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Enacted in an effort to discourage negative political advertising, American regulations mandate that candidates endorse their ads (“My name is _____, and I approve this message.”). Four studies suggest that mandatory endorsements enhance the perceived credibility of some ads these regulations were designed to discourage. This research tests for what types of messages mandatory endorsements have this effect, and why. Mandatory endorsements boosted evaluations of policy-focused attack ads—those typically plagued by overcomeable skepticism—but had no consistent effect on positive or character-focused ads. Mandatory endorsements boost ad believability—largely outside of participants’ awareness—for two reasons: (1) the tagline offers a legitimizing association with regulation and (2) the candidates’ own personally delivered endorsement language offers an implicit promise of the ads’ truth value. The authors discuss how these findings bring order to and extend previous work on mandatory endorsements and ironic effects of communications requirements. Finally, they consider how regulations could be reformed to promote the public good by informing (without misleading) the electorate.

Keywords: Stand by Your Ad, advertising regulation, negative advertising, political advertising, persuasion

Online Supplement: <http://dx.doi.org/10.1509/jmr.14.0654>

How Encouraging Niceness Can Incentivize Nastiness: An Unintended Consequence of Advertising Reform

The electorate detests negative political advertising. A 1996 Markle Foundation Survey found that “less negative campaigning” was the second-most-desired political reform. Another survey found that 61% of Americans gave the maximum ratings for how “bothersome” they found negative campaigning to be (Pew Research Center for the People & the Press 2003). Most dramatically, 80% of Americans agreed

with the statement “Negative attack-oriented campaigning is undermining and damaging democracy” (Institute for Global Ethics 2002).

Despite such widespread dislike, negative campaigning persists. Several decades ago, political reformers turned to regulation in an effort to disincentivize reliance on attack-oriented rhetoric. Building on a volunteer effort in Minnesota (Cappella and Brewin 1998) and a legislatively codified version in North Carolina (North Carolina State Board of Elections 1999), Senators John McCain and Russ Feingold included the Stand by Your Ad (SBYA) provision in their 2002 Bipartisan Campaign Reform Act. As any viewer of American political ads of the last decade has heard, candidates for federal office now must identify themselves by name and indicate that they “approve this message”—that is, stand by their ad. Beyond promoting transparency regarding which ads candidates have funded themselves, the designers of SBYA hoped to disincentivize reliance on negative ads

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(Gale et al. 2005). Speaking on the Senate floor, Senator John McCain explained that if candidates know they will have to identify themselves and approve the message, they will no longer be as willing to lean on negative political ads (Congressional Record 2004). The assumption is that candidates should fear a backlash if they are so clearly associated with, much less explicitly approve of, negative advertising.

In this article, we ask not whether forcing messengers to explicitly endorse the content of their messages changes which messages they send, but whether such mandatory endorsements affect recipients' assessments of those messages. We consider whether, in regulators' efforts to discourage the kind of messaging that recipients dislike, they ironically may be incentivizing reliance on that exact content. We are not the first to examine how SBYA influences people's responses to political ads. Indeed, a quick review of previous literature reveals findings that appear inconsistent on the very basic question of whether SBYA enhances (Gale et al. 2005), diminishes (Brooks and Murov 2012), or has no effect on (Meirick and Nisbett 2011) political advertising's effectiveness. Readers knowledgeable of this literature are left with the impression that SBYA either does not have predictable effects or has effects that emerge only when restricting analyses to specific groups (Brooks and Murov 2012; Meirick and Nisbett 2011). After developing our argument, we return to these superficial inconsistencies to discuss how our theoretical framework can give some order to this previous evidence.

What most clearly differentiates the present research from previous efforts is our thorough and systematic examination of whether, when, and why SBYA influences the effectiveness of political advertising. We study mandatory endorsements in the context of political marketing given the clear importance to current policy, but we see the most general theoretical value in our testing seven accounts of *why* mandatory endorsements have an effect. By understanding both how and why communications requirements have unintended consequences, we can be in a stronger position to understand which policies are more or less likely to promote the greater good through a well-informed electorate. We return to these implications in the "General Discussion" section.

Once political candidates were required to stand by ads, did their tone turn more positive? On the contrary, the tone of political ads has grown increasingly negative (Fowler 2012). The percentage of negative ads has grown from 29% in 2000, 44% in 2004, 51% in 2008, to 64% in 2012 (Fowler and Ridout 2013). In the final week of the 2016 presidential election, a stunning 92% of the ads were negative (Wallace 2016). Although this increase partially reflects the rise in advertising by independent political groups, which tend to run particularly negative messages, it also stems from an increase in negativity by the candidates themselves (Brooks and Murov 2012; Fowler and Ridout 2010, 2013). Thus, if mandating SBYA has disincentivized reliance on negative ads (by encouraging a backlash against those who use them), political campaigns seem to be unaware of this. Alternatively, the electorate may have habituated to the omnipresent tagline, meaning that its inclusion may not change the way viewers respond to ads.

In this article, we consider the possibility that when candidates are required to explicitly stand behind their ads, this may actually boost recipients' attitudes toward and the perceived credibility of those messages. We ultimately evaluate seven mechanistic accounts that could explain why mandatory

endorsements would boost advertising effectiveness. Before outlining those accounts (and the empirical patterns of results they predict), we first consider which types of political advertisements SBYA would be most likely to enhance. That is, our argument (as well as our data collection) begins by considering *the message question*: For what types of ads might a minimal cue such as a mandatory endorsement assuage skepticism and restore faith in the message? Once we identify the types of ads that SBYA can enhance, we move to *the tagline question*: When an ad can be enhanced by SBYA, which one (or more) of the seven accounts explains what it is about the tagline that endows the message with greater legitimacy?

BACKGROUND

The Message Question

First, we propose that negative—more than positive—ads have great potential if viewers can be persuaded to set aside their skepticism. Negative ads make a case against an opponent instead of merely in support of the sponsoring candidate (Kaid and Johnston 1991; Merritt 1984; Shapiro and Rieger 1992). It is this informational property that makes negative ads not merely attention capturing (Lau 1985) but especially informative (Sides, Lipsitz, and Grossmann 2010). In part, this is because in the absence of information, voters have generally positive feelings about political candidates (Bruner and Tagiuri 1954; Sears 1983), meaning that negative ads will force many to update their perceptions. In addition, by drawing implicit contrasts (as pure attack ads do) or explicit contrasts (88% of contrast ads are classified as negative in a forced dichotomy; Franz et al. 2008), negative ads argue how a candidate is superior to an opponent (Sides, Lipsitz, and Grossmann 2010). However, because viewers are especially skeptical of negative ads as potentially misleading (Merritt 1984; Surlin and Gordon 1977; Stewart 1975) and, thus, of questionable usefulness (Pinkleton 1997), the perceived truthfulness or legitimacy of negative advertising is a major barrier to such messages' effectiveness—a barrier that, if overcome, could unleash negative ads' potential.

Second, we suggest that the skepticism that plagues some negative ads may be more easily addressable than the skepticism that plagues other ads. Some attack ads reflect mere mudslinging that focuses not on policy stances but, instead, on candidates' character (e.g., their past moral failings; Johnson-Cartee and Copeland 1989). Unlike for policy-focused attack ads, whose main barrier to effectiveness is their perceived truth value, character-focused attack ads are more likely to be dismissed because the nature of the discourse is deemed less germane to a voting decision (Roddy and Garramone 1988). As one example, John McCain's "Celebrity" ad showed images of Britney Spears and Paris Hilton before cutting to Barack Obama speaking in front of chanting crowds in Europe; a narrator dismissively labeled him "the biggest celebrity in the world." The success of this ad likely depends on whether viewers resonate with the suggestion that Obama's celebrity status does not befit a statesman, not on whether the content of the message is seen as credible and believable. Mudslinging and character attacks push the bounds of what is civil or relevant; policy attacks are typically relevant, but may be noncredible.

Third, even when skepticism is what plagues both policy and character-focused ads, previous research and theory have

argued that viewers can be more easily persuaded to set aside their skepticism toward policy-focused ads. People initially assume the best about the moral character of specific individuals (Critcher and Dunning 2013) and continue to think positively about such character until clearly proven wrong (Critcher and Dunning 2014). This means that people may be more reluctant to embrace the truth value of character-focused attacks than policy-focused ones. If mandatory endorsements could encourage viewers to set aside their skepticism, endorsers may have an easier time assuaging the more addressable skepticism viewers have—the skepticism toward policy-focused attack ads. Although we directly test this proposal in Studies 1 and 2, we conducted a pretest ($N = 110$, Amazon’s Mechanical Turk [MTurk]) that could lend some confidence to this line of argumentation.

We explained to participants that policy-focused attacks “criticize an opponent for the policy positions that he or she has taken,” and that character-focused attacks “criticize an opponent’s personal characteristics or character” (Goldstein, Franz, and Ridout 2002; Goldstein et al. 2011). We wanted to test whether participants’ self-reported threshold is higher for believing character-focused attacks than policy-focused attacks. We asked participants whether they “would be more reluctant to believe a policy-focused attack (without extremely convincing evidence that the claim is true) or more reluctant to believe a character-focused attack (without extremely convincing evidence that the claim is true).” We counterbalanced which label was attached to which endpoint, but we converted responses onto the version with these labels: -3 (“more reluctant to believe a policy-focused attack”), 0 (“equally reluctant”), and $+3$ (“more reluctant to believe a character-focused attack”). Consistent with our argument, participants indicated greater reluctance to embrace character-focused attacks without extremely convincing evidence ($M = .80$, $SD = 1.81$, $t(109) = 4.65$, $p < .001$, $d = .44$). This suggests that if SBYA is able to overcome recipient skepticism toward negative ads, this should be easier to do for policy-focused attack ads, because the threshold to embrace such ads may be lower (H_1). Note that the validity of this pretest is premised on participants having some access to the elasticity of their own skepticism, though it is possible that such self-insight is completely lacking. In other words, this evidence is suggestive, but certainly not a substitute for the direct tests of the message question in Studies 1 and 2.

The Tagline Question

If Studies 1 and 2 find that requiring candidates to explicitly endorse their messages enhances the perceived legitimacy or credibility of policy attack ads (H_1), this could be for one of seven reasons. Studies 3 and 4 examine policy attack ads to answer the tagline question—When SBYA boosts ad evaluations, why does such a boost occur?—by considering seven accounts. We begin by testing whether SBYA enhances ad legitimacy due to reasoned accounts (H_{2a-c} ; Study 3)—that is, recipients’ explicit beliefs or lay theories, rooted in either confusion or speculation that SBYA is a valid signal of truthfulness. Once we determine whether SBYA’s influence is primarily reasoned or mindless (i.e., outside of awareness and independent of explicit beliefs about SBYA’s meaning), we dissect SBYA to distinguish which one (or more) of four critical components of mandatory endorsements explains SBYA’s influence (H_{3a-d} ; Study 4). Next, we briefly lay out

Table 1
SUMMARY OF HYPOTHESES

Hypothesis	Summary
H_1 (SBYA effectiveness)	Adding SBYA to an ad boosts its effectiveness, at least for policy-focused attack ads.
H_{2a} (positive signal)	H_1 is supported because recipients explicitly reason that SBYA signals candidates chose truer content.
H_{2b} (coercion confusion)	H_1 is supported because of confusion that SBYA is a freely chosen (not regulation-required) signal.
H_{2c} (regulation confusion)	H_1 is supported because of confusion that SBYA reflects that regulators verified that ads are true.
H_{3a} (civil conclusion)	H_1 is supported because the tagline offers a nonharsh conclusion to an otherwise harsh message.
H_{3b} (candidate speaks)	H_1 is supported because the candidate redirects attention to self after focusing on opponent’s shortcomings.
H_{3c} (implicit-promise)	H_1 is supported because “I approve this message” is interpreted as a personal promise of truthfulness.
H_{3d} (regulation legitimization)	H_1 is supported because association with regulation legitimizes the content of the message.

Notes: H_1 suggests the boundary conditions on the effect of SBYA, thereby defining its scope (the message question). The remaining seven hypotheses offer explanations of why SBYA may offer such a boost.

these non-mutually exclusive mechanisms, note (when possible) their psychological plausibility, and describe the empirical pattern of results they anticipate. Table 1 summarizes all eight hypotheses for easy reference, and Figure S1 in the Web Appendix considers the relationship among them.

Reasoned accounts (H_{2a-c}). People may explicitly reason that ads that include the SBYA tagline are truer, either because they think candidates choose to run truer ads when SBYA is mandated or merely because viewers are confused about what SBYA implies. The former possibility relates to the psychologically interesting question of the extent to which SBYA may operate through explicit reasoning as opposed to automatic inferences (Critcher and Risen 2014). The latter possibilities, as detailed next, reflect confusion-based artifacts.

Voters might reason that candidates will only want to put their name and endorsement behind ad content that is somewhat true—perhaps because it could be embarrassing or damaging should the veracity of those endorsed ads ever be successfully challenged. This positive signal hypothesis (H_{2a}) predicts that people’s explicit or lay beliefs that SBYA is a positive signal of an ad’s truthfulness explains any observed boost caused by SBYA. The second and third reasoned hypotheses also posit that people view SBYA to be a positive signal of ad validity but identify specific sources of voter ignorance or confusion as underlying such beliefs. One possibility is that people may not realize that message endorsement is mandatory. People may believe ads more if they (mistakenly) think the candidates themselves freely decided to include the added assurance that they stand behind the ad. By this coercion confusion hypothesis (H_{2b}), once participants are informed that the tagline is mandatory, the credibility boost SBYA offers should fade. The third possibility is that people may (incorrectly) think that regulators vet all ads with SBYA to make certain they meet a certain standard of truth. By this regulation confusion hypothesis (H_{2c}), once participants are informed that regulators do not verify that ads’ content is true, SBYA should no longer boost ad evaluations.

Determining the critical ingredient of SBYA itself (H_{3a-d}). If we do not localize SBYA's effects on confusion (H_{2b-c}), we can ask what it is about SBYA itself that may alleviate skepticism. Note that regardless of whether SBYA influences ad believability through a largely reasoned (H_{2a}, an explicit belief that SBYA is a positive signal) or mindless (mostly independent of participants' explicit theories and awareness) route, we can still ask what it is about SBYA that lends credibility. We discuss four non-mutually exclusive possibilities.

By the civil conclusion hypothesis (H_{3a}), the benefit of mandatory endorsement is that it offers a positive, disarming ending to an otherwise negative, aversive message. This predicts that any such neutral tagline—not merely the standard SBYA—should enhance advertisement legitimacy. A second hypothesis also finds significance not in SBYA's specific content but in the person delivering the tagline. By the candidate speaks hypothesis (H_{3b}), the attack on another candidate is bolstered because the candidate him- or herself appears in the ad to deliver the mandatory endorsement, thereby helping translate negativity toward the attacked opponent into esteem for the sponsoring candidate. This hypothesis suggests that mandatory endorsements should provide less credibility when not delivered by the candidates themselves.

A third possibility is a narrower version of the candidate speaks hypothesis, because it places import on what the candidate says. By this implicit promise hypothesis (H_{3c}), when candidates appear in the ad and offer their personal approval of the message, this is taken to reflect a personal promise that what was just said is true. Furthermore, because people tend to accept others' promises as sincere (Charness and Dufwenberg 2006; Ellingsen and Johannesson 2004; Kerr and Kaufman-Gilliland 1994; Ostrom, Walker, and Gardner 1992; Sally 1995), they may be inclined to trust the message. Although it is an open question whether *coerced* promises or assurances can be persuasive, prior research showing that coerced apologies are (Risen and Gilovich 2007) lends credibility to our extension. The hypothesis posits that the boost candidates experience from "approving this message" comes from the credibility implied by this personally delivered promise (and not by merely saying anything, as the candidate speaks hypothesis posits).

A fourth possibility argues that mandatory endorsements inspire confidence because they are associated with confidence-inspiring regulation. Steinhart, Carmon, and Trope (2013) note that in consumer product warnings, there is a tension between the negative information conveyed in the warning itself and the more abstract veneer of trustworthiness that the disclosure implies. We suggest that SBYA may have a similar ability to provide a disarming veneer not because of its forthcoming transparency (Steinhart, Carmon, and Trope 2013) but because of its legitimizing association with regulation. Although we are not aware of previous empirical findings directly testing this regulation legitimization hypothesis (H_{3d}), this logic was foreshadowed by a recent policy debate. In the late 2000s, when the U.S. Congress was debating whether to grant the Food and Drug Administration (FDA) authority to regulate the tobacco industry, some public health advocates worried that if consumers knew the FDA was regulating *some* aspects of the industry, then they may have greater trust in tobacco products as a whole (Benson 2010). We suggest that the regulation-complying tagline may offer similar reassurance. If so, then other salient taglines should

prompt a legitimizing boost as long as they are perceived to be the product of regulation.

Relation to and Inconsistent Evidence from Previous Research

The present article is not the first to investigate ironic effects of regulating communications. Some of that research has examined how forcing communicators to disclose information changes what else they communicate. For example, Cain, Loewenstein, and Moore (2005, 2011) required some communicators to disclose that they had a conflict of interest (i.e., a financial interest that would incentivize them to share information that was biased in a particular direction). However, such mandatory disclosure had an ironic effect on the communication itself: it encouraged communicators to distort their messages even more, presumably out of a fear that whatever they said would be discounted by message recipients.

In the present research, we do not examine such "communicator effects" (because we hold the ad content constant while varying the presence of the mandatory endorsement) but, instead, examine only perceiver effects (how mandatory endorsement changes the perceived believability of the communication). Cain, Loewenstein, and Moore (2005, 2011) show that the mandatory disclosure of the conflict of interest encouraged recipients to be more skeptical, though not as skeptical as they should have been. We predict instead that mandatory endorsements will *reduce* skepticism. This is because Cain, Loewenstein, and Moore examine a case in which the mandatory disclosure teaches recipients something they would not otherwise know—that communicators had an incentive to distort the truth. When politicians indicate that they "approve this message," recipients are likely not receiving similar news. That is, we suspect most people realize political ads have an intent to persuade.

As we alluded to previously, some researchers have looked for effects of mandatory endorsements (SBYA) on message effectiveness. Now that we have developed our account—in particular, the message question (for which we had clearer a priori hypotheses)—we consider why previous research may have yielded mixed results. First, not all such studies have experimentally varied the presence of SBYA for a single message but have instead compared reactions to ads with the SBYA tagline with different ads without SBYA (e.g., Gale et al. 2005). Second, some research has examined the influence of SBYA on a single ad in a single study, which raises the possibility that studies will produce different conclusions to the extent that SBYA's effects are limited to certain types of ads (the message question). Although null effects are inevitably difficult to interpret, the one article to show an overall boost for candidates who run SBYA ads tested this hypothesis on a single policy-focused attack ad (Gale et al. 2005), whereas the article showing a backlash used a character-focused attack (Brooks and Murov 2012). The article that showed no overall effect of SBYA did find that a policy attack ad was more persuasive for politically knowledgeable individuals (Meirick and Nisbett 2011). If the politically knowledgeable were also most likely to know that SBYA is mandated by regulation, this finding could be consistent with the regulation legitimization hypothesis (H_{3d}). Regardless, we differentiate our own efforts by testing for effects across many ads of multiple types (Studies 1 and 2) and by systematically examining why SBYA has the effect it does (Studies 3 and 4). It is only by answering this latter question that

smarter policies be pursued and a more generalized understanding of mandatory endorsement effects be achieved.

STUDY 1: TESTING THE MESSAGE QUESTION WITH ACTUAL POLITICAL ADS

We wanted to conduct an experimental test of whether SBYA enhances the effectiveness of policy-focused attack ads. H_1 predicted that adding SBYA to policy attack ads, but not necessarily those of the other three varieties, would make the ads seem more believable and, thus, prompt participants to have more positive evaluations of the sponsoring candidates. Both Studies 1 and 2 address the message question but differed in whether they used real political ads (Study 1) or ones written for the purpose of the present research (Study 2). For each participant, we chose four ads to retain the SBYA tagline but digitally edited out this tagline for the other four. Although these particular ads had never aired without the SBYA tagline, it is worth noting that it is not an unusual occurrence to see political ads without the tagline. That is, third-party groups run ads on behalf of candidates, and the candidates do not have to approve those messages. Testing whether SBYA enhances the believability of (some) real ads provides the best test of whether SBYA changes viewers' evaluations of ads they might actually see.

Method

Participants and design. The experiment used a 2 (tagline: SBYA present or absent) \times 2 (tone: attack or positive) \times 2 (content: policy or character) mixed design. Participants saw one ad of each of the eight varieties. Tagline and tone were fixed properties of each specific ad, but we varied between-subjects whether a specific ad was shown with the SBYA tagline or without it. Our eight ads comprised one Democratic and one Republican ad of each of the four tone \times content combinations. In this way, across participants which of the two ads defined by a specific tone \times content combination (e.g., policy attack) had SBYA was not confounded with the ad's political orientation. In the Web Appendix, we report results that come from including ad orientation as a fourth factor in our model. Combined with similar analyses from Studies 2–4 that test the influence of ad and/or participant political orientation, we find that SBYA's effects do not consistently depend on these variables.

Participants were undergraduate students ($N = 404$) at a university in the western United States who received either marketing course credit or \$15 for their participation in this and other studies in an hour-long session. Although we discuss all conditions and any exclusions in the main text, we include exploratory measures and analyses, additional descriptive statistics for our primary measures, and complete model results from our primary and alternatively specified multilevel models for all studies in the Web Appendix.

Procedure. Participants were told they would be watching eight political advertisements from different political races. The advertisements—four from Democrats and four from Republicans—were for candidates in 2006, 2008, and 2010 U.S. Senate races. There was a Democratic and a Republican ad of each type: policy attack, character attack, policy positive, or character positive (for a summary of ads and their content, see the Web Appendix). We varied between-participants whether the SBYA tagline was left in or digitally edited out for each ad. Table 2 shows the nonoverlapping slate of ads that two example participants (A and B) would have seen. To ensure that the presence or absence of the

Table 2
STUDY 1: SLATE OF ADS TO WHICH TWO EXAMPLE PARTICIPANTS (A AND B) WOULD HAVE BEEN EXPOSED

<i>Ad Type and Candidate</i>	<i>SBYA</i>	<i>No SBYA</i>
<i>Policy Attack</i>		
Joe Sestak (D-PA)	A	B
George Allen (R-VA)	B	A
<i>Character Attack</i>		
Bruce Lunsford (D-KY)	B	A
Mark DeWine (R-OH)	A	B
<i>Policy Positive</i>		
Mike Bennet (D-CO)	B	A
Norm Coleman (D-MN)	A	B
<i>Character Positive</i>		
Chuck Schumer (D-NY)	A	B
Mike McGavick (R-FL)	B	A

Notes: Each participant saw each of the eight candidates' ads in a random order. Within each ad type, each participant saw one ad with SBYA and one ad without SBYA. We varied between-participants which of the two ads within each ad type had SBYA. As depicted in the table, each participant saw an equal number of ads with and without SBYA in support of Democratic and Republican candidates.

SBYA tagline did not influence how ads were subsequently processed, we included ads for which the tagline was delivered at the end only. That said, in previous research the tagline's position has not been observed to matter (Brooks and Murov 2012).

For each participant, the eight ads appeared in a random order. After each ad, we probed participants' ad evaluations with five questions presented in a random order: "How credible is the ad?," "How believable is the ad?," "What is your overall evaluation of the ad?," "How trustworthy or untrustworthy is the ad?," and "How biased or unbiased is the ad?" Participants responded on seven-point scales anchored at 1 ("not at all credible," "not at all believable," "very negative," "very trustworthy," and "not at all biased," respectively) and 7 ("very credible," "very believable," "very positive," "not at all trustworthy," and "very biased," respectively). The midpoint (4) was labeled as "neutral" for each item. After reverse-scoring the final two responses, we summed the five measures to create an advertisement evaluation composite ($\alpha = .78$).

For each ad, participants also completed a second five-item composite that assessed their evaluation of the sponsoring candidate. Participants responded to each item on seven-point scales (1 = "very negative," and 7 = "very positive"; 1 = "not at all honest," and 7 = "very honest"; 1 = "not at all knowledgeable," and 7 = "very knowledgeable"; 1 = "not at all sincere," and 7 = "very sincere"; and 1 = "not very qualified," and 7 = "very qualified"). The midpoint (4) was always labeled "neutral." This candidate evaluation scale also had good internal reliability ($\alpha = .77$).

Results and Discussion

Advertisement evaluation. To assess whether the SBYA provision enhanced the perceived legitimacy of (some) ads, we constructed a random-slope, random-intercept linear model in this and all studies (for details as well as random-intercept-only models, see the Web Appendix). We defined three level 1 variables, all nested within participant: tagline (+1 = SBYA present, -1 = SBYA absent), tone (+1 = attack, -1 = positive),

and content (+1 = policy, -1 = character). This permitted the effect of the predictors to vary for each participant (random slope) but also accounted for differences between participants in how much they tended to rate the ads as more or less legitimate (random intercept). We included all interaction terms that could be created from these variables. We included a random effect of the categorical variable advertisement (coded 1–8 for each specific message). Although it is not straightforward to represent effect sizes from multilevel models using the familiar metrics, we describe in the Web Appendix how we translate these effects into the more familiar Cohen's *d* and explore this issue further in the "General Discussion" section.

There was no overall main effect of tagline ($B = .10$, $t(401.61) = 1.52$, $p > .12$), but the predicted tagline \times tone \times content interaction was significant ($B = .16$, $t(1,607.98) = 2.64$, $p = .01$), indicating that the SBYA tagline enhanced the perceived legitimacy of ads more for certain tone–content combinations than for others. As we predicted, SBYA enhanced the perceived legitimacy of policy attack ads ($B = .30$, $t(1,937.51) = 2.45$, $p = .01$) but did not alter the perceived legitimacy of ads of the other three varieties ($ts < 1.75$, $ps > .08$; see Table 3).

Candidate evaluation. Next, we assessed a similar model, one that predicted participants' evaluation of the sponsoring candidate. In this case, there was a marginally significant main effect of tagline ($B = .10$, $t(387.14) = 1.86$, $p = .06$). Of greater importance, the predicted tagline \times tone \times content interaction was again significant ($B = .15$, $t(1,772.80) = 2.96$, $p = .003$). Paralleling the results of the previous model, SBYA led to more positive evaluations of candidates who ran policy attack ads ($B = .21$, $t(2,065.85) = 2.00$, $p = .05$). Although SBYA did not influence candidate evaluations for positive policy ads or character attack ads ($ts < 1$), SBYA did enhance the evaluation of candidates who sponsored positive character ads ($t(2,078.39) = 2.82$, $p = .005$) (see Table 3). This latter effect was unexpected. Given that we did not observe this effect on the advertising evaluation composite and do not replicate it in Study 2, we hesitate to speculate on its occurrence here.

To connect the two aforementioned findings, we tested whether ad evaluations were connected to candidate evaluations. That is, if ad believability did not relate to candidate evaluations, then our two findings should not be discussed as connected consequences. Unsurprisingly, when we included ad evaluation in the model predicting candidate evaluations, ad evaluations predicted candidate evaluations ($B = .68$, $t(3,027.30) = 40.72$, $p < .001$) and the tagline \times tone \times content interaction became nonsignificant ($B = .06$, $t(2,238.91) = 1.14$, $p > .25$). This is consistent with the idea that SBYA's boost to ads' believability carries forward to fully mediate SBYA's effects on candidate evaluations. Although mediational

analyses are correlational and thus do not provide definitive support for a single causal direction, the important takeaway for our purposes is that these common consequences of SBYA on policy attack ads are connected (as indicated by the correlated common effects). If one wanted to directly test whether the believability of a message in support of a candidate makes that message more persuasive in changing evaluations of a candidate, one would instead want to find ways to directly vary the credibility of the message and measure the consequences for candidate evaluation (Shrout and Bolger 2002).

As we discuss more fully in the Web Appendix, participants—despite having just shown the effects reported previously—showed little insight into whether and when SBYA was likely to affect responses to political ads. We provide more careful tests of participants' actual awareness of being influenced by SBYA in Study 3.

STUDY 2: TESTING THE MESSAGE QUESTION WITH TIGHTLY CONTROLLED ADS

Study 2 again tested the question of whether SBYA would enhance the effectiveness of policy-focused attack ads in particular. But instead of using actual television ads, which differ on features beyond their tone and content focus, we wrote ad transcripts that either did or did not end with the SBYA tagline. In this way, Study 2 complements Study 1 by sacrificing the use of actual ads to offer a more internally valid test of the message question.

Participants read two ad transcripts, one with SBYA and one without SBYA. We manipulated whether the ad with SBYA was presented first or second. The within-subject nature of Study 1 permitted us to account for participant-level variance (some people believe all ads are relatively believable or relatively unbelievable), but might it have artificially sensitized participants to the cue? Although within-subject designs permit appropriately powered tests of higher-level interactions that between-subjects designs often lack the power to test, they can artificially call attention to cues that are manipulated within subject (Sawyer 1975). If such artificial sensitization explains our effects, then our critical tagline \times tone \times content interaction should emerge only on the second ad (once such within-subject sensitization can occur).

Method

Participants and design. The experiment also used a 2 (tagline: SBYA present or absent) \times 2 (tone: attack or positive) \times 2 (content: policy or character) design. However, in this case, participants were not exposed to eight ads (meaning they did not each see an ad of each possible combination); instead, they saw only two—one with SBYA and one without.

Table 3
STUDY 1: EFFECT OF SBYA ON THE ADVERTISEMENT AND CANDIDATE EVALUATION BY ADVERTISING TYPE

	Policy Ads		Character Ads	
	Negative	Positive	Negative	Positive
Perceived legitimacy of advertisement	.608 (.248)*	-.121 (.248)	-.025 (.050)	.431 (.248)
Candidate evaluation	.411 (.205)*	-.033 (.041)	-.005 (.041)	.576 (.204)**

* $p < .05$.

** $p < .01$.

Notes: Each value reflects the predicted boost that advertisements of each category receive by including SBYA. The values in parentheses are the corresponding standard errors. Each value was tested against 0. Significant positive results reflect a positive effect of SBYA.

Table 4
STUDY 2: SLATE OF TWO ADS TO WHICH TWO EXAMPLE PARTICIPANTS (A AND B) WOULD HAVE BEEN EXPOSED

Candidate and Ad Type	SBYA	No SBYA
<i>Robert Allen</i>		
Policy attack	A (one of the four picked at random)	B (one of the four picked at random)
Character attack		
Policy positive		
Character positive		
<i>Ronald Wright</i>		
Policy attack	B (one of the four picked at random)	A (one of the four picked at random)
Character attack		
Policy positive		
Character positive		

Notes: The order with which the Allen and Wright ad transcripts were shown was randomized and recorded.

Which of the four tone × content versions of each ad was shown was determined randomly for each participant. The two ads appeared in a random order, and for each participant we randomly selected one (and only one) of the ads to possess the SBYA tagline (see Table 4). Participants (N = 338) were recruited simultaneously from an undergraduate subject pool at a university in the western United States and from MTurk.

Procedure. Participants read transcripts of two political ads. One ad was about a fictitious candidate named Robert Allen; the other ad, Ronald Wright. Each ad always retained the same basic skeleton of a transcript, but we modified passages within each ad (by drawing on exact lines and themes from recent political ads) to create four versions: a policy attack ad, a character attack ad, a policy positive ad, and a character positive ad. For each participant, we randomly selected one of the two ads to which to add the SBYA tagline: “My name is Robert Allen [Ronald Wright], and I approve this message.” After each ad, participants completed the same five-item ad evaluation composite used in Study 1.

The Robert Allen ad related to the importance of providing assistance to small businesses to encourage economic recovery in America. The policy attack ad slammed the opponent for his lenient tax policy concerning corporate America. The Ronald Wright ads related to the financial concerns of ordinary Americans. The policy attack ad criticized a fictitious candidate named Thomas Taylor for endorsing tax, energy, and college tuition policies that would cost ordinary families thousands of dollars. The full transcripts of this and all Ronald Wright ads are in the Web Appendix.

Results and Discussion

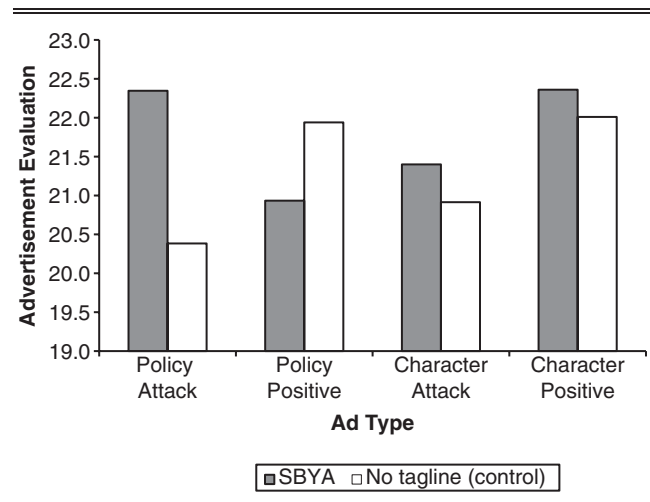
To test whether the SBYA tagline enhances the perceived legitimacy of policy-focused attack ads in particular, we followed a very similar analytic approach to that used in Study 1. We used a multilevel model in which tagline (+1 = SBYA present, -1 = SBYA absent) was nested within-participant in a random slope, random intercept model. We included effects of tone and content as well as the three two-way interaction terms and one three-way interaction term. We also included a random effect of ad, which accounted for the fact that participants saw two distinct ads (Allen or Wright).

Consistent with our main hypothesis, and conceptually replicating Study 1, we again observed a tagline × tone × content interaction (B = .39, t(608.49) = 2.74, p = .01; see Figure 1). To better understand the nature of the interaction, we tested for the influence of tagline (SBYA) for each of the four types of

ads. Again, SBYA enhanced the perceived legitimacy of policy attack ads (B = .99, t(565.29) = 3.60, p < .001). In contrast, SBYA marginally reduced the effectiveness of the positive policy ad (B = -.55, t(276.98) = -1.95, p = .05). We found that SBYA had less of an effect in either direction on character ads: the mandatory endorsement did not enhance the effectiveness of character attack ads (B = .22; t < 1) or character positive ads (B = .23; t < 1).

The within-subject nature of our studies enables us to attain much-needed statistical power to observe our predicted three-way interaction. But did the design hypersensitize participants to the presence (or absence) of the SBYA tagline? If so, this might exaggerate how important the SBYA tagline would be in real-world evaluation of the ads. According to this alternative hypothesis, our focal three-way interaction should be driven by whichever ad happened to appear second. However, we did not find that our focal tagline × tone × content interaction negatively interacted with the ad’s position (+1 = first, -1 = second; B = .06, t < 1). Indeed, the focal tagline × tone × content interaction replicated on the first ad (B = .44, t(330.23) = 2.21, p = .03). This offers a between-subjects confirmation of our answer to the message question.

Figure 1
STUDY 2: ADVERTISEMENT EVALUATION BY TAGLINE FOR ADS WITH DIFFERENT TONE AND CONTENT FOCUS



STUDY 3: REASONED (VS. MINDLESS) ACCOUNTS OF THE TAGLINE QUESTION

Studies 1 and 2 addressed the message question, and we consistently observed that SBYA helps address people's overcomeable skepticism toward policy attack ads. Having identified the scope of SBYA's influence, we proceed to test why the mandatory endorsement has this effect. Study 3 asks whether SBYA's influence can be explained by message recipients' explicit beliefs or lay theories about SBYA's significance: the positive signal hypothesis (H_{2a}), the coercion confusion hypothesis (H_{2b}), or the regulation confusion hypothesis (H_{2c}). Anticipating that SBYA's effectiveness may be more automatic or mindless than explicitly reasoned, we also assessed whether participants were aware of being influenced by SBYA. We test these hypotheses in the context of four new actual, policy-focused attack ads.

By the coercion confusion hypothesis, people incorrectly interpret candidates' endorsement as a freely chosen assurance of truthfulness. We informed all participants that the SBYA taglines were required by law. If the tagline's effectiveness is indeed rooted in such confusion, then making explicit to participants that the endorsements are mandatory should prevent an effect of tagline from emerging. By the regulation confusion hypothesis, it is not that people do not realize that SBYA is coerced by regulation, but that they misunderstand that regulators verify the content of ads that are subject to their oversight. To test this possibility, we made explicit to half of participants (and repeatedly reminded them) that the Federal Election Commission (FEC) had not evaluated the content of the ad. If such confusion underlies the effects, offering this information should significantly diminish the tagline's effect.

According to the positive signal hypothesis, viewers are not confused about the role that regulators play in requiring taglines but not vetting ads, but they do have an explicit belief (that may or may not be true) that candidates choose to run truer content when they know that they will be required to endorse the ad. Note this account is agnostic about why people have drawn this inference but instead proposes that SBYA's influence can be traced to such an explicit theory instead of a merely mindless response to some aspect(s) of SBYA. If the positive signal hypothesis completely accounts for SBYA's positive effects, then (1) participants should explicitly endorse the idea that SBYA is a positive signal of ads' truthfulness and (2) SBYA should not be predicted to enhance ad believability for participants who explicitly see no signal value in the tagline. We recruited a large sample ($N = 639$) so we would have the ability to determine whether *any* of the SBYA effect can be explained by explicit reasoning. This helps us avoid a fallacy of dichotomous thinking (that an effect is entirely reasoned or entirely mindless) that might come from a design in which we would not have the power to detect both contributing components.

Method

Participants and design. The experiment used a 2 (tagline: SBYA present or absent) \times 2 (unverified: unverified or control) mixed design, with only the first factor manipulated within subject. Although participants were exposed to two ads with SBYA and two ads without it (a within-subject manipulation), we did vary between-subjects which two ads had the SBYA tagline appended to them. There was a third factor—an ad's

political orientation—that ensured that all participants saw one Democratic and one Republican ad within each level of the tagline factor (for details, see the Web Appendix). In an effort to achieve a large sample ($N = 639$), participants were recruited simultaneously from subject pools at universities in the western and northeastern United States as well as from MTurk.

Procedure. Participants watched and evaluated four ads selected from a list of all policy attack ads (as found in archival data from the University of Wisconsin Advertising Project) run by candidates for the U.S. House of Representatives during the 2008 election season. We intentionally sampled two Republican and two Democratic ads. Furthermore, we sampled only from ads whose SBYA tagline was at the end of the ad.

The ads supported four candidates: Louisiana Republican Bill Cassidy, Nebraska Democrat Jim Esch, California Democrat Nick Leibham, and Louisiana Republican Steve Scalise. As before, we created two versions of each ad. One included the required SBYA tagline (SBYA). For the other version, we digitally edited out the tagline (tagline absent). All participants saw two ads with the SBYA tagline and two without it, but we varied between participants which ads did or did not include SBYA.

We also manipulated between participants the information provided about why SBYA appeared in some ads. All participants were told, truthfully, that the Bipartisan Campaign Reform Act of 2002 requires that candidates include the SBYA tagline in some, but not all, ads. The FEC was said to enforce this regulation. However, those in the unverified condition were also told, "Note that the FEC did not evaluate or verify the claims made in the ad." In this way, all participants were made aware that the SBYA tagline is not a statement made entirely voluntarily by the candidate, but is one that is required by a regulatory body. Only participants in the unverified condition received the further clarification that the regulatory body did not vet the content of the message. To ensure that participants did not miss these messages, we reinforced these points—in the relevant conditions—following each ad that included SBYA. After watching each ad, participants completed the same ad evaluation measures used in Studies 1 and 2.

We conducted a pretest in which we asked participants ($N = 430$) to watch the four policy attack ads and list factors that they thought indicated that an ad was actually true (or actually false) or that led them to evaluate ads more or less positively. From this pretest, we identified 16 unique factors. For the present purposes, crucial among them was the inclusion of the SBYA tagline ("My name is _____, and I approve this message."). The other 15 factors are provided in the Web Appendix.

After participants rated all four ads, we twice showed participants these 16 factors generated from the pretest. First, and as part of our test of the positive signal hypothesis, we probed participants' explicit theories about whether each feature signaled that "an ad is true and thus believable" or that "an ad is not true and thus not worthy of being believed." Responses were made on a 1 ("definitely signals ad is false") to 7 ("definitely signals ad is true") scale with the neutral midpoint of 4 explicitly labeled "no signal either way." We call these "explicit theories" because they represent participants' beliefs arrived at upon explicit reflection, which may or may not detail the psychological route by which the SBYA tagline boosts ad evaluations. Second, we tried to understand participants' awareness of how each of the 16 factors had just

influenced their evaluations of the ads as more positive (believable, credible, etc.) or as more negative (not believable, not credible, etc.). Participants rated each feature on a scale from 1 (“led me to evaluate more negatively”) to 7 (“led me to evaluate more positively”). The neutral midpoint of 4 was explicitly labeled “had no effect on my evaluation of the ad.” The full wordings of both prompts are in the Web Appendix.

Results and Discussion

We first examined whether SBYA increased ad evaluations (our focal DV across studies) despite the explicit information that the inclusion of the tagline was mandated by regulation (instead of voluntarily included by the candidate), and even though (for some) it was made explicitly clear that the content of the ads had not been vetted by regulators. We used a similar model to those used in our previous studies. We defined one level 1 variable, tagline (+1 = SBYA present, -1 = SBYA absent), that was nested within participant in a random slope, random intercept model. We included unverified (+1 = unverified, -1 = control) as a level 2 variable because it varied only between-participants. We also included the tagline \times unverified interaction term. Finally, we included a random effect of ad (a categorical variable distinguishing which of the four ads was being judged).

Coercion confusion hypothesis. Even though participants were explicitly informed that the tagline was mandated by regulation, we observed a positive main effect of tagline ($B = .18$, $t(2,040.83) = 3.68$, $p < .001$). A follow-up study reported in the Web Appendix used one of the four ads from the present study and replicated our basic SBYA effect (1) without calling any special attention to the tagline (as we did in the present study to address coercion confusion) and (2) without varying the tagline within subject (suggesting, as Study 2 did, that SBYA does not offer a boost only because between-ad variation sensitized participants to the tagline’s presence or absence).

Regulation confusion hypothesis. A nonsignificant tagline \times unverified interaction ($B = .06$, $t(2,041.48) = -1.13$, $p > .25$) meant that we found no evidence that confusion about regulators’ role helped explain SBYA’s effects. Even when we restrict our analyses to participants who were told that regulators did not vet ads, SBYA significantly enhanced ad evaluations ($B = .24$, $t(324.66) = 3.21$, $p = .001$).

Positive signal hypothesis. We proceeded to test whether participants explicitly believed that the SBYA tagline was a positive signal of ad truth value. Our explicit theories measure showed that on explicit reflection, participants, on average, had a slight belief that the SBYA tagline signaled that an ad was truer ($M = 4.29$, $SD = 1.32$, $t(637) = 5.59$, $p < .001$, $d = .22$; tested against 4.0, “no signal either way”). Did this explicit belief merely reflect a post hoc theory formulated upon explicit reflection, or did it explain (in full or in part) SBYA’s evaluative boost we observed previously? To understand whether the influence of SBYA depended on participants having an explicit belief that SBYA was a positive signal of an ad’s truthfulness, we added a level 2 variable, positive signal (transformed to reflect the explicit theory’s number of standard deviations from the neutral midpoint of 4), as well as its interactions with the other terms in the model. We centered the explicit theory measure at 4 because that makes the main effect of SBYA the effect for someone who views SBYA as having no signal value.

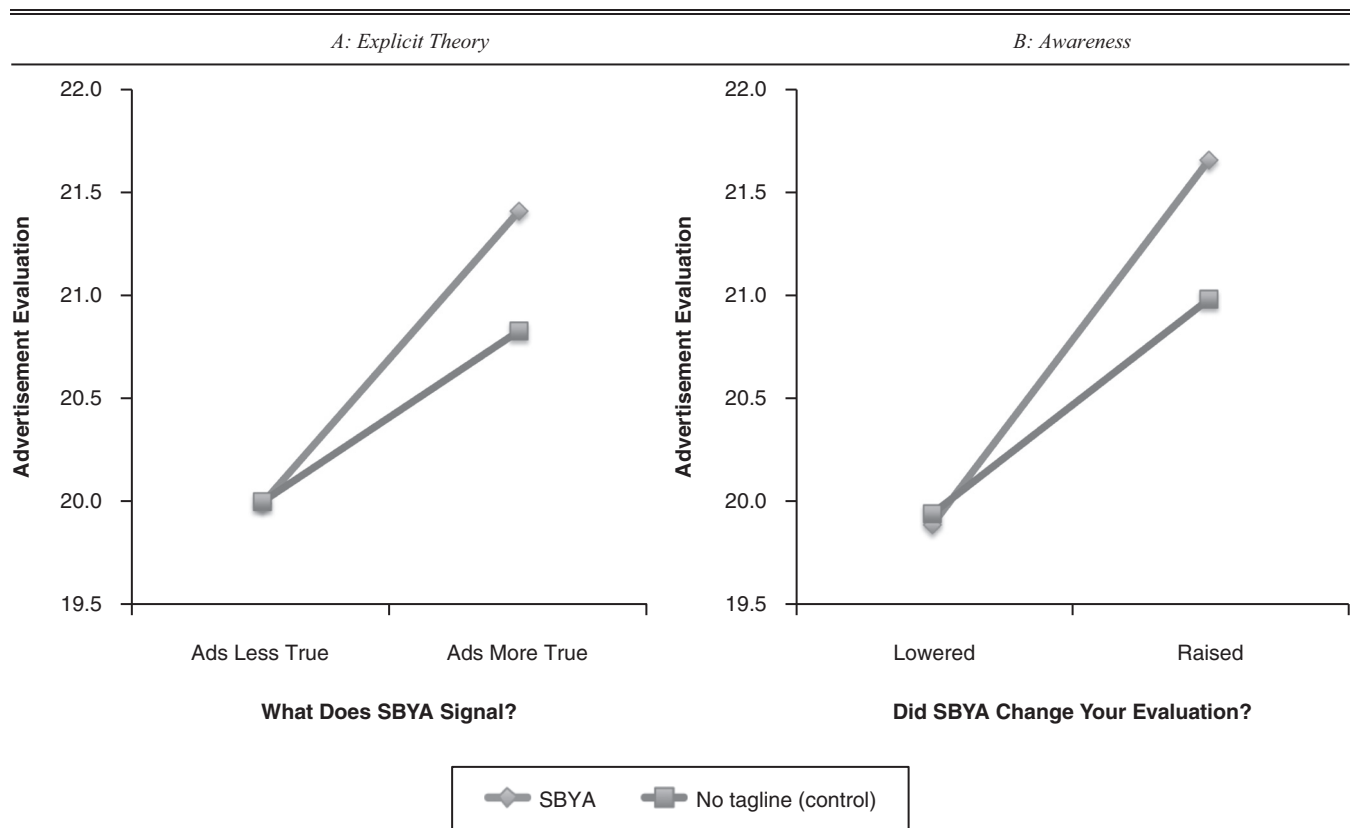
Tellingly, the main effect of SBYA was preserved ($B = .14$, $t(1,928.30) = 2.77$, $p = .01$). This shows that even among those who explicitly indicated that SBYA did not signal the ad’s truthfulness, they were still significantly influenced by SBYA. (This is reflected in Figure 2, Panel A, by the effect of SBYA at the neutral midpoint, identified by the tick mark on the x-axis.) We compare this SBYA beta to that observed in the original model and find that even when we accounted for participants’ explicit beliefs that SBYA was a positive signal of ad truthfulness, 80% of the originally observed SBYA effect remained. Additional analyses reported in the Web Appendix explore how measurement error in the explicit theories measure may have distorted this estimate; they suggest minimal distortion. The explicit theory \times SBYA interaction was also significant ($B = .15$, $t(1,929.38) = 2.91$, $p = .004$; see Figure 2, Panel A). In combination, this suggests that a small but statistically significant part of the SBYA effect is explained by participants’ explicit beliefs that SBYA is a positive signal of ad truthfulness. However, to provide a more stringent test of whether SBYA operates largely (even if not entirely) mindlessly, we examined whether SBYA enhanced ad believability outside of participants’ awareness.

Awareness. When considering their own evaluations in retrospect, participants reported a slight awareness that they had been more positive in their evaluations of ads with SBYA ($M = 4.12$, $SD = 1.39$, $t(638) = 2.10$, $p = .04$, $d = .09$). To understand whether participants were fully aware that SBYA had influenced their ad evaluations, we conducted a model similar to the model used to test the positive signal hypothesis but substituted terms related to awareness (standardized and centered at the neutral midpoint) in place of the explicit theory measure. Once again, a main effect of SBYA remained ($B = .16$, $t(1,919.15) = 3.08$, $p = .002$). This reflects that even when participants believed that SBYA had had no influence on the ad evaluations they just made, SBYA actually exerted a positive effect. We followed a similar procedure of comparing betas to find that even when we accounted for participant awareness, 86% of the originally observed SBYA effect remained. The awareness \times SBYA interaction was significant as well ($B = .18$, $t(1,919.07) = 3.63$, $p < .001$; see Figure 2, Panel B). This means that participants had some (very limited) awareness that SBYA boosted their evaluations. It is because of our large sample size that we were able to avoid an overly simplified dichotomous conclusion (i.e., that SBYA operates in a purely reasoned or purely mindless way). Instead, we find that SBYA operates mostly mindlessly, with only a fraction of the effects accessible to conscious awareness and predicted by explicit reasoning.

STUDY 4: DISSECTING SBYA TO ANSWER THE TAGLINE QUESTION

Study 3 ruled out confusion-based explanations, finding instead that SBYA’s influence operates largely outside of awareness, mostly independent of explicit beliefs of SBYA’s signal value. Study 4 aimed to test which features of SBYA encourage this confidence. We varied features of the tagline (by employing voice actors to record new taglines) as well as information about the taglines’ significance (to vary whether the taglines were supposedly regulation-compliant). This permitted us to distinguish between four hypotheses concerning why mandatory endorsements enhance (some) ads’ believability. When our ads included a tagline, they took

Figure 2
STUDY 3: ADVERTISEMENT EVALUATION BY TAGLINE



Notes: Advertisement evaluation is predicted for those participants who are one standard deviation above and below the neutral midpoint of the explicit theory of SBYA's signal (Panel A) or awareness of SBYA's influence (Panel B). The significant effects of SBYA at this midpoint (the tick marks on the horizontal axes) show that SBYA is predicted to enhance advertisement evaluations even when ads are assumed to carry no signal of truthfulness and even when participants believe SBYA had no influence on them.

one of two forms: SBYA ("My name is _____, and I approve this message") or a placebo ("My name is _____, and I am running for _____"). We used six distinct voice actors (one for each of the six policy attack ads) but modified the content of the tagline so that they seemed to be spoken by the candidate himself or by a third-person narrator (e.g., "This ad was sponsored by _____, and he approves this message"). Finally, some participants were told about the Bipartisan Campaign Reform Act and that candidates were required to include a tagline in certain ads. We varied whether participants were told that only the SBYA tagline was regulation-compliant or that both the SBYA and placebo taglines were regulation-compliant.

To find support for the civil conclusion hypothesis, we should see a main effect of tagline, indicating that both the SBYA and the placebo tagline elevate ad evaluation compared with no tagline. To find support for the regulation legitimization hypothesis, we should see a tagline \times regulation interaction indicating that (1) within each regulation condition, supposedly regulation-compliant taglines prompt higher ad evaluations than non-regulation-compliant ads and ads without taglines, and (2) the placebo tagline prompts higher ad evaluations when it is said to be regulated. To find support for either the candidate speaks hypothesis or the implicit promise hypothesis, we should see a tagline \times speaker interaction. We distinguish the two by

examining the role of tagline and speaker when taglines are actually used. The candidate speaks hypothesis predicts a main effect of speaker showing that ads are evaluated more positively when candidates (rather than third-party narrators) speak. The implicit promise hypothesis predicts that SBYA's superiority to the placebo tagline should emerge when the candidate delivers it (thereby permitting SBYA to reflect a personally delivered promise), but not when a narrator does.

Method

Participants and design. The experiment used a 3 (tagline: SBYA present, placebo, or SBYA absent) \times 2 (speaker: candidate or narrator) \times 3 (regulation: SBYA only, SBYA + placebo, or no information) mixed design, with only the first factor manipulated within subject. To make certain that tagline was not confounded with an ad's political orientation, even at the level of the participant, participants saw one Democratic and one Republican ad at each tagline level. As with our previous studies, we discuss results including this additional factor only in the Web Appendix. We counter-balanced across participants which pair of ads received which tagline manipulation. Participants (N = 565) were recruited simultaneously from an undergraduate subject pool at a university in the western United States and from MTurk.

Procedure. Participants learned they would be listening to eight different audio-only political advertisements. This set comprised six policy-focused negative ads and two policy-focused positive (filler) ads. These advertisements were sampled from the 2008 Wisconsin Advertising Project database and had all run during Congressional races in 2008. The ads supported Maryland Republican Bob Ehrlich, Wisconsin Democrat Steve Kagen, Georgia Democrat Jim Martin, Kentucky Republican Mitch McConnell, Texas Democrat Ciro Rodriguez, Mississippi Republican Roger Wicker, Iowa Republican Tom Latham (positive ad), and Indiana Democrat Jim Schellinger (positive ad). For all ads, we edited out the actual SBYA tagline. For the six policy attack ads, we had a voice actor (one actor for each ad; six actors in total) record four different versions of the final tagline, as described next. For the two filler ads, there was no tagline. This meant that for each participant, exactly half of the eight total ads had a tagline. We do not analyze reactions to the filler ads or discuss them further.

Before exposing participants to the radio ads, we offered some participants information about how government regulations supposedly applied to the ads. For those in the SBYA-only regulation condition, they were told the following:

The “Stand By Your Ad” (SBYA) provision of the Bipartisan Campaign Reform Act was enacted in 2002. In ads covered by this regulation, someone—either the candidate or a narrator—must say the candidate’s name and that they “approve this message.” If the ad is not subject to such regulation, no such tagline is required or included.

Participants in the SBYA or placebo regulation conditions received a similar message, but they were told that the law required one of two different taglines. We said that which tagline the regulation required “depended on a number of complicated factors” so that participants would not think that that choice was made freely by the candidate:

In ads covered by this regulation, someone—either the candidate or a narrator—must say the candidate’s name and (depending on a number of complicated regulatory factors) either that they “approve this message” or the name of the office they are seeking.

Those in the no-information regulation condition received no such information before watching the ads. Participants were reminded of the regulatory requirement just after watching an ad (but before completing any dependent measures) with a regulation-mandated tagline when this was relevant for the participants’ regulation condition and the specific version of the ad they saw.

All participants listened to the eight ads in a random order. For the six key policy-focused attack ads, two ads had the SBYA tagline (“approve this message”), two ads had our placebo tagline (e.g., “am running for Senate”), and two messages did not have a tagline. This pairing was varied between subjects to unconfound ad and tagline. We varied between subjects whether each tagline’s voice actor was supposedly the candidate (e.g., “My name is Steve Kagen, and I approve this message”) or a narrator (“This ad is sponsored by Steve Kagen; he approves this message”). After listening to each ad, participants completed the same five-item advertisement evaluation measure used in Studies 1–3 ($\alpha = .71$).

Results and Discussion

We began by testing the influence of our three key manipulations—tagline, speaker, and regulation—on the advertisement evaluation composite. We constructed a random slope, random intercept model predicting ad evaluation. In particular, tagline was a level 1 categorical variable that was nested within participant. Speaker was a level 2 variable that differentiated participants who heard taglines delivered by the candidates themselves (+1) or by a narrator (−1). Regulation was a level 2 categorical variable that differentiated participants who were told that SBYA was the only legitimate tagline, that either SBYA or placebo were legitimate taglines, or who were offered no such information. For these three variables, we tested for main effects as well as their higher-order interactions. Finally, we included a random effect of advertisement to account for the theoretically irrelevant variance attributable to differences in perceived legitimacy among the six specific ads.

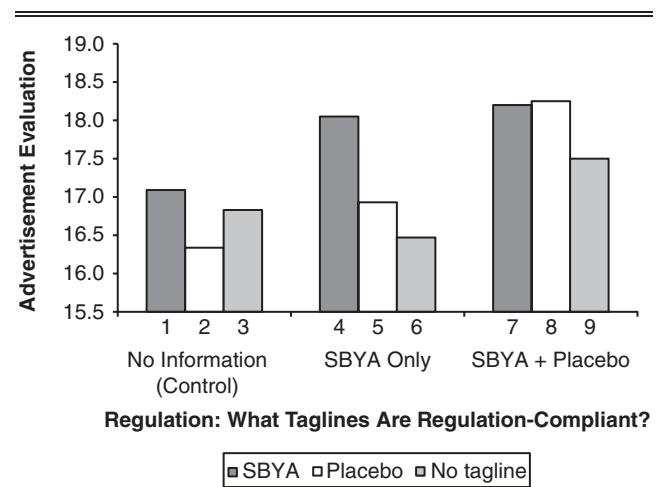
There was a main effect of tagline on the advertisement evaluation composite ($F(2, 598.72) = 18.64, p < .001$) as well as a main effect of regulation ($F(2, 721.15) = 8.39, p < .001$). But crucially, these main effects were qualified by a tagline \times regulation interaction ($F(4, 612.82) = 6.47, p < .001$; see Figure 3). The tagline \times speaker interaction achieved significance as well ($F(2, 597.78) = 3.34, p = .04$). Focused tests of our four accounts follow.

Civil conclusion hypothesis. First, we examined the main effect of SBYA. Although we replicate the finding that SBYA boosts ad evaluation compared with when there is no tagline ($B = .80, t(565.66) = 5.33, p < .001$), the placebo tagline did not offer the same boost ($B = .20, t(593.12) = 1.37, p > .17$). Because not just any civil tagline boosted ad evaluations, this contradicts the hypothesis.

Regulation legitimacy hypothesis. Second, we placed the tagline \times regulation interaction under further scrutiny to see if it predicted the pattern anticipated by the regulation legitimacy hypothesis. This account predicts a series of three significant

Figure 3

STUDY 4: PERCEIVED LEGITIMACY OF ADVERTISEMENT BY REGULATION AND TAGLINE MANIPULATIONS.



Notes: Statistical tests in the main text reference the bars by their number.

contrasts showing that within each regulation condition, the tagline(s) believed to be subject to regulation should encourage more perceptions of ad credibility than the tagline(s) not subject to regulation. Each contrast was significant. That is, under no information, SBYA (bar 1) enhanced ad evaluation compared with placebo or no tagline (bars 2 and 3, respectively) ($t(195.99) = 2.32, p = .02$). When participants were reminded (or informed) that only SBYA was subject to regulation, SBYA (bar 4) also boosted evaluations compared with placebo or no tagline (bars 5 and 6, respectively) ($t(895.97) = 5.54, p < .001$). However, when participants were told that either the SBYA or placebo tagline was regulation compliant, SBYA and placebo (bars 7 and 8, respectively) boosted ad evaluations compared with no tagline (bar 9) ($t(1,003.83) = 3.18, p = .001$). In a convergent test, we found the placebo tagline encouraged higher evaluations when it was said to reflect regulation (bar 8) versus not (bars 2 and 5) ($t(566.43) = 4.92, p < .001$). Whereas Study 3 showed that participants were not literally confused that regulators verified ads' content, we find here that the tagline's association with regulation provides legitimizing reassurance.

Candidate speaks hypothesis. Third, we closely scrutinized the tagline \times speaker interaction. The candidate speaks and implicit promise hypotheses make distinct predictions about the effects of tagline and speaker when a tagline (and, thus, a speaker) is actually present. As such, we restricted our analyses to ads with a tagline and again examined the effects of tagline and speaker. According to the candidate speaks hypothesis, we should find a main effect of speaker indicating that taglines are more effective when delivered by the candidate ($M = 17.40$) than by a narrator ($M = 17.54$). No such main effect was observed ($t < 1$).

Implicit promise hypothesis. Finally, according to the implicit promise hypothesis, we should find a tagline \times speaker interaction showing that the evaluative boost of SBYA (vs. the placebo tagline) is specific to when the candidate delivers it (thereby making it a coerced promise). Indeed, such an interaction emerged ($t(565.67) = 2.10, p = .04$). When the candidate spoke, the approval language—which, when delivered by the candidate, constitutes a promise—encouraged higher ad evaluations ($M = 17.86$) than the placebo language ($M = 16.95$; $t(567.70) = 4.37, p < .001$). Yet when the narrator spoke, participants did not differentiate the SBYA and placebo language; they both produced middling legitimacy that were not significantly different from each other ($M = 17.68$ vs. $M = 17.40$; $t(564.73) = 1.31, p > .19$).

We decomposed the interaction by testing the simple effect of tagline within each distinct speaker condition because this enabled us to examine the specific influence of the promise language while holding the effects of speaker constant. Yet the pattern of simple effects leaves open one intriguing possibility that SBYA's architects anticipated: Might the appearance of candidates at the end of their own attack ad cause some mild backlash, but one that the positive effects of regulation legitimization and implicit promise overcome? A hint of this can be observed in this pattern of means, in which the implicit promise more than counteracts the mild hit the candidate may have taken by appearing at the end of their own ad (but without making a promise). For practical purposes, the lesson remains that mandatory endorsements benefit candidates. But as we return to at the end of the next section, this pattern offers one idea for regulatory reform.

GENERAL DISCUSSION

When Senator John McCain predicted that the Bipartisan Campaign Reform Act would help to soften the tone of political advertising, he did so on the basis of a seemingly reasonable assumption. Like our participants in Study 1, McCain believed that political candidates who went negative would have to pay a political price if they had to stand behind noxious ads and make clear that they had "approved this message." By this logic, forcing a candidate to clearly stand behind a distasteful message should clarify where listeners should direct their disdain. Yet our research shows that mandatory endorsements ironically incentivize reliance on certain forms of negative advertising, making them more believable than they would have been otherwise. Thus, even if reformers primarily wanted to discourage character-based ad hominem attacks, they certainly did not want to lend artificial credibility to policy attacks.

Using an experimental approach that examined responses to ads presented in written, audio, and audiovisual form, four studies showed that adding the SBYA tagline ("My name is _____, and I approve this message") to policy-focused attack ads improved their credibility. Even before considering *why* mandatory endorsements might enhance advertisement evaluations, we reasoned that policy-focused attack ads (vs. ads of other varieties that are positive in nature or that focus on character) were especially amenable to being embraced. Although people are naturally skeptical of negative ads, they are more willing to set aside such skepticism (even without clear evidence) for policy attacks (vs. character attacks). Considered in combination, Studies 1 and 2 suggested that SBYA helped ads reach this more modest threshold. Not only did the SBYA tagline enhance the perceived believability of policy attack ads, but it also produced more positive evaluations for the ads' sponsoring candidates.

After our initial efforts addressing the message question, we turned to the tagline question of why SBYA offers this boost. After showing that participants were not merely confused about why candidates were endorsing their messages or whether the FEC had verified ads' content, Study 3 was able to assess the extent to which SBYA's effect emerged as a result of explicit (theories that SBYA signals truer ads) or mindless (and outside of awareness) processes. The study found that SBYA's effect is mostly (though not entirely) mindless. Study 4 found that two components of the mandatory endorsement explained why it offered the boost to SBYA-backed policy ads. More specifically, SBYA's legitimizing association with regulation and its specific language of endorsement ("I approve this message") are what gave the taglines the power to boost the ads.

One strength of our studies was that we tested our hypotheses across many different ads, thereby diminishing the influence of any single ad's idiosyncrasies. The consistent findings across ads and across studies give us confidence in the generality of the results. However, one disadvantage of relying on our designs is that the multilevel modeling they necessitate produces results not directly describable using a typical effect size metric such as Cohen's d . As we describe in the Web Appendix, we calculated this metric indirectly. Combining across our studies (and weighting the evidence by sample size), we find that SBYA legitimizes policy attack ads by .19 standard deviations, on average.

This effect size is remarkable for three reasons. First, regulators thought mandatory endorsements would disincentivize reliance on such ads, which should make any nonnegative effect size surprising. Second, given the large size of the electorate and the closeness by which many races are decided, campaigns invest heavily in efforts that have tiny effects. For example, the typical get-out-the-vote effort improves turnout by .19% (Green, McGrath, and Aronow 2013). In such an environment, a Cohen's d of .19 is quite important. Third, as we report more fully in the Web Appendix, Studies 3 and 4 found SBYA to have (directionally) stronger effects on ad evaluations than did the partisanship match (vs. mismatch) of participants and ads. It may seem intuitive that Democrats and Republicans believe that Democrats and Republicans, respectively, run truer ads. It is remarkable that mandatory endorsements have effects that are at least as large.

That said, one limitation of our studies is that they were tested in artificial contexts that dampen external validity. That is, participants were directed to pay attention to messages and then immediately offer their evaluations of them. In the real world, political marketing competes with other environmental clutter for attention and with other information for influence. As such, this aggregate effect size is more appropriately considered an upper bound, rather than the most likely point estimate, of SBYA's influence on actual voters' attitudes and behavior.

Future Directions for Theoretical and Applied Development

Our goal was to examine when and why mandatory endorsements influence the effects of advertising. With clearer answers to these questions, we are now able to consider these findings in light of the literatures on persuasion as well as mandatory disclosures:

Persuasion. In considering when messages are more or less persuasive, previous researchers have situated many such findings within the elaboration likelihood model (ELM; Petty and Cacioppo 1986; Wegener and Chien 2013). By this framework, attitudes change because people scrutinize the merits of arguments (high elaboration) or because they process information superficially but embrace a message because of a superficial cue (low elaboration). Having seen evidence that mandatory endorsements' effects occurred mostly independent of explicit reasoning and outside of awareness (Study 3), should readers interpret this to mean that mandatory endorsements operate through a low elaboration pathway?

We do not think such a conclusion is warranted. Note that our reason-based and mindless accounts do not map onto ELM's distinction, because we did not ask whether participants engaged in explicit reasoning about the merits of the advertisements' content (ELM's focus). Instead, we examined whether participants had explicit theories about the significance of the tagline or whether some feature of the tagline provided mindless reassurance about the credibility of the message. In this way, our focus was on responses to the mandatory endorsement itself, not on scrutiny of the message.

But does the fact that participants were responding largely mindlessly to the cue mean that we have documented a persuasive pathway that operates most strongly under low elaboration? Although this is ultimately an empirical question, this conclusion does not follow. This is because careful processing (high elaboration) need not override the influence of a noncentral cue but, instead, may be guided by it (Petty 2001).

This is especially relevant given that arguments in political ads are not easily evaluable (Brooks and Murov 2012), meaning that how participants interpret and elaborate on the message may be influenced by more than simply the message content. Given our mechanistic focus was on *why* mandatory endorsements boost advertisements rather than on what processing styles may be most encouraging of such effects, more data are needed to answer this complementary question.

That said, one piece of evidence suggests that mandatory endorsements may be just as persuasive under high as low elaboration. As we discuss in the Web Appendix, we found that participants' memory in Study 1 for whether an ad did or did not include the SBYA tagline did not moderate the strength of the SBYA effect on policy attack ads. If the accuracy of such memories is one (admittedly imperfect) proxy for how carefully participants were processing ads, it suggests that even participants who processed the messages more carefully (as reflected by their superior recall of the tagline) showed a similar boost. Experiments that directly manipulated how ads are processed would be necessary for testing whether the level of message elaboration moderates SBYA's legitimizing power.

Refining our understanding of when SBYA effects emerge. Although we found that SBYA's effects are largely specific to policy attack ads, the logic underlying our hypotheses permits more nuanced predictions that could be tested in future research. Drawing on previous theory, empirical results, and our own pilot data, we argued that policy-focused attack ads reflect an optimal mix of three features: viewer skepticism, potential informativeness, and a low(er) evidence-based threshold necessary to set such skepticism aside and embrace the useful message. If this reasoning is correct, SBYA's enhancing power may not extend to all policy attack ads, nor may it be limited solely to ads in this category. Future research might test whether positive ads that make somewhat dubious claims, or even character-focused ads that focus on well-substantiated and germane claims (thereby lowering the threshold to accept), might also benefit from SBYA.

Knowing that something is regulated. The support for our regulation legitimization hypothesis suggested that mandatory endorsements are effective when they are known to be mandatory, but this raises natural questions about how viewers know (or think they know) that an ad has been affected by regulation. In one recent advertisement, now-former Senator Mary Landrieu delivers the SBYA tagline before joking with her father about being required to include the endorsement. In this way, any viewers who are unaware that the endorsement is indeed mandatory are so informed, thereby potentially bolstering the message's effectiveness. But without such direct education or intimate familiarity with regulatory requirements, what leads message recipients to assume an ad has a legitimizing association with regulation? Study 4's results suggested that not just any tagline is assumed to be regulated—even when, like the placebo tagline, it is delivered by the message sponsor and reflects a clear break from the rest of the message. As such, some familiarity with the regulatory veneer may be necessary. Future research could examine whether nonpolitical marketers might exploit this phenomenon by including familiar markers of regulatory oversight in their messaging or packaging. For example, a street vendor's unnecessary inclusion of a standard nutrition label or a diet pill manufacturer's decision to enlarge the trademark logo on

its packaging might enhance the perceived legitimacy of their products.

Implications for the Regulation of Communication

In considering how regulations should be reformed, we posit three premises. First, we think the electorate is best served when they are exposed to information that is useful, not necessarily information that is most pleasant or positive. Second, we concur with prior research that policy-focused ads are especially useful in giving voters a clearer picture of what a candidate will do once elected. This is not to imply that candidates of bad character make good elected officials, but is instead to say that (1) some of the most relevant character information is communicated by candidates' policy positions and (2) many severe character infractions will be punished by the judicial system before voters have the chance to punish them at the ballot box. Third, in light of our strong support for the regulation legitimation hypothesis, it may be best to implement policies that do not communicate to voters an explicit association with regulation.

What form might such reforms take? Although we cannot conclusively endorse any particular policy, we close by considering three broad possibilities. First, given that one reason negative ads can be so beneficial is that they permit straightforward comparisons between candidates, it may be worthwhile for nonpartisan organizations or commissions to provide voters with such comparative information. As one example, the California Secretary of State's office sends to every voter a guide containing nonpartisan summaries of ballot issues, arguments in favor of and opposed to each such initiative, as well as candidate statements by those running for state-level office. But this guide is overwhelming in length, often totaling over 100 pages. Were a similar guide to be disseminated to voters about candidates' positions—perhaps concisely presented with *Consumer Reports*-style easy-to-digest tables that compare candidates on each issue—the electorate might enter voting booths better informed. Although this guide should itself gain credibility if perceived to be associated with regulation, it is better that impartial summaries—as opposed to ads in which one candidate is allowed to caricature the positions of his or her opponent—be the communication that gains this legitimacy.

Second, if the SBYA tagline—or some other sign that communicates a clear association with regulation—does continue, it may be informative to make explicit that no regulatory body has reviewed the content of the advertisement. For example, dietary supplement manufacturers that make functional claims are already required by U.S. regulators to clarify, “This statement has not been evaluated by the FDA.” Although Study 3 suggested that mandatory endorsements' boost was not reduced when participants learned that the FEC had not evaluated the content of the ad, we believe it is worthwhile (given that it would involve the most minimal reform) to determine whether disclosing this lack of content oversight in the ad itself—perhaps by the candidates themselves—might disrupt SBYA's enhancing effects.

Third, our results in Study 4 showed that candidate-delivered taglines may be benefiting from the implicit promise language that defines the SBYA tagline. But a comparison with our third-person narrator conditions showed that this implicit promise boost that comes from the candidate assuring recipients that he “approved the

message” may correct for a mild backlash that comes from the candidate appearing at the end of the negative ad. In this way, requiring a candidate to deliver a tagline (thereby forcing the candidate to appear in the ad), but one that has no approval language (thereby preventing the benefit of the implicit promise from coming into play) may satisfy the goal of keeping voters informed about who is running ads without ironically lending credibility to messages. Our placebo tagline may even be this practical remedy.

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