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Millennial-Style Learning: Search Intensity, Decision Making, and Information Sharing

Bruce I. Carlin,^a Li Jiang,^b Stephen A. Spiller^a

^a Anderson School of Management, University of California, Los Angeles, Los Angeles, California 90095; ^b Heinz College, Carnegie Mellon University, Pittsburgh, Pennsylvania 15213

Contact: bruce.carlin@anderson.ucla.edu (BIC); ljiang@andrew.cmu.edu (LJ); stephen.spiller@anderson.ucla.edu (SAS)

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Abstract. The growing use of online educational content and related video services has changed the way people access education, share knowledge, and possibly make life decisions. In this paper, we characterize how video content affects individual decision making and willingness to share in the context of a personal financial decision. We find that distracting advertising curtails the time people invest in searching for the best alternative and causes worse decisions. Content geared toward giving better instructions helps to overcome this effect. Such actionable content improves both search quality and financial decisions. However, including such content may decrease sharing unless it is perceived to be sufficiently useful. As such, there is a potential risk to adding actionable content to videos. Our work has important implications for policies guiding financial literacy training, and it also has broader impact for education in the information age.

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Keywords: video • finance • learning • information sharing • distracting advertising

1. Introduction

Demand for online educational content and related video services has exploded over the last few years, suggesting that we are experiencing a global paradigm shift in the way people access education, share knowledge, and possibly make decisions. According to YouTube, over one billion unique users access their content every month, and more U.S. adults 18–34 years of age access it than any individual cable network.¹ But what makes this outlet particularly powerful for directed education are its information-sharing capabilities and user engagement. Especially considering the growth of Facebook and Twitter, the impact of video education is growing as people of all ages are adopting this form of learning.

YouTube is not merely a tool for entertainment. Rather, people are becoming more informed through this channel and are actively sharing their knowledge with others. In 2013, 50% of adult Internet users watched educational videos, 56% viewed “how-to” content, and 30% posted educational or tutorial videos of their own.² Additionally, one-third of millennials engaged with educational videos on the platform (e.g., comments, likes, favorites on playlist creation).³ So while comedy and entertainment are clearly a part of the YouTube experience, this channel has become a major outlet for self-education and information sharing.

This suggests video content is a potentially useful channel to influence domain-specific literacy and

decision making of our population. In the best-case scenario, useful content goes viral or reaches a large promoter. This is not only interesting for academics but an opportunity for policy makers to improve the decisions that people make. Regrettably, the interplay between sharing media and taking the action advocated by that media has been largely overlooked by both.⁴

In this paper, we investigate how video content affects individual decision making and willingness to share, and how its efficacy is impacted by other sources of competing information. We study this in the context of a personal financial decision, while keeping in mind that the lessons from our work likely apply to other decision contexts. Addressing poor financial literacy is clearly a first-order concern, but efforts to ameliorate this problem have not taken advantage of learning via sharable online videos. Indeed, it appears that “just-in-time” financial education may be superior to traditional channels (Fernandes et al. 2014), videos may be superior to other methods of delivering financial literacy training (Lusardi et al. 2014, Heinberg et al. 2017), and vicarious learning through entertainment can be quite effective (Berg and Zia 2013).

The domain for our study is the market for credit cards. This is a setting in which hidden fees and price complexity are frequently used by financial institutions (e.g., Gabaix and Laibson 2006, Carlin 2009, Carlin and Manso 2011), and consumer sophistication drives both borrowing costs and the resulting savings rates

(e.g., Stango and Zinman 2015). Indeed, new regulations have been targeted at ameliorating this problem (e.g., Bar-Gill and Warren 2008, Agarwal et al. 2014), which has had some positive impact on consumer welfare (Agarwal et al. 2015). While several studies have focused on using reminders and focusing attention in consumer markets to achieve better outcomes (Karlán et al. 2016, 2015), little emphasis to date has been placed on using sharable online media to improve financial literacy and decision making.

We began our study by producing our own video, which is a cartoon in which a TV viewer uses a “magic remote” to uncover hidden messages while watching a credit card commercial.⁵ Instead of cherry-picking videos that already existed on YouTube, we made this investment so that we could control the content of the video and produce variation by creating two versions that differed in particular ways. Moreover, doing so removed the concern that subjects could have previously viewed our videos before participating in our experiments.

Both versions of the video contained three main messages: (1) beware of credit card fees, (2) interest rates may not be fixed, and (3) the credit limits may not be specified but do exist. When we created the videos, we incorporated elements from Heath and Heath (2007) to maximize the probability that people would share our video. As such, the videos were meant to be simple, humorous, engaging, and concrete, and they were meant to tell a story. Before conducting our main experiment, we pilot-tested the videos and confirmed them to be perceived by subjects to be sharable, enjoyable, and useful.

In our main experiment, 1,603 subjects first viewed a version of the video and subsequently were asked to choose one of four credit cards from an online offering. One of the credit cards was in fact the dominant choice, based on its interest rate, fees, and credit limit. All four credit cards were presented on a single page, with links to reveal key pricing and terms. This allowed us to keep track of how much time subjects spent analyzing prices and how many clicks they made before making a choice. Following the credit card choice, subjects were asked whether they wished to share the video with others (Berger 2011).

The study used a 2 × 2 between-subjects design. Subjects were randomized between viewing our baseline video and our treatment video, which was the baseline video plus an additional tag on the end that included both a summary of the three main messages and a segment that explained where to locate these pieces of information in a typical credit card pricing and terms pamphlet. Subjects were also randomized based on whether the credit card page included distracting advertising or not. Distracting advertising was communicated by labeling the credit cards with statements

such as “no annual membership fee” or “0% introductory APR,” even though all of the cards available had these same terms. As such, the advertising was truthful in an absolute sense but misleading in a relative sense: the labels appeared to be more diagnostic than they actually were. Subjects who were not treated with distracting advertising did not view labels at all on their credit card offerings.

Viewing the tagged video increased the choice of the best credit card, and distracting advertising led to worse decisions. These results confirm that subjects appeared to understand their tasks and take the study seriously. Participants treated with distracting advertising spent less time making the decision. By contrast, subjects deployed more attention upon seeing the tagged video, but only if they chose in the presence of distracting ads: advertising crowded out active information acquisition unless there were concrete instructions to follow. This finding reinforces the notion that a clever informative video is not enough: the information given must also be actionable.

In addition to these effects on choice and amount of attention, there were important differences in relative focus of attention. Participants allocated relatively more attention to the dominant card if they saw the tagged video than if they saw the baseline video. This improved allocation of attention led to higher choice quality with the tagged video.

Just as important, subjects perceived the tagged video to be more useful, and this increased the likelihood that subjects would apply for a credit card in the future. However, after controlling for perceived effectiveness, subjects were less inclined to share the tagged version than the baseline video. Therein lies the fundamental problem for encouraging good financial decisions through social media: the very videos that have the greatest potential to be useful and increase decision quality may also be the ones that are the least shareable. It is possible that actionable videos may not necessarily simultaneously increase individual information acquisition and percolation of “good” information in the marketplace.

Finally, distracting advertising not only decreased search intensity and choice quality but also affected the ability of people to recall the terms of their choices. At the end of the survey, we asked subjects what the terms of their chosen card were, as well as the range of terms among all of the options. Subjects who were treated with distracting advertising had significantly worse recall. This result implies that distracting advertising may be a pernicious cause of why many consumers in retail markets do not know the terms of their credit agreements.

On the basis of these results, our study yields several novel insights. First, online videos do have the potential to increase the quality of household financial

decisions, but merely presenting the information in an engaging sticky format is not sufficient. The information must be interpretable and implementable to direct attention appropriately. Second, effective does not necessarily mean sharable. Last, video content and competing information such as distracting advertisements affect choice through their respective effects on the amount and allocation of attention, which may lead to multiple effects on sharing. Factors that increase perceived effectiveness alone without affecting properties of the video may increase the likelihood of sharing, but an ineffectively tagged video that does not enhance perceived effectiveness could actually decrease sharing.

2. Method and Design

2.1. Video Production

The storyline and production of the video were accomplished with a professional animator. Our goal was to make the video informative for a common but important life choice, but also to make it entertaining enough to make it worth watching and sharing with others. We chose the domain of credit card traps because we felt it would be relevant to a broad cross section of the population. In choosing the story line and developing the video, we focused on Heath and Heath's (2007) features of "stickiness" to maximize its potential for effectiveness and longevity of effects.

The animated video leads the viewer through a story from a first-person perspective. The main character watches a credit card commercial on television and discovers a "magic remote" on his coffee table that allows him to uncover hidden captions in the commercials, see what the spokesman in the commercial is hiding by flipping the perspective of the camera, and detect hidden messages when rewinding the video.

The video is approximately two minutes long and conveys three basic points about credit cards. The first is that "no preset spending limit" is not the same as "no spending limit." This is a concern because consumers may unknowingly attempt to spend beyond their limit, incurring additional fees in the process. The second is that there are a lot of hidden fees that can add up. The third is that "fixed APR" does not necessarily mean an APR that cannot change.

In our experiment, we presented one of two different versions of the video to each subject. The first version, which we call our *baseline* video, is the stand-alone story as described above. The basic idea was to convey the three primary messages humorously, much like many popular online videos. The second version, which we call the *implemental* video, included a short addition to the end of the baseline video, which was composed of a recap of the three main messages and a schematic of where to find key information on the standard pricing and terms document that typically accompanies

credit card offers (see Figure 1). The implemental version was designed to make it crystal clear to the viewer exactly how to use the information that was contained in the baseline video. Indeed, previous research indicates that many consumers are not able to act on information like this if it is not clear how to use it (e.g., Beshears et al. 2013).

Before performing the experiment as described in detail below, we ran pretests to determine how the videos are perceived. Pretesting indicated that participants from our subject population found the videos to be engaging and shareable. It also indicated that the recap and the implementation schematic had additive effects on video effectiveness. As such, we used the combination of the two in our implemental video. This enabled a large enough effect size to observe whether it was moderated by other factors, but it also meant that the observed effects are the result of the combination of summary and implementation instructions.

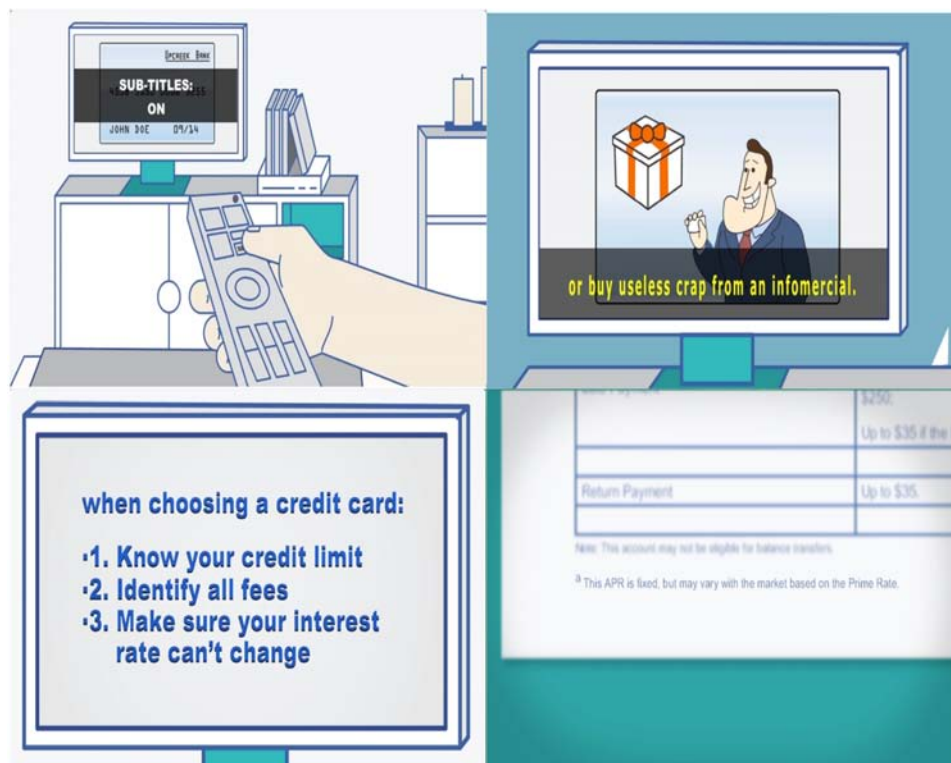
2.2. Credit Card Choice

After viewing a video, participants in the study made a hypothetical credit card choice from a website that we constructed to emulate real online websites.⁶ Figure 2 is a screenshot from our experiment. Subjects were asked to choose from among four credit cards. The initial screen had only cursory information about the four credit cards but had a "Pricing & Terms" link below each card. Using the links, participants could seek out diagnostic information such as APRs, fees, and spending limits in order to compare the terms offered from the various cards. Clicking on the "Pricing and Terms" link led subjects to view a standardized form similar to the ones typically used in online credit card offers (see Figure 2).

On the basis of the factors emphasized in the video, one of the credit cards was the dominant choice. That is, it was strictly better than the other three cards in at least one dimension and at least as good in all of the others. The position of the dominant card on the screen was randomized throughout the study. The only way to learn which card was the dominant choice, however, was to uncover and compare the pricing and terms of all four credit cards. As such, choice of the dominant card served as one dependent variable of interest and indicated high choice quality.

Since we were able to observe when subjects clicked on each link, how long they spent examining each term sheet, and the number of total clicks they used, we could estimate the effort subjects used to acquire information about their decision. It also allowed us to record where participants directed their attention and identify whether subjects simply rushed through the experiment.

Because consumers frequently have to contend with competing information when they make real decisions,

Figure 1. (Color online) Video Screenshots

Notes. The top left panel shows the video protagonist using the magic remote to turn on subtitles. The top right panel shows the subtitles that are displayed. The bottom left panel shows the recap. The bottom right panel shows the implementation instructions of how to act on that information. The tag, portrayed via the two bottom panels, was not shown to the baseline participants.

we chose to study a particularly pernicious source: distracting advertising. Indeed, as in the everyday consumer environment, the advertising may not be technically wrong, but it is misleading to consumers as it makes a product sound more attractive than it really is. In our study, we examined the effect of this competing information by assigning some participants to see no advertising and some to see relatively minor misleading advertising in the form of superfluous taglines associated with each credit card in the choice phase of the study. The taglines that we used were randomly assigned to the credit cards throughout the study. It is important to note that these taglines were not necessarily false, but they appeared to be more diagnostic than they truly were. For example, one card was labeled “No Annual Membership Fee,” which was true in an absolute sense. However, since all of the cards, in fact, had no membership fee, it was potentially misleading in a relative sense. Figure 3 contrasts the two versions of the screenshots that subjects viewed before making their choice. The top panel of Figure 3 shows a case with taglines, whereas the middle panel shows the case with no added information. The bottom panel shows a sample of a real offering in the marketplace.

2.3. Design

Two key factors were manipulated in the experiment in a 2 (Video: Baseline, Implemental) \times 2 (Advertisements: No Ads, Superfluous Ads) between-subjects experimental design. The Baseline video provided an entertaining presentation of three credit card traps. The Implemental video gave a brief recap and additional guidance after the Baseline video regarding where to find the information embedded in a pricing and terms disclosure. In the Superfluous Ads condition, when consumers were choosing a credit card, they saw taglines for each of four cards (“Minimum Payment Only \$10/month,” “0% Introductory APR,” “No Annual Membership Fee,” and “No Foreign Transaction Fee”). These taglines were entirely superfluous and distracting, as all statements applied equally to all of the cards. Association of superfluous statement with card terms was randomized across participants. In the No Ads condition, there were no taglines associated with any card.

2.4. Procedure

Participants began by watching one of the two videos, randomly assigned depending on the condition. After viewing the video, participants chose one credit card from a set of four. Each credit card was identified by

Figure 2. (Color online) Choice Stimuli Used in the Study

Now, suppose that you need to apply for a new credit card. You've received the four card offers below from four different issuers. Each one is accompanied by a description from that card's issuer. Which one would you choose?

Third National Bank  Minimum Payment Only \$10/month Pricing & Terms	Continental Bank  No Annual Membership Fee Pricing & Terms	Liberty Bank  0% Introductory APR Pricing & Terms	Partners' Bank  No Foreign Transaction Fee Pricing & Terms
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Click the Third National Bank Pricing & Terms link again to hide this information.

Pricing & Terms (Third National Bank)
 Please take a moment to carefully review the Pricing & Terms below.

Pricing Information

INTEREST RATES AND INTEREST CHARGES	
Purchase Annual Percentage Rate (APR)	0% Intro APR for the first 12 billing cycles that your account is open. After that, 13.99% ^a .
Balance Transfer APR	13.99% ^a
	29.99% ^a
Penalty APR and When It Applies	The Penalty APR will be applicable to your Account if you: <ul style="list-style-type: none"> • fail to make any Minimum Payment by the date and time due (late payment); • exceed your credit limit (if applicable); • make a payment to us that is returned unpaid.
How to Avoid Paying Interest on Purchases	Your due date will be a minimum of 21 days after the close of each billing cycle. We will not charge you periodic interest on purchases if you pay your entire balance by the due date each month.
Minimum Payment	\$10/month
Spending Limit	\$700

FEES	
Annual Membership Fee	\$0/year
Activation Fee	\$60
Foreign Transaction Fee	\$0
Penalty Fees	
Late Payment	Up to \$15 if the balance is less than \$100; Up to \$25 if the balance is \$100 to less than \$250; Up to \$35 if the balance is \$250 or more.
Return Payment	Up to \$35.

Note: This account may not be eligible for balance transfers.
^a This APR is fixed and will not vary.

Third National Bank
Continental Bank
Liberty Bank
Partners' Bank

Notes. The taglines (e.g., “No Annual Membership Fee”) were excluded in the “No Ads” condition. The pricing and terms information was only shown if participants clicked on “Pricing & Terms” under a card. If participants clicked on the pricing and terms hyperlink for one card while the screen displayed the information for a different card, the information would change to display the new information.





the issuing bank’s name (“Third National Bank,” “Continental Bank,” “Liberty Bank,” and “Partners’ Bank”), a picture of the credit card, and a “Pricing & Terms” hyperlink that revealed pricing and terms below the card display when clicked. Participants in the Superfluous Ads condition also saw taglines for each card. The survey recorded which option participants chose, how many times they viewed the pricing and terms for each card, and the amount of time spent viewing the pricing and terms for each card.⁷ The bank names and card images were always presented in the same order

from left to right. The specific taglines in the Superfluous Ads condition and the details of the pricing and terms that defined the substantive differences between cards were independently randomized across position so that different taglines were associated with different terms, and each was associated with different banks for different participants.





The relevant differences between the cards are given in Table 1. Matching the messages in the video, the dominant card weakly had the lowest APR, had a defined preset spending limit, had a fixed APR, and




Figure 3. (Color online) Credit Card Stimuli

Now, suppose that you need to apply for a new credit card. You've received the four card offers below from four different issuers. Each one is accompanied by a description from that card's issuer. Which one would you choose?

 Third National Bank Minimum Payment Only \$10/month Pricing & Terms	 Continental Bank No Annual Membership Fee Pricing & Terms	 Liberty Bank 0% Introductory APR Pricing & Terms	 Partners' Bank No Foreign Transaction Fee Pricing & Terms
Third National Bank	Continental Bank	Liberty Bank	Partners' Bank

Now, suppose that you need to apply for a new credit card. You've received the four card offers below from four different issuers. Which one would you choose?

 Third National Bank Pricing & Terms	 Continental Bank Pricing & Terms	 Liberty Bank Pricing & Terms	 Partners' Bank Pricing & Terms
Third National Bank	Continental Bank	Liberty Bank	Partners' Bank

 Chase Freedom® <ul style="list-style-type: none"> ✓ Earn a \$100 Bonus after you spend \$500 in your first 3 months from account opening. ✓ 5% Cash Back on up to \$1,500 spent in bonus categories each quarter ✓ No Annual Fee[†] <input type="checkbox"/> Compare <input type="button" value="Learn More"/> <small>[†]Pricing & Terms</small>	 Chase Slate® <ul style="list-style-type: none"> ✓ 0% Introductory APR for 15 months[†] ✓ \$0 Balance Transfer Fee when you transfer a balance during the first 60 days your account is open[†] ✓ \$0 Annual Fee[†] <input type="checkbox"/> Compare <input type="button" value="Learn More"/> <small>[†]Pricing & Terms</small>	 CHASE SAPPHIRE PREFERRED® <ul style="list-style-type: none"> ✓ 40,000 bonus points after spending \$3,000 in the first 3 months from account opening ✓ 2X Points on travel and dining ✓ \$0 Intro Annual Fee, after that \$95[†] Cardmember Stories <input type="checkbox"/> Compare <input type="button" value="Learn More"/> <small>[†]Pricing & Terms</small>
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Notes. The top panel shows the superfluous ads condition. The middle panel shows the no ads condition. The bottom panel shows the similar offering from Chase.com.

had a low activation fee. Table 1 also shows that all four cards had a 0% introductory APR, the same minimum monthly payment, no annual membership fee, and no foreign transaction fee. As such, the distracting taglines were true in an absolute sense. However, they provided no useful diagnostic information to make relative comparisons between the cards.

After the credit card choice, the survey assessed sharing. We assessed sharing using the measures reported by Berger (2011). Participants reported *willingness to share* and *likelihood of sharing* the video with

friends, family members, and coworkers on seven-point scales (from 1 = not at all to 7 = extremely). These items were each intended to be measures of the same underlying construct, the propensity to share the video. As they were each a noisy indicator of the same underlying construct, these six items were combined into a single sharing scale to reduce noise, as in previous work (Berger 2011).

We next measured how effective participants thought the video was and how confident they were in their choice. Participants responded to four items

Table 1. Critical Terms in Pricing and Terms Shown to Participants

Panel A				
Terms that vary across cards	Dominant card	High APR card	Unfixed APR card	High fee card
Purchase APR (%)	13.99	14.99	13.99	13.99
Spending limit (\$)	700	Variable	700	700
Activation fee (\$)	60	60	60	110
APR footnote	This APR is fixed and will not vary.	This APR is fixed and will not vary.	This APR is fixed, but we reserve the right to unilaterally change the APR for any reason with written notice.	This APR is fixed and will not vary.

Panel B				
Terms referenced by taglines do not vary	“0% introductory APR”	“Minimum payment only \$10/month”	“No annual membership fee”	“No foreign transaction fee”
Intro APR (%)	0	0	0	0
Minimum payment (\$)	10/month	10/month	10/month	10/month
Annual membership fee (\$)	0/year	0/year	0/year	0/year
Foreign transaction fee (\$)	0	0	0	0

Notes. Participants saw the entire pricing and terms sheet (see Figure 2); this table shows the critical terms. Panel A shows the terms that varied across cards, with the dominated terms based on the video in bold. Panel B shows the taglines and terms referenced by the taglines to show that the taglines were superfluous.

measuring choice efficacy and video effectiveness on a seven-point scale (1 = strongly disagree to 7 = strongly agree). These items were (1) “I am confident that I picked the best credit card,” (2) “Choosing the best credit card was easy,” (3) “The video helped me make my choice more efficiently,” and (4) “The video would help my best friend make the right credit card choice.” As each of these items was intended to be a measure of the same underlying construct, perceived effectiveness, we also combined these items into a single scale to reduce noise.

We next measured how participants would describe the video along several dimensions, drawing from the Olney et al. (1991) measures of advertisements: special (*Peculiar–Ordinary, Just like any other video–Different from any other video, Average–Special, Weird–Normal, Nothing special–Outstanding*), hedonic (*Unpleasant–Pleasant, Fun to watch–Not fun to watch, Not entertaining–Entertaining, Enjoyable–Not enjoyable*), utilitarian (*Important–Not important, Informative–Uninformative, Helpful–Not helpful, Useful–Not useful*), and interesting (*Makes me curious–Does not make me curious, Not boring–Boring, Interesting–Not interesting, Keeps my attention–Does not keep my attention*). These measures allowed us to assess how the videos were perceived and whether the implemental addition to the baseline video changed the assessment of that video.

To ensure that our stimuli were relevant for our sample, participants reported how frequently they share videos through each of several channels (Facebook, email, Twitter, Google+, Instagram, or other) and whether they have a credit card and, if so, how many.

The choice among four credit cards assumed participants would apply for a card, so we also measured the extent to which the video made participants more or less likely to apply for a credit card.

We included several measures of consumer memory for the video and reactions to the video, as well as a verification check that what we defined a priori as the best option was selected by participants when all relevant differentiating information was made explicit. First, participants were asked to report, to the best of their abilities, memory for APRs, activation fees, and membership fees. For each one, they reported the remembered value for the card they chose, the lowest value in the set, and the highest value in the set. Second, participants made a choice among four cards when all extraneous information was stripped away and all of the differentiating information from pricing and terms was made explicit. Third, participants reported an open-ended description of the main themes from the video and self-graded that description against five possible themes, including three themes that were part of the video, one that was not a theme in the video (i.e., a foil), and one “none of the above.”

Finally, participants described their reactions to the video in their own words and provided basic demographic information (age, sex, ethnicity, education, and income).

2.5. Participants

Table 2 summarizes the demographic data of our subjects. A total of 1,603 participants (753 women) recruited on Amazon Mechanical Turk completed

Table 2. Sample Characteristics: Frequency by Various Demographic Categories

	N	%
Age range		
18–24	363	22.6
25–34	696	43.4
35–44	273	17.0
45–54	154	9.6
55–64	87	5.4
65–85	26	1.6
Unknown	4	0.2
Income range		
<\$25K	375	23.4
\$25K–\$50K	501	31.3
\$50K–\$75K	346	21.6
\$75K–\$100K	197	12.3
\$100K–\$150K	137	8.5
>\$150K	47	2.9
Education		
High school	181	11.3
Some college	496	30.9
Two-year degree	179	11.2
Four-year degree	550	34.3
Master's	151	9.4
Doctoral	14	0.9
Professional	32	2.0
Sex		
Male	850	53.0
Female	753	47.0
Number of credit cards		
Zero	411	25.6
One	479	29.9
More than one	710	44.3
Unknown	3	0.2
Video-sharing frequency		
Never	161	10.0
Less than once per month	305	19.0
Once per month	215	13.4
Two to three times per month	276	17.2
Once per week	197	12.3
Two to three times per week	227	14.2
Once per day	222	13.8

this study.⁸ Ages ranged from 18 to 80 (excluding one implausible response), with a median age of 30. Median income fell between \$25,000 and \$50,000, and median education was some college but not a four-year degree. As our study focused on sharing of videos via social media, it was important to assess whether this action was relevant to our subject population. Indeed, 57% of the sample reported sharing a video via email or social media at least two to three times per month. Nearly three-quarters reported having a credit card; of those, 60% had more than one credit card.

3. Results

Before discussing our results, it should be noted that in our regression analyses, we did not code our treatment conditions with a dummy variable (1, 0). Rather,

we coded Video as 0.5 for Implemental and -0.5 for Baseline and Ads as 0.5 for Superfluous and -0.5 for No Ads. In a balanced design, this choice is equivalent to the use of dummy codes that have been mean-centered.⁹ Our use of $(-0.5, 0.5)$ contrast codes allows for direct interpretation of main effects of experimental treatments while still allowing for interaction effects in the model (e.g., Irwin and McClelland 2001, Spiller et al. 2013). Under this coding scheme, the coefficient on Video may be interpreted as the effect of video averaged across ad conditions, and the coefficient on Ad may be interpreted as the effect of ad averaged across video conditions. If we were to use dummy codes instead, the coefficient on Video would represent the effect of Video only for those in the No Ads group, and the coefficient on Ads would represent the effect of Ads only for those in the Baseline video group. As such, it would be impossible to readily interpret the significance of the main effects from the summary output of such a model. Of course, the full model fit does not depend on this linear transformation.

3.1. Comprehension and Memory Checks

Comprehension and memory varied by condition. We considered three measures: choice of the dominating option when all diagnostic information from pricing and terms is explicit, memory for attribute ranges, and self-graded free recall of memory of video information.

3.1.1. Explicit Choice. During explicit choice, all diagnostic information was highly salient because all other cluttering information was removed and no action had to be taken to reveal it. The vast majority of participants (83%) chose the card that we specified a priori as the dominant card. The choice share for each pricing and terms structure is shown in Table 3. Logistic regression revealed that neither ads nor the interaction of ads with video impacted explicit choice (p 's > 0.3), whereas video did ($B = 0.427$, $SE = 0.134$, $z = 3.188$, $p = 0.001$). Notwithstanding, for both videos, the proportion choosing the dominant card was very high (Traditional: 80%; Implemental: 86%). This finding underscores the importance of simplicity in improving the financial decisions that people make in retail markets.

Table 3. Card Choice When All Differentiating Terms Were Made Explicit and All Common Terms Were Hidden

Dominant	82.7%
High APR	9.9%
Unfixed APR	4.4%
High fee	3.1%

Note. Most participants chose the dominant card.

Table 4. Recall Accuracy for High and Low Terms As Well As Terms of Chosen Card

	N	Lowest value (%)	Chosen value (%)	Highest value (%)
Memory for APR	1,603	46.2	48.0	34.7
... Chose dominant	783	56.7	61.6	43.6
... Chose high APR	262	32.4	17.2	24.4
... Chose unfixed APR	397	44.1	50.4	31.2
... Chose high activation fee	161	23.0	26.1	16.8
Memory for activation fee	1,603	50.3	54.3	40.7
... Chose dominant	783	60.3	66.9	51.2
... Chose high APR	262	35.1	41.6	29.8
... Chose unfixed APR	397	53.9	57.7	39.3
... Chose high activation fee	161	18.0	5.6	10.6
Memory for membership fee	1,603	64.3	63.3	25.5
... Chose dominant	783	66.2	66.8	28.6
... Chose high APR	262	63.7	59.2	22.5
... Chose unfixed APR	397	65.0	64.2	25.7
... Chose high activation fee	161	54.7	50.3	14.9

Notes. Top row within each cell presents overall proportion correct; subsequent rows present accuracy conditional on choice. Note that participants who chose the dominant card have better memory across the board, and participants who did not choose the dominant card have particularly poor memory for the critical attribute of the card they chose. That is, participants who chose the high APR card have particularly poor memory for the APR of the card they chose, and participants who chose the high activation fee card have particularly poor memory for the activation fee of the card they chose. Wrong responses include those that were not in a readable format (e.g., a text explanation). A small percentage of those may be coded as right by a more generous scoring rule, but these are unlikely to systematically vary across conditions.

3.1.2. Memory of Card Attributes. Memory was assessed for each level (low, chosen, and high) of each attribute (APR, activation fee, membership fee). There were nine items in total, which are shown in Table 4 along with recall accuracy by item and conditional on card choice. Overall memory (the sum of nine indicator variables for whether an item was answered correctly or incorrectly) was higher by about one-third of an item after viewing the Implemental video than the Baseline video ($B = 0.347$, $SE = 0.142$, $t(1,599) = 2.444$, $p = 0.015$) and lower by about one item after viewing Superfluous Ads rather than No Ads ($B = -0.971$, $SE = 0.142$, $t(1,599) = -6.839$, $p < 0.001$). These factors did not interact ($p > 0.5$). This implies that distracting advertisements may be a cause of why the majority of consumers in credit card markets fail to remember the terms of their agreements. Moreover, as we shall see below, this is likely due to lower-quality search rather than cognitive load.

3.1.3. Memory of Video Themes. Participants were asked to provide an open-ended recall of the major themes from the video. They then self-graded their own open-ended responses. The proportion reporting each theme (including the foil theme) is shown in Table 5. We assessed memory for video themes as the sum of three indicator variables reflecting the key themes in the video minus the foil item that reflected a theme that was not present in the video. Memory was greater for the Implemental video than the Baseline video by about one-third of an item ($B = 0.387$, $SE =$

0.048 , $t(1,599) = 8.140$, $p < 0.001$). Neither Ad nor the interaction of Ad with Video had an effect (p 's > 0.4).

3.2. Attention and Choice

Attention paid to the pricing and terms of each card was operationalized in two ways: *the number of views of pricing and terms* and *the amount of time spent viewing pricing and terms*. Each of these variables exhibited a severe positive skew, so each was subjected to a natural log-transform (after adding 1 to account for 0s). There are two manners in which attention to pricing and terms could be affected by the experimental manipulations. First, the total amount of attention paid to pricing and terms could be affected. This would appear as common shifts to amount of time or number of views across all cards. Second, the way attention is allocated to pricing and terms of the dominant versus the other cards could be affected. This would appear as a differential shift to the amount of time or number of views of the dominant card to a greater or lesser extent than

Table 5. Self-Scored Memory for Key Themes in the Video

	Recall (%)
“Know your credit limit”	41.6
“Identify all fees”	79.7
“Make sure your interest rate can’t change”	67.7
“Find the card offering the most miles” (foil)	2.6
None of the above	10.3

Note. The fourth theme (regarding miles) was a foil and did not appear in the video.

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Table 6. Summary Statistics of Choice and Attention (Quartiles of Time Spent Viewing Pricing and Terms and Number of Views of Pricing and Terms)

	Chosen (%)	Time (s) (25th %ile)	Time (s) (50th %ile)	Time (s) (75th %ile)	Views (25th %ile)	Views (50th %ile)	Views (75th %ile)
Dominant card	48.9	1.00	17.00	33.65	1	2	5
High fee card	10.0	1.40	11.90	22.10	1	2	3
Unfixed APR card	24.8	2.20	14.50	28.55	1	2	4
High APR card	16.3	1.50	12.20	23.20	1	2	3.5
First card	26.2	2.70	18.90	26.63	1	2	4
Second card	25.0	2.40	14.30	18.58	1	2	5
Third card	24.8	0.95	11.10	15.94	1	2	4
Fourth card	24.0	0.00	12.00	16.72	0	1	3
0% intro APR	27.3	0.00	10.70	23.30	0	1	3
Minimum payment	17.9	0.00	9.40	22.50	0	1	3
No membership fee	42.4	0.00	12.00	27.40	0	1	3.5
No foreign transaction fee	12.4	0.00	7.40	22.20	0	1	3

Notes. The top four rows show results based on the structure of pricing and terms. The second set of four rows show results based on card position (from left to right). The third set of four rows show results based on the superfluous tagline, among those in the superfluous tagline condition. %ile, percentile.

the other cards. To capture these potential effects, we calculated two measures for each variable: the average across cards (with each card receiving equal weight) and the difference between cards. The averages served as proxies for *total attention deployed* in evaluating pricing and terms. Differences were calculated between the transformed value for the dominant card and the average transformed value across the other three cards. These differences served as proxies for the *allocation of attention* to the best card. Positive numbers indicated relatively more attention was allocated to the dominant card, whereas negative numbers indicated relatively more attention was allocated to the nondominant cards. The resulting attention measures were analyzed by regressing each measure on Video, Ads, and their interaction.

Distributional information for choice, amount of time spent viewing pricing and terms (in seconds), and number of views of pricing and terms for each card are given in Table 6. The dominant card was chosen nearly 50% of the time, with the unfixed APR card chosen 25% of the time, the high APR card chosen more than 15% of the time, and the high fee card chosen 10% of the time. The position of the cards did not significantly affect choice.

The superfluous taglines also impacted choice. A card labeled as having no membership fee was chosen 40% of the time, even though none of the cards had membership fees. Choice shares were lower than 25% for cards labeled as having no foreign transaction fees (12%) or low minimum payment (18%), despite actual fees and minimum payments remaining constant across cards. Time spent inspecting pricing and terms and views of pricing and terms largely tracked choice.

Full regression results are given in Tables 7 and 8, and means are graphed in Figures 4 and 5. Using either

metric (time or views), participants paid more attention when they saw no ads (versus superfluous ads; about seven seconds and 1.2 views per card difference) and when they saw the implemental video (versus the baseline video; about three seconds and 0.2 views per card difference). These effects are given by the coefficients on Video and Ads in the tables. Each of these effects was qualified by a significant interaction. As shown in the conditional effects tables, participants only deployed more attention upon seeing the implemental video if they chose in the presence of distracting ads (*Video | Ads* row). In the absence of distracting ads, the amount of attention was significantly smaller and did not vary (by views) or varied only very slightly (by time, where the difference was marginally significant; *Video | No Ads* row).¹⁰ No matter which video participants saw, they deployed less attention in the presence of superfluous ads, though to a lesser extent after viewing the implemental video (*Ads | Implemental* row versus *Ads | Baseline* row). These results held for both average views and average time as proxies for attention. Advertising, even though it was superfluous, crowded out active information acquisition unless there were concrete instructions provided by the video to follow instead.

Just as important, the implemental video enabled participants to better direct their attention. Using either the difference in log number of views or the difference in log amount of time spent as a proxy for attention, participants allocated relatively more attention to the dominant card if they saw the implemental video (about 30% more time) than if they saw the traditional video (about 14% more time). This is evident in the pattern of means of attention to each card and is tested by examining the difference between the dominant card and the other cards. Superfluous ads decreased the difference (16% more time versus 26% more time). Even

Table 7. Effects of the Experimental Manipulations on Amount of Attention and Allocation of Attention

Primary analysis								
Antecedent	Average ln(Seconds + 1)				Difference in ln(Seconds + 1)			
	Coeff.	SE	t(1,599)	p	Coeff.	SE	t(1,599)	p
<i>Constant</i>	2.246	0.033	68.617	<0.001	0.191	0.016	11.968	<0.001
<i>Video</i>	0.303	0.065	4.635	<0.001	0.124	0.032	3.881	<0.001
<i>Ads</i>	-0.712	0.065	-10.870	<0.001	-0.078	0.032	-2.455	0.014
<i>Video × Ads</i>	0.266	0.131	2.034	0.042	-0.057	0.064	-0.896	0.370
Conditional effects								
Antecedent	Average ln(Seconds + 1)							
	Coeff.	SE	t(1,599)	p				
<i>Video No Ads</i>	0.170	0.092	1.842	0.066				
<i>Video Ads</i>	0.437	0.093	4.708	<0.001				
<i>Ads Baseline</i>	-0.845	0.093	-9.121	<0.001				
<i>Ads Implemental</i>	-0.578	0.093	-6.250	<0.001				

Notes. Averages reflect average time used across the four cards. Differences reflect differences in time between the dominant card and the average of the other three cards. Because there was an interaction between the two experimental factors for averages, we also show the conditional effects for each factor at each level of the other factor.

though the information regarding pricing and terms was available for all of the participants, the extra content in the video helped the treated subjects to focus their attention on the important information.¹¹

We also examined how the choice of the dominant card varied as a function of the type of video, advertisements, and their interaction via logistic regression.

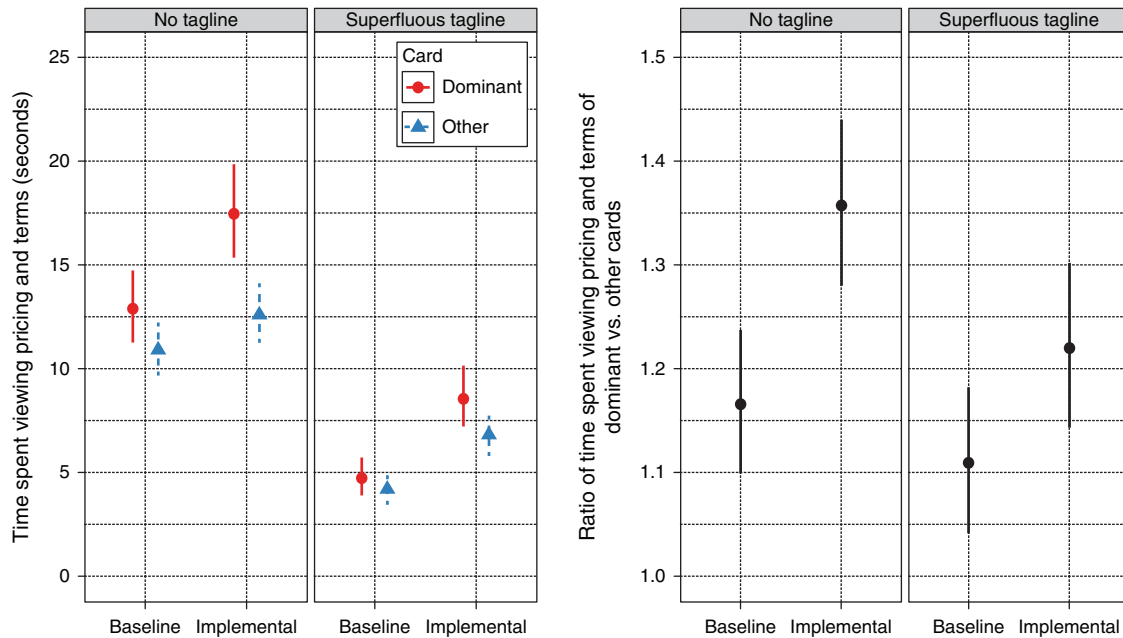
Those who saw superfluous ads were less likely to choose the dominant card than those who saw no ads (44.1% versus 53.6%; $z = -3.970$, $p < 0.001$). Participants who saw the implemental video were more likely to choose the dominant card than those who saw the baseline video (58.0% versus 39.7%; $z = 7.354$, $p < 0.001$). The interaction was not significant ($z = -1.683$,

Table 8. Effects of the Experimental Manipulations on Amount of Attention and Allocation of Attention

Primary analysis								
Antecedent	Average ln(Views + 1)				Difference in ln(Views + 1)			
	Coeff.	SE	t(1,599)	p	Coeff.	SE	t(1,599)	p
<i>Constant</i>	1.039	0.017	61.274	<0.001	0.139	0.008	16.521	<0.001
<i>Video</i>	0.060	0.034	1.767	0.077	0.090	0.017	5.350	<0.001
<i>Ads</i>	-0.405	0.034	-11.935	<0.001	-0.054	0.017	-3.189	0.001
<i>Video × Ads</i>	0.193	0.068	2.844	0.005	-0.012	0.034	-0.355	0.722
Conditional effects								
Antecedent	Average ln(Views + 1)							
	Coeff.	SE	t(1,599)	p				
<i>Video No Ads</i>	-0.037	0.048	-0.763	0.446				
<i>Video Ads</i>	0.156	0.048	3.255	0.001				
<i>Ads Baseline</i>	-0.501	0.048	-10.447	<0.001				
<i>Ads Implemental</i>	-0.308	0.048	-6.431	<0.001				

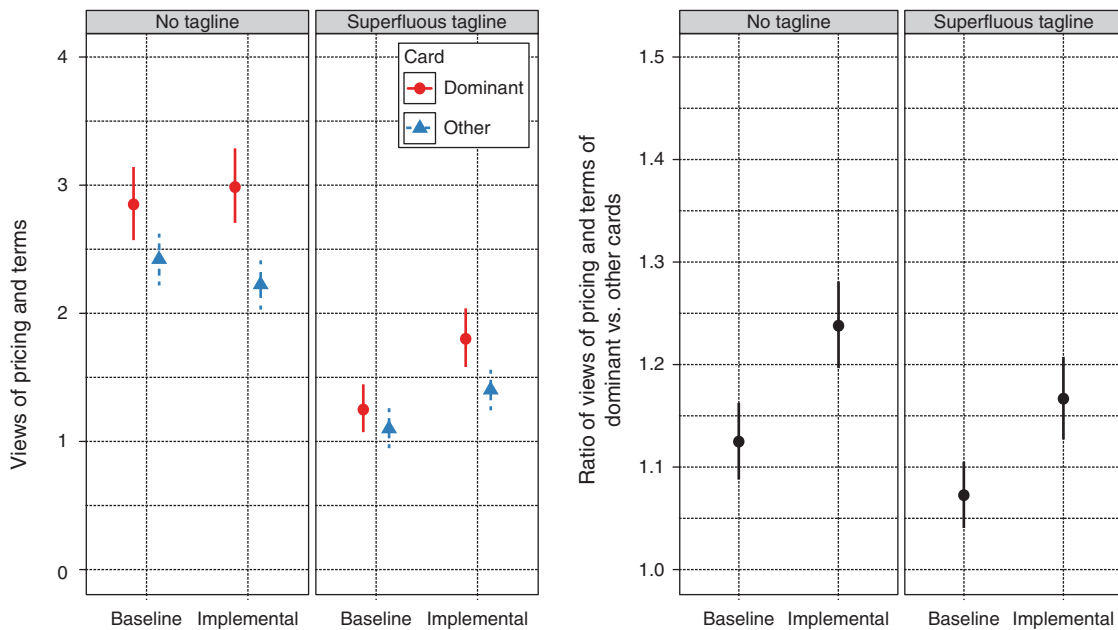
Notes. Averages reflect average number of views across the four cards. Differences reflect differences in the number of views between the dominant card and the average of the other three cards. Because there was an interaction between the two experimental factors for averages, we also show the conditional effects for each factor at each level of the other factor.

Figure 4. (Color online) Effects of Video and Ads on Attention as Measured via Time Spent Viewing Pricing and Terms



Notes. Left two panels: Time spent viewing pricing and terms of the dominant card and average of other cards. Analyses were conducted on transformed values; group means are transformed back into original units for interpretability. Error bars represent 95% confidence intervals. Right two panels: Approximate ratio of time spent viewing pricing and terms of the dominant card compared to views of other cards. Analyses were conducted on the difference between transformed values; plotted values are exponentiated means that may be interpreted as ratios, as $\ln(a/b) = \ln(a) - \ln(b)$. These ratios include 1 added to both the numerator and the denominator to account for 0s. Error bars represent 95% confidence intervals.

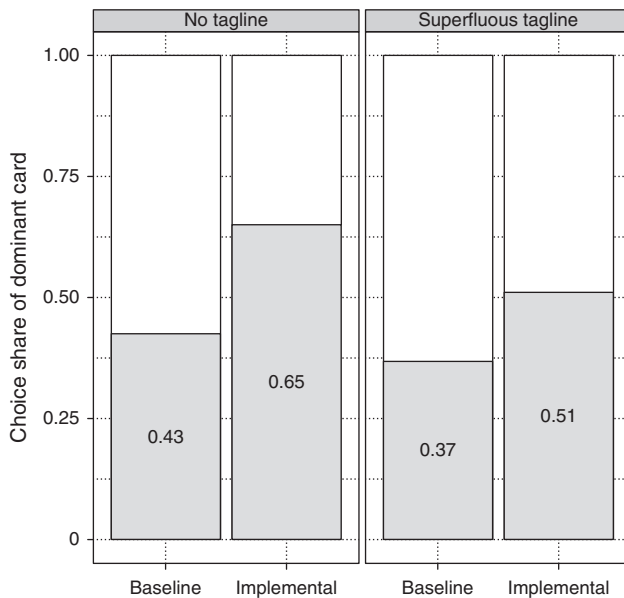
Figure 5. (Color online) Effects of Video and Ads on Attention as Measured via Number of Views of Pricing and Terms



Notes. Left two panels: Number of views of pricing and terms of the dominant card and average of other cards. Analyses were conducted on transformed values; group means are transformed back into original units for interpretability. Error bars represent 95% confidence intervals. Right two panels: Approximate ratio of number of views of pricing and terms of the dominant card compared to views of other cards. Analyses were conducted on the difference between transformed values; plotted values are exponentiated means which may be interpreted as ratios, as $\ln(a/b) = \ln(a) - \ln(b)$. These ratios include 1 added to both the numerator and the denominator to account for 0s. Error bars represent 95% confidence intervals.

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Figure 6. Choice Proportion of the Dominant Card in Each of the Four Experimental Treatments



$p = 0.101$). The cell proportions are given in Figure 6. This finding reinforces the notion that a clever informative video is not sufficient: the information must also be actionable.

We also considered three robustness checks. First, we analyzed the subset of participants who correctly identified all three video issues (and not the foil issue) at the end of the study. Second, we analyzed the subset of participants who chose the dominant card when all diagnostic information was highly salient during the explicit choice. Third, in addition to analyzing choice of the dominant card, we also analyzed whether participants made the self-specified best choice—that is, whether they chose the card during the main choice task that they chose when all pertinent information was salient at the end of the study during the explicit choice. For example, despite the warning in the video that the lack of a preset spending limit may be misleading, some participants may have decided that the advantages of the possibility of a higher spending limit outweighed the combined costs of both not having a preset spending limit and having a higher APR. This third robustness check allowed us to test effects on choice quality as defined by participants rather than the researchers. The main effects of the video and ad treatments remained consistent across each of these analyses.

One question that remains is whether differences in attention in these treatments account for these differences in choice quality. That is, if participants had not paid more attention to the terms in general, or had not allocated attention differentially across the dominant and the other card, would participants choosing

without distracting ads and participants choosing after viewing the implemental video still have chosen the dominant card?

While we did not exogenously vary attention independently of our other manipulations, we can still examine whether the correlational results are consistent with such an explanation using a mediation analysis (Hayes 2013, Zhao et al. 2010). Mediation analysis provides a test of whether the effect of a manipulation on a dependent variable operates via another variable or set of variables, the mediator(s). In this case, we can examine whether the effect of the video and the ad on choice quality operates via the amount and allocation of attention. As such, the *total effect* of the manipulation (here, video and ads) on the dependent variable (choice) can be divided into an *indirect effect* attributable to attention (effect of the manipulation on attention \times effect of attention on choice) and a *direct effect* (the effect of the manipulation on choice, controlling for attention). This helps us to answer the question of how the type of video and presence of ads affect choice.

We conducted mediation analyses using Hayes' (2013) PROCESS macro with confidence intervals of the indirect effect based on 10,000 bootstrapped samples. We tested whether there were indirect effects of superfluous ads, video, or their interaction on choice through the parallel mediators of the amount of attention and allocation of attention, operationalized both as time and as views. To do this, we conducted three sets of analyses on bootstrapped samples (see Hayes 2013 for details). The first set of analyses regressed the average attention on ads, video, and their interaction. The second regressed differences in attention on ads, video, and their interaction. The third regressed choice quality on ads, video, their interaction, average attention, and difference in attention. We find that the amount and allocation of attention are significant predictors of choice, and that the direct effects of video and ads are reduced after controlling for attention (see Table 9).

To test the indirect effects, we examine 95% bootstrapped confidence intervals (Hayes 2013, Zhao et al. 2010), presented in Tables 10 and 11. These indirect paths show that part of the effect of video, and most of the effect of ads, can be accounted for via attention, whether measured via clicks or time. The coefficient on video and ads are smaller once we have accounted for the effects of attention, and the indirect effect of the manipulations through attention on choice are significantly different from zero (as the 95% confidence intervals exclude zero). For example, Video affects Choice both by increasing the amount of attention participants paid to pricing and terms (see Table 10, row labeled *Video via Average Time*) and by affecting the allocation of attention participants paid to pricing and terms (see Table 10, row labeled *Video via Difference in*

Table 9. Logistic Regression Results Showing the Coefficients on Amount and Allocation of Attention on Choice, Controlling for Experimental Conditions

Antecedent	Choice (logits)				Choice (logits)			
	Coeff.	SE	z	p	Coeff.	SE	z	p
<i>Constant</i>	-1.150	0.118	-9.739	<0.001	-0.928	0.104	-8.934	<0.001
<i>Video</i>	0.620	0.117	5.314	<0.001	0.639	0.115	5.569	<0.001
<i>Ads</i>	-0.108	0.120	-0.897	0.370	-0.120	0.119	-1.008	0.314
<i>Video × Ads</i>	-0.410	0.234	-1.756	0.079	-0.502	0.230	-2.182	0.029
<i>Average Time</i>	0.351	0.045	7.813	<0.001				
<i>Difference in Time</i>	1.655	0.121	13.733	<0.001				
<i>Average Views</i>					0.492	0.087	5.632	<0.001
<i>Difference in Views</i>					2.811	0.210	13.391	<0.001

Note. Measures of attention are log-transformed after adding 1s to account for 0s.

Time). However, a residual effect of video above and beyond that explainable by attention remains. The conditional indirect paths indicate the conditional indirect effects separately by condition. These mediation analyses suggest multiple ways in which this video did (and other videos could) improve decisions. First, the implemental video increased choice quality by appropriately directing attention (whether measured via clicks or time). Second, the Implemental video increased choice quality by increasing the amount of time participants spent examining pricing and terms when facing superfluous advertising more so than when facing no ads. In this way, the video made a difference when it mattered most. Superfluous ads, on the other hand, substituted for meaningful consumer attention to diagnostic information and decreased choice quality.

Post hoc exploratory analyses further suggested that the Implemental video not only increased attention and allocation of attention, but also enabled partic-

ipants to better make use of their time. Amount of time spent was more strongly predictive of choosing the dominant option for participants who watched the Implemental video ($B = 0.556$, $SE = 0.074$, $z = 7.500$, $p < 0.001$) than participants who watched the Baseline video ($B = 0.212$, $SE = 0.065$, $z = 3.259$, $p = 0.001$; interaction: $B = 0.345$, $SE = 0.099$, $z = 3.498$, $p < 0.001$).

These results support the hypothesis that spending more time on financial decisions and focusing that attention efficiently leads to better decision making. Admittedly, one might consider a competing hypothesis in which time spent should be negatively correlated with choice quality. In such a case, people who make better decisions are better at sorting through financial information and move through the task in less time. However, the data and results in this paper do not seem to support that hypothesis. Time spent and better allocation of attention is positively correlated with choice quality.

Table 10. Mediation Results Showing the Indirect Effects of the Experimental Treatments on Choice via Amount and Allocation of Attention as Measured via (Transformed) Time Spent Viewing Pricing and Terms

Indirect effects on choice of . . .	Indirect effect	B	SE	LLCI	ULCI
<i>Video via Average Time</i>	+	0.107	0.026	0.059	0.163
<i>Video via Difference in Time</i>	+	0.205	0.056	0.099	0.316
<i>Ads via Average Time</i>	-	-0.250	0.040	-0.334	-0.178
<i>Ads via Difference in Time</i>	-	-0.129	0.054	-0.238	-0.024
<i>Video × Ads via Average Time</i>	+	0.094	0.048	0.006	0.197
<i>Video × Ads via Difference in Time</i>	ns	-0.094	0.107	-0.301	0.119
Conditional effects of video on choice					
<i>Via Average Time No Ads</i>	+	0.060	0.029	0.007	0.121
<i>Via Average Time Superfluous Ads</i>	+	0.153	0.041	0.079	0.241
Conditional effects of ads on choice					
<i>Via Average Time Baseline</i>	-	-0.297	0.051	-0.407	-0.205
<i>Via Average Time Implemental</i>	-	-0.203	0.041	-0.292	-0.130

Notes. The bounds of the 95% confidence interval are given in the right two columns. Implemental videos improved choice by both increasing attention and enhancing allocation of attention. Superfluous ads degraded choice by both decreasing attention and deteriorating allocation of attention. The conditional effect of video on choice via amount of attention was greater in the presence of ads. The conditional effect of ads on choice via the amount of attention was attenuated in the presence of the implemental video. LLCI, lower level for confidence interval; ULCI, upper level for confidence interval.

Table 11. Mediation Results Showing the Indirect Effects of the Experimental Treatments on Choice via Amount and Allocation of Attention as Measured via (Transformed) Number of Views of Pricing and Terms

Indirect effects on choice of . . .	Indirect effect	B	SE	LLCI	ULCI
<i>Video via Average Views</i>	ns	0.030	0.018	-0.002	0.068
<i>Video via Difference in Views</i>	+	0.253	0.051	0.157	0.359
<i>Ads via Average Views</i>	-	-0.199	0.041	-0.285	-0.124
<i>Ads via Difference in Views</i>	-	-0.151	0.050	-0.252	-0.057
<i>Video × Ads via Average Views</i>	+	0.095	0.038	0.031	0.184
<i>Video × Ads via Difference in Views</i>	ns	-0.034	0.095	-0.219	0.159
Conditional effects of video on choice					
<i>Via Average Time No Ads</i>	ns	-0.018	0.023	-0.068	0.023
<i>Via Average Time Superfluous Ads</i>	+	0.077	0.029	0.029	0.144
Conditional effects of ads on choice					
<i>Via Average Time Baseline</i>	-	-0.247	0.052	-0.361	-0.154
<i>Via Average Time Implemental</i>	-	-0.152	0.037	-0.233	-0.089

Notes. The bounds of the 95% confidence interval are given in the right two columns. Implemental videos improved choice by enhancing allocation of attention. Superfluous ads degraded choice by both decreasing attention and deteriorating allocation of attention. The conditional effect of video on choice was only in the presence of ads. The conditional effect of ads on choice was attenuated in the presence of the implemental video. LLCI, lower level for confidence interval; ULCI, upper level for confidence interval.

3.3. Perceptions and Attitudes Toward Sharing

To assess perceptions of the video and attitudes toward sharing, we began by analyzing how subjects viewed the different videos in terms of how special, hedonic, useful, or interesting they were. We combined the 17 individual ratings into four indices based on a priori classifications from prior research (Olney et al. 1991). Each index (with the mild exception of specialness) displayed good internal consistency as given by Cronbach’s α (special: 0.67; hedonic: 0.90; useful: 0.85; interesting: 0.82). The only difference was in perceived usefulness: the implemental video was rated as substantially more useful than the traditional video ($B = 0.668$, $SE = 0.060$, $t(1,599) = 11.185$, $p < 0.001$).

We also considered how the various treatments affected people’s tendency to share. Given that they were designed to assess the same construct and their high internal consistency (Cronbach’s $\alpha = 0.95$), the six sharing items were averaged into a single sharing measure and analyzed as a function of video, ads, and their interaction. Participants were slightly more likely to

share the implemental video than the Baseline video ($B = 0.240$, $SE = 0.089$, $t(1,599) = 2.708$, $p = 0.007$).

Perceived effectiveness of the video is key for better understanding the results. We combined the four effectiveness items into a single measure, given acceptable levels of internal consistency (Cronbach’s $\alpha = 0.76$) and regressed perceived effectiveness on the type of video, ads, and their interaction. Full regression results are given in the left side of Table 12. Ads did not have a main effect, but there was a main effect of video ($B = 0.889$, $SE = 0.057$, $t(1,599) = 15.636$, $p < 0.001$) qualified by a significant interaction ($B = -0.305$, $SE = 0.114$, $t(1,599) = -2.678$, $p = 0.007$). Overall, the implemental video was perceived to be substantially and significantly more effective than the baseline video.¹² However, this effect was somewhat weaker when there were superfluous ads that provided an apparent (though useless) alternative source of information ($B = 0.737$, $SE = 0.081$, $t(1,599) = 9.149$, $p < 0.001$) compared with when there were not ($B = 1.042$, $SE = 0.080$, $t(1,599) = 12.970$, $p < 0.001$).

Table 12. Effects of Manipulations on Perceived Effectiveness (Left) and Sharing, Controlling for Perceived Effectiveness (Right)

Antecedent	Perceived effectiveness				Sharing			
	Coeff.	SE	$t(1,599)$	p	Coeff.	SE	$t(1,598)$	p
<i>Constant</i>	4.574	0.028	160.831	<0.001	1.586	0.171	9.280	<0.001
<i>Video</i>	0.889	0.057	15.636	<0.001	-0.273	0.089	-3.083	0.002
<i>Ads</i>	-0.006	0.057	-0.113	0.910	0.032	0.083	0.390	0.697
<i>Video × Ads</i>	-0.305	0.114	-2.678	0.007	0.107	0.165	0.646	0.518
<i>Perceived Effectiveness</i>					0.577	0.036	15.917	<0.001

Note. Although not controlling for sharing, the effect of Video on sharing is positive; controlling for perceived effectiveness causes the coefficient on Video to reverse.

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Table 13. Mediation Results Showing the Indirect Effects of Manipulations on Sharing via Perceived Effectiveness

Indirect effects on sharing of . .	<i>B</i>	SE	LLCI	ULCI
<i>Video via Perceived Effectiveness</i>	0.513	0.045	0.430	0.604
<i>Ads via Perceived Effectiveness</i>	−0.004	0.033	−0.071	0.059
<i>Video × Ads via Perceived Effectiveness</i>	−0.176	0.068	−0.310	−0.047
Conditional effects of video on sharing				
<i>Via Perceived Effectiveness No Ads</i>	0.601	0.060	0.490	0.725
<i>Via Perceived Effectiveness Superfluous Ads</i>	0.425	0.052	0.327	0.532

Notes. The bounds of the 95% confidence interval are given in the right two columns. LLCI, lower level for confidence interval; ULCI, upper level for confidence interval.

Given these findings, we considered how the various treatments affected peoples' tendency to share in two ways: indirectly via perceived effectiveness (given in Table 13) and directly—that is, after controlling for the effect of perceived effectiveness (given in the right side of Table 12). The indirect effects given in Table 13 allow us to assess the extent to which the manipulations affected sharing through perceived effectiveness. We again used Hayes' (2013) PROCESS macro to examine the interactive effects of the video tag and superfluous advertisements on sharing through perceptions of video effectiveness. There is evidence of a positive indirect effect of the video on sharing through assessments of the video's effectiveness (the *Video via Perceived Effectiveness* row); however, this path is somewhat diminished in the presence of superfluous ads, as shown in the conditional effects rows.

There is an important caveat to these findings, shown in the right side of Table 12. After accounting for the indirect effects on sharing via perceived effectiveness, there was a residual direct effect of the video type on sharing. Notably, this direct effect is *negative* ($B = -0.273$, $SE = 0.089$, $t(1,599) = -3.083$, $p = 0.002$), providing evidence for competitive mediation (Zhao et al. 2010). In other words, if perceived effectiveness were held constant, the implemental video would be shared *less* than the baseline video, not more.

The implemental video increased sharing by increasing perceived effectiveness. However, it also decreased sharing directly—that is, not through sharing. The relative weights on these two paths may differ in different circumstances: if either of the perceived effectiveness links is weakened, sharing may be decreased. Therein lies a fundamental problem for encouraging good financial decisions through social media: the very videos that have the greatest potential to increase decision quality may also be the ones that are the least shareable if they are not compelling. Importantly, these ratings represent *perceived* effectiveness. If a video is effective but is not perceived by the viewer to be effective (and instead is viewed as “preachy” or merely instructive), there is the potential for a negative effect on sharing.

Subsequent post hoc regressions of sharing controlling for experimental manipulations on (a) average time and difference in time or (b) choice of dominant card showed that none of those measured process variables predicted sharing. However, if perceived effectiveness is included as a covariate along with average time, difference in time, and the experimental factors, allocation of time toward the dominant card predicts *less* sharing ($B = -0.152$, $SE = 0.066$, $t(1,596) = -2.302$, $p = 0.021$). Similarly, if perceived effectiveness is included as a covariate along with choice of dominant card and the experimental factors, the choice of dominant card also predicts *less* sharing ($B = -0.188$, $SE = 0.086$, $t(1,597) = -2.193$, $p = 0.028$). If perceived effectiveness is held constant, the outcome may be seen as a foregone conclusion and the video not worth sharing.

Finally, we considered the effects the treatments had on the propensity for people to apply for a credit card following the experiment. When choosing one of four credit cards, participants were not given a “no choice” option. One question then is whether their likelihood of applying for a credit card was affected by the ads or the video. Regressing the likelihood of applying on video, ads, and their interaction revealed that the ads had no main effect and the video had a positive main effect ($B = 0.462$, $SE = 0.066$, $t(1,599) = 7.045$, $p < 0.001$) qualified by an interaction ($B = -0.281$, $SE = 0.131$, $t(1,599) = -2.143$, $p = 0.032$). Participants rated themselves as significantly more likely to apply for a credit card when shown the implemental video than when shown the baseline video, although this was somewhat reduced in the presence of superfluous ads. Apparently, the baseline video made participants wary of credit cards without empowering them with the ability to make an effective choice; the instructions were not merely a “scare tactic.” By showing them where to find the necessary information, the tagged video increased the likelihood of applying relative to the baseline video.

4. Conclusion

On the basis of our analysis, we make the following conclusions. First, online videos do have the potential

to increase the quality of household financial decisions, but merely presenting the information in an engaging sticky format is not sufficient. The information must be interpretable and implementable to direct attention appropriately. Second, effective does not necessarily indicate sharable. In our study, had the tagged video not been perceived to be more effective (which may not always covary with actual effectiveness), it would have been considerably less likely to be shared. Finally, we provide process evidence suggesting how the video and distracting advertisements affected choice through their respective effects on amount and allocation of attention, as well as the importance of understanding multiple paths to sharing. Factors that increase perceived effectiveness alone without affecting properties of the video may increase the likelihood of sharing, but changing the video without affecting perceived effectiveness may actually decrease sharing.

Our work has several direct implications for policy makers. First, consumer protection can be preventative through implemental videos, not just reactive ex post with litigation. Online videos that incorporate implemental instructions improve the intensity of search, induce good decision making, and can lead to social learning. This first look at short, digestible online media as a route to good financial decisions calls for further research. To the extent regulatory agencies (e.g., the Consumer Financial Protection Bureau) and nonprofits (e.g., the National Endowment for Financial Education) are interested in taking preventative steps to reduce poor financial decisions, this research suggests a powerful channel.

Second, distracting advertising not only decreases search intensity and choice quality but also limits people from becoming informed in a robust way over the long term. Distracting advertising decreases attention devoted to information acquisition and decreases the ability of people to recall the terms of their credit agreements, but effective online videos may help to overcome this. As such, a two-pronged approach may be useful to protect consumers in the market: minimize distracting information where possible and provide concrete instructions via just-in-time information channels to enable good decisions.

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Endnotes

¹ Furthermore, according to YouTube, six billion hours of video content are watched per month, and 100 hours of new video content is uploaded to the website every minute (<http://web.archive.org/web/20140929030109/http://www.youtube.com/yt/press/statistics.html>, accessed September 29, 2014).

² According to Pew Research Center (2013).

³ See <http://www.statista.com/statistics/290404/millennials-popular-youtube-video-categories-male/> (accessed May 26, 2015) and <http://www.statista.com/statistics/290394/millennials-popular-youtube-video-categories-female/> (accessed April 14, 2016).

⁴ For example, while there has been considerable recent work that has examined the factors that influence sharing (e.g., Berger 2011, Berger and Milkman 2012, Chen and Berger 2013) and the effects of social media on sales (e.g., Stephen and Galak 2012), the interplay between the two remains a nascent field.

⁵ The video may be viewed as supplemental material at <https://doi.org/10.1287/mnsc.2016.2689>.

⁶ Specifically, we constructed our screenshots to be similar to the credit card offerings at www.chase.com. Additionally, we formatted pricing and terms disclosures similar to those at Chase. The bottom panel of Figure 3 provides a screenshot from the Chase website that demonstrates how it is similar to what we used in our experiment.

⁷ On a small number of observations (12 of 1,603), there was a malfunction such that the recorded time viewing each card was greater than the recorded total amount spent viewing the page. Treating these values as missing does not meaningfully affect any results.

⁸ In addition to the 1,603 participants who completed the study, another 300 participants abandoned the study partway through, most of them early on before choosing a credit card. Ninety-eight of the incompletes were unique, and 202 were duplicates or “false starts.” Abandonment rates did not meaningfully vary by condition. Results do not meaningfully differ when considering only the 1,489 responses without associated incomplete duplicates.

⁹ Our design is very nearly balanced, with only slight perturbations due to random assignment and attrition.

¹⁰ It is worth noting that this interaction is relatively small in magnitude and may be partly driven by nonlinearities resulting from the log transform.

¹¹ This finding underscores our assertion that subjects took the decision in our experiment seriously, even though they were not given explicit monetary incentives. Indeed, as one would expect, subjects who were further “educated” by the video spent more time investigating and searching for the dominant choice.

¹² Post hoc exploratory analyses suggest this effect is stronger among people who chose the dominant card ($B = 1.014$, $SE = 0.081$, $t(1,595) = 12.488$, $p < 0.001$) rather than those who did not ($B = 0.600$, $SE = 0.080$, $t(1,595) = 7.538$, $p < 0.001$; interaction $B = 0.414$, $SE = 0.114$, $t(1,595) = 3.644$, $p < 0.001$). Those who could not use the information successfully did not find it as useful.

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