Debt Relief and the Reallocation of Household Labor and Capital *

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Abstract

In developing economies, household labor and capital remain concentrated in low-productivity occupations despite the recent dramatic expansion of credit. I show that legacy household debt can itself constrain occupational reallocation, and that relieving it activates the reallocation of labor and capital across activities that credit expansion alone has not. Studying two large 2014 Indian farm-loan waivers that forgave farming debt equal to approximately 4.5% of state GDP, and exploiting quasi-random variation in eligibility across otherwise similar borrowers, I document three sets of results. First, debt relief does not restore farm borrowing. Instead, beneficiary households reduce their reliance on distant wage employment, a margin associated with debt-driven temporary migration. Second, they redirect borrowing from farm credit toward personal and small-business credit that finances entry into local non-farm businesses. Third, districts more exposed to relief exhibit greater business formation and higher nighttime-light activity, consistent with these responses aggregating. The results identify legacy household debt as a financial friction on the reallocation of labor and capital across activities, and show that a balance-sheet intervention can activate this reallocation and support structural transformation.

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Keywords: Debt relief; household finance; occupational mobility; household reallocation; rural non-farm entrepreneurship; temporary migration; India

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1 Introduction

Productivity gaps between rich and poor economies reflect, in substantial part, the misallocation of labor and capital across productive 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, as switching occupations often requires upfront investment and greater risk-bearing capacity (Evans and Jovanovic, 1989; Banerjee and Newman, 1993). Yet while credit access has expanded dramatically in the developing world in recent decades, it has done little to loosen the occupational bind.[1] Instead, evidence suggests that accumulating household debt, in the absence of debt resolution, can itself distort occupational choice as repayment obligations reduce risk-taking and necessitate safer cash flows (Field et al., 2013; Battaglia et al., 2024; Krishnan and Wang, 2019). These distortions have renewed interest in debt relief policy as a lever on household investment and labor allocation (Bolton and Rosenthal, 2002; Indarte and Kanz, 2024). However, whether debt relief in developing economies can reallocate household labor and capital across activities remains an unstudied empirical question.

Answering this question is complicated by the near-absence of rules-based personal insolvency in developing economies. In its place, governments have relied on episodic, large-scale taxpayer-funded debt relief, most visibly in India through farm-loan waivers that forgive outstanding agricultural debt for millions of farming households. Existing evidence from one such program finds hardly any improvement in farm investment and productivity, along with a decline in subsequent farm credit (Kanz, 2016; Giné and Kanz, 2018; Mukherjee et al., 2018). But this evidence evaluates debt relief on the incumbent margin, farming. Whether real adjustment comes through the reallocation of household labor and capital out of farming remains unexplored.

I study this question in the context of large farm-loan waivers enacted by two Indian states in 2014 that forgave farming debt equal to approximately 4.5% of the states' GDP. Exploiting quasi-random variation in eligibility for debt relief, I show that relief does not increase subsequent farm borrowing. Instead, it reallocates household labor and capital away from farming. Eligible households reduce their reliance on distant wage employment – a margin often used to service debt and consumption through distress-driven temporary migrations (Mosse et al., 2002; Morten, 2019). At the same time, eligible households 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 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.

These findings are consistent with a debt-overhang view of occupational choice under financial

[1] In India, for instance, while the share of adults with access to formal finance rose from 35% in 2011 to 78% in 2021 (World Bank, 2022), the share of the workforce in agriculture decreased only marginally from 49% to about 45% over that period (National Statistical Office, 2020).

constraints (Evans and Jovanovic, 1989; Banerjee and Newman, 1993). For an indebted farming household, entry into a non-farm business entails uncertain and delayed returns, while repayment obligations reduce the household's residual payoff from success. Safe and immediate earnings, including those sustained through temporary migration, can therefore dominate risky local self-employment even when the latter might be a better use of household labor. Indebtedness may also restrict access to the external finance required to enter the new activity (Stiglitz and Weiss, 1981). Debt relief relaxes both margins: it increases households' capacity to bear entrepreneurial risk and restores the borrowing capacity needed to finance entry. Guided by this interpretation, I trace whether and how households reallocate capital and labor across production activities, and take entrepreneurial risks following debt relief.

I study farm loan waivers announced in July 2014 by Andhra Pradesh (AP) and Telangana (TG), two agrarian states in southern India.[2] Amidst high farm indebtedness, the waivers were an attempt to both reduce debt burdens and enable fresh credit for farming households that constituted 55% of the states' total workforce. The programs' budget was about 4.5% of state GDP, ranking 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 difficult 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 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, with no scope for borrowers to self-select into or out of the program.

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.

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

[2]The programs were introduced by newly formed state governments after the bifurcation of the erstwhile state of Andhra Pradesh in June 2014.

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

branch, were ineligible either because they were sanctioned just after the cutoff date or because they belonged to excluded agricultural loan categories.[4] Households on the two sides of the rule therefore faced the same local economic conditions and aggregate consequences of the policy. What differed was just the household-level debt relief resulting from the arbitrary eligibility threshold. Building on this variation, my baseline tests use a difference-in-differences design two and a half years before and after the waivers. I complement this by regression-discontinuity comparisons using only the sanction-date cutoffs. I first test and confirm 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 then proceed to test the household-level reallocation effects of debt relief, starting with external labor supply. Extant work suggests that debt obligations influence labor supply by increasing households' need for immediate and reliable earnings (Donaldson et al., 2019; Zator, 2025). This mechanism is particularly relevant for farming households, whose own-production income is low, seasonal, and risky, and who therefore combine labor supply on their own farm with external wage employment (Kochar, 1999). Because wage opportunities are limited in rural areas, this external employment is often pursued through temporary migration: households send selected members to work in urban informal sectors such as construction, transport, and manufacturing, while others remain to operate the farm at home (Morten, 2019; Imbert and Papp, 2020a).[5] This partial migration is not a permanent relocation, but a household financial strategy through which outside earnings support consumption, farm production, and debt service at the origin (Lucas and Stark, 1985; Mosse et al., 2002).

The bank data allow me to observe the financial footprint of migration-linked employment. During the period I study, much informal-sector 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.[6] I therefore use cash deposits made outside the household's home district as a proxy for distant wage employment, and provide extensive validation of this measure in Appendix Section B.1. Notice that the effect of debt relief on distant wage employment is ex-ante ambiguous. By relaxing liquidity constraints and financing the upfront costs of moving, relief could expand access to distant labor markets (Bryan et al., 2014a). Alternatively, if temporary migration is a costly coping strategy sustained by the need for reliable cash flows, debt relief could reduce households' reliance on it. This possibility is especially plausible given the documented nonpecuniary costs of temporary migration, including precarious work and difficult living conditions for migrants, which substantially limit its welfare gains and cause households to withdraw from

[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] Roughly one in every five rural Indian households report at least one member engaged in short-term migration for work, typically at urban centers (Morten, 2019).

[6] 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).

migration when rural support becomes available (Imbert and Papp, 2020a,b; Lagakos et al., 2023).

The difference-in-differences estimates show that waiver eligibility reduces migration-linked outside-district deposits by 14% relative to the pre-waiver mean. A 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 in 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. Collectively, the evidence indicates that debt relief reduces households' reliance on distant wage employment and is consistent with labor reallocating toward the local economy.

If debt relief changes the allocation of household labor, does it also reallocate household capital across productive activities? Both waivers were explicitly intended to restore access to institutional farm credit, making renewed borrowing for the incumbent farm a natural benchmark. Yet balance-sheet repair may instead loosen the risk-bearing and financing constraints that inhibit entry into activities with uncertain and delayed returns (Evans and Jovanovic, 1989; Banerjee and Newman, 1993; Field et al., 2013; Krishnan and Wang, 2019). Recent evidence that debt forgiveness expands borrowing in other credit markets is consistent with such a release of balance-sheet capacity (Dinerstein et al., 2025). Whether this renewed financial capacity expands households' incumbent farm production or finances entry into alternative production activities is an empirical question.

I find that relief-eligible households are about 7% less likely to take a new farm loan after the waiver. This decline is consistent with prior evidence that farm-loan waivers fail to raise farm investment or productivity and reduce subsequent aggregate farm credit (Kanz, 2016; Giné and Kanz, 2018). By itself, however, the decline is ambiguous: it may reflect reduced household demand for farm credit or a lender response to the moral hazard created by discretionary relief. Notice that my within-branch-time design, however, absorbs common changes in branch-level credit supply, attenuating concerns of a supply-based explanation. The broader borrowing response is more convincing. While farm credit falls, eligible households increase their take-up of personal loans by roughly 34%. Because farm loans receive substantial policy support,[7] while personal loans are typically unsecured and riskier for banks, this shift is difficult to explain through a general contraction in credit supply.

The composition of personal credit is more revealing still. While personal loans could substitute for lost agricultural liquidity, they could also represent external finance for early-stage household enterprises (Robb and Robinson, 2014). The composition of new borrowing points to the latter: among major personal-loan categories, neither housing nor auto credit increases; the response

[7]India's Priority Sector Lending program mandates banks to allocate a specific portion of their lending—18% specifically toward agriculture and a sub-target of 10% for small and marginal farmers—to ensure vital rural credit access. Short-term farm loans are also capped at 7% interest and further qualify for up to 3% interest subvention from the government, lowering the effective interest rate to just 4%. For comparison, India's benchmark repo rate fluctuated between 5% and 8% during the 2010s.

is concentrated in self-employment and small-business loans. These loans finance local business establishments and proprietorships such as food processing units, tailoring and garment businesses, and transport services. Such borrowing is rare among farming households at baseline, but rises by about 40% relative to the pre-waiver mean and originates almost entirely at home-district branches. Thus, as labor returns from distant wage employment, household borrowing also shifts toward the local, albeit non-farm economy.

Two cross-sectional patterns speak to the economic forces underlying the reallocation. First, the increase in small-business credit is concentrated among households whose pre-waiver deposit activity originated disproportionately from industrial hubs, the destinations most associated with migrant wage employment. This is consistent with substitution away from distant wage work: households most reliant on migration-linked cash flows are also the households most likely to redirect borrowing toward local self-employment once the balance sheet is repaired. Second, the increase in small-business credit is stronger among households for whom the waiver retired a larger share of outstanding debt. This is consistent with a debt-overhang mechanism: households whose balance sheets are more fully relieved regain more capacity to bear the risk and finance the fixed costs of entering new activities. Together, the two patterns line up with substitution away from a costly coping strategy, and the release of a binding balance-sheet constraint on entrepreneurial entry.[8]

As a next 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-eligible households are more likely to register a new non-farm business after receiving relief. The effect is modest in levels but large relative to the low baseline rate of formal business formation among farming households. 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.

One concern is that the increase in non-farm entry could reflect low-quality, subsistence entrepreneurship or excessive risk-taking induced by debt relief. Two pieces of evidence weigh against this interpretation. First, self-employment and small-business loans issued to eligible households perform at least as well as comparable loans issued to ineligible households, indicating that the increase in business credit does not come at the expense of repayment performance. Second, the business-formation results remain unchanged when I restrict the sample to firms that remain active for at least five years after the waiver. Together, these findings weigh against an interpretation based on transitory or distortionary entrepreneurial entry.

I subject the two central reallocation results to additional tests that address residual concerns

[8]Two additional patterns are consistent with the same interpretation. 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 or observed default status. The latter null is difficult to interpret because performing borrowers may remain current by rolling over existing obligations, drawing on informal credit, or directing scarce household earnings toward repayment.

about identification and measurement. First, although the baseline design compares eligible and ineligible borrowers within the same branch, eligibility also varies across loan products, leaving open the possibility that unobserved borrower characteristics differ across eligible and ineligible loans. To address this, I restrict the control group to households that had borrowed in the same loan category targeted by relief at some point before the waiver, which substantially attenuates concerns about compositional differences. As a further check, I isolate the sanction-date component of the eligibility rule in a regression-discontinuity design that compares borrowers within the same loan category on either side of the cutoff. Borrowers immediately around the threshold obtain the same product from the same branch and differ only in whether their loan was sanctioned within a narrow window before or after the cutoff date. Both migration-linked deposits and small-business borrowing exhibit sharp discontinuities at the cutoff in the same direction as the baseline estimates.

Second, my main outcomes are constructed from bank records, which precisely measure debt-relief eligibility and financial activity but do not observe household composition, member-level employment, or enterprise activity outside the formal credit system. I therefore turn to an independent household panel covering twelve subsequent state-level waiver episodes. Farming households in waiver-announcing states exhibit fewer member absences, more returns of previously absent members, and greater entry into non-farm self-employment after the waivers. Destination-side tests further show that wage workers in districts more closely connected to waiver states are more likely to disappear from the panel after the waiver, consistent with return migration toward waiver-announcing states. These survey estimates rely on broader policy variation and are therefore less sharply identified than the main analysis, but they observe household mobility and occupation directly, in a separate sample, and through independent measures. The consistency across research designs, data sources, and measures makes it unlikely that the main findings arise from borrower selection or from the particular proxies available in the bank data.

Having established the household-level response across these complementary designs, I next ask whether the effects are visible in broader local outcomes. I find that 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 patterns do not identify the full aggregate or equilibrium effects of the waiver, but they indicate that the documented household responses have economically meaningful local counterparts.

Taken together, the results shift attention from the availability of credit to the structure of household balance sheets. The development-finance literature emphasizes that limited access to capital constrains occupational choice. My results point to a complementary liability-side friction: legacy debt can sustain reliance on immediate earnings and limit households' willingness and ability to finance productive activities with uncertain and delayed returns. Debt relief can therefore generate occupational reallocation not by expanding the activity that incurred the debt, but by changing how households deploy labor and capital across activities.

The findings speak to a household-level margin of structural transformation. In economies where agriculture absorbs a large share of labor, the expansion of rural non-farm enterprise is

central to diversification and productivity growth (Gollin et al., 2014). My results suggest that this transition may be constrained not only by limited access to new credit, but also by legacy liabilities that inhibit households' ability to redeploy labor and capital. The results, however, do not amount to a welfare case for farm-loan waivers. Their net effects depend on fiscal costs, distortions to repayment incentives and future credit supply, and losses at migrant destinations, 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. I nevertheless provide a transparent accounting exercise in the Appendix. Under baseline assumptions, the reallocation channel offsets roughly 28% of the fiscal cost, after allowing for reduced farm credit. This calculation is not a verdict on loan waivers; it shows that occupational reallocation is quantitatively meaningful and omitted from conventional assessments of debt relief.

2 Related Literature

This paper contributes to four related literatures. Existing work on financial frictions studies how access to credit, wealth, and collateral shape entrepreneurial entry, while a growing household-finance literature examines how debt burdens and their resolution affect labor supply, mobility, and investment. Development economics has documented the growing role of rural non-farm activity and temporary migration in structural change, and a specific literature on Indian farm-loan waivers has evaluated their effects on farming. I situate my paper's contribution within each of these in turn.

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 repayment obligations and the structure of credit contracts can discourage entrepreneurial risk-taking and entry (Field et al., 2013; Battaglia et al., 2024; Krishnan and Wang, 2019).

This paper identifies a distinct liability-side friction on occupational choice. Rather than studying an expansion in wealth, collateral, or new credit, I study the removal of debt incurred in an incumbent productive activity. This distinction is particularly important for household enterprises, where production, financing, and family labor supply are jointly determined (LaFave and Thomas, 2016). I show that relief from production-linked debt reorganizes household labor and finance across activities: external wage employment, incumbent production, and new entrepreneurial ventures. The contribution is therefore not simply that finance affects entrepreneurship, but that legacy debt shapes the joint allocation of household labor and capital across occupations.

Household Debt Relief and Real Activit:. 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, entrepreneurship, 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 long-term 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). Financial distress at the firm can also reshape workers' careers and subsequent business formation (Bernstein et al., 2019; Babina, 2020; Graham et al., 2023). These literatures generally treat the indebted household and the productive firm as separate decision-makers. That distinction collapses when the household is itself the producer. In this setting, I show that debt relief need not revive the activity that incurred the liability. Its principal real effects may instead arise through the reallocation of household labor, borrowing, and productive activity elsewhere.

The paper also contributes to research on debt resolution in developing economies. Theory shows that debt relief can improve investment and labor allocation when incomplete 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 production-linked household debt can affect occupational choice, a margin largely absent from existing evaluations of developing-country debt relief.

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). My third contribution is to identify a financial-frictions margin of this rural transformation. The results suggest that diversification may be constrained not only by limited access to new finance, but also by legacy liabilities 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. 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., 2014b; 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, Giné and Kanz (2018) document weaker repayment incentives and a contraction in subsequent agricultural credit, and Mukherjee et al. (2018) shows that the effects of relief vary with borrower distress. Together, this work shows that forgiving farm debt need not revive farming and may distort subsequent agricultural lending.

I study a complementary adjustment margin outside the targeted sector. Rather than asking whether relief improves the indebted farm, I examine whether it changes households' allocation of labor and finance across occupations. I show that farm-debt relief reduces reliance on distant wage employment and redirects borrowing and entrepreneurial entry toward local non-farm production. The setting also differs from the 2008 waiver because relief was not conditioned on default: most eligible accounts were performing when the programs were announced. This allows me to study occupational reallocation following debt relief in a setting less mechanically tied to prior delinquency.

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 is well suited to studying how legacy production debt shapes the allocation of household labor and capital across occupations. Farming was the dominant occupation but generated low and volatile income, agricultural debt burdens were among the highest in India at the time of the waiver, and households routinely allocated labor and income across farming, distant wage employment, and local non-farm activity. These features together allow me to observe reallocation across all three margins in response to a large, quasi-random shock to legacy household

debt. I first describe the organization of rural livelihoods and household production in the region, and then discuss 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 provides an insufficient and risky livelihood. Farms are generally small and fragmented, depend heavily on rainfall, and operate with limited insurance and imperfect access to product and credit markets (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 operated as household enterprises, these low returns affect not only farm production but also how households allocate labor and financing 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). Such migration provided access to outside income but was not costless. 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 also imposed 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. Distant wage work was therefore an important household adjustment margin, but not necessarily an unconstrained movement toward a preferred occupation or location.

Local non-farm production offered another alternative. Rural non-farm activity consisted largely of household-operated enterprises in retail, food processing, repair, transport, and other small-scale services. Such enterprises allowed households to deploy labor locally while diversifying away from farming, and the growth of this sector has become an increasingly important margin of

structural change in developing economies where the classical transition from agriculture into urban manufacturing has stalled (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 limited credit access, thin local demand, and exposure to income risk as important constraints on 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). Financial capacity was therefore one potentially important determinant of whether farming households could redirect labor and capital toward local enterprise.

Household 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). Average outstanding debt exceeded average annual farm income. Formal agricultural credit contributed to this exposure: farm loans counted toward banks' priority-sector lending requirements and generally carried subsidized interest rates.[9] Access to subsidized formal finance did not, however, eliminate debt stress. Nationally, non-institutional lenders still accounted for 44% of agricultural-household debt in 2013 (National Sample Survey Office, 2013). 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). Farm liabilities thus sat on a household balance sheet that also supported consumption, outside employment, and potential entry into other productive activities.

Survey evidence suggests that household labor and capital allocation was far from optimal. 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). At the same time, complete exit was difficult: local opportunities were limited, while farming provided access to subsidized credit and electricity, tax-exempt income, and subsistence production. Households consequently combined a low-return but supported farm enterprise with distant wage employment, while entry into local non-farm production remained limited. The resulting allocation of household labor and capital is therefore a natural setting to examine whether relieving legacy production debt reorganizes household activity across occupations. 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 stated that the waivers were intended to break the cycle of agricultural indebtedness. The Telangana government order, for example, described farmers as trapped in a

[9] 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% (which reduced up to 4% on prompt repayment), below the risk-free rate in 2014.

"vicious cycle" of debt, and argued that a one-time loan waiver was necessary to relieve outstanding distress 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 final 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, eliminating 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 states, and the fiscal cost amounted to roughly 4.6% 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, the schemes were designed to operate at the borrower level immediately. Borrowers whose loans were eligible for waiver were to regain eligibility for fresh finance. The policy was therefore intended to resolve legacy borrower debt and restore formal credit access as soon as it was implemented. Notably, and unlike the 2008 national waiver, relief was not conditioned on default: most eligible accounts were performing at the time of the waiver, so the relief primarily eased the balance sheets of borrowers who were servicing debt through informal borrowing, rollover, or the diversion of household earnings toward repayment.

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 construct measures of household activity across locations.

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 match director names or firm names with customer names as additional signals when available. Note that businesses may be registered at a different address, under the name of another household member, or may remain informal and therefore never appear in government registration records. The matched individual-level sample should thus be interpreted as a lower bound on business creation. 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.[10] 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 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

[10] 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.

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. These transactions require physical presence at a bank branch and are much more common than outside-district withdrawals, suggesting that they capture 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 remittances being accessed by household members remaining at home.

Taken together, these patterns are consistent with outside-district deposits capturing remittances associated with temporary, circular, or seasonal migration rather than permanent household relocation. They are also consistent with partial household migration: one member may migrate for wage work while other members remain in the village to operate the farm, service household debt, and maintain local borrowing relationships. I therefore use outside-district cash deposits as a proxy for migration-linked household activity in the main analysis. The empirical tests below examine whether relief from legacy farm debt reduces households' reliance on this migration-linked financial strategy.

5 Results

I organize the analysis around three questions. First, did the administrative eligibility rules generate meaningful household debt relief? Second, did relief change households' reliance on distant wage employment? Third, did it alter the allocation of household finance across farming and non-farm production? I then examine formal business entry, the performance of the resulting 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 household liabilities, providing the first stage for the reallocation analysis.

5.2 Debt Relief and Distant Wage Employment

I first examine whether debt relief changes households' reliance on external labor income. Repayment obligations can increase the value of immediate and reliable earnings (Donaldson

et al., 2019; Zator, 2025). This mechanism is particularly relevant for farming households, whose own-production income is seasonal and risky and who often supplement it by sending selected members to distant wage markets. Debt relief has an ambiguous effect on this margin. Additional liquidity could finance the cost of migration and expand access to higher-paying labor markets (Bryan et al., 2014a). Alternatively, if temporary migration is a costly strategy used to generate cash for consumption, production, and debt service, relief may reduce households' reliance on distant work.

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 stability of the coefficient as the specification moves from stack-by-time fixed effects to branch-by-loan-product-by-amount-by-rate-by-time fixed effects is important. 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 the effect becomes progressively larger over the subsequent event years. The timing links the decline closely to debt relief rather than to 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 reallocation in the location of household financial activity 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. Thus, the main result is not an artifact of using a binary transaction measure.

The destination of pre-waiver deposits provides a further test. 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 especially associated with informal migrant employment, the heterogeneity strengthens the interpretation of outside-district deposits as the financial footprint of distant wage work.

Taken together, the results show that debt relief reduces households' reliance on migration-linked outside earnings. The decline appears after the waiver, is concentrated on the extensive margin, is strongest for households previously connected to migrant destinations, and coincides with a shift in account activity toward the home district.

5.3 Reallocation of Household Finance

If debt relief changes the allocation of household labor, does it also reallocate household capital across productive activities? Both waivers were intended to restore beneficiaries' access to institutional farm credit, making renewed borrowing for the incumbent farm a natural benchmark. Yet balance-sheet repair may instead loosen the risk-bearing and financing constraints that inhibit entry into activities with uncertain and delayed returns (Evans and Jovanovic, 1989; Banerjee and Newman, 1993; Field et al., 2013; Krishnan and Wang, 2019). I therefore examine not only whether relieved households borrow again, but what they borrow for.

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).

The farm-credit decline alone is difficult to interpret. It could reflect lower household demand for agricultural credit, or a lender response to the moral hazard created by discretionary relief. The saturated fixed effects absorb common changes in credit supply within a branch and loan category, but they cannot rule out differential treatment of waiver-eligible borrowers. The composition of subsequent borrowing is therefore more informative.

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 explain through a generalized contraction in credit supply. Agricultural loans count toward banks' priority-sector obligations and receive interest subsidies, whereas personal loans are generally less protected, more expensive, and often unsecured. A lender unwilling to extend credit to waiver beneficiaries would not naturally replace subsidized farm lending with riskier personal credit.

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. The probability of receiving a new loan in these categories increases by 0.12 percentage points. Although small in levels, this represents a 40% increase relative to the low pre-waiver mean among farming households.

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 relief 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. Household finance therefore shifts toward non-farm credit relationships anchored at the origin rather than following workers to distant locations.

5.3.2 Mechanism Heterogeneity

Table 7 examines which households account for the increase in small-business credit. Two patterns align closely with the mechanism proposed in the introduction.

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. Households with the strongest pre-waiver connection to migrant labor destinations are therefore also the households most likely

to enter small-business credit after relief. This pattern is consistent with substitution away from migration-linked earnings and toward local enterprise finance, although the heterogeneity itself is not 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. The pattern is consistent with a liability-side financial friction: the greater the balance-sheet repair, the larger the subsequent movement into credit used for alternative production.

By contrast, the response does not vary with the borrower's percentile rank in total pre-waiver deposits. This weighs against an explanation in which the effect simply reflects differences in 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 do not support a simple account-cash-flow explanation for the rise in business credit.

The response also does not vary significantly with observed pre-waiver default. This null is less informative about financial constraints. Most eligible accounts were performing before the waiver, and a borrower may remain current by refinancing existing obligations, using costly informal credit, or directing scarce household cash flow toward repayment. Observed delinquency therefore need not identify the households for whom debt most tightly constrains occupational choice.

Overall, the credit results show a change in the composition and location of household finance. Debt relief does not restore borrowing for the incumbent farm. Instead, relieved households move toward locally originated personal and, in particular, self-employment and small-business credit.

5.4 Entry into Local Non-Farm Production

The borrowing response suggests that restored financial 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 total entrepreneurial entry.

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. The absolute effect is modest, but the baseline incidence of formal entry among farming households is extremely low; the estimates imply an increase of approximately 146% to 249% relative to the pre-waiver rate. 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 condition on businesses remaining active for at least five years after the waiver, weighing against an interpretation based entirely on transitory registration.

Taken together with the shift toward self-employment and small-business credit, the registration evidence indicates that balance-sheet relief supports entry into new local production rather than expansion of the indebted farm.

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 occupational reallocation. The data do not permit a complete comparison of productivity across occupations, but they allow me to examine whether the increase in business credit comes at the expense of repayment performance.

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.

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, 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 transformational, but they show that the expansion in business borrowing is not accompanied by lower observed credit quality.

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 to address the corresponding identification and measurement concerns.

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.

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. The direction of both discontinuities mirrors the difference-in-differences estimates. Because these comparisons hold the loan category and branch fixed and use only borrowers close to the sanction-date threshold, they substantially reduce the scope for unobserved product or borrower composition to explain the 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 occupational status. Appendix Section D.1 therefore turns to the Consumer Pyramids Household Survey and 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 observation of household composition and occupation.

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.

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. These districts lie outside the waiver state, so the results are consistent with departures from likely migrant destinations toward the states receiving debt relief.

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. The same pattern appears across administrative and survey data, borrower- and household-level samples, origin- and destination-side measures, and financial and occupational outcomes. This convergence makes it unlikely that the main findings are driven solely

by selection across loan products or by the particular transaction-based proxy used in the bank data.

5.7 Broader Local Outcomes

The borrower-level evidence shows that debt relief redirects household finance toward local enterprise. I conclude by asking whether this response is visible in broader local outcomes. 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.

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.

Table 10 reports analogous estimates for nighttime-light intensity. 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 losses at migrant destinations. They nevertheless show that the household-level shift toward local non-farm production has counterparts in broader measures of business formation and economic activity.

6 Conclusion

A leading finance-native explanation for why households misallocate labor and capital across occupations and sectors in developing economies is that credit frictions constrain occupational choice. Yet despite dramatic expansions in formal credit access across the developing world, households remain stuck in low-return occupations. This paper identifies one reason: the accumulated debt that expanded credit access has produced can itself act as a friction on occupational mobility, tying household labor and capital to the very activities the debt was borrowed for.

I document this in the context of two large 2014 Indian farm-loan waivers. Debt relief does not restore farm borrowing. Instead, it reallocates household labor and capital away from farming. Household labor returns from distant wage employment, a coping strategy sustained by the need for immediate cash flows to service debt. Household borrowing shifts from farm credit toward personal and small-business credit that finances entry into local non-farm enterprise. Districts more exposed to relief exhibit greater business formation and higher aggregate economic activity, consistent with

these responses scaling up.

These findings offer three broader lessons. First, they identify legacy debt as a distinct financial friction on the reallocation of labor and capital across activities. Unlike the credit-access frictions on which the literature has focused, this friction operates on the liability side of the household balance sheet and can be relaxed by a balance-sheet intervention rather than by an expansion of credit. Second, evaluations of debt relief that examine only outcomes within the targeted sector may miss the margin on which relief has its principal real effects. When production, labor, and financing are jointly determined at the household level, relief need not revive the indebted activity to generate productive adjustment; it can instead redirect household resources to alternative uses. Third, the results speak to a household-level margin of structural transformation. In economies where the classical transition from agriculture into urban manufacturing has stalled, the growth of local non-farm enterprise has become an increasingly important path out of low-productivity self-employment. My results suggest that this transition is constrained not only by limited access to new credit, as the rural non-farm growth literature has emphasized, but also by legacy liabilities that inhibit households' ability to bear risk and deploy capital outside the incumbent activity. Balance-sheet interventions can activate this reallocation directly.

The welfare consequences of large-scale farm-loan waivers are not settled by this evidence. Fiscal costs, future distortions to formal credit supply, and losses at migrant destinations must be weighed against the returns to new enterprise, the reallocation of household labor toward higher-return uses, and the pecuniary and non-pecuniary costs of migrant employment that are avoided. My accounting exercise suggests that the reallocation channel documented here offsets a quantitatively meaningful share of the fiscal cost, but it is not a verdict on whether such waivers are net welfare-improving. What the paper does establish is that occupational reallocation is a first-order margin of adjustment, one that conventional evaluations of debt relief in developing economies have not measured.

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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.

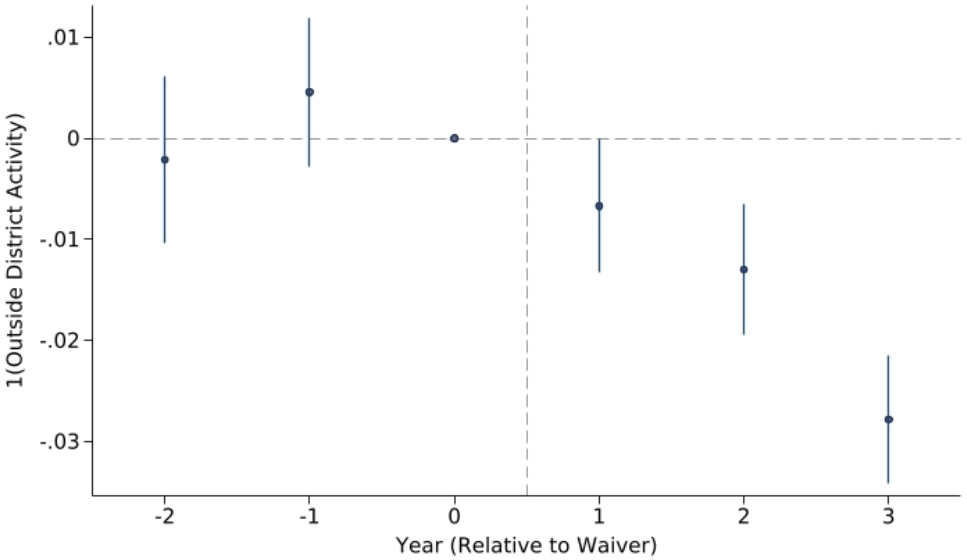


Figure 3: **Debt Relief and the Spatial Reallocation of Cash-Transaction Activity: Dynamics**

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.

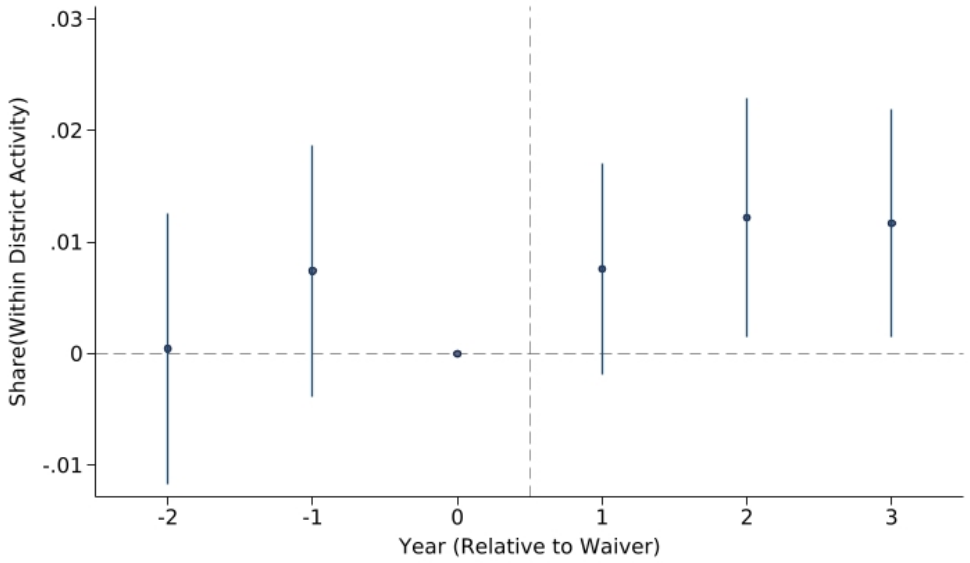


Figure 4: **Debt Relief and Self-Employment and Small-Business Credit: Dynamics**

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.

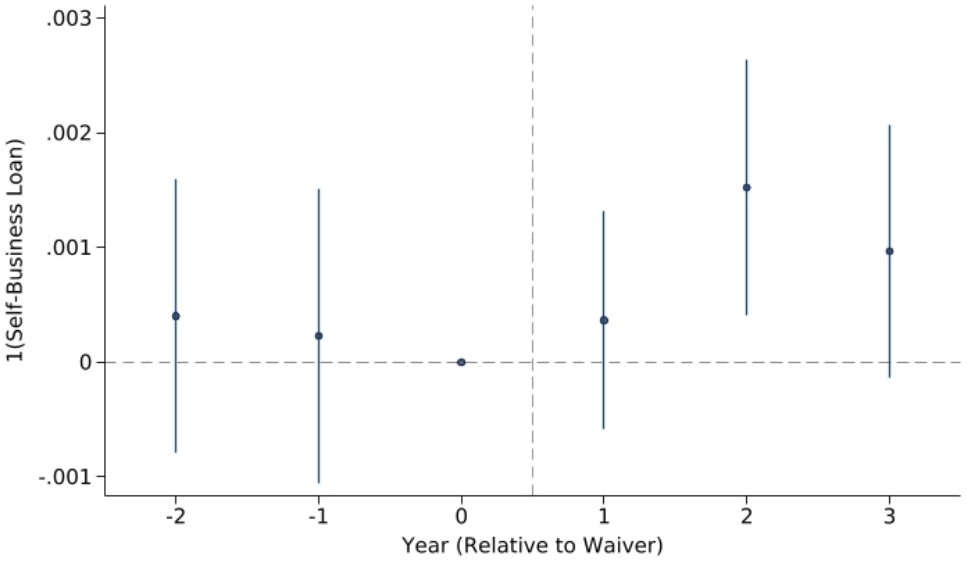


Figure 5: **Debt Relief and the Performance of Self-Employment and Small-Business Loans: Dynamics**

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.

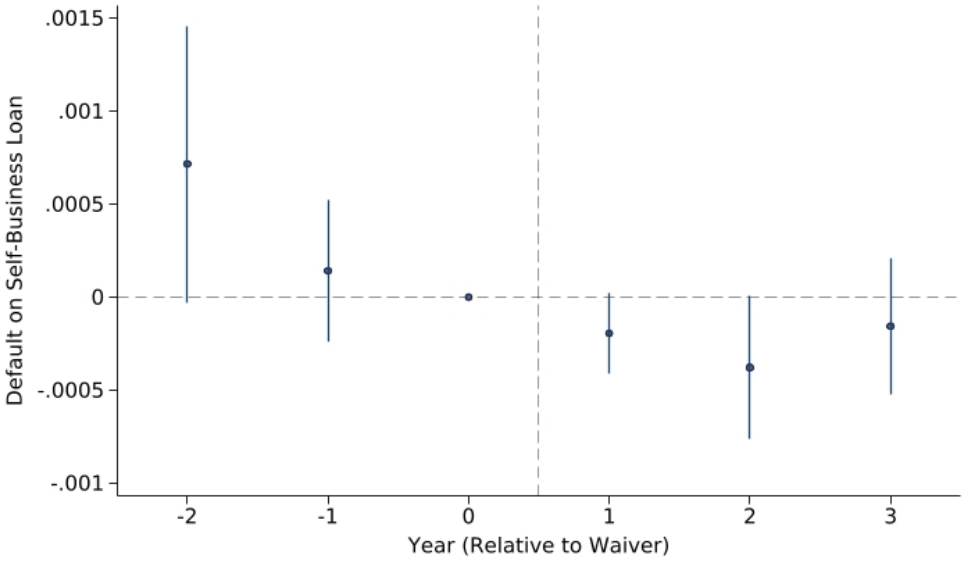


Figure 6: **Debt Relief and Registered Non-Farm Business Formation: Dynamics**

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.

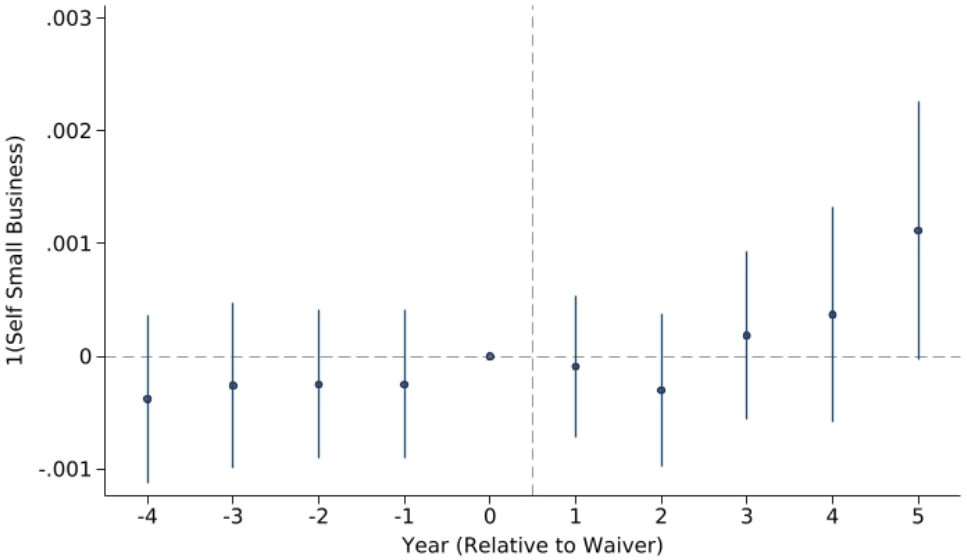


Figure 7: **Debt Relief Exposure and District-Level Business Registrations: Dynamics**

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.

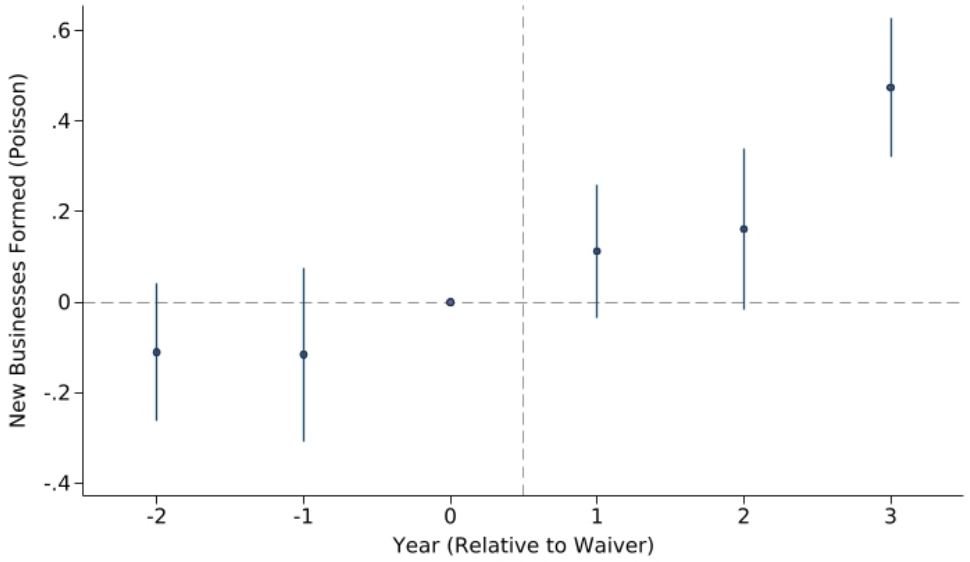


Figure 8: **Debt Relief Exposure and District-Level Nighttime Lights: Dynamics**

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.

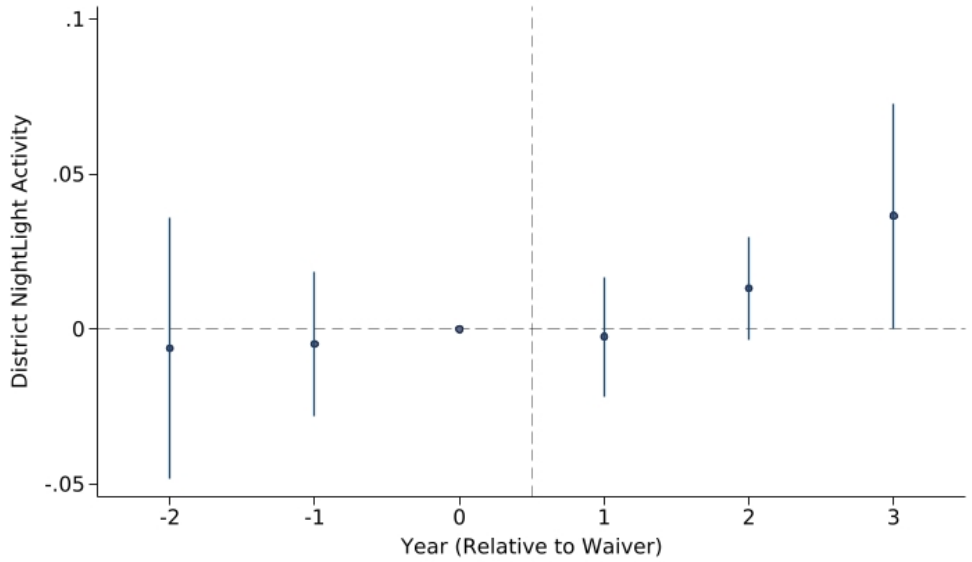


Table 1: **Summary Statistics**

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., 2014b; 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.

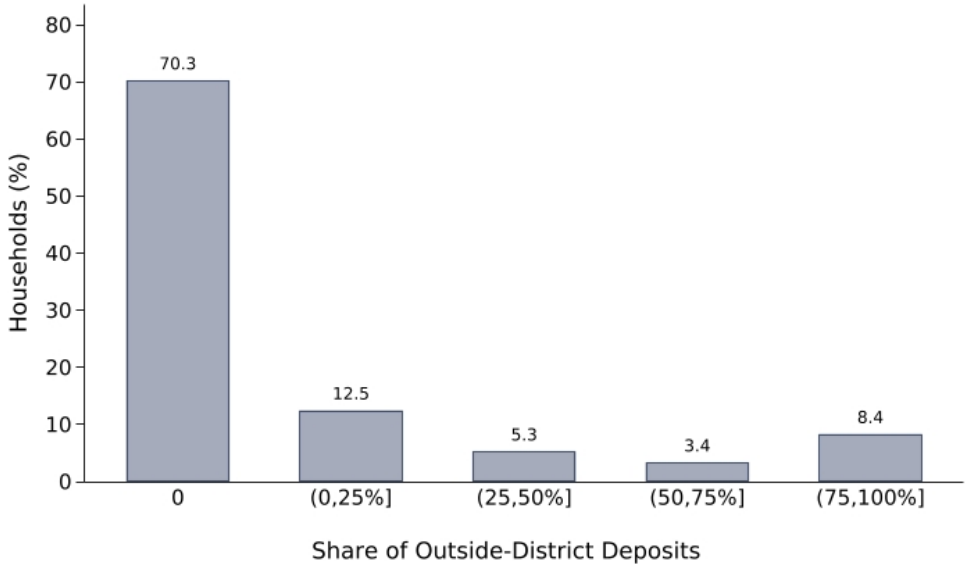
The duration and recurrent nature of outside-district remittance patterns suggest that the migration is temporary rather than permanent. Figure B.3 reports the duration of outside-district deposit spells. A spell is defined as a cluster of months containing outside-district deposits, allowing for up 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.

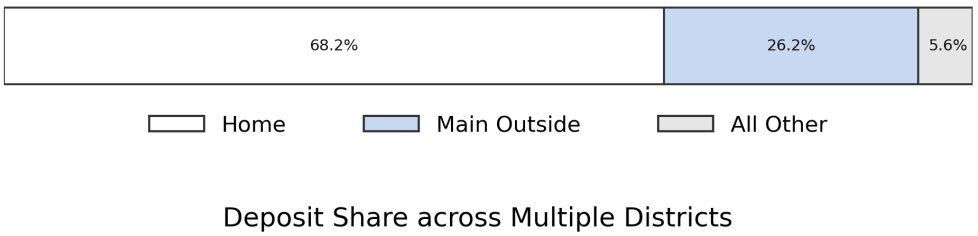
Figure B.4 examines whether these remittance episodes recur at similar times across years. For each borrower-year, I identify the first calendar month with an outside-district deposit and compare this month across adjacent active years. The figure reports whether timing recurs in at least half of adjacent-year 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.

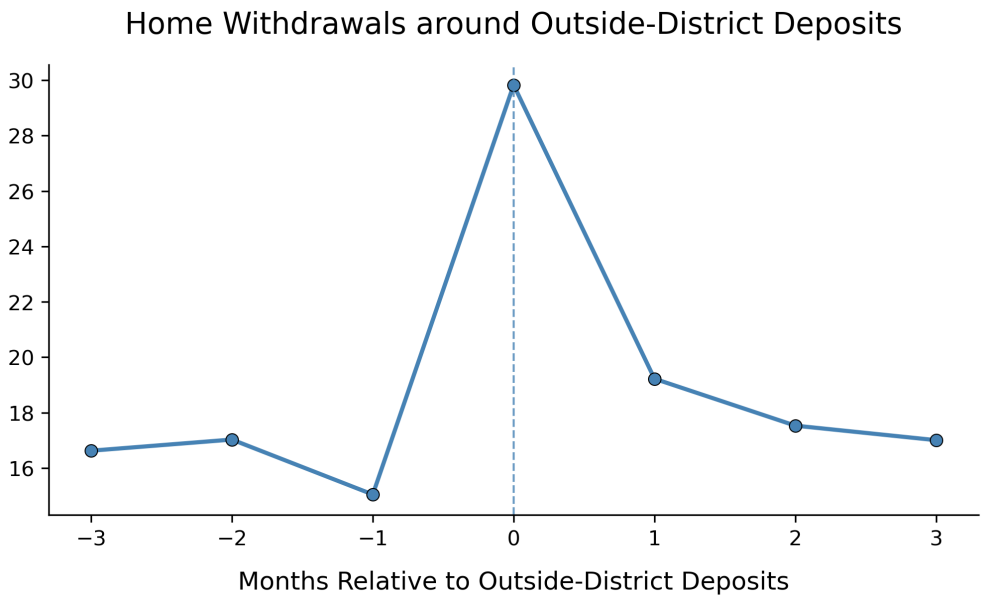


Panel A



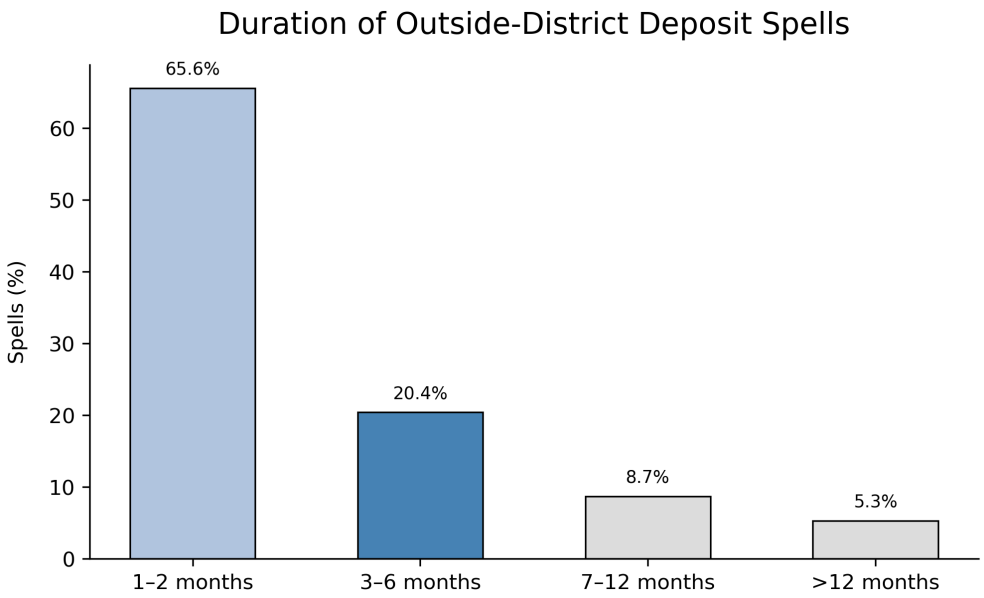
Panel B

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



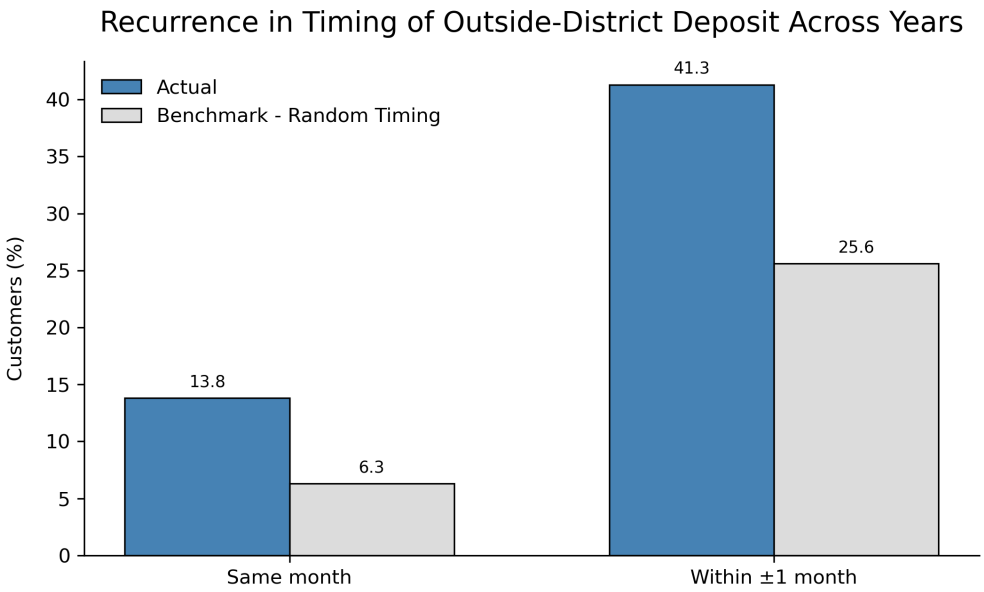
Notes: This figure plots the share of borrower-months with a home-district withdrawal around the start of an outside-district deposit episode. Month 0 is the first month of the episode. The contemporaneous rise in home-district withdrawals is consistent with funds deposited outside the home district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

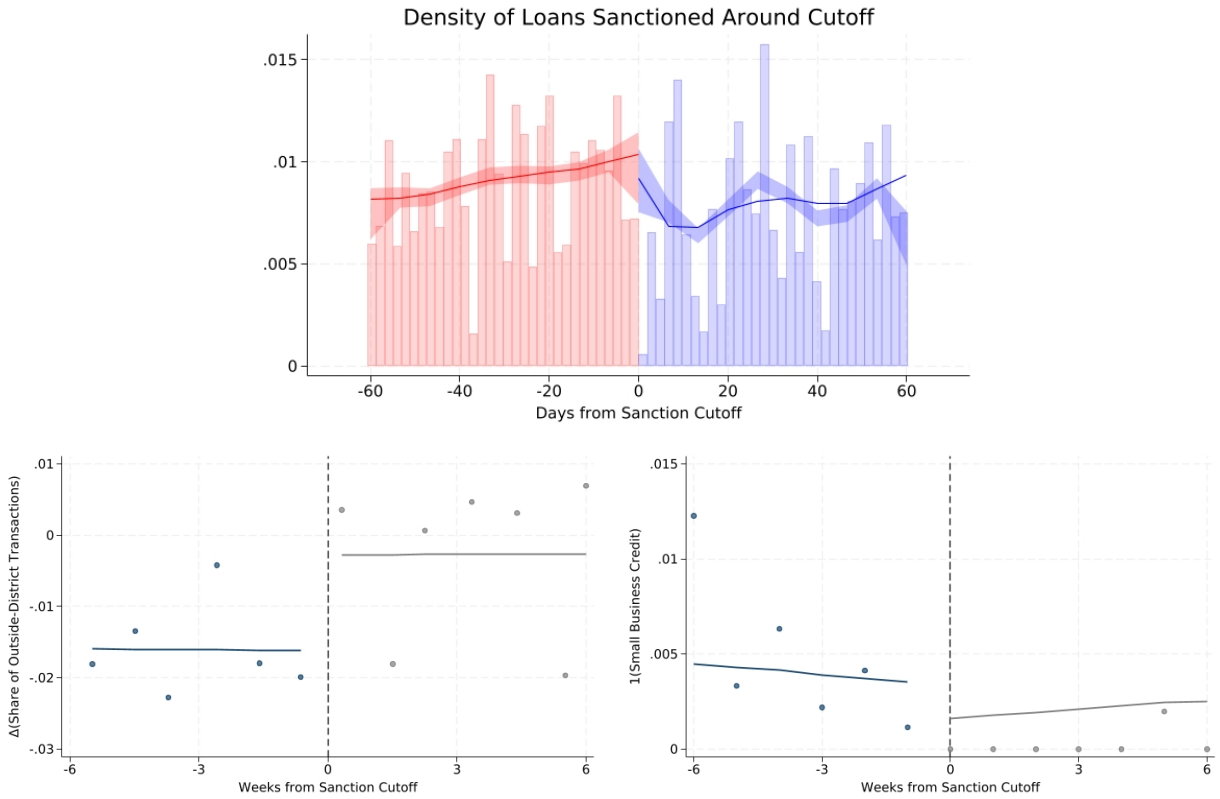
Small-business credit: The bottom-right panel presents the analogous plot for entry into small-business credit. Using the same six-week windows and linear polynomial specification, borrowers just

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