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Cash in hand and savings decisions☆

Lisa Spantig[a,b]

[a] RWTH Aachen University, School of Business and Economics, Templergraben 64, Aachen 52056, Germany
[b] Department of Economics, University of Essex, Wivenhoe Park, Colchester CO4 3SQ, UK

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ABSTRACT

Cash is an important means of transaction, generally assumed to be fungible. However, behavioral economics and consumer research show that 'cash in hand', physically holding on to cash and then handing it away, affects purchasing decisions. I study how cash in hand influences decisions in a different, but very important domain: savings. Savings accounts are a promising tool for reducing poverty, but the use of savings accounts is often puzzlingly low. Holding on to cash that needs to be physically deposited into a savings account may increase the psychological costs of saving. This study experimentally identifies the causal effect of cash in hand on savings deposits of female microfinance clients in the Philippines. In contrast to many laboratory and several field studies with similar interventions, I do not find reduced savings deposits due to cash in hand.

1. Introduction

Half of the world population uses only cash (Demirguc-Kunt et al., 2018). The standard assumption regarding cash is that it is fully fungible. Yet, economic research has shown in various contexts that fungibility can be reduced by labeling (e.g. Abeler and Marklein, 2017; Hastings and Shapiro, 2013; Kooreman, 2000) or by 'cash in hand', i.e. physically holding on to cash and handing it away (e.g. Reinstein and Riener, 2012; Luccasen and Grossman, 2018). Regarding cash in hand, both experimental (Prelec and Simester, 2001; Soman, 2003) as well as survey evidence (Mercatanti and Li, 2014) suggests that cash *payments* are lower than payments with other means of transactions. The evidence on violations of fungibility, both due to labeling and cash in hand, stems predominantly from high income countries. However, the fraction of the population that uses only cash is very small there, as compared to developing countries in which more than half of the population relies exclusively on cash transactions (Demirguc-Kunt et al., 2018).

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E-mail address: Lisa.Spantig@rwth-aachen.de

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In a natural setting dominated by cash, this paper investigates the causal effect of cash in hand on savings deposits. While labels have been studied and used to reduce fungibility and thus increase savings (e.g. Dupas and Robinson, 2013b), cash in hand effects have so far not been studied in the domain of savings. This paper focuses on depositing cash into savings accounts, as the use of accounts has been shown to reduce poverty.[1] The physical deposit of cash might reduce or even inhibit the use of savings accounts and other savings institutions and thus help explain low savings rates in accounts. Despite positive effects of savings on poverty reduction, formal savings rates remain low in developing countries: while 61 percent of the population in developing countries have a financial institution account, only 21 percent save in it (Demirguc-Kunt et al., 2018). Reasons for this are not fully understood (Karlan et al., 2014).

I experimentally study the effect of cash in hand on savings decisions of 300 microfinance clients in the Philippines. I pay participants cash to take part in an interview, and then allow them to deposit some of this payment in their savings accounts. To hold constant other factors that might influence savings decisions, and to identify the causal effect of cash in hand on savings decisions, I exogenously vary the point in time at which the participants receive the cash. All participants know how much they will receive as this is publicly announced before the start of the individual interviews. In the Cash in Hand (*CiH*) treatment, participants receive the cash at the beginning of the interview, so that they hold the cash for about 15 minutes before deciding how much of it to deposit. In the Cash Announced (*CA*) treatment, participants decide how much to put in the savings account before receiving the cash. Participants in *CiH* thus hold on to cash and hand over the amount they want to save, whereas individuals in *CA* verbally state the amount they want to save.

The main hypothesis is that cash in hand decreases savings *deposits*.[2] In the experiment, participants choose between taking cash home right after the interview or saving it for later. The presence of cash in the *CiH* treatment increases the tangibility of taking the money home right after the interview.[3] In contrast, the potential future benefits (and costs) of saving the cash remain intangible. This makes saving harder than in the *CA* treatment in which the option of taking the money home is less tangible. While the hypothesis is new in the context of savings deposits, the treatment variation builds on experimental laboratory studies that find (i) lower donations in the presence of cash (Reinstein and Riener, 2012; Luccasen and Grossman, 2018), (ii) lower spending with cash as compared to card payments (Prelec and Loewenstein, 1998; Feinberg, 1986; Runnemark et al., 2015) and (iii) stronger endowment effects if the item is physically present (Bushong et al., 2010; Peck and Shu, 2009; Strahilevitz and Loewenstein, 1998; Knetsch and Wong, 2009). Tangibility can help explain the findings of these three literatures. With cash present, the cost of giving it up is more salient which leads to lower donations with cash in hand. Similarly, the pain of paying is larger when cash is tangible. Given that cash itself is more tangible (in terms of amounts) than e.g. debit cards even though both means of transactions can be 'felt', spending is lower with cash as compared to cards. Lastly, tangibility can also explain why endowment effects are stronger in the presence of the item: the cost of giving it up is more salient and thus perceived to be higher. In sum, a physical transaction increases the psychological cost of an action due to the tangibility of the cash/item/donation to be transferred.

I find that cash in hand does not alter savings decisions. On average, participants save 42 percent of their experimental earnings, and both the means and the distributions of savings amounts are identical in *CiH* and *CA*. This null finding is robust to a cross-randomized variation in stake size. Given the extensive literature discussed in more detail below that employs similar manipulations, this is a surprising null result. The absence of a significant effect is neither driven by an ineffective manipulation nor by excessive noise. I rely on an established treatment variation and the null finding is robust to different estimation techniques and specifications. The lack of treatment effect heterogeneity further supports that a cash in hand effect does not exist in this setting. Lastly, the effect is quite tightly estimated, as under conventional power and significance thresholds, the minimal detectable effect size is 10 percent of the experimental earnings or a 25 percent change in savings (which I benchmark against laboratory studies with similar treatments and savings field experiments). This allows ruling out an economically relevant effect.

The study design and the experimental method of this paper have several advantages, combining a relevant field setting with the control of a laboratory experiment. All participants receive at least some of their income in cash and they already have a savings account, so that the experiment takes place in a natural setting with participants who have experience with the decision. The experimental setup allows isolating and cleanly identifying the causal effect of cash in hand, holding constant alternative (and previously studied) explanations for low savings such as temptation to spend the money, transaction costs or inertia in decision making. In addition, studying the clients of a microfinance organization that grants me access to administrative data, allows me to show that the experimental savings decision is correlated with actual (pre-experimental) savings behavior. The administrative data also permit investigating (and ruling out) selection into the experimental sample.

This study provides the first test of cash in hand in a field setting. In economic laboratory experiments, cash in hand has been compared to a display of a monetary amount on a computer screen. Studies have found lower charitable donations (Reinstein and Riener, 2012; Luccasen and Grossman, 2018), lower participation and smaller investments in an experimental

[1] Benefits of using savings accounts include an increase in business investment (Dupas and Robinson, 2013a), household consumption (Brune et al., 2016), financial well-being (Prina, 2015), as well as female empowerment (Ashraf et al., 2010), and a reduction in debt (Kast and Pomeranz, 2018). Importantly, Dupas et al. (2018) suggest that access alone is not enough and that active usage is required to obtain these beneficial effects.

[2] Section 4.2 discusses implications for savings at home.

[3] Tangibility here means "perceptible by the senses" or "capable of being perceived; especially capable of being handled or touched or felt". Rick and Loewenstein (2008) elaborate on the argument that future transactions are inherently less tangible and Section 4.3 discusses potential mechanisms in more detail.

game (Shen and Takahashi, 2017) and punishment to be more deterrent (Wang and Qin, 2015) with cash in hand. Less clear-cut results are presented by Charness et al. (2019) who find cash in hand effects on misreporting behavior in a loss, but not in a gain frame and Myrseth et al. (2015) who find no overall effect of cash in hand on public good contributions, but changes in treatment effect heterogeneity. Using a deposit into an existing savings account and a comparatively large sample, this study further questions the universality of cash in hand effects, by reporting a null effect and no treatment effect heterogeneity.

Studying the physical transaction of cash, this paper also contributes to consumer research investigating how the representation of money affects spending decisions. This literature has established how in cash transactions, parting with money is something vividly felt, which induces a high level of 'pain of paying' (Soman, 2003; Prelec and Simester, 2001; Thaler, 1999). Consequently, consumers spend less when paying with cash than when using other means of payment (Prelec and Loewenstein, 1998; Feinberg, 1986; Runnemark et al., 2015). Interestingly, the cash in hand or pain of paying effect even emerges when the cash transaction is only anticipated as in some of the above studies, the decision is made before the transaction. Contrary to this line of research, this study investigates a setting in which the representation of money is kept constant (all treatments only involve cash) and focuses on the physical transaction.

With its field setting, the paper also addresses the explanatory power of cash in hand effects in savings deposits, and demonstrates that they are unlikely to be of first-order importance as an explanation for low savings rates. It thereby contributes to a growing body of research in developing countries that tries to explain undersaving as compared to a world without institutional and behavioral frictions.[4] Most interventions tackling behavioral factors have focused on time-inconsistent decision making and/or inattention, providing some form of commitment device or reminders, which have resulted in mostly modest uptake (21%-40%) and usage rates (9%–21%) (Ashraf et al., 2006b; 2006a; 2010; Karlan et al., 2016; Brune et al., 2016). Cash in hand effects could explain the pattern found in most of these studies. For example, two recent field experiments compare defaulting payments into a savings account to handing out payments in cash (Somville and Vandewalle, 2018; Brune et al., 2017). Both studies find distinctly higher savings with automatic deposits, which could be due to e.g. inertia in decision making or small transaction costs.[5] Yet, the results are also consistent with a cash in hand effect decreasing savings deposits. As transaction costs and inertia are held constant in my experiment, my findings suggest that cash in hand in itself is not an important driver of savings in these studies and in savings deposits more generally.[6]

This study's design is closely related to endowment effect studies in the laboratory (cf. Ericson and Fuster, 2014) and employs a similar treatment manipulation in a relevant field setting. The manipulation of this study relies on findings showing that endowment effects are stronger when the item is physically present (Bushong et al., 2010; Peck and Shu, 2009; Strahilevitz and Loewenstein, 1998; Knetsch and Wong, 2009). Moreover, the time spent with cash in hand in my experiment is the upper bound of the time that, in laboratory experiments, the participants spend with their endowment. Conceptually, however, the cash in hand effect in savings decisions differs from the endowment effect. Endowment effects are usually described in terms of trading of goods (vs. money), while this study investigates 'trading' of cash now vs. cash in the future.[7] Despite physically handing over the cash, the money saved is still owned, which is not the case for traded goods (nor for money spent, as discussed above).

The potential policy relevance of cash in hand effects is not limited to the use of savings accounts. Given the widespread use of cash, it is important to further understand the role cash plays in economic decisions more generally. This is especially true in light of the recent policy debate in both developing and developed countries regarding a (faster) transition to a cashless economy. Yet, beyond labeling and cash in hand, surprisingly little is known about whether and how cash itself influences decision making. For policy design, however, it is important to understand in which instances fungibility is reduced due to behavioral responses. Why does cash in hand appear to influence spending, but not saving decisions? This paper calls into question the universality of cash in hand effects and points to interesting avenues for future research to better understand cash and its influence on decision-making.

The remainder of this paper is structured as follows: Section 2 introduces the research design, including the data sources, setting, procedures, and behavioral predictions. Section 3 presents the results, and discusses power, treatment effect heterogeneity, as well as ecological validity and representativeness of the sample. Section 4 discusses potential reasons for absence of a cash in hand effect, and Section 5 concludes.

[4] In many cases, low formal savings do not seem to be exclusively driven by liquidity constraints and being too poor to save (Banerjee and Duflo, 2007). In addition to direct savings costs such as transaction costs, regulatory barriers, and social constraints, explanations have focused on behavioral biases such as time-inconsistent decision making and inattention (Ashraf et al., 2006b; 2006a; 2010; Karlan et al., 2016; Brune et al., 2016; Dupas and Robinson, 2013b).

[5] More generally, defaults that make use of e.g. automatic payroll deductions in developing (Blumenstock et al., 2018) and developed countries (Thaler and Benartzi, 2004) appear to be a powerful tool to increase savings. However, they cannot be applied in settings that mostly rely on cash transactions and in which infrastructure for digital financial payments is lacking or not in use.

[6] In addition, this study also helps discern which features of savings deposits influence savings behavior. Harigaya (2017) shows that changing from deposits with account officers during regular meetings to deposits at one's own discretion with agents at corner stores, led to a decline in both savings balances and the frequency of deposits. This was mainly driven by lower peer pressure and the increased salience of the transaction fees. While the cash transaction was not altered in his study, the present study suggests that cash transactions do not inhibit savings.

[7] So far, endowment effects for cash have only been studied in the lab. While Bateman et al. (2005) find endowment effects for cash when trading for goods, Svirsky (2014) detects no endowment effect for cash when offered the possibility of exchanging it for other cash now. In the field, the most fungible 'item' that have been found to inhibit endowment effects are company shares (Anagol et al., 2018).

2. Research design

2.1. Data sources

I use a mixture of a controlled environment and a field setting, in which I observe actual savings decisions of a relevant population and randomly assign the presence of cash at the time of decision making. Embedding the savings decision in an interview allows me to collect a rich set of background characteristics.[8] I am able to link the interview data – collected on tablets during the sessions – to administrative data of weekly savings deposits and withdrawals as well as some basic demographics, including a poverty measure collected at the time of the last loan application. This allows me to assess how the experimental savings decision relates to savings behavior outside the experiment. An explanation of the variables can be found in Appendix J.

2.2. Setting and sampling

To ensure that I sample from a relevant population (who receive their income in cash), I work with clients from the Filipino microfinance organization Ahon Sa Hirap (ASHI), who provide financial services to poor women. Clients join the partner MFI to borrow for productive purposes, but when joining, the MFI also automatically opens a savings account for them.[9] The account offers an interest rate of four percent p.a. if a balance of at least 500 Philippine peso (PHP) is maintained over a duration of twelve months.[10] While the combination of saving and borrowing might seem counter-intuitive, it is a widespread practice in microfinance settings (Armendáriz and Morduch, 2010) and in this particular case, the two are complements rather than substitutes: Early repayments of the loan are not possible, so savings can serve as an insurance against potential future shocks and resulting repayment problems. Moreover, loans are usually taken to invest into one's own business, whereas participants state that emergencies (58 percent) and education (38 percent; up to three answers possible) are the main savings goals. The approval of loans does not depend on the savings balance or any savings behavior.

Clients usually self-select into groups of five and apply together to become members of the MFI. Two to eight of these borrower groups from the same neighborhood form a 'center' and meet weekly in a designated place to publicly conduct all transactions with the MFI in cash. Attending the weekly center meetings is mandatory and non-excused absences result in lower credit ratings. The marginal transaction cost of using the savings account is thus zero, as clients attend the meeting and can just deposit (or withdraw) savings. In addition, since all clients have an account by default, the hassle costs of opening an account do not matter in this setting.

All participants are women and the majority are self-employed (73 percent own a business). Eighty percent receive at least half and 59 percent receive all their income in cash. While mobile banking has progressed significantly in other countries, its coverage in the Philippines remains quite low, with only 11 out of 467 rural banks offering electronic banking facilities (one rural bank offers mobile banking) in the first half of 2017, and this is unlikely to change soon (Central Bank of the Philippines, 2017). As of 2017, only 5 percent of the population had a mobile money account and 25 percent used digital payments (Demirguc-Kunt et al., 2018). The Philippines thus constitute a setting in which the implications of cash transactions will remain relevant at least in the medium run.

Three branches of the partner MFI were selected based on their geographical proximity, to minimize the travel times for the research team. Within each branch, centers were selected based on meeting times and distance from each other, so that two centers could be visited per day. Section K in the Appendix provides evidence that this procedure did not result in a selected sample. The selection of the participants in the sample centers is closely linked to the experimental procedures, and is hence described in Section 2.4.

2.3. Experimental design

The experiment is embedded in a paid individual interview, which consists of three parts (see Appendix I.2 for details). The first part of the interview comprises questions regarding personal characteristics, the composition of the household, its financial situation, and personal business activities. Part 2 contains an incentivized elicitation of risk and time preferences as well as loss aversion. Part 3 includes survey questions regarding savings behavior, financial literacy, and hypothetical questions on narrow bracketing and attention to finances.

The experiment consists of a cash payment for participation in the interview and an unannounced savings decision. At the end of Part 1, participants are asked whether they want to save (some of) their earnings in their existing savings account. Before making the decision, participants are informed that if the amount saved is still in the account after four weeks, it will be matched with 20 percent. This match is added to the savings account by the research team; it has been employed to induce sufficient savings and to reduce potential influences of time preferences.

[8] The questionnaire can be found in Appendix I.2.

[9] This is not necessarily true for all microfinance clients. Yet, since clients do not join the MFI to save, my sample is still comparable to the clients of other MFIs.

[10] PHP 500 corresponds to about 2.5 times the average daily wages of the sample population, and were worth € 9.38 (US$ 9.96) at the time of the experiment. The inflation rate in the Philippines was about 2.5 percent.

Table 1
Timing of cash holding during the interview (Main Treatment Manipulation).

Minutes	Activity	Cash that participant holds	
		CiH Treatment	CA Treatment
		Receive cash E	
0:00	Part 1: General survey	E	0
15:00	*Savings decision S*	E	0
		Hand back S	**Verbally state S**
			Receive cash E-S
17:00	Part 2: Preference elicitation	E-S	E-S
25:00	Part 3: Savings survey	E-S	E-S
40:00	End: Preference payouts	E-S (+X)	E-S (+X)

Notes: *E* denotes the earnings received for participating in the interview (PHP 300 or PHP 500), *S* the amount saved, and *X* the potential payoffs from experimental preference elicitation.

Two treatments are implemented in a 2×2 between-subject design. The main treatment dimension varies cash in hand: Individuals receive the cash payment for participation either at the beginning of the interview or after the savings decision. Table 1 illustrates how much cash participants in *CiH* and *CA* hold during each part of the interview. Participants in *CiH* hold on to the cash during the first part. Treated participants thus make the savings decision by handing over (parts of) their cash holdings to the interviewer. In contrast, participants in *CA* make the savings decision without holding the money in their hands, but knowing that they will receive the remainder of their earnings just after making the savings decision. They verbally state the amount they want to save. The second treatment dimension varies the earnings amount to be either PHP 300 or PHP 500 (1.5 or 2.5 times the average daily wage; see Section 2.4 for more background on this treatment).

The structure of the interview serves two main purposes: First, it provides the possibility of controlling for potential spillover effects from the treatment onto the preference elicitation (i.e. subjects in *CiH* save less and thus are richer in Part 2, which in turn might alter their decisions). Cassidy (2019), for instance, shows experimentally that liquidity constraints can result in higher elicited present bias. Giving everyone the remainder of their earnings before the elicitation reduces this concern. Still, all questions, even those in Parts 2 and 3, can potentially be influenced by the treatment manipulation if *CiH* leads to lower savings and thus larger cash holdings in Parts 2 and 3. On the one hand, I make use of this feature when discussing potential channels in Section 4.3. On the other hand, I show that the answers in Part 3 are the same for the two groups (see Table 2), and use administrative data, where possible, to check that the respondents' answers to the interview questions are reliable and not influenced by the treatment (see Appendix H). Second, asking savings-related questions only in Part 3 prevents priming participants before the experimental savings decision. The decision to save is the first time savings are mentioned in either the session and the interviews.

2.4. Procedural details

Announcement of interviews. One week before the session took place in a selected center, all the clients in the center received an announcement letter, informing them of the possibility of taking part in paid individual interviews that would earn at least PHP 300. Familiarizing prospective participants with the procedure and coming back as announced establishes trust, such that all participants believe that they will be paid, irrespective of the treatment.[11] Further, it might create a reference point of PHP 300 for participation, which is why I cross-randomize an additional PHP 200 as a true windfall gain in the PHP 500 treatment.[12]

Recruitment of participants. Each session took place on the announced day during the weekly center meeting and started at the beginning of the meeting to make sure the interviews finished within the typical duration of the meeting (1.5–2 h). At the beginning of each session, the research team was briefly introduced and the expected duration of an interview (40 min) was stated before the clients could volunteer to participate. From the pool of all volunteers, ten participants were selected by a publicly drawn lottery. In all sessions, all present clients volunteered to take part, so that selection of present clients into the sample is not a concern. Unbeknownst to participants, the number drawn in the lottery not only determined the participation but also the treatment assignment – *CiH* or *CA* – and the interviewer.[13]

[11] To further increase trust, the announcement letter was read out and distributed by the MFI, a trusted institution (96 percent of participants think their savings are safe with this MFI). A question during the interview checked whether participants received the letter. While eight percent (7.3 percent) of participants in *CiH* (*CA*) stated not having received it, these shares do not differ between the treatments (χ^2, $p = .828$). The vast majority of participants can read and write (92 percent have at least completed elementary school) and are used to receiving written documents from the MFI. The English translation of the letter can be found in Appendix I.1.

[12] While the announced earnings of PHP 300 might already have established a reference point or entered the participants' budget plans, the additional PHP 200 should be treated as a true windfall gain. Note that I cannot disentangle the income effect (participants with PHP 500 are richer) from the surprise effect. The treatment necessary to disentangle the two would have been an announcement of PHP 500, which in turn might have induced selection into the sample. To rule out this selection, I opted for the present design.

[13] Randomization of interviewers avoids selection of interviewer–interviewee parings from either side. Randomization was done prior to the start of all sessions using Stata and the randomization protocol was implemented by myself.

Randomization of treatments. The two treatments are randomized on two different levels: *CiH* is randomized at the individual level and all interviewers conducted interviews in both treatments, balancing interviewer-specific effects. To rule out confusion of treatments, the computer-based program of the questionnaire provided detailed scripts and required treatment-specific entries (e.g. where cash in *CiH* is kept during Part 1). By relying on individual level randomization of cash in hand, in combination with this particular setting, I can rule out other potential explanations for undersaving (see also Appendix D) and cleanly estimate the cash in hand effect on savings decisions. The earnings amount is randomized at the session level and was only announced after the recruitment, to avoid potential selection effects.[14] Randomization at the session level is necessary since the earnings amount is publicly announced (in front of all members of the borrower center) to increase trust. Moreover, to avoid any denomination effects, the different bills were displayed during the announcement of the amount. In sessions with the PHP 300 treatment, the following notes were shown (and later handed to participants during the interview): one 100, two 50 and five 20 peso bills. In the PHP 500 treatment sessions, two 100, four 50 and five 20 peso bills were shown and handed out.[15] Pre-tests have shown that displaying the bills in front of the borrower center before the start of the interview establishes trust in receiving the money.

Sessions and earnings. Three hundred clients were interviewed in 31 different centers in semi-urban and rural areas of the Laguna Province on the main island in the Philippines, Luzon. Center meetings take place Monday to Thursday and usually start either at 9am or at 1pm, resulting in two sessions per day, which were conducted in the spring of 2017. Each center was revisited four weeks after the initial session to deposit the match in the savings account, when applicable. The average earnings from the sessions were PHP 417 (€ 7.82 or US$ 8.30), including payouts for survey participation and preference elicitation. 85 percent of all participants (93 percent of those who saved) were eligible for the match and additionally received the match payment after four weeks (more information in Appendix Table G.1).

Additional logistics. A team of five local interviewers were trained to conduct the interviews on Surface Pro-tablets using z-tree (Fischbacher, 2007). Selected participants were interviewed one-on-one by a local interviewer in private. Before the start of each interview, participants verbally gave informed consent for taking part. Right after the savings decision, the interviewer put the amount saved into an envelope which was later handed to the loan officer who would register the savings. First, five participants were interviewed in one round. Once these interviews were over, the next five interviews were conducted. Only two rounds of interviews were conducted in each center to avoid information flow from those already interviewed to the to-be-interviewed participants. Additionally, at the end of the interview, all participants were asked not to talk about the details of the interview with others. During the interview, participants were thus unaware of the potential existence of other treatments. All questions and instructions were translated into the local language, Tagalog, (and back-translated to English) and piloted before the start of the experiments.

3. Results

3.1. Balance

Table 2 provides the results from OLS regressions with the treatment dummies as independent variables, where CA_{300} is the omitted category. The dependent variables come from both the interviews (self-reports) and administrative data.

The *F*-test of the treatment dummies jointly explaining the respective variables is always insignificant at the 10 percent level. The successful randomization permits simple non-parametric comparisons of the treatments.

3.2. Main result: Savings deposits

Pooling the two earnings treatments, participants saved PHP 166.17 (SD: PHP 125.18) on average.[16] This corresponds to 42 percent of the average experimental earnings. Holding on to cash for on average 15 min and handing it over to save it did not alter the savings decisions (Wilcoxon rank-sum exact test, $p = .792$). Fig. 1 shows the savings amounts for all four treatments. While participants with higher earnings save more, there is no interaction effect of *CiH* and earnings (see also Table 3).

Not only are the mean amounts saved the same in *CiH* and *CA* (for both earnings amounts, respectively), but there is also no difference in the distribution of choices (see Fig. 2, Kolmogorov-Smirnov test, $p = 1$, irrespective of pooling or testing the two earnings amounts separately). Examining the distribution of amounts saved, it becomes apparent that focal points exist for absolute amounts. This provides evidence that the participants thought about the decision in absolute terms (see also the graph for shares of the earnings saved, Fig. A.1 that does not show such clear patterns for certain round percentages). It is thus rather unlikely that participants used a rule of thumb, such as 'save 40 percent of earnings', which would explain the equality of shares saved.

[14] Since I have an uneven number of centers in my sample, 15 centers received PHP 300 and 16 PHP 500. Cell sizes are thus as follows: 73 (73) individuals in *CiH* (*CA*) with PHP 300 and 77 (77) in PHP 500.

[15] Individuals in *CiH* knew they could change the bills into coins, but they did not receive coins, since the number of coins needed to make decisions in PHP 1 steps was perceived as unnatural and even offensive during pre-testing. Participants in *CiH* did not request change and participants in *CA* did not state amounts that would have required change.

[16] Here I deviate from the pre-specified analysis that would have used the share saved (all results are reported in the appendix). While the findings do not depend on this change, analyzing the amount saved is more intuitive, as participants appear to think about the decision in absolute numbers.

Table 2
Balance.

	CA_{300}	CiH_{300}	CA_{500}	CiH_{500}	R^2	F-test
Self-Reports ($N = 300$)						
Age	43.19	0.986	0.535	-1.532	0.001	0.945
Education (yrs)	8.712	-0.164	-0.102	0.450	0.003	0.718
Financial literacy (0–1)	0.514	0.027	0.077*	-0.073	0.018	0.229
SR attention to finances (y/n)	0.274	-0.0685	-0.0792	0.146	0.008	0.471
MR attention to finances (y/n)	0.301	-0.0822	-0.0936	0.121	0.007	0.337
HH size	5.288	0.466	0.024	-0.687	0.011	0.529
Owns business (y/n)	0.658	0.082	0.109	-0.095	0.009	0.617
Cash income (y/n)	0.918	0.014	0.017	0.0123	0.004	0.577
Currently saving (y/n)	0.753	0.082	0.078	-0.108	0.007	0.473
Savings at home (P)	1034	441.8	318.7	-662.1	0.005	0.606
Savings in account (P)	662.3	-16.17	364.5	-267.1	0.010	0.630
Travel cost to center (P)	0.685	-0.397	0.419	0.280	0.007	0.270
Travel time to center (min)	5.658	-0.164	0.680	0.489	0.006	0.788
Banks untrustworthy (0–1)	0.468	0.00548	-0.0555	0.00491	0.008	0.465
Savings in ASHI are safe (0–1)	0.932	0.0274	-0.0386	0.0408	0.018	0.297
Decision making power (0–1)	0.511	0.0522	0.000573	-0.0847*	0.013	0.155
Would like a private account (0–1)	0.616	0.0548	0.0394	-0.00285	0.006	0.611
Saving less due to...						
... claims from husband	0.538	-0.00905	-0.00861	-0.0246	0.002	0.939
... claims from family & friends	0.168	0.0137	0.0108	-0.0689	0.006	0.589
Administrative Data ($N = 296$)						
Savings balance (PHP)	764.9	64.35	2.396	-31.22	0.001	0.981
Loan amount (PHP)	23,722	1,014	-5,385*	355.1	0.035	0.105
PPI score (0–100)	43.42	1.556	-0.547	-1.492	0.002	0.916
Main income: Enterprise (y/n)	0.836	-0.0959	0.0865	-0.0470	0.032	0.656
Electricity (y/n)	0.458	0.0139	0.00920	-0.0281	0.000	0.992
Water (y/n)	0.153	0	-0.0229	0.0701	0.005	0.426
Landline (y/n)	0.0139	-0.0139	-0.0139	0.0272	0.007	.
Membership (months)	60.96	-1.219	-19.53	-5.209	0.052	0.130

Notes: The upper panel presents results for variables elicited during the experiment and the lower panel variables from pre-experimental administrative data. Higher values indicate larger agreement/better outcomes. Mean of the CA group and coefficients from OLS regressions with treatment dummies as independent variables and clustered SE (not shown) at the center level. The last column shows p-values of the F-test of joint significance of the treatment dummies. *** p<0.01, ** p<0.05, * p<0.1.

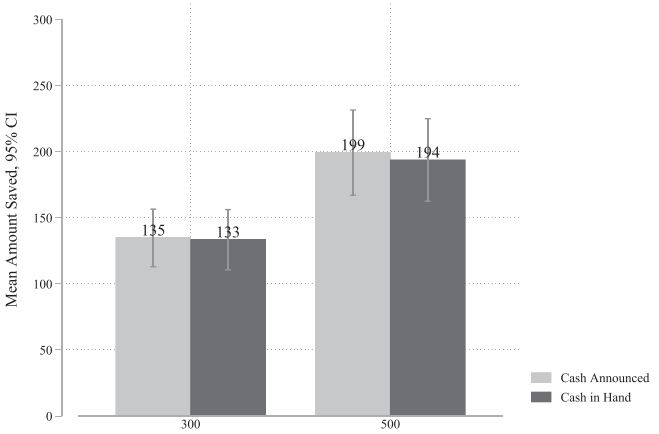
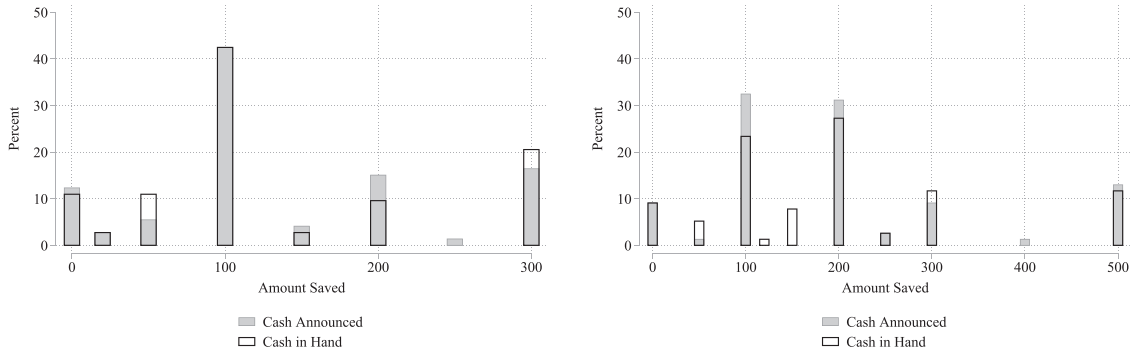


Fig. 1. Mean amount saved by cash in hand and earnings.

Using OLS regressions, I control for potential influences on the savings decisions, such as financial literacy, age, education, household size, owning a business, having money left after buying all necessities, decision making power, and transaction costs in terms of the time and money needed to travel to the center meeting. Table 3 confirms the non-parametric findings and shows that no interaction effect exists with the earnings amount (columns (3) and (4)). The inclusion of control variables



(a) 300 ₱ **(b)** 500 ₱

Fig. 2. Distributions of amount saved.

Table 3
Treatment effect on amount saved.

	(1) Amount Saved	(2) Amount Saved	(3) Amount Saved	(4) Amount Saved
CiH	-3.533	-3.522	-1.370	-7.961
	(13.25)	(13.85)	(17.26)	(18.07)
	[-28.49, 22.11]	[-28.87, 22.69]	[-34.30, 31.42]	[-41.00, 25.59]
Endowment 500			64.56**	60.44**
			(24.62)	(22.98)
			[17.28, 111.1]	[17.19, 103.3]
CiH x 500			-4.215	8.068
			(27.09)	(27.24)
			[-54.18, 46.89]	[-41.51, 59.23]
Mean in omitted	167.9	167.9	134.8	134.8
p: CiH + CiHx500 = 0			0.791	0.996
Observations	300	300	300	300
Adj. R^2	-0.003	0.035	0.053	0.097
Clustered SEs	yes	yes	yes	yes
Controls		yes		yes

Notes: OLS estimates with SE in parenthesis, wild cluster bootstrapped 95% CIs accounting for small number of clusters (centers) in brackets. Controls: age, education, financial literacy, hh size, business owner, money left, decision making power, distance to center, time to center, interviewer FE. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

in columns (2) and (4) does not alter the results and thus confirms successful randomization.[17] The coefficients of the *CiH* treatment dummies are small and statistically indistinguishable from zero. When testing the two coefficients in columns (3) and (4) for joint significance, the *p*-values of 0.79 and 0.99 clearly indicate that the null hypothesis of no *CiH* effect cannot be rejected. These results are robust to Tobit estimations that censor the lower limit at zero and the upper one at 500 (see Appendix Table A.2).

The above discussed results show no effect of *CiH* on savings deposits. For this to be a convincing and interesting result, it should be sufficiently powered.

3.3. Power

I conducted ex-ante power calculations to determine the sample size and now provide ex-post power calculations for the minimal detectable effect sizes (MDE). Setting $\alpha = 0.05$ and $1 - \beta = 0.8$ and using a two-sided *t*-test, I would be able to detect a difference of PHP 41, which is equivalent to ten percent of the average earnings, and 0.33 standard deviations of the amount saved.[18] The confidence intervals in Table 3 and A.1 point to even smaller MDEs. Since their interventions are similar, I use the MDE of PHP 41 and calculate standardized effect sizes for several laboratory studies of the endowment effect (see Appendix E for details). Table E.1 provides evidence that my power is sufficient to detect an effect smaller than

[17] The results are also robust to the inclusion of indicators for above median risk aversion, loss aversion, present bias, and future bias. Due to potential problems of reverse causation (e.g. *CiH* could result in higher elicited loss aversion), however, they are not included as controls in the reported regressions.

[18] In terms of the share saved, I would be able to detect a ten percentage point difference in shares saved between *CiH* and *CA* (pooling the two earnings treatments), which represents 0.32 SD of the mean share saved. Calculated with Stata's power twomeans command.

what most other studies find. The setting of my experiment, however, is closer to field experiments on savings behavior. I therefore also benchmark the effect size to such studies.[19] The present experiment would be able to detect a 25 percent change in savings deposits, which is a smaller effect than most of the recent field experiments find (Appendix E provides further details). I conclude that compared to both laboratory studies with similar treatment manipulations and field experiments with comparable settings, the minimal detectable effect is small. This implies that the null effect is not due to a lack of power.

3.4. Treatment effect heterogeneity and covariates

The null finding presented above might mask treatment effect heterogeneity. Based on the previous literature, several groups that respond differently to *CiH* come to mind. For instance, participants who use their savings account more might be less likely to be affected by the psychological cost of making the deposit.

I use the administrative data to classify different types of savers, trying to capture various dimensions of savings behavior. Participants might save large or small amounts. Moreover, (ir)regular deposits capture the habit of saving in terms of frequency, whereas (un)equally-sized deposits get at savings goals in terms of amounts. For all three dimensions, I have created binary indicators based on median splits of weekly savings deposits of all clients in the three participating branches for the last nine months (up to the date on which the first interviews were announced; see Appendix F for a detailed description and a balance table). I use these types and the deposit dummies (high, regular and equally-sized) to investigate treatment effect heterogeneity.

Other relevant dimensions might be narrow bracketing and loss aversion if the presence of cash creates a stronger reference point in consumption, or cognitive abilities as those with lower cognitive abilities are more likely to violate fungibility (Abeler and Marklein, 2017). In addition, a longer duration of the first part (that determines the duration of holding the cash in *CiH*) might lead to lower savings due to the longer exposure to cash, similar to stronger endowment effects with an increased duration of ownership (Strahilevitz and Loewenstein, 1998).

Instead of arbitrarily and repeatedly subsetting the data, I use a model selection procedure based on machine learning that automatically controls for multiple testing. I use LASSOplus (Ratkovic and Tingley, 2017), which estimates both treatment effect heterogeneity and important covariates and permits statistical inference. The algorithm is a Bayesian method in which the effects are simultaneously estimated and selected. First, each effect of a potential covariate and its interaction with *CiH* is consistently estimated and then, following a thresholding rule estimated from the data, small effects are trimmed to zero. Ratkovic and Tingley (2017) describe the method in detail and show in simulation studies that it is conservative and has a low false discovery rate.

In addition to the type data, I include education as a proxy for cognitive abilities, preference data dummies for above median loss and risk aversion as well as a dummy indicating present bias, indicators for narrow bracketing, an indicator for a high PPI score, an indicator for being amongst the first five participants interviewed in a session (round 1) and, for consistency, the control variables from the regressions.[20]

Fig. A.2 shows the density of selected effects. Consistent with the previous results, the *CiH* dummy has not been selected as a significant determinant of the savings decision. The algorithm did not detect a significant interaction effect either. Given that preferences and biases usually exhibit substantial hetereogeneity (e.g. Andersen et al., 2010; Bruhin et al., 2010), the absence of treatment effect heterogeneity further strengthens the null finding. Nonetheless, the analysis reveals some covariates of the savings decision to be significant, in line with previously identified determinants of savings, showing that meaningful heterogeneity exists in the sample. Five covariates, large deposits, high financial literacy, large households, interviewer 2, and round 1, are significantly and positively related to the savings decision.[21] This is a first indication that the controlled setting reflects actual savings decision making.

3.5. Ecological validity

Ecological validity assesses whether the study design is meaningful for the setting of interest. To establish that the experimental savings decision is a relevant proxy for actual savings behavior, I compare the decision to actual savings. The decision is positively correlated with the amount in the account (Spearman's $\rho = 0.138$, $p = .017$), but not with the previous week's deposit (Spearman's $\rho = 0.052$, $p = .37$), which is likely due to the high volatility of the deposits. Moreover, the decision is related to the total savings stock, which adds up all self-reported savings amounts from the survey (Spearman's $\rho = 0.173$, $p = .003$). As also confirmed by the LASSOplus estimation, my experimental setting thus reflects actual decision making and does not appear to be overly complicated or artificial. More generally, I find that the participants' answers to non-cash related questions in the interview are consistent with administrative data and are thus not influenced by the experiment as such (see Appendix H).

[19] I would like to thank Laura Schechter for this comment.

[20] Note that the estimation procedure is robust to including 'irrelevant' variables, as they are shrunk to zero in the selection process: For instance, only including the selected variables in the estimation and re-running it, results in all variables being selected.

[21] Effect sizes (in PHP): large deposits 23.27; high financial literacy 21.26; large households 33.52; interviewer 2 33.07; round 1 30.38. All selected covariates are balanced across treatments: Interviewer 2 and Round 1 by design and the other variables by randomization (see also Table 2).

4. Discussion

Why does cash in hand not alter the savings decisions? Given that the previous literature established cash in hand, pain of paying and endowment effects, it is surprising that *CiH* does not influence behavior in this setting. In the following, I will discuss the two main differences of this study with existing research: First, this being a field experiment with some necessary differences in the design and the sample, and second, the type of decision. Lastly, I will briefly address potential channels.

4.1. Field experimental design and sample

Design

The design heavily relies on the literature on cash in hand and endowment effects, so the main treatment is very similar. Additionally, the duration of cash holding has been adopted from the endowment effect literature. While it is unclear whether endowment effects emerge instantaneously, a consensus exists that 15 min are sufficient to induce the effect in the laboratory (cf. Ericson and Fuster, 2014). In addition, the laboratory study most closely related in terms of design varies cash in hand and a computerized display of earnings and finds an immediate effect of lower charitable donations with cash in hand (Reinstein and Riener, 2012). The treatment variation in itself is thus comparable to previous experimental economics research. Similar to controlled laboratory experiments, the design of this study excludes all other explanations for low savings, such as transaction costs, inertia in decision making, social constraints as well as lack of trust and regulatory barriers (see also Appendix D).

However, some features of my experiment are clearly different from the previous laboratory experiments, for instance, the stake size and the incentives involved in the experimental decision. The payment amounts were chosen to make the savings decision meaningful.[22] The 20 percent match of amounts saved was implemented to ensure that savings amounts vary and are not concentrated on the lower or upper end of the distribution and to reduce the potential influence of present bias.[23] While the calibration has been successful (reflected in the variety of the shares of earnings that the participants save, see Fig. 2 and A.1), the earnings amount and the match constitute a large additional incentive to save and thus makes the experimental decision high-stakes. If the cash in hand effect is comparatively small, it might only exist when impulsive, less important decisions are made. For incentivized, rather high-stakes decisions (the savings decision in my experiment), individuals might be able to override the impulsive response and decide rationally. Several points speak against this interpretation. In general, behavioral effects that have been demonstrated in laboratory experiments cannot be explained by stake size (see, e.g., Camerer, 2015).[24] For endowment effects, Anagol et al. (2018) show that they persist even in high-stakes field environments such as the stock market. For cash in hand effects, similar evidence is lacking as this is the first high-stakes study. However, based on the income treatment variation, it does not appear that the cash in hand effect is dependent on stake size. It is thus unlikely the high stakes in my experiment explain the different findings, but it might be interesting to further reduce the stake size (but keeping the decision meaningful) in future research.

In contrast to the pain of paying literature in consumer research, the cash in hand treatment variation of this study keeps the representation of money constant. All participants think about the deposit as a cash deposit and the only difference is whether the cash is physically present at the time of decision making. This is an important distinction to the pain of paying studies in which cash payments are compared to card payments. Moreover, the present design even keeps constant the denomination of the cash that participants decide about as the exact denomination was shown publicly prior to the interviews.[25] In this regard, the study provides a clean test of cash in hand effects, as both the denomination and the representation of money are kept constant across treatments and only the presence of cash is varied.

Sample

The sample of this study differs from student samples in several aspects. Participants are female microfinance clients who mostly rely on cash transactions and might be prone to using simple heuristics. In addition, they are exposed to institutional incentives to save and might have an intrinsic motivation to put money aside.

All participants are experienced with cash transactions and savings decisions. The fact that the experimental savings are related to pre-experimental savings in the account (as discussed in Section 3.5) shows that the experiment reflects real-life savings behavior. While some studies have found that endowment effects disappear for experienced participants (e.g. List, 2011; Engelmann and Hollard, 2010), recent evidence from the field documents endowment effects that persist

[22] While the payment amounts might appear large in comparison to the median weekly savings deposit of PHP 33, they are in the range of participants' cash holdings during the center meetings as the median weekly loan repayment installment is PHP 333. Moreover, I do not detect treatment effect heterogeneity, neither with respect to wealth nor with the savings amount in the account. *CiH* does not influence the ratio of experimental savings to savings in the account either.

[23] Present bias is balanced across treatments and has not been selected by LASSOplus as a significant determinant of the savings decision.

[24] I would like to than an anonymous reviewer for this comment.

[25] For spending cash, it has been shown that a single, larger denomination bill reduces spending as compared to the same amount of money in smaller bills (Raghubir and Srivastava, 2009). The treatment variation could have resulted in different savings decisions if participants in *CA* thought about the cash in a different denomination than the ones in *CiH* who actually hold the bills in their hands. The equality of distributions of the share and amount saved in *CiH* and *CA* (as depicted in Figs. A.1 and 2) shows that this is not a concern.

even for experienced traders (Anagol et al., 2018).[26] In addition, literature on cash in hand and pain of paying has documented these effects in samples that are also experienced in cash transactions. Therefore, experience is unlikely to be the driving force behind the null result.

Relying on a poor sample and knowing that poverty might impede cognitive function (Mani et al., 2013), participants might not deliberate about the decision, but act based on a simple heuristic when deciding. While the distribution of the amounts that participants save indicate that focal points are important (Fig. 2), the shares saved are statistically different from a 50:50 heuristic (t-test, $p = .002$ for *CA* and $p < .001$ for *CiH*).

The savings account only bears interest when a minimum balance of PHP 500 is maintained for a year. This might create an additional incentive to save for the 147 participants (73 in *CiH* and 74 in *CA*) below this threshold. This is not a concern, since participants above the threshold save rather more (Wilcoxon rank-sum test, $p = .062$) and the amount necessary to reach the threshold is not correlated with the savings decision of those who have fewer than PHP 500 in their savings account (Spearman's $\rho = -0.062$, $p = .456$).

For motivated savers, *CiH* could increase the salience of savings rather than consumption, and thus lead to larger savings. Approximating motivation (and experience) with the different types of savers, I do not find treatment effect heterogeneity that would support this. Given that all participants have a savings account, 81 percent state that they are currently saving, the incentive to save (20% match) and savings balances of on average PHP 790 (that makes it easy to meet the criteria for receiving the match) one would assume high motivation to save. It thus is rather surprising that only 15 percent save their entire earnings. Overall, participants do not seem to be overly motivated to save.

To summarize, in contrast to laboratory studies with comparable designs, the stake size and the constant representation of money as cash and its denomination render this study a clean, lower-bound test of cash in hand effects. The field setting provides an interesting but different sample in comparison to existing studies. More research is needed to tease out which aspects of the design and the setting discussed above actually contributed to the absence of a cash in hand effect or whether cash in hand effects generally do not exit in savings decisions. In particular the role of the sample and the representation of money appear to be worth further investigation.

4.2. Saving vs. spending decisions

Previous literature has studied the cash in hand effect in the context of purchasing and donation decisions. These decisions differ from savings decisions in various ways. Moreover, cash in hand can have different implications for savings deposits as compared to saving at home. I briefly discuss both points below.

Savings deposits vs purchases and donations

The *CiH* treatment varies how the transaction is made and this *cash transaction* is the same for savings deposits and purchases: Cash is handed away to someone else and is not physically present any more. This physical transaction is the key argument for pain of paying and the element subject to experimental variation in the cash in hand studies. Nonetheless, savings deposits and spending decisions can vary on several other aspects.

A first difference is that spending cash implies giving up both possession and ownership, while saving cash in an account only implies a temporary change in possession. Given that physical presence and possession are more important for attachment effects than actual ownership (Reb and Connolly, 2007), it is less likely that this aspect can explain the absence of a cash in hand effect in savings deposits.

A second, closely related difference is that spending cash implies giving it up entirely in exchange for utility derived from consumption, warm glow, etc. This is likely to be perceived as costly. Saving cash in an account, on the other hand, implies not having access to the cash for some time (in my setting, at least one week until it can be withdrawn). This can still be costly, for example in terms of reduced flexibility. Indeed, participants in my experiment appear to incur large costs by giving up flexibility: only 15 percent save their entire experimental earnings despite the high-powered incentives. In this setting, where participants are poor, face liquidity constraints and cash is the main mode of transaction, the need for cash and its high valuation are not surprising. It is thus not clear whether the perceived cost of handing the money away is strictly lower in this study than in others.

Lastly, savings decisions always entail an intertemporal trade-off, whereas spending decisions might not (in cases where cash and goods are exchanged simultaneously). A thorough comparison of the financial and consumption events and their respective timing that are associated with savings as compared to spending decisions depends on the exact trade-offs under consideration and is beyond the scope of this paper.[27] To the extent that intertemporal considerations are not or reversely influenced by cash in hand (as compared to spending decisions) and that these considerations outweigh the above discussed aspects, the difference in the decision domain could explain why this paper finds no effect of cash in hand.

[26] Whether experience fully eliminates the endowment effect is still an open question. List (2003), Engelmann and Hollard (2010), List (2011), Giné and Goldberg (2018) provide evidence that experience with a similar transaction eliminates the endowment effect. However, Harbaugh et al. (2001) show that general market experience does not reduce reluctance to exchange goods in the laboratory and Anagol et al. (2018) demonstrate that while trading experience reduces the endowment effect for stocks (by 17 percentage points as compared to non-experienced traders), they still document a sizable effect: a 60% higher likelihood of holding the stock for experienced traders.

[27] For example, Cohen et al. (2020) discuss some of the differences in detail, with a focus on issues that arise when modeling and measuring time preferences.

With a similar transaction, a change in possession and reduced flexibility associated with savings deposits, it is plausible that cash in hand effects could also exist for savings deposits, in particular in my setting. However, more research is needed to better understand the cash in hand effect in different decisions.

Savings deposits vs saving at home

In this study, I focus on savings deposits into accounts as the use of such accounts has been shown to be beneficial in terms of poverty reduction. However, other forms of saving exist. In particular, if a cash in hand effect exists for some transactions, it increases the amount of cash an individual holds. If this cash is defined as 'saving at home', the effect of cash in hand for either purchases or savings deposits would lead to an increase in savings at home. Moreover, if saving under the mattress is the only means of saving, the total amount of savings might increase. It should, however, be kept in mind that the availability of cash in developing countries often leads to sharing requests by others (Kast and Pomeranz, 2018), such that it is unclear whether reduced spending due to cash in hand would lead to higher saving at home in the medium or even long run.

In this experiment, I cannot closely track what happens to the cash that is not saved during the experiment. However, in a short follow-up survey on how the cash received in the experiment was used, no difference in spending patterns can be detected between *CiH* and *CA* (see Table G.1). This is consistent with the null finding on savings deposits. So far, cash in hand effect have been studied for single transactions and this paper examines a new type of transaction. To understand potential substitution effects between different types of transactions, a more comprehensive design would be needed.

4.3. Potential channels

Lastly, I briefly discuss evidence for the main potential channels, (i) cash in hand increasing the tangibility of taking the money home (Rick and Loewenstein, 2008) and (ii) the possession of cash in *CiH* creating an attachment effect (Reb and Connolly, 2007) and how these channels would affect different types of transactions.[28]

First, if *CiH* increases the tangibility of taking the money home, it should lead to lower savings deposits. Similar arguments apply for spending and donations (see, e.g. Reinstein and Riener (2012), for the argument that donations will be lower when the consumption to be sacrificed is highlighted by cash in hand). This decrease in handing away cash would mechanically lead to higher savings at home (see also Section 4.2). In this experiment, is unlikely that this channel plays an important role as only 17 percent of experimental participants state that having cash lets them focus on consumption only.[29] Additionally, the treatment did not affect consumption after the experiment (see Table G.1).

Second, if *CiH* increases attachment to cash, participants will save less if they need to make a physical transaction. The same is true for spending and donation decisions, where individuals would want to keep the money. Again, such an attachment would lead to higher savings at home. In terms of the extensive margin (handing over any cash to be deposited) there does not seem to be an attachment effect. Eleven percent (ten percent) do not save in *CiH* (*CA*). In terms of the intensive margin, respondents appear to prefer spending fewer, but larger bills. For example, in order to save PHP 100, participants can hand over five PHP 20 bills, two PHP 50 bills or one PHP 100 bill. Seventy-eight percent of those who save PHP 100 in *CiH* hand over one bill. This is in contrast to the denomination effect in spending where the likelihood of spending is reduced with larger bills (Raghubir and Srivastava, 2009). While the preference for depositing larger bills may be attributed to attachment to cash, it could also be explained by e.g. the ease of calculating when using fewer bills.[30]

Lastly, I find suggestive evidence for an additional channel that can be active when holding onto cash: a version of money illusion that changes the perception of disposable income and makes individuals feel richer. In Part 1 of the survey, individuals in *CiH* were already holding on to cash, whereas individuals in *CA* only knew that they would receive money later on. With cash present, participants reported higher amounts in the question 'How much money will you personally be able to take home at the end of today?' (means: *CiH* PHP 707, *CA* PHP 606; Fligner-Policello test, $p = .032$; Kolmogorov-Smirnov test, $p = .039$). Appendix C provides a detailed description of the test and presents additional results in line with such an effect. Based on the income effect (see also Appendix B), this channel should increase formal and informal savings, whereas its effect on spending behavior would depend on the type of spending. However, as Table C.3 shows, feeling richer is not associated with significantly higher savings. This suggests that this channel might only be active for impulsive and

[28] Reinstein and Riener (2012) discuss three potential channels why donations can be lower with cash in hand. First, a disutility from parting with cash, similar to pain of paying or endowment effects, might arise that decreases donations (what I call attachment to cash). Second, cash could highlight the consumption that is being sacrificed by donating the money (similar to the tangibility argument). Third, cash as a "reminder of money" might render experimental participants more selfish and thus reduces donations. In the context of saving, it is unclear how to think about selfishness, for example in a dual-self model: which self becomes more selfish and how does this selfishness relate to the 20% match? I would like to thank an anonymous referee for this comment.

[29] Eighty-three percent disagree or strongly disagree with the statement "If I have cash, I think about what I can buy, but I don't think about savings." There is no statistically significant interaction effect of only focusing on consumption and *CiH*.

[30] Note that in both these channels, the decrease of savings deposits assumes that the effect of tangibility on the perceived costs of parting with cash is larger than a potential effect of tangibility on the anticipatory utility of receiving cash for the future self (see Rick and Loewenstein, 2008, for the general discussion that earlier choices are more tangible). Absent a time-dimension in spending and donations, no such assumption is necessary for those transactions.

less important decisions (such as answering a non-incentivized question or small-stakes decisions) and is not strong enough to induce any effects in incentivized, rather high-stakes decisions (such as the savings decision in the experiment).[31]

Overall, I do not find evidence that *CiH* significantly impacts savings deposits. The discussion highlights the need for more research that focuses on cash in hand effects in different settings and different behaviors.

5. Conclusion

This study uses a well-established manipulation to test cash in hand effects in the domain of savings. In a cash-dominated setting, holding on to cash does not reduce savings deposits due to the physical cash transaction.

Given sufficient power to detect economically relevant effect sizes for the cash in hand effect and the documented correlation of experimental with real-world behavior, this null effect is an interesting result that complements the literature in several aspects. The findings are of interest for behavioral economics as they suggest that cash in hand effects found in the laboratory are not generalizable to all field settings. In a realistic, high-stakes decision environment, the physical deposit of cash does not distort decision making. The results of this study are also relevant for consumer research, as they imply that the pain of paying is unlikely to be driven by the physical transaction of cash. It rather seems that the representation of money and its denomination – both held constant in this study – contribute to the pain of paying.

Due to its prevalence, cash merits a better understanding of how it influences decisions. While this paper isolates the causal effect of cash in hand, more work is needed to understand whether and how cash in hand interacts with other variables, such as interest rates, temptation or claims on cash by family members that are relevant to the setting. The present design could be easily modified to test e.g. whether the presence of cash increases temptation or whether it facilitates planning. More generally, it will be important to understand how cash in hand effects differ for formal and informal savings on the one hand and spending behavior on the other hand. Examining cash in hand effects jointly for spending and (in)formal saving decisions will help explain the diverging findings of the literature focusing on spending and this study. Lastly, the suggestive finding that participants who hold on to cash feel richer should be replicated and investigated further as this might have important implications for experimental design.

In addition to studying cash itself, future research could study how cash as compared to electronic payments influences decision making. Currently, little empirical evidence exists and the welfare benefits appear to be mixed. For example, on the one hand (micro) digital finance appears to have many advantages such as changing financial behaviors and lifting about two percent of the population out of poverty in Kenya (Suri and Jack, 2016), facilitating risk sharing (Jack and Suri, 2014) or helping smooth income shocks (Riley, 2018). On the other hand, other evidence suggests that both repayment rates for digital credit in Kenya and Tanzania (Kaffenberger et al., 2018) and savings rates in a mobile banking setting in the Philippines (Harigaya, 2017) are lower than their cash counterparts.

An enhanced understanding of the behavioral benefits and costs of cash and other means of transactions will help design better policies, not only in developing countries. This study contributes to building this knowledge base, providing a clean test of cash effects in a relevant setting with an easy-to-adopt design for laboratory and field settings.

Declaration of Competing Interest

The author declares that she has no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2021.06.008.

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[31] An alternative interpretation of these findings would be that this channel is exactly counterbalanced by the other three potential channels. Given that there is no treatment effect heterogeneity with any of the variables that might interact with *CiH* or that would be indicative of one of the discussed channels, it seems more plausible that a *CiH* is absent my setting.

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