Is a Withdrawal a Rollout in Reverse? Wages and Credit After India Froze Its Job Guarantee

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Abstract

Is losing a safety net the reverse of gaining one? In March 2022 India's central government stopped funding its rural job guarantee in one state, West Bengal, and no work returned for more than four years. Comparing blocks within the same district by how much they had relied on the scheme before the freeze, we find that the demand for credit rose where lending fell. Loan applications increased while new personal, business and farm loans declined, so personal loans per application fell by 14 percent between blocks at the 10th and 90th percentile of reliance. The only lending that expanded was secured against gold, most of it to women. Small gold loans rose by 24 percent across the same range. They began to rise when the work stopped rather than when the pay stopped, and they fell back once the state paid the old wages in 2024. Wages did what the rollout studies predict in reverse. Rural casual wages fell more where the scheme had mattered more, and laboring households lost about a rupee of income for every rupee of scheme wages. Formal credit replaced about four rupees in every hundred lost, and lower spending absorbed most of the rest. A job guarantee has spillovers well beyond the households it pays, holding up the wages and the lending around them, and withdrawing it takes both away faster than households can replace them.

Keywords: job guarantee, public works, household credit, gold loans, consumption smoothing, MGNREGA, India
JEL: D15, G21, H53, I38, O16

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1 Introduction

What happens to poor households when a large safety net is taken away? For twenty years, since 2006, India's rural job guarantee has promised every rural household up to 100 days of paid manual work a year, and it is the largest program of its kind in the world (Muralidharan et al., 2023). The central government used to pay every wage the guarantee owed. Under a new law that took effect in July 2026, most states must pay 40 percent of the cost and anything above a yearly allocation, and each state must name up to 60 days a year, in the peak farming seasons, when no work is offered. A state that cannot pay its share may offer less work, so losing guaranteed work is now a possibility across rural India. Yet the evidence on the guarantee comes almost entirely from watching it arrive or improve. We study what happened when it was taken away. In March 2022 the central government suddenly stopped all funding for the guarantee in one state, West Bengal, and the work did not return for more than four years. We ask whether rural households replaced the lost guarantee with credit, and whether formal lenders treated the loss of guaranteed income as a sign that borrowers had become riskier. To answer, we also measure what the freeze did to local wages and to household income and spending.

Whether households can bear the loss of a guarantee, and what the loss does to the labor markets around them, matters to any government that might withdraw one, and it is very hard to answer. The easy answer is to run the earlier evidence backward. Studies of the guarantee's rollout and its better management found higher casual wages in the dry season (Imbert and Papp, 2015), higher incomes and less poverty (Muralidharan et al., 2023), and more bank credit in the districts that received it first (Chaturvedi and Rauf, 2025). If the guarantee raised wages and credit when it arrived, losing it should lower them by as much. But a withdrawal need not mirror a rollout, for two reasons. Some of what a program creates outlasts it. The job cards, bank accounts and credit histories created by the guarantee remain after the wages stop, so credit may not fall the way it rose. Households have also had years to arrange their lives around the program, so losing it may hurt more than gaining it helped. Marquez-Padilla et al. (2025) find such a gap in Mexico, where school enrollment moved by more when Progresa, a cash transfer to poor families tied to their children's schooling, ended than when it began.

Measuring the loss directly is difficult for several reasons. No government or researcher would withdraw a legal entitlement from millions of households as an experiment. Where programs shrink, they tend to shrink gradually, and often for reasons tied to local conditions. A credible test needs a loss large enough to move local markets, a cause unrelated to those markets, a comparison group that keeps the program, enough time to see households adjust, and records that show how they cope. The rollout could not offer such a test even in reverse. The government chose the poorest districts first, 200 in February 2006, then 130 in April 2007 and the rest in April 2008 (Imbert and Papp, 2015). Early districts were poorer than late ones by design, and the two could be compared only until the late districts joined.

The freeze in West Bengal meets most of these conditions, though it is not a clean experi-

ment. The guarantee dates from 2006, under the Mahatma Gandhi National Rural Employment Guarantee Act (MGNREGA), and the work is on local public projects. A household qualifies not by being poor but by asking. In West Bengal, home to about 99 million people in 2022 (National Commission on Population, 2020), the guarantee stopped. Wages for work already done stopped arriving in late December 2021. On 9 March 2022 the central government invoked Section 27 of the Act, which it has used against no other state, and stopped all funds, citing irregularities. People went on working without pay for months, which suggests they expected the money to come, and the work ran down by August 2022. Recorded work fell from 364 million person-days in 2021-22 to 37.9 million in 2022-23 and to almost nothing after that, while work in the neighboring states rose. The scheme did not return until 1 June 2026, after a change of state government. We call this stoppage the freeze, and Figure 1 shows the result by district. Across a state line, one side kept the guarantee and the other lost it for more than four years. The state later funded more work of its own, so our main estimates stop in March 2023. The freeze was not random, since the central government chose which state to stop, during a dispute with the state government, and this matters most for comparisons across the state line. Within the state, the closest threat is housing. Funds for rural housing were withheld at the same time, and blocks that relied more on the job scheme may also have lost more housing money, so we read our estimates as the effect of losing both. The 2022 monsoon was also poor across the state.

We address these challenges by making two comparisons, each with a different job. Following Marquez-Padilla et al. (2025), we first compare community development blocks in the same district that relied more and less on the scheme, before and after. Reliance, measured in days of scheme work per household a year over 2018-20, runs from 9.3 days at the 10th percentile to 36.7 at the 90th. The freeze stopped the scheme in every block, so a block that had relied more on the scheme lost more work. The central government also took the decision for the state as a whole, so it did not depend on how credit was changing in any one block compared with another. This comparison removes everything else happening in West Bengal, but it would miss effects that moved the whole state, including effects on wages across connected blocks. We therefore also compare the two sides of the state line, using pincodes in subdistricts within 30 kilometers of the border and the district-pair comparison of Ghosh and Vats (2026), and laboring households in West Bengal and in its neighbors. This comparison captures statewide effects, but it also picks up other differences between states, such as the cash payments to women that West Bengal began in September 2021. The event studies are flat for five years before the freeze, and the block design shows nothing in states where the work did not stop.

The freeze also brought a second event. In February 2024 the state government began paying arrears, the wages the central government had left unpaid for work done before the freeze. Blocks that had worked more on the scheme were owed more, so we can see whether the responses reverse when part of the lost income suddenly comes back. To see how households

cope, we combine credit bureau records of new formal loans by product, lender and borrower's sex with a household panel and a labor force survey.

We find that the freeze ran the rollout backward on wages and on the loans that rest on future income, and that it did one thing the rollout never showed, which we describe first. Ten more days a year of reliance, worth Rs 159 a month in lost wages per household at the scheme wage of Rs 191 a day, moved all new lending by only 0.5 percent, and a regression on total credit would conclude that nothing had happened, yet underneath the mix of borrowing changed. Small gold loans from banks rose 8.2 percent per 10 days of reliance, or 24 percent between blocks at the 10th and the 90th percentile, while personal loans fell 2.8 percent and business and farm loans fell by similar amounts. A lender holding a necklace does not need to know whether the borrower will have work next month, and a lender making a personal loan does. Applications, measured by lenders' inquiries to the credit bureau, rose at the same time, by 3.0 percent for all loans and 2.9 percent for personal loans, so households asked for more credit and ended up with fewer of the loans that rest on future income. We do not observe approvals or rejections, so we cannot show a cut in lending supply. The pattern is what earlier work predicts if lenders had counted the guarantee as a kind of collateral (Dey and Imai, 2015; Chaturvedi and Rauf, 2025), but rising demand from riskier applicants could produce it too. Across the state line, small gold is 14.6 percent higher and personal loans are 5.8 percent lower on the West Bengal side, close to what the block estimates imply at mean reliance.

The timing says how households reached for this credit. From December 2021 to July 2022 people kept working without pay, so the cash had stopped but the work had not, and gold borrowing rose only a little in those months. It climbed as the work ran out over the summer of 2022, and it peaked in December to February, the months when the scheme used to offer the most work, which fits households running through savings first and pledging gold once no end was in sight. Once the state began paying the arrears in February 2024, the gradient in gold lending fell from 9.5 to 3.9 percent within four months. The state's own work scheme started in the same months, so part of that fall may be due to work rather than to the arrears.

Wages and income, by contrast, fell about as the rollout studies predict in reverse. Rural casual wages fell 1.7 percent per 10 days of reliance. Agricultural laborers, wage laborers and small and marginal farmers, whom we call laboring households, lost about Rs 182 (about US$2.40) a month more in scheme wages per 10 days than other households in the same district, and their income fell by about the same amount, 1.7 percent, because market wages fell too. Of every Rs 100 of scheme wages lost, formal credit replaced about Rs 4 and lower spending absorbed Rs 58 (Figure 15). A forecast built from the rollout would have been about right on wages and on personal loans, and it would have missed the gold.

These results add to three literatures, of which the first asks whether the gains from anti-poverty programs survive when transfers end, mostly in short pilots (Haushofer and Shapiro, 2018; Baird et al., 2019; Blattman et al., 2020). Our closest model is Marquez-Padilla et al.

(2025), who show that the end of Prospera, as Progresa was later called, pushed teenage boys out of school and into work. We study a program of a different kind, one that pays adults for work and sets a floor under private wages. The second studies safety nets and credit. Work on India studies programs as they roll out (Bell and Mukhopadhyay, 2015; Surendra, 2020; Chaturvedi and Rauf, 2025; Ghosh and Vats, 2026; Tantri, 2025), and work on withdrawals comes from rich countries, where most people already hold a credit file (Argys et al., 2019; Bailey et al., 2023; Coombs et al., 2021; Dasgupta and Plum, 2023). We study a withdrawal among people who mostly do not hold a credit file. The income and the collateral channels move different loans in opposite directions there, so the mix of borrowing reveals both even when the total does not move, and the months of unpaid work before the freeze show when households turned to credit. The third studies how poor households get through a bad year. Wages move more with bad harvests where workers cannot easily borrow or leave (Jayachandran, 2006), a contraction in credit can itself lower wages (Breza and Kinnan, 2021), and a small fall in consumption can hide costly ways of keeping it up (Chetty and Looney, 2006). We set these margins side by side for one large loss in one place. To our knowledge, this is the first study of what happens to households and their lenders when an employment guarantee is withdrawn.

The rest of the paper proceeds as follows. Section 2 describes the scheme and the freeze, and Section 3 presents our data and methods. Section 4 reports the effects of the freeze on borrowing, and Section 5 reports its effects on wages and spending. Section 6 brings these results together to ask how costly the loss was for poor households, and Section 7 concludes.

2 Background

2.1 The Guarantee

Parliament passed the Act in August 2005, and the scheme reached every rural district by April 2008 (Chaturvedi and Rauf, 2025). Any rural household can register for a job card and ask for work, up to 100 days a year in total, at a wage notified by the central government for each state. Projects are small public works chosen locally, and wages have been paid into bank and post office accounts since 2008 (Surendra, 2020).

The scheme's reach goes beyond the people on its muster rolls. Imbert and Papp (2015) find that dry-season casual wages rose 4.7 percent across India and 9 percent in the states that ran the scheme at scale, with private work crowded out about one for one. Muralidharan et al. (2023) randomize better payment systems across 157 subdistricts of undivided Andhra Pradesh and find that household income rose 13.9 percent, mostly from market wages and not from the scheme's own pay. Imbert and Papp (2020) find that the scheme cut short-term migration by about half where it ran at scale.

Its reach into credit is less settled. Bell and Mukhopadhyay (2015) find that one more day of work lowers consumption borrowing by about half the daily wage and raises borrowing for

cultivation. Chaturvedi and Rauf (2025) find that bank credit rose by 5.5 to 22 percent in the first districts and personal credit by 5.9 to 27 percent, and they argue that job cards, identity documents and wages paid into accounts cut the cost of lending to poor households. Dey and Imai (2015), with a panel from Birbhum district in West Bengal, find that a record of steady work is what lets poor households buy on credit at the local shop. Muralidharan et al. (2023) find that total loans rose by 30 percent, most of it informal, which they read as greater confidence on both sides that loans could be repaid. West Bengal itself ran the scheme at scale. Over 2018-20 it provided 22 days per rural household a year, fifth among 20 large states, against 16 for India as a whole.

2.2 The Freeze

The freeze came in two steps, beginning when wage payments stopped in late December 2021 while work went on.[1] On 9 March 2022 the Ministry of Rural Development stopped all funds under Section 27 of the Act, which allows the central government to halt payments when a state does not comply with its directions. The state had received about Rs 75 billion from the scheme in 2021-22, and it received almost nothing in the next three fiscal years. The dispute ran between the central government and a state ruled by an opposition party. The charges of irregularities centered on four districts, but the stoppage covered the whole state at once.

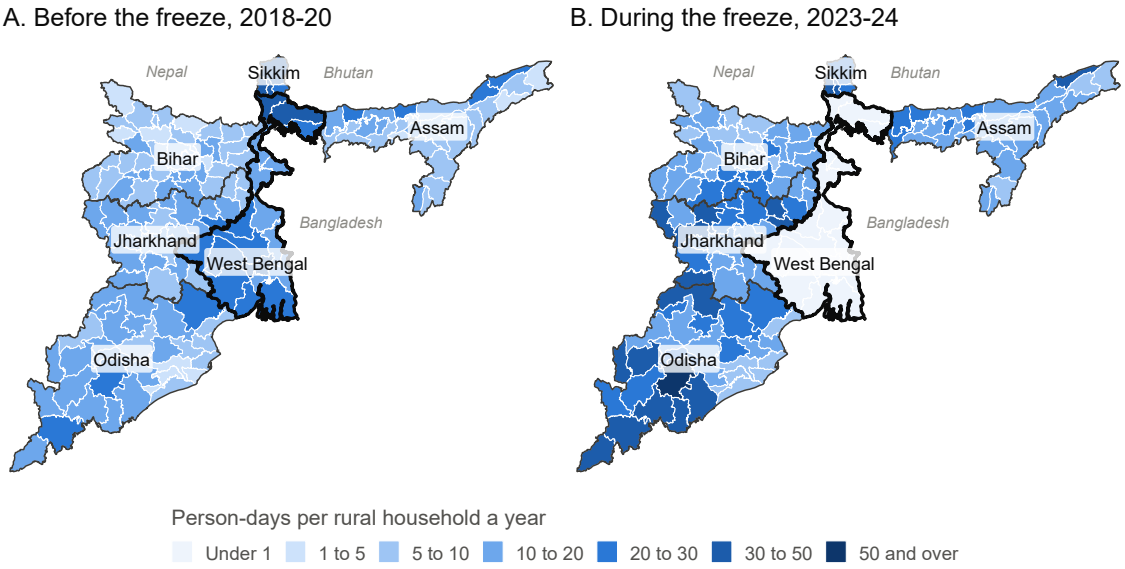


Figure 1: MGNREGA work in West Bengal and its neighbors, by district

Person-days per rural household a year, by 2011 district. Panel A is the average of 2018-19 and 2019-20. Panel B is 2023-24. Districts created after 2011 are added to the districts they came from. Person-days are from the Ministry of Rural Development's administrative records, and households from the 2011 census.

[1] Down To Earth dates the stoppage to 21 December 2021 and the monitoring group LibTech to 26 December 2021. On either date, our transition begins in December 2021.

Figure 2 shows what followed, as households kept working for several months in the hope of being paid, 131 million days between December and July, largely without pay. By August 2022 the work had effectively stopped. Between August 2022 and March 2023 the state recorded 4.2 million person-days against 228 million in the same months of 2018-20, a fall of 98 percent, and the fall reached every district (Figure A2). In the neighboring states the scheme kept running and grew, with 71 percent more work in 2022-23 than in 2018-20. The courts ordered the scheme restarted in 2025, but work resumed only on 1 June 2026, three weeks after a new state government took office. A month later, on 1 July 2026, a national law passed in December 2025 replaced MGNREGA in every state. It raises the guaranteed days from 100 to 125 a year. Most states, however, now pay 40 percent of the cost and anything above a yearly allocation set by the central government, and each state must name up to 60 days a year, in the peak farming seasons, when no work is offered. How much work states will cut under these rules is not yet known (PRS Legislative Research, 2025; Ministry of Rural Development, 2026). The freeze was a far larger loss than these pauses, and it comes closest to a state that stops offering work because it cannot pay its share.

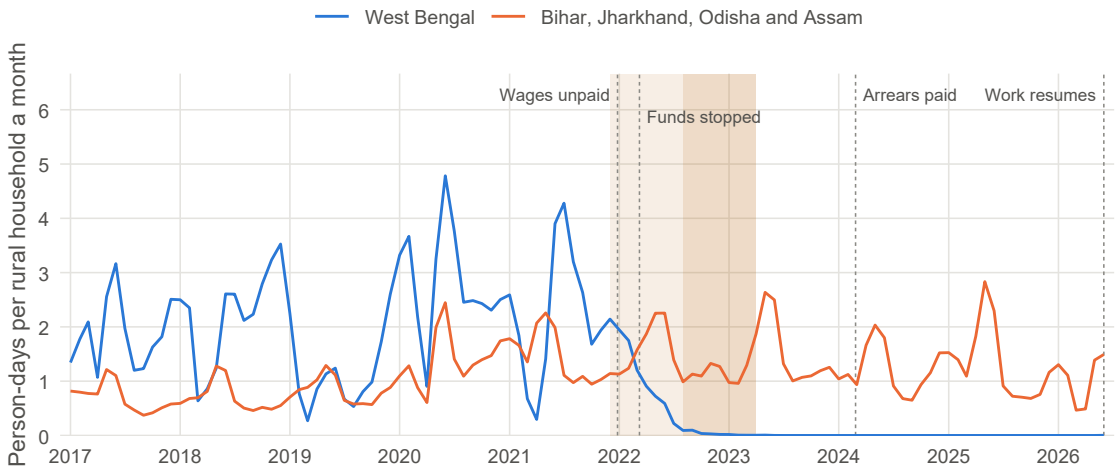


Figure 2: MGNREGA work by month

Person-days per 2011 rural household. Light shading marks the transition, December 2021 to July 2022. Dark shading marks the after period, August 2022 to March 2023. Person-days are from the Ministry of Rural Development's administrative records.

West Bengal responded in three ways (Table A1). Three months before wages stopped it had begun paying Lakshmir Bhandar, a monthly transfer to women aged 25 to 60, at Rs 500 and at Rs 1,000 for Scheduled Caste and Tribe women. It gave job card holders some work on its own projects, later claiming 36 percent of the earlier person-days for 2022-23, with no right to demand it and no public figures by district. And in February 2024 it set aside Rs 37 billion to pay 2.1 million job card holders the wages the central government had not paid, about Rs 17,600 each, an event we use below. Funds for the rural housing scheme also stopped, so we read our

estimates as the effect of losing the guarantee together with this linked money.

The freeze also met a dry year. The monsoon of 2022 was poorer in West Bengal than in any neighboring state, with a drought index below -1 in 14 of its 19 districts (Figure A3). A year like this is when the scheme used to matter most (Fetzer, 2020). Inside the state the driest districts relied least on the scheme, which is one reason our comparisons work inside districts, where the monsoon is common to everyone.

2.3 What Should Happen

A household's spending in any month must equal its income plus what it borrows and what it draws from savings,

$$c = y + b^u + b^g + b^i + a, \tag{1}$$

where b^u is unsecured formal credit, b^g is formal credit against gold, b^i is informal credit and a is savings drawn down. The guarantee enters this identity through income and through the limits on credit, and each gives a prediction.

The first concerns income, which falls by the scheme's wages and may fall by more. Displaced workers look for private work, and since public work crowded out private work about one for one at rollout (Imbert and Papp, 2015), market wages should fall, most in the dry months. Some workers can leave, since the scheme had halved short-term migration where it ran at scale (Imbert and Papp, 2020). Wages should fall further where workers cannot easily borrow or leave (Jayachandran, 2006).

The second concerns credit, since lenders limit unsecured credit to what they expect to recover, which rises with the lowest income a borrower can fall back on, $b^u \leq \kappa\, \underline{y}(g)$, as in the model of Surendra (2020). Losing the guarantee g lowers this limit just as demand rises, so personal, business and farm loans should fall even as applications rise. Credit against gold is limited only by the pledge, $b^g \leq \lambda G$, where G is the value of the gold and λ the lending ceiling, 75 percent for most of our sample. The guarantee does not enter this limit, so gold loans should rise, mostly once savings run out, in the months when the scheme used to provide work, and less where households hold little gold. The size can be judged too, since in the villages of Bell and Mukhopadhyay (2015) about half of a lost wage came back as borrowing, and banks and self-help groups together supplied 12 percent of consumption loans, so formal lenders would replace about 6 percent of lost wages if the same shares held here. Since these two movements oppose each other, and both are small beside the loans that a guarantee does not touch, total lending is the wrong place to look.

The third concerns spending, which takes whatever the other terms leave. It should fall by less than income. A small fall need not mean a small loss. Chetty and Looney (2006) show that the gain from insurance is about risk aversion times the fall in consumption, and these two can move in opposite directions, so a small fall can mean good private insurance or a household that cannot afford to let spending drop.

The back pay of 2024 provides a test. If households pledge gold to fill a gap in income, a payment of old wages should narrow the response in the same blocks. A rise that came from a credit cycle or a trend in poorer places would not turn at that date.

3 Data and Methods

3.1 Data

We measure formal borrowing in the records of TransUnion CIBIL, a credit bureau to which formal lenders report, accessed through the Centre for Advanced Financial Research and Learning. The data are new formal loans by pincode (postal code), month, product, lender group, borrower's sex and size, which we place into blocks by population. Our sample runs from January 2017 to March 2023, for 66 months, 50 of them before the freeze. We leave out nine months of COVID, April to September 2020 and April to June 2021. In the first spell the national lockdown and the loan moratorium of the Reserve Bank of India stopped most new lending, and new consumer durable loans in our data fell by 99 percent in April 2020. The second spell covers the second wave. In these months borrowing reflected whether lenders could lend more than what households needed. They also saw migrants return to the villages, and scheme work in the state reached 414 million person-days in 2020-21, against 338 million the year before, so they would place an unusual shock to reliance inside the period our comparison rests on. We stop in March 2023 because from 2023-24 the state claimed large amounts of work of its own, which cannot be placed by block, and we use later months only for the back pay. Our main outcome is small bank gold, meaning loans against gold under Rs 50,000 from public and private banks. A gold loan needs no proof of income or credit history, so it is the formal product that a household short of cash can most easily reach, and its size suits a household replacing wages, at about Rs 30,000 on average. Over our sample the bureau records 761,000 such loans, or Rs 22.54 per household a month, and women took 33 percent of the rupees.

Our measure of reliance, R_b, is person-days in block b per household a year, averaged over 2018-19 and 2019-20, the last stable years before the freeze, with households from the 2011 census. The person-days come from the Ministry's administrative records by month and block. A block with 10 more days of reliance lost Rs 159 more per household a month at the 2019-20 wage of Rs 191. Figure 3 maps it, and the mean block has 21.5 days, the 10th percentile 9.3 and the 90th 36.7, and 57 percent of the variance lies inside districts, which is the variation we use. As Marquez-Padilla et al. (2025) leave out large cities, we leave out 19 of the state's 341 blocks in which municipal towns dominate. This gives 322 blocks and 14.7 million households. Recorded days can overstate real work (Imbert and Papp, 2015; Drèze and Somanchi, 2025), which would attenuate our estimates if the padding were random.

To see what the bureau cannot, we use the Consumer Pyramids Household Survey of the Centre for Monitoring Indian Economy, a panel that visits each household every four months.

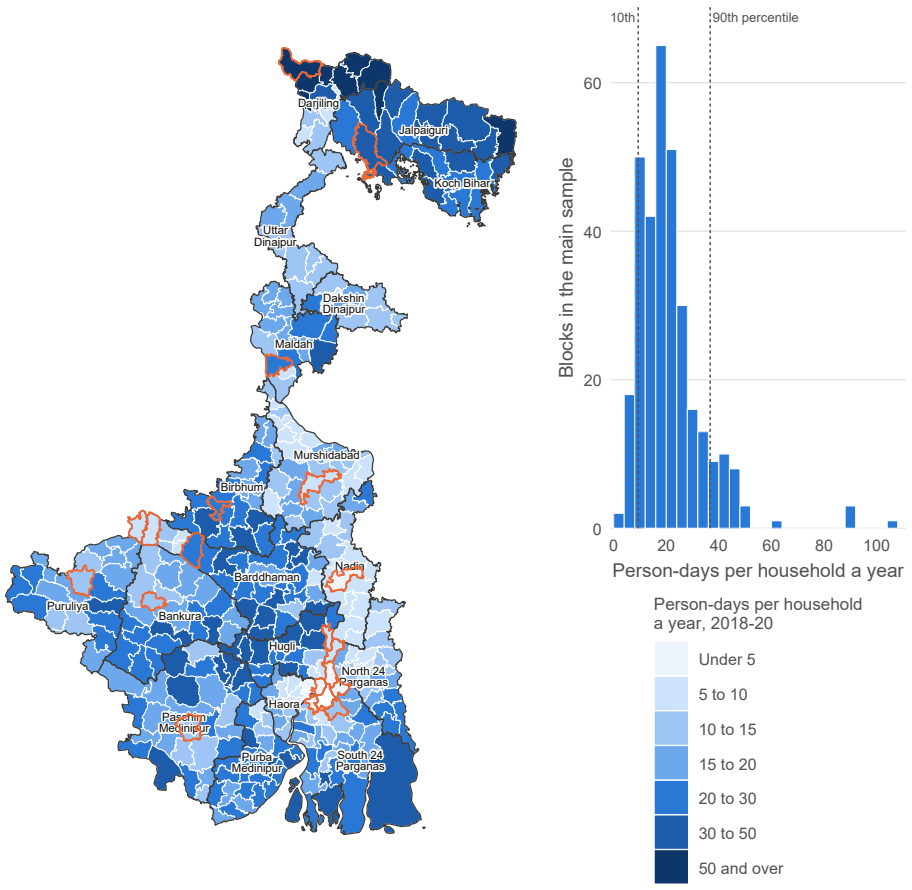


Figure 3: Reliance on MGNREGA by block, West Bengal, 2018-20

Person-days per 2011 household a year, average of 2018-19 and 2019-20, for 341 blocks. Dark lines mark 2011 districts. Orange outlines mark the 19 town-dominated blocks left out of the main sample. Kolkata has no block. The histogram covers the 322 blocks of the main sample.

In 2019 it interviewed about 2,900 rural households a wave in all 18 of West Bengal's rural districts. Each interview records income by source and spending by category for each of the previous four months, in rupees, along with each member's work and any member who has left, with the reason. We use interviews from January 2017 to April 2023 for our main estimates, and to April 2024 for the event studies, and we drop March to August 2020, when response collapsed. The district is the finest geography it releases, so reliance here is R_d, the district's person-days per household.

We group households by the survey's occupation groups in 2019, so that the grouping cannot respond to the freeze, and we call agricultural laborers, wage laborers and small and marginal farmers laboring households. They were 47 percent of rural households. They earned Rs 10,752 a month, 91 percent of it in wages, and spent Rs 8,273, 54 percent of it on food. In the labor force survey, pooled over 2017-18 to 2019-20, they did about 75 percent of the scheme's days, so a laboring household at mean reliance worked about 34.5 days a year, and each 10 days of reliance cost it 11.4 more days than other households, worth Rs 182 a month.

The panel has limits that shape what we claim. Its debt module records only yes or no, and almost nobody in rural West Bengal reports a loan from a moneylender or a relative, so we do not claim to measure informal credit in rupees. Almost every household reports holding gold and almost none reports adding to it, so we measure gold through the bureau. The panel misses many of the poorest households (Somanchi, 2021) and undercounts women's work (Afridi et al., 2022), and interviewers reconcile income with spending. We use changes within households, which guards against errors in levels, and we check the results against an official consumption survey.

For wages we use the Periodic Labour Force Survey, which records the earnings of casual workers by district from 2017-18 to 2022-23, and the Labour Bureau's monthly Wage Rates in Rural India, which lets us compare West Bengal with its neighbors. Table A5 lists every source, and Appendix A gives more detail.

3.2 Design and Estimation

Work stopped in two stages, and we set our windows by them. From December 2021 to July 2022 people still worked, largely without pay, and households could expect the wages to come, which we call the transition. From August 2022 to March 2023 there was almost no work in any district, and we call this the after period.

For credit we follow the dose-response specification of Marquez-Padilla et al. (2025). For block b in district d and month t,

$$y_{bdt} = \alpha_b + \gamma_{dt} + \sigma_t \mathrm{SC}_b + \eta_t \mathrm{ST}_b + \beta_1 R_b \mathbf{1}_{\text{Transition}} + \beta_2 R_b \mathbf{1}_{\text{After}} + \varepsilon_{bdt}, \tag{2}$$

where R_b is reliance in tens of days, α_b are block effects and γ_{dt} are district-by-month effects, so

we compare only blocks in the same district in the same month. The two trait terms let blocks with more Scheduled Caste or Tribe people follow their own path, because Lakshmir Bhandar paid those women double and because tribal share and reliance go together. We read β_2 as an average causal response to reliance, which requires strong parallel trends (Callaway et al., 2024). Every block lost some work, so the comparison is always with the least reliant. Our leading scale is percent, by Poisson, because a statewide rise in gold prices or in the share borrowing multiplies every block by the same factor (Chen and Roth, 2024; Roth and Sant'Anna, 2023). We also report rupees by least squares, to compare with lost wages. To trace timing and prior trends we interact reliance with every month,

$$y_{bdt} = \alpha_b + \gamma_{dt} + \sigma_t \mathrm{SC}_b + \eta_t \mathrm{ST}_b + \sum_q \theta_q R_b \mathbf{1}_{t=q} + \varepsilon_{bdt}, \tag{3}$$

omitting November 2021 and plotting the coefficients with the pre-freeze months averaging zero. We cluster standard errors by block.

For households we compare laboring households with others in the same district and month, in districts that relied more and less on the scheme. For household h in district d and month t,

$$y_{hdt} = \alpha_h + \gamma_{dt} + \omega_{o(h)t} + \rho_{\ell(h,t)} + \beta_1^H R_d L_h \mathbf{1}_{\text{Transition}} + \beta_2^H R_d L_h \mathbf{1}_{\text{After}} + \varepsilon_{hdt}, \tag{4}$$

where L_h marks a laboring household in 2019, $\omega_{o(h)t}$ are effects for household h's occupation group $o(h)$ in each month, and $\rho_{\ell(h,t)}$ are effects for the months between month t and the interview that recorded it. This is a triple difference in the spirit of Ghosh and Vats (2026), with laboring households in place of landowners and district reliance in place of the state border. Reliance is fixed within a district and laboring status by 2019 occupation group, so district-by-month effects absorb reliance times each period, occupation-by-month effects absorb laboring status times each period, and household effects absorb reliance times laboring status. District-by-month effects absorb everything common to a district, including the monsoon, and occupation-by-month effects absorb anything common to an occupation across the state. The assumption is that the gap between laboring and other households would have moved alike across districts with more and less reliance. It does not need districts themselves to move alike. We cluster by district, and dividing a rupee estimate by Rs 182 gives the response per rupee of scheme wages lost.

4 Effects of the Freeze on Borrowing

4.1 Small Gold Loans

Figure 4 reports the event study for small bank gold. For the 50 months before the freeze the coefficients stay close to zero, with no drift in either direction. They stay flat through the first months of the transition, when wages had stopped and work had not. The gradient starts to rise

in the spring of 2022 and continues as work falls, reaching 6.5 log points in August to November 2022 and 9.5 in December 2022 to February 2023, the first busy season with no work at all. The timing fits households running through savings first and pledging gold when income stops and no end is in sight. It fits less easily with a general credit boom in poorer blocks, which would not choose those months.

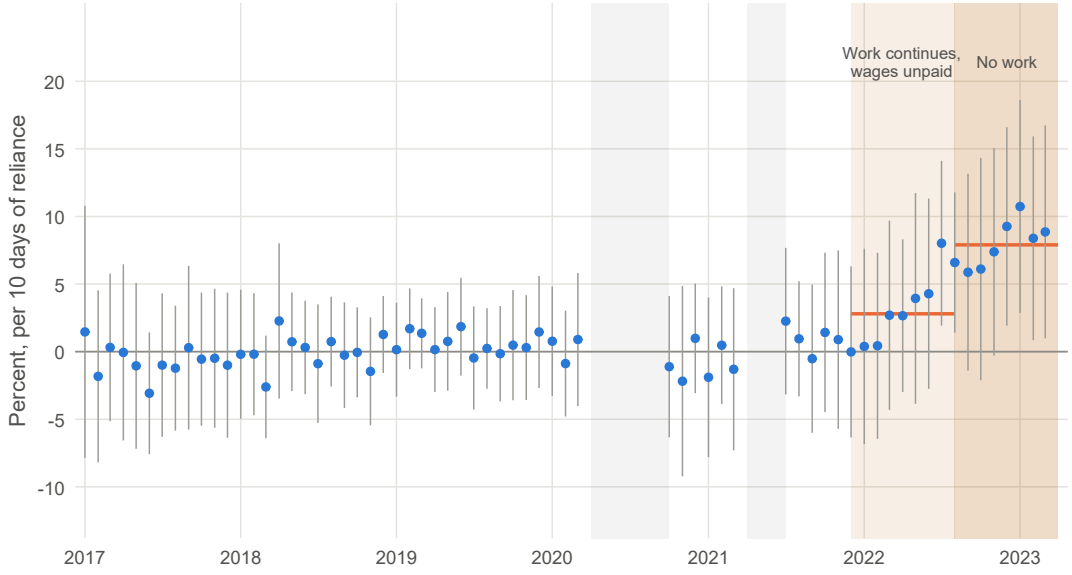


Figure 4: Event study for small bank gold

Coefficients θ_q from equation (3) estimated by Poisson, with 95 percent confidence intervals based on standard errors clustered by block. Orange lines mark the estimates of β_1 and β_2 from equation (2). Gray bands mark the months left out for COVID. Regressions include block effects, district-by-month effects, and Scheduled Caste and Tribe shares by month. 322 blocks.

Table 1 summarizes these patterns, and the coefficient on reliance in the transition is small and not significant. The coefficient in the after period is 0.079 (standard error 0.032, $p = 0.014$), so 10 more days of reliance raised small gold lending by 8.2 percent, with a 95 percent interval of 1.6 to 15.2 percent, and between blocks at the 10th and 90th percentile, 27.4 days apart, by 24 percent. In rupees the coefficient is 3.10, and the number of loans rises too, by 0.104 per 1,000 households a month on a mean of 0.75, so the response comes from more loans and not from larger ones.[2]

Women's borrowing drives the response, as Figure 5 shows. Their coefficient is 0.127 ($p = 0.007$), which implies 13.5 percent per 10 days and 42 percent between the 10th and 90th percentile blocks. Men's is 0.049 ($p = 0.17$), or 5.0 percent per 10 days. Women hold a large part of household gold, and they are also the people whose work the scheme moved most (Afridi et al., 2022). Large gold loans, which look like farm or business credit, move the other way by 2.8 percent, and all bank gold barely moves. A study of all gold loans would have found little.

[2]With 18 districts, clustering by district is a stricter test. A wild cluster bootstrap by district gives $p = 0.03$ for this coefficient.

Table 1: Effect of the freeze on bank gold loans, by loan size and borrower's sex

	Small bank gold (1)	Large bank gold (2)	All bank gold (3)
A. All borrowers			
Reliance × Transition	0.028	-0.012	-0.007
	[0.028]	[0.015]	[0.015]
Reliance × After	0.079**	-0.028	-0.014
	[0.032]	[0.018]	[0.018]
Reliance × After, rupees	3.10***	-7.40	-4.30
	[0.80]	[12.33]	[12.78]
Dependent variable mean, rupees	22.54	114.14	136.68
N	21,252	21,252	21,252
B. Women			
Reliance × Transition	0.044	-0.006	0.002
	[0.041]	[0.022]	[0.021]
Reliance × After	0.127***	-0.012	0.009
	[0.047]	[0.026]	[0.024]
Reliance × After, rupees	1.90***	-1.60	0.30
	[0.36]	[4.70]	[4.80]
Dependent variable mean, rupees	7.22	33.41	40.63
N	21,252	21,252	21,252
C. Men			
Reliance × Transition	0.018	-0.015	-0.011
	[0.031]	[0.018]	[0.017]
Reliance × After	0.049	-0.036*	-0.025
	[0.036]	[0.021]	[0.020]
Reliance × After, rupees	1.20**	-5.80	-4.60
	[0.48]	[9.80]	[10.00]
Dependent variable mean, rupees	15.21	80.38	95.59
N	21,252	21,252	21,252

Brackets contain standard errors clustered by block. Reliance is in tens of person-days per household a year. The first two rows of each panel are estimated by Poisson and read as log points per 10 days. The third row is by least squares, in rupees per household a month. All regressions include block effects, district-by-month effects, and Scheduled Caste and Tribe shares by month. Small loans are under Rs 50,000. Sample excludes nine COVID months and 19 town-dominated blocks. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

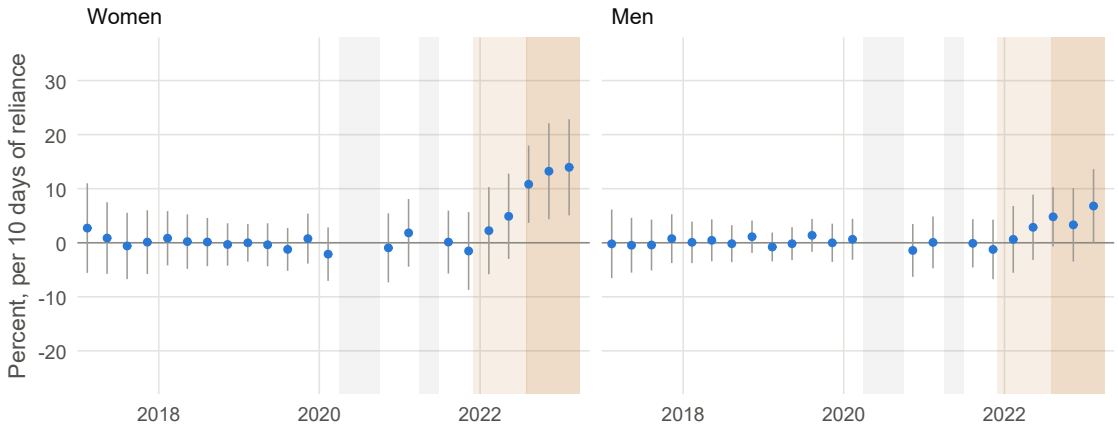


Figure 5: Small bank gold, women and men

Quarterly coefficients from equation (3) estimated by Poisson, with 95 percent confidence intervals based on standard errors clustered by block. Small loans are under Rs 50,000. Shading as in Figure 4. Figure A12 adds large gold and all bank gold.

Figure 6 shows the main result as a picture. Panel A maps each block's reliance against the mean of its own district, which is the variation our design uses. Panel B maps the change in small bank gold from before the freeze to the after period, also against the district mean. The two maps pick out the same blocks more often than not. Within the Medinipur districts, within the Twenty Four Parganas and within Barddhaman, blocks that relied more than their neighbors saw gold lending rise more than their neighbors did.

The limit on this way out is the gold itself. A gold loan needs gold and a bank nearby, and the places that lost the most had less of both. Table 2 splits blocks at the median by tribal share, by development and by small gold lending before the freeze, and the sharpest split is the last. Blocks with little gold lending relied more on the scheme, at 24.2 days against 18.7, yet women's borrowing there rose by 6.4 percent per 10 days against 18.1 percent elsewhere. Tribal blocks show the same pattern, at 6.0 against 16.3 percent, although they too relied more on the scheme. Bank gold lending is thinnest in Purulia and Bankura and in the northern hills, where some of the state's highest reliance is found (Figure A8). A weak response in those blocks should not be read as a lack of need. Borrowing against gold was open to households that already had gold and a bank nearby.

4.2 Credit Resting on Future Income

If the guarantee worked as collateral, lenders should have pulled back from loans that rest on future income. These include personal loans, which here means small unsecured loans under Rs 50,000, business loans and farm credit. Figure 7 and Table 3 show that they did, with flat trends before the freeze. After it, 10 more days of reliance lowers personal loans by 2.8 percent ($p = 0.03$), business loans by 3.5 percent and farm loans by 4.0 percent, the last two less precisely.

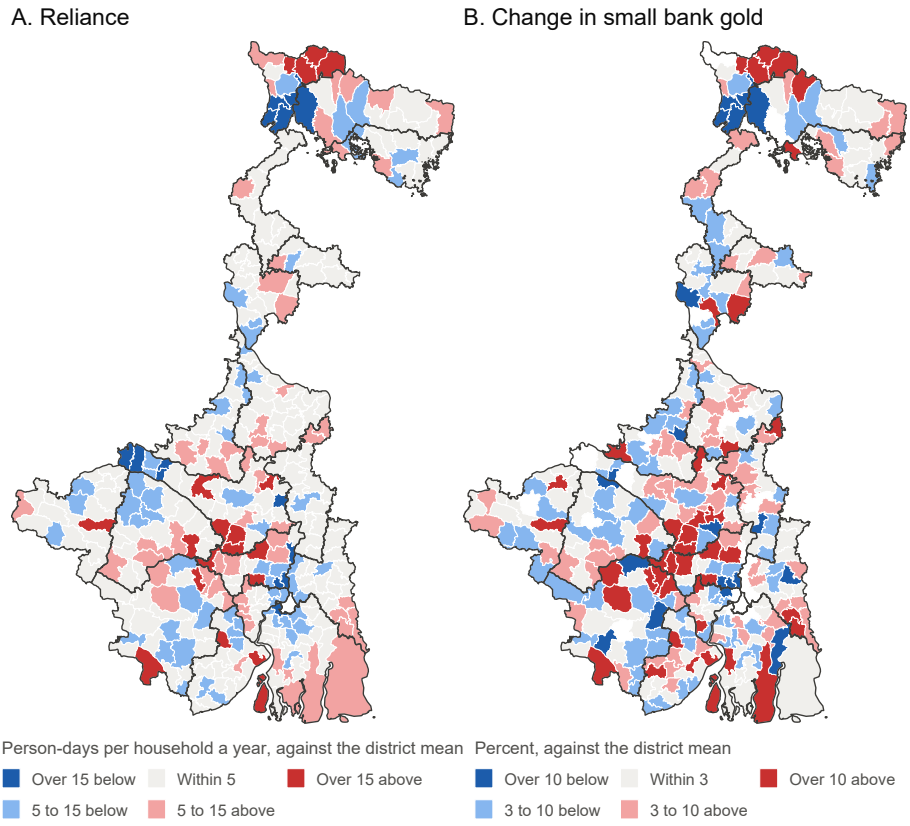


Figure 6: Reliance and the change in small bank gold, each against its district

Panel A shows each block's reliance less its district's mean. Panel B shows the percent change in small bank gold per household from before the freeze to the after period, less its district's mean. Blocks left out of the main sample and Kolkata are blank in Panel B. Dark lines mark 2011 districts.

Table 2: Where gold could and could not fill the gap

	Tribal share		Development		Gold before the freeze	
	High (1)	Low (2)	Low (3)	High (4)	Low (5)	High (6)
A. Women						
Reliance × After	0.058	0.151***	0.148**	0.098	0.062	0.166***
	[0.068]	[0.056]	[0.062]	[0.066]	[0.085]	[0.053]
Dependent variable mean, rupees	5.02	9.41	7.11	7.33	2.57	11.86
Mean reliance, days	24.6	18.4	21.7	21.3	24.2	18.7
N	10,626	10,626	10,626	10,626	10,626	10,626
B. Men						
Reliance × After	0.021	0.066	0.071	0.031	0.024	0.071*
	[0.055]	[0.044]	[0.050]	[0.052]	[0.066]	[0.042]
Dependent variable mean, rupees	12.65	17.76	15.50	14.92	6.22	24.19
Mean reliance, days	24.6	18.4	21.7	21.3	24.2	18.7
N	10,626	10,626	10,626	10,626	10,626	10,626

Brackets contain standard errors clustered by block. Dependent variable is small bank gold per household a month, by Poisson. Each column splits the 322 blocks at the median of the trait named above it. Gold before the freeze is small bank gold per household in April 2018 to March 2019. All regressions include block effects, district-by-month effects, Scheduled Caste and Tribe shares by month, and the transition interaction. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Between the 10th and 90th percentile blocks the fall in personal loans is 7 percent, close to the low end of the 5.9 to 27 percent rise that Chaturvedi and Rauf (2025) report for the rollout, which we read as an upper bound for a reversal, since accounts and credit histories survive a funding freeze.

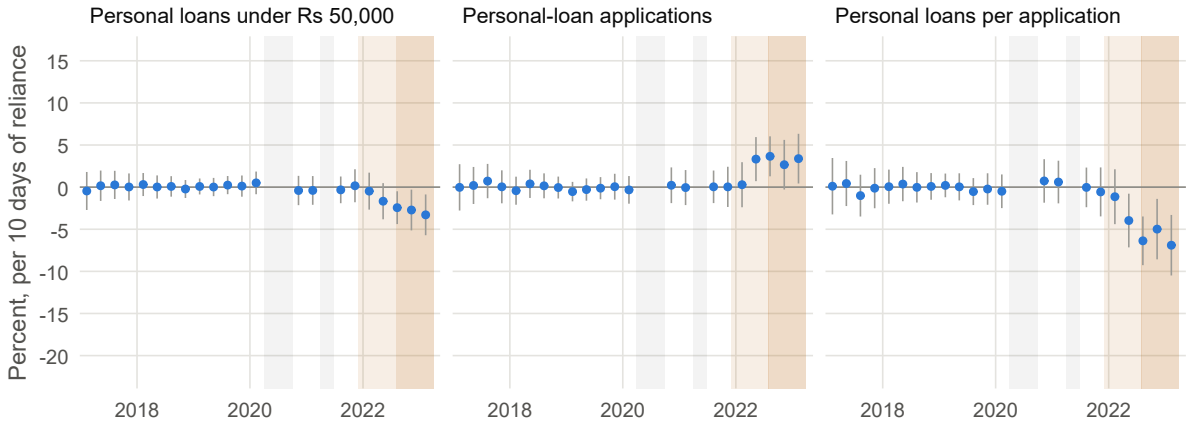


Figure 7: Personal loans and applications

Quarterly coefficients from equation (3) estimated by Poisson, with 95 percent confidence intervals based on standard errors clustered by block. Shading as in Figure 4. Personal loans per application is the difference between the other two panels. Figure A18 adds business and farm loans and all applications.

Lower lending could reflect less demand as well as less supply, and applications help tell the two apart, though they cannot settle it. The bureau records every inquiry a lender makes, which

we use as a measure of applications, following Ghosh and Vats (2026). An inquiry is not exactly an application, since one borrower comparing lenders can produce several. Applications rose, by 2.9 percent per 10 days of reliance for personal loans and 3.0 percent for all loans, starting during the transition, earlier than the rise in gold. So personal loans per application fall by 5.5 percent per 10 days ($p = 0.003$), or 14 percent between the 10th and the 90th percentile blocks. Households in high-reliance blocks were asking for more credit than before and ending up with fewer loans. We do not observe approvals or rejections, so this fits lenders saying yes less often but does not show it. Ghosh and Vats (2026) find that inquiries rise and acceptance does not change when a transfer arrives. Here loans per application are what move, which is consistent with lenders responding to the withdrawal, although rising demand from riskier applicants would produce the same pattern. Loans already made also went bad more often, with defaults on farm, personal and business loans, with microfinance, up 0.29 points per 10 days on a mean of 5.1 percent, while gold loans, where the pledge enforces repayment, do not move significantly (Table A12).

Table 3: Effect of the freeze on other credit and on applications

	Personal loans (1)	Business loans (2)	Farm loans (3)	All new loans (4)	All applications (5)	Personal loans per application (6)
Reliance × Transition	-0.011	-0.007	-0.013	-0.002	0.024***	-0.032**
	[0.010]	[0.019]	[0.023]	[0.006]	[0.009]	[0.016]
Reliance × After	-0.028**	-0.036	-0.041	-0.005	0.030***	-0.057***
	[0.013]	[0.024]	[0.028]	[0.007]	[0.011]	[0.019]
Mean	38.16	222.97	105.80	2904.76	33.27	0.56
N	21,252	21,252	21,252	21,252	21,252	21,252

Brackets contain standard errors clustered by block. All regressions are Poisson, with coefficients as log points per 10 days of reliance, and include block effects, district-by-month effects, and Scheduled Caste and Tribe shares by month. Means are rupees per household a month in columns (1) to (4), applications per 1,000 households a month in column (5) and loans per application in column (6). Personal loans are under Rs 50,000. Applications are bureau inquiries. All new loans leave out gold from non-bank lenders, which report with breaks. Column (6) is the difference of two Poisson coefficients, with a standard error from a stacked regression. Table A9 splits these by sex. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The total hides all of this, since all new loans fall by 0.5 percent per 10 days, with a 95 percent interval of a 1.8 percent fall to a 0.8 percent rise. The reason is not that the rise in gold offsets the falls, since at Rs 3.1 per household a month it is far smaller than they are. It is that personal, business, farm and gold loans together are about Rs 500 of the Rs 2,900 a household borrows in a month, and the rest, in mortgages, vehicle loans, cards and larger loans, did not move (Table A8). A regression on total credit would conclude that nothing had happened, and Bell and Mukhopadhyay (2015) warn against debt totals for this reason.

4.3 The Return of the Wages

The back pay offers a sharper test. In February 2024, after more than two years, the state paid wages owed to job card holders, and blocks that had worked more on the scheme were owed more.[3] If gold borrowing fills a gap in income, the gradient should narrow once income returns, and Figure 8 shows that it does. From April 2023 to February 2024 the coefficient is 9.5 percent per 10 days, so the response had not faded. The state claimed 280 million person-days of its own work for 2023-24, near the scheme's old level. A gradient that persists through those months says that whatever work there was did not reach the blocks that had relied most, and if the claim were true it would bias this test toward finding no reversal. In March to June 2024 it falls to 3.9 percent, a drop of 5.3 log points ($p = 0.05$), and for women it falls from 15.3 to 6.3 percent. The reversal is hard to square with any explanation based on trends in poorer places, since those would not turn in March 2024. The state's new work began in the same months, so some of the fall may be due to work and not to the arrears.

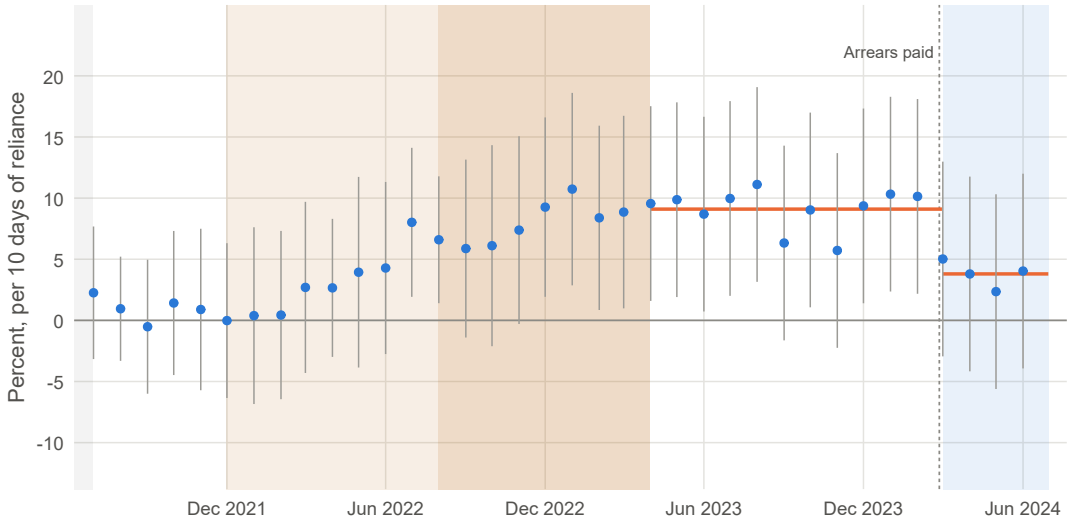


Figure 8: Small bank gold around the back pay of February 2024

Coefficients from equation (3) estimated by Poisson, extended to June 2024, with 95 percent confidence intervals based on standard errors clustered by block. Orange lines mark the averages for April 2023 to February 2024 and March to June 2024. The blue band marks the months after the state began paying arrears. Karmashree, the state's work scheme, was notified on 7 March 2024 with work to begin that May, so the last two months may also reflect it.

4.4 Checks

Figure 9 reports the estimate across eight specifications, including month effects alone, no trait terms, a development index or pre-freeze gold interacted with month, a linear trend in reliance

[3]Our reliance measure is from 2018-20. It proxies for the wages owed from late 2021, since districts kept their rank into 2021-22 and blocks kept their place inside districts from 2018-19 to 2019-20. A direct measure would need block records for 2021-22.

and the addition of town-dominated blocks. The coefficient on small gold stays between 0.063 and 0.094 for all borrowers and between 0.102 and 0.148 for women, always significant. It is smallest when town-dominated blocks return, as expected, since they add towns where the scheme barely mattered. Men's is never significant, and we consider it insufficiently robust. Keeping the nine COVID months changes the estimate by about 2 percent, and starting the sample in January 2015 raises it by about 11 percent.

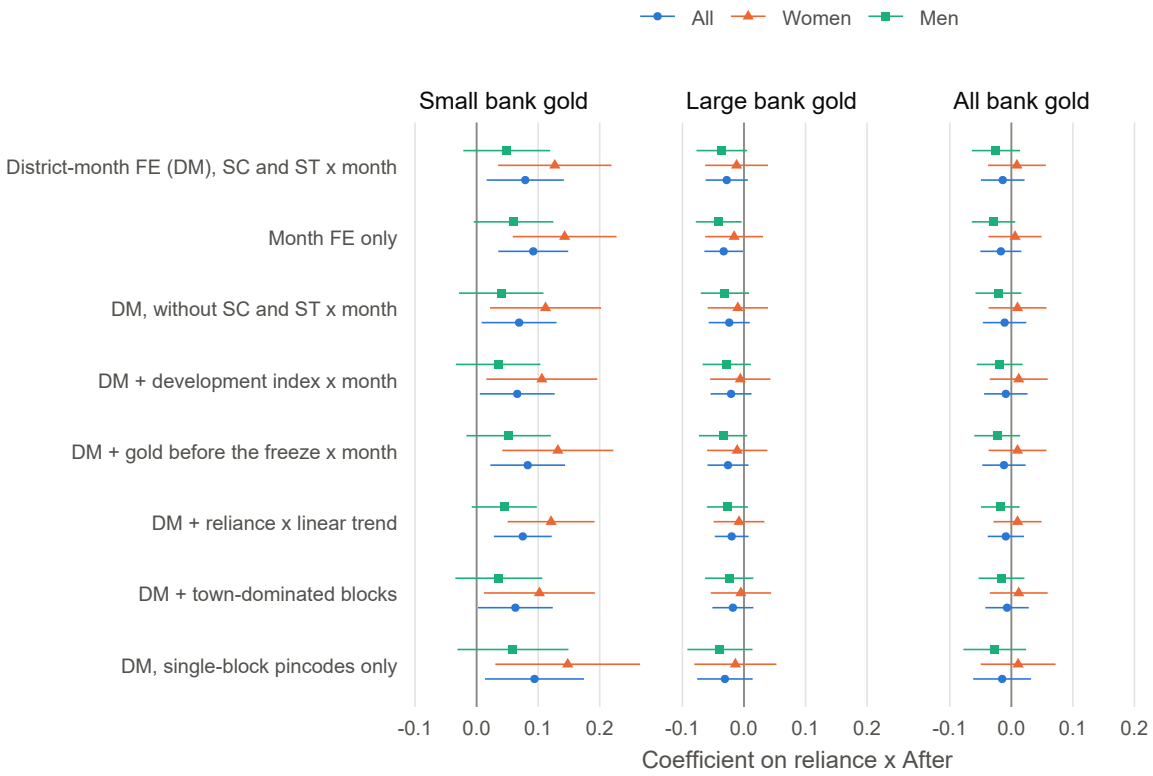


Figure 9: Effect of the freeze on gold loans, alternative specifications

Point estimates and 95 percent confidence intervals of β_2 in equation (2), estimated by Poisson, with standard errors clustered by block. Every regression includes block effects and the transition interaction. DM means district-by-month effects. The development index is the first principal component of the census-town share, literacy and the share of agricultural laborers in 2011. Single-block pincodes have at least 90 percent of their people in one block.

Appendix B reports the other checks, in which leaving out one district at a time keeps it between 0.065 and 0.097 (Figure A13). The interval first covers zero only when we allow a break in trend 1.6 times the largest move before the freeze (Rambachan and Roth, 2023). The same design in Maharashtra, Bihar and Jharkhand, where the work did not stop, shows no rise at the same date (Figure A16). The freeze did not change who appears in the files, and credit cards, mortgages and vehicle loans do not move (Table A8). Adding a stand-in for Lakshmir Bhandar lowers women's rupee coefficient only from Rs 1.9 to Rs 1.7 (Table A7).

Everything so far compares places inside West Bengal, which would miss anything that

moved the whole state. We therefore also compare pincodes on either side of the state's border, in subdistricts within 30 kilometers of it, with the district-pair comparison of Ghosh and Vats (2026) (Figure 10, Table A6). In the after period small gold is 14.6 percent higher on the West Bengal side and personal loans are 5.8 percent lower. The within-district slopes, taken to mean reliance, imply 18 and 6 percent, so two designs with different weaknesses arrive at similar answers.

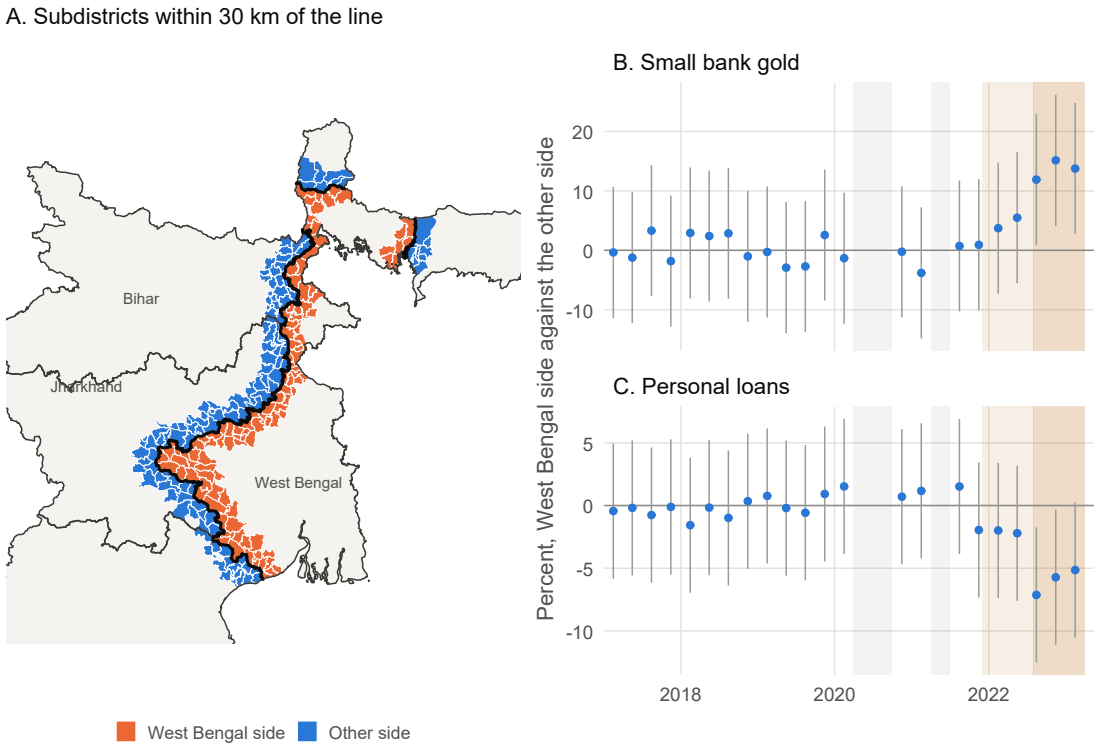


Figure 10: Credit on either side of West Bengal's border

Panel A maps the subdistricts whose pincodes form the sample, those whose centroid lies within 30 kilometers of West Bengal's land border with Bihar, Jharkhand, Odisha, Assam and Sikkim. Panels B and C show quarterly coefficients from equation (A1), estimated by Poisson, with 95 percent confidence intervals based on standard errors clustered by district pair. Shading as in Figure 4.

5 Effects of the Freeze on Wages and Spending

Since formal credit moved little in total, this section asks where the loss landed, and the next puts the two together.

5.1 Wages and Work

A job guarantee sets a floor under private wages, so its loss should reach workers who never took a day of scheme work, and Figure 11 shows that it did. In the Labour Bureau's monthly

series, West Bengal's agricultural wages moved with those of its neighbors for five years, began to fall behind in early 2022, and sat 3.8 percent lower in the after period.

Inside the state, the labor force survey records casual wages by district, so the comparison is across districts. Following Imbert and Papp (2015), who control for rainfall in the same way, for worker i in district d and quarter q we estimate

$$\ln w_{idq} = \alpha_d + \tau_q + \theta_1 S_{dq} + \theta_2 S_{dq}^2 + \beta_1^W R_d \mathbf{1}_{\text{Transition}} + \beta_2^W R_d \mathbf{1}_{\text{After}} + X_i'\lambda + \varepsilon_{idq}, \qquad (5)$$

where S_{dq} is the district's drought index in the quarter and X_i are worker controls. A comparison across districts would otherwise mix the freeze with the 2022 monsoon, since the driest districts relied least on the scheme. The assumption is that casual wages in more and less reliant districts would have moved alike once the drought is allowed for. Unlike equation (4), this needs whole districts to move alike, and with 18 districts it is the weaker of our designs.

Across West Bengal's districts, rural casual daily wages fall by 1.7 percent per 10 days of reliance (Table 4, column 1), or 4.5 percent between the 10th and the 90th percentile. The fall is 2.4 percent in January to March, when the scheme used to offer the most work, and 0.9 percent in August to December. Imbert and Papp (2015) find a gain of 4.7 to 9 percent in the dry season at rollout, so a fall of this size is what their estimates would predict in reverse. Wages fell more where bank deposits were thinner, 2.5 percent against 0.9 percent, as Jayachandran (2006) would lead us to expect.

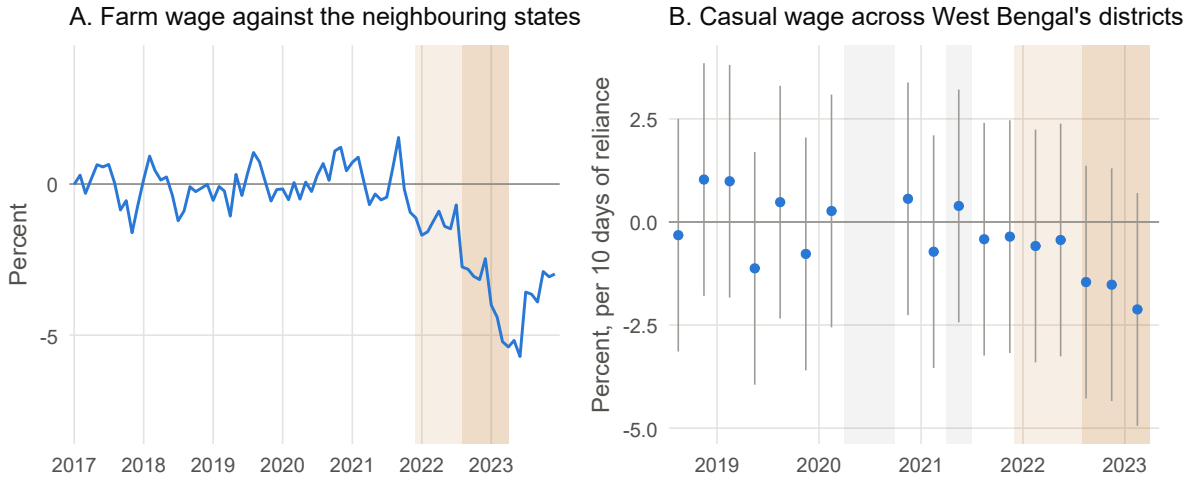


Figure 11: Rural wages in West Bengal

Panel A shows the percent gap in the monthly daily wage of male general agricultural laborers between West Bengal and the mean of Bihar, Jharkhand, Odisha and Assam, against the same gap before December 2021, from the Labour Bureau's Wage Rates in Rural India. Panel B shows quarterly coefficients on reliance for rural casual daily wages from the Periodic Labour Force Survey, from a version of equation (5) with district and quarter effects and the drought index and 95 percent confidence intervals based on standard errors clustered by district. Shading as in Figure 4.

Workers adjusted in different ways, and laboring women in high-reliance districts are less likely to report any work, by 0.8 points per 10 days on a mean of 13 percent, and men move into daily wage work (columns 2 and 3), while others left. The panel's roster records 1.1 more members gone for work per 100 laboring households per 10 days of reliance, on a mean of 6.5 (column 4). They are 97 percent men, with a mean age of 25. Imbert and Papp (2020) find that 7.3 public days per adult a year lowered short-term migration by about 1.75 points, which implies a rise of this order, and the private transfers below suggest that money came back. Appendix C gives more detail.

Table 4: How workers adjusted

	Log casual daily wage (1)	Women employed (2)	Men in daily wage work (3)	Members gone for work, per 100 households (4)
Per 10 days of reliance	-0.017**	-0.80**	0.90*	1.10**
	[0.008]	[0.40]	[0.50]	[0.45]
Mean	Rs 265	12.8	55.5	6.5
N	31,400	58,200	57,800	44,900
Source	Labor force survey	Panel	Panel	Panel

Brackets contain standard errors clustered by district. Column (1) is β_2^W from equation (5), for rural casual workers in the Periodic Labour Force Survey, with district and quarter effects, the drought index and its square, and worker controls. Columns (2) to (4) are reliance times a laboring household times the after period from equation (4) at the wave, for adults aged 18 to 59 in columns (2) and (3). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

5.2 Income and Spending

Figure 12 reports event studies for the panel, and Table 5 summarizes them. The gap between laboring and other households moves alike across districts for five years, drifts down during the transition, and steps down when work stops. Per 10 days of reliance, laboring households' income falls by Rs 182 a month, or 1.7 percent, and between the 10th and 90th percentile this comes to 4.6 percent. Wages account for more than all of it, falling by Rs 225. Private transfers rise by Rs 24 from a low base, which is the sign that remittances should give, and own production and business rise a little. Scheme wages lost were Rs 182, so after everyone had adjusted, income fell by about what the scheme had paid.

Spending falls by less than income, by Rs 105 a month or 1.3 percent ($p = 0.03$), and between the 10th and 90th percentile by 3.4 percent. Figure 13 shows which categories gave way and by how much. Food, 54 percent of a laboring household's budget, falls 1.0 percent and accounts for 42 percent of the fall, less than its share. Health falls 3.4 percent, education 3.0 percent and clothing 3.8 percent. Households held on to food as far as they could, and they cut what they could put off, including care and schooling. Figure 14 puts these changes on a map with the

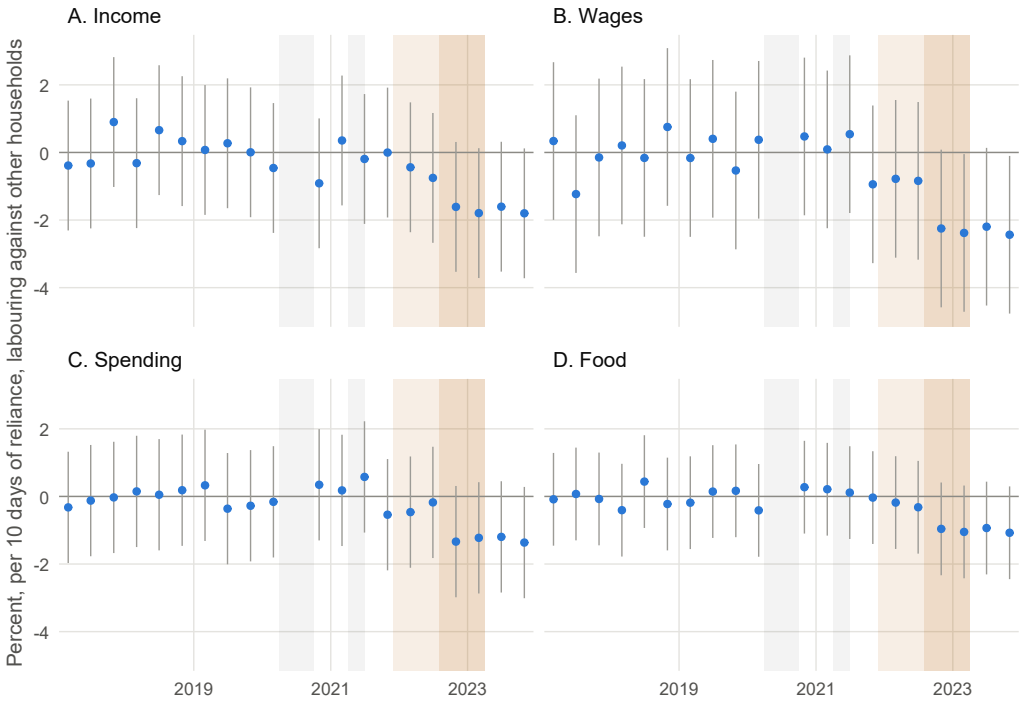


Figure 12: Event studies for laboring households

Coefficients on the interaction of reliance, a laboring household and each four-month wave from a version of equation (4), estimated by Poisson, with September to December 2021 omitted and the pre-freeze waves averaging zero. Lines are 95 percent confidence intervals based on standard errors clustered by district. Gray bands mark the months left out for COVID and survey quality. Light shading marks the transition and dark shading the after period. The last two waves lie after our main window.

Table 5: Effect of the freeze on laboring households' income and spending

A. Income, rupees a month	All	Wages	Own work	Private	State	Asset
$R \times L \times$ After, rupees	-182**	-225***	15	24**	4	2
	[75]	[84]	[11]	[10]	[5]	[2]
$R \times L \times$ After, log points	-0.017**	-0.023***	0.030	0.074**	0.044	
	[0.007]	[0.008]	[0.022]	[0.032]	[0.060]	
West Bengal $\times L \times$ After, log points	-0.034**	-0.046***	0.059	0.146**	0.378***	
	[0.013]	[0.016]	[0.042]	[0.061]	[0.071]	
Mean, laboring households, 2019	10,752	9,834	479	308	80	1.5
B. Spending, rupees a month	All	Food	Health	School	Clothes	Loans
$R \times L \times$ After, rupees	-105**	-45**	-13**	-10*	-13**	5
	[50]	[22]	[6]	[5]	[6]	[7]
$R \times L \times$ After, log points	-0.013**	-0.010**	-0.035**	-0.030*	-0.039**	0.016
	[0.006]	[0.005]	[0.015]	[0.016]	[0.016]	[0.024]
West Bengal $\times L \times$ After, log points	-0.025**	-0.020**	-0.068**	-0.060*	-0.077**	0.031
	[0.011]	[0.009]	[0.028]	[0.031]	[0.030]	[0.046]
Mean, laboring households, 2019	8,273	4,467	370	333	354	307
N, household-months, West Bengal			165,600			
N, with the four neighbors			618,900			

Brackets contain standard errors clustered by district. R is reliance in tens of days, and L marks a laboring household. Rows with rupees are by least squares, and rows with log points are by Poisson. All regressions use equation (4), January 2017 to March 2023 without March to August 2020, and the transition interaction is included and not shown. The row for West Bengal adds Bihar, Jharkhand, Odisha and Assam, with state-by-month effects, and its state transfers include Lakshmir Bhandar. Own work is own production and business profit. School is education spending, and loans are installments on loans. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

neighboring states beside them, and it shows the same hole as Figure 1.

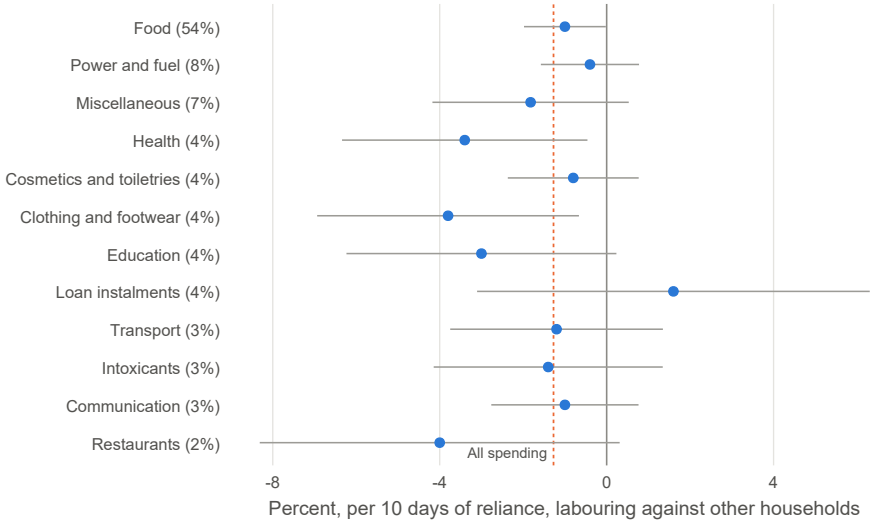


Figure 13: Which spending gave way

Coefficients from equation (4), by Poisson, converted to percent, with 95 percent confidence intervals based on standard errors clustered by district. Each category's share of laboring households' spending in 2019 is in brackets. The dashed line marks all spending. These categories add up to total spending.

Three checks support these estimates, beginning with the neighboring states. Laboring

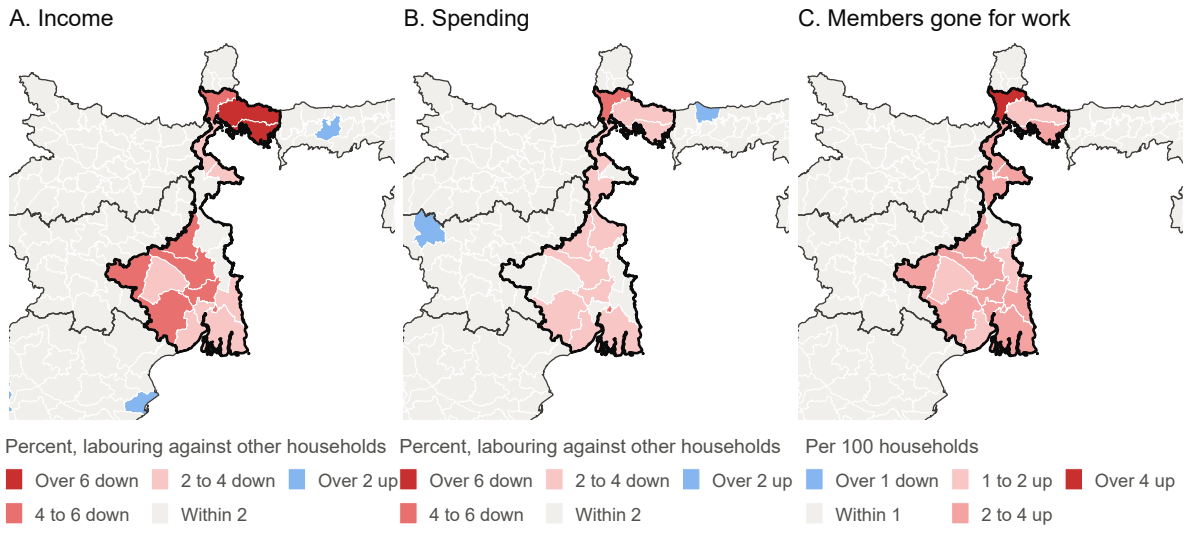


Figure 14: Laboring households in West Bengal and its neighbors

Each 2011 district's change from before the freeze to the after period in laboring households against other households in the same district, from the household panel, in percent in Panels A and B and in members per 100 households in Panel C. Heavy lines mark state borders, with West Bengal in black.

households in West Bengal, compared with those in Bihar, Jharkhand, Odisha and Assam, have income 3.3 percent lower and spending 2.5 percent lower, about what the slope implies at mean reliance, and this comparison is net of Lakshmir Bhandar. In the official consumption survey, which owes nothing to the panel's methods, spending by West Bengal's rural casual labor households grew 3.1 percent less than in the neighboring states between 2011-12 and 2022-24. And night lights, a record kept for other purposes, move the same way, falling 0.9 percent per 10 days of reliance (Appendix D). Interviews and attrition do not respond to the freeze, and salaried households do not move (Table A16).

6 How Costly Is the Loss For Poor Households?

We can now put these pieces together and ask how far the lost wages were replaced, and at what cost.

6.1 Where the Lost Wages Went

Figure 15 fills in equation (1), dividing each rupee estimate by the Rs 182 a month that each 10 days of reliance cost laboring households. Lower market wages took another Rs 52 for every Rs 100 of scheme wages lost. Lower wages also reached other households in the same district, and the comparison of equation (4) nets that part out, so we apply the fall in casual wages to the gap in casual earnings between the two groups. Laboring households earned about Rs 6,650

a month in casual wages outside the scheme. For other households we scale this by the share of men in daily wage work, 9.1 percent against 55.5 percent (Table A4), which gives about Rs 1,090 and a gap of about Rs 5,560. More private work and earnings from away returned Rs 29, which is what remains of the panel's fall in wage income once those two are taken out. Own production, transfers and asset sales returned Rs 24. Income therefore fell by about Rs 100.

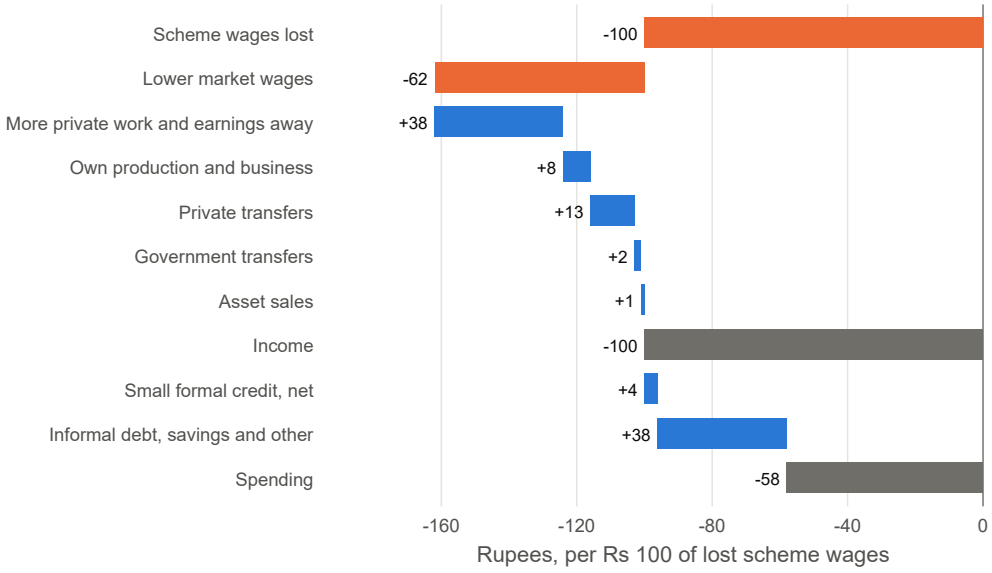


Figure 15: Where Rs 100 of lost scheme wages went

Rupees per Rs 100 of scheme wages lost by laboring households. Orange bars are losses, blue bars are what came back, and gray bars are totals. Income and spending are from the household panel, the market wage from the Periodic Labour Force Survey, and small formal credit from the credit bureau. Informal debt, savings and other is the residual. Table 6 gives the same numbers in rupees.

On the other side of the identity, spending fell by Rs 58, so households found Rs 42 from somewhere, of which formal credit gives about Rs 4. Small gold loans from banks and non-bank lenders together added Rs 5.7, about what the ratios in Bell and Mukhopadhyay (2015) suggest, and personal loans subtracted Rs 1.9. The figure would be smaller still if we counted the falls in large gold, business and farm loans, which go mostly to better-off households. The remaining Rs 38 must be informal debt, savings run down and whatever surveys miss. Its size fits Bell and Mukhopadhyay (2015), with half of a lost wage borrowed back and nearly all of that outside banks, and the panel points the same way, with more laboring households reporting debt for consumption and overdue shop bills (Table A15). Two cautions apply to the residual. Interviewers reconcile income and spending in this panel, so it could be larger, and the credit figure comes from a comparison across blocks for all households, so adding it assumes that the ratio carries over.

Withdrawals elsewhere look similar on one margin. When US states ended pandemic unemployment benefits early, Coombs et al. (2021) find that recipients cut spending by about half the loss. Spending takes a similar share here, in a setting with far less wealth and far less access

Table 6: The account of a lost wage

	Rupees a month, per 10 days	Per Rs 100 of lost scheme wages	Where it comes from
Scheme wages lost	-182	-100	Scheme records, labor force survey
Lower market wages	-95	-52	Labor force survey wage, applied to the gap in casual earnings
More private work and earnings away	52	29	Household panel, the rest of wages
Own production and business	15	8	Household panel
Private transfers	24	13	Household panel
Government transfers	4	2	Household panel
Asset sales	2	1	Household panel, rarely reported
Income	-182	-100	Sum, equal to the panel's total income
Small formal credit, net	7	4	Bureau: small gold less personal loans
Informal debt, savings and other	69	38	Residual
Spending	-105	-58	Household panel

The first column reads as rupees a month for laboring households against other households, per 10 days of reliance. Small formal credit comes from the block design of equation (2), as a ratio to lost wages, and it is small gold loans from banks and non-bank lenders less personal loans.

to credit. The difference lies in the top half of Figure 15. A job guarantee sets a floor under wages, so its loss spreads beyond those who worked on it. At mean reliance the fall in wages is close to the rise that Imbert and Papp (2015) find at rollout, so losing the guarantee took back about what its arrival had given.

6.2 What Smoothing Costs

How much should we care about these falls in spending? Relative to a block with no reliance, laboring households at mean reliance spent 2.7 percent less, and at the 90th percentile 4.6 percent less, and these look small. In the formula of Chetty and Looney (2006), the marginal gain from insurance is about risk aversion times the fall in consumption, so with risk aversion of 2 to 3 a rupee in these months was worth about 9 to 14 percent more than usual to a household at the 90th percentile. The same paper warns against stopping there. In its numerical example, falls of 11 to 13 percent go with welfare gains that differ threefold, since households that smooth well may do so because they are close to subsistence and very risk averse.

Our other results point toward that reading. To hold spending to Rs 58 of each Rs 100, young men left home. Some women gave up paid work and pledged jewelry, at about Rs 350 a month in interest on an average loan if the rate were 14 percent a year, about two thirds of the Rs 549 a month that a laboring household at mean reliance had earned from the scheme. Men took lower wages, and spending on health and school fell faster than spending on food. None of this

shows up in the total.

Set against this is what the central government saved, about Rs 75 billion a year in one state, and what employers gained. Part of what laborers lost in market wages was a gain to those who hire them, so the loss to the whole economy is smaller than the loss to laboring households. But that transfer ran away from households earning Rs 10,752 a month with little to fall back on, in a year when a poor monsoon made private work hard to find.

7 Conclusions

Is a withdrawal the reverse of a rollout? For wages it was, as West Bengal shows. When the central government stopped funding the job guarantee in West Bengal in March 2022, rural casual wages fell by about what the rollout studies had found them rising, and laboring households lost about a rupee of income for every rupee the scheme had paid, because the wages they earned elsewhere fell too. More than half of the loss came out of spending, faster from health and schooling than from food.

For credit, the withdrawal was not the reverse of the rollout. The rollout had raised bank lending, and the freeze lowered personal, business and farm loans, by about the low end of the rollout estimates, while applications rose. It also raised a loan the rollout never touched. Small gold loans rose where work had mattered most, by 24 percent between blocks at the 10th and 90th percentile of reliance, after a flat five-year history, with little change while people worked without pay, a step when the work stopped, and a fall after the old wages were paid. Total lending barely moved, not because these responses cancel but because the loans that respond to a guarantee are a small part of what rural households borrow. A study of total credit would have found nothing.

We read these movements as the two things a guarantee is to a household and to a lender. It is income, and households pledged gold when it stopped. It is also a floor under future income, and the loans that rest on that income went with it. We cannot show the second directly, because the bureau records inquiries and loans and not the decisions between them, and because rising demand from poorer applicants would leave the same trace. What the data show is that households in the blocks that lost the most asked for more credit and received fewer of these loans, and that defaults rose on personal, business and farm loans and not on gold.

Women carried much of the adjustment, since they lost paid work and pledged gold in their own names, while men took lower wages or left for work elsewhere. The bureau records only the name on a loan, so we cannot say who decided to pledge the gold or who spent the money, and the loan may serve the whole household.

The welfare loss is larger than the fall in spending suggests. Laboring households in blocks at the 90th percentile of reliance spent 3.4 percent less than those at the 10th, which looks modest, but they held spending there by sending young men away, giving up paid work, pledging jewelry

at interest, and cutting health and schooling before food. By the reasoning of Chetty and Looney (2006), a household that smooths this well may be one that cannot afford to let consumption fall, so the small number is a sign of strain rather than of resilience. The loss also reached beyond the people on the muster rolls. A guarantee sets a floor under private wages, so when it went, wages fell for casual workers across the district, and the hiring side gained what the laboring side lost. Lenders, too, had built the guarantee into their expectations, and the loans that rest on future income contracted for everyone in the blocks that had relied on it. A program that pays some households for work turns out to hold up the wages, the credit and the plans of many more.

The largest part of the adjustment remains to be mapped. Formal credit replaced about Rs 4 of every Rs 100 lost, and about Rs 38 came from informal debt, savings and other sources that no existing survey measures in rupees. The freeze lasted long enough, and reached enough households, that a survey of informal lending and gold holdings in these blocks could trace that rupee from the moneylender and the relative to the pawned necklace, and show how the formal and informal sides of a household's balance sheet move together when a guarantee is withdrawn. Linking the names on gold loans to the women who worked on the scheme would show who bore the cost inside the household.

The question is no longer confined to one state. Since July 2026 the law that replaced MGNREGA asks most states to pay 40 percent of the cost and lets each name 60 days a year without work (PRS Legislative Research, 2025; Ministry of Rural Development, 2026). A state that cannot pay its share may offer less work, and West Bengal shows what follows. Private wages rest on the guarantee, so its loss reaches workers who never took a day of it. Households replace a little of the loss with credit, at the cost of their gold, and absorb most of it by spending less. A guarantee is worth far more than the wages it pays, because an economy of wages, loans and plans has been built on top of it, and a government can withdraw it in a month while the households beneath it take years to rebuild.

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Appendix

Appendix A. The Setting and the Data

Work on the scheme and credit points in two directions. Bell and Mukhopadhyay (2015) survey 279 households in Odisha and use the reservation of village council seats for women as an instrument for access to work. One more day of work lowers consumption borrowing by about half the daily wage over the year and about one for one in the lean season, while it raises borrowing for cultivation. Surendra (2020) finds no average change in borrowing after the rollout, with more institutional and production loans for landless and Scheduled Caste and Tribe households. The model in that chapter gives a reason, since the scheme raises the worst income a borrower can fall back on and so lowers the cost of recovering a loan. Dey and Imai (2015), with a panel from Birbhum district in West Bengal, find that a record of steady work on the scheme is what lets poor households buy on credit at the local shop. Chaturvedi and Rauf (2025) find that bank credit rose by 5.5 to 22 percent in the first districts, depending on the estimator, and personal credit by 5.9 to 27 percent, and they argue that job cards, identity documents and wages paid into accounts cut the cost of lending to poor households. Muralidharan et al. (2023) find that total loans rose by 30 percent, most of it informal, which they read as greater confidence on both sides that loans could be repaid.

West Bengal ran the scheme at scale before 2022. Over 2018-20 it provided 22 days per rural household a year, fifth among 20 large states, against 16 for India as a whole. Like any large public program it had its politics. Halder and Mehra (2025) find that village councils aligned with the state's ruling party received about 12 percent more work in 2013 to 2017, and the central government later spoke of fake job cards. Administrative records can overstate real work (Imbert and Papp, 2015; Drèze and Somanchi, 2025). We return to what this means for our measure of reliance in Section 3.1.

The courts moved slowly once a union of agricultural workers petitioned the Calcutta High Court in September 2022. In June 2025 the court ordered the scheme restarted from 1 August 2025, and in October 2025 the Supreme Court dismissed the central government's appeal. The Ministry issued a conditional resumption order that December. Work resumed only on 1 June 2026, three weeks after a new state government took office, and a new law, the VB-G RAM G Act, replaced MGNREGA in the state from 1 July 2026. Our credit data end in June 2024, so every month after the freeze in our data lies inside it.

The state's three responses, which Table A1 compares with the scheme itself, deserve more detail. The first came before the freeze, when in September 2021 the state began paying Lakshmir Bhandar, a monthly transfer to women aged 25 to 60, at Rs 500 and at Rs 1,000 for Scheduled Caste and Tribe women. More than four fifths of eligible women were covered (Raghunathan et al., 2026). The transfer is no replacement for the guarantee in design, but it landed three months before wages stopped, and it paid more to the same Scheduled Tribe households that relied on

the scheme most. Mukherjee and Chattopadhyay (2026) find that it raised food consumption.

The second response was work, as the state gave job card holders work on its own departments' projects, and in March 2024 it notified this as the Karmashree scheme, with a target of 50 days a year. The state later claimed 110 million person-days for 2022-23 and 280 million for 2023-24, though it first reported the 2022-23 figure a year after the fact (Figure A1). This work differs from the guarantee in three ways. No household can demand it, contractors do much of the hiring, and over 2022-24 it averaged about Rs 213 a day, less than the scheme's notified wage. Even taken at face value, it replaced 36 percent of the 2018-20 level in 2022-23. No district or month figures are public.

The third response was arrears, for which in February 2024 the state budget set aside Rs 37 billion to pay 2.1 million job card holders the wages the central government had not paid, about Rs 17,600 each, with transfers beginning on 26 February 2024. This gives us a rare event, the same blocks receiving back part of what they had lost, and Section 4.3 uses it.

Another central freeze overlapped with this one. Funds for the rural housing scheme, PMAY-G, also stopped, and by June 2023 about 1.1 million approved houses had received no money. Labor on these houses is often booked through MGNREGA (Drèze and Somanchi, 2025), so the two freezes are hard to separate. We read our estimates as the effect of losing the guarantee together with this linked housing money.

The freeze also met a dry year. The monsoon of 2022 was the first to arrive after funds stopped, and Figure A3 shows that it was poor across the state. A standard drought index for June to September averaged -1.02 across West Bengal's districts and fell below -1 in 14 of 19, against -0.25 in Bihar, -0.46 in Jharkhand, -0.72 in Assam and 0.18 in Odisha. A year like this is when the scheme used to matter most. Fetzer (2020) finds that work on the scheme rose after bad monsoons and that bad monsoons stopped lowering agricultural wages once it arrived. Here the insurance had gone before the loss came. The drought also matters for how we compare places. West Bengal was drier than most of its neighbors, and inside the state the driest districts were those that relied least on the scheme, with a correlation of 0.74 between reliance and the index. A comparison across districts would mix the two shocks, while a comparison inside a district removes the monsoon, and this is one reason our main design works inside districts.

Table A1: MGNREGA and what the state offered alongside it

	MGNREGA	State-funded work	Lakshmir Bhandar
Who pays	Centre for wages, under the 2005 Act	State budget	State budget
Who receives	Any rural household asking for work	Job card holders, chosen by departments and contractors	Women aged 25 to 60 with a bank account, outside government service
Benefit	Up to 100 days a year at the notified wage, Rs 191 in 2019-20	Daily wage on department works, at least 50 days under Karmashree from 2024	Rs 500 a month, Rs 1,000 for Scheduled Caste and Tribe women. Rs 1,000 and Rs 1,200 from 2024
A legal right to work	Yes, with an unemployment allowance	No	No work involved
Dates	Rolled out 2006 to 2008, frozen in West Bengal 9 March 2022, work back 1 June 2026	Counted from 2022-23, notified as Karmashree on 7 March 2024	First payments September 2021
Size	305 million person-days a year in 2018-20	110 million person-days claimed for 2022-23, 280 million for 2023-24	22 million women promised
Block-level records	2018-19 and 2019-20	None public	None, stand-in built from the 2011 census

Amounts are nominal. Person-days are from the Ministry of Rural Development's administrative records and the state's budget speeches. The state's own figures are unaudited claims. The count of women promised the transfer is from Kotiswaran and Jana (2026), and its rules from Mukherjee and Chattopadhyay (2026).

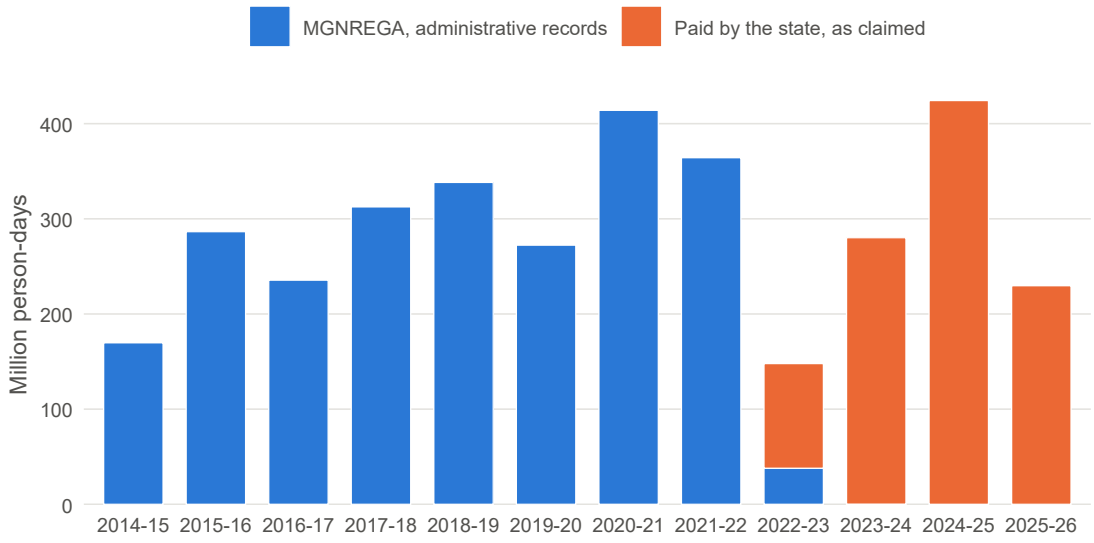


Figure A1: West Bengal's person-days by fiscal year

Blue bars are MGNREGA person-days in the Ministry of Rural Development's administrative records. Orange bars are the person-days the state says it funded itself, from budget speeches of February 2024 and February 2026 and press reports for 2024-25, unaudited and available only as state totals. The state's figure for 2022-23 first appeared in February 2024.

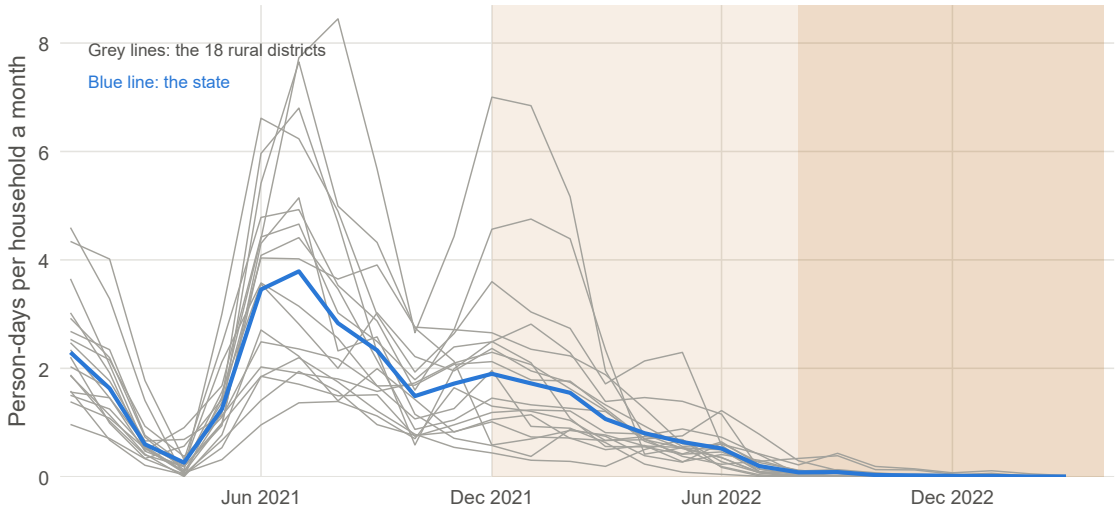


Figure A2: MGNREGA work in every district of West Bengal

Person-days per 2011 household in the state's 18 rural districts. Gray lines are districts and the blue line is the state. Light shading marks the transition, December 2021 to July 2022. Dark shading marks the after period, August 2022 to March 2023. Person-days are from the Ministry of Rural Development's administrative records.

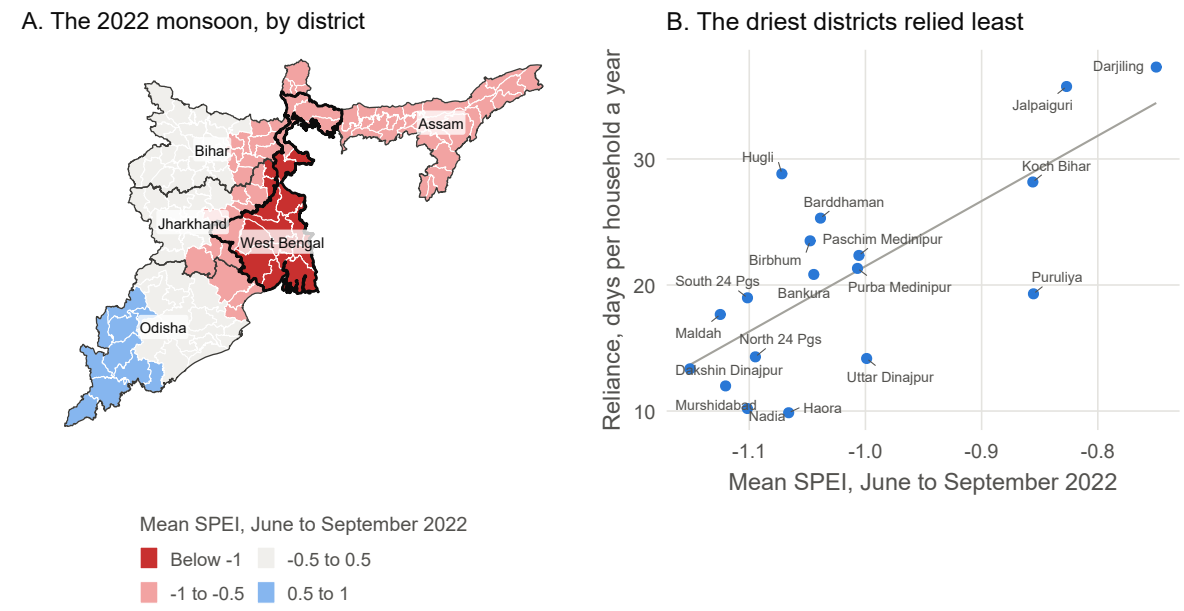


Figure A3: The 2022 monsoon in West Bengal and its neighbors

Panel A shows the mean of the Standardized Precipitation Evapotranspiration Index over June to September 2022, weighted by population within each 2011 district. Negative values are drier than normal. Panel B plots reliance against the same index for West Bengal's 18 rural districts, with the fitted line. SPEI is from the Global SPEI database. Reliance is person-days per 2011 household a year, 2018-20.

To place loans in blocks, we use the 2011 population of each pincode that falls in each block, and we share each pincode's loans accordingly. Added up by district, the result equals the bureau's district totals in every month and detail. Sharing by population blurs the comparison, since it spreads some loans across block lines. Comparing with pincodes that lie almost wholly in one block suggests that our estimates are attenuated by about a fifth.

Our main outcome is small bank gold, meaning loans against gold under Rs 50,000 from public and private banks. We chose it for three reasons. A gold loan needs no proof of income or credit history, so it is the formal product that a household short of cash can most easily reach, and its size suits a household replacing wages. In 2017, 61 percent of bank gold loans were under the line, averaging about Rs 30,000 over our sample, while larger gold loans average more than four times as much and look like farm or business credit, which theory says should move the other way. The outcome is also measured well, once two gaps in the files are allowed for. Non-bank gold lenders report with breaks in 2017 and 2019, so we use non-bank gold only from 2020. Microfinance reporting grows sixteenfold over the sample, from about 1,700 loans a month in these blocks in 2016 to about 28,500 in 2021, so we do not study it as an outcome of its own and include it only in the measure of defaults. Over our sample the bureau records 761,000 small bank gold loans in these blocks worth Rs 22.9 billion, or Rs 22.54 per household a month, rising from Rs 18.16 before the freeze to Rs 37.74 after it. Women took 33 percent of the rupees and first-time borrowers 5.7 percent of the loans. Table A3 gives means for every

outcome by period.

We identify the effects of the freeze by comparing changes in borrowing across blocks with different reliance on the scheme. Our measure of reliance, R_b, is person-days in block b per household a year, averaged over 2018-19 and 2019-20, with households from the 2011 census. The person-days come from the Ministry's administrative records by month and block, which we obtain through the data publisher Dataful. We use these two years as the last stable ones before the freeze. Work in 2020-21 surged because of COVID, and 2021 was a state election year. We prefer days per household to the share of households working, because a day of work is what was lost, and because reliance converts directly to lost wages at the notified wage, Rs 191 in 2019-20. A block with 10 more days of reliance lost Rs 159 more per household a month. Figure A4 shows that the two measures line up closely. Each 10 days of reliance goes with 10 percentage points more households working in the busiest month, on a mean of 19 percent.

Figure 3 maps reliance, and Table A2 summarizes it. The mean block has 21.5 days, the 10th percentile 9.3, the 90th 36.7. Blocks in the northern hills and tea districts rely most, and blocks around Kolkata rely least. By district, reliance runs from 9.2 days in Haora and 9.8 in Nadia to 36.6 in Jalpaiguri and 39.5 in Darjiling (Figure A10). More important for our design, 57 percent of the variance of reliance lies inside districts. Reliance is positively correlated with the Scheduled Tribe share (0.39), is unrelated to the Scheduled Caste share (0.02), and within districts is lower in more developed blocks (correlation of -0.37 with a development index). Figure A7 maps these traits, and we let Scheduled Caste and Tribe areas follow their own path for this reason.

We leave out 19 of the state's 341 blocks in which municipal towns dominate. The census counts statutory towns outside blocks, yet their pincodes sit inside block maps, so these blocks record town loans over a small rural household count. They hold about 0.90 million households and mean reliance of 14.2 days (Figure A5). They are unlikely to serve as a comparison for villages, for the reason Marquez-Padilla et al. (2025) leave out large cities. Our main sample has 322 blocks, 14.7 million households and 66.3 million people in 2011, for 21,252 block-months. In robustness analyses we add the town-dominated blocks back.

Two features of the data deserve comment. All rupee amounts are nominal, and gold prices rose over the period. Figure A9 shows the average bank gold loan rising from Rs 61,800 in 2017 to Rs 93,800 in early 2023, with the share under Rs 50,000 falling from 61 to 35 percent. A common rise in prices is removed when blocks are compared in percent and not when they are compared in rupees, one reason we lead with percent below. Next, recorded days may overstate real days where the central government's charge of irregularities was true. If padding were random this would attenuate our estimates. If it were concentrated where real work was low, it would weaken them further.

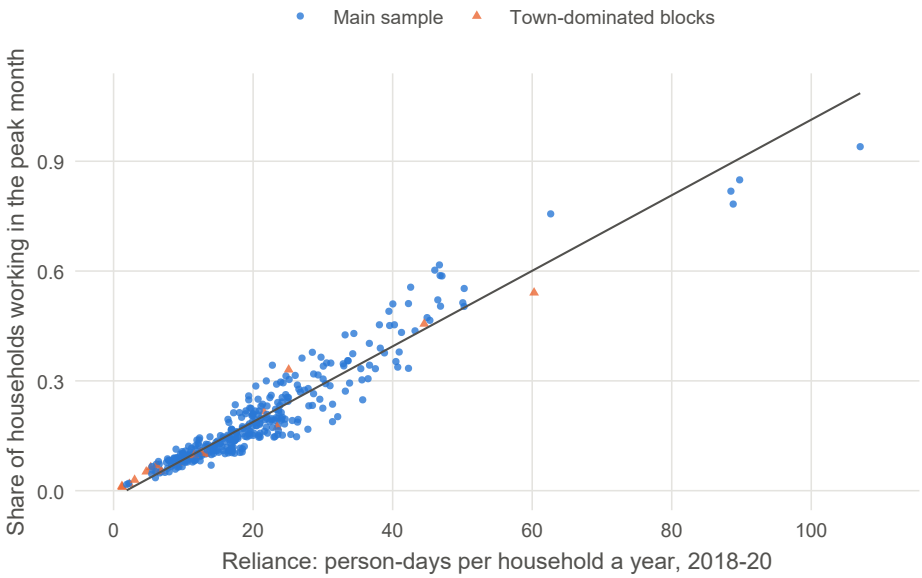


Figure A4: Households working on the scheme and reliance, by block

Each point is a block. The vertical axis is households working in the block's busiest month, averaged over 2018-19 and 2019-20, over 2011 households. Reliance is person-days per 2011 household a year. The line fits the main sample.

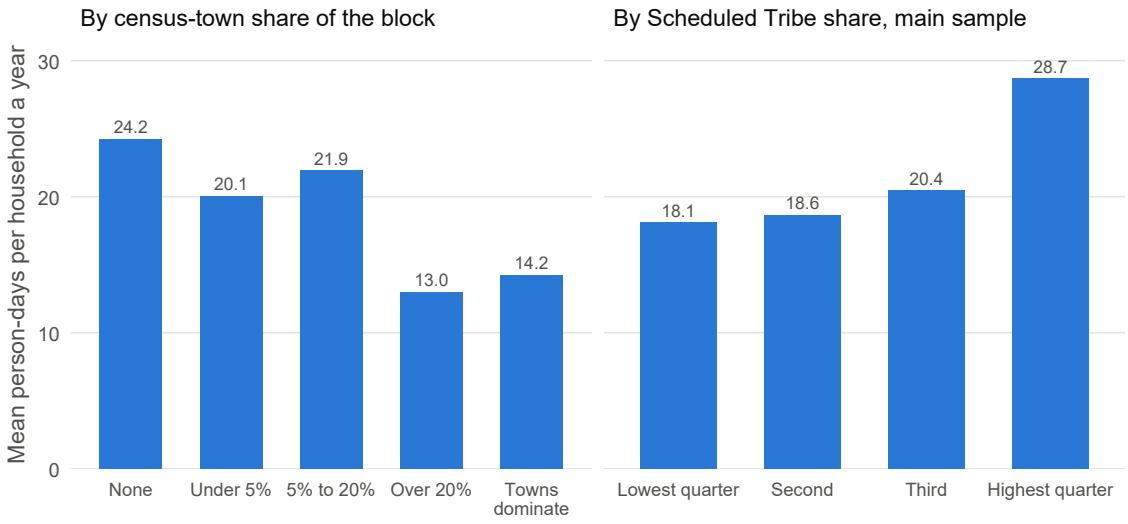


Figure A5: Reliance by type of block

Mean of reliance across blocks. The left panel groups all 341 blocks by the share of people in census towns, with the 19 town-dominated blocks last. The right panel groups the main sample by quarters of Scheduled Tribe share.

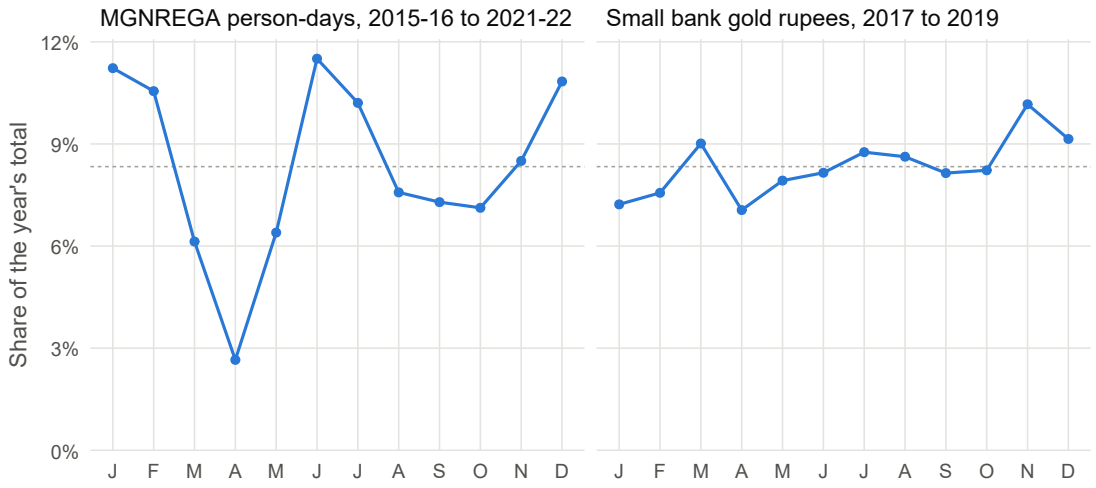


Figure A6: The calendar of MGNREGA work and of small gold loans

Share of each year's total falling in each calendar month, averaged over years. The dotted line marks an equal share. The left panel covers West Bengal's person-days. The right panel covers small bank gold in the main sample, before the freeze.

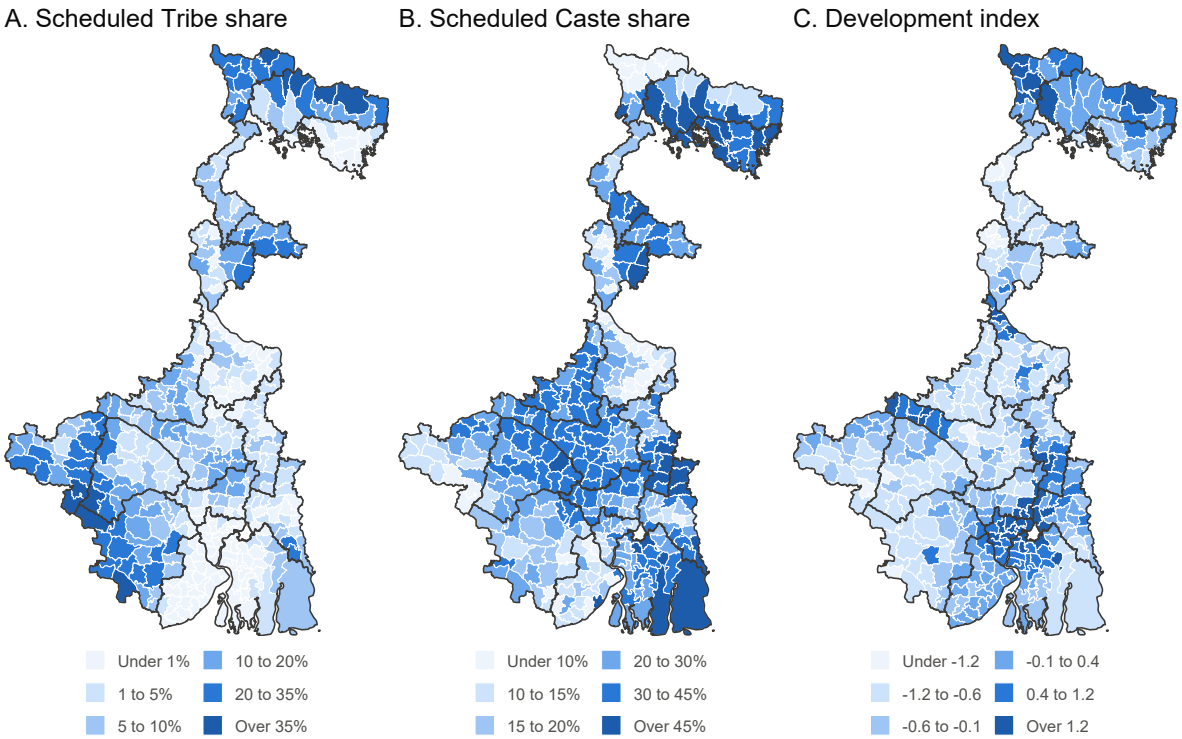


Figure A7: Scheduled Tribe share, Scheduled Caste share and development, by block

Shares of each block's people in the 2011 census. The development index is the first principal component of the census-town share, literacy and the share of agricultural laborers, scaled to mean zero and standard deviation one. Dark lines mark 2011 districts.

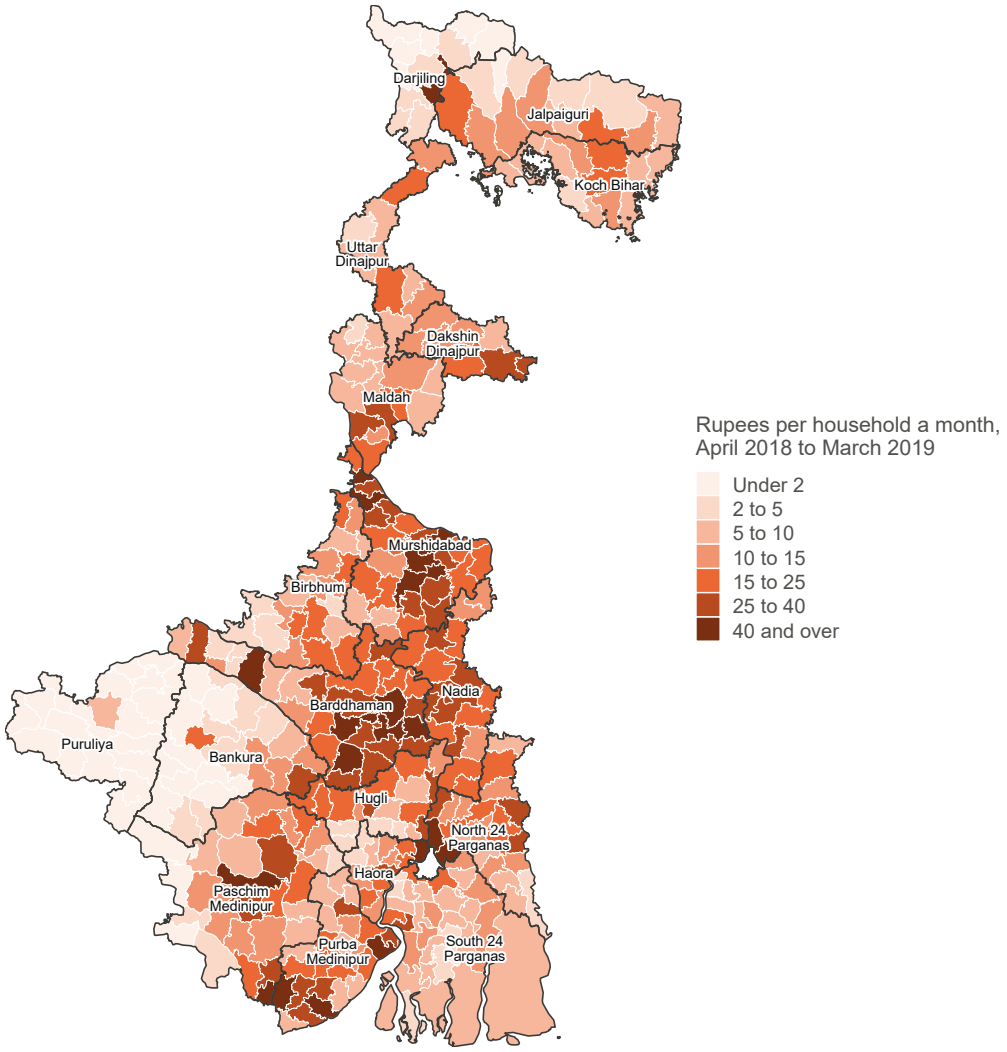


Figure A8: Small bank gold per household before the freeze, by block

Rupees of new small bank gold loans per 2011 household a month, April 2018 to March 2019. Dark lines mark 2011 districts.

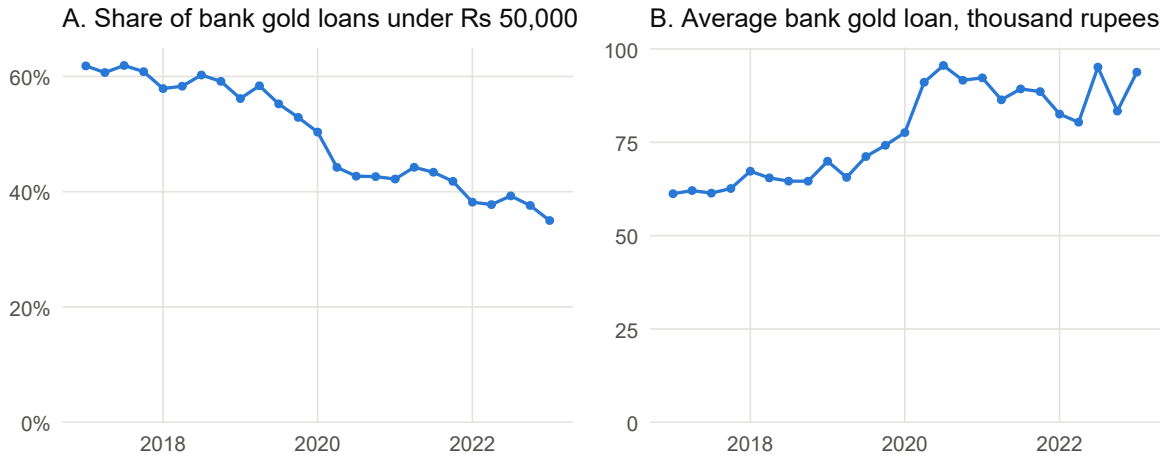


Figure A9: The size of bank gold loans

Main sample, by quarter. All amounts are nominal.

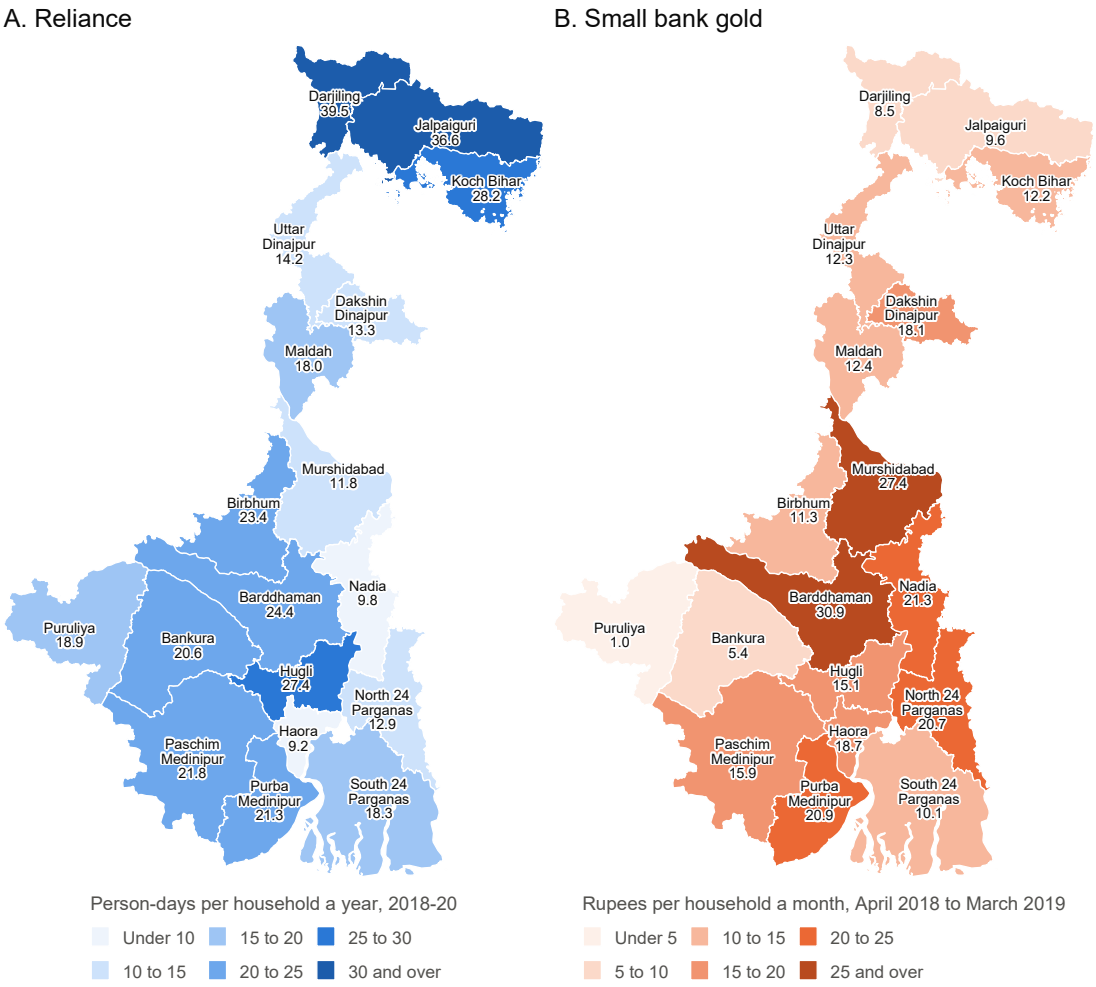


Figure A10: West Bengal's districts, reliance and small bank gold before the freeze

Values on the map are for each 2011 district, adding up all its blocks. Panel A is person-days per 2011 household a year, average of 2018-19 and 2019-20. Panel B is rupees of new small bank gold loans per 2011 household a month, April 2018 to March 2019. Kolkata has no block.

Table A2: Descriptive statistics on reliance

	Blocks	Mean	Std. dev.	10th	25th	50th	75th	90th
All blocks	341	21.1	13.3	8.8	12.3	18.7	24.7	36.7
All blocks, weighted by households	341	19.6	11.3	8.3	11.3	17.5	24.1	34.4
Main sample	322	21.5	13.0	9.3	12.6	19.1	25.0	36.7
Main sample, weighted by households	322	20.0	10.9	8.9	11.8	18.0	24.2	34.4

Person-days per 2011 household a year, average of 2018-19 and 2019-20. Town-dominated blocks are those whose people by WorldPop, which counts towns inside the block's boundary, are at least twice the census count for the block.

Table A3: New loans by period

	Rupees per household a month				Loans per 1,000
	Before	Transition	After	All months	households a month
Small bank gold	18.16	34.75	37.74	22.54	0.75
Small bank gold, women	5.27	12.66	13.95	7.22	0.24
Small bank gold, men	12.75	22.05	23.73	15.21	0.51
Large bank gold	73.53	220.32	261.75	114.14	0.86
All bank gold	91.69	255.07	299.50	136.68	1.67
Personal loans under Rs 50,000	19.28	86.80	107.49	38.16	4.40
Mudra and business loans	169.99	321.03	456.06	222.97	0.63
Farm loans and KCC	91.24	137.39	165.25	105.80	0.78
Vehicle loans	468.22	659.92	714.30	521.28	2.33
Credit cards	136.26	238.73	277.04	165.74	2.04
Mortgages	427.81	764.57	779.36	511.24	0.37
All new loans	2322.05	4400.12	5051.32	2904.76	23.55

Means across block-months in the main sample. Before is January 2017 to November 2021 without the COVID months, transition is December 2021 to July 2022, and after is August 2022 to March 2023. Amounts are nominal.

The survey's design shapes how we use it, and five features deserve comment. Wages from the scheme are not recorded apart. They sit inside wage income, and the survey's government transfers explicitly leave them out, so we see lost scheme wages only net of what replaced them. Income and spending are not separate measurements, since the interviewer shows the household its implied saving and lets it revise both, so we do not read saving from the gap between them in any one household. Monthly values are recalled, up to four months later, so every regression includes months since the interview. The debt module carries no amounts and no flows. Almost nobody in rural West Bengal reports a loan from a moneylender or a relative, 0.1 and 0.7 percent of laboring households in 2019, while 74 percent report a shop bill overdue by at least two months, and this last share collapses in January 2024 for reasons that remain unexplained. We therefore read debt only as changes before 2024, and we do not claim to measure informal credit in rupees. Finally, nearly all households hold some gold (99.7 percent) and almost none report adding to it in a four-month window (0.05 percent), while income from selling assets averages Rs 1.5 a month. The survey cannot tell us who had gold to pledge, which is why we measure gold through the bureau. The panel also misses many of the poorest households. Somanchi (2021) shows that it counts 6 percent of households without a television against 29 percent in the National Family Health Survey, and that female illiteracy in West Bengal is 7.6 percent in the panel against 27.1 percent in that survey, and it also undercounts women's work (Afridi et al., 2022). We use changes within households, which guards against errors in levels. The households it misses are probably those that relied most on the scheme, so we read its responses as those of households somewhat better placed than the ones we would most like to see, and we check them against an official consumption survey in Appendix D.

Table A5 lists our sources, and three groups of public data add what the bureau and the panel cannot give. The first is wages, for which the Periodic Labour Force Survey records each person's activity on each of the last seven days and the earnings of casual workers, with a district code, for 2017-18 onward. It codes scheme work as its own activity. West Bengal's sample holds only about a dozen scheme workers in any one week, too few to follow scheme work month by month, so we pool the three pre-freeze years to measure who did the scheme's days, and we use the survey for wages and days of private work. The Labour Bureau's Wage Rates in Rural India gives monthly wages by occupation and state from 600 villages, which lets us compare West Bengal with its neighbors month by month.

The second is an official survey that owes nothing to CMIE. The Household Consumption Expenditure Survey of 2011-12, 2022-23 and 2023-24 gives long differences in spending. It has two rounds after the freeze and allows only a coarse comparison, but it guards against the panel's weaknesses.

The third is local activity, for which we use annual VIIRS night lights added up to blocks, which Cook and Shah (2022) use to study the scheme's arrival. Night lights are not a measure of welfare, and their value is that they are recorded by someone other than a surveyor and are hard to move by accident. We use the SPEI drought index to measure the monsoon.

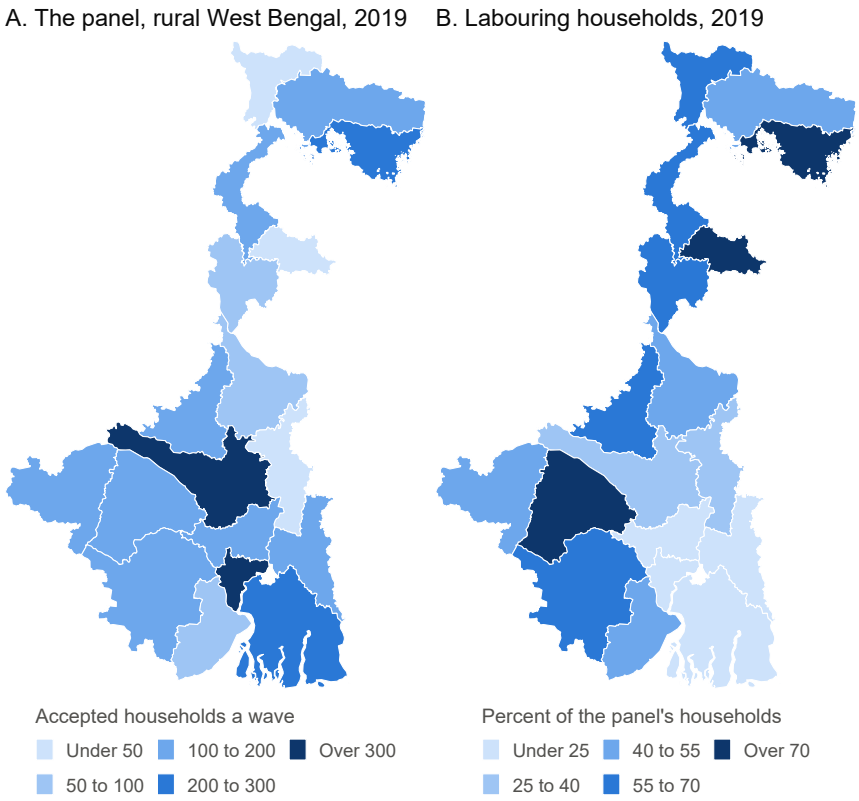


Figure A11: The household panel in rural West Bengal, 2019

Panel A shows accepted rural households per wave in the Consumer Pyramids Household Survey, by 2011 district, averaged over the three waves of 2019. Panel B shows the share of those households in CMIE's groups of agricultural laborers, wage laborers, and small and marginal farmers.

Table A4: The household panel in rural West Bengal, 2019

	Laboring households	Other households
Income and spending, rupees a month		
Total income	10,752	15,156
Wages	9,834	10,135
Business profit	26	3,170
Own production	453	323
Private transfers	308	695
Government transfers	80	137
Asset sales	1.5	0.3
Total spending	8,273	11,310
Food	4,467	5,308
Health	370	627
Education	333	492
Loan installments	307	746
Debt and gold, percent of households		
Any debt	81.7	88.2
Bank	14.5	29.5
Self-help group	6.5	5.5
Microfinance institution	2.6	2.6
Shop, overdue two months	74.2	78.6
Moneylender	0.1	0.4
Relatives and friends	0.7	0.5
Holds gold	99.7	99.9
Saved in gold in the last four months	0.05	0.26
People		
Members present	3.83	4.03
Women aged 18 to 59 employed, percent	12.8	17.5
Men aged 18 to 59 employed, percent	86.8	86.9
Men aged 18 to 59 in daily wage work, percent	55.5	9.1
Members gone for work, per 100 households	6.5	8.4
Boys aged 15 to 17 in school, percent	80.8	80.7
Girls aged 15 to 17 in school, percent	91.3	94.4

Consumer Pyramids Household Survey, accepted rural interviews in West Bengal, 2019. Monthly values use the survey's state weights. Laboring households are CMIE's groups of agricultural laborers, wage laborers, and small and marginal farmers. Members gone for work counts roster members whose recorded reason for leaving is employment. Shops means a bill overdue by at least two months.

Table A5: The data

Data	Source	Unit	Span used	Used for
New loans and inquiries	TransUnion CIBIL, through CAFRAL	Pincode, month, product, lender, sex, score band, size	January 2015 to June 2024	Borrowing, applications, defaults
Scheme records	Ministry of Rural Development, through Dataful	Block and month, 2018-20. District and month, all states	April 2014 to June 2026	Reliance, the first stage
Census 2011	Office of the Registrar General	Block	2011	Households, castes, development
Consumer Pyramids	CMIE	Household panel, 2,900 rural households a wave here. Monthly income and spending by recall. People, debt flags and gold every four months	January 2017 to April 2024	Income, spending, work, migration, debt
Periodic Labour Force Survey	Ministry of Statistics, public	Person, daily activity over seven days, casual wages, by district	2017-18 to 2022-23	Wages and days of work
Wage Rates in Rural India	Labour Bureau, public	State, occupation and month, from 600 villages	January 2017 to March 2024	Rural wages against neighbors
Consumption Expenditure Survey	Ministry of Statistics, public	Household, by NSS region	2011-12, 2022-23, 2023-24	Official check on spending
VIIRS night lights	Earth Observation Group, through SHRUG	Annual composite, block	2017 to 2023	Local activity
Deposits and Credit of Banks	Reserve Bank of India, public	District	2019	Wages where banks are thin
SPEI drought index	Global SPEI database, public	0.5 degree grid, month, weighted to districts	1901 to 2024	The 2022 monsoon

CAFRAL is the Centre for Advanced Financial Research and Learning. CMIE is the Centre for Monitoring Indian Economy. SHRUG is the Socioeconomic High-resolution Rural-Urban Geographic Platform.

Appendix B. More on Borrowing

Figure A12 separates loans by size and borrowers by sex. In the first column, small gold rises for all borrowers, most clearly for women, whose coefficients reach about 14 percent by early 2023. For men the rise is less than half as large and the intervals cover zero in most quarters. In the second column, large gold loans drift down after the freeze, without a clear break. In the third column, all bank gold shows almost no change, because large loans account for most of the rupees. A study of all gold loans would have found little.

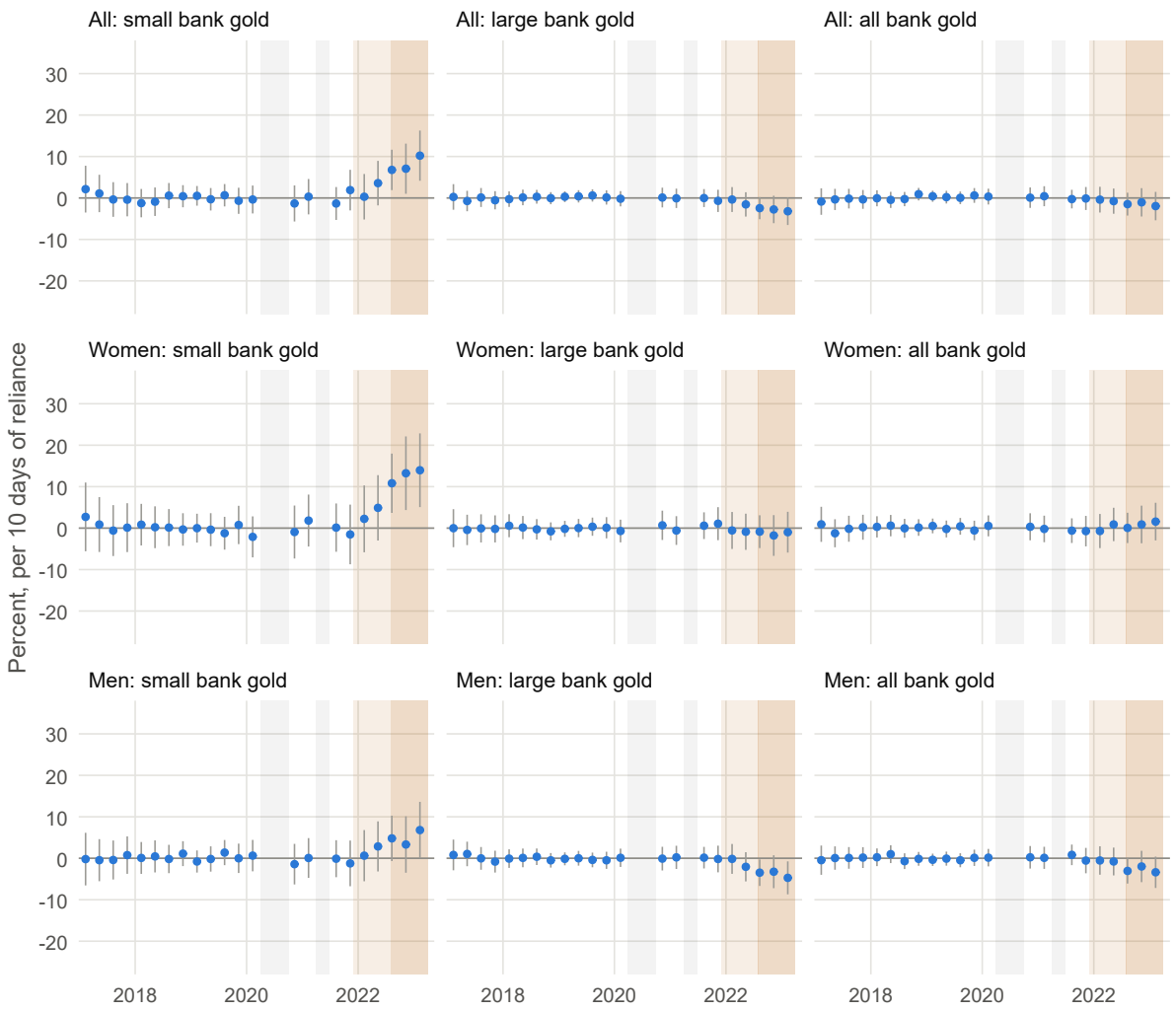


Figure A12: Event studies for bank gold, by loan size and borrower's sex

Quarterly coefficients from equation (3) estimated by Poisson, with 95 percent confidence intervals based on standard errors clustered by block. Small loans are under Rs 50,000. Shading as in Figure 4. All regressions include block effects, district-by-month effects, and Scheduled Caste and Tribe shares by month.

These numbers are easier to read across the actual range of reliance. Between a block at the 10th percentile and a block at the 90th percentile, 27.4 days apart, the implied rise is 24 percent or Rs 8.5 per household a month. Between the quartiles, 12.4 days apart, it is 10 percent. Applied to the whole sample at household-weighted mean reliance, the estimate implies about 3,100 more small bank gold loans a month, on a base of about 17,700, when the wages lost came to about Rs 4.7 billion a month.

Figure 4 and Table 1 suggest that the freeze raised small gold lending, principally through women. This appendix examines how sensitive that finding is. Figure 9 reports estimates of β_2 in alternative specifications, for each loan size and for all borrowers, women and men. The first row repeats our baseline, and the second drops the comparison within districts and uses month

effects only, requiring parallel trends across the whole state. The third drops the Scheduled Caste and Tribe paths. The fourth lets the month effects vary with a block's development index, built from literacy, the census-town share and the share of agricultural laborers, the closest we can come to the marginalization controls of Marquez-Padilla et al. (2025). The fifth lets month effects vary with gold lending before the freeze, by quintile, so that blocks are compared with others at similar levels. The sixth adds a linear trend in reliance. The seventh adds back the town-dominated blocks. The eighth keeps only loans from pincodes lying almost wholly in one block.

For women's small gold loans, the checks do not change the conclusion. Every specification implies a significant rise, with coefficients from 0.102 to 0.148. For all borrowers the coefficient runs from 0.063 to 0.094, always significant at the 5 percent level. It is smallest when town-dominated blocks return, as expected, since they add towns where the scheme barely mattered, and when development enters, since development and reliance are related inside districts. It is largest on loans from single-block pincodes, consistent with attenuation from sharing loans across blocks. Men's coefficients are positive in all eight rows and significant in none. We consider the effect on men's small gold borrowing insufficiently robust. For large gold and all bank gold, the estimates stay small and their intervals cover zero throughout. Given these findings, we focus on small bank gold in what follows, and we report women and men separately.

To probe the role of particular places, Figure A13 repeats the main specification leaving out one district at a time. The coefficient for all borrowers stays between 0.065 and 0.097. The largest comes from leaving out Darjiling, whose hill blocks rely most and borrow least, and the smallest from leaving out Hugli, so no single district carries the result.

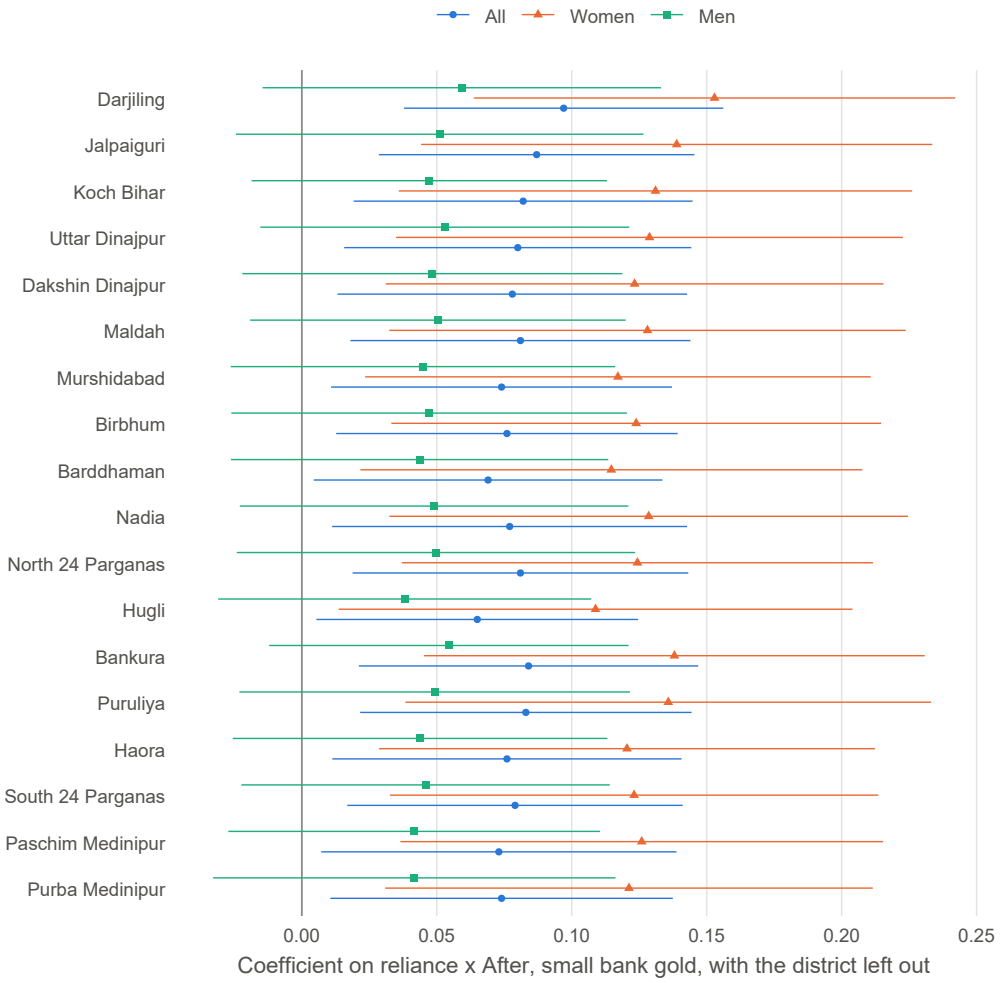


Figure A13: Effect of the freeze on small bank gold, leaving out one district

Point estimates and 95 percent confidence intervals of β_2 in equation (2), by Poisson, with standard errors clustered by block. Each row leaves out the district named on the left.

Our linear specification assumes that each day of reliance counts the same. To relax this, Figure A14 replaces reliance with bins and reports each bin against blocks with under 10 days. The response rises with reliance across the range where most blocks lie. Blocks with 30 to 40 days borrow 25 percent more than blocks under 10 days, against the 21 percent the straight line implies for the same blocks, so we view the linear estimate as somewhat conservative. Above 40 days the estimates are noisy, since few blocks lie there. Figure A15 changes the measure of reliance to rural households in the denominator, to each of the two years alone, and to the share of households working in the peak month, and the patterns do not change.

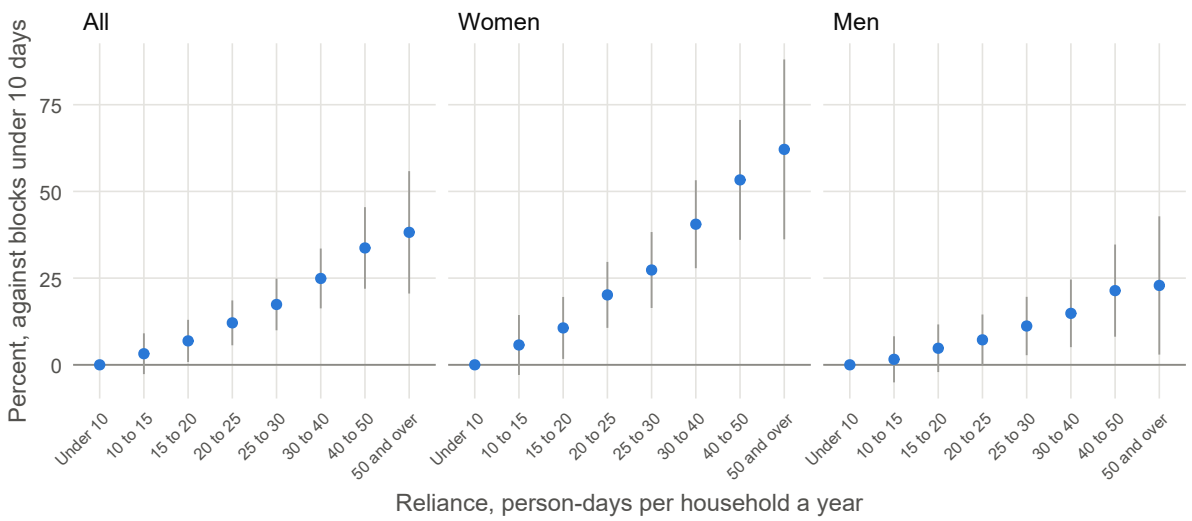


Figure A14: Effect of the freeze on small bank gold, reliance in bins

Coefficients on the interaction of each bin with the after period, against blocks under 10 days, with 95 percent confidence intervals based on standard errors clustered by block. The regressions include the bins' transition interactions, block effects, district-by-month effects, and Scheduled Caste and Tribe shares by month. Bins hold 41, 61, 75, 64, 25, 30, 18 and 8 blocks.

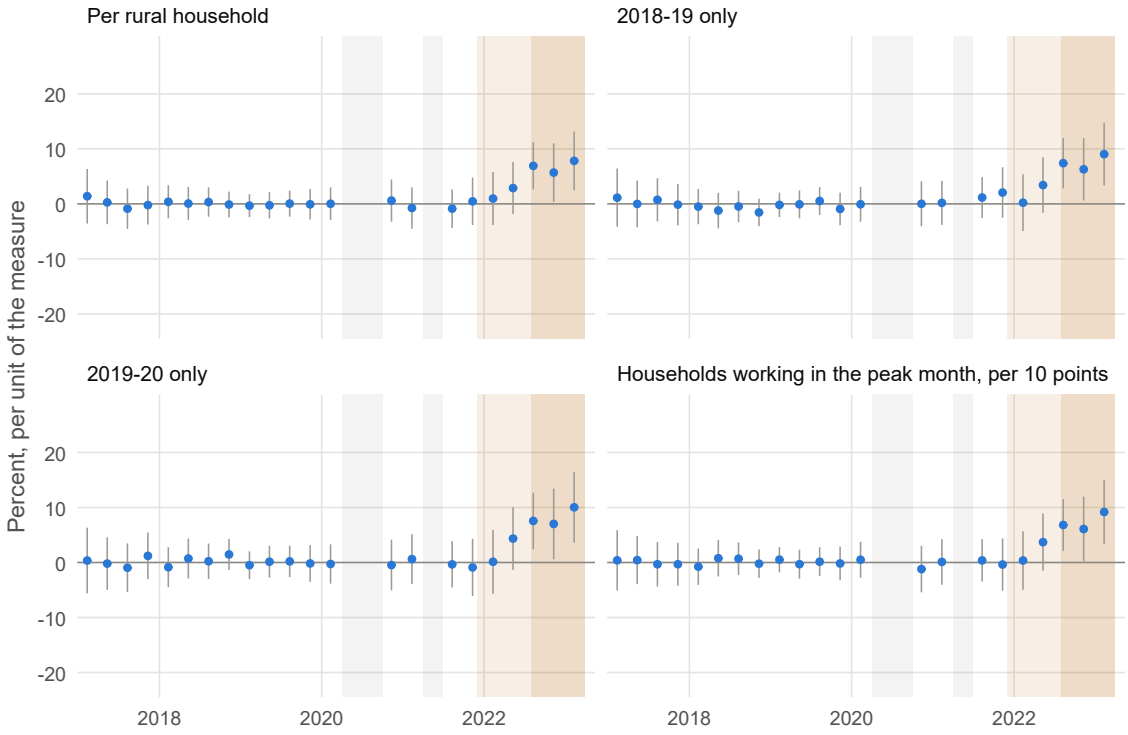


Figure A15: Event studies for small bank gold, other measures of reliance

Re-estimation of the first panel of Figure A12 with other measures. The first panel uses 2018-20 person-days per rural household, the next two use a single year, and the last uses the share of households working in the peak month, per 10 percentage points. Shading as in Figure 4.

The freeze hit one state, so within-state comparisons cannot rule out a shock to poorer places everywhere, such as a recovery from COVID that favored blocks like these, but a comparison outside the state can. Figure A16 runs the same specification in Maharashtra, where the scheme kept running at its earlier level, and in Bihar and Jharkhand, neighbors with the same block geography, where work rose after 2021.[4] None shows a rise in gold lending with reliance at the same date. Finally, Figure A17 sets aside regression altogether and plots small bank gold by thirds of reliance, each against its own level in 2018-19. The three lines move together through the lockdowns and the recovery, and they separate only after work stops.

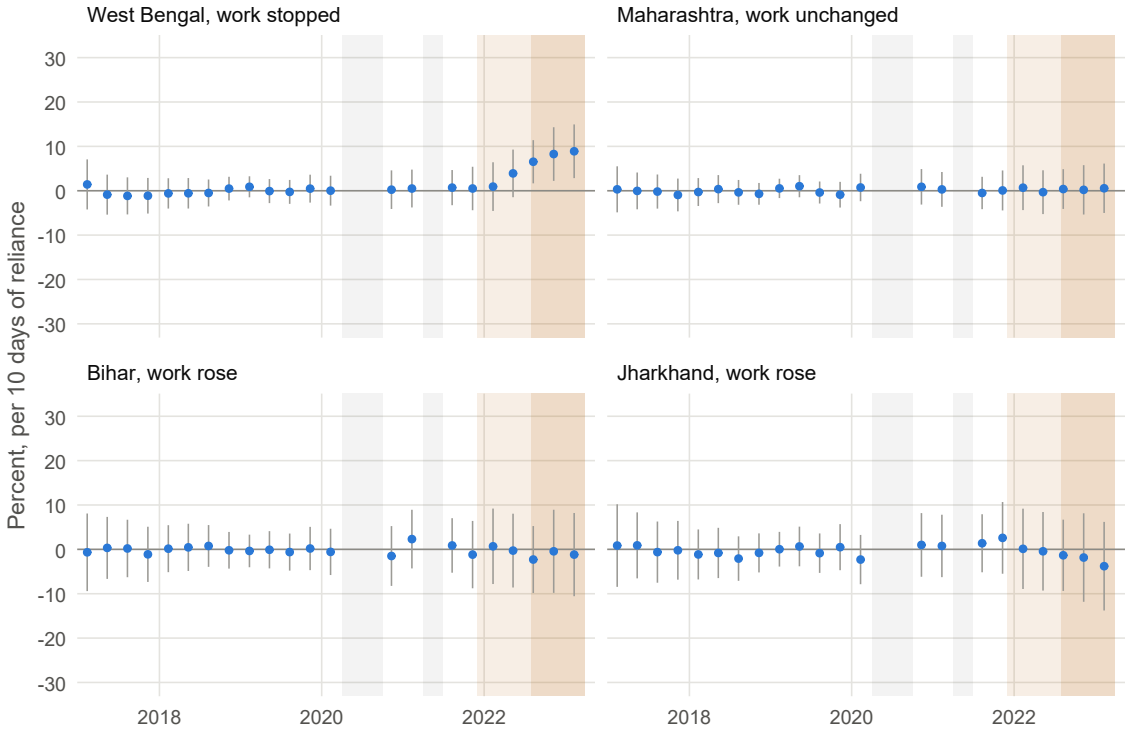


Figure A16: Event studies for small bank gold, the same design in other states

Quarterly coefficients from equation (3), per 10 days of reliance, by state, with 95 percent confidence intervals based on standard errors clustered by block. Maharashtra's work in August 2022 to March 2023 stayed at its usual level. Bihar's and Jharkhand's rose. Shading marks West Bengal's transition and after periods.

[4]Odisha and Assam are left out because their blocks do not match census subdistricts. Reliance is lower in these states, with means of 5.6, 7.3 and 12.5 days, so we report estimates per 10 days and the intervals are wide. With work rising in Bihar and Jharkhand, a small negative coefficient is what the logic of the design predicts.

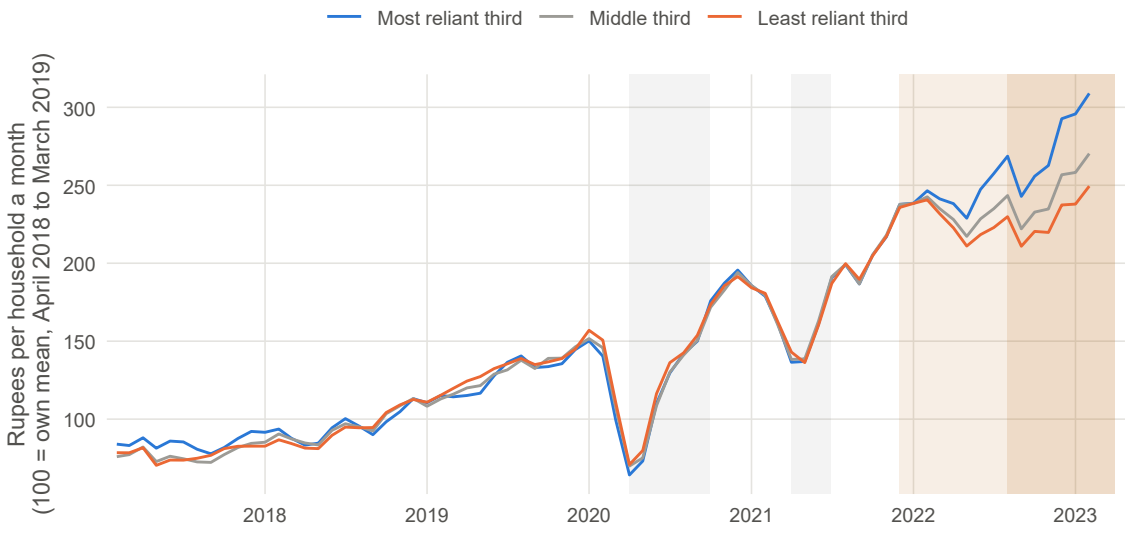


Figure A17: Small bank gold by thirds of reliance

Mean across blocks of rupees per 2011 household a month, each third against its own mean of April 2018 to March 2019, three-month centered moving average, no fixed effects. The thirds have mean reliance of 10.8, 19.1 and 34.6 days. Shading as in Figure 4.

Two further checks ask how much weight the flat history can carry. Following Rambachan and Roth (2023), we allow the trend after the freeze to depart from parallel by a multiple of the largest move between months before it. For small bank gold the 95 percent interval first covers zero when that multiple reaches 1.6, so the conclusion survives a break in trend larger than any the series shows before the freeze. Spatial standard errors with a 50 kilometer cutoff, of the kind used by Muralidharan et al. (2023) and Cook and Shah (2022), give $p = 0.021$ for the main coefficient.

The within-district slope leaves out anything common to a whole state, including a statewide fall in credit or wages. To see the level, we compare pincodes on either side of West Bengal's border with Bihar, Jharkhand, Odisha, Assam and Sikkim. A pincode enters if it lies in a subdistrict whose centroid is within 30 kilometers of the line, and, as in Ghosh and Vats (2026), each is compared with pincodes in the bordering district across the line,

$$y_{zpt} = \alpha_z + \gamma_{pt} + \beta_1^S \mathrm{WB}_z \mathbf{1}_{\mathrm{Transition}} + \beta_2^S \mathrm{WB}_z \mathbf{1}_{\mathrm{After}} + \varepsilon_{zpt}, \tag{A1}$$

where z is a pincode, p a pair of bordering districts and γ_{pt} pair-by-month effects, with standard errors clustered by district pair. Rainfall and prices should vary little across a line this close, but three things do change at it. West Bengal pays Lakshmir Bhandar and its neighbors do not, the state joined PM-KISAN only in May 2021, and workers can cross. The first two mean this comparison mixes the freeze with other state policy, and the third biases it toward zero, since Muralidharan et al. (2023) find that gains from the scheme spill across mandal lines. We therefore read it as a check on the within-district slope and not as a cleaner estimate. With one

treated state, inference at the state level is limited in a way clustering cannot repair (Ferman and Pinto, 2019).

Figure 10 and Table A6 report the results of equation (A1). The map shows the 109 subdistricts on the West Bengal side within 30 kilometers of the line and the 122 on the other. Small bank gold moves with its neighbors across the line until early 2022 and then separates. In the after period it is 14.6 percent higher on the West Bengal side, and 21.9 percent higher for women. Personal loans are 5.8 percent lower, applications are 6.1 percent higher, and all new loans move by only 0.9 percent. The within-district slopes, taken to mean reliance, imply 18 percent for gold and 6 percent for personal loans. The two designs lean on different assumptions and arrive at similar answers, with the state line a little smaller, as spillovers across it would suggest. Mortgages, which we would not expect to move, do not.

Table A6: Effect of the freeze across the state line

	Small bank gold (1)	Small gold, women (2)	Large bank gold (3)	Personal loans (4)	Applications (5)	All new loans (6)	Mortgages (7)
West Bengal × Transition	0.046	0.070	-0.012	-0.021	0.041*	-0.003	0.001
	[0.040]	[0.058]	[0.022]	[0.020]	[0.022]	[0.012]	[0.016]
West Bengal × After	0.136***	0.198***	-0.030	-0.060***	0.059**	-0.009	0.003
	[0.045]	[0.066]	[0.024]	[0.022]	[0.024]	[0.014]	[0.018]
N, pincode-months	58,740	58,740	58,740	58,740	58,740	58,740	58,740

Brackets contain standard errors clustered by district pair. Coefficients are log points from Poisson regressions of equation (A1), with pincode effects and district-pair-by-month effects. The sample is pincodes in subdistricts whose centroid lies within 30 kilometers of the line. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

If replacement programs softened the blow, our estimates understate what the guarantee was worth. If they were aimed at the blocks that relied most, they could also confound the comparison. Section 2.2 described the state's work and Lakshmir Bhandar. We cannot control for the state's work, because no figures exist by block or month, but we can bound it. The state's claim for 2022-23 was 36 percent of the 2018-20 level, it is unaudited, and the state did not report it until 2024. Most of this work seems to have come after our main window. The state reported most of its 2023-24 wage spending after August 2023, and departmental work had reached only about 80,000 workers by May 2023.[5]

Lakshmir Bhandar we can approximate with a stand-in for its payment per household in each block, built from the number of women aged seven and over per household in the 2011 census, multiplied by Rs 500 or Rs 1,000 according to the block's Scheduled Caste and Tribe

[5]The wage figure is from Scroll, 7 October 2023, and the worker count from the state's single-window reporting portal, archived 4 June 2023. The first falls outside our main window. The second suggests how little was in place at the end of it.

share. We add it to equation (2):

$$\begin{aligned} y_{bdt} = \alpha_b + \gamma_{dt} + \sigma_t \mathrm{SC}_b + \eta_t \mathrm{ST}_b + \beta_1^M M_b \mathbf{1}_{\text{Transition}} + \beta_2^M M_b \mathbf{1}_{\text{After}} \\ + \beta_1^L L_b \mathbf{1}_{\text{Transition}} + \beta_2^L L_b \mathbf{1}_{\text{After}} + \varepsilon_{bdt}, \end{aligned} \tag{A2}$$

where L_b is the Lakshmir Bhandar stand-in in hundreds of rupees per household a month, and M_b is the MGNREGA exposure, either reliance in tens of days or lost wages in hundreds of rupees per household a month. The transfer began before the transition, so its coefficient reflects any change from before September 2021, and its goal here is only to see whether accounting for it alters the estimated effect of the freeze.

Table A7 reports the results in rupees, with women in Panel A and men in Panel B. Column (1) repeats the rupee estimate from Table 1, and column (2) adds Lakshmir Bhandar, under which women's estimate falls from Rs 1.9 to Rs 1.7 and men's from Rs 1.2 to Rs 1.1, neither by a meaningful amount. Columns (3) and (4) use lost wages as the exposure, so each coefficient reads as rupees of gold borrowing per Rs 100 of lost wages. For women the figure is Rs 1.19, and Rs 1.07 with the transfer. The transfer's own coefficient is small and negative for women, at Rs 0.28 per Rs 100 of transfer, and not significant. Cash paid to women appears to have done little to keep them away from the gold loan counter. A transfer of Rs 500 to Rs 1,000 a month is large next to the monthly loss in the average block, but it reached women with or without a link to the guarantee, and it did not rise when the work stopped.

Table A7: Effect of the freeze on small bank gold, with Lakshmir Bhandar, by sex

	(1)	(2)	(3)	(4)
A. Women				
Reliance × After	1.90***	1.70***		
	[0.36]	[0.38]		
Lost wages per household × After			1.19***	1.07***
			[0.23]	[0.24]
Lakshmir Bhandar per household × After		-0.28		-0.28
		[0.37]		[0.37]
N	21,252	21,252	21,252	21,252
B. Men				
Reliance × After	1.20**	1.10**		
	[0.48]	[0.51]		
Lost wages per household × After			0.75**	0.69**
			[0.30]	[0.32]
Lakshmir Bhandar per household × After		-0.10		-0.10
		[0.44]		[0.44]
N	21,252	21,252	21,252	21,252

Brackets contain standard errors clustered by block. Dependent variable is small bank gold in rupees per household a month, by least squares. Reliance is in tens of days. Lost wages and Lakshmir Bhandar are in hundreds of rupees per household a month. All regressions include block effects, district-by-month effects, Scheduled Caste and Tribe shares by month, and the transition interactions of the variables shown. Sample excludes nine COVID months and 19 town-dominated blocks. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The freeze could have changed who appears in the credit files, a concern for internal validity. Lenders could have left high-reliance blocks, making loans harder to see, or households could have migrated. The scheme had been holding migration down (Imbert and Papp, 2020), and the household panel records more members leaving for work after the freeze (Table A14). Migration is itself a result of interest and also a threat, since our household counts are fixed at 2011. A family that leaves a block no longer borrows there, which would bias our gold estimates downward.

Table A8 reports estimates of equation (2) for measures of who appears in the data and for products that should not respond. The share of pincodes with any new loan shows no change, and at 98.8 percent it has little room to move, so bureau coverage did not thin in high-reliance blocks. The share of first-time borrowers among all new loans shows no change either. Credit cards, mortgages and vehicle loans go to households far from the scheme, and they do not move with reliance. Every point estimate is smaller than its standard error, and the 95 percent intervals for the three products exclude changes of more than 3.5 percent per 10 days in either direction. Whatever raised small gold lending and lowered personal lending in these blocks, it

did not touch lending to households above them in the income distribution.

Table A8: Effect of the freeze on who appears in the files, and on products that should not respond

	Pincodes with any new loan (1)	First-time share of new loans (2)	Credit cards (3)	Mortgages (4)	Vehicle loans (5)
Reliance × After	0.00021	-0.00006	-0.010	0.006	0.005
	[0.00043]	[0.00137]	[0.012]	[0.008]	[0.005]
Dependent variable mean	0.988	0.187	165.74	511.24	521.28
N	21,252	21,252	21,252	21,252	21,252

Brackets contain standard errors clustered by block. Columns (1) and (2) are by least squares, with the dependent variable as a share. Columns (3) to (5) are by Poisson, with the mean in rupees per household a month. All regressions include block effects, district-by-month effects, Scheduled Caste and Tribe shares by month, and the transition interaction. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

We now ask where the response was largest. The scheme's design and the nature of a gold loan predict specific patterns. The scheme reached poorer and more tribal areas, which suggests larger effects there. A gold loan needs gold and a bank nearby, which suggests the reverse. Reliance already captures how much each block lost, so the question is how much a given loss moves borrowing.

Table 2 splits blocks at the median by Scheduled Tribe share, by development and by small gold lending before the freeze. For women (Panel A), the rise is 16.3 percent per 10 days in blocks with low tribal shares and 6.0 percent in those with high ones, and the second is not significantly different from zero. Women's response is somewhat larger in less developed blocks. The sharpest split is by gold lending before the freeze. Where it was high, women's borrowing rises by 18.1 percent per 10 days. Where it was low, the rise is 6.4 percent, not significant. The pattern for men is similar and weaker throughout (Panel B).

The monsoon offers one more split, since in the 14 districts where the drought index fell below -1, which hold 80 percent of our blocks, the slope for small gold is 9.2 percent per 10 days. In the other, mostly northern, districts it is 4.3 percent and not significantly different from zero. Credit against gold is thin in the north for other reasons, as Figure A8 shows, so this split can only suggest that a loss of work and a dry year together pushed households harder than a loss of work alone.

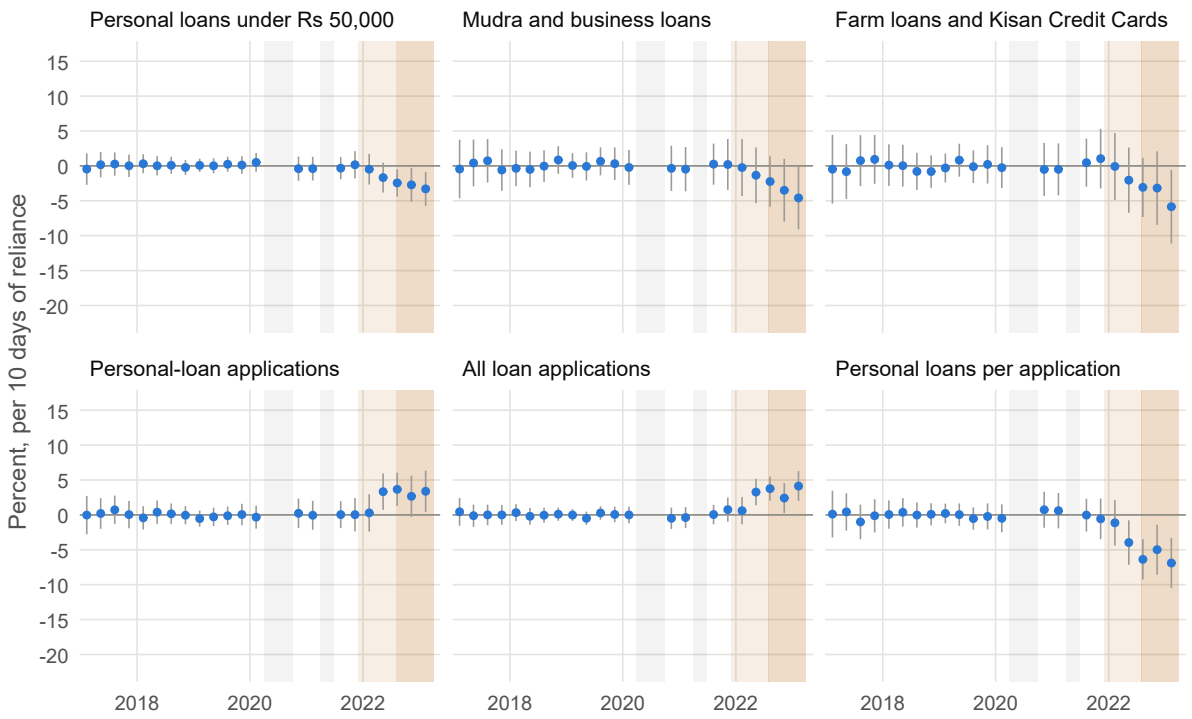


Figure A18: Event studies for other credit

Quarterly coefficients from equation (3) estimated by Poisson, with 95 percent confidence intervals based on standard errors clustered by block. Shading as in Figure 4. All regressions include block effects, district-by-month effects, and Scheduled Caste and Tribe shares by month. Personal loans per application is the difference between the panels for personal loans and personal-loan applications.

Table A9: Effect of the freeze on other credit and on applications

	Personal loans under Rs 50,000 (1)	Mudra and business loans (2)	Farm loans and KCC (3)	All new loans (4)
A. All borrowers				
Reliance × Transition	-0.011	-0.007	-0.013	-0.002
	[0.010]	[0.019]	[0.023]	[0.006]
Reliance × After	-0.028**	-0.036	-0.041	-0.005
	[0.013]	[0.024]	[0.028]	[0.007]
Dependent variable mean, rupees	38.16	222.97	105.80	2904.76
N	21,252	21,252	21,252	21,252
B. Men				
Reliance × Transition	-0.013	-0.009	-0.017	-0.003
	[0.012]	[0.021]	[0.025]	[0.007]
Reliance × After	-0.034**	-0.043*	-0.052*	-0.008
	[0.015]	[0.026]	[0.031]	[0.008]
Dependent variable mean, rupees	33.29	187.46	84.06	2403.68
N	21,252	21,252	21,252	21,252
C. Women				
Reliance × Transition	-0.002	0.001	-0.002	0.002
	[0.017]	[0.031]	[0.034]	[0.009]
Reliance × After	-0.006	-0.002	-0.006	0.005
	[0.021]	[0.037]	[0.041]	[0.010]
Dependent variable mean, rupees	4.78	34.46	20.46	490.31
N	21,252	21,252	21,252	21,252

D. Applications, all borrowers	Personal-loan applications	All applications	Personal loans per application
Reliance × Transition	0.021	0.024***	-0.032**
	[0.013]	[0.009]	[0.016]
Reliance × After	0.029*	0.030***	-0.057***
	[0.016]	[0.011]	[0.019]
Mean, per 1,000 households	7.79	33.27	0.56
N	21,252	21,252	21,252

Brackets contain standard errors clustered by block. All regressions are Poisson, with coefficients as log points per 10 days of reliance, and include block effects, district-by-month effects, and Scheduled Caste and Tribe shares by month. KCC is the Kisan Credit Card. All new loans leave out gold from non-bank lenders, which report with breaks. Applications are bureau inquiries. Personal loans per application is the difference of two Poisson coefficients, with a standard error from a stacked regression. Sample excludes nine COVID months and 19 town-dominated blocks. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Panels B and C split by sex and show the mirror of the gold result. These falls are concentrated among men, at 3.3 percent for personal loans, 4.2 percent for business and 5.1 percent for

farm credit. Women's coefficients are near zero, as men hold most of these loans. Read together with Table 1, the picture is of credit in men's names shrinking and gold loans in women's names growing, in the same households or at least in the same blocks.

Table A10: Effect of the freeze on other credit, in rupees

	Personal loans	Mudra and business	Farm loans and KCC	All new loans	Credit cards	Mortgages
	(1)	(2)	(3)	(4)	(5)	(6)
Reliance × After, rupees	-3.0	-16.1	-6.6	-25.3	-2.8	4.7
	[7.6]	[34.0]	[6.3]	[328.0]	[19.7]	[108.6]
Mean in the After window, rupees	107.49	456.06	165.25	5051.32	277.04	779.36
N	21,252	21,252	21,252	21,252	21,252	21,252

Brackets contain standard errors clustered by block. Least squares, with the dependent variable in rupees per household a month. All regressions include block effects, district-by-month effects, Scheduled Caste and Tribe shares by month, and the transition interaction. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A11 looks more closely at who lent and who borrowed. Panel A splits small gold loans by lender. Public sector banks hold most of the rural branches, and they made 75 percent of the rupees in our sample. Their lending rises by 9.4 percent per 10 days. Private banks' lending rises by 3.1 percent, not significantly different from zero. Column (3) brings in the non-bank finance companies, the specialized gold lenders that make about 4.0 times as many small gold loans as banks. Their records have breaks before 2020, so we use January 2020 onward, which leaves only 14 months before the freeze. Their lending rises by 4.8 percent per 10 days, or Rs 6.0 per household a month on their much larger base, twice the rupee response at banks.

Table A11: Effect of the freeze on small gold loans, by lender, credit history and season

A. Lender

	Public banks	Private banks	NBFCs, from 2020
	(1)	(2)	(3)
Reliance × After	0.090**	0.031	0.047*
	[0.035]	[0.041]	[0.024]
Dependent variable mean, rupees	17.00	5.55	93.92
N	21,252	21,252	9,660

B. Credit history of the borrower

	First-time	With a score
	(4)	(5)
Reliance × After	0.144***	0.074**
	[0.044]	[0.033]
Dependent variable mean, rupees	1.24	21.31
N	21,252	21,252

C. Season, within the After window

	December to February	Other months
	(6)	(7)
Reliance × After	0.098***	0.067*
	[0.038]	[0.035]
Mean in those months, rupees	39.01	36.98
N	19,642	20,286

Brackets contain standard errors clustered by block. All regressions are Poisson, with coefficients as log points per 10 days of reliance, and include block effects, district-by-month effects, Scheduled Caste and Tribe shares by month, and the transition interaction. Columns (1), (2), (4) and (5) use the full sample. Column (3) uses January 2020 onward. Columns (6) and (7) each keep the months before the freeze, the transition, and the after months named. NBFCs are non-bank finance companies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Panel B splits borrowers by credit history. Loans to people with no prior record in the bureau rise by 15.5 percent per 10 days, about twice the 7.7 percent for people with a score. First-time borrowers are a small share of small gold loans, at 5.7 percent, so they do not account for most of the rupees. But the gap says something about who was reached. A loan against gold was, for some households, the first formal credit they had ever taken.

Panel C splits the after period by season. The coefficient is 10.3 percent per 10 days in December to February and 6.9 percent in the other after months. Pooled in this way, December to February gives 9.8 log points, close to the 9.5 log points that the event study shows for December 2022 to February 2023. These are the months when the scheme used to pay the most, so the pattern fits Bell and Mukhopadhyay (2015), who find that the scheme substituted for consumption borrowing about one for one in the lean season. The same months passed with

little change during the transition, when people were still working.

A loan that smooths a passing loss should be repaid, and we examine this in two ways. Panel A of Table A12 looks at loans opened before the freeze whose first year of repayment overlapped it. The outcome is the share going 90 days past due within 12 months of opening, and the regressor is reliance times an indicator for a loan whose first year reached into the freeze. For personal, farm, business and microfinance loans combined, 10 more days of reliance raises this share by 0.29 percentage points, on a mean of 5.1 percent. Between the 10th and 90th percentile blocks this comes to 0.8 points, or 16 percent of the mean. This is smaller than the fall that Tantri (2025) reports after health insurance arrived, 2.4 to 2.8 points on an 8 percent base for microfinance loans, comparing whole districts. For small bank gold the coefficient is 0.12 points, not significantly different from zero, on a mean of 0.9 percent. The lender is secure, and a household that wishes to keep its gold has every reason to pay. The cost of a failed gold loan falls on the borrower and does not appear as a default.

Table A12: Repayment and the back pay of February 2024

A. Loans 90 days past due within 12 months, percentage points

	Small bank gold	Personal, farm and business	Consumer and vehicle	All products
	(1)	(2)	(3)	(4)
Reliance × Exposed loan	0.12	0.29***	0.04	0.10**
	[0.11]	[0.08]	[0.04]	[0.05]
Mean share, percent	0.91	5.09	2.68	3.50
N	15,929	16,100	16,100	16,100

B. Small bank gold after the back pay of February 2024

	All	Women	Men
	(5)	(6)	(7)
Reliance × Apr 2023 to Feb 2024	0.091**	0.142***	0.058
	[0.037]	[0.054]	[0.041]
Reliance × Mar to Jun 2024	0.038	0.061	0.027
	[0.042]	[0.061]	[0.047]
Difference	-0.053**	-0.081**	-0.031
	[0.027]	[0.040]	[0.031]
N	26,082	26,082	26,082

Brackets contain standard errors clustered by block. Reliance is in tens of days. In Panel A the unit is a block and month of opening, for loans opened from January 2017, and the regression uses least squares with block and district-by-month effects. A loan is exposed if its 12 months of repayment reach December 2021 or later. Column (2) includes microfinance. The mean share is for loans opened before December 2020. In Panel B, regressions are Poisson with the specification of equation (2), extended to June 2024, and reference to the months before the freeze. The transition and after interactions are included and not shown. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

How much of the loss does formal credit account for? Figure A19 sets each credit estimate against lost wages, converting the percent estimates to rupees at each product's after-period mean (Chen and Roth, 2024) and dividing by the Rs 159 a month lost per 10 days. For every Rs 100 of lost wages, small gold loans from banks rose by Rs 1.9, with a 95 percent interval of Rs 1.0 to Rs 2.9 on the rupee estimate. After allowing for attenuation from placing pincodes in blocks, the figure is about Rs 2.4. Non-bank small gold adds Rs 3.8, for a total of Rs 5.7. As Section 2.3 showed, about Rs 6 is what the ratios in Bell and Mukhopadhyay (2015) suggest for formal lenders.

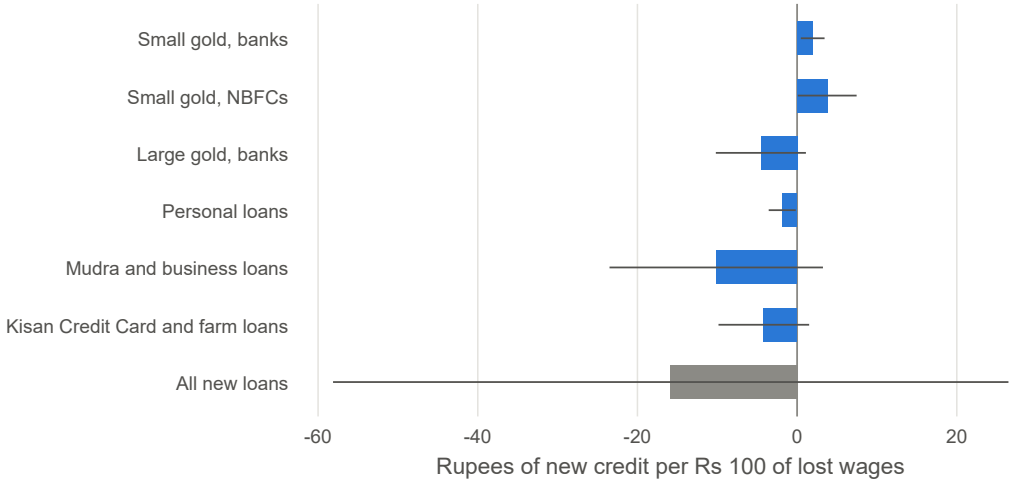


Figure A19: New formal credit per Rs 100 of lost wages

Each bar is the Poisson estimate of β_2 for the product, times the product's after-period mean, divided by Rs 159, the wages lost per household a month per 10 days of reliance. Lines are 95 percent confidence intervals. Non-bank small gold uses January 2020 onward. All new loans leave out non-bank gold.

On the other side, large bank gold fell by Rs 4.5, personal loans by Rs 1.9, business loans by Rs 10.1 and farm credit by Rs 4.2. Only the fall in personal loans is precise enough on its own to rule out zero, and the sum is imprecise. All new loans fell by Rs 16 per Rs 100 lost (standard error Rs 22). The estimates do not establish that formal credit fell on net. They do rule out the idea that it filled the gap. Taken literally, gold added about Rs 6 and everything else subtracted about Rs 21.

Formal credit is limited by what households own, and the size of the loans makes this concrete. A small gold loan of Rs 30,000 equals about 157 days of work at the 2019-20 wage. With lenders capped at 75 percent of the gold's value, it requires about Rs 40,000 of jewelry.[6] The bureau does not record interest rates. At 14 percent a year, the middle of the formal range that Surendra (2020) reports, interest comes to about Rs 350 a month, about two thirds of the Rs 549 that a laboring household at mean reliance had earned from the scheme each month (34.5 days at Rs 191).

[6]The Reserve Bank of India raised this ceiling to 90 percent for non-agricultural gold loans in August 2020 and let the higher ceiling lapse on 31 March 2021.

Appendix C. More on Work, Income and Spending

Table A13 reports the labor market, and Panel B starts with the labor force survey. Days of public work fall by 0.035 a week per 10 days of reliance. The scheme's records imply 0.071, so the survey confirms about half of what the records claim, the share Imbert and Papp (2015) find in their own comparison of the two. Private casual days rise by 0.020 a week, with an interval that covers both zero and a one for one return to private work. The clearer result is the wage, since rural casual daily wages fall by 1.7 percent per 10 days of reliance across districts, which comes to 4.5 percent between the 10th and the 90th percentile. The fall is 2.4 percent in January to March, when the scheme used to offer the most work, and 0.9 percent in August to December. Imbert and Papp (2015) find a gain of 4.7 to 9 percent in the dry season at rollout, so a fall of this size is what their estimates would predict in reverse. Women's wages fall 2.3 percent and men's 1.5 percent. Jayachandran (2006) finds that wages move more where workers have less access to banks, and the same holds here, imprecisely, with a fall of 2.5 percent in districts with below-median bank deposits per person and 0.9 percent in the rest. Some of the fall could run through credit itself, since Breza and Kinnan (2021) find that a contraction in microcredit lowered casual wages by about 4 percent. But formal lending fell little here, so this channel is unlikely to be large.

Table A13: Effect of the freeze on work and wages

A. Household panel, adults aged 18 to 59, percentage points

	Women employed	Men employed	Women in daily wage work	Men in daily wage work
Reliance × Laboring × After	-0.80**	-0.10	-0.50*	0.90*
	[0.40]	[0.40]	[0.30]	[0.50]
Mean, laboring households, 2019	12.8	86.8	10.2	55.5
N, person-waves	58,200	57,800	58,200	57,800

B. Labor force survey, rural adults

	Public works days a week	Private casual days a week	Log casual daily wage		
			All	Women	Men
Reliance × After	-0.035***	0.020	-0.017**	-0.023*	-0.015*
	[0.010]	[0.015]	[0.008]	[0.012]	[0.008]
Reliance × Jan to Mar			-0.024**		
			[0.011]		
Reliance × Aug to Dec			-0.009		
			[0.010]		
Mean before the freeze	0.05	0.72	Rs 265	Rs 210	Rs 280
N, persons or wage earners	94,200	94,200	31,400	7,900	23,500

Brackets contain standard errors clustered by district. Reliance is in tens of days. Panel A uses equation (4) at the wave, with household, district-by-wave and occupation-group-by-wave effects, for adults aged 18 to 59 in households interviewed from January 2017 to April 2023. The transition interaction is included and not shown. Panel B uses rural adults in the Periodic Labour Force Survey, 2017-18 to 2022-23, in equation (5), with district and quarter effects, the drought index and its square, and worker controls. Wages are the daily earnings of casual workers, deflated by the state's price index for agricultural laborers. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Panel A of Table A13 turns to the panel. Laboring women in high-reliance districts are less likely to report any work, by 0.8 points per 10 days on a mean of 13 percent, while men's employment does not change. Afridi et al. (2022) find the same asymmetry in the opposite direction, with the scheme's capacity protecting rural women's employment far more than men's in the pandemic. Men shift into daily wage work instead, by 0.9 points on a mean of 55 percent. So men kept working at lower pay, and some women stopped.

The other side of lower wages is a gain, since they are a cost to those who earn them and a saving to those who pay them. Muralidharan et al. (2023) find that profits on own land fell by 19 log points when a better-run scheme raised wages, so the reverse should show up here. Organized farmers' business profit rises 1.4 percent per 10 days of reliance (standard error 1.2), the expected sign, too noisy to rest on. Part of the loss in market wages was probably a transfer to those who hire labor, which matters for how we read the total in Section 6.1.

When work stops, people can leave, and Table A14 counts members whom the roster records as gone for employment. They are 97 percent men, with a mean age of 25. Per 10 days of reliance, laboring households have 1.1 more such members per 100 households (standard error

0.45), on a mean of 6.5, a rise of 46 percent between the 10th and 90th percentile, while members present fall slightly. Imbert and Papp (2020) find that 7.3 public days per adult a year lowered short-term migration by about 1.75 points. Applied here, this implies a rise of the same order, and the private transfers above suggest that money came back.

Table A14: Effect of the freeze on leaving

	Members gone for work, per 100 households	Members present
Reliance × Laboring × After	1.10**	-0.020
	[0.45]	[0.013]
Mean	6.5	3.83
N	44,900	44,900

Brackets contain standard errors clustered by district. Reliance is in tens of days. Both columns use equation (4) at the wave. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The panel's debt module is a poor instrument, for the reasons in Section 3.1, but the direction of its changes helps. Table A15 shows that more laboring households report debt for consumption, by 1.5 points per 10 days of reliance, and fewer report debt for business, by 0.7 points, the same split by purpose that Bell and Mukhopadhyay (2015) predict and that the bureau shows by product. More report a shop bill overdue by two months, by 1.4 points. Dey and Imai (2015) show, in a West Bengal district, that steady work on the scheme is what let poor households buy on credit at the local shop, so these overdue bills look like the same relationship under strain. Fewer report debt from a microfinance institution, by 0.5 points, as we would expect if these lenders behaved like others who pulled back. Asked directly, more laboring households say their family is worse off than a year ago, by 1.7 points per 10 days, or 4.7 points between the 10th and 90th percentile, on a mean of 25 percent.

Table A15: Effect of the freeze on debt and how families feel

A. Household has debt from, percentage points

	Any source	Bank	Self-help group	Microfinance institution	Shop, overdue two months
Reliance × Laboring × After	0.90	0.30	-0.40	-0.50*	1.40*
	[0.70]	[0.60]	[0.40]	[0.28]	[0.75]
Laboring households' mean, 2019	81.7	14.5	6.5	2.6	74.2

B. Household has debt for, and how it feels, percentage points

	Consumption	Business	Family worse off than a year ago
Reliance × Laboring × After	1.50**	-0.70**	1.70**
	[0.75]	[0.35]	[0.70]
Laboring households' mean, 2019	75.5	8.1	25.5
N, household-waves			32,400

Brackets contain standard errors clustered by district. Reliance is in tens of days. All regressions use equation (4) at the wave, with household, district-by-wave and occupation-group-by-wave effects, September to December 2018 through January to April 2023. Each outcome is a yes or no, multiplied by 100. Moneylenders, relatives and non-bank lenders are too rarely reported to show. Rural West Bengal's overdue shop bills are unusually common and fall sharply in January 2024, after this sample ends. The transition interaction is included and not shown. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A16: Checks on the household panel

A. Specifications, log points per 10 days of reliance	Income	Spending
Baseline	-0.017**	-0.013**
	[0.007]	[0.006]
Adding monsoon x laboring x month	-0.017**	-0.012**
	[0.007]	[0.006]
Survey weights	-0.018**	-0.014*
	[0.009]	[0.007]
Laborers only, farmers out	-0.021**	-0.015**
	[0.008]	[0.007]
Households in every wave	-0.016**	-0.012*
	[0.008]	[0.007]
Recall of one or two months	-0.018**	-0.014**
	[0.008]	[0.007]
Adding development x laboring x month	-0.015**	-0.011*
	[0.007]	[0.006]
B. Who stays in the panel, Reliance × Laboring × After		
Accepted interview, points	0.30	
	[0.90]	
Left the panel, points	0.20	
	[0.60]	
C. Households the scheme should not reach, Reliance × After		
Salaried households, log income	-0.002	
	[0.009]	

Brackets contain standard errors clustered by district. Reliance is in tens of days. Panel A reports the coefficient on reliance times a laboring household times the after period, by Poisson, in versions of equation (4). The monsoon row adds each district's June to September 2022 drought index times a laboring household times each month. Panel B is by least squares on household-waves, including those that refused or could not be found. Panel C uses households whose head holds a permanent salaried job, with household and month effects. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Appendix D. Records Kept for Other Purposes

A household survey can be wrong in ways that are hard to see from inside it. Table A17 therefore turns to night lights, a record kept for other purposes. Across blocks in the same district, night lights fall by 0.9 percent per 10 days of reliance by 2023, or 2.4 percent between the 10th and 90th percentile, the opposite of the gain that Cook and Shah (2022) report at rollout, and Figure A20 maps the change by block. The table also reports the official consumption survey, in which spending by West Bengal's rural casual labor households grew 3.1 percent less than in

the neighboring states between 2011-12 and 2022-24. Neither relies on what a household told the panel's interviewers, and both move in the same direction as the panel.

Table A17: Evidence from outside the panel

	(1) Night lights	(2) Log monthly spending, casual labor households
Reliance × After	-0.009**	
	[0.004]	
West Bengal × later round		-0.031**
		[0.014]
Comparison	Blocks within districts	West Bengal and neighbors
Years	2017 to 2023	2011-12 and 2022-24
N	2,254	

Brackets contain standard errors, clustered by block in column (1) and by NSS region in column (2). Column (1) uses annual VIIRS night lights across 322 blocks, 2017 to 2023, with block and district-by-year effects. Column (2) compares rural casual labor households in West Bengal with those in Bihar, Jharkhand, Odisha and Assam, with spending deflated by the state's rural price index. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

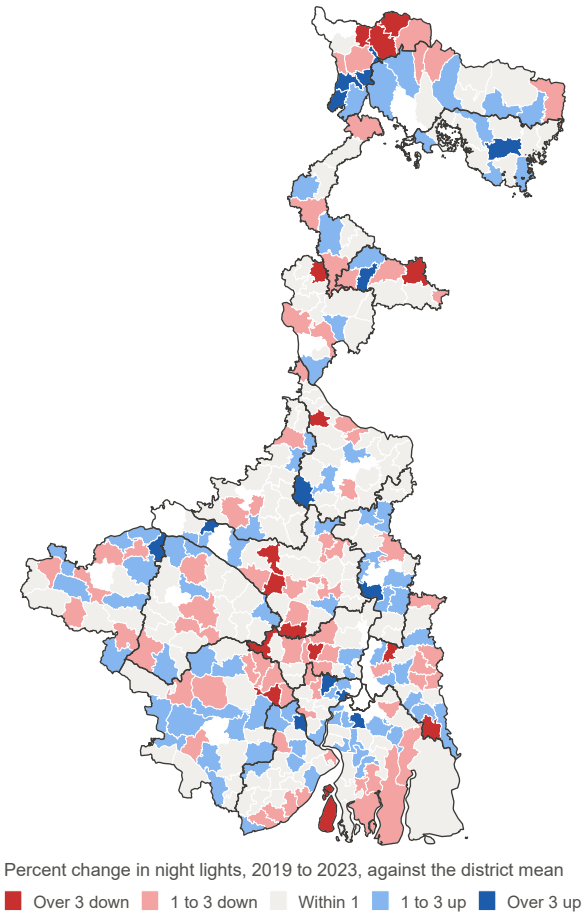


Figure A20: Night lights by block

Percent change in VIIRS night lights from 2019 to 2023 for each block, less the mean of its district. Dark lines mark 2011 districts. Blocks left out of the main sample are blank.