ofgem Making a positive difference for energy consumers

Small Scale Database trial

Research Results

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Overview

This paper reports the results of a small-scale trial carried out by Ofgem between November 2016 and April 2017. The trial was designed to inform Ofgem's development of the database remedy, which was recommended by the Competition and Markets Authority following its investigation into the energy market.

This trial explored whether sending disengaged energy customers (on more expensive default tariffs) information by post about energy deals with other suppliers led to increased switching (and savings) compared to a control group where no information was sent.

The results of this trial have been encouraging, and the effect sizes observed greater than expected for a letter-based intervention among a disengaged group of customers. A postal intervention on eligible consumers increased switching rates compared against a control group between 5 percentage points (pp) and 8pp, depending on the intervention and the duration of the assessment period.

The baseline switching rate for the control group was between 6.8% and 9.6% for a switching period of 14 and 19 weeks respectively. This is higher than expected and a number of factors could have influenced this outcome.

However, we need to be careful about what inferences we can draw from this trial, and the conclusions reflect uncertainty driven by a number of factors.

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1.Executive summary

1.1. This trial explored whether sending disengaged energy customers information about energy deals with other suppliers led to increased switching (and savings) compared to a control group which received no information.

1.2. In June 2016,[1] the Competition and Markets Authority (CMA) proposed a number of remedies aimed at improving customer engagement in the energy market. The CMA 'database remedy' proposed that Ofgem create and control a database of 'disengaged customers' on default tariffs, to allow rival suppliers to prompt these customers to engage in the retail energy markets. In response to the CMA's report, Ofgem set out an implementation strategy[2] in which we were clear that where appropriate we would refine the CMA's proposed design for the remedy to make sure it is effective.

1.3. As part of the ongoing evidence gathering exercise to develop the database remedy, Ofgem conducted a small scale randomised control trial (RCT) to understand:

1. How customers behave in response to receiving on 'opt out' letter;
2. How customers respond to receiving postal marketing from other suppliers (the CMA proposed approach);
3. How customers respond to receiving personalised information about tariffs from other suppliers consolidated into one postal communication from Ofgem and;
4. How suppliers behave in response to receiving data on customers from rival suppliers.

1.4. Alongside the RCT Ofgem's Consumer Research and Insight team also conducted qualitative research with customers, in the form of in-depth telephone interviews, to understand their experiences and actions. The findings from this qualitative research can be found in section 6 of this paper.

Summary of the design

1.5. This trial was designed as an RCT. A total sample of 2,400 eligible customers (who had not switched energy supplier for more than three years) was chosen from two large energy suppliers on a 50/50 basis (labelled Supplier A and Supplier B). Each customer was randomly assigned to receive either:

[1] See https://assets.publishing.service.gov.uk/media/5773de34e5274a0da3000113/final-report-energy-market-investigation.pdf
[2] See https://www.ofgem.gov.uk/publications-and-updates/cma-remedies-implementation-strategy

- Up to six marketing letters from other suppliers[3] (the 'CMA' arm) or;
- A Best Offer Letter from Ofgem (the 'BOL' arm) or;
- No letter (the control arm)

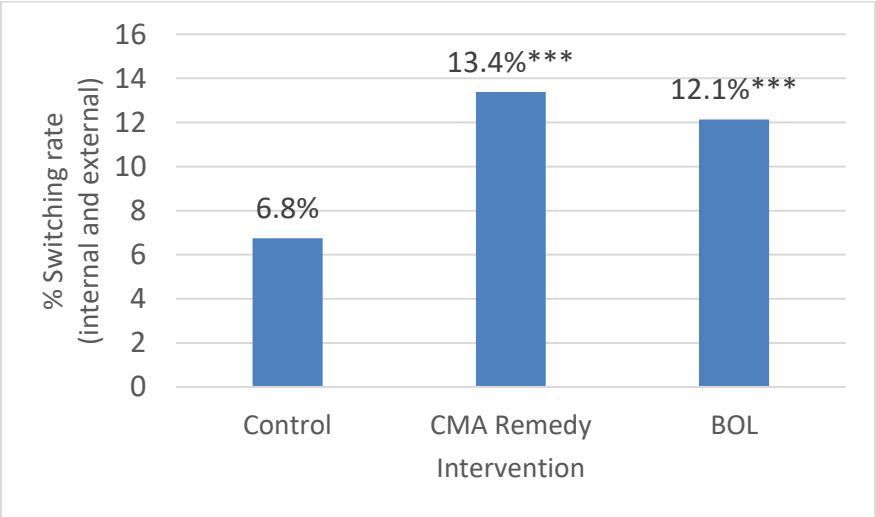
1.6. The primary outcome measure from the trial was the number of customers that initiated a switch from the beginning of the trial to the end of assessment period (from the 23rd November to the end of February 2017). We also collected further data up until the beginning of April 2017 in order to explore longer-term effects.[4]

1.7. Secondary outcomes were quality of switching (i.e. savings made) and other possible impacts on engagement behaviour.

Summary results

1.8. The key outcome of the trial was the switching rate measured through data on customer switch requests. We chose this metric rather than completed switches because a) it takes longer for that information to be available and b) switches can fail for reasons unrelated to customer engagement.

Figure 1 – Key outcomes



1.9. As shown in Figure 1, the results from the analysis are encouraging. In accordance with our analysis specification we compared switching requests from 23rd November 2016 to 28th February 2017.[5] On average participants in the control group experienced a switching rate of 6.75%, participants in the BOL trial

[3] Other suppliers included three smaller suppliers who were chosen to market to customers in the CMA arm, and the larger supplier (other than their own) who provided customers for the trial.
[4] These results are also reported in section 5 of this paper.
[5] The main analysis in this paper follows the Intention to Treat Approach. It covers all results from the opt-out and marketing periods. However, this table presents results for a switching window that is compatible with the analysis window initially specified.

arm 12.13% and in the CMA arm 13.38%. These results were statistically significant at the 99% confidence level.

1.10. This means that the letters sent by suppliers in the CMA group increased switching by around 6.6 percentage points (pp) and the letters sent by Ofgem by 5.4pp compared to the control group.[6]

1.11. We also collected data on tariffs which allowed us to estimate average savings by switch. Note the data available on savings is not of sufficiently high quality to draw any firm conclusions. Of the data available we estimated that savings were around £131 per year. There were no significant differences between the three trial arms. However, this is based on data from only 61% of switches. We had data on tariff switches with the same supplier (internal switches) but only patchy data on switches to another supplier (external switches).

1.12. Qualititative interviews with a selection of customers indicated that for some, the BOL was received more positively than the CMA marketing which some customers described as 'intrusive'.

1.13. The qualitative interviews suggested that very few of the customers interviewed actually switched to one of the suppliers who they received information about, most common actions were:

- Customers were prompted to look on a price comparison website (PCW) and look for a better deal.
- Customers were prompted to call their current supplier and use the BOL or marketing as evidence to negotiate a cheaper internal tariff.

Further considerations

1.14. The baseline switching rates and effect size were higher than anticipated. In making inferences we need to be careful to take account of eligibility, seasonal and supplier influences and other factors that were not under the control of the research team.

1.15. Although it is difficult to generalise, we are confident that these results are internally valid[7] so the difference between switching rates in the three trial arms can be attributed to the intervention. We checked for the balance of the three trial arms against a set of key factors and the results of these tests were positive. This means we think the randomisation process worked well and the impact we observed is unbiased.

1.16. Further work would be needed to understand what the outcomes would be if the trial was run at a different time of the year, with different eligibility criteria and with different suppliers. While we have identified a significant impact, it is not possible to categorically say that this could be replicated in other circumstances.

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[6] When rounding is applied.

[7] Internal and external validity are two key properties of well-designed RCTs. Internal validity means that the methodology for estimating and attributing impacts is robust. External validity means that the results can be generalised to a wider population.

2. Why we did a trial

2.1. In 2016, the CMA[8] identified several Adverse Effects on Competition (AEC). One of them was Domestic Weak Customer Response AEC.

2.2. The features identified by the CMA as giving rise to the Domestic Weak Customer AEC were the following:

a) Customers have limited awareness of, and interest in, their ability to switch energy supplier, which arises in particular from the following fundamental characteristics of the domestic retail gas and electricity supply markets: (i) the homogeneous nature of gas and electricity; and (ii) the role of traditional meters and bills.
b) Customers face actual and perceived barriers to accessing and assessing information arising, in particular, from the following aspects of the domestic retail gas and electricity markets: (i) the complex information provided in bills and the structure of tariffs; and (ii) a lack of confidence in, and access to, PCWs by certain categories of customers, including the less well-educated and the less well-off.
c) Customers face actual and perceived barriers to switching, such as where they experience erroneous transfers which have the potential to cause material detriment to those who suffer from them.

2.3. In the Report, the CMA decided a package of remedies to mitigate or prevent the AECs and/or associated detriment that it found. One of these remedies is the Database Remedy. The CMA recommended that Ofgem create and control a database of 'disengaged customers' on default tariffs, to allow rival suppliers to prompt these customers to engage in the retail energy markets. This remedy targets those consumers that do not currently engage in the market, to provide them with additional support and encouragement to do so. It focuses on getting the best outcomes for the consumer, while at all times protecting personal data and handling it securely.[9]

2.4. The remedy involves (a) a requirement on suppliers to disclose to the Gas and Electricity Markets Authority (GEMA)[10] certain details of their domestic and microbusiness consumers who have been on their standard variable, or any other default, tariff for three or more years (stage 1); and (b) a recommendation that GEMA (i) retain the relevant data, (ii) test the operation of the database, (iii) use and, subject to sufficient safeguards being in place, disclose the relevant data (via a secure database) to rival suppliers for the purposes of prompting such customers to engage in the retail energy markets, and (iv) monitor the impact of the database with a view to maximising its effectiveness (stages 2 and 3).

[8] See https://www.gov.uk/cma-cases/energy-market-investigation
[9] Data protection and legal aspects of the Database Remedy are out of scope of this paper.
[10] Ofgem

2.5. In response to the CMA's report, Ofgem set out an implementation strategy[11] in which we were clear that where appropriate we would refine the CMA's proposed design for the remedy to make sure it is effective. The team have used an agile approach to understand user needs and develop a service that meets them. As part of the evidence gathering process the team decided to test the viability of the CMA model, against an alternative variation.

2.6. The 'Best Offers letter' was conceived as a potential alternative way of encouraging engagement for customers on the database. Findings from consumer research showed that for many people, the hassle of finding a deal to switch to was a major barrier to switching. For those who were aware of Ofgem, they were seen as a trusted and independent organisation, who had customers' best interests at heart. The Best Offers letter aimed to remove this hassle by presenting customers with three cheaper deals available and consolidating the key information about those tariffs into one single letter.

2.7. If this remedy is successful, we expect to see a positive difference in customer switching behaviour (including consumers switching between suppliers or moving onto a new tariff with their existing supplier). It is also expected that improved engagement will deliver a more competitive market and, therefore, a reduction of the detriment associated with this AEC.

[11] See https://www.ofgem.gov.uk/publications-and-updates/cma-remedies-implementation-strategy.

3. Trial design

3.1. At the initial stage of this research, several design options and methodologies were considered. Some of these options involved discussions on how to choose suppliers, on the sample size, the nature of the intervention and the statistical approach to analyse the outcomes.

3.2. The Behavioural Insights Team (BIT) were commissioned to help Ofgem in designing the trial and drafting a trial protocol[12] which sets out the analytical approach and explains the key characteristics of this trial. This section provides a summary of the design in the protocol.

3.3. The trial aimed to identify changes in switching rates after the implementation of two policy options. This section describes the main characteristics of the trial in terms of which consumers were eligible, participation constraints, description of the intervention and control groups, key outcomes, randomisation and analytical methods.

3.4. It should be noted that the trial was not set up in a way that would allow us to make broader inferences about the likely effectiveness of the interventions at a national level. Constraints around building a large customer database and issues relating to data security meant that the trial was purposely small scale and intended to explore the reactions of consumers and suppliers under 'real life' conditions.

Eligibility criteria

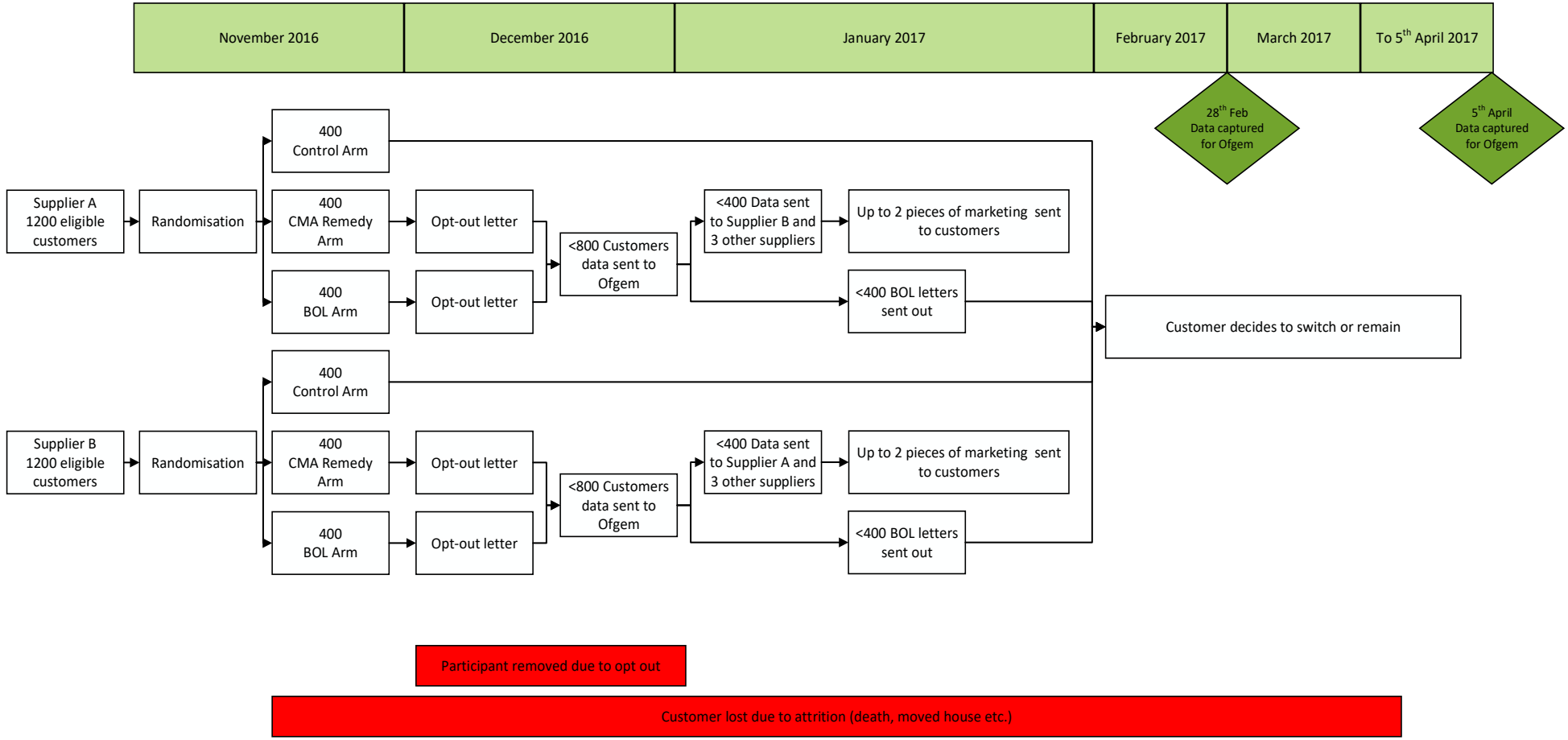
3.5. Eligibility criteria focus on disengaged consumers on Standard Variable Tariffs (SVT) for three years or more as defined by the CMA. To focus on this segment, we excluded the following consumers[13] whose choices are already limited for other reasons:

- Any customer on a default tariff for less than 3 years
- Customers without a dual fuel tariff (Single-fuel customers)
- Customers on the priority services register
- Customers who get warm home discount
- Customers who have any debt that would exclude them from being able to switch
- Customers who have any special communication requests e.g. large print or braille
- Customers who have non-standard metering arrangements, including pre-payment meters, restricted meters or smart meters
- Customers who are involved in another trial or marketing programme
- Customers who have opted out of receiving marketing (Section 11 customers).

[12] This protocol followed the CONSORT guidelines for RCTs. The trial protocol was drafted as a provisional/working document but is available on request.
[13] See table 1 in Annex 1 for ineligibility frequency data.

3.6. It is important to note that it was not possible to identify in our dataset the proportion of eligible participants with respect to the whole population of consumers on SVT for three years or more for the two suppliers who provided the sample. This limits our ability to make inferences to the whole population of consumers on these tariffs.

Figure 2 - Summary of intervention timeline



Description of the intervention

3.7. The trial consisted of three arms. There were two intervention arms and one control arm (business as usual):

- Supplier marketing arm (CMA approach)
- Ofgem "Best Offers Letter" arm
- Control arm

3.8. Participants for the three trial arms were chosen at random from two large energy suppliers (A and B), subject to the eligibility criteria outlined in section 3.5. At this stage, we thought that it would be a disproportionate action to request a random sample from all suppliers.

3.9. Supplier marketing arm (CMA approach)
In the first trial arm, data on these eligible customers in the Database (who had not opted out) were given to other energy suppliers. These suppliers were then able to market energy deals to them within a pre-specified time frame between 1st and 31st January 2017. Ideally, to improve comparability between the interventions, we would have given a strict timetable to each supplier, but we wanted to understand suppliers' response so chose not to apply too much constraint.

3.10. Best offers letter arm
In the second trial arm, Ofgem sent the customer a single letter which outlined three "best offers" in the market[14] tailored to that customer's annual energy use. The letter was sent on the 9th January 2017.

3.11. The control arm
Customers in the control group did not receive an opt-out letter and no personal information about the control group was transferred to Ofgem. They were monitored by their supplier and their decision to switch within the pre-specified time period was reported to Ofgem for analysis purposes.

3.12. Opt-out in the two treatment arms
The two treatment arms received a letter at the end of November from their own energy supplier which told them that unless they opted out their details were going to be shared with Ofgem. For customers in the CMA arm it was made clear that their details would be shared with other suppliers for the purposes of marketing.[15] If customers did not want their details to be shared, they could opt-out via email, post or phone within 28 days.

3.13. After the 28 day period the energy suppliers sent the details of the customers who had not opted-out to Ofgem. Note that, an option would have been to randomise customers after the opt-out stage. However, as the two interventions required different opt-out procedures, this was not possible. Figure 2 charts the flow of

[14] These offers were cheaper offers on the market that matched the customer's existing payment method.
[15] There were subtle differences in the content of the opt-out letters to explain how their data would be used.

participants from initial recruitment through to the collection of the final outcome data.

3.14. Figure 3 explains the intervention timeline and the key activities that took place. Note that we could not deliver exactly the same interventions by supplier and trial arm. This is because in some cases the logistics of working with five different suppliers[16] meant slightly different time lines and because the Best Offer Letter (BOL) and the supplier marketing letter (CMA) could not be coordinated perfectly.

Figure 3 – Timing of communications

3.15. The three key aspects that we could not totally control were as follows:

- The control group did not receive[17] the opt-out letter. This opt-out letter could be considered as an additional prompt to engage, and the impact on switching should be considered when making comparisons.
- The letter from Ofgem was sent on the 9th January 2017. The CMA letters went out at different times during the period 1st to 31st January 2017. This makes it difficult to compare how long customers had to react in the two intervention arms.
- During the duration of the trial both suppliers announced a price increase and some customers would have received a price increase notification (PIN) letter during the trial. There was also associated media coverage and a TV programme by Martin Lewis, an influential journalist, explaining energy tariffs and advising on switching. We believe these should have affected the three trial arms equally, but

[16] We chose participants from two main suppliers and three additional suppliers took part in marketing activities.
[17] Ideally we would have randomised after the opt-out letter but we considered the impact on consumers in the control group and decided to used analytical methods to reduce bias. See explanation in the analytical section.

could have influenced baseline switching rates and potentially had an interactive effect with the other letters customers received.

3.16. It was not possible to ensure the same cut-off for the two arms. In other words – given the variable timing of the interventions and the fact that customers in the CMA arm likely received multiple offers at different times, it is not appropriate to measure switching relative to when letters were delivered. Instead, it is more appropriate to measure switching over a fixed time period, recognising that customers may have been prompted earlier or later in the CMA arm.

3.17. There seems to be different responses to the opt-out letter. Since we are not sure when consumers received their intervention, we should be careful when comparing the BOL and CMA interventions. To mitigate this problem, we have collected data up to the point where there was no further increases in switching compared to the control. Using a longer period removes the possible bias generated by giving some consumers longer time to switch.[18]

3.18. We note that this approach may have introduced bias, as customers in the two intervention arms may behave differently in response to opt-out letters. However the opt-out rate was below 2%, which allows us to conclude there wasn't a significant bias from this aspect of the trial design. While we acknowledge that the two interventions are not strictly comparable due to the differences in opt-out and marketing letters, the opt-out rate was not big enough to create problems of sample selection bias.

Primary and secondary outcomes

3.19. The overall objectives of the trial were to understand:

1. How customers behave in response to receiving on 'opt out' letter;
2. How customers respond to receiving postal marketing from other; suppliers (the CMA proposed approach);
3. How customers respond to receiving personalised information about tariffs from other suppliers consolidated into one postal communication from Ofgem;
4. How suppliers behave in response to receiving data on customers from rival suppliers

3.20. The primary outcome measure is whether a customer signals their intention to switch (either to a different tariff with their own supplier or to another energy supplier) within the pre-specified time frame (23rd November – 28th February). The primary analysis compared switching rates amongst all participants in each arm, including those who opted out. We chose the metric of customers requesting a switch rather than completed switches because a) it takes longer for that information to be available and b) switches can fail for reasons outside our control

[18] Assuming that all consumers had enough time to switch the difference in timings should disappear towards the end of the period.

(e.g. outstanding debt to an energy supplier). It means the analysis is based on the initial treatment assignment and not on the treatment eventually received.[19]

3.21. In our analysis the treatment of opted-out/ineligible participants may be influenced by the opt-out letter, which was different under the three different trial arms. As opt-out rates were negligible and ineligibility was uniformly distributed across the three trial arms (See table A1 in the Annex), we don't think this poses a serious problem to the validity of the results. However, we should consider the two interventions as the combination of "Opt-out CMA and Intervention CMA" and "Opt-out BOL and Intervention BOL."

3.22. In addition to gaining a better understanding of baseline switching rates and learning lessons for further research, a key secondary outcome was the quality of switching. This was measured by:

- Estimated savings from switching using tariff information
- Number of complaints, calls and engagement measures

Sample selection and randomisation

3.23. Ofgem was mindful of the risks associated with handling and processing large volumes of customer data so we purposely kept the trial small scale. A total sample of 2,400 eligible customers from two suppliers were randomly selected to participate in the trial. Each of the two suppliers received instructions to pick 1,200 customers and allocate them into the three trial arms by stratified randomisation to ensure a representative sample and balanced trial arms. The power calculations aimed at being able to detect[20] a 0.5pp and 2.6pp increase in switching rates from an assumed baseline switching rate of between 0.5% and 5% respectively.

3.24. To ensure balanced trial arm groups we used stratified randomisation to select and allocate participants. Stratified sampling divides the population of eligible customers into different blocks based on their characteristics (the strata) and selects a random sample from within each stratum. For the trial, we used electricity, gas consumption and time with supplier to stratify the sample.

3.25. After each supplier randomly picked[21] and allocated their customer sample, suppliers provided data to Ofgem and BIT conducted balance checks on the random trial arm allocation. The balance tests showed no significant differences on 5% significance level and only one significant difference on the 10% level in observable characteristics between each arm.

3.26. Furthermore, we also carried out ex-post balance checks to ensure that the treatment allocation is balanced. These checks are reported in the next section.

[19] This is known in the literature as Intention to Treat (ITT).
[20] With 80% power and 95% confidence level.
[21] Our trial protocol provided step by step instructions for the randomisation process.

Analytical model

3.27. The initial analytical approach combined regression methods and descriptive statistics. For the primary outcome – in order to account for sample selection bias from the opt-out process – we initially proposed a Heckman selection model. We also proposed a series of probability models to estimate the impact of the intervention in a number of secondary outcomes.

3.28. However, only 45 participants opted out. With such a low number of opt-outs it was not practical to run a sample selection model. We consider that the possible bias from opt-outs would be negligible so, instead, we opted for a probability model. Since the outcome variable (switching) could only take two values (Yes or No) the data was analysed using a probit model.[22]

$$\Pr(Switch = 1) = \theta(\beta_0 + \beta_1 D_1 + \beta_2 D_2 + \gamma_i X_i) \qquad (1)$$

Where: θ is the cumulative normal distribution, β and γ are the regression coefficients, D_i is a dummy for each of the interventions and X_i is a vector of covariates controlling for customer characteristics.

3.29. To control for drivers of switching (covariates), we requested additional data on engagement indicators and socio-demographic characteristics of consumers. In the engagement clusters we have the following variables:

- Received marketing from current supplier in past year
- Submitted own meter reading in last year
- Payment method
- Volume of calls made to current supplier in past year
- Volumes of marketing (For CMA arm only[23])

3.30. The socioeconomic controls were:

- Annual Electricity Consumption
- Age of contact person
- Annual gas consumption
- Consumer requests opt out
- Number of household members/ occupants
- Dwelling type
- Number of bedrooms
- Type of tenure
- Customer becomes ineligible during duration of trial
- Reason for ineligibity

3.31. The virtue of RCTs is that they generate unbiased estimates of the average treatment effect regardless of whether we can take account of other causes or

[22] The initial specification was a Heckman selection model to control for bias from the opt out mechanism. However, as there was a negligible amount of opt out we decided to use a probit specification.
[23] The BOL arms received one letter only and we assume the control group received none.

not. So our regression method without controls is unbiased and equivalent to a two sample mean test. However, covariates reduce the variability in outcomes and increase the precision of the estimates. They also help in segmenting the population and check the integrity of the randomisation.

3.32. The fitted model did not result in enough explanatory power to increase the precision of the estimates, against more straightforward bi-variate tests. Therefore, we slightly adjusted our initial plans and for reporting secondary outcomes using average effects on intention to treat basis as in:

$$ITT_y \equiv E\left[ITT_{i,D}\right] = E[d_i(0)] - E[d_i(1)] \quad (2)$$

Where ITT_y is the intent to treat effect of z_i on d_i for each subject i where z = [0,1]

3.33. In addition to our regression results, we have provide descriptive statistics about the overall percentage of switches that were external and internal by trial arm. We have also reported opt-out rates, comparisons of internal and external switches, annual savings and household characteristics.

4. Data verification

4.1. The quality of data is key to understand the robustness of the results of the trial. If randomisation worked well, the differences between the three trial arms should be attributed to the intervention. Moreover, we have collected data on switches, tariffs and consumer characteristics to carry a number of data checks and data verification work prior to the analysis.

4.2. We carried out a number of ex-post controls. First, we checked for outliers and data anomalies. After checking the quality of the dataset, we carried out other ex-post checks to verify whether the randomisation process had produced a balanced distribution across key characteristics.

4.3. Table A1 in the Appendix shows the number of customers that became ineligible during the trial. A total of 24.5% of customers became ineligible for a number of reasons.[24] For example, people moved house or fell into debt. The distribution of ineligible customers was spread across trial arms, although customers in the CMA group experienced a slightly lower level (19%).

4.4. Table A2 shows the number of people who opted out. Only 45 people opted out so we decided there was no need to correct for selection bias that could have been introduced by the opt-out process. The number of customers who opted out was larger in the CMA group (30 compared to 15) but it was still a low number. Due to the low number of observations, it was not possible to check if this was a sign of lesser or greater engagement in this trial arm.

4.5. Table A3 shows that there is a large proportion of people in the sample above the age of 65, but there are no differences between the trial arms (this could be due to a significant large proportion of people above this age meeting the trial eligibility criteria).

4.6. Finally, tables A4 to A10 show the distribution in the trial arm across a number of potential drivers of switching behaviour. These tables shows a fairly regular distribution across the trial arms in terms of the following characteristics:

[24] Although this figure seems large, we need to consider that eligibility for this trial had a strict qualification criteria and participants were selected for the trial 4 months before the end of the trial. The time of the trial, from January to March, could also have had an effect on eligibility as this is one of the most active periods in the market.

- Energy Consumption
- Payment method
- Region
- Number of household members (low numbers above 5 members, otherwise no significant difference)
- Dwelling type

4.7. Tables A1-A8 in the Appendix show that the trial is well balanced[25]. Therefore, we are confident that the differences between the trial arms should be attributed to the intervention.

[25] We run a series of Chi-square tests, which did not find any significant issues in terms of the distribution of key variables across the three trial arms.

5. Results

Primary Outcomes

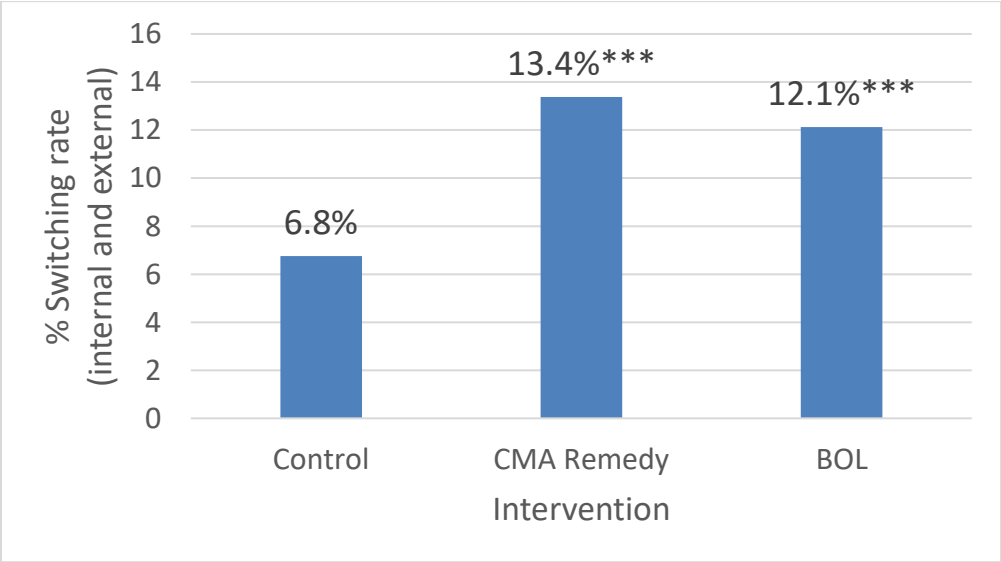
5.1. The results from the analysis are encouraging. In accordance with our analysis specification we compared switching requests from 23rd November 2016 to 28th February 2017. As shown in Table 1, on average participants in the control group experienced a switching rate of 6.75%, participants in the BOL trial arm 12.13% and in the CMA arm 13.38%. These results were statistically significant at the 99% confidence level.

Table 1 - Key outcomes

Switching requests	Control Freq (Percent)	BOL Freq (Percent)	CMA Freq (Percent)
NO	746 (93.25%)	703 (87.88%)	693 (86.63%)
YES	54 (6.75%)	97 (12.13%)	107 (13.38%)
Total	800	800	800

5.2. This means that the letters sent by suppliers in the CMA group increased switching by around 6.6 percentage points and the letters sent by Ofgem by 5.4 percentage points compared to the control group.[26]

Figure 4 – Switching (internal and external) by trial arm

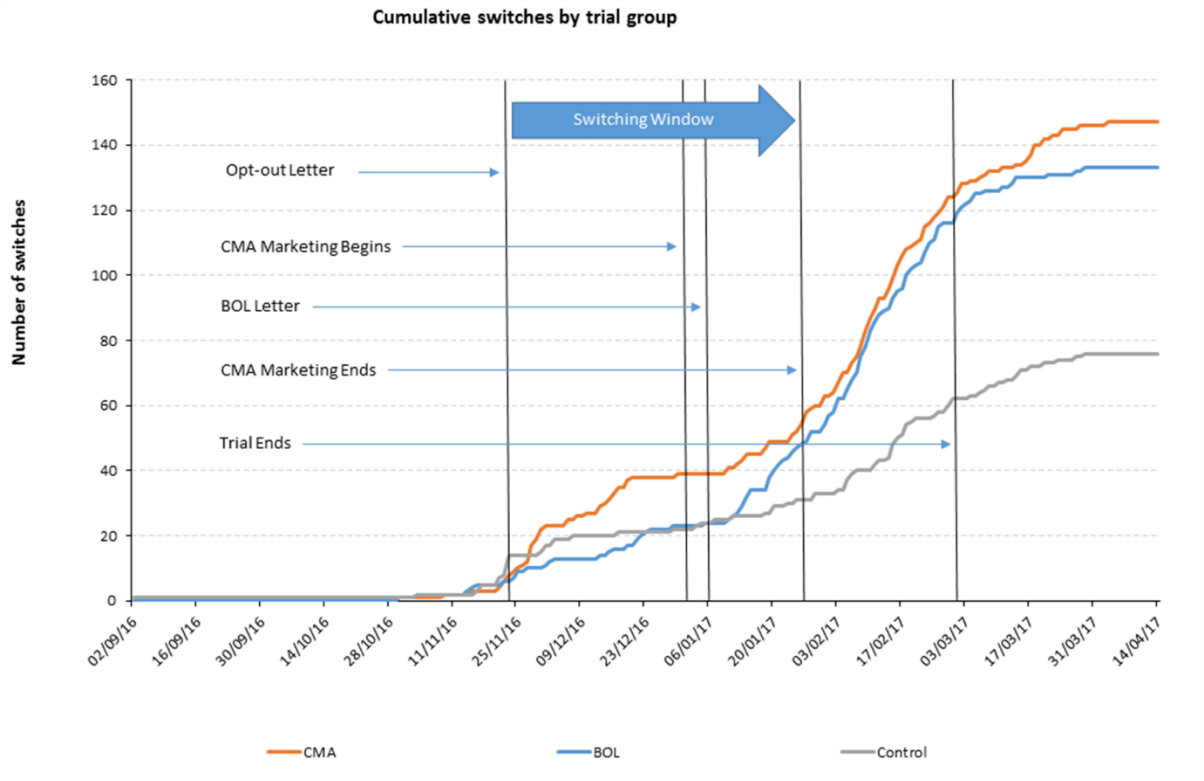


[26] When rounding is applied.

5.3. The baseline switching rates and effect size observed were higher than previously expected.[27] Although we would expect switching to be higher in winter months, eligibility for this trial focused on consumers that have not switched for three years or more. The rate of switching in the Control arm during the 14-week trial period was 6.8%, which is higher than historic rates.

Switching over time

Figure 5 – cumulative switches



5.4. Figure 5 shows the cumulative trend of switching requests by trial arm against the control group. It records all switches that took place from 28 November 2016[28] to the 14 April 2017. It also shows the key milestones of the trial. The period from 25 November 2017 to 06 January 2017 was the opt-out period. It is interesting that during the opt-out period there was a large number of switches in the CMA arm (this difference is not significant due to small numbers). Due to practical issues associated with synchronising the activities of all suppliers, the period of intervention was different for the CMA and the BOL groups[29] so they have slightly different switching windows.

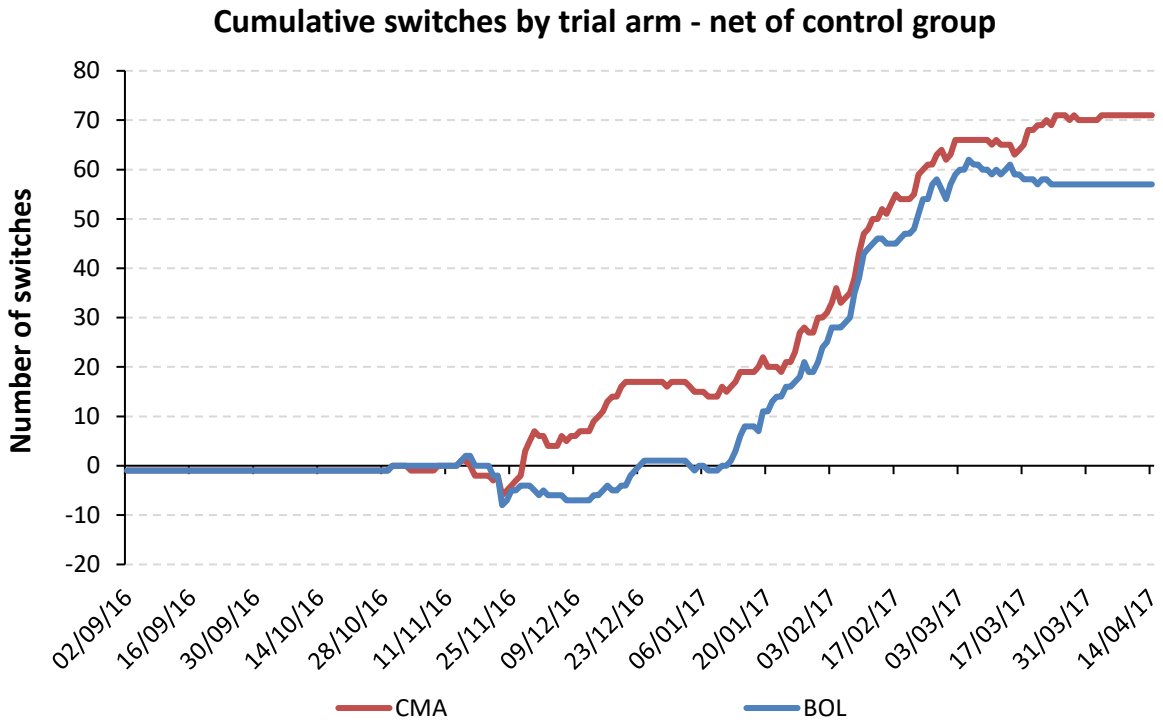
5.5. There were some external factors outside the control of the trial that could have influenced switching. During the trial period, price increases were announced at

[27] https://www.ofgem.gov.uk/data-portal/large-suppliers-internal-and-external-switching-rate-fuel-type-gb
[28] A very low proportion of switches took place before 28 November 2016 because randomisation took place ahead of sending opt-out letters.
[29] See section 6 for further analysis.

different times. One of these in particular received a lot of attention by national media. There were other factors that could have influenced switching rates. For example, the winter period could have a positive impact on switching as well as the fact that, coinciding with the trial period, there was a programme on energy prices by Martin Lewis (and influential TV journalist and money saving expert).

5.6. Figure 6 shows cumulative switches in the BOL and CMA groups net of the control. It represents the impact of the two interventions. From the time of the opt-out letter (25 November) to the intervention date (6 January) the BOL switching rates are quite similar to the control group. However, there is a clear increase in the CMA group. It is difficult to know the reason for this difference because Ofgem did not have full control of this intervention.

Figure 6 – cumulative switches – net of control



5.7. However, it is quite interesting to see that by the end of March there are no further increases in the BOL and CMA groups with respect of the control. We could make two inferences from this graph. First, it seems that although the beginning of the trial shows some unexplainable outcomes, after an abrupt start the switching trend grows smoothly. Second, the effects of the intervention seem to have lasted until the end of March, longer than the 4 weeks in our initial plan.

5.8. For this reason, we have analysed results both for the period ending 28 February and up to the end of March, when both the two cumulative curves in Figure 4 become flat, meaning that no additional switches took place. The results are not qualitatively different but, naturally, we observe a higher switching rate over the longer period relative to the control.

Regression results

5.9. The key question of this RCT was to identify the impact of the interventions on switching rates. Table 2 shows the impacts of the BOL and CMA intervention against the trial arm using the model specified in equation (1).

5.10. We ran four different models including all 2,400 participants. First, in columns 1-2, we selected cases up to the 28th of February (Jan-Feb). In columns 3-4 we use all switches in the data collection period (Jan-April). We also ran a reduced model (columns 1-3) which are equivalent to the ITT estimate in equation (2) and a full one (columns 2-4) using all the available covariates available in our data set, equivalent to equation (1).

5.11. Columns 1-4 in table 2 below show the marginal coefficients for each of the variables. For example for the two intervention groups (BOL and CMA) it shows the impact against the control group. In addition to the impacts of each of the interventions, the model produces some evidence on the impact of the covariates on switching behaviour.

Table 2 – regression results

VARIABLES	(1) Jan-Feb No Controls	(2) Jan-Feb	(3) Jan-March No Controls	(4) Jan-March
Trial arm = BOL	0.0537***	0.0564***	0.0725***	0.0766***
	(0.0146)	(0.0144)	(0.0169)	(0.0165)
Trial arm = CMA	0.0662***	0.0618***	0.0875***	0.0848***
	(0.0150)	(0.0145)	(0.0172)	(0.0166)
Supplier B		0.0528***		0.114***
		(0.0166)		(0.0192)
Payment method = Receipt of bill		-0.0303*		-0.0346*
		(0.0155)		(0.0184)
Type of tenure = Private renting		-0.00461		-0.0225
		(0.0217)		(0.0240)
Type of tenure = Social renting		-0.0274		-0.0499**
		(0.0191)		(0.0214)
Number of household occupants		0.00232		0.00432
		(0.00539)		(0.00601)
Calls made to current supplier last year		0.0191***		0.0210***
		(0.00369)		(0.00428)
Region = East Midlands		-0.00784		-0.0165
		(0.0313)		(0.0358)
Region = London		-0.0263		-0.0365
		(0.0287)		(0.0336)
Region = North Wales		-0.0568*		-0.0745**
		(0.0294)		(0.0346)
Region = West Midlands		-0.0286		-0.0428
		(0.0277)		(0.0320)
Region = North East		0.0217		0.000385
		(0.0326)		(0.0358)
Region = North West		0.00315		0.00421
		(0.0299)		(0.0343)
Region = Southern		-0.00782		-0.0490
		(0.0307)		(0.0335)
Region = South East		-0.0581**		-0.0610**
		(0.0241)		(0.0294)
Region = South Wales		0.00536		-0.0249
		(0.0524)		(0.0558)
Region = South West		-0.00369		0.00446
		(0.0319)		(0.0378)
Region = Yorkshire		-0.0174		-0.0563*
		(0.0269)		(0.0297)
Region = South Scotland		-0.0569		-0.100**

		(0.0382)		(0.0410)
Region = omitted (North Scotland)		-		-
Annual Electricity Consumption		-1.49e-06		-1.27e-06
		(3.33e-06)		(3.79e-06)
Annual Gas Consumption		6.34e-07		1.78e-07
		(9.44e-07)		(1.08e-06)
Dwelling type = Detached		-0.0475		-0.0242
		(0.0295)		(0.0325)
Dwelling type = Flat		-0.0576*		-0.0370
		(0.0325)		(0.0365)
Dwelling type = Semi		-0.00197		0.0223
		(0.0283)		(0.0304)
Dwelling type = Terrace		-0.0402		-0.0303
		(0.0279)		(0.0300)
hassubmittedownmeterreading = Yes		0.0615***		0.0632***
		(0.0141)		(0.0165)
receivedmarketingfromcurrents = Yes		0.0458***		0.0682***
		(0.0154)		(0.0172)
Agegroup = 41-65		-0.0870***		-0.0743***
		(0.0210)		(0.0231)
Agegroup = 2, >65		-0.0597***		-0.0453**
		(0.0198)		(0.0213)
Observations	2,400	2,393	2,400	2,393
Pseudo R2	0.013	0.082	0.014	0.084

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

5.12. The reduced model calculates the probability of switching using only a dummy for each intervention (columns 1 and 3 in table 2). This method produces exactly the same result as a frequency count along with measures of association (See Table 1). For example, the switching rate in the BOL arm in Table 1 was 12.13% and in the Control was 6.75%. The difference between these two is 5.38pp, which is the same (except for rounding errors) as the figure reported in the first row, column 1 of Table 2.

5.13. Columns 2 and 4 use the same model, but add covariates controlling for engagement and socio-demographic characteristics. Because randomisation produces groups that are in expectation similar across important characteristics, the impact on switching of the intervention is similar to the results in Columns 1 and 3. This provides further reassurance of the validity of the randomisation.

5.14. Some of the interesting results from column (4) is that the supplier B made a significant positive impact on switching of 11pp compared to Supplier A. This is not surprising given the attention in the media to their PIN although the effect could also be driven by other factors. Payment by receipt of bill decrease switching by 3pp compared to Direct Debit and people living in flats experience a 5pp decrease compared to other types of accommodation, perhaps because people in flats are more likely to be renting.

5.15. It is interesting to note that energy consumption did not have a significant effect on switching but variables relating to engagement i.e. submission of meter readings and number of calls made had a significant impact of 6pp and 2pp respectively. This means that people who have previously engaged with their supplier in some way are more likely to respond positively. The number of marketing letter received also had a significant impact on switching of 5pp. The more letters received, the more likely switching was. In this context the performance of the BOL is particularly encouraging given it was just one single communication.

5.16. We divided the sample into three age groups 18-40, 41-64 and 64 plus. Younger consumers were more likely to switch whereas people in the age 41-64 were 7.4pp less likely to switch and 65 Plus 4.5pp less, compared to the 18-40 group. It is also worth noticing that people in the age group 65 plus represented 57% of participants (see table A3 in Appendix I).

5.17. Despite being able to identify the impact of the intervention with respect to the control group robustly, the specification of the model was quite weak. A low pseudo-R2 indicates that the model has low explanatory power. In particular, a large number of the variables were not significant. Surprisingly, energy consumption does not seem to be an important factor explaining the probability of switching. Many sociodemographic variables are not significant, except for age of the contact person.[30] However, it is worth mentioning that all engagement variables are highly significant.

5.18. As the regression approach did not drastically improved the precision of our estimates, for the rest of the analysis we decided to use an approach based on descriptive statistics.

Secondary outcomes

Internal versus external switching

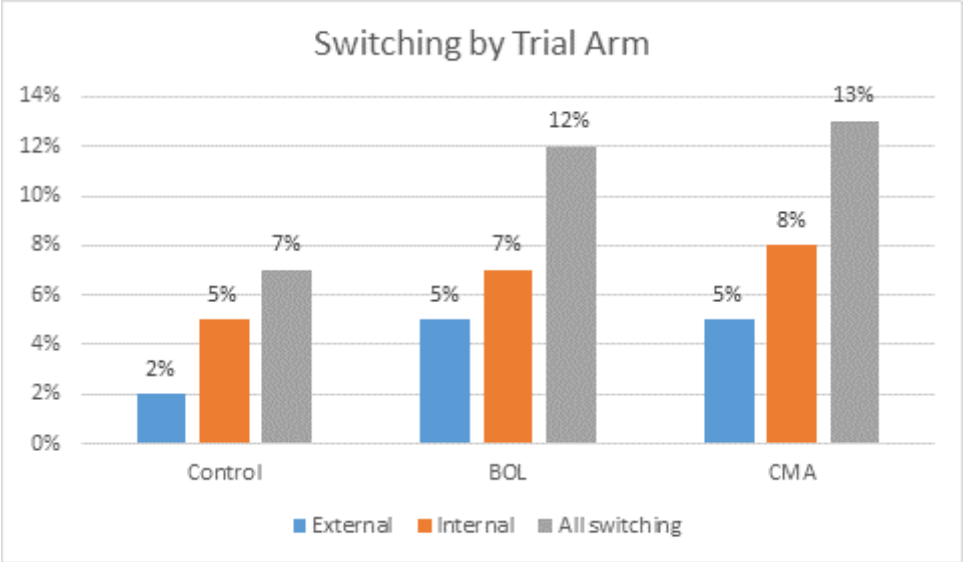
Table 3 – External and internal switching

External or internal switch?	Supplier name Supplier A Freq (percent)	Supplier B Freq (percent)	Total
No Switch	1,075 (89.6%)	966 (80.5%)	2,041 (85.0%)
External	48 (4.0%)	113 (9.4%)	161 (6.7%)
Internal	77 (6.4%)	121 (10.1%)	198 (8.3%)
Total	1,200	1,200	2,400

5.19. Table 3 shows internal and external switches by energy supplier. In general the amount of internal switching was higher. The internal switching rate was 8.3% compared to a 6.7% external switching rate. In addition, the number of switches was slightly higher for Supplier B (19.5% vs 10.5% for supplier A).

Figure 7 – Internal and external switching by trial arm

[30] Some of these variables may turn out not significant because they have a small effect and the sample size was not big enough to identify it.



5.20. When looking by trial arm we see that in all arms, customers were more likely to switch internally (i.e. change tariff with their existing supplier) than externally (i.e. change supplier). It may reflect customers' desire to avoid perceived 'hassle' in changing supplier and/or their ability to use a BOL or marketing material as leverage to switch to a cheaper internal tariff.

Quality of switch (savings)

5.21. To explore the quality of switching, we explored whether switching led to significant savings or not. We recommend caution in interpreting these results, however, because we had very little information on tariffs for external switches, and information on new tariffs only covered 61% of all switches. We estimated savings by switch using last year's consumption data combined with information from the old and new tariffs. Table 4 shows that the average savings from all switches was £131 per year, and there were no significant differences between each of the trial arms.

Table 4 – switching savings

	Average savings from switching (£)			
Trial Arm	All fuel	Electricity	Gas	Number of Observations
CMA	131.14	23.68	107.46	105
BOL	128.27	24.27	104.00	62
Control	135.16	15.60	119.56	52
Total	131.28	21.93	109.35	219

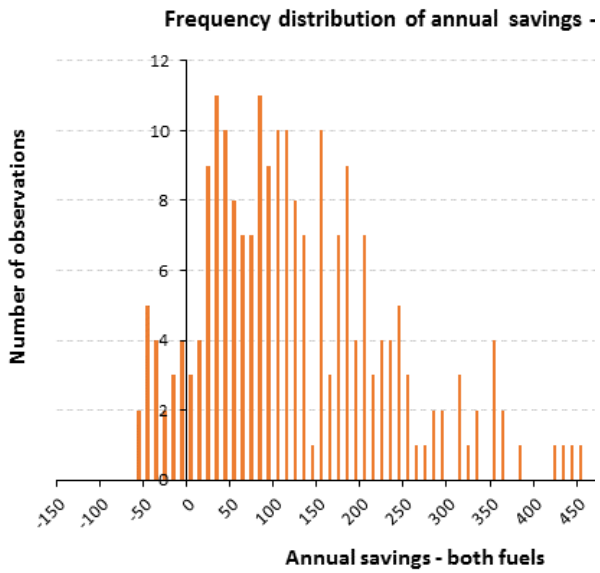
5.22. Table 5 shows savings by external and internal switches. Unfortunately, we have limited information available, particularly for external switches so we cannot draw any meaningful conclusions from this data.

Table 5 – savings by switch type

	Internal Switches		External Switches	
Trial Arm	Average savings	N. of Observations	Average savings	N. of Observations
CMA	134	83	121	22
BOL	128	62	-	0
Control	135	52	-	0
All	132	197	121	22

5.23. In addition to average savings, it is interesting to look at the distribution of these savings. Figure 8 shows that 50 per cent of switches achieved significant savings (above £100) on total energy bills. It also shows in a small number of cases the switches resulted in negative savings. We don't have any data on the reasons why these customers chose the tariff they did, and therefore cannot explain this fully. However, one hypothesis is that given the price rises that were announced during the trial, some customers may have made decisions about which tariff to switch to knowing that their prices would be increasing. Indeed, we know that the vast majority of these customers are likely to have saved money from their switch, as the cost of their previous tariff subsequently went up. It is also important to consider other non-price factors involved in any switch (e.g. tariffs with long-term fixes, no exit fees, supplier brand effects, other benefits etc).

Figure 8 – distribution of savings



Other outcomes

5.24. We also compared other trial outcomes. The number of calls is significantly higher from those who switched compared to consumers that did not switch (See Table A11 in the Appendix). For example, 61% of consumers that did not switch did not make any calls compared to 36% of those who switched. No complaints were registered.

5.25. We were also interested in how suppliers would respond to the trial. Suppliers were restricted to sending no more than two letters per customer (to ensure the customers were not overwhelmed with information). The proportion of consumers

that received 6 letters was 75% and only 11% did not receive marketing[31]. In general there were very few consumers that received between 2 and 5 communications (See table 6).

Table 6 – volume of marketing letters sent (CMA arm)

Volumes of Marketing	Percentage of Consumers
0	11%
1	0%
2	7%
3	0%
4	5%
5	3%
6	74%

5.26. We are confident that the results presented in this section identify the impact of the intervention on switching rates robustly. We have also observed an increase in baseline switching with respect to historical switching rates. In addition, the regression results suggest that some drivers of switching behaviour could be age, dwelling type, supplier and other variables closely related to engagement such as submission of own meter readings. On the other hand, we found that energy consumption does not impact on switching rates. The results of this trial provide limited information on the quality of switches.

5.27. However our conclusions need to reflect there is considerable uncertainty driven by a number of factors. Some of our concerns are the lack of data on quality of switches, the lack of a well-specified econometric model explaining switching behaviour, and a number of specific design feature of the RCT which would be difficult to replicate in other circumstances.

[31] It is not possible to assess the reasons why 11% did not received any marketing.

6. Qualitative findings

Methodology

6.1. Following the RCT, Ofgem's Consumer Research and Insight team conducted qualitative research with customers, in the form of in-depth telephone interviews, to understand their experience of receiving a BOL or CMA marketing, and any actions which followed.

6.2. The two suppliers in the trial recruited customers for the interviews, and only participants who had not opted out of being included on the database were included in the sample. The actual interviews were conducted in-house by Ofgem staff during the period of 20th February to 8th March, after the switching window had closed, but soon enough afterwards that customers could still recall receiving the BOL or marketing.

6.3. 50 interviews were scheduled and 36 were completed. The breakdown of the sample is shown in Table 7.

Table 7 – qualitative sample breakdown

Interviews Scheduled	50
Interviews completed	36
Refused/void	16
Supplier A customers	20
Supplier B customers	14
CMA Arm	17
BOL Arm	17

Findings

6.4. As with any qualitative research, these findings give a sense of customers' opinions and experiences, but cannot be assumed to be representative of the wider trial sample.

The Opt Out Process

6.5. Possibly due to the passing of time, the majority of those interviewed did not remember receiving the opt-out letter; however, of those who did remember receiving the letter, most said that they understood it. Only a small minority told us they did not opt-out because they wanted to see the energy deals.

6.6. The opt out letters were sent in December 2016 and the interviews were conducted in later February/March so there was a fairly long time between

receiving the letter and the interview. As such it is not surprising that the majority did not remember receiving the opt out letter, as recall after that length of time is an issue. A total of 21 did not remember receiving the opt out letter at all and 13 had some recollection. Of those who remembered seeing the opt-out letter, there was an even split between those who did something and those who did nothing.

6.7. Of those who did register receiving the letter the majority reported understanding it. There is very little evidence from the people who recalled the letter that they didn't understand it. A few respondents sounded confused when they talked about it, "it was odd" for example knowing they'd had a letter about data sharing, but not recalling any other information, or thinking the letter was about offers.

6.8. Only a small minority told us they did not opt-out because they wanted to see the energy deals. Of those who remembered seeing the opt-out letter, there was an even split between those who later went on to do something and those who were not engaged by the trial.

6.9. Of those interviewed, only a small number (five) actively decided not to opt-out. Three wanted to see the deals, and were fully engaged, for example they kept the opt-out letter, and waited for the deals to arrive. One respondent, although interested in the offers, told us they did not feel they had much control when it came to their personal data anyway "it's what they do". A couple of respondents told us that they understood, were not very interested and just did not feel the need to opt-out.

6.10. Another respondent understood and wanted to discuss the letter with a family member before they did anything, and so ended up not opting-out by default.

6.11. In this sample, for the majority it did not really register, for those who did retain the information very few actively engaged with it. Respondents also told us they were too busy (it was not a priority), and that they were "not interested".

Customer understanding of the next steps to the opt out letter

6.12. Other than those who had actively decided not to opt-out, there is little evidence that the "next steps" of the letter really registered.

6.13. Seven respondents told us that they had read the opt-out letter and been aware that because they had not opted-out post would come. These were the five who told us they had actively decided not to opt-out, the respondent who had wanted to discuss the letter with a family member before they did anything and another who had just ignored the letter, as they were not interested.

6.14. For the others it was not so much that they did not understand the next steps, but that they had not engaged the letter, and were not able to discuss it, as they did not remember it.

Customer response to CMA marketing

6.15. The majority remembered receiving marketing and were able to recall some details of it – though few knew exactly. Interestingly the four who said they had

not received any marketing all acted in some way – though they told us it was not because of the trial.

6.16. Out of 17 in the CMA arm, the majority remembered receiving something. Most understood that they were on an expensive deal and around half recalled details of at least one saving.

6.17. Some respondents reported understanding that they were not on a cheap deal, which implies that they had at least skimmed the marketing. There was often a vague awareness of either deals or alternative suppliers.

6.18. Recall of the actual suppliers not was as strong. Many were not able to say exactly who had contacted them.

6.19. Three of our sample engaged with the market because of the marketing they received; one changing supplier and two changing tariff. Two of those three respondents were able to name at least two suppliers who had contacted them. The other was annoyed by the marketing, he felt like a lot came through and didn't engage with it (but later visited a PCW, and changed tariff with his existing supplier as he did understand he wasn't on the best tariff).

6.20. Four were prompted to do something (e.g. think about the deals, or go on a PCW) by the marketing, of those, one told us that they received marketing from one supplier, and looked them up on the internet and was able to recall the bad review they had read. Others were often able to recall a small amount of details, for example the name of one supplier or the details of one saving, but they reported barriers to investigating further such as feeling the saving was too small or feeling they didn't have the time to do anything further. One member of this arm was prompted to contact their supplier, but he thought the only marketing he received came from his supplier, so he was probably actually prompted by a PIN.

6.21. A further six recalled receiving CMA marketing and were not engaged, they often had very little recall of the detail of the marketing they received.

Customers' reaction to the marketing

6.22. Respondents were often quite negative about the marketing, a few reported it being "intrusive" and thrown straight into the recycle bin (or wood burner). However, there was often a vague awareness of either deals or alternative suppliers and there was comparably more activity in this group.

6.23. Of the three who switched supplier or tariff actively, one compared the three pieces of marketing they received and chose the cheapest offer. Another was prompted to go onto a PCW and changed supplier as a result. One respondent reported that over a period of about 6 weeks it had felt like they were getting letters, deals, offers every day. This respondent told us they had never heard of some of the companies who sent him offers. And wouldn't trust them, he went on a PCW to look at what he could save and called his supplier, "to get a better deal" he eventually changed his tariff.

6.24. Very few made positive comments about the marketing, an example of a more positive comment being "It was all right, but not enough savings". Of those who

considered acting on what they received, one researched a supplier they had received marketing from, but was put off by a bad review, one decided the saving was too small and another felt they didn't have the time to do anything further. Another respondent was prompted to visit a PCW by the marketing, but also had not done anything further.

6.25. Another respondent who recalled the marketing but was not engaged had just bought a wood burner and wanted to see how the wood-burner effected their bills before they changed. They reported being very annoyed with the "intrusive" marketing and throwing it in the wood burner. They did however change tariff, but they told us that this was due to receiving a PIN.

6.26. Reasons participants gave for not engaging included not want to get involved with changing tariff. They gave reasons such as not being interested in "wheeling and dealing"- and just wanting to be "charged for energy and pay for it". One told us that they skim read the marketing and noticed they could save £200 – but told us that it seemed like too much effort to do anything about it. Therefore, they just put it in the recycling bin. Another told us not having access to a computer to check the deals was a barrier.

6.27. Interestingly, of the four who did not remember receiving marketing, all took some action, though they all said it was not because of the trial. One respondent had received the opt-out letter and waited to receive marketing, but told us none came through. This prompted him to call his supplier to check the deal he was on. He told us that he was not interested in switching supplier. "I'm one of those rich pensioners you hear about". He did not feel the need to save money, but told us that he might consider a fixed tariff (he had read about SVTs it in the newspaper). The respondent who had considered opting out but not had a chance to discuss it with a family member did not remember receiving any marketing, but changed tariff when her direct debit wasn't covering her energy consumption. Two respondents switched tariff and supplier, though both claimed it had nothing to do with the trial.

Customer response to the BOL

6.28. Although participants were much more positive about the BOL than the CMA, there was comparably very little reported action. Everyone was happy to receive the letter from Ofgem, the supplier sending it would be "weird".

6.29. In comparison to receiving the CMA letters, nobody expressed a negative reaction to receiving the BOL letter, however in the sample we spoke to it did not prompt as much action. We didn't speak to anyone who changed supplier as a result of receiving the BOL, although one person did change tariff, and another contacted their supplier to ask about their tariff.

6.30. All the respondents we spoke to who recalled the BOL understood it and many described it as "clear". The respondent who changed tariff told us that the BOL was "easy to understand" and well laid out with good deals. She told us that it made her 'sit up and take notice'. She rang her current supplier and used the evidence of better deals elsewhere to ask for a cheaper deal, saying that she did not want to go with an "unknown" supplier.

6.31. One respondent told us that they had been thinking of switching. When they received both a PIN from their supplier and the BOL, the combination was enough push to investigate other offers through a PCW. In this case, it was clear that the trial sparked interest, but the PIN sparked action.

6.32. For those who received the BOL letter and considered the offers unknown suppliers and, for two respondents, perceived lack of information about non-direct debit offers were both identified as barriers. Not recognising the supplier was raised as an issue by some, but not all. Some told us they felt savvy enough to go onto a PCW if they wanted to switch.

6.33. Respondents who chose not to engage reported not being interested and the letter not being a priority. One respondent had switched supplier 5 years ago and so told us that she had "already switched".

6.34. The same proportion to the CMA arm did not remember receiving the letter, however in this case they tended not to do anything. In some cases they thought a partner might have opened it and chosen not to leave it out for them. A couple of respondents thought they would have used it if they had seen it.

6.35. Nobody objected to Ofgem sending the letter (although it was raised that they should not have to). It was often seen as a "government communication", which it would be "weird" for a supplier to send. Ofgem was seen as "impartial", "they make sure it's fair".

6.36. Most participants did not express surprise that they were on an expensive energy tariff [but this would have been flagged by the interview invite]. Some were not concerned by this, but many looked at the communications and considered the offers.

Qualitative conclusions

6.37. Of those customers interviewed, very few ultimately switched to one of the suppliers who they received information about, most common actions were:

- Prompted to look on a price comparison website (PCW) and look for a better deal
- Prompted to call supplier with the idea of using it as evidence to negotiate a cheaper tariff

6.38. For those who did not change supplier or tariff, common reported barriers included:

- Not recognising the suppliers (although not for everyone)
- Not having the time/ internet access to research further (it was easier in this situation for some to just call supplier)
- Not wanting to be drawn into a situation where they are "wheeling dealing"
- Perceptions of untrustworthy suppliers, deals that would "eventually go up anyway"

6.39. Both suppliers sent out a price increase notification (PIN), which is mentioned by some respondents as an additional trigger to action, respondents also mentioned

hearing about high energy prices, e.g. reading about energy prices in the newspaper.

6.40. Reactions were often negative in the CMA arm, describing it in terms such as "intrusive", and some objecting to receiving so much post. In our sample (which was skewed to provide a broad range of scenarios), it did engage the most people, often to change tariff with their existing provider. A few of the people in the sample who had acted claimed not to have seen any marketing – though there is the possibility that the increased post still primed them for action.

6.41. Reactions were more positive to the BOL letter, which was understood and considered clear, however respondents had far less to say about it, and although due to the small sample a conclusion can't be reliably drawn, it does seem this communication was more easily dismissed.

7. Conclusions and recommendations for future research

7.1. The results of this trial have been encouraging. A postal intervention on eligible consumers increased switching rates compared against a control group between 5pp and 8pp, depending on the intervention and the duration of the assessment period.

7.2. The baseline switching rate for the control group was between 6.8% and 9.6% for a switching period of 14 and 19 weeks respectively. This is higher than expected and a number of factors could have influenced this outcome.

7.3. While we think that the impact against this baseline is robust, we cannot be sure that the baseline would not change under other conditions. It would be challenging to replicate the switching rates obtained in this study at another season of the year, with a different set of consumers and choosing different suppliers.

7.4. It is also difficult to be more specific about some aspects of the intervention in terms of what works. The opt-out letter in itself was a prompt for some people to switch, which is interesting. Also, the CMA could be viewed as multiple prompts, so in that respect, the BOL, which was just one letter, was surprisingly effective.

7.5. We do not have enough confidence to generalise these results. For example, there were external influences during the trial that should be taken into account. Different trial arms had different timelines; both suppliers changed prices during the trial period (widely commented in the media). In addition, although the PIN went to all groups and therefore didn't create any bias it is likely to strongly affect the outcome we were measuring i.e. switching so it is difficult to untangle the potential cumulative effect of receiving the PIN and the treatment letters in close succession.

7.6. There are missing data for external switches, especially for new tariffs. Therefore, we do not have full evidence to draw conclusions on the quality of switching and the results should be interpreted with care.

7.7. Although randomisation should unbiased results, none of the regressions we ran produced a well specified model. Our analysis has limited explanatory power in explaining switching behaviour. However, it indicates some factors that influence this rate and others that do not. We recommend exploring an empirical model that could explain switching rates and carrying out more RCTs may help.

7.8. In spite of these caveats, the internal validity of this trial is high. Minimum quality checks revealed no systematic errors in the data collection. There were some genuine outliers in the data set but the three trial arms are well balanced across a number of key factors, so we are confident that the randomisation was robust.

7.9. The qualitative findings indicate that although customers may have had more positive reactions to receiving a BOL than marketing material, both interventions did have some impact. Customers might not necessarily switch to the specific

suppliers or tariffs named in a piece of communication, but instead use a BOL or marketing material as a prompt to get a better deal with their existing supplier, or use a PCW to switch externally.

7.10. Finally, this trial has offered great insights into the process of carrying out RCTs for studying consumer behaviour. This covers logistical aspects such as timings for the intervention, practical experience in using and collecting data from suppliers and a set of thought provoking results. As part of its wider work focusing on increasing engagement Ofgem will be taking forward a programme of Ofgem led and supplier led trials as part of the prompts to engage project, where the learning from this trial will be hugely valuable.

8. Appendix

Analysis Tables

Table A1.Inteligible consumers by trial arm

Trial arm allocated	Customer becomes ineligible during duration of trial: no	yes	Total
Control	574	226	800
	71.75	28.25	100.00
BOL	591	209	800
	73.88	26.13	100.00
CMA	648	152	800
	81.00	19.00	100.00
Total	1,813	587	2,400
	75.54	24.46	100.00

Table A2. Number of consumers that opted out by trial arm

Opt out received?	Trial arm: Control	BOL	CMA	Total
No	800	785	770	2,355
	100.00	98.13	96.25	98.13
Yes	0	15	30	45
	0.00	1.88	3.75	1.88
Total	800	800	800	2,400
	100.00	100.00	100.00	100.00

Table A3. Age of consumers by trial arm

Trial arm allocated	agegroup: 18-40	41-65	>65	Total
Control	151	188	461	800
BOL	156	205	439	800
CMA	139	198	463	800
Total	446	591	1,363	2,400

Table A4. Number of consumers above and below average consumption by trial arm

Trial arm allocated	totalenercat: Below Ave	Avobe Ave	Total
Control	446	354	800
BOL	462	338	800
CMA	468	332	800
Total	1,376	1,024	2,400

Table A5 Payment method by trial arm
DD= Direct Debit ROB = Receipt of Bill

Payment method	Trial arm: Control	BOL	CMA	Total
DD	666	672	659	1,997
	33.35	33.65	33.00	100.00
ROB	134	128	141	403
	33.25	31.76	34.99	100.00
Total	800	800	800	2,400
	33.33	33.33	33.33	100.00

Table A6 Distribution of consumers across regions by trial arm

Region	Trial arm allocated: Control	BOL	CMA	Total
Eastern	101	96	82	279
	36.20	34.41	29.39	100.00
East Midlands	53	45	57	155
	34.19	29.03	36.77	100.00
London	77	91	102	270
	28.52	33.70	37.78	100.00
North Wales	40	35	37	112
	35.71	31.25	33.04	100.00
West Midlands	78	58	69	205
	38.05	28.29	33.66	100.00
North East	57	64	60	181
	31.49	35.36	33.15	100.00
North West	62	66	76	204
	30.39	32.35	37.25	100.00
Southern	66	45	51	162
	40.74	27.78	31.48	100.00
South East	96	113	103	312
	30.77	36.22	33.01	100.00
South Wales	17	12	15	44
	38.64	27.27	34.09	100.00
South West	46	54	65	165
	27.88	32.73	39.39	100.00
Yorkshire	85	101	64	250
	34.00	40.40	25.60	100.00
South Scotland	20	18	16	54
	37.04	33.33	29.63	100.00
North Scotland	2	2	3	7
	28.57	28.57	42.86	100.00
Total	800	800	800	2,400
	33.33	33.33	33.33	100.00

Table A7 Type of Dwelling by trial Arm
B=Bungalow D=Detach F=Flat S=Semi-Detach T=Terrace

Dwelling type	Trial arm: Control	BOL	CMA	Total
B	51	54	61	166
	30.72	32.53	36.75	100.00
D	120	120	133	373
	32.17	32.17	35.66	100.00
F	120	94	99	313
	38.34	30.03	31.63	100.00
S	251	251	257	759
	33.07	33.07	33.86	100.00
T	258	281	250	789
	32.70	35.61	31.69	100.00
Total	800	800	800	2,400
	33.33	33.33	33.33	100.00

Table A8 Number of household members by trial arm

Number of household members/ occupants	Trial arm: Control	BOL	CMA	Total
1	209	225	247	681
	30.69	33.04	36.27	100.00
2	266	250	221	737
	36.09	33.92	29.99	100.00
3	195	202	218	615
	31.71	32.85	35.45	100.00
4	87	74	70	231
	37.66	32.03	30.30	100.00
5	33	28	31	92
	35.87	30.43	33.70	100.00
6	5	17	12	34
	14.71	50.00	35.29	100.00
7	2	2	0	4
	50.00	50.00	0.00	100.00
8	3	2	1	6
	50.00	33.33	16.67	100.00
Total	800	800	800	2,400
	33.33	33.33	33.33	100.00

Table A9 Number of internal and external switches by supplier

External or internal switch?	Supplier name: A.	B	Total
No Switch	1,075	966	2,041
	52.67	47.33	100.00
	89.58	80.50	85.04
External	48	113	161
	29.81	70.19	100.00
	4.00	9.42	6.71
Internal	77	121	198
	38.89	61.11	100.00
	6.42	10.08	8.25
Total	1,200	1,200	2,400
	50.00	50.00	100.00
	100.00	100.00	100.00

Table A10 Volumes of marketing by supplier

Volumes of marketing	Supplier name: A.	B	Total
0	844	841	1,685
1	1	0	1
2	2	55	57
3	1	0	1
4	10	31	41
5	22	1	23
6	320	272	592
Total	1,200	1,200	2,400

Table A11 Number of switches by frequency of phone calls

number calls	Successful switch No	Yes	Total
0	1,239	130	1,369
	60.71	36.21	57.04
1-2	549	159	708
	26.90	44.29	29.50
2<	253	70	323
	12.40	19.50	13.46
Total	2,041	359	2,400
	100.00	100.00	100.00

Table A12 Number of switches by opt-out catergory

Opt out Received?	Successful switch No	Yes	Total
No	2,107	248	2,355
	89.47	10.53	100.00
Yes	33	12	45
	73.33	26.67	100.00
Total	2,140	260	2,400
	89.17	10.83	100.00

Pearson chi2(1) = 11.9018 Pr = 0.001