Debt Traps? Market Vendors and Moneylender Debt in India and the Philippines[†]

By Dean Karlan, Sendhil Mullainathan, and Benjamin N. Roth*

A debt trap occurs when someone takes on a high-interest-rate loan and is barely able to pay back the interest, and thus perpetually finds themselves in debt (often by refinancing). Studying such practices is important for understanding financial decision-making of households in dire circumstances, and also for setting appropriate consumer protection policies. We conduct a simple experiment in three sites in which we paid off high-interest moneylender debt of individuals. Most borrowers returned to debt within six weeks. One to two years after intervention, treatment individuals were borrowing at the same rate as control households. (JEL D14, D18, D91)

Small-scale entrepreneurs throughout the developing world often rely on moneylenders for working capital, borrowing on a daily or weekly basis at exorbitant interest rates. The ubiquity of this type of borrowing is a long-standing puzzle for development economists: why do these entrepreneurs not save a little bit and then borrow less, given their implicitly high risk-free rate of return to savings? We report on three experiments (one in India and two in the Philippines) in which we gave cash grants and brief financial training to market vendors with high-interest-rate debt. We then test how long before, and whether, individuals go back to using high-interest-rate debt again.

One striking pattern emerges: most vendors fall back into debt within six weeks, and two years later the likelihood and volume of borrowing at high interest rates is nearly identical for treatment and control vendors. The timing of reversion differs across the experiments. A brief, focused financial education training does little to mitigate this effect. While the average treatment impact on borrowing is small and short-lived, there are also important differences across experimental sites. In the first of the two Philippine experiments, about a third of the treated vendors stayed out of debt for at least six

months, although even there we find no difference between treatment and control debt levels after two years. Also notably, in the second of the 2 Philippine experiments, after 18 months the enterprise is generating considerably higher profits (albeit a puzzle thus still remains as to why individuals do not save, given the high returns to capital).

These results illuminate the difficulty that small-scale entrepreneurs face in escaping usage of high-interest-rate moneylender debt. Not only do borrowers seem unable to save their way out of debt, but also, even after receiving a windfall grant and being lifted from debt, they fall back into borrowing at some point in the following months.

This may happen for several reasons and no doubt no single one explains everyone's reversion. Thus disentangling and assessing the relative importance of each would require more observations and data than afforded by our sample, but we explore several possibilities and consider these fruitful areas for further study. For example, the vendors may be susceptible to unavoidable consumption and income shocks that force them to return to borrowing. Vendors may suffer from self-control problems such as present bias. Vendors may not have access to a reliable savings technology. Finally, vendors may not understand the long-term cost of repeated borrowing at high interest rates.

Repeated borrowing at high interest rates is not unique to the samples we study (see Banerjee 2004 for a survey of the literature documenting high-interest-rate borrowing across developing countries). More specifically, Banerjee and Duflo (2007) report borrowing rates from 11 percent (rural poor in East Timor) to 93 percent (in Pakistan) across 13 developing countries, oftentimes from moneylenders. Most relevant for the context of one of our sites, in the same state as our India experiment, the Yale University Economic Growth Center—Center for Microfinance Tamil Nadu Panel Survey reports 13 percent of urban and 39 percent of rural households borrowing at a monthly interest rate above 5 percent.[1] This is also a relevant issue in the United States. Skiba and Tobacman (2011) report high serial correlation in borrowing from payday lenders among low-income Americans. Bertrand and Morse (2011) report rates of using one payday loan to pay down another (and specifically find that informing borrowers of this tendency makes them less likely to immediately do so).

Our study is also related to the literature on unconditional cash grants for small-scale entrepreneurs (e.g., de Mel, McKenzie, and Woodruff 2008). Many of these studies estimate return to capital in excess of 5 percent per month. Those studies are on a more representative sample frame than we study here, as they did not specifically target individuals borrowing from high-interest moneylenders, and rather than focus on profits, we focus on debt reversion. Although a different context, Hankins, Hoekstra, and Skiba (2011) analyze the impact of a cash windfall from winning the lottery and find that winners who received $50,000 to $150,000 filed for bankruptcy at the same rate as smaller winners.

I. Experimental Design and Data

We conducted three experiments: Chennai, India in 2007 (1,000 market vendors), Cagayan de Oro, Philippines in 2007 (250 market vendors), and Cagayan de Oro,

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[1] https://egcenter.economics.yale.edu/egc-cmf-tamil-nadu-panel-survey-overview

Philippines in 2010 (701 market vendors, from different markets than in 2007). Each experiment took place in urban market settings. Going stall to stall, we identified individuals eligible for the study based on the following criteria: (i) they were the decision-maker of the business; (ii) they borrowed consistently from a professional moneylender (defined as interest rate at or above 5 percent per month, although most debt was considerably more expensive) for the past five years; and (iii) they had an outstanding balance of US$100 or below in the Philippines or US$50 or below in India with moneylenders.[2] Borrowers in the Indian sample paid an average daily interest rate of 4.7 percent, while borrowers in both Philippines samples paid an average monthly interest rate of 13 percent.

Both 2007 experiments included the same four equal-sized treatment arms: debt payoff; financial education; debt payoff and financial education; and control. We implemented a brief financial training in small groups of roughly 25 vendors by a professional survey team. In the Philippines, 105 out of the 125 vendors invited to the training attended[3] and only nominal compensation was given for attendance.[4] In India, 434 out of 500 individuals attended the financial training. The training emphasized two messages: (i) that lending at high rates from moneylenders led to a large loss of money and thus consumption every year compared with other alternatives, and (ii) that savings and consumption discipline could help vendors avoid having to take out loans at all. The training included interactive activities, discussion, and lecture. The online Appendix includes a summary of the financial planning curriculum.

In the 2010 Philippines experiment, participants were randomized into one of four groups: debt payoff; savings account; debt payoff and savings account; and control. All three treatment groups also received a 5–10 minute financial education lesson (i.e., slightly briefer than the 2007 financial education, but no longer separately tested as its own treatment arm). Because of problems with insufficient compliance with account opening requirements (specifically, providing necessary documentation and identification), only 10 savings accounts were opened, and thus there is nothing to analyze with respect to the savings account treatment arms. For analysis, we collapse this wave to two groups: debt payoff and financial training (with and without the offered savings account) and control.

In all three sites, the financial training was conducted prior to the announcement of the debt payoff. Several days after the financial training, surveyors went to those selected for debt payoff and informed them they had won a prize through the lottery. In the Philippines, as debt collectors generally collect repayments daily in this area, surveyors waited until the collectors arrived and paid off the debt in view of the vendors. In India, individuals were instructed to come to a central location to pick up money if they were in a debt payoff treatment group. The amount paid was equal to the amount the debt collector said was owed, up to 5,000 pesos (US$100) in the Philippines or 3,000 rupees (US$50) in India. Across debt payoff treatments,

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[2] We restricted the sample to borrowers with less than this amount of debt so that we could pay off their entire debt, thus making analysis of the extensive margin more likely relevant.

[3] Many of the 20 vendors who declined to come to the training said that they could not afford to take time away from their stalls. Also, several vendors were not present at the time when invitations were given.

[4] Vendors were given transportation, light refreshments, a coffee mug, and a certificate of completion in the Philippines, and in India they were given a steel money box with locks and engraving that said "IFMR, Financial training class, July 2007" (in Tamil) with the intent of reminding them in the future about the lessons learned in the class and Rs 100 to compensate generously for transport costs.
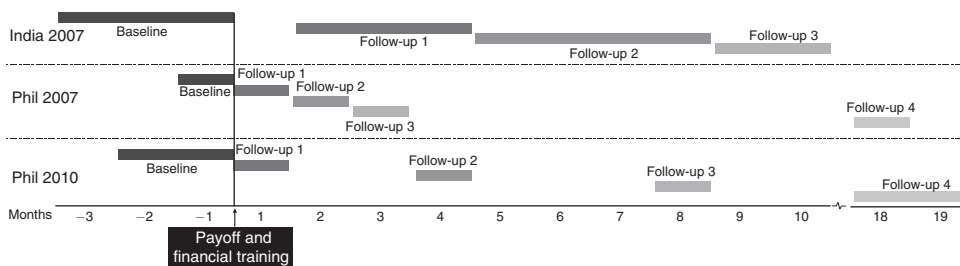
FIGURE 1. SURVEY AND INTERVENTION TIMELINE

average debt relief was 2,172 pesos (US$44) in the Philippines 2007 site, 3,234 pesos (US$70) in the Philippines 2010 site, and Rs 751 (US$17) in the India site.[5]

The 2007 Philippines and India experiments did not employ stratification. The 2010 Philippines experiment stratified on quintile of preexisting debt level. Baseline surveys measured (i) the history of their savings and debt over the past few years; (ii) basic cognitive skills and educational level; (iii) mental health that could be linked to ability to fulfill plans; (iv) business information, e.g., assets and revenues; and (v) demographic data. Online Appendix Table 1 presents baseline statistics and verification of orthogonality of assignments to treatment.

We conducted four follow-up surveys, starting 4–6 weeks after the debt payoff, and ending at the latest 2 years after the debt payoff. See Figure 1 for a summary of the timing of each experiment's data. Follow-up surveys measured (i) whether the vendors had moneylender and other types of debt; (ii) what changes have occurred in the business (i.e., if the business has expanded significantly or not); (iii) any shocks the household has experienced, and how they coped; (iv) some components of consumption; (v) savings.

In the 2007 India experiment, 881 of 1,000 completed all 4 follow-up surveys. In the 2007 Philippines experiment, 206 of 250 completed all 4 follow-up surveys. In the 2010 Philippines experiment, 569 of 701 completed all 4 follow-up surveys. Online Appendix Table 1 presents orthogonality results for comparison of attrition rates. Out of 13 comparisons across the 3 sites, we reject equality of survey attrition rates in one test, the Philippines 2010 site (76 percent completed all surveys in control whereas 84 percent completed all surveys in treatment, p-value $= 0.022$).

II. Results

Tables 1–3 present the impact of our treatments in India 2007, Philippines 2007, and Philippines 2010, respectively.[6] Across all columns in each table, the specification is

$$y_{it} = \alpha + \sum_{j,t} \beta_{jt} Treatment_{ji} \times wave_t + \gamma_1 y_{i0} + \gamma_2 v_i + wave_t + \epsilon_{it},$$

[5]Exchange rates are as of January 1, 2007 for Philippines 2007, January 1, 2010 for Philippines 2010, and February 1, 2007 for India 2007.

[6]Online Appendix Tables A2–A4 present robustness checks with respect to outliers for key outcomes (debt level, household expenditures, and take-home enterprise profit) by winsorizing the top 1 percent and 5 percent of each outcome.

TABLE 1—PRIMARY TREATMENT EFFECT ESTIMATES, INDIA 2007

	Any moneylender debt (1)	Amount moneylender debt (USD) (2)	Coped via savings (3)	Coped via borrowing (4)	Coped via cutting consumption (5)	Household monthly total expenditures (USD) (6)	Any savings (7)	Take-home profit typical day (USD) (8)
Only payoff in 1st follow-up (2–4 months)	−0.17 (0.04)	−8.35 (1.99)	0.12 (0.05)	−0.06 (0.06)	−0.06 (0.03)	26.51 (21.44)		−0.04 (0.15)
Only payoff in 2nd follow-up (5–8 months)	−0.06 (0.04)	0.31 (2.09)	0.08 (0.05)	−0.08 (0.05)	0.00 (0.03)	14.50 (18.49)		−0.18 (0.16)
Only payoff in 3rd follow-up (9–10 months)	0.03 (0.04)	−0.51 (2.14)	0.06 (0.04)	−0.02 (0.06)	0.00 (0.02)	8.62 (19.10)	0.00 (0.03)	0.03 (0.17)
Only training in 1st follow-up (2–4 months)	−0.02 (0.03)	−1.57 (2.02)	−0.04 (0.05)	0.05 (0.05)	−0.01 (0.04)	−14.37 (19.60)		−0.02 (0.14)
Only training in 2nd follow-up (5–8 months)	0.03 (0.04)	0.05 (1.88)	0.05 (0.05)	−0.07 (0.05)	0.01 (0.03)	−33.95 (15.65)		−0.15 (0.16)
Only training in 3rd follow-up (9–10 months)	0.02 (0.04)	−1.77 (2.08)	0.02 (0.04)	−0.04 (0.06)	0.01 (0.02)	−18.68 (16.26)	0.06 (0.02)	−0.33 (0.16)
Payoff + training in 1st follow-up (2–4 months)	−0.20 (0.04)	−6.46 (2.19)	0.10 (0.05)	−0.10 (0.06)	0.01 (0.04)	2.43 (19.85)		0.32 (0.15)
Payoff + training in 2nd follow-up (5–8 months)	−0.09 (0.04)	−0.55 (2.10)	0.04 (0.05)	−0.04 (0.05)	−0.01 (0.03)	−16.70 (15.95)		0.16 (0.16)
Payoff + training in 3rd follow-up (9–10 months)	−0.05 (0.04)	−1.40 (2.12)	0.02 (0.04)	−0.01 (0.06)	−0.01 (0.02)	−1.21 (17.49)	0.06 (0.02)	−0.06 (0.17)
Observations	2,643	2,632	1,742	1,742	1,742	2,418	881	2,601
Observations, F1	881	870	593	593	593	880	0	869
Observations, F2	881	881	529	529	529	873	0	877
Observations, F3	881	881	620	620	620	665	881	855
Control mean dependent variable	0.69	24.83	0.16	0.63	0.07	238.01	0.92	2.87
Control SD dependent variable	0.46	25.57	0.37	0.48	0.25	181.84	0.28	1.76
Baseline outcome measure included?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value for test: payoff F1 = payoff + training F1	0.49	0.36	0.65	0.51	0.02	0.31		0.03
p-value for test: payoff F2 = payoff + training F2	0.53	0.70	0.49	0.42	0.64	0.05		0.03
p-value for test: payoff F3 = payoff + training F3	0.03	0.68	0.36	0.87	0.62	0.61	0.01	0.58
p-value for test: training F1 = payoff + training F1	0.00	0.02	0.01	0.01	0.48	0.45		0.03
p-value for test: training F2 = payoff + training F2	0.00	0.77	0.83	0.51	0.56	0.18		0.04
p-value for test: training F3 = payoff + training F3	0.07	0.86	0.91	0.59	0.28	0.29	0.71	0.07
p-value for test: payoff F1 = payoff F3	0.00	0.00	0.27	0.54	0.08	0.47		0.70
p-value for test: training F1 = training F3	0.44	0.93	0.28	0.23	0.52	0.86		0.09
p-value for test: payoff + training F1 = payoff + training F3	0.00	0.06	0.17	0.21	0.65	0.88		0.06

Notes: (i) Robust standard errors clustered at the vendor level in parentheses. (ii) All regressions also include the baseline value of the outcome variable (coded as zero, with an indicator for observations missing at baseline, whenever unavailable) and a survey wave fixed effect. (iii) Column 1 is whether they borrowed from a moneylender for working capital, column 2 is the amount they borrowed from a moneylender for working capital in USD, column 3 is whether they coped with a household income shock by using savings (missing if no household income shock), column 4 is whether they coped with a household income shock by borrowing (missing if no household income shock), column 5 is whether they coped with a household income shock by cutting consumption (missing if no household income shock), column 6 is total monthly household expenditures in USD, column 7 is whether they currently have any form of savings, and column 8 is their approximate take-home business profit on a typical day in USD. (iv) For columns 3–5, at baseline, of those that experienced an income shock, 58.1 percent experienced one from family sickness/accident/death, 23.39 percent from slow business, and 16.1 percent from unemployment.

Table 2—Primary Treatment Effect Estimates, Philippines 2007

	Any moneylender debt	Amount moneylender debt (USD)	Coped via savings	Coped via borrowing	Coped via cutting consumption	Household monthly total expenditures (USD)	Any savings	Take-home profit typical day (USD)	Total working capital (USD)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Only payoff in 1st follow-up (1 month)	−0.37 (0.08)	−46.73 (14.98)	0.21 (0.13)	−0.45 (0.12)	−0.01 (0.14)	−16.79 (17.78)			
Only payoff in 2nd follow-up (2 months)	−0.31 (0.08)	−64.56 (12.83)	0.08 (0.13)	0.21 (0.17)	−0.26 (0.18)	−15.63 (20.79)			
Only payoff in 3rd follow-up (3 months)	−0.28 (0.09)	−45.64 (17.62)	0.04 (0.17)	−0.28 (0.18)	0.02 (0.18)	−0.15 (22.92)			
Only payoff in 4th follow-up (18 months)	0.11 (0.09)	9.33 (14.27)	−0.06 (0.14)	0.17 (0.13)	−0.08 (0.11)	−10.59 (47.49)	0.09 (0.09)	2.79 (2.19)	0.09 (33.87)
Only training in 1st follow-up (1 month)	0.00 (0.07)	−2.48 (14.55)	0.37 (0.15)	−0.27 (0.15)	0.03 (0.15)	−32.12 (17.84)			
Only training in 2nd follow-up (2 months)	0.00 (0.07)	−1.89 (16.77)	0.17 (0.13)	0.33 (0.16)	−0.20 (0.17)	−46.14 (22.53)			
Only training in 3rd follow-up (3 months)	0.01 (0.08)	−5.32 (17.88)	0.03 (0.16)	−0.16 (0.19)	0.02 (0.19)	−33.23 (24.43)			
Only training in 4th follow-up (18 months)	−0.02 (0.09)	−12.91 (13.35)	−0.14 (0.14)	0.00 (0.14)	0.14 (0.14)	15.12 (56.53)	0.04 (0.09)	0.07 (2.41)	83.73 (41.42)
Payoff + training in 1st follow-up (1 month)	−0.47 (0.08)	−64.22 (12.59)	0.18 (0.14)	−0.33 (0.14)	0.12 (0.14)	−1.68 (20.91)			
Payoff + training in 2nd follow-up (2 months)	−0.49 (0.08)	−72.39 (13.80)	0.32 (0.18)	0.16 (0.20)	−0.12 (0.19)	−24.62 (24.66)			
Payoff + training in 3rd follow-up (3 months)	−0.44 (0.09)	−65.63 (14.62)	0.19 (0.16)	−0.44 (0.16)	−0.04 (0.18)	−19.71 (24.83)			
Payoff + training in 4th follow-up (18 months)	−0.05 (0.08)	−5.22 (14.31)	0.01 (0.14)	−0.24 (0.13)	0.16 (0.14)	−29.61 (50.01)	0.11 (0.09)	5.35 (4.83)	59.99 (36.13)

(*Continued*)

where y_{it} refers to an outcome for respondent i in wave t, *Treatment*$_{ji}$ is an indicator for whether respondent i was assigned treatment j, *wave*$_t$ is a dummy for wave t, y_{i0} is the baseline value of the outcome variable for respondent i, and v_i is a vector of dummy variables for quintile of preexisting debt at baseline for respondent i (used in Philippines 2010 only). Throughout, standard errors are clustered at the respondent level.

Columns 1 and 2 of each table report treatment impact on the primary outcome variable: moneylender borrowing (column 1 is binary for "has any outstanding moneylender debt," and column 2 is the amount of outstanding moneylender debt). In India 2007 (Table 1), borrowers granted debt relief only were 17pp (SE = 4pp) less likely to borrow from a moneylender after the first follow-up, and borrowed only US$8 (SE = $2) less on average, relative to a control mean debt of US$25. Financial training had no direct impact on borrowing (treatment effect is a reduction in borrowing of 2pp, SE = 3pp). The combined treatments of debt payoff and financial training produced results similar to the debt payoff only treatment arm: a reduction in borrowing of 20pp (SE = 4pp) and a reduction of US$6 (SE = $2). By the second follow-up (5–8 months), however, treatment effects had already mostly

TABLE 2—PRIMARY TREATMENT EFFECT ESTIMATES, PHILIPPINES 2007 (*Continued*)

	Any moneylender debt (1)	Amount moneylender debt (USD) (2)	Coped via savings (3)	Coped via borrowing (4)	Coped via cutting consumption (5)	Household monthly total expenditures (USD) (6)	Any savings (7)	Take-home profit typical day (USD) (8)	Total working capital (USD) (9)
Observations	824	822	298	298	298	823	206	194	155
Observations, F1	206	206	82	82	82	206			
Observations, F2	206	206	53	53	53	205			
Observations, F3	206	206	58	58	58	206			
Observations, F4	206	204	105	105	105	206	206	194	155
Control mean dependent variable	0.69	81.90	0.26	0.53	0.48	271.57	0.57	7.52	87.81
Control SD dependent variable	0.46	89.63	0.44	0.50	0.50	194.98	0.50	7.62	121.80
Baseline outcome measure included?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value for test: payoff F1 = payoff + training F1	0.29	0.20	0.86	0.33	0.41	0.39			
p-value for test: payoff F2 = payoff + training F2	0.06	0.46	0.23	0.81	0.48	0.65			
p-value for test: payoff F3 = payoff + training F3	0.10	0.18	0.39	0.33	0.73	0.43			
p-value for test: payoff F4 = payoff + training F4	0.08	0.24	0.64	0.00	0.06	0.69	0.76	0.53	0.14
p-value for test: training F1 = payoff + training F1	0.00	0.00	0.30	0.73	0.56	0.10			
p-value for test: training F2 = payoff + training F2	0.00	0.00	0.46	0.41	0.67	0.32			
p-value for test: training F3 = payoff + training F3	0.00	0.00	0.36	0.12	0.72	0.61			
p-value for test: training F4 = payoff + training F4	0.69	0.50	0.29	0.10	0.89	0.43	0.44	0.25	0.61
p-value for test: payoff F1 = payoff F4	0.00	0.01	0.11	0.00	0.71	0.90			
p-value for test: training F1 = training F4	0.85	0.59	0.01	0.16	0.57	0.42			
p-value for test: payoff + training F1 = payoff + training F4	0.00	0.00	0.36	0.60	0.81	0.57			

Notes: (i) Robust standard errors clustered at the vendor level in parentheses. (ii) All regressions also include the baseline value of the outcome variable (coded as zero, with an indicator for observations missing at baseline, whenever unavailable) and a survey wave fixed effect. (iii) Column 1 is whether they borrowed from a moneylender for working capital, column 2 is the amount they borrowed from a moneylender for working capital in USD, column 3 is whether they coped with a household income shock by using savings (missing if no household income shock), column 4 is whether they coped with a household income shock by borrowing (missing if no household income shock), column 5 is whether they coped with a household income shock by cutting consumption (missing if no household income shock), column 6 is total monthly household expenditures in USD, column 7 is whether they currently have any form of savings, column 8 is their approximate take-home business profit on a typical day in USD, and column 9 is worth of current business assets plus amount spent on an average restocking trip minus current or daily loans owed. (iv) For columns 3–5, at baseline, of those that experienced an income shock, 60.4 percent experienced one from slow business, 25.5 percent from family sickness/accident/death, and 17.9 percent from unemployment.

dissipated: −6pp (SE = 4pp) for debt payoff only; and −9pp (SE = 4pp) for debt payoff and financial training. By the third follow-up (9–10 months), all results were no longer statistically significant: +3pp (SE = 4pp) for debt payoff only and −5pp (SE = 4pp) for the debt payoff and financial training. Effects on amount borrowed similarly dissipated (column 2).

Effects were somewhat larger and more persistent in the Philippines 2007 site (Table 2). Borrowers offered debt relief were 37pp (SE = 8pp) less likely to borrow

TABLE 3—PRIMARY TREATMENT EFFECT ESTIMATES, PHILIPPINES 2010

	Any moneylender debt (1)	Amount moneylender debt (USD) (2)	Coped via savings (3)	Coped via borrowing (4)	Coped via cutting consumption (5)	Household monthly total expenditures (USD) (6)	Any savings (7)	Take-home profit typical day (USD) (8)	Total working capital (USD) (9)
Payoff + training in 1st follow-up (1 month)	−0.09 (0.04)	−32.53 (10.09)	−0.07 (0.08)	0.16 (0.09)	−0.08 (0.09)	2.39 (8.06)		0.65 (0.41)	38.31 (17.23)
Payoff + training in 2nd follow-up (4 months)	−0.05 (0.04)	−14.31 (10.19)	−0.01 (0.06)	-0.06 (0.09)	0.03 (0.08)	−6.73 (9.75)		1.00 (0.53)	39.94 (17.55)
Payoff + training in 3rd follow-up (8 months)	−0.01 (0.04)	−13.01 (11.29)	−0.06 (0.07)	0.13 (0.09)	−0.09 (0.09)	−2.39 (9.21)		1.58 (0.63)	37.31 (19.85)
Payoff + training in 4th follow-up (18–19 months)	−0.04 (0.04)	−12.04 (18.62)	0.01 (0.04)	0.19 (0.06)	0.00 (0.05)	−3.40 (10.67)	0.04 (0.04)	1.15 (0.59)	81.90 (28.61)
Observations	2,272	2,158	670	670	670	2,073	569	2,133	2,054
Observations, F1	569	569	132	132	132	494		554	552
Observations, F2	566	565	145	145	145	496		541	538
Observations, F3	568	568	131	131	131	517		543	541
Observations, F4	569	456	262	262	262	566	569	495	423
Control mean dependent variable	0.70	125.77	0.16	0.37	0.27	220.48	0.47	6.89	10.35
Control SD dependent variable	0.46	148.71	0.37	0.48	0.45	116.53	0.50	4.10	227.51
Baseline outcome measure included?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value for test: payoff + training F1 = payoff + training F4	0.31	0.25	0.39	0.74	0.40	0.58		0.36	0.12

Notes: (i) Robust standard errors clustered at the vendor level in parentheses. (ii) All regressions also include the baseline value of the outcome variable (coded as zero, with an indicator for observations missing at baseline, whenever unavailable), a survey wave fixed effect, and a vector of dummy variables for quintile of preexisting debt at baseline. (iii) Column 1 is whether they borrowed from a moneylender for working capital, column 2 is the amount they borrowed from a moneylender for working capital in USD, column 3 is whether they coped with a household income shock by using savings (missing if no household income shock), column 4 is whether they coped with a household income shock by borrowing (missing if no household income shock), column 5 is whether they coped with a household income shock by cutting consumption (missing if no household income shock), column 6 is total monthly household expenditures in USD, column 7 is whether they currently have any form of savings, column 8 is their approximate take-home business profit on a typical day in USD, and column 9 is worth of current business assets plus amount spent on an average restocking trip minus current or daily loans owed. (iv) For columns 3–5, at baseline, of those that experienced an income shock, 59.2 percent experienced one from family sickness/accident/death, 6.9 percent from slow business, and 6.3 percent from unemployment.

from a moneylender at the first follow-up and by the third follow-up they remain 28pp (SE = 9pp) less likely to borrow. They borrowed US$47 (SE = $15) less at first follow-up and by third follow-up borrowed US$46 (SE = $18) less, relative to a control mean borrowing of US$82. At the final measurement, treatment effects have entirely dissipated, but we note that we focused the final measurement on just moneylender loans used for working capital. Thus, for this site, we unfortunately cannot distinguish whether the treatment effect truly dissipated or whether it was merely concentrated in the portion of debt not captured in our final survey wave.[7]

[7] While in follow-ups 1–3, the debt module covers all outstanding loans, the debt module in follow-up 4 asks about all outstanding loans but only if the respondent has taken loans as a source of funding for working capital at least 1 day of the past 30 days. The change was made for clarity in response to surveyor feedback. While 189 respondents had at least 1 outstanding loan, only 95 respondents had taken out loans as a source of funding for working capital at least 1 day of the past 30 days.

Financial training had no impact on its own (first follow-up: 0pp, SE = 7pp; second follow-up: 0pp, SE = 7pp; third follow-up: 1pp, SE = 8pp; fourth follow-up: −2pp, SE = 9pp). The treatment effects for the combined debt payoff and financial training were larger than the debt payoff treatments (e.g., 47pp reduction versus 37pp reduction for the first follow-up), however the *t*-test of equality of treatment effects yields *p*-values of 0.29, 0.06, 0.10, and 0.08 across the four waves. Hence there is some suggestive evidence that the financial training in the Philippines slowed the reversion rate back into moneylender debt, but by the final follow-up at 18 months the effect of the combined treatment was also null (−5pp, SE = 8pp).

The Philippines 2010 (Table 3) results are more similar to the India 2007 site: borrowers granted debt relief were only about 9pp less likely to borrow from a moneylender after the first follow-up (SE = 4pp), and borrowed only US$33 less on average, relative to a control mean debt of US$126 (SE = $10). The second (4 months), third (8 months), and fourth (18–19 months) follow-ups all yielded null results: −5pp (SE = 4pp); −1pp (SE = 4pp); −4pp (SE = 4pp) for the likelihood of having moneylender debt.

Columns 3–5 report impacts on household coping mechanisms in response to income shocks. In India and the Philippines 2010 all impacts were small and short lived, while effects in the Philippines 2007 are larger, though still modest.[8]

In India, at first follow-up vendors who got debt relief were 12pp (SE = 5pp) more likely to use savings to cope with low household income, but by second follow-up that effect is not statistically significant. Similarly, they are 6pp (SE = 3pp) less likely to respond to low household income by reducing consumption, but the effect vanishes by second follow-up. Financial training seems to have no impact.

In Philippines 2007, at first follow-up, households offered debt relief only are 45pp (SE = 12pp) less likely to respond to low income by borrowing and those offered debt relief and financial training are 33pp (SE = 14pp) less likely. This effect dissipates for the debt relief only treatment arm, but mostly does not for the combined debt relief and financial training treatment arm: second follow-up treatment effect is +16pp (SE = 20pp); third follow-up is −44pp (SE = 16pp); fourth follow-up is −24pp (SE = 13pp).

Effects in the Philippines 2010 are similar to India (again). If anything, Table 3 column 4 shows a 16pp (SE = 9pp) increase in the likelihood that households respond to an income shock by borrowing money. While the size of this effect fluctuates across waves, in the fourth follow-up households granted debt relief are 20pp (SE = 6pp) more likely to respond to an income shock by borrowing money. This may be spurious, however, since the effect is not present in the second or third follow-ups.

Column 6 reports impacts on household expenditures.[9] Across the board, debt relief had little to no impact on expenditure. Column 7 reports impacts on vendors' savings. In all sites, vendors who received debt relief are no more likely to have any form of savings at the final follow-up (though in India there is a small increase in savings for those who received financial training).

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[8] Across columns 3–5 in Tables 1–3 we treat an observation as missing if they do not suffer a household shock in the relevant wave. In online Appendix Tables A5–A7 we present the same regressions where instead households who do not suffer a shock are coded as not responding to a shock through the relevant method.
[9] Online Appendix Tables A8–A10 separately examine individual components of household expenditures.

Finally, column 8 reports impact on profits. In India effects are small and short-lived. By the second follow-up, we cannot reject that profits of the treatment and control groups are the same. In Philippines 2007 the point estimate on profits is large, though imprecisely estimated. In Philippines 2010 there seems to have been a sustained impact on profits. At fourth follow-up, vendors offered debt relief earned an extra US$1.15 (SE = $0.59) of profits on a typical day, relative to a control mean of US$6.90.

Taken together these results suggest that, for the most part, the impact of debt relief was modest and fleeting on debt levels. In India and the Philippines 2010, effects on borrowing were small at first follow-up and dissipated by the second follow-up. In Philippines 2007 the impact on borrowing was somewhat larger and more persistent, though more than half of the vendors offered debt relief were already back in debt by first follow-up.

III. Interpretation and Discussion

It is puzzling how and why small-scale entrepreneurs repeatedly borrow for working capital at high moneylender interest rates. It implies stark savings constraints (why did individuals not save their way out of debt, given the high return on savings), or high returns to capital (but then again, why not save and expand even more), or other behavioral constraints. With many possible explanations for this phenomenon, distinguishing between them requires more power than our sample affords. But with an eye toward guiding future research on this subject we discuss a few explanations below and put forward some suggestive evidence.

A first possibility is that entrepreneurs have such high returns to investment that continuous borrowing at high interest rates is justified from the perspective of long-term profit maximization. The results from the two Philippines experiments but not the India experiment provide some support for this hypothesis—at final measurement, profits in the 2007 treatment group were $5.35/day (SE = $4.83) higher than in the control group and profits in the 2010 treatment group were $1.15/day (SE = $0.59) higher than in the control group. Thus the 2007 debt payoff led to a very large but noisily estimated treatment effect on enterprise income, and the 2010 led to a fairly large ($1.15 = 1.7% daily return) and statistically significant result. This hypothesis is bolstered by the fact that the 2010 treatment group invested an additional $81.90 in working capital relative to the control group at final follow-up (SE = $28.61). However, even if vendors found it profitable to invest their grants in their business and return to debt, a puzzle still remains: as vendors reach diminishing returns to investment, they should no longer borrow to expand their investment, and standard theory then predicts a corresponding reduction in borrowing as vendors slowly save their way out of debt (Ananth, Karlan, and Mullainathan 2007)

Table 4 examines several other potential explanations by estimating heterogeneous treatment effects using the following specification:

$$y_{it} = \alpha + \sum_j \beta_j Treatment_{ji} + \sum_k \theta_k H_{ki} + \sum_{j,k} \phi_{jk} H_{ki} \times Treatment_{ji}$$

$$+ \gamma_1 y_{i0} + \gamma_2 v_i + \epsilon_{it},$$

TABLE 4—HETEROGENEOUS TREATMENT EFFECTS, FOLLOW-UP 1

	India 2007		Philippines 2007		Philippines 2010	
	Any moneylender debt (1)	Amount moneylender debt (USD) (2)	Any moneylender debt (3)	Amount moneylender debt (USD) (4)	Any moneylender debt (5)	Amount moneylender debt (USD) (6)
Only payoff	−0.29	−18.47	−0.72	−91.41		
	(0.16)	(9.42)	(0.17)	(34.97)		
Only training	−0.24	−21.53	−0.30	−45.03		
	(0.15)	(11.82)	(0.13)	(33.60)		
Payoff and training treatment	−0.68	−33.10	−0.60	−65.50	−0.09	−10.82
	(0.16)	(10.03)	(0.17)	(34.54)	(0.07)	(23.24)
Time inconsistent preferences	−0.03	−0.85	−0.15	−8.65	−0.02	9.91
	(0.06)	(3.27)	(0.17)	(32.33)	(0.08)	(24.93)
Has savings at a bank	0.00	1.04	0.00	22.10	0.02	11.28
	(0.09)	(4.69)	(0.12)	(23.48)	(0.06)	(18.68)
Financial literacy index (7 vars)	−0.03	−0.72	−0.03	1.31	0.00	−10.89
	(0.02)	(1.56)	(0.04)	(8.66)	(0.03)	(10.50)
Math skills index (4 vars)	0.01	1.22	0.01	−1.19	0.01	0.28
	(0.02)	(1.64)	(0.04)	(7.23)	(0.03)	(7.87)
Predicted probability of household income shock	−0.12	−22.09	−0.36	−32.12	−0.15	−39.15
	(0.17)	(11.19)	(0.20)	(49.05)	(0.23)	(71.12)
Only payoff × time inconsistent preferences	0.04	2.95	0.61	33.45		
	(0.08)	(4.37)	(0.22)	(37.95)		
Only payoff × has savings at a bank	−0.10	−1.43	−0.08	−45.68		
	(0.13)	(5.79)	(0.20)	(34.90)		
Only payoff × financial literacy index	−0.02	−0.39	0.04	10.76		
	(0.04)	(2.09)	(0.08)	(13.66)		
Only payoff × math skills index	−0.04	−2.74	−0.03	22.69		
	(0.03)	(1.94)	(0.10)	(22.18)		
Only payoff × predicted probability of household income shock	0.18	14.36	0.63	111.66		
	(0.23)	(13.35)	(0.34)	(78.05)		
Only training × time inconsistent preferences	−0.08	−3.55	0.29	−13.57		
	(0.08)	(4.17)	(0.19)	(40.72)		
Only training × has savings at a bank	−0.02	−1.62	−0.14	−37.52		
	(0.14)	(6.29)	(0.18)	(37.00)		
Only training × financial literacy index	0.01	2.60	0.05	−19.47		
	(0.03)	(2.23)	(0.06)	(15.39)		
Only training × math skills index	−0.01	−1.76	−0.09	−18.74		
	(0.02)	(1.92)	(0.10)	(11.69)		
Only training × predicted probability of household income shock	0.37	31.54	0.57	108.24		
	(0.22)	(16.94)	(0.25)	(57.12)		
Payoff + training × time inconsistent preferences	0.12	2.60	0.28	37.18	0.07	−10.18
	(0.08)	(4.54)	(0.28)	(44.87)	(0.10)	(28.47)
Payoff + training × has savings at a bank	−0.02	0.55	0.05	−10.08	−0.03	1.59
	(0.13)	(6.98)	(0.21)	(29.17)	(0.08)	(21.78)
Payoff + training × financial literacy index	0.01	0.15	0.00	−1.65	−0.01	7.28
	(0.04)	(2.32)	(0.09)	(10.28)	(0.03)	(11.88)
Payoff + training × math skills index	−0.03	−1.51	−0.07	−5.87	−0.06	−8.96
	(0.04)	(2.65)	(0.12)	(12.21)	(0.04)	(9.64)
Payoff + training × predicted probability of household income shock	0.67	38.57	0.11	−6.49	−0.04	−89.64
	(0.23)	(13.96)	(0.34)	(55.58)	(0.27)	(78.18)
Observations	881	870	206	206	564	564
Control mean dependent var	0.76	28.91	0.87	94.55	0.77	125.71
Control SD dependent var	0.43	27	0.34	76.98	0.42	130.57
Baseline outcome measure included?	Yes	Yes	Yes	Yes	Yes	Yes

Notes: (i) Robust standard errors in parentheses. (ii) All regressions also include the baseline value of the outcome variable (coded as zero, with an indicator for observations missing at baseline, whenever unavailable) and columns 5 and 6 include a vector of dummy variables for quintile of preexisting debt at baseline. (iii) Columns 1, 3, and 5 are whether they borrowed from a moneylender for working capital; columns 2, 4, and 6 are the amount they borrowed from a moneylender for working capital in USD. See narrative in paper for details on the covariates.

where H_{ki} is the kth characteristic of interest for vendor i, and all other variables are defined as above. Robust standard errors are clustered at the vendor level. We present the estimates for the first follow-up in Table 4, and online Appendix Tables A11, A12, and A13 present the same for the second, third, and fourth follow-ups.

The second possibility we consider is that vendors suffer from time inconsistent preferences, such as present bias, or temptation challenges (Strotz 1955; Shefrin and Thaler 1992; Laibson 1997; O'Donoghue and Rabin 1999; Fudenberg and Levine 2006). In Table 4 we explore heterogeneous treatment effects of debt relief and financial training with respect to the level of present bias displayed by vendors at baseline. Vendors were asked to make hypothetical trade-offs between differing amounts of money delivered at different times and were defined to be present biased if they exhibited lower patience for trade-offs that included immediate payoffs.[10] We find little evidence of heterogeneity with respect to time inconsistency, although the confidence intervals on the heterogeneity are wide and often include the point estimate for the main treatment effect.

A third possibility is that borrowers lack a savings technology that allows them to accumulate capital and stay out of debt. Without a secure place to store capital, saving may be constrained by family pressures (e.g., Ashraf, Karlan, and Yin 2010; Schaner 2015; Jakiela and Ozier 2016), self-control (Ashraf, Karlan, and Yin 2006b; Dupas and Robinson 2013b), or mere safety and low transaction costs (Dupas and Robinson 2013a; Prina 2015; Dupas et al. 2018). We explore heterogeneous treatment effects with respect to whether the vendor had a formal bank account at baseline. The results are noisy and fluctuate in sign, providing little evidence in support of this theory (but also fail to reject this theory). In India, vendors offered financial training were given a money box as a light touch intervention to support savings (similar to the box used by Ashraf, Karlan, and Yin 2006a, b and Dupas and Robinson 2013a). That these borrowers returned to debt at equal rates as their payoff-only counterparts is weakly suggestive that a safe place to save is not the constraining factor. We also attempted to explore this hypothesis directly by opening formal savings accounts for some vendors in the Philippines 2010 experiment, but because of the implementation obstacles referenced above, this treatment was abandoned.

A fourth possibility is that vendors do not understand the long-term cost of repeated borrowing at high rates. The financial training was explicitly designed to address this potential misperception, and had little effect on vendors' overall borrowing. That said, as with all training modules, there remains a confound between the efficacy of communication and the value of information being communicated; i.e., perhaps the training was not well executed. We also explore heterogeneous treatment effects with respect to an index of financial literacy and ability to answer basic mathematical questions.[11] In India, of vendors offered debt relief, those with

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[10] The time inconsistent preferences indicator variable is based on two questions at baseline that capture time preference reversal. It equals 1 for those willing to forfeit a certain amount of money to receive the sum today instead of in 30 days but unwilling to forfeit this same amount of money to receive the sum in 6 months instead of 7 months.

[11] The financial literacy index is made up of 7 baseline variables: whether they regret a purchase from the past week, whether they spend time developing a financial plan (not included in India 2007), whether they have ever created a financial plan (not included in Philippines 2010), whether the household mostly/always sticks to the financial plan (not included in Philippines 2010), whether the household regularly sets a detailed budget, whether they agree that without a plan household spending would go up, and whether the household regularly tracks their

higher mathematical ability are more likely to stay out of debt at first follow-up, although across other sites the estimates are noisy and fluctuate in sign.

A final possibility is that vendors suffer frequent shocks to their income and upon experiencing such a shock revert to borrowing as a method of smoothing. Vendors who suffer an income shock but already have substantial debt to moneylenders may face a binding borrowing constraint, with no other option than to reduce their consumption. By offering vendors debt relief, we may have relaxed this borrowing constraint, improving their ability to smooth their income until they fell back into debt and their borrowing constraint once again became binding. This is consistent with the results from Philippines 2010 in which we see that vendors offered debt relief are more likely to respond to low household income by borrowing in both the first follow-up and fourth follow-up, although in India and Philippines 2007 the effect on borrowing to smooth income shocks seems to be reversed.

To test this hypothesis, we use baseline characteristics to predict likelihood to experience an income shock in each follow-up wave and each site. We then include likelihood of a shock in the test for heterogeneous treatment effects.[12] The estimates for India and Philippines 2007 provide weak evidence suggesting that vendors more likely to experience an income shock were less successful at staying out of debt.

These mechanisms are important distinctions for policy. If, for example, the issue is driven by financial literacy and poor planning, such as an underestimate of the likely future costs of borrowing, public policies which require disclosure policies focused on this likelihood may change behavior (e.g., see Bertrand and Morse 2011). Similarly, if return to debt is driven by unabsorbed shocks, this makes salient how debt is filling in a missing insurance market for the poor. Improvements to social protection policies and insurance markets may help alleviate a problem erroneously perceived as a credit market problem. Evidence for this argument also comes from South Africa, in which borrowing at high interest rates (circa 200 percent APR) from a consumer micro-lender helped individuals absorb shocks, which led to an increase in likelihood of being employed a year later and consequently a reduction in the poverty headcount ratio (Karlan and Zinman 2010). This suggests both that shutting down or restricting credit markets, even at high interest rates, may lead to downward spirals even worse than perpetual borrowing. Thus the large gap between borrowing and lending interest rates in developing countries should be a call to action to understand how to bring

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spending. Each of the components is standardized against the control mean at baseline and all the components are then averaged and standardized to create a single score. The math skills index is made up of 3 baseline variables in India 2007 and Philippines 2007 and 4 baseline variables in Philippines 2010, each a word problem testing basic math skills. Each of the components is standardized against the control mean at baseline and all the components are then averaged and standardized to create a single score.

[12] The likelihood of experiencing an income shock comes from 11 separate probit regressions using 12 baseline variables to predict the probability of a household income shock in each survey wave within each site. The baseline variables used for these prediction models are the total value of business assets (USD), the value of merchandise bought per trip to market on average day (USD), the approximate taken-home profit typical day (USD), whether they save when excess profits on a good day, whether they have borrowed from moneylender for working capital, the amount borrowed from moneylenders for working capital (USD), whether an unexpected event caused a household income shock, whether they currently have any form of savings, their current amount of savings (USD), household size, household total expenditures (USD), and total monthly household income (USD). See online Appendix Tables A14–A16 for the output of each probit regression.

down costs for lending, not a cause to shut down credit markets. At current rates, however, this experiment demonstrates the importance of learning more about what individuals are doing with high interest moneylender debt, the circumstances of their decisions to borrow, and what market changes could satisfy their needs without warranting such expensive borrowing.

Further research could help tremendously both to understand how and why and for whom debt reversion occurs (in line with the above discussion), as well as establish specific interventions which ameliorate the issue. For example, options include a stronger implementation of the savings intervention; an alternative approach to the financial education; and more behavioral science-motivated interventions such as reminders or commitment contracts.

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