SVERIGES RIKSBANK
WORKING PAPER SERIES

435

Supply-Chain Finance: An Empirical Evaluation of Supplier Outcomes

Niklas Amberg, Tor Jacobson and Yingjie Qi

May 2024

WORKING PAPERS ARE OBTAINABLE FROM

www.riksbank.se/en/research
Sveriges Riksbank • SE-103 37 Stockholm
Fax international: +46 8 21 05 31
Telephone international: +46 8 787 00 00

Supply-Chain Finance: An Empirical Evaluation of Supplier Outcomes*

Niklas Amberg† Tor Jacobson‡ Yingjie Qi§

Sveriges Riksbank Working Paper Series
No. 435, May 2024

Abstract

Buyers and suppliers have diverging interests about trade-credit maturities: buyers desire long payment periods as a source of cheap funding, while suppliers prefer swift payments to avoid locking up scarce liquidity in idle assets. A fast-growing financial product innovation—supply-chain finance (SCF)—offers to resolve these diverging interests, but its net effect on suppliers is a priori unclear. We study the effects of SCF programs on suppliers using unique invoice-level data from a large Swedish bank. We find that SCF programs relax suppliers' liquidity constraints and thereby enable them to grow their sales, employment, and investments.

Keywords: Trade credit; supply-chain finance; reverse factoring; financial constraints.

JEL: G21, G32, D22.

---

*We thank Thorsten Beck, Thomas Flanagan (discussant), Jonathan Hsu (discussant), Vasso Ioannidou, Elena Loutskina, Adriano Rampini, Karin Thorburn, and Fasheng Xu for valuable comments and suggestions. We are also grateful to several representatives of the data-providing bank for their patience in helping us understand the general features of the supply-chain finance market, as well as the specific characteristics of the bank's own SCF programs. The opinions expressed in this article are the sole responsibility of the authors and should not be interpreted as reflecting the views of Sveriges Riksbank.

†Research Division, Sveriges Riksbank. E-mail: niklas.amberg@riksbank.se.

‡Research Division, Sveriges Riksbank. E-mail: tor.jacobson@riksbank.se.

§Copenhagen Business School. E-mail: yingjiee.qi@gmail.com.

1 Introduction

A common clash of interest in business-to-business transactions is the maturity of trade credit, i.e., the length of the period between the supplier's delivery of goods and the buyer's payment: buyers tend to want long maturities and suppliers short maturities, because whereas a buyer's trade payable is an interest-free loan that strengthens its working-capital position, the corresponding trade receivable of the supplier is an idle asset locking up scarce liquidity. These diverging interests are particularly problematic when the bargaining positions of the supplier and the buyer are unequal: Murfin and Njoroge (2015) show, for example, that when large retailers impose longer payment periods on their smaller suppliers, the latter are forced to curtail investment.

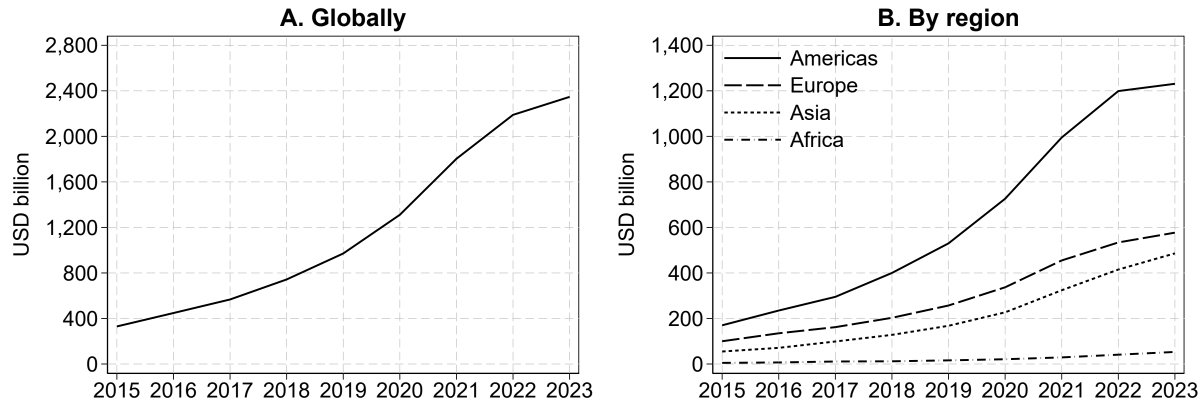
Governments around the world have recently responded to the problem of prolonged payment periods by legislating limits on trade credit maturities. While such legislation clearly improves the working-capital situation of financially constrained suppliers, it may also have unintended side effects by interfering with the useful purposes served by trade credit. Two recent empirical papers illustrate this trade-off. On the one hand, Barrot (2016) finds that a French reform that limited the maturity of invoices issued by trucking firms led to increased efficiency at the industry level, with growth in firm entry and reductions in firm default, especially among financially constrained suppliers. On the other hand, a restriction imposed on the trade-credit terms that a large Chilean retailer could obtain from its smaller suppliers caused the retailer to internalize procurement to its own subsidiaries and reduce purchases from its suppliers (Breza and Liberman, 2017). These findings suggest that one-size-fits-all legislation of trade credit maturities is a blunt tool with unclear net effects on suppliers.

In this paper, we study a financial product innovation that instead offers a market-based resolution of suppliers' and buyers' incompatible interests regarding trade credit maturities. This innovation, known as supply-chain finance (or reverse factoring), was first introduced already in the 1980s, but has grown dramatically in recent years and is today an important product on financial markets across the world: a report from a leading trade association estimates the global supply-chain finance volume in 2023 to be 2.35 trillion USD, up from 0.33 trillion in 2015 (Figure 1), which is almost as large as the total volume of commercial and industrial loans extended by U.S. commercial banks (2.77 trillion USD in 2023).

A supply-chain finance (SCF) program is a financial arrangement that involves three parties: a supplier, a buyer, and a bank (or some other financier).[1] The purpose is to separate the

[1] There is, unfortunately, still no consensus on the exact definitions of the terms supply-chain finance and

Figure 1: Estimated SCF volumes, 2015–2023



This figure illustrates the development of SCF volumes in the world as a whole (Panel A) and by region (Panel B) between 2015 and 2023, according to the estimates provided in the trade association BCR Publishing's annual *World Supply Chain Finance Report*. The figures are printed with BCR Publishing's permission.

dates on which the supplier gets its invoices paid and the dates on which the buyer pays the same invoices, and thereby to improve the working-capital situation for both parties. This is achieved in a way which is similar to traditional factoring: the bank steps in as a factor and purchases the invoices from the supplier at a discount shortly after they are issued, and then receives payment from the buyer when the invoices fall due. The bank thus finances each invoice during the period between the purchase and the payment, and receives interest via the discounted price at which it purchases the invoices.

The main difference between traditional factoring and supply-chain finance is the involvement of the buyer. In traditional factoring, there is no agreement between the buyer and the bank of any kind; instead, the supplier sells invoices to the bank without actively involving its buyers in the process. This exposes the bank to certain risks, such as performance risk and dispute risk, which makes it necessary for the bank to conduct credit assessments of both the supplier and the buyer involved. An SCF program, on the contrary, is set up at the initiative of a buyer, who signs a confirmation letter in which it commits to pay every invoice issued by its suppliers within the program no later than the due date (also known as an irrevocable payment undertaking), as well as approves every invoice to be financed by the bank. This fea-

reverse factoring. We treat supply-chain finance and buyer-centric reverse factoring as interchangeable terms, but stick to the former throughout the paper and spell out exactly what we mean by this term in the following paragraphs. Note, though, that some treat supply-chain finance as a broader concept than we do—including, for example, pre-shipment finance and inventory finance—whereas others define supply-chain finance as we do while treating reverse factoring as a narrower concept.

ture of SCF programs, to be described in more detail below, implies that there is no need for the bank to screen or monitor the suppliers from a credit-risk perspective. Instead, the only risk faced by the bank in an SCF program is the default risk of the buyer, which means that the buyer's creditworthiness determines the financing cost and other conditions of an SCF program. SCF programs thus enable less creditworthy suppliers to piggyback on buyers with stronger creditworthiness and thereby obtain better financing conditions than they could on their own. For this reason, the typical SCF program is initiated by a large, well-established buyer whose creditworthiness on average is stronger than those of its suppliers.

Given these features, SCF programs are often described as a win-win solution for the supplier and the buyer. However, SCF programs are in the great majority of cases also associated with prolonged payment periods: a buyer may, for example, pay the supplier within 40 days prior to the SCF program, but then stretch the payment period to 90 days while ensuring that the supplier is paid by the bank after ten days, at a discount computed based on a financing period of 80 days.[2] The net effect of SCF programs on suppliers therefore depend on how large the interest-rate reduction is compared to the cost of longer payment periods, which increase the discount charged by the bank for any given interest rate. Thus, while SCF programs clearly benefit the buyers that set them up—a buyer with a strong bargaining position wouldn't initiate an SCF program unless it benefited from doing so—the effects of SCF programs on suppliers are a priori less clear.

In this paper, we therefore undertake an empirical evaluation of the real and financial effects of SCF programs on suppliers. We do so using data from two sources. The first is a unique data set covering the entire SCF business of one of the major Swedish banks (henceforth 'the Bank'). The Bank started offering SCF programs on a small scale already in 2005 and now operates SCF programs in four Nordic countries—Denmark, Finland, Norway, and Sweden—as well as in Germany and the UK. The data that the Bank has granted us access to comprise details on all invoices issued by suppliers in its Nordic SCF programs as well as on the programs themselves, most importantly the start date of each program and the identities of the suppliers and buyers involved. The suppliers in the Bank's Nordic SCF programs are from countries all over the world, but we restrict the analysis on suppliers from the ten countries with the most suppliers in the data, which are all European. The second data set is Bureau van Dijk's Orbis, which comprises harmonized financial accounts data for public and private firms across the world, and is frequently used in cross-country firm-level studies (see,

[2]The financing cost of SCF is collected from the supplier, since the bank purchases its invoices at a discount, but the cost can in principle be shared with the buyer by increasing the price of the good or service.

e.g., Gopinath et al., 2017; Díez, Fan and Villegas-Sánchez, 2021; and Gourinchas et al., 2024). We use Orbis to retrieve key items from firms' balance sheets and income statements, such as accounts receivable, sales, employment, cash holdings, total assets, debt, and fixed assets.

The empirical analysis centers on the hypothesis that SCF programs enable suppliers to grow by alleviating their liquidity constraints. We test this hypothesis using a matched difference-in-differences approach, in which we first match each supplier that enrolls in an SCF program with the Bank (treated firms) to a firm that never enrolls in an SCF program with the Bank (control firms) using nearest-neighbor matching, and then estimate difference-in-differences regressions on the matched sample. The key identifying assumption underlying this approach is that SCF suppliers would have developed similarly to their matched control firms had they not enrolled in SCF programs when they did. We provide reasons for why this assumption is likely to hold in section 2.3 below. We also corroborate the assumption empirically by showing that treated firms and control firms (i) are similar across a broad set of covariates in the year before treatment, and (ii) follow parallel trends over a five-year pre-treatment period for all outcome variables that we study.

The empirical analysis proceeds in three steps. The first is to verify that SCF program participation indeed reduces the number of days it takes for a supplier's invoices to be paid. We find that suppliers' receivables days on average decline by eight days in the years following SCF enrollment, which amounts to 20 percent of their average pre-SCF receivable days. The corresponding decline in the ratio of accounts receivable to assets is around four percentage points, which implies that a substantial amount of funding is freed up by the shorter receivable collection period that follows from a supplier's enrollment in an SCF program.

The second step in the analysis is to examine the real effects of SCF programs. We find that suppliers' sales, employment, and fixed assets significantly increase by around 10-15 percent in the years following SCF enrollment, but that the effect on suppliers' profitability, as measured by the EBITDA margin, is precisely zero. SCF programs thus enable suppliers to grow, but not necessarily to become more efficient.

The third and final step is to uncover the mechanisms underlying the real effects of SCF programs. We provide three pieces of evidence corroborating our hypothesis that the real effects are due to relaxed liquidity constraints. First, we apply a test inspired by Banerjee and Duflo (2014): suppliers' net debt should be constant or increase following SCF enrollment if the estimated real effects of SCF programs in fact are due to relaxed liquidity constraints. This is because a liquidity-constrained supplier would use the funds freed up by an SCF program to expand production or invest, rather than to reduce net debt by paying down debt or increasing cash holdings, as an unconstrained supplier would. We find that the estimated treatment effect of SCF programs on net debt is not significantly different from zero, which is consistent with the liquidity-constraint hypothesis. Second, we show that the share of the SCF buyer in a supplier's total sales on average *declines* somewhat after the supplier's enrollment in an SCF program. Hence, the increase in suppliers' sales following SCF enrollment is mainly due to increased sales to non-SCF buyers, which speaks against the alternative hypothesis that the real effects are due to higher demand from the SCF buyer.

Third, and finally, we test for heterogeneity in treatment effects across suppliers that are more and less likely to be financially constrained prior to SCF enrollment. We find that SCF programs have statistically significant real effects on constrained suppliers, but not on unconstrained suppliers, and that the differences in the magnitudes of the respective treatment effects are large. Moreover, we show that net debt increases substantially for constrained suppliers following SCF enrollment, whereas unconstrained suppliers primarily respond by increasing cash holdings. These findings further corroborate the liquidity-constraint hypothesis. Moreover, the observed increase in net debt for constrained suppliers suggest that the real effects of SCF programs is due not only to the liquidity immediately freed up by the reduced need to finance receivables, but also to some combination of (i) an improvement in the supplier's access to external finance outside of the SCF program, and (ii) a reduction in its desired liquidity buffers as a consequence of lower liquidity risk.

This paper is to our knowledge the first empirical study of how supply-chain finance programs affect suppliers. Whereas a large theoretical literature in operations research studies the costs and benefits of SCF for suppliers and buyers, the empirical literature is limited due to the scarcity of suitable data on SCF programs (see Gelsomino et al., 2016, for an extensive literature review and Kouvelis and Xu, 2021, for an important recent theoretical contribution). Our key contribution is therefore to document the real effects of SCF programs on suppliers—as well as the underlying mechanism by which these effects arise—and thereby provide novel evidence on the functioning of a fast-growing financing solution that has become an important part of financial markets in recent years. In doing so, we add to the literature on the real effects of trade credit arrangements (e.g., Barrot, 2016, and Breza and Liberman, 2017), as well as to the broader literature on how working-capital constraints affect firms' ability yo develop and grow (e.g., Banerjee and Duflo, 2014, and Benmelech, Bergman and Seru, 2021).

The empirical papers closest to ours are Shou, Shao and Wang (2021)—who find positive effects of SCF programs on the profitability of Chinese buyers—and Wuttke, Rosenzweig and Heese (2019) and Huang et al. (2020), who use survey data to study the determinants of supplier enrollment in SCF programs. More recently, Chuk, Lourie and Yoo (2024) use proprietary data obtained from a large financial institution that acts as a third-party intermediary in SCF transactions to show that payable days within supplier-buyer pairs indeed are prolonged when supply-chain finance programs are adopted.

2 Data and Empirical Framework

2.1 Data sources

The empirical analysis is based on two main data sets. The first was provided to us by one of the major Swedish banks ('the Bank') and contains detailed information about its entire Nordic receivables-financing business. More specifically, the data set covers every loan facility extended by the Bank within its receivables-financing business, which consists of three main product offerings: lending secured by receivables, traditional factoring, and supply-chain finance (or 'reverse factoring'). We observe both facility-level information—such as start date, interest rate, and advance rate—and information about each invoice financed under a given loan facility—for example the date the invoice was issued, the amount to be paid, the due date, the date the invoice was actually paid, and the respective identities of the supplier and the buyer. The receivables-finance data spans the period 2014–2022 and comprise around 90,000 loan facilities and nine million invoices in total. The main information that we extract from the Bank data is the identities of the suppliers that participate in SCF programs and the year in which they enrolled. Importantly, while the data only cover the period 2014–2022, we observe the start date of all loan facilities that were active at some point during this period. We can therefore include suppliers that enrolled in SCF programs prior to 2014 in the analysis as long as they remained enrolled in 2014.

The second main data set is Bureau van Dijk's Orbis, which comprises harmonized financial-accounts data, as well as demographic and other corporate data, for almost 500 million firms—public as well as private—from more than 100 countries.[3] The data in Orbis is sourced from more than 170 governmental and commercial information providers across the globe; the financial-accounts data for European firms, which is what we use in this paper, are primarily sourced from official national business registers. We use the Orbis Historical

[3] For a thorough description of the Orbis database, see Kalemli-Özcan et al. (2024).

Disk to ensure that our data comprise all firms that were active at any given point in time, rather than only firms that are still active today (Kalemli-Özcan et al., 2024). Since firms in most countries can choose the start and end month of their fiscal year, many observations in the Orbis data do not correspond to calendar years. We deal with this by interpolating the financial statements so that each observation corresponds to a calendar year.[4]

2.2 Institutional details regarding the Bank's SCF programs

The first step in the initiation of an SCF program is that a buyer submits a proposal to the Bank to organize an SCF program on its behalf. The Bank then evaluates the buyer to determine if it meets the criteria for becoming an SCF buyer. The requirements on buyers are quite restrictive: the Bank will typically only initiate SCF programs for large and established buyers with strong creditworthiness. If the Bank and the buyer can agree on the terms of an SCF program, the next step is for the buyer and the Bank to determine which of the buyer's suppliers to include in the program. The main requirement is that the inclusion of the supplier generates a profit for the Bank, which in practice means that the supplier has to have a sufficiently large annual sales volume to the SCF buyer and thus generate business volumes that are large enough to cover the fixed costs associated with including it.[5] While the Bank does not apply a strict size threshold, a supplier typically has to be at least medium-sized according to the official EU firm-size definition to generate business volumes large enough to motivate its inclusion in an SCF program.

Two key contracts are drawn up at the inception of an SCF program. First, the supplier and the Bank signs a contract which states that the Bank intends to buy the invoices that the supplier issues to the SCF buyer. The Bank also ensures at this stage that the seller understands how to correctly assign invoices, so that the Bank obtains right in rem in all invoices issued within the program. Second, the buyer signs a "confirmation letter" in which it commits to pay the Bank for all invoices issued within the program no later than the respective due dates. These contracts ensure that the financing provided by the Bank through an SCF program is only exposed to the credit risk of the buyer, which is what enables suppliers in SCF programs to obtain funding at conditions determined by the buyer's creditworthiness.

Once an SCF program is up and running, the financing process works as follows. The sup-

[4]The length of the fiscal year is typically twelve months, but it can be shorter or longer when a firm enters or exits, or when it changes the timing of its fiscal year. Our interpolation procedure handles these cases by rescaling flow variables so that they correspond to twelve-month periods.

[5]Recall that the Bank is not exposed supplier-side credit risk and that the supplier's creditworthiness is therefore not part of the evaluation.

plier sends the buyer an invoice assigned to the Bank following each delivery. After inspecting the delivery, the buyer approves the invoice and passes it on to the Bank, a critical step in the process since the buyer's approval eliminates any dispute risk around the invoice. The Bank then pays the supplier the invoice amount minus the discount—the average time between invoice issuance and receipt of payment is around seven days in our data, of which the majority is the time taken by the buyer to approve the invoice. After this, the supplier is essentially out of the process as far as that particular invoice is concerned: the receivable disappears from the supplier's balance sheet and is replaced by an increase in cash holdings corresponding to the amount received from the Bank. The previous receivable then shows up as a loan on the asset side of the Bank's balance sheet; for the buyer, meanwhile, the liability remains as a payable on the balance sheet throughout the entire period.[6] Finally, the buyer pays the Bank no later than the due date of the invoice, which extinguishes the payable from the buyer's balance sheet and the loan from the Bank's balance sheet, and concludes the financing of the invoice.

The discount charged by the Bank is computed on the basis of an interest rate—which itself is the sum of a base rate and a margin—and the remaining days of the invoice at the time the Bank purchases it, according to the following formula:

$$Discount = NominalAmount \cdot \left[\frac{Days}{DayCount} \cdot i \right], \tag{1}$$

where $NominalAmount$ is the face value of the invoice, i is the interest rate (base rate plus margin), $Days$ is the number of remaining days until the invoice's due date, and $DayCount$ is the number of days in the year according to the day count convention used in the program (typically 360 or 365).

2.3 Empirical approach

The purpose of the empirical analysis is to evaluate the real and financial effects of SCF programs on suppliers. We do so using a matched difference-in-differences approach, in which we first match each supplier that enrolls in an SCF program with the Bank (treated firms) to a firm that never enrolls in an SCF program with the Bank (control firms), and then estimate

[6] The classification of SCF liabilities as accounts payable (rather than bank debt) on the buyer's balance sheet has been the subject of controversy in recent years, with accounting firms and credit rating agencies calling for increased transparency around buyers' use of SCF. The accounting associations FASB and IASB have therefore introduced new rules, according to which buyers may continue to classify SCF liabilities as trade payables, while being required to disclose the size and key conditions of the programs in their financial statements.

standard two-way fixed effects regressions on the matched sample.

More specifically, we begin by defining a firm as treated starting from the year in which it enrolls in an SCF program with the Bank in the capacity of supplier. We then match each treated firm with one control firm using Abadie and Imbens' (2006) nearest-neighbor matching approach and a small set of matching variables measured in the year prior to each treated firm's SCF enrollment. We conduct the matching with replacement—which implies that a control firm may be matched to several treated firms—and using the Mahalanobis weighting matrix to account for scale differences between the matching covariates.

Having constructed the matched sample, we estimate the following two-way fixed effects regression with event-time indicators:

$$Y_{i,t} = \alpha_i + \eta_{j,t} + \sum_{k=-5}^{5} \gamma^k \cdot \mathbb{1}\left[t - T_i = k\right] + \sum_{k=-5}^{5} \beta^k \cdot \mathbb{1}\left[t - T_i = k\right] \cdot D_i + \varepsilon_{i,t}, \tag{2}$$

where $Y_{i,t}$ is the outcome of interest for firm i in year t, α_i and $\eta_{j,t}$ are firm and industry-year fixed effects, D_i is a treatment indicator equal to one if firm i enrolls in an SCF program with the Bank at some point during the sample period, and $\mathbb{1}\left[t - T_i = k\right]$ is an indicator for being k years from the SCF enrollment year. Note that the enrollment year T_i, and thus also the indicator variable $\mathbb{1}\left[t - T_i = k\right]$, are defined at the level of matched pairs—i.e., a control firm is assigned the same value for T_i as the treated firm to which it is matched. The coefficients of interest are the β^k for the post-treatment years ($k \geq 0$), which capture the effect of SCF program participation over a k-year horizon. The β^k for the pre-treatment years, meanwhile, capture any differences in pre-treatment trends between SCF firms and non-SCF firms and are thus a key diagnostic for assessing the validity of the research design. We winsorize all dependent variables at the first and the ninety-ninth percentiles and cluster standard errors by firm in all regressions.

It is occasionally convenient to have a single estimated treatment effect to refer to. We will therefore also make use of the following compact pre-post version of equation (2):

$$Y_{i,t} = \alpha_i + \eta_{j,t} + \gamma \cdot \mathbb{1}\left[t - T_i \geq 0\right] + \beta \cdot \mathbb{1}\left[t - T_i \geq 0\right] \cdot D_i + \varepsilon_{i,t}, \tag{3}$$

where $[t - T_i \geq 0]$ is an indicator for years after the SCF enrollment year, and all other variables are defined as before. The estimate of the coefficient β from (3) captures the average effect of SCF enrollment on the dependent variable over the entire post-treatment period.

Two remarks on the empirical approach are in order. First, we do not deem endogeneity stemming from a supplier's decision to join an SCF program at a specific point in time to be a major threat to the analysis. To begin with, SCF programs are set up at the initiative of large buyers with many suppliers, which implies that the *opportunity* to join an SCF program at a given point in time is essentially beyond any individual supplier's control.[7] Moreover, few suppliers turn down an invitation to join an SCF program, according to the Bank; hence, there is little scope for endogeneity stemming from suppliers' *decisions* about whether to enroll in SCF programs once the opportunities arise.[8] Hence, it is likely that the timing of a supplier's enrollment in an SCF program is largely exogenous to the characteristics of the supplier, which is necessary to obtain unbiased estimates of the effects of SCF program participation on suppliers. We will later corroborate this claim by showing that treated firms and control firms (i) are similar across a broad set of covariates in the year before treatment, and (ii) follow parallel pre-treatment trends for all outcome variables that we study.

It is, on the other hand, true that firms enrolling in SCF programs tend to differ from the average firm in the economy in certain dimensions—for example, SCF suppliers are on average larger and older than other suppliers. These differences are due to the fact that only suppliers of large and established buyers get the opportunity to enroll in SCF programs, which implies that the pool of potential SCF suppliers is a non-random subset of the population of firms. Hence, the *external* validity of our results are limited to suppliers of large and established buyers, but the fact that only a subset of suppliers get the opportunity to enroll in SCF programs is not in itself a threat to the *internal* validity of the results. These considerations guide our matching strategy, which is to conduct the matching based on a small set of basic firm characteristics to increase the likelihood that controls are drawn from the pool of potential SCF suppliers.

Second, a series of recent papers have pointed out that two-way fixed effects models can produce severely biased results when applied to settings with staggered treatment, like ours (see, e.g., de Chaisemartin and D'Haultfœuille, 2020, Callaway and Sant'Anna, 2021, and Baker, Larcker and Wang, 2022). In short, the bias arises when treatment effects vary over time and already-treated units act as controls, and can be severe enough to lead staggered DiD effect estimates to have the opposite sign of the true treatment effect even if treatment

[7]Senior staff at the Swedish receivables-finance division of the Bank informed us in an interview that they could only recall a single case in which an SCF program was set up at the initiative of a supplier.

[8]The specific reason for why so few suppliers turn down an offer to join an SCF program does not matter here—for example, whether it is because they judge SCF programs to be beneficial or because they are pressured into enrolling by important buyers. What matters is that most suppliers—for whatever reason—make the same choice and thus that there is limited variation across suppliers that could give rise to selection bias.

assignment is random. We ensure that our estimates are not subject to this type of bias by only selecting never-treated firms as controls when matching. The feasibility of this stringent “clean control” condition in our setting is due to the large number of never-treated suppliers with characteristics similar to the treated suppliers, which allows us to find good matches for the treated firms among the never-treated firms.

2.4 Sample construction and matching

To construct the sample for the empirical analysis, we begin by identifying all firms that show up as suppliers in SCF programs in the data from the Bank. While the majority of buyers that set up SCF programs with the Bank are Nordic firms, many of their suppliers are not. Hence, our initial data set consists of suppliers from all over the world, although the majority are Nordic firms and most of the remainder are firms from other European countries. We restrict the sample to firms from the ten countries with the most suppliers, which are the following (ordered by number of suppliers in the raw data): Denmark, Sweden, Germany, Finland, Norway, the Netherlands, Poland, Lithuania, Estonia, and Spain. Imposing this restriction leaves 911 of the initial 985 suppliers in the data.

The next step is to merge the SCF suppliers with the Orbis data to obtain an annual firm-level panel with financial-accounts data. The challenge in doing so is that we do not have an unambiguous firm identifier common to both data sets. We therefore begin by undertaking exact matching based on company names and then search manually in the Orbis database to identify the correct match for the suppliers that we fail to match exactly on name. Through these two steps, we are able to identify almost 75 percent of the SCF suppliers in the Orbis data; for the remaining suppliers, we are either unable to identify a candidate match altogether, or to identify a candidate that we are sufficiently confident is the correct one. The final sample consists of 652 SCF suppliers with the following country distribution: Denmark (258), Sweden (213), Germany (33), Finland (32), Norway (32), Lithuania (22), Estonia (17), Spain (16), the Netherlands (15), and Poland (14).[9] The SCF enrollment years for the sample suppliers span the period 2005–2022.

The final step in the sample construction is to match one control supplier to each SCF supplier. As described above, we conduct the matching based on covariates measured in year

[9] A limitation of the Orbis data is that sales is missing for around half of the Danish SCF suppliers. This is a consequence of Danish law, which does not mandate all firms to report sales figures in their financial accounts. To avoid reducing sample size unnecessarily, we keep these suppliers and include them in the estimations whenever possible (we exclude sales from the set of matching variables when we match control firms to these suppliers). The number of observations will therefore be lower in estimations where sales is part of the dependent variable.

$T_i - 1$, where T_i is the year in which supplier i enrolled in an SCF program with the Bank. We match exactly on country and two-digit NACE industry, and then find the nearest neighbor within the respective country-industry cells based on the Mahalanobis distance between the treated firms and the potential controls in terms of the following matching variables: log assets, log sales, number of employees, and the ratios of debt to assets, EBITDA to assets, and cash holdings to assets.

The outcome of the matching procedure is summarized in Table 1, in which we assess the covariate balance between treated firms and their matched control firms in year $T_i - 1$. We conduct the comparison in terms of the matching variables (Panel A), as well as a set of variables not used in the matching (Panel B), and based on a comparison metric proposed by Imbens and Rubin (2015), namely, the normalized difference in means. This metric captures the difference in means across two groups expressed in terms of standard deviations and has the desirable property that the likelihood of rejecting equality of means does not increase mechanically with sample size, which is the case for standard t-tests. Table 1 demonstrates that treated firms and control firms are quite similar across all variables under consideration—to see this, note that most normalized differences are close to zero and that the magnitude of the largest difference is only 0.32.[10] Hence, treated firms do not appear to differ meaningfully from control firms in the year before their enrollment in an SCF program.

3 Results

3.1 The effect of SCF program participation on receivable days

The first step in the empirical analysis is to verify that SCF program participation indeed reduces the number of days it takes for suppliers' invoices to be paid (receivable days). We do so by estimating the baseline difference-in-differences model with a supplier's average receivable days as dependent variable (we follow standard practice and proxy average receivable days as the ratio of accounts receivable to sales multiplied by 365). The results of this exercise, plotted in Panel A of Figure 2, show that the receivable days of treated firms and control firms follow parallel trends in the years leading up to the former's enrollment in the SCF program. The receivables days of treated firms then decline significantly relative to control firms—the decline takes place gradually over the supplier's first year in the SCF program and

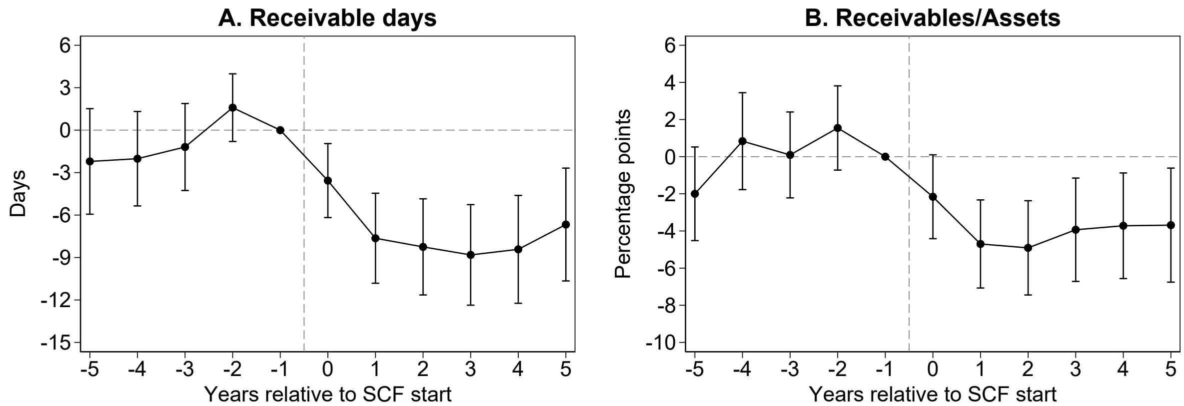
[10]As a point of reference, Imbens and Rubin (2015) observe a maximum normalized difference of 0.30 in an analysis of the data from an experiment with random treatment assignment and judge this to be evidence of strong covariate balance.

Table 1: Covariate balance

	Treated (SCF firms)		Matched controls		Normalized
	Mean	SD	Mean	SD	difference
A. Covariates used in matching					
Assets (MEUR)	76.6	287.0	75.4	302.9	0.00
Sales (MEUR)	172.5	579.5	161.5	609.6	0.02
Number of employees	302	948	254	859	0.05
Debt/Assets	0.68	0.20	0.62	0.23	0.32
EBITDA/Assets	0.13	0.17	0.13	0.15	–0.02
Cash/Assets	0.08	0.12	0.08	0.12	–0.01
B. Other covariates					
Accounts receivable/Sales	0.11	0.08	0.10	0.08	0.20
Accounts payable/Assets	0.18	0.14	0.14	0.14	0.28
Asset tangibility	0.26	0.22	0.30	0.24	–0.17
EBITDA/Sales	0.05	0.07	0.05	0.07	–0.06
Net working capital/Assets	0.34	0.21	0.32	0.22	0.12
Investment/Assets	0.07	0.12	0.08	0.14	–0.03
Number of firms	652		652		
Number of unique firms	652		626		

This table compares treated firms with the matched control firms in the year before the treated firm enrolled in an SCF program with the bank. The covariates listed in Panel A were used in the matching; those in Panel B were not. Asset tangibility is defined as the ratio of fixed assets to total assets. The normalized difference is defined as $\left(\bar{X}_{Z<0} - \bar{X}_{Z\geq 0}\right) / \left[\left(S^2_{Z<0} + S^2_{Z\geq 0}\right)/2\right]^{0.5}$, where $\bar{X}$ and S are the means and standard deviations of the comparison variables in the respective groups (Imbens and Rubin, 2015).

Figure 2: The effect of SCF program participation on acounts receivable



This figure plots the β^k coefficients from the estimation of equation (2) with average receivable days (Panel A) and the ratio of accounts receivable to assets (Panel B), respectively, as dependent variables. Average receivable days are defined as the ratio of accounts receivable to sales multiplied by 365. The vertical lines correspond to 95-percent confidence intervals.

then stabilizes at a statistically significant decrease of around eight days, which corresponds to around 20 percent of average pre-SCF receivable days. The results are very similar when we instead estimate the model with the ratio of accounts receivable to assets as dependent variable: after a gradual decline over the first year after SCF enrollment, the ratio stabilizes at a level around four percentage points below its pre-SCF level (Panel B, Figure 2).

The estimated decrease of eight days is very close to what one would expect, as the following back-of-the-envelope calculation demonstrates. The average ratio of accounts receivable to sales was 0.11 among treated suppliers in the year before SCF enrollment (Table 1, Panel B), which implies an average receivable period of just over 40 days. The average time to payment for invoices in the SCF program, on the other hand, is around seven, which implies that the days to payment for invoices to the SCF buyer on average decrease by 33 days when the supplier enrolls in the SCF program (provided that the pre-SCF maturity of invoices to the SCF buyer corresponded to the supplier's average invoice maturity). Finally, the average share of the SCF buyer in the SCF supplier's sales was 20 percent in the SCF enrollment year (see Figure 4 below). Given this, we would expect a supplier's receivable period to decline by $33 \cdot 0.20 = 6.6$ days on average when enrolling in an SCF program, which, reassuringly, is close to what we estimate in the data.[11]

[11] The slightly higher estimate in the data suggests that the pre-SCF maturity of invoices to the SCF buyer was around 5-7 days higher than the supplier's average invoice maturity.

Table 2: Real effects of SCF program participation

	(1) Sales	**(2)** Employment	**(3)** Fixed assets	**(4)** Profitability
Treated × Post	0.123***	0.088***	0.155***	–0.003
	[0.041]	[0.030]	[0.053]	[0.003]
Firm FE	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes
Number of obs.	8,243	10,770	11,157	8,243
Adjusted R^2	0.955	0.954	0.925	0.531

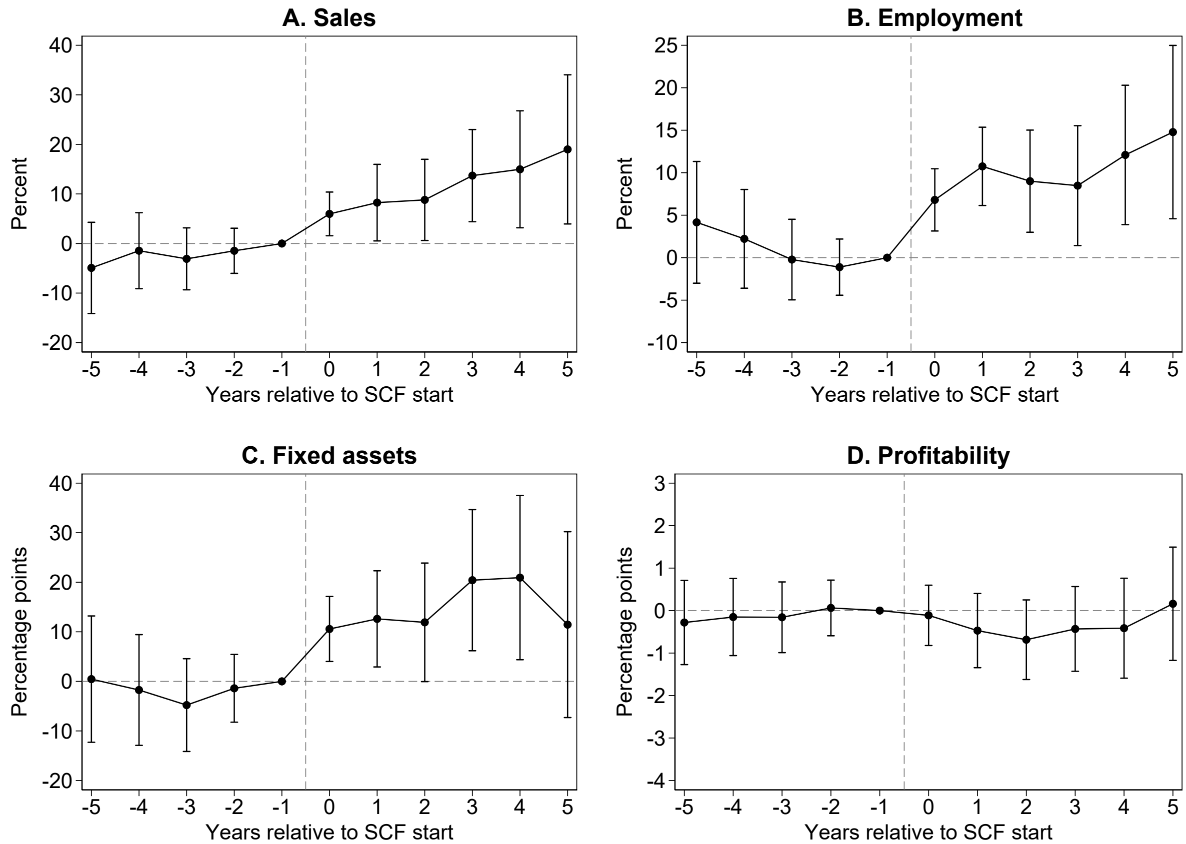
This table reports estimation results for the compact difference-in-differences model specified in (3) where the dependent variable is log sales, log employment, the ratio of investments to assets, and the ratio of EBITDA to sales, respectively. Treated × Post refers to the main interaction term in equation (3), $\mathbb{1}\left[t - T_i \geq 0\right] \cdot D_i$. Standard errors clustered by firm are reported in square brackets. ***, **, and * denote statistical significance at the ten, five, and one percent levels, respectively.

3.2 The real effects of SCF program participation

Having documented that SCF program participation leads to a statistically and economically significant decline in suppliers' receivable days, we turn to the real effects of SCF programs on suppliers. We assess the real effects by estimating the difference-in-differences models with, in turn, log sales, log employment, log fixed assets, and profitability (EBITDA margin) as dependent variables. We begin by reporting results for the compact pre-post model.

Column (1) in Table 2 shows that enrollment in an SCF program leads a supplier's sales to increase by a statistically significant 12.3 percent. The estimated employment and fixed-assets effects, reported in columns (2) and (3), are also statistically significant and of similar magnitude as the sales effect: employment increases by 8.8 percent, and fixed assets by 15.5 percent, in the years following a supplier's enrollment in an SCF program. These results suggest that SCF programs have a substantial positive effect on the real activity of suppliers. The results in column (4) show, on the other hand, that the effect of SCF programs on suppliers' profitability, as measured by the EBITDA margin, is precisely zero. Hence, SCF programs appear to enable suppliers to operate on a larger scale, but not more efficiently—or, conceivably,

Figure 3: Real effects of SCF program participation



This figure plots the β^k coefficients from the estimation of equation (2) with log sales (Panel A), log employment (Panel B), log fixed assets (Panel C), and the ratio of EBITDA to sales (Panel D) as dependent variables. The vertical lines correspond to 95-percent confidence intervals.

any efficiency gains enabled by SCF program participation are offset by the discount on the invoice payment that the supplier receives from the bank.

We now turn to the results from the estimation of the dynamic difference-in-differences model, which are reported in Figure 3. Note first that the pre-treatment trends are parallel for all four dependent variables, which demonstrates that the real activity of treated firms and control firms developed similarly in the five-year period leading up to the former's enrollment in an SCF program. This further corroborates the claim that the timing of a supplier's enrollment in an SCF program is largely exogenous to the characteristics of the supplier itself. Next, the coefficients for the post-treatment periods show that the sales, employment, and investment effects of SCF program participation develop gradually over the post-treatment period, which shows the beneficial effects of SCF program participation are long-lasting (although the effect estimates become somewhat less precise for longer estimation horizons, in particular for fixed assets).

3.3 Uncovering the mechanism: The role of liquidity constraints

Our hypothesis is, as discussed above, that the increase in suppliers' real activity following enrollment in SCF programs is due to relaxed liquidity constraints. There are, generally speaking, three channels through which an SCF program may relax a supplier's liquidity constraints and thereby enable it to grow. First, the reduction in receivable days induced by an SCF program immediately improves a supplier's liquidity by freeing up funds that were previously locked up in receivables, which can then be used to finance production expansions. Second, to the extent that lenders look favorably on firms with shorter receivable collection periods, suppliers' own access to external finance may improve following enrollment in an SCF program. Third, SCF programs essentially eliminate the risk of late payments on invoices issued to SCF buyers and thereby reduce the liquidity risk faced by suppliers. SCF suppliers may consequently be able to reduce precautionary liquidity buffers and thereby free up funds for operational purposes.[12] In what follows, we conduct a series of empirical exercises to shed light on the mechanisms underlying the real effects of SCF programs.

3.3.1 The effect of SCF programs on suppliers' net debt

We begin by assessing the effect of SCF programs on suppliers' net debt. The idea, which follows the logic of Banerjee and Duflo (2014) closely, is the following. A supplier whose production is suboptimally small due to liquidity constraints will use the funds freed up by an SCF program to expand production; an unconstrained supplier operating at its desired level of output will, on the other hand, use the unlocked liquidity to reduce net debt by either paying down debt or increasing cash holdings. Hence, we should *not* observe a decline in net debt following SCF enrollment if the estimated real effects of SCF programs indeed are due to relaxed liquidity constraints.

We implement the test by estimating the pre-post version of the difference-in-differences model with net debt and its respective components as dependent variables. The results are reported in Table 3. The estimated effect of SCF program participation on net debt, reported in column (1), is small and statistically insignificant, which is consistent with the hypothesis that relaxed liquidity constraints explain the real effects of SCF programs on suppliers.

[12]See Amberg et al. (2023) for a general argument along these lines.

Table 3: Effects of SCF program participation on financial variables

	(1) Net debt/ Assets	**(2)** Cash/ Assets	**(3)** Total debt/ Assets	**(4)** Short-term debt/Assets	**(5)** Long-term debt/Assets
Treated $\times$ Post	0.005	0.009*	0.016*	0.015*	0.001
	[0.011]	[0.005]	[0.009]	[0.009]	[0.006]
Firm FE	Yes	Yes	Yes	Yes	Yes
Industry-year FE	Yes	Yes	Yes	Yes	Yes
Number of obs.	10,910	10,992	11,344	11,428	11,345
Adjusted R^2	0.703	0.614	0.717	0.719	0.693

This table reports estimation results for the compact difference-in-differences model specified in (3) with net debt and its respective components as dependent variables. Treated $\times$ Post refers to the main interaction term in equation (3), $\mathbb{1}\left[t - T_i \geq 0\right] \cdot D_i$. Standard errors clustered by firm are reported in square brackets. ***, **, and * denote statistical significance at the ten, five, and one percent levels, respectively.

More specifically, that the estimated net-debt effect is not significantly different from zero suggests that the primary explanation for the real effects is the first mechanism discussed above—namely, the immediate impact of shorter receivable collection periods on suppliers' liquidity—because the second and third mechanisms imply that net debt should *increase* following a supplier's enrollment in an SCF program.

In columns (2)-(5), we unpack the overall net-debt effect by looking at each component of net debt separately. Columns (2) and (3) show that the overall (insignificant) increase in the ratio of net debt to assets of 0.5 percentage points reported in column (1) is due to an increase in the ratio of debt to assets of 1.6 percentage points and an increase in the ratio of cash holdings to assets of 0.9 percentage points. Hence, the increase in borrowing is largely offset by the simultaneous increase in cash holdings. The results in columns (4) and (5), meanwhile, show that the increase in total debt is entirely due to an increase in short-term debt.

The corresponding estimation results from the the dynamic difference-in-differences model are reported in Figure B1 in Online Appendix B; the main take-away is that treated firms and control firms follow parallel pre-treatment trends in terms of net debt as well as all components of net debt. Otherwise, the dynamic effect estimates yield the same conclusion as one obtains from Table 3: SCF programs do not have a significant effect on net debt, as the modest increases in cash holdings and debt offset each other. That suppliers on average increase both debt and cash holdings following SCF enrollment is at first sight puzzling, but turns out to have a straightforward explanation, as we will show in section 3.3.3 below.

3.3.2 The share of SCF buyers in SCF suppliers' total sales

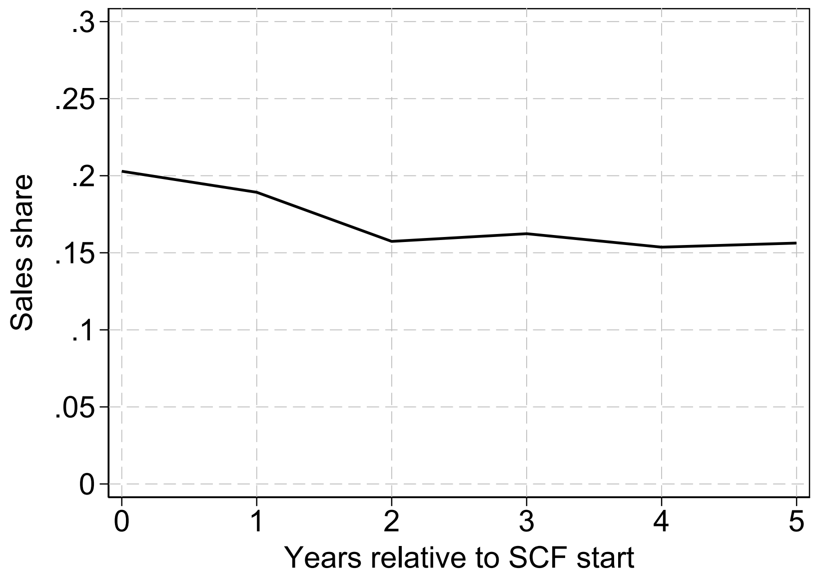
If liquidity constraints explain the real effects of SCF programs, we should observe a broad-based increase in suppliers' sales following SCF enrollment, rather than an increase driven exclusively by higher sales to the SCF buyer. An alternative hypothesis is that buyers initiate SCF programs when they grow fast and have high demand for intermediate inputs, and that the sales increase of their suppliers following SCF enrollment therefore is due to higher demand rather than relaxed liquidity constraints. If so, we would expect the share of the SCF buyer in a supplier's total sales to grow following the latter's enrollment in an SCF program, whereas we would expect it to remain constant or decrease if the overall sales increase is due to relaxed liquidity constraints. Our second test of the liquidity-constraint hypothesis is therefore to assess how the sales share of SCF buyers in SCF suppliers' total sales develop in the years following the latter's enrollment in SCF programs.

To this end, we aggregate all invoices issued by the SCF supplier to the SCF buyer in each year and compute the sales share of the SCF buyer as the total invoiced amount during a given year divided by the supplier's total sales for the same year, as reported in its financial accounts. We then plot how the average sales share of the SCF buyer develops in the years following a supplier's enrollment in an SCF program. Before doing so, two data limitations are important to point out. First, we only observe the invoices issued by SCF suppliers to SCF buyers after the SCF program has commenced, which implies that we cannot assess pre-treatment trends in the sales share of the SCF buyer.[13] Second, we only observe invoice-level data from 2014 and onwards, and can therefore not include suppliers that enrolled in SCF programs earlier than this in our assessment of SCF buyers' sales shares.

We plot the average sales share of the SCF buyer in the SCF supplier's total sales in Figure 4. The figure shows that the average sales share is 20 percent in the year in which the supplier enrolls in the SCF program; it then gradually declines over the following two years before

[13] Since few SCF programs begin exactly at the start of a calendar year and we only observe invoices issued after the start of a program, we need to re-weight the sales volume to the SCF buyer in the first year to get the sales share right. We do this by dividing the observed sales volume to the SCF buyer in year $t = 0$ with the share of the year in which the SCF program was active. Note also that a few observed shares are either smaller than zero or larger than one; we handle these by dropping the former and setting the latter to one.

Figure 4: The average sales share of the SCF buyer in the SCF supplier's total sales



This figure plots the average share of the SCF buyer in an SCF supplier's total sales in the years following the latter's enrollment in an SCF program with the Bank. The sales share is computed as the ratio of the total invoice amount to the SCF buyer during a given year, as observed in the bank data, to the supplier's total sales during the same year, as reported in its financial accounts. We re-weight the invoice amount to the SCF buyer in year $t = 0$ by dividing it with the share of the year in which the SCF program was active; the latter is computed as the number of days from the first SCF invoice to the last day of the year, divided by 365.

stabilizing at around 16 percent from that point onwards. The decline in the sales share of the SCF buyer suggests that the sales increase observed after a supplier's enrollment in an SCF program is predominantly accounted for by increased sales to non-SCF buyers. This is consistent with the notion that SCF programs enable suppliers to grow by relaxing liquidity constraints, and speaks against the alternative hypothesis that the sales growth observed after SCF enrollment is due to higher demand from the SCF buyer.

3.3.3 Heterogeneity in treatment effects depending on ex-ante financial constraints

The final test of the liquidity-constraint hypothesis is based on cross-sectional heterogeneity in treatment effects across firms that are more and less likely to be financially constrained. The logic is straightforward: if the real effects of SCF programs are due to relaxed liquidity constraints, we should observe stronger real effects among firms that are ex-ante more likely to be financially constrained. We implement the test by estimating a triple-differenced version of the compact difference-in-differences model, where the additional interaction term is an indicator for whether a firm is financially constrained.

Our measure of financial constraints is, following Aretz, Campello and Marchica (2020), based on Ferrando et al.'s (2015) financial-constraint index, which is created on the basis of data from ECB's SAFE survey on firms' access to finance. The SAFE index is—like other widely used proxies for financial constraints, such as the Kaplan-Zingales and Whited-Wu indices—computed using the coefficients from a regression of a financial constraint indicator on a set of observable firm characteristics. This index is well-suited for our empirical setting, since it is derived from firm-level data that largely cover the same countries and time periods as our data; we thereby mitigate the problem that a specific financial-constraint index may become uninformative when applied to firms that are different from those used to construct the index (see, e.g., Whited and Wu, 2006, and Farre-Mensa and Ljungqvist, 2016).

We proceed by computing the SAFE index for each firm in our sample and then assigning firms in the bottom tercile of the distribution to the group of firms that ex-ante are less likely to be financially constrained (low SAFE score/unconstrained) and firms in the top tercile to the group of firms that ex-ante are more likely to be constrained (high SAFE score/constrained).[14] Having done so, we estimate the following triple-differenced version of the compact difference-differences model:

$$Y_{i,t} = \alpha_i + \eta_{j,t} + \gamma \cdot \mathbb{1}\left[t - T_i \geq 0\right] + \delta \cdot \mathbb{1}\left[t - T_i \geq 0\right] \cdot C_i + \beta \cdot \mathbb{1}\left[t - T_i \geq 0\right] \cdot D_i + \tau \cdot \mathbb{1}\left[t - T_i \geq 0\right] \cdot D_i \cdot C_i + \varepsilon_{i,t}, \quad (4)$$

where C_i is an indicator equal to one if firm i falls in the top tercile of the SAFE score distribution and zero if it falls in the bottom tercile of the distribution. All other variables are defined as before. The estimated treatment effect is given by $\hat{\beta}$ for unconstrained firms and by $\hat{\beta} + \hat{\tau}$ for constrained firms; the difference between the two is given by $\hat{\tau}$. We report the estimation results in Table 4.

To begin with, columns (1)-(3) report the estimated treatment effects for the three real outcomes for which the baseline treatment effect is positive and significant, namely, sales, employment, and fixed assets. In each case, the treatment effect is large in magnitude and statistically significant for constrained firms; for example, the employment of a financially constrained supplier is estimated to increase by 17 percent in the years following enrollment in an SCF program. For unconstrained firms, on the other hand, the estimated treatment

[14] We refer the reader to Online Appendix A for details on our implementation of the SAFE index and to Ferrando et al. (2015) for details on the index itself.

Table 4: Cross-sectional heterogeneity in treatment effects

	Real effects			Financial effects			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
	Sales	Employ-ment	Fixed assets	Net debt/ Assets	Cash/ Assets	Total debt/ Assets	Receivable days
Unconstrained	0.094	–0.025	0.084	–0.021	0.029**	0.01	–7.077***
	[0.063]	[0.062]	[0.109]	[0.021]	[0.014]	[0.015]	[2.249]
Constrained	0.275***	0.165***	0.283**	0.039*	–0.003	0.038*	–7.231***
	[0.080]	[0.062]	[0.119]	[0.022]	[0.007]	[0.021]	[2.263]
Difference	0.181*	0.190**	0.199	0.060*	–0.032**	0.028	–0.154
	[0.105]	[0.090]	[0.166]	[0.031]	[0.016]	[0.026]	[3.256]
Number of obs.	4,343	4,397	4,519	4,306	4,354	4,528	4,317

This table reports subsample treatment effects from estimations of the triple-differenced model specified in (4). Firms in the bottom tercile of the SAFE score distribution are classified as unconstrained, and those in the top tercile as constrained (a high SAFE score corresponds to tight financial constraints). The estimated treatment effects is given by $\hat{\beta}$ for unconstrained firms and by $\hat{\beta} + \hat{\tau}$ for constrained firms; the difference between the two is given by $\hat{\tau}$. The number of observations refer to the the total number of observations across both estimations in each column. Standard errors clustered by firm are reported in square brackets. ***, **, and * denote statistical significance at the ten, five, and one percent levels, respectively.

effects are statistically insignificant for all three variables and considerably smaller in magnitude than the corresponding estimates for constrained firms. The difference in treatment effects across the two groups is, however, not statistically significant in the case of fixed assets.

To shed light on the drivers of the differences in real effects across constrained and unconstrained suppliers, we turn next to an assessment of heterogeneity in the financial effects of SCF programs. We do this by estimating the triple-differenced model with net debt as well as its two main components, cash and total debt, as dependent variables. The results are reported in columns (5)-(7). The estimates show that the effect of SCF programs on net debt is positive for constrained suppliers: the ratio of net debt to assets increases by 3.9 percentage points in the years following SCF enrollment, as a consequence of increased borrowing. This implies that for financially constrained suppliers, the indirect channels through which SCF programs may relax liquidity constraints—increasing credit supply and reducing the size of desired liquidity buffers—are almost as important as the direct effect coming from the reduced necessity to lock up funds in receivables.

Unconstrained suppliers, on the other hand, significantly increase their cash holdings following SCF enrollment, which is precisely what one would expect from a supplier that operated at its desired level of output when enrolling in an SCF program. The net-debt response of unconstrained suppliers is nevertheless statistically insignificant, as part of the increase in cash holdings is offset by an (insignificant) increase in borrowing. The differing responses of constrained and unconstrained suppliers help explain the puzzling finding that both cash holdings and borrowing increase following SCF enrollment in the full sample: namely, these responses suggest that the cash increase is driven by unconstrained suppliers and the borrowing increase by constrained suppliers.

Finally, a potential concern with the subsample results presented in Table 4 is that the average treatment intensity could differ between constrained and unconstrained suppliers. To see this, note that while our treatment indicator is binary, the underlying intensity of treatment varies with the share of the SCF buyer in a supplier's total sales—the larger is this share, the larger is the effect of an SCF program on a supplier's overall receivable days. Hence, if the average treatment intensity differs between constrained and unconstrained suppliers, the heterogeneous treatment effects documented in Table 4 could be due to different treatment intensities, rather than the differing degrees to which firms are financially constrained (recall that the liquidity-constraint hypothesis predicts differing real effects *conditional on a given treatment intensity*). To assess this concern, we test whether the effect of SCF programs on suppliers' receivable days differ between constrained and unconstrained firms. The results, reported in column (7) of Table 4, show that the estimated treatment effects are virtually identical for the two groups of firms. It is thus unlikely that differing treatment intensities explain the heterogeneity in the treatment effects documented in Table 4.

In sum, the results presented in this section show, first, that the real effects of SCF programs are accounted for by suppliers that are likely to be financially constrained ex-ante; and second, that the behavior of these suppliers following enrollment in SCF programs is what one would expect if they indeed are constrained. These results further corroborate the claim that SCF programs enable suppliers to grow by relaxing their liquidity constraints.

4 Concluding Remarks

Supply-chain finance, a financial product innovation that offers to resolve buyers' and suppliers' diverging interests about trade credit maturities, has grown dramatically in recent years. SCF programs are often described as win-win solutions that benefit both suppliers and buyers, but the net effect of SCF participation on suppliers is a priori unclear and the extant empirical evidence is scant. In this paper, we use unique invoice-level data covering the SCF programs operated by a large Swedish bank to empirically evaluate the real and financial effects of SCF program participation on suppliers. We find that SCF programs have large positive real effects—suppliers' sales, employment, and fixed assets grow significantly in the years following SCF enrollment—and that the underlying mechanism through which these effects arise is relaxed liquidity constraints. These findings underscore that working-capital constraints are a serious impediment to firms' ability to develop and grow (cf. Banerjee and Duflo, 2014).

One important policy implication of our findings concerns the legal limits on payment periods in business-to-business transactions that many governments have imposed in recent years, for example the European Commission's Late Payment Directive. Trade credit issuance has both costs and benefits for suppliers, as shown by Breza and Lieberman (2017), which implies that legal limits on trade credit need not be uniformly positive even for the suppliers that are intended to benefit from such legislation. Our results indicate that SCF programs can be a way of preserving the benefits of trade credit for facilitating trade, while reducing the costs of issuing trade credit for suppliers. Given this, regulators may want to consider exceptions for market-based solutions, such as SCF-programs, when imposing legal limits on payment periods.[15]

References

Abadie, Alberto, and Guido W. Imbens. 2006. "Large Sample Properties of Matching Estimators for Average Treatment Effects." *Econometrica*, 74(1): 235–267.

Amberg, Niklas, Tor Jacobson, Anna Rogantini Picco, and Vincenzo Quadrini. 2023. "Dynamic Credit Constraints: Theory and Evidence from Credit Lines." Sveriges Riksbank Working Paper No. 422.

Aretz, Kevin, Murillo Campello, and Maria-Teresa Marchica. 2020. "Access to collateral and the democratization of credit: France's reform of the Napoleonic Security Code." *Journal of Finance*, 75(1): 45–90.

[15] Kouvelis and Xu (2021) make a similar argument on the basis of a theoretical model

Baker, Andrew C., David F. Larcker, and Charles C.Y. Wang. 2022. "How much should we trust staggered difference-in-differences estimates?" *Journal of Financial Economics*, 144(2): 370–395.

Banerjee, Abhijit V., and Esther Duflo. 2014. "Do Firms Want to Borrow More? Testing Credit Constraints Using a Directed Lending Program." *Review of Economic Studies*, 81(2): 572–607.

Barrot, Jean-Noël. 2016. "Trade Credit and Industry Dynamics: Evidence from Trucking Firms." *Journal of Finance*, 71(5): 1975–2016.

Benmelech, Efraim, Nittai Bergman, and Amit Seru. 2021. "Financing Labor." *Review of Finance*, 25(5): 1365–1393.

Breza, Emily, and Andres Liberman. 2017. "Financial Contracting and Organizational Form: Evidence from the Regulation of Trade Credit." *Journal of Finance*, 72(1): 291–324.

Callaway, Brantly, and Pedro H.C. Sant'Anna. 2021. "Difference-in-Differences with multiple time periods." *Journal of Econometrics*, 225(2): 200–230.

Chuk, Elizabeth, Ben Lourie, and Il Sun Yoo. 2024. "Supply Chain Finance: An Early Empirical Examination." Manuscript.

de Chaisemartin, Clément, and Xavier D'Haultfœuille. 2020. "Two-Way Fixed Effects Estimators with Heterogeneous Treatment Effects." *American Economic Review*, 110(9): 2964–96.

Díez, Federico J., Jiayue Fan, and Carolina Villegas-Sánchez. 2021. "Global declining competition?" *Journal of International Economics*, 132: 103492.

Farre-Mensa, Joan, and Alexander Ljungqvist. 2016. "Do Measures of Financial Constraints Measure Financial Constraints?" *Review of Financial Studies*, 29(2): 271–308.

Ferrando, Annalisa, Matteo Iudice, Carlo Altomonte, Sven Blank, Marie-Hélène Felt, Philipp Meinen, Katja Neugebauer, and Iulia Siedschlag. 2015. "Assessing the financial and financing conditions of firms in Europe: The financial module in CompNet." ECB Working Paper No. 1836.

Gelsomino, Luca Mattia, Riccardo Mangiaracina, Alessandro Perego, and Angela Tumino. 2016. "Supply chain finance: a literature review." *International Journal of Physical Distribution & Logistics Management*, 46(4): 348–66.

Gopinath, Gita, Şebnem Kalemli-Özcan, Loukas Karabarbounis, and Carolina Villegas-Sanchez. 2017. "Capital Allocation and Productivity in South Europe*." *Quarterly Journal of Economics*, 132(4): 1915–1967.

Gourinchas, Pierre-Olivier, Sebnem Kalemli-Özcan, Veronika Penciakova, and Nick Sander. 2024. "SME Failures Under Large Liquidity Shocks: An Application to the COVID-19 Crisis." *Journal of the European Economic Association*, forthcoming.

Huang, Qiuping, Xiande Zhao, Min Zhang, KwanHo Yeung, Lijun Ma, and Jeff Hoi yan Yeung. 2020. "The joint effects of lead time, information sharing, and the accounts receivable period on reverse factoring." *Industrial Management & Data Systems*, 120(1): 215–230.

Imbens, Guido W., and Donald B. Rubin. 2015. *Causal Inference for Statistics, Social, and Biomedical Sciences: An Introduction.* Cambridge: Cambridge University Press.

Kalemli-Özcan, Sebnem, Bent E. Sørensen, Carolina Villegas-Sanchez, Vadym Volosovych, and Sevcan Yesiltas. 2024. "How to Construct Nationally Representative Firm Level Data from the Orbis Global Database: New Facts on SMEs and Aggregate Implications for Industry Concentration." *American Economic Journal: Macroeconomics*, forthcoming.

Kouvelis, Panos, and Fasheng Xu. 2021. "A Supply Chain Theory of Factoring and Reverse Factoring." *Management Science*, 67(10): 6071–6088.

Murfin, Justin, and Ken Njoroge. 2015. "The Implicit Costs of Trade Credit Borrowing by Large Firms." *Review of Financial Studies*, 28(1): 112–145.

Shou, Yongyi, Jinan Shao, and Weijiao Wang. 2021. "How does reverse factoring affect operating performance? An event study of Chinese manufacturing firms." *International Journal of Operations & Production Management*, 41(4): 289–312.

Whited, Toni M., and Guojun Wu. 2006. "Financial Constraints Risk." *Review of Financial Studies*, 19(2): 531–559.

Wuttke, David A., Eve D. Rosenzweig, and Hans Sebastian Heese. 2019. "An empirical analysis of supply chain finance adoption." *Journal of Operations Management*, 65(3): 242–261.

Online Appendix for "Supply-Chain Finance: An Empirical Evaluation of Supplier Outcomes"

Niklas Amberg, Tor Jacobson, and Yingjie Qi

A Details on the computation of the SAFE score

The measure of financial constraints on which the cross-sectional heterogeneity analysis in section 3.3.3 is based is the SAFE index, developed by Ferrando et al. (2015) on the basis of data from ECB's SAFE survey on firms' access to finance. Ferrando et al. (2015) create the SAFE index on the basis of the coefficients from a probit regression in which the dependent variable is a financial constraint indicator and the explanatory variables are financial leverage (total liabilities minus accounts payable divided by total assets), the inverse of the interest coverage ratio (interest payments over EBITDA), the profit margin (EBIT over sales), asset tangibility (fixed assets over total assets), cash holdings (cash and cash equivalents over total assets), and log total assets, along with time, industry, and country fixed effects. The financial constraint indicator is based on firms' responses in the SAFE survey. More specifically, it is equal to one for firms that report (i) having had a loan application rejected, either fully or partially, (ii) having rejected a loan offer because the cost was too high, or (iii) having abstained from applying for a loan for fear of rejection.

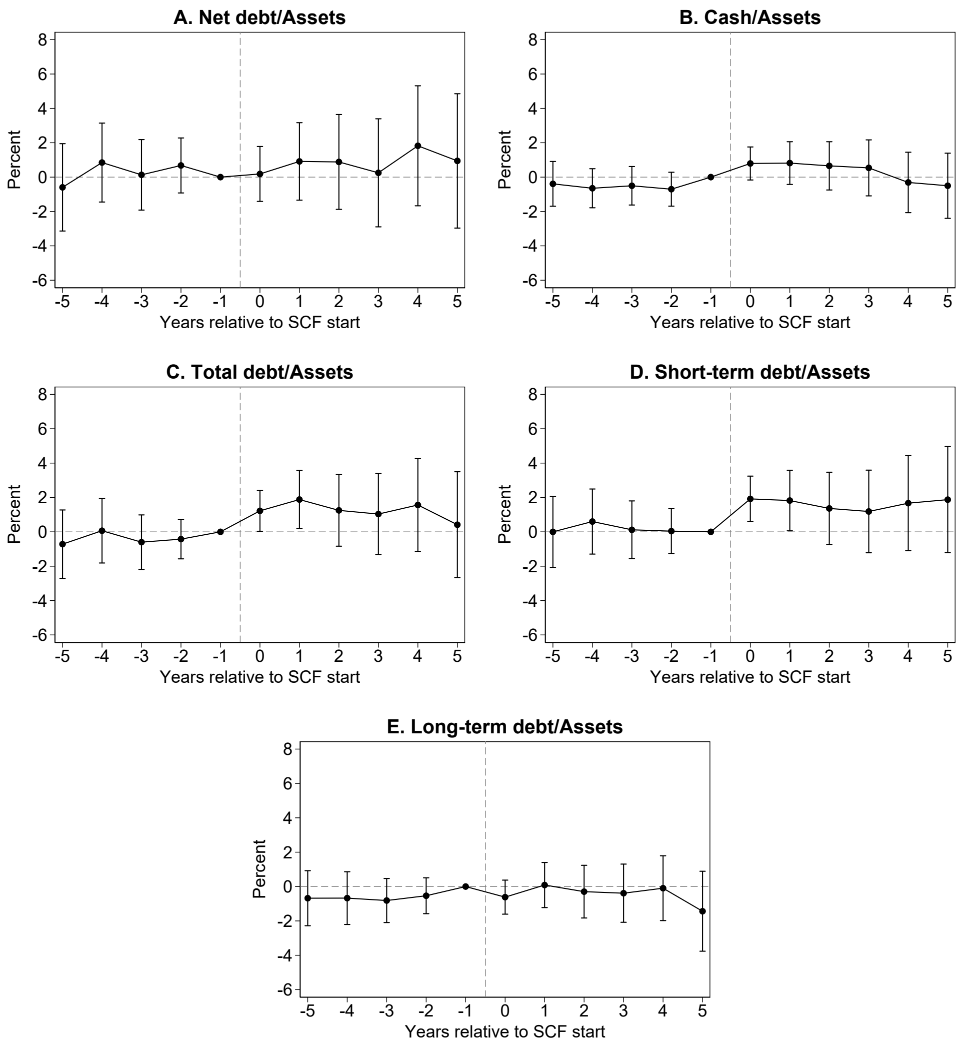
Ferrando et al. (2015) work with two different measures of financial leverage and construct one constraint index for each measure. We measure financial leverage as total liabilities minus accounts payable divided by total assets, and therefore use the following version of the SAFE index (see Table 4 in Ferrando et al., 2015):

$$\begin{aligned} SAFE_{i,t} = {} & 0.51 \cdot FinancialLeverage_{i,t} + 0.31 \cdot InverseICR_{i,t} - 0.22 \cdot ProfitMargin_{i,t} \\ & - 0.09 \cdot AssetTangibility_{i,t} - 1.26 \cdot CashHoldings_{i,t} - 0.02 \cdot \ln(Assets_{i,t}). \end{aligned} \tag{A1}$$

When computing the SAFE index for the firms in our sample, we first average all variables over the pre-treatment period (as in Aretz, Campello and Marchica, 2020) and then winsorize the averaged variables at the first and 99th percentiles. Note also that since the inverse of the interest coverage ratio is a meaningful measure only for firms making positive profits, Ferrando et al. (2015) only include firms with positive values of this ratio when estimating the probit regression that generates the coefficients for the index. Given this, we only compute the SAFE index for suppliers whose inverse interest coverage ratios were positive on average over the pre-treatment period; suppliers with negative ratios are thus excluded from the cross-sectional heterogeneity analysis.

B Additional tables and figures

Figure B1: Effects of SCF program participation on financial variables



This figure plots the β^k coefficients from the estimation of equation (2) with net debt, cash holdings, total debt, short-term debt, and long-term debt—all scaled by total assets—as dependent variables. The vertical lines correspond to 95-percent confidence intervals.

Recent Working Papers:

For a complete list of Working Papers published by Sveriges Riksbank, see www.riksbank.se

The Macroeconomic Effects of Trade Tariffs: Revisiting the Lerner Symmetry Result *by Jesper Lindé and Andrea Pescatori*	2019:363
Biased Forecasts to Affect Voting Decisions? The Brexit Case *by Davide Cipullo and André Reslow*	2019:364
The Interaction Between Fiscal and Monetary Policies: Evidence from Sweden *by Sebastian Ankargren and Hovick Shahnazarian*	2019:365
Designing a Simple Loss Function for Central Banks: Does a Dual Mandate Make Sense? *by Davide Debortoli, Jinill Kim and Jesper Lindé*	2019:366
Gains from Wage Flexibility and the Zero Lower Bound *by Roberto M. Billi and Jordi Galí*	2019:367
Fixed Wage Contracts and Monetary Non-Neutrality *by Maria Björklund, Mikael Carlsson and Oskar Nordström Skans*	2019:368
The Consequences of Uncertainty: Climate Sensitivity and Economic Sensitivity to the Climate *by John Hassler, Per Krusell and Conny Olovsson*	2019:369
Does Inflation Targeting Reduce the Dispersion of Price Setters' Inflation Expectations? *by Charlotte Paulie*	2019:370
Subsampling Sequential Monte Carlo for Static Bayesian Models *by David Gunawan, Khue-Dung Dang, Matias Quiroz, Robert Kohn and Minh-Ngoc Tran*	2019:371
Hamiltonian Monte Carlo with Energy Conserving Subsampling *by Khue-Dung Dang, Matias Quiroz, Robert Kohn, Minh-Ngoc Tran and Mattias Villani*	2019:372
Institutional Investors and Corporate Investment *by Cristina Cella*	2019:373
The Impact of Local Taxes and Public Services on Property Values *by Anna Grodecka and Isaiah Hull*	2019:374
Directed technical change as a response to natural-resource scarcity *by John Hassler, Per Krusell and Conny Olovsson*	2019:375
A Tale of Two Countries: Cash Demand in Canada and Sweden *by Walter Engert, Ben Fung and Björn Segendorf*	2019:376
Tax and spending shocks in the open economy: are the deficits twins? *by Mathias Klein and Ludger Linnemann*	2019:377
Mind the gap! Stylized dynamic facts and structural models *by Fabio Canova and Filippo Ferroni*	2019:378
Financial Buffers, Unemployment Duration and Replacement Labor Income *by Mats Levander*	2019:379
Inefficient Use of Competitors' Forecasts? *by André Reslow*	2019:380
How Much Information Do Monetary Policy Committees Disclose? Evidence from the FOMC's Minutes and Transcripts *by Mikael Apel, Marianna Blix Grimaldi and Isaiah Hull*	2019:381
Risk endogeneity at the lender/investor-of-last-resort *by Diego Caballero, André Lucas, Bernd Schwaab and Xin Zhang*	2019:382
Heterogeneity in Households' Expectations of Housing Prices – Evidence from Micro Data *by Erik Hjalmarsson and Pär Österholm*	2019:383
Big Broad Banks: How Does Cross-Selling A Affect Lending? *by Yingjie Qi*	2020:384
Unemployment Fluctuations and Nominal GDP Targeting *by Roberto Billi*	2020:385
FAQ: How do I extract the output gap? *by Fabio Canova*	2020:386

Drivers of consumer prices and exchange rates in small open economies *by Vesna Corbo and Paola Di Casola*	2020:387
TFP news, stock market booms and the business cycle: Revisiting the evidence with VEC models *by Paola Di Casola and Spyridon Sichlimiris*	2020:388
The costs of macroprudential deleveraging in a liquidity trap *by Jiaqian Chen, Daria Finocchiaro, Jesper Lindé and Karl Walentin*	2020:389
The Role of Money in Monetary Policy at the Lower Bound *by Roberto M. Billi, Ulf Söderström and Carl E. Walsh*	2020:390
MAJA: A two-region DSGE model for Sweden and its main trading partners *by Vesna Corbo and Ingvar Strid*	2020:391
The interaction between macroprudential and monetary policies: The cases of Norway and Sweden *by Jin Cao, Valeriya Dinger, Anna Grodecka-Messi, Ragnar Juelsrud and Xin Zhang*	2020:392
Withering Cash: Is Sweden ahead of the curve or just special? *by Hanna Armelius, Carl Andreas Claussen and André Reslow*	2020:393
Labor shortages and wage growth *by Erik Frohm*	2020:394
Macro Uncertainty and Unemployment Risk *by Joonseok Oh and Anna Rogantini Picco*	2020:395
Monetary Policy Surprises, Central Bank Information Shocks, and Economic Activity in a Small Open Economy *by Stefan Laséen*	2020:396
Econometric issues with Laubach and Williams' estimates of the natural rate of interest *by Daniel Buncic*	2020:397
Quantum Technology for Economists *by Isaiah Hull, Or Sattath, Eleni Diamanti and Göran Wendin*	2020:398
Modeling extreme events: time-varying extreme tail shape *by Bernd Schwaab, Xin Zhang and André Lucas*	2020:399
The Effects of Government Spending in the Eurozone *by Ricardo Duque Gabriel, Mathias Klein and Ana Sofia Pessoa*	2020:400
Narrative Fragmentation and the Business Cycle *by Christoph Bertsch, Isaiah Hull and Xin Zhang*	2021:401
The Liquidity of the Government Bond Market – What Impact Does Quantitative Easing Have? Evidence from Sweden *by Marianna Blix Grimaldi, Alberto Crosta and Dong Zhang*	2021:402
Five Facts about the Distributional Income Effects of Monetary Policy *by Niklas Amberg, Thomas Jansson, Mathias Klein and Anna Rogantini Picco*	2021:403
When domestic and foreign QE overlap: evidence from Sweden *by Paola Di Casola and Pär Stockhammar*	2021:404
Dynamic Macroeconomic Implications of Immigration *by Conny Olovsson, Karl Walentin, and Andreas Westermark*	2021:405
Revisiting the Properties of Money *by Isaiah Hull and Or Sattath*	2021:406
The cost of disinflation in a small open economy vis-à-vis a closed economy *by Oleksandr Faryna, Magnus Jonsson and Nadiia Shapovalenko*	2021:407
On the Performance of Cryptocurrency Funds *by Daniele Bianchi and Mykola Babiak*	2021:408
The low-carbon transition, climate commitments and firm credit risk *by Sante Carbone, Margherita Giuzio, Sujit Kapadia, Johannes Sebastian Krämer, Ken Nyholm and Katia Vozian*	2022:409
Seemingly Irresponsible but Welfare Improving Fiscal Policy at the Lower Bound *by Roberto M. Billi and Carl E. Walsh*	2022:410
Pension Reform and Wealth Inequality: Evidence from Denmark *by Torben M. Andersen, Joydeep Bhattacharya, Anna Grodecka-Messi and Katja Mann*	2022:411

Sveriges Riksbank
Visiting address: Brunkebergs torg 11
Mail address: se-103 37 Stockholm

Website: www.riksbank.se
Telephone: +46 8 787 00 00, Fax: +46 8 21 05 31
E-mail: registratorn@riksbank.se