

# Evaluation\*

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## Abstract

Neat and succinct abstract right here...

## 1 Introduction

## 2 Methods

While we were unable to pre-register the analysis because we have had access to and been working with the Money Dashboard data for months, we proceeded in the same spirit: we first wrote a draft of the paper in the form of a pre-analysis plan, following Olken (2015), then tested the entire code base – data pre-processing, balance checks, main analysis, and extensions – with a 1 percent sample, and finally ran the entire analysis.

### 2.1 Dataset

Limitations:

- We have more data for users that signed up later. So average user in the study is not the average MDB user. If time of signup is mainly driven by financial savyness, then study sample is closer to overall population than MDB sample (if we rank groups as early joiners > late joiners > never joiners in terms of financial sophistication). If, however, signup reflects something like openness to newness, then it's not necessarily correlated with financial savyness. Either way, we might ignore it for now. We could test whether behaviour differs between early or late adopters, but that doesn't seem important enough.

### 2.2 Sample selection

To assess the impact of app use on users' financial behaviour we need to observe their relevant financial history for a sufficiently long period of time prior to and after signup. For our purpose here, "relevant financial history" includes the complete set of spending transactions and all savings account inflows and outflows, and "sufficiently long period" is a period of 6 months prior to and after signup, with the month of signup being the first month of the latter period.<sup>1</sup>

Table 1 provides an overview of the precise conditions we applied to implement these criteria and their effect on the sample size. The set of functions that implement each condition can be found on [path to github](#).

### 2.3 Treatment

A user changes treatment status from untreated to treated when they start using the app. Figure 1 shows the treatment history for 200 randomly selected users.

### 2.4 Outcomes

For a more nuanced understanding of how app use affects savings we also consider net-savings – total savings account inflows minus outflows – as a proportion of monthly income to see

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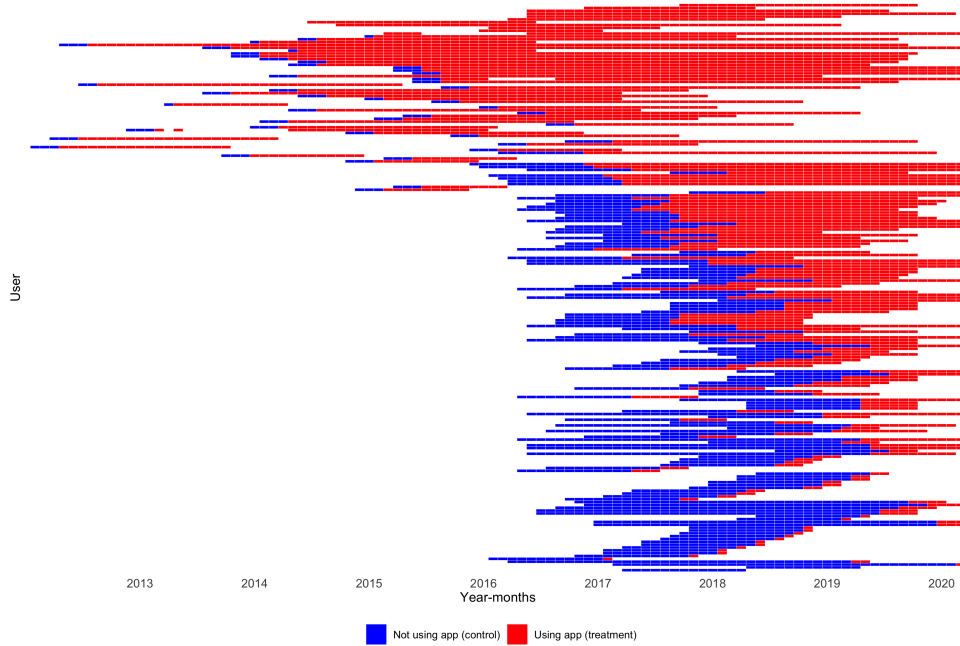
<sup>1</sup>In Appendix B.4 we show results with different window lengths. **The results are unchanged.**

Table 1: Sample selection

	Users	User-months	Txns	Txns (m£)
Raw sample	27,329	803,012	67,088,876	12,722
Annual income of at least £5,000	9,338	256,891	24,582,995	4,463
At least one savings account	5,579	167,705	16,952,445	3,249
At least 12 months of data	4,884	163,602	16,657,929	3,207
Monthly spend of at least £200	2,506	82,860	9,303,691	1,823
Final sample	2,506	82,860	9,303,691	1,823


Notes: Number of users, user-months, transactions, and transaction volume in millions of British Pounds left in our sample after each sample selection step. Link to sample selection code: [🔗](#).

Figure 1: Treatment assignment plot



Notes: Each horizontal line shows for one of 200 randomly selected users the observed pre and post signup periods in blue and red, respectively. The faint vertical white lines indicate month borders, whitespace indicates periods in which we do not observe the user. To the left of the observed period, this is because the app cannot access data before that point when the user signs up; to the right, because they have stopped using the app.

Table 2: Outcome description

Outcome (name in dataset)	Definition	Rationale
Primary outcome		
Net savings ( <i>sa_netflows_norm</i> )	Inflows into minus outflows out of all of a user’s savings accounts divided by monthly income. To capture only “user-generated” flows, we exclude interest and “save the change” transactions, as well as transactions of less than £5 in absolute value. Monthly income and raw inflows and outflows are winsorised at the 1 percent level. 	We focus on net inflows to capture effective savings.
Secondary outcomes		
Positive net savings dummy ( <i>has_pos_sa_netflows</i> )	Dummy equal to 1 if there were positive net savings (as defined above).	Captures extensive margin of savings (change in number of deposits).
Positive net savings ( <i>pos_sa_netflows</i> )	Equal to net savings if there were positive net savings.	Captures intensive margin of savings (change in deposit amount).
...add from text below		

whether a willingness to save more might be offset by a (later) need to withdraw funds, and a dummy variable for whether a user has any savings account inflows in a given month to see whether the app helps users save at all. To investigate possible channels, we consider total spend, highly discretionary spend, banking charges, the total amount of borrowing, as well as payday borrowing, all as proportion of monthly income.

**Adjusting for multiple hypothesis tests** We think of our secondary outcomes as exploratory and do not make any adjustments for multiple hypothesis testing.<sup>2</sup> An alternative approach, based on Anderson (2008), would be to group outcomes into “savings”, “spending”, “borrowing”, and “fees”, and consider them as different dimensions of a latent variable of interest which we might call “financial management skills”. We do not do that for two reasons: first and foremost, because we think it is natural to think of the amount saved as the ultimate outcome and of other outcomes as providing a more nuanced understanding of savings behaviour or as suggesting possible channels through which app use affects savings. Thinking of savings as the main goal is also reflected in Money Dashboard’s main promise, which is to help users spend less and save more, as shown in Figure 6. Second, as pointed out in Carlin et al. (2017), incurring overdraft fees is not an unambiguous sign of a financial mistake, as the opportunity to go into overdraft confers a benefit to the consumer.<sup>3</sup>

<sup>2</sup>For a recent game-theoretically motivated discussion of when and how to correct for multiple hypothesis testing, see Viviano et al. (2021).

<sup>3</sup>For further discussions on fees, see Jørring (2020) and Stango and Zinman (2009).

## 2.5 Covariates

- Motivation to save more (change in savings prior to app use? opening of savings account)

## 2.6 Difference-in-difference

Control group design:

- We only have data for a self-selected group of people who choose to use Money Dashboard. This has a couple consequences:
  - By virtue of signing up to an app that helps them manage their money, these users are different from those who don't sign up. As a result, we are unable to answer the question of whether use of Money Dashboard helps the average person in the population as a whole save more.<sup>4</sup> Instead, we are answering the question whether Money Dashboard succeeds in helping its *users* save more.
  - Something precipitated users to sign up at a particular point in time but not before. To the extent that the decision to sign up is linked to a desire to save more, “desire to save more” is a confounding effect we cannot directly control for. We try to address this to some extent by trying to control for signs of motivation for savings (e.g. starting to save more even before app use)
- Money Dashboard can access up to three years of historic data for each account a user links to their account.
- Each user for whom we have sufficient data thus serves as both a treatment unit and a potential control unit.
- We use a difference-in-differences design to estimate the effect of app use. Because we do not have a separate control group, we use the per-signup data of Money Dashboard users as control periods and use matching to find comparable control user for each treatment user.
- To do this, we use the matching estimator for panel data proposed by Imai, Kim, and Wang (2021). Following paper, we conduct the following steps:
  - For each treated observation, we find a set of control observations with that share the same treatment history for a period of  $L$  periods before the treatment and  $F$  periods after the treatment. In our baseline specification, we rely on a year's worth of data around the treatment period and set  $L = 6$  and  $F = 0, 1, 2, 3, 4, 5$ .
- Identification assumption is that potential outcomes only depend on treatment status of the past  $L$  periods. In general, this means that if treatment has a cumulative effect over time, the full effect is reached after  $L$  periods. In our context, this means that any effect on

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<sup>4</sup>One way to get closer to that answer is to re-weight our sample on observable demographic variables so as to match the UK population as a whole. But our sample differs from the population as a whole both in ways that are observable (demographic variables) and unobservable (self-awareness that they need help managing their money, cognitive resources to engage with the app, motivation to do so). Re-weighting would only help us deal with the first of these.

savings behaviour from using the app is fully realised after  $L$  periods. (I think this means that if we look at the treatment effect for  $F$  periods forward, the effect should not become stronger after  $F = L$ ).

- the once implemented.
- Selection of covariates: all variables that simultaneously affect treatment and outcomes. No need to control for fixed effects: these capture unobserved time-invariant factors that make an individual sign up to MDB and affect its spending habits. Given that these are time invariant, and that all users eventually sign up, there is no difference between control and treatment units in these factors. Month of year: should probably include, as can affect p of signup and spending behaviour.

DiD identification assumptions:

- No spillover effects: the potential outcome of unit  $i$  at time  $t$  is independent of the treatment status of other units. This is violated if a user's partner or friends also use the app and, through sharing their experiences or motivations, influence the user's savings behaviour.
- Carryover effects no longer than  $L$  periods: a user's potential outcome in period  $t$  is independent of treatment status in periods more than  $L$  periods ago. Given that we are dealing with an absorbing treatment, this is not a very strong assumption in our context, and we choose  $L$  based on what we think is an informative number of periods to observe pre-app use behaviour.<sup>5</sup>.
- Parallel trends: the spending trajectory between treated and control units would have continued to be parallel if the treated unit hadn't started using the app. This is violated whenever an intended change in savings behaviour also provided the impetus for the user to start using the app, which is likely to occur frequently. To the extent this is the case, we have an omitted variable "motivation to save more", which both changes the user's savings behaviour and their treatment status. Because of this, what we are measuring is not a pure ATT of app use – the effect of app use on savings over and above the change precipitated by a change motivation – but the effect of app use for users motivated to save more.

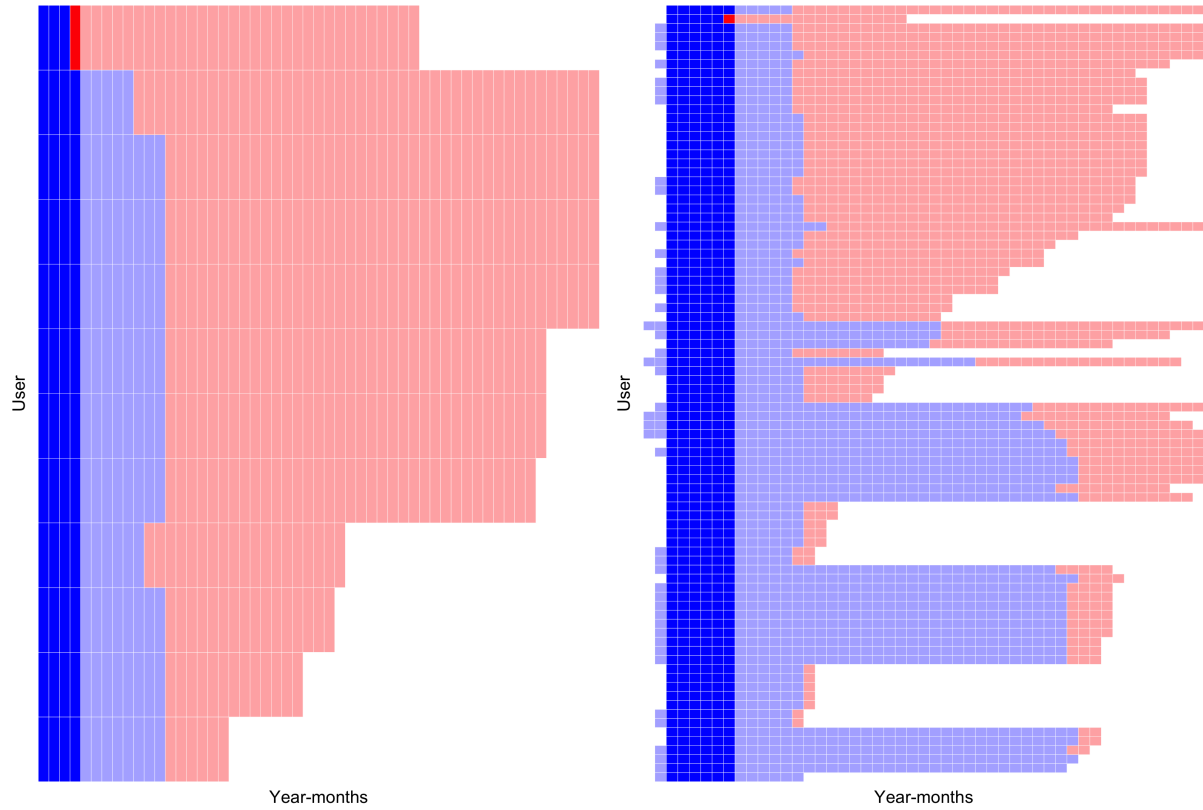
Is estimate causal?

- King and Zeng (2006) show that there are four sources of bias (omitted variable, post-treatment, interpolation, extrapolation).
- Discuss each in turn to argue that effect is causal (for our population of interest, which are people signing up to MDB).

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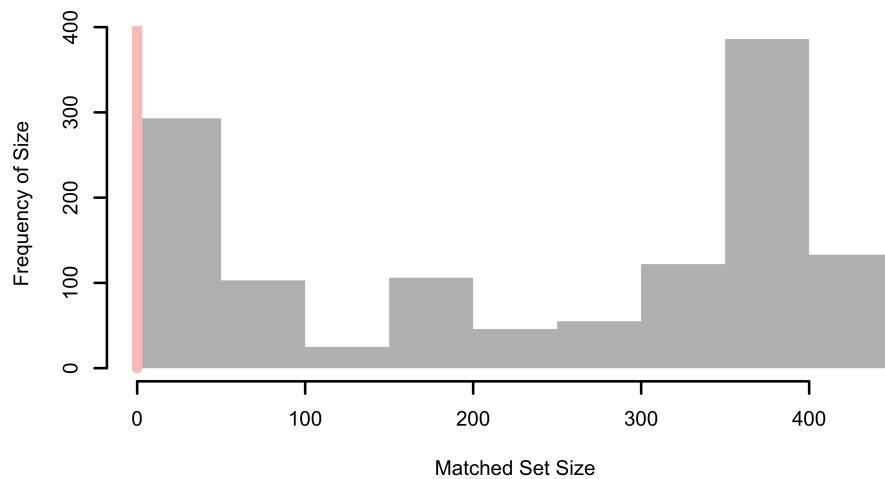
<sup>5</sup>An absorbing treatment is one that cannot be reversed, and hence we only change from untreated to treated once.

Figure 2: Match set examples



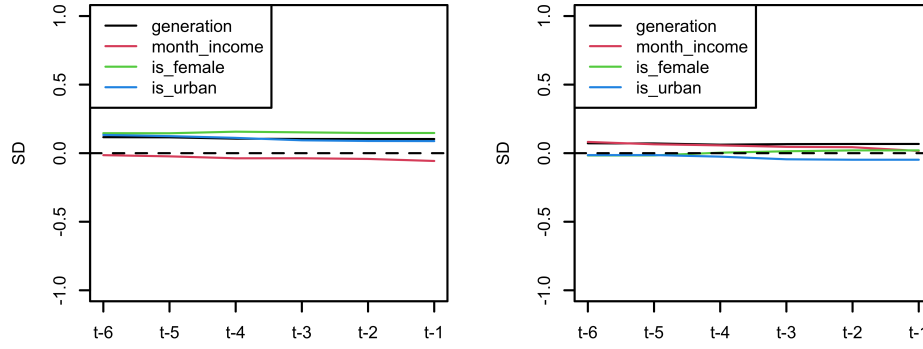
Notes:

Figure 3: Distribution of size of matched control units



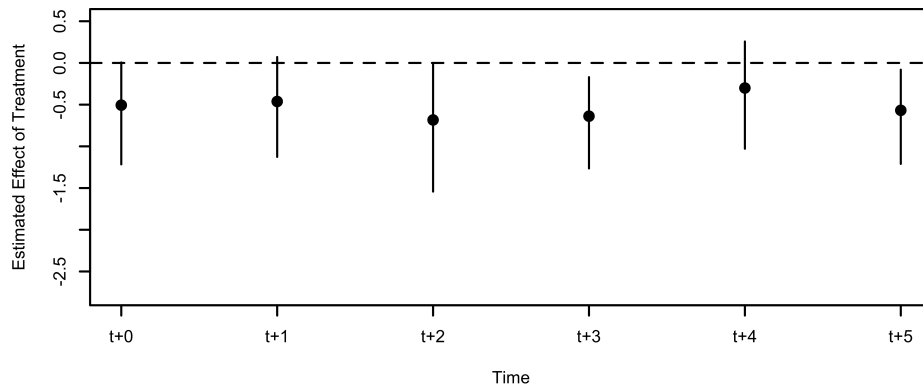
Notes: In the first step of the matching procedure, each user gets assigned a set of potential control users that share the same treatment history for a specified number of periods before the user signs up to teh app (6 months, in our baseline specification), but that do not sign up themselves for a specified number of periods after the treatment user has signed up (another 6 months, in our baseline specification). The figure shows the distribution of the sizes of these sets of potential control users. The pink vertical bar on the left shows to count of users for whom no control users could be found.

Figure 4: Covariance balance



Notes: Average covariate standard deviation between treatment and control units for each pre-treatment period using the entire set of potential controls on the left and, on the right, the refined set of controls, which, in our baseline specification, consists of the nearest neighbour match based on the propensity score.


Figure 5: Matching estimates



Notes:



## 2.7 Code access

We provide links to code that creates key elements of the paper such as variable definitions and sample selection directly in the relevant places in the paper so they can be accessed conveniently. The links are indicated with the GitHub logo, . The hope is that this helps the curious reader clarify questions about subtleties they might have while reading the paper. The complete projects GitHub repo is at [https://github.com/fabiangunzinger/mdb\\_eval](https://github.com/fabiangunzinger/mdb_eval).

## 3 Results

Main results: Figure and associated table akin to sakaguchi2022default Figure 2 and Table 6.

### 3.1 Descriptive analysis

### 3.2 Difference-in-difference analysis

### 3.3 Subgroups

To analyse which groups benefit most from adopting Money Dashboard, we split our sample by gender, generation, income quartiles, and pre-adoption savings behaviour.

We define generations as follows: boomers were born between 1946 and 1964, Gen X between 1965 and 1980, Millennials between 1981 and 1996, and Gen Z after 1997.<sup>6</sup>

Subgroup analysis: same Fig an Tab as in main analysis, but with line for each subgroup. One figure for each of: gender, generations, income terciles, per-adoption average savings tercile (inspired by Carlin et al. (2017), see Fig 5 and Table 4).

See also section 6 in Gargano and Rossi (2021)

## 4 Discussion

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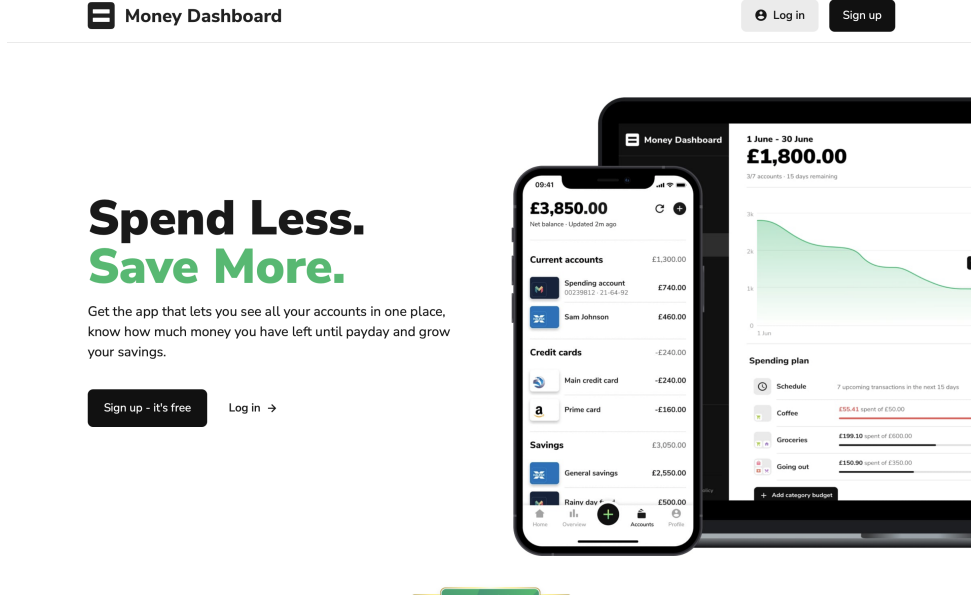
<sup>6</sup>Based on age ranges provides by [Beresford Research](#).

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## A Money Dashboard application

Figure 6: Money Dashboard website screenshot



Notes: Screenshot from the top of the Money Dashboard website, at [moneydashboard.com](https://moneydashboard.com), accessed on 29 April 2022.

## B Alternative specifications

### B.1 Static TWFE

$$y_{it} = \alpha_i + \lambda_t + \beta T_{it} + \gamma X_{it} + \epsilon_{it} \quad (1)$$

- Comparison: pre vs post signup within each individual.
- Assumption: there are no time-varying unobserved effects that affect both  $y$  and  $T$ .
- Discussion: there is something that made the individual sign up in the first place, and it might well be an individual level shock that we don't observe (unexpected large expense, loss of job, etc.)
- See Imai and Kim (2021) for problems with twfe

### B.2 Dynamic TWFE

$$y_{it} = \alpha_i + \lambda_t + \sum_{s=-6}^5 \beta_s T_{its} + \gamma X_{it} + \epsilon_{it} \quad (2)$$

- See Sun and Abraham (2021) for problems with that and compare to their approach as implemented in fixest.

### B.3 Alternative window lengths

Show results for 12 months on either end.

#### **B.4 Alternative matching method**

#### **B.5 Post treatment periods as control**

#### **B.6 Synthetic controls**

See Abadie and L'Hour ([2021](#)) for how to use synthetic controls for disaggregated data.

#### **B.7 Classic two-way fixed-effects model**