“I DIDN’T WANT THAT!” AN EXPERIMENTAL STUDY ON 
DECEPTIVE ONLINE POST-TRANSACTION MARKETING OFFERS

A Thesis in
Information Sciences and Technology

by

Alan Nochenson

© 2014 Alan Nochenson

Submitted in Partial Fulfillment 
of the Requirements 
for the Degree of 

Master of Science

May 2014
The thesis of Alan Nochenson was reviewed and approved* by the following:

Jens Grossklags
Assistant Professor of Information Sciences and Technology
Thesis Advisor

Jim Jansen
Associate Professor of Information Sciences and Technology

David Reitter
Assistant Professor of Information Sciences and Technology

Peter Forster
Graduate Program Administrator
Assistant Dean for Online Programs and Professional Education

*Signatures are on file in the Graduate School
ABSTRACT

Post-transaction marketing offers are often designed to mislead consumers into purchasing products that are only tangentially related to their primary purchase. These products are presented directly following the completion of legitimate first-party transactions. To sell these offers, marketers use deceptive tactics that include illegally acquiring data from merchants and exploiting cognitive psychology. These practices are not only unpopular with consumers, but are possibly illegal. A US Senate report released in 2009 estimated that between 1999 and 2009 over 35 million people were misled into purchasing offers that they did not want and did not use. Due to the lack of regulations, and the global nature of the Internet, these deceptive practices are far from being extinct. In order to address this problem, this thesis presents a scenario, based on actual cases, where a small music shopping site has partnered with a third-party marketer to show post-transaction marketing offers to consumers. After a successful primary transaction in the music store, customers are automatically redirected to a post-transaction offer. The goal of this thesis is to examine the effectiveness of two ways of intervening to mitigate the harm of the deceptive post-transaction offer. This problem can be addressed from the perspective of the first-party e-commerce site or the perspective of the third-party marketers. This thesis presents the results of two experiments, each with over 450 participants, which test solutions from these two perspectives. The first experiment highlights the difficulty in reducing the harm of post-transaction offers from the first-party merchant perspective – only the strongest intervention significantly reduced the likelihood of participants purchasing a post-transaction offer. The second experiment empirically tests how different presentations of post-transaction offers play a role in how many participants purchase the offers. By gathering data on the effectiveness of intervening from the first and third-party perspectives, this thesis aims to inform regulators about these issues for use in future policy decisions.
## TABLE OF CONTENTS

List of Figures .......................................................................................................................... vi

List of Tables ........................................................................................................................... vii

Acknowledgements .................................................................................................................. viii

Chapter 1 Introduction ............................................................................................................. 1

Contributions .................................................................................................................... 3
  General contributions ............................................................................................... 3
  Experiment 1 ........................................................................................................... 4
  Experiment 2 ........................................................................................................... 7

Structure of Thesis ........................................................................................................... 11

Chapter 2 Background ............................................................................................................. 12

Opt-in and opt-out options ............................................................................................... 12
Incentives for deceptive tactics ........................................................................................ 14
Effectiveness of post-transaction marketing schemes ...................................................... 14
Experimenting on Amazon Mechanical Turk ................................................................. 16

Chapter 3 Methodology ........................................................................................................... 18

Why an experimental study? ............................................................................................ 18
Experimental Design ........................................................................................................ 19
Payment Structure ............................................................................................................ 21
Experimental Setup .......................................................................................................... 21
Experimental Framework ................................................................................................. 22
Experiment 1 .................................................................................................................... 27
  Motivations for Experiment 1 .................................................................................. 27
  First-party intervention types ................................................................................... 28
  Experimental Treatments ......................................................................................... 29
Experiment 2 .................................................................................................................... 31
  Ethical considerations ............................................................................................... 32

Chapter 4 Results across experiments ...................................................................................... 34

Demographic information ................................................................................................ 35
General conversion rate .................................................................................................... 36
Value of post-transaction service ..................................................................................... 37
Limitations ....................................................................................................................... 38

Chapter 5 Results for Experiment 1 ......................................................................................... 40

Effectiveness of Warnings ............................................................................................... 40
LIST OF FIGURES

Figure 1. Overview of the experimental procedure. See the figures that follow for detail on the individual screens. .................................................................20

Figure 2. Instructions page. This page was shown after the consent form. It details the specific task that participants will perform in the music store. ............................22

Figure 3. Shopping page. This page allowed participants to sample songs and add one to their cart. ..........................................................................................23

Figure 4. Checkout page. This page required participants to enter information in order to complete their song purchase. The box at the bottom right was only present in one treatment of Experiment 1 .................................................................24

Figure 5. Post-transaction offer page. This page presented participants with the deceptive post-transaction offer. The page as it appears here is how it appeared throughout Experiment 1, and in one treatment of Experiment 2. ..................................................25

Figure 6. Post-experimental survey. Participants were required to fill out this survey in order to finish the experiment. The full survey is shown in the appendix. .........26

Figure 7. Completion page. This page informed participants that they have successfully completed the experiment. .................................................................27

Figure 8. Visual display of opt-out and opt-in treatments (Experiment 2). .............................31

Figure 9. Age distribution of participants in the study ..........................................................36

Figure 10. Conversion rates within treatments (Experiment 1). Bars present 95% confidence intervals .................................................................40

Figure 11. Logistic regression. The effect of self-reported factors on conversion (Experiment 2). ....................................................................................42

Figure 12. Conversion rates within treatments (Experiment 2). T3 has the least conversions and T4 has the most. There is no difference between T1 and T2, or between T5 and T6. T1+2 have more conversions than T5+6. Bars present 95% confidence intervals ..............................................................................47

Figure 13. “No thanks” rate by Treatment (Experiment 2) (split by opt-in and opt-out. In opt-in treatments, clicking “No thanks” resulted in a non-conversion (declined offer), while in opt-out treatments, clicking “No thanks” resulted in a conversion. Bars present 95% confidence intervals .........................................................48

Figure 14. Conversion rates for treatments that show evidence of data pass and those that do not. The graph on the left is non-pooled; the graph on the right is pooled. Bars present 95% confidence intervals.............................................50
LIST OF TABLES

Table 1. Experimental treatments and their properties (Experiment 1). .................................. 29
Table 2. Experimental treatments and their properties (Experiment 2). .................................. 32
Table 3. Attrition rates by page (Both experiments)............................................................... 34
Table 4. Education of participants in the study across (Both experiments). ......................... 35
Table 5. Post-transaction offer ratings (Both experiments). .................................................. 37
Table 6. Experimental treatments and the respective conversion rates (Experiment 1). ........ 41
Table 7. Details of treatments and their conversion rates (Experiment 2)............................ 45
ACKNOWLEDGEMENTS

I would like to thank everyone that has shown their support for my research in general and the research that makes up the content of this thesis. Firstly, I would like to thank my advisor, Jens Grossklags, for guiding me and helping me to build my skills and confidence as a researcher, and for his many contributions, theoretical and otherwise, to this research. I would also like to thank the members of my committee, David Reitter and Jim Jansen, for their input and detailed feedback. I would like to thank Frank Ritter and the anonymous referees and associate editors at the conferences that this work has been submitted to and for their suggestions about improving this study and its presentations in conference papers and in this thesis. I need also to thank Naomi Borrebach for her editing, patience, and support throughout the process of conducting and writing up this research.

1 This thesis is comprised of the following conference papers:


Chapter 1

Introduction

Deceptive post-transaction marketing offers can be harmful. These offers are typically presented to customers on e-commerce sites after making a primary purchase. These post-transaction offers are usually of little to no value to consumers. Therefore, third-party marketers often rely on deceptive tactics to mislead customers into purchasing these offers. These deceptive tactics include collaborating with merchants to acquire consumer credit card numbers and exploiting cognitive psychology.

Deceptive post-transaction offers are a significant problem. As of 2008, the Federal Trade Commission (FTC) had brought cases against over 300 companies and individuals (Ballaré and Von Bergen 2008) for practices that are “likely to mislead the…reasonable consumer” (Cliffdale Associates 1983). To investigate the extent of these practices, the US senate compiled a report in 2009 that detailed how nearly 4 million customers were currently enrolled in deceptive post-transaction offers and over 35 million offers had been purchased since 1999. More shockingly, 99% of the people surveyed in this report that purchased offers never used the product and were not satisfied with their purchase (US Senate 2009).

In order to address this problem, this thesis presents a scenario, based on actual cases (e.g. Meyer 2009), where a small music shopping site has partnered with a third-party marketer to show post-transaction marketing offers to consumers. After a successful primary transaction in the music store, customers are automatically redirected to a post-transaction offer. The goal of this thesis is to examine the effectiveness of two ways of intervening to mitigate the harm of the deceptive post-transaction offer. This problem can be addressed from the perspective of the first-party e-commerce site or the perspective of the third-party marketers. This thesis presents the
results of two experiments, each with over 450 participants, which test solutions from both of these perspectives.

The first experiment attempts to intervene at the level of first-party merchants to reduce the harmful effects of these post-transaction offers. This experiment considers interventions that use prominent interstitial (full page) warning screens and those that do not. On these interstitial screens, the effect of interventions that require participant action to advance the study (active interventions) as well as those that do not require input from participants (passive interventions) are tested. Through experimentation, we find that intervention from the first-party perspective is a difficult task – only the most effortful of the interventions have a noticeable effect on reducing the number of people that purchase the post-transaction offer. From this, we conclude that intervention from this perspective may be helpful, but that intervention from the third-party perspective is also needed.

The second experiment intervenes at the level of the third-party marketers. It empirically tests how effective regulations required by the United States government (through the “Restore Online Shoppers’ Confidence Act” (ROSCA; U.S. House 2010)) actually are at reducing the amount of people that purchase deceptive post-transaction offers. ROSCA prescribes that marketers who use these post-transaction offers clearly disclose the terms of their offer and require an “additional affirmative action” to complete a sale (U.S. House 2010). This experiment tests a variety of interface differences and ways of presenting offers to customers (e.g. opt-in vs. opt-out) to see their effects on conversion of customers. The following section further discusses the aims and contributions of this thesis as a whole and of the experiments conducted within.
Contributions

General contributions

The contributions of this thesis can be divided into general contributions of the study and contributions made by the individual experiments conducted within. The general contributions of the study are:

- The experiments conducted in this thesis are the first experiments on deceptive post-transaction marketing. Therefore, the results of these experiments are potentially informative to policy makers to form future regulatory policies.

- These experiments are part of a small body of sophisticated studies conducted on Amazon Mechanical Turk.

First experiments on deceptive post-transaction marketing

Researchers and government bodies have previously examined the ecosystem of retailers and marketers to highlight the extent of post-transaction marketing and the strategies that are currently being used to deceive people (e.g. US Senate 2009; Meyer 2009). There has not, at this point, been a study that isolates the process that customers encounter when faced with deceptive post-transaction offers. By conducting a controlled experiment (in an naturalistic environment), this thesis is able to test the effects of various presentations of deceptive post-transaction offers on consumers. This thesis also tests the effect of warning messages before deceptive offers on consumers’ willingness to purchase these offers. This thesis is the first to gather experimental data on these different phenomena.
This thesis presents Mechanical Turk experiments

The experiments conducted in this thesis contribute to a small body of sophisticated studies that have been conducted on Amazon Mechanical Turk (e.g. Downs et al. 2010; Garg et al. 2012; Nochenson and Grossklags 2013; Reitter et al. 2013). It has become commonplace to use Amazon Mechanical Turk for research that consists solely on surveys. Research that uses Mechanical Turk for a study that has a self-contained environment with experimental treatments is rare. Therefore, this thesis is noticeable since it adds to the growing literature of such experiments. Participants in both experiments conducted in this thesis visit a website specifically designed to experiment with deceptive post-transaction marketing offers.

Experiment 1

The first experiment conducted in this thesis investigates mitigation of the problem of deceptive post-transaction marketing from the perspective of first-party retailers. There are three research questions addressed by this experiment:

- Does warning participants before they see a deceptive post-transaction offer affect the ability of the marketer to use the offer to deceive them?
- Are full-page interstitial warnings more effective than warnings that are part of the preceding first-party transaction page?
- Are active interventions more effective than passive ones?
Effectiveness of warnings

The main purpose of the first experiment is to test if better informed consumers are more likely to be able to resist deceptive post-transaction offers. This experiment offers warning messages to participants before they see the post-transaction offer to better inform them about the nature of the page that follows. Presumably, consumers that see these warnings will be better informed and will purchase fewer low-value offers than less-informed consumers.

To examine the effectiveness of these warnings, Experiment 1 includes a single control treatment (T0) that does not offer participants any warning message before the post-transaction offer. It also includes four experimental treatments that present different warning messages in different modalities. One of these treatments that presents a warning (T4) had a significantly lower conversion rate (rate of purchase of the post-transaction offer) than the control treatment. All other experimental treatments had a lower conversion rate than the control treatment, though those effects were not significant. Therefore, warning participants before they encounter deceptive post-transaction offers does appear to be effective, though the warning needs to be designed in the thoughtful way.

Full page warnings versus on-page warnings

To investigate the best way to frame warnings, the effects of both full-page and on-page warnings are tested in Experiment 1. Presumably, full-page warnings will be more effective than on-page warnings at reducing the number of people that purchase post-transaction offers. However, it is possible that alerting participants to the possible deception to follow (though a full-page or on-page warning) will have the same effectiveness, regardless of the form of that
warning. From a policy standpoint, it is likely easier to get compliance with a policy that requires on-page warnings than one that requires full-page interstitial warnings (since interstitial warnings are more likely to disrupt the flow of a customer). Therefore, the relative effectiveness of on-page and full-page warnings is of interest to policy makers.

To study this, a single treatment (T1) in Experiment 1 shows participants an on-page warning, while three treatments show participants a warning on a full interstitial page. The treatment that showed an on-page warning had a higher conversion rate than the treatments that used a full-page interstitial warning. However, the reduction in conversion rate from the on-page warning treatment to the full-page warning treatment was only marginally significant for one of the full-page treatments (T4). Thus, there is support for the hypothesis that full-page interstitial warnings are a more effective intervention than on-page interventions, though the nature of the interstitial warning does matter.

Active versus passive interventions

A third phenomenon tested in Experiment 1 is the role of active versus passive interventions. Both types of interventions are a subset of the interventions that use a full-page interstitial screen to warn participants about the post-transaction offer to follow. Active interventions are those that require participants to perform an action to dismiss the warning, while in passive interventions, warning messages are automatically dismissed after a set period of time. It is likely that active interventions reduce the number of conversions more than passive ones, since the requirement to perform a task is more likely to lead to participants reading the text on the page.

---

2 On-page warnings refer to warnings that are part of the preceding first-party transaction page that precedes the post-transaction offer page. These are contrasted with full-page interstitial warnings that are presented on a separate page between the first-party transaction page and the post-transaction offer.
Active and passive interventions were realized in this experiment by a page containing a warning message that participants dismiss by pressing a button and a page containing a warning message that automatically closes itself after a set period of time, respectively. In this experiment, there was a single active intervention that required participants to press a button, and there were two passive interventions. One passive intervention closed itself after being shown to participants for 5 seconds and the other closed itself after being shown for 10 seconds. There was no significant difference in conversion rates between the two passive interventions. The active intervention resulted in a marginally significant lower rate of conversion than the passive interventions. Thus, there is support for the hypothesis that active interventions are better than passive ones at informing participants prior to the post-transaction offer.

**Experiment 2**

The second experiment conducted in this thesis investigates mitigation of the problem of deceptive post-transaction marketing from the perspective of third-party retailers. This experiment addresses the following three questions:

- Are post-transaction offers that require consumers to opt out more harmful than those that, by default, do not opt in consumers?
- Does the use of data sharing agreements between merchants and marketers (i.e. data pass) help them sell more post-transaction offers?
- Does consumer knowledge of the presence of data-pass arrangements decrease the number of conversions?
**Opt-in versus opt-out**

Post-transaction offers can be classified as either opt-in or opt-out. Opt-in offers are the norm for product purchases on the web. An opt-in offer proceeds as follow: a consumer purchases one item and is then asked if they would like to purchase another. If they wish to purchase the second item, the total amount of their transaction will reflect the purchase of two items, not just one. Opt-out offers assume that consumers want the second product, even before asking them. Then the consumer only needs to be asked if they wish to *not* purchase the second product. For example, a consumer goes to a travel website where they purchase their ticket, and are then told they have purchased travel insurance along with their ticket. If the consumer instructs the website that they do not want travel insurance, the amount of the travel insurance will not be added onto the final transaction. It is likely that opt-out offers will have higher conversion rates than opt-in offers since ignorant consumers will follow the default (which in opt-out cases leads to an extra purchase).

In Experiment 2, three treatments are opt-in and three are opt-out. In the opt-in treatments, participants were required to hit a large button to consent to the sale of the post-transaction offer presented after they purchased a song. In the opt-out treatments, if participants did not hit the button, they were charged for their purchase of the post-transaction offer. Each treatment also had other characteristics, which are discussed below. Therefore, when determining the effectiveness of opt-in and opt-out treatments, it is only reasonable to compare pairs of treatments that have all the same properties except for opt-in or opt-out status. After running the experiment, the opt-out version in one pair of treatments was significantly higher than its opt-in counterpart\(^3\). The conversion rate for that opt-out treatment was three times that of the opt-in one. This gives weight to the hypothesis that opt-out offers are more harmful than opt-in offers.

\(^3\) In all other pairs of opt-in and opt-out treatments, there was no significant difference in conversion rate.
**Effectiveness of data pass**

Data pass is the practice of merchants sharing personal information with other parties. In this context, merchants pass credit card data, which they acquire from the primary transaction, to third-party marketers who use the data for post-transaction marketing offers. These types of arrangements are frowned upon by credit card companies and the United States government (US Senate 2009). Visa’s rules explicitly forbid sharing credit card numbers or sensitive information with parties not included in a transaction (Metzger 2010). However, even though data pass between merchants and third-parties is explicitly forbidden, merchants can partner with third parties to subvert these regulations. Through these partnerships, they can, for example, delay charging a consumer’s credit card until after a post-transaction marketing offer is presented. Though this behavior is certainly not in the spirit of the law, the legality of this practice is non-obvious.

In this experiment, participants do not actually enter any payment information (e.g. credit card numbers). Instead, they are given an endowment that they use to conduct the primary transaction (the purchase of a song) and to purchase the post-transaction offer. To simulate the transfer of payment information from first-party merchants to third-party marketers, this experiment asked for some personal, non-financial, information from participants (including their email addresses). Some of this personal data (email addresses) were passed to the post-transaction offer from the first-party merchant, thus simulating data pass.4

This experiment studies whether or not the use of data pass arrangements increases the number of consumers who purchase post-transaction offers. Four of the treatments in this experiment used data pass, while two did not. It is expected that for opt-in treatments, using data

---

4 Thus, the data pass in this study involves the transfer of email addresses instead of credit card information. And, since this study involves a music store and a marketer that were invented solely for it, the email addresses are not actually being passed from one company to another, though it appears that way.
pass will increase the number of conversions, since it reduces the amount of effort participants need to expend to purchase the post-transaction offer. The opposite effect is expected for opt-out treatments, since data pass eases the process of opting out. The experimental results show that, in opt-in treatments, data pass arrangements resulted in significantly higher conversion rates than counterpart treatments that did not use data pass. Similarly, the opt-out treatment that had the highest conversion rate is the one that did not use data pass. Thus, the experimental results support the reasoning that data pass is effective at reducing barriers to purchasing post-transaction offers.

**Consumer knowledge of data-pass arrangements**

When it comes to using the tactic of data pass, merchants and marketers need to decide whether or not to disclose this practice to consumers. Using data pass raises the likelihood of participants purchasing a post-transaction offer. Therefore, this strategy is attractive for merchants and marketers to use. However, the effect of informing participants that their information is being transferred is still unclear. Consumers who know data pass is taking place are presumably more likely to purchase post-transaction offers. Data passed from first to third parties gives consumers an indication that the two sites are partners, or even that they are not two distinct sites. Therefore, consumers are likely to trust the offer and to not read it as closely as they would have if they were not given this positive indicator.

To test the effect of informing consumers about data-pass arrangements, the post-transaction offer in Experiment 2 either displays or hides the data that is passed from the first-party music store. In two treatments, the data is shown on the post-transaction page in a box that is pre-filled. In another two treatments, that same information is passed, but the box that would be filled is hidden from the participants. The experiment is concerned with the effect of hiding the
The experimental results indicate that both treatments that hide the presence of data-pass arrangements had significantly lower conversion rates than their counterpart treatments that disclosed the data-pass arrangement. This gives reason to believe that hiding the presence of data pass does indeed reduce the likelihood of consumer purchase of post-transaction offers.

**Structure of Thesis**

The structure of the thesis is as follows: Chapter 2 discusses background information that was used to motivate this study and is useful in understanding it. Chapter 3 discusses the methodology employed in this study. It details reasons for conducting this type of study and the specific design of both of the experiments conducted here, including motivations, procedure and treatments. Chapter 4 presents general results captured across both experiments. Chapter 5 and Chapter 6 present the results of Experiment 1 and Experiment 2, respectively. Chapter 7 distills the results from the proceeding sections into a discussion about the implications of the results found in the context of the contributions presented earlier. Chapter 8 concludes the thesis with high-level lessons learned and Chapter 9 details next steps for this strand of research.
Chapter 2

Background

Opt-in and opt-out options

This thesis touches upon several dimensions of computer-human interaction research. In particular, it highlights the impact of opt-in and opt-out mechanisms. When considering post-transaction marketing offers, an inappropriate opt-in or opt-out design may contribute to consumers making decisions that they later regret. For example, a consumer that clicks the smaller non-affirmative button when faced with an offer expects that action to not have negative consequences (US Senate 2009). But, with opt-out post-transaction offers, this consumer would actually have tacitly consented to the purchase of an unwanted offer. To the best of our knowledge, we are unaware of any empirical evaluations of the default opt-in/opt-out settings with respect to post-transaction marketing and the impact such settings have on consumer behavior.

The controversy regarding whether individuals should be asked to explicitly consent to the use of personal data (“opt-in”) or instead should be asked to object to the use of data they wish to prohibit (“opt-out”), existed even before the widespread usage of the Internet. Different organizations hold different attitudes towards these two approaches. Since the European Union (EU) Data Directive of 1995, European law requires that opt-in choices must be used instead of opt-out choices for consumer protection. The more recent E-Privacy Directive, suggests that access to a user’s data can only be gained if the “user concerned has given his or her consent, having been provided with clear and comprehensive information… about the purposes of the processing (European Union 2009).” However, it is unclear to what degree these regulations apply to post-transaction marketing (in which the consumer has already agreed to share data
during a primary transaction). In contrast, industry organizations such as the Direct Marketing Association recommend an opt-out procedure (Direct Marketing Association 2002). This recommendation is not surprising from industry sources, since opt-out offers are much more beneficial for them than opt-in ones.

Previous e-commerce studies have shown opt-in and opt-out mechanisms have a significant impact on users’ participation in online activities (Staten and Cate 2003; Lai and Hui 2006). In academia, empirical studies have been conducted to examine the impact of opt-in and opt-out mechanisms within a variety of contexts. From the service providers’ point of view, Staten and Cate (2003) pointed out that opt-in strategies would raise companies’ costs and lower profits. Such procedures may also generate more unneeded offers to uninterested or unqualified consumers, and raise the number of missed opportunities for qualified consumers. From the consumers’ point of view, Lai and Hui (2006) studied how frames, defaults, and privacy concerns impact users’ online newsletter subscription behaviors. They found that opt-in approaches result in lower levels of participation than opt-out approaches (Lai and Hui 2006).

While the e-commerce studies on the effects of opt-in and opt-out mechanisms are relevant to this study, in this scenario the resulting trade-off is very problematic. From a business perspective, the goal of e-commerce offers is to increase user participation and possibly the amount of personal data being shared to ultimately increase profit. However, from a consumer and regulatory perspective, as well as from the viewpoint of legitimate businesses, the prevalence of deceptive and misleading marketing practices undermines trust in e-commerce and might have a negative long-term impact on sales. On the one hand, opt-out is in the interest of legitimate business to develop frictionless and convenient e-commerce experiences. On the other hand, these practices permit a grey-zone of business activities that undermines online commerce.
Incentives for deceptive tactics

Post-transaction marketing offers are lucrative for the parties involved, except for consumers. As Messinger et al. (2011) put it: “the main argument for allowing [these policies]…holds that these policies make commerce more efficient and flexible for the seller and buyer.” However, the tactics that are generally used to display these offers rarely lead to a situation beneficial for consumers. If consumers were fully informed about the terms of the offers, the small contingent that would purchase these offers would have nothing to be regretful about. The offer would be purchased with complete consent and knowledge. However, the way these offers are typically displayed to consumers may not lead to truly informed consent, since they exploit limits and biases of human decision-making.

However, deceptive post-transaction offers are immediately beneficial for sellers and marketers (though there may eventually be backlash from consumers). Sellers experience increased revenue in the short term due to deals with marketers. Marketers experience more sales and therefore a greater profit, part of which they pass to sellers. Data pass arrangements reduce the overhead that sellers need to invest in to offer additional services along with their products. But, the main reason that sellers are likely interested in using these offers is simple: they work.

Effectiveness of post-transaction marketing schemes

Deceptive post-transaction offers work because they exploit behavioral biases and limits of human cognition. One reason that post-transaction offers so often lead to purchases may be because third-party marketers exploit consistency of appearance, which induces a flow-state (Meyer 2009). Flow is the state when people “lose themselves” in a task and are likely to not be

---

5 A 2009 US Senate report found that 99% of the purchasers of offers never used the product purchased and were not satisfied with their purchase (US Senate 2009).
paying careful attention due to the speed at which they are processing the current task (Csikszentmihalyi 1975). The post-transaction marketing scam addressed here itself appears similar in appearance to the first-party site it follows in order to exploit the flow a user has (e.g. for entering information or clicking buttons) and have that carry over to their offer. In the language of Kahneman et al. (1991), these offers are designed in order to have customers utilize instinctive System I cognitive processes instead of more carefully reasoned System II ones.

In addition to relying on flow states to cause poor choices, marketers also exploit cognitive biases, some of which are more prominent under System I reasoning. Two prominent biases that are exploited by deceptive post-transaction offers are optimism biases and conditioned-response biases (Meyer 2009). The optimism bias that people exhibit leads them to think that whatever is presented in front of them is legitimate. Since customers are not constantly on the lookout for scams (i.e. they are generally optimistic), marketers are able to exploit this trust by making themselves appear like first-party sites.

Conditioned response biases are also exploited by marketers. These biases describe how people react to what they expect to be shown, not to what is actually shown. Therefore, when marketers place a box asking for an email to be filled in or an offer where customers can save 50%, they expect it to be connected more strongly to the previous pages, since that is typically how shopping online works. Through exploiting these biases, marketers are able to deceive customers into purchasing products they would not usually purchase, since customers believe the purchase is a discount on a previous offer or since they believe the act of entering additional information is part of the initial transaction.
Experimenting on Amazon Mechanical Turk

The study that conducted here uses Amazon Mechanical Turk to experimentally test which factors can mitigate the effect of deceptive post-transaction offers. Amazon Mechanical Turk (AMT) is a service that was launched in 2005 in order to allow "Requesters" to outsource Human Intelligence Tasks (HITs) to Mechanical Turk workers (Turkers). The service can be used for a variety of tasks including conducting surveys (e.g. Felt et al. 2012) and behavioral studies (e.g. Christin et al. 2012). Mechanical Turk is a useful tool for conducting research because the payment/quality ratio per-subject of participants is lower than it is typical with traditional laboratory studies and the demographic mix of participants is likely more diverse than university student convenience samples (Kam et al. 2007).

The payment/quality ratio being lower on Mechanical Turk means that researchers are able to obtain a large sample at a small cost (and generally in a short amount of time) with nearly no downsides with respect to demographic diversity. The demographic structure of Amazon Mechanical Turk is a topic that has been investigated by a number of authors in the last few years (e.g. Ross et al. 2010; Ipeirotis 2010; Mason and Suri 2012). These studies have shown that the country-of-origin of Turkers is nearly 50% American and 50% Indian, with small representations from other countries. In this study, we restrict participants to those that are US-based in order to ensure quality (which can sometimes be problematic with non-US based populations). Slightly over half of workers are female and the median age of Turkers is around 30 years (Mason and Suri 2012). This demographic diversity makes Mechanical Turk an attractive platform for recruiting subjects and conducting experiments.

A number of authors have conducted experimental economics experiments on Mechanical Turk in recent years. For example, Horton et al. (2011) used Mechanical Turk to replicate three experiments that have been extensively studied in economic laboratories. One of
the replicated experiments involved participants playing a one-shot prisoner’s dilemma game (with payments one-tenth the size online as in a physical lab). Each replication that was conducted was completed in fewer than 48 hours and cost less than $1 per subject on average. Despite the low stakes and anonymity of Mechanical Turk, the subjects’ behavior was consistent with findings from the standard laboratory (Horton et al. 2011).

Studies conducted on Amazon Mechanical Turk are occasionally critiqued (see, for example, the discussion in Sprouse 2011) for not being able to control the experimental environment as well as standard laboratory experiments. While this critique is certainly valid, it is not always a negative. Traditional laboratory studies introduce a number of additional factors that are not present or are reduced in similar studies on Mechanical Turk. For example, if the current study was reproduced in a traditional laboratory environment, participants would be placed into an unfamiliar computing environment to complete the task. Seemingly insignificant factors such as screen resolution of the experimental computer or height of the chair participants sit in can combine to confound experimental results. These factors can make participants uncomfortable, which may change how they act in the experiment. Since the purchasing behaviors that this study is interested in naturally occur in the privacy of a participants’ home, this is the most ecologically valid place to test subjects. While participants may leave the experimental environment to use the restroom or eat a snack, for example, this same behavior is present in actual purchasing episodes. Therefore, using Mechanical Turk allows us to better capture unknown ecological factors in the analysis than a typical experimental environment would.
Chapter 3

Methodology

Why an experimental study?

The artifact that was developed for this research was both unique and time-consuming to build. This begs the question of why this study was run as opposed to observing this behavior naturally. A purely naturalistic study would involve using data from actual companies about their use of deceptive post-transaction marketing. While that may have been preferred, the incentive structure involved for both first and third parties would have made observation of this type of practice difficult. Specifically, since third parties do not wish to publicize their involvement in this practice (since that would lead to fewer purchases and hence decreased revenues), they would most likely not be cooperative. And, with regards to first parties, there are a number of potential problems. First-party merchants that use this practice fall into one of two categories. They are either too large to cooperate with a study like this, or too small such that they would prefer to not have the interruption in their everyday affairs. First-party merchants may also not be explicitly aware of the depictive tactics being used by their third-party marketing partners. If these merchants were aware of the use of deceptive tactics by their partners, they would likely not wish to share their data about these practices. Sharing this data would likely lead to negative publicity from consumers. Thus, merchants are unlikely to participate in a naturalistic study about deceptive post-transaction marketing.
**Experimental Design**

A small music store entitled MelodiesFor.us was created for this study. The procedure of this experiment is briefly discussed here and is discussed in more detail (and with screenshots below). The sequence of the experiment is as follows:

1. From the Amazon Mechanical Turk interface, (potential) participants were informed of the basic idea of the study and were able to “Accept” the task. After acceptance, participants were presented with a consent form (per the University IRB). This form details the payment structure and gives more details about the experiment.

2. After acceptance, participants were directed to an instruction screen, which explained to participants that they were to enter a music store. They were told they would receive $1.50 to purchase a song (valued at $0.99). They were instructed that “any transaction in the shopping environment reduced your starting budget as described in the shopping environment” and that they would receive any leftover funds from their accounts at conclusion of the experiment.

3. Participants were then directed to the music store MelodiesFor.us. On this page, participants were able to view and sample six songs. Eventually, participant had to add one song to their cart to continue the experiment.

4. Participants then went to purchase the song and were shown a checkout page with that requested additional information for purchase confirmation. This page requested their Amazon Mechanical Turk ID, age, zip code, and email. Participants then confirmed the purchase of the song.

5. Participants were then presented with an intervention screen (in all but one of the treatments of Experiment 1 and none of the treatments in Experiment 2).
6. Participants were presented with the post-transaction offer screen, which offered a product called “SafeDelivery” (see below for details). If, based on the choices on this screen, a participant subscribed to the service, their endowment would be reduced by $0.50 leaving $0.01 of their budget.

7. After the post-transaction offer screen, participants were asked to complete a survey.

8. When participants successfully completed the survey, they were shown a message that the task had finished, and were instructed to indicate as such in the Mechanical Turk interface to receive payment.

Figure 1. Overview of the experimental procedure. See the figures that follow for detail on the individual screens.
Payment Structure

In accordance with a typical experiment in the experimental economics tradition, participants in both experiments in this study were compensated with a show-up fee and a bonus payment based on their performance. The show-up fee for participants was $0.50, which they earned for completing all parts of the experiment. Participants that did not complete all parts of the experiment were not compensated in any way.

In the music store, participants were given a sum of $1.50 (separate from the show-up fee) to purchase one song for $0.99. They were then redirected to the post-transaction offer. If participants did not purchase the offer, they were paid as a bonus their remaining endowment ($0.51). If they did purchase the post-transaction offer, their budget was reduced by the cost of the service ($0.50) and they were paid the remaining $0.01 as a bonus.

Experimental Setup

Both experiments were run on the Amazon Mechanical Turk service. Participants began the task by discovering it from the search results page, where they then previewed the task. This preview told potential participants that they would be participating in a research study on music purchasing behavior. They were told that they would be put in a purchasing environment, and would be paid based on their actions in the task. Once participants agreed to participate in the task through the Mechanical Turk interface, they were shown a link to the study. Once in the study, participants were immediately shown a consent form. This consent form detailed the way they were to be paid, the contact information for the researchers running the experiment, and detailed

---

6 It would have been possible to consistently pay participants that showed up, even if they did not complete the task successfully. However, since the time commitment that participants invested in attempting the task was minimal (compared to a traditional laboratory study), participants were only compensated if they finished the task in its entirety.
how they were able to leave the task at any time if they felt uncomfortable or did not wish to continue for any reason (they would however, not be paid for a partial effort). The consent form did not detail the exact procedure that the potential participant was to encounter; since that would have compromised the study (i.e. telling them about the post-transaction offer would render the interventions superfluous).

**Experimental Framework**

After participants consented to the terms of the experiment, they were redirected to a music store entitled MelodiesFor.us. They were again shown instructions and told that they would be asked to fill out a post-experimental survey, and that without completion of the survey, they would not be paid.

![Instructions page](image)

Figure 2. Instructions page. This page was shown after the consent form. It details the specific task that participants will perform in the music store.

Once participants read the instructions and began the task, they were redirected to the “shopping” page of MelodiesFor.us. This page displayed 6 songs which participants were able to sample and then add to their cart. All songs were taken from the site Jamendo.com and are
released under a Creative Commons license. Songs of different genres were selected in order to maximize the likelihood of all participants finding at least one song they liked and continuing the task.

Figure 3. Shopping page. This page allowed participants to sample songs and add one to their cart.

After adding a song to their cart and hitting the “Purchase” button, participants were redirected to the “Checkout” page. This page required them to fill in a small amount of personal information to complete their purchase. If the treatment called for it (only in Experiment 1; see Table 1), a small box was displayed with an intervention in the bottom right-hand corner.
Figure 4. Checkout page. This page required participants to enter information in order to complete their song purchase. The box at the bottom right was only present in one treatment of Experiment 1.

Once participants entered their personal information, they were redirected to an interstitial page (if necessary for the treatment in Experiment 1) which displayed the appropriate intervention message (see Table 1). After bypassing the intervention (or if no intervention is necessary), participants were shown the post-transaction offer page. The offer page offered participants the SafeDelivery service. This service emails the song which was just downloaded by participants to their email address. The fee for using this service was $0.50, which is half the price of the original song. However, since participants already were in possession of a copy of the song at this time, the service offered is of little to no value.

The page that presented this offer was identical for each treatment in Experiment 1, and different for treatments in Experiment 2. In Experiment 2, the only differences between treatments were select words in the bottom left box and in the large offer details box, along with the appropriate interface changes dictated by the treatment (opt-in or opt-out, and the need to fill in an email address or not).
Figure 5. Post-transaction offer page. This page presented participants with the deceptive post-transaction offer. The page as it appears here is how it appeared throughout Experiment 1, and in one treatment of Experiment 2.

The layout of the offer page was specifically designed to look similar, but not identical to, the rest of the MelodiesFor.us site. The consistency in interface likely increases the perceived trustworthiness of the post-transaction offer. This tactic, commonly used on real sites, likely does not leave marketers liable to legal action. The offer page includes wording that is not straightforward. It offers a “50% discount” on another copy of the song (i.e. for $0.50 extra, participants can get a second copy of the song). The offer is specifically worded so that the casual reader would mistake the discount on an additional product for a discount on the original (also a common practice). Once participants loaded the offer page, a download of their selected song was initiated by the site (as a zip file).
Figure 6. Post-experimental survey. Participants were required to fill out this survey in order to finish the experiment. The full survey is shown in the appendix.

After visiting the post-transaction offer page and either purchasing the offer or not, participants were directed to the post-experimental survey. This survey asked participants for basic demographic information (age, education, gender), information about the post-transaction offer they had just seen, individual reflections on the experience of participating in this experiment, information about general shopping behavioral, attitudes towards privacy and risk avoidance online, and included a few questions from the Social Comfort section of the Internet Use scale in Davis et al. (2002). Additionally, the survey asked a number of integrity questions that served to ensure participants filled out the survey in a mindful way.\footnote{The types of questions that we call integrity questions are also sometimes referred to as “screeners” in the language of Berinsky et al. (2012). These questions aim to ensure that participants are paying attention during the survey (and hopefully during the remainder of the experiment). According to Berinsky et al. (2012), many articles that use these types of checks simply exclude participants who fail them in an effort to reduce noise. Following the recommendations made in that paper, this study does not discard failed responses and to instead highlights to “shirkers” that they missed an integrity question. If a participant missed an integrity question they were redirected back to the survey and could not move on until the integrity questions were answered correctly.}
Experiment 1

Motivations for Experiment 1

The intervention that is likely to reduce the number of people that purchase deceptive post-transaction marketing offers is likely to be from the perspective of the third-party marketer. However, the marketers that display deceptive post-transaction offers have little incentive to comply with these types of regulations. Compliance with regulations would not give marketers involved in these tactics any benefits over non-compliance. Seemingly, compliance reduces the possibility of penalties and raises public perception of the marketer. However, neither of these benefits to not using deceptive tactics is exclusive to compliance. Markets that use deceptive post-transaction offers are able to avoid penalties by, for example, relocating to different jurisdictions. And, the increased goodwill gained by not using deceptive tactics is unnecessary for
a marketer that is able to present a different face (i.e. company name) alongside each advertisement.

Due to the mismatch of incentives of interventions of the third party perspective, we decided to first investigate how well first-party interventions would work. These interventions are likely to be effective because first-party merchants generally require a positive reputation to draw customers and are unable to change their public image to avoid penalties. For these reasons, first parties are more easily led to comply with regulations.

**First-party intervention types**

The concept of using interventions to reduce unwanted or insecure behaviors from people is a well-studied concept. Experiment 1 used interstitial (full page) interventions for some of its treatments since studies such as Egeleman et al. (2009) conclude that interstitial warnings lead to more desirable behavior (in that study, interstitial warnings led participants to be more privacy-conscious). However, to test the effect of interstitial warnings, this experiment also included a treatment that does not have an interstitial warning message (as well as a control treatment that does not have any warning message).

Within the treatments that present an intervention message on an interstitial screen, we used both passive and active interventions. The passive interventions that we used in this study showed participants a message on an interstitial screen which closed after a certain period of time. We chose to use time-based intervention messages partially due to the results of Good et al. (2007). That paper presented a study similar to the current one, in the context of software installations (where the discouraged behavior is installing software that includes spyware). By presenting short notices before installation, they were able to reduce the number of installations of this bundled malware, much like we are aiming to do here with the purchase of deceptive post-
transaction offers. They find that participants who completed their study in a more leisurely pace were less likely to install the malware. Therefore, we posit that slowing down participants (through time-based interventions) will make them less likely to purchase the deceptive post-transaction offer.

Additionally, this experiment tests a treatment where participants needed to click a button to continue after reading the interstitial message (an active intervention). This decision was motivated by recommendations such as those from the US government in the recently created Restore Online Shoppers’ Confidence Act, which requires in situations such as these for “the consumer to perform an additional affirmative action, such as clicking on a confirmation button” (U.S. House 2010).

Table 1. Experimental treatments and their properties (Experiment 1).

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Intervention</th>
<th>Description</th>
</tr>
</thead>
<tbody>
<tr>
<td>E1T0</td>
<td>No intervention</td>
<td>No intervention was present.</td>
</tr>
<tr>
<td>E1T1</td>
<td>On-page</td>
<td>A small box on the checkout page alerted participants that the next page was a marketing offer.</td>
</tr>
<tr>
<td>E1T2</td>
<td>Full page, passive (5 sec)</td>
<td>An interstitial screen indicated the next page is a marketing offer. Participant was redirected after 5 seconds.</td>
</tr>
<tr>
<td>E1T3</td>
<td>Full page, passive (10 sec)</td>
<td>An interstitial screen indicated the next page is a marketing offer. Participant was redirected after 10 seconds.</td>
</tr>
<tr>
<td>E1T4</td>
<td>Full page, active</td>
<td>An interstitial screen indicated the next page is a marketing offer. Participant was redirected after they clicked a button.</td>
</tr>
</tbody>
</table>

**Experimental Treatments**

In Experiment 1, the treatments differed only in their presentation of an intervention. All of these interventions warned participants about the post-transaction offer that they were about to
encounter. All parts of the experiment were identical among treatments, except for the presence and type of intervention displayed to participants. Each treatment was assigned a number ranging from 0 to 4, with 0 being the least obtrusive (most passive) and 4 being the most obtrusive (most active), and the others being on a scale between them. The “no intervention” treatment T0 is the baseline that all other treatments will be compared to, and is therefore numbered 0. Treatments T0 and T1 did not show participants any interstitial screen. Participants in those treatments purchased a song and were redirected immediately to the post-transaction offer page without any interstitial. Treatment T1 showed a small box on the checkout page of the first-party transaction warning participants about the post-transaction offer that follows (see Figure 4). In treatments T2, T3, and T4, participants were presented with an interstitial page warning them about the offer page to follow between the checkout page and the post-transaction offer. In treatments T2 and T3, the interstitial page was dismissed automatically after a set period of time (5 seconds for the “short” intervention, and 10 seconds for the “long” one). In T4, participants were required to hit a button labeled “Okay” to progress from the interstitial to the offer page. The message that we displayed to participants varied only in the last sentence of the intervention message. The generic intervention message displayed in treatments T1 through T4 was:

“On the following page, you will be presented with an offer from a third party.”

This message was not at all displayed for T0. In T1, this was the entire message and this message was displayed in the lower right-hand corner of the “checkout” page (Figure 4). In the “Timer” treatments T2 and T3, this was followed by the phrase:

“You will be redirected in X seconds”

In the above message, X was replaced with either 5 or 10, depending on the treatment. In the “Button” treatment T4, the first message was followed by the text below, as well as a button:

“Please click the button below to continue”
These supplemental messages were always presented on a separate line from the main intervention message, and were presented in an outlined box that was constant size, regardless of the message enclosed.

Experiment 2

Six treatment conditions were all expressed on the post-transaction page in Experiment 2. In treatments that were opt-in, participants were required to hit a large button to opt into the song email delivery service (costing $0.50), while in those that were opt-out, participants were required to hit the same large button (with different text) to opt out of the service and not be subscribed to the email delivery service (for $0.50). Treatments also varied on the information that needed to be entered to subscribe to the service. In some treatments, users were required to enter a valid email address in order to hit the large button, while in others the email address was filled in (from previously entered information to simulate data-pass), and in others users were not shown a box, and simply had to click the large button (i.e. the box was essentially pre-populated and hidden).

Figure 8. Visual display of opt-out and opt-in treatments (Experiment 2).
In Experiment 2, there were 6 different treatments, where in a given treatment, the post-transaction offer was either opt-in or opt-out, and an email either needed to be entered (not pre-populated), or was pre-populated, or was hidden and pre-populated. Due to interface differences and mechanisms of subscribing to the service, each treatment had customized text. Treatment T3 in Experiment 2 is the same as Treatment 0 of Experiment 1 (E2T3 = E1T0). This means that additional participants were not assigned to the second treatment, but results were just reused, since these treatments are identical.

Table 2. Experimental treatments and their properties (Experiment 2).

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Opt-in/out</th>
<th>Email pre-populated?</th>
</tr>
</thead>
<tbody>
<tr>
<td>E2T1</td>
<td>In</td>
<td>Yes</td>
</tr>
<tr>
<td>E2T2</td>
<td>Out</td>
<td>Yes</td>
</tr>
<tr>
<td>E2T3</td>
<td>In</td>
<td>No</td>
</tr>
<tr>
<td>E2T4</td>
<td>Out</td>
<td>No</td>
</tr>
<tr>
<td>E2T5</td>
<td>In</td>
<td>Yes-hidden</td>
</tr>
<tr>
<td>E2T6</td>
<td>Out</td>
<td>Yes-hidden</td>
</tr>
</tbody>
</table>

Ethical considerations

The subject matter of this experiment lends itself to being perceived as deceptive, despite its benign nature. The University Institutional Review Board approved this study and gave it an exempt (from full review) status. This status is reserved for research that is non-invasive and poses no more than minimal risk to participants. Deceptive studies are explicitly not allowed under this category. However, the study aimed at reproducing somewhat unscrupulous (but real) tactics that are seen on e-commerce sites. So, participants did not particularly enjoy their

---

8 The experiments conducted in this thesis were run under Penn State IRB Protocol #42911, which was approved on April 22, 2013.
encounter with a deceptive post-transaction offer (this information was gathered from feedback received from participants at the conclusion of the experiment).

Also, while Amazon Mechanical Turk participants are familiar with general research studies, sophisticated studies that present a task more complicated than a survey are rare. Therefore, this type of study may be unfamiliar to the average Mechanical Turk user, which introduces that possibility of confusion on the side of participants about the legitimacy of experimental procedures and payments.

To complete this study, participants were required to perform a purchasing task and complete a short post-experimental survey. Over the course of the task, participants were also required to enter a small amount of personally identifiable information. The data that the survey asked for was not of a sensitive nature. However, the requirement to provide this data was possibly too invasive for some participants, which explains why they did not complete the experiment. The consent form (Appendix A) presented before the task detailed that participants were under no obligation to complete the task and that they were able to leave the task at any time if it made them feel uncomfortable.
Chapter 4

Results across experiments

There were 1222 people that began this study (across both experiments). Of this group, 927 (73%) finished the study. Participants were only paid if they completed all parts of the task, including the post-experiment survey. Since participants were required to traverse a number of webpages and fill in information on some of them, it was expected that some participants would not complete the study in its entirety. Table 3 details the exact page that participants finished on, a metric that has been titled attrition.

Table 3. Attrition rates by page (Both experiments).

<table>
<thead>
<tr>
<th>Last page</th>
<th># of Participants (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Consent/instructions form (Appendix A &amp; Figure 2)</td>
<td>16 (1%)</td>
</tr>
<tr>
<td>Music shopping (Figure 3)</td>
<td>36 (3%)</td>
</tr>
<tr>
<td>Checkout (Figure 4)</td>
<td>139 (11%)</td>
</tr>
<tr>
<td>Post transaction offer (Figure 5)</td>
<td>97 (8%)</td>
</tr>
<tr>
<td>Post survey (Figure 6)</td>
<td>47 (4%)</td>
</tr>
<tr>
<td>Finished</td>
<td>927 (73%)</td>
</tr>
</tbody>
</table>

On the “Checkout” page and the post transaction offer page, participants had to enter in their email address, zip code, age, and Mechanical Turk ID. 11% of all participants chose not to provide this information, and instead left the task without payment. Presumably, the solicitation of their information caused participants to make a conscious privacy choice. That is, participants on this page needed to evaluate if providing this information was worth the reward offered. This presence of this choice explains the high rate of attrition on this page – since participants had to

9 There were 12 participants (0.94%) that made it to the last page of the experiment but did not complete it correctly due to any number of technical difficulties. They have been removed from the analysis.
give personal information, 11% of them did not feel the reward offered by the task was worth the personal information they were asked to provide.

**Demographic information**

From this point forward, all analysis will be conducted on only those participants that successfully completed the all parts of the experiment. The participants that completed this experiment were not out of the ordinary for a study on Mechanical Turk. Of the 927 participants that finished the study (across both experiments), the mean age was 32 years ($\sigma = 11$ years). 48% of the participants were female (444 participants). Consistent with the ages given, most participants had completed some college or more. This demographic mix of participants was consistent with previously-completed surveys on the demographics of participants on Mechanical Turk (Ipeirotis 2010; Horton et al. 2011).

Table 4. Education of participants in the study across (Both experiments).

<table>
<thead>
<tr>
<th>Education Level</th>
<th>Count</th>
<th>%</th>
</tr>
</thead>
<tbody>
<tr>
<td>Graduate or Professional Degree</td>
<td>100</td>
<td>11%</td>
</tr>
<tr>
<td>Four year college degree</td>
<td>290</td>
<td>31%</td>
</tr>
<tr>
<td>Two year college degree</td>
<td>98</td>
<td>11%</td>
</tr>
<tr>
<td>Some college</td>
<td>329</td>
<td>36%</td>
</tr>
<tr>
<td>High school degree</td>
<td>106</td>
<td>11%</td>
</tr>
<tr>
<td>Some high school</td>
<td>4</td>
<td>0.4%</td>
</tr>
</tbody>
</table>
Out of all of the participants across all experiments and all treatments, 279 people (28.7%) purchased the post-transaction offer. The highest conversion rate for any treatment across both experiments was in T4 in Experiment 2 (E2T4), which converted nearly 70% of the participants that encountered this treatment. The lowest conversion rate was in T4 of Experiment (E1T4), which converted only 9% of participants. This vast difference is due to the deceptiveness of the offer and how informed participants were in each treatment. In the worst treatment, participants were actively trying to be deceived, whereas in the best treatment, participants were actively trying to be informed (against even their own proclivity toward the opposite).
Value of post-transaction service

A complaint against this strand of research could possibly focus on the value of the post-transaction offer. The post-transaction offers presented by marketers are sometimes of some value to consumers. In this study, however, we aimed to simulate a situation where the post-transaction offer is of little to no value, and therefore, the act of reducing the number of conversions is easily seen as a socially optimal desire. However, if participants in the study had said the offer was truly valuable, the whole study would be of little value. However, this was not the case. As expected, 44% of participants rated the offer’s value the lowest and only 5% of people rated it the highest. The median value that participants rated the post-transaction offer was only 2. This finding is consistent with how the service was supposed to appear (near zero-value).

Table 5. Post-transaction offer ratings (Both experiments).

<table>
<thead>
<tr>
<th>Rating</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
</tr>
</thead>
<tbody>
<tr>
<td>Count</td>
<td>434</td>
<td>178</td>
<td>176</td>
<td>87</td>
<td>52</td>
</tr>
<tr>
<td>% of total</td>
<td>44%</td>
<td>18%</td>
<td>18%</td>
<td>9%</td>
<td>5%</td>
</tr>
<tr>
<td>% convert</td>
<td>14%</td>
<td>24%</td>
<td>45%</td>
<td>62%</td>
<td>81%</td>
</tr>
<tr>
<td>(61/434)</td>
<td>(43/178)</td>
<td>(79/176)</td>
<td>(54/87)</td>
<td>(42/52)</td>
<td></td>
</tr>
</tbody>
</table>

There is a strong linear relationship between how valuable participants thought the product is and how likely they were to have purchased it ($R^2 = 0.99, p < 0.001$). Participants that rated the product very valuable (rating of 5) purchased it 81% of the time, whereas participants that rated a 3 purchased 55% of the time, and those that rated a 1 purchased it 14% of the time. This relationship is as expected, though it is surprising that nearly 15% of the participants that thought the service was not at all valuable still purchased it.
Limitations

The results that are presented above and the results that will be presented below for each of the experiments are mainly reported using chi squared statistics. Many chi squared tests were performed to investigate the research questions previously stated. However, not all of these results were significant. Since there were a large number of these tests performed, that increases the likelihood of false positives. That is, if data is tested enough time, significant results will show up by chance alone. One standard way to correct this problem is by using the Bonferroni correction (Dunn 1961). This correction raises the threshold for what are considered statistically significant results (e.g. from the usual 95% value of $1 - p$ to 97.5% or a $p = 0.025$). This correction was not used in the analysis that follows. This correction makes effects more difficult to see and was not used primarily since this study was exploratory in nature. A future step that would validate the results presented here would be to repeat these experiments taking a more directed approach which minimizes the number of unnecessary statistical tests and uses the correction.

Additionally, both experiments presented in this thesis are conducted using a single scenario (i.e. music purchasing). While there is no obvious reason to believe that the purchasing episode in a music store would lead to different behavior than purchasing episodes in other environments, the results do not necessarily generalize. To state with confidence the generalizability of the results in this thesis, it is necessary to confirm the presence of the same effects in participants in experiments that vary interface differences and even the overarching scenario. For example, it would be possible to change the experimental music store into a video store or a document purchasing store. Or, on a smaller note, it is possible to move around key interface elements uniformly within treatments. If the effects found here are able to be recreated within these other interfaces and scenarios, this would give weight to the generalizability of the
conclusions made by this study. However, time constraints did not permit additional experiments to be run. This is a natural area for other researchers to continue this work, in the classic tradition of confirming or denying experimental effects.

Unless otherwise stated, all chi squared tests conducted below are conducted pairwise between treatments that share the same features and differ only on the feature in question. The alternative way to conduct these tests is to pool treatments that do not have significantly different values on a certain feature, and compare pooled treatments to other pooled treatments that have a different value on the feature of interest. This can only be done effectively when the feature that treatments are being pooled over is not significantly different between the treatments that are being pooled. In the analysis that follows, pairwise comparisons are used unless it is stated that pooled comparisons are used.
Chapter 5

Results for Experiment 1

There were 651 people that began Experiment 1. Of this group, 476 (72%) finished the experiment. All analysis will be conducted on only those participants that successfully completed the all parts of the experiment.

Effectiveness of Warnings

The main effect of interest in this experiment is if any of the interventions offered from the first-party perspective lowered the relative amount of purchasers to the post-transaction offer. Treatment T0 did not present participants with any type of intervention, and, as expected, had the highest conversion rate (percentage of participants that purchased the offer and had an email sent to them). Treatment T4, which presented participants with a button on an interstitial screen, had the lowest conversion rate. This difference is statistically significant ($T0 > T4; \chi^2 = 3.8, p = .05$).

![Figure 10. Conversion rates within treatments (Experiment 1). Bars present 95% confidence intervals.](image-url)
Conversion rate seems to be a pseudo-linear function of the inverse amount of effort that participants are required. That is, the lowered-numbered treatments (which require less effort) have higher conversion rates, where the higher-numbered and more effortful treatments have lower conversion rates, respectively. This is consistent with the theory underlying the reason for the interventions; the more effort that is required for a user to continue through a task, the greater likelihood they will read the message surrounding the invention which will decrease the propensity of an individual to purchase the post-transaction offer.

Table 6. Experimental treatments and the respective conversion rates (Experiment 1).

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Intervention</th>
<th>Full page intervention?</th>
<th>Active or passive?</th>
<th>% Conversion (转换数/总数)</th>
</tr>
</thead>
<tbody>
<tr>
<td>T0</td>
<td>No intervention</td>
<td>No</td>
<td>N/A</td>
<td>18.7% (17/91)</td>
</tr>
<tr>
<td>T1</td>
<td>On-page</td>
<td>No</td>
<td>N/A</td>
<td>18.1% (15/83)</td>
</tr>
<tr>
<td>T2</td>
<td>Full page, passive (5 sec)</td>
<td>Yes</td>
<td>Passive</td>
<td>16.5% (16/97)</td>
</tr>
<tr>
<td>T3</td>
<td>Full page, passive (10 sec)</td>
<td>Yes</td>
<td>Passive</td>
<td>17.7% (17/96)</td>
</tr>
<tr>
<td>T4</td>
<td>Full page, active</td>
<td>Yes</td>
<td>Active</td>
<td>9.0% (9/100)</td>
</tr>
</tbody>
</table>

Full page warnings versus on-page warnings

Treatment T1, which showed participants an on-page intervention, had a conversion rate double that of treatment T4, which presented a full page interstitial warning (this relationship is marginally significant; $T1 > T4; \chi^2 = 3.3, p = .07$). The conversion rate for treatment T1, which presented an on-page intervention, was higher than all of the treatments that presented full-page interventions, though only one of these relationships is marginally significant.
Active versus passive interventions

Treatment T3, which showed participants a full-page warning message that automatically closed after 10 seconds, had a conversion rate of nearly double that of Treatment T4, which required participant to press a button to dismiss a full-page warning message (this relationship is marginally significant; $T3 > T4$; $X^2 = 3.1, p = .08$).

Self-reported Factors

Participants filled out a survey after they viewed the post-transaction offer. The survey presented a number of different questions (aside from demographic ones), including validated scales and other appropriate questions (some taken from Davis et al. 2002).

$$ Log(Email\ sent?) = \text{Intercept} + \beta_1(\text{Peacefulness online}) + \beta_2(\text{Perceived safety online}) $$

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Question</th>
<th>Estimate (std. error)</th>
<th>P-value (z-value)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Intercept</td>
<td></td>
<td>-1.64 (0.54)</td>
<td>0.0025** (-3.06)</td>
</tr>
<tr>
<td>Peacefulness online</td>
<td>“I find it peaceful to be online” (5-pt Likert)</td>
<td>-0.24 (0.11)</td>
<td>0.0259* (-2.23)</td>
</tr>
<tr>
<td>Perceived safety online</td>
<td>“I feel safe when I am on the Internet” (5-pt Likert)</td>
<td>0.25 (0.11)</td>
<td>0.0252* (2.24)</td>
</tr>
</tbody>
</table>

DOF: 466  AIC: 407.44  **$p < 0.01$  *$p < 0.05$

Figure 11. Logistic regression. The effect of self-reported factors on conversion (Experiment 2).
A stepwise logistic regression was run to evaluate if any of these questions were related to the likelihood of participants purchasing the post-transaction offer. The only two variables that survived after eliminating ones that did not affect the model’s fit (evaluated through AIC\(^{10}\)), that remained were participants’ self-reported peace online and self-reported feeling of safety online (from Davis et al. 2002).

Both of these factors are significant predictors of if a participant will purchase the offer (for each \(p < 0.05\)). The odds ratios for these factors are 0.79 and 1.29, respectively. This means that a participant that rated their peace online as 1 point higher (on a Likert scale) than another would be (on average) 21\% less likely to purchase the offer. It is likely that more peaceful participants are not as rushed, and therefore are able to read and understand the interventions that were offered to them. This effect points to the presence of one of the biases that was previously mentioned that marketers take advantage of, the conditioned response bias. Since participants believe that sites are usually legitimate, the peaceful participant will not be looking for any problems, and will therefore expect a normal situation and respond accordingly, despite the situation being abnormal).

\(^{10}\) AIC is the Akaike information criterion. It measures the quality of statistical models. The model above was formed by first creating a logistic regression model that used all survey variables as predictors of conversion. Then, variables were taken out one by one based on how little the removal affected the fit of the model. This was evaluated by testing the differences between the AIC of a model with a given term and the AIC of the same model with that term. This process of removing terms is elimination.
Also, a participant that rated their perceived safety online as one point higher than another person would be 29% more likely to purchase the offer. The safer participants feel online (indicated by a higher rating on this question), the more likely they are to trust the site in front of them and therefore to ignore instructions written about an offer, and therefore the more likely they are to purchase the offer. This relates to the optimism bias previously discussed. Participants that feel safer are likely to be more optimistic, and therefore more easily tricked by deceptive offers.

**Who purchased the post-transaction offer?**

Despite best efforts at intervention from the first-party perspective, there were still a number of people that purchased the post-transaction offer in this experiment. Across all treatments in this experiment, 74 people purchased the offer (16%). It is possible that the people that purchased the offer are less educated, younger/older, or some other difference in demographic from those people that did not purchase the offer.

With regards to gender, the purchasing group was nearly evenly split, with 53% women (39/74). The non-purchasing group, on the other hand, only had 43% women (170/391). This difference is not statistically significant (by chi squared test) though this trend indicates that with increased power a significant relationship between gender and conversion rate may appear. There is no significant difference (by chi squared test) in education levels between the two groups. More work is needed to investigate the demographic differences that may exist between those that purchase post-transaction offers and those that do not.
Chapter 6

Results for Experiment 2

While Experiment 1 dealt with interventions from the perspective of first-parties, Experiment 2 dealt with third-parties exclusively. By varying the interface characteristics of the post-transaction offer and the process of purchasing the offer, this experiment investigates the effect of these differences.

Attrition by Treatment

There were some differences in the amount of attrition among the different treatments in Experiment 2. Since the treatments only affected the post-transaction offer page, this is the page that these attrition differences are examined on. Participants in T3 (opt-in, not-pre-pop) had the lowest attrition rate, while participants in T4 (opt-out, no-pre-pop) had the highest. Since T3 is the least deceptive treatment (since it requires the most conscious intervention and is opt-in), it is unsurprising that it has the lowest attrition. Conversely, T4 is the most deceptive treatment (since it is opt-out and requires the most invention to actually opt-out) and has the most attrition.

Table 7. Details of treatments and their conversion rates (Experiment 2).

<table>
<thead>
<tr>
<th>Treatment</th>
<th>Opt-in/out</th>
<th>Email pre-populated?</th>
<th>% Conversion (Email sent) (# convert/# total)</th>
</tr>
</thead>
<tbody>
<tr>
<td>T1</td>
<td>In</td>
<td>Yes</td>
<td>45.3% (43/95)</td>
</tr>
<tr>
<td>T2</td>
<td>Out</td>
<td>Yes</td>
<td>44.4% (40/90)</td>
</tr>
<tr>
<td>T3</td>
<td>In</td>
<td>No</td>
<td>18.7% (17/91)</td>
</tr>
<tr>
<td>T4</td>
<td>Out</td>
<td>No</td>
<td>68.6% (59/86)</td>
</tr>
<tr>
<td>T5</td>
<td>In</td>
<td>Yes-hidden</td>
<td>32.6% (30/92)</td>
</tr>
<tr>
<td>T6</td>
<td>Out</td>
<td>Yes-hidden</td>
<td>34.4% (33/96)</td>
</tr>
</tbody>
</table>
Attrition rates were also lower for treatments that were opt-in (T1, T3, T5) than for their opt-out counterparts (T2, T4, and T6, respectively). Since opt-in is the preferred way for websites to behave (as defined by policy makers and consumer advocates, and the legal way according to ROSCA), this result is not surprising. When faced with a choice that is unusual or seemingly untrustworthy, it makes sense that participants are more likely to close their browsers. Paradoxically, in these situations, participants would remain signed up for the service (since they do not opt-out by continuing) by closing the experiment at this point (which is exactly what marketers want). From here onwards, analysis of this experiment will be restricted to those participants that successfully completed the experiment (550 people).

**Effect of experimental treatments**

In Experiment 2, the treatments that varied among participants all aimed at reducing the likelihood of a participant to purchase the post-transaction offer (which assumes purchasing is an undesired result). Of the 550 participants that finished the experiment successfully, 40% (222 participants) purchased the post-transaction offer (across treatments). Treatments do have significantly different conversion rates ($\chi^2 = 51.6, p < 0.0001$).

T3’s conversion rate is much lower than all other treatments ($\chi^2 = 20.2, p < 0.0001$). This was the expected result for this treatment, since it is the most straightforward of all the treatments. In order to purchase to the post-transaction offer in this treatment, participants were required to enter their email address and hit a large button. Due to the increased effort required to subscribe, and the fact that clicking “No thanks” would not have subscribed participants in this condition, it is not surprising it has the lowest conversion rate.
The result is contrasted by perhaps the most deceptive treatment, T4. This treatment’s conversion rate is much higher than all other treatments ($\chi^2 = 32.4, p < 0.0001$). In this treatment, participants purchased the post-transaction offer by default, and were required to enter their email address to unsubscribe. Due to the increased effort needed to unsubscribe, nearly 70% of users simply clicked “No thanks”, which left them subscribed to the service. This result was also expected, since it is a result of the conditioned response bias. Since participants generally think hitting “No thanks” to an offer will not subscribe them, many participants will click this option since they presume it to be harmless.

Figure 12. Conversion rates within treatments (Experiment 2). T3 has the least conversions and T4 has the most. There is no difference between T1 and T2, or between T5 and T6. T1+2 have more conversions than T5+6. Bars present 95% confidence intervals.
Opt-in versus opt-out

The first group of effects to look at are effects of opt-in versus opt-out in treatments that have the same email pre-population (yes, no, yes-hidden). When determining the effectiveness of opt-in and opt-out treatments, it is only reasonable to compare pairs of treatments that have all the same properties except for opt-in or opt-out status. Therefore the pre-populated treatments T1 and T2 can be compared, the pre-populated and hidden treatments T5 and T6 can be compared, and the not pre-populated treatments T3 and T4 can be compared. In the case where the user’s email is pre-populated (T1 and T2), there is no significant difference in conversion between people who were shown offers with the opt-in option and those with the opt-out option. In the case where the user's email is pre-populated and hidden (T5 and T6), there is also no significant difference in conversion between participants that were presented with opt-in and opt-out offers. When participants’ email addresses were not pre-filled, the opt-out treatment T4 had a significantly higher conversion rate than its opt-in counterpart T3 ($T4 > T3$; $\chi^2 = 45, p < 0.0001$).

Figure 13. “No thanks” rate by Treatment (Experiment 2) (split by opt-in and opt-out. In opt-in treatments, clicking “No thanks” resulted in a non-conversion (declined offer), while in opt-out treatments, clicking “No thanks” resulted in a conversion. Bars present 95% confidence intervals.
Effectiveness of data pass

To investigate the effectiveness of data pass, two metrics can be used. Like above, it is possible to investigate the rate of conversion between groups of participants that were presented with offers that utilized data pass and those that did not. Additionally, it is interesting to look at the rate of people that chose the default option on the post-transaction offer page by clicking “No thanks” to see the effects of cognitive biases. Whether using conversion rate or “No thanks” rate as a metric, it is important to only compare treatments that have the opt-in or opt-out properties.

Using the metric of conversion rate, experimental results show that data pass (simulated by a pre-filled box on the post-transaction offer page) increases the conversion rate for treatments that present the offer as opt-in and decreases the conversion rate for treatments that present the offer as opt-out. Treatment T1, which presents an opt-in offer using data pass, has a conversion rate over double that of Treatment T3, which presents an opt-in offer without data pass (this relationship is significant; $T1 > T3; \chi^2 = 15.0, p < 0.001$). Treatment T4, which presents the post-transaction offer using data pass and an opt-out strategy, has a conversion rate over 50% higher than Treatment T2, which uses opt-out and does not use data pass (this relationship is significant; $T4 > T2; \chi^2 = 10.4, p < 0.01$).

Using the metric of “No thanks” rate, experimental results show that data pass leads fewer participants to select default options. In Treatment T1, which presents an opt-in offer using data pass, fewer people select “No thanks” compared to Treatment T3, which presents an opt-in offer without data pass (this relationship is significant; $T1 < T3; \chi^2 = 13.8, p < 0.001$). Also T4, which presents the post-transaction offer using data pass and an opt-out strategy, fewer people select “No thanks” compared to Treatment T2, which uses opt-out and does not use data pass (this relationship is significant; $T2 < T4; \chi^2 = 9.5, p < 0.01$). The extra effort involved in typing an email address into a box reduces the number of people that click the more effort-some
option regardless if that option is to opt in or opt out of the offer. Or, by making it more convenient to choose the “No thanks” option, consumers will do so, even to their own detriment.

Figure 14. Conversion rates for treatments that show evidence of data pass and those that do not. The graph on the left is non-pooled; the graph on the right is pooled. Bars present 95% confidence intervals.

Consumer knowledge of data-pass arrangements

The pre-populated and hidden treatments T5 and T6 are interesting with regards to the other treatments. These treatments do not require a conscious privacy choice to be made, since there is no solicitation of information (email) nor is there clear indication (i.e. a pre-populated box) that information is being transferred. Treatments T5 and T6 (pooled), which do not disclose the use of data pass, have conversion rates that are 25% lower than treatments T1 and T2 (pooled) (this relationship is significant by a chi squared with pooled T1+T2 pooled versus T5+T6 pooled, $\chi^2 = 14.6, p < 0.05$).
Self-reported factors

As in Experiment 1, participants in Experiment 2 completed a post-experimental survey. Once again, a stepwise logistic regression was run to identify which survey questions were reasonable predictors of a participant’s conversion. After eliminating variables that did not strongly affect the model’s fit (evaluated through AIC), there were no significant predictors from the survey that predicted how likely participants were to purchase the post-transaction offer. This is likely the case since the effect of treatment in this experiment is much stronger than the effect of treatment in Experiment 1. Therefore, the experimental treatment that participants were assigned is a much better predictor of their conversion rate than any of the survey factors. It is possible that some of these factors would be significant within various treatments. To test this, another regression was run, this time with the treatment effect interacting with each survey variable. This regression yielded exactly what was thought – the treatment effect is the only significant predictor, since it is so strong in this experiment (all interaction effects were overshadowed by the treatment effect).
Chapter 7

Discussion

It is clearly difficult to reduce the effects of harmful post-transaction offers from the first-party perspective. However, it is far from impossible to use first-party interventions effectively. Every treatment that offered participants an intervention had a lower conversion rate than the control treatment that did not offer any first-party intervention (however, only one of these relationships was significant). Therefore, warning participants before they encounter deceptive post-transaction offers does appear to be effective, though the warning needs to be designed in a thoughtful way.

Of the interventions offered, full-page ones were more effective at reducing conversion rates than on-page interventions (though these relationships are only marginally significant). This may be because full-page warnings interrupt consumers’ flow and cause them to more seriously evaluate the warning in front of them compared to an on-page warning.

The passive interventions offered in this experiment were less effective than the active one. Since participants that were shown the active intervention could not bypass the warning page by waiting alone (as in other treatments), they were more likely to read the warning text before clicking the button and therefore there were more informed, which likely led to their likelihood to purchase the post-transaction offer. This finding is consistent with the recommendation offered by the Restore Online Shoppers’ Confidence Act (U.S. House 2010), which prescribes the use of active interventions to consent to a purchase. It is possible that the passive interventions did not show a noticeable reduction in conversion rates due to the small size of the effect. Since the baseline effect is small in Experiment 1 (around 20% of participants converting), testing the reduction of this effect is difficult to do without extremely large sample sizes. However, the larger the sample size used, the greater the chance of overfitting the data. That is, the more
participants used (on any task), the greater likelihood of local noise becoming significant, and the results of the experiment becoming not generalizable. Therefore, to avoid overfitting, a large, but not huge, set of participants was used.

The main contribution of Experiment 1 was to evaluate the impact of interventions from the first-party perspective on reducing the harm of deceptive post-transaction offers on consumers. It seems that it is possible to use active interventions to reduce the damage, but that there may be a group that will purchase these offers, regardless of the negative impacts of the offers or how informed participants are. This group may simply believe that all Danish pastries come from Denmark or that all tissues are made by Kleenex. One can hardly blame sellers who end up “deceiving” this group (Cliffdale Associates 1983).

The interventions that were offered in Experiment 1 were only from the first-party perspective. This experiment assumed that regulators would have an easier time regulating first-party merchants than their advertising counterparts. It is disappointing to see that intervention from the first-party perspective is so difficult. It is possible to experimentally test additional types of active interventions and see if any of them would reduce the impact in a great way. However, this would be playing a game of catch-up – while it may be possible to design ever-better active interventions from the first-party perspective, these may only go so far.

A better solution in the long term is to address the market failure itself by correcting incentives (Beales et al. 1981). As previously discussed, third-party marketers do not, currently, have sufficient incentives to desist from tactics that are unscrupulous. Since their primary motive is profit, it is difficult to completely change how they act without a sufficiently strong change in incentives. Once these changes were identified, they could help inform policy.

But, until a suitable policy intervention is offered, it is useful to try to intervene from the perspective of the third-party marketers by modifying how offers are displayed and how consumers are subscribed to the offers. Experiment 2 systematically varied the data pass
arrangements and the process of gaining user consent (i.e. opt-in or opt-out). Sizeable samples were collected for six experimental treatments to help to test the effectiveness of public policy and self-regulatory efforts by marketers and online businesses. Specifically, in the United States, the “Restore Online Shoppers’ Confidence Act” (ROSCA) likely outlaws the opt-out treatments that were part of Experiment 2. Given that in opt-out treatments, the same number of participants (or greater) of participants purchased the post-transaction service compared to their opt-in counterparts, this policy direction appears partially effective.

Along with discouraging opt-out offers, ROSCA also takes a stance against the use of data pass arrangements. This study shows that this stance is a smart one, since the use of data pass in Experiment 2 led more people to purchase deceptive post-transaction offers in opt-in situations, which are the norm in e-commerce. And, if they have their way, deceptive marketers would use a combination of opt-out and data pass strategies to yield very high conversion rates, as shown in this experiment.

ROSCA also requires that the terms of the post-transaction offer be present before purchase. Experiment 2 finds that offers that disclose their use of data pass actually experience increased conversion rates compared to those that do not disclose the use of data pass. Therefore, the requirement of clear terms is insufficient. It is possible that the visual similarities between the first-party music store and the post-transaction offer, along with the presence of information given previously to the first-party merchant, led participants to believe that a second distinct transaction was not taking place. This is supported by the post-experimental survey that found that nearly 50% of the participants believed that first and third-party companies are a single entity, or they were unable to tell afterwards.
Chapter 8

Conclusion

Experiment 2 investigated the effects of different presentations of post-transaction offers on consumers’ likelihood to purchase these offers and examined strategies for reducing this likelihood. This experiment aimed to test the effectiveness of current regulatory policy and possibly point to new strategies that policy-makers can use. This experiment showed that opt-in offers are generally less harmful than their opt-out counterparts and consumers are likely to attempt to bypass offer pages with as little as possible effort. The key insights can be used by policy makers to affect change if and only if they are able to effectively regulate third-party marketers.

When a policy maker is not able to regulate the action of third-party marketers, as is often the case, the interventions offered from Experiment 2 are not very helpful. Experiment 1 understands that regulating marketers is much more difficult than regulating merchants. Therefore, it addressed the problem of intervention to reduce the harm of post transaction offers from the perspective of first-party merchants. First party merchants have incentives to keep their customers happy, which discourages the use of explicitly deceptive tactics. Third-party marketers, however, do not have strong incentives to be straightforward with their customers since customers do not visit the marketer’s offer of their own will (i.e. customers only see offers because first party merchants automatically redirect them to the offers).

Experiment 1 showed that reducing the number of people that purchase deceptive post-transaction offers, even in the most optimistic case, is a difficult task. Only one of the interventions was significant in reducing the amount of people that purchased the post-transaction offer. This result points to the idea that there may simply be a “bottom.” That is, there may be a certain group of people that are just going to purchase post-transaction offers. This bottom may
be defined by their true approval of the offer or by their refusal to integrate warning signs. Since it is unlikely that consumers truly like deceptive services (as evidenced by the relatively few positive ratings the post-transaction offer in this experiment received), the more likely explanation as to why some people still purchase these offers is that they refuse to integrate the warning signs about the deceptive post-transaction offer until it is too late. The open question is: what can or should be done about this group of people? It may be possible to “scrape the bottom” and continually reduce the number of people in this category by providing ever-refined interventions at ever-increasing costs (and every decreasing marginal effectiveness). However, it may be argued that only through mistakenly purchasing an unwanted offer will people from this group will move out of it for future transactions. Whatever the strategy, it is important to identify where the bottom lies and to grab all of the “low-hanging fruit” (i.e. to implement all reasonable interventions from either perspective that will significantly reduce the amount of people that fall for these deceptive offers). This thesis attempts to identify these interventions from both first-party and third-party perspectives.
Chapter 9

Future work

The experiments conducted within this thesis present some of the first work in the space of behavioral experiments regarding post-transaction marketing. Naturally, due to this, there are many next steps in this line of work. As with any experimental work, the results of this study cannot be appropriately generalized to similar situations without sufficient replication of the effects in these situations. There is no apparent reason for the effects to be different in different contexts, though reasons may arise while running additional studies. Similarly, the power of many of the statistical tests that were performed within this thesis was weakened by the exploratory nature of the study which required many tests to be run. Running more focused studies to replicate the effects found here would largely solve this problem. Once the effects presented in this thesis are recreated in similar situations and analyzed in a more focused way, they can be taken, with reliability, to policy-makers to inform future policies regarding deceptive post-transaction offers. It would also be beneficial to include in future studies additional control treatments. Specifically, it would be interesting to evaluate if the effects presented here are still present in experiments that present a post-transaction offer that is purely beneficial to participants. That is, it would be interesting to evaluate if the effects are the result of an offer being present, as opposed to the character of the offer. However, this process of replication and testing different situations is time-consuming and may not be immediately feasible given limited resources. However, technological solutions may help to mitigate the problem.

Specifically, using the preliminary results presented within this thesis, it is possible to design technological tools to better inform consumers when they face deceptive offers. As the results above show, if consumers are able to be adequately informed, it is possible to mitigate the problem of deceptive post-transaction offers. To create this type of tool, it would be necessary to
use concepts such as an extension to a web browser or a desktop program that monitors web traffic. In the realm of privacy policies, this type of research is not uncommon (e.g. Langheinrich 2002; Kolter and Pernul 2009), though similar studies based on the creation and evaluation of tools have no yet been run in the context of post-transaction marketing. This is a clear direction to affect change without the necessity of the rigor needed for policy recommendations, and may be a good start to mitigating the widespread problem of deceptive post-transaction offers.
References


Csikszentmihalyi M (1975) Beyond Boredom and Anxiety. Josey–Bass, San Francisco


US Senate (2009) Staff Report for Chairman Rockefeller, Aggressive Sales Tactics on the Internet and their impact on American Consumers, Committee on Commerce, Science, and Transportation.
Appendix A

Pre-experiment consent form

Title of Project: Music purchasing study

Principal Investigator: Alan Nochenson / 309 IST Building / University Park, PA 16802 / anochenson@psu.edu

Advisor: Jens Grossklags / 329A IST Building / University Park, PA 16802 / jensg@ist.psu.edu

1. Purpose of the Study: We aim to study music purchasing behaviors.
2. Procedures to be followed: To complete this study, you will be given $1.50 with which you have to purchase the single song from a music store. In the store, you may sample the songs and interact with the shopping environment. Afterwards, you will fill out a survey questionnaire.
3. Duration/Time: The whole process should take you about 10-15 minutes on average.
4. Statement of Confidentiality: Your participation in this research is confidential. In the event of a publication or presentation resulting from the research, no personally identifiable information will be shared.
5. Right to Ask Questions: Please contact Alan Nochenson at anochenson@psu.edu with questions or concerns about this study.
6. Payment for participation: For this study, we have given to you (in addition to the participation reward) a starting budget of $1.50. Using this budget, you have to purchase exactly one song in the music store. Any transaction in the shopping environment reduces your starting budget as described in the shopping environment. At the end of the study, you will receive any remaining money as a bonus payment.
7. Voluntary Participation: Your decision to take part in this research is voluntary. You can stop at any time. You do not have to answer any questions you do not want to answer. If this is the case, please abandon the Mechanical Turk HIT and exit the task.
8. You must be 18 years of age or older to consent to take part in this research study. If you agree to take part in this research study and to the conditions outlined above, please click the button below. If you do not consent, please exit the survey now and abandon the Mechanical Turk HIT.

I agree to the above terms
Appendix B

Post-experimental Survey

This survey that participants were required to fill out follows. Participants were required to answer every question, and questions were checked for valid answers (e.g. age had to be a two digit number).
Shopping questions

Which song did you purchase?  Select an option ▼

How often do you purchase music online?  Select an option ▼

How much do you spend on average per month on music purchases?  $

How often do you stream music online?  Select an option ▼

Was anything in this task disconcerting or troubling to you?  Select an option ▼

Why did you choose the above answer?

SafeDelivery questions

What does the SafeDelivery service do?

Did you purchase the SafeDelivery service?  Select an option ▼

How valuable is the SafeDelivery service to you?
  Not valuable • • • • • Very valuable

Why did you choose the above rating?

How valuable is the discount offered by the SafeDelivery service to you?
  Not valuable • • • • • Very valuable

What is the relationship between MelodiesFor.us and SafeDelivery?
  • MelodiesFor.us and SafeDelivery are the same site
  • MelodiesFor.us and SafeDelivery are partners
  • MelodiesFor.us and SafeDelivery are unrelated
  • I do not know what the relationship between MelodiesFor.us and SafeDelivery is.
### Online experience questions

How many times have you received the wrong product when ordering online?
- 0
- 1-2
- 3-4
- 5+

Have you ever received a product that you did not remember ordering?
- Yes
- No

Have you ever been the target of any kind of purchasing scam?
- Yes
- No

How concerned are you about your privacy online?
- Not concerned
- Very concerned

How concerned are you about your security online?
- Not concerned
- Very concerned

### Online experience questions

<table>
<thead>
<tr>
<th>Question</th>
<th>Select an option</th>
</tr>
</thead>
<tbody>
<tr>
<td>How often do you check your credit card statements?</td>
<td>▼</td>
</tr>
<tr>
<td>How often do you read privacy statements online?</td>
<td>▼</td>
</tr>
</tbody>
</table>
Personal Questions

Please indicate how much you agree with each of the statements below.

I feel safe when I am on the Internet.
   totally disagree  ○○○○○○○○  totally agree

I often find it peaceful to be online.
   totally disagree  ○○○○○○○○  totally agree

When I am online, I can be carefree.
   totally disagree  ○○○○○○○○  totally agree

My use of the Internet sometimes seems beyond my control.
   totally disagree  ○○○○○○○○  totally agree

When I am online, I don’t think about my responsibilities.
   totally disagree  ○○○○○○○○  totally agree

I am more careful purchasing things offline than I am online.
   totally disagree  ○○○○○○○○  totally agree

Additional questions

Please answer the following questions exactly as asked.

Please select the rightmost option.
   totally disagree  ○○○○○○  totally agree

Please select the middle option.
   totally disagree  ○○○○○○  totally agree

Please select the leftmost option.
   totally disagree  ○○○○○○  totally agree