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Blended Finance and Female Entrepreneurship

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Abstract

Blended finance programs combine public and private funding to ease credit constraints of specific firm segments. While rapidly gaining popularity, little evidence exists on their economic impact. To address this gap, we match credit registry data with firm-level tax records to trace out the impacts of a blended finance program for female entrepreneurs in Türkiye. Using a synthetic difference-in-differences estimator, we show that participating banks durably increase lending to women – both in absolute terms and relative to male entrepreneurs. The average treatment effect on treated banks' share of lending to female entrepreneurs is 23 percent. Banks expand credit to existing borrowers, poach clients from competitors, and crowd in first-time borrowers. Female clients of treated banks increase net borrowing and investment, especially those with higher capital productivity. Beneficiary firms grow their sales and profits, diversify suppliers, and exit less. There are no discernible impacts on aggregate firm populations at the district level, reflecting the program's relatively modest scale. Implications for program design are discussed.

Keywords: Blended finance, credit access, female entrepreneurship, misallocation

JEL classification numbers: D22; G21; G32; H81; J16; L26

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1 Introduction

Access to credit remains unevenly distributed across firms. While many large companies borrow from banks or tap bond markets with relative ease, credit remains elusive for small firms lacking collateral or without a sufficient track record (Carpenter and Petersen, 2002; Beck, Demirgüç-Kunt, and Maksimovic, 2005). Information asymmetries make banks wary of lending to such firms (Jaffee and Russell, 1976; Stiglitz and Weiss, 1981) which may forgo sound investments as a result.[1] In the aggregate, these frictions perpetuate resource misallocation and slow productivity growth (Buera and Shin, 2013; Midrigan and Xu, 2014).

Across much of the world, female-owned firms find it especially difficult to borrow (Klapper and Parker, 2011) and hence remain overrepresented in the left tail of the firm-size distribution (Jayachandran, 2021). Recent models calibrated to the Indian (Chiplunkar and Goldberg, 2021) and U.S. economy (Morazzoni and Sy, 2022) suggest that improving credit access for female entrepreneurs can boost aggregate productivity and welfare substantially.

Motivated by such productivity gains, many countries have started to implement blended finance programs for female entrepreneurs.[2] As part of such programs, a public development finance institution (DFI) provides private banks with loans that contain a use-of-proceeds clause. Banks then blend these public funds with commercial funding of their own, and on-lend them to the type of borrowers specified in the use-of-proceeds clause (e.g. female entrepreneurs). Two other elements are common: technical assistance to the participating banks (such as for staff training and IT upgrading) and risk sharing via a credit guarantee.[3]

Recent examples of blended finance programs for female entrepreneurs include the Women Entrepreneurs Opportunity Facility by the International Finance Corporation (IFC) (USD 4.5 billion); the Banking on Women program (IFC, USD 3 billion); the Affirmative Fi-

[1]See, for example, Demirgüç-Kunt and Maksimovic (1998); Rajan and Zingales (1998); Demirgüç-Kunt, Maksimovic, and Beck (2005); Aghion, Fally, and Scarpetta (2007); Ayyagari, Demirgüç-Kunt, and Maksimovic (2008); Beck, Demirgüç-Kunt, and Maksimovic (2008).

[2]These programs also reflect basic equity considerations. Duflo (2012) discusses the link between women's empowerment and economic development, and the need to support gender equality for its own sake.

[3]Section 2 provides details. See also Arping, Lóránth, and Morrison (2010), OECD (2018) and IFC (2022).

nance Action for Women in Africa by the African Development Bank (USD 1.3 billion); the SheInvest program by the European Investment Bank (USD 2 billion); and the Women Entrepreneurship Banking program by the Inter-American Development Bank (USD 0.8 billion). Yet, despite the billions of dollars of blended finance disbursed annually, empirical evidence on the effectiveness of these programs remains scarce. Critics point out that this lack of evidence means scarce development finance may be wasted (Eurodad, 2013).

For blended finance to have sustained impact, the following causal chain must hold. First, some financial friction causes a segment of firms with positive net present value projects to be credit constrained (the target group). This may be because local lenders are financially constrained themselves and/or because of information asymmetries between lenders and the target group. Second, the blended finance program successfully relaxes banks' liquidity constraints and/or makes them more willing to lend to the target group. Third, on completion of the program, banks continue to lend to the target segment, for example, because staff training has reduced loan officer bias or credit guarantees have shifted loan officers' risk perceptions. Fourth, the program-induced credit expansion not only helps firms to borrow, but this also translates into positive real impacts, such as higher sales and profits.

We provide compelling evidence on this causal chain for a quintessential blended finance program in support of female entrepreneurs in Türkiye. This Women in Business (WIB) program was rolled out during 2014–19 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 female-owned businesses and we trace out in detail the financial and real impacts of this shock by combining several micro datasets.

Our main dataset is the Turkish credit registry, which has no reporting threshold and hence covers the universe of loans. We track firms' borrowing over time and across lenders, and gauge their risk profile based on credit history and repayment performance. Uniquely, the registry not only contains data on defaults on bank loans but also on obligations vis-à-vis suppliers. It also provides the gender of borrowers, a piece of information absent from most other credit registries. Second, we access administrative records from the Ministry of Treasury and Finance on annual balance sheets and income statements for all tax-paying businesses. We again observe the gender of firm owners so we can also track women entrepreneurs who are not in the credit registry (non-borrowers). Third, we use comprehensive firm-level data from fiscal receipts collected by the same ministry for the purpose of calculating value-added tax (VAT). These data cover almost all buyer-supplier links in Türkiye and provide a comprehensive and granular picture of domestic firm networks. We use firms' unique tax identification numbers to carefully match firm records across the three datasets.

Using these combined data, we set out to answer three questions. First, can blended finance durably increase bank lending to female entrepreneurs? Second, which types of female-owned businesses (if any) gain better access to credit? Third, what are the real economic impacts (if any) on these firms? Answering these questions not only sheds light on the efficacy of this particular program, but also on the mechanisms through which blended finance can ease credit constraints more generally.

To identify program effects, we use a two-way fixed-effects (TWFE) model built around the staggered program introduction across the five participating banks. We aggregate our loan-level data up to a bank-quarter panel. Because standard TWFE estimators can return biased estimates when treatment effects vary across units and time, we follow the "stacking" methodology of Gormley and Matsa (2011) and Cengiz, Dube, Lindner, and Zipperer (2019). Using this baseline strategy, we document that the program durably increases lending to female entrepreneurs—in absolute terms and relative to male-owned firms. Participating banks expand their new loan issuance to female entrepreneurs twice as fast as control banks and, as a result, increase the portion of all business lending allocated to women by 23 per cent of the pre-program sample mean.

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 the treatment status of banks to be random. It only requires that outcomes of the treated and control banks would have evolved similarly, absent the blended finance program. We offer three approaches to mitigate concerns on this account. First, 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 include all of these characteristics as controls in our regression framework.

Second, we exploit the granularity of our data by implementing an estimator at the bank-quarter-gender level. We tighten identification through bank×gender and bank×quarter fixed effects. The former absorb all time-invariant bank-level heterogeneity in terms of gender-specific policies and biases, whereas the latter control non-parametrically for any time-varying but gender-neutral bank policies and strategies. The identifying assumption is that program entry does not coincide with other gender-specific policy changes in a bank. Reassuringly, we derive point estimates that closely resemble the earlier ones.

Third, we implement a version of the synthetic difference-in-differences (SDID) methodology recently proposed by Arkhangelsky, Athey, Hirshberg, Imbens, and Wager (2021) which allows for time-staggered treatment adoption. This approach suits us because, like synthetic control methods, it reweighs and matches preexposure trends to reduce the reliance on parallel trend assumptions. Moreover, just like a standard difference-in-differences estimator, it is invariant to additive unit-level shifts and allows for large-panel inference. The results confirm that the blended finance program strongly increases lending to female entrepreneurs. Program impacts do not mean revert but settle at a higher steady state for each of the treated banks—although treatment effects are heterogeneous in terms of their size and dynamics.

Next, we leverage the granular nature of the credit registry data to analyze which female-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 50 per cent of the increase in the share of lending allocated to women. The other half reflects lending to new borrowers: 31 per cent of the increased lending is to female borrowers poached from other lenders and 19 per cent is to firms that had never previously borrowed from *any* bank. 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 how the program-induced influx of new female borrowers affected loan quality. On the one hand, the program's training component was designed to help banks expand lending to female entrepreneurs in a profitable way. If the training was effective, we would therefore expect no effect (or a positive effect) on credit quality. On the other hand, the program may have pushed loan officers to take too much risk, thus eroding loan quality. To analyze this issue, we consider a cross-section of more than 150,000 female first-time borrowers. We compare those who received their first loan from a bank in the blended finance program with those borrowing for the first time from a control bank, while saturating the regression with bank×district and district×loan-disbursement quarter fixed effects.

There is no evidence that the blended finance program undermined credit quality. First-time female borrowers are equally likely to default—either on bank credit or on debts to suppliers—irrespective of whether they borrow from a treated or a control bank. They are also as likely to receive a follow-up loan from their first lender or, in contrast, to leave that bank in the medium term. Interestingly, first-time borrowers of a treated bank are almost 15 percentage points more likely to establish multiple banking relationships over time and to increase their debt capacity in the process. This suggests the program helped banks reach out to an underserved, though creditworthy, segment of the entrepreneurial pool.

In the final part of the paper, we consider whether the positive credit supply shocks caused by the blended finance program helped female-owned firms perform better. Following, Chodorow-Reich (2014) and Cong, Gao, Ponticelli, and Yang (2019), we construct a firm-specific measure of exposure to the plausibly exogenous credit shock caused by the program. We find that a 1 per cent increase in the program-induced credit supply at the firm level translates into 0.87 per cent of additional net borrowing and 0.13 per cent more investment. Firms also diversify their supplier base, and increase their sales and profits, on average, by 0.13 and 0.82 per cent, respectively. These impacts ensure that beneficiary firms are 2.4

percentage points less likely to exit the market in the first year after the commencement of the program. Importantly, not all firms benefit equally: those that were initially more credit constrained, as evidenced by a higher average revenue product of capital, borrow and invest more. Finally, we follow Greenstone, Mas, and Nguyen (2020) and Berton, Mocetti, Presbitero, and Richiardi (2018), and relate district-level credit supply shocks to district-level outcomes. We find no equilibrium impacts on the aggregate district-level population of female entrepreneurs, likely reflecting the relatively limited scale of the program.

In all, our findings underscore how blended finance programs that combine liquidity support with comprehensive bank-level training can improve access to credit not just on the intensive margin—for existing, productive female borrowers—but also on the extensive margin, for female entrepreneurs previously lacking credit access entirely.

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. A common approach is to implement reforms that improve credit markets in general.[4] While this can make markets more competitive, the track record is mixed in terms of benefiting small firms.[5] A second approach, widespread in low-income countries, is to rely on state banks. On the extensive margin, these banks can be instructed to open branches in underserved regions. Burgess and Pande (2005) and Fonseca and Matray (2022) show how this reduced poverty in rural India and boosted entrepreneurship and employment in urban Brazil, respectively.[6] On the intensive margin, state banks can be directed to lend more to specific firm segments through their existing branches. Banerjee and Duflo (2014) show how this allowed medium-sized firms in India to borrow and grow. These encouraging results are overshadowed by an extensive

[4] 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).

[5] For example, foreign bank entry can lead to cream-skimming and *less* credit for small firms (Detragiache, Tressel, and Gupta, 2008) while bank competition can hurt small firms if it prevents the formation of durable lending relationships (Petersen and Rajan, 1994).

[6] Relatedly, Agarwal, Kigabo, Minoiu, Presbitero, and Silva (2021) show that a government-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.

literature on how political interference tends to distort lending by state banks.[7]

Our contribution is to estimate the impact of a different and increasingly popular financial inclusion policy, blended finance, and to shed light on the mechanisms via which blended finance can help specific firm segments to borrow and grow. We show that not only small firms without prior borrowing history may be credit constrained, but also those that already have some access to bank credit—including many with a relatively high revenue product of capital. These empirical observations are also of interest in light of macro models that stress how financial frictions can restrain the entry and subsequent growth of productive (would-be) entrepreneurs (Banerjee and Moll, 2010; Buera, Kaboski, and Shin, 2011, 2015).

Second, we contribute to the literature on the real implications of bank funding shocks. Existing work documents how *negative* shocks translate into tighter financing conditions for firms and lower corporate investment, sales, and employment.[8] More recent research explores the firm-level impacts of *positive* bank funding shocks stemming from stimulus packages, credit facilities, or other government support.[9] We exploit a setting in which only few banks experienced such a positive funding shock, thus allowing for a (synthetic) difference-in-differences estimation approach. Moreover, we focus on a blended finance program, an increasingly popular financial inclusion policy that the recent literature has so far overlooked.

Third, we provide new insights into financial constraints as a barrier to female entrepreneurship. While in various low-income countries women own the majority of firms, many of these businesses remain relatively small (De Mel, McKenzie, and Woodruff, 2008; Hardy and Kagy, 2018). This not only reflects restrictive gender norms (Field, Jayachandran, and Pande, 2010) or discriminatory laws (Naaraayanan, 2020) but also frictions within the financial system itself (Brock and De Haas, 2023). Our contribution here is twofold. First, we show how a program that trains loan officers to identify and target promising female

[7] See Shleifer and Vishny (1994), La Porta, Lopez-de Silanes, and Shleifer (2002), Sapienza (2004), Dinç (2005), Khwaja and Mian (2005), Carvalho (2014), and Bircan and Saka (2021).
[8] For example, Peek and Rosengren (1997), Peek and Rosengren (2000), Chava and Purnanandam (2011), Schnabl (2012), De Haas and Van Horen (2013), and Chodorow-Reich (2014).
[9] Such as Paravisini (2008), Brown and Earle (2017), Jiménez, Peydró, Repullo, and Saurina Salas (2018), Cong et al. (2019), Joaquim, Netto, and Ornelas (2022), and Bazzi, Muendler, Oliveira, and Rauch (2023).

entrepreneurs can have real impacts while maintaining loan quality.[10] This contrasts, for example, with the muted impact of most microcredit programs.[11] These programs involve no or minimal targeting, so much credit ends up with subsistence borrowers rather than small businesses with growth potential (Banerjee, Breza, Duflo, and Kinnan, 2019).

Our results also inform the debate about the returns to capital among male versus female small-scale entrepreneurs. An experimental literature documents positive returns on cash grants for male but not female entrepreneurs (De Mel et al., 2008; Fafchamps, McKenzie, Quinn, and Woodruff, 2014). Bernhardt, Field, Pande, and Rigol (2019) show that such a performance gap may reflect women investing capital in the enterprise of their male partner. A key aspect of the program we study is that loan officers were trained to detect such within-household transfers of financial resources, and to deny credit in such cases. The positive growth impacts we document can thus be attributed to firms that are genuinely owned by women. This shows, that when lenders can effectively rule out capital pooling, relaxing the capital constraints of female-owned businesses can in fact translate into meaningful returns.

The remainder of this paper is organized as follows. Section 2 provides more details about the Turkish blended finance program. Section 3 then describes our data and identification strategy, after which Section 4 presents our results. Section 5 concludes and discusses some implications for program design.

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

[10] Augsburg, De Haas, Harmgart, and Meghir (2015) show that simply incentivizing (while not training) loan officers to take more credit risk does not generate larger impacts but increases loan delinquency.

[11] See Angelucci, Karlan, and Zinman (2015), Attanasio, Augsburg, De Haas, Fitzsimons, and Harmgart (2015), Banerjee, Duflo, Glennerster, and Kinnan (2015), Crépon, Devoto, Duflo, and Parienté (2015), and Tarozzi, Desai, and Johnson (2015).

of Ankara, İstanbul, and İzmir. The program was developed in recognition of the large and persistent gender gap in financial access across Türkiye. 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). The blended finance program was designed to address such frictions, which continue to cause a mismatch between the financial products and lending conditions offered by banks and those demanded by female entrepreneurs.

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. Figure 1 shows the district-level market shares of these banks as measured by their branch presence in 2014. Where present, participant banks controlled between 20 and 60 per cent of all branches in a district. Like other Turkish banks, these banks serve customers through a branch network covering the entire country. The Turkish banking sector is relatively competitive and consists of around 30 banks that all operate nationwide.

The banks had to on-lend these funds to women-owned small businesses. Participating banks 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 female-owned small businesses by the end of 2017. Importantly, because of different negotiation dynamics, banks received the program funding at different points in time and therefore started to disburse sub-loans at different times as well. The vertical red lines in Figure 2 indicate when each of the five banks started to lend as part of the program. As discussed in Section 3.3, this staggered rollout is a crucial part of our identification strategy.[12]

Second, the program contained a EUR 29.4 million FLRC that guaranteed up to 10 per cent of each participating bank's sub-loan portfolio. The cover acted as a temporary

[12] The program was implemented during a period—2014–2017—when the Turkish economy was growing and neither banks nor borrowers were under systemic economic distress.

incentive for banks to lend to an underserved borrower segment and, in doing so, to learn about female-owned firms' true risk profiles (without immediately taking on all the associated risk themselves). The FLRC only applied to first-time borrowers.

Third, the program involved a technical assistance program of consultancy services to help banks expand lending to women-owned small businesses by changing their business models and delivery mechanisms. Commercial banks may lack the experience to analyze the credit risk of particular types of borrowers and to lend to them profitably (Tahir, Girod, Rex, and Belot, 2021). Consultants therefore helped the participating banks enter a new market segment (or scale up their existing activity) while managing risks and profitability.

The technical assistance began with an in-depth analysis of each bank's approach (if any) regarding lending to female entrepreneurs. This resulted in tailored consultancy packages that included, for example, classroom training on gender-responsive sales, marketing, and communication; online training modules for bank staff on gender awareness and overcoming behavioral constraints; and the optimization of management information systems to gather and analyze gender-disaggregated data. Banks were also supported in developing new financial products and procedures that cater to women entrepreneurs—including longer grace periods and more flexible collateral requirements (including accepting jewelry, gold, and chattel mortgages of business assets). Moreover, loan officers were trained to detect instances where, after bankruptcy, men opened a new business in their wife's name in order to bypass the credit-scoring system and to secure fresh credit ('fake women entrepreneurs').

Importantly, the technical assistance explicitly focused on the sustainability of the program's impact after it finished. To this end, banks received training-of-trainers modules to anchor attitudes regarding lending to female entrepreneurs. The aim was to durably change banks' lending practices so they would continue to lend to female-owned enterprises even after having repaid the public credit lines.

3 Data and identification

3.1 Data

We use three main datasets for our empirical analysis. The first is the national credit registry, which provides loan data from all Turkish lenders. The registry contains detailed information on each commercial loan granted to both *capital companies* and *non-capital companies* (or *personal companies*) by all banks on a monthly basis.[13] There is no minimum threshold for loan size, which is crucial to studying borrowing by entrepreneurs and small firms. We retain all commercial loans granted to personal companies between January 2014 and March 2020. For these loans, the registry includes information on borrower gender (by definition, gender is missing for capital companies, since we do not observe the gender of all shareholders).

The credit registry also provides unique information on whether and when a firm issued a commercial check (to another firm) that subsequently bounced. Smaller Turkish companies especially use commercial checks to pay suppliers. If a check bounces, the issuing company receives a judicial fine and incurs reputational loss with existing and potential suppliers. The credit registry records all bounced checks, and banks have access to this information at the time of a loan application. Banks can therefore assess the *ex ante* riskiness of borrowers not only by checking companies' past loan defaults but also their inability to meet obligations vis-à-vis suppliers. Using this rich data, we construct several time-varying borrower characteristics, such as each firm's relationship history with its bank, whether it's a first-time borrower, and its loan and check repayment history.

The second dataset includes information on domestic firm networks, originally 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

[13] The Turkish Commercial Code classifies companies into "capital companies" and "non-capital companies". A capital company is characterised by limited liability, 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. Non-capital companies are typically owned by a single shareholder, who is often self-employed as an individual manufacturer, storekeeper, or merchant and incorporated as a sole proprietorship. Hence, they are often referred to as "personal companies".

exceeds 5,000 Turkish liras (around USD 1,600 in 2016) in a given year. This low threshold means we observe the vast majority of buyer-supplier links in Türkiye.

Our third dataset contains administrative tax records, also from the Ministry of Treasury and Finance. It provides annual balance sheets and income statements for the universe of businesses liable to pay corporate tax. Since our focus is on small female-owned firms, we retain the tax records of all personal companies. Tax identification numbers allow us to match the records of entrepreneurs across the three datasets and to track them over time.

Table 1 presents summary statistics for the female-owned firms we observe in all three datasets. The average firm owns assets worth 1.05 million liras (USD 350k at average 2016 exchange rates) of which 18 per cent are fixed assets. Outstanding credit is, on average, 0.25 million liras (USD 80k) and these firms record an annual profit of about 0.19 million liras (USD 60k). The average firm has eight main business customers and suppliers, although there exists substantial variation. For example, where for many firms we observe only one supplier, others buy inputs from more than 250 different suppliers.

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. The negative effect of such capital misallocation on aggregate production is particularly severe if highly productive firms are more credit constrained. A natural hypothesis to test (Midrigan and Xu, 2014) is then whether the blended finance program allowed firms with higher capital productivity to grow more, thus gradually reducing the cross-firm dispersion in ARPK.

3.2 Selection into the program

Our main identification strategy, discussed in Section 3.3.1 below, exploits the staggered rollout of the blended finance program across five treatment banks. We compare the lending dynamics of these banks with those of 21 control banks: similar Turkish banks that were not part of the blended finance program.[14] An advantage of this difference-in-differences setup is that it does not require explicit assumptions on how banks select into treatment but instead relies on parallel trend assumptions (Ghanem, Sant'Anna, and Wüthrich, 2022). 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 these five banks differed strongly from the control banks prior to the program in terms of observable characteristics.

Table 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 greater than that of control banks. Both groups also have similar shares of lending to women within that segment. Moreover, along various other dimensions, treated banks are remarkably similar to control banks, too. Both groups have comparable liquidity, profitability, non-performing loans (NPLs), loan-loss reserves, and capital adequacy ratios.

3.3 Identification

Our identification strategy comprises two consecutive steps: a staggered difference-in-differences estimator and a SDID model. We discuss both in turn.

3.3.1 Staggered difference-in-differences

We exploit the staggered introduction of the blended finance program by five banks to identify the effect on lending to female-owned firms. We first aggregate the raw loan-level data from

[14] These 21 banks are all other commercial banks that operated continuously in Türkiye during our sample period. We exclude investment banks, local development banks, Islamic finance banks, and very small banks that do not lend consistently to small businesses.

the credit registry to the bank (b)–quarter (t) level and estimate the following TWFE model:

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

where y_{bt} is the flow of new loans to female entrepreneurs by bank b in quarter t. The granular nature of our data allows us to consider lending to three types of female-owned firms: 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 clients). WIB_b singles out the treated banks while $Post_{bt}$ equals 1 from the first quarter when a treated bank starts lending as part of the program onwards, and 0 otherwise. β_1 then gives the average treatment effect on the treated (ATT): the effect on lending to female entrepreneurs by treated banks following their program start date and relative to control banks. We saturate this model with various time-varying bank traits, X_{bt}, on top of the standard bank (γ_b) and quarter (δ_t) fixed effects: (log) total assets, liquidity, profitability, NPLs, loan-loss reserves, capital adequacy ratios, and the bank's market share in corporate credit and in lending to small firms. These covariates are summarized in Table 2, defined in Appendix A, and discussed in the previous section.

As shown in Figure 2, each treated bank enters the program at a different point sometime between the second quarter of 2015 and the first quarter of 2017. Recent studies show that, in research designs where units start to receive the treatment at different times, the standard TWFE estimator returns biased estimates when the treatment effect varies across units and/or time periods (Goodman-Bacon, 2021; Callaway and Sant'Anna, 2020; Borusyak, Jaravel, and Spiess, 2021; De Chaisemartin and d'Haultfoeuille, 2020). This literature has also introduced methods to aggregate the ATT across units and we follow the stacking methodology of Gormley and Matsa (2011) and Cengiz et al. (2019) to do so.

Specifically, for each treated bank, we take the observations of that bank and of all never-treated banks to create a cohort.[15] For each cohort, we redefine the quarters around the

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

joining date as relative time indicators, $t \in [-8, 8]$, and stack the data across all five cohorts to estimate the ATT via a difference-in-differences regression. We interact the controls X_{bt} and the fixed effects with cohort indicators, which is more conservative than including the fixed effects on their own (Gormley and Matsa, 2011). To account for correlation in lending to female entrepreneurs over time within a bank, we cluster standard errors by bank.

The identification assumption is that the program rollout to participating banks is unrelated to aggregate credit demand by female entrepreneurs due to unobserved factors. A threat to identification would arise if banks increase lending to women as part of the program at a time of increased loan demand specifically by women borrowers. Recognising this potential threat to our identification, we implement two additional empirical approaches.

First, we tighten identification by reshaping the data at the bank (b)–quarter *(t)*–gender *(g)* level so that we can saturate the staggered difference-in-differences model with bank×gender and bank×quarter fixed effects. The former account for all time-invariant bank-level heterogeneity in terms of gender-specific policies and biases, whereas the latter control non-parametrically for time-varying but gender-neutral bank policies and strategies. The identifying assumption is that program entry does not coincide with other gender-specific policy changes in a bank. We again stack and estimate:

$$y_{bgt} = \alpha + \beta_1 WIB_b * Post_{bt} * Female_g + \gamma_{bg} + \delta_{bt} + \epsilon_{bgt} \qquad (2)$$

Our second approach is to apply an SDID estimator, which we discuss now.

3.3.2 Synthetic difference-in-differences

Given the small number of treated banks, we can employ a version of the SDID approach of Arkhangelsky et al. (2021) that allows for time-staggered treatment adoption. For each treated bank, we create one synthetic control bank (using the 21 non-treated banks) and estimate Equation (1). We calculate standard errors using the bootstrap method.

The advantage of SDID is that it relies less on the standard parallel trends assumption. The estimator finds one set of weights that align pre-program trends in the outcome of untreated banks with those for treated banks, and another set of weights that align pre-program time periods with post-program ones. These weights are then used in a TWFE regression to estimate the ATT. The estimator effectively makes the TWFE regression "local" (Arkhangelsky et al., 2021) by putting more weight on (i) banks outside the program that are similar to participant banks in terms of past development, and (ii) earlier periods similar to those during which the program was active. In doing so, the method addresses the type of pretesting concerns raised by Roth (2022).

4 Results

4.1 Main results

4.1.1 Staggered difference-in-differences

Figure 2 shows that *before* the entry of the first bank into the program, to-be-treated banks (red solid line) and control banks (blue solid line) were on similar trajectories in terms of the gender composition of their stock of small business loans. Once the first banks get access to blended finance, they start to allocate more credit to female-owned firms. Nothing changes for control banks. The result is a gradual and partial closing of the gap between treated and control banks in terms of the gender composition of their portfolio of small business loans.

Figure 3 offers further preliminary evidence on program impact. For each treated bank we normalize the quarter in which it introduced the program to t=*0*. The red line plots the change (in percentage points) in the average share of these banks' small business portfolio allocated to female entrepreneurs. The blue line does the same for never-treated banks. There is again virtually no time trend in the gender allocation of credit by these control banks. In contrast, there is a clear and persistent increase of 1 percentage point (equivalent to 11 per cent relative to the pre-program mean of 9 per cent) in the share of the stock of small business loans allocated to female entrepreneurs by treated banks.

We now determine treatment effects more formally by using the stacking method to estimate Equation (1). In column (1) of Table 3, we consider total lending to all female-owned firms. In the four panels, the dependent variables are a bank's total (log) amount of new lending (i.e. the credit flow) to female borrowers (panel A); the total (log) number of new female borrowers (panel B); the share of total new lending allocated to female borrowers (panel C); and the share of new female borrowers in all new borrowers (panel D).

Once the program is in place, treated banks increase new lending to female entrepreneurs on average by 15.6 per cent (=1.302/8.350) more than control banks (panel A, column 1). They also increase the number of female business borrowers by 16.0 per cent (=0.747/4.655) more (panel B, column 1). These impacts are significant at the 1 per cent statistical level and based on regressions saturated with time-varying bank controls as well as bank and quarter fixed effects (all interacted with cohort fixed effects). They are also economically large as they reflect changes in nominal loan flows from a low base.

Panels C and D of column (1) show that banks also increased the *share* of total new lending allocated to female-owned businesses. That is, the program led to a change in the gender allocation of total entrepreneurial lending: treated banks increased the portion of all business lending allocated to women by 2 percentage points on average. This is an economically meaningful effect (an increase of 23 per cent), given that treated banks allocated only around 8.6 per cent of their total lending to female entrepreneurs in 2014.

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 2 and 3 already provided preliminary evidence that, in fact, they were. To assess this formally, Appendix Figure A.1 shows event-study estimates for new lending to female entrepreneurs. We exclude the quarter before a bank enters the program as our reference period. Reassuringly, pre-trends appear to be parallel, making it less likely that treatment and control banks would have displayed different lending patterns in the absence of the program. Treatment effects are also persistent and increase over time. The average magnitude of the quarterly treatment effects matches closely the baseline estimate in column (1), panel A, of Table 3. We return to these dynamics in Section 4.1.2 where we discuss our SDID estimates.

An important question is to which types of female-owned businesses banks start to lend once they enter the blended finance program? Three mechanisms are possible, each with different implications for entrepreneurs. First, banks can start to lend more to existing female clients. This could further relax the credit constraints of these repeat borrowers (and possibly affect their performance, something we investigate in Section 4.4). Second, banks can poach borrowers from other banks. In this case, inter-bank competition will increase and loan terms may improve for those who switch lenders. Yet, the total number of female borrowers will not increase in this scenario. Third, banks may start to lend to first-time borrowers: female entrepreneurs who had never borrowed before. Only in this scenario does the pool of female business borrowers deepen due to the blended finance program.

Because we have time-series information on the universe of Turkish business borrowers, we can disentangle these mechanisms and quantify their importance. We first use the credit registry to classify each bank's borrowers as either repeat or new ones. Repeat borrowers received at least one loan during the treatment period and had also borrowed from the same bank in the pretreatment period. New borrowers are those who received at least one loan from a treated bank during the treatment period but had never borrowed from that bank before. We further divide these into poached and first-time borrowers. While poached borrowers never borrowed from that particular bank, they *did* borrow from another bank in the past. In contrast, first-time borrowers not only borrowed for the first time from a particular treated bank but had never borrowed from *any* bank.[16]

Columns 2 to 4 of Table 3 show that the program helped banks to lend more to repeat as well as new borrowers, both in absolute terms and relative to male entrepreneurs. The

[16] 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.

program therefore not only increased lending to female entrepreneurs on the intensive margin (repeat borrowers), it also expanded credit on the extensive margin by crowding in new clients. Panel A shows that in absolute terms, banks expanded lending on the intensive margin, on average, by 15.7 per cent (=1.217/7.742) more than control banks.[17] Credit to poached and to first-time borrowers increased by 16.9 per cent (=1.051/6.205) and 14.2 per cent (=0.840/5.911) more, respectively.

Panels C and D of Table 3 show that the increase in the *share* of lending allocated to women entrepreneurs is mainly driven by new clients, especially first-time borrowers that had never before borrowed (a 4.0 percentage points increase relative to control banks, column 4, panel C). That is, while absolute credit amounts increased across all borrower types (panels A and B), the shift in gender allocation was strongest for poached and first-time borrowers.

In Table 4, we tighten identification by reshaping the data at the bank (b)–quarter *(t)*–gender *(g)* level so we can saturate the staggered difference-in-differences model with bank×gender and bank×quarter fixed effects (further interacted with the cohort fixed effects). The former account for all time-invariant bank-level heterogeneity in terms of gender-specific policies and biases, whereas the latter control non-parametrically for time-varying but gender-neutral bank policies and strategies. We are interested in the estimated coefficient of the triple interaction term that identifies how women entrepreneurs are affected differentially compared to male entrepreneurs once treatment banks enter the program.

The results in panel A of Table 4 confirm those of Table 3: once a bank enters the program, it expands the flow of new lending to women relative to men—and this impact is again larger for poached clients and first-time borrowers. Panel B shows that in terms of the number of clients, the effect is strongest for first-time borrowers. This indicates that the program successfully incentivized and enabled participant banks to crowd in more female

[17] Other financial inclusion policies have also expanded credit on the intensive margin. For the U.S., Brown and Earle (2017) and Bachas, Kim, and Yannelis (2021) show that firms eligible for Small Business Administration (SBA) loans used them to complement existing loans, indicating that these borrowers were credit constrained. Likewise, Banerjee and Duflo (2014) show that a directed lending program in India did not crowd out prior business borrowing, again suggesting that existing borrowers were credit constrained.

relative to male entrepreneurs into the formal financial system.

How much of the increase in new lending to female entrepreneurs—relative to male ones—is driven by each of the three borrower types? Appendix Table A.1 reports results from estimating Equation (2) with another dependent variable: the quarterly change in a bank's new lending by gender and borrower type, scaled by its average stock of total lending to both female and male entrepreneurs over the quarter.[18] Half (=0.021/0.042) of the increase in new lending to women relative to men is driven by existing female clients, 31 per cent (=0.013/0.042) by poached clients, and the remaining 19 per cent (=0.008/0.042) by first-time borrowers. Although repeat clients account for half of new lending by volume, they account for a small share in the growth of the number of entrepreneurs in treated banks' loan portfolios during the program. Instead, the relative increase in the number of female entrepreneurs who get access to credit is mostly due to borrowers poached from other banks (37 per cent = 0.011/0.030) and first-time borrowers (33 per cent = 0.010/0.030).

4.1.2 Synthetic difference-in-differences

Table 5 presents our SDID estimates, which rely less on the usual parallel trends assumption. The results align well with the standard difference-in-differences results in Table 3 in terms of economic and statistical significance. They confirm that the blended finance program allowed banks to lend more to female entrepreneurs, both in absolute terms (column 1, panels A and B) and relative to male-owned businesses (column 1, panels C and D). For example, the SDID estimate in panel C of column 1, indicates that the share of female entrepreneurs in total new small business lending increased on average by 1.8 percentage points relative to the synthetic control banks. This compares to an estimate of 2 percentage points in the non-SDID results (Table 3, panel C, column 1).

Columns 2 to 4 of Table 5 confirm that the program helped banks to lend more to repeat

[18] To be precise, this variable is $\Delta X_{ibgt} = (X_{ibg,t} - X_{ibg,t-1})/(0.5 \times Y_{b,t} + 0.5 \times Y_{b,t-1})$, where the numerator is the flow of new credit for a given gender g and borrower type i, and the denominator is the average stock of total lending to all entrepreneurs over the quarter t for bank b.

as well as new borrowers. Panel A shows that in absolute terms, the largest impact was on the intensive margin, where banks expanded their lending on average by 17.4 per cent (=1.347/7.742). Interestingly, and in contrast to Table 3, in terms of the *share* of female lending, we now find a statistically significant impact not only on loans to first-time borrowers (column 4) but also to repeat clients (column 2). This is the main instance where the SDID estimator yields a more precisely estimated average treatment effect.[19]

Figure 4 visualizes bank-specific ATT's for a key outcome variable: (log) total loan volume to female entrepreneurs. This shows how, before program entry, each of the five to-be-treated banks is on a clear parallel path compared with its synthetic control. Figure 4 also reveals interesting heterogeneity across banks in the timing and magnitude of the ATTs, reflecting, for example, differences in banks' ability to absorb the training components that were integral to the program. For instance, Bank 1 (indicated in red) increases its lending to female-owned entrepreneurs immediately and, after eight quarters, is one of the two banks with the largest treatment effect. In contrast, the program takes longer to affect Bank 3 (indicated in green) and that bank's treatment effect is also lesser after two years. While the mean ATT (averaged over the eight treatment quarters and across the five banks) is 10.5 per cent, this varies between 6.9 per cent (Bank 3) and 12.6 per cent (Bank 1).[20]

The results from the SDID estimation give confidence that, in the absence of program participation, treated banks were unlikely to be subject to shocks that might shift their risk preferences or capacity for lending to women differently from control banks. Indeed, Abadie, Diamond, and Hainmueller (2010) show that a primary reason to use a synthetic

[19] There are two reasons why regular and SDID estimates can differ. First, the synthetic estimator creates a synthetic control bank for each treated bank so that, by construction, both are on parallel pre-trends. If trends are not fully parallel in the regular difference-in differences, the SDID addresses the related bias. Second, the SDID weighs individual bank ATTs in proportion to the number of treated units and time periods in each cohort. It first estimates the ATT for each of the five banks and then uses these weights to calculate the average ATT (Clarke, Pailañir, Athey, and Imbens, 2023). Earlier treated banks, with more treatment quarters observed in the data, thus carry more weight in the average ATT. For these banks, the ATT itself also captures impact in quarters beyond the first eight quarters following treatment.

[20] In Appendix Figure A.2, we extend the treatment period from two to three years. Also over this longer time horizon, treated banks continue to lend more to female entrepreneurs than before the program. There is thus no evidence of treatment effects reverting back to pre-program levels.

control method is to account for the effect of unobservable factors that have an impact on the common time trend in the treatment and control groups.

Abadie (2021) proposes backdating as a diagnostic test of the credibility of the synthetic control counterfactuals. In Appendix Figure A.3, we therefore artificially backdate the start of the blended finance program by four quarters. The placebo and actual start dates are indicated by the striped and solid black lines, respectively. The lack of estimated effects before the actual intervention date indicates that the synthetic controls successfully reproduce the trajectory of lending to female entrepreneurs for each of the five treatment banks. This is even the case when the introduction of the program is backdated by a full year so that the estimator uses no information on the timing of the actual intervention. Reassuringly, even then, the estimated effects only appear at the time of the actual intervention, $t=0$. The trajectory of the five bank-specific ATTs also closely resembles that in Figure 4, although some individual estimates are slightly less statistically significant in his backdating test.

4.2 First-time borrowers

Tables 3–5 reveal how the blended finance program not only encouraged banks to lend more to existing female clients but also to first-time borrowers. This raises the question of whether the program allowed banks to successfully expand their lending frontier to an underserved market segment or, in contrast, whether it pushed banks to lend to marginal borrowers, thus eroding loan quality. Conceptually, the additional credit supply may either reduce underinvestment by credit-constrained firms or, in contrast, exacerbate free cash flow problems. In the latter case, targeted firms invest in marginal projects with a worse risk-return profile than inframarginal ones (Jensen, 1986; Stulz, 1990) or more individuals with projects with a negative net present value start a firm (De Meza, 2002).

To investigate this issue, we use a stacked cross-sectional dataset of more than 150,000 female first-time borrowers. We consider the period during which the blended finance program was active (2015q2–2017q2) and create a cohort for each of the five treated banks.

For example, Bank 1 started the program in 2015q2, so we take 2015q2–2017q1 (the eight quarters it spends in the program) and identify all its first-time female borrowers during this period. We then add all female-owned firms that received a first loan from a never-treated bank during this period.[21] We call this cross-section of first-time borrowers 'cohort 1'. Likewise, Bank 2 enters in 2015q3, so we take the first-time borrowers of this bank and all control banks during 2015q3–2017q2 to create 'cohort 2'. We again track each borrower in this cohort during the eight quarters after receiving her first loan. The other three cohorts are similarly constructed. We then stack the five cohorts and run the following regression:

$$y_{i(b)dz} = \beta * \textit{First-time}\, WIB\ \textit{borrower}_{i(b)dz} + FE_{bd} + FE_{dz} + \epsilon_{i(b)dz} \tag{3}$$

where $y_{i(b)dz}$ captures the ex-post outcomes of first-time female business borrower i who obtained her first-ever loan from bank b in district d in quarter z. All dependent variables $y_{i(b)dz}$ are measured over the eight quarters after a firm takes out its first loan. *First-time* WIB *borrower*$_{i(b)dz}$ is an indicator that equals 1 if the borrower obtained her first-ever loan from a treated bank, and 0 if obtained from a control bank. The bank×district×cohort fixed effects (FE_{bd}) capture any local unobserved heterogeneity in the characteristics of first-time borrowers that individual banks may target. The district×first quarter×cohort fixed effects (FE_{dz}) capture any time variation in the traits of female borrowers entering a system in a particular district (induced by local demand or supply shocks that affect all female entrepreneurs in that district). Identification then comes from comparing first-time female borrowers who enter the system via treated versus control banks in the same district and quarter. Standard errors are clustered at the bank level.

Table 6 presents the results. We first consider the ex-post riskiness of first-time female borrowers. *Check default* equals 1 if a borrower failed to meet the obligations of one or more commercial checks to her suppliers (0 otherwise) during the eight quarters since first taking out a loan. Similarly, *Loan default* indicates whether the borrower defaulted on any

[21] For each first-time borrower, we also know in which exact quarter it entered the system ('first quarter').

of her loans. We find, within the same district and quarter, that first-time female borrowers are equally likely to default—either on bank credit or on debts to suppliers—irrespective of whether they borrowed from a treated or a control bank. There is therefore no evidence that the blended finance program undermined credit quality among new borrowers.

Next, in columns 4–6 we consider whether first-time borrowers follow a different borrowing trajectory depending on whether they receive their first loan from a treated or control bank. In particular, do female entrepreneurs who gain access to credit from a treated bank remain loyal to that bank or do they switch to competitor banks in search of better credit terms? We find that, independent of whether they borrow from a treated or a control bank, female entrepreneurs are as likely to receive a follow-up loan from their first lender (column 3) or to leave that bank within two years (column 4).

Interestingly, first-time borrowers from treated banks are almost 15 percentage points more likely to establish at least one new banking relationship within two years than those who borrow from a control bank (column 5). These firms are not more likely to leave their initial lender (column 3), which suggests an increased probability of obtaining loans from two or more banks. Indeed, column (6) shows that these borrowers receive, on average, 0.2 more loans within two years. This indicates that the program helped banks reach out to an underserved though creditworthy segment of the entrepreneurial pool and acted as a gateway for enduring financial inclusion. As mentioned above, lending to these female entrepreneurs did not entail a shift in the riskiness of the new borrower pool. Our results therefore support the underinvestment hypothesis, and are at odds with the free cash flow hypothesis.

4.3 Loan characteristics

4.3.1 Collateralization

The WIB program supported banks in streamlining their loan approval processes for small businesses, including by expanding the types of assets accepted as collateral (such as gold and jewelry) or by relying less on collateral in the first place (cash flow-based lending). While the focus was on making the loan application process less onerous for female entrepreneurs, some measures benefited applicants of either sex. In Appendix Table A.2, we check if there were meaningful changes in bank's collateral policies due to the program.[22] The dependent variables measure a bank's share of uncollateralized lending in total lending in a given quarter. We follow our baseline DiD approach in columns (1)–(4), where we focus on lending to female entrepreneurs. In columns (5)–(8), we use similar interaction regressions as in Table 4 to directly compare impacts on uncollateralized lending to female versus male entrepreneurs.

The first two columns of Table A.2 provide evidence of banks relaxing their collateral requirements for female entrepreneurs—though only for repeat borrowers. The coefficient in column (2) indicates that, compared to control banks, treated banks increase their share of uncollateralised lending by 8.9 per cent more. Yet, the results in columns (5) and (6) imply that male repeat borrowers benefited in equal measure.

We take away three messages from Table A.2. First, the results suggest that both female and male repeat borrowers benefited from training and policy changes that reduced banks' reliance on collateral. Second, banks did not soften collateral requirements for new clients (neither poached nor first-time borrowers). This helps explain why a large part of the program impact occurred on the intensive margin. Third, these results speak against a channel in which program-related credit guarantees were the main driver of less stringent collateral requirements. These guarantees only applied to female borrowers, so we would have expected stronger gender differences in Table A.2.

4.3.2 Loan quality

The analysis in Section 4.2 shows that the blended finance program did not cause more defaults among first-time borrowers. We now check whether this holds for other borrower types as well. In Table A.3 we consider the share of NPLs in total lending, where an NPL is defined as any loan that is either overdue by at least 90 days or has been written off

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

by the bank. We follow our baseline difference-in-differences approach in columns (1)–(4), to focus on lending to female entrepreneurs. In columns (5)–(8), we use similar interaction regressions as in Table 4 to directly compare the impact on loan performance between female versus male entrepreneurs. Throughout the table, there is no evidence of any impact on loan performance, be it among repeat, poached or (in line with Table 6) first-time borrowers. All estimated coefficients are close to zero and not statistically significant.

4.4 Blended finance and real outcomes

We now leverage our granular tax records data on firm-level outcomes to gauge how much the blended finance program allowed beneficiary firms to grow their business. Our focus is on repeat clients, as the analysis in Table 5 illustrates that, in absolute terms, treated banks most expanded their lending to existing clients. We first estimate how this expansion on the intensive margin affected individual borrowers (Sections 4.4.1 and 4.4.2) and then consider average treatment effects at the district level (Section 4.4.4).

4.4.1 Isolating firm-level credit supply shocks

To estimate the firm-level impact of the blended finance program, we first isolate time-varying credit supply shocks to individual female-owned firms. We follow Chodorow-Reich (2014), Berton et al. (2018), and Cong et al. (2019) and exploit variation in bank lending at the national level to calculate the credit supply shock each female entrepreneur is exposed to:

$$\Delta \hat{L}_{idst} = \sum_{b \in B} \omega_{bi,t=0} \times \Delta \text{log} L_{b,-ds,t} \tag{4}$$

where ω is the relationship strength between firm i and bank b in baseline year 2014 (i.e., the share of preexisting borrowing by firm i that comes from bank b) and $\Delta \text{log} L_{b,-ds,t}$ is the yearly change in (log) lending by bank b between the years $t-1$ and t to all female entrepreneurs, except those in the same district d and those operating in the same sector

s as firm i. By excluding these firms, we avoid our exposure measure being affected by correlated credit demand shocks at the district or sector level.

This identification strategy relies on two testable assumptions (Chodorow-Reich, 2014; Greenstone et al., 2020). First, bank-firm relationships are assumed to persist over time so firms cannot easily move from one lender to another. To test how applicable this is in our empirical setting, we regress an indicator variable equal to '1' if firm i takes a new loan from bank b in year t ('0' otherwise) on an indicator equal to '1' if firm i had a preexisting relationship with bank b at time $t-1$ ('0' otherwise). We estimate this regression on a sample of all possible bank-firm pairs. That is, for each firm-year, we create an observation for each bank and then track past and current lending relationships.

Table 7 shows the results of this regression. We find that preexisting banking relationships are highly predictive of where firms take out new loans. The coefficient in column (1) is 0.98 and very precisely estimated. This indicates that, if a firm takes out a new loan at time t, the probability of borrowing from a bank with which the firm had a prior relationship is 98 per cent. This coefficient ranges between 0.90 and 0.99 when we saturate the regressions with firm fixed effects (column 2), bank fixed effects (column 3), or both (column 4). When including firm fixed effects, we essentially identify the effect based on female entrepreneurs with several preexisting borrowing relationships, thus controlling for demand most stringently.[23]

The second assumption underpinning our identification is that cross-sectional variation in bank lending reflects either supply forces due to participation in the blended finance program or observable borrower characteristics, but is unexplained by unobservable borrower traits that affect credit demand. To assess the realism of this assumption, we estimate the following equation at the firm-bank-year level on a sample of all actual firm-bank relationships:

$$\Delta Credit_{ibdst} = \alpha + \beta_1 \Delta \log L_{b,-ds,t} + FE_{ibt} + \epsilon_{ibdst} \tag{5}$$

[23]The dataset used in Table 7 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 fixed effects. Likewise, including bank or year fixed effects does not reduce the number of observations.

where $\Delta\text{Credit}_{ibdst}$ is the yearly change in (log) credit to firm i by bank b in year t, FE_{ibt} are various combinations of fixed effects, and $\Delta\log L_{b,-ds,t}$ is defined as in Eq. (4).

Table 8 presents the results for the full dataset (columns 1–2) and for multi-lender firms only (columns 3–4). We are interested in whether our coefficient estimates are relatively stable when we control for borrower traits. Comparing columns (1) and (2) shows that, when we include firm fixed effects, following Khwaja and Mian (2005), the point estimate remains stable and precisely estimated. This also holds when we focus on multi-lender firms only and replace firm fixed effects with firm×year fixed effects (columns 3–4). In the latter specification, we focus on firms that borrow from multiple banks in the same year, thus fully absorbing firm-specific shocks to business opportunities and the resulting credit demand.

4.4.2 Credit supply shocks and firm-level outcomes

We can now move on to estimate the firm-level impact of firm-specific credit supply shocks induced by banks' participation in the blended finance program. To do so, we estimate the following regression at the firm-year level:

$$\Delta \mathrm{y}_{it} = \alpha + \beta_1 \mathrm{WIB} \times \Delta\hat{L}_{idst} + \beta_2 \text{Non-WIB} \times \Delta\hat{L}_{idst} + \gamma_i + \delta_t + \epsilon_{it} \tag{6}$$

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 based on Equation (4). We differentiate between shocks emanating from banks participating in the program (WIB) and those that did not (non-WIB).

Column (1) of Table 9 confirms, first of all, that bank-specific credit supply shocks translate into more borrowing by female business borrowers. A 1 per cent increase in the credit supply from prior lenders translates into 0.67 per cent 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 (0.61). An F-test at the bottom of column (2) shows this difference to be statistically significant. The higher transmission of WIB-induced credit shocks suggests that the blended finance program helped banks lend more to prior borrowers that were still relatively credit constrained and could put the additional credit to good use.

We provide more direct evidence on this in column (3) of Table 9. Here we interact the program and non-program credit supply shocks with each firm's pre-program ARPK: a firm's capital productivity as proxied by the preexisting log ratio of total sales to fixed assets. Strikingly, this interaction term is statistically significant only for banks in the blended finance program. Column (3) shows that a 1 per cent increase in the credit supply from prior WIB lenders translates into 0.69 per cent higher borrowing for a firm with average ARPK. The positive coefficient of 0.065 on the interaction term indicates that, as a result of banks' participation in the blended finance program, firms with a one standard deviation larger ARPK experienced an additional 0.12 (=1.774*0.065) per cent increase in bank loans. The expansion in the credit supply by program lenders hence translated especially into more borrowing by female-owned firms characterized by a relatively efficient use of capital. The program was therefore effective in steering banks to expanding their lending, on the intensive margin, to borrowers most in need of additional capital.

Table 10 provides similar regressions but instead focuses on real outcomes. We thus assess whether the increased use of credit by female-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 9. 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.[24] We find that a 1 per cent larger firm-specific credit

[24]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 per cent level, except for the difference in exit rates (column 6).

supply shock due to the blended finance program results, within a year, in a 0.13 per cent increase in investment (column 1); a 0.13 per cent increase in sales (column 4); a 0.82 per cent increase in profitability (column 5); a 2.40 percentage point lower likelihood of firm exit (column 6); and a 0.14 per cent increase in the number of different suppliers used by the firm (column 8).[25] We find no impact, on average, on firms' capital productivity (column 2), cost of goods sold (COGS, column 3); or number of customers (column 7). Overall, the results in Table 10 paint a picture of female-owned firms using the additional program-related borrowing to invest and sell more; source inputs from a broader set of suppliers; generate more profits; and, consequently, reduce the likelihood of firm exit.

In Figure 5, we provide point estimates for WIB$\times\Delta\hat{L}_{idst}$ based on similar regressions as in Table 10. 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.[26] Overall, these dynamics make intuitive sense. We find that, once a firm gets access to additional credit, investments increase immediately in year one and two after which the effect dies out in year three. Sales and profits increase in year one (as well as the number of suppliers) but, while sales continue to increase, the impact on profits is more short-lived. Business survival probability rises, but with some delay.

In Table 11, we ask if the real firm-level impacts depend on the initial capital productivity of the business. The first two coefficients in column (1) show that the positive investment effect of the blended finance program is indeed driven more by firms with high initial ARPK. In line with the financial impacts in Table 9, the program not only helped such firms in particular to borrow more but also to use this additional funding to step up their investments.[27] As a result, we see in column (2) that ARPK goes down (that is, converges) for these firms. This suggests that the blended finance program allowed banks to channel more credit towards

[25]Unfortunately, our data do not contain any information on firm employment.

[26]See Appendix Table A.4 for the two- and three-year results.

[27]Investment by female-owned firms with a higher ARPK is also more sensitive to non-program credit supply shocks, as shown in the third line of column (1). The positive elasticities with regard to both types of credit shocks suggest that our ARPK measure is indeed a good proxy for a firm's true marginal product of capital (cf. Cong et al. (2019)).

high-productivity firms. Columns (4) and (5) furthermore show that, while firms expanded their sales and profits due to the program, this was less the case for high-ARPK firms—at least in the short term when these firms were still investing more.

4.4.3 Quantifying the impact of the program

In this subsection, we present instrumental variables (IV) estimates to quantify the effect of access to credit from WIB banks and non-WIB banks on firm outcomes. In the first stage, we regress 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 9, 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 Table A.5 shows results from this exercise, which we can use to get a sense of the average increase in investment, sales, profit, exit, and number of suppliers caused by a Turkish lira in credit.[28] 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.5, an increase of TRY 50,000 (approximately USD 16,000 in 2016) in borrowing from a WIB bank corresponds to an increase of TRY 4,900 in gross fixed assets for a female entrepreneur on average. The same increase in borrowing corresponds to an increase of TRY 29,232 in sales (column 4) and TRY 32,553 in profits (column 5) for the average firm. 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 65 per cent (=32,553/50,000). We note that, in contrast, an increase in the stock of lending for female clients of non-WIB banks is not associated with a meaningful change in either investment, sales, or profits in this sample. Hence, WIB banks

[28]The IV estimates do not reveal statistically significant effects for the remaining outcomes.

seem to have done particularly well in identifying credit constrained female entrepreneurs, who used their new loans to improve their profits significantly. As a result, an increase of TRY 50,000 in borrowing is associated with a 0.5 percentage point decline in a firm's exit probability (column 6). Last, column (7) indicates that the same amount of borrowing also enabled the average female entrepreneur to increase her number of suppliers by 0.24.

4.4.4 District-level outcomes

We conclude our analysis by following Greenstone, Mas, and Nguyen (2020) and Gutierrez, Jaume, and Tobal (2023) and relating district-level credit supply shocks to district-level outcomes. This allows us to assess if there were identifiable general equilibrium effects at that level. We estimate the following regression at the district-year level:

$$\Delta \mathrm{y}_{dt} = \alpha + \beta_1 \mathrm{WIB} \times \Delta \hat{L}_{dt} + \beta_2 \text{Non-WIB} \times \Delta \hat{L}_{dt} + \gamma_d + \delta_t + \epsilon_{dt} \tag{7}$$

where $\Delta \mathrm{y}_{dt}$ capture district-level outcomes calculated as follows:

$$\Delta \mathrm{y}_{dt} = \frac{\mathrm{y}_{dt} - \mathrm{y}_{d,t-1}}{0.5 \times \mathrm{y}_{dt} + 0.5 \times \mathrm{y}_{d,t-1}} \tag{8}$$

The district fixed effects (γ_d) absorb time-invariant determinants of district-level productivity and demand, while the year fixed effects (δ_t) control for the influence of common shocks occurring at the annual level. The time-varying district-level credit supply shocks ($\Delta \hat{L}_{dt}$), stemming either from WIB or non-WIB banks, are constructed similarly as the firm-specific shocks following Equation (4). That is, we first aggregate total outstanding loans at the bank-district-year level. Using this panel, we run a regression of the change in outstanding loans on bank-year and district-year fixed effects. We then take the bank-year fixed effects as our bank-level credit supply variable and merge it with the initial bank-district-year level dataset. Next, we multiply the bank-level credit supply with banks' market share in each district in the preceding year. Lastly, we take the sum of this variable as our shift-share measure of annual shocks to the supply of bank credit at the district-level.

Table 12 provides the results. Column (1) shows that both WIB and non-WIB district-level credit supply shocks translate into significantly more aggregate borrowing by female-owned firms. That is, the blended finance program had a measurable impact on the total amount of bank credit allocated to female-owned firms in a district. Yet, when we consider columns (2) to (5), we find no evidence of statistically significant impacts on district-level real economic outcomes such as overall firm exit rates (column 2) or the growth of the total number of female entrepreneurs (column 3), their aggregate sales (column 4), and their aggregate profits (column 5).[29] The fact that we find no *aggregate* real effects at the district level (but *do* find tangible positive effects in our firm-level analysis) suggests the overall scale of the blended finance program was insufficient to affect the population of female entrepreneurs as a whole. Indeed, in most districts, the vast majority of female-owned firms remained excluded from bank credit altogether.

5 Conclusions

Blended finance programs have been scaled up rapidly in recent years, with the aim of making credit more accessible to female-owned firms or other target segments. Yet, despite their popularity, there is hardly any rigorous evidence of the impact of these programs. In this paper, we leverage credit registry data and other granular information to offer such evidence for a quintessential blended finance program in Türkiye. We find that policy-induced credit expansion in the form of a blended finance program can durably increase lending to female entrepreneurs—both in absolute terms and relative to male entrepreneurs. The average treatment effect on the share of lending to female entrepreneurs was 23 per cent, as banks expanded credit to existing borrowers (especially those with high capital productivity), poached clients from competitors, and crowded in first-time borrowers. That is, the blended finance program benefited female-owned firms that had been previously overlooked

[29] Figure A.4 shows that, in future years, these effects remain muted and not significantly different from zero.

or underserved by banks and did so without undermining loan quality. After receiving credit, these firms invested more, expanded sales, increased profits, and were more likely to stay in business over a three-year horizon.

Blended finance programs bundle liquidity support, comprehensive training, and risk sharing. An interesting area for future research would be to disentangle and quantify the relative importance of these three components. This would help fine-tune future blended finance programs and make them more impactful. For example, our results show that, while banks relaxed collateral requirements for existing borrowers as part of the program, they did not do so for new borrowers. This explains why a large part of the program impact occurred on the intensive margin. A higher (temporary) first-loss risk cover may be needed to incentivize banks to further expand their lending to first-time female borrowers.

One option to strengthen program impact (other than scaling up) would be to introduce performance-based incentives. Participating banks then receive an interest discount on their DFI loans that is conditional on achieving specific goals at the portfolio level, such as a higher share of female borrowers among all or first-time clients. Such high-powered incentives, applied temporarily and phased out over time, may help to further shift bank lending towards underserved target segments in a profitable and durable way.

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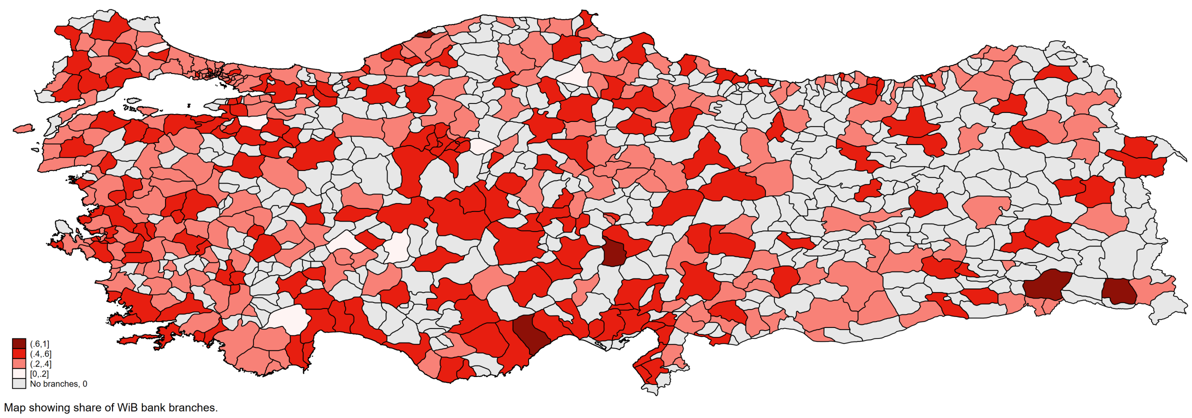
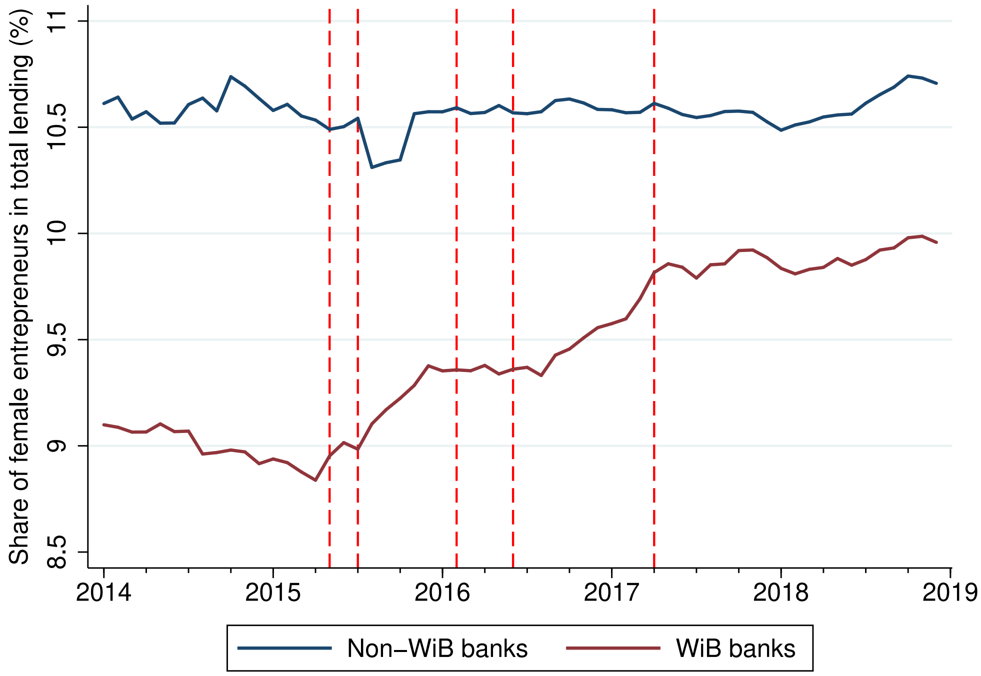
Figures and Tables

Figure 1: Pre-program market share of branches operated by WIB banks



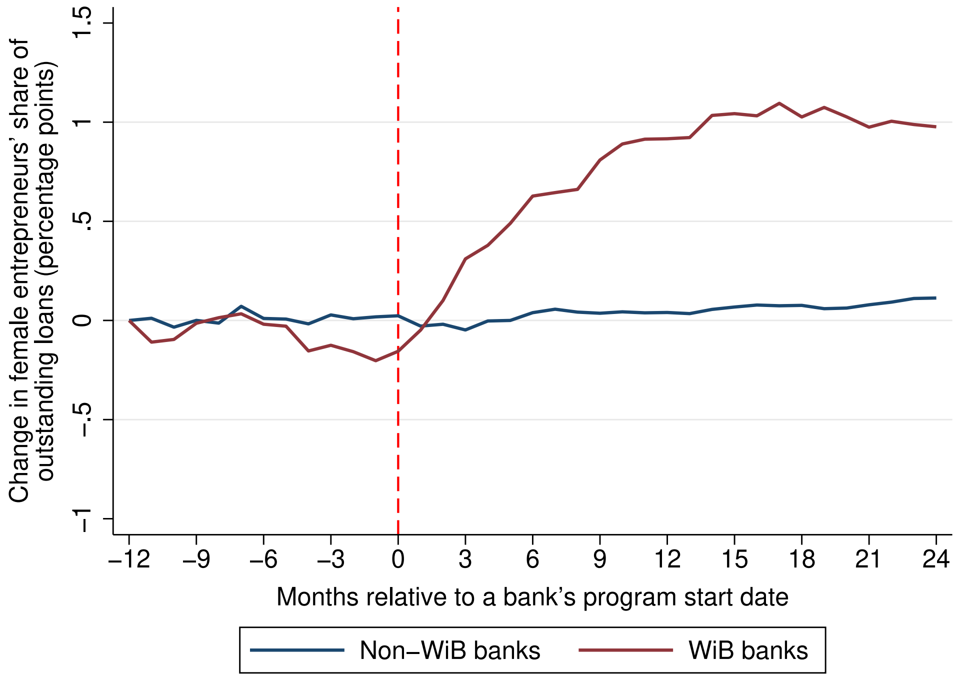
Notes: This district-level map of Turkey shows for each district the share of bank branches that are operated by WIB banks as of end-2014.

Figure 2: Staggered rollout of the WIB program and share of lending to female entrepreneurs



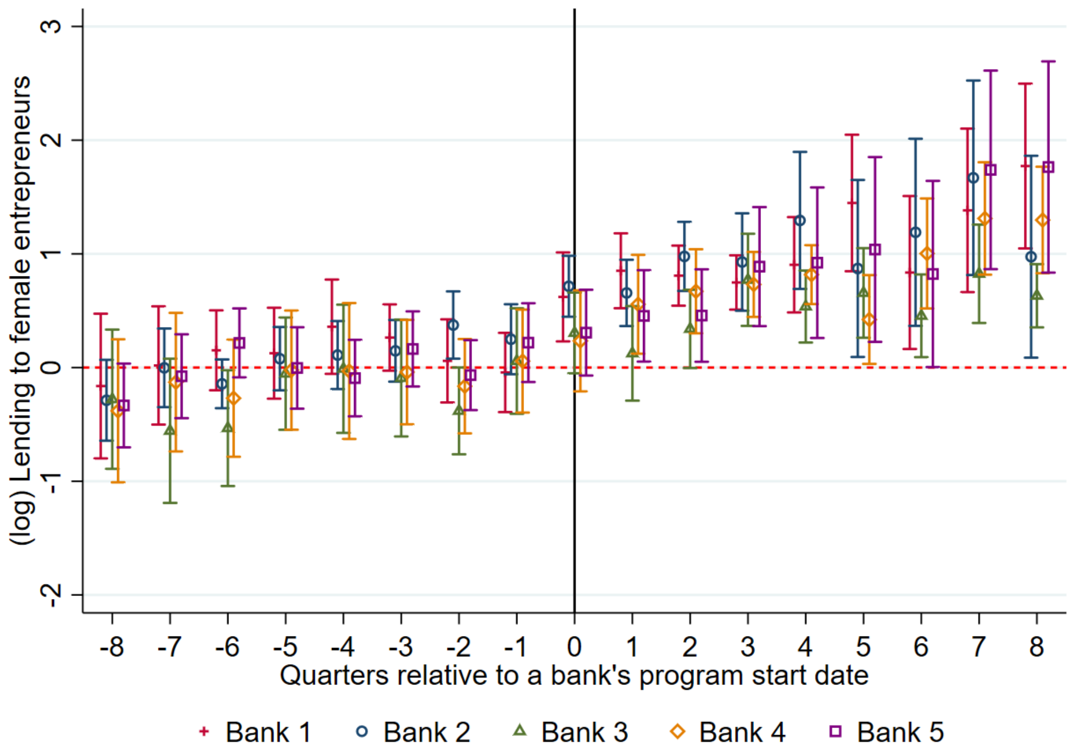
Notes: This figure shows total outstanding loans to female entrepreneurs as a percentage of the total outstanding stock of loans to all entrepreneurs for treated (WIB) banks in red and non-treated (non-WIB) banks in blue. The vertical dashed lines indicate when each of the five treated banks disbursed their first loan as part of the WIB program: May 2015, July 2015, February 2016, June 2016, and April 2017.

Figure 3: Change in the share of lending to female entrepreneurs around WIB entry



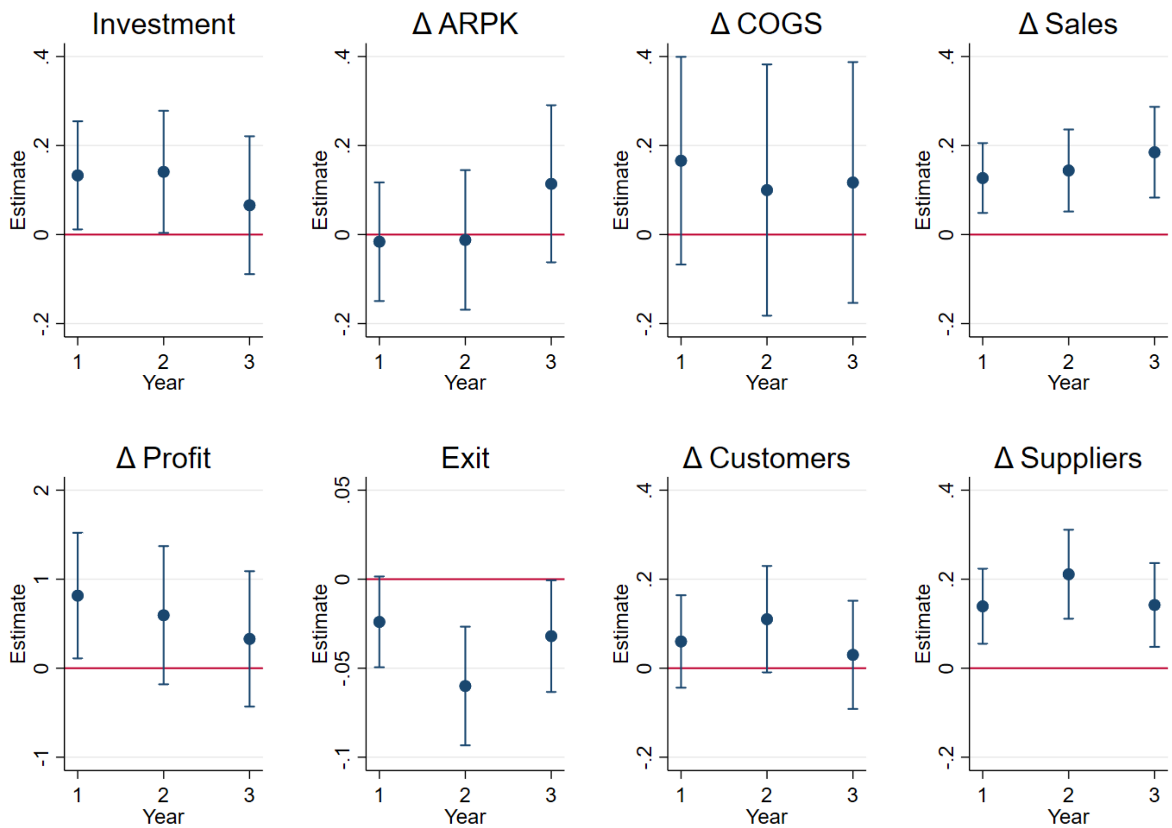
Notes: This figure shows the average bank-level change in the share of female entrepreneurs in the stock of outstanding loans to all entrepreneurs before and after banks start participating in the WIB program. For each of the five treated banks, we normalize the month in which the bank disbursed its first loan as part of the program to 0. For banks that never participated in the WIB program, we use their monthly observations corresponding to the normalized time scale for each WIB participant bank. We then calculate the average share of lending to female entrepreneurs in each month, relative to the start of the program, for WIB banks and for non-WIB banks separately.

Figure 4: Blended finance and lending to female entrepreneurs: Event-study estimates based on synthetic difference-in-differences



Notes: This figure shows estimates of Equation (1) for each individual WIB bank in an event-study setup using the synthetic difference-in-differences methodology of Arkhangelsky et al. (2021). The dependent variable is (log) total loan volume to female entrepreneurs. Error bands show 95 per cent confidence intervals.

Figure 5: Dynamic firm-level impacts of the WIB credit-supply shock



Notes: This figure shows estimates of Equation (6) on 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. Error bands show 95 per cent confidence intervals.

Table 1: Summary statistics for female entrepreneurs

	Observations	Mean	S.D.	Min	p25	Median	p75	Max
Total assets	51,842	1.049	1.841	0.001	0.321	0.632	1.170	129.978
Fixed assets	51,842	0.187	0.578	0.000	0.020	0.069	0.178	65.241
Bank credit	51,842	0.252	0.505	0.001	0.055	0.126	0.274	36.011
Profit	51,842	0.194	0.450	-5.378	0.050	0.124	0.239	54.324
Sales	51,842	1.305	2.229	0.000	0.377	0.806	1.517	120.474
Cost of goods sold	51,842	1.111	1.988	0.000	0.288	0.665	1.280	109.882
Number of customers	44,992	8.016	13.824	1.000	2.000	3.000	8.000	309.000
Number of suppliers	48,729	8.114	9.383	1.000	3.000	5.000	10.000	257.000
ARPK	51,842	2.590	1.774	-2.107	1.357	2.495	3.690	7.790

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 numbers of customers and suppliers.

Table 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 3: Blended finance and lending to female entrepreneurs: Staggered difference-in-differences estimates

	All borrowers	Repeat borrowers	Poached borrowers	First-time borrowers
	(1)	(2)	(3)	(4)
A. Lending to female entrepreneurs				
Post x WiB Bank	1.302***	1.217***	1.051***	0.840***
	(0.282)	(0.310)	(0.249)	(0.192)
R-squared	0.960	0.860	0.870	0.918
Observations	1,870	1,870	1,870	1,870
Mean dep. var.	8.350	7.742	6.205	5.911
B. Number of female entrepreneurs				
Post x WiB Bank	0.747***	0.679***	0.518***	0.448***
	(0.141)	(0.157)	(0.136)	(0.125)
R-squared	0.961	0.960	0.944	0.951
Observations	1,870	1,870	1,870	1,870
Mean dep. var.	4.655	4.231	3.107	3.094
C. Share of female lending				
Post x WiB Bank	0.020***	0.011	0.035***	0.040***
	(0.007)	(0.009)	(0.008)	(0.011)
R-squared	0.236	0.109	0.145	0.208
Observations	1,870	1,870	1,870	1,870
Mean dep. var.	0.086	0.075	0.081	0.141
D. Share of female entrepreneurs				
Post x WiB Bank	0.015*	0.012	0.031***	0.040***
	(0.008)	(0.009)	(0.010)	(0.011)
R-squared	0.339	0.200	0.121	0.248
Observations	1,870	1,870	1,870	1,870
Mean dep. var.	0.100	0.092	0.094	0.144
Bank controls x Cohort FE	y	y	y	y
Bank x Cohort FE	y	y	y	y
Quarter x Cohort FE	y	y	y	y

Notes: This table shows estimates of Equation (1) using the stacking method of Gormley and Matsa (2011) and Cengiz et al. (2019). The dependent variable is (log) lending to female entrepreneurs in Panel A; (log) number of female entrepreneurs with a loan in Panel B; share of female lending in Panel C; and share of female entrepreneurs among all entrepreneurial borrowers in Panel D. Column (1) reports totals for all female entrepreneurs, while the remaining columns report totals by type of borrower. Standard errors are clustered at the bank level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 per cent levels, respectively.

Table 4: Blended finance and lending to female entrepreneurs: Tighter difference-in -differences estimates

	All borrowers	Repeat borrowers	Poached borrowers	First-time borrowers
	(1)	(2)	(3)	(4)
A. Lending to entrepreneurs				
Post x WiB Bank x Female entrepreneur	0.058*	0.063*	0.152***	0.181***
	(0.030)	(0.033)	(0.055)	(0.066)
R-squared	0.941	0.939	0.935	0.936
Observations	3,740	3,740	3,740	3,740
Mean dep. var.	9.702	9.191	7.400	6.681
B. Number of entrepreneurs				
Post x WiB Bank x Female entrepreneur	0.035	0.014	0.078	0.190***
	(0.045)	(0.050)	(0.068)	(0.072)
R-squared	0.995	0.994	0.990	0.992
Observations	3,740	3,740	3,740	3,740
Mean dep. var.	5.597	5.191	3.909	3.653
Bank x Quarter x Cohort FE	y	y	y	y
Bank x Gender x Cohort FE	y	y	y	y

Notes: This table shows estimates of Equation (2) using the stacking method of Gormley and Matsa (2011) and Cengiz et al. (2019). 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 female entrepreneurs, while the remaining columns report totals by type of borrower. Standard errors are clustered at the bank level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 per cent levels, respectively.

Table 5: Blended finance and lending to female entrepreneurs: Synthetic difference-in-differences estimates

	All borrowers	Repeat borrowers	Poached borrowers	First-time borrowers
	(1)	(2)	(3)	(4)
A. Lending to female entrepreneurs				
ATT	1.382***	1.347***	0.890***	0.574**
	(0.434)	(0.437)	(0.318)	(0.278)
B. Number of female entrepreneurs				
ATT	0.444***	0.501***	0.329**	0.194
	(0.142)	(0.165)	(0.135)	(0.229)
C. Share of female lending				
ATT	0.018***	0.014**	0.016	0.041***
	(0.005)	(0.007)	(0.010)	(0.014)
D. Share of female entrepreneurs				
ATT	0.019**	0.014	0.020*	0.052***
	(0.009)	(0.011)	(0.011)	(0.015)

Notes: This table reports the average treatment effect on the treated (ATT) estimates using the synthetic difference-in-differences methodology of Arkhangelsky et al. (2021). Each result corresponds to a different regression. ***, **, and * indicate statistical significance at the 1, 5, and 10 per cent levels, respectively.

Table 6: Access to blended finance and adverse selection of first-time borrowers

Dependent variable:	Check default	Loan default	Loans from entry bank	Termination of entry bank	New banking relationship	Loans from new banks
	(1)	(2)	(3)	(4)	(5)	(6)
First-time WiB borrower	0.002	-0.003	0.012	-0.014	0.146***	0.213***
	(0.003)	(0.002)	(0.029)	(0.011)	(0.031)	(0.031)
R-squared	0.105	0.120	0.093	0.209	0.103	0.089
Observations	400,237	400,237	400,237	400,237	400,237	400,237
Mean dep. var.	0.002	0.0002	0.624	0.329	0.147	0.123
Bank x District x Cohort FE	y	y	y	y	y	y
District x First Quarter x Cohort FE	y	y	y	y	y	y

Notes: This table shows estimates of Equation (3). Standard errors are clustered at the bank level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 per cent levels, respectively.

Table 7: 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***
	(0.001)	(0.001)	(0.001)	(0.001)
R-squared	0.480	0.486	0.525	0.530
Observations	14,012,300	14,012,300	14,012,300	14,012,300
District FE	y	n	y	n
Industry FE	y	n	y	n
Year FE	y	y	y	y
Bank FE	n	n	y	y
Firm FE	n	y	n	y

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 per cent levels, respectively.

Table 8: 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)
$\Delta \log L_{b,-ds,t}$	0.194***	0.188**	0.268***	0.279***
	(0.071)	(0.088)	(0.073)	(0.063)
R-squared	0.025	0.244	0.188	0.456
Observations	783,176	702,740	253,491	217,530
District FE	y	n	n	n
Industry FE	y	n	n	n
Year FE	y	y	y	n
Firm FE	n	y	y	n
Firm-year FE	n	n	n	y

Notes: This table shows estimates of Equation (5). The sample includes all existing bank-firm relationship pairs. Columns (1)–(4) report results for all firms, while columns (5)–(8) 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 per cent levels, respectively.

Table 9: Credit supply by WIB participation and borrowing by female entrepreneurs

Dependent variable:		ΔCredit	
	(1)	(2)	(3)
$\Delta\hat{L}_{idst}$	0.667***		
	(0.058)		
WiB $\times\Delta\hat{L}_{idst}$		0.871***	0.693***
		(0.067)	(0.093)
Non-WiB $\times\Delta\hat{L}_{idst}$		0.611**	0.659***
		(0.064)	(0.093)
WiB $\times\Delta\hat{L}_{idst}\times$ pre-program ARPK			0.065**
			(0.031)
Non-WiB $\times\Delta\hat{L}_{idst}\times$ pre-program ARPK			-0.017
			(0.029)
R-squared	0.281	0.281	0.281
Observations	51,842	51,842	51,842
Mean dep. var.	-0.005	-0.005	-0.005
F-test WiB $\times\,\Delta\hat{L}_{idst}$ = Non-WiB $\times\,\Delta\hat{L}_{idst}$		11.23	
p-value		0.001	
Year FE	y	y	y
Firm FE	y	y	y

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

Table 10: Credit supply by WIB participation and female firm-level outcomes

Dependent variable:	Investment	ΔARPK	ΔCOGS	ΔSales	ΔProfit	Exit	ΔCustomers	ΔSuppliers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WiB $\times \Delta \hat{L}_{idst}$	0.133**	-0.016	0.166	0.127***	0.815**	-0.024*	0.060	0.139***
	(0.062)	(0.068)	(0.119)	(0.040)	(0.360)	(0.013)	(0.053)	(0.043)
Non-WiB $\times \Delta \hat{L}_{idst}$	0.012	-0.051	-0.067	-0.034	0.214	-0.009	0.020	0.054*
	(0.041)	(0.049)	(0.059)	(0.028)	(0.208)	(0.008)	(0.035)	(0.032)
R-squared	0.258	0.246	0.217	0.303	0.178	0.376	0.234	0.218
Observations	51,842	51,842	51,842	51,842	51,842	51,842	42,080	47,502
Mean dep. var.	0.102	-0.049	0.050	0.052	-0.190	0.034	0.006	-0.007
F-test WiB $\times \Delta \hat{L}_{idst}$ = Non-WiB $\times \Delta \hat{L}_{idst}$	3.933	0.255	3.758	15.375	3.219	1.356	0.557	3.837
p-value	0.048	0.613	0.053	0.000	0.073	0.245	0.456	0.051
Year FE	y	y	y	y	y	y	y	y
Firm FE	y	y	y	y	y	y	y	y

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

Table 11: Credit supply by WIB participation and female firm-level outcomes by initial ARPK

Dependent variable:	Investment	ΔARPK	ΔCOGS	ΔSales	ΔProfit	Exit	ΔCustomers	ΔSuppliers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WiB $\times \Delta \hat{L}_{idst}$	-0.034	0.413***	0.322	2.318***	0.386***	-0.003	0.315***	0.134
	(0.080)	(0.110)	(0.250)	(0.723)	(0.069)	(0.023)	(0.086)	(0.082)
WiB $\times \Delta \hat{L}_{idst}$ $\times$ initial ARPK	0.060*	-0.155***	-0.056	-0.546***	-0.094***	-0.008	-0.092***	0.002
	(0.032)	(0.041)	(0.066)	(0.189)	(0.021)	(0.006)	(0.025)	(0.022)
Non-WiB $\times \Delta \hat{L}_{idst}$	-0.269***	0.300***	0.035	0.582	0.008	-0.005	0.108*	0.014
	(0.057)	(0.090)	(0.143)	(0.468)	(0.058)	(0.015)	(0.057)	(0.079)
Non-WiB $\times \Delta \hat{L}_{idst}$ $\times$ initial ARPK	0.096***	-0.120***	-0.035	-0.123	-0.014	-0.001	-0.030*	0.013
	(0.023)	(0.031)	(0.037)	(0.111)	(0.015)	(0.004)	(0.017)	(0.021)
R-squared	0.259	0.247	0.217	0.178	0.304	0.376	0.235	0.218
Observations	51,842	51,842	51,842	51,842	51,842	51,842	42,080	47,502
Mean dep. var.	0.102	-0.049	0.050	0.052	-0.190	0.034	0.006	-0.007
Year FE	y	y	y	y	y	y	y	y
Firm FE	y	y	y	y	y	y	y	y

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

Table 12: District-level outcomes

Dependent variable:	Δ Credit	Exit rate	Δ Entrepreneurs	Δ Sales	Δ Profit
	(1)	(2)	(3)	(4)	(5)
WiB $\times \Delta \hat{L}_{dt}$	0.243***	-0.028	-0.044	-0.101	-0.253
	(0.080)	(0.038)	(0.078)	(0.136)	(0.521)
Non-WiB $\times \Delta \hat{L}_{dt}$	0.122**	-0.001	-0.020	-0.015	-0.082
	(0.050)	(0.011)	(0.031)	(0.034)	(0.088)
R-squared	0.328	0.264	0.266	0.230	0.171
Observations	3,352	3,352	3,352	3,352	3,352
Mean dep. var.	0.225	0.112	0.116	0.194	0.181
Year FE	y	y	y	y	y
District FE	y	y	y	y	y

Notes: This table shows coefficient estimates of Equation (7). Standard errors are clustered at the district level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 per cent levels, respectively.

Online Appendix for

Blended Finance and Female Entrepreneurship

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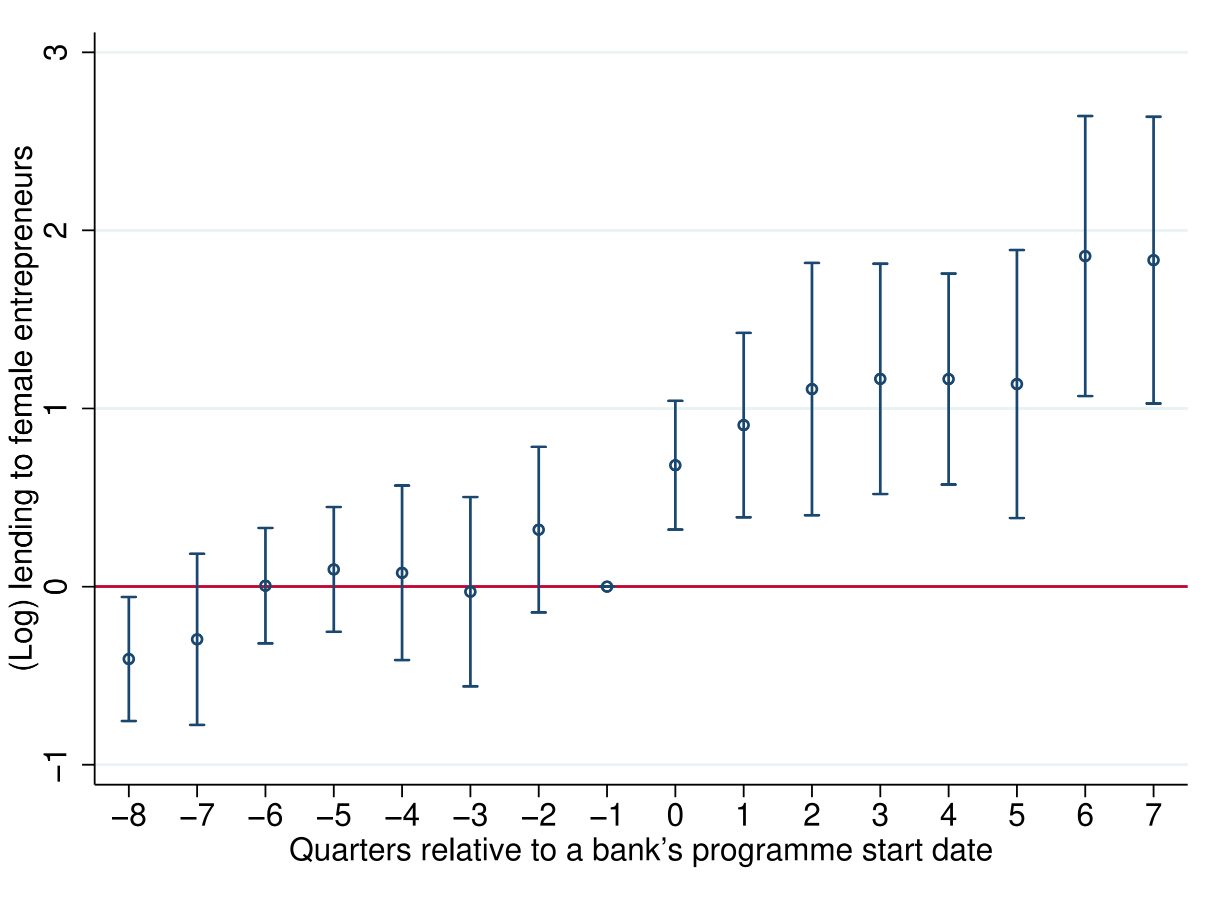
Appendix A. Variable Definitions

Variable Name	Description	Source
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 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
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 risk-weighted assets	CBRT
Market share in credit	A bank's national market share in lending to all corporates	CBRT
Market share in entrepreneurial credit	A bank's national market share in lending to small businesses and entrepreneurs	CBRT
Share of female lending	A bank's share of credit to female-owned small businesses and female entrepreneurs in total credit to all small businesses and entrepreneurs	CBRT
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 commencement of the program	CBRT
First-time borrowers	Entrepreneurs who had never taken out a loan before commencement of the WIB program began and who first appear in the credit registry with a loan after the program began	CBRT
Check default	Indicator variable equal to 1 if an entrepreneur has defaulted on a commercial check to their supplier in the past two years, 0 otherwise	CBRT
Loan default	Indicator variable equal to 1 if an entrepreneur has defaulted on a bank loan in the past two years, 0 otherwise	CBRT
Loans from entry bank	Number of loans a first-time borrower obtains over the next two years from the bank that gives the same borrower their first-ever loan	CBRT
Termination of entry bank	Indicator variable equal to 1 if an entrepreneur no longer has credit outstanding with her entry bank two years after receiving their first-ever loan from that bank, 0 otherwise	CBRT

Variable Name	Description	Source
New banking relationship	Indicator variable equal to 1 if an entrepreneur who is a first-time borrower obtains a loan from a second bank during the first two years after obtaining their first-ever loan, 0 otherwise.	CBRT
Loans from new banks	Number of loans a first-time borrower obtains over the next two years from other banks since obtaining their first-ever loan from a bank	CBRT
Credit	Total credit stock at year-end in Turkish lira	MTF
Sales	Total amount of revenue measured at year-end in Turkish lira	MTF
Investment	Annual change in (log) gross fixed assets (gross property, plant, equipment)	MTF
ARPK	Average revenue product of capital; ratio of a business's total sales to fixed assets	MTF
Cost of goods sold	Reported end-of-year total cost of goods sold	MTF
Profit	Reported end-of-year profit	MTF
Firm exit	Indicator variable equal to 1 if an entrepreneur's business no longer appears in the annual tax filings, 0 otherwise	MTF
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

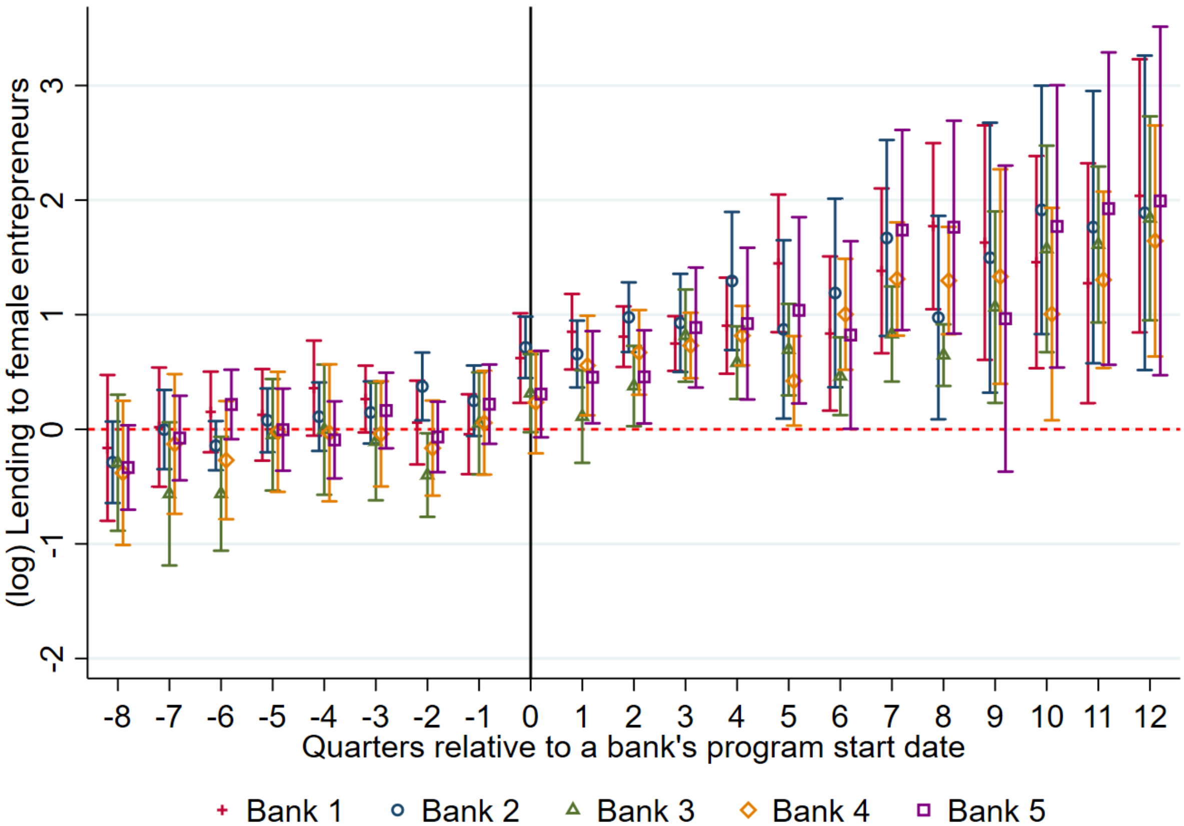
Notes: CBRT stands for the Central Bank of the Republic of Türkiye, and MTF stands for the Ministry of Treasury and Finance. Small businesses are defined as companies with a single shareholder who has unlimited liability for the company's debts and undertakings, typically incorporated as sole proprietorships.

Figure A.1: Blended finance and lending to female entrepreneurs: Event-study estimates



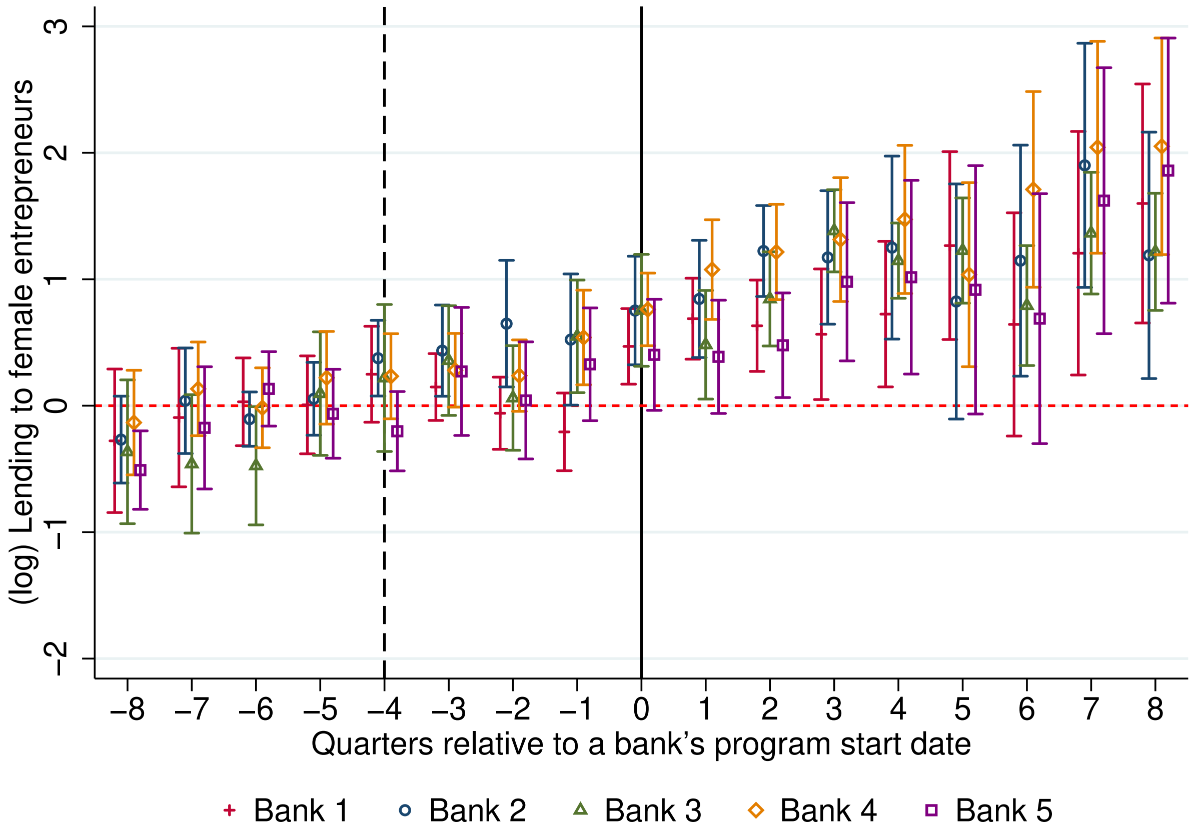
Notes: This figure shows event-study estimates of Equation (1) using the stacking method of Gormley and Matsa (2011) and Cengiz et al. (2019). The dependent variable is (log) lending to all female entrepreneurs, as in column (1) in Panel A of Table 3. Error bands show 95% confidence intervals.

Figure A.2: Long-run event-study estimates of WIB participation based on synthetic difference-in differences



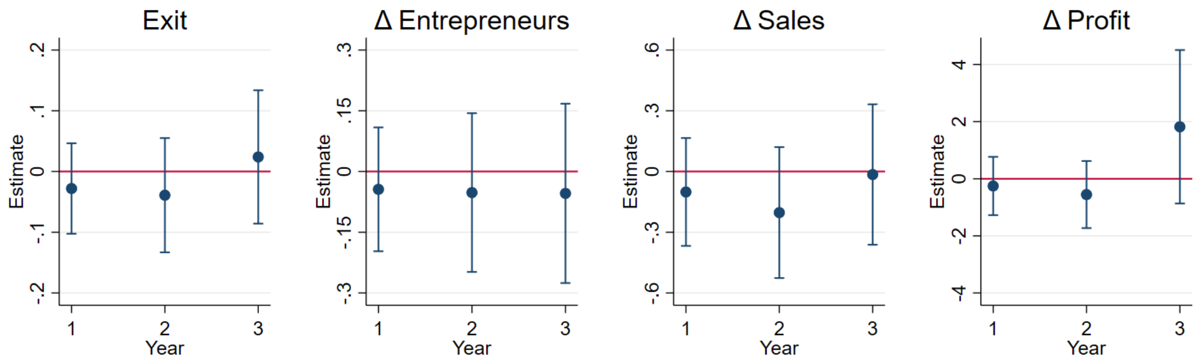
Notes: This figure shows long-run estimates of Equation (1) for each individual WIB bank in an event-study setup using the synthetic difference-in-differences methodology of Arkhangelsky et al. (2021). The dependent variable is (log) total loan volume to female entrepreneurs. Error bands show 95 per cent confidence intervals.

Figure A.3: Synthetic DiD estimates: Backdating the WIB program introduction



Notes: This figure shows estimates of Equation (1), when treatment is artificially backdated by four quarters, for each individual WIB bank in an event-study setup using the synthetic difference-in-differences methodology of Arkhangelsky et al. (2021). The dependent variable is (log) total loan volume to female entrepreneurs. Error bands show 95 per cent confidence intervals.

Figure A.4: Dynamic impacts of the WIB credit-supply shock at the district level



Notes: This figure shows estimates of Equation (7) 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 per cent confidence intervals.

Table A.1: Decomposition of new lending to female entrepreneurs by borrower type

	All borrowers	Repeat borrowers	Poached borrowers	First-time borrowers
	(1)	(2)	(3)	(4)
A. Lending to entrepreneurs				
Post x WiB Bank x Female entrepreneur	0.042***	0.021***	0.013***	0.008***
	(0.007)	(0.005)	(0.003)	(0.003)
R-squared	0.803	0.803	0.664	0.833
Observations	3,740	3,740	3,740	3,740
Mean dep. var.	0.205	0.144	0.037	0.024
B. Number of entrepreneurs				
Post x WiB Bank x Female entrepreneur	0.030***	0.009	0.011**	0.010***
	(0.005)	(0.007)	(0.004)	(0.004)
R-squared	0.893	0.924	0.802	0.807
Observations	3,740	3,740	3,740	3,740
Mean dep. var.	0.126	0.088	0.024	0.015
Bank x Quarter x Cohort FE	y	y	y	y
Bank x Gender x Cohort FE	y	y	y	y

Notes: This table shows estimates of Equation (2) using the stacking method of Gormley and Matsa (2011) and Cengiz et al. (2019). The dependent variable is quarterly change in lending to entrepreneurs by gender and borrower type, scaled by average stock of total lending to male and female entrepreneurs over the quarter, in Panel A; and quarterly change in number of entrepreneurs with access to credit by gender and borrower type, scaled by average stock of total number of entrepreneurs over the quarter, in Panel B. Standard errors are clustered at the bank level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 per cent levels, respectively.

Table A.2: Impact of the WIB program on the share of uncollateralised loans

	All borrowers	Repeat borrowers	Poached borrowers	First-time borrowers	All borrowers	Repeat borrowers	Poached borrowers	First-time borrowers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WiB Bank x Post	0.059*	0.089***	0.035	0.054				
	(0.032)	(0.031)	(0.026)	(0.036)				
WiB Bank x Post x Female					-0.005	-0.003	-0.005	-0.000
					(0.005)	(0.004)	(0.012)	(0.019)
Adjusted R-squared	0.608	0.680	0.678	0.726	0.758	0.756	0.745	0.818
Observations	1,870	1,870	1,870	1,870	3,740	3,740	3,740	3,740
Bank controls x Cohort FE	y	y	y	y	n	n	n	n
Bank x Cohort FE	y	y	y	y	n	n	n	n
Quarter x Cohort FE	y	y	y	y	n	n	n	n
Bank x Gender x Cohort FE	n	n	n	n	y	y	y	y
Bank x Quarter x Cohort FE	n	n	n	n	y	y	y	y

Notes: This table shows coefficient estimates of Equation (1) in columns (1)–(4) and of Equation (2) in columns (5)–(8) using the stacking method of Gormley and Matsa (2011) and Cengiz et al. (2019). The dependent variable is the share of uncollateralized lending in the total volume of entrepreneurial lending. The results in columns (1)–(4) reflect data on female entrepreneurs, whereas results in columns (5)–(8) reflect data on both female and male entrepreneurs. Columns (1) and (5) report results for all types of entrepreneurs, whereas the remaining columns report results for repeat borrowers (columns 2 and 6), poached borrowers (columns 3 and 7), and first-time borrowers (columns 4 and 8). Standard errors are clustered at the bank level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 per cent levels, respectively.

Table A.3: **Impact of the WIB program on non-performing loans**

	All borrowers	Repeat borrowers	Poached borrowers	First-time borrowers	All borrowers	Repeat borrowers	Poached borrowers	First-time borrowers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
WiB Bank x Post	-0.003	-0.003	0.006	0.009				
	(0.007)	(0.007)	(0.010)	(0.007)				
WiB Bank x Post x Female					-0.003	-0.002	-0.003	0.001
					(0.003)	(0.004)	(0.004)	(0.006)
Adjusted R-squared	0.309	0.263	0.178	0.195	0.430	0.190	0.385	0.277
Observations	1,870	1,870	1,870	1,870	3,740	3,740	3,740	3,740
Bank controls x Cohort FE	y	y	y	y	n	n	n	n
Bank x Cohort FE	y	y	y	y	n	n	n	n
Quarter x Cohort FE	y	y	y	y	n	n	n	n
Bank x Gender x Cohort FE	n	n	n	n	y	y	y	y
Bank x Quarter x Cohort FE	n	n	n	n	y	y	y	y

Notes: This table shows coefficient estimates of Equation (1) in columns (1)-(4) and of Equation (2) in columns (5)–(8) using the stacking method of Gormley and Matsa (2011) and Cengiz et al. (2019). The dependent variable is the NPL ratio for entrepreneurial credit. The results in columns (1)–(4) reflect data on female entrepreneurs, whereas results in columns (5)–(8) reflect data on both female and male entrepreneurs. Columns (1) and (5) report results for all types of entrepreneurs, whereas the remaining columns report results for repeat borrowers (columns 2 and 6), poached borrowers (columns 3 and 7), and first-time borrowers (columns 4 and 8). Standard errors are clustered at the bank level and shown in parentheses. ***, **, and * indicate statistical significance at the 1, 5, and 10 per cent levels, respectively.

Table A.4: Credit supply by WIB participation and dynamic firm-level outcomes

Dependent variable:	Investment	ΔARPK	ΔCOGS	ΔSales	ΔProfit	Exit	ΔCustomers	ΔSuppliers
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Two-year effects								
WiB $\times \Delta \hat{L}_{idst}$	0.141**	-0.012	0.100	0.144***	0.595	-0.060***	0.110*	0.211***
	(0.070)	(0.080)	(0.144)	(0.047)	(0.396)	(0.017)	(0.061)	(0.051)
Non-WiB $\times \Delta \hat{L}_{idst}$	-0.004	-0.028	-0.080	-0.019	-0.192	-0.004	0.052	0.055
	(0.049)	(0.058)	(0.079)	(0.035)	(0.328)	(0.013)	(0.039)	(0.034)
R-squared	0.409	0.396	0.383	0.456	0.311	0.593	0.396	0.387
Observations	47,231	46,817	49,392	48,881	49,392	51,842	39,508	44,650
B. Three-year effects								
WiB $\times \Delta \hat{L}_{idst}$	0.066	0.114	0.117	0.185***	0.329	-0.032**	0.030	0.142***
	(0.079)	(0.090)	(0.138)	(0.052)	(0.388)	(0.016)	(0.062)	(0.048)
Non-WiB $\times \Delta \hat{L}_{idst}$	-0.063	0.115*	-0.107	0.075*	0.190	0.010	0.072	0.064*
	(0.055)	(0.062)	(0.093)	(0.042)	(0.310)	(0.012)	(0.051)	(0.035)
R-squared	0.502	0.481	0.499	0.549	0.377	0.858	0.487	0.503
Observations	43,066	42,533	45,285	44,631	45,285	51,842	36,231	40,904
Year FE	y	y	y	y	y	y	y	y
Firm FE	y	y	y	y	y	y	y	y

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

Table A.5: Effect of change in credit on female firm-level outcomes, IV estimates

Dependent variable:	Investment (1)	ΔARPK (2)	ΔCOGS (3)	ΔSales (4)	ΔProfit (5)	Exit (6)	ΔCustomers (7)	ΔSuppliers (8)
WiB ×ΔCredit	0.131**	-0.029	0.141	0.112***	0.839**	-0.025*	0.063	0.145***
	(0.064)	(0.070)	(0.118)	(0.041)	(0.368)	(0.013)	(0.053)	(0.043)
Non-WiB ×ΔCredit	0.013	-0.087	-0.122	-0.065	0.324	-0.014	0.032	0.087*
	(0.069)	(0.080)	(0.103)	(0.050)	(0.345)	(0.013)	(0.061)	(0.052)
1^{st}-stage F-statistic	56.514	56.514	56.514	56.514	56.514	56.514	38.392	47.228
Observations	51,842	51,842	51,842	51,842	51,842	51,842	42,080	47,502
Mean dep. var.	0.102	-0.049	0.05	0.052	-0.19	0.034	0.006	-0.007
F-test WiB ×ΔCredit = Non-WiB ×ΔCredit	2.925	0.513	4.091	13.46	1.91	0.64	0.254	1.506
p-value	0.088	0.474	0.044	0.000	0.167	0.424	0.614	0.220
Year FE	y	y	y	y	y	y	y	y
Firm FE	y	y	y	y	y	y	y	y

Notes: This table shows second-stage coefficient estimates of an IV regression for different firm-level outcomes as dependent variables. The first stage follows Equation (6) 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 per cent levels, respectively.