# Evaluation Paper 23/1: An evaluation of our 2019 overdrafts intervention

Technical Annex

April 2023

# Contents

# 1 Introduction

1. This Technical Annex is a supplement to our evaluation paper (EP23/1). It presents the methodology and results of our analysis of the Overdraft remedies introduced in PS19/16: High-Cost Credit Review: Overdraft policy statement in more technical detail.

2. This annex is structured as follows: we first give an overview of our intervention and intended outcomes, followed by a description of the data sources used. We then present descriptive statistics for the overdraft market, followed by two chapters on the causal evaluation of pricing and repeat use remedies, respectively.

## Policy background

3. In 2018 the Financial Conduct Authority (FCA) published a review of the UK overdraft market in CP18/42: High-Cost Credit Review: Overdrafts consultation paper and policy statement. Our analysis found evidence of significant harm from high prices and complex pricing structures.

4. Our study found that unarranged overdraft fees were concentrated among the most deprived consumers as measured by the Index of Multiple Deprivation (IMD). We found that total fees and charges for unarranged overdrafts could result in prices 10 times higher than those for payday loans. Consumers in the most deprived decile were three times more likely to incur annual fees in excess of £200 compared to the least deprived decile. We also found other concerns such as a positive correlation between the time spent in overdraft and the probability of borrowing more through overdraft, as well as low ability of consumers to compare different overdraft pricing structures.

5. In 2019 we announced a package of remedies in PS19/16. The remedies aimed to reduce the harm we identified in CP18/42. These remedies were split into two groups - pricing remedies and repeat use remedies. These were:

### Pricing Remedies

1) stopping banks and building societies from charging higher prices for unarranged overdrafts than for arranged overdrafts
2) banning fixed fees for borrowing through an overdraft – calling an end to fixed daily or monthly charges, and fees for having an overdraft facility
3) requiring banks and building societies to price overdrafts by a single annual interest rate
4) requiring banks and building societies to advertise arranged overdraft prices with an APR to help customers compare them against other products
5) issuing new guidance to reiterate that refused payment fees should reasonably correspond to the costs of refusing payments

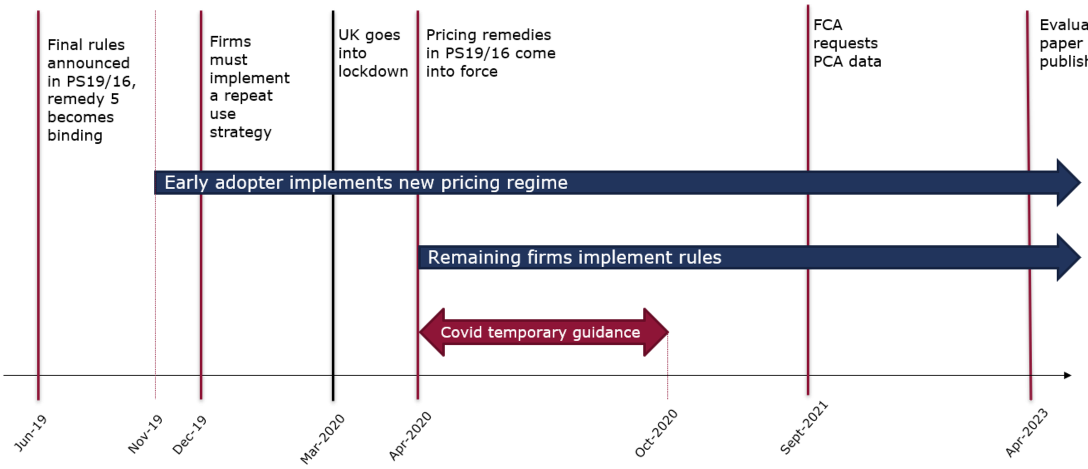
Repeat use remedies

6) requiring banks and building societies to do more to identify customers who are showing signs of financial strain or are in financial difficulty and develop and implement a strategy to reduce repeat overdraft use

6. Remedy 5 was the first one to become binding on 07 June 2019. Remedy 6 became binding on 18 December 2019 and remedies 1-4 became binding on 06 April 2020.

7. While the sector as a whole did not introduce pricing remedies until April 2020, one firm, which we refer to as the early adopter, started moving its customers to the new pricing regime in November 2019. Figure 1 below illustrates the implementation timeline.

**Figure 1: Timeline of our intervention and other events affecting the overdraft market**




8. We committed to an evaluation of our overdraft remedies in PS19/16. In this paper we present our analysis of the remedies' impact on total arranged and unarranged overdraft charges, total overdraft balances, total charges relative to borrowing, and the distribution of these effects across IMD deciles.

9. We evaluate remedies 1-4 as a separate package. This is because they have the same treatment assignment mechanism and are targeting the same set of consumer outcomes. Furthermore, all four remedies target the same population as they apply to the entire overdraft market. We present the results from this part of the analysis in Chapter 4.

10. We cannot estimate the effects of remedy 5. This part of the policy became binding early and applied to the entire sector. All consumers in the market would have been exposed to the policy at the same time without any variation in treatment intensity. We anticipate, however, that remedy 5 would have a smaller effect on consumer outcomes, compared to remedies 1-4. This is because remedy 5 is a clarification on existing rules and is not as prescriptive as the other remedies. We also show in

Chapter 3 that total revenue from refused payment fees was relatively small compared to that from arranged interest rates and arranged fixed fees (see Figure 11) prior to our intervention. We are therefore confident that our estimates are a good approximation to the total benefits arising from the package.

11. We evaluate the impact of remedy 6 separately from remedies 1-4, as it has a distinct treatment assignment mechanism, and it affects a specific part of the total overdraft user population. Remedy 6 only affects those individuals identified by their bank as being in a pattern of repeat use and/or in actual or potential financial difficulty. We present result from this part of the analysis in Chapter 5.

## Expected outcomes

### Pricing remedies

12. Our analysis in CP18/42 assumed that firms would increase arranged overdraft interest rates to compensate for the loss in revenue from fixed fees. We expected this change in the pricing structure to cause consumers to adjust the amount they borrow. We assumed that people with higher overdraft balances would adjust their borrowing relatively more compared to people with smaller overdraft balances. This is because the higher arranged interest rate contributes relatively more to total charges when the average borrowing amount is high.

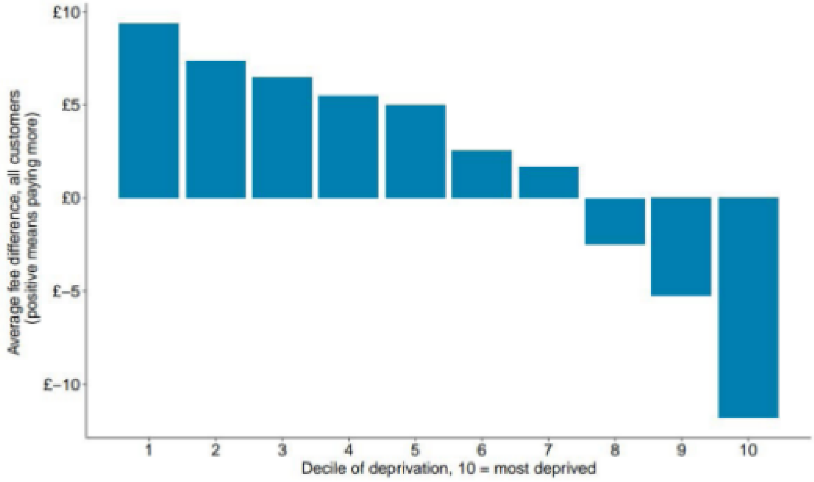
13. We expected that despite the increase in the interest rate component of overdraft prices, our intervention would reduce the total overdraft cost for the most deprived consumers. The policy was expected to achieve this directly through the removal of fixed fees and the alignment of arranged and unarranged overdraft costs, and indirectly through (i) a reduction in overdraft borrowing, and (ii) enhanced competition due to consumers being able to compare overdraft products more effectively against alternatives. These channels are described in Figure 5: Pricing interventions causal chain in CP18/42.

14. Under these central assumptions, we calculated the expected reduction in charges under 2 scenarios. Our central scenario assumed interest rates would stabilise towards the higher end of interest rates observed in the market before we intervened. Under this scenario, we calculated that pricing remedies could reduce overdraft charges by £101m per year for consumers living in the 3 most deprived IMD deciles in the UK. Under the main scenario, consumers living in the 7 least deprived deciles were expected to see an increase in overdraft charges.

15. We also modelled an optimistic scenario where the equilibrium interest rates post-intervention were closer to the lower end of interest rates in the market pre-intervention. Under the optimistic scenario, all IMD deciles were expected to see a reduction in fixed fees with the most deprived IMD deciles seeing the biggest absolute reduction.

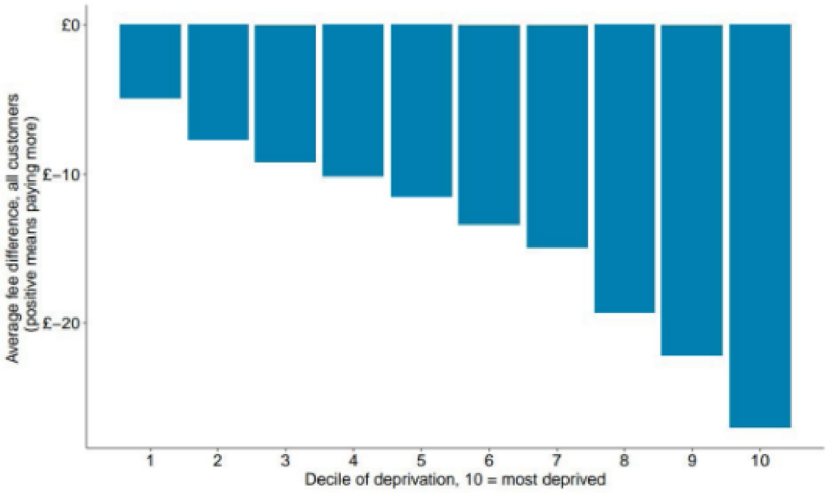
16. Figures 2 and 3 below illustrate the expected change in total annual charges per person by IMD decile under our main and optimistic scenarios respectively.

**Figure 2: The expected average (mean) change in annual overdraft charges for consumers as a result of our proposed pricing interventions (Scenario: Baseline and higher APR)**




*Source: Figure 9, CP18/42*

**Figure 3: The expected average (mean) change in annual overdraft charges for consumers as a result of our proposed pricing interventions (Scenario: Baseline and lower APR)**




*Source: Figure 10, CP18/42*

17. In Chapter 4 of this technical annex we show that our ex-post estimates of the effect of the policy are in line with the central scenario for the most deprived IMD decile and closer to the optimistic modelling scenario for the remaining deciles.

## Repeat use remedies

18. We did not quantify the expected benefits arising from repeat use remedies. However, we expected them to affect the same set of outcomes as pricing remedies. We calculated that for the repeat use remedies to break even against the compliance costs expected in CP18/42, customers identified as repeat users would need to make, on average, a one-off saving of just £3 in fees and charges. This assumption was based on 4m consumers receiving repeat use treatment in the first year of the policy and 1.3m receiving treatment in every subsequent year.

19. Total charges therefore are the main outcome of interest when we analyse pricing remedies and repeat use remedies. Our evaluation also focuses on the effects on total overdraft borrowing, as well as the cost per pound borrowed.

# 2 Data

20. In this section we give a high-level overview of the data sources used in the evaluation. In the causal analysis presented in chapters 4 and 5, we use subsets of our data. We give more detail on these subsets in the respective chapters.

21. The main data source for this evaluation is a sample of transaction-level data from the 6 biggest Personal Current Account (PCA) providers in the UK. These account for c. 83% of all PCAs in the UK. In September 2021, we requested data on PCAs from these firms, including information on balances, transactions, repeat use treatment status, prices and other supplementary variables such as savings account balances and risk scores.

22. We performed extensive data quality assurance on all data supplied. Firms had three submission attempts. Data quality issues identified in the first two attempts were referred back to participating firms. We asked participating firms to address issues at each iteration. This review ensured that the data supplied was in the correct format and that no major data quality issues such as missing variables or wrong data types were present.

23. We linked the PCA data to a Credit Rating Agency (CRA) dataset we hold. This dataset covers a six-year period and contains the credit history (i.e. credit file data) for a random sample of c. 10% of the adult UK population and their financial associates (i.e. individuals that these consumers may have joint credit accounts with). For each credit product held, the dataset shows opening and closing dates, limits, regular repayments (amount, duration and frequency), balances, and arrears status. The match rate from the PCA to the CRA data is 91% as the sampling for the PCA data was designed to capture individuals captured in the CRA data. The match rate is lower than 100% due to:

    i. consumers not having a credit record in the CRA data

    ii. consumers changing their address between the sampling times for the CRA data and the PCA data

24. In the remainder of this chapter we outline our approach to sampling and give an overview of the main variables used in the analysis.

## Sampling

25. The total sample used for the evaluation of pricing remedies covers c. 1.6m consumers across the 6 firms. The sample used to evaluate repeat use remedies covers c. 3.6m consumers.

26. Consumers in our pricing sample were chosen based on randomly selected dates of birth. We chose 12 birthday dates and requested that firms supply data for people born on one of these dates. As we aimed to select approximately 300,000 consumers per firm. We therefore required one of the firms to supply data on customers born on 5 of the 12 dates. Two of the firms supplied data for the same 5 dates, plus an

additional 4 dates. The remaining firms supplied data for all 12 birth dates. When we restrict the sample to the 5 birthday dates common to all firms, we can build a customer-level view that allows us to observe all of a given customers' accounts with the 6 participating firms. This gives us a subsample of 579,760 individuals, which we use for the causal analysis of pricing remedies in Chapter 4.

27. We targeted this sample size to give our econometric analysis in Chapter 4 sufficient statistical power. The large sample size also enables us to perform analysis within smaller sub-populations, e.g., within a particular IMD decile.

28. The sampling period is 01 May 2018 - 20 September 2021, and anybody with an open account in that period who meets at least one of the sampling conditions above, is included. Having over three years of data gives us 18 months of pre-intervention outcomes and 10 months of post-intervention outcomes (we exclude the period November – October 2020 due to the early adoption by one firm and the Covid temporary guidance which was in force between June and October 2020; we also exclude September 2021, as we do not have a full month of data for this period). The large pre- and post-intervention windows allow us to apply longitudinal econometric methods to our evaluation.

## Data structure and key variables

29. We requested data in a format where every customer is uniquely identified with an ID generated for the request. The ID is a random combination of numbers generated using the date of birth, postcode, and initials of consumers. Our CRA data contains an ID generated using this algorithm, hence we can link our PCA sample to the CRA data. Accounts are identified via a combination of the participating firms' name and an ID generated by the firm specifically for the request.

### *Static customer information*

30. For every firm customer in the PCA data, we observe the unique customer identifier, the date when they joined a given participating firm, their leave date (if applicable), as well as an identifier for which sub-sample (pricing or repeat use) of the data they belong to.

31. From the CRA data we can obtain the date of birth of the consumers in the PCA sample (the data we received from firms were pseudonymised and did not contain personal information) and their postcode. This allows us to control for age in our modelling and to identify the Index of Multiple Deprivation (IMD) decile based on customers' most recent address held by their PCA provider at the time of sampling. We therefore only observe the IMD decile of PCA holders at a single point in time.

### *Note on IMD deciles*

32. When we perform analysis by different groups of financial deprivation, we follow the approach in CP18/42, i.e. we use the Ministry of Housing, Communities and Local Government (MHCLG)'s IMD as a proxy for consumer vulnerability.

33. According to MHCLG, "The Index of Multiple Deprivation ranks every small area in England from 1 (most deprived area) to 32,844 (least deprived area)." We then take

this score and convert it to deciles where decile 1 is the least deprived and decile 10 is the most deprived.

34. We note that MHCLG orders the IMD deciles in ascending order, meaning that decile 1 is the most deprived and decile 10 is the least deprived. However, for consistency with CP18/42 we order IMD scores in descending order.

35. We note that the IMD scores for Scotland, Wales and Northern Ireland are computed on a different scale than the English ones so we cannot make direct comparisons between regions. Therefore, any analysis we present by IMD decile includes only English residents. When we extrapolate results to the UK adult population, we assume that the deprivation distribution in England is representative of that of the UK.

### *Static account information*

36. All PCAs belonging to customers in the sample are in scope. For every account in the sample, we observe the open and close date. We also observe to which customer it belongs, whether the account is a joint account and how many account holders there are. Firms have different current account products that have different names and different pricing structures – we observe the product name of each account in the sample.

### *Daily account information*

37. For every account in our sample, we observe several variables with daily frequency. These are end-of-day balance, the applicable arranged overdraft limit, as well as the following components of overdraft pricing:

    i. Arranged overdraft interest in effective annual rate (EAR) format - i.e. the annual rate after taking into account compounding

    ii. Arranged overdraft daily fee (in £)

    iii. Arranged overdraft monthly fee (in £)

    iv. Unarranged overdraft interest rate in EAR format

    v. Unarranged overdraft daily fee (in £)

    vi. Unarranged overdraft monthly fee (in £)

### *Transaction data*

38. We requested data at the transaction level for each account in our sample. Our transaction data include the account ID to which the transaction belongs, the transaction date, time, value, as well as a transaction type category (generated by the participating firm’s system), a string generated by the firms’ system describing the transaction (for categories where there is no personal data, e.g., store purchases) as well as an indicator for whether the transaction was declined.

*A note on market-wide statistics*

39. When we report statistics aggregated at the market level, we apply either of two approaches, depending on the context. When we want to analyse outcomes at the consumer-level we restrict the sample to the five birthday dates common to all participating firms. This is to make sure we capture all accounts that an individual may have. We then extrapolate to the wider market by multiplying averages times the number of consumers with an overdraft facility. When we analyse outcomes at the account level, we use all accounts in our data and compute quantities at the firm level; we then extrapolate to the market on the basis of our sample firms' market shares. We specify in the text which approach we take in each case.

## Repeat use data

40. As well as the PCA dataset, we also requested that firms supply a dataset for the evaluation of the repeat use remedy (the 'repeat use dataset').

41. Our rules introduced in PS19/16 required firms to *"[] do more to identify customers who are showing signs of financial strain or are in financial difficulty and develop and implement a strategy to reduce repeat use"*. Our rules did not specify how firms should define repeat use or what the specific strategy to reduce repeat use should be. This means that the definition of repeat use as well as the metrics that a given firm uses to determine repeat use differ at the firm level.

42. We examined the firms' repeat use strategies and from each of the participating firms we requested those variables that were used in their strategies, i.e. a bespoke set of variables, as well as common variables:
    - A unique account identifier
    - The account's total overdraft charges for each month (our first outcome variable)
    - The account's average overdrawn balance over each month (our second outcome variable)

43. Firms perform repeat use assessments on a monthly basis. We therefore observe repeat use variables with a monthly frequency.

44. Our repeat use sample covers the full population of customers who triggered the repeat use definition of their PCA supplier within the sampling period. We also asked firms to supply data for accounts that came close to meeting the criteria for repeat use but fell slightly short of meeting them. This enables us to apply a Regression Discontinuity Design (see Chapter 5), which requires a large number of observations around the cut-offs that determine treatment.

45. We provide more information about the repeat use dataset in Chapter 5.

# 3 UK overdraft trends

46. In this chapter we give an overview of the number of overdraft customers in the market, the average account balance, the trends in borrowing behaviour, overdraft revenues and repeat use.

## Personal Current Accounts

47. To get the total number of PCA holders in the UK we use an estimate of the UK adult population from the ONS and combine that with the CMA's finding that 97% of UK adults have a current account. Adults are individuals over 16 years old.

48. Using this approach, in CP18/42, we estimated that, in 2016, 52 million adults in the UK had a personal current account. In 2020, the figure is 53 million (based on 54.3 million adults in 2020).

49. Each customer may have more than 1 account. We estimate that the average number of accounts per person is 1.67.

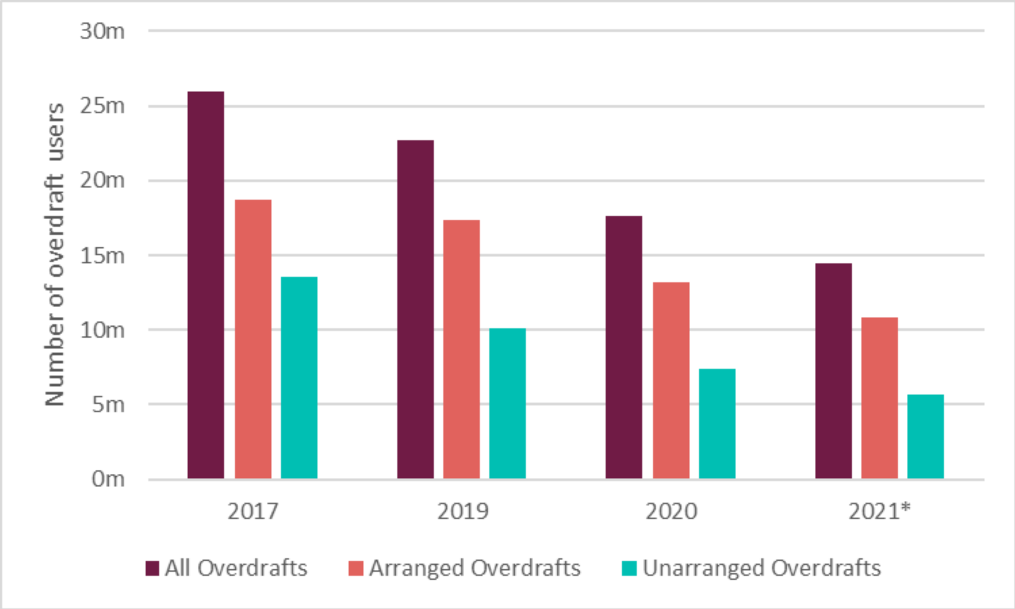
50. Not all accounts are active. Therefore, for some parts of our analysis we restrict accounts to 'main accounts'. For consistency with our analysis in CP18/42 and the CMA's Retail banking market investigation, we consider any account with median monthly deposits of at least £500 to be a 'main account'. Out of the 2.4m accounts in our main sample, 2.1m are main accounts.

## Number of overdraft customers

51. Of those consumers with a current account, some will make use of an arranged and/or unarranged overdraft facility, whereas others will not.

52. In this section we present the split between arranged and unarranged overdraft use. We do this by restricting our sample to the five dates common across the 6 participating banks, estimating the proportion of people who use each line and then extrapolating to the wider market.

**Figure 4: Number of overdraft users**



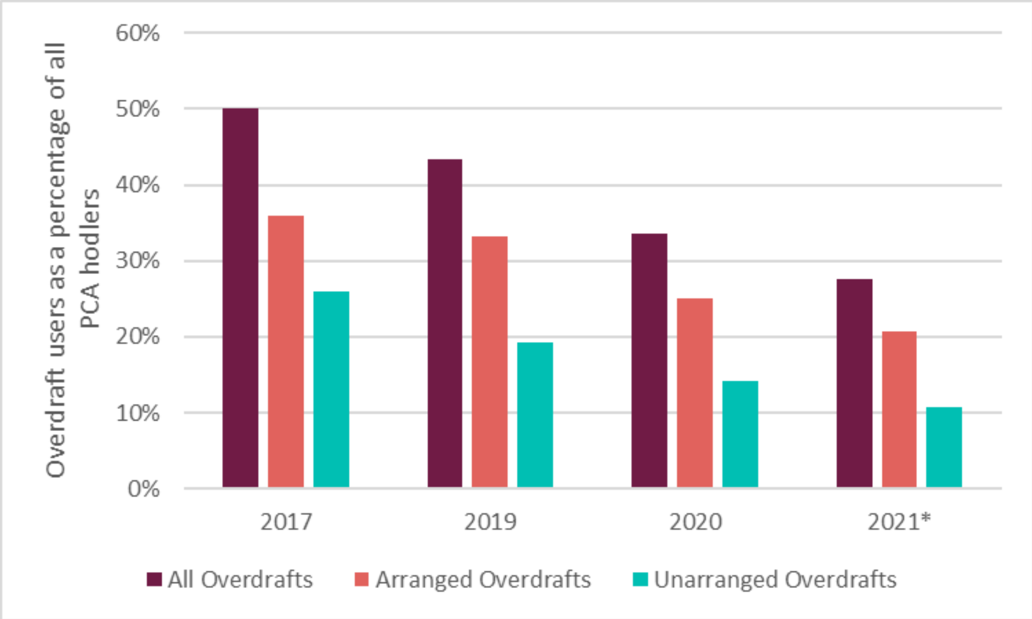

**Figures for 2021 are based on data up to August*

*Source: FCA analysis of PCA data. The source for 2017 figures is CP18/42.*

53. According to our data, the number of overdraft users in 2019 was 23 million, out of which 17 million used their arranged overdraft while 10 million used their unarranged overdraft. The total number of overdraft users fell in 2020, to 18 million overdraft users, while arranged and unarranged overdraft users were 13 million and 7 million respectively. The number of PCA customers who use an overdraft is likely to have continued decreasing in 2021. Our data for 2021 covers the period from January to August and shows that the number of overdraft users was 15 million, out of which 11 million used an arranged overdraft and 6 million used an unarranged overdraft. Although we cannot be certain, as we do not observe how many additional consumers used an overdraft between September and December 2021, we would expect overdraft usage for the entire 2021 to be lower than for 2020. When looking solely at the period January – August for each year, our data suggest a 25.83% fall in the total number of overdraft users between 2019 and 2021. This is driven by a 28.72% decrease in the number of arranged overdraft users and a 26.53% decrease in the number of unarranged overdraft users.

54. This decrease in the number of overdraft users may be driven by developments in the market such as the pandemic, the introduction of overdraft balance alerts by the CMA in 2018, as well as by our pricing and repeat use remedies. Although we do not directly estimate the causal impact on the probability of becoming an overdraft user, in Chapter 4 we estimate the causal effect of our policies on overdraft balances. We find that for some consumer groups, the policy has caused a reduction in monthly borrowing through overdraft, consistent with the trend observed in Figure 4.

**Figure 5: Proportion of overdraft users (out of all PCA holders)**




**Figures for 2021 are based on data up to August*
Source: FCA analysis of PCA data. The source for 2017 figures is CP18/42.

55. Our analysis in CP18/42 found that in 2017, 36% of people with a personal current account used an arranged overdraft and 26% used an unarranged overdraft. In 2021, these proportions were 21% and 11% respectively.

## Average PCA balances

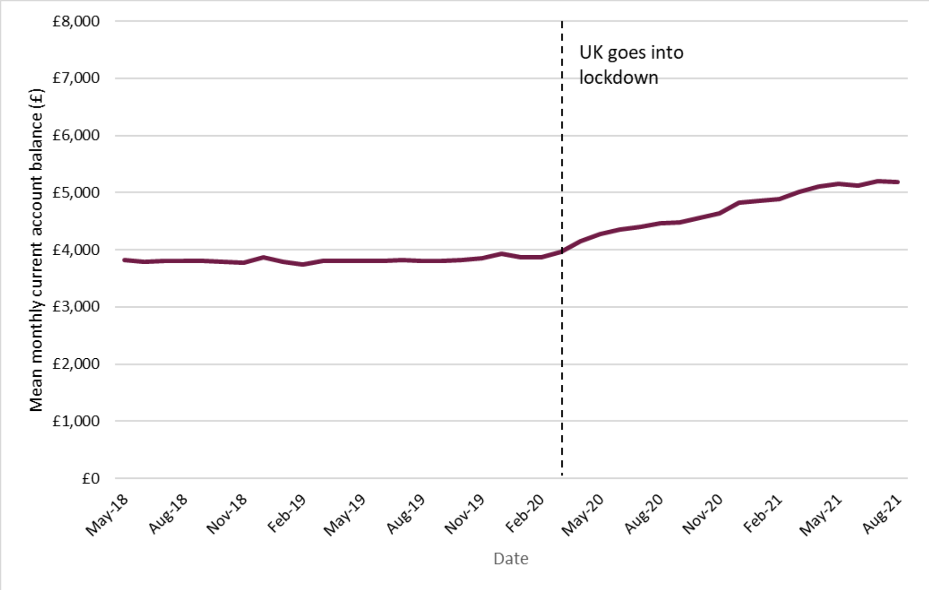
56. In this section we analyse current account balances, followed by overdraft balances. This gives us an indication of the intensity with which customers use their overdraft facility.

57. We restrict our sample to main accounts and divide by the number of holders on an account. We then calculate, at the bank level, the average account balance and then extrapolate to the market using bank market shares.

58. Figure 6 below shows that until the first quarter of 2020, the average account balance was stable at just under £4,000. This was followed by a steep increase in the first quarter of 2020, stabilising at just over £5,000 in July-August 2021.

59. These observations are consistent with a reduction in the proportion of overdraft users between 2020 and 2021.

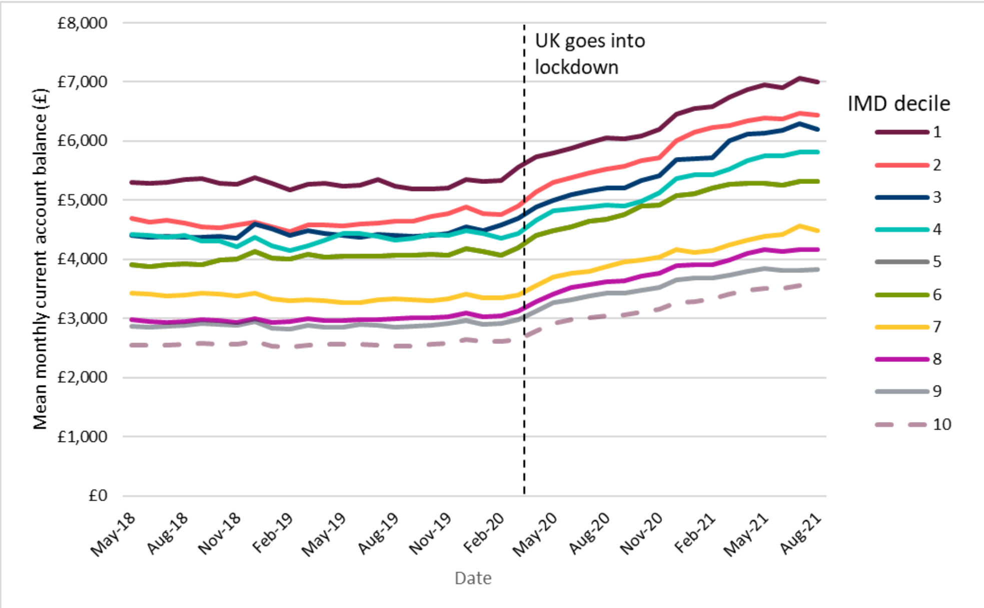
**Figure 6: Mean monthly current account balance, all PCA holders**




*Source: FCA analysis of PCA data*

60. We also look at current account balances by IMD decile. We group together accounts that belong to customers in a given IMD decile and then calculate the mean account balances within that group. The market-wide trend for account balances holds across all IMD deciles. We see a continuous increase in mean current account balances after March 2020 for all 10 groups.

**Figure 7: Mean monthly current account balance by IMD decile, all PCA holders**




*Note: 10 indicates the most deprived decile*
*Source: FCA analysis of PCA data*

61. Later in our analysis, we use IMD deciles as a proxy for financial vulnerability. The fact that the mean account balances scale monotonically with IMD decile (less deprived deciles have higher balances) increases our confidence that the IMD decile is a good proxy for financial vulnerability.

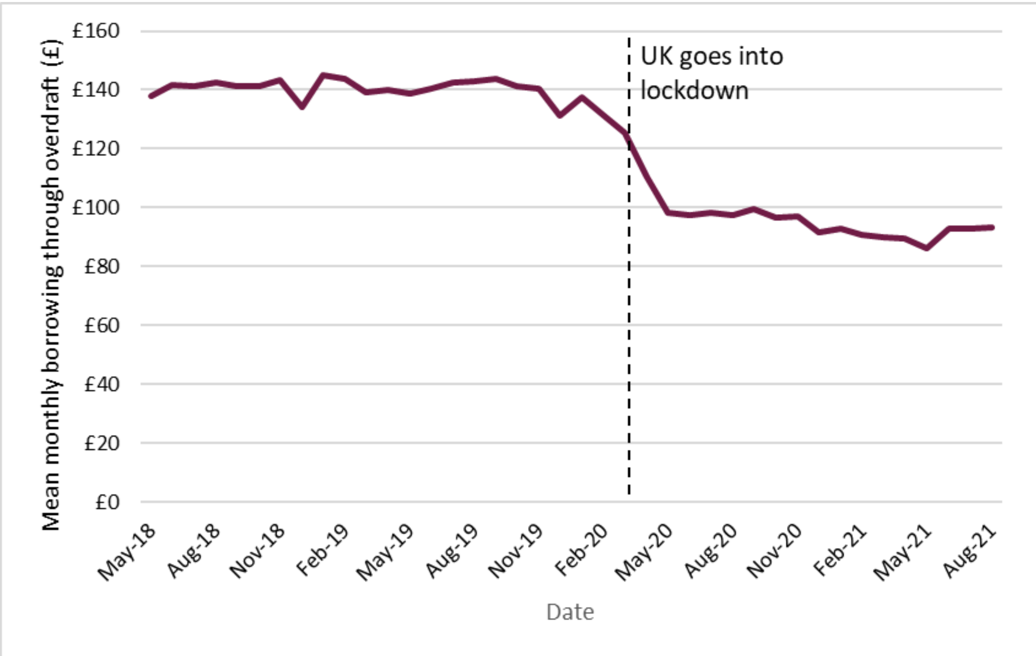
## Borrowing behaviour

62. We next analyse borrowing behaviour at the account level. We restrict the sample to accounts that were main accounts in at least one year of our reporting period. We further restrict the sample to those consumers who had a negative balance on at least one day in the sampling period.

63. To obtain the average overdrawn amount across accounts for a given month, we first calculate the average daily balance of each account in that month, assigning a balance value of zero for days where the account balance is non-negative. For accounts with multiple holders, we divide the reported daily balance by the number of account holders. We then aggregate across all accounts in our sample.

64. We see that in line with overall PCA balances, monthly borrowing through overdraft exhibited a flat trend pre-2020 and then fell sharply as the pandemic developed. This observation is consistent with the falling proportion of overdraft users and the increase in the average balance on current accounts.

**Figure 8: Mean monthly borrowing through overdraft**

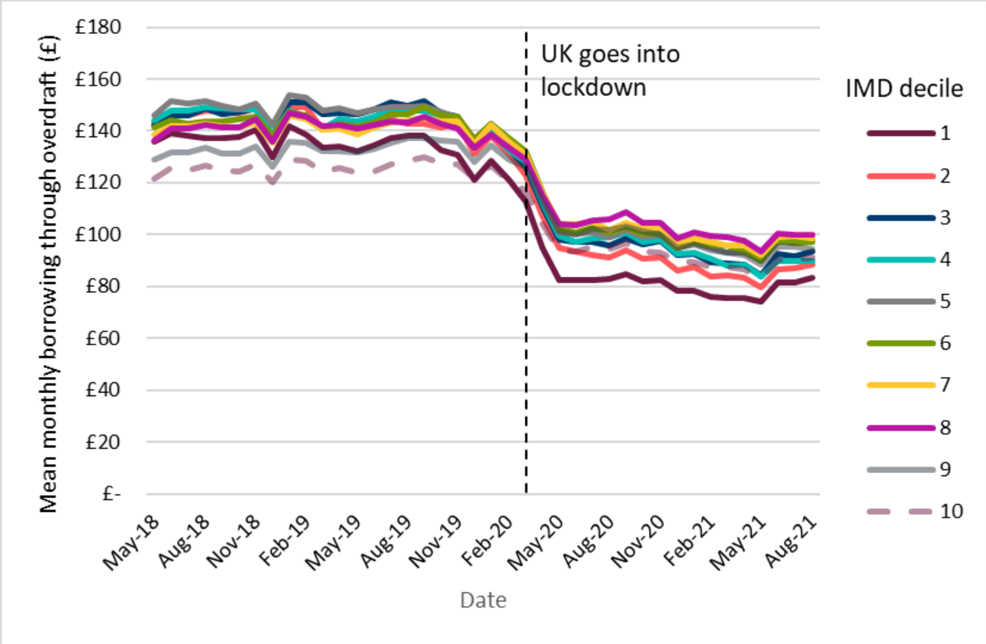



*Note: the sample includes only those accounts which had a negative balance on at least one day in the sampling period*
*Source: FCA analysis of PCA data*

65. Breaking overdraft borrowing by IMD decile shows that the pattern is stable across deciles. However, unlike average account balances, the average overdrawn balance does not increase monotonically with deprivation. This is expected because, while less wealthy consumers may have a higher need to borrow through their overdraft,

their ability to do so may be limited (e.g. because firms may not be willing to extend large arranged overdraft limits to lower income customers). Therefore, the relationship between deprivation and average borrowing balance is not monotonic.

**Figure 9: Mean monthly borrowing through overdraft by IMD decile (accounts that had a negative balance on at least one day in the sampling period)**




*Note: the sample includes only those accounts which had a negative balance on at least one day in the sampling period*
*Source: FCA analysis of PCA data*

## Arranged vs. unarranged borrowing

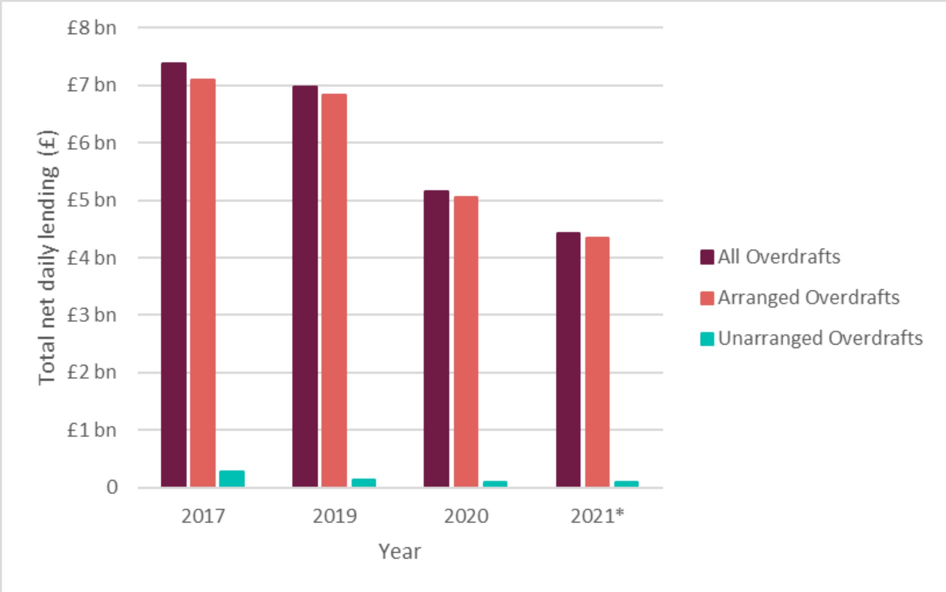
66. Unarranged lending occurs when a customer goes over their arranged overdraft limit or when they go overdrawn without an overdraft arrangement in place.

67. Figure 10 below illustrates that the total daily lending volumes for both arranged and unarranged overdrafts have been falling since 2017.

68. In CP18/42 we found that in 2017 firms lent around £279m each day through their unarranged overdraft line. In that year, unarranged lending equalled around 2% of arranged lending. In 2020, which is the last year for which we have full data, the total daily lending volume for unarranged overdrafts was £104m - a little over a third of 2017 values.

69. Arranged lending also decreased. In 2017 banks lent £7.1bn each day through their arranged line; in 2020 total daily arranged lending was £5.1bn.

**Figure 10: Total daily arranged and unarranged overdraft lending assets 2017-2021**




**Figures for 2021 are based on data up to August*
*Source: FCA analysis of PCA data*

## Total overdraft charges and the cost of overdraft credit

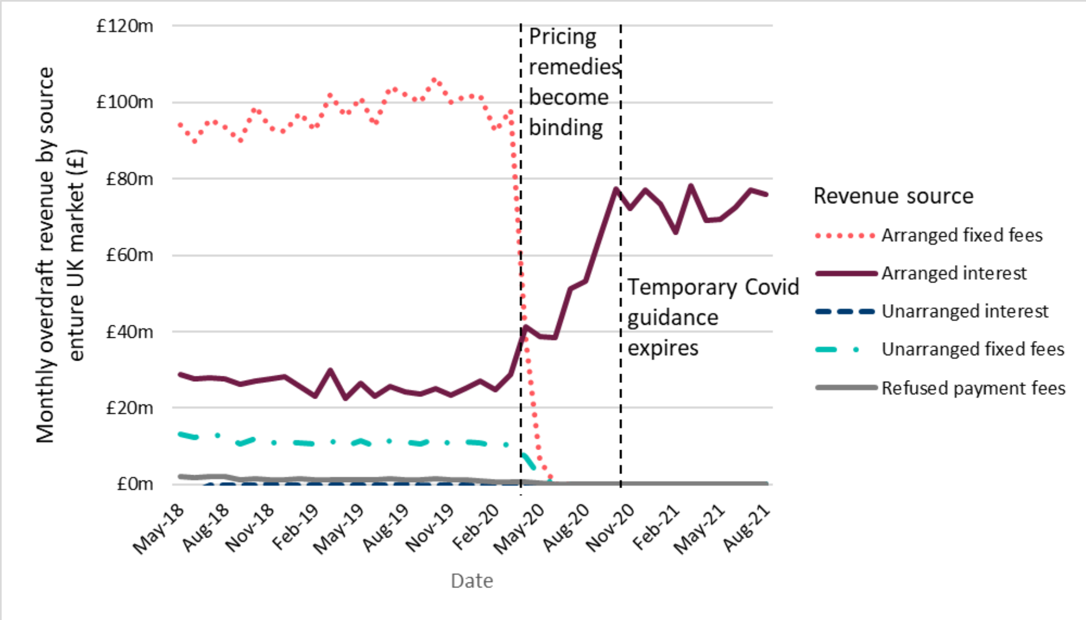
70. Figure 11 shows monthly overdraft revenues for the entire sector. Revenue is broken down to five sources - arranged fixed fees, arranged interest, unarranged fixed fees, unarranged interest and refused payment fees. We first calculate, for each participating firm, revenue from each stream and then use market shares to extrapolate to the market.

71. We observe that prior to our intervention, fixed fees for arranged lending were the main source of revenue for the sector, followed by arranged interest and unarranged fixed fees. Unarranged interest rates were never a significant component of banks' revenue. The figure also shows that refused payment fees were already a relatively insignificant revenue stream for the sector prior to the implementation of the fixed fee guidance (remedy 5) in June 2019.

72. The figure shows that the fixed fees from both arranged and unarranged overdrafts fell to zero shortly after our remedies became binding in April 2020. This shows that firms have fully complied with the policy. As per our expectations in CP18/42, arranged interest rates have increased since the introduction of pricing remedies to compensate for the fall in revenue.

73. This increase is particularly strong around October 2020, when our temporary Covid guidance was lifted. The temporary guidance will have put a downward pressure on interest rates as it required firms to "ensure that customers are not worse off when compared to prices charged prior to the publication of PS19/16".

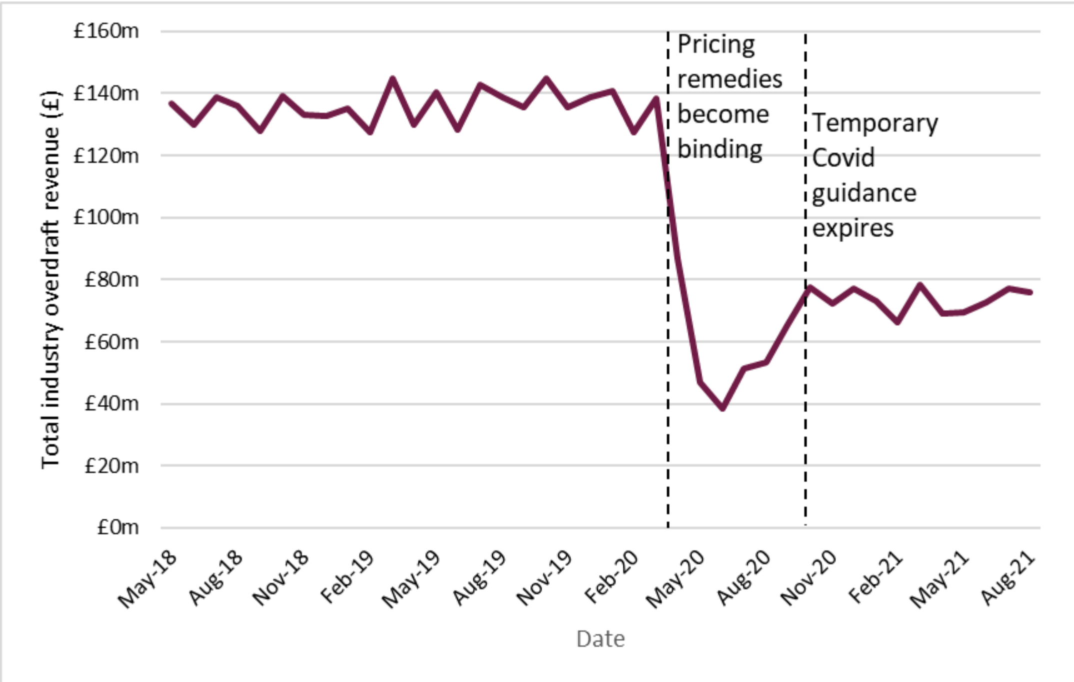
**Figure 11: Overdraft revenues by source – entire UK market**




*Source: FCA analysis of PCA data*

74. Figure 12 below shows how total overdraft revenue in the sector has changed over time, as the sum of the above 5 revenue streams.

**Figure 12: Total overdraft revenues – entire UK market**




*Source: FCA analysis of PCA data*

75. Average monthly industry revenue was £136m (£1.6bn per year) in the period May 2018 – March 2020 inclusive. Revenues fell sharply around April 2020, before

rebounding to a lower level - around £73 million per month (0.9bn per year) in the period October – August 2021.

76. The reduction in revenue is likely due to multiple factors, including the changes from PS19/16, reduced borrowing balances due to lockdown measures, as well as downward pressures on interest rates arising from the Covid temporary guidance. A large part of the fall in revenues can be explained by the lower demand for overdrafts (as evidenced by earlier observations of falling balances and rates of overdraft use); in the causal part of the paper (see Chapter 4) we show that about £500m of the £700m reduction in annual overdraft revenues was due to our pricing remedies.

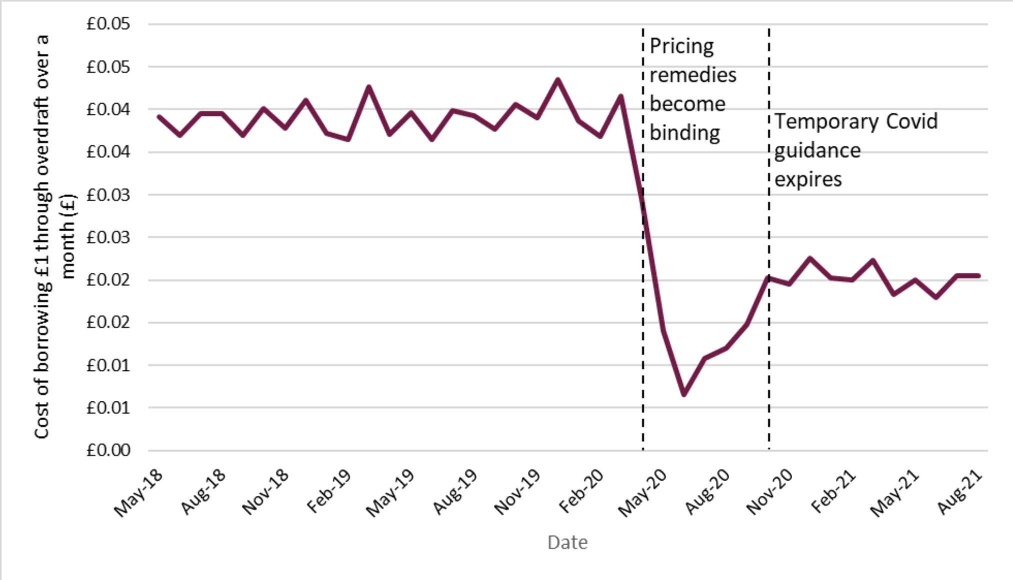
## The cost of overdraft credit

77. How the policy impacted the effective price of overdrafts is an important question which we investigate causally further in the paper. Here we begin with some initial observations. We define the cost per pound borrowed as the total monthly overdraft revenues divided by the total overdraft borrowing in a month. We also refer to this quantity as the "effective price" of overdrafts and use the terms interchangeably.

78. In order to estimate the effective price of overdrafts, we take the customers whose birthday falls on one of the 5 dates that are in the sampling rules for all 6 firms. This means that for these customers, we observe all accounts they may hold across the 6 providers. This allows us to build a customer-level view of charges. We then restrict the sample to those customers who incurred overdraft charges. We divide the amount they paid in charges in each month by the borrowing in that month. Figure 13 below shows that the cost per £1 borrowed was relatively stable around 4p, falling sharply around the time when our remedies became binding (and the first lockdown) and rebounding at around 2p after the lifting of the temporary Covid guidance.

79. We convert our measure of effective price to an effective annual rate (EAR) by applying the formula $EAR = (1 + effective\ price)^{12}$. Using this conversion, we estimate that the EAR firms charged was on average 61% before our intervention and 25% in the period after the lifting of the temporary Covid guidance.

**Figure 13: Estimated cost per pound borrowed – entire UK market**




*Source: FCA analysis of PCA data*

## Outcomes for customers in repeat use

80. In CP18/42 we identified a subset of overdraft users that were 'repeat users' of overdrafts and as a result were paying high fees and charges. We asked firms to ensure they were adequately monitoring overdraft use for these consumers and intervening appropriately to help them avoid financial difficulty. We gave firms scope to design their own strategies to do this, and to define what they consider to be a 'repeat user'.

81. In CP18/42 we looked at individuals that used their overdraft in every month of 2016 to give us an insight into the outcomes for the kinds of consumers we hoped the firms' 'repeat use strategies' would benefit. In line with this approach, in this evaluation we focus on individuals using an overdraft in every month of 2019, 2020 and 2021 respectively (for 2021 we restrict our scope to individuals that used an overdraft in every month until August, as we do not have data for the period September – December). Where we refer to 'repeat users' in the rest of this chapter, we mean individuals satisfying this definition, unless we specify otherwise.

82. One mechanism by which we expect our intervention to benefit repeat users is to reduce the likelihood of an individual being a repeat user. Therefore, we expect many of the repeat users before our intervention would no longer be repeat users following our intervention. To understand the changes experienced by those who were repeat users prior to the intervention (but not *necessarily* afterwards) we have also tracked changes to lending balances and charges for individuals who were repeat users in 2019, over the period January 2019 to August 2021.

83. The figures based around our definition of repeat use, in the first two subsections here, are aggregated at the customer level. We focus our analysis on 'main accounts', i.e. accounts that have median monthly deposits of greater than £500

across a given year. We also focus specifically on individuals for whom we requested data from all 6 banks in our sample. This is to ensure that, to the extent possible, our analysis looks at information on all accounts owned by these individuals.

84. As done for pricing, to scale from our sample to the whole population, we assume that the distribution of birth dates is random. By filtering to birth dates that we asked all the firms for, we balance the sample by firm's share of customers. Therefore, our average figures are appropriately weighted to banks by market share. We then use the assumption that 97% of UK adults have a PCA (taken from the CMA's report) and multiply this by ONS estimates of the UK adult population to get an estimate of the number of UK adults with a PCA. Finally, we multiply our averages by the estimated number of UK adults with a PCA to gain aggregate figures for the entire population.

85. In some instances, we compare repeat users to 'non-repeat-users', i.e. customers who do not use their overdraft in every month of the year (or do not use their overdraft at all during the year). In other instances, we compare against our entire sample of PCA customers; we say where this is the case.

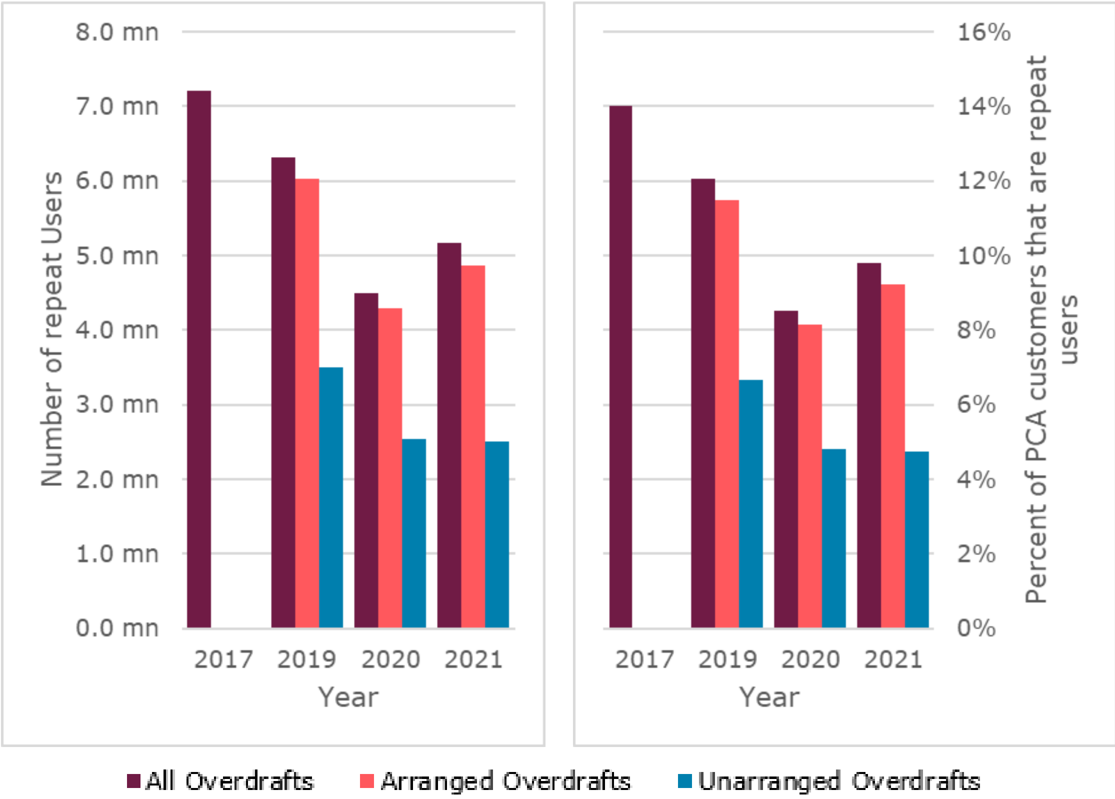
## Repeat users in 2019, 2020 and 2021

86. In this section we look at the amounts borrowed, and charges paid in 2019, 2020, and 2021 for repeat users in those years. For the 2021 figures we only have data for the first 8 months of the year so we have scaled the figures with the assumption that the average amount paid or borrowed for each of the first 8 months will be the same as for the final 4 months of the year.

### *Number of repeat users*

87. In CP18/42 we estimated that in 2017, 7.2 million people, or 14% of all PCA customers, were repeat users. Figure 14 suggests that there has been a material decrease since, down to 6.3 million repeat users in 2019 (12% of all PCA customers in that year), 4.2 million in 2020 (9% of all PCA customers) and 5.2 million in 2021 (10% of all PCA customers*).* Figure 14 shows that the number of customers using an arranged overdraft in every month followed a similar trend to those using any overdraft, however the number of repeat users of unarranged overdrafts fell between 2020 and 2021.

**Figure 14: Percentage and number of PCA customers who are repeat users**

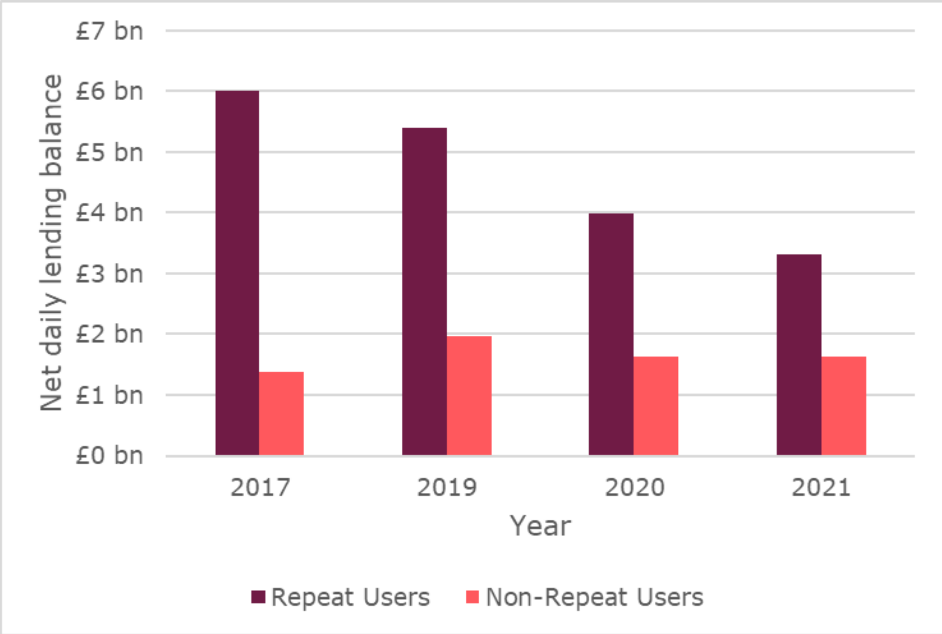



*Source: FCA analysis of PCA data*

*Borrowing by repeat users*

88. Figure 15 shows the total lending balance for repeat and non-repeat users. We calculate this by multiplying, for each year, the mean overdrawn end-of-day balance across accounts by the number of accounts.

**Figure 15: Net daily lending balances for repeat users and non-repeat users**




*Source: FCA analysis of PCA data*

89. In 2017, repeat users had a total lending balance of £6.0bn. In 2019 this fell to £5.4bn.

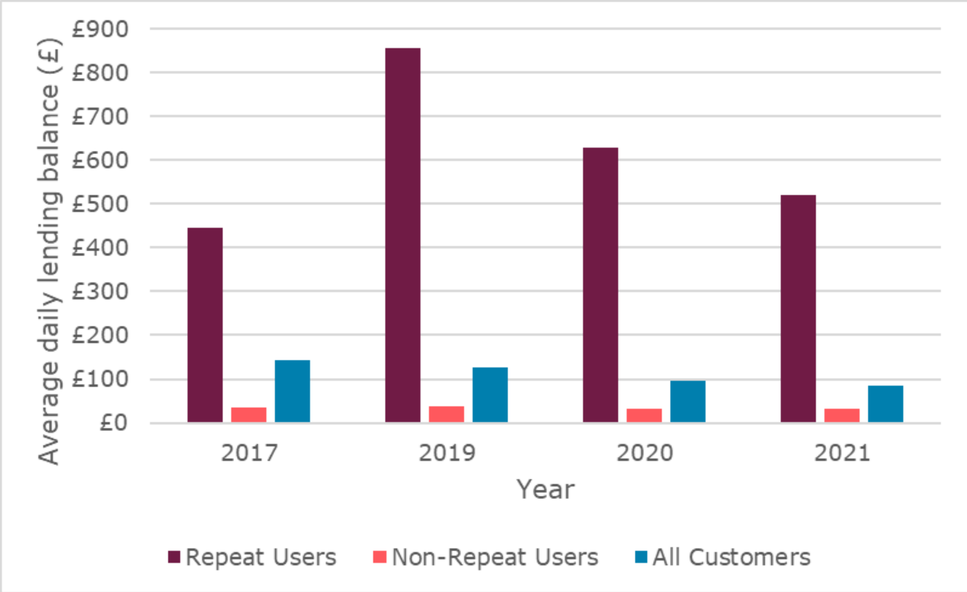
90. Total net daily lending for all customers fell between 2019 and 2021. This seems to have been largely driven by a reduction in borrowing by repeat users whose total net daily lending fell from £5.4bn to £3.3bn, compared to a smaller relative fall for non-repeat users from £2.0bn to £1.6bn.

91. We expect the fall in lending was larger for repeat users than non-repeat users as they were more likely to benefit from and respond to the interventions introduced over the period.

92. Figure 16 below shows that the decline in total lending to repeat users was not just driven by a decline in the number of repeat users. It shows that the average daily lending for repeat users fell from £854 in 2019 to £521 in 2021. This is consistent with our expectation that our remedies also led to a decrease in borrowing for repeat users.

93. We note that this could also be in part due to a cohort effect – the heaviest repeat-use borrowers in 2019 may have been exactly those treated by their firms' repeat use strategies, and because of this treatment they may not be repeat users in 2020 and 2021. However, our causal analysis in sections 4 and 5 of this Annex confirms that our remedies reduced customers' borrowing.

**Figure 16: Average daily lending for repeat users and non-repeat users**

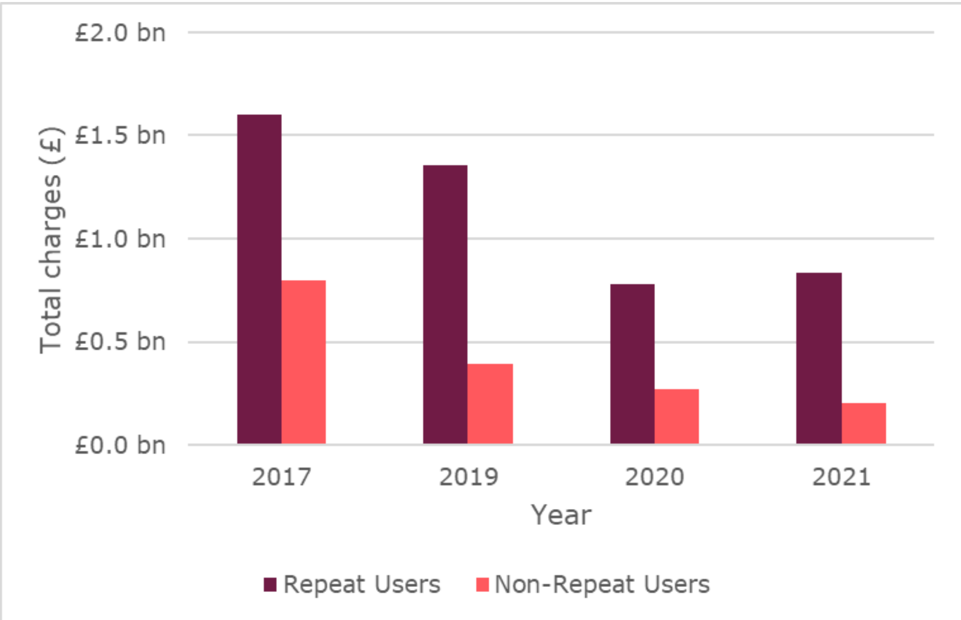



*Source: FCA analysis of PCA data*

*Charges for repeat users*

94. Repeat users typically pay more in overdraft fees and charges. In 2017, repeat users paid £1.6bn in overdraft fees and charges. Figure 17 shows that in 2019 they paid £1.4bn. In 2021 Repeat Users paid a total of £0.8bn. This represents a fairly significant decline against the amounts paid in 2019. For repeat users, there was a total decline of £0.5bn.

**Figure 17: Total Charges paid by repeat users and non-repeat users**

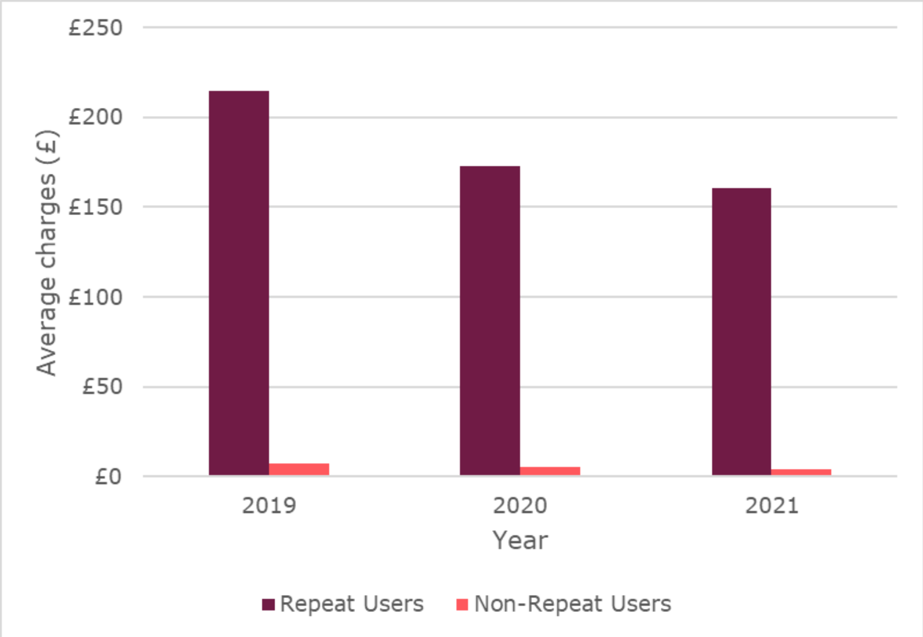



*Source: FCA analysis of PCA data*

95. Figure 18 shows that on average repeat users paid £215 in charges in 2019. In 2021 the average annual charge was £161.

96. As with borrowing earlier, we note that the fall in average charges could be due to a cohort effect. However, our causal analysis in chapters 4 and 5 of this Annex again confirms that our remedies reduced customers' overdraft charges.

**Figure 18: Average charges for repeat users and non-repeat users**




*Source: FCA analysis of PCA data*

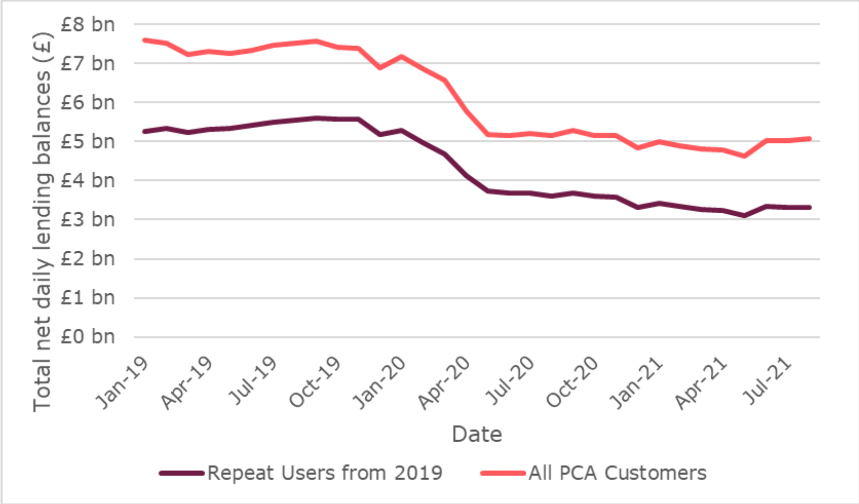
## Following repeat users from 2019

97. In this section we take the cohort of consumers that used their overdraft in every month of 2019 and follow their outcomes through 2019, 2020, and 2021. We expect that this group will have benefitted from the interventions.

### *Borrowing by customers who were repeat users in 2019*

98. Figure 19 shows that the total net daily lending to customers who were repeat users in 2019 (blue line) declined in 2020 and 2021.

**Figure 19: Total net daily lending for repeat users in 2019 and all PCA customers**

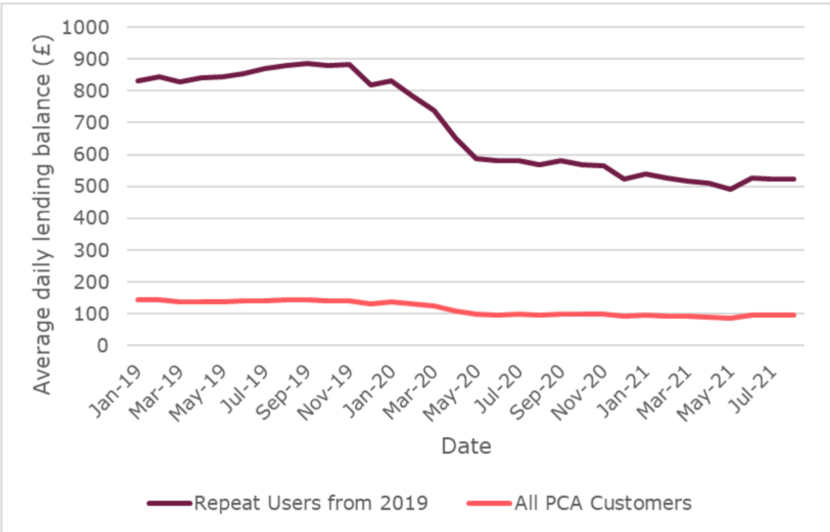



*Source: FCA analysis of PCA data*

99. The decline in borrowing for repeat users in 2019 is similar in magnitude to the decline in borrowing for all PCA customers. Between January 2019 and January 2021, borrowing for repeat users in 2019 fell from £5.2bn to £3.3bn per day, a total decline of £1.9bn. In the same period, borrowing across all PCA customers fell from £7.6bn to £5.1bn, a total decline of £2.5bn. This suggests that the decline in borrowing across the market was largely driven by the decline in repeat users' borrowing.

100. Figure 20 shows that the aggregate fall in lending balances is mirrored in the average fall in lending balances for repeat users.

**Figure 20: Average overdraft utilisation by repeat users in 2019 and all PCA customers**




*Source: FCA analysis of PCA data*

# 4 Analysis of the causal impact of pricing remedies

## Introduction

101. In this chapter, we investigate the causal impact of our pricing remedies. In Chapter 3 we found that total overdraft borrowing, total overdraft revenues, as well as the effective price of overdrafts had fallen significantly since the beginning of 2020. However, we cannot conclude that movements in these quantities are attributable solely to our remedies. Changing spending patterns after the pandemic, the effects of our Temporary Covid guidance and other factors may have also influenced outcomes.

102. To isolate the effect of our policy from other factors, we exploit variation in the level of fixed fees and unarranged interest rates customers were subject to before our intervention. This variation effectively creates quasi-random differences in the treatment intensity where treatment is defined as the change in the pricing components individuals face on their overdraft.

103. Prior to our intervention some consumers will have paid low to no fixed fees, meaning that their pricing structure was relatively unchanged by our pricing remedies. Other consumers would have been paying high fixed fees before our intervention and will have seen radical changes in their pricing structure due to our remedies. This variation allows us to estimate the treatment effect of pricing remedies on the outcomes of interest using a difference-in-difference approach with continuous treatment similar to Card and Krueger's 1994 paper.

104. We note that the refused payment fee remedy became binding for all individuals at the same time, so we cannot estimate the effects due to this component of the policy. Under the assumption that the refused payment fee guidance impacted consumers with high and low treatment intensity equally, our approach uncovers the effect due to the other pricing remedies.

105. We also consider unintended consequences of our remedies. We investigate whether our intervention has led to reduced arranged overdraft limits (which would indicate a reduced access to overdrafts), as well as whether borrowing balances on other credit products have increased due to our intervention. To do this, we check for decreases in overdraft limits and increases in borrowing balances on a set of consumer credit products using the same econometric model. Our results present no evidence that pricing remedies have led to unintended consequences. However, we do not make statements about the precise effect of our remedies on these outcomes due to limitations we describe in the sections below.

106. This chapter proceeds as follows: we first describe the subsample of data we use for the causal estimation; we then present our estimation approach with a description of the institutional setting, the identifying assumptions, and the econometric model; we conclude with a discussion of results.

## Data

107. For this part of the analysis, we use a subsample of our PCA dataset, and link the data to an internally held Credit Rating Agency (CRA) dataset. We restrict the sample to customers born on one of the 5 dates common to all the participating banks. This allows us to build a customer-level view of outcomes by identifying all accounts a given customer may have with a participating bank.

108. Restricting the sample in this way results in a total sample size of 579,760 individuals. We further restrict the sample to those consumers for whom we observe at least one positive price component in both periods, meaning that the individuals in our sample have an arranged or unarranged facility, i.e., they could incur overdraft charges. After some further data cleaning discussed below, the final sample size we use in our regression analysis consists of 314,089 individuals.

109. Our data contains the main outcome variables (total charges, average monthly borrowing, and effective price at the individual level), along with a set of covariates which determine overdraft use (including current account balances, savings balances, risk scores, etc.).

110. By linking our sample to the CRA data, we obtain variables such as the age of customers, their IMD decile, and borrowing through other credit products.

111. Our data also include measures of the different pricing components that overdraft customers face. This allows us to observe pricing structures at the customer level and identify by how much a given individual is affected by our intervention.

112. Our sampling period runs from May 2018 to August 2021, which is the last full month of data in our sample. All variables are aggregated at the month level. We describe the key variable definitions in the sections below.

### Main outcome variables

113. We consider three main outcome variables - average monthly borrowing, total monthly charges, and effective overdraft price.

### Average borrowing

114. We define average monthly borrowing as the time-weighted end-of-day overdraft balance in a given month. To calculate this, we look at the end-of-day PCA balances on a given account and assign a value of 0 for days when the account had a non-negative balance. We then take the average of daily balances for a given month. We also divide the account balances by the number of holders on the account to control for joint accounts. We then sum the average borrowing on all accounts a consumer has. Hence, for a consumer with N accounts, where each account $j = 1, \ldots, N$ has $K_j$ holders, the average monthly borrowing in month $t$ with $M$ days is calculated as:

$$\text{Average monthly borrowing}_{it} = \frac{1}{MN}\sum_{d=1}^{M}\sum_{j=1}^{N} min\left(\frac{\text{End-of-day overdraft balance}_{djt}}{K_j}, 0\right)$$

where $d$ identifies the particular day in month $t$.

## Total charges

115. The next outcome variable is the total monthly overdraft charges a customer pays across all their accounts. We identify overdraft charges from transactions data (see Chapter 2 for a discussion of the transactions data). To calculate total charges, we sum all transactions that relate to overdraft interest and fees in a given month on all of a customer's accounts. We divide each transaction amount by the number of holders on an account to control for joint accounts. Therefore, the total monthly charges in month $t$ for a customer with $N$ accounts where each account $j = 1, \ldots, N$ has $K_j$ holders is:

$$\text{Total monthly charges}_{it} = \sum_{j=1}^{N} \frac{\text{Monthly charges}_{jt}}{K_j}$$

## Effective price

116. The final outcome we consider is the effective price of overdrafts at the consumer level. This is simply defined as:

$$\text{Effective price}_{it} = \frac{\text{Total monthly charges}_{it}}{\text{Average monthly borrowing}_{it}}$$

117. The effective price can therefore be interpreted as the cost per pound borrowed in a given month - e.g., a value of 0.05 would represent a cost of 5 pence per £1 borrowed over a month.

118. To prevent extreme cases of effective price affecting results, we drop observations where the effective price for borrowing £1 over a month is more than £1 or under £0. These cases can occur respectively when:

   i. due to the charging cycle, a customer pays a high amount in a given calendar month due to debts accrued in the preceding month, but their average borrowing in that month is low, or

   ii. when the customer received refunds for their overdraft facility in a given month that exceed the charges they paid in that month

119. Removing these observations causes us to lose 1.78% of our sample when analysing effective price.

## Treatment variables

120. To identify treatment intensity, we look at the individual pricing components customers faced in every month. We have identified six such components:

   i. Arranged interest rate (EAR)

   ii. Arranged daily fee (£)

   iii. Arranged monthly fee (£)

   iv. Unarranged interest rate (EAR)

v. Unarranged daily fee (£)

vi. Unarranged monthly fee (£)

121. Participating firms provided us with daily data on each of these pricing components at the account level. To build a monthly view of the price applied to a given account, we take a simple average within a month.

122. Since customers may hold different products with different banks, one customer may face multiple levels of the same pricing component. What they actually pay through a given component will depend on which of their accounts they use more often. For example, take a consumer with two accounts, one of which has only an arranged daily fixed fee applied to it while the other has only an arranged interest rate. If the consumer uses the former account more, the arranged daily fee will be the more relevant pricing component for that consumer.

123. We aggregate the pricing components from the account level to the consumer level and account for differences in use such as in the above example. We do this by taking a weighted average of pricing components across all of a given consumer's accounts. The weight is given by the overdraft balance on a given account in the month relative to the total borrowing balance across all of the consumer's accounts in that month. Where borrowing is zero across all accounts, we just take the simple average of pricing components across that consumer's accounts.

124. For example, the arranged interest rate faced by consumer $i$ in month $t$ is defined as:

$$\text{Arranged interest rate}_{\text{it}} = \begin{cases} w_{jt} \times \text{EAR}_{jt}, \text{if average monthly borrowing}_{jt} \neq 0 \\ \dfrac{1}{N}\displaystyle\sum_{j=1}^{N} \text{EAR}_{jt}\text{ , if average monthly borrowing}_{jt} = 0 \end{cases}$$

125. Where $EAR_{jt}$ is the effective annual rate for arranged borrowing on account $j$ in month $t$. The weight $w_{jt}$ is given by:

$$w_{jt} = \frac{\text{Average monthly borrowing}_{jt}}{\sum_{j=1}^{N} \text{Average monthly borrowing}_{jt}}, \text{if} \sum_{j=1}^{N} \text{Average monthly borrowing}_{jt} \neq 0$$

126. where $\text{Average monthly borrowing}_{jt}$ is the average monthly borrowing on account $j$ in month $t$.

127. We note that in our sample, a small group of overdraft customers (654) appear to be paying fixed fees beyond October 2020 (the last period when the Temporary Covid guidance applied). The presence of such customers may be due to specific individual cases, or due to a reporting error. These customers have negligible impact on our results, but we remove them from our sample for consistency.

## Covariates

128. Our data include a list of variables correlated with overdraft use. These are:

i. IMD decile

ii. Unarranged overdraft eligibility

iii. Total current account balances

iv. Total monthly savings

129. The IMD decile variable was discussed in Chapter 2 of this annex. We identify the IMD decile of consumers based on the most recent postcode that their PCA provider had on record at the time of the data collection, hence IMD decile is time-invariant.

130. Unarranged overdraft eligibility is a dummy variable taking the value 1 if the consumer is eligible for unarranged lending and zero otherwise. Unarranged overdraft eligibility is equal to 1 if the consumer is eligible for unarranged lending on at least one of their accounts.

131. Total current account balances are calculated as the sum of the average daily balance on all accounts a consumer holds.

132. The total monthly savings is the sum of the average end-of-day balances across all savings accounts held by a given consumer.

## Additional outcome variables

133. To check for unintended consequences, we estimate our preferred model on a set of additional outcome variables. These include:

i. the overdraft limit, defined as the sum of the overdraft limits across all accounts belonging to a given consumer

ii. average monthly balances on other consumer credit products, including:

a. credit cards

b. charge cards

c. rent to own (RTO)

d. mail order

e. personal loans

f. store cards

g. home collected credit

h. consumer hire

i. high-cost short term credit (HCSTC)

j. the sum of all of the above

134. The products we selected are all credit products with a monthly repayment frequency that we observe in the data. This allows us to construct an estimate of the total monthly borrowing for these products and look for treatment effects using the same methodology as for the main outcome variables.

135. We identify balances on these credit products from our CRA data.

## Descriptive statistics

136. We next split our data into a pre-intervention period (from May 2018 to October 2019) and a post-intervention period (from November 2020 to August 2021 inclusive). Defining the post-intervention period in this way ensures our results are not affected by our temporary Covid guidance.

137. Splitting the sample in this way reduces the time periods in our sample from 28 to 2, making our analysis more computationally efficient. This could result in a loss of statistical power due to losing the month-to-month variation in the data. However, in our Assumptions section, we show that overdraft outcomes are stable across time (i.e. they do not exhibit any trends or cyclicality), which means that we do not lose much variation by taking this approach.

138. We also show that the treatment effect of pricing remedies is likely to be stable over time (see figures 21-23), which justifies focusing on the average monthly treatment effect, rather than estimating a treatment effect for every post-intervention period.

139. We report in Table 1 below the descriptive statistics for all variables in our dataset before and after the intervention. We restrict the sample to customers who had an overdraft facility or were eligible for unarranged borrowing. This leaves us with 314,089 customers out of 579,760. This means that the proportion of PCA customers who have an overdraft facility in our sample is 54.18%.

140. We note that the sample size reduces when we look at savings accounts and the effective price. In the case of savings accounts this is due to some consumers in the sample not having a savings account with one of the participating firms. In the case of effective price, this is due to some consumers not incurring overdraft charges, and hence no effective price can be calculated for them.

**Table 1: Descriptive statistics of consumer-level subsample, by pre- and post-intervention period**

| | Pre-intervention | | | | Post-intervention | | | |
|---|---|---|---|---|---|---|---|---|
| **Variable** | **Sample size** | **Mean*** | **Min** | **Max** | **Sample size** | **Mean*** | **Min** | **Max** |
| **Monthly charges (£)** | 314,089 | 3.03 (10.70) | 417 | 728 | 314,089 | 1.55 (9.03) | -540 | 890 |
| **Monthly borrowing (£)** | 314,089 | 126.49 (531.74) | 0 | 81,512 | 314,089 | 86.53 (471.88) | 0 | 79,940 |
| **Arranged limit (£)** | 314,089 | 725.23 (1,281.56) | 0 | 96,291 | 314,089 | 662 (1,176) | 0 | 56,996 |
| **Proportion eligible for unarranged borrowing** | 314,089 | 49% (48%) | 0 | 1 | 314,089 | 45% (49%) | 0 | 1 |
| **Current account balance (£)** | 314,089 | 6,118 (19,264.96) | -81,512 | 2.4m | 314,089 | 8,365 (27,980) | -79,941 | 5.5m |
| **Savings account balance (£)** | 214,510 | 16,139 (63,867) | -3,621 | 7.9m | 208,517 | 18,535 (70,788) | -114 | 7.6m |
| **Effective price (pence per £1 borrowed)** | 155,126 | 4.14 (7) | 0 | 100 | 101,982 | 1.89 (0.03) | 0 | 89 |

**Standard deviation in parentheses*

*Source: FCA analysis of PCA data*

141. Consistent with our descriptive statistics of overdraft revenues in Chapter 3 (see Figure 12), the average charges in this subsample fall from £3.03 per month in the pre-intervention period to £1.55 per month in the post-intervention period. Monthly borrowing falls from £126.49 to £86.53. Also, as per our earlier findings in Chapter 3, we find an increase in the average current account balances as well as average savings. The proportion of unarranged overdraft eligibility is relatively stable across the two periods falling slightly from 49% to 45%.

142. The effective price in our sample falls from £0.04 per £1 borrowed over a month to £0.02 per £1 borrowed. We note that this is equivalent to a 62% EAR in the pre-intervention period and 25% EAR in the post-intervention period. The formula we apply to convert pence per pound borrowed to an EAR is $(1 + effective\ price)^{12}$.

143. We also consider the pricing elements applied to consumers' accounts in our sample. Table 2 shows that the arranged interest rate firms applied to accounts in the post-intervention period is on average 31%. The fact that the average effective price in the post treatment period is lower than this may be explained by the application of interest free buffers and grace periods, which we do not explicitly control for. Furthermore, consumers who face lower than average interest rates may be using their overdrafts more intensely, which would result in a lower average effective price in our sample.

144. The table also shows that all fixed fee components of overdraft pricing fall to zero in the post-intervention period. Unarranged interest rate increases from 1.7% to 3.1%. We note that the unarranged overdraft costs in the pre-intervention period were high

due to the high unarranged daily and monthly fees. Therefore, despite the increase in the unarranged interest rate, the cost of unarranged overdrafts is lower post-intervention due to the removal of fixed fees. The maximum arranged and unarranged interest rates post-intervention are 50% and 40% respectively, which is lower than the pre-intervention average effective price of 62% we reported above.

145. We note that approximately 20,000 individuals in our sample are not paying an arranged overdraft EAR, but are paying an unarranged overdraft EAR only, hence the minimum value of arranged interest rates in the post-intervention period is 0.

**Table 2: Descriptive statistics of pricing at the consumer level, by pre- and post-intervention period**

| | Pre-intervention | | | | Post-intervention | | | |
|---|---|---|---|---|---|---|---|---|
| **Variable** | **Sample size** | **Mean*** | **Min** | **Max** | **Sample size** | **Mean*** | **Min** | **Max** |
| **Arranged interest (EAR)** | 314,089 | 7.7% (8.4%) | 0 | 19.9% | 314,089 | 31.0% (12%) | 0 | 49.9% |
| **Arranged daily fee (£)** | 314,089 | 0.672 (1.025) | 0 | 3 | 314,089 | 0 (0) | 0 | 0 |
| **Arranged monthly fee (£)** | 314,089 | 0.511 (1.529) | 0 | 6 | 314,089 | 0 (0) | 0 | 0 |
| **Unarranged interest (EAR)** | 314,089 | 1.7% (4.6%) | 0 | 0.219 | 314,089 | 3.1% (9.5%) | 0 | 39.9% |
| **Unarranged daily fee (£)** | 314,089 | 1.984 (2.206) | 0 | 6 | 314,089 | 0 (0) | 0 | 0 |
| **Unarranged monthly fee (£)** | 314,089 | 2.519 (7.876) | 0 | 72 | 314,089 | 0 (0) | 0 | 0 |

**Standard deviation in parentheses*

*Source: FCA analysis of PCA data*

146. Finally, Table 3 below summarises the average change from pre- to post-intervention in the level of each pricing element. The standard deviation in column 4 shows that there is substantial variation in the movements of pricing components. The fact that the maximum values for the change in fixed fees are zero means that there are customers who saw no change in some fixed fee components. We find that out of 314,089 individuals in our sample, 59,608 consumers saw no change in the fixed fee component of their overdraft, i.e. they did not pay fixed fees prior to our intervention.

**Table 3: Descriptive statistics of changes in pricing components from the pre- to the post-intervention period**

| Pricing component | Sample size | Mean | Standard deviation | Minimum | Maximum |
|---|---|---|---|---|---|
| **Arranged interest (EAR)** | 314,089 | 23.2% | 12% | -20% | 50% |
| **Arranged daily fee (£)** | 314,089 | -0.672 | 1.03 | -3 | 0 |
| **Arranged monthly fee (£)** | 314,089 | -0.511 | 1.53 | -6 | 0 |
| **Unarranged interest (EAR)** | 314,089 | 1.4% | 1.1% | -22% | 40% |
| **Unarranged daily fee (£)** | 314,089 | -1.984 | 2.21 | -6 | 0 |
| **Unarranged monthly fee (£)** | 314,089 | -2.519 | 7.88 | -72 | 0 |

*Source: FCA analysis of PCA data*

## A note on representativeness and linking to CRA data

147. For a large part of our analysis, we include variables from the CRA data. This creates the risk that our sample becomes biased as the population holding credit products present in the CRA data is different to the population of overdraft users. For example, we may expect more deprived IMD deciles to be overrepresented in the CRA data.

148. To ensure this is not the case, we look at the distribution of PCA customers across the IMD deciles in our subsample (before removing customers without an overdraft facility) after linking to the CRA data.

149. Table 4 below shows that each IMD decile in England represents roughly 7-8% of the entire subsample. As a percentage of English residents in our sample, each decile represents roughly 10% of the sample. This is what we would expect with random sampling of consumers with a PCA. This increases our confidence that linking our main sample to the CRA data does not bias our analysis. Finally, in the results section we show that our central estimates are stable after controlling for variables coming from the CRA data.

**Table 4: Sample size by IMD decile, all consumers observed across the 6 participating firms**

| IMD decile* | Sample size | Proportion of total sample |
|---|---|---|
| 1 | 43,021 | 7% |
| 2 | 43,790 | 8% |
| 3 | 44,489 | 8% |
| 4 | 45,004 | 8% |
| 5 | 46,203 | 8% |
| 6 | 46,190 | 8% |
| 7 | 46,600 | 8% |
| 8 | 46,487 | 8% |
| 9 | 43,857 | 8% |
| 10 | 40,478 | 7% |
| Consumers outside of England | 72,255 | 12% |
| Unmapped consumers | 61,386 | 11% |
| Total sample | 579,760 | 100% |

**IMD decile 10 = most deprived*
*Source: FCA analysis of PCA data*

## Methodology

150. In this section we introduce our estimation strategy. We use an econometric approach that compares outcomes for people who saw relatively big changes in their pricing structure to people who saw small changes in pricing. Under the assumption that all changes in pricing are attributable to our policy and that time-varying factors determining overdraft use are common to people with high treatment intensity and low treatment intensity, the interpretation of our results is causal.

### Institutional setting

151. Before PCA providers adopted the new regime, they could set daily and monthly fixed fees as well as higher interest rates for unarranged overdrafts compared to their arranged line. This means that before the intervention, some consumers would have faced pricing structures that were very different from those in the post-intervention period. For example, some consumers only paid fixed daily fees for their arranged and unarranged borrowing, but under the new regime they face a single interest rate. Conversely, other consumers were facing a pricing structure similar to that in the post-intervention period where they paid low to no fixed fees.

152. The standard deviation on the pre-intervention pricing we observe Table 3 demonstrates this variation in pricing structures. We also show later in the paper that c. 18% of our sample did not pay any fixed fees prior to the intervention.

153. If PCA provider and product choice was purely random, then a comparison of means between a high treatment intensity group and a low treatment intensity group would be sufficient to uncover the treatment effect of the policy. However, PCA provider choice is not entirely random and the methodology we apply (see below), accounts for that. Our method only assumes that the time-varying components for high and low treatment intensity consumers are the same, which implies that trends in outcomes for the two groups would be parallel. We present evidence for this in figures 21-23.

154. Additionally, Table 5 shows that pre-intervention values of consumer characteristics are not strongly correlated with changes in any of the pricing components. The highest correlation coefficients we observe are those for unarranged overdraft (UOD) eligibility and the various pricing components, but even in those cases the correlation is mild. This means that consumer characteristics are weakly correlated with pricing changes, increasing our confidence that unobservable factors determining overdraft use are uncorrelated with treatment intensity.

**Table 5: Correlation coefficients for changes in pricing components and pre-intervention consumer characteristics**

| Variable | Arranged interest rate | Arranged daily fee | Arranged monthly fee | Unarranged interest | Unarranged daily fee | Unarranged monthly fee |
|---|---|---|---|---|---|---|
| **Account balance** | 0.02 | -0.12 | 0.09 | 0 | 0.04 | 0.03 |
| **Monthly charges** | -0.1 | 0.03 | 0.26 | 0.01 | 0.03 | 0 |
| **Monthly borrowing** | -0.02 | -0.05 | 0.27 | 0.02 | -0.03 | 0 |
| **OD Limit** | 0.05 | 0.05 | -0.25 | -0.08 | 0.04 | 0.02 |
| **UOD eligibility** | 0.17 | -0.45 | 0.25 | 0.28 | -0.24 | 0.16 |
| **Average savings** | -0.04 | 0.01 | 0.05 | 0 | 0.02 | 0.01 |
| **IMD decile** | 0.01 | 0 | 0.06 | -0.01 | -0.03 | -0.03 |
| **Year of birth** | -0.12 | 0.17 | -0.1 | 0.13 | -0.03 | -0.02 |

*Source: FCA analysis of PCA data*

155. These results are not surprising, given what we know about consumer’s choice of PCA providers. The Personal Current Account Investigation commissioned by the CMA in 2015 surveyed 4,549 consumers alongside face-to-face conversations with 43 individuals. Survey respondents ranked quality of customer service as most important, followed by convenience, interest rates, and charges.

156. However, the face-to-face conversations suggested that most consumers open a PCA when they started working and made their choice based on three factors:

i. the bank their family/employer use

ii. the first provider they encountered

iii. recommendations

157. These three factors suggest close to random choice of current accounts as customers chose based on extrinsic factors rather than intrinsic factors likely to influence treatment assignment.

158. The difference between survey results, qualitative conversations, and structural estimations also suggest customers are not clear on why they chose their PCA provider. It suggests they pick (almost) randomly – based on, for instance, the nearest branch to them – then justify this choice ex post on other factors.

159. Therefore, the process that determines PCA provider choice, and by extension, the pricing structure that a given consumer faces, is not driven by differences in pricing. This is consistent with the parallel trends we present below, hence we are confident that a Difference in Differences (DID) methodology is appropriate.

## Econometric model

160. The large variation in changes in pricing components (see Table 3) is equivalent to variation in the intensity of treatment and allows us to compare outcomes between more affected and less affected consumers.

161. We do this by implementing a Difference in Differences in Reverse (DDR) model with continuous treatment following Kim and Lee (2019) and Card and Krueger (1994).

162. We estimate the following equation:

$$\Delta Y_i = \alpha + GAP_i'\beta + X_{i,t-1}'\gamma + \epsilon_i \qquad (1)$$

163. Where $\Delta Y_i$ is the change between the average monthly pre-intervention outcome of interest (borrowing, charges or effective price) to its average post-intervention value for consumer $i$.

164. $\alpha$ is an intercept term which captures the common time trend in outcomes for all consumers, $X_{i,t-1}$ is a $k$-vector of covariates in the pre-intervention period for customer $i$, $GAP_i$ is vector of length 6 which captures the level change in the following pricing components for consumer $i$: arranged interest, arranged daily fee, arranged monthly fee, unarranged interest, unarranged daily fee and unarranged monthly fee. For example, a customer who paid only an arranged daily fee of £3 and unarranged daily fee of £10 in the pre-intervention period and then a single interest rate of 30% for both arranged and unarranged borrowing post-intervention, would have a value of $GAP$ of $[0.3, -3, 0, 0.3, -10, 0]'$. $\epsilon_i$ is an error term.

165. $\beta$ is a vector of length 6 which contains the coefficients of interest. For example, when the outcome variable is total monthly charges, the entries in $\beta$ capture by how much a unit change in a particular pricing component is associated with changes in monthly charges.

166. An important part of our policy was to make pricing clearer for consumers, which may have affected outcomes separately from the actual changes in the pricing components. We do not observe a variable that captures consumers' understanding of prices, hence this effect will be picked up by our estimate for $\beta$. Therefore, when we analyse monthly borrowing, our estimates of $\beta$ cannot be interpreted as demand

curve slopes. However, when we take the sum of the individual entries in $\beta$ multiplied by the average change in the respective pricing component in the sample, we get an estimate of the average treatment effect (ATE) of our policy. This effect will capture the direct effects of the policy through the reduction in certain pricing components, as well as the indirect behavioural effect through the improved understanding of prices.

167. As we analyse differences in outcomes from the pre-intervention to the post-intervention period, any time-fixed determinants of overdraft outcomes are differenced out.

168. Equation (1) corresponds to Equation (1b) in Card and Krueger (1994). In this specification, treatment is continuous – i.e. instead of a treated and untreated group, we observe continuous differences in treatment intensity between individuals. This specification allows us to use all the variation in fixed fees and interest rates.

169. As a robustness check, we estimate an alternative specification where we define treatment as the removal of fixed fees. This approach is closer to a standard DID case with binary treatment. In this setting, overdraft customers who paid fixed fees in the pre-intervention period are considered treated, while the control group consists of those individuals who did not pay any fixed fees pre-intervention. This allows us to verify the parallel trends assumption with a plot of the outcome variables we consider (see the Assumptions section below).

170. The estimation equation for this model is:

$$\Delta Y_i = \alpha + D_i\beta + X'_{i,t-1}\gamma + \epsilon_i \tag{2}$$

171. Where $D_i$ is a dummy variable taking the value 1 if customer $i$ paid fixed fees prior to the intervention. This equation is analogous to Equation (1a) in Card and Krueger (1994) with the caveat that the control group in this case always receives treatment (never subject to fixed fees). We therefore use a terminology following Kim and Lee (2019), where we refer to customers who never paid fixed fees as the "always treated" group and the ones who were affected by the policy as the "switched group". In this case, the "always treated" group is the one with low treatment intensity and the "switched group" has high treatment intensity.

172. Kim and Lee (2019) refer to this setting as Difference-in-Differences in Reverse (DDR). The main difference between classical DID and DDR is that in DDR the parallel trends assumption must hold in the post-intervention rather than in the pre-intervention period. Our data structure allows us to examine both pre- and post-intervention trends. We discuss this in more detail in the assumptions section.

173. Under the assumption that the other pricing remedies affect both groups equally, $\beta$ in Equation (2) only captures the effect of banning fixed fees.

174. There are two disadvantages to the approach in Equation (2), however. These are:

   i. The model does not allow us to utilise variation in pricing changes within the two groups, hence it has lower precision.

   ii. The assumption that the other pricing remedies affect both groups equally is relatively strong. The "always treated" group may have received some additional treatment through, for example, the requirement that unarranged overdraft interest rates should not exceed arranged interest rates. This requirement may have affected the always treated group differently to the switched group, which would introduce bias in a DDR setting. The direction of this bias is also difficult to assess. We discuss this issue in the assumptions section.

175. As such, we use the results from estimating Equation (2) as a robustness check for estimates based on Equation (1). Since the banning of fixed fees was a key part of the policy, we would expect the estimated effect from Equation (2) to have the same sign and similar magnitude to the effect estimated using Equation (1).

176. When running these regression models, we define pre-intervention outcomes as the monthly average from May 2018 to October 2019. October 2019 is the last month before any bank introduced the new pricing regime. The post-intervention period runs from November 2020 – the first month in our sampling period where the temporary Covid guidance on overdrafts does not apply, to August 2021 – the last full month of data in our sample. This ensures that post-intervention outcomes are not affected by the Temporary Covid guidance.

## Distributional impacts

177. Our modelling in CP18/42 predicted reductions in overdraft charges by IMD decile, hence we are not only interested in the average treatment effect of the policy, but also in its distribution across IMD deciles.

178. We therefore modify Equation (1) by adding interaction terms between the 6 pricing components and the 10 IMD deciles. The interacted model is:

$$\Delta Y_i = \alpha + IMD_{1,i} \times GAP_i'\beta_1 + IMD_{2,i} \times GAP_i'\beta_2 + \cdots + \qquad (3)$$

$$+ IMD_{10,i} \times GAP_i'\beta_{10} + \cdots + X_{i,t-1}'\gamma + \epsilon_i$$

179. where $IMD_{k,i}$ for $k \in (1,2,\ldots,10)$ is a dummy variable that takes the value 1 if the IMD decile of consumer $i$ is equal to $k$ and 0 otherwise. $\beta_k \ for \ k \in (1,2,\ldots,10)$ is the vector of coefficients on $GAP$ specific for IMD decile $k$.

180. Running this model allows us to recover coefficients on $GAP$, and treatment effects, specific to a given decile.

## Assumptions

181. The interpretation of estimates of $\beta$ as causal relies on a set of identifying assumptions:

**Assumption 1:** conditional on covariates, overdraft customers exposed to different levels of treatment intensity are subject to the same unobservable time-varying factors determining overdraft outcomes

**Assumption 2:** our model does not include controls affected by the treatment

**Assumption 3:** all changes in pricing components captured in $GAP_i$ are due to our policy

**Assumption 4** (only relevant when estimating Equation (2)): there are no spill-over effects to the always treated group

182. Assumptions 1 through 3 are common to all models we estimate, while Assumption 4 applies only when estimating model 2. Below we discuss each assumption in turn.

*Assumption 1: Unobservable time-varying factors are common to low and high treatment intensity customers, conditional on observable characteristics*

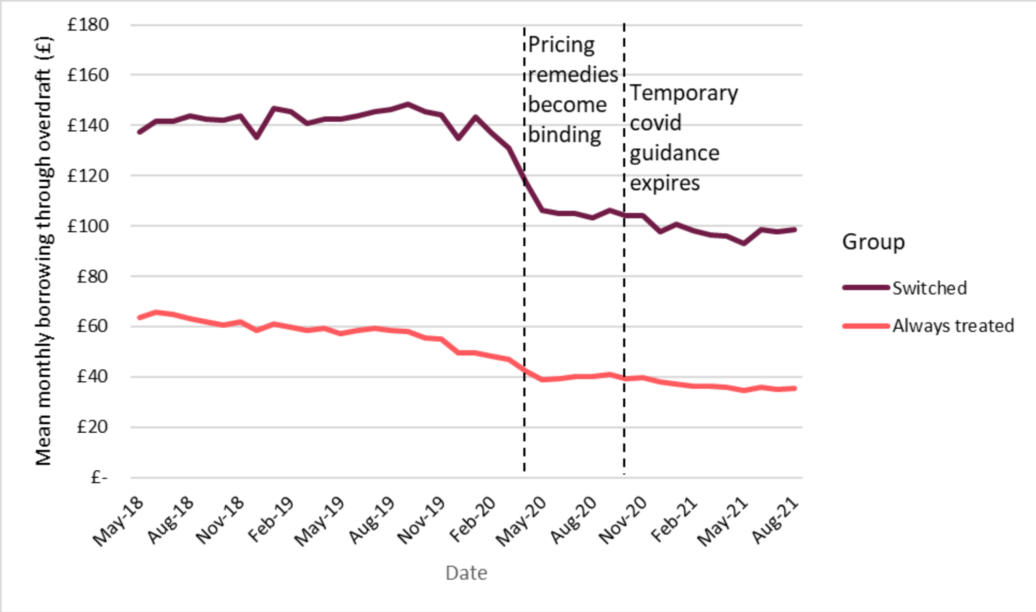
183. Our preferred specification uses continuous treatment rather than binary treatment. Nonetheless, the parallel trends assumption from the canonical DID model still applies in the sense that time-varying factors determining overdraft outcomes must be independent of treatment intensity.

184. To demonstrate why we think this is the case, we split the sample into an always treated and a switched group as per the specification in Equation (2) and plot the time trends in outcomes. We show below that the assumption is satisfied when treatment is binary. This increases our confidence that when we model treatment as continuous, the assumption still holds.

185. The canonical DID estimator assumes that trends are parallel pre-intervention – when both groups are untreated. In Equation (2), however, one group is always treated, which may result in non-parallel trends pre-intervention. Therefore, we look for parallel trends in the post-intervention period, when both groups are treated, following Kim and Lee (2019).

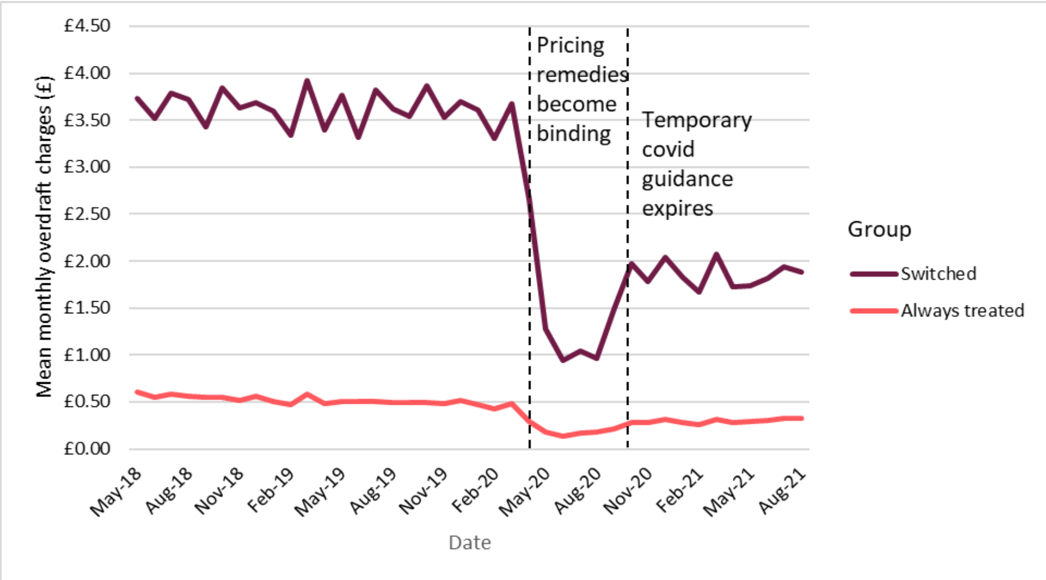
186. There are 254,481 overdraft customers in our sample who paid fixed fees in the pre-intervention period (i.e., the "switched" group) and 59,608 who did not (the "always treated" group). The figures below plot the time trends for the switched and always treated groups as defined in Equation (2).

**Figure 21: Trends in monthly borrowing - always treated vs. switched group**



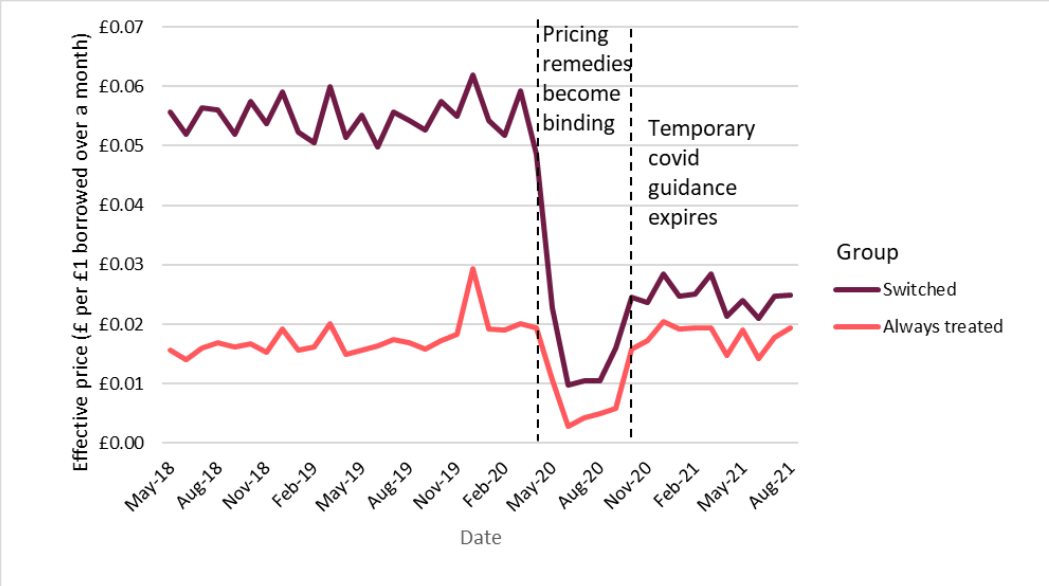

*Source: FCA analysis of PCA data*

**Figure 22: Trends in monthly charges - always treated vs. switched group**




*Source: FCA analysis of PCA data*

**Figure 23: Trends in effective price - always treated vs. switched group**




*Source: FCA analysis of PCA data*

187. Consumers in the switched group, had on average higher levels of monthly borrowing, higher average monthly charges and were paying a higher effective price for their overdraft borrowing pre-intervention.

188. In the post-intervention period both groups are subject to treatment (absence of fixed fees) and we would expect the parallel trends to hold in that period. We can visually verify that this is the case with respect to all three outcomes. The fact that we have parallel trends without conditioning on covariates means that we can relax Assumption 2 to "Common time-varying components for treated and untreated", which means that our estimator for $\beta$ is robust even when we do not control for observable covariates. Nonetheless, we include controls for completeness. In the results section we show that estimates are stable to including controls, consistent with unconditional parallel trends.

189. With DDR, parallel trends in the pre-intervention period are not required, but the fact that we have parallel trends before we intervened increases our confidence that time-varying factors determining overdraft use are common to both groups.

190. We would expect that after the intervention, the outcomes for the switched group converge to those for the always treated. This is precisely what we observe across the set of outcomes considered here.

*Assumption 2: No controls affected by the treatment*

191. One standard issue with DID is the inclusion of time-varying covariates that are affected by treatment. Inclusion of such controls may attenuate results as the coefficients on those variables will capture some of the effects of treatment.

192. Our data contains variables such as the average overdraft balance, balances on savings accounts, eligibility for unarranged borrowing, as well as borrowing amounts

through other credit products. All these variables predict overdraft use and we may wish to control for them in our regressions. However, these variables may also be affected by treatment, and so their inclusion could attenuate results.

193. Caetano et. Al (2022) point out that researchers normally deal with this issue by including only the pre-intervention value of controls. In our context, this approach may still introduce biases as our research design includes a control group that effectively receives some level of treatment (i.e. is not subject to fixed fees) in both periods.

194. We therefore only include time-fixed controls – the IMD decile and year of birth of consumers. We cannot include time-varying controls that evolve independently from treatment (monthly income, for example), as we do not observe such variables in our data.

*Assumption 3: All changes in pricing are entirely due to our policy*

195. When estimating our models, we assume that changes in pricing are entirely due to our policy. This is necessary as we equate treatment to changes in pricing components at the consumer level. Intuitively, if something else other than the policy (e.g. a Bank of England (BoE) rate change), affected the overdraft pricing components, we would falsely be attributing the effects of this event to our policy.

196. The assumption is weak when we consider fixed fee components – it is straightforward to assume that if the arranged daily fee on a given account in the pre-intervention period was £5 and fell to £0 in the post-intervention period, this is entirely due to our intervention. We are confident in this as we show in Figure 24 below that the average levels of fixed fees in our sample were stable in the pre-intervention period and the reduction is sharp and large in magnitude at the time of intervention.

197. The assumption is stronger when we consider the interest rate components of pricing. Although the average interest rate is stable before and after the intervention, other market factors may have contributed to lower interest rates in the post-intervention period.

198. One such factor is the introduction of competition remedies in CP18/42. These remedies mandated that bank and building societies:

i. provide digital eligibility tools

ii. improve visibility and content of key information about overdrafts

iii. remove overdrafts from the definition of available funds

iv. provide text messages and digital push notifications about customers' overdraft use

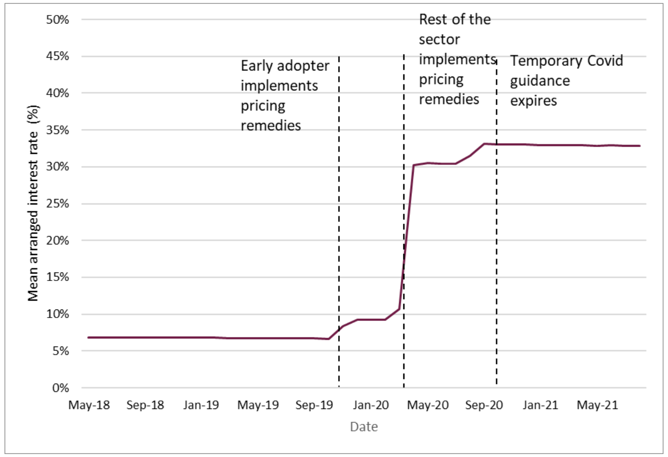
These remedies came into force in December 2019. However, we believe that competition remedies do not bias our results because they were introduced as a complement to an existing CMA regulation from February 2018. The CMA regulation would already be impacting all consumers in our sample from the beginning of the sampling period. Therefore, the marginal effect of competition remedies is likely to be small in comparison to that of pricing remedies. Furthermore, we do not see discrete changes in overdraft prices at the time of the introduction of competition remedies in December 2019 (see Figure 24).

199. Nonetheless, competition remedies may interact with our pricing remedies. Hence, our estimates of benefits capture the effect of pricing remedies against a counterfactual where only the competition remedies were introduced.

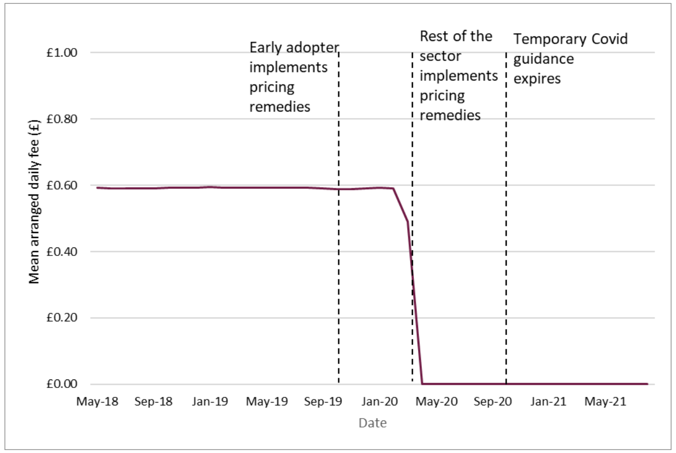
200. Another source of downward pressure on overdraft interest rates is the Bank of England rate cut from 0.75 percentage points to 0.1 percentage points in March 2020. This roughly coincides with the introduction of our policy and so may bias results – reductions in overdraft charges due to lower borrowing costs for banks could be attributed to our pricing remedies. However, we believe that the effect would be negligible as the APR offered on overdrafts (31% on average in the post-intervention period) is much higher than the BoE rate (0.1 in the post-intervention period). Even with a 1:1 carry-through, overdraft interest rates are not likely to have been much higher in the absence of the BoE rate cut.

201. To demonstrate why we believe price changes are attributable solely to our policy, we examine the time series of the 6 different overdraft pricing components. We discuss below what this means for our identification strategy.

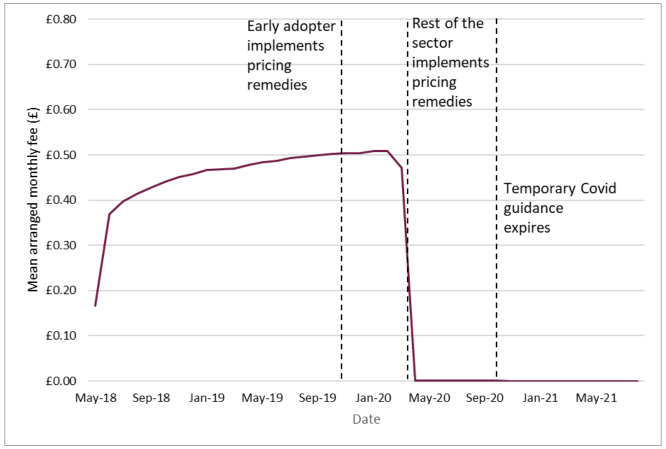
**Figure 24: Dynamics of pricing components**



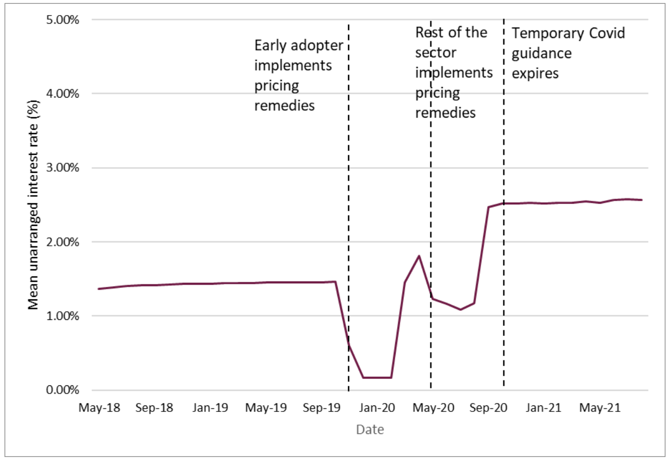

(a) Arranged interest rate



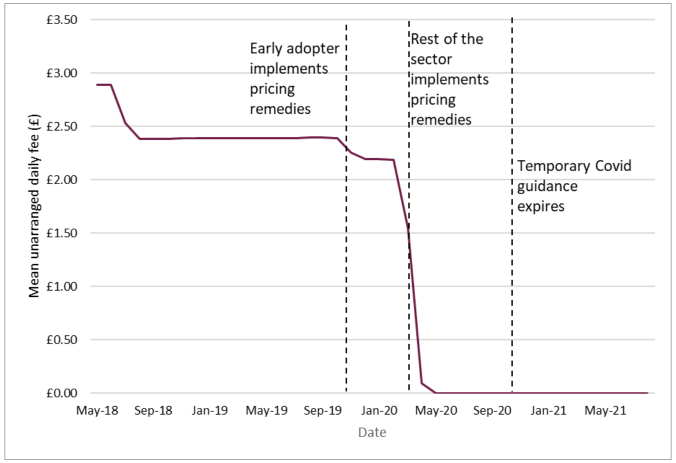

(b) Arranged daily fee



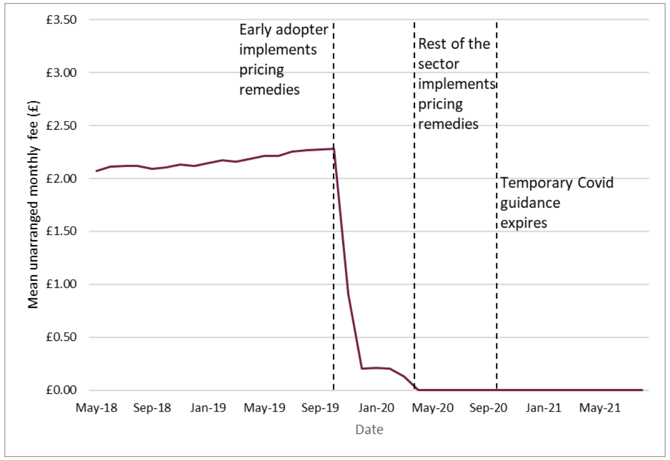

(c) Arranged monthly fee




(d) Unarranged interest rate




(e) Unarranged daily fee




(f) Unarranged monthly fee

*Source: FCA analysis of PCA data*

202. Panel (a) of Figure 24 shows that average arranged overdraft interest rates were essentially flat before November 2019 when the first firm in our sample introduced the new pricing regime. After that we see a sharp increase once the sector adopts the new pricing regime, followed by a further increase close to the expiration of the Temporary Covid guidance.

203. The average arranged daily fee in panel (b) is also flat in the pre-intervention period, and then sharply drops in April 2020.

204. Panel (c) shows a slight increasing trend in the pre-intervention period for arranged monthly fees. This increase is followed by a sharp drop after pricing rules became binding. If the pre-intervention trend had continued in the absence of our intervention, the counterfactual monthly fee would have been higher than the pre-treatment average. However, the presence of such a trend would attenuate results rather than inflate our benefit estimates. This is because our analysis assumes that the intervention has reduced arranged monthly fees from £0.51 (see Table 2) to £0, when in reality the reduction may have been larger. Hence, in the presence of this trend we would expect our results to be a lower bound on the actual effect of the policy.

205. Panel (d) shows that the unarranged overdraft interest rate falls sharply at the time the early adopter in our sample begins to move their customers to the new pricing regime. The unarranged interest rate then exhibits strong fluctuations in the period February - October 2020. This variation is likely caused by our Temporary Covid guidance on overdrafts, which required PCA providers to ensure customers were not paying more than under the old regime. Firms met this requirement in various ways - from the introduction of interest-free buffers to the suspension of interest rates for extended periods. These measures could explain the fluctuations we observe. To ensure that our results are not affected by the Temporary guidance, we define our post-intervention period from November 2020 (inclusive) onwards - after the guidance had expired.

206. The average unarranged overdraft daily fees dropped sharply in August 2018 by approximately £0.50 (contrary to what we would expect given the central bank interest rate increase in that period), but other than that, they exhibit a flat profile pre-intervention, followed by a sharp drop at the time of our pricing remedies becoming binding.

207. The average unarranged monthly fee (panel (e)) exhibits an increasing trend pre-intervention and a sharp drop at the time of the early adopter introducing the new pricing regime. So, similar to the arranged monthly fee in panel (c), this does not introduce an upward bias to our benefit estimates.

208. The absence of a downward trend in any of the pricing components pre-intervention, coupled with the sharp discrete changes at the time of introduction of our remedies, indicates that changes in pricing are solely due to our policy. We also note the lack of rebound of prices to pre-intervention levels, which means that the treatment effects we estimate are likely to be stable over time.

*Assumption 4: No spill-over effects*

209. In classical DID settings, the researcher needs to assume that the control group does not experience treatment. When estimating Equation (2) treatment is defined as the removal of fixed fees and we cannot control for changes in the interest rate component of pricing (because the interest rate will be affected by the banning of fixed fees, i.e. it is a *bad control*). This means that the "always treated" group may be affected by the policy if the interest rate on their accounts changed due to the other elements of our pricing remedies. In order to interpret $\beta$ as the causal effect of removing fixed fees then, we would have to assume that the change in interest rate components due to the other elements of the policy is the same for both groups.

210. This assumption is not testable as we do not know how much of the change in the interest rate is due to the banning of fixed fees and how much is due to the other elements of the policy.

211. To illustrate what this assumption entails, we look at the average pre- and post-intervention levels of arranged and unarranged EAR for the always treated and the switched group. Arranged EAR for the switched group increased from 5.79% to 29.16%. For the always treated group, the average arranged EAR increased from 16.09% to 33.11%. We would therefore need to assume that if fixed fees were not removed, the change in the interest rate for the switched group would have been the same as that for the always treated group – i.e. an increase of 17.02 percentage points.

212. Likewise, unarranged EAR for the switched group increased from 1.85% to 2.89%, while for the always treated group, the average unarranged EAR increased from 0.74% to 1.44%. We would therefore have to assume that if fixed fees were not removed, the unarranged overdraft interest rate would have increased by 0.7 percentage points for the switched group.

213. These are relatively strong assumptions, the violation of which would introduce biases to our estimates of Equation (2). We cannot know for sure whether the actual effect of the other pricing remedies on the switched group is more or less than these magnitudes, so the direction of the bias would be difficult to assess.

214. We therefore prefer the specification in Equation (1) as it allows us to model directly the effect of the changes in the interest rate component of pricing. When we take this approach, we control for the level of treatment that consumers in the always treated group may have received through the changes in the interest rate component of their facility.

## Results

### Continuous treatment

215. We begin with a presentation of the results from estimating the parameters of Equation (1). We estimate two models for each of the three main outcome variables – one without controls, and one which controls for the geography and year of birth of consumers. Results are presented in Table 6 below.

216. We control for geography by including dummy variables for the IMD decile if the consumer has an English residency, as well as dummy variables that indicate whether the consumer is based in Scotland, Wales, or Northern Ireland. We also include the year of birth of consumers to control for age.

217. We show that coefficients are stable after adding controls. This is what we would expect under the unconditional parallel trends assumption. We are therefore confident that results are representative of the UK market and that linking our main sample to the CRA data does not introduce major biases.

**Table 6: Regression output from estimating Equation (1)**

| | Outcome variable | | | | | |
|---|---|---|---|---|---|---|
| | Monthly borrowing | | Monthly charges | | Effective price | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **AOD interest rate** | 97.921 *** | 99.362 *** | -0.430 ** | -0.410 ** | -0.020 *** | -0.019 *** |
| | (8.215) | (8.294) | (0.189) | (0.191) | (0.003) | (0.003) |
| **AOD daily fee** | -0.862 | -1.475 | 0.454 *** | 0.505 *** | 0.019 *** | 0.020 *** |
| | (0.965) | (0.983) | (0.022) | (0.023) | (0.0004) | (0.0004) |
| **AOD monthly fee** | 35.677 *** | 36.200 *** | 0.953 *** | 0.951 *** | 0.007 *** | 0.007 *** |
| | (0.485) | (0.490) | (0.011) | (0.011) | (0.0001) | (0.0001) |
| **UOD interest rate** | 157.323 *** | 153.944 *** | 2.237 *** | 2.307 *** | 0.011 *** | 0.012 *** |
| | (8.038) | (8.215) | (0.185) | (0.189) | (0.003) | (0.003) |
| **UOD daily fee** | 8.433 *** | 8.638 *** | 0.295 *** | 0.273 *** | 0.003 *** | 0.003 *** |
| | (0.404) | (0.412) | (0.009) | (0.009) | (0.0001) | (0.0001) |
| **UOD monthly fee** | -0.743 *** | -0.780 *** | 0.005 ** | 0.007 *** | 0.0001 *** | 0.0002 *** |
| | (0.098) | (0.099) | (0.002) | (0.002) | (0.00003) | (0.00003) |
| **Geography** | NO | YES | NO | YES | NO | YES |
| **Year of birth** | NO | YES | NO | YES | NO | YES |
| **Constant** | -32.391 *** | -471.219 *** | -0.014 | 20.566 *** | 0.002 *** | 0.041 |
| | (1.804) | (84.283) | (0.042) | (1.941) | (0.001) | (0.036) |
| **Observations** | 314,089 | 310,602 | 314,089 | 310,602 | 93,245 | 92,706 |
| **$R^2$** | 0.018 | 0.019 | 0.026 | 0.027 | 0.085 | 0.089 |
| **Adjusted $R^2$** | 0.018 | 0.019 | 0.026 | 0.027 | 0.085 | 0.089 |
| **Residual Std. Error** | 394.466 | 395.933 | 9.086 | 9.119 | 0.075 | 0.075 |
| **F Statistic** | 980.039 *** | 302.813 *** | 1,381.900 *** | 429.172 *** | 1,436.475 *** | 452.872 *** |
| ***Note:*** | *p <0.1; **p <0.05; ***p <0.01 | | | | | |

*Source: FCA analysis of PCA data*

218. Table 6 contains the coefficients on the various pricing components in $GAP$ when the outcome variable is monthly borrowing (columns 1 and 2), monthly charges (columns 3 and 4) and effective price (column 5 and 6).

219. Our policy aimed to both directly reduce the cost of borrowing by removing fixed fees and change behaviour by making prices simpler.

220. If prior to the intervention consumers perceived prices as lower than what they were actually paying (due to complicated pricing structures), we could expect them to

perceive the cost of overdrafts as higher in the post-intervention period, when prices are more salient, and reduce borrowing accordingly.

221. If we had a proxy variable for consumers' understanding of the price, and were able to include this in our regression, we could expect that its coefficient would capture the effect of making prices simpler. We would then expect all the coefficients on pricing components to have a negative sign. However, we do not observe such a variable. Therefore, the coefficients of pricing components pick up the response to changes in the actual price, as well as the effect of improved understanding of pricing.

222. We cannot predict which coefficients would be more or less affected by this (for example we see that the coefficient on unarranged monthly fees has the expected negative sign), hence we cannot interpret the estimates of $\beta$ as the slope of a demand curve. However, we are interested in the combined effect of all 6 pricing components rather than the effect of changes in each individual pricing component.

223. When we take the sum of the coefficients in our regressions weighted by the average change in the relevant pricing component in the sample, we get an estimate of the ATE of the policy (which captures the effects of both changing the pricing structure and customers' comprehension of prices). We discuss our ATE estimates in the section below.

224. We note that coefficients are relatively stable to the inclusion of the IMD and year of birth. We show below that the implied ATE is also relatively unaffected by the inclusion of controls.

225. Similar to the discussion on monthly borrowing, we are interested in the joint effect of movements in the different pricing components on charges and effective prices, rather than in any individual component.

*Average treatment effect*

226. When estimating the model with continuous treatment, every coefficient in $GAP$ captures the response to the change in a particular pricing component. We therefore multiply the coefficient estimates on $GAP$ by the average value of $GAP$ (i.e. the average change in the corresponding pricing component) in the sample. Our ATE estimate is therefore $\overline{GAP}'\hat{\beta}$ where $\overline{GAP}$ is the sample average of $GAP$ and $\hat{\beta}$ are the coefficient estimates from Table 6.

227. For example, when the outcome variable is charges, the estimates for coefficients on $GAP$ are $(-0.41, 0.51, 0.95, 2.31, 0.27, 0.007)$ (see Table 6, column 4). The average value of the $GAP$ in the sample are $(0.23, -0.67, -0.51, 0.014, -1.98, -2.52)$ (see Table 3). The average treatment effect is therefore: $-0.41 \times 0.23 + 0.51 \times (-0.67) + 0.95 \times (-0.51) + 2.31 \times 0.014 + 0.27 \times (-1.98) + 0.007 \times (-2.52) \simeq 1.45$.

228. Table 7 reports the average treatment effects from the models in Table 6 computed in this way.

**Table 7: Average treatment effects of our pricing remedies (consumers with an overdraft facility)**

| Model | Outcome variable | Controls included? | Central estimate | 95% lower bound | 95% upper bound |
|---|---|---|---|---|---|
| 1 | Average monthly borrowing | No | -7.564 | -10.819 | -4.309 |
| 2 | | Yes | -7.447 | -10.732 | -4.162 |
| 3 | Total monthly charges | No | -1.461 | -1.536 | -1.386 |
| 4 | | Yes | -1.448 | -1.524 | -1.373 |
| 5 | Effective price | No | -0.028 | -0.029 | -0.027 |
| 6 | | Yes | -0.028 | -0.029 | -0.027 |

*Source: FCA analysis of PCA data*

229. Our preferred estimates are based on the models with controls. To extrapolate the reduction in charges of £1.45 a month (£17.40 per year) to the population of overdraft users we multiply by the total number of account holders (53 million) and again by the proportion of customers who either have an overdraft facility or are eligible for an arranged overdraft (54.18%). The total benefit figure we get is therefore a £500 million reduction in annual charges.

## Binary treatment

230. We now repeat the analysis by running the specification in Equation (2). Since treatment in this case is binary, the coefficient interpretation on the treatment variable changes. When estimating the model with continuous treatment, every coefficient in $GAP$ captures the response to the change in a particular pricing component. In the above section we therefore multiplied the coefficient estimates on $GAP$ times the average value of $GAP$ in the sample to get the average treatment effect. In the specification with binary treatment, $\beta$ captures the response to the total package and is therefore our estimate for the average treatment effect on the treated. Therefore, the coefficients on $D$ reported in Table 8 below are comparable to the ATE estimates with continuous treatment we reported in Table 7.

**Table 8: Regression output from estimating Equation (2)**

| | Outcome variable | | | | | |
|---|---|---|---|---|---|---|
| | Monthly borrowing | | Monthly charges | | Effective price | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ***D*** | -20.590*** (1.811) | -20.938*** (1.839) | -1.537*** (0.042) | -1.530*** (0.043) | -0.033*** (0.001) | -0.033*** (0.001) |
| **Geography** | NO | YES | NO | YES | NO | YES |
| **Year of birth** | NO | YES | NO | YES | NO | YES |
| **Constant** | -23.273*** (1.630) | 187.362** (82.365) | -0.230*** (0.038) | 24.179*** (1.941) | 0.0001 (0.001) | -0.263*** (0.037) |
| **Observations** | 314,089 | 310,602 | 314,089 | 310,602 | 93,245 | 92,706 |
| **$R^2$** | 0.0004 | 0.001 | 0.004 | 0.005 | 0.018 | 0.021 |
| **Adjusted $R^2$** | 0.0004 | 0.001 | 0.004 | 0.005 | 0.018 | 0.021 |
| **Residual Std. Error** | 398.056 | 1.723 | 9.186 | 0.041 | 0.077 | 0.0003 |
| **F Statistic** | 129.217*** | 16.582*** | 1,352.031*** | 104.580*** | 1,718.297*** | 135.567*** |
| ***Note:*** | *$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$* | | | | | |

*Source: FCA analysis of PCA data*

231. As explained in our assumptions section, we would expect that, due to unconditional parallel trends, including customer-level controls would not change estimates. Our results are in line with this expectation - across all outcome variables, including dummies for geography and the age of consumers does not change the coefficient estimates significantly.

232. The group that saw their fixed fees removed reduced borrowing by £21 a month more relative to the always treated group. They also saw monthly charges reduce by £1.53 and effective price reduce by £0.033 per £1 borrowed relative to the group that never paid fixed fees. These results are similar in magnitude to our estimates based on Equation (1) shown in Table 6 which means that the ban of fixed fees is driving a significant proportion of the total benefits (subject to Assumption 4).

233. Our sample is representative of PCA customers with an overdraft facility. Hence, we assume that in the population of PCA holders, the proportion which paid fixed fees pre-intervention is the same as in our sample – 81.02%. We therefore extrapolate the average savings to 81.02% of the PCA holders with an overdraft facility in the UK.

234. Annual savings in charges for a customer in the switched group are £18.36 (£1.53 × 12). We multiply this number times the adult population who hold a PCA account, and has an overdraft facility (54.18%), times the proportion of the switched group in the population (81.02%). We get an annual reduction in charges of £427 million for the total population of affected consumers.

235. Under the assumption of no spill-over effects (Assumption 4), these are the average benefits solely due to the removal of fixed fees. As discussed in the assumptions section, it is likely that this assumption is violated. We therefore interpret these findings as a robustness check. The closeness in magnitude of total savings

estimated using Equation (1) and Equation (2) increases our confidence that our main set of results are not biased due to the violation of the parallel trends assumption. They also indicate that a large part of the total benefits we estimate are potentially driven by the ban on fixed fees.

## Results by IMD decile

236. We now examine how our results vary by IMD decile. To do this, we estimate Equation (3) interacting the changes in pricing elements with the IMD decile.

237. Table 9 reports the central average treatment effect estimates for borrowing, charges and effective price for each decile.

**Table 9: Average treatment effects of pricing remedies by IMD decile**

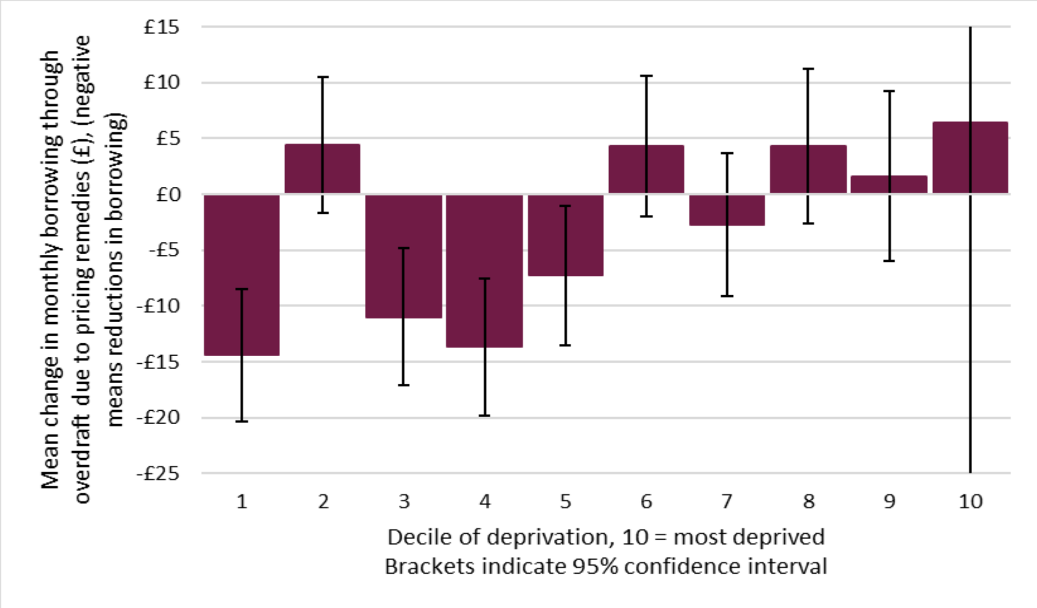
| IMD decile – 10=most deprived | Borrowing | Charges | Effective price |
|---|---|---|---|
| **1** | -£26.70 | -£1.39 | -£0.02 |
| **2** | £8.20 | -£1.36 | -£0.03 |
| **3** | -£20.30 | -£1.59 | -£0.03 |
| **4** | -£25.30 | -£1.68 | -£0.03 |
| **5** | -£13.40 | -£1.72 | -£0.03 |
| **6** | £7.90 | -£1.49 | -£0.03 |
| **7** | -£5.10 | -£1.75 | -£0.03 |
| **8** | £8.00 | -£1.45 | -£0.03 |
| **9** | £3.00 | -£1.58 | -£0.03 |
| **10** | £11.90 | -£1.57 | -£0.03 |

*Source: FCA analysis of PCA data*

238. The effect on borrowing varies across IMD deciles. Although the pattern is not clear, less deprived consumers seem to be likely to reduce borrowing in response to pricing remedies. Conversely, more deprived deciles increase borrowing – average monthly balances for the most deprived decile increasing by £11.90.

239. However, our central estimates with respect to borrowing are uncertain. Figure 25 shows that confidence intervals tend to be large, especially for more deprived deciles. This means that we cannot conclude that the policy has resulted in an increase in average borrowing balances for the most deprived consumers. However, it is important to note that less deprived households seem to have reduced borrowing in response to the policy and for deciles 1, 3, 4 and 5, the 95% confidence interval lies entirely below zero.

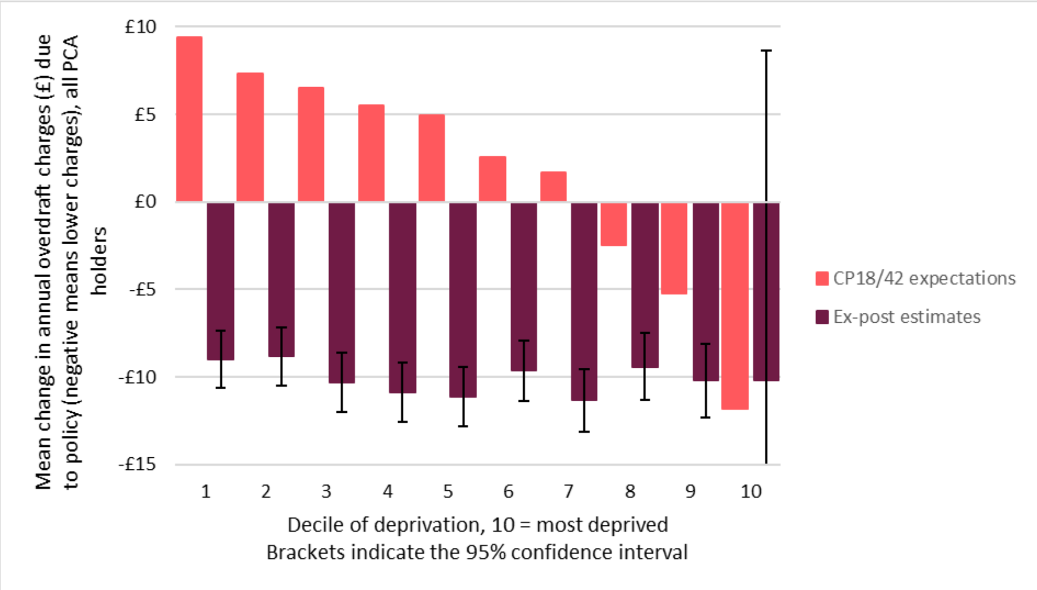
**Figure 25: Distribution of changes in monthly borrowing per person by due to pricing remedies by IMD decile, consumers with an overdraft facility**




*Source: FCA analysis of PCA data*

240. Reductions in charges are uniform across IMD deciles. Results reported in Table 9 show that for the population of consumers with an overdraft facility, the average reduction in monthly charges is between £1.36 and £1.75. We can scale these figures to the population of PCA holders and convert to annual reductions to make direct comparisons with predictions in CP18/42.

**Figure 26: Distribution of changes in annual charges due to pricing remedies by IMD decile, all PCA holders, CP18/42 expectations vs ex-post estimates**

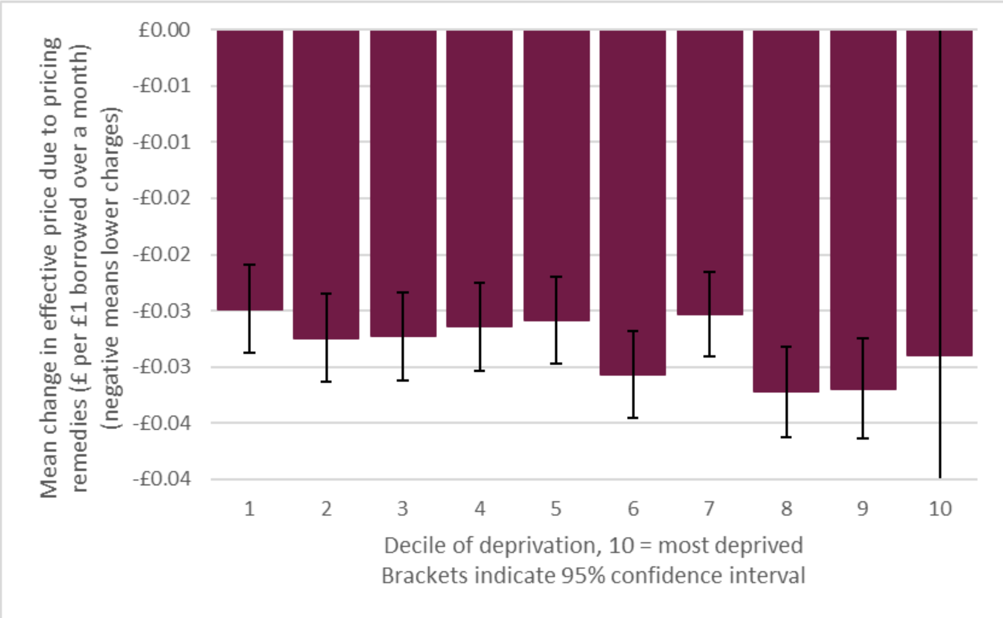



*FCA analysis of PCA data*

241. Figure 26 shows that reductions in charges tend to exceed our central scenario predictions reported in CP18/42. Savings are particularly high for the less deprived deciles – benefits for the 7 least deprived deciles tend to be closer to the optimistic scenario in CP18/42. Savings for the 3 most deprived deciles also exceed our central scenario with the exception of the most deprived decile where our prediction was £11.80 reduction in annual charges. Our ex-post estimate of £10.20 reduction is slightly lower than this figure, but still in line with our central modelling scenario.

242. The larger reductions in charges for the least deprived consumers (compared to predictions) are most likely due to the reduction in overdraft balances we observe for less deprived deciles (see Figure 26)*.* Figure 27 shows that the effective price has fallen uniformly for all deprivation deciles, with a slightly larger reduction for the 5 most deprived deciles. This indicates that the direct effects of pricing remedies have not fallen disproportionately on less deprived households.

**Figure 27: Distribution of changes in effective price due to pricing remedies by IMD decile, consumers who incurred overdraft charges**




*FCA analysis of PCA data*

## Unintended consequences

243. In CP18/42 we stated that, although unlikely, firms might reduce arranged overdraft limits for riskier consumers. If they lose access to overdrafts, consumers may switch to borrowing through more expensive forms of credit.

244. To check whether any of these adverse outcomes have materialised, we re-run Equation (1) by swapping out the outcome variable with a set of variables that would indicate unintended consequences. We first check for treatment effects on a set of 9 credit products, among which are High-Cost Short Term Credit (HCSTC), Rent to Own and Retail Finance (which includes regulated Buy Now Pay Later (BNPL) agreements). We then check for treatment effects on overdraft limits.

245. We note that our research design is centred around identifying effects in the overdraft market. Our data on credit products comes from a different data source and covers a different population to that of overdraft customers. Furthermore, the frequency with which the data is reported is different to our PCA dataset, hence measurement error may be present. Due to these limitations, we treat estimates in this section as indicative and do not make statements about the treatment effect of overdraft pricing remedies on outcomes in other markets. To make such statements, we would need to estimate a structural model of cross-price elasticities across all products, which is beyond the scope of this publication.

246. Due to the reducing sample sizes when we analyse other credit products, we cannot perform the analysis by IMD decile. However, high-cost credit products where increasing balances would be a concern tend to be held by more deprived consumers. For instance, our data show that 75% of Rent to Own holders come from the three most deprived IMD deciles.

247. We check for significant increases in balances of other forms of high-cost credit to ensure our remedies have not caused consumers to incur high charges in other markets.

**Table 10: Regression output from estimating Equation (1) with credit product balances as the outcome variable**

| | Outcome variable | | | | |
|---|---|---|---|---|---|
| | Credit cards | Charge cards | RTO | Mail order | Personal loans |
| | (1) | (2) | (3) | (4) | (5) |
| **AOD interest rate** | 698.266*** | -4,506.517* | -171.695 | 305.755*** | 937.090 |
| | (191.702) | (2,499.127) | (2,800.561) | (86.819) | (766.175) |
| **AOD daily fee** | 8.982 | -257.398 | -93.331 | 9.067 | -129.213 |
| | (22.544) | (324.521) | (471.746) | (10.708) | (95.164) |
| **AOD monthly fee** | 116.930*** | 77.766 | -39.527 | 2.995 | -35.698 |
| | (10.997) | (158.490) | (121.670) | (4.453) | (37.284) |
| **UOD interest rate** | 1,330.482*** | -4,498.714 | 3,307.733 | 480.388*** | 443.877 |
| | (208.723) | (2,816.330) | (2,397.318) | (99.039) | (969.704) |
| **UOD daily fee** | 71.316*** | 124.724 | 169.690 | 17.504*** | 265.021*** |
| | (9.785) | (128.647) | (174.203) | (4.600) | (40.339) |
| **UOD monthly fee** | -4.962** | -13.799 | 35.777 | -0.911 | -46.575*** |
| | (2.165) | (33.963) | (40.858) | (1.093) | (8.528) |
| **Constant** | -26,215*** | -22,741 | 46,388 | -11,763*** | -139,281*** |
| | (2,141) | (30,291) | (39,896) | (1,097) | (10,332) |
| **Geography** | YES | YES | YES | YES | YES |
| **Year of birth** | YES | YES | YES | YES | YES |
| **Observations** | 182,602 | 1,887 | 151 | 44,315 | 58,894 |
| **$R^2$** | 0.002 | 0.007 | 0.128 | 0.005 | 0.005 |
| **Adjusted $R^2$** | 0.002 | -0.002 | 0.016 | 0.004 | 0.004 |
| **Residual Std. Error** | 7,149.221 | 9,796.844 | 2,860.514 | 1,652.637 | 16,722.530 |
| **F Statistic** | 22.973*** | 0.798 | 1.147 | 11.991*** | 16.575*** |
| ***Note:*** | **p<0.1; **p<0.05; ***p<0.01* | | | | |

*Source: FCA analysis of PCA data*

**Table 11: Regression output from estimating equation 1 with credit product balances as the outcome variable, continued**

| | Outcome variable | | | | |
|---|---|---|---|---|---|
| | Store cards (6) | Home credit (7) | Consumer hire (8) | HCSTC (9) | All credit (10) |
| **AOD interest rate** | 53.646 | 2.309 | 2,834.519 | -251.918 | 694.459** |
| | (79.569) | (1.603) | (6,525.906) | (184.466) | (285.654) |
| **AOD daily fee** | 11.343 | 0.122 | -180.455 | -57.762** | -51.725 |
| | (10.310) | (0.188) | (750.693) | (27.492) | (33.764) |
| **AOD monthly fee** | -3.205 | 0.202** | 277.990 | 23.746** | 95.809*** |
| | (4.304) | (0.092) | (348.427) | (9.275) | (16.023) |
| **UOD interest rate** | -29.269 | -0.424 | 10,165.110 | 203.611 | 1,706.209*** |
| | (91.507) | (1.755) | (8,567.122) | (190.761) | (308.384) |
| **UOD daily fee** | -4.267 | -0.131 | 229.992 | 24.022** | 158.295*** |
| | (4.417) | (0.082) | (355.382) | (11.593) | (14.502) |
| **UOD monthly fee** | 1.488 | -22,741 | 46,388 | -11,763*** | -139,281*** |
| | (1.055) | (30,291) | (39,896) | (1,097) | (10,332) |
| **Constant** | -12,160*** | 23.960 | -73,844 | -11,097*** | -52,605*** |
| | (1,053) | (18) | (102,926) | (3,465) | (3,172) |
| **Geography** | YES | YES | YES | YES | YES |
| **Year of birth** | YES | YES | YES | YES | YES |
| **Observations** | 24,948 | 1,887 | 151 | 44,315 | 58,894 |
| **$R^2$** | 0.006 | 0.007 | 0.128 | 0.005 | 0.005 |
| **Adjusted $R^2$** | 0.006 | -0.002 | 0.016 | 0.004 | 0.004 |
| **Residual Std. Error** | 1,185.895 | 9,796.844 | 2,860.514 | 1,652.637 | 16,722.530 |
| **F Statistic** | 9.372*** | 0.798 | 1.147 | 11.991*** | 16.575*** |
| ***Note:*** | **p<0.1; **p<0.05; ***p<0.01* | | | | |

*Source: FCA analysis of PCA data*

248. Tables 10 and 11 above present the results from estimating Equation (1) with a set of credit balances as the outcome variable. We also restrict the sample to those consumers who held the respective credit product in either the pre- or post-intervention period.

249. Our results show that individual coefficient estimates tend to be statistically insignificant in most models (with the notable exception of credit cards). However, the overall treatment effect depends on the joint movements in the 6 pricing components. We therefore perform an F-test for joint significance of the coefficients in $GAP$. Below we report the estimated ATE and whether the coefficient estimates on $GAP$ are jointly significant.

**Table 12: Average treatment effect on credit product balances due to pricing remedies**

| Product | Average balance in post-intervention period | ATE estimate on product holders | Coefficients jointly significant at the 95% level? |
|---|---|---|---|
| Credit cards | £3,822 | -£17.59 | Yes |
| Charge cards | £2,712 | -£1226.02 | No |
| Rent to own | £1,863 | -£137.32 | No |
| Mail order | £978 | £36.78 | Yes |
| Personal loans | £10,368 | -£21.04 | Yes |
| Store cards | £639 | £13.47 | No |
| Home collected credit | £209 | £0.24 | Yes |
| Consumer hire | £2,725 | £415.46 | No |
| HCSTC | £373 | -£89.04 | Yes |
| All credit products | £6,571 | -£87.66 | Yes |

*Source: FCA analysis of PCA data*

250. We find statistically significant effects with respect to Credit Cards, Mail Order, Personal Loans, Home Collected Credit, HCSTC and the sum of all credit products.

251. The only economically significant increase in credit balances is that on mail order products where we find that the average balance in the post-intervention period is £978 per month, with a treatment effect of £37, implying the counterfactual value of £941. Mail order is not an insignificant market as approximately 40,000 customers out of the 314,089 individuals with an overdraft facility in our sample held this product in the sampling period.

252. However, as a percentage of the post-intervention balances, the treatment effect is 3.8%. In contrast, the treatment effect on overdraft balances is 9.41% reduction in borrowing. Finally, we note that we estimate significant reductions in credit card balances and HCSTC balances. When we sum the balances on all credit products, we estimate a decrease in the average balance of £88 compared to the average post-intervention balance of £6,571. We interpret this as a lack of evidence that a significant increase in borrowing on more expensive products has taken place as a result of the policy.

253. Finally, we run Equation (1) by swapping the outcome variable with the overdraft limit. We find a positive association between changes in prices and overdraft limits. Multiplying the coefficients in Table 13 by the average values of the price changes in the sample (see Table 3) we find that the estimated treatment effect on overdraft limits is an increase of £128.80. If our policies had caused firms to reduce access to credit, we would expect negative treatment effects on limits, but we see the opposite.

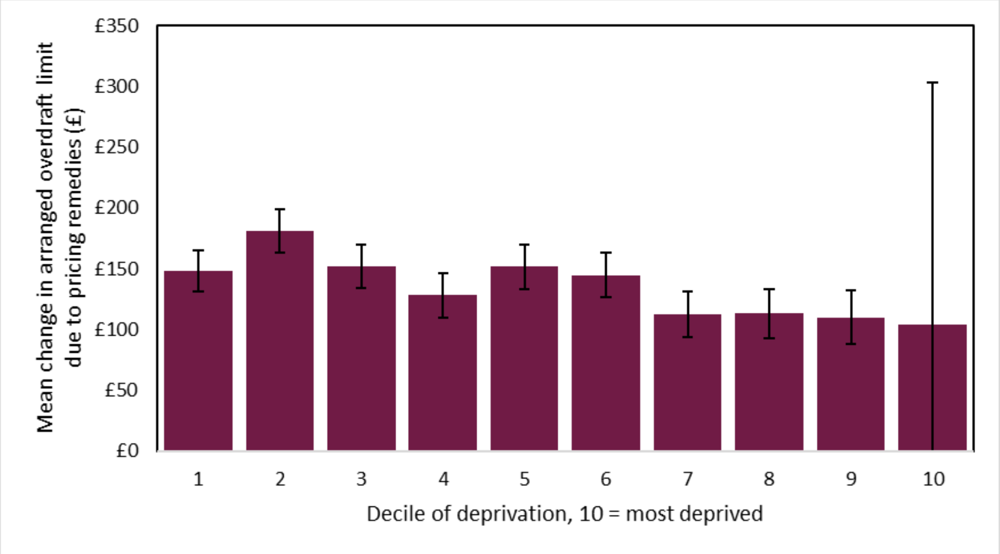
**Table 13: Regression output from estimating equation 1 with arranged overdraft limits as the outcome variable**

| | Outcome variable |
|---|---|
| | Arranged overdraft limit |
| AOD interest rate | 660.011*** |
| | (13.125) |
| AOD daily fee | 40.151*** |
| | (1.554) |
| AOD monthly fee | 57.173*** |
| | (0.776) |
| UOD interest rate | 48.704*** |
| | (13.000) |
| UOD daily fee | -18.071*** |
| | (0.650) |
| UOD monthly fee | 2.134*** |
| | (0.157) |
| Geography | YES |
| Year of birth | YES |
| Constant | -4,613.394*** |
| | (132.577) |
| Observations | 310,602 |
| $R^2$ | 0.041 |
| Adjusted $R^2$ | 0.041 |
| Residual Std. Error | 626.787 |
| F Statistic | 789.700*** |
| *Note:* | *p<0.1; **p<0.05; ***p<0.01 |

*Source: FCA analysis of PCA data*

254. Since we observe the overdraft limit for all consumers in our subsample (314,089), we can estimate Equation (3) with arranged limits as the outcome variable. We report the change in overdraft limits by IMD decile in Figure 28 below. We see that limits have increased by more for the less deprived consumers with IMD decile 1 seeing an increase of £148 on average, and IMD decile 10 seeing an increase of £104 as a result of the policy.

**Figure 28: Mean change in arranged overdraft limits due to pricing remedies by IMD decile, all consumers with an overdraft facility**




*Source: FCA analysis of PCA data*

## Conclusions

255. Our results suggest that pricing remedies have resulted in lower average charges and lower effective price for consumers. The lower charges for the least deprived IMD deciles appear to be partly driven by a reduction in the average borrowing balance. There is no evidence of a reduction in average overdraft balances by the more deprived consumers, suggesting that savings are driven by the direct effects of the policy.

256. We find that the reductions in the effective price of overdrafts are more uniformly distributed across IMD deciles, suggesting that the direct effects of the policy do not disproportionately benefit the less deprived consumers.

257. We find that consumers, particularly more deprived ones, do not appear to have experienced a reduction in access to overdraft credit as indicated by the increase in limits due to the policy. We also find no strong evidence of increases in borrowing through more expensive forms of credit.

# 5 Analysis of the causal impact of repeat use remedies

## Introduction

258. In CP18/13 and CP18/42 we found repeat overdraft use leads to a high total cost of credit, which may exceed the cost of alternative forms of credit. The evidence outlined in CP18/42 suggests that causes of this include consumers not knowing when or how to switch to alternative forms of credit and potential behavioural biases. The evidence also shows that repeat use is associated with a deteriorating financial position, in particular:

- an increased likelihood of using unarranged overdrafts the longer a consumer uses an arranged overdraft
- an increase in the median number of days a month for which the consumer is overdrawn
- a declining current account balance
- an increasing credit card balance

259. Consumer research indicates that, for some types of consumers, going into debt can cause stress and anxiety. These consumers are more reluctant to engage with financial information. To help mitigate these harms, we asked firms to:

- Develop a strategy for reducing repeat use. We defined "repeat use" in the rules as "a pattern of overdraft use where the frequency and depth of use may result in high cumulative charges that are harmful to the customer or indicate that the customer is experiencing or is at risk of financial difficulties".
- Incorporate, within their strategy, policies, procedures, and systems to monitor customers' overdraft use, identify repeat users, and sub-divide the latter into two categories based on indicators of actual or potential financial difficulties:
  - those for whom there are signs of actual or potential financial difficulties
  - all other repeat users
- Incorporate interventions within their strategy for customers belonging in 1 of 2 categories:
  - Customers in category A are those that the firm identifies as repeat users for whom there are signs of actual or potential financial difficulties. For such customers, the firm must seek dialogue with the customer and present options for reducing use (the guidance to the rules gives examples of options), explaining that if the issue continues, suspension or removal of the overdraft may occur (unless that would worsen the customer's financial position).
  - Customers in category B are all other customers that the firm identifies as repeat users. The firm must communicate with such customers, highlighting their pattern of use and indicating that this may be resulting in high avoidable costs. The firm must continue to monitor the customer, and if the pattern of use continues, the firm must send a similar communication after a reasonable period, and then at least annually.

260. While we provided a broad definition for repeat use and guidance for development of the strategies, we gave firms licence to interpret that definition and develop the details of their strategies how they saw best. As a result, each firm has its own definitions for repeat use and what it considers to be signs of actual or potential difficulty, based on different variables, and different thresholds. Each firm's strategies also involve different communication channels and periodicity. Some firms have chosen to divide repeat users into short- and long-term repeat users with a different strategy for each; some have chosen to do the same for repeat users in financial difficulty. For this reason, we evaluate the impact of each individual strategy separately.

261. In this chapter we discuss our approach to causally evaluating the impact of firms' treatment of repeat use customers and repeat use customers in financial difficulty. We focus on two outcome variables: overdraft charges and overdrawn balance.

262. For brevity, in the rest of this chapter we use the following short-hands:

- `RU customers' and `FD customers' refer to the customers that each firm identifies, respectively, as repeat users and repeat users in (actual or potential) financial difficulty
- `RU strategy' and `FD strategy' refers to the strategies that firms use to treat these customers
- where firms distinguish between long-term and short-term users/strategies, we refer to these as `LT' and `ST' respectively

263. In the first section we provide an overview of the data provided to us by participating firms, and some statistics describing the number of accounts captured by each strategy and the typical features of the owners of those accounts. In the second section we describe the methodology for our causal analysis. In the third section we describe the results of this evaluation, and our interpretation of them.

## Data

264. We asked the firms to provide us with account-level data aggregated at the monthly level on accounts that met, or came close to meeting, the firms' RU and FD customer definitions over a given period. We refer to this as the "repeat use dataset". For each firm, this period (which we hereby refer to as the "sampling period") begins when the firm started assessing accounts against its definitions (which for most firms, is December 2019) to September 2021. We asked all firms to provide the following information for each account over that period:

- The month and year to which the data observation refers
- A unique account identifier
- The account's total overdraft charges for each month (our first outcome variable)
- The account's average overdrawn balance over each month (our second outcome variable)
- An indicator of whether the account holder was classed as a repeat user in each month
- An indicator of whether the account holder was classed as being in potential or actual financial difficulty in each month

- An indicator of whether a repeat use communication had been sent to the account holder
- An indicator of whether a financial difficulty communication had been sent to the customer
- All relevant metrics used to determine whether the account holder is in repeat use or showing signs of potential/actual financial difficulty

265. We also asked each firm for their RU and FD definitions and their strategies for each definition.

266. We note that the firms in our sample have different methods for calculating their customers' average overdraft balance. This means that cross-firm comparisons may not be reliable.

- Firm 1 has calculated the average end-of-day chargeable debit balance on an account across a month. This is the overdraft balance, minus any interest free buffer. If the overdrawn amount is less than the buffer for a given day then the overdraft balance for that day is coded as £0 when the firm is calculating the account's monthly average overdraft balance.
- Firms 2, 3 and 6 have calculated an average end-of-day balance on an account across a month, recording credit values as negative and debit values as positive.
- Firms 4 and 5 have calculated an average of end-of-day balances on an account across a month recording daily debit values as positive and daily credit values as £0.

## Descriptive statistics

### Number of accounts in the firms' RU and FD strategies

267. Over the sampling period, 3.7m accounts were treated through the firms' RU strategies and 2.4m through their FD strategies. Table 14 below, shows the number of accounts treated at each firm. The FD strategy is separate from the RU strategy, however there is an overlap of accounts that are treated on both at different points in time, as they tend to have similar selection criteria (with the financial difficulty strategy criteria being more stringent to ensure only those account holders in financial difficulty are selected).

***Table 14: Number of accounts in Repeat Use and Financial Difficulty Strategies by firm***

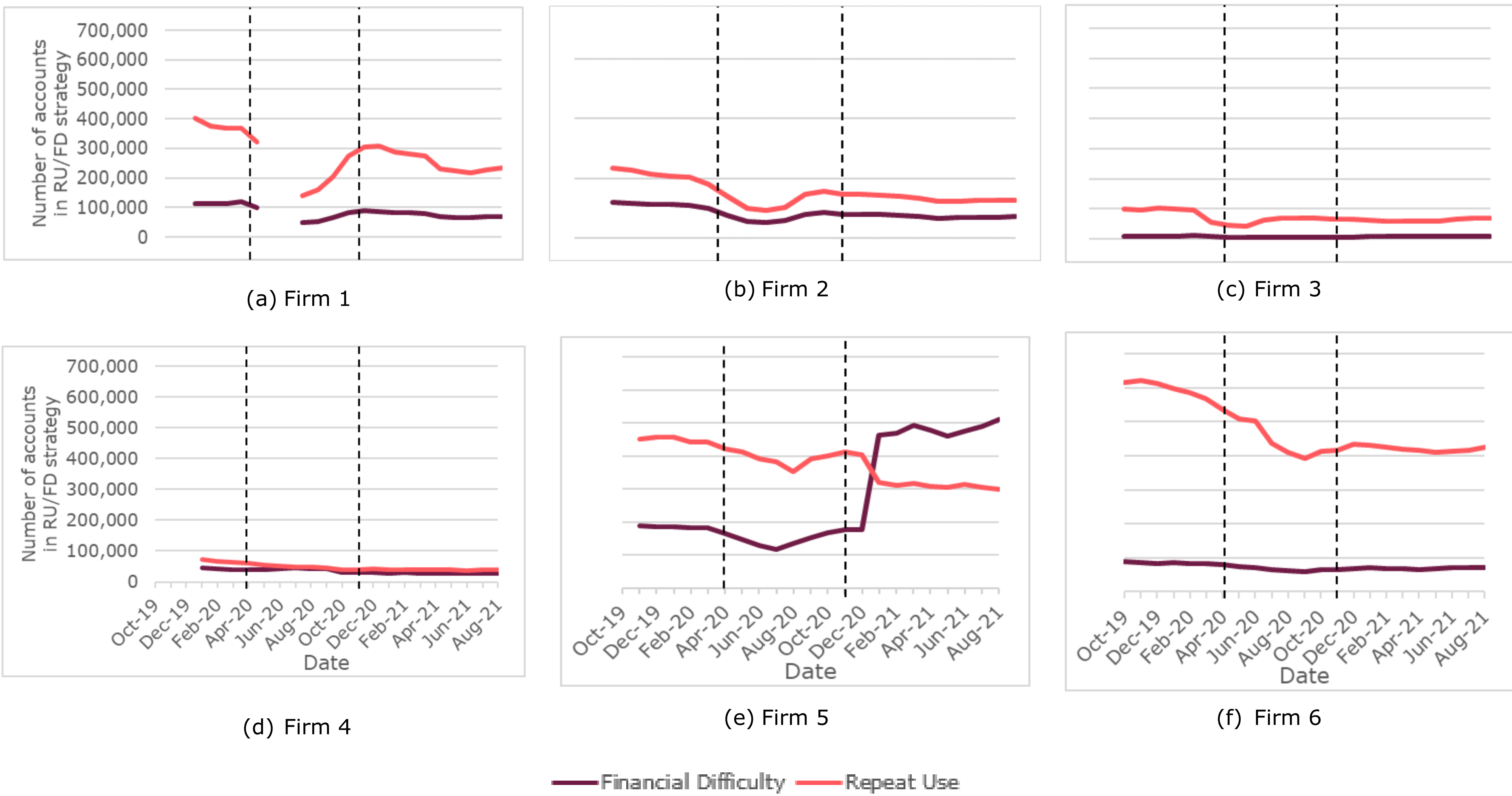
| Firm Name | Number in Repeat Use strategy | Number in Financial Difficulty Strategy |
|---|---|---|
| **Firm 1** | 594,812 | 272,635 |
| **Firm 2** | 429,226 | 299,084 |
| **Firm 3** | 145,489 | 38,580 |
| **Firm 4** | 143,003 | LT: 100,013<br>ST: 35,092 |
| **Firm 5** | LT: 1,221,068<br>ST: 209,237 | LT: 705,398<br>ST: 751,064 |
| **Firm 6** | LT : 729,108<br>ST: 167,515 | 200,878 |

*Source: FCA analysis of repeat use dataset*

268. Figure 29 shows how the number of customers captured in each firm's strategies varies by month. In this figure, an account is in repeat use if it meets the firm's threshold and exits repeat use when it no longer meets it. This gives us an idea of the number of accounts receiving help in any given month. When we evaluate the causal impact of RU and FD strategies, however, we consider an account as 'always treated' once they receive communications under the relevant strategy. We take this approach because we are interested in customer outcomes even after they stop receiving help on the strategy.

269. At the end of March 2020, we issued temporary guidance to PCA providers in response to Covid-19. This guidance coincided with the introduction of the pricing remedies that were part of the package of overdraft interventions. The guidance asked firms to ensure their customers were not made worse off by the overdraft interventions. The start of this guidance is indicated by the first vertical line, the end of it by the second.

**Figure 29: Number of accounts in each firm's RU and FD strategies, by month**




*Source: FCA analysis of PCA data*

270. Firm 1 paused its assessments against their RU and FD strategies between March 2020 and May 2020. During this period the firm implemented alternative overdraft support in line with the Temporary Covid guidance. Firm 5 adjusted the threshold for its short-term FD strategy in December 2020, hence the increase in accounts in the strategy at this point (see panel (e)). The firm lowered the threshold, so it captured all accounts that would have been on its short-term RU strategy and then removed its short-term RU strategy.

271. This difference in number of accounts treated across the different firms reflects the difference in selection criteria that each firm applies, and to some extent the differing size in the firm's customer base. However, we find firms with the larger customer base do not necessarily treat the most accounts and vice versa. This makes comparisons of our benefit estimates across firms difficult, particularly for our first estimation method which calculates a treatment effect for accounts close to the threshold.

272. All the firms, except firms 3 and 4, see a significant decline in the number of individuals in each strategy after March 2020, as they implemented Covid-19 support measures which reduced the number of people meeting the criteria. At firms 3 and 4 the decline was smaller as the criteria for selection onto the strategy was already high, so these customers still qualified despite the support. Even after the support measures are removed in October 2020, there is a general downward decline in the number of customers meeting the criteria for each strategy.

## Econometric methodology

273. We analyse each firm separately. This is because our rules are not prescriptive on which customers firms should treat, nor on the exact features of treatment. Therefore, each firm's approach to identify and treat customers is unique.

274. We analyse the impact of the strategies of firms 1, 3 and 6 using a fuzzy Regression Discontinuity Design (RDD). This approach allows us to isolate the causal impact of the firms' strategies by comparing outcomes for customers that barely qualify for treatment against outcomes for customers that barely do not qualify.

275. RDD is either not feasible or not appropriate for firms 2, 4 and 5. For these firms, we estimate the causal impact of the RU and FD strategies by comparing outcomes for customers treated by the firm in question against outcomes for customers of other firms, who have similar features (e.g. similar overdrawn balances over the previous year) but are not treated because their firms use different treatment criteria, or who are from the same firm and have similar features, but are not treated because they have failed to meet one of the criteria for treatment.

### Fuzzy Regression Discontinuity Design

276. Our fuzzy RDD approach is analogous to an instrumental variable approach and works in two stages. We follow the methodology used in Angrist and Lavy (1997) and Lee and Lemieux (2009):

- in the first stage, we estimate the probability that an individual is treated, given their characteristics

- in the second stage, we estimate the impact of a higher treatment probability on our main outcome variables, overdraft charges and borrowing

*Fuzziness in identifying treatment*

277. Each firm determines whether a customer should be treated on a strategy based on a combination of criteria, some of which relate to categorical variables (e.g. whether the customer has missed a payment on a credit product) while others relate to continuous variables (e.g. whether the overdraft charges paid by the customer in the last month exceed £30). Depending on the broader account characteristics, meeting one of these criteria may be enough to qualify for treatment; in others, a combination of them may need to be met. This creates 'fuzziness' around the threshold of each rule, as, for any given criterion, we may have accounts that do not satisfy that criterion but qualify, and accounts that satisfy the given criterion but fail to qualify. However, in such cases we would expect that the probability of treatment is higher if the account meets the criterion.

278. To account for this, our fuzzy RDD estimates the effect of an increase *in the probability of treatment* on our outcome variables, rather than the effect of the treatment itself.

279. For this approach we need one of the variables relating to the treatment criteria to act as our `running variable' – the central variable that probabilistically determines treatment. For each firm, we select the continuous variable that best predicts treatment at the criterion threshold as our running variable. Following the above example of overdraft charges exceeding £30, we would consider overdraft charges to predict treatment well (hence to be a good running variable) if the firm treats the vast majority of individuals who pay £31 in a given month, but only a handful of individuals who pay £29. This jump in the proportion of individuals treated is referred to as a `discontinuity in the probability of treatment'.

*First stage: Estimating the conditional probability of treatment*

280. Having chosen our running variable, we filter our sample to observations that have a value of the running variable that is close to the threshold. We then estimate the probability than an individual is treated, conditional on the running variable through the following equation:

$$E[D_{i1}|x_{i1}] = \gamma_0 + \gamma_1 x_{i1} + \gamma_2 x_{i1}^2 + \cdots + \gamma_p x_{i1}^p + \left[\gamma_0^* + \gamma_1^* x_{i1} + \gamma_2^* x_{i1}^2 + \cdots + \gamma_p^* x_{i1}^p\right]\widehat{d_{i1}} + b_m \quad (4.1)$$

281. Where:

- The subscript 1 indicates that all observations relate to the 1st period of treatment for our treatment group and the 1st period of 'near-treatment' for our control group.
- $E[D_{i1}|x_{i1}]$ is the probability of individual $i$ being treated given the value of the running variable $x$ at the period when assessment is done.
- $\widehat{d_{i1}}$ is the treatment dummy, equal to 1 if the individual is past the threshold on the running variable, and 0 if they are not.

- $x$ is the running variable, centred around the threshold (we subtract the value of the threshold from the running variable, so the value is negative if it is below the threshold and positive if it is above). We expect that the probability is not linear in the running variable, so we include polynomials of $x$ up to and including a value $p$ (we discuss the values of $p$ we use further below). We also expect the relationship between the probability of treatment and the running variable to change when the account exceeds the threshold, so we interact each order of the polynomial with the treatment dummy through the clause: $[\gamma_0^* + \gamma_1^* x_i + \gamma_2^* x_i^2 + \cdots + \gamma_p^* x_i^p]\hat{d}_i$.
- $\gamma_p$ (where $p > 0$) is the coefficient of the effect of the running variable (to the power of $p$) on the probability of treatment when the running variable is below the threshold. When the running variable is above the threshold, its effect on the probability of treatment is given by $\gamma + \gamma^*$.
- $b_m$ is vector of time-fixed effects relating to the month and year in which the observation is recorded.

282. Through this regression we obtain the fitted values of $D_{i,1}$, $\widehat{D_{i,1}}$, which represent our estimated probability of treatment for each customer $i$. We use these fitted values for our second-stage regression.

283. As described in the variable descriptions, we expect the relationship between the running variable and the probability of treatment to be different depending on whether the value of the running variable lies below or above the threshold. Consequently, we expect the relationship between the running variable and the outcome will be different for individuals who are treated compared to individuals who are not treated. To account for this when estimating the treatment effect, we need to include in our second-stage regression an interaction between the treatment status and the running variable. However, we cannot do this directly due to the fuzziness in the firm’s treatment decision.

284. Instead, we first estimate the expected value of the interaction of the treatment status and the running variable through a separate regression, and then we use the fitted values from this regression in our second-stage equation. Specifically, we estimate the following equation $p$ times, each time increasing the polynomial’s order by 1 (starting from order 1), saving the fitted values each time.

$$E[D_{i1}x_{i1}^p | x_{i1}] = \gamma_0 + \gamma_1 x_{i1} + \gamma_2 x_{i1}^2 + \cdots + \gamma_p x_{i1}^p + [\gamma_0^* + \gamma_1^* x_{i1} + \gamma_2^* x_{i1}^2 + \cdots + \gamma_p^* x_{i1}^p]\widehat{d_{i1}} + b_m \tag{4.2}$$

We include the fitted values from this, $\widehat{D_{i,1}x_{i,1}}, \ldots, \widehat{D_{i,1}x_{i,1}^p}$, in our second-stage equation.

285. In most cases we estimate equations 4.1 and 4.2 up to $p = 2$. This balances the risk of spuriously overfitting the equation and the risk of having large differences between the estimated probability of treatment and the actual portion of individuals who were treated for a given value of the running variable (i.e. it minimises the residual error). In some cases, the residual error was still large at this order, so we estimated the equation to $p = 3$.

286. We consider an individual always treated once they first become treated. If they have entered treatment in the past, then exited the strategy due to improvement in their outcomes, we still consider them to be part of the treatment group.

287. A person may come near the treatment threshold more than once in different periods. For the purpose of constructing our control group, if the same non-treated person comes close to the threshold multiple times, each time they do so they are considered as a distinct individual in the group.

*Second stage: Estimating the treatment effect*

288. In the second stage we use our estimated probability of treatment for each customer $i$ that we obtained from the first stage ($\widehat{D_i}$), and the expected value of the interaction between the treatment status and the running variable ($\widehat{D_{i,1}x_{i,1}^p}$) to estimate average treatment effects on our outcome variables. This two-stage process is an example of an 'instrumental variable' approach. We are using 'instruments' (the threshold dummy, and the interaction between the running variable and the threshold dummy) that are correlated with our endogenous variables (the treatment status, and the interaction between the treatment status and the running variable), to estimate the effect of the endogenous variable on our exogenous variables (borrowing and charges).

289. We do this by estimating the following pooled OLS regression:

$$
\begin{aligned}
y_{i,t} - \rho_{t-T_i+1} D_{i,t}\left(-\left(D_{i,1}-1\right)\right) \\
= \kappa_{1,t}x_{i,1} + \kappa_{2,t}x_{i,1}^2 \ldots + \kappa_{p,t}x_{i,1}^p + \rho_t\widehat{D_{i,1}} + \kappa_{1,t}^*\widehat{D_{i,1}x_{i,1}} + \kappa_{2,t}^*\widehat{D_{i,1}x_{i,1}^2} \\
+ \kappa_{p,t}^*\widehat{D_{i,1}x_{i,1}^p} + b_m + \epsilon_i
\end{aligned}
\tag{5}
$$

290. Where:

- $y_{i,t}$ is the outcome variable in a given month (either average overdrawn balance in the month, or total overdraft charges) for individual $i$, $t$ periods since treatment
- $\widehat{D_{i,1}}$ is the probability of treatment, taken from the first-stage regression for the treatment status (4.1)
- $\widehat{D_{i,1}x_{i,1}^p}$ is the fitted value from the first-stage regression for the interaction between the treatment status and the running variable to the power $p$ (equation 4.2)
- $\rho_t$ is the treatment effect in period $t$
- $x_{i,1}$ is the value of the running variable for individual $i$. We expect treatment to cause the running variable to change over time, so in every period of treatment: .
  - for treated individuals, this is the value of the running variable in first period of treatment
  - for non-treated individuals, this is the value for the period when the non-treated individual came close enough to treatment to be included in the control group. We remind that, as explained earlier, if the same non-treated

person comes close to the threshold multiple times, each time they do so they are considered as a distinct individual in the control group

- $\kappa_{p,t}$ is the effect in period $t$ of the running variable (to the power of $p$)
- $b_m$ is the time fixed effect in $m$, where $m$ is the month and year in which the outcome is observed.

291. We are presented with a 'contamination' problem when estimating this equation. In period 1, we have a control group and a treatment group, and we can estimate the first period treatment effect $\rho_1$. In period 2, there is a chance that some of our control group become treated. We call the portion that have become treated the 'contaminated control'. To decontaminate this group, we take the first period treatment effect, $\rho_1$, and subtract it from the outcomes of the contaminated group in order to estimate what their outcome would have been had they not been treated. We can then include this decontaminated group in our control and compare them to our treated group to estimate the second period treatment effect $\rho_2$. We are following a similar approach to decontaminate our control sample in all following periods.

292. We decontaminate our sample by including in the second-stage equation the clause

$$\rho_{t-T_i+1} D_{i,t} \left(-\left(D_{i,1} - 1\right)\right)$$

In this clause, $T_i$ is the first period of treatment for individual $i$, and $D_{i,t}$ is their treatment status in period $t$. $D_{i,1}$ is their treatment status in the first period of (near) treatment. For those who started in the control group this will be 0; for those who started in the treatment group this will be 1. If the individual was treated in period $t$ and was also treated in period $1$ (i.e. they are part of the original treatment group), then this clause will equal 0. If, instead, the individual was treated in period $t$, but they were not treated in period $1$ and they were treated in period 2 (i.e. if they are part of the control group, but become treated in a later period) then the clause will yield the treatment effect relating to the number of periods post-intervention that they are treated:

i. $\rho_{t-T_i+1} D_{i,t} \left(-\left(D_{i,1} - 1\right)\right) = \rho_{t-1+1} 1\left(-(1-1)\right) = \rho_{t-1+1}(0)$

ii. $\rho_{t-T_i+1} D_{i,t} \left(-\left(D_{i,1} - 1\right)\right) = \rho_{t-2+1} 1\left(-(0-1)\right) = \rho_{t-1+1}(1)$

293. In our second-stage equation, we estimate the coefficients on the running variable to a polynomial of the same order as in the first-stage equation (Equation 4.1). We would expect the treatment to change the values for the selection variable and the controls, so we fix the value for controls and running variable as equal to their value in the period in which the individual enters treatment/near treatment.

294. To improve our confidence regarding the robustness of our decontamination procedure, we run the second-stage equation (Equation 5) three times in each period. Taking period 2 as an example:

- The central estimate of the treatment effect is obtained by calculating the second-stage equation using the central estimate of the treatment effect in $t = 1$ to decontaminate the control.

- We approximate a lower 95% confidence interval, by using the upper bound of the confidence interval of the treatment effect in $t = 1$ to decontaminate the control. We take the lower bound of the resultant treatment effect in $t = 2$ as an approximation of the lower bound of the 95% confidence interval, accounting for the uncertainty from the estimation in this and previous periods.
- We approximate an upper 95% confidence interval, by using the lower bound of the confidence interval of the treatment effect in $t = 1$ to decontaminate the control. We take the upper bound of the resultant treatment effect in $t = 2$ as an approximation of the upper bound of the 95% confidence interval, accounting for the uncertainty from the estimation in this and previous periods.

295. For accounts in a firm's repeat use strategy, there is a risk that they receive treatment under the firm's financial difficulty strategy. This would cause us to attribute effects of the financial difficulty strategy to the repeat use strategy. Although, we are filtering to accounts that are close to the threshold for treatment, we observe that a small proportion of these accounts do go on to be treated in the financial difficulty strategies in the 12 months for which we observe treatment. Therefore, the effect we are measuring is the effect of the repeat use and financial difficulty strategies as a package used to help accounts that marginally qualify for the repeat use strategies. However, tables 18 and 19 in the results section show that there is only a small impact of the financial difficulty strategy in excess of the impact of the repeat use strategy, so we would attribute most of this impact to the repeat use strategy.

*Assumptions*

1. We assume the distribution of individuals along the running variable is continuous around the cut-off point. There should be no jumps in this distribution at the cut-off point or any other sign of customers manipulating the outcomes determining treatment to increase chances of being included in or excluded from the programme. In practice, consumers are not aware of the rules that firms are using to select into treatment, so they cannot self-select.
2. We assume individuals close to the cut-off point are very similar, on average, in observed and unobserved characteristics and differences further away from the cut-off point can be explained by differences in the running variables.
3. We assume our instruments in the first-stage equation (the threshold dummy, and the interaction terms) are strongly correlated with our endogenous variables in the second-stage equation (treatment status, and the interaction terms).
4. When we run the decontamination procedure, we assume the treatment effect used to decontaminate outcomes is an accurate estimate of the treatment effect on the contaminated group.

296. We tested Assumption 1 through a McCrary density test, which tests for discontinuities in the distribution of units around the threshold. We tested Assumption 3 through an F-test on the first-stage regression, with the null hypothesis that the instruments are weakly correlated with the endogenous variables. For one strategy (Firm 6 RU ST) we find that the instruments are weak, and that estimating the equation through an OLS without instrumenting the treatment status does not bias the result. When we look at the relationship between the treatment status and the running variable, we see there is no discontinuity at the

threshold for this strategy, but instead a linear increase in the probability of being treated as the running variable increases. We therefore estimate an OLS substituting the actual treatment status into equation 4 in place of the probability of treatment.

297. We cannot test Assumption 4; however, we are using a panel of data with observations from a number of months. Whenever an individual comes near enough to the threshold in these months, they are included as a unique observation. Therefore, it is likely that our treatment effect estimate was calculated using some of the observations we are now decontaminating, and they are being decontaminated with an estimate that was based, at least in part, on their outcomes. This supports this assumption.

### Matching approach

298. Our available data for firms 2, 4 and 5 do not allow us to estimate a treatment effect through a regression discontinuity design. Firms 2 and 4 provided the necessary data for RDD on treated accounts only for the periods that they were undergoing treatment – they did not provide any data on treated accounts after they exited treatment. For Firm 5, the variable used by the firm to decide whether or not an account is treated is discrete, with large jumps in outcomes between values – this makes it inappropriate for RDD.

299. For these firms we estimate the causal impact of the RU and FD strategies through a matching approach, by comparing outcomes for customers treated by the firm in question against outcomes for customers of other firms, who have similar features (e.g. similar overdrawn balances over the previous year) but are not treated because their firms use different treatment criteria, or who are from the same firm and have similar features, but are not treated because they have failed to meet one of the criteria for treatment.

300. Our matching approach follows the methodology used by Blundell, Dearden and Sianesi (2004) and Rosenbaum and Rubin (1983) and consists of two stages:

- First, we create a control group of untreated individuals with similar characteristics to our treated individuals.
- Then, we estimate the treatment effect by comparing the outcomes between the two groups.

301. Where the data provided by firms 2, and 4 for the repeat use remedies analysis do not contain all the necessary information on borrowing and charges, we supplemented these with the transactional PCA data these firms supplied (used for the pricing analysis in Chapter 2), as we had asked firms to ensure some of their repeat use strategy accounts were also included in the transaction level PCA dataset. We join the two datasets using a unique ‘account id’ that the firms supplied us with, retaining only the repeat users who are also present in the PCA data. These retained accounts form our treated group.

302. We also joined the repeat use dataset for Firm 5 with the PCA data, however we have all the necessary information on borrowing and charges for this firm, so we are able to keep all the individuals from the Firm 5 repeat use dataset in this case.

303. We use a sample of the untreated accounts in the PCA transaction data to draw candidates for the control groups for Firm 2, 4 and 5's RU and FD strategies.

304. We join the two datasets before implementing the matching procedure. Matching the treated and control groups is a very computationally intensive process. Therefore, to reduce sample size in some cases, we take a random sub-sample of the treated and untreated observations before implementing the matching procedure. Sample sizes for the treated and untreated groups are reported in Table 19.

305. We then match treated and untreated individuals, through the process outlined in section 'First stage: Matching treated and untreated individuals', to form a treated and a control group which have similar characteristics to one another. We explain in the next section how this affects the number of accounts in the treated and control groups.

**Table 15: Number of accounts obtained from PCA data and repeat use dataset to be used for the matching estimation**

| Firm name | Treated accounts | Untreated accounts |
|---|---|---|
| **Firm 2 RU** | 35,000 | 40,000 |
| **Firm 2 FD** | 35,000 | 45,000 |
| **Firm 2 FD** | 35,000 | 45,000 |
| **Firm 2 FD** | 35,000 | 45,000 |
| **Firm 2 FD** | 35,000 | 45,000 |
| **Firm 2 FD** | 35,000 | 45,000 |
| **Firm 4 RU** | 35,000 | 45,000 |
| **Firm 4 LT FD** | 16,519 | 45,000 |
| **Firm 4 ST FD** | 12,381 | 40,000 |
| **Firm 5 LT RU** | 45,000 | 45,000 |
| **Firm 5 ST RU** | 19,993 | 45,000 |
| **Firm 5 LT FD** | 40,000 | 45,000 |
| **Firm 5 ST FD Pre Jan 2021** | 28,963 | 40,000 |
| **Firm 5 ST FD Post Jan 2021** | 40,000 | 40,000 |

*Source: FCA analysis of PCA data and repeat use dataset*

### *First stage: Matching treated and untreated individuals*

306. To construct our control group, we take advantage of the fact that different banks have different treatment criteria (e.g. an individual identified as a repeat user by Firm 2 may not be identified as a repeat user by Firm 1) and that within a firm we may find treated individuals with similar characteristics to untreated individuals, who

have not been treated due to some difference which is not necessarily a strong predictor of future borrowing behaviour. For example, an individual may need to miss a payment on an internal credit product to trigger treatment, if they haven't then they will not qualify for treatment, but this is not as strong a predictor of future behaviour as our matching variables.

307. For each individual treated by Firm 2, 4 or 5 in a given period, we identify an untreated individual with similar characteristics who, in that period, was not treated. These untreated individuals constitute our control group.

308. To ensure we are comparing treated and untreated customers with similar characteristics we employ a propensity score nearest neighbour matching approach. Blundell, Dearden and Sianesi (2004) describe the propensity as a type of balancing score. It balances the probability of being treated conditional on the observed characteristics. This ensures that the treated and the control group are matched closely on the variables which affect the probability of treatment the most. We estimate the probability of treatment conditional on the observed characteristics through the following logistic regression:

$$\mathrm{E}(\mathrm{D}_{\mathrm{i}1}|\mathrm{X}_{11}) = \frac{1}{1 + e^{-(\beta_0 + \beta_1 X_i)}} \tag{6}$$

- $X_i$ is a vector of the six observed characteristics we use to calculate a propensity score for individual $i$:
  - Total overdraft charges in the last month
  - Total overdraft charges in the last three months
  - Total overdraft charges in the last year
  - Average overdraft borrowing in the last month
  - Average overdraft borrowing in the three months
  - Average overdraft borrowing in the last year
- $\beta_1$ is a vector of coefficients, indicating the effect of the characteristic on the probability of treatment.

309. We then match each treated individual to the untreated individual with the closest propensity score to theirs. We do not discard any individuals from the treated group when implementing the matching procedure, so the number in Table 15 represents the number of treated individuals used to estimate the treatment effect through the process described in the second stage.

310. We discard untreated individuals from the control group if there is no one similar to them in our treated group. We match with replacement in the control group. This means we may duplicate an untreated individual and add them to the control more than once if they are the best match for more than one account in the treated group. In this way we ensure we have the same number of treated and control observations, despite removing a number of accounts from the untreated group that are not similar to any of the accounts in the treated group.

### *Second stage: Estimating the effect of treatment*

311. We estimate the treatment effect through a simple linear regression:

$$y_{it} - \rho_{t-T_i+1} D_{i,t}\left(-\left(D_{i,1} - 1\right)\right) = \rho_t D_{i,1} + b_m + \epsilon_{\mathrm{i}} \quad (7)$$

312. Where:

- the subscript 1 indicates that all observations relate to the 1st period of treatment for all treated individuals
- $y_{it}$ is the outcome variable (total monthly charges or average overdrawn balance in the month) for individual $i$ in period $t$
- the clause: $-\rho_{t-T_i+1} D_{i,t}\left(-\left(D_{i,1} - 1\right)\right)$ performs the same decontamination procedure as in the RDD methodology
- $\rho_t$ is the treatment effect $t$ periods after treatment
- $D_{i,1}$ is the treatment status for individual $i$ in the first period after (near) treatment. We use the value of their treatment status in $t = 1$ as a determinant of whether or not they are in our treated or control group in every period, not their treatment status in the given period due to the contamination issues. If we used the value of their treatment status in the given period, rather than period $t = 1$, we would include individuals from the control group in our treated group. Instead we keep them in the control and, if they have become treated, decontaminate their outcomes.
- $b_m$ is a vector of time fixed effects relating to the point in time (i.e. month and year).

*Assumptions*

1. We assume that the chosen matching variables are good predictors of future overdraft borrowing and charges.
2. We assume there is a common support between the control and treated groups. In other words, we assume that there is sufficient crossover in the matching variables between treated and untreated individuals to create a control group which is representative of the treatment group.
3. As with the RDD methodology, we assume the treatment effect used to decontaminate outcomes is an accurate estimate of the treatment effect on the contaminated group.

## Results

313. Below we set out the results from our RDD and matching approaches to estimate the effect of the firms' strategies on overdraft borrowing and charges. The section is set out as follows:

- First stage RDD: results of the estimation of the conditional probability of treatment
- First stage matching: results of the implementation of the matching procedure
- Second stage (both approaches): estimates of the treatment effect.

314. The firms set their own criteria for inclusion on the strategies and as a result treat different types of consumers. Therefore, the results should not be read as comparison of effectiveness of strategies across firms.

### First stage RDD: Estimation of the conditional probability of treatment

315. In this section we outline the results of the estimation of the first stage of our RDD approach. Table 16 below shows the coefficient estimates from Equation 4.1. These coefficients indicate how the probability of treatment on a given strategy is affected by the value of the running variable for that strategy.

- The coefficient of the threshold dummy gives the jump in the probability of treatment as the running variable value crosses the threshold. For example, a coefficient of $x$ means that crossing the relevant threshold of the running variable increases the probability than an individual is treated by $x$ percentage points.
- The coefficients on each degree of the polynomial of the running variable shows how probability of treatment changes as the running variable increases but remains below the threshold.
- The coefficient on the interaction of the threshold dummy and the running variable shows how the probability of treatment changes as the running variable increases, for values of the running variable that exceed the threshold.

The table also shows a number of diagnostic tests:

- First, there are F-tests for the validity of the instruments for each of the endogenous variables (shown in the first column). The F-test for the repeat use status instruments is conducted on equation 4.1. For the interactions, these are tested on the related form of equation 4.2. These test the hypothesis that the instrument is weakly correlated with the endogenous variable, if we can reject the null hypothesis (p-value is less than 0.05) then we do not have weak instruments, and our regression is more likely to be valid.
- Second, we have the Wu-Hausman test statistic. This is a test that compares the results of an ordinary least squares (OLS), to the two-stage instrumental variable approach we are using here (Equation 5). Under the null hypotheses both estimators are consistent, and OLS is the preferred option as it is more efficient (i.e. has a smaller variance). Rejecting the null (with a p-value <0.05) indicates that OSL is inconsistent, i.e. treatment is endogenous, and a fuzzy RDD approach is more favourable.
- Thirdly we have the McCrary density test statistic. This is a test on the sample of observations for any discontinuities around the threshold in the number of units with a given value of the running variable. The test checks if there are large jumps in the number of units with a value of the running variable that just exceeds or just falls short of the threshold. A discontinuity would be a sign that the firm or the consumer are manipulating their variables to qualify, or not qualify for treatment. This kind of selection would bias our estimate as we would have an unobserved characteristic affecting treatment (e.g. the individual or the firm's desire for that individual to be treated). The null hypothesis is that there is no discontinuity. A p-value less than 0.05 means we reject the null and conclude there is likely to be a discontinuity. In these cases, we consider why there might be a discontinuity, and how that may affect our estimate of the treatment effect

316. Table 16 shows the results from estimating Equation (4.1) for each firm's RU and FD strategy. With the exception of Firm 6's ST RU strategy, the instrument tests in the table suggest our instruments are strong, as they are strongly correlated with our endogenous variables.

317. The large p-values of the instrument test for Firm 6 ST RU suggests we may have weak instruments. We find a larger statistic for the Wu-Hausman test for this strategy too. These two tests combined suggest we do not have suitable

instruments, and that estimating an OLS model over the same sample, with the treatment variable included directly instead of through instruments is preferable to the proposed fuzzy RDD approach. We therefore switch to OLS estimation for this strategy, regressing outcomes on the actual treatment status and actual value of the interaction between the treatment status and the running variables.

318. Using the Wu-Hausman test, we reject the null hypothesis that OLS is consistent for the remaining strategies. This suggests that the fuzzy RDD approach is preferable.

319. The McCrary density test statistic suggests we have a large difference between the number of people just crossing the threshold and the number of people falling short, for the FD strategy at firms 1 and 6. This result could imply that the consumers (or their PCA providers) are manipulating their metrics in order to either qualify or avoid treatment. In practice, consumers do not know about the level of the threshold so there is no reason to suspect they are doing this consciously. When we investigate potential reasons for this, we see that the running variable used is the same for both these firms and there are already disincentives in place to discourage consumers from crossing the threshold, such as balance notifications or credit score consequences. This is likely to cause bunching at the threshold as consumers attempt to reduce their borrowing to avoid crossing it.

320. There is a risk that the consumers that are treated are those that are less able to reduce their borrowing or less likely to respond to existing prompts and disincentives than consumers who are not treated. If this is the case, then this may mean the effect of treatment on them is smaller than the effect would be on the control group. We are interested in the effect of the treatment on the treated group, not on all consumers, so this does not cause significant concern.

321. However, if we consider the disincentives as a brake on borrowing that is effective for the control group, but ineffective for the treated group, then we may misinterpret results. A larger increase in borrowing for the treated group relative to the control group would be attributed to treatment, when in reality the difference stems from the 'brake' on borrowing for the control group that does not affect the treated group in the same way. This would lead to us underestimating the impact the treatment has on reducing borrowing and charges.

322. We present our results with this caveat, however, do not believe it significantly biases our results as we observe that a portion of the control group does become treated in the future, suggesting this 'brake' may affect both groups to a similar degree when we consider a longer time horizon (as we do in our analysis).

**Table 16: Repeat use first-stage regression coefficients and test statistics**

| Instrumental variable: | Dependent variable: Repeat Use Status | | | | Dependent variable: Financial Difficulty Status | | | |
|---|---|---|---|---|---|---|---|---|
| | ***Firm 1*** | ***Firm 3*** | ***Firm 6 LT*** | ***Firm 6 ST*** | ***Firm 1*** | ***Firm 3 Pre*** | ***Firm 3 Post*** | ***Firm 6*** |
| **Threshold Dummy** | 0.604***<br>(0.061) | -0.224***<br>(0.076) | 0.902***<br>(-0.007) | 0.118<br>(0.312) | 0.872***<br>(0.004) | 1.971***<br>(0.004) | 0.616***<br>(0.034) | 0.280<br>(0.318) |
| **Running Variable** | 52.665***<br>(2.164) | 40.656***<br>(1.164) | 0.196<br>(-1.715) | 11.942<br>(90.374) | 9.518***<br>(1.342) | 0.330***<br>(0.047) | 3.483<br>(4.353) | 3.968***<br>(0.573) |
| **Running Variable$^2$** | 17.249***<br>(1.256) | 16.993***<br>(0.973) | 0.057<br>(-0.998) | 2.515<br>(25.393) | | 0.599***<br>(0.116) | 1.352<br>(1.686) | 1.405**<br>(0.690) |
| **Threshold Dummy x Running Variable** | -54.453***<br>(9.295) | 27.731***<br>(8.090) | -9.293**<br>(-4.301) | 28.471<br>(90.375) | -9.280***<br>(1.394) | -797.033***<br>(2.038) | -6.119<br>(7.327) | -17.434<br>(32.508) |
| **Threshold Dummy x Running Variable$^2$** | -17.198***<br>(3.894) | -29.764***<br>(3.005) | | -8.618<br>(25.398) | | 183.240***<br>(0.515) | 0.130*<br>(0.071) | 3.427<br>(10.172) |
| **Instrument tests:** | | | | | | | | |
| **Repeat use status** | <2x10$^{-16}$*** | <2x10$^{-16}$*** | <2x10$^{-16}$*** | 0.150 | <2x10$^{-16}$*** | <2x10$^{-16}$*** | <2x10$^{-16}$*** | <2x10$^{-16}$*** |
| **Repeat use status x Running Variable** | <2x10$^{-16}$*** | <2x10$^{-16}$*** | <2x10$^{-16}$*** | 0.267 | <2x10$^{-16}$*** | <2x10$^{-16}$*** | <2x10$^{-16}$*** | <2x10$^{-16}$*** |
| **Repeat use status x Running Variable$^2$** | <2x10$^{-16}$*** | <2x10$^{-16}$*** | | <2x10$^{-16}$*** | | <2x10$^{-16}$*** | <2x10$^{-16}$*** | <2x10$^{-16}$*** |
| **Model Specification tests** | | | | | | | | |
| **Wu-Hausman test** | 0.0448** | 0.0143** | 0.0897* | 0.126 | <2x10$^{-16}$*** | <2x10$^{-16}$*** | <2x10$^{-16}$*** | 0.0067*** |
| **McCrary density test** | 0.178 | 0.501 | 0.229 | 0.604 | <2x10$^{16}$*** | 0.519 | 0.615 | <2x10$^{16}$*** |
| **Note: *p<0.1; **p<0.05; ***p<0.01** | | | | | | | | |

*Source: FCA analysis of repeat use dataset*

## First stage matching: Implementation of the matching procedure

323. In this section we outline the results of the implementation of the matching procedure. We calculate propensity scores for the individuals in our treated group and in our control group. For each person in our treated group, we identify the person in the control with the propensity score which is closest in value to them. We then add this person to our control. We repeat this for every person in our treated group. We allow an individual in the control to be matched with more than one treated person. If they do match with more than one treated person, we duplicate their entry in the control group.

324. In Table 17 we show the average propensity score for each strategies' control and treatment group before and after implementing the matching procedure described in the methodology section. We want the propensity score of a treated group to be as close as possible to the respective control group.

325. We report on a number of measures of the quality of the match:

- Treated and control mean before and after implementing the matching.
- The standardised mean difference between the treated and control groups. The closer to 0 this is, the closer the means of the treated and control groups are.
- The variance ratio. This is a measure of the similarity of the distribution of values of the propensity score in the control and treated group. A value between 0.8 and 1 indicates a good match.

326. Table 17 shows that we are successful in balancing the propensity scores for the control and treated groups for every strategy. In every case, the variance ratio is equal to 1 and the standardised mean difference is reduced to 0. This means that the control group for a strategy is perfectly balanced with the treated group on the propensity score.

**Table 17: Comparison of treated and control samples propensity scores before and after matching**

| Firm name: | Treated mean | Control mean pre matching | Control mean post matching | Standardised mean difference pre matching | Standardised mean difference post matching | Variance ratio pre matching | Variance ratio post matching |
|---|---|---|---|---|---|---|---|
| **Firm 2 RU** | 0.57 | 0.37 | 0.57 | 0.93 | 0.00 | 2.02 | 1.00 |
| **Firm 4 RU** | 0.63 | 0.28 | 0.63 | 1.39 | 0.00 | 1.55 | 1.00 |
| **Firm 5 ST RU*** | 0.38 | 0.27 | 0.38 | 0.61 | 0.00 | 2.41 | 1.00 |
| **Firm 5 LT RU** | 0.52 | 0.48 | 0.52 | 0.35 | 0.00 | 3.28 | 1.00 |
| **Firm 2 FD** | 0.54 | 0.36 | 0.54 | 1.02 | 0.00 | 1.01 | 1.00 |
| **Firm 4 ST FD** | 0.31 | 0.17 | 0.31 | 0.71 | 0.00 | 2.65 | 1.00 |
| **Firm 4 LT FD** | 0.32 | 0.28 | 0.32 | 0.36 | 0.00 | 1.33 | 1.00 |
| **Firm 5 ST FD Pre Jan 2020*** | 0.46 | 0.39 | 0.46 | 0.67 | 0.00 | 0.73 | 1.00 |
| **Firm 5 ST FD Post Jan 2020*** | 0.59 | 0.41 | 0.59 | 1.13 | 0.00 | 0.65 | 1.00 |
| **Firm 5 LT FD** | 0.54 | 0.46 | 0.54 | 0.70 | 0.00 | 0.76 | 1.00 |

**Firm 5 adjusted their ST FD strategy in Jan 2020 to incorporate accounts that would've been in the ST RU strategy (and therefore had no ST RU strategy after this point). Due to this change we evaluate the strategies up to this period and after this period separately.*
*Source: FCA analysis of PCA data and repeat use dataset*

## Estimates of the treatment effects

327. In the following section we show the estimates of the treatment effect on each period after treatment in each firm. We estimate the effect on monthly borrowing and charges; these measures are defined in the 'Data' section of this chapter. When looking at RU strategies, we estimate a reduction caused by treatment against a counterfactual of no treatment. When looking at FD strategies, we estimate a reduction caused by treatment against a counterfactual of remaining on the RU strategy, rather than entering the FD strategy. This is because individuals who qualify for a firm's FD strategy are a subset of all those who qualify for that firm's RU strategy.

328. Charges are a flow variable, measured as the total over a month, whereas monthly borrowing is a stock variable, measured as the average of the end-of-day balance, for each day over the period of a month. This makes the interpretation of the estimated effect for each as follows:

- The savings created by reduced charges up to period $t$, are the sum of the savings in every period from 1 to $t$. For example, if the effect of treatment is to reduce charges by £5 versus the counterfactual in period 1, £10 vs the counterfactual in period 2 and £15 vs the counterfactual in period 3, then the total savings up to period 3 is £30.
- The reduction in monthly borrowing in period $t$ is the estimated effect in period $t$ only. For example, if the effect of the treatment is to reduce monthly borrowing by £50 in period 1, £100 in period 2 and £150 in period 3, then the total reduction in period 3 is £150, as they are borrowing £150 less than they would've without treatment.

329. Tables 19 and 20 and figures 30 and 31 show the estimated effect for the RU strategies; tables 22 and 23 and figures 32 and 33 show the estimated effect of the FD strategies.

330. Each table shows the estimated effect of the strategy, with the estimated lower and upper 95% confidence interval bounds in the brackets below. These are calculated through the process described in the methodology section, so reflect the uncertainty introduced by the decontamination procedure, and in the estimation of the regression. The graphs show the same figures: the bar is the estimated effect, and the error bars are the estimated lower and upper 95% confidence interval. If the confidence interval does not cross 0 (in other words, the upper and lower bound are both positive, or the upper and lower bound are both negative), then we have a 'statistically significant' estimate of the effect to the 95% confidence level.

331. Each period represents a month, so period 1 is the first month of treatment, and period 12 is the 12$^{th}$ month of treatment.

### *Repeat Use*

332. Below we outline the effect of the repeat use strategies on monthly borrowing and overdraft charges. The statistical precision of our results is affected by several factors including the number of observations in our treated and control groups.

333. Table 18 shows how many people are included in our estimation of the effect of each of the strategies in the first period of treatment. We lose some observations after each month because our sampling period has an end date, so if any of an individual's second to twelfth periods of treatment occur after September 2021, then we will not observe their outcomes in these periods of treatment.

**Table 18: Sample sizes used in repeat use effect estimations**

| Firm | Treated group size, $t = 1$ | Control group size, $t = 1$ |
|---|---|---|
| **Firm 1** | 10,933 | 15,187 |
| **Firm 2** | 35,000 | 15,112 |
| **Firm 3** | 5,371 | 30,924 |
| **Firm 4** | 35,000 | 12,347 |
| **Firm 5 ST** | 19,993 | 12,471 |
| **Firm 5 LT** | 44,999 | 23,446 |
| **Firm 6 ST** | 15,312 | 29,553 |
| **Firm 6 LT** | 6,662 | 64,200 |

*Source: FCA analysis of PCA data and repeat use dataset*

## Monthly borrowing

334. Our results describing the effect of the strategies on monthly borrowing are summarised in tables 19 and 22 below. The coefficient in the table shows the difference between the monthly borrowing we observe, and the monthly borrowing we would expect under the counterfactual, in the number of months after treatment denoted by the column header. This is, therefore, our estimate of the impact of the repeat use strategy on monthly borrowing.

335. We estimate that all strategies except those of Firm 3 and Firm 5 had statistically significant negative effects on individuals' borrowing in most periods.

336. We see the effect growing in magnitude over time for each of these firms, consistent with our expectation that it takes some time for consumers' behaviour to adjust to the strategies' nudges. The magnitude of the effects at firms 1 and 4 reduce after approximately 6 months. For Firm 1, the change in magnitude of the treatment effect (it moves closer to £0) suggests that by the 11th period after treatment, the repeat use treatment is no more effective than no treatment at reducing monthly borrowing. For Firm 4, from month 6 onwards the treatment is still more effective at reducing borrowing than no treatment, but less effective than it was in earlier periods.

337. We do not find a statistically significant effect for Firm 3. This is due to a small sample size and having a large variation in the outcomes of the individuals which couldn't be explained entirely by the variation in their treatment status and running variable, making our comparisons between the treated and control group weaker.

338. We find a statistically significant effect of the opposite sign to what we would expect for Firm 5's strategies. Given that a treatment is unlikely to cause an individual to increase their monthly borrowing (as it is in practice a nudge to reduce overdraft usage), we believe this is because our matching failed to construct accurate treatment and control groups. Despite matching well on observables, we expect there are unobserved factors, affecting future charges, that we have failed to balance on. We believe we see this at firm 5 and not firm 2 and 4 because firm 5 applies a wide range of selection criteria to build a consumer-level view. We cannot match consumers on these criteria as we do not observe them in other firms. The selection criteria of firm 2 and 4 is less comprehensive and more closely linked to the matching variables. This means the factors we don't observe in our matching are less likely to be observed and used to select candidates for treatment at firms 2 and 4 than firm 5.

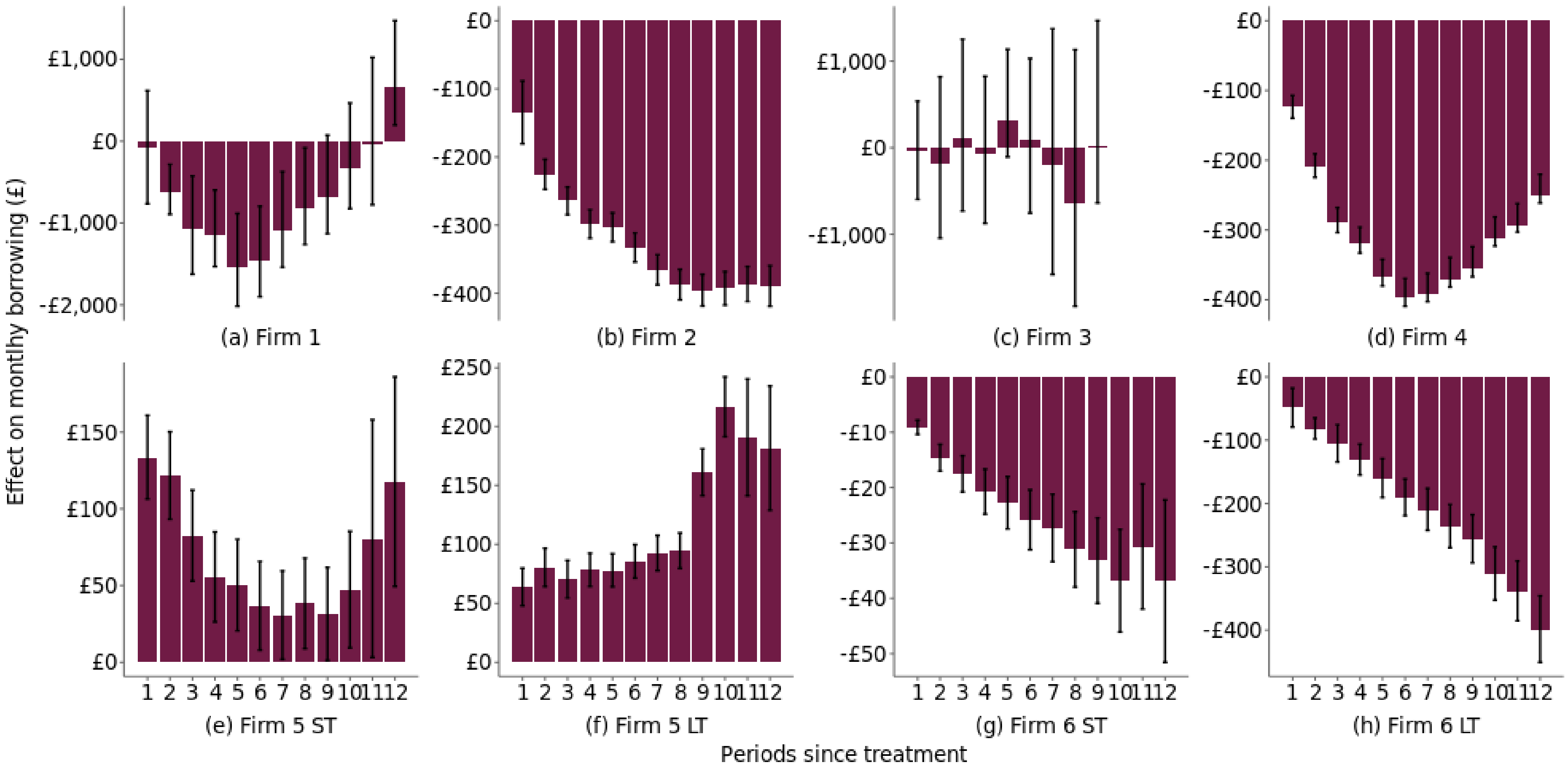
339. Although some firms may choose to reduce interest charges manually for those on their strategy, the main way in which we would expect these individuals to reduce their interest charges, is by reducing their borrowing.

**Table 19: Effect of repeat use strategies on overdraft borrowing in each period after treatment**

| | Effect on borrowing in period (£): | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 |
| **Firm 1** | -74 (-765 - 616) | -625* (-894 - - 283) | -1068* (-1624 - - 425) | -1141* (-1529 - - 597) | -1544* (-2015 - - 884) | -1467* (-1899 - - 794) | -1095* (-1540 - - 372) | -822* (-1261 - - 83) | -684 (-1130 - 72) | -335 (-822 - 466) | -31 (-776 - 1022) | 665 (196 - 1470) |
| **Firm 2** | -134 (-181 - - 88) | -225 (-247 - - 203) | -264 (-285 - - 244) | -298 (-319 - - 277) | -303 (-324 - - 282) | -333 (-354 - - 311) | -365 (-387 - - 343) | -387 (-409 - - 365) | -395 (-419 - - 372) | -393 (-417 - - 368) | -386 (-412 - - 360) | -389 (-419 - - 359) |
| **Firm 3** | -29 (-595 - 536) | -179 (-1041 - 817) | 112 (-729 - 1251) | -64 (-872 - 821) | 321 (-106 - 1137) | 93 (-755 - 1030) | -195 (-1460 - 1372) | -641 (-1827 - 1131) | 15 (-637 - 1464) | | | |
| **Firm 4** | -123* (-140 - - 107) | -210* (-225 - - 191) | -290* (-304 - - 268) | -320* (-333 - - 296) | -368* (-380 - - 343) | -398* (-409 - - 370) | -392* (-403 - - 362) | -370* (-382 - - 340) | -356* (-367 - - 324) | -313* (-323 - - 281) | -293* (-303 - - 262) | -251* (-262 - - 220) |
| **Firm 5 ST** | 134* (106 - 161) | 122* (93 - 150) | 83* (53 - 112) | 56* (26 - 85) | 50* (20 - 80) | 37* (8 - 66) | 31* (2 - 60) | 38* (9 - 68) | 31* (1 - 62) | 47* (9 - 85) | 80* (3 - 158) | 118* (49 - 186) |
| **Firm 5 LT** | 64* (48 - 80) | 81* (64 - 97) | 71* (55 - 86) | 78* (64 - 92) | 78* (64 - 92) | 86* (71 - 100) | 93* (78 - 108) | 95* (80 - 110) | 161* (141 - 181) | 217* (192 - 242) | 191* (141 - 240) | 182* (129 - 234) |
| **Firm 6 ST** | -9* (-10 - -8) | -15* (-17 - -12) | -18* (-21 - -14) | -21* (-25 - -17) | -23* (-27 - -18) | -26* (-31 - -20) | -27* (-33 - -21) | -31* (-38 - -24) | -33* (-41 - -25) | -37* (-46 - -28) | -31* (-42 - -19) | -37* (-52 - -22) |
| **Firm 6 LT** | -49* (-79 - -18) | -82* (-98 - -65) | -105* (-135 - - 76) | -131* (-155 - - 106) | -160* (-191 - - 129) | -191* (-219 - - 161) | -210* (-242 - - 176) | -237* (-270 - - 202) | -257* (-294 - - 218) | -312* (-353 - - 268) | -339* (-385 - - 291) | -400* (-451 - - 346) |

*Source: FCA Analysis of PCA data and repeat use dataset*
*To calculate whether or not an individual had met Firm 3's threshold, we need 12 months of data. This means we only have the remaining 9 months of data left to estimate effects over.*
**Significant at 95% confidence level*

**Figure 30: Effect of repeat use strategies on overdraft borrowing in each period after treatment**




*Source: FCA analysis of PCA data and repeat use dataset*

## Overdraft charges

340. Our results are summarised in Table 20 and Figure 31 below. The coefficient in the table is the treatment effect in a given month. It represents the average difference between the actual outcomes for the individuals in our sample who on the strategy and the expected outcomes under the counterfactual scenario where they are not on a strategy. For charges, this difference is the amount saved in overdraft charges in the month.

341. Reducing monthly overdraft borrowing is the primary way in which we would expect treated consumers to reduce their overdraft charges. Therefore, we would expect our results for changes to charges to reflect our results for changes to monthly borrowing

342. We estimate that the strategies of firms 2, 4, and 6, were successful at reducing overdraft charges in most periods. In most cases, the magnitude of this effect grows over time. We would expect this, as consumers need to reduce their balances and change their behaviours to reduce their charges, which can take time, as we see from the results in the previous section.

343. For Firm 1, we see a reduction in charges, which grows over the first 6 months, but then begins to fall as charges move closer to the counterfactual. This may suggest that the behaviour change caused by the strategy is only temporary, or that under the counterfactual, the individual would reduce their charges anyway, just 6-months later than if they had been treated. Either way, the individual saves money over the 12 months versus the counterfactual due to the savings they accrue in the first 6.

344. For Firm 3, we don’t see a statistically significant reduction against the counterfactual. Like the explanation for the wide confidence intervals around the monthly borrowing effect estimates, this is due to the small sample size we have available to evaluate the effect.

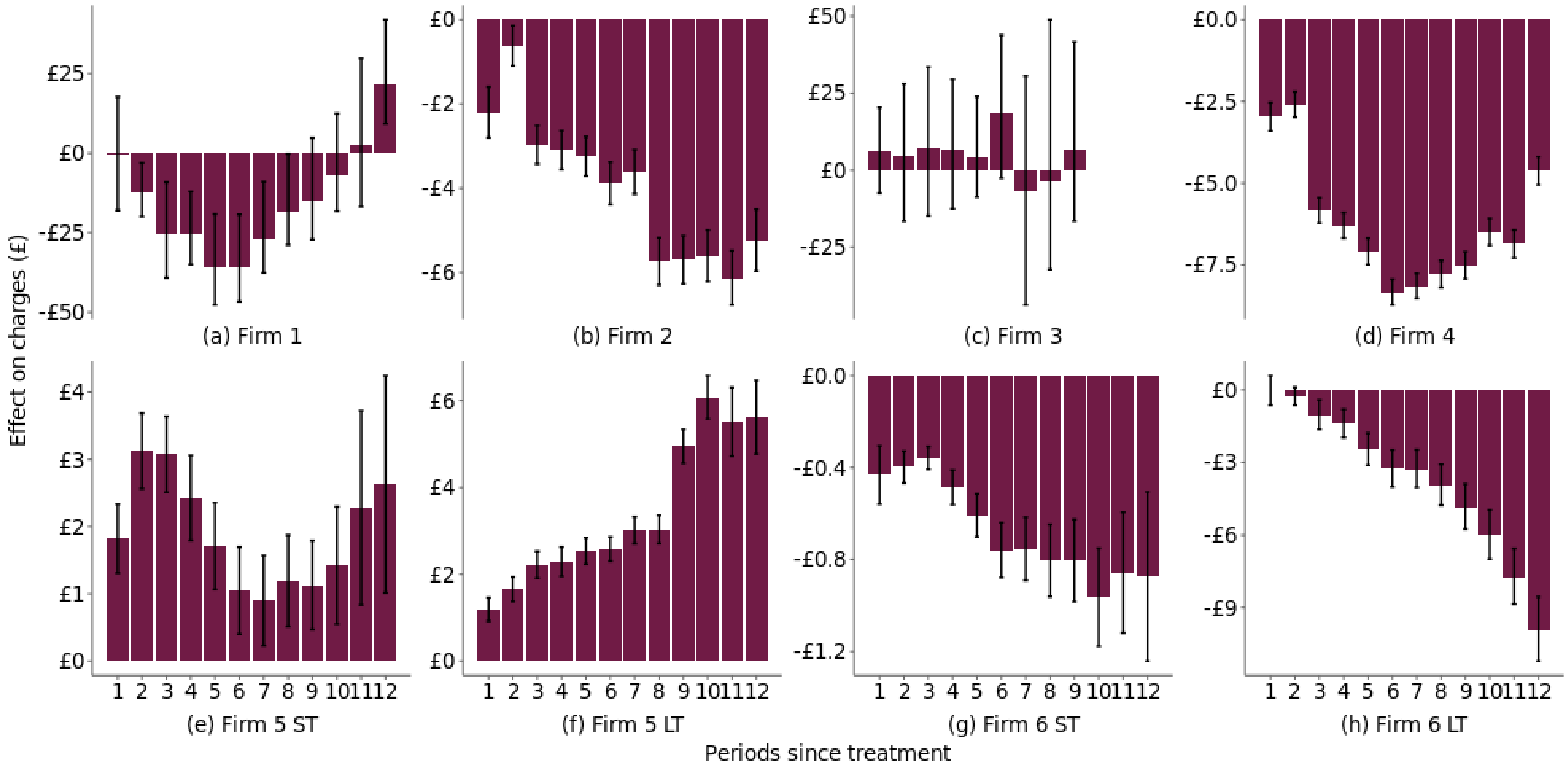
345. For Firm 5, we find a statistically significant positive effect, like we did for borrowing. Again, we think it is more likely that the effects stems from unobserved differences between the treated and control group than it is the effect of the strategy.

**Table 20: Effect of repeat use strategies on overdraft charges in each period after treatment**

| | Effect on charges in period (£): | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **1** | **2** | **3** | **4** | **5** | **6** | **7** | **8** | **9** | **10** | **11** | **12** |
| **Firm 1** | -0.22 (-18.07 - 17.62) | -12.41 (-19.95 - - 3.09) | -25.27* (-39.19 - - 9.16) | -25.49 (-35.03 - 12.09) | -35.74* (-47.75 - - 19.18) | -35.89* (-46.66 - - 19.28) | -26.69* (-37.62 - - 8.99) | -18.24* (-28.91 - - 0.25) | -14.88 (-27.1 - 4.78) | -6.73 (-18.25 - 12.38) | 2.7 (-16.91 - 29.68) | 21.68 (9.26 - 41.94) |
| **Firm 2** | -2.20* (-2.8 - - 1.6) | -0.63* (-1.1 - - 0.15) | -2.97* (-3.43 - - 2.52) | -3.10* (-3.56 - - 2.64) | -3.25* (-3.71 - - 2.78) | -3.89* (-4.39 - - 3.38) | -3.62* (-4.14 - - 3.09) | -5.74* (-6.3 - - 5.18) | -5.70* (-6.27 - - 5.13) | -5.61* (-6.22 - - 5) | -6.13* (-6.78 - - 5.48) | -5.24* (-5.97 - - 4.51) |
| **Firm 3** | 6.35 (-7.51 - 20.21) | 4.43 (-16.52 - 27.99) | 7.09 (-14.77 - 33.36) | 6.77 (-12.62 - 29.43) | 3.92 (-8.76 - 23.8) | 18.28 (-2.59 - 43.76) | -6.98 (-43.74 - 30.5) | -3.65 (-32.23 - 48.8) | 6.71 (-16.5 - 41.63) | | | |
| **Firm 4** | -2.97* (-3.4 - - 2.53) | -2.59* (-2.98 - - 2.2) | -5.83* (-6.22 - - 5.45) | -6.29* (-6.67 - - 5.9) | -7.08* (-7.49 - - 6.68) | -8.33* (-8.72 - - 7.93) | -8.14* (-8.52 - - 7.76) | -7.78* (-8.18 - - 7.37) | -7.51* (-7.92 - - 7.09) | -6.49* (-6.9 - - 6.07) | -6.86* (-7.29 - - 6.43) | -4.63* (-5.05 - - 4.2) |
| **Firm 5 ST** | 1.82* (1.31 - 2.33) | 3.13* (2.57 - 3.69) | 3.08* (2.51 - 3.64) | 2.43* (1.8 - 3.06) | 1.71* (1.07 - 2.36) | 1.05* (0.4 - 1.7) | 0.9* (0.23 - 1.57) | 1.2* (0.52 - 1.88) | 1.13* (0.47 - 1.79) | 1.42* (0.55 - 2.29) | 2.28* (0.83 - 3.73) | 2.63* (1.02 - 4.25) |
| **Firm 5 LT** | 1.19* (0.93 - 1.46) | 1.65* (1.37 - 1.93) | 2.22* (1.91 - 2.53) | 2.29* (1.95 - 2.63) | 2.54* (2.23 - 2.84) | 2.58* (2.3 - 2.86) | 3.01* (2.7 - 3.32) | 3.03* (2.71 - 3.35) | 4.94* (4.56 - 5.33) | 6.07* (5.58 - 6.57) | 5.51* (4.72 - 6.3) | 5.62* (4.77 - 6.46) |
| **Firm 6 ST** | -0.43* (-0.56 - - 0.31) | -0.4* (-0.47 - - 0.33) | -0.36* (-0.41 - - 0.31) | -0.49* (-0.56 - - 0.41) | -0.61* (-0.7 - - 0.52) | -0.76* (-0.88 - - 0.64) | -0.75* (-0.89 - - 0.62) | -0.8* (-0.96 - - 0.65) | -0.81* (-0.98 - - 0.63) | -0.96* (-1.18 - - 0.75) | -0.86* (-1.12 - - 0.59) | -0.87* (-1.24 - - 0.51) |
| **Firm 6 LT** | -0.03 (-0.64 - 0.58) | -0.27 (-0.64 - 0.11) | -1.03* (-1.64 - - 0.42) | -1.4* (-1.96 - - 0.81) | -2.47* (-3.12 - - 1.79) | -3.26* (-4 - - 2.49) | -3.27* (-4.02 - - 2.47) | -3.95* (-4.78 - - 3.08) | -4.85* (-5.76 - - 3.89) | -6.02* (-7 - - 4.97) | -7.75* (-8.86 - - 6.56) | -9.93* (-11.21 - - 8.55) |

*Source: FCA Analysis of PCA data and repeat use dataset*
*To calculate whether or not an individual had met Firm 3's threshold, we need 12 months of data. This means we only have the remaining 9 months of data left to estimate effects over.*
**Significant at 95% confidence level*

**Figure 31: Effect of repeat use strategies on overdraft charges in each period after treatment**




*Source: FCA analysis of PCA data and repeat use dataset*

*Financial Difficulty*

346. We estimate the effect of the FD strategies by comparing individuals who are treated against a counterfactual scenario where they were not treated on the firm's FD strategy but are instead treated on the firm's RU strategy. Therefore, the estimates can be read to be the impact of the FD strategy on top of the impact of the RU strategy.

347. At the beginning of the period Firm 5 had a ST RU strategy and a ST FD strategy. In January 2021, they removed their ST RU strategy and adjusted the criteria of their ST FD strategy, so it included everyone that would've fallen into the ST RU strategy. We estimate the effect of the ST FD strategy before and after this change separately.

348. Firm 3 changed the criteria for their FD strategy in February 2021. We estimate the effect of the strategy up to (and including) January 2021 and the effect of the strategy from February 2021 to September 2021 separately.

349. Table 21 shows how many people are included in our estimation in the effect of each of the strategies in the first period of treatment. As with the repeat use sample size, we lose observations between $t = 1$ and $t = 12$. This is because our sampling period has an end date, so if any of an individual's second to twelfth periods of treatment are after September 2021, then we will not observe their outcomes in these periods of treatment.

**Table 21: Sample sizes used in repeat use effect estimations**

| Firm | Treated group size $t = 1$ | Control group size $t = 1$ |
|---|---|---|
| **Firm 1** | 41,360 | 38,343 |
| **Firm 2** | 35,000 | 16,166 |
| **Firm 3 Pre** | 662 | 1,366 |
| **Firm 3 Post*** | 1,008 | 1,960 |
| **Firm 4 ST** | 12,381 | 8,899 |
| **Firm 4 LT** | 16,519 | 11,989 |
| **Firm 5 ST Pre** | 28,963 | 16,688 |
| **Firm 5 ST Post*** | 40,000 | 16,865 |
| **Firm 5 LT** | 40,000 | 18,542 |
| **Firm 6** | 2,411 | 9,424 |

*Source: FCA analysis of PCA data and repeat use dataset*

### Monthly borrowing

350. Table 22 and Figure 32 show the effect of the firms' FD strategies on monthly overdraft borrowing. We find very mixed estimates of the effect of the financial difficulty strategy at different firms.

351. As with the repeat use strategies, the firm with the strongest effect on monthly borrowing seems to be Firm 1. The magnitude of this effect seems to grow over time. We estimate a reduction in monthly borrowing after 12 months on the strategy for all the firms except Firm 4 LT and Firm 6. The estimated reduction is not statistically significant for Firm 3, neither before nor after the strategy change.

352. We find effects in the opposite direction to what we would expect for Firm 5 and for Firm 4's LT strategy. Like firm 5's RU strategies, these two strategies use a comprehensive range of variables as criteria for treatment. We do not observe all these variables at other firms. Therefore, it is more likely we have failed to match on an unobserved characteristic which is important to future outcomes, that is observed by firms 4 and 5 when judging whether someone has qualified for their FD strategies, and this is biasing our estimates.

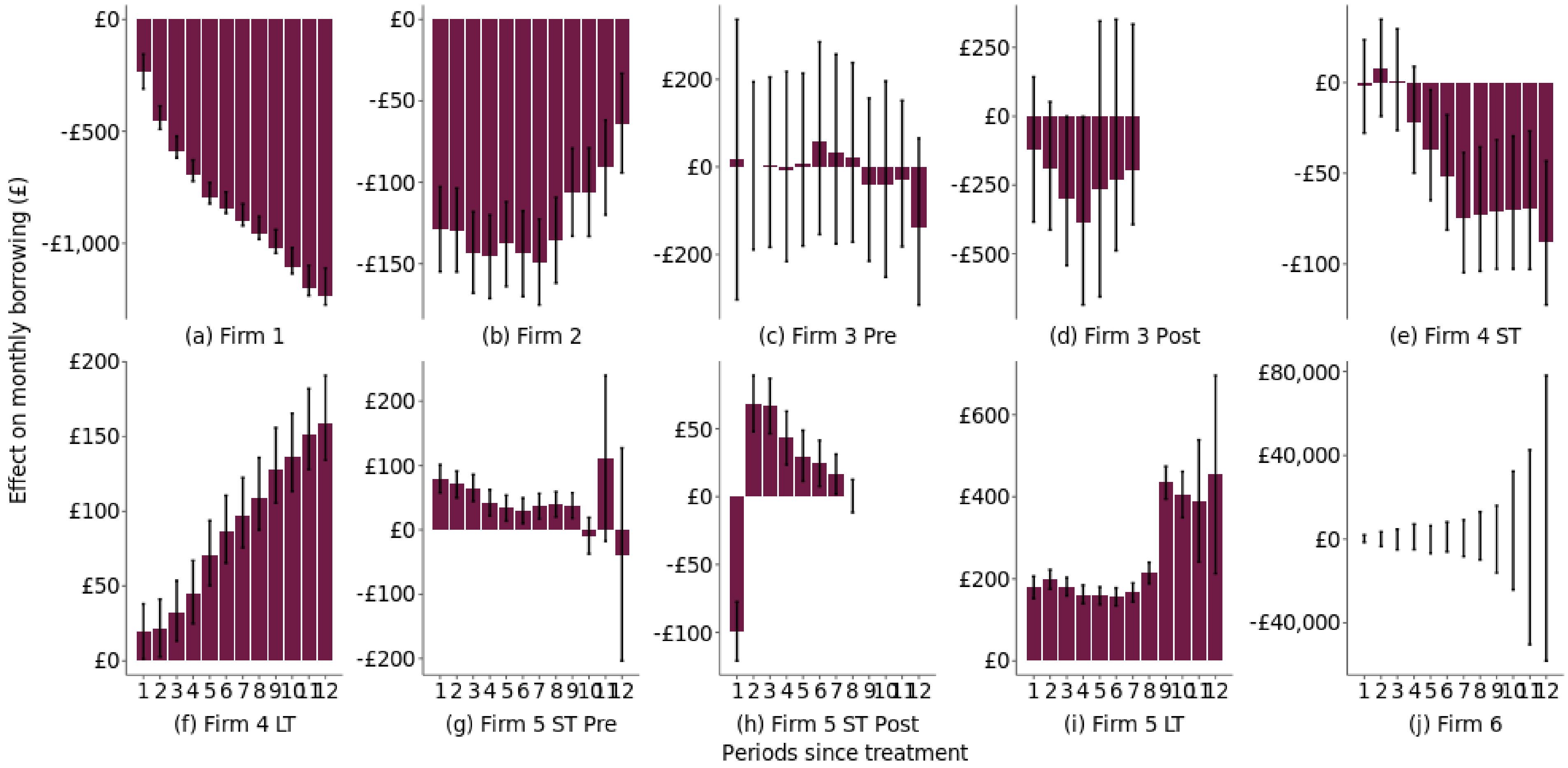
353. The lack of significance for Firm 3's estimates is due to the small number of observations, as shown in Table 18.

**Table 22: Effect of financial difficulty strategies on borrowing in each month after treatment**

| | Effect on borrowing in period (£) | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | **1** | **2** | **3** | **4** | **5** | **6** | **7** | **8** | **9** | **10** | **11** | **12** |
| **Firm 1** | -233* (-310 - -156) | -452* (-491 - -386) | -586* (-618 - -521) | -694* (-723 - -628) | -797* (-823 - -729) | -841* (-866 - -771) | -898* (-922 - -824) | -958* (-982 - -881) | -1020* (-1045 - -939) | -1110* (-1136 - -1020) | -1201* (-1233 - -1099) | -1232* (-1275 - -1112) |
| **Firm 2** | -129* (-155 - -103) | -129* (-155 - -104) | -143* (-168 - -118) | -146* (-171 - -120) | -138* (-164 - -112) | -144* (-170 - -118) | -149* (-175 - -123) | -136* (-162 - -109) | -106* (-133 - -79) | -106* (-133 - -79) | -91* (-120 - -62) | -64* (-94 - -33) |
| **Firm 3 Pre** | 16 (-303 - 336) | -1 (-189 - 194) | 5 (-183 - 205) | -6 (-216 - 218) | 8 (-181 - 214) | 57 (-154 - 285) | 31 (-176 - 257) | 23 (-172 - 238) | -40 (-215 - 157) | -40 (-252 - 196) | -29 (-182 - 151) | -140 (-315 - 65) |
| **Firm 3 Post** | -121 (-385 - 142) | -193 (-413 - 52) | -300* (-542 - -1) | -390* (-686 - -1) | -263 (-656 - 345) | -233 (-488 - 351) | -199 (-394 - 334) | | | | | |
| **Firm 4 ST** | -2* (-28 - 24) | 8* (-19 - 35) | 0* (-26 - 30) | -22* (-50 - 9) | -37* (-65 - -4) | -52* (-81 - -18) | -75* (-105 - -39) | -73* (-104 - -36) | -71* (-103 - -32) | -70* (-103 - -30) | -69* (-103 - -27) | -88* (-123 - -43) |
| **Firm 4 LT** | 20* (1 - 38) | 21* (3 - 41) | 33* (13 - 54) | 45* (25 - 67) | 71* (50 - 94) | 86* (65 - 110) | 97* (76 - 123) | 109* (87 - 136) | 128* (105 - 156) | 136* (113 - 166) | 152* (128 - 182) | 159* (134 - 191) |
| **Firm 5 ST Pre** | 79* (58 - 101) | 71* (50 - 91) | 65* (45 - 86) | 42* (22 - 63) | 34* (14 - 54) | 30* (10 - 49) | 37* (17 - 56) | 40* (20 - 59) | 38* (18 - 58) | -9 (-37 - 19) | 111 (-18 - 240) | -38 (-204 - 127) |
| **Firm 5 ST Post** | -99* (-121 - -77) | 69* (48 - 89) | 67* (46 - 87) | 43* (24 - 63) | 30* (11 - 49) | 25* (8 - 41) | 17* (2 - 31) | 1 (-12 - 13) | | | | |
| **Firm 5 LT** | 179* (152 - 206) | 199* (175 - 222) | 181* (158 - 203) | 162* (140 - 184) | 159* (138 - 180) | 156* (135 - 177) | 167* (144 - 190) | 214* (188 - 240) | 435* (395 - 474) | 405* (349 - 461) | 390* (241 - 539) | 454* (212 - 696) |
| **Firm 6** | 223 (-1563 - 2009) | 26 (-3482 - 3420) | 15 (-5141 - 4613) | 2 (-5060 - 7171) | 3 (-6862 - 6409) | -15 (-6142 - 8105) | -39 (-8265 - 9109) | -79 (-9911 - 13018) | -101 (-16165 - 15896) | -93 (-24193 - 32285) | -101 (-50421 - 42545) | -101 (-58140 - 78129) |

*Source: FCA Analysis of PCA data and repeat use dataset*
**Significant at 95% confidence level*

**Figure 32: Effect of financial difficulty strategies on borrowing in each period after treatment**




*Source: FCA analysis of PCA data and repeat use dataset*

### Overdraft charges

354. Table 23 and Figure 33 show the effect of the FD strategies on overdraft charges. The effects on charges mirror the effects on borrowing, as we would expect given reducing borrowing is the primary channel by which account holders will reduce their charges.

355. We do not find evidence of reductions in charges for firms 5, 6 and for Firm 4's LT strategy. For Firm 4 LT and Firm 5, this could be because there are unaccounted differences between the treated and control groups, and this is biasing our estimates. Both before and after the change to Firm 3's strategy, we estimate a reduction, however we have fewer observations, meaning our estimates are imprecise, indicated by the large confidence interval, so this estimate is not statistically significant.

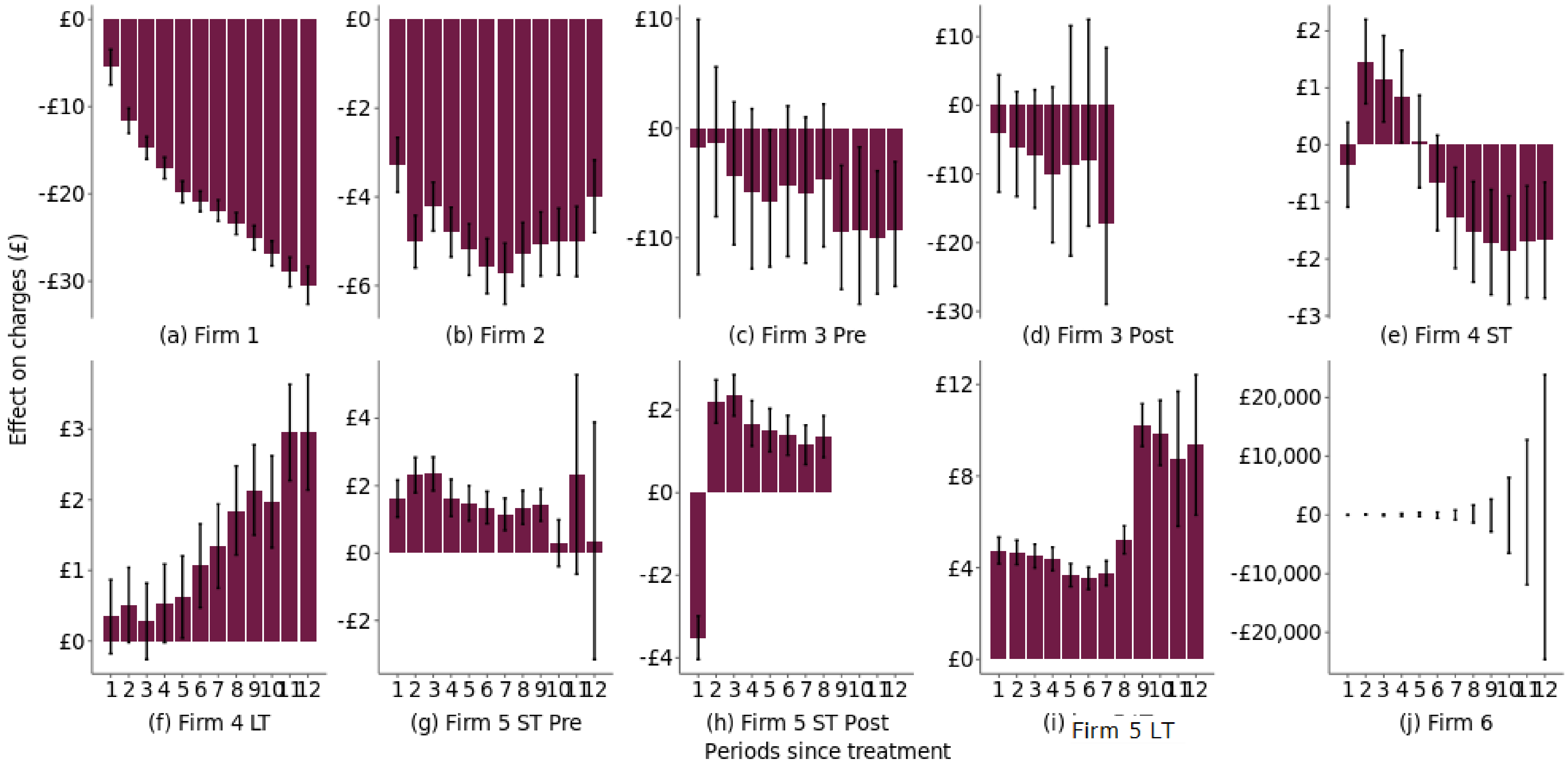
356. The uncertainty around our estimates for Firm 6 is very large, as indicated by the wide confidence interval. We expect this is because we start with a very small sample of treated and control individuals, so the uncertainty introduced by using a fuzzy approach, and the decontamination procedure, is large and grows exponentially over the period.

**Table 23: Effect of financial difficulty strategies on overdraft charges in each period after treatment**

| | Effect on charges in period (£): | | | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 |
| **Firm 1** | -5.48* (-7.52 - - 3.44) | -11.63* (-13.06 - - 10.20) | -14.73* (-16 - - 13.45) | -17.03* (-18.25 - - 15.81) | -19.76* (-20.99 - - 18.52) | -20.85* (-22 - - 19.67) | -21.9* (-23.1 - - 20.69) | -23.4* (-24.65 - - 22.14) | -25.04* (-26.41 - - 23.66) | -26.84* (-28.26 - - 25.4) | -28.97* (-30.64 - - 27.27) | -30.5* (-32.65 - - 28.33) |
| **Firm 2** | -3.28* (-3.89 - - 2.66) | -5.01* (-5.6 - - 4.42) | -4.22* (-4.76 - - 3.67) | -4.8* (-5.35 - - 4.24) | -5.19* (-5.77 - - 4.61) | -5.56* (-6.18 - - 4.94) | -5.73* (-6.42 - - 5.04) | -5.29* (-6 - -4.58) | -5.06* (-5.78 - - 4.34) | -5.01* (-5.76 - - 4.26) | -5* (-5.79 - - 4.22) | -3.98* (-4.8 - -3.17) |
| **Firm 3 Pre** | -1.71 (-13.35 - 9.93) | -1.37 (-8.08 - 5.6) | -4.32 (-10.64 - 2.41) | -5.76 (-12.82 - 1.77) | -6.71* (-12.66 - - 0.14) | -5.19 (-11.71 - 2.02) | -6.01 (-12.3 - 1.01) | -4.7 (-10.82 - 2.19) | -9.49* (-14.71 - - 3.42) | -9.33* (-16.06 - - 1.72) | -10.02* (-15.13 - - 3.89) | -9.32* (-14.44 - - 3.05) |
| **Firm 3 Post** | -4.12 (-12.65 - 4.41) | -6.06 (-13.28 - 2) | -7.26 (-14.94 - 2.26) | -10.12 (-19.97 - 2.66) | -8.66 (-21.95 - 11.58) | -8.08 (-17.61 - 12.51) | -17.2 (-28.97 - 8.38) | | | | | |
| **Firm 4 ST** | -0.35 (-1.09 – 0.39) | 1.46* (0.72 – 2.2) | 1.16* (0.4 – 1.91) | 0.85 (0.04 – 1.66) | 0.06 (-0.75 – 0.87) | -0.67 (-1.5 – 0.16) | -1.28* (-2.17 - - 0.4) | -1.52* (-2.41 - - 0.64) | -1.71* (-2.63 - - 0.79) | -1.85* (-2.79 - - 0.9) | -1.7* (-2.68 - - 0.72) | -1.67* (-2.69 - - 0.66) |
| **Firm 4 LT** | 0.35 (-0.18 – 0.87) | 0.51 (-0.02 – 1.04) | 0.28 (-0.26 – 0.82) | 0.54 (-0.02 – 1.09) | 0.62* (0.04 – 1.21) | 1.07* (0.47 – 1.66) | 1.34* (0.75 – 1.94) | 1.85* (1.22 – 2.48) | 2.14* (1.5 – 2.78) | 1.97* (1.32 – 2.62) | 2.95* (2.27 – 3.63) | 2.96* (2.14 – 3.77) |
| **Firm 5 ST Pre** | 1.61* (1.07 - 2.16) | 2.31* (1.79 - 2.83) | 2.35* (1.85 - 2.85) | 1.64* (1.09 - 2.18) | 1.48* (0.96 - 2) | 1.35* (0.87 - 1.83) | 1.15* (0.67 - 1.63) | 1.35* (0.85 - 1.86) | 1.43* (0.95 - 1.9) | 0.29 (-0.4 - 0.99) | 2.33 (-0.62 - 5.29) | 0.36 (-3.16 - 3.88) |
| **Firm 5 ST Post** | -3.51* (-4.04 - - 2.98) | 2.21* (1.68 - 2.73) | 2.35* (1.85 - 2.85) | 1.67* (1.13 - 2.22) | 1.51* (0.99 - 2.03) | 1.38* (0.9 - 1.86) | 1.16* (0.68 - 1.63) | 1.35* (0.85 - 1.85) | | | | |
| **Firm 5 LT** | 4.75* (4.17 - 5.33) | 4.67* (4.14 - 5.21) | 4.52* (4 - 5.03) | 4.39* (3.88 - 4.9) | 3.68* (3.17 - 4.19) | 3.54* (3.06 - 4.02) | 3.77* (3.23 - 4.31) | 5.23* (4.62 - 5.83) | 10.23* (9.3 - 11.16) | 9.89* (8.47 - 11.31) | 8.76* (5.81 - 11.71) | 9.37* (6.3 - 12.43) |
| **Firm 6** | -13.02 (-53.55 – 27.51) | 45.38 (-24.23 – 140.28) | 13.49 (-158.09 – 134.21) | 2.88 (-228.95 – 238.12) | 31.69 (-269.83 – 400.37) | 8.86 (-546.63 – 464.54) | 1.11 (-832.35 – 819.79) | 51.45 (-1305.76 – 1666.66) | 59.45 (-2826.36 – 2662.59) | -21.83 (-6502.68 – 6347.83) | 71.55 (-11927.43 – 12809.46) | 85.71 (-24641.38 – 23901.16) |

*Source: FCA Analysis of PCA data and repeat use dataset*
**Significant at 95% confidence level*

**Figure 33: Effect of financial difficulty strategy on overdraft charges in each period after treatment**




*Source: FCA analysis of PCA data and repeat use dataset*

## Conclusions

357. Our results suggest that to varying degrees, the RU strategies were broadly successful at reducing charges and borrowing. We find mixed results from the FD strategies, when compared to the RU strategies, but we would expect them to reduce charges and borrowing when compared to being on no strategy at all.

358. In most cases our models performed well, producing relatively precise results (i.e. with small confidence intervals, suggesting a high degree of certainty) in the direction we expected. In some cases, the uncertainty introduced by our decontamination procedure and the instrumental variable approach made it difficult to provide estimates. We also suspect some upward bias in our matching estimates due to an inability to control for certain unobserved characteristics, meaning we underestimate the reduction in charges and monthly borrowing.

359. Our analysis is at the account level rather than consumer level, so there are some caveats to our results. In particular, we have not been able to identify the effect of the strategy on other PCAs the account holder may have. While there is a possibility that some account holders may reduce borrowing through the treated account and increase borrowing at untreated accounts, we think this is unlikely to undermine our estimates. This is because the treatment through the repeat use and financial difficulty strategies is almost completely behavioural. In most cases, the strategies do not place limitations on what the account holder can do, only encourage them to consider whether they are using their overdraft appropriately and offer help to reduce overdraft use. There is no clear reason why this kind of treatment would cause a consumer to reduce their overdraft use through one account, and increase it through another, unless the other account offered preferential borrowing conditions. In this case, our estimate of savings may be an overestimate, but we would still expect a saving, as they would be borrowing on preferential terms.

360. A second caveat is that we have not been able to control for the effect on other forms of borrowing. Therefore, our estimate of the effect may be an overestimate if consumers routinely reduce overdraft use and replace this with other forms of borrowing. We expect firms to only reduce credit limits for customers who are in actual or potential financial difficulty in circumstances that comply with our guidance in CONC 5D and our Overdraft Finalised Guidance (Published September 2020). Such reduction in limits is rare. More commonly where customers are in actual or potential financial difficulty firms will offer forbearance and refer consumers to a debt advisor. Therefore, in most cases if a consumer does substitute to another form of borrowing, the driver of this would be because the prompts from the RU or FD strategy have made them aware of the cumulative cost of their borrowing and they are substituting to less expensive products such as credit cards and short-term personal loans. In this case, our estimate of savings may be an overestimate, but again we would still expect a saving, as they would be borrowing on preferential terms.

361. We have not been able to test for a substitution to other forms of borrowing for the repeat use remedy but have for the pricing remedy. There we find a lack of evidence that a significant increase in borrowing on more expensive products has taken place as a result of this policy package. We consider it unlikely therefore that it would have taken place for repeat users, as, if they are encouraged to substitute away from overdrafts, it would be only to use cheaper alternatives.