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## BLENDED FINANCE AND FEMALE ENTREPRENEURSHIP

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# BLENDED FINANCE AND FEMALE ENTREPRENEURSHIP


## Abstract

We study the real and allocative consequences of relaxing gender-specific credit constraints by analyzing a blended finance program designed to expand bank lending to female entrepreneurs in Turkey. Merging credit registry data, firm-level tax records, and matched employer-employee data, we find that participating banks increase their share of credit to women by over 18%, a sustained effect driven by lending to repeat, poached, and first-time borrowers. Beneficiary firms increase investment, employment, sales, and profits, diversify their business networks, and exit less. Treated banks also expand lending to male entrepreneurs, though the increase is smaller and disproportionately directed toward higher-productivity firms, suggesting reallocation rather than crowding out. District-level effects on female entrepreneurship are absent, consistent with the program's modest scale. Our results provide well-identified evidence for mechanisms central to quantitative models of female entrepreneurship and capital misallocation.



JEL Classification: D22, G21, G32, H81, J16, L26, O16

Keywords: Blended finance



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# Blended Finance and Female Entrepreneurship*

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*July 22, 2026*

**Abstract**

We study the real and allocative consequences of relaxing gender-specific credit constraints by analyzing a blended finance program designed to expand bank lending to female entrepreneurs in Turkey. Merging credit registry data, firm-level tax records, and matched employer-employee data, we find that participating banks increase their share of credit to women by over 18%, a sustained effect driven by lending to repeat, poached, and first-time borrowers. Beneficiary firms increase investment, employment, sales, and profits, diversify their business networks, and exit less. Treated banks also expand lending to male entrepreneurs, though the increase is smaller and disproportionately directed toward higher-productivity firms, suggesting reallocation rather than crowding out. District-level effects on female entrepreneurship are absent, consistent with the program's modest scale. Our results provide well-identified evidence for mechanisms central to quantitative models of female entrepreneurship and capital misallocation.

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**Keywords:** Blended finance, credit constraints, female entrepreneurship, capital misallocation

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# 1 Introduction

In many low- and middle-income countries, women-owned enterprises are concentrated at the lower end of the firm-size distribution, in part because female entrepreneurs face greater challenges when seeking external financing (Demirgüç-Kunt, Klapper, and Singer, 2013). While recent models show that reducing such gender-specific barriers can improve aggregate productivity (Cuberes and Teignier, 2016; Morazzoni and Sy, 2022; Chiplunkar and Goldberg, 2024), empirical evidence on the real and allocative consequences of doing so remains scarce. We exploit a nationwide blended finance program in Turkey to provide such evidence.

A growing number of countries are introducing blended finance programs in which a public development finance institution (DFI) provides private banks with loans that contain a use-of-proceeds clause. Banks blend this public money with commercial funding of their own, and on-lend the combined funds to the type of borrowers specified in the use-of-proceeds clause (e.g., female entrepreneurs). Two other elements are standard: technical assistance to banks (such as for staff training) and risk sharing via a credit guarantee.[1] These programs have proliferated: we document 36 major initiatives targeting female entrepreneurship globally, collectively representing USD 22 billion in committed capital.[2] Despite this scale, empirical evidence on their effectiveness remains limited, raising concerns that scarce development finance may be wasted.

For blended finance to have sustained impact, (i) a program must successfully relax banks' lending constraints, (ii) banks must continue to serve the target segment after the program ends, and (iii) the resulting credit expansion must translate into real firm-level gains. We provide compelling evidence for this causal chain, and quantify the main channels, for a quintessential blended finance program in support of female entrepreneurs in Turkey.

The Women in Business (WiB) program was rolled out during 2015–2017 and combined DFI credit lines to five commercial banks with risk mitigation and technical assistance. It caused a sudden positive credit-supply shock to women-owned businesses, whose financial and

[1] Section 2 provides more details. See also Flammer, Giroux, and Heal (2026).

[2] Appendix Table A.1 provides a systematic overview. Key programs include the IFC's Women Entrepreneurs Opportunity Facility (USD 3 billion), the AfDB's Affirmative Finance Action for Women in Africa (USD 540 million), and the EIB's SheInvest program (USD 2 billion).

real impacts we trace by combining four administrative datasets. We start with the Turkish credit registry, which has a minimal reporting threshold, covers nearly all loans, and records borrower gender, defaults on bank credit, and obligations vis-à-vis suppliers. Using unique tax identification numbers, we merge these data with firm-level tax records (annual balance sheets and income statements), VAT data on buyer-supplier links, and matched employer-employee records with information on employees, their gender, and wages.

We address three questions. First, can blended finance durably increase bank lending to female entrepreneurs and, if so, does credit expand uniformly across the productivity distribution, as expected if pre-program exclusion reflected gender-based rationing rather than productivity-based screening? Second, which women-owned firms gain better access to credit, and does the program reallocate capital away from relatively low-productivity male entrepreneurs? Third, what are the real effects on beneficiary firms? In particular, do outcomes such as capital deepening among high-productivity female borrowers and higher female wages align with predictions from models of gender-based capital misallocation?

We start by aggregating our loan-level data to a bank-quarter panel around the staggered program entry of the five banks. We pursue two complementary identification strategies: a stacked difference-in-differences estimator (Cengiz, Dube, Lindner, and Zipperer, 2019) to address the bias that standard TWFE estimators can produce under heterogeneous treatment effects, and the synthetic difference-in-differences (SDiD) methodology of Arkhangelsky, Athey, Hirshberg, Imbens, and Wager (2021), which reweights control units and time periods to reduce reliance on parallel trend assumptions.

The selection of the five participating banks was not random but reflects idiosyncratic negotiations between the DFI and a larger set of banks. Importantly, however, our identification strategy does not require treatment status to be random. It requires that outcomes of treated and control banks would have evolved similarly absent the blended finance program (an assumption our SDiD estimator reduces reliance on through its reweighting approach). We offer three pieces of evidence to mitigate concerns on this account.

First, the program targeted banks with persistently low (but stable) female lending, implying that selection, if anything, attenuates our estimates toward zero. Second, balance tests show that although treated banks were larger than non-participating banks, the two groups were otherwise similar across pre-program characteristics. We conservatively control for these characteristics in our regressions. Third, using the Rambachan and Roth (2023) sensitivity framework, we show that our estimates remain significant under arbitrary linear deviations and substantial nonlinear violations of parallel trends.

The stacked and synthetic DiD approaches yield consistent results: the program durably increases lending to female entrepreneurs, both in absolute terms and relative to firms owned by men. Participating banks increase the portion of all business lending allocated to women by between 18.3 percent (SDiD) and 27.7 percent (stacked DiD) of the pre-program sample mean. Credit expansion to female entrepreneurs is remarkably uniform across the productivity distribution, while the simultaneous (and smaller) expansion of male lending tilts toward higher-productivity firms. These effects persist well beyond the program period, with no evidence of reversal over the 12 quarters following each bank's program entry.

We then analyze which women-owned firms benefit from the policy-induced credit expansion. We find, first, that participating banks start to lend more to their existing female clients. This accounts for 54 percent of the increase in the share of lending allocated to women. The remainder reflects lending to new borrowers, split between women poached from other lenders (28 percent) and first-time bank borrowers (18 percent). A formal spillover analysis confirms no offsetting decline in control banks' lending to female entrepreneurs. In short, the program expanded credit to repeat borrowers that were still credit-constrained (intensive margin) while also crowding in new female borrowers (extensive margin).

There is no evidence that the program-induced credit expansion came at the cost of loan quality. Non-performing loan rates and early-stage credit deterioration are unaffected across all borrower types, for both female and male entrepreneurs. Repeat borrowers at treated banks do experience a temporary increase in bounced commercial checks, but this effect is not gender-specific and it does not translate into formal loan distress. This pattern is consistent with short-run working capital pressures as expanding firms scale operations, rather than with a deterioration in the quality of banks' lending decisions.

In a next step, we start to trace the effects of the program's credit-supply shocks on women-owned firms. We first apply the same stacked DiD estimator and confirm that existing female borrowers at participating banks experienced significantly larger credit expansions after their bank's program entry, with triple-difference estimates showing these effects are differentially larger for female than male borrowers at the same bank.

To estimate real effects on repeat borrowers, we construct a firm-specific exposure to the plausibly exogenous credit shock induced by the program. A 1 percent increase in program-induced credit raises net borrowing by 0.87 percent, investment by 0.12 percent, and employment by 0.17 percent. Wage effects are concentrated among female employees (0.46 percent), driven primarily by higher wages rather than headcount expansion, with no effect on male wages. Firms also diversify their supplier and customer networks while increasing sales and profits by 0.13 and 0.93 percent, respectively. A policy-relevant 20 percent increase in WiB credit reduces exit probability by 0.56 percentage points (17 percent of the 3.3 percent mean exit rate). Not all firms benefit equally: those with initially higher capital productivity borrow disproportionately more, and their revenue product of capital converges toward the mean, consistent with capacity expansion by previously constrained firms.

Credit-supply shocks unrelated to the program also expand borrowing but generate no comparable real effects, suggesting that the program's combination of funding, risk-sharing, and technical assistance produced qualitatively different lending. Exploiting the phased *within-bank* rollout of loan officer training, we provide suggestive evidence that the program's technical assistance component was an important driver of this differential lending impact.

The results so far pertain to firms with pre-existing bank relationships. Yet 18 percent of the program-induced credit expansion went to first-time borrowers. Because they lack prior borrowing histories, we instead compare women obtaining their first-ever bank loan from a treated or control bank in the same district and quarter. First-time female borrowers at WiB banks experience greater credit access and improved real outcomes, although this extensive-margin design is more susceptible to selection than the repeat-borrower analysis.

Finally, following Greenstone, Mas, and Nguyen (2020) and Gutierrez, Jaume, and Tobal (2023), we relate district-level credit supply shocks to district-level outcomes. Districts with greater exposure to treated banks experience significant increases in aggregate credit and borrower numbers, but no changes in female firm entry, exit, or market share, consistent with a supply-side relaxation of credit constraints without a strong demand response. We do find higher male entry and exit rates, in line with competitive reallocation rather than simple crowding out. Aggregate real outcomes (sales, profits, and employment) remain unaffected for both genders, reflecting the program's modest scale relative to district economies.

**Related literature.** We contribute to three strands of the literature. First, we offer new evidence on public policies to ease small firms' access to credit. One approach improves credit markets through reforms such as strengthening creditor rights, with mixed evidence of benefits for small firms (De Haas and González-Uribe, 2025). Another uses state banks to open branches in underserved regions (Burgess and Pande, 2005; Fonseca and Matray, 2024) or expand lending to targeted firm segments (Banerjee and Duflo, 2014), though such lending is often distorted by political interference.[3] We instead study blended finance, a popular but understudied policy that channels public finance through commercial rather than state banks, and provide well-identified evidence on its effectiveness and on mechanisms central to macro models of female entrepreneurship and capital misallocation (Cuberes and Teignier, 2016; Morazzoni and Sy, 2022; Chiplunkar and Goldberg, 2024; Ranasinghe, 2024). We show that credit constraints bind not only for firms without prior borrowing histories but also for repeat borrowers, many with high capital productivity. Relaxing them increases investment, employment, and profitability and enables previously constrained high-productivity firms to expand, supporting models in which financial frictions impede the entry and efficient scaling of productive entrepreneurs (Banerjee and Moll, 2010; Buera, Kaboski, and Shin, 2011).

Second, we contribute to the literature on the real implications of bank funding shocks. While existing work focuses on *negative* shocks,[4] more recent research traces the firm-level

[3]See La Porta, Lopez-de Silanes, and Shleifer (2002); Dinç (2005); Carvalho (2014); Bircan and Saka (2021).
[4]E.g., Chava and Purnanandam (2011); Chodorow-Reich (2014); Beck, Degryse, De Haas, and Van Horen (2018).

effects of *positive* shocks from stimulus packages or credit facilities.[5] A distinctive feature of our setting is that we can compare the real effects of program-induced credit-supply shocks with those of non-program shocks at other banks: only the former generate significant firm-level gains, suggesting that the type of lending matters, not just its quantity.

Third, we provide insights into financial constraints as a barrier to female entrepreneurship. Women-owned firms in developing countries often remain small, reflecting not only restrictive gender norms (Field, Jayachandran, and Pande, 2010) and discriminatory laws (Naaraayanan, 2020) but also frictions in the financial system (Demirgüç-Kunt et al., 2013; Brock and De Haas, 2023). We show that channeling credit to female entrepreneurs through trained commercial bank loan officers can generate real impacts while maintaining loan quality. This contrasts with the muted effects of most microcredit programs, where standardized lending with minimal screening channels much credit to subsistence borrowers rather than growth-oriented firms (Banerjee, Breza, Duflo, and Kinnan, 2019).[6]

## 2 Institutional background

Launched in 2014, the Women in Business (WiB) program was a blended finance program by the European Union, EBRD, Turkish Ministry of Labor and Social Security, and Turkish employment agency İşkur. The program was developed in recognition of the large and persistent gender gap in financial access across Turkey. For example, according to the Global Findex Database (2021), Turkish men are more than twice as likely as women to borrow from a bank. While part of this gap reflects demand, Brock and De Haas (2023) show that supply-side frictions also matter. Using a lab-in-the-field experiment, they find that identical loan applications receive more stringent evaluations when submitted by women, including more demanding guarantor requirements. The blended finance program was designed to address such frictions.

[5] E.g., Paravisini (2008) and Cong, Gao, Ponticelli, and Yang (2019).

[6] Augsburg, De Haas, Harmgart, and Meghir (2015) show that simply incentivizing microcredit loan officers to take on more credit risk does not generate larger social impacts but can increase delinquency. On the limited impact of microcredit, see Angelucci, Karlan, and Zinman (2015), Attanasio, Augsburg, De Haas, Fitzsimons, and Harmgart (2015), and Banerjee, Duflo, Glennerster, and Kinnan (2015).

Like most blended finance frameworks, the program comprised three components: credit lines to banks; risk mitigation in the form of a first-loss risk cover (FLRC); and technical assistance. The first component consisted of EUR 300 million in credit lines to five banks, which committed to on-lend the funds to women-owned small firms while contributing an additional 40 percent in co-financing. A total of EUR 417 million had been disbursed to more than 12,000 women-owned small businesses by the end of 2017.

The selection of participating banks involved three steps. First, candidate banks underwent due diligence to assess financial soundness: adequate capital and liquidity, proper management, and independence from political interference. Second, from this eligible pool, the program targeted banks that had stabilized at persistently low levels of lending to female entrepreneurs. Banks already on upward trajectories in female lending had less need for the intervention and were not targeted. Third, consultants (Frankfurt School of Finance & Management) conducted baseline assessments to identify institutional barriers, such as limited liquidity for female borrowers and loan officers' reluctance to serve what they perceived as higher-risk applicants. Appendix Figure A.1 shows the district-level share of bank branches operated by WiB banks as of end-2014. Because of different negotiation dynamics, banks entered the program at different points in time and therefore started to disburse sub-loans at different times as well.

Second, the program contained a EUR 29.4 million FLRC that guaranteed up to 10 percent of each participating bank's sub-loan portfolio. The cover acted as a temporary incentive for banks to lend to an underserved borrower segment and, in doing so, to learn about women-owned firms' true risk profiles (without immediately taking on all risk themselves).

Third, the program provided technical assistance to help banks expand lending to women-owned firms. Assistance covered gender-responsive marketing, gender-disaggregated management information systems, product design (e.g., longer grace periods and flexible collateral), and loan-officer training. While all loan officers were informed about the program from the outset, training was rolled out gradually across districts because of trainer capacity constraints.

# 3 Data

## 3.1 Data sources

We merge four datasets. The first is the credit registry, which records commercial loans granted by all banks on a monthly basis, including borrower gender. The registry covers all borrowers with outstanding credit above a minimal threshold of TRY 1,000 (approximately USD 320 in 2016), effectively capturing the universe of formal lending relationships. We retain commercial loans to non-capital companies between January 2012 and June 2020.[7]

The credit registry also records bounced commercial checks. This constitutes an important signal of borrower risk as smaller firms routinely use checks to pay suppliers. A bounced check triggers a judicial fine and reputational damage, and banks observe this information at loan origination. In addition, we collect data on Stage 2 loan classifications under Basel III accounting standards. Stage 2 loans are those that have experienced a significant increase in credit risk since origination but have not yet defaulted, capturing temporary payment delays or missed installments. This provides a more granular and frequent indicator of early credit deterioration than outright default, which is rare in our sample.

The second dataset includes data on firm networks, collected by the Ministry of Treasury and Finance for the purpose of calculating VAT. These data cover all domestic firm-to-firm transactions whenever the total transaction value exceeds 5,000 Turkish liras in a given year.

Third, we access matched employer-employee data from the Turkish social security institution (SGK), part of the Ministry of Family, Labour and Social Services. These data contain detailed and complete information on firms' employees, their gender, and their wages.

Our fourth dataset consists of administrative tax records obtained from the Ministry of Treasury and Finance. It includes annual balance sheets and income statements of personal businesses reported alongside income tax filings. Because only firms exceeding certain thresholds

[7] The Turkish Commercial Code distinguishes capital and non-capital companies. A capital company is owned by multiple shareholders and is typically incorporated as a joint stock or limited liability company. In contrast, shareholders in a non-capital company face unlimited liability and are typically sole proprietors (manufacturers, storekeepers, or merchants), hence often referred to as "personal companies".

in asset size or sales must submit financial statements, some smaller credit registry borrowers cannot be matched to real outcome variables; the firm-level results therefore pertain to the subset for whom tax records are available. Tax identification numbers enable us to match entrepreneurs across the four datasets and to track them over time.

Table A.2 presents summary statistics for the women-owned firms in the merged dataset. The average firm owns assets worth 1.04 million liras (USD 350k at average 2016 exchange rates). Outstanding credit is, on average, 0.25 million liras (USD 80k) and these firms record an annual profit of about 0.18 million liras (USD 60k). The average firm has seven (eight) main business customers (suppliers), although there exists substantial variation. The average female entrepreneur employs 4.5 employees, of whom 2.8 men and 1.6 women. We calculate the average revenue product of capital (ARPK), a proxy for capital productivity (Hsieh and Klenow, 2009), as the log ratio between total sales and fixed assets. Firms at the 75th percentile of the capital productivity distribution display an ARPK 2.7 times that of firms at the 25th percentile. This suggests substantial capital misallocation among the firms we study.

We do not observe interest rates, loan maturities, or grace periods; owner characteristics beyond gender (such as age, education, or prior entrepreneurial experience); rejected loan applications; collateral type and value; or reliable firm founding dates for small firms. This means we cannot directly test whether banks shifted lending toward entrepreneurs with different observable human capital traits, nor can we analyze pricing effects or approval rates.

### 3.2 Selection into the program

Our main identification strategy, described in Section 4.1.1, exploits the staggered rollout of the blended finance program across five banks, whose lending dynamics we compare with those of 21 similar Turkish banks outside the program. An advantage of this difference-in-differences design is that identification rests on parallel trends rather than random assignment. This permits level differences across banks but requires that program entry timing was uncorrelated with bank-specific supply factors (such as pre-existing plans to expand lending to women) or with differential exposure to demand shocks from female entrepreneurs. Although entry dates are quasi-random (reflecting different negotiation dynamics and the internal checks each bank had to clear with the DFI), it remains useful to verify that treated and control banks did not differ substantially on observables before the program.

Table A.3 shows that the five treated banks are, on average, larger than control banks, reflecting their more prominent role in the market for large corporate borrowers. Despite these greater shares, treated banks' share of small-business lending is not significantly larger than that of control banks, and both groups lend to women in similar proportions within that segment. Treated and control banks are also comparable along other dimensions, including liquidity, profitability, non-performing loans (NPLs), loan-loss reserves, and capital adequacy ratios.

# 4 Identification

## 4.1 Bank-level analysis

### 4.1.1 Staggered difference-in-differences

We aggregate the loan-level credit registry data to the bank ($b$)–quarter ($t$) level and estimate:

$$y_{bt} = \alpha + \beta_1 WiB_b \times Post_{bt} + \beta_2 X_{bt-1} + \gamma_b + \delta_t + \epsilon_{bt} \tag{1}$$

where $y_{bt}$ is the flow of new loans to female entrepreneurs by bank $b$ in quarter $t$. We distinguish between lending to repeat borrowers of bank $b$; borrowers new to bank $b$ that were previously borrowing from another bank (poached clients); and borrowers new to bank $b$ that had never borrowed before (first-time borrowers). $WiB_b$ singles out the treated banks while $Post_{bt}$ equals 1 from the first quarter when a treated bank enters the program onwards. $\beta_1$ gives the average treatment effect on the treated (ATT). We saturate this model with one-period lagged time-varying bank controls $X_{bt-1}$ (log total assets, liquidity, profitability, NPLs, capital adequacy ratio, and market shares in corporate credit) on top of bank ($\gamma_b$) and quarter ($\delta_t$) fixed effects.[8]

As shown in Figure 1A, each treated bank enters the program at a different point between Q2 2015 and Q2 2017. The standard TWFE estimator returns biased estimates under staggered adoption when treatment effects vary across units or time (De Chaisemartin and d'Haultfoeuille, 2020; Goodman-Bacon, 2021). We therefore follow the stacking methodology of Gormley and Matsa (2011) and Cengiz et al. (2019). For each treated bank, we take the observations of that bank and of all never-treated banks to create a cohort.[9] We redefine quarters around the joining date as relative time indicators, $t \in [-12, 12]$, and stack the data across all five cohorts. Controls and fixed effects are interacted with cohort indicators. Standard errors are clustered by bank using a wild cluster bootstrap (Cameron, Gelbach, and Miller, 2008) with Rademacher weights and the restricted estimator (Canay, Santos, and Shaikh, 2021).[10]

Although treated banks were observationally similar to control banks before the program (except for their size), this does not guarantee they were on parallel trends. Yet Figure 1A shows that before the first bank entered the program, treated and control banks followed similar trajectories in the gender composition of their stock of small business loans. Figure 1B reinforces this pattern using an event-time framework that normalizes each treated bank's entry quarter to *t=0*. Control banks display virtually no time trend in the female lending share.

To assess this more formally, we turn to Figure 2. Panel A reports estimates from a Gardner, Thakral, Tô, and Yap (2024) two-stage difference-in-differences estimator, which residualizes outcomes on unit and time fixed effects estimated from untreated observations only.[11] This avoids the negative weighting problems of standard TWFE in staggered settings, transparently aggregating cohort-specific treatment effects. Pre-treatment coefficients center around zero,

[8]All bank controls in $X_{bt-1}$ enter one quarter lagged. Including or excluding these lagged controls, or adding them one at a time, leaves both the average estimates and the event-study dynamics very similar.

[9]Control banks are those that never participated in the blended finance program.

[10]MacKinnon, Nielsen, and Webb (2023) note that the wild cluster restricted bootstrap may under-reject when treated clusters are few, with bimodal distributions serving as a diagnostic. Our bootstrap distributions are unimodal and approximately normal across all main outcomes.

[11]Canay et al. (2021) show that asymptotic validity depends on the choice of bootstrap weights and estimation method. Because standard wild cluster bootstrap commands (e.g., `boottest` in Stata) are not compatible with two-stage DiD estimators, we report GMM-clustered standard errors for the Gardner et al. (2024) event-study specifications.

supporting parallel trends between treated and control banks. Panel B assesses the sensitivity of these estimates to violations of parallel trends through the Rambachan and Roth (2023) robust inference framework. It reports fixed-length confidence intervals under the smoothness restriction $\Delta^{SD}(M)$, where $M$ bounds the maximum change in the slope of the differential trend between consecutive periods. The estimates remain statistically significant at $M = 0$, which permits arbitrary linear violations of parallel trends, and the robust confidence sets stay bounded away from zero as $M$ increases (allowing for non-linear or accelerating differential trends), with a breakdown value of $M \approx 0.4$.

#### 4.1.2 Synthetic difference-in-differences

The evidence in the previous section supports parallel trends but cannot guarantee them, and two threats remain: bank-specific supply factors (e.g., pre-existing plans to expand SME lending) and differential exposure to demand shocks from female entrepreneurs. To reduce reliance on the parallel-trends assumption rather than only test its plausibility, we implement the synthetic difference-in-differences (SDiD) estimator of Arkhangelsky et al. (2021), adapted for staggered treatment following Ciccia (2024). SDiD assigns unit-specific weights to control banks to minimize pre-treatment imbalance with treated units (subject to a ridge penalty) and time-specific weights to pre-treatment periods to emphasize those most informative for counterfactual prediction. These weights enter a TWFE regression, making the estimator effectively local and reducing sensitivity to functional-form assumptions that standard DiD imposes.

Because our setting involves staggered treatment adoption, we implement SDiD separately for each treated bank and aggregate bank-specific ATTs following Ciccia (2024), weighting by the number of post-treatment periods observed for each bank. Standard errors are computed via block bootstrap at the bank level. Figure A.2 illustrates both sets of weights: unit weights vary across the 21 control banks (Panel A) but no single bank dominates; time weights (Panel B) assign greater weight to more recent pre-treatment quarters.

## 4.2 Firm-level analysis

### 4.2.1 Linking program exposure to firm-level credit

To link bank-level program exposure to firm-level outcomes, we first compare firms whose main pre-program bank subsequently entered the WiB program to firms whose main bank did not. For each firm, we define the 'main bank' as the lender holding the largest credit share in the baseline year 2014. We then apply the same stacking estimator as in the bank-level analysis, using the identical staggered timing variation at the firm level:

$$y_{ibt} = \alpha + \beta_1 WiB_{m(i)} \times Post_{m(i),t} + \gamma_i + \delta_{dt} + \theta_{bt} + \epsilon_{ibt} \tag{2}$$

where $y_{ibt}$ is an outcome for firm $i$ borrowing from bank $b$ in period $t$, $WiB_{m(i)}$ indicates whether firm $i$'s main bank $m$ was treated, and $Post_{m(i),t}$ equals one from that bank's program start date onwards. Firm fixed effects ($\gamma_i$) absorb time-invariant firm characteristics, while district-year and bank-year fixed effects ($\delta_{dt}$ and $\theta_{bt}$) control for common shocks.

### 4.2.2 Firm-level credit-supply shocks

The analysis above assigns each firm to a single bank. In practice, many firms borrow from multiple lenders. To exploit variation in firm-level exposure to the program, we follow Chodorow-Reich (2014), Berton, Mocetti, Presbitero, and Richiardi (2018), and Cong et al. (2019) and construct a continuous credit-supply shock based on each firm's preexisting borrowing shares across WiB and non-WiB banks:

$$\Delta \hat{L}_{idst} = \sum_{b \in B} \omega_{bi,t-1} \times \Delta \text{log} L_{b,-ds,t} \tag{3}$$

where $\omega$ is the relationship strength between firm $i$ and bank $b$ in the preceding year (i.e., the share of pre-existing borrowing by firm $i$ that comes from bank $b$) and $\Delta \text{log} L_{b,-ds,t}$ is the yearly change in (log) lending by bank $b$ to all female entrepreneurs, except those in the same district $d$ and sector $s$ as firm $i$. This identification strategy relies on two testable assumptions. First, bank–firm relationships must persist over time so that firms cannot easily switch lenders. Second, cross-sectional variation in bank lending must reflect supply forces rather than unobservable borrower traits that affect credit demand. We test both assumptions formally in Section 5 and find strong support for each.

We estimate the firm-level impact of program-induced credit-supply shocks as follows:

$$\Delta \mathrm{y}_{it} = \alpha + \beta_1 \mathrm{WiB} \times \Delta\hat{L}_{idst} + \beta_2 \text{Non-WiB} \times \Delta\hat{L}_{idst} + \gamma_i + \delta_t + \epsilon_{it} \tag{4}$$

where $\Delta \mathrm{y}_{it}$ is an outcome (over a one-, two-, or three-year horizon) and $\Delta\hat{L}_{idst}$ is the firm-level credit-supply shock from Equation (3). We differentiate between shocks emanating from WiB and non-WiB banks. $\gamma_i$ and $\delta_t$ are firm and year fixed effects, respectively.

#### 4.2.3 First-time borrower analysis

The preceding analyses are restricted to firms with pre-existing bank relationships. To examine whether the WiB program also affected first-time female borrowers (who lack prior lending histories and cannot be assigned firm-level credit-supply shocks) we estimate the following cross-sectional specification on a stacked sample:

$$y_{idz} = \beta \cdot \mathrm{WiBFT}_{idz} + \alpha_z + \alpha_d + \varepsilon_{idz} \tag{5}$$

where $i$ indexes a first-time female borrower, $d$ her district, and $z$ the quarter in which she took out her first-ever loan. The treatment indicator $\mathrm{WiBFT}_{idz}$ equals one if the borrower's first loan was issued by a WiB bank after that bank's program entry date, and zero otherwise. The control group consists of first-time female borrowers whose first loan was issued by a never-treated bank in the same post-treatment window. Following Cengiz et al. (2019), we stack the sample across the five treatment cohorts, including for each cohort the borrowers who entered the credit system within eight quarters of the treated bank's entry quarter $ft_b$, $[ft_b, ft_b + 8]$. We include entry-quarter $\times$ cohort fixed effects ($\alpha_z$) and district $\times$ cohort fixed effects ($\alpha_d$), which absorb cohort-specific variation in borrower entry conditions and time-invariant district heterogeneity, respectively. Standard errors are clustered at the bank $\times$ cohort level.

We examine credit outcomes as well as growth in sales, investment, profits, and supplier and customer networks for all firms matched to tax records. The identifying assumption is that, conditional on entering the credit market in the same district and quarter, borrowers at WiB and non-WiB banks would have had similar outcomes absent the program. Because treatment is assigned by the identity of the entry bank (a post-treatment matching outcome), the fixed effects ensure comparisons within the same local market but cannot rule out differential screening or sorting across lenders. We therefore interpret the real-effects comparisons as suggestive.

### 4.3 District-level analysis

To capture equilibrium effects beyond individual bank–firm relationships, we construct district-level credit-supply shocks following Greenstone et al. (2020) and Gutierrez et al. (2023). We regress changes in outstanding loans at the bank–district–year level on bank $\times$ year and district $\times$ year fixed effects, extract the bank $\times$ year fixed effects as bank-level credit-supply shifters (analogous to the firm-level shocks in Equation (3) but aggregated to the district level) and weight them by each bank's lagged market share to obtain a shift-share measure of annual shocks to the supply of bank credit at the district level. We relate these shocks to district-level outcomes by estimating:

$$\Delta \mathrm{y}_{dt} = \alpha + \beta_1 \mathrm{WiB} \times \Delta \hat{L}_{dt} + \beta_2 \text{Non-WiB} \times \Delta \hat{L}_{dt} + \gamma_d + \delta_t + \epsilon_{dt} \tag{6}$$

where $\Delta \mathrm{y}_{dt}$ are district-level outcomes expressed as midpoint growth rates.[12] District fixed effects ($\gamma_d$) absorb time-invariant determinants of local productivity and demand; year fixed effects ($\delta_t$) control for common annual shocks.

[12]Specifically, $\Delta \mathrm{y}_{dt} = (\mathrm{y}_{dt} - \mathrm{y}_{d,t-1})/(0.5 \times \mathrm{y}_{dt} + 0.5 \times \mathrm{y}_{d,t-1})$.

# 5 Results

## 5.1 First-stage results

### 5.1.1 Effects on lending to female entrepreneurs

We now estimate Equation (1) using the stacking (columns 1–4) and SDiD estimator (columns 5–8).[13] Table 1 considers four dependent variables: (log) new lending to female entrepreneurs (Panel A); (log) number of female borrowers (Panel B); the female share of total new lending volume (Panel C); and the female share of total new borrowers (Panel D). These estimates form the first stage of our analysis: a lending response to female entrepreneurs is a necessary condition for the firm-level real effects we examine subsequently.

The stacking estimate of 1.45 log points (column 1, Panel A) implies roughly a fourfold increase in quarterly lending flows to female entrepreneurs relative to control banks. This large effect likely reflects both the low pre-program baseline and the activation of lending along the extensive margin through first-time and newly acquired borrowers, representing a discrete shift toward a previously underserved segment. The number of female borrowers increases by 0.84 log points (column 1, Panel B), corresponding to an increase of approximately 130 percent, with a somewhat smaller estimate of 0.52 log points using SDiD. The larger increase in lending volumes relative to borrower counts implies an expansion not only in the number of female borrowers but also in average loan size per borrower.

Panels C and D translate these effects into more directly interpretable magnitudes. Treated banks increase the female share of new lending by 2.3 percentage points (column 1, Panel C), or 27.7 percent relative to the pre-program mean of 8.3 percent. The SDiD estimate is 1.5 percentage points (18.3 percent). The female share of new borrowers increases by 2.2 percentage points (22.4 percent of the pre-program mean) using the stacking estimator and 1.5 percentage

[13] We validate the SDiD estimator in two ways. First, following Abadie (2021), we backdate the program start by four quarters and verify that no effects appear before the actual intervention date (Appendix Figure A.5). Second, we conduct a leave-one-out validation, systematically excluding one pre-treatment quarter, estimating the model on remaining periods, and comparing the prediction to the held-out observation. Appendix Table A.4 reports signal-to-noise ratios (ATT divided by placebo RMSE) ranging from 3 to 7 across the five banks, confirming that the synthetic controls accurately reproduce pre-treatment trajectories.

points (15.3 percent) using SDiD (though this estimate is not statistically significant).

Returning to Figure 1A, treated banks allocate progressively more credit to female entrepreneurs once they gain access to blended finance, while the control-bank series stays flat. In event time (Figure 1B), treated banks show a persistent increase of approximately 1 percentage point, or 11 percent relative to the pre-program mean of 9 percent. Because Figure 1 tracks the female share of the outstanding loan stock (which adjusts slowly), this shift is the cumulation of the larger new-lending (flow) share effect estimated in Panel C of Table 1.[14]

Appendix Figure A.3 presents dynamic SDiD estimates of Equation (1), with the dependent variable being (log) total loan volume to female entrepreneurs. Panel A shows the ATT across all five treated banks, using the Ciccia (2024) aggregation procedure. By construction under SDiD, pre-treatment differences are approximately constant. Divergence emerges at treatment onset and grows steadily over time. Panel B shows bank-specific ATTs. All five banks show positive treatment effects, though with heterogeneity in timing and magnitude that likely reflects differences in capacity to absorb the program's training. Bank 1 responds almost immediately and displays one of the largest cumulative effects, while others adjust more gradually.

### 5.1.2 Decomposing the lending response

These aggregate effects mask important heterogeneity in how banks expanded lending to female entrepreneurs. Because we observe the universe of business borrowers over time, we can classify each bank's female clients into three types: *repeat* borrowers (who had an existing relationship with the same bank before the program), *poached* borrowers (who switched from another bank), and *first-time* borrowers (who had never borrowed from any bank).

Columns 2–4 (stacking) and 6–8 (SDiD) of Table 1 show that the program expanded lending to repeat as well as new borrowers, both in volumes (Panel A) and borrower counts (Panel B). Panel C shows that these effects also shifted portfolio gender composition across all three

[14] Applied to total small-business lending at treated banks during the program period, this corresponds to approximately TRY 1.5 billion (USD 470 million) in additional credit to female entrepreneurs. Dividing by the number of female borrowers at treated banks during the program period implies an average increase of approximately TRY 29,000 (USD 9,300) per borrower. This is roughly 12 percent of the pre-program average total credit stock of TRY 249,000 (Table A.2).

margins. Treated banks increased the female share of new lending by 17.1 percent relative to the pre-program mean for repeat borrowers (column 2), 54.2 percent for poached borrowers (column 3), and 34.8 percent for first-time borrowers (column 4). The SDiD estimates are 21.7, 20.9, and 29.3 percent, respectively (columns 6–8). Panel D shows a similar pattern for borrower counts, though the SDiD estimates for all and repeat borrowers are imprecisely estimated. The relatively large effects for poached and first-time borrowers indicate that the expansion in female lending was driven not only by deepening relationships with existing clients but also by attracting borrowers from competing banks and extending credit to previously unbanked firms.

How much of the relative increase in new lending to female entrepreneurs is driven by each borrower type? We reshape the data to the bank-quarter-gender level and estimate a triple-difference specification with bank $\times$ quarter fixed effects (absorbing time-varying but gender-neutral bank shocks) and bank $\times$ gender fixed effects (absorbing time-invariant gender-specific lending propensities). The coefficient on Post $\times$ WiB Bank $\times$ Female entrepreneur identifies the differential increase in lending to female relative to male entrepreneurs at treated banks. Appendix Table A.5 uses the quarterly change in a bank's new lending by gender and borrower type, scaled by the average total lending stock, as the dependent variable. Of the relative increase in lending to women, 54 percent (=0.037/0.068) is driven by repeat borrowers, 28 percent (=0.019/0.068) by poached clients, and 18 percent (=0.012/0.068) by first-time borrowers. Repeat borrowers account for a smaller share of the growth in the *number* of female entrepreneurs (41 percent, Panel B), with poached (30 percent) and first-time borrowers (29 percent) making up the rest.

Lastly, Appendix Figure A.4 presents SDiD event-study estimates by borrower type, complementing the aggregate dynamics in Appendix Figure A.3. Post-treatment, effects for repeat borrowers (Panel A) rise steadily and are precisely estimated. For poached borrowers (Panel B), effects turn positive shortly after program entry and remain elevated. For first-time borrowers (Panel C), effects emerge more gradually and with less precision, consistent with the longer lead times involved in originating relationships with previously unbanked entrepreneurs. Across all three panels, effects show no sign of reversal over the 12-quarter horizon.

Two further results characterize how the expansion operated. Treated banks relaxed collateral requirements for repeat and poached borrowers, but not for first-time borrowers, consistent with much of the program impact falling on the intensive margin (Appendix Table A.6). This relief accrued equally to male borrowers, which is inconsistent with the female-only credit guarantees being its source and instead points to broader training and policy changes that reduced banks' reliance on collateral. The expansion also left loan quality intact: effects on non-performing and Stage 2 loans are near zero across borrower types, and the one sign of stress (a higher incidence of defaulted (bounced) commercial checks among repeat borrowers) is not gender-specific and is more consistent with short-run working capital pressures during expansion than with misallocation (Appendix Table A.7).

#### 5.1.3 Lending across the productivity distribution

Having established that the program expanded lending along both the intensive and extensive margins, we now ask whether banks directed their incremental credit toward more or less productive entrepreneurs. We classify each bank's pre-program borrowers into quartiles based on borrower-level pre-treatment average revenue product of capital (ARPK, defined as the log ratio of sales to fixed assets) and estimate Equation (1) separately for each quartile.

Appendix Table A.8 reveals a striking asymmetry. For female entrepreneurs (columns 1–4), the program expands both lending volumes (Panel A) and borrower counts (Panel B) across all ARPK quartiles. Treated banks expanded credit to female entrepreneurs uniformly across the productivity distribution (F-tests cannot reject equality with the bottom quartile in any specification). Unlike the threshold-based selection predicted by Chiplunkar and Goldberg (2024), in which reducing entry barriers would expand female entrepreneurship primarily among marginal entrants near the productivity threshold, this broad-based pattern aligns more closely with the discrimination channel documented by Brock and De Haas (2023), in which credit rationing operates independently of firm productivity.

For male entrepreneurs (columns 5–8), the pattern is different. Treatment effects rise monotonically from the lowest to the highest ARPK quartile, and equality with the bottom quartile is rejected at conventional levels for all higher quartiles. That is, once enrolled in the program, banks become more selective in their male lending, redirecting it toward higher-productivity entrepreneurs. This productivity-based reallocation of male credit is consistent with Morazzoni and Sy (2022), where closing the gender gap in credit access induces exit among marginally less productive male entrepreneurs, shifting the remaining borrower pool toward higher productivity.

Appendix Table A.9 complements this by examining program impact on the overall ARPK distribution among a bank's new borrowers. For female borrowers (columns 1–4), the program has no significant effect on median ARPK and a marginally significant positive effect on the mean, but substantially widens the distribution: the interquartile range and the 90–10 spread both increase. This reflects compositional broadening (treated banks extend credit to women across the productivity spectrum) rather than an increase in misallocation. The triple-difference estimates (columns 5–8) confirm that, relative to male borrowers at the same bank, the female borrower pool becomes more spread out in terms of ARPK. This does not indicate that the program attracted low-quality female borrowers (the female-only results show no absolute decline in mean or median ARPK) but rather reflects the upward shift in the male borrower pool's productivity as banks became more selective with male lending. The net effect is a compositional shift in bank portfolios toward female entrepreneurs across the productivity spectrum and toward higher-productivity male borrowers, consistent with less gender-based capital misallocation (Morazzoni and Sy, 2022).

#### 5.1.4 Banks' program participation and firm-level credit

This section links the bank-level credit-supply shocks to firm-level outcomes, asking whether the lending expansion documented above reached individual firms. We exploit pre-existing bank–firm relationships: because these are highly persistent in Turkey (Appendix Table A.10), a firm's pre-program main bank determines its program exposure, letting us apply the staggered identification of Section 5.1 with firm-level dependent variables.

Table 2 presents the results. Columns 1–2 restrict the sample to female-owned firms and compare those whose main bank (defined by largest credit share in 2014) entered the WiB program to those whose main bank did not. The unit of observation is the bank–firm–year, so firms borrowing from multiple lenders contribute multiple observations. The fixed effects in columns 1–2 absorb lending-bank shocks, while treatment is defined by the firm's main bank in 2014; identification therefore comes from comparing firms within the same lending bank whose main banks differ in WiB status. Female firms connected to a WiB bank received 5.3 percent more credit following their bank's program entry (column 1). This estimate is robust to the inclusion of bank × district × year fixed effects, which absorb all time-varying local demand conditions common to borrowers of the same bank in the same district (column 2, 5.6 percent). Columns 3–4 expand the sample to include male borrowers and estimate a triple-difference specification. The coefficient on WiB Bank × Post × Female identifies the differential effect on female relative to male entrepreneurs at the same bank. Female borrowers at WiB banks experience a significantly larger credit expansion, confirming that the program shifted the gender composition of lending at the firm level, not just in bank-level aggregates.

## 5.2 Second-stage results: Real effects

### 5.2.1 Firm-level credit-supply shocks and real outcomes

We trace program-induced credit-supply shocks to firm outcomes using the Chodorow-Reich (2014) approach of Section 4.2.2, which exploits variation in each firm's pre-existing exposure to WiB and non-WiB banks. Two conditions support it. First, pre-existing relationships strongly predict where firms borrow: Appendix Table A.10 shows a 98 percent probability of borrowing from a prior lender (column 1), with coefficients between 0.90 and 0.99 across progressively demanding fixed-effects specifications.[15] Second, Appendix Table A.11 confirms that cross-sectional variation in bank lending reflects supply rather than demand: point estimates are stable with firm fixed effects (Khwaja and Mian, 2005) and, restricting to multi-lender firms with firm × year fixed effects (columns 3–4), fully absorbing firm-specific demand shocks.

[15] The dataset in Appendix Table A.10 is a firm–bank–year panel where each firm forms a potential pair with each bank. Because each firm is a *potential* borrower of each bank, no observations are dropped when including firm or bank fixed effects.

Having established that both assumptions hold, we now turn to the firm-level impact estimates. Column 1 of Table 3 confirms that bank-specific credit-supply shocks translate into more borrowing by female borrowers. A 1 percent increase in the credit supply from prior lenders results in 0.68 percent more borrowing. In column 2, we differentiate between credit-supply shocks stemming from program and non-program banks. Credit-supply shocks coming from banks participating in the blended finance program translate more fully into additional borrowing at the firm level (an elasticity of 0.87) when compared with shocks coming from banks outside of the program (0.62). An F-test at the bottom of column 2 shows this difference is statistically significant. The higher transmission of WiB-induced credit shocks suggests that the program helped banks lend more to prior borrowers that were still credit constrained.

Column 3 provides more direct evidence by interacting the program and non-program credit-supply shocks with an indicator for whether the firm's pre-program ARPK (the log ratio of total sales to fixed assets) was above the median. This interaction is statistically significant only for WiB banks. A 1 percent increase in WiB credit supply translates into 0.73 percent higher borrowing for below-median ARPK firms, and into 1.01 percent ($= 0.727 + 0.286$) higher borrowing for above-median ARPK firms. The program thus steered banks toward more intensive-margin lending to female-owned firms with higher capital productivity.

Table 4 provides similar regressions but instead focuses on real outcomes. We thus assess whether the increased use of credit by women-owned firms that benefited from their lenders' participation in the blended finance program, resulted in positive real outcomes. A first interesting observation is that the non-WiB credit shocks tend not to have significant impacts at the firm level. To be clear: these credit-supply shocks *did* expand firm borrowing, as per Table 3. Yet, this increased borrowing did not translate into meaningful real impacts.

In contrast, we find a coherent pattern of firm-level real impacts originating from the program-related credit-supply shocks.[16] We find that a 1 percent larger firm-specific credit-supply shock due to the blended finance program results, within a year, in a 0.12 percent

[16] F-tests at the bottom of the table show that the impacts of WiB versus non-WiB credit-supply shocks are statistically different at least at the 10 percent level, except for the difference in exit rates (column 6).

increase in investment (column 1); a 0.13 percent increase in sales (column 4); a 0.93 percent increase in profitability (column 5); and a reduction in exit probability (column 6): a 10 percent credit-supply shock lowers exit by 0.27 percentage points, or 8 percent of the mean. The number of unique customers and suppliers increases as well (columns 7 and 8). We find no impact, on average, on firms' capital productivity (column 2) or cost of goods sold (COGS, column 3). Overall, women-owned firms use the additional program-related borrowing to invest and sell more, generate higher profits, and reduce exit risk.

Next, Appendix Table A.12 exploits the full VAT transaction matrix to examine whether credit-supply shocks reshaped female firms' commercial relationships. On the supplier side (Panel A), WiB shocks raise total transaction value but leave both concentration measures unchanged, so input sourcing expands proportionally across existing sectors and partners. On the customer side (Panel B), treated firms similarly expand transaction values, but both the industry and partner HHIs decline significantly: firms diversify sales across sectors and trading partners. Credit-enabled growth thus reshapes downstream relationships (entering new industries, reaching new customers) while preserving upstream input sourcing. Treated firms also become more central on both sides, more precisely so on the customer side (column 4, Panel B), and their customers employ a higher female share (column 5, Panel B), with no supplier-side counterpart. None of these effects appear for non-WiB shocks.

Table 5 investigates the impact of program-induced credit shocks on firm employment. A 1 percent larger firm-specific credit-supply shock due to the blended finance program resulted in a 0.17 percent increase in firm-level employment (column 1). The elasticity of male employment with regard to credit shocks is larger (at 0.12 percent) than for female employment (0.06 percent), as can be gleaned from columns 2 and 3. While non-program related credit shocks also translate into positive employment effects, these impacts are consistently smaller, as indicated by the F-statistics at the bottom of Table 5. Lastly, we observe an increase in the firm's total monthly wage bill of 0.13 percent, which is entirely due to a 0.46 percent increase in wages paid to female employees in response to the program-related credit shocks (column 6).

Appendix Figure A.6 reports WiB$\times\Delta\hat{L}_{idst}$ estimates from similar regressions, split into real effects in the first, second, and third years after a firm's lender(s) gain program access. The dynamics are intuitive. Investment and employment rise immediately in years one and two; the investment effect then fades by year three while employment stays positive. Sales and profits rise in year one, but sales continue increasing while the profit effect is short-lived. Survival probability rises with some delay. Appendix Table A.14 confirms these patterns: investment effects persist into year two but dissipate by year three, consistent with a transition to a higher capital stock; sales and supplier diversification persist and strengthen over the horizon; employment remains marginally significant. Profit and female-wage-bill effects weaken over time.

The attenuation of some firm-level effects over time can be understood through both partial and general equilibrium channels. Through the partial equilibrium lens, the pattern is consistent with standard models of firm investment under credit constraints. Once a firm gains access to additional credit, investment rises sharply as entrepreneurs act on previously unfunded projects. The investment effect remains elevated in year two but moderates in year three, consistent with a transition to a new steady state: firms that were previously underinvested close the gap with their optimal capital stock, after which the flow of new investment naturally returns toward baseline levels even though the capital stock remains, in principle, permanently higher.

Through the general equilibrium lens, several mechanisms may compress profitability over time. Table 5 shows that WiB-induced credit-supply shocks increase wages paid to female employees by 0.46 percent, with no significant effect on male wages. This concentration of wage effects among women is consistent with the equilibrium labor market adjustments predicted by Chiplunkar and Goldberg (2024), in which expanding female entrepreneurship increases demand for female labor, bidding up female wages (the 0.46 percent increase in the female wage bill primarily reflects higher per-worker compensation, as female employment rises by only 0.064 percent). While the compositional hiring prediction of their model (disproportionate hiring of female workers by female entrepreneurs) is not directly supported in our data, the price-side consequence is. Rising labor costs, combined with the modest positive competitive spillovers documented in Section 5.3, help explain the attenuation of profit effects over time.

Table 3 showed the program steered credit disproportionately toward above-median ARPK firms; Appendix Table A.15 asks whether this translated into heterogeneous real effects. The investment interaction with initial ARPK is positive but imprecise (column 1), so we cannot conclude that high-ARPK firms invest differentially more. However, column 2 shows ARPK converges significantly for these firms. Combined with the results in Table 3, this convergence indicates that above-median ARPK firms expanded productive capacity (through fixed capital, working capital, or channels not fully captured by the investment variable). The decline is consistent with previously constrained high-productivity firms expanding their capital stock, in line with the mechanism in Ranasinghe (2024). But because ARPK is log sales over fixed assets, it is also consistent with a mechanical transition in which fixed-asset expansion temporarily depresses the ratio absent efficiency changes. We therefore read the pattern as capacity expansion by constrained firms rather than a direct measure of allocative efficiency gains.[17]

Columns 4 and 5 show that sales and profit gains were smaller for high-ARPK firms. These are relative attenuations, not absolute declines: baseline coefficients indicate significant increases in both, with almost exactly offsetting interaction terms, so net effects for above-median firms are near zero. Lower-ARPK firms may have had more underutilized capacity, translating credit into sales more immediately; the event-study timing (Appendix Figure A.6) supports this, with investment effects strongest in years 1–2 while sales rise over the three-year horizon. Employment effects are not meaningfully moderated by initial ARPK (column 9).

#### 5.2.2 Mechanisms: The role of loan officer training

As discussed in Section 2, the program comprised three components: credit lines to banks, risk mitigation through a first-loss risk cover (FLRC), and technical assistance via loan officer training. Cleanly disentangling their individual contributions is difficult given the integrated design, but three observations provide suggestive evidence on their relative importance.

First, treatment effects persist over a three-year horizon, well beyond the subsidy period.

[17] Appendix Table A.13 estimates Equation 4 separately for firms with initial sales below and above the median. Investment effects concentrate among above-median firms and profit effects are larger (though not significantly so) among below-median firms, while employment effects are positive, significant, and comparable across both groups. This suggests the program relaxed different binding constraints along the size distribution: smaller firms eased working capital pressures and margins, larger firms funded fixed capital formation.

Even after the credit lines were repaid, treated banks continued lending more to female entrepreneurs than before the program and relative to synthetic controls, with no reversion to pre-program levels. This suggests that bank liquidity was not the primary driver, though it does not fully rule it out: if initial loans were profitable and well-repaid, banks could have generated internal funds and informational capital that sustained lending.

Second, both female and male repeat borrowers at treated banks benefited from reduced collateral requirements. If the FLRC, which applied *only* to female borrowers, were the key driver, we would expect stronger gender differences in uncollateralized lending. Their absence suggests the FLRC was not the main mechanism.

Third, and most directly, we leverage the district-by-district roll-out of loan officer training at three of the five treated banks. All officers learned of the program from inception through launch events and intranet pages; what varied across districts was the timing of intensive classroom training, which capacity constraints prevented from being simultaneous. The curriculum covered gender-informed credit assessment, relationship management, and marketing, applied through role-play and case studies.

In Appendix Table A.16, a TWFE specification compares the gender composition of lending by officers in already-trained districts against officers of the same bank in not-yet-trained and never-trained districts, with bank $\times$ district $\times$ quarter and bank $\times$ gender $\times$ quarter fixed effects. The triple-interaction coefficient identifies how female entrepreneurs are affected differentially from male entrepreneurs once a district's officers are trained, conditional on program participation and net of bank-wide effects. In trained districts, officers shifted lending toward female entrepreneurs, particularly for poached and first-time borrowers (Panel B); volume effects are less precise, except for poached borrowers (Panel A). Because all officers knew of the program's liquidity and guarantee support from day one, the staggered schedule identifies the incremental effect of skills-based technical assistance above those components. This pattern is more consistent with a relaxation of supply-side barriers, including the implicit biases documented by Brock and De Haas (2023) in the same context, than with demand-driven changes, providing suggestive evidence that training was an important driver of program impacts.

### 5.2.3 Quantifying the impact of the program

We now present instrumental variables (IV) estimates to further quantify how access to credit from WiB and non-WiB banks affects firm outcomes. The first stage regresses the change in (log) firm-level credit on the credit-supply shocks estimated earlier, separating female entrepreneurs' WiB and non-WiB borrowing. It is analogous to column 2 of Table 3, but with two endogenous variables (change in WiB and non-WiB credit) instrumented by the two corresponding credit-supply shocks. The second stage relates predicted credit changes to outcomes.

Appendix Tables A.17 and A.18 report the average effect of a Turkish lira in credit on investment, employment, wages, sales, profit, exit, and suppliers.[18] The average stock of bank credit for female entrepreneurs is TRY 250,000 (roughly USD 80,000 in 2016). Using column 1 of Appendix Table A.17, an increase of TRY 45,802 (roughly USD 16,000) in WiB borrowing corresponds to a TRY 4,489 rise in gross fixed assets, a TRY 31,320 rise in sales (column 4), and a TRY 37,248 rise in profits (column 5). The average firm also employs 0.2 more workers (Appendix Table A.18, column 1), paying TRY 249 more in monthly wages (column 4).

Since profits are net of interest, a lira of extra WiB lending raises the average firm's profits by 81.3 percent (=37,248/45,802). By contrast, additional lending by non-WiB banks is not associated with meaningful changes in investment, sales, or profits, and its employment effect is less than half that of WiB banks. WiB banks thus appear to have identified credit-constrained female entrepreneurs who used the loans to improve profitability; an increase of TRY 50,000 in borrowing is associated with a 0.56 percentage-point decline in exit probability (column 6).

### 5.2.4 First-time borrowers

The results so far concern female firms with prior bank links. Yet 18 percent of the program-induced credit expansion went to first-time borrowers, who lack the prior relationships the Chodorow-Reich (2014) approach requires to assign firm-specific shocks. We instead adopt a cohort-based cross-sectional design exploiting the timing of first entry into the credit registry.

Among women obtaining their first-ever bank loan in the same district and quarter, those

[18] As with the reduced-form estimates, the IV estimates reveal no significant effects for the remaining outcomes.

borrowing from a WiB bank received a program-induced credit-supply shock while those borrowing from a control bank did not. Following Cengiz et al. (2019), we estimate Equation (5) on a stacked sample: for each of the five treatment cohorts, all first-time female borrowers whose entry bank is the treated bank or a never-treated bank and who entered in the eight quarters following that bank's program entry. Treatment equals one if the first loan was issued by a WiB bank after its program entry. We include entry-quarter $\times$ cohort and district $\times$ cohort fixed effects, absorbing cohort-specific time variation in entry conditions and time-invariant district heterogeneity. Identification is from cross-bank variation: within a district and entry quarter, it compares subsequent outcomes of first-time borrowers at WiB versus non-WiB banks. Standard errors are clustered at the entry-bank $\times$ cohort level. Because treatment is assigned by entry-bank identity, the design conditions on a post-treatment matching outcome; the fixed effects place treated and control borrowers in the same local credit market at the same time but cannot rule out differential screening or sorting across lenders.

Appendix Table A.19 presents results for first-time borrowers matched to tax records. Over the two-year horizon, those at WiB banks obtain more loans and total credit than those at control banks (columns 1–2), confirming that the program expanded access for women entering the banking system for the first time. They also exhibit higher investment, sales, and profits (columns 3–10); as treatment is defined by the entry bank, we read these as suggestive rather than causal. The magnitudes are meaningful: the investment effect is 25 percent relative to the control mean. A positive effect on cost of goods sold alongside the sales expansion indicates that first-time WiB borrowers scaled up operations rather than improving margins on existing activity. They also expand commercial networks, with a rise in customers (column 9) and a marginally significant rise in suppliers (column 10), mirroring the repeat-borrower patterns.

We find no effect on ARPK (column 4) or exit (column 8). The ARPK null is consistent with first-time borrowers using credit to expand capacity (raising capital and sales proportionally) rather than the ARPK convergence documented for high-ARPK repeat clients in Appendix Table A.15. Appendix Table A.7 confirms the expansion did not erode loan quality: non-performing rates and early-stage deterioration are unaffected for first-time borrowers at treated banks. In sum: the favorable firm-level patterns for repeat borrowers are also present among first-time borrowers, though the extensive-margin design warrants more cautious interpretation.

### 5.3 Spillover analysis

A substantial share of the credit expansion reflects borrowers poached from other banks (28 percent of the increase in new lending to female entrepreneurs). And while each treated bank holds a small share of the national market, they account for 13 to 60 percent of bank branches in the districts where they operate (Appendix Figure A.1). This raises two concerns. First, if the program displaced female lending at control banks, comparing treated to control banks would overstate its true impact, violating SUTVA. Second, for first-time borrowers, the program may not have expanded the pool of female borrowers but merely diverted women who would otherwise have borrowed from a control bank. We address both using the framework of Berg, Reisinger, and Streitz (2021), adapted to our bank–district setting.

The approach exploits cross-district variation in the pre-program market share of treated banks, using the log change in new loan issuance to female entrepreneurs (2014–2019) as the outcome. The key variables are a treatment indicator for WiB banks; its interaction with the leave-one-out market share of other treated banks in the same district (capturing within-treatment-group competition); and the corresponding interaction for non-treated banks (capturing competitive spillovers to control banks). The coefficient on the latter directly tests whether control banks operating alongside treated banks alter their lending behavior in response to program-induced competition. Under a displacement story, this coefficient should be negative.

Appendix Table A.20 presents the results. Columns 1–3 report baseline estimates without spillovers, progressively adding controls for population density and rural share. Columns 4–7 introduce the full spillover specification for all lending (column 4) and for repeat (column 5), poached (column 6), and first-time borrowers (column 7). The direct treatment effect remains large and precisely estimated when allowing for within-district spillovers, indicating that our results are not driven by displacement. Second, for poached borrowers (column 6), we find marginally significant *positive* spillovers to control banks in high-WiB-penetration districts, consistent with a competitive response: non-WiB banks intensify lending to creditworthy female entrepreneurs when facing subsidized competitors. Third, for repeat and first-time borrowers (columns 5 and 7), spillovers are small and statistically insignificant. Appendix Figure A.7 corroborates these patterns. Panel A shows that treated banks consistently exhibit higher lending growth than control banks across the full distribution of district-level WiB market shares, with only modest increases in lending by control banks at high exposure levels. Panel B shows a more pronounced positive relationship for control banks in the case of poached borrowers, consistent with marginally significant positive spillovers. Even at high levels of WiB penetration, treated banks' lending growth remains substantially above that of control banks.

We also directly test for substitution among first-time borrowers. Under substitution, the program diverts women who would have borrowed from a control bank, implying that non-treated banks in high-WiB-penetration districts should lose first-time female borrowers. Appendix Table A.21 uses the same bank-district spillover specification as Appendix Table A.20, with the log change in first-time female borrowers at the bank-district level as the dependent variable. WiB banks experienced roughly 11 to 13 log points higher growth in first-time female borrowers than non-WiB banks (columns 1–3), an effect stable across specifications conditioning on population density and rural share. The full spillover model (column 4) supports the net-entry interpretation on two counts: the coefficient on (1 - WiB) x Lending share is small (-0.100) and statistically insignificant, so non-WiB banks in higher-WiB-presence districts did not lose first-time female borrowers; and the direct WiB effect remains large and precise (0.133) even after allowing for within-district spillovers. The program thus expanded the pool of women entering formal credit markets rather than reallocating first-time borrowers across lenders.

### 5.4 District-level results

The preceding sections documented substantial firm-level effects on credit access, investment, sales, and productivity. A natural question is whether these gains aggregate to the district level or are offset by competitive reallocation across firms within local markets. Following Greenstone et al. (2020) and Gutierrez et al. (2023), we relate district-level credit-supply shocks to district-level outcomes, estimating Equation (6). This also speaks to demand-side forces: if the program spurred new business formation through heightened awareness, we would expect extensive-margin entry responses in more exposed districts.

Table 6 presents the results. Districts with greater exposure to WiB banks experienced significant increases in both total credit (column 1) and the number of borrowers (column 2), both significant at the 1 percent level. These effects are roughly twice as large as those associated with non-WiB credit-supply shocks, suggesting that the program generated meaningful aggregate credit expansion at the district level.

The remaining columns examine firm entry and exit, market structure, and aggregate outcomes. We find no significant effect on female entry rates (column 3), exit rates (column 5), or market share (column 7). Had the program induced substantial extensive-margin responses, more exposed districts should show higher female entry or rising female market share. Their absence suggests the program primarily affected credit access for the existing stock of female entrepreneurs rather than stimulating new business creation, consistent with a supply-side relaxation of credit constraints rather than demand-side responses.

We do, however, find significant positive effects on both male entry (column 4) and exit rates (column 6), with F-tests rejecting equality of WiB and non-WiB coefficients for both margins. This simultaneous entry and exit indicates competitive reallocation rather than crowding out: treated banks became more selective in male lending, with treatment effects on male credit rising monotonically across ARPK quartiles (Appendix Tables A.8 and A.9). The elevated exit rate is consistent with lower-productivity males facing increased competition as credit allocation becomes more efficient. This matches the competitive displacement in Chiplunkar and Goldberg (2024), where removing barriers facing female entrepreneurs displaces low-productivity males who operated only because their competitors faced higher entry and hiring frictions, and Morazzoni and Sy (2022), where closing the gender gap induces exit among marginally less productive male entrepreneurs. The concurrent rise in male entry points to heightened turnover among marginal entrepreneurs as banks expand overall SME lending.

The final columns examine aggregate real outcomes. We find no statistically significant effects on district-level sales, profits, or employment for either female- or male-owned firms (columns 8–13). This null result persists across one-, two-, and three-year horizons (Appendix Figure A.8), ruling out delayed adjustment as an explanation.

Several considerations reconcile the firm-level effects with these null aggregate results. The most important is scale: the program disbursed EUR 417 million to roughly 12,000 women-owned businesses, a small fraction of the roughly 260,000 formally registered female-owned firms in Turkey. While district-level exposure significantly raised aggregate lending to women (column 1), the intervention was too modest relative to local economies to shift district-wide aggregates. This aligns with calibrated magnitudes in the macro literature. Cuberes and Teignier (2016) show income losses from gender gaps are proportional to those gaps, so partial removal yields modest aggregate effects; Chiplunkar and Goldberg (2024) find that removing only a subset of barriers (entry costs without hiring frictions) yields far smaller gains than comprehensive reform; and Morazzoni and Sy (2022) show that fully closing the gender gap in credit access raises aggregate output by up to 4 percent, implying correspondingly smaller gains from a partial intervention.

Reallocation likely attenuates aggregate impacts further. Roughly 28 percent of the lending increase reflects poached borrowers, so some firm-level gains may come at the expense of competing firms, consistent with the "business-stealing" effects in Cai and Szeidl (2024). High-ARPK firms benefit disproportionately and experience ARPK convergence, consistent with capacity expansion by previously constrained firms; such expansion operates through firm-level scaling rather than aggregate quantity growth.

In sum, the program generated meaningful firm-level improvements, including capacity expansion among previously constrained high-productivity firms, but was too small and targeted to produce detectable aggregate effects. In most districts, the vast majority of women-owned firms remained excluded from bank credit altogether.

# 6 Conclusions

Blended finance programs have scaled up rapidly to make credit more accessible to women-owned firms and other target segments, yet rigorous evidence on their impact is scarce. We provide such evidence for a program in Turkey, leveraging credit registry data, firm-level tax records, VAT transaction data, and matched employer-employee records. We find that policy-induced credit expansion can increase lending to female entrepreneurs. Treatment effects show no sign of reversal, and a spillover analysis confirms the program did not displace female lending at control banks. The average effect on the share of lending to female entrepreneurs exceeded 18 percent, as banks lent more to repeat borrowers, poached clients from competitors, and crowded in first-time borrowers.

A distinctive feature of our setting is that we can compare the real effects of program-induced credit-supply shocks with those of non-program shocks at other banks. Both expand borrowing, but only WiB-induced shocks generate significant gains in investment, sales, profits, employment, and network diversification. This indicates that the type of lending matters, not just its volume: the program's combination of funding, risk-sharing, and technical assistance produced qualitatively different credit.

Our findings speak to the quantitative macro literature on gender-based distortions. The uniform credit expansion across the productivity distribution points to credit frictions not strongly selective on observable productivity (Morazzoni and Sy, 2022), differing from the threshold-based selection in Chiplunkar and Goldberg (2024); the reallocation of male lending toward higher-productivity firms, however, is consistent with both. ARPK convergence among initially high-ARPK treated female firms is consistent with capacity expansion by previously constrained firms (Ranasinghe, 2024), though a mechanical contribution from investment lags cannot be ruled out. That these firm-level gains do not translate into district-level effects is consistent with calibrated magnitudes in these models: partial constraint relaxation at limited scale generates micro-level improvements without macro-level quantity changes.

Some limitations deserve mention. We do not observe loan applications, so we cannot measure gender differences in application or approval rates, nor interest rates or maturities, which limits our assessment of the full cost of credit. We also cannot track transitions from entrepreneurship to wage work, needed to fully assess the occupational selection channel emphasized by Chiplunkar and Goldberg (2024); the absence of district-level declines in the total number of male or female entrepreneurs suggests this channel is not quantitatively large.

Blended finance programs bundle liquidity support, training, and risk sharing, and disentangling their relative importance is an important area for future research. Our within-bank loan officer training analysis suggests technical assistance was a key driver, but a more conclusive decomposition would help fine-tune program design. Because banks relaxed collateral requirements for repeat but not first-time borrowers (explaining why much of the impact fell on the intensive margin) a temporary, higher first-loss risk cover may be needed to expand lending to first-time female borrowers. More broadly, performance-based incentives that condition interest discounts on DFI loans on portfolio goals (such as a higher share of female borrowers among first-time clients) may shift bank credit toward underserved segments profitably and durably.

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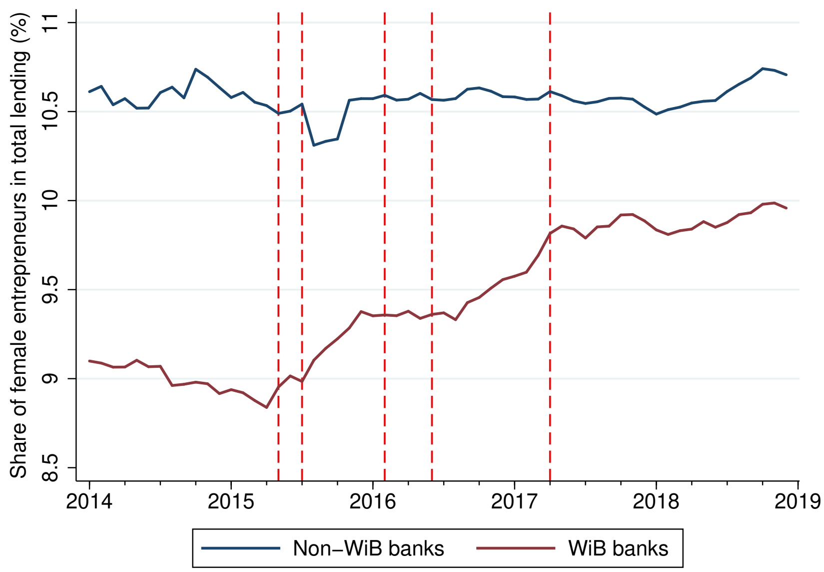
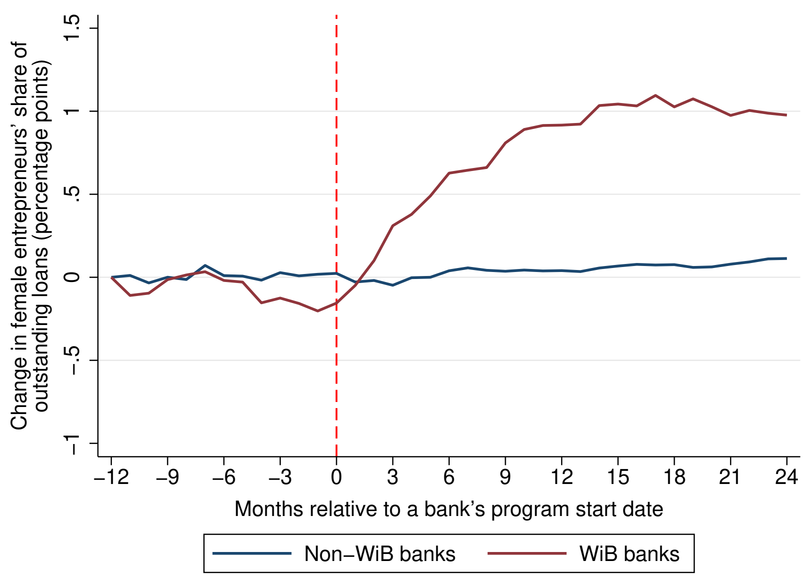
**Figure 1: The WiB program and lending to female entrepreneurs**

Panel A: Female share of outstanding loans by staggered treatment status



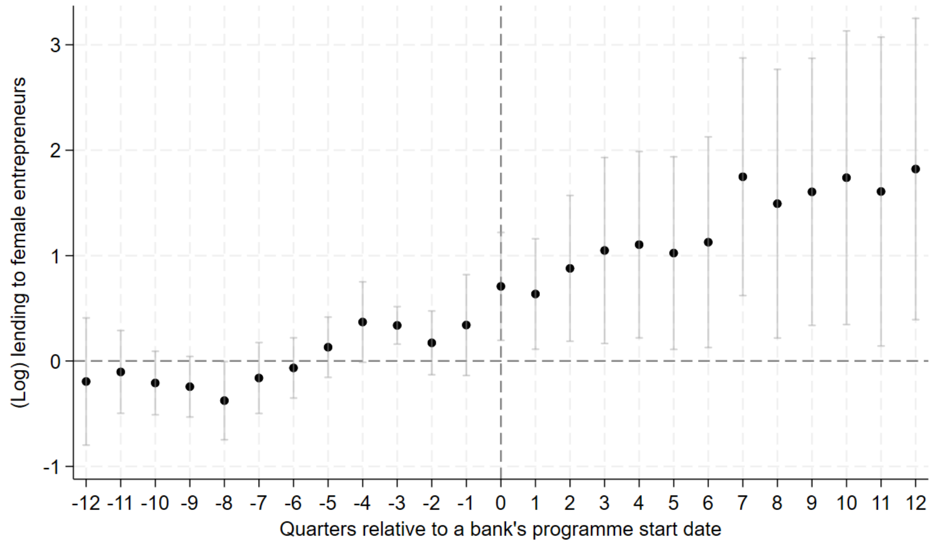

Panel B: Change in female lending share relative to program entry



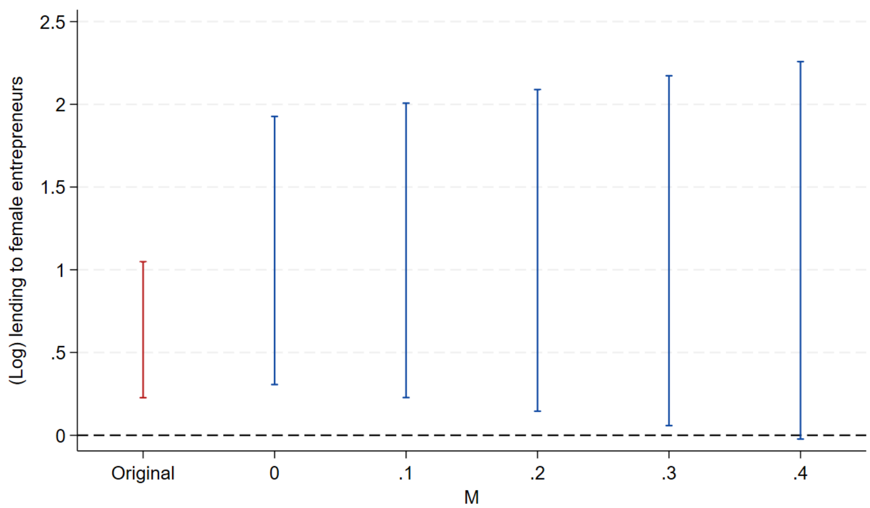

Notes: Panel A shows the share of outstanding loan stock allocated to female entrepreneurs for WiB banks (red) and non-WiB banks (blue). Vertical dashed lines indicate when each treated bank disbursed its first WiB loan (May 2015, July 2015, February 2016, June 2016, April 2017). Panel B shows the average change in the share (in % points) of outstanding loan stock allocated to female entrepreneurs around program entry, with *t=0* set to each WiB bank's first program disbursement. Non-WiB banks are assigned event time based on the corresponding WiB bank's entry date in each cohort. Both series are normalized to zero at *t=-12*.

**Figure 2: Event-study estimates and parallel trends sensitivity**

Panel A: Event-study estimates




Panel B: Sensitivity to parallel trends violations




Notes: Panel A presents event-study estimates using the Gardner et al. (2024) two-stage difference-in-differences estimator. The dependent variable is (log) lending to female entrepreneurs. Vertical bars show 95% confidence intervals based on standard errors clustered at the bank level. Panel B reports sensitivity analysis following Rambachan and Roth (2023), constructing robust confidence intervals that allow for violations of parallel trends. The parameter $\bar{M}$ bounds the maximum non-linearity in differential trends between treated and control banks; $\bar{M} = 0$ assumes linear extrapolation of pre-trends, higher values allow for departures from linearity.

**Table 1: Bank-level program effects on lending to female entrepreneurs**

| | Stacked estimator | | | | SDiD estimator | | | |
|---|---|---|---|---|---|---|---|---|
| | All | Repeat | Poached | First-time | All | Repeat | Poached | First-time |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **A. Lending to female entrepreneurs** | | | | | | | | |
| Post x WiB Bank | 1.450*** | 1.269*** | 1.330*** | 1.096*** | 1.471*** | 1.401*** | 0.981*** | 0.708** |
| | (0.317) | (0.333) | (0.298) | (0.247) | (0.425) | (0.455) | (0.313) | (0.295) |
| R-squared | 0.870 | 0.875 | 0.868 | 0.903 | - | - | - | - |
| Observations | 2,750 | 2,750 | 2,750 | 2,750 | 858 | 858 | 858 | 858 |
| Mean dep. var. | 8.207 | 7.538 | 6.198 | 5.907 | 8.589 | 7.964 | 6.683 | 6.379 |
| **B. Number of female entrepreneurs** | | | | | | | | |
| Post x WiB Bank | 0.843*** | 0.765*** | 0.620*** | 0.480*** | 0.518*** | 0.526*** | 0.321*** | 0.187 |
| | (0.165) | (0.197) | (0.114) | (0.137) | (0.167) | (0.175) | (0.124) | (0.196) |
| R-squared | 0.953 | 0.952 | 0.934 | 0.943 | - | - | - | - |
| Observations | 2,750 | 2,750 | 2,750 | 2,750 | 858 | 858 | 858 | 858 |
| Mean dep. var. | 4.600 | 4.150 | 3.087 | 3.078 | 4.956 | 4.512 | 3.426 | 3.394 |
| **C. Share of female lending** | | | | | | | | |
| Post x WiB Bank | 0.023*** | 0.012* | 0.045*** | 0.048*** | 0.015*** | 0.015** | 0.018* | 0.041*** |
| | (0.004) | (0.007) | (0.011) | (0.009) | (0.005) | (0.006) | (0.010) | (0.013) |
| R-squared | 0.288 | 0.183 | 0.218 | 0.262 | - | - | - | - |
| Observations | 2,750 | 2,750 | 2,750 | 2,750 | 858 | 858 | 858 | 858 |
| Mean dep. var. | 0.083 | 0.070 | 0.083 | 0.138 | 0.082 | 0.069 | 0.086 | 0.140 |
| **D. Share of female entrepreneurs** | | | | | | | | |
| Post x WiB Bank | 0.022*** | 0.012* | 0.038*** | 0.049*** | 0.015 | 0.010 | 0.019* | 0.048*** |
| | (0.006) | (0.007) | (0.013) | (0.010) | (0.010) | (0.009) | (0.011) | (0.012) |
| R-squared | 0.377 | 0.299 | 0.199 | 0.308 | - | - | - | - |
| Observations | 2,750 | 2,750 | 2,750 | 2,750 | 858 | 858 | 858 | 858 |
| Mean dep. var. | 0.098 | 0.087 | 0.095 | 0.141 | 0.098 | 0.086 | 0.097 | 0.145 |
| Bank controls x Cohort FE | Yes | Yes | Yes | Yes | No | No | No | No |
| Quarter x Cohort FE | Yes | Yes | Yes | Yes | No | No | No | No |
| Bank x Cohort FE | Yes | Yes | Yes | Yes | No | No | No | No |

Notes: This table reports estimates of Equation (1). Columns (1)–(4) use the stacked difference-in-differences estimator by Cengiz et al. (2019); columns (5)–(8) use synthetic difference-in-differences by Arkhangelsky et al. (2021). Panel A reports (log) lending volume; Panel B reports (log) number of borrowers; Panel C reports the share of lending to female entrepreneurs; Panel D reports the share of female entrepreneurs in the number of new borrowers. Column (1) includes all female borrowers; columns (2)–(4) disaggregate by borrower type. Columns (5)–(8) mirror this structure. Bank controls (lagged one quarter) include asset size, liquidity, profitability, NPL ratio, capital adequacy, and market share in corporate credit. Standard errors in parentheses are clustered at the bank level using wild cluster bootstrap for columns (1)–(4) and block bootstrap for columns (5)–(8). ***, **, and * denote statistical significance at 1, 5, and 10 percent. R-squared is not reported for SDiD, as it is not defined for this estimator.

**Table 2: Firm-level program effects on credit to repeat borrowers**

| | Log Credit | | | |
|---|---|---|---|---|
| | Female borrowers | | All borrowers | |
| | (1) | (2) | (3) | (4) |
| Post x WiB Bank | 0.053*** | 0.056*** | 0.020*** | 0.020*** |
| | (0.012) | (0.013) | (0.004) | (0.004) |
| Post x WiB Bank x Female | | | 0.065*** | 0.060*** |
| | | | (0.010) | (0.010) |
| R-squared | 0.545 | 0.601 | 0.544 | 0.563 |
| Observations | 1,456,881 | 1,417,136 | 15,928,918 | 15,880,034 |
| Firm x Cohort FE | Yes | Yes | Yes | Yes |
| Bank x Year x Cohort FE | Yes | No | Yes | No |
| District x Year x Cohort FE | Yes | No | Yes | No |
| Bank x District x Year x Cohort FE | No | Yes | No | Yes |
| Gender x Year x Cohort FE | No | No | Yes | Yes |

Notes: This table estimates WiB program effects on firm-level credit using the estimator by Cengiz et al. (2019). The unit of observation is the bank-firm-year. The dependent variable is log(new credit disbursed by bank $b$ to firm $i$ in year $t$), restricted to repeat borrowers who maintained a lending relationship with the same bank across the pre- and post-program periods. Sample period: 2014–2020. Columns (1)–(2) restrict the sample to female-owned firms. Columns (3)–(4) include male and female firms and report triple-difference estimates; the coefficient on WiB Bank × Post × Female identifies the differential program effect on female relative to male entrepreneurs. Treatment is defined based on the firm's main bank in 2014 (the bank holding the largest credit share); 'Post' equals one from the year of that bank's WiB entry onward. All specifications include fixed effects as indicated. Standard errors clustered at the firm level in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%.

**Table 3: Credit-supply shocks by WiB participation and borrowing by female entrepreneurs**

| Dependent variable: | | $\Delta$Credit | |
|---|---|---|---|
| | (1) | (2) | (3) |
| $\Delta\hat{L}_{idst}$ | 0.676*** | | |
| | (0.063) | | |
| WiB $\times\Delta\hat{L}_{idst}$ | | 0.866*** | 0.727*** |
| | | (0.068) | (0.082) |
| Non-WiB $\times\Delta\hat{L}_{idst}$ | | 0.623*** | 0.630*** |
| | | (0.068) | (0.089) |
| WiB $\times\Delta\hat{L}_{idst}\times$ ARPK | | | 0.286*** |
| | | | (0.104) |
| Non-WiB $\times\Delta\hat{L}_{idst}\times$ ARPK | | | −0.015 |
| | | | (0.113) |
| R-squared | 0.281 | 0.281 | 0.282 |
| Observations | 51,342 | 51,342 | 51,342 |
| Mean dep. var. | −0.005 | −0.005 | −0.005 |
| F-stat WiB $\times$ $\Delta\hat{L}_{idst}$ = Non-WiB $\times$ $\Delta\hat{L}_{idst}$ | | 10.394 | |
| $p$-value F-stat | | 0.001 | |
| Year FE | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |

Notes: This table shows coefficient estimates of Equation (4). Standard errors are clustered at the firm level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

**Table 4: Credit-supply shocks by WiB participation: Female firm growth, performance, and survival**

| Dependent variable: | Investment | ΔARPK | ΔCOGS | ΔSales | ΔProfit | Exit | ΔCustomers | ΔSuppliers |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| WiB $\times \Delta \hat{L}_{idst}$ | 0.122** | −0.001 | 0.153 | 0.132*** | 0.927*** | −0.027** | 0.117** | 0.168*** |
| | (0.059) | (0.072) | (0.113) | (0.042) | (0.351) | (0.012) | (0.055) | (0.048) |
| Non-WiB $\times \Delta \hat{L}_{idst}$ | 0.011 | −0.043 | −0.082 | −0.027 | 0.255 | −0.009 | 0.017 | 0.068* |
| | (0.041) | (0.047) | (0.061) | (0.029) | (0.205) | (0.008) | (0.035) | (0.035) |
| R-squared | 0.259 | 0.247 | 0.223 | 0.305 | 0.178 | 0.377 | 0.251 | 0.226 |
| Observations | 51,342 | 51,342 | 51,342 | 51,342 | 51,342 | 51,342 | 39,336 | 46,385 |
| Mean dep. var. | 0.103 | −0.050 | 0.050 | 0.052 | −0.189 | 0.033 | 0.004 | −0.015 |
| F-stat WiB $\times \Delta \hat{L}_{idst}$ = Non-WiB $\times \Delta \hat{L}_{idst}$ | 3.354 | 0.321 | 4.163 | 14.303 | 3.682 | 2.011 | 3.186 | 4.093 |
| $p$-value F-stat | 0.067 | 0.571 | 0.041 | 0.000 | 0.055 | 0.156 | 0.074 | 0.043 |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: This table shows coefficient estimates of Equation (4). Standard errors are clustered at the firm level and shown in parentheses. The dependent variable is indicated on top of each column and defined in Appendix A. ARPK: Average Revenue Product of Capital. COGS: Cost of Goods Sold. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

**Table 5: Credit-supply shocks by WiB participation and employment outcomes of female firms**

| Dependent variable: | ΔEmp. (1) | ΔMale Emp. (2) | ΔFemale Emp. (3) | ΔWage Bill (4) | ΔMale Wage Bill (5) | ΔFemale Wage Bill (6) |
|---|---|---|---|---|---|---|
| WiB $\times \Delta\hat{L}_{idst}$ | 0.173*** | 0.117*** | 0.064* | 0.131*** | −0.088 | 0.464* |
| | (0.045) | (0.036) | (0.034) | (0.049) | (0.170) | (0.242) |
| Non-WiB $\times \Delta\hat{L}_{idst}$ | 0.060* | 0.058** | 0.011 | 0.047 | 0.161 | 0.115 |
| | (0.031) | (0.024) | (0.022) | (0.037) | (0.104) | (0.148) |
| R-squared | 0.204 | 0.198 | 0.188 | 0.213 | 0.197 | 0.174 |
| Observations | 39,504 | 39,504 | 39,504 | 39,504 | 39,504 | 39,504 |
| Mean dep. var. | −0.018 | −0.017 | 0.003 | 0.130 | 0.072 | 0.110 |
| F-stat WiB $\times \Delta\hat{L}_{idst}$ = Non-WiB $\times \Delta\hat{L}_{idst}$ | 6.524 | 2.762 | 2.405 | 2.837 | 2.072 | 2.099 |
| $p$-value F-stat | 0.011 | 0.097 | 0.121 | 0.092 | 0.150 | 0.147 |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: This table shows coefficient estimates of Equation (4). Standard errors are clustered at the firm level and shown in parentheses. The dependent variable is indicated on top of each column and defined in Appendix A. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

## Table 6: District-level credit-supply shocks and aggregate outcomes

| Dependent variable: | Δ Credit | Δ Borrowers | Entry rate | | Exit rate | | Δ Female market share | Δ Sales | | Δ Profit | | Δ Employment | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | Female | Male | Female | Male | | Female | Male | Female | Male | Female | Male |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) |
| WiB $\times \Delta \hat{L}_{dt}$ | 0.245*** | 0.215*** | 0.072 | 0.069** | −0.017 | 0.028* | −0.178 | −0.086 | 0.128 | −0.164 | 0.047 | 0.165 | 0.062 |
| | (0.080) | (0.066) | (0.079) | (0.028) | (0.037) | (0.015) | (0.111) | (0.136) | (0.100) | (0.538) | (0.108) | (0.169) | (0.044) |
| Non-WiB $\times \Delta \hat{L}_{dt}$ | 0.119** | 0.129** | −0.031 | 0.012* | 0.001 | −0.004 | −0.069 | −0.017 | 0.059 | 0.489 | 0.052 | −0.133 | 0.118 |
| | (0.049) | (0.052) | (0.037) | (0.007) | (0.010) | (0.004) | (0.057) | (0.034) | (0.043) | (0.451) | (0.040) | (0.086) | (0.138) |
| R-squared | 0.328 | 0.335 | 0.223 | 0.412 | 0.276 | 0.415 | 0.222 | 0.213 | 0.209 | 0.208 | 0.146 | 0.177 | 0.204 |
| Observations | 3,332 | 3,332 | 3,332 | 3,332 | 3,332 | 3,332 | 3,332 | 3,332 | 3,332 | 3,332 | 3,332 | 3,245 | 3,245 |
| Mean dep. var. | 0.225 | 0.152 | 0.244 | 0.174 | 0.112 | 0.100 | 0.047 | 0.192 | 0.145 | 0.119 | 0.145 | 0.104 | 0.174 |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| District FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| $F$-test $H_0$: WiB = Non-WiB | 1.731 | 1.090 | 1.420 | 4.042 | 0.232 | 4.586 | 0.768 | 0.257 | 0.365 | 0.599 | 0.002 | 2.437 | 2.785 |
| $p$-value of $H_0$ | 0.189 | 0.297 | 0.234 | 0.045 | 0.630 | 0.033 | 0.381 | 0.612 | 0.546 | 0.439 | 0.965 | 0.119 | 0.252 |

Notes: This table shows coefficient estimates of Equation (6). Standard errors are clustered at the district level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

*Online Appendix for*

# Blended Finance and Female Entrepreneurship

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## Appendix A. Variable Definitions

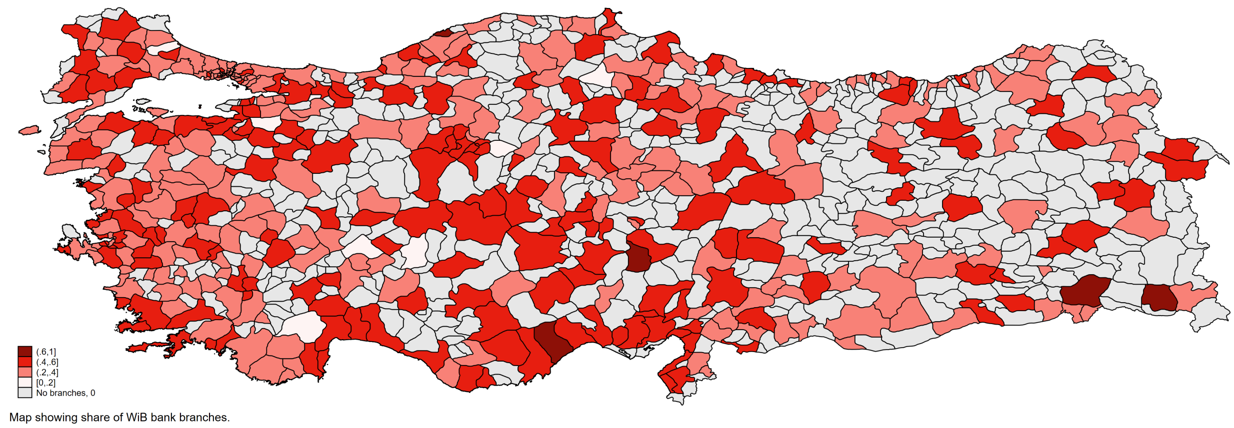
| Variable Name | Description | Source |
|---|---|---|
| **Panel A: Bank-level variables** | | |
| *Asset size* | Total value of all assets on a bank's balance sheet in (log) Turkish liras | CBRT |
| *Liquidity* | Ratio of a bank's liquid assets – defined as cash, money market funds, and securities in trading books such as stocks and bonds – to total assets | CBRT |
| *Profitability* | Ratio of a bank's profits to total assets | CBRT |
| *Non-performing loans (NPL)* | Stock of loans that are more than 90 days past due or have been written off by the bank earlier, scaled by total assets | CBRT |
| *Delinquency ratio* | Share of loans classified as Stage 2 under Basel III accounting standards (loans with a significant increase in credit risk since origination that have not yet defaulted) in a bank's total lending to entrepreneurs | CBRT |
| *Check default* | Number of entrepreneurs who defaulted on a commercial check within 24 months after loan origination | CBRT |
| *Loan-loss reserves* | Total amount of funds a bank sets aside to cover potential loan losses, scaled by total assets | CBRT |
| *Capital adequacy* | Tier 1 capital scaled by total assets | CBRT |
| *Market share corporate credit* | National market share in lending to all corporates | CBRT |
| *Market share entrepreneurial credit* | National market share in lending to small businesses and entrepreneurs | CBRT |
| *Share of female lending* | Share of credit to female-owned small businesses and female entrepreneurs in total credit to all small businesses and entrepreneurs | CBRT |
| *Share of uncollateralised lending* | Share of loans without collateral requirements in a bank's total lending to entrepreneurs | CBRT |
| **Panel B: Borrower classifications** | | |
| *Repeat borrowers* | Entrepreneurs who had taken out at least one loan from the same bank before commencement of the WiB program and at least one loan from the same bank after the program began | CBRT |
| *Poached borrowers* | Entrepreneurs who took out at least one loan from a bank after the WiB program began and at least one loan from another bank before the program started | CBRT |
| *First-time borrowers* | Entrepreneurs who had never taken out a loan before the start of the WiB program and first appear in the credit registry with a loan after the program began | CBRT |
| **Panel C: Credit and lending variables** | | |
| *Credit* | Total credit stock at year-end in Turkish lira | CBRT |
| *Loans from treated banks* | Cumulative number of loans a first-time borrower obtains over eight quarters from her entry bank | CBRT |
| *Credit from treated banks* | Total credit amount a first-time borrower obtains over eight quarters from her entry bank, in Turkish lira | CBRT |

| Variable Name | Description | Source |
|---|---|---|
| *Lending share (spillover analysis)* | Leave-one-out market share of treated banks in a district's entrepreneurial lending as of 2014 | CBRT |
| **Panel D: Firm-level financial variables** | | |
| *Total assets* | Total value of all assets on a firm's balance sheet, in millions of Turkish lira | MTF |
| *Fixed assets* | Gross fixed assets (property, plant, and equipment), in millions of Turkish lira | MTF |
| *Sales* | Total amount of revenue at year-end in Turkish lira | MTF |
| *Investment* | Annual change in (log) gross fixed assets (property, plant, equipment) | MTF |
| *ARPK* | Average revenue product of capital; log ratio of a business's total sales to fixed assets (dummy=1 if ARPK is above median; 0 otherwise) | MTF |
| *Cost of goods sold* | Reported end-of-year total cost of goods sold | MTF |
| *Profit* | Reported end-of-year profit | MTF |
| *Firm entry* | Indicator variable equal to 1 if a business first appears in the annual tax filings, 0 otherwise | MTF |
| *Firm exit* | Indicator variable equal to 1 if a business no longer appears in the annual tax filings, 0 otherwise | MTF |
| **Panel E: Business network variables** | | |
| *Number of customers* | Number of unique businesses in a year to which an entrepreneur sells products and/or services as observed in the VAT register | MTF |
| *Number of suppliers* | Number of unique businesses in a year from which an entrepreneur buys products and/or services as observed in the VAT register | MTF |
| *Trade value* | Log annual transaction value with trade partners (suppliers or customers) as registered in the VAT register | MTF |
| *HHI (partner concentration)* | Herfindahl–Hirschman index based on the shares of individual trade partners in a firm's total purchases or sales | MTF |
| *Industry HHI* | Herfindahl–Hirschman index based on the industry composition of a firm's trade partners | MTF |
| *Centrality* | PageRank measure of a firm's centrality in the firm-to-firm trade network, where higher values indicate a more central position | MTF |
| *Female share of trade partners employees* | Share of a firm's trade partners' (suppliers' or customers') employees that are female | MTF |
| **Panel F: Employment variables** | | |
| *Number of employees* | Total number of employees at the firm | SGK |
| *Number of male employees* | Number of male employees at the firm | SGK |
| *Number of female employees* | Number of female employees at the firm | SGK |
| *Total wage bill* | Total monthly wages to employees | SGK |
| *Total male wage bill* | Total monthly wages to male employees | SGK |

| Variable Name | Description | Source |
| --- | --- | --- |
| *Total female wage bill* | Total monthly wages to female employees | SGK |
| **Panel G: District-level variables** | | |
| *Female market share* | Share of sales from female–owned firms. | MTF |
| *Population density* | Number of inhabitants per square kilometre | CBRT |
| *Rural share* | Share of a district's population residing in rural areas | CBRT |

Notes: CBRT stands for the Central Bank of the Republic of Turkey, MTF for the Ministry of Treasury and Finance, and SGK for the social security institution Sosyal Güvenlik Kurumu. Small businesses are defined as companies with a single shareholder who has unlimited liability for the company's debts and undertakings, typically incorporated as sole proprietorships.

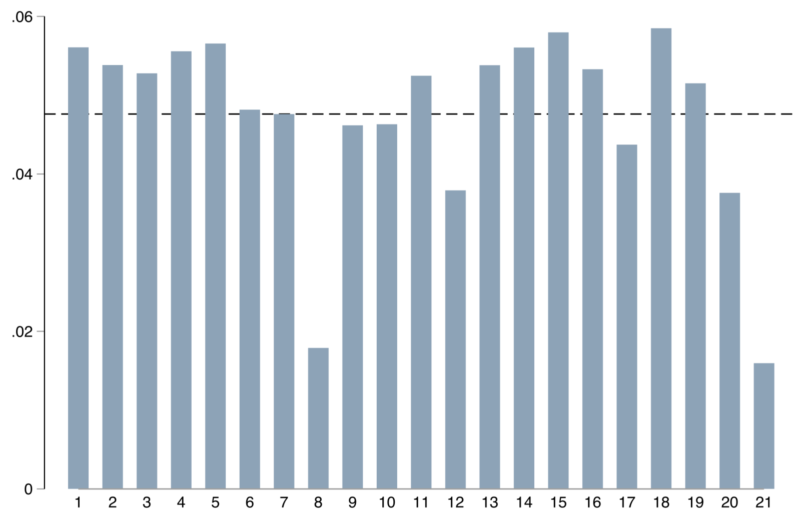
**Figure A.1: Pre-program share of bank branches operated by WiB banks**



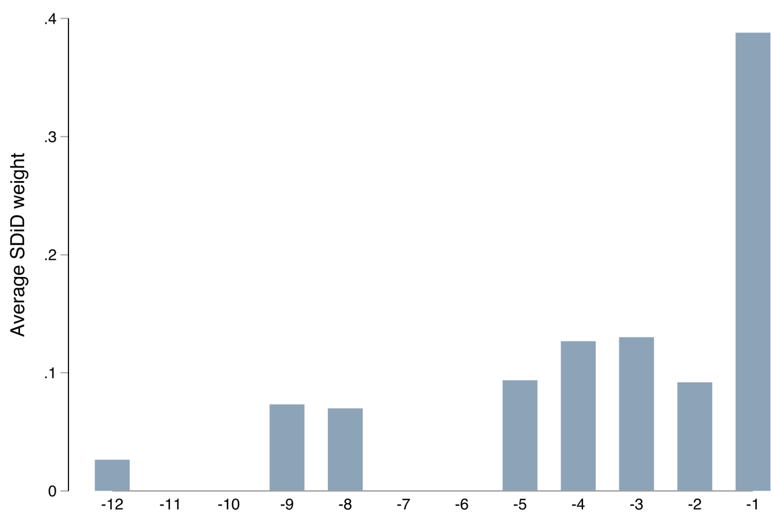

Notes: This map shows for Turkish districts the share of branches operated by WiB banks as of end-2014.

**Figure A.2: Synthetic DiD estimates: Unit and time weights**

Panel A: Unit weights



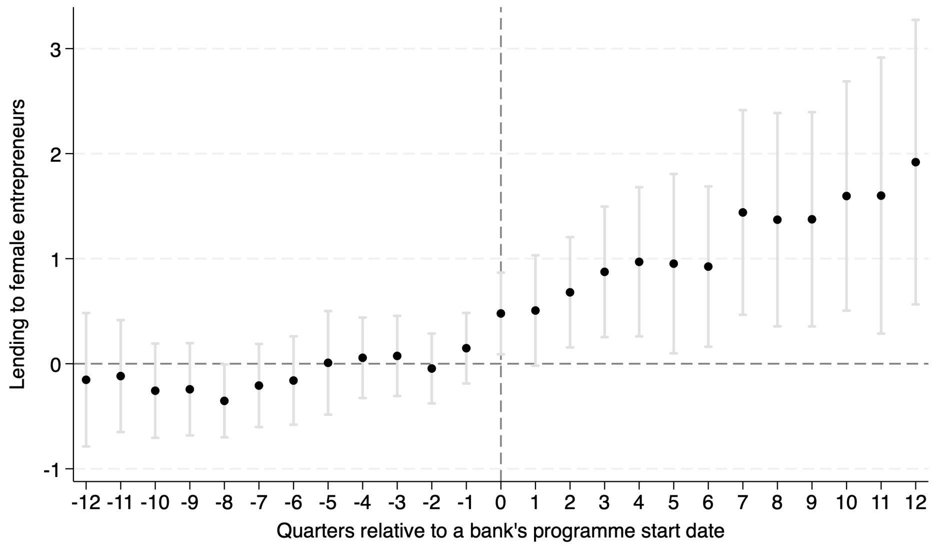

Panel B: Time weights



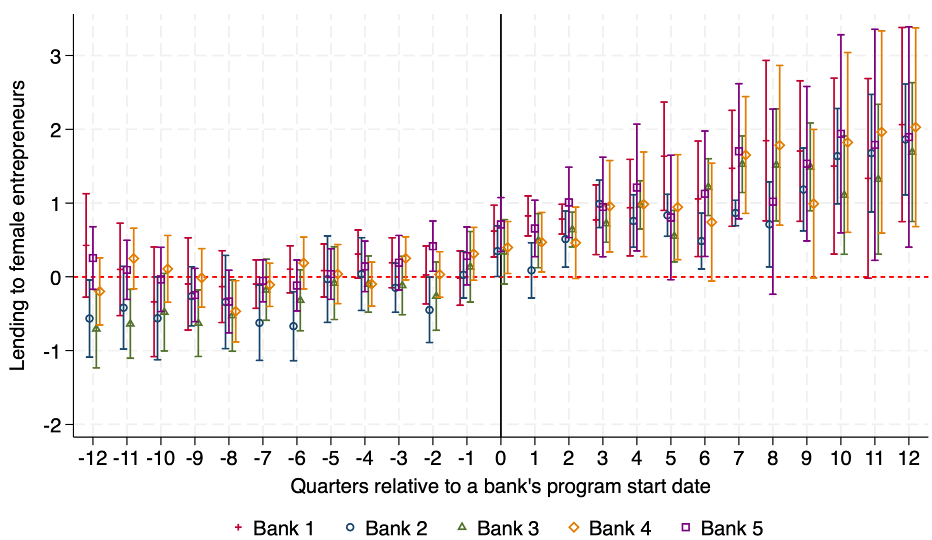

Notes: Panel A shows unit (bank) weights from the SDiD estimates in Figure A.3, averaged across treated banks. The dashed horizontal line indicates the value if all units were weighted equally. Panel B shows time (quarter) weights from the SDiD estimates in Figure A.3 averaged across treated banks.

**Figure A.3: Event-study estimates using synthetic difference-in-differences**

Panel A: Lending to female entrepreneurs (aggregated ATT)

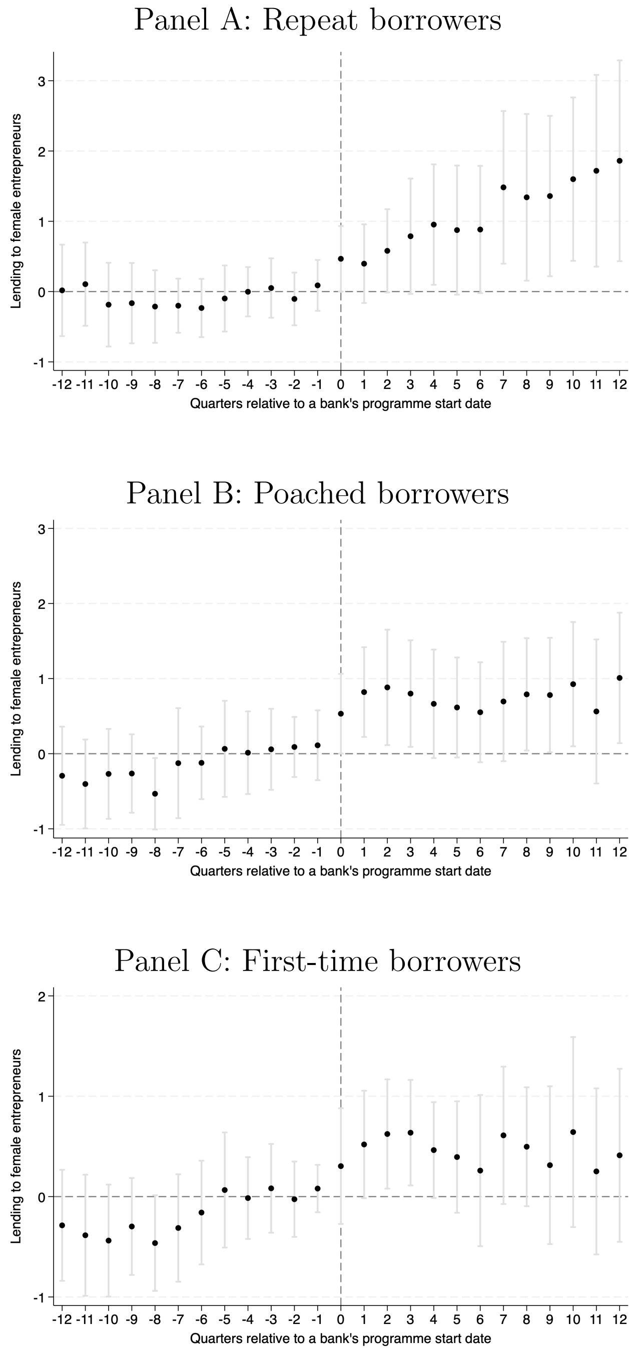



Panel B: Lending to female entrepreneurs (bank-by-bank ATT)




Notes: This figure shows estimates of Equation (1) in an event-study setup using the synthetic difference-in-differences methodology of Arkhangelsky et al. (2021). Panel A uses the aggregation procedure of Ciccia (2024) to show the aggregated average treatment effect on the treated (ATT) across all WiB banks. Panel B shows the corresponding bank-by-bank ATT estimates. The dependent variable is (log) total loan volume to female entrepreneurs. Error bands show 95 percent confidence intervals.

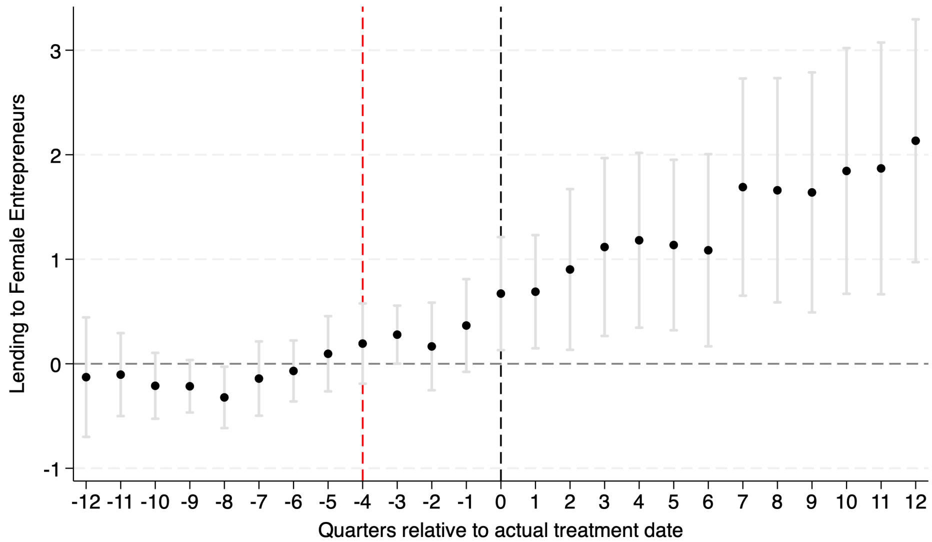
**Figure A.4: Event-study estimates using synthetic DiD, by borrower type**



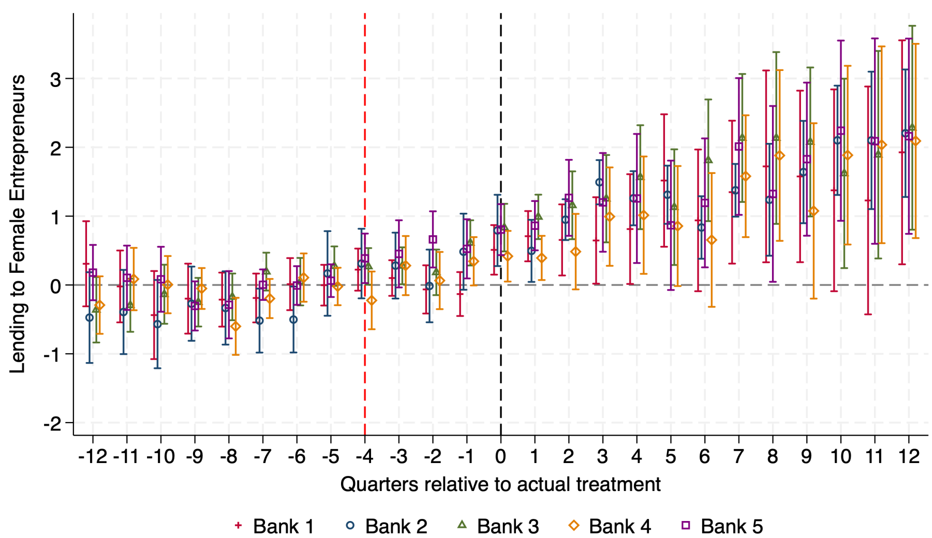

Notes: This figure presents event-study estimates of Equation (1) using the synthetic difference-in-differences methodology of Arkhangelsky et al. (2021). We apply the aggregation procedure of Ciccia (2024) to obtain the average treatment effect on the treated (ATT) across all WiB banks. The dependent variable is (log) total loan volume to female entrepreneurs. Panel A shows the ATT for repeat borrowers; Panel B for poached borrowers; Panel C for first-time borrowers. The x-axis denotes quarters relative to a bank's program entry. Error bands show 95 percent confidence intervals based on block-clustered bootstrap standard errors.

**Figure A.5: Synthetic DiD estimates: Backdating the WiB program introduction**

Panel A: Lending to female entrepreneurs (aggregated ATT)

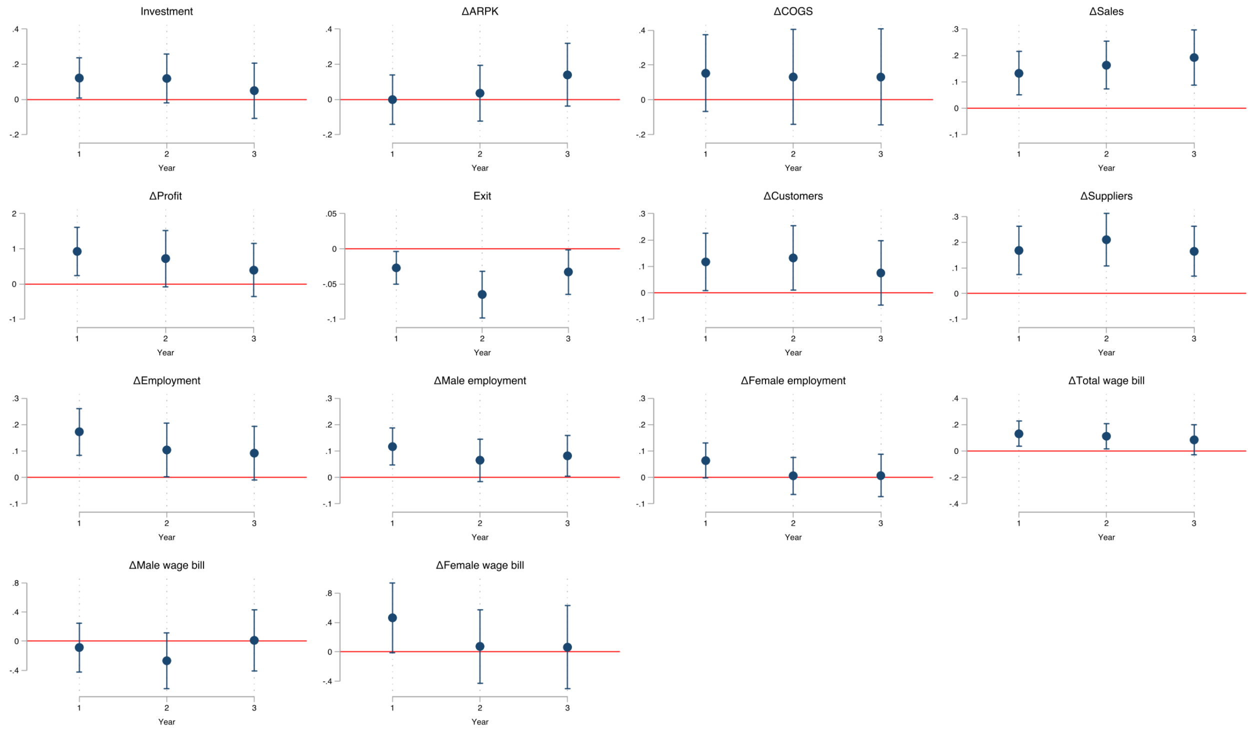



Panel B: Lending to female entrepreneurs (bank-by-bank ATT)




Notes: This figure shows estimates of Equation (1) when treatment is artificially backdated in an event-study setup using the synthetic difference-in-differences methodology of Arkhangelsky et al. (2021). Panel A shows the aggregated average treatment effect on the treated (ATT) across all WiB banks, using the aggregation procedure described in Ciccia (2024). Panel B shows the bank-by-bank ATT estimates. The dependent variable is (log) total loan volume to female entrepreneurs. The dashed vertical red lines indicate the placebo (backdated) program start date, while the dashed vertical black lines indicate the actual intervention date at *t=0*. Error bands show 95 percent confidence intervals.

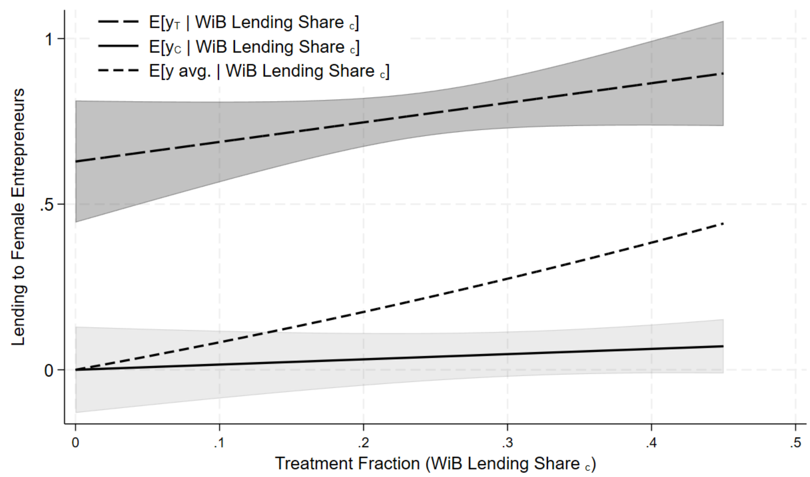
**Figure A.6: Dynamic impacts of the WiB credit-supply shock on female firms**



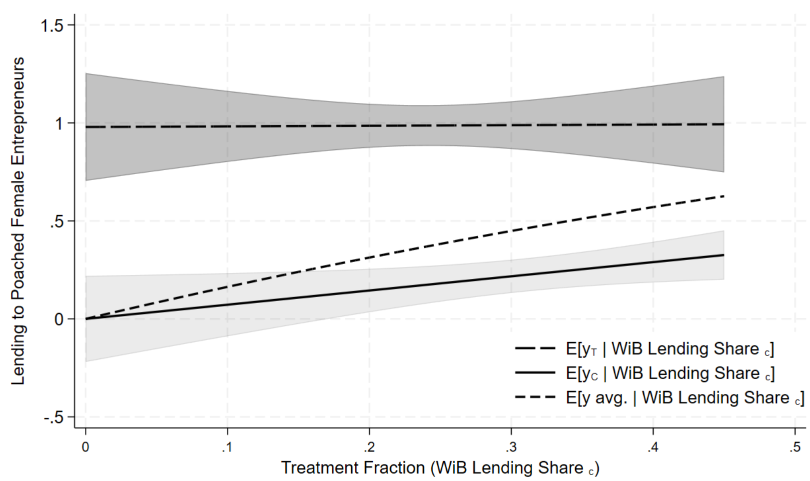

Notes: This figure shows estimates of Equation (4) for the term WiB $\times \Delta \hat{L}_{idst}$. Each point estimate within each panel comes from a separate regression. The dependent variable is indicated on top of each panel and defined in Appendix A. ARPK: Average Revenue Product of Capital. COGS: Cost of Goods Sold. Error bands show 95 percent confidence intervals.

**Figure A.7: Spillover analysis**

Panel A: Lending to all female entrepreneurs

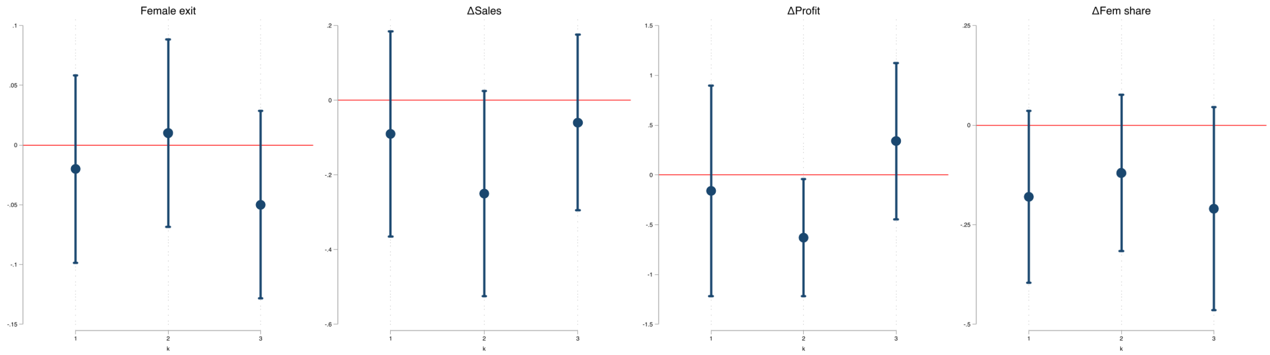



Panel B: Lending to poached female entrepreneurs




Notes: This figure illustrates the district-level spillover effects of lending to female entrepreneurs for all (Panel A) and poached (Panel B) borrowers, following Berg et al. (2021). The figures plot the change in (log) lending to female entrepreneurs between 2019 and 2014 as a function of the pre-existing market share of WiB banks in 2014. $\mathbb{E}[y_T|\text{WiB Lending Share}]$ shows the average change in lending from treated banks, conditional on a given district treatment fraction; $\mathbb{E}[y_C|\text{WiB Lending Share}]$ shows the corresponding change from control banks; and $\mathbb{E}[y_{avg}|\text{WiB Lending Share}]$ shows the weighted average across both. 90% Confidence intervals are shown.

**Figure A.8: Dynamic impacts of the WiB credit-supply shock at the district level**




Notes: This figure shows estimates of Equation (6) on the term WiB $\times \Delta \hat{L}_{dt}$. Each point estimate within each panel comes from a separate regression. The dependent variable is indicated on top of each panel and defined in the text. Error bands show 95 percent confidence intervals.

**Table A.1: Overview of blended finance programs targeting female entrepreneurship**

| # | Program | Lead institution(s) | Launch year | Amount (USD mn) | Geography |
|---|---|---|---|---|---|
| 1. | Global SME Finance Facility | IFC | 2012 | 6,266 | Global |
| 2. | Women Entrepreneurs Opportunity Facility | IFC & Goldman Sachs | 2014 | 3,000 | 56 countries |
| 3. | Banking on Women | IFC | 2012 | 3,000 | 83 countries |
| 4. | SheInvest | EIB & Dev. Bank of Rwanda | 2019 | 2,000 | Africa, Asia, LatAm |
| 5. | We-Fi Rounds I, III, IV | We-Fi & IFC | 2018 | 1,790 | 37 countries |
| 6. | Global Gender-Smart Fund | KfW, IFC, Dev. Bank of Austria | 2009 | 550 | 44 countries |
| 7. | We-Fi AFAWA & ADFI | We-Fi & AfDB | 2019 | 540 | 26 African countries |
| 8. | TurWiB II | EBRD | 2021 | 532 | Turkey |
| 9. | Vietnam Prosperity Bank | ADB | 2022 | 500 | Vietnam |
| 10. | We-Fi WeForLAC, WE3A, WECOUNT | We-Fi & IDB | 2019 | 381 | 13 LatAm countries |
| 11. | We-Fi World Bank Program | We-Fi & World Bank | 2018 | 341 | 27 countries |
| 12. | TurWiB I | EBRD | 2013 | 334 | Turkey |
| 13. | We-Fi WAVES | We-Fi & ADB | 2018 | 229 | 5 Asia-Pacific countries |
| 14. | IFC Blended Finance for SMEs | IFC, GSMEF, WEOF, We-Fi | 2020 | 215 | IDA countries |
| 15. | Women Entrepreneurs Peru | BBVA Peru & IDB Invest | 2022 | 200 | Peru |
| 16. | We-Fi BRAVE Women | We-Fi & IsDB | 2018 | 175 | 8 countries |
| 17. | We-Fi Women of the Steppe | We-Fi & EBRD | 2019 | 170 | 6 countries |
| 18. | Women Inclusive Finance Pakistan | ADB | 2023 | 156 | Pakistan |
| 19. | Ngern Tid Lor Thailand | ADB | 2023 | 150 | Thailand |
| 20. | EAP WiB I | EBRD | 2016 | 114 | Turkey |
| 21. | Women Entrepreneurship Banking | IDB | 2012 | 110 | LatAm & Carib |

*Continued on next page*

Table A.1 – *Continued from previous page*

| # | Program | Lead Institution(s) | Launch Year | Amount (USD mn) | Geography |
|---|---|---|---|---|---|
| 22. | KazWiB II | EBRD | 2019 | 106 | Turkey |
| 23. | Women's World Banking Fund II | EU, BMZ, DFC, EIB, others | 2017 | 103 | India, Africa, Colombia |
| 24. | Women Entrepreneurs Peru (IDB) | IDB | 2023 | 100 | Peru |
| 25. | CA WiB Programme | EBRD | 2019 | 98 | Turkey |
| 26. | Egypt WiB | EBRD | 2014 | 91 | Turkey |
| 27. | KazWiB I | EBRD | 2015 | 80 | Turkey |
| 28. | Women Entrepreneurs Bond | Fedecredito & IDB Invest | 2023 | 80 | El Salvador |
| 29. | MASSIF | FMO | 2006 | 75 | Low/middle income |
| 30. | Women Owned and Managed SMEs Credit Line (Garantibank) | EBRD | 2012 | 74 | Turkey |
| 31. | Guinea Bissau PAIFJ | AfDB | 2021 | 71 | Guinea Bissau |
| 32. | Climate Gender Equity Fund | USAID, Amazon, others | 2023 | 60 | Global |
| 33. | Morocco WiB | EBRD | 2018 | 59 | Turkey |
| 34. | WB WiB II | EBRD | 2016 | 55 | Turkey |
| 35. | Expanding Access to Finance for Women-Owned SMEs | ADB | 2024 | 31 | Vietnam |
| 36. | WB WiB I | EBRD | 2014 | 26 | Turkey |
| 37. | Additional programs (23) | Various | 2015-2024 | 164 | Various |
| 38. | Total (59 programs) | | | 22,026 | 100+ countries |

Notes: This table presents a compilation of major blended finance programs targeting female entrepreneurship. Sources: (i) searches of DFI and MDB public disclosures; (ii) Convergence database (https://www.convergence.finance/); and (iii) Uxolo database. Amounts represent committed capital in USD millions. We assume that 13% of new financing (totaling USD 48,200 mn) under the Global SME Finance Facility lead by the IFC financed women-owned SMEs. This is the share of total financing under the IFC's MSME Finance Facility that went to women entrepreneurs. We-Fi = Women Entrepreneurs Finance Initiative; IFC = International Finance Corporation; EIB = European Investment Bank; AfDB = African Development Bank; ADB = Asian Development Bank; IDB = Inter-American Development Bank; EBRD = European Bank for Reconstruction and Development; IsDB = Islamic Development Bank; KfW = German Development Bank; FMO = Dutch Development Bank; DFC = U.S. Development Finance Corporation; LatAm = Latin America. The final row aggregates 23 smaller programs (each equal to or under USD 20 million) for space considerations.

**Table A.2: Summary statistics for female entrepreneurs**

| | Observations | Mean | S.D. | Min | p25 | Median | p75 | Max |
|---|---|---|---|---|---|---|---|---|
| Total assets | 51,342 | 1.040 | 1.820 | 0.001 | 0.320 | 0.629 | 1.164 | 129.978 |
| Fixed assets | 51,342 | 0.159 | 0.259 | 0.000 | 0.020 | 0.068 | 0.175 | 1.655 |
| Total credit | 51,342 | 0.249 | 0.496 | 0.000 | 0.054 | 0.126 | 0.272 | 36.011 |
| Sales | 51,342 | 1.235 | 1.438 | 0.000 | 0.377 | 0.806 | 1.515 | 10.571 |
| Cost of goods sold | 51,342 | 1.049 | 1.304 | 0.000 | 0.289 | 0.666 | 1.280 | 9.568 |
| Profit | 51,342 | 0.181 | 0.207 | -0.156 | 0.050 | 0.124 | 0.238 | 1.279 |
| Number of customers | 44,586 | 7.254 | 10.793 | 0.000 | 2.000 | 3.000 | 8.000 | 67.000 |
| Number of suppliers | 48,309 | 7.628 | 7.973 | 0.000 | 3.000 | 5.000 | 9.000 | 50.000 |
| Number of employees | 42,034 | 4.525 | 5.144 | 1.000 | 2.000 | 3.000 | 5.000 | 31.000 |
| Number of male employees | 42,034 | 2.847 | 3.430 | 0.000 | 1.000 | 2.000 | 3.000 | 21.000 |
| Number of female employees | 42,034 | 1.587 | 2.537 | 0.000 | 0.000 | 1.000 | 2.000 | 15.000 |
| Total wage bill | 42,034 | 8.041 | 9.327 | 0.338 | 2.632 | 5.117 | 8.922 | 64.984 |
| Total male wage bill | 42,034 | 5.013 | 6.108 | 0.000 | 1.698 | 3.226 | 5.984 | 40.508 |
| Total female wage bill | 42,034 | 2.896 | 4.773 | 0.000 | 0.000 | 1.647 | 3.555 | 35.135 |
| ARPK | 51,342 | 2.604 | 1.769 | -2.073 | 1.373 | 2.507 | 3.702 | 7.798 |
| Trade value suppliers | 49,142 | 5.970 | 1.725 | 0.000 | 5.225 | 6.332 | 7.082 | 13.525 |
| Trade value customers | 49,428 | 4.998 | 2.505 | 0.000 | 3.717 | 5.757 | 6.850 | 13.911 |
| Supplier HHI | 49,142 | 0.481 | 0.299 | 0.000 | 0.232 | 0.421 | 0.718 | 1.000 |
| Customer HHI | 49,428 | 0.519 | 0.378 | 0.000 | 0.160 | 0.500 | 0.953 | 1.000 |
| Supplier industry HHI | 47,782 | 0.650 | 0.295 | 0.043 | 0.383 | 0.634 | 0.988 | 1.000 |
| Customer industry HHI | 42,469 | 0.678 | 0.309 | 0.028 | 0.386 | 0.753 | 1.000 | 1.000 |
| Customer centrality | 49,142 | 0.452 | 0.579 | 0.224 | 0.249 | 0.348 | 0.425 | 26.687 |
| Supplier centrality | 49,428 | 0.714 | 0.623 | 0.357 | 0.437 | 0.564 | 0.732 | 24.500 |
| Supplier share of female employees | 27,239 | 0.211 | 0.133 | 0.000 | 0.137 | 0.168 | 0.260 | 1.000 |
| Customer share of female employees | 22,761 | 0.329 | 0.213 | 0.000 | 0.156 | 0.290 | 0.494 | 1.000 |

Notes: This table shows summary statistics for female-owned firms (observations are at the firm×year level) for which we observe yearly financial information from tax records and which are present in the credit registry. The sample period is 2014–2020. All variables are measured in millions of Turkish lira, except for the wage bill variables, which are in thousands of Turkish lira, and except for numbers of customers and suppliers.

**Table A.3: Pre-program bank characteristics**

| | Treated banks | Mean | Control banks | Mean | Diff. |
|---|---|---|---|---|---|
| Asset size | 5 | 18.663 | 21 | 16.902 | -1.762** |
| Market share in corporate credit | 5 | 0.078 | 21 | 0.027 | -0.051*** |
| Market share in entrepreneurial credit | 5 | 0.056 | 21 | 0.034 | -0.022 |
| Share of female lending | 5 | 0.090 | 21 | 0.102 | 0.012 |
| Liquidity | 5 | 0.144 | 21 | 0.184 | 0.040 |
| Profitability | 5 | 0.009 | 21 | 0.008 | -0.002 |
| Non-performing loans | 5 | 0.021 | 21 | 0.021 | 0.000 |
| Loan-loss reserves | 5 | 0.009 | 21 | 0.008 | -0.001 |
| Capital adequacy | 5 | 0.106 | 21 | 0.108 | 0.002 |

Notes: This table presents summary statistics as of end-2014 for the five treated and 21 control banks. Asset size is in (log) Turkish lira (000s). Liquidity, profitability, non-performing loans, loan-loss reserves, and capital adequacy are all scaled by total assets. Market share in corporate credit is a bank's national market share in lending to corporates. Market share in entrepreneurial credit is a bank's national market share in lending to small businesses for which we can identify the gender of the owner. Small businesses are defined as companies with a single shareholder who has unlimited liability for the company's debts and undertakings, typically incorporated as sole proprietorships. Share of female lending is a bank's share of credit to female-led small businesses in credit to all small businesses.

**Table A.4: Pre-treatment predictive accuracy of synthetic controls and signal-to-noise ratios**

| Bank | ATT | SE | RMSE | MAE | S/N |
|---|---|---|---|---|---|
| Bank 1 | 1.563 | 0.443 | 0.237 | 0.198 | 6.605 |
| Bank 2 | 1.627 | 0.548 | 0.416 | 0.362 | 3.909 |
| Bank 3 | 1.611 | 0.536 | 0.298 | 0.263 | 5.408 |
| Bank 4 | 1.446 | 0.391 | 0.359 | 0.231 | 4.029 |
| Bank 5 | 0.919 | 0.325 | 0.242 | 0.194 | 3.802 |

Notes: This table evaluates the out-of-sample predictive performance of the SDiD (Arkhangelsky et al., 2021) synthetic controls. For each treated bank and each pre-treatment quarter, we exclude that quarter, re-estimate the SDiD model on the remaining pre-treatment data, and predict the omitted outcome using the resulting weights. The difference between the actual and predicted value is a placebo prediction error. Repeating this for all pre-treatment quarters yields a distribution of prediction errors summarized by the Root Mean Squared Error (RMSE) and the Mean Absolute Error (MAE). The signal-to-noise ratio equals the bank-specific Average Treatment Effect on the Treated (ATT) divided by the placebo RMSE and measures how large the estimated treatment effect is relative to typical pre-treatment prediction error. Ratios well above one indicate that treatment effects substantially exceed normal synthetic-control prediction noise.

**Table A.5: Program effects on lending to female entrepreneurs, by borrower type**

| | All (1) | Repeat (2) | Poached (3) | First-time (4) |
|---|---|---|---|---|
| **A. Lending to entrepreneurs** | | | | |
| Post x WiB Bank x Female entrepreneur | 0.068*** | 0.037*** | 0.019*** | 0.012*** |
| | (0.008) | (0.003) | (0.004) | (0.003) |
| R-squared | 0.798 | 0.799 | 0.660 | 0.820 |
| Observations | 5,500 | 5,500 | 5,500 | 5,500 |
| Mean dep. var. | 0.195 | 0.135 | 0.038 | 0.022 |
| **B. Number of entrepreneurs** | | | | |
| Post x WiB Bank x Female entrepreneur | 0.066*** | 0.027*** | 0.020*** | 0.019*** |
| | (0.012) | (0.010) | (0.005) | (0.007) |
| R-squared | 0.874 | 0.911 | 0.840 | 0.829 |
| Observations | 5,500 | 5,500 | 5,500 | 5,500 |
| Mean dep. var. | 0.131 | 0.086 | 0.028 | 0.017 |
| Bank x Quarter x Cohort FE | Yes | Yes | Yes | Yes |
| Bank x Gender x Cohort FE | Yes | Yes | Yes | Yes |

Notes: This table reports triple-difference estimates using the stacked difference-in-differences estimator of Cengiz et al. (2019). The data are reshaped at the bank-quarter-gender level. All specifications include bank $\times$ quarter $\times$ cohort and bank $\times$ gender $\times$ cohort fixed effects. In Panel A, the dependent variable is the quarterly change in lending to entrepreneurs by gender and borrower type, scaled by the bank's average stock of total lending to male and female entrepreneurs over the quarter. In Panel B, the dependent variable is constructed analogously using the number of entrepreneurs rather than lending volumes. Column (1) aggregates across all borrower types; columns (2)–(4) decompose by repeat, poached, and first-time borrowers as defined in Section 3.3. Standard errors are clustered at the bank level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

## Table A.6: Program effects on the share of uncollateralised loans

| | Female borrowers | | | | All borrowers | | | |
|---|---|---|---|---|---|---|---|---|
| | All | Repeat | Poached | First-time | All | Repeat | Poached | First-time |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| WiB Bank x Post | 0.065** | 0.101*** | 0.054* | 0.075 | | | | |
| | (0.029) | (0.029) | (0.031) | (0.045) | | | | |
| WiB Bank x Post x Female | | | | | -0.016* | -0.013 | -0.004 | -0.010 |
| | | | | | (0.009) | (0.010) | (0.012) | (0.020) |
| R-squared | 0.652 | 0.705 | 0.689 | 0.710 | 0.887 | 0.885 | 0.894 | 0.912 |
| Observations | 2,750 | 2,750 | 2,750 | 2,750 | 5,500 | 5,500 | 5,500 | 5,500 |
| Mean dep. var. | 0.640 | 0.648 | 0.527 | 0.515 | 0.700 | 0.716 | 0.569 | 0.546 |
| Bank x Cohort FE | Yes | Yes | Yes | Yes | No | No | No | No |
| Quarter x Cohort FE | Yes | Yes | Yes | Yes | No | No | No | No |
| Bank x Quarter x Cohort FE | No | No | No | No | Yes | Yes | Yes | Yes |
| Bank x Gender x Cohort FE | No | No | No | No | Yes | Yes | Yes | Yes |

Notes: This table estimates program effects on the share of uncollateralized lending in total entrepreneurial lending. Columns (1)–(4) estimate Equation (1) for female borrowers only; columns (5)–(8) estimate this equation for both genders, where the coefficient on WiB Bank × Post × Female identifies the differential effect on female relative to male entrepreneurs. Within each set, columns report results for all borrowers, repeat, poached, and first-time borrowers, respectively. All specifications use the stacked difference-in-differences estimator of Cengiz et al. (2019). Standard errors clustered at the bank level in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%.

## Table A.7: Program effects on loan quality

| | Female borrowers | | | | All borrowers | | | |
|---|---|---|---|---|---|---|---|---|
| | All | Repeat | Poached | First-time | All | Repeat | Poached | First-time |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| **A. Non-Performing Loans** | | | | | | | | |
| Post x WiB Bank | -0.010 | -0.007 | -0.001 | -0.000 | | | | |
| | (0.008) | (0.008) | (0.010) | (0.008) | | | | |
| Post x WiB Bank x Female | | | | | -0.005 | -0.004 | -0.005 | -0.002 |
| | | | | | (0.004) | (0.004) | (0.004) | (0.008) |
| Mean | 0.029 | 0.026 | 0.026 | 0.021 | 0.025 | 0.024 | 0.024 | 0.017 |
| R-squared | 0.314 | 0.286 | 0.215 | 0.260 | 0.748 | 0.645 | 0.711 | 0.675 |
| Observations | 2,750 | 2,750 | 2,750 | 2,750 | 5,500 | 5,500 | 5,500 | 5,500 |
| Bank x Cohort FE | Yes | Yes | Yes | Yes | No | No | No | No |
| Quarter x Cohort FE | Yes | Yes | Yes | Yes | No | No | No | No |
| Bank x Quarter x Cohort FE | No | No | No | No | Yes | Yes | Yes | Yes |
| Bank x Gender x Cohort FE | No | No | No | No | Yes | Yes | Yes | Yes |
| **B. Delinquent loans** | | | | | | | | |
| Post x WiB Bank | 0.017 | 0.036 | 0.008 | 0.004 | | | | |
| | (0.033) | (0.031) | (0.042) | (0.030) | | | | |
| Post x WiB Bank x Female | | | | | 0.003 | 0.005 | -0.004 | 0.001 |
| | | | | | (0.009) | (0.009) | (0.011) | (0.008) |
| Mean | 0.128 | 0.121 | 0.145 | 0.118 | 0.115 | 0.111 | 0.134 | 0.107 |
| R-squared | 0.410 | 0.457 | 0.412 | 0.522 | 0.792 | 0.779 | 0.760 | 0.802 |
| Observations | 2,750 | 2,750 | 2,750 | 2,750 | 5,500 | 5,500 | 5,500 | 5,500 |
| Bank x Cohort FE | Yes | Yes | Yes | Yes | No | No | No | No |
| Quarter x Cohort FE | Yes | Yes | Yes | Yes | No | No | No | No |
| Bank x Quarter x Cohort FE | No | No | No | No | Yes | Yes | Yes | Yes |
| Bank x Gender x Cohort FE | No | No | No | No | Yes | Yes | Yes | Yes |
| **C. Check default** | | | | | | | | |
| Post x WiB Bank | 0.555*** | 0.611*** | -0.055 | -0.382* | | | | |
| | (0.142) | (0.123) | (0.123) | (0.227) | | | | |
| Post x WiB Bank x Female | | | | | -0.049** | -0.016 | -0.005 | 0.083 |
| | | | | | (0.021) | (0.025) | (0.014) | (0.067) |
| Mean | 2.742 | 2.631 | 1.540 | 0.916 | 3.522 | 3.429 | 2.181 | 1.303 |
| R-squared | 0.929 | 0.940 | 0.860 | 0.841 | 0.988 | 0.993 | 0.982 | 0.976 |
| Observations | 2,750 | 2,750 | 2,750 | 2,750 | 5,500 | 5,500 | 5,500 | 5,500 |
| Bank x Cohort FE | Yes | Yes | Yes | Yes | No | No | No | No |
| Quarter x Cohort FE | Yes | Yes | Yes | Yes | No | No | No | No |
| Bank x Quarter x Cohort FE | No | No | No | No | Yes | Yes | Yes | Yes |
| Bank x Gender x Cohort FE | No | No | No | No | Yes | Yes | Yes | Yes |

Notes: This table estimates program effects on loan quality using the stacked difference-in-differences estimator of Cengiz et al. (2019). Columns (1)–(4) estimate Equation (1) for female borrowers only; columns (5)–(8) estimate this equation for both genders, where the coefficient on WiB Bank × Post × Female identifies the differential effect on female relative to male entrepreneurs. Within each set, columns report results for all borrowers, repeat, poached, and first-time borrowers, respectively. In Panel A, the dependent variable is the non-performing loan (NPL) ratio: the share of loans that are either overdue by at least 90 days or written off. In Panel B, the dependent variable is the delinquency ratio: the share of loans classified as Stage 2 under Basel III accounting standards. Stage 2 loans are those that have experienced a significant increase in credit risk since origination but have not yet defaulted, capturing temporary payment delays or missed installments. This broader measure complements the NPL ratio by detecting early signs of credit deterioration before loans reach default. In Panel C, the dependent variable is the number of borrowers with a default on its check obligation within 24 months after loan origination. Standard errors clustered at the bank level in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%.

## Table A.8: Program effects on lending by borrowers' ARPK quartile

**A. Lending to entrepreneurs**

| | Female borrowers | | | | Male borrowers | | | |
|---|---|---|---|---|---|---|---|---|
| | Q1 | Q2 | Q3 | Q4 | Q1 | Q2 | Q3 | Q4 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| WiB Bank x Post | 1.461*** | 1.552*** | 1.434*** | 1.605*** | 1.045*** | 1.401*** | 1.414*** | 1.711*** |
| | (0.300) | (0.308) | (0.334) | (0.336) | (0.290) | (0.331) | (0.359) | (0.371) |
| Quarter x Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Bank x Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| $H_0$: Quartile = Quartile 1 | | 0.144 | 0.863 | 0.149 | | 0.046 | 0.031 | 0.012 |
| $R^2$ | 0.886 | 0.887 | 0.869 | 0.886 | 0.848 | 0.836 | 0.841 | 0.844 |
| N | 2,625 | 2,625 | 2,625 | 2,625 | 2,625 | 2,625 | 2,625 | 2,625 |
| Mean dep. var. | 6.303 | 6.173 | 6.147 | 6.100 | 9.297 | 9.114 | 8.981 | 8.990 |

**B. Number of entrepreneurs**

| | Female borrowers | | | | Male borrowers | | | |
|---|---|---|---|---|---|---|---|---|
| | Q1 | Q2 | Q3 | Q4 | Q1 | Q2 | Q3 | Q4 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| WiB Bank x Post | 0.718*** | 0.718*** | 0.682*** | 0.681*** | 0.849*** | 0.947*** | 0.956*** | 0.966*** |
| | (0.140) | (0.142) | (0.148) | (0.128) | (0.187) | (0.188) | (0.192) | (0.197) |
| Quarter x Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Bank x Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| $H_0$: Quartile = Quartile 1 | | 0.996 | 0.574 | 0.237 | | 0.029 | 0.027 | 0.019 |
| $R^2$ | 0.950 | 0.946 | 0.946 | 0.952 | 0.948 | 0.941 | 0.944 | 0.940 |
| N | 2,625 | 2,625 | 2,625 | 2,625 | 2,625 | 2,625 | 2,625 | 2,625 |
| Mean dep. var. | 2.951 | 2.891 | 2.851 | 2.812 | 4.745 | 4.713 | 4.628 | 4.629 |

Notes: This table estimates program effects on lending by borrower's ARPK quartile in a given quarter. Quartile cutoffs are based on the pre-treatment ARPK distribution in 2014, where ARPK is defined as the log ratio of total sales to fixed assets. Columns (1)–(4) estimate Equation (1) for female borrowers by each quartile separately; columns (5)–(8) estimate the same specification for male borrowers. All specifications use the stacked difference-in-differences estimator of Cengiz et al. (2019). Standard errors clustered at the bank level in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%.

## Table A.9: Program effects on ARPK distribution of borrowers

| | Female borrowers | | | | All borrowers | | | |
|---|---|---|---|---|---|---|---|---|
| | Mean | Median | IQR | P90-P10 | Mean | Median | IQR | P90-P10 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| WiB x Post | 0.104* | 0.079 | 0.316*** | 0.622*** | | | | |
| | (0.060) | (0.062) | (0.078) | (0.159) | | | | |
| WiB x Post x Female | | | | | −0.053 | −0.083 | 0.132** | 0.326** |
| | | | | | (0.055) | (0.051) | (0.065) | (0.146) |
| Bank x Cohort FE | Yes | Yes | Yes | Yes | No | No | No | No |
| Quarter x Cohort FE | Yes | Yes | Yes | Yes | No | No | No | No |
| Bank x Quarter x Cohort FE | No | No | No | No | Yes | Yes | Yes | Yes |
| Gender x Quarter x Cohort FE | No | No | No | No | Yes | Yes | Yes | Yes |
| Bank x Gender x Cohort FE | No | No | No | No | Yes | Yes | Yes | Yes |
| $R^2$ | 0.250 | 0.260 | 0.449 | 0.558 | 0.699 | 0.705 | 0.713 | 0.789 |
| N | 2,237 | 2,237 | 2,237 | 2,237 | 4,460 | 4,460 | 4,460 | 4,460 |
| Mean dep. var. | −0.132 | −0.251 | 1.756 | 3.427 | −0.150 | −0.272 | 1.909 | 3.811 |

Notes: This table estimates program effects on moments of the average revenue product of capital (ARPK) distribution among a bank's borrowers in a given quarter. ARPK is defined as the log ratio of total sales to fixed assets. Columns (1)–(4) estimate Equation (1) for female borrowers only; columns (5)–(8) estimate this equation for both genders, where the coefficient on WiB $\times$ Post $\times$ Female identifies the differential effect on female relative to male entrepreneurs. Within each set, the dependent variables are the mean (columns 1 and 5), median (columns 2 and 6), interquartile range (IQR: difference between the 75th and 25th percentiles; columns 3 and 7), and the 90–10 spread (P90–P10: difference between the 90th and 10th percentiles; columns 4 and 8) of the ARPK distribution. The IQR and P90–P10 measures capture the dispersion of capital productivity across a bank's flow of new borrowers, where a positive coefficient indicates that the program widened the ARPK distribution (consistent with treated banks extending credit to a broader range of female entrepreneurs in terms of capital productivity). All specifications use the stacked difference-in-differences estimator of Cengiz et al. (2019). Standard errors clustered at the bank level in parentheses. ***, **, and * denote significance at 1%, 5%, and 10%.

**Table A.10: Persistence of bank-firm relationships**

| Dependent variable: | New loan | | | |
|---|---|---|---|---|
| Sample: | All possible firm-bank relationship pairs | | | |
| | (1) | (2) | (3) | (4) |
| Pre-existing relationship | 0.980*** | 0.993*** | 0.898*** | 0.911*** |
| | (0.000) | (0.000) | (0.000) | (0.000) |
| R-squared | 0.480 | 0.487 | 0.525 | 0.530 |
| Observations | 14,017,850 | 14,017,850 | 14,017,850 | 14,017,850 |
| District FE | Yes | No | Yes | No |
| Industry FE | Yes | No | Yes | No |
| Year FE | Yes | Yes | Yes | Yes |
| Bank FE | No | No | Yes | Yes |
| Firm FE | No | Yes | No | Yes |

Notes: This table shows estimates from a regression of an indicator variable equal to 1 if firm $i$ takes a new loan from bank $b$ at time $t$, and 0 otherwise, on an indicator variable equal to 1 if firm $i$ had a preexisting relationship with bank $b$ at time $t-1$, and 0 otherwise. The sample includes all possible bank-firm relationship pairs (that is, for each firm and year in the sample, there is an observation for each bank). Standard errors are clustered at the firm level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

**Table A.11: Bank-level credit supply and lending to female entrepreneurs**

| Dependent variable: | $\Delta$(log) Credit to female entrepreneur | | | |
|---|---|---|---|---|
| Sample: | All firms | | Multi-lender firms | |
| | (1) | (2) | (3) | (4) |
| $\Delta \log L_{b,-ds,t}$ | 0.195*** | 0.189** | 0.268*** | 0.279*** |
| | (0.070) | (0.087) | (0.073) | (0.063) |
| R-squared | 0.025 | 0.245 | 0.188 | 0.456 |
| Observations | 783,476 | 703,034 | 253,670 | 217,695 |
| District FE | Yes | No | No | No |
| Industry FE | Yes | No | No | No |
| Year FE | Yes | Yes | Yes | No |
| Firm FE | No | Yes | Yes | No |
| Firm-year FE | No | No | No | Yes |

Notes: The sample includes all existing bank-firm relationship pairs. Columns (1)–(2) report results for all firms, while columns (3)–(4) restrict the sample to firms with multiple lenders. Standard errors are clustered at the firm level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

**Table A.12: Credit-supply shocks by WiB participation and female firms' business networks**

| | Value | HHI | Industry HHI | Centrality | Female share |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| **Panel A: Supplier network** | | | | | |
| WiB $\times \Delta \hat{L}_{idst}$ | 0.193*** | 0.002 | −0.019 | 0.058* | 0.017 |
| | (0.067) | (0.016) | (0.012) | (0.030) | (0.014) |
| Non-WiB $\times \Delta \hat{L}_{idst}$ | 0.019 | 0.007 | 0.014* | 0.003 | −0.006 |
| | (0.045) | (0.010) | (0.008) | (0.018) | (0.009) |
| Firm FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.825 | 0.676 | 0.818 | 0.763 | 0.187 |
| $N$ | 48,774 | 49,094 | 47,218 | 49,094 | 27,239 |
| Mean dep. var. | 5.994 | 0.488 | 0.649 | 0.715 | 0.003 |
| F-stat: WiB = Non-WiB | 6.617 | 0.102 | 7.237 | 3.024 | 3.090 |
| $p$-value | [0.010] | [0.749] | [0.007] | [0.082] | [0.079] |
| **Panel B: Customer network** | | | | | |
| WiB $\times \Delta \hat{L}_{idst}$ | 0.174** | −0.049*** | −0.028** | 0.045** | 0.091*** |
| | (0.083) | (0.019) | (0.014) | (0.021) | (0.019) |
| Non-WiB $\times \Delta \hat{L}_{idst}$ | −0.021 | 0.001 | −0.009 | 0.021 | 0.001 |
| | (0.060) | (0.013) | (0.010) | (0.025) | (0.017) |
| Firm FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.873 | 0.704 | 0.808 | 0.762 | 0.290 |
| $N$ | 49,094 | 49,094 | 41,385 | 48,774 | 22,761 |
| Mean dep. var. | 5.020 | 0.520 | 0.673 | 0.453 | −0.023 |
| F-stat: WiB = Non-WiB | 5.083 | 6.702 | 1.872 | 0.911 | 18.981 |
| $p$-value | [0.024] | [0.010] | [0.171] | [0.340] | [0.000] |

Notes: This table reports estimates of Equation (4) for network outcomes of female-owned firms. Each column reports a separate regression of a firm-level network measure on the WiB credit-supply shock and the non-WiB credit-supply shock, including firm and year fixed effects. Panel A constructs the firm-to-firm trade network from supplier declarations; Panel B from customer declarations. Column (1) reports the log total transaction value. Column (2) reports the Herfindahl-Hirschman index (HHI) based on partner shares; column (3) reports the HHI based on the industry composition of partners; lower values indicate more diversified trade. Column (4) reports PageRank centrality, where higher values indicate a more central position in the trade network. Column (5) reports the share of a firm's trade partners employees that are female. See Appendix A for full definitions. The $F$-test tests WiB = Non-WiB; $p$-values in brackets. Standard errors clustered at the firm level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

## Table A.13: Credit-supply shocks by WiB participation and female firm outcomes: Firm-size heterogeneity

| Split by: Initial Sales | Investment | | ΔCOGS | | ΔSales | | ΔProfit | |
|---|---|---|---|---|---|---|---|---|
| | Below (1) | Above (2) | Below (3) | Above (4) | Below (5) | Above (6) | Below (7) | Above (8) |
| WiB $\times \Delta \hat{L}_{idst}$ | −0.013 | 0.255*** | −0.006 | 0.312** | 0.114* | 0.147*** | 1.315** | 0.624 |
| | (0.081) | (0.084) | (0.174) | (0.148) | (0.063) | (0.056) | (0.593) | (0.391) |
| Non-WiB $\times \Delta \hat{L}_{idst}$ | −0.059 | 0.085 | −0.206** | 0.065 | −0.069* | 0.023 | −0.037 | 0.592*** |
| | (0.058) | (0.052) | (0.093) | (0.081) | (0.039) | (0.043) | (0.326) | (0.229) |
| R-squared | 0.275 | 0.244 | 0.221 | 0.229 | 0.289 | 0.323 | 0.173 | 0.191 |
| Observations | 25,669 | 25,673 | 25,669 | 25,673 | 25,669 | 25,673 | 25,669 | 25,673 |
| Mean dep. var. | 0.097 | 0.108 | 0.079 | 0.020 | 0.066 | 0.038 | −0.203 | −0.174 |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| $p$-value $H_0$: Above = Below | | [0.022] | | [0.164] | | [0.695] | | [0.331] |

| Split by: Initial Sales | Exit | | ΔCustomers | | ΔSuppliers | | ΔEmployment | |
|---|---|---|---|---|---|---|---|---|
| | Below (1) | Above (2) | Below (3) | Above (4) | Below (5) | Above (6) | Below (7) | Above (8) |
| WiB $\times \Delta \hat{L}_{idst}$ | −0.015 | −0.037** | 0.139 | 0.114* | 0.115 | 0.203*** | 0.172*** | 0.146*** |
| | (0.018) | (0.017) | (0.090) | (0.069) | (0.076) | (0.062) | (0.065) | (0.055) |
| Non-WiB $\times \Delta \hat{L}_{idst}$ | −0.012 | −0.008 | −0.046 | 0.070 | 0.076 | 0.059 | 0.015 | 0.087** |
| | (0.012) | (0.012) | (0.056) | (0.044) | (0.055) | (0.046) | (0.041) | (0.038) |
| R-squared | 0.386 | 0.365 | 0.222 | 0.278 | 0.214 | 0.238 | 0.211 | 0.198 |
| Observations | 25,669 | 25,673 | 17,028 | 22,308 | 21,598 | 24,787 | 17,942 | 21,562 |
| Mean dep. var. | 0.038 | 0.028 | 0.021 | −0.008 | −0.003 | −0.026 | −0.013 | −0.020 |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| $p$-value $H_0$: Above = Below | | [0.374] | | [0.826] | | [0.370] | | [0.760] |

Notes: This table shows coefficient estimates of Equation (4), estimated separately for firms with initial sales below and above the median. The dependent variable is indicated on top of each pair of columns and defined in Appendix A. The last row reports $p$-values from an F-test of the null hypothesis that the WiB credit-supply shock coefficients are equal across the two subsamples. Standard errors are clustered at the firm level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

## Table A.14: Firm-level credit-supply shocks by WiB participation and dynamic female firm outcomes

| Dependent variable: | Investment | ΔARPK | ΔCOGS | ΔSales | ΔProfit | Exit | ΔCustomers | ΔSuppliers | ΔEmployment | ΔMale employment | ΔFemale employment | ΔTotal wage bill | ΔMale wage bill | ΔFemale wage bill |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) | (13) | (14) |
| **A. Two-year effects** | | | | | | | | | | | | | | |
| WiB $\times \Delta \hat{L}_{idst}$ | 0.119* | 0.035 | 0.132 | 0.163*** | 0.720* | -0.065*** | 0.132** | 0.210*** | 0.104** | 0.065 | 0.006 | 0.112** | -0.268 | 0.072 |
| | (0.071) | (0.081) | (0.139) | (0.046) | (0.406) | (0.017) | (0.062) | (0.052) | (0.052) | (0.041) | (0.036) | (0.048) | (0.194) | (0.255) |
| Non-WiB $\times \Delta \hat{L}_{idst}$ | -0.004 | -0.012 | -0.063 | -0.006 | -0.109 | -0.001 | 0.055 | 0.095*** | 0.017 | 0.028 | 0.017 | -0.008 | -0.024 | 0.110 |
| | (0.050) | (0.059) | (0.077) | (0.033) | (0.311) | (0.013) | (0.045) | (0.035) | (0.034) | (0.027) | (0.027) | (0.038) | (0.125) | (0.190) |
| R-squared | 0.410 | 0.397 | 0.387 | 0.457 | 0.312 | 0.598 | 0.403 | 0.384 | 0.356 | 0.351 | 0.333 | 0.365 | 0.332 | 0.318 |
| Observations | 46,788 | 46,385 | 48,916 | 48,413 | 48,916 | 51,342 | 36,575 | 43,410 | 35,333 | 35,333 | 35,333 | 35,333 | 35,333 | 35,333 |
| **B. Three-year effects** | | | | | | | | | | | | | | |
| WiB $\times \Delta \hat{L}_{idst}$ | 0.049 | 0.139 | 0.131 | 0.192*** | 0.395 | -0.033** | 0.075 | 0.165*** | 0.092* | 0.082** | 0.007 | 0.084 | 0.012 | 0.064 |
| | (0.080) | (0.091) | (0.141) | (0.053) | (0.385) | (0.016) | (0.062) | (0.050) | (0.052) | (0.039) | (0.041) | (0.058) | (0.215) | (0.287) |
| Non-WiB $\times \Delta \hat{L}_{idst}$ | -0.059 | 0.115* | -0.090 | 0.083* | 0.229 | 0.012 | 0.104** | 0.116*** | -0.005 | 0.025 | 0.001 | -0.002 | 0.068 | 0.080 |
| | (0.056) | (0.064) | (0.091) | (0.043) | (0.323) | (0.012) | (0.044) | (0.036) | (0.035) | (0.030) | (0.028) | (0.038) | (0.148) | (0.202) |
| R-squared | 0.501 | 0.481 | 0.504 | 0.552 | 0.378 | 0.863 | 0.489 | 0.501 | 0.459 | 0.454 | 0.437 | 0.460 | 0.422 | 0.415 |
| Observations | 42,731 | 42,212 | 44,917 | 44,271 | 44,917 | 51,342 | 33,661 | 39,829 | 32,041 | 32,041 | 32,041 | 32,041 | 32,041 | 32,041 |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: This table shows coefficient estimates of Equation (4). The dependent variable is indicated on top of each column and defined in Appendix A. ARPK: Average Revenue Product of Capital. COGS: Cost of Goods Sold. Standard errors are clustered at the firm level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

**Table A.15: Credit-supply shocks by WiB participation and female firm outcomes by initial ARPK**

| Dependent variable: | Investment (1) | ΔARPK (2) | ΔCOGS (3) | ΔSales (4) | ΔProfit (5) | Exit (6) | ΔCust. (7) | ΔSuppl. (8) | ΔEmp. (9) |
|---|---|---|---|---|---|---|---|---|---|
| WiB $\times \Delta \hat{L}_{idst}$ | 0.081 | 0.149* | 0.233 | 0.243*** | 1.725*** | −0.018 | 0.205*** | 0.105* | 0.203*** |
| | (0.062) | (0.081) | (0.159) | (0.053) | (0.509) | (0.016) | (0.069) | (0.062) | (0.054) |
| WiB $\times \Delta \hat{L}_{idst} \times$ ARPK | 0.086 | −0.325*** | −0.174 | −0.244*** | −1.751*** | −0.019 | −0.196** | 0.128* | −0.069 |
| | (0.097) | (0.117) | (0.174) | (0.065) | (0.532) | (0.020) | (0.087) | (0.074) | (0.070) |
| Non-WiB $\times \Delta \hat{L}_{idst}$ | −0.089** | 0.106* | −0.025 | 0.013 | 0.579* | −0.015 | 0.050 | 0.013 | 0.077* |
| | (0.038) | (0.056) | (0.095) | (0.039) | (0.349) | (0.012) | (0.044) | (0.055) | (0.040) |
| Non-WiB $\times \Delta \hat{L}_{idst} \times$ ARPK | 0.198*** | −0.294*** | −0.111 | −0.077 | −0.632* | 0.011 | −0.067 | 0.104* | −0.038 |
| | (0.074) | (0.085) | (0.110) | (0.049) | (0.357) | (0.015) | (0.059) | (0.059) | (0.052) |
| R-squared | 0.259 | 0.247 | 0.223 | 0.305 | 0.179 | 0.377 | 0.251 | 0.226 | 0.204 |
| Observations | 51,342 | 51,342 | 51,342 | 51,342 | 51,342 | 51,342 | 39,336 | 46,385 | 39,504 |
| Mean dep. var. | 0.103 | −0.050 | 0.050 | 0.052 | −0.189 | 0.033 | 0.004 | −0.015 | −0.018 |
| F-stat WiB $\times \Delta \hat{L}_{idst}$ = Non-WiB $\times \Delta \hat{L}_{idst}$ | 7.064 | 0.250 | 2.196 | 15.864 | 4.204 | 0.049 | 4.463 | 1.624 | 4.771 |
| $p$-value F-stat | 0.008 | 0.617 | 0.138 | 0.000 | 0.040 | 0.825 | 0.035 | 0.203 | 0.029 |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: This table shows coefficient estimates of Equation (4). The dependent variable is indicated on top of each column and defined in Appendix A. ARPK: Average Revenue Product of Capital. COGS: Cost of Goods Sold. Standard errors are clustered at the firm level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

### Table A.16: Loan officer training and lending to female entrepreneurs

| | All borrowers | Repeat borrowers | Poached borrowers | First-time borrowers |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| **A. Lending to entrepreneurs** | | | | |
| Post x Trained x Female entrepreneur | -0.122 | -0.074 | 0.369** | -0.039 |
| | (0.131) | (0.152) | (0.162) | (0.137) |
| R-squared | 0.911 | 0.899 | 0.822 | 0.814 |
| Observations | 13,351 | 13,351 | 13,351 | 13,351 |
| Mean dep. var. | 6.124 | 5.733 | 3.502 | 3.279 |
| **B. Number of entrepreneurs** | | | | |
| Post x Trained x Female entrepreneur | 0.070 | 0.060 | 0.201*** | 0.128*** |
| | (0.046) | (0.049) | (0.054) | (0.042) |
| R-squared | 0.959 | 0.953 | 0.877 | 0.881 |
| Observations | 13,351 | 13,351 | 13,351 | 13,351 |
| Mean dep. var. | 2.502 | 2.222 | 1.050 | 1.010 |
| Bank x District x Quarter FE | Yes | Yes | Yes | Yes |
| Bank x Gender x Quarter FE | Yes | Yes | Yes | Yes |

Notes: This table shows estimates, using the stacking method of Gormley and Matsa (2011) and Cengiz et al. (2019), of the impact of the district-by-district roll-out of the TurWiB training program for loan officers. The estimates are based on data for the three (out of five) participant banks for which sufficiently granular data on the training roll-out is available. The dependent variable is (log) lending to entrepreneurs in Panel A and (log) number of entrepreneurs with access to credit in Panel B. Column (1) reports totals for all entrepreneurs, while the remaining columns report totals by type of borrower. Standard errors are clustered at the district level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

**Table A.17: Credit-supply shocks and female firm outcomes, IV estimates**

| Dependent variable: | Investment | ΔARPK | ΔCOGS | ΔSales | ΔProfit | Exit | ΔCustomers | ΔSuppliers |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Log-change in WiB credit | 0.120** | -0.013 | 0.125 | 0.120*** | 0.960*** | -0.028** | 0.116** | 0.175*** |
| | (0.060) | (0.073) | (0.114) | (0.044) | (0.360) | (0.013) | (0.057) | (0.048) |
| Log-change in Non-WiB credit | 0.012 | -0.072 | -0.145 | -0.051 | 0.379 | -0.014 | 0.024 | 0.108* |
| | (0.067) | (0.077) | (0.102) | (0.048) | (0.335) | (0.014) | (0.059) | (0.056) |
| $R^2$ | 0.007 | 0.003 | -0.008 | -0.016 | -0.012 | -0.000 | -0.008 | -0.018 |
| Observations | 51,342 | 51,342 | 51,342 | 51,342 | 51,342 | 51,342 | 39,336 | 46,385 |
| Mean dep. var. | 0.103 | -0.050 | 0.050 | 0.052 | -0.189 | 0.033 | 0.004 | -0.015 |
| F-stat $H_0$: WiB = Non-WiB | 2.487 | 0.516 | 4.581 | 12.669 | 2.249 | 1.030 | 2.249 | 1.517 |
| $p$-value F-stat | 0.115 | 0.473 | 0.032 | 0.000 | 0.134 | 0.310 | 0.134 | 0.218 |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: This table shows second-stage coefficient estimates of an IV regression for different firm-level outcomes as dependent variables. The dependent variable is indicated on top of each column and defined in Appendix A. ARPK: Average Revenue Product of Capital. COGS: Cost of Goods Sold. The first stage follows Equation (4) with yearly credit growth for firms working with WiB and Non-WiB banks separately as the endogenous variables and the credit-supply shocks from WiB and non-WiB banks serving as instruments. Standard errors are clustered at the firm level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

**Table A.18: Credit-supply shocks by WiB participation and employment outcomes of female firms, IV estimates**

| Dependent variable: | $\Delta$Employment (1) | $\Delta$Male employment (2) | $\Delta$Female employment (3) | $\Delta$Total wage bill (4) | $\Delta$Total male wage bill (5) | $\Delta$Total female wage bill (6) |
|---|---|---|---|---|---|---|
| WiB $\times\Delta$Credit | 0.189*** | 0.134*** | 0.066* | 0.143*** | -0.031 | 0.490* |
| | (0.049) | (0.039) | (0.037) | (0.054) | (0.179) | (0.257) |
| Non-WiB $\times\Delta$Credit | 0.095* | 0.096** | 0.016 | 0.075 | 0.292 | 0.174 |
| | (0.053) | (0.042) | (0.039) | (0.064) | (0.186) | (0.259) |
| $1^{st}$-stage F-statistic | 47.092 | 47.092 | 47.092 | 47.092 | 47.092 | 47.092 |
| N | 39,504 | 39,504 | 39,504 | 39,504 | 39,504 | 39,504 |
| Mean | -0.018 | -0.017 | 0.003 | 0.130 | 0.072 | 0.110 |
| F-stat WiB = Non-WiB | 3.239 | 0.821 | 1.653 | 1.321 | 2.641 | 1.321 |
| $p$-value F-stat | 0.072 | 0.365 | 0.199 | 0.250 | 0.104 | 0.251 |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: This table shows second-stage coefficient estimates of an IV regression for different firm-level outcomes as dependent variables. The dependent variable is indicated on top of each column and defined in Appendix A. The first stage follows Equation (4) with yearly credit growth for firms working with WiB and Non-WiB banks separately as the endogenous variables and the credit-supply shocks from WiB and non-WiB banks serving as instruments. Standard errors are clustered at the firm level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

**Table A.19: Blended finance and real outcomes for first-time female borrowers**

| Dependent variable: | Number of loans | Loan amount | Δ Investment | Δ ARPK | Δ COGS | Δ Sales | Δ Profit | Exit | Δ Customers | Δ Suppliers |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| First-time WiB borrower | 1.695*** | 3.799*** | 0.038** | −0.011 | 0.074*** | 0.042*** | 0.202** | −0.013 | 0.051*** | 0.029* |
| | (0.172) | (0.285) | (0.015) | (0.025) | (0.018) | (0.015) | (0.081) | (0.009) | (0.013) | (0.017) |
| R-squared | 0.309 | 0.396 | 0.084 | 0.084 | 0.086 | 0.083 | 0.059 | 0.065 | 0.139 | 0.122 |
| Observations | 32,385 | 32,385 | 22,192 | 21,898 | 29,536 | 28,917 | 29,536 | 32,385 | 18,601 | 24,734 |
| Mean dep. var. | 0.353 | 0.851 | 0.154 | −0.084 | −0.026 | 0.022 | −0.507 | 0.085 | −0.043 | −0.078 |
| District × Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Quarter × Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: The unit of observation is a firm obtaining its first bank loan during the treatment window. WiB borrower equals one if the firm's entry bank has joined the WiB program by the time of the firm's first loan. Columns 1–2 measure credit outcomes over 8 quarters after entry: cumulative number of loans from treated banks, total amount of credit from treated banks. Columns 3–10 report one-year differences from the entry year for real outcomes. All regressions include district × quarter × cohort fixed effects with standard errors clustered at the bank × cohort level. *, **, *** denote significance at 10%, 5%, and 1%.

**Table A.20: Spillover effects on lending to female entrepreneurs**

| | All | | | | Repeat | Poached | First time |
|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| WiB | 0.603*** | 0.643*** | 0.651*** | 0.629*** | 0.488*** | 0.979*** | 0.505** |
| | (0.058) | (0.056) | (0.057) | (0.127) | (0.148) | (0.207) | (0.201) |
| WiB × Lending share | | | | 0.591 | 0.542 | 0.031 | -0.043 |
| | | | | (0.426) | (0.498) | (0.643) | (0.661) |
| (1 - WiB) × Lending share | | | | 0.158 | 0.051 | 0.724* | -0.134 |
| | | | | (0.212) | (0.247) | (0.399) | (0.310) |
| Population density | No | Yes | Yes | Yes | Yes | Yes | Yes |
| Rural share | No | No | Yes | Yes | Yes | Yes | Yes |
| $R^2$ | 0.019 | 0.085 | 0.086 | 0.082 | 0.079 | 0.031 | 0.018 |
| N | 4,971 | 4,966 | 4,966 | 4,597 | 4,597 | 4,597 | 4,597 |

Notes: This table presents OLS estimates of spillover effects on lending to women entrepreneurs at the bank-year-district level, following Berg et al. (2021). The dependent variable is the log difference (2014–2019) in new loan issuance to women entrepreneurs. Columns 1-3 show baseline estimates without spillovers, progressively adding controls for population density and rural share. Columns 4-7 estimate the full spillover model for all lending (column 4) and by borrower type: repeat (column 5), poached (column 6), and first-time (column 7). *WiB* indicates whether a bank is ever treated. *Lending share* is the leave-one-out market share of treated banks in a district in 2014. *(1 - WiB)* × *Lending share* captures spillovers to non-treated banks. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.

**Table A.21: Spillover analysis - Number of first-time female borrowers**

| | First-time borrowers | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| WiB | 0.114*** | 0.127*** | 0.130*** | 0.133** |
| | (0.025) | (0.025) | (0.025) | (0.057) |
| WiB × Lending share | | | | −0.156 |
| | | | | (0.196) |
| (1 - WiB) × Lending share | | | | −0.100 |
| | | | | (0.114) |
| Population density | No | Yes | Yes | Yes |
| Rural share | No | No | Yes | Yes |
| $R^2$ | 0.003 | 0.013 | 0.013 | 0.016 |
| N | 4,971 | 4,966 | 4,966 | 4,597 |

*Notes:* This table presents OLS estimates of spillover effects on the number of first-time women borrowers at the bank-year-district level, following Berg et al. (2021). The dependent variable is the log difference (2014–2019) in the number of first-time female borrowers. Columns 1-3 show baseline estimates without spillovers, progressively adding controls for population density and rural share. Column 4 estimate the full spillover model. *WiB* indicates whether a bank is ever treated. *Lending share* is the leave-one-out market share of treated banks in a district in 2014. *(1 - WiB)* × *Lending share* captures spillovers to non-treated banks. Standard errors clustered at the district level. *** p<0.01, ** p<0.05, * p<0.1.