

Knowing When to Quit: Default Choices, Demographics and Fraud

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Abstract

A long literature in psychology and economics has shown that default options, which occur when consumers take no action, influence consumer choices. However, it is often unclear whether default options cause individual consumers to choose optimally or simply change their choices. We study the effects of default options in a novel setting where the optimal choice is clear: the decision to escape from fraud. We employ data from one of the largest telemarketing fraud cases ever brought by the Federal Trade Commission (FTC). The telemarketer enrolled consumers into costly membership programs, which the vast majority of consumers never used. A court order issued during the FTC lawsuit created a natural experiment whereby some consumers were sent letters informing them they had to take action to remain enrolled, while similarly situated consumers received letters merely reminding them how to quit. We find that cancelling consumers by default increased cancellations by 63.4 percentage points, to 99.8%. We then examine the impact of requiring consumers to actively cancel their memberships. We find that consumers residing in poorer, less educated Census blocks and those more likely to be minorities were more likely to cancel their subscriptions prior to the FTC lawsuit, but were relatively less likely to actively cancel in response to a letter. We also find that sending letters while maintaining the requirement that they actively cancel only slightly reduced the expected costs of continued enrollment compared to sending no letters.

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1 Introduction

The structure of choices can have a substantial effect on an individual’s behavior. 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 behavioral economics and psychology has shown that consumers tend to disproportionately 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 policymaker typically cannot observe the optimal choice for each individual.

In this paper, 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 program. 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 U.S. 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 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 their memberships. Under the court order, consumers who enrolled more than six months before the lawsuit received a letter (“enrollment 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 canceled 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 her subscription to

¹See <http://www.robbevans.com/assets/case-files/ftnincreport01.pdf>

be cancelled. Indeed, eighteen times more consumers who received a cancellation letter took unnecessary action to call and 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 canceling as soon as possible. Nonetheless, we find that consumers who were received an enrollment letter and enrolled just before the cut-off were 63.4 percentage points less likely to cancel their subscriptions than consumers who received cancellation letters and enrolled just after the cut-off.²

We also present evidence that sending letters and requiring consumers to actively cancel did little to protect those consumers. Consumers from low socioeconomic status (SES) neighborhoods 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 enrollment letters, black individuals living in Census blocks with SES measures in the bottom quartile in 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 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 enrollment letters.

Our work is consistent with prior research finding that defaults affect individual behavior. For instance, Madrian and Shea (2001) find that substantially more employees contribute to an employer-sponsored 401(k) plan when the default choice is to contribute, compared to when they are required to opt-in.⁴ While previous studies have established that consumers

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

³As we note in section 5, 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*.

⁴For other examples of default effects, see Johnson and Goldstein (2003) and Rozin et al. (2011) DellaV-

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 fail 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 paper 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 (2014) on worksheets for the Earned Income Tax Credit.⁶ Hortaçsu et al. (2010) show that people in lower SES neighborhoods are less likely to switch away from incumbent electricity suppliers to cheaper entrants offering nearly identical products. Beshears et al. (2012) 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 can 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. Moreover, the heterogeneous responses we find suggest that individuals from lower income and less educated neighborhoods are less likely to benefit from an informational intervention like the enrollment letters sent in the case we study.

Section 2 presents background on our empirical setting and Section 3 discusses the data. Section 4 presents our regression discontinuity results on the effect of the two default options on consumers' decisions to cancel. Section 5 presents results on the consequences of requiring consumers to actively cancel. We conclude with Section 6.

igna (2009) reviews this literature. More recent works finding that small enrollment hurdles can outweigh significant economic incentives include Bhargava and Manoli (2014), Choi et al. (2009), Bettinger et al. (2012) and Tasoff and Letzler (2014).

⁵Many 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.

⁶Bhargava and Manoli (2014) is also similar to our work in that they study an informational intervention sent by mail. Other examples studying such interventions include Hastings and Weinstein (2008) and Liebman and Luttmer (2015).

2 Background

2.1 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 catalog 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 center.

The firm built its customer base by cold-calling households and offering its subscriptions under false pretenses. 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 checks. Next, the telemarketers would ask consumers to "verify" their account number. The telemarketers read off the first nine digits on the bottom of the checks (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 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 canceled their subscriptions. Many discounts available to buyers' club members were available to the general public. The credit catalog sold items widely available elsewhere at

⁷Most of the details of Suntasia's business practices described in this section 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>.

⁸According to the court-appointed receiver's report (*ibid*), "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."

prices that 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, canceling these subscriptions required calling three different customer service 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 U.S. 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.

2.2 FTC Legal Action

On July 23, 2007, the U.S. 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 membership. 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 who enrolled before February 1, 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 to contact the firm to cancel their subscriptions (what we call the “enrollment letter”),

⁹See <http://www.ftc.gov/news-events/press-releases/2007/07/ftc-stops-massive-deceptive-telemarketer>.

¹⁰See “*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.robbevans.com/assets/case-files/ftnincorder01.pdf>

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

¹²See “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>

while the remaining customers would receive a letter notifying them to 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 program—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 enrollment 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 These letters included a tear off form with a check box labeled, “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 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 judgment 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

3 Data

The data used in our empirical analysis primarily comes from Suntasia’s customer database, which was turned over to the FTC by the court-appointed receiver during the lawsuit. The

¹³Consumers who enrolled between August 1, 2006 and January 31, 2007 were sent a second, reminder enrollment 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.

¹⁴In addition, the reminder enrollment 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 Appendix B

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

¹⁶A 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>

data contain one observation for each completed debit or refund the company made and include the amount of the transaction; the transaction date; and 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 program. 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 program 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 U.S. 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 lack a usable address.

To improve the accuracy of our measures of race and ethnicity, we use consumers’ surnames from the Suntasia database and the racial makeup of their Census block and employ 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.

The full Suntasia transaction data contain 2,119,786 “months” covering 617,143 unique

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

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

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 subscription programs, 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 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 enrollment. Subscribers who remained enrolled long enough that they would be required to actively cancel following the lawsuit (six months or more) have 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 4.

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 4, 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, February 1, 2007. For comparison, the fourth column shows average demographic characteristics for the U.S. population. Overall, the neighborhood-level demographic characteristics of consumers who subscribed just before or just after February 1, 2007 are similar to each other and to the pool of subscribers as a whole. Compared to the U.S. population, Suntasia subscribers are somewhat more likely to be black and live in neighborhoods with a slightly smaller percentage of homeowners and college graduates, but on the whole the differences are small.²¹ This is consistent with surveys in the U.S. and U.K., which find that victims of fraud come from all parts of society, with little correlation between victimization rates and educational attainment, age, or income.²²

¹⁹That 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.

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

²¹Due 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.

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

4 Effect of Default Choices

The court created a natural experiment by assigning people who enrolled before February 1, 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 enrollment 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 actively cancel had been enrolled for 14 months at the time of the FTC lawsuit and likely differed in unobservable ways from consumers who 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 January 31, 2007 should be any different from a consumer who enrolled one day later because the cut-off date of February 1 was arbitrarily chosen. Thus, we can use the discontinuity in letter type over enrollment 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.

4.1 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 (enrollment 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 or immediately after February 1, 2007. Figure A.1 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 enrollment date, which is allowed to vary across

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

the February 1 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

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

where T denotes the cut-off date, February 1, 2007; X_i denotes a vector of subscription characteristics; D_i denotes a vector of Census block demographic characteristics; and $f(t - T)$ is a flexible piecewise function of enrollment 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

4.2 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 variables 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 February 1 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

²⁴Using 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.

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

bandwidth, including only consumers who enrolled within 15 days of the cutoff.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 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 realize they needed to respond to all of the letters to fully cancel. It is possible that consumers paying more per month were less attentive, and thus both less likely to notice charges from Suntasia on their checking 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 neighborhoods in which recipients of the notification letters reside were similar to the U.S. 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 skeptical of consumers. Further, the majority of consumers exit immediately after being charged. After five months of enrollment, only 22% of consumers remain enrolled in one or more subscriptions. However, at this point the rate of cancellations stabilizes at around 10% per month, such that 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 actively cancel their subscription 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 to require consumers to actively cancel was sub-optimal, and would have allowed ongoing fraudulent charges. We quantify the effects of setting this sub-optimal default in the next section.

²⁶The absolute minimum length of the RD bandwidth would be 9 days on either side of the cutoff. No cancellation letter recipient who enrolled between February 1 and February 8 took action to continue their subscription, making a logit with narrower bandwidth impossible to estimate.

5 Consequences of Requiring Action to Cancel

Our regression discontinuity results in section 4 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 enrollment letter recipients would have continued to pay for worthless subscriptions. We first examine how the effect of the enrollment letters on cancellation varied across the 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.

5.1 Methodology

We quantify the effects of the enrollment letters by comparing the response rate to those letters to the historical cancellation rate while Suntasia was in operation.²⁷ We rely on panel variation to identify the effect of the enrollment 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 enrollment letters, to the historical cancellation rate of consumers in their sixth month of enrollment 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

$$h_{im}(c) = \Pr[c = C_i | C_i \geq c] = \frac{1}{1 + \exp[-(\alpha L_{ic} + \beta_1 D_i + \beta_2 L_{ic} \cdot D_i + \delta_1 X_{ic} + \theta_c + \gamma_m)]},$$

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 , D_i is a vector of demographic characteristics, and 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

27

flexibly estimate the baseline hazard rate and control for censoring, while calendar-month of enrollment effects γ_m control for changes in Suntasia’s enrollment practices over time.

5.2 Distributional Effects of Requiring Action to Cancel: Who Pays?

One potential consequence of requiring consumers to actively cancel 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 enrollment letter. To better understand the incidence of this default option, we test for heterogeneous responses to the enrollment letters across the Census block and block group demographic characteristics.²⁸ We first provide descriptive evidence of heterogeneity in cancellation rates by comparing raw cancellation rates for consumers living in high and low SES neighborhoods 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 remaining subscribers changes. To account for this, we next estimate the duration model explained in section 5.1 to provide a more robust estimate of the heterogeneity in cancellation rates.

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.²⁹ 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 enrollment between the highest and lowest quartile of each demographic variable.³⁰ Consumers living in richer and more educated Census block groups were somewhat less likely to cancel in their first month of enrollment, as were consumers more likely to be white based on Census block and surname. For instance, although on average 59% of consumers quit

²⁸We note that these results should be primarily interpreted as showing correlations between cancellation rates and neighborhood 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).

²⁹Home ownership rates are available at the block level, while household income, education and English language speaking 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.

³⁰The demographic quartiles in each column in table 3 are calculated from the population analyzed in the column as opposed to the quartiles of the entire sample of Suntasia subscribers.

during their first month, consumers living in Census blocks with more than 89% homeowners (the top quartile) were 5.7 percentage points less likely to cancel than consumers in Census blocks with fewer than 31% homeowners (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 enrollment prior to the FTC lawsuit. Here 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 enrollment 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 reverses. Focusing again on the percent of homeowners in the consumers' Census blocks, we see that consumers in the top quartile had a cancellation rate 4.4 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 enrollment 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 enrollment letter, as compared to the correlation between demographics and cancellation rates before the FTC lawsuit.

Table 4 shows the AMEs of neighborhood demographics on cancellation with and without the enrollment 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 hold other factors constant. Column 1 shows the AMEs from a specification using measures of neighborhood income and education to proxy for socioeconomic 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 neighborhoods with more renters, lower household income, and more high school dropouts, although the latter 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 Sunasia subscriptions while the company was in operation. Some of these relationships flip in consumers' responses to the enrollment letters. As in table 3, we see that consumers who lived in Census blocks with more renters and more high school dropouts were less likely to cancel in response to the enrollment letters, while those living in neighborhoods 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 percent high school dropouts and percent homeowners 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 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. Likely, consumers were less likely 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 fully counteract the challenge of enacting multiple cancellations. 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 homeowner and the three race measures to move toward zero, although the AMEs of the race measures remain large and statistically significant.³¹

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 neighborhood SES (Census block-group household income and education and Census block homeownership) 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

³¹In 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 enrollment, and specific Suntasia subscription types. These results are available from the authors by request.

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.³²

In the responses to the enrollment 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 enrollment letter recipient in a high SES Census block would be 10.4 percentage points more likely to actively cancel, compared to a black enrollment 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.³³ 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 neighborhoods.

We note that our data cannot speak to the mechanism behind the heterogeneity we observe in cancellation rates.³⁴

³²The differences between our constructed SES groups shown in the “No Letter” portion of both figures are statistically significant at the 5% level.

³³We 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 A.2.

³⁴Note also that 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 Lacko and Pappalardo (2007) and Murphy et al. (1998).

5.3 Counterfactual Costs of the Requiring Action to Cancel: How Much Would They Pay?

If Suntasia had received permission from the court to resume charging consumers, many consumers would have likely paid for months before finally ending their subscriptions. Although the enrollment letter failed to move a substantial number of consumers to the right choice and actively cancel, it would have reduced these costs to some extent. In this section we use our duration model to quantify how much money enrollment 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 enrollment 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

$$S_{im}(c, X_{ic}, D_i) = 1 - \frac{1}{1 + \exp[-(\alpha L_{ic} + \beta_1 D_i + \beta_2 L_{ic} \cdot D_i + \delta X_{ic} + \theta_c + \gamma_m)]}.$$

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

$$E(C_i) - \bar{C}_i = \sum_{j=\bar{C}_i}^{\infty} \prod_{l=\bar{C}_i}^j S_{im}(l, X_{i\bar{C}_i}, D_i), \quad (1)$$

and the expected remaining cost is

$$A_{i\bar{C}_i} \cdot (E(C_i) - \bar{C}_i), \quad (2)$$

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 actively cancel, we calculate expected remaining months for three cases: assuming they received a cancellation letter in period \bar{C}_i , assuming they received an enrollment letter in period \bar{C}_i , and assuming they received no notification letters.³⁵ In practice, we calculate the sum in equation 1 iteratively for each consumer,

³⁵Equivalently, the final case would apply if the FTC lawsuit had not occurred.

stopping either when the consumer’s expected remaining months change by less than 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 her 59th month automatically cancels.³⁶ 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 actively cancel had instead been assigned to cancel by default, they would be exit immediately and pay no more money.³⁷ Had the company resumed operations, we predict that the consumers who were required to actively cancel and received a notification letter would have continued their subscriptions for an average of 10 additional months, paying an average of \$423.08. This is only a slight reduction from the predicted enrollment and costs with no notification at all—11.5 months and \$477.86.

6 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 paper, 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 program 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 enrollment letter recipients in our study. However, in the case we study, it was likely optimal for *every* consumer to cancel their subscription.

Consumers from lower SES neighborhoods and minorities were substantially less likely to cancel in response to the enrollment letters, yet were slightly more likely to cancel before the FTC lawsuit compared to consumers from higher SES neighborhoods who were likely to be white. Thus, the information provision policy disproportionately benefited consumers

³⁶Assuming 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.

³⁷This follows from our 0.01 month convergence rule.

from wealthier neighborhoods. 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 neighborhoods than for subscribers from higher SES neighborhoods. We also find that the enrollment letters reduced the expected future costs of subscriptions by only about 13% and would have allowed consumers to waste an average of \$423.

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 neighborhoods 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 neighborhoods were more likely to respond to the enrollment letter because those individuals were better at reading complicated letters or had more neighbors they could turn to for help. Although other explanations are possible, this would be consistent with other findings such as Bhargava and Manoli (2014).

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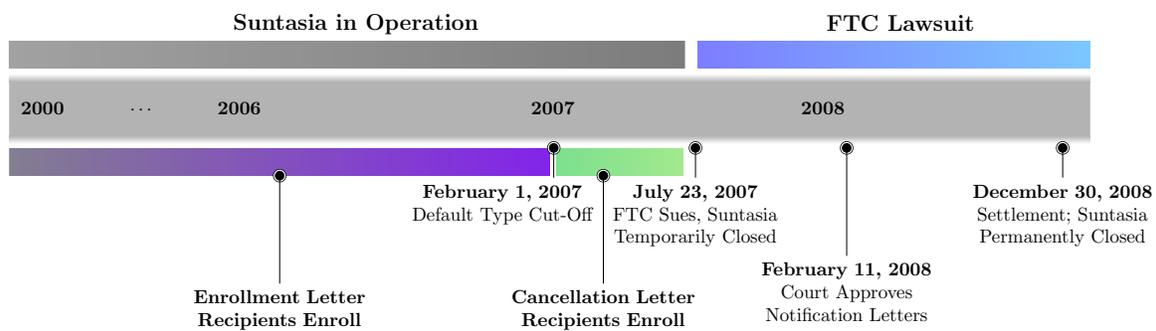


Figure 1: Suntasia Case Timeline

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.

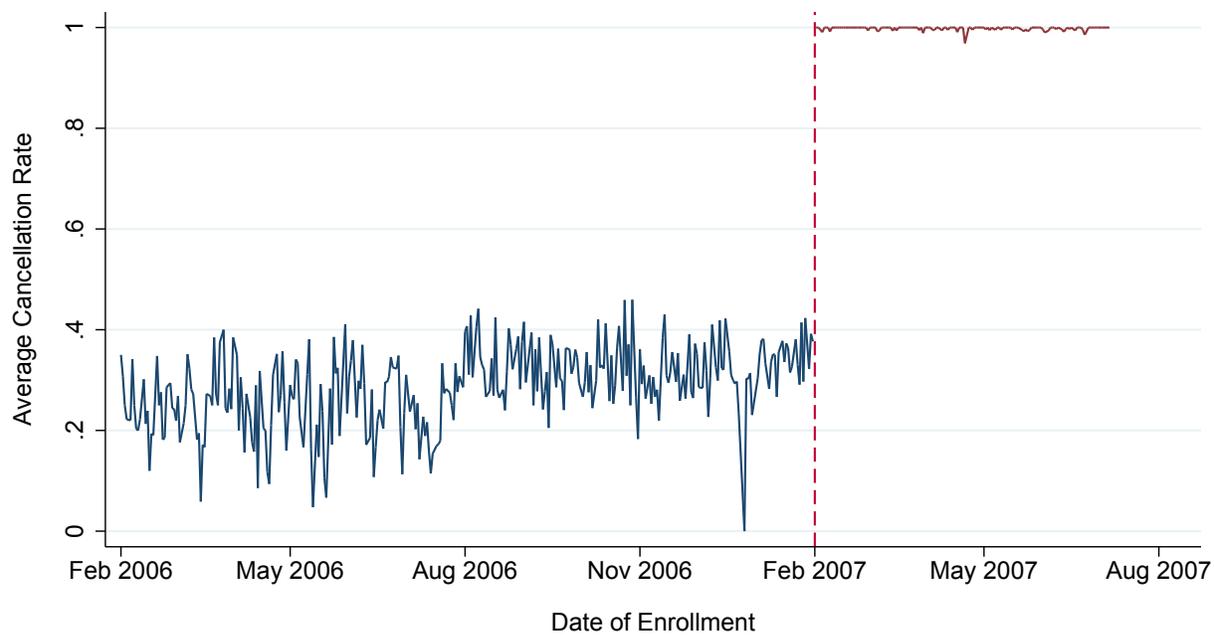
(Product name) and the Temporary Receiver have developed a business plan to manage future operations of the company.

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.....

Figure 2: Letter Template, Requiring Action to *Continue* a Subscription



Note: Excludes customers who enrolled after February 1 2007 and received notification of enrollment letters

Figure 3: Cancellations in Response to the Court-Ordered Notification Letters

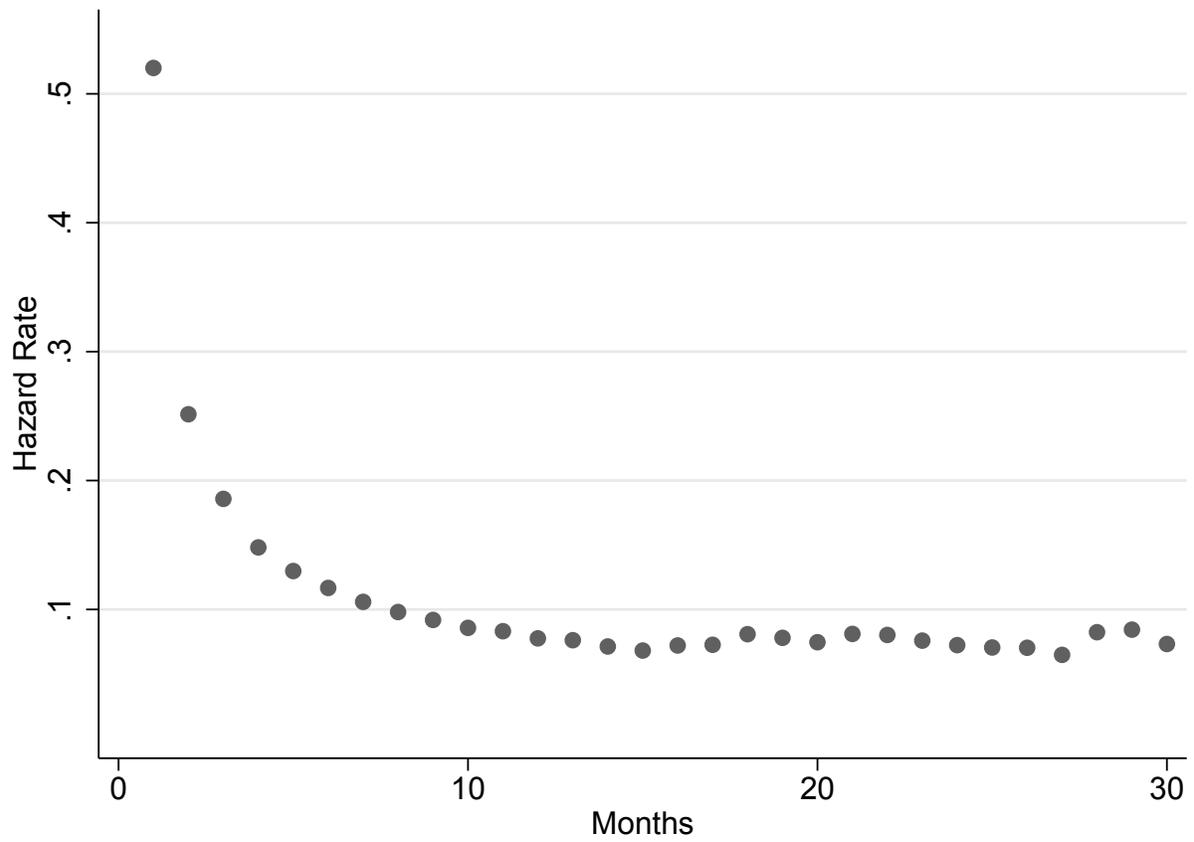
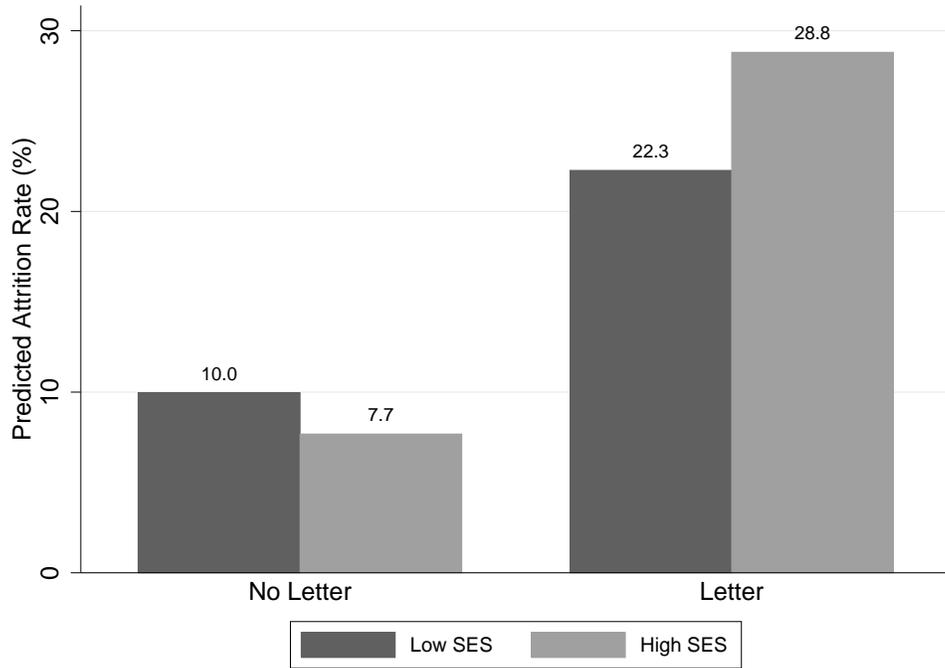
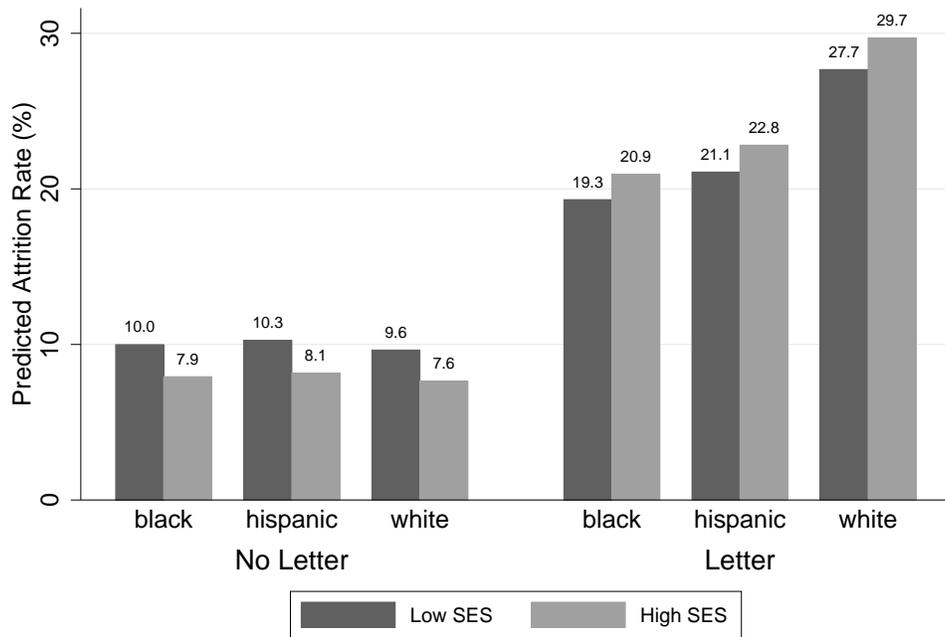


Figure 4: Hazard Of Cancelling All Sunasia Subscriptions, Aggregated into 30-day Periods from Date of Enrollment

(a) Socioeconomic Status Variables Only



(b) Socioeconomic Status and Race Variables



Note: SES calculated as average value of education and income variables within the lower and upper quartiles of that variable's distribution. Model fit for consumers present in billing cycle 6.

Figure 5: Predicted cancellation rates, with and without notification letters.

Table 1: Census Block Demographic Characteristics of Suntasia Subscribers

	(1) All Subscribers	(2) 30-Day Enrollment	(3) 30-Day Cancellation	(4) All U.S.
% Homeowner	60.4 (33.5)	58.5 (34.2)	58.0 (34.2)	66.2
Median Income	41853 (18679.0)	42190 (19185.2)	41777 (19026.3)	41994
% HS Dropouts	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>	494152	14423	16428	

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

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 Billing Cycle (\$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 Enrollment Date	Yes	Yes	Yes
<i>N</i>	42159	5256	2782
Enrollment Letter Reponse Rate	0.291	0.357	0.368

* $p < .05$

Coefficients are average marginal effects from a logit regression of the probability of cancelling in response to the court-ordered notification letters.

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

	1st Billing Cycle	Billing Cycles 6–8	
	No Letter	No Letter	Enrollment Letter
Median Income	-0.049*	-0.015*	0.031
% Homeowner	-0.057*	-0.014*	0.046*
% HS Dropouts	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	494152	196561	4728

* $p < 0.05$

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 6 months at the time of the FTC lawsuit and received notice of cancellation letters. Probability of race variables are calculated by combining the percent ethnicity by surname with percent ethnicity by Census block using the BISG method of Elliot et al. (2009)

Table 4: Marginal Effects of Demographics on Decisions to Cancel Suntasia Subscriptions, With and Without Enrollment Letters

	(1)		(2)		(3)	
% Homeowner						
Pre-Lawsuit	-0.023*	(0.0011)	-0.021*	(0.0011)	-0.021*	(0.0011)
Enrollment Letter	0.037*	(0.0114)	0.018	(0.0116)	0.008	(0.0111)
Median Income (\$0000s)						
Pre-Lawsuit	-0.003*	(0.0003)	-0.003*	(0.0003)	-0.003*	(0.0003)
Enrollment Letter	-0.002	(0.0024)	-0.001	(0.0024)	-0.002	(0.0023)
% HS Dropouts						
Pre-Lawsuit	0.007	(0.0039)	0.002	(0.0041)	0.002	(0.0041)
Enrollment 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)
Enrollment 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)
Enrollment 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)
Enrollment Letter			-0.074*	(0.0127)	-0.069*	(0.0123)
Probability Black						
Pre-Lawsuit			0.005*	(0.0010)	0.005*	(0.0010)
Enrollment Letter			-0.095*	(0.0105)	-0.090*	(0.0101)
Probability Asian/Other						
Pre-Lawsuit			0.006*	(0.0021)	0.006*	(0.0021)
Enrollment Letter			-0.059*	(0.0208)	-0.056*	(0.0205)
Amount Paid this Cycle						
Pre-Lawsuit					0.108*	(0.0009)
Enrollment Letter					-0.068*	(0.0078)
Number of Subscriptions						
Pre-Lawsuit					-0.206*	(0.0008)
Enrollment Letter					-0.042*	(0.0052)
<i>N</i>	1594023		1593914		1593914	
Effect of Letter Receipt	0.161		0.160		0.166	

* $p < 0.05$

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-enrollment fixed effects, an indicator for letter receipt, the amount paid in the current period, the number of programs enrolled in, counts of the number of payments received, and Census block demographics (alone and interacted with enrollment 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.

Table 5: Expected Cost to Consumers of Cancellation, Enrollment, and No Notification

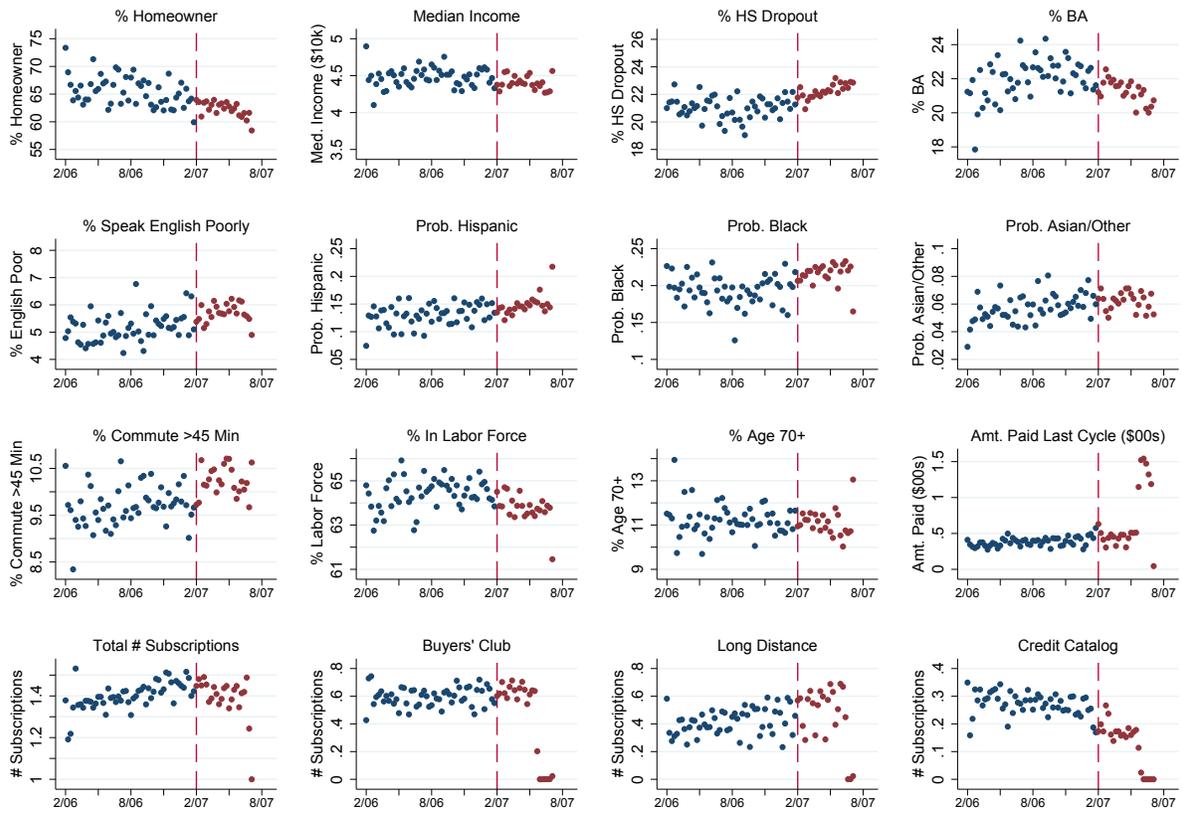
	(1) Billing Cycles	(2) \$ Paid
Cancellation Letter	0.00	0.00
Enrollment Letter	10.16	423.04
No Letter	11.53	477.82

A Appendix: Data Construction

We merged data from several sources:

1. Suntasia’s master customer database records the customer’s name, address, phone number, and enrollment date. The database contained 1,730,747 entries identified by unique, 10 digit phone numbers. 34,311 entries contained non-empty addresses that had a duplicate elsewhere in the table, but most of these repeat addresses appeared to belong to distinct residents of multifamily housing with quite different names. We dropped entries that did not correspond to any entries in the transaction data (described further in 2). We geocoded the remaining addresses with an about 80% match rate and then merged them with Census demographics (described further in 3). We matched customers’ surnames with Census data on race by surname (described further in 4) with a 90.7% match rate.
2. Suntasia’s transaction database contained one observation per transaction. Its entries list the customer’s phone number, the amount credited or debited, the transaction date, the company account in which debits of customer accounts were deposited (empty if the transaction was a credit to the customer), and a unique transaction number. The raw customer-transactions database contains 4,286,544 observations of credits or debits for a total of 820,744 customers (uniquely identified by the phone number). This count includes transactions for consumers even if we did not have a master entry with their name and address information. The database does not explicitly identify the type of subscription associated with each transaction, but as table A.1 shows, for the most part the amount of the charge uniquely identifies the subscription type. Because the initial fee for the credit catalog subscriptions is the same as the one-time fee for the “trip” product that does not involve ongoing subscription charges, we drop a small number of consumers who only have a single \$149 transaction.
3. Census data at the block and/or block group level. We utilized Census block level, short form data on race and ethnicity of the population over 18, and the homeownership rate. We utilized Census block group level long form data on the ability to speak English, educational attainment, and income.
4. Race imputed from surname for the most common surnames in the 2000 Census. The data consist of one observation for each of the 151,671 surnames that occurred 100 or more times in the 2000 Census. Each entry includes a count of the number of individuals with that surname and the fraction of the people with that surname who

identify themselves as 1) white, 2) black, 3) Hispanic, 4) Asian-Pacific Islander, 5) American Indian/Alaska Native and 6) two or more races. The Census sometimes omits entries for the least common races to protect citizens' privacy. In these cases, we spread the missing probability evenly over the omitted races so the fractions for each surname summed to 1. It is important to note that Census staff obtained six mutually exclusive race categories by eschewing the 2000 Census's distinction between race and ethnicity. This simplification considered any person to be solely Hispanic if they identified themselves as having Hispanic ethnicity, or if they identified their race as "other" and wrote in an identifiably Latino/Hispanic group. All non-Hispanic individuals who identified with only one race, or with a non-Hispanic "other", were assigned that race. Census staff combined the Asian and Hawaiian/Pacific Islander categories. See Word et al. (2007) for details.



Note: Excludes customers who enrolled after February 1, 2007 and received notification of enrollment letters.

Figure A.1: Testing for Discontinuous Changes in Covariates Around February 1, 2007

Table A.1: Cost and Characteristics of Suntasia’s Subscription Program

Product	Monthly Fee	Initial Fee	Trial Period
Buyers’ club	19.95	40.00	14 days
Long Distance	49.95	0.00	21 days
Credit catalog	9.95	149.95	7 days
Trip	0.0	149.95	N/A

Table A.2: Robustness Check: Marginal Effects of Opt-Out Letters By Demographics, Using Census Race/Ethnicity Only

Marginal Effects		
% Homeowner		
Pre-Lawsuit	-0.0232*	(0.00114)
enrollment Letter	0.0224	(0.0116)
Median Income		
Pre-Lawsuit	-0.00269*	(0.000252)
enrollment Letter	-0.00231	(0.00241)
% HS Dropouts		
Pre-Lawsuit	0.00670	(0.00417)
enrollment Letter	-0.0217	(0.0420)
% BA		
Pre-Lawsuit	-0.0124*	(0.00319)
enrollment Letter	-0.0310	(0.0308)
% Speak English Poorly		
Pre-Lawsuit	0.00119	(0.00705)
enrollment Letter	-0.151*	(0.0739)
% Hispanic		
Pre-Lawsuit	-0.00634*	(0.00286)
enrollment Letter	-0.0471	(0.0286)
% Black		
Pre-Lawsuit	0.000144	(0.00132)
enrollment Letter	-0.104*	(0.0132)
% Asian/Other		
Pre-Lawsuit	-0.0171*	(0.00444)
enrollment Letter	-0.0691	(0.0419)
<i>N</i>	1594023	
Effect of Letter Receipt	0.161	

* $p < 0.05$

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-enrollment fixed effects, an indicator for letter receipt, the amount paid in the current period, the number of programs enrolled in, counts of the number of payments received, and Census block demographics (alone and interacted with enrollment 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.

B Appendix: Additional Letter Templates

Product Letterhead

NOTICE OF CANCELLATION RIGHT

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 continue your membership unless you decide to cancel it. If you continue your membership, (Product name) will resume charging your checking account the monthly fee of \$ in the near future. **Please respond to this notice within 20 days from the postmark date of this notice.**

(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 cancel 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 cancel your services and your membership by calling 1 800.....

Product Letterhead

NOTICE TO RESUME BILLING

Date

We wrote to you on (date) and we have not received a response. Please carefully review the information in this letter.

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 continue your membership unless you decide to cancel it. **If you continue your membership, (Product name) will resume charging your checking account the monthly fee of \$ after 20 days from the postmark date of this notice.**

(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 cancel 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 cancel your services and your membership by calling 1 800.....

Product Letterhead

NOTICE TO RESUME BILLING

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(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 continue your membership unless you decide to cancel it. **If you continue your membership, (Product name) will resume charging your checking account the monthly fee of \$ after 30 days from the postmark date of this notice.**

(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 cancel 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 cancel your services and your membership by calling 1 800.....