

Motor Finance Consumer Redress Scheme

Technical Annex 1

Updated data, analysis of loss, and liability
and cost methodologies

March 2026

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Chapter 1

Introduction

- 1.1** As part of CP25/27, we published a detailed annex ([Technical Annex 1](#)) providing details on the data used to inform our quantitative evidence base and the methodologies and associated results for our analysis of loss, liabilities estimates and firm cost estimates.
- 1.2** This updated Technical Annex covers the same areas with the addition of new analysis on the impact of commercial arrangements in the motor finance market. We summarise feedback received on the previous Technical Annex and our response to this, including any relevant new analyses and any changes to the way the analysis informs final policy positions. We also provide details of changes to our methodology used to calculate firm redress liabilities and scheme non-redress costs. Where appropriate, we provide comparisons with previous estimates.
- 1.3** Whilst we received broad feedback covering legal, commercial and operational aspects of our proposed intervention and the supporting evidence base, this Annex focuses only on technical feedback and evidence submitted. Wider feedback covering legal, operational and policy issues and how we responded to these questions are in PS26/3. Feedback on the Cost Benefit Analysis is covered in the updated CBA.
- 1.4** This Annex is structured as follows:
- Data room overview to support our consultation
 - Data guide update
 - Analysis of loss update
 - Commercial arrangements analysis and literature review
 - Academic review
 - Redress liability estimates update
 - Non-redress cost estimates update
- 1.5** For brevity we do not repeat sections of the original Technical Annex that are unchanged, such as large parts of the data guide. Where original analysis has been updated, this Technical Annex makes clear what should be read as superseding previous analyses.

Chapter 2

Data room

Operating environment

- 2.1** To support stakeholders in responding to the Motor Finance Consumer Redress Scheme Consultation (CP25/27), we established a physical data room in our London office for respondents to access, interrogate and manipulate data used to inform the analysis. Access took place during the consultation window between 21 October 2025 and 12 December 2025.
- 2.2** The data room operated on working days and required advance booking. It initially opened between 09:00 and 17:00, and then from 30 October the opening hours were extended to 09:00–18:00. Weekend access, upon request, was introduced from 22 November.
- 2.3** The physical environment of the data room included the provision of FCA laptops containing relevant data and materials. The laptops had no internet or email capability. Microsoft Office, Notepad ++ and RStudio with commonly used analytical packages were preinstalled on the laptops.
- 2.4** Security restrictions applied, no personal devices were allowed, and the room was monitored by FCA security personnel.
- 2.5** Access to the data room was conditional on meeting certain criteria. To be granted access, applicants needed to demonstrate appropriate qualifications and experience for handling large datasets and performing statistical analysis. Evidence required included CVs of each proposed user that showed:
- experience with R or similar coding languages
 - background in data science, statistics, econometrics, economics, or other quantitative disciplines
 - examples of previous modelling work
- 2.6** Before access, each individual user was required to:
- sign a confidentiality agreement
 - confirm compliance with s348 FSMA and MAR restrictions
 - agree that data use was solely for advising their firm for the purposes of responding to the Motor Finance Consumer Redress Scheme consultation
 - adhere to restrictions on extraction and disclosure

Data in the data room

2.7 The laptops in the data room contained the analysis documents, datasets and code necessary to replicate and interrogate elements of the FCA's analytical work. Documents included:

- CP25/27: Motor finance consumer redress scheme
- Technical Annex 1: Data, analysis of loss, and liability and cost methodologies – describes the data we collected in more detail, our analysis of consumer impacts of inadequate disclosure, and how we calculated costs
- Technical Annex 2: State of Competition in the Motor Finance Market – our understanding of how competition in the motor finance market works
- Technical Annex 3: Market Impacts – our analysis of the market impacts of our proposed redress scheme in more detail
- Technical Annex 4: Market Impacts Methodology – for our market impacts analysis
- Diagnostic Report: FCA review of motor finance commission arrangements
- Research Note: Motor Finance Consumer Awareness Survey analysis. Additional methodological details are set out in a Technical Annex
- Motor Vehicle Finance Consumer Research – externally commissioned research to understand consumers' usage, understanding and decisions concerning motor finance
- Market impact assessment on cost of capital and investment in the motor finance market – externally commissioned market impact study on the effects of motor finance redress liabilities
- CP19/28: Our work on motor finance – final findings
- A README file for the code and the data (see below)

2.8 The data and code included:

- Pseudo-anonymised version of the loan level data used in the Technical Annex as described in CP25/27 Technical Annex 1, paragraphs 1.18 – 1.26
- Pseudo-anonymised version of the Credit Reference Agency data, as described in CP25/27 Technical Annex 1, paragraphs 1.74–4 – 1.77, from 18 November 2025 onwards
- R code enabling full replication of the DCA Commission Model Impact Analysis, the analysis of the relationship between commission and the cost of credit, and the Difference-indifferences Analysis of the Impact of the 2021 ban of Motor Finance DCAs from CP25/27 Technical Annex 1

- 2.9** Participants with approved access to the data room were permitted to remove, in electronic form, relevant information¹ they considered necessary to present or explain their analyses or findings. To remove information, participants needed to make an extraction request to the FCA, upon which the FCA conducted a limited internal review to ensure that the material was suitably aggregated (similar to the level of aggregation in this Technical Annex).
- 2.10** Individuals or firms with access to the data room were also offered a meeting with the FCA's data experts to discuss any queries on redress methodology.

Users of the data room

- 2.11** We received 66 applications for access to the data room from 16 firms. Following assessment against the required competency criteria, 53 applicants were approved and 13 were declined on the basis that they did not provide sufficient evidence of the necessary qualifications or experience.
- 2.12** The 53 approved participants comprised economics consultants acting on behalf of lenders and trade bodies, together with individuals from lenders, consumer groups and professional representatives.
- 2.13** In total, 29 unique users from 11 firms accessed the data room and submitted 34 requests for output extraction. All 11 firms that accessed the data room subsequently submitted a response to the consultation.

1 "Relevant information" as defined in the confidentiality agreement referred to any information obtained from the data room which was

- any Personal Data (which means any information that was contained in the data room which could be used with or without other information outside of the data room to identify an individual),
- any unique identifier whose purpose is to identify a natural or nonnatural person referred within the consultation,
- the name used for any such unique identifier,
- any Confidential Information (has the meaning set out in s348 of the Financial Services and Markets Act 2000),
- any Inside Information (has the meaning set out in Article 7 of the UK version of Regulation (EU) No 596/2014 of the European Parliament and of the Council), and
- any Commercially Sensitive Information (ie information that, if disclosed, would be likely to prejudice the commercial interests of any natural or non-natural person).

Chapter 3

Data guide update

Introduction

- 3.1** Since the publication of CP25/27 we have updated and expanded some of our sources of data on the motor finance market used to underpin our analyses. We have done this where we have deemed it appropriate, proportionate and necessary. This chapter sets out:
- Which datasets have been updated and/or expanded
 - New sources of data used
 - Updated descriptive statistics where relevant
- 3.2** Subsequent chapters focus on new and/or updated analyses.
- 3.3** This updated data guide is not an exhaustive list of all relevant motor finance data collected. We refer readers to the data guide in the CP25/27 Technical Annex 1² for other datasets used (but not updated, such as Credit Reference Agency data) and unchanged descriptive statistics.
- 3.4** We cover the following datasets that have been updated and/or expanded:
- a. Loan level data (LLD)
 - b. Motor finance review data (MRFD)
 - c. Data drop 1 (DD1)
 - d. Data drop 6 (DD6)
 - e. Motor finance commission monitoring survey
 - f. Expanded motor finance commission monitoring survey
- 3.5** Given the LLD is the primary data source for empirical modelling of loss and other factors, we provide further details of specific refinements, such as data cleaning procedures, that we have made to it in response to feedback in the next chapter. In this chapter, we focus on how the dataset has been expanded.

2 Table 2 describes the datasets. Table 14 describes the datasets used for each analysis.

Loan level data (LLD)

- 3.6** The full description of the original LLD can be found in CP25/27 Technical Annex 1 paragraphs 1.18 – 1.26.
- 3.7** In the period following CP25/27, we expanded this dataset with data from an additional lender who had a material market share in the motor finance market (hereafter referred to as “additional lender with significant market share”). As a result, the market coverage in the dataset increased. This inclusion improved the representativeness of the sample, offering a more comprehensive view of the sector and improved the diversity of lending practices captured in the analysis, resulting in a dataset that is more fit for purpose. For confidentiality reasons, we do not disclose specific figures regarding the increase in coverage, as this could compromise the anonymity of the additional lender with significant market share that supplied additional data.
- 3.8** We present summary statistics of the updated LLD (2018-2022 for original submissions, 2017-2022 for the additional lender with significant market share) in the table below. For confidentiality reasons, we do not disclose specific figures regarding the number of observations, as this could compromise the anonymity of the additional lender with significant market share that supplied the data. Non-DCA commission models (flat fees and other non-DCA) represent 48% of all loans in the dataset, while DCA models (increasing DiC, reducing DiC, and scaled commissions) represent 37%. Most loans are arranged as personal contract purchase (51%) or hire purchase (34%) agreements.

Table 1: LLD descriptive statistics, numerical variables (please refer to Glossary Table 1 for a description of the variables)

Variable	Proportion of the sample (%)	Min	Max	Mean	Median	Standard Deviation
Agreed Regular Payment Amount (£)	99.95	0	8,519.32	292.68	253.64	187.83
Annual Percentage Rate (%)	99.95	-1.2	140.7	9.52	8.8	6.72
Balloon Payment Amount (£)	94.63	0	359,999.00	6,406.45	4,111.00	8,987.88
Broker Base APR (%)	44.18	-520	100	8.58	8	8.28
Broker Finance Commission (£)	99.29	-10,830.30	30,665.25	737.49	526.44	784.6
Broker maximum APR (%)	44.1	-520	100	10.31	9.9	8.77
Broker Recommended APR (%)	72.02	0	99.2	10.64	9.9	6.15
Broker Total Earnings (£) ³	65.4	0	16,363.67	530.88	0	836.46
Broker Volume Bonus (£)	74.11	0	21,817.96	80.57	0	407.51
Credit Score	99.3	-998	1,596.00	827.47	935	328.91
Customer annual gross income (£)	82.5	0	8,450,000.00	34,601.27	27,000.00	65,213.89
Deposit Amount (£)	96.68	-2,584.00	2,365,990.00	4,435.80	2,150.00	7,566.20
Discount Applied (£)	45.31	0	13,285.81	179.31	0	587.14
Interest charges included in total charge for credit (£)	66.33	-458.88	84,424.77	3,231.90	2,725.43	2,776.79
Loan Term (months)	100	1	120	47.67	48	10.48
Non-interest charges included in total charge for credit (£)	53.55	-9,717.00	1,257.78	27.45	1	104.14
Original Loan Principal (£)	100	600	364,500.00	16,737.38	13,794.00	13,140.81
Purchase price of vehicle (£)	99.94	-7,328.29	660,000.00	20,507.30	16,650.00	16,430.19
Total cost of credit (£) ⁴	99.95	-7,681.07	84,424.77	3,405.30	2,834.99	2,894.54

Note: All the descriptive statistics are adjusted using sampling weights.

3 The distribution of broker total earnings is very skewed and the median is 0 (less than the median of broker finance commission). This suggests data quality issues with the variable broker total earnings – note that we do not use this variable in the analysis

4 TCC is calculated as the sum of interest and non-interest charges.

Table 2: The LLD descriptive statistics, categorical variables

Variable	Category	Proportion of the sample (%)	Proportion of the sample (weighted %)
Commission Model Category⁵	Reducing DiC	10.31	10.26
	Scaled	23.98	23.87
	Increasing DiC	2.31	2.3
	Flat fee	35.17	35.49
	Other non-DCA	12.59	12.53
	None	4.34	4.32
	Other (unknown)	11.29	11.23
	Missing	0	0
Customer employment status	Employed	52.72	52.9
	Missing	32.63	32.47
	Self-employed	4.84	4.86
	Retired	4.73	4.71
	Unknown	3.42	3.4
	Unemployed	1.52	1.52
	Student	0.14	0.14
Interest Calculation Method	Fixed	37.39	37.21
	Compound	33.13	32.97
	Simple	29.43	29.78
	Other	0.04	0.04
	Missing	0	0
Motor Finance Product Category	Personal Contract Purchase	51.27	51.02
	Hire Purchase	34.14	34.46
	Other	9.96	9.91
	Loan	2.55	2.53
	Lease	1.99	1.98
	Balloon	0.09	0.09
	Missing	0	0

⁵ Please see CP25/27 Diagnostic Report paragraphs 3.6 and 3.8 for details. For other non-DCA's definition, please see paragraphs 4.96 – 4.99.

Variable	Category	Proportion of the sample (%)	Proportion of the sample (weighted %)
Origination Channel Category	Independent Motor Dealer	42.9	42.76
	Franchised Motor Dealer	38.24	38.05
	Online Car and Finance Broker	8.4	8.64
	Unknown	6	5.99
	Other	2.43	2.43
	Online Finance Only Broker	2.03	2.14
	Missing	0	0
Vehicle Condition Category	Used	75.16	75.27
	New	24.84	24.73
	Missing	0	0

Note: The weighted proportion adjusts for firms' relative size in the market using sampling weights.

Motor finance review data (MFRD)

- 3.9** In 2018, we collected the MFRD (see paragraph 3.2 in CP19/28 for the description). In 2024, we collected the LLD by gathering updated data from the same firms that responded to our MFRD request. The MFRD dataset comprises information submitted by 20 motor finance firms on agreements originated in 2017. Although it includes a slightly different set of variables compared to the LLD⁶, all variables used in the DCA Commission Model Impact Analysis in CP25/27 Technical Annex 1 (paragraphs 2.17 – 2.55) were present in the MFRD.
- 3.10** Regarding lender coverage, 16 of the 20 firms in the MFRD also appear in the LLD. However, 2 of the 16 common firms did not submit credit score variables and are not included in the DCA Commission Model Impact Analysis.
- 3.11** The MFRD followed a different sampling rule compared to the LLD. Firms in that sample were asked to submit the lesser of:
- 10% of agreements entered in 2017, or
 - 1,000 agreements
- 3.12** We discuss how we account for this different sampling method below.
- 3.13** Summary statistics for the MFRD are presented below. In 2017, flat fee loans accounted for 36% of total loans, while DCA loans (increasing DiC, reducing DiC, and scaled commissions) represented approximately half of the total loans. Similarly to the LLD,

⁶ We did not request data from two lenders who stopped operating in the motor finance market after 2017. One lender did not respond to the data request.

most loans are arranged as personal contract purchase (46%) or Hire Purchase (39%) agreements. The typical loan, as characterised by the median, is £11.2k and has an APR of 8%.

Table 3: MFRD descriptive statistics, numerical variables

Variable	Number of non-missing observations	Proportion of the sample (%)	Min	Max	Mean	Median	Standard Deviation
Annual Percentage Rate	15,672	100	0	84.89	10.22	8.08	7.83
Credit Score	14,301	91.25	117	1,396	855.18	931	268.43
Deposit Amount (£)	15,507	98.95	0	115,000	2,860.06	1,050	5,288.33
Loan Term (months)	15,672	100	10	120	46.03	48	10.69
Original Loan Principal (£)	15,672	100	572.79	167,351	13,794.46	11,242.65	10,752.45

Note: All the descriptive statistics are adjusted using sampling weights.

Table 4: MFRD descriptive statistics, categorical variables

Variable	Category	Number of observations	Proportion of the sample (%)	Proportion of the sample (weighted %)
Commission Model Category	Flat fee	6,516	41.58	35.81
	Increasing DiC	3,817	24.36	22.95
	Reducing DiC	1,985	12.67	15.13
	Scaled	1,834	11.7	12.13
	Other	968	6.18	9.54
	None	392	2.5	3.86
	Profit share	160	1.02	0.56
	Missing	0	0	0

Variable	Category	Number of observations	Proportion of the sample (%)	Proportion of the sample (weighted %)
Motor Finance Product Category	Personal Contract Purchase	3,495	22.3	46.49
	Hire Purchase	9,916	63.27	38.79
	Other	1,095	6.99	5.69
	Lease	184	1.17	4.53
	Loan	887	5.66	3.51
	Balloon	95	0.61	0.99
	Missing	0	0	0
Origination Channel Category	Franchised Motor Dealer	5,301	33.82	60.55
	Independent Motor Dealer	5,187	33.1	22.7
	Unknown	1,359	8.67	5.77
	Other	603	3.85	5.31
	Online Finance Only Broker	2,422	15.45	4.01
	Online Car and Finance Broker	800	5.1	1.67
	Missing	0	0	0
Vehicle Condition Category	Used	13,645	87.07	69.63
	New	2,023	12.91	30.33
	Missing	4	0.03	0.04
Year Signed	2017	15,672	100	100
	Missing	0	0	0

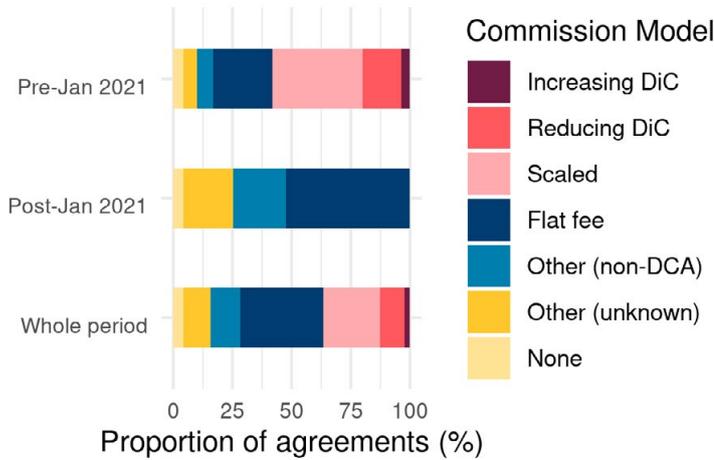
Note: The weighted proportion adjusts for firms' relative size in the market using sampling weights.

3.14 We discuss the use of this dataset in the next chapter.

Descriptive Statistics of the updated LLD and MFRD

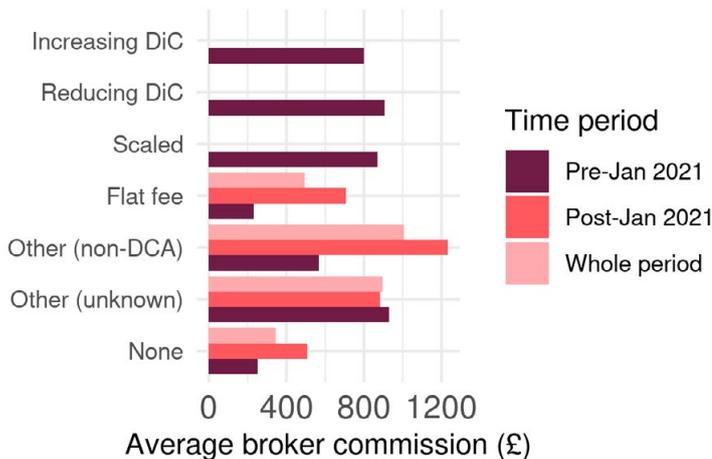
- 3.15** In CP25/27 Technical Annex 1, we presented a series of charts of descriptive statistics from the LLD's 2019-2022 sample, containing 445,470 agreements across all commission models.
- 3.16** New versions of the figures (originally Figures 4,5,6 and 7 from CP25/27 Technical Annex 1) have been added below, alongside accompanying text, to reflect the updated LLD.
- 3.17** Similarities and differences between descriptive statistics figures included in Technical Annex 1 of CP25/27 and PS26/3 can be summarised as:
- **Sample size:** The sample size increased by just under 56%, including the data added for 2017 and 2018 (the original LLD only contained data from 2019-2022), as well as the additional lender (the original LLD contained 18 firms, the updated LLD contains 19).
 - **Agreement share:** We gathered extra information from several lenders regarding loan agreements originally labelled as "other" commission model. Extra information was also gathered from an additional lender with significant market share. This extra information led to an increase in the proportion of agreements across our entire sample marked as "other" (either "other non-DCA" or "other unknown").
 - **Pre-ban averages:** Other (unknown) commission models had the highest mean value of broker commission (£) and APR (%) prior to the January 2021 DCA ban in the updated LLD. In the original LLD, reducing DiC models had the highest mean value of broker commission in the pre-ban period, and scaled models had the highest mean value of APR in the pre-ban period.
 - **Whole period averages:** Other (non-DCA) commission models had the highest mean value of broker commission (£), and other (unknown) commission models had the highest mean value of APR (%), across the whole period of our sample (2017-2022) in the updated LLD. This is consistent with the original LLD, where "other" commission models had the highest values of both broker commission (£) and APR (%) across the whole sampling period.
- 3.18** In the updated LLD, 63% of agreements had a loan origination date prior to the FCA's January 2021 motor finance DCA ban, and 37% of agreements were originated after the ban.

Figure 1: Agreements by commission model, 2017-2022



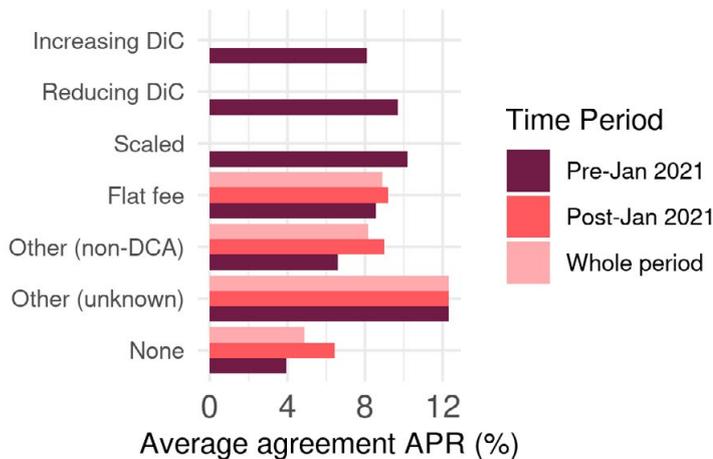
3.19 Pre-January 2021, 58% of agreements were DCA (increasing DiC, reducing DiC or scaled), 25% were flat fee and 7% other non-DCA. After January 2021, there were no DCA agreements – of the loans in our sample, 52% were flat fee, and 22% other non-DCA. Details on the loans reported under “other” and “none” in the updated LLD can be found in the Analysis of Loss update section.

Figure 2: Average (mean) broker commission (£) by commission model, per agreement, 2017-2022



3.20 The figure above shows that brokers were likely to earn higher commission from DCA agreements than non-DCA agreements in the pre-ban period. Prior to the DCA ban, the average broker commission for a DCA agreement was £876.34 (average computed as the weighted mean of increasing DiC, reducing DiC, and scaled commission model agreements), for a flat fee agreement was £232.91, and for other non-DCA agreements £565.42. Following the DCA ban, the mean broker commission for a flat fee agreement increased to £705.90 and for other non-DCA agreements to £1,233.44.

Figure 3: Average (mean) APR by commission model, per agreement, 2017-2022



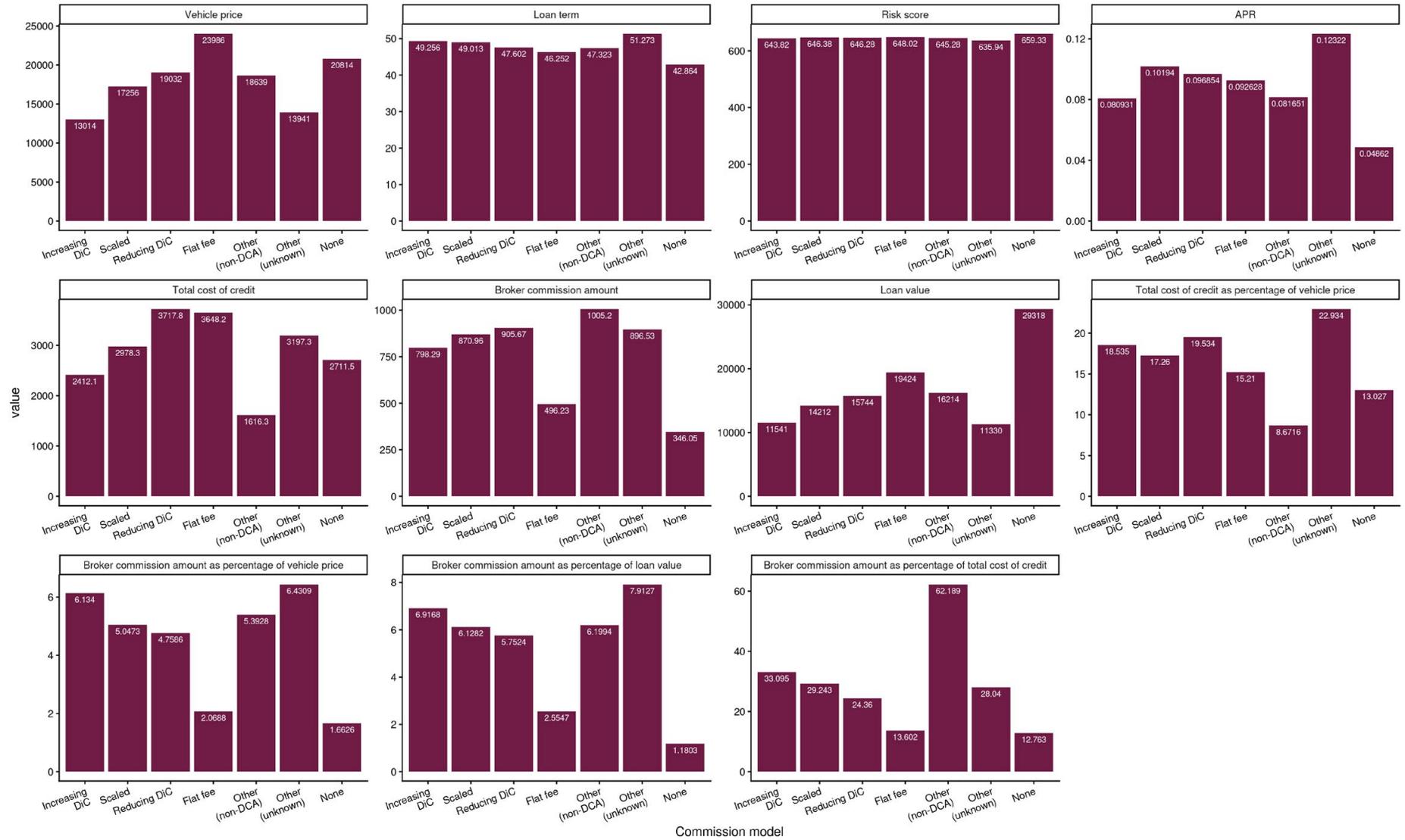
3.21 The figure above shows that mean APR (a measure of interest costs and fees paid by the consumer) was generally higher for DCA agreements than for flat fee agreements. Prior to the DCA ban, in this dataset the mean APR for DCA agreements was 9.92% (weighted mean of increasing DiC, reducing DiC, and scaled commission models), for flat fee agreements 8.55%, and for other non-DCA agreements 6.61%. Following the DCA ban, the mean APR for flat fee agreements in our sample increased to 9.2%, and for other non-DCA agreements to 8.98%.

3.22 The figure below shows how key characteristics of loans differ across commission model types.⁷

- Flat fee loans were made on more expensive cars. This is because most new cars are sold through flat fee loans.
- The median loan term of DCA agreements (49 months) is similar to the median loan term of non-DCA agreements (48 months).
- The median credit score of DCA agreements is slightly higher than the median credit score of non-DCA agreements.
- Reducing DiC and scaled commission agreements have higher agreement APRs than flat fee and other non-DCA agreements, increasing DiC agreements do not. All DCA models have a higher total cost of credit as a proportion of the vehicle price than flat fee and other non-DCA agreements.
- Other non-DCA models (ie non-DCA models that are not flat fee agreements) have the highest mean broker commission, when expressed in absolute (£) terms, followed by reducing DiC models. When expressed in proportionate terms (ie broker commission as a percentage of vehicle price), other commission models (unknown whether they are DCA or non-DCA models) have the mean highest commission, followed by increasing DiC models.

⁷ Note: to obtain "total cost of credit as percentage of vehicle price" and "broker commission amount as percentage of vehicle price" we calculate the ratio of the average amounts.

Figure 4: Key features by commission model type



Data drop 1 (DD1)

- 3.23** In CP25/27, our redress liability estimates used the dataset known as DD1 which is made up of high-level agreement data from 34 firms within the UK motor finance market (circa 89% market coverage)⁸ from 6 April 2007 to 25 October 2024. Full details of this data can be found in the Data Guide in CP25/27 Technical Annex 1.
- 3.24** We identified some data discrepancies among a small number of firms' submissions and received new data from some firms. Where we identified data discrepancies, we replaced the affected records with the updated data. We also appended DD1 with the additional data provided by some firms. For a comprehensive overview of the data collected and descriptive statistics, refer to the Updated Diagnostic Report (Diagnostic Report: FCA review of motor finance commission arrangements – March 2026 update).
- 3.25** We reviewed a sample of 36 agreements across 7 firms to assess whether volume bonuses were accurately reported in DD1 submissions. This sample was taken from a limited number of case files we assessed in our work on tied or panel relationships and the disclosure of right of first refusal in the Updated Diagnostic Report⁹, where we also held the underlying agreement documents from our s166 review. The review provided reasonable assurance that, in most cases, firms were correctly reporting volume bonuses as commission in DD1. We did identify several exceptions linked to adjustments, retrospective bonus arrangements, or missing documentation. Two agreements could not be verified due to absent evidence, though neither involved a volume bonus.

Data drop 6 (DD6)

- 3.26** In the period following CP25/27, we collected data from 9 captive firms and 3 white label firms on the number of agreements that were potentially heavily impacted by tied arrangements. This dataset covers the period from 6 April 2007 to 1 November 2024. For more information about the collection process and descriptive statistics on DD6, refer to the Updated Diagnostic Report.

Motor finance commission monitoring survey

- 3.27** On 12 April 2024 we issued a Dear CEO letter to lenders and brokers in the market, reminding them of their potential liabilities and alerting them to an upcoming data collection that would track firms' financial resources and DCA-related complaint volumes. We began collecting data on a quarterly basis from May 2024. The motor finance commission monitoring survey collects evidence on how motor finance lenders and brokers plan to maintain adequate financial resources, manage the additional operational costs arising from increased cases, manage the additional operational costs arising from increased cases and, where relevant, the costs of resolving those cases.

⁸ Based upon number of outstanding agreements in June 2024.

⁹ See paragraphs 5.82 – 5.91 of Updated Diagnostic Report.

- 3.28** We have now collected 6 rounds of data from 32 lenders covering 89% of the market. We expanded the dataset in early 2025 (Round 5) to include questions on compliance costs associated with any future redress scheme, as detailed in CP25/27 Technical Annex 1. On 15 December 2025 we issued the sixth round of the survey, which included the following questions, in addition to our typical financial resilience questions:
- a.** We would like to understand the estimated total redress which may have involved inadequate disclosure of a DCA, a high commission, tied arrangement or very high commission agreement, as set out in Chapter 8 of CP25/27. (£)
 - b.** We would like to understand the number of regulated consumer motor finance agreements over the period, which may have involved inadequate disclosure of a DCA, a high commission, tied arrangement or very high commission, as set out in Chapter 8 of CP25/27. (No.)
 - c.** What is the estimated one-off investment your firm expects to make in the future, in systems, capital, or infrastructure specifically to manage our proposed consumer redress scheme. (£)
 - d.** What is your firm's estimated average time to screen an agreement as per our proposed consumer redress scheme? (minutes)
 - e.** How many FTE staff does your firm expect to allocate to screening agreements under our proposed consumer redress scheme? (FTE)
 - f.** What is the average total hourly cost per staff member assigned to complaint handling involved in our proposed consumer redress scheme? (£)
 - g.** What is your firm's estimated average time to process a single complaint within our proposed consumer redress scheme, from initial receipt to final resolution? (minutes)
 - h.** What is the average total hourly cost per staff member assigned to screening agreements under our proposed consumer redress scheme? (£)
- 3.29** We asked firms to base their redress liability estimates and operational cost calculations on the approach set out in Chapter 8 of CP25/27.

Expanded motor finance commission monitoring survey

- 3.30** We also sent a motor finance commission monitoring survey to 39 smaller firms, within the scope of the motor finance scheme for which we had not previously carried out proactive financial resilience analysis. This was because our initial monitoring focused on the firms that made up the majority of the market rather than on the relatively smaller firms.
- 3.31** Since CP25/27 was published, firms have been able to more accurately assess their potential liabilities and, therefore, we felt it was necessary to undertake a proactive assessment of financial resilience risks present in the relatively smaller market participants. The purpose was to identify those with weak financial resilience to enable timely supervisory interventions.

- 3.32** Just as in the motor finance commission monitoring survey, we sought to understand these smaller firms' financial position and their ability to meet the estimated liability figures. Specifically, we asked the firms to submit the following list of data points:
- a.** Total estimated redress liabilities under DCA, high commission and very high commission, undisclosed tied arrangements (£)
 - b.** Total redress provisions made (£)
 - c.** Total redress already paid (£)
 - d.** Total non-redress liabilities and/or provisions which do not relate to motor finance commission liabilities (£)
 - e.** Net asset/liability position (£)
 - f.** Total liquid assets (£)
- 3.33** This expanded motor finance commission monitoring survey used fewer, higher-level questions to reflect a proportionate approach for smaller firms. We also asked several additional questions to identify the firm's market segment, legal structure and business identity, which are not listed above.
- 3.34** We used the answers to question (a) as an input within our estimation of the market-wide redress liabilities which relate to tied arrangements, for smaller firms.

Chapter 4

Analysis of loss update

Summary

- 4.1** We received extensive feedback on our analysis of loss. Much of this technical feedback was received from law firms and economic consultancies, submitting on behalf of firms, trade bodies, and consumer groups/representatives.¹⁰
- 4.2** Several respondents (trade bodies and firms) commented on the “DCA Impact of Disclosure” (CP25/27 Technical Annex 1 paragraphs 2.135 – 2.142) and “Non-DCA Impact of Disclosure” analysis (CP25/27 Technical Annex 1 paragraphs 2.143 – 2.163), arguing that the premise of harm from non-disclosure was not supported by our empirical work and there was no credible causal link demonstrated between non-disclosure and consumer harm.
- 4.3** We acknowledge this feedback and the limitations of these two analyses. However, since both pieces of analysis on the impact of disclosure, as presented in the CP25/27, were indicative and caveated, and have not directly informed the policy set out in the final PS we have not revised this analysis. We, nonetheless, do provide a response to feedback below.
- 4.4** This issue notwithstanding, most technical feedback received focussed on three key areas: the LLD used, the DCA Commission Model Impact Analysis, and the analysis of the relationship between commission and the cost of credit for flat fee loans.
- 4.5** Other feedback received covered:
- **The LLD:** respondents raised concerns on the quality and coverage of this data set, as well as the time period covered.
 - **The DCA commission model impact:** feedback centred on whether it could correctly measure loss, on perceived statistical flaws in the underlying models, and the choice of counterfactual and how results were being applied across the market.
- 4.6** The relationship between commission and cost of credit for flat fee loans: respondents raised questions on the results and reliability of the analysis.
- 4.7** The difference-in-difference analyses of the impact of the 2021 ban of DCAs: respondents raised questions on the results and reliability of the analysis.
- 4.8** We have carefully considered feedback received and, where appropriate and proportionate, made a series of improvements to the data used and our analysis.

¹⁰ Throughout this document, when discussing feedback received, we will attribute it to the broader group – lenders, trade bodies, or consumer groups/representatives – rather than to the economic consultancies and law firms that often submitted on their behalf.

Main findings from updated analysis

- 4.9 Data:** regarding the LLD, we added data from an additional lender with significant market share to improve the sample coverage, expanded the time horizon, revisited cleaning procedures to improve data quality and completed a market representativeness assessment.
- 4.10 Modelling, DCA Commission Model Impact Analysis:** we updated the *DCA Commission Model Impact Analysis* in response to technical feedback and undertook extensive work to test the estimate of an APR adjustment of APR-17. In our updated analysis, which includes additional data, we estimate that the APRs for non-DCA loans were on average 17% lower than the APRs for DCA loans in 2017-2021, and on average 13% lower in 2019-2021.
- 4.11 Modelling, Analysis of the relationship between commission and the cost of credit for flat fee loans:** we updated the *analysis of the relationship between commission and the cost of credit for flat fee loans* model. The extension shows a positive relationship between the commission level and the cost of credit but limited clear evidence that the association between broker commission and total cost of credit increases with commission level.
- 4.12 Other models:** the results from CP25/27 Technical Annex 1 regarding *Difference-in-Differences analysis of the impact of the 2021 ban of motor finance DCA, DCA impact of disclosure, and Non-DCA impact of disclosure* were indicative due to data and time constraints, and did not directly inform the policy proposed. While we did not update those models, we have included summaries of the technical feedback received on each of these models.
- 4.13** Overall, in response to feedback we received, we have made significant changes to the data and analysis. In our view, these changes improve the robustness of our analysis and strengthen our evidence base for the analysis of loss with respect to DCA motor finance agreements.
- 4.14** We have set out the limitations of our analyses and the inference that can be drawn from them.
- 4.15** We also received extensive feedback regarding the lack of empirical evidence on tied arrangements provided in CP25/27 Technical Annex 1. We carefully considered this feedback and present our response in the next chapter of this annex.

Impact of disclosure

Our position in CP25/27

DCA Impact of Disclosure

- 4.16** In CP25/27 Technical Annex 1 we described how we tried to use linear regression methods to test whether the non-disclosure of the existence and/or nature of commission for DCA agreements increased the cost of borrowing via consumers' total cost of credit as a proportion of their original loan principal.

4.17 This analysis was subject to limitations. The data used in this analysis comprised of the s166 skilled person review and DCA casefile review, neither of which contained sufficient instances of adequate disclosure (see paragraphs 2.4 and 2.5 in the Updated Diagnostic Report). Specifically, 0% of casefiles involved customers being informed of a DCA, and 23% of non-DCA casefiles having customers being told the nature of the commission arrangement. This meant we were unable to robustly analyse the available data due to the sample size being too small to reliably identify any effect of disclosure.

Non-DCA Impact of Disclosure

4.18 In CP25/27 we presented analysis of whether the non-disclosure of the existence and/or nature of commission for non-DCA loans increased the consumer's cost of borrowing via the agreement APR. To test our hypothesis, we used the DD2 dataset which contained agreement specific data for 599 non-DCA loan agreements. While there were enough observations in the DD2 dataset with some evidence of disclosure to conduct empirical analysis, we noted that the level of disclosure assessed across these observations 'would [not] be considered [to have] adequate disclosure in the proposed scheme' (CP25/27 Technical Annex 1 paragraph 2.145).

4.19 We estimated two linear regressions with fixed effects, using APR (or log (APR)) on a binary variable for any level of disclosure, controlling for observable characteristics of the loan agreement. We considered two levels of disclosure: no disclosure and any disclosure. We found that any level of disclosure was associated with a decrease in APR of 3.4 percentage points compared to loans where there was no disclosure (weakly statistically significant result). However, we acknowledged that the results were purely indicative rather than conclusive due to a small sample size, no direct measure of consumer credit risk and the results being sensitive to functional form assumptions. The analysis did not give us an accurate picture of the existence and the scale of the impact.

Stakeholder feedback

4.20 We received feedback on the analysis we conducted on the disclosure of both DCA and non-DCA agreements. Many respondents (firms and trade bodies) argued that the premise of the proposed scheme – that consumers would have acted differently had they been properly informed of the commission arrangement – was not supported by our suite of empirical analyses. One lender said that the impact of disclosure was not the focus of the analysis presented in CP25/27, and that limited empirical testing had been undertaken to assess whether disclosure of commission arrangements materially changes consumer behaviour.

4.21 Firms and trade bodies further commented that the evidence reviewed, including the econometric work published in CP25/27, did not demonstrate a credible causal link between non-disclosure and consumer detriment, nor did it show that disclosure meaningfully influences consumer decisions. They remarked that their own research suggested customers primarily focus on monthly payments and tend to shop around regardless of commission disclosure.

4.22 Relating to the non-DCA impact of disclosure, one trade body noted that no controls or borrower characteristics were included meaning that there can be no causal interpretation discerned from the 3.4 percentage point reduction in APR under disclosure, compared to no disclosure of non-DCA agreements.

Our response

4.23 We acknowledge the feedback regarding the lack of controls, which meant the results could not be interpreted causally. However, we noted that both pieces of analysis on the impact of disclosure, as presented in the CP25/27, were indicative due to these limitations and have subsequently not directly informed the final policy design set out in PS26/3.

4.24 Please refer to paragraphs 4.78 – 4.81 for an extensive discussion on the issue regarding loss arising from inadequate disclosure, and paragraphs 4.91 – 4.95, for a discussion of one respondent's alternative evidence regarding the potential effect of disclosure, using the post-October 2024 period (following the Court of Appeal ruling) as a proxy for an environment of improved disclosure.

Loan level data (LLD)

Our position in CP25/27

4.25 The LLD, as presented in CP25/27, contained agreement-level information from 18 firms' agreements between 2019-2021.

4.26 In CP25/27 we used the LLD for our empirical analysis of the DCA Commission Model Impact Analysis and the analysis of the relationship between commission and the cost of credit for flat fee loans models. We compared the consumer outcomes for agreements where the broker was paid an increasing DiC, reducing DiC or scaled commission (DCA loans) with outcomes where the broker was paid a flat fee commission (non-DCA loans).

4.27 When noting the limitations of the dataset in CP25/27, we noted that the data may not be representative of the market¹¹. Further details of the modelling approach used can be found in CP25/27 Technical Annex 1.

Summary of technical consultation feedback

4.28 We received extensive feedback on the LLD. Some respondents (firms, trade bodies, and consumer groups) raised the following:

- **Lack of representativeness:** respondents remarked that a sample that consisted of only 18 firms was too small to fully capture the diversity and complexity of the entire motor finance market. The consequence of this was, based on their view, that the conclusions of our analysis using that sample may not have been representative of the whole sector, or this sample of firms may have been unsuitable to be reliably generalised across the sector.

11 CP25/27 Technical Annex 1, Table 2.

- **Limited timeframe:** the data timeframe was limited (Jan 2019 – Jan 2021) and includes the COVID-19 pandemic that added to volatility in the used vehicle market. Further, some respondents observed that this was a period of high regulatory scrutiny and lender behaviour was already changing. They noted the sample period may not have reflected normal market behaviour and misconduct as it was a time of transition, making it unsuitable to compute loss over a wider timeframe. Respondents told us that lender behaviour and consumer outcomes in Jan-2019 to Jan-2021 were not representative of behaviour and outcomes before 2019.
- **Data quality issues:** the dataset lacked sufficient filtering and checks on data quality. Feedback highlighted erroneous datapoints, such as datapoints with a negative value for deposit amount, loan term or APR. Such datapoints could have introduced bias into the final estimates, leading to over- or under-estimates of harm.

Our response

4.29 We carefully considered this feedback and made the following improvements to the LLD:

- We added an additional lender with significant market share to the sample
- We expanded its time horizon
- We revisited our cleaning procedures
- We assessed the data's representativeness

4.30 Below we provide further details.

Adding an additional lender with significant market share to the sample

4.31 Some respondents told us that in their view, a sample of 18 firms was too small to reflect the full diversity and complexity of the motor finance market.

4.32 We expanded the dataset with an additional lender with significant market share who agreed to submit their data. Please refer to the data guide for further details.

Addition of 2017 and 2018 data

4.33 Our analysis of loss in the CP25/27 Technical Annex 1 used the LLD data from 2019-2021. We did not include the 2017–2018 LLD in the CP25/27 Technical Annex 1 models because this earlier data had known issues. We prioritised internal validity, ensuring the results were based on data that was the most consistent, comparable, and suitable for robust modelling and, therefore, focused on using the cleanest data.

4.34 Some respondents told us that in their view, this period coincided with the COVID-19 pandemic and therefore reflected abnormal market conditions that were not representative of the wider redress scheme period. They argued that the COVID-19 pandemic and the global chip shortage led to significant volatility in the used-vehicle market, resulting in higher vehicle prices and lower sales volumes.

4.35 We explored utilising the s166 review and data drop 2 (DD2)¹² data to address the limited timeframe concern. These datasets contained information that covered the period 2007-2021. However, both datasets had small sample sizes and missed key variables (in particular, credit score is not available in DD2). The purpose of the s166 dataset was for the skilled person to gather evidence regarding firm practices in connection with historical motor finance commission arrangements, rather than to conduct extensive data modelling. Hence using s166 and DD2 datasets would not have been sufficient to adequately address concerns regarding the limited time coverage of *DCA Commission Model Impact Analysis*. Therefore, we could not re-estimate the APR-adjustment model using this data.

4.36 We further explored utilising the DD1 dataset to complement the current APR-adjustment model. However, this option lacked crucial variables used in the APR-adjustment model. For example, credit score, which was needed in the model as it is a key explanatory variable for APR, was missing. Without these variables to control for, we would not have been confident that the comparison between DCA and non-DCA loans could be considered like-for-like, which may have undermined the robustness of the findings.

4.37 Taking all these factors into account, we decided to extend the LLD, both through using additional years of available data and including an additional lender with significant market share (as described above). While the earlier data had known issues, we addressed these using appropriate statistical methods so that the estimate is more temporally representative.

4.38 The 2017 data is sourced from the MFRD. Please refer to the data guide for further details. The 2018 data is sourced from the LLD. The number of firms who submitted the data varies by year for the LLD, and we therefore needed to re-weight firms for which we lacked 2018 data (in the original LLD, 7 of 18 firms are missing 2018 data) before proceeding with the modelling so that these firms were not underrepresented in our estimates. To address this potential bias, we applied an imputation procedure based on resampling from 2019. We discuss this procedure in the analysis section below. The following table summarises how we extend our dataset to 2017-2021.

Table 5: summary of data source, known issues, and resolution

Date range	Source	Issue	Resolution
2017	Motor Finance Review (MFR)	Different sampling	Re-weighting
2018	The LLD	Missing data for some firms	Re-sampling from 2019-2021
2019-2021	The LLD	Timeframe is too short	Extending the dataset

4.39 For the MFRD, firms were asked to submit the lesser of:

- 10% of agreements entered in 2017, or
- 1,000 agreements

¹² CP25/27 Technical Annex 1 paragraphs 1.62 – 1.67.

Based on this rule, given the sampling was different to the LLD, we calculated weights as follows:

- Firms with fewer than 1,000 agreements: Weight = 10
- Firms with exactly 1,000 agreements: Weight = $N/1,000$, where N is the total population of agreements for the firm

4.40 We derived N from the DD1 dataset for 16 out of 19 firms. For the remaining three firms (not present in DD1), we used population numbers from aggregated motor finance firm data collected in 2023. Our external statistician agreed with how these weights were constructed.

4.41 For the 2018 data, we had missing data for 7 of the 19 firms. To ensure that the data was representative of the firms in the sample, we needed to impute data for firms for which we lacked 2018 information before estimating the model. We implemented the following procedure:

1. Identified all firms missing 2018 data within our sample.
2. Estimated the total number of agreements for these firms in 2018 using a two-step approach based on available data sources:
 - a. If a firm appeared in the DD1 dataset, we used the agreement count recorded for 2018 in DD1.
 - b. If a firm did not exist in DD1, we referred to the CRA dataset (see the CP25/27 Technical Annex 1, Table 2), to obtain its 2018 agreement count.
3. Sampled the number of agreements obtained in step (2) from each firm's 2019 data.
4. Relabelled the year of the agreement from 2019 to 2018.
5. Agreements in 2018-2021 had a weight of $365/36$, since we requested firms to submit agreements corresponding to 36 selected days of birth of the borrower.

4.42 We applied the above procedure for six out of the seven firms for which we had missing 2018 data. The firm we omitted only submitted data for agreements commencing in 2019. Even if we had imputed data in 2018, we would still have missed information for the rest of the time series. Therefore, we did not impute data for this firm, although it is still included in the overall analysis. In the final estimation 17.6% of the 2018 sample was imputed before we filtered any agreements.

4.43 Since we introduced randomness into the process through our sampling of 2019 agreement data, we employed bootstrapping techniques to estimate the APR-adjustment. We repeated the imputation procedure described in steps 1-5 above 30 times¹³ and estimated the DCA Commission Model Impact Analysis model, and resultant APR adjustment, for each dataset. Bootstrapping allowed us to verify the stability of the estimates across each imputation. This approach ensured that our results reflected both the variability inherent in the data and the additional uncertainty introduced by the sampling procedure.

13 Prior to ingesting the new submission, we conducted 100 replications for each model to assess the stability of the estimates. The results demonstrated consistent stability across these runs. Consequently, we decided to reduce the number of replications to 30 in subsequent analyses, as this adequately balances computational efficiency with the reliability of our findings.

- 4.44** In essence, we utilised each firm's 2019 data as a suitable proxy for 2018. The rationale for not simply weighting up the firms' 2019 figures was that a regression needed to be performed during the APR-adjustment step, where the specific year was an important variable within the model. Our external statistician advisor reviewed this methodology and is satisfied with the approach outlined above. Their endorsement provides further confidence in the robustness of these estimation procedures.
- 4.45** We also considered using firms for which we had data in 2018 as a proxy. Specifically, resampling agreement counts from the 2018 data set across firms, rather than relying on within-firm 2019 figures. The choice between these two approaches ultimately hinged on whether the subset of firms that submitted 2018 data was more representative of the missing firms, or if the within-firm year-on-year consistency offered a closer approximation. This consideration reflected the trade-off between leveraging cross-sectional versus longitudinal proxies when addressing gaps in the dataset.
- 4.46** We chose to leverage longitudinal information at the firm level rather than impute missing firms with those that provided information. Our sample included heterogeneous firms, and missing firms were likely to differ from non-missing ones in various observable and unobservable ways. Therefore, using available contemporaneous data to fill gaps may have biased the sample toward the characteristics of non-missing firms.
- 4.47** In support of our choice to use each firm's 2019 data as a proxy, we tested the temporal stability of the main characteristics used in the analysis. This evidence was computed using the 2019 to 2021 information. For continuous variables we computed yearly averages (and their standard deviation), the coefficient of variation and min-max range. For categorical variables we computed the number of times the categories changes, the share of the dominant category and the mode. Note again that these measures of temporal stability are defined for six of the seven missing firms, as one firm only submitted information for 2019 agreements.
- 4.48** In accordance with our confidentiality obligations under section 348 FSMA¹⁴, we have redacted the results of this exercise to prevent the disclosure of any individual firm identities. We found that the categorical variables are very stable over 2019-2021. In general, firms used credit scores from the same bureau, specialised in either used / new vehicles, motor finance product, commissioning model and original channel. The continuous variables displayed more variation over time. Risk score, original loan principal and loan term were the most stable variables over time for all firms (having a low to moderate coefficient of variation, never exceeding 0.11). The APR was stable for most firms except two, where the coefficient of variation was over 0.13. The deposit amount had the highest variability, with a coefficient of variation higher than 0.20 for most firms except one. Based on this evidence, we found it reasonable to assume that the main firm characteristics remain stable over time, which supported the assumption underlying our imputation procedure.

14 Section 348(1) prevents us (subject to certain exceptions) from disclosing confidential information without the consent of the person who provided us with the information, and the person about whom the information relates, if a different person. Confidential information is defined as information which is not in the public domain relating to a person's business or other affairs, which we have received in relation to our functions under FSMA (and other legislation, including the Financial Services Act 2012).

4.49 In summary, the updated dataset combined two data sources collected using different sampling strategies and was partially based on imputations of missing agreements in 2018. We mitigated the effect of different sampling strategies by reweighting the data accordingly. We also believe that our imputation procedure leads to conservative estimates of harm¹⁵ as we imputed data for 2018 using agreements in 2019, a year for which we hypothesise that harm was overall lower. As discussed in PS26/3 paragraph 1.29, available evidence suggests that more harmful forms of DCA were more prevalent in earlier years.

4.50 This extension enables a more comprehensive view of market conditions prior to 2019, helping to mitigate the impact of pandemic-related anomalies and improving the representativeness of the analysis. By broadening the dataset, our analysis now reflects a wider range of lending practices.

Revisiting data cleaning

4.51 To address concerns around data quality, we revisited our cleaning procedures to exclude data errors and anomalies. First, we discarded observations which were either erroneous or incomplete. These included observations where (i) either the deposit amount, loan term, loan amount, or APR was negative or missing or (ii) the deposit amount was greater than the purchase price of the vehicle. Second, we discarded observations which were not in scope of the scheme such as lease agreements. Finally, we discarded zero APR agreements as they are now excepted from the definition of a relevant arrangement. The table below shows the proportion of observations dropped at each stage.

Table 6: Data cleaning procedure

Cleaning Type	Detail	Percent coverage
None	Initial dataset, with new lender and 2017 & 2018 data (2017-2021)	100.0
Error	Deposit amount less than 0 or missing	96.6
Error	Loan term less than 0 months or missing	96.6
Error	Loan amount less than 0 or missing	96.6
Error	APR less than 0 or missing	96.6
Error	Deposit greater than purchase price	96.4
Not in scope	Lease agreement ¹⁶	96.3
Regression model	NA in at least one of 10 columns	92.6
Log specification/ excepted from the definition of a relevant arrangement	APR equals 0	86.2

15 The estimate is lower than it would be if the actual 2018 data were available. This is because we expect harm to have been greater in earlier years (see PS26/3 Chapter 11 for detail discussion), but for firms missing 2018 data we impute their values using 2019 data, when harm was lower. As a result, the overall estimate incorporates imputed data that reflects 'less harm' than we believe would have been observed in 2018.

16 See CP25/27 paragraph 1.38.

4.52 Removing these erroneous observations addressed concerns regarding invalid datapoints. All subsequent specifications presented here were estimated with these erroneous observations excluded, ensuring any bias introduced by them was eliminated.

4.53 We ran our main specification including outlier observations but removed outliers in a series of robustness checks. We considered three methods of removing outliers as shown in the table below.

Table 7: Outlier definitions

Method	Detail	Percent coverage
None	Full dataset after cleaning	100.0
Outlier: 1%	Remove deposit or loan amount in top 1%	98.2
Outlier: 0.1%	Remove deposit or loan amount in top 0.1%	99.8
Outlier: £100k	Remove deposit or loan amount over £100k	99.8

4.54 These changes improved the quality of the data and improved the robustness of the analysis using the data.

Assessment of the representativeness of the updated LLD

4.55 To assess the representativeness of the updated LLD, we compared it against Data Drop 1 (DD1)¹⁷, which was designed to be representative of the whole motor finance market after reweighting. Since DD1 was randomly sampled based on strata, it covered 89% of the regulated UK motor finance market, based on number of outstanding loan agreements in June 2024. Once reweighted, it was representative of over 99% of the market – see CP25/27 Technical Annex 1 paragraphs 3.56 – 3.60 for further details.

4.56 There were two parts to our assessment. First, we analysed the distribution of key variables (commission model, APR and commission amounts) using descriptive statistics. This allowed us to evaluate whether the LLD was comparable to DD1 in terms of the typical values, degree of variation, and composition of these variables, and to identify any material differences that could have affected representativeness. Second, we used regression analysis to test whether the relationship between APR and commission model was sufficiently similar across the two datasets. This enabled us to assess whether the revised LLD preserved the underlying economic relationships relevant for the analysis of loss.

4.57 This exercise focused on the period from January 2017 to January 2021. While the DCA Commission Model Impact Analysis presented in the CP25/27 Technical Annex 1 covered the period from January 2019 to January 2021, this exercise also included data from 2017 and 2018 (explained above), as well as observations from the additional lender.

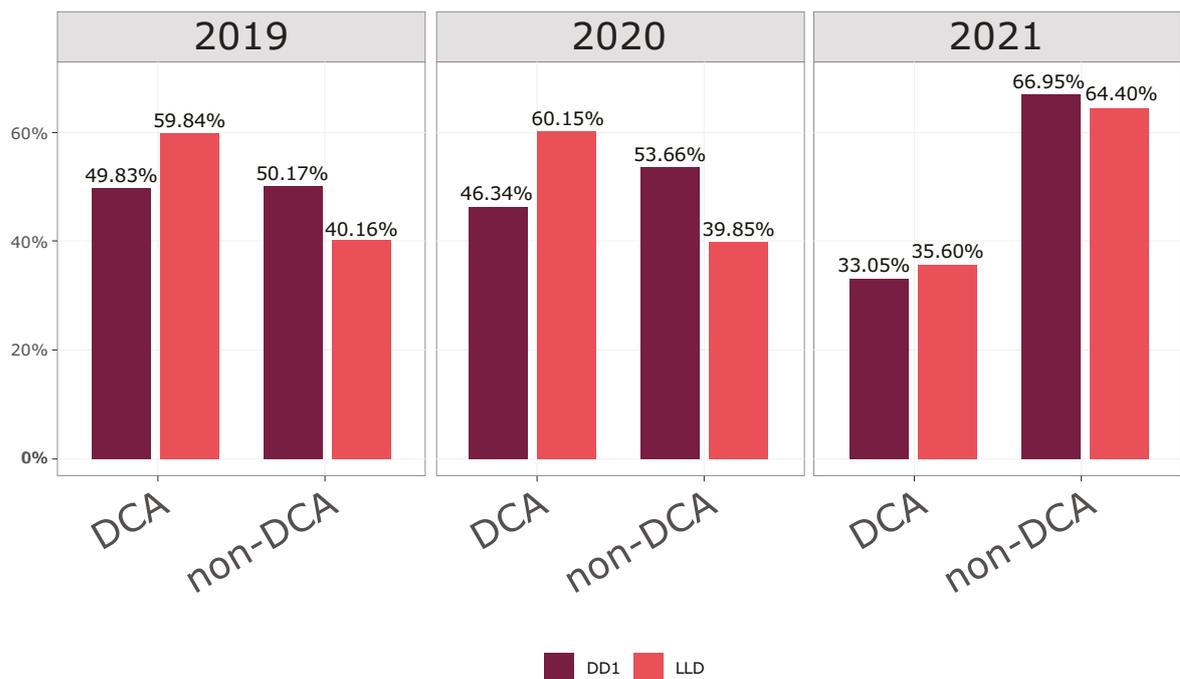
¹⁷ CP25/27 Technical Annex 1 paragraphs 1.31 – 1.60.

4.58 To ensure analytical consistency, we applied the same data cleaning procedures used for the DCA Commission Model Impact Analysis to both the LLD and DD1. These procedures aimed to address data quality concerns and ensure that errors and anomalies were appropriately handled. The data cleaning procedures result in excluding approximately 1% of the DD1 loans granted between January 2017 and January 2021. Given the small proportion of observations affected, this is unlikely to have had a material impact on the representativeness of DD1 for the overall market.

4.59 If the LLD was representative, we would expect the distribution of variables to resemble those observed in DD1. To assess this, we focus on the period from 2019 to 2021 and exclude 2017 and 2018, as these earlier years rely partly on imputed observations and follow a different sampling approach, making distributional comparisons harder to present consistently. We also did not pursue formal tests of equality of distributions between the LLD and DD1, as these were not particularly informative in this context. Such tests were designed to detect any differences in observed distributions and, given the very large number of observations, even economically small differences were likely to be identified as statistically significant. Moreover, some differences between the LLD and DD1 were to be expected at this stage, reflecting differences in lender coverage and sampling methodologies across the two datasets.

4.60 Figure 5 presents the proportion of loans categorised by commission model in the DD1 and the LLD datasets. In the first two years, DD1 suggested that the distribution of DCA and non-DCA loans was broadly even, while in the LLD there was a higher prevalence of DCA loans. In January 2021, the distribution of loans across commission models was near identical in both datasets.

Figure 5: Share of loans by agreement type



- 4.61** We next examined the distribution of APRs in DD1 and the LLD and found they were broadly similar for both DCA and non-DCA loans (Figure 6).
- 4.62** In contrast, commission amounts differed more notably with DD1 exhibiting generally higher commission levels than the LLD (Figure 7). This difference likely reflected variations in reporting practices across datasets. In DD1, reported commission amounts included volume-based bonuses earned under the agreement, whereas such bonuses may not have been included in the commission variable in the LLD. Consequently, the observed difference did not necessarily suggest that the LLD understated typical commission charges. Rather, it potentially indicated that commission measures were not fully comparable across datasets.
- 4.63** The descriptive analysis in this document indicates that the LLD and DD1 datasets were broadly consistent across the variables considered. The share of loans across product types and commission models were similar. The median APR and total cost of credit were also broadly aligned. The most substantial difference was in commission amounts, where the LLD reported lower values.

Figure 6: Distribution of APRs, comparing the LLD and DD1

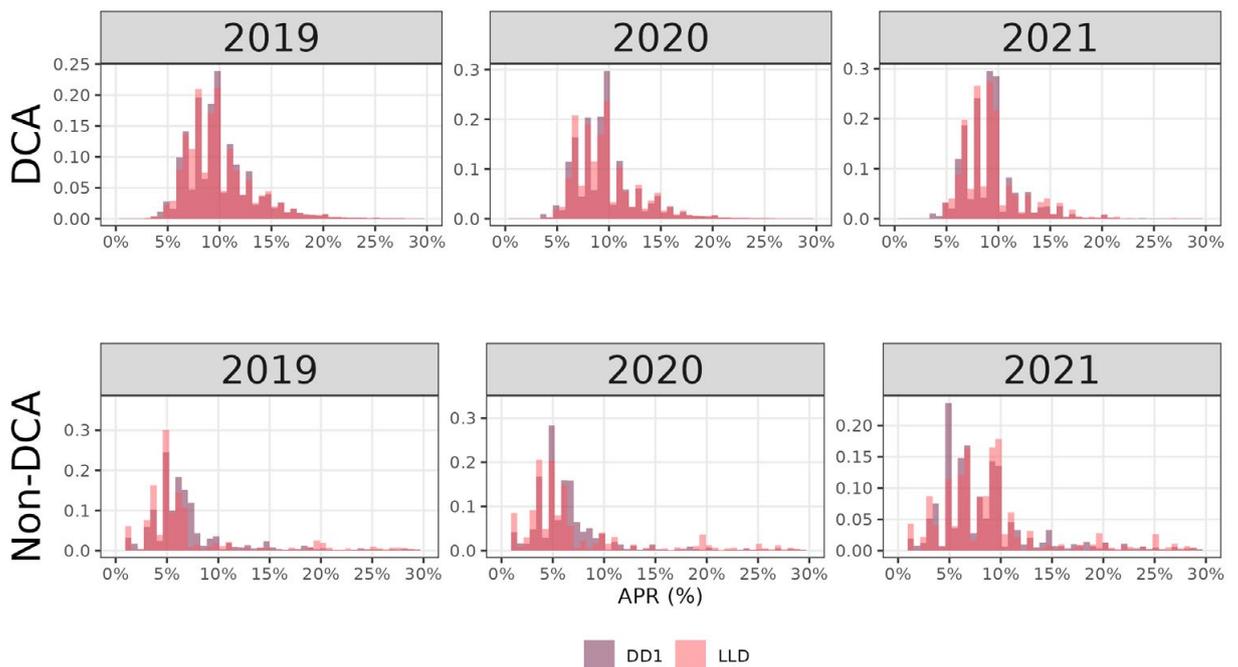
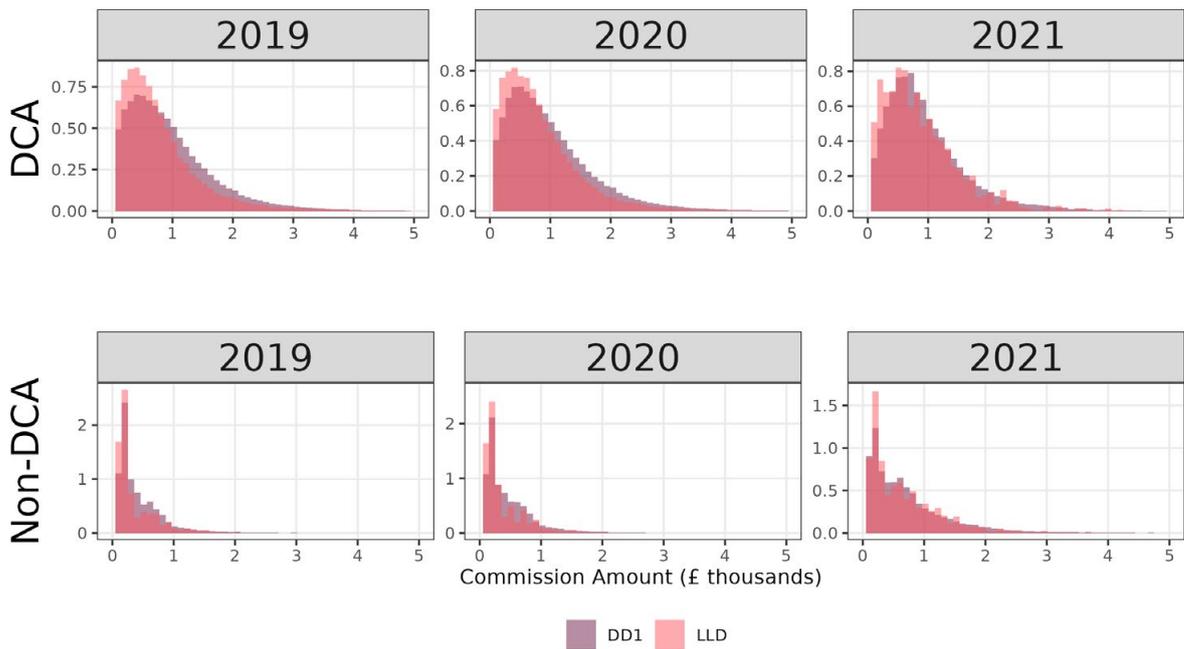


Figure 7: Distribution of commission amounts in £, comparing the LLD and DD1



4.64 Ultimately, differences between the LLD and DD1 only matter for the purpose of the analysis of loss to the extent that they resulted in different estimates of the relationship between DCA and APR or other variables common to both. To assess the representativeness of the LLD for the purpose of estimating the relationship between DCA and APR, we regressed APR on the set of variables common to both datasets, conducting the analysis separately for the LLD and DD1. By comparing the significance and relative impact of commission model across the two regressions, we were able to evaluate the consistency of relationships between datasets. The purpose of this exercise was not to estimate causal effects, rather to assess if the relationship between loans' APR and commission models was similar across datasets.

4.65 Specifically, we ran the following regression separately for each dataset:

$$APR_i = \alpha_f + \tau_t + \beta_1 DCA_i + \beta_2 LoanValue_i + \beta_3 ContractLength_i + \beta_4 ProductType_i + \varepsilon_i$$

where:

- α_f : Firm fixed effects (ie dummies for firm indicating where agreement i originated from)
- τ_t : Year fixed effects (ie dummies for the year indicating when agreement i originated from)
- APR_i : annual percentage rate for agreement i
- $LoanValue_i$: The original loan value of agreement i in £
- DCA_i : binary variable indicating whether agreement i is a discretionary commission agreement
- $ContractLength_i$: The length of agreement i in months

- *ProductType_i*: The type of motor finance agreement for agreement *i* – we standardised this variable across the two datasets, and they are classified into Hire Purchase, Lease, Loan, Personal Contract Purchase and Other

4.66 The coefficient associated with the commission model, denoted as β_1 , reflected the average difference in the APR of DCA and non-DCA loans, after controlling for all the variables included in the regression. Considering that DD1 contains all agreements from firms covering a large proportion of the market¹⁸, including the entire population of the largest 19 firms, if the LLD was representative, we would have expected the size of the coefficient to be similar across the two regressions. If the coefficient was similar across both datasets, then we could take that as supporting the hypothesis that that LLD was representative of the wider market for the purpose of estimating the impact of DCA on APR (versus non DCA). Thus, the hypothesis that the LLD was representative can be expressed as a test on the equality between the coefficients (estimated β_1) across the datasets.

4.67 The estimates of the coefficient on commission model are presented in Table 8 and Figure 8. The results indicated that the coefficient obtained from the LLD closely matched that from DD1 and improved after the addition of data from an additional lender. Importantly, the LLD estimates with the new submission are contained in the 95% confidence interval of the DD1 estimate. This provided reassurance because it showed that the estimated relationship between APR and whether a loan was under a DCA model was broadly consistent across the two datasets; given that the reweighted DD1 was representative of the market, and that this conditional correlation was central to our DCA commission impact analysis, the alignment between the estimates increased our confidence in the representativeness of the estimated relationship.

¹⁸ Because the UK motor finance market is concentrated in a small number of large lenders, the dataset offers only a limited coverage of very small lenders. Thus, a weighting adjustment is applied to ensure representativeness of the wider market.

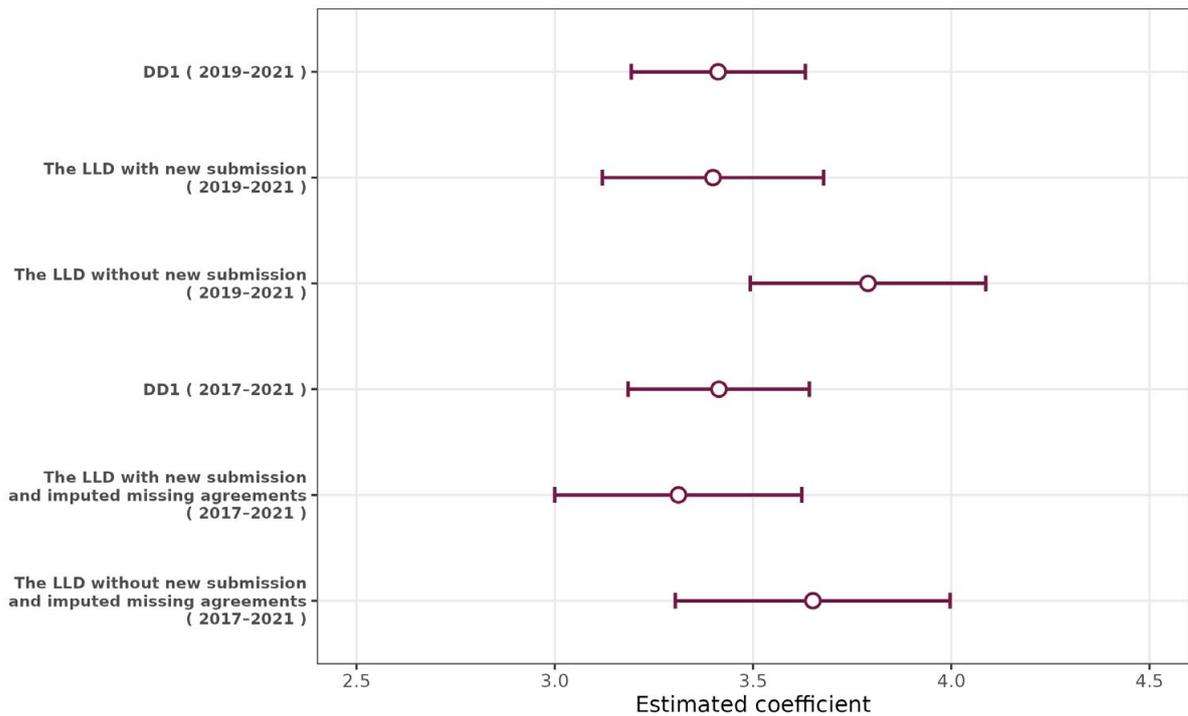
Table 8: Regression results

Period	Sample	Estimated coefficient	Standard Error	P-value	95% CI	R ²	Within R ²
2019-2021	DD1	3.412	0.112	0	[3.193; 3.632]	0.807	0.209
	The LLD with new submission	3.399	0.142	0	[3.120; 3.678]	0.804	0.212
	The LLD without new submission	3.79	0.152	0	[3.493; 4.087]	0.822	0.232
2017-2021	DD1	3.414	0.117	0	[3.185; 3.642]	0.783	0.194
	The LLD with new submission and imputed missing agreements ¹⁹	3.312	0.159	0	[3.000; 3.623]	0.794	0.205
	The LLD without new submission and imputed missing agreements	3.651	0.177	0	[3.304; 3.997]	0.821	0.223

Notes: Standard errors are clustered at broker level. Confidence intervals are computed assuming the estimator follows a normal distribution.

¹⁹ It should be noted that we hold incomplete data for the 2018 agreements. We discuss this in paragraph 4.33. To address this, we applied an imputation procedure based on resampling from 2019 data, please see detail in paragraph 4.41. For this reason, the estimated coefficient, standard error, p-value, 95% CI, R square and within R squares are the average (mean) across 30 bootstrap samples estimation.

Figure 8: Regression results



Notes: Point estimates are shown as circles and 95% confidence intervals as solid lines. Standard errors are clustered at broker level. Confidence intervals are computed assuming the estimator follows a normal distribution.

4.68 The analysis was limited to variables that were common to both datasets. This meant that some important drivers of APR, such as credit scores and vehicle quality available in the LLD, were not included in the regressions. Consequently, the analysis did not control for these factors. This means they should not be taken as our best estimates of the impact of commission model on APR. But this was not the aim of this analysis; rather it was to show that the association between APR and commission model – controlling for a smaller but similar set of covariates – was similar across the two datasets. This gave confidence that the estimated relationship would be similar had the full set of covariates been available in both datasets.

4.69 In our view, the descriptive statistics and the regression analysis suggested that the LLD was reasonably representative of the motor finance market. The descriptive statistics showed some differences in the distribution of key variables between the LLD and DD1, including differences in the typical values and dispersion of variables such as APR and commission. These differences were expected, given differences in lender coverage and sampling methodology used to collect the data. More importantly, the regression analysis showed that the relationship between APR and commission model was very similar across the two datasets. As this relationship is central to the assessment of consumer loss, the regression evidence carried significant weight in our assessment of representativeness.

DCA Commission Model Impact

Our position in CP25/27

- 4.70** In CP25/27 Technical Annex 1²⁰, we assessed how the cost of borrowing for a typical consumer varies according to the commission model used by the broker or lender, testing specifically if the use of DCA agreements led to higher borrowing costs. Our analysis drew on the LLD from 2019–2022, restricted to agreements originated between 1 January 2019 and 27 January 2021, with any observations after the DCA ban removed.
- 4.71** We carried out a matched ordinary least squares regression where we matched 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.
- 4.72** We found significant differences between the APR for flat fee loans and some types of DCA loan. Specifically, we found that the APR for reducing DiC Loans is 20-22% higher than flat fee loans, when comparing similar customers and the APR for scaled loans is 21%-24% higher than flat fee loans when comparing similar customers. Both results were statistically significant. We also found that there was no statistically significant difference in the APR for increasing DiC loans and flat fee loans, when comparing similar customers. We attributed this to the limited number of firms who used this commission model in the analysed period.
- 4.73** We set out in CP25/27 how this analysis allowed us to identify an average 17% APR difference between the APR under the agreement that gave rise to an unfair relationship and the APR the consumer would have paid under a flat-fee loan, which we used as the basis for estimating loss. The outputs of this analysis were referred to as *APR-17*.

Summary of technical consultation feedback

- 4.74** In response to our proposed approach to calculating the loss-based redress (*APR-17*), we received extensive feedback from respondents which we grouped into the following themes:
- **Disclosure vs unfairness:** Firms and trade bodies argued the *APR-17* adjustment did not serve as an effective measure of loss. It reflected the economic loss resulting from having a DCA rather than a non-DCA agreement, not the loss attributable to inadequate disclosure. Because no agreements in the dataset featured adequately disclosed DCAs, the analysis could not isolate the effect of disclosure itself. As a result, *APR-17* captured differences in product structures and commission incentives rather than the harm caused by consumers not being informed about those commission arrangements.
 - **Unsuitable counterfactual:** *APR-17* was estimated by comparing DCA vs flat fee agreements, controlling for observable characteristics. However, some respondents (including both firms and consumer groups) argued that flat fee agreements were not a suitable baseline comparator to reflect the economic loss that arose from inadequate disclosure.

20 Paragraphs 2.17 – 2.55.

- **Heterogeneity:** It was proposed in CP25/27 for APR-17 to be applied uniformly across the motor finance market for eligible agreements. However, some respondents said that the economic loss may differ across different dimensions and a uniform application risked over- or under-compensating some agreement holders. The argument against heterogeneity was made by both firms and trade bodies, as well as consumer groups.
- **Limited timeframe:** APR-17 was estimated using a limited timeframe from the LLD, and this was highlighted by most respondents who submitted technical feedback.
- **Statistical methodology flaws:** APR-17 was estimated using a matching procedure which in the view of some respondents (firms, trade bodies, and consumer groups/representatives) was not robust to modelling decisions.

4.75 As well as providing criticism of our analysis, some respondents offered suggestions to improve our assessment of loss caused by inadequate disclosure of unfair relationships. Following detailed consideration of the suggestions made in consultation feedback, we made several proportionate revisions to our approach which strengthened our analysis.

Disclosure vs unfairness

Feedback and suggested changes

- 4.76** Many respondents (largely firms and trade bodies) suggested that the analysis in CP25/27 did not provide empirical evidence to support the fundamental assumption that the inadequate disclosure of DCA agreements and/or high commission agreements would have been likely to have caused consumer harm arising from the loss of the opportunity to negotiate lower borrowing costs or to seek alternative sources of finance.
- 4.77** One suggestion, put forth by a trade body, was to empirically test the impact of disclosure on consumer harm by considering the time period between October 2024 and the publication of CP25/27 (October 2025). During this period, disclosure changed materially given the Court of Appeal decision in *Johnson*²¹.

Our response

- 4.78** As set out in CP25/27 Technical Annex 1 paragraph 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).*" These limitations regarding data still applied to the work leading up to PS26/3.
- 4.79** We received further supporting evidence through a literature review that focused on broader evidence relating to the impact of disclosure. The conclusion from the literature review aligns with the academic literature review documented in CP25/27 Technical Annex 1 (paragraphs 2.168 – 2.179). We acknowledge there is a lack of evidence showing a consistent link between disclosure and subsequent influence on consumer behaviour.

21 *Johnson v FirstRand Bank Limited, Wrench v FirstRand Bank Limited and Hopcraft v Close Brothers* [2024] EWCA Civ 1106.

4.80 In our view, our analysis of loss is a reasonable proxy for how much consumers may have lost out due to inadequate disclosure of DCA agreements. Please see PS26/3 Chapter 2 for our assessment of the section 404 statutory test.

4.81 With respect to the suggestion of using data between October 2024 and 2025, we are of the view that the consideration of this period did not provide a suitable proxy for adequate disclosure (see paragraphs 4.78 – 4.81 for an extensive discussion). As we stated in CP25/27 paragraph 3.37: *"We note that many firms have been fully disclosing commission arrangements and tied arrangements in the light of the Court of Appeal decision in Johnson and have reported very little or no impact on consumer behaviour so far. We also acknowledge that the academic literature is inconclusive on the impact of disclosure. However, we do not think that this is enough to counter the conclusions we have reached. Disclosure practices have only changed recently and the behaviour of firms in response to increased transparency is likely to be a longer-term impact as firms adjust their position over time in order to remain competitive. Furthermore, DCAs are now banned and many of the cases we are dealing with in the scheme related to non-disclosure in relation to those arrangements. We note too that the court in Johnson determined that it was "not necessary for [the claimant] to prove that he would not have proceeded with the transaction had he been made aware of the fact and amount of commission" (see paragraph 327, [2025] UKSC 33)."*

Unsuitable counterfactual

Feedback and suggested changes

4.82 In CP25/27 Technical Annex 1, our approach to estimating economic loss (APR-17) involved comparing DCA agreements with comparable flat fee agreements. However, firms and consumer groups suggested that this may produce a biased estimate for the following reasons:

- **Fundamental differences between DCA and Non-DCA agreements:** DCA agreements differed significantly from non-DCA agreements before the ban. They suggested that if DCA agreements were mainly for used vehicles and non-DCA for new vehicles, it would be difficult to find enough agreements within the same vehicle condition category to make a robust comparison—even if vehicle condition is controlled for. They noted this may lead to two problems:
 - An unbalanced final sample
 - Excessive trimming (ie, removing observations in the treated and control group that are not similar to each other in terms of the considered characteristics) to achieve balance, resulting in a non-representative sample of firms for the estimation of harm

- **Contemporaneous spillover effects:** Submissions claimed (albeit with little supporting evidence) that before the ban, APRs for non-DCA agreements were influenced by the existence of DCA agreements. It was purported that brokers tended to offer non-DCA agreements to customers with higher APR elasticity (ie more price sensitive) and DCA agreements to those with lower elasticity (ie less price sensitive). Consequently, respondents said, in their view, this could create bias:
 - APRs for DCA agreements were disproportionately high
 - APRs for non-DCA agreements were disproportionately low compared to a counterfactual world without DCA

4.83 Several options were proposed with respect to alternative control groups. The main groupings were:

- **Statistical comparators:** Alternative statistical models to estimate what would have happened absent DCA agreements while controlling for other factors (proposed by firms and consumer groups / representatives).
- **Benchmarking:** An approach that sought to compare observed outcomes to a “fair” or unaffected reference group (two options were suggested to us, one by a lender and one by a trade body).

Our response

Statistical comparators

4.84 For those respondents who suggested using alternative statistical comparators, they made three suggestions on changing our control group for the econometrics analysis:

- Using post-ban non-DCA agreements
- Using agreements after the 2024 CoA ruling in *Johnson* to approximate better disclosure
- Other non-DCA agreements beyond the flat fee commission model

4.85 We respond below to each of these in turn.

Post-DCA ban non-DCA agreements as a comparator

4.86 Some respondents who accessed the consultation-period data room replicated the matched regression models from CP25/27 Technical Annex 1, using post-ban non-DCA agreements as the comparison group. This led to lower estimated losses than presented in CP25/27. The rationale for the post-ban statistical control group was that it addressed potential contemporaneous spillover effects on non-DCA APRs when DCA agreements were present in the market.

4.87 However, we noted that the proposed comparison group effectively treated “DCA and no ban” as the treatment group and “no DCA and ban” as the control group. This design was problematic because it conflated two distinct effects:

- Effect of the ban
- Effect of DCA arrangements

- 4.88** By comparing agreements that differed on both dimensions simultaneously (presence of DCA and regulatory environment), we could not have isolated the impact of DCA agreements from the general impact that the ban had on the market. Any observed difference in APR could have been driven by changes in market behaviour following the ban rather than by the presence or absence of DCA agreements. This made the control group unsuitable for estimating loss attributable specifically to DCA agreements.
- 4.89** Even without the DCA ban, using a control group from a different period would not have captured all the variables that change from year to year – such as market conditions, the pandemic, cost of living crisis – making it even more challenging to isolate the impact of DCA. In econometric terms, this violated the principle of holding all other factors constant except the one being tested. As a result, the comparison would not have provided a clean counterfactual and would have introduced bias into the harm estimates.
- 4.90** For these reasons, our view was that the risk of obtaining biased estimated of loss would be higher if we had used post-ban agreements as the control group than if we continued to use our current control group. Further work, such as additional data collection, would have involved significant cost and would not have altered our assessment. It would therefore have been disproportionate, and as such we did not adopt this alternative control group in our analysis.

Using agreements after the 2024 Court of Appeal ruling in Johnson as a comparator

- 4.91** One respondent (a trade body) submitted analysis that posited that the CoA ruling in *Johnson* created an exogenous variation to commission disclosure practices, arguing that they (disclosures) became more transparent after the ruling. The analysis accordingly compared changes in outcomes of interest before the ruling in *Johnson* with changes after the ruling (October 2024) in two datasets. Their estimates showed that key outcomes such as APRs, loan amounts, commissions, and number of loans remained largely unchanged after the shift in disclosure practices. In the respondent's interpretation this could have implied that disclosing commission practices did not trigger significant changes in consumer behaviour and market outcomes. This analysis explicitly estimates harm from inadequate disclosure, rather than using a proxy for that harm.
- 4.92** The sample used in the respondent's analysis consisted of loan agreements made after the DCA ban in 2021, so all agreements included were non-DCA. Since most disclosure practice breaches were associated with DCA agreements, this could explain why the analysis did not find any impact of disclosure on the outcomes considered in the analysis.

- 4.93** Further, the underlying data lacked detailed information on disclosure. The analysis assumed that disclosure practices significantly improved following the CoA ruling in *Johnson* and while there is some evidence that this was the case²², we could not exclude that some heterogeneity in disclosure practices still existed in the market. Without exact information, the treatment group (before the CoA ruling in *Johnson*) may have combined agreements with better disclosure of commissions with those that had (potentially) worse disclosure²³. This diluted the actual effect of disclosure, driving the estimated effect downwards.
- 4.94** The analysis also assumed that changes in earlier years were a valid control group for changes in the year of the ruling in *Johnson*, ie, 'normal changes' that would have happened in the absence of the ruling. No formal validation test was presented for this analysis. In contrast, we noted there were major changes in the market after the CoA judgment. For example, some firms temporarily paused their motor finance business²⁴. Hence, agreements made after the CoA ruling in *Johnson* may have reflected these wider market conditions. Without stronger validation of the control group, the results remained suggestive and did not represent strong evidence of causal impacts.
- 4.95** Our conclusion was that this proposed approach would not have given robust evidence of any causal relationship associated with disclosure practices. Again, to mitigate these risks would have involved significant effort and potentially further data collection, which was not proportionate given the existing model specification. Therefore, we did not adopt this as an alternative control group in our main analysis.

Other non-DCA agreements beyond flat fee commission model as a comparator

- 4.96** We received feedback from both firms and consumer groups/representatives that flat fees represented only one type of non-DCA agreement. Other commission models – such as risk-based pricing – were excluded from our comparison group, even though some firms indicated these were the predominant non-DCA models during the relevant sampling period.
- 4.97** We decided to exclude "other" commission models in our CP25/27 analysis because they were unspecified and we could not classify them as either DCA or non-DCA agreements.
- 4.98** In the period following the publishing of CP25/27 and leading up to PS26/3, we gathered information from firms that submitted data agreements to understand further what the "other" category consisted of. In response to the information gathered from firms, we further classified all agreements initially classified as "other" into the categories of "other (non-DCA)" and "other (unknown)."

22 See <https://www.fca.org.uk/news/statements/fca-consult-compensation-scheme-motor-finance-customers> and Updated Diagnostic Report paragraph 6.39.

23 See Table 18 from Updated Diagnostic Report: <https://www.fca.org.uk/publication/corporate/diagnostic-report-motor-finance.pdf>

24 See <https://www.bbc.co.uk/news/articles/cpv19vwylyno>, <https://www.ft.com/content/444c1dac-4e7e-4480-b920-f5689548c0c7> for examples.

4.99 Building on the further classification, we amended the DCA Commission Model Impact Analysis presented in PS26/3 to include agreements previously classified as “other” in the CP25/27 analysis. The updated control group for the analysis published in PS26/3 contains agreements classified as “other (non-DCA)” for issuing firms who confirmed that they did not issue any DCA agreements during the sampling period (2018-2021). For firms that issued DCA agreements *aside from* increasing DiC/reducing DiC/Scaled commission models, we further classified agreements initially reported as “other” as “other (unknown).”

Benchmarking approach

4.100 We outline here the main technical reasons for rejecting a benchmarking approach. For a full description and our rationale, including implementation and operational complexity concerns, please see PS26/3 Chapter 11.

4.101 Two respondents – one lender, and one trade body – suggested using a benchmarking approach (each providing different options). We acknowledge that benchmarking approaches can, in certain circumstances, be more adaptable compared to statistical methods as they enable counterfactuals to vary at the level of individual agreements. They can also address the issue of disclosure, using 2025 loans would be a ‘clean’ proxy counterfactual, not affected by poor disclosure nor by the presence of DCA agreements in the market.

4.102 However, whether a benchmark effectively targets the actual harm suffered is contingent on a suitable benchmark being chosen. Both benchmarking options proposed by respondents suggested using post-ban agreements (segmented for targeting). As discussed earlier, we believe this had significant limitations and was not a robust counterfactual. Even taking steps to de-trend, the data using the rate on £10k personal loans reported by the Bank of England, other factors – such as shifts in market dynamics or lender behaviour – may still have confounded the results. There was also a risk that these agreements were influenced by broader market changes following the ban, which could have undermined their validity as a benchmark for counterfactual outcomes. Consequently, relying on post-ban agreements could have introduced bias, making it difficult to draw robust conclusions about the actual harm suffered.

4.103 Moreover, when contrasted with the statistical comparator approach that our analysis of loss adopted, the benchmarking method offered less flexibility in controlling for the various factors that influence APR. To achieve comparable control, it would have been necessary to construct separate, segment-specific benchmarks, which would in turn have increased the complexity of the analysis exponentially.

4.104 Lastly, the benchmarking approaches did not permit statistical inference and rested on the assumption that differences between the observed and counterfactual distributions were statistically meaningful. In contrast, our current statistical comparator approach enabled us to conduct proper statistical inference and test whether the differences between DCA and the comparator arose from statistical noise or represented a genuine effect.

- 4.105** We received a supplementary report on 20 March 2026 that aimed to address weaknesses in the respondent's original benchmarking proposal. In the report, the respondent undertook (1) matching between pre and post DCA ban agreements to control for the compositional differences after the DCA ban and (2) extended the set of control variables to account for other factors that affect APR.
- 4.106** These changes, in our view, improve the analytical robustness of the respondent's benchmarking proposal. However we remain of the view that certain limitations persist that render it unsuitable for our scheme. Despite matching pre and post DCA ban agreements on product dealer type, loan amount, loan term and vehicle age, macroeconomic conditions during the period may have influenced the composition and pricing of agreements in ways that matching cannot adequately address (see paragraph 4.86). For instance, changes in interest rates, shifts in inflation, or fluctuations in household income and employment levels during the cost-of-living crisis could have materially affected both the composition of borrowers entering the market and the pricing strategies adopted by lenders.

Heterogeneity in the DCA Commission Model Impact Analysis

Feedback and suggested changes

- 4.107** In CP25/27 we estimated separate models for each DCA commission model and selected the lowest statistically significant adjustment estimate and applied it across the entire market for the whole period.
- 4.108** Feedback was received that we lacked evidence to demonstrate that economic loss was uniform across the motor finance market and should not have imposed a single estimate across the market. Respondents argued that collapsing all dimensions into a single pooled APR adjustment poorly targeted the loss experienced by individual consumers. Dimensions inadequately covered in a uniform adjustment included:
- Time period of agreements
 - Different types of DCA
 - Other unfair relationships (non-DCA high commission and tied arrangements)
 - Type of lender (eg captives)
 - Risk segments (eg prime vs sub-prime)
 - Firm/lender
 - Different APR segments

Our response

- 4.109** In our revised analysis we aggregated all DCA arrangements (increasing DiC, reducing DiC and scaled) into a single combined DCA category. This approach is more consistent with our treatment of other consumer and loan characteristics (eg vehicle condition, credit segment).

- 4.110** For completeness, further down we present results separately for the different DCA types. We also conducted analysis to assess whether losses varied over time, which we also share below.
- 4.111** We also note that for both reducing DiC and scaled categories, the estimated loss values were comparable. Therefore, aggregating the results or using a commission model specific estimate did not make a material difference to the estimated loss. The increasing DiC category remains included in the sample, although its estimated harm was considered unreliable due to the limited prevalence of such agreements during the analysis period.
- 4.112** Please refer to PS26/3 Chapter 11 for a discussion on the rationale of applying a uniform APR-adjustment to agreements under the hybrid remedy.

Limited timeframe

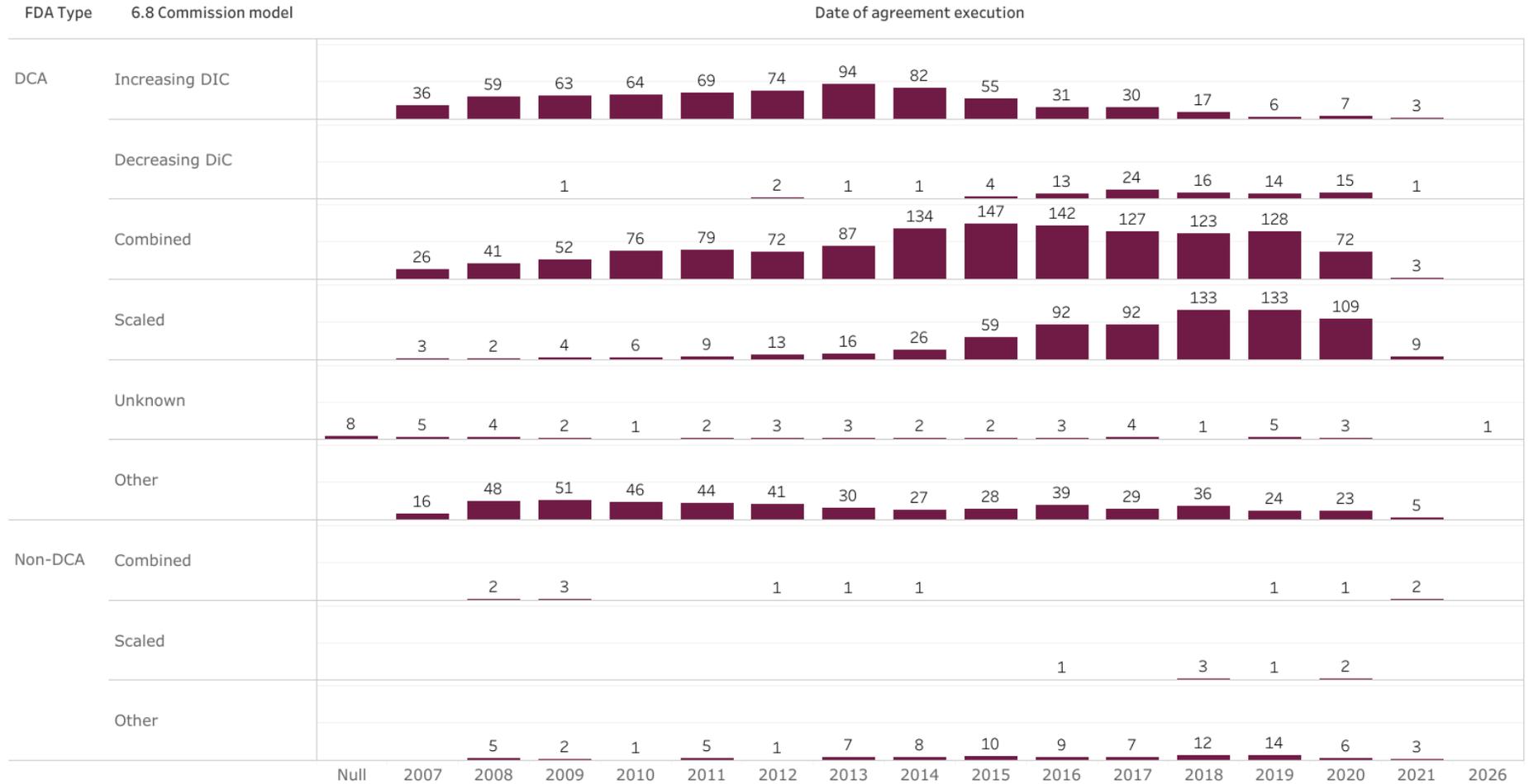
Feedback and suggested changes

- 4.113** Respondents raised concerns that the analysis underpinning the APR-17 centred on a limited data range which overlapped the pandemic resulting in an APR-adjustment that was not reflective of the market prior to 2019.
- 4.114** We received empirical evidence from some respondents that suggested the APR difference between DCA and non-DCA agreements, after controlling for various factors, could range from 20% to more than 46% if a longer dataset back to 2007 was incorporated.

Our response

- 4.115** We considered different options to improve the temporal representativeness of the APR-adjustment model with different data sources.
- 4.116** Since our analysis made a relative comparison between DCA loans and flat fee loans over this period, this criticism's validity hinged on whether the harm varied with macroeconomic conditions (for example, if the economic loss from DCA was different during periods such as the pandemic compared to non-pandemic years). Therefore, factors such as the pandemic, for which there was no further empirical evidence indicating differential effects on DCA and non-DCA, were expected to have limited influence on the APR adjustment resulting from the model.
- 4.117** Nevertheless, the evidence suggested that the product mix varied over time, which may have impacted the estimated economic loss from DCA agreements. In particular, increasing DiC agreements were more prevalent in earlier years (see Figure 9). For this reason, given the scheme dates to 2007, it was proportionate and reasonable to explore using data up to that year so the APR adjustment accounts for harm over the entire period if we assume consumer loss varies time and extrapolation from later years is not valid.

Figure 9: 'Type of commission model used over time (no. of agreements)



Source: s166²⁵ review data

4.118 For this reason, we extended the LLD timeframe for the analysis, as set out in paragraphs 4.33 – 4.42.

Statistical methodology flaws

Feedback and suggested changes

4.119 In CP25/27, we applied a two-stage estimation approach to test whether DCA agreements led to higher borrowing costs. In the first stage, we performed matching and derived the associated weights. In the second stage, we ran a weighted regression on the matched sample using those weights.

4.120 We received feedback from several respondents, including concerns about the modelling assumptions inherent to this approach and questions around the interpretation of the estimate, particularly due to trimming (ie, removing observations in the treated and control group that are not similar to each other in terms of the considered characteristics). Because the feedback covers several different aspects of the model, we set out each point of feedback and our corresponding response separately in the sections below, as we explain the final specification of the model.

Our response

4.121 Similar to the model in CP25/27 Technical Annex, we used a two-stage estimation process for the analysis presented in PS26/3. While the analysis in CP25/27 used data for 2019-2021, we extended the dataset to cover the period from 2017 to 2021. In the first stage of the analysis presented in PS26/3, we conducted matching and retrieved the weights. In the second stage, we ran a weighted regression on the matched sample using the weights from matching.

Matching procedure

4.122 In the first stage of the updated analysis, we ran a matching procedure. The treatment group was composed of DCA agreements, while the control group was composed of non-DCA agreements. Treated units were matched using the following covariates: deposit amount, loan amount, loan term, risk score, vehicle condition (ie new/used vehicles), product type (eg PCP, HP), origination channel and year-month. In each specification the matching procedure discarded observations to achieve a good balance between the treatment group and the control group.

4.123 Consultation responses suggested that the final dataset used to calculate the APR-17 harm estimate excluded many DCA agreements captured by the scheme, meaning the estimate was not representative of the population of DCA agreements. This was suggested for two main reasons: first, significant sample filtering was undertaken (eg restricting date window, removing missing and negative APRs, excluding 'other' and 'none' agreement types) and then the matching and regression methods themselves made additional implicit or explicit exclusions.

- 4.124** We discuss sample exclusions in paragraph 4.135, and the updates to the analysis to address consultation responses that sought to broaden the sample in significant ways and impacted described statistics, in paragraph 3.17. Regarding the data restrictions imposed by matching, we observed that all matching methods required excluding some observations to ensure comparisons were made between sufficiently similar units. This was essential for obtaining unbiased estimates, as it avoided comparing agreements that differed in ways unrelated to the commission model. In our analysis, using CEM matching, approximately 14% of non-DCA agreements and 9% of DCA agreements were excluded because they were too dissimilar to the rest of the sample to allow credible like-for-like comparisons. When using propensity score matching (PSM), we excluded approximately 67% of non-DCA agreements and 0% of DCA agreements.
- 4.125** A key consideration in choosing between matching methods was which achieved the greatest similarity (or “balance”) between DCA and non-DCA agreements in the resulting matched sample. We assessed balance using the Standardised Mean Difference (SMD) across key characteristics²⁶, where lower values indicated greater similarity between the two groups. Figure 12 compares SMDs in the samples matched using CEM and PSM. The results showed that DCA and non-DCA agreements were more similar in the CEM-matched sample than in the PSM-matched sample. This provided a clear justification for using the CEM-matched sample as the basis for our main analysis.
- 4.126** A potential drawback of the exception of observations implied by our preferred method was that the resulting matched sample may no longer have been representative of the whole population. To assess this risk, we compared key characteristics of agreements in the raw (unmatched) data with those in the samples obtained using CEM and PSM. If matching preserved representativeness, the distribution of characteristics of matched agreements should have broadly resembled those observed in the unmatched data, which we had already shown to be representative of the population. Figures 10 and 11 present the results for discrete and continuous variables, respectively. It’s clear the CEM-matched sample resembled more closely the unmatched data than the sample matched using PSM nearest neighbour matching. This alleviated the concerns around potential unrepresentativeness of the estimation sample. We could not rule out that the excluded DCA agreements differed in unobserved ways from the included DCA agreements and how this may have affected the estimate of harm. It was not possible to quantify the extent of this potential bias, but we decided to accept this trade-off to achieve better balance.
- 4.127** We also considered entropy balancing as a method to make DCA and non-DCA agreements comparable. We found that the results using this method were unstable and gave a wide range of estimates depending on the exact specification of the control group. For this reason, we did not opt for entropy balancing for our main specification.
- 4.128** Taking all the above considerations into account, we relied on CEM as the matching procedure for our main specification, as it achieved better balance between the sample of DCA and non-DCA agreements while preserving sample representativeness. Below, we present results using PSM and entropy balancing as robustness checks.

26 Standardized Mean Difference (SMD) captures the difference in means between the matched sample and the unmatched sample for each variable after normalizing by a standardization factor. The graph illustrates the SMD associated with the CEM matching procedure and the nearest matching procedure for each variable.

Figure 10: Matching diagnostics – Comparing variables between matched sample and unmatched sample (discrete variables)

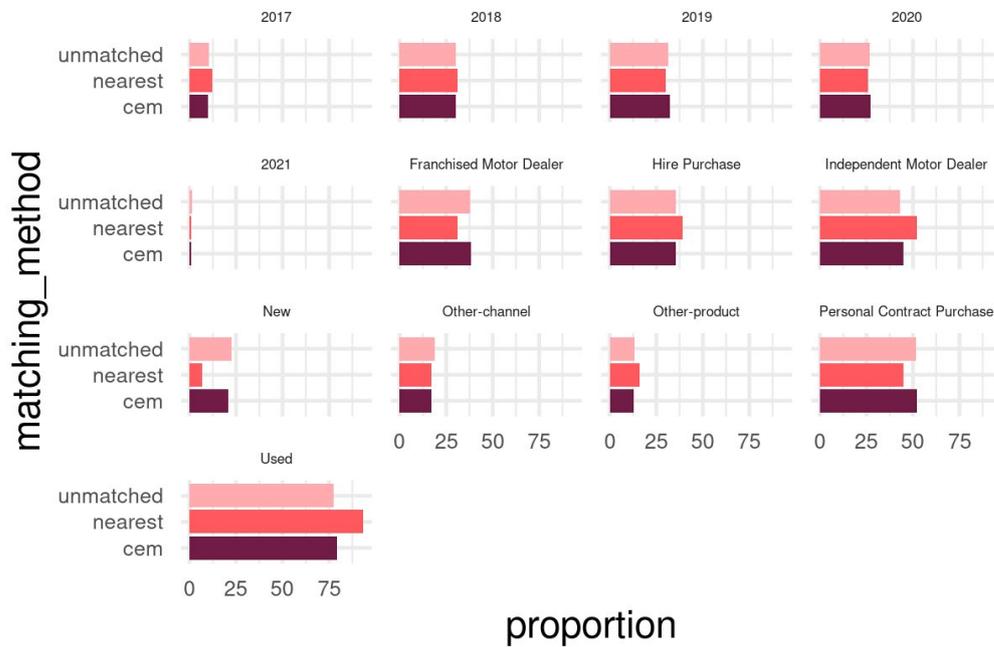


Figure 11: Matching diagnostics – Comparing variables between matched sample and unmatched sample (continuous variables)

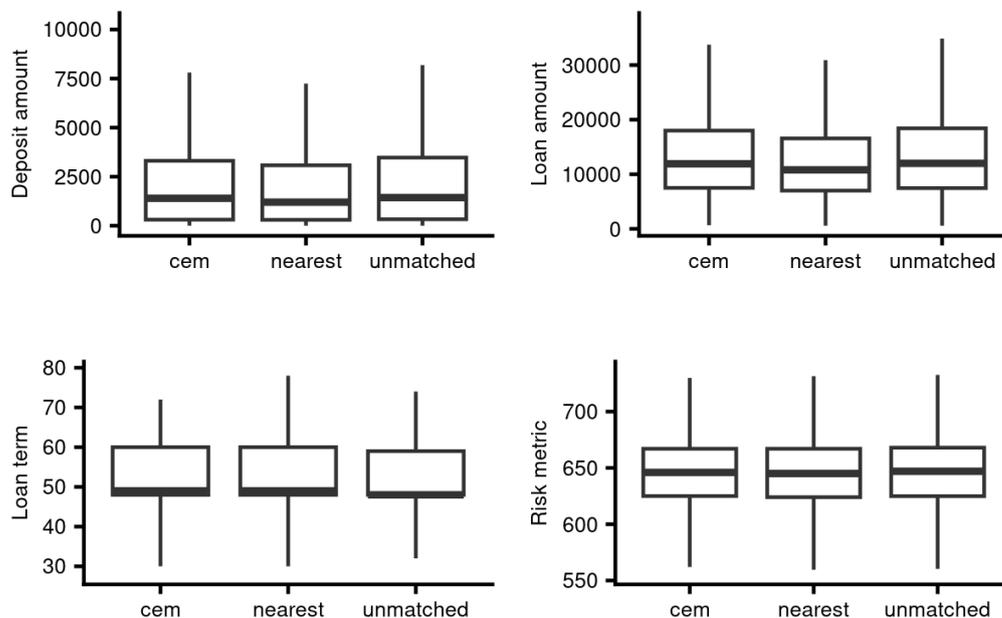
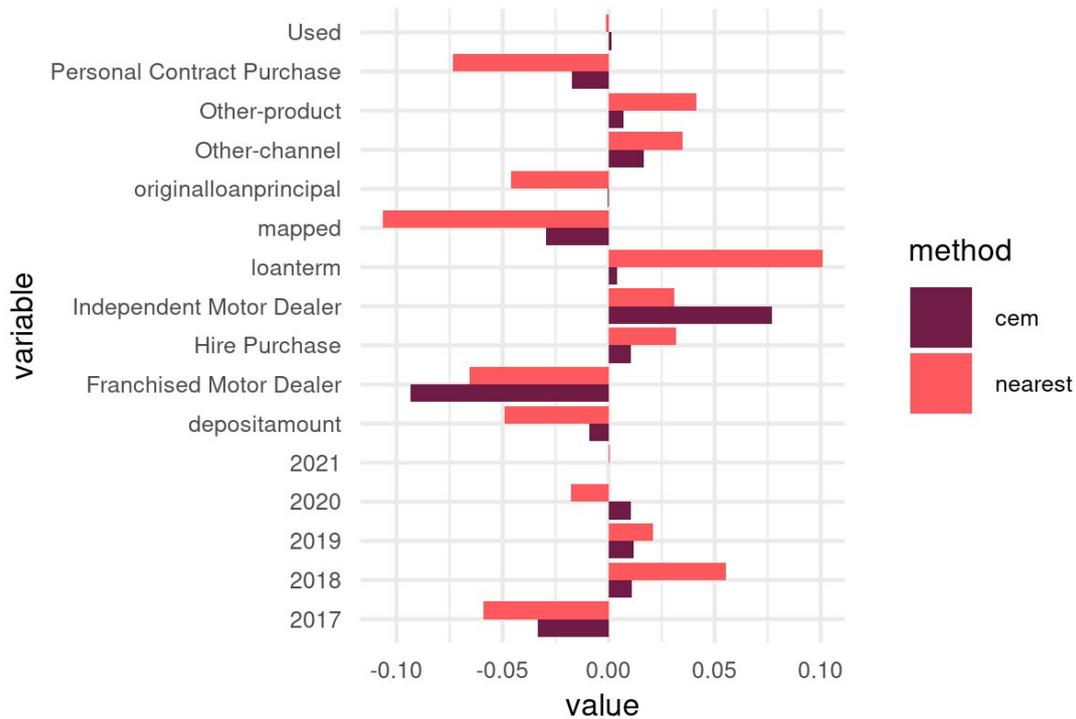


Figure 12: Balance tests results – matching variables SMDs are lower when using CEM than PSM



4.129 Both matching approaches produced weights that were used in a second stage fixed-effect regression (described below). This process was repeated 30 times, as there were 30 imputed datasets for 2018 (see paragraph 4.43).

Fixed-effects regression

4.130 We retained the natural log of APR as our outcome measure. Our rationale was that the analysis was intended to inform policy and feeds directly into the scheme. If the outcome was a proportional APR-based adjustment scheme, then log APR was the appropriate dependent variable.

4.131 Further, APR best captured the cost of credit for the consumer, which was our outcome of interest (as the best proxy of harm). Solely using the interest rate did not do this as it did not include fees and other items. We, therefore, did not think we should have been considering interest rate as our primary outcome in the analysis of harm. Further, APR is a measure of cost of credit that is readily available and comparable across firms.

4.132 We acknowledge that by taking the natural log of the outcome variable, we dropped agreements with a zero or negative APR (see Table 6). One respondent who accessed the data room conducted an analysis showing that zero-APR loans were more prevalent among non-DCA. Consequently, in their view, dropping zero-APR loans could lead to estimating lower losses. However, since zero-APR agreements are now excepted from the definition of a relevant arrangement, excluding zero-APR agreements when applying a log transformation was reasonable, as these agreements will be excepted from the definition of a relevant arrangement in any case.

4.133 Key independent variable: Indicator for type of commission model (DCA = 1; non-DCA = 0).

- DCA: Increasing DiC, Reducing DiC, Scaled
- Non-DCA: Flat-fee, Other, Fixed percentage advance commission

4.134 We included the following control variables in the regression: deposit amount, loan amount, loan term and risk score.

4.135 The following variables were not used in CP25/27 Technical Annex 1 and we kept the same approach for the following reasons:

- **Customer's income:** We omitted this variable due to its data quality. Specifically, 14.5% of the values were missing, and a further 12.7% were recorded as zeros, which raised concerns about reliability and interpretability within a regression analysis. As a result, including this variable would likely have compromised the robustness of our findings.
- **Customer's employment status:** We did not include this variable due to concerns regarding its data quality. Specifically, more than 30% of the values were missing, which undermined the reliability and interpretability of this variable in our analysis. Including it could therefore have compromised the robustness of our regression results.
- **Vehicle discount:** We omitted this variable because it is what in econometrics is called a "bad control"²⁷, meaning that it was likely to be affected by the commission model of the agreement. The inclusion of a bad control would have biased the estimated effect of interest. That said, we also did not think vehicle discount was relevant to the scheme. A vehicle discount is not intended to offset the increase in APR from a DCA, and rather was intended to make the deal appear more attractive. Whether a discount decreased perceived harm or increased a consumer's willingness to take credit are behavioural factors that we couldn't objectively measure. Furthermore, the Supreme Court did not take account of the wider fairness of the purchase in *Johnson v FirstRand Bank Limited (2025)*, so we do not consider it is a necessary component to consider in determining unfairness and factoring into the redress payment.

4.136 We included the following fixed effects in the regression: customer postcode area²⁸, customer year of birth, vehicle manufacturer²⁹, month and year of loan origination date, vehicle condition (new or used), motor finance product³⁰, origination channel³¹ and lender-year subgroups.

27 Angrist, J. D., & Pischke, J.-S. (2009). *Mostly harmless econometrics: An empiricist's companion*. Princeton University Press.

28 To reduce the dimensionality, and avoid dropping singleton, we group rare postcodes (defined by 90th percentile of frequency), and label as to "other".

29 To reduce the dimensionality, and avoid dropping singleton, we group rare vehicle manufacturers (defined by 90th percentile of frequency), and label as to "other".

30 Agreements are sorted into three categories for motor finance product: personal contract purchase, hire purchase and other.

31 Agreements are classified into one of three categories for origination channel: franchised motor dealer, independent motor dealer and other. One firm do not have this variable readily available. To ensure we could run the regression and matching exercises, we imputed the missing origination channel category values for this firm. Our approach assigns to each agreement the broker's most frequently observed origination channel, based on the distribution of that broker's activity from other lenders' submission. Using this method, we were able to impute an origination channel category for around 75% of the missing values.

- 4.137** On lender-year fixed effect, we received feedback that the effective number of firms in the sample becomes lower after incorporating this fixed effect. By incorporating lender-year fixed effect, our regressions only used information from firms that offered both DCA and non-DCA loans. However, we acknowledge that there may be important lender-specific and year-specific effects that affect the cost of credit, so inclusion of these controls is important. The comparison within lender-year of differences in APR was closest to what we would have ideally estimated were an experiment possible, and so was the most suitable analysis possible.
- 4.138** We retrieved the weights from the matching procedure. These matching weights were then multiplied by the sampling weights that we described earlier.
- 4.139** We clustered our standard errors at the credit broker level, which accounted for potential correlation in errors³² within dealers/brokers.
- 4.140** We conducted the following robustness checks. Each check started from the main specification and altered one component at a time. We ran:
- **Nearest neighbour:** Two regressions with nearest neighbour matching procedure instead of CEM (both with and without a caliper³³).
 - **Entropy balancing:** One regression where weights were calculated using entropy balancing rather than a matching procedure.
 - **Outliers:** Three regressions removing different groups of outliers: (i) removing the top percentile of loan amount and deposit amount; (ii) removing the top 0.1% of loan amount and deposit amount; (iii) removing any observation with loan amount or deposit amount greater than £100k.
 - **Shortened date range:** One regression with a restricted date-range from 2019-2021.
 - **DCA commission models separate:** Three regressions with the treatment group limited to one of the three DCA commission models.
 - **Flat fee model only:** One regression with the control group limited to flat fee agreements only.
- 4.141** Most of the modelling choices described in this section were the same as in CP25/27. The main differences across our main specification were that we: (i) constructed the treatment group by pooling together all DCA agreements, rather than considering them separately (ii) we relied on the extended LLD dataset (2017-2021), constructed as explained earlier, and (iii) we included a greater number of robustness checks to explore the sensitivity of the results to our modelling choices. Below, we summarise our main specification and rationale.

32 Errors measure how much the model's "errors", the differences between what the model predicts and what actually happens tend to vary. In a conventional regression, we assume these errors are uncorrelated across observations, meaning each data point behaves independently. But in many real datasets, observations within a group (like loans from the same broker or time period) share unobserved factors that make their errors move together. If we ignore this and use conventional standard errors, we are violating the test's core assumption, which makes the statistical test invalid because it will underestimate uncertainty and overstate significance. Clustering fixes this by recognising that some observations are related, adjusting the standard errors upward so the test reflects the true amount of independent information in the data.

33 A caliper restricts matching by imposing a maximum allowable distance between treated and control observations (typically measured using the propensity score). Treated units are only matched to control units within this tolerance.

Table 9: Summary of modelling decision and rationale

Decision	Rationale
<p>Used post-cleaning, updated LLD for 2018-2021 data For 2017 data, utilised the MFRD</p>	<p>The APR-adjustment modelling approach required (1) a large volume of agreements and (2) detailed agreement-level features. The additional lender with significant market share in the LLD improved the representativeness of the sample, offered a more comprehensive view of the sector, and improved the diversity of lending practices captured in the analysis. Extending the dataset to 2017 enabled a more comprehensive view of market conditions prior to 2019, helping to mitigate the impact of pandemic-related anomalies and improving the representativeness of the analysis. Details in paragraph 4.29, onwards.</p>
<p>For MFRD, applied weighting</p>	<p>The MFR data followed a different sampling rule compared to the LLD, and weights were constructed accordingly. Firms were asked to submit the lesser of: 10% of agreements entered in 2017, or 1000. Based on this rule, weights were calculated as follows: Firms with fewer than 1,000 agreements: Weight = 10. Firms with exactly 1,000 agreements: Weight = $N/1,000$, where N is the total population of agreements. Not applying weights would have resulted in overweighting small lenders in the sample. Details in paragraph 4.39, onwards.</p>
<p>For 2018 LLD missing firms, imputed data using 2019 data using random resampling</p>	<p>To ensure that the data was representative of the firms in the sample, we utilised each firm's 2019 data as a suitable proxy for 2018. The rationale for not simply weighting up the firms' 2019 figures was that a regression needed to be performed during the APR-adjustment step, where the specific year was an important variable within the model. Details in paragraph 4.41, onwards.</p>
<p>Used non-DCA agreements as the comparator</p>	<p>We expanded our original flat fee comparison group. It now contains agreements classified as "other (non-DCA)" for issuing firms who confirmed that they did not issue any DCA agreements during the sampling period (2018-2021). Details in paragraph 4.96, onwards.</p>

Decision	Rationale
<p>Used log(APR) to measure outcome</p>	<p>APR best captures the cost of credit for the consumer, which was our outcome of interest (as the best proxy of harm). Interest rate could not do this as it did not include other elements of the total cost of a loan. We therefore did not think we should have been considering interest rate as our primary outcome in the analysis of loss. Furthermore, APR measure of cost of credit that is readily available and comparable across firms.</p> <p>Zero-APR agreements were implicitly excluded in the analysis. However, since zero-APR agreements are now excepted from the definition of a relevant arrangement, excluding zero-APR agreements when applying a log transformation is reasonable, as these agreements will be excepted from the definition of a relevant arrangement in any case.</p> <p>Details in paragraphs 4.130, onwards.</p>
<p>Used CEM method</p>	<p>We preferred the method that achieved the best similarity ("balance") between DCA and non-DCA agreements.</p> <p>This is because, given</p> <ol style="list-style-type: none"> 1) all the important confounders were controlled for and 2) there was good balance between DCA and the comparator, the estimates were less biased. <p>Details in paragraph 4.124, onwards.</p>
<p>Used the following variables for matching:</p> <ul style="list-style-type: none"> • Loan principal amount • Credit score • Year of loan origination • Loan term • Deposit amount • Vehicle condition (new, used) • Motor finance product type (Personal Contract Purchase, Hire Purchase, Balloon, Loan, Lease, Other) 	<p>We included this set of covariates to control for observable factors that jointly influenced both whether the agreement was a DCA and APR. Variables captured key dimensions of consumer characteristics, product features, and market conditions that could otherwise have confound the estimated treatment effect. Conditioning on them helped ensure that comparisons were made between observationally similar units, strengthening the credibility of the causal interpretation.</p> <p>We excluded customer's postcode, vehicle manufacturer and customer's year of birth from the matching variables because their high dimensionality would result in very small cells, making it infeasible to have achieved adequate balance and to have retained sufficient sample size. Including postcode in matching would have substantially reduced common support without materially improving control for confounding. However, we control for these factors in the regression.</p> <p>Details in paragraph 4.122.</p>

Decision	Rationale
<p>Used the following variables for the regression:</p> <ul style="list-style-type: none"> • Binary indicator of DCA • Deposit amount • Loan principal • Credit score • loan term • Fixed effects including customer postcode area, customer year of birth, vehicle manufacturer (cleaned, please refer to data cleaning), month and year of the loan origination date, vehicle condition, motor finance product, origination channel and lender-year subgroup 	<p>We used a binary indicator as the policy decision was to apply a uniform APR adjustment. This approach was more consistent with our treatment of other consumer and loan characteristics (eg vehicle condition, credit segment).</p> <p>Details in paragraph 4.133.</p> <p>We included this set of covariates to control for observable factors that jointly influenced both whether the agreement was a DCA, and the agreement APR. Variables captured key dimensions of consumer characteristics, product features, and market conditions that could otherwise confound the estimated treatment effect. Conditioning on them helped ensure that comparisons were made between observationally similar units.</p> <p>Details in paragraph 4.134 onwards.</p>

Main results

4.142 The main coefficient of interest for the analysis was the coefficient on the key independent variable. This represented the approximate change in APR associated with a change in commission model (from non-DCA to DCA). As in the CP, we took this coefficient and applied the following transformations:

- 1.** For each specification, we calculated the average raw coefficient (in mean) from the 30 bootstrap samples, denoted as *average raw coefficient*.
- 2.** We calculated $\% \text{ change} = [\exp(\text{average raw coefficient}) - 1] \times 100$. This captured the precise change in APR associated with a change in commission model.
- 3.** We calculated $\text{APR Adjustment} = [100 / (100 + \% \text{ change})]$. This captured the APR of a non-DCA agreement relative to the APR of a DCA agreement.
- 4.** We calculated $\text{APR-[X]} = [100 - \text{APR Adjustment}]$. This captured the amount by which the APR of a non-DCA agreement was lower compared to a DCA agreement.

4.143 The following table presents both the raw coefficient and the transformed APR-[X] for each specification:

Table 10: Results by different model specifications

Model specification	Raw coefficient	APR-[x] [95% confidence interval]
Main specification (cem, 2017-2021, combining all DCA, including new submission of additional lender with significant market share)	0.18	16.8 [13.5, 19.8]
Outlier specification (without top 1%)	0.18	16.7 [13.1, 20.2]
Outlier specification (without top 0.1%)	0.18	16.6 [13.2, 19.8]
Outlier specification (without loans or deposit amounts over 100k)	0.18	16.6 [13.2, 19.8]
Nearest neighbour specification	0.17	15.7 [12.4, 18.8]
Nearest neighbour specification (with caliper=0.2)	0.17	15.7 [12.4, 18.8]
Entropy balancing matching	0.15	14.1 [13.8, 14.4]
Flat fee only control group	0.20	18.4 [15.3, 21.3]
Increasing DiC only treatment group	0.01 (not statistically significant across all bootstrap samples)	1.3 [-6.1, 8.2]
Reducing DiC only treatment group	0.20	18.5 [13.7, 23.0]
Scaled only treatment group	0.20	18.6 [15.1, 22.0]
2019 – 2021 agreements (including new submission of additional lender with significant market share)	0.14	12.8 [9.5, 16.1]
2019 – 2021 agreements (without new submission)	0.17	15.5 [11.8, 19.1]
2017 – 2021 agreements (without new submission)	0.19	17.0 [13.6, 20.3]

- 4.144** The main specification led to an estimated APR adjustment of 17% after rounding to the nearest integer. This result did not materially change when outliers were removed using any of the methods in Table 7. The nearest neighbour specification was associated with a slightly lower, but still comparable estimate (APR-16).
- 4.145** When we broke down the results by commission models, as done in CP25/27, we found that Reducing DiC and Scaled commission models were associated with higher levels of economic loss, while Increasing DiC displayed a small and not statistically significant level of economic loss. These results mirrored the ones in the CP. In our view, the statistically not significant result for Increasing DiC was due to the lower number of agreements with this commissioning model in the sample³⁴. Finally, entropy balancing was associated with a lower estimate (APR-14).
- 4.146** We also found differences in the average economic loss estimated in different time periods. When we restricted the estimation sample to 2019-2021, we found that economic loss from DCA agreements was lower than in 2017-2021 (APR-13 versus APR-17).
- 4.147** The table below shows the APR adjustment calculated year by year. For each year we (i) considered only observations from that year, (ii) performed matching and (iii) ran a weighted regression as done under the main specification. The table shows that the estimate of harm is higher in earlier years and lower in later years when compared to the overall estimate calculated under the main specification.
- 4.148** In our view, it would not have been appropriate in principle to extrapolate these four observations into other time periods in a linear trend.

Table 11: APR adjustment by year

Year	APR adjustment [95% confidence interval]
Main specification (2017-2021)	16.8 [13.5, 19.8]
2017	24.2 [18.5, 29.5]
2018	19.3 [14.9, 23.4]
2019	14.8 [11.3, 18.1]
2020	11.1 [7.8, 14.4]

34 See CP25/27 Technical Annex 1 paragraph 2.48.

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

Our position in CP25/27

- 4.149** In CP25/27 Technical Annex 1, we presented an *analysis of the relationship between commission and the cost of credit for flat fee loans*. The results of this analysis were indicative and not necessarily causal but were used to help inform the approach to high commission.
- 4.150** To implement this analysis, we aggregated the 2019-2021 LLD to the level of lender x broker pairs and then ran weighted regressions of consumer total cost of credit (TCC) on commission levels (both measured in pounds), controlling for other important drivers of TCC including loan amount and duration, credit score band, vehicle condition (new versus used) etc and lender fixed effects in some specifications. We also ran a set of subgroup analyses to assess whether the relationship varied by commission level.
- 4.151** Our analysis attempted to estimate the extent to which higher commission payments were associated with a higher total cost of credit to consumers. We ran a regression of total cost of credit in pounds on commission in pounds and interpreted the coefficient on commission as the relevant estimate of the average associated relationship. We found no statistically significant average effect across the sample.³⁵
- 4.152** We also ran subgroup analyses by selecting data where the commission payment relative to the size of the loan, total cost of credit or similar exceeded some threshold. We interpreted increases in the coefficient on commission as evidence of a stronger relationship between broker's commission and customer's cost of credit. We 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 found that as we moved from the subset containing the top 50 percentiles towards higher commission subgroups, the relationship between commission and the cost of credit got stronger, and exceeded 1 around the 75th percentile, and the average commission to cost of credit proportion and commission to loan proportion for agreements in this subgroup were 33.5%/10% respectively. In other words, we observed an average association between total cost of credit and commission greater than 1 around the 75th percentile of loans (ordered as above) through the 99th percentile.
- 4.153** CP25/27 Technical Annex 1 noted that the results of the model (described above in paragraphs 4.151 – 4.152) were indicative, and that a sensitivity test simulation "suggest[ed] the estimator should not be treated as a precise threshold" (CP25/27 Technical Annex 1 paragraph 2.131). In chapter 4 of CP25/27, these model results were referred to as the 'point at which our analysis best indicates that borrowing costs may have been more strongly affected, and in some cases disproportionately elevated, by the commission, such that its size would likely have been a major consideration in the consumer's mind had they been aware of it when they took out the loan.'

³⁵ The headline result of a £1 increase in broker's commission being associated with, on average a £0.61 in customer's total cost of credit, was not statistically significant at any level as published in the CP25/27 Technical Annex 1.

Summary of technical consultation feedback

4.154 This regression analysis presented in CP25/27 Technical Annex 1, as well as its application to the proposed policy contained in CP25/27, were the subject of significant feedback. Regarding the econometric analysis, the following points were made:

- **Failure to identify causal relationship:** Some firms, trade bodies, and consumer groups made the case that the regression was essentially a cross-sectional correlation between total cost of credit and total commission. Responses emphasised that the analysis as set out in CP25/27 Technical Annex 1 failed to establish the direction of causality (ie higher interest driving the commission), that there could be biases due to the role that demand and supply play in determining the association of commission with total cost of credit, that there may be biases from other omitted variables (eg discounts, higher lender cost), and that the model made no use of variation over time.
- **Invalid statistical inference:** Many responses from firms emphasised that the subgroup analysis conducted in CP25/27 Technical Annex 1 (paragraph 2.128) could be interpreted as a repeated testing exercise and/or data mining that resulted in an overstating of the significance of results. Another key concern raised was that the statistical tests only tested whether the coefficient was greater than zero but should have tested whether they were greater than one. Additionally, one response raised concerns that there were too few clusters (firms) to perform valid statistical inference.
- **Subgroup analysis unconvincing:** Responses from firms also raised concerns that the estimates generated by the model were overly sensitive to alternative specifications (eg decile analysis) and the trimming of outliers. Additionally, our analysis was challenged as the model set out to frame lender x broker pairings as the appropriate level of observation, however the subgroup analysis was thought of by some to have circumvented this grouping. Lastly on the subgroup analysis, some firms commented that the subgroup analysis was effectively conditioning on the error term and was therefore invalid.
- **Sample period of analysis:** Some respondents argued that the sample period used for the analysis was too narrow relative to the full redress period. They suggested this limited coverage risked failing to capture the full range of market conditions, business models and pricing practices relevant to earlier years.

Our response

4.155 Our response to the technical stakeholder feedback summarised above is set out below.

- **Failure to identify causal relationship:** We agree that this regression did not attempt to estimate a causal relationship, (as set out in CP25/27 Technical Annex 1, paragraph 2.132).
- **Invalid statistical inference:** To address feedback regarding the potential for multiple testing, as well as other comments regarding statistical inference, we made a series of methodological updates to the analysis.

- **Subgroup analysis unconvincing:** As discussed in detail below in the section on methodological updates to the model, further tests and changes to the specification demonstrated that the model was indeed sensitive to alternative specifications (ie resulted in different estimates). Additionally, we implemented methods (eg allowing the relationship between the two variables to vary with commission level) that in our opinion addressed the potential risk of conditioning on the errors term.

4.156 However, while these modifications did improve the robustness of the analysis, we determined that the model presented in CP25/27 and then updated could not be relied on to evidence a precise high commission threshold. The updated results no longer indicated a strengthening association between consumer cost and commission, making it impossible to use this to robustly set a threshold³⁶. We have, therefore, not addressed the remaining comments, as further work would not affect this position or be proportionate.

4.157 The remaining parts of this section are, therefore, for completeness and transparency to cover areas explored to improve robustness prior to reaching this conclusion.

Methodological updates to the model

4.158 In considering feedback, we explored several changes to the regressions to strengthen the analysis. The three main changes were:

1. Ensuring consistent treatment of errors in the data across all strands of the analytical work – this included the addition of data as discussed in the data guide.
2. Running the regression with the key cost of credit and commission variables expressed as proportions of the loan principal, to address the concern that results might have been affected by the size of the loan and to reduce sensitivity to outlying values.
3. Experimenting with alternative ways of assessing whether the association was stronger at higher commission levels that were not subject to the critique of the percentile analysis that it risked generating misleading results by conditioning on the error term. The approaches we tried were:
 - a. Running a percentile analysis similar to that presented in CP25/27 Technical Annex 1 but where percentiles were defined based on commission as a proportion of loan principal (ie the main explanatory variable).
 - b. Including a polynomial in commission.
 - c. Estimating a piecewise polynomial regression.

4.159 These changes affected the results previously published in CP25/27 in two important ways:

- **Finding 1:** Removal of errors and lease agreements in the LLD (see paragraph 4.23 onwards for further details) moved the overall sample estimate slightly, showing a statistically significant (at the 0.1% level) effect. We found a clear and statistically significant relationship between commission and the cost of credit: consumer's total cost of credit increased by £0.79 for every £1 increase in broker's commission on average (rather than a non-statistically significant £0.60 for every £1 increase, as published in CP25/27 Technical Annex 1).

³⁶ While we did not find a strengthening association (ie the association becomes stronger as commission increases), we still found a positive association between consumer cost and commission, as noted in paragraph 4.163.

- **Finding 2:** Once econometric issues were addressed with the subgroup analysis, there was little clear evidence that the association between broker commission and total cost of credit increased with commission level, with both expressed as a proportion of loan principal.

4.160 Our changes were not comprehensive in that they did not address all the concerns raised by respondents. However, based on the findings, there was little clear evidence that the association between broker commission and total cost of credit increased with commission level and we could not rely on this analysis to determine the high commission arrangement threshold proposed in Chapter 4 of CP25/27.

Finding 1

- 4.161** As discussed above in paragraph 4.23 onwards, we received many consultation responses that discussed our treatment of the LLD, which is the dataset underlying the model for the *analysis of the relationship between commission and the cost of credit for flat fee loans*.
- 4.162** In CP25/27 we published that on average, across the whole sample, a £1 increase in broker’s commission was associated with a £0.60 increase in the customer’s total cost of credit – a result that was not statistically significant (see Table 12 below).
- 4.163** After re-running the original model as published in CP25/27 Technical Annex 1 with the updated 2019–2021 LLD (with one additional firm and erroneous information and lease agreements removed, as described above), we found that on average, a £1 increase in broker’s commission was associated with a £0.79 increase in the customer’s total cost of credit – a result that was statistically significant at the 0.1% level (see Table 12 below).

Table 12: Regression with CP25/27 specification, with data cleaning

	CP25/27 specification	Updated specification
Broker commission in £	0.62	0.79 ***
Standard error of estimate	(0.35)	(0.09)
Num. obs.	4,766	3,824
Num. groups: lenderfrn	13	11
R ² (full model)	0.90	0.90
R ² (proj model)	0.88	0.88
Adj. R ² (full model)	0.90	0.90
Adj. R ² (proj model)	0.88	0.88

Signif. codes: 0 ‘***’ 0.001 ‘**’ 0.01 ‘*’ 0.05 ‘.’ 0.1 ‘ ’ 1

- 4.164** Noting the strengthened evidence of a positive association between commission and cost of credit above, we maintain the view stated in CP25/27 Technical Annex 1 (paragraph 2.133): *“we would caution against inferring that this means commission is ‘harmful’ at these levels. Brokers may reduce distribution costs for firms which could benefit consumers too if firms pass these savings on to them. If the savings from reduced distribution costs are greater than the increased total cost of credit, then [...] consumers may be benefiting from the commission arrangement that creates the more efficient distribution.”*

Finding 2

- 4.165** After expressing broker's commission and customer's cost of credit in % terms of loan principal (using relative variables as opposed to £ amounts, the latter used in CP25/27 Technical Annex 1), we implemented three approaches to address other econometric issues raised by consultation responses in studying how the estimated association varied by commission level: a percentile approach (where percentiles were defined using commission level, so as to not unintentionally condition on the error term), including a polynomial in commission in the regression, and estimating a piecewise polynomial regression.
- 4.166** In the table below, which presents the findings of the updated model with key variables in relative terms (using the dataset with errors and lease agreements removed), we attempted to address the concern raised in consultation responses that interpreted the subgroup analysis as conditioning on the error term. Here we ran a similar percentile analysis, progressively excluding higher percentiles of the commission rate distribution (calculated on the aggregated data) and studied how the estimated association of commission and cost of credit varied. Defining percentiles based on commission rate as a proportion of the loan principal (ie the key explanatory variable included in the regression variable) addressed the concern that prior analysis effectively conditioned on the error term.
- 4.167** The results presented in the table below showed little clear evidence that the association of commission with total cost of credit (expressed in terms relative to loan principal) varied with commission level – for most commission rate percentiles, the cost of credit relative to loan principal appeared to increase by a range of 0.5-0.6 percentage points for every 1 percentage point increase in broker's commission as a proportion of loan principal.

Table 13: Association between total cost of credit (expressed in terms relative to loan principal) and commission level, by different commission rate percentiles

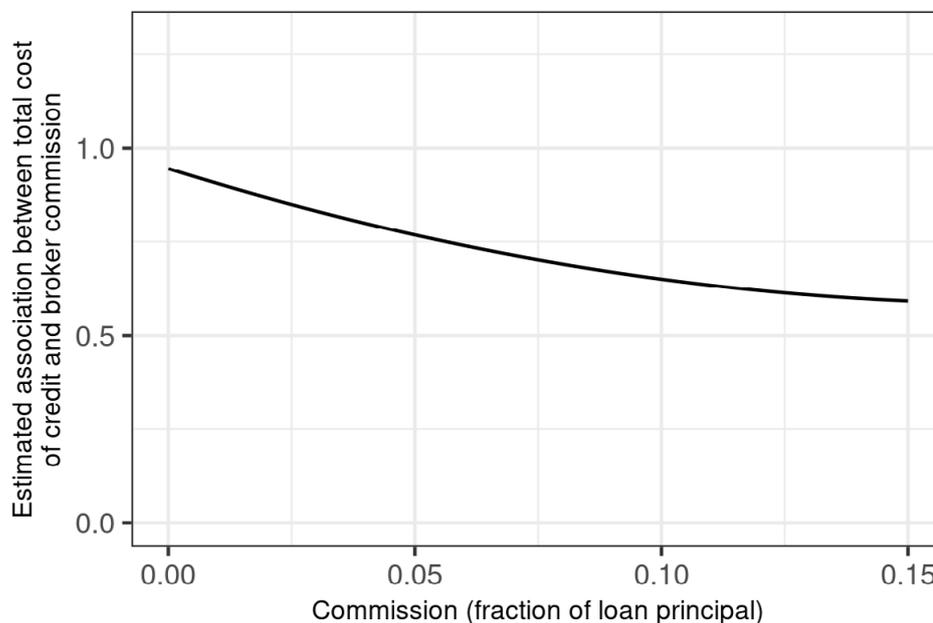
Percentile	Estimate	Std. Error	Significance
0.30	0.62	0.18	**
0.35	0.62	0.18	**
0.40	0.62	0.18	**
0.45	0.60	0.17	**
0.50	0.60	0.17	**
0.55	0.59	0.16	**
0.60	0.58	0.16	**
0.65	0.57	0.16	**
0.70	0.56	0.16	**
0.75	0.50	0.17	*
0.80	0.51	0.18	*
0.85	0.49	0.25	.
0.90	0.59	0.30	.

Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1

4.168 As an alternative approach, we ran a regression including a polynomial in commission (relative to the loan principal). This allowed the association between cost of credit and commission level to vary with the commission level.

4.169 The results from this approach are presented in the figure below. This figure shows how the estimated association between total consumer cost and commission varied with commission level. The relationship is slightly downwards sloping, suggesting – if anything – there might be a slightly decreasing relationship (though caution should be taken given the absence of statistical significance).

Figure 13: Results from regression including a polynomial in commission (relative to the loan principal)



4.170 We also attempted to estimate a piecewise polynomial (linear) regression, allowing the association to vary across discrete bands. The results of doing this were noisier than those presented above but again did not indicate a clear upward trend in the association.

4.171 After implementing these changes, there was little clear evidence from the model that the association of commission with cost of credit for flat fee loans increases with commission level, leading to our conclusion that the model could not be used as a basis to indicate the high commission arrangement threshold proposed in Chapter 4 of CP25/27.

4.172 Following from the earlier discussion, given we no longer observed an increasing relationship as indicated in the figure above, the model could no longer be used for this purpose.

4.173 However, it is important to recognise that this does not imply the absence of harm (since all that this requires is a positive association between commission and cost of credit, not a strengthening one).

Difference-in-differences Analysis of the Impact of the 2021 ban of Motor Finance DCA

Our position in CP25/27

- 4.174** In the original analysis (CP25/27 Technical Annex 1, pages 55 to 62), we sought to 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.
- 4.175** To do that, we conducted difference-in-differences (DiD) analysis between DCA motor finance loans and personal loans. That is, we compared the changes between personal loans' APR (comparison group) against the changes between motor finance agreements' APR of motor finance firms that employed DCAs.
- 4.176** As published in CP25/27 Technical Annex 1, we found evidence that the average APR for motor finance agreements reduced after the ban on the 28th of January 2021. We estimated that the DCA ban reduced the average APR for motor finance agreements by firms who employed DCA agreements before the ban, by 1.76 percentage points (statistically significant at the 5% level). This reduction was equivalent to 20% of the average motor finance APR before the ban.

Summary of technical consultation feedback

- 4.177** The technical feedback received on this piece of analysis can be categorised into four main themes:
- **Model specification:** Some firms and trade bodies noted that the DCA Commission Model Impact Analysis above used a log APR variable, however this analysis used an APR variable, with the lack of consistency across various models causing an inflation of the results of the latter (ie the average reduction of 1.76 APR percentage points). Further, as noted in paragraphs 2.56 and 2.57 of CP25/27 Technical Annex 1, it is unclear whether this analysis adequately satisfies the parallel trends assumption that is critical for the validity of this analysis, which was echoed across consultation responses. One respondent also noted that the timing of the FCA's DCA ban coincided with realised pandemic-related market impacts, and so the model would likely not have been able to isolate the effects of its identified intervention.
 - **Personal loans as a comparator:** We received feedback from firms and trade bodies that the selection of comparison groups was not a robust one, since motor finance and personal loans are two distinct products (different loan terms, provision costs, advance limitation etc.) that would therefore react differently to the same market shifts. Additionally, we received feedback on the timespan of our data (discussed in detail above in the LLD update section), as it may have been beneficial for the parallel trends check to be able to observe the trends of the two comparison groups prior to 2019.
 - **Econometric considerations:** Responses from firms and trade bodies suggested different clustering methods, as well as controlling for seasonal changes, which would have lowered the estimates published in CP25/27 Technical Annex 1.

- **Overhang effect:** It was suggested that the DiD results are consistent with an 'overhang' effect, whereby effects persist after the ban due to a smaller treatment effect estimated in the immediate period following the ban. This would lead to our results having underestimated the effect.³⁷

Our response

4.178 The difference-in-differences analysis of the impact of the 2021 ban of motor finance DCAs, as detailed in CP25/27 Technical Annex 1, was indicative and has not directly informed the policy set out in the PS. As such, an updated model will not be presented to accompany the PS.

- **Model specification:** We acknowledge the validity of the points surrounding the lack of certainty on meeting the parallel trends assumption (as acknowledged in CP25/27 Technical Annex 1, paragraph 2.83), and on the inability of the model to isolate the intervention from the potential of coinciding market shocks, including the pandemic (as acknowledged in CP25/27 Technical Annex 1, paragraph 2.84). On the other hand, DiD by design removes any effects of the pandemic that were common to the personal loans market and the motor finance market. Running the model in logs changes the underlying parallel trends assumption, so does not necessarily improve the reliability of the results.
- **Personal loans as a comparator:** We originally selected personal loans as a comparison group because they share similar borrower characteristics and market trends, but were not subject to the DCA ban, allowing us to better attempt to isolate the policy's causal effect. However, as discussed above, due to the lack of evidence supporting the parallel trends assumption (and therefore potential unsuitability of the comparison group) we decided to place less weight on this analysis. Additionally, due to limitations regarding data availability, we could not observe trends in the datasets prior to 2019 at the time CP25/27 was published.
- **Econometric considerations:** The analysis was reviewed by academic advisors, and their feedback did not suggest issues with our clustering methods. Further, at their suggestion we had already conducted a robustness check to account for seasonality, which reduced the estimated average treatment effect from -1.76 percentage points to -0.64 percentage points (both results statistically significant at the 5% level), as noted in CP25/27 Technical Annex 1 paragraphs 2.81 – 2.82.
- **Overhang effect:** We do not rule out the possibility of a potential overhang effect. However, due to limitations regarding data availability, as discussed previously, we could not test the persistence or deepening of effects across a significant period following the intervention.

37 See also paragraphs 4.91-4.95 for a discussion on the use of post-ban data.

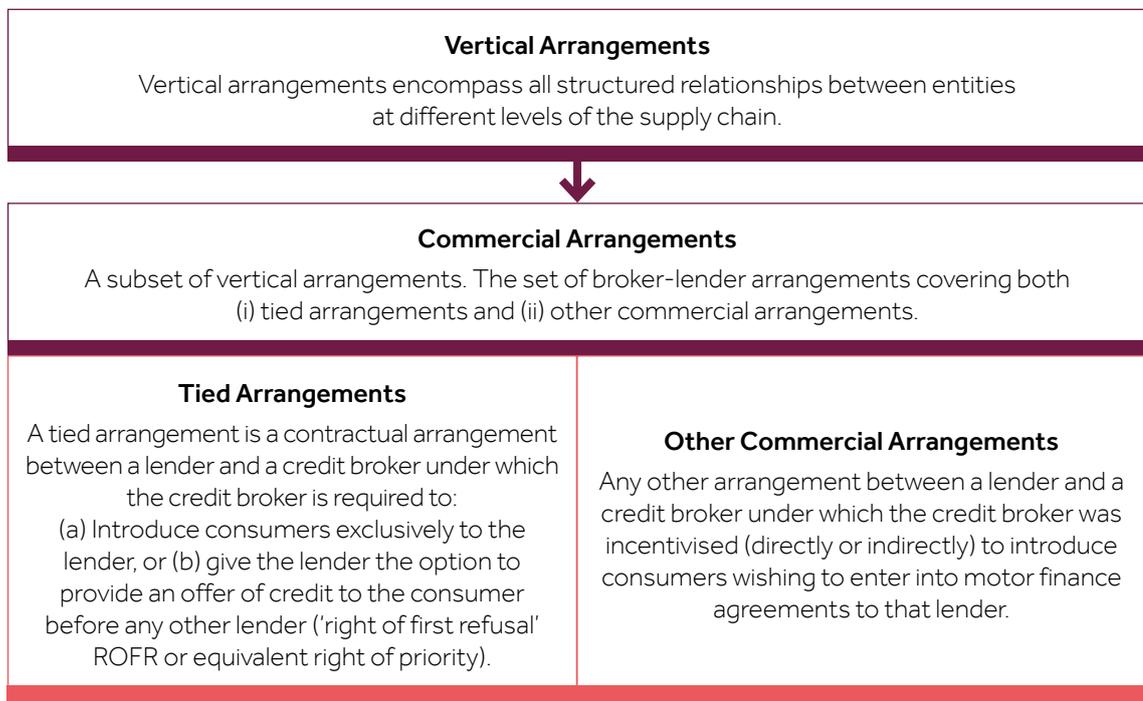
Chapter 5

Commercial arrangements

Summary

- 5.1** In CP25/27 we set out our initial view on the potential loss arising from 'tied arrangements'. We defined tied arrangements as contractual requirements which oblige the broker either to introduce customers exclusively to a single lender or to provide that lender with a priority opportunity to make an offer before approaching others. It is recognised that there are a range of commercial arrangements between brokers and firms, but only those contractual arrangements under which the broker is required to prioritise or exclusively use a particular lender were treated as tied arrangements for the purposes of the scheme.
- 5.2** After publishing CP25/27 we have broadened our analysis to consider the impact of a wider set of commercial arrangements, summarised below.

Figure 14: Commercial arrangement definitions



- 5.3** We received feedback on our approach to commercial arrangements. We grouped feedback relevant to the technical element of commercial arrangements across the following themes:

- **The scope of the redress scheme is too narrow:** Consumer groups felt the scope of the scheme in relation to tied arrangements was too narrow and should include other commercial arrangements.

- **Lack of economic theory and/or empirical evidence:** Lenders argued that we had not demonstrated economic loss from tied arrangements, and so they should not be included within the scheme. Some cited economic theory showing how tied arrangements can be good for consumers in competitive markets.
- **Alternative empirical evidence supplied to challenge the policy position:** Lenders submitted analysis on tied arrangements demonstrating they do not lead to higher prices for consumers.

5.4 In response to this feedback, we undertook two further pieces of analysis to inform our final policy position on commercial arrangements. These were:

- DD1 analysis of other commercial arrangements
- A literature review on tied and other commercial arrangements (the entire 'Literature review: vertical arrangements in credit markets' is available from paragraph 5.28 onwards).

5.5 Our analysis using the DD1 dataset provided descriptive associations only and did not support a causal assessment of whether entering other commercial arrangements affected pricing or generated consumer loss. The analysis was constrained by the absence of sufficiently granular and consistently defined measures of commercial arrangement status and by wider data gaps that limited credible identification of such arrangements.

5.6 Our literature review found that vertical relationships, including tied and other commercial arrangements, could be neutral or even beneficial in competitive markets. The academic evidence suggested they may lower transaction costs, address free-riding and reduce information asymmetries. However, the literature rarely distinguished between tied and other commercial arrangements or between disclosed and undisclosed arrangements. This limited how far the findings could be applied when these differences were important to the assessment of loss in unfair consumer credit relationships.

5.7 In principle the non-disclosure of tied arrangements can give rise to information asymmetries between firms and consumers, undermining the efficiency rationale that the vertical restraints literature typically invokes. Yet the specific question of whether non-disclosure of tied and other commercial arrangements causes consumer harm was not directly addressed in the literature. Overall, the literature did not provide a definitive conclusion on whether consumer loss can arise from non-disclosure of tied and other commercial arrangements.

Consultation feedback

5.8 This section summarises feedback on the issue of commercial agreements relating to the analysis in CP25/27 and empirical evidence submitted in response to CP25/27, as well as our subsequent economic analysis that, along with legal and regulatory analysis, informed our final policy position in Chapter 2 and 5 of PS26/3.

5.9 In CP25/27 we asked the following questions:

Question 16: Do you agree with our proposed definition of a tied arrangement? If not, please explain why you do not agree and any other options we should consider.

Question 17: Do you agree with our assessment that, because incentive-based arrangements are not binding on brokers individual credit introduction decisions and operate at the level of brokers wider commercial relationships, failure to adequately disclose an incentive-based agreement would not result in an unfair relationship? If not, please explain why you disagree.

Question 18: Are there any other types of arrangement that you consider should be included in our proposed definition of a tied arrangement? If so, please explain why.

5.10 We grouped feedback relevant to the technical elements of commercial arrangements across the following themes:

- **The scope of the redress scheme is too narrow:** Consumer groups felt the scope of the scheme in relation to tied arrangements was too narrow and should include other commercial arrangements.
- **Lack of economic theory and/or empirical evidence:** Lenders argued that we had not demonstrated economic loss from tied arrangements, and so they should not be included within the scheme. Some cited economic theory showing how tied arrangements can be good for consumers in competitive markets.
- **Alternative empirical evidence supplied to challenge the policy position:** Lenders submitted analysis on tied arrangements demonstrating they do not lead to higher prices for consumers.

Below we summarise feedback received across these themes.

Scope of the scheme is too narrow

5.11 A number of consumer bodies said the approach to include only tied arrangements limits the schemes' effectiveness and undermines consumer protection. They said that was because the definition of tied arrangements was too narrow, pointing out that other incentive-based arrangements such as volume bonuses and stocking facilities lead to material conflicts of interest that could influence broker behaviour as strongly as formal contractual obligations.

5.12 Some consumer bodies also argued that the exclusion of incentive-based arrangements was inconsistent with our own disclosure rules and our guidance which requires the disclosure of such arrangements.

Lack of economic theory and/or empirical evidence

- 5.13** Firms rejected the view that inadequate disclosure of a tied arrangement could, on its own, give rise to an unfair relationship. One of the key critiques featured in the feedback was that there was a lack of economic theory and/or empirical evidence to justify the decision to include tied arrangements within the scope of the scheme.
- 5.14** One firm supported this view with a literature review suggesting that tied arrangements can enhance efficiency by encouraging investment, reducing free-riding, and strengthening broker–lender relationships. They argued that such arrangements are typically pro-competitive unless accompanied by restrictive practices or significant market power. The respondent maintained that there is no strong evidence that undisclosed commercial ties caused consumer harm. They noted that ROFR clauses were generally used by firms to secure higher quality business and that the existing literature does not point to harmful effects.

Alternative empirical evidence submitted

- 5.15** We received a number of alternative empirical analyses from firms which were predominantly used to show that there is no harm to the consumer under a commercial arrangement.
- 5.16** One lender carried out a regression analysis to assess the relationship between tied arrangements and the cost of credit on car loans using a working sample of 3 million of its UK car finance agreements. They estimated ordinary least squares fixed effects models and found no statistically significant APR difference between tied and non-tied loans. They defined the tied indicator as all non-DCA agreements before 2016 and “non-tied” as those from 2016 onwards.
- 5.17** A submission on behalf of a group of firms challenged the proposed presumption that undisclosed exclusivity of an ROFR was inherently unfair. They utilised a sample of 120,000 loans from 2013 to March 2019 linked to 229 operating agreements to compare APR distribution for loans with and without ROFR using density and cumulative distribution plots. They found that APR distributions looked similar between ROFR and non-ROFR loans, concluding that the data did not show higher APRs for loans with an ROFR in place.
- 5.18** Another firm provided empirical analysis based on descriptive statistics, presenting time series graphs across three risk bands: sub-prime, near-prime, and prime. Their analysis showed no meaningful difference in mean APRs between dealers who received wholesale funding from the lender and the counterfactual group of national dealers across the risk groups.
- 5.19** While we considered these submissions, we noted important limitations in the methodologies employed. The analyses were primarily descriptive rather than causal and faced challenges including varying proxy definitions of tied status, potential misclassification of arrangements, and differing time-period assumptions that may not have aligned with when ties were either relied on or complied with. More fundamentally, these analyses did not address the core issue of disclosure. The evidence on potential consumer loss or absence of loss from non-disclosure of ties remains limited. We therefore could not draw definitive conclusions about the fairness of non-disclosed tied arrangements based on their submitted APR comparisons alone.

Summary of new analysis undertaken and findings

5.20 Since publishing CP25/27 in October 2025 and further to feedback received, we did two pieces of analysis to inform our final policy position on commercial arrangements. These were:

- DD1³⁸ analysis of other commercial arrangements³⁹
- A literature review on tied and other commercial arrangements

DD1 analysis of other commercial arrangements

5.21 With respect to the analysis of DD1 to explore other commercial arrangements, we used a difference-in-differences approach on a matched sample drawn from DD1, linking agreement-level data to rates & terms (R&T) documents. Rates & terms documents are separate contractual records that set out the pricing and commercial terms agreed between firms and brokers and are the primary source from which we derive indicators of the status of other commercial arrangements. Other commercial arrangements in rates & terms documents include volume-based terms packages, finance-penetration-linked rate adjustments, stocking-facility rate reductions, advance-commission loans, and other lender support mechanisms that operated at the dealer or group level rather than on an agreement-specific commission basis.

5.22 The outcome of interest was agreement-level APR. The treatment was defined as a lender-broker pairing transitioning from no other commercial arrangements to other commercial arrangements, with the first year in which other commercial arrangements appeared in the rates & terms documents treated as the intervention point. The control group consisted of lender-broker pairings whose available rates and terms documents consistently indicated the absence of other commercial arrangements throughout the observation period.

5.23 The analysis was based on a matched sample of DD1 covering 2007-2017, rather than the full DD1 extract to 2024. This was because the other commercial arrangements indicators were derived from rates & terms documents matched to DD1, and the matched lender-broker pairings that met our control and treatment definitions were only observed up to 2017. After 2017, coverage of rates & terms became partial and uneven, with many firms providing incomplete, unclear, or missing documents, or only "NA" entries where no usable terms existed. In addition, disclosure fields were not recorded in a consistent format across years, and valid matching requires multiple rates & terms documents across different years for the same lender-broker pairing, a condition frequently unmet beyond 2017. These data limitations prevented the construction of accurate post 2017 indicators, so the matched sample was necessarily limited to the years where the underlying documentation was complete and reliable.

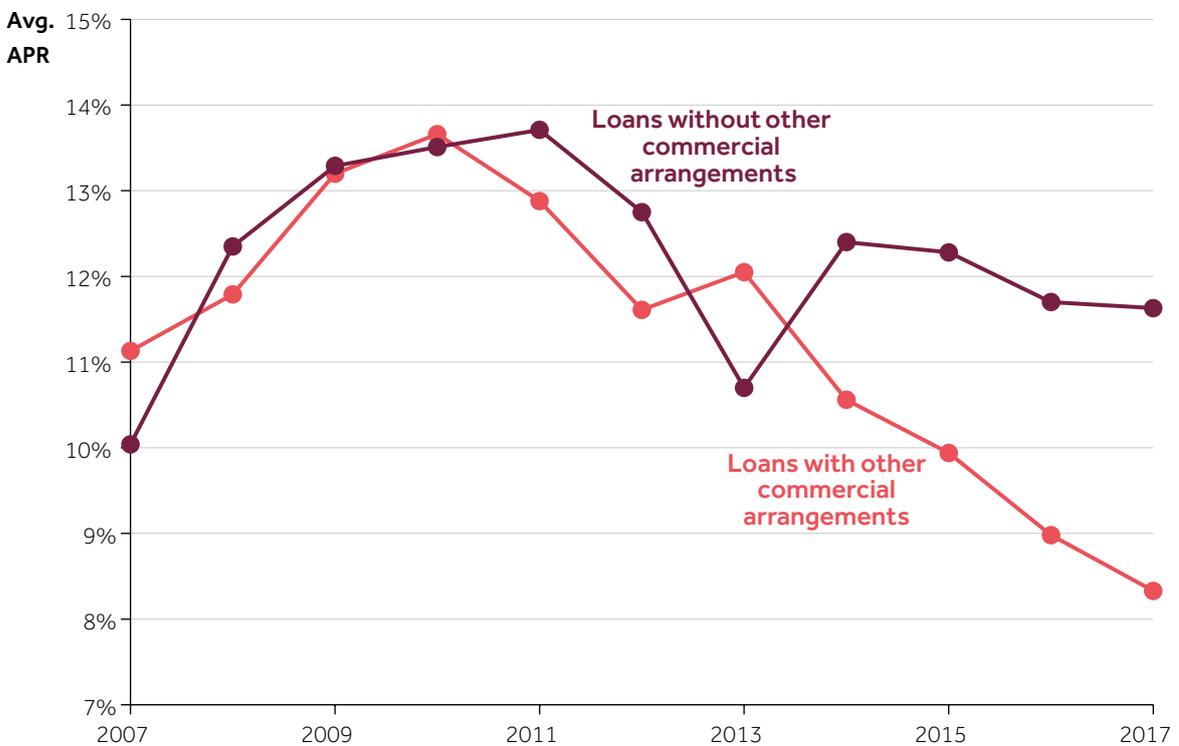
38 CP25/27 Technical Annex 1 paragraph 1.22.

39 For clarity, "other commercial arrangements" in the DD1 analysis refers specifically to arrangements including volume-based terms packages, finance-penetration-linked rate adjustments, stocking-facility rate reductions, advance-commission loans, and other lender support mechanisms that operated at the dealer or group level rather than on an agreement-specific commission basis.

Descriptive findings

5.24 Agreements with identified other commercial arrangements exhibited lower average APRs than those without other commercial arrangements (+2.4%) when considered over the entire period (2007-2017). The gap between the two narrowed over time: in 2007, loans with other commercial arrangements carried a higher average APR than loans without (11.13% versus 10.04%), but by 2013-2017 this pattern reversed (ranging from 12.05% versus 10.70% in 2013 to 8.33% versus 11.63% in 2017). However, these findings are unadjusted for risk, credit characteristics, or vehicle condition and should be treated as descriptive only.

Figure 15: Average annual percentage rate by other commercial arrangement status, 2007-2017 (DD1 matched sample)



Difference-in-differences analysis

5.25 Given the staggered timing of treatment, whereby different lender-broker pairings adopted other commercial arrangements in different years, the analysis needed an estimator that accounted for treatment effect heterogeneity across cohorts. So, we applied the Sun and Abraham (2021)⁴⁰ estimator, which addressed the well-documented bias in standard two-way fixed effects regressions under staggered adoption. A credible difference-in-differences design also requires that, in the absence of treatment, the treated and control groups would have followed parallel trends in annual percentage rates. We tested this assumption formally and observed significant violations of the parallel trends assumption ($p < 0.001$). On this basis, we do not report the difference-in-differences estimates, as this analysis was not robust. Although some post-treatment reductions in annual percentage rate appeared in the event study, strong

40 Sun, Liyang & Abraham, Sarah, 2021. "Estimating dynamic treatment effects in event studies with heterogeneous treatment effects." *Journal of Econometrics*, Elsevier, vol. 225(2), pages 175-199.

pre-treatment trends meant these could not be attributed to the adoption of other commercial arrangements. Two-way fixed effects⁴¹ estimates were likewise unstable and statistically insignificant.

Conclusion

- 5.26** On this basis, the DD1 dataset provided descriptive associations only. It did not support a causal assessment of whether entering other commercial arrangements affected pricing or generated consumer loss. The analysis was constrained by the absence of sufficiently granular and consistently defined measures of commercial arrangement status and by wider data gaps that limited credible identification.

Literature review

- 5.27** In principle, the non-disclosure of tied arrangements can give rise to information asymmetries between firms and consumers, undermining the efficiency rationale that the vertical restraints literature typically invokes. Yet the specific question of whether non-disclosure of tied and other commercial arrangements causes consumer harm has not been directly addressed in the literature. Overall, the literature did not provide a definitive conclusion on whether consumer loss can arise from non-disclosure of tied and other commercial arrangements. The full literature review ('Literature review: vertical arrangements in credit markets') is below.

Literature review: vertical arrangements in credit markets

Executive summary

- 5.28** This literature review examines how vertical arrangements between brokers and firms influence prices in comparable markets. Vertical arrangements include tied arrangements (contractually exclusive) and other commercial arrangements (non-exclusive), which respectively require or incentivise brokers to direct consumers to a particular lender. This review is not exhaustive and focuses on the strands of literature most relevant to broker-lender relationships.
- 5.29** The review aims to address two core questions:
- Are vertical arrangements detrimental to consumers?
 - To what extent does disclosure of vertical arrangements change consumer behaviour, and potentially mitigate any detrimental effects?
- 5.30** On the first question, the economic literature generally treats vertical arrangements – whether involving tied or other commercial arrangements – as potentially benign or welfare-improving in competitive markets, on the basis that they can reduce transaction costs, resolve free-rider problems, and mitigate information asymmetries. However,

41 Wooldridge, J.M. (2021). Two-Way Fixed Effects, the Two-Way Mundlak Regression, and Difference-in-Differences Estimators. SSRN Electronic Journal.

this literature does not systematically distinguish between tied and other commercial arrangements, or between disclosed and undisclosed arrangements, limiting its applicability where these distinctions are at issue.

- 5.31** On the second question, for this review, disclosure refers to regulatory requirements obliging intermediaries to reveal commercial arrangements or incentive structures to consumers. In principle, the non-disclosure of tied arrangements can give rise to information asymmetries between firms and consumers, undermining the efficiency rationale that the vertical restraints literature typically invokes. Yet the literature does not directly address the specific question of whether non-disclosure of tied arrangements causes consumer loss. Overall, the literature does not provide a definitive conclusion on whether consumer loss arises from non-disclosure of tied arrangements.

Definition and conceptual framing

- 5.32** Vertical arrangements refer to the commercial or contractual arrangements between a lender and a broker that establish a structured relationship between the parties. In the motor finance market, we use this term to include both tied and other commercial arrangements.
- 5.33** Tied arrangements are the most restrictive type of vertical relationship. They arise where a broker is contractually required to introduce consumers exclusively to a particular lender, or where the lender is given a ROFR (or equivalent right of priority). This means the broker must allow that lender the opportunity to offer credit before approaching any alternatives. These arrangements, if operating in practice, significantly limit the broker's access to a broader panel of firms and can materially influence consumer outcomes. This was central in the Supreme Court ruling in *Johnson*, where the Supreme Court found the agreement unfair in part because the ROFR clause had been concealed. The consumer had been led to believe that the broker would consult multiple firms and had not been properly informed of the exclusivity embedded in the arrangement.
- 5.34** Other commercial arrangements sit at the less restrictive end of the spectrum. These still involve a vertical relationship but take the form of weaker or less formalised arrangements such as volume bonuses, or incentive structures that encourage the broker to direct business towards a particular lender. While other commercial arrangements do not impose strict exclusivity, they can still influence broker behaviour and the way consumers are introduced to firms.
- 5.35** At the opposite end is a no-tie relationship in which the intermediary has no contractual obligation and no commercial incentive, such as bonuses, commissions, or preferential terms, to favour any particular lender. The broker is free to select from the full panel of firms without financial or contractual steering pressures.

Theoretical foundations: information frictions, principal-agent conflicts and incentive misalignment

5.36 In considering the competitive and consumer welfare impacts of exclusive arrangements between brokers and firms, it is important to consider the incentives of the key parties in the market. Vertical arrangements between broker and lender can change how intermediaries search, advise and place consumers. The fact that brokers, as intermediaries, perform functions on behalf of both borrowers and firms means that their duties and loyalties do not sit exclusively with either party. In *Johnson (Hopcraft v Close Brothers Limited; Johnson v FirstRand Bank Limited; Wrench v FirstRand Bank Limited* [2025] UKSC 33), the Supreme Court confirmed that motor dealers (acting as brokers) do not owe fiduciary duties to customers and '*pursue their own commercial interests throughout the transaction*'⁴². This legal position highlights the central theoretical issue of incentive alignment: even if a tie improves efficiency, it may also tilt recommendations towards what benefits the intermediary or lender, rather than what best serves the consumer.

Potential consumer benefits/no loss in competitive markets

5.37 In theory, other commercial arrangements could reduce information asymmetries and deliver lower prices or better service through economies of scale. When firms and intermediaries share data and processes, brokers may gain better insight into product features and pricing. This could help consumers make more informed choices.

5.38 As tied arrangements represent a stronger form of other commercial arrangements described above, they may further strengthen the relationship between the broker and the lender, to the benefit of the lender. In theory, this can also deliver benefits to consumers, by encouraging both parties to invest in ways that reduce vertical supply frictions. This type of integration may simplify application procedures, shorten processing times, and support more consistent delivery of products.

5.39 Economic theory on vertical agreements indicates that vertical coordination can generate efficiencies within a supply chain by better coordinating between the vertical elements in the supply chain. Such coordination can reduce double marginalisation by limiting successive mark-ups⁴³ and can address free-riding on sales or service investments⁴⁴.

5.40 Specific to exclusive arrangements, Lafontaine and Slade (2005) find that, in competitive markets, exclusive dealing is frequently associated with lower prices and improved service outcomes rather than consumer loss. There is, however, a trade-off. Increasing scale and tighter vertical ties may lower prices via efficiencies. But they can also enhance market power and facilitate greater extraction of consumer surplus or foreclosure where conditions allow. So, overall impacts depend on market structure and the underlying incentives of the firms involved.

42 Hopcraft and another (Respondents) v Close Brothers Limited (Appellant); Johnson (Respondent) v FirstRand Bank Limited (London Branch) t/a MotoNovo Finance (Appellant); Wrench (Respondent) v FirstRand Bank Limited (London Branch) t/a MotoNovo Finance (Appellant)

43 Double Marginalization and Vertical Integration

44 Vertical Restraints | Economics for Competition Lawyers | Oxford Law Pro | Oxford Academic

- 5.41** More broadly, the literature emphasises that many restrictions or preferential arrangements can be efficiency-enhancing (eg, improving investment incentives, resolving information problems, or reducing transaction costs)⁴⁵, with loss typically linked to market power and foreclosure rather than the mere existence of a vertical link⁴⁶.
- 5.42** It is recognised that both tied arrangements and other commercial arrangements can lead to misalignment between the incentives of the consumer, the intermediary and the lender. But such arrangements can also be benign or welfare-improving in sufficiently competitive markets, especially where they reduce search frictions and intensify lender competition.
- 5.43** This framing is consistent with how competition authorities treat vertical agreements. The UK CMA's VABEO guidance explicitly states that 'vertical agreements do not generally give rise to competition concerns unless one or more of the parties to the agreement possesses market power on the relevant market or the agreement forms part of a network of similar agreements' (CMA, 2022)⁴⁷. The EU⁴⁸ and VBER⁴⁹ similarly block exempt broad categories of vertical agreements, highlighting potential positive effects such as lower prices, improved quality, and non-price competition in competitive settings (EU Vertical Guidelines, 2022).
- 5.44** However, brokers' commissions can also raise total costs, indicating that the net effect depends on the design of these ties. In competitive markets, vertical arrangements may reduce frictions and improve information; in other settings, remuneration structures can create steering risks. The balance of these forces is ultimately an empirical question.

Evidence from intermediation in the mortgage market

- 5.45** In retail credit distribution, broker intermediation creates a vertical structure (firms upstream, brokers downstream) and vertical arrangements between these parties are essential for this channel to work. Empirically, mortgage-market studies primarily document how intermediation can intensify competition and reduce frictions. This is the market with the most extensive empirical evidence, and so we first turn to these studies to understand the channels through which vertical agreements could matter (information production, switching/search costs, and the incentives created by remuneration). These papers typically evaluate the brokered channel as a whole rather than direct estimates of the welfare effect of any form of commercial arrangements.
- Myśliwski and Rostom (2022) find the presence of brokers reduces firms' market power and lowers average monthly mortgage costs by 21%, while cutting welfare losses from search frictions by 70%.

45 Joskow, P.L. (2025). Vertical Integration. Handbook of New Institutional Economics, pp.417–446. doi:https://doi.org/10.1007/978-3-031-50810-3_18.

46 Vertical Integration, Springer Nature Link (formerly SpringerLink)

47 Vertical agreements block exemption order guidance, page 4.

48 EUR-Lex – 52022XC0630(01) – EN – EUR-Lex

49 Regulation – 2022/720 – EN – EUR-Lex

- Belgibayeva et al. (2025) show that borrowers who originated directly with a lender are less likely to remortgage and more likely to stay with the same lender (10% external remortgage rate vs 24% for broker-originated loans), while broker involvement reduces the non-pecuniary 'cost of taking action', a channel that can increase switching and intensify competition. This is consistent with the view that operational/contractual lender-broker linkages can lower transaction costs of switching (process, paperwork, search), even though the paper's object of study is the broker channel rather than a specific vertical clause.
- Robles-Garcia (2019) finds that commission bargaining between firms and brokers facilitates the entry/expansion of lower-cost 'challenger' firms, strengthening upstream competition. At the same time, the agency problem in broker pay can distort advice, yielding mixed equilibrium effects (ie benefits from stronger competition but risks from incentive misalignment). This is particularly relevant for vertical relationships including other commercial arrangements, as the lender-broker remuneration structure creates incentives that can lead to steering.
- Using US data on total mortgage costs, Zhang (2025) reports that brokers reduce total borrowing costs by 4.75 basis points (bps), combining 5.67 bps lower interest rates with 9.18 bps higher fees. Shopping around delivers the largest saving (7.4 bps), with broker engagement the next most effective in the study. Here, vertical ties are indirectly involved: brokers need access to multiple firms' offers and operational connectivity, but the study focuses on the overall intermediation effect, not the impact of specific lender-broker ties.

5.46 The mortgage market evidence reflects the overall equilibrium effect of the broker channel, which combines (a) brokers' services and (b) the vertical agreements funding and supporting them (commissions, lender infrastructure). It does not isolate the causal impact of any single 'tie' (eg training, marketing support, or specific incentives). These vertical agreements can also support operational efficiency. For example, lender-funded training may improve brokers' understanding of credit risk and product suitability. Marketing support can reduce acquisition costs, which could allow firms to offer lower rates. Volume-based incentives might enable firms to plan capacity and manage funding more effectively. If these efficiencies are passed on, consumers could benefit through lower prices or better-quality service.

5.47 Taken together, in sufficiently competitive markets, vertical relationships can result in no loss (or be beneficial) if they mainly reduce frictions and improve information, with the resulting efficiencies potentially passed through to consumers. However, since empirical studies largely focus on the broker channel, not individual ties, the net effect depends on the design of the tie, the scope for pass-through and discipline by competition.

Potential loss (conflict of interest and steering)

5.48 The principal-agent literature provides a theoretical foundation for analysing ties. When consumers (principals) rely on brokers (agents) to secure credit from firms, three features are salient: (i) agents may respond to supply-side incentives (the lender's

reward) rather than purely to consumer welfare⁵⁰; (ii) disclosure of ties addresses informational asymmetry but does not necessarily alter incentive misalignment⁵¹; (iii) consumer outcomes depend strongly on the capabilities of the demand side, particularly consumers' financial understanding, confidence in evaluating options, and ability to challenge intermediary recommendations. The Supreme Court's findings in *Johnson* align with this framework: the Supreme Court held that motor dealers "*pursue their own commercial interests throughout the transaction*," and in assessing unfairness, it emphasised that Mr Johnson was "*commercially unsophisticated*" and that it was questionable how far a lender could reasonably expect a customer to read or understand the detail of the documents provided⁵². Consumers who are less experienced, less financially literate or time-pressured are more exposed to steering, and disclosure alone may be unlikely to protect them. These vulnerabilities interact with market structure and the extent to which inducements can be avoided, influencing the degree of loss⁵³.

- 5.49** While this literature mostly focuses on incentives rather than exclusivity, the same logic applies to tied arrangements. The distinguishing feature of a tied arrangement is that it effectively removes the broker's outside options. This changes the broker's optimisation problem: steering is no longer merely a response to differential rewards, but a structural consequence of restricted choice. Tied arrangements therefore exacerbate principal-agent problems not only by misaligning incentives, but by reducing the scope for consumer optimal search. In contrast to other commercial arrangements incentives, which brokers can theoretically choose to ignore, tied arrangements which are operating reshape the feasible set of recommendations itself.
- 5.50** Inderst & Ottaviani (2012) model how commissions and kickbacks distort advice: disclosure or caps may reduce steering in some cases but may also reduce effort or distort competition in others.
- 5.51** While Guiso et al. (2021) focus on banks' direct relationships with borrowers rather than third-party brokers, their findings reinforce the broader principal-agent logic: misalignment arises when financial incentives conflict with consumer welfare. They show that banks steer borrowers towards products that reduce the bank's risk exposure or increase margins, such as favouring adjustable-rate mortgages when long-term funding costs are high, rather than those best suited to the borrower. The incentives identified include balance-sheet exposure to interest rate risk, cost of long-term funding, liability structure (deposit vs bond financing), differences in bond spreads, profit margin considerations, and commission-based schemes. The authors estimate that steering in the Italian mortgage market leads to welfare costs, measured as the mean-variance utility shortfall relative to the household's optimal mortgage choice, equivalent to approximately 11% to 19% of the annual mortgage payment, with naive borrowers bearing the largest losses. Importantly, this framework also shows why the

50 Jean-Jacques Laffont and Martimort (2009) argue that agents often act to maximise their own payoff, which can diverge from the principal's welfare when incentives are misaligned. Shah (2014) similarly notes that compensation structures and supply-side rewards can distort brokers' actions away from client interests.

51 Macho-Stadler and Pérez-Castrillo (2020) explain that disclosure reduces hidden information but does not eliminate moral hazard or conflicts of interest inherent in agency relationships.

52 *Hopcraft and another (Respondents) v Close Brothers Limited (Appellant); Johnson (Respondent) v FirstRand Bank Limited (London Branch) t/a MotoNovo Finance (Appellant); Wrench (Respondent) v FirstRand Bank Limited (London Branch) t/a MotoNovo Finance (Appellant)*

53 Rashied et al. (2024) show that market concentration and structure significantly influence consumer welfare and the effectiveness of regulatory interventions.

welfare effect of restricting 'steering' is mixed: advice can convey useful information, so blunt restrictions may reduce distortion but also reduce information provision. By contrast, alignment of banks' incentives with consumer welfare improves when borrowers are sophisticated, when advice fills a genuine information gap (rather than simply pushing a product, and when regulators or incentive schemes constrain intermediaries to act in the consumer's best interest, such as through financial-literacy programmes that reduce borrowers' naivety.

5.52 These examples are drawn from the mortgage market because it is among the most empirically studied retail credit market. However, the underlying principal-agent mechanisms, whereby financial incentives distort intermediary recommendations away from consumer-optimal outcomes, could generalise to other intermediated consumer-finance settings, including motor finance.

5.53 A behavioural perspective sharpens this principal-agent framing by identifying specific cognitive limitations and biases that make consumers potentially vulnerable to commercial arrangements. Many of these vulnerabilities are exacerbated when key features of the product or intermediary relationship are not disclosed. In principle, disclosure can mitigate some biases by making costs and conflicts salient, although its effectiveness depends on how it is designed and interpreted:

- Behavioural vulnerabilities
 - **Complexity and limited processing capacity.** Credit products typically involve multiple non-linear dimensions (APR, term, fees, residuals). Carlin (2009) shows that firms can profitably design strategically complex retail financial contracts, exploiting consumers' limited ability to process complex fee and rate structures and thereby allowing price dispersion to exist even in competitive markets. This can help explain why consumers struggle to evaluate the impact of other commercial arrangements on pricing, even when some information is disclosed.
 - **Shrouded attributes and consumer myopia.** Xavier Gabaix and David Laibson (2006) show that in markets with behavioural biases, firms are incentivised to 'shroud' or hide costs rather than compete on transparent pricing. Markets can settle into an equilibrium where firms exploit myopic consumers through hidden, overpriced add-ons, while extracting surplus from rational consumers through pricing schemes enabled by the continued presence of naive consumers. As long as a share of consumers remains myopic, hidden fees and inefficient pricing can persist even under competition.
 - **Trust and reliance on advisers.** Mullainathan, Noeth & Schoar (2012) use an audit study to show that financial advisers often reinforce clients' existing mistakes and steer them towards higher-fee products rather than correcting biases or minimising costs. Consumers appear to apply a trust heuristic, deferring to advisers in the face of complexity rather than independently optimising.

- How disclosure can mitigate these biases
 - In the Inderst & Ottaviani (2012) theoretical framework, disclosure plays a behavioural role by converting naïve consumers, who do not appreciate advisers' incentives, into 'wary' ones who adjust their interpretation of advice once conflicts are made explicit. This can reduce misdirected trust and improve outcomes under some conditions.
 - In large scale UK field experiments, Adams et al. (2020) find that even when consumers are provided with high quality, personalised disclosure about better savings products, many still fail to act (only 8.9% acted at baseline, and the average disclosure lifted switching by just 0.7%). But improvements occur when disclosures are made more salient, more visually prominent (a front page rate comparison doubled switching from 3% to 6%), or easier to process (a prefilled 'just sign here' form raised it from 3% to 12%), demonstrating that behavioural frictions like pessimistic beliefs and inattention mediate the effect of disclosure.
- Limits and unintended effects of disclosure
 - **Conflict-of interest disclosure and "perverse" effects.** Experimental work by Cain, Loewenstein & Moore (2005) and Sah, Loewenstein & Cain (2013) finds that conflict-of interest disclosure does not reliably prompt consumers to discount biased advice. In some settings, disclosure even increases compliance with conflicted advice: advisers feel 'morally licensed' to exaggerate, while advisees feel social pressure not to appear distrusting ('insinuation anxiety').

Regulatory implications: why disclosure alone is insufficient

5.54 The behavioural mechanisms discussed above can exacerbate the misalignment of incentives. Other commercial arrangements can also create incentives to steer. Complexity and trust heuristics can make it hard for consumers to recognise the cost of that steering or to resist it. Disclosure of conflicts may change stated understanding without reliably changing decisions and can sometimes worsen outcomes. This is consistent with the findings of a literature review commissioned in CP25/27 Technical Annex 1 from Professor Zinman⁵⁴, who concluded that there is little high-quality empirical evidence that disclosure reliably improves consumer outcomes in two-sided markets, and that prospects for using information interventions to improve consumer and market outcomes remain unclear. On the other hand, an undisclosed or partially disclosed arrangement reduces the information available to the consumer to make an informed decision on the suitability of the finance product, regardless of whether they would utilise the information.

5.55 Guiso et al theoretical framing implies ties become problematic when incentives favour the intermediary's profit over the consumer's best terms; they can be beneficial when appropriately aligned (eg reward for search effort) and when disclosure is paired with structural safeguards. These safeguards could include prohibiting commission schemes tied to loan terms and banning 'dual redress, where originators receive payment from both lender and borrower (CFPB, 2013), as well as enforcing qualification standards under the SAFE Act (CFPB, 2008) and Regulation Z (CFPB, 2013). Disclosure alone is

54 CP25/27: Technical Annex 1: Consumer Redress Scheme for Motor Finance – Data, Analysis of Loss and Liability and Cost Methodologies, p.85.

insufficient, Dasgupta (2023) argues it must be embedded in regulatory frameworks that enforce accountability, such as Basel-style regimes combining transparency with supervisory oversight (IMF, 2024). Further, Guiso et al. (2019) show that tightening fiduciary standards, such as those introduced under the Obama administration for the US in 2016, can curb distorted advice and better align intermediary behaviour with consumer interests.

- 5.56** Another strand of literature analyses the competitive effects of vertical agreements, emphasising that they can enable *foreclosure* when a firm with sufficient market power uses such arrangements to deter entry or disadvantage existing competitors. Classic treatments define foreclosure as a dominant firm's denial of proper access to an essential input or distribution channel, thereby extending market power from one segment to another. Rey & Tirole (2003) provide a comprehensive overview, showing how vertical agreements, including exclusive dealing, access restrictions, and tie-ins, can raise rivals' costs or prevent efficient entry when market power exists upstream or downstream. Similarly, the antitrust literature highlights that foreclosure concerns arise when a bottleneck input or distribution route cannot be easily replicated, allowing the upstream firm to squeeze downstream rivals or insulate its position against future competition.
- 5.57** In practice, foreclosure is most likely where the upstream firm controls an *essential facility* or enjoys durable market power. Where such conditions do not hold, vertical restraints generally do not generate exclusionary effects. In the motor finance market, existing evidence indicates that competition is sufficiently robust at both the lender and broker levels, limiting the ability of any single firm to use exclusive arrangements to preclude rivals⁵⁵. This aligns with the broader conclusion in the vertical restraints literature that, in competitive markets, without bottlenecks, dominance, or barriers to entry, exclusivity is less likely to raise rivals' costs or lead to meaningful foreclosure. As a result, foreclosure does not arise as a primary concern in this setting.
- 5.58** Overall, economic theory suggests that in competitive markets, ties can be benign (or beneficial) through information and efficiency channels. Loss usually occurs when market power or foreclosure is present rather than due to the vertical arrangement itself. However, some empirical evidence from related markets suggest that ties can generate conflicts of interest and steering, particularly when products are complex and consumers either lack financial literacy or fall within categories typically considered vulnerable.

Empirical evidence: vertical agreements (both tied and other commercial arrangements) in credit markets

Empirical studies suggest that other commercial arrangements can affect pricing, product choice and loan quality

- 5.59** Evidence directly examining other commercial arrangements shows that these incentives can materially distort outcomes for borrowers.

55 Please see CP25/27: Technical Annex 2: State of Competition in the Motor Finance Market, published on 7 October 2025.

- **Volume-based dealer incentives in car finance.** Jansen et al. (2023) analyse the US automotive market and show how convex, stair-step dealer bonus schemes, characterised by large, discrete payments for meeting monthly new vehicle sales targets, shape both sales behaviour and loan outcomes. These programs typically pay nothing if the dealer misses the target, but release substantial bonuses once thresholds are crossed, with marginal vehicles at month end generating tens of thousands of dollars in additional dealership revenue and triggering extra salesperson 'spiffs'. The resulting end of month pressure induces aggressive discounting, concentrates originations on the final day and shifts borrowers into new-car loans. Using data on sub-prime loans across more than 3,500 dealerships, the authors find that default rates for loans in the highest payment to income quartile rise from 13.6% to 19.7% for loans originated on the final day of the month. This demonstrates how volume/threshold incentives can spill over into consumer credit quality and overall borrowing costs, which can in turn lead to potential consumer loss through increased default risk and distorted loan allocation.
- **Broker-lender redress in mortgages.** Allen et al. (2023) study mortgage intermediaries who receive upfront commissions, ongoing trailer fees, and volume bonuses from multiple firms. These non-exclusive redress structures significantly influence the amortisation horizons recommended to borrowers. The authors find that brokers steer consumers toward longer amortisation periods, increasing total interest paid over the life of the loan. Their counterfactual simulations suggest that removing such incentive schemes would shorten amortisation by an average of 4.6 years and save affected households more than \$34,000. This provides direct evidence that other commercial arrangements incentives influence product choice, repayment structure, and long-term borrowing costs.
- **Contingent commissions in commercial insurance intermediation.** Cummins and Doherty (2006), examining the US commercial property-casualty insurance market, provide empirical evidence that both premium-based and contingent commissions, including those conditioned on profitability, persistency or volume, are passed through to policyholders via higher premiums. These redress structures constitute a form of other commercial arrangements between intermediaries and insurers. The authors also highlight potential efficiency enhancing effects. When contingent commissions are profit-based, they can improve underwriting quality by incentivising intermediaries to share more accurate risk information, mitigating adverse selection. In addition, stronger intermediary-insurer relationships can help reduce winner's curse dynamics in competitive bidding environments, promoting more efficient market outcomes.

5.60 Together, evidence from other markets (specially the US auto, Canadian mortgage and US commercial insurance markets), rather than the UK motor finance market show that other commercial arrangements can alter the terms that intermediaries recommend, raise the total cost of borrowing, and weaken loan quality. Moreover, in insurance, contingent commissions (a potential form of other commercial arrangements) are passed through into premiums yet, when profit linked, can enhance market efficiency by improving information flows, reducing adverse selection and spurring more aggressive bidding.

Empirical evidence on tied arrangements and captive finance

- 5.61** Unlike other commercial arrangements, which rely on discretionary incentives, tied arrangements are defined by formal contractual exclusivity. This section examines the empirical impact of these vertical agreements through the lens of captive finance companies, firms owned by manufacturers, who frequently operate under such exclusive or preferred-lender agreements.
- **Captive finance exclusivity and loan performance.** Barron, Chong & Staten (2008) examine exclusive relationships between automotive manufacturers and their captive finance subsidiaries, assessing how these tied arrangements affect credit outcomes. Using TrenaData credit bureau information covering more than 3,000 US counties from 2000-2003, the authors show that captive financed loans consistently perform worse than bank-originated loans. Average delinquency rates are 2.26% for captive loans compared with 1.49% for bank loans, and annual results exhibit the same pattern; in 2003, delinquency rates were 1.44% for captive loans versus 0.97% for bank loans. Panel regressions controlling for income, unemployment, debt burdens, housing wealth, divorce rates and county average credit scores confirm that captive loans remain significantly more likely to become delinquent. These findings highlight how tied arrangements in captive finance arrangements can prioritise manufacturer sales volumes over borrower credit robustness, embedding structural credit-risk externalities for consumers. That said, because the analysis uses aggregate (county-level) data rather than borrower-level microdata, the results should be read as selection-robust at the county level, not as a borrower-level causal estimate.
- 5.62** In conclusion, while tied arrangements (in particular captive finance) can be understood as an efficiency-seeking arrangement that broadens access to credit, including for more marginal borrowers, they may also be associated with slightly weaker loan performance compared with bank loans. This pattern appears to reflect a combination of broader access, incentives within vertically integrated structures to support manufacturer sales volumes and may indicate that consumer loss may be borne out of more risky lending strategies.
- A wider set of studies examine other intermediary incentives that are not other commercial arrangements in a legal sense but operate through the same mechanism***
- 5.63** That is, intermediaries respond to supplier-side rewards even when these do not align with consumer interests. These findings are relevant because other commercial arrangements work through this same behavioural channel.
- 5.64** Empirical work shows that commercial arrangements such as volume-based dealer incentives, target bonuses and lender broker/distributor mark ups have material effects on pricing and consumer outcomes in credit markets, although evidence on the effects of disclosure of such arrangements is extremely limited. For example:
- **Discretionary dealer mark-ups in auto finance (Grunewald et al., 2020):** The study analyses dealer-lender arrangements in US auto finance where dealers earn a share of profits from discretionary interest rate markups. They document that

consumers are substantially less sensitive to loan prices than vehicle prices, enabling dealers to exploit this wedge by tailoring price bundles. On average, consumers behave as if they would pay \$1 in finance charges to reduce the car price by only \$0.86 and misperceive finance costs by at least \$380 (around 20% of total charges). Counterfactual simulations show that banning dealer discretion, similar to the FCA's 2021 policy, would lower average transaction prices by \$283 (a 0.92% decrease) and raise consumer surplus by 3.24% to 5.63%, with the largest gains for less financially sophisticated and lower income borrowers. While this is an important contribution, the modelling approach sacrifices some empirical realism (eg, assuming other contract terms are largely exogenous rather than jointly negotiated) to enable tractable competition-policy analysis.

- **Evidence on disclosure from advice markets (Wan & Lighthall, 2022):** The closest empirical analogue on disclosure comes from the investment-advice market. Wan and Lighthall (2022) conducted a randomised controlled experiment with 618 U.S. participants to examine the US Form CRS 'relationship summary'. They find that relationship disclosures between investment advisers and broker-dealers significantly improve participants' comprehension of account types and lead 21.5% of participants to switch from adviser to broker-dealer accounts. Increased switching is concentrated among more experienced investors and is associated with greater comprehension gains and exposure to more detailed disclosures, suggesting that disclosure can meaningfully shift perceptions and choices, but in a heterogeneous way across consumer groups. Importantly, this work concerns disclosure of relationship type and capacity, rather than volume-based vertical agreements or target bonuses and is set in the investments rather than consumer-credit context.
- **Incentive design in intermediated services (Thon et al., 2024):** This study examines incentive structures in a large European travel distribution network where intermediaries bridge upstream firms and end customers. In a randomised trial involving 1,257 independent agencies, the authors compare non-monetary support (priority hotline access to reduce effort costs) with monetary incentives targeted either at sales agents (travel vouchers) or agency owners (cash payments). They find that non-monetary support increased bookings by 16% overall and 25% among low-commission agencies, generating significant profit gains, while vouchers improved sales only in low-incentive contexts and owner-targeted payments had no effect. Mechanism analysis highlights that reducing effort costs and targeting incentives directly at agents is more effective than rewarding intermediary owners, which suffers from 'double marginalisation' as intermediaries have to be motivated to motivate their workers. These findings underscore that other commercial arrangements can amplify sales effort, but their design critically determines effectiveness and profitability.
- **Commission-based pay for loan officers (Agarwal & Ben-David, 2014):** Shifting loan officers from fixed pay to commission-based remuneration increased origination volumes by 31% and defaults by 28%. Behavioural distortions included greater reliance on discretionary internal risk ratings (which doubled in weight but added noise) and nonfundamental drivers such as end-of-month timing, officer tenure and gender effects, all correlated with higher default risk. These results illustrate how high-powered incentives can boost productivity but degrade credit quality and consumer welfare when misaligned with long-term performance.

5.65 Direct empirical studies show that other commercial arrangements, such as volume bonuses for dealers and commission structures for mortgage brokers, can meaningfully distort pricing, product choice and loan quality. A wider literature review, which we have discussed above including work on dealer markups (Grunewald et al., 2020), advice market disclosure (Wan & Lighthall, 2022), intermediary incentive design (Thon et al., 2024), and commission based pay (Agarwal & Ben-David, 2014), demonstrates that other intermediary incentives function through the same behavioural channel: when suppliers reward intermediaries for particular outcomes, those incentives often override consumer interest. Evidence from advice markets demonstrates that relationship disclosures can alter comprehension and, for some consumers, account choice, but these effects are heterogeneous and context dependent. Crucially, there is almost no direct, field-level evidence on the effects of disclosing other commercial arrangements of the type relevant here (for example, volume-based referral incentives or non-exclusive commercial agreements between firms and brokers) on loan pricing, steering or consumer welfare in credit markets. The available studies therefore support concern about other commercial arrangements themselves but leave the effectiveness of other commercial arrangements disclosure largely unresolved.

Empirical evidence: disclosure of other commercial arrangements from other sectors

5.66 Direct empirical studies of other commercial arrangements disclosure in consumer credit are scarce. However, behavioural research from sectors such as advertising and healthcare provides analogues of the mechanics of disclosure.

- Eisend et al. (2020) synthesise 61 studies (57 datasets) of sponsorship disclosure and finds that disclosure increases brand recognition (30%) and persuasion knowledge (25%) but reduces credibility (-13%) and brand attitude (-10%), with no significant effect on behavioural intention.
- Other work (eg, Jhavar et al., 2024) shows that disclosure of influencers' relationships leads to avoidance and switching due to perceived "psychological contract violation".
- In healthcare, DeJong, Aguilar & Tseng (2016) link minor hospitality by pharmaceutical firms to large increases in prescribing of the promoted drug (18-118%). Despite public disclosure systems, other commercial arrangements retained their influence.

5.67 These findings imply that disclosure of other commercial arrangements may increase consumer awareness and reduce trust in intermediary recommendations but does not reliably change transaction prices or reduce steering where the intermediary retains incentive power. Caution is required when translating to credit markets because decision complexity, bundling (car and finance), negotiation and intermediation differ materially. Nevertheless, the limited impact of information disclosures on behaviour is well documented in wider behavioural science literature (eg Mertens *et al.*, 2022).

Conclusion

5.68 This review has examined the literature on vertical arrangements between brokers and firms, addressing two core questions. On whether vertical arrangements are detrimental to consumers, economic theory suggests they can be benign or welfare-improving in competitive markets through efficiency and information channels. However, empirical evidence from related markets shows that both tied and other commercial arrangements can distort intermediary behaviour, raise borrowing costs, and weaken loan quality, particularly where products are complex and consumers are less financially sophisticated. That said, this literature does not systematically distinguish between tied and other commercial arrangements, or between disclosed and undisclosed arrangements, limiting its applicability where these distinctions are at issue.

On whether disclosure mitigates any detrimental effects, the evidence base is limited and mixed. In principle, the non-disclosure of tied arrangements can give rise to information asymmetries between firms and consumers, undermining the efficiency rationale that the vertical arrangements literature typically invokes. Yet the specific question of whether non-disclosure of tied arrangements causes consumer loss has not been directly addressed in the literature. As a result, the literature neither proves nor disproves consumer loss from non-disclosure of tied arrangements.

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Chapter 6

Academic review

Introduction

- 6.1** We commissioned two independent academics to conduct comprehensive reviews of Chapters 3–5 of this Technical Annex. Prof. John Gathergood (Professor of Financial Economics, University of Nottingham) and Dr. Kyle Butts (Assistant Professor of Economics, Sam M. Walton College of Business, University of Arkansas) each undertook separate reviews of a redacted version of this Technical Annex prior to its publication, prepared in accordance with the Market Abuse Regulation.
- 6.2** We also provided them with a summary of consultation feedback, primarily the comments raised in economic consultancy reports. This summary grouped the feedback into themes and set out our assessment of the strength of the criticisms contained therein. In addition, we outlined the two benchmarking approaches proposed through consultation feedback and asked for their views and advice.
- 6.3** We requested for them to structure their review using five suggested questions for the Data guide update, Analysis of loss update and Commercial arrangements:
- 1.** Confirmation that we are interpreting the results of our analysis correctly
 - 2.** Feedback on whether we have appropriately acknowledged the limitations of our analysis
 - 3.** Views on the reasoning and justification behind our modelling assumptions
 - 4.** An assessment of the overall strength of the evidence
 - 5.** Suggestions on how we could amend the analysis, given more time to do so
- 6.4** We summarise their views below.

Data guide update

Interpretation of results

- 6.5** Prof. Gathergood confirmed that, based on the material presented in the Annex, the updated datasets and differences between these and the previous version of the datasets were described and interpreted correctly in this Technical Annex. Prof. Gathergood also confirmed that our interpretation of the results (in the form of summary statistics and figures) in this section of the document were correct.
- 6.6** Dr. Butts had no further comment on this aspect of the data guide.

Acknowledgement of limitations

- 6.7** Prof. Gathergood confirmed that we set out the limitations of the data used, such as the absence of evidence on loan agreements in the DD1 data set as described in paragraph 3.23, onwards.
- 6.8** Dr. Butts had no further comment on this aspect of the data guide.

Reasoning and justification of modelling assumptions

- 6.9** Prof. Gathergood agreed that the inclusion of new lender data in the LLD was justified as it improved the coverage of the data set and therefore the generalisability of the results to the wider market. He also agreed the decision to omit firms not supplying credit scores (paragraph 3.10) was justified given the high information content of credit scores for the modelling exercise, as this was a key variable for understanding the risk profile of an individual applicant and was central to the analysis of pricing, loan availability and other credit-related decisions.
- 6.10** Dr. Butts emphasised that imputation is a standard procedure and, in this context, it was important to keep the sample representative of the lender market as a whole.

Overall strength of the evidence

- 6.11** Prof. Gathergood viewed the changes made to the data samples used for this Technical Annex as strengthening the analysis compared with those used in CP25/27. In particular, the updated LLD dataset which includes new data from an additional lender with significant market share and a longer time period, which led to an increased sample size (a substantial 56% increase), added to the generalisability of the results. New data also became available, such as the latest round of the motor financing commission monitoring survey. The analysis presented in this Technical Annex drew on a larger and more thorough data set compared with that used in CP25/27 Technical Annex 1.
- 6.12** Dr. Butts agreed that adding an additional firm improved the robustness of the results and the representativeness of the dataset (as shown in our analysis). Furthermore, Dr Butts viewed the additional years as having helped to improve the overall representability of the dataset, especially given the discussion noting that lenders were under more scrutiny in later years.
- 6.13** Dr. Butts agreed that the extra data cleaning steps in response to consultation comments improved the reliability of the analysis.

Potential improvements if given additional time

- 6.14** In Prof. Gathergood's view, there is limited scope to amend the analysis in ways which would improve the results if we were given more time to do so. He arrived at this view based on two factors.
- Additional time would not permit us to overcome the data limitations that we faced as those relate to limitations in the data supplied. Data supplied limitations, such as the large share of "other (unknown)" in the LLD data set would not be overcome. This is because the non-labelling problem cannot be resolved because the LLD contains has no further information which could be used to overcome this limitation. Similarly, the absence of credit score data for some firms in the DD1 data cannot be resolved by us as we have no reasonable means for inferring or imputing credit score in these data.
 - The additional data (firm monitoring survey data) that would arrive over time is unlikely to substantively change our findings. Data from the ongoing surveys, such as the motor finance commission monitoring survey, is not relevant to the modelling presented in the document under review here (though is relevant for other purposes).
 - If additional historical LLD were to become available, (for example, if we were to request and receive earlier-period data), then the analysis may change. As we noted, there is some indication that estimates of loss may be higher in the earlier period. If data from earlier periods became available then we might expect to find some variation in the economic loss from DCA estimates as estimated on data from those earlier periods compared with later periods. However, Prof. Gathergood was agnostic as to whether the data from earlier periods would return higher or lower estimates of loss – ie. whether there is some trend over time in the estimated economic loss.
- 6.15** In terms of presentation of the results in the document, if given more time, Prof. Gathergood believes it would be helpful if the order in which the variables are listed in the table "The LLD descriptive statistics..." were modified to be the same order as they appear in Table 13 in CP25/27 Technical Annex 1.
- 6.16** Dr. Butts stated that, if more time were available, it would be useful to assess the robustness of the results to the imputation procedure.

Analysis of loss update – DCA commission model impact

Interpretation of results

- 6.17** Prof. Gathergood's view was that we interpreted the following results correctly:
- LLD representativeness results, including the comparison of distributions of key variables and the comparison of coefficients from a parsimonious regression of APR conducted on both data sets
 - Matching diagnostics
 - The fixed effects specification and related modelling choices results

The redacted Technical Annex did not contain results from the fixed effects specification and so he could not comment on the interpretation of these results.

6.18 Dr. Butts did not comment specifically on the interpretation of the results.

Acknowledgement of limitations

6.19 Prof. Gathergood's view was that the limitations concerning the data, the econometric models and results were appropriately acknowledged.

6.20 Dr. Butts view was that we appropriately acknowledged the limitations to the analysis of disclosure, ie, that no definite conclusions could be drawn.

Justification of modelling assumptions

6.21 Prof. Gathergood's view was that the steps we took to respond to the critiques to the LLD data (limited timeframe, inaccuracies, lack of representativeness) were correct: tests of representativeness, extension of the timeframe and updating of the data cleaning procedures. Regarding the limited timeframe, he also agreed with our decision not to use DD2 on the grounds of its lack of key variables.

6.22 Prof. Gathergood noted there was limited scope to try alternative approaches (see also below), given the lack of data on agreements with compliant disclosure and the challenges in using experiments to explore the impact of disclosure on consumer choices.

6.23 Prof. Gathergood agreed with the arguments we presented on alternative counterfactuals in paragraph 4.84 onwards that concluded alternative statistical comparators would be inferior to the one used.

6.24 On the heterogeneity of harm, Prof Gathergood's view was that evidence suggested that harm likely varied according to important customer and loan characteristics. As a result, it was not reasonable to assume on econometric grounds that the output from the analysis of loss was homogenous and unvarying across consumers or loans. He acknowledged that the decision to rely on the mean estimate of loss could be justified on the basis of tractability. Given that, the results presented from our analysis remained robust.

6.25 Prof. Gathergood's view was that we took reasonable and justified steps to address critiques to the statistical methodology.

6.26 Prof. Gathergood thought that the decision to drop DCA agreements that did not have a suitable non-DCA counterfactual was justified on the grounds that we were using matching, and the percentage of dropped observations was not high.

6.27 Prof. Gathergood's view was that the use of APR as a measure of loss was correct as it captured the total cost of an agreement better than other measures. Prof. Gathergood agreed on the use of log APR given that zero APR loans are excluded from the scheme and that APR is log-normally distributed.

- 6.28** On the covariates, Prof. Gathergood's view was that it was sensible to exclude covariates with limited coverage (eg, customer employment status and income) and that these might have been partially captured by some of the fixed effects (postcode and year of birth in particular). Finally, Prof. Gathergood agreed that vehicle discount was likely endogenous to APR, and therefore it was right to not be included in the model.
- 6.29** Prof. Gathergood's view was that our decision to keep lender-year fixed effects in the regressions was justified. These fixed effects help control for selection into lender driven by unobservable characteristics that also correlate with APR and therefore strengthen the analysis.
- 6.30** Dr. Butts's view was that our choice of the outcome variable (APR) was justified, as choosing other variables may not have been feasible given data quality.
- 6.31** Dr. Butts's view was that alternative comparison groups would not have offered stronger counterfactuals than the one that we used in our analysis.

Overall strength

- 6.32** Prof. Gathergood agreed with paragraph 4.13 that the updates to the data sources and key models strengthened our evidence base for the analysis of loss. The analysis of representativeness of the LLD data increased the confidence in the generalisability of the results. This, coupled with the inclusion of an additional lender and the lengthening of the time frame added strength to the analysis. Prof. Gathergood thought the additional sensitivity checks we conducted added strength to the analysis.
- 6.33** Given our active engagement with Dr. Butts throughout the process, we were able to incorporate many of his suggestions to strengthen the analysis where feasible. This helped improve the overall robustness of the final work.
- 6.34** Dr. Butts agreed with our approach to responding to the critiques regarding the LLD, ie, adding an additional firm and extending the time frame.
- 6.35** Dr. Butts' view was that the LLD representativeness analysis was a strong way to address critiques regarding the dataset.
- 6.36** Dr. Butts' view was that the matching and regression procedure was more robust than a regression alone as it was less reliant on modelling assumptions.
- 6.37** Dr. Butts did not think that the possible presence of contemporaneous spillovers may challenge the strength of our results.
- 6.38** Overall, given the several amendments we made in response to feedback, Dr Butts had confidence in the overall robustness of our results.

Potential improvements if given additional time

- 6.39** Overall, Prof. Gathergood's view was that that there was very limited scope to undertake additional or alternative analysis that would have improved our current analysis, even if more time were available, given the data we held.

- 6.40** On the effect of disclosure, Prof. Gathergood noted that counterfactual models that compare compliant and non-compliant agreements using observational data would be challenging given the absence of compliant agreements in the data (as confirmed by our s166/Skilled person review).
- 6.41** Moreover, Prof. Gathergood noted that analysis using alternative comparison groups (eg, agreements after the ban and benchmarks) would have been statistically inferior to the approach that we took, as they would have been subjected to more confounders and based on untestable assumptions that would not permit statistical inference.
- 6.42** Prof. Gathergood thought there was also limited scope to run an experiment on this issue because it would be difficult to model full disclosure (in the absence of good examples of disclosure and given the variation in the way disclosure can happen).
- 6.43** Dr. Butts suggested that if we had more time, we could have included a table of descriptive statistics on the DCA agreements that we discarded through the matching procedure, compared to the ones that are kept, to understand the differences.
- 6.44** Dr. Butts also suggested that we could have run our models with the 2018 data left as missing and see how much of a difference it made to the estimates. If the estimates remained broadly similar, then less scrutiny would be needed on the imputation procedure.

Analysis of loss update – Commission and cost of credit for flat fee loans

Interpretation of results

- 6.45** Prof. Gathergood agreed the following results were interpreted correctly:
- first approach found no evidence for non-linearities in the relationship
 - the second approach indicated that the relationship may have been slightly downward sloping.
- 6.46** Prof. Gathergood also agreed with the inference drawn at paragraph 4.161 that the model results should not be used as a basis to indicate the high commission arrangement threshold proposed in Chapter 4 of CP25/27.
- 6.47** Dr. Butts agreed with our interpretation of the results of this analysis, ie, that they were too sensitive and of questionable veracity.

Acknowledgement of limitations

- 6.48** Prof. Gathergood agreed that limitations concerning the data, the econometric models and results were appropriately acknowledged.

- 6.49** Prof. Gathergood also noted that we were responsive to the critiques of the analysis in CP25/27 Technical Annex 1. In particular, the analysis of the relationship between commission and total cost of credit, which led to a change in the way the evidence is used and interpreted.
- 6.50** Dr. Butts agreed that we acknowledged the limitations of this work appropriately.

Justification of modelling assumptions

- 6.51** Prof. Gathergood noted that the two econometric methods—polynomial fitting and percentile dummies—are standard econometric practice. He agreed that these methods were appropriate for this analysis as they were prior-free regarding assumptions about the variables.
- 6.52** Dr. Butts did not comment on the modelling assumptions of this analysis.

Overall strength

- 6.53** Prof. Gathergood agreed that the new analysis presented consistent results across approaches and increased precision. Prof. Gathergood's view was that the new analysis provided a strong evidence base for the decision to no longer rely on this modelling approach to evidence a precise high commission threshold.
- 6.54** Dr. Butts agreed with our decision not to rely on this analysis.

Potential improvements if given additional time

- 6.55** On the association between total cost of credit and commission level, Prof. Gathergood suggested running a joint test of equality of coefficients shown in the table at paragraph 4.164. This would validate our claim that "[the results] show little clear evidence that the association of commission with total cost of credit (expressed in terms of relative to loan principal) varies with commission level".
- 6.56** At paragraph 4.168, Prof. Gathergood suggested stating the order of polynomial used in the regression (for which results are plotted in "Figure 13 – Results from regression including a polynomial in commission (relative to the loan principal)").
- 6.57** Dr. Butts did not have any comments on potential improvements on this analysis.

Commercial arrangements

Interpretation of results

- 6.58** Prof. Gathergood agreed with the findings of the literature review on commercial arrangements. He recommended several structural amendments and proposed changes to draw out the key points from the relevant literature in an earlier draft. We updated the literature review accordingly. Prof. Gathergood also confirmed that he

reviewed the responses to his earlier comments and that they were taken on board and addressed appropriately.

- 6.59** Prof. Gathergood agreed with the interpretation of the results of the new DD1 analysis of other commercial relationships. He also agreed that the descriptive analysis should be interpreted only as descriptive and not be used to infer conclusions about the cost of other commercial arrangements compared to agreements without other commercial arrangements.
- 6.60** Prof. Gathergood agreed that the difference-in-differences analysis did not meet pre-tests for satisfying the modelling assumptions of that framework, and so should not be relied upon as a modelling approach.
- 6.61** Dr. Butts did not comment on the commercial ties analysis.

Acknowledgement of limitations

- 6.62** Prof. Gathergood agreed that we appropriately acknowledged the limitations of the analysis.
- 6.63** Dr. Butts did not comment on the commercial ties analysis.

Justification of modelling assumptions

- 6.64** Prof. Gathergood agreed with our use of the Sun and Abraham (2021) estimator. He thought the approach was appropriate in this setting as it adjusted for contamination of the control group. Prof. Gathergood agreed with our approach of including pre-tests of the assumptions underlying the difference-in-differences framework, specifically of common trends and characteristics. Prof. Gathergood agreed with the decision not to pursue difference-in-differences analysis: the data did not conform to the empirical assumptions of the difference-in-differences framework, and hence that framework was not appropriate for the analysis.
- 6.65** Dr. Butts did not comment on the commercial ties analysis.

Overall strength

- 6.66** In Prof. Gathergood's view, there were no other compelling approaches to econometric modelling available given the limitations of the data and, as such, he thought the lack of inferences that could be drawn from analysis of the DD1 data supported the conclusion at paragraph 5.26 that the DD1 data set provided descriptive associations only.
- 6.67** Dr. Butts did not comment on the commercial ties analysis.

Potential improvements if given additional time

- 6.68** Prof. Gathergood sees limited opportunity for further analysis given the data we have. The DD1 data set contains observations of lender-broker pairs which switched from not exhibiting other commercial arrangements to exhibiting these arrangements at points in time. The modelling approach adopted suggested exploiting differences-in-differences was the most appropriate approach in this setting, but the data did not meet the pre-test requirements. He does not see other analytical options available given the nature of the data available.
- 6.69** Furthermore, Prof. Gathergood stated that while other forms of econometric analysis could be implemented on the DD1 data set, they would be of limited value. It would be possible to estimate pooled cross-sectional models at the broker-lender pair level in which the dependent variable is the APR, and the independent variable is an indicator variable for whether the broker-lender relationship involved another commercial arrangement. It may also be possible to estimate panel models with fixed effects.
- 6.70** These approaches would, however, most likely deliver biased estimates of the effect of another commercial arrangement on the cost of agreements due to i) selection of firms into commencing another commercial arrangement, ii) unobservable factors changing contemporaneously with the onset of another commercial arrangement.
- 6.71** Dr. Butts did not comment on the commercial ties analysis.

Chapter 7

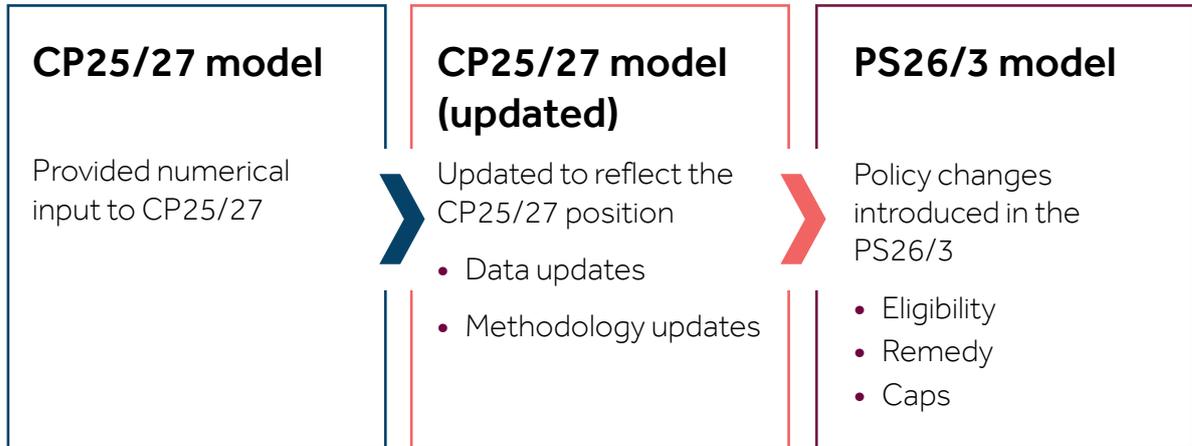
Updates to our redress liability estimates

Summary

Changes since CP25/27

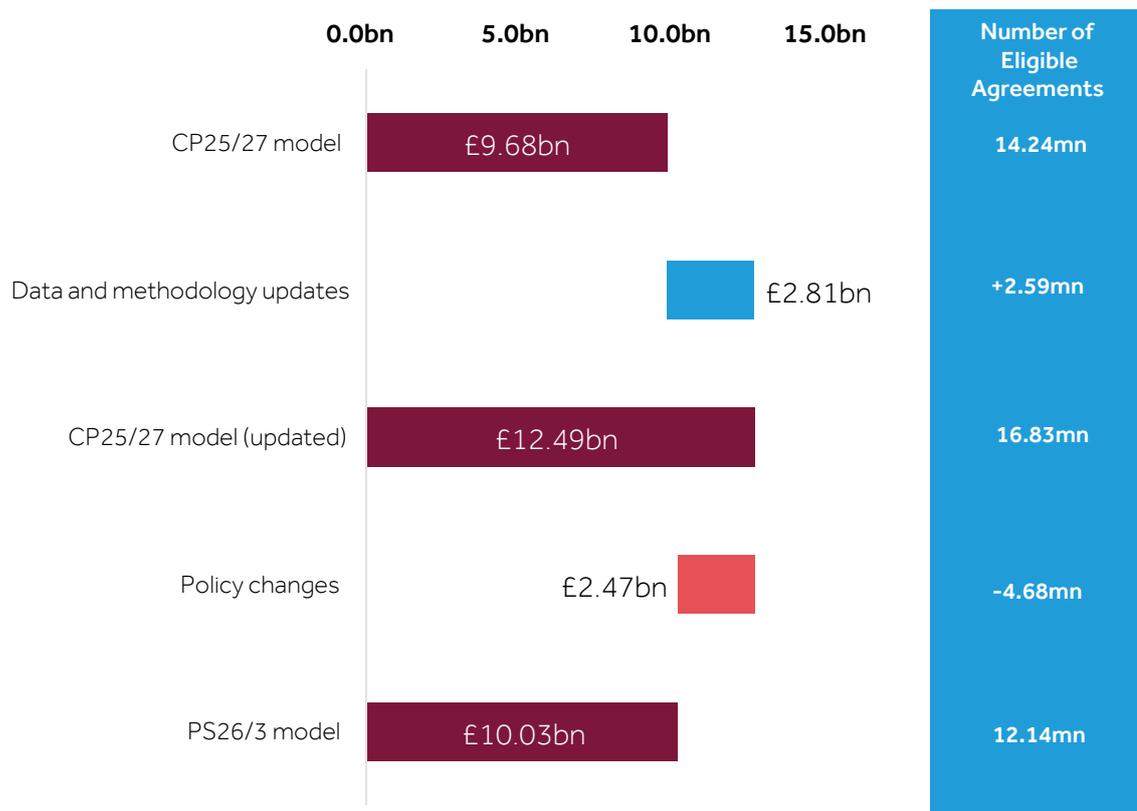
- 7.1** This annex sets out our final redress liability estimates associated with the updated policy decisions detailed in PS26/3 and the methodological updates made to our approach following the publication of CP25/27 Technical Annex 1.
- 7.2** These estimates largely follow the same approach detailed in CP25/27 Technical Annex 1. In this chapter we describe changes we have made to the methodology since the publication of CP25/27. We have included a summary of the features of the updated policy detailed in PS26/3 which are not captured in the modelling of redress liability estimates.
- 7.3** The definitive sources of the FCA's policy and requirements are the Policy Statement (PS26/3) and the accompanying rules. This chapter of the Technical Annex is intended to summarise and explain the analytical work supporting those decisions. While we have sought to ensure consistency between this Technical Annex and the final policy and rules, any differences in language or presentation are purely explanatory in nature and do not reflect substantive policy divergence. We are confident that the modelling presented here reflects the underlying policy intent as far as is reasonably possible, recognising the inherent data limitations and that proportionate and evidence-based assumptions have been applied throughout the model.
- 7.4** Our redress liability modelling is designed to estimate market-wide redress using the data available to us. It is not designed to give precise redress values for individual agreements or to set out a prescriptive approach for firms to calculate their redress liabilities. Firms may have access to more information relating to their agreements and will need to calculate redress for individual agreements based on the policy described in PS26/3 and the associated rules.
- 7.5** As with the analysis underpinning the estimates presented in CP25/27, there are several limitations to this analysis. Our estimates are necessarily indicative and subject to uncertainty (see 'Sensitivity analysis' and 'Limitations of analysis' sections below).
- 7.6** We present our analysis to estimate market-wide redress liabilities in 3 phases:
- a.** CP25/27 model as described in CP25/27, with the results and sensitivities presented in Annex 6 of CP25/27.
 - b.** CP25/27 model (updated) as described in CP25/27 updated to include additional data and to reflect the policy with an APR floor adjustment, early settlement and dynamic interest rates, which were set out in CP25/27 but not implemented in our model of redress liabilities.
 - c.** PS26/3 model that reflects the additional data and scheme design in PS26/3.

Figure 16: Model evolution from CP25/27 to PS26/3



7.7 The aggregated impacts of these changes on the model evolution are shown in Figure 17.

Figure 17: Impacts of model evolution from CP25/27 to PS26/3



Summary of estimates

7.8 In PS26/3, we state that redress will be assessed under two schemes. Scheme 1 applies to agreements made between 6 April 2007 and up to but not including 1 April 2014. Scheme 2 covers agreements from 1 April 2014 up to and including 1 November 2024⁵⁶.

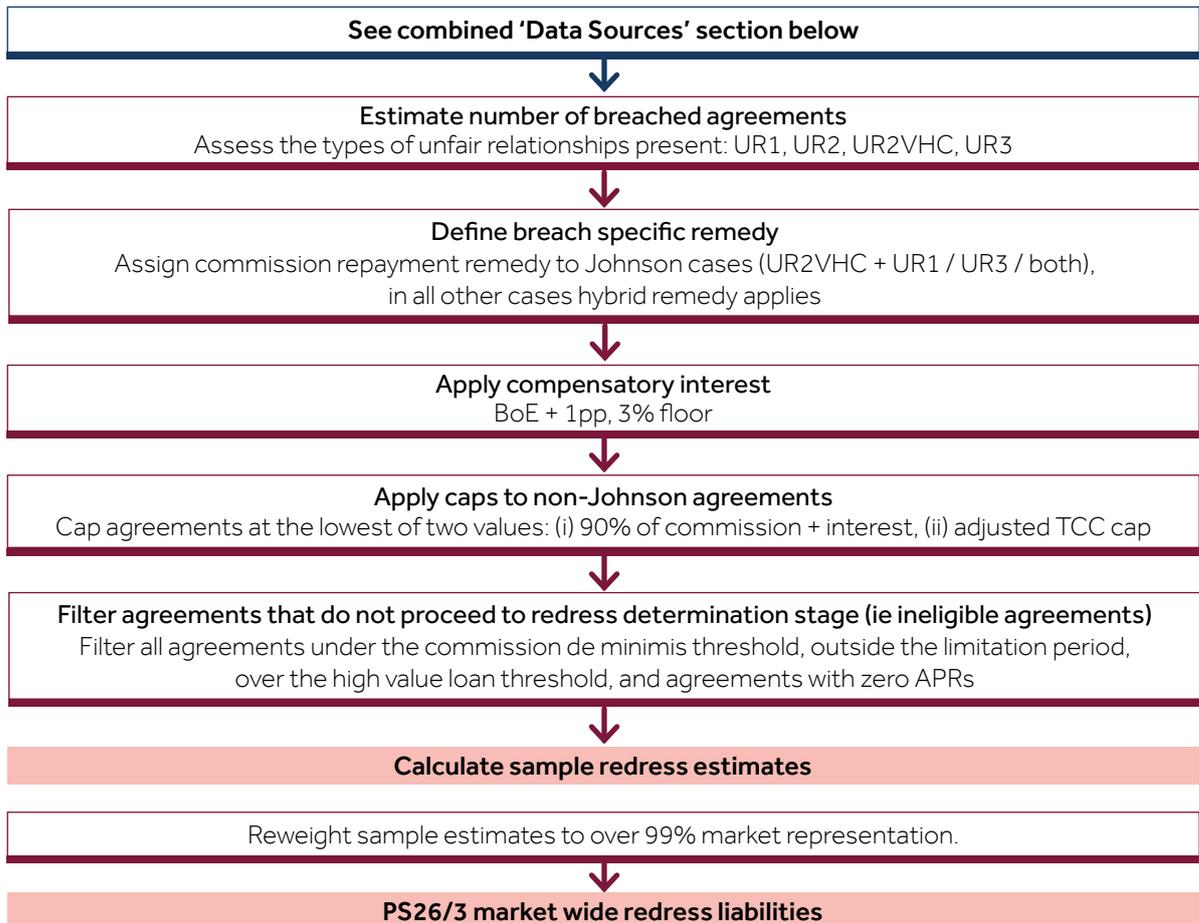
7.9 Within this, remedies are applied in accordance with a tiered framework, separated by:

- Cases that align closely with the facts in *Johnson* (UR2VHC + UR1/UR3/both).
- All other cases.

Methodology

7.10 In Figure 18 we have set out the process followed to generate redress liability estimates for PS26/3.

Figure 18: Steps in the estimation of the PS26/3 redress liabilities



Notes: This figure shows the steps in our modelling process and do not set out a prescriptive approach for firms to calculate their redress liabilities. The definitive sources of the policy and requirements are in PS26/3 and the accompanying rules. The adjusted total cost of credit (TCC) cap is the realised TCC, adjusted to account for a minimal cost of credit offered at the 5th percentile of the non-zero APRs in the year an agreement was signed.

⁵⁶ Our model uses DD1 as the primary dataset for agreement-level characteristics. DD1 does not include all agreements originated between 25 October 2024 and 1 November 2024, meaning approximately 40,000 agreements within that window are not captured. This represents a minor data misalignment and is not expected to materially affect the liability estimates. Additionally, the modelling includes agreements starting after 6 March 2007, meaning that we do not include agreements on the 6 March 2007. This represents a data misalignment that may lead to a minor understatement of the total redress liabilities.

7.11 First, we identified the type and combination of breaches of unfair relationships applicable to each agreement, as the remedy applied is partially dependent on the specific breaches identified. For the purposes of this technical annex, these categories are defined as follows:

- **Unfair Relationship 1 (UR1):** Cases with inadequate disclosure of a DCA. The conditions of this unfair relationship breach remain unchanged from CP25/27.
- **Unfair Relationship 2 (UR2):** Cases with inadequate disclosure of high commission. In PS26/3, we set high commission as equal to or greater than 39% of the total cost of credit (TCC) and 10% of the loan value. This is an increase from the threshold of 35% of TCC and 10% of loan value set out in CP25/27 Technical Annex 1.
- **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 TCC and 22.5% of the loan amount. This threshold remains unchanged from CP25/27.
- **Unfair Relationship 3 (UR3):** Cases with an inadequately disclosed contractual tie between the lender and broker.

Table 14: Summary of unfair relationship breaches (excluding caps and exclusions) and the parameters used to identify these in our analysis - CP25/27 vs PS26/3

Unfair relationship type	Description	Modelled UR criteria in CP25/27	Modelled UR criteria in PS26/3	Highlighted modelling change
UR1 – Inadequate disclosure of DCA	This captured agreements with inadequate disclosure of DCA.	UR1 captures cases where 3 criteria are satisfied simultaneously: <ul style="list-style-type: none"> • the agreement is identified as a DCA, • the agreement is commission bearing, and • the APR exceeds the minimum possible APR associated with that agreement. 	Same as CP25/27.	No change.

Unfair relationship type	Description	Modelled UR criteria in CP25/27	Modelled UR criteria in PS26/3	Highlighted modelling change
UR2 – High Commission	This captures cases with inadequate disclosure of high commission.	UR2 captures cases where the commission paid to the broker is: <ul style="list-style-type: none"> • $\geq 35\%$ of the Total Cost of Credit (TCC), and • $\geq 10\%$ of the loan amount 	High commission now captures cases where commission paid to the broker is $\geq 39\%$ of TCC and $\geq 10\%$ of loan value.	We have uplifted the threshold for total cost of credit from 35% to 39%.
UR2VHC – Very High Commission	This captures cases with inadequate disclosure of very high commission.	UR2VHC captures cases where the commission paid to the broker is: <ul style="list-style-type: none"> • $\geq 50\%$ of TCC, and • $\geq 22.5\%$ of loan amount. 	Same as CP25/27	No change.
UR3 – Tied Arrangement	This captures agreements with an inadequately disclosed contractual tie between a broker and a lender.	Randomly selected 14% of commission bearing agreements in our DD1 data and assigned them as a UR3 breach. We then excluded agreements made by 3 sub-prime lenders as they reported to us through the firm monitoring programme that they have never used these types of arrangements with brokers.	New UR3 data reported from firms with an adjustment made for captive and white label tied arrangements with franchised dealers that may now qualify for the exception from relevant arrangements and be deemed ineligible for redress.	We have used new data from firms to inform our UR3 estimates and then narrowed the scope for UR3 by removing franchised-dealer, captive and white label tied arrangements where they can evidence the relationship should have been clear to the consumer.

- 7.12** In accordance with the scheme rules, the remedy applied depends on the combination of unfair relationship breaches:
- Where UR2VHC and UR1 both present, or where UR2VHC and UR3 are both present, or where UR2VHC and UR1 and UR3 are all present, the remedy is commission plus interest (the *commission repayment* remedy).
 - In all other cases, a hybrid remedy applies.
- 7.13** The hybrid remedy here refers to the midpoint between the APR adjustment and the commission repayment remedy. In Scheme 1, the APR adjustment is characterised by an APR-21 adjustment. In Scheme 2, it is characterised by an APR-17 adjustment (see 'Remedy changes' section below for how we incorporate this change into our methodology).
- 7.14** In this chapter of the Technical Annex, we have used the terms 'eligible' and 'ineligible' as shorthand for agreements that progress to redress determination and those that do not. This shorthand only describes our estimates used in our redress liability model and does not set out the scheme rules. PS26/3 and the rules are the definitive sources of our policy and requirements.
- 7.15** Our redress liability modelling was designed to provide proxy estimates of market-wide total redress liabilities, average and median redress, and the number of agreements we treat as 'eligible' and 'ineligible', using the data available to us. In our modelling, an eligible agreement is one where we identify at least 1 UR from Table 14 and it is not removed by the filters applied in the model. These filters reflect policy changes and other eligibility treatments we apply in the modelling, including commission de minimis, limitation period cases, zero APR and high value loan thresholds.
- 7.16** Total redress liabilities are calculated as the sum of modelled redress amounts across eligible agreements included in the model outputs. Average redress is calculated using total redress liabilities divided by the number of eligible agreements with non-missing redress values. Eligible agreements that have their redress capped to £0 still contribute to the eligible agreement count and are included in the average calculations.
- 7.17** In Chapter 11 of PS26/3 we explain our decision to introduce a cap on the redress payable for non-Johnson agreements. In our modelling, the cap was determined as the lowest of two values: 90% of commission plus associated compensatory interest, and the realised TCC adjusted downward by an amount corresponding to the TCC recalculated using the non-zero 5th percentile APR in the year of loan origination.
- 7.18** To estimate the total liability across the market we re-weighted our sample estimates, so the final figures are representative of over 99% of the market. As set out in the [Data Guide in Technical Annex 1 of CP25/27](#), DD1 represents around 89% of the lending market. This is based upon number of outstanding loans in June 2024.

7.19 For the remainder of this chapter, when we refer to *sample* estimates, this reflects around 89% of the market, and *market-wide* estimates are representative of over 99% of the market⁵⁷.

Total liabilities (Scheme 1 and Scheme 2)

7.20 We present our key redress liability estimates from 2007 to 2024 for both schemes (Scheme 1 and Scheme 2) below. These figures reflect an estimate of the liability, assuming redress is claimed for all agreements that are eligible (100% uptake rate). Table 15 below reports estimated total liabilities under the hybrid remedy, and agreements which meet the criteria for UR2VHC & UR1 and UR2VHC & UR3 and UR2VHC & UR1 & UR3 receive the commission repayment remedy.

Table 15: Estimation market-wide results

Redress liabilities (£bn)	Mean redress per eligible agreement (£)	Median redress per eligible agreement (£)	Number of eligible agreements
£10.03bn	£829	£661	12.14m

Notes: These estimates are subject to the key assumptions set out in the 'Limitations of analysis' section of CP25/27 Technical Annex 1, the 'Limitations of analysis' section below and other caveats included in PS26/3. The redress liabilities are reweighted to over 99% of the market (market-wide estimates), but the mean and median redress per agreement are calculated from in-sample estimates (covering 89% of the market). This is due to the nature of how the out-of-sample estimates are calculated as an aggregate addition, not at the agreement level. Agreements receive the hybrid remedy, unless they meet the criteria for UR2VHC & UR1 and UR2VHC & UR3 and UR2VHC & UR1 & UR3 in which case they receive the commission repayment. Agreements receiving the hybrid remedy have the 'adjusted TCC' and '90% of commission plus compensatory interest' caps applied.

7.21 Table 16 shows the breakdown by unfair relationship and the associated redress amounts. In the table below we have provided summary statistics for the lenders within our sample redress liability estimates. For clarity, the rows in this table (and the associated tables for Scheme 1 and Scheme 2 below) describe the sets of agreements that have at least the corresponding unfair relationship type in the first column (but could have more than one). As such, there will be overlaps in the agreements that comprise the data summarised in each row.

⁵⁷ Market coverage based upon number of outstanding agreements in June 2024. Further detail on sampling and reweighting is set out in [Technical Annex 1 of CP25/27](#).

Table 16: Distribution of unfair relationship breaches, sample estimates

Unfair relationship type	Breach rate (%)	Redress liabilities (£bn)	Mean redress per eligible agreement (£)	Median redress per eligible agreement (£)
UR1	34.14%	£8.60bn	£810	£641
UR2	8.37%	£3.13bn	£1,203	£1,044
UR3	3.70%	£0.93bn	£807	£653
UR2(VHC) & UR3	0.03%	£24m	£3,119	£2,949
UR2(VHC) & UR1	0.28%	£248m	£2,900	£2721
UR2(VHC) & UR1 & UR3	0.02%	£20m	£3,160	£3,026

Notes: The redress liabilities attributed to UR1, UR2 and UR3 will not add up to the total liability in Table 15 as some agreements in Table 16 may have multiple breaches. Breach rate is defined as the total number of eligible agreements with at least the given UR (this could include agreements with multiple UR's) divided by the total number of agreements in the scheme period (around 31.1m). All estimates in this table cover 89% of the market (sample estimates), whereas the totals in Table 15 are reweighted to reflect market-wide estimates. UR2VHC & UR1 and UR2VHC & UR3 and UR2VHC & UR1 & UR3 are calculated using the commission repayment remedy where UR2VHC is equal to or greater than 50% of the total cost of credit and no less than 22.5% of the loan. Agreements receiving the hybrid remedy have the 'adjusted TCC' and '90% of commission plus compensatory interest' caps applied.

Scheme 1 (April 2007 to April 2014)

7.22 Scheme 1 covers agreements originated from 6 April 2007 until but not including 1 April 2014. Table 17 reports estimated total liabilities under the final policy position for Scheme 1, for the lenders within our sample data.

Table 17: Estimation results – Scheme 1 (pre-April 2014), sample estimates

Redress liabilities (£bn)	Mean redress per eligible agreement (£)	Median redress per eligible agreement (£)	Number of eligible agreements
£3.05bn	£734	£569	4.16m

Notes: Estimates in this table are based on in-sample estimates covering 89% of the market (sample estimates). Agreements receive the hybrid remedy, unless they meet the criteria for UR2VHC & UR1 and UR2VHC & UR3 and UR2VHC & UR1 & UR3 then they receive the commission repayment remedy. Agreements receiving the hybrid remedy have the 'adjusted TCC' and '90% of commission plus interest' caps applied.

7.23 Table 18 shows the breakdown by unfair relationship and the associated redress amounts for Scheme 1.

Table 18: Distribution of unfair relationship breaches – Scheme 1 (pre-April 2014), sample estimates

Unfair relationship type	Breach rate (%)	Redress liabilities (£bn)	Mean redress per eligible agreement (£)	Median redress per eligible agreement (£)
UR1	46.59%	£3.03bn	£735	£570
UR2	3.00%	£378m	£1,424	£1,204
UR3	2.48%	£155m	£706	£553
UR2(VHC) & UR3	0.01%	£5m	£3,813	£3,304
UR2(VHC) & UR1	0.21%	£59m	£3,923	£2,944
UR2(VHC) & UR1 & UR3	0.01%	£5m	£3,831	£3,305

Notes: The redress liabilities attributed to UR1, UR2 and UR3 will not add up to the total liability in Table 17 as some agreements in Table 18 may have multiple breaches. Estimates in this table cover 89% of the market (sample estimates). Breach rate is defined as the total number of eligible agreements with at least the given UR (this could include agreements with multiple UR's) divided by the total number of agreements in the scheme period (around 8.9m). UR2VHC & UR1 and UR2VHC & UR3 and UR2VHC & UR1 & UR3 are calculated using the commission repayment where UR2VHC is equal to or greater than 50% of the total cost of credit and no less than 22.5% of the loan. Agreements receiving the hybrid remedy have the 'adjusted TCC' and '90% of commission plus compensatory interest' caps applied.

Scheme 2 (April 2014 to November 2024)

7.24 Scheme 2 covers agreements entered into from 1 April 2014 until and including 1 November 2024⁵⁸. Table 19 reports estimated total liabilities under the final policy position for Scheme 2, for the lenders within our sample.

Table 19: Estimation results – Scheme 2 (2014-2024), sample estimates

Redress liabilities (£bn)	Mean redress per eligible agreement (£)	Median redress per eligible agreement (£)	Number of eligible agreements
£6.84bn	£881	£717	7.77m

Notes: Estimates in this table are based on in-sample estimates covering 89% of the market (sample estimates). Agreements receive the hybrid remedy, unless they meet the criteria for UR2VHC & UR1 and UR2VHC & UR3 and UR2VHC & UR1 & UR3 then they receive the commission repayment. Agreements receiving the hybrid remedy have the 'adjusted TCC' and '90% of commission plus interest' caps applied.

⁵⁸ Our model uses DD1 as the primary dataset for agreement-level characteristics. DD1 does not include all agreements originated between 25 October 2024 and 1 November 2024, meaning approximately 40,000 agreements within that window are not captured. This represents a minor data misalignment and is not expected to materially affect the liability estimates.

7.25 Table 20 shows the breakdown by unfair relationship and the associated redress amounts for Scheme 2.

Table 20: Distribution of unfair relationship breaches – Scheme 2 (2014-2024), sample estimates

Unfair relationship type	Breach rate (%)	Redress liabilities (£bn)	Mean redress per eligible agreement (£)	Median redress per eligible agreement (£)
UR1	29.20%	£5.57bn	£858	£692
UR2	10.51%	£2.75bn	£1,178	£1,029
UR3	4.18%	£773m	£831	£680
UR2(VHC) & UR3	0.03%	£19m	£2,993	£2,912
UR2(VHC) & UR1	0.31%	£189m	£2,798	£2,667
UR2(VHC) & UR1 & UR3	0.02%	£16m	£3,026	£3,004

Notes: The redress liabilities attributed to UR1, UR2 and UR3 will not add up to the total liability in Table 19 as some agreements in Table 20 may have multiple breaches. Estimates in this tables are in-sample estimates covering 89% of the market (sample estimates). Breach rate is defined as the total number of eligible agreements with at least the given UR (this could include agreements with multiple UR's) divided by the total number of agreements in the scheme period (around 22.2m). UR2VHC & UR1 and UR2VHC & UR3 are calculated using the commission repayment where UR2VHC is equal to or greater than 50% of the total cost of credit and no less than 22.5% of the loan. Agreements receiving the hybrid remedy have the 'adjusted TCC' and '90% of commission plus compensatory interest' caps applied.

7.26 The remainder of this section is as follows:

- **Data sources:** listing the data sources relied upon for this analysis.
- **Data updates:** summarising our updates to redress liability estimates related to data.
- **Methodology updates:** improvements to our CP25/27 methodology leading to our CP25/27 model (updated).
- **Policy updates:** incorporating policy changes since CP25/27 into our final redress liability estimations leading to our PS26/3 model.
- **Sensitivity analysis:** updated sensitivity analysis based on our PS26/3 model that illustrates the sensitivity of redress liabilities to key parameters.
- **Limitations of analysis:** a summary of caveats, data limitations and features of PS26/3 not reflected in our final redress estimates.

Data sources

7.27 We have relied on multiple sources of data to generate our estimates. These include:

- DD1
- s166 review and DD2 combined sample data
- Round 6 motor finance commission monitoring survey

- DD6
- Expanded motor finance commission monitoring survey
- FCA analysis that may involve other datasets (eg Analysis of loss)
- Bank of England February 2026 Monetary Policy Report

7.28 For details, see our 'Data guide update' (Chapter 3) above and the [Data Guide in CP25/27 Technical Annex 1](#).

7.29 The next two sections describe changes to our original CP25/27 model estimates to include additional data and reflect policy on the APR floor adjustment, early settlement and dynamic interest rates (methodology updates), which were set out in CP25/27 but not implemented in the model of redress liabilities. They show how we have reached the CP25/27 model (updated).

Data updates to redress liability estimates in CP25/27

7.30 This section summarises the updates to our redress liability estimates related to data improvements following the release of CP25/27. These changes correspond to the CP25/27 model (updated), where the CP25/27 model was refreshed to incorporate newly available or corrected firm-level information from:

- **Updated firm-level DD1 data:** we updated the firm-level DD1 dataset to incorporate new or corrected submissions from several firms.
- **Firm-level UR3 data:** we collected additional data from firms related to UR3 post-CP25/27. This allowed us to update our approach to estimation. We provide details of this in the 'Methodological updates to redress liabilities in CP25/27' section below.

7.31 Together, these updates allowed us to calculate more robust breach rates and produce improved redress liability estimates.

7.32 Table 21 shows the impact of the updated firm-level DD1 data from what was published in CP25/27. The firm-level UR3 estimates represent a data and methodology update, but we have presented them alongside the methodology update because this methodological refinement was only possible once we obtained new data from firms. See the 'Firm-specific UR3 estimates' section below.

Table 21: Market-wide impact of data updates on redress liability estimates in CP25/27

Data updates	Redress liabilities (£bn)	Difference (£bn)	Eligible agreements	Difference
CP25/27 model	£9.68bn		14.24m	
Updated firm-level DD1 data	£10.44bn	£0.75bn	15.35m	1.11m
CP25/27 with data updates	£10.44bn		15.35m	

Notes: Values are rounded to 2 decimal places, which may result in the estimated liabilities or eligible agreements not summing exactly due to rounding differences. The redress liabilities figures in this table are quoted at the market-wide level.

Updated firm-level DD1 data

- 7.33** In CP25/27, our redress liability estimates used the dataset known as DD1 which is made up of high-level agreement data from 34 firms within the UK motor finance market (circa 89% market coverage)⁵⁹. Full details of this data can be found in the [Data Guide in CP25/27 Technical Annex 1](#).
- 7.34** During our analysis, we identified data discrepancies among a small number of firms submissions and received new data from some firms. We have included this new data in our estimates. For more information see Chapter 4 of the [Updated Diagnostic Report](#) and the 'Data guide update' (Chapter 3) in this Technical Annex.
- 7.35** The inclusion of the new or resubmitted data increased our liability estimates by £0.75bn and results in an additional 1.11m eligible agreements.

Methodological updates to redress liabilities in CP25/27

- 7.36** This section summarises the methodology refinements introduced in the CP25/27 model (updated), following the data updates described above. When we published CP25/27, there were some simplifying assumptions or limitations in the modelling approach to estimating the redress liabilities as was explained in CP25/27 paragraphs 8.70, 8.51 and 1.29. These included:
- **APR adjustment floor:** our estimates in CP25/27 did not include the impact of flooring overall redress with the APR adjustment (APR-17) (see [CP25/27 paragraph 8.51](#)). Whilst this change is included within the updated CP25/27 redress estimate shown in Table 22 below, the APR floor adjustment is not carried forward into the final policy position in PS26/3.
 - **Compensatory interest:** our estimates in CP25/27 applied compensatory interest to all overpayments in the APR adjustment, and the commission repayment remedy to the estimated redress payment date (31 December 2026) using a constant weighted average compensatory interest rate of 2.09% p.a. The policy described in CP25/27, however, required compensatory interest to be applied to all overpayments and remedies at the annual average Bank of England base rate per year plus 1pp rounded up to the nearest 0.25pp from the date of overpayment to the date redress is paid (see [CP25/27 paragraph 1.29](#)).
 - **Early settlements:** our estimates in CP25/27 did not include the full impact of early settlement if it occurred (see [CP25/27 Chapter 8](#)).
 - **Firm-specific UR3 estimates:** we updated our approach to estimating the prevalence of inadequately disclosed tied arrangements (UR3) by drawing on additional firm-level data gathered after CP25/27.
- 7.37** Table 22 illustrates the cumulative impact of these changes on the total estimated redress liabilities. The CP25/27 model is used as the baseline for comparison, following the data updates described above.

59 Based upon number of outstanding agreements in June 2024.

Table 22: Market-wide impact of methodology updates on redress liability estimates in CP25/27

Methodology updates	Redress liabilities (£bn)	Difference (£bn)	Eligible agreements	Difference
CP25/27 with data updates	£10.44bn		15.35m	
APR floor adjustment	£10.82bn	£0.39bn	15.35m	0.00m
Dynamic compensatory interest	£11.73bn	£0.91bn	15.35m	0.00m
Early settlement	£12.08bn	£0.34bn	15.35m	0.00m
Firm-specific UR3 estimates	£12.49bn	£0.42bn	16.8m	1.48m
CP25/27 model (updated)	£12.49bn		16.83m	

Notes: Values are rounded to 2 decimal places, which may result in the estimated liabilities or eligible agreements not summing exactly due to rounding differences. The firm-specific UR3 estimates represent a data and methodology update, but we have presented them alongside the methodological update because this methodological refinement was only possible once we obtained new data from firms. The impacts presented are influenced by the sequencing of changes in the model, meaning they represent cumulative rather than isolated effects. The redress liabilities figures in this table are quoted at the market-wide level.

APR floor adjustment

7.38 Our estimates in CP25/27 did not include the effect of flooring overall redress at the APR adjustment (APR-17) (see CP25/27 paragraph 8.51). We included this change in the CP25/27 model (updated) redress estimate in Table 22 to accurately reflect the policy position in CP25/27, but we did not carry the APR floor adjustment into the final policy position in PS26/3. This methodological update increases estimated total redress liabilities by £0.39bn. This impact is estimated after the DD1 data update was applied to the CP25/27 model.

Compensatory interest

7.39 In paragraph 8.74 of CP25/27, we proposed 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 annual average Bank of England base rate for that year plus 1 percentage point (pp), rounded to the nearest 0.25pp.

7.40 In paragraph 3.38 of CP25/27 Technical Annex 1 we explained that we had calculated the interest rate for each year as the base rate plus 1pp rounded to the nearest 0.25pp and used the number of eligible agreements written each year to give a single weighted interest rate for the whole scheme. This calculation resulted in a constant 2.09% p.a. simple interest rate applied to all agreements for modelling purposes. It also did not adjust for higher interest rates which would apply after 1 November 2024 to the estimated average payment date for redress.

7.41 This simplified approach overestimated rates during low-rate periods, such as 2009 to 2021, and underestimated rates during high-rate periods. This weighting methodology did not account for the cumulative impact of eligible agreements which have run to term but would continue to accrue interest on associated redress in later years at higher rates.

- 7.42** We have since updated our approach to calculating compensatory interest to align with the approach proposed in CP25/27. Accordingly, for each year covered by the scheme, the applicable compensatory interest rate is the annual average Bank of England base rate plus 1pp.
- 7.43** For future years where full-year data is not available (for example 2026), we used the most recent available projections from the Bank of England's February 2026 Monetary Policy Report⁶⁰. These imply a base rate of around 3.3% by Q4 2026⁶¹ and therefore a compensatory interest rate of approximately 4.3%. Our model assumes an average payment date for redress of 31 December 2026 and therefore does not use projections beyond this point, even though the rules in PS26/3 include projections for 2027 and 2028 (see Chapter 11 of PS26/3)⁶².
- 7.44** Instead of using a single weighted average interest rate across the entire population of agreements (as done in paragraph 3.38 of CP25/27 Technical Annex 1), we have calculated the compensatory interest year-by-year. The first step is to calculate the annual compensatory interest rate for each year covered by the scheme, as detailed in paragraph 8.74 of the CP25/27. To do this we take the annual average of the daily Bank of England base rate for that year and add 1pp. As part of this move from a single weighted rate to a year-by-year calculation, we also implemented the final policy change (future details in the later sections) to remove the rounding-up of the Bank of England base rate plus 1pp. In our modelling, the removal of rounding was not implemented as a separate policy change step because it would have been inefficient to build and test a rounding mechanism that would immediately be removed to reflect the final policy position.
- 7.45** Given that the hybrid remedy is derived from both the APR adjustment (which comprises a schedule of loan payments) and the commission repayment remedy (which comprises a single payment of commission), the application of compensatory interest in each case is slightly different.
- 7.46** For the APR adjustment, compensatory interest for an agreement is calculated for each individual monthly overcharge made by the consumer throughout the duration of their loan. This duration is the time the loan was active until early settlement if it occurred or the end of the loan if not. The calculation has two components: within-year interest, which accrues from the payment date to the end of that calendar year, and subsequent full-year interest, which accrues for each complete year thereafter until the estimated redress payment date. The total compensatory interest for an agreement in our redress liability model is the sum of these two components across all payment years of the loan term, such that:

60 Monetary Policy Report – February 2026 | Bank of England. These projections of the base rate from the Bank of England are based exclusively on forward rates derived from Overnight Index Swap (OIS) rates.

61 Recent market developments, including the geopolitical conflicts in the Middle East, may lead to an upward shift in market-implied interest-rate expectations for 2026.

62 Chapter 11 of PS26/3 says that, from 2026 onwards, the compensatory interest rate is calculated using the average of the quarterly Bank of England base rate projections for each year. PS26/3 sets the compensatory interest rate at 4.49% for 2026, which is based on the average of the actual Q1 Bank of England base rate and the market-implied path for Q2 to Q4 in 2026, plus the required 1pp uplift. For future years (2027 and 2028), it is simply the average of the quarterly projections in that year, plus the 1 pp. In contrast, our redress liability modelling applies the Q4 2026 projection to 2026 in its entirety and is intended to reflect liabilities across firms as of 31 December 2026. Our modelling therefore applies a compensatory interest rate of 4.3% for 2026 and does not include compensatory interest rates for 2027 and 2028. We have conducted sensitivity analysis on the impacts of different compensatory interest rates (see the 'Sensitivity analysis' section).

$$I_{total} = \sum_{i=1}^T (I_{i,within} + I_{i,subsequent})$$

Where:

- i is the payment year
- T is the final payment year of the loan term

7.47 Within-year interest (partial-year accrual): for any year i , each monthly repayment accrues interest only for the remaining days in that calendar year after the payment date. Summing across all repayments in that year⁶³:

$$I_{i,within} = \frac{R_i \cdot C}{365} \sum_{k=1}^{|P_i|} \Delta_k$$

Where:

- R_i is the annual compensatory interest rate for year i
- C is the constant monthly overcharge amount
- $|P_i|$ represents the total number of payments made in year i
- Δ_k is the number of days remaining in year i after payment k is made

7.48 In each subsequent full year, interest accrues for the full year at the relevant annual compensatory interest rate. Summing across all years j after the payment year i up to the estimated redress payment date (up to and including year $r = 2026$). This applies to overcharges from the first payment year, intermediate years, and the final payment year identically. For agreements spanning multiple payment years, the accumulated cross-year interest is captured by summing this expression across all payment years i .

$$I_{i,subsequent} = C \cdot |P_i| \cdot \sum_{j=i+1}^r R_j$$

Where:

- j is the calendar year of subsequent full-year interest accrual
- r is the final year when redress is assumed to be paid. We assume in our redress liability model that redress payments occur on the last day of 2026
- R_j is the annual compensatory interest rate for year j

7.49 Total compensatory interest for a given agreement is therefore:

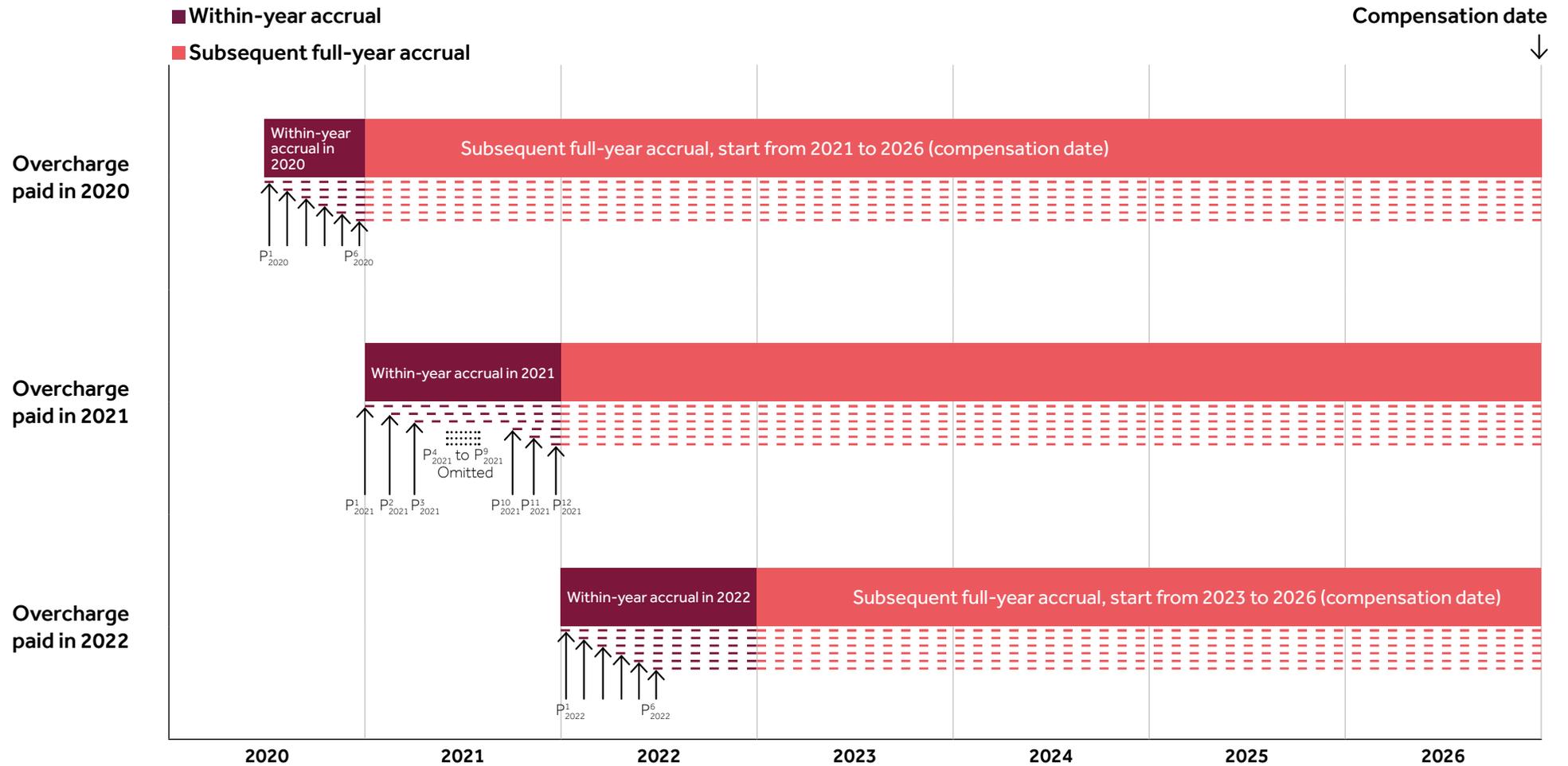
$$I_{total} = \sum_{i=1}^T \left(\underbrace{\frac{R_i \cdot C}{365} \sum_{k=1}^{|P_i|} \Delta_k}_{\text{within year}} + \underbrace{C \cdot |P_i| \cdot \sum_{j=i+1}^r R_j}_{\text{subsequent full years}} \right)$$

Where:

- T is the final payment year of the loan term

⁶³ We note that 365 was used to represent the number of days in a year. Our modelling does not include the impact of leap years where this number would be 366.

Figure 19: Illustrative breakdown of compensatory interest for the APR adjustment by payment year and component



Notes: In 2021, the 4th to 9th payments are omitted for presentation clarity.

7.50 For the commission repayment remedy, we apply compensatory interest to the commission amount from the beginning of the agreement through to anticipated redress payment date. We use the same adjusted Bank of England base rate plus 1pp as the simple interest rate such that the compensatory interest varies accordingly in each year between agreement start and anticipated redress payment.

7.51 Broadly speaking, our handling of compensatory interest for the commission repayment remedy can be considered a simplified case of the APR adjustment case described above. The simplification is that only a single payment (the commission itself) is considered:

$$I_{commission} = \frac{R_s \cdot Commission}{365} \cdot \delta_s + Commission \cdot \sum_{j=s+1}^r R_j$$

within year
subsequent full years

Where:

- *Commission* is the commission amount for the agreement
- *s* is the year the agreement was signed (start year)
- *R_s* is the annual compensatory interest rate for year *s*
- *δ_s* is the number of days remaining in year *s* after the agreement start date
- *j* is the calendar year of subsequent full-year interest accrual
- *r* is the final year when redress is assumed to be paid. We assume in our redress liability model that redress payments occur on the last day of 2026.
- *R_j* is the annual compensatory interest rate for year *j*

7.52 For clarity, our modelling implementation of the above approach makes some assumptions that include:

- The numeric day of the month that the agreement starts is the same as the day of the month that the agreement ends.
- The payments are made throughout the loan term on the same day of the month.

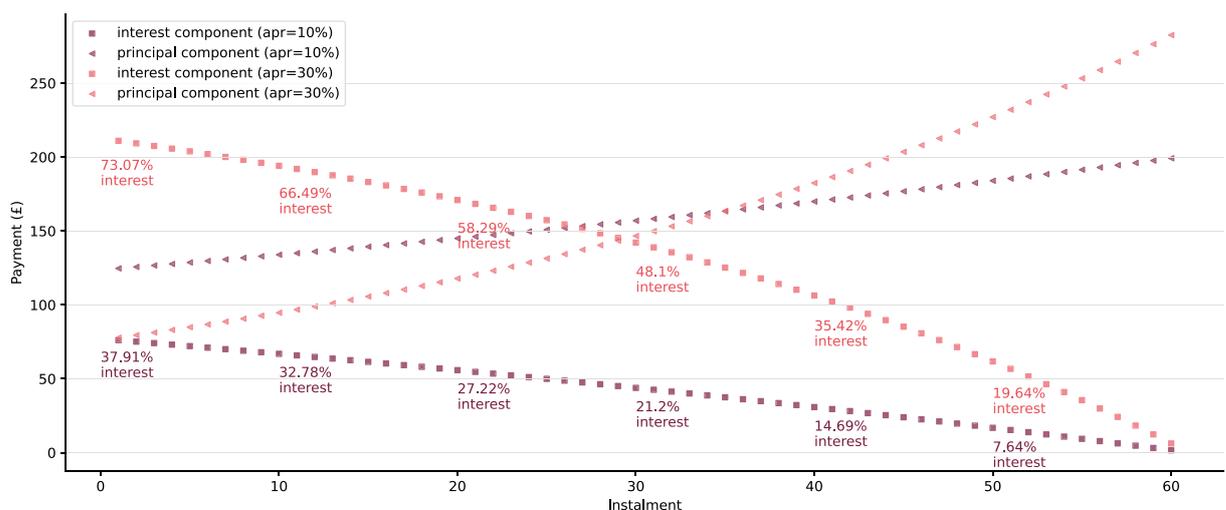
7.53 This methodological update increased our estimated total redress liabilities by £0.91bn. This impact is estimated after the DD1 data update and APR floor adjustment was applied to the CP25/27 model.

Early settlement

7.54 In the CP25/27, we did not account for early settlement in our calculations of redress. We explain below how we have updated our approach to handle instances of redress associated with agreements that settle early. For simplicity and analytical scalability, we assume that all instances of early settlement comprise full early settlement and not partial.

- 7.55** As we detail in paragraph 3.39 of CP25/27 Technical Annex 1, in the absence of early settlement consideration, the estimated APR adjustment redress reflects the difference between the factual total monthly payment (as calculated using a financial formula) and the counterfactual total monthly payment (under a lower APR), multiplied by the number of months the agreement is active, plus compensatory interest. However, this did not fully capture the total redress associated with an agreement which is settled early as it does not consider the early settlement payment itself.
- 7.56** A partially amortising loan, such as a motor finance agreement, is one where the consumer typically pays a consistent, regular monthly payment. This monthly payment is used by the lender to pay down the remaining principal and interest on the loan.
- 7.57** The APR for a given agreement varies depending on the precise structure and timing of drawdowns and repayments in the agreement. Contrasting two otherwise identical loans; those which contain a higher interest charge in the monthly payments will have a higher APR. In the examples below, we consider two loans identical in all aspects except for the APR, where one is arbitrarily 'low' and the other arbitrarily 'high'.
- 7.58** As shown in Figure 20, the monthly repayments on the higher APR loan pay more toward loan interest in each month compared to the lower APR loan. The complement of this is that at a given point within the loan schedule, a lower APR loan will have paid back more of the principal than in the higher APR loan instance (again, with all other loan parameters held constant).
- 7.59** If these loans run to term, both loans will pay back the same amount of principal. As illustrated in the graph below, the higher APR loan pays a larger monthly proportion towards the interest repayment than the same loan with a lower APR.

Figure 20: Illustrative amortised loan repayment schedule



Notes: Not an actual agreement. For illustrative purposes only.

- 7.60** For agreements which settle early, the number of months the agreement is active is less than the full term of the agreement. At the point of early settlement, the consumer is required to pay back the remainder of the principal (the early settlement value), but not the remaining interest which is known as the rebate (they may be subject to early repayment charges however).
- 7.61** This impacts our redress calculation as in the counterfactual scenario, where a consumer paid a lower APR on their loan, the early settlement value comprising the remaining principal would be commensurately lower.
- 7.62** In our updated modelling, we calculate this difference between factual and counterfactual early settlement value, apply compensatory interest applied to it, and add the resulting sum to the APR counterfactual remedy total. This is done on an agreement-by-agreement basis. The compensatory interest approach is the same as in the method applied in the PS26/3 model.
- 7.63** For clarity, our implementation of the above approach makes some assumptions that include: all instances of early settlement are full early settlement, not partial. We assume there are no arrears in any agreement, as we do not have this information. We do not account for additional fees associated with early settlement such as admin fees, or payment holidays etc. APR is an appropriate proxy for the interest rate.
- 7.64** Implementing this change within our methodology resulted in a £0.34bn increase in the estimated total redress liabilities. This impact is estimated after the addition of the DD1 data update, APR floor adjustment, and the dynamic compensatory interest rate update.

Firm-specific UR3 estimates

- 7.65** In paragraphs 3.30 to 3.32 of CP25/27 Technical Annex 1, we set out our approach to estimating the prevalence of inadequately disclosed tied arrangements (UR3). We defined UR3 as cases involving an inadequately disclosed contractual tie between the lender and broker, including undisclosed ROFR.
- 7.66** Due to lack of firm-specific data, our CP25/27 estimate of UR3 redress liabilities applied a 14% breach rate across all agreements, where commission was more than £0. This rate was derived from the manual case-file review, as detailed in the [Diagnostic Report](#)⁶⁴. This percentage was applied uniformly across all commission bearing agreements, with the exception of 3 sub-prime firms that had reported through the firm monitoring programme that they had never used these types of arrangements with brokers.
- 7.67** Following publication of CP25/27, we gathered additional data to refine our estimates of UR3 prevalence at a firm level. This included:
- **Round 6 motor finance commission monitoring survey:** In our most recent round of firm monitoring (Round 6), we asked a sample of lenders and brokers to provide estimates of the total number of agreements with inadequately disclosed tied

64 CP25/27 Diagnostic Report paragraphs 5.82-5.91.

arrangements, and the estimated value of the related redress liabilities. A number of DD1 firms reported that they had never used tied arrangements.

- **DD6 data request (captive and white label firms):** We requested data from 15 captive firms and 4 white label firms covering the period from 6 April 2007 to 1 November 2024. This data request asked firms to provide the number of tied arrangements split by year, DCA/non-DCA, new/used vehicles, and franchised/non-franchised/unknown dealer status. Full details of this data are set out in Chapter 7 of the Updated Diagnostic Report.
- **Expanded motor finance commission monitoring survey:** For the smaller firms that were not included in the main monitoring survey, we asked them a set of higher level but similar questions to those in the Round 6 survey. This included a question on the number of agreements which relate to inadequately disclosed tied arrangements.

7.68 For a comprehensive review of these data sources, please refer to the 'Data guide update' in Chapter 3 in this Technical Annex.

7.69 Using the additional data acquired post-CP25/27 we now calculate firm specific breach rates using the following formula.

$$\text{Firm specific breach rate (\%)} = \frac{\text{Firm level UR3 agreement count}}{\text{Firm level total agreements}}$$

7.70 Firm level UR3 agreement counts rely upon firms' self-reported figures. We applied a hierarchy to the data sources by which data set selection is prioritised in the order of the bullet points above. Where data is not available in the preferred data set, only then do we select from the next available option.

7.71 We then assigned UR3 breaches, in line with the firm-specific breach rate, randomly across all agreements where commission is greater than £0 for each firm.

7.72 We note that this was not the final approach applied. The 'Excepting captive and white label lending, through a franchised dealer, from being an eligible arrangement under UR3' section below details the model updates which are related to the policy changes presented in PS26/3.

7.73 As this data is reported by firms, this analysis relies on the assumption that firms have accurately reported the total number of agreements which breach UR3. We are aware that there are likely data quality issues associated with this information as explained in more detail in Chapter 7 of the Updated Diagnostic Report.

7.74 The revised firm-specific breach rates also show that the original 14% assumption for UR3 did not hold uniformly across firms⁶⁵. For some firms, the 14% estimate overstated the likely prevalence of inadequately disclosed tied arrangements, while for others, particularly captive and white-label firms, the 14% assumption was too low. As a result, the effects of applying firm-specific breach rates varied significantly across firms.

65 In CP25/27 we did not apply the original 14% assumption to agreements at the firm-level but rather randomly selected 14% of commission bearing agreements in our DD1 data and assigned them as a breach. We then excluded agreements made by 3 sub-prime lenders as they reported to us through the firm monitoring programme that they have never used these types of arrangements with brokers.

7.75 The impact of incorporating this additional data on the total estimated redress liability was an estimated increase of £0.42bn and resulted in a increase in the number of eligible agreements by 1.48m. This impact is estimated after the addition of the DD1 data update, APR floor adjustment, the dynamic compensatory interest rate update and early settlement.

Redress liability updates following final policy positions

7.76 We received a range of feedback on CP25/27. Having considered this feedback and determined our approach to the final policy, we have made several updates to how the corresponding redress liability estimates are modelled. These policy changes are organised into three categories: **eligibility changes, remedy changes, and caps on redress.**

7.77 Below we have presented the impact of each policy change relative to the final policy position. To assess the impact of each policy change relative to the final PS26/3 configuration, we compared the final model output (PS26/3 model) with a series of additional model runs, which iteratively exclude each policy change while leaving all others unchanged. This approach enabled us to determine the contribution of each individual policy by observing how the model output differs when that specific policy is removed. For example, when the commission de minimis is removed and all other policy changes remain constant, the total redress liability estimate increases by £0.14bn.

Table 23: Impact of removing individual policy changes on the PS26/3 model, market-wide redress liabilities (£bn) and number of eligible agreements

	Redress liabilities (£bn)	Difference in redress liabilities (£bn)	Eligible agreements	Difference in eligible agreements
PS26/3	£10.03bn		12.14m	
Commission de minimis	£10.17bn	£0.14bn	13.67m	1.52m
Other eligible exclusions	£10.31bn	£0.28bn	12.28m	0.14m
Excluding captives and white labels from UR3	£10.79bn	£0.76bn	14.04m	1.90m
Changes to high commission thresholds	£10.27bn	£0.24bn	12.33m	0.18m
APR-17 for Scheme 2, APR-21 for Scheme 1	£9.90bn	-£0.13bn	12.14m	0.00m
Johnson remedy changed (UR2VHC + UR1 / UR3 / both)	£9.94bn	-£0.09bn	12.14m	0.00m
Compensatory interest floor of 3%	£9.41bn	-£0.62bn	12.14m	0.00m
Adjusted TCC cap and 90% C+I cap applied	£10.94bn	£0.91bn	12.14m	0.00m

Notes: Values are rounded to 2 decimal places, which may result in the estimated liabilities or eligible agreements not summing exactly due to rounding differences. Other eligible exclusions include agreements that are ineligible because of the limitation period, agreements lasting last than a month and agreements over the high value loan threshold. We do not show the impact of the zero APR filter. Most of these agreements are already removed when we estimate the number of breached agreements and apply the other filters. Adding a zero APR filter, on top of the other changes, removed about 20,000 agreements and has no impact on redress liabilities.

Eligibility changes

7.78 We have made several policy decisions which create exclusion to what is an eligible and ineligible agreement. These changes affect both the set of eligible agreements and how we calculate redress liabilities.

Commission de minimis

7.79 In Chapter 5 of PS26/3 we set out that we have decided to introduce a commission de minimis threshold of £120 for Scheme 1 cases and £150 for Scheme 2 cases. We have decided that at these levels commission is not, on its own, capable of making the relationship unfair. These de minimis thresholds are set on the pre-compensatory interest commission amount.

7.80 To reflect this in the model, we filtered agreements at or below the commission de minimis threshold of the scheme relevant to the agreement. These agreements are not considered 'eligible' for our modelling purposes and therefore receive no redress.

7.81 We have estimated that the total impact of removing the commission de minimis, with all other final policy changes in the PS26/3 model held constant, is an increase of £0.14bn in the estimated redress liabilities, which is associated with a 1.52m increase in the number of eligible agreements.

Other eligible exclusions

7.82 Beyond the commission de minimis, there is also an exclusion for high value loans. We therefore removed cases where the limitation period has expired and where the agreement has lasted less than a month as a proxy for agreements that were cancelled during the initial cooling off period.

Limitation period

7.83 In Chapter 4 of PS26/3 we explain that we can only include in the scope of the schemes, cases where the limitation period for bringing a legal claim has not expired before the scheme rules are made. While it is not possible to predict how many cases would be considered out of time for the scheme, we have assumed for the purpose of modelling redress liabilities estimates that among all cases with an agreement ending more than 6 years before the scheme rules are made that:

- All cases involving only a high commission arrangement (with no DCA, no tied arrangement) would be assessed as out of time for the scheme (on the presumption that there is clear and prominent disclosure of the fact or possibility of commission to the consumer in each case, such that the 6-year statutory limitation period was not extended). This is in line with our guidance, but note is likely to be an overestimate of the number of cases given prominent disclosure of the fact of commission is required; and
- All cases involving a DCA or a tied arrangement would be in time for the scheme.

To reflect this in the model, we have excluded all agreements that involve only a high-commission breach (UR2) where the agreement ended more than 6 years before March 2026. In reality, the proportion of agreements considered out with the limitation period is likely to be less than 100% and therefore our estimated redress liability for some agreements may be understated. Consumers whose agreements are time-barred from the scheme retain their usual rights to refer their complaint to the Financial Ombudsman Service or take legal action through the courts.

High value loans

7.84 In Chapter 4 of PS26/3, we state that agreements with loan values exceeding the 99.5th percentile of loan values for each scheme year, rounded to the nearest £1,000, would be excluded of the scheme. For 2007 and for agreements entered before 6 April 2008, any loan above £25,000 is excluded, as these agreements were outside the scope of the Consumer Credit Act (CCA) and therefore outside the scheme.

7.85 To capture this in the model, we computed from DD1 the 99.5th percentile loan value threshold for each scheme year, rounded to the nearest £1,000, except that for 2007 and for pre-6-April-2008 agreements in 2008, the applicable threshold is capped at £25,000 in line with the CCA coverage limit, as set out in the table below. Agreements exceeding the relevant annual threshold are removed from the eligible population.

Table 24: Annual loan value exclusion thresholds

Year	Threshold (99.5th percentile, rounded to nearest £1,000)	Year	Threshold (99.5th percentile, rounded to nearest £1,000)
2007	£25,000 (CCA limit)	2016	£61,000
2008 (pre-6 April)	£25,000 (CCA limit)	2017	£65,000
2008 (post-6 April)	£38,000	2018	£68,000
2009	£39,000	2019	£70,000
2010	£43,000	2020	£73,000
2011	£45,000	2021	£75,000
2012	£47,000	2022	£80,000
2013	£51,000	2023	£82,000
2014	£56,000	2024	£82,000
2015	£60,000		

7.86 The impact of this on the redress liability estimates is that, by construction, this exclusion removes the top 0.5% of agreements in each scheme year from the modelled population.

7.87 Consumers with agreements where the loan exceeds the threshold are not precluded from seeking redress; they retain the usual ability to pursue cases directly with their lender and refer matters to Financial Ombudsman or the courts outside the scheme framework. We note that these thresholds are derived from DD1, which does not capture all firms operating in the high value vehicle segment. However, DD1 includes major manufacturer-affiliated finance providers whose groups encompass a range of luxury brands, and we consider the resulting thresholds to be a reasonable and practicable proxy for identifying high value agreements.

Agreements lasting less than a month

7.88 In Chapter 4 of PS26/3, we discuss very early settlement and cancelled/unwound agreements. We have decided to exclude agreements that were cancelled or fully unwound during the initial cooling off period from the scheme. Agreements that were settled early remain scheme cases, but the TCC caps will typically limit redress to a small amount. For modelling purposes, we proxied this policy change by excluding agreements with a term of less than 1 month. This does not match the policy implemented by the rules, but we considered it a proportionate assumption that captures the effect of the cancellation and unwind exclusion.

7.89 We have estimated that the total impact of reverting the other eligibility exclusions, with all other final policy changes in the PS26/3 model held constant, is an increase of £0.28bn in the estimated redress liabilities, which is associated with a 0.14m increase in the number of eligible agreements.

Excepting captive and white label lending, through a franchised dealer, from being an eligible arrangement under UR3

7.90 In Chapter 5 of PS26/3 we set out decision to amend the scheme so that, where there is appropriate evidence, captive and white label tied arrangements with franchised dealerships are excepted under the scheme from being relevant arrangements (and so not unfair under the scheme).

7.91 For the purposes of modelling the redress estimates, we assumed that all captive and white-labelled motor finance arrangements through franchised dealerships can show that their agreements are ineligible for a UR3 breach⁶⁶. Agreements sold through non-franchised dealers with tied arrangements have continued to be assessed as eligible, although some may be able to benefit from the 'non-operative tie' rebuttal and 'no better deal' rebuttal as explained in Chapter 10 of PS26/3.

7.92 As set out above we used the additional data sources collected post-CP25/27 to calculate a firm-specific breach rate for each firm in our sample.

7.93 To reflect this policy change in our modelling, we used data collected in DD6 to adjust lender estimates of the total count of agreements in breach of UR3, reported in Round 6 motor finance commission monitoring survey, DD6 or expanded motor commission survey monitoring data sets.

66 However, they can still be eligible agreements in our modelling where there is a UR1 or UR2 breach.

- 7.94** These firm-reported UR3 estimates were originally provided based on the CP25/27 policy position, under which such agreements were treated as creating an unfair relationship. Our liability estimates for the PS26/3 model assume all captive and white labelled motor finance agreements sold through franchised dealers are ineligible for a UR3 breach.
- 7.95** We used the DD6 dataset to estimate revised UR3 breaches for captive and white label lenders. Under this approach we assumed that agreements sold through franchised dealerships can be evidenced as ineligible for a UR3 breach, so only agreements sold via non-franchised or unknown brokers are treated as potential UR3 breaches. For captive lenders that did not report any such agreements, the model assumes zero breaches. Additionally, for three captive lenders with no DD6 submissions, we also assumed zero breaches, which may result in an underestimation of total UR3 breaches for this group.
- 7.96** For lenders which the data showed offered some agreements through franchised dealerships, we subtracted the number of agreements sold through franchised dealers, reported in DD6, from the total UR3 agreement count, as described above.
- 7.97** To estimate the revised firm-level UR3 agreement count for lenders with no captive or white label agreements, we set the revised UR3 agreement account equal to the agreement count in the CP25/27 model (updated).
- 7.98** As this data was reported by firms, this analysis relied on the assumption that firms had accurately reported the total number of agreements which breached UR3, as well as the number of agreements sold through franchised and non-franchised.
- 7.99** Where data is not available, we reverted to the approach applied in the modelling for CP25/27, where we randomly assigned a UR3 breach to 14% of firm agreements. We had relevant data for all firms except one, for which we applied this approach.
- 7.100** We have estimated that the total impact of including the captive and white label lender agreements through a franchised dealer in our redress liability estimate, with all other final policy changes in the PS26/3 model held constant, results in an increase of £0.76bn in the estimated redress liabilities, which is associated with a 1.90m increase in the number of eligible agreements.

Changes to high commission (UR2) thresholds

- 7.101** In Chapter 5 of PS26/3 we discuss our decision to set high commission as equal to or greater than 39% of the total cost of credit and 10% of the loan value. This is an increase from the threshold of 35% of TCC and 10% of loan value set out in CP25/27.
- 7.102** We used an outlier approach to identify the agreements which are within the scope of UR2. Agreements above the 85th percentile of commission as a proportion of total credit and loan amount are considered an outlier. For a comprehensive discussion of the reasons for the 85th percentile see Chapter 5 of the PS26/3. The variables required to determine the constituent features underlying our UR2 definitions are all present in DD1 except the GMFV. The GMFV value is imputed into this dataset using the regression

detailed in CP25/27 Technical Annex 1⁶⁷. We set agreements where commission is greater than or equal to 39% of the TCC and 10% of the loan value simultaneously as fulfilling the high commission criterion, indicating a high-commission breach.

- 7.103** We have estimated that the total impact of reverting the commission thresholds from 39% to 35%, with all other final policy changes in the PS26/3 model held constant, is an increase of £0.24bn in the estimated redress liabilities, which is associated with a 0.18m increase in the number of eligible agreements.

Remedy changes

- 7.104** The policy decisions in this section relate to how redress is calculated for eligible agreements, including changes to the APR adjustment methodology, the treatment of specific breach combinations, and the application of compensatory interest.

APR floor adjustment removal

- 7.105** As described in the 'Methodological updates to redress liabilities in CP25/27' section, the APR adjustment floor was proposed in CP25/27 Technical Annex 1 but was ultimately not included in the final policy. Because it is absent from the final model and given our approach of estimating impacts by removing each policy element iteratively, we are unable to estimate an impact for this specific policy change. However, in the methodological section, we provide an estimated impact for the inclusion of the APR adjustment floor within the updated CP25/27 model.

Apply APR-17 within the hybrid remedy for Scheme 2, and APR-21 for Scheme 1

- 7.106** As explained in Chapter 11 of PS26/3 we decided to apply APR-17 within the hybrid remedy for Scheme 2, and APR-21 for Scheme 1.
- 7.107** In our redress model, each agreement was assigned to a scheme based on its signing date: Scheme 1 covers agreements signed from 7 April 2007 to 31 March 2014 (inclusive), and Scheme 2 covers agreements signed from 1 April 2014 to 1 November 2024 (inclusive)⁶⁸. A scheme-specific counterfactual APR multiplier is applied to each agreement: 0.79 for Scheme 1 (representing APR-21) and 0.83 for Scheme 2 (representing APR-17). This replaced the CP25/27 approach of applying a uniform multiplier of 0.83 across all agreements.
- 7.108** We have estimated that the total impact of applying the counterfactual APR-17 across both schemes, with all other final policy changes in the PS26/3 model held constant, is decrease of £0.13bn in the estimated redress liabilities.

67 CP25/27 Technical Annex 1 paragraph 1.52.

68 A limitation of DD1 is that it is missing some data between 25 October 2024 and 1 November 2024. The policy is that Scheme 2 involves agreements until 1 November 2024. As a result, approximately 40,000 agreements falling between these two dates are not captured in DD1 and have not been included in the liability modelling. This represents a minor data misalignment and is not expected to materially affect the liability estimates. Additionally, the modelling includes agreements starting after 6 March 2007, meaning that we do not include agreements on the 6 March 2007. This represents a data misalignment that may lead to a minor understatement of the total redress liabilities.

UR2VHC+UR3, UR2VHC+UR1 and UR2VHC+UR1+UR3 all receive commission repayment remedy (Johnson remedy changed)

- 7.109** As explained in Chapter 11 of PS26/3, we have decided that agreements meeting both the very high commission threshold (UR2VHC) and either a tied arrangement breach (UR3), or a discretionary commission arrangement breach (UR1) or both UR1 and UR3 simultaneously should receive the commission repayment remedy.
- 7.110** In CP25/27, only agreements with UR2VHC and UR3 simultaneously were treated as cases that closely align with the facts in *Johnson* and received the commission repayment remedy, while agreements with UR2VHC and UR1 simultaneously were treated like other UR1/UR2/UR3 cases and received the hybrid APR adjustment.
- 7.111** In our redress model, we identified cases that closely align with the facts in *Johnson* as those where UR2VHC coincides with UR3, UR1 or both together, and on this basis then assigned the commission remedy to those agreements. All other eligible agreements received the hybrid remedy.
- 7.112** We have estimated that the total impact of reverting the *Johnson* remedy back to the CP25/27 policy, with all other final policy changes in the PS26/3 model held constant, is a decrease of £0.09bn in the estimated redress liabilities.

Compensatory interest rate floor of 3% and removal of rounding

- 7.113** As explained in Chapter 11 of PS26/3, we have decided to apply a 3% p.a. floor to compensatory interest rate and no longer round up the average annual Bank of England base rate plus 1pp to the nearest quarter percentage point.
- 7.114** In CP25/27, for simplicity we applied a single constant compensatory interest rate of 2.09% across all agreements. We now implement the Bank of England base rate plus 1pp year-by-year for every agreement, (as set out in the 'Methodological update to redress liabilities in CP25/27' section above), and, as per the policy change introduced here, we apply a 3% p.a. floor with no rounding up to the nearest quarter percentage point.
- 7.115** The estimated impact of reverting the compensatory interest remedy back to the CP25/27 model (updated) method (which excludes the 3% floor), decreased redress liabilities by £0.62bn, with all other final policy changes in the PS26/3 model held constant.

Caps on redress

- 7.116** In Chapter 11 of PS26/3, we set out our decision to apply caps to redress payable except those cases entitled to receive the commission repayment remedy. We state that firms must calculate three caps and apply the lowest one. The 3 caps required in practice are:
- 1.** 90% of commission plus compensatory interest calculated on that amount (C+I cap).
 - 2.** The realised total cost of credit (TCC), adjusted to account for a minimal cost of credit offered at the 5th percentile of the non-zero APRs in the year an agreement was signed (adjusted TCC cap).

3. The unadjusted TCC, calculated on a simpler basis as a backstop for where the adjusted cap results in redress which exceeds what the consumer actually paid for credit.

7.117 For the purposes of our modelling, we apply only the first two caps, with a floor at zero in order to prevent agreements with APRs less than the 5th percentile threshold of APR in that year from being assigned negative redress. In the PS26/3 model, redress is simply the lower of the hybrid remedy after applying the 90% the C+I cap and the adjusted TCC cap⁶⁹. Cases that closely align with the facts in *Johnson* are not capped and receive full commission repayment.

7.118 Overall, the redress liability arising from around 36% of agreements which receive the hybrid remedy is limited by the caps. The caps are binding for around 56% of agreements in Scheme 1 and around 25% of agreements in Scheme 2.

TCC cap at the realised TCC adjusted to account for a minimal cost of credit offered to only 5% of the market at the time (excluding 0% APR deals)

7.119 The cap is defined as the difference between the realised and adjusted TCC and operates as the upper limit on any redress award to ensure consumers are not placed in a better position than they would have been had their agreement been taken out at or below the 5th percentile of non-zero APRs for the year it was entered into. As set out in PS26/3, this respects the principle that redress should restore fairness, rather than place consumers in a better position than they would otherwise have been in (where, in the absence of unfairness, they would likely still have paid some minimal cost of credit). Where the APR is at or below the relevant 5th-percentile (non-zero) APR, the cap would be zero or negative. These agreements still fall within scope for assessment, but in line with our policy the redress is £0, as the consumer received a rate at or better than the specified 5th percentile threshold.

7.120 The adjusted TCC cap is constructed by first calculating the consumer's realised total charge for credit using a TCC function, applied to the loan's APR and associated parameters. For each origination year, we then identify the 5th percentile of non-zero APRs across all agreements (as shown in Table 25) and use this value to compute an adjusted TCC using the same function, substituting the 5th-percentile APR in place of the APR.

7.121 Compensatory interest is applied to both the realised TCC and the low-APR counterfactual TCC from the beginning of the agreement to the estimated redress payment date in line with the methodology described in the 'Compensatory interest' methodological update section above. The adjusted cap is calculated as the difference between the realised TCC with compensatory interest and the low APR TCC with compensatory interest.

69 See 'Limitations of analysis' section below and Chapter 11 in PS26/3.

7.122 This is different from how the adjusted TCC cap is implemented in rules, where it is calculated as a variant of the APR adjustment component of the hybrid remedy which uses payment differentials relative to an agreement at the specified 5th percentile thresholds, rather than a market adjusted APR (see Chapters 11 and 12 of PS26/3).

Table 25: 5th percentile of non-zero APRs by year

Year	5th percentile (non-zero) APR (%)	Year	5th percentile (non-zero) APR (%)
2007	6.9	2016	2.9
2008	5.9	2017	3.2
2009	5.9	2018	2.9
2010	5.7	2019	3.9
2011	4.9	2020	3.9
2012	4.7	2021	3.1
2013	4.8	2022	3.9
2014	4.2	2023	4.9
2015	4.3	2024	3.9

90% of commission plus interest

7.123 After applying the adjusted TCC cap we then capped redress at 90% of commission plus compensatory interest. This cap was calculated by multiplying the commission amount by 0.9, then applying the associated amount of compensatory interest. If this was lower than the redress after the adjusted TCC cap, we used this value as the redress amount.

7.124 We have estimated that the total impact of removing both the 90% C+I cap and the adjusted TCC cap, with all other final policy changes in the PS26/3 model held constant, is an increase of £0.91bn in the estimated redress liabilities.

Sensitivity analysis

7.125 In [Annex 6 of CP25/27](#) we set out a sensitivity analysis of our CP25/27 model estimates to alternative policy choices on approaches to calculating redress, different compensatory interest rates, and varying thresholds for defining a high commission payment.

7.126 Below we have provided an updated sensitivity analysis based on our PS26/3 model. We set how our estimate of market-wide redress liabilities of £10.03bn arising from our final rules in PS26/3 would vary under different scenarios⁷⁰. The £10.03bn figure assumes 100% of cases where there is an eligible claim receive redress under our schemes.

7.127 These sensitivities are presented to demonstrate how our model estimates would change if certain parameters varied. Table 26 reflect these for Scheme 1 and Scheme 2 combined, Table 27 reflects these for Scheme 1 and Table 28 reflects these for Scheme 2. They are subject to the same limitations as our main estimates and are not intended as an exhaustive assessment of all possible permutations.

7.128 Table 26, 27 and 28 summarise the impact of these scenarios on:

- Redress liabilities (£bn)
- Number of eligible agreements for redress
- Mean and median redress per eligible agreement (based on sample date representing 89% of the market)

Key observations

7.129 We highlight the following key observations for each scenario:

- 1.** PS26/3 model: This scenario reflects our estimates of modelling our final rules which results in total redress liabilities of £10.03bn.
- 2.** PS26/3 model with compensatory interest rate of the Bank of England base rate plus 0pp (rather than plus 1pp): Using a compensatory interest rate of the Bank of England base rate plus 0pp, total redress liabilities decrease to £9.81bn.
- 3.** PS26/3 model with compensatory interest rate of the Bank of England base rate plus 2pp (rather than plus 1pp): Using a compensatory interest rate of the Bank of England base rate plus 2pp, total redress liabilities increase to £10.27bn.
- 4.** PS26/3 model with increasing the compensatory interest rate assumption of 4.3% for 2026 to 5.0% for 2026: This scenario is a proxy to indicate the impact of recent market developments, including the geopolitical conflicts in the Middle East, that has led to a recent upward shift in market-implied interest-rate expectations for 2026. Changing the compensatory interest rate assumption for 2026 from 4.3% to 5.0% results in total redress liabilities of £10.08bn.
- 5.** PS26/3 model but changing the end date from 31 December 2026 to 31 December 2027: For this scenario, we use base rate projections from the Bank of England's February 2026 Monetary Policy Report⁷¹ for future years where full-year data is not available (for example 2026 and 2027). These imply a base rate of 3.3% by Q4 2026 and 3.5% in Q4 2027 and therefore an associated compensatory interest rate of approximately 4.3% in 2026 and 4.5% in 2027. Changing the end date from 31 December 2026 to 31 December 2027 with these assumptions results in total redress liabilities of £10.35bn.

⁷⁰ Some scenarios are similar to those in Annex 6 of CP25/27, and we have updated others to give clearer insight based on the final policy.

⁷¹ Monetary Policy Report - February 2026 | Bank of England. These projections of the base rate from the Bank of England are based exclusively on forward rates derived from Overnight Index Swap (OIS) rates.

6. PS26/3 model but changing the de minimis threshold from £120 to £150 for Scheme 1 and £150 to £200 for Scheme 2. Changing these de minimis thresholds results in total redress liabilities of £9.93bn.
7. PS26/3 model but changing the UR2 threshold to a combination of commission at 34% of the total cost of credit and 9% of the loan amount (corresponding to the 80th percentile of the market). Changing the UR2 threshold to the 80th percentile of the market results in total redress liabilities of £10.51bn. The change in redress is primarily in Scheme 2. This is likely the result of the average loan value per agreement increasing over time more than average commission amount and the average total cost of credit per agreement.
8. PS26/3 model but changing the UR2 threshold to a combination of commission at 44% of the total cost of credit and 12% of the loan amount (corresponding to the 90th percentile of the market). Changing the UR2 threshold to the 90th percentile of the market results in total redress liabilities of £9.58bn. The change in redress is primarily in Scheme 2. This is likely the result of the average loan value per agreement increasing over time more than average commission amount and the average total cost of credit per agreement.
9. PS26/3 model but changing our modelled caps to:
 - a. 80% of commission plus compensatory interest (C+I cap) instead of 90% and
 - b. Adjusted TCC cap using the 10th percentile of non-zero APRs across all agreements by year instead of the 5th percentile.

Changing the modelled caps results in total redress liabilities of £9.53bn.

Table 26: Sensitivity of our market-wide redress liability estimates to alternative policy choices (Scheme 1 & Scheme 2 combined)

Scenario	Description	Redress liabilities (£bn)	Eligible agreements	Mean redress (£)	Median redress (£)
1	PS26/3 model: commission repayment remedy for cases that closely align with the facts in <i>Johnson</i> ; hybrid remedy for all other cases; compensatory interest at base rate plus 1pp	£10.03bn	12.14m	£829	£661
2	PS26/3 model with compensatory interest rate of Bank of England base rate plus 0pp	£9.81bn	12.14m	£812	£647
3	PS26/3 model with compensatory interest rate of Bank of England base rate plus 2pp	£10.27bn	12.14m	£850	£677
4	PS26/3 model with increasing the compensatory interest rate assumption of 4.3% for 2026 to 5.0% for 2026	£10.08bn	12.14m	£834	£664
5	PS26/3 model but changing the end date from 31 December 2026 to 31 December 2027	£10.35bn	12.14m	£856	£682
6	PS26/3 model but changing the de minimis threshold to £150 for Scheme 1 and £200 for Scheme 2	£9.93bn	11.63m	£858	£689
7	PS26/3 model with UR2 threshold at 80th percentile (34%/9%)	£10.51bn	12.57m	£840	£671
8	PS26/3 model with UR2 threshold at 90th percentile (44%/12%)	£9.58bn	11.73m	£820	£651
9	PS26/3 model with 80% C&I cap and 10th percentile adjusted TCC cap	£9.53bn	12.14m	£788	£623

Notes: The market-wide redress liabilities are reweighted to over 99% of the market (market-wide estimates), but the mean and median redress per agreement are calculated from in-sample estimates (covering 89% of the market).

Table 27: Sensitivity of our redress liability estimates to alternative policy choices (Scheme 1), sample estimates

Scenario	Description	Redress liabilities (£bn)	Eligible agreements	Mean redress (£)	Median redress (£)
1	PS26/3 model: commission repayment remedy for cases that closely align with the facts in <i>Johnson</i> ; hybrid remedy for all other cases; compensatory interest at base rate plus 1pp	£3.05bn	4.16m	£734	£569
2	PS26/3 model with compensatory interest rate of Bank of England base rate plus 0pp	£2.99bn	4.16m	£719	£558
3	PS26/3 model with compensatory interest rate of Bank of England base rate plus 2pp	£3.12bn	4.16m	£751	£581
4	PS26/3 model with increasing the compensatory interest rate assumption of 4.3% for 2026 to 5.0% for 2026	£3.06bn	4.16m	£737	£571
5	PS26/3 model but changing the end date from 31 December 2026 to 31 December 2027	£3.14bn	4.16m	£755	£585
6	PS26/3 model but changing the de minimis threshold to £150 for Scheme 1 and £200 for Scheme 2	£3.01bn	3.96m	£761	£588
7	PS26/3 model with UR2 threshold at 80th percentile (34%/9%)	£3.05bn	4.16m	£734	£569
8	PS26/3 model with UR2 threshold at 90th percentile (44%/12%)	£3.05bn	4.16m	£733	£569
9	PS26/3 model with 80% C&I cap and 10th percentile adjusted TCC cap	£2.80bn	4.16m	£674	£514

Notes: All estimates in Table 27 are based on in-sample estimates covering 89% of the market (sample estimates).

Table 28: Sensitivity of our redress liability estimates to alternative policy choices (Scheme 2), sample estimates

Scenario	Description	Redress liabilities (£bn)	Eligible agreements	Mean redress (£)	Median redress (£)
1	PS26/3 model: commission repayment remedy for cases that closely align with the facts in <i>Johnson</i> ; hybrid remedy for all other cases; compensatory interest at base rate plus 1pp	£6.84bn	7.77m	£881	£717
2	PS26/3 model with compensatory interest rate of Bank of England base rate plus 0pp	£6.69bn	7.77m	£861	£702
3	PS26/3 model with compensatory interest rate of Bank of England base rate plus 2pp	£7.02bn	7.77m	£903	£735
4	PS26/3 model with increasing the compensatory interest rate assumption of 4.3% for 2026 to 5.0% for 2026	£6.88bn	7.77m	£886	£721
5	PS26/3 model but changing the end date from 31 December 2026 to 31 December 2027	£7.08bn	7.77m	£911	£742
6	PS26/3 model but changing the de minimis threshold to £150 for Scheme 1 and £200 for Scheme 2	£6.79bn	7.47m	£909	£744
7	PS26/3 model with UR2 threshold at 80th percentile (34%/9%)	£7.31bn	8.18m	£894	£731
8	PS26/3 model with UR2 threshold at 90th percentile (44%/12%)	£6.41bn	7.37m	£869	£704
9	PS26/3 model with 80% C&I cap and 10th percentile adjusted TCC cap	£6.60bn	7.77m	£850	£686

Notes: All estimates in Table 28 are based on in-sample estimates covering 89% of the market (sample estimates).

Limitations of analysis

7.130 This section sets out the limitations to our estimates of redress liabilities and known divergences between policy and redress liability model. PS26/3 and the rules are the definitive sources of our policy and requirements. Our modelling reflects the policy intent as far as reasonably possible, but it is limited by the data available to estimate liabilities across many millions of agreements. We have applied proportionate, evidence-based judgement throughout.

Limitation to our estimates of redress liabilities

7.131 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. The limitations set out in [CP25/27 Technical Annex 1](#) continue to apply to the PS26/3 model in most circumstances.

UR3 modelling updates and limitations

7.132 The CP25/27 model limitation relating to the random assignment of UR3 breach rates has been revised. Under the CP25/27 model (updated) and PS26/3 model, firm-specific UR3 data collected post-CP25/27 allows us to calculate firm-level breach rates directly, with random allocation now applied using firm-specific breach rates for UR3. We are significantly reliant on the breach rates provided by the firms, including whether these are unique breaches or concurrent with another breach.

7.133 A limitation of the Round 6 motor finance commission monitoring survey and DD6 is that the information used is reported by firms and has not been independently verified. We therefore rely on the accuracy of firm submissions when using this data. Inaccurate reporting of this data could impact our estimates. The breach estimates are not reported on a yearly or agreement level basis therefore the calculated firm-level breach rate is randomly assigned for each firm across all agreements where commission is greater than zero and years. For more information on DD6 and its limitations, please refer to the [Updated Diagnostic Report](#).

Other data limitations

7.134 As noted in the [CP25/27 Technical Annex 1 \(Table 2\)](#) and in the [Updated Diagnostic Report](#), a limitation of DD1 is that it is missing some data between 25 October 2024 and 1 November 2024. The policy is that Scheme 2 involves agreements until 1 November 2024. As a result, approximately 40,000 agreements falling between these two dates are not captured in DD1 and have not been included in the liability modelling. This represents a minor data misalignment and is not expected to materially affect the liability estimates. Additionally, the modelling includes agreements starting after 6 March 2007, meaning that we do not include agreements on the 6 March 2007. This represents a data misalignment that may lead to a minor understatement of the total

redress liabilities.

- 7.135** The high value loan thresholds are derived from DD1, which does not capture all firms operating in the high value vehicle segment. However, DD1 includes major manufacturer-affiliated finance providers whose groups encompass a range of luxury brands, and we consider the resulting thresholds to be a reasonable and practicable proxy for identifying high value agreements.
- 7.136** We assume the data reported by firms is correct. While we did not audit the complete dataset, we undertook data cleaning procedures and imputed some missing data as described in the [Data Guide in CP25/27 Technical Annex 1](#).
- 7.137** Another limitation is missing values in DD1, and we set this out in more detail and how we address this in the [Data Guide in Technical Annex 1 of CP25/27](#).

Other modelling limitations

- 7.138** In all our modelling (except scenario 6 in the sensitivity analysis), we assume that redress payments occur on 31 December 2026, which is treated as the estimated redress date for all agreements (as an assumed average).
- 7.139** We have not presented an updated version of the randomisation sensitivity analysis shown in the [CP25/27 Technical Annex 1](#). This is because our approach has not changed substantially since CP25/27.
- 7.140** The sampling approach (also explained in the [Technical Annex 1 of CP25/27⁷²](#)) was designed based on the number of outstanding agreements as of June 2024. This means that when we scale figures to estimate market-wide values, we are implicitly assuming that the market structure observed in 2024 is representative of the entire period under review.

Known divergences between policy and PS26/3 redress liability model

- 7.141** We use our redress liability model to estimate market-wide redress based on the data available to us. Firms will calculate redress for individual agreements using PS26/3 and the rules, and they may have more complete information than we do.
- 7.142** PS26/3 and the rules are the definitive sources of our policy and requirements. This section sets out the main known divergences between the policy in PS26/3 and our model and explains why they occur. Any differences in wording are for explanation only and do not change the policy.

Arrears

- 7.143** Due to the lack of agreement-level data on late payments and their impact on the total cost of credit, we do not model arrears in our PS26/3 model. If an agreement ends in arrears, details on how a firm may interpret their ability to 'set off' redress with the amount left in arrears may be found in Chapter 11 of PS26/3.

72 CP25/27 Technical Annex 1 paragraph 3.52-3.60.

Early settlements

- 7.144** We assume that all early settlements are full rather than partial. A full early settlement involves a single early settlement payment which comprises of the remaining principal to be paid. The remaining interest is unpaid and referred to as the rebate. In a partial early settlement, consumers may make payments above the regular monthly payment to reduce their remaining balance more quickly. If this occurs before full early settlement, the resulting full early settlement values would be lower because less balance remains outstanding. In the absence of early settlement, the loan schedule could be re-amortised unless the contract involves fixed terms. The variations introduced by these considerations are highly bespoke and rely on data not available to us. As a result, we do not allow for partial early settlement in our modelling.
- 7.145** When calculating full early settlement values, we use the remaining balance calculated with commonly available financial formula. We note that this deviates from the actuarial approach outlined in the Consumer Credit (Early Settlement) Regulations 2004 (as amended by the Consumer Credit (EU Directive) Regulations 2010)⁷³, which instead redistributes payments to smooth out variations in the proportion of each monthly payment that is used to pay down principal vs interest. However, our modelling approach is consistent with our handling of monthly payments throughout the APR adjustment calculations. It therefore provides a consistent representation of the impact of early settlement.

Caps on redress

- 7.146** As discussed in Chapter 11 of PS26/3, firms are required to calculate all three redress caps and apply the lowest resulting value. The caps are:
- 1.** 90% of commission plus compensatory interest (C+I cap).
 - 2.** The realised total cost of credit (TCC), adjusted to account for a minimal cost of credit offered at the 5th percentile of the non-zero APRs in the year an agreement was signed (adjusted TCC cap).
 - 3.** The unadjusted TCC, calculated on a simpler basis as a backstop for where the adjusted cap results in redress which exceeds what the consumer actually paid for credit.
- 7.147** For the purposes of modelling redress liabilities, we apply only the first two caps, as the third cap is implicitly contained within the adjusted TCC calculation where full payment schedule data is available. In practice, firms are still required to calculate the third cap because the adjusted TCC cap⁷⁴ may, in cases of incomplete data (eg where early settlement information is missing), produce a higher figure than a simple realised TCC based cap.
- 7.148** Under the rules, redress is £0 where the APR is at or below the relevant 5th percentile non-zero APR. This reflects that the consumer paid the same, or less than, they would have at the 5th percentile threshold. Above that threshold, the cap is calculated as the sum of the differences between each payment actually made and the payment

73 [Guidance on the regulations implementing the Consumer Credit Directive](#), p.110.

74 Implemented in the rules as a variant of the APR adjustment methodology rather than as a direct adjustment to the single realised TCC value.

that would have been made at the 5th percentile non-zero APR, with compensatory interest applied using the same assumptions as the APR adjustment component of the hybrid remedy. This approach ensures consistency across methodologies, but because it relies on reconstructed payment schedules, it may produce a higher cap than the simple unadjusted TCC cap where firms are missing data. In these cases, the realised TCC cap operates as a backstop so that redress does not exceed the consumer's actual credit costs when firms cannot accurately calculate the adjusted TCC cap. It is also a considerably simpler calculation than the adjusted cap and will allow firms to make settlement offers at this value without having to undertake the additional work to calculate the adjusted cap.

Limitation period

- 7.149** In our modelling, we assume 100% of agreements linked to high commission that fall outside the 6-year limitation period are correctly identified and treated as ineligible for redress. We therefore exclude these agreements from the total redress estimate and as eligible agreements. In practice, the proportion identified may be lower than 100%. This would mean that our modelling understates redress liabilities for these agreements.

Agreements lasting less than one month

- 7.150** In Chapter 4 of PS26/3, we set out our approach to very early settlement and cancelled or unwound agreements. We exclude agreements that were cancelled or fully unwound during the initial cooling off period from the scheme. Agreements that were settled early remain scheme cases, but the TCC caps will usually limit redress to a small amount. For modelling, we proxy this policy by excluding agreements with a term of less than 1 month. This does not reflect the rules exactly, but we consider it a proportionate assumption that captures the effect of excluding cancelled and unwound agreements.

Compensatory interest Bank of England rate projections

- 7.151** Chapter 11 of PS26/3 says that, from 2026 onwards, the compensatory interest rate is calculated using the average of the quarterly Bank of England base rate projections for each year. PS26/3 sets the compensatory interest rate at 4.49% for 2026, which is based on the average of the actual Q1 Bank of England base rate and the market-implied path for Q2 to Q4 in 2026, plus the required 1pp uplift. For future years (2027 and 2028), it is simply the average of the quarterly projections in that year, plus the 1 pp.
- 7.152** In contrast, our redress liability modelling applies the Q4 2026 projection to 2026 in its entirety and is intended to reflect liabilities across firms as of 31 December 2026. Our modelling therefore applies a compensatory interest rate of 4.3% for 2026 and does not include compensatory interest rates for 2027 and 2028. We have conducted sensitivity analysis on the impacts of different compensatory interest rates (see the 'Sensitivity analysis' section above).

Rebuttals

7.153 Our estimate of the potential total redress liabilities does not include any of the 4 possible rebuttals outlined in Chapter 10 of PS26/3, namely the rebuttals of adequate disclosure, the sophisticated customer, a non-operative tied arrangement and no better deal. We do not have sufficient data to accurately assess how many rebuttals may be submitted in relation to each rebuttal or how many would ultimately succeed. As a result, our estimate of total redress liabilities may be overstated if firms successfully rely on these rebuttals for a material number of agreements, and if those agreements do not result in redress.

Chapter 8

Updated methodology for non-redress cost estimates

Summary

- 8.1** This section supplements the CBA for the Policy Statement and provides a more detailed explanation of the analytical framework and methodology underpinning our quantitative estimates. It sets out the key methodological steps, data sources, and assumptions adopted in reaching non-redress cost estimates, and clarifies how these have been applied in the counterfactual and policy scenarios.
- 8.2** We identify where the methodology and assumptions for the CBA have changed since the consultation stage and as set out in the [Technical Annex](#) for CP25/27. For each material update, we explain the rationale for the change, including any new evidence or refinements to the analytical approach.
- 8.3** As was the case previously, costs are categorised into lender and consumer costs. Our monetised costs to firms comprise costs associated with in-house case-handling, costs associated with the handling of cases referred to the Financial Ombudsman Service ('the Financial Ombudsman'), and other costs (such as those incurred when implementing staff training programmes). Our monetised costs to consumers comprise time and effort costs.
- 8.4** 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), firms' own responses to questions asked in our motor finance commission monitoring survey, and various pieces of desk-based research using publicly available information.
- 8.5** All estimates except 'other lender costs' are obtained from our non-redress costs model set out in this section. The 'other lender costs' are estimated through the application of our [Standardised Cost Model \(SCM\)](#).
- 8.6** We consider the impacts of Scheme 1 and 2 separately against the counterfactual scenario – broken down to represent agreements across each time period.
- 8.7** The table below summarises the quantified non-redress impacts under each scheme.

Table 29: Summary of quantified non-redress impacts (central estimates, nominal)

	Counterfactual	Counterfactual pre 2014	Counterfactual – post 2014	Scheme 1 + 2 – Intervention	Scheme 1 only – Intervention	Scheme 2 only – Intervention
Total Administrative costs	£856.3 m	£256.1 m	£600.2 m	£1,097.2 m	£322.9 m	£774.4 m
Total Financial Ombudsman fees	£7,040.3 m	£1,476.9 m	£5,563.4 m	£546.7 m	£153.7 m	£393.0 m
Total Court costs	£64.4 m	£18.1 m	£46.3 m	£0.0 m	£0.0 m	£0.0 m
Total Other costs	£0.0 m	£0.0 m	£0.0 m	£6.0 m	£2.1 m	£3.9 m
Total lender costs	£7,961.0 m	£1,751.2 m	£6,209.8 m	£1,649.9 m	£478.6 m	£1,171.3 m
Consumer time costs	£250.8 m	£61.4 m	£189.4 m	£77.4 m	£23.3 m	£54.1 m
Total costs (including consumer time costs)	£8,211.8 m	£1,812.6 m	£6,399.3 m	£1,727.3 m	£501.9 m	£1,225.4 m

8.8 We present estimates of the benefits against the counterfactual, sensitivity analyses, and the present value (PV) and net present value (NPV) of the scheme in the CBA. Non-quantified costs and benefits are also assessed in the CBA.

Data sources

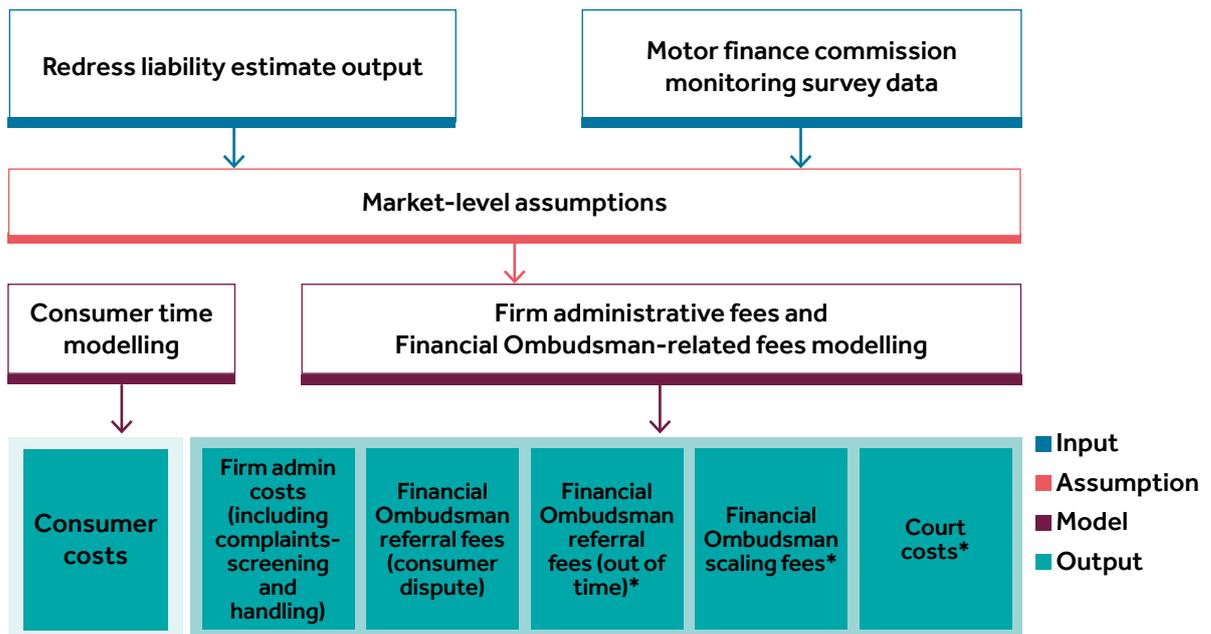
8.9 Our analysis draws on a range of data sources, including requests we have made, as well as publicly available and commercial data sets. These include:

- Loan level data (LLD)
- s166 customer assessment form data / skilled person review + DCA casefile review
- Data drop 1 (DD1)
- Data drop 2 (DD2) / Non-DCA casefile review
- Credit reference agency (CRA)
- Cost of living consumer credit data collection
- Motor finance commission monitoring survey (recently updated in January 2026 to include Round 6)
- Motor finance lender survey
- Motor finance broker survey
- Financial Lives Survey
- Motor Finance Consumer Awareness Survey
- Yonder Consumer Research

Overview of modelling approach

- 8.10** A visual depiction of the high-level modelling approach to non-redress costs is shown in the figure below and is applied to both the counterfactual and intervention scenario. 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 21: Overview of modelled non-redress cost inputs and outputs associated with the CRS and the counterfactual



* Financial Ombudsman scaling fees are assumed to be £0, the Financial Ombudsman referral rate (out-of-time) is assumed to be 0%, and court costs are assumed to be £0 under our proposed intervention.

- 8.11** The 3 types of costs modelled quantitatively under both the counterfactual and intervention scenario are the same as set out in paragraph 4.9 of the [Technical Annex](#) for CP25/27.
- 8.12** In our counterfactual scenario, we now also estimate the court costs incurred by firms under a case-led framework. These include external costs, such as hearing fees and solicitor payments, as well as internal costs, including employee time spent on case preparation and related administrative activities.
- 8.13** In CP25/27, the proposed policy comprised of one screening exercise. We now estimate the costs associated with each successive round of screening (comprising, high loan value agreements; de-minimis; APR; unfair relationship (UR) eligibility checks; and limitation period screening). Each round of screening removes agreements from the model. To estimate the costs associated with each screening exercise, we combine the number of agreements which enter each stage, with firm-level wage data, and estimates on the time it takes for firms to screen agreements. For all screening checks except the unfair relationship check, data limitations mean that we impute market-wide assumptions on the time it will take firms to screen agreements. For UR screening, we use firm-level estimates on the time taken to screen agreements (from the Round 6 motor finance commission monitoring survey).

- 8.14** The potential Financial Ombudsman related costs incurred by firms have increased. This follows the Financial Ombudsman's consultation (published since CP25/27) to increase case fees from £650 to £680, we have reflected this change in our CBA by updating our cost assumptions accordingly.
- 8.15** The starting point for lender non-redress cost models is the total number of motor finance agreements held across the market from April 2007 to November 2024. We estimate this at 9.3m agreements for agreements between April 2007 and March 2014 and at 23.6m agreements for agreements between April 2014 and November 2024, totalling 32.9m agreements across the two schemes.
- 8.16** For the counterfactual model, a complaint incidence rate for agreements April 2007 to March 2014 of 50% is applied and for agreements April 2014 to November 2024 a case incident rate of 60% is applied to the total number of agreements at the lender level. After the complaint incidence rate is applied to the total number of agreements, an expected 19.0m case are received by firms.
- 8.17** Lender-level complaint-handling modelling estimates how many case firms can be resolved within the 8-week deadline and how many will exceed it. This is only calculated under the counterfactual scenario because we assume no complaints time out under the intervention scenario. These estimates feed into the calculation of Financial Ombudsman fees and administrative costs. We assume firms incur variable administrative case-handling costs, and that Financial Ombudsman fees arise from referral fees (due to time-outs or consumer disagreement with outcomes) and associated scaling fees.
- 8.18** A voluntary 5-month implementation period is introduced for Scheme 1 and a 3-month period for Scheme 2 to ensure that firms have sufficient time to establish and embed processes for assessing liability and, where necessary, gather information from dealers. During this period, firms may incur one-off implementation costs, including updates to internal systems to record, track and assess claims.
- 8.19** Firms may also incur one-off costs associated with drafting and updating policies and procedures, training relevant staff, and undertaking legal and compliance review. These transitional activities are intended to facilitate a smoother operational rollout and reduce the risk of delays once the schemes become fully operational post-implementation.
- 8.20** For both schemes, it is assumed that firms carry out an initial screening process for all agreements to assess whether they are likely to be scheme cases (with at least one relevant arrangement), or not. For cases already received, if they are deemed scheme cases they will proceed through the scheme steps and have a provisional redress decision issued. It is at this point, when the consumer is aware of what they will be offered, that they are given an opportunity to opt-out of the scheme.
- 8.21** Firm-led opt-in applies to agreements which are considered scheme cases with at least one relevant arrangement and where no complaint has been made. 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 72% for Scheme 1 and 76% for Scheme 2.

- 8.22** Following the screening and opt-in/opt-out process, it is assumed that 10.6m cases will proceed to full case assessment. These lender-level case estimates feed into the estimation of Financial Ombudsman related fees and administrative costs.
- 8.23** The administrative costs incurred by firms 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. Under the intervention, it is assumed for modelling purposes that the Financial Ombudsman related fees incurred by firms arise only from Financial Ombudsman referral fees because of consumers disagreeing with case outcomes, given the complaint determination timelines will be determined by CRS rules rather than those within DISP.
- 8.24** 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 cases.
- 8.25** In the counterfactual scenario, it is estimated that consumers spend a total of 33m hours making, submitting and overseeing their case, which equates to a value of time estimate of £251m. In the intervention scenario, it is estimated that consumers spend 10m hours making, submitting and overseeing their case, which equates to a value of time estimate of £77m.

Model inputs

- 8.26** We combine the expected number of cases (from the output of redress liability estimates) with firms' expected complaint-handling capacity (from our motor finance commission monitoring survey). This allows us to model the expected complaint handling capacities of firms to calculate the number of cases handled and the number of cases not handled in the relevant timeframes. The number of cases not handled in the relevant timeframe become eligible for Financial Ombudsman referral. We use these figures to estimate administrative costs and Financial Ombudsman fees incurred by each lender.
- 8.27** 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 firms and, after reweighting, is representative of over 99% of the market.
 - Data received from our motor finance commission monitoring survey, collected most recently in January 2026, which surveys 34 firms 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

8.28 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. These estimates are based on DD1 data and are reweighted to represent over 99% of the market using outstanding loan values as of June 2024. See the redress liabilities section for updates to DD1 since CP25/27. The estimates 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 survey (Round 6)

8.29 The results of the motor finance commission monitoring survey inform our understanding of firms' capacity to handle cases and ultimately the administrative costs associated with complaint handling.

8.30 Specifically, we ask firms the following questions in our monitoring survey:

- What is the estimated one-off investment your firm expects to make in the future, in systems, capital, or infrastructure specifically to manage our proposed consumer redress scheme? (£)
- What is your firm's estimated average time to screen an agreement as per our proposed consumer redress scheme? (minutes)
- How many FTE staff does your firm expect to allocate to screening agreements under our proposed consumer redress scheme? (FTE)
- What is the average total hourly cost per staff member assigned to complaint handling involved in our proposed consumer redress scheme? (£)
- What is your firm's estimated average time to process a single complaint within our proposed consumer redress scheme, from initial receipt to final resolution? (minutes)
- What is the average total hourly cost per staff member assigned to screening agreements under our proposed consumer redress scheme? (£)

8.31 We provide a summary of firms' own responses in table below. Firm responses in the table below only include responses from Round 6 of the motor finance commission monitoring survey.

Table 30: firms' responses to our survey questions

Element used in our modelling	Lowest	Median	Highest
One-off investment	£0	£1,043,013	£44,300,000
Complaint screening time, intervention (minutes)	0*	4	75
FTE screening time employees	0*	5	60
Hourly cost of case-screening employees (excluding overheads)	£0*	£24	£65
FTE case-handling employees	1	52	3,077**
Complaint processing time (excluding complaint screening) (minutes)	4.8	52.5	400
Hourly cost of case-handling employees (including overheads)	£10	£22	£70

* Reported automation of screening processes.

** Reported as a group. We weighted this response by the relative number of cases registered to date to each group member.

- 8.32** Our review of firm responses indicates that the median screening time for an agreement is approximately 4 minutes, or a mean value of 11 minutes. This has a significant impact on reducing administrative costs in our updated CBA as we had previously, in the absence of survey data, taken a conservative assumption that an agreement takes approximately 60 minutes to screen.
- 8.33** For the counterfactual, we had previously used results from an earlier iteration of our motor finance commission monitoring survey. In May 2025, prior to the Supreme Court's decision, we asked firms how many minutes they expect their staff to spend processing DCA and non-DCA cases from initial receipt to resolution. We now use Round 6 of the motor finance commission monitoring survey, as firm responses are more up to date and reflective of our both the Supreme Court's decision and CP25/27.

Standardised Cost Model

- 8.34** Our Standardised Cost Model (SCM) enables us to estimate the compliance costs which firms 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.
- 8.35** We have amended some assumptions in the SCM since the consultation. We annually uplift the salary data used in the model based on annual updates to the Annual Survey of Hours and Earnings (ASHE). These changes have a minor impact (increase) on the overall compliance costs incurred by firms.

8.36 Previously, we had applied a salary uplift of 7.9% in 2024 using data from the ONS on full time percentage increases in median gross annual earnings for financial and insurance activities. We now apply a new salary uplift as follows:

- 7.1% in 2024 using revised ONS full-time % increase in median gross annual earnings for financial and insurance activities.
- 12.3% in 2025 using new ONS full-time % increase in median gross annual earnings for financial and insurance activities.

Assumptions

8.37 The tables below set out the assumptions used to model costs under the counterfactual and the intervention scenarios. The tables provide low case, central case and high case assumptions, along with reasoning for the choice of each assumption. Where there have been material changes to our assumptions since the consultation stage, we outline these changes along with the rationale for our amendments.

8.38 Where assumptions remain unchanged from the consultation stage, they are not discussed further in this section for brevity. The tables below therefore focus on assumptions that have changed or require further explanation.

Table 31: Changes to quantitative assumptions used to model the counterfactual scenario

Input	Low	Central	High	Original reasoning in CP25/27	Change from CP25/27 and rationale for change
Complaint incidence rate	48% (2007 – 2024) 40% (2007 – March 2014) 51% (April 2014 – 2024)	58% (2007 – 2024) 50% (2007 – March 2014) 61% (April 2014 – 2024)	68% (2007 – 2024) 60% (2007 – March 2014) 71% (April 2014 – 2024)	<p>The complaint incidence rate measures how many cases occur compared to the total number of agreements. To estimate this rate for a case-led scheme CRS), the lower bound was based on results from six PPI ComRes surveys conducted in 2015 with over 20,000 UK adults. The surveys found that 47% of eligible consumers had already complained about PPI, and another 12% said they intended to complain before the deadline. Since later evidence suggests additional cases were made after 2015, the analysis used 59% (47% + 12%) as the 'low' estimate.</p> <p>The surveys also showed that 16% of respondents were unsure whether they would complain. If the same proportion of these undecided consumers eventually complained as those who had already complained or intended to (59%), the overall complaint rate would rise to just under 69%. This figure was therefore used as the 'central' estimate.</p> <p>A 'high' estimate was created by adding the difference between the 'low' (59%) and 'central' (69%) estimates to the central estimate. The analysis also considered evidence from the British Steel Pension Scheme (BSPS) redress scheme, an opt-out scheme under Section 404, which had an uptake rate of 74.4%. Opt-out schemes typically achieve the highest participation rates because consumers are automatically included and do not need to initiate a complaint themselves.</p>	<p>Following stakeholder feedback that our original opt-in scheme case intervention uptake rate was too high, we have made some adjustments, including revisiting our counterfactual assumptions.</p> <p>Taking 69% as an estimate for PPI uptake, we revisit our counterfactual rate by attempting to account for the impact of features present under PPI unlikely to be a feature in our counterfactual. This includes proactive firm-led communications and a substantial public communications campaign.</p> <p>Data suggests that proactive firm-led communications exercises under PPI generated 3.9ppt of total eligible cases eligible for redress, and the public communications campaign generated a further 7.5ppt, combining to an additional ~11ppt of the total eligible cases. Assuming these would not be present in our counterfactual leads to a revised counterfactual estimate of 58%.</p> <p>We have also estimated different case incidence rates under the two schemes. Data from a consumer survey undertaken for the consultation suggests that consumers with agreements falling under Scheme 1 would be 10ppt less likely to join than those with agreements falling under the more recent Scheme 2. Applying this differential to an overall case incidence rate of 58% estimates an incidence rate of 50% for Scheme 1 and 61% for Scheme 2.</p> <p>We apply the same differential of 10ppt between our "low" and "central" and "central" and "high" estimates as in the consultation, resulting in a "low" estimate of 48% and a "high" estimate of 68%.</p>
Financial Ombudsman fees	£650	£680	£680	We applied a standard case fee of £650 for complaints referred to the Financial Ombudsman.	Following the Financial Ombudsman Service's consultation to increase case fees from £650 to £680, we have reflected this potential change by updating our cost assumptions accordingly.

Input	Low	Central	High	Original reasoning in CP25/27	Change from CP25/27 and rationale for change
Financial Ombudsman scaling fees	£455	£476	£476	We uplifted the PPI scaling fee by the proportionate increase in Financial Ombudsman case fees since PPI. These fees increased from £500 to £650 over the period, such that we applied a 30% uplift to the PPI scaling fees.	We use the same methodology as we did the CP25/27, however in the central and high scenarios we account for the Financial Ombudsman's proposed increase in case fees. Thus, we use £680 as the numerator of the equation, meaning we apply a 36% uplift in the central and high scenarios (£476). In the low scenario, the 30% uplift remains in place (£455).
Proportion of complaints submitted through PRs/CMCs	27.88%	43.75%	75.5%	In the consultation, we assumed a 'central' estimate of 'proportion of cases submitted through PRs/CMCs' of 76% in the counterfactual. This was calculated using responses from 30 firms which suggested that the majority of firms have experienced 76% of their cases so far from PRs/CMCs. Our 'high' estimate was taken from a Financial Ombudsman <u>announcement</u> in Q1 2024/25 reporting that around 90% of motor finance commission cases were submitted by PRs in Q1 2024/25. We deducted the difference between our 'central' and 'high' estimates from our 'central' estimate to obtain our 'low' estimate of 61%.	We have updated our counterfactual estimates in light of the revised Financial Ombudsman referral fee structure <u>announced last year</u> which, in our view, will reduce CMC engagement in the counterfactual. <u>Reporting</u> from PPI firm monitoring indicates that, by 2014, 47% of cases were made through CMCs. Given we expect the revised referral fee structure would reduce CMC engagement, we estimate CMC engagement to be lower than this in our counterfactual. Our 'central' estimate is now 44%, equivalent to our intervention 'central' estimate in the consultation. Our 'high' estimate is 76%, taken from the 'central' counterfactual estimate of the consultation. Our 'low' estimate is 28%, taken from our revised 'central' estimate for the intervention (see Table 32 below).
Inclusion of court costs	N/A	N/A	N/A	We did not quantify the court costs incurred by firms in the consultation CBA.	We included questions on the estimation of court costs in Round 6 of our motor finance commission monitoring survey. Using data from firms and banks we were able to use this data to model costs for courts in our estimations. Firms supplied court case volumes and cost breakdowns, allowing us to calculate total annual unit costs and the current number of cases progressing through court. With a firmer baseline, we were able to model how total costs scale under alternative volume scenarios, including breakeven analysis showing that court case volumes would need to increase by around a factor of ten to materially affect the NPV in the counterfactual.

Notes: We have updated the firm-level inputs to be based on the Round 6 motor finance commission monitoring survey estimates.

Table 32: Updated quantitative assumptions used to model non-redress costs in the intervention scenario

Input	Low	Central	High	Original reasoning in CP25/27	Change from CP25/27 and rationale for change
CRS join rate for opt-in process (Schemes 1 + 2)	<p>Scheme cases</p> <p>58% (2007 – 2024)</p> <p>54% (2007 – 2014)</p> <p>59% (2014 – 2024)</p> <p>Non-scheme cases</p> <p>14% (2007 – 2024)</p> <p>10% (2007 – 2014)</p> <p>15% (2014 – 2024)</p>	<p>Scheme cases</p> <p>70% (2007 – 2024)</p> <p>66% (2007 – 2014)</p> <p>71% (2014 – 2024)</p> <p>Non-scheme cases</p> <p>20% (2007 – 2024)</p> <p>16% (2007 – 2014)</p> <p>21% (2014 – 2024)</p>	<p>Scheme cases</p> <p>95% (2007 – 2024)</p> <p>91% (2007 – 2014)</p> <p>96% (2014 – 2024)</p> <p>Non-scheme cases</p> <p>26% (2007 – 2024)</p> <p>22% (2007 – 2014)</p> <p>27% (2014 – 2024)</p>	<p>Consumers in the CRS were grouped by whether they had already complained and whether the lender’s initial letter indicated they were likely or unlikely to be owed redress. Letter type A (likely owed) encouraged participation, while Letter type B (unlikely owed) discouraged it. In opt-out schemes, consumers receiving Letter B were typically not invited. Participation rates varied by group, with opt-in schemes generally higher than case-led schemes.</p> <p>Behavioural research showed that direct communication and being told one was likely to be eligible for redress increased CRS join rates by around 14 ppts, resulting in a central estimate of 83% for Letter A recipients. Conversely, a “likely ineligible for redress” message reduced participation due to loss aversion by twice the +14ppt benefit (ie -28ppt), giving a central estimate of 41% for Letter B recipients.</p> <p>Low and high estimates adjusted these assumptions: the low estimate assumed stronger discouragement for Letter B (42-point reduction), while the high estimate assumed almost all Letter A recipients joined and Letter B had only moderate dissuasion, giving 55% participation. Communication campaigns could also have influenced these rates.</p>	<p>Stakeholders queried this overall opt-in scheme case uptake rate and suggested it was too high, based on their own insights and experiences such as Payment Protection Insurance (PPI).</p> <p>While stakeholders did not provide specific views on the credibility of the intervention benefit estimate of +14ppt, in light of the overall strength of stakeholder feedback on our opt-in scheme case intervention uptake rate, we have also modestly revised downward this intervention benefit.</p> <p>Our original estimate of +14ppt was taken as the mid-point of an uncertainty range of +9ppt to +20ppt, based on relevant behavioural science evidence. We have reduced this to the mid-point of our original +14ppt and the low estimate of our range, +9ppt. This equates to roughly +12ppt, a reduction of 2ppt.</p> <p>Our “low” estimate is our revised counterfactual estimate of 58%, because the scheme is designed to encourage uptake of those who are eligible to opt in (scheme cases). Our “high” estimate is the estimated uptake rate of existing complainants, who will not need to proactively join the scheme (ie consumers with scheme cases in the opt-out channel).</p>

Input	Low	Central	High	Original reasoning in CP25/27	Change from CP25/27 and rationale for change
					<p>Our opt-in ineligible uptake rates account for changes in the scheme design; whereby participants will no longer receive firm-led communications if they are unlikely to be eligible for redress.</p> <p>We first applied the same logic as in the consultation (see Table 37 in <u>CP25/27 Technical Annex 1</u>) with new counterfactual (58%) and opt-in scheme case intervention benefits (+12ppt) to estimate the uptake rate with these new estimates, assuming no policy change, resulting in an uptake rate of 34%.</p> <p>We then account for the policy change by adjusting for (i) a 35% forgone benefit of receiving a communication, taken from related evidence from <u>behavioural science research</u> and (ii) underlying awareness of a scheme. Applying these adjustments leads to a final opt-in non-scheme case uptake rate of 20%.</p> <p>We apply the same calculations to our "low" and "high" counterfactual estimates to arrive at our "low" and "high" opt-in non-scheme case estimates.</p>

Input	Low	Central	High	Original reasoning in CP25/27	Change from CP25/27 and rationale for change
Financial Ombudsman referral rate for case assessment (consumer dispute)	0%	5%	10%	<p>Firms would be required to assess cases 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 firms, especially for opt-in intervention. We are uncertain on the exact proportion of cases which will be referred to the Financial Ombudsman, however we expect the proportion to be lower than in the counterfactual.</p>	<p>Stakeholder feedback indicated that the Financial Ombudsman referral rate of 7.5% was too high in the intervention scenario. We have revised this estimate downwards to 5% in order to reflect feedback from the Ombudsman that its new charging structure for CMCs has substantially reduced the number of referrals through CMCs and PRs. It is our view that this revised assumption – in line with the referral rate for BSPS – better reflects the expected number of cases that are reviewed by the Financial Ombudsman.</p>

Input	Low	Central	High	Original reasoning in CP25/27	Change from CP25/27 and rationale for change
				<p>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 cases (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 firms 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.</p>	

Input	Low	Central	High	Original reasoning in CP25/27	Change from CP25/27 and rationale for change
Financial Ombudsman referral rate for agreement screening (consumer dispute)	<p>High loan value screening: 12%</p> <p>De-minimis screening: 0%</p> <p>APR screening: 0%</p> <p>UR screening: 0%</p> <p>Limitation period screening: 12%</p>	<p>High loan value screening: 30%</p> <p>De-minimis screening: 0%</p> <p>APR screening: 0%</p> <p>UR screening: 5%</p> <p>Limitation period screening: 30%</p>	<p>High loan value screening: 48%</p> <p>De-minimis screening: 0%</p> <p>APR screening: 0%</p> <p>UR screening: 10%</p> <p>Limitation period screening: 48%</p>	<p>We did not estimate the Financial Ombudsman referral rate for high loan value screening, de-minimis screening, APR screening, and limitation period screening as they were not part of the proposed Scheme.</p> <p>The Financial Ombudsman referral rate for UR screening was estimated to be equal to that at case assessment.</p>	<p>Agreements screened out under the limitation period screening are no longer treated as scheme cases. Thus, the Financial Ombudsman referral rate for these agreements is estimated to be in line with those in the counterfactual as we are now, in effect, treating them as agreements that sit within a case-led scenario. We estimate that the Financial Ombudsman referral rate for these agreements will be slightly higher than the referral rate for agreements in the counterfactual, as consumers will be told that they can explore alternative means to complain (such as through the Financial Ombudsman).</p> <p>We uplift our counterfactual Financial Ombudsman referral rate by 20% in each of the low, central, and high scenarios. Our estimated referral rates are therefore 12% in the low scenario, 30% in the central scenario, and 48% in the high scenario.</p> <p>We also apply this assumption to agreements that represent high loan values above the 99.5th percentile. These are not considered Scheme cases and can be referred to the Financial Ombudsman.</p>

Input	Low	Central	High	Original reasoning in CP25/27	Change from CP25/27 and rationale for change
					<p>De-minimis and APR: Agreements screened out under commission de-minimis, or zero APR are considered ineligible for a full Financial Ombudsman case assessment (to determine if an agreement is fair and reasonable) under our Scheme design. Thus, the Financial Ombudsman referral rate for these agreements under a full case fee of £680 is estimated to be 0% under the low, central, and high estimates.</p> <p>UR screening: The Financial Ombudsman referral rate for agreements that do not proceed to full case assessment is assumed to be equivalent to those that do. This reflects the possibility that some consumers may disagree with a firm's assessment of their eligibility under the Scheme and may therefore refer the matter to the Financial Ombudsman. Our point estimates for the low, central, and high scenarios are therefore 0%, 5%, and 10% respectively.</p>
Financial Ombudsman fee	£650	£680	£680	See "Financial Ombudsman fee" in the counterfactual assumptions.	Following the Financial Ombudsman Service's consultation to increase case fees from £650 to £680, we have reflected this change by updating our cost assumptions accordingly in the central and high cost scenarios. In the low cost scenario, we assume that the case fee remains at £650.

Input	Low	Central	High	Original reasoning in CP25/27	Change from CP25/27 and rationale for change
<p>Average time per agreement for one-off screening (UR screening only)</p>	<p>Calculated at the lender-level using motor finance commission monitoring survey results</p>			<p>The time per agreement for one-off screening measured how long it took to review agreements and determine consumer eligibility, resulting in either a likely eligibility or ineligibility notification. Screening time depended on automation and data availability.</p> <p>For DCA and high commission breaches, screening was largely automated, covering around 11.4m DCA agreements and 2.9m high commission arrangements. The remaining 14% of agreements involved tied arrangements, which required more manual work to locate contracts and verify details, as some firms did not have tied arrangements.</p> <p>Estimated screening times per agreement were 30 minutes in the low (fully automated) scenario, 60 minutes in the central (partially automated) scenario, and 90 minutes in the high (mostly manual) scenario.</p>	<p>We estimate this at the firm-level, using data taken from firms in Round 6 of their motor finance commission monitoring survey responses. We did not ask firms questions around the time taken to screen agreements in Round 5 of the survey.</p> <p>In the high scenario, we double firm's reported responses as a sensitivity check.</p>

Input	Low	Central	High	Original reasoning in CP25/27	Change from CP25/27 and rationale for change
<p>Average time per agreement for one-off screening (High loan value; De-minimis; APR; limitation period)</p>	<p>High loan value screening: 5 minutes</p> <p>De-minimis screening: 5 minutes</p> <p>APR screening: 5 minutes</p> <p>Limitation period screening: 10 minutes</p>	<p>High loan value screening: 5 minutes</p> <p>De-minimis screening: 5 minutes</p> <p>APR screening: 10 minutes</p> <p>Limitation period screening: 15 minutes</p>	<p>High loan value screening: 5 minutes</p> <p>De-minimis screening: 5 minutes</p> <p>APR screening: 15 minutes</p> <p>Limitation period screening: 20 minutes</p>	<p>We did not estimate the time involved with these as they were not part of the proposed Scheme.</p>	<p>High loan value, de-minimis, and APR screening: Under the Scheme’s design features there are exceptions to be applied by firms for high loan value, de minimis commission, and zero APR agreements. As set out in Table 6, the circumstances under which these exceptions apply are discussed in more detail in the Policy Statement.</p> <p>We anticipate that screening for these 3 conditions will generally be a quick process which does not involve in-depth analysis or calculations. Further, Round 6 of our Firm Commission Monitoring Survey suggests that many firms are investing in technology to increase the speed of the UR screening process. We anticipate that could be used to speed up the screening process for excepted agreements.</p> <p>We set our low, central, and high estimates for these screening exercises at 5 minutes each for high loan value and de minimis screening to reflect the quick nature of these checks. For APR screening, we anticipate that these checks could take slightly longer. If a 0% APR is applied, this would be a quick check. However, checking for low APR may require firms to conduct analysis. We therefore estimate that these checks would take 5, 10, and 15 minutes in the low, central, and high scenarios.</p>

Input	Low	Central	High	Original reasoning in CP25/27	Change from CP25/27 and rationale for change
					<p>We apply these assumption to every agreement and multiply this time by the cost of case-screening employees to estimate the costs to firms arising from these screening exercises.</p> <p>Limitation period: Firms will be required to carry out limitation period screening on all agreements at the time they assess whether the case is a Scheme case (that is, before assessing liability or any redress). The time required to complete this screening will vary depending on the type of case and the level of disclosure available. In some cases, firms may need to undertake a more detailed review to determine whether adequate disclosure was provided. On average, we estimate that this screening exercise will take approximately 15 minutes per case, which we use in our central estimate. For our lower and upper bounds, we apply 10 and 20 minutes respectively.</p>
Hourly cost per case-handling employee (eligibility assessment)	Calculated at the lender-level using motor finance commission monitoring survey results			The hourly cost per case-handling employee was taken as a proxy for the cost of case-handling employees.	We estimate this at the firm-level, using data taken from firms in Round 6 of their motor finance commission monitoring survey responses. We did not ask firms questions around the time taken to screen agreements in Round 5 of the survey. We uplift this by 21% to account for overheads.

Input	Low	Central	High	Original reasoning in CP25/27	Change from CP25/27 and rationale for change
One-off investment for handling cases	Calculated at the lender-level using motor finance commission monitoring survey results			<p>In August 2025, we asked firms to consider their estimated one-off investments they would expect to make in systems, capital, or infrastructure specifically to manage a potential CRS.</p> <p>These responses are considered at the lender-level. Firms provided wide ranging estimates of how much they expect to invest in systems to enhance their case-handling capabilities.</p>	We have updated our estimates to reflect data from Round 6 (January 2026) of our motor finance commission monitoring survey.
Case-handling full-time equivalent (FTE) employees	Calculated at the lender-level using motor finance commission monitoring survey results			<p>In August 2025, we asked firms to consider how many FTE staff they expect to allocate to handling case for a potential consumer redress scheme following the Supreme Court decision.</p> <p>These are considered at the lender-level, as different firms have different numbers of agreements and expect to receive different numbers of case.</p>	We have updated our estimates to reflect data from Round 6 (January 2026) of our motor finance commission monitoring survey.
Hourly cost per case-handling employee (redress determination)	Calculated at the lender-level using motor finance commission monitoring survey results			<p>In August 2025, we asked firms to consider the average total hourly cost per staff member assigned to complaint handling involved in a potential consumer redress scheme following the Supreme Court decision.</p> <p>These are considered at the lender level, as case-handling employees at different firms have differing wages. For example, some firms may choose to outsource case-handling employees which would reduce this cost.</p>	We have updated our estimates to reflect data from Round 6 (January 2026) of our motor finance commission monitoring survey.

Input	Low	Central	High	Original reasoning in CP25/27	Change from CP25/27 and rationale for change
Time taken to process a complaint	Calculated at the lender-level using motor finance commission monitoring survey results			<p>In August 2025, after the Supreme Court decision, we asked firms to consider estimated average time to process a single complaint within a potential consumer redress scheme, from initial receipt to final resolution.</p> <p>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 firms. For instance, firms with a low proportion of agreements which are owed redress may be able to identify cases which are owed redress faster.</p>	We have updated our estimates to reflect data from Round 6 (January 2026) of our motor finance commission monitoring survey.
Proportion of complaints submitted through PRs/CMCs	12%	27.88%	43.75%	<p>In the consultation, we assumed a 'central' estimate 44% in the intervention scenario. This was estimated as the mid-point of our 'high' estimate of 76%, the 'central' estimate in the consultation, and our 'low' estimate of 12%. The 12% is taken from the FCA's <u>2024 Financial Lives Survey</u> evidence on the proportion of consumers with low financial capability. We approximate this as a minimum proportion of consumers who may require support in navigating a counterfactual case-handling process.</p>	<p>Our 'high' estimate is now 44%, our updated 'central' estimate in the counterfactual. Our 'low' estimate remains 12% from the FCA's 2024 Financial Lives Survey evidence on the proportion of consumers with low financial capability. Our 'central' estimate of 28% is the mid-point of these two.</p>

Modelling details

8.39 The steps involved in the modelling of lender administrative costs (including court costs) and Financial Ombudsman related fees for the counterfactual scenario are described in the table below.

Table 33: 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)	<p>Calculate number of cases received by firms</p> <p>Use complaint incidence rate assumptions to estimate the total number of cases received by firms.</p>	<p>Complaint incidence rate (low case 48%, central case 58%, high case 68%)</p> <p>Lender level MF agreement numbers (32.9m total market wide)</p>	19.0m cases received by firms
2)	<p>Calculate lender complaint handling capacity</p> <p>Use key inputs to estimate the number of cases that can be handled by each lender within the 8-week response deadline:</p> <ul style="list-style-type: none"> • Lender-level FTE working on complaint handling • Lender-level complaint handling time estimates • Variable administrative cost per complaint 	<p>Accurate lender self-reporting of FTE working on cases, time taken to process a complaint and average hourly staff wage.</p> <p>Complaints received across 36 months, with a quarter in the first 6 months, a quarter in the last 6 months, and the remaining half across the remaining 24 months. Complaints are received at a constant rate within each of these 3 periods.</p>	<p>6.0m cases not handled by firms within 8-week deadline (but note some are handled by firms after this time; see below)</p> <p>17.2m cases handled by firms (13.0m handled within the 8-week deadline, 4.2m handled after the 8-week deadline)</p>
3)	<p>Calculate lender administrative costs</p> <p>Estimate the variable administrative costs incurred by each lender using:</p> <p>Total number of cases handled</p> <ul style="list-style-type: none"> • Lender-level complaint handling time estimates • Lender-level hourly staff wage • Lender-level one-off investment estimates • Lender-level FTE employees 	<p>Accurate lender self-reporting of time taken to process a complaint, one-off investment costs required to scale up, and average hourly staff wage.</p> <p>Variable administrative cost per complaint non-labour cost and complexity uplift is 21% in all cases.</p>	<p>£582m variable administrative costs</p> <p>£274m one-off investments</p>

Step	Description/Method	Assumptions	Market-wide central case output (nominal)
4)	<p>Calculate Financial Ombudsman referrals and Financial Ombudsman referral fees from time out</p> <p>Estimate the number of cases that get referred to Financial Ombudsman as a result of not being handled by firms within the 8-week response deadline, using:</p> <ul style="list-style-type: none"> • Total number of cases not handled within the 8-week deadline • Financial Ombudsman referral rate from out of time 	<p>Financial Ombudsman referral rate from out of time (10% low case, 30% central case, 50% high case)</p> <p>Financial Ombudsman referral fee is £680 in all cases</p>	<p>1.8m Financial Ombudsman referrals from time out</p> <p>£1.2bn Financial Ombudsman referral fees from time out (1.8m *£680)</p>
5)	<p>Calculate Financial Ombudsman referrals and Financial Ombudsman referral fees from consumer dispute</p> <p>Estimate the number of cases that are handled by firms and subsequently referred to the Financial Ombudsman by the consumer, using:</p> <ul style="list-style-type: none"> • Total number of cases handled by the lender • Financial Ombudsman referral rate from consumer dispute 	<p>Financial Ombudsman referral rate from consumer dispute (low case 10%, central case 25%, high case 40%)</p> <p>Financial Ombudsman referral fee is £680 in all cases</p>	<p>4.3m Financial Ombudsman referrals from consumer dispute</p> <p>£2.9bn Financial Ombudsman referral fees from consumer dispute (4.3m *£680)</p>
6)	<p>Calculate total Financial Ombudsman related fees</p> <p>Estimate the total Financial Ombudsman related fees incurred by each lender by summing:</p> <ul style="list-style-type: none"> • Financial Ombudsman referral fees from timeout • Financial Ombudsman referral fees from consumer dispute • Financial Ombudsman scaling fees 	<p>Financial Ombudsman scaling fee (£455 low case, £478 central case, £478 high case)</p>	<p>6.1m Financial Ombudsman referrals overall</p> <p>£7.0bn Financial Ombudsman related fees (£2.9bn in Financial Ombudsman scaling fees and £4.1bn in Financial Ombudsman referral fees)</p>

Step	Description/Method	Assumptions	Market-wide central case output (nominal)
7)	<p>Calculate total court costs</p> <p>Estimate total court costs for each lender, using:</p> <ul style="list-style-type: none"> • Firm-level average court fees per motor finance-related court case since January 2019 • Firm-level estimated average cost of external legal representation per motor finance-related court case since January 2019 • Firm-level estimated average cost of any other external expenses associated with a motor finance-related court case since January 2019 • Firm-level average total hourly cost per internal staff member assigned to handling a single motor finance-related court case since January 2019 • Firm-level estimated time involved by internal staff to handle a motor finance-related court case • Firm-level estimates on the number of court cases per year since January 2019 	<p>Accurate lender self-reporting for all questions related to court costs.</p> <p>Estimates since January 2019 are representative of the counterfactual period.</p>	Court costs of £0.1bn
8)	<p>Calculate total lender non-redress costs</p> <p>Estimate market-level total lender non-redress costs by adding together administrative costs, Financial Ombudsman-related fees and court costs.</p>		£8.0bn lender non-redress costs

8.40 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 34: Modelling steps to estimate lender administrative costs and Financial Ombudsman related fees for the intervention scenario – Scheme 1

Step	Description/Method	Assumptions	Market-wide central case output (nominal)
1)	<p>Calculate number of cases that proceed under the CRS following the lender screening process</p> <p>Use agreements and case data and CRS join rate assumptions to estimate the number of cases that proceed to the full case assessment under the CRS via the opt-in process for cases not already made and opt-out process for cases already made.</p> <p>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 cases already registered to firms (and thus, the number not registered to firms).</p>	<p>CRS join rates for opt-in process (scheme cases: 54% low case, 66% central case, 91% high case; non-scheme cases: 10% low case, 16% central case, 22% high case)</p> <p>CRS join rates for opt-out process (scheme cases: 95% low and central cases, 100% high case; non-scheme cases: 0% in all cases)</p>	<p>9.3m cases (7.4m through opt-in process and 1.9m through opt-out process for cases already received)</p>

Step	Description/Method	Assumptions	Market-wide central case output (nominal)
2)	<p>Calculate lender screening costs</p> <p>Use key inputs to estimate the lender-level screening costs incurred (ie costs associated with screening agreements to determine if they are likely to be eligible for redress or not), using:</p> <ul style="list-style-type: none"> • Lender-level screening time per agreement • Lender-level number of agreements • Lender-level hourly staff wage for screening agreements 	<p>Accurate lender self-reporting of average hourly staff wage.</p> <p>Average screening time per agreement for high loan value screening: 5 minutes for low, central, and high case.</p> <p>Average screening time per agreement for de-minimis screening: 5 minutes for low, central, and high case.</p> <p>Average screening time per agreement for APR screening: 10 minutes for low, central, and high case.</p> <p>Average screening time per agreement for UR screening: Round 6 survey estimates taken in the low and central cases. We apply a 100% uplift for the high case.</p> <p>Average screening time per agreement for high loan value screening: 15 minutes for low, central, and high case.</p>	<p>£112m screening costs</p>
3)	<p>Calculate lender variable administrative costs</p> <p>Estimate the variable administrative costs associated with full case handling incurred by each lender using:</p> <ul style="list-style-type: none"> • Lender-level number of cases to be assessed after applying CRS uptake rates • Lender-level complaint handling time estimates • Lender-level hourly staff wage 	<p>Lender self-reporting of time taken to process a complaint and average hourly staff wage is accurate.</p> <p>Variable admin cost per complaint non-labour cost and complexity uplift: 21% in all cases</p>	<p>£129m variable administrative costs</p>

Step	Description/Method	Assumptions	Market-wide central case output (nominal)
4)	<p>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 6)</p>	<p>Accurate lender self-reported one-off investments to scale up for a CRS</p>	<p>£81m one-off investment costs</p>
5)	<p>Calculate total administrative costs Estimate market-wide administrative costs by summing together screening costs, variable administrative costs and one-off investment costs</p>		<p>£323m total administrative costs</p>

Step	Description/Method	Assumptions	Market-wide central case output (nominal)
6)	<p>Calculate Financial Ombudsman referrals and Financial Ombudsman referral fees from consumer dispute</p> <p>Estimate the number of cases that are considered by firms and subsequently referred to the Financial Ombudsman by the consumer using:</p> <ul style="list-style-type: none"> • Total number of cases handled by the lender under the CRS • Total number of cases already received but deemed non-scheme cases at screening stage • Financial Ombudsman referral rate from consumer disagreeing with the outcome at each stage of the process (screening through to case assessment) 	<p>Financial Ombudsman referral rate from consumer disagreeing with case assessment: 0% low case, 5% central case, 10% high case</p> <p>Financial Ombudsman referral rate from consumer disagreeing with high loan value screening: 12% low case, 30% central case, 48% high case</p> <p>Financial Ombudsman referral rate from consumer disagreeing with de-minimis screening: 0% low case, 0% central case, 0% high case</p> <p>Financial Ombudsman referral rate from consumer disagreeing with APR screening: 0% low case, 0% central case, 0% high case</p> <p>Financial Ombudsman referral rate from consumer disagreeing with UR screening: 0% low case, 5% central case, 10% high case</p> <p>Financial Ombudsman referral rate from consumer disagreeing with limitation period screening: 12% low case, 30% central case, 48% high case</p> <p>Firms complete all case assessments within time frame, such that no agreements time out</p> <p>Breach rate of cases already received is the same as the overall lender-level breach rate estimate</p>	<p>0.2m Financial Ombudsman referrals from consumer dispute (through both disagreement with final redress determination and initial eligibility screening channels)</p> <p>£154m Financial Ombudsman referral fees (£680 * 0.2m)</p>

Step	Description/Method	Assumptions	Market-wide central case output (nominal)
7)	<p>Calculate “Other firm costs”</p> <p>Estimate costs associated with familiarisation and gap analysis, training and dissemination, IT projects, Board and Executive Committee reviews, and sales, customer, and other changes through our Standardised Cost Model.</p>	<p>See Appendix 1 of our Statement of Policy on Cost Benefit Analyses for a list of core assumptions used. We apply our regulatory judgement in using assumptions.</p> <p>We use the survey estimates for the number of staff which require training.</p>	<p>Familiarisation and gap analysis: £0.3m</p> <p>Training and dissemination: £1.6m</p> <p>IT projects: £0.0m (accounted for in one-off costs)</p> <p>Board and Executive Committee reviews: £0.2m</p> <p>Sales, customer, and other changes: £0.0m</p>
8)	<p>Calculate total lender non-redress costs</p> <p>Estimate scaled-up market-level total lender non-redress costs by adding together total administrative costs and Financial Ombudsman related fees.</p> <p>*Note it is assumed that firms incur no Financial Ombudsman scaling fees in the intervention scenario.</p>		<p>£0.5bn lender non-redress costs (£0.3bn administrative costs, £0.2bn Financial Ombudsman related fees, and £0.0bn other firm costs)</p>

8.41 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 35: Modelling steps to estimate lender administrative costs and Financial Ombudsman related fees for the intervention scenario – Scheme 2

Step	Description/Method	Assumptions	Market-wide central case output (nominal)
1)	<p>Calculate number of cases that proceed under the CRS following the lender screening process</p> <p>Use agreements and case data and CRS join rate assumptions to estimate the number of cases that proceed to the full case assessment under the CRS via the opt-in process for cases not already made and opt-out process for cases already made.</p> <p>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 cases already registered to firms (and thus, the number not registered to firms).</p>	<p>CRS join rates for opt-in process (scheme cases: 59% low case, 71% central case, 96% high case; non-scheme cases: 15% low case, 21% central case, 27% high case)</p> <p>CRS join rates for opt-out process (scheme cases: 95% low and central cases, 100% high case; non-scheme cases: 0% in all cases)</p>	<p>23.6m cases (18.8m through opt-in process and 4.9m through opt-out process for cases already received)</p>

Step	Description/Method	Assumptions	Market-wide central case output (nominal)
2)	<p>Calculate lender screening costs</p> <p>Use key inputs to estimate the lender-level screening costs incurred (ie costs associated with screening agreements to determine if they are likely to be eligible for redress or not), using:</p> <ul style="list-style-type: none"> • Lender-level screening time per agreement • Lender-level number of agreements • Lender-level hourly staff wage for screening agreements 	<p>Accurate lender self-reporting of average hourly staff wage.</p> <p>Average screening time per agreement for high loan value screening: 5 minutes for low, central, and high case.</p> <p>Average screening time per agreement for de-minimis screening: 5 minutes for low, central, and high case.</p> <p>Average screening time per agreement for APR screening: 10 minutes for low, central, and high case.</p> <p>Average screening time per agreement for UR screening: Round 6 survey estimates taken in the low and central cases. We apply a 100% uplift for the high case.</p> <p>Average screening time per agreement for high loan value screening: 15 minutes for low, central, and high case.</p>	£299m screening costs
3)	<p>Calculate lender variable administrative costs</p> <p>Estimate the variable administrative costs associated with full case handling incurred by each lender using:</p> <ul style="list-style-type: none"> • Lender-level number of cases to be assessed after applying CRS uptake rates • Lender-level complaint handling time estimates • Lender-level hourly staff wage 	<p>Lender self-reporting of time taken to process a complaint and average hourly staff wage is accurate.</p> <p>Variable admin cost per complaint non-labour cost and complexity uplift: 21% in all cases</p>	£283m variable administrative costs

Step	Description/Method	Assumptions	Market-wide central case output (nominal)
4)	<p>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 6)</p>	<p>Accurate lender self-reported one-off investments to scale up for a CRS</p>	<p>£193m one-off investment costs</p>
5)	<p>Calculate total administrative costs Estimate market-wide administrative costs by summing together screening costs, variable administrative costs and one-off investment costs</p>		<p>£774m total administrative costs</p>
6)	<p>Calculate Financial Ombudsman referrals and Financial Ombudsman referral fees from consumer dispute Estimate the number of cases that are considered by firms and subsequently referred to the Financial Ombudsman by the consumer using:</p> <ul style="list-style-type: none"> • Total number of cases handled by the lender under the CRS • Total number of cases already received but deemed non-scheme cases at screening stage • Financial Ombudsman referral rate from consumer disagreeing with the outcome at each stage of the process (screening through to case assessment) 	<p>Financial Ombudsman referral rate from consumer disagreeing with case assessment: 0% low case, 5% central case, 10% high case Financial Ombudsman referral rate from consumer disagreeing with high loan value screening: 12% low case, 30% central case, 48% high case Financial Ombudsman referral rate from consumer disagreeing with de-minimis screening: 0% low case, 0% central case, 0% high case Financial Ombudsman referral rate from consumer disagreeing with APR screening: 0% low case, 0% central case, 0% high case Financial Ombudsman referral rate from consumer disagreeing with UR screening: 0% low case, 5% central case, 10% high case</p>	<p>0.6m Financial Ombudsman referrals from consumer dispute (through both disagreement with final redress determination and initial eligibility screening channels) £393m Financial Ombudsman referral fees (£680 * 0.6m)</p>

Step	Description/Method	Assumptions	Market-wide central case output (nominal)
		<p>Financial Ombudsman referral rate from consumer disagreeing with civil limitations screening: 12% low case, 30% central case, 48% high case</p> <p>Breach rate of cases already received is the same as the overall lender-level breach rate estimate</p>	
7)	<p>Calculate “Other firm costs”</p> <p>Estimate costs associated with familiarisation and gap analysis, training and dissemination, IT projects, Board and Executive Committee reviews, and sales, customer, and other changes through our Standardised Cost Model.</p>	<p>See Appendix 1 of our Statement of Policy on Cost Benefit Analyses for a list of core assumptions used. We apply our regulatory judgement in using assumptions.</p> <p>We use the survey estimates for the number of staff which require training.</p>	<p>Familiarisation and gap analysis: £0.5m</p> <p>Training and dissemination: £3.1m</p> <p>IT projects: £0.0m (accounted for in one-off costs)</p> <p>Board and Executive Committee reviews: £0.3m</p> <p>Sales, customer, and other changes: £0.0m</p>
8)	<p>Calculate total lender non-redress costs</p> <p>Estimate scaled-up market-level total lender non-redress costs by adding together total administrative costs and Financial Ombudsman related fees.</p> <p>*Note it is assumed that firms incur no Financial Ombudsman scaling fees in the intervention scenario.</p>		<p>£1.2bn lender non-redress costs (£0.8bn administrative costs, £0.4bn Financial Ombudsman related fees, and £0.0bn other firm costs)</p>

Adapting our assumptions on the counterfactual complaints-handling period

- 8.42** As part of our sensitivity analysis, our revised counterfactual (9-year complaints handling period) employs an equivalent methodology to that in the central scenario counterfactual. All assumptions are also equivalent to those used in the counterfactual for the low, central, and high scenarios respectively, except for two key changes. The key changes are around the flow of complaints to lenders, and the application of the FOS scaling fee.
- 8.43** We assume that a quarter of complaints are submitted at a constant rate within the first year, a quarter are submitted at a constant rate in the last year (year 9), and the remaining half are submitted at a constant rate between years 2 and 8. This enables us to model the peaks and troughs in complaint submissions which firms are likely to experience, providing an indication of the impact on complaint time-outs over time (and thus, the number of complaints which may become eligible for Financial Ombudsman referral through the out-of-time channel).
- 8.44** We apply the FOS scaling fees to cases referred to the FOS during the two peak periods (years 1 and 9). All cases referred to the FOS in years 2 to 8 do not have a scaling fee applied.

Further modelling considerations

Estimating court costs in the counterfactual

Firm inputs

- 8.45** The results of the Round 6 motor finance commission monitoring survey inform our cases. As part of the survey, we asked firms and banks the following questions:
- Since January 2019, what is your firm's estimated average court fees per motor-finance-related court case? (£)
 - Since January 2019, what is your firm's estimated average external legal-representation cost per case? (£)
 - Since January 2019, what is your firm's estimated average cost of any other external expenses associated with a motor-finance-related court case? (£)
 - Firms excluded court fees and legal-representation costs from this category.
 - Since January 2019, what is your firm's average total hourly cost per internal staff member assigned to handling court cases? (£)
 - How much total internal staff time is spent on an average motor-finance-related court case? (minutes)
 - Since January 2019, how many motor-finance-related court cases per year has your firm dealt with? (No.)

8.46 As part of our data cleaning approach, we removed responses with non-usable or incomplete returns. We were able to use 27 of 32 firms in the sample (84.4%) and 3 of the 5 (60%) banks surveyed. We retained all usable data points, including small or high values, given the limited number of historic court cases and the likelihood that current-state litigation patterns do not reflect potential future states.

8.47 For the CBA, we do not include avoided court costs for firms that lose cases. These firms are treated as non-compliant and would not realise litigation savings in practice. Our analysis therefore focuses on firms that successfully defend claims, for whom the CRS would reduce:

- external legal costs
- court fees
- internal resource use.

8.48 To estimate total court-cost impacts, we multiplied firms' reported external costs and internal labour-time costs by their reported annual number of court cases. This produced a unit court-case cost, and total cases per year, presented below.

Table 36: Summary of annual court related volumes and costs

Total unit cost per year	£245,147
Total court cases per year	2,278

8.49 The total unit cost represents the aggregate annual costs to firms of litigating motor finance commission cases through the courts. The total court cases reflect the total number of cases currently progressing through the courts each year. Together, these figures indicate that annual court volumes are low relative to the size of the market, but each case is expensive to defend and may create a high per case cost burden.

8.50 To test this further, the table below summarises the sensitivity analysis used to test how total costs scale with different case-volume assumptions. We report the corresponding annual and 5-year nominal costs for each scenario.

Table 37: Sensitivity analysis firm court cases volumes and costs (nominal).

Scenario	Court cases	Annual Cost (£) (Nominal)	5 Year Cost (£) (Nominal)
50%	1,139	10,738,521	53,692,605
Base-case	2,278	21,477,042	107,385,209
150%	3,417	32,215,563	161,077,814
200%	4,556	42,954,084	214,770,418
1000%	22,781	214,770,418	1,073,852,091

8.51 A breakeven scenario shows that case volumes would need to be around 10 times the base-case assumption to have any material effect on the NPV. At this level, total 5-year nominal costs would exceed £1bn, whereas current firm-reported court-case volumes are far too low to generate a meaningful impact.

Consumer inputs

8.52 We did not include in the PS the potential costs to consumers due to data limitations, however, for completeness, we illustratively model what a consumer litigation pathway could look like.

8.53 There is significant uncertainty regarding the specific pathway through which consumers would choose to take a claim to court. To reflect this uncertainty, we consider the following three illustrative scenarios to represent variations in expected costs.

- **Low-cost scenario:** The consumer represents themselves in court, incurring only court fees and the opportunity cost of their time.
- **Central scenario:** The consumer typically uses a CMC or partial legal support, adding up to an additional 30% of redress in fees.
- **High-cost scenario:** The consumer engages full legal representation, incurring significantly higher legal fees and indirect costs.

8.54 In the low-cost scenario, consumers pursue small-claims cases without legal representation. Court fees are modest and vary by claim value, and we assume consumers litigate only where expected redress exceeds the average award (£785). Based on ICF data, consumers spend around 19 hours engaging with the small-claims process, which we value at £373 using the 2025 median wage. Data suggests 64% of cases are estimated to result in a redress award.

8.55 In the central scenario, the consumer uses representation a CMC operating on a no-win-no-fee basis, typically taking 30% of the redress recovered. Responses from 30 firms indicate that, on average, 75.5% of cases received to date have been submitted by PRs/CMCs. Even with representation, consumers still incur time costs, which we estimate at 6 hours on average.

8.56 In the high-cost scenario, cases requiring solicitors involve higher direct costs: updated ICF estimates imply fees of around £642, with similar residual time requirements. Group litigation can reduce per-claim fixed costs through cost-sharing but may extend timelines and dilute individual awards; given this uncertainty, it is noted qualitatively rather than modelled.

Table 38: Consumer court costs by scenario

	Low-cost scenario	Central scenario	High-cost scenario
Share of court cases decided in consumer's favour and redress (expected value)	63.7%	63.7%	63.7%
Direct and indirect cost to consumer (£)	£75 + opportunity cost of time (t=19)	£75 + 30% CMC involvement + Opportunity cost of time (t=6)	£75 + £642 legal fees + Opportunity cost of time (t=6)
Redress (£)	£785.37	£785.37	£785.37
Net expected benefit (redress–costs)	£52.17	£157.86	(-) £334.12

8.57 Overall, the scenarios highlight that while the probability of a successful outcome and the expected redress is constant, the costs associated with pursuing court action drive markedly different consumer outcomes. Self-representation yields only a marginal gain once time costs are included, and solicitor-led cases result in a negative expected return.

8.58 Only CMC-supported cases generate a meaningfully positive net benefit, even after representative fees. Taken together, the analysis shows that for most consumers, the economics of court-based redress are weak, reinforcing the value of alternative, lower-cost routes for resolving motor-finance disputes.

Consideration of postage costs

8.59 In CP25/27, we did not estimate the postage costs to firms of sending prescribed communication letters to consumers. As our rules are now more flexible and firms can choose their means of communication, the associated postage costs of recorded delivery are not treated as a cost of compliance and are therefore not included in the updated CBA.

8.60 For completeness, we estimate the postage costs of recorded delivery to update our consultation CP25/27 CBA estimate for comparison against the Policy Statement estimates provided in our updated CBA.

8.61 To estimate the cost of sending recorded delivery mail, we use the following equation:

$$\begin{aligned} \text{Cost of Sending Recorded Delivery Mail} \\ = \text{Postage Cost per Letter} * \text{CRS Join Rate} * \text{Number of Agreements} \end{aligned}$$

8.62 In our analysis, we assume that:

- Firms will be sending small letters (sized up to 240mm x 165mm x 5mm and up to 100g).
- Postage for Royal Mail UK 2nd Class Signed for costs of £2.77 per letter.

- 8.63** We have assumed that consumers are engaged with the process and respond to firm communications upon receipt, removing the need for firms to issue follow-up correspondence. In practice, engagement is likely to be lower and require additional reminders, which would increase overall postage costs beyond what we have estimated. Nonetheless, this represents a conservative estimate of postage costs under a mandatory recorded delivery approach. Firms may also incur costs to validate consumer contact details.
- 8.64** Our estimates may slightly overestimate the cost per letter, as firms could use cheaper mailing houses or receive discounts for sending post in bulk or for using barcode technology. However, this is less applicable for smaller firms.
- 8.65** Our original consumers communication requirements would have increased non-redress costs to firms. Under our communications proposals in CP25/27 and updated scheme requirements from the PS, we estimate postage costs to firms of £44.3m in Scheme 1 and £106.2m in Scheme 2, totalling £150.5m.
- 8.66** If all firms chose to send postal communications under our updated scheme requirements, the cost figures fall to £18.7m in Scheme 1 and £41.7m in Scheme 2, totalling £60.4m. Although these figures are modest relative to the overall estimated nominal non-redress liabilities of £0.41bn in Scheme 1 and £0.90bn in Scheme 2, our original requirements could have had a disproportionate impact on smaller firms with limited redress liabilities.

Limitations of the model

- 8.67** We provide a list of limitations below. We do not list any limitations which are equivalent to those listed in CP25/27, such that this section sets out new limitations and previous limitations which have been resolved in whole or in part.

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

- 8.68** In the CP, where firm responses were missing in the motor finance commission monitoring survey, we used the firm's previous survey responses in their place. Where these were also missing, we used the median value from all lenders who provided a response to each question. To improve the accuracy of these imputed values, we now take the stratum-level median, as firms within each stratum are more likely to have similar responses to the overall median.

Use of the motor finance commission monitoring survey in the counterfactual

- 8.69** We use the Round 6 motor finance commission monitoring survey responses for the counterfactual scenario, where we had previously used the Round 4 responses. We deemed the Round 6 survey responses more appropriate because the Round 4 survey was asked both prior to the Supreme Court decision, and prior to CP25/27.

Assumptions around the time taken to conduct eligibility screening checks on agreements

- 8.70** In the CP, we had assumed that firms would take around 60 minutes to screen each agreement for UR conditions. Our motor finance commission monitoring survey shows that firms expect to take less time than we previously expected when screening agreements (the median response was 4 minutes). Several firms mentioned that one-off investments are reduce the time required to screen agreements due to investing in software or processes which automate screening processes.
- 8.71** The policy now includes 5 rounds of screening, one of which being the UR screening referred to in the prior paragraph. We have estimated the time each of the remaining 4 rounds of screening take through mapping out the steps which firms would need to take determine whether an agreement passes or fails each screening exercise. We did not ask firms for estimates on the time that they will take to conduct these additional exercises.
- 8.72** Given that the estimates are at the market level, firm-level investments and processes are not accounted for. For example, a firm which has almost fully automated the UR screening process may experience a positive externality through being able to use similar automation processes for some of the other screening processes without incurring significant costs.

Redress model data limitations

- 8.73** Our non-redress model relies on outputs from the redress model. Any limitations around the number of agreements which breach each round of screening, the number of agreements which are eligible for redress, and the number of agreements in total also apply to the non-redress model.

Application of the CRS join rate

- 8.74** Our non-redress intervention model applies the CRS join rate after the limitation period screening exercise. As a result, we assume that firms assess all agreements which pass the APR screening exercise for limitation period. In reality, firms will only have to assess agreements for limitation period if the associated consumer has opted into the scheme at that stage. This likely leads to a slight overestimation of Financial Ombudsman referral costs and screening costs for limitation period checks.

Chapter 9

Glossary

Table X – Loan level data Variable Description

Name	Field Definition & Criteria	Data item format
Lender FRN	Lender FCA register reference number	Integer
Loan Identifier	Unique identifier for the loan. The loan ID should not change through the life of the transaction. If the original loan ID cannot be maintained in this field, enter the original ID followed by the new ID, comma delimited	Positive integer; no commas, decimal points or other non-numeric characters
Loan Officer Identifier	Unique identifier assigned to the loan officer responsible for originating the loan. This ID should remain consistent across all loan transactions associated with the same officer to ensure accurate tracking and analysis. The identifier should be anonymised to protect individual privacy, yet distinct enough to differentiate between different loan officers.	Positive integer; no commas, decimal points or other non-numeric characters
Customer postcode	Customer full postcode at the time of entering into the credit agreement. If the full postcode is unavailable, the outer postcode will suffice.	Character string containing valid postcode without any spaces (eg E201JN)
Customer date of birth	Customer date of birth	In format YYYY-MM-DD that can be converted to a valid date (eg 2021-12-31)
Customer employment status	Customer employment status at the time of entering into the credit agreement	Integer: 1 = Employed; 2 = Self-employed; 3 = Student ; 4 = Retired ; 5 = Unemployed, 6 = Unknown
Customer annual gross income	Customer gross income at the time of entering into the credit agreement	Integer in range 0-20000000000; no commas, decimal points or other non-numeric characters

Name	Field Definition & Criteria	Data item format
Loan Status Category	Current status of account	Integer: 1 = Performing; 2 = Non-performing (Arrears, Default or Foreclosure); 3 = Redeemed; 4 = Sold off to debt collector; 5 = Other
Origination Channel Category	Origination channel of the agreement	Integer: 1 = Franchised Motor Dealer; 2 = Independent Motor Dealer; 3 = Online Car and Finance Broker; 4 = Online Finance Only Broker; 5 = Other
Credit Broker Name	Name of credit broker. Credit brokers include motor dealers. Please provide the name of the dealership Group in such cases. If not sold through broker, then enter 'NA'	Free text
Credit Broker FRN	Credit broker FCA register reference number	Integer
Vehicle Manufacturer Name	Name of the vehicle manufacturer	Free text
Vehicle Condition Category	Condition of the vehicle when purchased on the current finance agreement	Integer: 1 = New (New cars are those with zero or delivery mileage); 2 = Used (Cars with at least one previous owner)

Name	Field Definition & Criteria	Data item format
Motor Finance Product Category	Type of the motor finance agreement	Integer: 1 = Hire Purchase; 2 = Balloon; 3 = Loan; 4 = Personal Contract Purchase ; 5 = Other; 6 = No Data; 7 = Lease
Loan Origination Date	Date of original loan advance	In format YYYY-MM-DD that can be converted to a valid date (eg 2021-12-31)
Loan Term	Original contractual term (number of months)	Integer
Deposit Amount	Amount of deposit/downpayment on origination of loan (this should include the value of traded-in vehicles etc.)	Numeric in £ ; no commas, can include decimals
Balloon Payment Amount	The balloon payment amount or the guaranteed minimum future value, set at the start of the agreement	Numeric in £ ; no commas, can include decimals
Purchase price of vehicle (£)	The purchase price of the vehicle, this includes all other add-ons and delivery costs, and is net of any deductions eg deposit contributions	Numeric in £; no commas, can include decimals
Discount Applied	Total discounts received by the customer	Numeric in £; no commas, can include decimals
Original Loan Principal	The total credit value of the agreement. This should be the opening balance of the credit agreement, net of deposit (AA17) and discount (AA20), excluding any interest and interest bearing fees (AA36)	Numeric in £; no commas, can include decimals
Annual Percentage Rate	Annual Percentage Rate (APR). The yearly cost of the loan, including interest, insurance and the origination fee, expressed as a percentage.	Numeric in decimals (eg 0.04 if you charge a 4% fee)
Interest Calculation Method	Interest calculation method used to calculate the amount of interest paid by the customer	Integer: 1 = Simple; 2 = Compound; 3 = Fixed; 4 = Other

Name	Field Definition & Criteria	Data item format
Agreed Regular Payment Amount	Amount of agreed regular payment	Numeric in £ ; no commas, can include decimals
Frequency of Repayment	How often regular repayment amount is due	Integer: 1 = Weekly; 2 = Fortnightly; 3 = Monthly; 4 = Quarterly; 5 = Annually; 6 = Other
Credit Score	Borrower's credit score at the time of entering into the credit agreement	Numeric in range 0-2000000000 ; no commas, can include decimals
Credit Score Bureau Category	Provider of credit score	Integer: 1 = Callcredit; 2 = Experian; 3 = Equifax; 4 = TransUnion; 5 = Internal; 5 = Other; 6 = None
Credit Score Date	The date of the bureau score for this borrower	In format YYYY-MM-DD that can be converted to a valid date (eg 2021-12-31)
Commission Model Category	Commission model used to calculate the commission payments made to the broker	Integer: 1 = Increasing difference in charges; 2 = Reducing difference in charges; 3 = Scaled commission; 4 = Flat fee commission; 5 = Profit sharing; 6 = Portfolio Remuneration; 7 = Other; 8 = None

Name	Field Definition & Criteria	Data item format
Broker Finance Commission (£)	Credit broker actual commission paid expressed in (£)	Numeric in £; no commas, can include decimals
Broker Recommended APR (%)	Credit broker contracted recommended APR. Where there is no contracted recommended APR, then provide the credit broker average achieved APR for the last complete 12 month period	Numeric in decimals (eg 0.04 if you charge a 4% fee)
Broker Base APR (%)	Credit broker contracted base rate or minimum APR. Ensure that this is an APR type rate which calculates interest costs on the basis of a declining principal. For example please convert flat interest rates which calculates interest costs on basis of constant principal to APR type rates of interest. Please set out your workings and assumptions separately if applicable	Numeric in decimals (eg 0.04 if you charge a 4% fee)
Broker maximum APR (%)	Credit broker contracted capped or maximum APR. Ensure that this is an APR type rate which calculates interest costs on the basis of a declining principal. For example please convert flat interest rates which calculates interest costs on basis of constant principal to APR type rates of interest. Please set out your workings and assumptions separately if applicable	Numeric in decimals (eg 0.04 if you charge a 4% fee)
Broker Volume Bonus (£)	Credit broker volume bonus or head office bonus achieved on the agreement	Numeric in £; no commas, can include decimals
Non interest charges included in total charge for credit (£)	Non interest charges included in total charge for credit eg fees and charges paid by the customer upfront or at the end of the agreement for which no interest is payable.	Numeric in £ ; no commas, can include decimals
Interest charges included in total charge for credit (£)	Interest charges included in total charge for credit eg interest payable, this could also include fees and charges added to the loan balance and not paid upfront	Numeric in £ ; no commas, can include decimals
Total cost of credit (£)	The total amount of customer fees and interest charge. This should be the sum of the non-interest charges (AA35) and interest charges (AA36). For agreements where you cannot separate out the costs, include the combined amount.	Numeric in £ ; no commas, can include decimals
Broker Total Earnings (£)	Catch-all earnings variable for credit broker achieved earnings not included in other categories	Numeric in £ ; no commas, can include decimals

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Pub ref: 2-008567.1

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