FINANCIAL CONDUCT AUTHORITY

Evaluation Paper 25/1: Our ban on contingent charging and other remedies in 2020: effects on market structure, pricing, and uptake of advice

Technical Annex

January 2025

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1 Introduction

1. This Technical Annex is a supplement to our evaluation paper (EP25/1, 'Our ban on contingent charging and other remedies in 2020: effects on market structure, pricing, and uptake of advice'). It presents the methodology and results of our econometric analysis of remedies in PS20/6, 'Pension transfer advice: feedback on CP19/25 and our final rules and guidance'.

2. This annex is structured as follows: we first give an overview of our intervention, followed by a description of the data sources used and the descriptive statistics. We then present a detailed analysis of the causal impact of the ban on contingent charging.

Policy background

3. In July 2019 we published a consultation on the UK defined benefit (DB) transfer advice market in CP19/25, 'Pension transfer advice: contingent charging and other proposed changes'. Our analysis found evidence of significant harm from unsuitable advice and prices too high or quality too low.

4. Our analysis found that the market failure driving these harms was information asymmetry where one party to a transaction is less well informed. This becomes a problem when one party exploits their greater knowledge. In the market for DB transfer advice, the asymmetry give rise to the 'principal-agent' problem, whereby the agent (in this case the adviser) may not be acting in the interest of the principal (the consumer), and the latter is not able to assess the value of the services for which they are paying.

5. In June 2020, we announced a package of remedies in PS20/6, 'Pension transfer advice: feedback on CP19/25 and our final rules and guidance'. The remedies aimed to reduce the harm we identified in CP19/25 and had 6 main elements summarised in Table 1.

6. This evaluation used data already submitted to the FCA and did not require supplementary data requests. To limit the burden on firms, we did not go back to firms where obvious data entry errors were made.

7. For this evaluation, we use the terms 'DB transfer advice' or 'pension transfer advice' to refer to both pension transfers and pension conversions. While these processes are similar, they have distinct differences:
   - Pension transfers generally involve exchanging safeguarded pension benefits for flexible benefits in a different pension scheme.
   - Pension conversions generally involve exchanging safeguarded pension benefits for flexible benefits in the same pension scheme.

Table 1: Summary of remedies and inclusion in the evaluation

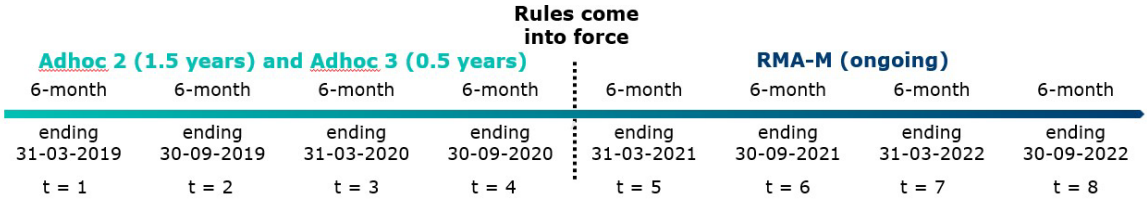
Group	#	Summary of remedy	Included in this evaluation
Contingent charging	1	Banned contingent charging, with exceptions for consumers with certain personal circumstances.	Yes
Abridged advice and triage services	2	Enabled firms to give a short-form of advice (abridged advice)	Yes
		Introduced perimeter guidance on triage services	No
Addressing ongoing conflicts of interest	3	Advisers must consider an available workplace pension as a receiving scheme for a transfer and demonstrate why any alternative is more suitable.	Yes
Empowering consumers	4	Initial disclosure on charging structure. Suitability report and statement of risks, both requiring a client signature. Keep records to demonstrate consumers understand the risks of proceeding with a transfer.	No
Enabling advisers	5	Require pension transfer specialists (PTS) to complete 15 hours of continuing professional development (CPD) each year in addition to any other CPD they undertake.	No
Effective regulation	6	Collect new data on pension transfer advice and amend the existing data we collect on professional indemnity insurance (PII).	Used for evaluation

2 Data and measurement

8. This section provides an overview of the data sources used for the analysis and other measurement issues. For example, time frame of the sample, definitions of treatment and control firms and outcome variables.

9. We used firm-level data on DB and other safeguarded benefit advice from 3 data sources covering 6-month periods from 1 October 2018 to 30 September 2022. This equates to eight 6-month data periods as shown in Figure 1. Each dataset covers all firms (and their appointed representatives) providing regulated DB transfer advice.

Figure 1: Data sources and periods covered



10. The mandatory advice requirement[1] for transferring from a DB to a defined contribution (DC) scheme only applies if the value of benefits exceeds £30,000. In these circumstances, members need to take advice if they are considering transferring out of the DB scheme instead of taking any other option available to them within the scheme.

Data sources

11. Table 2 summarises the data sources used for the evaluation.

12. The first dataset ('Ad hoc 2') was an ad hoc data request sent to 1,965 firms with full permission for DB transfer advice. It covers advice given from 1 October 2018 to 31 March 2020 (three 6-month data periods).

13. The second dataset ('Ad hoc 3') was an ad hoc data request with 1,568 returns from firms with full permission for DB transfer advice. It covers advice given from 1 April 2020 to 30 September 2020 (one 6-month data period).

14. The third dataset was a new section of the RMAR regulatory return (RMA-M), created as part of our intervention. This last data source covers advice given from 1 October 2020 to 30 September 2022 (four 6-month data periods). The data is consistent with the datasets from the previous ad-hoc requests, as it includes the same questions and covers all firms with full (but not limited) permission to advise on pension transfers, conversions, and opt-outs for retail customers.

[1] In 2015, pension freedoms gave DC pension savers more flexibility in how they could access their pension savings. To protect consumers who might otherwise seek to transfer out of DB schemes and other schemes with safeguarded benefits, legislation introduced mandatory advice where the value of the safeguarded benefits given up exceeds £30,000.

Table 2: Data sources

	Ad hoc 2	Ad hoc 3	RMA-M
Pre or post intervention	Pre-intervention	Pre-intervention	Post-intervention
Type	Data request	Data request	Regulatory return
Frequency	6 months	6 months	6 months
Timeframe	1 Oct 2018 - 31 Mar 2020 (3 periods)	1 Apr 2020 - 30 Sep 2020 (1 period)	1 Oct 2020 - 30 Sep 2022 (4 periods)
Same questions?	Yes	Yes	Yes, plus additional questions
Level	Firm-level	Firm-level	Firm-level

Note: 2023 and 2024 RMA-M data is available, but we only examine data until 30 September 2022 in the causal analysis. Some commentary on what occurred in the market in 2023 and 2024 is in Chapter 3 of the main report.
Source: FCA analysis of data requests and regulatory returns

15. Combining the three data sources resulted in a panel dataset with consistent time periods and questions. Specifically, we had 4 data periods before (1 October 2018 to 30 September 2020) and 4 data periods (1 October 2020 to 30 September 2022) after the main rules came into force on 1 October 2020. Although we had data before 1 October 2018 and after 30 September 2022, we decided to limit the timeframe of the analysis to the above eight 6-month periods for the following reasons:
    - The data request covering advice given between April 2015 and 2018 did not collect similar information as the other data requests, hence the data sources could not be easily harmonised[2].
    - External factors in recent years, such as the gilt crisis and subsequent higher interest rates, had a significant impact on the DB transfer market. We provide further details on this issue in Chapter 3 of the main report. Therefore, we did not include more recent data in the analysis.

Key variables

16. Table 3 presents key variables used in the analysis and their definitions. Our dataset included information on several firm-level characteristics. For example, business models and charging structure.

17. Most importantly, firms reported the number of clients provided with DB transfer advice and the number of clients charged contingently and non-contingently at each point in time (see charging structure variables in Table 3). This allows identifying firms providing DB transfer advice and the charging structure that firms applied before and after the ban.

[2] A previous data request was sent to 3,042 firms in October 2018 with the full permission for DB transfer advice covering a 3.5-year period from April 2015 to September 2018. However, this data request did not have questions that were consistent with questions in other data requests after 1 October 2018.

Table 3: Key variables

Category	Variable	Description
Qualifying question	Provided Advice	Has the firm or its appointed representatives provided DB transfer advice in the reporting period?
Business model	Full DB Transfer Advice	How many retail clients did the firm and its appointed representatives provide with only full pension transfer or conversion advice?
	Abridged Advice	How many retail clients did the firm and its appointed representatives provide with abridged advice?
	Pension Transfer Specialists (PTS)	How many pension transfer specialists were employed by, or working under the responsibility of, the firm and its appointed representatives at the end of the reporting period?
Personal recommendations to transfer	Personal Transfer	How many retail clients did the firm and its appointed representatives provide with a personal recommendation to transfer their pension?
	Total Transfer Value	What was the total transfer value of the clients provided with a recommendation to transfer?
	Total Revenue	What was the total revenue derived from initial advisory charges including advice on the investment proceeds of the clients provided with a recommendation to transfer?
	Carve Out	How many satisfied the requirement for one or more of the exceptions to the ban on contingent charging and so charged in full or partially on a contingent basis?
Personal recommendations not to transfer	Personal Not Transfer	How many retail clients did the firm and its appointed representatives provide with a personal recommendation not to transfer their pension?
	Not To Transfer Abridged	How many retail clients did the firm and its appointed representatives provide with a personal recommendation not to transfer their pension after receiving abridged advice?
	Not To Transfer Value	From Personal Not Transfer, what was the total transfer value of the clients provided with a recommendation not to transfer?
	Not To Transfer Initial	From Personal Not Transfer, what was the total revenue derived from initial advisory charges of the clients provided with a recommendation not to transfer?
	Not To Transfer Revenue	From Not To Transfer Abridged, what was the total revenue derived from abridged advice on pension transfers?
	Insistent Client Basis	For how many retail clients did the firm arrange a pension transfer on an insistent client basis after providing full DB transfer advice?
Ongoing services	Arrange Pension Transfer	How many retail clients did the firm arrange a pension transfer for?
	Agreed Ongoing Service	Of the retail clients that the firm arranged a pension transfer for, how many agreed to an ongoing advice service provided by the firm or its appointed representatives?
Charging structure	Contingent Charge	How many retail clients were advised under a charging structure which meant the advisory charge was only payable if the retail client proceeded with the transfer? (Charging fully or partially contingent on a transfer taking place).
	Non-contingent Charge	How retail clients many were advised under a charging structure which meant that the advisory charge remained the same whether or not the retail client proceeded with the transfer? (Charging completely non-contingent)

Category	Variable	Description
Product & investment solutions	Ongoing Product Charges Less	How many retail clients proceeded to transfer or convert into an investment solution that had annual ongoing product and investment charges (excluding ongoing advice charges) of 0.75% or less?
	Ongoing Product Charges Between	How many retail clients proceeded to transfer or convert into an investment solution that had annual ongoing product and investment charges (excluding ongoing advice charges) of more than 0.75% and less than or equal to 1.5%?
	Ongoing Product Charges More	How many retail clients proceeded to transfer or convert into an investment solution that had annual ongoing product and investment charges (excluding ongoing advice charges) of more than 1.5%?
	Transfer To Workplace Pension	How many retail clients proceeded to transfer into a workplace pension?

Data cleaning and sample construction

18. We carried out the following tasks on the combined dataset:
    - An initial data review and quality assessment of each firm response and importing data into statistical software.
    - An in-depth objective and subjective review and cleaning of all responses. This included but was not limited to:
      - Establishing common variable formation.
      - Distinguishing between zeroes and missing values and making corrections where appropriate.
      - Examining the distribution of variables to detect possible errors.
      - Removing duplicated observations.
      - Correcting (whenever possible) obvious data entry errors or removing observations for which data entry errors could not be recoded.
      - Removing entries where firms had not provided more than zero customers with full DB transfer advice or abridged advice. That is, removing entries where the firm submitted a regulatory return but did not provide regulated DB transfer advice.

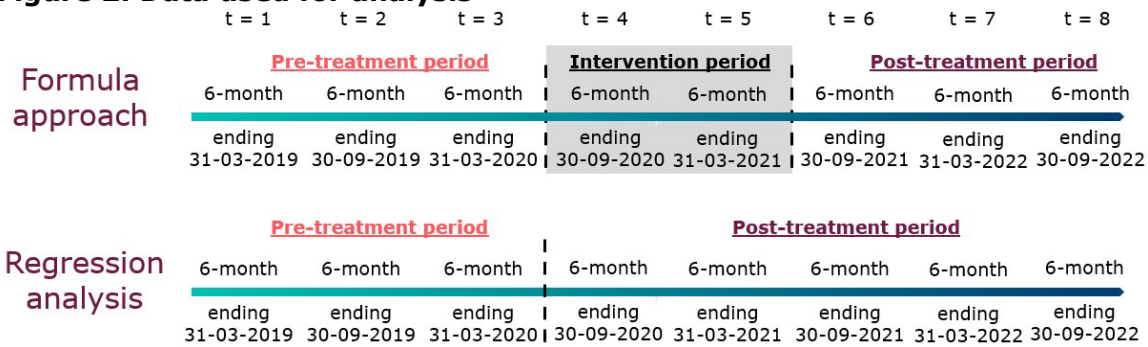
19. This process reduced the number of observations from 7140 to 6187, a reduction of 953 observations. The majority of this reduction resulted from removing submissions from firms that did not provide a customer with regulatory DB transfer advice in the reporting period.

Timeframe of the sample

20. The data periods used for calculating the formula approach and regressions outlined in the Design section of Chapter 4 are detailed in Figure 2.

21. In paragraphs 69 to 73, we detail our quantitative and qualitative reasons for including period t = 4 in the intervention period (for the formula approach) and post-treatment periods (for all our regression analysis). This inclusion is notable because it suggests that firms are responsive to the announcement of a ban. Intuitively, this makes sense because firms are likely to take pre-emptive action and exit the market upon hearing the announcement, rather than waiting for the rule to come into effect.

Figure 2: Data used for analysis



Note: The grey shaded area is the 'intervention period', which includes the 6-month period where the rules were announced (5 June 2020) and the 6-month period after the rules came into force (1 October 2020).

Definition of treatment and control groups

22. As explained in more detail in the Design section of Chapter 4, our causal analysis is based on the comparison between a 'treatment group' (firms subject to the contingent charging ban), and a 'control group', (firms that were already complying with the ban).

23. We constructed treatment and control firms based on information on the charging structure at the firm level. The treatment group consists of firms who charged at least 1 consumer on a contingent basis in the 2 years prior to intervention. The control group includes only firms that charged no consumers on a contingent basis in the 2 years prior to intervention.

24. Where possible, we imputed missing information on the charging structure of the firms based on firm responses in other time periods prior to intervention. This was done for 261 observations associated with 148 firms in the pre-intervention period. Most of these firms only had 1 period of missing information on the number of customers charged contingently.

25. There were 105 firms that could not be identified as charging contingently or non-contingently before the ban. For 105 of these firms, information on the charging structure was missing for every period before the ban was implemented. Therefore, we could not reliably assign them to the treatment or control group. These firms were removed from the estimation sample, but we add them back as a robustness check in Chapter 4.

26. To understand the implications of removing these 105 firms from the sample, we compared firm-level characteristics of these firms with the total sample, the treatment and the control group (see Table 4). Firms who did not report their charging structure appear similar to the rest of the sample along all characteristics, with the exception of the number of consumers provided with full DB transfer advice, the number of PTS and the size of pension transfer values. This suggests that larger firms were less likely to report information on their charging structure. Although not significantly different from the total sample, DB transfer revenue, along with some other firm characteristics, appeared to be more similar to the treatment group than the control group. This may suggest the missing observations that could not be identified were more likely to be large firms who charged on a contingent basis. Therefore, we also conduct a robustness check where we assumed all our excluded firms were charging contingently (see Robustness checks section in Chapter 4).

Qualitative information from our Supervision teams also suggest that the vast majority of these firms were charging on a contingent basis.

27. After cleaning, our dataset contains data on 882 contingent charging firms and 397 non-contingent charging firms with 4111 observations and 1586 observations, respectively.

Table 4: Summary statistics of missing data (mean values and standard errors)

	Control	Treatment		
	Non-contingent	**Contingent**	**Missing**	**Total**
Number of consumers (full DB transfer advice)	26.9 (190.7)	26.7 (131.0)	72.4 (339.6)	29.8 (169.6)
Number of PTS	3.4 (8.3)	3.7 (21.7)	9.4 (31.3)	4.0 (20.1)
Transfer values (£mn)	8.9 (52.2)	10.4 (60.7)	30.3 (141.0)	11.3 (67.3)
DB transfer revenue (£'000)	49.7 (195.1)	175.6 (1466.4)	219.1 (867.5)	145.8 (1230.0)
PII premium to income ratio	3.4 (5.7)	3.9 (4.8)	3.7 (4.3)	3.8 (5.0)
DB transfer revenue to income ratio	7.4 (22.5)	14.1 (33.1)	10.2 (23.2)	12.1 (30.1)
Observations	1586	4111	404	6101

Note: Standard errors (in parenthesis).
Source: FCA analysis of data requests and regulatory returns

Outcome variables

28. The analysis focused on two outcome variables:
    1. **Number of firms providing DB transfer advice:** A change in this variable indicates whether the ban is associated with an exit of firms from the DB transfer advice market.
    2. **Firm-level DB transfer advice status:** Changes in this variable indicate whether the ban affected the firm propensity to offer DB transfer advice. Essentially, this variable is the firm-level counterpart of outcome 1.

Number of firms providing DB transfer advice

29. This variable counts the total number of firms that, in each reporting period:
    - Provided DB transfer advice.
    - Provided more than zero customers with full DB transfer advice or abridged advice. That is, the firm provided regulated DB transfer advice.

30. Note that this variable is the number of firms providing DB transfer advice and is not the number of firms who survive. Liaison with firms shows that many firms continue to provide other financial services but may have stopped providing DB transfer advice. DB transfer advice may be a small component of many firms' business.

DB transfer advice status at the firm level

31. To construct this outcome variable, we constructed a balanced panel dataset where each firm observed at the beginning of the analysis period is assigned an indicator variable, equal to 1 if the firm is observed also in subsequent periods, that is, it continued to provide DB transfer advice. If a firm dropped from the sample, we set the indicator variable to zero. Effectively, this procedure assumes that if a firm dropped from the sample, it had stopped providing DB transfer advice.

32. A summary of the balanced panel dataset is presented in Table 5. We do not have firm characteristics data for firms that did not provide any DB transfer advice in a given period as we generated this outcome. We only have firm characteristics for firms that provided DB transfer advice in any 1 period, so Table 6 in Chapter 3 is also applicable to this dataset.

Table 5: Balanced panel dataset

Group	**#**
Number of firms	1279
Number of observations = 0	4535
Number of observations = 1	5697
Total number of observations	10232

Source: FCA analysis of data requests and regulatory returns

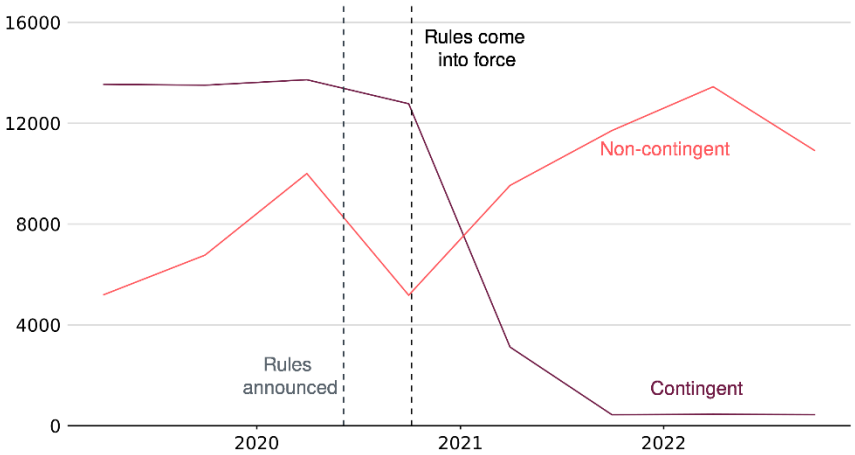
3 Descriptive statistics

34. Before our ban on contingent charging, firms providing DB transfer advice could charge consumers contingently, partially contingently or non-contingently. This means that before the intervention, some firms would have different charging structures than in the post-intervention period. For example, some firms may have charged all their consumers partially or fully contingently, but after the ban all their consumers would be charged non-contingently (except for those that qualify for a 'carve-out'). Conversely, firms who charged only non-contingently before the ban, continued to do so after the ban (except for those that qualify for a 'carve-out').

35. Figure 3 demonstrates this variation in charging structure before and after the ban.

Figure 3: Firms appear to have complied with the ban on contingent charging

Number of consumers advised under contingent or non-contingent charging structure



Note: The final rules were published on 5 June 2020, the main rules came into force on 1 October 2020 and transitional arrangements ended on 1 January 2021.
Source: FCA analysis of data requests and regulatory returns

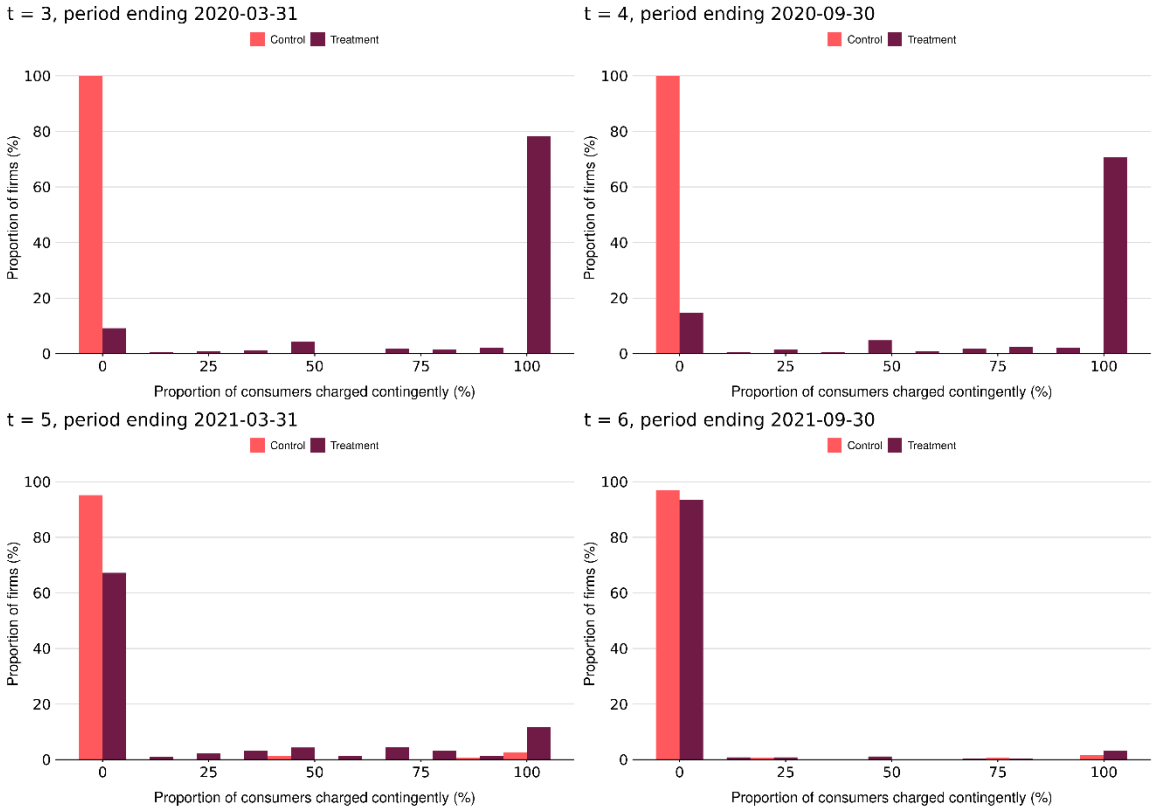
36. Figure 4 includes multiple histograms showing the distribution of firms charging consumers on a contingent basis by treatment and control firms from October 2019 to September 2021 (the 2 periods before and after the rules came into force). The horizontal axis is the proportion of consumers charged on a contingent basis and the vertical axis is the proportion of firms by treatment and control group. Figure 4 shows that:

- **The treatment and control groups appear in line with expectations**. In the pre-intervention periods, most firms in the treatment group charged 100% of their consumers on a contingent basis (see the top 2 charts). Also 100% of firms in the control group charged their consumers on a non-contingent basis (by definition).
- **Firms complied with the ban**. The ban on contingent charging came into force for the entire market on 1 October 2020. There is no evidence to suggest problems with compliance regarding this ban. There was nearly full compliance with the ban by April 2021 to September 2021, which was the first full period with a ban on contingent charging and no transitional arrangements.
- **Some firms made use of the transition period to 1 January 2021** (see first bottom chart). Firms who had commenced the process of charging on a

contingent basis before 1 October 2020 were allowed to finish the advice during a transition period to 1 January 2021. Just under 70% of firms in the treatment group were charging 0% of their clients on contingent basis in the first 6-months post-intervention (October 2020-March 2021) compared to more than 90% in the 6-month period ending 2021-09-30 (April 2021-September 2022).

- **A minority of firms in the control group used the exception to charge contingently**. The control group did not fully charge non-contingently in the post-intervention periods. This is not surprising because of the exemption where firms can charge contingently in a limited number of cases (for example, serious ill health or serious financial difficulty). More analysis on the 'carve-out' is presented in Chapter 6 of the main report.
- **Firms might have adjusted their charging structure in anticipation of the rules**. In the period before the rules were announced (October 2019 to March 2020) less than 5% of firms charged 0% of their clients on a contingent charging basis and more than 80% charged 100% of their clients on contingent basis. However, from March 2020 to September 2020, where the rules were announced (5 June 2020) and just before the rules came into force (1 October 2020), more than 15% of firms charged 0% of their clients on a contingent basis and around 70% charged 100% of their clients on contingent basis. It is important to acknowledge that these charts do not include firms that left the market and only shows that more firms may have moved to a non-contingent model prior to the intervention.

Figure 4: Distribution of firms charging consumers on contingent basis before and after the ban



Note: This data does not include firms that stopped providing DB transfer advice. The histogram is divided into 10 bins, representing the distribution of the data across the x-axis range.
Source: FCA analysis of data requests and regulatory return

37. Table 6 provides summary statistics for key characteristics of firms in our estimation sample. We conducted Welch two-sample t-test for equality of means to detect statistically significant differences across firm characteristics in our pre-intervention period.

38. Treatment and control firms look broadly similar across key characteristics such as the number of consumers receiving full DB transfer advice, number of PTS, and transfer values. DB transfer revenue and the associated DB transfer revenue to income ratio to appear different. However, we would expect this to be the case as we said charging on a contingent basis is associated with higher fees. Mean PII premiums to income ratios are also not significantly different when conducting a Welch two-sample t-test for equality of means. As outlined in Chapter 4 of the main report, contingent firms generally faced higher median premiums as a percentage of their income. However, the median premium-to-revenue ratios for both contingent and non-contingent firms followed a similar pattern. There was a sharp upward trend in 2019 and 2020, which then levelled off in 2021 and 2022.

Table 6: Pre-intervention summary statistics for treatment and control

	Control	Treatment	Welch test
	Non-contingent	**Contingent**	**P -value**
Number of consumers (full DB transfer advice)			
Mean (SD)	26.9 (230.7)	23.5 (117.8)	0.684
1 pct, 99 pct	[1, 291.7]	[1, 437.2]	
Number of PTS			
Mean (SD)	3.3 (8.1)	2.8 (5.4)	0.154
1 pct, 99 pct	[1, 23.5]	[1, 25]	
Transfer values (£mn)			
Mean (SD)	8.3 (61.8)	8.5 (53.9)	0.929
1 pct, 99 pct	[0, 106.2]	[0, 105.0]	
DB transfer revenue (£'000)			
Mean (SD)	41.8 (232.7)	156.9 (1455.0)	0.000
1 pct, 99 pct	[0, 489.1]	[0, 1917.1]	
PII premium to income ratio			
Mean (SD)	3.2 (6.9)	3.4 (4.5)	0.473
1 pct, 99 pct	[0.1, 23.4]	[0.1, 19.1]	
DB transfer revenue to income ratio			
Mean (SD)	7.5 (23.6)	12.4 (24.9)	0.000
1 pct, 99 pct	[0, 116.0]	[0, 128.9]	
Observations	856	2169	

Note: We have used 1st percentile and 99th percentile as opposed to min and max to account for a small number of outliers in the tails of the distribution for some data variables.
Source: FCA analysis of data requests and regulatory returns

4 Analysis of the causal impact of the contingent charging ban

39. In this chapter, we provide evidence of the causal impact of the contingent charging ban on the number of firms providing DB transfer advice and on the probability of offering DB transfer advice at the firm level.

40. As described in Chapter 4 of the main report, we found that the number of firms providing DB transfer advice had fallen significantly since the rules were announced in 2020. However, we cannot conclude that movements in these quantities are attributable solely to the ban on contingent charging. The pandemic, our other interventions and broader market conditions may have also influenced the number of firms providing advice.

41. To isolate the effect of the contingent charging ban on other factors, we used a Difference-in-Differences (DiD) design analysis. This approach allows estimating the Average Treatment Effect on the Treated (ATT), which is the causal impact of the ban on the firms who were subject to the ban (the treatment group). As noted earlier, the treatment group are firms who, in the 2 years before the ban, charged at least 1 customer on a contingent basis. These are compared to a control group of firms who never charged contingently before the intervention.

42. Our results show that there is some evidence that the ban on contingent charging reduced the number of firms providing DB transfer advice by about 129 firms in the year after the announcement of the intervention and 195 firms up until the end of September 2022. Results for the probability of offering DB transfer advice at the firm-level, however, are less-clearcut. The absence of consistent results across approaches suggests that other market forces may have had an impact on the decline of the number of firms offering DB transfer advice.

Design

43. To isolate the effect of the ban on contingent charging from other factors, we used a DiD design analysis[3].

44. DiD allows estimating impacts by comparing outcome variables before and after the intervention for a treatment group (firms subject to the ban), and a comparison group, (firms who were not subject to the ban). The idea behind DiD is that outcomes for the comparison group approximate counterfactual outcomes for the treatment group. That is, what would have happened to the treated firms in the absence of the intervention.

45. We define the treatment group as those firms who, in the 2 years before the ban, charged at least 1 customer on a contingent basis. For the comparison group, we exploit the fact that some firms never charged on a contingent basis before the ban. In effect these firms were already observing a ban on contingent charging and therefore would be unaffected by the regulation. The existence of a group of firms that already did not charge contingently provides a natural comparison group for the impact evaluation.

[3] For further information on the general DiD methodology see: Angrist & Pischke, 2009, Mostly Harmless Econometrics: An Empiricist's Companion, p.227-243; and Wooldridge; 2009; Introductory Econometrics A Modern Approach Fourth Edition, p.450-455.

46. Under some assumptions (discussed in the next section) DiD identifies the ATT, which is interpreted as the effect of the ban on the firms who were subject to it.

47. The simplest DiD setting is where there are only 2 periods (1 before and 1 after an intervention) and 2 groups (1 treatment and 1 control group). In this standard "2x2" framework, the ATT can be computed by taking the difference between values of the outcome variable for the treatment and control group before and after the policy. Let's define:

- $y_{C,1}$ – outcome for the control group before the change
- $y_{T,1}$ – outcome for the treatment group before the change
- $y_{C,2}$- outcome for the control group after the change
- $y_{T,2}$- outcome for the treatment group after the change

The ATT can be computed as:

$$ATT = (y_{T,2} - y_{C,2}) - (y_{T,1} - y_{C,1}) \quad (1)$$

Equation (1) is also known as the "DiD formula".

48. It can be shown that the ATT can also be computed using a regression specification of this form:

$$Y = \beta 0 + \beta 1 * Treatment * Post + \beta 2 * Treatment + \beta 3 * Post + \varepsilon \quad (2)$$

Where:

- $\boldsymbol{\beta_1}$ is the ATT, the coefficient of interest
- $\boldsymbol{\beta_0}$ is the average outcome of the control group before the treatment
- $\boldsymbol{\beta_2}$ is the difference between the control group and the treatment group before the treatment
- $\boldsymbol{\beta_3}$ is the difference between the average outcome average outcome of the control group before and after the treatment
- $\boldsymbol{\varepsilon}$ is the error term
- $\boldsymbol{Y}$ is the outcome. In our case it is either the number of firms or the DB transfer advice status at the firm level

49. Depending on the outcome under analysis, we used either the DiD formula and a regression approach to estimate the ATT.

Number of firms providing DB transfer advice

50. We used 2 approaches to estimate the impact of the ban on the number of firms providing DB transfer advice: the DiD formula (Equation 1) and a regression approach (Equation 2).

51. Using the DiD formula, we can compute the impact of the ban during our 'intervention period' as:

$$ATT = (y_{T,5} - y_{C,5}) - (y_{T,3} - y_{C,3}) \quad (3)$$

Where $t = 5$ is the end of our 'intervention period' and the difference is always taken with respect to $t = 3$, the last pre-intervention period.

52. The above approach only allows computing point estimates and not standard errors. We also use a 2x2 regression approach (Equation 2), which provides standard errors for the estimated ATT.

53. When comparing the results from the 2 approaches above, it is important to note that the 2 methods handle the time dimension differently. When we used the DiD formula, the effect was calculated for a specific period, allowing for a more granular analysis of the impact of the intervention. For instance, in our study, the reduction of 129 units is observed specifically in the period immediately following the ban (t = 5 with respect to t = 3). This can be interpreted as the short-term effect of the intervention.

54. On the other hand, the linear regression approach aggregates the effect across the entire post-intervention period. The reason for this aggregation is that including dummy variables for each period would require many additional variables, which is not feasible given the small sample size for this aggregated analysis. Consequently, the linear regression approach sacrifices some granularity in favour of a more parsimonious model. While this aggregation provides an overall estimate of the intervention's impact, it does not allow for a detailed examination of the effect in each period.

Probability of providing DB transfer advice

55. As we were working with a binary outcome in a DiD setting, we first ran a linear probability model (LPM) with standard errors clustered at the firm-level. We then also ran a logit model, also with standard errors clustered at the firm-level.

56. For the LPM, we used a 2x2 regression approach as in Equation 2. However, the dependent variable is now an indicator for whether firms provide DB transfer advice (either 0 if they don't or 1 if they do).

57. There are several advantages to using a LPM in a DiD setting with a binary outcome including ease of interpretation, its robustness to misspecification and its consistency with the DiD framework[4].

58. Logistic regressions compare how the change in the probability of an outcome shifts in response to covariates. As a further robustness check, we also use a logistic regression to estimate how the probability of providing DB transfer advice shifts based on whether the firm was subject to the ban on contingent charging or not.

59. The logistic model can be expressed as:

$$\log\left(\frac{P(y_{i,t}=1)}{P(y_{i,t}=0)}\right) = \beta_0 + \beta_1 * Treatment_{i,t} * Post_t + \beta_2 * Treatment_i + \beta_3 * Post_t + \epsilon_{i,t} \quad (4)$$

Where:

- The left-hand side is the log-odds of the binary outcome. The outcome of interest (y) is equal to 1 if the firm is providing DB transfer advice and equal to 0 if the firm is not providing DB transfer advice.
- $\boldsymbol{\beta_1}$ is still the coefficient of interest, representing the ATT on the log-odds outcome. It represents the log-odds change of the outcome (firms providing DB transfer advice) for firms in the treatment group during the post-treatment period, compared to firms not in the treatment group during the same period.

[4] See: Angrist & Pischke, 2009, Mostly Harmless Econometrics: An Empiricist's Companion, Chapter 3. Saturated models fit the conditional expectation function perfectly regardless of the distribution of the outcome variable.

60. We calculate marginal effects of the interaction term for the logit model to interpret the results in terms of probability changes and to compare the results to the LPM.

61. It is worth noting that results of the logistic regression and LPM are not directly comparable. While in a linear setting the estimated coefficients are ATTs under parallel trends, the coefficients or marginal effects of a logistic regression cannot be confidently interpreted in this way.

Validating model assumptions

62. The interpretation of estimates as causal rely on 3 identifying assumptions:
    - There are common trends between the treatment and control groups in the absence of the intervention (also known as 'parallel trends').
    - No spillover effects.
    - No anticipation effects.

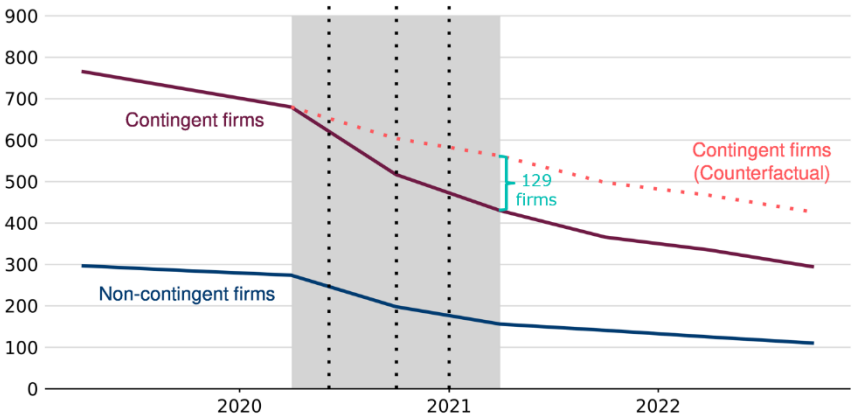
    In this section we discuss these identifying assumptions. At the end of the section, we discuss the issue of attribution, including how confidently we can rule out alternative explanations for our results.

Parallel trends

63. To validate this assumption, we looked at trends in the number of firms providing DB transfer advice in the treatment and control period.

Figure 5: Time series of the effect of the ban on contingent charging on the number of firms providing DB transfer advice

Number of firms providing DB transfer advice. Line plot of DiD.



Note: The grey shaded area is the 'intervention period', which includes the 6 month period where the rules were announced and the 6 month period where the rules came into force. The first black dotted line is when the final rules were published on 5 June 2020, the second is when the rules came into force on 1 October 2020 and the third is when transitional arrangements ended on 1 January 2021.
Source: FCA analysis of data requests and regulatory returns

64. Visually, prior to the intervention the number of contingent charging firms and the number of non-contingent charging firms were following a similar, declining trend. It could be argued that the declining trend is larger for contingent charging firms compared to non-contingent charging firms, violating the parallel trends assumption. However, the period-by-period differences are relatively small before the announcement of the plan. Further, our placebo tests in paragraphs 69 to 73 do not falsify the parallel trends assumption, however, these tests rely of small sample sizes[5].

[5] If we found an 'effect' for that treatment date where there should not have been one, that is evidence that could be something wrong with the design, which may imply a violation of parallel trends.

65. We would expect that after the intervention, the number of firms providing DB transfer advice would decline more among the contingent than the non-contingent firms. This is what we observe in Figure 5.

66. The recent DiD literature (for instance, Khan-Lane and Lang, 2019) argue that DiD will generally be more plausible if the treatment and control groups are similar in level to begin with, not just in trends and that large differences in levels before the treatment should have a plausible justification, even in the presence of parallel trends.

67. In our case, the outcome variable is the total number of firms providing advice in each period. Contingent charging (where advisers only get paid if a transfer proceeds) was the prevalent charging structure before the intervention and that is why our treatment and control groups are different in levels.

No spill-over effects

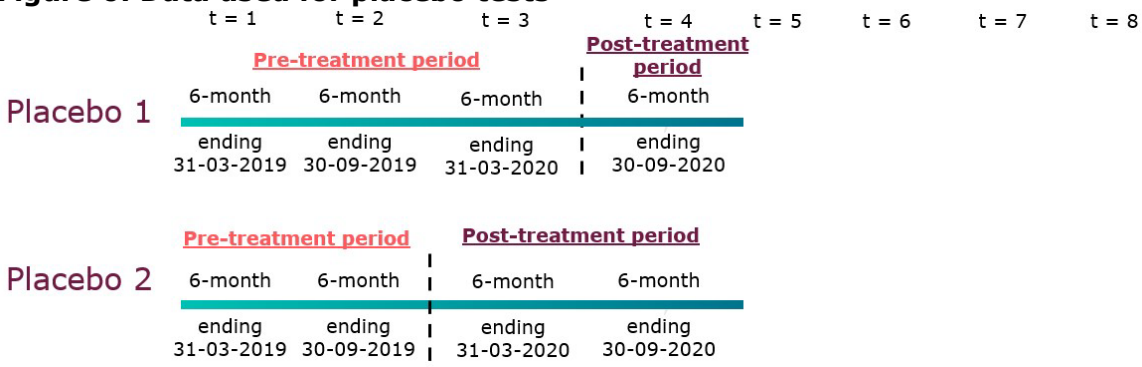
68. In classical DID settings, the researcher needs to assume that the control group does not experience treatment. By definition, our control group does not experience treatment, so there are no spill-over effects to the treated group. It is possible that supply-side reactions among contingent firms could alter competitive dynamics (for example, they stop providing DB transfer advice) and thus spur reactions of other firms, but we do not consider this a major risk over our short intervention and evaluation period. These dynamics are more likely to occur over longer durations. Further, Table 8 and Figure 8 also suggest no large changes in the composition of the treatment and control groups over time.

No anticipation effects

69. We have qualitative information from our pensions and supervision teams that many firms stopped offering DB transfer advice after the announcement of our rules on 5 June 2020. Further, there is some evidence in Figure 4 that some firms who remained in the market moved to a non-contingent model prior to the rules coming into force.

70. This raises the issue of when to set the start of our post-intervention period. We conducted two placebo tests by shifting the intervention start date to test this (see Figure 6). In the first test (placebo 1), we moved the start date forward by 1 period, and in the second test (placebo 2), we shifted it forward by 2 periods.

Figure 6: Data used for placebo tests



71. The results show a statistically significant impact of the ban 1 period before the actual intervention (column 1 in Table 7). No significant effect was found when the start date was moved two periods before the intervention (column 2 in Table 7), suggesting there is no "effect" for this treatment date, that is, there is not an effect where there should not be one.

72. As we are using aggregated data the sample sizes for these placebo tests are small and their results should be interpreted with caution. However, we have included them as they support the anecdotal evidence from supervision and policy teams.

73. For the remainder of the analysis, we have therefore included the period where rules were announced (t = 4) in the intervention period and post-treatment period. The placebo tests along with the logical reasoning and anecdotal evidence suggest this is appropriate. This is notable because it suggests that firms are responsive to the announcement of a ban. Intuitively, this makes sense because some firms are likely to take pre-emptive action and exit the market upon hearing the announcement, rather than waiting for the rule to come into effect. In effect by doing this, we are saying the announcement of the ban had an effect on the number of firms providing advice in the DB transfer advice market and we are counting these firms towards our outcome variable.

Table 7: Regression output for placebo tests from estimating Equation (2)

	Outcome variable (number of firms providing advice)	
	Placebo 1	**Placebo 2**
	(1)	(2)
ATT	-119**	-91
	(30)	(93)
Contingent	438***	454***
	(30)	(22)
Post	-87***	-55
	(8)	(39)
Constant	285***	291***
	(8)	(6)
Observations	8	8
R^2	0.981	0.954
Adjusted R^2	0.989	0.920

*Note: *p<0.1; **p<0.05; ***p<0.01. Robust standard errors (in parenthesis).*
Source: FCA analysis of data requests and regulatory returns

Attribution

74. So far, we have assumed that changes in the number of firms providing advice are entirely due to the contingent charging ban. However, if something else other than the policy (for example, the pandemic and the other elements of the policy), affected the charging structure of firms, we would falsely be attributing the effects of this event to our policy. In this section, we provide arguments that rule out alternative explanations for the effects found.

Effect of the pandemic

75. We argue that, as contingent and non-contingent firms broadly display similar characteristics, the pandemic would have had the similar effects on both groups, thus not affecting counterfactual trends after the policy. This is supported by Table 8

which shows no significant differences for firm characteristics in contingent and non-contingent firms in the period before the announcement of the ban and the pandemic. Therefore, we assume the pandemic would have had similar effects on both contingent and non-contingent firms.

Table 8: Means of firm characteristics in period t = 3

Firm characteristics	Contingent	Non-contingent	Welch t-test (p-value)
Number of consumers (full DB transfer advice)	26.4	33.1	0.727
PTS	2.9	3.4	0.315
Staff numbers	94.2	79.1	0.759
Transfer values (£mn)	£11.4mn	£10.7mn	0.907

*Note: *p < 0.1, **p < 0.05, ***p < 0.01. Welch two sample t-test for equality of means. Period 3 is the last pre-intervention period.*
Source: FCA analysis of data requests and regulatory returns

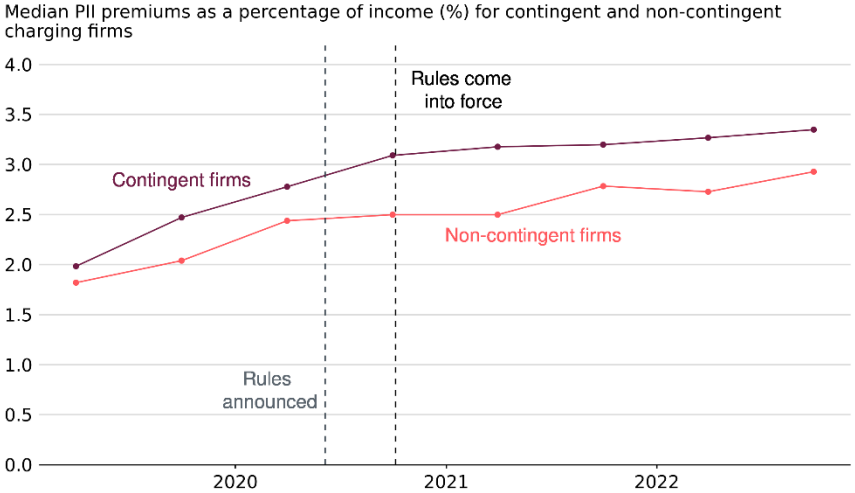
Effect of rising PII premiums

76. Our supervision work and feedback from firms suggests that perhaps changes in market outcomes may have been driven by rising PII premiums, rather than our interventions in 2020. For example, perhaps firms charging on a contingent basis experienced larger increase in premiums compared to non-contingent charging firms. To analyses these concerns, we examined PII data. We also analysed the data by split the firms into our those that charge contingently and non-contingently.

77. We see that contingent firms generally face higher premiums as a percentage of their income (see Figure 7). However, the median premium-to-revenue ratios for both contingent and non-contingent firms followed a similar pattern. There was a sharp upward trend in 2019 and 2020, which then levelled off in 2021 and 2022. Therefore, we assume that PII premiums did not confound with the ban on contingent charging.

Figure 7: Median PII premium-to-revenue ratios are structurally higher for contingent firms but follow a similar pattern to non-contingent firms

Median PII premiums as a percentage of income (%) for contingent and non-contingent charging firms



Note: PII premium and income data are matched to each period using the closest date to the policy start date.
Source: FCA analysis of regulatory returns

Other elements of the intervention

78. The intervention consisted of several remedies besides the ban on contingent charging, as shown in Table 1. Below, we discuss each remedy and provide evidence supporting the assumption that changes in the number of firms providing advice are due entirely to the ban on contingent charging, rather than other elements of the intervention.

Ban on contingent charging except in exceptional circumstances

79. We banned contingent charging, with exceptions for consumers in specific personal circumstances, known as a 'carve-out'. We evaluated the impact of this ban by noting that many firms never used contingent charging. Regarding the 'carve-out', a very small minority of firms in the control group used this exception due to serious ill-health or financial difficulty. The percentage of consumers using the carve-out was under 2% for most periods, and only a few firms utilised it. Because the usage of the carve-out was minimal, we argue it does not impact the number of contingent and non-contingent firms differently. Detailed analysis on the carve-out is presented in Chapter 6 of the main report.

Abridged advice and changes to triage services

80. Our intervention enabled firms to give a short form of advice (abridged advice) and added to our perimeter guidance on triage services.
    - Abridged advice was optional, so we believe it did not cause firms to stop providing advice. Further, firms providing abridged advice were not correlated with firms charging structure (that is, charging contingently or non-contingently).
    - Changes to triage services likely had no impact on the number of firms providing DB transfer advice, as it is a non-advised service and not part of regulated advice. The changes mainly clarified how firms can avoid giving advice when delivering triage services.

Addressing ongoing conflicts

81. This remedy required advisers to consider an available workplace pension as a receiving scheme for a transfer and to demonstrate why any alternative is more suitable. This rule applied equally to both contingent and non-contingent firms, so we do not believe it affected firms differently.

Empowering consumers

82. This element of the intervention had multiple components:
    - initial disclosure on charging structure
    - suitability report and statement of risks, both requiring a client signature
    - keeping records to demonstrate consumers understand the risks of proceeding with a transfer

83. This intervention was applied uniformly to both the treatment and control groups. We have no evidence to suggest it affected contingent or non-contingent firms differently.

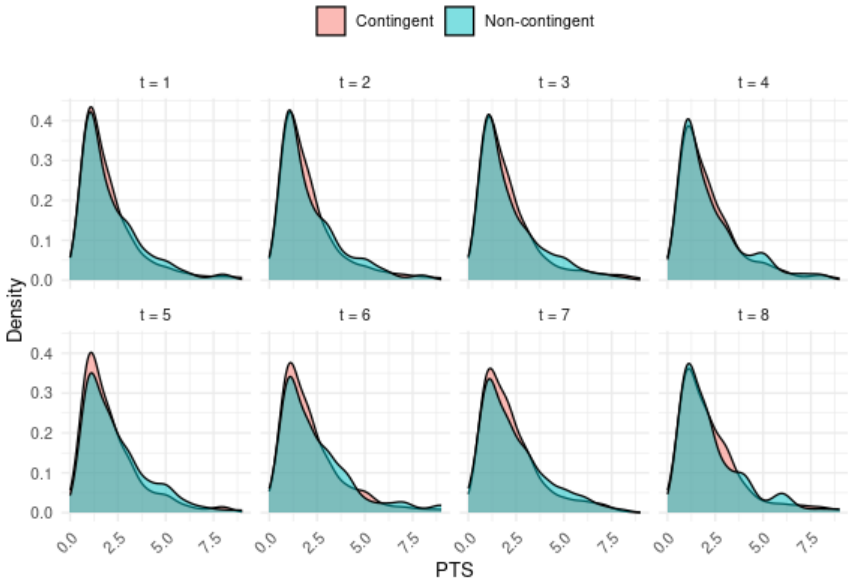
Enabling advisers

84. This element required PTSs to complete 15 hours of continuing professional development (CPD) each year in addition to any other CPD they undertake. We found no evidence suggesting this element affected contingent or non-contingent firms differently.

85. Moreover, as shown in Table 6 & 7 and Figure 8, the means and distributions of the number of PTSs in the treated and control groups are not significantly different, suggesting this intervention had the same impact on both groups.

86. Firm characteristics are broadly similar when looking at the distributions for the number of PTS for the treatment and control groups.

Figure 8: Density plot of the number of PTS by treatment group and period



Note: The x-axis upper limit is set to the 95th percentile for presentation purposes.
Source: FCA analysis of data requests and regulatory returns

Effective regulation

87. This part of the intervention involved collecting new data on DB transfer advice and amending the existing data collection on professional indemnity insurance (PII). We used this data in our analysis, and it does not impact contingent and non-contingent firms differently.

Results

88. This section presents results from our analysis. We summarise the result from the different methodological approached in Table 9.

Table 9: Summary of results from different methodological approaches

	Model 1	Model 2	Model 3	Model 4
Data	**Aggregated**	**Aggregated**	**Firm-level**	**Firm-level**
Model	**DiD formula**	**Linear regression**	**LPM**	**Logit**
Dependent variable	Number of firms providing DB transfer advice	Number of firms providing DB transfer advice	Probability of providing DB transfer advice	Probability of providing DB transfer advice
Key interpretation	129 firms stopped providing DB transfer advice in the intervention period, due to the ban on contingent charging	195 firms stopped providing DB transfer advice, due to the ban on contingent charging (statistically significant at 1% level)	No statistically significant effect. Being subject to a ban on contingent charging decreases the probability of a firm providing advice by about 3 percentage points after the intervention	Being subject to a ban on contingent charging decreases the probability of a firm providing advice by about 6 percentage points (statistically significant at 5% level), after the intervention

Source: FCA analysis of data requests and regulatory returns

Impact on the number of firms providing DB transfer advice

89. We used two approaches to estimate the impact of the ban on the number of firms providing DB transfer advice: the DiD formula (Equation 1) and a regression approach (Equation 2).

90. Using the DiD formula from Equation 3, we calculated that 129 firms left the market during the intervention period due to the ban on contingent charging. This calculation is presented in Table 10.

91. As we are using a DiD formula with aggregated data on the number of firms providing advice, this only allows us to estimate a point estimate.

Table 10: Calculation for DiD formula approach

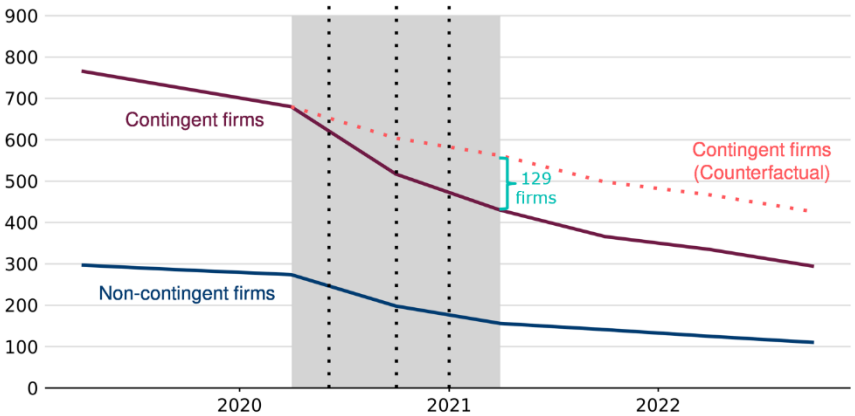
Model 1	Treated	Control
	Contingent	**Non-contingent**
T3	680	280
T5	430	159
T5 – T3	-250	-121
ATT	-129	

Source: FCA analysis of data requests and regulatory returns

92. Representing the results in Table 10 graphically, along with the counterfactual provides Figure 9 (which is the same as Figure 5).

Figure 9: Time series of the effect of the ban on contingent charging on the number of firms providing DB transfer advice

Number of firms providing DB transfer advice. Line plot of DiD.



Note: The grey shaded area is the 'intervention period', which includes the 6 month period where the rules were announced and the 6 month period where the rules came into force. The first black dotted line is when the final rules were published on 5 June 2020, the second is when the rules came into force on 1 October 2020 and the third is when transitional arrangements ended on 1 January 2021.
Source: FCA analysis of data requests and regulatory returns

93. We also estimate the ATT using the regression specification in Equation 2. The estimated impact of the ban on the number of firms providing DB transfer advice is -195 and statistically significant at the 1% level, suggesting that the ban on contingent charging led to 195 firms leaving the market for DB transfer advice in the entire post intervention period (from April 2020 to September 2022).

Table 11: Regression output estimating Equation (2) for the number of firms providing DB transfer advice

	Model 2
Data	**Aggregated**
Model	**Linear regression**
ATT	-195***
	(50)
Contingent	438***
	(24)
Post	-139***
	(17)
Constant	285***
	(6)
Observations	16
R^2	0.943
Adjusted R^2	0.929

*Note: *p<0.1; **p<0.05; ***p<0.01. Robust standard errors (in parenthesis).*
Source: FCA analysis of data requests and regulatory returns

94. The coefficient of the Contingent dummy, which represents firms with contingent charging structures, is 438. This suggests there are 438 more contingent charging firms in the pre-intervention period, on average, compared to non-contingent firms.

95. The coefficient on the Post dummy indicates a significant decrease of 139 firms providing DB transfer advice after the intervention for non-contingent charging firms (the control group).

Impact on the probability of providing DB transfer advice

96. In this section we present our analysis using a binary outcome to obtain estimates of the probability of a firm offering DB transfer advice, after being subject to the ban on contingent charging. To estimate the effect of the ban on contingent charging on providing on whether firms are providing DB transfer advice, we used a LPM and a logit model.

97. For the LPM and the logit model, we used a 2x2 regression approach as in Equation 2 and Equation 4, respectively. However, the outcome is now the probability of a firm providing DB transfer advice (ranging between 0 and 1).

98. The LPM is a simple linear regression model applied to binary outcomes. Although it has limitations, for example, it can predict probabilities outside the [0, 1] range, it is easy to interpret and computationally straightforward.

99. We present the results from the LPM in column (1) in Table 12[6]. The estimated ATT is -0.028, suggesting that the ban leads to a decrease in the probability of offering DB transfer advice by 2.83 percentage points. Although the sign of the effect is as expected, the result is not statistically significant at conventional levels.

Table 12: Regression output for LPM and logit model

	Model 3	Model 4	
Model	**LPM**	**Logistic**	**Marginal effect from logistic model**
	(1)	**(2)**	**(3)**
ATT	-0.028	-0.274**	-5.95pp**
	(0.025)	(0.115)	(0.0256)
Contingent	0.101***	0.576***	
	(0.018)	(0.101)	
Post	-0.351***	-1.480***	
	(0.021)	(-0.094)	
Constant	0.719***	0.939***	
	(0.016)	(0.080)	
Observations	10,232	10,232	10,232

*Note: *p<0.1; **p<0.05; ***p<0.01. Standard errors (in parenthesis) clustered at the firm-level. The average marginal effect represents the average change in probability of providing DB transfer advice between a firm subject to and not subject to the ban in the post-treatment period.*
Source: FCA analysis of data requests and regulatory returns

100. We used the logistic regression model in to analyse the marginal effect of being subject to the ban on contingent charging.

When we estimated the ATT using a logistic regression model (Equation 4), we obtained a marginal effect of -0.06, suggesting that contingent charging firms were six percentage points less likely to provide DB transfer advice due to the ban. The marginal effect is statistically significant at 5% level (see column 3 in Table 12).

[6] We also followed Callaway and Sant'Anna (2021), which adapts DiD methods to more general settings. Callaway and Sant'Anna (2021) allows the parallel trends assumption to hold conditional on covariates. We ran the binary outcome data with time-invariant covariates such as the number of PTS, PII premium to income ratios and DB transfer revenue to total income ratios. Similar to the LPM results, the results following Callaway and Sant'Anna (2021) with additional covariates indicated no significant impact of the ban on contingent charging on probability of a firm providing DB advice, although the the direction of the effects is what we expected. The Wald pre-test of the parallel trends assumption showed that the parallel trends assumption would not be rejected at conventional significance levels.

Robustness checks

101. As explained earlier, firms that could not be identified as charging contingently or non-contingently were removed from the main analysis.

102. As outlined in Chapter 2, there is some evidence that the missing observations that could not be identified were more likely to be large firms who charged on a contingent basis. Therefore, we conducted a robustness check where we assumed all our excluded firms were charging contingently.

103. The results of this robustness check are presented in Table 13. Overall, we reach similar conclusions as in the main analysis. The direction and size of the results are similar, but the magnitudes of the estimated ATT are larger.

Table 13: Robustness check ATT output for different methodological approaches

	Model 1	Model 2	Model 3	Model 4
Dependent variable	**Number of firms**	**Number of firms**	**DB transfer advice status**	**DB transfer advice status**
Model	**DiD formula (t5-t3)**	**Linear Regression**	**LPM**	**Logit**
ATT	-171	-238***	-0.031	-0.261**
		(62)	(0.024)	(0.113)
Observations		16	11,072	11,072

*Note: *p<0.1; **p<0.05; ***p<0.01. Standard errors (in parenthesis). The linear regression for the number of firms used robust standard errors while standard errors for the LPM and logit models were clustered at the firm-level.*
Source: FCA analysis of data requests and regulatory returns