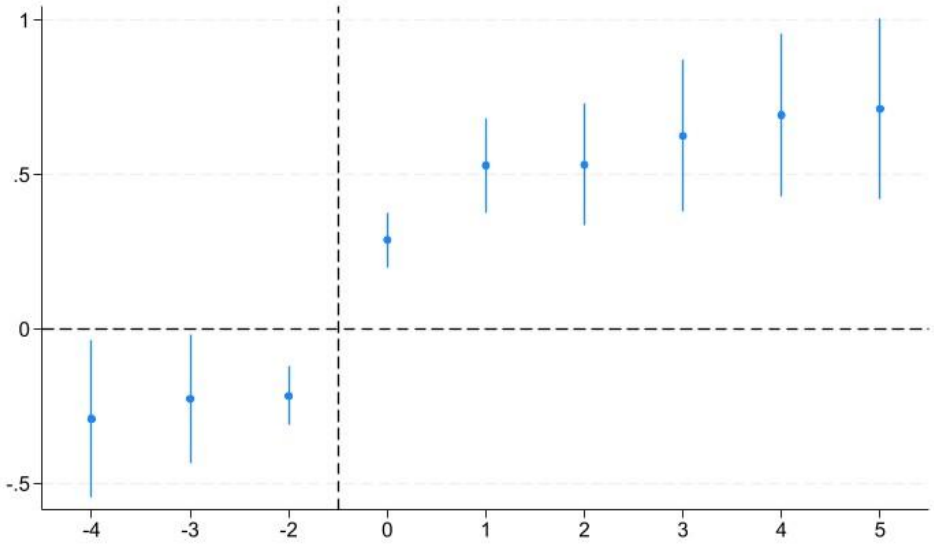



Figure C2. Annual effects of GVC-funding on the number of employees, PPML.