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The Formality Effect

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Abstract

This paper documents the existence of a "Formality Effect" in government communications. Across three online studies and three field experiments in different policy contexts (total N = 67,632), we show that, contrary to researcher and practitioner predictions, formal government communications are more effective at influencing resident behavior than informal government communications. In exploring mechanisms, we show that formality operates as a heuristic for credibility and importance. Recipients view the source of a formal letter as more competent and trustworthy, and view the request itself as more important to take action on, despite no evidence of change in comprehension nor in perceived ease of taking action. These findings have immediate implications for government communicators and open the door for a renewed focus on how the design and presentation of information impacts behavior.

Government effectiveness depends, in part, on successful state-resident interactions, much of which occurs through written communication. Yet, there are significant gaps between what the government asks residents to do and how residents actually behave.[1,2,3,4] This can have far-reaching policy consequences. Residents' willingness to respond to government requests affects a range of outcomes, including how public funds are spent, who benefits from public services, and even electoral outcomes.[5,6,7,8] As such, an interdisciplinary literature across behavioral science, public management, and economics focuses on understanding and increasing responsiveness to government communications, with a particular emphasis on testing the impact of language and messaging. In this paper, we argue that similar attention should be paid to the design and presentation of information. Across three online studies and three field experiments in different policy contexts (total $N = 67,632$), we provide evidence of a "Formality Effect" in which, contrary to expert predictions, more formal government communications induce intended behaviors more effectively than less formal government communications. Exploring mechanisms, we show that formality operates as a heuristic for source credibility and importance. These findings have immediate implications for scholars who study what motivates behavior, as well as for practitioners who regularly communicate with the public.

A large body of empirical research leverages behavioral science to test methods of increasing the effectiveness and persuasiveness of communications. Most efforts have focused on adjusting language to target and reduce known barriers to action, such as inattention, inertia, and complexity. For instance, studies have found that simplified language, social norms, and planning prompts can all increase the likelihood that recipients take a requested action after receiving a written communication.[9,10,11,12,13] In the context of government communications specifically, studies have tested the use of similar tools to increase take-up of government programs or responses to government requests, but results have been mixed.[14,15,16,17]

Beyond the message content, all such communications require a series of decisions on how to present the information—which we call design attributes—such as what font, colors, graphics, or tone to use. Yet, there is limited empirical evidence on how the design and presentation of information in government communications affects behavior. Rather, decisions on information presentation in these contexts often rely on principles of graphic design, which emphasize the importance of using visuals, colors, and contrast.[18,19,20] The main outcomes in this literature often center on visual salience or improved clarity in directing readers' attention to key information.[21,22] A parallel literature, mainly focusing on online communications, suggests that informal linguistic cues, informal visual elements (e.g., emoticons) and conversational human voice (CHV) may also increase the efficacy of communications by influencing perceptions of reputation and trust, although empirical evidence is mixed.[23,24,25,26]

Taken together, these best practices and principles contribute to a widespread belief that colorful, attention-grabbing, and informal government communications are more effective at improving resident engagement. Indeed, in an online prediction survey of 351 practitioners and researchers, we find that experts' predictions about how design attributes will affect behavior reflect these assumed best practices. That is, respondents—both in academia and in

government—overwhelmingly predicted that government communications with images, color, informal language, and lower reading levels would be more effective at encouraging resident action (see Table 1 and Supplementary Tables 1 and 2).

We group these design attributes along two axes, aesthetics and language, to define "formality." On the first axis, attributes of a "formal aesthetic" can include standard typeface and font size (e.g., size 12, Times New Roman font), black font with minimal formatting, and no graphics or images aside from a logo. Conversely, an "informal aesthetic" can include colors, formatting, novelty fonts, and pictures or graphics.[27,28] On the second axis, attributes of "formal language" can include impersonal language (e.g., using third person) or more complex writing (e.g., higher reading level), while "informal language" includes personalized or less complex writing.[29,30]

Contrary to expert predictions, we posit that formality increases the effectiveness of government communications—defined as the likelihood that residents take a requested action—by acting as a heuristic for credibility and importance. We situate this framework within a broader literature on persuasion, which has identified five distinct factors that can affect the persuasiveness of communication: source, message, channel, receiver, and destination.[31,32,33] We hypothesize that formality can influence how the first two factors—source and message—affect resident behavior.

The "source" refers to the perceived sender of a communication. Previous research has demonstrated that recipients are more likely to take action when they perceive the requester to be credible.[33] Indeed, source credibility has been shown to impact a wide range of outcomes, including willingness to exercise, willingness to change one's mind, and acceptance of advertising claims.[34,35,36,37] At a fundamental level, sources are perceived to be more credible if they are perceived to be experts (e.g., an authority; knowledgeable) and trustworthy (e.g., honest; genuine).[33,38] But in one-off written communications, such as many government communications, it can be difficult to establish expertise or trustworthiness, especially in a context where overall trust in government is low[39,40] and misinformation is perceived to be increasingly prevalent.[41] Moreover, the source is fixed for many government communications; adjusting the source to signal greater expertise, for instance, is not an option for many government communicators who are communicating on behalf of their agency or department.

In such an environment, recipients may rely on heuristics to ascertain source credibility. We hypothesize that formality operates as a heuristic for credibility by conforming with residents' expectations about how government communications should look, and by signaling trustworthiness and competence. There is some recent evidence that suggests this might be true in related contexts. For example, Gretry et al. (2017) find that when a brand is unfamiliar, informality in communications reduces trust.[24] While the government in abstract is not an unfamiliar brand, a specific government messenger or agency may be, especially in their first communication with a given resident, and so credibility may need to be established. In a more relevant policy context, Bullock and Hubner (2020) find that when politicians use more informal language on social media, they are perceived to be less credible, in part because this violates

constituents' expectations of how politicians should sound.[42] Therefore, we hypothesize that more formal language may be particularly important in establishing credibility in contexts where there are limited cues for source credibility or in cases where residents hold strong priors about how a communication should look or sound, as is often the case in the context of government. We predict that government communications are expected to be more formal than similar communications from nonprofits or the private sector, which would make formality especially consequential in this context.

The "message" is the second factor that can influence the persuasiveness of a communication. Even if the source is seen as credible, recipients must view the requested action as important, relevant, and feasible in order to follow through. This is particularly important in the context of government as requests made of residents may be utilitarian in nature, and so may not be considered enjoyable or desirable. Prior studies have tested methods of encouraging action by making a request appear more consequential, such as by creating a sense of urgency or making the risks of inaction more salient.[15,43,44,45]

We hypothesize that formality can operate as a heuristic for the importance of a message through three potential channels. First, if requests from the government are expected to be formal and are inherently viewed as more important to respond to than equivalent requests from non-governmental actors, then formality may increase the likelihood that a request is perceived to be "from the government," thus increasing its perceived importance. Second, if recipients gauge how important an ask is by taking cues from how important the sender believes it to be, then formality may increase perceived importance of taking action. That is, a message with a more formal tone may imply that the sender themselves believes the request to be important. Third, more formal language may signal the importance of taking action regardless of who the source is. Some evidence points in this direction. For instance, using a more formal (or "corporate") tone has been found to be more effective in some customer service interactions.[23] Conversely, using humor or an informal tone in crisis communication has been shown to reduce the perceived severity of the situation.[46,47] Taken together, this suggests that a more formal tone may imply greater seriousness, and thus signal that the request is more important or consequential.

By operating as a heuristic for both credibility and importance, we predict that there exists a Formality Effect such that formal government communications more effectively influence resident behavior than informal government communications. Across six studies, we test these hypotheses and potential mechanisms. First, we demonstrate that formality is malleable and recognizable, and distinct from complexity (Study 1). Language and aesthetics can be independently manipulated to increase perceived formality, and the effect of manipulating both together is larger than the sum of each individual manipulation. We then document the effect of formality in government communications in three large-scale field experiments in three separate policy contexts (Studies 2-4). In each study, we find that the more formal communication increases engagement relative to a more informal counterpart.

Finally, we test our hypothesized mechanisms in an additional two online studies (Studies 5 and 6). We find that people believe it is more important to respond to government requests,

compared to equivalent requests from a nonprofit or a private sector sender, and that people expect government communication to be more formal, even without viewing the communication itself. Then, using the treatment materials from each field experiment, we show that formality increases source credibility through the channels of perceived expertise and trustworthiness. Additionally, we show that the formal letters are viewed as more important to act on and, in turn, increase self-reported likelihood of acting, without any evidence of affecting comprehension, and despite a marginally *negative* impact on perceived ease of taking action.

Results

Study 1: Formality is malleable and recognizable

In a pre-registered online experiment ($N = 688$), we demonstrate that "formality" in written communications is a recognizable construct that can be manipulated along the two defined axes: aesthetics and language. In a factorial design, participants were randomly assigned to see one of four letters that varied either or both axes of formality: (1) informal aesthetic and informal language; (2) formal aesthetic and informal language; (3) informal aesthetic and formal language; or (4) formal aesthetic and formal language. They were then asked how formal they believed the letter's design was; how formal the language was; and, overall, how formal they found the letter. All letters are shown in Supplementary Figure 1.

Figure 1 shows treatment effects by condition. Manipulating each axis independently significantly increased perceived formality relative to the informal letter (condition 1). Using a formal aesthetic increased perceived formality by 0.41 standard deviations (SD), even while keeping the language informal ($t = 3.71$, $p < .001$, 95% CI[0.19, 0.63]). Using formal language, while keeping the aesthetic design informal, increased perceived formality by 0.24 SD ($t = 2.20$, $p = .03$, 95% CI[0.03, 0.45]). Manipulating both axes at the same time (condition 4) increased perceived formality by more than manipulating each dimension independently: 0.85 SD ($t = 8.46$, $p < .001$, 95% CI[0.65, 1.05]).

Examining each dimension of formality separately, we find that the two letters that used a formal aesthetic (conditions 2 and 4) were rated as having a significantly more formal design than the letters with an informal aesthetic (0.81 SD, $t = 11.65$, $p < .001$, 95% CI[0.67, 0.94]). Likewise, the letters that used formal language (conditions 3 and 4) were rated as having significantly more formal language than the letters with informal language attributes (0.51 SD, $t = 7.03$, $p < .001$, 95% CI[0.37, 0.66], see Supplementary Table 5).

These results demonstrate that formality is a construct of which people hold a shared understanding, and that can be manipulated by changing aesthetic and language attributes. Importantly, these findings demonstrate that formality is conceptually distinct from linguistic complexity. In other words, it is possible to vary the perceived formality of a communication by shifting aesthetics without increasing linguistic complexity, defined as using a higher reading level and a passive voice, which would risk making communications less accessible.[48] In fact,

these findings suggest that aesthetic attributes may more strongly influence perceptions of formality than language attributes.
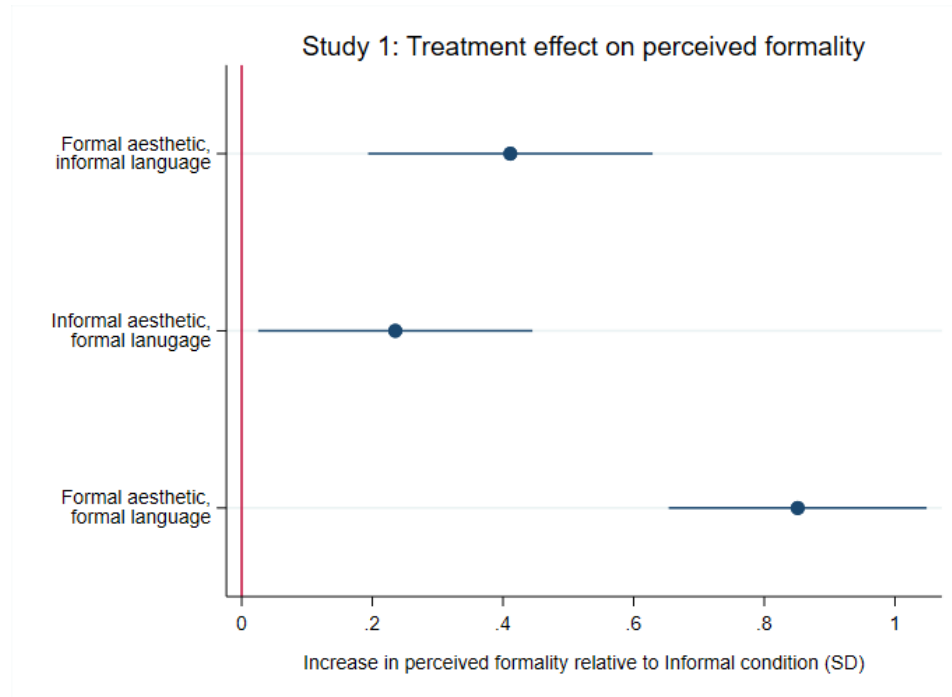

Figure 1. Study 1 treatment effects on perceived formality, in standard deviations, relative to a letter with informal language and informal aesthetic. Data are presented as average treatment effects with 95% confidence intervals, derived from OLS models (N = 688 online survey participants; see Supplementary Table 5 for full statistics). Materials available in Supplementary Figure 1.

Studies 2-4: Field experiments

Three field experiments (total $N = 65,172$) offer evidence of the Formality Effect using real government communications at the state and local level. Each experiment involved direct collaborations with government agencies and targeted behaviors in different domains: self-certification of small businesses, enrollment in a local government program, and take-up of the California Earned Income Tax Credit (CalEITC) (see Table 2).

Study 2 was conducted in January-March 2017 by the Behavioral Insights Team (BIT) in collaboration with a US city that sought to collect information from local businesses in order to determine whether they qualified as a local, women-owned, or minority-owned business. The partner city sent 10,000 businesses, selected at random from the city's business registry database, one of two letters—formal or informal—to encourage them to self-register as a local, minority-owned, or woman-owned business. This request was framed as part of the city's broader effort to purchase more goods and services from local, minority-, or women-owned businesses. The more formal letter was black-and-white, addressed "Dear Business Owner," and included about 260

words. The more informal letter cut the length by half, included a personalized greeting that named the business owner when possible, and used informal tone and punctuation (i.e., "We want to work with you!" and "Good luck in the new year!"). It also included colorful design elements including a red box around the call to action, and red font emphasizing the purpose of the letter. (All letters are available in Supplementary Figures 2 and 3.) While these two letters vary on both axes of formality—aesthetics and language—the informal letter is also much shorter and written at a lower reading level. Previous behavioral evidence would suggest that these two factors—a lower reading level and less text—should increase the effectiveness of the informal letter.[12]

Instead, businesses that were assigned to receive the formal letter were 1.9 pp or 25% more likely to self-register than businesses that were assigned to receive the informal letter ($p = .001$, $t = 3.36$ 95% CI [0.77, 2.93], see Supplementary Table 6).

Study 3 was conducted in September-October 2017 by BIT in collaboration with a US city that wanted to increase enrollment in an emergency medical transportation program. For less than $5 per month, city residents could purchase a membership that would fully cover the cost of emergency ambulance rides within the city even if they did not have medical insurance.

In a randomized experiment, the city sent 35,172 residents who had not already signed up to the program one of two letters—again, formal or informal—to encourage them to enroll in the program. In this experiment, formality was manipulated primarily by aesthetics; the language was relatively similar across both letters. Specifically, the formal letter was black and white and written as a letter: it included a signature, a greeting ("Dear Fellow Resident"), and a subject line. The informal letter resembled a flyer: it included a picture and multiple colorful graphics to draw attention to key information, including the enrollment period and the cost savings residents could expect from participating. While the core message in both letters was the same, the informal letter was one paragraph shorter and the text was reordered to put the most salient information at the top. As in Study 2, previous behavioral evidence would suggest that these additional adjustments to the informal letter would increase its effectiveness.[12,49]

Yet, residents assigned to receive the formal letter were 0.8 percentage points (pp) or 45% more likely to enroll in the medical transport program than residents who received the informal letter ($p < .001$, $t = 5.15$, 95% CI [0.50, 1.10], see Supplementary Table 7).

Study 4 was a pre-registered field experiment conducted in 2019 with the California Policy Lab (CPL), California Franchise Tax Board (FTB), and Golden State Opportunity (GSO). This study was part of a series of randomized experiments testing the impact of informational outreach on take-up of the California Earned Income Tax Credit (CalEITC) among low-income Californians. The results of this series of studies on the primary behavioral outcome—claiming of the CalEITC—are reported in Linos et al. (2022). The authors found no meaningful effect of any treatment on claiming of the CalEITC.

Here, however, we focus on a subset of one experiment ($N = 20,000$), in which we examine the difference between a formal and informal government mailer on engagement with the message. In this case, the letters included the exact same language, and formality was

manipulated exclusively by aesthetics. The formal letter was black and white and formatted as a traditional FTB letter. The informal letter used identical language as the formal letter, but included many visual design tools (e.g., colors, banners, and large boxes) to draw recipients' attention to certain information.

We find that 3.6% of recipients who received a formal letter clicked through to the website compared to 2.8% of recipients who received an informal flier—a 0.8 percentage point or 28% increase ($p = .002$, $z = 3.16$, 95% CI[.30, 1.3], see Supplementary Table 8). We note that this study included other treatments, including testing different non-governmental messengers, that are not reported here. The effect of formality is statistically similar across different messengers.

Predictions for Studies 2-4

In the same prediction survey ($N = 351$) described above (Table 1) where we asked researchers and practitioners to predict what attributes of government communications would be most effective, we also showed them the three sets of letters used in Studies 2-4. For each set of letters, participants were asked to predict which was more effective. In each case, over 84% of researchers and practitioners predicted that the *informal* letter would be more effective at getting people to take the requested action than the formal letter (see Table 2, Column 6). In reality, as shown (see Table 2, Column 5), participants who received the *formal* communication were 26% to 45% more likely to take the requested action than participants who received the informal communication. Table 2 and Supplementary Table 3 summarize these results.

Of note, experts' predictions about the effectiveness of each individual letter (Table 2) were consistent with their a priori beliefs about the effectiveness of different communication attributes (Table 1). That is, over 70% of the time, participants who predicted that an informal communication attribute would be more effective (e.g., using color instead of black and white) *also* predicted that the more informal letters were more effective.

While Studies 2-4 offer evidence of The Formality Effect in different policy contexts, field experiments of this nature cannot provide a clear understanding of the mechanisms through which the intervention operated. As such, we turn to online experiments to more carefully identify and disentangle the channels through which The Formality Effect may operate, in line with our conceptual framework. Specifically, Study 5 tests whether residents have strong expectations about how government communications *should* look, and whether these are higher than for equivalent communications sent by other sectors. Study 6 then examines whether formality operates as a heuristic for credibility and importance, as our conceptual model predicts.

Table 1. Predictions regarding general attributes of effective government communications

	(1) All respondents (N = 351)	(2) Academic (N = 177)	(3) Government (N = 97)
Attribute: Color			
Color	89.43%	90.40%	88.54%
Black and white	10.57%	9.60%	11.46%
Attribute: Language			
Formal	27.14%	29.55%	21.65%
Informal	72.86%	70.45%	78.35%
Attribute: Reading level			
High reading level	8.26%	12.43%	2.06%
Low reading level	91.74%	87.57%	97.94%
Attribute: Images			
Images	88.86%	89.27%	90.72%
Text only	11.14%	10.73%	9.28%

Notes: Results from a survey of 472 professionals recruited via social media, professional, and academic networks. Cells indicate the percentage of participants who completed the full survey (*N* = 351) who predicted that government communications with the corresponding attribute would be most effective at encouraging recipients to take the requested action. Participants were presented with binary choices for each attribute, and all attributes were presented in a random order. Column 2 reflects the subset of respondents who completed the survey and indicated that they worked in academia; column 3 reflects the subset of respondents who completed the survey and indicated that they worked in government.

Table 2. Studies 2-4: Field experiment results and corresponding predictions

(1)	(2)	(3)	(4)	(5)	(6)
Study	Policy domain	Outcome Measured	Sample Size	Formality Effect	% experts who predict direction of effect correctly (*N* = 351)
2	Business self-certification	Registration as a local, minority-owned, or woman-owned business	10,000	1.9 pp (25%) over base rate of 7.3%	10.0%
3	Local government service	Enrollment in an emergency medical transportation service offered by a local government	35,172	0.8 pp (45%) over base rate of 1.8%	15.7%
4	Earned Income Tax Credit	Website visits to learn more about the CalEITC	20,000	0.8 pp (28%) over base rate of 2.8%	10.8%

Notes: Column 5 presents results from each field experiment on the primary outcome (see Methods). Column 6 presents results from the prediction survey of experts and practitioners referenced in the introduction. This survey asked respondents to predict which of the letters used in each field experiment would be more effective, and to predict the effectiveness of general attributes of government communications (see Table 1). Results from each field experiment are shown in Supplementary Tables 6, 7, and 8, and results from the prediction survey are shown in Supplementary Tables 1, 2, and 3.

Study 5: Expectations about government communications

In an online experiment (*N* = 584), we examined people's expectations about government communications, relative to similar communications from other senders. In a factorial design, all participants were randomly assigned to a sender (government, nonprofit, or private company) and to a type of request (sign up for emergency alerts, attend an event, pay a fine). Participants were told to imagine that they received a letter in the mail from the sender corresponding with their treatment assignment, and that the letter asked them to take the action corresponding with their treatment assignment. We then measured participants' expectations about (1) formality of the letter; (2) the importance of taking action; and (3) the likelihood of facing consequences for not taking action.

As shown in Table 3, participants expected communications from the government to be significantly more formal than communications from either nonprofit or private company senders, regardless of the type of request ($F(2, 565) = 19.98$, joint $p < .001$). Participants also believed that it would be more important to act on a request from a government sender ($F(2, 565) = 28.32$, joint $p < .001$), and that they would be more likely to face consequences for not acting on it, compared to the same request from a nonprofit or private company sender ($F(2, 565) = 13.99$, joint $p < .001$).

When considering variation by type of request, we see similar patterns when the request was to sign up for emergency alerts or pay a fine. However, when the request was to attend a neighborhood event, there was no statistically significant difference by sender (government vs. nonprofit vs. private company) in perceived importance, nor in perceived risk of facing consequences (see Supplementary Table 10).

Together, these findings demonstrate that people expect government communications to be more formal than similar communications from other sources, and believe requests from the government are intrinsically more important to respond to. This suggests that residents' expectations about government communications may be one channel through which formality affects source credibility. Study 6 tests our hypothesized mechanisms more directly.

Table 3. Study 5: Expectations of government communications

	(1) Importance of taking action	(2) Likelihood of facing consequences	(3) Expected formality
Sender: Nonprofit	-0.513	-0.415	-1.110
	(0.083)	(0.080)	(0.225)
	[0.000]	[0.000]	[0.000]
Sender: Private company	-0.536	-0.240	-1.336
	(0.085)	(0.076)	(0.228)
	[0.000]	[0.002]	[0.000]
Ask: Attend event	0.116	-0.084	-0.492
	(0.084)	(0.072)	(0.226)
	[0.167]	[0.240]	[0.030]
Ask: Pay fine	0.814	1.355	1.264
	(0.092)	(0.083)	(0.230)
	[0.000]	[0.000]	[0.000]
Observations	584	584	584
Mean for govt	2.930	2.197	6.900
R-W p-values on Nonprofit sender	.001	.001	.001
R-W p-values on Private sender	.001	.003	.001

Notes: Estimates from linear models controlling for age, gender, college education, party affiliation, trust in government, and frequency of interaction with government. Importance and likelihood of facing consequences are both measured on 1 to 4 scale where 4 reflects "very important" or "very likely," respectively. Expected formality measured on a 1 to 10 scale where 10 reflects "extremely formal." Robust standard errors in parentheses; *p*-values in brackets. R-W p-values reflect Romano-Wolf multiple hypothesis corrected p-values on the primary predictors of interest.

Study 6: Formality as a heuristic

In a pre-registered online experiment ($N = 1,189$), we tested possible mechanisms underlying the Formality Effect. All participants were randomly assigned to see a single letter that was used in one of the field experiments, and then asked a series of questions to gauge their perceptions of the sender and the message. The results presented here (Table 6) pool outcomes across policy domains (business self-certification, local government service, EITC), but as shown in Supplementary Tables 12-14, we see similar patterns within each domain individually. Additional outcomes are also shown in Supplementary Table 15.

Confirming our definition of formality, the three formal letters from Studies 2-4 were rated by participants as 0.78 SD more formal than the three corresponding informal letters ($t = 14.68$; $p < .001$; 95% CI[0.67, 0.88]). Overall, there was also no significant difference in participants' comprehension of the letter: 93.7% of participants who saw an informal letter answered a subsequent comprehension question correctly, compared to 95.2% of participants who saw one of the formal letters ($t = 1.11$; $p = 0.27$; 95% CI[-0.01, 0.04]). As shown in the supplement, participants who saw a formal letter spent longer on the corresponding survey screen than participants who saw an informal letter ($t = 4.97$, $p < .001$, 95% CI[9.65, 22.2]; see Supplementary Table 15).

Testing our first hypothesis, we found that participants who saw one of the formal letters were 21 pp more likely to believe the letter was from the government ($M_{informal} = 52.0\%$; $t = 7.97$; $p < .001$; 95% CI[0.16, 0.26]). As predicted, participants who saw a formal letter also viewed it as significantly more credible than participants who saw an informal letter ($M_{informal} = 3.88$; $t = 6.41$; $p < .001$; 95% CI[0.24, 0.46]).

To explore the channels through which formality may influence credibility, participants were also asked about characteristics of the sender. Mapping onto existing literature, we examine different dimensions of trustworthiness and expertise by measuring perceptions of the sender's competence, genuineness, and authority. Participants who saw a formal letter perceived the sender to be more trustworthy and more of an expert than participants who saw an informal letter (all $p < .001$; see Table 4, panel A). We see similar effects of formality on source credibility when we create an index of sender perceptions as the mean of all individual measures (see Supplementary Table 16). After correcting for multiple hypotheses, all effects remain highly significant.

Next, we tested whether formality also operates as a heuristic for importance. Participants who saw a formal letter believed it was significantly more important to act upon than participants who saw an informal letter ($M_{informal} = 3.15$; $t = 6.83$; $p < .001$; 95% CI[0.36, 0.64]). Relatedly, participants who saw a formal letter were more likely to think the sender believed the action was important and relevant to the recipient, and less likely to believe the sender was trying to "scam them" (all $p < .01$; see Table 4, panel B). Again, after correcting for multiple hypothesis testing, all effects remain highly significant, and we find similar effects on an index of message perceptions (see Supplementary Table 16).

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In line with the results of the three field experiments, participants who saw a formal letter also reported being significantly more likely to take action had they received the letter in the mail ($M_{informal}$ = 2.94; t = 8.32; p < .001; 95% CI[0.51, 0.82]). In the supplementary materials we show that in a regression that includes all predictors, perceptions of credibility, whether the sender and message are important, and whether the message is a scam most strongly predict self-reported likelihood of acting (Supplementary Tables 17 and 18). However, given the design of Study 6 and the high correlation between the responses to some of these measures, it is challenging to fully disentangle the relative role and importance of each individual mechanism.

At the same time, we can exclude some mechanisms that could increase reported likelihood of taking action. For example, participants who saw a formal letter were directionally less likely to believe that the process of taking action would be easy ($M_{informal}$ = 3.95; t = -1.58; p = .11; 95% CI[-0.18, 0.02]), but still reported being significantly more likely to act. This suggests that in some circumstances, perceptions that an action is important could mitigate the impact of process complexity, which has been shown to be a significant barrier to taking action,[50] although this warrants further investigation.

More broadly, these findings build on Study 1 to further demonstrate that formality can be manipulated without affecting comprehension. Existing best practices emphasize the importance of clear and simple communication in resident-government interactions.[51] We find that formality can influence behavior without compromising recipients' understanding.

Table 4. Study 6: Mechanisms of The Formality Effect

Panel A: Perceptions of sender

	(1) Source is government	(2) Source is credible	(3) Sender is competent	(4) Sender is trustworthy	(5) Sender is genuine	(6) Sender is an authority	(7) Sender put in effort
Formal	0.209	0.350	0.214	0.218	0.208	0.399	0.091
	(0.026)	(0.055)	(0.051)	(0.060)	(0.060)	(0.061)	(0.055)
	[0.000]	[0.000]	[0.000]	[0.000]	[0.001]	[0.000]	[0.096]
Letter: Govt. service	-0.306	-0.128	0.122	0.123	0.140	-0.100	0.411
	(0.031)	(0.061)	(0.059)	(0.070)	(0.069)	(0.067)	(0.066)
	[0.000]	[0.037]	[0.039]	[0.077]	[0.043]	[0.135]	[0.000]
Letter: EITC	-0.259	-0.538	-0.196	-0.447	-0.408	-0.719	0.019
	(0.031)	(0.067)	(0.064)	(0.078)	(0.077)	(0.079)	(0.069)
	[0.000]	[0.000]	[0.002]	[0.000]	[0.000]	[0.000]	[0.788]
Observations	1,189	1,189	1,189	1,189	1,189	1,189	1,189
Mean for Informal	0.520	3.878	3.965	3.650	3.755	3.793	0.074
R-W p-values	.001	.001	.001	.001	.001	.001	.11

Panel B: Perceptions of message

	(1) Message is formal	(2) Sender thinks msg. is important	(3) Sender thinks msg. is relevant	(4) Message is important	(5) Message is a scam	(6) Likelihood of acting	(7) Ease of action
Formal	1.501	0.121	0.210	0.501	-0.256	0.663	-0.079
	(0.102)	(0.045)	(0.057)	(0.073)	(0.065)	(0.080)	(0.050)
	[0.000]	[0.007]	[0.000]	[0.000]	[0.000]	[0.000]	[0.113]
Letter: Govt. service	-0.340	0.141	0.224	0.011	-0.041	-0.044	0.291
	(0.124)	(0.053)	(0.070)	(0.090)	(0.078)	(0.098)	(0.059)
	[0.006]	[0.008]	[0.001]	[0.904]	[0.600]	[0.657]	[0.000]
Letter: EITC	-0.468	-0.063	0.073	0.050	0.406	0.124	-0.235
	(0.129)	(0.058)	(0.074)	(0.091)	(0.082)	(0.098)	(0.064)
	[0.000]	[0.278]	[0.320]	[0.580]	[0.000]	[0.204]	[0.000]
Observations	1,189	1,189	1,189	1,189	1,189	1,189	1,189
Mean for Informal	6.133	4.315	4.002	3.154	2.288	2.935	3.951
R-W p-values	.001	.014	.001	.001	.001	.001	.129

Notes: Estimates from OLS models controlling for age, gender, college education, party affiliation, trust in government, and letter topic. Source is government (Panel A, column 1) is a binary measure; formality (Panel B, column 1) is measured on a 1 to 10 scale; all other outcomes are measured on a 1 to 5 scale. See Methods and Supplementary Methods, Study 6, for more detail, and Supplementary Table 12 for additional outcomes. Robust standard errors in parentheses; *p*-values in brackets. R-W p-values reflect Romano-Wolf multiple hypothesis corrected p-values on the primary predictors of interest.

Discussion

Across three policy contexts and six studies, we document the existence of a counterintuitive Formality Effect, whereby residents are more likely to engage with and respond to formal government communications than informal ones, in part because formality acts as a heuristic for source credibility and importance. This research builds on and extends a growing interdisciplinary literature that focuses on testing the impact of message and language variations

aimed at reducing behavioral barriers to action. On average, behavioral messaging has been found to have a positive impact in government communications.[52] Our findings suggest that designing communications with the Formality Effect in mind could improve the effectiveness of light-touch interventions above and beyond current best practice.

This research also has several limitations that suggest directions for future research. First, online studies and field experiments are each limited in different ways. The online studies provide cleaner tests of mechanisms, but only measure behavioral intent, rather than actual action. The field experiments, on the other hand, capture real-world behavior more accurately, but because they were conducted in partnership with government partners, the different treatment arms were not as tightly controlled as a typical online study. Still, the fact that we see similar results across contexts and methodologies strengthens our fundamental hypothesis that a Formality Effect exists in government communication and requires further study.

Second, all experiments were conducted in the US, limiting the generalizability of this research to a context where trust in government is low and beliefs about misinformation in communication are high. It is possible that there is a ceiling effect in contexts where trust in government is high, such that any government communication—regardless of how it is presented—is seen as credible. Future research could explore whether and how the Formality Effect translates to other cultural contexts.

Third, we have documented two potential channels through which formality can affect resident behavior: the recipient's perception of the source and the message. Further research could investigate whether formality can also affect the recipient's perception of themselves or their relationship to the sender. For instance, some prior evidence on altercasting suggests that emphasizing specific social roles can increase persuasion, which could in turn affect behavior.[53] It is unclear whether formality in communication affects one's sense of self. Relatedly, it is possible that formality could affect the extent to which a recipient feels respected by the sender. This, too, could be explored as a potential channel through which formality may affect behavior.

Additional studies could also aim to disentangle the impact of formality on engagement and action. Acting on a received communication entails two distinct behavioral steps: First, residents must receive and read the communication; then they must decide to act. Study 6 suggests that formality affects recipients' likelihood of taking action once they engage with a communication. However, due to the nature of the online and field experiments, we could not measure whether formality also increases the likelihood of engaging in the first place.

Fourth, the studies were conducted in a general equilibrium where government communication tends to be more formal than private sector communications. Therefore, our effects likely depend on current expectations about government communication, which could evolve over time. Additional studies should examine whether the Formality Effect extends to non-governmental contexts, including nonprofits and private sector communications, where expectations on formality may differ. Importantly, while we look at average effects in a government context, future research could shed light on how demographics or individual

characteristics interact with the Formality Effect within and across contexts to provide further insights into when (and for whom) we might expect the Formality Effect to be strongest.

Last, while Studies 2-4 span three different policy contexts, there are many behaviors that governments ask of their residents that are not reflected in these field experiments. Requests made of residents can range from punitive and consequential (e.g., paying a fine) to voluntary and relatively inconsequential (e.g., attending a town hall meeting). While Study 5 addresses some of these contexts, self-reported intent does not always predict behavior.[54] Thus, future field experiments could document the impact of formality in a wider range of policy contexts. Similarly, given that all the studies in this paper focus on mail-based written communication from state and local government agencies, future research could test whether The Formality Effect persists at the federal level, as well as across communication modalities, including text messages, in-person interactions, websites, or social media interactions.

Ultimately, these findings offer immediate implications for policymakers and researchers, while also laying the foundation for continued research on government-resident interactions, communication, and the potential of behavioral approaches to affect both.

Methods

Ethics and Pre-registration

Studies 1, 5, and 6 were approved by the Harvard University Committee on the Use of Human Subjects (IRB22-0824). The prediction survey was approved by the University of California, Berkeley Committee for Protection of Human Subjects (2021-06-14435). Study 4 was approved by the California Health and Human Services Agency Committee for the Protection of Human Subjects (2019-002). Studies 2 and 3 were conducted by the Behavioral Insights Team and the local government partner agencies, and were aimed at improving existing government processes and service delivery rather than contributing to generalizable knowledge. Four of the authors on this manuscript were employees at BIT at the time Studies 2 and 3 were conducted (and were not affiliated with their current agencies or universities). BIT had its own internal ethics review process and thus, these two studies did not go through formal IRB approval at an academic institution.

Studies 1, 4, and 6 were pre-registered at OSF (Study 1: https://osf.io/93dhw, https://osf.io/jcvbs; Study 4: https://osf.io/z8ebc; Study 6: https://osf.io/aew8z).

Analysis

All studies were analyzed using Stata 15.

Study 1

Sample. Participants were recruited through Prolific to complete a 2-minute online survey for which they were paid $0.40 each. All participants consented to participate. The sample was

balanced on gender, and limited to participants located in the United States, who were fluent in English, who had not participated in pilot studies, and who passed an initial attention check. Other sample characteristics can be found in Supplementary Table 4.

A total of 696 participants (mean age = 37.9 years, *SD* = 13.0; 50.0% female) completed the study. After relevant data quality exclusions (see Supplementary Methods, Study 1), balanced evenly across conditions ($\chi^2(3)$= 3.04, *p* = .39), our final analytic sample consists of 688 respondents (mean age = 37.8 years, *SD* = 13.0; 48.3% female).

The sample size calculation was informed by pilot experiments that yielded effects of 0.2 to 0.4 standard deviations. We estimated that with a sample of 170 participants per condition, and standard assumptions of a 5% significance level and 80% power, this study would have a minimum detectable effect of 0.3 SD.

Methods. All participants who passed an initial attention check were randomly assigned to one of four conditions: (1) informal aesthetic and informal language; (2) formal aesthetic and informal language; (3) informal aesthetic and formal language; or (4) formal aesthetic and formal language. For each condition, we adapted the language and design of a real government communication to manipulate the relevant formality construct. After viewing the letter corresponding with their treatment assignment, all participants were asked to rate how formal the design was, how formal the language was, and how formal the letter was overall. Each dimension of formality was measured on a 1 to 10 scale in which 10 reflected "extremely formal." Treatment materials are available in Supplementary Figure 1.

We analyze the difference in perceived formality by condition via a covariate-adjusted OLS model, controlling for age, gender, college education, party affiliation, and trust in government, with robust standard errors. All assumptions for interpretation were met. Because we had one primary outcome, as detailed in our pre-registered analysis plan, we do not correct for multiple hypotheses. The results were considered significant if *p* < .05 and all tests were two-sided.

Note that we originally pre-registered and ran Study 1 in January 2022 on Amazon Mechanical Turk (https://osf.io/wkzcj). At the time of the original pre-registration, the study had been run, but not yet analyzed. However, due to data quality issues, we re-ran the study on Prolific in December 2022 and updated the original pre-registration accordingly prior to running the new version of the study (https://osf.io/jcvbs). The study reported in this manuscript deviates from the original pre-registered study in the following ways: First, we added two secondary outcomes. In addition to measuring formality as a primary outcome, the present study also measures formality of design and formality of language as separate dimensions. Second, the present study was conducted on Prolific.

Study 2

Sample. Of approximately 27,000 businesses registered with the partner city for Study 2, 10,000 were selected at random to comprise the sample for this study. The sample size was determined by available data and logistical considerations. While demographic information on study participants is not available, using publicly available data, we estimate that the city's population is around one-third non-Hispanic White, 3% Black, and 50% Hispanic.

Methods. In a simple randomization, the 10,000 businesses that were randomly selected to be part of the study sample were then randomly assigned with equal probability to receive one of two letters sent by the local city government: (1) formal; or (2) informal. The formal letter offered information about the process for self-certifying as a local or minority-owned business, as well as background information on why the city was requesting this information. The letter was written with impersonal and complex language and used a formal aesthetic (see Supplementary Figure 2). The informal letter offered the same high-level information, but included less detail, and used an informal aesthetic and simpler, personalized language (see Supplementary Figure 3). All letters were addressed to the business owner, including name and address. The informal letter included a personal greeting "Dear [name]," while the formal letter included an impersonal greeting "Dear Business Owner."

Both letters directed recipients to visit a city website where they could self-certify as a local, minority-owned, or woman-owned business. The city collected all data submitted through the website. Our primary outcome was valid self-certifications in the 30-day period after the letters were mailed. A valid self-certification was one in which the respondent self-certified that they were a local, minority-owned, or woman-owned business.

In an intent-to-treat analysis, we analyzed the average effect of assignment to the formal condition relative to the informal condition via a covariate-adjusted OLS model that controls for business license type, preferred communication modality, and initial business registration year. While we report results from a linear probability model for ease of interpretability, as a robustness check we include results from a logistic model in Supplementary Table 6. Results do not differ meaningfully between the two models. In both models, results were considered significant if $p < .05$ and all tests were two-sided.

Study 3

Sample. The sample for Study 3 included 52,756 single-family utility customers who were not enrolled in the emergency medical transport program at the time of the study. The sample size was determined by available data and logistical considerations. While demographic information on study participants is not available, we estimate using publicly available data on the city that the population is about half non-Hispanic White, almost 15% Black, and around 20% Hispanic.

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Methods. In a randomized experiment, the Behavioral Insights Team and partner city designed and tested the impact of three informational mailers on program enrollment. All mailers were sent by the local government partner. Each household in the sample was randomly assigned to one of three conditions with equal probability:

1. *Formal:* Households assigned to the formal condition received a mailer that offered the same information and instructions as in the status quo condition, but with a formal aesthetic and formal language (see Supplementary Figure 4).
2. *Status quo (informal):* Households assigned to the status quo condition received the standard city mailer that offered information about the program and instructions for enrolling. The mailer was informal in language and aesthetic (see Supplementary Figure 5)
3. *Social norms (informal):* Households assigned to the social norms condition received a mailer with the same information and design as in the status quo condition, but with an added sentence emphasizing that a majority of city residents were already participating in the program

The randomization was stratified by whether the billing (mailing) address matched the physical (premise) address. Cases where the mailing address and premise address matched were more likely to be owner-occupied addresses, and thus the resident was more likely eligible to sign up for the membership program, which was only for city residents. Overall, 91.9% of mailing addresses in the sample universe matched their corresponding premise address.

The primary outcome was enrollment in the membership program in the 30 days after the letters were mailed. This was measured via administrative data on enrollments collected by the program. In an intent-to-treat analysis, we evaluate the average effect of assignment to the formal condition relative to the status quo condition ($N = 35,172$) via a covariate-adjusted OLS model that controls for randomization strata and an indicator for whether the mailing address was located in the partner city. The social norms condition is not included in the analysis reported in this manuscript. We use robust standard errors to correct for heteroskedasticity and obtain unbiased standard errors. Additionally, while we report results from a linear probability model for ease of interpretability, we also include results from a logistic model in Supplementary Table 7. Results do not meaningfully differ between the two models. The results were considered significant if $p < .05$ and all tests were two-sided.

Study 4

Sample. The sample for Study 4 was drawn from a database purchased from private marketing firm TargetSmart. The sample was comprised of low-income Californians and limited to individuals between the ages of 18 and 70. The total sample was approximately 1.2 million individuals in one million households, determined by available data.

Methods. This study was a subset of a broader field experiment that included 96,370 low-income Californian households. We focus on a subset of 20,000 households that were randomly assigned to receive either a formal or informal informational mailer about the CalEITC. All letters and flyers contained the same information in English and Spanish and were mailed in February 2019 (see Supplementary Figures 6 and 7). The randomization was stratified by county, zip code, whether the primary contact was missing a date of birth in the dataset, and household treatment assignment from a prior study conducted among the same sample by the research team.

Each informational mailer directed recipients to visit the Franchise Tax Board's website to learn more about the CalEITC. The website offered information about CalEITC eligibility and the process of claiming the credit. The mailers were sent by the state government.

Our primary outcome of interest is unique click-throughs to the website, which were measured in aggregate by treatment condition through trackable URLs that were included in each letter. We analyze differences in click-through rates by condition using a two-sided two-sample test of proportions. Because outcome data are aggregate click-throughs, we cannot formally test whether assumptions for hypothesis testing are met.

Study 5

Sample. Participants were recruited through Prolific to complete a 1-minute online survey for which they were paid $0.25 each. All participants consented to participate. The sample was balanced on gender, and limited to participants located in the United States, who were fluent in English, and who had not participated in pilot studies. Participants who did not pass an initial attention check were excluded. A total of 597 participants completed the survey (mean age = 41.6 years, *SD* = 13.8; 48.9% female). After relevant data quality exclusions (see Supplementary Methods, Study 5), balanced evenly across conditions ($\chi^2(8) = 1.58$, *p* = .99), our final analytic sample consists of 584 participants (mean age = 41.4 years, *SD* = 13.8; 49.0% female). All other sample characteristics can be found in Supplementary Table 9.

We estimated that with a sample of approximately 70 participants per condition, and standard assumptions of a 5% significance level and 80% power, this study would have a minimum detectable effect of 0.48 SD for scale-based outcomes. This was considered sufficient based on results of pilot experiments.

Methods. All participants were assigned to one of three conditions corresponding with different senders (government, nonprofit, or private company) and one of three conditions corresponding with different types of requests (signing up for emergency alerts, attending a community event, or paying a fine). Participants first saw a short description corresponding with their experimental condition, such as: "*Imagine you receive a letter in the mail from the government asking you to sign up for emergency alerts.*" Each participant saw the same description, but the sender and request varied depending on their condition assignment. Thereafter, participants were asked (1) how important they believed it would be to take action; (2) how likely they would be to face

consequences if they did not take action; and (3) how formal they would expect the communication to be.

We evaluated differences in perceived importance, likelihood of consequences, and expectations of formality across by sender condition via covariate-adjusted OLS models, controlling for age, gender, college education, party affiliation, trust in government, and self-reported frequency of interaction with government. Models with pooled outcomes also included fixed effects for the type of request. All assumptions for interpretation were met.

To correct for multiple hypotheses, we calculated Romano-Wolf stepdown adjusted p-values, which control the family-wise error rate. Results were considered significant if the adjusted $p < .05$ and all tests were two-sided.

Study 6

Sample. Participants were recruited through Prolific to complete a 4-minute online survey for which they were paid $0.80 each. All participants consented to participate. The sample was balanced on gender, and limited to participants located in the United States, who were fluent in English, and who had not participated in pilot studies. We also excluded participants who did not pass an initial attention check. A total of 1,289 participants (mean age = 38.8 years, $SD = 13.6$; 48.5% female) completed the study. After relevant data quality exclusions (see Supplementary Methods, Study 6), balanced evenly across experimental condition ($\chi^2(5) = 4.62$, $p = .46$), our final analytic sample consists of 1,189 respondents (mean age = 38.8 years, $SD = 13.6$; 47.8% female). All other sample demographics are listed in Supplementary Table 11.

We estimated that with a sample of 200 participants per condition, and standard assumptions of a 5% significance level and 80% power, this study would have a minimum detectable effect of 0.28 SD for scale-based outcomes and 12 percentage points for binary outcomes. This was considered reasonable based on results of pilot experiments.

Methods. All participants were assigned to one of six conditions corresponding with the six government letters used in Studies 2-4. In total, three conditions were associated with an informal letter from Studies 2-4, and three conditions were associated with a formal letter from Studies 2-4.

We evaluated the impact of assignment to one of the conditions associated with a formal letter on eight primary outcomes: (1) perceived formality; (2) comprehension; (3) sender; (4) importance of taking action; (5) source credibility; (6) perceived difficulty of taking action; (7) likelihood of taking action; and (8) time spent on the survey screen with the letter.

We also evaluated nine outcomes related to perceptions of the sender. Each measured agreement with a statement that began, "The sender of this letter…": (1) put in a lot of effort; (2) thinks it's important for me to take action; (3) is important; (4) thinks this letter is relevant for me; (5) is competent; (6) is trustworthy; (7) is genuine; (8) is an authority; (9) is trying to scam

me. All questions were presented in a random order. See Supplementary Methods, Study 6, for question text and study procedures.

Each outcome was evaluated via a covariate-adjusted OLS model, controlling for participant age, gender, college education, party affiliation, and trust in the government. All models with pooled outcomes also included fixed effects for the letter's policy domain. The data meet all assumptions for interpretation except that of equal variances. We thus use robust standard errors in all analyses.

To correct for multiple hypotheses, we calculated Romano-Wolf stepdown adjusted p-values, which control the family-wise error rate. Results were considered significant if the adjusted $p < .05$ and all tests were two-sided.

Supplementary Materials are available on our website ([here](#)).

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