

KNOWING WHEN TO QUIT: DEFAULT CHOICES, DEMOGRAPHICS AND FRAUD*

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We study defaults in a novel setting where the optimal choice is clear: the decision to escape from fraud. A government lawsuit created a natural experiment whereby some consumers enrolled in a fraudulent subscription programme were cancelled by default, while others had to cancel actively. We find that cancelling subscriptions by default increased cancellations to 99.8%, 63.4 percentage points more than requiring active cancellation. We also find that consumers residing in poorer, less-educated Census blocks were more likely than average to cancel prior to the lawsuit but were less likely to actively cancel when notified they could do so.

The structure of choices can have a substantial effect on an individual's behaviour. In particular, the default option, the outcome that occurs when an individual takes no action, can have important impacts. Standard economic theory says that, in absence of substantial transaction costs, the default should not matter, as agents will choose to switch away when the default is not appropriate for them. However, a long literature in behavioural economics and psychology has shown that consumers tend to take no action and stick with the default option. This finding has led to policies designed to guide consumers to better decisions by selecting a default option that is seen as reasonable for most consumers (Camerer *et al.*, 2003; Sunstein and Thaler, 2003). A challenge for these policies and a shortcoming of the literature studying defaults is that the policy-maker typically cannot observe the optimal choice for each individual.

In this article, we study the effect of default options in a novel setting where the optimal choice is clear: the decision to escape from a fraudulent subscription programme. Between 2000 and 2007, a fraudulent telemarketing firm charged hundreds of thousands of consumers on a monthly basis for essentially worthless subscriptions, despite the fact that the sales calls were so deceptive and forgettable that the firm's own research showed many of its customers were unaware of their subscriptions.¹ The US Federal Trade Commission (FTC) sued the firm in 2007, temporarily closing it down. During the litigation, the firm sought to resume operations and the court approved a business plan requiring the firm to notify subscribers enrolled at the time of the lawsuit. These notifications created a natural experiment that varied the default options for consumers' subscriptions. Subscribers

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¹ See <http://www.robbevans.com/assets/case-files/ftmincreport01.pdf>

enrolled at the time of the lawsuit received a letter offering them the choice to continue to participate in and be charged for the subscriptions, or to cancel. Under the court order, consumers who enrolled more than six months before the lawsuit received a letter ('enrolment letter') telling them their subscriptions would continue by default – these consumers had to fill out and mail a form or make a phone call to cancel their subscriptions. In contrast, consumers who were enrolled six months or less were sent an otherwise identical letter ('cancellation letter') informing them their subscriptions would be cancelled by default – these consumers had to take action to continue their subscriptions. We exploit this exogenous variation in choice structures to estimate the impact of default options on consumers' decisions in a regression discontinuity framework.

We find that making cancellation the default was remarkably effective in ending consumers' subscriptions. Almost every cancellation letter recipient allowed their subscription to be cancelled. Indeed, eighteen times more consumers who received a cancellation letter took unnecessary action to cancel their memberships than actively renewed any subscription. Given that very few consumers ever used any features of the subscriptions, nearly every subscriber would have been better off cancelling as soon as possible. Nonetheless, we find that consumers who received an enrolment letter because they subscribed just before the cut-off were 63.4 percentage points less likely to cancel their subscriptions than consumers who received cancellation letters and subscribed just after the cut-off.²

We also present evidence that sending letters to the firm's customers and requiring Suntasia customers to cancel actively did little to protect those consumers. Consumers from low socio-economic status (SES) neighbourhoods and racial and ethnic minorities were even less likely to respond to the notification letters than consumers from higher SES communities and consumers who were likely to be white.³ Prior to the FTC lawsuit, subscribers residing in low SES Census blocks were more likely to cancel their subscriptions than subscribers residing in high SES blocks who had been enrolled for the same amount of time. For consumers who were required to actively cancel their subscriptions after the FTC lawsuit, this relationship reverses. Our estimates suggest that among the group of consumers who received enrolment letters, black individuals living in Census blocks with SES measures in the bottom quartile of our sample were 8.2 percentage points less likely to cancel, compared to white individuals living in Census blocks with SES measures in the top quartile. It is not clear whether the heterogeneity we observe stems from differences in consumers' comprehension of the letter as opposed to the salience of the letter or consumers' abilities or inclination to respond by cancelling their subscriptions. Given that some consumers who received cancellation letters actually took unnecessary action to cancel, there may well have been issues with the comprehension of this particular informational intervention. We also estimate that requiring consumers to actively cancel their subscriptions following

² That is not to say that the enrolment letters had no impact. Indeed, 29% of enrolment letter recipients cancelled, compared to a monthly cancellation rate of <10% for subscribers enrolled six months or longer when the firm was operational.

³ As we note in Section 4, since our race variables are measured more precisely than those on education and income, any differences over race and ethnicity likely reflect effects of education and income rather than race *per se*.

the FTC lawsuits would have led these consumers to spend an average of \$423 before they ended their subscriptions, compared to \$477 had the firm resumed charging consumers without sending them enrolment letters.

Our work is consistent with prior research finding that defaults affect individual behaviour. For instance, Madrian and Shea (2001) find that substantially more employees contribute to 401(k) tax-advantaged retirement savings plans when the default choice is to contribute, compared to when they are required to opt-in.⁴ While previous studies have established that consumers tend not to take action to escape the default option, this is one of the first papers to offer compelling, individual-level evidence about whether staying with the default constitutes a mistake.⁵ Choi *et al.* (2011) offers evidence that workers failed to take advantage of unambiguously superior, if complex, arbitrage opportunities with their 401(k) plans. In contrast, our results show that consumers made mistakes in a simple environment.

This article is also related to a growing literature suggesting that low SES individuals and those with lower levels of financial literacy benefit less from purely informational interventions. Examples include Hastings *et al.* (2010) on choice of pension fund managers and Bhargava and Manoli (2015) on worksheets for the Earned Income Tax Credit.⁶ Hortaçsu *et al.* (2015) show that people in lower SES neighbourhoods are less likely to switch away from incumbent electricity suppliers to cheaper entrants offering nearly identical products. Beshears *et al.* (2015) demonstrate that low income employees are more strongly influenced by 401(k) defaults. Our results may also relate to laboratory findings such as Viswanathan *et al.* (2009) that low literacy consumers struggle with informational interventions.

In addition to providing strikingly clear evidence that inappropriate defaults cause people to make mistakes, our results also have policy implications. Individuals are time- and attention-constrained and subject to cognitive biases. Even in the face of outright fraud they may not respond to an informational prompt that leads them to the right action. Our results show that the difference between the effect of an informational intervention and the effect of choosing the right default can be substantial, both in terms of behavioural changes and welfare. Moreover, the heterogeneous responses we find suggest that individuals from lower income and less educated neighbourhoods are less likely to benefit from an informational intervention like the enrolment letters sent in the case we study.

Section 1 presents background on our empirical setting and Section 2 discusses the data. Section 3 presents our regression discontinuity results on the effect of the two default options on consumers' decisions to cancel. Section 4 presents results on the consequences of requiring consumers to cancel actively. We conclude with Section 5.

⁴ For other examples of default effects, see Johnson and Goldstein (2003). DellaVigna (2009) reviews this literature. More recent work finds that small hurdles can outweigh significant economic incentives including Choi *et al.* (2009), Rozin *et al.* (2011), Bettinger *et al.* (2012), Tasoff and Letzler (2014) and Bhargava and Manoli (2015).

⁵ Many papers, including Madrian and Shea (2001) find strong aggregate evidence that a large proportion of consumers make mistakes, but cannot determine which consumers make mistakes.

⁶ Bhargava and Manoli (2015) is also similar to our work in that they study an informational intervention sent by mail. Other studies of such interventions include Hastings and Weinstein (2008) and Liebman and Luttmer (2015).

1. Background

1.1. *Suntasia Marketing*

Suntasia Marketing ran a large telemarketing operation from 2000 to 2007, selling subscriptions to a buyers' club, a long distance phone service, and a catalogue of items available on credit.⁷ Nearly one million consumers in the United States were enrolled in a Suntasia subscription at some point. The firm collected more than \$171 million from consumers over the period it was in operation. At its peak, the firm employed more than 1,000 employees at a Florida call centre.

The firm built its customer base by cold-calling households and offering its subscriptions under false pretences. Telemarketers claimed to represent the consumer's bank, indicating that the consumer had received a 'free' reward and that the telemarketer simply needed to verify their information. The telemarketers determined where the consumer banked, and thus the routing number on the consumer's cheques. Next, the telemarketers would ask consumers to 'verify' their account number. The telemarketers read off the first nine digits on the bottom of the cheques (the routing number) and asked the consumer to read off the account number to 'confirm' the rest. The telemarketer would then read out the terms, conditions, and pricing of the subscriptions so quickly that it was difficult for most consumers to tell what they were agreeing to.⁸ Consumers were often signed up to multiple subscriptions in a single phone call.

The subscriptions were designed to be difficult to use and had poorly disclosed limitations. For instance, the buyers' club subscription offered consumers up to \$100 in gasoline (motor fuel) rebates. However, the gasoline rebates were paid \$10 per month for 10 months, and the consumer had to mail in each voucher before the firm would mail the next one. Unused vouchers expired. The other subscriptions had similar restrictions, few if any of which were disclosed at the time of sale. While an especially savvy and determined consumer likely could have extracted value from their Suntasia subscriptions, the subscriptions were effectively worthless for the vast majority of customers. It is unlikely that the subscriptions even offered meaningful option value. For example, consumers could claim gasoline rebates and airline rebates even if they cancelled their subscriptions. Many discounts available to buyers' club members were available to the general public. The credit catalogue sold items widely available elsewhere and its prices reflected an implicit interest rate of 100% per year. Only 2.2% of consumers of the long distance plan for whom we have at least 135 days of data made any calls.

In addition, the firm made it difficult for consumers to cancel their subscriptions. Although Suntasia often sold consumers three subscriptions in a single telemarketing call, cancelling these subscriptions required calling three different customer service

⁷ Most of the details of Suntasia's business practices described in this subsection are based on the report of the receiver appointed by the court to take over the firm during the FTC lawsuit. The full text of the report can be found at <http://www.robbevans.com/assets/case-files/ftnincreport01.pdf> (last accessed: 2 February 2016).

⁸ According to the court-appointed receiver's report (see footnote 7), 'With very few exceptions, the telemarketers spoke so quickly that it was difficult to understand in a meaningful way what was being offered and what was expected of the consumer'.

numbers, one for each subscription. Customer service representatives used scripts designed to dissuade callers from cancelling. Similarly, Suntasia's customer service policies were designed to make it difficult for consumers to obtain refunds. Suntasia's telemarketing practices generated a near-record number of consumer complaints to US law enforcement agencies and the Better Business Bureau.⁹ Consumers paid Suntasia an average of \$239 over the course of their subscriptions, with consumers enrolled for several months paying much more.

1.2. FTC Legal Action

On 23 July 2007, the US Federal Trade Commission sued Suntasia and obtained a court order that halted most of the firm's operations.¹⁰ Figure 1 shows a timeline of the lawsuit. Control of the company was immediately handed over to a neutral, court-appointed receiver. In the ensuing litigation, the firm sought to resume operations, insisting that consumers who had been charged for several months were surely aware of the charges and interested in continuing their memberships. The FTC opposed the firm's request. In February 2008, the court approved a preliminary injunction, ruling that the company had likely engaged in illegal practices. In the same order, the court also approved an amended plan of operations for the company – still under control of the receiver – that included notifications mailed to all subscribers still enrolled in Suntasia's subscriptions.¹¹

The court found it implausible that consumers could remain unaware of repeated charges for several months.¹² Thus, the court ordered that all remaining customers

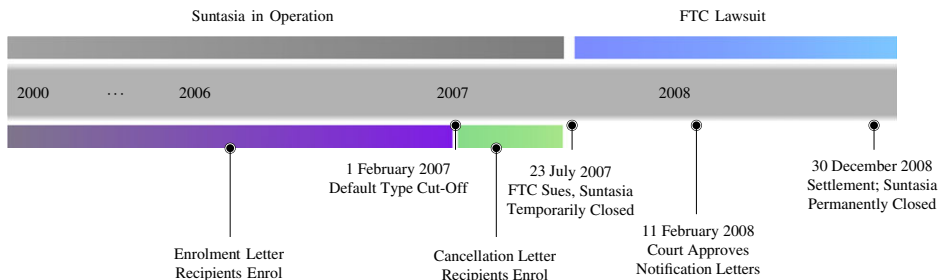


Fig. 1. *Suntasia Case Timeline*

⁹ See <http://www.ftc.gov/news-events/press-releases/2007/07/ftc-stops-massive-deceptive-telemarketer> (last accessed: 2 February 2016).

¹⁰ See 'Ex Parte Temporary Restraining Order With Asset Freeze and the Appointment of a Temporary Receiver', Case No. 8:07-CV-1279-T-3-TGW, docket #10, accessible electronically at <http://www.robbevens.com/assets/case-files/ftnincorder01.pdf> (last accessed: 2 February 2016).

¹¹ The court hearings regarding the preliminary injunction and the amended plan of operations were delegated to a magistrate judge, who issued a 'report and recommendation' on 31 December 2007. The US District Court judge, who had ultimate authority over the case, issued an order adopting the magistrate's recommendation in full on 11 February 2008.

¹² See Report and Recommendation re 6 Motion for Preliminary Injunction, Case No. 8:07-CV-1279-T-3-TGW, docket #168, par. 30–35, accessible electronically at <https://casetext.com/case/federal-trade-commission-v-ftn-promotions-10#.U4ihkVC8Cn4> (last accessed: 2 February 2016).

who enrolled before 1 February 2007 (those who had been customers for more than six months before the lawsuit began) or had demonstrable contact with the company be sent a letter notifying them they could contact the firm to cancel their subscriptions (what we call the ‘enrolment letter’), while the remaining customers would receive a letter notifying them they could contact the firm to continue their memberships (what we call the ‘cancellation letter’).¹³

Figure 2 shows the template for the letters sent to consumers who had to act in order to continue their subscriptions. The letters were sent on the letterhead of the specific Suntasia subscription programme – that is, they did not appear to come from the FTC or the court but from the company. The somewhat complicated and legalistic nature of the notification letters reflects the divergent views of the FTC, the firm and the court. The enrolment letters differed primarily in the headline, which read ‘Notice of Cancellation Right’ instead of ‘Notice of Cancellation’, and replaced the word ‘cancel’ with ‘continue’ throughout.¹⁴ The enrolment letters included a tear off form with a

Product Letterhead
NOTICE OF CANCELLATION
Date

Our records show that you are a member of (Product name) and this is an important notice to you regarding that membership. On July 23, 2007 (Product name) was sued by the Federal Trade Commission in the United States District Court in Tampa, Florida. The Court appointed a Temporary Receiver to take control of operations. As a result, your checking account has not been charged for your membership in (Product name) since late July, 2007.

(Product name) and the Temporary Receiver have developed a business plan to manage future operations of the company.

The business plan is set up to automatically cancel your membership unless you decide to continue it. If you continue your membership, (Product name) will resume charging your checking account the monthly fee of \$ in the near future. If you do nothing, your checking account will not be charged and your membership will be cancelled.

(Product name) has been upgraded with additional discounts and benefits. A description of those discounts and benefits is attached to this notice.

If you wish to continue with your services and your membership please complete the information below and mail the notice to us in the enclosed self-addressed envelope. You may also continue your services and your membership by calling 1 800.....

Fig. 2. *Letter Template, Requiring Action to Continue a Subscription*

¹³ Consumers who enrolled between 1 August 2006 and 31 January 2007 were sent a second, reminder enrolment letter 20 days later if they did not respond to the first letter. A small number of subscribers who enrolled before August 2006 were sent two letters by mistake. A letter was sent for each subscription the consumer held, so many consumers received two or three sets of letters.

¹⁴ In addition, the reminder enrolment letter had a headline reading ‘Notice to resume billing’, and a specific deadline in the third paragraph. We show templates for the other letters in online Appendix C.

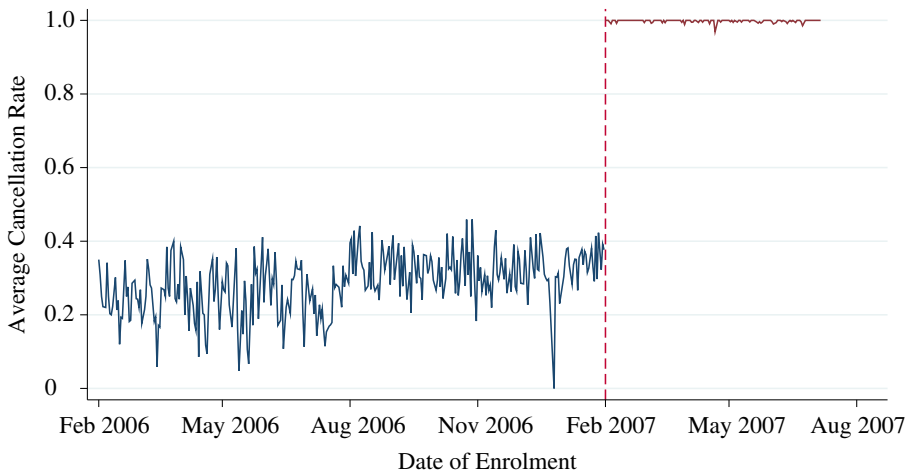


Fig. 3. *Cancellations in Response to the Court-ordered Notification Letters*

Note. Excludes customers who enrolled after 1 February 2007 and received enrolment letters.

check box labelled, ‘No, I do not want to continue with my membership services and please cancel my membership’ followed by name, address, phone, and signature blanks. The letter also informed consumers of the opportunity to quit by using an interactive voice response phone system.

As Figure 3 shows, essentially all consumers informed that their memberships would be cancelled by default allowed their memberships to be cancelled, while only around 30% of consumers informed that their memberships would continue by default took action to cancel. Suntasia asked the court for permission to resume charging consumers who did not cancel their subscriptions, whether by action or inaction.¹⁵ The court never resolved the argument because the firm ran out of money and agreed to a settlement in December of 2008 that abandoned existing customers. The consent decree settling the case included a judgement against the defendants for more than \$171 million. Due to inability to pay, the defendants turned over only about \$16 million in money and assets that were paid back to consumers.¹⁶

2. Data

The data used in our empirical analysis primarily come from Suntasia’s customer database, which was turned over to the FTC by the court-appointed receiver during the lawsuit. The data contain one observation for each completed debit or refund the company made and include the amount of the transaction; the transaction date; and

¹⁵ After seeing the near zero response rate to the cancellation letters and irate communications from some enrolment letter recipients, the receiver suggested a survey of enrolment letter non-respondents. The firm fought the proposed survey in court, arguing that it should be able to resume charging customers whose non-response indicated that they wanted to remain enrolled.

¹⁶ A separate settlement with Wachovia Bank, which processed payments for Suntasia and other fraudulent telemarketers, returned another \$33 million to Suntasia customers. See <http://www.ftc.gov/news-events/press-releases/2010/09/victims-suntasia-telemarketing-scam-sent-second-round-redress> (last accessed: 2 February 2016).

the customer's name, address, and phone number. For the most part, the data show that consumers were charged separately for each subscription every 30 days, following the expiration of a free trial period of 7, 14 or 21 days, depending on the subscription programme. Consumers continued to be charged until they cancelled their subscriptions. Consumers with multiple subscriptions might cancel each subscription on a different day and, moreover, the database does not contain an explicit indicator for when consumers exit. To abstract from these timing issues, we aggregate the transaction data into 30-day 'months' beginning on the day Suntasia first charged each consumer's bank account. We assume consumers cancel at some point in the 30 day 'month' containing their last recorded transaction.

To focus on consumers for whom it was clearly optimal to quit, we exclude from our analysis a small number of consumers we observe making use of their subscriptions. The Suntasia database includes information on consumers' usage of the long distance calling programme for some periods from 2005 onwards. We can identify users of the buyers' club vouchers by the dollar amount of payments made to consumers by the firm.¹⁷ We also limit our sample to consumers who enrolled in 2002 or later, as earlier records appear to have serious data consistency problems. We merge the transaction data with a separate database from the receiver that indicates which consumers were sent notification letters, which letter they received and how each recipient responded.

In addition, we merge the Suntasia data with US Census demographic data. While the Suntasia data capture rich information about consumers' choices, they contain no information on consumer demographics. Thus, we geocode consumers' addresses and match them to aggregate demographic data from the 2000 Decennial Census at the block and block group level. Some consumers could not be matched to Census blocks, largely because the Suntasia data often lacked a usable address.

To improve the accuracy of our measures of race and ethnicity, we used consumers' surnames from the Suntasia database and the racial make-up of their Census block and employed the Bayesian Improved Surname and Geocoding (BISG) method of Elliott *et al.* (2009). The BISG method calculates the probability of being each ethnicity conditional on surname and geographic ethnicity distributions.¹⁸ We obtain surname race probabilities from the 2000 Census (Word *et al.*, 2007).

The full Suntasia transaction data contain 2,119,786 'months' covering 617,143 unique consumers. In addition to addresses that could not be geocoded, some Census blocks have missing data for some variables. We are left with 471,710 consumers who enrolled between 2002 and the FTC lawsuit in July 2007 and have no missing values on any variables of interest.

Because of attrition from the subscription programmes, the majority of consumers in the full sample were not enrolled at the time of the FTC lawsuit and thus are not part of our analysis of default effects. The data show that while Suntasia was in operation, many consumers cancelled their subscriptions rapidly. Figure 4 plots the hazard rate of

¹⁷ Less than 5% of the consumers in the database show any evidence of having used any of the subscriptions.

¹⁸ The BISG method is widely used in health research (Haviland *et al.*, 2011; Friedman *et al.*, 2012; Langer-Gould *et al.*, 2014) and survey methodology research (Kalton, 2009; Elliott *et al.*, 2013). Our results using race variables are robust to using Census block information alone.

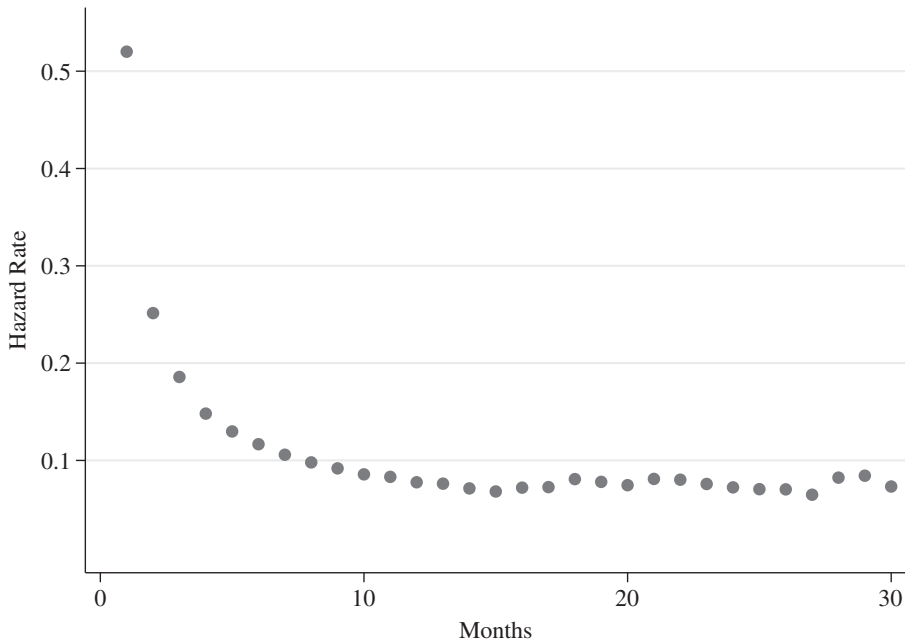


Fig. 4. *Hazard of Cancelling All Suntasia Subscriptions, Aggregated into 30-day Periods from Date of Enrolment*

cancellation by month for periods where consumers did not receive letters.¹⁹ More than 52% cancelled in their first month. The exit rate drops with length of enrolment. Subscribers who remained enrolled long enough that they would be required to cancel actively following the lawsuit (six months or more) had an exit rate of about 10% per period.²⁰ 53,417 consumers were still enrolled at the time of the lawsuit and were sent notification letters. Of these consumers, 42,198 could be matched to Census demographic information and are used in our analysis in Section 3.

Table 1 shows descriptive statistics for the full sample of 471,710 subscribers and two subsets of the letter recipients, and compares them to the US population. The first column shows characteristics of the Census blocks and block groups of all subscribers. When we estimate the effect of the default options in Section 3, we focus on consumers who enrolled close to the court-ordered cut-off date that determined the default option. The second and third columns of Table 1 show characteristics of the samples used in our analysis of the default option, those who enrolled within 30 days before or after the cut-off for the default option, 1 February 2007. For comparison, the fourth column shows average demographic characteristics for the US population. Overall, the neighbourhood-level demographic characteristics of consumers who subscribed just before or just after 1 February 2007 are similar to each other and to the pool of

¹⁹ That is, the graph treats consumers who were enrolled at the time of the FTC lawsuit as censored as of their last month in the data.

²⁰ Given that 29% of enrolment letter recipients cancelled, the letters did increase the cancellation rate relative to the underlying rate of attrition.

Table 1
Census Block Demographic Characteristics of Suntasia Subscribers

	(1) All subscribers	(2) 30-day enrolment	(3) 30-day cancellation	(4) All US
% Home-owner	60.4 (33.5)	58.5 (34.2)	58.0 (34.2)	66.2
Med. income	41,853 (18,679.0)	42,190 (19,185.2)	41,777 (19,026.3)	41,994
% HS drop-outs	22.0 (14.5)	22.0 (14.9)	22.3 (14.8)	19.6
% BA	20.9 (15.9)	21.6 (16.2)	21.4 (16.3)	24.4
% Speak English poorly	5.31 (8.69)	5.71 (9.10)	5.60 (8.91)	4.10
Probability Hispanic	0.13 (0.30)	0.15 (0.31)	0.15 (0.31)	0.13
Probability black	0.22 (0.34)	0.21 (0.34)	0.22 (0.35)	0.12
Probability Asian/other	0.059 (0.16)	0.065 (0.17)	0.064 (0.17)	0.062
<i>N</i>	494,152	14,423	16,428	

Notes. Means of Census block level characteristics from 2000 Census. Standard deviations presented in parentheses. 30-day enrolment and 30-day cancellation denote consumers who enrolled within 30 days before or after 1 February 2007, respectively. Probability of race variables are calculated by combining the percentage of ethnicity by surname with percentage of ethnicity by Census block using the BISG method of Elliott *et al.* (2009).

subscribers as a whole. Compared to the US population, Suntasia subscribers are somewhat more likely to be black and live in neighbourhoods with a slightly smaller percentage of home-owners and college graduates but, on the whole, the differences are small.²¹ This is consistent with surveys in the US and UK, which find that victims of fraud come from all parts of society, with little correlation between victimisation rates and educational attainment, age or income.²²

3. Effect of Default Choices

This Section estimates the effect of the two default options on consumers' decisions to cancel their Suntasia subscriptions. The court created a natural experiment by assigning people who enrolled before 1 February 2007 to continue being enrolled by default and people who enrolled on or after that date to be cancelled by default. We exploit that exogenous variation with a regression discontinuity design that compares people who enrolled close to the cut-off date. We cannot simply calculate the difference in cancellation rates between the total response rate of enrolment letter recipients to the total non-response rate of the cancellation letter recipients as such an analysis could suffer from omitted variables bias. The average consumer who was required to cancel actively had been enrolled for 14 months at the time of the FTC

²¹ Due to the size of our sample, many of the differences between groups displayed in Table 1 are statistically significant. We maintain that most are not in any sense economically significant.

²² See Anderson (2004, 2007, 2013) and George (2006).

lawsuit and likely differed in unobservable ways from consumers whose subscriptions were cancelled by default – these consumers had been enrolled an average of 2.3 months at the time of the FTC lawsuit. As we showed in Figure 4, consumers in their second month cancelled at a much higher rate than consumers in their fourteenth month, even before the FTC lawsuit. However, there is no reason to believe that a consumer who enrolled on 31 January 2007 should be any different from a consumer who enrolled one day later, because the cut-off date of 1 February was arbitrarily chosen. Thus, we can use the discontinuity in letter type over enrolment date to estimate the causal effect of changing the default option. We first discuss our estimation strategy, which follows the standard procedure for regression discontinuity designs, and then present results.

3.1. Methodology

We estimate the effect of the default option using a regression discontinuity design. A regression discontinuity design exploits discontinuous changes in a treatment variable (the default option) at a specific level of a running variable (the enrolment date). A regression discontinuity design can identify the causal effect of the default option so long as nothing else besides the default changed sharply between consumers who enrolled immediately before and immediately after 1 February 2007. Figure A1 in the Appendix shows that all of our covariates are either not changing or changing smoothly through the cut-off date. While it is possible that some unobservable factor changed discontinuously for subscribers who enrolled around the cut-off, it is difficult to imagine something changing enough to produce the size of effect that we observe in Figure 3.

Following the standard procedure for regression discontinuity designs,²³ we regress the probability of cancelling all subscriptions in response to the notification letters on an indicator for default type and a flexible function of enrolment date, which is allowed to vary across the 1 February cut-off. We limit our sample to consumers who enrolled close to the cut-off. Specifically, we estimate the probability that consumer i , who enrolled on day t , exited in response to the letter using the following logit regression:²⁴

$$\Pr[\text{exit}_i] = \frac{1}{1 + \exp[-\alpha + \tau 1(t \geq T) + f(t - T) + \delta \mathbf{X}_i + \beta \mathbf{D}_i]},$$

where T denotes the cut-off date, 1 February 2007; \mathbf{X}_i denotes a vector of subscription characteristics; \mathbf{D}_i denotes a vector of Census block demographic characteristics; and $f(t - T)$ is a flexible piecewise function of enrolment date:

$$f(t - T) = \begin{cases} f_l(t - T) & : t < T \\ f_r(t - T) - f_l(t - T) & : t \geq T. \end{cases}$$

In practice, we specify $f_l(t - T)$ and $f_r(t - T)$ as quadratic.²⁵

²³ See Lee and Lemieux (2010) for an overview of regression discontinuity designs and best practices.

²⁴ Using OLS to estimate a linear probability model yields essentially identical results. We use the logit specification here for consistency with the discrete-time hazard analysis in the following section, where the logit specification ensures a sensible baseline hazard function.

²⁵ Gelman and Imbens (2014) recommend using a linear or quadratic piecewise function of the running variable (here, the enrolment date) rather than a global polynomial of higher order.

3.2. *Default Effect Results*

Table 2 shows the average marginal effect (AME) of making cancellation the default from our regression discontinuity specification described above. Column (1) reports a specification using all letter recipients. The AME of having cancellation as the default option is a 68.4 percentage point increase in the probability of cancelling. Consistent with the pattern in Figure 3, the change in default options moves the cancellation rate from around 30% to more than 99.5%. However, as discussed above, this may include some degree of omitted variable bias, as many of the consumers who enrolled right before the FTC lawsuit would have cancelled quickly on their own and may be more savvy than consumers who continued being charged by Suntasia for years. As it turns out, the size of the bandwidth does not materially affect the AME of the default option. Column (2) limits the sample to 30 days on either side of the 1 February cut-off and, although the point estimate of the AME is smaller, at 63.6 percentage points, this is entirely due to the higher rate of response in the group that was required to actively cancel and enrolled within the 30-day window. Column (2) reports a higher cancellation rate than in column (1) for consumers who were required to act in order to continue their subscriptions. Column (3) shows estimates using an even smaller bandwidth, including only consumers who enrolled within 15 days of the cut-off.²⁶ The tighter bandwidth produces a slightly smaller point estimate for the AME of assigning consumers to be cancelled by default but we cannot reject equality with the AME reported in column (2).

Turning to the AME of the subscription characteristics, consumers who were paying more each month and who had more subscriptions were less likely to cancel all of their

Table 2
Regression Discontinuity Analysis of Default Choices: Logit Average Marginal Effects

	Window		
	(1) All	(2) 30 days	(3) 15 days
Cancellation letter	0.684* (0.00411)	0.636* (0.0143)	0.621* (0.0185)
Amount paid last month (\$00s)	-0.0112* (0.00427)	-0.0121 (0.00967)	-0.0129 (0.0121)
Number of subscriptions	-0.0258* (0.00292)	-0.0426* (0.00810)	-0.0434* (0.0120)
Census block demographic variables	Yes	Yes	Yes
Piecewise quadratic trends in enrolment date	Yes	Yes	Yes
<i>N</i>	42,159	5,256	2,782
Enrolment letter response rate	0.291	0.357	0.368

Note. * $p < 0.05$. Coefficients are average marginal effects from a logit regression of the probability of cancelling in response to the court-ordered notification letters.

²⁶ The absolute minimum length of the RD bandwidth would be nine days on either side of the cut-off. No cancellation letter recipient who enrolled between 1 February and 8 February took action to continue their subscription, making a logit with narrower bandwidth impossible to estimate.

subscriptions in response to the letters. Given the near 100% cancellation rate from consumers who had to act to continue their subscriptions, the AMEs of subscription characteristics are based mostly on variation in the group that was required to act to cancel. Consumers with multiple subscriptions received and had to respond to multiple letters, which added more opportunities for error. It may also be they did not realise they needed to respond to all of the letters to cancel fully. It is possible that consumers paying more per month were less attentive and, thus, both less likely to notice charges from Suntasia on their chequing account statements and less likely to notice and respond to the notification letters.

Although selection is unlikely to be driving the difference between the two default options for consumers who enrolled near the cut-off, the set of letter recipients who enrolled near the cut-off is itself selected in ways that may be important. While Table 1 shows that the neighbourhoods in which recipients of the notification letters reside were similar to the US population on Census block demographics, there may be unobservable differences. To be a part of this sample, an individual had to be deceived into agreeing to a Suntasia subscription, eliminating at least the most sceptical of consumers. Further, the majority of consumers exit immediately after being charged. After five months of enrolment, only 22% of consumers remain enrolled in one or more subscriptions. However, the rate of cancellations slows dramatically at this point to around 10% per month and stabilises at around 8% per month after twelve months. As such, any given consumer enrolled between five and seven months has around a 90% probability of remaining enrolled for another month. It is this sample of consumers that we use in our regression discontinuity analysis.

The fact that very few consumers used the features of their Suntasia subscriptions suggests that almost every consumer would have wanted to cancel. Our results indicate that if every consumer who was required to cancel their subscription actively had instead been given the opposite default option, almost all would have allowed their subscriptions to be cancelled. As a result it seems clear that the default option of requiring consumers to cancel actively was sub-optimal, and would have allowed ongoing fraudulent charges. We quantify the effects of setting this sub-optimal default in the next Section.

4. Consequences of Requiring Action to Cancel

This Section considers some of the consequences of requiring enrolment letter recipients to act in order to cancel their subscriptions. Our regression discontinuity results in Section 3 show that if the consumers who were enrolled by default had instead been cancelled by default, nearly every consumer would have cancelled their subscriptions. Further, cancellation was the optimal choice for nearly every consumer. If Suntasia had resumed operations instead of closing permanently at the conclusion of the FTC lawsuit, many enrolment letter recipients would have continued to pay for worthless subscriptions. We first examine how the effect of the enrolment letters on cancellation varied across Census block and block group demographic characteristics, relative to sending no letters. Second, we calculate the expected monetary cost from requiring consumers to actively cancel.

4.1. Methodology

We quantify the effects of the enrolment letters by comparing the response rate to those letters to the historical cancellation rate while Suntasia was in operation. We rely on panel variation to identify the effect of the enrolment letter. For instance, consider consumers enrolled in a Suntasia subscription for six months. We estimate the effect of the notification letter by comparing the cancellation rate of consumers who were in their sixth month at the time of the FTC lawsuit and received enrolment letters to the historical cancellation rate of consumers in their sixth month of enrolment any time prior to the lawsuit. Specifically, we estimate a discrete time hazard model following Allison (1982), using a logit regression to estimate the probability of exit in month c by consumer i who enrolled in calendar-month m as:

$$h_{im}(c) = \Pr[c = C_i | C_i \geq c] = \frac{1}{1 + \exp[-(\alpha L_{ic} + \beta_1 \mathbf{D}_i + \beta_2 L_{ic} \cdot \mathbf{D}_i + \delta_1 \mathbf{X}_{ic} + \theta_c + \gamma_m)]}, \quad (1)$$

where C_i denotes the month when consumer i cancels, L_{ic} is a dummy equal to one if consumer i receives a letter in period c , \mathbf{D}_i is a vector of demographic characteristics, and \mathbf{X}_{ic} is a vector of (possibly time-varying) subscription characteristics, including the amount paid to Suntasia during the month, the number of subscriptions, the number of payments and an indicator for whether the consumer received both an initial and a reminder letter. The effects of the demographic variables are allowed to vary depending on whether or not consumers received a letter. In one specification, we also interact the amount paid during the month and number of subscriptions with letter receipt. The month fixed effects θ_c flexibly estimate the baseline hazard rate and control for censoring, while calendar-month-of-enrolment effects γ_m control for changes in Suntasia's enrolment practices over time.

4.2. Distributional Effects of Requiring Action to Cancel: Who Pays?

One potential consequence of requiring consumers to cancel actively is that consumers of lower SES, who would be relatively more harmed by continued charging of their bank accounts, may have been disproportionately less likely to respond to the moderately complicated enrolment letter. To understand the incidence of this default option better, we test for heterogeneous responses to the enrolment letters across the Census block and block group demographic characteristics.²⁷ We first provide descriptive evidence of heterogeneity in cancellation rates by comparing raw cancellation rates for consumers living in high and low SES neighbourhoods before and after the FTC lawsuit. Because many consumers cancelled their subscriptions each month even before the FTC lawsuit, these raw differences may simply capture selection effects as the composition of the remaining subscribers changes. To account for this, we next estimate the duration model explained in Section 4.1 to provide a more robust estimate of the heterogeneity in cancellation rates.

²⁷ We note that these results should be primarily interpreted as showing correlations between cancellation rates and neighbourhood characteristics rather than individual demographics. Although it is common to use aggregate demographic variables as proxies for individual characteristics, this interpretation is problematic (Geronimus and Bound, 1998).

We begin by presenting descriptive statistics on cancellation rates across demographic groups. Because we do not observe individual demographics, we approximate by grouping consumers by the demographics of their Census block, Census block group, and surname.²⁸ We first calculate quartiles of the Census demographic characteristics among our sample of consumers. Next, we calculate the cancellation rate in each quartile and compare the rates for the highest and lowest quartiles of each demographic variable.

Table 3 shows the differences in cancellation rates across quartiles of each demographic variable. The first column of Table 3 shows the difference in cancellation rates in the first month of enrolment between the highest and lowest quartile of each demographic variable.²⁹ Consumers living in richer and more educated Census block groups were somewhat less likely to cancel in their first month of enrolment, as were consumers more likely to be white based on Census block and surname. For instance, although on average 59% of consumers quit during their first month, consumers living in Census blocks with more than 89% home-owners (the top quartile) were 5.7 percentage points less likely to cancel than consumers in Census blocks with fewer than 31% home-owners (the bottom quartile). The second column of Table 3 shows the difference in cancellation rates between the highest and lowest quartiles for consumers in their sixth, seventh, and eighth months of enrolment prior to the FTC lawsuit. Here

Table 3
Differences in Average Cancellation Rates of Upper and Lower Quartiles of Census Block Demographic Characteristics

	1st month	Months 6–8	
	No letter	No letter	Enrolment letters
Median income	-0.049*	-0.015*	0.031
% Home-owner	-0.057*	-0.014*	0.046*
% HS drop-outs	0.019*	0.012*	-0.051*
% BA	-0.018*	-0.009*	0.047*
% Speak English poorly	0.003	0.003	-0.051*
Probability black	0.031*	0.005*	-0.055*
Probability Hispanic	0.005*	0.007*	-0.034
Probability Asian/other	0.005*	0.001	-0.022
Average cancellation rate	0.586	0.107	0.344
N	494,152	196,561	4,728

Notes. *p < 0.05. Values are differences in the average rate of attrition between consumers in the upper and lower quartiles of the demographic variable listed on the left. All columns exclude consumers who were enrolled less than six months at the time of the FTC lawsuit and received notice of cancellation letters. Probability of race variables are calculated by combining the percentage of ethnicity by surname with percentage of ethnicity by Census block using the BISG method of Elliott *et al.* (2009).

²⁸ Home ownership rates are available at the block level, while household income, education and English language speaking ability are measured at the block group level. Our Bayesian race probabilities combine block-level race information with data on the surname of the particular consumer.

²⁹ The demographic quartiles in each column in Table 3 are calculated from the population analysed in the column as opposed to the quartiles of the entire sample of Suntasia subscribers.

we see the same pattern, with somewhat larger differences relative to the much lower average cancellation rate of about 11%. The final column of Table 3 shows the difference in cancellation rates between the highest and lowest quartiles of the demographic variables for enrolment letter recipients who had been enrolled for six, seven or eight months at the time of the FTC lawsuit. Here the correlation between the demographic variables and cancellation rates is reversed. Focusing again on the percentage of home-owners in the consumers' Census blocks, we see that consumers in the top quartile had a cancellation rate 4.6 percentage points higher than that of consumers in the lowest quartile, compared to an average response rate of about 34% for consumers who received the enrolment letters.

We now present the results of our duration model. As with the descriptive results presented in Table 3, we are interested in whether Census block and block group demographic characteristics are correlated with cancellation in response to the enrolment letter, as compared to the correlation between demographics and cancellation rates before the FTC lawsuit.

Table 4 shows the AMEs of neighbourhood demographics on cancellation with and without the enrolment letters. The AMEs of the Census demographic variables without letters and the AMEs with letters are equivalent to the statistics in columns (2) and (3) of Table 3 but now we hold other factors constant. Column (1) shows the AMEs from a specification using measures of neighbourhood income and education to proxy for socio-economic status. Consistent with the descriptive statistics in Table 3, we see that, controlling for covariates, consumers were more likely to cancel before the FTC lawsuit if they lived in neighbourhoods with more renters, lower household income and more high school drop-outs, although the last of these is statistically insignificant. We find that consumers residing in Census blocks with more people who reported speaking English well were somewhat more likely to cancel Suntsia subscriptions while the company was in operation. Some of these relationships flip in consumers' responses to the enrolment letters. As in Table 3, we see that consumers who lived in Census blocks with more renters and more high school drop-outs were less likely to cancel in response to the enrolment letters, while those living in neighbourhoods with low levels of English proficiency become even less likely to exit.

Column (2) of Table 4 adds measures of race and ethnicity by surname and Census block. Because our measures of race incorporate information about the consumer's last name, to some extent results with these variables can be interpreted as stemming from individual characteristics. Nonetheless, these variables are highly collinear with the other Census block variables and we urge caution in interpreting the marginal effect of our race measures as the effect of race. The AMEs with letters of percentage of high school drop-outs and percentage of home-owners shrink and become insignificant, likely because the race variables are measured more precisely and are ultimately proxying for SES. Consumers likely to be racial or ethnic minorities by surname and Census block were substantially less likely to respond to the notification letters compared to consumers likely to be white, yet were slightly more likely to cancel before the FTC lawsuit.

Column (3) of Table 4 adds interaction terms for amount paid and the number of subscriptions in the current month with letter receipt and reports the AMEs with and without letters. Prior to the FTC lawsuit, consumers were more likely to cancel the

Table 4

Marginal Effects of Demographics on Decisions to Cancel Suntaisia Subscriptions, With and Without Enrolment Letters

	(1)	(2)	(3)
% Home-owner			
Pre-lawsuit	-0.023* (0.0011)	-0.021* (0.0011)	-0.021* (0.0011)
Enrolment letter	0.037* (0.0114)	0.018 (0.0116)	0.008 (0.0111)
Median income (\$0,000s)			
Pre-lawsuit	-0.003* (0.0003)	-0.003* (0.0003)	-0.003* (0.0003)
Enrolment letter	-0.002 (0.0024)	-0.001 (0.0024)	-0.002 (0.0023)
% HS drop-outs			
Pre-lawsuit	0.007 (0.0039)	0.002 (0.0041)	0.002 (0.0041)
Enrolment letter	-0.119* (0.0397)	-0.023 (0.0409)	-0.032 (0.0391)
% BA			
Pre-lawsuit	-0.013* (0.0032)	-0.014* (0.0032)	-0.014* (0.0032)
Enrolment letter	-0.041 (0.0307)	-0.037 (0.0306)	-0.038 (0.0294)
% Speak English poorly			
Pre-lawsuit	-0.015* (0.0049)	-0.025* (0.0054)	-0.025* (0.0054)
Enrolment letter	-0.126* (0.0520)	-0.113* (0.0563)	-0.112* (0.0537)
Probability Hispanic			
Pre-lawsuit		0.010* (0.0013)	0.010* (0.0013)
Enrolment letter		-0.074* (0.0127)	-0.069* (0.0123)
Probability black			
Pre-lawsuit		0.005* (0.0010)	0.005* (0.0010)
Enrolment letter		-0.095* (0.0105)	-0.090* (0.0101)
Probability Asian/other			
Pre-lawsuit		0.006* (0.0021)	0.006* (0.0021)
Enrolment letter		-0.059* (0.0208)	-0.056* (0.0205)
Amount paid this cycle			
Pre-lawsuit			0.108* (0.0009)
Enrolment letter			-0.068* (0.0078)
Number of subscriptions			
Pre-lawsuit			-0.206* (0.0008)
Enrolment letter			-0.042* (0.0052)
N	1,594,023	1,593,914	1,593,914
Effect of letter receipt	0.161	0.160	0.166

Notes. *p < 0.05. Coefficients are average marginal effects from a discrete time hazard model calculated from a logit regression of the probability of cancellation each 30-day period on period fixed effects and covariates. All regressions include month-of-enrolment fixed effects, an indicator for letter receipt, the amount paid in the current period, the number of programmes enrolled in, counts of the number of payments received, and Census block demographics (alone and interacted with enrolment letter receipt). The reported marginal effects are calculated separately for periods when consumers received letters and periods without letters. Standard errors clustered by Census block group shown in parentheses.

more money they paid in the most recent month and less likely to cancel completely the more subscriptions they had. As we observed in our regression discontinuity results, consumers were less likely to cancel in response to the letters the more money they were paying each month. Consumers with more subscriptions were still less likely to cancel but the effect shrinks substantially in response to the letters. It is likely that, consumers were less prone to cancel with additional subscriptions because each subscription had to be cancelled individually and this remained true for responses to the notification letters. Consumers with multiple subscriptions received multiple letters, which would increase the salience of the letter intervention but apparently not enough to counteract the challenge of enacting multiple cancellations fully. Consistent with the proposition that the relative size and precision of the AMEs of the SES measures has more to do with the level of aggregation than the real importance of those measures, adding the truly individual level subscription interactions causes the AMEs for percent home-owner and the three race measures to move towards zero, although the AMEs of the race measures remain large and statistically significant.³⁰

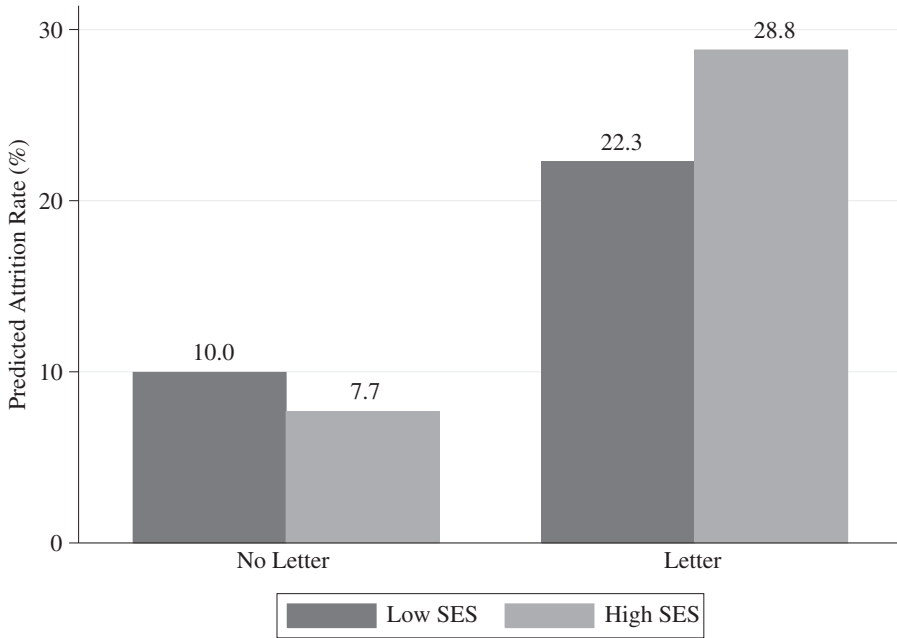
To show the sign reversal over the Census demographic characteristics following the FTC lawsuit more clearly, we calculate predicted cancellation rates for the extremes of the race and SES distributions for consumers enrolled six months or longer. To obtain these predictions, we first partition the direct predictors of neighbourhood SES (Census block-group household income and education and Census block home-ownership) into quartiles, similar to the analysis in Table 3. We then predict the cancellation rate for a hypothetical consumer of high SES using the model estimated in column (1) of Table 4 and setting the value of each variable equal to its mean within the highest quartile. Conversely, we predict the cancellation rate for a consumer of low SES using the mean of the lowest quartile of each variable. We calculate similar predictions by both race and SES by setting a given race variable to one and the others to zero. Figure 5 plots the predicted attrition rates from columns (1) and (2) of Table 4. Panel (a) allows for effects by SES variables only as in column (1) of Table 4, while panel (b) shows predictions by SES and race as in column (2). Without letters, consumers living in low SES Census blocks were about 2.3 percentage points more likely to cancel, a 30% difference compared to consumers in high SES Census blocks. Allowing the effect to vary by race, we see that without letters, blacks, Hispanics, and whites are all predicted to have similar cancellation rates but consumers living in low SES Census blocks were still about 2 percentage points more likely to cancel.³¹

In the responses to the enrolment letters, however, Figure 5(a) shows that a consumer in a high SES Census block would be 6.5 percentage points more likely to cancel than a consumer residing in a low SES Census block. Allowing the effects to vary by race in Figure 5(b), we predict that a white enrolment letter recipient in a high SES Census block would be 10.4 percentage points more likely to actively cancel, compared

³⁰ In unreported results, we have found the effects of heterogeneity discussed here to be robust to controlling for the interaction of letter receipt with the dollar value and number of subscriptions; to excluding any particular Census region; and to limiting the sample by excluding earlier years of data, the first month of enrolment and specific Suntasia subscription types. These results are available from the authors by request.

³¹ The differences between our constructed SES groups shown in the 'No Letter' portion of both Figures are statistically significant at the 5% level.

(a) Socioeconomic Status Variables Only



(b) Socioeconomic Status and Race Variables

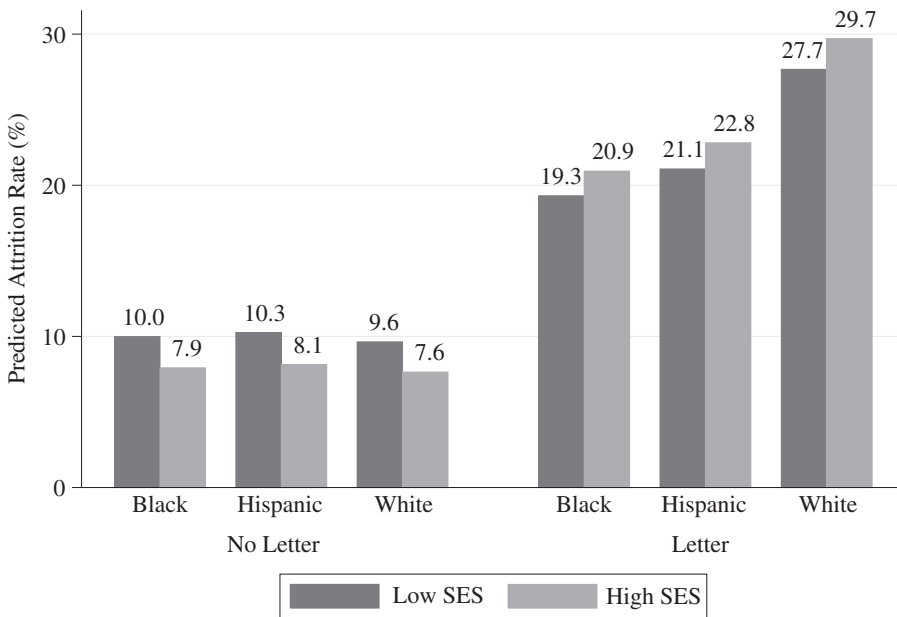


Fig. 5. Predicted Cancellation Rates, With and Without Notification Letters

Note. SES calculated as the average value of the education and income variables within the lower and upper quartiles of that variable.

to a black enrolment letter recipient residing in a low SES Census block, a 54% difference. Within racial and ethnic groups, consumers in high SES blocks are predicted to have a slightly higher cancellation rate than those residing in low SES blocks, though these differences are not statistically significant. That said, we stress again that our results on race may not be picking up the effects of race *per se*. Our data on race more closely reflects individual characteristics due to the use of surname information.³² It is likely that our measures of race are picking up aspects of SES or similar factors that are correlated with race but not picked up by our Census block measures of education and income. Regardless of what factor drives our result, it is important to note that although all groups of consumers benefit from receiving a notification letter reminding them of the need to actively cancel, the letters disproportionately benefit consumers living in whiter and more affluent neighbourhoods.

We note that our data cannot speak to the mechanism behind the heterogeneity we observe in cancellation rates.³³

4.3. Counterfactual Costs of Requiring Action to Cancel: How Much Would Consumers Pay?

If Suntasia had received permission from the court to resume charging consumers, it is likely that many consumers would have paid for months before finally ending their subscriptions. Although a substantial number of consumers failed to make the right choice by cancelling after receiving an enrolment letter, these letters still would have reduced costs to some extent. In this subsection we use our duration model to quantify how much money enrolment letter recipients would have paid to Suntasia had the firm resumed operations, as compared to receiving a cancellation letter, or no letter at all. We use the specification from column (3) of Table 4 to predict the amount that enrolment letter recipients would have paid Suntasia had the firm resumed operations. We assume for simplicity that the amount paid each month and the number of subscriptions remains constant over time for each consumer. This reduces the problem to estimating the expected remaining months before the consumer cancels and multiplying it by the amount the customer was paying per month at the time of the FTC lawsuit. Let \bar{C}_i denote the month that letter recipient i was in at the time of the FTC lawsuit. The probability that consumer i remains enrolled after month $c \geq \bar{C}_i$ is:

$$S_{im}(c, \mathbf{X}_{ic}, \mathbf{D}_i) = 1 - \frac{1}{1 + \exp[-(\alpha L_{ic} + \beta_1 \mathbf{D}_i + \beta_2 L_{ic} \cdot \mathbf{D}_i + \delta \mathbf{X}_{ic} + \theta_c + \gamma_m)]}. \quad (2)$$

³² We note that we get essentially the same hazard results using Census block race measures, although we lose precision for coefficients on percent Hispanic and percent Asian. See Appendix Table A2.

³³ Note also that although our analysis treats the response to the notification letters as a single decision, an affirmative response requires a sequence of steps and we might expect differential responses to each of these steps. In particular, consumers must understand the contents of the letter. Previous research by the FTC's Bureau of Economics has shown that simple, plain-language disclosures can substantially increase consumer comprehension and responses compared to complicated, technical explanations. See for instance Murphy *et al.* (1998) and Lacko and Pappalardo (2007).

Then the expected remaining duration in months from C_i is the sum of the conditional probabilities of continued enrolment from month \bar{C}_i forward:

$$E(C_i) - \bar{C}_i = \sum_{j=\bar{C}_i}^{\infty} \prod_{l=\bar{C}_i}^j S_{im}(l, X_{i\bar{C}_i}, D_i), \tag{3}$$

and the expected remaining cost is:

$$A_{i\bar{C}_i} \cdot [E(C_i) - \bar{C}_i], \tag{4}$$

where $A_{i\bar{C}_i}$ denotes the amount of money consumer i spent in month \bar{C}_i .

For each consumer who was required to cancel actively, we calculate expected remaining months for three cases: assuming they received a cancellation letter in period \bar{C}_i ; assuming they received an enrolment letter in period \bar{C}_i ; and assuming they received no notification letters.³⁴ In practice, we calculate the sum in (3) iteratively for each consumer, stopping either when the consumer’s expected remaining months change by <0.01 months, or at month 59, the latest observed in our data. To be conservative in our estimate of expected costs, we assume that any consumer still enrolled after their 59th month automatically cancels.³⁵ To calculate the expected cost with no notification letters, we set $L_i = 0$ in period \bar{C}_i , and in all calculations we set $L_i = 0$ for all months $c > \bar{C}_i$. That is, notification letters are assumed to only affect consumers in the period they are received.

Table 5 shows our estimates of predicted remaining months and cost to consumers for three counterfactuals. If the consumers who were required to cancel actively had instead been assigned to cancel by default, they would exit immediately and pay no more money.³⁶ Had the company resumed operations, we predict that the consumers who received a notification letter requiring them to cancel actively would have continued their subscriptions for an average of 10 additional months, paying an average of \$423.04. This is only a slight reduction from the predicted enrolment and costs with no notification at all – 11.5 months and \$477.82.

Table 5
Expected Cost to Consumers of Cancellation Letter, Enrolment Letter, and No Notification

	(1) Months enrolled	(2) \$ Paid
Cancellation letter	0.00	0.00
Enrolment letter	10.16	423.04
No letter	11.53	477.82

³⁴ Equivalently, the final case would apply if the FTC lawsuit had not occurred.

³⁵ Assuming instead that the average hazard rate for the 59th month persists in perpetuity and relying solely on the convergence criteria has no qualitative impact on our results.

³⁶ This follows from our 0.01 month convergence rule.

5. Conclusion

A large literature on the effects of default choice structures shows that agents are more likely to choose the default option than other options. In this article, we show that this is true even when the optimal decision is clear. Our results further indicate that informational interventions are not always an effective way of encouraging consumers to make those optimal decisions. Conversely, our results suggest that changing defaults is not a panacea when optimal choices are less clear, since many people may stay with even very inappropriate defaults. A standard model for a policy that exploits default effects involves enrolling consumers into a supposedly beneficial programme and requiring them to take action to exit if they do not want to remain enrolled. It is common for studies on these policies to find that fewer than half of the target population take action to exit, as was the case with the enrolment letter recipients in our study. However, in the case we study, it was likely to be optimal for every consumer to cancel their subscription.

Consumers from lower SES neighbourhoods and minorities were substantially less likely to cancel in response to the enrolment letters, yet were slightly more likely to cancel before the FTC lawsuit compared to consumers from higher SES neighbourhoods who were likely to be white. Thus, the information provision policy disproportionately benefited consumers from wealthier neighbourhoods. Although the differences across demographic characteristics were smaller than the overall effect of requiring consumers to actively cancel, setting the correct default had bigger benefits for subscribers from lower SES neighbourhoods than for subscribers from higher SES neighbourhoods. We also find that the enrolment letters reduced the expected future costs of subscriptions by only about 13% and would have allowed consumers to waste an average of \$423.

Our results have important implications for policy-makers considering informational interventions to rectify market failures. There are current proposals in the UK to use informational remedies in the markets for retail energy and retail banking. However, we study a situation where two-thirds of recipients of an informational remedy made the wrong choice and the effectiveness was even lower for consumers from low SES neighbourhoods. This finding speaks to the importance of testing informational interventions before implementation. Note, however, that although our data and setting allow for very clean identification, our sample consists entirely of individuals who fell victim to a telemarketing scam and thus may not be representative of the broader population.

An important task for future research is to investigate the mechanism behind the heterogeneity in response rates. The higher cancellation rate prior to the FTC lawsuit among individuals in poorer neighbourhoods may reflect the declining marginal utility of income – low income individuals would have a greater utility gain from monitoring their bank accounts for fraudulent charges. In contrast, it could be that individuals in high SES neighbourhoods were more likely to respond to the enrolment letter because those individuals were better at reading complicated letters or had more neighbours they could turn to for help. Although other explanations are possible, this would be consistent with other findings such as Bhargava and Manoli (2015).

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Additional Supporting Information may be found in the online version of this article:

Appendix A. Additional Tables and Figures.

Appendix B. Data Construction.

Appendix C. Additional Letter Templates.

Data S1.

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