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Abstract

We study the real and allocative consequences of relaxing gender-specific credit constraints by analyzing a blended finance program that expanded 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 existing, poached, and first-time female borrowers. Beneficiary firms increase investment, employment, sales, and profits, diversify their business networks, and exit less. While treated banks also expand lending to male entrepreneurs, this increase is smaller and disproportionately directed toward higher-productivity firms, suggesting reallocation rather than crowding out. District-level effects on female entrepreneurship are absent, reflecting 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, credit constraints, female entrepreneurship, capital misallocation

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Blended Finance and Female Entrepreneurship*

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April 15, 2026

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We study the real and allocative consequences of relaxing gender-specific credit constraints by analyzing a blended finance program that expanded 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 existing, poached, and first-time female borrowers. Beneficiary firms increase investment, employment, sales, and profits, diversify their business networks, and exit less. While treated banks also expand lending to male entrepreneurs, this increase is smaller and disproportionately directed toward higher-productivity firms, suggesting reallocation rather than crowding out. District-level effects on female entrepreneurship are absent, reflecting 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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1 Introduction

In many low- and middle-income countries, women-owned enterprises are disproportionately concentrated at the lower end of the firm-size distribution, in part because female entrepreneurs face greater challenges when seeking external financing (Klapper and Parker, 2011; 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), direct 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 funding 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 approximately 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 (Eurodad, 2013).

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.

[1] Section 2 provides more details. See also OECD (2018) and Flammer, Giroux, and Heal (2024).

[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).

The Women in Business (WiB) program was rolled out during 2015–2017 and combined DFI credit lines to five commercial banks with a risk-mitigation mechanism and technical assistance. The program caused a sudden positive credit-supply shock to women-owned businesses and we trace in detail the financial and real impacts of this shock by combining four administrative datasets. We start with the Turkish credit registry, which has a minimal reporting threshold and covers nearly all loans. It 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), comprehensive VAT transaction data covering buyer-supplier links, and matched employer-employee records containing 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 would be expected if pre-program exclusion reflected gender-based rationing rather than productivity-based screening? Second, which types of women-owned firms gain better access to credit, and does the program trigger reallocation away from relatively low-productivity male entrepreneurs (Morazzoni and Sy, 2022)? Third, what are the real economic impacts on beneficiary firms? In particular, do firm-level outcomes (such as capital deepening among high-productivity female borrowers and female wage gains) align with predictions from models of gender-based misallocation (Cuberes and Teignier, 2016; Chiplunkar and Goldberg, 2024; Ranasinghe, 2024)?

We start our analysis 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 was designed to engage banks with persistently low (but stable) female lending, meaning that selection, if anything, attenuates our estimates toward zero. Second, we provide balance tests showing that, while the treated banks were larger than non-participating banks, both groups were similar along many other pre-program traits. We conservatively condition on these traits in our regression framework. Third, we implement the Rambachan and Roth (2023) sensitivity framework and show that our estimates remain significant under arbitrary linear deviations and withstand 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 leverage the granular nature of the credit registry data to 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 existing borrowers that were still credit-constrained (intensive margin) while also crowding in new female borrowers (extensive margin).

We then ask whether the program-induced credit expansion came at the cost of loan quality. There is no evidence that it did: 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 supplier payment delays, 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-induced 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 existing borrowers, we follow Chodorow-Reich (2014) and Cong, Gao, Ponticelli, and Yang (2019) to construct a firm-specific measure of exposure to the plausibly exogenous credit shock caused by the program. We find that a 1 percent increase in the program-induced credit supply at the firm level translates into 0.87 percent of additional net borrowing, 0.12 percent more investment, and 0.17 percent more employment. Female wage bill effects concentrate among female employees (0.46 percent), driven primarily by higher per-worker wages rather than headcount expansion, with no significant effect on male wages. Firms also diversify their supplier and customer networks, and increase their sales and profits, on average, by 0.13 and 0.93 percent, respectively. Scaling to a policy-relevant magnitude, a 20 percent increase in credit from a WiB bank reduces exit probability by 0.56 percentage points, or 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 discussed 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 these firms lack pre-existing bank relationships, we adopt a cohort-based cross-sectional design that compares women who obtain their first-ever bank loan from a treated versus a control bank in the same district and quarter. First-time female borrowers at WiB banks show expanded credit access and favorable real outcomes, though the extensive-margin design is more exposed 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. We find that districts with greater exposure to treated banks experience significant increases in aggregate credit and the number of borrowers, but no effects on female firm entry, exit, or market share. This is consistent with a supply-side relaxation of credit constraints without a strong demand-side response. We do find significant increases in both male entry and exit rates, in line with competitive reallocation rather than simple crowding out. Aggregate real outcomes (sales, profits, employment) are unaffected for both genders, reflecting the program's modest scale relative to district-level aggregates.

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 (De Haas and González-Uribe, 2025). One approach is to implement reforms that improve credit markets in general, though the track record in terms of benefiting small firms is mixed.[3] A second approach is to rely on state banks: directing them to open branches in underserved regions (Burgess and Pande, 2005; Fonseca and Matray, 2024) or to lend more to specific firm segments (Banerjee and Duflo, 2014).[4] Yet state bank lending is often distorted by political interference.[5]

Our contribution is to estimate the impact of blended finance, a popular but understudied financial inclusion policy, and to provide well-identified evidence 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 those with existing bank access, including many with a high revenue product of capital. Relaxing these constraints generates firm-level gains in investment, employment, and profitability, while enabling capacity expansion among previously constrained high-productivity firms, as reflected in convergence of their capital productivity. These findings also connect to models of how financial frictions restrain the entry and growth 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,[6] more recent research traces the firm-level effects of *positive* shocks from stimulus packages or credit facilities.[7] 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-

[3]Examples include strengthening creditor rights (La Porta, Lopez-de Silanes, Shleifer, and Vishny, 1998) and collateral laws (Calomiris, Larrain, Liberti, and Sturgess, 2017); introducing credit registries (Pagano and Jappelli, 1993); and allowing foreign bank entry (Claessens and Laeven, 2004).

[4]Relatedly, Agarwal, Kigabo, Minoiu, Presbitero, and Silva (2021) show that a state-subsidized geographic expansion of savings and credit cooperatives in Rwanda served as an entry point for first-time borrowers, some of which subsequently also received credit from commercial banks.

[5]See La Porta, Lopez-de Silanes, and Shleifer (2002), Dinç (2005), Khwaja and Mian (2005), Carvalho (2014), and Bircan and Saka (2021).

[6]For example, Chava and Purnanandam (2011), Schnabl (2012), Chodorow-Reich (2014), and Beck, Degryse, De Haas, and Van Horen (2018).

[7]Such as Paravisini (2008), Brown and Earle (2017), and Cong et al. (2019).

level gains, suggesting that the type of lending matters, not just its quantity.

Third, we provide new 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).[8]

2 Institutional background

Launched in 2014, the Women in Business (WiB) program was a blended finance program set up by the European Union, the EBRD, the Turkish Ministry of Labor and Social Security, and the Turkish employment agency İşkur. Its goal was to enable and stimulate Turkish banks to expand lending to women-owned small businesses, especially outside the metropolitan areas of Ankara, İstanbul, and İzmir. The program was developed in recognition of the large and persistent gender gap in financial access across Turkey. According to data from the Global Findex Database 2021, for example, Turkish men are more than twice as likely as Turkish women to borrow from a bank. While part of this gap reflects gender differences in the demand for financial services, supply-side constraints also play an important role. Brock and De Haas (2023) document the empirical relevance of such supply-side frictions. Using a lab-in-the-field experiment with Turkish loan officers, they show that identical loan applications are evaluated more stringently when presented as coming from female applicants, with loan

[8] 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).

officers imposing more demanding guarantor requirements (reflecting implicit gender biases). The blended finance program was designed to address such frictions.

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 credit lines to five banks for a total of EUR 300 million. The participating banks committed to on-lend these funds to women-owned small firms. They also had to supplement these credit lines with their own funding by a factor of 0.4 to expand lending further. 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 followed a three-stage process. First, candidate banks underwent due diligence to assess financial soundness: adequate capital and liquidity, proper management, and independence from political interference. The program also focused on relatively large banks with nationwide branch networks. 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, selected banks underwent baseline assessments by contracted consultants (Frankfurt School of Finance & Management) to identify institutional barriers that the program aimed to address (e.g., limited dedicated liquidity for female borrowers or loan officers' reluctance to serve what they perceived as higher-risk borrowers).

Figure 1 shows the district-level share of bank branches operated by WiB banks as of end-2014. Where present, participant banks control between 13 and 60 percent of all branches in a district. 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, recognizing that commercial banks may lack the experience to screen particular borrower types and to lend to them profitably (Tahir, Girod, Rex, and Belot, 2021). Consultants analyzed each bank's existing approach and designed tailored packages covering, for example, gender-responsive sales and marketing; online modules on gender awareness; and management information systems to gather gender-disaggregated data. Banks were also supported in developing products catering to women entrepreneurs, including loans with longer grace periods and flexible collateral requirements (accepting jewelry, gold, and chattel mortgages of business assets). Training for loan officers also covered negotiation techniques tailored to women business owners and detection of fraudulent female-fronted applications. While all loan officers were informed about the program from day one via bank-wide launch events, the timing of their training varied across districts due to capacity constraints on the part of the pool of trainers. We exploit this variation in Section 5.2.3. To sustain these practices beyond the program period, banks also received training-of-trainers modules.

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.[9]

The credit registry also records bounced commercial checks. This constitutes an impor-

[9]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”.

tant 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 information on domestic firm networks, collected by the Turkish Ministry of Treasury and Finance for the purpose of calculating VAT. These VAT data cover all domestic firm-to-firm transactions whenever the total transaction value exceeds 5,000 Turkish liras in a given year. This low threshold means we observe the vast majority of buyer-supplier links in Turkey.

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 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 1 presents summary statistics for the women-owned firms in the merged firm-level dataset. The average firm owns assets worth 1.04 million liras (USD 350k at average 2016 exchange rates) of which 15 percent are fixed assets. 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. Total wage expenditure averages 8,000 liras, with male wages nearly twice female wages, largely reflecting the gender composition of the workforce.

We calculate the average revenue product of capital (ARPK), a proxy for a firm's capital productivity (Hsieh and Klenow, 2009), as the log ratio between total sales and fixed assets. We document substantial variation in firms' ARPK, with those at the 75th percentile of the capital productivity distribution displaying an ARPK 2.7 times that of firms at the 25th percentile. This suggests substantial capital misallocation among the firms we study. A natural hypothesis to test (Midrigan and Xu, 2014) is whether the blended finance program allowed firms with higher capital productivity to grow more, thus gradually reducing the cross-firm dispersion in ARPK.

Several potentially relevant dimensions are not available in our data. 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 (only an indicator for collateralization); or reliable firm founding dates for small firms. These limitations mean 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. The available data nevertheless allow us to analyze credit access, capital allocation, and real firm outcomes in considerable detail.

3.2 Selection into the program

Our main identification strategy, discussed in Section 4.1.1 below, exploits the staggered rollout of the blended finance program across five treated banks. We compare the lending dynamics of these banks with those of 21 control ones: similar Turkish banks that were not in the program.[10] An advantage of this difference-in-differences setup is that identification relies

[10]These are all other commercial banks that operated in Turkey during our sample period. We exclude investment banks, development banks, and very small banks.

on the parallel trends assumption rather than on random assignment (Ghanem, Sant'Anna, and Wüthrich, 2022). This allows for level differences across banks but requires that program entry timing was not correlated 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 the dates at which the five banks join the program are quasi-random (due to different negotiation dynamics and the internal bureaucratic checks that each bank had to clear with the DFI) it is nevertheless useful to check whether they differed strongly from the control banks in terms of observable characteristics prior to the program.

Table A.2 compares treated and control banks along several characteristics as of end-2014. The five treated banks are, on average, larger than the control banks. However, this difference is driven by their more prominent role in the credit market for large corporate borrowers. Despite their greater market shares in this market, treated banks' share in lending to small businesses is not significantly greater than that of control banks. Both groups also have similar shares of lending to women within that segment. Along various other dimensions, treated banks are quite similar to control banks, too. Both groups have comparable liquidity, profitability, non-performing loans (NPLs), loan-loss reserves, and capital adequacy ratios.

4 Identification

Our analysis proceeds in three stages. Section 4.1 tests whether the program expanded bank lending to female entrepreneurs. Section 4.2 then traces how these program-induced credit-supply shocks transmitted to firm-level outcomes for existing as well as first-time borrowers. Section 4.3 extends the analysis to the district level to capture equilibrium effects.

4.1 Bank-level analysis

4.1.1 Staggered difference-in-differences

We exploit the staggered introduction of the blended finance program across five banks to identify the effect on lending to women-owned firms. We aggregate the loan-level credit registry data to the bank (b)–quarter (t) level and estimate:

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

where y_{bt} is the flow of new loans to female entrepreneurs by bank b in quarter t. We distinguish between lending to existing 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. β_1 gives the average treatment effect on the treated (ATT). We saturate this model with time-varying bank controls X_{bt} (log total assets, liquidity, profitability, NPLs, capital adequacy ratio, and market shares in corporate credit) on top of bank (γ_b) and quarter (δ_t) fixed effects.

As shown in Figure 2A, 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; Borusyak, Jaravel, and Spiess, 2024; Callaway and Sant'Anna, 2021; 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.[11] 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, which is more conservative than including the fixed effects on their own (Gormley and Matsa,

[11] The control banks are those that never participated in the blended finance program.

2011). Standard errors are clustered by bank throughout. With only five treated clusters, we implement wild cluster bootstrap procedures (Cameron, Gelbach, and Miller, 2008), using Rademacher weights and the restricted estimator as recommended by Canay, Santos, and Shaikh (2021).

4.1.2 Synthetic difference-in-differences

Our identification assumption is that treated and control banks would have followed parallel trajectories in lending to female entrepreneurs absent the WiB program. Two threats could violate this: 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 this assumption, 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.1 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 (γ_i) absorb time-invariant firm characteristics, while district-year and bank-year fixed effects (δ_{dt} and θ_{bt}) control for common shocks. Any effect of $WiB_{m(i)}$ on firm outcomes thus operates through the pre-existing lending relationship, making the bank-level treatment directly traceable to downstream borrower outcomes.

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:

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

where ω 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 $\Delta\log L_{b,-ds,t}$ 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. Excluding same-district and same-sector firms avoids the exposure measure being affected by correlated credit demand shocks.

This identification strategy relies on two testable assumptions (Chodorow-Reich, 2014; Greenstone et al., 2020). 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. γ_i and δ_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 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. For each treated bank b with treatment date ft_b, the cohort includes all first-time female borrowers whose entry bank is either bank b or a never-treated bank, and who entered the credit system in the eight quarters following treatment, $[ft_b,\ ft_b + 8]$. We include entry-quarter $\times$ cohort fixed effects (α_z) and district $\times$ cohort fixed effects (α_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 (cumulative loan counts and amounts from treated banks) as well as growth in sales, investment, profits, and supplier and customer networks for all firms matched to tax records. The key 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:

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

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

5 Results

5.1 Bank-level results

5.1.1 Effects on lending to female entrepreneurs

Figure 2A 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. Once treated banks gain access to blended finance, they allocate progressively more credit to female entrepreneurs while nothing changes for control banks, gradually closing the gap in portfolio gender composition. Figure 2B 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. Treated banks, in contrast, show a persistent increase of approximately 1 percentage point, which is equivalent to 11 percent relative to the pre-program mean of 9 percent of the loan stock.[13]

We now estimate Equation (1) using the stacking (columns 1–4) and SDiD estimator (columns 5–8).[14] Table 2 considers four dependent variables: (log) new lending to female

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

[13] 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 1).

[14] We validate the SDiD estimator using two diagnostic tests. First, following Abadie (2021), we backdate

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). Columns 1 and 5 focus on lending to all types of borrowers, whereas columns 2–4 and 6–8 split the overall lending response into credit to repeat, poached, and first-time borrowers.

Panels A and B report effects on log lending volumes and log borrower counts. 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 total 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 points (15.3 percent) using SDiD.

Figure 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. Pre-treatment coefficients are centered around zero with no discernible trend, indicating that treated banks closely tracked

the program start by four quarters and verify that no effects appear before the actual intervention date (Appendix Figure A.4). 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.3 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.

their reweighted synthetic controls prior to the program. Divergence emerges at treatment onset and grows steadily over the post-treatment horizon.

Panel B shows the corresponding bank-specific ATTs. Before program entry, each of the five treated banks follows a parallel path relative to its synthetic control, confirming that the method successfully reproduces bank-level pre-program lending trajectories. Post-treatment, all five banks show positive effects, though with notable heterogeneity in timing and magnitude that likely reflects differences in capacity to absorb the program's training components. 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).[15]

Columns 2–4 (stacking) and 6–8 (SDiD) of Table 2 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 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

[15]The credit registry goes back to 2006, so we check each borrower's history going back to this year to determine if they are a first-time borrower or not.

with existing clients but also by attracting borrowers from competing banks and extending credit to previously unbanked firms.

How much of the increase in new lending to female entrepreneurs (relative to male ones) is driven by each of the three borrower types? To answer this, we reshape the data at the bank-quarter-gender level and estimate a triple-difference specification that includes bank $\times$ quarter and bank $\times$ gender fixed effects. The former absorb time-varying but gender-neutral bank shocks; the latter absorb time-invariant gender-specific lending propensities. The coefficient on Post $\times$ WiB Bank $\times$ Female entrepreneur then identifies the differential increase in lending to female relative to male entrepreneurs at treated banks.

Appendix Table A.4 reports estimates using the quarterly change in a bank's new lending by gender and borrower type, scaled by the bank's average total lending stock over the quarter, as dependent variable. This exercise shows that of the total increase in new lending to women relative to men, 54 percent (=0.037/0.068) is driven by repeat borrowers, 28 percent (=0.019/0.068) by poached clients, and the remaining 18 percent (=0.012/0.068) by first-time borrowers. Although repeat borrowers account for over half of the increase in lending volumes, they account for a smaller share of the growth in the *number* of entrepreneurs in treated banks' portfolios (41 percent, Panel B). The relative increase in the number of female entrepreneurs who gain access to credit is also due to borrowers poached from other banks (30 percent) and first-time borrowers (29 percent).

Lastly, Appendix Figure A.3 presents SDiD event-study estimates by borrower type, complementing the aggregate dynamics in Figure 3. Pre-treatment coefficients are centered around zero for all three categories, confirming that the synthetic controls reproduce pre-program trajectories across each margin. 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.

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.5 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 quite different. Treatment effects on lending 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.6 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, including both high- and low-ARPK entrepreneurs who had previously been excluded) 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 a reduction in gender-based capital misallocation (Morazzoni and Sy, 2022).

5.1.4 Assessing the identifying assumptions

The preceding results document large, broad-based effects on lending to female entrepreneurs. We now present additional evidence supporting the assumptions underlying these estimates.

Although treated banks were observationally similar to control banks before the program (except for their size), this does not guarantee that they were on parallel trends. Figures 2A and 2B already provided preliminary evidence that, in fact, they were. To assess this more formally, Panel A of Appendix Figure A.2 presents event-study estimates using the 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. This avoids the negative weighting problems of standard TWFE in staggered settings and providing a transparent aggregation of cohort-specific treatment effects.[16] Pre-treatment co-

[16] 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, for the Gardner et al. (2024) event-study specifications we report GMM-clustered standard errors, whose Monte Carlo evidence indicates near-nominal rejection rates even with

efficients center around zero, supporting parallel trends between treated and control banks. Treatment effects turn positive at program entry, increase steadily over the 12-quarter post-treatment horizon, and match closely the baseline estimate in column 1, Panel A, of Table 2.

To formally assess the sensitivity of our results to violations of parallel trends, we implement the Rambachan and Roth (2023) robust inference framework. Panel B of Appendix Figure A.2 reports fixed-length confidence intervals under the smoothness restriction $\Delta SD(M)$, where M parameterizes the maximum allowable change in the slope of the differential trend between consecutive periods. Our estimates remain statistically significant at $M = 0$, which permits arbitrary linear violations of parallel trends. As M increases, allowing for non-linear or accelerating differential trends, the robust confidence sets remain bounded away from zero, with a breakdown value of $M \approx 0.4$.

5.1.5 Collateral and repayment

The WiB program supported banks in streamlining loan approvals, including by expanding accepted collateral types and reducing collateral requirements. While aimed at female entrepreneurs, some measures benefited both sexes. Appendix Table A.7 tests for changes in collateral policies using a bank's share of uncollateralized lending as the dependent variable.[17] Columns 1–4 estimate our baseline specification for female borrowers; columns 5–8 add gender interactions. The first three columns of Table A.7 provide evidence of banks relaxing their collateral requirements for female entrepreneurs, though only for repeat and poached borrowers. The coefficient in column 2 indicates that, compared to control banks, treated banks increase their share of uncollateralised lending by 15.6 percent more. Yet, the results in columns 5–8 imply that male repeat borrowers benefit in equal measure.

We take away three messages from Table A.7. First, the results suggest that both female and male repeat and poached borrowers benefited from training and policy changes that

few treated units. 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.

[17] The credit registry does not contain information on the type of collateral that underpins loans.

reduced banks' reliance on collateral. Second, banks did not soften collateral requirements for first-time borrowers. This helps explain why a large part of the program impact occurred on the intensive margin. Third, these results speak against credit guarantees as the main driver of reduced collateral requirements, since the guarantees applied only to female borrowers yet we observe no gender differences in Table A.7.

Next, Table A.8 examines program impact on loan quality. Columns 1–4 estimate Equation 1 for female borrowers only, while columns 5–8 estimate a triple DiD , where the coefficient on Post $\times$ WiB Bank $\times$ Female identifies the differential effect on female relative to male entrepreneurs. Panel A considers non-performing loans (NPLs): loans overdue by at least 90 days or written off. Across all borrower types, estimated coefficients are close to zero and statistically insignificant. Panel B turns to Stage 2 loans. Under Basel III standards, these are loans that have experienced a significant increase in credit risk but have not yet defaulted (an indicator of early credit stress). The results again show no significant effects for any borrower group. Panel C examines the number of borrowers with a defaulted commercial check within 24 months after loan origination. Repeat borrowers at treated banks do experience an increase in supplier payment delays (columns 1–2, approximately 20-23 percent relative to the pre-program mean). The triple-difference estimates show that the increase in supplier payment delays is not gender-specific among repeat borrowers (column 6). For the aggregate borrower pool, the triple-difference coefficient is in fact negative and significant (column 5), indicating that female borrowers are less affected than male borrowers. Overall, this pattern is more consistent with short-run working capital pressures during business expansion than with credit misallocation.

5.2 Firm-level results

5.2.1 Banks' program participation and firm-level credit

This section links the bank-level credit-supply shocks to firm-level outcomes. The bank-level results establish that the WiB program expanded lending to female entrepreneurs; we now ask whether this expansion reached individual firms. To connect bank-level treatment to firm-level outcomes, we exploit pre-existing bank–firm relationships. Because these relationships are highly persistent in Turkey (Table A.9), a firm's pre-program main bank effectively determines its program exposure, allowing us to apply the same staggered identification used in Section 5.1 with firm-level outcomes as dependent variables.

Table 3 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 than their male counterparts, confirming that the program shifted the gender composition of lending at the firm level, not just in bank-level aggregates.

5.2.2 Firm-level credit-supply shocks and real outcomes

We trace program-induced credit-supply shocks to firm outcomes using the Chodorow-Reich (2014) approach described in Section 4.2.2, which exploits variation in each firm's pre-existing exposure to WiB and non-WiB banks. Two identifying conditions support this approach. First, pre-existing banking relationships are highly predictive of where firms borrow. Table A.9 shows a 98 percent probability of borrowing from a prior lender (column 1), with coefficients between 0.90 and 0.99 under progressively demanding fixed-effects specifications.[18] Second, Table A.10 confirms that cross-sectional variation in bank lending reflects supply rather than demand: point estimates remain stable when including firm fixed effects (Khwaja and Mian, 2005) and when restricting to multi-lender firms with firm $\times$ year fixed effects (columns 3–4), fully absorbing firm-specific demand shocks.

Having established that both assumptions hold, we now turn to the firm-level impact estimates. Column 1 of Table 4 confirms, first of all, 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. We find that the 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.[19]

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 ($= 0.727 +$

[18] The dataset in Table A.9 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.

[19] It is important to distinguish the non-WiB credit-supply shock coefficients from program spillovers. In Tables 4–8 and 10, the coefficients on non-WiB credit-supply shocks capture the standard transmission of idiosyncratic credit-supply variation at non-treated banks (e.g., changes in liquidity, capital, or internal lending policies) to firm outcomes. The positive relationship between these shocks and firm-level employment or investment reflects the elasticity of firm activity to credit supply and is not program-induced. To directly test for program-induced competitive spillovers, we implement the Berg, Reisinger, and Streitz (2021) framework, discussed in Section 5.3.

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 5 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 4. 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.[20] 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 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, Table 6 exploits the full VAT transaction matrix to examine whether program-induced credit-supply shocks affected the structure of female firms' commercial relationships. On the supplier side (Panel A), WiB credit-supply shocks increase the total transaction value, but neither concentration measure changes significantly, suggesting that input sourcing expands proportionally across existing sectors and partners. On the customer side (Panel B), treated firms similarly expand total transaction values. Both the industry HHI and the partner HHI decline significantly, indicating that treated firms diversify their sales across sectors and across individual trading partners. The concentration measures thus reveal an

[20] 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).

asymmetric pattern: credit-enabled growth primarily reshapes firms' downstream commercial relationships, allowing them to enter new industries and reach a broader set of customers, while on the supplier side, expansion preserves the pre-existing structure of input sourcing.

Treated firms also increase their centrality on both sides of the market, consistent with improved positioning within the production network. The effect is more precisely estimated on the customer side (column 4, Panel B), suggesting that credit-enabled growth helps firms integrate into more central segments of the trade network. Finally, treated firms see a significant increase in the average female employee share among their customers (column 5, Panel B) with no corresponding effect on the supplier side, suggesting that program-induced growth generates downstream commercial linkages with firms employing more women. None of these network effects appear for non-WiB credit-supply shocks.

In Table 7, we investigate the impact of program-induced credit shocks on firm-level employment. We find that 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 7. 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).

Figure 4 provides point estimates for WiB$\times\Delta\hat{L}_{idst}$ based on similar regressions as in Tables 5, 6, and 7. We now focus on dynamic effects by showing separate estimates for real impacts in the first, second, and third years after a firm experiences a positive credit supply when its lender(s) access the blended finance program. Overall, these dynamics make intuitive sense. Once a firm gets access to additional credit, investments and employment increase immediately in year one and two after which the effect dies out in year three for investments but stays positive for employment. Sales and profits increase in year one but, while sales continue to increase, the impact on profits is more short-lived. Business survival probability rises, but with some delay.

Appendix Table A.12 confirms these patterns. Investment effects remain elevated in year two but dissipate by year three, consistent with a transition to a higher capital stock. Sales and supplier diversification persist over the three-year horizon, with sales effects strengthening further, while employment remains marginally significant. Profit effects weaken over time and the female wage bill effect dissipates 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 7 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. That this attenuation reflects equilibrium adjustment rather than firm-level failure is confirmed by the persistence of supplier diversification and commercial network expansion: firms remain operationally integrated even as financial margins moderate, consistent with a transition toward a new equilibrium operating scale rather than the disappearance of program effects.

Table 4 showed that the program steered additional credit disproportionately toward above-median ARPK firms. In Table 8, we ask whether this productivity-dependent credit expansion translated into heterogeneous real effects. The investment interaction with initial ARPK is positive but imprecisely estimated (column 1), so we cannot conclude that high-ARPK firms invest differentially more through the program. However, column 2 shows that ARPK converges significantly for these firms. Combined with the financial results in Table 4, which show that the program helped above-median ARPK firms borrow more, this convergence indicates that these firms expanded their productive capacity, whether through fixed capital formation, working capital, or other channels not fully captured by the investment variable. The ARPK decline is consistent with previously constrained high-productivity firms expanding their capital stock, as predicted by Ranasinghe (2024). However, because ARPK is constructed as log sales over fixed assets, the convergence pattern is also consistent with a mechanical transition dynamic in which rapid fixed-asset expansion temporarily depresses the ratio even absent changes in underlying efficiency. We therefore interpret the pattern as evidence of capacity expansion by constrained firms rather than as a direct measure of allocative efficiency gains.

Columns 4 and 5 show that, while firms expanded their sales and profits due to the program, this was less the case for high-ARPK firms. Importantly, these are relative attenuations, not absolute declines: the baseline coefficients indicate significant increases in both sales and profits, with interaction terms that are almost exactly offsetting, so the implied net effects for above-median firms are close to zero. Lower-ARPK firms may have had more underutilized capacity and could translate additional credit into sales more immediately; the extended event-study evidence (Figure 4) supports this timing, as investment effects are strongest in years 1–2 while sales continue to increase over the three-year horizon. Column 9 shows that employment effects are not meaningfully moderated by initial ARPK.

We also examine whether treatment effects vary with pre-program firm size. In Appendix Table A.11, we estimate Equation 4 separately for firms with initial sales below and above the median. Investment effects concentrate among above-median firms, while profit effects are larger in magnitude among below-median firms, though this difference is not statistically significant. Employment effects are positive and significant across both groups, with comparable magnitudes. This pattern suggests that the program relaxed different binding constraints across the size distribution: smaller firms used additional credit primarily to ease working capital pressures and improve margins, while larger firms channeled it into fixed capital formation.

5.2.3 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 in the form of comprehensive loan officer training. While cleanly disentangling the individual contributions of these components is challenging, given the integrated design of the intervention, three observations provide suggestive evidence on their relative importance.

First, treatment effects persist over a three-year horizon, well beyond the formal subsidy period. Even after the credit lines had been repaid, treated banks continued lending more to female entrepreneurs than before the program and relative to synthetic controls, with no evidence of reversion to pre-program levels. This persistence suggests that improved access to liquidity for banks was not the primary driver of the impacts we document. We note, however, that persistence alone does not fully rule out the importance of liquidity support: if initial loans were profitable and well-repaid, banks could have generated internal funds and informational capital that sustained lending after repaying the credit lines.

Second, as discussed in Section 5.1.5, both female and male repeat borrowers at treated banks benefited from reduced collateral requirements. If the FLRC, which *only* applied to female borrowers, was the key driver of program impact, we would expect stronger gender differences in uncollateralized lending. The absence of such differences suggests the FLRC was not the main mechanism behind less stringent collateral requirements.

Third, and most directly, we leverage data on the district-by-district roll-out of loan officer training at three of the five treated banks. All loan officers were informed about the program from inception through bank-wide launch events and intranet pages. What varied across districts was the timing of intensive classroom training, which could not be implemented simultaneously due to capacity constraints. As discussed in Section 2, the training curriculum covered gender-informed credit assessment, relationship management, and marketing, with practical application through role-playing and case-study exercises.

In Appendix Table A.13, we employ a TWFE framework in which we compare the gender composition of lending by loan officers in already-trained districts versus loan officers of the same bank in not-yet-trained and never-trained districts. This specification includes both bank $\times$ district $\times$ quarter and bank $\times$ gender $\times$ quarter fixed effects. The coefficient of interest is a triple interaction term that identifies how female entrepreneurs are affected differentially compared to male entrepreneurs once a bank's loan officers in a specific district have received training. This demanding specification identifies the impact of training on gender-specific lending, conditional on bank participation in the overall program and abstracting from bank-wide effects.

Appendix Table A.13 shows that in trained districts, loan officers shifted their lending composition toward female entrepreneurs, particularly for poached and first-time borrowers (Panel B). Impacts on overall lending volume are less precisely estimated, with the exception of lending to poached borrowers (Panel A). Because all loan officers knew about the program's existence, liquidity provisions, and guarantee support from day one, the staggered training schedule identifies the incremental effect of skills-based technical assistance over and above the liquidity and guarantee components available to all branches from inception. 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 institutional context, than with purely demand-driven changes. These within-bank effects provide suggestive evidence that loan officer training was an important driver of the overall program impacts.

5.2.4 Quantifying the impact of the program

We now present instrumental variables (IV) estimates to further quantify the effect of access to credit from WiB banks and non-WiB banks on firm outcomes. In the first stage, we regress the change in (log) firm-level credit on the credit-supply shocks that we estimated earlier, differentiating between female entrepreneurs working with WiB banks and non-WiB banks. This first stage is analogous to column 2 of Table 4, except that we have two endogenous variables (change in credit from WiB banks and change in credit from non-WiB banks) instrumented by two credit-supply shocks coming from WiB banks and non-WiB banks. In a second stage, we relate the predicted changes in credit to changes in firm-level outcomes.

Appendix Tables A.14 and A.15 show results from this exercise, which we use to get a sense of the average increase in investment, employment, wage bill, sales, profit, exit, and number of suppliers caused by a Turkish lira in credit.[21] The average stock of bank credit for female entrepreneurs in the sample is TRY 250,000 (approximately USD 80,000 in 2016). Therefore, using the estimate in column 1 of Table A.14, an increase of TRY 45,802 (approximately USD 16,000 in 2016) in borrowing from a WiB bank corresponds to an increase of TRY 4,489 in gross fixed assets for a female entrepreneur on average. The same increase in borrowing leads to an increase of TRY 31,320 in sales (column 4) and TRY 37,248 in profits (column 5) for the average firm. Moreover, the average firm employs an additional 0.2 employees due to the program-related positive credit shock (Table A.15,

[21] Similar to the reduced-form estimates, the IV estimates do not reveal statistically significant effects for the remaining outcomes.

column 1), paying an additional TRY 249 in monthly wages (column 4).

As profits reflect a firm's earnings after interest payments, these numbers suggest that a lira of extra loans by WiB banks to their female clients increases the average firm's profits net of interest by 81.3 percent (=37,248/45,802). We note that, in contrast, an increase in lending to female clients by non-WiB banks is not associated with a meaningful change in either investment, sales, or profits in this sample, while the impact on employment is less than half of that of WiB banks. Hence, WiB banks seem to have done relatively well in identifying credit constrained female entrepreneurs, who used the new loans to improve their profitability. As a result, an increase of TRY 50,000 in borrowing is associated with a 0.56 percentage point decline in a firm's exit probability (column 6).

5.2.5 First-time borrowers

The results discussed so far pertain to female firms with pre-existing bank relationships. Yet 18 percent of the program-induced credit expansion went to first-time borrowers. Because these firms lack prior banking relationships, the Chodorow-Reich (2014) approach cannot assign firm-specific credit-supply shocks. We instead adopt a cohort-based cross-sectional design that exploits the timing of first entry into the credit registry.

Among women who obtain their first-ever bank loan in the same district and quarter, those who borrow from a WiB bank received a program-induced credit-supply shock, while those who borrow from a control bank did not. We estimate Equation (5) on a stacked sample of first-time female borrowers, following Cengiz et al. (2019). For each of the five treatment cohorts, the sample includes all first-time female borrowers whose entry bank is either the treated bank or a never-treated bank and who entered the credit system in the eight quarters following that bank's program entry. The treatment indicator equals one if the borrower's first loan was issued by a WiB bank after its program entry date, and zero otherwise. We include entry-quarter $\times$ cohort and district $\times$ cohort fixed effects, which absorb cohort-specific aggregate time variation in borrower entry conditions and time-invariant district heterogeneity, respectively. The coefficient is identified from cross-bank variation: within a given district and entry quarter, it compares the subsequent outcomes of first-time borrowers at WiB banks to those at non-WiB banks. Standard errors are clustered at the entry-bank $\times$ cohort level. Because treatment is assigned by the identity of the entry bank, this design conditions on a post-treatment matching outcome. The fixed effects ensure that treated and control first-time borrowers operate in the same local credit market at the same time, but cannot rule out differential screening or sorting across lenders.

Table 9 presents the results for the subsample of first-time borrowers matched to tax records. Columns 1–2 examine credit outcomes over the eight-quarter horizon following first entry. First-time borrowers at WiB banks obtain significantly more loans and substantially more total credit from these banks compared to those at control banks, confirming that the program expanded access to finance for women entering the formal banking system for the first time. Columns 3–10 examine real outcomes. First-time borrowers at WiB banks also exhibit higher investment, sales, and profits relative to first-time borrowers at control banks in the same district and quarter. Because treatment is defined by the entry bank, we interpret these comparisons as suggestive rather than causal. The magnitudes are economically meaningful: relative to the control group mean, the investment effect corresponds to an approximately 25 percent increase. The positive effect on cost of goods sold, combined with the sales expansion, indicates that first-time WiB borrowers scaled up their operations rather than simply improving margins on existing activity.

First-time WiB borrowers also expand their commercial networks. Column 9 shows a significant increase in the number of customers, and column 10 shows a marginally significant increase in the number of suppliers. These network effects mirror the patterns documented for repeat borrowers and suggest that the program enabled first-time borrowers to establish broader commercial relationships.

We find no significant effect on ARPK (column 4) or firm exit (column 8). The null effect on ARPK is consistent with first-time borrowers using new credit to expand capacity (increasing both capital and sales proportionally) rather than experiencing the ARPK convergence documented for high-productivity repeat borrowers in Table 8. Appendix Table A.8 confirms that the credit expansion did not come at the expense of loan quality: non-performing loan rates and early-stage credit deterioration are unaffected for first-time borrowers at treated banks. Taken together, these results suggest that the favorable firm-level patterns documented 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 program-induced credit expansion reflects borrowers poached from other banks (28 percent of the increase in new lending to female entrepreneurs). Moreover, while treated banks each account for a small share of the national market, they hold between 13 and 60 percent of bank branches in the districts where they operate (Figure 1). This raises two concerns. First, if the program displaced female lending at control banks, the comparison of treated to control banks would overstate the program's true impact, violating the stable unit treatment value assumption. Second, for first-time borrowers specifically, an alternative interpretation is that the program did not expand the pool of female borrowers but simply diverted women who would have borrowed from a control bank. We address both concerns using the framework of Berg et al. (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.16 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 separately for repeat (column 5), poached (column 6), and first-time borrowers (column 7). Three findings emerge. First, across all specifications, the direct treatment effect remains large and precisely estimated when allowing for within-district spillovers, indicating that our main 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 economically small and statistically insignificant. Appendix Figure A.5 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 the marginally significant positive spillover in column 6. 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 a substitution story, the program would divert 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.17 examines this possibility using the same bank-district spillover specification as Table A.16, with the log change in the number of first-time female borrowers at the bank-district level as the dependent variable. Columns 1-3 show that WiB banks experienced approximately 11-13 log points higher growth in first-time female borrowers than non-WiB banks, an effect that is stable across specifications conditioning on population density and rural share. Column 4 introduces the full spillover model. Two results support the net-entry interpretation. First, the coefficient on (1 - WiB) x Lending share is small (-0.100) and statistically insignificant, indicating that non-WiB banks in districts with greater WiB presence did not lose first-time female borrowers. Second, the direct WiB effect remains large and precisely estimated (0.133) even after allowing for within-district spillovers. Taken together, these results indicate that the program expanded the pool of women entering formal credit markets rather than reallocating existing first-time borrowers across lenders.

5.4 District-level results

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

Table 10 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 real outcomes. We find no significant effect on female entry rates (column 3) or female exit rates (column 5), and no significant change in female market share (column 7). If the program had induced substantial extensive-margin responses (such as new business formation prompted by increased awareness or differential growth of female-owned firms) we would expect districts with greater exposure to treated banks to experience higher rates of female firm entry or rising female market share. The absence of such effects suggests that the program primarily affected credit access for the existing stock of female entrepreneurs rather than stimulating new business creation, consistent with supply-side relaxation of credit constraints rather than demand-side responses.

We do, however, find significant positive effects on both male entry rates (column 4) and male exit rates (column 6), with F-tests rejecting equality of WiB and non-WiB coefficients at conventional levels for both margins. This pattern of simultaneous entry and exit is consistent with competitive reallocation rather than simple crowding out, and aligns with the productivity-based selection documented in our firm-level analysis. Tables A.5 and A.6 show that treated banks became more selective in their male lending: treatment effects on male credit rise monotonically across ARPK quartiles. The elevated male entry rate may reflect high-productivity male entrepreneurs gaining improved access as banks expand overall SME lending, while the elevated exit rate is consistent with lower-productivity male entrepreneurs facing increased competition as credit allocation becomes more efficient. The increase in male exit aligns with the competitive displacement predicted by Chiplunkar and Goldberg (2024), whose model shows that removing barriers facing female entrepreneurs displaces low-productivity males who previously operated only because their female competitors faced higher entry and hiring frictions. It is also consistent with Morazzoni and Sy (2022), where eliminating the gender gap in credit access causes marginally less productive male entrepreneurs to exit the entrepreneurial pool. The simultaneous increase in male entry points to heightened firm turnover among marginal male entrepreneurs, possibly reflecting new opportunities 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 (Figure A.6), ruling out delayed adjustment as an explanation.

Several considerations help reconcile the substantial firm-level effects documented before with these null aggregate results. The most important is program scale. The program disbursed EUR 417 million to approximately 12,000 women-owned businesses, a small fraction of the roughly 260,000 formally registered female-owned firms operating in Turkey during this period. While district-level exposure significantly increased aggregate lending to women (column 1), the magnitude of the intervention relative to local economies was too modest to generate detectable shifts in district-wide aggregates. This interpretation connects directly to calibrated magnitudes in the macro literature. Cuberes and Teignier (2016) show that income losses from gender gaps are proportional to the extent of those gaps, implying that partial removal generates correspondingly modest aggregate effects. Chiplunkar and Goldberg (2024) similarly find that removing only a subset of barriers (for example, entry costs without addressing hiring frictions) yields far smaller aggregate gains than comprehensive reform. And Morazzoni and Sy (2022) show that fully eliminating the gender gap in credit access raises aggregate output by up to 4 percent, implying that a partial intervention reaching a small fraction of female entrepreneurs would generate correspondingly smaller gains. A partial intervention targeting a small fraction of female entrepreneurs would be expected to generate firm-level and allocative gains without detectable aggregate quantity changes.

Reallocation effects likely further attenuate aggregate quantity impacts. Approximately 28 percent of the lending increase reflects poached borrowers, implying that some firm-level gains may come at the expense of competing firms within the same markets, consistent with the "business stealing" effects documented by Cai and Szeidl (2024). Our productivity analysis shows that high-ARPK firms benefit disproportionately from credit expansion and experience ARPK convergence following treatment, consistent with capacity expansion by previously constrained firms. Such capacity expansion need not translate into increases in aggregate quantities; it may instead operate through firm-level scaling and productivity improvements.

In sum, the evidence suggests 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 quantity effects. The fact that we find tangible positive effects in our firm-level analysis but no aggregate real effects at the district level illustrates that the overall scale of the blended finance program was insufficient to affect the population of female entrepreneurs as a whole. In most districts, the vast majority of women-owned firms remained excluded from bank credit altogether.

6 Conclusions

Blended finance programs have been scaled up rapidly in recent years, with the aim of making credit more accessible to women-owned firms and other target segments. Yet, despite their popularity, there is hardly any rigorous evidence on the impact of these programs. We leverage credit registry data, firm-level tax records, VAT transaction data, and matched employer-employee data to offer such evidence for a blended finance program in Turkey.

We find that policy-induced credit expansion can enduringly increase lending to female entrepreneurs. Treatment effects persist over a three-year horizon with no sign of reversal, and a formal spillover analysis confirms that the program did not displace female lending at control banks. The average treatment effect on the share of lending to female entrepreneurs was over 18 percent, as banks lent more to existing borrowers, poached clients from competitors, and crowded in first-time borrowers. The blended finance program benefited female firms that had been previously overlooked or underserved by banks and did so without undermining loan quality.

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 types of shocks expand borrowing, but only WiB-induced shocks generate significant firm-level gains in investment, sales, profits, employment, and commercial network diversification. This suggests that the type of lending matters, not just its volume, and that the program's combination of funding, risk-sharing, and technical assistance produced qualitatively different credit. These positive real effects extend to first-time female borrowers, who lack pre-existing bank relationships and represent genuine extensive-margin expansion of the credit market.

Our findings speak to the growing quantitative macro literature on the consequences of 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 Chiplunkar and Goldberg (2024) and Morazzoni and Sy (2022). ARPK convergence among initially high-productivity 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. This intensive-margin convergence coexists with a widening of ARPK dispersion among the borrower pool as a whole, reflecting the broad-based nature of the credit expansion across the productivity distribution. That these firm-level gains do not translate into detectable district-level effects is consistent with calibrated magnitudes in these models: partial constraint relaxation at a limited scale can generate micro-level improvements without macro-level quantity changes.

Several limitations of our analysis deserve mention, too. Our data do not include loan applications, so we cannot directly measure gender differences in application or approval rates. We also do not observe interest rates or loan maturities, which limits our ability to assess the full cost of credit to borrowers. Our data do not allow us to track transitions from entrepreneurship to wage work, which would be needed to fully assess the occupational selection channel emphasized by Chiplunkar and Goldberg (2024). The absence of declines in the total number of male or female entrepreneurs at the district level suggests this channel is unlikely to be quantitatively large, but we note it as a limitation.

Blended finance programs bundle liquidity support, training, and risk sharing. An important area for future research would be to disentangle and quantify the relative importance of these components. Our within-bank loan officer training analysis provides suggestive evidence that technical assistance was a key driver, but a more conclusive decomposition would help fine-tune future program design. For example, our results show that while banks relaxed collateral requirements for existing borrowers, they did not do so for first-time borrowers. This explains why a large part of the program impact occurred on the intensive margin. A higher first-loss risk cover, applied temporarily, may be needed to incentivize banks to further expand lending to first-time female borrowers. More broadly, performance-based incentives that condition interest discounts on DFI loans on achieving portfolio goals (such as a higher share of female borrowers among first-time clients) may help to further shift bank credit toward underserved segments in a profitable and durable way.

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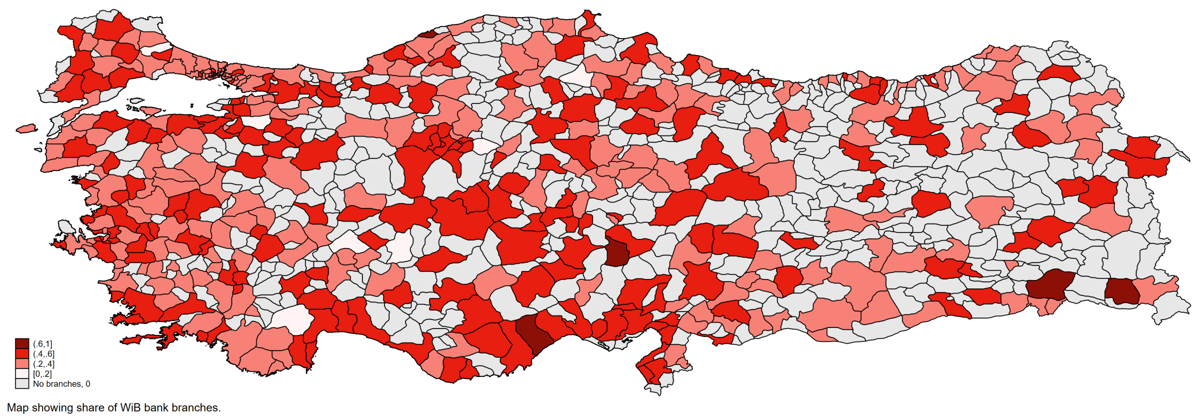
Figures and Tables

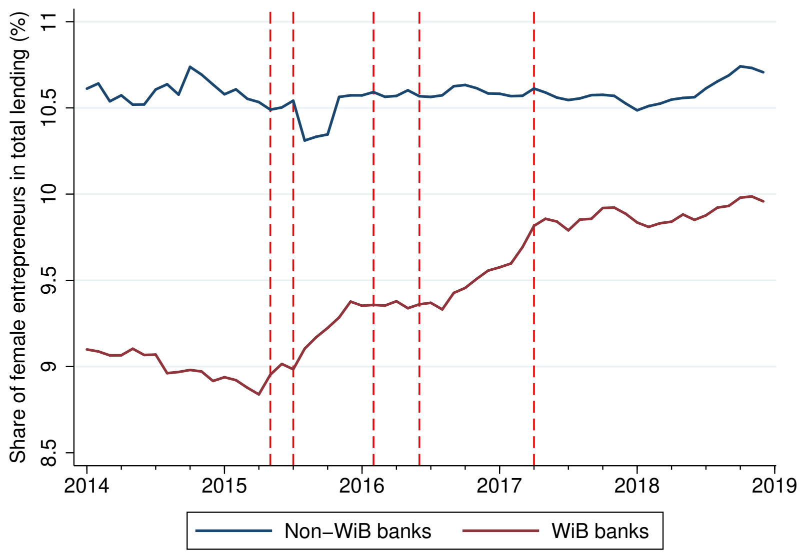
Figure 1: Pre-program share of bank branches operated by WiB banks



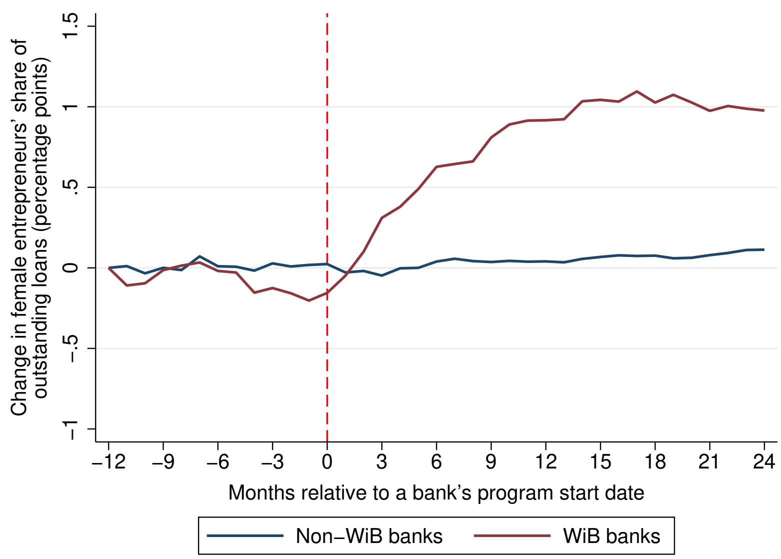
Notes: This map shows for Turkish districts the share of branches operated by WiB banks as of end-2014.

Figure 2: The WiB program and lending to female entrepreneurs

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



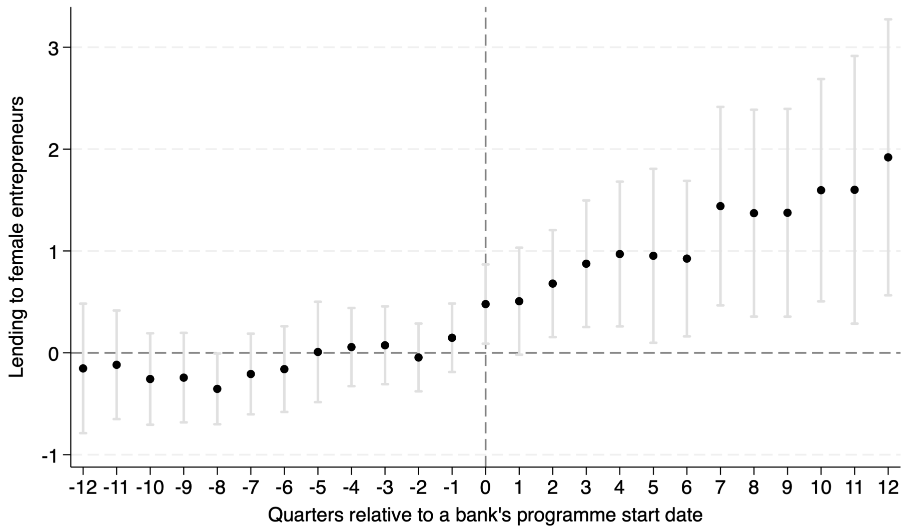
Panel B: Change in female lending share relative to program entry



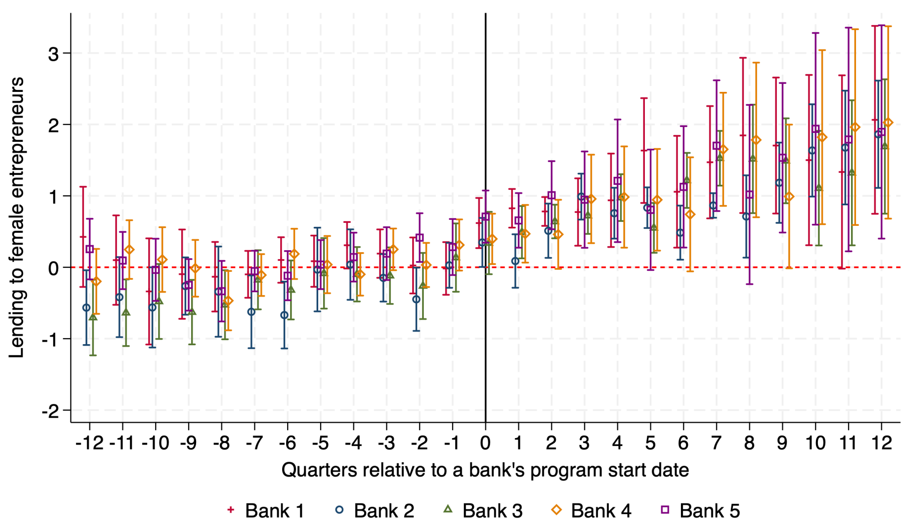
Notes: Panel A shows the share of outstanding loan stock allocated to female entrepreneurs for WiB banks (red) and non-WiB banks (blue). Vertical dashed lines indicate when each treated bank disbursed its first WiB loan (May 2015, July 2015, February 2016, June 2016, April 2017). Panel B shows the average 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 3: Event-study estimates using synthetic difference-in-differences

Panel A: Lending to female entrepreneurs (aggregated ATT)

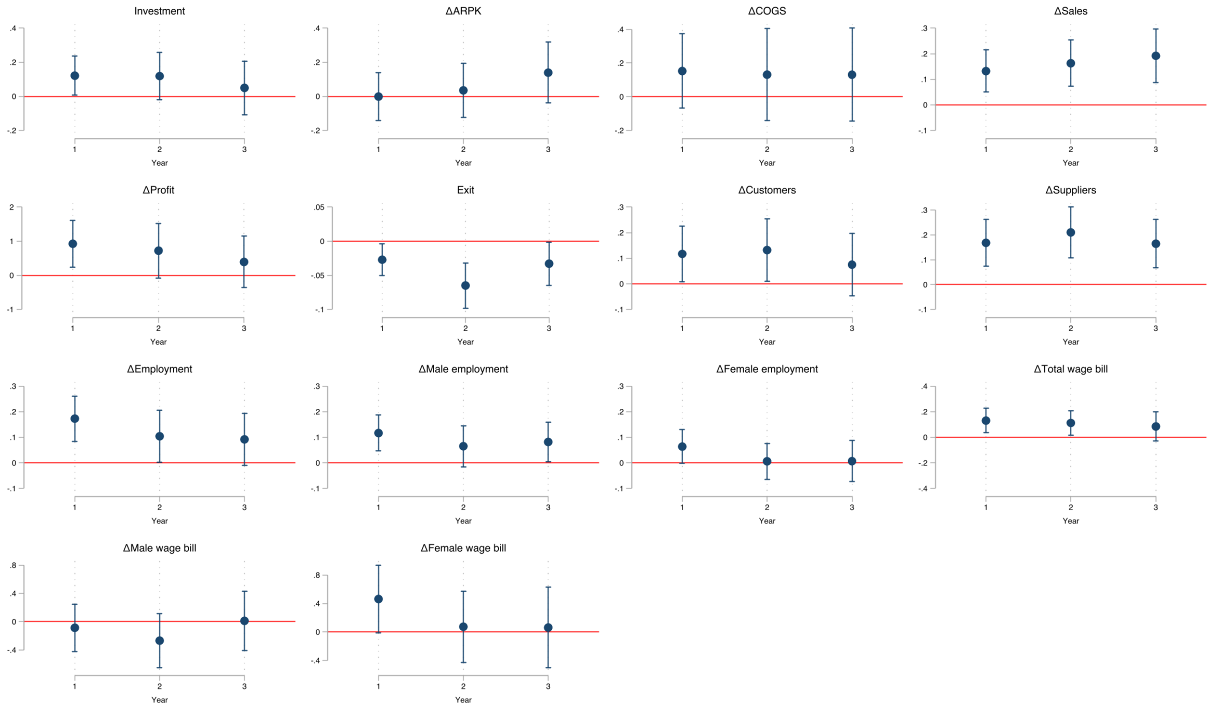


Panel B: Lending to female entrepreneurs (bank-by-bank ATT)



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 4: 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.

Table 1: 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 2: 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 3: Program effects on firm-level credit for existing 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 4: Credit-supply shocks by WiB participation and borrowing by female entrepreneurs

Dependent variable:		ΔCredit	
	(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 5: Credit-supply shocks by WiB participation: Female firm growth, performance, and survival

Dependent variable:	Investment (1)	ΔARPK (2)	ΔCOGS (3)	ΔSales (4)	ΔProfit (5)	Exit (6)	ΔCustomers (7)	ΔSuppliers (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 6: 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 7: 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 8: Credit-supply shocks by WiB participation and female firm outcomes by initial ARPK

Dependent variable:	Investment	ΔARPK	ΔCOGS	ΔSales	ΔProfit	Exit	ΔCust.	ΔSuppl.	ΔEmp.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(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 9: Blended finance and real outcomes for first-time female borrowers

Dependent variable:	Number of loans	Loan amount	Δ Invest-ment	Δ ARPK	Δ COGS	Δ Sales	Δ Profit	Exit	Δ Cus-tomers	Δ Sup-pliers
	(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 10: 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

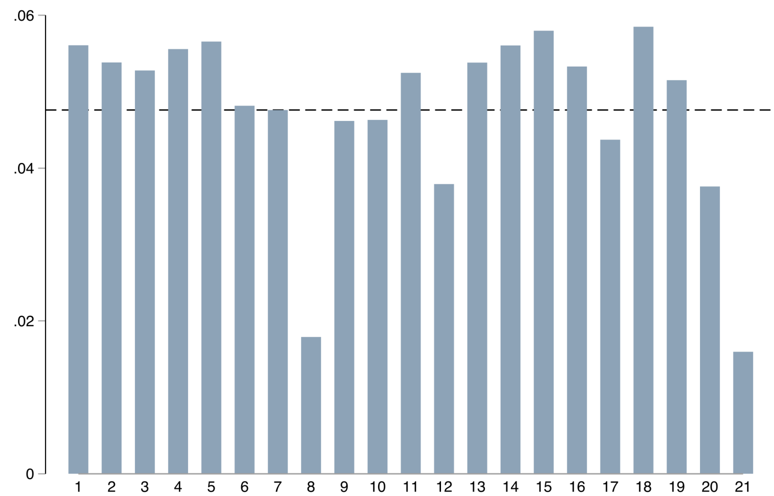
Variable Name	**Description**	**Source**
Credit from treated banks	Total credit amount a first-time borrower obtains over eight quarters from her entry bank, in Turkish lira	CBRT
Lending share (spillover analysis)	Leave-one-out market share of treated banks in a district's entrepreneurial lending as 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

Variable Name	Description	Source
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
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

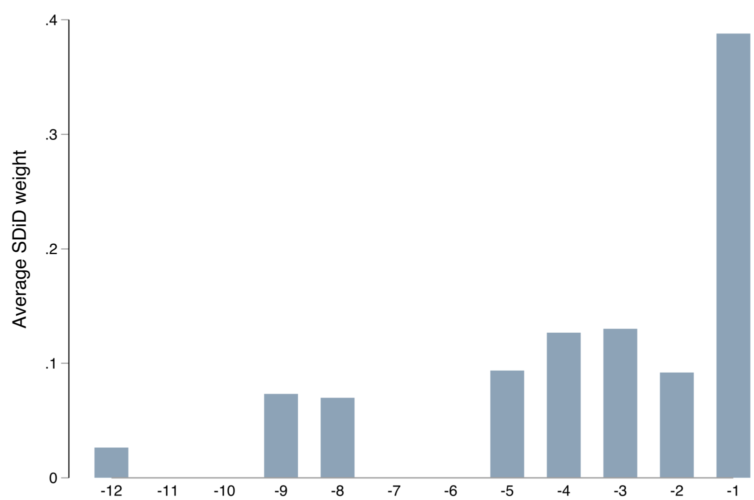
Notes: CBRT stands for the Central Bank of the Republic of Turkey, MTF for the Ministry of Treasury and Finance, and SGK for the social security institution Sosyal Güvenlik Kurumu. Small businesses are defined as companies with a single shareholder who has unlimited liability for the company's debts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

Figure A.1: Synthetic DiD estimates: Unit and time weights

Panel A: Unit weights



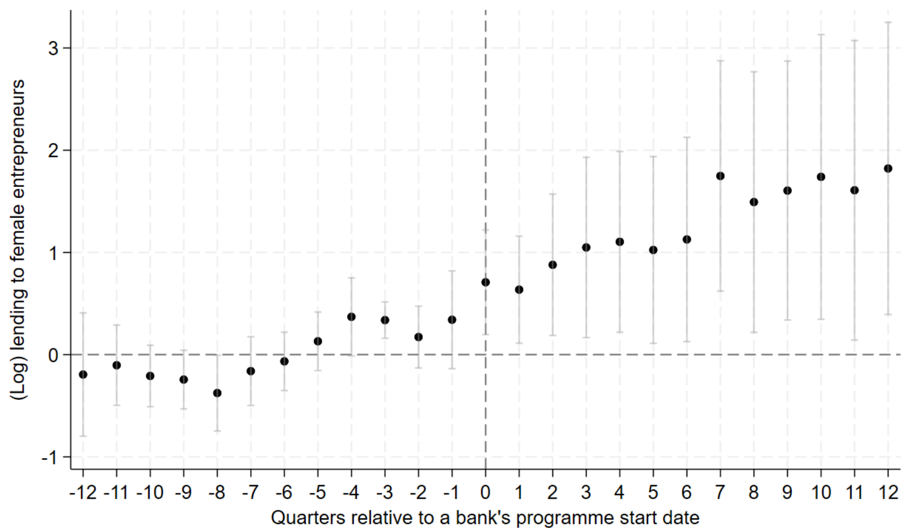
Panel B: Time weights



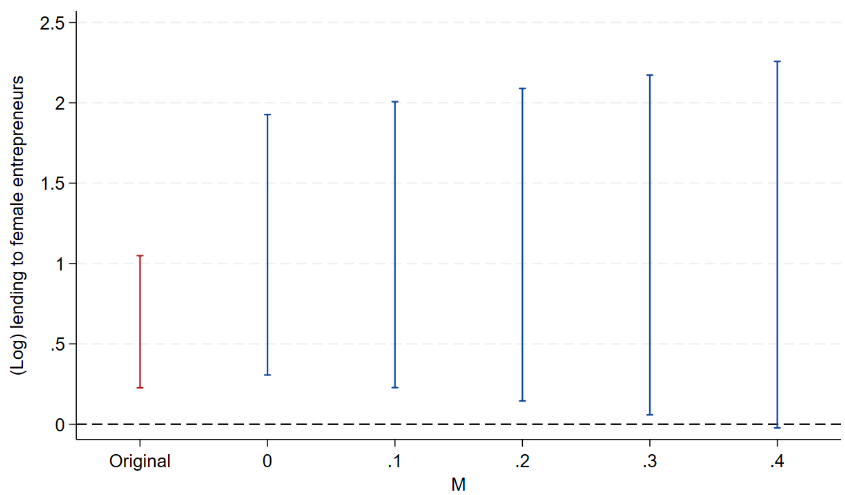
Notes: Panel A shows unit (bank) weights from the SDiD estimates in Figure 3, averaged across treated banks. The dashed horizontal line indicates the value if all units were weighted equally. Panel B shows time (quarter) weights from the SDiD estimates in Figure 3 averaged across treated banks.

Figure A.2: Event-study estimates and parallel trends sensitivity

Panel A: Event-study estimates



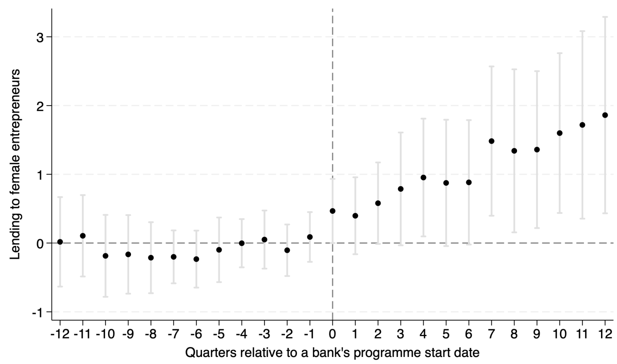
Panel B: Sensitivity to parallel trends violations



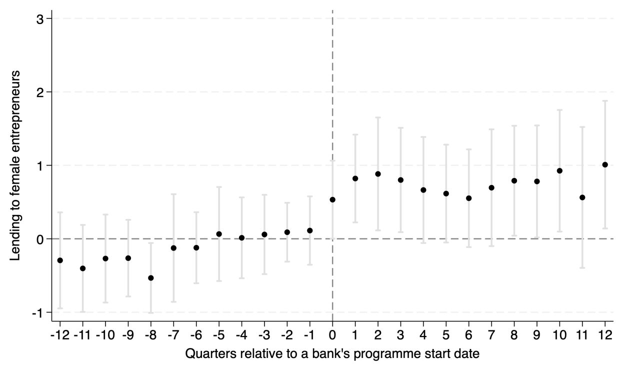
Notes: Panel A presents event-study estimates using the Gardner et al. (2024) two-stage difference-in-differences estimator. The dependent variable is (log) lending to female entrepreneurs. Vertical bars show 95% confidence intervals based on standard errors clustered at the bank level. Panel B reports 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.

Figure A.3: Event-study estimates using synthetic DiD, by borrower type

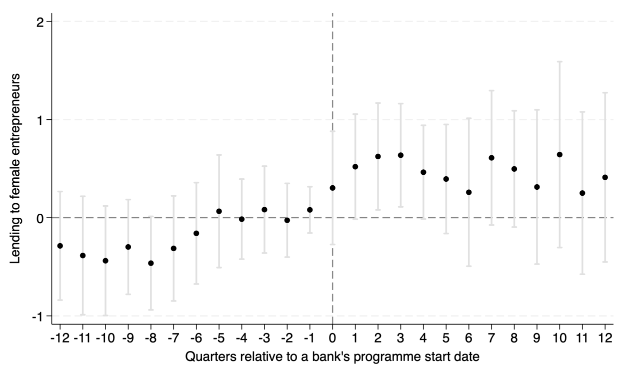
Panel A: Repeat borrowers



Panel B: Poached borrowers

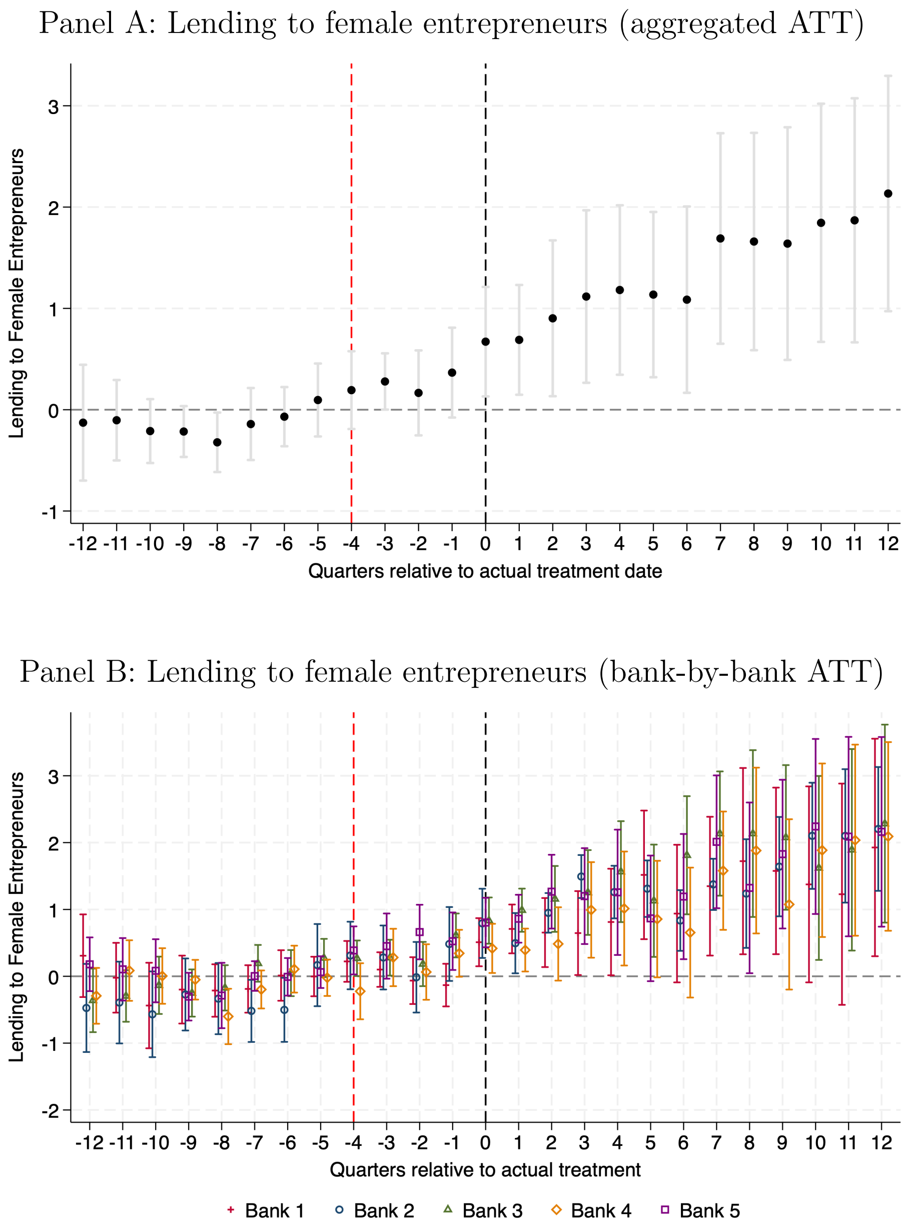


Panel C: First-time borrowers



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.

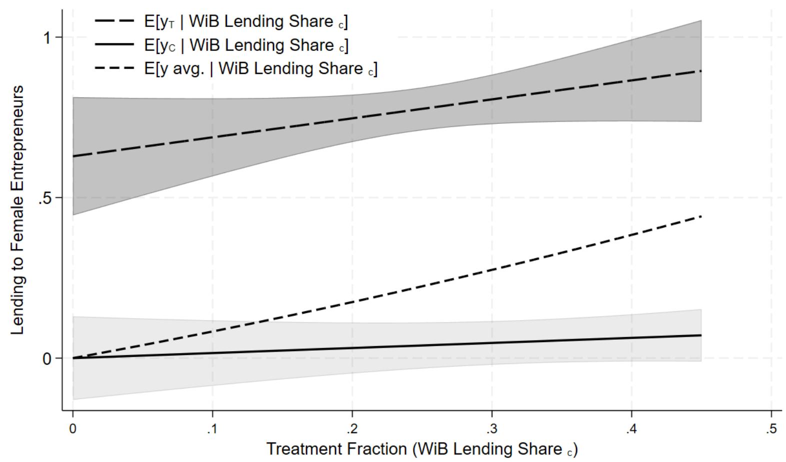
Figure A.4: Synthetic DiD estimates: Backdating the WiB program introduction



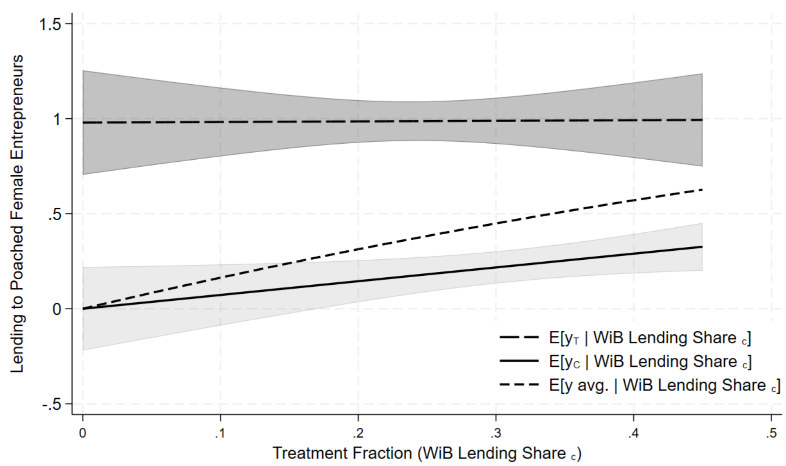
Notes: This figure shows estimates of Equation (1) when treatment is artificially backdated in an event-study setup using the synthetic difference-in-differences methodology of Arkhangelsky et al. (2021). Panel A shows the aggregated average treatment effect on the treated (ATT) across all WiB banks, 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.5: Spillover analysis

Panel A: Lending to all female entrepreneurs

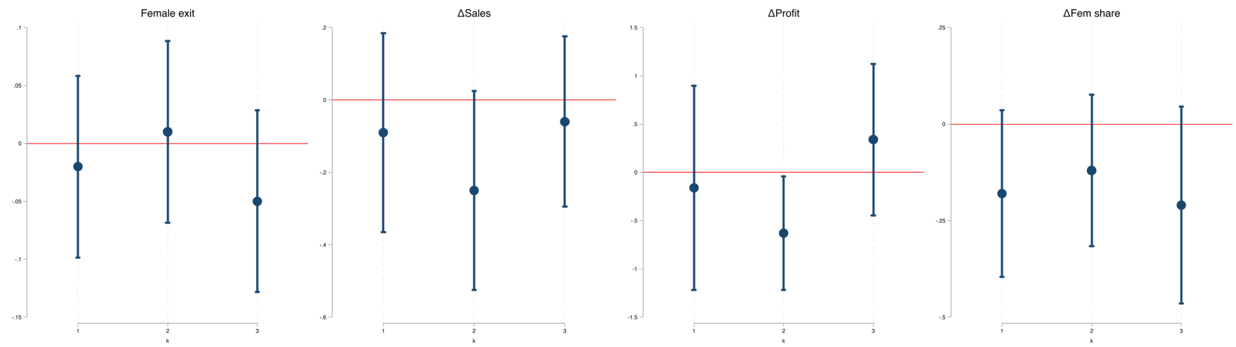


Panel B: Lending to poached female entrepreneurs



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.6: 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

Continued on next page

Table A.1 – *Continued from previous page*

#	Program	Lead Institution(s)	Launch Year	Amount (USD mn)	Geography
21.	Women Entrepreneurship Banking	IDB	2012	110	LatAm & Carib
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: 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.3: 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.4: Program effects on lending to female entrepreneurs, by borrower type

	All	Repeat	Poached	First-time
	(1)	(2)	(3)	(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.5: 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.6: Program effects on ARPK distribution of borrowers

	Female borrowers				All borrowers			
	Mean (1)	Median (2)	IQR (3)	P90-P10 (4)	Mean (5)	Median (6)	IQR (7)	P90-P10 (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.7: 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.8: 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.9: 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.10: Bank-level credit supply and lending to female entrepreneurs

Dependent variable:	Δ(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.11: 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.12: 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 employ-ment	ΔFemale employ-ment	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.13: Loan officer training and lending to female entrepreneurs

	All borrowers (1)	Repeat borrowers (2)	Poached borrowers (3)	First-time borrowers (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.14: Credit-supply shocks and female firm outcomes, IV estimates

Dependent variable:	Investment (1)	ΔARPK (2)	ΔCOGS (3)	ΔSales (4)	ΔProfit (5)	Exit (6)	ΔCustomers (7)	ΔSuppliers (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.15: Credit-supply shocks by WiB participation and employment outcomes of female firms, IV estimates

Dependent variable:	ΔEmployment (1)	ΔMale employment (2)	ΔFemale employment (3)	ΔTotal wage bill (4)	ΔTotal male wage bill (5)	ΔTotal 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.16: 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.17: 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.