Welfare Effects of Digital Credit: A Randomized Evaluation in Nigeria

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I. Introduction

Over the past several years, "digital loans" have transformed the consumer credit landscape in developing countries. These products allow individuals with no formal financial history to access small loans via a mobile phone and have become enormously popular in recent years. In Kenya, a 2018 survey indicated that 27% of all adults had an outstanding digital credit loan—much higher than the number who had microfinance loans (<5%) (Totolo 2018). In Nigeria, despite the low penetration of formal financial services, more than 50 different companies currently offer digital loan products.

In principle, increased access to credit could have positive effects for both households and small enterprises. Yet, despite the strong demand for these loans, critics argue that they may not improve borrower well-being, because

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loan terms are opaque and may induce borrowers to fall deep into debt (Donovan and Park 2019). Interest rates are high—typically from 138% to more than 1,000% annual percentage rate (Francis, Blumenstock, and Robinson 2017)—and are in some cases accompanied by high rates of default (Johnen, Parlasca, and Mußhoff 2021). Lenders have been criticized for using predatory practices on people who have little experience with formal financial products (Hindenburg Research 2020).

In many respects, the debate around digital credit in developing countries echoes that surrounding payday lending in wealthy nations but with higher stakes: these loans are in many cases the only source of formal credit available to billions of people, many of whom live near subsistence levels and have limited access to a social safety net (Francis, Blumenstock, and Robinson 2017).

This paper presents the results of the first randomized controlled trial to assess the welfare effects of digital loans. In partnership with a large financial services provider (FSP) in Nigeria, we increased the availability of credit to a random subset of new loan applicants. Some loan applicants who would normally have been denied credit were approved (the extensive margin treatment); some loan applicants were randomly offered larger initial loans than they would have otherwise received (the intensive margin treatment). After roughly 3 months, we surveyed 1,618 individuals by phone to study the effect of increased access to digital credit.

Our analysis followed a preregistered preanalysis plan and produces several results. First, as expected, being auto-approved for a digital loan increased use of formal credit, as measured several months after the initial loan application.

Borrowing from the FSP increased by US$31 (US$86 PPP) on average.[1] We observe modest substitution away from informal sources of credit and a statistically insignificant improvement in financial health, measured using a standardized 14-question financial health index. In the intensive margin treatment, for each dollar increase in the value of the initial loan offer, total borrowing from the FSP increased by a total of US$1.2 (US$3.4 PPP), including subsequent loans.

Second, being auto-approved for digital credit substantially increased subjective well-being by 0.12 standard deviations. These effects are robust to a variety of econometric specifications, including methods that correct for nonrandom survey attrition. This effect is comparable to the effect of cash transfers and multifaceted antipoverty programs, which are 10–20 times more costly to implement (Ridley et al. 2020). Most of the improvement comes from

[1] Our conversions use the November 2020 exchange rate of 1 USD = 378.78 NGN. Our purchasing power parity (PPP)-adjusted conversions use the exchange rate 1 USD PPP = 135.39 NGN.

reduced indicators of depression, as measured by a standardized Patient Health Questionnaire-9 (PHQ-9) survey; it is also supported by a statistically insignificant increase in reported life satisfaction. In contrast to the large treatment effects of auto-approval, the offering of larger loans has only small and statistically insignificant effects on subjective well-being.

Third, we are able to rule out large effects—either positive or negative—on the other key dimensions of welfare that we prespecified, including income and expenditures, financial health, and resilience to shocks. The absence of significant positive effects may not be surprising given the small size of the initial loan offer (these ranged from roughly US$3 to US$35); however, the absence of significant negative effects suggests that the widespread concern over the predatory nature of these loans may not be justified, at least in our context.

These large effects of providing small amounts of on-demand liquidity are consistent with a growing literature that suggests the inability to access small but critical resources in times of need can be damaging for mental health (Haushofer and Fehr 2014; Banerjee et al. 2020). The quantitative results from our randomized controlled trial are also consistent with the qualitative stated opinions of the FSP's customers: 85% of our sample reported that loan terms were fair, and 94% reported not regretting taking out a loan from the FSP. Likewise, we do not find evidence of some of the behavioral mistakes that are seen with payday lending. In contrast with Allcott et al. (2022), who find that inexperienced payday-lending borrowers in the United States underestimate future borrowing, we find that new applicants actually overestimate future borrowing: applicants predict they have a 62% chance of borrowing from the partner FSP in the next 30 days on average, but in fact only 42% do.

In summary, we do not find substantial negative effects on borrowers. The few robust, significant effects we observe are positive, and access to digital credit has a substantial positive effect on subjective well-being. One caveat to this generally positive assessment is that our study focuses on the relatively short-term effects of small loans to new borrowers; we cannot say whether different effects would be observed over longer time horizons to long-term customers.

Related literature. This paper complements two recent quasi-experimental evaluations of the welfare effects of digital credit that exploit discontinuities in loan approvals that are based on credit score. Suri, Bharadwaj, and Jack (2021) find small but generally positive longer-term effects of digital loans in Kenya, particularly with respect to household resilience to shocks. Brailovskaya, Dupas, and Robinson (2024) find some evidence of positive effects on (self-reported) financial well-being from digital loans in Malawi. They also find that experimentally giving borrowers additional information about the (high) fees and risks of default increased demand for digital credit.

Our results also relate to a larger literature on the welfare effects of expanding credit access in low- and middle-income countries. Most relevant to our results, Angelucci, Karlan, and Zinman (2015) and Fernald et al. (2008) find that access to microfinance reduces symptoms of depression, though Fernald et al. (2008) also observe it increases stress. We compare our results on subjective well-being to these and other studies in section IV.D after presenting our main results. More broadly, empirical studies of credit have highlighted the high returns to capital for small enterprises (de Mel, McKenzie, and Woodruff 2008, 2009; Karlan et al. 2014) and heterogeneous effects on household consumption and welfare (Karlan and Zinman 2010; Attanasio et al. 2015; Augsburg et al. 2015; Banerjee et al. 2015; Crépon et al. 2015; Tarozzi, Desai, and Johnson 2015; Meager 2019).[2] However, the digital loans that we study are different from typical microfinance loans: they are much smaller, can be accessed instantaneously, are shorter-term, and typically charge substantially higher interest rates. Thus one might expect these digital loans to be more subject to behavioral impulses and to be used for different needs—and thus to have different effects.

The debate around digital credit also parallels concerns around payday lending in wealthy nations, which also offers repeat, short-term, high-interest-rate loans (cf. Bhutta, Skiba, and Tobacman 2015). That literature documents both positive and negative effects on borrowers (Zinman 2010; Melzer 2011, 2018; Morse 2011; Morgan, Strain, and Seblani 2012; Carrell and Zinman 2014; Bhutta, Skiba, and Tobacman 2015; Bhutta, Goldin, and Homonoff 2016; Gathergood, Guttman-Kenney, and Hunt 2019; Skiba and Tobacman 2019).

II. Setting

Our study population is a random sample of new applicants of a popular digital credit lender in Nigeria. Nigeria has relatively high rates of financial inclusion relative to neighboring countries: 51% of adults report using formal financial services (EFInA 2021). An estimated 89% of Nigerians own a mobile phone, and 28% of adults report using digital financial services (EFInA 2021).

A. The Digital Credit Product

Our study examines the welfare effects of small loans offered by a private FSP in Nigeria. Consumers can apply for loans via the FSP's smartphone application ("app"), which requests access to behavioral data from their smartphone. The FSP predicts creditworthiness from these data by using a proprietary algorithm (similar to that proposed by Björkegren 2010; Björkegren and Grissen 2020).

[2] Our auto-approval design is similar to the design of Karlan and Zinman (2011), which similarly identifies effects of access to finance for borrowers that were otherwise below the threshold to qualify.

Approved applicants are presented with a menu of three loan offers of different value, which are determined by the FSP. Applicants must have a bank account to register but do not need a formal financial history.

In general, loans range from 1,000 Nigerian naira (NGN), which was roughly 2.60 USD at the time, to 200,000 NGN (528 USD).[3] Applicants can opt to apply for smaller loans than the maximum loan offered. Loans are typically due after 28 days, and the interest rates we observe range from 15% to 22% per month (implying an annual percentage rate of 195% to 287%). The FSP is generally tolerant of late repayments but can charge a late fee of 6%. If a customer defaults, that customer is ineligible to apply for future loans from the FSP and could be reported to the Credit Reference Bureau, which would limit that individual's access to loans from other digital providers. But if a customer repays his or her loan on time, that individual becomes eligible for larger loans.

In our study sample ($N = 1{,}618$), the average initial loan amount is approximately 5,600 NGN (15 USD); over the roughly 3 months between enrollment and survey, average total borrowing is 21,300 NGN (56 USD). Figure A1 (figs. A1–A15 are available online) shows how loan values increase as customers repay prior loans. In this sample, we observe that 9% of borrowers default on their first loan, while 24% default at least once over the course of the study. Seven percent of all loans end in default.

The product we examine is broadly similar to other digital credit products offered across sub-Saharan Africa (Francis, Blumenstock, and Robinson 2017). In particular, it is similar to the M-Shwari loan product in Kenya analyzed by Suri, Bharadwaj, and Jack (2021) and the Kutchova product in Malawi analyzed by Brailovskaya, Dupas, and Robinson (2024), though our FSP's loans tend to be slightly larger.[4] Loan default rates can be high; in Malawi, Brailovskaya, Dupas, and Robinson (2024) report a default rate of 15%, while Suri, Bharadwaj, and Jack (2021) report that 6.5% of control households default on their first loan.

[3] For context, the legally mandated monthly minimum wage in Nigeria was 30,000 NGN. The value of the initial loan offer is capped at 35.75 USD for our study respondents, who are first-time borrowers.

[4] For M-Shwari and Kutchova, applicants must have a mobile money account for at least 6 months. Monthly interest rates are 7.5% and 10%, respectively, and both lenders charge a late fee (7.5% and 2.5%, respectively). We summarize features of these loan products in table A1 (tables A1–A16 are available online). In Suri, Bharadwaj, and Jack (2021), the average loan size (conditional on borrowing) is approximately US$4.80, and customers borrow roughly US$40 over the 18-month study period. In Brailovskaya, Dupas, and Robinson (2024), the average loan size is roughly US$4.00, and the average total value of all loans taken out over 3 months is roughly US$18 (conditional on borrowing).

B. Descriptive Evidence

Qualitative surveys suggest that borrowers like the FSP's product, and demand for loans is high. Among the approved applicants we observe in the data from the FSP, 85% take out a loan. Among those surveyed (details on the survey are provided below), 86% report that the FSP's loan terms are fair, and 94% of borrowers report not regretting taking out a loan from the FSP.

We also look for evidence of the sort of behavioral trap observed with payday loans by Allcott et al. (2022), who find that payday borrowers frequently underestimate future borrowing. However, we find that our borrowers actually overestimate future borrowing from the FSP (figs. A2 and A3): the average surveyed applicant predicts that he or she has a 62% chance of borrowing from the FSP in the next 30 days, whereas in practice only 42% borrow within that period. As in Allcott et al. (2022), the magnitude of misprediction decreases with experience (measured by the number of FSP loans taken out prior to survey).

III. Experimental Design and Estimation Strategy

As part of a research collaboration with the partner FSP, a randomly selected sample of the FSP's applicants was included in a randomized controlled trial (RCT) to measure the effect of digital loans on well-being. This section describes the experimental design, the data we collected, and our estimation strategy, all of which was preregistered in our preanalysis plan (American Economic Association RCT Registry no. AEARCTR-0005029)

A. Experimental Design

As part of its normal business operations, the partner FSP frequently runs RCTs (A/B tests). We worked with the FSP to launch a new RCT, which included a randomly selected 8% of all new applicants who installed the app between August 2019 and February 2020. Applicants were cross-randomized across two different treatment arms:

- Auto-approval treatment. Half of all participants (4% of all new applicants) were automatically approved for credit, regardless of credit score—we refer to this as the "auto-approval" group. The other half of participants ("standard approval" group) were approved for an application only if their proprietary credit score at the time of application exceeded a threshold set by the FSP (at any given time, a uniform threshold was applied to all applicants, but the value of the threshold changed periodically).[5]

[5] Applicants in both groups could still be denied credit if their application raised fraud detection flags.

TABLE 1

TREATMENT ASSIGNMENT: SURVEY SAMPLE

Initial Offer (NGN)	Standard Approval (1)	Auto-Approval (2)
1,000	.18	.17
2,000	.20	.16
5,000	.20	.22
10,000	.21	.21
13,000	.22	.24
Observations	984	634

Note. Subjects were randomly assigned the value of their initial loan (as shown) and whether they were subject to the standard approval process with a minimum credit-score requirement (col. 1) or auto-approval that did not have a minimum credit score (col. 2). Each cell in cols. 1 and 2 indicates the proportion of subjects (of that column) assigned to each initial loan value.

- Initial loan value. All applicants who were approved received a randomly assigned maximum initial loan offer, selected from 1,000, 2,000, 5,000, 10,000, or 13,000 NGN (between about 2.75 and 35.75 USD). Customers who repaid their initial loan on time would subsequently be eligible for future loans according to the FSP's standard loan ladder.

Table 1 summarizes treatment assignment for the individuals in our study. The cross-randomized design thus allows us to study the extensive margin effect of receiving access to any loan (via auto-approval randomization) as well as the intensive margin effect of receiving access to larger loans (via loan-value randomization). However, because of the practical constraints imposed by the lender's business operations, we estimate these effects on slightly different populations. In particular, while we can study intensive margin effects for all borrowers who were approved for loans, we can estimate the extensive margin effect only on borrowers whose credit score was below the threshold. This is because the lender was not interested in randomly denying loans to people who would otherwise have been approved—and we did not want to push for such a design for ethical reasons. Nonetheless, there is inherent interest, both for lenders and for regulators, in understanding the consequences of expanding loan access to borrowers below existing credit-score thresholds; indeed, understanding the profitability of this population was one reason why the lender we worked with conducted A/B tests as part of its normal operations.

1. Subject Recruitment, Surveys, Attrition, and Weighting

Altogether, 29,772 applicants were potentially eligible for a loan and assigned to one of our treatment arms.[6] All of these individuals were invited via text

[6] Some individuals were never assigned a credit score and thus were not eligible for a loan, typically because they never opened the app after installation or their data did not successfully upload to the

message to participate in a phone survey. Invitations were staggered over time to ensure that we could quickly follow up with a phone call to the respondent; on average, about 1,500 individuals were invited per week over a period of 20 weeks.[7] We began surveying applicants roughly 3 months after the first enrollment, starting the week of November 11, 2019, and concluding the week of February 7, 2020. We were unable to conduct any additional surveys after this time because of the onset of the COVID-19 pandemic, which, among other disruptions, led the FSP to pause operations in Nigeria. In total, we contacted roughly 3,000 applicants via phone, and our main analysis focuses on the sample of 1,618 applicants who responded to our phone calls and completed the survey (approximately 5% of all applicants; see fig. A4).

To account for a potentially nonrandom survey response, our empirical analysis uses sample weights. Appendix A1 (apps. A1–A3 are available online) discusses in greater detail the logic behind these weights, but in practice our findings are robust to several alternate approaches to weighting and to bounding exercises (Lee bounds). The main results presented in this paper use a parsimonious approach that weights by the inverse probability of response for each week and treatment arm. In appendix A1, we additionally report results based on a specification that uses no sample weights, as well as a specification that uses "enriched" inverse probability weights that model nonresponse as a function of a rich set of observables derived from administrative data from all subjects, including those who do not respond to the survey.[8] We also examine the robustness of our main results to attrition by using Lee bounds (Lee 2009), which involves trimming our data by assuming that differential attritors are located at the top (or bottom) of the distribution.

2. Sample Characteristics and Balance

As shown in tables A2 and A3, applicants in our sample are predominantly male (76%), about 30 years of age on average, and educated at the secondary school or university level. Respondents are distributed across the various states

FSP. At the time of implementation and surveying, we did not yet know who these individuals would be, so in practice we also have administrative data on 17,165 individuals (and survey data on 440 of these individuals) who never were assigned credit scores, which we do not use in this study.

[7] We paused invitations (and surveys) during the Christmas holiday period in December 2019.

[8] This "enriched" weighting scheme relies on the assumption that survey response should not relate to our survey outcomes after conditioning on the outcomes observed in administrative data. Specifically, we use a logistic function to model survey response as a function of treatment assignment and several measures of past activity captured on the FSP's app, including the number of loan applications and rejections, number of approved loans, and number of repayments and default. As shown in table A8, we find that while survey response is correlated with the auto-approval treatment, this effect is not significant after controlling for the administrative outcomes used to construct the enriched weights.

of Nigeria, with Lagos having the largest share (34%). A majority of respondents are employed in either their own business (40%) or in salaried jobs (39%). Our study population is thus not representative of the full Nigerian population but is broadly representative of bank account and smartphone owners (see table A4, which uses data from Demirgüç-Kunt et al. [2022] to compare characteristics of account owners with those of nonaccount owners).

Characteristics are balanced across treatment arms (table A5). We test for balance in a number of ways. First, we examine balance between the auto-approval and standard approval arms for each individual characteristic (col. 2). Then, we report the F-statistic from a joint test of significance of all fixed characteristics (col. 3). Finally, we test whether the initial loan offer amount is independent of each characteristic (col. 6). Overall, we find no significant differences between the average characteristics of the auto-approval and standard approval groups, except for the initial amount offered to applicants in Lagos.

B. Estimation Strategy

We are interested in understanding how use of digital credit affects the welfare of applicants. Our two randomized treatments Z_i^1 and Z_i^2 create exogenous variation in credit access and use. We estimate the effect of these treatments on each outcome Y_i by using regressions of the form

$$\begin{aligned} Y_i = \pi_0 + \pi_1 Z_i^1 \times \text{Underthreshold}_i + \pi_2 Z_i^1 \times \text{Overthreshold}_i \\ + \pi_3 Z_i^2 + \boldsymbol{\pi}_4 \mathbf{X}_i + \nu_{\text{week}} + \nu_{\text{enumerator}} + \varepsilon_i. \end{aligned} \tag{1}$$

To reduce sampling variation, we include a prespecified vector of controls ($\boldsymbol{X}_i$: respondent gender, education, ethnicity, location, age, household size, head of household, whether the individual was below the credit-score threshold at the time of enrollment) and fixed effects for week of enrollment and the survey enumerator (ν_{week} and $\nu_{\text{enumerator}}$). All regressions in our analysis use the weights described in section III.A.1.

We have two randomized treatments. The first, Z_i^1, is a dummy variable indicating whether the respondent is assigned to the auto-approval group. Because this treatment primarily affects the eligibility of applicants whose credit score would normally disqualify them from receiving a loan, we interact it with a dummy variable indicating whether the respondent's credit score was below the threshold normally required for approval at the time of enrollment (i.e., $Z_i^1 \times \text{Underthreshold}_i$ and $Z_i^1 \times \text{Overthreshold}_i$).[9] We are primarily

[9] Credit scores change over time and individuals may reapply, so some individuals who are initially above the threshold may still be affected by auto-approval in subsequent loan applications.

interested in the coefficient associated with $Z_i^1 \times \text{Underthreshold}_i$, which indicates the effect of being automatically approved to receive a loan for an individual who would have normally been rejected. To account for baseline differences between borrowers above and below the threshold, we always include the uninteracted control (Underthreshold_i), which is not randomly assigned. The second treatment, Z_i^2, is the value of the largest initial loan offered to approved applicants; this value is randomly assigned from the set {1,000, 2,000, 5,000, 10,000, 13,000}.[10] Our main specification does not include an interaction between Z_i^1 and Z_i^2; results including the interaction are included in table A16.[11] Overall, our experiment focuses on borrowers with low credit scores, and the "treatment effects" we report can be interpreted as a local average treatment effect for individuals below the threshold (at the time of installation), using individuals below the threshold (assigned to control/standard approval) as the reference group.

IV. Results

Our main analysis highlights three sets of results. First, we show how our two randomized treatments—and in particular the extensive margin that auto-approves loans for applicants with low credit scores—increase borrowing and affect other financial behaviors of applicants. Second, following our preanalysis plan, we show how increased access to loans affects several prespecified indexes of welfare; while most effects are statistically insignificant, there are large and significant improvements in subjective well-being. Third, we explore the subjective well-being results to better understand where these effects may be coming from and to contextualize them relative to related interventions. Results are similar under different assumptions on sample weights, except where specified (see app. A1.1).

A. Effects on Borrowing

The effects of the two randomized treatments on the financial behaviors of applicants are shown in table 2. The first two rows indicate the effect of the extensive margin treatment, being auto-approved for a loan. We show the effect separately for people below (row 1) and above (row 2) the minimum credit-score

[10] In principle, this treatment does not affect those who are ineligible to borrow (i.e., those who are under the approval threshold in the standard approval group). In table A15, we additionally report results from a specification that pools the effect of the initial offer treatment over all eligible applicants.

[11] As Muralidharan, Romero, and Wüthrich (2025) point out, the effects from our main specification represent the "composite effect" of a weighted average over all interactions with other treatments. This is because we are primarily interested in π_1, i.e., the average effect of auto-approval on applicants under the threshold, across all initial loan values.

TABLE 2

EFFECTS OF DIGITAL CREDIT ACCESS ON BORROWING AND FINANCES

	Borrowing						Finances		
	Total Borrowing from FSP (NGN) (1)	Any Loan (p.p.) (2)	Any Non-FSP Loan (p.p.) (3)	Formal Borrowing Index (SD) (4)	Informal Borrowing Index (SD) (5)	Loans Taken Out/ Total Income (6)	Total Saving (Asinh) (7)	Income (Category) (8)	Expenditure (Asinh) (9)
Auto-approval × Underthreshold	11,687.9	.368	−.122	.236	.088	.079	−.755	−.136	.010
	(1,799.8)***	(.051)***	(.061)**	(.025)***	(.024)***	(.014)***	(.730)	(.245)	(.252)
Auto-approval × Overthreshold	1,226.0	.009	.012	.011	−.011	−.010	−.425	.166	.104
	(1,476.4)	(.018)	(.029)	(.010)	(.011)	(.011)	(.362)	(.112)	(.106)
Initial offer (thousands of NGN)	1,234.6	−.001	−.005	.002	.001	.005	−.008	.008	−.012
	(136.2)***	(.002)	(.003)*	(.001)*	(.001)	(.001)***	(.034)	(.012)	(.010)
Mean dependent variable (standard approval group)	20,036.8	.832	.457	−.000	−.000	.101	6.340	2.370	9.960
Observations	1,611	1,611	1,611	1,611	1,611	1,553	1,440	1,553	1,473

Note. Each column is a separate regression. Each regression controls for respondent gender, education, head of the household, ethnicity, location (state), household size, age, and respondent's credit-score status (1 = underthreshold) at the time of enrollment. We include enumerator and week of enrollment fixed effects. Twenty-nine respondents did not report their age—we code these values as 0 and include a dummy variable that controls for these missing values. The index variables in cols. 4 and 5 include data on the number and amount of loans from formal and informal sources, respectively. All regressions include weights (inverse probability of responding by week and treatment arm) as described in sec. III.A.1. In cols. 6 and 7, monthly income is an ordinal variable, defined using the following brackets: 0–9,999 NGN, 10,000–49,999 NGN, 50,000–99,999 NGN, 100,000–249,999 NGN, and ≥250,000 NGN. The outcome variable in col. 6 is the ratio of self-reported borrowing (over 3 months, in NGN) and self-reported income (over 3 months—we use the midpoint of each respondent's monthly income brackets and multiply by 3). Further details on how each outcome variable is constructed are provided in app. A2.3. The coefficients in col. 7 are from an ordinal logit regression. Parentheses contain robust standard errors. p.p. = percentage points.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

threshold.[12] The third row indicates the effect of the intensive margin treatment, the randomly assigned largest initial loan offer.

Broadly, we find that both treatments increase the amount that applicants borrowed from the partner FSP but that only the extensive margin treatment increases the likelihood that applicants take out any loan. The auto-approval treatment also affects other aspects of financial behavior but generally only among applicants below the credit-score threshold.

In greater detail, column 1 of table 2 reports the effects of both treatments on the total amount borrowed from the FSP (as observed in administrative data from the FSP, over the period between when the applicant installed the app and when the applicant was surveyed). For applicants under the threshold at the time of enrollment (row 1), auto-approval increases borrowing from the FSP by 11,688 NGN (31 USD, or 86 USD PPP). The next row indicates that, for applicants above the threshold, auto-approval increases borrowing by a statistically insignificant 1,226 NGN. In the third row, we observe that, for each additional 1,000 NGN offered in the initial loan, borrowing from FSP increases by 1,235 NGN. Because the value of the initial loan ranges from 1,000 to 13,000 NGN, the initial-offer treatment induces a predicted difference in borrowing as large as 16,000 NGN. For comparison, individuals in the standard approval (i.e., not auto-approved) group borrow a total of 20,000 NGN on average from the FSP.

The remaining columns of table 2 indicate the effects on other financial behaviors. In column 2, we observe that auto-approval increases the proportion of applicants under the threshold who take out any loan by 37 percentage points. This effect is driven by having a loan from the FSP: column 3 indicates that auto-approval lowers the proportion of applicants with a non-FSP loan. The value of the randomly assigned initial offer has no effect on taking out any loan (col. 2) or on the proportion of non-FSP loans (col. 3).

Columns 4 and 5 of table 2 indicate that increased access to digital credit causes applicants to substitute from informal credit toward formal credit. For applicants below the credit-score threshold, the auto-approval treatment increases an index of formal borrowing by 0.24 standard deviations and decreases an index of informal borrowing by 0.09 standard deviations. Each index is the weighted average of the *z*-scores of the number and amount of loans reported taken out in the past 3 months from formal sources (digital credit, bank, microfinance, or cooperative) or informal sources (friends and family, moneylenders, or airtime

[12] The auto-approval treatment could, in principle, affect people who were above the credit-score threshold at the time of enrollment if later the individual's credit score decreased or the threshold were raised. In practice, such effects are generally small and insignificant (row 2 of table 2).

credit). This substitution away from informal credit is driven by a large reduction in borrowing from friends and family and a small reduction in borrowing from moneylenders; there is no effect of our treatments on the use of other digital lenders, banks, or cooperatives—see also figure A5 for a visual summary of these effects.[13]

Both the extensive and intensive treatments significantly increase the applicant's ratio of loans taken out to income (both for 1 month; col. 6 of table 2)—the closest our data will allow us to get to a debt/income ratio. Auto-approval increases loans taken out by 7.9 percentage points of income on average for applicants under the threshold; likewise, each additional 1,000 NGN in the initial loan increases this ratio by 0.5 percentage points. Relative to the mean ratio of 10.0% of income in the standard approval group, these are substantial increases. However, in absolute terms, households have limited use of credit (compare, for instance, to the United States, where the average ratio of household debt payments to income is nearly 100% [Ahn, Batty, and Meisenzahl 2018]). Column 7 of table 2 reports the effect on savings. Although our estimates are not statistically significant for either treatment, their magnitude suggests being auto-approved may result in slight depletion of savings. For applicants under the credit-score threshold, the point estimate implies an average depletion of savings of 433–601 NGN (1.14–1.59 USD), depending on sample weights, but is never statistically significant.[14] For applicants above the threshold, our main estimates imply a range of depletion between 531 and 565 NGN (1.40 and 1.49 USD), but under alternate assumptions of sample weights, we observe depletion of savings of 2,359 NGN (6.24 USD), which is statistically significant at the 95% level (see app. A1.1).

Finally, columns 8 and 9 of table 2 indicate that neither treatment had significant effects on the applicant's self-reported income or expenditures. The lack of an effect on these outcomes is consistent with prior studies that find limited effects of microcredit on consumption or expenditures. Figure 1 contextualizes our results within this literature: the first row shows the point estimate and 95% confidence interval of our main treatment effect (of auto-approval on people under the credit-score threshold), and the remaining rows and coefficient plots indicate the estimates reported from several related papers.

[13] As context, 80% of those in our sample report borrowing from the partner FSP, and a third of those in our sample report borrowing from other digital sources. Borrowing from nondigital formal sources is limited; only 6% of those in our sample report borrowing from a bank, and only 2% of those in our sample report that they borrow from a microfinance institution or from a cooperative. Table A3 compares self-reported and administrative data about borrowing.

[14] We convert our main estimates from units of inverse hyperbolic sine to levels by predicting the average difference in levels that these estimates would imply within the sample.

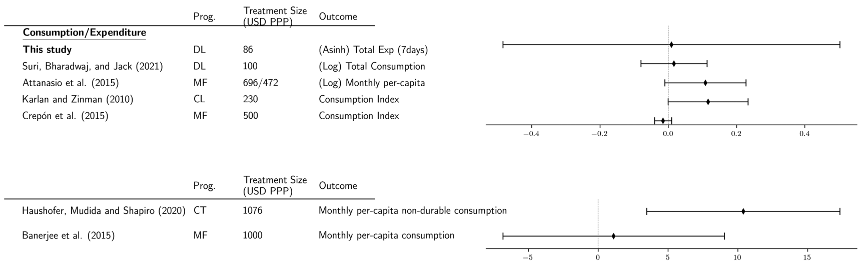

Figure 1. Effect size comparisons: consumption/expenditure. Plotted are the estimated treatment effects on expenditure from evaluations of digital credit products and various antipoverty programs. The first row indicates the point estimate from this study (i.e., the auto-approval × underthreshold coefficient, based on average borrowing from the partner FSP in the past 3 months). Subsequent rows indicate point estimates of the treatment effect from other studies. Treatment effects are in standard deviations in the top right plot and in USD PPP in the bottom right plot. Black bars indicate 95% confidence intervals. The "Prog." (program) column refers to the type of intervention: DL refers to digital loans, CT refers to cash transfers, CL refers to consumer loans, and MF refers to microfinance. The "Treatment Size" column is the size of the treatment in USD PPP. For this study and Suri, Bharadwaj, and Jack (2021), we report the mean total loan amount borrowed. For results from studies that focus on microfinance, we report the initial treatment loan size, as summarized in table 1 of Banerjee, Karlan, and Zinman (2015). For Haushofer, Mudida, and Shapiro (2020) we report the size of the cash transfer.

As shown in table 3, we find that applicants below the credit-approval threshold are less likely to repay loans overall. During the 3-month window, 21% of borrowers above the threshold default at least once, relative to 37% of borrowers below the threshold (the difference is significant with $p < .01$). This is, of course, expected, because the credit score is largely designed to model predicted default.

TABLE 3
DEFAULT BY TREATMENT GROUP

	Observations (1)	All (2)	Standard Approval (3)	Auto-Approval (4)	*t*-Statistic (5)	*p*-Value (6)
Credit score:						
Overthreshold	1,204	.21	.22	.20	.22	.83
Underthreshold	145	.37	.39	.36	1.16	.25
Initial offer:						
1,000	230	.20	.21	.19	.47	.64
2,000	249	.20	.22	.16	1.07	.29
5,000	275	.20	.18	.22	−.96	.34
10,000	288	.25	.25	.25	.14	.89
13,000	307	.28	.26	.30	−.75	.46
All	1,349	.23	.23	.23	−.19	.85

Note. Presented is a summary of default rates among borrowers (individuals who took out at least one loan from the FSP). Column 1 contains the number of observations in each group (row). Column 2 summarizes default rates for the full sample, while cols. 3 and 4 summarize the default rates in the standard approval and auto-approval groups, respectively. Columns 5 and 6 contain the *t*-statistics and *p*-values, respectively, from a *t*-test comparing cols. 3 and 4.

B. Welfare Effects

Beyond the direct effects on borrowing, we evaluate the effect of access to digital credit on several key dimensions of applicant welfare. We focus on four families of outcomes that we preregistered and prespecified prior to conducting the endline survey: financial health, resilience, women's economic empowerment, and subjective well-being.[15] For each family, we construct a summary index that aggregates multiple related variables. We standardize each variable by subtracting the mean and dividing by the standard deviation of the standard approval group (i.e., the control group for the auto-approval treatment). We then construct the summary index as the weighted mean of the *z*-scores of the component variables. (See app. A2.3 for full details of how each index is constructed.) In the event that a family has more than one summary index of interest, we report *p*-values that adjust for multiple hypothesis testing (using the Sidak-Holm adjustment). The effect of our two randomized treatments on these four families of outcome indexes are presented in table 4.

Financial health. Results in column 1 of table 4 indicate that neither treatment significantly affected an index of the overall financial health of the applicant, as measured by the respondent's answers to 14 standardized questions (Consumer Finance Protection Bureau 2017), though the point estimate is slightly positive (the index is scaled to range between 0 and 1). Additionally, our confidence intervals rule out large negative effects on this index: the coefficient on the auto-approval treatment has a lower bound of −0.009, and the lower bound on the initial offer treatment is −0.0024. Among the 14 individual questions, the two treatments had generally beneficial but statistically insignificant effects (fig. A6).

Resilience. Increased access to digital credit did not significantly affect the applicant's self-reported ability to cope with negative shocks. Column 2 of table 4 shows the effect on the applicant's ability to experience a negative economic shock without forgoing expenditure or adjusting behavior. The coefficient estimate is negative but noisy and not statistically significant (95% confidence interval: −0.36 to 0.33 SD). This index, based on the questions used in Suri, Bharadwaj, and Jack (2021), is defined for respondents who reported experiencing at least one shock in the 3 months prior to survey (82% of the total sample). We find no evidence that our randomized treatments affect the shocks a person experiences (fig. A7).

Column 3 of table 4 reports an index of the applicant's ability to pay a large amount in an emergency and manage without income. The coefficient is very small and close to zero, and the confidence intervals are fairly tight. Figure A8

[15] Deviations from the preanalysis plan are described in app. A2.2.

TABLE 4
EFFECTS OF DIGITAL CREDIT ACCESS ON PRESPECIFIED MEASURES OF WELFARE

		Resilience			
	Financial Health Index (1)	Financial Resilience Index (SD) (2)	Resilience Index (SD) (3)	WEE Index (SD) (4)	Subjective Well-Being Index (SD) (5)
Auto-approval × Underthreshold	.024	.001	−.019	−.067	.121
	(.018)	(.001)	(.179)	(.069)	(.032)***
	[.164]	[.631]	[.915]	[.331]	[.000]
Auto-approval × Overthreshold	.005	.000	.097	.040	.002
	(.008)	(.000)	(.078)	(.038)	(.016)
	[.566]	[.574]	[.382]	[.284]	[.912]
Initial offer (thousands of NGN)	.001	.000	−.002	−.002	.002
	(.001)	(.000)	(.007)	(.004)	(.001)*
	[.102]	[.608]	[.814]	[.502]	[.100]
Underthreshold	−.007	−.001	−.179	−.020	−.065
	(.014)	(.001)	(.136)	(.051)	(.027)**
	[.618]	[.296]	[.296]	[.699]	[.017]
Mean dependent variable (standard approval group)	.704	.000	.000	.004	.000
Observations	1,611	1,403	1,312	1,611	1,611

Note. Each column is a separate regression. Details on how each index is constructed are provided in app. A2.3. In brief: col. 1 includes 14 standardized questions about financial health; col. 2 includes 7 questions about coping with negative shocks (conditional on having experienced a negative shock); col. 3 includes two questions about the respondent's ability to access resources in the event of a shock; col. 4 is an index of women's economic empowerment (WEE) that includes data on female decision-making, purchases and mobility, and beliefs about female autonomy; col. 5 includes a measure of self-reported life satisfaction and a standardized measure of depression. Each regression controls for respondent gender, education, head of the household, ethnicity, location (state), household size, age, and respondent's credit-score status (1 = underthreshold) at the time of enrollment. We include enumerator and week of enrollment fixed effects. Twenty-nine respondents did not report their age—we code these values as 0 and include a dummy variable that controls for these missing values. Parentheses contain robust standard errors, and square brackets contain *p*-values. For resilience outcomes, we report *p*-values after adjusting for multiple hypothesis testing by using the Sidak-Holm adjustment. All regressions include weights (inverse probability of responding by week and treatment arm) as described in sec. III.A.1.
* $p < .10$.
** $p < .05$.
*** $p < .01$.

shows the effect on each of the individual responses that compose the index; most effects are insignificant, though there is suggestive evidence that auto-approval helps applicants manage shocks without having to sell household assets (fig. A8, row 7).

We note that the result in column 2 of table 4 differs from the significant increase in resilience documented by Suri, Bharadwaj, and Jack (2021), which finds that individuals just approved for credit are significantly less likely to forgo expenses when faced with a shock (their estimated coefficient, 0.063; SE, 0.030). The lower bound on the 95% confidence interval is −0.00075 SD, which rules out large negative effects on resilience.[16] An important difference between the two contexts is that their study population had digital credit accounts for at least 18 months prior to being surveyed, whereas we observe effects after roughly 3 months.

Women's economic empowerment. Several recent studies document the potential for financial services to empower women in developing countries (e.g., Suri and Jack 2016; Field et al. 2021). In our setting, we do not find consistent evidence that increased access to digital credit affects women's economic empowerment. Our focal outcome in column 4 of table 4 is a summary index that aggregates data on female decision-making, purchases and mobility, and beliefs about female financial autonomy. Beliefs were asked of all respondents. Female behavior is asked of respondents who were either married ($N = 551$) or had a live-in partner ($N = 56$); mobility was asked also of the women who did not fall into those categories. In all cases, we elicited responses about the affected woman in the household: either the respondent herself (if the respondent is a woman) or the respondent's spouse or live-in partner (if the respondent is a man and has a female partner).

Because our study population turned out to be 79% male, it is perhaps not surprising that effects on the summary index are not statistically significant (95% confidence interval: -0.20 to 0.06 SD; col. 4 of table 4). We observe some evidence of positive effects on the decision making and mobility indexes and negative effects on the purchase and financial autonomy indexes in table A6, but these effects are not statistically significant after adjusting for multiple hypothesis testing.[17]

These results are broadly consistent with prior studies that find no or limited effects of microcredit on women's empowerment (as summarized later in fig. 3). The most straightforward comparison is to studies that report summary indexes (i.e., Banerjee et al. 2015; Crépon et al. 2015; Karlan and Zinman 2011): in all cases, we observe that our confidence intervals overlap.

C. Subjective Well-Being

Perhaps our most notable finding is that access to digital credit increases subjective well-being substantially by 0.12 standard deviations (95% confidence interval: 0.05 to 0.18 SD) in the first row of table 4, column 5. In contrast, the amount that a borrower is allowed to access (row labeled "Initial offer") has a

[16] Our prespecified measure of resilience differs slightly from that used by Suri, Bharadwaj, and Jack (2021). In results not shown, we construct a measure of resilience exactly following Suri, Bharadwaj, and Jack (2021; their table 4A). We find no effect of auto-approval on this measure (coefficient, 0.04; SE, 0.05), but the 95% confidence intervals overlap, so we are unable to reject that effects are of the same size.

[17] Effects are generally not significant for the components of each of these indexes, as seen in fig. A9.

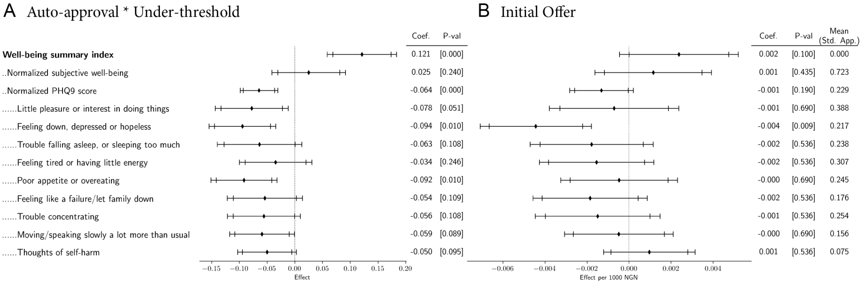

Figure 2. Subjective well-being. Presented are reduced-form results for measures of subjective well-being. The regression specification is described in section III.B. Whiskers represent 95% and 90% confidence intervals. In each regression, we control for respondent gender, education, ethnicity, location (state), household size, head of household, age, and respondent's credit-score status (1 = underthreshold) at the time of enrollment. We also include enumerator and week of enrollment fixed effects. All regressions include weights (inverse probability of responding by week and treatment arm) as described in section III.A.1. Because we have only one main prespecified outcome (the well-being summary index) for this family, we report the unadjusted *p*-value for this outcome. We adjust *p*-values for false discovery rate (FDR) for the normalized subjective well-being question and the normalized PHQ-9 score. We also adjust *p*-values for FDR for the nine components of the PHQ-9 scale. Note that the PHQ-9 scale can range from 0 to 27; for ease of visual presentation, we divide the total PHQ-9 score for each respondent by 27, so that the value ranges from 0 to 1. Lower values on the PHQ-9 scale indicate lower levels of depression. Thus negative coefficients for the normalized PHQ-9 score and its components represent improvements in depression.

very small and statistically insignificant effect on subjective well-being. We measure subjective well-being with a summary index that places equal weight on self-reported life satisfaction and a standardized measure of depression, that is, the nine questions from the PHQ-9. What components of the well-being index are driving the aggregate result? As can be seen in figure 2*A*, the positive effect of loan access stems largely from an improvement in the PHQ-9 score. Loan auto-approval causes applicants to report being less depressed and report feeling less likely to suffer from poor appetite or overeating. We find small effects on a number of other components of the PHQ-9, though most of these are only significant at the 10% level after multiple hypothesis testing adjustments. In analysis that was not prespecified (due to an anticipated lack of statistical power), we test whether certain subgroups were most likely to see increases in subjective well-being but do not detect any statistically significant heterogeneity (see table A7). We also find that this result is robust to alternative weighting (table A10) and bounding exercises (table A14).

D. Discussion

The improvements in subjective well-being we find are large and comparable with those of intensive antipoverty interventions that are transfers that need not be repaid. This comparison can be seen in the left panel of figure 3. For instance, the meta-analysis by Ridley et al. (2020) finds that multifaceted

	Prog.	Treatment Size (USD PPP)	Outcome
Subjective Wellbeing			
This study	DL	86	Subjective Wellbeing Index
Ridley et al. (2020)	MA	1224	Misc.
Haushofer, Mudida and Shapiro (2020)	CT	1076	Psychological Wellbeing Index
Karlan and Zinman (2010)	CL	231	Mental Health Index
Karlan and Zinman (2011)	MF	621	Subjective Wellbeing Index
Angelucci, Karlan and Zinman (2015)	MF	451	Depression Index
Augsburg et al. (2015)	MF	1648	Perceived Stress Index
Women's Economic Empowerment			
This study	DL	86	HH Dec. Making Index
Angelucci, Karlan and Zinman (2015)	MF	451	HH Dec. Making (No. of household issues)
Tarozzi et al. (2015)	MF	500	HH Dec. Making (fraction of issues)
This study	DL	86	Financial Dec. Making Index
Angelucci, Karlan and Zinman (2015)	MF	451	Financial Dec Making (Any participation)
Banerjee et al. (2015)	MF	1000	Index of women independence
Crepón et al. (2015)	MF	1082	Index of women independence
This study	DL	86	Purchase Index
This study	DL	86	Mobility Index
Haushofer, Mudida and Shapiro (2020)	CT	1076	Intimate partner violence
Karlan and Zinman (2010)	CL	231	Control and Outlook Index
Angelucci, Karlan and Zinman (2015)	MF	451	HH Conflict (No of household issues)

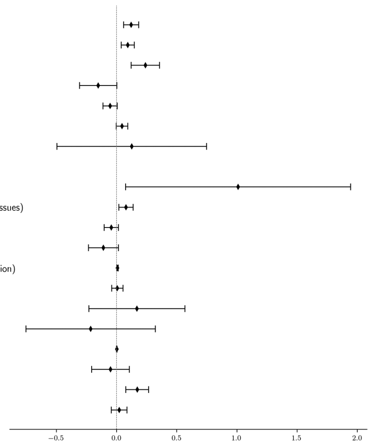

Figure 3. Effect size comparisons: well-being and women's economic empowerment. Plotted are estimated treatment effects on expenditure from evaluations of digital credit products and various antipoverty programs. Dots are point estimates, and black bars correspond to 95% confidence intervals. The "Prog." (program) column indicates the type of program: DL refers to digital loans, CT refers to cash transfers, CL refers to consumer loans, MF refers to microfinance, and MA refers to meta-analysis. For this study, we report the average borrowing from the partner FSP in the past 3 months. Unless specified otherwise, treatment effects are in standard deviations, and positive coefficients indicate positive outcomes. In Ridley et al. (2020), the outcomes considered in their meta-analysis of results include instruments to detect mental illnesses and symptoms of depression, indexes of psychological well-being, and a perceived stress scale. The "Treatment Size" column is the size of the treatment in USD PPP. For this study and Suri, Bharadwaj, and Jack (2021), we report the mean total loan amount borrowed. For results from studies that focus on microfinance, we report the initial treatment loan size, as summarized in table 1 of Banerjee, Karlan, and Zinman (2015). For Haushofer, Mudida, and Shapiro (2020), we report the size of the cash transfer.

antipoverty programs increase well-being by 0.17 standard deviations, and cash transfer programs on average increase mental health by 0.105 standard deviations; Angelucci, Karlan, and Zinman (2015) find that access to microfinance reduces a depression index by 0.045 standard deviations.

Perhaps most striking is the fact that these other programs involve much larger transfers: in Ridley et al. (2020), the average multifaceted antipoverty program cost US$1,707 PPP and the average cash transfer was US$956 PPP; and the average loan value in Angelucci, Karlan, and Zinman (2015) was US$840 PPP. In our experiment, respondents borrowed an additional US$23 (US$63 PPP) when assigned to auto-approval—though some still owed money to the FSP at the time of the survey.[18]

To better understand what might be driving these large effects on subjective well-being, we provide additional context and speculation based on analysis that

[18] Forty-six percent of the credit that applicants received had yet to be repaid at the time of the survey. Part of this arises from default (where the default is higher among borrowers below the threshold—see table 3.

was not prespecified. To begin, we observe that short-run needs are the most common reasons that our sample reports taking a loan (fig. A10). These needs include everyday use (49%), business purposes (42%), medical expenses (37%), paying house/shop rent (37%), and emergencies (20%). While the loans are small, such uses could plausibly reduce the indicators of depression observed in figure 2.[19] This may be especially true in the Nigerian context, where rates of depression and mental disorder are quite high.[20]

Why might the small loans we study improve subjective well-being to an extent comparable to that of larger cash transfer programs? One difference is that these loans are disbursed immediately upon request. Our experiment considers applicants who requested immediate access to small amounts of credit and compares those who randomly received loans with those who did not. In contrast, cash transfer programs often allocate broadly, at times determined by the program.

The comparison to microcredit is more nuanced. Microcredit typically provides larger loans but has an involved application and repayment process so may be less suited to immediate needs. Other evidence suggests that access to microcredit can reduce symptoms of depression; for instance, Fernald et al. (2008) find that increased access to microcredit decreased depressive symptoms from 15% to 2% for men (but had no significant effect for women)—but it was accompanied by increased stress. Field et al. (2012) likewise find that the design of the microcredit loans can contribute to stress: changing the repayment schedule to monthly rather than weekly resulted in borrowers being 51% less likely to report feeling "worried, tense, or anxious" about repaying. Part of the improvements in subjective well-being from digital loans may arise from the security of knowing that one can borrow in the future as needs arise. Borrowers in our sample anticipate future borrowing and may view digital loans and a line of credit similarly.

That we find large welfare effects for small loans—and that we find no effect of providing larger loan offers—suggests that even small relaxations of credit constraints delivered in moments of need may alleviate some mental health burdens, at least in the short run.[21] Overall, these results are consistent with a growing

[19] More speculatively, because we observe that the auto-approval treatment (but not the initial offer treatment) causes people to borrow less from friends and family (fig. A5); just as the auto-approval treatment (but not the initial offer treatment) reduces depression, it may be that self-reliance contributes to the increase in subjective well-being.

[20] According to our end-line surveys, 47% of our sample was screened as having mild depression and 10% as having moderate or severe depression. More broadly, the 2018–19 Nigerian General Household Survey estimates that 20% of heads of households in Nigeria are chronically depressed (Perng et al. 2018). By comparison, only 12.5% of individuals in the United States reported some level of psychological distress (Dhingra et al. 2011).

[21] This is also consistent with a recent meta-analysis that finds no association between the size of a cash transfer and its effect on mental health (Romero et al. 2021).

body of evidence supporting the notion that being unable to access small but critical resources in times of need may be quite damaging for mental health (Haushofer and Fehr 2014; Banerjee et al. 2020) and other measures of well-being (Merfeld and Morduch 2022).

V. Conclusion

The dramatic uptake of digital loans across the developing world suggests strong pent-up demand for consumer credit. However, the structure of the digital loan market—which offers new borrowers short-term loans at high interest rates and results in high rates of default—has led to widespread public concern over the potential consequences of this financial transformation.

Our RCT finds that increasing access to digital loans can improve subjective well-being among applicants, measured roughly 3 months after the date of the initial loan. The magnitude of this effect is similar to that of costly antipoverty interventions, even though the digital loans we study do not consume government or donor resources. This result highlights how even small relaxations of constraints can substantially improve mental health. At the same time, we do not find that offering applicants larger loans has any significant effect. We can also rule out large positive—and negative—effects of digital credit on other short-term dimensions of welfare, including income and expenditures, resilience to shocks, and financial health.

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