Debt Relief and the Reallocation of Household Labor and Capital *

Naman Nishesh†

August 2026

Abstract

Why do households in developing economies remain self-employed in low-productivity activities, such as agriculture, despite substantial expansions in formal credit? In this paper, I find evidence that legacy household debt can itself become a financial friction that impedes reallocation of household resources. I study two large agricultural debt-forgiveness programs in India and exploit quasi-random variation in relief eligibility among otherwise similar farming households. Using administrative bank microdata linked to business-registration records, I find that debt relief does not revive borrowing for the indebted farm. Instead, beneficiary households reduce reliance on distant wage employment sustained through temporary internal migration, redirect borrowing toward locally originated personal and small-business credit, and start more formal local non-farm businesses. Districts more exposed to relief also experience greater business formation and economic activity. The results identify legacy household debt as a financial friction on occupational choice and show that relieving liabilities can enable households to reallocate labor and capital across activities, supporting structural change within the household.

JEL Codes: G51, J24, L26, O12, O16
Keywords: Debt relief; household finance; occupational choice; labor reallocation; rural non-farm entrepreneurship; temporary migration; India

*I thank Aymeric Bellon, Abhinav Gupta, Yunzhi Hu, Camelia Kuhnen, Paige Ouimet, Elena Simintzi, and Prasanna Tantri for helpful discussions. I also thank participants at the UNC Finance PhD Seminar and UNC Finance PhD Alumni Conference for valuable comments. I am especially grateful to Prasanna Tantri, Mohan Ramanath, and the Centre for Analytical Finance at the Indian School of Business, Hyderabad, for helping facilitate access to the bank data and Consumer Pyramids survey data. I gratefully acknowledge financial support from the Koneru R. Rao Global Fellowship and the UNC Dissertation Completion Fellowship. All errors are my own.

†UNC Kenan-Flagler Business School (naman_nishesh@kenan-flagler.unc.edu)

1 Introduction

Productivity gaps between rich and poor economies reflect, in substantial part, the misallocation of labor and capital across activities, with households in developing economies remaining disproportionately self-employed in low-productivity occupations, such as agriculture (La Porta and Shleifer, 2014; Gollin et al., 2014). A leading explanation for this phenomenon has long been that credit frictions bind occupational choice because switching occupations requires upfront investment and greater risk-bearing capacity (Schmalz et al., 2017). Yet while household credit has expanded dramatically in the developing world in recent decades, it has done little to loosen this occupational bind. At the same time, credit expansion has raised growing concerns about household over-indebtedness in developing economies, where personal bankruptcy institutions are often absent or ineffective (Indarte and Kanz, 2024). This raises a complementary possibility: household debt itself may become a financial friction that binds labor and capital to incumbent activities, as repayment obligations reduce households' capacity to bear risk and increase the value of safer and more immediate cash flows (Donaldson et al., 2019). Whether debt relief can reallocate household labor and capital and loosen these occupational constraints in developing economies remains an important, yet unstudied, empirical question.

Agricultural debt relief in India is an ideal setting to study this question. India has roughly 93 million agricultural households, engaging nearly half of India's workforce while contributing only about 15% to its GDP, making labor concentration in low-productivity farming particularly stark. At the same time, farming households have received decades of preferential access to formal credit. Institutional credit to the sector increased from about 10% of agricultural value added in 2000 to over 50% by 2025, supported by policies mandating banks to direct a fifth of their lending to the sector, often at heavily subsidized rates. Agricultural households today have access to 77 million government-supported credit lines carrying an effective interest rate as low as 4%, below the government's own cost of borrowing. Yet despite this generous credit provision, farming households remain exposed to indebtedness and repayment distress.[1] This coexistence of indebtedness and low-productivity farming raises the concern that debt pressures distort household resource allocation. Another indication that these distortions may be costly is the widespread reliance on temporary migration for wage work, with some household members leaving home for informal jobs elsewhere to generate additional cash for debt service and consumption (Mosse et al., 2002; Imbert and Papp, 2020b). Persistent farmer indebtedness has also made debt relief a recurring policy response: Indian central and state governments have announced 24 major farm-loan waiver programs in the last two decades – large, taxpayer-financed debt forgiveness programs that repay households' outstanding agricultural debt.

Whether these debt relief programs help reallocate household labor and capital outside of farming, and in what direction, is ex ante ambiguous. On the labor side, relief could increase labor supply and access to distant labor markets, both by reducing labor needs on the farm

[1] The mounting debt burden facing Indian farmers continues to draw sustained attention from policymakers (Patel, 2017), media (CBC News, 2021; Gupta, 2024), and academics (Merriott, 2016).

and by relaxing liquidity constraints that finance the upfront costs of migration (Bryan et al., 2014). Alternatively, if repayment obligations push households toward costly temporary migration to generate safe and immediate cash flows, debt relief may reduce reliance on migrant wage work (Mosse et al., 2002; Imbert and Papp, 2020b).[2] On the capital side, farm-loan waivers could restore access to institutional agricultural credit and revive investment in the incumbent farm. Yet prior evidence on Indian farm-loan waivers finds little evidence that debt relief increases investment or productivity in agriculture (Kanz, 2016; Giné and Kanz, 2018). Balance-sheet repair may instead relax the risk-bearing and financing constraints that inhibit entry into alternative non-farm productive activities, a reallocation margin unexplored in prior work.

I study this reallocation using farm loan waivers announced in July 2014 by Andhra Pradesh (AP) and Telangana (TG), two agrarian states in southern India. Utilizing administrative microdata from a large public bank and exploiting quasi-random variation in eligibility for debt relief, I find that relief significantly reallocates household labor and capital *outside* farming. Eligible households rely less on distant wage employment sustained through temporary migration. At the same time, they reduce subsequent farm borrowing, take up more personal and small-business credit, and start more local non-farm enterprises. Aggregate district-level evidence is consistent with these responses scaling up: areas more exposed to relief exhibit greater non-farm business formation and higher nighttime-light activity. Taken together, the results suggest that legacy debt can act as a financial friction on households' labor and capital allocation, and debt relief can alleviate this friction and support structural transformation within the household.

The debt relief programs were introduced by newly formed state governments after the bifurcation of the erstwhile state of Andhra Pradesh in June 2014. The announcement came in the backdrop of high farm indebtedness: more than 90% of agricultural households had outstanding debt, the highest share among Indian states (National Sample Survey Office, 2013), and the average household's debt-to-income ratio was about 1.25. The waivers were an attempt to both reduce debt burdens and enable fresh credit for farming households that constituted 55% of the state's total workforce. The total budget was about 4.5% of state GDP, ranking, on this margin, among the largest ever household debt-forgiveness packages in the world. Relief was economically meaningful for farming households as relief caps, ₹150,000 in AP and ₹100,000 in TG, exceeded households' average annual farm income (roughly ₹80,000) in the region.[3]

Estimating the causal effect of debt relief is challenging because relief is typically targeted to distressed households, whose subsequent outcomes would differ even in the absence of relief. My setting provides a rare break in this link. Eligibility for the 2014 farm loan waivers was based on two predetermined loan characteristics: loan type and sanction date. Relief applied to crop-related loans sanctioned before state-specific cutoff dates: December 31, 2013 in AP and March 31, 2014 in TG. Because the final rules were announced only in July 2014, these loan characteristics were

[2] Recent household-finance evidence from developed economies shows that household debt and debt relief can affect labor supply, job search, and geographic mobility (Brown and Matsa, 2020; Bernstein, 2021; Di Maggio et al., 2026; Dinerstein et al., 2025).

[3] For reference, \$1 ≈ ₹60 around that time.

fixed before borrowers could respond, making manipulation implausible. Further, participation was non-optional: banks were required to submit lists of all eligible borrowers directly from their own records, attenuating concerns of self-selection.

In my empirical tests, I compare households with loans eligible for debt relief (treated) to otherwise similar farming households (control group) whose loans, originating within the same bank branch, were ineligible either because they were sanctioned just after the cutoff date or because they belonged to excluded agricultural loan categories.[4] These households therefore faced the same local economic conditions and aggregate consequences of the policy and differed only in the debt relief generated by the arbitrary eligibility rules. My baseline estimates use a difference-in-differences design two and a half years before and after the waivers, complemented by a regression-discontinuity design around the sanction-date cutoffs.

My primary data comes from the administrative records of a large public-sector bank in the region. The data span 2009 to 2016 and cover loan accounts, borrower addresses, repayment outcomes, and new credit, allowing me to measure households' pre-waiver farm debt, eligibility status, and post-waiver changes in borrowing up to 2.5 years after the waiver. The data also record in-person cash deposits and withdrawals at bank branches. Because these transactions require physical presence, the branch location allows me to measure the location of household financial activity. I then combine the bank data with government business-registration records, using borrower addresses matched to registered business addresses, to examine household entry into new non-farm entrepreneurship.

The bank data also allow me to observe the financial footprint of migration-linked employment. Rural farming households often send selected members to cities for temporary wage work while others remain at home to operate the farm; these migrants are commonly employed in cash-intensive informal sectors and periodically remit earnings back to the household (Morten, 2019; Imbert and Papp, 2020a). During the period I study, much of this employment was cash-based, so workers commonly deposited earnings in person at bank branches near the workplace while family members accessed the funds at home.[5] I therefore use cash deposits made outside the household's home district as a proxy for distant wage employment (and outside-home branch or outside home-state as robustness), and provide extensive validation of this measure in Appendix Section B.1.

I begin my empirical tests by confirming that the eligibility rules generated meaningful relief: eligible borrowers experience significantly lower loan balances and default rates on existing farm debt within one year of the waiver. I next examine whether relief influences households' reliance on distant wage employment. The difference-in-differences estimates show that waiver eligibility reduces migration-linked outside-district transactions by 14% relative to the pre-waiver mean. A

[4] In the control group, I retain only households who had taken at least one crop loan at some point in the five years before the waiver. This ensures that control households had access to, and prior use of, the loan-product category targeted by the waiver.

[5] Survey evidence confirms that bank deposits were among the primary channels through which domestic migrants remitted earnings home in this period (Oliver and Radcliffe, 2011). Low-income rural households in India also rarely held multiple bank accounts, making it unlikely that migrant members maintained separate accounts at destination branches (Agarwal et al., 2017a).

dynamic event-study plot shows no evidence of differential pre-trends, with a significant decline for eligible borrowers precisely after the waiver. Further tests show that the decline occurs entirely on the extensive margin and coincides with greater financial activity at home. The effects are also concentrated among households with deposits originating from metropolitan areas and industrial hubs, destinations associated most with migrant employment. Finally, complementary survey evidence is consistent with household members returning from migrant work elsewhere. These patterns accord with evidence that temporary migration entails substantial nonpecuniary costs, including precarious work and difficult living conditions, and declines when rural support becomes available (Imbert and Papp, 2020a,b; Lagakos et al., 2023). Collectively, the evidence indicates that debt relief reduces households' reliance on distant wage employment and is consistent with labor reallocating toward the local economy.

Turning to household capital allocation, I find that relief-eligible households are about 7% less likely to take a new farm loan after the waiver, consistent with prior evidence that farm-loan waivers fail to raise agricultural investment or productivity and reduce subsequent farm credit (Kanz, 2016; Giné and Kanz, 2018). By itself, this decline could reflect either lower demand for farm credit or reduced lender willingness to extend new agricultural loans. The broader borrowing response, however, points toward household-level reallocation rather than a general contraction in credit supply. Eligible households increase their take-up of personal loans by roughly 34%, despite agricultural credit being actively supported through government policies and substantial interest subsidies, while personal credit is typically less subsidized and often unsecured. A broad deterioration in formal credit access is therefore difficult to reconcile with borrowing falling in the policy-supported farm category while expanding in less protected credit markets.

The composition of this additional personal credit reveals where the reallocation is going. Among the major personal-loan categories, neither housing nor auto credit increases; instead, the response is concentrated in self-employment and small-business loans that can provide external finance for early-stage household enterprises (Robb and Robinson, 2014). These loans finance local establishments and proprietorships such as food-processing units, tailoring and garment businesses, and transport services. Although rare among farming households at baseline, such borrowing rises by about 40% relative to the pre-waiver mean and originates almost entirely from home-district branches. The absence of differential pre-trends further suggests that this shift emerges only after debt relief rather than reflecting pre-existing borrowing trajectories. Thus, alongside the decline in distant wage employment, household finance shifts toward local non-farm activity.

Two cross-sectional patterns sharpen the interpretation of this credit reallocation. First, the increase in small-business credit is concentrated among households with greater pre-waiver exposure to industrial hubs, as measured by the origin of their deposits. Because these destinations are closely associated with migrant wage employment, this pattern links the credit response to households for whom migration-related cash flows were more salient before relief. Second, the increase in small-business credit is stronger among households for whom the waiver extinguished a larger share of outstanding debt. This gradient is consistent with a balance-sheet channel, whereby greater relief

relaxes households' capacity to bear entrepreneurial risk and finance entry into alternative activities. Together, these patterns suggest that reallocation is strongest precisely where debt overhang is more likely to bind: among households more reliant on migration-linked cash flows and those receiving economically larger balance-sheet relief.[6]

As a more direct test of household entry into non-farm production, I link bank borrowers to formal company and Micro, Small, and Medium Enterprise (MSME) registries using borrower and business addresses. Because firms may remain informal, register elsewhere, or be recorded under another household member, the resulting measure captures only a subset of entrepreneurial entry. Even so, waiver eligibility significantly increases the likelihood of registering a new non-farm business, with the estimated increase amounting to more than twice the pre-waiver rate of business formation. Together with increased personal and small-business credit, this evidence indicates that debt relief supports the redeployment of household capital into new local production rather than the expansion of the indebted farm enterprise.

Several additional tests support the interpretation and robustness of these results. New self-employment and small-business loans to eligible households perform at least as well as comparable loans to ineligible households, and the business-entry result is unchanged when I restrict attention to firms that remain active for at least five years, weighing against transitory or low-quality entrepreneurial entry. A regression-discontinuity design around the sanction-date cutoffs reproduces the central migration and small-business-credit effects within the same loan category. Finally, I turn to the Consumer Pyramids Household Survey (CPHS), which directly records household-member migration and occupation and therefore complements the bank-based proxies. Across twelve state-level farm-loan waivers, following debt relief, farming households exhibit fewer member absences, more returns of previously absent members, and greater entry into non-farm self-employment. These results provide independent validation of the migration and entrepreneurship responses and extend the external validity of the findings beyond the two programs and bank sample studied in the main analysis.

Finally, at a more aggregate level, districts more exposed to debt relief experience greater formal business formation and higher nighttime-light activity, a widely used measure of local economic activity (Henderson et al., 2012). These estimates indicate that the household-level reallocation documented in the microdata is reflected in broader local economic activity. Taken together, the findings point to a liability-side financial friction on household reallocation. The development-finance literature has traditionally emphasized limited access to capital as a constraint on entry into new activities. Related finance research shows that the allocation of bank credit can shape the reallocation of labor and capital toward more productive firms (Bai et al., 2018). My results identify a complementary household-balance-sheet margin: especially when credit to the

[6]Households that cease outside-district transactions after the waiver are also more likely to take small-business credit, although this comparison is endogenous. The response does not vary with pre-waiver deposit volume, suggesting that a simple wealth effect may not be first-order. Finally, there is no differential effect by pre-waiver default status, although this is difficult to interpret because performing borrowers may have remained current by rolling over existing obligations, a concern explicitly noted in the debt-relief notification.

incumbent activity is already abundant, accumulated debt can itself inhibit households' ability to redeploy labor and capital. This friction matters for structural transformation at the household level. Recent work emphasizes that, in developing economies where agriculture continues to absorb a large share of labor, structural change increasingly proceeds through the diversification of rural economies into productive non-farm activities rather than solely through rural-to-urban migration (Foster and Rosenzweig, 2004; Haggblade et al., 2010). My findings identify household debt overhang as a constraint on this process and show that debt relief can facilitate rural diversification by enabling farming households to reallocate labor and capital toward local non-farm production.

These results do not, however, imply that farm-loan waivers are necessarily desirable policy. Their welfare effects depend on fiscal costs, distortions to repayment incentives and credit supply, and labor-supply effects along migrant-destination corridors, weighed against the returns to new enterprise and the avoided costs of distant employment. A full welfare analysis is beyond the scope of the paper. A simple accounting exercise suggests that the reallocation channel offsets roughly 28% of the fiscal cost after accounting for reduced farm credit, indicating that labor and capital reallocation is quantitatively meaningful and represents an important margin omitted from conventional assessments of farm-loan waivers. The distortions associated with repeated discretionary waivers also point to the potential value of rules-based debt resolution (Indarte and Kanz, 2024). Such protection itself affects household borrowing and credit pricing (Severino et al., 2026), so even its design involves important trade-offs. My findings suggest, however, that the ability to resolve legacy household debt can affect the allocation of labor and capital across productive activities and, subsequently, structural transformation in developing economies.

2 Related Literature

This paper contributes to four related literatures. I situate my paper's contribution within each of these in this section.

Financial Frictions and Occupational Choice: Models of occupational choice under financial constraints show that wealth, access to finance, and risk-bearing capacity shape households' entry into self-employment and entrepreneurship (Evans and Jovanovic, 1989; Banerjee and Newman, 1993; Stiglitz and Weiss, 1981; Cagetti and De Nardi, 2006). A large empirical literature identifies these frictions through shocks to liquidity, personal wealth, collateral, and credit access (Holtz-Eakin et al., 1994; Hurst and Lusardi, 2004; de Mel et al., 2008; Andersen and Nielsen, 2012; Corradin and Popov, 2015; Schmalz et al., 2017; Bellon et al., 2021). Recent evidence further shows that the structure of credit contracts and implicit insurance through personal bankruptcy can influence entrepreneurial risk-taking and entry (Field et al., 2013; Battaglia et al., 2024; Krishnan and Wang, 2019).

I study a distinct liability-side friction, the removal of debt incurred in an incumbent productive activity, and its effect on occupational choice. I show that debt relief reorganizes household labor and finance across activities: external wage employment, incumbent production, and new

entrepreneurial ventures.

Household Debt Relief and Real Activity: A growing household-finance literature shows that debt burdens affect labor supply, job search, geographic mobility, human-capital investment, and occupational choice (Donaldson et al., 2019; Brown and Matsa, 2020; Bernstein, 2021; Bernstein and Struyven, 2022; He and Le Maire, 2023; Zator, 2025; Chakrabarti et al., 2023; Hampole, 2022). Debt resolution, in turn, affects earnings, employment, mobility, consumption, and subsequent borrowing (Dobbie and Song, 2015; Agarwal et al., 2017b; Dobbie and Song, 2020; Ganong and Noel, 2020; Di Maggio et al., 2026; Dinerstein et al., 2025). Related work links student debt in particular to entrepreneurial entry and human capital investment (Ambrose et al., 2015; Krishnan and Wang, 2019; Morazzoni, 2024). A parallel corporate-finance literature studies how debt overhang, financial distress, and restructuring affect investment, firm growth, and employment (Myers, 1977; Lang et al., 1996; Hennessy, 2004; Gertner and Scharfstein, 1991; Ponticelli and Alencar, 2016; Kalemli-Özcan et al., 2022).

These two literatures treat the indebted household and the productive firm as separate decision-makers. That separation fails for household-owned enterprises, where the same household operates production and allocates labor between own-enterprise and outside employment. Such enterprises, including self-employed and family-operated small businesses, account for roughly half of global employment and face substantial credit and risk-bearing constraints. I show that in such settings, when production and labor decisions are jointly made within the household, debt relief can generate real adjustment through reallocation across outside employment and new production rather than expansion of the incumbent enterprise.

Existing evidence comes almost entirely from developed economies. In this paper, I shift attention to debt resolution in developing economies. Theory shows that debt relief can improve investment and labor allocation when inefficient markets prevent private debt contracts from fully accommodating adverse states (Bolton and Rosenthal, 2002). Indarte and Kanz (2024) emphasize that this problem is especially salient in developing economies, where formal household borrowing has expanded without comparable development of rules-based debt-resolution institutions. I provide causal evidence that relieving household debt can affect real activity through shifts in household occupational choice, particularly through increased non-farm entrepreneurship.

Rural Diversification, Migration, and Structural Change: The movement of labor and capital from agriculture toward rural non-farm activity is an important margin of structural change in developing economies. Existing work studies how agricultural productivity, local demand, infrastructure, and access to finance shape the growth of rural manufacturing, services, and household enterprise (Foster and Rosenzweig, 2004, 2008; Haggblade et al., 2010; Lanjouw and Murgai, 2009; Burgess and Pande, 2005; Bustos et al., 2016, 2020; Garg et al., 2025). This margin has become particularly important as the classical transition from agriculture into urban manufacturing has weakened in many developing economies (Rodrik, 2016). In this paper, I propose a financial-frictions margin of this rural transformation. The results suggest that diversification

may be constrained not just by limited access to credit, but also by legacy farm debt that inhibit households' ability to bear risk and deploy capital outside the incumbent farm.

The paper also relates to research on temporary and partial migration in developing countries. Rural households commonly combine own production with wage employment elsewhere, using migration to obtain liquidity and insure against volatile local income (Stark and Bloom, 1985; Lucas and Stark, 1985; Rosenzweig and Stark, 1989; Mosse et al., 2002; Deshingkar and Start, 2003; Morten, 2019). Migration decisions are shaped by liquidity constraints, informal insurance, and the availability of local employment (Bryan et al., 2014; Angelucci, 2015; Cai, 2020; Munshi and Rosenzweig, 2016). At the same time, temporary migration entails substantial pecuniary and nonpecuniary costs, and rural support programs can reduce households' reliance on it (Imbert and Papp, 2015, 2020a,b; Lagakos et al., 2023). I identify household debt as a distinct determinant of this allocation: repayment obligations can sustain reliance on distant wage earnings, while debt relief reduces that reliance. The paper thereby connects household balance sheets to both rural non-farm diversification and debates over distress-driven migration.

Farm-Loan Waivers in India: Recurring farm-loan waivers in India have raised concerns about fiscal costs, strategic default, repayment discipline, and future formal credit supply (Narayanan and Mehrotra, 2019; Mahambare et al., 2022). Existing evaluations focus primarily on outcomes within agriculture. Studying the 2008 national waiver, Kanz (2016) finds no increase in farm investment or productivity, while Giné and Kanz (2018) documents weaker repayment incentives and a contraction in subsequent agricultural credit. These concerns are particularly salient in the 2008 program because eligibility itself required loans to be delinquent, directly linking relief to prior repayment behavior; related work shows that subsequent repayment effects vary importantly with borrower distress (Mukherjee et al., 2018).

I study a complementary adjustment margin outside agriculture: whether relieving farm debt changes households' allocation of labor and capital across activities. My setting also separates debt relief from prior delinquency. Eligibility depended on predetermined loan type and sanction date irrespective of loan performance, and most eligible borrowers were performing before the waiver. This allows me to compare otherwise similar borrowers within the same branches, and, in the RD design, within the same loan product around arbitrary sanction-date cutoffs, while absorbing local credit-supply responses common to treated and control households. I show that farm debt relief reduces reliance on distant wage employment and redirects households toward local non-farm production, a margin unexplored in this literature.

3 Institutional Details

I study agricultural debt relief in Andhra Pradesh and Telangana, two southern Indian states that formed undivided Andhra Pradesh until its bifurcation in June 2014. Figure 1 locates the two states within India. The setting brings together the three features central to the paper's question: household labor remained concentrated in low-productivity farming, agricultural borrowing was

heavily supported yet household debt burdens were exceptionally high, and farming households routinely combined own production with distant wage employment and limited local non-farm activity. These features make it possible to study whether legacy debt tied to the incumbent farm constrains the allocation of household labor and capital even in a setting with extensive access to subsidized agricultural credit. I first describe household production and labor allocation in the region, and then the design and implementation of the 2014 farm-loan waivers.

3.1 Household Production, Labor Allocation, and Debt

The economy of undivided Andhra Pradesh was heavily dependent on farming. Nearly 70% of the population lived in rural areas in the 2011 Census, and more than 60% of the working-age population was engaged in farming. Yet agriculture accounted for only 20% of state GDP in 2013 (Central Statistics Office, 2013). This employment-output gap reflects a central feature of Indian agriculture and, more broadly, of low-productivity self-employment in developing economies: farming absorbs a large share of household labor while generating relatively low income and output per worker (Gollin et al., 2014; La Porta and Shleifer, 2014).

For many households, farming alone provided an insufficient and risky livelihood. Farms were generally small and fragmented, depended heavily on rainfall, and operated with limited insurance against production and price risk (National Sample Survey Office, 2013; Deshingkar and Akter, 2009). In 2013, the average cultivated area per agricultural household was less than one hectare (National Sample Survey Office, 2013). Farm output was also volatile: agricultural growth averaged about 3.6% between 2007 and 2013, compared with 8.6% in industry, and foodgrain production fell by 14.8% in 2011–12 following unfavorable seasonal conditions (Central Statistics Office, 2013; Directorate of Economics and Statistics, 2012). Because farms are predominantly household enterprises, these low and volatile returns shape not only production decisions but also how households allocate labor and finance across farming and alternative activities.

One important adjustment margin was temporary or partial migration for wage work. One or more household members could work in towns or cities while others remained in the village to cultivate the farm and manage household affairs. Approximately 19% of rural households reported migration, and 58% of urban migrants originated in rural areas (National Sample Survey Office, 2010). Earnings sent home supported consumption, farm production, and debt repayment (Deshingkar and Akter, 2009; Keshri and Bhagat, 2012; Mosse et al., 2002). By generating relatively reliable cash income when farm returns were seasonal and risky, distant wage work could help households meet current repayment and consumption needs. It was also costly. Migrants were often employed in informal construction, manufacturing, transport, and service work characterized by precarious housing, long hours, and limited social protection (Breman, 1996; Imbert and Papp, 2020a). Work away from home imposed additional nonpecuniary costs from household separation and difficult urban living conditions. Consistent with these features, Lagakos et al. (2023) show that nonpecuniary migration costs substantially limit the welfare gains from rural–urban migration, while Imbert and Papp (2020a) show that improvements in rural employment opportunities reduce migration. Temporary migration was therefore both an important source of household cash flow

and a potentially costly coping strategy.

Local non-farm production offered another use of household labor and capital. Rural non-farm activity consisted largely of household-operated enterprises in retail, food processing, repair, transport, and other small-scale services. The growth of this sector is an increasingly important margin of structural change in developing economies where the classical transition from agriculture into urban manufacturing has weakened (Haggblade et al., 2010; Foster and Rosenzweig, 2008; Rodrik, 2016). Its prevalence in Andhra Pradesh at the time of the waiver, however, was limited: in 2013, non-farm business generated less than 6% of average agricultural-household income in the state (National Sample Survey Office, 2013). Prior work identifies financial constraints, thin local demand, and exposure to income risk as important barriers to rural non-farm activity (Lanjouw and Murgai, 2009; McKenzie and Woodruff, 2008; Darko et al., 2025). Evidence from India further shows that rural bank expansion increased non-agricultural output and reduced poverty, and that proximity to bank branches is associated with greater non-agricultural entrepreneurship (Burgess and Pande, 2005; Garg et al., 2025). Entry into local enterprise therefore required households to deploy both labor and financing away from the incumbent farm and toward an activity with more uncertain and delayed returns.

These allocation choices were made against a background of exceptionally high indebtedness. In 2013, more than 90% of agricultural households in undivided Andhra Pradesh reported outstanding debt, the highest share among Indian states (National Sample Survey Office, 2013), and average outstanding debt exceeded average annual farm income. Importantly, this indebtedness coexisted with extensive access to subsidized formal agricultural credit: farm loans counted toward banks' priority-sector lending requirements and generally carried subsidized interest rates.[7] Formal credit did not eliminate debt stress. Nationally, non-institutional lenders still accounted for 44% of agricultural-household debt in 2013 (National Sample Survey Office, 2013), and households borrowed not only to finance cultivation but also to smooth consumption, meet health and social expenditures, and service earlier obligations (Ramachandran et al., 2010; National Sample Survey Office, 2013). Repayment obligations therefore sat on the same household balance sheet that financed consumption and alternative productive activity, potentially increasing the value of immediate wage cash flows while reducing households' capacity to bear risk and finance entry elsewhere.

Survey evidence underscores the resulting tension in household allocation. More than 40% of farmers nationally reported that they would leave farming if a viable alternative were available, with dissatisfaction particularly high among small and marginal farmers (Birthal et al., 2015). Yet exit was difficult: local opportunities were limited, while farming provided subsistence production and access to policy-supported inputs and credit. Households consequently combined a low-return but supported farm enterprise with distant wage employment, while entry into local non-farm production remained limited. The region therefore provides a natural setting to ask whether

[7] Under priority-sector lending rules, banks were required to direct 18% of net credit to agriculture, including a sub-target for small and marginal farmers. Typical crop loans carried an interest rate of 7%, falling as low as 4% with prompt repayment, below the risk-free rate in 2014.

relieving legacy farm debt changes how households deploy labor and capital across these activities. It is against this background that Andhra Pradesh and Telangana announced large farm-loan waivers in 2014.

3.2 The 2014 Andhra Pradesh and Telangana Farm Loan Waivers

Both governments explicitly framed the waivers as a response to persistent agricultural indebtedness. The Telangana government order, for example, described farmers as trapped in a "vicious cycle" of debt and argued that a one-time waiver was needed to relieve outstanding obligations and enable fresh investment. The policy was first discussed during the 2014 state election campaign in April 2014, but without details on the precise eligibility rules or beneficiary accounts. Final operational guidelines were issued in July 2014. The rules made eligibility depend on loan characteristics that had already been determined months earlier: the type of loan and the date on which it was disbursed.

In Andhra Pradesh, eligible loans included crop loans and agricultural gold loans for crop purposes sanctioned up to December 31, 2013. In Telangana, eligible loans included short-term production loans and crop loans against gold sanctioned up to March 31, 2014. The schemes covered institutional loans from scheduled commercial banks, regional rural banks, cooperative banks, and related agricultural credit institutions; informal loans were not covered. The waivers also excluded several non-crop agricultural and allied activities, including loans for horticulture working capital, produce pledge loans, warehouse receipt loans, and other indirect agricultural finance. These predetermined administrative cutoffs are central to the research design: eligibility was assigned using loan dates and loan types fixed before borrowers knew the final rules, sharply limiting scope for manipulation around the waiver thresholds.

The debt relief was economically consequential. Andhra Pradesh waived eligible loans up to ₹150,000 per household, while Telangana waived eligible loans up to ₹100,000 per household. These ceilings exceeded average annual farm income across the two states, and the fiscal cost amounted to roughly 4.5% of state GDP. Eligibility was determined from bank administrative records rather than self-reported claims: banks submitted branch-level borrower information using government-provided templates, and district administrations monitored the process. Although governments reimbursed banks in staggered installments, eligible borrowers were to regain access to fresh institutional finance as the schemes were implemented. The policy therefore provides a large balance-sheet intervention whose stated objective included reviving the incumbent farm. Notably, and unlike the 2008 national waiver, relief was not conditioned on prior default. Most eligible accounts in my bank sample were performing when the programs were announced, separating debt relief from the mechanical selection of non-performing borrowers and allowing the analysis to focus on real household adjustment after legacy liabilities are removed.

4 Data

4.1 Bank Administrative Data

The primary data come from rich administrative records of a large public-sector bank from 2009 to 2016. The bank had an extensive branch network in South India and was one of the two largest banks in Andhra Pradesh and Telangana during the sample period.

The data contain three main components. First, customer-level records provide borrower names, addresses, and links to all associated loan and savings accounts. Second, loan-account records contain detailed information on each borrowing relationship, including loan type, sanctioned amount, sanction date, interest rate, outstanding balance, and loan performance over time. These records allow me to identify agricultural borrowers, classify loan products eligible for debt relief, measure pre-waiver debt exposure, and track subsequent borrowing and default outcomes. Third, savings-account records contain account type and in-person cash deposits and withdrawals made at bank branches. Because these cash transactions require physical presence, I link each transaction to the location of the transaction branch and use this information to measure where household financial activity occurs.

4.2 Business-Registration Data

I complement the bank data with two sources of official business-registration records. The first source covers incorporated firms registered with the Ministry of Corporate Affairs. I obtain these records from InstaFinancials, which aggregates corporate filings and provides consolidated information on each firm's name, date of incorporation, industry, registered address, and directors. The second source is the government registry of micro, small, and medium enterprises (MSMEs) until 2020 under the Udyog-Aadhaar memorandum. These records contain establishment names, registration dates, industries, registered addresses, and whether the establishment is classified as micro, small, or medium.

I use these data in two ways. First, I link business registrations to individual bank customers using names and addresses. Specifically, I match customer addresses in the bank data to registered business addresses, and use director names or firm names matched to customer names as additional signals when available. Businesses may register at a different address, be recorded under another household member, or remain informal and therefore never appear in government records. The individual-level match should thus be interpreted as a conservative measure of entrepreneurial entry. Second, I aggregate business registrations to the district level and merge them with district-level measures of waiver exposure constructed from the bank data. Because addresses are noisy and district boundaries changed over time, all matching is done within harmonized districts.

4.3 Sample Construction and Summary Statistics

I construct the analysis sample from borrowers in Andhra Pradesh and Telangana who had outstanding agricultural credit at any point in the year before the waiver, from July 2013 to June 2014. I begin with all borrowers with agricultural loan exposure in this window and then restrict

the sample to borrowers for whom I can observe the relevant loan sanction date. This restriction is important because eligibility depends on whether the loan was sanctioned before the state-specific cutoff. The bank data contain both term loans and cash-credit products, including Kisan Credit Cards. For cash-credit accounts, however, the data record the date on which the credit line was sanctioned, not the date on which the borrower drew down credit or the date on which a specific crop loan became due. I therefore exclude these accounts from the baseline sample and focus on loan accounts for which sanction dates are well-defined.

I classify treated borrowers using the eligibility rules described in Section 3.2. Treated borrowers are those with eligible agricultural loans sanctioned before the relevant state cutoff: crop loans and agricultural gold loans for crop purposes disbursed up to December 31, 2013 in Andhra Pradesh, and eligible short-term production loans and crop loans against gold disbursed up to March 31, 2014 in Telangana. Control borrowers are agricultural borrowers with outstanding farm credit in the pre-waiver year whose loans were not eligible, either because they were sanctioned after the cutoff or because they belonged to excluded agricultural categories, such as horticulture working-capital loans or other non-crop agricultural credit. To make the control group more comparable, I retain only borrowers who had taken at least one short-term crop loan at some point between 2009 and June 2014. This restriction ensures that control borrowers had access to, and prior use of, the loan-product category targeted by the waiver.

The bank records identify individual customers rather than households. I therefore use the borrower as the empirical unit and interpret each sampled borrower as representing a household. I do not attempt to construct households by aggregating borrowers with the same address because rural addresses frequently identify only a village, locality, or street rather than an exact dwelling. Address-based aggregation would therefore risk combining unrelated borrowers into the same household. Treating each borrower as a separate household is also reasonable in this setting because low-income rural households rarely maintained multiple bank accounts before the 2014 expansion of financial inclusion under the Pradhan Mantri Jan Dhan Yojana (Agarwal et al., 2017a). Nevertheless, different members of the same household may occasionally appear as separate borrowers in the data. In such cases, an apparently ineligible borrower could belong to a household that received relief through another member. This within-household treatment contamination would generally reduce observed differences between eligible and ineligible borrowers, attenuating the estimated effects.

The resulting borrower-level sample contains about 90,000 borrowers. I organize the data as a borrower-year panel around the waiver. The baseline panel spans three years before and after the waiver, with year 0 defined as July 2013 to June 2014, the year immediately preceding implementation.[8] This structure allows me to compare eligible and ineligible borrowers before and after the waiver while holding fixed borrower characteristics and controlling for pre-policy trends.

Table 1 presents summary statistics at the borrower level. The average borrower had ₹158,000

[8]Because the bank data end in 2016, the final post-waiver year is only partially observed. Similarly, the earliest pre-waiver year is partially observed when using symmetric July–June event years.

in outstanding loans at the time of the waiver, with a median of ₹78,000. Agricultural borrowing was the dominant exposure in the sample: the mean share of agricultural loans was 0.87, and the median borrower had all loan exposure in agriculture. About 52% of borrowers were eligible for the waiver. Average debt relief in the full sample was ₹45,970, with a median of ₹20,372; among eligible borrowers, mean relief was ₹87,696 and median relief was ₹92,080.

The transaction variables show that the bank data contain substantial information on in-person account activity. Eighty-nine percent of borrowers had at least one cash transaction at a bank branch, with an average of more than nine cash transactions and ₹72,260 in cash deposits over the pre-period. About 23% of borrowers conducted at least one cash transaction outside their home district, although the average share of transactions conducted outside the home district was modest. This pattern is consistent with a sample of predominantly rural farm borrowers while leaving meaningful variation in migration-linked banking activity.

Finally, only about 1.3% of borrowers take a self-employment or small-business loan. This is a conservative measure of household engagement in non-farm enterprise: I observe loans taken by the sampled borrower but may miss business borrowing undertaken by another household member or through a separate business entity. Households may also use personal credit to finance early-stage businesses (Robb and Robinson, 2014). The observed share should therefore be interpreted as a lower bound on business-related borrowing in the bank data, rather than as a measure of total household entrepreneurship.

4.4 Migration-Linked Activity in the Banking Data

The bank data provide a proxy for migration-linked household activity because they record the location of in-person cash deposits and withdrawals. I focus on cash deposits made at branches outside the borrower's home district. During the period I study, much informal-sector wage employment was cash-based, and migrant workers commonly remitted earnings home through bank deposits (Oliver and Radcliffe, 2011). Because these transactions required physical presence at a branch, a cash deposit away from the home district provides a financial footprint of activity at the destination. Outside-district deposits are also much more common than outside-district withdrawals, consistent with remittance-like deposits rather than routine banking away from home.

Internet Appendix Section B.1 provides detailed evidence supporting this interpretation. First, outside-district deposits are concentrated among a meaningful minority of borrowers and, when present, are typically concentrated in a single outside district, consistent with a stable connection between the household's home district and a primary migrant destination. Second, outside-district deposit spells are short rather than year-round, and their timing recurs across years more frequently than would be expected under random timing. Third, home-district withdrawals rise sharply when outside-district deposit episodes begin, consistent with earnings deposited near a workplace being accessed by household members remaining at home. Low-income rural households also rarely held multiple bank accounts during this period, making it less likely that the pattern reflects separate destination accounts maintained by migrant members (Agarwal et al., 2017a).

Taken together, these patterns are consistent with outside-district deposits capturing

remittances associated with temporary, circular, or seasonal migration rather than incidental travel or permanent household relocation. They also fit the partial-migration structure described in Section 3.1: one member may work away while others remain in the village to operate the farm, consume the remitted income, and maintain local borrowing relationships. I therefore use outside-district cash deposits as a proxy for migration-linked wage employment in the main analysis. The empirical tests below ask whether relieving legacy farm debt changes households' reliance on this source of relatively safe outside cash flow.

5 Results

I organize the analysis around three questions that mirror the economic ambiguity in the introduction. First, did the administrative eligibility rules generate meaningful household debt relief? Second, did relief expand households' participation in distant labor markets or reduce reliance on migration-linked wage work? Third, did restored balance-sheet capacity flow back into the incumbent farm or toward alternative non-farm production? I then examine formal business entry, the quality of new business credit, alternative research designs and data sources, and broader local outcomes.

5.1 Empirical Specification and First-Stage Evidence

The baseline analysis compares the evolution of outcomes for waiver-eligible and ineligible borrowers before and after the 2014 waivers. For borrower i in waiver stack s and event year t, I estimate:

$$y_{i,s,t} = \beta \, \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(Post_t) + \alpha_{i,s} + \Gamma_{s \times b(i) \times \ell(i) \times a(i) \times r(i) \times t} + \varepsilon_{i,s,t}. \tag{1}$$

$\mathbb{1}(Treat_{i,s})$ identifies borrowers with loans eligible for relief, and $\mathbb{1}(Post_t)$ identifies event years after the waiver. The borrower fixed effects $\alpha_{i,s}$ absorb all time-invariant differences between eligible and ineligible borrowers. The most saturated specification includes event-time fixed effects interacted with the branch that issued the pre-waiver farm loan, the loan product, and quintile bins of the loan's amount and interest rate. The coefficient β therefore compares changes in outcomes among borrowers who obtained similar pre-waiver loans from the same branch but differed in waiver eligibility. These fixed effects absorb common changes in local economic conditions, branch-level lending, loan-product supply, and trends associated with the size and price of the original loan. Standard errors are clustered at the borrower level.

For the dynamic specifications, I replace the post-waiver interaction in Equation (1) with interactions between eligibility and indicators for event time:

$$y_{i,s,t} = \sum_{\tau \neq 0} \beta_\tau \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(t = \tau) + \alpha_{i,s} + \Gamma_{s \times b(i) \times \ell(i) \times a(i) \times r(i) \times t} + \varepsilon_{i,s,t}, \tag{2}$$

where event year 0, the year immediately preceding implementation, is omitted. The pre-waiver coefficients provide a direct assessment of differential trends, while the post-waiver coefficients trace the timing of adjustment.

Table 1 provides context for the analysis. The average borrower had approximately ₹158,000 in outstanding debt before the waiver, 87% of which was agricultural. Fifty-two percent of borrowers were eligible for relief, and mean relief among all sampled borrowers was approximately ₹46,000. Outside-district banking activity and business borrowing were concentrated among a minority of households: 23% conducted at least one cash transaction outside the home district, while only about 1% held self-employment or small-business credit.

Table 2 verifies that administrative eligibility generated economically meaningful balance-sheet relief. Within one year of the waiver, eligible borrowers experience an approximately 11% reduction in outstanding farm debt in the Poisson specification and a 7.9 percentage-point decline in the probability of default on existing farm debt. The estimates include borrower fixed effects and the full set of branch-by-loan-characteristic-by-time fixed effects. The eligibility rules therefore produced a large and immediate change in legacy household liabilities, establishing the balance-sheet shock underlying the reallocation tests that follow.

5.2 Debt Relief and Distant Wage Employment

I first examine whether debt relief changes households' reliance on distant wage employment. The sign is ex ante ambiguous. Relief could increase labor supply and access to distant labor markets by reducing labor needs on the farm and relaxing liquidity constraints that finance the upfront costs of migration (Bryan et al., 2014). Alternatively, repayment obligations may raise the value of relatively safe and immediate wage income, sustaining costly temporary migration used to finance consumption, farm production, and debt service (Mosse et al., 2002; Donaldson et al., 2019; Zator, 2025). Under this channel, balance-sheet repair should reduce households' reliance on migrant wage work. This possibility is consistent with evidence that temporary migration carries substantial nonpecuniary costs and declines when rural support becomes available (Imbert and Papp, 2020a,b; Lagakos et al., 2023).

5.2.1 Validating Migration-Linked Deposits

The outcome is an indicator for whether a borrower's savings account receives an in-person cash deposit at a branch outside the home district in a given year. Appendix Section B.1 provides several pieces of evidence supporting its interpretation as a proxy for migration-linked remittances.

First, Figure B.1 shows that outside-district deposits are economically important for a non-trivial subset of borrowers and are highly concentrated geographically. Among borrowers with outside-district activity, more than four-fifths of the outside deposit value typically originates in a single destination district. Second, Figure B.2 shows that the incidence of withdrawals at home nearly doubles in the month in which an outside-district deposit episode begins, consistent with funds deposited near a workplace being accessed by household members at the origin. Third, Figure B.3 shows that 86% of outside-district deposit episodes last six months or less. Finally, Figure B.4 shows that the timing of these episodes recurs across years substantially more often than under a randomized benchmark. Together, these spatial and temporal patterns are difficult to reconcile with incidental travel or permanent relocation and are consistent with temporary or

circular migration-linked activity.

5.2.2 Main Estimates

Table 3 presents the baseline estimates. Across specifications, waiver eligibility reduces the annual probability of receiving an outside-district deposit by 1.4 to 1.7 percentage points. In the most saturated specification, the coefficient is -0.0159, relative to a pre-waiver mean of 0.113, implying a decline of approximately 14%. The coefficient is stable as the specification moves from stack-by-time fixed effects to branch-by-loan-product-by-amount-by-rate-by-time fixed effects, indicating that the result is not driven by differential trends across branches, loan products, or borrowers with different pre-waiver loan terms.

Figure 2 presents the corresponding event study. The estimates show no evidence of differential pre-waiver trends. Outside-district deposit activity begins to decline after the waiver and becomes progressively lower over the subsequent event years. The timing is consistent with a response to balance-sheet relief rather than a continuation of persistent differences between eligible and ineligible borrowers.

The reduction in outside activity is accompanied by a spatial shift toward the household's home district. Table 4 shows that waiver eligibility raises the probability of conducting any in-person cash transaction within the home district by 7.3 percentage points, or 12% relative to the pre-waiver mean. The number of home-district transactions rises by approximately 6%, while the number of outside-district transactions falls by approximately 16%. As a result, the share of transactions conducted outside the home district falls by 0.9 percentage points, or roughly 10%. Figure 3 shows no differential pre-trend and a sustained post-waiver increase in the share of activity conducted at home. The evidence therefore points to a geographic reallocation of household financial activity toward the origin rather than a mechanical decline in account use.

Several additional tests reinforce this interpretation. Table A.1 shows that the result is not specific to district boundaries: eligibility also reduces transactions outside the home branch and outside the home state. Table A.2 shows that the decline occurs on the extensive margin. Eligible borrowers become 1.6 percentage points less likely to make any outside-district deposit, while the value deposited conditional on remaining active outside the district does not change significantly. The same table shows a 6.2 percentage-point increase in the incidence of home-district deposits. Table A.3 further shows that both the total value and the share of transaction value occurring outside the home district decline. Relief therefore primarily reduces whether households maintain migration-linked outside activity, rather than merely reducing the amount transacted conditional on remaining connected to the destination.

The destinations of pre-waiver deposits provide a further test of the migration interpretation. Table 5 interacts waiver eligibility with the pre-waiver share of deposits originating in metropolitan areas and industrial hubs. The decline in outside-district deposits is substantially stronger among households previously connected to these destinations. In the fully saturated specifications, moving from no prior exposure to full exposure increases the estimated decline from roughly 8% to 32% for industrial hubs and from roughly 2% to more than 20% for metropolitan areas. Because these

locations are particularly associated with informal migrant employment, the heterogeneity shows that the response is strongest where outside deposits are most plausibly tied to distant wage work.

Taken together, the results indicate that debt relief reduces households' reliance on migration-linked wage earnings. The response goes in the opposite direction from the liquidity-access channel in which additional resources finance more migration: outside activity falls after the waiver, on the extensive margin, while financial activity shifts toward the home district. The pattern instead accords with a setting in which repayment pressure makes relatively safe migrant earnings valuable despite the substantial costs of temporary migration (Mosse et al., 2002; Imbert and Papp, 2020a,b). I interpret this as household-level labor reallocation rather than a worker-level transition into any particular local occupation; the bank data do not identify what returning members do once they are home. The capital and survey evidence below helps characterize that broader household adjustment.

5.3 Reallocation of Household Finance

I next ask whether balance-sheet repair also reallocates household finance across productive activities. The direction is again ambiguous. Both waivers explicitly sought to restore beneficiaries' access to institutional agricultural credit, so renewed borrowing for the incumbent farm is a natural benchmark. Prior work on the 2008 national waiver, however, finds little improvement in agricultural investment or productivity and lower subsequent farm credit (Kanz, 2016; Giné and Kanz, 2018). Balance-sheet repair may instead release risk-bearing and financing capacity for entry into alternative activities with uncertain and delayed returns (Evans and Jovanovic, 1989; Field et al., 2013). I therefore examine not only whether relieved households borrow again, but what they borrow for and where that credit originates.

5.3.1 The Composition of New Credit

Table 6 reports estimates for new borrowing by loan category. Waiver eligibility reduces the annual probability of obtaining a new agricultural loan by 2.8 percentage points. Relative to a pre-waiver mean of 41.8%, this is a decline of approximately 7%. The result is consistent with evidence from the 2008 national waiver that debt relief did not increase farm investment or productivity and was followed by lower agricultural lending (Kanz, 2016; Giné and Kanz, 2018). In this setting, the decline is particularly informative because restoring access to institutional farm credit was an explicit policy objective.

The farm-credit decline alone remains difficult to interpret. It could reflect lower household demand for agricultural credit or a lender response to the prospect of moral hazard following discretionary relief. The latter concern is central to evaluations of the 2008 waiver, where eligibility itself required delinquency. Here, relief was not conditioned on prior default and most eligible accounts were performing before the waiver. The saturated fixed effects also absorb credit-supply changes common to borrowers with similar pre-waiver loans within the same branch. They cannot, however, rule out lenders treating waiver beneficiaries differently after relief. The composition of subsequent borrowing is therefore more informative than the farm-credit decline in isolation.

While agricultural borrowing declines, eligible households become 2.8 percentage points more likely to obtain a personal loan, an increase of approximately 34% relative to the pre-waiver mean. This pattern is difficult to reconcile with a broad deterioration in formal credit access. Agricultural loans count toward banks' priority-sector obligations and receive substantial interest subsidies, whereas personal loans are generally less subsidized, more expensive, and often unsecured. A lender broadly unwilling to extend credit to waiver beneficiaries would not naturally replace policy-supported farm lending with less protected personal credit. The evidence does not eliminate borrower-specific supply responses, but it indicates that the dominant change is a reallocation in the composition of household finance rather than a generalized credit contraction.

The increase in personal credit is also not broad-based. Neither auto nor housing borrowing rises. Instead, the response is concentrated in self-employment and small-business loans, including credit to retail traders, microenterprises, self-employed professionals, entrepreneurs, and government employment-generation programs. These categories are particularly relevant for early-stage household enterprises, which often rely on personal borrowing when business balance sheets and collateral are limited (Robb and Robinson, 2014). The probability of receiving a new loan in these categories increases by 0.12 percentage points, or about 40% relative to the pre-waiver mean. Thus, the additional borrowing is concentrated in credit that can finance entry into alternative production rather than household durables or the incumbent farm.

Figure 4 traces the dynamics of outstanding self-employment and small-business credit. There is no evidence of a differential pre-waiver trend. The increase appears after the waiver and becomes most pronounced in the second post-waiver year, consistent with some delay between balance-sheet repair and entry into a new credit relationship. Table A.4 shows that the result is robust across increasingly saturated fixed effects: the probability of holding self-employment or small-business credit rises by approximately 0.11 to 0.14 percentage points, or 65% to 82% relative to the pre-waiver mean.

The location of origination further links the credit response to the local economy. Table A.5 separates new loans issued at the branch that made the pre-waiver farm loan from loans issued elsewhere. The increase in personal borrowing occurs almost entirely at the home branch. The same is true for self-employment and small-business credit: home-branch issuance rises significantly, while away-branch issuance does not. Alongside the decline in distant wage activity, this pattern shows household finance becoming more locally anchored after relief and shifting toward non-farm production at the origin.

5.3.2 Mechanism Heterogeneity

Table 7 examines which households account for the increase in small-business credit. Two cross-sectional patterns sharpen the debt-overhang interpretation.

First, the response is stronger among households whose pre-waiver deposits originated disproportionately from industrial hubs. The interaction between waiver eligibility and industrial-hub exposure is positive and statistically significant. Because these destinations are closely associated with migrant wage employment, the result indicates that the shift toward

local business finance is strongest among households for whom migration-linked cash flows were more salient before relief. This uses pre-waiver exposure rather than the endogenous post-waiver migration response, although the heterogeneity itself should not be interpreted as independently causal.

Second, the response increases with the share of the household's outstanding debt extinguished by the waiver. The interaction is positive and highly significant, and the combined effect for households receiving more complete balance-sheet relief is nearly twice the pre-waiver incidence of small-business borrowing. This gradient is consistent with a liability-side financial friction: households receiving economically larger balance-sheet repair exhibit the largest subsequent movement into credit used for alternative production.

Two additional patterns help distinguish this interpretation from simpler alternatives. The response does not vary with the borrower's percentile rank in total pre-waiver deposits, weighing against an explanation based simply on the scale of prior bank cash flows. Table A.6 similarly provides no evidence of a broad-based positive shock to total savings-account cash flows: estimates for cumulative deposits and withdrawals are mixed across transformations and negative in the Poisson specifications. These variables combine earnings, remittances, liquidity needs, and banking behavior and are not direct measures of income, but they provide little support for a simple cash-flow or wealth explanation for the rise in business credit.

The response also does not vary significantly with observed pre-waiver default. I interpret this null cautiously. Relief in these programs was not conditioned on delinquency, and most eligible accounts were performing before the waiver. A borrower can remain current by refinancing existing obligations, using costly informal credit, or directing scarce household cash flow toward repayment, so observed delinquency need not identify the households for whom legacy debt most tightly constrains reallocation.

Overall, the credit evidence points to a change in both the composition and location of household finance. Debt relief does not restore borrowing for the incumbent farm. Instead, relieved households increase locally originated personal and, in particular, self-employment and small-business credit. The response is strongest where migration-linked cash flows were initially more salient and where the waiver removed a larger share of outstanding debt, patterns consistent with reallocation being greatest where debt overhang is more likely to bind.

5.4 Entry into Local Non-Farm Production

The borrowing response suggests that restored balance-sheet capacity is used to enter non-farm production. I test this implication directly by linking bank borrowers to incorporated-firm and MSME registration records. The outcome equals one when a borrower is linked to a newly registered non-farm business through names and addresses. Because enterprises may remain informal, register at a different address, or be recorded under another household member, this measure captures only a subset of entrepreneurial entry and should be interpreted as a lower bound on household business formation.

Table 8 shows that waiver eligibility increases formal non-farm business formation. Across

specifications, the annual probability of registration rises by 0.03 to 0.05 percentage points. Given the very low baseline incidence of formal entry among farming households, the preferred saturated specifications imply an increase amounting to more than twice the pre-waiver rate of business formation. The coefficient remains stable as the specification adds branch-by-time, branch-by-loan-product-by-time, and the full branch-by-product-by-amount-by-rate-by-time fixed effects. The result is therefore not driven by differential local business cycles or by trends associated with particular pre-waiver loan products.

Figure 6 presents the corresponding event study over a wider window. The estimates are close to zero throughout the pre-waiver period. Formal entry begins to rise several years after the waiver and is largest in the fourth and fifth post-waiver years. The lag is consistent with the time required to establish and formally register a business and distinguishes the result from a short-lived response around the announcement. The estimates are also unchanged when I restrict the matched sample to businesses that remain active for at least five years after the waiver, weighing against an interpretation based on transitory registration.

Together with the shift toward self-employment and small-business credit, the registration evidence shows that balance-sheet repair is associated with actual entry into new local production rather than merely a relabeling of household borrowing. The combination is also notable relative to the stated policy objective: relief does not revive borrowing for the indebted farm, but household finance and business formation move toward alternative non-farm activity.

5.5 Does New Business Credit Reflect Distortionary Risk-Taking?

A natural concern is that the waiver encourages low-quality, subsistence entry or excessive risk-taking rather than productive household reallocation. The data do not permit a complete comparison of productivity across activities, but they allow me to examine whether the expansion in business credit comes at the expense of repayment performance and whether the resulting businesses persist.

Table 11 compares the post-waiver performance of self-employment and small-business loans held by eligible and ineligible borrowers. There is no evidence that business loans issued to eligible borrowers default more frequently. In the most saturated specification, the coefficient on the default indicator is economically small and statistically indistinguishable from zero. The share of the outstanding business-loan balance in default is, if anything, 0.05 percentage points lower among eligible borrowers. Thus, the additional business credit does not appear to be generated by a deterioration in observed lending quality.

Figure 5 shows the dynamics. Default performance does not deteriorate after the waiver; the post-waiver estimates are weakly negative, with no evidence that worsening repayment precedes the increase in business credit. Combined with the persistence of the matched businesses documented above, these findings weigh against an interpretation in which debt relief merely induces transitory entry financed by poorly performing loans. They do not establish that every new enterprise is high-productivity or that the welfare return to entry is positive, but they show that the business response is not accompanied by lower observed credit quality or short-lived registration.

5.6 Identification and External Validation

The preceding estimates rely on two features of the bank data: eligibility varies across loan types and sanction dates, and distant employment is inferred from the geography of cash transactions. I conduct two complementary exercises that address these identification and measurement concerns while also testing whether the reallocation pattern appears in a broader sample.

5.6.1 Regression Discontinuity at the Sanction-Date Cutoffs

The baseline sample already restricts the control group to borrowers with prior access to the crop-loan category targeted by relief. Nevertheless, eligible and ineligible borrowers may differ along unobserved dimensions associated with their specific pre-waiver loan products. Appendix Figure C.1 therefore isolates variation generated solely by the sanction-date cutoffs, comparing borrowers within the same loan category and branch around the predetermined threshold.

The top panel tests for manipulation in the density of loans around the state-specific thresholds. The cutoff dates were announced several months after they had passed, making anticipatory sorting mechanically implausible, and the McCrary test does not reject continuity at conventional levels. The bottom panels compare borrowers with loans sanctioned immediately before and after the cutoff. Migration-linked deposits fall sharply at the eligibility threshold, while the probability of taking small-business credit rises. Both discontinuities mirror the difference-in-differences estimates. Because the RD holds the loan category and branch fixed and uses only borrowers close to the threshold, it substantially reduces the scope for unobserved product composition or borrower type to explain the central results.

5.6.2 Complementary Household-Survey Evidence

The bank records precisely measure eligibility, borrowing, and transaction location, but they do not directly observe member-level mobility or occupation. Appendix Section D.1 therefore turns to the Consumer Pyramids Household Survey (CPHS), which directly records household-member absence, return, and occupation, and examines twelve state-level farm-loan waiver episodes between 2014 and 2019. These tests use a different sample, broader policy variation, and direct survey measures. They sacrifice some of the identification precision of the bank analysis in exchange for independent measurement and external validity beyond the two 2014 programs and the bank sample.

Table D.1 examines member absence and return. Among farming households in waiver-announcing states, the point estimates for member absence are negative, while the incidence and number of previously absent members returning to the household increase significantly after waiver announcements. The increase in the probability of a return is approximately 0.13 percentage points, and the number of returning members rises by 0.24 percentage points relative to other households in the same waiver states. These direct mobility measures point in the same direction as the decline in migration-linked deposits in the administrative data.

Table D.2 examines the destination side. Because CPHS does not directly record each migrant's origin, I use pre-existing social connectedness between destination districts and waiver-announcing states to identify locations more likely to host migrants from those states. After a waiver, wage

workers in districts with above-median connectedness are 2.3 percentage points more likely to disappear from the survey panel; the corresponding effect is 3.1 percentage points for top-tercile connectedness. Transitions into likely-away status rise by 1.5 to 2.3 percentage points. Because these districts lie outside the waiver state, the pattern is consistent with labor withdrawing from likely migrant destinations after debt relief at the origin.

Finally, Table D.3 examines occupation directly. Farming households in waiver-announcing states become 0.54 percentage points more likely to report a self-employed or entrepreneurial member, and 0.35 percentage points more likely to transition into self-employment after the waiver. Thus, the survey data independently reproduce both sides of the bank-based reallocation pattern: greater return toward farming households and greater entry into non-farm self-employment.

No single survey test provides the same quasi-experimental precision as the bank design. Their value lies in convergence and external validity. The same pattern appears across administrative and survey data, borrower- and household-level samples, origin- and destination-side mobility measures, and financial and occupational outcomes. This convergence makes it less likely that the main findings are driven by selection across loan products or by the particular transaction-based proxy available in the bank data, and shows that similar responses appear across a broader set of Indian debt-relief episodes.

5.7 Broader Local Outcomes

The borrower-level evidence shows that debt relief redirects household finance toward local enterprise. I conclude the results by asking whether this reallocation has broader local counterparts. These estimates are intentionally complementary: district exposure is not randomly assigned, and the results should not be interpreted as the full aggregate or general-equilibrium effects of the waivers.

Table 9 relates district-level business registrations to the local share of bank borrowers eligible for relief. Districts with above-median exposure experience a Poisson coefficient of 0.520 after the waiver, corresponding to approximately 68% greater business formation relative to less-exposed districts. Using top-tercile exposure yields a coefficient of 0.333, or approximately 40%. The estimates include district and year fixed effects, so they compare changes within districts while absorbing common statewide time shocks. These magnitudes indicate that the borrower-level shift toward enterprise has an economically meaningful local counterpart, although district exposure itself should not be interpreted as quasi-random.

Figure 7 shows no discernible difference in registrations before the waiver. Business formation rises after implementation and grows over the subsequent three years. The timing is consistent with the delayed household-level registration response in Figure 6, rather than an immediate mechanical change in registration around the announcement.

Table 10 reports analogous estimates for nighttime-light intensity, a broader proxy for local economic activity. Above-median-exposure districts experience an increase of approximately 2.6%, while top-tercile-exposure districts experience an increase of approximately 4.0%. Figure 8 again shows no differential pre-waiver trend. The increase emerges gradually after the waiver and is

largest by the third post-waiver year.

The district estimates do not establish the net aggregate effect of the programs. They cannot separately identify spillovers, displacement across districts, fiscal costs, or labor-supply effects along migrant-destination corridors. They nevertheless show that the household-level shift toward local non-farm production has counterparts in broader measures of business formation and economic activity, making the reallocation channel quantitatively relevant beyond the individual borrower outcomes.

6 Conclusion

A central question in development finance is why household labor and capital remain concentrated in low-productivity activities even after large expansions in formal credit. This paper highlights a liability-side counterpart to the usual credit-access explanation. In settings where household production is heavily financed and rules-based personal bankruptcy is limited, legacy debt can itself impede reallocation: repayment obligations increase the value of safe and immediate cash flows while reducing households' capacity to bear risk and finance entry into alternative activities. Debt accumulated alongside credit expansion may therefore become part of the occupational constraint that additional credit alone does not resolve.

I study this mechanism using large farm-loan waivers in two southern states of India in 2014. Eligibility depended on predetermined loan type and sanction date rather than prior default, allowing me to compare otherwise similar farming borrowers who experienced different balance-sheet relief. The results show a coordinated reallocation of household labor and capital. Eligible households reduce migration-linked distant wage activity and shift financial activity toward the local economy. They take fewer new farm loans, but more personal and small-business credit, overwhelmingly originated locally, and become more likely to register new non-farm businesses. The business-credit response is strongest among households previously more exposed to migrant labor destinations and among those for whom the waiver extinguishes a larger share of debt, consistent with reallocation being greatest where debt overhang is more likely to bind.

The findings have two broader implications. First, they identify accumulated household liabilities as a financial friction on real reallocation, particularly in a setting where credit to the incumbent activity is already abundant, directed, and subsidized. Expanding credit can relax financing constraints ex ante, but once households carry substantial legacy obligations, balance-sheet repair may be necessary to restore risk-bearing and financing capacity for alternative uses. Second, the results identify a financial determinant of rural diversification. Recent work emphasizes the growing importance of productive rural non-farm activity as a margin of structural transformation when the traditional movement from agriculture into urban manufacturing is weak (Foster and Rosenzweig, 2004; Haggblade et al., 2010; Rodrik, 2016). The evidence here shows this process operating within affected farming households: relief reduces reliance on costly migration-linked wage work and redirects household finance and entry toward local non-farm production. More generally, evaluations that focus only on repayment or investment in the sector

whose debt is forgiven may miss an important real consequence of household debt relief: the reallocation of resources across activities.

These findings should not be read as a normative case for farm-loan waivers. The programs entail important costs that I do not fully quantify, including fiscal outlays, possible deterioration in repayment incentives and future credit supply, and labor-market spillovers in migrant destinations. At the same time, the estimates show that their consequences extend beyond borrower repayment and agricultural lending. In a simple accounting exercise, I find that the gains associated with household reallocation offset about 28% of the fiscal cost, after allowing for lower farm credit. This suggests that the reallocation margin is quantitatively meaningful, but not large enough to justify such waivers as welfare-improving. More broadly, the costs of repeated discretionary waivers highlight the importance of debt-resolution design. Indarte and Kanz (2024) argue that legal personal bankruptcy can provide a rules-based avenue for discharging unsustainable debt and reduce the expectation-driven distortions associated with ad hoc relief. Bankruptcy protection also affects household borrowing and credit pricing (Severino et al., 2026), so it is not costless. The evidence in this paper nevertheless suggests that institutions governing the resolution of legacy household liabilities may matter not only for repayment and credit markets, but also for the allocation of labor and capital across productive activities.

References

Agarwal, S., S. Alok, P. Ghosh, S. Ghosh, T. Piskorski, and A. Seru (2017a): "Banking the Unbanked: What do 280 Million New Bank Accounts Reveal about Financial Access?" *Columbia Business School Research Paper*.

Agarwal, S., G. Amromin, I. Ben-David, S. Chomsisengphet, T. Piskorski, and A. Seru (2017b): "Policy Intervention in Debt Renegotiation: Evidence from the Home Affordable Modification Program," *Journal of Political Economy*, 125, 654–712.

Ambrose, B. W., L. Cordell, and S. Ma (2015): "The Impact of Student Loan Debt on Small Business Formation," Working Paper 15-26, Federal Reserve Bank of Philadelphia.

Andersen, S. and K. M. Nielsen (2012): "Ability or Finances as Constraints on Entrepreneurship? Evidence from Survival Rates in a Natural Experiment," *Review of Financial Studies*, 25, 3684–3710.

Angelucci, M. (2015): "Migration and Financial Constraints: Evidence from Mexico," *Review of Economics and Statistics*, 97, 224–228.

Bai, J. J., D. Carvalho, and G. M. Phillips (2018): "The Impact of Bank Credit on Labor Reallocation and Aggregate Industry Productivity," *The Journal of Finance*, 73, 2787–2836.

Bailey, M., R. Cao, T. Kuchler, J. Stroebel, and A. Wong (2018): "Social connectedness: Measurement, determinants, and effects," *Journal of Economic Perspectives*, 32, 259–280.

Banerjee, A. V. and A. F. Newman (1993): "Occupational Choice and the Process of Development," *Journal of Political Economy*, 101, 274–298.

Battaglia, M., S. Gulesci, and A. Madestam (2024): "Repayment Flexibility and Risk Taking: Experimental Evidence from Credit Contracts," *The Review of Economic Studies*, 91, 2635–2675.

Bellon, A., J. A. Cookson, E. P. Gilje, and R. Z. Heimer (2021): "Personal Wealth, Self-Employment, and Business Ownership," *Review of Financial Studies*, 34, 3935–3975.

Bernstein, A. (2021): "Negative Home Equity and Household Labor Supply," *Journal of Finance*, 76, 2963–2995.

Bernstein, A. and D. Struyven (2022): "Housing Lock: Dutch Evidence on the Impact of Negative Home Equity on Household Mobility," *American Economic Journal: Economic Policy*, 14, 1–32.

Birthal, P. S., D. Roy, M. T. Khan, and D. S. Negi (2015): "Farmers' Preference for Farming: Evidence From a Nationally Representative Farm Survey in India," *The Developing Economies*, 53, 122–134.

Bolton, P. and H. Rosenthal (2002): "Political intervention in debt contracts," *Journal of Political Economy*, 110, 1103–1134.

Breman, J. (1996): *Footloose Labour: Working in India's Informal Economy*, Cambridge: Cambridge University Press.

Brown, J. and D. A. Matsa (2020): "Locked in by Leverage: Job Search during the Housing Crisis," *Journal of Financial Economics*, 136, 623–648.

Bryan, G., S. Chowdhury, and A. M. Mobarak (2014): "Underinvestment in a profitable technology: The case of seasonal migration in Bangladesh," *Econometrica*, 82, 1671–1748.

Burgess, R. and R. Pande (2005): "Do Rural Banks Matter? Evidence from the Indian Social Banking Experiment," *American Economic Review*, 95, 780–795.

Bustos, P., B. Caprettini, and J. Ponticelli (2016): "Agricultural Productivity and Structural Transformation: Evidence from Brazil," *American Economic Review*, 106, 1320–1365.

Bustos, P., G. Garber, and J. Ponticelli (2020): "Capital Accumulation and Structural Transformation," *The Quarterly Journal of Economics*, 135, 1037–1094.

Cagetti, M. and M. De Nardi (2006): "Entrepreneurship, Frictions, and Wealth," *Journal of Political Economy*, 114, 835–870.

Cai, S. (2020): "Migration under Liquidity Constraints: Evidence from Randomized Credit Access in China," *Journal of Development Economics*, 142, 102247.

CBC News (2021): "Burdened by Debt and Unable to Eke Out a Living, Many Farmers in India Turn to Suicide," `https://www.cbc.ca/news`, accessed via CBC News International.

Central Statistics Office (2013): "State Domestic Product and Other Aggregates: Andhra Pradesh," Tech. rep., Ministry of Statistics and Programme Implementation, Government of India, New Delhi.

Chakrabarti, R., V. Fos, A. Liberman, and C. Yannelis (2023): "Tuition, Debt, and Human Capital," *The Review of Financial Studies*, 36, 1667–1702.

Corradin, S. and A. Popov (2015): "House Prices, Home Equity Borrowing, and Entrepreneurship," *Review of Financial Studies*, 28, 2399–2428.

Darko, F. A., A. Dey, and S. K. Ritadhi (2025): "Rural Non-Farm Employment in Rajasthan: Determinants, Welfare Effects, and Enterprise Performance," Policy Research Working Paper 11080, World Bank.

de Mel, S., D. McKenzie, and C. Woodruff (2008): "Returns to Capital in Microenterprises: Evidence from a Field Experiment," *Quarterly Journal of Economics*, 123, 1329–1372.

Deshingkar, P. and S. Akter (2009): "Migration and Human Development in India," *UNDP Human Development Research Paper*.

Deshingkar, P. and D. Start (2003): "Seasonal Migration for Livelihoods in India: Coping, Accumulation and Exclusion," Working Paper 220, Overseas Development Institute, London.

Di Maggio, M., A. Kalda, and V. Yao (2026): "Second Chance: Life with Less Student Debt," *The Journal of Finance*, 81, 507–550.

Dinerstein, M., S. Earnest, D. K. Koustas, and C. Yannelis (2025): "Student loan forgiveness," Tech. rep., National Bureau of Economic Research.

Directorate of Economics and Statistics (2012): "Season and Crop Report: Andhra Pradesh 2011–12," Tech. rep., Government of Andhra Pradesh, Hyderabad.

Dobbie, W. and J. Song (2015): "Debt Relief and Debtor Outcomes: Measuring the Effects of Consumer Bankruptcy Protection," *American Economic Review*, 105, 1272–1311.

——— (2020): "Targeted Debt Relief and the Origins of Financial Distress: Experimental Evidence from Distressed Credit Card Borrowers," *American Economic Review*, 110, 984–1018.

Donaldson, J. R., G. Piacentino, and A. Thakor (2019): "Household Debt Overhang and Unemployment," *Journal of Finance*, 74, 1473–1502.

Evans, D. S. and B. Jovanovic (1989): "An Estimated Model of Entrepreneurial Choice under Liquidity Constraints," *Journal of Political Economy*, 97, 808–827.

Field, E., R. Pande, J. Papp, and N. Rigol (2013): "Does the Classic Microfinance Model Discourage Entrepreneurship among the Poor? Experimental Evidence from India," *American Economic Review*, 103, 2196–2226.

Foster, A. D. and M. R. Rosenzweig (2004): "Agricultural Productivity Growth, Rural Economic Diversity, and Economic Reforms: India, 1970–2000," *Economic Development and Cultural Change*, 52, 509–542.

——— (2008): "Economic Development and the Decline of Agricultural Employment," in *Handbook of Development Economics*, ed. by T. P. Schultz and J. Strauss, Elsevier, vol. 4, chap. 47, 3051–3083.

Ganong, P. and P. Noel (2020): "Liquidity versus Wealth in Household Debt Obligations: Evidence from Housing Policy in the Great Recession," *American Economic Review*, 110, 3100–3138.

Garg, S., S. Gupta, and S. Mallick (2025): "Financial access and entrepreneurship by gender: evidence from rural India," *Small Business Economics*, 64, 729–754.

Gertner, R. and D. Scharfstein (1991): "A Theory of Workouts and the Effects of Reorganization Law," *Journal of Finance*, 46, 1189–1222.

Giné, X. and M. Kanz (2018): "The Economic Effects of a Borrower Bailout: Evidence from an Emerging Market," *The Review of Financial Studies*, 31, 1752–1783.

Gollin, D., D. Lagakos, and M. E. Waugh (2014): "Agricultural productivity differences across countries," *American Economic Review*, 104, 165–170.

Gupta, J. (2024): "Seeds of Debt: How Loan Waivers Affect the Farmers and the Economy," Forbes India.

Haggblade, S., P. Hazell, and T. Reardon (2010): "The Rural Non-Farm Economy: Prospects for Growth and Poverty Reduction," *World Development*, 38, 1429–1441.

Hampole, M. V. (2022): "Student Debt and Labor Market Trajectories," Working paper.

Harris, J. R. and M. P. Todaro (1970): "Migration, Unemployment and Development: A Two-Sector Analysis," *American Economic Review*, 60, 126–142.

He, A. X. and D. Le Maire (2023): "Household Liquidity Constraints and Labor Market Outcomes: Evidence from a Danish Mortgage Reform," *The Journal of Finance*, 78, 3251–3298.

Henderson, J. V., A. Storeygard, and D. N. Weil (2012): "Measuring Economic Growth from Outer Space," *American Economic Review*, 102, 994–1028.

Hennessy, C. A. (2004): "Tobin's Q, Debt Overhang, and Investment," *Journal of Finance*, 59, 1717–1742.

Holtz-Eakin, D., D. Joulfaian, and H. S. Rosen (1994): "Sticking It Out: Entrepreneurial Survival and Liquidity Constraints," *Journal of Political Economy*, 102, 53–75.

Hurst, E. and A. Lusardi (2004): "Liquidity Constraints, Household Wealth, and Entrepreneurship," *Journal of Political Economy*, 112, 319–347.

Imbert, C. and J. Papp (2015): "Labor Market Effects of Social Programs: Evidence from India's Employment Guarantee," *American Economic Journal: Applied Economics*, 7, 233–263.

——— (2020a): "Costs and benefits of rural-urban migration: Evidence from India," *Journal of Development Economics*, 146, 102473.

——— (2020b): "Short-term Migration, Rural Public Works, and Urban Labor Markets: Evidence from India," *Journal of the European Economic Association*, 18, 927–963.

Indarte, S. and M. Kanz (2024): "Debt Relief for Households in Developing Economies," *Oxford Review of Economic Policy*, 40, 139–159.

Kalemli-Özcan, Ş., L. Laeven, and D. Moreno (2022): "Debt Overhang, Rollover Risk, and Corporate Investment: Evidence from the European Crisis," *Journal of the European Economic Association*, 20, 2353–2395.

Kanz, M. (2016): "What Does Debt Relief Do for Development? Evidence from India's Bailout Program for Highly-Indebted Rural Households," *American Economic Journal: Applied Economics*, 8, 66–99.

Keshri, K. and R. B. Bhagat (2012): "Temporary and Seasonal Migration: Regional Pattern, Characteristics and Associated Factors," *Economic and Political Weekly*, 47, 81–88.

Krishnan, K. and P. Wang (2019): "The Cost of Financing Education: Can Student Debt Hinder Entrepreneurship?" *Management Science*, 65, 4522–4554.

La Porta, R. and A. Shleifer (2014): "Informality and Development," *Journal of Economic Perspectives*, 28, 109–126.

Lagakos, D., A. M. Mobarak, and M. E. Waugh (2023): "The welfare effects of encouraging rural–urban migration," *Econometrica*, 91, 803–837.

Lang, L., E. Ofek, and R. M. Stulz (1996): "Leverage, Investment, and Firm Growth,"

Journal of Financial Economics, 40, 3–29.

Lanjouw, P. and R. Murgai (2009): "Poverty decline, agricultural wages, and nonfarm employment in rural India: 1983–2004," *Agricultural Economics*, 40, 243–263.

Lucas, R. E. B. and O. Stark (1985): "Motivations to Remit: Evidence from Botswana," *Journal of Political Economy*, 93, 901–918.

Mahambare, V., S. Dhanaraj, and P. Mittal (2022): "The Political Budget Cycles in the Presence of a Fiscal Rule: The Case of Farm Debt Waivers in India," *Journal of Policy Modeling*, 44, 701–721.

McKenzie, D. and C. Woodruff (2008): "Experimental Evidence on Returns to Capital and Access to Finance in Mexico," *World Bank Economic Review*, 22, 457–482.

Merriott, D. (2016): "Factors associated with the farmer suicide crisis in India," *Journal of epidemiology and global health*, 6, 217–227.

Morazzoni, M. (2024): "Student Debt and Entrepreneurship in the US," Working paper.

Morten, M. (2019): "Temporary Migration and Endogenous Risk Sharing in Village India," *Journal of Political Economy*, 127, 1–46.

Mosse, D., S. Gupta, M. Mehta, V. Shah, J. Rees, and K. P. Team (2002): "Brokered Livelihoods: Debt, Labour Migration and Development in Tribal Western India," *Journal of Development Studies*, 38, 59–88.

Mukherjee, S., K. Subramanian, and P. Tantri (2018): "Borrowers' distress and debt relief: Evidence from a natural experiment," *The Journal of Law and Economics*, 61, 607–635.

Munshi, K. and M. Rosenzweig (2016): "Networks and Misallocation: Insurance, Migration, and the Rural-Urban Wage Gap," *American Economic Review*, 106, 46–98.

Myers, S. C. (1977): "Determinants of Corporate Borrowing," *Journal of Financial Economics*, 5, 147–175.

Narayanan, S. and N. Mehrotra (2019): "Loan Waivers and Bank Credit: Reflections on the Evidence and the Way Forward," *Vikalpa*, 44, 198–210.

National Sample Survey Office (2010): "Migration in India: NSS 64th Round (July 2007–June 2008)," NSS Report 533, Ministry of Statistics and Programme Implementation, Government of India, New Delhi.

——— (2013): "Key Indicators of Situation of Agricultural Households in India: NSS 70th Round," NSS Report 576, Ministry of Statistics and Programme Implementation, Government of India, New Delhi.

Oliver, J. and D. Radcliffe (2011): "How Do Migrant Workers Move Money in India?" Consultative Group to Assist the Poor Blog.

Patel, U. R. (2017): "Farm Loan Waivers: The Fiscal and Financial Implications," Address at the Bank for International Settlements (BIS) meeting, speech by the Governor, Reserve Bank of India.

Ponticelli, J. and L. S. Alencar (2016): "Court Enforcement, Bank Loans, and Firm Investment: Evidence from a Bankruptcy Reform in Brazil," *Quarterly Journal of Economics*, 131, 1365–1413.

Ramachandran, V. K., M. Swaminathan, and V. Rawal (2010): "Agricultural Workers and Their Households: A Study of Six Villages in India," *Review of Agrarian Studies*, 1, 1–48.

Robb, A. M. and D. T. Robinson (2014): "The Capital Structure Decisions of New Firms," *The Review of Financial Studies*, 27, 153–179.

Rodrik, D. (2016): "Premature Deindustrialization," *Journal of Economic Growth*, 21, 1–33.

Rosenzweig, M. R. and O. Stark (1989): "Consumption Smoothing, Migration, and Marriage: Evidence from Rural India," *Journal of Political Economy*, 97, 905–926.

Schmalz, M. C., D. A. Sraer, and D. Thesmar (2017): "Housing Collateral and

Entrepreneurship," *Journal of Finance*, 72, 99–132.

Severino, F., M. Brown, and R. Chakrabarti (2026): "Personal Bankruptcy Protection and Household Debt," *The Review of Financial Studies*, advance Access, August 12, 2026.

Stark, O. and D. E. Bloom (1985): "The New Economics of Labor Migration," *American Economic Review*, 75, 173–178.

Stiglitz, J. E. and A. Weiss (1981): "Credit Rationing in Markets with Imperfect Information," *American Economic Review*, 71, 393–410.

Zator, M. (2025): "Working More to Pay the Mortgage: Household Debt, Interest Rates, and Family Labor Supply," *Journal of Finance*, 80, 1171–1207.

Figure 1: **Geographic Setting: Andhra Pradesh and Telangana**

This figure locates Andhra Pradesh and Telangana within India. The two states formed undivided Andhra Pradesh until its bifurcation in June 2014.



Figure 2: **Debt Relief and Migration-Linked Outside-District Deposits: Dynamics**

This figure presents borrower-level event-study estimates of the effect of waiver eligibility on migration-linked outside-district deposits. The dependent variable is an indicator equal to one if borrower i's savings account records any in-person cash deposit made at a bank branch outside the home district in year t. These deposits require physical presence at the transaction branch and exhibit spatial and temporal patterns consistent with remittances associated with partial migration; Internet Appendix Section B.1 provides detailed validation. The borrower's home district is defined as the district of the bank branch that issued the borrower's pre-waiver farm loan. The x-axis plots years relative to the waiver, with year 0—the year immediately preceding implementation—as the omitted period. The vertical dashed line marks the waiver announcement. The y-axis plots the estimated coefficients, β_τ, from:

$$\mathbb{1}(OutsideDeposit)_{i,s,t} = \sum_{\tau=-2}^{3} \beta_\tau \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(t = \tau) + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

$Treat_{i,s}$ is an indicator for borrowers eligible for debt relief in waiver stack s, α_i denotes borrower fixed effects, and $X_{i,t}$ denotes flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Vertical bars report 95% confidence intervals.

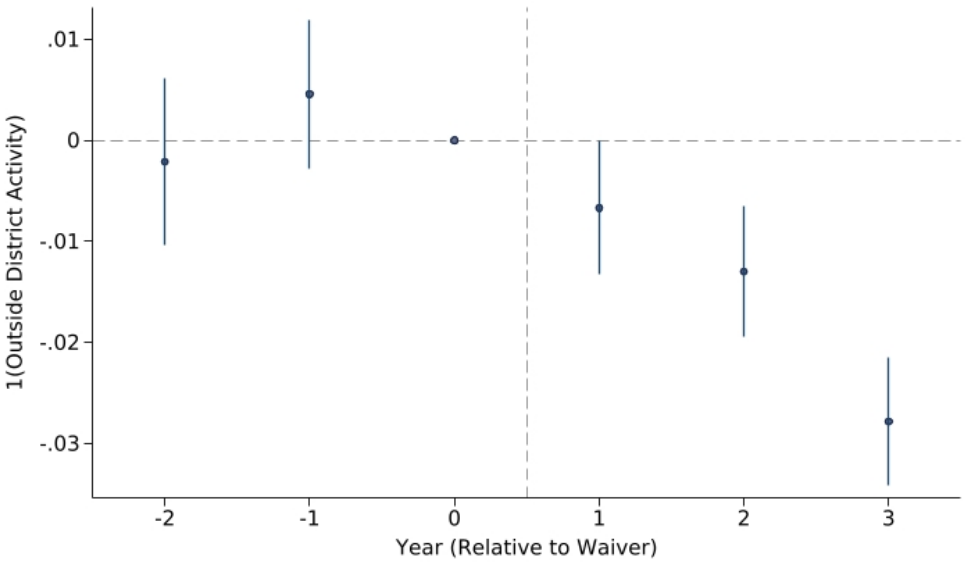


Figure 3: **Debt Relief and the Spatial Reallocation of Cash-Transaction Activity: Dynamics**

This figure presents borrower-level event-study estimates of the effect of waiver eligibility on the location of in-person cash-transaction activity. The dependent variable is the share of borrower i's in-person cash transactions made at bank branches within the home district in year t. The borrower's home district is defined as the district containing the branch that issued the borrower's pre-waiver farm loan. The x-axis plots years relative to the waiver, with year 0—the year immediately preceding implementation—as the omitted period. The vertical dashed line marks the waiver announcement. The y-axis plots the estimated coefficients, β_τ, from:

$$y_{i,s,t} = \sum_{\tau=-2}^{3} \beta_\tau \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(t = \tau) + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

$Treat_{i,s}$ is an indicator for borrowers eligible for debt relief in waiver stack s, α_i denotes borrower fixed effects, and $X_{i,t}$ denotes flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Vertical bars report 95% confidence intervals.

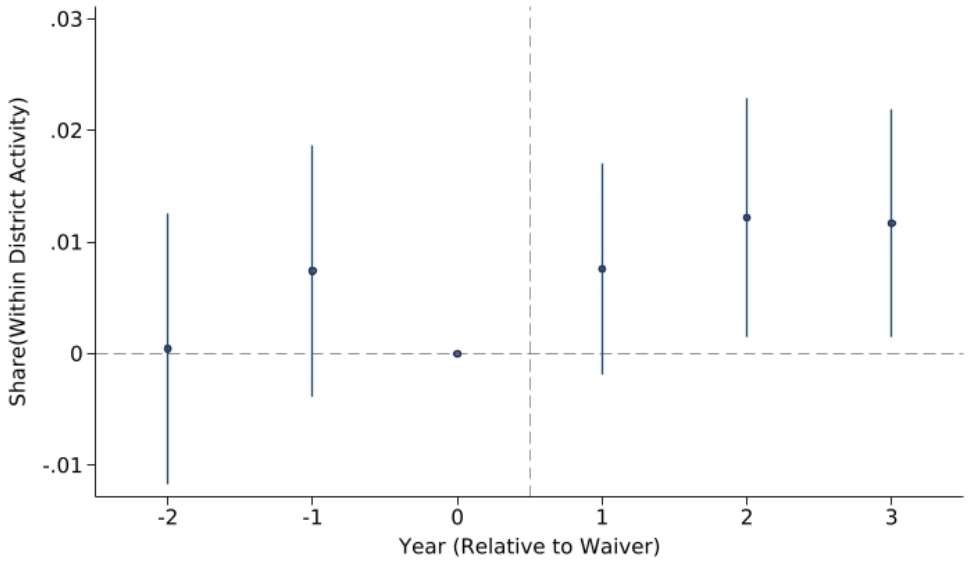


Figure 4: **Debt Relief and Self-Employment and Small-Business Credit: Dynamics**

This figure presents borrower-level event-study estimates of the effect of waiver eligibility on self-employment and small-business credit. The dependent variable is an indicator equal to one if borrower i has an outstanding loan in a self-employment or small-business category in year t. These categories include credit to self-employed professionals, retail traders, microenterprises, entrepreneurs, and employment-generation programs. The x-axis plots years relative to the waiver, with year 0—the year immediately preceding implementation—as the omitted period. The vertical dashed line marks the waiver announcement. The y-axis plots the estimated coefficients, β_τ, from:

$$\mathbb{1}(SmallBusinessCredit)_{i,s,t} = \sum_{\tau=-2}^{3} \beta_\tau \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(t = \tau) + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

$Treat_{i,s}$ is an indicator for borrowers eligible for debt relief in waiver stack s, α_i denotes borrower fixed effects, and $X_{i,t}$ denotes flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Vertical bars report 95% confidence intervals.

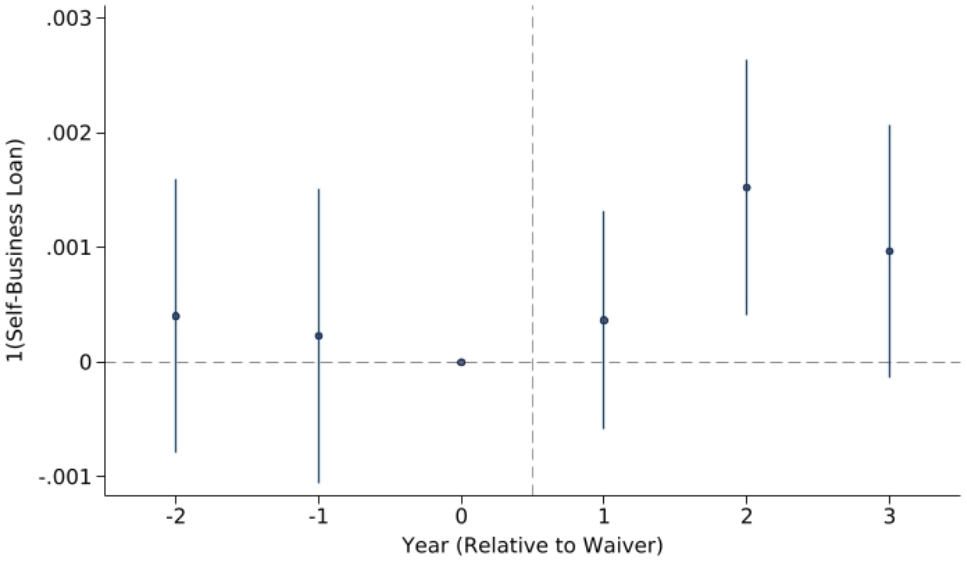


Figure 5: **Debt Relief and the Performance of Self-Employment and Small-Business Loans: Dynamics**

This figure presents borrower-level event-study estimates of the effect of waiver eligibility on the performance of self-employment and small-business credit. The dependent variable is the share of borrower i's outstanding loan balance in these categories that is marked in default in year t. The x-axis plots years relative to the waiver, with year 0—the year immediately preceding implementation—as the omitted period. The vertical dashed line marks the waiver announcement. The y-axis plots the estimated coefficients, β_τ, from:

$$DefaultShare^{SB}_{i,s,t} = \sum_{\tau=-2}^{3} \beta_\tau \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(t = \tau) + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

$Treat_{i,s}$ is an indicator for borrowers eligible for debt relief in waiver stack s, α_i denotes borrower fixed effects, and $X_{i,t}$ denotes flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Vertical bars report 95% confidence intervals.

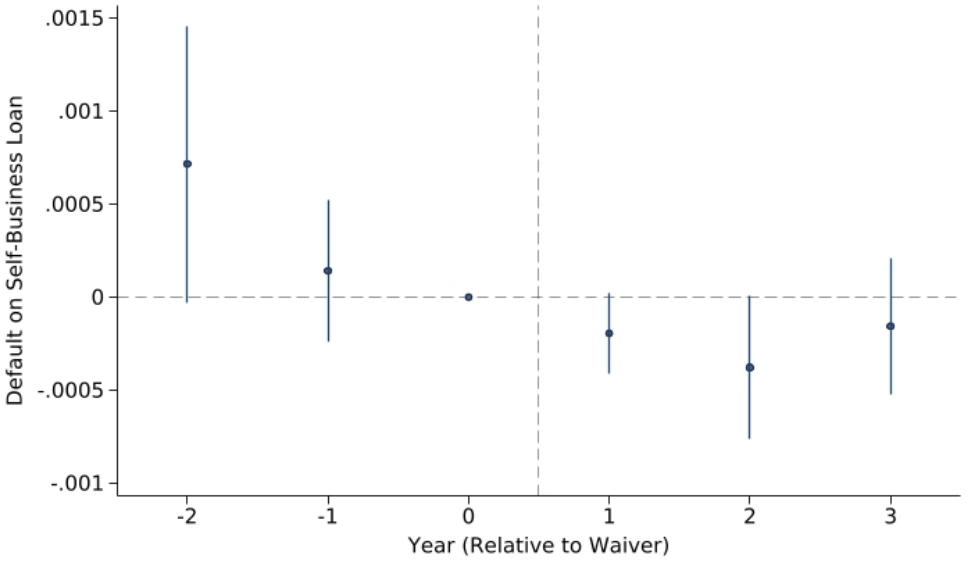


Figure 6: **Debt Relief and Registered Non-Farm Business Formation: Dynamics**

This figure presents borrower-level event-study estimates of the effect of waiver eligibility on registered non-farm business formation. I link borrowers in the bank data to government business-registration records using borrower addresses from the bank and firms' registered addresses. The dependent variable is an indicator equal to one if borrower i is linked to a business newly registered in year t. Because businesses may remain informal, register at another address, or be registered under another household member, this outcome is a conservative measure of total entrepreneurial entry. The x-axis plots years relative to the waiver, with year 0—the year immediately preceding implementation—as the omitted period. The vertical dashed line marks the waiver announcement. The y-axis plots the estimated coefficients, β_τ, from:

$$\mathbb{1}(NewRegisteredBusiness)_{i,s,t} = \sum_{\tau=-4}^{5} \beta_\tau \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(t = \tau) + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

$Treat_{i,s}$ is an indicator for borrowers eligible for debt relief in waiver stack s, α_i denotes borrower fixed effects, and $X_{i,t}$ denotes flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Vertical bars report 95% confidence intervals. Because registered entry is rare and may occur with a lag, the figure uses a wider event window than the main bank-outcome analyses.

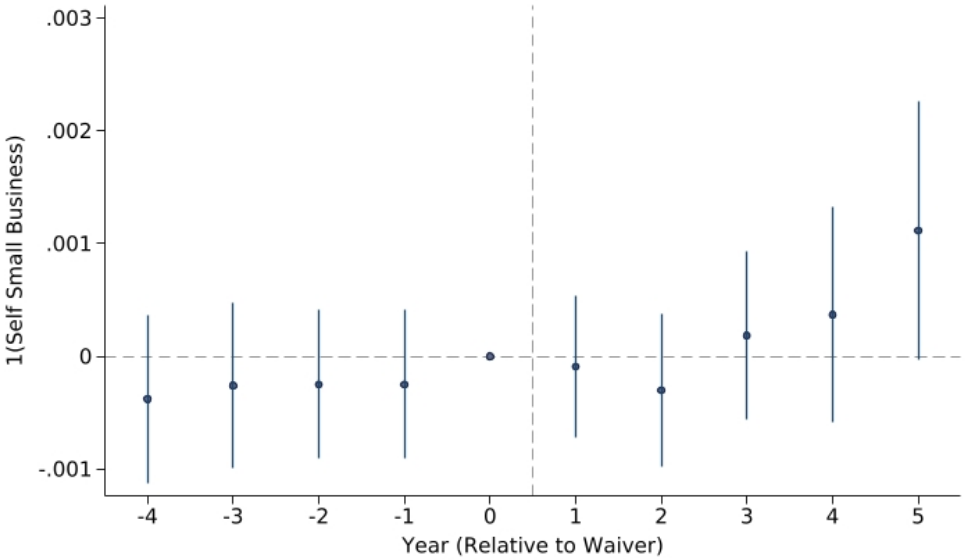


Figure 7: **Debt Relief Exposure and District-Level Business Registrations: Dynamics**

This figure presents district-level event-study estimates relating local exposure to waiver eligibility to new incorporated-business registrations. The dependent variable is the number of businesses newly incorporated in district d in year t. District exposure is constructed from the bank sample: $TreatedDistrict_d$ equals one if the district's share of farm borrowers eligible for relief is above the state median. I estimate the following Poisson specification:

$$\mathbb{E}\left[NewBusinesses_{d,t} \mid TreatedDistrict_d, \alpha_d, \lambda_t\right] = \exp\left(\sum_{\substack{\tau=-2 \\ \tau\neq 0}}^{3} \beta_\tau TreatedDistrict_d \times \mathbb{1}(t=\tau) + \alpha_d + \lambda_t\right).$$

The x-axis plots years relative to the waiver. Year 0, the year immediately preceding implementation, is omitted and normalized to zero; the vertical dashed line marks the waiver announcement. α_d and λ_t denote district and year fixed effects. The plotted coefficients compare changes in registrations in above-median-exposure districts with changes in below-median-exposure districts, relative to the omitted year. In the Poisson specification, the exact percentage effect is $100 \times [\exp(\beta_\tau) - 1]$. Standard errors are clustered at the district level, and vertical bars report 95% confidence intervals. These estimates are complementary evidence on broader local outcomes and are not interpreted as borrower-level causal effects or full equilibrium effects.

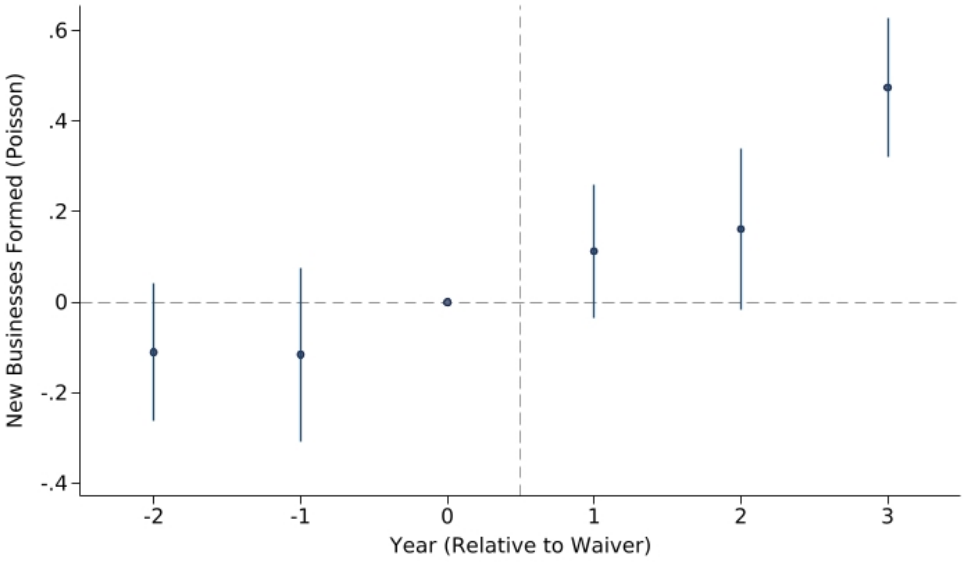


Figure 8: **Debt Relief Exposure and District-Level Nighttime Lights: Dynamics**

This figure presents district-level event-study estimates relating local exposure to waiver eligibility to nighttime-light activity. The dependent variable is the logarithm of average VIIRS nighttime-light intensity in district d in year t. District exposure is constructed from the bank sample: $TreatedDistrict_d$ equals one if the district's share of farm borrowers eligible for relief is above the state median. I estimate:

$$\log(NightLight_{d,t}) = \sum_{\substack{\tau=-2 \\ \tau\neq 0}}^{3} \beta_\tau TreatedDistrict_d \times \mathbb{1}(t=\tau) + \alpha_d + \lambda_t + \varepsilon_{d,t}.$$

The x-axis plots years relative to the waiver. Year 0, the year immediately preceding implementation, is omitted and normalized to zero; the vertical dashed line marks the waiver announcement. α_d and λ_t denote district and year fixed effects. The plotted coefficients compare changes in nighttime lights in above-median-exposure districts with changes in below-median-exposure districts, relative to the omitted year. Standard errors are clustered at the district level, and vertical bars report 95% confidence intervals. These estimates are complementary evidence on broader local outcomes and are not interpreted as borrower-level causal effects or full equilibrium effects.

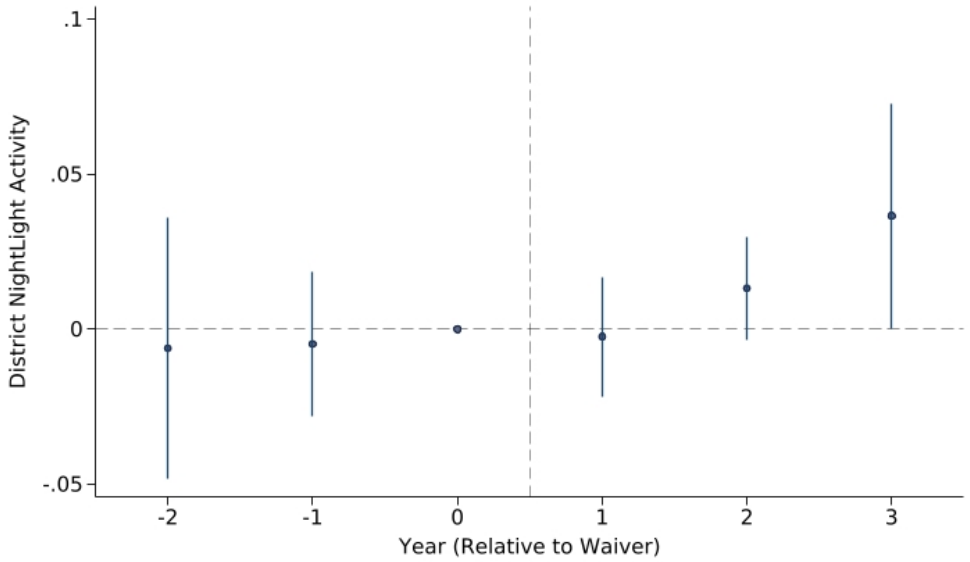


Table 1: **Summary Statistics**

This table reports borrower-level summary statistics for the baseline analysis sample. Unless otherwise indicated, transaction and borrowing variables are measured over the pre-waiver period. Monetary values are reported in Indian rupees.

	Mean	SD	p25	p50	p75	*N*
Loan Outstanding (INR)	158164.2	224561	36118	78367	180579.5	88012
Share of Agricultural Loan	0.87	0.31	1	1	1	88012
Interest Rate (%)	9.75	3.17	7	11	12.2	88012
Loan in Default	0.13	0.28	0	0	0.02	88012
$\mathbb{1}$(Waiver Beneficiary)	0.52	0.50	0	1	1	88012
Loan Waived (INR)	45970.3	54373.0	0	20371.5	96984.5	88012
$\mathbb{1}$(Cash Transaction at Bank)	0.89	0.31	1	1	1	88012
# Cash Transactions at Bank	9.15	19.50	1	3	10	88012
$\mathbb{1}$(Cash Transaction outside District)	0.23	0.42	0	0	0	88012
Share Transactions Outside District	0.07	0.18	0	0	0	88012
Total Cash Deposits (INR)	72260.2	162854.4	0	2000	65000	88012
$\mathbb{1}$(Self-Business Loan)	0.01	0.11	0	0	0	88012

Table 2: **Debt Relief Eligibility and Farm Debt Burdens: First-Stage Evidence**

This table verifies that the administrative eligibility rules generated meaningful borrower-level debt relief. I report differential post-waiver changes in farm debt outcomes for eligible borrowers relative to otherwise similar ineligible farm borrowers within one year of the farm loan waiver, estimated from:

$$y_{i,s,t} = \beta \, \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(Post_t) + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

The variable of interest is the interaction between an indicator for waiver eligibility, $\mathbb{1}(Treat_{i,s})$, and an indicator for the post-waiver period, $\mathbb{1}(Post_t)$. Column 1 uses the logarithm of the farm-loan balance. Column 2 uses the raw farm-loan balance and is estimated by Poisson pseudo-maximum likelihood. Column 3 uses an indicator for default on a farm loan. All columns include borrower fixed effects and flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01).

	(1)	(2)	(3)
	Outstanding Agri Debt		
Outcome:	**Log**	**Poisson**	**𝟙(Default)**
𝟙(Treated) × 𝟙(Post)	-1.2199***	-0.1155***	-0.0786***
	(0.0413)	(0.0137)	(0.0031)
Stack × Farmer F.E.	Yes	Yes	Yes
Branch × Loan Type × Amt Bucket × Rate Bucket × Time F.E.	Yes	Yes	Yes
Observations	165,684	154,912	165,684
R-Squared	0.767	0.924	0.764
Y-Mean	8.076	140,198	0.294
Magnitude (%)	-15.11	-10.91	-16.04

Table 3: **Debt Relief and Migration-Linked Outside-District Deposit Transactions**

This table presents borrower-level estimates of the effect of waiver eligibility on migration-linked outside-district deposit transactions. The dependent variable is an indicator equal to one if borrower i's savings account records any in-person cash deposit made at a bank branch outside the home district in year t. These deposits require physical presence at the transaction branch and exhibit spatial and temporal patterns consistent with remittances associated with partial migration; Internet Appendix Section B.1 provides detailed validation. The borrower's home district is defined as the district containing the branch that issued the borrower's pre-waiver farm loan. I estimate:

$$\mathbb{1}(OutsideDepositTxn)_{i,s,t} = \beta\, \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(Post_t) + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

$Treat_{i,s}$ is an indicator for borrowers eligible for debt relief in waiver stack s, $Post_t$ is an indicator for years after the waiver, and α_i denotes borrower fixed effects. $X_{i,t}$ denotes flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01).

	(1)	(2)	(3)	(4)
Outcome:	**𝟙(Outside District Deposit Transactions)**			
𝟙(Treated) × 𝟙(Post)	-0.0168***	-0.0144***	-0.0141***	-0.0159***
	(0.0016)	(0.0020)	(0.0026)	(0.0039)
Stack × Farmer F.E.	Yes	Yes	Yes	Yes
Stack × Time F.E.	Yes	No	No	No
Branch × Time F.E.	No	Yes	No	No
Branch × Loan Type × Time F.E.	No	No	Yes	No
Branch × Loan Type × Amt Bucket × Rate Bucket × Time F.E.	No	No	No	Yes
Observations	526,682	526,167	521,972	491,053
# Unique Farmers	87,825	87,754	87,138	82,535
R-Squared	0.453	0.462	0.471	0.516
Y-Mean	0.115	0.115	0.114	0.113
Magnitude (%)	-14.6	-12.58	-12.3	-14.06

Table 4: **Debt Relief and the Spatial Reallocation of Cash-Transaction Activity**

This table presents borrower-level estimates of the effect of waiver eligibility on the location of in-person cash-transaction activity. The estimates test whether the decline in migration-linked outside-district deposits is accompanied by greater physical transaction activity at home locations. I estimate:

$$y_{i,s,t} = \beta \, \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(Post_t) + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

The borrower's home district is defined as the district containing the branch that issued the borrower's pre-waiver farm loan. In column 1, the dependent variable is an indicator equal to one if borrower i conducts any in-person cash transaction within the home district in year t. Columns 2 and 3 use the number of cash transactions within and outside the home district, respectively. Column 4 uses the share of cash transactions conducted outside the home district. $Treat_{i,s}$ is an indicator for borrowers eligible for debt relief in waiver stack s, $Post_t$ is an indicator for years after the waiver, and α_i denotes borrower fixed effects. $X_{i,t}$ denotes flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01).

	(1)	(2)	(3)	(4)
Outcome:	**𝟙(Txn Within District)**	**#(Txn Within District)**	**#(Txn Outside District)**	**Share(Txn Outside District)**
𝟙(Treated) × 𝟙(Post)	0.0729***	0.1344**	-0.0601**	-0.0088**
	(0.0062)	(0.0606)	(0.0234)	(0.0035)
Stack × Farmer F.E.	Yes	Yes	Yes	Yes
S × B × LT × AB × RB × T F.E.	No	Yes	No	Yes
Observations	491,053	491,053	491,053	265,725
# Unique Farmers	82,535	82,535	82,535	64,012
R-Squared	0.502	0.639	0.608	0.627
Y-Mean	0.601	2.418	0.371	0.088
Magnitude (%)	12.14	5.56	-16.2	-9.97

Table 5: **Debt Relief and Migration-Linked Deposits: Heterogeneity by Pre-Waiver Exposure to Metropolitan and Industrial Destinations**

This table examines whether the effect of waiver eligibility on migration-linked outside-district deposits varies with borrowers' pre-waiver exposure to metropolitan areas and industrial destinations. I estimate:

$$y_{i,s,t} = \beta_1 \, \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(Post_t) + \beta_2 \, \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(Post_t) \times Z_i + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

The dependent variable is an indicator equal to one if borrower i's savings account records any in-person cash deposit made outside the home district in year t. The borrower's home district is defined as the district containing the branch that issued the borrower's pre-waiver farm loan. Z_i measures the pre-waiver share of the borrower's cash transactions made at branches located in a metropolitan area or industrial city. $Treat_{i,s}$ is an indicator for borrowers eligible for debt relief in waiver stack s, $Post_t$ is an indicator for years after the waiver, and α_i denotes borrower fixed effects. The double interaction of Z_i and $Post_t$ is also estimated, but is not reported for brevity. $X_{i,t}$ denotes flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01).

	(1)	(2)	(3)	(4)
Outcome:	**𝟙(Outside District Activity)**			
	Industry Hub		**Metropolitan Area**	
𝟙(Treated) × 𝟙(Post)	-0.0150***	-0.0092**	-0.0024	-0.0024
	(0.0017)	(0.0043)	(0.0041)	(0.0041)
𝟙(Treated) × 𝟙(Post) × Share(Pre-period Dep)	-0.0120***	-0.0275***	-0.0263***	-0.0263***
	(0.0041)	(0.0093)	(0.0069)	(0.0069)
Stack × Farmer F.E.	Yes	Yes	Yes	Yes
Stack × Time F.E.	Yes	No	Yes	No
Branch × Loan Type × Amt Bucket × Rate Bucket × Time F.E.	No	Yes	No	Yes
Observations	494,946	459,796	459,796	459,796
# Unique Farmers	81,528	76,331	76,331	76,331
R-Squared	0.452	0.519	0.528	0.528
Y-Mean	0.118	0.117	0.117	0.117
Magnitude - Main (%)	-12.67	-7.92	-2.1	-2.1
Magnitude - Main + Interaction (%)	-21.3	-31.7	-22.4	-23.6

Table 6: **Debt Relief and Change in Household Borrowing**

This table presents borrower-level estimates of the effect of waiver eligibility on new borrowing by loan type. I estimate:

$$y_{i,s,t} = \beta \, \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(Post_t) + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

Each dependent variable is an indicator equal to one if borrower i receives a new loan in the category reported in the corresponding column in year t. The categories distinguish farm credit from personal, vehicle, housing, and self-employment and small-business credit. Self-employment and small-business categories include credit to self-employed professionals, retail traders, microenterprises, entrepreneurs, and employment-generation programs. $Treat_{i,s}$ is an indicator for borrowers eligible for debt relief in waiver stack s, $Post_t$ is an indicator for years after the waiver, and α_i denotes borrower fixed effects. $X_{i,t}$ denotes flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01).

	(1)	(2)	(3)	(4)	(5)
Outcome:	**𝟙(Agri)**	**𝟙(Personal)**	**𝟙(Auto)**	**𝟙(Home)**	**𝟙(Self-Emp or Small Business)**
𝟙(Treated) × 𝟙(Post)	-0.0281***	0.0280***	-0.0008	0.0001	0.0012**
	(0.0053)	(0.0040)	(0.0006)	(0.0008)	(0.0006)
Stack × Farmer F.E.	Yes	Yes	Yes	Yes	Yes
Stack × Branch × Loan Type × Amt Bucket × Rate Bucket × Time F.E.	No	Yes	No	Yes	Yes
Observations	491,053	491,053	491,053	491,053	491,053
# Unique Farmers	82,535	82,535	82,535	82,535	82,535
R-Squared	0.556	0.443	0.307	0.329	0.326
Y-Mean	0.418	0.083	0.003	0.003	0.003
Magnitude (%)	-6.72	33.61	-27.03	4.34	40.19

Table 7: **Debt Relief and Small Business Borrowing: Heterogeneity by Pre-Waiver Borrower Characteristics**

This table examines whether the effect of waiver eligibility on households' uptake of new small business credit varies with their pre-waiver characteristics. I estimate:

$$y_{i,s,t} = \beta_1 \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(Post_t) + \beta_2 \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(Post_t) \times Z_i + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

The dependent variable is an indicator equal to one if borrower i receives a new small-business credit in year t. Z_i measures the pre-waiver characteristics of the borrower, with each column representing a separate characteristic as stated: share of cash deposits from branches in industrial hubs in column 1, eligible loan relief amount as a share of total outstanding in column 2, percentile rank of total cash deposits in column 3, and an indicator for whether any outstanding loan account was delinquent in column 4. $Treat_{i,s}$ is an indicator for borrowers eligible for debt relief in waiver stack s, $Post_t$ is an indicator for years after the waiver, and α_i denotes borrower fixed effects. The double interaction of Z_i and $Post_t$ is also estimated, but is not reported for brevity. $X_{i,t}$ denotes flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01).

	(1)	(2)	(3)	(4)
Outcome:	**𝟙(Small Business Credit)**			
	Share of Deposits From Industrial Hubs	**Share of Outstanding Loan Relieved**	**Pct(Total Deposits)**	**𝟙(Default)**
𝟙(Treated) × 𝟙(Post)	0.0003	-0.0007	0.0010**	0.0008**
	(0.0004)	(0.0005)	(0.0004)	(0.0004)
𝟙(Treated) × 𝟙(Post) × Pre-waiver Characteristics	0.0020**	0.0020***	0.0000	-0.0005
	(0.0010)	(0.0005)	(0.0000)	(0.0006)
Stack × Borrower F.E.	Yes	Yes	Yes	Yes
Branch × Loan Type × Amt Bucket × Rate Bucket × Time F.E.	Yes	Yes	Yes	Yes
Observations	459,796	478,471	483,790	491,053
# Unique Farmers	76,331	78,992	79,955	82,535
R-Squared	0.355	0.355	0.353	0.355
Y-Mean	0.001	0.001	0.001	0.001
Magnitude - Main (%)	18.32	-45.35	67.31	53.69
Magnitude - Main + Interaction (%)	155.6	94.4	66.9	17.69

Table 8: **Debt Relief and Registered Non-Farm Business Formation**

This table presents borrower-level estimates of the effect of waiver eligibility on registered non-farm business formation over the five years before and after the waiver. I estimate:

$$y_{i,s,t} = \beta \, \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(Post_t) + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

The dependent variable is an indicator equal to one if borrower i is linked to a business newly registered in year t in either the incorporated-firm or MSME registry. I link borrowers to registration records using borrower, director, business-name, and address information. Because businesses may remain informal, register at another address, or be registered under another household member, the matched outcome is a conservative measure of total entrepreneurial entry. $Treat_{i,s}$ is an indicator for borrowers eligible for debt relief in waiver stack s, $Post_t$ is an indicator for years after the waiver, and α_i denotes borrower fixed effects. $X_{i,t}$ denotes flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01).

	(1)	(2)	(3)	(4)
Outcome:	**𝟙(Registered Small Business)**			
𝟙(Treated) × 𝟙(Post)	0.0004**	0.0003**	0.0005*	0.0005*
	(0.0002)	(0.0001)	(0.0003)	(0.0003)
Stack × Farmer F.E.	Yes	Yes	Yes	Yes
Stack × Time F.E.	Yes	No	No	No
Stack × Branch × Time F.E.	No	Yes	No	No
Stack × Branch × Loan Type × Time F.E.	No	No	Yes	No
Stack × Branch × LT × AB × RB × Time F.E.	No	No	No	Yes
Observations	968,132	967,351	960,575	909,865
# Unique Farmers	88,012	87,941	87,325	82,715
R-Squared	0.526	0.54	0.549	0.59
Magnitude (%)	184.83	145.99	220.76	248.5

Table 9: **Debt Relief Exposure and District-Level Business Registrations**

This table presents district-level estimates relating local exposure to waiver eligibility to new business registrations over the three years before and after the waiver announcement. I estimate:

$$y_{d,t} = \beta \, TreatedDistrict_d \times \mathbb{1}(Post_t) + \alpha_d + \lambda_t + \varepsilon_{d,t}.$$

The dependent variables measure new business registrations in district d in year t, as reported in the corresponding columns. District exposure is constructed from the bank sample using the share of farm borrowers in the district who are eligible for relief. $TreatedDistrict_d$ identifies districts with an above-median or top-tercile eligible-borrower share within the state, as indicated by the column. $Post_t$ denotes years after the waiver, while α_d and λ_t denote district and year fixed effects. Standard errors are clustered at the district level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01). The estimates are interpreted as complementary evidence on broader local outcomes rather than as borrower-level causal effects or full equilibrium effects.

	(1)	(2)
Outcome:	**New Business Formation (Poisson)**	
	Above-Median	Top-Tercile
$\mathbb{1}$(Treated District) × $\mathbb{1}$(Post)	0.5203***	0.3333***
	(0.1009)	(0.0613)
District F.E.	Yes	Yes
Time F.E.	Yes	Yes
Observations	240	168
# Unique Districts	40	28

Table 10: **Debt Relief Exposure and District-Level Nighttime Lights**

This table presents district-level estimates relating local exposure to waiver eligibility to nighttime-light activity, measured using VIIRS data from 2012 to 2018. I estimate:

$$y_{d,t} = \beta \, \mathbb{1}(TreatedDistrict_d) \times \mathbb{1}(Post_t) + \alpha_d + \lambda_t + \varepsilon_{d,t}.$$

The dependent variable is the logarithm of average nighttime-light intensity in district d in year t. District exposure is constructed from the bank sample using the share of farm borrowers in the district who are eligible for relief. In column 1, $TreatedDistrict_d$ identifies districts with an above-median eligible-borrower share within the state; in column 2, it identifies districts in the top tercile. $Post_t$ denotes years after the waiver, while α_d and λ_t denote district and year fixed effects. Standard errors are clustered at the district level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01). The estimates are interpreted as complementary evidence on broader local outcomes rather than as borrower-level causal effects or full equilibrium effects.

	(1)	(2)
Outcome:	**Nightlight**	
	Above-Median	Top-Tercile
𝟙(Treated District) x 𝟙(Post)	0.0257**	0.0395**
	(0.0118)	(0.0147)
District F.E.	Yes	Yes
Time F.E.	Yes	Yes
Observations	161	105
# Unique Districts	23	15
R-sqaured	0.998	0.999

Table 11: **Debt Relief and the Performance of Self-Employment and Small-Business Loans**

This table examines whether self-employment and small-business credit held by waiver-eligible borrowers performs differently after the waiver from comparable credit held by ineligible farm borrowers. I estimate:

$$y_{i,s,t} = \beta \, \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(Post_t) + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

The coefficient β captures the differential post-waiver change in default outcomes for eligible borrowers. Columns 1 and 2 use an indicator for default on a self-employment or small-business loan. Columns 3 and 4 use the share of the borrower's outstanding balance in these loan categories that is in default. Columns 1 and 3 include borrower and time fixed effects. Columns 2 and 4 add flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01).

	(1)	(2)	(3)	(4)
Outcome:	**Default on Self-Employment Loan**			
	𝟙(Default)		*Frac Loan in Default*	
𝟙(Treated) × 𝟙(Post)	-0.0008***	-0.0001	-0.0009***	-0.0005**
	(0.0003)	(0.0005)	(0.0002)	(0.0002)
Stack × Farmer F.E.	Yes	Yes	Yes	Yes
Stack × Time F.E.	Yes	No	Yes	No
Branch × Loan Type × Amt Bucket × Rate Bucket × Time F.E.	No	Yes	No	Yes
Observations	526,682	491,053	526,682	491,053
# Unique Farmers	87,825	82,535	87,825	82,535
R-Squared	0.519	0.528	0.463	0.533

Internet Appendix

A.1 Additional Tests and Robustness

Table A.1: **Alternative Measures of Distant Cash Transactions**

This table reports robustness estimates for Table 3 using alternative definitions of distant cash transaction activity. I estimate:

$$y_{i,s,t} = \beta \, \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(Post_t) + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

The dependent variables measure in-person cash transactions outside the borrower's home branch and outside the home state. These outcomes test whether the main district-based result is specific to the chosen geographic boundary or reflects a broader reduction in away-location banking activity. $Treat_{i,s}$ is an indicator for borrowers eligible for debt relief in waiver stack s, $Post_t$ is an indicator for years after the waiver, and α_i denotes borrower fixed effects. $X_{i,t}$ denotes flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01).

	(1)	(2)	(3)	(4)
Outcome:	**𝟙(Txn Outside Home)**		**𝟙(Txn Outside State)**	
𝟙(Treated) × 𝟙(Post)	-0.0128***	-0.0137***	-0.0124***	-0.0093***
	(0.0018)	(0.0043)	(0.0011)	(0.0029)
Stack × Farmer F.E.	Yes	Yes	Yes	Yes
Stack × Time F.E.	Yes	No	Yes	No
Stack × Branch × LT × AB × RB × Time F.E.	No	Yes	No	Yes
Observations	526,682	491,053	526,682	491,053
# Unique Farmers	87,825	82,535	87,825	82,535
R-Squared	0.487	0.548	0.404	0.466
Y-Mean	0.151	0.149	0.058	0.059
Magnitude (%)	-8.49	-9.24	-21.29	-15.77

Table A.2: **Debt Relief and Spatial Reallocation of Cash Deposits: Extensive and Intensive Margins**

This table decomposes changes in the location of borrowers' cash deposits into extensive and intensive margins. I estimate:

$$y_{i,s,t} = \beta \, \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(Post_t) + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

Columns 1 and 2 examine deposits outside the borrower's home district. Column 1 uses an indicator for any outside-district cash deposit in year t; column 2 uses the logarithm of outside-district deposit value, conditional on a positive outside-district deposit. Columns 3 and 4 analogously examine deposits within the home district. The borrower's home district is defined as the district containing the branch that issued the borrower's pre-waiver farm loan. $Treat_{i,s}$ is an indicator for borrowers eligible for debt relief in waiver stack s, $Post_t$ is an indicator for years after the waiver, and α_i denotes borrower fixed effects. $X_{i,t}$ denotes flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01).

	(1)	(2)	(3)	(4)
	Outside District		**Within District**	
Outcome:	**𝟙(Txn)**	**Log(Amount) Condl.**	**𝟙(Txn)**	**Log(Amount) Condl.**
𝟙(Treated) × 𝟙(Post)	-0.0164***	-0.0096	0.0621***	-0.2880***
	(0.0038)	(0.1084)	(0.0060)	(0.0575)
Stack × Farmer F.E.	Yes	Yes	Yes	Yes
Stack × Branch × LT × AB × RB × Time F.E.	Yes	Yes	Yes	Yes
Observations	491,053	23,818	491,053	103,142
# Unique Farmers	82,535	7,990	82,535	33,347
R-Squared	0.512	0.724	0.443	0.693
Y-Mean	0.112	9.827	0.309	9.219
Magnitude (%)	-14.63	-0.1	20.08	-3.12

Table A.3: **Debt Relief and Outside-District Transaction Values**

This table reports robustness estimates using the value of outside-district cash transactions and the share of total cash-transaction value occurring outside the home district. I estimate:

$$y_{i,s,t} = \beta \, \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(Post_t) + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

The dependent variables are reported in the corresponding columns and measure the value or share of in-person cash activity outside the borrower's home district. These outcomes complement the indicator-based migration proxy by testing whether waiver eligibility also changes the intensity of away-location banking activity. $Treat_{i,s}$ is an indicator for borrowers eligible for debt relief in waiver stack s, $Post_t$ is an indicator for years after the waiver, and α_i denotes borrower fixed effects. $X_{i,t}$ denotes flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01).

	(1)	(2)	(3)	(4)
Outcome:	**Outside District Txn (INR)**		**Share(Outside District Txn - INR)**	
	Reghdfe	*Poisson*	*Reghdfe*	*Poisson*
𝟙(Treated) × 𝟙(Post)	-0.1627***	-0.1387**	-0.0073**	-0.1178*
	(0.0381)	(0.0665)	(0.0035)	(0.0656)
Stack × Farmer F.E.	Yes	Yes	Yes	Yes
Stack × Branch × LT × AB × RB × Time F.E.	Yes	Yes	Yes	Yes
Observations	491,053	101,881	265,725	81,342
# Unique Farmers	82,535		64,012	
R-Squared	0.534		0.599	
Y-Mean	1.113	21559.05	0.076	0.205
Magnitude (%)	-14.62		-9.64	

Table A.4: **Debt Relief and Self-Employment and Small-Business Credit**

This table presents borrower-level estimates of the effect of waiver eligibility on being self-employed or operating a small-business. I estimate:

$$y_{i,s,t} = \beta \, \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(Post_t) + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

The dependent variable is an indicator equal to one if borrower i has an outstanding loan in a self-employment or small-business category in year t. These categories include credit to self-employed professionals, retail traders, microenterprises, entrepreneurs, and employment-generation programs. $Treat_{i,s}$ is an indicator for borrowers eligible for debt relief in waiver stack s, $Post_t$ is an indicator for years after the waiver, and α_i denotes borrower fixed effects. $X_{i,t}$ denotes flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01).

	(1)	(2)	(3)	(4)
Outcome:	**𝟙(Self-Employment Loan)**			
𝟙(Treated) × 𝟙(Post)	0.0014***	0.0013***	0.0011***	0.0012**
	(0.0003)	(0.0003)	(0.0004)	(0.0006)
Stack × Farmer F.E.	Yes	Yes	Yes	Yes
Stack × Time F.E.	Yes	No	No	No
Stack × Branch × Time F.E.	No	Yes	No	No
Stack × Branch × Loan Type × Time F.E.	No	No	Yes	No
Stack × Branch × Loan Type × Amt Bucket × Rate Bucket × Time F.E.	No	No	No	Yes
Observations	404,893	404,370	400,477	372,969
# Unique Farmers	85,551	85,474	84,829	80,204
R-Squared	0.223	0.248	0.291	0.41
Y-Mean	0.002	0.002	0.002	0.002
Magnitude (%)	82.15	80.76	65.08	75.79

Table A.5: **Non-Farm Borrowing by Loan-Origination Location**

This table decomposes the new non-farm borrowing results in Table 6 by the location at which the new loan is originated. I estimate:

$$y_{i,s,t} = \beta \, \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(Post_t) + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

The dependent variables indicate whether borrower i receives a new personal loan or a new self-employment or small-business loan in year t, separately for loans originated at the borrower's home branch and at other branches. The home branch is the branch that issued the borrower's pre-waiver farm loan. This decomposition tests whether the increase in non-farm credit is concentrated near the borrower's home location. $Treat_{i,s}$ is an indicator for borrowers eligible for debt relief in waiver stack s, $Post_t$ is an indicator for years after the waiver, and α_i denotes borrower fixed effects. $X_{i,t}$ denotes flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01).

	(1)	(2)	(3)	(4)
Outcome:	**𝟙(New Debt Issued)**			
	Personal		**Self-Business**	
	Home	**Away**	**Home**	**Away**
𝟙(Treated) × 𝟙(Post)	0.0274***	0.001	0.0006**	-0.0001
	(0.0038)	(0.0008)	(0.0003)	(0.0001)
Stack × Farmer F.E.	Yes	Yes	Yes	Yes
Branch × Loan Type × Amt Bucket × Rate Bucket × Time F.E.	Yes	Yes	Yes	Yes
Observations	491,053	491,053	491,053	491,053
# Unique Farmers	82,535	82,535	82,535	82,535
R-Squared	0.443	0.417	0.356	0.229
Y-Mean	0.079	0.003	0.001	0
Magnitude (%)	34.88	35.05	47.6	-235.26

Table A.6: **Cumulative Cash Deposits and Withdrawals**

This table presents borrower-level estimates of the effect of waiver eligibility on cumulative cash deposits and withdrawals in savings accounts. I estimate:

$$y_{i,s,t} = \beta \, \mathbb{1}(Treat_{i,s}) \times \mathbb{1}(Post_t) + \alpha_i + X_{i,t} + \varepsilon_{i,s,t}.$$

The top panel reports estimates for cumulative cash deposits. Columns 1–4 use alternative transformations: the logarithm of deposits, the logarithm of one plus deposits, the inverse hyperbolic sine of deposits, and a Poisson pseudo-maximum likelihood specification using the raw deposit value. The bottom panel reports analogous estimates for cumulative cash withdrawals. These cash-flow measures combine earnings, remittances, liquidity needs, and banking behavior and should not be interpreted as direct measures of household income. $Treat_{i,s}$ is an indicator for borrowers eligible for debt relief in waiver stack s, $Post_t$ is an indicator for years after the waiver, and α_i denotes borrower fixed effects. $X_{i,t}$ denotes flexible time fixed effects interacted with pre-waiver lending characteristics: the branch that issued the borrower's pre-waiver farm loan, the loan product, and quintile bins of the amount and interest rate on that loan. Standard errors are clustered at the borrower level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01).

	(1)	(2)	(3)	(4)
Outcome:	**Cumulative Deposits**			
	Log(Deposit)	**Log(1+Deposit)**	**Pct(Deposit)**	**Deposit (Poisson)**
𝟙(Treated) × 𝟙(Post)	-0.0365*	0.0145	-0.1288	-0.0693***
	(0.0213)	(0.0444)	(0.3048)	(0.0157)
Observations	328,394	491,053	491,053	387,981
# Unique Employees	63,201	82,535	82,535	66,097
R-Squared	0.921	0.863	0.862	
Y-Mean	9.403	5.653	40.643	70664.57
Magnitude (%)	-0.39	0.26	-0.32	
Outcome:	**Cumulative Withdrawals**			
	Log(Withdrawal)	**Log(1+Withdrawal)**	**Pct(Withdrawal)**	**Withdrawal (Poisson)**
𝟙(Treated) × 𝟙(Post)	0.0155	-0.2323***	-0.2848	-0.0305***
	(0.0143)	(0.0503)	(0.3145)	(0.0117)
Stack × Farmer F.E.	Yes	Yes	Yes	Yes
Stack × Branch × LT × AB × RB × Time F.E.	Yes	Yes	Yes	Yes
Observations	343,859	491,053	491,053	419,261
# Unique Employees	67,932	82,535	82,535	71,164
R-Squared	0.911	0.824	0.849	
Y-Mean	10.839	6.577	41.622	85396.44
Magnitude (%)	0.14	-3.53	-0.68	

B.1 Migration as a Household Financial Strategy

Two conceptually distinct views of migration coexist in the development literature. The first, associated with Harris and Todaro (1970) and subsequent spatial-equilibrium models, treats migration as an individual decision to permanently relocate in response to persistent differences in expected earnings or amenities across locations. The second view instead treats migration as a collective household decision in which selected members work elsewhere while the household continues to make production, consumption, and financing decisions jointly, precisely because incomplete local credit and insurance markets make full separation costly (Stark and Bloom, 1985). Migrant earnings therefore remain tied to the origin household through remittance arrangements involving insurance, exchange, and repayment (Lucas and Stark, 1985). This partial, often temporary, migration helps households smooth consumption and insure against local risk (Rosenzweig and Stark, 1989; Munshi and Rosenzweig, 2016; Morten, 2019).

Partial migration is especially relevant for rural agricultural households. Because farming and other household responsibilities keep some labor tied to the origin, while farm income is low and exposed to local shocks, households often diversify by allocating selected members to wage work elsewhere (Deshingkar and Akter, 2009; Morten, 2019). Rather than abandoning agriculture, households combine farming at home with migrant labor income, so migration access can affect agricultural investment and production as well as consumption (Morten, 2019). This arrangement is widespread in India. The 2007–08 National Sample Survey reports that approximately 19% of rural households have at least one out-migrant and that 58% of migrants residing in urban areas originate in rural households (National Sample Survey Office, 2010). In the village panel studied by Morten (2019), approximately one in five households reports a migrant.

For indebted agricultural households, this allocation can also be shaped by repayment pressure. Evidence from rural India links temporary labor migration to debt, labor advances, and obligations that cannot be met from agricultural income alone (Mosse et al., 2002; Deshingkar and Start, 2003). Migrant earnings can therefore serve not only to diversify income but also to provide liquidity for consumption and debt service at the origin. Wage differentials remain relevant, but are not sufficient to characterize the migration decision: some households send members away because their local production income cannot cover current financial commitments.

This distinction matters because migration carries costs beyond the direct expenses of travel, job search, and maintaining a second residence. Migrants are often employed in informal and precarious jobs, live in poor conditions, work long hours, and lack social protection (Breman, 1996; Imbert and Papp, 2020a). For agricultural households, separation may also disrupt the allocation of labor and care at the origin, since fewer members remain to cultivate land, manage the household, and respond to local shocks. Liquidity can therefore have opposing effects on migration. When households cannot finance the cost of moving, additional cash or credit can enable migration (Bryan et al., 2014; Angelucci, 2015; Cai, 2020). But when financial burdens necessitate migration and the costs of separation are large, an improvement in the household's financial position can reduce the need to send members away (Imbert and Papp, 2020a; Lagakos et al., 2023).

B.1.1 Identifying Migration-Linked Remittance in Bank Data

The bank data provide a way to observe the financial footprint of partial migration. They record the branch location of in-person cash deposits and withdrawals. Because these transactions require physical presence, their geography reveals where account activity occurs. This feature is especially informative in the sample period, when migrant workers were commonly paid in cash and bank deposits were an important formal channel for sending earnings to households at the origin (Oliver and Radcliffe, 2011).

For an agricultural household, the relevant pattern is straightforward. A member working elsewhere can deposit cash earnings into the household's account at a nearby branch, while members remaining at the origin withdraw those funds locally to finance consumption, cultivation, and debt repayment. The account therefore remains anchored to the household's home even though some deposits occur near the migrant's place of work.

In line with this interpretation, I use cash deposits made outside the borrower's home district as a proxy for migration-linked remittance activity. The measure does not directly identify the depositor, the depositor's employment, or the purpose of the transaction. Its validity instead rests on whether outside-district activity exhibits the spatial and temporal patterns implied by partial migration. I document five such patterns: outside-district deposits are economically relevant for a subset of farm borrowers, concentrated in a single primary destination, accompanied by withdrawals at home, short in duration, and recurrent in timing across years. No single pattern conclusively implies partial migration, but together they are difficult to reconcile with incidental travel, diffuse banking activity, or permanent relocation. I describe each piece of evidence below.

Figure B.1 begins by showing the extent and geography of outside-district deposits. Panel A shows that approximately 70% of borrowers receive no deposits from outside their home district. Among the remaining borrowers, however, outside activity can be substantial: for about 12% (8%) of borrowers, more than half (three-quarters) of total deposit value comes from outside the home district. The measure therefore isolates a non-trivial subset of farm borrowers with economically meaningful financial activity away from home, consistent with survey evidence suggesting one in every five households relies on remittances from a migrant worker.

Panel B of Figure B.1 shows that this activity is highly concentrated geographically. Among borrowers with any outside-district deposits, more than four-fifths of outside deposit value comes from a *single* primary outside district. Figure B.2 provides complementary evidence that these deposits function as remittances. The figure plots home-district withdrawals around an outside-district deposit. Home-withdrawal incidence almost doubles in the exact month when the borrower account receives an outside-district deposit: the likelihood of cash withdrawal at the home district rises to about 30% from 15–17% in the months before the deposit, and subsequently declines toward its earlier level. This synchronized response is consistent with funds deposited outside the district being accessed by household members at home.

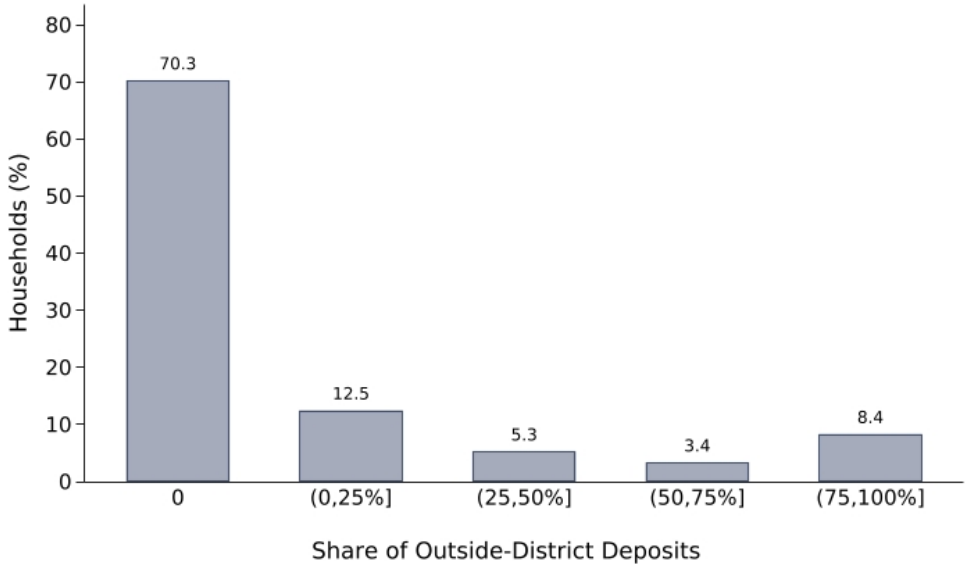
The duration and recurrent nature of outside-district remittance patterns suggest that the migration is temporary rather than permanent. Figure B.3 reports the duration of outside-district

deposit spells. A spell is defined as a cluster of months containing outside-district deposits, allowing for up to two inactive months within the episode to accommodate intermittent banking. The spells are predominantly short. Approximately 66% last one to two months, and another 20% last three to six months. In total, 86.0% last six months or less, while only 5.3% extend beyond one year. Persistent relocation would be more likely to generate outside-district activity throughout the year and over longer horizons. The observed duration is more consistent with temporary or circular migration-linked activity than with permanent relocation.

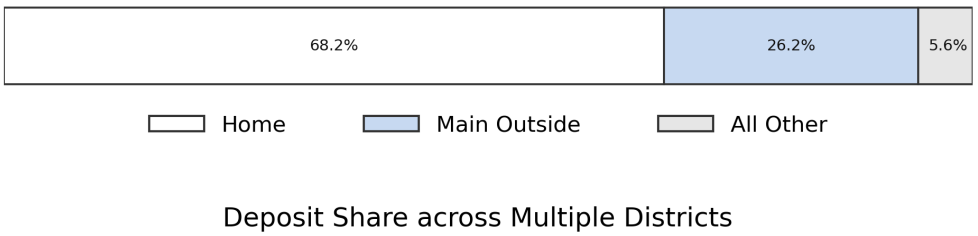
Figure B.4 examines whether these remittance episodes recur at similar times across years. For each borrower-year, I identify the first calendar month with an outside-district deposit and compare this month across adjacent active years. The figure reports whether timing recurs in at least half of adjacent-year pairs and compares the observed rate with a simulated benchmark that randomly assigns starting months while preserving each borrower's number of active years. The timing of outside activity is substantially more regular than under the benchmark. In the data, about 14% of borrowers begin an episode in exactly the same calendar month across years, compared with 6% under random timing. Within a one-month window, the observed recurrence rate is 41.3%, compared with 25.6% under the benchmark. Outside-district deposits therefore occur in repeated, regularly timed episodes rather than at arbitrary points in the year. Taken together, I interpret outside-district deposits as a proxy for remittances associated with temporary or partial migration. The measure does not directly observe migration status, employment, or return to the origin household.

Figure B.1: **Distribution and Geographic Concentration of Outside-District Deposits**

This figure presents descriptive evidence on outside-district cash deposits in the bank data. Panel A shows the distribution of the share of deposit value received from branches outside the borrower's home district. The x-axis groups borrowers by this share, and the y-axis reports the fraction of borrowers in each bin. Panel B examines geographic concentration among borrowers with any outside-district deposits. The bar decomposes total deposit value into deposits received in the home district, deposits received from the borrower's primary outside district, and deposits received from all other outside districts.

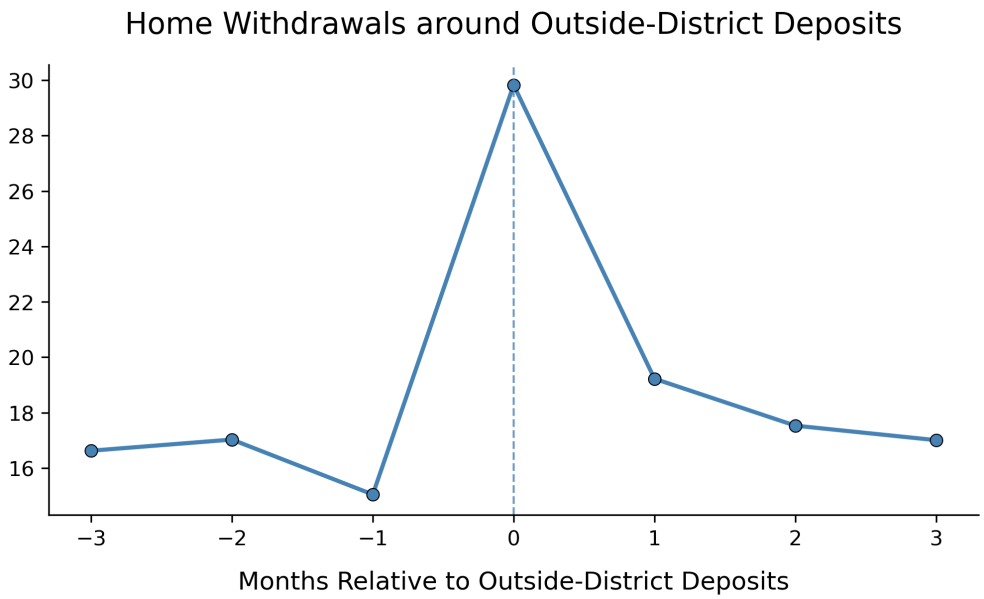


Panel A



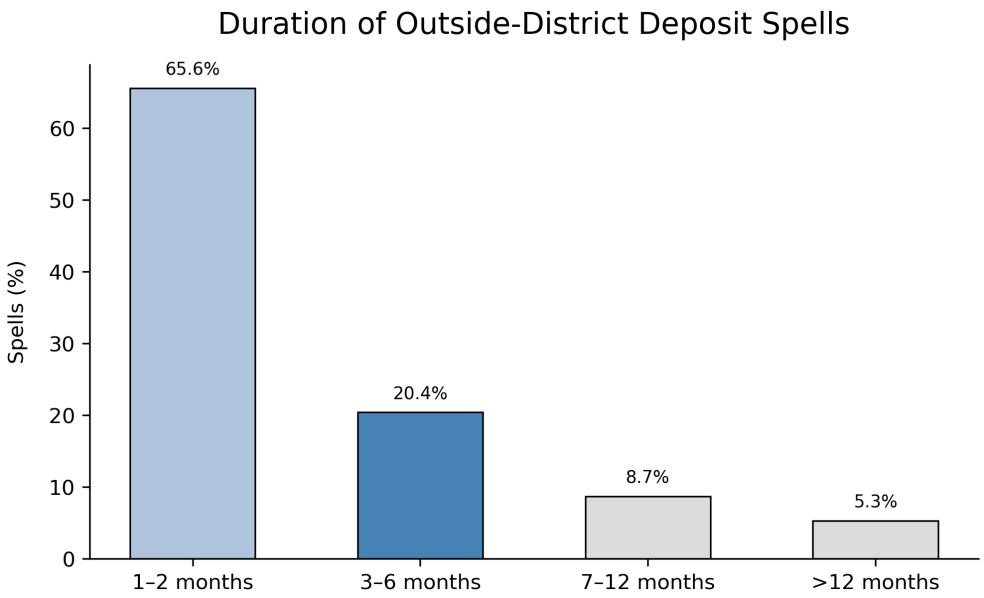
Panel B

Figure B.2: Home-District Withdrawals Around Outside-District Deposit Episodes



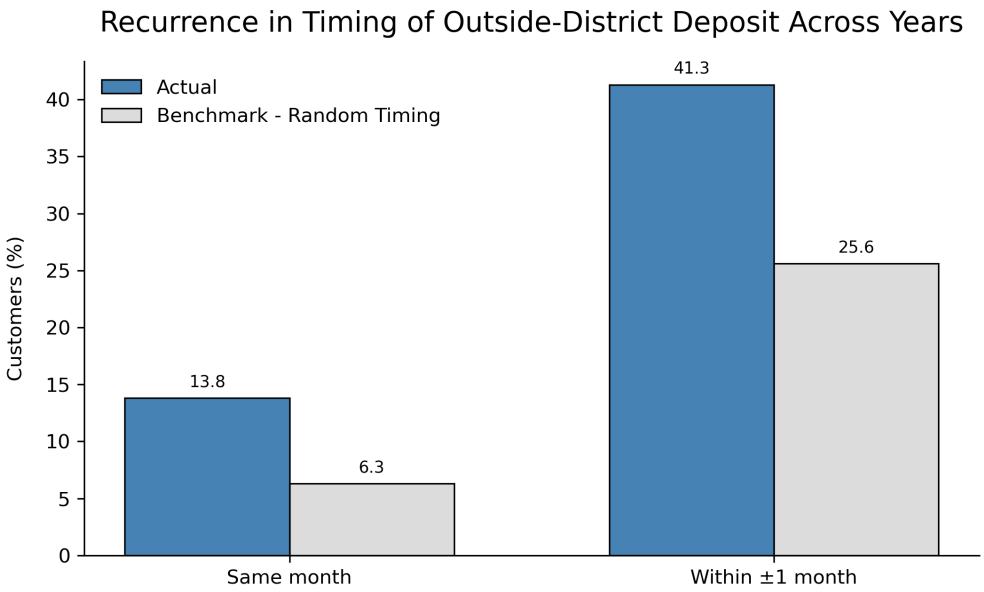
Notes: This figure plots the share of borrower-months with a home-district withdrawal around the start of an outside-district deposit episode. Month 0 is the first month of the episode. The contemporaneous rise in home-district withdrawals is consistent with funds deposited outside the home district being accessed by household members remaining at home.

Figure B.3: Duration of Outside-District Deposit Episodes



Notes: This figure reports the distribution of outside-district deposit-episode durations. An episode is defined as a cluster of months containing outside-district deposits, allowing inactive months within an episode to accommodate intermittent banking. Borrowers with only one active outside-district deposit month over the full sample are excluded. Most episodes are short: 86.0% last six months or less. The duration pattern is more consistent with temporary or circular migration-linked activity than with permanent relocation.

Figure B.4: Recurrence in the Timing of Outside-District Deposits Across Years



Notes: This figure reports borrower-level recurrence in the timing of outside-district deposit activity. For each borrower-year, I identify the first month with an outside-district deposit. I compare this month across adjacent active years and record whether timing recurs in at least half of adjacent-year pairs. The benchmark randomly assigns deposit timing across calendar months while preserving each borrower's number of active years.

C.1 Regression-Discontinuity Evidence at the Sanction-Date Cutoff

The main analysis identifies waiver effects from a difference-in-differences design that exploits within-branch variation in eligibility across otherwise comparable borrowers. The eligibility is based on (i) loan type and (ii) the date the loan was sanctioned. This appendix complements the main results with a sharper regression-discontinuity design around just the loan sanction-date cutoffs that determined waiver eligibility: December 31, 2013 in Andhra Pradesh and March 31, 2014 in Telangana. Loans sanctioned on or before the cutoff date were eligible for debt relief; loans sanctioned after were not. This design compares borrowers with sanction dates just before and just after the cutoff, holding fixed the borrower profile, the branch, and the loan category, and isolates variation in eligibility that is orthogonal to borrower or lending characteristics.

Validity of the cutoff: A first requirement is that borrowers, branches, or loan officers did not manipulate the timing of loans around the cutoff to influence eligibility. Because the cutoff dates were announced only in mid-2014, several months after the sanction-date thresholds had passed, ex post manipulation is not mechanically feasible. As a formal test, the top panel of Figure C.1 plots the density of first loans sanctioned within borrower-branch-loan-category cells around the eligibility cutoff. A McCrary density test does not reject continuity at the cutoff at conventional levels, with a p-value above 0.27. The observed density is somewhat lower immediately after the threshold, but this reflects the fact that both cutoff dates coincide with administrative calendar-year and fiscal-year boundaries in India, which mechanically affect loan sanctioning activity in the days immediately following.

Because the sanction-date cutoff varies at the loan level and eligibility is determined at the borrower level, I collapse the borrower-year panel used in the main difference-in-differences analysis to a single observation per borrower. For migration-linked activity, the borrower-level outcome is the post-versus-pre change in the share of the borrower's savings-account deposits made at branches outside the home district. For small-business credit, the outcome is an indicator equal to one if the borrower takes any self-employment or small-business loan after the waiver announcement. Both outcomes are then evaluated as a function of the borrower's loan sanction date relative to the state-specific cutoff.

Migration-linked deposits: The bottom-left panel of Figure C.1 presents the regression-discontinuity plot for the change in migration-linked deposit activity. I aggregate borrowers into six-week windows of sanction date around the eligibility cutoff and fit a linear polynomial in the running variable (days since cutoff) on each side. Borrowers just eligible for relief exhibit a sharp decline in the share of outside-district deposits relative to borrowers just ineligible, mirroring the main difference-in-differences estimates on migration-linked deposits. The discontinuity at the cutoff is consistent with the interpretation that debt relief, rather than a differential trend in borrower or branch characteristics, drives the decline in migration-linked

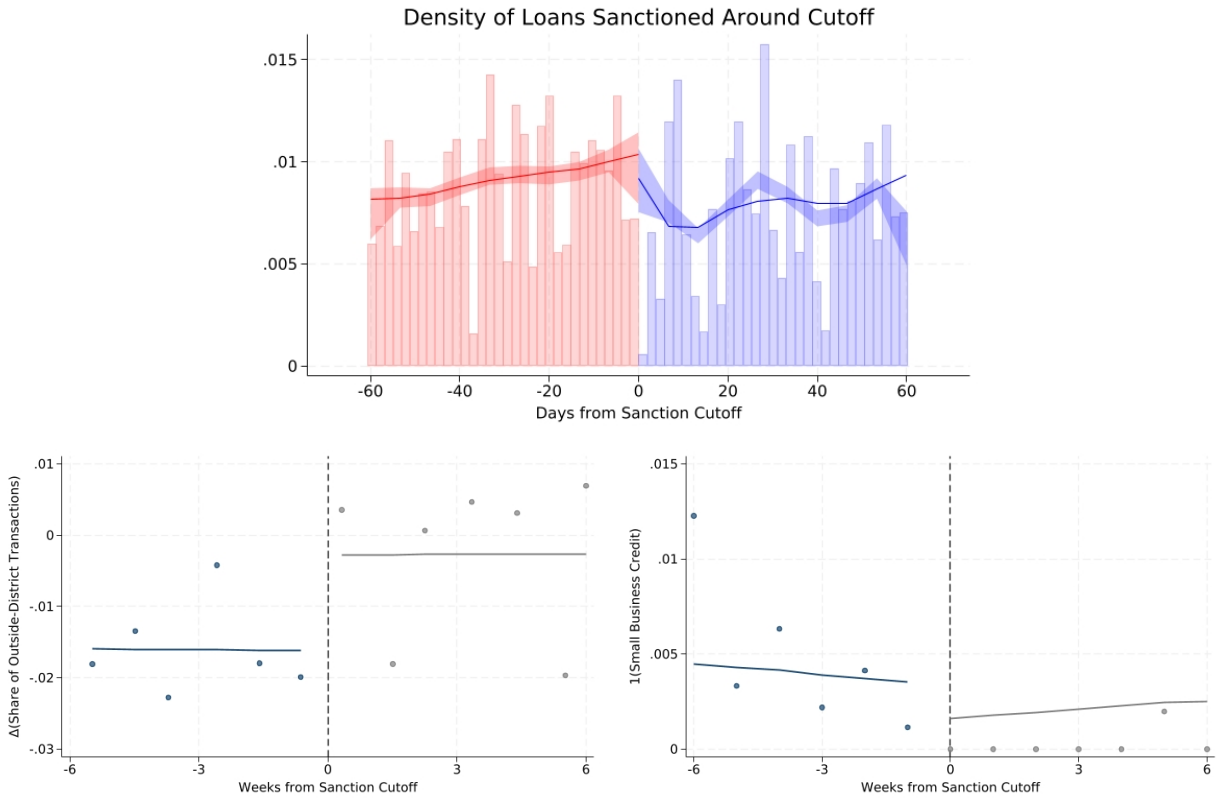
activity.

Small-business credit: The bottom-right panel presents the analogous plot for entry into small-business credit. Using the same six-week windows and linear polynomial specification, borrowers just eligible for relief are significantly more likely to obtain a small-business loan after the waiver than borrowers just ineligible, mirroring the main difference-in-differences estimates on the composition of household borrowing. The magnitude and direction of the discontinuity are consistent with the interpretation that eligibility for debt relief, not borrower or branch characteristics correlated with sanction date, drives entry into small-business credit.

Taken together, the regression-discontinuity evidence corroborates the two central findings from the bank data: waiver eligibility is followed by reduced reliance on distant wage employment and by increased entry into small-business credit. Because the design isolates variation in eligibility that is arguably orthogonal to borrower and branch characteristics, it addresses residual concerns that the main difference-in-differences estimates could be confounded by unobserved differences between eligible and ineligible borrowers within the same branch. The consistency of the estimates across the two designs strengthens the causal interpretation of the main results.

Figure C.1: **RDD Evidence Around the Sanction-Date Eligibility Cutoffs**

This figure presents regression-discontinuity evidence using the loan sanction-date cutoffs that determined waiver eligibility: December 31, 2013 in Andhra Pradesh and March 31, 2014 in Telangana. The top panel plots the density of first loans sanctioned within borrower-branch-loan-category cells around the eligibility cutoff date, and reports the McCrary density test for continuity. The bottom-left panel presents the regression-discontinuity plot for migration-linked remittance activity, proxied by cash transactions into borrowers' savings accounts at branches outside the home district. The bottom-right panel presents the analogous plot for entry into small-business credit, measured as the probability of obtaining any self-employment or small-business loan after the waiver announcement. Both bottom panels aggregate outcomes into week-long windows around the cutoff and fit a linear polynomial in the running variable (days since cutoff) on each side. Borrowers with loans sanctioned on or before the cutoff (to the left of the vertical line) are eligible for debt relief, while borrowers with loans sanctioned after are not. Standard errors are clustered at the borrower level.



D.1 Complementary Survey Evidence on Migration and Non-Farm Entrepreneurship

The main analysis relies on administrative bank data, which provide precise measures of debt relief eligibility and detailed observation of borrowers' financial activity, but do not directly observe household composition, member-level employment, or non-financial activities. I complement these measures with survey evidence from the Consumer Pyramids Household Survey (CPHS), a high-frequency panel that tracks the same households and household members every four months, allowing me to observe changes in household composition, member-level mobility, and self-employment activity over time. This appendix uses the CPHS panel to provide independent, external validation of two central findings from the main analysis: that debt relief is followed by reduced reliance on distant wage employment among farming households, and that it is accompanied by entry into non-farm entrepreneurship.

Sample and empirical strategy: CPHS began fielding in January 2014, with the first wave ending in April 2014. Because this is the only wave that predates the Andhra Pradesh and Telangana waivers of June 2014, the CPHS panel does not provide sufficient pre-period coverage to study the two waivers I focus on in the main analysis. I therefore exploit a broader source of variation: twelve state-level farm-loan waivers announced across Indian states between 2014 and 2019, and use the pre-Covid CPHS panel to estimate household-level responses to these waiver episodes. This exercise trades identification precision for panel coverage of household composition and member-level activity that the bank data cannot deliver, and it is intended as complementary evidence rather than as an independent test.

Household mobility: CPHS records member-level status in each wave, including whether a member is present, whether the household or member could not be located, and whether the residence was locked. I use these panel transitions to construct household-level indicators of member absence and of the return of previously absent members. Table D.1 presents difference-in-differences estimates of these outcomes around waiver announcements, comparing farming households in waiver-announcing states to other households. Columns 1 and 2 use the number and incidence of absent members; columns 3 and 4 use the number and incidence of return members. The estimates are negative for absence and positive for return, with the effects concentrated among farming households. The direction and heterogeneity of these estimates mirror the main analysis: waiver episodes are followed by less migration-linked absence and more return to surveyed households, particularly among households most exposed to agricultural debt relief.

Destination-side evidence: A limitation of the household-level test in Table D.1 is that it captures changes in surveyed households residing in waiver-announcing states, but does not observe activity at the destinations from which migrant members would return. If waivers induce reverse migration, the pattern should be visible not only as return migration to waiver-state households

but also as departures from the destination cities and industrial areas that host migrants from these states. I therefore turn to destination-side variation to provide complementary evidence more closely tied to reverse migration.

The empirical challenge is that CPHS does not record each individual's place of migration (at least until 2019), so I cannot directly identify workers who originated/migrated in a particular region. I instead exploit variation in the strength of pre-existing socioeconomic ties between potential destination districts and each waiver-announcing state. The intuition is that destination districts with stronger ties to a given waiver state are more likely to host migrant workers from that state, so departures from those districts after the waiver would be disproportionately associated with return migration to the waiver state.

I proxy for these ties using the Social Connectedness Index (SCI) developed by Bailey et al. (2018), which measures the relative frequency of Facebook friendship links between pairs of locations and has been widely used as a measure of pre-existing social and economic ties between regions. I restrict the CPHS sample to individuals classified as wage laborers, the group most likely to have originated as migrant workers, and construct district-level SCI connectedness to each waiver-announcing state. For each waiver episode, I exclude the waiver-announcing state itself and any state that announces its own waiver within a three-year window around the event, and classify districts in the remaining states by their SCI connectedness to the waiver state. I then compare, within these unaffected states, the post-waiver disappearance from the panel of wage laborers in high- versus low-connectedness districts, controlling for stack-by-person and stack-by-state-by-time fixed effects.

Table D.2 presents the results. Wage laborers in districts more socially connected to a waiver-announcing state are more likely to disappear from the CPHS panel after the waiver. The effect is present under both a median split of connectedness (columns 1 and 3) and a top-tercile classification (columns 2 and 4), and it appears both in the broad likely-away measure and in transitions into likely-away status in the current wave. Because these districts lie outside the waiver state itself but are strongly linked to it, the pattern is consistent with departures from destination areas that plausibly hosted migrant workers from waiver states, in the direction of the waiver state.

Non-farm entrepreneurship: Finally, I use the same household-level design as in TableD.1 to examine whether farming households in waiver-announcing states are more likely to enter non-farm entrepreneurship after waiver announcements. CPHS classifies each member's occupational status in each wave, including self-employment and small-business ownership. TableD.3 reports two outcomes: an indicator for whether any household member is classified as self-employed or as an entrepreneur in the current wave (column 1), and an indicator for a transition into self-employment or entrepreneurship, defined as households with no such member in the previous wave and at least one in the current wave (column 2). Both outcomes rise after waiver announcements, with the effects concentrated among farming households.

Taken together, the survey evidence points in the same direction as the bank-based analysis: farm-loan waivers are followed by reductions in migration-linked absence, corresponding increases in the return of previously absent members, greater departures from destination areas socially connected to waiver states, and higher rates of entry into non-farm entrepreneurship among farming households. Because CPHS captures different households than those in the bank sample and uses different measures of the underlying activity, these results are best read as external validation of the reallocation channel documented in the main text, not as a direct replication of the bank estimates.

Table D.1: **Farm Loan Waivers and Household Mobility in Survey Data**

This table presents complementary survey evidence on household mobility around state farm-loan waiver episodes using the CPHS panel. I estimate:

$$y_{h,s,t} = \beta_1 \mathbb{1}(WaiverState_s) \times \mathbb{1}(Post_{s,t}) + \beta_2 \mathbb{1}(WaiverState_s) \times \mathbb{1}(Post_{s,t}) \times \mathbb{1}(FarmingHH_h) + \alpha_h + \lambda_t + \varepsilon_{h,s,t}.$$

The outcomes use CPHS panel status to classify household members as away or as having returned after a prior absence. Column 1 uses the number of members classified as migrants; column 2 uses an indicator for any such member. Column 3 uses the number of members classified as return migrants; column 4 uses an indicator for any return migrant. $WaiverState_s$ identifies states announcing a farm-loan waiver, $Post_{s,t}$ identifies survey waves after the announcement, and $FarmingHH_h$ identifies farming households. All columns include household and survey-wave fixed effects. Standard errors are clustered at the household level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01). These outcomes provide complementary evidence on mobility and should not be interpreted as direct observations of the bank borrowers in the main sample.

	(1)	(2)	(3)	(4)
Output:	**#Migration**	**𝟙(Migration)**	**#Reverse Migration**	**𝟙(Reverse Migration)**
𝟙(Waiver State) × 𝟙(Post-Waiver)	0.0069	0.0044*	-0.0017*	-0.0008
	(0.0051)	(0.0026)	(0.0010)	(0.0005)
𝟙(Waiver State) ×𝟙(Post-Waiver) ×𝟙(Farming HH)	-0.0029	-0.001	0.0024***	0.0013***
	(0.0039)	(0.0020)	(0.0008)	(0.0005)
Household F.E.	Yes	Yes	Yes	Yes
Time F.E.	Yes	Yes	Yes	Yes
Observations	5,084,861	5,084,861	5,084,861	5,084,861
# Unique Households	203,789	203,789	203,789	203,789
R-Squared	0.04	0.042	0.042	0.047

Table D.2: **Destination-Side Evidence Consistent with Return Migration to Waiver States**

This table presents complementary destination-side evidence from districts outside waiver states that are socially connected to waiver-announcing states. The Social Connectedness Index (SCI) proxies for pre-existing migration or socioeconomic links between a potential destination district and a waiver state. For each waiver episode, I exclude the waiver-announcing state and retain states that do not announce a waiver within a three-year window around the event. I estimate:

$$y_{i,d,s,t} = \beta \, \mathbb{1}(HighConnected_{d,s}) \times \mathbb{1}(Post_{s,t}) + \alpha_{i,s} + \Gamma_{s \times state(d) \times t} + \varepsilon_{i,d,s,t}.$$

The dependent variable indicates whether individual i is classified as away from the surveyed household in district d and waiver stack s. CPHS records an individual as likely away when the member shifted, the household or member was not found, or the house was locked. In columns 1 and 3, $HighConnected_{d,s}$ identifies districts with above-median SCI connectedness to the waiver state; in columns 2 and 4, it identifies districts in the top tercile. Columns 1 and 2 use the broad likely-away measure, while columns 3 and 4 identify transitions into likely-away status in the current survey wave. All columns include stack-by-person and stack-by-state-by-time fixed effects. Standard errors are clustered at the district level. Significance levels: *(p < 0.10), **(p < 0.05), ***(p < 0.01). Because the data do not directly observe each individual's origin or destination, the estimates are interpreted as evidence consistent with departures from likely migrant destinations and return toward waiver states, not as a direct measure of return migration.

	(1)	(2)	(3)	(4)
Output:	**𝟙(Likely Away)**		**𝟙(Likely Away - This Wave)**	
𝟙(Above-Med Connectedness) ×𝟙(Post)	0.0232**		0.0147***	
	(0.0092)		(0.0055)	
𝟙(Top-tercile Connectedness) ×𝟙(Post)		0.0308**		0.0229***
		(0.0128)		(0.0077)
Stack ×Person F.E.	Yes	Yes	Yes	Yes
Stack ×State ×Time F.E.	Yes	Yes	Yes	Yes
Observations	1,535,414	1,077,739	1,535,414	1,077,739
R-Squared	0.571	0.57	0.301	0.3

Table D.3: **Farm Loan Waivers and Household Non-farm Entrepreneurship in Survey Data**

This table presents complementary survey evidence on household non-farm entrepreneurship around state farm-loan waiver episodes using the CPHS panel. I estimate:

$$y_{h,s,t} = \beta_1 \, \mathbb{1}(WaiverState_s) \times \mathbb{1}(Post_{s,t}) + \beta_2 \, \mathbb{1}(WaiverState_s) \times \mathbb{1}(Post_{s,t}) \times \mathbb{1}(FarmingHH_h) + \alpha_h + \lambda_t + \varepsilon_{h,s,t}.$$

The outcomes use CPHS panel status to classify household members as being self-employed or an entrepreneur. Precisely, in column 1, the outcome takes the value of 1 if any member in the household is marked as being self-employed or an entrepreneur in the given wave, and 0 otherwise. In column 2, the outcome is 1 only if a household, where none of the members were self-employed or entrepreneurs in the previous wave, reports at least one self-employed or an entrepreneur member in the current wave. $WaiverState_s$ identifies states announcing a farm-loan waiver, $Post_{s,t}$ identifies survey waves after the announcement, and $FarmingHH_h$ identifies farming households. All columns include household and survey-wave fixed effects. Standard errors are clustered at the household level. Significance levels: *(p $<$ 0.10), **(p $<$ 0.05), ***(p $<$ 0.01). These outcomes provide complementary evidence on mobility and should not be interpreted as direct observations of the bank borrowers in the main sample.

	(1)	(2)
Output:	**1(Self-Employment)**	**1(New Self-Employment)**
1(Treated State) × 1(Post-Waiver)	0.0385***	-0.0011***
	(0.0014)	(0.0001)
1(Treated State) × 1(Post-Waiver) × 1(Farming HH)	0.0054***	0.0035***
	(0.0017)	(0.0001)
Household F.E.	Yes	Yes
Time F.E.	Yes	Yes
Observations	5,084,861	5,084,861
# Unique Households	203,789	203,789
R-Squared	0.349	0.035
Y-Mean	0.216	0.005
Magnitude - Main (%)	17.79	-20.93
Magnitude - Interaction (%)	20.27	42.52