Worker-Owned Capital and Productivity: Experimental Evidence from a Large Platform Firm in India*

Shreya Sarkar
Job Market Paper†

March 13, 2026

Draft updated frequently. Please **click here** for the most recent version.

Abstract

Worker productivity often depends on privately owned, portable inputs that workers finance themselves. When liquidity constraints bind, investment in these inputs may be inefficiently low, depressing both worker earnings and firm profits. I study whether firm subsidies that enable acquisition of worker-owned capital can raise productivity while remaining privately profitable for the firm. I partner with a large home-services platform firm in India where workers travel to customer homes to complete service jobs. In this setting, efficient last-mile connectivity is a key input for worker productivity, making a "scooter" essential worker-owned capital. I implement a randomized experiment in which workers receive subsidies toward scooter loan down payments. The subsidy increases scooter adoption by 32 percentage points. Administrative data show large increases in jobs completed and earnings. Treated workers travel farther, accept more lucrative early-morning and late-evening jobs, and deliver quality improvements. The firm recovers the subsidy within 18-29 weeks through increased revenue share and reduced attrition, subsequently scaling the program to additional cities. A complementary discrete-choice experiment shows demand for this worker-owned capital is more responsive to reducing upfront payment requirements than to lowering overall prices, consistent with liquidity constraints as the primary barrier. The results demonstrate that enabling investment in worker-owned capital can be both productivity-enhancing and privately optimal for firms while benefiting workers.

Keywords: *productive assets, physical capital, investment, labor market outcomes, firm productivity, female workers, liquidity constraints*

*I thank Jeremy Magruder, Supreet Kaur, Aprajit Mahajan and Gautam Rao for help and advice throughout different stages of this project. I would also like to thank Francis Annan, Prabhat Barnwal, Marco Gonzalez-Navarro, Yuen Ho, Ronak Jain, Nikita Kohli, Elisabeth Sadoulet, as well as various seminar participants at Berkeley ARE Workshop, Berkeley Development Lunch and IZA Bonn, for their valuable feedback and comments. I thank Vini Prerna and Arun Kumar for excellent research assistance. I thank numerous collaborators at my partner firm for their support and partnership throughout this project. Financial support from Gender Growth and Labor Markets and CEGA Development Economics Challenge is gratefully acknowledged.

†Department of Agricultural and Resource Economics, University of California, Berkeley, Email: shreyasarkar@berkeley.edu

1 Introduction

Worker productivity often hinges on worker-financed, portable capital – assets employees purchase and use on the job but that firms typically do not provide. Think of corrective lenses that let a tailor stitch without rework; cushioned, non-slip footwear that allows a security guard or a healthcare worker to stand longer and miss fewer shifts; basic tools owned by construction workers; or a vehicle, smartphone, or reliable internet connection that expands a service or delivery worker's feasible set of tasks and coordination. While *capital* is typically analyzed in the context of entrepreneurs or firms, these individually-owned, job-complementary assets might also shape worker productivity (De Mel et al., 2008; Bandiera et al., 2017). Such worker-owned capital has three salient features: (i) it raises output in the worker's current tasks (faster completion, fewer errors, less idle time); (ii) it is general, in the sense that the same asset retains value across employers and matches; and (iii) because the asset is owned by the worker, it can deliver private gains — higher earnings, safer conditions, more stable schedules — even when the employer does not directly invest.

Investment in worker-owned capital may remain inefficiently low for two reasons.[1] The first reason is surplus-sharing and appropriability. When workers bear the up-front cost but the marginal product is shared with the firm, they rationally underinvest; this is the classic rent-sharing (hold-up) problem observed in many settings (e.g., sharecropping and tenancy), where the investor captures only a fraction of the marginal return (Laffont and Matoussi, 1995; Shaban, 1987; Jacoby and Mansuri, 2008). Symmetrically, firms have weak incentives to finance such general (portable) assets because returns are only partly appropriable as the same asset raises the worker's productivity with other employers, so the firm cannot capture the full marginal product (Becker, 1964). The second reason is liquidity constraints. Even when private returns are high, lumpy up-front payments can block adoption, especially in developing-country settings. In equilibrium, these two forces depress privately optimal adoption below the joint-surplus benchmark.

If workers are liquidity constrained and the firm can appropriate only part of the resulting productivity gains, an employer subsidy for such capital could raise worker welfare and firm earnings simultaneously. Motivated by this, and in collaboration with a large Indian platform and its female workforce, I examine three questions: (i) to what extent does a firm-provided subsidy induce adoption of worker-owned capital; (ii) how does induced adoption affect labor supply, on-the-job productivity, and the firm's profits; and (iii) which frictions does the subsidy relax — purchase-time liquidity versus surplus-sharing concerns.

To answer these questions, I study an employer program that subsidizes the down payment on a standard consumer loan used to purchase a scooter on credit, thereby relaxing the worker's upfront financing constraint (while simultaneously reducing the scooter price). The platform matches

[1] By "inefficiently low" I mean underinvestment relative to a joint-surplus benchmark: a planner maximizing the sum of firm profits and worker utility, net of the full economic cost of the asset and financing, taking wages and output prices as given and abstracting from externalities and general-equilibrium effects. In the absence of (i) the firm's appropriation problem for general capital and (ii) workers' purchase-time liquidity/present-bias frictions, adoption would be higher.

workers to in-home service jobs, for example at-home salon and spa services including haircuts, facials, and manicures. As these jobs are completed at the customer's homes, reliable last-mile mobility is a first-order production input. A scooter is a worker-owned capital good that could directly expand the feasible set of jobs and reduce per-job costs. In the study cities — typical of many low and middle income countries' urban environments — public transit does not reliably provide last-mile connectivity.[2] As a result, workers commute either by personally owned vehicles (typically scooters) or by private taxis (e.g. ride-hailing). In a baseline survey of 1,531 workers, only about 31% of women reported commuting by their own scooter, yet 79% stated a preference to do so, suggesting a usage-to-preference gap of about 50 percentage points.[3] Respondents also rated scooter commuting as faster, more reliable, cheaper, and safer than taxis, consistent with higher on-the-job effectiveness in this setting. Given female workers' low current scooter use despite strong stated demand, and the centrality of last-mile mobility for platform work, the firm's female workforce is the natural population in which to test whether easing access to worker-financed, portable capital (here, a scooter) raises productivity and firm profits.

The experiment enrolls 350 full time women workers in Delhi, Bengaluru, and Pune. Participants applied through an in app invitation and met a pre specified credit access screen.[4] Control workers were eligible for standard two wheeler credit to purchase a USD 1,200 scooter with a 15 percent down payment of USD 180. Treated workers received a USD 120 subsidy applied at the time of down payment. This policy delivers a 10% price rebate and, crucially, covers two thirds of the down payment, reducing the out-of-pocket payment at purchase from USD 180 to USD 60. I observe an eight-month panel at daily job level for each worker, yielding high-frequency measures of jobs, hours, and earnings linked to verified scooter purchase and subsequent use. This worker–platform panel - combining granular, job-level outcomes with a direct input–output mapping — is unusual in developing-country settings and rare for female workers in particular. Because treatment status is randomized and scooter adoption is directly observed, I can cleanly link worker use of own capital to productivity and earnings at the worker level and to profits at the firm level, isolating the adoption margin rather than confounding it with selection or access to credit.

Ex ante, adoption of a credit-financed scooter is uncertain given the asset's size relative to income: a USD 1,200 scooter is about 20 weeks of earnings, and the loan entails fixed monthly installments over about 26 months. In the experiment, 49% of treated workers purchased a scooter upon offer of

[2]For instance see: *India—Making Public Transport and Urban Spaces Safer and Inclusive for Women: Policy Brief* (2022); World Bank (2023); *Urban Transport: Policy Note Series* (2021); Shah et al. (2017); *Gender Equality in Transport: Toolkit* (2021).

[3]Among men in the same survey, approximately 77% reported commuting by own scooter and 92% stated a preference to do so, implying a usage–to-preference gap of about 10 percentage points, much smaller than for women (see Figure 1).

[4]From August to October 2024 the firm sent in app invitations to 3,000 randomly selected female salon workers across Delhi, Pune, and Bengaluru. About 1,200 workers responded which is a 40 percent rate with 50 percent from Delhi, 25 percent from Bengaluru, and 25 percent from Pune. The research team completed light verification of government ID and address to screen out low commitment inquiries. Enumerators then administered a baseline survey and collected documentary proof of each applicant's credit score. Eligibility required a baseline CIBIL score of at least 700. Operationally 74 percent of firm workers met this threshold.

the subsidy. Because the subsidy is disbursed only at purchase and paid directly to the lender at the down payment, this purchase implies take-up of credit rather than cash financing. The randomized offer generates a 32 percentage-point increase in purchase relative to control (control mean 17%), a 188% relative rise, providing a strong first stage on the adoption margin. Adoption among workers in the control group is concentrated 4 to 6 months after the launch of the experiment, consistent with gradual self-financed accumulation. In contrast, treatment induces earlier and substantially higher take-up, i.e., both an extensive-margin increase and an acceleration in the timing of adoption.[5]

Subsidy treatment assignment was randomized within strata defined by baseline commute mode. Nearly 70% of the workers in the sample were not using a scooter at baseline and constitute the main analysis sample ("*new adopters*," defined by non-use of scooter at baseline); the remaining 30% were already using a scooter – but wanted to buy a new one – and serve as a placebo comparison ("*incumbent users*").[6] If the subsidy operates by inducing a travel mode shift — from taxis and ride-hailing to own-scooter commuting — no productivity effect is expected among incumbent users, for whom commuting mode is unchanged. Accordingly, all treatment effects are reported separately for new adopters and incumbents.

For new adopters, the intent-to-treat effect on the number of jobs is an increase of 0.54 jobs per week ($p<0.05$) relative to a control mean of 6.12, which corresponds to a 9% increase weekly over an 8-month period. Weekly earnings increase by INR 602 ($p<0.01$) over a control mean of INR 4,758, a 13% increase. Labor supply – the sum of hours marked available to work, waiting for work, time at the customer's location completing work, and travel time – does not change, remaining near 53 hours per week.[7] Since workers earn more with unchanged hours, weekly productivity (earnings per total hours) increases by INR 9.82 ($p<0.01$) from a control mean of INR 94, a 10% gain.

A potential threat to interpretation is endogenous job assignment: if the platform began routing more (or more lucrative) jobs to treated workers in response to their higher completion rates, the observed gains could reflect reallocation by the matching algorithm rather than improved mobility. I find no evidence for such a mechanism. First, event-study estimates show an immediate jump in completed jobs at the time of treatment, and this higher level is sustained over the panel; there is no delayed jump. This timing is inconsistent with a platform-reassignment story, which would require a period of improved post-treatment performance before routing rules are updated. Instead, the pattern points to a direct, contemporaneous effect of the subsidy-induced mobility change on

[5] We restrict to women who had already expressed interest in buying a scooter. This is appropriate for our question—how contract *design* affects take-up among inframarginal (already credit-eligible, interested) workers—rather than whether expanding access brings in new borrowers. The approach aligns with evidence that capital interventions yield returns mainly for people who have previously sought credit (Beaman et al., 2023).

[6] "New adopters" or "Incumber users" are defined by *self-use of a scooter for commuting* at baseline, not by household ownership. Some respondents reported a scooter in the household (e.g., owned by a spouse or relative) that they did not use themselves. Stratification therefore hinges on baseline *use* because the mechanism of interest is a modal shift in the worker's own commute; household ownership without self-use does not change the worker's commuting technology. Baseline status is recorded in the pre-treatment survey.

[7] The lack of change in hours is consistent with the absence of a gift-exchange or other behavioral mechanism that would operate through increased effort.

output. Second, treated and control workers receive statistically indistinguishable numbers of job offers over the study horizon. The offer mix is likewise balanced across treatment arms — by price, distance, and time of day — with no shift toward higher-priced or otherwise favorable jobs for the treated. Finally, institutional details from the firm indicate that the assignment algorithm does not use completion rates to preferentially route demand for equity reasons; persistent non-completion is handled via retraining and counseling rather than through advantaged or disadvantaged job allocation. Together, these facts argue against assignment-driven reallocation and support increased productivity operating through improved mobility.

For the incumbent users (the placebo stratum), coefficients are small and statistically indistinguishable from zero, as expected given that these women already commuted by scooter at baseline. In the pooled sample, including both new adopters and incumbent users, earnings and productivity rise (significant at the 10 percent level), with effects driven entirely by new adopters. Taken together, these results indicate that the program's benefits stem from new (first-time) adopters switching to a more efficient mode of transportation, rather than from behavioral responses among workers who already owned scooters.

The richness of the firm's job-level data allows me to further unpack the productivity gains. Each transaction links a worker identifier to timestamps, locations (and thus home-to-job distances), and job attributes, allowing me to estimate treatment effects on job selection and quality rather than only on aggregates. Among new adopters, treatment expands the spatial (and temporal) reach of completed jobs: weekly distance traveled rises by 6.6 km — an 11% increase — with the entire effect concentrated in 10–25 km trips. There is no detectable change for $<$10 km jobs, indicating that the margin of adjustment is additional medium-to-long trips rather than substitution away from nearby work.[8] Further, I find a reallocation of work toward peak periods. Among new adopters, the number of jobs completed between 8–11 a.m. and after 6 p.m. increases by 0.37 per week (control mean 3.10). In the firm's administrative data, these windows traditionally show female-supply shortfalls and the highest unfilled-job rates; shifting work into them therefore expands output precisely where capacity is most constrained.

An important question is whether the increase in jobs completed comes at the expense of service quality. To investigate this quantity-quality tradeoff, I focus on two outcomes: (i) customer ratings and (ii) late arrival at customer's location. Average ratings among treated workers increase by 0.03 stars ($p < 0.05$) relative to a control mean of 4.83 on a 5-star scale. This closes the gap to a perfect rating by approximately 18%. Given that ratings are heavily skewed above 4, such movements are nontrivial. Moreover, the improvement pushes the average past the firm's internal quality threshold of 4.85, which the platform considers a "good" rating, making this shift operationally meaningful. By contrast, I find no statistically significant change in late arrivals to jobs. This null result is consistent with post-investment selection into farther or peak-hour assignments, which raises travel-time variance even as underlying service quality improves. Follow-up survey evidence

[8] Distances are measured door-to-door and include both outbound and return segments.

supports the quality channel. Treated workers report less back pain and lower stress, plausibly contributing to higher on-the-job service quality as reflected in ratings. Among incumbent users, I find no treatment-induced reallocation – no detectable changes in distance traveled, time-of-day patterns, or job quality – consistent with their placebo role and the fact that their commuting technology was unchanged at baseline.

The firm earns a commission on each completed service, so increases in the number and value of jobs mechanically raise firm revenue. Among new adopters, total revenue generated per worker rises by INR 1,180, a 12% increase ($p<0.01$). Because this gain coincides with higher worker earnings and more completed jobs, it suggests an expansion of realized output or joint surplus, not a pure transfer. A natural concern is that the rise in total revenue reflects business stealing rather than higher output. I rule this out in two ways. First, an event-study restricted to control workers shows no contemporaneous decline in their outcomes around the treatment date: completed jobs and earnings for control workers remains flat around exposure and then drift up modestly in later months, mirroring platform growth. Second, the treatment-induced gains are concentrated in far-distance and late-evening jobs — the very segments with the highest pre-treatment non-fill rates — so added supply is most plausibly converting previously unserved demand into completed transactions rather than displacing other workers. Taken together, these facts indicate that the revenue increase reflects higher aggregate output rather than reallocation across workers.

Turning from revenue to surplus, I estimate profit per worker–week. Among new adopters, profit rises by INR 265 per worker–week, a 13% increase relative to the control mean of INR 2,108. The sign and magnitude are consistent with the platform's 20–30 percent commission on job value and the documented shift toward higher-value jobs. A natural concern is that higher productivity might accelerate exit if outside options are attractive (three rival platforms operate in these cities and offline salon jobs are available). I therefore examine retention directly. I find no evidence of treatment-induced attrition. In the control group, one-year attrition is about 10%; treatment lowers this by about 4 percentage points ($p=0.15$). The estimate is imprecise but economically meaningful and indicates that profit gains are not offset by worker exit. Among the incumbent users, the control group attrition rate is approximately 2% and the treatment effect is near zero, consistent with a retention ceiling and with the premise that mobility was not binding for this group. Taken together, the profit increase for new adopters is not undone by higher attrition despite substantial outside opportunities.

To evaluate the firm's return on the subsidy, I focus on the treatment effect on the treated (TOT). Take-up of the subsidy was around 50% among those offered, and no workers in the control group were offered the subsidy; with this one-sided non-compliance, I estimate TOT by instrumenting take-up of the subsidy with the randomized offer of the subsidy. This estimand is the relevant one for the firm's accounting, since the firm incurs the cost only when a worker accepts the subsidy. The IV estimate implies that profit per treated worker increases by about INR 558 per week, from which the firm recovers the subsidy in 18 weeks and the implied 12-month return on investment is

about 153%.

The high measured return naturally raises the question of why the firm had not already implemented such subsidies. While the experiment cannot isolate the precise source of ex-ante underinvestment, discussions with the firm point to two first-order frictions. First, uncertainty about return on investment: prior to the experiment, the firm had weak priors about the magnitude and durability of productivity gains and about the share of those gains it would capture through its commission structure, rendering the investment's net present value highly uncertain. Second, experimentation to resolve this uncertainty is costly, and the firm faced constrained administrative capacity and competing operational priorities that delayed both the experiment and subsequent scaling decisions. This pattern is consistent with growing evidence that firms may underexperiment with potentially profit-improving practices (Bloom et al., 2013; Hanna et al., 2014). Consistent with the experimental results demonstrating program profitability, the firm scaled the subsidy program to three additional cities, extending the offer to over 3,000 additional female workers. This ex-post expansion provides revealed-preference evidence that, once belief frictions were attenuated, the subsidy is viewed internally as a profitable lever for growth.[9]

To quantify returns to workers, I instrument scooter purchase with randomized subsidy assignment. Treatment assignment increased scooter purchase by 32 percentage points. With two-sided noncompliance, the IV estimates identify a local average treatment effect (LATE) for compliers. The estimated (IV) effect of purchasing a scooter is an increase of INR 2,018 in weekly earnings. The discounted payback period of the scooter is for the new adopters is thus 54 weeks.[10] Thirteen months after rollout, 90% of treated workers remain active, with attrition below that of the control group. Extrapolating the observed effects and survival rate for an additional 12 months implies a 24-month ROI of 73%. [11]

The magnitude of these returns sharpens the central puzzle – why did workers not invest pre-subsidy? I address this next by examining the frictions the subsidy relaxes for the workers. I explore two reasons for underinvestment by workers: (i) incomplete internalization of surplus (workers bear the full cost but capture only a share of output) and (ii) a binding liquidity constraint at adoption (difficulty assembling the down payment). Guided by a simple model of the worker's investment decision under these frictions, I implement an incentive-compatible discrete-choice exercise with a new set of workers. Each worker evaluates a within-person, randomized menu of four contracts: (1) the regular loan — 15% down payment with the remaining 85% financed through loan (similar to the field experiment "control" contract); (2) a proportional price rebate that reduces the total purchase price and scales both the down payment and financed principal proportionally — primarily lowering lifetime cost while doing little to relax upfront cash; (3) a low down-payment contract that cuts cash due today by shifting it into financed principal (raising lifetime cost); and (4) the subsidy contract

[9] Ongoing work analyzes the performance of the scaled program and whether the high returns generalize outside the original experimental sites.

[10] Payback is computed using incremental LATE weekly earnings (INR 2,018) against the present value of the financed purchase cost, discounted. We do not net out the weekly EMI in the earnings flow—doing so would double count financing costs. Details in Section 5 and Appendix B.

[11] Even without taking the subsidy into account, the implied 24-month ROI for the worker is 57%.

— a grant that directly reduces the upfront payment (akin to the field experiment "treatment" contract).[12]

Overall demand varies sharply by contract features: 16% under the regular loan, 31% with a pure price rebate, 66% with a low down-payment contract, and 95% with a subsidy. This already indicates that workers are much more responsive to low down-payment contracts than to a price rebate despite their high overall expense. The mechanism is clearest in the heterogeneity of contract demand by liquidity. Among liquidity-constrained workers (those who report they are unable to assemble INR 15,000 at purchase), demand is largely unresponsive to the regular loan and to a price rebate, but rises to 70% with a low down payment and to 98% with a down-payment subsidy. By contrast, among workers not liquidity-constrained, all alternative contracts raise adoption relative to the benchmark. The two price-reducing offers (rebate and subsidy) perform similarly and yield higher take-up than the low–down-payment option. Taken together, I find that liquidity at the time of purchase (down payment) is the pivotal margin: relief targeted at the upfront cash requirement generates three-to-four times larger adoption responses than an equal-value reduction in lifetime price. Further, any incomplete internalization of the firm's surplus (a pure price effect) is, at most, second-order relative to the binding liquidity constraint at adoption.

Contribution. My first contribution is to provide experimental evidence on returns to worker-owned capital within a firm, and to quantify the surplus it creates on both sides of the employment relationship. A large theoretical literature predicts underinvestment by firms in workers' general (portable) human capital because employers cannot fully appropriate the resulting productivity gains (Becker, 1964).[13] When labor markets are perfectly competitive, even high-return general training is privately unattractive to firms. By contrast, with empirically relevant frictions — informational asymmetries, slow employer learning, search and mobility costs — wages can lie below marginal products, creating rents that make firm investment in even general capital privately profitable (Acemoglu and Pischke, 1999; Autor, 2001; Katz and Ziderman, 1990).

I extend this framework from general *human* capital to general *physical* capital owned by workers. This margin is understudied yet first-order: ownership of simple, portable assets can differentially raise on-the-job productivity among co-workers who have access versus those who do not.[14]. In low- and middle-income settings, such assets are often lumpy and expensive relative to cash flow, making adoption sensitive to purchase-time liquidity. This paper shows that a firm-side contract that lowers the upfront cost of acquisition enables workers to make this investment; enabling worker-owned capital raises measured productivity and generates positive private returns to the firm. The results demonstrate that even in a modern platform-firm environment with comparatively low switching costs, labor-market frictions are sufficient for employers to profitably induce investment in worker-

[12] One stated "yes" is implemented by lottery, ensuring truthful revelation and yielding policy-relevant elasticities.

[13] Recent empirical work has shown evidence of this in agricultural setting with planting technology and in garment factory setting with soft skills (Adhvaryu et al., 2023; Cefala et al., 2024).

[14] Recent work studying randomized provision of eyeglasses that correct presbyopia generated sizable productivity gains for manual workers at minimal cost (Reddy et al., 2018)

owned, general capital and experience no treatment-induced attrition.

My second contribution lies in studying liquidity for workers. This contribution connects two strands of literature. A long line of field experiments studies capital placed with independent producers (entrepreneurs or households), finding high returns to in-kind assets or cash and more mixed effects for standard microcredit (De Mel et al., 2008; Fafchamps et al., 2014; Banerjee et al., 2015b; Meager, 2019; Crépon et al., 2024; Bandiera et al., 2017). In contrast, I bring the capital-provision question *inside a production organization*, relevant directly for workers. There is emerging work showing consequence of liquidity for job choice: a modest, well-timed cash infusion moves Teach For America applicants into teaching (Coffman et al., 2019).

In my experiment, by focusing on workers who already qualify for standard loans yet are unable to access credit, I shift the focus from expanding credit access for marginal borrowers to identifying constraints for a more understudied population, the inframarginal borrowers (Banerjee et al., 2015a).[15] By randomizing employer-mediated contracts that decouple (i) relief of the upfront down payment from (ii) a reduction in total lifetime price, I find evidence that upfront liquidity — not a pure price cut — moves investment decisions among workers.[16] Advancing a large literature — mostly in agriculture — that shows that advancing cash at the moment inputs must be purchased materially raises adoption of high-return inputs or insurance contracts, consistent with binding short-run liquidity and present-bias (Duflo et al., 2008, 2011; Casaburi and Willis, 2018; Jack et al., 2025)

The third contribution of my paper is to the literature on mobility and productivity, especially for female workers already in the labor force. Prior work shows that lowering the generalized cost of travel raises work and earnings: subsidizing seasonal bus travel in Bangladesh increased migration and income, and randomized access to motorcycle financing in Brazil boosted formal employment and wages (Bryan et al., 2014; Van Doornik et al., 2024). I build on these insights within a firm–worker context and for women specifically.

A large, complementary literature documents that mobility constraints suppress women's labor supply in both rich and poor settings (Le Barbanchon et al., 2021; Abou Daher et al., 2023; Borker et al., 2021; Garlick et al., 2025). I extend the evidence from labor-force entry to what improved mobility does for productivity conditional on employment. By enabling private mobility through worker-owned scooters, I show that employed women complete more jobs, shift toward farther and late-evening tasks where returns are higher, earn more, and deliver at least as high service quality; in parallel, savings improve and attrition falls. These results establish that mobility is not only an entry constraint but a first-order production input for working women, with measurable gains in

[15] The workers in my experiment meet the lender's underwriting criteria but still cannot execute the loan (in my setting - due to the down-payment requirement).

[16] The measurement strategy follows the contract-choice tradition in consumer credit—e.g., subprime auto studies that elicit preferences over down-payment and price menus (Adams et al., 2009) — but extends it in two important ways: the choices are incentive compatible with real stakes, and the setting is an employment relationship where the firm can appropriate part of the surplus generated by worker-owned capital.

output, job mix, and retention inside the firm.

Finally, these results hold direct policy relevance in India. Tamil Nadu's Amma Two-Wheeler Scheme, launched in 2018, offers a 50% subsidy (up to INR 25,000) explicitly to help low-income working women to purchase a scooter.[17] Uttar Pradesh has earmarked INR 400 crore (USD 46 million) for the new Rani Laxmi Bai Scooty Scheme to provide scooters to meritorious Class-12 girls entering college, and Rajasthan continues statewide distribution under the Kalibai Bheel Medhavi Chhatra Scooty program through its Higher Education Department — both indicating substantial, recurring budget commitments.[18] My evidence points to a complementary, private-sector path: a within-firm contract that eases the upfront cost of a scooter for working women, financed and implemented by employers who can appropriate part of the productivity gains.

The rest of the paper is organized as follows. Section 2 describes the setting, the platform firm's operations, and the role of mobility in the platform job. Section 3 presents the field experiment, including the intervention, data sources, summary statistics, and empirical specification. Section 4 reports the main results, with subsections on worker outcomes and firm outcomes. Section 5 quantifies returns on investment for the firm from the subsidy and for workers from scooter purchase. Section 6 develops the mechanism test, guided by a simple model and an incentive-compatible discrete-choice experiment, it separates liquidity constraints from price rebates to explain why the subsidy raised adoption.

2 Study Setting

2.1 The Firm

For the study I partner with a large app-based platform-firm in India that intermediates home services. The firm matches customers with electricians, plumbers, beauticians, cleaners, and other specialists through a mobile application. As of early 2025, it operates in over 30 cities, engages roughly 50,000 workers, and has served over 10 million customers. This organizational form exemplifies the formalization of service work through digital intermediation in the Global South (NITI-Aayog, 2022; Datta et al., 2023). Conceptually, the setting is closest to U.S. platforms such as Thumbtack or Taskrabbit, but it differs along several institutional margins. Unlike the U.S. service platforms, where customers select an independent worker based on observables such as hourly fee and experience, in this firm customers book a service and the firm assigns the worker via an internal assignment algorithm; services are standardized and accompanied by firm-led onboarding and periodic training; worker performance is routinely monitored against operational thresholds; and worker onboarding includes rigorous background checks and identity verification.

The experiment focuses on the firm's beauty services category, in which technicians deliver at-home salon services—hair care, waxing, facials, pedicures, and manicures. Tasks are short to medium in

[17] News Article - Launch of Tamil Nadu Scheme. Further details of Tamil Nadu Scooter Scheme.
[18] News Article - Launch of Uttar Pradesh Scheme

duration (typically one to three hours), drawn from standardized menus with standardized inputs supplied by the worker, and demand exhibits pronounced diurnal peaks in early-morning and late-evening slots. Both men and women work in this category; service is gender-matched (female technicians serve female clients; male technicians serve male clients). Customers select bundles of services in the app, which determines the booking value. Because services are performed at the customer's residence, efficient travel is a first-order input to effective labor supply and an important determinant of margins.

Workers face two types of salient outside options: (i) employment with similar app-based intermediaries - there are about two to three national competitors in the at-home beauty segment - and (ii) employment at brick-and-mortar salons (fixed location, manager-set schedules, lower travel but less autonomy).[19] Multi-homing is feasible - the platform does not require exclusivity, so workers could allocate time across competing apps and salon jobs. In practice, most female workers on this platform neither multi-home nor hold other jobs; in my experimental sample, 99% report this platform as their sole source of labor income.

At the start of each week, workers declare availability windows in the app (days and time blocks) and can also renege by toggling off in real time. In this setting, I measure *available hours* as the total time a worker is online and eligible to receive offers —explicitly inclusive of time waiting for jobs, performing jobs, and traveling between locations — which is the operative measure of labor supply. Customer requests are routed to available workers via a centralized algorithm that *sequentially* dispatches offers; workers observe job details (expected earnings, distance, duration, time) and choose to accept or decline. Service prices are standardized and fixed at the category level; there is no intra-day dynamic pricing to clear the market. Pre-set surge multipliers apply only to early-morning and late-evening hours. The firm takes a 20–30% commission on completed jobs.

The platform continuously tracks punctuality, acceptance, cancellations, and customer ratings, enforcing minimum thresholds. Frequent cancellations or sustained inactivity trigger short blocks that temporarily remove the worker from the pool until re-training. All workers complete a formal on-boarding prior to activation and periodic firm-led skill upgrades.

2.2 Mobility as a Production Input for Jobs

As service delivery occurs at customers' residences, commute time, reliability, and routing flexibility are first-order inputs to production. In these cities — broadly representative of urban areas in developing economies — last-mile public transport is patchy; own two-wheeler scooters and private taxis (including ride-hailing) are the relevant modes.[20] In this context, a scooter is *worker-owned capital* that augments effective labor supply by potentially lowering per-job travel time and variance, expanding feasible spatial-temporal coverage, and reducing variable transport costs.

[19] A third option—independent customer acquisition via neighborhood networks or digital classifieds—exists but is less common in our sample.

[20] Car ownership and use are rare in this worker demographic.

Prior to the experiment, we conducted a phone survey of 1531 randomly selected platform workers which showed a large gender gap in actual commute modes for the same salon-service job on the platform. About 77% of men report commuting by their own two–wheeler scooters, relative to 31% of women (Figure 1, Panel A). However, preferences are similarly high across genders: 79% women and 92% men state that they would prefer commuting by own two–wheeler scooter rather than private bike-taxis or three–wheelers (Figure 1, Panel B). This usage–preference gap motivates focusing the intervention on women, for whom adoption is far from saturation. Baseline survey highlights key issues with relying on ride-hailing rather than using one's own scooter. The main ones include difficulty obtaining rides at peak times; higher and more volatile fares in early/late hours; refusals by bike-taxi drivers to carry large service kits; concerns about harassment and unsafe driving. Workers also emphasize that search and waiting time displaces productive hours and rest between appointments.

Administrative data show suggestive productivity differences at baseline. Relative to workers who depend on private taxis for commute, women who already commute by scooter show more higher daily earnings and spend less on transport, while the number of services completed per day is similar across groups (Figure 2). These patterns are consistent with scooters enabling acceptance of better-paying jobs at lower effective cost.[21]

These institutional facts — (i) mobility as a key input, (ii) large gender gaps in usage despite similar preferences, and (iii) baseline productivity gradients correlated with scooter use — motivate an experimental subsidy targeted to women. In the analysis, I separate *new adopters* (women not using a scooter at baseline) from *incumbent users* (already using a scooter at baseline). If the subsidy operates by inducing a *change in mobility modality* (from taxis/three-wheelers to own-scooter commuting), we should not expect productivity effects on the platform among incumbent users, for whom commuting modality is unchanged. Accordingly, all estimates are reported separately for new adopters and incumbent users.

3 Field Experiment: Subsidy for Worker Owned Capital

3.1 Intervention Details

The field experiment randomly offered a one-time purchase subsidy for a two-wheeler ("scooter") — the worker-owned capital in this setting — to female workers who had ex-ante indicated interest in buying a scooter. Figure A1 shows an example scooter in this setting. [22]

[21] Wealth covaries strongly with both productivity and scooter use (Figure A2); the probability of commuting by own scooter rises monotonically across wealth quintiles (Figure A2).Household scooter ownership is also monotone in an asset-based wealth index: 37% of households in the poorest quintile possess at least one scooter, 63% in the second, and over 90% from the third through the richest quintiles.

[22] Throughout this paper the term scooter (or the colloquial "scooty") denotes the lightweight, step-through, gear-less two-wheelers, typically 90-125 cc petrol or electric vehicles such as the Honda Activa and TVS Scooty, that account for 6.85 million wholesale sales in FY 2025, a 17% year-on-year increase and a quarter of India's total two-wheeler market (Autocar Pro). Unlike the stand-up, dockless "kick-scooters" popularised by Lime or Bird in North America,

In the study period, the average invoice price of a scooter was INR 106,000 (approximately USD 1,200). Prices varied across makes and models but only modestly: the interquartile range spanned INR 90,000–123,000 (USD 1,094–1,440). At prevailing earnings, the purchase price corresponds to about twenty weeks of income for this population, implying that acquisition is typically credit-financed. Consistent with standard vehicle-finance contracts in this market, the loan requires a down payment equal to 15% of the invoice price, due at purchase; the remaining 85% is financed at a fixed nominal annual interest rate of 18%. Borrowers select the tenure within a customary range of 12–36 months; the realized mean in the experiment is 26.625 months (s.d. = 7). The loan is collateralized by the asset, so persistent delinquency can trigger repossession of the scooter.

Eligibility required a baseline CIBIL credit score ≥ 700. Operationally, 74% of firm workers satisfied this threshold. Two considerations motivated this criterion. First, it guarantees that any worker randomly offered the subsidy could, if she chose, transact and purchase the scooter on credit. Second, it permits a clean focus on *post–credit-access* frictions: the study concentrates on infra-marginal borrowers to whom the market was already willing to lend, yet who nevertheless did not purchase at the prevailing terms.

CIBIL is one of the main credit bureaus in India. Because its scoring model differs from U.S. FICO, direct level-by-level comparisons are not meaningful; heuristically, a CIBIL score of 700 is often mapped to 600–650 FICO, but this correspondence is only approximate.[23] I document two diagnostic facts about the eligibility screen. First, the empirical CDF of baseline credit scores is smooth in a neighborhood of 700, with no visible bunching, consistent with the absence of manipulation; moreover, a majority of applicants lie above 700 (Figure A4). Second, baseline covariates are well balanced just below versus just above 700: education, marital status, recent earnings, jobs completed, travel expenditure, pre-experiment scooter-loan activity, and household composition show no economically meaningful discontinuities (some individual p-values are marginal, but the joint pattern indicates tight balance). Note that a lower CIBIL score at a point in time may also reflect a recently originated loan. These checks support the interpretation that conditioning on CIBIL ≥ 700 secures credit availability without inducing material composition shifts.

Treated workers receive a flat, one-time purchase subsidy of INR 10,000 (USD 120), paid directly to the dealership at the time of purchase rather than to the worker. The transfer reduces the effective purchase price by about 10% relative to the sample mean scooter price. Because it is applied at the

these Indian scooters are ridden seated, licensed for road traffic, and designed for trips well beyond first- and last-mile range. Their step-through frame, continuously-variable (gearless) transmission and low weight reduce physical exertion and make them especially attractive to novice and female riders. Manufacturers increasingly market scooters to women because females constitute only about 10% of current two-wheeler owners, leaving substantial head-room for growth (The Economic Times 2025). At the national level nearly half of all Indian households already own some form of two-wheeler (The Times of India 2023).

[23] There is no authoritative CIBIL to U.S. (FICO) conversion. Some providers (e.g., Nova Credit) offer proprietary, odds-based translations that remap a foreign bureau score into a U.S.-equivalent score range; Nova's documentation and sample reports describe this as a mapping based on underlying default-odds tables rather than a deterministic crosswalk. For intuition only (not a numeric crosswalk), note that Indian bureaus use a 300–900 scale and describe ≈700+ as "good" while FICO scores typically span 300–850 with "good" defined as 670–739. (Source).

point of sale, the subsidy is maximally salient at the down-payment stage: it lowers the scooter's net price exactly when the down payment is due, while leaving the financed principal unchanged. Control workers receive no subsidy; they are, however, connected to the lender and may purchase on the standard market contract. Later, in Section 6, I present a simple model that separates liquidity from price channels to interpret the mechanisms.

From August to October 2024, an in-app invitation to apply for the scooter-subsidy study was sent to 3,000 randomly selected female salon workers across Delhi, Pune, and Bengaluru. Approximately 1,200 workers (a 40% response rate) contacted the research team: 50% from Delhi, 25% from Bengaluru, and 25% from Pune. A light-touch verification (government ID and address check) was completed by the research team to screen out low-commitment inquiries. Enumerators then administered a baseline survey and obtained documentary proof of each applicant's credit score. Workers meeting the credit-access criterion (CIBIL $\geq$ 700) entered rolling, city-specific cohorts. Within each cohort, I stratified by baseline commute mode (already using a scooter vs. not using a scooter) and randomized assignment, offering the subsidy to 60% of eligible workers. The resulting experimental sample comprises 350 women. [24]

3.2 Data Sources

The empirical analysis combines three complementary sources: (i) high-frequency administrative records on worker productivity and firm revenue, (ii) a pre-intervention baseline survey, and (iii) three waves of post-intervention follow-up surveys.

The administrative data spans March 2024–June 2025 and comes from two complementary files maintained by the platform. The *calendar dataset* provides a day-by-day snapshot of intended and realized availability. In particular, workers declare the hours they are willing to work (up to a week in advance) and may revise these declarations at any time; the file therefore records, for each worker–day, (i) whether the worker reported herself available or absent, (ii) whether availability covered the full day or specific hourly blocks, and (iii) whether the worker was temporarily suspended for performance shortfalls. Because availability is the gateway to matching, these records define the *active matching pool* by day. I then merge the calendar data with the *job-level master data* at the worker–day level, and later aggregate to worker–weeks to smooth transitory shocks.[25]

The *job-level master dataset* records *every offer* sent to a worker on a day she has marked herself available—irrespective of whether the offer is accepted—thereby revealing the full *choice set* conditional on availability. For each offer the file records: the worker's location identifier and the client location

[24] Sample-selection funnel: From 1,200 initial inquiries, 697 were screened out at the light ID and address verification and baseline scheduling stage, leaving 503 candidates (41.9% of initial). The documentary credit-score screen (CIBIL $\geq$ 700) excluded a further 153, yielding 350 eligible workers (29.2% of initial; 69.6% of post-ID stage). Randomization was conducted within rolling city cohorts, stratified by baseline scooter use, with the subsidy offered to 60% of eligible workers.

[25] Results using daily data are qualitatively identical and sufficiently powered. I report worker–week aggregates to smooth idiosyncratic day shocks (e.g., weather, illness, platform outages), absorb strong day-of-week seasonality; the weekly horizon also aligns with the platform's operational accounting cycle.

(used to compute home–to–client distance), the scheduled start time and expected duration, the combination of service-tasks involved, the worker payout (income) that would be earned if accepted, and the platform's commission, revenue per job. Conditional on acceptance, the dataset adds operational and quality outcomes: completion status, arrival-time stamps (punctuality), cancellations or reschedules, the client's rating, and any complaint flags. This structure allows me to (i) trace *where* and *when* work occurs (distance bands and time-of-day windows), (ii) measure reliability and quality using the same indicators the firm uses operationally, and (iii) observe *both sides of the transaction* — worker earnings and the firm's realized profits — at high frequency.

The baseline survey, administered immediately before the intervention, records a rich set of demographic and socioeconomic characteristics such as household composition, primary commuting mode, whether the platform job is the respondent's sole employment, prior scooter-loan uptake, and related covariates. To measure pre-specified, mechanism-relevant outcomes that are not observable in administrative records, I fielded three follow-up surveys at approximately equal intervals. Each wave collects a job-by-job commuting log for the preceding day — transport mode and round-trip expenditure for every completed job. Waves 1 and 3 additionally elicit the employment status and earnings of all adult household members to measure any spillovers. To assess women's autonomy, the instruments include modules on self-confidence, decision-making authority, and freedom of movement. I construct a self-confidence index by averaging standardized responses to four Likert-scale items covering household decision-making, task self-efficacy, pursuit of personal interests, and independent mobility. Leisure is summarized by an index based on four binary indicators for the prior week: visiting friends, visiting relatives, undertaking an outside leisure outing (cinema, mall, or park), and engaging in self-care. Survey attrition remains below ten percent in every wave and is balanced across treatment arms.

3.3 Summary Statistics and Baseline Balance

Table A2 presents summary statistics and baseline balance. Looking at the full sample, a typical worker in is about 34 years old. About 75% are married and nearly 88% report having at least one kid. About 30% of the sample already use their own scooter for work, and among women who don't use their own mode to commute, about 80% hail two or three wheeler taxis (remaining rely on a family member to drop them, while use of public transport like metros or buses is negligible - due to the last mile connectivity requirement for these jobs). 90% of the sample report knowing how to drive a scooter at baseline. On average, respondents report two earning members in a household, with about 8% reporting having an unemployed husband. For 98% workers, platform work is their sole and regular source of earnings.

The wealth index is constructed as the first principal component from a principal component analysis of household economic indicators, including housing ownership (owned or rental) and ownership of consumer durables such as a refrigerator, television, laptop, personal vehicle, air conditioner, and

washing machine.[26] At baseline, scooter use is strongly increasing in wealth: the share who drive their own scooter rises from 13.5% in the poorest quintile to 21.2%, 30.8%, 40.3%, and 45.8% in successively richer quintiles (Figure A2).

At baseline, workers had about three years of platform tenure, completed about one service per day, and earned around 790 per day and 5,200 per week; the mean app rating was 4.84 out of 5. Baseline means are similar across treatment and control in the pooled sample and within the incumbent-user sub group and new-adopter sub-groups; differences are small and rarely statistically significant.

Figures 3 document two empirical regularities in the job-level administrative data that motivate the subsequent analysis of scooter adoption. Panel (a) of Figure 3 shows that job-value rise with client distance from the worker's home location; Panel (b) of the same figure shows that earnings per minute are higher for jobs booked in the early-morning (7–11h) and late-evening (17–19h) windows than for mid-day slots. These unconditional patterns indicate that the return to mobility is not homogeneous: the marginal value of an additional kilometer of reach and of availability at off-peak hours is higher. Because scooters primarily relax travel-time and travel-variance constraints, they are plausibly most productive exactly on these margins.

Similarly, Figure 4 shows that, on average, 84% of jobs for a given day are fulfilled (completed) in the system. However, completion rates are substantially lower at the early and late time windows relative to mid-day, and decline with client distance, consistent with higher travel frictions and tighter punctuality constraints in those segments. Put differently, the platform compensates workers more for far-away and early or late jobs, but those jobs also carry a higher probability of non-completion. Taken together, these facts rationalize why a scooter *could* raise earnings not merely by doing more of the same jobs or by lowering per-job travel costs. Adoption may expand the feasible set toward (i) farther jobs and (ii) early or late bookings that pay more, while (iii) improving reliability on those riskier segments by reducing travel time and its variance. The figures thus provide descriptive context and pre-analysis guidance for heterogeneous treatment effects by distance and booking hour.

3.4 Empirical Strategy

To estimate the impact of the subsidy offered, I use the following empirical specification:

$$Y_{it} = \beta_1 Subsidy_i + \beta_0 Y_{0i_{DPL}} + \delta_C \times \lambda_S + \gamma_t + + \varepsilon_{it} \tag{1}$$

where Y_{it} is the outcome variable of interest (e.g. worker's weekly jobs or earnings or firm's profits), $Subsidy_i$ is the indicator for whether the worker is offered the subsidy (i.e. treatment), $Y_{0i_{DPL}}$ is the baseline value of the dependent variable, δ_C are randomization cohort $\times$ λ_S randomization strata fixed effects, γ_t represents week fixed effects for all administrative data analysis - as data is aggregated to the weekly level. Standard errors are clustered at the worker level. The analysis

[26] This is the standard method used to create wealth indices in development economics literature such as in De Mel et al. (2008).

uses 8 months of post-randomization data collapsed at the weekly level. $\hat{\beta}_1$ thus provides the intent-to-treat causal estimate of being offered a subsidy.

For outcomes that are observed only at discrete survey waves (e.g., travel expenditure or household income), I re-specify equation (1) with survey-round fixed effects (instead of calendar week fixed effects) so that treated and control workers interviewed in the same wave are compared after netting out any round-specific shocks.

4 Results: Impact of Subsidy for Worker Owned Capital

4.1 Take-up of the Subsidy

I first study take-up of the *subsidy offer* and, separately, take-up of the *scooter purchase*. By design, only treated workers could redeem the subsidy: 49% of the treatment group used the INR 10,000 offer to purchase a scooter on credit, whereas 0% in control did so because no subsidy was offered. Turning to adoption, over the period from rollout to the final survey round (approximately ten months), the *treatment effect on purchasing a scooter* is 32 percentage points: 49% in treatment versus 17% in control, with the control rate plausibly reflecting baseline purchasing absent the program (as shown in Table 1). Figure A5 plots cumulative take-up in the treatment arm. Take-up rises sharply from 15% at randomization to just above 35% after one month, reaching nearly 45% by month 2. Thus, about 90% of the eventual treatment response materializes within the first two months, consistent with a prompt demand response to the cash incentive. Thereafter, the curves largely plateau, over the subsequent eight months, additional uptake is modest.

Table A3 shows that take-up rates are statistically indistinguishable between two baseline groups: *incumbent users* (those already commuting by scooter) and *new adopters* (those not using a private scooter at baseline). Two mechanisms can rationalize comparably high take-up among incumbents. First, replacement and quality upgrading. Many incumbents relied on aging or shared vehicles with high maintenance costs (the mean scooter age among incumbents is seven years), so a discounted new scooter is privately attractive under the subsidy. Second, intra-household reallocation. An additional vehicle may be valuable to other household members, which raises the marginal benefit of purchase even for incumbents.

The estimated treatment effect on purchase is highly stable across observable characteristics. Interacting treatment with more than twenty baseline covariates yields effects concentrated in the 0.30–0.36 range, indicating that the subsidy increases purchases broadly rather than only for specific types. Only a small subset of interactions reaches conventional significance. Among new adopters, take-up is increasing in age and in the number of household members who can ride a scooter.

4.2 Effects on Worker Outcomes

4.2.1 Jobs, Earnings and Productivity

Using an eight-month post-randomization panel aggregated at the worker–week level, I estimate intent-to-treat (ITT) effects from administrative data via Equation 1. The main sample of interest comprises *new adopters* — workers who did not use a private scooter at baseline (just over 70% of the experimental sample).[27] Panel A Column 1 of Table 2 shows that treated new adopters complete 0.54 additional jobs per week (s.e. 0.23; $p < 0.05$) relative to a control mean of 6.12. This is a 9% increase, sizable at the weekly horizon and persistent over the eight-month window.

Weekly earnings rise by INR 602.40 (s.e. 204.41; $p < 0.01$) over a control mean of INR 4,757.98, a 13% gain. The earnings effect exceeds the output effect, indicating a composition margin toward higher-value jobs rather than a pure increase in volume (Table 2 Panel A Column 2). To measure the response on the extensive margin of labor supply, I use "hours online" from the administrative records, defined as all time windows during which the worker is in the active matching pool—waiting for matches, traveling to jobs, or performing jobs. Using this measure, I find no statistically significant change in time supplied relative to control. The null effect therefore indicates no meaningful expansion in the total time the worker allocates to the platform ecosystem; the earnings gains documented above come from higher productivity within given time supplied rather than from supplying more time.

Because hours do not rise, the earnings gain translates into a higher return per hour online. Earnings-per-online-hour increase by INR 9.82 (s.e. 3.59; $p < 0.01$) relative to a control mean of INR 94.05, an improvement of about 10%. Taken together — +9% jobs, +13% earnings, and no detectable change in hours — these estimates imply that the subsidy allows new adopters to reallocate into higher-value jobs and to earn more per unit of time, rather than simply working longer. The magnitudes are large in weekly terms and remain stable when I vary controls, sample windows, or aggregation.

Incumbent users — workers already commuting by scooter at baseline — serve as a natural placebo group for productivity effects: purchases here are predominantly replacement (or upgrading an older scooter) or intra-household reallocation of the new scooter rather than first-time expansions of mobility. Consistent with that interpretation, Panel B shows no detectable effects on any outcome. The ITT estimates are small and imprecise relative to the corresponding control means, suggesting that the subsidy does not raise measured productivity for incumbents (Table 2 Panel B).

In the pooled sample (Table 2 Panel C), the subsidy raises weekly earnings by INR 322 (s.e. 170; $p < 0.10$) from a control mean of INR 5,098, and increases earnings per online hour by INR 5.37 (s.e. 3.21; $p < 0.10$); effects on jobs and hours online are small and imprecise. These averages mask sharp heterogeneity: the gains are concentrated among *new adopters* (baseline non-users), who earn more without supplying additional online time—consistent with reallocation toward higher-value tasks—whereas *incumbent users* (baseline users) exhibit no detectable effects on any margin,

[27] Recall that randomization was stratified at baseline by new adopters vs. incumbent users.

consistent with replacement or intra-household reallocation rather than capacity expansion.[28]

Commuting impacts. Using three follow-up surveys per respondent, I collect detailed, job-level travel data that allow precise measurement of per-job commuting expenditures and time. As shown in Table A7 column 1, the subsidy offer reduces travel expenditure for new adopters (Panel A) and incumbent users (Panel B). For new adopters, per-job travel expenditure falls by about 16%. For incumbent users — who already rode scooters prior to treatment — the decline is about 20 percent. The larger percentage drop for incumbents may seem at odds with the bigger overall gains for new adopters, but the comparison reflects composition: treated new adopters reallocate toward farther jobs (as discussed in next sub-section and Table 3), which mechanically dampens the per-job saving even as their total earnings and job counts rise. In fact, it might appear surprising that travel expenditure should fall for incumbent users. The fall however is mechanically consistent with upgrading an aging asset: replacing the median seven-year-old scooter raises kilometers-per-liter by 25 percent, which, holding fuel prices and routing fixed, maps one-for-one into lower cost per kilometer and therefore lower cost per job. Time outcomes display the expected asymmetry: travel time per job declines for incumbents, while first-time users show no precise change—plausibly reflecting learning and parking frictions during the transition to own-vehicle commuting. Overall, these patterns are exactly what a worker-owned-capital view predicts: the offer compresses the shadow price of mobility for all workers, and when the asset was already in use, quality upgrading (higher mileage) and smoother operations translate into both monetary savings and time efficiencies.

Spillover effects on other household members. As shown in Table A7 column 3, the subsidy raises family-member earnings, and this gain is concentrated in households of incumbent users. There is no movement in the husband's unemployment rate (Table A7 column 4), so the adjustment operates on the intensive margin — more hours or higher-pay tasks — rather than through new employment. This pattern is consistent with the worker-owned-capital mechanism operating on the *other* household worker: incumbent user's were able to allocate an extra scooter to the second worker in the household, who was previously constrained in accessing productive work. In contrast, for new adopters the estimated change in family-member income is not statistically distinguishable from zero, consistent with the idea that the primary margin for the households of new adopters is the treated worker's own transition to scooter use rather than an immediate reallocation to another member. Taken together, the evidence points to worker-owned capital relaxing binding constraints within the household and lifting earnings where capacity to use the asset pre-existed.

4.2.2 Reallocation by Distance and Time Window

I next examine how the subsidy changes the composition of completed jobs for new adopters. Table 3 shows a sizable expansion in spatial reach: treated workers travel an additional 6.67 kilometers

[28] Daily level estimates are qualitatively identical and adequately powered; I present worker–week aggregates to smooth day shocks. Tables A4, A5 and A6 show robustness for main results for new adopters, incumbernt users and pooled sample. The results are similar to the main specficiation presented and discussed in Table 2.

per week (s.e. 2.55, $p < 0.01$) relative to a control mean of 58.40 km, an 11.4% increase . Given that the new adopters are doing more jobs, the fact that they travel more is not surprising.

However, interestingly, the composition of trips shifts toward longer hauls (Table 3, Columns 2-3). There is no detectable change in *near jobs*, defined as jobs with round-trip distance ≤ 10 km, the estimate statistically indistinguishable from zero relative to the control mean of 3.49. By contrast, *far jobs* — assignments with round-trip distance > 10 km and ≤ 25 km (i.e., 5–12.5 km each way) — increase by 0.54 per week (s.e. 0.23, $p < 0.05$) over a control mean of 2.68, a 20% rise. A Wald test rejects the null that the near- and far-job coefficients are equal ($p = 0.005$), indicating that the distance margin is driven by additional medium–to–long trips rather than changes in nearby work.

Consistent with the descriptive facts that early-morning (before 11h) and late-evening (after 17h) bookings carry both higher pay and higher non-completion risk, while mid-day (12–16h) jobs are easier to fulfill, scooter access reallocates labor supply toward the riskier, better-paid windows (Table 3 Column 5). Mid-day activity is essentially unchanged (Table 3 Column 4). By contrast, early and late jobs rise by 0.37 per week (s.e. 0.21, $p < 0.10$) from a control mean of 3.10 — about 12% increase. A formal test cannot reject equality of the early/late and mid-day effects ($p = 0.195$), but paired with the distance results, the pattern is economically coherent: adoption expands workers' feasible set exactly where travel-time and travel-variance constraints bite most (farther, off-peak work).

This reallocation toward segments with both higher posted returns and higher baseline failure risk translates into substantial gains in contingent pay: surge earnings increase by INR 18.67 per week (s.e. 6.66, $p < 0.01$) over a control mean of INR 43.97 — a 40% rise as seen in Table 3 Column 6.

4.2.3 Job Quality

Client ratings rise by 0.03 points (s.e. 0.01, $p < 0.10$) from a baseline mean of 4.83 on a 5-point scale. Because ratings are tightly bunched above 4 (top-coding is pervasive), this is a meaningful shift: it closes roughly 17% of the residual gap to a perfect score and pushes the average past 4.85, a threshold the firm informally treats as "excellent" (Table 3 Column 7).

Despite the reallocation toward farther and off-peak jobs, the "late to job" indicator does not improve, as seen in Table 3 Column 8: +0.18 (s.e. 0.16) on a control mean of 2.49 incidents per worker, statistically indistinguishable from zero. This pattern is consistent with offsetting forces: scooters reduce travel time and its variance (mitigating delays), but learning and parking or search frictions for new adopters, along with longer average trip distances, limit mechanical gains in punctuality. The net quality effect therefore appears to operate primarily through the interaction margin — workers arrive less stressed and with lower physical strain (e.g., less back pain from ride-hailing and waiting), leading to modest but economically relevant improvements in client ratings without a detectable change in tardiness.

4.3 Effects on Firm Outcomes

4.3.1 Firm Revenue and Profits

For new adopters, i.e. workers who were not using a scooter pre-experiment, the subsidy translates into sizable, internally consistent gains for the platform. Table 4 shows impact on the firm. Panel A Column 1 shows that revenue rises by INR 1,178.95 per worker–week (s.e. 404.54; $p < 0.01$) on a control mean of INR 9,369.21, an increase of 13%. After paying workers, the platform's revenue net of payouts increases by INR 575.72 (s.e. 202.63; $p < 0.01$) on a control mean of INR 4,611.23 (Panel A Column 2). Profits move one-for-one with these margins (in percentage terms): +INR 265.26 (s.e. 91.02; $p < 0.01$) relative to a mean of INR 2,108.07 (12–13%) (Table 4 Column 3). The pattern is consistent with the mechanism that by relaxing mobility constraints, adoption shifts labor supply toward farther and off-peak jobs with higher marginal revenue, raising both gross receipts and the platform's commission base.

Among incumbent users, point estimates are small and imprecise across gross revenue, net revenue, and profits (Table 4 Panel B). This null is predicted by the mechanism. The subsidy does not relax a binding mobility constraint for incumbents, who had already selected into using a scooter and into the high-return distance or time margins. Control means in fact show that incumbent users generate more revenue than new adopters at baseline — gross revenue INR 11,219 vs. 9,369, net revenue 5,420 vs. 4,611, and profits 2,524 vs. 2,108 — consistent with prior selection into scooter use and high-return segments. The subsidy lifts new adopters' gross revenue by INR 1,179, net revenue by INR 576, and profits by INR 265 per week, closing about two-thirds of the incumbent–new baseline gap on each margin.

When pooling the full sample, the ITT estimates are positive and significant (Table 4 Panel C). Gross revenue rises by INR 681.59 per worker–week, revenue net of payouts by INR 336.30, and profits by INR 153.36 (all $p < 0.05$). These pooled estimates are a weighted average of large, precisely estimated gains for first-time adopters and near-zero effects for incumbents, so they primarily reflect the share of new adopters in the sample and the magnitude of their treatment effects. In aggregation, pronounced heterogeneity at the subgroup level appears as moderate but statistically meaningful improvements at the firm level.

4.3.2 Attrition

Table 4 Column 4 examines whether improved mobility induces exits from the platform over the subsequent year. In the control group, inactivity at one year is 0.109 for new adopters and 0.022 for incumbent users, which indicates that incumbents are much less likely to leave in control group. The ITT effect on attrition for new adopters is −0.040 with s.e. 0.045 (Table 4 Panel A Column 4), which implies a reduction in attrition of about four percentage points from a 10.9 percent baseline although the estimate is imprecise. For incumbents the ITT effect is −0.015 with s.e. 0.037 on a 2.2 percent baseline. Pooled across all workers the ITT effect is −0.036 with s.e. 0.031 on a 8.5

percent baseline.

Taken together, these estimates show higher productivity for treated workers with no detectable increase in exits. This pattern matches the Acemoglu and Pischke (1999) view that firm investment is sustainable when labor market frictions or wage compression allow the firm to retain part of the surplus generated by the investment. Here the scooter subsidy is akin to firm-sponsored training. Workers become more productive, yet separations do not rise.

Several mechanisms could plausibly explain this behavior on the ground. The platform aggregates time-sensitive demand and reduces search costs, which are difficult to reproduce off platform. Reputation and ratings are partly platform specific, which limits portability. Switching costs are present because new adopters learn routes and customer routines over time. Outside opportunities may offer less schedule flexibility and weaker payment reliability, features that are especially valuable for women who face tighter household time constraints. Safety and the option value of surge access further raise the value of staying. These frictions help the platform retain a share of the marginal product created by the subsidy, which rationalizes firm investment in worker-owned capital without inducing higher attrition.

5 Return to Investment

5.1 Returns to Subsidy for the Firm

To compute returns to the firm, the relevant causal estimate is the treatment effect on the treated. The firm pays the subsidy only when a worker takes it up. Since the subsidy was offered only to the treatment group and never to the control group, the experiment generates one-sided noncompliance. Take-up was 49 percent in treatment and zero in control, which delivers a strong first stage. I therefore estimate the effect of subsidy by instrumenting take-up of subsidy with the offer of subsidy. Under the standard exclusion and monotonicity assumptions, the IV coefficient identifies the average causal effect for adopters in the treatment arm. This is the treatment effect on the treated that is required for the return calculation.

Before moving to the returns to the firm, I first establish that the measured gains do not come from work displacement of other workers. A controls-only event study that indexes weeks by each control worker's work week shows no decline in weekly jobs after assignment as seen in Figure A7. Second, the intensive-margin gains we saw above for treated workers are concentrated in early and late hours and at farther distances where the platform records high unfilled demand and higher surge pay, which points to previously unused capacity rather than crowd-out. Third, a representative city has roughly three thousand active workers and about thirty compliers in our experiment. The estimated effect is about two extra jobs per complier per week. Even if every additional job performed by a complier were taken from someone else, the implied loss would be about thirty times two divided by three thousand which is approximately two hundredths of a job per worker per week. This magnitude is economically small. Taken together, these facts indicate that the revenue and profit gains reflect

added production rather than reallocation.

With job displacement unlikely to be first order, I proceed to quantify the payback period and the firm's return on investment. Table 5 Panel A shows that the TOT weekly profits rise by INR 557.87 for first-time users and by INR 345.95 in the pooled sample. To compute recovery time, I compare the subsidy cost to the weekly profit lift. The implied payback period is about eighteen weeks for the new adopter group and about twenty-nine weeks for the pooled sample (that is, including new adopters and incumbent users).

For the twelve-month return, I cumulate incremental weekly profits over the first year and account for observed survival. Months one through eight are realized in the data. For months nine through twelve I scale the weekly effect by the observed twelve-month survival rate. I then subtract the subsidy cost and divide by the cost to obtain an annualized return per subsidy delivered. Using this procedure, the return per treated worker is about 153% for new adopters. The corresponding figure for the pooled sample is about 60%. These estimates use conservative assumptions and match the implied payback windows reported above.

Two conclusions follow. First, for workers who newly adopt a scooter, the firm recovers its outlay within the first half-year and earns a large positive return by month twelve. Second, even after averaging in incumbents who were unconstrained at baseline, the investment remains profitable over the first year. Combined with the flat attrition results, the pattern is consistent with the firm recouping part of the productivity gain in a labor market with meaningful frictions, and the subsidy therefore expands joint surplus rather than re-shuffling output across workers.

Given large returns to subsidy for the firm, a natural question that arises is why the firm had not already financed scooter adoption. Three considerations emerge consistently in interviews and internal discussions with firm managers. First, contractibility is imperfect. Scooters are a general-purpose asset that workers can redeploy outside the platform, which makes full financing risky when separation risk is non-trivial and collateral is weak. Second, ex ante uncertainty about payback was material. Prior to the experiment, managers could not verify whether incremental profits would be sufficient to recover the subsidy within a reasonable horizon, especially given concerns about displacement and survival. Third, organizational frictions matter. Administrative capacity was limited, competing projects drew on the same teams, and piloting a new reimbursement workflow had non-trivial fixed costs.

The experiment reduces each of these barriers. It produces clean evidence on take-up, payback, and survival, and it shows no displacement of untreated workers. In response, the firm has scaled the program to additional cities. With frictions that allow the platform to retain part of the surplus, the firm now finds it optimal to invest in worker-owned capital.

5.2 Returns to Scooter Purchase for the Worker

For workers the object of interest is the payoff to purchasing a scooter on credit. In the treatment group, 49% purchase; in control, 17% also purchase. Because some treated workers do not buy and some control workers do, adoption exhibits two–sided noncompliance. I therefore estimate the effect of purchasing a scooter by instrumenting actual purchase with randomized offer of the subsidy. Under standard exclusion and monotonicity assumptions the IV coefficient identifies the local average treatment effect (LATE) for compliers. This is the effect relevant for workers who adopt when offered.

A potential threat to the exclusion restriction is a direct income effect for always-takers in the treatment group. About 17% would have purchased a scooter irrespective of assignment. In treatment they also receive the INR 10,000 subsidy, which could raise earnings or hours through a wealth shock rather than through adoption itself. Three considerations make this unlikely to matter for our estimates. First, the transfer is small relative to baseline earnings. INR 10,000 corresponds to 2.3 weeks of income, and labor supply is not very responsive to such modest wealth shocks at weekly horizons. Second, even if there is some response, the elasticity of labor supply to small wealth changes is plausibly very low, so any bias would be limited in magnitude. Third, compliers constitute the majority of treated adopters, which mechanically attenuates any influence from always-takers. Taken together, these points make a direct-income violation of exclusion unlikely. I therefore interpret the IV coefficient as the causal effect of purchasing a scooter for compliers.

Table 5 Panel B shows that for new adopters the IV estimate of weekly earnings is INR 2,018 with a standard error of INR 768. The weekly loan payment in the standard credit contract is approximately INR 1,040. Incremental earnings thus exceed the installment from the first week, so the investment is cash-flow positive immediately. These estimates imply an expected payback period of approximately 54 weeks (see Appendix B for details).

I next quantify a two-year return on investment for adopters. A two-year horizon is natural for workers in this setting. In my experimental sample, women's average tenure on the platform is close to three years, and conditional on surviving the first year many remain attached for six years. I therefore report a 24-month return that aligns with the asset's useful life and observed firm attachment. The calculation cumulates incremental weekly earnings over two years, discounts future flows at the worker's borrowing rate, and applies survival adjustment for months nine through twenty-four using the observed hazard rate. I then compare the present value of incremental earnings with the financed purchase cost. [29] The implied two-year ROI for the new adopters is about 73%. While this computation assumes the worker's cost net of subsidy, even without accounting for the subsidy, the ROI over 24 months is 57%. For completeness I also compute the annual internal rate of return for the worker's asset which is over 100%.[30]

[29] See Appendix B for details.

[30] The IRR is the discount rate that equates the present value of incremental earnings to the purchase cost, whereas the ROI compares the present value of incremental earnings minus cost to cost over a fixed horizon. Using the same

Why, then, did many workers not invest prior to the experiment? Mis-calibrated beliefs do not appear to be the primary explanation. Baseline surveys show that the average worker expected larger gains than those realized ex post. Mean expected monthly revenue gains are around INR 27,400 while realized gains are closer to INR 13,600, and expected job increases average 19 per month versus an realized increase closer to 8 (as seen in Figure A8). Belief heterogeneity does predict heterogeneity in treatment effects, but on average expectations are optimistic rather than pessimistic (see Figure A9).

These facts point away from under-expectation and toward direct frictions that the subsidy relaxes, such as liquidity, risk, and learning costs associated with first-time adoption. I investigate these mechanisms in the next section using a targeted mechanism experiment.

6 Mechanism Experiment: Liquidity Constraint vs. Price Response

6.1 Model of Investment Choice by Workers

In the field intervention the subsidy lowers the required down payment at purchase. Subsidy bundles two channels that can raise adoption. It reduces the lifetime user cost of the asset and it relaxes the up-front liquidity constraint. The resulting take-up response does not identify which margin is pivotal, and that distinction is central for identifying the operative constraint on worker investment. Informal interviews with workers pointed to two potential distinct constraints. Some workers report not investing because they would bear the full acquisition cost while capturing only part of the surplus, which weakens private incentives. Others report not investing because the down payment is binding given limited cash on hand at the time of purchase.

To isolate these channels, consider two counterfactual contracts that hold everything else fixed. The first is a pure price contract that reduces the purchase price and leaves the down payment nearly unchanged. The second is a financing contract that smooths the initial cash requirement by allowing the worker to pay only a small amount at purchase and to roll the remaining up-front amount into the financed principal. These two thought experiments motivate the model. The first maps to a return-appropriation friction that depresses the private value of investment relative to its social value. The second maps to a liquidity constraint at the down payment margin. The model below organizes predictions for adoption under each contract, which I test in the mechanism experiment that follows.

6.1.1 Model Set Up.

Consistent with the economic environment in this setting, assume workers are loan-eligible conditional on bringing the required down payment. The lender requires a fixed share $\delta \in (0, 1)$ of the list price $S > 0$ of scooter at time of purchase; the remainder ($\phi \equiv 1 - \delta$) is financed on credit. The financed principal accrues interest at a per-rupee present value interest $\iota \geq 0$. The worker chooses the level

inputs as above, the implied annual IRR is over 100 percent. Full details are in Appendix B.

of investment k, given liquid cash L, retained share $\alpha \in (0,1]$, and technology f with $f' > 0 > f''$. In Appendix C, I re-write the model using a k as a discrete lumpy investment decision instead of continuous (as shown here) - and the predictions are the same. Presenting continuous k version here for simplicity.

The two levers that the firm can implement to impact the investment decision of the worker are, first, provide a **price rebate** contract, where the rebate is $\theta \in [0,1)$ so the list price is $(1-\theta)S$. Or second, a **low down payment (no discount) contract** (henceforth referred to as **low-DP** contract) $g \geq 0$ (reduces cash due today by g and adds g to financed principal).

Cost to the Worker. Hence, the down payment required is $D(\theta, g) = \delta(1-\theta)S - g$. The financed principal becomes $\mathrm{P}(\theta, g) = (1-\delta)(1-\theta)S + g$. And the present value interest charge becomes: $\iota \times \mathrm{P}(\theta, g) = \iota\big[\phi(1-\theta)S + g\big]$. Since $\mathrm{D} + \mathrm{P} = (1-\theta)S$, the total present value cost per unit becomes

$$c(\theta, g) = (1-\theta)S + \iota\big[\phi(1-\theta)S + g\big] = (1-\theta)\,\bar{P} + \iota g, \text{ where, } \bar{P} \equiv S\big[1 + \iota(1-\delta)\big].$$

Worker solves. The worker's maximization problem is given by:

$$\max_{k \geq 0} \; \alpha f(k) - c(\theta, g)\, k \qquad \text{s.t.} \qquad D(\theta, g)\, k \leq L.$$

where, the down-payment she can offer is constrained by the liquidity L. In order to solve this, let's consider two cases. One where liquidity does not bind, and the other where liquidity does bind.

Case 1. Liquidity does not bind (interior solution). If the constraint is slack, the multiplier is $\lambda = 0$ and the interior solution satisfies:

$$\boxed{\alpha f'(k^*) = (1-\theta)\bar{P} + \iota g, \qquad \alpha f''(k^*) < 0}.$$

Prediction 1A Since $\partial c / \partial \theta = -\bar{P}$, the comparative static gives:

$$\frac{\partial k^*}{\partial \theta} = \frac{\bar{P}}{\alpha(-f''(k^*))} > 0.$$

This shows, that if the worker receives a price rebate contract, it should strictly increase investment.

Prediction 1B Because $\partial c / \partial g = \iota > 0$,

$$\frac{\partial k^*}{\partial g} = \frac{\iota}{\alpha f''(k^*)} < 0.$$

Low-DP contract should in fact reduce investment when liquidity does not bind (as it raises the present value marginal cost through ιg).

Corollary 1: Price rebates act like higher worker shares When liquidity is not binding (the down–payment constraint is slack), a price rebate θ is *equivalent* to raising the worker's retained claim α. Intuitively, a proportional discount scales down every rupee the worker must repay by the same factor $(1-\theta)$, so the problem is identical to one with a larger claim on returns by worker. Formally, along the price menu (g fixed, in particular $g=0$),

$$\boxed{k^*(\alpha,\theta;\, g=0) \;=\; k^*\Big(\frac{\alpha}{1-\theta},\, 0;\, g=0\Big), \qquad \alpha^{\text{eff}} = \frac{\alpha}{1-\theta}.}$$

Case 2. When liquidity binds, $D(\theta,g)\,k = L$ binds, the choice is pinned by the cash cap:

$$\boxed{k^*(\theta,g) = \frac{L}{D(\theta,g)} = \frac{L}{\delta(1-\theta)S - g}}.$$

Prediction 2A Since $D_\theta = -\delta S$,

$$\frac{\partial k^*}{\partial \theta} = -\,\frac{L}{D(\theta,g)^2}\,D_\theta = \frac{L\,\delta S}{D(\theta,g)^2} > 0.$$

A price rebate raises purchase because it reduces the cash due today by $\delta S\,\theta$.

Prediction 2B Since $D_g = -1$,

$$\frac{\partial k^*}{\partial g} = -\,\frac{L}{D(\theta,g)^2}\,D_g = \frac{L}{D(\theta,g)^2} > 0.$$

A low-DP contract raises purchase because it reduces the cash due today one–for–one. This is in contrast with the Case 1 when liquidity did not bind.

Corollary 2: Magnitude of investment response between two levers when liquidity binds. While the constraint binds,

$$\frac{\partial k^*/\partial g}{\partial k^*/\partial \theta} = \frac{1}{\delta S}.$$

A price rebate reduces the required down payment only by the *fraction* δ of the price cut (change in D equals $\delta S\,\Delta\theta$), whereas a low-DP contract g reduces the down payment *one–for–one* (change in D equals Δg). Therefore, while liquidity binds:

$$\text{A change in } g \text{ should increase } k^* \text{ much more than an equal-sized change in } \theta$$

because g relaxes the cash hurdle directly, whereas θ only relaxes it through the down–payment share δ.[31][32]

[31] For example, 10% rebate with $\delta = 0.15$ lowers the down payment by $0.15 \times S \times 0.10$, while the low–DP contract lowers it by g one–for–one; the latter is mechanically much larger.

[32] That is, a one rupee reduction in downpayment via g vs. a one rupee reduction via a price rebate θ.

6.1.2 Summary of the Model Predictions.

Worker's decision to invest under a price rebate contract vs. a low-DP contract is critically dependent on her liquidity for down payment.

1. If liquidity does not bind:
   (a) **Price rebate (θ) increases investment.** Along any interior solution, $\theta \uparrow$ lowers the lifetime unit cost and *raises* k^*.
   (b) **Down payment relief (g)reduces investment.** Holding liquidity slack, $g \uparrow$ increases the lifetime unit cost and *lowers* k^*.
2. When liquidity does bind:
   (a) **Price rebate increases purchase.** Because $D_\theta = -\delta S$, a rebate reduces the down payment by $\delta S\,\theta$ and hence *raises* k^*.
   (b) **Repayable low–DP increases purchase.** Because $D_g = -1$, g reduces the down payment one–for–one and thus *raises* k^*.

6.2 Testing Model Predictions: Discrete Choice Experiment

Guided by the model, I test take-up using an incentive compatible discrete choice experiment. Each respondent evaluated four financing contracts that correspond directly to the model's two levers. The contracts were (also summarize in Table A10):

1. **Benchmark:** List price INR 100,000, down payment 15 percent (INR 15,000), the remaining INR 85,000 financed at the market rate over 24 months.
2. **Price rebate (θ):** The list price is reduced by INR 10,000 to INR 90,000, the down payment remains 15 percent of price which implies INR 13,500 due today, and INR 76,500 is financed. This primarily lowers lifetime user cost.
3. **Low–DP, no rebate (g):** The list price is held at INR 100,000, the down payment is reduced to INR 5,000, and INR 95,000 is financed. This relaxes the day-of-purchase liquidity requirement without changing lifetime price.
4. **Subsidy** The worker brings the standard down payment of INR 15,000, immediately receives INR 10,000 at purchase, and finances the usual 85 percent. This reproduces the field intervention and combines a price rebate with low-DP relief at the decision moment.

For the discrete choice experiment women were recruited from Lucknow who were active in the platform's salon vertical. Lucknow was selected as a non–study city to avoid interference with the field experiment while keeping technology and demand comparable. Recruitment proceeded via in-app invitations sent to all eligible female workers on the Lucknow roster, following the same

protocol as in the main study. The invitation was issued twice over a three-week window and asked workers who were considering a scooter purchase to contact the research team.

Workers were first invited to contact the research team if they were considering purchasing a scooter in the near future. The invitation explained that the firm sought to learn about preferences over alternative financing contracts. Enrolled respondents were then asked to evaluate four contracts. For each contract, enumerators explained the cash required today, the monthly installment, total interest, and the implied effective annual rate; respondents recorded a yes or no purchase decision for that contract. Contract order was randomized within respondent across the 24 possible permutations. To anchor choices in incentives, a post-survey lottery selected ten respondents; for each selected respondent one contract was drawn at random from the set just evaluated and implemented if the respondent had said yes to it. This procedure yields Becker–DeGroot–Marschak incentive compatibility and one hundred decisions per contract, for a total of 400 worker–contract observations.

Comparing acceptance across contracts identifies the elasticity to benchmark "control" contract, contract with reduced lifetime price holding liquidity nearly fixed, the elasticity to liquidity holding lifetime price fixed, and the response to the bundled policy that mirrors the field intervention. The design therefore takes the model's two levers, price rebate θ and low-DP (g), to the data in a way that isolates the operative constraint behind adoption.

6.3 Results: Discrete Choice Experiment

Table 6 Column 1 shows that the take-up under the benchmark (control) contract is about 16 percent, which mirrors the control-group purchase rate in the field experiment and supports external comparability. The price rebate contract lifts take-up to about 37 percent, a gain of 21 percentage points. Low DP (no discount) raises take-up to about 69 percent, a gain of 53 percentage points. The increase under low DP is about two and a half times the increase under the price rebate (53 pp vs. 21 pp), so the elasticity of adoption to relaxing the down-payment requirement is much larger than the elasticity to lowering lifetime price by a comparable amount. The subsidy arm that bundles both margins reaches about 95 percent (79 pp increase over 16% baseline) and is weakly preferred to either single lever. This ordering is exactly what the model predicts in the liquidity-binding case, where shifting cash due today generates a much larger movement in adoption than reducing the asset's lifetime cost.

I next examine heterogeneity that speaks directly to the liquidity mechanism. Before the survey, each respondent stated whether she could mobilize the standard down payment of INR 15,000 within a week. This split yields two pre-specified groups that align with the model's cases.

Workers who cannot mobilize INR 15,000 (liquidity constrained). Table 6 Column 2 shows that take-up under the benchmark is low, around 13 percent. Moving lifetime price with a price rebate produces only a small change that is not distinguishable from the benchmark contract. By contrast, relaxing the cash hurdle with low DP (no discount) raises take-up to roughly 70–75 percent, a

large and precisely estimated jump. The subsidy that combines both margins pushes take-up to the top of the scale, near full adoption. Within this group, the effect of low DP is statistically and economically larger than the effect of the price rebate. This is exactly the pattern predicted by the model when the down-payment constraint binds.

Workers who can mobilize INR 15,000 (not liquidity constrained). Table 6 Column 3 shows that take-up in the control (benchmark) contract itself is higher - 23% - for this group relative to the liquidity constrained. The Low-DP (no discount) contract increases take-up by 0.39 over this benchmark, even though it raises lifetime cost by rolling the down payment into principal. This sign conflicts with the interior-case prediction (Prediction 1B), which, under slack liquidity, implies lower adoption than the benchmark. At the same time, Low-DP remains significantly below both price-reducing contracts (Price Rebate and Subsidy), consistent with the model's ranking on lifetime price. The positive Low-DP effect within the non-liquidity-constrained group is consistent with a shadow value of liquidity even when the INR 15,000 constraint does not strictly bind. In particular, if agents value cash-on-hand for buffer-stock or risk-management reasons (e.g., income volatility early in tenure) or face non-pecuniary costs of assembling the upfront cash (time, hassle, and screening frictions), then the marginal utility of liquidity enters the first-order condition and makes a reduction in attractive despite a higher lifetime price.[33]

Among take up rates in the non-benchmark contracts, take-up is lower under low DP (no discount) than under the price rebate, consistent with the model's prediction when liquidity does not bind and rolling cash into principal raises lifetime cost. The subsidy also yields high take-up but is statistically indistinguishable from the price rebate in this subgroup, while both exceed low DP. This pattern aligns with the interior-case prediction of the model - which should be the take-up patter among the non-liquidity constrained group.

Thus, the discrete choice experiment separates the price and liquidity margins in a way that maps directly to the model. Where liquidity binds, moving cash due today dominates a proportional cut in lifetime price. Where liquidity does not bind, the two levers perform similarly and the subsidy does not add much beyond either lever on its own. The evidence therefore points to the down-payment constraint as the operative margin for a large share of workers, while also indicating that for workers with adequate cash on hand either contract is sufficient to induce adoption.

7 Conclusion

In this paper, I examine the contribution of worker-owned capital to worker productivity and firm profitability. In a platform-firm setting in India, where salon services are delivered at customers' homes and efficient mobility is a key production input, I evaluate an innovative subsidy program that

[33] Not liquidity constrained' is based on a coarse threshold (ability to mobilize INR 15,000). Given that all subjects are low-income platform workers, this partition is unlikely to carve out a group with zero shadow value of liquidity. Measurement error in self-reported liquidity and heterogeneity in cash-flow risk might blur the strict interior-case prediction.

incentivizes investment in "scooters," a form of worker-owned capital. Half (50%) of treated workers avail the subsidy to purchase a scooter, priced at 20 weeks of their income, on credit. New adopters — workers who switch from unreliable private taxis or ride-hailing — experience large productivity gains, whereas incumbent users already operating a scooter for work exhibit no detectable effects. Using daily job-level data over more than eight months, intent-to-treat estimates show that new adopters increase jobs completed by 9% and weekly earnings by 13%, with no increase in labor supply, implying a 10% rise in weekly labor productivity. These workers disproportionately complete jobs that would otherwise be at risk of going unmatched in the system — specifically, farther-distance assignments and late-evening jobs — and they record higher work quality as measured by improved ratings. On the firm side, revenue and profit per worker-week rise in ways not explained by business stealing, indicating a clear increase in total surplus. Finally, I find no treatment-induced attrition, despite the platform context, eliminating the principal channel through which the firm would have been unable to recoup its subsidy.

Consequently, both the firm and workers realize large returns on investment. The firm recoups the subsidy within 18 weeks and, within the first year, earns a 153% return on investment per new adopter. On the worker side, the cost of capital is recovered within thirteen months (even absent the subsidy), and under minimal assumptions, the two-year return exceeds 73%. These magnitudes naturally raise the question of why the firm did not previously incentivize such investment. While the paper cannot adjudicate this experimentally, conversations with firm leadership indicate that the principal deterrent was the absence of credible evidence on the subsidy's effectiveness — specifically, its returns for workers and for the firm, as well as implications for attrition. Following the intervention, the firm scaled the subsidy program to additional cities, providing a revealed-preference measure of the firm's assessment of the program's value.

On the worker side, underinvestment is likewise puzzling given sizable returns even without the subsidy. To probe mechanisms, I conduct an incentive-compatible discrete-choice experiment with a new sample of workers that orthogonally varies lifetime price and day-of-purchase liquidity via the required down payment, thereby disentangling a liquidity mechanism from a pure price reduction (which could operate through surplus sharing). I find that many workers are sharply liquidity constrained at the down-payment margin: take-up is substantially higher under a contract with a low down payment and a higher total price than under an alternative with a higher down payment but a lower total price. This pattern indicates that purchase-time liquidity can be a binding constraint for workers.

These findings matter for three reasons. First, the evidence indicates that liquidity constraints play a first-order role in labor market functioning—not only for firms and entrepreneurs, where most of the literature has focused, but for workers as well. In environments where workers are liquidity constrained, firm-financed or subsidized investment in productivity-enhancing, worker-owned capital can shift both workers and the firm onto a higher earnings path. Second, even in a platform setting with ostensibly low switching costs and ready outside options, the firm recovers its subsidy because

attrition remains low; workers do not exit, allowing the firm to appropriate part of the surplus created by the investment. Third, the results open a clear agenda for future work: to identify the specific frictions that sustain retention in such settings and to assess external validity across contexts and populations. In particular, it is important to test whether similar retention and surplus sharing would arise in male-dominated segments of the platform economy, or whether the predominantly female workforce studied here plays a role in the observed low attrition.

Finally, analogous policies are proliferating in developed-country settings in which firms help workers overcome liquidity or credit constraints. In the United States, for example, ride-hailing platforms have partnered with vehicle providers (e.g., Uber–Hertz) to facilitate access to high-quality cars for work. These initiatives are explicitly aimed at improving retention and productivity. It is therefore natural to ask how such models perform in developing-country contexts, where liquidity constraints may bind more tightly, and to study which program designs most effectively translate employer support into persistent gains for both workers and firms.

References

Acemoglu, Daron and Jörn-Steffen Pischke, "The structure of wages and investment in general training," *Journal of political economy*, 1999, *107* (3), 539–572.

Adams, William, Liran Einav, and Jonathan Levin, "Liquidity constraints and imperfect information in subprime lending," *American Economic Review*, 2009, *99* (1), 49–84.

Adhvaryu, Achyuta, Namrata Kala, and Anant Nyshadham, "Returns to on-the-job soft skills training," *Journal of Political Economy*, 2023, *131* (8), 2165–2208.

Autor, David H, "Why do temporary help firms provide free general skills training?," *The Quarterly Journal of Economics*, 2001, *116* (4), 1409–1448.

Bandiera, Oriana, Robin Burgess, Narayan Das, Selim Gulesci, Imran Rasul, and Munshi Sulaiman, "Labor markets and poverty in village economies," *The Quarterly Journal of Economics*, 2017, *132* (2), 811–870.

Banerjee, Abhijit, Dean Karlan, and Jonathan Zinman, "Six randomized evaluations of microcredit: Introduction and further steps," *American Economic Journal: Applied Economics*, 2015, *7* (1), 1–21.

_ , Esther Duflo, Rachel Glennerster, and Cynthia Kinnan, "The miracle of microfinance? Evidence from a randomized evaluation," *American economic journal: Applied economics*, 2015, *7* (1), 22–53.

Barbanchon, Thomas Le, Roland Rathelot, and Alexandra Roulet, "Gender differences in job search: Trading off commute against wage," *The Quarterly Journal of Economics*, 2021, *136* (1), 381–426.

Beaman, Lori, Dean Karlan, Bram Thuysbaert, and Christopher Udry, "Selection into credit markets: Evidence from agriculture in Mali," *Econometrica*, 2023, *91* (5), 1595–1627.

Becker, Gary S, "Human Capital New York," *Columbia University for the National Bureau of Economic Research*, 1964.

Bloom, Nicholas, Benn Eifert, Aprajit Mahajan, David McKenzie, and John Roberts, "Does management matter? Evidence from India," *The Quarterly journal of economics*, 2013, *128* (1), 1–51.

Borker, Girija et al., "Safety first: Perceived risk of street harassment and educational choices of women," 2021.

Bryan, Gharad, Shyamal Chowdhury, and Ahmed Mushfiq Mobarak, "Underinvestment in a profitable technology: The case of seasonal migration in Bangladesh," *Econometrica*, 2014, *82* (5), 1671–1748.

Casaburi, Lorenzo and Jack Willis, "Time versus state in insurance: Experimental evidence from contract farming in Kenya," *American Economic Review*, 2018, *108* (12), 3778–3813.

Cefala, Luisa, Pedro Naso, Michel Ndayikeza, and Nicholas Swanson, "Under-training by employers in informal labor markets: Evidence from burundi," Technical Report, Working Paper 2024.

Coffman, Lucas C, John J Conlon, Clayton R Featherstone, and Judd B Kessler, "Liquidity affects job choice: Evidence from Teach for America," *The Quarterly Journal of Economics*, 2019, *134* (4), 2203–2236.

Crépon, Bruno, Mohamed El Komi, and Adam Osman, "Is it who you are or what you get? Comparing the impacts of loans and grants for microenterprise development," *American Economic Journal: Applied Economics*, 2024, *16* (1), 286–313.

Daher, Chaza Abou, Erica M Field, Kendal M Swanson, and Kate H Vyborny, "Drivers of change: How intra-household preferences shape employment responses to gender reform," Technical Report, National Bureau of Economic Research 2023.

Datta, Ayona, Mark Graham et al., "Informal Labor, Platform Work, and Digital Intermediation in the Global South," *Global Networks*, 2023, *23* (1), 123–145.

Doornik, Bernardus Van, Armando Gomes, David Schoenherr, and Janis Skrastins, "Financial access and labor market outcomes: Evidence from credit lotteries," *American Economic Review*, 2024, *114* (6), 1854–1881.

Duflo, Esther, Michael Kremer, and Jonathan Robinson, "How high are rates of return to fertilizer? Evidence from field experiments in Kenya," *American economic review*, 2008, *98* (2), 482–488.

_ , _ , and _ , "Nudging farmers to use fertilizer: Theory and experimental evidence from Kenya," *American economic review*, 2011, *101* (6), 2350–2390.

Fafchamps, Marcel, David McKenzie, Simon Quinn, and Christopher Woodruff, "Microenterprise growth and the flypaper effect: Evidence from a randomized experiment in Ghana," *Journal of development Economics*, 2014, *106*, 211–226.

Garlick, Robert, Erica Field, and Kate Vyborny, "Women's Mobility and Labor Supply: Experimental Evidence from Pakistan," 2025.
Gender Equality in Transport: Toolkit

***Gender Equality in Transport: Toolkit**, Technical Report, Asian Development Bank 2021.*

Hanna, Rema, Sendhil Mullainathan, and Joshua Schwartzstein, *"Learning through noticing: Theory and evidence from a field experiment," The Quarterly Journal of Economics, 2014, 129 (3), 1311–1353.*
India—Making Public Transport and Urban Spaces Safer and Inclusive for Women: Policy Brief

India—Making Public Transport and Urban Spaces Safer and Inclusive for Women: Policy Brief, Technical Report, World Bank 2022.

Jack, B Kelsey, Seema Jayachandran, Namrata Kala, and Rohini Pande, "Money (not) to burn: payments for ecosystem services to reduce crop residue burning," *American Economic Review: Insights*, 2025, *7* (1), 39–55.

Jacoby, Hanan G and Ghazala Mansuri, "Land tenancy and non-contractible investment in rural Pakistan," *The Review of Economic Studies*, 2008, *75* (3), 763–788.

Katz, Eliakim and Adrian Ziderman, "Investment in general training: The role of information and labour mobility," *The economic journal*, 1990, *100* (403), 1147–1158.

Laffont, Jean-Jacques and Mohamed Salah Matoussi, "Moral hazard, financial constraints and sharecropping in El Oulja," *The Review of Economic Studies*, 1995, *62* (3), 381–399.

Meager, Rachael, "Understanding the average impact of microcredit expansions: A Bayesian hierarchical analysis of seven randomized experiments," *American Economic Journal: Applied Economics*, 2019, *11* (1), 57–91.

Mel, Suresh De, David McKenzie, and Christopher Woodruff, "Returns to capital in microenterprises: evidence from a field experiment," *The quarterly journal of Economics*, 2008, *123* (4), 1329–1372.

NITI-Aayog, "India's Booming Gig and Platform Economy: Perspectives and Recommendations on the Future of Work," Technical Report June 2022.

Reddy, Priya Adhisesha, Nathan Congdon, Graeme MacKenzie, Parikshit Gogate, Qing Wen, Catherine Jan, Mike Clarke, Jordan Kassalow, Ella Gudwin, Ciaran O'Neill et al., "Effect of providing near glasses on productivity among rural Indian tea workers with presbyopia (PROSPER): a randomised trial," *The Lancet Global Health*, 2018, *6* (9), e1019–e1027.

Shaban, Radwan Ali, "Testing between competing models of sharecropping," *Journal of Political Economy*, 1987, *95* (5), 893–920.

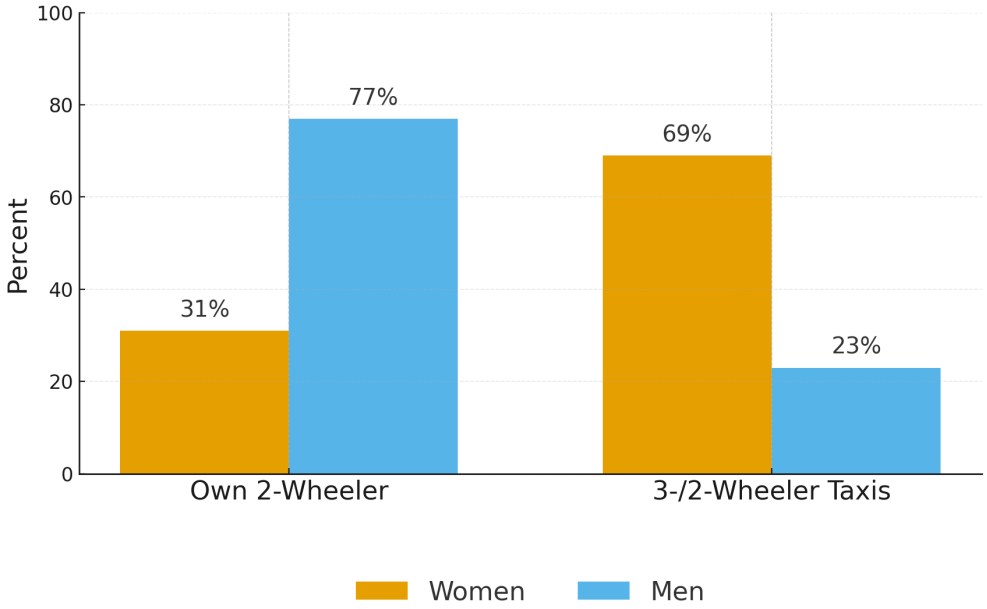
Shah, Sonal et al., "Women and Transport in Indian Cities," Technical Report, ITDP India 2017. *Urban Transport: Policy Note Series*

***Urban Transport: Policy Note Series**, Technical Report, WRI India 2021.*

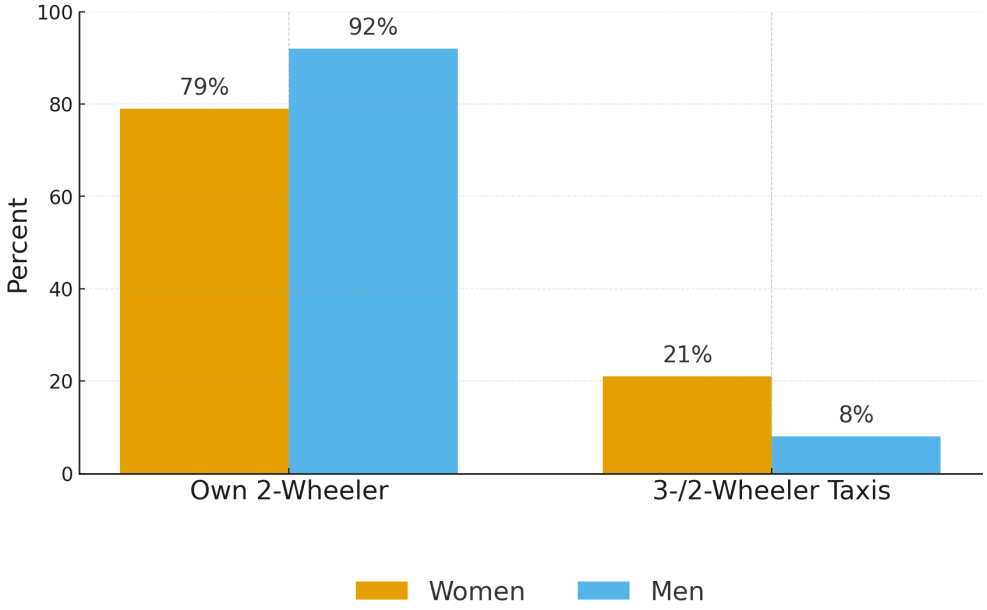
World Bank, *"How to Help Ensure Safe and Inclusive Public Spaces and Public Transport for Women in India," 2023.*

Figure 1: Gender Differences in Actual and Preferred Mode of Commute (Pre-Experiment Survey)

(a) Actual Model of Commute



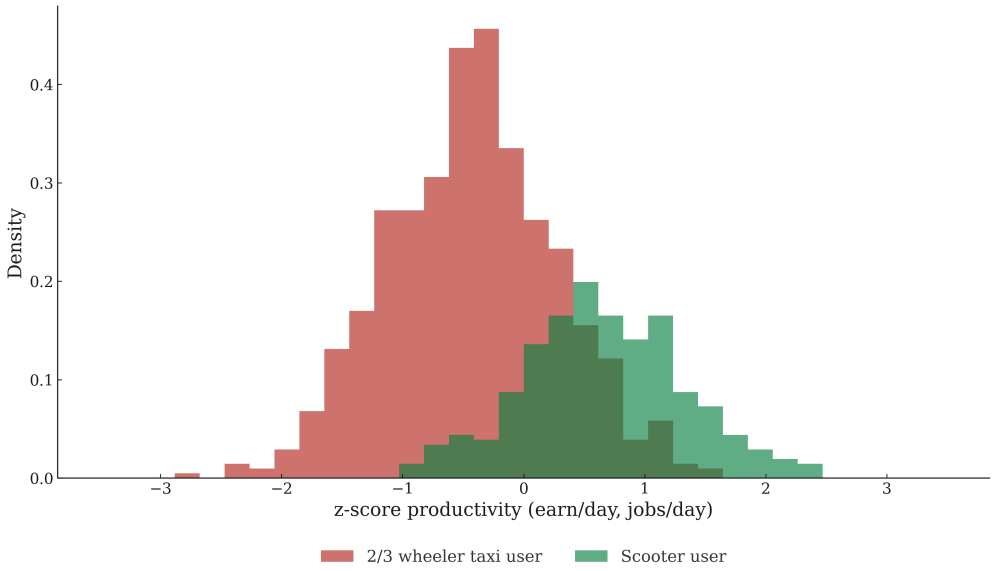
(b) Preferred Mode of Commute



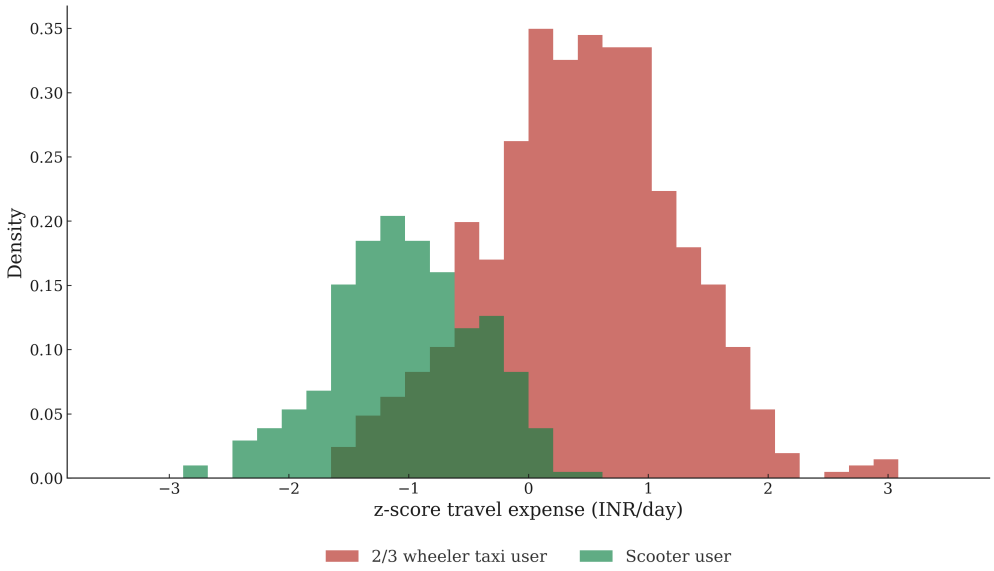
Notes: Figure reports evidence from a pre-experiment phone survey of N=1,531 randomly selected platform workers performing the same salon-service job. Panel A shows actual primary commute mode; Panel B shows stated preference. "Own 2-wheeler" denotes using a personally owned scooter; "3-/2-wheeler taxis" denotes private bike-taxis or three-wheelers (auto-rickshaws). In Panel A, 77% of men versus 31% of women report commuting by own scooter (the remainder use taxis: 23% and 69%, respectively). In Panel B, 92% of men and 79% of women state they would prefer to commute by own scooter rather than taxis. The sizeable usage–preference gap for women contrasted with near-saturation among men motivates focusing the intervention on women and is consistent with frictions of ride-hailing highlighted in the baseline (limited availability at peak times, higher/volatile off-hour fares, refusals to carry equipment, safety concerns, and search/wait time).

Figure 2: Correlation Between Baseline Mode of Commute, Productivity and Travel Expenditure

(a) Correlation: Baseline Mode of Commute and Productivity



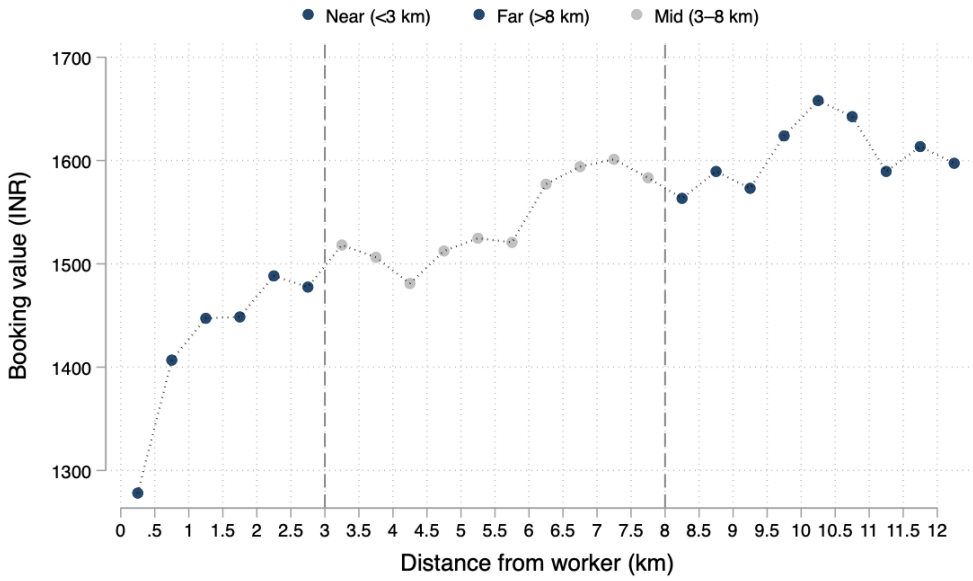
(b) Correlation: Baseline Mode of Commute and Travel Expenditure for Work



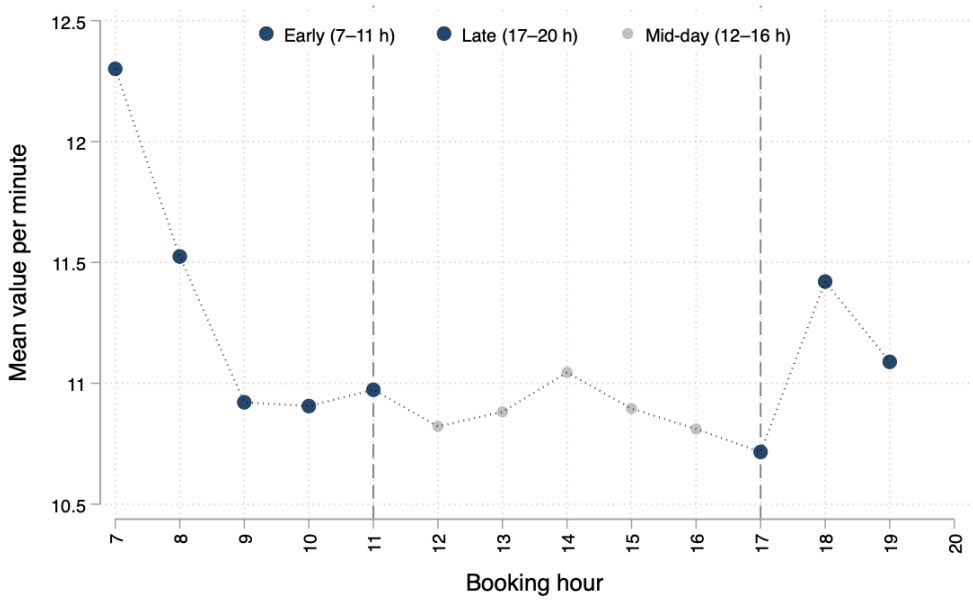
Notes: This figure compares outcomes by baseline commuting mode using pre-experiment administrative data for women drawn from a phone survey of N=1,531 randomly selected platform workers (the survey was 50% women). "Scooter user" denotes workers who primarily commute on a personally owned two-wheeler. "2/3-wheeler taxi user" denotes reliance on private motorcycle taxis (two-wheelers) or auto-rickshaws (three-wheelers). Each panel plots histograms of standardized variables (mean = 0, s.d. = 1). Panel A displays productivity, measured by daily earnings. Panel B displays daily travel expenditure for work. At baseline, scooter users earn more per day and spend less on transport than taxi users. These are descriptive correlations (unadjusted) and should not be interpreted causally.

Figure 3: Correlation Between Job Value and Job Distance and Timing

(a) Correlation: Job Value and Job Distance

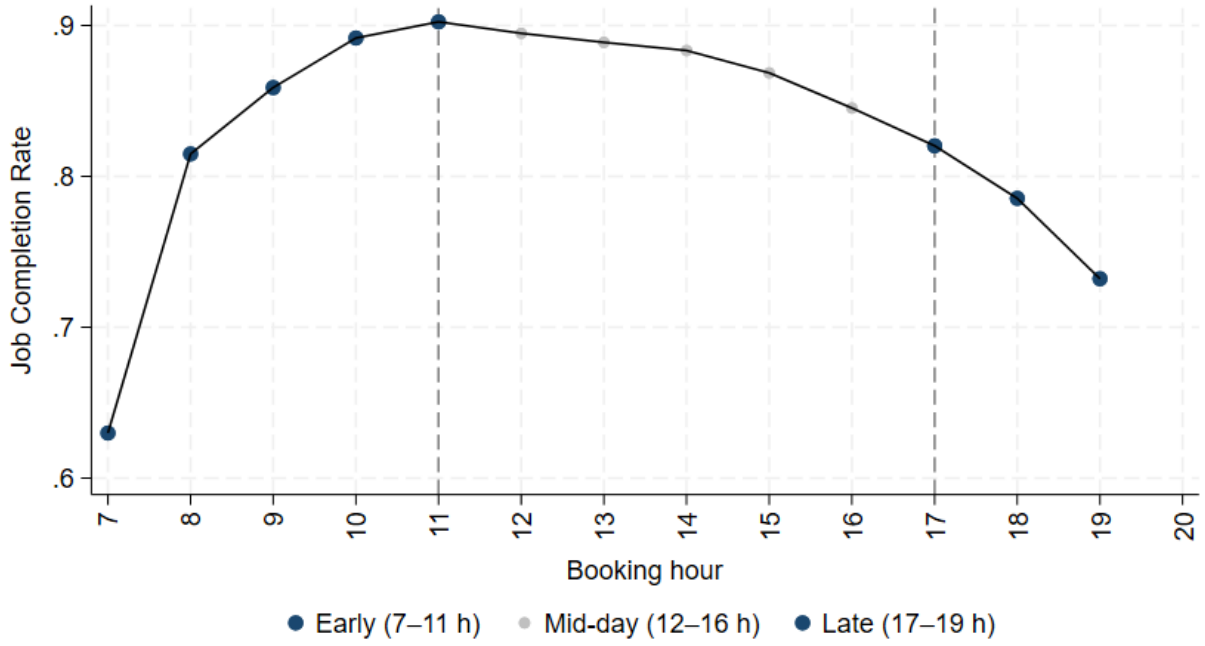


(b) Correlation: Job Value and Job Timing



Notes: Pre-experiment administrative job-level data from the study platform. Panel (a) plots mean booking value (INR) against straight-line distance from the worker's registered home location; booking value increases with distance. Panel (b) plots mean value per minute—booking value divided by service-time minutes—by booking hour; earnings per minute are higher for early-morning (7–11 h) and late-evening (17–20 h) bookings than for mid-day (12–16 h). These are unconditional correlations (non-causal). They motivate analyzing scooter adoption as a technology that relaxes travel-time and travel-variance constraints precisely where the returns to mobility — longer distances and off-peak hours — are higher.

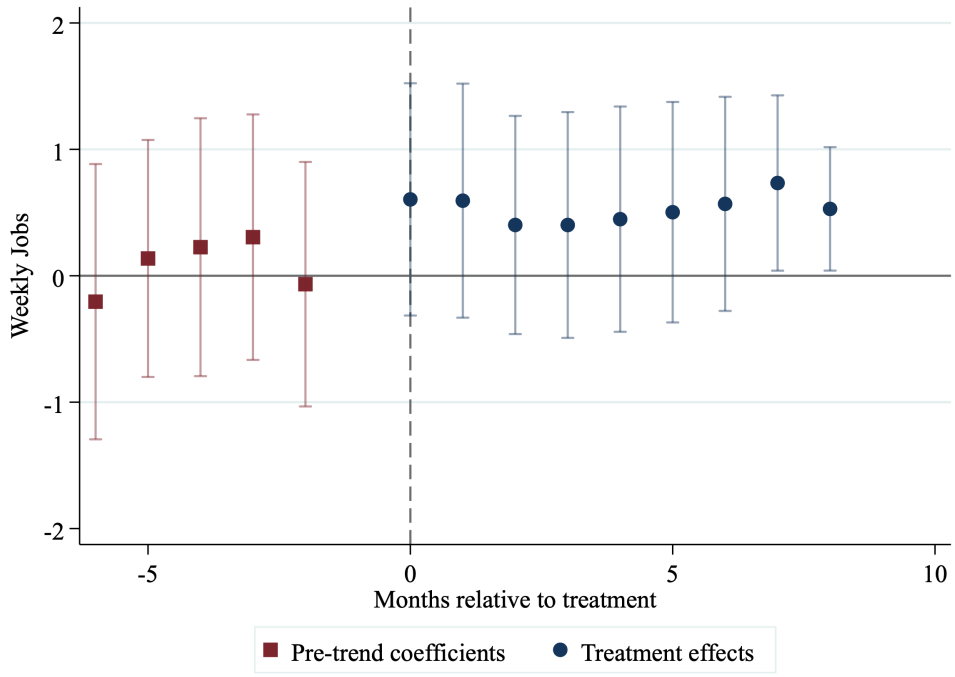
Figure 4: Correlation Between Job Completion Rate and Job Start Time



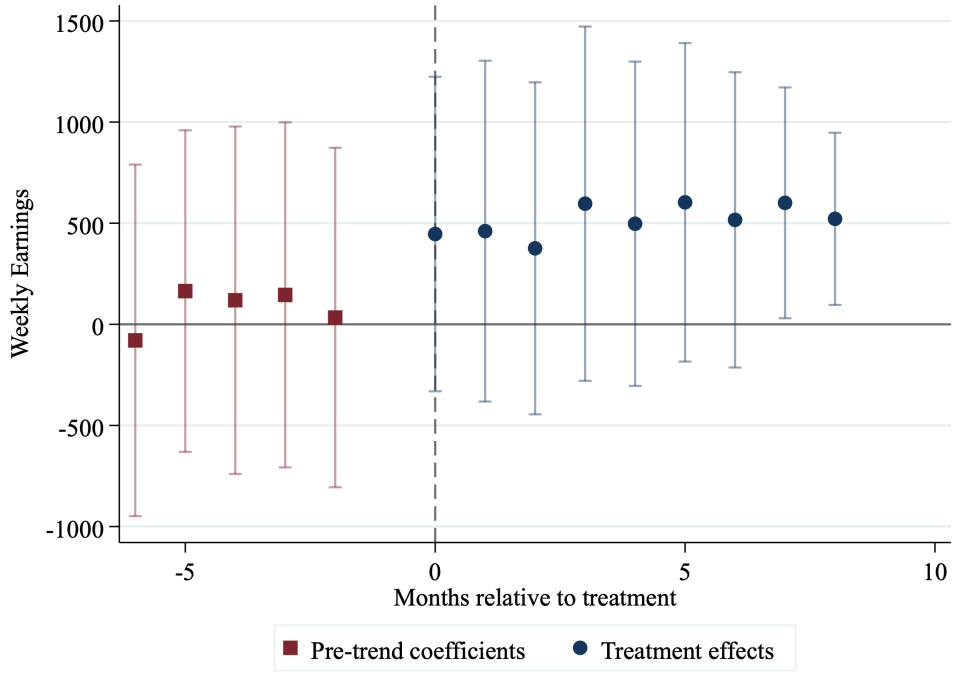
Notes: This figure uses pre-experiment administrative, job-level data from the study platform to show how fulfillment varies over the day. The outcome is the job completion rate, defined as the share of booked jobs that are marked completed in the system on the scheduled day. The x-axis is the booking (scheduled start) hour; the line plots the mean completion rate by hour. The overall mean completion rate in the baseline data is 0.84. Completion is highest around mid-day and is noticeably lower at the edges of the working day. In particular, completion rates in the early window (7–11 h) and the late window (17–19 h) are below those in the mid-day window (12–16 h). These are unconditional, descriptive patterns (non-causal). They are consistent with greater travel and punctuality frictions for early/late and farther jobs, and they motivate analyzing heterogeneous treatment effects by booking hour and distance when studying scooter adoption.

Figure 5: Impact on Job Outcomes Over Time for New Adopters

(a) Impact on Jobs Over Time for New Adopters



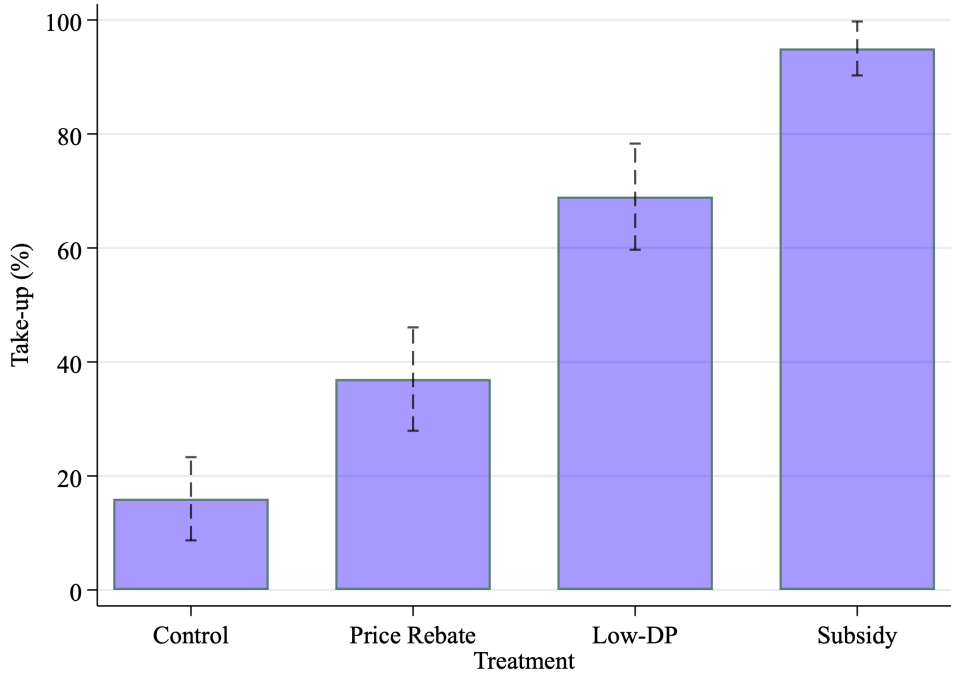
(b) Impact on Earnings Over Time for New Adopters



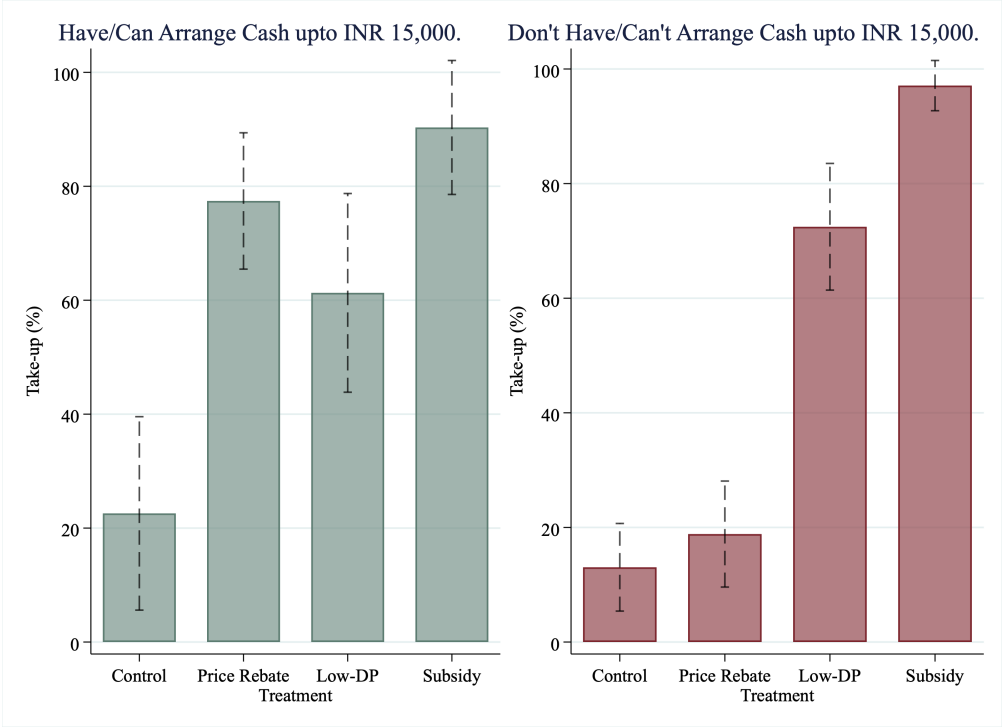
Notes: This figures plots the event–time difference-in-differences estimates for new adopters in the experimental sample. The horizontal axis reports months relative to scooter subsidy offer, with 0 denoting the month of offer. Estimates are obtained from weekly panel regressions that include cohort fixed effects and calendar-week fixed effects. Event-time indicators are interacted with the treatment group; for controls these indicators are zero in all weeks. The indicator for month -1 (the last pre-adoption month) is omitted, so each coefficient measures the average outcome at event time k relative to month -1, net of cohort and week effects. Weekly coefficients are binned to monthly event times for display. Squares plot pre-trend coefficients for $k \in (-6,\ldots,-2)$; circles plot post-adoption coefficients for $k \in (0,\ldots,+8)$. Vertical bars show 95 percent confidence intervals with standard errors clustered at the worker level. Panel (a) reports weekly jobs; Panel (b) reports weekly earnings (INR). Pre-adoption estimates are near zero, indicating no detectable pre-trends relative to month -1. Beginning in month 0 — when exposure is partial by construction — and persisting thereafter, both weekly jobs and weekly earnings are higher than in the last untreated month.

Figure 6: Discrete Choice Incentive Compatible Experiment: Contract Take-Up Rates

(a) Full Sample



(b) Sample Split by Liquidity Constraint for Down Payment



Notes: This figure shows take-up in an incentive-compatible discrete-choice experiment where workers chose among four scooter-finance contracts that differentially relax the day-of-purchase cash requirement or reduce lifetime price. "Control" is the benchmark loan: list price INR 100,000 with a 15% down payment (INR 15,000) and the remaining INR 85,000 financed over 24 months at the market rate. "Price Rebate" lowers the list price to INR 90,000 while keeping the 15% down payment (INR 13,500) and financing INR 76,500; this primarily reduces lifetime user cost without easing upfront liquidity. "Low-Down Payment (DP), no rebate" keeps the list price at INR 100,000 but reduces the down payment to INR 5,000, financing INR 95,000; this relaxes the day-of-purchase liquidity requirement while holding lifetime price fixed. "Subsidy" replicates the field intervention: the worker brings the standard INR 15,000 down payment, receives INR 10,000 at purchase, and finances the usual 85%; this combines a net price reduction with immediate relief of the upfront cash requirement. Bars report the percent choosing to adopt under each contract; whiskers are 95% confidence intervals. Panel (a) shows the full sample. Panel (b) splits the sample by whether the worker can assemble INR 15,000 at purchase (a measure of liquidity constraint).

Table 1: Impact on Take-Up of Subsidy and Scooter Purchase

	(1) Subsidy	(2) Scooter	(3) Subsidy	(4) Scooter
Treatment	0.50***	0.32***	0.47***	0.30***
	(0.04)	(0.05)	(0.04)	(0.06)
Treatment × Incumbent Users			0.09	0.05
			(0.08)	(0.11)
Control Mean	0	0.17	0	0.17
N: Workers	350	350	350	350

Notes: This table reports the first-stage effects of the randomized offer on two binary outcomes: (i) **Subsidy**: whether the worker accepts the financing subsidy contract; and (ii) **Scooter**: whether the worker purchased a scooter during the study window. "Treatment" is an indicator for being randomly offered the subsidy. "Incumbent User" is an indicator for using a scooter at baseline for completing jobs on the platform. Columns (1)–(2) estimate intent-to-treat effects in the pooled sample. Columns (3)–(4) add an interaction with "Incumbent User" to allow the effect of the offer to differ for incumbents versus new adopters; in those columns, the coefficient on *Treatment* is the effect for baseline non-users, while *Treatment × Incumbent User* is the difference relative to baseline users of scooters. Outcomes are indicators. "Control Mean" reports the mean of the outcome in the control group (no offer). "N: Workers" is the number of workers in the specification. All regressions include randomization-cohort (version) fixed effects; standard errors are clustered at the worker-level. Significance: * ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$).

Table 2: Impact on Jobs, Earnings and Productivity

	(1) Jobs (weekly)	(2) Earnings (weekly)	(3) Hours-online (weekly)	(4) Earnings-by-hours-online
Panel A: New Adopters				
Treatment Effect (ITT)	0.54**	602.40***	1.29	9.82***
	(0.23)	(204.41)	(1.28)	(3.59)
Control Mean	6.12	4,757.98	53.08	94.05
N: Worker-Weeks	8,167	8,167	8,167	8,167
Panel B: Incumbent-Users				
Treatment Effect (ITT)	-0.34	-89.14	-2.51	-0.92
	(0.34)	(289.32)	(1.99)	(5.50)
Control Mean	7.24	5,799.22	55.97	106.64
N: Worker-Weeks	3,570	3,570	3,570	3,570
Panel C: Pooled Sample				
Treatment Effect (ITT)	0.18	322.23*	0.13	5.37*
	(0.19)	(170.48)	(1.01)	(3.21)
Control Mean	6.49	5,097.52	54.03	98.19
N: Worker-Weeks	11,737	11,737	11,737	11,737

Notes: This table presents intent-to-treat (ITT) estimates of the effect of being offered a scooter subsidy on weekly outcomes constructed from platform administrative records. The unit of observation is the worker–week; daily records are aggregated to the weekly level over the eight months following randomization. Columns report four outcomes: (1) Jobs - the count of services completed in the week; (2) Earnings - total platform earnings in Indian rupees (INR) in the week; (3) Hours-online (Labor Supply) - the total number of hours the worker is "online," defined as all time windows in which the worker is in the active matching pool (waiting for jobs, traveling to clients after being matched, or performing jobs); and (4) Earnings-by-hours-online - weekly earnings divided by hours-online, a measure of productivity. Regression follows estimation strategy described in Equation 1 that includes cohort (randomization batch) fixed effects and calendar-week fixed effects. Standard errors, in parentheses, are clustered at the worker level. "Control Mean" gives the mean of the outcome for control observations in the corresponding panel. "N: Worker–Weeks" is the total number of worker–week observations used in that specification. Currency is INR. Stars denote p-values (* $p<0.10$, ** $p<0.05$, *** $p<0.01$). Panel A restricts the sample to new adopters (70% of the sample), defined as workers who did not commute by their own scooter at baseline (the main analysis for the intervention). Panel B restricts to Incumbent-Users, defined as workers who were already commuting by their own scooter at baseline; this stratum serves as a placebo group for productivity effects. Panel C pools both strata to report intent-to-treat effects in the combined experimental sample. Randomization was stratified by these baseline adoption strata.

Table 3: Impact on Labor Supply Reallocation Across Space, Time, and Job Quality on New Adopters

	Job Selection Effects						**Job Quality Metrics**	
	(1) Total KMs	(2) Near (0–10 km)	(3) Far (10–25 km)	(4) Mid-day (12–16)	(5) Early/Late (<11 or >17)	(6) Surge Earnings	(7) Job Rating	(8) Late to Job
Treatment effect	6.67***	-0.29	0.54**	0.21	0.37*	18.67***	0.03*	0.18
	(2.55)	(0.24)	(0.23)	(0.19)	(0.21)	(6.66)	(0.01)	(0.16)
p-value (Near = Far)		[0.005]						
p-value (Late = Afternoon)				[0.195]				
Control mean	58.40	3.49	2.68	3.07	3.10	43.97	4.83	2.49
N	8,167	8,167	8,167	8,167	8,167	8,167	5,695	8,061

Notes: This table defines and estimates treatment effects on margins of job selection (space and time) and on job quality for the **new-adopter** stratum only. The unit of observation is the worker–week; outcomes are built from administrative logs and aggregated to weekly frequency.

- Job Selection Effects (Cols. 1–6). "Total KMs" is the total kilometers associated with all completed jobs in the week, computed using the worker's registered home location and the client location for each job. "Near (0–10 km)" and "Far (10–25 km)" are counts of completed jobs whose distance from home falls in those bins. The residual >25 km bin increases modestly, not shown here. "Mid-day (12–16)" and "Early/Late (<11 or >17)" are counts of completed jobs whose scheduled start falls in those booking-hour windows. "Surge Earnings" is the extra rupee amount of weekly earnings attributed to surge pricing premia recorded by the platform.
- Job Quality Metrics (Cols. 7–8). "Job Rating" is the client rating of the service (platform 1–5 scale), averaged across the worker's completed jobs in the week. "Late to Job" is the incidents of completed jobs in the week for which the platform marks the worker as late to the scheduled start time.
- Rows labeled "p-value (Near = Far)" and "p-value (Late = Afternoon)" report Wald tests of equality between the corresponding treatment effects in columns (2) vs. (3) and (5) vs. (4), respectively.
- Regression follows the estimation strategy in Equation 1 and includes cohort (randomization batch) fixed effects and calendar-week fixed effects. Standard errors, in parentheses, are clustered at the worker level. "Control Mean" is the mean outcome for controls in this stratum. "N: Worker–Weeks" is the number of worker–week observations. Currency is INR. Stars denote p-values (* p<0.10, ** p<0.05, *** p<0.01).

Table 4: Impact on Firm Revenue, Profits and Worker Attrition

	Firm Earnings			Worker Attrition
	(1) Revenue Total	(2) Revenue Net-of-Worker-Payout	(3) Profits	(4) Inactive Post-1-Year
Panel A: New Adopters				
Treatment Effect (ITT)	1,178.95***	575.72***	265.26***	-0.040
	(404.54)	(202.63)	(91.02)	(0.045)
Control Mean	9,369.21	4,611.23	2,108.07	0.109
N:Worker-Weeks	8,167	8,167	8,167	245
Panel B: Incumbent-Users				
Treatment Effect (ITT)	-154.35	-77.38	-34.73	-0.015
	(572.98)	(287.75)	(128.92)	(0.037)
Control Mean	11,219.38	5,420.16	2,524.36	0.022
N:Worker-Weeks	3,570	3,570	3,570	105
Panel C: Pooled Sample				
Treatment Effect (ITT)	681.59**	336.30**	153.36**	-0.036
	(330.25)	(165.15)	(74.31)	(0.031)
Control Mean	9,972.53	4,875.01	2,243.82	0.085
N:Worker-Weeks	11,737	11,737	11,737	350

Notes: This table reports intent-to-treat (ITT) effects of being offered the scooter subsidy on firm-side outcomes constructed from platform administrative records. Panels A–C mirror the experimental strata: Panel A restricts to new adopters (baseline non-users of a private scooter), Panel B to incumbent users (baseline users), and Panel C pools both strata. The unit of observation in columns (1)–(3) is the worker–week over the eight months after randomization. Column (4) is measured at the worker level one year after randomization; the sample size in that column is the number of workers observed at that horizon. Outcomes: Column (1) "Revenue—Total" is the platform's gross booking value attributed to the worker's completed jobs in the week (INR). Column (2) "Revenue—Net-of-Worker-Payout" deducts the worker's payout for those jobs from gross revenue, i.e., the platform's net revenue before other costs. Column (3) "Profits" is the platform's accounting profit for those jobs as recorded in the administrative data (net revenue after subtracting recorded variable costs such as refunds/adjustments and platform incentives). Column (4) "Inactive Post-1-Year" is an indicator equal to 1 if the worker is inactive on the platform at the one-year mark and 0 otherwise. Regression follows the estimation strategy described in Equation 1: cohort (randomization-batch) fixed effects and calendar-week fixed effects are included. Standard errors, in parentheses, are clustered at the worker level. "Control Mean" is the mean of the outcome for control observations in the corresponding panel. "N: Worker–Weeks" is the number of worker–week observations in columns (1)–(3); column (4) reports the number of workers. Currency is INR. Stars denote p-values (* p<0.10, ** p<0.05, *** p<0.01).

Table 5: Return on Investment (ROI) for the Firm and the Workers

	Treatment Effect IV	Recovery Period	ROI 12 months	ROI 24 months
Panel A: Firm's return to subsidy				
New Adopters	557.87***	18 weeks	153%	345%
	(197.29)			
Pooled Sample	345.95***	29 weeks	60%	180%
	(151.17)			
Panel B: Workers' return to scooter purchase				
New Adopters	2,018***	54 weeks	-2.8%	73%
	(768.03)			

Notes: This table reports return-on-investment (ROI) measures for the platform (Panel A) and for workers (Panel B) using administrative outcomes. Causal effects are estimated by instrumental variables, with the randomized subsidy offer as the instrument. All payoff calculations are *discounted present values* computed at a weekly rate.

- Horizon and survival: For months 1–12 I cumulate discounted weekly increments from weeks $t = 1$ to 52. Weeks 1–35 are realized in the data; weeks 36–52 are multiplied by the 12-month survival probability s. (See Section B for formulas and parameter values.)
- Panel A (firm).
  - Column 1, "Treatment Effect (IV)": IV estimate of the weekly increment in the platform's profit per worker.
  - Column 2, "Recovery Period": *discounted payback time*—the smallest week T such that $\sum_{t=1}^{T} \beta^t \times$ (IV weekly profit) $\geq C$, where C is the subsidy cost per adopting worker.
  - Columns 3–4, "ROI 12 months / 24 months": percentage return over the stated horizon with PV computed under the horizon and survival conventions above.
- Panel B (workers).
  - Column 1, "Treatment Effect (IV)": IV estimate of the weekly increment in worker earnings, instrumenting actual purchase with the subsidy offer.
  - Column 2, "Recovery Period": *discounted payback time*—the smallest week T such that $\sum_{t=1}^{T} \beta^t \times$ (IV weekly earnings) $\geq$ PV(financed purchase cost). The financed cost is the *present value of the loan payment stream* (principal and interest, net of subsidy), as detailed in Appendix B.
  - Columns 3–4, "ROI 12 months / 24 months": percentage returns computed as $\big($PV of incremental weekly earnings $-$ PV(financed cost)$\big)$/PV(financed cost) over the stated horizons, under the same discounting and survival conventions.
  - The negative 12-month ROI for workers reflects a discounted payback of roughly 54 weeks; returns turn positive only once the present value of incremental earnings exceeds the present value of loan obligations.
- Currency is INR; time is in calendar weeks. Sample restricted to **new adopters** because impacts for incumbent users are indistinguishable from zero on platform jobs (Table 2), so pooling would attenuate policy-relevant returns to adoption. Stars denote p-values (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). All formulas, parameter choices (r, s, δ, C), and robustness alternatives are provided in Appendix B.

Table 6: Discrete Choice Incentive Compatible Mechanism Experiment

	Full Sample	Liquidity Constrained	Not Liquidity Constrained
	(1)	(2)	(3)
Price Rebate	0.210***	0.058	0.548***
	(0.059)	(0.061)	(0.106)
Low-DP Contract	0.530***	0.594***	0.387***
	(0.060)	(0.069)	(0.124)
Subsidy	0.790***	0.841***	0.677***
	(0.044)	(0.045)	(0.105)
Control Mean	0.16	0.13	0.23
p(Price Rebate = Low-DP)	0.00	0.00	0.14
p(Price Rebate = Subsidy)	0.00	0.00	0.13
p(Low-DP = Subsidy)	0.00	0.00	0.01
N: Worker-Contract	400	276	124

Notes: This table reports results from the discrete-choice experiment. The dependent variable equals one if the worker states she would accept the displayed contract. Regressors are indicators for contract menus. "Control" is the benchmark loan: list price INR 100,000 with a 15% down payment (INR 15,000) and the remaining INR 85,000 financed over 24 months at the market rate. "Price Rebate" lowers the list price to INR 90,000 while keeping the 15% down payment (INR 13,500) and financing INR 76,500; this primarily reduces lifetime user cost without easing upfront liquidity. "Low-Down Payment (DP), no rebate" keeps the list price at INR 100,000 but reduces the down payment to INR 5,000, financing INR 95,000; this relaxes the day-of-purchase liquidity requirement while holding lifetime price fixed. "Subsidy" replicates the field intervention: the worker brings the standard INR 15,000 down payment, receives INR 10,000 at purchase, and finances the usual 85%; this combines a net price reduction with immediate relief of the upfront cash requirement. Each worker evaluated the four contracts, yielding $100 \times 4 = 400$ worker–contract observations. Column 1 reports the results for full sample. Column 2 restricts the sample to workers who report being unable to assemble INR 15,000 at the time of purchase, which we use as an ex-ante indicator of a binding purchase-time liquidity constraint (INR 15,000 corresponds to the benchmark down payment). Column 3 restricts the sample to workers who report being able to assemble INR 15,000 at the time of purchase, i.e., workers without a binding purchase-time liquidity constraint under the benchmark down payment. All specifications include version (order) fixed effects. SEs are clustered at the worker level. Significant at * 10%, ** 5% and ***1% level.

A Appendix A: Additional Results

Figure A1: Example of Intervention Scooters



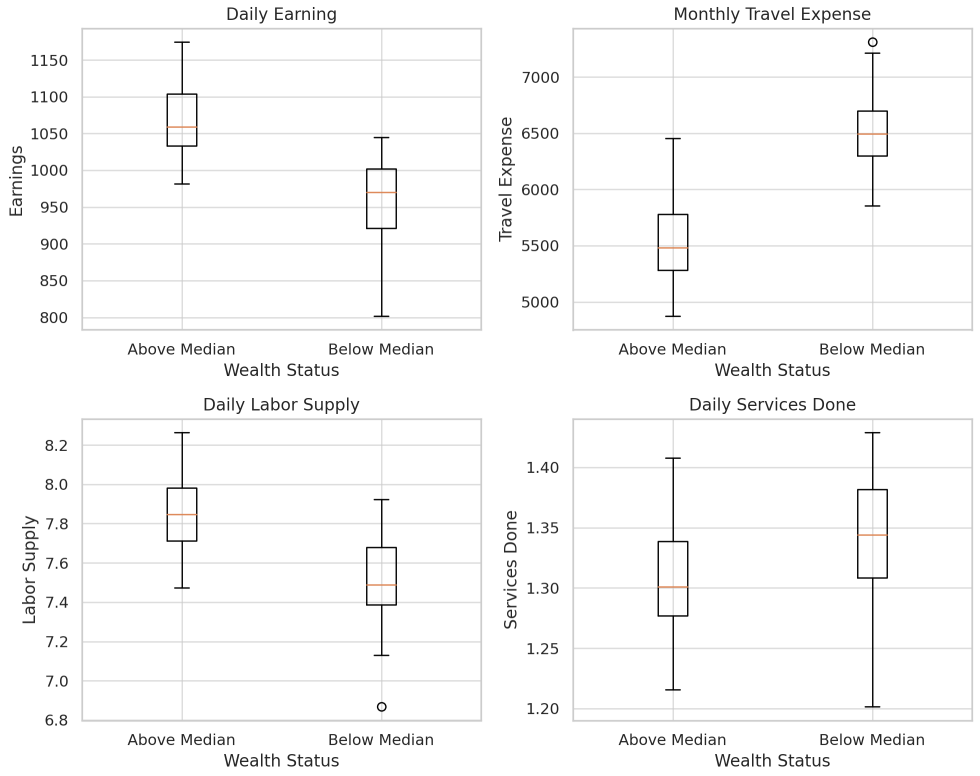
Notes: This figure illustrates the type of lightweight, step-through, gear-less two-wheeler scooters, often referred to as "scooty", that are prevalent in India. **Unlike the stand-up, dockless** "kick-scooters" popularized by companies like Lime or Bird in North America, these Indian scooters are ridden seated, licensed for road traffic, and designed for trips well beyond first- and last-mile range (more like the popular Italian brand scooters 'Vespas'. These vehicles, typically 90-125 cc petrol or electric models such as the Honda Activa and TVS Scooty, feature a step-through frame, continuously-variable transmission, and low kerb weight (e.g., the 93 kg TVS Scooty Pep Plus), making them particularly appealing to novice and female riders due to reduced physical exertion.

Figure A2: Correlation Between Wealth and Scooter Use and Productivity at Baseline

(a) Correlation Between Wealth and Baseline Scooter Use



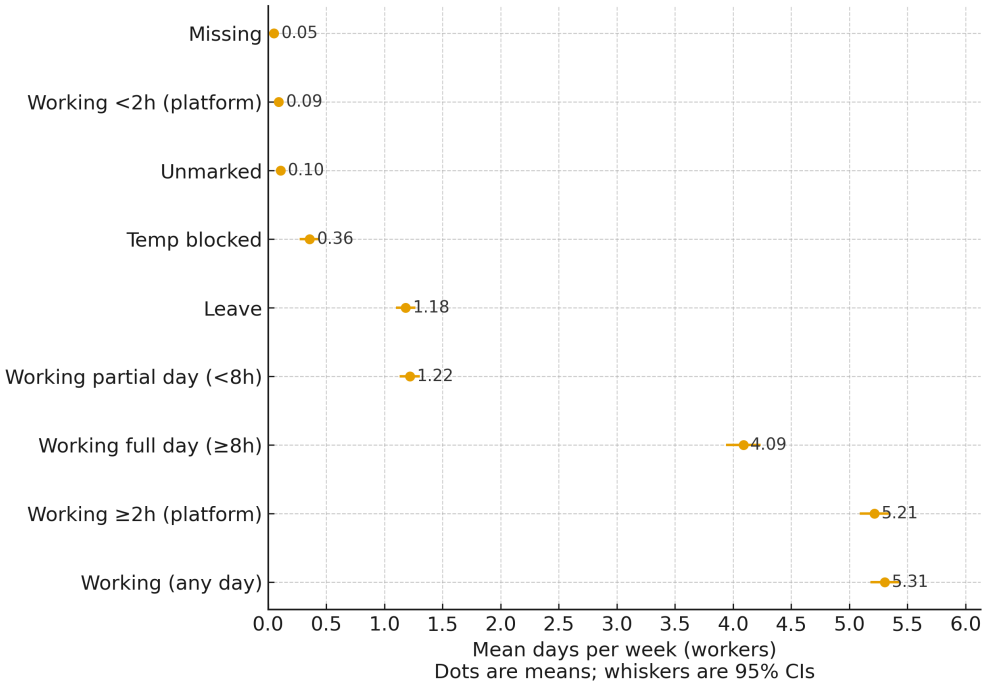
(b) Correlation Between Wealth and Baseline Productivity



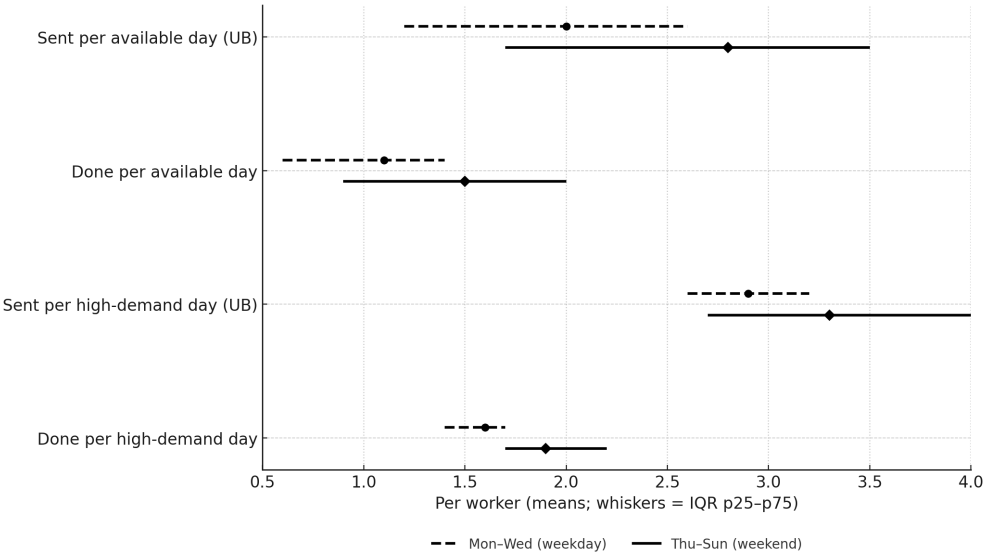
Notes: This figure documents baseline correlations between wealth and mobility/productivity measures. Wealth is measured at baseline and used to partition workers into wealth groups. Panel (a) plots the share of workers who report commuting by their own scooter at baseline by wealth quintile (poorest to richest). Percent labels indicate the fraction using an own scooter within each quintile. Panel (b) presents box-and-whisker plots comparing outcomes for workers above vs. below the median of the baseline wealth distribution. Outcomes are: daily earnings (INR/day), monthly travel expense for work (INR/month), daily labor supply (hours online per day), and daily services completed (jobs/day). For each outcome, the box shows the interquartile range with the median line; whiskers extend to 1.5× the interquartile range (points beyond are plotted as outliers, if any). These are descriptive correlations at baseline and should not be interpreted causally.

Figure A3: Descriptive Facts: Daily Calendar Availability and Job Completion Rate for Workers

(a) Calendar Availability

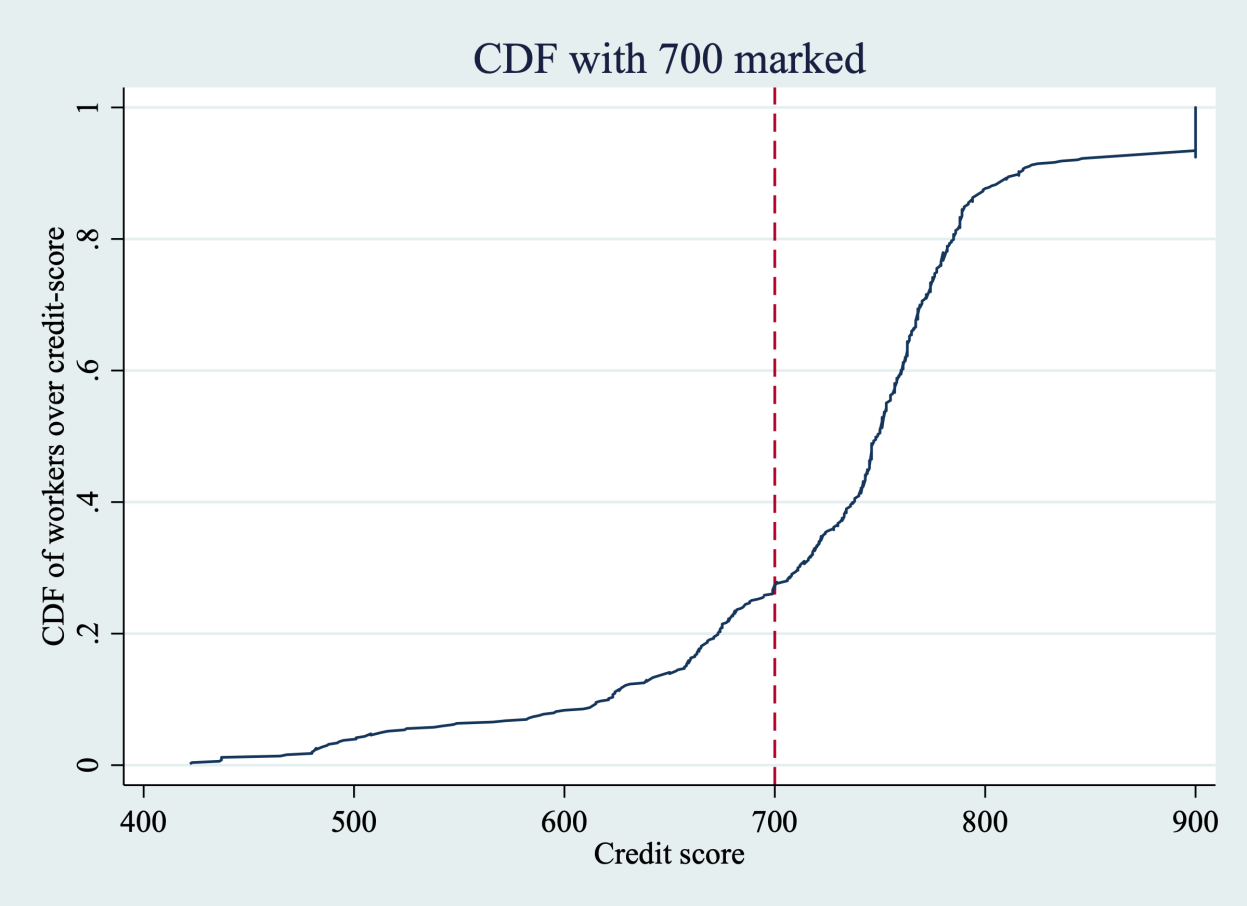


(b) Job Completion Rate Per Day



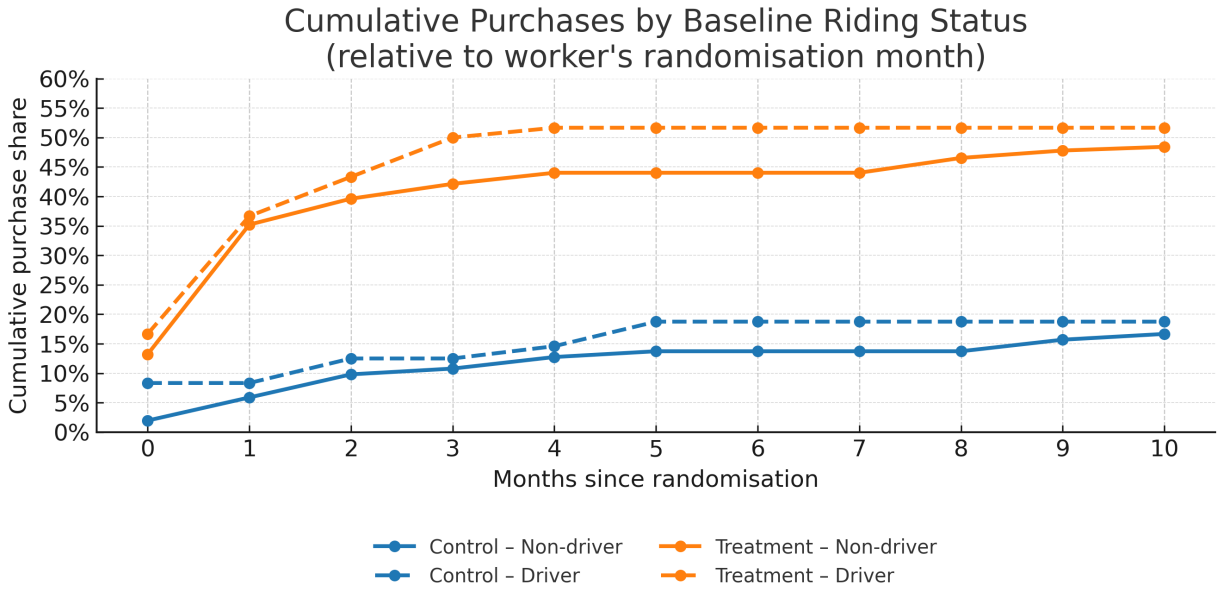
Notes: This figure uses administrative logs to describe scheduling marks on the worker calendar and average daily activity; all statistics are descriptive. Panel (a) shows, for each status label, the **average number of days per week per worker that a given day is marked with that label in the platform calendar: Working (any day), Working >2h (platform), Working full day (>8h), Working partial day (<8h), Leave, Temp blocked, Unmarked, Working <2h (platform), and Missing. Points are sample means; whiskers are 95% confidence intervals. Panel (b) shows, on an average worker-day, the number of jobs the platform sends (offers) versus the number of jobs completed, separately for weekdays (Mon–Wed, dashed) and weekend/high-demand days (Thu–Sun, solid). "Sent per available day (UB)" counts offers on days the worker is marked available; "Done per available day" counts completed jobs on those days. "Sent per high-demand day (UB)" and "Done per high-demand day" restrict to high-demand days. Whiskers indicate the interquartile range (p25–p75).

Figure A4: Worker's Credit Score (CIBIL) Distribution



Notes: This figure shows the empirical cumulative distribution function (CDF) of workers' baseline credit-bureau scores (CIBIL). The x-axis reports the credit score on the CIBIL scale; the y-axis shows the fraction of workers with a score less than or equal to each value. Scores are measured prior to the experiment. The vertical dashed line marks a score of 700 for reference.

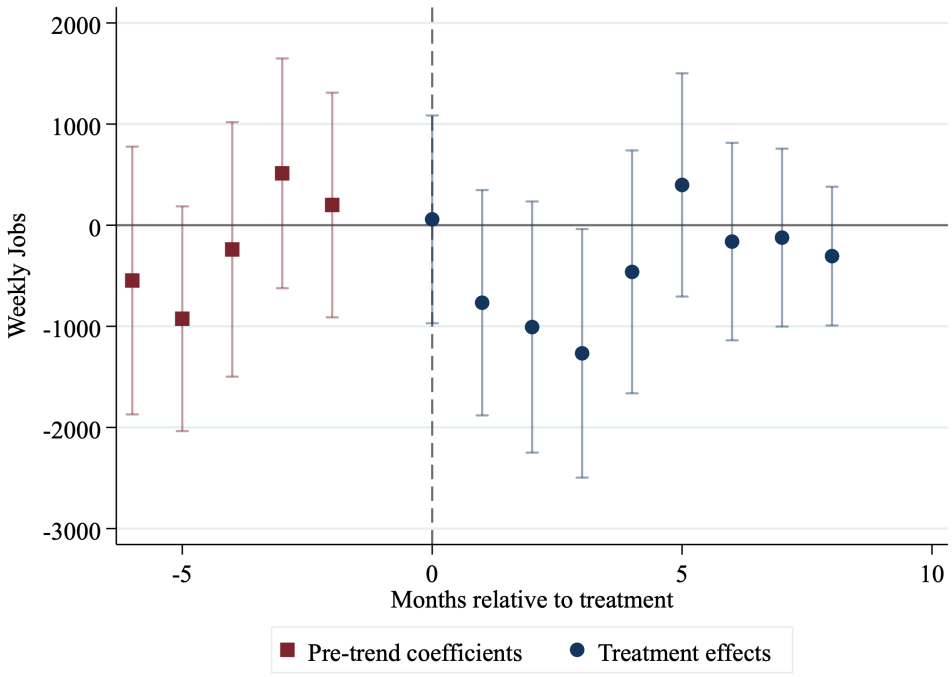
Figure A5: Take-up Rate Over Time



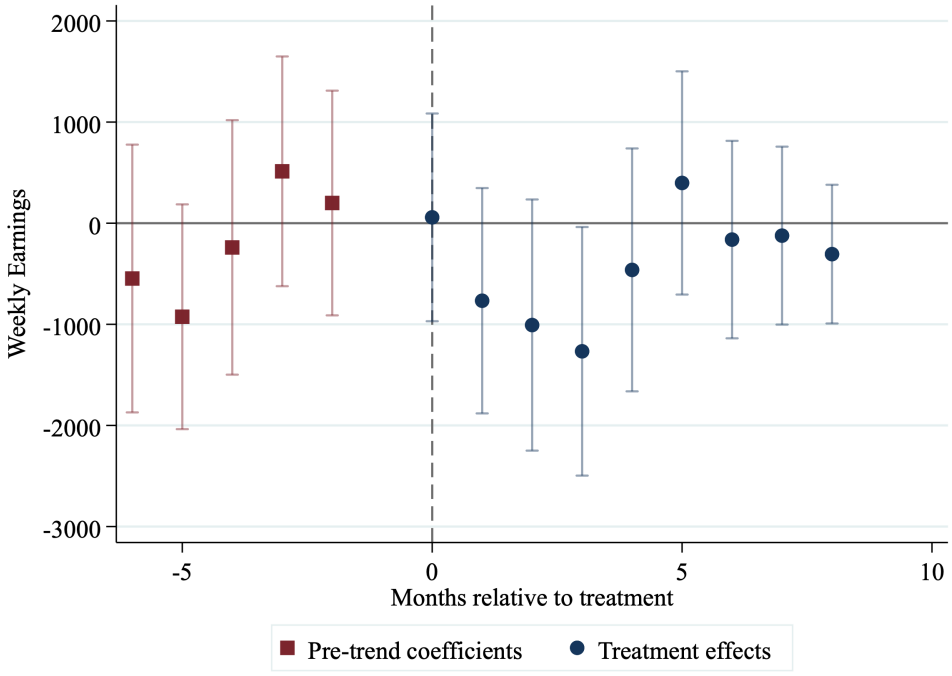
Notes: This figure plots the cumulative share of workers who purchase a scooter over event time. Event time is measured in months since each worker's cohort randomization; month 0 is the randomization month. Curves are shown separately by treatment status and by baseline riding status (driver = reported commuting by own scooter at baseline; non-driver otherwise). Solid lines denote non-drivers; dashed lines denote drivers.

Figure A6: Impact on Job Outcomes Over Time for Incumbent-Users

(a) Impact on Jobs Over Time for Incumbent Users

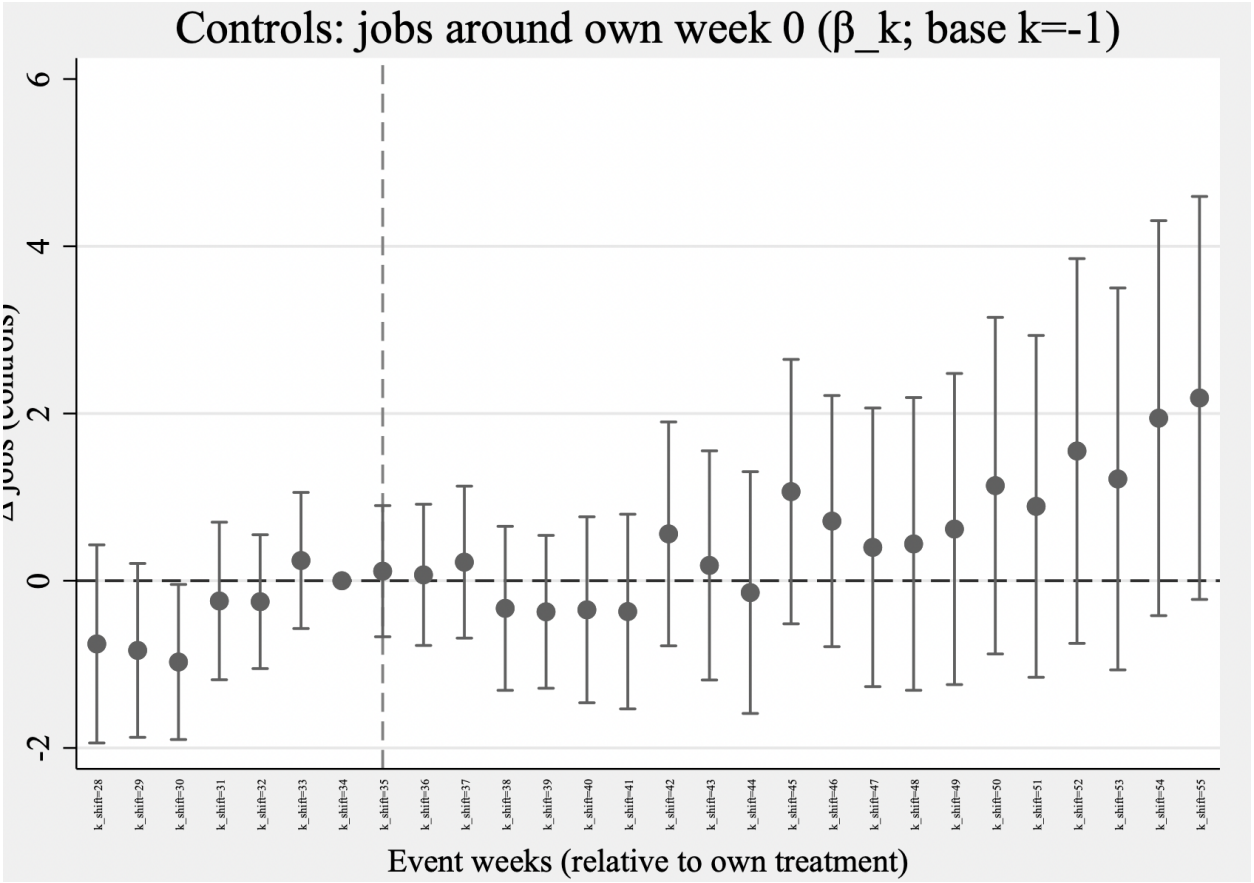


(b) Impact on Earnings Over Time for Incumbent Users



Notes: This figures plots the event–time difference-in-differences estimates for incumbent users in the experimental sample. The horizontal axis reports months relative to scooter subsidy offer, with 0 denoting the month of offer. Estimates are obtained from weekly panel regressions that include cohort fixed effects and calendar-week fixed effects. Event-time indicators are interacted with the treatment group; for controls these indicators are zero in all weeks. The indicator for month -1 (the last pre-adoption month) is omitted, so each coefficient measures the average outcome at event time k relative to month -1, net of cohort and week effects. Weekly coefficients are binned to monthly event times for display. Squares plot pre-trend coefficients for $k \in (-6,\ldots,-2)$; circles plot post-adoption coefficients for $k \in (0,\ldots,+8)$. Vertical bars show 95 percent confidence intervals with standard errors clustered at the worker level. Panel (a) reports weekly jobs; Panel (b) reports weekly earnings (INR). Pre-adoption estimates are near zero, indicating no detectable pre-trends relative to month 1. Beginning in month 0—when exposure is partial by construction—and persisting thereafter, both weekly jobs and weekly earnings are higher than in the last untreated month.

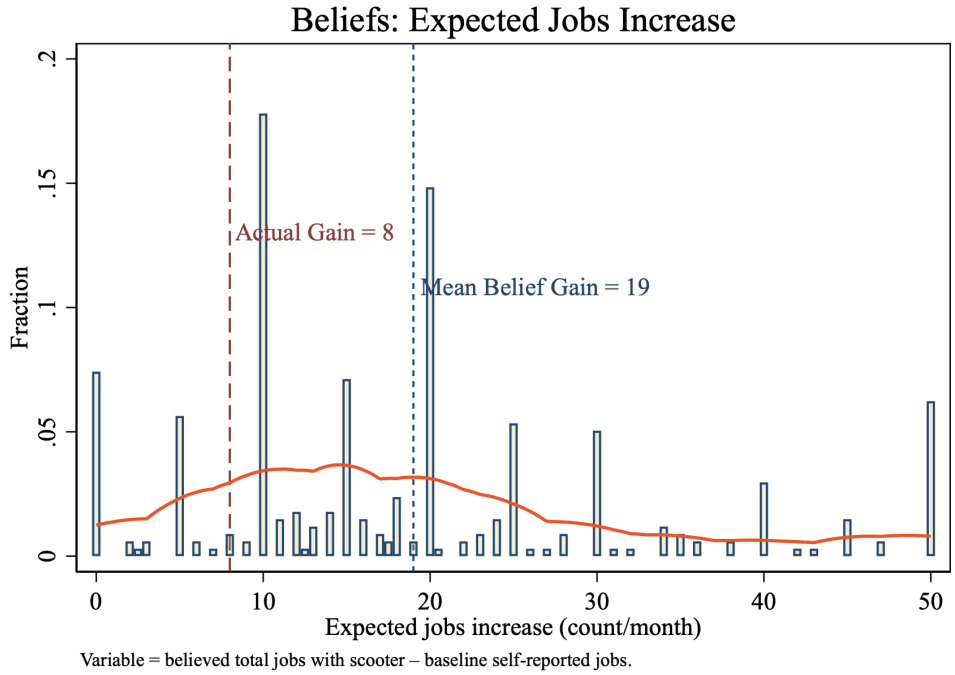
Figure A7: Impact on Jobs of Control Workers Over Time



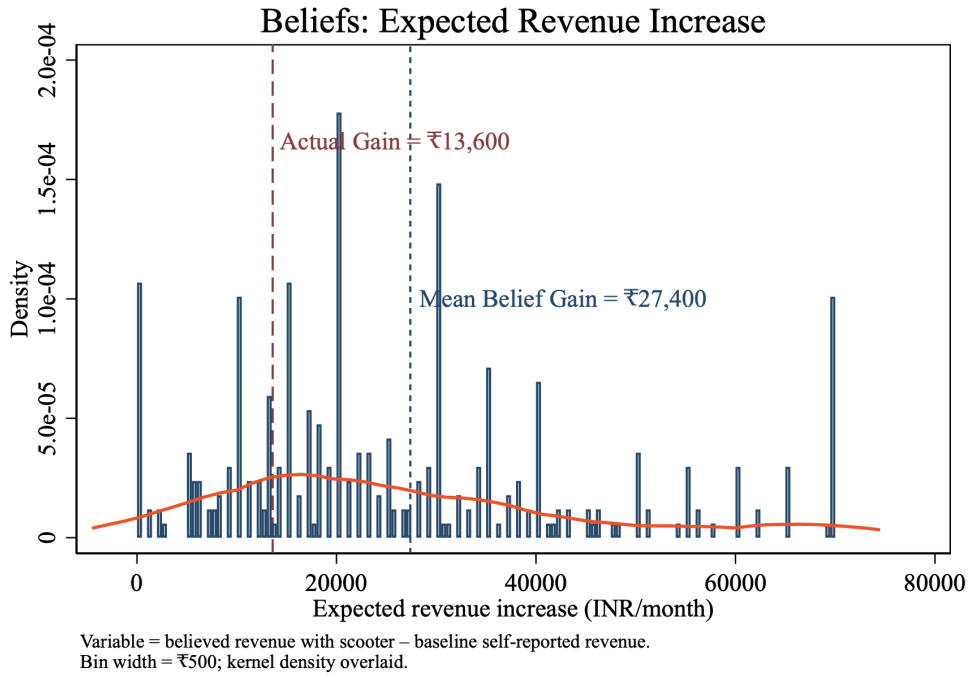
Notes: This figure reports a placebo event–study using only *control* workers. Outcome: weekly jobs. Event time k is measured in weeks relative to each control worker's cohort week 0, defined as the calendar week in which that cohort was assigned treatment in the experiment (month 0 in the main design). The omitted category is $k = -1$ (the last pre–period). Regressions include cohort (randomization–batch) fixed effects and calendar–week fixed effects. Points plot the event–time coefficients; vertical bars are 95% confidence intervals (standard errors clustered at the worker level).

Figure A8: Baseline Beliefs About Jobs and Revenue Increase Conditional on Scooter Adoption

(a) Baseline Beliefs: Jobs



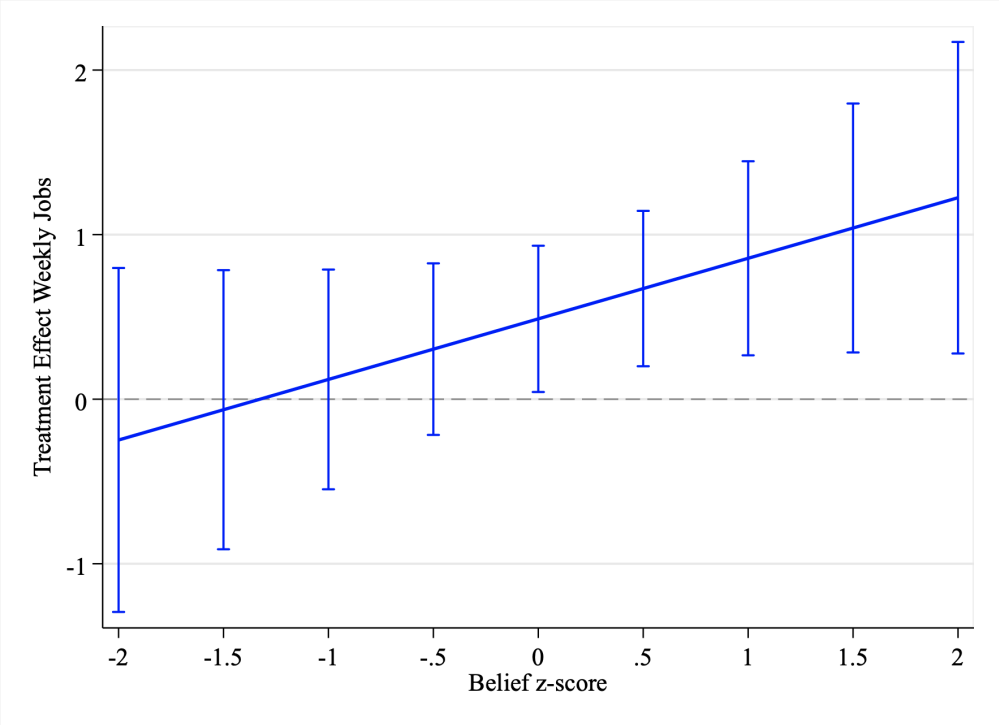
(b) Baseline Beliefs: Earnings



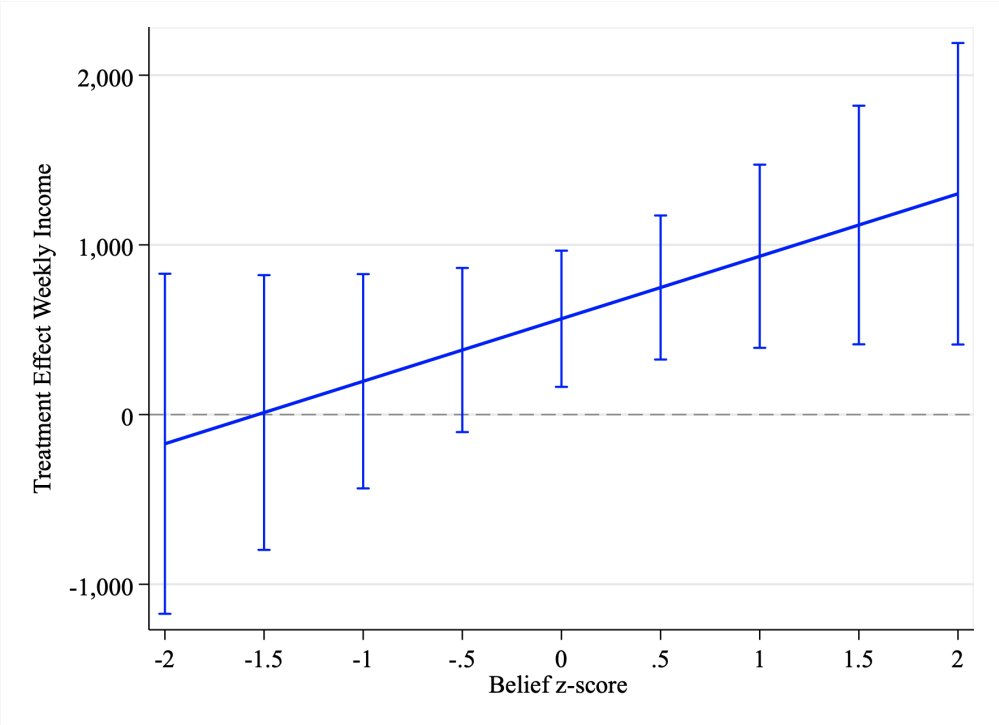
Notes: This figure summarizes baseline beliefs about gains from adopting a scooter, elicited prior to the experiment. Panel (a) plots the distribution of workers' expected monthly increase in jobs, defined as believed total jobs with a scooter minus self-reported baseline jobs (count/month). Bars show the empirical histogram (y-axis = fraction of respondents). The vertical dashed lines mark the sample mean of believed gains (blue) and the realized experimental gain used for reference (red). Panel (b) plots the distribution of workers' expected monthly increase in revenue, defined analogously as believed revenue with a scooter minus self-reported baseline revenue (INR/month). Bars show the histogram (bin width = INR 500) with a kernel density overlaid; the vertical dashed lines again mark the mean believed gain (blue) and the realized experimental gain used for reference (red).

Figure A9: Heterogeneity in Treatment Effects by Beliefs

(a) Heterogeneity in Impact on Jobs by Beliefs



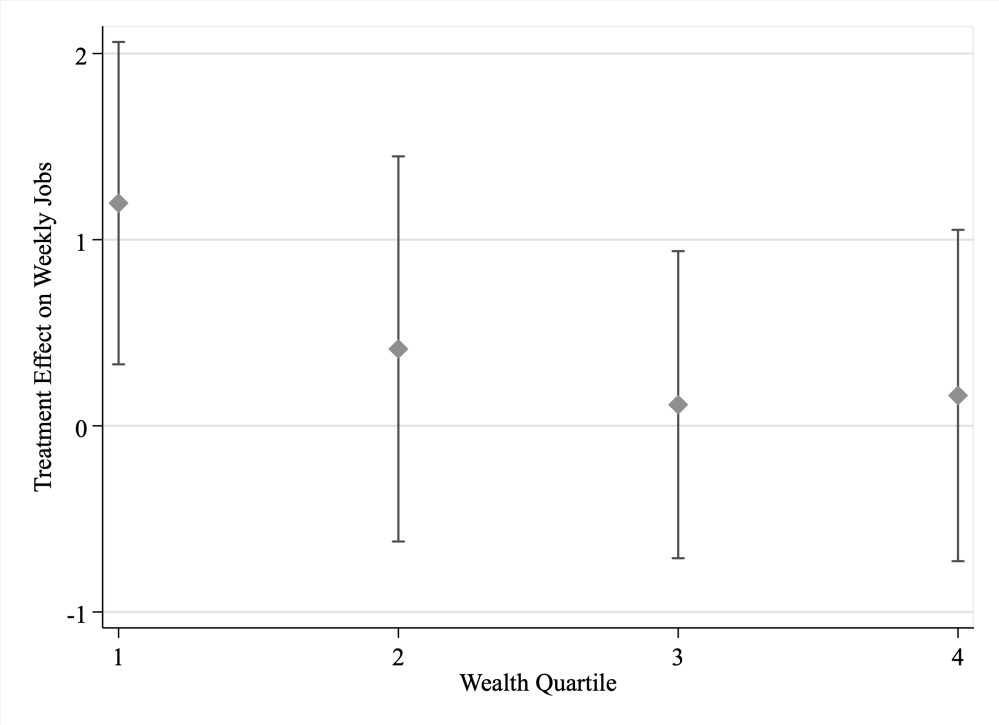
(b) Heterogeneity in Impact on Earnings by Beliefs



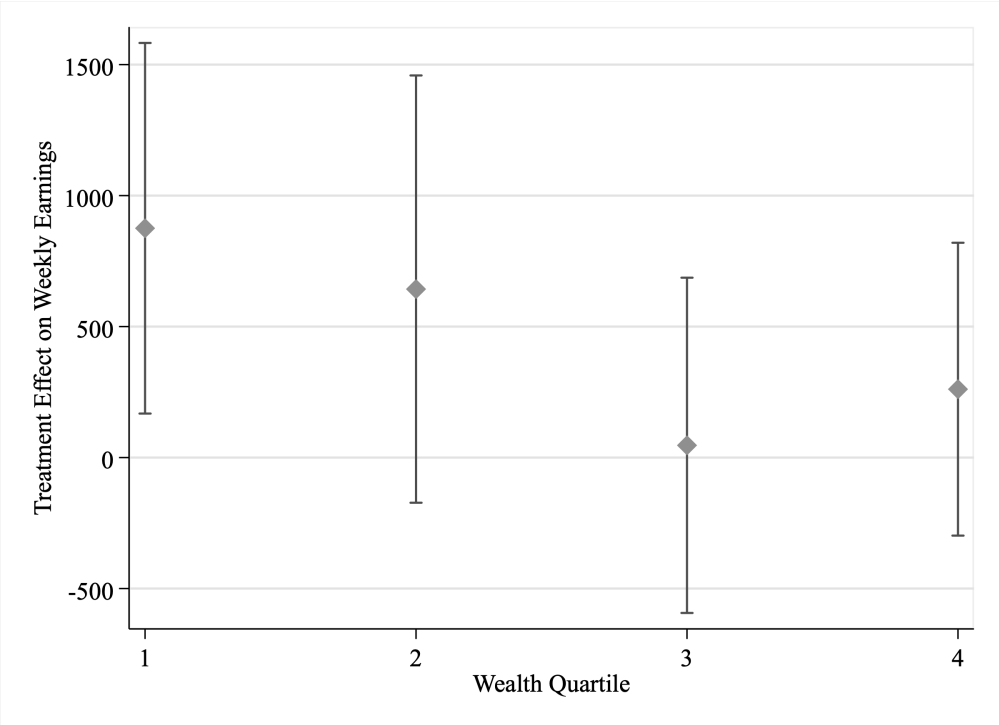
Notes: This figure shows heterogeneity in treatment effects by baseline beliefs about gains from adopting a scooter. Beliefs are elicited pre-experiment and standardized to a z-score (mean = 0, s.d. = 1); higher values indicate larger expected gains. For each outcome, treatment effects are estimated using Equation 1 with cohort (randomization-batch) fixed effects and calendar-week fixed effects. Panel (a) reports weekly jobs; Panel (b) reports weekly earnings (INR).

Figure A10: Heterogeneity in Treatment Effects by Wealth

(a) Heterogeneity in Impact on Jobs by Wealth



(b) Heterogeneity in Impact on Earnings by Wealth



Notes: This figure examines heterogeneity in treatment effects by baseline wealth. Wealth is measured at baseline and partitioned into quartiles (1 = poorest, 4 = richest). For each quartile, treatment effects are estimated using Equation 1 with cohort (randomization-batch) fixed effects and calendar-week fixed effects. Points plot the estimated treatment effect within each wealth quartile (from interactions of the treatment indicator with quartile dummies). Vertical bars are 95 percent confidence intervals with standard errors clustered at the worker level. Panel (a) reports weekly jobs; Panel (b) reports weekly earnings (INR).

Table A1: Baseline Balance by CIBIL (Credit) Score Among (All) Applicants

Outcome	Coef	p-val
Applied / tried for scooter loan	-0.056	0.208
Children in HH	-0.071	0.056
Earnings (last month)	-1.4e+03	0.632
Education (years)	-0.030	0.831
Got scooter loan (baseline)	-0.092	0.226
Jobs (last month)	-0.304	0.859
Married	-0.047	0.568
Travel expenditure (weekly)	-558.566	0.179

Notes: Notes: This table shows baseline balance in observables among workers who applied to the subsidy program. The comparison is by credit-score group defined at the CIBIL threshold of 700: (N<700) ("lower score") versus (N≥ 700) ("higher score"), where (N) denotes the CIBIL score. For each outcome, the "Coef" column reports the difference in means from an OLS regression of the baseline variable on an indicator for (N<700). Thus, a negative coefficient indicates a lower average for the (N<700) group. The "(p)-val" column reports the two–sided (p)-value for the null of equal means across the two groups. Standard errors are clustered at worker level. All variables are measured prior to treatment using baseline data. Variable definitions: "Applied / tried for scooter loan" indicates any prior application attempt; "Children in HH" is the number of children residing in the household; "Earnings (last month)" is total labor income in the month before baseline; "Education (years)" is completed schooling; "Got scooter loan (baseline)" indicates pre-existing vehicle finance; "Jobs (last month)" counts platform jobs completed in the month before baseline; "Married" is marital status at baseline; and "Travel expenditure (weekly)" is total transport spending during the baseline week. Monetary values are INR. Overall, the two score groups appear similar on observables: estimated differences are small in magnitude and generally not statistically significant.

Table A2: Baseline Balance

	Pooled Sample			New Adopters			Incumbent Users		
	Control	Treat	P-val (Diff)	Control	Treat	P-val (Diff)	Control	Treat	P-val (Diff)
Panel A: Baseline Survey Data									
Age	34.97	34.90	0.961	34.40	34.50	0.831	36.17	35.95	0.739
Married	0.75	0.75	0.501	0.75	0.72	0.949	0.73	0.85	0.173
Have Kids	0.90	0.89	0.944	0.84	0.88	0.379	1.00	0.93	0.088
Use 3-Wheelers/Bike-Taxis	0.55	0.57	1.000	0.81	0.78	.	0.00	0.00	.
Drives own scooter	0.32	0.27	0.236	0.00	0.00	.	1.00	1.00	.
Knows scooty driving	0.89	0.86	0.339	0.85	0.81	0.378	0.98	1.00	0.348
Total HH mems	3.83	4.00	0.418	3.72	4.01	0.145	4.04	3.98	0.694
Num HH know driving	2.26	2.25	0.605	2.11	2.17	0.856	2.58	2.47	0.380
Total earning members	1.96	2.08	0.235	1.97	2.08	0.460	1.94	2.08	0.446
Wealth index	-0.00	0.06	0.885	-0.36	-0.17	0.498	0.76	0.66	0.718
Husband unemployed	0.08	0.07	0.991	0.10	0.07	0.806	0.04	0.05	0.790
Service subcategory	4.40	4.26	0.211	4.42	4.26	0.080	4.34	4.25	0.781
Travel expenditure	5,359	5,673	0.574	6,223	6,578	0.334	3,542	3,350	0.372
Panel B: Baseline Admin Data									
Platform Experience	3.36	3.15	0.294	3.02	3.15	0.815	4.06	3.17	0.037
Daily working hours	7.92	7.93	0.675	7.69	7.90	0.318	8.40	7.99	0.529
Weekly working hours	51.14	50.93	0.991	49.26	50.62	0.567	54.99	51.72	0.395
Daily services delivered	1.01	1.01	0.790	1.00	1.01	0.997	1.05	1.03	0.614
Weekly services delivered	6.67	6.69	0.818	6.53	6.64	0.969	6.95	6.80	0.584
Daily app earnings	792	774	0.672	763	766	0.611	852	794	0.206
Weekly app earnings	5,212	5,108	0.688	5,009	5,058	0.587	5,628	5,235	0.185
Daily rating	4.84	4.84	0.766	4.84	4.83	0.939	4.83	4.86	0.170

Notes: This table shows balance at baseline. Columns (1)–(3) report statistics pooled sample; columns (4)–(6) new adopters; columns (7)–(9) for the incumbent users. Within each triplet, the first two columns report Control and Treatment means, respectively; the third column reports the two–sided p-value from a test of equality of means between Treatment and Control in that subgroup. Panel A variables come from the baseline survey. Panel B variables come from platform administrative records measured at baseline; daily measures are averaged to the worker level and weekly measures are totals over the baseline week. "Daily rating" is on the platform's 1–5 scale. Among incumbent users, baseline "Platform Experience" is modestly imbalanced (control 4.06 years vs. treatment 3.17 years, (p=0.037)); all incumbent placebo results are unchanged when we control for baseline experience. Monetary variables are INR. The wealth index is constructed from baseline assets (first principal component).

Table A3: Take Up Characteristics

	New Adopters		Incumbent-Users		Pooled Sample	
	(1) Treatment	(2) Treatment× Cov	(3) Treatment	(4) Treatment× Cov	(5) Treatment	(6) Treatment× Cov
Panel A: Survey Covariates						
Treatment (no covariate)	0.317***		0.355***		0.319***	
Drives own scooter					0.305***	0.038
Age	0.350***	0.138**	0.341***	0.153	0.338***	0.137**
Married	0.316***	0.039	0.360***	-0.013	0.319***	0.018
Have Kids	0.320***	0.069	0.371***	0.000	0.326***	0.083
Use 3-Wheelers/Bike-Taxis	0.357***	-0.072	0.355***	0.000	0.319***	-0.029
Knows scooty driving	0.313***	-0.004	0.352***	0.000	0.318***	-0.005
Total HH mems	0.313***	0.161***	0.362***	-0.131	0.317***	0.103**
Num HH know driving	0.338***	0.124*	0.340***	0.070	0.324***	0.093*
Total earning members	0.313***	0.078	0.334***	-0.203**	0.313***	-0.009
Wealth index	0.340***	0.099	0.340***	0.044	0.322***	0.064
Husband unemployed	0.318***	0.034	0.336***	-0.007	0.318***	0.055
Service subcategory	0.322***	-0.039	0.358***	-0.009	0.321***	-0.014
Travel expenditure	0.302***	0.033	0.411***	0.099	0.321***	0.036
Panel B: Admin Covariates						
Platform Experience	0.322***	0.087	0.334***	-0.051	0.311***	0.044
Daily working hours	0.309***	-0.090	0.367***	-0.141*	0.315***	-0.083
Weekly working hours	0.309***	-0.077	0.375***	-0.170**	0.314***	-0.080
Daily services delivered	0.314***	-0.061	0.372***	-0.233**	0.316***	-0.073
Weekly services delivered	0.315***	-0.053	0.373***	-0.237**	0.316***	-0.068
Daily app earnings	0.312***	-0.054	0.365***	-0.068	0.318***	-0.015
Weekly app earnings	0.313***	-0.047	0.365***	-0.073	0.318***	-0.011
Daily rating	0.322***	0.036	0.333***	-0.015	0.317***	0.044

Notes: The dependent variable is an indicator for scooter take-up during the study window (intent-to-treat; equals 1 if the worker purchases a scooter on credit by the end of follow-up). Each row reports coefficients from a model of the form where X_i is the baseline covariate named in the row. The coefficient reported under the "Treatment" column is the impact of Treatment on take-up, the coefficient under "Treatment×Cov" is heterogeneity in the treatment effect with respect to various covariates. The row "Treatment (no covariate)" reports the take-up effect with no interaction. Columns (1)–(2) restrict tonew adopters (baseline non-drivers), Columns (3)–(4) to incumbent-users (baseline drivers), and Columns (5)–(6) to the pooled sample. Panel A covariates come from the baseline survey; Panel B covariatses come from baseline administrative records (daily measures averaged, weekly measures summed, at the worker level). All specifications include cohort–stratum fixed effects (randomization strata). SEs are clustered at worker level. Significant at * 10%, ** 5% and ***1% level.

Table A4: Robustness: Impact on Jobs and Earnings of New Adopters

	New Adopters					
	(1)	(2)	(3)	(4)	(5)	(6)
	Jobs	Earnings	Jobs	Earnings	Jobs	Earnings
Treatment effect (ITT)	0.54**	602.40***	0.56**	633.75***	0.54**	603.99***
	(0.23)	(204.41)	(0.23)	(207.36)	(0.25)	(224.51)
Treatment effect (LATE)	1.79**	2,018.46***	1.86**	1,898.57**	1.79**	2,021.89**
	(0.79)	(768.03)	(0.85)	(767.17)	(0.87)	(842.71)
Control mean	6.12	4,757.98	6.12	4,757.98	6.08	4,651.74
N (worker–weeks)	8,167	8,167	8,167	8,167	7,787	7,787
Strata FE	✓	✓	✓	✓		
Week FE	✓	✓	✓	✓		
Baseline Y	✓	✓	✓	✓	✓	✓
Lin Adjustment			✓	✓		
Strata × Week FE					✓	✓

Notes: This table presents robustness checks for the main effects on weekly jobs and weekly earnings among new adopters using administrative worker–week data over the eight months after randomization. Each column is a separate regression; odd columns report jobs and even columns report earnings. "Treatment effect (ITT)" is the coefficient on the randomized offer indicator. "Treatment effect (LATE)" is an instrumental-variables estimate that instruments actual subsidy receipt with the randomized offer and is interpreted for compliers (see Section 5). Standard errors, in parentheses, are clustered at the worker level. "Control mean" is the mean for controls in this stratum; (N) is the number of worker–week observations. Specification features indicated by check marks: "Strata FE" = randomization-cohort fixed effects; "Week FE" = calendar-week fixed effects; "Baseline Y" = the corresponding baseline outcome included as a covariate; "Lin Adjustment" = the Lin (2013) regression-adjusted specification—baseline covariates (including the baseline outcome) are demeaned within strata and fully interacted with treatment to improve precision under randomization; "Strata × Week FE" = cohort-by-week fixed effects allowing fully flexible common shocks by cohort.

Table A5: Robustness: Impact on Jobs and Earnings of Incumbent Users

	Imcumbent Users					
Treatment effect (ITT)	−0.34	−89.14	−0.33	−83.45	−0.36	−105.22
	(0.34)	(289.32)	(0.33)	(291.94)	(0.37)	(318.16)
Treatment effect (LATE)	−0.89	−236.72	−1.58	−273.25	−0.95	−280.07
	(0.88)	(764.43)	(1.44)	(726.09)	(0.97)	(841.57)
Control mean	7.24	5,799.22	7.24	5,799.22	7.41	5,932.15
N (worker–weeks)	3,570	3,570	3,570	3,570	3,415	3,415
Strata FE	✓	✓	✓	✓	✓	✓
Week FE	✓	✓	✓	✓	✓	✓
Lin Adjustment			✓	✓		
Strata × Week FE					✓	✓

Notes: This table presents robustness checks for the main effects on weekly jobs and weekly earnings among incumbent users using administrative worker–week data over the eight months after randomization. Each column is a separate regression; odd columns report jobs and even columns report earnings. "Treatment effect (ITT)" is the coefficient on the randomized offer indicator. "Treatment effect (LATE)" is an instrumental-variables estimate that instruments actual subsidy receipt with the randomized offer and is interpreted for compliers (see Section 5). Standard errors, in parentheses, are clustered at the worker level. "Control mean" is the mean for controls in this stratum; (N) is the number of worker–week observations. Specification features indicated by check marks: "Strata FE" = randomization-cohort fixed effects; "Week FE" = calendar-week fixed effects; "Baseline Y" = the corresponding baseline outcome included as a covariate; "Lin Adjustment" = the Lin (2013) regression-adjusted specification—baseline covariates (including the baseline outcome) are demeaned within strata and fully interacted with treatment to improve precision under randomization; "Strata × Week FE" = cohort-by-week fixed effects allowing fully flexible common shocks by cohort.

Table A6: Robustness: Impact on Jobs and Earnings of Pooled Sample

	Pooled Sample					
Treatment effect (ITT)	0.18	322.23*	0.17	330.89*	0.17	318.98*
	(0.19)	(170.48)	(0.20)	(173.06)	(0.21)	(184.41)
Treatment effect (LATE)	0.65	1,103.50*	0.65	1,028.48*	0.64	1,093.73*
	(0.62)	(568.02)	(0.64)	(553.30)	(0.67)	(613.90)
Control mean	6.49	5,097.52	6.49	5,097.52	6.46	5,041.09
N (worker–weeks)	11,737	11,737	11,737	11,737	11,512	11,512
Strata FE	✓	✓	✓	✓	✓	✓
Week FE	✓	✓	✓	✓	✓	✓
Lin Adjustment			✓	✓		
Strata × Week FE					✓	✓

Notes: This table presents robustness checks for the main effects on weekly jobs and weekly earnings among pooled sample using administrative worker–week data over the eight months after randomization. Each column is a separate regression; odd columns report jobs and even columns report earnings. "Treatment effect (ITT)" is the coefficient on the randomized offer indicator. "Treatment effect (LATE)" is an instrumental-variables estimate that instruments actual subsidy receipt with the randomized offer and is interpreted for compliers (see Section 5). Standard errors, in parentheses, are clustered at the worker level. "Control mean" is the mean for controls in this stratum; (N) is the number of worker–week observations. Specification features indicated by check marks: "Strata FE" = randomization-cohort fixed effects; "Week FE" = calendar-week fixed effects; "Baseline Y" = the corresponding baseline outcome included as a covariate; "Lin Adjustment" = the Lin (2013) regression-adjusted specification—baseline covariates (including the baseline outcome) are demeaned within strata and fully interacted with treatment to improve precision under randomization; "Strata × Week FE" = cohort-by-week fixed effects allowing fully flexible common shocks by cohort.

Table A7: Impact on Travel Expenditure, Commute Time or Workers, and Spillover on Family Members' Earnings and Jobs

	Commuting outcomes		Spillovers on family	
	Travel Cost (INR)	Travel Time (Minutes)	Household Income (INR) (Non-Worker)	Prob. Husband Unemployed
	(1)	(2)	(3)	(4)
Panel A: New Adopters				
Treatment effect	-18.16*	-3.35	2,407.86	0.02
	(9.60)	(4.41)	(2,612.37)	(0.04)
Control mean	120.47	48.51	23,141.25	0.11
N: Worker-Survey Round	971	1,029	325	456
Panel B: Incumbent Users				
Treatment effect	-13.56*	-8.15**	7,742.88**	-0.03
	(7.61)	(3.96)	(3,251.54)	(0.04)
Control mean	65.36	45.47	18,038.76	0.11
N: Worker-Survey Round	448	467	147	206
Panel C: Pooled Sample				
Treatment effect	-15.77**	-4.63	4,889.24**	0.00
	(6.53)	(3.28)	(1,990.01)	(0.03)
Control mean	100.13	47.42	21,311.01	0.11
N: Worker-Survey Round	1,426	1,500	472	662

Notes: This table reports intent-to-treat estimates on commuting outcomes for workers and spillovers on family members, using follow-up survey data. Commuting outcomes (Cols. 1–2): Workers were contacted in three follow up survey rounds after randomization. In each call, they were asked to list every job performed on the day prior to the call and, for each job, to report (i) the out-of-pocket travel cost (INR) and (ii) the minutes spent traveling for that job. The analysis is conducted at the job level (one observation per job reported across the three waves); coefficients therefore capture per-job effects on travel cost and travel time. Household spillovers (Cols. 3–4): Household income (excluding the platform worker) and the spouse's employment status were collected in two follow-up survey rounds. These outcomes are analyzed at the **worker–survey-round** level. Panels A–C correspond to new adopters, incumbent users, and the pooled sample, respectively. Regressions follow Equation 1 and include cohort (randomization-batch) fixed effects and **survey-wave** fixed effects; standard errors, in parentheses, are clustered at the worker level. "Control mean" is the mean for controls in the corresponding panel; (N) reports the number of observations for that column (jobs for Cols. 1–2; worker–survey rounds for Cols. 3–4). Currency is INR. Stars denote p-values (* ($p<0.10$), ** ($p<0.05$), *** ($p<0.01$)).

Table A8: Impact on Distance Traveled and Job Timing: Incumbent Users

Panel A: Distance						
	Total KMs	**0–6 KMs**	**6–12 KMs**	**12–18 KMs**	**18–24 KMs**	**24 KMs+**
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect (ITT)	-2.62	-0.19	-0.17	-0.09	-0.17**	-0.04
	(5.13)	(0.17)	(0.18)	(0.12)	(0.08)	(0.12)
Treatment Effect (TOT)	-6.76	-0.51	-0.45	-0.25	-0.46*	-0.09
	(13.14)	(0.49)	(0.47)	(0.33)	(0.24)	(0.30)
Control Mean	93.69	1.93	2.40	1.29	0.70	0.94
N: worker-weeks	3,467	3,467	3,467	3,467	3,467	3,467
Panel B: Timing						
	Pre 10 AM	**Pre 11 AM**	**12–4 PM**	**Post 5 PM**	**Post 6 PM**	**Surge**
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect (ITT)	-0.18	-0.29	-0.21	-0.11	-0.06	-6.08
	(0.14)	(0.20)	(0.26)	(0.14)	(0.06)	(9.51)
Treatment Effect (TOT)	-0.53	-0.74	-0.62	-0.33	-0.17	-26.75
	(0.46)	(0.65)	(0.77)	(0.41)	(0.18)	(29.24)
Control Mean	1.65	2.46	3.59	1.28	0.46	69.49
N: worker-weeks	3,499	3,499	3,499	3,499	3,499	3,499

Notes: This table shows the impact of treatment on distance (Panel A) and job-timing (Panel B) for the incumbent-users sub-group. Panel A column 1 shows the impact on total distance traveled in a week for jobs, columns 2-6 show the impact on total number of jobs completed in a week within 0-6 KMs (2), 6-12 KMs (3), 12-18 KMs (4), 18-24 KMs (5), and 24+ KMs (6); round-trip from worker's home to customer's home location. Panel B columns 1-5 show the total number of jobs completed in a week that had a start time before 10 AM (1), before 11 AM (2), between 12 noon to 4 PM (3), post 5 PM (4) and post 6 PM (5). Panel B column 6 shows total amount earned in terms of 'surge' value in a given week. First row of each panel shows the intent-to-treat (ITT) estimates and the second row of each panel shows the treatment-on-the-treated (TOT) effects. Specifications include baseline value of dependent variable, randomization-cohort × randomization-strata fixed effects, and week fixed effects. Observations are at the worker-week level for 8 months post randomization period for each worker from administrative firm data. SEs are clusterd at the worker-level. Significant at * 10%, ** 5% and ***1% level.

Table A9: Treatment Effects on Job Quality Indicators: Incumbent Users

	Job Rating	**High Rating**	**Bad Rating**	**Absenteeism**	**Temp Block**
	(1)	(2)	(3)	(4)	(5)
Treatment Effect (ITT)	0.00	0.01	0.01	0.15	0.07
	(0.02)	(0.07)	(0.01)	(0.16)	(0.20)
Treatment (TOT)	0.01	0.03	0.01	0.30	0.19
	(0.05)	(0.19)	(0.03)	(0.29)	(0.54)
Control Mean	4.85	1.22	0.08	1.25	0.51
N: Worker-Weeks	2,693	3,570	3,570	3,570	3,570

Notes: This table shows the impact of treatment on job quality indicators for the incumbent users sub-group. Column 1-3 show the quality improvement on the customer side, i.e., improvement in worker's work quality. Column 1 shows the impact on ratings at the job rating, column 2 counts the total number of jobs that received a full five-star rating, column 3 counts the total number of jobs that received a bad rating of 0 or 1 in a given week. Columns 4-5 show the quality improvement from the firm's perspective. Column 4 shows the impact on total days in a week when the worker was fully unavailable to work. Column 5 shows the total number of days in any given week when the worker was temporarily blocked and asked to undergo additional training due to poor quality performance. First row shows the intent-to-treat (ITT) estimates and the second row of each panel shows the treatment-on-the-treated (TOT) effects. Specifications include baseline value of dependent variable, randomization-cohort $\times$ randomization-strata fixed effects, and week fixed effects. Observations are at the worker-week level for 8 months post randomization period for each worker from administrative firm data. SEs are clusterd at the worker-level. Significant at * 10%, ** 5% and ***1% level.

Table A10: Contract Menus and Payment Burden (Amounts in thousands of INR)

Contract	Down Payment	Principal	Interest[a]	Total	PV of Total [b]
Benchmark	15.0	85.0	33.2	133.2	112.2
Price discount	13.5	76.5	29.8	119.8	101.0
Down payment relief	5.0	95.0	37.1	137.1	113.7
Down payment discount	5.0 (net)[c]	85.0	33.2	123.2	102.2

[a] "Interest" is computed at a *flat* 18% per annum over a 26-month tenure on the original financed balance: Interest = Principal $\times$ 0.18 $\times$ (26/12) = 0.39 $\times$ Principal.
[b] PV of Payments = Down Payment + $\sum_{t=1}^{26} \frac{\text{EMI}}{(1+0.18/12)^t}$, where the monthly installment is EMI = (Principal + Interest)/26. Equivalently, PV = Down Payment + EMI $\cdot \frac{1-(1+0.18/12)^{-26}}{0.18/12}$. Values are rounded to one decimal (thousands of INR).
[c] "Down payment discount" keeps the contractual down payment at 15% of list price (INR 15,000), but the firm contributes INR 10,000 at purchase; the worker pays INR 5,000 up front. The financed principal and loan terms are identical to the Benchmark.

B Appendix B: Return to the Firm and Worker: Details

B.1 Return on Investment (ROI) for the Firm

This appendix details the computation of the firm's return on its scooter–subsidy program. The object is the *present value* (PV) of incremental profits generated by a treated worker over twelve months, net of the subsidy cost. Cash flows are discounted at an annual nominal rate of $r = 22\%$, consistent with a risk–adjusted hurdle rate for short-cycle working-capital investments in this setting for the firm.

B.1.1 Setup and timing

Let

- δ denote incremental *weekly* profit per treated worker (INR/week);
- s denote the 12-month survival rate (probability the worker remains active at 12 months) (estimated in Table 4 Column 4);
- C denote the per-worker subsidy cost (INR);
- $\beta \equiv (1+r)^{-1/52}$ denote the weekly discount factor.

I adopt a 52-week horizon with weeks $t = 1, \ldots, 35$ realized and weeks $t = 36, \ldots, 52$ projected using the observed survival s. Under no differential attrition by treatment, the survival profile is common, so I scale only the projected weeks by s.

B.1.2 Present value and ROI

The PV of incremental gains is

$$\mathrm{PV}_G(r) \;=\; \sum_{t=1}^{35} \beta^t \, \delta \;+\; \sum_{t=36}^{52} \beta^t \, s \, \delta. \tag{2}$$

The corresponding ROI is

$$\mathrm{ROI}(r) \;=\; \frac{\mathrm{PV}_G(r) - C}{C}. \tag{3}$$

B.1.3 Parameterization and estimate

For new adopters (compliers), I use

$$\delta \;=\; 557.87 \text{ INR/week (from Table 5)}, \qquad s \;=\; 0.891, \qquad C \;=\; 10{,}000 \text{ INR}.$$

Evaluating (2)–(3) at $r = 22\%$ (so $\beta = (1.22)^{-1/52}$):

$$\mathrm{PV}_G(0.22) \;=\; 25{,}382.96 \text{ INR}, \qquad \mathrm{ROI}(0.22) \;=\; \frac{25{,}382.96 - 10{,}000}{10{,}000} \;=\; 1.538 \;\; (\text{or } 153.8\%).$$

For comparison, the *undiscounted* calculation ($r = 0$) yields

$$\mathrm{PV}_G(0) \;=\; 35\delta + 17s\delta \;=\; 27{,}975.51 \text{ INR}, \qquad \mathrm{ROI}(0) \;=\; 1.798 \;\; (\text{or } 179.8\%).$$

Sensitivity of ROI: Based on different discount rate of the firm, the ROI varies only slightly as presented in table below. (Calculations in the table are for the **12-month** horizon.)

Table A11: ROI Sensitivity to the Discount Rate (r) – 12 months

Annual discount rate r	20%	22%	24%
$\mathrm{PV}_G(r)$ (INR)	25,585.84	25,382.96	25,185.49
$\mathrm{ROI}(r)$	1.559 (155.9%)	1.538 (153.8%)	1.519 (151.9%)

Notes: Computed from (2)–(3) with $\delta = 557.87$, $s = 0.891$, $C = 10{,}000$. A one–percentage–point change in r around 22% moves ROI by roughly 0.7–1.0 percentage points.

Twenty-four months (New Adopters/compliers). Extending (2) to $T = 104$ weeks and scaling weeks 36–104 by the observed 12-month survival s, the present value is

$$\mathrm{PV}_G^{24m}(0.22) = \sum_{t=1}^{35} \beta^t \delta + \sum_{t=36}^{104} \beta^t s\delta = 44{,}559.00 \text{ INR}.$$

With $C = 10{,}000$, the **24-month ROI** is

$$\mathrm{ROI}^{24m}(0.22) = \frac{44{,}559.00 - 10{,}000}{10{,}000} = 3.456 \quad (\text{or } 345.6\%).$$

B.1.4 Pooled Sample

For the pooled sample (compliers), the incremental weekly profit is $\delta = 345.95$ INR (see Table 5 Column 1 Pooled Sample), the 12-month survival rate is $s = 0.915$, and the per-worker subsidy cost is $C = 10{,}000$ INR. Using the same timing convention and discount factor $\beta = (1 + r)^{-1/52}$ with $r = 22\%$, the present value of incremental gains from (2) is

$$\mathrm{PV}_G^{\mathrm{pooled}}(0.22) = \sum_{t=1}^{35} \beta^t \delta \; + \; \sum_{t=36}^{52} \beta^t s\delta = 15{,}859.96 \text{ INR}.$$

The implied **12-month ROI** from (3) is therefore

$$\mathrm{ROI}_{12m}^{\mathrm{pooled}}(0.22) = \frac{15{,}859.96 - 10{,}000}{10{,}000} = 0.586 \quad (\text{or } 58.6\%).$$

Twenty-four months (pooled sample). Extending the horizon to 104 weeks (scaling weeks 36–104 by $s = 0.915$) gives

$$\mathrm{PV}_G^{\mathrm{pooled},\,24m}(0.22) = \sum_{t=1}^{35} \beta^t \delta + \sum_{t=36}^{104} \beta^t s\delta = 28{,}071.84 \text{ INR},$$

and hence the **24-month ROI** is

$$\mathrm{ROI}_{24m}^{\mathrm{pooled}}(0.22) = \frac{28{,}071.84 - 10{,}000}{10{,}000} = 1.807 \quad (\text{or } 180.7\%).$$

B.2 Worker ROI: New Adopters

This section computes the worker's discounted return on the scooter purchase. Let δ_w denote the incremental *weekly* earnings attributable to adoption (INR/week), C_w the worker's *investment cost*, and r_w the annual discount rate relevant for the worker (APR). We take $r_w = 18\%$ as in the main text; the weekly discount factor is $\beta_w = (1 + r_w)^{-1/52}$. Survival is incorporated analogously to the firm calculation: weeks $t = 1, \ldots, 35$ are realized; weeks $t = 36, \ldots, 52$ are multiplied by the observed 12-month survival $S_{52} = 0.891$. For the 24-month horizon, we scale weeks $36, \ldots, 104$ by the same survival (constant-hazard approximation).

Parameters (new adopters).

$$\delta_w = 2{,}018 \text{ INR/week}, \quad r_w = 18\% \Rightarrow \beta_w = (1.18)^{-1/52} \approx 0.99682, \quad S_{52} = 0.891.$$

We report returns using the worker's *out-of-pocket* cost that nets out the firm subsidy: $C_w^{\text{net}} = 106{,}000 - 10{,}000 = 96{,}000$ INR.[34]

B.2.1 Definition

Let $S_t = 1$ for $t \leq 35$ and $S_t = S_{52}$ for $t \geq 36$. The present value (PV) of incremental earnings over horizon T weeks is

$$G_{1:T}(r_w) \;=\; \sum_{t=1}^{T} \beta_w^t \, \delta_w \, S_t. \tag{4}$$

Worker ROI over horizon T is

$$\text{ROI}_T^{\text{worker}}(r_w) \;=\; \frac{G_{1:T}(r_w) - C_w}{C_w}. \tag{5}$$

B.2.2 Twelve months (52 weeks)

Evaluating (4) at $T = 52$ gives

$$G_{1:52}(0.18) \;=\; \sum_{t=1}^{35} \beta_w^t \delta_w \;+\; \sum_{t=36}^{52} \beta_w^t \, S_{52} \, \delta_w \;=\; 93{,}306.82 \text{ INR}.$$

Using the net cost $C_w^{\text{net}} = 96{,}000$,

$$\text{ROI}_{12m}^{\text{worker}}(0.18; \text{net}) = \frac{93{,}306.82 - 96{,}000}{96{,}000} = -0.028 \;\; (\text{or } -2.8\%).$$

For reference, with the gross cost $C_w^{\text{gross}} = 106{,}000$,

$$\text{ROI}_{12m}^{\text{worker}}(0.18; \text{gross}) = \frac{93{,}306.82 - 106{,}000}{106{,}000} = -0.120 \;\; (\text{or } -12.0\%).$$

The *discounted* payback time τ solves $\sum_{t=1}^{\tau} \beta_w^t \delta_w S_t \geq C_w$. With the net cost, $\tau \approx 54$ weeks; with the gross cost, $\tau \approx 61$ weeks.

[34] For transparency, we also report the "gross" figures using $C_w^{\text{gross}} = 106{,}000$ INR.

B.2.3 Twenty-four months (104 weeks)

Under the same survival adjustment for weeks 36–104,

$$G_{1:104}(0.18) = \sum_{t=1}^{35} \beta_w^t \delta_w + \sum_{t=36}^{104} \beta_w^t S_{52} \delta_w = 166{,}216.21 \text{ INR}.$$

The corresponding ROIs are

$$\text{ROI}_{24m}^{\text{worker}}(0.18; \text{net}) = \frac{166{,}216.21 - 96{,}000}{96{,}000} = 0.731 \quad (\text{or } 73.1\%)$$

$$\text{ROI}_{24m}^{\text{worker}}(0.18; \text{gross}) = \frac{166{,}216.21 - 106{,}000}{106{,}000} = 0.568 \quad (\text{or } 56.8\%).$$

Interpretation. At a worker APR of 18% with survival-adjusted projections, the investment is close to break-even at twelve months on a discounted basis when the firm subsidy is netted from the worker's cost, with discounted payback arriving around week 54. Over twenty-four months, the worker's discounted ROI is large and positive (about 73% net of the subsidy, 57% on a gross-cost basis).

B.3 Worker Internal Rate of Return (IRR): New Adopters

Object and rationale. Following Bandiera et al. and Doornik et al., we compute the worker's internal rate of return (IRR) as the constant weekly rate r^* that equates the present value of expected incremental earnings from adoption to the upfront investment cost. Relative to ROI at a fixed discount rate, IRR is a discount-rate–free summary of the project's profitability from the worker's perspective.

Cash–flow mapping. Let δ_w denote incremental *weekly* earnings attributable to adoption (INR/week), C_w the upfront investment cost (INR), and S_t the probability the worker remains active at week t (survival). The weekly IRR r^* solves

$$C_w \;=\; \sum_{t=1}^{T} \frac{\delta_w \, S_t}{(1+r^*)^t}, \qquad T = 104 \text{ weeks (24 months)}. \tag{6}$$

We use the survival profile from the firm/worker ROI sections: $S_t = 1$ for $t = 1, \ldots, 35$ (realized) and $S_t = S_{52} = 0.891$ for $t = 36, \ldots, 104$ (projection).

Parameterization (new adopters).

$$\delta_w \;=\; 2{,}018 \text{ INR/week}, \qquad C_w \;=\; 106{,}000 \text{ INR}, \qquad S_{52} \;=\; 0.891.$$

Result. Solving (6) for r^* yields a weekly IRR of

$$r^* \;=\; 0.013565 \quad \Rightarrow \quad \text{IRR}_{\text{annual}} \;=\; (1+r^*)^{52} - 1 \;=\; 1.01509 \;\approx\; \mathbf{101.5\%} \textbf{ per year}.$$

Hence, the worker's asset purchase clears a very high internal rate of return on a 24-month, survival-adjusted cash–flow basis.

Comparison to Bandiera et al. (2017) and Van Doornik et al. (2024). Bandiera et al. (2017) estimate the internal rate of return to BRAC's asset-transfer program in rural Bangladesh, finding IRR between **16**% and **22**% annually, depending on the assumed opportunity cost of beneficiaries' time and on the persistence of benefits beyond the evaluation window. The horizon there spans multiple years (two-year program with follow-ups up to 5–7 years), and benefits are measured as sustained increases in earnings/consumption and asset accumulation.

Van Doornik et al. (2024) study access to motorcycle credit in Brazil and summarize the investment's profitability as an *annual real rate of return* of roughly **17**% over a **ten-year** horizon (earlier working-paper versions report ∼12%); their return consolidates persistent gains in formal employment and salaries relative to the financed asset's cost.

Methodologically, our IRR is the same object—solve $C = \sum_t \delta_w S_t/(1+r^*)^t$—but two features of this platform context push our estimate much higher: *(i) timing and intensity of cash flows*, with sizeable weekly earnings gains beginning immediately (no buildup/harvest cycle), and *(ii) horizon/compounding*, as we compute a weekly IRR over 24 months with survival-adjusted flows. With $\delta_w \approx$ INR 2,018/week against $C_w = 106{,}000$, net benefits arrive quickly, so the break-even rate r^* annualizes to ≈101%—an outcome consistent with very front-loaded payoffs. By contrast, Bandiera et al. and Van Doornik et al. report *annual* returns over much longer asset/program horizons with smaller per-period gains relative to cost, yielding IRRs near 16–22% and annual returns near 12–17%, respectively. In short, the first order importance of scooters in this work setting (directly complementing production process), differences in horizon and cash-flow timing (immediate and continual vs. seasonal/persistent), account for the gap; the underlying definition of IRR is the same across studies.

C Appendix C: Investment Modeled as Discrete Choice

C.1 Alternate Modeling Choice: Investment Decision as Discrete

Motivation. I present a discrete adoption model as a robustness check because the scooter purchase is lumpy decision. This formulation captures nonconvexities (eligibility thresholds, fixed fees) that plausibly matter at the decision margin. Importantly, for the *predictions we need for the incentive/choice experiment*—comparative statics of take-up when we vary lifetime price versus the day-of-purchase liquidity requirement—both the discrete and the continuous-k specifications imply the same qualitative mapping (with the discrete model imposing a threshold rule and the continuous model providing a smooth approximation to that threshold). We therefore keep the continuous-k benchmark as our main specification because it is more transparent for interpretation (closed-form comparative statics, units in INR per week and INR of down payment), and yields the same policy-relevant predictions for contract design. Given these equivalences and the empirical fit, it is reasonable to proceed with the continuous formulation in main text, while reporting the discrete model to demonstrate robustness.

C.1.1 Binary (Lumpy) Adoption

Environment and present value. Time is discrete (e.g., weekly). Adoption is binary, $k \in \{0,1\}$, and generates a constant incremental flow surplus $f(1) > 0$ each period relative to non-adoption $f(0) = 0$. The worker receives share $\alpha \in (0,1]$, discounts between adjacent periods at factor $\beta \in (0,1)$, and faces a per-period separation (exit) hazard $s \in [0,1)$. Hence the expected one-step continuation factor is $\beta(1-s)$. For notational economy define

$$\rho \;\equiv\; 1-\beta(1-s) \in (0,1],$$

so that the infinite-horizon present value of the constant flow is

$$F \;\equiv\; \sum_{t=0}^{\infty} \big[\beta(1-s)\big]^t f(1) \;=\; \frac{f(1)}{1-\beta(1-s)} \;=\; \frac{f(1)}{\rho}.$$

The worker's present-value gain from adoption is therefore $\alpha F = \alpha\, f(1)/\rho$.

Price, financing, and liquidity. The list price is $S > 0$. A proportional price rebate $\theta \in [0,1)$ scales the price to $(1-\theta)S$. The lender requires a fixed down-payment share $\delta \in (0,1)$; the remaining $(1-\delta)$ is financed. A *repayable low-down-payment relief* $g \geq 0$ reduces cash due today one-for-one but is rolled into principal. Let $\iota \geq 0$ denote the per-rupee *present-value* finance charge on principal.

Define the PV cost and the cash due today:

$$\underbrace{C(\theta,g)}_{\text{PV cost}} = (1-\theta)\Big[S + \iota(1-\delta)S\Big] + \iota g \;\equiv\; (1-\theta)\,\bar{C} \;+\; \iota g, \qquad \underbrace{D(\theta,g)}_{\text{cash today}} = \delta(1-\theta)S - g,$$

where $\bar{C} \equiv S + \iota(1-\delta)S$. Let $L \geq 0$ denote liquid wealth at $t = 0$.

Worker's problem (lumpy choice).

$$\max_{k\in\{0,1\}} \quad \alpha F\, k \;-\; C(\theta,g)\, k \quad \text{s.t.} \quad D(\theta,g)\, k \leq L.$$

Because k is binary, solve by comparing the two feasible alternatives.

Case 1: Liquidity does not bind

Assume $D(\theta, g) \leq L$ so $k = 0$ and $k = 1$ are both feasible.

Objective values: $V(0) = 0$ and $V(1) = \alpha F - C(\theta, g)$.

Result (adoption condition).

$$k^\star = 1 \iff \alpha F \geq C(\theta, g) \iff \alpha \geq \alpha^*(\theta, g) \equiv \frac{C(\theta, g)}{F} = \frac{(1-\theta)\bar{C} + \iota g}{F}.$$

Comparative statics (non-binding liquidity). Differentiate the *threshold* α^*:

$$\frac{\partial \alpha^*}{\partial \theta} = -\frac{\bar{C}}{F} < 0, \qquad \frac{\partial \alpha^*}{\partial g} = \frac{\iota}{F} > 0.$$

Thus, with liquidity slack: (i) a larger rebate θ lowers PV cost and *raises* adoption; (ii) larger repayable relief g *raises* PV cost via interest (ιg) and *reduces* adoption.

Price–share equivalence (interior). Set $g = 0$. Suppose liquidity is slack for all θ under consideration, e.g. $L \geq \delta S$ so $D(\theta, 0) \leq L$. The condition for adoption is

$$\alpha F \geq (1-\theta)\bar{C} \iff \frac{\alpha}{1-\theta} F \geq \bar{C}.$$

Define the transformed share $\alpha' \equiv \alpha/(1-\theta)$. Then

$$\mathbf{1}\{\alpha F \geq (1-\theta)\bar{C}\} = \mathbf{1}\{\alpha' F \geq \bar{C}\}.$$

Proposition. With $g = 0$ and non-binding liquidity, a proportional rebate θ is behaviorally equivalent to raising the worker's effective share from α to $\alpha' = \alpha/(1-\theta)$. □

Case 2: Liquidity binds

Assume $D(\theta, g) > L$. Then $k = 1$ is infeasible and

$$k^\star = 0 \quad \text{(regardless of NPV)}.$$

Restoring feasibility. Feasibility requires $D(\theta, g) \leq L$, i.e.

$$\delta(1-\theta)S - g \leq L.$$

Two equivalent ways to view the policy instruments:

(i) Minimal rebate to clear liquidity (given g):

$$\theta_{\text{liq}}(L, g) \equiv 1 - \frac{L+g}{\delta S}, \qquad \text{so } \theta \geq \theta_{\text{liq}}(L, g) \iff D(\theta, g) \leq L.$$

(ii) Minimal repayable relief to clear liquidity (given θ):

$$g_{\text{liq}}(L,\theta) \;\equiv\; \delta(1-\theta)S - L, \qquad \text{so } g \;\geq\; g_{\text{liq}}(L,\theta) \;\iff\; D(\theta,g) \leq L.$$

Once feasibility is restored, adoption also requires NPV non-negativity:

$$\alpha F \;\geq\; C(\theta,g) = (1-\theta)\bar{C} + \iota g.$$

Comparative statics (binding region). On the *liquidity* margin,

$$\frac{\partial D}{\partial \theta} = -\delta S \;<\; 0, \qquad \frac{\partial D}{\partial g} = -1 \;<\; 0.$$

Hence both θ and g relax the cash constraint; locally, g relaxes it one-for-one, while a rebate relaxes it by $\delta S\, d\theta$.

On the *NPV* margin (once feasible), the same signs as in Case 1 apply:

$$\frac{\partial C}{\partial \theta} = -\bar{C} \;<\; 0, \qquad \frac{\partial C}{\partial g} = \iota \;>\; 0.$$

Thus a higher θ helps both on liquidity and NPV; a higher g helps liquidity but hurts NPV.

Magnitude on the liquidity margin. Consider a small policy *budget* of Δ rupees.

Equal-budget rebate: a price discount of Δ implies $d\theta = \Delta/S$, reducing the down payment by

$$dD_{\text{rebate}} = \frac{\partial D}{\partial \theta}\, d\theta = (-\delta S)\,\frac{\Delta}{S} = -\delta\,\Delta.$$

Repayable relief: setting $dg = \Delta$ reduces the down payment by

$$dD_{\text{relief}} = \frac{\partial D}{\partial g}\, dg = -\,\Delta.$$

Therefore, per rupee of policy outlay, repayable relief moves the cash constraint by a factor of about $1/\delta$ more:

$$\left|\frac{dD_{\text{relief}}}{dD_{\text{rebate}}}\right| = \frac{1}{\delta} \;>\; 1.$$

If adoption at the extensive margin is governed by the density of liquidity $f_L(\cdot)$ at the threshold $L^* \equiv D(\theta,g)$, the induced change in the adoption rate satisfies, to first order,

$$dA_{\text{rebate}} \approx f_L(L^*)\,\delta\,\Delta, \qquad dA_{\text{relief}} \approx f_L(L^*)\,\Delta,$$

so $dA_{\text{relief}}/dA_{\text{rebate}} \approx 1/\delta$.