

# Do Money Dashboard users spend less and save more?

Fabian Gunzinger\*

Warwick Business School

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## Abstract

In this paper, I test whether using Money Dashboard is associated with a reduction in discretionary spending and an increase in emergency savings. I find that users reduce their discretionary spend by between £100 and £150 (11-17% of average discretionary spend) once they start using the app and sustain that reduction throughout the six-month post-signup period I consider. In contrast, I cannot find an increase in short-term or long-term savings. Looking at disaggregated measures of discretionary spend further suggests that the reduction in spend is the result of maintained month-to-month changes in behaviour rather than one-off cancellations of direct-debit transactions, that it results from reductions across a number of spending categories, and that it is a result of changes along the extensive rather than the intensive margin – users reduce the number of transactions they make rather than the average transaction value. The two main limitations of my approach that prevent me from interpreting my results as causal effects are the self-selection into using Money Dashboard and the inability to disentangle the effects of different features of the app.

## 1 Introduction

This paper evaluates whether using Money Dashboard, a UK-based financial aggregator app, is associated with a reduction in discretionary spend and an increase in emergency savings – short-term savings intended as a buffer against unexpected financial shocks.

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\*[fabian.gunzinger@warwick.ac.uk](mailto:fabian.gunzinger@warwick.ac.uk). I thank Neil Stewart for invaluable support and encouragement, and Redzo Mujcic and Zvi Safra for helpful comments.

The question is important because a large number of adults in the UK and the US do not have enough savings to cover unexpected expenses like car or medical bills: in the UK, 25 percent of adults would be unable to cover an unexpected bill of £300 (Phillips et al. 2021), while in the US, about 30 percent would be unable to cover a \$400 bill (Governors of the Federal Reserve System 2022).

A large literature has documented a variety of factors that can influence undersaving and financial decision-making more generally: time-preferences and present bias (Laibson 1997, Frederick et al. 2002, Read et al. 2018, Ericson and Laibson 2019, Cohen et al. 2020), inertia (Madrian and Shea 2001), over-extrapolation (Choi et al. 2009), limited self-control and willpower (Thaler and Shefrin 1981, Benhabib and Bisin 2005, O'Donoghue and Rabin 1999, Fudenberg and Levine 2006, Loewenstein and O'Donoghue 2004, Gul and Pesendorfer 2001), cognitive limitations and financial literacy (Agarwal, Driscoll, et al. 2009, Agarwal and Mazumder 2013, Korniotis and Kumar 2011, Agarwal, Amromin, et al. 2010, Fernandes et al. 2014, Jørring 2020), attitude towards money and spending (Rick, Cryder, et al. 2008, Rick, Small, et al. 2011), one's perceived locus of control (Perry and Morris 2005), degree of optimism (Puri and D. T. Robinson 2007), the ability to frame decisions broadly rather than narrowly (Kumar and Lim 2008), propensity to gamble (Kumar 2009), one's social network (Bailey et al. 2018, Kuchler and Stroebel 2021), the degree of one's financial planning (Ameriks et al. 2003), and habits (Blumenstock et al. 2018, Schaner 2018, De Mel et al. 2013).<sup>1</sup>

Researchers have also identified a number of tools that can help people overcome some of these biases. Probably the most studied approach is changing default options. Changes in defaults are consequential and often successful because people often stick with the status quo (Samuelson and Zeckhauser 1988), interpret defaults as a recommendation (McKenzie et al. 2006), implicitly view them as reference points they do not want to move away from (Johnson and Goldstein 2003, Kahneman and Tversky 1979), and often end up sticking with them even if they intend to otherwise because of procrastination (Carroll et al. 2009, Ericson 2017). Default options have been applied across range of areas and have, for instance, been found to increase retirement savings contributions (Thaler and Benartzi 2004, Madrian and Shea 2001, Beshears, Choi, Laibson, and Madrian 2009) and organ donations (Johnson and Goldstein 2003, Gimbel et al. 2003, Abadie and Gay 2006).

Another extensively studied approach is the commitment device, whereby an individual restricts their future choice set in order to avoid choosing a self-

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<sup>1</sup>For two thorough reviews of the literature, see Agarwal, Chomsisengphet, et al. (2017) and Greenberg and Hershfield (2019).

defeating action. Not everyone makes use of these devices when they are offered (Bryan et al. 2010) and they do not work in all contexts (Laibson 2015, C. D. Robinson et al. 2018), but they have been found to help individuals increase their savings rates (Ashraf et al. 2006), quit smoking (Giné et al. 2010), make healthier food choices (Schwartz et al. 2014), and exercise more regularly (Royer et al. 2015).

A third approach are implementation intentions, a particular type of planning for the achievement of one's goals that involves "if-then" intentions, such as "if I get paid, then I transfer 10 percent of it into my savings account" (P. M. Gollwitzer and Sheeran 2006, Rogers, Milkman, et al. 2015). Such intentions have been found to support perseverance in pursuing one's goals (Oettingen and P. Gollwitzer 2010) and to increase overall goal attainment across different age groups, life domains, and types of obstacles (P. M. Gollwitzer and Sheeran 2006).

Finally, social norms messaging, whereby people are informed about how their own behaviour compares to that of a relevant peer group, has been successful in inducing high-energy-use households to lower their energy consumption (Schultz et al. 2007, Allcott 2011, Allcott and Rogers 2014, Brandon et al. 2017). Such information can be especially useful for domains where people usually are uninformed about the behaviour of their peers and do thus not have a reference point against which to calibrate their own behaviour – a situation most people find themselves in when it comes to discretionary spending and saving. However, social norms messaging can also backfire and has been found to lower participation in pension savings plans (Beshears, Choi, Laibson, Madrian, and Milkman 2015) and completion rates of an online course (Rogers and Feller 2016), making it important to test messages before deploying them on a large scale to avoid unintended consequences.

FinTech aggregator apps such as Money Dashboard are a promising mean to help people manage their spending and savings for two reasons: eventually, as these apps become more sophisticated, they can be used to implement and test the above mentioned tools at a large scale, and thus potentially bring their benefit from research labs or specifically designed tools used by a small minority to the broader population. In the meantime, they provide additional features that – on their own – can help people manage their finances more successfully. In particular, they provide easy access to financial information that makes it easier to monitor one's spending and saving, and often also offer tools such as budgeting and the setting of spending goals. Financial information that is more easily accessible and is aggregated in ways that help people keep track of their goals might be beneficial because rational inattention theory predicts that it makes people more likely to access that information, which, in turn, might lead to

better consumption decisions Brunnermeier and Nagel (2008), DellaVigna (2009), and Sims (2003). Tools that help with budgeting and with setting spending goals have the potential to help users make consumption decisions more in line with their intentions because such tools can act as commitment devices that – if users experience disutility from falling short of their goals – introduce a cognitive cost to overspending or undersaving.

In this paper, I specifically test whether using Money Dashboard – an aggregator app that offers aggregation of all financial accounts and budgeting features – is associated with a reduction in discretionary spending and an increase in emergency savings. I use a new estimator proposed by Callaway and Sant'Anna (2021) that corrects for recently identified problems in two-way fixed effects estimates.

I find that users reduce their discretionary spend by between £100 and £150 per month (11-17% of average discretionary spend) once they start using the app and sustain that reduction throughout the six-month post-signup period I consider. Looking at disaggregated measures of discretionary spend further shows that the reduction is the result of maintained month-to-month changes in behaviour rather than one-off cancellations of direct-debit transactions, that it results from reducing spending on a number of different categories of purchases rather than a single one, and that it is a result of changes along the extensive rather than the intensive margin – users reduce the number of transactions they make rather than the value of the average transaction. Interestingly, users do not seem to use these additional funds to build up emergency savings: net-inflows into savings accounts do not change after signup. I can also neither find significant increases in flows into investment and pension accounts or additional savings accounts that are not linked to the app, nor additional loan repayments. This suggests that users either leave unspent funds to accrue in their current accounts (which would not count to my definition of savings), or allocate them across a number of different uses in small amounts I am not powered to detect.

There are two main limitations to my approach. First, the data is not generated by a randomised experiment. The gold-standard to evaluate whether use of Money Dashboard improves financial outcomes would be a randomised controlled-trial, where out of a sample of potential users (ideally random and representative of the UK population), we would randomly grant access to the app to some users and then compare outcomes of those treated users with the control group of users who did not have access. Instead, the data I have access to only contains data for individuals who self-selected into using the app. Individuals will choose to do so for a number of different reasons, all of which are unobserved in the data, and at least some of which would probably have changed their financial outcomes even if they had not signed up to the app. Any changes in financial outcomes we observe

are thus “aggregate” or “net” effects of these unobservables and the “pure” causal effect of app use.

To see this, think of the net effect as  $\text{net effect} = \text{causal effect} + \text{“need”}(\downarrow) + \text{“motivation”}(\uparrow)$ , where the arrows indicate the direction of the bias, and consider three cases that illustrate three stylised but plausible scenarios for signup. First, consider a user who signs up in the hope that the app will help them reign in discretionary spending that has gotten out of hand. If it takes the user some time to fully adjust their spending, then even if the app does help them make these adjustments, the estimated positive effect of app use will be biased downward. Next, consider a user who decides to start bringing their own lunch to work instead of eating out in an effort to save for a new car and signs up to MDB in the hope that the app will help them keep track of their spending. Such a user would probably have reduced their discretionary spend even if they had not signed up to the app, thus creating an upward bias on our estimated net effect. Finally, consider a user who signs up to MDB purely because they happened to see an advert for the app on the Bus and got curious. In this case, we can think of signup being close to random – almost as if the user had been allocated to the treatment group in our ideal experiment – and the estimated net effect will closely resemble the causal effect of app use. Hence, under the weak assumption that at least some users sign up for reasons that are not as good as random, our estimated effects will be biased upwards or downwards depending on the relative proportion of users whose unobservable reasons for signup create an upward and downward bias.

The second limitation is that even if I were able to isolate the effect of the app, I am not able to differentiate between the contributions of different features of the app such as improved access to information and budgeting.

Despite these limitations, which mean that we cannot interpret results as causal effects of using Money Dashboard, the finding that app use is associated with an economically significant and sustained drop in discretionary spend, and the finding that this reduction comes about by users making fewer transactions in a number of different spending categories month-by-month provides interesting insight of how users reduce discretionary spend, either because of or simply while using Money Dashboard. Furthermore, the results make it very plausible that Money Dashboard, and possibly apps like it, does have a positive causal effect, and thus suggests that further research, focused on identifying the causal effect of the app overall as well as that of its component parts, would be interesting and worthwhile.

My work mainly contributes to three strands of the literature. The first is the nascent literature that studies the effect of FinTech apps on financial outcomes

suggests. This literature suggests that these apps can indeed lead to improved financial outcomes through a number of different channels: providing users with information about their spending relative to peers has been found to reduce spending (D'Acunto et al. 2020); offering budgeting options, to reduce spending (Lukas and Howard 2022); goal setting, to increase savings (Gargano and Rossi 2021); and facilitating access to financial information, to reduce non-sufficient funds fees (Carlin et al. 2022) and discretionary spend (Levi and Benartzi 2020).

The two studies that are most closely related to my work are Lukas and Howard (2022) and Gargano and Rossi (2021). Lukas and Howard (2022) also use data from Money Dashboard to study the effect of budgeting on discretionary spend. In line with my finding, they find that budgeting reduces discretionary spend in the associated categories and that this effect persists throughout the six-month post-budgeting period they study. Their approach also suffers from the two main limitations of my paper discussed above: budgeting is not randomly assigned, and they cannot isolate the effect of budgeting from other MDB features that might help users reduce their spending. There are two main differences to my work: first, Lukas and Howard (2022) do not study the effect of MDB use on savings. Second, they use data from between January 2014 and December 2016, and do not select their sample to ensure that it only contains users for whom they can observe all financial accounts. As I discuss in Section 2.2, there is a large number of users in the raw dataset for whom we are unlikely to observe all transactions, making careful sample selection critical to ensure that observed changes in behaviour do not merely reflect a shift of transactions between observed and unobserved accounts. Gargano and Rossi (2021) study the effect goal setting on savings and find that setting savings goals does increase savings rates. Their approach does not suffer from the two limitations of my paper since they exploit the random assignment of some users into a group of beta testers that can set savings goals, and because the savings app that provides the data for their study was initially designed as a simple savings app and provides no additional features that could plausibly influence savings behaviour. However, the flip-side of being able to cleanly identify the effect of goal setting is that most apps that people use to increase their savings or reduce their spending do – like Money Dashboard – have multiple features that might alter financial behaviour, so that studying their joint effect is of interest, too.

The second strand of related literature is the very recent literature on studying interventions to help increase emergency savings an area of household finances that has until recently had no attention. In addition to studies testing the effect of FinTech apps on savings, there is a strand of research that studies the use of auto-enrolment into employer-sponsored savings accounts – similar to the ones

used to increase pension savings (Thaler and Benartzi 2004, Choi et al. 2004, Choukhmane 2019) – and finds an increase in both participation (relative to opt-in accounts) and account balances (Beshears, Choi, Iwry, et al. 2020, Berk et al. 2022).

Finally, my work also contributes to a rapidly growing literature of using financial-transaction data from banks or financial aggregator apps to understand consumer financial behaviour. As already mentioned, Kuchler and Pagel (2020) use data from a financial aggregator app to estimate time preferences. Similar data has been used to show that consumer spending varies across the pay cycle (Gelman et al. 2014, Olafsson and Pagel 2018), to test the consumer spending response to exogenous shocks (S. R. Baker 2018, Baugh et al. 2014), and to better understand the generational differences in financial platform usage patterns (Carlin et al. 2019). Some researchers use transaction-data directly provided by banks. Ganong and Noel (2019) show that consumer spending drops sharply after the predictable income drop from exhausting unemployment insurance benefits, Meyer and Pagel (2018) analyse how individuals reinvest realised capital gains and losses, and Muggleton et al. (2020) show that chaotic spending behaviour is a harbinger of financial distress.<sup>2</sup>

The remainder of this paper is organised as follows: Section 2 introduces the dataset used, discusses preprocessing and presents summary statistics; Section 3 introduces the empirical approach used in the analysis; Section 4 presents the results; and Section 5 concludes. To make it easier for interested readers to clarify questions about details and subtleties of data preprocessing and analysis steps, I provide links to the scripts that implement the steps discussed in the text in the relevant places throughout the text.<sup>3</sup>

## 2 Data

### 2.1 Dataset description

I use data from Money Dashboard (MDB), a UK-based financial aggregation app that allows users to link accounts from different banks to obtain an integrated view of their finances in order to help them to reduce their spending and increase their savings. Figure 1 shows a screenshot from MDB’s website (left) and an example of its user interface (right). The complete dataset contains more than 500 million transactions made between 2012 and June 2020 by about 270,000

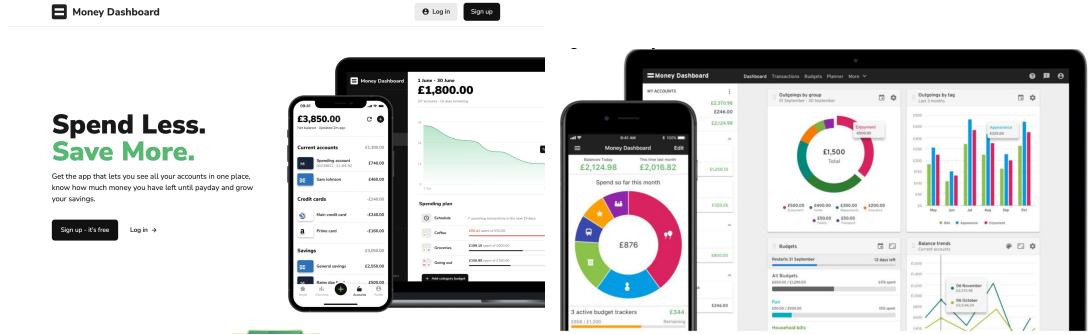
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<sup>2</sup>For a comprehensive review of the literature using financial transaction data, see S. R. Baker and Kueng (2022).

<sup>3</sup>The projects GitHub repo that contains all files used to produce the results can be found at [https://github.com/fabiangunzinger/mdb\\_eval](https://github.com/fabiangunzinger/mdb_eval).

users, and provides information such as date, amount, and description of the transaction as well as account and user-level information.

Figure 1: Money Dashboard website and user interface



Notes: Screenshot from the top of the Money Dashboard website (left) and example of its user interface (right), illustrating some of the transaction grouping and budgeting functions.

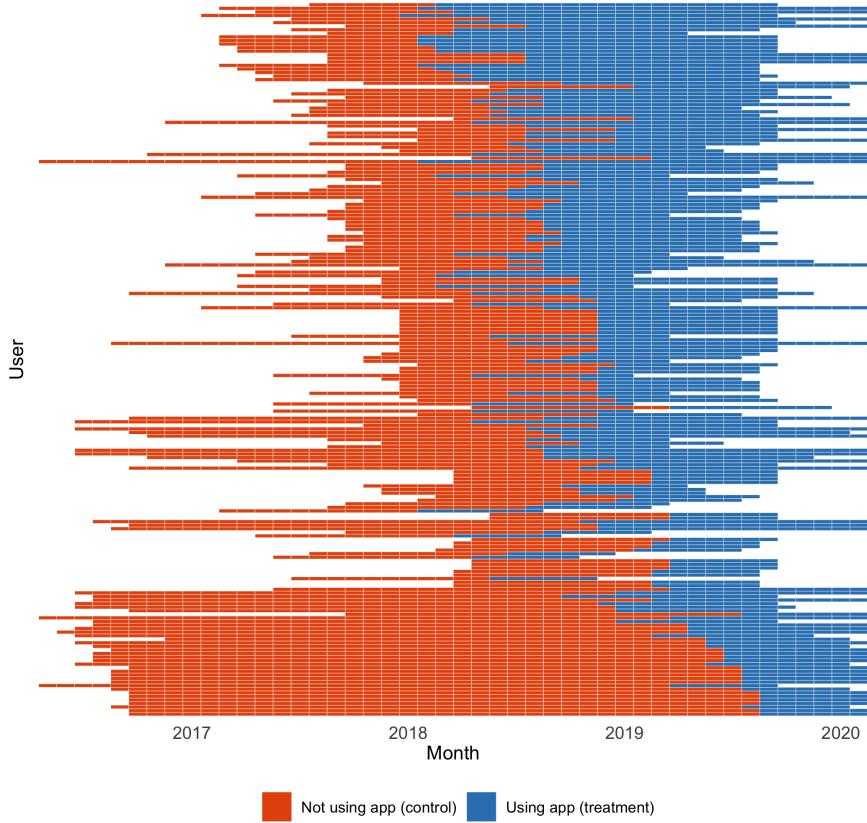
Crucially, for this paper, MDB can access up to three years of historic data for each linked account. Figure 2 shows how we can leverage pre-signup data provided in the MDB data to construct valid control groups. Each row of cells shows data for one of 200 randomly selected users, with red cells indicating data from pre-signup months and blue cells indicating data from post-signup months. Signup thus occurred sometime during the first blue coloured month, and we can use users who have not yet signed up (whose cells are still red) as a comparison group. In my analysis below, I will use this data structure to compare the outcomes of users who start using the Money Dashboard app at a particular point in time with users who have not yet started using the app at that time.

The data's main advantage for the study of consumer financial behaviour is that it allows us to observe all savings and spending transactions for users who linked all their financial accounts. This is important since it means that we can be sure that any changes we observe in some of a user's account are not offset by changes in unlinked accounts.<sup>4</sup> Furthermore, data is collected automatically and in real-time rather than through surveys that collect data with a time-lag and often rely on consumer's ability and willingness to provide accurate information.

The data's main limitation is that because users's self select into using MDB, the sample is not representative of the wider UK population: it is well documented that FinTech app users are more likely to be male, younger, and higher-income earners than the average person (Carlin et al. 2019). Also, as pointed out in Gelman et al. (2014), a willingness to share financial information with a third party might not only select on demographic characteristics, but also for an increased need for financial management or a higher degree of financial sophistication. How-

<sup>4</sup>For an example where debt-reduction in one set of accounts is offset by an increase in debt by another set of account, see Medina (2021).

Figure 2: Pre and post-signup data availability



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

ever, because, as discussed in the introduction, the aim of this paper is to assess the effect of MDB on people who choose to use it, the lack of representativeness is not an issue.<sup>5</sup> A second limitation is that while we can observe user's complete financial behaviour if they add all their financial accounts to the app, it is not trivial to distinguish between users who do and do not do that. I address this challenge in the sample selection process documented below. A third issue is that while the app is able to classify many transactions into types, it misclassified some transactions and cannot classify others altogether. I address this as part of the cleaning process documented below.

## 2.2 Preprocessing

**Data cleaning:** I use the dataset described above for a number of projects, and perform a number of steps to create a minimally cleaned version of the

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<sup>5</sup>For an example of how re-weighting can be used to mitigate the non-representative issue, see Bourquin et al. (2020).

dataset that is the basis for all such projects. These steps are performed in a dedicated data repository and not run as part of this project.<sup>6</sup> Here, I briefly describe the main cleaning steps and their rationale. I drop the fewer than 1 in 1000 transactions with a missing description string because these cannot be categorised. I also group transactions into transfer, spend, and income subgroups, following Muggleton et al. (2020) to define spend subgroups and Hacıoğlu-Hoke et al. (2021) to define income subgroups.<sup>7</sup> Finally, I classify as duplicates and drop transactions with identical user ID, account ID, date, amount, and transaction description. This will drop some genuine transactions, such as when a user buys two identical cups of coffees at the same coffee shop on the same day. However, data inspection suggests that in most cases, we remove genuine duplicates.

To minimise the influence of outliers, I winsorise all variables at the 1 percent level or – if we winsorise on both ends of the distribution – at the 0.5 percent level.<sup>8</sup> I rely on winsorisation (replacing top values with percentile values) instead of trimming (replacing top values with missing values) because data inspection suggests that in most cases, very large (absolute) values are not the result of data errors, which would call for trimming, but reflect genuine outcomes, which makes winsorising appropriate because it leaves these observations in the data while lowering their leverage to influence results.

**Sample selection:** The three main goals of sample selection are to select a sample of users for whom I can be reasonably certain to observe all relevant financial accounts,<sup>9</sup> account histories of at least 12 months, and who are not using MDB for business purposes. Table 1 lists the precise conditions I apply to implement these criteria and their effect on sample size.<sup>10</sup>

In my main analysis, I show effects of app use for the 12-months period from 6 months before and 5 months after MDB signup, treating the month of signup as period 0. To ensure that results are not affected by the number of accounts we observe for an individual, it is thus critical that we can observe at least 12 months of history for all accounts a user adds to the platform. This ensures that in the extreme case where a user adds an account they had used for some time in the fifth month after they signed up, I observe all transactions on that account throughout the 12-month period of interest. To see why this is important, consider a user who has made monthly payments of £100 into a savings account for two years by the time they sign up to MDB but links that account only five

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<sup>6</sup>The module with all cleaning functions is available on [Github](#).

<sup>7</sup>The precise list used to classify transactions is available on [Github](#).

<sup>8</sup>The code that performs the winsorisation are available on [Github](#).

<sup>9</sup>Relevant accounts include all current, savings, and credit-card accounts, but exclude long-term savings and investment accounts.

<sup>10</sup>The code that implements the selection criteria is available on [Github](#).

Table 1: Sample selection

	Users	User-months	Txns	Txns (m£)
Raw sample	271,856	7,948,520	662,112,975	124,573
Drop test users	270,782	7,878,398	656,047,534	122,887
App signup after March 2017	88,368	2,320,421	202,580,838	38,816
At least one savings account	50,226	1,334,328	125,841,337	26,645
At least one current account	48,794	1,303,164	123,468,715	26,263
At least £5,000 of annual income	20,647	541,746	55,857,451	11,760
At least 10 txns each month	14,229	369,944	40,662,904	8,529
At least £200 of monthly spend	10,438	272,228	31,529,498	6,837
No more than 10 active accounts	9,788	248,975	27,589,696	5,426
Complete demographic information	7,720	202,633	22,671,753	4,378
Working age	7,568	197,951	22,279,279	4,213
Final sample	7,568	197,951	22,279,279	4,213

Notes: Number of users, user-months, transactions, and transaction volume in millions of British Pounds left in our sample after each sample selection step.

months after joining. In this case, we can only observe the historical payments if MDB can access at least 12 months of history. If this were not the case, and MDB can only, say, access 6 months of history, we would erroneously conclude that the user started saving £100 starting in the month they signed up. Data exploration suggests that all major banks start providing 12 months of historical data from April 2017 onwards, which is why include only users who sign up after that data.<sup>11</sup>

To ensure that I can be reasonably certain to observe users who have added all their financial accounts to the app, I restrict our sample to users with at least one savings and current account, with an annual after-tax income of at least £5,000, and a minimum of 10 transactions and a spend of £200 every month. To remove users who might use the app for business purposes, I drop users with more than 10 active accounts in any given month.

Finally, I drop test users – users who signed up to the app before it was publicly available – and users that are not of working age (younger than 18 or older than 65) because these groups might have objectives other than to reduce spending and increase savings.<sup>12</sup> And I retain only users for whom we can observe the complete set of demographic covariates (gender, age, region) to ensure all users can be used throughout the analysis.

These steps do impact the sample size significantly. In fact, while it is not surprising that only considering users that signed up after March 2017 would reduce the sample size considerably, the large reduction in users due to requiring at least one savings account, an annual after-tax income of at least £5,000, and

<sup>11</sup>The Jupyter Notebook containing the data exploration is available on [Github](#).

<sup>12</sup>I cannot identify test users precisely, but drop users who signed up prior to or during 2011, the first year the app was in operation.

at least 10 monthly transactions are surprising, and suggest that the raw dataset contains a large number of users that do not use Money Dashboard as intended. The drastic effect of the selection procedure on sample size also underlines how important careful sample selection is to ensure that we can be reasonably sure to observe all relevant financial transactions for all users in the sample, and thus to be confident that observed changes in financial behaviour do not reflect a shift of transactions between observable and unobservable accounts.

## 2.3 Variable definitions

The main outcome variables in my analysis are *discretionary spend* and *savings*.

**Discretionary spend:** Discretionary spend aims to capture spend transactions over which users have a lot of control. It is calculated solely based on card transactions with cash transactions excluded. This is because cash transactions are recorded in the MDB data as ATM withdrawals, and it is impossible to know what these funds were used for. Furthermore, I do not include gasoline expenses in discretionary spend because many people use their car to commute to work so that it is unclear what proportion of these expenses are discretionary. A list of transaction tags used to identify discretionary spend transactions and the code used to create the variable are available on [Github](#).

**Savings:** Savings are calculated as net-inflows into all of a user's savings accounts that are linked to Money Dashboard. To capture only user-generated savings-account flows and not interest payments and similar automated transactions, I include only transaction with an absolute value of £5 or higher. The code used to calculate savings account flows is also available on [Github](#).

## 2.4 Summary statistics

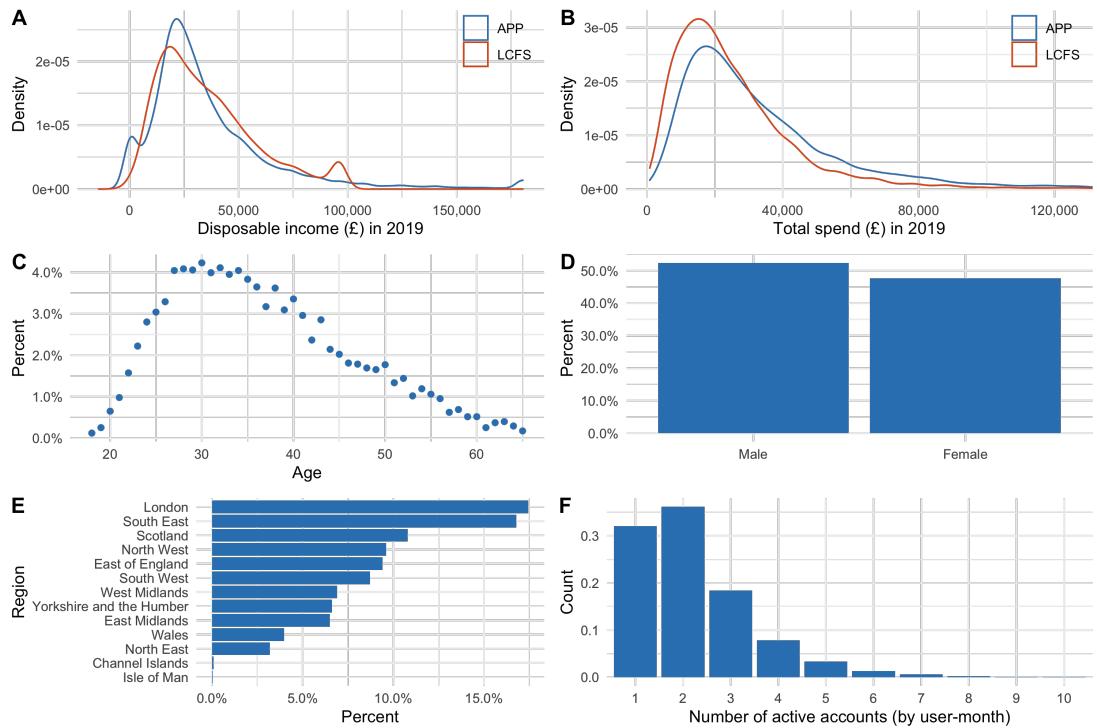
Figure 3 provides a view of some salient sample characteristics. Panels A and B show that while distributions of disposable income and total spend in 2019 broadly mirror that of the ONS Living Cost and Food Survey (LCFS) data, the MDB data tends to slightly underestimate incomes and overestimate spending.<sup>13</sup> Given that our sample is likely biased towards high-earners, as discussed above, we would expect both income and spend to be higher than in the LCFS. Two likely caveats to the data probably create discrepancies relative to the LCFS

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<sup>13</sup>I accessed the LCFS data via the UK Data Service at the following url: <https://beta.ukdataservice.ac.uk/datacatalogue/studies/study?id=8686>.

data. First, as discussed in Section 2.2, I drop unclassified transactions from the data, which biases both income and spending downwards (i.e. spending would probably be even higher compared to the LCFS). Second, it is likely that it is more challenging to automatically classify income transactions than spend transactions, which might create an additional downward bias on the income distribution.<sup>14</sup> Panels C, D, and E show that, as discussed in Section 2.1, the sample is skewed towards users that are younger, male, and live in the South East. Finally, Panel D shows that most users use transact with one or two accounts per month.

Figure 3: Sample characteristics



Notes: Panels A and B show the distribution of disposable income and total spending in 2019, respectively, benchmarked against the 2018/19 wave of the ONS Living Cost and Food Survey (LCFS). The remaining panels show the data distributions of age, gender, region, and the number of active accounts.

Table 2 provides additional summary statistics. It shows, for instance, that the average number of monthly transactions is slightly above 100 (with a mean of 112 and a median of 101), that half of all user-month incomes lie between £1,407 and £3,596, and that inflows into savings accounts closely mirror outflows. It also suggests that discretionary spend accounts for about 30 percent of total monthly spend

<sup>14</sup>Bourquin et al. (2020) present an alternative algorithm to identify income transactions and find that this leads to higher estimated incomes. I do not use their algorithm because I only use disposable income as a covariate to capture relative income differences between users.

Table 2: Summary statistics

Statistic	Mean	St. Dev.	Min	Pctl(25)	Median	Pctl(75)	Max
Txn count	111.87	59.67	10	70	101	142	327
Month income (£)	2,853.49	2,495.24	0.00	1,407.21	2,217.37	3,595.92	15,027.54
Savings account inflows (£)	747.25	2,448.33	0.00	0.00	0.00	400.00	18,809.50
Savings account outflows (£)	762.40	2,422.94	0.00	0.00	0.00	400.00	18,099.38
Savings account netflows (£)	-7.39	2,883.70	-20,000.00	0.00	0.00	50.00	21,675.41
Month spend (£)	2,760.39	2,609.92	200.02	1,225.41	2,016.70	3,318.95	17,092.19
Age	37.46	9.97	18	30	36	44	65
Female dummy	0.44	0.50	0	0	0	1	1
Urban dummy	0.84	0.37	0	1	1	1	1
Active accounts	3.11	1.74	1	2	3	4	10
Discretionary spend (£)	860.63	736.08	0.00	369.34	662.98	1,118.28	4,181.71

### 3 Estimation

We want to estimate the effect of app use over time. Given our data, a natural way to do this would be to use a dynamic two-way fixed effects model that includes user and year-month fixed effects and dummies indicating time since app signup. The estimated coefficients on these dummies are then conventionally interpreted as dynamic treatment effects. However, a series of recent papers in econometric research demonstrate that while this approach is extremely common in applied research, two-way fixed effects models do not produce valid estimates of dynamic treatment effects in most settings (Roth et al. 2022). In particular, in settings with staggered treatment assignment, where units are first exposed to treatment at different points in time (as is the case in our setting), dynamic two-way fixed effects are valid only if there is homogeneity in treatment effects across treatment adoption cohorts. In most settings, including my own, this is a very strong assumption.

The problem is exacerbated in static two-way fixed effects models, which require homogeneity in treatment effects across treatment adoption cohorts and in time since treatment. Goodman-Bacon (2021) shows that the static two-way fixed effect DiD estimator is a weighted average of all possible two-units and two-time-periods difference-in-differences in which one unit changes its treatment status and the other does not. Because this includes “forbidden comparisons”, where already treated units are used as control units, this can lead to a scenario where the treatment effect of some units has a negative weight, unless one makes the additional assumption of time-invariant treatment effects (in which case, the “forbidden” comparisons have an effect size of zero). These negative weights, in turn, can lead to situations where the treatment effect estimate is negative even though all unit-level treatment effects are positive. Sun and Abraham (2021) show that the dynamic specification suffers from very similar problems.<sup>15</sup>

Because of this, I use a new estimator proposed by Callaway and Sant’Anna

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<sup>15</sup>For two excellent reviews of this new literature, see Roth et al. (2022) and A. C. Baker et al. (2022)

(2021), which allows for arbitrary treatment effect heterogeneity across treatment adoption cohorts and time, and allows for the incorporation of a parallel trends assumption conditional on covariates. I implement the estimator using the author-provided R package ‘[did](#)’. In describing the estimator, I follow the approach of Callaway and Sant’Anna (2021) of first defining the causal parameter of interest, and then discussing identification, estimation, and inference, while using the simplified notation used in Roth et al. (2022).

**Causal parameter of interest:** The basic building block of the framework, and the causal effect of interest, is a generalised version of the average treatment effect of the treated (ATT) of the difference-in-differences framework, which the authors call the group-time average treatment effect: the average treatment effect at time  $t$  for the group (or cohort) of individuals first treated at time  $g$ , defined as:

$$ATT(g, t) = \mathbb{E}[Y_{i,t}(g) - Y_{i,t}(0)|G_i = g], \quad (1)$$

where  $Y_{i,t}(g)$  is the potential outcome in time period  $t$  of an individual  $i$  in group  $g$ , and  $Y_{i,t}(0)$  is the (counterfactual) potential outcome of that same individual if they had remained untreated.  $G_i$  indicates the first period in which individual  $i$  was first exposed to the treatment.

A key feature of these group-time average treatment effects is that they allow for heterogeneous treatment effects both across time and across groups. To see this, first consider, for example, the group of individuals first treated in April 2019 (i.e.  $G_i = 2019-04$ , for all individuals in the group) at two different points in time, June and July 2019, say, and notice that Equation 1 allows for  $ATT(2019-04, 2019-06) \neq ATT(2019-04, 2019-07)$ : hence,  $ATT(g, t)$  allows for heterogeneity across time. Similarly, consider also another group of individuals first treated in March 2018, and notice that Equation 1 allows for  $ATT(2019-04, 2019-06) \neq ATT(2018-03, 2019-06)$ : hence,  $ATT(g, t)$  allows for heterogeneity across groups.

**Identification:** These effects are identified if two main assumptions hold: there is limited and known anticipation of treatment, and the assumption of parallel trends between treatment and comparison groups holds either unconditionally or conditionally on a set of covariates. Additional assumptions are (i) that the treatment is absorbing in the sense that once an individual is treated they will remain treated forever, (ii) that individuals in the data are randomly and independently drawn from a larger population, and (iii) an overlap condition that ensures that there is a positive number of users that is first exposed to the treatment at any

period and that – under the conditional parallel trends assumption – propensity scores for initial treatment times based on covariates are bounded away from zero. In my context, the first additional assumption could be violated only if a user closes their account on the app and then signed up again later on, all within the roughly within the two-year data periods I use. This cannot be more than a tiny minority of users. The second assumption holds less trivially. One way to think of a super population from which the users in the dataset are drawn is to think of knowledge about the app as partially random, and about the super population of all individuals who would have signed up had they learned about the apps existence.

Because the purpose of this section is to convey the core idea of the estimation approach, I keep things as simple as possible and discuss only the case with no anticipation effects and where the parallel trend assumption holds unconditionally. Callaway and Sant'Anna (2021) show that the same overall approach also works when allowing for known anticipation and conditional parallel trends. Intuitively, known anticipation merely shifts the reference period from the period immediately before treatment to the period before anticipation of treatment begins. When relying on the conditional parallel trends assumption, the overall group-time average treatment effect  $ATT(g, t)$  is the average of unconditional group-time average treatment effects for each value of the covariate vector  $X_i$ . As discussed in Roth et al. (2022), estimation is challenging when  $X_i$  is continuous or can take on a large number of values, since then we will typically lack data to estimate unconditional group-time treatment effects for each value of  $X_i$ . There are different semi- and non-parametric approaches that can be used in such cases. I use the first-step regression estimator throughout for all results shown in this paper.<sup>16</sup>

Given these two assumptions, Callaway and Sant'Anna (2021) show that  $ATT(g, t)$  is identified by comparing the expected change of group  $g$  between periods  $t$  and  $g - 1$  with that of a comparison group,  $g'$ , that is not yet treated at time  $t$ :

$$ATT(g, t) = \mathbb{E}[Y_{i,t} - Y_{i,g-1}|G_i = g] - \mathbb{E}[Y_{i,t} - Y_{i,g-1}|G_i = g'], \text{ for any } g' > t. \quad (2)$$

As this holds for all  $g'$  that are not yet treated at time  $t$ , it also holds for an average over all such groups, collected in set  $\mathcal{G}$ , so that

$$ATT(g, t) = \mathbb{E}[Y_{i,t} - Y_{i,g-1}|G_i = g] - \mathbb{E}[Y_{i,t} - Y_{i,g-1}|G_i \in \mathcal{G}]. \quad (3)$$

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<sup>16</sup>See Callaway and Sant'Anna (2021) and Roth et al. (2022) for more details on the estimator.

This equation encapsulates two main results, which are formally proven in Section 2 of Callaway and Sant'Anna (2021): that the period just before treatment,  $g - 1$ , is a valid reference period, and that the group of all individuals that have not yet been treated at time  $t$  are a valid comparison group for estimating treatment effects in time  $t$ . If a group of never-treated individuals is available, then these could also serve as a comparison group. But because my dataset does not contain such individuals, I do not discuss this case here.

As a result of this, units who are treated in the very first period in the dataset are dropped from the sample, since there exists no possible control group based on which to identify their treatment effect, and since they are not useful as a control group themselves. Similarly, unit treated in the very last period in the data are also dropped, since there exists no “not-yet-treated” group that could serve as a comparison group for them.

**Estimation:**  $\text{ATT}(g, t)$  can be estimated by replacing expectations with their sample analogues,

$$\widehat{\text{ATT}}(g, t) = \frac{1}{N_g} \sum_{i:G_i=g} [Y_{i,t} - Y_{i,g-1}] - \frac{1}{N_{\mathcal{G}}} \sum_{i:G_i \in \mathcal{G}} [Y_{i,t} - Y_{i,g-1}], \quad (4)$$

where  $N_g$  is the number of individuals first treated at time  $g$  (i.e. members of group  $g$ ), and  $N_{\mathcal{G}}$  is the number of individuals not yet treated at time  $t$  (i.e. all members of all groups in  $\mathcal{G}$ ).

Once these building blocks are estimated, aggregating them to event-study type treatment effects that provide the (weighted) average treatment effect  $l$  periods away from treatment adoption across different adoption groups can be achieved by calculating

$$\text{ATT}_l = \sum_g w_g \text{ATT}(g, g + l), \quad (5)$$

where  $w_g$  are the groups' relative frequencies in the treated population. When calculating these event study parameters, I use a panel balanced in event times, with all units being observed for at least 5 treatment periods. This avoids the  $\text{ATT}_l$  being influenced by different group compositions at different periods  $l$ .<sup>17</sup>

**Inference:** Inference is based on a bootstrap procedure that can account for clustering and produces simultaneous (or uniform) confidence bands that account for multiple hypothesis testing.<sup>18</sup> The interpretation of uniform and pointwise

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<sup>17</sup>See Section 3.1.1 in Callaway and Sant'Anna (2021) for a more detailed discussion.

<sup>18</sup>For more details on the inference procedure, see Section 4.1 in Callaway and Sant'Anna (2021).

confidence intervals differ in the following way: a uniform 95% confidence band accounts for multiple hypothesis testing in that it is constructed such that *all* shown coefficients cover their corresponding true value 95 percent of the time. In contrast, a more commonly used pointwise 95% confidence band is constructed such that the confidence interval for each parameter covers the true parameter 95 percent of the time.

## 4 Results

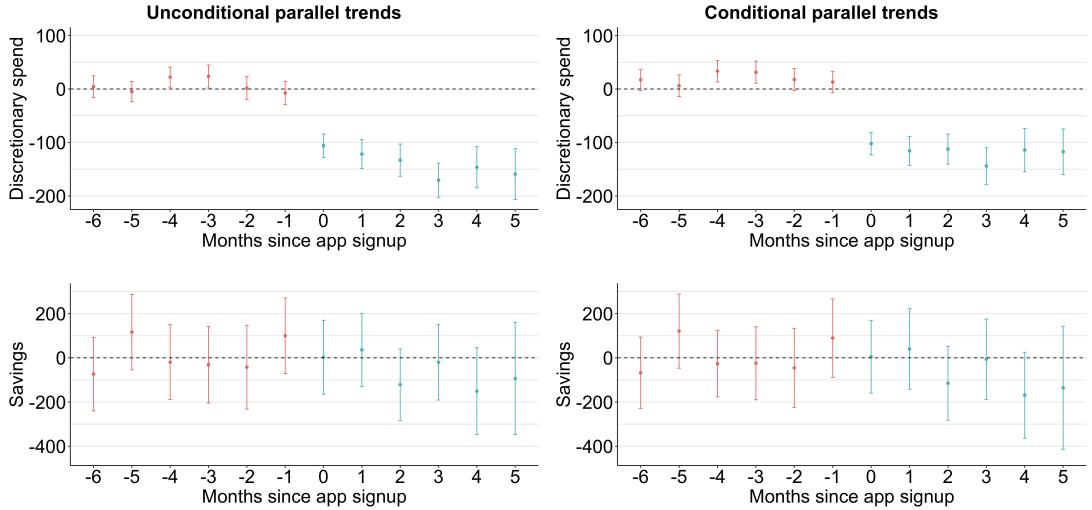
### 4.1 Main results

Figure 4 shows the effect of app use on monthly discretionary spend (top row) and monthly savings (bottom row) under the unconditional (left column) and conditional (right column) parallel trends assumptions. As discussed in Section 3, the conditional parallel trends assumption states that the outcome variable of treatment and control units with the same set of covariates would have evolved in parallel fashion. Throughout my analysis, the set of covariates I use are month income, month spend, the number of active accounts, and age. Estimates are group-time average treatment effects aggregated by time since treatment exposure, as defined in Equation 5. All results are presented with a uniform 95% confidence band, based on bootstrapped standard errors that account for autocorrelation in the data and are clustered at the user level, as discussed in Section 3.

We can see that discretionary spend falls by between £100 and £150 per month once users start using the app, depending on the parallel trends assumption used. Given that average monthly discretionary spend is about £860 (see Table 2), this corresponds to a drop in discretionary spend of about 11-17 percent, which is substantial. These results are in line with those found in Levi and Benartzi (2020), which find a 11.6 percent reduction in discretionary spend following the use of an aggregator app. Interestingly, we can also see that at least for the first six months of app use, the effect is persistent.

That the results based on the unconditional and the conditional parallel trends assumptions are very similar but not identical is not surprising. Conditional parallel trends are important in contexts where (i) there are covariate specific trends in outcome paths and (ii) the distribution of covariates differs between treatment and control groups. A classic example of the latter would be comparing individuals who did and did not sign up for a job-training program, where characteristics such as age and employment history are often quite different (Heckman et al. 1997). In our context, however, where all individuals eventually sign up to Money Dashboard and our comparison group is a set of “not-yet-treated” rather

Figure 4: Main results



Notes: The effect of app use on monthly discretionary spending (top row) and monthly savings (bottom row) under the unconditional (left column) and conditional (right column) parallel trends assumption. Point estimates represent group-time average treatment effects aggregated to periods since treatment exposure, as defined in Section 3. Red lines represent point estimates and uniform 95% confidence bands for pre-treatment periods allowing for clustering at the user level. If the null hypothesis that parallel trends hold in all periods is correct, these should be equal to zero. Blue lines provide similar information for post-treatment periods.

than ‘‘never-treated’’ individuals, we would not expect covariate values between individuals in the treatment and control groups to vary as much. At the same time, we would expect the results to differ somewhat. If, as is plausible, discretionary spend is a constant fraction of income and total spend, then we would expect parallel trends only for individuals with similar incomes and total spend. Similarly, if we think that discretionary spend increases in the number of accounts we observe per user, then parallel trends in spend only holds for users for whom we observe the same number of accounts.

We can also see that in periods -4 and -3, the null-hypothesis of identical pre-signup trends is being rejected (both under conditional and unconditional trends). The estimated differences from zero are not large, but a sign that the results should be interpreted with caution. In future work, I plan to use the approach introduced by Rambachan and Roth (2022) to test how sensitive my results are to deviations from parallel trends.

Finally, we can see that, in contrast to discretionary spending, savings do not change once users start using the app. Confidence intervals are much larger than for discretionary spend because – as can be seen in Table 2 – savings account inflows are often zero, which leads to a standard deviation much larger than that for discretionary spend.

The absence of an increase in savings is somewhat surprising, since we might have expected that users transfer at least some of their saved funds into their savings accounts to either build up a emergency savings cushion or as a contribution

towards specific savings goals. The absence of such transfers naturally raises the question “where does the money go?”, to which I turn in the next section.

## 4.2 Where do additional funds go?

Above, we found that while users reduce their discretionary spend after using MDB by between £100 and £150, they do not transfer additional funds into their savings accounts.

One possible explanation is that users do save more but transfer money either into longer-term savings vehicles such as investment funds or into savings accounts they have not linked to MDB. The first three panels (from the top left) in Figure 5 show flows into investment of pension funds, current account outflows that users manually labelled as “savings”, and – as an alternative but cruder measure of transfers into savings accounts – the total amount of transfers from current accounts into other accounts held by the user.<sup>19</sup> None of these change significantly during users’ pre and post signup periods, rejecting the idea that users systematically move additional funds into either investment vehicles or other savings accounts. An exception is the spikes in user-labelled savings in the month just before and the month of signup. Rather than suggesting an additional inflow of funds, however, this is more likely to be an artefact of users only manually labelling transactions during active app use rather than also labelling historical transactions. This could happen in two ways: either users can see the entire account history available to MDB after linking an account but only label transactions during periods of active app use and the period immediately before signup, or MDB only shows users data from the month before signup onwards, so that users are unable to manually label transactions in earlier periods.

Another possibility is that users use the additional funds to pay down debt. The bottom right panel in Figure 5 shows loan repayments, which do not change significantly during the 12-month period around app signup either.

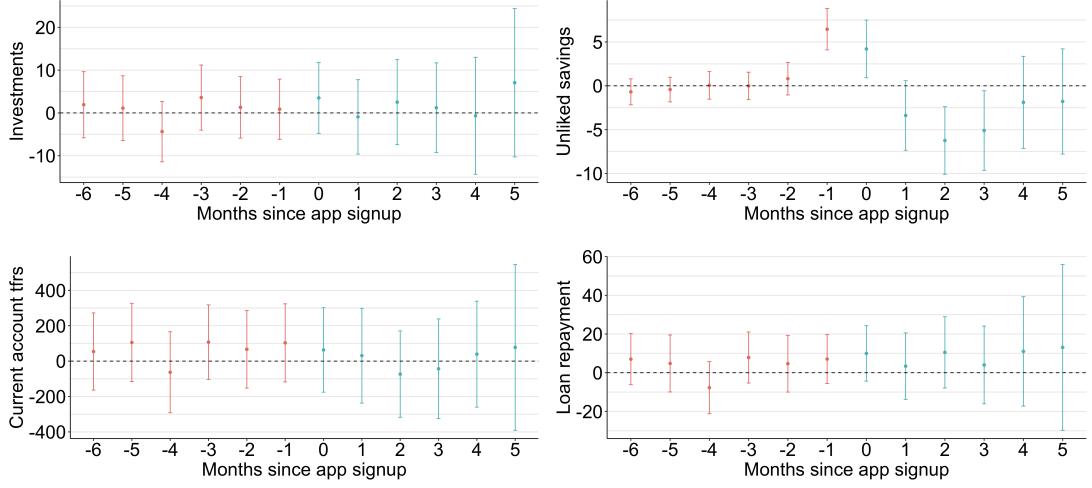
The finding that saved funds do not flow into any of the plausible channels can be explained in three ways (and any combination thereof): (i) users might leave saved funds in their current accounts, (ii) users are homogenous and allocate saved funds across a large number of different uses in amounts I am not powered to detect, (iii) users are heterogeneous and allocate funds across a small number of uses that differ between users, so that, in the aggregate, I am not powered to detect these changes either.

Explanations (ii) and (iii) seem plausible and would make for interesting future research. But why would users just let accumulate funds in their current

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<sup>19</sup>The code where these variables are defined is available on [Github](#).

Figure 5: Possible destinations of additional savings



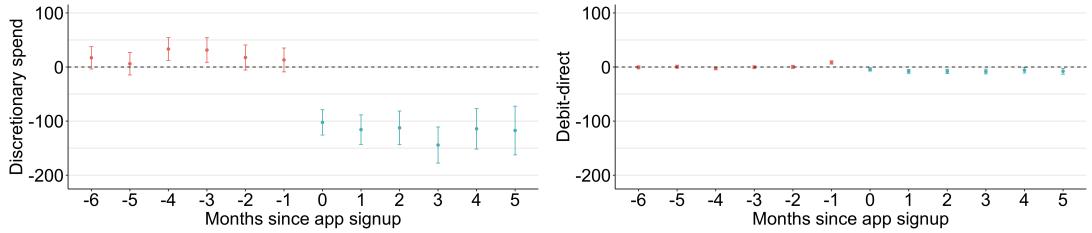
Notes: Point estimates represent group-time average treatment effects aggregated to periods since treatment exposure, as defined in Section 3. Red lines represent point estimates and uniform 95% confidence bands for pre-treatment periods allowing for clustering at the user level. If the null hypothesis that parallel trends hold in all periods is correct, these should be equal to zero. Blue lines provide similar information for post-treatment periods.

accounts instead of moving funds into higher-interest savings accounts? One possible explanation is that people intend to make these transfers but that inertia and procrastination from following through. Another explanation is that historically low interest rates during the entire sample period did not provide much of an incentive to save in the first place. Both of these explanations could be explored further in future research.

### 4.3 Disaggregated discretionary spend

Another question of interest based on the main findings above is along what dimensions users reduce their discretionary spend. There are three dimensions of interest.

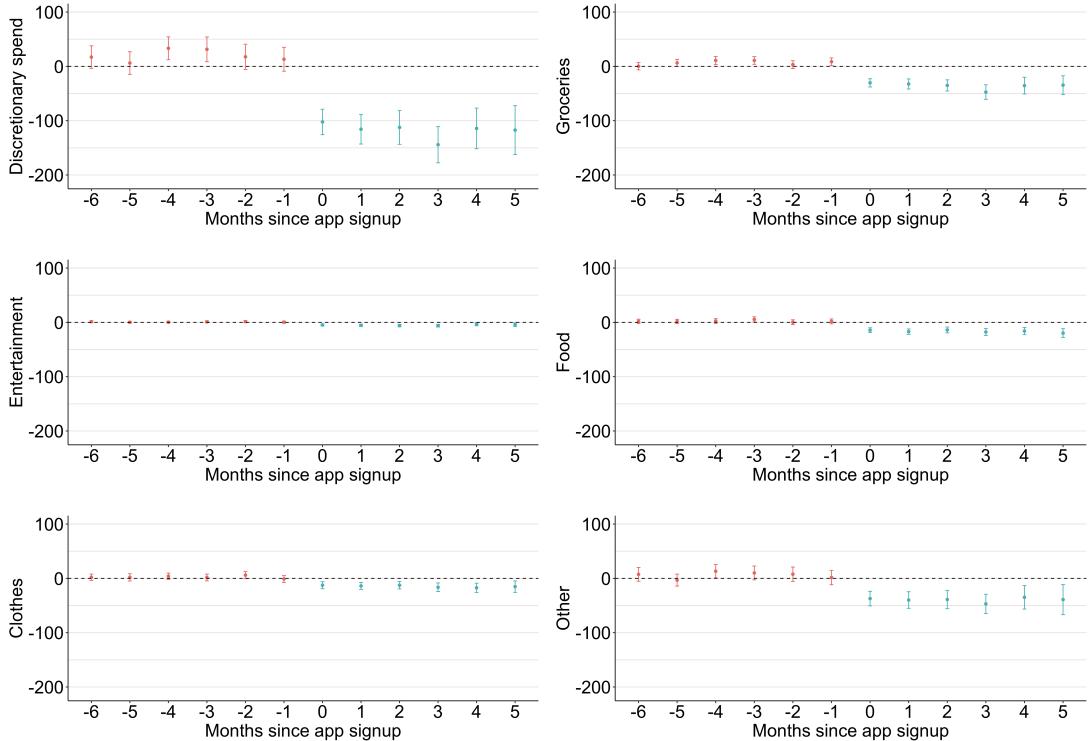
Figure 6: Change in direct-debit discretionary spend



Notes: Effect of app use on total discretionary spend (for reference) on the left and discretionary spend paid via debit-direct contracts on the right. Point estimates represent group-time average treatment effects aggregated to periods since treatment exposure, as defined in Section 3. Red lines represent point estimates and uniform 95% confidence bands for pre-treatment periods allowing for clustering at the user level. If the null hypothesis that parallel trends hold in all periods is correct, these should be equal to zero. Blue lines provide similar information for post-treatment periods.

First, users can achieve the persistent reduction in discretionary spend seen in Figure 4 by either maintaining a change in behaviour consistently month-to-month by, for instance, making fewer purchases or spending less on certain items, or, alternatively, make a decision during the month of signup that automates the reduction in spend by cancelling debit-direct contracts. Figure 6 shows in the left panel the reduction of discretionary spending shown in Figure 4 above for reference, and in the right panel discretionary spend paid via debit-direct contracts. It shows that debit-direct discretionary spend falls only marginally after app use, indicating that it is a change in behaviour sustained month-to-month that drives the reduction in discretionary spend. A caveat to this analysis is that the MDB data allows for only imperfect identification of debit-direct transactions for two reasons: first, only some banks label debit-direct transactions as such in the transaction descriptions provided to MDB and it is commonly applied labels that I use to identify the transactions. Second, MDB redacts some transaction descriptions entirely or in part to ensure the anonymity of its users, and it's possible that some of these transactions are debit-direc<sup>20</sup>

Figure 7: Change in discretionary spend by subgroup

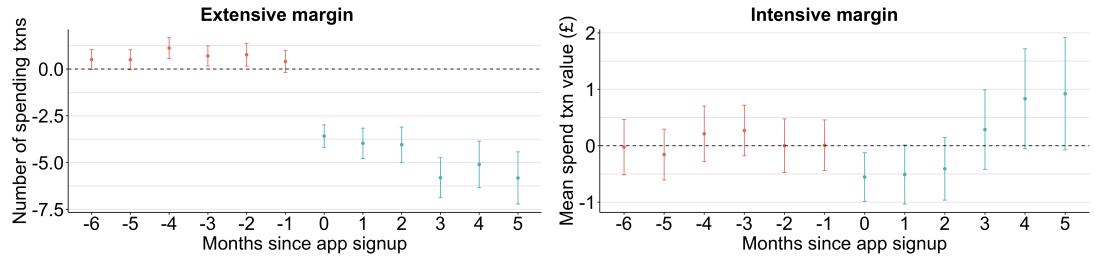


Notes: Effect of app use on total discretionary spend (for reference) and its subgroups. Point estimates represent group-time average treatment effects aggregated to periods since treatment exposure, as defined in Section 3. Red lines represent point estimates and uniform 95% confidence bands for pre-treatment periods allowing for clustering at the user level. If the null hypothesis that parallel trends hold in all periods is correct, these should be equal to zero. Blue lines provide similar information for post-treatment periods.

<sup>20</sup>The code used to calculate debit-direct discretionary spend is available on [Github](#).

The second interesting way to disaggregate discretionary spend is into different component groups based on the type of spend; for this purpose, I group transactions into “groceries”, “clothes”, “entertainment”, “food”, which captures spending on restaurant meals and take-away, and “other”.<sup>21</sup> Figure 7 again reproduces the plot from the main results in the top-left panel for reference and then plots results for each of the five subgroups. While we can see that users mainly reduce spending on groceries and the residual category “other”, followed by food and clothes, the overall picture that emerges is that these differences are quite small, and that the overall reduction in discretionary spend results from changes along all these margins.

Figure 8: Intensive and extensive margin



Notes: The effect of app use on monthly discretionary spend disaggregated into the effect on the extensive (left column) and intensive (right column) margins. The extensive margin is the number of discretionary transactions per month, and the intensive margin is the average value of a discretionary spend transaction. Point estimates represent group-time average treatment effects aggregated to periods since treatment exposure, as defined in Section 3. Red lines represent point estimates and uniform 95% confidence bands for pre-treatment periods allowing for clustering at the user level. If the null hypothesis that parallel trends hold in all periods is correct, these should be equal to zero. Blue lines provide similar information for post-treatment periods.

Finally, it is interesting to consider whether users reduce discretionary spend along the extensive or the intensive margin – whether they make fewer transactions or reduce the average spend per transaction. Figure 8 shows the number of transactions (the extensive margin) in the left panel and the average spend per transaction (the intensive margin) in the right panel. We can see that the reduction in spend is clearly the result of changes along the extensive margin – on average, users make about five fewer discretionary spend purchases per month once they sign up to MDB. The average transaction value of a discretionary spend purchase in the data is about £25, adding up to the total effect we find in Figure 4.

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<sup>21</sup>The list used to classify transactions is available on [Github](#). I classify transactions tagged by MDB as “entertainment” as “other discretionary spend”, since many such transactions are purchases with Amazon, which we cannot precisely classify.

## 5 Conclusion

In this paper, I test whether using Money Dashboard is associated with a reduction in discretionary spending and an increase in emergency savings. I use a new estimator proposed by Callaway and Sant'Anna (2021) that corrects for recently identified problems in two-way fixed effects estimates.

I find that users reduce their discretionary spend by between £100 and £150 (11-17% of average discretionary spend) once they start using the app and sustain that reduction throughout the six-month post-signup period I consider. Looking at disaggregated measures of discretionary spend further shows that the reduction is the result of maintained month-to-month changes in behaviour rather than one-off cancellations of direct-debit transactions, that it results from reducing spending on a number of different categories of purchases rather than a single one, and that it is a result of changes along the extensive rather than the intensive margin – users reduce the number of transactions they make rather than the value of the average transaction.

Interestingly, users do not seem to use these additional funds to build up emergency savings: net-inflows into savings accounts do not change after signup. I can also neither find significant increases in flows into investment and pension accounts or additional savings accounts that are not linked to the app, nor additional loan repayments.

We cannot interpret these results as causal effects because I cannot isolate the effect of MDB use from possible confounding factors that make users self-select into using the app in the first place. And we cannot attribute the effect of MDB use to any specific feature of the app, such as providing easy access to financial information or goal-setting features.

However, the findings provide novel insight into how people reduce their spending and are strongly suggestive that using a financial aggregator app can help people reduce their spending. While – as my results show – such a reduction in spend does not automatically translate into an increase in emergency savings, they provide an opportunity for such savings. To harness that opportunity, FinTech apps should experiment with incorporating tools that make saving easy.

Further research could focus on causally establishing the link between app use and discretionary spend, as well as testing the effectiveness of various tools to help users turn those additional funds into savings. Another area that would be useful to understand is to what extent the effect of app use might depend on a “dosage effect” – whether the effectiveness of app features is moderated by the intensity of app use. However, work testing such an effect will have to isolate dosage of exposure from personal characteristics that make certain users engage more

with the app. Finally, the finding that people may direct saved funds towards a number of different purposes can be the result of differences in funds use across people – different people use funds for different purposes – or within people – people all use the funds for a variety of different purposes – or, most plausibly, a combination of the two. If people spread the additional funds across many different purposes then it might be the case that they miss out on directing them instead towards the use with the highest long-term payoff, such as paying downs of credit-card debt or investing in government-subsidised savings vehicles.<sup>22</sup> This behaviour would be similar to the finding by **gathergood2019individual** that – instead of first paying down debt of their highest-interest credit card, people tend to allocate repayments across all their cards in proportion to outstanding debt. Further research establishing to what extent there is within-person variation of fund use, and whether such funds are directed towards the most effective uses would provide further useful insights into savings behaviour, and could serve as the basis for further development of FinTech apps to help users direct saved funds towards the most effective users.

One danger of viewing low savings mainly as a result of behavioural biases is that while these biases likely do play some role and designing environments and tools to help correct them are thus part of the solution, it is at least conceivable that this is an area where the focus on behaviour-level solutions distracts from an effort to find more effective society-level solutions, a danger inherent in behavioural science research convincingly highlighted in Chater and Loewenstein (2022): if the main problem is that many people are unable to earn enough to save, then the value of helping them manage their low incomes more effectively pales in comparison with efforts to help them earn more.

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<sup>22</sup>In the UK, for instance, the first £4000 paid into a Lifetime ISA each year are matched by the government by a £1000 payment, corresponding to a risk-free interest rate of 25 percent.

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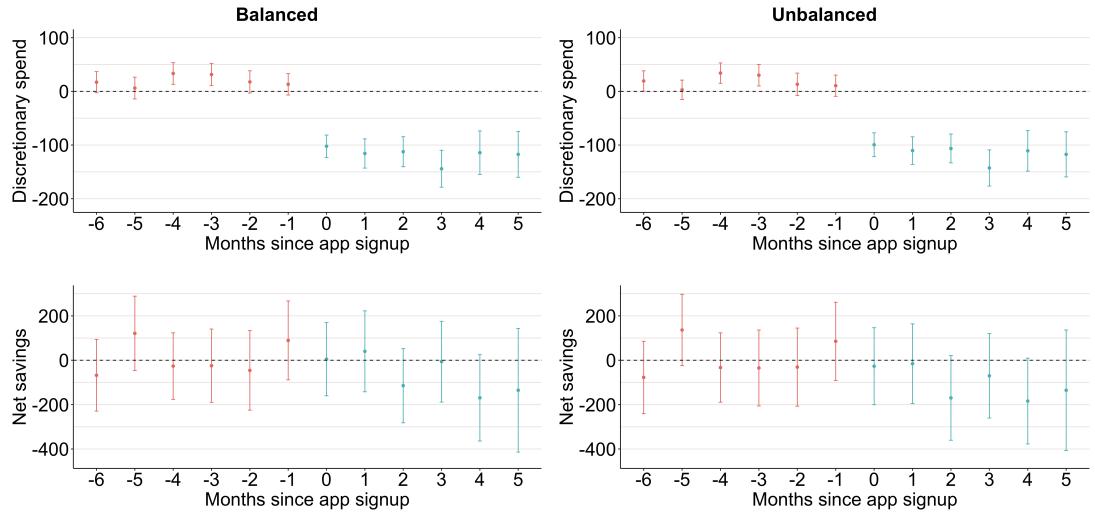
## A Unbalanced aggregation

The baseline specification relies on a panel balanced in event time and thus only includes users that have been exposed to treatment for at least 5 periods. Figure 9 reproduces the baseline results in the left panel and compares it with results based on the full sample.

As discussed in section 3.1.1 of Callaway and Sant'Anna (2021), these two approaches entail a trade-off. When using the full sample, the aggregated parameters are a function of the weighted average treatment effects for each group  $l$  periods after treatment (which is what we want) as well as compositional changes due to different groups being included for different periods  $l$  and different weights attached to these groups. While parameters aggregated using a panel balanced in event time do not suffer from compositional and weighting changes, but are calculated based on a smaller number of groups.

As expected, using the full data reduces the size of the confidence intervals. But the results are otherwise very similar.

Figure 9: Balanced and unbalanced aggregation



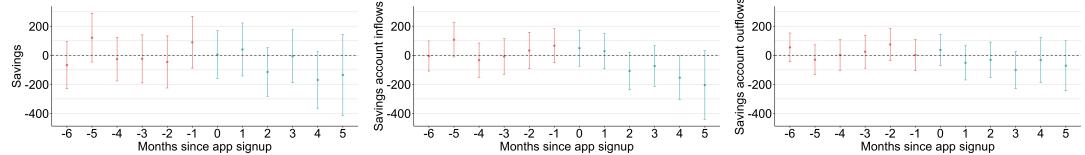
Notes: Main results based on balanced and unbalanced panels. Point estimates represent group-time average treatment effects aggregated to periods since treatment exposure, as defined in Section 3. Red lines represent point estimates and uniform 95% confidence bands for pre-treatment periods allowing for clustering at the user level. If the null hypothesis that parallel trends hold in all periods is correct, these should be equal to zero. Blue lines provide similar information for post-treatment periods.

## B Inflows and outflows

The leftmost panel in Figure 10 reproduces the results from Figure 4 for net-inflows into savings accounts using the conditional parallel trends assumption for reference, and decomposes these net-inflows into inflows (middle) and outflows (right). We can see that net-inflows remain unchanged because both inflows and

outflows remain unchanged and not because a change in inflows if offset by a commensurate change in outflows.

Figure 10: Decomposing net-inflows into inflows and outflows



Notes: Decomposition of net-inflows into savings accounts (left) into inflows (middle) and outflows (right). Point estimates represent group-time average treatment effects aggregated to periods since treatment exposure, as defined in Section 3. Red lines represent point estimates and uniform 95% confidence bands for pre-treatment periods allowing for clustering at the user level. If the null hypothesis that parallel trends hold in all periods is correct, these should be equal to zero. Blue lines provide similar information for post-treatment periods.