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Personalization in Email Marketing: The Role of Noninformative Advertising Content

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Abstract. In collaboration with three companies selling a diverse set of products, we conducted randomized field experiments in which experimentally tailored email ads were sent to millions of individuals. We found consistently that personalizing the emails by adding consumer-specific information (e.g., recipient's name) benefited the advertisers. Importantly, such content is not likely to be informative about the advertised product or the company. In our main experiment, we found that adding the name of the message recipient to the email's subject line increased the probability of the recipient opening it by 20% (from 9.05% to 10.80%), which translated to an increase in sales' leads by 31% (from 0.39% to 0.51%) and a reduction in the number of individuals unsubscribing from the email campaign by 17% (from 1.2% to 1.0%). We present similar experiments conducted with other companies, which show that the effects we document extend from objectives ranging from acquiring new customers to retaining customers who have purchased from the company in the past. Our investigation of several possible mechanisms suggests that such content increases the effort consumers make in processing the other content in the rest of the advertising message. Our paper quantifies the benefits from personalization and sheds light on the role of noninformative advertising content by analyzing several detailed measures of recipient's interaction with the message. It provides external validity to psychological mechanisms and has clear implications for the firms that are designing their advertising campaigns.

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Keywords: advertising • experimental design • field experiments • advertising content • email marketing • personalization • advertising tailoring

1. Introduction

This paper empirically studies the effects of email ad *personalization*, such as including the consumer's name and her place of work. Specifically, we study the effect of content that is consumer specific (i.e., personalized); at the same time, we focus on content of this nature that does not provide any direct information about the advertised product.¹ The advertising literature (Resnik and Stern 1977) has shown more generally that the use of noninformative content is widespread. For example, consider a banner ad or a TV commercial for a drug. Several aspects of the ad, such as gender, ethnicity, and voice of the spokesperson, are aspects that might have little to do with the advertised product, but we do not know whether and how such content affects actual consumer decision making.

The role of noninformative content in an email marketing context deserves empirical investigation for multiple reasons.² First, email is an important marketing instrument because most businesses engage in it (Experian Marketing Services 2014), and email

marketers often have access to information about consumers, such as the recipient's name, that can potentially be incorporated into the marketing message to personalize it (Ansari and Mela 2003).³ Second, even if personalization is auxiliary to the proposition in the ad message, it can influence a consumer's response through mechanisms proposed in the psychology literature (Petty et al. 1983) that we discuss in Section 2.2. Since these mechanisms have different implications for advertisers, it is of interest to understand the specific mechanisms at play in a given context. Furthermore, by focusing on the types of personalized content we include in our analyses, we are also able to study the effects of noninformative ad content—a broader issue facing advertisers. In general, it can be difficult to argue that any given content is noninformative in nature; even though it may not provide direct information about the advertised product, it could serve as a signal of quality, thereby providing indirect information (Nelson 1974). Our context and the manner in which we experimentally vary the content limit such concerns.

Given the above motivation, we focused on the following empirical questions. Our first research question was, from the perspective of advertisers, does personalization of advertising messages by adding consumer-specific information affect consumer behavior and improve the campaign's performance? Furthermore, is its effect generalizable across firms and product categories? Second, from the perspective of broader understanding about the role of advertising, how can noninformative content in an ad affect consumer behavior? If it does have an effect, what could be the underlying mechanism driving it?

To answer these questions, we partnered with three firms operating in three very different markets that use email marketing for different purposes. Our main partner company (company C) is one of the biggest players in a niche market, selling online test preparation products in many parts of the world.⁴ The main objective of its email marketing is to acquire new customers. To assess the generalizability of our effects we replicated our main findings with two firms in different settings. First, we partnered with the largest online marketplace in South America, MercadoLibre. MercadoLibre uses its email marketing to engage with returning customers. A replication of our effect with MercadoLibre allowed us to generalize the findings (a) beyond a customer acquisition campaign, (b) with *experienced* customers of a well-known brand, (c) who have previously purchased from it, (d) have received personalized emails from the advertiser in the past, and (e) have likely already formed an opinion about the advertiser. In addition, MercadoLibre previously used the content we experimentally manipulate, and this experimental content was supplied by the consumers when they made their previous purchases. So we were able to examine the effect of removal of the experimental stimulus from a setting in which recipients are used to seeing it in their emails. Additionally, we partnered with Stanford's marketing team, which sends emails to engage individuals who are likely to be aware of and know about Stanford. This opportunity gave us the ability to further generalize our findings to a noncommercial setting, with a prestigious well-known brand. Together, across these contexts, we conducted a series of large-scale randomized field experiments that varied personalization of email messages sent to millions of individuals. To create variation in personalization, we focused on the basic information email marketers are likely to have access to (e.g., the email recipient's name). Table 1 lays out the flow of the experiments.

Given that our partner companies and other such marketers have access to the name of the recipient, we began in Experiment 1 by investigating the causal effect of the presence of a consumer's first name in the subject line of the emails they receive. Recipients in a randomly chosen treatment group received emails

Table 1. Flow of the Experiments in the Paper

Experiment 1	Show the existence of the main effect of noninformative personalization (name of the recipient)
Experiments 2 and 3	Show the generalizability of the results in Experiment 1 to other empirical contexts, collaborating with other companies
Experiment 4	Provide additional evidence of the mechanism driving the effects

that mentioned their first names in the subject line. Emails to the corresponding control group did not mention the first name in the subject line, but were the same as the treatment group in every other respect. The body of the email began by greeting the recipient using the recipient's name, for *both* groups. This equivalence ensured that the total content of information provided by the email was the same for any recipient who opens the email. Additionally, all email IDs in the company's mailing list were corporate email IDs that contained the recipient's name and the name of the company where she works combined in some form (e.g., Firstname.Lastname@company.com, FLastname@company.com, etc.). Therefore, it is reasonable to assume that the recipients in both groups were aware that the advertiser knew their names and those of their employers. We examined the effect of this additional mention of the recipient's name in the subject line on several outcomes. We considered outcomes that the marketer would want to increase, such as opening of the email and generation of sales' leads. We also considered outcomes such as unsubsubscription from the email campaign, which the marketer would want to decrease. Being able to track multiple outcomes in the purchase funnel sheds light on the underlying mechanism by which such ad content works.

We found that including the name of the recipient in the subject line of the email increased the probability of the recipient opening the email by 20%. This translated into a downstream 31% increase in sales' leads and a 17% decrease in the number of recipients unsubscribing from the email campaign. Our findings provide evidence for the effectiveness of personalization and for the role of noninformative content; i.e., that the effects are statistically and economically significant. The findings motivated our partner company to alter its default email strategy to include the recipient's name in the subject line of the email.

To assess the generalizability of our findings across other settings, we replicated this experiment at our other partner companies (MercadoLibre and Stanford) in Experiments 2 and 3. In both cases we found data patterns similar to the above. Accumulating evidence across Experiments 1–3 using the continuously cumulating meta-analysis technique (Braver et al. 2014), we

found that adding the recipient's name to the subject line increased positive outcomes such as leads and clicks and reduced negative outcomes such as unsubscribes from the emailing list. Additionally, to test whether these effects were specific to just this form of content (the message recipient's name), in another experiment with company C we experimentally included the name of the company where the recipient works in the body of the email for a randomly chosen treatment group. Similar to the previous experiment, we found that the treatment group had higher sales' leads and lower unsubscription rates.

Having shown the effect of personalization in a variety of settings, we examined the mechanism driving these effects. As noted previously, the email marketing context is well suited to such an undertaking because it allowed us to observe several details about the recipient's reaction to the ad message—whether the recipient actually saw the advertising message (opened the email), whether the recipient unsubscribed from the email list, etc. Therefore, even though our field experiments did not permit collection of process data such as eye tracking or thought protocols, we were able to make inferences about the psychological mechanism from observed open rates, sales' leads, and unsubscribe rates. In Experiment 4, we sought to understand which of the three psychological processes—personalized content (i) orients the recipient's attention towards the email, (ii) acts as a positive cue that directly increases the recipient's liking of the advertiser, (iii) increases recipients' elaboration on the content of the email—was reflected in our data. We manipulated personalization (by including the name of the recipient in the body of the message), and in addition, we manipulated message content. Our results were most consistent with the idea that personalization increases recipients' elaboration on the content of the email (mechanism (iii)). We found little support for the two alternative mechanisms operating in isolation.

This paper makes several contributions. First, it shows that personalization, even when it does not provide information about the product or company, can still benefit advertisers significantly. This finding sheds light on why advertising typically includes noninformative content. Previous studies in the economics and marketing literature, such as Bertrand et al. (2010) and Sudhir et al. (2016), have shown the importance of advertising content on a firm's outcome. Our paper complements that literature and extends it to the specific domain of content that is personalized but is not about the product being advertised. Second, it shows that the effect of such content on the efficacy of advertising occurs through increased processing of the advertising message and is not likely to be driven by changes in the consumer's innate utility function. This finding has implications for how advertisers and policy

makers think about the role of advertising (DellaVigna and Gentzkow 2010). Third, our inquiry into the mechanism also contributes to the consumer behavior literature. Lab studies have shown the existence of various mechanisms (see, e.g., Petty et al. 2013) by which personalized content could influence outcomes. We show their relevance to firms by testing for the prevalence of those mechanisms in field settings.

Our paper also contributes to marketing practice by quantifying the effect of personalization of ad content. Understanding the mechanism helps us offer recommendations on the design of advertising campaigns. Our partner companies benefited from our findings by modifying the way in which they incorporate consumer-specific information into their marketing campaigns at almost no extra cost of execution. As mentioned earlier, motivated by our findings, company C now adds the recipient's name to the email subject line by default. For companies such as MercadoLibre, that have previously used such a practice and that might be concerned about any negative fallout from the use because of privacy concerns in using this information, our results provide some reassurance by showing the benefits even after repeated use.

The rest of the paper is organized as follows. Section 2 discusses the literature related to our paper and how our paper contributes to it. Section 3 presents the main experiment, and our findings from it, in detail. We investigate the generalizability of our findings from the main experiment to other settings and other content in Section 4. Section 5 presents the final experiment that examines the mechanism underlying our effects in more detail. We conclude our paper in Section 6.

2. Relevant Literature

This paper relates to several streams of research on advertising in marketing. We categorize this research into (1) field studies on advertising content and (2) psychology-based theories and empirical research with lab data. In the rest of this section, we discuss the findings from past research and discuss our paper's relative contribution.

2.1. Empirical Research on Advertising Content

Previous research (Eastlack and Rao 1989, Lodish et al. 1995) has found that advertising is more likely to increase sales when the content of the ad changes. Closest to our work, a few studies have looked at the effects of specific aspects of advertising content on consumer decisions in a direct-mail context. Bertrand et al. (2010) conducted a field experiment in collaboration with a consumer lender, varied several aspects of the advertising mail (e.g., by mentioning uses of a loan), and found a jointly (marginally) significant effect of content. In the context of direct mail sent for charity donations, Sudhir et al. (2016) conducted a field experiment that varied

the framing of the message and found that monthly framing of the donation, including a story of an in-group person, yielded better outcomes. In the context of online banner ads, Lambrecht and Tucker (2013) and Bleier and Eisenbeiss (2015) study the effect of personalizing ads by displaying products already seen by the consumer.

Our research complements the above literature by studying the inclusion of the message-recipient's information in an ad message, which is often accessible to the advertisers engaged in direct response media. Unlike the above examples, the content we study is *not* about the product being sold in the ad, but is auxiliary to advertising messages across product categories. In this respect, our paper is close to Wattal et al. (2012), who related the use of the email-recipient's name in an email to the actions taken subsequently by the recipient. They found that personalization hurts the advertiser, which is the opposite of what we found consistently across a diverse set of contexts. A plausible explanation for the divergence of findings between the two studies is the difference in methodology. Wattal et al. (2012) compared the behavior of individuals selected to be sent personalized emails with the behavior of those not selected. This selection can lead to other inherent differences between the comparison groups, which can influence the effect being estimated. Although this possibility does not hamper a good description of observed data, addressing it is important for estimating causal effects and making recommendations, which is the objective of our paper. We overcame the selection problem by using randomization to get to a priori similar comparison groups. Methodologically, our paper is unique in empirical research on advertising for two primary reasons. We ran multiple experiments to show the generalizability of our findings across empirical settings, and also to distinguish between potential mechanisms that might be driving the effects.

Privacy concerns and personalization. Past research has shown that consumers' concerns about privacy violation can affect how they respond to advertising. In the context of social advertising, Tucker (2014) found that personalization of ads from a charity work better when consumers' perceived control over their data is increased because of a platform-wide policy change. Goldfarb and Tucker (2011a) found that consumers respond negatively when targeting of ads and their obtrusiveness both increase across campaigns, and they suggested that this effect is likely to be caused by privacy concerns. Goldfarb and Tucker (2011b) showed that privacy-related regulation impacts the effectiveness of advertising for websites that might benefit from using consumer data. Lab studies have also found evidence of negative consumer reactions to personalization (e.g., White et al. 2008). Goldfarb and Tucker (2012)

and Tucker (2012) discussed the implications of privacy concerns for advertising and data-based marketing.

Our paper contributes to the literature by showing that consumer information can cause a significant increase in the effectiveness of advertising and by providing evidence for an underlying mechanism driving the effects. Adding consumer-specific content to email advertising could, in principle, raise privacy concerns and decrease the effectiveness of the campaign. For example, mentioning one's name in an email might concern a consumer who perceives it as a violation of privacy. Such a concern could lead to a negative reaction toward the advertiser reducing the consumer's propensity to buy the advertised product. However, our empirical evidence shows the opposite—a positive effect of adding consumer-specific information on the campaign's performance. Two factors can explain our finding. First, both the control and treatment groups in our experiments received emails, and the content of the emails was very similar (they are exactly the same except for the experimental stimulus). Therefore, any privacy-related concerns emanating from receiving an email, and the bulk of its content, were controlled for when measuring our effects. Second, our experimental stimuli were designed to not change the consumer's impression of the advertiser, including the perception of the information the firm has access to. For example, adding the recipient's name in an email that already addresses the recipient by the recipient's name (in addition to the email ID having the name in it) is not likely to raise concerns (Experiment 1). Similarly, seeing one's name in an email from a company is not likely to raise concerns if the name was given voluntarily by the recipient in the past (Experiments 2 and 3).

2.2. Psychology-Based Research

In this section, we review research and theory on how personalization could affect people's attitudes and behavior. The manipulations used in our experiments concern personalization without adding information. Specifically, our manipulations involve altering the communication to include the recipient's name or place of work. Such noninformative personalization is the most basic form of tailoring. Unlike some forms of tailoring that alter message content (i.e., the arguments in the message) to more effectively persuade the recipient (see, e.g., Dijkstra 2008 for an overview), the personalization we studied provides no information that is objectively, by itself, relevant to persuasion. As Dijkstra (2008) put it, "Personalization refers to incorporating recognizable aspects of a person in the content information. This could be one typical feature, such as the target person's first name or it could be a set of features that in the specific configuration also has a similar probability to refer to the individual. Personalization

typically provides no persuasive content information; in isolation, the personalization items have no persuasive power” (p. 768). Likewise, Hawkins et al. (2008) noted, “Tactics for personalization need not be directly linked to the behavioral outcomes of interest, nor do the resulting tailored messages need to provide the recipient with new information about himself or herself” (p. 458). Although personalization is by itself not informative, it could affect persuasion in multiple ways (see Dijkstra 2008; Hawkins et al. 2008; and Petty et al. 2000, 2009 for overviews of possibilities). Below, we provide a brief overview of three ways personalization could affect the efficacy of email marketing and indicate how patterns of results on our outcome measures would point toward the likelihood of one or more of these possibilities operating.

2.2.1. Potential Behavioral Mechanisms. *Orienting Attention.* One way personalization could affect the success of email marketing is by drawing attention to the email itself. This is because self-associated information attracts attention. For example, people have been shown to automatically orient their attention to their own name in distraction-rich environments (Wolford and Morrison 1980, Cherry 1953, Tacikowski and Nowicka 2010). Even infants as young as six-months old will orient to the sound of their name embedded in a field of noise (Newman 2005). Because people naturally orient toward information relevant to themselves, an email with one’s own name (or the name of the company where she works) could be more likely to be noticed and read. This orienting attention mechanism does not involve changing how the email is processed; rather, it merely affects the likelihood that the email is noticed and read at all. This mechanism is most likely to be operable when people are unlikely to notice or read the email by default.

Serving as a Positive Cue. Although personalization, as implemented in the present studies, does not provide any information about the (marketing) company or offering, psychological theories suggest that it could still shape opinions toward the offering and affect resulting behavior. One way it can do so is by acting as a positive cue. Put simply, people like things associated with the self. For example, previous research has shown that people are more persuaded by persuasive messages that match aspects of their personality or identity (Perloff and Brock 1980, Reed 2004). People value objects they own more than others do (Kahneman et al. 1990), and they even prefer letters from their own names, relative to others (Nuttin 1985). Because most people have favorable attitudes and feelings toward themselves, the presence of information related to themselves could also activate favorable effect that then transfers to the advertised product (see, e.g., Staats and Staats 1958). Hence, the presence of identity-relevant information in a communication

could serve as a positive cue to liking and thereby exert a direct and positive effect on attitudes. According to dual-process models of persuasion such as the elaboration likelihood model (Petty and Cacioppo 1986a), this peripheral cue-based persuasion is more likely to operate when people lack either the motivation or ability to elaborate and hence are not thinking carefully about the communication. That is, rather than relying on careful scrutiny of the communication, the recipient could act on the basis of the positive effect generated by the association with the self. In terms of the mechanisms discussed in the economics literature, the serving as a positive cue mechanism relates to the utility-shifting effect of advertising. DellaVigna and Gentzkow (2010) call this the “preference-based effect of advertising.” Bagwell (2007) refers to it as the “persuasive effect of advertising.” This mechanism has clear implications. For example, if advertising content increases a consumer’s liking of a product, the firm selling the product gains market power and can potentially charge a higher price.

Increasing Elaboration. A third way personalization could affect our outcome variables is through increasing elaboration of the communication. In contrast to the peripheral cue mechanism, the increased elaboration stemming from personalization would not directly affect liking of the offering, but rather affect liking of the offering via its effects on the extent to which the communication is carefully processed. Based on how compelling the communication is, this increased elaboration could either increase or decrease evaluations and associated behavior. Personalization could affect elaboration because it makes recipients perceive the communication as being relevant for them. As Hawkins et al. (2008) wrote in a review of tailored communications, “by saying or implying that the communication is ‘for you,’ personalization strategies call attention to behavioral information or make it seem more relevant and meaningful to the recipient” (p. 458). The linkage to the self can increase the message recipient’s involvement with the communication. As Petty and Cacioppo (1990) put it, “The critical aspect of issue involvement is that the topic of the message is perceived as important *to the self*. That is, a message perceived as relevant to *my* value of freedom, *my* goal of obtaining an education, *my* values, goals, siblings, or possessions” (p. 368, emphasis in original). Accordingly, increasing perceived self-relevance is a well-established means of increasing message elaboration (Petty et al. 1983, Wheeler et al. 2005). According to the elaboration likelihood model, variables such as personalization are most likely to determine the extent of elaboration when baseline elaboration likelihood is not constrained to be high or low (i.e., is moderate). Hence, we would expect personalization to operate through this mechanism when people are determining whether

or not they wish to think carefully about a communication. If this mechanism is operational, the advertiser would want to use the personalized content only when the proposition in the ad message contains strong and compelling arguments. High (versus low) elaboration of weak or specious arguments can result in reduced persuasion (Wheeler et al. 2005, Petty et al. 1983), something marketers would obviously prefer to avoid.

2.2.2. Inferring Psychological Process. All three of the above mechanisms can operate under some conditions. In the present research, we sought not just to see whether personalization could increase sales but also to understand the process by which it does so in our particular context. That our studies were field experiments necessarily limited our ability to collect process variables. Nevertheless, these three processes predict different patterns of results on our outcome variables, and we can use these patterns to shed light on the likely operating mechanism. Consider our first outcome variable, opens. All three of our mechanisms are consistent with more opens of personalized emails. The orienting attention mechanism clearly predicts that personalization is likely to attract attention and therefore increase the likelihood that the email is opened. The serving as a positive cue mechanism can also predict more opens. Assuming that people are more likely to open emails for things one feels more positively about, emails with personalization are more likely to be opened because the personalization makes recipients like the sender more. Last, the increasing elaboration mechanism can also predict the likelihood of opening the email. Because personalization increases the feeling that the communication is relevant to the recipient, it can increase the likelihood that the recipient will open the email.

The mechanisms diverge in their predictions for the other two outcome variables. The orienting attention mechanism merely predicts that the email is more likely to be noticed and opened. If the only effect of personalization is to increase opens, one would expect that, all else being equal, unsubscribes should also increase (i.e., unsubscribing requires first opening the email). Hence, an orienting attention mechanism would predict an increase in the probability of a message recipient unsubscribing. The other two mechanisms would predict different patterns. The serving as a positive cue mechanism suggests that personalization directly increases liking of the offering, which would suggest that unsubscribes could decrease, even if open rates were higher. The increasing elaboration mechanism makes a somewhat more nuanced prediction. Because this mechanism involves paying more attention to the content of the communication, effects on outcomes such as unsubscribes or leads should depend on the content of the email. Assuming the content is reasonably compelling (companies are unlikely

to send emails portraying their company negatively), this mechanism also predicts decreased unsubscribe rates.

Note that a key differentiator between the serving as a positive cue and increasing elaboration mechanisms is the extent to which message content matters in determining attitudes and subsequent behavior. The serving as a positive cue mechanism suggests that personalization increases liking directly, and hence is not dependent on the content of the communication. Specifically, “a simple cue in the persuasion contexts affects attitudes in the absence of argument processing... some cues will do this because they trigger relatively primitive affective states that become associated with the attitude object. Other cues work, however, because they invoke guiding rules or inferences... Since cues are most likely to operate when subjects are either unmotivated or unable to process issue-relevant arguments... the data show a strong main effect for the cue treatment, but little effect for argument quality” (Petty and Cacioppo 1986b, pp. 33–35). The increasing elaboration mechanism, by contrast, suggests that personalization can increase favorability, but it can also potentially decrease favorability if the content of the communication is sufficiently specious. Hence, the increasing elaboration mechanism involves shifting how much people think carefully about the persuasive communication. That is, it “either motivates or enables subjects to see the strengths of cogent arguments and the flaws in specious ones” (Petty and Cacioppo 1986b, p. 36). With an increasing elaboration mechanism, the increased elaboration heightens sensitivity to the strength of arguments in the rest of the message. To distinguish between these two mechanisms, we manipulated the content of the communication in Experiment 4. Manipulating the strength of the arguments in a message has been used for decades in psychology to draw inferences about the extent to which people are carefully processing a persuasive communication (direct measurement of elaboration is difficult even in a lab setting; see Petty and Cacioppo 1986a). The basic idea is that, “if a manipulation enhances argument processing in a relatively objective manner... a message with strong arguments should tend to produce more agreement when it is scrutinized carefully than when scrutiny is low” (Petty and Cacioppo 1986a, p. 138). Accordingly, “Because greater elaboration is associated with greater scrutiny of information relevant to the attitude object, the level of elaboration in a group has also been inferred from the extent to which strong (compelling) reasons in support of a position are more effective at persuading people to support the position than are weak (specious) reasons” (Wegener et al. 1995, p. 464; see Petty et al. 1992 for a review). Hence, because increasing elaboration increases sensitivity to the information in the communication, one can

infer the level of elaboration from the extent to which people are responsive to shifts in the strength of the communication.

Accordingly, Experiment 4 introduced a manipulation of message content, specifically whether the potential of a discount was stated in the communication. Because a discount can be considered a compelling argument to consider the product, inclusion of a discount possibility in the communication should increase favorability toward the offering, but it should do so only to the extent that people are carefully processing the message. This manipulation therefore allowed us to distinguish between the serving as a positive cue and increasing elaboration mechanisms. The serving as a positive cue mechanism suggests that personalization should directly increase liking, independent of message content. This mechanism therefore predicts that personalization increases favorability toward the offering regardless of the presence of a discount. The increasing elaboration mechanism, by contrast, suggests that personalization increases sensitivity to the content of the communication. This mechanism therefore predicts that personalization increases favorability toward the offering more when the email mentions the discount. This would therefore be reflected in more leads and fewer unsubscribes

when the email is both personalized and mentions the possibility of a discount.

3. Experiment 1: Main Experiment

Our main partner company is a prominent player in a niche market. It sells online and offline training programs for preparation of standard tests like the Chartered Financial Analyst (CFA) and the Certified Public Accountant (CPA). To preserve the anonymity of our partner company we cannot disclose its name. We refer to this company as company C. Its main target market comprises working professionals looking to improve their skills by taking certification tests. Their products are expensive; they are priced in the order of \$1,000.

The main objective of company C's email marketing is to acquire new customers, who are working professionals in various parts of the world. Specifically, email marketing is used to generate "sales' leads" (or simply, leads). An individual replying to an email with an intent to make a purchase, after knowing the price, is called a sales lead. These leads are forwarded to the sales team who then sells the products to the individuals. Sales' leads are valuable for the organization—the management puts a value of \$100 on one sales lead.

The content of the email ad message sent by the company varies by product. Figure 1 shows a typical email

Figure 1. A Typical Email Message Sent by Our Main Collaborator Company (C)

Subject: Learn Financial Modeling & Business Analytics from Industry Experts in Sydney
Dear %%First Name%%:

Greetings,

This e-mail is in regards to our upcoming Financial Modeling 4 Days Intense Classroom Training in Sydney this month.

This course will help you ...

Our offerings include:

- 4 days of classroom training
- ...
- 1 year online training (accredited best content)

Course Outline and Learning Outcomes:

- Techniques, tips & tricks to use Microsoft Excel to build financial models
- ...
- Macros and VBA to implement Monte Carlo Simulation

Standard Course Price: USD 1197

Register on or before 25th October 2014 to avail USD 1077 (Exam fee included).

Workshop Dates: 15th, 16th, 22nd, 23rd November 2014

I apologize if this course is not of your interest. You can easily reply back to this e-mail or click on the unsubscribe link below.

Best Regards,

%%Sender's Name%%

[Unsubscribe me from this list](#)

message. It mentions the product, its detailed features, and its potential benefits to the consumer. Importantly, it also mentions the price of the product. Toward the end of the message it allows the recipient to unsubscribe from the emailing list and avoid receiving emails from company C in the future. This feature is typical of email marketing messages. The list of email IDs used in company C's campaigns are obtained through purchase or curated in-house from public sources (and not provided by consumers, unlike the other contexts we study in later experiments). All email IDs in company C's list are corporate email IDs and comprise the name of the person in some form (e.g., first-name.lastname@company.com, or the first name initial combined with the last name @company.com). This feature of the data set is a consequence of the way the list of email IDs is compiled by company C.

3.1. Experiment Design

Before our first experiment, the email messages from company C mentioned the recipient's name in the beginning of the body of the email message (e.g., "Dear Jack"). Our first experiment tested the value of repeating this information by including the recipient's name in the email's subject line. A total of 68,088 email IDs were randomized into the following two conditions.

- *Control group.* Recipients in this group were sent emails in the typical format used by company C. The names of the recipients were not mentioned in the subject line.

- *Treatment group.* Recipients were sent emails with their names added to the subject line. Specifically, the name was appended to the beginning of the subject. For example, suppose the subject line in a control email to a person named Jack Smith was "Learn Financial Modeling from Industry Experts." Then the subject line for the corresponding treatment group was "Jack, Learn Financial Modeling from Industry Experts."

The only difference between the emails received by the treatment and control group was that the subject line mentioned the recipient's name in the treatment group but not the control group. The rest of the marketing campaign, the number of emails, and other content, remained exactly the same.

The experimental addition to the message sent to the treatment group qualifies as personalized noninformative content. Why? First, one's name is known to be relevant to a person (Allport 1937). Second, the recipient's name does not convey any information about the product being sold by the advertiser. Therefore, adding it would not change the understanding about the product or the message. Furthermore, the ad messages were delivered through emails, which are individual specific. Also, all email addresses in our context contained the recipient's name in some form. Therefore, as noted above, one might assume that all of the

recipients in our experiment were already aware of the message-sender's knowledge of their names. Third, the name of the recipient was mentioned in the beginning of the email message (the body of the email) for both groups. Therefore, we just varied the *additional* mention of the recipient's name. This makes it very likely that the experiment varied noninformative content of the message.

3.2. Empirical Findings

We examined the effect of a random allocation of an email ID to a condition in which the recipient's name was included in the email's subject line. We observed multiple dependent measures: first, the number of sales' leads generated, which was the main dependent measure of interest for company C. We also observed the instances in which the recipient clicked on the link to unsubscribe from the email campaigns, indicating no interest in making a purchase. This gave us an estimate of the recipient's negative response. In addition, we observed the number of emails that were opened in each condition. This measure is widely used in email marketing because it gives a measure of a consumer reading an ad message, which is relevant. It is also a useful measure from a researcher's perspective because it provides a measure of actual "exposure" to an ad, which is hard to obtain for most other advertising channels. However, we note that this measure is noisy and dependent on the software used by the email recipient.⁵ The company aims to maximize opens and leads, while minimizing unsubscribes.

In our results from all of the experiments, we report the mean outcomes in the experimental conditions and standard errors around the means. We use these estimates to gauge the quantitative significance of the difference in outcomes. We also statistically test whether the means change because of the experimental stimuli and present the p -values using t -tests allowing unequal variance across experimental conditions. We use these p -values to interpret the statistical significance of the experimental stimuli. Additionally, we present standardized effect sizes (Cohen's d) so readers can compare the effects we estimate with other settings.⁶

Table 2 shows the results from Experiment 1. Column 1 shows that the probability that an email is opened increased by 20%, from 9.05% to 10.80% when the name of the recipient was included in the subject line (p -value < 0.01). If the treatment group had the same propensity to buy from the advertiser as the control group, conditional on opening the emails, then we would expect some increase in the leads and the number of unsubscribes. Note from column 2, that 0.39/9.05 of the consumers generated a lead after opening in the control group. Therefore, without looking at the estimates for the treatment group, we would expect the

Table 2. Results from Experiment 1—Company C Adding the Recipient's Name to the Subject Line

	(1) Opens/Sent		(2) Leads/Sent		(3) Unsubscribes/Sent		(4) Leads/Opens		(5) Unsubscribes/Opens	
	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
Mean (%)	10.80	9.05	0.51	0.39	1.00	1.20	4.72	4.29	9.24	13.28
Standard error (%)	(0.15)	(0.16)	(0.04)	(0.04)	(0.05)	(0.05)	(0.35)	(0.36)	(0.48)	(0.61)
Number of observations	33,322	34,766	33,322	34,766	33,322	34,766	3,599	3,146	3,599	3,146
<i>p</i> -value	<0.01		0.02		0.01		0.39		<0.01	
Effect size (Cohen's <i>d</i>)	0.06		0.02		−0.02		0.02		−0.13	

Notes. The table presents results from Experiment 1 in which the treatment group received the same emails as the control group, except that the subject line mentioned the recipient's name. For each experimental group we report (a) averages across individuals, (b) standard errors (in parentheses), (c) *p*-value testing whether the means are equal across the two conditions, (d) the number of observations, and (e) effect size in terms of Cohen's *d*. Column (1) shows the open rate (opens/sent). It shows that the treatment condition has a higher opens/sent relative to the control condition, and this increase is statistically significant. Column (2) compares the likelihood of a sales lead (leads/sent) across the two conditions and finds that the leads are also higher in the treatment condition. Column (3) shows that the unsubscription rate (unsubscribes/sent) is lower in the treatment condition. That is, the experimental treatment causes more people to open the emails and generate leads, but fewer people unsubscribe from the campaign. Columns (4) and (5) compare leads and unsubscribes conditional on opens and show that conditional on opening the email, unsubscribes reduce but the leads remain statistically similar.

lead rate would increase to 0.47% ($0.39/9.05 \times 10.8$) because of the increase in the emails opened. Similarly, we would expect the unsubscription rate to increase to 1.43% ($1.20/9.05 \times 10.8$).⁷ However, columns 2 and 3 in Table 2 show that the lead rate increase was larger, 0.51% (*p*-value = 0.02), and the unsubscription rate decreased to 1.00% (*p*-value = 0.01) instead of increasing. Columns 4 and 5 in Table 2 show that unsubscribes conditional on opening the emails are lower in the treatment condition compared to the control condition (*p*-value < 0.01) and lead conditional on open are higher in the treatment condition, although this increase is not statistically significant (*p*-value = 0.39). Note that the effect sizes (Cohen's *d*) are of the order of 0.01, which is considered "small." However, it is known that, in general, ad impressions have small effect sizes. For example, Lewis and Rao (2015) note that in their representative campaign a 25% return on investment would imply an effect size of 0.0047.

Are these effects economically meaningful? The costs incurred by company C in adding the recipient's name to the subject line were close to zero. On the other hand, the gains were significant. Within this experimental campaign, 458 more recipients opened their email and were exposed to their selling proposition in the treatment condition relative to the control. They generated 35 extra leads, which is valued at \$3,500 by the company. Eighty-five fewer people unsubscribed from the campaign, which increased the audience for future marketing campaigns. In addition to the immediate returns in this campaign, the improvement in the numbers suggests that the perception of the company's marketing effort improved because of the experimental variation. Given these considerations, after this experiment, company C decided to add the recipient's name in the subject line in all of their future email communications.

3.3. Mechanism Driving the Effects

Our findings from Experiment 1 in Table 2 provide some evidence about the underlying mechanism. Specifically, that the addition of the recipient's name to the subject reduced the chances of unsubscribes tells us that seeing the name in the subject line affects the consumers' behavior, beyond just making them open the email. Our findings also showed that the leads conditional on opening the emails increased. This supports the proposition that seeing the name in the subject line has an effect beyond just making the individuals open the email. This is inconsistent with the possibility that orienting attention is the only mechanism at play. Either or both of the other two mechanisms are playing a role.

Can this finding be explained by selection? One might think of an alternative explanation in which the recipient's name causes a selected set of people to open the email, who may have different preferences compared to people who would have opened the email anyway. This mechanism can explain differences in leads/opens and unsubscribes/opens, but not a decrease in unsubscribes/sent (i.e., the total number of unsubscribes) when the name is added. To fix ideas, suppose 10,000 emails are sent in the treatment and control condition. One thousand people open the email in the control condition, and 100 unsubscribe (unsubscribes/sent = 1%). Also, these 100 people are unaffected by the name. The treatment causes 200 extra people to open (so 1,200 people open in the treatment condition). If the people who open because of the experimental treatment (200) have different preferences, and have a very low propensity to unsubscribe, the number of unsubscribes will remain 100. On the other hand, if they have a very high propensity of unsubscribing, the number of unsubscribes will go up to 300 (=100 + 200). Therefore, unsubscribes/sent will only increase (i.e., they have to

lie between $1\% = 100/10,000$ and $3\% = 300/10,000$), contrary to what we see in the data.

In addition to the potential behavioral mechanisms considered above, adding the recipient's name could also potentially affect the likelihood of the email being classified as "spam" by the email servers. This could increase the likelihood of the consumers seeing the emails and responding to it if the name decreases the likelihood of the email being classified as spam. This possibility, like orienting attention discussed above, predicts an increase in the probability of the recipient opening an email, but not our finding of the total number of unsubscribes decreasing. Therefore, this mechanism is unlikely to be driving our effects. Furthermore, our perusal of websites that host practitioner forums on this topic had no posts that suggested that personalization, such as ours, affected spam classification.

4. Generalizability of the Findings

Experiment 1 showed that personalizing email messages benefited company C's email advertising campaign aimed at acquiring new customers, and that the effects were statistically and economically significant. The potential underlying psychological mechanisms should, in theory, generalize to other contexts as well. To support this assertion empirically, we then tested the generalizability of the findings (i) across settings (beyond a niche market company such as company C) with varying campaign goals (customer retention rather than acquisition) and (ii) to different (experimentally controlled) message content (beyond the name of the recipient). If personalization were informative (i.e., if it affected consumers' beliefs about the product), it would be unlikely to have any effect when consumers knew the advertiser well, had well-formed beliefs, and had purchased from the advertiser in the past. Therefore, showing that personalization operates in these contexts would provide additional evidence that the personalization manipulations in these studies were noninformative but effective. In looking at the results from these experiments, it is also important to keep in mind that scenarios involving customer retention are targeted toward existing customers who may have seen similar marketing emails in the past from the advertiser, and might have higher baseline response rates and lower unsubscribe rates. Consequently, it was unclear a priori whether the experimental stimulus would have a detectable and significant effect on the outcomes of interest in those scenarios.

4.1. Generalization Beyond the Current Context

Our partner companies were (a) MercadoLibre and (b) Stanford University. These two organizations provide empirical settings very different from our main collaborator company. We discuss these differences before describing the specifics of the experiments.

Experiment 2 with MercadoLibre. MercadoLibre is a large e-commerce website selling a wide variety of products in 13 countries. It is the largest online seller in Latin America with hundreds of millions of dollars in annual revenues (MercadoLibre.com). It spends tens of millions of dollars on online and offline marketing. Unlike company C, which sells a niche product, MercadoLibre has a wider appeal and is well known in its market. Its target market comprises a general audience rather than working professionals. Furthermore, the email campaign we focus on with MercadoLibre is aimed at *re-engaging old customers* (unlike C's campaign that aimed to acquire new customers). Consequently, the names and email IDs used in MercadoLibre's campaign are provided by the consumers themselves. Also, prior to collaborating with us, an email in MercadoLibre's campaign included the recipient's name in the subject line. An example email from MercadoLibre is shown in Figure 2.

Experiment 3 with Stanford University newsletter. Stanford's marketing team sends monthly newsletters to spread awareness about the latest research, and also about the executive education programs offered at the business school. This setting comprises a very well-known university attempting to engage a target market that is familiar with the advertiser. Therefore, it is significantly different from both company C and MercadoLibre, and can help us further gauge the limits of our findings. An example email newsletter from Stanford is shown in Figure 3. Like MercadoLibre, and unlike company C, the email IDs and names are provided by the email recipients themselves.

The design of Experiments 2 and 3 was similar to Experiment 1. Email IDs were randomly divided into a treatment group and a control group. The control and the treatment emails were exactly the same, except the subject line of the treatment emails had the recipient's name appended in the beginning. In the case of MercadoLibre, the treatment email was the usual email they send, and the control email was the one with the name of the recipient removed from the subject line.

Unlike company C, the main outcome of interest for both MercadoLibre and Stanford marketing is whether the recipient of the email clicks on a link in it. This measure, referred to as a "click," is commonly used in online marketing. It suits their specific objective of engaging individuals and drawing them to their websites, rather than selling specific products. Therefore, we focused on three dependent measures: whether the recipient (a) opened an email, (b) clicked on a link in the email, and (c) unsubscribed from the mailing list.

4.1.1. Findings from Experiment 2. The scale of MercadoLibre's campaign allowed us to draw a large sample size. A total of 1,111,130 emails were sent as a part of Experiment 2. Table 3 shows the results from this experiment. Column 1 shows that the probability

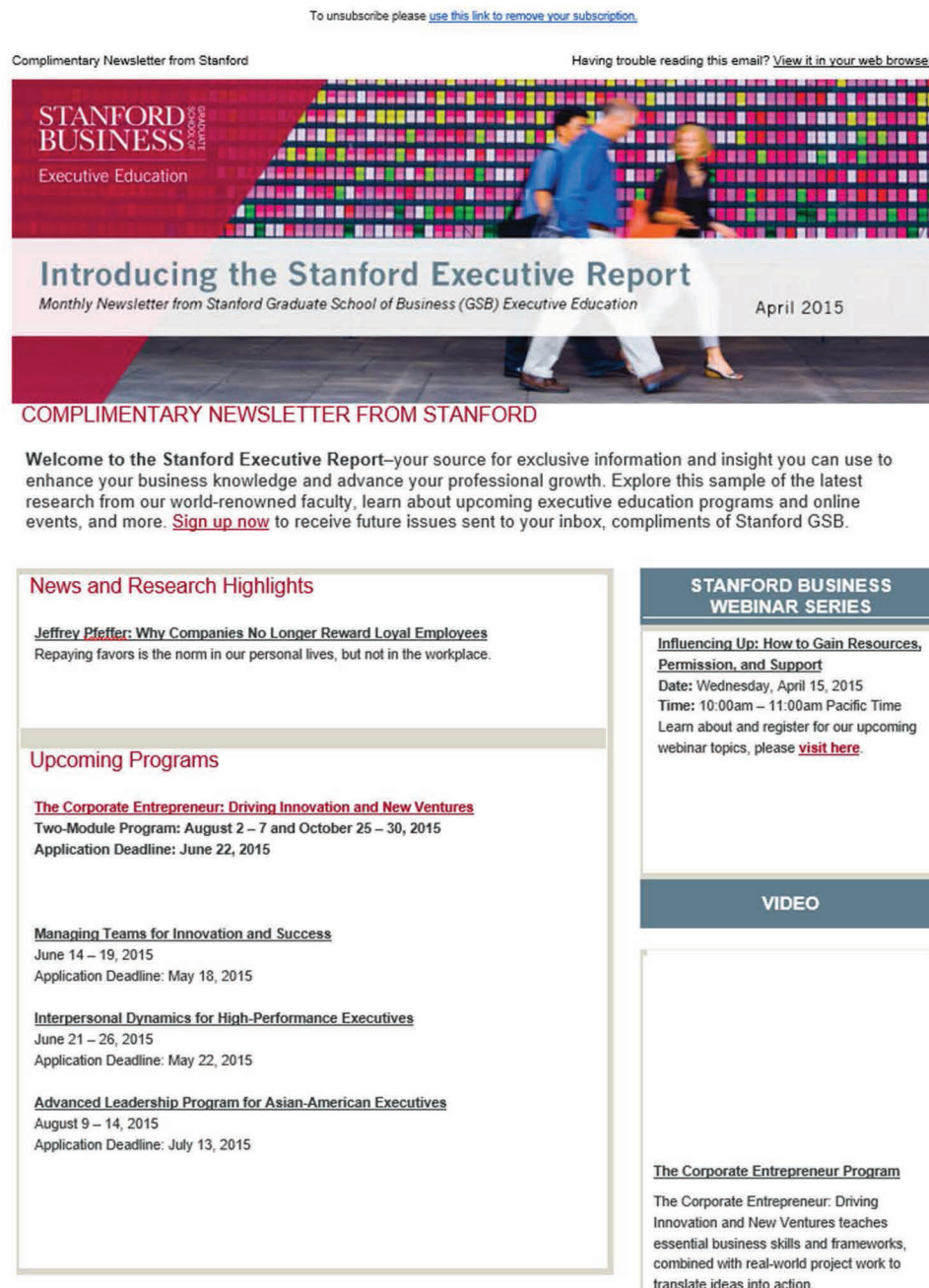
Figure 2. (Color online) An Example Email Message Sent by MercadoLibre

of opening an email increased from 21.78% to 23.07% when the name of the recipient was added to the subject line. This is a 6% increase (p -value < 0.01), leading to 7,177 more recipients opening their emails in the treatment group. If the treatment group had the same interest in MercadoLibre as the control, then conditional on opening the emails, we would expect an increase in the clicks and the number of unsubscribes. The clicks would increase from 2.10% in the control group to 2.22% ($= 23.07/21.78 \times 2.10$) and the unsubscribe rate would increase from 0.055% to 0.058% ($= 23.07/21.78 \times 0.055$). However, point estimates in columns 2 and 3 of Table 3 show that the click-rate increase was larger (p -value < 0.01), to 2.25% (a 7% increase, leading to 859 more clicks), and the unsubscribe rate was *lower* in the treatment condition; it is 0.049%, which is 11% lower than the control

condition (38 fewer recipients unsubscribed, p -value $= 0.17$). These findings are directionally similar to those of Experiment 1. They indicate a similar underlying mechanism. Seeing the name in the subject line affected consumer behavior beyond just making them open the email. Indeed, our estimates in columns 4 and 5 show that the clicks conditional on opening the emails were higher in the treatment condition (p -value $= 0.27$) and unsubscribes conditional on opening the emails were lower (p -value $= 0.04$).

4.1.2. Findings from Experiment 3. Table 4 shows the results from Experiment 3. Note that the sample size ($N = 5,000$) with Stanford was significantly smaller relative to the other experiments. Therefore, we expected this experiment to have lower power. Column 1 shows that the likelihood that an email was opened increased significantly from 12.8% to 15.8% (a 23% increase

Figure 3. (Color online) An Example Email Sent by the Stanford Business School



of 73 more opens, p -value < 0.01) when the name of the recipient was included in the subject line. If the treatment group had the same interest in Stanford marketing as the control group, conditional on opening the emails, then we would expect the click rate to increase from 1.48% in the control group to 1.83% ($= 15.8/12.8 \times 1.48$) and the unsubscribe rate to increase as well. However, similar to the other two experiments, columns 2 and 3 show that the click rate increased by a larger amount, by 32% (12 clicks) to 1.96% because of the treatment, and the number of unsubscribes decreased rather than increase. Because

of the low power in the experiment, the effect on click and unsubscribe rate was not estimated precisely enough to be statistically significant. However, this finding is consistent with the proposition that seeing the name in the subject line has an effect beyond just making the individuals open the email.

4.1.3. Meta-Analysis of Findings from Experiments 1–3. To understand the cumulative effects of our findings thus far, we meta-analyzed the findings from the three experiments. We applied the standard methodology used in analyzing multiple studies with binary variable outcomes (see Higgins and Green 2008 for

Table 3. Results from Experiments 2—MercadoLibre Removing the Recipient's Name from the Subject Line

	Experiment 2									
	(1) Opens/Sent		(2) Clicks/Sent		(3) Unsubscribes/Sent		(4) Clicks/Opens		(5) Unsubscribes/Opens	
	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
Mean (%)	23.07	21.78	2.25	2.10	0.049	0.055	9.75	9.62	0.21	0.25
Standard error (%)	(0.06)	(0.06)	(0.02)	(0.02)	(0.003)	(0.003)	(0.04)	(0.04)	(0.01)	(0.01)
Number of observations	555,541	555,589	555,541	555,589	555,541	555,589	128,187	121,010	128,187	121,010
<i>p</i> -value	<0.01		0.01		0.17		0.27		0.04	
Effect size (Cohen's <i>d</i>)	0.03		0.01		−0.003		0.004		−0.01	

Notes. The table presents results from Experiment 2 conducted with MercadoLibre in which the treatment group received the same emails as the control group, except that the subject line mentioned the recipient's name. For each experimental group we report (a) averages across individuals, (b) standard errors (in parentheses), (c) *p*-value testing whether the means are equal across the two conditions, (d) the number of observations, and (e) effect size in terms of Cohen's *d*. Column (1) shows the open rate (opens/sent). It shows that the treatment condition has a higher opens/sent relative to the control condition, and this increase is statistically significant. Column (2) compares the likelihood of a click (clicks/sent) across the two conditions and finds that the clicks are also higher in the treatment condition. Column (3) shows that the unsubscription rate (unsubscribes/sent) is lower in the treatment condition but the difference is not statistically significant. Columns (4) and (5) compare leads and unsubscribes conditional on opens and show that conditional on opening the email, unsubscribes reduce but the leads remain statistically similar.

Table 4. Results from Experiment 3—Stanford Marketing Adding the Recipient's Name to the Subject Line

	Experiment 3									
	Opens/Sent		Clicks/Sent		Unsubscribes/Sent		Clicks/Opens		Unsubscribes/Opens	
	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control	Treatment	Control
Mean (%)	15.8	12.8	1.96	1.48	0.72	0.84	12.44	11.53	4.57	6.54
Standard error (%)	(0.7)	(0.7)	(0.28)	(0.24)	(0.17)	(0.17)	(1.66)	(1.78)	(1.05)	(1.38)
Number of observations	2,500	2,500	2,500	2,500	2,500	2,500	394	321	394	321
<i>p</i> -value	<0.01		0.19		0.63		0.71		0.25	
Effect size (Cohen's <i>d</i>)	0.09		0.04		−0.01		0.03		−0.09	

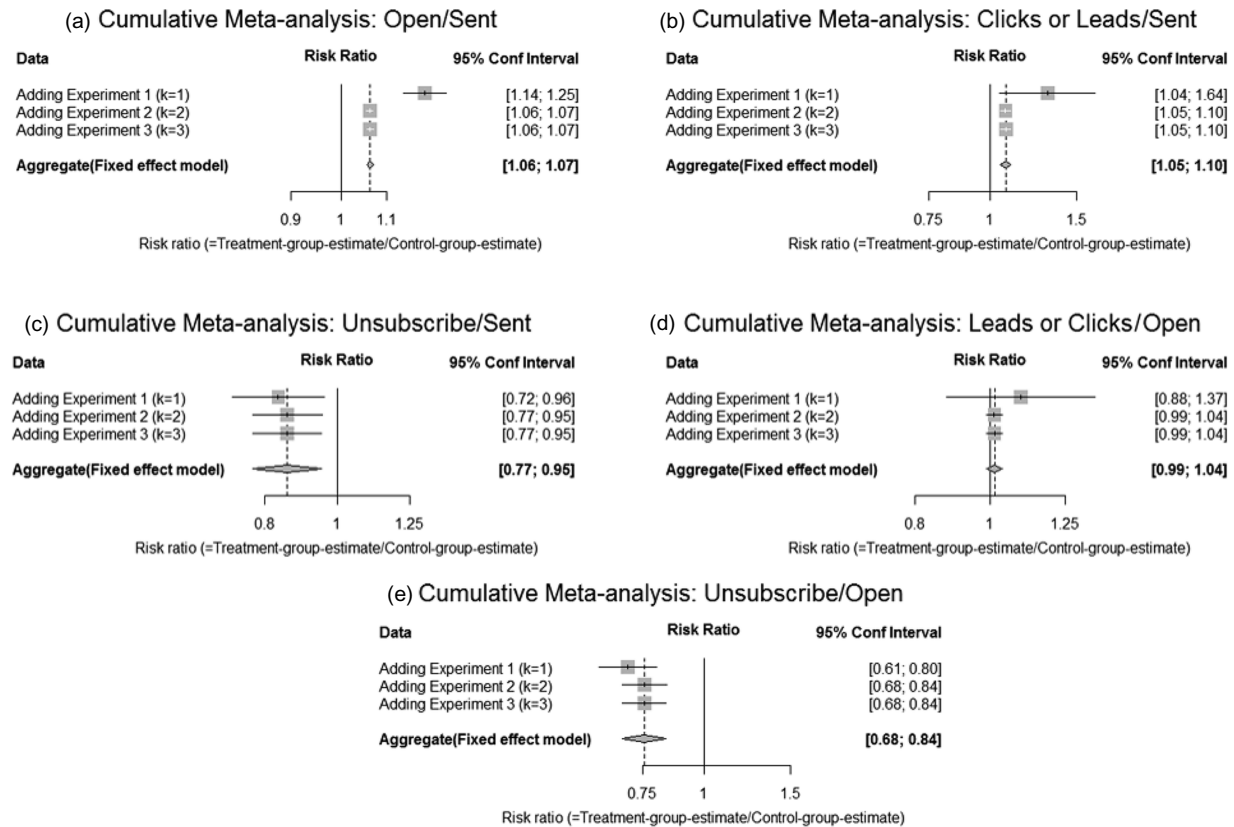
Notes. The table presents results from Experiment 3 conducted with Stanford in which the treatment group received the same emails as the control group, except that the subject line mentioned the recipient's name. For each experimental group we report (a) averages across individuals, (b) standard errors (in parentheses), (c) *p*-value testing whether the means are equal across the two conditions, (d) the number of observations, and (e) effect size in terms of Cohen's *d*. Column (1) shows the open rate (opens/sent). It shows that the treatment condition has a higher opens/sent relative to the control condition, and this increase is statistically significant. Column (2) compares the likelihood of a click (clicks/sent) across the two conditions and finds that the clicks are also higher in the treatment condition. Column (3) shows that the unsubscription rate (unsubscribes/sent) is lower in the treatment condition but the difference is not statistically significant. Columns (4) and (5) compare leads and unsubscribes conditional on opens and show that conditional on opening the email, unsubscribes reduce but the leads remain statistically similar.

more discussion on the methodology). Specifically, we aggregated the risk-ratio estimates across studies. For a specific study, the risk-ratio is the ratio of the treatment group mean and the control group mean.⁸ Following the recommendation of the previous literature, we used the Mantel–Haenszel method to aggregate evidence and estimate confidence intervals (see Borenstein et al. 2009, p. 331). Using other methods, such as the inverse variance method, does not affect our findings.

For presentation of the aggregate evidence, we followed the recommendations in Braver et al. (2014). For each of the five measures we studied, Figure 4 shows how the estimates and their confidence intervals change as we accumulated evidence from Experiments 1 through 3 using a forest plot. The first forest plot (Figure 4(a)) shows the meta-analysis for open

rates. The accumulated estimate at each stage is represented by a box, and the line through the box represents the 95% confidence interval. The first line shows that the risk-ratio estimate for Experiment 1 is 1.19, which implies a 19% increase in the likelihood of a recipient opening an email when the recipient's name is added to the subject line. The confidence interval does not include 1, indicating a significant difference between the treatment and control groups. Adding evidence from Experiment 2, the aggregated estimate changes to 1.06. Subsequently, adding evidence from Experiment 3 does not change the estimate by much, which is not surprising given the low statistical power in Experiment 3. The diamond below the studies represents the confidence interval of the overall effect. It shows that overall, the experimental variation increases open rate by 6% (risk-ratio = 1.06). The estimate is quite precise

Figure 4. Meta-Analysis Across Experiments 1–3



Notes. The figure presents a meta-analysis across Experiments 1–3. Following standard practice in meta-analyzing dichotomous dependent measures, we study risk ratios: the ratio of the estimates in the treatment and the control conditions. The figure shows the estimates and their confidence intervals change as we accumulate evidence from Experiments 1 through 3 using forest plots. The first forest plot (panel (a)) analyzes open rates. The accumulated estimate at each stage is represented by a box, and the line through the box represents the 95% confidence interval. The first line shows that the risk-ratio estimate for Experiment 1 is 1.19, which implies a 19% increase in the likelihood of a recipient opening an email when the recipient's name is added to the subject line. The confidence interval does not include 1, indicating a significant difference between the treatment and control groups. Adding evidence from Experiment 2, the aggregated estimate changes to 1.06. Subsequently, adding evidence from Experiment 3 does not change the estimate by much, which is not surprising given the low statistical power in Experiment 3. The diamond below the studies represents the confidence interval of the overall effect. It shows that overall, the experimental variation increases the open rate by 6% (risk-ratio = 1.06). The estimate is precise and statistically different from 1 (no change). Moving to the other plots we note that the likelihood of a positive outcome (leads or clicks) increases and the likelihood of a negative outcome (unsubscribing) decreases. In both these cases, the aggregate confidence interval does not include 1, rejecting the hypothesis that there is no overall effect. Panels (d) and (e) show analysis on measures conditional on opening the email.

and statistically rejects the null hypothesis of no change. Moving to the other plots, we note that the likelihood of a positive outcome (leads or clicks) increases and the likelihood of a negative outcome (unsubscribing) decreases. In both cases, the aggregate confidence interval does not include 1, rejecting the hypothesis that there is no overall effect. Figure 4(d) and 4(e) show analysis on measures conditional on opening the email. We found that the chances of unsubscribing conditional on opening decreased significantly because of the experimental variation. As seen in experiment-by-experiment analysis, chances of a positive outcome conditional on open (lead or click/open) did not change statistically significantly. Examining heterogeneity across the three experiments, we found that the effect on opening was significantly different in Experiment 1 compared to the

rest. For the remaining four measures we did not find any evidence of significant heterogeneity across the studies.

This analysis consolidates our findings across settings that adding a recipient's name in the subject line of an email significantly increases the likelihood of the recipient opening it and responding positively, and it significantly reduces the likelihood of the recipient responding negatively. The effect in the treatment group relative to the control group was consistent across the settings.

5. More Evidence on the Mechanism

The findings so far suggest that the effects of adding the recipient's name are not just limited to the recipient noticing the advertising message, which is the

orienting attention mechanism from Section 3.3. With Experiment 4, we attempted to identify whether the increasing elaboration (noninformative content increases message processing) or serving as a positive cue (noninformative content increases the recipient's liking of the advertiser) mechanism is the driver of the effects in this context. This is important because these mechanisms have different implications for advertisers designing ad campaigns. The serving as a positive cue mechanism predicts that the consumer's probability of buying the product increases because personalization directly increases the consumer's liking of the product. This would imply that the consumer's willingness to pay increases, which would allow the advertiser to increase prices. On the other hand, the increasing elaboration mechanism does not make such unidirectional predictions. This mechanism predicts that the observed effects are a consequence of increased elaboration on the content of the email. This increased elaboration increases the consumer's sensitivity to the quality of the information in the email. For example, if the email contains compelling content (e.g., the potential for a discount), that content would have a larger effect to the extent that the email recipients elaborate on it. Hence, if personalization increased elaboration of the email, it would increase the effect of the discount on our outcome variables. Unlike serving as a positive cue, increasing elaboration ties the effectiveness of noninformative content to the value of the other content in the advertising message.

5.1. Experiment 4

With the above motivation, we designed an experiment with company C to distinguish between the serving as a positive cue and increasing elaboration mechanisms. Our strategy was to examine evidence for increasing elaboration by examining whether the effectiveness of noninformative personalization content depended on the other content in the message. We implemented this using a 2×2 experiment design. One dimension of the experiment design varied the level of personalized content. Specifically, we randomized the inclusion of the recipient's name in the body of the email. Recall that the subject line always contained the name in all of the emails from company C after Experiment 1. Therefore, any individual in Experiment 4 saw her name in the subject line of the email. Hence, we varied only the presence of the name in the body of the email. The second dimension varied the quality of the email arguments by manipulating the presence of text that conveyed the potential for the consumer to receive a discount. A discount was a good piece of content to vary because it is likely to have a broad informative appeal (i.e., it is a compelling argument for message recipients). However, because our survey of the industry literature suggested that discounts can make the

Table 5. The 2×2 Design of Experiment 4

(A) Name not mentioned in the email body; No discount	(C) Name not mentioned in the email body; Discount mentioned
(B) Name mentioned in the email body; No discount	(D) Name mentioned in the email body; Discount mentioned

Notes. The table shows the four experimental conditions created by the 2×2 design of Experiment 4. Condition A is the control condition. Condition B adds the recipient's name to the body of the email in the control condition. Recall that the subject line already contains the recipient's name. So condition B repeats the name in the body of the message. Condition C mentions a potential discount in the body of the email in the control condition. Condition D adds both the name and a potential discount to the control email.

email likely to be filtered by spam filters,⁹ we did not specify a discount percentage to soften the language. Table 5 shows the four conditions in Experiment 4.¹⁰

The following should be noted about this experiment design. Because the recipient's name was present in the subject line in all emails, all individuals in Experiment 4 saw their name in the subject line of the email. Hence, the personalization manipulation in Experiment 4 was whether the email recipient saw her name for the second time in the email.¹¹ Therefore, a priori, we expected the effect of this manipulation to be smaller than the effect of adding the recipient's name in the subject line, as implemented in Experiments 1–3.

Figure 5 shows a snapshot from an email in the experimental-condition D, with both the recipient's name and the possible discount mentioned in the body of the email. It highlights the elements that we experimentally manipulated using a red circle. The experimental mention of the name and the discount were inserted at different points in the email and had a few lines of text between them. The line below the name presents a link to the recipient if she wished to gather more information about the advertiser. The landing page from this link provided general information about the company, its products, etc.

What did we expect to find in the data? Given this design, the increasing elaboration mechanism predicts that adding the recipient's name to the body of the email increases processing of the message. It makes the recipient consider any arguments in the email more carefully. Therefore, according to an increasing elaboration mechanism, adding the recipient's name to the message body increases favorable responding to the extent that the message contains compelling arguments (e.g., the presence of a discount). On the other hand, serving as a positive cue makes a simpler prediction, namely, that personalization should increase favorable responding regardless of the message content.

Figure 5. (Color online) Variation Generated by Experiment 4

Email id: [name@company.com](#)

Subject:
%%Name%%, Learn Financial Modeling & Business Analytics from Industry Experts in Sydney

Message:
Greetings,
This e-mail is in regards to our upcoming Financial Modeling 4 Days Intense Classroom Training in Sydney this month.
This course will help you ...
...
%%Name%%,
Get more information about us from ...
...
Do reply to this email, you may be eligible for a discount.
...
I apologize if this course is not of your interest. You can easily reply back to this e-mail or click on the unsubscribe link below.
Best Regards,
Alice Bradly

[Unsubscribe](#) me from this list

Notes. The figure illustrates the two dimensions of the 2×2 experiment design of Experiment 4 (shown in Table 5): (1) the recipient's name in the body of the email and (2) the possibility of a discount. These two experimentally varied portions of the content are circled (in solid red) for presentation. Note that the recipient's name is present in the subject line as well. Hence, the experiment varies the second occurrence of the recipient's name in the email. The email also provides the user with a link to the website in case she wants to get more information (highlighted with a dashed circle).

5.1.1. Empirical Findings. The results from Experiment 4 are shown in Table 6. A total of about 1.4 million emails were sent as part of this experiment. In each of the four conditions, about 10,000 emails were estimated to be opened. The average detected open rate for this campaign was about 3%, although it is lower in conditions with the discount. This was expected in light of the discussion above. Note that the difference is very small and works against the test, making our test more conservative.

Using an ANOVA (analysis of variance) test, we found that the leads were significantly different across the four conditions ($F(3,1411505) = 3.27, p = 0.02$). Con-

dition D, which showed the recipient's name and mentioned the discount generates the largest number of leads. Comparing leads in the absence of a discount (condition B versus A), we found that adding the recipient's name to the body of the email, when it was already mentioned in the subject line, did not increase the leads further (p -value = 0.38).¹² Comparing leads in the absence of the recipient's name (condition C versus A) in the body of the email, adding the possibility of the discount increased the leads, but the effect was not significant (p -value = 0.27). However, when the name of the recipient was present in the email (condition B versus D), the presence of the discount increased the

Table 6. Results from Experiment 4—Company C Examining How the Value of Repeating the Recipient's Name in the Message Changes with the Mention of a Discount

	(A)	(B)	(C)	(D)
Number of observations	352,722	353,099	352,615	353,074
Number of emails opened	10,780	10,749	10,120	10,429
Opens/Sent (%)	3.06 (0.03)	3.04 (0.03)	2.87 (0.03)	2.95 (0.03)
Leads/Sent (%)	0.024 (0.002)	0.021 (0.002)	0.028 (0.003)	0.032 (0.003)
Unsubscribes/Sent (%)	0.17 (0.007)	0.16 (0.007)	0.15 (0.007)	0.16 (0.007)
Leads/Opens (%)	0.56 (0.07)	0.51 (0.07)	0.68 (0.08)	0.88 (0.09)
Unsubscribes/Opens (%)	1.51 (0.12)	1.48 (0.12)	1.58 (0.12)	1.48 (0.12)

Notes. The table presents results from Experiment 4 conducted with company C that studies the effect of repeating the recipient's name in the message and a mention of a discount. For each experimental group we report (a) the number of observations, (b) number of opens, (c) averages of various observed outcomes across individuals, and (d) the standard errors (in parentheses). Note that the likelihood of a lead (leads/sent) is the highest in the condition that repeats the recipient's email and mentions the discount. Similarly, the likelihood of a lead conditional on opening the email is also the highest in condition D. The Unsubscribes show no clear pattern across the conditions.

Table 7. Experiment 4—Testing the Equality of Leads Conditional on Opening the Email for Each Pair of Conditions (a Total of Six Comparisons)

<i>p</i> -values from testing the null hypothesis: lead opening the email is equal across the pair of conditions in Experiment 4			
	(A)	(B)	(C)
(B)	0.61		
(C)	0.26	0.11	
(D)	<0.01	<0.01	0.09

Notes. The table presents *p*-values testing the equality of likelihood of a lead conditional on opening (lead/open) across every pair of the four conditions in Experiment 4. Leads in condition D are larger than any other condition. The second-highest leads are in condition C, which is statistically distinguishable from condition D at the 90% confidence level.

leads from 0.021% to 0.032% (*p*-value < 0.01). Therefore, we found a detectable effect of mentioning a discount only when the user's name was mentioned in the body of the email. We found that the unsubscribe rate did not change significantly across conditions. We found a similar pattern for leads conditional on opening the emails. Using an ANOVA test we found that the probability of a lead conditional on opening an email was significantly different across conditions ($F(3,42074) = 4.45$, $p < 0.01$), with the highest value in the condition when both the discount and the name were mentioned in the email. Table 7 shows *p*-values from testing the equality of this mean for each pair of conditions. It shows that the leads conditional on opening were significantly higher in the condition that mentioned the discount and repeated the recipient's name in the message body. As with the comparison of unconditional means, we found a detectable effect of mentioning a discount only when the user's name was mentioned in the body of the email. These findings are consistent with the predictions of increasing elaboration mechanism—the effect of the discount was larger in the presence of the recipient's name.¹³ We statistically tested whether the difference in effectiveness of adding the name for the second time was significantly different with and without the mention of a discount (B-A versus D-C), but have low statistical power for this test. Specifically, even though leads/opening were higher by 30% when the name was added in the presence of the discount, and leads/opening were lower by 10% when the name was added in the absence of a discount mention, which is an economically significant difference, a *t*-test was able to reject the hypothesis that the differences are equal with *p*-value = 0.12. When we increased power by controlling for observed characteristics, the *p*-value decreased to 0.07, as seen in the analysis below.

5.1.2. Exploratory Analysis of Recipient's Engagement with the Email.

We conducted further analyses

to examine how the two pieces of content—repeated use of the recipient's name (which is noninformative) and the mention of a potential discount (consequential for purchase decision making, presumably)—affected a recipient's deliberation about the advertisers' message. The challenge in conducting such an analysis is that deliberation is difficult to measure, in general. One could survey lab participants to gauge their thought process. However, we did not have a direct measure of consumers' deliberation in our setting. What we did observe is consumers' engagement with the email. Although engagement with the email is related to deliberation (the construct we want to measure), the relationship is imperfect. Upon more deliberation, a consumer might choose to engage with the email more. For example, if deliberation made the consumer think of positive arguments, she might choose to engage with the email more to get more information. However, the opposite is possible as well. Upon more deliberation, a consumer might not need information to inform her decision. If on more deliberation, the consumer came across a strong reason to buy (or not), she may engage less with the email. Therefore, we proceeded to analyze the effect of the experimental changes on the consumer's engagement with the email, with the caveat that engagement and deliberation do not have a one-to-one correspondence with each other.

We considered two measures of a consumer's engagement with the email. The first measure is an indicator of whether the user reopened the email after first opening it. The second measure is an indicator of whether the recipient of the message clicks on the link embedded in the message that directs the user to more information on the advertiser's website (mentioned earlier and highlighted in Figure 5). The action of reopening an email indicates that the recipient read it again and spent more time on the message. Also, clicking on the link in the email indicates that the recipient spent time considering the advertiser's proposition.

Table 8 shows the results from a regression analysis. The unit of observation is a recipient who opened the email. The explanatory variables were (a) whether the recipient's experimental condition repeated the name in the email body, (b) whether the recipient's experimental condition mentioned the possibility of a discount, and (c) an interaction term of both (a) and (b). We controlled for the observed characteristics of the recipient and the ad by adding fixed effects for the city, date, and the course that was being sold. Columns (1) and (2) use an indicator of a lead as the dependent measure. These results support our findings from mean comparisons done earlier. Mentioning a discount in the email increased the chances of a lead, whereas adding the recipient's name to the message body did not directly affect leads. However, adding the name

Table 8. Regression Analysis of the Data from Experiment 4: More Evidence on the Mechanism

	DV related to purchase		DV related to engagement with the email ad			
	(1) Lead Coeff. (std err)	(2) Lead Coeff. (std err)	(3) Reopen Coeff. (std err)	(4) Reopen Coeff. (std err)	(5) Click to gather info Coeff. (std err)	(6) Click to gather info Coeff. (std err)
Name present	0.00066 (0.00078)	−0.00075 (0.00099)	0.0094** (0.0040)	0.00784 (0.00562)	0.00912** (0.0036)	0.00547 (0.00508)
Discount present	0.0023** (0.00079)	0.00085 (0.0011)	−0.00737* (0.0040)	−0.00895 (0.00563)	−0.0114** (0.0036)	−0.01523** (0.00505)
Name × Discount		0.0029* (0.00157)		0.00314 (0.0080)		0.00749 (0.0072)
Intercept	0.00497** (0.00064)	0.00557** (0.0007)	0.2142** (0.0034)	0.2149** (0.00395)	0.1752** (0.00318)	0.1763** (0.00367)
City	Yes	Yes	Yes	Yes	Yes	Yes
Date	Yes	Yes	Yes	Yes	Yes	Yes
Course	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	42,078	42,078	42,078	42,078	42,078	42,078

Notes. The table shows results from regression analysis of the data from Experiment 4, conditional on opening the email. Each column shows results from a different specification, each controlling for message recipient and product characteristics. Columns (1) and (2) show the effect of repeating the recipient's name, mentioning a discount, and the interaction of the two on generation of a lead. Columns (3) and (4) show the results when the dependent measure is changed to a dummy indicator of whether the recipient reopened the email after opening it once. Columns (5) and (6) change the dependent measure to whether the recipient explores the advertiser's website by clicking on the link provided in the email. Columns (1) and (2) show that the conclusions from mean comparisons in Table 6 hold when observables are controlled for. This is expected given experimental randomization. The remaining columns show that both measures of the recipient's engagement with the message increase because of the recipient's name, and decrease because of the discount (coefficients for "name present" and the interaction term are jointly significant in columns 4 and 6). This is consistent with the mechanism in which seeing the name makes the recipients more engaged with the message. The presence of a discount makes the decision easier and reduces the recipient's interaction with the message.

* $p < 0.1$; ** $p < 0.05$.

enabled the discount to work, as can be seen in column (2). Columns (3) to (6) use as dependent variables the two measures of engagement with the message. They show that adding the name increased both the likelihood of the recipient reopening the email later and also clicking on the link present in the email.¹⁴ On the other hand, mentioning a discount reduced engagement.¹⁵ This is consistent with a discount making it easy for a person who is on the margin, of whether she wants to make a purchase, reducing the need to engage more. This exploratory analysis shows that the two pieces of content affected the recipient's interaction with the email message in a very different manner. The name increased the recipients' engagement with the email. It increased leads in the presence of a discount in the message. The discount did not increase engagement with the message. By itself, it also did not increase leads but did so when the name was also present in the email. These findings shed light on how the pieces of content affect the behavior of a recipient of an email marketing message.

5.1.3. Discussion: Findings from Experiment 4. The results from Experiment 4 are consistent with the following mechanism: viewing one's name in the ad-message increases the likelihood of the individual reading the message more carefully and processing

it, which in turn increases the chances of a positive outcome if the content is compelling. It implies that the effect of adding the personalized, noninformative piece of content (e.g., repeating the recipient's name) depends on the other content of the email. By itself, personalization may have limited value, at least in contexts such as the ones we studied in this paper.

This mechanism is important to consider while interpreting the effect of advertising content. Past research has categorized the effect of advertising into informative (belief changing) and persuasive (preference changing). If these are the only two kinds of effects of ad content, one might infer the effect of uninformative content as evidence for advertising shifting consumer preferences, which is consequential for the role of advertising (DellaVigna and Gentzkow 2010). However, as seen in our data, the effect of noninformative ad content can exist through increased deliberation and message processing, which allows it to work without directly changing preferences. This mechanism has significant implications for researchers studying advertising.

5.1.4. Discussion: Our Experimental Stimuli Relative to Other Personalized Content. The experimental content we focused on in our paper—the person's own name—is one among the large possible set of content that a

consumer may associate with herself. This set includes one's traits (e.g., extroverted), identities (e.g., American), roles (e.g., professor), past behaviors (e.g., smoked a cigarette), and so forth (see, e.g., Kihlstrom and Cantor 1984, McConnell 2011, Markus and Wurf 1987). As a result, personalization can be manipulated, and message elaboration can be increased, by inclusion of many different types of information about the recipient.

We chose recipient name as our primary personalization variable for multiple reasons. First, the name of the recipient is one of the personalization elements that marketers are most likely to have access to. Therefore, showing effects using recipients' names provides a demonstration of the applicability and practicability of our findings. Second, although people vary in the extent to which a feature of their self-concept (e.g., their race) is naturally salient (e.g., Deshpandé and Stayman 1994) or important (Markus 1977), their names are likely to be a central and unvaryingly accessible feature, making it a widely useful personalization variable. Third, compared with many other forms of personalization such as referring to a recipient's geographic location, gender, or racial group, the recipient's name is least likely to be informative regarding the characteristics of the product or the advertising company. For example, in our setting, mentioning the geographic location might convey to the recipient the location where the classes are to be held, which is likely to influence the purchase decision. Hence, using recipient name serves both to demonstrate the practical implications of our findings as well as to provide a clean test of our theory.

To empirically examine the extension of the effects beyond the recipient's name, we also conducted an experiment with company C in which we manipulated whether the name of the recipient's employer is mentioned in the email. Consistent with the effects found in the above experiments, adding the employer's name increased sales' leads and reduced unsubscribing rates. The experiment is described in detail in the online appendix.

5.1.5. Managerial Implications. Firms realize the importance of making their marketing messages relevant to consumers and have actively invested in it. Online retailers make efforts to tailor their email messages based on the products purchased by the consumers in the past, because this information predicts the consumers' future shopping interests. Similarly, media platforms invest in technologies that enable targeting of ads that are relevant to the users, because relevant ads are more likely to be clicked on. However, even though informativeness of a marketing message is important, we argue that it is just one component that goes into generating returns from marketing spending. Another important component is the amount of thought consumers give to the message.

These two components are complementary; the returns from increasing the informativeness of messages are likely to be limited if the thought consumers give to the message is limited. We show how the latter can be improved. Inclusion of content that is about the message recipient (such as the recipient's name) increases the amount of deliberation the message gets and improves the overall outcome if the message is relevant. In several situations, including ours, marketers have access to such information, and including the information is virtually costless for the firm.

Knowing the mechanism driving the effects of non-informative personalization is critical to knowing whether it will be effective or is even a good idea. As described in Section 1, both the orienting attention and serving as a positive cue mechanisms imply unidirectional effects—any effect of personalization from these mechanisms would likely be positive, making it riskless for firms to personalize their email marketing. The increasing elaboration mechanism, by contrast, could either increase or decrease persuasion, depending on how compelling the information is in the email (Wheeler et al. 2005, Petty et al. 1983). Put another way, the returns of personalization with this mechanism are dependent on the other content in the message, making personalization a more effective strategy when the email contains strong arguments.

There is another important implication of the mechanism for marketers, namely, the long-term efficacy of the advertisement. Unlike the orienting attention and serving as a positive cue mechanisms, increasing elaboration is one means of increasing attitude strength, a characteristic of persistent, resistant, and influential attitudes (see Petty et al. 1995). For example, attitudes formed as a result of greater elaboration are more stable over time and are more resistant to attempts at counter persuasion (e.g., Haugtvedt and Petty 1992). Similarly, the attitudes formed as a result of greater message elaboration are more predictive of behavior than are less elaborated attitudes (e.g., Cacioppo et al. 1986, Petty et al. 1983). Hence, if personalization operates through increasing elaboration, it may not only create more desirable responding from consumers, at least when accompanied by compelling message content, but the attitudes formed from personalized messages may be more durable and influential as well.

6. Conclusion

The above findings complement the past research on advertising content (Sudhir et al. 2016, Bertrand et al. 2010) by clearly demonstrating the significant effects of the content used to *personalize* email ads. This paper illustrates the importance of content that is *unrelated* to the product being advertised but is related to the ad's target audience. Such content, "auxiliary" to the core message in an ad, is relevant to the recipient of the

advertising and is not likely to be informative about the advertised product. We show evidence for the effect of such content in several settings that are diverse in terms of marketing objectives and baseline message characteristics. Furthermore, this paper sheds light on the mechanism by which this effect operates, by making use of detailed data on consumer response observable in the context of email advertising, such as a user opening an email, unsubscribing from the campaign, in addition to the usual end outcomes related to sales. Our investigation of the mechanism shows that personalized content can be noninformative but still be valuable in garnering a consumer's interest and increasing the likelihood of her processing and responding to the rest of the advertising message. The specific mechanism for which we find support is that the effect of the noninformative content depends on how the consumer evaluates the rest of the (potentially informative) content in the ad. Thus, our results show that to predict the success of an ad campaign, one needs to consider the *entire* content of the ads, in addition to the usual factors such as reach, frequency, and timing that are typically incorporated in models of consumer demand.

Our findings raise several questions about the “boundary” of the effect of personalization of this nature—under what conditions do we expect the effects to vanish? Our data shed some light on this issue. As seen in Experiment 4, the benefits from adding such content is lower when the appeal of the rest of the message is lower. Past research on privacy also suggests some boundary conditions. For example, in the context of email marketing, consumers may expect the sender to know their name, and not be surprised to see it in the email. However, this may not be the case for other broader yet customizable channels such as Internet banner advertising. Privacy concerns could hurt the advertiser in such settings. Other boundaries for this effect could also exist. How the effect of personalization content changes when more email marketers engage in personalization is yet to be studied. As the consumers' beliefs, norms, and marketing practice change across markets, the effect of advertising content could also change. For example, mentioning the recipient's name could lead to a negative reaction in markets that are more privacy sensitive and where usage of the recipient's name is considered as being unacceptable. We have not been able to explore such issues in this paper.

One limitation of this paper is that we do not study long-term effects of ad content. Our mechanism implies that the personalized content we include increases the consumers' processing of the ad message. This implies that inclusion of such content would also increase the consumer's retention of the advertised information over time and may be consequential in the long-term effects of advertising (e.g., Cacioppo et al.

1986, Haugtvedt and Petty 1992, Petty et al. 1983). This mechanism also has implications for the placement of personalization content. Because it draws a consumer's focus, such content might be most useful preceding information that is most important for a firm to convey. Such implications exist in theory but need empirical support. Another limitation of our analysis is that we study the effects of specific instances of personalization content. Although these instances are well suited for our goal of studying how noninformative content affects consumer decisions, more empirical effort is needed to evaluate the effectiveness of other content. Further research is also needed to extend our findings to other nontextual elements of the ad content.

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Appendix A. Experimental Results Using Regressions

This paper presents results from the experiments by comparing means across conditions. In this section, we follow a different approach and analyze the data using linear regression, controlling for observable characteristics about the recipients and email campaign. Because of randomization, we expect the observable characteristics to be independent of the assignment to the experimental condition. Therefore, this analysis serves as a robustness check. Because we do not observe any varying email-specific characteristics for Stanford's campaign, we omit Experiment 3 from this analysis. Tables A.1–A.3 show the results from these regressions. Across the tables, we see that the results do not change by adding controls. For example, column (1) of Table A.1 shows that the likelihood of opening increases by 0.0186 (95% confidence interval = [0.0232, 0.0140]) when a recipient is randomly allocated to the condition that mentions the recipient's name in the subject line. This estimate is statistically indistinguishable from the difference in means between the treatment and control group in column (1) of Table 2, which is equal to 0.0175 (= 0.1080 – 0.0905).

Table A.1. Regression Analysis of the Data from Experiment 1: Robustness to Controls

	Open (0/1) (1) Coeff. (std err)	Lead (0/1) (2) Coeff. (std err)	Unsubscribe (0/1) (3) Coeff. (std err)	Lead/Open (0/1) (4) Coeff. (std err)	Unsub/Open (0/1) (5) Coeff. (std err)
Condition with name	0.0186** (0.0023)	0.0012** (0.0005)	−0.0021** (0.0008)	0.0081 (0.005)	−0.0493** (0.0076)
Intercept	0.0905** (0.0016)	0.0039** (0.0004)	0.0120** (0.0005)	0.0429** (0.0036)	0.1328** (0.0061)
City	Yes	Yes	Yes	Yes	Yes
Date	Yes	Yes	Yes	Yes	Yes
Course	Yes	Yes	Yes	Yes	Yes
Number of observations	68,088	68,088	68,088	6,745	6,745

Notes. The table shows results from Experiment 1 using regression analysis, controlling for observable characteristics using fixed effects. Specifically, we control for the location of the recipient, the date when the email was sent, and the course being sold in the email. The independent variable is the experimental allocation of the user to a condition with the individual's name in the email. Each column analyzes a different dependent measure. Across columns we find results analogous to the findings from mean comparison presented in Table 2.

* $p < 0.1$; ** $p < 0.05$.

Table A.2. Regression Analysis of the Data from Experiment 2: Robustness to Controls

	Open (0/1) (1) Coeff. (std err)	Click (0/1) (2) Coeff. (std err)	Unsubscribe (0/1) (3) Coeff. (std err)	Click/Open (0/1) (4) Coeff. (std err)	Unsub/Open (0/1) (5) Coeff. (std err)
Condition with name	0.01293** (0.00079)	0.00155** (0.00028)	−0.00006 (0.00004)	0.00119 (0.00118)	−0.00044** (0.00019)
Intercept	0.21781** (0.00554)	0.02095** (0.00019)	0.00055** (0.00003)	0.09626** (0.00084)	0.00255 (0.00014)
Country	Yes	Yes	Yes	Yes	Yes
Number of observations	1,111,130	1,111,130	1,111,130	249,197	249,197

Notes. The table shows results from Experiment 2 using regression analysis, controlling for observable characteristics (the recipient's country) using fixed effects. The independent variable is the experimental allocation of the user to a condition with the individual's name in the email. Each column analyzes a different dependent measure. Across columns we find results analogous to the findings from mean comparison presented in Table 2.

* $p < 0.1$; ** $p < 0.05$.

Table A.3. Regression Analysis of the Data from Experiment 4: Robustness to Controls

	Lead (0/1) (1) Coeff. (std err)	Unsubscribe (0/1) (2) Coeff. (std err)	Lead/Open (0/1) (3) Coeff. (std err)	Unsub/Open (0/1) (4) Coeff. (std err)
Name only (B)	−0.000032 (0.000036)	−0.000071 (0.000097)	−0.000751 (0.000991)	−0.000157 (0.001645)
Discount only (C)	0.000042 (0.000039)	−0.000187** (0.000095)	0.000846 (0.001084)	0.000752 (0.001708)
Name and Discount (D)	0.000081** (0.000040)	−0.000119 (0.000096)	0.002979** (0.001157)	−0.000179 (0.001666)
Intercept (baseline: A)	0.00024** (0.00002)	0.0017** (0.0007)	0.0056** (0.0007)	0.0151** (0.0012)
City	Yes	Yes	Yes	Yes
Date	Yes	Yes	Yes	Yes
Course	Yes	Yes	Yes	Yes
Number of observations	1,411,509	1,411,509	42,078	42,078

Notes. The table shows results from Experiment 4 using regression analysis, controlling for observable characteristics using fixed effects. Specifically, we control for the location of the recipient, the date when the email was sent, and the course being sold in the email. The independent variables are dummy indicators of whether the user was allocated to the specific experimental condition. Each column analyzes a different dependent measure. Across columns we find results analogous to the findings from mean comparison presented in Table 6.

* $p < 0.1$; ** $p < 0.05$.

Endnotes

¹ Obviously, personalized content can also be very informative. For example, an email to a resident of New York from a travel site announcing promotional fares for flights from New York City is both personalized and informative. We explicitly exclude such content from our study because it may inform the consumer about the product and, therefore, affect purchase behavior.

² To fix ideas, by the term “noninformative content” we mean content of an ad message that is not about and does not convey any information about the advertiser or the advertised product. For any piece of content, it is difficult to say whether it would be noninformative, in general, because it is possible that some consumer might use it to make inferences. In our experiments we take steps to avoid such concerns.

³ If firms are currently using this information in their email campaigns, one could argue that they already have knowledge of its effects. In such instances, our research helps quantify the magnitude of this effect and sheds light on the mechanism that drives the effect. An industry survey of 418 emails from 20 large retailers found that 96% of the emails did not address the recipients by name (SimpleRelevance 2014); this underscores the potential for using names in emails.

⁴ They help their customers prepare for certificate exams such as Chartered Financial Analyst and Financial Risk Management.

⁵ To track opens, the emailing software embed a “pixel” (a very tiny image) into the email message. When the message is opened, the pixel is loaded from the Internet, enabling the tracking of the act of opening an email. However, some email clients might block this step leading to opens not being tracked. Therefore, this measure is incomplete because it might miss some individuals who opened the email.

⁶ In addition to comparison of means, we also conducted regression analysis to check for robustness of the results when observed factors are controlled for. As expected because of randomization, we find that controlling for the recipient’s city, date when the email was sent, and the product advertised does not change the estimated effects. The results are presented in the appendix.

⁷ We expect this calculation to overestimate the conditional lead rate and underestimate the unsubscribed rate. A priori, we expect the additional people who open the email because of the experimental treatment to have a lower propensity to buy from the advertiser and a higher propensity to unsubscribe from the email campaign, compared to the rest of the individuals who opened the email. This is because this set of people chose not to open the email without their names in the subject line.

⁸ In general, meta-analysis of risk ratio is preferred over other measures such as risk difference because it is expected to have the least heterogeneity across studies. This is true in our case as well.

⁹ For example, <http://www.leadformix.com/blog/2013/09/top-100-spam-trigger-words-and-phrases-to-avoid/>.

¹⁰ An alternative role of the discount is possible. Consumers may find a discount aligned with their identity, if they perceive themselves as deal seeking. By this mechanism, mentioning a discount may improve people’s response because they find the message more aligned with their identity. While this is possible, this mechanism cannot explain why the effect of mentioning a discount would be tied to the recipient’s name included in the content, which is what we find.

¹¹ This fact further mitigates concerns of any inferences a person may draw about the advertising company from the experimental manipulation. Such variation—repeating the recipient’s name in the body of the message when it has already been added to the subject line—was also tested in another experiment. Because that experiment is similar to the distinction between conditions A and B in Experiment 4,

with similar inference from the estimates, we did not include it in the paper.

¹² Recall that this is a measure of the recipient seeing her name for the second time, unlike what we estimated in Experiment 1. This is because in Experiment 4, the subject line of the email always has the name of the person in it.

¹³ We also examined whether the effects vary with the uniqueness of the recipient’s name. However, we did not find any evidence of systematic heterogeneity in the effects.

¹⁴ In each of the regressions with interaction terms (columns 4 and 6), the coefficients corresponding to the dummy indicator of a name being repeated and the interaction term are jointly statistically significant ($p = 0.05$ for column 4 and $p = 0.03$ for column 6).

¹⁵ Theory does not make a clear prediction of how information, such as a discount, might affect engagement measures. It may decrease engagement among recipients who are on the margin of buying. It may also increase engagement among users who get interested because of the discount.

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