Management Science

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http://pubsonline.informs.org

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MANAGEMENT SCIENCE
Vol. 61, No. 5, May 2015, pp. 949–962
ISSN 0025-1909 (print) | ISSN 1526-5501 (online)
http://dx.doi.org/10.1287/mnsc.2014.2069

The Hidden Cost of Accommodating Crowdfunder Privacy Preferences: A Randomized Field Experiment

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Online crowdfunding has received a great deal of attention as a promising avenue to fostering entrepreneurship and innovation. Because online settings bring increased visibility and traceability of transactions, many crowdfunding platforms provide mechanisms that enable a campaign contributor to conceal his or her identity or contribution amount from peers. We study the impact of these information (privacy) control mechanisms on crowdfunder behavior. Employing a randomized experiment at one of the world's largest online crowdfunding platforms, we find evidence of both positive (e.g., comfort) and negative (e.g., privacy priming) causal effects. We find that reducing access to information controls induces a net increase in fund-raising, yet this outcome results from two competing influences—treatment increases willingness to engage with the platform (a 4.9% increase in the probability of contribution) and simultaneously decreases the average contribution (a $5.81 decline). This decline derives from a publicity effect, wherein contributors respond to a lack of privacy by tempering extreme contributions. We unravel the causal mechanisms that drive the results and discuss the implications of our findings for the design of online platforms.

Keywords: crowdfunding; privacy; priming; anonymity; randomized experiment
History: Received November 11, 2013; accepted August 8, 2014, by Lee Fleming, entrepreneurship and innovation. Published online in *Articles in Advance* January 29, 2015.

1. Introduction

Over the last eight years, a growing proportion of the venture finance gap has been filled by novel funding mechanisms. Online crowdfunding, defined as "a collective effort by people who network and pool their money together, usually via the internet, in order to invest in and support efforts initiated by other people or organizations" (Ordanini et al. 2010, p. 444), has received a great deal of attention from entrepreneurs and policymakers as a promising avenue to fostering entrepreneurship and innovation.

One of the primary hurdles typically faced by new entrepreneurs is the identification and sourcing of capital (Wetzel 1987). Crowdfunding simplifies this process by providing entrepreneurs with broader reach and visibility (Agrawal et al. 2014, Kim and Hann 2013). However, a notable implication of shifting the fund-raising process online is the increased visibility and traceability of transactions. Most crowdfunding platforms maintain a public record of all transactions, including information about contributors' identities, the amount of their contributions, and the campaigns they chose to support. Perhaps cognizant of the possibility that some crowdfunders may shy away from scrutiny (while others may seek it), many crowdfunding platforms now provide users with transaction-level information controls that enable concealment (revelation) of identity or contribution amounts, at the contributor's discretion.

Ostensibly, providing crowdfunders with the ability to determine the visibility of their contributions to peers should increase their level of satisfaction, and thus their willingness to transact, resulting in increased fund-raising. A large number of studies in recent years support this logic. Scholars have noted the growing prevalence of privacy concerns among consumers (Goldfarb and Tucker 2012) and have demonstrated the positive effects of privacy assurances, policies, and seals on user information sharing and product purchase (Hui et al. 2007, Tsai et al. 2011). At the same time, a number of studies have demonstrated the value of social recognition and reputational gains as drivers of user contributions to online communities (Wasko and Faraj 2005, Zhang and Zhu 2011).

However, providing users with information control mechanisms can also be costly. Recent work suggests that users may ignore these features if they perceive that they are inflexible, difficult to understand, or a challenge to use (Das and Kramer 2013, Sleeper et al. 2013), potentially opting not to transact at all. It has also been shown that prompting individuals with questions about scrutiny or their privacy can elicit privacy concerns via priming effects (John et al. 2011, Joinson et al. 2008, Tucker 2014). This, in turn, can have a negative influence on users' willingness to engage with a purveyor, platform, or other users.

We therefore set out to understand the impact that transaction-level information controls have on crowdfunders' willingness to contribute, as well as their subsequent behavior, conditional on conversion. More specifically,

- we seek to identify and quantify the causal relationship between a crowdfunding platform's provision of information control features and potential contributors' willingness to transact;
- further, we look to understand any associated shifts in behavior, conditional on transaction.

Evaluating the impact of information control provision on user behavior is inherently challenging because of various biases associated with observational and survey-based attempts to evaluate privacy-sensitive individuals who frequently are, by definition, unwilling to be scrutinized or profiled. Moreover, concerns about privacy may not be accurately reflected in interview or survey-based settings because of the gap between consumers' claimed privacy concerns and their actual behavior in response to those concerns (Strandburg 2005). Experimental subjects expect a researcher to collect their information, and they are unlikely to have concerns about receiving unwanted solicitations from third-party organizations or individuals down the line, because standard data collection policies in experimental protocols prohibit the sharing of data without consent (Wattal et al. 2012).

Meanwhile, observational studies are confronted with their own comparable issues. Researchers must contend with a lack of available data as subjects strive to conceal their actions. Further, issues of endogeneity stemming from self-sorting and self-selection (Heckman 1979) are also likely to arise from any changes in user privacy conditions. To clarify, consider the example of the privacy-sensitive consumer. Such consumers may opt to exit a marketplace entirely following, for example, the removal of a privacy assurance. Unless this selection effect were to somehow be accounted for explicitly, either in the data or through estimation techniques, it would be impossible to draw valid, generalizable conclusions about the effect that such a change had on user behavior. Although various econometric techniques are available to address these issues, each is heavily laden with assumptions. Further, data-based adjustments are often challenging, if not impossible, to implement because subjects who do not participate will often go unobserved.

Fortunately, Web-based experimentation with impression- or session-level data can alleviate many of these concerns. We partnered directly with the purveyor of a leading global online crowdfunding platform to design and execute a randomized control trial, unbeknownst to the subjects under study (i.e., website visitors). Subjects in our sample were thus observed while making real-life decisions, with real economic consequences. Our results are therefore not subject to the reporting biases inherent in survey research of privacy issues, nor are they subject to issues of self-selection, because we observe subjects even when they choose not to transact.

We randomly manipulate the presentation timing of an information control question, displaying it either before or after the completion of payment. This intervention allows us to understand what impact information control features have on users' willingness to engage with the website, in terms of whether they contribute to crowdfunding campaigns (willingness to transact) and, given contribution, their contribution amounts.

We found, counter to intuition, that delaying the presentation of information controls drove a 4.9% increase in users' probability of completing a transaction. At the same time, conditional on transacting, the dollar amount of the average campaign contribution declined (by \$5.81) with treatment. Fortunately for the purveyor of this platform, the increase in the rate of participation was sufficient to offset the decline in average contributions, resulting in an immediate net benefit from the intervention. Accordingly, the purveyor has since adopted the postpayment setup on a permanent basis.

Our subsequent analyses indicate that the treatment reduced the variance of contribution amounts, with an asymmetrically stronger effect on large contributions. That is, our treatment reduced the prevalence of both large and small contributions, although the decline in large contributions was more pronounced. We submit that this occurred because contributors in the postpayment condition, having reduced access to privacy controls, perceived greater publicity for their actions. Accordingly, they regressed toward the mean to avoid drawing unwanted attention (e.g., unsolicited requests from other crowdfunding campaigns). This result provides empirical evidence of the impact of publicity on individuals' behavior, which has seen theoretical consideration in the prior literature on monetary donations to public goods (Daughety and Reinganum 2010) and which has also been demonstrated in regard to other types of online contributions, such as user-generated content in the form of restaurant reviews (Wang 2010). This implies that a careful balance must be maintained between users' privacy concerns and the behavior that can ensue from accommodating those concerns.

This work contributes to the growing literature on crowdfunding (Agrawal et al. 2014, Burtch et al. 2013a). Studies have looked at various drivers of campaign fund-raising outcomes, including pitch framing and information disclosure (Ahlers et al. 2012, Herzenstein et al. 2011b), fund-raisers' social networks (Lin et al. 2013, Mollick 2014), and geography and culture (Burtch et al. 2014a, Lin and Viswanathan 2013, Agrawal et al. 2015). However, perhaps the most common subject of study has been peer influence among contributors (Burtch 2011, Burtch et al. 2013b, Herzenstein et al. 2011a, Kim and Viswanathan 2014, Zhang and Liu 2012). Bearing in mind that peer influence depends on the visibility of peers' actions, our work considers the underlying context and mechanisms that enable those effects. In that vein, this work is most closely related to Burtch et al. (2014b), who examine how and when users choose to make use of information controls in crowdfunding. We build on that work by examining the causal effect on crowdfunders' behavior from merely providing (or not providing) information controls.

Our work also builds on the literatures dealing with privacy and reputation, in that we consider the dual, potentially countervailing impacts of privacy feature provision on users' (i) conversion and (ii) subsequent behavior, conditional on conversion. To our knowledge, these parallel effects have not been separately examined in prior work. Last, our work contributes to the literature on anonymity in charitable contribution. A number of studies in recent years have noted the role of perception management and social image in prosocial behavior (Andreoni and Bernheim 2009, Daughety and Reinganum 2010). Our results indicate that these types of concerns similarly play into crowdfunder behavior, which in turn speaks to the presence of altruistic motives in online crowdfunding markets.

2. Methods: Randomized Experiment

2.1. Study Context

Our experiment was conducted at one of the largest global reward-based crowdfunding platforms, which enables anyone to raise money for a project or venture. The marketplace attracts upward of 200,000 visitors per day and facilitates millions of dollars in contributions each month. Since 2008, the platform has attracted over one million users from more than 200 countries.

The platform allows fund-raising for any purpose. When campaign owners first submit their project, they are required to specify how the money will be used, rewards that contributors can claim, the target amount to be raised, the number of days the fund-raiser will run for, and the funding format (keeping what is raised versus a provision point mechanism/threshold fund-raiser).

Campaigns are presented to website visitors in order of popularity. Popularity is measured algorithmically by the platform operator, based on a variety of factors, including organizer effort, fund-raising progress, media coverage, etc. The home page highlights new campaigns and those that are ending soon. A visitor can also filter ongoing campaigns by location, proximity ("near me"), or category.[1]

Individuals who decide to contribute must first specify how many dollars they would like to supply. Next, contributors provide their email addresses and, if a reward is being claimed, their shipping addresses. At this point, in our control condition, the contributor is presented with a question about how the contribution record should appear to website visitors. Contributors can conceal either their identity or the amount of their contribution (but not both).[2] Importantly, a contributor's identity and amount will always be visible to the campaign organizer and platform operator; this information control prompt only masks details from a contributor's peers.

2.2. Experimental Design

Figure 1 presents a design mock-up of the information control question that is posed to users during the course of contribution. Each user is asked to specify which pieces of information about the contribution he or she would like to display publicly.[3] Our experimental treatment imposes a delay in the presentation of this question, from before payment to after payment. This treatment mimics removal of the mechanism from the platform, in a watered-down form. This treatment allows us to assess the economic

[1] The campaign organizer (rather than the marketplace purveyor) determines the campaign category. As such, there are no strict rules around the assignment of categories; thus these groupings are fuzzy and may overlap.

[2] Information-hiding mechanisms of this sort are relatively common in online crowdfunding. Some other prominent platforms that employ these features include GoFundMe.com, GiveForward.com, and Crowdrise.com.

[3] Although it is possible for a user to create an account using a pseudonym, the high frequency with which these information control mechanisms are used (in approximately 50% of contribution instances) indicates that a majority of users reveal their true identity in their user profile.

Figure 1 Privacy Control Prompt

Your contribution will currently appear to everyone as "John Doe—$20."
Would you like to change the appearance?

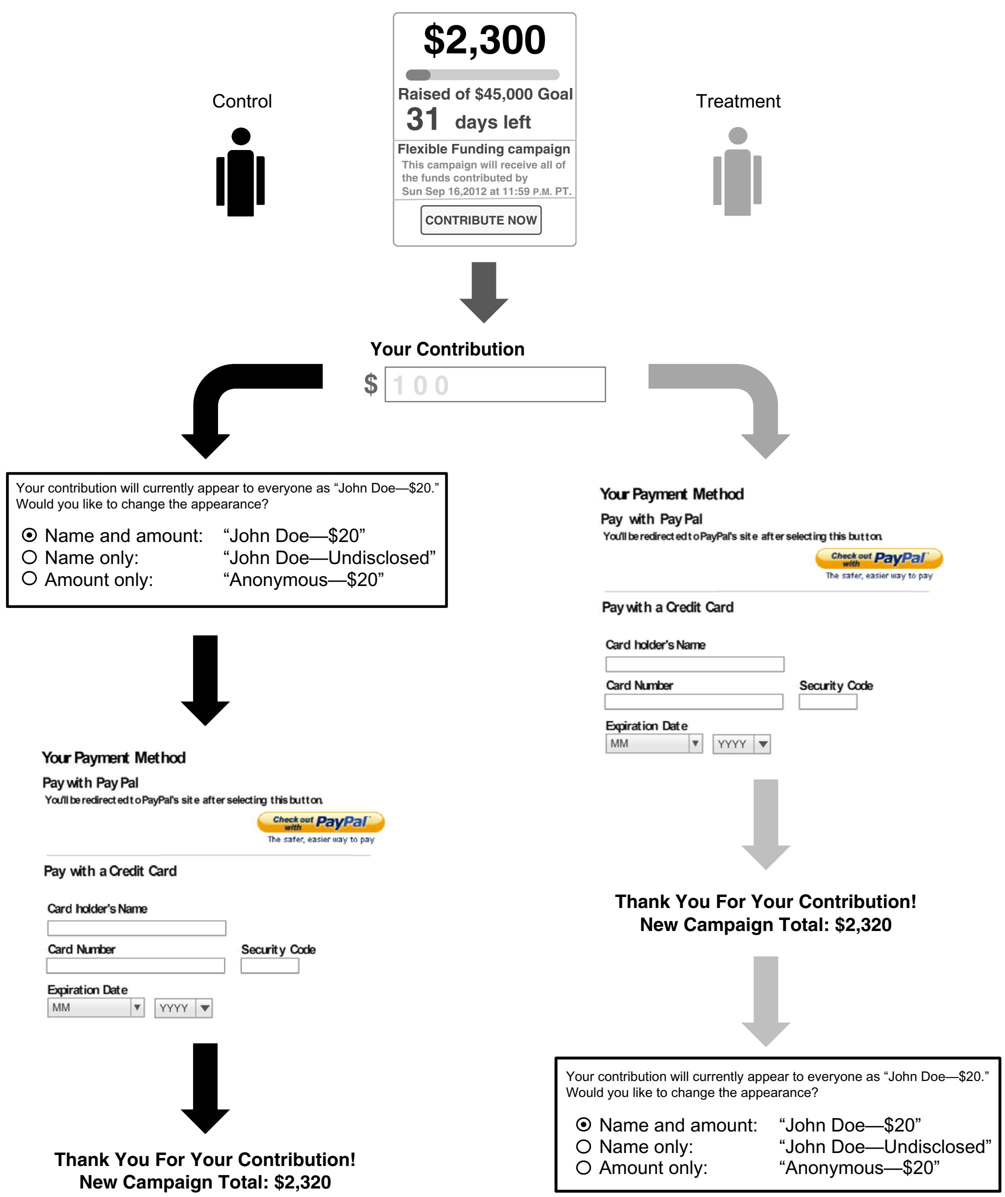
- Name and amount: "John Doe—$20"
- Name only: "John Doe—Undisclosed"
- Amount only: "Anonymous—$20"

impact of providing information controls, in terms of both users' willingness to transact and contribution amounts, conditional on transaction. Ultimately, we aim to assess whether these mechanisms deliver a net benefit or detriment to campaign fund-raisers and the platform operator.

As noted above, in the prepayment (control) condition, the information control question is presented to the user just prior to payment. In the postpayment (treatment) condition, the mechanism is not presented until after payment has been completed. Figure 2 provides a visual comparison of the experimental flow experienced by subjects in our treatment and control conditions.

The timing of the information control prompt (i.e., before versus after payment) may have two foreseeable, countervailing impacts on user behavior. On one hand, placing the mechanism after payment may reduce any potential privacy or scrutiny priming effects, because users are not prompted to consider these issues before making their payments. In turn, this effect could be expected to increase conversion rates. On the other hand, delaying presentation might reduce willingness to transact if users already have

Figure 2 (Color online) Contribution Flow and Information Control Prompt Positioning



privacy or scrutiny in mind (e.g., privacy-sensitive individuals). Because of these competing effects, it is not immediately clear what impact our treatment will have on fund-raising.

This treatment allows us to gain insights into the economic impacts of providing information control mechanisms. Because we only delay the presentation of the mechanism and do not remove it entirely, any identified effects are presumed to be conservative estimates of how provision impacts behavior. Moreover, because we cannot ensure that every campaign visit is associated with a first-time contributor,[4] some subjects in our treatment condition may anticipate the eventual provision of information-hiding mechanisms. Such anticipatory behavior can only mute the effects of our treatment, again resulting in conservative estimates.

2.3. Econometric Specification

Our estimation approach relies primarily on ordinary least squares (OLS) with campaign fixed effects. All of our estimations additionally incorporate time fixed effects (in terms of the absolute day on which the observation took place as well as the day of week) and a variety of other control variables[5] pertaining to both the contributor and the campaign.

We estimate our models in a stepwise fashion, beginning with a simple model that includes only our treatment indicator, *Treatment* (*T*), as well as campaign and time fixed effects. We then incrementally incorporate the other controls—namely, the funds raised by the campaign to date (*Campaign Balance*); the number of days of elapsed fund-raising (*Campaign Days*); and a binary indicator of whether the visitor arrived on a mobile device (*User Mobile*) as well as indicators for his or her browser type (*User Browser*), language (*User Language*), and country, based on Internet protocol address (*User Country*). Equation (1) captures our econometric specification:

$$Conversion_{ijt} = \beta \times T_{ijt} + \gamma \times X_{jt} + \lambda \times Z_{it} + \varphi_j + \omega_t + \varepsilon_{ijt}. \quad (1)$$

We index users with i, campaigns with j, and time in days with t. The coefficient of interest is β, capturing the effect of our treatment on conversion rates; X is a vector of dynamic campaign controls for fund-raising and duration; Z is a vector of user/visit controls, including browser, language, country, and device; φ is a vector of campaign fixed effects; ω is a vector of day and day of week fixed effects; and last, ε is our error term.

We employ a similar specification to estimate our treatment's effect on conditional and unconditional contribution. A notable difference in our conditional contribution estimations, however, is that we are able to identify all subjects in the sample. Accordingly, we can incorporate additional contributor controls associated with the user account, such as his or her tenure on the platform (*User Tenure*) and an indicator of whether he or she has an explicit organizer relationship with the campaign (*Organizer*). Equation (2) captures our specification for the conditional conversion model. Our estimations considering contribution per visitor (unconditional contribution) are identical, except that they exclude the account-based contributor controls:

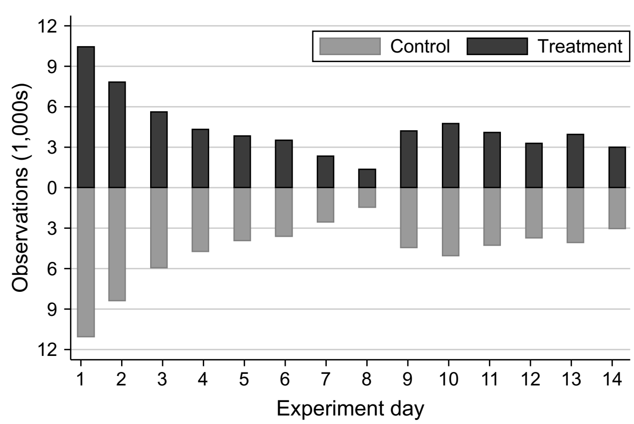
$$Contribution_{ijt} = \beta \times T_{ijt} + \gamma \times X_{jt} + \lambda \times Z_{it} + \varphi_j + \omega_t + \varepsilon_{ijt}. \quad (2)$$

In addition to providing our main regression results, we offer a set of ancillary analyses intended to explore and validate the mechanism underlying our treatment effect. Further, we provide a series of robustness checks—e.g., alternative estimators, sample splits, and manipulation checks.

2.4. Data and Descriptive Statistics

Our experiment was conducted over a 14-day period. We observed 128,701 visitors that entered the

Figure 3 Treatment and Control Proportions Over Time



[4] We offer one robustness check in which we examine our treatment's effect on conditional contribution among only new users (i.e., those registering in the 24 hours prior). We can be reasonably sure that recent joiners are first-time contributors and thus are unlikely to hold any prior expectations about the availability of information control features. Our results remain consistent in this estimation.

[5] We do not incorporate user fixed effects because we are unable to identify users who do not contribute any funds. This is not a major concern, however, because our treatment is randomized, and thus it is extremely unlikely to be correlated with omitted variables. Our estimations also demonstrate that incorporating the various controls at our disposal does little to influence the magnitude or significance of our treatment effect estimates.

Table 1 Descriptive Statistics: Control vs. Treatment

	Control			Treatment			
Variable	Mean	SD	*N*	Mean	SD	*N*	Total *N*
Conversion	0.259	0.438	66,369	0.323	0.467	62,332	128,701
Conditional Contribution	89.337	260.948	17,222	85.939	263.440	20,106	37,328
Organizer	0.017	0.128	17,222	0.019	0.136	20,106	37,328
Binary Info Hiding	0.470	0.500	17,222	0.208	0.406	20,106	37,328

Note. Binary Info Hiding is an indicator of whether the contributor chose to hide any information in his or her transaction (either his or her name or the amount).

Table 2 Descriptive Statistics: Sample Wide

Variable	Mean	SD	Min	Max	*N*
Treatment	0.48	0.50	0.00	1.00	128,701
Conversion	0.29	0.45	0.00	1.00	128,701
Unconditional Contribution	25.38	146.73	0.00	10,000.00	128,701
Campaign Balance	148,134.60	253,854.80	0.00	1,142,523.00	128,701
Campaign Days	25.77	22.74	1.00	252.00	128,701
User Mobile	0.22	0.42	0.00	1.00	128,701
Organizer[a]	0.02	0.13	0.00	1.00	37,328
User Tenure[a]	43.15	127.49	0.00	1,835.00	37,328
Day of week					
Monday	0.12	0.32	0.00	1.00	128,701
Tuesday	0.08	0.28	0.00	1.00	128,701
Wednesday	0.19	0.39	0.00	1.00	128,701
Thursday	0.19	0.39	0.00	1.00	128,701
Friday	0.17	0.37	0.00	1.00	128,701
Saturday	0.14	0.34	0.00	1.00	128,701

[a]Sample is based only on converted users.

campaign contribution flow and thus joined our subject pool. Of these, 62,332 were assigned to the treatment condition (48.4%) and 37,328 chose to contribute funds (29%). The distribution of subjects entering each condition, over the course of our experiment, is presented in Figure 3. Table 1 provides a breakdown of notable descriptive statistics across each stage of the contribution flow, across conditions. Table 2 provides sample-wide descriptive statistics for all of our variables.

Figures 4, 5, and 6 depict differences in the probability of conversion, expected conditional contribution, and expected unconditional contribution, respectively, between our control and treatment groups. In each case, we see rather stark shifts in user behavior, with conversion rates increasing and conditional average contributions decreasing.

We also collected additional data about the prevalence with which campaign visitors view prior records of contribution. We obtained data from the platform operator about user navigation patterns. Specifically, we obtained data for roughly 145,000 campaign visitors about the last campaign tab they viewed before navigating elsewhere. We observed that nearly 30% of visitors examined the list of past contributions immediately before navigating elsewhere (either to contribute or exit). Considering that we only observe the last tab viewed, the proportion of visitors navigating to the funders tab is in fact likely to be much higher than this. This provides clear

Figure 4 Conversion Rate Between Control and Treatment Groups

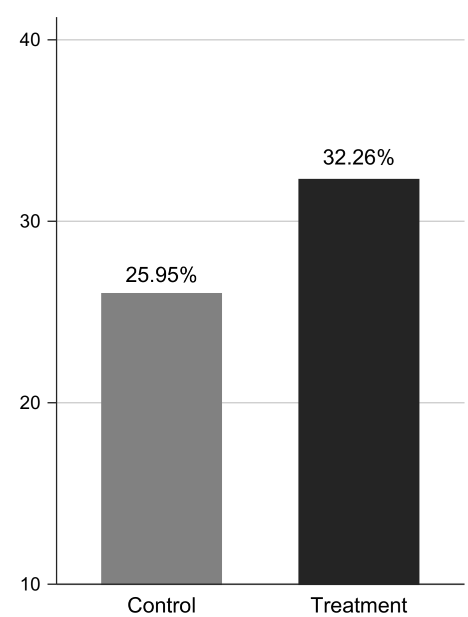


Figure 5 Average Conditional Contribution Between Control and Treatment Groups

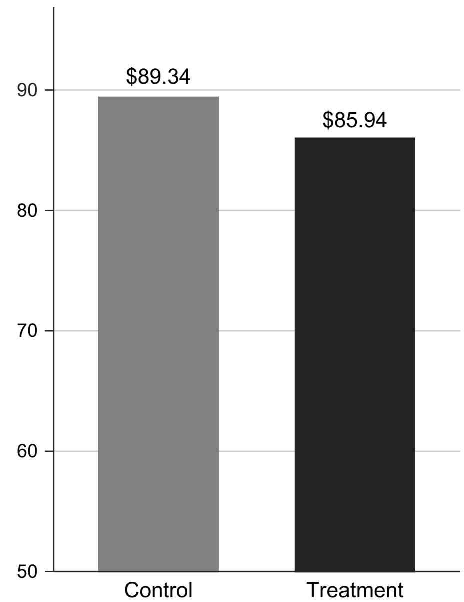
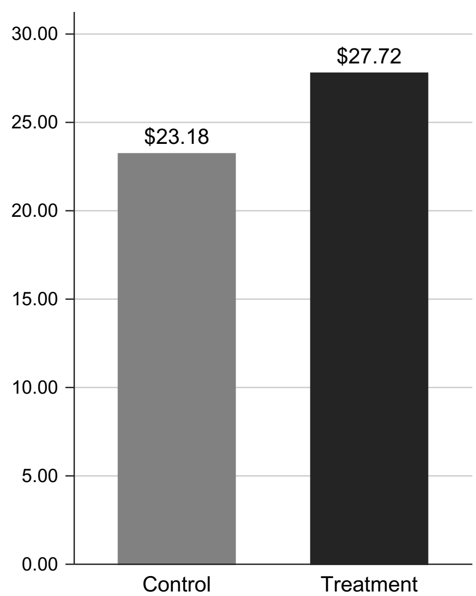


Figure 6 Average Unconditional Contribution Between Control and Treatment Groups



empirical evidence of the potential role of scrutiny and publicity.

3. Results

We began by studying the treatment effect on the probability of visitor contribution. As noted previously, the perception of control over one's information, and thus one's privacy, can have multiple countervailing effects. On one hand, users' perception of control can result in increased rates of participation if privacy-sensitive users are made more comfortable (Hui et al. 2007, Tsai et al. 2011). On the other hand, prompting users with privacy- or scrutiny-related questions can prime users with privacy concerns, thereby reducing participation (John et al. 2011, Tucker 2014). This latter notion is also supported by recent work that has found that individuals actually place less emphasis on privacy when they are not initially endowed with it (Acquisti et al. 2013).

We also assessed the treatment's impact on users' dollar contributions. A number of studies note that individuals go to great lengths to conceal information when they are concerned about how others will view it (Ariely and Levav 2000, Huberman et al. 2005). Here, individuals may prefer to conceal their contributions if they may be viewed as "cheap." Alternatively, large donors might fear drawing attention or unwanted solicitations for future donations from other campaigns. For these reasons, we anticipated that the prominence of the information control prompt would be positively associated with extreme contributions (very small or very large), because cognizance of the option to conceal information was expected to make users more willing to engage in such activity.

We first report results for the impact of our treatment on the probability of campaign contribution (see Table 3). We saw that the treatment reduces privacy sensitivity, resulting in an approximate 4.9% increase in the probability of conversion (column (4)). Examining the change in dollar contributions, conditional on conversion (see Table 4), we found that the average contribution declined by approximately $5.81 (column (5)). This result reinforces our earlier observation that offering information controls may have a somewhat complex effect, in that it can have a variety of countervailing impacts. Taken together, the above two results indicate that the provision of information hiding mechanisms, and perhaps privacy controls in general, can have counterintuitive, detrimental impacts on user behavior from the purveyor's standpoint, raising users' concerns and lowering their willingness to transact on the platform.[6]

When we consider the above effects in tandem (i.e., the combination of increased participation and reduced contribution), we find that the increase in conversion rates dominated. These results are reported in Table 5. Thus, our treatment ultimately resulted in a net benefit for the platform purveyor in terms of overall fund-raising outcomes. We saw

[6] At the same time, it should also be noted that the provision of these features could reduce consumer surplus. If opting out of a particular transaction is actually an optimal choice, removing privacy controls and thus privacy priming from the contribution process may actually drive the crowdfunder toward suboptimal behavior. As such, the treatment may impose some unobserved costs on crowdfunders.

Table 3 Regression Results: Conversion Rate (Linear Probability Model with Fixed Effects; Dependent Variable Is *Conversion*)

Explanatory variable	(1)	(2)	(3)	(4)
Treatment	0.057*** (0.007)	0.057*** (0.007)	0.055*** (0.007)	0.049*** (0.007)
Campaign Balance	—	1.17e–07** (3.77e–08)	1.25e–07** (3.27e–08)	2.09e–07*** (1.78e–08)
Campaign Days	—	−0.015*** (0.002)	−0.013*** (0.002)	−0.013*** (0.001)
User Mobile	—	—	−0.152*** (0.009)	−0.156*** (0.009)
User Browser	Not included	Not included	Not included	Included
User Language	Not included	Not included	Not included	Included
User Country	Not included	Not included	Not included	Included
Day of week effects	Included	Included	Included	Included
Time effects	Included	Included	Included	Included
Campaign effects	Included	Included	Included	Included
Observations	128,701	128,701	128,701	128,701
F-statistic	27.80 (20,5077)	33.33 (22,5077)	57.78 (23,5077)	1.2e+08 (214,5077)
R^2	0.14	0.14	0.15	0.18

Notes. Robust standard errors are reported in parentheses for coefficients, clustered by campaign. Sample includes all users who entered the contribution flow.
$^{**}p < 0.01$; $^{***}p < 0.001$.

Table 4 Regression Results: Conditional Contribution (OLS with Fixed Effects; Dependent Variable Is *Contribution*)

Explanatory variable	(1)	(2)	(3)	(4)	(5)
Treatment	−5.472* (2.727)	−5.472* (2.727)	−5.472* (2.726)	−5.525* (2.720)	−5.810* (2.679)
Campaign Balance	—	4.22e–06 (0.000)	4.22e–06 (0.000)	5.21e–06 (0.000)	7.79e–07 (0.000)
Campaign Days	—	0.703 (1.115)	0.703 (1.108)	0.617 (1.108)	−2.954+ (1.781)
User Mobile	—	—	−0.064 (4.148)	0.458 (4.152)	−3.095 (5.772)
User Tenure	—	—	—	−0.024** (0.009)	−0.020* (0.008)
Organizer	—	—	—	81.792** (25.129)	82.868** (25.184)
User Browser	Not included	Not included	Not included	Not included	Included
User Language	Not included	Not included	Not included	Not included	Included
User Country	Not included	Not included	Not included	Not included	Included
Day of week effects	Included	Included	Included	Included	Included
Time effects	Included	Included	Included	Included	Included
Campaign effects	Included	Included	Included	Included	Included
Observations	37,328	37,328	37,328	37,328	37,328
F-statistic	1.79 (20,3581)	2.20 (21,3581)	2.06 (23,3581)	2.73 (25,3581)	1.2e+09 (216,3581)
R^2	0.17	0.17	0.17	0.17	0.18

Notes. Robust standard errors are reported in parentheses for coefficients, clustered by campaign. Sample includes only converted users (i.e., those who contributed at least some amount of money). Estimation includes additional user-profile-specific controls, *User Tenure* and *User Mobile*, because all users are identified.
$^{+}p < 0.10$; $^{*}p < 0.05$; $^{**}p < 0.01$.

Table 5 Regression Results: Unconditional Contribution (OLS with Fixed Effects; Dependent Variable Is *Contribution*)

Explanatory variable	(1)	(2)	(3)	(4)
Treatment	4.375*** (1.015)	4.396*** (1.022)	4.232*** (1.003)	3.552*** (0.953)
Campaign Balance	—	3.01e–05*** (7.34e–06)	3.10e–05*** (7.10e–06)	3.83e–05*** (6.23e–06)
Campaign Days	—	−1.498** (0.481)	−1.344** (0.467)	−1.300** (0.443)
User Mobile	—	—	−15.131*** (2.482)	−16.386*** (2.604)
User Browser	Not included	Not included	Not included	Included
User Language	Not included	Not included	Not included	Included
User Country	Not included	Not included	Not included	Included
Day of week effects	Included	Included	Included	Included
Time effects	Included	Included	Included	Included
Campaign effects	Included	Included	Included	Included
Observations	128,701	128,701	128,701	128,701
F-statistic	3.38 (20,5077)	5.41 (22,5077)	6.75 (23,5077)	6.5e+08 (214,5077)
R^2	0.06	0.06	0.06	0.06

Notes. Robust standard errors are reported in parentheses for coefficients, clustered by campaign. Sample includes all users who entered the contribution flow.
$^{**}p < 0.01$; $^{***}p < 0.001$.

an estimated increase of roughly \$3.55 in the average contribution per visitor, following treatment.

4. Supporting Analyses

We next conducted a set of secondary analyses to understand the underlying mechanisms of the observed effects. We examined whether average contributions were indeed falling because of a decline in the variance of contributions (i.e., fewer extreme contributions), as suspected. Further, we looked for heterogeneity in the treatment effect around sensitive campaign topics. We undertook four additional analyses in this regard.

First, we sought to quantify any shifts in the deviation of contributions relative to the overall campaign average. This reference point is appropriate because the definition of an *extreme* contribution should depend on the characteristics of the campaign being supported and the social norms surrounding it. We determined the absolute deviation from the average for each contribution record. We then regressed that absolute deviation[7] on our binary indicator of treatment. The results are presented in Table 6. Taking the exponential of our coefficient estimate, we found that the treatment produced an approximate 21% decrease in deviations from the campaign average.

Table 6 **Regression Results: Publicity Effect (Dependent Variable Is log(*Absolute Deviation*))**

Explanatory variable	OLS fixed effects
Treatment	-0.192^{***} (0.044)
Controls[a]	Included
Observations	33,746
F-statistic	7.5e+09 (216,2517)
R^2	0.05

Notes. Robust standard errors are reported in parentheses for coefficients, clustered by campaign. Sample includes all observations that resulted in contribution, with the exception of those that arrived to a campaign first (i.e., first contribution in the sequence). Accordingly, the sample only includes campaigns that received more than one contribution.

[a]The same set of controls used in Table 4, column (5), are incorporated in the estimation.

$^{***}p < 0.001$.

Second, we examined the total variance in contribution amounts between our treatment and control conditions, identifying a statistically significant decrease ($F = 1.059$, $p < 0.001$). Additional tests based on Levene's robust test statistic, as well as that proposed by Brown and Forsythe, were similarly significant ($p < 0.001$). This result provides further support for our interpretation of the treatment effect on contribution amounts as deriving largely from subjects' increased perception of publicity.

Third, we examined the degree to which information hiding was associated with larger or smaller contributions (the tails of the distribution) and whether the association was balanced between the two. We constructed two binary indicators of contribution size, *Small* or *Large*, based on whether the contribution amount fell into the bottom or top 1%, 5%, or 10% of the overall distribution, respectively. We then ran three regressions, modeling a binary indicator of information hiding as a function of each pair of indicators, in addition to our various controls. We obtained the results reported in Table 7. We saw that contributions at either tail are significantly more likely to be associated with information hiding, and we saw an asymmetric effect: larger contributions were almost twice as likely to be associated with information hiding. Moreover, the difference between the two coefficients was statistically significant ($F(1, 3581) = 6.92$, $p < 0.01$).

Fourth, and last, to explore whether our treatment effect varied with the sensitivity of the campaign topic, we constructed an indicator of topic sensitivity and interacted it with our treatment indicator. We first examined the list of campaign categories, of which there were 24. Among these, we identified four categories that are potentially quite sensitive, where individuals' feelings and opinions are somewhat ideological in nature: politics, religion, education, and the environment.[8] Based on this, our new indicator variable, *Sensitive*, reflected whether a campaign fell into one of these four categories. The results of this estimation are reported in Table 8 (note that the main effect of campaign type is not identified in this estimation, because the value is static and thus collinear with the fixed effects). We observed that, as anticipated, our treatment effect was much stronger for sensitive campaign topics. Taken together, these results collectively provide support for the notion that publicity plays a central role in our treatment effect.

5. Additional Analyses and Alternative Explanations

We also considered alternative explanations for our results. These analyses, as well as the robustness checks that follow, are provided in the supplementary appendix. One seemingly likely alternative explanation for the observed positive effect of our treatment on conversion rates pertains to simplification of the user interface (UI). Specifically, we might be concerned that the increase in conversion rates was actually due

[7] We employ the log of absolute deviation to obtain percentage effects. We also include outlier contributions in this estimation, given that such observations contribute in large part to extreme donations in our sample.

[8] A complete list of campaign categories is provided in Table S7 of the supplementary appendix (available as supplemental material at http://dx.doi.org/10.1287/mnsc.2014.2069). Examples of less sensitive topics include video and Web, games, and food.

Table 7 Regression Results: Contribution Size and Information Hiding (Linear Probability Model with Fixed Effects; Dependent Variable Is *Binary Info Hiding*)

Explanatory variable	10%	5%	1%
Large	0.070*** (0.010)	0.128*** (0.012)	0.153*** (0.026)
Small	0.033*** (0.009)	0.043** (0.014)	0.075** (0.027)
Controls[a]	Included	Included	Included
Observations	37,328	37,328	37,328
F-statistic	2,022.83 (216,3581)	3.2e+09 (216,3581)	1.4+e09 (216,3581)
R^2	0.21	0.21	0.21

Notes. Robust standard errors are reported in parentheses for coefficients, clustered by campaign. The sample includes all converted visitors (i.e., those who contributed at least some amount of money).

[a]The treatment indicator, campaign-level fixed effects, day fixed effects, day of week fixed effects, browser language effects, browser type effects, etc., are incorporated in the estimation.

$^{**}p < 0.01$; $^{***}p < 0.001$.

Table 8 Regression Results: Topic Sensitivity (Linear Probability Model with Fixed Effects; Dependent Variable Is *Conversion*)

Explanatory variable	Coefficient
Treatment	0.044*** (0.007)
Treatment × *Sensitive*	0.090*** (0.033)
Controls[a]	Included
Observations	128,701
F-statistic	1.2e+08 (212,5077)
R^2	0.17

Notes. Robust standard errors are reported in parentheses for coefficients, clustered by campaign. The sample includes all visitors who entered the contribution flow.

[a]The same set of controls used in Table 3, column (4), are incorporated in the estimation.

$^{***}p < 0.001$.

to removal of a radio button from the prepayment contribution process, which could have simply streamlined the UI. However, this is unlikely to explain the observed effects for a number of reasons.

First, we explored the duration of time it took contributors to complete payment between our treatment and control groups. We found no evidence that the treatment group completed its payments more quickly ($t = -1.26$, $p = 0.21$).[9] This is important, because we would expect to see significantly shorter visit durations in our treatment group if reduced complexity and effort were to explain our results.

Second, we examined moderating effects associated with visitors' mobile device usage. The UI complexity explanation would suggest that our treatment effect should be amplified for mobile users, who should be more sensitive to UI changes because of the limitations of smartphone screen size, among other features. However, we find no evidence of a positive moderating effect. This result, reported in Table S1 of the supplementary appendix, runs directly counter to a UI complexity explanation.[10]

Third, and last, it is important to keep in mind that UI complexity is completely incapable of explaining the significant decline we observe in average contribution amounts with treatment. Taken in tandem, the above analyses and this last notable fact make it unlikely that UI complexity can explain our findings.

We next sought to delve deeper into the publicity effect. We considered that campaign organizers might contribute to their own campaigns, which we refer to as *self-contribution*. Noting this, we find it conceivable that the contribution effect we observed was largely attributable to campaign organizers' ceasing self-contribution in the face of publicity. To assess this, we constructed a binary indicator of self-contribution and regressed it on our treatment indicator, as well as our set of control variables. If our results were driven by campaign organizers' ceasing self-contribution, then we would expect our treatment indicator to have a significant, negative effect on the probability of any contribution being made by a campaign organizer. As reported in Table S2 of the supplementary appendix, we observed no evidence of this. It therefore seems unlikely that our results are due to a decline in the rate of organizers supporting their own campaigns.

6. Robustness Checks

We explored the robustness of our results in a number of ways. First, we considered the impact of outlier observations. We repeated our primary estimations excluding observations that fell within the top 5% of the distribution in terms of contribution amounts. We also repeated our estimations excluding observations

[9] This t-test was performed on logged visit duration to meet the assumptions of normality. This analysis also excluded outlier observations in terms of visit durations—namely, visits in excess of 1,500 seconds or 25 minutes. We exclude these observations because they likely represent visits where a browser window was left open and inactive.

[10] Note that we do observe a decline in visit durations, but they are not severe enough to produce statistical significance. We provide clustered histograms of logged visit durations comparing the treatment and control groups, as well as comparing mobile and desktop users, in in Figures S1 and S2 of the supplementary appendix.

associated with campaigns in the top 5% of the distribution of funding targets. Our results remained generally unchanged in both cases.

Next, we considered the use of alternative estimators. We explored both the conditional logit and probit estimators for our conversion model, and we considered fixed effects Poisson and negative binomial estimators for our contribution models. The results of the additional estimations for the treatment's effect on conversion are once again provided in Table S3 of the supplementary appendix. Similarly, the results we obtained using Poisson and negative binomial estimators for our conditional and unconditional contribution models are reported in Tables S4 and S5 of the supplementary appendix, respectively. In each case we report marginal effects. In all three cases, we see results that are consistent with those reported in our primary results.

We then reran our estimation using a subsample of our data, focusing only on converted visits among users who registered on the platform within the prior 24 hours.[11] The logic here was that new users should be unlikely to hold any expectations about the availability of information controls on the platform, and they should therefore be less likely to notice any changes in the website design. Repeating our conditional contribution estimation on this subsample of observations, we obtained the results reported in Table S6 of the supplementary appendix, which exhibit a roughly equivalent treatment effect. We can therefore be confident that our results are not driven by subjects' awareness of alternative conditions.

As a final validation of our results, we considered possible sources of heterogeneity in the treatment effect on conversion. First, we examined possible differences across campaign types that draw different average contribution amounts. We began by calculating average contribution amounts for each campaign type. We then constructed an indicator variable capturing whether a campaign was a "high-spend" category or not, based on whether the campaign was in the top half of this list. We then reestimated our linear probability model, incorporating an interaction between the high-spend indicator and our treatment indicator. Doing so, we found no significant effects. We then repeated this process based on median campaign contribution size and again observed no significant differences.[12]

[11] Because we can identify everyone, we are able to comprehensively determine the date on which they joined the platform.

[12] We also examined whether the treatment effect was attenuated when subjects arrived following an anonymous contributor (e.g., if such subjects anticipated eventual access to information controls, even when that access was delayed). However, we found no evidence of this.

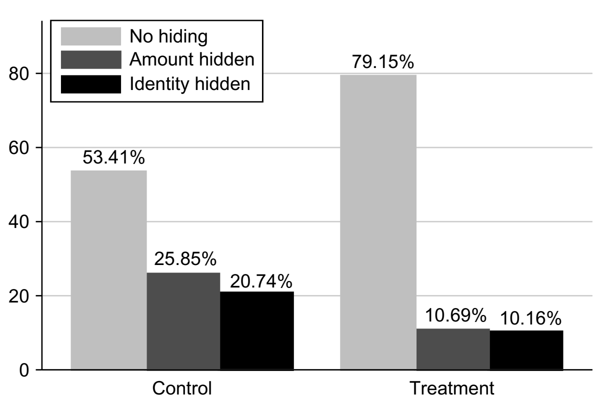
7. Manipulation Checks

Following the above, we undertook a manipulation check for our intervention, assessing shifts in the pattern of information-hiding mechanism usage between the pre- and postpayment conditions. Logically, delaying access to the information-hiding mechanism should drive a reduction in its use if our intervention is having the anticipated effect. As such, we looked for a general downward shift in the mechanism's usage in our treatment condition. As anticipated, the rate of information hiding was found to be much lower, indicating that our treatment did indeed have the desired effect. In particular, in the control condition approximately 47% of contributions involved information hiding compared with the treatment condition, where approximately 21% of contributions involved information hiding. These results are depicted graphically in Figure 7.

We next examined whether information hiding (and our treatment's effect on information hiding) depended on campaign characteristics. To examine this, we constructed campaign category dummies and interacted them with our treatment indicator. We then regressed a binary measure of information hiding on these various dummy interactions (note that we employed fixed effect estimators; thus the main effects of campaign type were not identified in this estimation).

We found that our treatment had a large, highly negative effect on hiding behavior, as we would expect from the model-free results above ($\beta = -0.279$, $p < 0.001$). However, we found no evidence that the effect was moderated by campaign category, with one exception: the video and Web category, where the treatment effect was significantly attenuated ($\beta = 0.106$, $p < 0.01$). Our suspicion is that this is because the baseline level of information hiding is already quite low for contributions toward projects in this category; thus the potential impact of the treatment is much lower to begin with. In particular, the

Figure 7 **Probability of Information Hiding by Type** ($N = 37{,}328$)



rate of information hiding in the video and Web category in the control condition is 0.33, yet the rate is 0.48 among all other categories. In fact, the next lowest rate is 0.41, in the theatre category. Next, we considered potential interactions between our treatment and the size of the project target. However, we again came to a similar conclusion: the main effect of treatment was comparable to that reported in our category type analysis ($\beta = -0.266$, $p < 0.001$), and the interaction effect, although statistically significant, was extremely small ($\beta = 6.46\text{e–}10$, $p < 0.01$). Moreover, when we reestimated this model-replacing project goal with its log, the interaction was completely insignificant. Given these results, it appears that our treatment effect is quite generalizable and does not depend heavily on the type of campaign being supported.

8. Managerial Implications

Our findings indicate that the results of past fieldwork might not tell the entire story when it comes to the impacts of privacy assurances and information controls on consumer behavior. Although numerous studies in the literature have employed laboratory and field experiments to evaluate these issues, generally reporting that these mechanisms increase customer information sharing and transaction likelihood, it is possible (even likely) that past results cannot account for changes in the volume or composition of the converted population that are likely to arise following modifications to a website interface.

Our results can inform crowdfunding stakeholders in a number of ways. First, the provision of information controls should be considered with care. Although it is likely that our results would generalize to other reward- and donation-based crowdfunding platforms, or perhaps even equity-based crowdfunding, this will depend heavily on a number of factors. The degree to which the platform enables transparency, reputation, and recognition is likely to be important, for example. Therefore, the design of the platform in this regard should be context dependent. One key factor to consider is the nature of the campaigns typically funded on a given platform. Potentially controversial campaigns are likely to induce greater cognizance and use of information control features. Firms operating platforms with sensitive content will therefore need to take greater care in the design and implementation of information controls.

Additionally, given that there is an inherent tension between enabling recognition for contributors and avoiding issues of privacy and publicity, platform operators and campaign organizers should consider supplemental approaches to mitigating privacy priming in the presence of information controls. For example, organizers might present privacy seals and other forms of reassurance alongside information control prompts. Campaign organizers might also offer recognition to contributors for large contributions by providing participatory rewards and recognizing contributors for their effort—e.g., awarding large contributors naming rights to products or thanking them for their participation on the company website—rather than tying recognition to the transaction itself. Contributors could then maintain obscurity by concealing contribution activity while still benefitting from recognition.

Campaign organizers might also offer the crowd an opportunity to participate and contribute via other effort-based avenues. Although some contributors might shy away from public monetary contributions, they might be willing to publicly partake in the campaign on an effort basis instead, by volunteering expertise or ideas. Notably, some platforms provide these options (e.g., Spot.us provides an option to "Donate Talent" to a campaign).

With regard to crowdfunding contributors, our work reinforces the prior finding in other contexts that individuals are often uncertain of privacy risks, and that these perceptions are largely driven by available cues (John et al. 2011). We have shown that the mere presence of information-related prompts can severely impact conversion rates and platform contributions.

It is also important to discuss potential limitations of our work. A key issue that arises here concerns user names and pseudonyms. It could be argued that crowdfunders can simply employ a pseudonym if they are really concerned about being observed. However, empirically, we have seen that more than one-third of contributions in our sample involve information hiding. This indicates that many crowdfunders do in fact place value on their user profile.

Moreover, this issue is more complex than it might appear at first glance. If users wish to accrue recognition for their actions, it is in their interest to incorporate aspects of their true identity into their user profile. Even for those users who do not do so, online personas tend to persist across transactions and interactions and thus can carry their own reputation (Dellarocas 2003). This kind of identity disclosure in online personas has been shown to have significant economic outcomes in electronic markets (Ghose and Ipeirotis 2011, Ghose et al. 2012). It is worth noting that reputation and recognition are both factors that have proven to be quite important in off-line venture capital, because high-profile and well-regarded investors are better able to drive follow-on investment (Hochberg et al. 2007, Sørensen 2007, Sorenson and Stuart 2001). Indeed, recent work in crowdfunding has found that expert contributors play a similarly key role in driving follow-on contribution in some markets (Kim and Viswanathan 2014). Moreover, other recent work has noted the role of campaign organizers' social embeddedness in the crowdfunding as a driver of fund-raiser success (Younkin and Kashkooli 2013), as well as the critical role of indirect reciprocity (Zvilichovsky et al. 2013).

9. Conclusion

Online spaces are characterized by increased visibility and traceability, and crowdfunding platforms, in particular, publicly record transactions, which include the identity or dollar amounts of campaign contributions. Financial transactions tend to be sensitive in nature; thus publicity and scrutiny may impede transactions. Bearing in mind these issues of visibility, many crowdfunding platforms offer transaction-level information controls so that contributors can decide what will be made publicly visible about their transactions.

Unfortunately, prompting users with information- and scrutiny-related questions can have detrimental effects. On one hand, prompts of this sort can prime users with privacy concerns. On the other hand, withholding these features could make privacy-conscious users less comfortable. With the above tension in mind, we have examined the effect of transaction-level information controls on the behavior of online crowdfunders. Employing a randomized field experiment, we considered the double-edged sword presented by the provision of these features during the course of the crowdfunder contribution process. We considered both positive effects (increased comfort and security) and negative effects (privacy priming). We find that delaying the presentation of these mechanisms increases conversion rates yet simultaneously lowers average dollar contributions.

Although we provide evidence suggesting that privacy priming and publicity effects drive these outcomes, future work can explore the role of mechanism design, wording, and presentation format. It is possible that one or both of these effects would be moderated by specific attributes of the mechanism, such as the wording of the text, the granularity of information-hiding options (e.g., providing an additional option of presenting a discretized "range" of the contribution, such as "$10–$20"), or the positioning of the mechanism in the user interface (Egelman et al. 2009).

It is also important to consider the contextual nature of these results and the degree to which they would generalize to other, noncrowdfunding contexts. It is possible that our results would not extend to a purchase context, where issues of social capital, reputation, etc., might be less pronounced. Further, in regard to the net positive outcome in contributions that we have observed, although users are given complete freedom here to specify the size of their contributions, thereby allowing for a shift in the distribution of contributions that can offset the decline in participation, we would observe that, when introducing a privacy control question in other contexts, engagement or contribution may not be up to the user.

To clarify, if transaction amounts are fixed (e.g., a transaction on Amazon.com that involves a product with a fixed price, a voting-type setup where voters may vote once and only once), then any decline in participation could not be offset by a parallel increase in contribution amounts. In that scenario, the impact of our intervention on participation and unconditional contribution would be strictly negative as a matter of course. This point highlights the fact that the impact of privacy control provision on user participation and contribution is contextual in a number of different respects, which need to be evaluated in tandem.

Our work shows the potential of large-scale in vivo randomized experiments to robustly estimate treatment effects around online user behavior, circumventing numerous threats to validity. The methods themselves are widely applicable to research in online contexts, which has ever-increasing relevance and practicality for numerous fields of study. Indeed, given the plethora of influences and information sources available to users in online settings, the complex, messy nature of these contexts means that endogeneity of effects grows increasingly likely. Randomized experiments thus appear to be the best course of action in achieving causal inference, going forward.

Supplemental Material

Supplemental material to this paper is available at http://dx.doi.org/10.1287/mnsc.2014.2069.

Acknowledgments

The authors are grateful to reviewers and participants at the 2013 Workshop on Information Systems and Economics (WISE 2013), the 2013 INFORMS Annual Meeting, the 2014 Crowds 2.0 Conference hosted at the NYU Stern Center for Business Analytics, the 2014 Academy of Management Annual Meeting, the 37th Annual NBER Summer Institute on the Economics of IT and Digitization, and the 2014 ZEW Conference on the Economics of Information and Communication Technologies. The authors also thank Sean Taylor for providing helpful comments. The authors acknowledge funding from the Kauffman Foundation [Grant 20140050].

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Zvilichovsky D, Inbar Y, Barzilay O (2013) Playing both sides of the market: Success and reciprocity on crowdfunding platforms. Working paper, Tel Aviv University, Tel Aviv, Israel.

CORRECTION

In the print version of this article, "The Hidden Cost of Accommodating Crowdfunder Privacy Preferences: A Randomized Field Experiment" by Gordon Burtch, Anindya Ghose, and Sunil Wattal (*Management Science*, vol. 61, no. 5, pp. 949–962), Figure 2 (p. 952) is incorrect; the "Treatment" and "Control" labels are in the wrong position. The figure has been corrected in the online version, and an erratum will be printed in the January 2016 issue.