Consumer Redress Scheme for Motor Finance

Technical Annex 1

Data, Analysis of Loss and Liability and Cost Methodologies

7 October 2025

Contents

Data Guide

Introduction

1.1 We have gathered quantitative data on the motor finance market to inform policy development. This annex describes the data sources collected, their coverage and how they support our analysis. It is not an exhaustive list of all motor finance-relevant data collected. Rather, we describe in detail the data sources underpinning the core analysis in this consultation.

1.2 Our initial review of the market commenced in 2017, when we set out to assess whether the products caused harm to consumers, and whether the market was functioning as well as it could. In early 2019, we published the final report of this initial review, and in October of that year engaged in a consultation regarding discretionary commission models (DCAs).

1.3 Since our intervention in the motor finance market (effective January 28, 2021), we have continued to collect data from firms active in the market. We have engaged in several data requests (some repeated and/or ongoing, with various breadth).

1.4 Most recently, we initiated data collections in the motor finance market alongside ongoing actions regarding complaints:

1.5 In January 2024 the Financial Ombudsman issued two final decisions in favour of complainants regarding DCAs. Following the Financial Ombudsman's decisions, we exercised our powers under Section 166 (s166) of the Financial Services and Markets Act 2000 to appoint a *Skilled Person* (in this case, a professional services firm) to collect evidence on firm practices in relation to historical motor finance commission arrangements from 6 April 2007[1] to 28 January 2021 ("s166 review"). This review collected casefile data and focused largely on Discretionary Commission Arrangements (DCAs) but also collected information on a small set of agreements involving non-DCAs.

1.6 Alongside the s166 review, in early 2025 we worked with an external statistician to collect a dataset that was constructed to provide high-level agreement data from a sample of lenders within the motor finance market, covering the period from 6 April 2007 to 25 October 2024 (DD1). We requested data from 36 lenders, of which 34 were able to provide suitable data.

1.7 On 12 April 2024 we issued a Dear CEO letter to motor finance lenders and credit brokers reminding firms to maintain adequate financial resources and plan for additional operational costs from increased complaints and, where applicable, to meet the costs of resolving those complaints. In that letter we also notified firms that we would monitor financial resources held by firms and DCA-related complaints volumes and may be collecting further data from selected firms for these purposes. We began collecting data on a quarterly basis from May 2024.

1.8 In September 2024 we reached out to the lenders involved in the 2017-2019 market review to collect high-level agreement data from January 2018 to August 2024 ("Loan level data"), to collect a similarly sampled collection as what was taken during the previous review.

[1] 6 April 2007 was when the Financial Ombudsman's jurisdiction expanded to include consumer credit disputes including those relating to motor finance. Prior to this date the Financial Ombudsman did not have authority to handle these types of complaints. The unfair relationships provisions in Part IX of the Consumer Credit Act 1974 also came into force on 6 April 2007.

1.9 Also in September 2024, we initiated a quantitative and qualitative data collection regarding 2022 and 2023 agreements from motor finance lenders and brokers, to understand how the market for motor finance worked following the FCA's 2021 intervention and how it could potentially be impacted following a redress scheme.

1.10 Following the 25 October 2024 Court of Appeal Judgment (CoA) in *Johnson*, in January 2025 we collect a sample of casefile data from 36 lenders of non-discretionary arrangements (non-DCA) agreements, with this sample again constructed in line with advice from an external statistician and covering 6 April 2007 to 25 October 2024 ("DD2").

1.11 In April 2025, we expanded upon the DD1 data request asking for agreement level data on all regulated motor finance agreements to cover the period 26 October 2024 to 31 March 2025 ("DD3").

1.12 Each data collection served a different purpose which means they have differing coverage and samples.

1.13 For the analysis contained in our Cost Benefit Analysis (CBA), we have also relied on data sources collected for general purposes (the cost of living consumer credit data collection and Credit Reference Agency data).

1.14 In this section we provide further details of 9 data sources. We touch on why and when they were collected, how they have been used, and their strengths and limitations.

Table 1: List of datasets

No.	Name of dataset	Analysis
1	Loan level data	Analysis of loss Market impacts (Technical Annex 3) State of competition (Technical Annex 2) Overview of loan level data (Diagnostic Report)
2	Section 166 (s166) customer assessment form data / Skilled person review + DCA casefile review	Analysis of loss Skilled Person and DCA casefile review findings (Diagnostic Report)
3	Data drop 1 (DD1)	Redress liability estimates Market impacts (Technical Annex 3)
4	Data drop 2 (DD2) / Non-DCA casefile review	Analysis of loss Non DCA case file review findings (Diagnostic Report)
5	Credit Reference Agency (CRA)	Analysis of loss
6	Motor finance commission monitoring surveys	Non-redress cost estimates Market impacts (Technical Annex 3)
7	Cost of living consumer credit data collection	Market impacts (Technical Annex 3)
8	Motor Finance Lender Survey	Market impacts (Technical Annex 3) State of competition (Technical Annex 2)
9	Motor Finance Broker Survey	State of competition (Technical Annex 2)

Summary of datasets

1.15 The table below contains a summary of the datasets, while the sections below contain additional information and relevant summary statistics.

Table 2: Summary of key data sources

Dataset	Coverage	Key Variables	Strengths and Limitations
Loan level data ***Requested in September 2024 (received in October 2024) to build upon the market sample collected for the FCA's Motor Finance Review (commenced in 2017).***	10% quasi-random sample of agreements from 18 motor finance lenders We estimate the lenders cover 61-65% of the regulated motor finance market in 2018-2021 Full dataset contains approx. 700,000 agreements across all commission models Covers all agreement types, all commission structures and all origination channels	Type of agreement Terms of the agreements Vehicle information (make, condition) Customer creditworthiness Origination details Commission structures Customer credit score	*Strengths:* Comprehensive loan level data across majority of motor finance market *Limitations*: Inconsistency across reported customer credit scores, so normalisation process completed by FCA team (potential for estimation and/or scaling errors) Some lenders failed to comply with time request (January 2018 – August 2024), so final sample is time-restricted to January 1 2019 – December 31 2022 Data may not be representative of the market
Section 166 (s166) Customer assessment form data / Skilled person review + DCA casefile review	11 motor finance lenders (s166 / Skilled person review), 12 motor finance lenders (DCA casefile review) We estimate the lenders captured in s166 cover 66.1% of the motor finance market as of 2023 by value of outstanding	Terms of the agreement Vehicle information (make, condition) Loan origination details Commission structure	*Strengths*: Sampling methodology developed by an external statistician *Limitations*: Small sample size

Dataset	Coverage	Key Variables	Strengths and Limitations
	motor finance loans in 2023 (DCA casefile review lenders capture an estimated 18.2%) Each lender for s166 / Skilled person review was required to provide 299 DCA customer casefiles and 10 non-DCA customer casefiles Each lender for the DCA casefile review was asked to provide 3 to 8 DCA customer casefiles Full DCA dataset contains approx. 3,263 customer casefiles (s166 / Skilled person review) + 70 customer case files (DCA casefile review sample), with a total of 3,333 agreements.	Disclosure information – how lenders disclosed DCA commission structures	
Data drop 1 (DD1)	High-level agreement data from 34 lenders within the UK motor finance market, covering the period 6 April 2007 to 25 October 2024 Contains approx. 31 million agreements that were started during this period Lenders represented around 89% of the lending market	Lender name Number of outstanding loans Year the contract was signed Contract signed and end date (contract length can be derived) Agreement type (both DCAs and non-DCAs)	*Strengths*: Sampling methodology developed by an external statistician Contains all agreements from lenders covering a large proportion of the market, including the entire population of the largest 19 lenders. *Limitations*:

Dataset	Coverage	Key Variables	Strengths and Limitations
	based on outstanding loan values in 2023	APR Product type Loan value Commission amount	Limited data points for smaller lenders. Some missing data. Sampling was taken to be representative at one point in time, rather than across entire sample period.
Data drop 2 (DD2) / Non-DCA casefile review	Sampled from 36 motor finance lenders Lenders covered an estimated 89% of the lending market based on outstanding loan values in 2023 Contains a random sample of between 3 to 15 non-DCA casefiles per lender, for a total of 599 customer casefiles (once the s166 review's 109 non-DCA casefiles are also added) Covers only non-DCA agreements	Term of the agreement APR Vehicle price Loan origination details Commission structure Disclosure information – how lenders disclosed non-DCAs Consent information – did consumers *consent* to the commission structure, and how and when did they do so?	*Strengths*: Sampling methodology developed by an external statistician *Limitations*: No customer credit score information available
Credit Reference Agency (CRA)	Our data originates from a Credit Reference Agency Representative 10% sample of UK credit users whose data is captured by this CRA	Lender name Loan type (e.g. Personal loan, motor finance) Loan characteristics (origination date, amount,	*Strengths*: While not fully universal, CRA data captures lending activity across a wide range of firms and

Dataset	Coverage	Key Variables	Strengths and Limitations
	Not all lenders report to this Credit Reference Agency; hence we cannot assert that the 10% sample is fully representative of the motor finance industry	term, repayment frequency, outstanding amount by month) Customer's year of birth Credit file information across all credit products reported to the CRA (e.g. Loan details, performance)	loan types, providing broad visibility of the market The dataset is drawn as a 10% random sample of reported accounts, which reduces selection bias and ensures the sample provides a robust basis for analysis Can be aggregated to the lender level and used to cross-check figures reported in other datasets *Limitations*: Not all lenders report to each CRA, which means the dataset may omit certain segments of the market The accuracy of CRA data is contingent on the quality and completeness of submissions made by lenders The APR is not provided directly by lenders in the CRA data and must instead be derived through calculation, which introduces the potential for estimation error

Dataset	Coverage	Key Variables	Strengths and Limitations
Motor finance commission monitoring surveys	Series of information requests from May 2024 and on-going. As of October 2025, we have completed five iterations of this information request. Sample of the lenders include 37 lenders with a market share of c.95% based on outstanding lending balance as of December 2023 and 17 credit brokers covering c.50% market share based on 2021 group revenues. In early 2025, we expanded the data request to include questions on compliance costs linked to any potential future redress scheme.	5 sections included in the survey. (Note, not all firms were sent an information request including all sections below) 1. Firm details 2. Firm financial performance 3. Firm financial resilience 4. Firm complaints and claims 5. Motor finance commissions liability	*Strengths*: Good coverage of the motor finance lenders and credit brokers. Includes current and forecast financial performance, allowing FCA to conduct time series analysis. Trend analysis also conducted on other data points such as complaints volumes. Profit after tax, net assets (capital resources) and liquidity positions validated to financial accounts and regulatory returns, where possible. *Limitations*: Coverage of credit brokers is somewhat limited, compounded by extensive consolidation in the market in recent years Quality of responses varied across lenders, due to size of the business and quality of reporting systems in place.
Cost of living consumer credit data collection	A quantitative data collection covering monthly or quarterly reporting periods from	The requests sent to lenders in relation to activity carried out by the legal entity	*Strengths*: Represented over 95% of lending activity in relation to finance for

Dataset	Coverage	Key Variables	Strengths and Limitations
	September 2022 to June 2025. Sent to all medium to large lenders (around 550 lenders) who held credit agreements for personal use in 2022. Each lender received one or more templates for different types of credit agreements, including finance for motor vehicles (99 lenders – 61 full 38 core). Larger lenders reported more detailed data in our 'full' versions of the requests, while smaller lenders reported our 'core' versions of the request. Based on cleaned and mapped lending data from submissions of the CCR003 regulatory return, it was estimated that the 'full' data request was completed by lenders representing 98% of new lending and outstanding balances in relation to finance for motor vehicles intended for personal use.	associated with an FRN. The full request comprised of 7 sections: 1. Agreements outstanding and new lending Total number and balance of outstanding agreements. Total number, amount of credit, charge for credit, and average duration and APR for new agreements. 2. Motor finance Breakdown of outstanding and new agreements by product type and whether the finance was for new or used cars. Agreements in arrears 3. Debt sale and purchase	motor vehicles intended for personal use. Template explicitly related to finance for motor vehicles and not other types of credit agreement that may be offered by the lender. Almost three years of time series available for trends and consistency checks Ongoing data quality checks for reporting inconsistencies and errors. *Limitations*: Aggregate data only. No information on the types of borrowers. No information on breakdown of costs to the borrower, or commission. Only high-level data available for smaller lenders in the sample. Data quality checks did not cover all data points collected.

Dataset	Coverage	Key Variables	Strengths and Limitations
		4. Agreements with collections 5. Forbearance Operational information	
Motor Finance Lender Survey	Collected data on all 2022 and 2023 agreements from 39 motor finance lenders and/or leasing providers Data covers 1.94m new agreements in 2023 with a total value of £36.99bn We estimate the motor finance lenders included cover 89% of the lending market based on outstanding loan values in 2023 Sample reflects a range of motor finance lenders covering the new, used and sub-prime segments and includes captive, banking, and independent retail lenders	Total volume and value of written agreements Breakdown of agreements by vehicle condition, customer creditworthiness, APR band, intermediary type, commission structure, product type Average agreement APR by vehicle condition and customer creditworthiness Qualitative information on barriers to entry, plans for expansion, pricing, consumer behaviour, lender specific questions (e.g. consumer lending type, investments, intermediary involvement (if applicable)) and potential exit of lenders.	*Strengths*: Comprehensive coverage of the motor finance market Provides breakdown of agreements by various agreement characteristics allowing segment-level insights *Limitations*: Lenders may have interpreted variables differently, or may not report their data in the way we requested it (e.g. data may not align directly to our defined creditworthiness segments) There are inconsistencies in some lenders' responses between the total volume and value of agreements and the sum of volume and value when broken down by different agreement characteristics

Dataset	Coverage	Key Variables	Strengths and Limitations
			Some lenders may have included leasing agreements in their responses
Motor Finance Broker Survey	Collected data on all 2022 and 2023 agreements from 24 motor finance brokers Data covers approx. 586,000 new intermediated agreements in 2023 with a total value of £10.59 bn The brokers in our sample make up about 30% of total motor finance lending reported in our lenders' surveys The sample is focussed on large and medium-sized brokers, and a random sample of smaller brokers. A range of broker types are covered	Total volume and value of intermediated agreements Breakdown of agreements by vehicle condition, customer creditworthiness, APR band, product type, commission structure Average agreement APR by vehicle condition and customer creditworthiness Qualitative information on factors brokers have discretion over, the role of commission, how they attract customers, how they construct their lending panel, and questions to understand likely strategic behaviour in response to a redress scheme	*Strengths*: The sample should provide good coverage of intermediated business due to its focus on larger brokers Provides breakdown of agreements by various agreement characteristics allowing segment-level insights *Limitations*: The sample does not comprehensively cover the long tail of smaller brokers Brokers may have interpreted variables differently, or may not report their data in the way we requested it (e.g. data may not align directly to our defined creditworthiness segments) There are inconsistencies in some brokers' responses between the total volume and value of agreements and the sum of volume and value when broken

Dataset	Coverage	Key Variables	Strengths and Limitations
			down by different agreement characteristics

1. Loan level data (2024 Request for Information)

1.18 We received loan level information from 18 motor finance lenders in Autumn 2024. In an attempt to extend the original dataset, these are the same lenders that we requested data from in our Motor Finance Review commencing in 2017, except for one that is no longer authorised to operate, and one who did not respond to the request for information.

1.19 Using our in-house Credit Reference Agency (CRA) data, we estimate the lenders in our sample covered 61-65% of the UK motor finance market in 2018-2021. The lenders were selected to cover a significant proportion of the motor finance market and cover both mainstream lenders and lenders linked to vehicle manufacturers. They also cover prime and non-prime consumers.

1.20 The data was intended to consist of a 10% quasi-random sample[2] of each lenders' motor finance agreements with an origination date between Jan 2018 – Aug 2024.[3] In total, we received information on 677,588 agreements from lenders.

1.21 We received approximately 4,400 observations where either the broker commission or the APR were recorded as blank, and about 100 observations where either of these variables were negative, and so these were removed. Additionally, 5 lenders did not comply with the data request and only submitted data between Jan 2019 – Dec 2022. To avoid biasing the sample, we restricted each piece of analysis to the agreements that were made from Jan 2019 – Dec 2022.

1.22 In total, following cleaning, our final time-restricted dataset contains 445,470 agreements. The sample covers all motor finance agreement types, vehicle conditions, agreement commission structures, and origination channels.

1.23 Of the 445,470 agreements in the time-restricted sample, 235,228 agreements had a loan origination date prior to the FCA's January 2021 motor finance DCA ban, and 210,242 agreements had a loan origination date following the ban. Key variables (APR, brokers' commission) split over the pre-ban and post-ban period are visualised below in Figures 4 to 6.

1.24 Of the 235,228 agreements in our dataset issued prior to the ban, 109,663 (47%) were reported as having DCA models (increasing DiC, reducing DiC, and scaled commission models), 77,561 (33%) were reported as having flat fee models, 45,797 (20%)were reported as falling under an "other" commission model, and 2,207 (0.01%) were reported as falling under "no[ne]" commission model. Of the 210,242 agreements reported to have been issued from the day of the ban onwards, 77 (0.00%) were reported as having DCA models, 133,259 (63%) were reported as having flat fee models, 71,181 (34%) were reported as falling under "other" commission model, and 5,725 (3%) were reported as falling under "no[ne]" commission model.

1.25 The dataset contains variables for contract type and status, contract features, vehicle information, customer creditworthiness, origination details, and commission

[2] This sample was drawn based on specific customer dates of birth, three in each month (hence why we describe it as "quasi-random").

[3] One of the lenders submitted a ~6% random sample. In order to correct for this an ensure the sample was representative, we reweighted the data in each piece of analysis with a method appropriate for that analysis' unit of observation.

structures (broker earnings, APR recommendations, volume bonuses). A full variable list can be found in the footnotes.[4]

1.26 The credit score variable in this dataset is constructed through either a one-step (for analysis which required data up until 2022) or two-step (for analysis which only required data up until 2021) to reflect the need to make different credit score methodologies comparable. In the sample we have a measure of credit risk, however the methodology and scale for this varies, between lenders, between credit scoring agencies and between years. Further we expect different lenders to have different strategic approaches meaning some target more or less risky customers. Our process comprises of:

- A first step where we normalise credit scores within lender-agency-year subgroups. This gives consumers a value between 0 and 1 depending on which percentile of the distribution for that subgroup they are in.
- Different lenders have different approaches to risk, and their appetite may evolve over time, therefore, the normalised scores are not directly comparable between lenders and between years. However, their choice of scoring agency is random with respect to risk, so all values are comparable within lender -year subgroups.
- For the second step, we use a second source – the credit reference agency (CRA) dataset. This is a random sample taken from a credit scoring agency of all the loans they have provided credit references against. In this dataset, we filter to motor finance loans only, and to the lenders in our sample only. The version of the dataset we used only contains data up until the end of 2021, and so this second step was not conducted for our analyses that used data up until the end of 2022.
- The CRA dataset contains a uniform measure of credit score, internally calculated by the FCA, which is comparable between lenders, and across years. We map the normalised credit score from the motor finance dataset to the distribution of uniform credit score for that lender-year grouping in the CRA dataset giving us a uniform credit score in the motor finance dataset. We assume that someone with a normalised credit score in the 10th percentile of their lender-year grouping in the motor finance dataset, will have a uniform credit score comparable to someone in the 10th percentile of the same lender-year grouping in the CRA dataset, and so add this credit score to the loan recorded in the motor finance dataset. By mapping to a uniform credit score, we can then compare this within **and** between lenders and years.

2. Section 166 (s166) Customer assessment form data / Skilled person casefile review and DCA casefile review

1.27 We exercised our powers under s166 of the Financial Services and Markets Act 2000 to appoint a *Skilled Person* (in this case, a professional services firm) to collect evidence on firm practices in relation to historical motor finance commission arrangements. This *Skilled Person* provided a report under section 166(3)(b) of the *Financial Services and Markets Act, 2000.*

[4] Full list of variables collected from motor finance lenders included in the 2024 loan level data collection: Loan Status Category, Origination Channel Category, Credit Broker Name, Credit Broker FRN, Vehicle Manufacturer Name, Vehicle Condition Category, Motor Finance Product Category, Loan Origination Date, Loan Term, Deposit Amount, Balloon Payment Amount, Purchase price of vehicle (£), Discount Applied, Original Loan Principal, Annual Percentage Rate, Interest Calculation Method, Agreed Regular Payment Amount, Frequency of Repayment, Credit Score, Credit Score Bureau Category, Credit Score Date, Commission Model Category, Broker Finance Commission (£), Broker Recommended APR (%), Broker Base APR (%), Broker maximum APR (%), Broker Volume Bonus (£), Non interest charges included in total charge for credit (£), Interest charges included in total charge for credit (£), Total cost of credit (£), Broker Total Earnings (£).

1.28 The s166 review, actioned by the firm appointed under scope for *Skilled Person,* collected data on both DCA and non-DCA agreements but focused substantially on the former. Only a small number of non-DCAs were surveyed (less than 5% of the sample, around 10 casefiles per lender). The s166 Customer Assessment Form data (DCA portion) contains comprehensive consumer case file data on 3,263 agreements from 11 lenders, representing 66% of the value of the motor finance market in 2023[5]. The lenders were selected because they were the biggest lenders and/or seen to be the most impactful, and this sample is therefore not considered representative of the market.

1.29 We were advised by an external statistician to collect 150 additional customer files, sampling from 12 lenders (representing 18% of the value of the motor finance market in 2023) using a two-stage stratified random sampling. However, the final sample size achieved from this second stage was 70, which is referred to as the "DCA casefile review." Combining the DCA casefiles from s166 and this additional sample, we have a final sample size of 3,333 DCAs. We refer to this dataset as 's166 + DCA casefile review'.

1.30 A Customer Assessment Form (CAF) was designed to enable a consistent format for capturing the output of the file reviews. The CAF captures details for each agreement regarding how lenders disclosed DCAs and other data points including, for example, the terms of the agreement, details of the interest rate, commission arrangement and payment and the broker involved.

3. Data Drop 1 (DD1) / Agreement data

1.31 The DD1 dataset was constructed to provide high-level agreement data from a sample of lenders within the UK motor finance market, covering the period 6 April 2007 to 25 October 2024. Lenders were stratified based on number of outstanding loan agreements (derived from the Cost of Living data) at June 2024 or June 2023 if we did not have the most up to date data for a lender, and were allocated into 6 distinct strata ranging from the largest to the smallest providers and then randomly sampled from these strata to form the sample. In line with statistical advice, some strata were fully included, while others were partially sampled, to ensure the representativeness of the final dataset. Stratum 0 reflects all the lenders which were part of the s166 dataset.

Table 3: Stratification of lenders by number of agreements and actual DD1

Stratum	Number of agreements	Total number of Lenders	Total number of loan agree-ments	% of total Agree-ments	Number of lenders to be selected	Actual number of lenders selected
0(s166)	S166 lenders	10	3,864,541	58.3	10	10
1	Above 100,000	9	1,781,323	26.9	9	9
2	10,000 - 99,999	21	877,468	13.2	5	5
3	1000 - 9,999	22	91,284	1.4	5	5
4	100 - 999	39	8,991	0.1	5	5

[5] Based on year-end outstanding motor finance lending balances in 2023

Stratum	Number of agreements	Total number of Lenders	Total number of loan agree-ments	% of total Agree-ments	Number of lenders to be selected	Actual number of lenders selected
5	10 - 99	23	1,006	0.0	5	0
6	Up to 9	15	112	0.0	5	0
Total		139	6,624,725		44	34*

*Notes: Of the 36 lenders sampled, two failed to submit any data; therefore, the number of lenders in DD1 with valid submissions is 34.

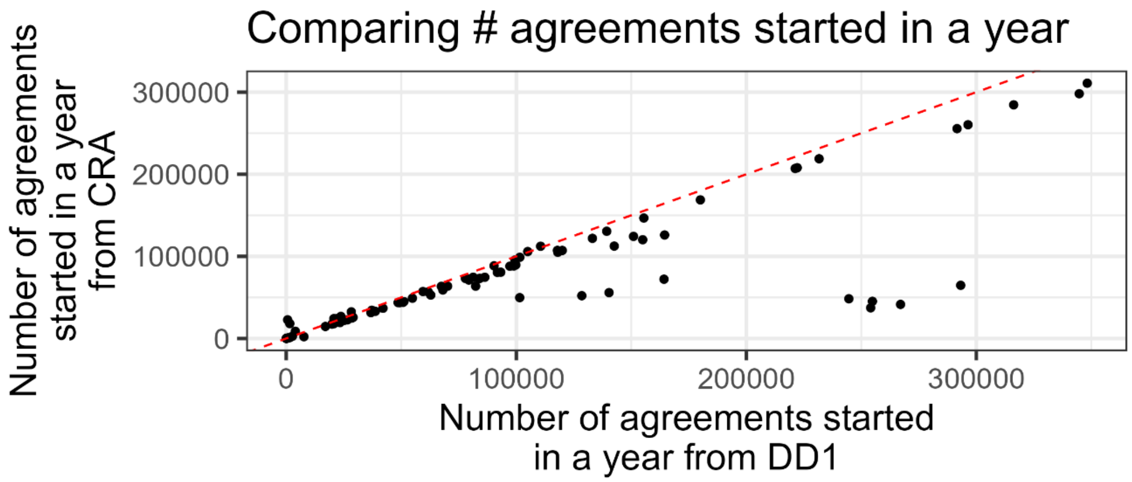
1.32 As shown in the table above, the distribution of agreements in the final sample is heavily concentrated in the largest strata. The two largest strata together account for approximately 85% of sampled agreements, while strata 3 to 6 contribute less than 2% combined. This mirrors the wider structure of the UK motor finance market, where a small number of large lenders dominate total lending volumes. At the same time, the dataset offers only limited coverage of very small lenders, which are sparsely represented or absent. This concentration has implications for how we scale our lender-level analysis and motivates the weighting adjustments applied to ensure representativeness of the UK motor finance market. The approach we took to weighting this data is detailed in the redress liability estimates.

1.33 The final DD1 dataset includes agreement-level data from 34 lenders, covering the period 6 April 2007 to 25 October 2024 and amounting to around 31 million agreements that were started during this period. Overall, this coverage represents around 89% of the regulated UK motor finance market.

1.34 As a further validation step, we cross-checked DD1 with CRA data between 2019-2023. We compared the number of agreements originated in each year from the CRA with the DD1 aggregated. Of the 34 lenders in the DD1 data, 23 could be matched to the CRA data, as different lenders report to different CRAs and our CRA dataset only contained 23 of the lenders in DD1.

1.35 We present the results visually in the figures below. The x-axis represents the number of agreements originated in each year from the DD1 dataset, and the y-axis represents the estimated number of agreements originated in each year from the CRA. Each data-point represents a lender x year combination. The red dashed line indicates the 45-degree line, which serves as a reference for perfect alignment between the two datasets. Points lying on this line suggest identical values in both sources.

Figure 1: Number of agreements started in a year



1.36 The data points are predominantly clustered around the red dashed line (correlation coefficient of 0.84), indicating general consistency between the DD1 and CRA datasets. However, observations falling below the red line represent instances where, for a given lender in a specific year, the DD1 dataset records a greater number of agreements than the CRA dataset. This reflects that the DD1 dataset includes more agreements for those lender-year combinations than are present in the CRA dataset. A possible explanation is that the lender reports to another credit reference agency and is therefore not captured in our dataset.

1.37 We also compared the total commission paid (annual) between DD1 and the 2024 loan level data. Of the 34 lenders captured in the DD1 data, 9 could be matched to the loan level data. There is a slight tendency for DD1 to have higher values relative to the loan level data for higher values of total commission paid and total number of agreements, and to be lower relative to the loan level data for lower values of total commission paid and total number of agreements.

Data cleaning

1.38 To ensure that the dataset is suitable for empirical analysis, we apply data cleaning procedures to address missing values, reformat unsuitable data observations and to maintain internal consistency across lenders and years.

1.39 As described above, DD1 is agreement level data which is used to estimate lender-level and market-wide redress liabilities. The variables used in this analysis are defined in the table below.

Table 4: Definitions of DD1 variables

Variable name	Definition
Lender name	The name of the regulated financial institution providing the motor finance agreement.
Agreement type	Categorical variable indicating whether the agreement is a DCA, non-DCA, or blank if not reported.
Date signed	The exact date on which the borrow and lender formally entered into the agreement.
Date ended	The actual end date of an agreement. In cases where the actual end date is missing, we calculate the contractual end date to fill the gap, as detailed in the table below.

Variable name	Definition
Outcome of agreement	Details how the agreement concluded (for example, ran to term, early settlement, repossession).
APR	The APR is the total cost of borrowing expressed as a yearly percentage of the amount borrowed. It includes the interest rate, and any mandatory fees or charges associated with the credit agreement, excluding contingent charges.
Product type	The type of motor finance product issued under the agreement (for example, PCP).
Loan value	The total amount of credit advanced to the consumer under the terms of the agreement.
Commission amount	The monetary value of commission paid by the lender to the broker in relation to the agreement, where applicable.
Broker name	The name of the broker involved with facilitating the agreement.

We measure all variables at the agreement level and treat missing values using the imputation methods described in the subsequent sections.

1.40 To work out how many times a customer might have been overcharged on a DD1 agreement, we need to know the dates and how long each agreement lasted. Where the 'date ended' is missing or unusable, it is calculated according to the methods outlined in the table below. Throughout the analysis, the unit of observation are individual agreements, and redress liabilities are therefore calculated at the agreement level.

1.41 The table below sets out the cleaning methodology rules applied to the agreements with missing values in one or more of the three key variables: 'Date signed', 'Date ended', and 'Contract length'. 'Date signed', 'Contract length' and 'Date ended' are observed variables in the dataset. Where one of these variables is missing, we apply a set of imputation rules to infer its value based on the available information. For example, if 'Contract length' is missing but both dates are valid, we recalculate it as ('Data ended' – 'Date signed'). Where sufficient information is not available to reconstruct the missing variable(s), agreements are either passed through but receive no redress (as detailed in the table below for those with valid date signed but not end date or contract length) or are excluded from further analysis. The final column shows the number of affected agreements.

Table 5: Imputation and cleaning rules for missing agreement dates and contract lengths

Date signed	Date ended	Contract length	Cleaning method	Number of agreements
Valid	Missing	Valid	Date ended = Date signed + Contract length	4,781,919
Missing	Valid	Valid	Date signed = Date ended – Contract length	13,874
Valid	Missing	Missing	Pass through but receive no redress	1,663
Valid	Valid	Missing	Contract length = Date ended – Date signed	4

Date signed	Date ended	Contract length	Cleaning method	Number of agreements
Missing	Valid	Missing	Agreements are removed from analysis	39,220
Missing	Missing	Valid	Agreements are removed from analysis	
Missing	Missing	Missing	Agreements are removed from analysis	0

1.42 We construct additional date- and time-related variables that served as inputs to calculations later in the modelling process. These are set out in the table below.

Table 6: Definitions of additional date- and time-related variables

Additional variable	Definition
Actual contract length	The difference between contract signed date and end date
Term until redress payment (in months from start)	The difference between the anticipated redress discretionary payment date and contract signed date
Term until redress payment (in months from end)	The difference between the anticipated redress payment date and the date of contract ends
Year signed	The year of contract signed date

Notes: The anticipated redress payment date is 31st December 2026.

1.43 Of the 1,859,219 entries in DD1 with either an empty or 'unknown' value for 'Agreement type', each should be uniquely classified as either DCA or non-DCA. To address this, we identified lenders that have never entered into DCA agreements and assigned all their missing entries as non-DCA. The remaining 1,830,847 missing values were conservatively classified as DCA meaning we will tend to overestimate the number of agreements in breach.

1.44 Where multiple versions of the same entity were identified in either of the 'Lender name' and 'Outcome of agreement' variables, manual remapping was performed to set these to the same thing. For example, two lender names of 'example lender ltd' and 'example lender limited' would be remapped to be equivalent. This remapping affected 2,949,118 agreements. Additionally, two example outcomes of agreement of 'early settlement paid off' and 'early sett.: Paid-off' would be remapped to be 'early settlement'. This remapping reduced the number of unique agreement outcomes from 15 to 7 and ensured consistency across the dataset. Whilst this affected 19,415,101 in the column, most changes were small, such as the example above.

1.45 We remove the negative values for 'loan value', 'commission amount', and 'APR', as they would affect the subsequent redress calculations. This resulted in the loss of 8 observations due to negative loan value, 152,460 observations due to negative commission amount and 97,430 observations due to negative APR values.

1.46 Furthermore, we observe that the value zero appeared 15 times in the 'loan value', 6,847,435 times in the 'commission amount' and 2,284,975 times in the 'APR'. These agreements with 0s were included in the calculations of number of agreements but excluded from the calculation of lender-year redress. Consequently, they contribute to the total number of agreements in a given lender, year split, but do not contribute to the associated lender, year redress averages.

1.47 We address missing values in variables including 'loan value', 'commission amount', and 'APR', using a partition averaging approach. Specifically, where information is not available for a given variable (for example, 'commission amount'), the missing values are imputed using a summary statistic (for example, mean, median, minimum, maximum) calculated over progressively broader partitions of the dataset:

- Group the data into lender, year (of agreement) partitions.
- Calculate the chosen summary statistic (e.g. mean, median, minimum) over a given variable using the available valid data in that lender, year grouping.
- Fill in the missing entries in that variable within the same lender, year grouping with the grouping specific value of the summary statistic calculated in step 2.
- If there are still missing entries in that variable, repeat steps 1 to 3 inclusive, but instead of grouping the data by lender and year, group only by lender.
- If there are still missing entries in that variable, repeat steps 1 to 3 inclusive, but instead of grouping the data by lender and year, group only by year.
- If there are still missing entries in that variable, the summary statistic applied to the whole dataset is used to fill in remaining gaps.

1.48 The above partition approach is applied in DD1 to impute 107,119 'loan value' values, 402,403 'commission amount' values and 620,494 'APR' values. In each of these cases, the median was the chosen summary statistic. This was chosen as opposed to the mean so that our analysis would be more robust to outliers and the skewed nature of the underlying distributions.

1.49 The table below summarises the data cleaning procedures applied to key variables used in the analysis. For each variable, we report the number of affected observations, the method used to address data issues, the number of values imputed, and the number of observations removed.

1.50 This imputation strategy offers several advantages. Most importantly, it preserves sample size – particularly the count of total number of agreements – which is critical for enabling subsequent analysis. However, it may also introduce a degree of smoothing in the data, potentially attenuating lender -level heterogeneity or temporal variation. Such smoothing could bias estimates toward more conservative values, especially in the presence of non-random missing variables missingness. Despite these limitations, the use of lender-specific information and consistent imputation rules helps preserve meaningful variation while minimising distortion. This supports credible inference without compromising sample integrity.

Table 7: cleaning procedures

Variable name	Number of affected observations	Approach to cleaning	Number of imputed values	Number of observations dropped
Lender name	2,949,118	Manual remapping	2,949,118	
Agreement type	1,859,219	Identify lenders that have never	1,859,219	

Variable name	Number of affected observations	Approach to cleaning	Number of imputed values	Number of observations dropped
		entered into DCA agreements and assign all their missing entries as non-DCA		
Outcome of agreement	19,415,101	Manual remapping	19,415,101	
APR - missing values	620,494	Partition approach	620,494	
APR – negative values	97,430	Remove the negative values		97,430
Loan value – missing values	107,119	Partition approach	107,119	
Loan value – negative values	8	Remove the negative values		8
Commission amount – missing values	402,403	Partition approach	402,403	
Commission amount – negative values	152,460	Remove the negative values		152,460

Notes: please refer to Table 5 for details of the method used to address missing date and time related variables.

1.51 The original DD1 dataset included 31,013,940 agreements in total. The adjustments we made set out above as part of the data cleaning process mean we are left with a data set comprised of 30,724,822 agreements.

1.52 To calculate lender level liability numbers, we make the following calculations:

- GMFV
    - During a PCP contract consumers pay for the difference between the car's purchase price and its GMFV, plus interest.
    - We do not observe this in DD1 and we need it to calculate monthly payments. We use the s166+DCA casefile review data and DD2 sample data to estimate the relationship between GMFV and loan value and contract length using a linear regression $+ \beta \cdot CONTRACTLENGTH + \gamma$, giving coefficients of $\alpha = 0.6394$, $\beta = -267.1$, $\gamma = 9315$. We use the coefficients to estimate GMFV values in the DD1 dataset.

Table 8: GMFV Estimation

	1.4 GMFV (if applicable)
(Intercept)	9314.66 ***
	(628.14)

1.6 Term of loan	-267.10 ***
	(13.57)
2.9 Total Credit Value (£)	0.64 ***
	(0.01)
R2	0.88
Adj. R2	0.88
Num. obs.	1078

- Total cost of credit (TCC)
  - For agreements that are not a Personal Contract Purchase (PCP), it is calculated by:
    - Calculating the monthly rate for each agreement
      $$r = (1 + APR)^{1/12} - 1$$
    - Calculating monthly payments based on the monthly rate, contract length, and the value of the loan:
      $$monthly\ payment = loan\ value \times \frac{r(1+r)^n}{(1+r)^n - 1}$$
    - Aggregating this to payments over the total term of the agreement by multiplying by the original contract length:
      $$total\ payments = monthly\ payment \times agreement\ term$$
    - Calculating the total interest cost by subtracting the loan value from the total payments:
      $$TCC = total\ payment - loan\ value$$
  - For Personal Contract Purchase (PCP) agreements, it is estimated by:
    - Calculating the monthly rate of each agreement as above
    - Calculating monthly payments based on the monthly rate, contract length, value of the loan and an estimated balloon payment (GMFV value). This GMFV value is estimated using the regression above.

$$\widehat{GMFV} = 9314.66 + -267.1 \times loan\ term + 0.64 \times loan\ value$$

$$monthly\ payment = \frac{r(loan\ value - \frac{\widehat{GMFV}}{(1+r)^n})}{1-(1+r)^{-n}}$$

    - Aggregating this to payments over the total term of the agreement by multiplying by the original contract length
      $$monthly\ payment \times agreement\ term$$
    - Calculating the total interest cost by subtracting the loan value from the sum of total payments including the balloon payment
      $$TCC = total\ payments + \widehat{GMFV} - loan\ value$$

Table 9: Definitions of constructed variables

Additional variable	Definition
GMFV	The pre-agreed residual value of the vehicle at the end of the finance term, which the borrower may use to settle the final payment under a PCP agreement.
TCC	The true cost to the borrower of the motor finance agreement. It reflects the

Additional variable	Definition
	total financial burden associated with the credit, including interest and fees.

DD1 Sensitivity analysis

1.53 Below we set out the sensitivity analysis we have undertaken to check the robustness of the various assumptions we have made.

1.54 We first look at the robustness of the assumptions made in relation to missing values and then at the possible impacts of the randomisation employed.

Missing loan value, commission amount and APR

1.55 There are around 100,000 missing values in 'loan value', 400,000 missing values in 'commission amount', and 620,000 missing values in 'APR' within the DD1 agreement-level dataset. In each case, we impute the missing values using the median calculated over partitions: first by lender and year, then by lender only, and finally by year only.

1.56 We tested the sensitivity of the redress estimates model to these assumptions by:

- Replacing the median over partitions with the 95th percentile (rather than the maximum, as outliers would skew the results).
- Replacing the median over partitions with zero in all cases.

1.57 The first test resulted in a 4% increase in the total redress liabilities estimate, while the second test produced a 2% decrease. These changes were mainly driven by shifts in UR1 and UR2 (high commission cases) redress liabilities, alongside a smaller increase in UR3 (undisclosed tied arrangement) redress liabilities in the first test.

1.58 Overall, the differences observed are small relative to the total and provide confidence that the initial approach of imputing missing values with the median over partitions is robust.

Agreement End Date

1.59 There are around 4.8 million observations (15% of the total) missing 'date ended' information. To address this, we assume that **all these agreements run to term and do not finish early**, despite the diagnostic report shows that 63% settle before term. Under this assumption, rebates are not calculated for agreements that would otherwise end early. As a result, **the liability estimates produced under the loss-based redress approach are likely to be overstated**.

1.60 We tested the sensitivity of this assumption in two ways:

- We calculated the average realised contract term (not the initially agreed contract term) for each lender. This average was then added to the start date of agreements missing an end date, to calculate a likely end date. For instance, if a lender's agreements run for two years on average, we imputed missing end dates by adding two years to the given start date for agreements for that given lender.
- We calculated the average realised contract term (not the initially agreed contract term) at both the lender level, and by loan length (rounded to the nearest year). This provides a more granular estimate; for instance, if two-year loans for a lender end after one year on average, then missing end dates for similar loans were imputed by adding a year to the start date for two-year agreements within this lender.

1.61 The impact of the first test is a 1% decrease to the total redress liabilities estimate, and the second test produced a 2% decrease. In both cases, the reduction is driven by a reduction in total UR2 and UR3 redress liabilities. These changes are modest in scale and reinforce confidence that our assumption, that agreements run to term, is a reasonable and robust basis for the analysis.

4. Data Drop 2 (DD2) / non-DCA casefile review

1.62 Following the 25 October 2024 Court of Appeal Judgment (CoA), in January 2025 we widened our work (building on the s166 DCA dataset above) to collect a sample of agreement level data from 36 lenders of non-discretionary arrangements (non-DCA) agreements, with this sample again constructed in line with advice by an external statistician. The statistician found that the final DD2 sample adequately reflects the population.

1.63 Based on the sample design advice received from the external statistician, for DD2 we sampled from 36 lenders, who covered 89% of all agreements across the motor finance sector for the period 6 April 2007 to 25 October 2024.

1.64 The number of casefiles reviewed per lender depended on the stratum ('band') the lender fell within (with stratums arranged by size of lender), with the largest lenders completing 15 casefile reviews and the smallest lenders 3. We then also utilised the 10 non-DCA casefiles per lender that had been provided as part of the s166 (11 lenders in the DD2 catchment of 36 lenders) as part of this review, taking the s166 lenders' sample to 25 casefiles per lender. A total of 599 casefiles were sampled[6] from the 36 lenders[7].

1.65 The sampling methodology weighted the sample to larger lenders but did not exclude smaller lenders. The number of very small lenders included in the sample was limited (i.e. lenders with fewer than 100 outstanding agreements in June 2024) as our review highlighted that few lenders of that size paid commission to brokers across the relevant period.

1.66 For the DD2 casefile reviews, a second Customer Assessment Form (CAF) was designed to enable a consistent format for capturing the output. The design of the DD2 CAF was informed by the CoA judgement in October 2024. Further, as there were distinct CAFs for the s166 review (DCAs) and DD2 non-DCA review, the two sets of information on customers are not directly comparable.

1.67 In particular, the DD2 CAF contains similar data to the s166 review CAF, including how lenders disclosed commission and other data points including, for example, the terms of the agreement, details of the interest rate, commission arrangement and payment and the broker involved. However, aligning with the outcome from the CoA judgement, the DD2 CAF collected additional information on whether customers *consented* to commission payments including when and how this consent was given.

Section 166 Review, DCA casefile review and DD2 Combined sample data

1.68 For our redress liability estimation, we created a combined dataset of the detailed DCA and non-DCA sample data in S166+DCA casefile review data and DD2. Below we set out the steps we took create and clean this data. The combined Section 166 Review and DD2 data discussed below follow from previous sections detailing the sampling, coverage, and representativeness of the individual datasets.

[6] One casefile was subsequently deleted when it was found to be a cancelled agreement with no commission paid out

[7] 2 lenders merged after January 2021 and were treated as separate entities for the purpose of this work

1.69 A variety of variables in the s166+DCA casefile review data and DD2 data required imputation and reformatting. Given that both the s166+DCA casefile review data and DD2 datasets are sample datasets, they are combined. In this section they will collectively be referred to as 'sample data'. Prior to appending the two datasets, equivalent variables across the s166+DCA casefile review data and DD2 datasets were identified (as detailed in the table below) and their variable names aligned to ensure structural consistency.

Table 10: Mapping names of equivalent variables across s166+DCA casefile review data and DD2 Datasets

Variable description	s166+DCA casefile review data column reference	DD2 column reference	Notes on equivalence/use
Annual Percentage Rate (APR) of agreement (%)	APR % of agreement (%)	APR % of Agreement Fixed	Variable names standardised to DD2 column reference
Evidence of commission payment from lender to broker	Was a commission payment paid by the lender to the broker?	Is there evidence on file that a commission payment was paid by the lender to the broker?	Variables given the same name to capture the presence/absence of commission
Guaranteed Minimum Future Value (GMFV) (if applicable)	If the contract was a PCP, please state the GMFV.	GMFV (if applicable)	This variable is only relevant for PCP contracts
Total Credit Value (£)	Total credit value (£)	Total Credit Value (£)	Equivalent

1.70 Having made the combined sample dataset, the following cleaning steps were required:

- In the sample data, we imputed 3 missing observations in the 'APR' variable with the partitions approach described above, using the median as the summary statistic.
- In the 3333 DCA agreements in the sample data, 1807 values were missing in the 'minimum APR' variable (called 'Minimum APR that could have been charged for the transaction (%)'). We replaced these missing values with 0. This impacts the assignments of the unfair relationship related to DCAs, albeit to a minor extent due to the other constraints we can place on UR1.
- We filled in 2,697 missing observations in the 'GMFV' variable with 0.
- There are 4 missing 'contractual end dates' in the sample data which we have filled with the addition of 'start date' and 'term of loan'.
- There are 7 missing 'start dates' in the sample data which we have filled with the subtraction of 'loan term' from the 'contractual end dates'.
- There are 284 missing 'observed end dates' which we have filled in with the addition of 'start date' and 'loan term'.

- There are 28 missing 'commission amounts' in the sample data which we have filled in with the average over partitions approach, using the median as the summary statistic.
- There are 363 missing values in the 'Non interest charges included in the total charge for credit' variable. We filled in the missing values with the average over partitions approach, using the median as the summary statistic.
- There are 233 missing values in the 'Interest charges included in the total charge for credit' variable. We filled in the missing values with the average over partitions approach, using the median as the summary statistic.

Table 11: Summary of data cleaning procedures for key variables

Variable name	Number of affected observations	Approach to cleaning	Number of imputed values	Number of observations dropped
APR	3	Partition approach	3	0
Minimum APR	1807	Replaced with 0	1807	0
Guaranteed Minimum Future Value (GMFV)	2697	Replaced with 0	2697	0
Contractual end dates	4	Start date + Term of loan	4	0
Observed end dates	284	Contractual end ending date – Term of loan	284	0
Commission amounts	28	Partition approach	28	0
Non interest charges included in the total charge for credit	363	Partition approach	363	0
Interest charges included in the total charge for credit	233	Partition approach	233	0

Notes: The partition approach is discussed in detail above

Section 166 Review and DD2 Combined sample data Sensitivity tests

Minimum APR

1.71 There are 1,807 instances within the sample data (i.e. S166 + DCA casefile review data, based on the review of approximately 3,400 DCA case files) where the 'minimum APR' that could have been charged for a transaction is missing. We have imputed these missing values with zero. This assumption impacts the allocation of cases to UR1 (undisclosed DCA arrangements), although the effect is limited given the additional constraints applied to UR1.

1.72 To test the robustness of this approach, we constructed two sensitivity checks:

- We replaced the zero value with a 'minimum APR' observed for a selected lender and assessed the impact. This produced no change in the key headline redress liability figures.
- We replaced the zero with the 'minimum APR' observed across different partitions (by lender and year, then by lender only, and finally by year only). This led to a 1% decrease in the total redress figure, driven by a reduction in UR1 redress liabilities.

1.73 Taken together, these tests provide confidence that imputing missing 'minimum APR' values with zero is a reasonable and robust assumption. The alternative methods of substitution yield only marginal differences in the overall results.

5. Credit reference agency data

1.74 The Credit Reference Agency (CRA) data provides data on UK consumer credit files. This dataset includes detailed credit history and financial behaviours for a sample of UK credit users.

1.75 The data used in this analysis originates from a Credit Reference Agency (CRA). The dataset consists of a representative 10% sample of UK credit users whose data is captured by this CRA. Not all lenders report to this CRA, hence we cannot assert that the 10% sample is fully representative of the motor finance industry. The dataset also includes data on associated individuals (like joint mortgage holders). The data includes complete credit files for each person, covering all credit items reported to the CRA. We used data covering the period of 2018-2024 (1,037,111 motor finance agreements in total).

1.76 The key variables contained in the dataset and used in our analysis include lender name, loan type (e.g. personal loan, motor finance), origination date, origination amount, loan term, year of birth, repayment frequency, and the outstanding amount by month, all at the loan level. We also aggregate the dataset to the lender level to cross-check the number of agreements per year across different datasets.

1.77 While a measure of credit risk is not provided directly in the CRA data, the FCA internally constructs a measure of credit risk using the other variables reported. This modelled credit risk score was used in the normalisation process for the credit risk variable listed above in the Loan Level data section.

6. Motor finance commission monitoring surveys

1.78 On 12 April 2024 we issued a Dear CEO letter to motor finance lenders and credit brokers reminding firms to maintain adequate financial resources and plan for additional operational costs from increased complaints and, where applicable, to meet the costs of resolving those complaints. In that letter we also notified firms of an upcoming additional data collection monitoring financial resources of firms and DCA-related complaints volumes. We began collecting data on a quarterly basis from May 2024.

1.79 This data was targeted at supporting our assessment of firms' financial resilience. We also use this data to monitor customer complaints and how firms are handling them; and identify early signs of business model changes or corporate actions in response to our intervention.

1.80 In early 2025, we expanded the data request to include questions on compliance costs linked to any future redress scheme. The sample coverage included 37 lenders with approximately 95% market share based on outstanding motor finance lending balances as of December 2023 and 17 large credit brokers with approximately 50% market share based on 2021 group revenues.

Specifically, in August 2025 (Round 5) we asked firms within the lender sample:

a) Following the recent Supreme Court decision, what is the estimated **one-off investment** your firm expects to make in systems, capital, or infrastructure specifically to manage a potential consumer redress scheme? (£)

b) Following the recent Supreme Court decision, what is your firm's **estimated average time** to process a single complaint within a potential consumer redress scheme, from **initial receipt to final resolution**? (minutes)

c) Following the recent Supreme Court decision, how many **FTE staff** does your firm expect to allocate to handling complaints for a potential consumer redress scheme? (FTE)

d) Please specify the type of staff resource(s) involved in your response to [the FTE question above]. For example, junior call handlers, lawyers, etc.

e) Following the recent Supreme Court decision, what is the **average total hourly cost per staff member** assigned to complaint handling involved in a potential consumer redress scheme? (£) Please provide estimates **excluding overheads** such as national insurance, allowances and office costs.

f) Please list any **factors** which may **increase** or **decrease** the **estimates** provided in [the questions above].

g) Of the complaints in scope of a potential consumer redress scheme, what **proportion** do you anticipate **requiring information** from a **broker** or **intermediary** in order to assess? (%)

h) Has your organisation consistently and systematically retained credit agreement records that include **identifiable personal data** of **borrowers** (e.g. name, date of birth) from April 2007 onwards? If not, what is the **earliest date** from which consistently and systematically **retained records** remain accessible? Consistent and systematic retention refers to the routine, policy-driven storage of records as part of your organisation's standard data management practices. It does not include isolated or incidental retention of individual agreements outside of a structured retention process.

i) What **proportion** of motor finance complaints **received** so far are from **Professional Representatives** (i.e., claims management companies and SRA-regulated law firms)? (%)

j) How, if at all, do you expect the **proportion** of **complaints** in scope of a potential consumer redress scheme **received** from **Professional Representatives** to **change** when complaints submissions resume?

k) Did your firm ever operate a **commercial tie** with a first right of refusal with any of your credit brokers, since April 2007? If yes, please **state the years** you had this in operation. If no, please state No.

1.81 A similar set of questions split by DCA and non-DCA questions was asked in the Round 4 survey in May 2025, prior to the August 2025 Supreme Court ruling, which did not reference any potential consumer redress scheme. For example, the equivalent question to b) above for non-DCA complaints was:

- On average, how many minutes do you expect your staff to spend processing a non-DCA complaint from initial receipt to resolution? (minutes)

7. Cost of living consumer credit data collection

1.82 The CCR003 Consumer Credit data: Lenders regulatory return is reported by all firms who hold permission to carry out consumer credit lending. The return includes aggregate information about each firm's regulated credit agreement book at the end of a reporting period, and the value of new advances made during the reporting period. The lending activity reported in CCR003 submissions for annual reporting periods ending between 1 July 2021 to 30 June 2022 were assigned to certain products, including finance for motor vehicles. Any firm identified as reporting more than £1m in their regulated credit agreement book or more than £1m in new advances in relation to finance for motor vehicles, and this lending was not solely or predominantly for business use, was included in the sample of firms sent the finance for motor vehicles template of the Cost of Living (CoL) Consumer Credit Data Collection. We estimate that these firms represented more than 99% of finance for motor vehicles for personal use during 2022.

1.83 The CoL dataset, which was collected between September 2022 and June 2025, provided data on firms' credit activities so that we could monitor and assess the impact of the rising cost of living on consumers. Larger firms reported more detailed data in our 'full' versions of the data request, while smaller firms reported our 'core' versions of the data request. The requests were sent to firms in relation to activity carried out by the legal entity associated with an FRN. For our purposes, we use the following information:

- Firm name and unique identifier: each reporting firm is uniquely identified;
- The number of outstanding loan agreements by volume for 2024;
- A more detailed breakdown of outstanding agreements by product type (volume).

1.84 This information was used to stratify firms by size in the DD1 sampling design. Before stratification, the CoL dataset included 93 firms, covering 6.6 million outstanding agreements as of June 2024.

1.85 To validate the number of outstanding agreements from the CoL data, we cross-checked it against the credit reference agency (CRA) dataset. Of the lenders in the CoL data, 54 could be matched to the CRA data. As discussed, different lenders may report to different credit reference agencies, and as such the CRA dataset we have access to is limited in its coverage of lenders.

1.86 We present the results visually in the figures below. The x-axis represents the number of outstanding agreements from the CoL dataset, and the y-axis represents the estimated number of outstanding agreements from the CRA. The red dashed line indicates the 45-degree line, which serves as a reference for perfect alignment between the two datasets. Points lying on this line suggest identical values in both sources.

1.87 The points are closely clustered around the red dashed line (correlation of 0.87 for 2023, 0.86 for 2024), which provides confidence that the CoL dataset accurately captures the number of outstanding agreements by each motor finance lender. Hence, we are confident that the lenders have been appropriately divided into strata based on the CoL data.

Figure 2: Number of outstanding agreements by dataset 2023

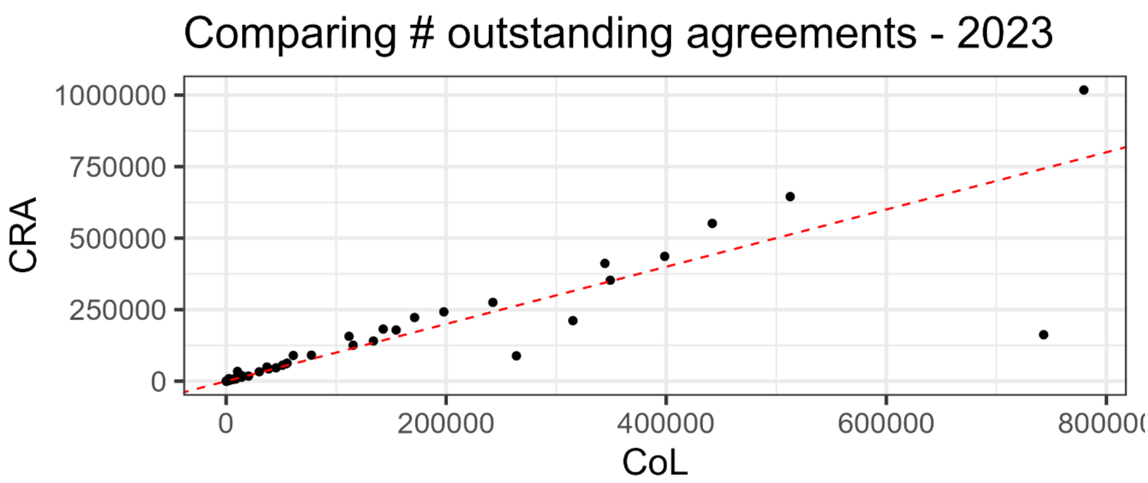
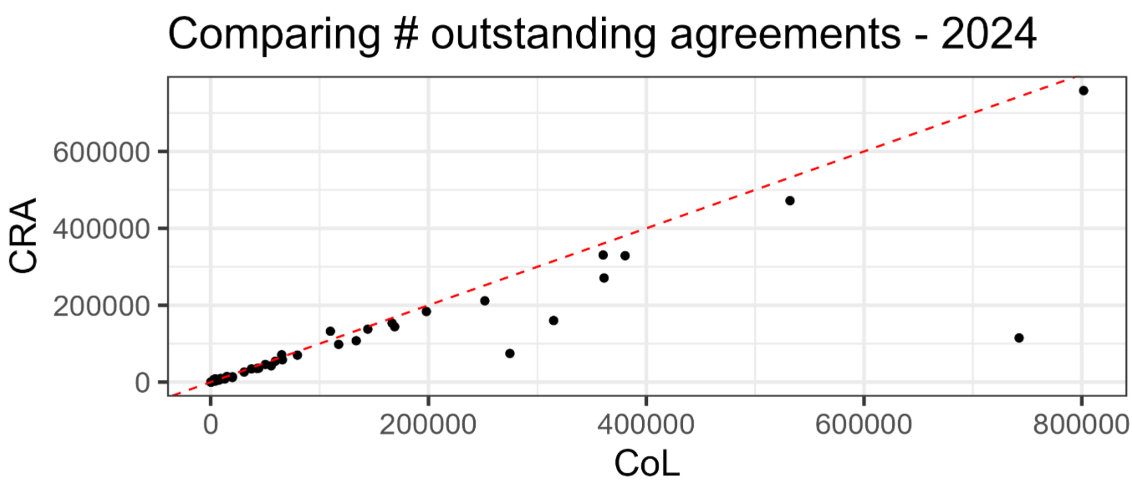


Figure 3: Number of outstanding agreements by dataset 2024



8. Motor Finance Lender Survey

1.88 We requested information from a sample of motor finance lenders in September 2024. The request included qualitative and quantitative elements to inform our understanding of how the market for motor finance works.

1.89 The survey captured information such as agreement characteristics (e.g. value, volume, APR, creditworthiness, product type, condition) for new agreements entered into in 2022 and 2023[8], and qualitative questions to understand the barriers to entry, plans for expansion, pricing, consumer behaviour, lender specific questions (e.g. consumer lending type, investments, intermediary involvement (if applicable)) and potential exit of lenders.

1.90 Sampling for the lenders survey again stratified lenders into 6 strata based on number of agreements outstanding in June 2023, described in the table below. All

[8] Full list of variables collected from motor finance lenders in the Motor Finance Lender Survey: total lending in 2022/2023 (number and value of agreements by year, minimum and maximum agreement values, motor finance revenue), total lending by product, agreement, and consumer characteristics (product type, vehicle condition, customer creditworthiness segmentation), pricing (APR means and ranges by vehicle condition and customer creditworthiness segmentation), total lending by APR band, intermediary relationships (number and type of brokers, top 10 brokers by number and value of agreements), commission arrangements (total commission and number of agreements by commission model, average and maximum commission per agreement as a proportion of loan by commission model).

lenders in stratum 1 and stratum 2 were included in the sample, and 5 lenders from each of the remaining stratum were randomly selected. This resulted in an initial sample of 59 lenders. However, a small number of lenders were excluded from the sample because although some lenders were part of the same wider group, their activities were out of scope for this analysis, or they had ceased operations prior to the data collection period. This sampling approach was designed to obtain a representative view of how the market was currently operating, rather than considering historic market participation.

Table 12: Stratification of lenders by number of agreements and actual Motor Finance Lender Survey Sample

Stratum	Number of agree-ments	Total number of lenders	Total number of loan agree-ments	% of total agree-ments	Number of lenders to be selected	Actual number of lenders selected
1	Above 100,000	17	5,283,019	85.6%	17	17
2	10,000 – 99,999	22	750,260	12.2%	22	20
3	1,000 – 9,999	30	124,939	2.0%	5	4
4	100 – 999	41	14,426	0.2%	5	6
5	10 – 99	44	1,790	0.0%	5	3
6	Up to 9	39	164	0.0%	5	5
Total		193	6,174,598		59	54

1.91 Thirty-nine lenders offering motor finance or an alternative product such as leasing responded to the survey. The respondents included 11 'captive' lenders that are part of a car manufacturing group, 13 lenders that are part of banking group, and 15 independent retail lenders. Nine of the independent retail lenders were also part of a group structure, with 6 operating as sole entities. Two thirds of the respondents specialise in motor finance only. Two respondents are vehicle leasing specialists and do not provide traditional motor finance.

1.92 The respondents to the survey held approximately 89% market share based on outstanding motor finance lending balances as of December 2023 as reported in the CoL dataset, and approximately 86 - 88% based on the number and value of new motor finance agreements in 2023.

9. Motor Finance Broker Survey

1.93 We also requested information from a sample of motor finance brokers in September 2024.

1.94 This captured similar information to the lender survey such as agreement characteristics for new agreements intermediated in 2022 and 2023.[9] It also encompassed qualitative questions to understand which factors brokers have discretion over, the role of commission, how brokers attract customers, how they construct their lending panel, and to understand potential responses of brokers to the impact of a redress scheme.

1.95 To sample brokers, we identified the top 20 brokers by regulated credit broking revenue and the top 20 brokers by number of regulated credit broking transactions in 2023 from supervisory data. Additional brokers were added based on a random sample of 20 firms with over £1m in revenue and market knowledge to obtain a reasonable representation of the market for assessing competition and market impacts. In total, we requested information from 50 brokers, which represented about 81% of regulated credit broking transactions in 2023, of which 48% (24) provided responses in a format we could process for our analysis. The brokers who responded to our survey make up about 29-30% of total motor finance lending reported in our lenders' surveys.

1.96 The sample reflects a range of motor finance brokers covering the new, used and sub-prime segments. The sampling approach captures the different types of firms that act as brokers in the sale of motor finance and the biggest players by revenue and volume of transactions.

1.97 The brokers that responded included 5 franchised motor dealers, 3 independent motor dealers, 12 finance brokers, 1 Original Equipment Manufacturer (OEM), 2 online platform / software providers and a principal firm with appointed representatives that previously conducted motor finance credit broking.

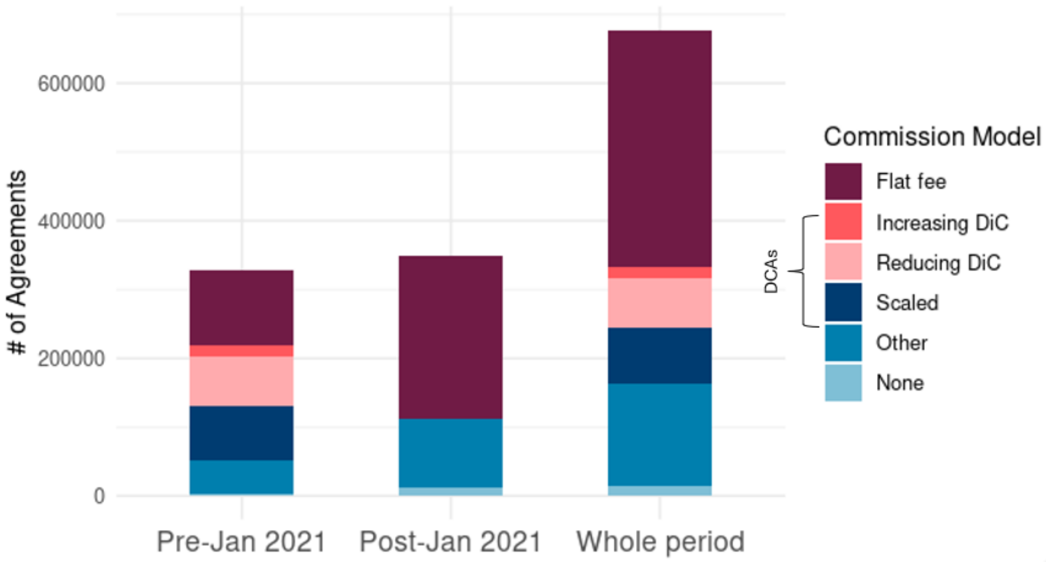
[9] Full list of variables collected from motor finance brokers in the Motor Finance Broker Survey: business model (channels used, physical site locations by region), total intermediation in 2022/2023 (number and value of agreements by year, minimum and maximum agreement values, motor finance revenue), total intermediation by product, agreement, and consumer characteristics (product type, vehicle condition, customer creditworthiness segmentation), pricing (APR means and ranges by vehicle condition and customer creditworthiness segmentation), total intermediation by APR band, lender relationships (number and type of lenders, top 10 lenders by number and value of agreements), commission arrangements (total commission and number of agreements by commission model, average and maximum commission per agreement as a proportion of loan by commission model).

Descriptive Statistics

1.98 The following charts provide descriptive statistics from the 2024 loan level dataset's time-restricted sample (covering January 1 2019 – December 31 2022), containing 445,470 agreements across all commission models.[10]

1.99 Of the 445,470 agreements in the time-restricted sample, 235,228 agreements had a loan origination date prior to the FCA's January 2021 motor finance DCA ban, and 210,242 agreements had a loan origination date following the ban.

Figure 4: Agreements by commission model, 2019-2022

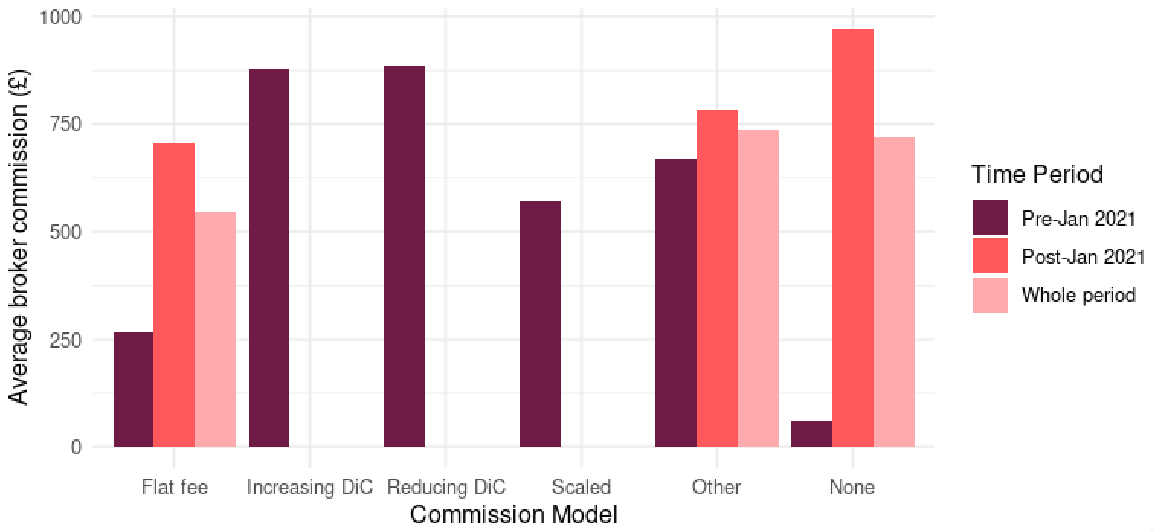


Source: Loan level data (collected in autumn 2024), time-restricted sample (January 1 2019 to December 31 2022)

1.100 The figure above shows that for the loans in our time-restricted sample issued prior to the DCA ban (pre-January 2021), 47% had DCA models (increasing DiC, reducing DiC, and scaled), and 33% were reported as having flat fee models. The remaining pre-ban loans in the sample were reported as "other" (20%) and "none" (0.01%). Following the ban, 63% of agreements in our sample were reported as having flat fee models (34% were reported as "other", and 3% as "none"). Details on the treatment of the loans reported under "other" and "none" in the loan level data can be found below.

[10] These agreements were quasi-randomly sampled from 18 firms that are estimated to cover 61-65% of the motor finance market share (as of 2018-2021).

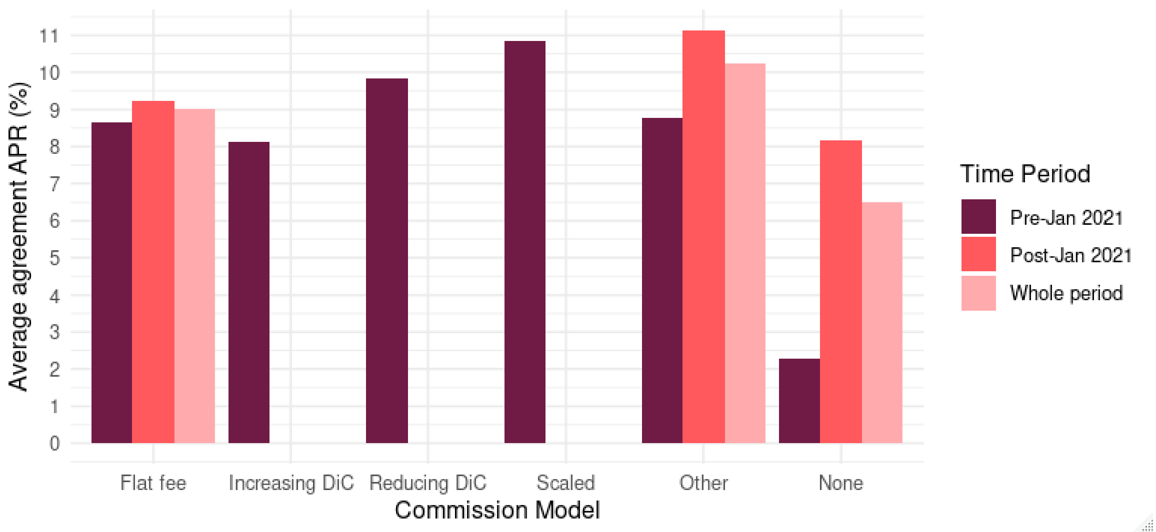
Figure 5: Average broker commission (£) by commission model, per agreement, 2019-2022



Source: Loan level data (collected in autumn 2024), time-restricted sample (January 1 2019 to December 31 2022)

1.101 The figure above shows that brokers were likely to earn higher commission from DCA agreements than non-DCA arrangements in the pre-ban period. Prior to the DCA ban, the average broker commission for a DCA agreement was £747 (weighted average of increasing DiC, reducing DiC, and scaled commission models), and the average broker commission for a flat fee agreement was £268.[11] Following the DCA ban, the average broker commission for a flat fee agreement increased to £706, which could be due to the compositional effects of the market transitioning from mostly DCA agreements to mostly flat fee agreements (see the figure below).

Figure 6: Average APR (%) by commission model, per agreement, 2019-2022



Source: Loan level data (collected in autumn 2024), time-restricted sample (January 1 2019 to December 31 2022)

[11] The averages (means) provided in this figure are calculated inclusive of agreements with £0 reported commission.

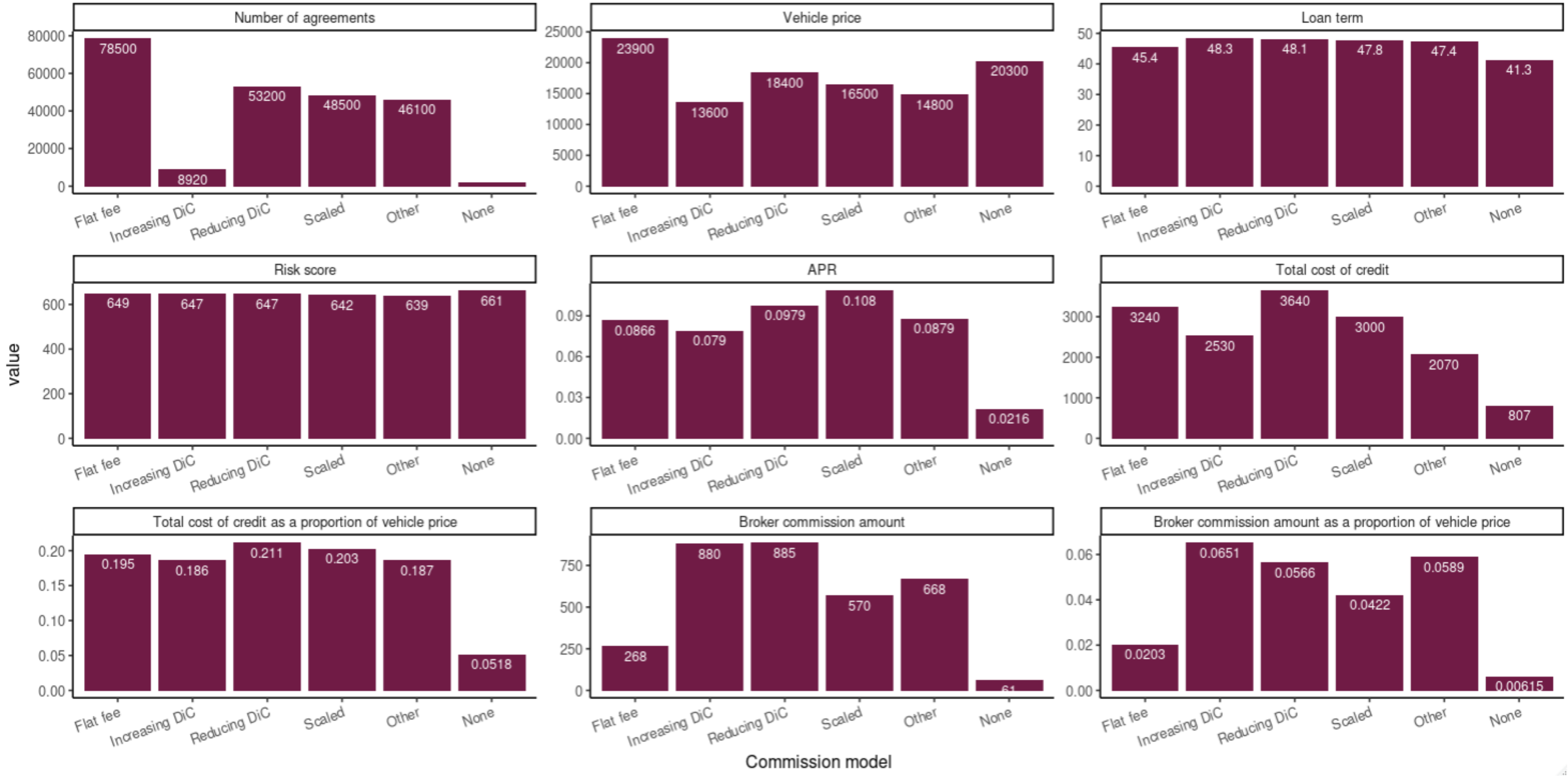
1.102 The figure above shows that average APR (a measure of interest costs and fees paid by the consumer) was generally higher for DCA agreements than for flat fee agreements. Prior to the DCA ban, , in this dataset the average APR for a DCA agreement was 10.1% (weighted average of increasing DiC, reducing DiC, and scaled commission models), and the average APR for a flat fee agreement was 8.7%. Following the DCA ban, the average APR for a flat fee agreement in our sample increased to 9.2%.[12]

1.103 The figure below shows how key characteristics of loans differ across commission model types.

- Most agreements were flat fee loans. With respect to other loans, there are a similar number of reducing DiC (n=53,161) and Scaled (n=48,451) loans, however only 8,915 increasing DiC loans were made. From around 2015 onwards, lenders moved away from increasing DiC models following our engagement in the market.
- Flat fee loans were made on more expensive cars. This is because most new cars are sold through flat fee loans.
- DCAs tend to have slightly longer loan terms.
- Average credit score is similar across all commission models.
- Reducing DiC and scaled loans have higher APR than flat fee loans, increasing DiC do not. All DCA models except for increasing DiC have a higher total cost of credit as a proportion of the vehicle price.
- All DCA models have a significantly higher broker commission amount than flat fee models, though the difference between flat fee and scaled is the smallest.

[12] The averages presented in the "Descriptive Statistics" section are representative only of one dataset – the "Loan Level Data" listed first in the Data Guide. The same statistics (average agreement APR, average commission) may be different when calculated using different samples / datasets described in the Data Guide.

Figure 7: Key features by commission model type



1.104 In our analysis we primarily focus on flat fee and DCA agreements for the following reasons:

- "None" represents a very small number of loans (see Table 13 below). More than half have negative, zero or missing value for commission.
- "Flat fee" is the most common type of non-DCA commission structure and is a well-defined group with a consistent commission structure
- "Other" captures a variety of what we assume are non-DCA commission models that are not flat fee. We do not hold information on the exact commission structures for loans in this category which makes it challenging to assess the incentives faced by brokers. In addition, it is possible some lenders may have included DCA agreements in this category.

1.105 However, as a robustness check, we include "None" and "Other" agreements in the analysis of the 2021 DCA ban.

Table 13: Loan level data summary statistics

Variable Name	Number of non-missing observations	Proportion of sample (%)	Minimum	Maximum	Mean	Median	Standard deviation
Agreed Regular Payment Amount	677,209		0	11,440	296	261	186
Original Loan Principal	677,588		695	364,500	17,002	14,593	13,033
Purchase price of vehicle (£)	677,138		-7,328	660,000	20,911	17,737	16,456
Annual Percentage Rate	677,227		-0.01	1.41	0.10	0.09	0.07
Interest charges included in total charge for credit (£)	597,174		-459	89118	3,745	3,098	3,312
Non-interest charges included in total charge for credit (£)	479,769		-9,717	1,258	22	1	90
Total cost of credit (£)[13]	677,131		-7,681	89,118	3,735	3,105	3,264
Loan Term in months	677,570		1	120	47	48	10
Balloon Payment Amount (£)	628,910		0	359,999	6,989	5,580	9,290

[13] TCC is calculated as the sum of interest and non-interest charges

Variable Name	Number of non-missing observations	Proportion of sample (%)	Minimum	Maximum	Mean	Median	Standard deviation
Broker Base APR	373,008		-5.2	1	0.09	0.09	0.08
Broker Finance Commission (£)	672,237		-10,830	30,665	647	458	674
Broker maximum APR	372,395		-5.2	1	0.11	0.10	0.08
Broker Recommended APR	418,999		0	1	0.11	0.1	0.07
Broker Total Earnings (£)	377,295		0	12,141	226	0	502
Broker Volume Bonus (£)	653,071		0	21,818	85	0	434
Customer annual gross income (£)	535,221		0	4,000,000	30,624	26,235	36,493
Commission Model Category	677,588						
Flat fee		51					
Increasing DiC		2					
Reducing DiC		11					
Profit share		0					
Portfolio		0					
Scaled		12					
Other		22					
None		2					
Credit Score	672,511		-998	1,596	801	894	345

Variable Name	Number of non-missing observations	Proportion of sample (%)	Minimum	Maximum	Mean	Median	Standard deviation
Customer employment status	677,588						
Employed		70					
NA		10					
Retired		6					
Self-employed		7					
Student		0					
Unemployed		2					
Unknown		5					
Deposit Amount	677,211		-6,386	2,365,990	4,501	2,250	7,687
Discount Applied	677,588		0	14,460	129	0	537
Interest Calculation Method	677,588						
Compound		11					
Fixed		51					
Other		0					
Simple		39					
Motor Finance Product Category	677,588						
Balloon		0					
Hire Purchase		28					
Lease		3					
Loan		3					
Other		14					

Variable Name	Number of non-missing observations	Proportion of sample (%)	Minimum	Maximum	Mean	Median	Standard deviation
Personal Contract Purchase		54					
Origination Channel Category	677,588						
Franchised Motor Dealer		49					
Independent Motor Dealer		36					
Online Car and Finance Broker		9					
Online Finance Only Broker		3					
Other		2					
Unknown		0					
Vehicle Condition Category	677,588						
New		31					
Used		69					

Analysis of loss

Summary

2.1 This annex describes analysis that tests whether consumers experienced loss and if so, the scale of that loss. We completed five analyses (see Table 14 below) and commissioned a literature review to assess whether there are systematic, direct financial losses in the market. These five analyses were reviewed by two separate, independent academics. Details of their reviews can be found later in this section.

2.2 Our analysis tests whether consumers' direct financial costs were higher when there was an unfair relationship compared to when there was not. This provides an approximate measure of consumer loss.

2.3 Chapter 4 of the CP sets out the following features of agreements which constitute an unfair relationship when the features are not adequately disclosed:

- A Discretionary Commission Arrangement (DCA)
- A high commission arrangement
- The broker has a contractual obligation to source a loan for the customer from a particular lender, known as a tied arrangement

2.4 These features, when inadequately disclosed, create an unfair relationship that could increase costs for consumers. For example, the lack of disclosure may create an information asymmetry which makes it challenging for consumers to assess the value of the agreement or compare across alternative agreements. This may allow prices to rise above where they would be if the unfair relationships did not exist.

2.5 We cannot directly test the financial costs associated with inadequate disclosure of the features listed above due to a lack of data on agreements with adequate disclosure as well as on tied arrangements. For example, the nature of the discretionary commission arrangements was not disclosed to any consumers in our sample (s166 / Skilled person review data). Our analysis shows that this is not sufficient to identify the impact of disclosure on consumers' direct financial costs. Furthermore, our loan level data does not include information on whether the lender had a tied arrangement with a broker.

2.6 We therefore take an alternative approach and test the following research questions:

1. Does the use of DCAs increase the cost of borrowing for consumers compared to similar flat fee loans?
2. Were consumer prices lower in the period following the January 2021 ban on DCAs in the motor finance market?
3. Is a higher broker commission associated with a higher total cost of credit for consumers across flat fee loans?
4. Is broker disclosure of the existence of a non-DCA commission structure associated with a lower agreement APR?

2.7 These tests help to improve our understanding of the financial loss that may be associated with unfair relationships, in the absence of being able to observe the counterfactual of adequately disclosed DCAs, high commissions or tied arrangements.

2.8 We find that:

1. The APR for some DCA loans are more than 20% higher than a similar flat fee loan. The APR is 20-22% higher for reducing DiC loans, 21-24% higher for scaled DCA loans and there is no statistically significant difference for increasing DiC DCA loans.

2. There is some evidence that the ban of discretionary commission arrangements in 2021 reduced the average interest paid by consumers (APR) by 1.76 percentage points. This is equivalent to ~20% of the average APR on motor finance agreements for lenders that offered DCAs between 2019 and January 2021.

3. There is no statistically significant relationship between the consumer cost of credit and commission paid to brokers for flat fee loans on average.

   However, there is a statistically significant relationship between broker commission and consumer total cost of credit for sub-groups of flat fee loans with high commission arrangements. On average, we found that a £1 increase in broker commission is associated with more than a £1 increase in the total cost of credit, when the commission (as a proportion of the loan principal) is above the 75th percentile. This relationship is statistically significant. For these agreements, the average commission is 10% of the total loan amount and commission is on average 33% of the total cost of credit.

4. The average APR was 3.4 percentage points higher when the broker did not disclose payment of commission in non-discretionary commission arrangements compared to when the broker did disclose payment of commission.

2.9 This suggests:

- There is strong evidence that DCA agreements increased costs for consumers. Undisclosed discretionary commission increased the APR for the typical loan by more than 20% compared to a loan with a flat fee arrangement. Expressed differently, the APRs for DCA loans would have been 17% lower had they had a flat fee commission arrangement. The ban of discretionary commission arrangements may have reduced interest paid by consumers by 1.76 percentage points.

- There is some evidence that for high commission payments in flat fee loan arrangements, higher broker commission payments are associated with a higher consumer total cost of credit.

- There is some evidence that non-disclosure of commission payments was associated with a 3.4-percentage point increase in average APR in non-DCA agreements.

Limitations of Analysis

2.10 Based on our wider work in the motor finance market, we've identified three types of loss (1) consumers pay more than they would in a transparent market, (2) consumer receives an unsuitable product and (3) erosion of trust. Our analysis, however, only estimates the loss arising from (1), so the resulting figures should be interpreted as a lower bound of the total loss, since types (2) and (3) are not captured. This is not to suggest that types (2) and (3) are unimportant; rather, they are harder to quantify and estimate due to data limitations and the challenges of establishing a reliable counterfactual.

2.11 There are also multiple channels through which loss can be transmitted, but our analysis only focuses on the cost of credit. Other pathways exist but are not captured, meaning the results should be interpreted as a lower bound of the total impact. This is not to suggest that other pathways are unimportant; they are harder to measure and are not captured here.

2.12 While some parts of the analysis explore subgroup differences, the econometric estimates presented here primarily capture the average loss across the sample of analysis. Consequently, variation in impact across specific groups may not be fully reflected, and certain subgroups could experience disproportionately higher or lower loss than the reported average.

2.13 In some instances, we are limited by our sample size, meaning we do not have sufficient statistical power to detect loss[14].

2.14 This analysis does not establish a 'fair' level of commission. We do not think all commission is harmful, even where it is passed on to consumers, as brokers perform an important and valuable function. Where the amount of the commission passed on to consumers is reflective of the service provided, this is not harmful. A different kind of analysis, which considers the value of the service, or the excess profit brokers are making is required to understand the point at which excess commission is harmful. Similarly, the analysis does not establish a 'fair' price or cost of credit but attempts to understand how various factors may have affected prices.

2.15 Our estimates are subject to various modelling assumptions. Although we have conducted a range of robustness checks to assess sensitivity, some assumptions cannot be directly tested (see details in each analysis). The implication is that our results should be interpreted with appropriate caution.

2.16 See below for a summary of our analysis and relevant findings.

[14] In this context, statistical power refers to having sufficient data to reliably detect a true effect, where one exists.

Table 14: summary of analysis

Analysis	Research Question	Data *(see previous section)*	Methodology	Results	Limitations
DCA Commission Model Impact Analysis	Does the use of DCAs increase the cost of borrowing for consumers compared to similar flat fee loans?	Loan level dataset	Matched regression analysis	Evidence of loss – on average, **greater than 20% increase in APR** for loans made under a reducing DiC / Scaled commission model, compared to flat fee commission loans	We do not observe consumers' propensity to negotiate, but we expect that it is largely randomly distributed between different model types (and where it is not, it is correlated with measures such as credit score). Since our outcome variable is log(APR), our analysis implicitly drops agreements with an APR of zero, so the results should be interpreted as applying to the segment of the market with non-zero APRs.
Difference-in-differences Analysis of the Impact of the 2021 ban of Motor Finance DCAs	Were consumer prices lower in the period following the January 2021 ban on DCAs in the motor finance market?	Loan level dataset; Credit reference agency data	Difference-in-differences analysis of prices in the pre-ban and post-ban periods	Evidence of loss – **average agreement APR was 1.76 p.p. lower** following the DCA ban. This is equivalent to approximately a 20% reduction in average APR.	Many of the coefficients before the treatment are statistically significant, indicating some variation, which could suggest a potential violation of the parallel trends assumption. We will exercise caution in stating that the parallel trends assumption holds fully and reflect this limitation in the weight we give to this analysis. APR is imputed for the control group, and hence it is subject to measurement errors.
Analysis of the	Is a higher broker commission	Loan level dataset	Fixed-effects regression	No evidence of loss across whole sample; evidence of	From the data, we are only able to observe the APR component of the

Analysis	Research Question	Data *(see previous section)*	Methodology	Results	Limitations
relationship between commission and the cost of credit for flat-fee loans	associated with a higher total cost of credit for consumers across flat fee loans?		analysis on dataset aggregated at the broker-lender level	loss across various subgroups. As an example of one such subgroup, when sorting loan agreements with the highest levels of broker commission (£) as a proportion of loan amount (£), we see a statistically significant relationship between commission and the cost of credit above £1 (that is, a consumer's total cost of credit increases by more than £1 for every £1 that a broker's commission increases) around the 75th percentile and above. For this subgroup, commission is equivalent to an average of 33% of the total cost of credit, and 10% of the original loan principal.	purchased package. There may be other avenues for broker commission to affect the cost of credit for flat fee agreements that we would not be able to observe in the data. For example, brokers that receive lower commissions may be less willing to provide customers discounts on the sale price of cars, or vice versa. We note some results are sensitive to modelling assumptions.
DCA Impact of Disclosure	Can we statistically test if broker disclosure of the existence of	Section 166 (s166) Customer assessment	Power calculation	Inconclusive evidence due to insufficient number of observations with adequate disclosure.	The sample is insufficient to reliably detect these effects due to low rates of adequate disclosure. As such, we are unable to draw any conclusions about the

Analysis	Research Question	Data *(see previous section)*	Methodology	Results	Limitations
	a DCA commission structure associated with a lower agreement APR?	form data / Skilled person review + DCA casefile review			effect of non-disclosure on consumer loss for DCA agreements based on this analysis.
Non-DCA Impact of Disclosure	Is broker disclosure of the existence of a non-DCA commission structure associated with a lower agreement APR?	Data Drop 2	Regression analysis	Evidence of loss – *any* disclosure of commission structures to consumers is associated with an **average agreement APR of 3.4 p.p. lower** than for agreements where brokers fail to disclose they are receiving commission.	Our sample is limited, and due to a lack of observed "full" disclosure, we lack enough cases with good "full" disclosures to effectively compare them to cases without in the non-DCA data set. And so, we proceeded with defining the minimum level of disclosure to qualify as "any disclosure". The results may not necessarily be causal (i.e. holding all else constant). We do not observe customer credit scores in this sample, and while we have relatively good variation across disclosure categories within lenders, we do not have this within brokers. Results are not robust to different modelling assumptions.

DCA Commission Model Impact Analysis

Summary of Analysis

2.17 We test whether DCA commission models increase costs for consumers (whether via APR or total cost of credit), when compared to similar flat fee agreements. We estimate how the cost of borrowing for a typical consumer changes depending on which commission model the broker/lender uses, holding all other factors constant.

2.18 To estimate this, we match[15] a sample of flat fee loans to a sample of increasing DiC loans, a sample of reducing DiC loans and a sample of scaled loans (see the table below for more information on commission model types). We then run a regression with inputs including, which commission model the broker/lender used, some features of the loan (including loan size and term) and some information about the consumer (including credit risk).

2.19 We find that:

- **The APR for reducing DiC loans is 20-22% higher than flat fee loans**, when comparing similar customers. This finding is highly statistically significant.
- **The APR for scaled loans is 21-24% higher than flat fee loans**, when comparing similar customers. This finding is highly statistically significant.
- **There is no statistically significant difference in the APR for increasing DiC loans and flat fee loans**, when comparing similar customers. We expect this is because only a limited number of brokers used this commission model in our analysed period, and these brokers (and the loans they extended) were substantially different to flat fee loans, such that the differences could not be effectively controlled for.

2.20 The table below summarises the measured impact of the commission models against different outputs. Our primary model uses the matched dataset to examine the percentage impact of commission model choice on APR.

Table 15: Summary of commission model coefficients

Commission model	Matched	Unmatched				
	% impact on APR	% impact on APR	APR (p.p.)	TCC	TCC / vehicle price (p.p.)	% impact on TCC
Increasing DiC		+20%***	+1***	+786***	+3***	
Reducing DiC	+20%-22%***	+34%***	+3***	+1,082***	+5***	+22%***
Scaled	+21%-24%***		+2***	+220*	+3***	

'***' p<0.001, '**' p<0.01, '*' p<0.05, '.' p<0.1, we omit coefficients that are not statistically significant at 0.1 level. Ranges in the % impact on APR depends on the matching model.

Introduction

2.21 We compare outcomes for consumers who had loans agreed by brokers remunerated with a flat fee commission model (non-DCA loans) and brokers renumerated with increasing DiC / Reducing DiC / Scaled commission model (DCA loans).

[15] This is a process which takes a subsample of the control group (flat fee loans) which has similar characteristics to our treatment group (increasing DiC, reducing DiC and scaled loans).

Data

2.22 We use the 2019-2022 loan level data – more information can be found in the data guide. We restrict the analysis to agreements that were originated between 1st January 2019 and 27th January 2021 (prior to the ban of DCAs).

2.23 Due to the nature of the model, the following observations were dropped:

- Missing for any of the following variables: Loan principal, loan term, commission model used, date of birth, vehicle condition, motor finance product category, origination channel (n=9,778)
- For our primary analysis, if APR is zero. This is because the outcome of interest is log(APR) (n=20,988)

2.24 In the final dataset, we have 227,867 agreements for the following analysis.

Methodology

2.25 We test if the use of DCAs leads to higher borrowing costs. Our primary outcome variable is log(APR). Using this, we can estimate the percentage impact of the commission model on APR. We also consider several secondary outcomes, which also measure cost:

- APR
- Total cost of credit (sum of total interest and fees paid)
- Log(total cost of credit)
- Total cost of credit as a proportion of vehicle cost
- Loan term

2.26 We perform a matched ordinary least squares regression. Our primary specification for this is on a matched dataset, as we observe considerable differences in the characteristics of flat fee loans and DCA loans. As a robustness check, we also run them on unmatched data for our secondary outcomes.

2.27 To create the matched dataset, we restrict the sample to observations with non-missing values for all matching variables, ensuring that each observation can be placed into a coarsened stratum and matched accordingly.

2.28 The process used was as follows: it regards other commission models (Increasing DiC, Reducing DiC, and Scaled) as separate treatment groups. The control group is always flat fee agreements.

2.29 For each treatment commission model: we matched the treated agreement to flat fee agreements using (1) propensity score matching and (2) coarsened exact matching.

2.30 For both matching methods (1) and (2), treated units are matched based on covariates including:

- Loan principal amount
- Credit score – see previous section for more information.
- Year of loan origination
- Loan term
- Deposit amount
- Vehicle condition (new, nearly new, used)
- Motor finance product type (Personal Contract Purchase, Hire Purchase, Balloon, Loan, Lease, Other)

- Origination channel (franchised motor dealer, independent motor dealer, online car and finance broker, online finance only broker, other, unknown)

2.31 We then run fixed-effects regression models on matched and unmatched samples by increasing DiC, decreasing DiC and scaled model.

2.32 **Dependent Variable:** Log of APR (natural logarithm of the Annual Percentage Rate). It should be noted that 0 APR agreements are implicitly discarded in the process.

2.33 **Estimation Method:**

- OLS (Ordinary Least Squares) regression with high-dimensional fixed effects
- Uses the feols function from the fixest package in R

2.34 Model Specification:

- Key independent variables: Commission model indicators (with flat fee as reference)
- For the matched regressions, a binary variable equals one if the agreement is not a flat fee commission model (i.e. treated)
- For the unmatched regression, a factor variable for each commission model, where the reference group is the flat fee commission model
- Control variables include: deposit amount, loan principal, credit score, loan term
- Fixed effects including customer postcode area, customer year of birth, vehicle manufacturer, month and year of the loan origination date, vehicle condition, motor finance product, origination channel and lender-year subgroup

2.35 Weighting:

- For unmatched regressions: Sampling weights
- For matched regressions using nearest neighbour: We retrieve the weights from the matching procedure (nearest neighbour or CEM). These matching weights are then multiplied by the sampling weights

2.36 Standard errors are clustered at the credit broker level, this accounts for potential correlation in residuals within dealers/brokers.

2.37 It should be noted that our estimator is a doubly robust estimator. It is consistent if either the propensity score model or the outcome regression is correctly specified – hence different covariates are selected in the matching and regression stage[16]. This is because the matching model is specified to balance treatment assignment, while the outcome regression may additionally include variables that improve efficiency by explaining variation in the outcome. This separation allows us to mitigate bias and at the same time reduce variance.

2.38 In our regression models, we use the natural logarithm of APR as the dependent variable rather than the raw APR value. This log transformation is common in economics and finance for several reasons:

- It helps normalise the distribution of interest rates
- It reduces the impact of outliers
- It allows us to interpret coefficients as approximate percentage changes

2.39 We acknowledge that restricting the sample to agreements with APR > 0 may introduce sample selection bias if zero-APR agreements are not random. Therefore,

[16] Tan, Z. (2006). Regression and weighting methods for causal inference using instrumental variables. *Journal of the American Statistical Association*, *101*(476), 1607-1618.

the estimates based on log(APR) should be interpreted as applying only to this segment (positive APR agreements) of the market.

2.40 The coefficients from the log-transformed model represent the approximate percentage change in APR associated with each commission model (relative to the flat fee baseline). However, for more precise interpretation, we apply the transformation: [exp(coefficient) - 1] × 100.

Results

2.41 In our matched models, we find significant differences between the APR for flat fee loans and some types of DCA loan, which are not explained in our model by other covariates. The regression results are shown in below. These coefficients suggest the use of these models increases APR on reducing DiC and scaled agreements by a considerable proportion:

Table 16: Regression results from matched models

	Matched – PSM		
	Log(APR)		
Model name:	**Increasing DiC**	**Reducing DiC**	**Scaled**
Independent Vars			
Increasing DiC	-0.0312 (0.0388)		
Reducing DiC		0.1987*** (0.0377)	
Scaled			0.2135*** (0.0226)
Covariates			
Deposit amount	3.78e-6* (1.74e-6)	-5.35e-7 (3.96e-7)	-1.49e-6*** (4e-7)
Loan principal	-8.83e-6*** (1.19e-6)	-4.6e-6*** (6.69e-7)	-4.83e-6*** (6.32e-7)
Risk score	-0.0007** (0.0003)	-0.0007*** (9.97e-5)	-0.0008*** (7.14e-5)
Loan term	-0.0011. (0.0006)	-0.0021*** (0.0005)	-0.0066*** (0.0004)
Fixed effects included: customer partial postcode, customer's year of birth, vehicle manufacturer's name, year month of the loan's origination date, motor finance product categories, origination channel categories, lender x year			
Statistics			
S.E.: Clustered	credit broker	credit broker	credit broker
Observations	11,246	58,240	53,688
R2	0.82591	0.57555	0.68349
Within R2	0.06449	0.10334	0.15942

'***' p<0.001, '**' p<0.01, '*' p<0.05, '.' p<0.1

Table 17: Commission model log(APR) impact - regression results from CEM

	Matched – CEM		
	Log(APR)		
Model name:	**Increasing DiC**	**Reducing DiC**	**Scaled**
Independent Vars			
Increasing DiC	-0.0390 (0.0321)		
Reducing DiC		0.1863*** (0.0269)	
Scaled			0.1886*** (0.0192)
Covariates			
Deposit amount	3.3e-6 (3.05e-6)	-1.84e-6** (6.44e-7)	-1.3e-6. (7.41e-7)
Loan principal	-9.07e-6*** (1.21e-6)	-7.68e-6*** (4.39e-7)	-9.46e-6*** (5.49e-7)
Risk score	-0.0007*** (0.0002)	-0.0007*** (9.3e-5)	-0.0007*** (0.0001)
Loan term	-0.0006 (0.0007)	-0.0020** (0.0006)	-0.0061*** (0.0004)
Fixed effects included: customer partial postcode, customer's year of birth, vehicle manufacturer's name, year month of the loan's origination date, motor finance product categories, origination channel categories, lender x year			
Statistics			
S.E.: Clustered	credit broker	credit broker	credit broker
Observations	22,293	91,495	76,951
R2	0.88902	0.79155	0.66917
Within R2	0.04856	0.15383	0.14171

'***' p<0.001, '**' p<0.01, '*' p<0.05, '.' p<0.1

2.42 The table reports the regression estimates introduced in the methodology section. Each column represents a different model we estimated, matching DCA agreements to flat fee agreements before estimating the regression.

2.43 The labels shown in the first column of the table represent the independent variables included in the models. Each estimate can be interpreted as the partial effect of that independent variable on the natural logarithm of the APR, after controlling for all other covariates included in the specification.

2.44 Beneath each coefficient, the numbers in parentheses are the estimated standard errors. These measure the uncertainty around the coefficient estimates: smaller standard errors imply more precise estimates, while larger ones indicate greater uncertainty. To aid interpretation, statistical significance is denoted using conventional markers: '***' p<0.001, '**' p<0.01, '*' p<0.05, '.' p<0.1.

2.45 The p-value associated with each coefficient represents the probability of observing an effect at least as extreme as the one estimated if the true difference were actually zero (the null hypothesis). Smaller p-values provide stronger evidence against the null hypothesis.

2.46 As described in the methodology, the regression coefficients are first estimated on the logarithmic scale of the dependent variable. However, for a more intuitive interpretation, we transform them into percentage changes using the formula [exp(coefficient) − 1] × 100. The transformed values are reported in the table below.

Table 18: Transformed coefficients

	Propensity Score Matching	Coarsened Exact Matching
Increasing DiC	[exp(-0.0312)-1]*100=-3.1%	[exp(-0.0390)-1]*100=-3.8%
Reducing DiC	[exp(0.1987)-1]*100=22.0%	[exp(0.1863)-1]*100=20.5%
Scaled	[exp(0.2135)-1]*100=23.8%	[exp(0.1886)-1]*100=20.8%

2.47 These findings suggest:

- an average Reducing DiC loan in the data, with APR of 9.8% would have had an APR of 8.0% - 8.1% if it were agreed through a broker using a flat fee model[17]
- an average Scaled loan in the data, with APR of 10.8% would have had an APR of 8.72% - 8.94% if it were agreed through a broker using a flat fee model[18]

2.48 We do not find a statistically significant change in log(APR) for Increasing DiC loans. We believe this is related to the move away from the use of increasing DiC models, meaning only a small sample of brokers, mostly extending lower value loans, were using these models. Given FCA statements that these models made the conflict of interest between brokers and consumers more difficult to manage, it is possible that the brokers still using these were only the ones that lenders felt they had robust oversight over.

APR-17

2.49 The outputs of this analysis are sometimes referred to in the CP as APR-17. We calculate this as follows:

1. The lower matched estimates show an agreement with reducing DiC/scaled commission have an APR 20% higher compared to a flat fee agreement
2. We then convert to find the equivalent flat fee relative to the APR of a DCA as 100/(100+20) = 83%
3. This means the APR of a flat fee agreement is 100-83=17% lower compared to a DCA agreement

Robustness

2.50 In this section we explore the robustness of our main findings by (1) running an unmatched model (2) testing alternative outcome measures (3) testing sensitivity to fixed effects (4) using an alternative specification for our standard errors. This is in addition to the two different matching methods we discussed above.

2.51 We find a significant positive impact for reducing DiC and a significant impact for increasing DiC models in the unmatched regression. This suggests that APRs are 34% and 20% higher for reducing DiC and increasing DiC loans relative to flat fee loans, respectively. However, we find no significant impact for scaled models. We

[17] Lower bound: 1/(1+0.22) * 9.8 = 8.0, Upper bound: 1/(1+0.205) * 9.8 = 8.1

[18] Lower bound: 1/(1+0.238) * 10.8 = 8.72, Upper bound: 1/(1+0.208) * 10.8 = 8.94

believe these results are biased by the differences between the types of loans and customers who are served through each of these models, which is why these differ from our matched results. Our analysis of the impact of commission model on other outcomes supports this, as we consistently find reducing DiC and Scaled models lead to increased costs to consumers.

2.52 We also run our unmatched specification for a number of other outcomes related to the cost of borrowing, we find:

- **APR:** We find significant increases in APR compared to flat fee loans associated with reducing DiC loans of 2.9 percentage point, associated with Scaled loans of 1.8 percentage points and associated with increasing DiC loans of 1.2 percentage points
- **Total cost of credit:** We find significant increases in total cost of credit compared to flat fee loans of £785.9 associated with increasing DiC, £1082.3 associated with reducing DiC loan, and £219.6 associated with scaled loans.
- **Log(total cost of credit):** The regression outputs suggest a statistically significant increase in the total cost of credit of approximately 22% for reducing DiC models, but a reduction for scaled models of approximately 9%. There is no significant finding for increasing DiC models.
- **Total cost of credit as a proportion of car price:** we find a significant increase in the total cost of credit as a proportion of car price of 5 percentage points for reducing DiC loans, 2.6 percentage points for scaled loans, and 2.7 percentage points for increasing DiC.

2.53 We conducted two additional sets of robustness checks for our main model.

- **Fixed effect sensitivity**: We estimated multiple models, each time omitting one fixed effect to assess its influence. The results are qualitatively similar: (1) We do not find a statistically significant change in log(APR) for Increasing DiC loans across all the models. (2) The lower bound of the estimates for Reducing DiC and Scaled model is 17.88%, and the upper bound remains unchanged.
- **Standard error specification**: We re-estimated the models using robust standard errors instead of clustered standard errors. As expected, the point estimates are unchanged. However, the estimate for the increasing DiC is statistically significant at the 1% level for CEM and 10% level for PSM.

2.54 We are confident that our model captures the key observed drivers of price, and unobserved drivers will either be correlated with those we observe. We do not observe the consumers propensity to negotiate, however, we expect that this is largely randomly distributed between different model types, and where it is not, it correlates with measure such as credit score or vehicle condition.

Conclusion

2.55 We find significant evidence of loss arising from inflated interest costs associated with reducing DiC and scaled commission models. This is supported by a wide range of robustness checks. Our main results suggest that APRs for reduced DiC and scaled loans are 21-23% higher relative to similar flat fee loans.

Difference-in-Differences Analysis of the Impact of the 2021 ban of Motor Finance DCAs

Summary of Analysis

2.56 We test whether motor finance consumer prices were lower in the period following the FCA's intervention effective January 2021 to ban DCAs in the market.

2.57 This analysis was conducted using our loan level dataset collected from 18 motor finance lenders. Please refer to the previous section for further details on the dataset.

2.58 We conduct Difference-in-Differences (DiD) analysis between a DCA motor finance loans and personal loans. That is, we compare the changes between personal loans' APR (comparison group) against the changes between motor loan agreements' APR of motor finance lenders that employed DCAs.

2.59 **We find evidence that the average APR for motor finance agreements reduced after the ban on the 28th of January 2021.** We estimate that the DCA ban reduced the average APR for motor finance agreements by lenders who employed DCA before the ban, by 1.76 percentage points, this is statistically significant at the 5% level. This reduction is equivalent to 20% of the average motor finance APR before the ban.

Introduction

2.60 In evaluating the impact of the 2021 motor finance DCA ban (hereafter "the intervention") on consumer prices, we expect the intervention to have reduced consumer loss by lowering average financing costs. Therefore, the research question of interest is whether the ban on DCAs in motor finance decreased consumers' financing costs, on average. If we find consumer prices to have been reduced after the ban, this is indicative that there was loss in the market prior to the intervention.

Data

2.61 For loans in the treatment group, we use our loan level dataset. We also utilised the Credit Reference Agency (CRA) data to retrieve transactional level information on personal loans – the Data Guide provides more information on each. We use agreements originated between 2019 – 2023 to ensure we have common coverage between the CRA dataset and 2024 loan level data. We filter out agreements with an annual percentage rate that is less than zero and agreements where the brokers' commission in £ is less than zero.

2.62 To better satisfy the DiD assumptions, we make the following sample restrictions regarding our comparison group.

- To enhance the comparability of the loans, we restrict to personal loans that have a monthly repayment frequency.
- To reduce the potential for spillover effect (please see details in section "*No spillover between treated and comparison group"*), we remove personal loans made by motor finance lenders. Although personal loans are not directly impacted by the ban, these lenders may adjust their pricing strategy across their entire loan portfolio—potentially engaging in cross-substitution—leading to indirect effects.

1,379,446 agreements were used in the analysis, after filtering.

Methodology

2.63 For DiD analysis, selecting an appropriate comparison group is crucial for the credibility of the approach. Intuitively, a suitable comparison group must satisfy the following conditions:

- **Parallel trends:** loans in the comparison group exhibit trends similar to those within the treatment group (discussed further in the assumptions section).
- **No spillover effects:** loans in the comparison group remain unaffected by the ban, whether directly (e.g., restrictions on specific practices) or indirectly (e.g., through broader market or equilibrium effects).

2.64 Personal loans serve as a suitable counterfactual because they share similar borrower characteristics and market trends but were not subject to the DCA ban, allowing us to isolate the policy's causal effect.

2.65 We also considered using a group of motor finance lenders that did not employ the DCA model before the ban as the comparison group. Our rationale is that lenders that did not employ DCA models before the intervention were already in compliance with the intervention prior to its implementation, and therefore, were not directly affected by the intervention. However, this comparison group may be sensitive to 'spillover' effects. This group (lenders not using DCAs and therefore not affected directly by the ban) operates in the same or related markets to our treated group (lenders using DCAs and therefore affected by the ban).

2.66 We exploit that only motor finance providers and motor finance credit brokers, including motor dealers are subject to the ban[19], so personal loans are not subject to the intervention (and therefore can serve as a comparison group).

2.67 We use the lender-reported APR as the outcome variable. APR is a good measure of the total cost of borrowing for consumers, as it goes beyond the interest rate to consider various other costs for borrowers and is generally standardised across lenders. For the treatment group, lenders were asked to calculate the APR of each agreement using an original loan principal net of any deposits and discounts and including any further fees. For our control group, the APR was not directly reported, so we used the existing fields in the Credit Reference Agency (CRA) data to impute the implied interest rate as follows:

2.68 We construct the nominal interest by solving the variable "rate" in the following non-linear equation for each personal loan. This equation assumes payments are amortised.

$$\frac{Loan\ principal}{Regular\ payment} + \frac{(rate + 1)^{-N} - 1}{rate} = 0$$

2.69 where:

- Regular payment: the amount of agreed regular payment. We standardise that to monthly payment equivalence. That is times 4.33 for weekly payments, times 2 for fortnightly payment, divided by 3 for quarterly payment and divided by 12 for annual payment.
- Loan principal: the balance of the account when account first opened.

[19] 1.8 in PS20-8

- Rate: this is the nominal interest rate we aim to recover for the loan. We multiply the solved rate by 12 to so it has an annualised interest rate interpretation
- N: the loan term e.g. in months

2.70 Due to data quality, we could not impute a legitimate interest rate for some of the personal loans. We exclude any negative rates and limit the data to the 95th percentile to ensure consistency. With any imputation, there will be some measurement error, but as long as it remains stable over time, the difference-in-differences approach will account for it.

2.71 We estimate the DiD using the Callaway & Sant'anna (2021) estimator[20] with an event study specification. The estimator semi-parametrically estimates the average treatment effects on the treated (ATT)[21], the weighted difference between the treatment and comparison group, compared to the base period[22]. It has shown greater robustness than ordinary least squares, especially when controlling for covariates in cases with treatment effect heterogeneity. Here, we anticipate notable heterogeneity in treatment effects, as the impact is likely to vary significantly across customers with differing credit ratings.

Assumptions

2.72 If certain assumptions are met, the DiD approach has a high level of internal validity. This is because it eliminates bias from both (i) time trends which affect both the treatment and comparison groups equally and (ii) time-invariant unobserved characteristics which differ across the treatment and comparison groups.

Assumption 1: No spillover between treated and comparison group

2.73 The validity of the applied design hinges on a fundamental no-contamination assumption. Intuitively, this assumption states that treatment of one unit does not affect the outcomes of comparison units. In our context, the assumption would be violated if a comparison group adjusted their behaviour in response to changes in APR in the treated group.

2.74 Our **comparison group,** consisting of personal loans, could have reacted to market conditions influenced by the DCA ban. For example,

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[20] Callaway, B., & Sant'Anna, P. H. (2021). Difference-in-differences with multiple time periods. *Journal of econometrics*, *225*(2), 200-230.

[21] Intuitively, the estimator does the following:

1. Calculate propensity score of being treated in a particular time. We use the following covariates: borrower's year of birth, loan term, amount of deposit
2. Conduct two-by-two difference-in-differences, weighted by the inverse propensity score calculated in (1), a varying base period as per https://bcallaway11.github.io/posts/event-study-universal-v-varying-base-period
3. We do not cluster our standard error in this analysis. While clustering at the lender level would be appropriate conceptually as it aligns with the sampling-based approach suggested by Abadie et al. (2023), the small number of clusters limits the reliability of such estimates. As Callaway and Sant'Anna (2021) stated in Remark 10: "*If the number of cluster is 'small', however, the application of the aforementioned bootstrap procedure is not warranted."*

Abadie, A., Athey, S., Imbens, G. W., & Wooldridge, J. M. (2023). When should you adjust standard errors for clustering?. *The Quarterly Journal of Economics*, *138*(1), 1-35.

[22] We use a varying base period. In pre-treatment periods, using a varying base period amounts to computing a pseudo-ATT in each treatment period by comparing the change in outcomes for a particular group relative to its comparison group in the pre-treatment periods.

- Lenders that issue motor loans may also issue personal loans. While these personal loans are not directly affected by the ban, the lenders may have adjusted their pricing strategy across their entire loan portfolio, potentially engaging in cross-substitution and causing indirect effects. Therefore, we exclude from the analysis personal loans issued by motor finance lenders[23].
- Customers could have shifted from motor finance to personal loans, increasing overall demand for personal loans. This higher demand could have put upwards pressure on interest rates due to supply and demand dynamics. We investigate this by examining the number of motor finance agreements and personal loan agreements over time. If spillover exists, we expect a noticeable increase in personal loan agreements as customers substitute away from motor finance. When tested, we did not observe a significant increase in personal loan agreements following the ban.

Assumption 2: Parallel trends between treated and comparison group

2.75 For the DiD strategy to be valid in our context for either the primary or secondary comparison groups, the parallel trends assumption must hold for each. This assumption states that, in the absence of DCA loan agreements, APRs for customers of treatment and comparison group lenders would have followed the same trend.

2.76 To check the validity of this assumption for the comparison group, we looked at trends in APRs for personal loans (comparison group) and motor finance agreements (treated group) in the period before the ban. Once again, we consider parallel trends to be credible if the differences in APR between personal loans and motor finance agreements are small and not statistically different from zero. While through a visual check we did not observe noticeable differences between treatment and comparison group before the ban, which strengthens the validity of the parallel trends assumption, an event study regression found that differences may still exist despite not being present in a visual check.

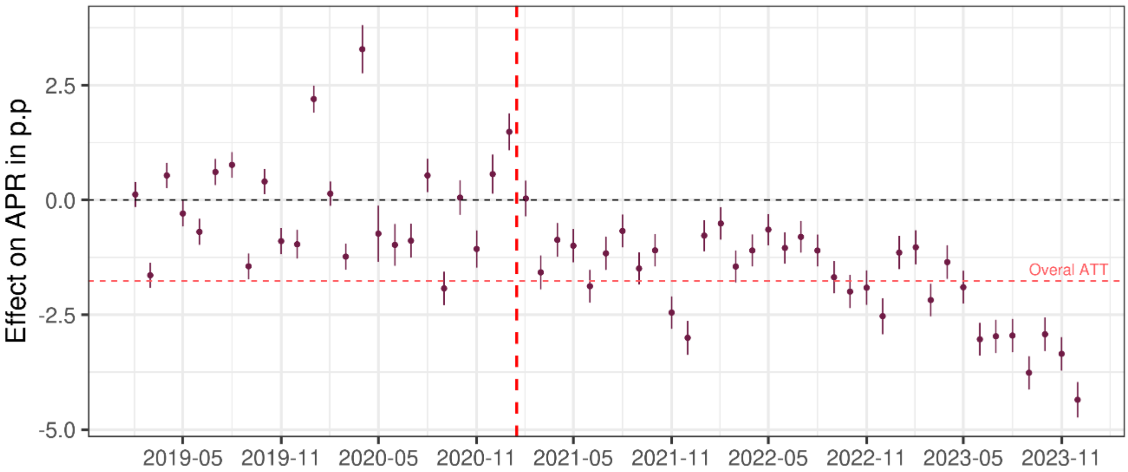
Results

2.77 We find evidence that the average APR for motor finance agreements reduced after the ban was implemented on 2021-01-28.

2.78 We present our results graphically in a coefficient plot (Figure 8). The dots are the differences in the ATT for each month (i.e. the difference in APR between the treated and comparison observations at that point in time, adjusted for pre-treatment differences). The whiskers are the associated 95% confidence intervals. The overall ATT (pink dashed horizontal line) is the weighted average of post-treatment ATTs, with weights based on sample size. The red vertical line indicates the DCA ban date, 2021-01-28.

[23] We identify these lenders using information from the CRA and our loan-level data. If a lender's FRN appears in the product table under "motor" loans, we exclude those observations from our analysis.

Figure 8: Difference-in-differences results



Source: FCA analysis of the loan level data

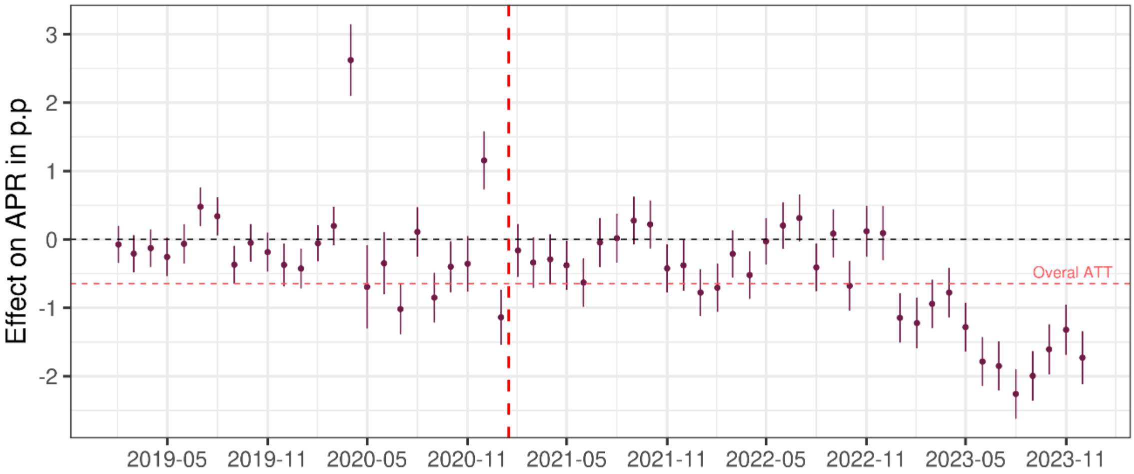
2.79 While we do not observe a clear upward or downward trend prior to the intervention, we acknowledge that examining the coefficients before the intervention provides a more robust validation of the parallel trends assumption than simply analysing the raw trends. Many of the coefficients before the treatment are statistically significant, indicating some variation, which could suggest a potential violation of the parallel trends assumption. While the results are scattered, we exercise caution in stating that the parallel trends assumption holds fully. It should be noted that the analysis is only used as supporting evidence of loss from DCAs. The estimates are not directly applied in the redress scheme.

2.80 We further observe that APRs fell for DCA loans after the ban, relative to the trend in personal loans. Aggregating by the number of observations in each month yields a treatment effect of -1.76 percentage points[24]. The estimate is statistically significant at the 5% level. This is equivalent to ~20% of the average APR on motor finance agreements for lenders that offered DCA between 2019 and January 2021.

2.81 We conducted a robustness check to account for seasonality. Specifically, we regressed APR on interaction terms between the treatment indicator and calendar-month dummies and then used the residualised outcome (i.e. the observed APR minus the predicted APR from this regression) in the Callaway Sant'anna estimator. We present our finding below:

[24] Standard error 0.14, 95% confidence interval [-2.04, -1.49].

Figure 9: Difference-in-differences results



2.82 Parallel trends seemed to improve with this model specification, evident by the less cyclical estimate in the pre-treatment period. The treatment effect is reduced to -0.64 percentage points, and it is statistically significant at the 5% level.

Caveats

2.83 Many of the coefficients before the treatment are statistically significant, indicating some variation, which could suggest a potential violation of the parallel trends assumption. While the results are scattered, we exercise caution in stating that the parallel trends assumption holds fully and our results should be viewed with this limitation in mind.

2.84 Our difference-in-differences analysis critically assumes parallel trends between personal loans and motor finance markets, but we cannot rule out sector-specific shocks coinciding with our intervention date in January 2021. The true counterfactual trend remains unobservable, and we lack data on important covariates such as credit scores and pandemic-related employment stability that might differentially affect the two markets. These limitations necessitate caution when interpreting our estimated treatment effects as causal.

2.85 Using interest rate imputation method on personal loan data assumes fixed, regular payments over the full term, no fees or early repayments, and that interest compounds at the same frequency as payments. In reality, fees, irregular schedules, partial periods, or rounding in payment amounts can distort the inferred rate. Any mismatch between contract terms and actual repayment behaviour will make the calculated rate differ from the true effective APR.

Conclusion

2.86 We estimated whether banning DCAs in January 2021 reduced motor finance borrowing costs, analysing 1,379,446 agreements between 2019-2022 through Difference-in-Differences methodology.

2.87 We compared DCA motor finance loans against personal loans (comparison group), which found the DCA ban reduced average motor finance APRs by 1.76 percentage points (statistically significant at 5% level). A key assumption of difference-in-differences analysis is that, absent the intervention, treatment and control groups would have followed parallel trends. We observe signals that this assumption may be violated in our setting and reflect this limitation in the weight we place on this analysis.

Analysis of the relationship between commission and the cost of credit for flat fee loans

Summary of Analysis

2.88 We test whether higher commission costs are associated with a higher cost of credit for consumers. We test whether relationships between brokers and lenders which benefit the former through high average commission levels, also reward lenders through lending being done at higher average interest rates.

2.89 In non-DCA commission models the flat fee structure means that there is not a direct link between commission and interest costs for individual loans. However, there may be an indirect link, which may emerge when we examine, not the predictors of interest cost on individual agreements, but the predictors of average interest cost for all agreements made by a broker on behalf of a lender.

2.90 We know that information asymmetries exist in the motor finance market due to a lack of disclosure which could allow for commissions to rise above the perfectly competitive level.

2.91 Brokers may select to work with lenders that offer higher commission rates; negotiate higher commission rates with lenders; or engage in arrangements with lenders known as tied arrangements. The lender may then try to recoup some of the cost of this additional commission from the consumer via higher interest costs.

2.92 The degree to which the incidence of the higher commission is borne by the consumer or lender will reflect many factors including elasticities of supply and demand. We would expect to see the consumer bear some of this cost in a well-functioning market, however excessive costs to the consumer could be indicative of market failures.

2.93 By aggregating the time-restricted Loan Level dataset (see Data Guide for more information) for data averaged across aggregated broker*lender pairings and employing a linear regression model on the aggregated data set, we can examine and test this relationship, controlling for other important determinants of the cost of credit as much as possible. We also conduct subgroup analysis to understand how the relationship may vary at different levels of commission.

2.94 When looking at the sample of lender*broker pairs who made flat fee agreements from 2019-22, we find that:

- where lenders pay brokers more in commission, they also tend to charge higher interest rates for agreements made through that broker, suggesting some of the commission cost to the lender is passed on to consumers via higher interest rates.
- A £1 increase in a broker's commission is associated with a £0.60 increase in the consumer's total cost of credit, however this is **not statistically significant.**

2.95 While the findings are not statistically significant in aggregate, within certain segments of the sample, we find indicative evidence of statistically significant higher consumer costs.

- When only considering loan agreements where broker commission was equal to or greater than 50% of the customer's total cost of credit (2% of flat fee loans in our sample), for every £1 increase in broker's commission, the consumer's total cost of credit increases by £1.54.

- When sorting loan agreements by the highest levels of broker commission (£) as a proportion of the motor vehicle purchase price (£), we see a statistically significant and material relationship with costs across the 90th and 95th percentile (£1.84 and £2.64, respectively for every £1 increase in broker's commission).
- When sorting loan agreements with the highest levels of broker commission (£) as a proportion of loan amount (£), we see a statistically significant relationship with costs above £1 around the 75th percentile and above (reaching £1.77 for the 90th percentile, where commission is greater than 9.1% of the loan amount, and £2.33 for the 95th percentile, where commission is greater than 11.1% of the loan amount).

2.96 We would note that these results are not necessarily causal. While we are confident that our model captures and controls for the most important drivers of interest cost like the typical risk profile and typical agreement features of loans arranged by a broker on behalf of a lender, there may be unobserved differences between the agreements which do not correlate with the observed ones, which we therefore cannot control for. Further, there may be unobserved costs associated with higher annual percentage rates (APRs) which justify the broker earning higher commission. For example, higher APRs will likely be charged to sub-prime customers, where there may be more work required by the broker to help the lender assess risk.

Introduction

2.97 Brokers may work with many lenders. Under a flat fee commission structure, they may influence the cost of credit for consumers by selecting to work with lenders that offer both higher commission and higher APRs or by negotiating a higher commission with a given lender, where the incidence of that commission may fall on the consumer and/or the lender. They could also have an arrangement with a specific lender referred to as a tied arrangement. Our analysis aims to estimate whether the consumer incurs some of the cost of commission through a higher cost of credit (controlling, as much as possible, for other factors).

2.98 Under perfect competition, we would expect to observe the level of commission having a minimal effect on the cost of credit for the consumer and brokers would only charge commission to cover the cost of providing their services. However, this might break down when there are information asymmetries and the consumer does not observe the commission charged or know what might be available from other brokers. The degree to which commission is associated with the cost of credit will also reflect elasticities of supply and demand and we would expect to observe consumers bearing some of the cost of commission in a well-functioning market. However, excessive costs could be indicative of market failures.

2.99 We do not directly observe broker costs or details of any tied arrangements between brokers and lenders, but we assume we are able to control for all relevant determinants of cost including their average customer risk scores and average features of the loans. Once we control for these factors, our hypothesis is that if we observe that high commissions are associated with high costs of credit for consumers, we believe this is evidence of market failures leading to high costs.

2.100 As mentioned above, under the flat fee structure we hypothesise the relevant channel where brokers can influence the cost of credit is through the lenders they work with and therefore, we conduct our analysis by aggregating our data to lender*broker pairs. We prefer this setup over analysing individual loans as we think the individual level regression could risk overestimating significance because of the additional observations, and the variation it would capture is not policy relevant because we think there is not an incentive for brokers to influence interest at the agreement level.

2.101 We consider a stylised model with two broker-lender relationships – Lender A x Broker 1 and Lender B x Broker 2. The types of loans made through each of the relationships are identical in every way, except Broker 2 has negotiated a higher rate of commission with Lender B of £*x*.

2.102 If the average total cost of credit for loans made by Broker 2 (on behalf of Lender B) is higher than the average total cost of credit for loans made by Broker 1 (on behalf of Lender A), this suggests that Lender B has passed on some of the higher commission cost to the consumers.

2.103 If the difference in the costs of credit is less than the difference between the commission arrangements, this suggests that Lender B's margin is smaller than Lender A, and their interests and the consumer's interest are aligned in negotiating a lower commission payment to Broker 2.

2.104 However, if the difference in the cost of credit is greater than the difference in the commission payment, then this suggests that paying higher commission has in fact increased Lender B's margin. In this case, the broker's and lender's interests are aligned, in conflict to the consumer's. In the real world, this may arise where a broker has a panel, and the lender uses the higher commission to incentivise the broker to give greater prominence to their loan offer through first refusal arrangements or presenting it before other loan offers from members of their panel.

2.105 In our econometric model, we attempt to control for comparable differences between loans made through broker-lender relationships with high commission and low commission, however, for the above stylised example to be applied to the results of our model, we must make the following assumptions:

- Our model captures all the major factors which measure the cost to lenders of lending money which may vary between loans (for example, consumer credit score, loan term, loan amount, commission cost)
- These costs vary linearly – if increasing loan term by one month increases the cost to the lender by £10, then increasing it by 6 months increases the cost by £60.
- Therefore, we treat our conclusions as indicative of costs to consumers occurring through commission arrangements. Where the relationship between costs and commission is very strong, and if it seems to increase as the level of commission increases, then we take this to suggest that there are greater costs in this area.

2.106 In interpreting the results, it is important to note we cannot be conclusive about loss. For example, in interpreting the relationship between commission and the cost of credit:

- Where it is less than 1, a large proportion of the commission is still being passed on to the consumer and although negotiating a higher commission arrangement reduces profit margins for lenders, it could still increase overall profits, if it incentivises the sales of more loans – profit may still be maximised considering volume x average profit per loan.
- Where it is more than 1, we cannot say if *disclosure* about this commission will or will not impact the level of commission and therefore cost of credit.

2.107 The relationship between commission and the cost of credit might also reflect other factors such as the relative power of the broker and lender in contract negotiations. Where a broker can offer a large benefit to the lender (through greater distribution, brand recognition etc), the lender may pay higher commission which it cannot fully recover through higher cost of credit.

Data

2.108 For this analysis we draw on our loan level dataset (see Data Guide), filtering for only Flat Fee loans.

2.109 In our model, we aggregate the data into broker*lender pairings and consider average observations in the data relevant to each pairing. Below, we outline the average features in the dataset at the agreement level, and at the broker*lender pairing level.

2.110 Once we remove all DCA agreement structures from the data to examine only flat fee agreements (where a broker is paid either a fixed amount, or a fixed proportion of the loan principal), and clean further the data, we are left with 207,882 flat fee agreements covering 14 lenders, 4,080 brokers, and 4,774 distinct broker*lender pairings. The number of individual agreements under each broker*lender pairing ranging from 1 to 7,458 agreements.

Methodology

2.111 This analysis takes broker*lender pairings as its unit of analysis, by aggregating to this level and taking the average of the relevant observations for each pairing. As discussed in the introduction, we do this as the flat fee agreement structure means that a higher commission amount cannot be passed through directly to loans by flexing APR in line with commission for a given agreement, but instead, if it is passed on, is done at the aggregate level through lenders agreeing to contracts which pays a broker higher commission across all loans, in return for the broker offering a higher APR (and therefore total cost of credit for the customers) across all loans.

2.112 This analysis follows a similar linear regression model used in 2019 CP (annex 3, point 14), with a few caveats as expressed below, including adjusting a weighted least squared approach, we weight by the number of loans agreed by the broker*lender pairing.

2.113 The model specifications for this analysis are as follows:

$$I_i = a + \beta_1 C_i + \delta_n + \gamma L_i + \epsilon_i$$

2.114 Where

- I_i is the customer's average total cost of credit for agreements made by broker*lender pairing i. This is expressed in pounds sterling (£)
- C_i is the average commission paid to a broker in pounds sterling (£) across agreements made by broker*lender pairing i
- δ_n is a vector of dummy variables to capture the lender fixed effects.
- L_i is a vector of covariates[25], including normalised credit score, loan size and loan term. The covariates are the average observation for broker*lender pairing i
- We cluster the standard errors at the broker and lender level

[25] The full list of covariates for our primary model specification, including the ones for which we controlled for using proportions, includes: original loan principal (£), balloon payment amount (£), loan-to-value decile, loan term (months), normalised credit score (scale of 0 to 1), vehicle condition, motor finance product, and loan origination channel.

2.115 β_1 is the coefficient of interest in this regression. It represents the average £ increase in the total cost of credit to the consumer for each £1 increase in broker finance commission, holding all other variables in the model constant.

2.116 We weight our regression by the number of agreements each broker*lender pairing is responsible for. This ensures that the results are representative of average market outcomes.

2.117 To account for unobserved heterogeneity at the lender level, the model includes lender fixed effects (where we find some evidence of marginal, yet statistically significant heterogeneity across lenders). Including lender fixed affects impacts the results materially.

2.118 We conducted subgroup analysis across different dimensions:

- **Commission as a proportion of loan amount**: We have apportioned the loan-level data into 99 subgroups, representing loans in the top 99 percentiles of commission amount as a proportion of loan amount, top 98 percentiles, top 97 percentiles and so on until the top 1 percentile.
- **Commission as a proportion of vehicle price**: We created subgroups representing loans in the top 25%, 10%, and 5% of broker commission as a proportion of vehicle purchase price.
- **Commission as a proportion of total cost of credit**: We created subgroups representing: (1) loans where broker commissions represent at least 50% of the total cost of credit, and (2) loans where broker commissions represent both at least 40% of the total cost of credit AND at least 20% of the loan principal amount.

2.119 For percentiles with respect to loans ordered by commission as a proportion of loan amount, we were interested in at which point in the ordered data consumer’s total cost of credit began to increase by *more* than £1 for every £1 that broker’s commission increased. As such, as described above we apportioned the loan level data set into 99 subgroups, aggregated each subgroup’s data at the lender-broker level (calculating means of loan characteristics), and then created a loop to run fixed-effects regression models on each of the 99 aggregated datasets, controlling for comprehensive loan features (principal, term, balloon payment), credit quality metrics (normalised score, LTV deciles), product types (PCP, HP, etc.), origination channels, and vehicle condition.

2.120 For percentiles with respect to commission as a proportion of vehicle price, we selected only loans with ratios at or above that threshold. Then, for each subgroup we aggregated the data at the lender-broker level (calculating means of loan characteristics). We then ran fixed-effects regression models on the aggregated datasets, controlling for comprehensive loan features as above.

Results

2.121 The regression results can be viewed in Table 19 below.

2.122 The primary model specification Model 2a uses broker commission (£) as an input and identifies customer’s total cost of credit (£) as the outcome variable, factoring in two-way clustering of standard errors at the broker and lender level.

2.123 While Model 2a is our primary specification, the other models (Model 1a, Model 1b, and Model 2b) serve as robustness check on the selected input (dependent) variable (different metrics regarding consumer cost, i.e. average APR or average total cost of credit) as well as the inclusion of lender fixed effects and/or clustered standard errors, as seen in Table 19.

2.124 We have chosen to use input and output variables that reflect the commission level and cost of credit in absolute (£) terms, rather than relative (%) terms regarding loan size, as we have other variables that control for loan size and other factors that control for the characteristics of agreements.

Table 19: Regression results commission-cost relationship

Relationship between broker commission and total cost of credit/APR				
Dependent variable	**Average total cost of credit**		**Average APR**	
	Model 2a	Model 2b	Model 1a	Model 1b
Average broker commission (£)	**0.60**	0.33***	0.00*	-0.00
(intercept)		-3763.69 ***		0.11***
		(255.43)		(0.01)
Clustered standard errors	**Y**	N	Y	N
Lender fixed effects	**Y**	N	Y	N
Num. obs.	**4758**	4758	4758	4758
Num. groups: lenderfrn	**13**		13	
Adj. R^2 **(full model)**	**0.90**	0.81	0.97	0.83
Signif. Codes: 0 '*' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1**				
Control variables (not shown) : loan characteristics (principal, term, balloon payment amount, loan-to-value decile, origination channel, motor finance product category), normalised credit score				

2.125 The main result, considering customer's total cost of credit (£) and broker commission (£), is material yet not statistically significant. **The primary specification finds if broker commission increases by £1.00, the customer's total cost of credit will increase by £0.60, on average (across broker*lender pairings), but this is not statistically significant.**

2.126 Although the direction of the results is in line with our hypothesis that higher commission costs may result in a higher cost of credit for consumers, the lack of statistical significance makes these results inconclusive. This could be due to limitations regarding unobserved characteristics or factors associated with higher credit costs which justify the broker earning higher commission.

2.127 We also employed subgroup analysis to understand if the relationship at the broker*lender changes based on a selection of loan agreements at more extreme values of commission. To complete the subgroup analysis, we separated out the agreements on the loan-level, based on certain criteria, before aggregating at the broker*lender level and running the main model specification.

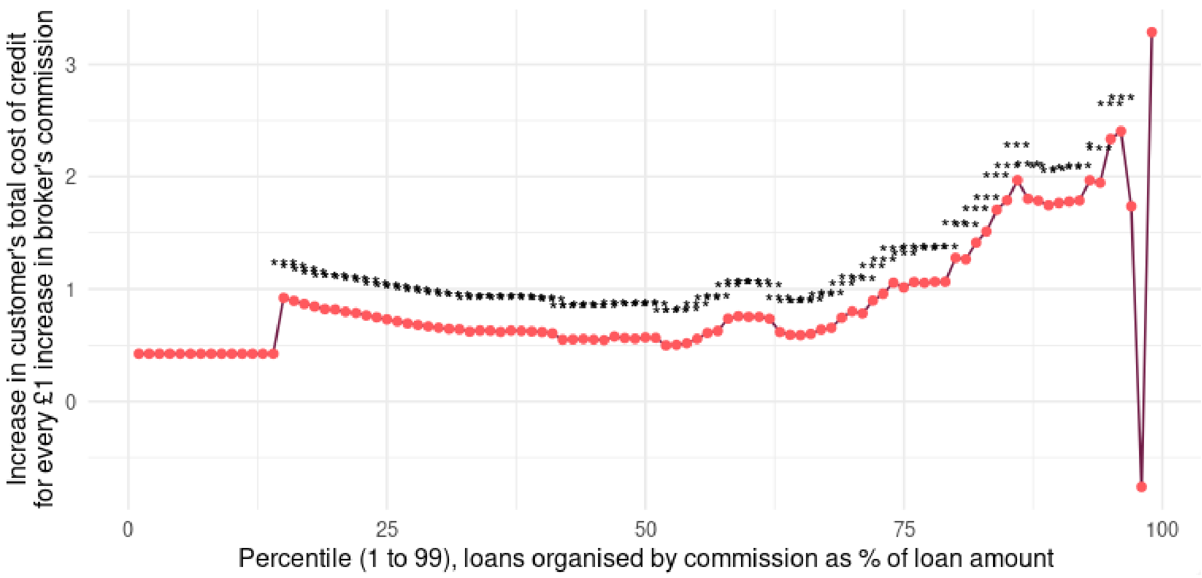
2.128 **Subgroup analysis:** We find a statistically significant relationship between commission and the cost of credit when we consider only loans with higher commission (as a proportion of the loan amount or credit cost), rather than all loans, and in some cases, this is above 1:

- **Commission as a proportion of loan principal:** As described earlier, we have apportioned the loan-level data into 99 subgroups, representing loans in the top 99 percentiles of commission amount as a proportion of loan amount, top 98 percentiles, top 97 percentiles and so on until the top 1 percentile. We find that as we move from the subset containing the top 50 percentiles towards higher commission subgroups, the relationship between commission and the cost of credit gets stronger, and exceeds 1 around the 75^{th} percentile (where for this group commission is on average 10% of the total loan amount, and commission is on average 33% of the total cost of credit), as seen in Figure 10. After this point, the relationship between commission and the costs of credit strengthens further, reaching 1.77 at the 90^{th} percentile (where commission is on average 12.8% of the loan amount, and commission is on average 34.6% of the total cost of credit) and 2.33 at the 95^{th} percentile (where commission is on average 15.0% of the loan amount, and commission is on average 35.1% of the total cost of credit). All these findings are highly statistically significant (see the table below for headline figures, and Figure 10 below for the relationship estimated at each percentile of commission as a proportion of the loan principal).
- **Commission as a proportion of motor vehicle price**: for loans with broker commissions (as proportion of vehicle price) exceeding the top 25^{th}, 10^{th} and 5^{th} percentile value for the dataset, a loan with £1 more commission has £0.88, £1.84, and £2.64 higher total cost of credit, respectively. All three of these results are highly statistically significant, indicating that for agreements where the vehicle is less expensive and/or the broker commission is higher, higher broker commissions are likely to occur alongside much higher total costs of credit. However, for these three groups, we only see a relationship with the cost of credit greater than £1 per £1 extra commission for the 90^{th} and 95^{th} percentile.
- **Loans where commission is equal to or greater than 50% of the customer's total cost of credit**: We estimate loans in this group with £1 higher commission have £1.54 higher total cost of credit. However, we prefer to use subgroupings based on commission as a proportion of some measure of the loan amount, than using total cost of credit as a loan with high commission as a proportion of the cost of credit may may be a loan where the proportion is high because the lender has taken the cost of commission themselves, without passing it on to the consumer (thus depressing the cost of credit, and increasing commission as a proportion of it).

Table 20: Relationship between average commission payment and average total cost of credit for a broker x lender pairing

Sample	Impact of increasing average broker commission on total cost of credit (£)
All non-DCAs	0.60
Subgroup - Proportion of loan principal (>75th percentile)	1.01***
Subgroup - Proportion of loan principal (>90th percentile)	1.77**
Subgroup - Proportion of loan principal (>95th percentile)	2.33***
Subgroup - Proportion of vehicle cost (>75th percentile)	0.88 ***
Subgroup - Proportion of vehicle cost (>90th percentile)	1.84 ***
Subgroup - Proportion of vehicle cost (>95th percentile)	2.64 ***
Subgroup - >50% of total cost of credit	1.54 ***
Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1	

Figure 10: Estimated relationship between commission and the cost of credit for loans exceeding the percentile value for commission as a % of loan amount



Caveats

2.129 **Assumptions:** We make 2 key assumptions in this analysis, with associated limitations.

- Unobserved characteristics are correlated with observed characteristics, therefore controlling for observed characteristics eliminates any bias that might be introduced by unobserved characteristics
- The relationship between APR and the explanatory variables (commission, covariates etc) is linear and constant at all levels of APR and of the explanatory variables.

2.130 **Limitations:** From the data, we are only able to observe the APR component of the purchased package. There may be other avenues for broker commission to increase the cost of credit for flat fee agreements that we would not be able to observe in the data. For example, brokers that receive lower commissions may be less willing to provide customers discounts on the sale price of cars, or vice versa.

2.131 We would advise caution in interpreting the estimates yielded by the subgroup analysis as definitive. We conducted a series of Monte Carlo simulations to examine the statistical properties of the 'threshold at which consumer's total cost of credit increase by more than £1 for every £1 the broker's commission increases (around the 75^{th} percentile of loans ordered as such)'. We found that the Type I error rate, the probability of incorrectly detecting a threshold in the ordered loans when in fact there is none, ranges between 19 percentage points – 51 percentage points above the nominal level under certain conditions. The estimator lacks proper calibration for identifying precise tipping points. While the simulations suggest the estimator should not be treated as a precise threshold, the simulation does not invalidate the broader finding that the relationship between commission and the cost of credit is generally increasing in commission as a proportion of loan amount / commission as a proportion of motor vehicle price, as this is outside the scope of the Monte Carlo simulation.

Conclusion

2.132 While we found some evidence of significant costs associated with high commission for specific subgroups in the aggregated broker*lender dataset, these results are not necessarily causal. We are confident that our model captures and controls for the most important drivers of interest cost like the typical risk profile and typical agreement features of loans arranged by a broker on behalf of a lender. However, there may be unobserved differences between the agreements which do not correlate with the observed ones, which we therefore cannot control for.

2.133 Further, we would caution against inferring that this means commission is 'harmful' at these levels. Brokers may reduce distribution costs for lenders which could benefit consumers too if lenders pass these savings on to them. If the savings from reduced distribution costs are greater than the increased total cost of credit, then even where the estimated relationship between commission and the cost of credit is greater than 1, consumers may be benefiting from the commission arrangement that creates the more efficient distribution.

2.134 Overall, there is mixed evidence to say that commissions on non-DCA loans were likely to cause an increase in borrowing costs for consumers. For typical non-DCA loans there is limited evidence that higher commissions are associated with higher borrowing costs. However, for subgroups where commission is particularly high (as a proportion of the size of the loan or cost of the loan to the consumer), there is evidence that higher commission arrangements are associated with borrowing costs that are comparably higher or in some cases amplified disproportionately, and the size of this amplification increases as we look at higher commission levels. These findings are statistically significant.

DCA Impact of Disclosure

Summary of Analysis

2.135 We test whether the non-disclosure of the existence and/or nature of commission for DCA loans increases the cost of borrowing via the consumer's total cost of credit as a proportion of their original loan principal. We attempted to use the s166 / Skilled person review data + DCA Casefile review, which contains wide-reaching agreement-specific data for consumer, lender and broker information across 3,333 DCA loan agreements.

2.136 DCAs exist such that the portion of the intermediary's compensation composed by the commission earned from the lender following the consumer's signing of the agreement is *directly* tied to the price paid by the consumer for that agreement (i.e. agreement APR). Therefore, we hypothesise that for consumers to simply be aware of the *existence* of any commission when engaging on a motor finance agreement does not provide them with full disclosure. Disclosure of the *discretionary* commission aspect of DCAs is an important factor, as the customer would then be aware that the broker is incentivized to encourage them to pay a higher APR (price) to maximize their own compensation.

2.137 The level of disclosure is the key observable variable of each agreement in this dataset, and the CAF attempts to assess this level for each casefile with five questions and one related sub-question (see Table 21 below). As no cases were assessed to have met the standard of disclosure of the DCA commission arrangement we cannot assess the impact of DCA disclosure.

Analytical Motivation and Caveat

2.138 Understanding the potential existence and extent of possible loss relating from poor disclosure is crucial in assessing the empirical relationship between commission arrangements and consumer prices. As such, this analysis was considered to be of priority importance in informing any redress scheme, but we were unable to complete it due to the following issues with the sample presented in the s166 dataset. We determine that we cannot conduct this analysis with the s166 CAF dataset, because:

- We have a relatively small sample (n=2,216) of consumers who received disclosure of any kind compared to the total number of cases in the sample.
- We observe no consumers who received adequate disclosure, which means we lack sufficient statistical power to distinguish an actual difference from statistical noise.

Introduction

2.139 To evaluate the potential loss under different levels of disclosure, we would ideally conduct a linear regression analysis. This method would allow us to compare the outcome of interest (the total charge for credit) between cases of better and worse disclosure, whilst controlling for various other factors that could influence the outcome. These factors are typically the characteristics of the loan, lender or customers. By including these variables in our model, we can isolate the specific impact of disclosure on the outcome of interest. However, given the small sample size, we seek to understand whether we had sufficient observations to conduct the linear regression.

Data

2.140 We use the s166 / Skilled person review + DCA casefile review dataset, restricted to agreements with a DCA commission model, with a final sample of n=3,333.

2.141 We define different levels of disclosure using questions 7.1 - 7.6 in the Consumer Assessment Files (CAF) data. The table below shows the definitions and number of cases recorded for each level of disclosure:

Table 21: Level of disclosure defined in the CAF

Question number	Question description	# case of "Yes" (% of sample)
7.1	Was evidence on file to show the customer was provided with details on the amount of commission payment.	0
7.2	Was evidence on file to show the customer was informed that commission 'may' be received by the broker.	1,933 (58%)
7.3	Was evidence on file to show the customer was informed that commission 'may' be received by the broker and that, if it was, the customer would be told of this.	2 (0%)
7.4	If 7.3 is selected and commission was received, is there evidence that the customer was subsequently informed?	0
7.5	Was evidence on file to show the customer was informed that commission 'would' be received by the broker but not that the broker was acting under a discretionary commission arrangement.	281 (8.4%)
7.6	Was evidence on file to show the customer was informed that commission 'would' be received by the broker and that the broker was acting under a discretionary commission arrangement.	0 (0%)

Conclusion

2.142 The sample is insufficient to reliably detect these effects. For example, a sample size of zero (case of 'Yes' for 7.6, Was evidence on file to show the customer was informed that commission 'would' be received by the broker and that the broker was acting under a discretionary commission arrangement.) would yield a minimum detectable effect size of infinity in a power calculation. This suggests that the dataset is not fit for purpose to conduct the intended analysis. As such, we are unable to draw any conclusions about the effect of non-disclosure on consumer loss for DCA agreements based on this analysis. While the data is insufficient for empirical analysis, this does not indicate that there may not be loss resulting from non-disclosure of commission.

Non-DCA Impact of Disclosure

Summary of Analysis

2.143 We test whether the non-disclosure of the existence and/or nature of commission for non-DCA loans increases the consumer's cost of borrowing via the agreement APR.

2.144 To test this hypothesis, we use the Data Drop 2 (DD2) data, which contains wide-reaching agreement-specific data for consumer, lender and broker information across 599 non-DCA loan agreements. The limitations of the previous analysis do not apply here as there are a sufficient number of observations in the DD2 dataset to conduct empirical analysis.

2.145 We test the effect of having any amount of disclosure, following the definitions of disclosure in the diagnostic report. Note that this does not correspond to what would be considered adequate disclosure in the proposed redress scheme which is a stronger requirement. We run a fixed-effects linear regression. We run the model on both the level of APR and log(APR)as a robustness check.

2.146 We find a **decrease in APR (3.4. p.p.)** associated with any disclosure compared to loans where there was no disclosure, which is weakly statistically significant.

Introduction

2.147 To estimate loss from non-disclosure of commission in non-DCAs, we look at cases where brokers did and did not disclose that they may/would receive a commission payment. In doing so, brokers could be implicitly (or explicitly) suggesting that they are impartial, meaning consumers assume the broker is offering a competitive rate.

2.148 This analysis is of a small sample over a long period with 599 loans between 2007 and 2024.

Data

2.149 We use the DD2 dataset, containing specific consumer, lender and broker information for 599 agreements made under non-DCA commission models. See Data Guide for more details.

2.150 While our analysis of the Impact of Disclosure for DCAs (see previous section) was underpowered, the DD2 sample provides a sufficient number of observations to complete similar analysis. Further, as noted in the Data Guide, the Customer Assessment Form (CAF) was redesigned between the s166 / Skilled person review and the DD2 collection, and so the two datasets are not directly comparable and a different analytical method was used for each.

Methodology

2.151 We estimate two linear regressions with fixed effects. Both regress the agreement APR (or log(APR)) on a binary variable for any level of disclosure, controlling for observable characteristics of the loan agreement.

2.152 The model specifications differ only with regards to the outcome variable. Both models employ lender and year fixed effects[26].

2.153 We consider two levels of disclosure:

[26] We controlled for the loan agreement's year of agreement, purchase price of vehicle, the guaranteed minimum future value of car (where applicable), loan term, and total credit value.

- No disclosure
- Any disclosure (see the diagnostic report Table 13 for details)

2.154 Note that GMFV is the guaranteed minimum future value used on motor finance deals where the consumer does not pay off the full value of the loan, but a portion of it, then trades the car back in for a guaranteed minimum price to settle the rest of the balance. These deals typically have higher APRs.

Table 22: Summary of the number of and average features of agreements by disclosure status

Disclosure level	Number of agreements	Number of lenders	Proportion with GMFV*	Average APR (%)	Average total cost of credit (£)	Average commission (£)	Average loan size (£)	Average motor vehicle price (£)	Average agreement length (days)
No disclosure	121	20	22%	17.7	2,826	515	11,039	15,651	1,341
Any disclosure	468	39	30%	15.6	3,431	795	11,658	14,459	1,459

Analysis

2.155 The model specifications for this analysis are as follows:

$$Y_i = \beta_0 + \beta_1 I_i + \beta_2 x_i + \delta_n + \epsilon_i$$

2.156 Where

- Y_i is the outcome (dependent) variable - APR
- I_i is a vector of categorical dummy variables for agreement *i*, with the value 0 being "no disclosure" and 1 being "any disclosure."
- x_i is a vector of covariates, including loan term in months, total credit value (£) and purchase price the vehicle (£).
- δ_n is a vector of dummy variables to capture the (1) whether GMFV is applicable to the agreement, (2) the type of commission model, and (3) lender (FRN) fixed effects
- ϵ_i is the error term, clustered at the lender (FRN) level

Results

2.157 We find that any level of disclosure (i.e. if there was evidence on file to show that at minimum the consumer was informed that commission "may" be received by the broker), resulted in an agreement APR that was around 3.4% lower than for agreements where there was no evidence of any disclosure, as seen in the figure below (this result is weakly statistically significant).

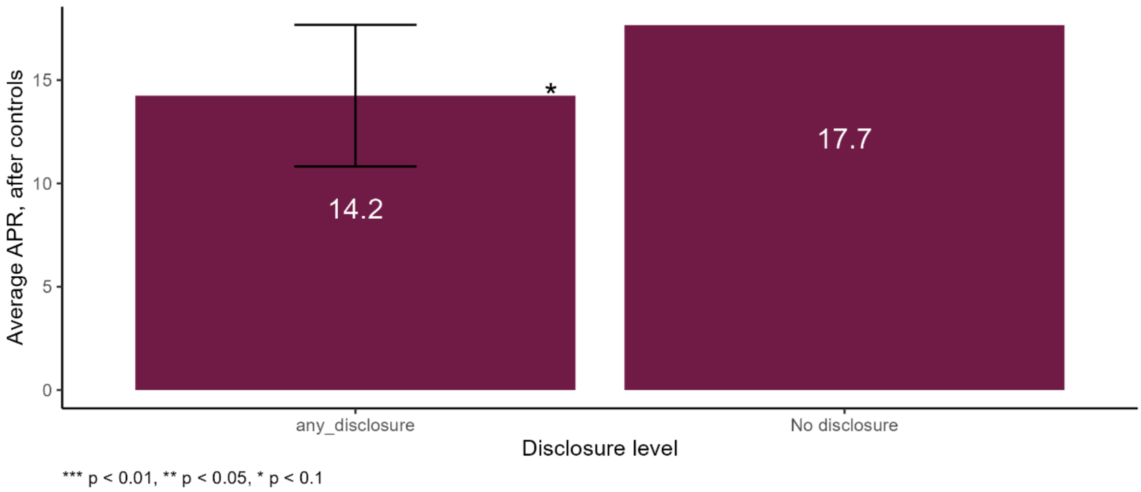
Table 23: Impact of disclosure on APR - non-DCAs

	APR	Log APR
Any disclosure	-3.41434 (1.74812)	-0.11641 (0.07891)
Term of loan	-0.11279 (0.06576)	-0.00147 (0.00199)
Total credit value	-0.00003 (0.00006)	-0.00001 (0.00000) **

Purchase price of vehicle	-0.00004 (0.00004)	-0.00000 (0.00000)
Num. obs.	545	527
R^2 (full model)	0.75398	0.79659
R^2 (proj model)	0.07512	0.10108
Adj. R^2 (full model)	0.71403	0.76224
Adj. R^2 (proj model)	0.06721	0.09309

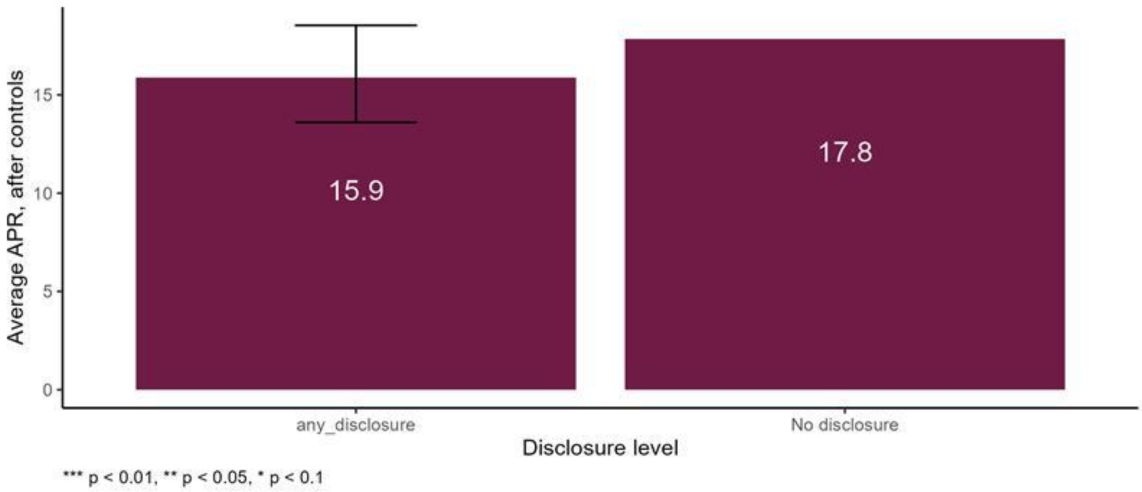
*'***' p<0.001, '**' p<0.01, '*' p<0.05, '.' p<0.1*

Figure 11: Average APR across different levels of disclosure for DD2 loan agreements, "any" disclosure versus no disclosure



2.158 As a robustness check, we conducted the same analysis, but using log(APR) as the outcome measure. It implicitly removes any 0 APR agreements from the regression. We present our finding below:

Figure 12: Average APR across different levels of disclosure for DD2 loan agreements, "any" disclosure versus no disclosure, logged outcome



2.159 When estimating the model with APR in levels, the results were weakly statistically significant. However, after transforming the APR using the logarithm, the effect was no longer significant. This suggests that the findings are sensitive to modelling assumptions, and the estimated relationship may depend on the functional form imposed.

Caveats

2.160 Our sample is limited, and due to a lack of observed "full" disclosure, we lack enough cases with good "full" disclosure to effectively compare them to cases without in the non-DCA data set. And so, we proceeded with defining the minimum level of disclosure to qualify as "any disclosure".

2.161 The results may not necessarily be causal (i.e. holding all else constant). We do not observe customer credit scores in this sample, and while we have relatively good variation across disclosure categories within lenders, we do not have this within brokers.

Conclusion

2.162 We have attempted to assess the impact of disclosing that commission may/would be paid and find it is associated with a 3.4 p.p. reduction in APR. We take these findings as indicative rather than conclusive as:

- this is of a small sample (599 loans),
- we do not have a measure of consumer credit risk in the dataset, so are not able to control for all the factors which may drive APR, and
- our results are sensitive to functional form assumptions.

2.163 Due to the caveats mentioned above regarding the limited sample and lack of understanding regarding key characteristics (broker variation, customer credit risk), we treat this analysis of indicative of an impact from disclosure but are not certain on the existence and scale of the impact based on this piece of analysis alone.

Academic Review

2.164 We commissioned two independent academics to conduct comprehensive reviews of our core analyses (see the Table 24 below). Dr. Kyle Butts (Assistant Professor, Sam M. Walton College of Business, Department of Economics, University of Arkansas) and Dr. Sheisha Kulkarni (Assistant Professor, McIntire School of Commerce, University of Virginia) each completed separate reviews of our analytical documents. The reviewers suggested constructive changes to improve the robustness of the analysis and appropriateness of the model specifications, given our research questions and data access. The suggestions have been implemented where possible, as shown below.

Table 24: Actions from academic reviews

Analysis	Suggestion	Necessary for adequate robustness?	FCA Response
DCA Commission Model Impact Analysis	Create a uniform risk [credit] score that is consistent across lenders.	Necessary for adequate robustness	We created a measure that is comparable across lenders, see Data Guide for details.
	Try alternative matching approaches (e.g. coarsened exact matching) to check robustness across model specifications.	Necessary for adequate robustness	We applied propensity score matching and coarsened exact matching, these gave similar results and we present them as a range in our main results.
	Show the different models adding different fixed effects and show that the coefficient stays relatively stable despite the different estimation strategies.	Necessary for adequate robustness	Addressed. We estimated multiple models, each time omitting one fixed effect to assess its influence. The results are qualitatively similar: (1) We do not find a statistically significant change in log(APR) for Increasing DiC loans across all the models. (2) The lower bound

Analysis	Suggestion	Necessary for adequate robustness?	FCA Response
			of the estimates for Reducing DiC and Scaled model is 17.88%, and the upper bound remains unchanged.
	Use robust standard errors.	Necessary for adequate robustness	We re-estimated the models using robust standard errors instead of clustered standard errors. As expected, the point estimates are unchanged. However, the estimate for the increasing DiC is statistically significant at the 1% level for CEM and 10% level for PSM.
Difference-in-differences Analysis of the Impact of the 2021 ban of Motor Finance DCAs	Provide background information to see if the personal loans market might make a good comparison group. Additionally, the analysis could include some important characteristics of the lender to relax the common trends assumption to be for lenders of similar characteristics (e.g. the lender's total loan amount).	Helpful for adequate robustness, but low priority	Not addressed. We focused our efforts on the DCA Commission Model impact analysis, as its findings have directly informed our policy proposal.
	Conduct an analysis using the non-DCA loans as the "treated" group and personal loans as the "control" group to get a sense of the magnitude of equilibrium / "spillover" effects.	Helpful for adequate robustness, but low priority	Not addressed. We prefer the personal loan comparison group to avoid spillover effects and therefore decided not to pursue the non-DCA control group.

Analysis	Suggestion	Necessary for adequate robustness?	FCA Response
	It might be helpful to limit the analysis to personal loans that are similar sizes to car loans, or go to similarly risky borrowers.	Helpful for adequate robustness, but low priority	Partially addressed - loan size was included as one of the covariates in the inverse probability weighting (IPW) estimation stage. This means that when constructing the weights, we accounted for differences in loan size between treatment and control groups, so that the weighted samples are more comparable on this dimension.
	You could smooth out some of the seasonal variation in the coefficients: you could either aggregate to quarters, or you could use a seasonal filter.	Necessary for adequate robustness	We conducted a robustness check: we regressed APR on interaction terms between the treatment indicator and calendar-month dummies, and then used the residualised outcome (i.e. the observed APR minus the predicted APR from this regression) in the Callaway Sant'anna estimator.
Analysis of the relationship between commission and	Control for predictors of commission and interest cost at loan level prior to aggregating the data.	Relevant for adequate robustness, but low priority	Not addressed. We prioritised having variation at the level of observation

Analysis	Suggestion	Necessary for adequate robustness?	FCA Response
the cost of credit for flat-fee loans			(broker * lender aggregated level).
	Include discussion on differences with effects at micro/aggregated level.	Helpful for adequate robustness, but low priority	We include a discussion of this. Also see row below on the suggestion regarding disaggregated data and broker*lender fixed effects.
	Run a hedonic regression of car attributes and see if lenders with higher commissions charge a lower price for like-for-like cars (to see if price negotiations interact with cost of credit negotiations).	Helpful for adequate robustness, but low priority	Not addressed. We have limited data on car attributes.
	Consider assuming a structural form for the relationship between commission and the cost of credit e.g. Berry (1994). Otherwise, define how we interpret the relationship and relevant assumptions.	Helpful for adequate robustness, but low priority	Not addressed. However, we have added some more information on how we think about the micro foundations.
	Identify “bad actors” vs systemic issues. You could estimate the broker*lender pair fixed effects and plot them to see if there are some that are quite high. Or if particular contracts or borrower characteristics are more likely to result in higher commission.	Helpful for adequate robustness, but low priority	Not addressed. Our primary focus is estimating widespread loss rather than identifying individual actors.
	Consider keeping the data disaggregated and put in broker*lender fixed effects.	Necessary for adequate robustness	We do not expect the effects to happen at the agreement level, and the aggregated level regression is not

Analysis	Suggestion	Necessary for adequate robustness?	FCA Response
			equivalent to the disaggregated one with broker*lender fixed effects. We think the individual level regression could risk overestimating significance because of the additional observations, and the variation it would capture is not policy relevant because we think there is not an incentive for brokers to influence interest at the agreement level. Furthermore, this approach would leverage variation across consumers within a broker-lender. This variation would include "bad variation" due to differences in risk profile across consumers.
Impact of disclosure (DCA and non-DCA)	Provide summary tables of the characteristics of lenders that fall into each bin.	Helpful for adequate robustness, but low priority	Not addressed. We focused our efforts on the DCA commission model impact analysis.
	Remove "whether the consumer negotiated on the interest rate" – likely a bad control.	Relevant for adequate robustness, but low priority	Not addressed. We focused our efforts on the DCA commission model impact analysis.
	Suggest using a multinomial logit	Helpful for adequate	Not addressed. We focused our

Analysis	Suggestion	Necessary for adequate robustness?	FCA Response
	regression on borrower characteristics that predict disclosure or commission type.	robustness, but low priority	efforts on the DCA commission model impact analysis.
	You may be able to get around not having credit risk if you do a regression of credit risk in your other data set on variables that you have in *both* data sets.	Helpful for adequate robustness, but low priority	Not addressed. We focused our efforts on the DCA commission model impact analysis.
	APR should be in logs.	Necessary for adequate robustness	We present both results.
Other comments	Consider where clustered standard errors are/are not appropriate across our analysis.	Necessary for adequate robustness	See discussion throughout analysis.

Literature review

2.165 We commissioned Professor Zinman, Professor of Economics at Dartmouth College, to conduct a literature review on the effects of disclosures and other information-sharing mechanisms on consumer behaviour and market outcomes in two-sided markets (i.e. markets in which two sets of agents interact through an intermediary). This was intended to provide a broader perspective of the potential impact of disclosures and other information-sharing mechanisms in similar markets. This is copied out in full below.

2.166 However, the review found little directly relevant and high-quality empirical evidence, particularly with respect to clear applications for policy and practice. It also found limited evidence-based prescriptions for how to design and implement effective information interventions in market settings. In providing an overview of information more broadly (including, but not limited to, disclosure in two-sided markets) the review found that, the positive impacts of disclosure may be limited, suggesting that trials show 'modest effects'.

2.167 A separate review of FCA behavioural research on testing information-giving interventions has found some positive impact of disclosure, but again an absence of universal success. That review concluded that *how* information is disclosed matters on top of *if* it is disclosed. More broadly, an academic review on the impact of different types of behavioural interventions shows that information-based remedies (including disclosures) may be on average less impactful than other types of behavioural interventions, and especially so in the finance domain relative to others (e.g. health, environment, food).

Critical Literature Review for FCA on Effects of Disclosures and other Information Sharing on Consumer Behavior and Market Outcomes in 2-Sided Markets

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2.168 Literature review specifications - Brief high-level synthesis of related work as defined below, with a focus on:

- Summarizing state of empirical evidence on effects of disclosures and other information sharing[27]—including content, timing, delivery mode-- on consumer behaviour (principally negotiation), consumer product choices, and market outcomes in 2-sided markets.
- Highlighting key gaps in the evidence base, particularly with respect to evidence required for making inferences re: best practices.
- Consumer credit markets, with a particular to attention to:
    - Key product markets where negotiation is important, and platform intermediaries therein
    - Any evidence on heterogeneity in effectiveness of practices or interventions across different groups of consumers or market characteristics

2.169 My high-level synthesis: There is little if any directly pertinent and high-quality empirical evidence, particularly with respect to clear applications for policy and practice. The evidence that does exist, on consumer credit markets and beyond, including disclosures, is far from uniformly encouraging. Prospects of using information interventions to improve consumer and/or market outcomes are unclear, and there is a dearth of convincing evidence-based prescriptions for how to design and implement effective information interventions in market settings.

2.170 Broadly speaking, Heidhues and Koszegi's (2018) overview of information provision (not limited to disclosure, consumer finance, platform markets, and/or negotiation) offers a pessimistic reading of pertinent work: "… a number of limitations to education ["education" = their label for information interventions] have been identified in the literature…. much of the evidence suggests that education campaigns often have little to no effect" (p. 592).[28]

2.171 On disclosure as a particular tool for information provision, Loewenstein et al.'s (2014, p. 391) review of theory and empirics (with the latter mostly coming from lab experiments) summarizes:

[27] I am not including financial education in the set of information interventions, given its relatively high cost and low take-up rate. See e.g., Ibarra et al. (2021) for a cautionary tale about trying to implement financial education at scale. The question of how financial advice markets affect the quality and quantity of information is potentially relevant as well, although beyond the scope of this review. For recent entrees to this literature see, e.g., D'Acunto and Rossi (2023) and Reuter and Schoar (2024). For more theory-focused reviews see Inderst and Ottoviani (2012) and Inderst (2015). N.b. that both the academic literature and business practice focus far more on household assets than liabilities, suggesting important systemic gaps in both knowledge and practice.

[28] Heidhues and Koszegi (2018, Section 7.3) offers a useful primer on informational interventions when the fundamental problem is consumer "naivete": a decision maker not (fully) recognizing what they don't know or anticipate (e.g., about the market and/or their own behavior).

… different psychological factors complicate, and in some cases radically change, the economic predictions. For example, limited attention, motivated attention, and biased assessments of probability on the part of information recipients can significantly diminish, or even reverse, the intended effects of disclosure requirements. In many cases, disclosure does not much affect the recipients of the information but does significantly affect the behavior of the providers, sometimes for the better and sometimes for the worse.

2.172 Work on disclosure since Loewenstein et al.'s review has increased the evidence base only modestly:

a. As my paper with Garz et al. on consumer financial protection (2021, Section 2.3.2) summarizes: "Both theory and empirics point to the limitations of traditional disclosures."

b. Nor does subsequent work offer concrete alternatives to traditional disclosure that rise anywhere close to the level of being classifiable as best-practice. E.g., Seira et al. (2017) test various alternative disclosures at-scale in the Mexican credit card market and find no effects or "modest effects at best".[29]

2.173 On platform markets more particularly, the lion's share of pertinent research focuses on (non)disclosure of intermediary conflicts of interest, with a particular focus on financial advising (see also footnote 1).[30] I endorse Burke et al.'s (2015, p. 9) view of this work:

In principle, disclosure can increase investor awareness of conflicts of interest, potentially mitigating their impacts. However, our review of existing studies indicates that disclosure of conflicts of interest may not improve outcomes for all consumers. When conflicts of interest are disclosed, many consumers do not know how to respond appropriately due to various factors, such as lack of a way to accurately estimate the adviser's bias in a recommendation, or the cost of searching for a second opinion. Many consumers fail to adjust their behavior sufficiently, if at all, when conflicts are disclosed. Disclosure can also cause unintended consequences: Consumers may feel a "burden of disclosure" to follow the advice, and advisers may respond to the disclosure by providing even more biased advice, resulting in decreased welfare for consumers.

2.174 The voluminous theoretical literature on platform and information (non-)markets (in addition to many of the above cites, see also e.g., Bergemann and Ottaviani 2021; Jullien, Pavan, and Rysman 2021) makes clear that almost "anything goes"—theoretical predictions on how consumers and markets respond to changes in the information environment hinge on assumptions about various aspects of market structure, participant characteristics and decision making, etc.

2.175 As such it unsurprising, and totally warranted, that previous reviews of pertinent empirical work call into question the external validity of studies based on artificial settings like lab experiments and surveys (e.g., Burke et al. 2015, p. 9). An implication is that painstaking, rigorous, and ecologically-valid empirical work will be required to establish an evidence base regarding which information-sharing practices and interventions work to improve consumer and market outcomes. Loewenstein et al. (2014, p. 413) highlight the value of field experiments in particular. Such work is in very short supply, as outlined above.

[29] Daniel Schwartz (University of Chile) has I think implemented a large-scale experiment with a Latin American credit card issuer on "statement balance warnings" that may have an informational component, but there is no working paper up on his homepage yet.

[30] I do not cover the literature on disclosure in online advertising, where of course social media and search platforms are the key players. For an entry point to that research see, e.g., Ershov and Mitchell (2025).

2.176 Good examples of ecologically valid studies—field studies, in a setting of policy interest or with similar characteristics to the setting of primary policy interest, testing some realistic information intervention at-scale, and/or using methods that permit inferences about effects at-scale (in "general equilibrium", as we economists like to say)—include:

a. Anagol et al. (2017) find that a 2010 policy in the Indian life insurance market, requiring disclosure of commissions for a specific product (equity-linked life insurance), results in agents recommending alternative products with high commissions but no disclosure requirement. See also Stango and Zinman (2011) for suggestive evidence on how differential and limited enforcement of mandated disclosure affected car loan borrowers in the U.S., during the early days (second decade) of the Truth-in-Lending-Act.
b. The agricultural development literature has some interesting RCTs on how providing price and/or other market information to farmers affects their bargaining and outcomes. See e.g., Soldani et al. (2023) and Pereira et al. (2023).
c. I am working with a research team to develop an RCT in the U.S. car purchase and financing market. (This is a platform market in the sense of dealers intermediating most financing and often doing so literally through a platform that connects them with lenders.) We are currently working to develop and pilot interventions that provide car buyers with simple tips on how to search and negotiate, and/or provide agents to negotiate on their behalf.
d. Han and Yin's (2025) conduct a survey information experiment with customers of a credit card issuer in China (n.b. credit cards have platform market elements of course, but not much negotiation between consumers). This paper is interesting in that it does not presume a particular information gap before intervening but starts by diagnosing one. See my payday borrowing paper with Allcott et al. (2022) for a similar approach re: behavioral biases.
e. Several other studies have ingredients that may be of interest, in terms of their setting and/or interventions tested.[31]

2.177 Another striking evidence gap is on consumer bargaining. There is very little high-quality empirical evidence on how, and how effectively or efficiently, consumers bargain. Two interesting but not directly pertinent exceptions are:

a. Byrne et al.'s (2022) randomized audit study on consumer negotiation in the Australian retail electricity market. Here bargaining is the treatment meant to effect contract outcomes (in contrast the RCT my team is working to develop, where the treatment is information/training and bargaining is one of the outcome families of interest).
    i. As Byrne et al. note: "Although theoretical research on bargaining with incomplete information dates back at least 40 years, few empirical studies on the topic exist" (p. 2502).
b. The only bargaining-focused paper I could find re: a consumer financial product is Allen et al.'s (2019) structural modeling of the Canadian mortgage market. They do not consider information provision, nor did that market have important platform features best I can tell, at least during the study period

[31] Bertrand and Morse (2011) test behaviorally-informed disclosures with a single payday lender (not a platform market, no bargaining and it would be important to test interventions across a broader swath of suppliers and with better data coverage of the entire market). Homonoff et al.'s (2021) evaluate a light-touch, large-scale test of encouraging student loan borrowers (not a platform market, no bargaining) to check their FICO score. Bai et al. (2023) assess effects of randomly placing online purchase orders and reviews in the children's T-shirt marketplace on AliExpress.

(1999-2002). The paper does infer large search frictions, which is consistent with substantial information costs and/or gaps on the consumer side.

2.178 Another gap is the dearth of work on intermediaries in vehicle financing.[32] Grunewald et al. (2023) is an important start, but they sacrifice some empirical realism (e.g., by assuming that other contract terms are set more or less exogenously, rather than negotiated) to get their model working for the purposes of analyzing competition policy.

2.179 Recapping my high-level synthesis: academic research provides little if any prescriptive guidance thus far. Various literatures are trending in helpful directions, but progress is slow and for the most part not directly applicable to addressing the questions at the heart of this review. Rigorous field studies, in particular settings, will be required to generate actionable evidence for policy and practice.

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Redress liability estimates

Summary

3.1 This annex contains the analysis used to estimate lender-level and market-wide redress liabilities. It informs our preferred redress methodology and associated policy discussion in the consultation.

3.2 We estimate the number of regulated motor finance agreements that may be assessed and determined under our proposed scheme, and estimate the associated total compensation owed to affected consumers under this scheme and the alternative options that we have considered.

3.3 The analysis tests the prevalence of the 3 types of unfair relationships proposed in Chapter 4 of CP25/27. For the purposes of this technical annex, we refer to these unfair relationships as follows:

- **Unfair Relationship 1 (UR1)**: Cases with inadequate disclosure of DCA.
- **Unfair Relationship 2 (UR2)**: Cases with inadequate disclosure of high commission.
  - **UR2 Very High Commission (UR2VHC)**: within UR2, we separately identify agreements with very high commission levels, defined as agreements where commission paid to the broker was equal to or greater than 50% of the total cost of credit and 22.5% of the loan amount.
- **Unfair Relationship 3 (UR3)**: Cases with an inadequately disclosed contractual tie between the lender and broker.

3.4 We explore lender-level redress liability impacts by estimating redress owed for each agreement which meets at least one of the unfair relationship breaches above. We consider the following redress methodologies as proposed in Chapter 8:

- **Loss-based APR adjustment remedy**: apply a 17% difference (reduction) to the APR the consumer actually paid to produce a "market-adjusted APR". The difference between each payment actually made under the agreement and that which would have been paid using the reduced rate shows how much extra the consumer overpaid in total. The summation of these monthly differences, with the addition of compensatory simple interest would form the basis of the redress amount.
- **Commission repayment remedy**: this approach mirrors the Supreme Court's decision in *Johnson* – repayment of commission plus interest at a commercial rate.
- **Hybrid approach**: the proposed hybrid remedy would average the outcomes of the proposed loss-based APR adjustment remedy, and the commission repayment remedy.

3.5 Further detail around the proposed determinations of unfair relationships and redress liabilities can be found in Chapter 4 and Chapter 7 of the CP, respectively.

3.6 To estimate the market-wide redress liability we scale up the lender-level estimates using sampling weights at the stratum level, which is discussed in more detail below.

3.7 We present our key redress liability estimates below. These figures reflect an estimate of the liability, assuming redress is claimed for all agreements found to be in breach. Table 25 below reports estimated total liabilities under the loss-based APR adjustment remedy, commission repayment based, and hybrid approaches.

3.8 For brevity, we use the terms 'eligible' and 'ineligible' as shorthand to describe whether an agreement does or does not contain at least one of the features we propose could give rise to an unfair relationship. We note that in practice, consumers with agreements without such features may still be invited to opt into the scheme and have their case assessed (see Chapter 6 of the CP). Our use of this shorthand is not meant to be a description of scheme rules.

Table 25: Estimation result under 3 redress methods

Description	Redress liabilities (£bn)	Mean redress per eligible agreement (£)	Median redress per eligible agreement (£)
Loss-based APR adjustment	£6.2bn	£441.81	£333.00
Commission repayment - based	£13.2bn	£948.91	£693.88
Hybrid	£9.7bn	£695.32	£541.53

Notes: These estimates are subject to the key assumptions set out in the "Limitations of analysis" section and other caveats included in the CP. The redress liabilities are scaled to 100% of the market, but the mean and median redress per agreement are calculated for those within the sample. For the loss based APR adjustment remedy and hybrid remedy, agreements which meet the criteria for UR2 (VHC) & UR3 receive Commission repayment based redress.

3.9 Tables 26 and 27 present our estimates specific to each type of unfair relationship. UR1, UR2 & UR3 are estimated under the hybrid approach and the combination of UR2VHC & UR3 is estimated under the Commission repayment-based approach, as detailed in Chapter 8 of the CP. These figures do not account for multiple types of unfair relationship related to one agreement, therefore when summed will not equal the total liability.

3.10 We present the pre- and post-2014 figures separately to highlight the significant differences in estimated breach rates for UR1 within each period.

Table 26: Distribution of unfair relationship breaches under the hybrid approach (2007-2013)

Unfair relationship type	Breach rate (%)	Redress liabilities (£bn)	Mean redress per eligible agreement (£)	Median redress per eligible agreement (£)
UR1	55.5%	£2.7bn	£570.95	£442.45
UR2	4.7%	£0.4bn	£1,128.69	£965.79

Unfair relationship type	Breach rate (%)	Redress liabilities (£bn)	Mean redress per eligible agreement (£)	Median redress per eligible agreement (£)
UR3	10.4%	£0.5bn	£548.14	£425.31
UR2(VHC) & UR3	0.027%	£6.4 million	£2,826.61	£2,562.09

Table 27: Distribution of unfair relationship breaches under the hybrid approach (2014-2024)

Unfair relationship type	Breach rate (%)	Redress liabilities (£bn)	Mean redress per eligible agreement (£)	Median redress per eligible agreement (£)
UR1	30.3%	£4.9bn	£731.04	£580.30
UR2	11.3%	£2.8bn	£1,106.03	£959.93
UR3	10.6%	£1.6bn	£738.73	£573.84
UR2(VHC) & UR3	0.053%	£29.3 million	£2,472.07	£2,332.32

Notes: The redress liabilities attributed to UR1, UR2 and UR3 in Table 26 and 27 will not add up to the total liability in Table 25 as some agreements in Table 25 may have multiple breaches. These are also liabilities counted in sample whereas the total in Table 25 are scaled up to reflect market wide estimates. These estimates are subject to the key assumptions set out in the "Limitations of analysis" section. UR2VHC & UR3 calculated using the Commission repayment remedy where commission is equal to or greater than 50% of the total cost of credit and no less than 22.5% of the loan amount and there is a tied relationship.

3.11 We set out the limitations and assumptions below. For the purposes of the redress liabilities analysis, the most important limitations concern missing values in the data, which have been addressed using averaging methods or assuming other values to retain as many observations as possible. Additionally, the dataset was sampled based on number of outstanding agreements as of June 2024, with the implicit assumption that the distribution of agreements at that point is a reasonable approximation of earlier years. While these limitations introduce a level of uncertainty, we consider this approach proportionate and robust for estimating redress liabilities.

Data Sources

3.12 We draw on 2 sources of data:

- DD1
- Section 166 Review and DD2 Combined sample data

3.13 For details, please see our Data Guide.

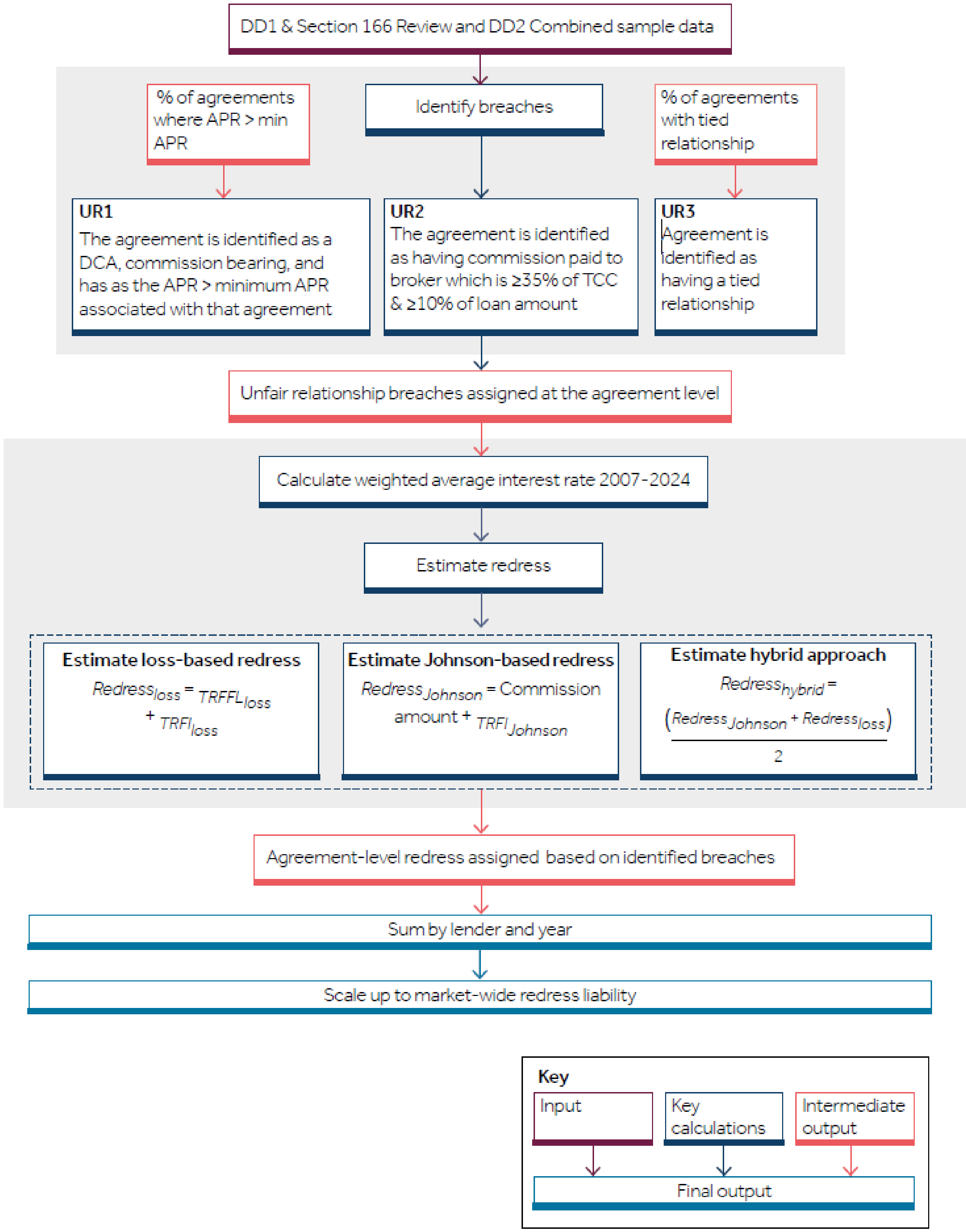
Methodology

3.14 We modelled redress liabilities in three stages:

- Identification of agreements that meet the criteria of an unfair relationship
- Estimating lender-level liabilities
- Scaling to estimate market-wide redress liability

3.15 The Figure below summarises the approach we took. We explain each of the three stages in more detail below.

Figure 13: Summary of approach to estimating maximum redress liability



Identification of agreements that meet the criteria of an unfair relationship

3.16 In this section we set out the approach taken to identifying the number of agreements that meet the criteria of an unfair relationship, as set out in Chapter 4, and are therefore considered cases which will be looked at under the proposed rules of the proposed scheme.

3.17 Table 28 below sets out a summary of the criteria, which we consider when assessing whether an agreement was subject to an unfair relationship. Where we have identified an unfair relationship, we refer to this agreement as in breach.

Table 28: Summary of the types of in-scope unfair relationships and the parameters used to identify these in our analysis

Unfair relationship type	Description	Unfair relationship criteria
UR1	This captures cases with inadequate disclosure of DCA.	UR1 captures cases where three criteria are satisfied simultaneously: • the agreement is identified as a DCA, • the agreement is commission bearing, and • the APR exceeds the minimum possible APR associated with that agreement.
UR2	This captures cases with inadequate disclosure of high commission.	We consider an agreement to be in UR2 breach if the commission paid to the broker is: • ≥35% of the Total Cost of Credit (TCC), and • ≥10% of the loan amount. **UR2VHC**: a very high commission paid to the broker, defined as ≥50% of the TCC and ≥22.5% of the loan amount.
UR3	This captures cases with an inadequately disclosed contractual tie between the lender and broker.	In determining a breach rate, we assessed incidence of undisclosed rights of first refusal within a small sample of our set of sample agreements from the s166 exercise. Based on the review of a total of 570 DCA casefiles, 77 (13.5%) had documentation on the file that indicated an undisclosed right of first refusal.

3.18 The subsections below provide an explanation of how we implemented our analysis to identify agreements which meet the criteria above.

UR1

3.19 As set out above, we define agreements that breach UR1 as those that satisfy all of these criteria:

- the agreement is identified as a DCA,
- the agreement is commission bearing, and
- the APR is higher than the minimum possible APR associated with that agreement.

3.20 As set out in the Data Guide, DD1 includes variables which identify the first two criteria, so therefore any agreements which do not meet these are excluded.

3.21 DD1 does not include the minimum APR, however it does include APR. To identify agreements where the APR is greater than the minimum possible, we exclude agreements that have an APR of 0. This means, we restrict the number of candidate UR1 breaches in DD1 to those which satisfy the first two criteria and have an APR larger than 0.

3.22 The combined sample data does include variables for APR and minimum APR. Following the data cleaning steps set out in the Data Guide, we calculate the proportion of commission bearing, DCA agreements for which APR > minimum APR, to estimate the proportion of the candidate breaches in DD1, outlined above, which also meet this criterion.

3.23 We then assign UR1 breaches to this proportion of agreements in the subset of candidate breaches in DD1 at random. In result we now have an identifier at the agreement level of whether an agreement is in breach of UR1.

3.24 This approach of first restricting the potential DD1 agreements in breach based on the criteria we can identify in both DD1 and the combined sample data, before allocating at random, enables us to reduce the level of variation in our results related to the randomisation element. This allows us to avoid using assumptions informed by analysis of the combined sample data set where the information is available in DD1, and therefore the random element is only applied to a much smaller set of agreements.

3.25 From our analysis of the combined sample data, we have estimated the proportion of agreements for which APR > minimum APR is 95.3%. When breaches are randomly assigned to this proportion of the candidate DD1 agreements, the run-to-run variations in the associated total redress liability estimates is small.

UR2

3.26 All variables required to identify whether an agreement meets the criteria for UR2 are present or can be calculated in DD1.

3.27 Breach cases are identified through the following process. First, the TCC is calculated using agreement-level variables, including monthly rate, monthly payments derived from that rate, contract length, loan value and GMFV (guaranteed minimum future value). A detailed explanation of how we calculated TCC in DD1 is outlined in the Data Guide.

3.28 We count the number of agreements where the commission is greater than or equal to 35% of the TCC and 10% of the loan value, as this meets the high commission criterion and indicates a UR2 breach.

3.29 In the specific case of very high commission under UR2 breaches (UR2VHC), we count the number of agreements for which commission is greater than or equal to 50% of the TCC and 22.5% of the loan value.

UR3

3.30 3.30 In our DD1 data we do not observe whether the broker disclosed the existence of a tie. Our review of s166 data shows that 13.6% of agreements had an undisclosed tie. We randomly select that proportion of commission bearing agreements in our DD1 data and assign them as a breach. We then exclude agreements made by 3 subprime lenders as they have reported to us through the firm monitoring programme that they have never used these types of arrangements with brokers.

3.31 The manual review involved of a total of 570 DCA casefiles from the s166 data. We found that 77 (13.5%) indicated an undisclosed 'hard tie' where the broker was tied to a specific lender or, where the broker had access to a panel of lenders, a specific lender had a 'right of first refusal' in which the broker had to offer new loans to them first and give them the business if they accepted the proposal. Agreements meeting these conditions were classified as UR3 breaches.

3.32 We do not incorporate the estimated non-DCA prevalence of undisclosed ties detailed in the Diagnostic Report in this analysis. As we estimate this is slightly lower than our estimate for DCAs, our resulting estimates of liability in relation to UR3 will be higher than if we had applied this lower rate to non-DCAs. If we took account of non-DCA prevalence of undisclosed ties and instances where there was tie but where there were insufficient documents to determine if disclosed, the estimate of the breach rate for non DCA is very slightly higher.

3.33 An agreement is classified as being in breach if it satisfied any one of the UR1, UR2, or UR3 conditions. We calculate the redress due for each agreement that satisfies the criteria, following the approaches set out in Chapter 8.

3.34 At the end of this process, we find that 13.9m agreements within our sample are in breach. Scaling this figure up to represent the number of breach agreements across the market (see 3.50 below) gives an estimate of 14.2m agreements.

Estimating lender level liabilities

3.35 To estimate lender-level liabilities, we consider three alternative redress approaches as set out in Chapter 8:

- **Loss-based APR adjustment remedy**
- **Commission repayment remedy**
- **Hybrid approach**

3.36 In Chapter 8, we propose that compensatory interest should be calculated using a set rate of simple interest for each year covered by the scheme. This would be based on the time-weighted average of the Bank of England base rates for that year plus 1 (percentage point) and rounded up to the nearest quarter percentage point. This approach aligns with the policy change announced by the Financial Ombudsman following its recent consultation, where the Bank of England base rate + 1pp will replace 8% as the rate for compensatory interest for complaints referred after 1 January 2026.

3.37 In the scheme, lenders will need to calculate the compensatory interest for each agreement based on the period covered.

3.38 For the purposes of calculating compensatory interest in this model, we adopt a simplified methodology: we calculate the interest rate for each year as the base rate plus 1 percentage point and use the number of eligible agreements in each year to give a single weighted interest rate for the whole period (see Table 29 below). This rate is applied to every agreement in the dataset. This results in an effective compensatory interest rate of 2.09% applied in the model, and has been rolled forward to the theoretical redress payment date (end 2026).

Table 29: Compensatory interest calculations for model

Year	Base rate	Base rate plus 1pp	Rounded up to nearest quarter pp	Total eligible agreements
2007	5.61%	6.61%	6.75%	420,489
2008	4.67%	5.67%	5.75%	675,565
2009	0.64%	1.64%	1.75%	735,125
2010	0.50%	1.50%	1.50%	775,050
2011	0.50%	1.50%	1.50%	782,187
2012	0.50%	1.50%	1.50%	772,060
2013	0.50%	1.50%	1.50%	860,668
2014	0.50%	1.50%	1.50%	1,021,342
2015	0.50%	1.50%	1.50%	1,088,352
2016	0.40%	1.40%	1.50%	1,195,598
2017	0.29%	1.29%	1.50%	1,227,968
2018	0.60%	1.60%	1.75%	1,246,718
2019	0.75%	1.75%	1.75%	1,115,848
2020	0.23%	1.23%	1.25%	859,429
2021	0.11%	1.11%	1.25%	478,637
2022	1.47%	2.47%	2.50%	406,953
2023	4.68%	5.68%	5.75%	313,886
2024	5.18%	6.18%	6.25%	243,731
Weighted average	**2.09%**			

Note: The Bank of England base rate in each year is the monthly average of the BoE rate. The 2007 base rate is averaged from 6 April to 31 December 2007 and the 2024 base rate is from 1 January to 25 October 2024 to align with the DD1 data.

3.39 We then calculate loss-based redress:

- **Step 1:** Calculate an agreement's monthly payments using a financial formula that calculates the fixed periodic payment needed to fully repay a loan based on a constant interest rate. Note that the regressed GMFV is used as the future value, whilst using the loan value as the present value. The number of payment periods is taken to be the full contractual length of the agreement, to reflect what these values were when the agreement was taken out. Where the GMFV is greater than 0, the number of payment periods is incremented by 1 as this is how balloon payments (the final payment made at the end of an agreement with a GMFV) with are typically handled, through an additional payment period beyond the contractual term of the loan.

- **Step 2:** Step 1 is repeated, using the counterfactual APR resulting from the analysis of loss work. The counterfactual multiplier is 0.83.
- **Step 3:** The difference between steps 1 and 2 is multiplied by the number of months the agreement was active (strictly less than the contractual length of the agreement if it finished early). This gives the total redress from financial loss ($TRFFL_{loss}$).
- **Step 4:** For each overcharge, calculate the interest it would accrue from the date of payment until the theoretical redress payment date (31 December 2026).
- **Step 5:** Using the (constant) monthly overcharge value (e.g. £25), calculate the interest on that month using the (simple) interest rate (e.g. 2.09% as we have calculated) / 12 months = 0.17% monthly: £25*(2.09%/12months) ~= £0.04 / month.
- **Step 6:** In the first month of the agreement (e.g. June 2020), calculate how many months ago that was from the anticipated redress payment date and multiply the interest amount from step 5 by that figure: £0.04*(theoretical redress payment date of December 2026 - June 2020) = £0.04*(78 months) = £3.12
- **Step 7:** In the second month of the agreement (July 2020), repeat step 6 but for one fewer months: £0.04*(77 months) = £3.08
- **Step 8:** Continue this pattern until all months have been accounted for and you have reached the final complete month where £0.04*(1 month) = £0.04 is owed.
- **Step 9:** The sum of all those contributions comprises the total redress from interest ($TRFI_{loss}$).
- **Step 10:** Loss based APR adjusted redress is then calculated as $Redress_{loss}$ = $TRFFL_{loss} + TRFI_{loss}$

3.40 Commission repayment-based redress is calculated with the following steps:

- **Step 1:** Calculate the amount of interest accrued on the commission amount from the start of the agreement to the anticipated redress payment date using a simple interest rate (the compensatory interest rate) to find the total redress from interest ($TRFI_{Commission\ repayment}$)
- **Step 2:** Add this to the commission value itself to find Commission repayment-based redress as $Redress_{Commission\ repayment}$ = Commission amount + $TRFI_{Commision\ repayment}$

3.41 The hybrid redress is calculated as:

$$Redress_{hybrid} = \frac{(Redress_{Commission\ repayment} + Redress_{loss})}{2}$$

3.42 As outlined in the Data Guide, for agreements that have zero commission, APR or loan value, their redress values in all cases are set to missing, rather than 0, so that they do not affect the calculation of average redress per agreement but do contribute to the total count of agreements for a lender in each year.

3.43 With the 3 possible remedy types calculated, redress is assigned at the agreement level in line with our proposal in Chapter 8:

- Agreements with UR2VHC and UR3 simultaneously are awarded $Redress_{Johnson}$ only.
- Agreements with UR1 or UR2 or UR3 are awarded $Redress_{hybrid}$ only.

3.44 With each agreement assigned its corresponding redress amounts, the data is then aggregated to lender, year level using standard built in methods available in the Python Pandas package. This creates an output that contains the total number of agreements, the total number of eligible (UR1 or UR2 or UR3 breaches) agreements, the mean and median redress and the total redress for eligible agreements for a given lender, year combination. Note that the count of agreements will include those with zero commission, zero APR and zero loan value. But these are excluded from receiving redress and won't be counted as eligible agreements.

Redress estimates randomisation sensitivity testing

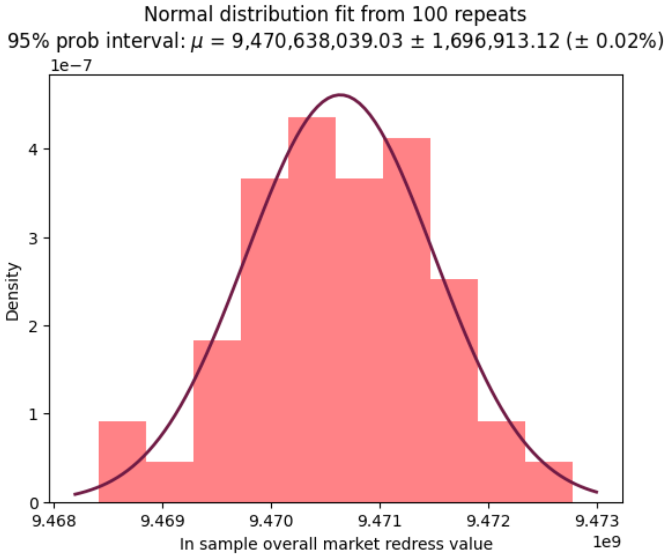
3.45 In addition to the key assumptions above the assignment of 2 of the 3 breach types in the DD1 agreement level data depend upon an element of randomisation within the model, as set out in detail in Annex 3. This necessarily varies from model run to model run, producing a variation in the lender level liability estimates for a given set of assumptions and threshold values.

3.46 As a result of the above, each time the redress model is run, a different set of agreements from DD1 are assigned to these 2 types of unfair relationship, and the overall liability outputs change accordingly.

3.47 For this reason, we have tested the sensitivity of the model to the randomisation required to assign these breaches into DD1, detailed in Annex 3. To enhance our confidence in the outputs of the redress liabilities model, the same thresholds and assumption values have been used in 100 otherwise identical model runs. The variation in the key output figures across this multitude of repeats enables us to estimate the uncertainty in our outputs.

3.48 This sensitivity analysis implies that the in-sample market wide redress outputs are stable to within ±£2 million of the £9.5 billion total across over 100 model repeats. The figure below shows a histogram of this data against numerical value, with a normal distribution fit applied. The uncertainty is being calculated as 1.96 x standard deviation of the fitted curve for a 95% interval.

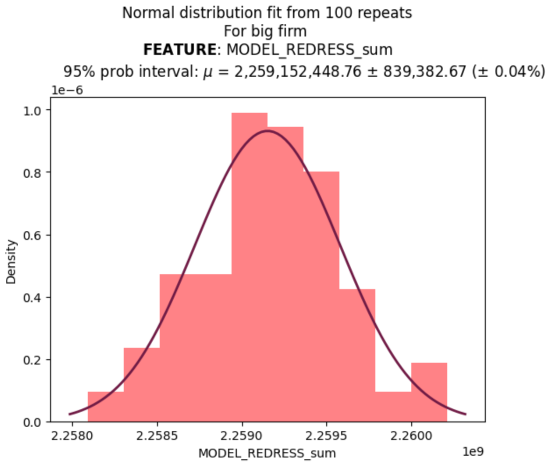
Figure 14: Distribution of market redress estimates

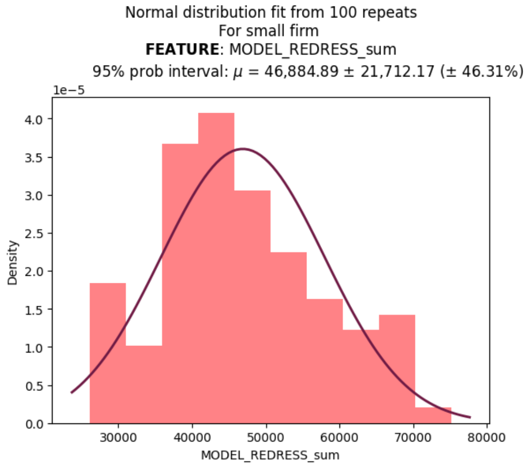


3.49 This stability is also present in the in-sample market wide median redress output of £561.97 ± £6.67 after 100 re-runs.

3.50 The replicability of outputs does however differ between large and small lenders in our data. For the very large lenders whose full population data dominates DD1, a sufficient number of agreements are reliably picked as part of the random allocation of the unfair relationships that their run-to-run variation is much smaller compared to a very small lender who may have as little as a few thousand total agreements. For these smaller lenders, there is a much lower chance that they will receive a tied arrangement allocation and therefore tend to exhibit a larger variation in outputs albeit centred on a liability that is correspondingly lower in value overall. Figures 15 and 16 below show this contrast between a large lender, and a small lender in their overall redress outputs. The lender specific redress in these instances are £2.26bn ± 0.04% and £47,000 ± 46% respectively.

Figure 15: Distribution of redress estimates for example lenders





3.51 We conclude that given the stability of the overall in-sample market wide liability figures, and the comparable stability of (large) lender specific redress estimates, we have confidence in the stability of the redress liability model with respect to variation introduced as a product of the random allocation of two of the three unfair relationships.

Reweighting to estimate market-wide redress liability

3.52 As outlined in the Data Guide, we applied a stratified random sampling approach to the DD1 dataset. Lenders were first placed into strata, as detailed in the Data Guide. Within the strata which did not include the full population of firms, lenders were then randomly sampled for inclusion in the analysis, following the procedure advised by an external statistician. In some cases, selected lenders were subsequently deemed to be out of scope as they did not offer any commission arrangements.

3.53 To ensure that our estimates are representative of the market, we have reweighted our analysis based on the sample selected compared to the population of lenders in the relevant stratum, as advised by an external statistician.

3.54 To do this we calculate stratum specific weights based upon the size of the sample selected relative to the total population of lenders within that stratum. For Stratum 0 and Stratum 1 we have data for all lenders, therefore the stratum weight is equal to 1.

3.55 To account for lenders excluded as they are out of scope (for example, they did not have any commission paying arrangements), we adjusted the total number of lenders in each stratum by the in-scope rate, defined as the proportion of selected lenders that were confirmed to be within scope. This adjustment ensures that the weighting reflects the actual population of eligible lenders.

Approach to calculating stratum weights

3.56 In line with advice from the external statistician, weights were calculated to reflect the inverse probability of selection within each stratum, adjusted for the in-scope rate. The steps are as follows:

- **Determine the in-scope rate** for each stratum:

$$In\ scope\ rate_h = \frac{\text{Number of in} - \text{scope lenders selected in stratum h}}{\text{Total number of lenders selected in stratum h}}$$

- **Adjust the total number of lenders** in each stratum:

$$Adjusted\ total\ lenders_h = \text{Total number of lenders in stratum h} \times In\ scope\ rate_h$$

- **Calculate the selection probability** for each lender in stratum *h*:

$$p_h = \frac{\text{Number of lenders selected in stratum h}}{Adjusted\ total\ lenders_h}$$

- **Compute the weight** for each lender in stratum *h* as the inverse of the selection probability:

$$w_h = \frac{1}{p_h}$$

3.57 Table 30 below summarises these calculations.

Table 30: Summary of stratum weightings applied to DD1

Stratum	Number of agreements	Total number of lenders	Adjusted total number of lenders	Number of lenders in DD1 sample	Stratum weighting (w_h)	% of agreements
0(s166)	S166 lenders	10	10	10	1.00	58.14%
1	Above 100,000	9	9	9	1.00	26.80%
2	10,000 - 99,999	21	13	5	2.63	13.63%
3	1000 - 9,999	22	11	5	2.20	1.27%
4	100 - 999	39	11	5	2.17	0.16%
5	10 - 99	23	0	0	N/A	0.002%
6	Up to 9	15	0	0	N/A	0.000%
Total		139	53	34*		100%

Notes: Of 36 lenders sampled, two failed to submit any data. Therefore, the total number of lenders in DD1 with valid submissions is 34. '% of agreements' reflects the number of agreements within each stratum out of the estimated total number of outstanding agreements in June 2024.

3.58 To produce estimates that are representative of the market, we append the weights (wh from Table 30) to the aggregated lender-level data using the stratum assigned to each lender. A column is then created to indicate whether each row corresponds to in-sample or out-of-sample lenders. For existing rows, this column was set to 'in sample'.

3.59 The three columns that require reweighting to reveal their out of sample counterparts are the total number of agreements, the total number of eligible agreements and the total redress. We calculate out-of-sample values by subtracting the in-sample amounts from the product of those amounts and the corresponding weights. For example, out of sample stratum 2 total agreements = (in sample stratum 2 total agreements)($w2 - 1$). This yields the additional entries attributed to the wider market beyond our DD1 data for that stratum. In the aggregated output, out-of-sample estimates are distinguished from in-sample estimates.

3.60 Aggregated outputs of the out-of-sample estimates alongside the in-sample estimates are representative of over 99%[33] of the market (using outstanding loan values from June 2024). In the dataset, out-of-sample estimates are flagged to distinguish them from the original in-sample data.

Key variables

3.61 The outputs of the reweighted lender level liability estimates comprise the following key variables:

[33] Adding up the % of agreements column from Table 30 for strata 0 to 4 is representative of over 99% of the market (using outstanding loan values from June 2024). Further detail on the underlying data can be found in the Data Guide.

Table 31: Key variables used in the reweighted lender level liability estimates

Variable name	Definition
Total number of eligible agreements	The total number of agreements that were signed in a given year that have at least one type of unfair relationship breach and are consequently due redress.
In or out	Inclusion status. It is a categorical variable, encoding whether the lender is part of the DD1 data, or if the data represents the estimated out of sample contribution.
Firm name	A unique lender identifier.
Year	Calendar year of observation (2007-2024).
Firm stratum	Indicate variable (numeric 0-4), denoting lender-specific classification.
Total number of agreements	The total number of agreements that were signed in a given year.
Total redress	The sum of all redress values for eligible agreements for a given lender, and or year grouping.

Limitations of analysis

3.62 In preparing these estimates, we have necessarily relied on a set of assumptions to address gaps in the underlying data. These assumptions reflect what we believe is a proportionate and evidence-based approach. We have also identified mitigation strategies to manage potential risks or limitations. These strategies ensure that, despite data constraints, the estimates remain sufficiently robust.

3.63 A general caveat is that the stratification of DD1 is based on the number of outstanding agreements per lender as of June 2024. This presents a limitation to the analysis, as it implicitly assumes that the market structure observed in 2024 is representative of the entire period under review. In reality, the true sampling weights for years prior to 2024 are unobserved, and the 2024 weights have been extrapolated retrospectively.

3.64 As described above, during the sampling of DD1, lenders which did not have any commission arrangements were considered out of scope. This means when re-weighting our estimates the final figures are representative of 99% lenders which have provided commission arrangements. Therefore, our sample may underrepresent agreements which did not pay commission. As UR3 does not require commission to be present for a breach to have occurred our estimate of redress owed may underestimate the true level associated with this type of unfair relationship. The impact of this on total liability estimates is limited as we have population data for around 89% of the market (based on outstanding loan agreements as of June 2024) and the strata which require scaling are the relatively smaller lenders, with less agreements.

3.65 Addressing these limitations of DD1 would require the collection of a substantial volume of historical data from lenders, which we consider would not have been proportionate to the aims of the analysis and would have created unnecessary additional burden.

3.66 It is important to note that the market has grown significantly in this period, with volumes more than doubling and overall value tripling since 2007. This expansion is also reflected in the DD1 dataset. While we acknowledge this limitation, we consider the approach to be proportionate and a reasonable approximation of the market and potential redress exposure, for the following reasons:

- Many legacy loan books have been acquired by lenders currently operating in the market.
- The DCA ban introduced in 2021 appears to have prompted changes in lenders' charging structures—reflected in the DD1 dataset—without materially altering the lender-level market composition (for example, through market exit).
- Lenders that no longer exist are not expected to pay redress, unless their loan books were acquired by another organisation.
- The changes in market size we observe in other data over time are also observed in DD1.

3.67 Another limitation is missing values in our data. To address this, we applied a partition-averaging approach, as we set out in the Data Guide. This method allows us to retain as many observations as possible and avoids dropping valuable data. For some variables, such as 'loan value', 'commission amount', and 'APR', values of 0 allowed to persist through the model to contribute to the total number of agreements per lender, but not contribute to the lender level redress values themselves.

3.68 Although these imputations provide a consistent and transparent way of handling missing information, they also introduce an element of approximation. In particular, the approach may smooth over genuine differences between lenders or across years and estimates may be sensitive if the pattern of missing variables is not random.

3.69 As set out in more detail above, elements of how breach rates are assigned to specific agreements rely on random allocation which introduces a level of uncertainly. Smaller lenders with less agreements, expectantly have a much lower chance that they will receive a tied arrangement allocation and therefore tend to exhibit a larger variation in outputs albeit centred on a liability that is correspondingly lower in value overall. Further, their generally small size may also mean that they may have been less likely to have such relationships.

3.70 For modelling purposes, we have assumed a redress payment date of 31 December 2026. In practice, payment dates will vary across agreements and lenders. This means the compensatory interest calculation will only run up to the actual date redress is paid and not what has been assumed for simplicity purposes within the modelling.

Non-redress cost estimates

Summary

4.1 This section sets out our analysis to estimate, where possible and proportionate, specific costs of setting up an industry-wide consumer redress scheme. We compare these costs to a counterfactual scenario outlined in the consultation CBA.

4.2 Costs can be categorised into lender and consumer costs. Our monetised costs to lenders comprise of costs associated with in-house complaints-handling and costs associated with the handling of complaints referred to the Financial Ombudsman. Our monetised costs to consumers comprise of time and effort costs. The data we have used to estimate these costs comprises lender-level agreements data (including the number of agreements that meet the criteria of an unfair relationship), lenders' own responses to questions asked in our motor finance commission monitoring survey, and various pieces of desk-based research using publicly available information.

4.3 We consider two intervention options (options 1 and 2) against the counterfactual scenario, considering the net effect of each against the counterfactual. Option 1 involves a CRS covering agreements from 6 April 2007 to 1 November 2024 whereas option 2 involves a CRS covering agreements from April 2014, with a complaints-led process from April 2007 to April 2014. In summary, we find that the total net benefit from non-redress impacts of operating a CRS under intervention option 1 compared to the counterfactual is £6.7bn (in nominal terms), and the total net benefit from operating a CRS under intervention option 2 compared to the counterfactual is £5.2bn (in nominal terms). A CRS from 2007 is therefore the preferred intervention option considered in this CP's accompanying CBA.

4.4 Table 32 below summarises the quantified non-redress impacts under each option.

4.5 All estimates except th "other lender costs" are obtained from our non-redress costs model. This interacts lender-level inputs (such as the wages of complaints-handlers) with market-level inputs (such as the CRS join rate) to obtain lender-level estimates. We scaled our estimates up to account for out-of-sample lenders by placing lenders into a set of strata and conducting a stratum-level uplift, providing us with market-level estimates. Key market-level assumptions which our analysis is sensitive to include the Financial Ombudsman referral rate and the number of complaints which are registered. The "other lender costs" were estimated through our Standardised Cost Model (SCM). We provide more detail on the methodology behind our cost estimates below.

Table 32: Summary of quantified non-redress impacts (central estimates, nominal)

	Counterfactual	Option 1 – Intervention	Option 2 – Intervention
Variable administrative costs	£683 m	£878 m	£807 m
One-off investments	£27 m	£108 m	£108 m
Screening costs	£0 m	£797 m	£568 m
Other lender costs*	£0 m	£3 m	£3 m
Total administrative costs	**£709 m**	**£1,786 m**	**£1,486 m**
Financial Ombudsman fees (disagree with final redress determination)	£3,164 m	£866 m	£1,572 m
Financial Ombudsman fees (disagree with redress determination from initial screening)	£0 m	£105 m	£83 m
Financial Ombudsman fees (out of time)	£1,917 m	£0 m	£221 m
Total Financial Ombudsman referral fees	**£5,081 m**	**£971 m**	**£1,876 m**
Financial Ombudsman scaling fees	£3,556 m	£0 m	£853 m
Total Financial Ombudsman fees	**£8,637 m**	**£971 m**	**£2,728 m**
Total lender costs	**£9,346 m**	**£2,758 m**	**£4,214 m**
Consumer time costs	£225 m	£151 m	£166 m
Total costs	**£9,572 m**	**£2,906 m**	**£4,380 m**

* These include other direct costs for lenders as a result of the redress scheme including familiarisation, training and dissemination and Board and Executive Committee review.

4.6 We present estimates of the benefits of our proposed intervention against the counterfactual, a sensitivity analysis of how the magnitude of these impacts could change, and the present value (PV) and the net present value (NPV) of these options in the CBA. Non-quantified costs are also assessed in the CBA, such as the potential impact of complaints which go through the legal system instead of through a CRS.

Data sources

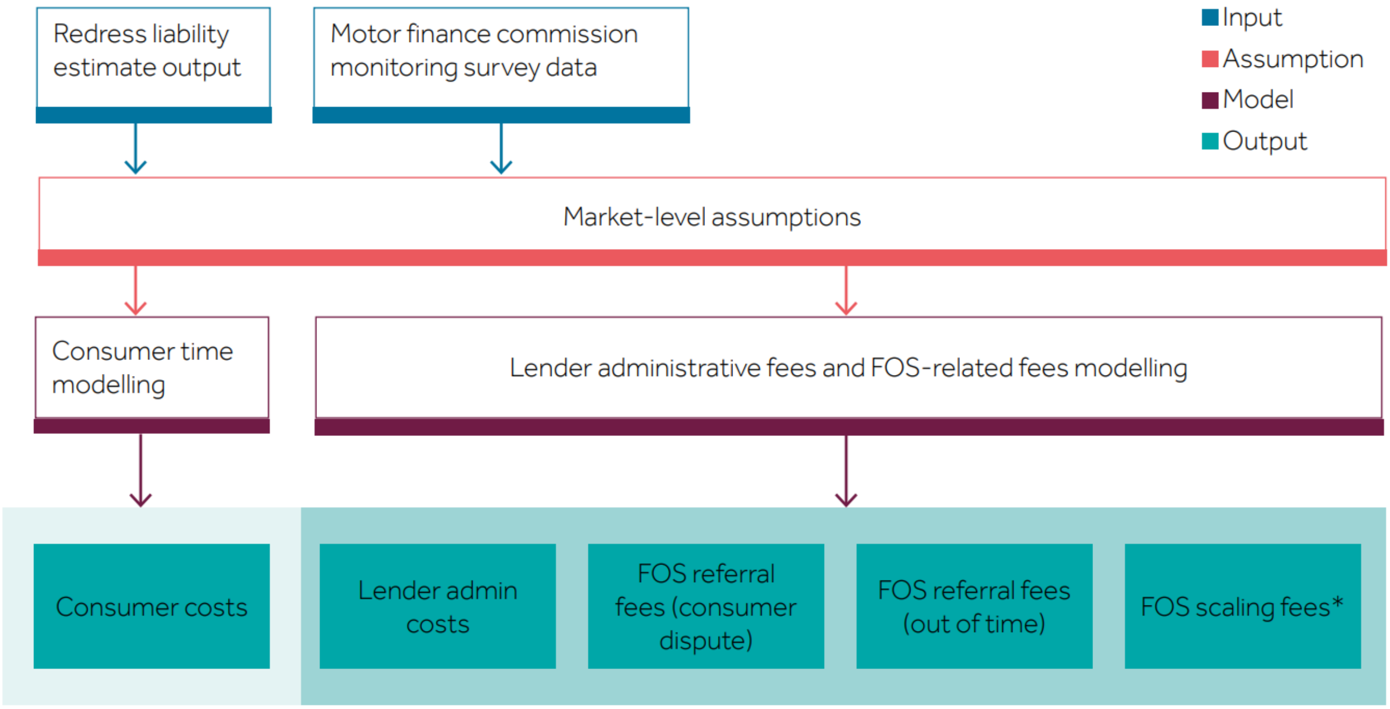
4.7 We draw on 2 main sources of data (see Data Guide):

- The output from the modelling of redress liability estimates that uses DD1 (agreement numbers).
- Motor finance commission monitoring survey

Overview of modelling approach

4.8 A visual depiction of the high-level modelling approach to non-redress costs is shown in the figure below and applied to both the counterfactual and intervention scenarios. Data inputs are at the lender-level, with market level assumptions applied to obtain lender-level lender cost outputs and market-wide consumer costs outputs.

Figure 16: Overview of modelled non-redress cost inputs and outputs associated with the CRS



* Financial Ombudsman scaling fees are assumed to be £0 under our proposed intervention.

4.9 The 3 types of costs modelled quantitatively under both the counterfactual and intervention scenarios are:

- Lender administrative costs associated with handling complaints from receipt to resolution. The modelled lender administrative costs also include one-off investments required and complaint screening costs.
- Financial Ombudsman related costs to lenders, which are charges levied by the Financial Ombudsman for dealing with complaints, primarily FOS referral fees. When modelling the counterfactual, we also include FOS scaling fees.
- Consumer costs, which are costs to consumers associated with the time taken to submit and deal with complaints.

4.10 The potential Financial Ombudsman related costs incurred by lenders are:

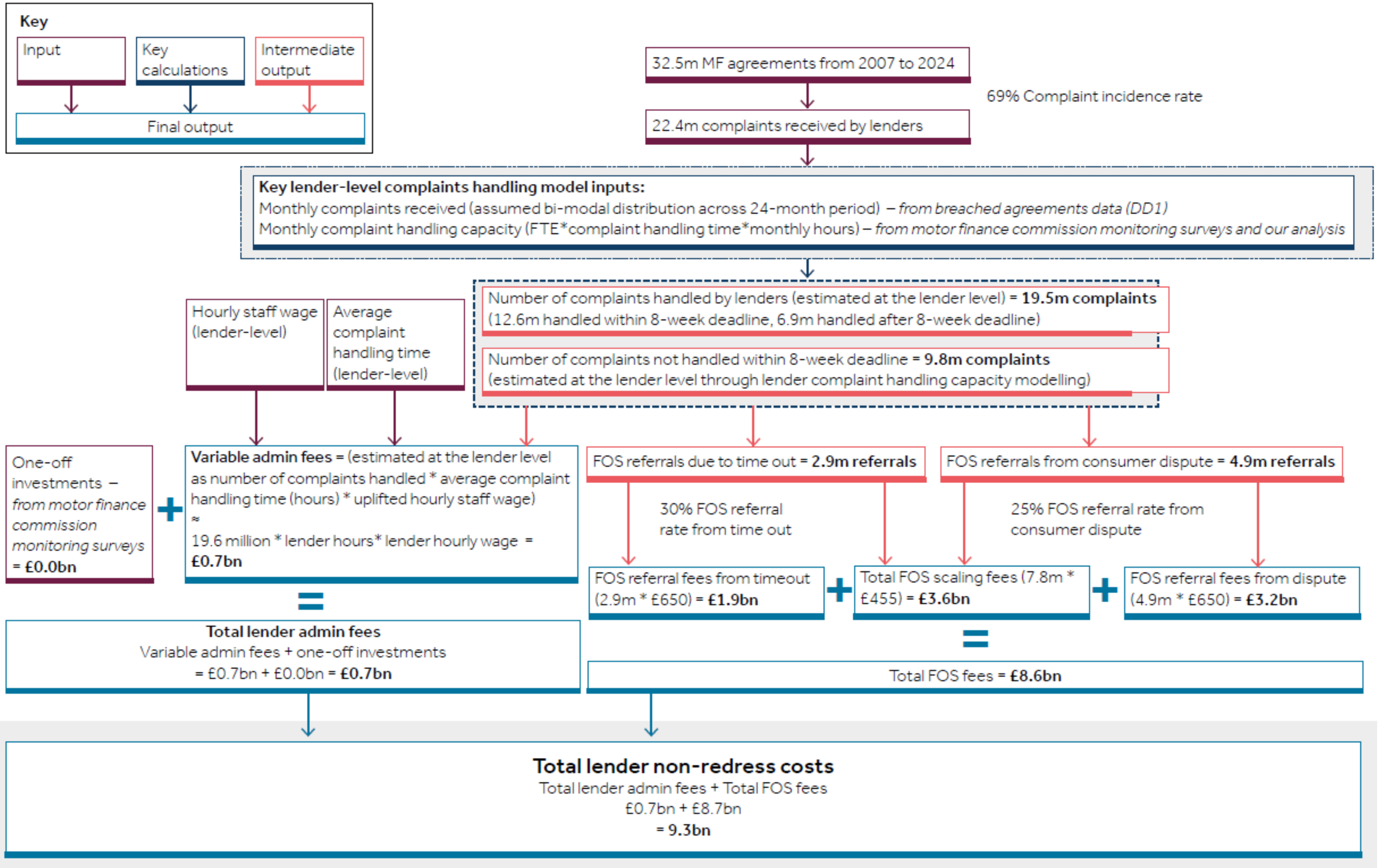
- Financial Ombudsman referral fees. These are costs associated with complaints that are referred to the Financial Ombudsman. Complaints become eligible for Financial Ombudsman referral if either i) the complaint has not been resolved by the lender within the response deadline; or ii) the consumer disagrees with the outcome of the redress determination.
- Financial Ombudsman scaling fees. These are indirect costs that are incurred as a result of the Financial Ombudsman being required to scale its operations to deal with the sharply increasing number of complaints received. These are

assumed to be indirect costs which are charged to all lenders within the Financial Ombudsman's remit as part of their annual Financial Ombudsman levy. The Financial Ombudsman has not confirmed (nor has it denied) that any such scaling fees will be charged directly to the lenders involved in motor finance redress.

4.11 The model works by estimating lender administrative costs and Financial Ombudsman related fees at the lender-level, scaling these to the market-level, and then applying consumer costs separately based on the total number of complaints made across the market, to arrive at an estimate for market-wide modelled non-redress costs.

4.12 The figure below presents the modelling process for counterfactual lender cost calculations. The total lender non-redress costs in the counterfactual central case are estimated to be £9.3 billion, comprised of £0.7bn million in administrative costs and £8.6 billion in Financial Ombudsman related fees.

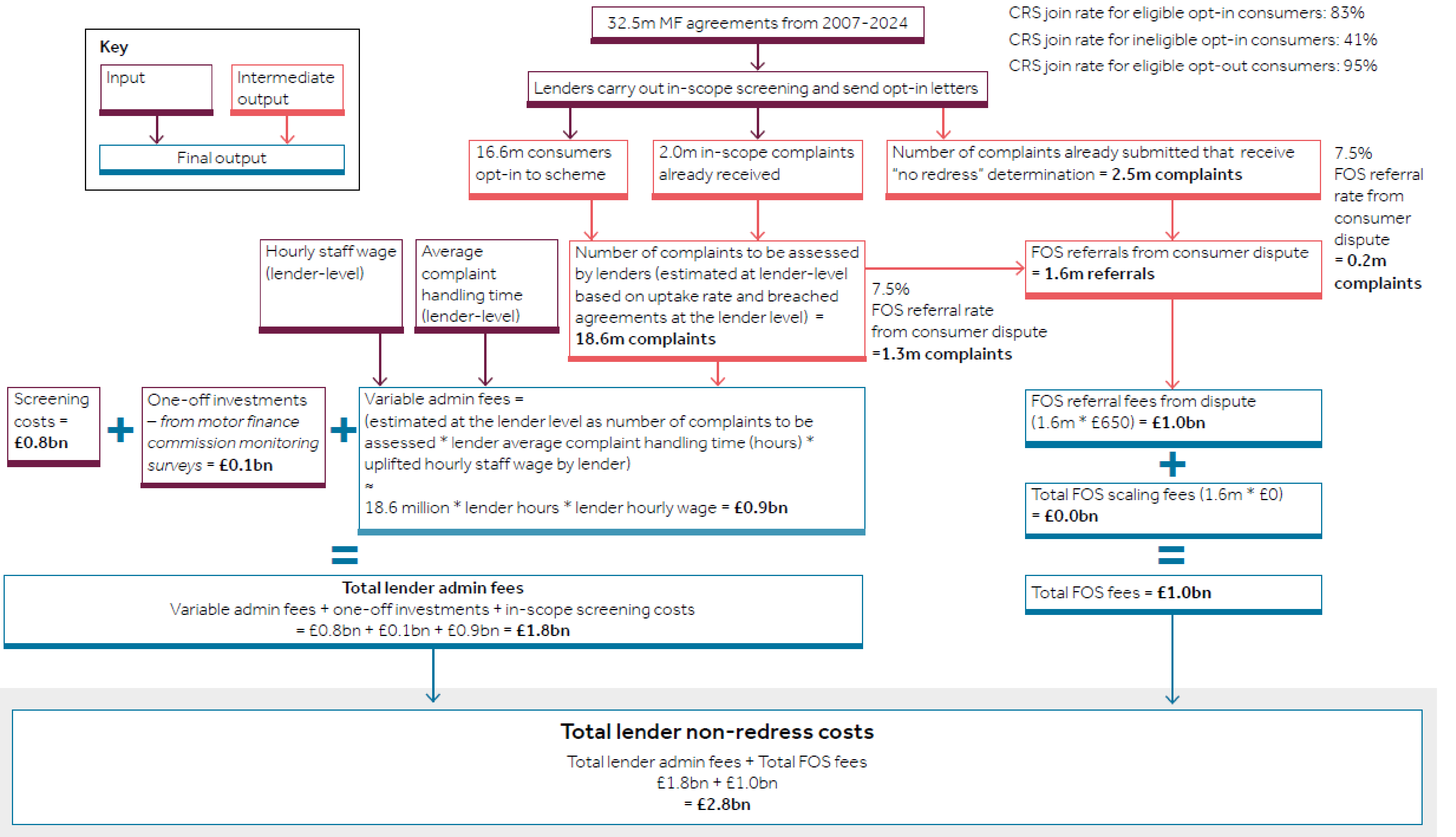
Figure 17: Illustrative diagram of counterfactual lender cost calculations (nominal estimates)



Note: Figures provided to 1 decimal place.

4.13 The figure below presents the modelling process for the intervention lender cost calculations. The total lender non-redress costs in the intervention central case (a CRS from 2007 onwards) are estimated to be £2.8 billion, comprised of £1.8 billion in administrative costs and £1.0 billion in Financial Ombudsman related fees.

Figure 18: Illustrative diagram of intervention (option 1) lender cost calculations (nominal estimates).



Note: Figures provided to 1 decimal place.

4.14 The starting point for both lender non-redress cost models is the total number of motor finance agreements held across the market from 2007 to 2024, estimated at 32.5 million agreements.

4.15 For the counterfactual model, a complaint incidence rate of 69% is applied to the total number of agreements at the lender level. After the complaint incidence rate is applied, an expected 22.4 million complaints are received by lenders (see Table 36 for justification of the complaint incidence rate assumption). Lender-level complaint handling modelling is carried out to estimate the number of complaints that can be handled by lenders within the 8-week complaint response deadline, and the number of shortfall complaints which cannot be handled within the deadline. These lender level estimates feed into the estimation of Financial Ombudsman related fees and administrative costs. It is assumed that the only administrative costs lenders incur are the variable administrative fees associated with case handling. It is assumed that the Financial Ombudsman related fees incurred by lenders arise from Financial Ombudsman referral fees from both case time out and consumers disagreeing with case outcomes, as well as Financial Ombudsman scaling fees.

4.16 For the intervention model, it is assumed that lenders carry out an initial screening process for all agreements to assess whether they are likely to be eligible or not for redress. For complaints already received, if they are deemed eligible, they will proceed through an opt-out process, and if they are deemed not eligible, they will receive a "no redress" determination at this stage, with the right to refer their determination to the Financial Ombudsman. For all other agreements, they will enter a firm-led opt-in scheme and receive an opt-in letter which communicates whether the case is likely to be eligible for redress or not. The CRS join rates are assumed to vary for each of these case groups and estimates for the number of cases that go through each channel are calculated at the lender level. The overall estimated CRS join rate for consumers with breached agreements is 84.7% - see Table 37 for full justification of CRS join rates. Following the screening and opt-in/opt-out process, it is assumed that 18.6 million cases will proceed to full case assessment, with 2.5 million complaints that have already been received falling out at this stage due to being identified as ineligible for redress. These lender-level case estimates feed into the estimation of Financial Ombudsman related fees and administrative costs. The administrative costs incurred by lenders include the lender level screening costs required to assess all agreements, one-off investment costs to prepare for the CRS, and the variable administrative fees associated with full case handling. It is assumed for modelling purposes that the Financial Ombudsman related fees incurred by lenders arise only from Financial Ombudsman referral fees as a result of consumers disagreeing with case outcomes, given the complaint determination timelines will be determined by CRS rules rather than those within DISP.

4.17 Based on the lender-level modelling summarised above, we estimate the number of cases where consumers complain to the lender only, where consumers complain to both the lender and the Financial Ombudsman, and where consumers complain through a professional representative. The estimated average time spent by a consumer through each of these complaint pathways is estimated and the time value of £7.57 per hour is used to estimate the total value of consumer time spent dealing with complaints. In the counterfactual scenario, it is estimated that consumers spend a total of 30 million hours dealing with complaints, which equates to a value of time estimate of £225 million. In the preferred intervention scenario, it is estimated that consumers spend 20 million hours dealing with complaints, which equates to a value of time estimate of £151 million.

4.18 Some costs are accounted for outside this model. These include additional compliance costs that lenders may incur, and costs to the FCA. Further details on these costs and other costs qualitatively assessed are provided in the *Additional costs not modelled section* below.

Model inputs

4.19 We combine the expected number of complaints (from the output of redress liability estimates) with lenders' expected complaint-handling capacity (from our Motor finance commission monitoring survey). This allows us to model the expected complaint handling capacities of lenders to calculate the number of complaints handled and the number of complaints not able to be handled in the relevant timeframes. We use these figures to estimate administrative costs and Financial Ombudsman fees incurred by each lender.

4.20 The modelling of these lender-level costs is based on:

- The output from the modelling of redress liability estimates that uses DD1. DD1 data provides lender-level agreement numbers for 34 lenders and, after reweighting, is representative of over 99% of the market.
- Data received from our motor finance commission monitoring survey, collected in May and August 2025, which surveys 34 lenders on a range of cost factors including one-off investments, FTE and wages. After reweighting, the survey is representative of over 99% of the market.

Redress liability estimates

4.21 We use total agreements and agreements that meet the criteria of an unfair relationship by year, lender and strata from our Redress liability estimates as our main inputs to our model. See Redress liability estimates for this analysis. These estimates are based on DD1 data (see Data Guide) and reweighted to represent over 99% of the market using outstanding loan values as of June 2024. They quantify redress liabilities, total agreements and agreements that meet the criteria of an unfair relationship at lender, stratum and market level.

Motor finance commission monitoring surveys

4.22 The results of the motor finance commission monitoring surveys inform understanding of lenders' complaint handling capacity and ultimately the administrative costs associated with complaint handling. See the Data Guide for the specific questions asked.

4.23 The lender-level survey responses used from the motor finance commission monitoring survey in the counterfactual and intervention modelling are:

- One-off investments (fixed costs)
- Complaint handling FTE
- Time taken to process a single complaint
- Average hourly cost per staff member

4.24 We provide a summary of lenders' own responses in table below.

Table 33: Lenders' responses to our survey questions.

Element used in our modelling	Lowest	Median	Highest
One-off investment	£0.0 m	£0.25 m	£25.0 m
FTE complaints-handling employees*	0.4	22	7,515
Complaint processing time, intervention (excluding complaint screening)	4 minutes	75 minutes	420 minutes
Complaint processing time, counterfactual**	4 minutes	60 minutes	350 minutes
Hourly cost of complaints-handling employees (including overheads)	£17.38	£29.58	£84.70

Note: Where lenders have provided a range of estimates, we have taken their mid-point for this exercise. For our Round 5 survey estimates, we use our Round 4 survey estimates for at least 1 response for 12 lenders, and median values for at least 1 response for 1 lender. For our Round 4 survey estimates for the complaint processing time, we use the Round 3 survey estimates for 2 lenders, and the median for 1 lender.
*Lenders which provided FTE responses which included wider teams (such as operations and programme support) have been excluded from this exercise.
**This question was asked in our May 2025 survey, prior to the Supreme Court decision. Where lenders have provided different responses to their DCA and non-DCA responses, we take an average of the lower and upper bound provided for both.

4.25 The input used from the motor finance commission monitoring survey data for the intervention only is the estimated average time to process a single complaint under a potential CRS. For the counterfactual, we use results from our previous motor finance commission monitoring survey. In May 2025, prior to the Supreme Court's Decision, we asked lenders how many minutes they expect their staff to spend processing DCA and non-DCA complaints from initial receipt to resolution. Through these two questions, we were able to gauge the time it would take for one complaints-handler to handle one complaint, at the lender level, for both a complaint in the counterfactual and under our proposed intervention.

4.26 We modelled the time taken to process a single complaint under a potential CRS at the lender level because we noticed a high variance between lenders. For instance, lenders within the survey had various levels of process automation (including planned process automation).

Combining data sources and scaling to market-level

4.27 By combining the motor finance commission monitoring survey data with the number of lender-level agreements from the redress liability output, and a set of market-level assumptions, we model the volume of complaints received and handled by individual lenders across the market.

4.28 The lender-level data obtained from both the motor finance commission monitoring survey and the redress liability output (from DD1) data contain information for different lenders, with a crossover of 22 lenders for which we receive information from both data sources. The table below summarises the number of lenders within each stratum that are contained within each data source. Both the motor finance commission monitoring sample and DD1 sample contain all lenders within Stratum 0 and Stratum 1. For the remaining stratum, weights are assigned to in-sample estimates to estimate the out-of-sample estimates across each stratum to provide market level outputs.

Table 34: Summary of count of lenders by stratum in each data source

	Total lenders	Lenders in DD1 sample	Lenders in motor finance commission monitoring sample	Lenders in BOTH DD1 and motor finance commission monitoring
Stratum 0	10	10	10	10
Stratum 1	9	9	9	9
Stratum 2	21	5	11	3
Stratum 3	22	4	4	0
Stratum 4	39	4	0	0
Stratum 5	23	0	0	0
Stratum 6	15	0	0	0
SUM	139	32	34	22

4.29 Unlike the DD1 dataset, lenders included in the motor finance commission monitoring survey were not randomly selected within each stratum. Instead, they were chosen to provide broad market coverage. Nonetheless, the data provides good coverage across stratum 2 and some coverage in stratum 3. Given this, we believe there is a strong case for weighting by stratum:

- for consistency with our overall approach,
- because it is likely to yield more accurate results than simple scaling.

4.30 The DD1 dataset also results in good representation across the strata. The motor finance commission monitoring survey has complete population data for those lenders in strata 0 and 1, improved coverage in stratum 2, and equivalent coverage in stratum 3. There is no coverage for stratum 4 in the motor finance commission monitoring survey data; however, this stratum accounts for only 0.01% of the market and has therefore been excluded.

4.31 We use the available data for the 22 lenders in both the DD1 and motor finance commission monitoring samples as our in-sample lenders. These lenders cover c.88% of the market by outstanding loan agreements as of June 2024.

4.32 To estimate the number of agreements that meet the criteria of an unfair relationship held by out-of-sample lenders in strata 2, 3, and 4, we combine two sources:

- actual agreement counts from lenders in the DD1 sample (excluding those in the motor finance commission monitoring survey sample), and
- weighted estimates for the remaining out-of-sample lenders in each stratum.

These figures are derived from the redress liability estimates

4.33 For the motor finance commission monitoring survey data, we use all the available data held within the sample to estimate the out-of-sample figures. The variables estimated are the number of FTE working on complaints, fixed investment costs, screening costs, and the variable administrative cost per complaint. For the FTE and fixed investment costs, we estimate out-of-sample figures (estimates for lenders where *both* DD1 and motor finance commission monitoring data are not available) as set out below:

- *Stratum 2* – For the out-of-sample stratum 2 lenders (17 lenders) we aggregate the figures for the 8 lenders we have motor finance commission monitoring data for (but not agreement data for) and weight these outputs by 17/8=2.1 to arrive at the total estimates for out-of-sample stratum 2 lenders.
- *Stratum 3* – For the out-of-sample stratum 3 lenders (22 lenders) we aggregate the figures for the 4 out-of-sample lenders we have motor finance commission monitoring data for (but not agreement data for), and weight these outputs by 22/4=5.5 to arrive at the total estimates for out-of-sample stratum 3 lenders.
- *Stratum 4* – We have no stratum 4 lenders in the motor finance commission monitoring survey so exclude these agreements from our estimate. Stratum 4 represents less than 0.1% of the market so our estimates are representative of over 99% of the market.

Table 35: Summary of stratum weightings by for motor finance commission monitoring survey

Stratum	Total lenders	Lenders in both	Out-of-scope lenders *	Out-of-sample lenders **	Lenders in monitoring but not DD1	Weights applied to out-of-sample lenders
Stratum 0	10	10	0	0	0	0
Stratum 1	9	9	0	0	0	0
Stratum 2	21	3	1	17	8	2.1
Stratum 3	22	0	0	22	4	5.5
Stratum 4	39	NA	NA	NA	NA	NA
Stratum 5	23	NA	NA	NA	NA	NA
Stratum 6	15	NA	NA	NA	NA	NA

*The out-of-scope stratum 2 lender merged with a lender in stratum 0, so the motor finance commission monitoring data and the number of agreements attributed to this lender, have instead been incorporated into stratum 0 estimates.

**Out-of-sample lenders comprise those which we do not have a complete set of data for (i.e., we do not have DD1 data and/or motor finance commission monitoring data).

4.34 For the variable administrative cost per complaint, we assume that the out-of-sample figures for each stratum are the median across the lenders of that stratum for which we have motor finance commission monitoring data.

4.35 Adding together the in-sample and out-of-sample estimates across each stratum provide estimates at the market level. These estimates are broadly representative of over 99% of the market. We believe that applying our strata analysis is more accurate than a simple scaling exercise because our samples are weighted towards large lenders.

4.36 As we are consulting on our proposal, lender-level survey estimates used as inputs to inform our analysis are based on lenders’ expectations prior to this consultation. We look forward to receiving further feedback during the consultation.

Assumptions

4.37 The tables below set out the assumptions used to model costs under the counterfactual scenario the intervention scenarios. The tables provide low case, central case and high case assumptions, along with reasoning for the choice of each assumption.

Table 36: Quantitative assumptions used to model the counterfactual scenario

Input	Low	Central	High	Reasoning
Complaint incidence rate	59%	69%	79%	The complaint incidence rate refers to the occurrence of complaints relative to the total population of agreements. The lower bound of the estimated complaint incidence rate for a complaints-led CRS is based on Figures 1 and 2 of a series of six PPI ComRes surveys. ComRes surveyed 20,001 UK adults aged 18+ between 6 March and 7 September 2015 and found that as of the end of the survey, 47% of eligible consumers had already registered a complaint about PPI. A further 12% indicated that they intended to complain prior to the deadline. We believe that more than 12% eventually did complain, as Annex F of our PPI complaints deadline final report suggests that a substantial proportion of complaints for PPI were registered after 2015. Therefore, our lower bound is based on the 47% of eligible consumers who had already registered a complaint about PPI, plus the 12% who indicated that they intended to complain (59%). In the ComRes surveys, 16% of respondents stated that they did not know whether they would register a complaint at the time. If we assume that 59% of these 16% of respondents registered a complaint (in line with the proportion of consumers who had already registered a complaint about PPI plus the proportion who intended to complain), then the complaint incidence rate becomes just under 69%. We use 69% as our central estimate. We add the difference between the low and central estimates to the central estimate to attain our high estimate. We also considered the uptake rate from the British Steel Pension Scheme, which was a S404 opt-out CRS (74.4%). It was calculated using Figure 3.2 of our corporate document on actions from the FCA, Financial Ombudsman, and the FSCS for BSPS holders. This does not include consumers who had proactively complained to Financial Ombudsman before the redress scheme started or consumers who were lost to 'attrition' through the running of the scheme. Our research suggests

Input	Low	Central	High	Reasoning
				that S404 opt-out redress schemes have the highest uptake rate and complaint incidence rate, as consumers are not required to initiate the complaints process.
Financial Ombudsman referral rate (out of time)	10%	30%	50%	We assume a Financial Ombudsman referral rate (out of time) ranging from 10% to 50%, with a central estimate of 30%. The Financial Ombudsman referral rate (out of time) may be driven higher as consumers who have already initiated a complaint may be more inclined to follow up after the 8-week deadline, particularly with the complaint still fresh in mind. Referral rates will also depend on how professional representatives engage with the scheme. We expect the new professional representative Financial Ombudsman fees to reduce early referrals, as professional representatives are likely to wait for lenders to review complaints before approaching Financial Ombudsman to avoid incurring these fees. The absence of our guidance in the counterfactual will create uncertainty about timeframes, which could increase referrals. This uncertainty is why our range is relatively wide.
Financial Ombudsman referral rate (consumer disagrees)	10%	25%	40%	Under the counterfactual scenario, lenders would not be required to follow a prescribed redress calculation methodology, unlike in the intervention scenario. This introduces uncertainty around what constitutes a breach and how redress should be calculated, which may erode consumer confidence in the process. A higher proportion of consumers may be dissatisfied with the redress offered and more likely to escalate their complaints to the Financial Ombudsman. Additionally, increased scope for professional representative involvement could further contribute to a rise in Financial Ombudsman referrals through consumers disagreeing with the outcome. We are uncertain about this assumption, so we apply a broad range of 10% to 40% to reflect the potential variability in outcomes.
Financial Ombudsman fee	£650	£650	£650	Standard FOS fee from the Financial Ombudsman's Plans and Budget Consultation 2025-26.

Input	Low	Central	High	Reasoning
Financial Ombudsman scaling cost per complaint	£455	£455	£455	We base the Financial Ombudsman scaling cost per complaint on the experience of PPI cases. In PPI, firms with over 25 referrals in a year paid an additional £350 per case. This reflected the actual costs incurred in handling a high volume of complaints. Since the PPI scaling fee was introduced, the Financial Ombudsman case fee has increased from £500 to £650, a 30% rise. We have applied this 30% increase to the £350 scaling cost, resulting in a revised figure of £455.
Firm response deadline	8 weeks	8 weeks	8 weeks	Standard deadline for firms to respond to complaints under normal complaints procedure.
Working hours per month	134.17	134.17	134.17	We assume that a complaints-handling employee would work 7-hour days for 230 days per year. 230 days per year is based on a 365-day year, with 104 weekend days, 31 days of holiday allowance (including bank holidays).
Variable cost per complaint non-labour cost	21%	21%	21%	Salary estimates are uplifted by 21.0% to account for non-wage labour costs. This is based on DBT analysis of the UK National Accounts and is consistent with the approach taken in other recent Government Impact Assessments.
Average time per agreement for one-off screening costs	N/A			There are no one-off screening costs required for lenders in the counterfactual because lenders would only be required to screen agreements related to complaints. This is covered in lenders' estimates for the time taken to process a complaint.
One-off investment for handling complaints	Calculated at the lender-level using motor finance commission monitoring survey results			We use our May 2025 lender-level survey results as an input, where we asked lenders to consider what one-off investments they expect to make in systems or infrastructure to manage complaints.

Input	Low	Central	High	Reasoning
Complaints-handling full-time equivalent (FTE) employees	Calculated at the lender-level using motor finance commission monitoring survey results			In August 2025, we asked lenders to consider how many FTE staff they expect to allocate to handling complaints for a potential consumer redress scheme following the SC decision. We decided to use August 2025 survey results instead of the results to the May 2025 survey (which asked for the number of FTE lenders expect to be assigned to handling DCA and non-DCA complaints before the SC and not taking into account a potential CRS), as the SC decision has impacted the scope of consumer complaints.
Hourly cost per complaints-handling employee	Calculated at the lender level using motor finance commission monitoring survey results			In August 2025, we asked lenders to consider the average total hourly cost per staff member assigned to complaint handling involved in a potential consumer redress scheme following the SC decision. We decided to use August 2025 survey results instead of the results to the May 2025 survey (which asked for the total hourly cost per staff member handling DCA and non-DCA complaints), as the wage data is more up-to-date.
Time taken to process a complaint	Calculated at the lender level using motor finance commission monitoring survey results			We use our May 2025 lender-level survey results as an input, where we asked lenders to consider how many minutes they expect their staff to spend processing a DCA and non-DCA complaints.
NPV annual discount rate	3.5%	3.5%	3.5%	The standard **3.5% discount rate** is applied to future costs and benefits as per HM Treasury's Green Book.
Average consumer complaint time (firm only), minutes	60	122.5	185	To obtain the consumer time estimates, we mapped possible complaint journeys which consumers could follow. We then assigned the amount of time which we expect consumers to take under each of these possible journeys. For each journey, we estimated an upper and lower bound for consumers. For example, a consumer at the lower end of the estimates would provide the right information to the lender when submitting a complaint, whereas a consumer at the higher end of the estimates would not provide sufficient information at the start such that the lender would have to contact the consumer.

Input	Low	Central	High	Reasoning
				We sense-checked our estimates with the lower bounds for the time estimates for a straightforward claim in our analysis published in CP21/1, and adjusted our estimates accordingly. Our lower bound estimate for a straightforward complaint was 1.5 hours in CP21/1, whereas our central estimate here is just over 2 hours.
Average consumer complaint time (firm and Financial Ombudsman), minutes	100	187.5	275	As above. We expect that consumers who subsequently complain to the Financial Ombudsman will spend more time making a follow-up complaint. We note that our time estimates in CP21/1 for Financial Ombudsman referral are higher than our estimates in this Annex. The low time estimate for a straightforward complaint in CP21/1 where the consumer refers their complaint to the Financial Ombudsman after complaining to a lender is 4.5 hours, whereas our central estimate here is just over 3 hours. This is because we then conducted the analysis at the consumer level, whereas here we attain estimates per consumer at the market level. We consider that it is unlikely that many consumers would spend the maximum amount of time for a straightforward complaint from our CP21/1 estimates.
Average consumer complaint time – using a PR/CMC, minutes	48	60	72	As above. For a consumer complaining through a professional representative, we expect there to be less variation between consumers. This is because the professional representative will handle the majority of the claims process without need for the consumer to act. Consumers may need to provide further information to the professional representative in order to progress the complaint. The lower bound for our time estimates in CP21/1 is identical to our central estimate in this analysis.

Input	Low	Central	High	Reasoning
Value of consumer time	£7.49	£7.57	£35.20	The Department for Transport (DfT) publishes the value of time for different types of consumers. The central assumption of £7.57 per hour is sourced from the TAG Data Book May 2025 using 2025 as the base year. Research we commissioned from Institute of Transport at the University of Leeds found that the DfT estimate is broadly transferable from the transport to finance sector and that it is the best available proxy in the absence of specific evidence on the value of time savings/losses in the finance context. This study also sought to build assurance around our practice of transferring values from the transport sector to the finance context and proposed a series of sensitivity tests which could be conducted around the DfT's estimates. For the low estimate we multiplied the DfT estimates by 0.9895 to adjust for the slight differences between representative samples of travellers and consumers of financial products/services. For the high estimate we multiply the DfT estimate by 4.65 as it is the upper bound of the sensitivity tests recommended. This sensitivity test is recommended when the monetary reward from spending time searching and switching financial products is estimated separately (for example, redress for consumers which we provide estimates for in Annex 7 of the CP).).
Proportion of complaints submitted through a PR/CMC*	61.0%	75.5%	90.0%	Our central estimate is derived from our motor finance commission monitoring survey. Responses from 30 lenders suggest that the majority of lenders have experienced 75.5% of their complaints so far from PRs/CMCs. The Financial Ombudsman announced that around 90% of motor finance commission cases were submitted by PRs in Q1 2024/25. However, PRs/CMCs are more likely than consumers who complain directly to lenders to refer complaints to the Financial Ombudsman, such that the proportion of complaints submitted to the Financial Ombudsman via a PR/CMC is likely to be higher than the proportion of complaints submitted to lenders via PR/CMC. As such, we take this as our upper bound estimate.

Input	Low	Central	High	Reasoning
				Our motor finance commission monitoring survey suggests that 75% of lenders have experienced over 66% of their complaints so far from PRs/CMCs. We deduct the difference between our central and high estimates from our central estimate to obtain our low estimate of 61%.

*This figure is not applied in the lender cost model – the only channel through which estimates of the proportion of complaints submitted through PRs/CMCs impacts costs is through the estimated impact on consumer time.

Table 37: Quantitative assumptions used to model non-redress costs in the intervention scenario

Input	Low	Central	High	Reasoning
CRS join rate for opt-in process	Eligible letter: 69% Ineligible letter: 27%	Eligible letter: 83% Ineligible letter: 41%	Eligible letter: 95% Ineligible letter: 55%	Consumers can be separated into 4 groups based on whether the consumer has already registered a complaint or not, and whether the lender concludes that the consumer is likely to be owed redress or not in its initial letter to the consumer. The former determines whether the consumer will be in the opt-in or the opt-out CRS, and the latter determines whether the consumer receives a likely owed redress letter (letter type A) or a unlikely owed redress letter (letter type B). We propose that consumers who receive letter type B in the opt-out process are not invited to join the CRS. Each of these consumer types is likely to have a different CRS join rate. We expect the CRS join rate for a S404 opt-in CRS to be higher than a complaints-led CRS. For the CRS join rate in the opt-in process, we consider the CRS join rate for those who receive letter type A and those who receive letter type B. We have considered the likely behavioural benefits for consumers receiving a letter informing them that they are likely to be eligible for redress, relative to the counterfactual. To estimate this effect, we have broken the potential behavioural benefits into two parts: (i) the benefits of receiving a direct communication versus not and (ii) the additional benefit of receiving a communication that informs the recipient that they are likely eligible for redress.

Input	Low	Central	High	Reasoning
				The behavioural benefits of receiving a direct communication versus not include: reduced effort, increased trust, and perceptions of endowed progress. These all may increase the CRS join rate. From a review of empirical evidence, the impact of these effects tend to fall within a range of approximately 4-15ppt. We've estimated the benefit near the mid-point of this range at 9ppt. We further reviewed the empirical evidence on the impact of 'eligibility' reporting in communications. While evidence is relatively sparse, especially in the finance domain, we find related evidence in other domains such as health. While the evidence is varied, evidence points towards a positive and modest increase, which we estimate to be a further 5ppt. This implies a total behavioural benefit of +14ppt relative to the equivalent cohort in the counterfactual (69% + 14ppt). This is our central estimate for those who receive letter type A. In reviewing relevant empirical evidence, we did not find direct evidence to measure the impact of behaviour on receiving a communication suggesting the recipient was unlikely to be eligible. As such, we have relied on broader behavioural science principles to inform the CRS join rate among these recipients. Loss aversion is a well-established finding in behavioural science, whereby consumers disproportionately dislike losses compared to comparable gains, although this can be greater or less under different circumstances. Reviews of many studies of loss aversion estimate an average ratio of 2:1, meaning losses are approximately twice as negative as gains are positive on consumer utility. Applied to these communications, this suggests the dissuasive impact of a "likely ineligible" communication will be stronger than the persuasive impact of a "likely eligible" letter. We apply a 2:1 ratio which assumes that a "likely ineligible" communication will be twice as impactful as dissuading consumers as the "likely eligible" communication persuades (i.e. that the dissuasive influence of being informed of unlikely eligibility is twice as impactful as the persuasive influence of being informed of

Input	Low	Central	High	Reasoning
				likely eligibility). We estimate that this will reduce the CRS join rate by 28ppt compared to the equivalent cohort of consumers in the counterfactual (69% - 28ppt). For our low estimate, we expect that those who receive letter type A would be more likely to register a complaint than our expectation for all consumers under the counterfactual. As such, our lower bound is 69%. Our low estimate for those who receive letter type B assumes that a "likely ineligible" communication will be 3 times as impactful at dissuading consumers compared to the "likely eligible" communication in our central estimate. As such, our lower bound decreases the CRS join rate by 42ppt compared to the equivalent cohort of consumers in the counterfactual. For our high estimate we expect that the vast majority of consumers receiving letter type A would join the CRS. We assume that 5% of consumers receiving the "likely eligible" communication would drop out of the CRS due to factors such as being uncontactable (e.g., moving house). For consumers receiving letter type B, we assume that the "likely ineligible" communication will be just as impactful at dissuading consumers from joining the CRS as the "likely eligible" communication is at persuading consumers to join the CRS. As such, our high estimate is 55% (69% - 14ppt). We note that communications from us, and any awareness campaign could impact the CRS join rate.
CRS join rate for opt-out process	Eligible letter: 95% Ineligible letter: 0%	Eligible letter: 95% Ineligible letter: 0%	Eligible letter: 100% Ineligible letter: 0%	We expect the CRS join rate among consumers who have already complained to lenders and are in the opt-out scheme to be 95% for those receiving letter type A. This is because consumers who have already complained are in the opt-out scheme (whether that be through a PR/CMC or directly). In addition to our expectation that the consumers are highly motivated (given their prior complaints absent any scheme), the use of an opt-out default (where consumers will commence a CRS unless they proactively opt out of doing so) makes it unlikely that these motivated consumers will proactively choose not to seek redress through a CRS. It is possible that a small proportion of these consumers drop out of the redress scheme half-way through. These consumers may have become

Input	Low	Central	High	Reasoning
				uncontactable, for example if they have changed address. As such, our central estimate stands at 95%. Our low estimate for those receiving letter type A is equivalent to our central estimate at 95%. This allows for some consumers to become uncontactable, but we do not think that this will be common. Our high estimate is at 100%, which assumes that all consumers receiving letter type A who have already complained choose to opt into the CRS. This is under an assumption that no consumers become uncontactable. Consumers who receive the letter communicating that they are ineligible for redress and not invited to join the CRS, such that their CRS join rate is 0%. Some consumers may refer their complaint in such an event. Consumers referring their complaint to the Financial Ombudsman upon receiving the ineligibility determination letter are accounted for in the Financial Ombudsman referral rate (consumer dispute). We note that communications from us, and any awareness campaign could impact the CRS join rate.
Financial Ombudsman referral rate (out of time)	0%	0%	0%	In the intervention scenario, we assume no complaints would be referred to the Financial Ombudsman through lenders running out of time to deal with them. This is because we anticipate that all agreements will be assessed by lenders within the timeframe of the scheme. If any agreements do become eligible for Financial Ombudsman referral through this channel (for example, if lenders do not scale up their complaints-handling capabilities enough), then it is possible that some complaints are referred to the Financial Ombudsman. However, we will work with lenders to ensure that all agreements are assessed within the time limit. Under the intervention scenario, we assume complaints will not time out due to FCA coordination with lenders, thereby reducing Financial Ombudsman exposure. The £650 Financial Ombudsman fee, in many cases, will exceed the redress payment, providing a strong financial incentive for lenders to settle complaints directly, further limiting referrals.

Input	Low	Central	High	Reasoning
Financial Ombudsman referral rate (consumer dispute)	0%	7.5%	15%	Lenders would be required to assess complaints through a methodology which we prescribe. We expect that this would reduce the need for consumers to refer their complaint to the Financial Ombudsman. Despite this, some consumers may still refer their complaint. Consumers who receive the "you are out of the CRS" letter after complaining before the CRS launched may also choose to refer their complaint to the Financial Ombudsman. There would be less scope for PR/CMC involvement in the intervention, which could reduce the Financial Ombudsman referral rate. Rates of consumer disagreement are likely also to depend on exact intervention design, including communications approaches of individual lenders, especially for opt-in intervention. We are uncertain on the exact proportion of complaints which will be referred to the Financial Ombudsman, however we expect the proportion to be lower than in the counterfactual. We note that the Financial Ombudsman referral rate through both channels for the British Steel Pension Scheme (BSPS), a S404 opt-out CRS, was 5% overall. We think that the increased prevalence of CMCs and other PRs could increase this further, however the new Financial Ombudsman fee structure for CMCs means that Financial Ombudsman referrals through CMCs are less likely than before the pause on complaints (30% through both channels). As such, our central estimate of 7.5% is uplifted by 2.5ppt above the Financial Ombudsman referral rate for BSPS. Our high estimate presents a scenario where both lenders make more mistakes in their redress determinations and CMC/PR prevalence is high. Whereas our low estimate presents a scenario where all CMCs/PRs and consumers are content with their redress offers.
Financial Ombudsman fee	£650	£650	£650	See "Financial Ombudsman fee" in the counterfactual assumptions.
Financial Ombudsman	NA	NA	NA	Unlike the counterfactual scenario, we anticipate that the volume of complaints referred to the Financial Ombudsman under the CRS will be sufficiently low and with clear rules and approaches for the Financial Ombudsman to follow, meaning no additional fees

Input	Low	Central	High	Reasoning
scaling cost per complaint				beyond the standard Financial Ombudsman fee would be required. The design of the CRS is intended to minimise the need for Financial Ombudsman involvement.
Working hours per month	134.17	134.17	134.17	See "working hours per month" in the counterfactual assumptions.
Variable cost per complaint non-labour cost and complexity uplift	21%	21%	21%	See "variable cost per complaint non-labour cost and complexity uplift" in the counterfactual assumptions.
Average time per agreement for one-off screening costs	30	60	90	The time per agreement for one-off screening refers to the period of time required to review agreements and determine consumer eligibility for redress. The outcome will be either a notification of likely eligibility or ineligibility. The time required depends on the degree of automation and data availability. For inadequate disclosure of DCA and high commission unfair relationships, screening is expected to be highly automated, allowing most lenders to identify breaches quickly. This applies to approximately 11.4 million agreements with inadequate disclosure of DCA unfair relationships and 2.9 million agreements inadequate high unfair relationships. For the remaining agreements, we estimate that around 14% of agreements had inadequately disclosed tied arrangements. Identifying these requires lenders to search for tied arrangements and locate the relevant broker-lender contract for a certain contract period, which is more time-consuming. Some lenders do not have tied arrangements, but for those that do, this process is more manual. For DCA and high commission breaches, screening is expected to be highly automated, allowing most lenders to identify breaches rapidly. This applies to approximately 11.4 million DCA agreements and 2.9 million high commission arrangements.

Input	Low	Central	High	Reasoning
				For the remaining agreements, around 14% of non-DCA cases involve tied arrangements. Identifying these requires lenders to search annually for tied arrangements and locate the relevant broker-lender contract for the period, which is more time-consuming. Some lenders do not have tied arrangements, but for those that do, this process is more manual. Our estimates for screening time per agreement are: 30 minutes in the low scenario (maximum automation), 60 minutes in the central scenario (partial automation), and 90 minutes in the high scenario (predominantly manual). These reflect the additional manual work required to identify tied arrangements.
One-off investment for handling complaints	Calculated at the lender-level using motor finance commission monitoring survey results			In August 2025, we asked lenders to consider their estimated one-off investments they would expect to make in systems, capital, or infrastructure specifically to manage a potential CRS. These responses are considered at the lender-level. Lenders provided wide ranging estimates of how much they expect to invest in systems to enhance their complaints-handling capabilities.
Complaints-handling full-time equivalent (FTE) employees	Calculated at the lender-level using motor finance commission monitoring survey results			In August 2025, we asked lenders to consider how many FTE staff they expect to allocate to handling complaints for a potential consumer redress scheme following the Supreme Court decision. These are considered at the lender-level, as different lenders have different numbers of agreements and expect to receive different numbers of complaints.
Hourly cost per complaints-handling employee	Calculated at the lender-level using motor finance commission monitoring survey results			In August 2025, we asked lenders to consider the average total hourly cost per staff member assigned to complaint handling involved in a potential consumer redress scheme following the SC decision.

Input	Low	Central	High	Reasoning
				These are considered at the lender level, as complaints-handling employees at different lenders have differing wages. For example, some lenders may choose to outsource complaints-handling employees which would reduce this cost.
Time taken to process a complaint	Calculated at the lender-level using motor finance commission monitoring survey results			In August 2025, after the SC decision, we asked lenders to consider estimated average time to process a single complaint within a potential consumer redress scheme, from initial receipt to final resolution. We modelled the time taken to process a single complaint under a potential CRS at the lender-level because we noticed a high variance between lenders. For instance, lenders with a low proportion of agreements which are owed redress may be able to identify complaints which are owed redress faster.
NPV annual discount rate	3.5%	3.5%	3.5%	See "NPV annual discount rate" in the counterfactual assumptions.
Average consumer complaint time for opt-in process (firm only), minutes	20	75	130	To attain the consumer time estimates, we mapped possible complaint journeys which consumers could follow. We then assigned the amount of time which we expect consumers to take under each of these possible journeys. For each journey, we estimated an upper and lower bound for consumers. For example, a consumer at the lower end of the estimates would provide the right information to the lender when submitting a complaint, whereas a consumer at the higher end of the estimates would not provide sufficient information at the start such that the lender would have to contact the consumer. We sense-checked our estimates with the lower bounds for the time estimates for a straightforward claim in our analysis published in CP21/1, and adjusted our estimates accordingly. Our lower bound estimate for a straightforward complaint was 1.5 hours in CP21/1, whereas our central estimate here is 1.25 hours.

Input	Low	Central	High	Reasoning
Average consumer complaint time for opt-out process (firm only), minutes	10	40	70	As above. Consumers are required to follow fewer steps in the opt-out process compared to the opt-in process. To be eligible to be in the opt-out process, consumers must have already incurred the sunk cost of complaining to a lender. We sense-checked our estimates with the lower bounds for the time estimates for a straightforward claim in our analysis published in CP21/1, and adjusted our estimates accordingly. Our lower bound estimate for a straightforward complaint was 1.5 hours in CP21/1, whereas our central estimate here under 1 hour.
Average consumer complaint time for opt-in process (firm and Financial Ombudsman), minutes	60	137.5	225	As above. We expect that consumers who subsequently complain to the Financial Ombudsman will spend more time making a follow-up complaint. We note that our time estimates in CP21/1 for Financial Ombudsman referral are higher than our estimates in this Annex. The low time estimate for a straightforward complaint in CP21/1 where the consumer refers their complaint to the Financial Ombudsman after complaining to a lender is 4.5 hours, whereas our central estimate here is just over 2 hours. This is because we then conducted the analysis at the consumer level, whereas here we attain estimates per consumer at the market level. We consider that it is unlikely that many consumers would spend the maximum amount of time for a straightforward complaint from our CP21/1 estimates.
Average consumer complaint time for opt-out process (firm and Financial	50	102.5	165	As above. Consumers are required to follow fewer steps in the opt-out process compared to the opt-in process. To be eligible to be in the opt-out process, consumers must have already incurred the sunk cost of complaining to a lender.

Input	Low	Central	High	Reasoning
Ombudsman), minutes				We note that our time estimates in CP21/1 for Financial Ombudsman referral are higher than our estimates in this Annex. The low time estimate for a straightforward complaint in CP21/1 where the consumer refers their complaint to the FOS after complaining to a lender is 4.5 hours, whereas our central estimate here is just under 2 hours. This is because we then conducted the analysis at the consumer level, whereas here we attain estimates per consumer at the market level. We consider that it is unlikely that many consumers would spend the maximum amount of time for a straightforward complaint from our CP21/1 estimates.
Average consumer complaint time – using a PR/CMC, minutes	48	60	72	As above. For a consumer complaining through a PR/CMC, we expect there to be less variation between consumers. This is because the PR/CMC will handle the majority of the claims process without need for the consumer to act. Consumers may need to provide further information to the PR/CMC if required to further the complaint. The lower bound for our time estimates in CP21/1 is identical to our central estimate in this analysis.
Value of consumer time	£7.49	£7.57	£35.20	See "value of consumer time" in the counterfactual assumptions.

Input	Low	Central	High	Reasoning
Proportion of complaints submitted through PRs/CMCs*	12.0%	43.75%	75.5%	Our low, central, and high estimates for the counterfactual are 61.0%, 75.5%, and 90.0%, respectively. In the intervention scenario, lenders will contact consumers directly, reducing the need for PR/CMC involvement. This direct engagement may encourage consumers to continue the claims process independently. As a result, we estimate a lower proportion of complaints submitted through a PR/CMC under the intervention compared to the counterfactual. Given that we expect the prevalence of PRs/CMCs to fall in the intervention compared to the counterfactual, our high estimate is taken as the central estimate for the counterfactual. Our central estimate for the counterfactual (and therefore our central estimates in the intervention) is derived from our motor finance commission monitoring survey, which suggested that the majority of lenders have experienced 75.5% of their complaints so far from PRs/CMCs. We expect that a small portion of consumers would seek help from a PR/CMC in the intervention. Our May 2024 FLS suggests that around 12% of UK adults had low financial capability and we use this figure as a proxy for our lower bound estimate. Our central estimate is derived as the mid-point between our low and high estimates.

*This figure is not applied in the lender cost model – the only channel through which estimates of the proportion of complaints submitted through PRs/CMCs impacts costs is through the estimated impact on consumer time.

Modelling details

Lender administrative costs and Financial Ombudsman related fees

4.38 The steps involved in the modelling of lender administrative costs and Financial Ombudsman related fees for the counterfactual scenario are described in the table below.

Table 38: Modelling steps to estimate lender administrative costs and Financial Ombudsman related fees for the counterfactual scenario

Step	Description/Method	Assumptions	Market-wide central case output (nominal)
1)	**Calculate number of complaints received by lenders** Use complaint incidence rate assumptions to estimate the total number of complaints received by lenders.	• Complaint incidence rate (low case 59%, central case 69%, high case 79%) • Lender level MF agreement numbers (32.5 million total market wide)	• 22.4 million complaints received by lenders
2)	**Calculate lender complaint handling capacity** Use key inputs to estimate the number of complaints that can be handled by each lender within the 8-week response deadline: • FTE working on complaint handling • Lender-level complaint handling time estimates (Round 4 survey) • Variable administrative cost per complaint	• Accurate lender self-reporting of FTE working on complaints, time taken to process a complaint and average hourly staff wage	• 9.8 million complaints not handled by lenders within 8-week deadline (but note some are handled by lenders after this time; see below) • 19.5 complaints handled by lenders (12.6 million handled within the 8-week deadline, 6.9 million handled after the 8-week deadline)
3)	**Calculate lender administrative costs** Estimate the variable administrative costs incurred by each lender using: • Total number of complaints handled	• Accurate lender self-reporting of time taken to process a complaint, one-off investment costs required to scale up, and average hourly staff wage	• £683 million variable administrative costs • £27 million fixed administrative costs

Step	Description/Method	Assumptions	Market-wide central case output (nominal)
	• Lender-level complaint handling time estimates (Round 4 survey) • Lender-level hourly staff wage • Lender level one-off investment estimates (Round 4 survey)	• Variable administrative cost per complaint non-labour cost and complexity uplift is 21% in all cases	
4)	**Calculate Financial Ombudsman referrals and Financial Ombudsman referral fees from time out** Estimate the number of complaints that get referred to Financial Ombudsman as a result of not being handled by lenders within the 8-week response deadline, using: • Total number of complaints not handled within the 8-week deadline • Financial Ombudsman referral rate from out of time	• Financial Ombudsman referral rate from out of time (10% low case, 30% central case, 50% high case) • Financial Ombudsman referral fee is £650 in all cases	• 2.9 million Financial Ombudsman referrals from time out • £1.9 billion Financial Ombudsman referral fees from time out (2.9 million *£650)
5)	**Calculate Financial Ombudsman referrals and Financial Ombudsman referral fees from consumer dispute** Estimate the number of complaints that are handled by lenders and subsequently referred to the Financial Ombudsman by the consumer, using: • Total number of complaints handled by the lender • Financial Ombudsman referral rate from consumer dispute	• Financial Ombudsman referral rate from consumer dispute (low case 10%, central case 25%, high case 40%) • Financial Ombudsman referral fee is £650 in all cases	• 4.9 million Financial Ombudsman referrals from consumer dispute • £3.2 billion Financial Ombudsman referral fees from consumer dispute (4.9 million *£650)
6)	**Calculate total Financial Ombudsman related fees** Estimate the total Financial Ombudsman related fees incurred by each lender by summing: • Financial Ombudsman referral fees from timeout • Financial Ombudsman referral fees from consumer dispute • Financial Ombudsman scaling fees	• Financial Ombudsman scaling fee £455 in all cases	• 7.8 million Financial Ombudsman referrals overall • £8.6 billion Financial Ombudsman related fees (£3.6 billion in Financial Ombudsman scaling fees and £5.1 billion in Financial Ombudsman referral fees)

Step	Description/Method	Assumptions	Market-wide central case output (nominal)
7)	**Calculate total lender non-redress costs** Estimate scaled-up market-level total lender non-redress costs by adding together administrative costs and Financial Ombudsman -related fees.	• Scaling carried out as detailed in the Combining data sources and scaling to market-level section above	• £9.3 billion lender non-redress costs

4.39 The steps involved in the modelling of lender administrative costs and Financial Ombudsman related fees for the preferred intervention scenario are described in the table below.

Table 39: Modelling steps to estimate lender administrative costs and Financial Ombudsman related fees for the intervention scenario

Step	Description/Method	Assumptions	Market-wide central case output (nominal)
1)	**Calculate number of complaints that proceed under the CRS following the lender screening process** Use agreements and complaints data and CRS join rate assumptions to estimate the number of cases that proceed to full case assessment under the CRS via the opt-in process for complaints not already made and opt-out process for complaints already made. While the CRS join rates are market-level assumptions, we calculate the proportion of agreements in each of the four consumer groups at the lender level using i) the number of breached agreements and total number of agreements, and ii) the number of complaints already registered to lenders and the number not registered to lenders.	• CRS join rates for opt-in process (eligible: 69% low case, 83% central case, 95% high case; ineligible: 27% low case, 41% central case, 55% high case) • CRS join rates for opt-out process (eligible: 95% low and central cases, 100% high case; ineligible: 0% in all cases)	• 18.6 million complaints (16.6 million through opt-in process and 2.0 million through opt-out process for complaints already received)
2)	**Calculate lender screening costs** Use key inputs to estimate the lender-level screening costs incurred (i.e. costs associated with screening agreements to determine if they are likely to be eligible for redress or not), using: • Average screening time per agreement • Lender level number of agreements • Lender level hourly staff wage	• Accurate lender self-reporting of average hourly staff wage is accurate • Average screening time per agreement (30 mins low case, 60 mins central case, 90 mins high case)	• £797 million screening costs

Step	Description/Method	Assumptions	Market-wide central case output (nominal)
3)	**Calculate lender variable administrative costs** Estimate the variable administrative costs associated with full case handling incurred by each lender using: • Total number of complaints to be assessed • Lender-level complaint handling time estimates • Lender level hourly staff wage	• Lender self-reporting of time taken to process a complaint and average hourly staff wage is accurate • Variable admin cost per complaint non-labour cost and complexity uplift: 21% in all cases	• £878 million variable administrative costs
4)	**Calculate one-off investment costs to scale up for a CRS** Estimate the market-wide one-off investment costs using individual lender level estimates of scaling up required for a CRS from the motor finance commission monitoring survey (Round 5)	• Accurate lender self-reported one-off investments to scale up for a CRS	• £108 million one-off investment costs
5)	**Calculate total administrative costs** Estimate market-wide administrative costs by summing together screening costs, variable administrative costs and one-off investment costs		• £1.8 billion total administrative costs
6)	**Calculate Financial Ombudsman referrals and Financial Ombudsman referral fees from consumer dispute** Estimate the number of complaints that are considered by lenders and subsequently referred to the Financial Ombudsman by the consumer using: • Total number of complaints handled by the lender under the CRS • Total number of complaints already received but deemed ineligible at screening stage	• Financial Ombudsman referral rate from consumer disagreeing (0% low case, 7.5% central case, 15% high case) • Breach rate of complaints already received is the same as the overall lender-level breach rate estimate	• 1.6 million Financial Ombudsman referrals from consumer dispute (through both disagreement with final redress determination and initial eligibility screening channels) • £1.0bn Financial Ombudsman referral fees (£650 * 1.6 million)

Step	Description/Method	Assumptions	Market-wide central case output (nominal)
	• Financial Ombudsman referral rate from consumer disagreeing		
7)	**Calculate total lender non-redress costs** Estimate scaled-up market-level total lender non-redress costs by adding together total administrative costs and Financial Ombudsman related fees. *Note it is assumed that lenders incur no Financial Ombudsman scaling fees in the intervention scenario.	• Scaling carried out as detailed in the *Combining data sources and scaling to market-level section* above	• £2.8 billion lender non-redress costs (£1.8bn administrative costs and £1.0bn Financial Ombudsman related fees)

Consumer costs

4.40 The final modelled component of non-redress costs is the cost associated with the time consumers will spend dealing with complaints. To calculate this, we use the DfT consumer value of time per hour estimate of £7.57 in our calculations (using 2025 as the base year).

4.41 We approached the modelling of consumer costs by estimating the average time spent on a complaint by a consumer under different complaint categories, as set out in the table below. We use low, central and high case estimates based on a combination of our analysis and an existing FCA consultation on CMCs.

Table 40: Estimated time spent by consumers dealing with a complaint (nominal).

Complaint category	Complaint channel	Lower case estimate (minutes)	Central case estimate (minutes)	High case estimate (minutes)
Complaint submitted by consumer to lender only	Counterfactual channel	60	122.5	185
	Opt-in channel	20	75	130
	Opt-out channel	10	40	70
Complaint submitted by consumer to lender and the Financial Ombudsman	Counterfactual channel	100	187.5	275
	Opt-in channel	60	137.5	225
	Opt-out channel	50	102.5	165
Complaint submitted by consumer through a PR/CMC	All channels*	48	60	72

* Time spent complaining is not expected to vary if complaining through a PR/CMC.

4.42 As described in the assumption tables, our central case assumption is that the proportion of complaints submitted through a professional representative is 43.75% under a CRS, and 75.5% absent any CRS.

4.43 For each of the consumer complaint categories in the table above, the market-wide number of complaints within each category is obtained from the outputs of the modelling carried out for lender administrative and Financial Ombudsman related costs as described above.

4.44 To estimate the total cost to consumers under the counterfactual and intervention scenarios in the model, for each complaint category we calculated the product of the number of market-wide consumers in the complaint category, the estimated time spent dealing with complaints, and the value of time per hour. This allows us to arrive at an estimate for the total value of consumer time spent dealing with complaints. We summed the consumer costs from each complaint category.

Outputs

4.45 The modelling described above was carried out for the counterfactual scenario and two intervention options considering different time periods for application of the CRS. Outputs are modelled for:

- Counterfactual scenario (no intervention)
- Option 1: Intervention with a CRS from April 2007 to November 2024[34]
- Option 2: Intervention with complaints-led process from April 2007 to April 2014 and a CRS from April 2014 to November 2024

4.46 The net benefits of intervention options 1 and 2 are considered relative to the counterfactual. Our estimates of the net benefits are presented in the table in the introduction section above.

4.47 The cost outputs of each option are set out in the tables below.

[34] Our assessment of non-redress costs covers agreements to 24 October 2024, which does not fully align with the proposed CRS policy end date of 1st November 2024. We consider this different to be immaterial to our estimates.

Table 41: Counterfactual non-redress cost estimates (nominal).

	Low Case	Central case	High case
Variable administrative costs	£634 m	£683 m	£700 m
One-off investments	£27 m	£27 m	£27 m
Screening costs	£0 m	£0 m	£0 m
Other lender costs*	£0 m	£0 m	£0 m
Total administrative costs	**£661 m**	**£709 m**	**£726 m**
Financial Ombudsman fees (disagree with final redress determination)	£1,197 m	£3,164 m	£5,060 m
Financial Ombudsman fees (disagree with redress determination from initial screening)	£0 m	£0 m	£0 m
Financial Ombudsman fees (out of time)	£494 m	£1,917 m	£4,034 m
Total Financial Ombudsman referral fees	**£1,691 m**	**£5,081 m**	**£9,094 m**
Financial Ombudsman scaling fees	£1,184 m	£3,556 m	£6,366 m
Total Financial Ombudsman fees	**£2,875 m**	**£8,637 m**	**£15,460 m**
Total lender costs	**£3,535 m**	**£9,346 m**	**£16,186 m**
Consumer time costs	£130 m	£225 m	£1,315 m
Total costs	**£3,665 m**	**£9,572 m**	**£17,502 m**

* These include other direct costs for lenders as a result of the redress scheme including familiarisation, training and dissemination and Board and Executive Committee review.

4.48 These nominal counterfactual cost estimates produce the following NPV estimates.

Table 42: NPV estimates of non-redress costs to lenders in the counterfactual

	Low case	Central case	High case
Total lender non-redress costs	**£3,476.0 m**	**£9,188.9 m**	**£15,913.2 m**
Administrative costs	£649.8 m	£697.7 m	£714.2 m
Financial Ombudsman fees (disagree)	£1,176.4 m	£3,110.5 m	£4,974.8 m
Financial Ombudsman fees (out of time)	£486.0 m	£1,884.3 m	£3,965.8 m

Financial Ombudsman scaling fees	£1,163.7 m	£3,496.4 m	£6,258.4 m

Table 43: Option 1 non-redress cost estimates (nominal)

	Low Case	Central case	High case
Variable administrative costs	£728 m	£878 m	£1,060 m
One-off investments	£108 m	£108 m	£108 m
Screening costs	£399 m	£797 m	£1,196 m
Other lender costs*	£3 m	£3 m	£3 m
Total administrative costs	**£1,238 m**	**£1,786 m**	**£2,367 m**
Financial Ombudsman fees (disagree with final redress determination)	£0 m	£866 m	£2,193 m
Financial Ombudsman fees (disagree with redress determination from initial screening)	£0 m	£105 m	£210 m
Financial Ombudsman fees (out of time)	£0 m	£0 m	£0 m
Total Financial Ombudsman referral fees	**£0 m**	**£971 m**	**£2,402 m**
Financial Ombudsman scaling fees	£0 m	£0 m	£0 m
Total Financial Ombudsman fees	**£0 m**	**£971 m**	**£2,402 m**
Total lender costs	**£1,238 m**	**£2,758 m**	**£4,769 m**
Consumer time costs	£41 m	£151 m	£1,136 m
Total costs	**£1,275 m**	**£2,906 m**	**£5,902 m**

* These include other direct costs for lenders as a result of the redress scheme including familiarisation, training and dissemination and Board and Executive Committee review.

Table 44: Option 2 non-redress cost estimates (nominal).

	Low Case	Central case	High case
Variable administrative costs	£685 m	£807 m	£957 m
One-off investments	£108 m	£108 m	£108 m
Screening costs	£284 m	£568 m	£852 m
Other lender costs*	£3 m	£3 m	£3 m
Total administrative costs	**£1,080 m**	**£1,485 m**	**£1,920 m**
Financial Ombudsman fees (disagree with final redress determination)	£354 m	£1,572 m	£3,236 m
Financial Ombudsman fees (disagree with redress determination from initial screening)	£0 m	£83 m	£165 m
Financial Ombudsman fees (out of time)	£54 m	£221 m	£489 m
Total Financial Ombudsman referral fees	**£408 m**	**£1,876 m**	**£3,890 m**
Financial Ombudsman scaling fees	£286 m	£853 m	£1,554 m
Total Financial Ombudsman fees	**£694 m**	**£2,728 m**	**£5,444 m**
Total lender costs	**£1,775 m**	**£4,214 m**	**£7,363 m**
Consumer time costs	£66 m	£166 m	£1,161 m
Total costs	**£1,840 m**	**£4,380 m**	**£8,524 m**

* These include other direct costs for lenders as a result of the redress scheme including familiarisation, training and dissemination and Board and Executive Committee review.

4.49 Option 2 considers that the intervention taken is a CRS that applies to MF agreements made between April 2014 and November 2024, and that agreements between April 2007 and April 2014 would be subject to a complaints-led process. To attribute agreement figures between these periods, we portion one quarter of agreements in 2014 to the pre-April 2014 period and three quarters of the agreements in 2014 to the post-April 2014 period. The costs for this option are estimated by subtracting the costs associated with a hypothetical CRS intervention from April 2007 to April 2014 from the costs associated with a CRS intervention from April 2007- November 2024 (Option 2), and adding the costs associated with the baseline (complaints-led process) from April 2007 to April 2014.

4.50 Overall, the quantified central estimates for the 2 intervention options compared to the counterfactual produce costs and benefits estimates and NPV calculations, as presented in the tables below. See the CBA for details of the costs and benefits assessment.

Table 45: Estimated costs and benefits of intervention options 1 and 2 compared to the counterfactual scenario

	Option 1	Option 2
One-off benefits	-	-
Annual benefits (for 2 years)	£3,870.2 m	£2,983.9 m
One-off costs	£881.8 m	£652.4 m
Annual costs	£117.4m in year 1; £109.0m in year 2	£81.7m in year 1; £73.3m in year 2

Table 46: NPV calculations

	Option 1	Option 2
PV benefits (excluding gains to consumer redress)	£7,609.5 m	£5,867.0 m
PV costs	£1,104.5 m	£804.9 m
NPV	£6,505.0 m	£5,062.1 m

Limitations

Limitations of modelling

Lender-level differences not accounted for

4.51 Parts of the modelling apply market level assumptions and therefore do not account for lender-level differences that will impact non-redress costs. For example, the model does not account for lender-level differences in the proportion of consumers which may join the CRS which may significantly affect individual lender costs.

Reliability of lender estimates within the motor finance commission monitoring survey

4.52 Given that the motor finance commission monitoring survey was conducted prior to details of any proposed redress scheme being published, lender estimates used to model lender costs may be unreliable. Many lenders indicated that without further clarity regarding the details of the redress scheme they are unable to provide meaningful estimates. We use lenders' own estimates for one-off investments, the time taken to assess a complaint, FTE complaints-handlers, and hourly wages (uplifted by 21% to account for overheads). These inputs are uncertain and could change.

4.53 We assume that lenders' own responses to questions which impact the number of complaints that can be handled in a given time are accurate. This does not impact our estimates for the intervention, as lenders are expected to be able to scale their operations in time to work through every agreement within the CRS period. However, we assume that lenders do not scale their operations above their reported responses in the counterfactual, which could lead us to underestimate administrative costs and overestimate Financial Ombudsman referrals through the out-of-time channel.

Missing values from lender estimates within the motor finance commission monitoring survey

4.54 For any missing parameters within the motor finance commission monitoring survey, we fill gaps with estimates from the previous motor finance commission monitoring survey with sufficiently similar questions (e.g., we use Round 4 motor finance commission monitoring survey responses for the FTE question if lender did not provide an estimate in the Round 5 motor finance commission monitoring survey). Where lenders within the survey sample have never provided a response to a question (including in previous surveys with sufficiently similar questions), we use the median values for lenders who have provided responses.

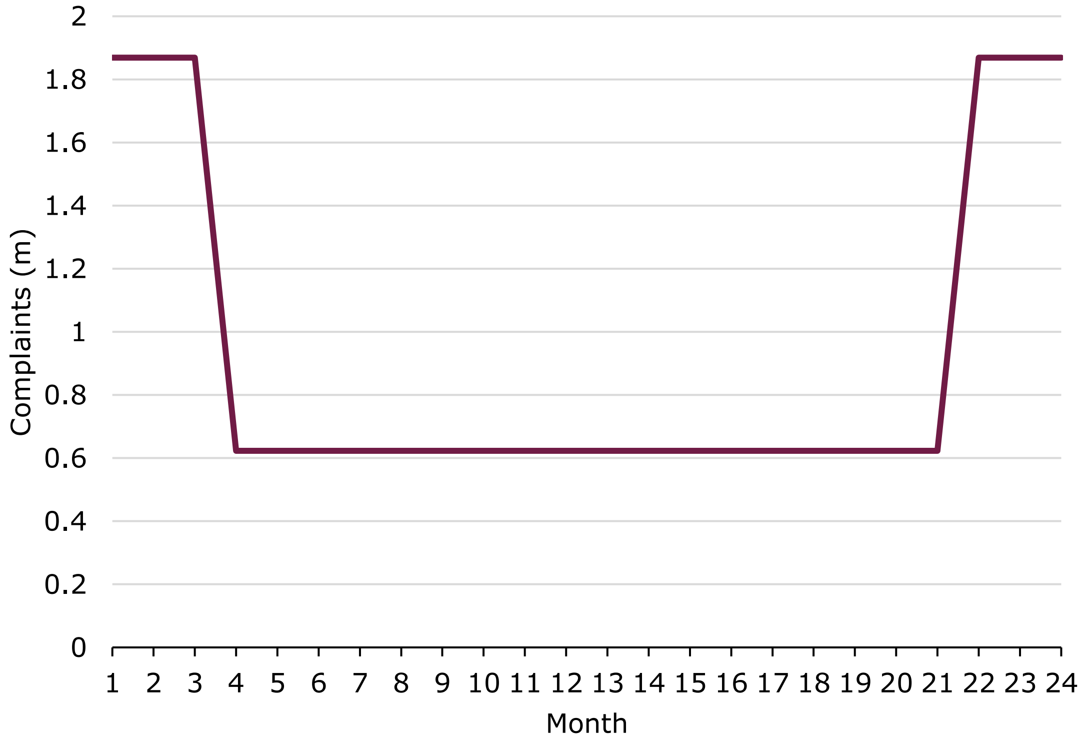
Reweighting methodology

4.55 We extrapolate results assuming that the lenders within each stratum sample are representative of those outside the sample. As a result, we expect that the sample is representative of over 99% of the market. We do not scale up our estimates to account for the additional 0.1% of the market.

When complaints are registered

4.56 Evidence from PPI indicates that complaints are unlikely to be evenly distributed across a CRS complaint period. PPI data showed peaks in complaint volumes at the start and end of the redress scheme period, with some variation linked to advertising and other factors. Lenders will also hold a backlog of complaints they can assess from day one of the CRS. We have therefore assumed in our counterfactual that 25% of complaints will arrive in the first quarter, 25% in the final quarter, and the remaining 50% will be spread across the intervening 18 months as shown in figure below.

Figure 19: Illustrative distribution of monthly complaints registered with lender over complaint period.



Redress scheme length

4.57 We assume for modelling purposes that lenders have 2-years to respond to complaints from the date the redress scheme starts in our intervention scenario, and complaints are registered across 2 years in our counterfactual scenario. After the 2 years, the deadline to respond to complaints will revert to 8 weeks. Within the CRS, we assume that all agreements are fully assessed within this period.

Other limitations

Brokers' involvement in the redress scheme

4.58 Lenders may require information from brokers to assess certain complaints. We are unsure of the extent to which lenders will need to contact brokers for this information, as well as how much time and effort brokers will be required to put in to provide this information. We have assessed brokers' ongoing involvement qualitatively within the CBA (see the "Benefits to other parties" section).

Evolution of lenders' capacity to handle complaints

4.59 Lenders that experience a higher number of complaints than anticipated may hire more complaints-handling staff, which would reduce the number of complaints which become eligible for Financial Ombudsman referral. We do not assume lenders hire more staff than the number of FTE they provided in the motor finance commission monitoring survey.

Automation of lenders' complaints-handling processes

4.60 Lenders may invest in automation processes to reduce the cost of handling complaints. Particularly larger lenders with more agreements to process. In August 2025, we asked lenders to consider the estimated one-off investment their lender expects to make in systems, capital, or infrastructure specifically to manage a potential CRS, as well as their expected FTE complaints handlers, the wages of the FTE, and the complaint processing speed. However, lenders did not know the structure of our proposed CRS at the time, so these estimates could change.

Group reporting in motor finance motor finance commission monitoring surveys

4.61 Some groups of lenders report their survey responses together. This has implications for some motor finance commission monitoring questions such as the number of FTE complaints-handlers. We do not have agreement data for all these lenders, however we do have access to the number of motor finance complaints which had been submitted to them as of July 2025. We weight these responses between lenders within each group based on the number of complaints submitted to each of them as of July 2025.

Additional costs not modelled

4.62 Some costs are accounted for outside this model.

4.63 Our Standardised Cost Model (SCM) enables us to estimate the compliance costs which lenders will incur. The SCM allows us to estimate familiarisation and gap analysis (including legal costs arising from our new rules), training and dissemination, and Board and Executive approval costs. See the consultation CBA for further details of these.

4.64 There will also be costs incurred by the FCA such as costs of oversight of the redress scheme and data requests, estimated at a total nominal cost of £31.2 million over the next two financial years as detailed in the consultation CBA.

4.65 There are additional factors which will impact the costs associated with the redress scheme, but we have been unable to quantify and are instead qualitatively assessed outside our model. Cost factors that may impact the non-redress costs but are only qualitatively assessed include:

- Compromises and firm failure, which could mean that some lenders either reach an agreement with creditors to reduce the redress owed to them, or stop paying redress after a certain amount due to firm failure.
- PRs/CMCs, which involves the costs associated with involvement from CMCs and SRA-regulated law firms. We model the impacts of PRs/CMCs on the value of consumer time incurred, however we do not model the impacts which PR/CMC involvement may have on costs to lenders.
- Credit Reference Agency usage, which involves the costs associated with the purchase of data from CRAs to enable lenders to assess complaints.
- Ongoing costs to brokers, which involves the ongoing costs associated with brokers providing information to lenders required for case assessment.
- Sunk costs already incurred by lenders or those incurred where consumers apply through the CRS before going to court.

4.66 See the consultation CBA for further details of these cost factors.