**Stories and Intergroup Relations**

Understanding the experiences of others, particularly out-group members, is important for a functioning democracy. One possible way to encourage mutual understanding is telling stories, messages that use plot, characters, and narrative suspense to make a point. An extensive literature in communications and psychology suggests that stories are processed differently than other messages, in a way that makes them particularly conducive to communicating experience and reducing barriers to persuasion. Drawing on this work, some democratic theorists, as well as empirical scholars studying prejudice reduction, suggest that storytelling might improve intergroup attitudes and behaviors. This paper evaluates the relative effects of narrative and non-narrative messages in the news media, one of the most common places for citizens to encounter stories about other groups. We do so using an experiment that manipulates whether a newspaper article opens with an anecdotal lead, a story about an individual affected by the events described in the article. Contrary to recent work, we find no evidence that articles that lead with a story are more effective than articles that employ a more traditional, non-narrative lead. These results suggest that narrative messages have little advantage over non-narrative messages in common forms of political communication.

In any diverse society, building understanding across group lines is necessary for a functioning democracy. Pluralism in group attachments is a fact in most democracies, and political science has increasingly recognized the central role these group attachments play in structuring how people understand the political world (Huddy, Mason and Aaroe 2015, Achen and Bartels 2016, Kinder and Kalmoe 2017, Mason 2018). While these group attachments can provide the basis for active involvement in politics, they can also be the basis for dividing the world into “us” and “them,” leading to stereotyping, prejudice, and even violence between groups. Given the role of negative intergroup attitudes and stereotypes in political violence around the world, and the rise in affective polarization in the United States (Iyengar, Sood, and Llekes 2012), understanding the processes that shape intergroup relations and tools that can be used to reduce intergroup hostility is essential.

One possible tool to achieve this goal is storytelling. Stories, messages that use characters, plot, and narrative suspense to make a point, can be powerful tools for communicating personal experience and individual perspectives because they are processed differently than other kinds of persuasive messages. While messages constructed as arguments are processed centrally or peripherally depending on the claim they advance and the evidence supporting that claims (Petty and Cacioppo 1986), the rhetorical structure of stories encourages people to mentally simulate the setting, events, and experiences described by the story from the perspective of the story’s characters (Green and Brock 2000, Slater and Rouner 2002, Mar and Oatley 2008). Narrative theorists describe this as a process of *narrative* *transportation* (Gerrig 1993), sometimes accompanied by *identification* with the story’s characters (Slater and Rouner 2002). A successful story carries its audience to the world of the story, which the audience experiences from the perspective of the story’s characters, as though the events happening in the story world were really happening. According to this account, storytelling might make it possible for members of different groups to share in each other’s experiences, broadening their understanding of important political issues and reducing suspicious and hostility towards other groups.

This possibility has drawn attention in political science. Feminist democratic theorists have long argued that the rhetorical form of storytelling can help members of different social groups better understand each other by communicating the experiences in a way that is intelligible across lines of difference (Young 1996, 2000; Sanders 1997). More recently, empirical research has documented some support for these claims. Most prominently, Broockman and Kalla (2020) found that canvassers and phone bankers who use engage in a “non-judgmental sharing of narratives” reduced anti-immigrant and transphobic attitudes, relative to a message that omits narratives (Study 1), and to a no-message placebo (Studies 2, 3). This builds on other work which finds that intensive forms of storytelling, including narrative fiction (Johnson et al. 2013), feature films (Czaja 2013), documentaries (Chattoo and Feldman 2017), and TV shows (Murrar and Brower 2018, study 1; Moyer-Gusé et al. 2019), can improve various forms of intergroup attitudes. Yet, studies on more commonly encountered forms of political communication, such as the news articles that we test in this paper, have more ambivalent findings about the effectiveness of narratives (Wojcieszak and Kim 2016, Oliver et al. 2012, Beauvais and Stolle 2019).

In this paper, we evaluate the effectiveness of stories as devices to improve intergroup attitudes, affect, and stereotypes using an experiment that manipulates the presence of a story at the beginning of a newspaper article. Journalists often introduce newspaper articles using brief stories about individuals affected by the events described in the article, a kind of introduction commonly referred to as an anecdotal lead (Harrower 2006; Craig 2006, Ch. 2; Wahl-Jorgenson 2013). We assign experimental subjects to read a story about a politically relevant out-group that opens with an anecdotal lead, or with a lead written in a more traditional, non-narrative, manner. We then measure affect, stereotypes, preferences for social distance, and policy attitudes relevant to the out-group.

Contrary to expectations, we find no effect of including an anecdotal lead relative to a standard, non-narrative lead. The anecdotal lead condition does not lead to substantively or statistically significant differences in prejudice, stereotyping, social distance preferences, or policy attitudes. This holds regardless of whether the subject themselves identifies with the group described in the anecdotal lead or whether their partisan identification places them on the same “side” as the group. In sum, stories, at least as they commonly appear in journalism, may not be effective means of increasing intergroup understanding. While storytelling may be a way to increase intergroup understanding, doing so might require much more intensive treatments than those involved in common forms of political communication.

**Stories**

A story is a selective recounting of temporally-ordered events, which take place in a setting, or story-world, and involve the actions and interactions of characters (Mar and Oatley 2008, Shenhav 2014). In this way, stories are structured differently from other political communication, which generally takes the form of an argument, or a claim supported by evidence. Theories in psychology and communications argue that this structure is not merely of aesthetic importance: stories are psychologically processed in a fundamentally different way than other messages (Green and Brock 2000, Escalas 2007, Moyer-Gusé 2008). Psychologists describe processing a story as a process of narrative *transportation* (Gerrig 1993): the sequential events of the story transport the audience out of the physical world they inhabit and into the story’s world, where they experience the events of the story as though they are actually happening (Green and Brock 2000, Mar and Oatley 2008). When the story ends, the audience returns to the actual world, but changed by the experiences they had while in the story’s world. This process of mentally simulating the world described in the story can thus have important impacts on beliefs and behaviors.

The unique structure of stories does more than lead an audience to mentally simulate a different world. Generally, an audience will identify with[[1]](#footnote-2) one of the story’s characters and simulate the story’s world and events from the perspective of that characters. Kaufmann and Libby describe this as a process of experience-taking, an “imaginative process of spontaneously assuming the identity of a character in a narrative and simulating that character’s thoughts, emotions, behaviors, goals, and traits as if they were one’s own.” When deeply engaged with a narrative, this identification leads the audience to not just mentally simulate the story’s events as though they are actually happening, but to simulate these events as experienced by the character, simulating that character’s thoughts, feeling, and emotions as if they were the audience’s own. In this way, stories allow us to access and, through the process of imaginative mental simulation, share in thoughts and experiences that may be very different from our normal, day-to-day lives.

This imaginative simulation can change attitudes and beliefs in ways consistent with the experiences described in the story. For example, Green and Brock (2000) show that reading a short story in which the protagonists’ young sister is murdered by a psychiatric patient made subjects more likely to believe that random acts of violence were common, reduced support for allowing institutionalized psychiatric patients the ability to leave their institution, and reduced belief in a just world; moreover, all of these effects were mediated by the degree to which readers were transported into the world of the story. This basic finding – that stories can change attitudes and beliefs implicated in the story, and that this effect is mediated by the degree of narrative immersion and identification with the story’s characters – has been replicated by studies in a number of studies across several fields (see reviews de Graff et al. 2016, Kim et al 2017, Moyer-Gusé and Dale 2017).

Moreover, the fact that stories persuade by immersing their audience in the experiences of the story’s characters allows storytelling to overcome some common forms of resistance to persuasion. Counter-attitudinal messages generally cause reactance and counter-arguing, a key mechanism that allows people to maintain stable attitudes. However, the indirect route of narrative persuasion allows it to circumvent these reactions, possibly because it hides the persuasive intent behind a message (Moyer-Guse 2008). This, in turn, allows stories to produce attitude change where more direct messages would be ignored or counter-argued. This effect is dependent, however, on the degree of immersion into the narrative world; Moyer-Guse and Nabi (2010), for example, shows that the degree of identification with a story’s characters predicts the amount of counter-arguing message recipients engage in.

**Can Stories Affect Intergroup Relations?**

The existing literature suggests that stories can change attitudes and beliefs about the real world and that these changes are mediated by narrative immersion as well as identification with characters. But transportation does not happen automatically. The audience must perceive the message as a story and, moreover, as a story that is worth processing as a story; this idea is captured in the familiar literary term “willing suspension of disbelief.” Mar and Oatley argue that this suspension of disbelief depends less on whether the story seems realistic and more on whether it achieves “verisimilitude” with respect to human emotion, and how this emotion drives action by individuals and interaction among characters (2008). If the emotions, motivations, and interactions are unrecognizable to us as those of the real people the audience may not to “willingly suspend disbelief” and the story will fail to trigger the mental simulation that gives stories their persuasive power. Such a story will have little or no effect on beliefs and behaviors and might even trigger a backlash effect as the story is perceived instead as a particularly weak or manipulative argument.[[2]](#footnote-3)

This suggests that persuasion about intergroup attitudes, beliefs, and stereotypes may be a particularly hard case for storytelling. To the extent that members of out-groups inhabit different social worlds, their emotions, motivations, and interactions may seem strange or “unrealistic” to an audience. The tendency to seek social distance from disliked out-groups may exacerbate this problem – for example, we may perceive emotional reactions by out-group members as inappropriate or “unreal,” even if we would see the same reactions as appropriate and “realistic” if experienced by an in-group member. At a more prosaic level, it may simply be more difficult to mentally simulate the story’s setting when that setting is very different from the environment of our own day-to-day lives. Identifying with a character who occupies a different social position is likely to be more difficult than identifying with a character who shares a similar social position to oneself, particularly if one is asked to identify with a character from a subordinate social group. Even if an audience is willing to take on the perspective of an out-group character, changing general intergroup attitudes and stereotypes requires generalizing any empathy generated for the story’s characters to that character’s group, something that intergroup contact theory shows is not automatic (Pettigrew 1998). All of these factors may make it more difficult for stories about out-group members to transport their audience to the story’s world.

Finally, messages that depict disliked out-groups in positive ways may be perceived as intended to persuade regardless of whether they are structured as stories. This may be particularly true for message formats, such as newspaper articles or campaign ads, that are perceived as inherently political or persuasive. Disguising messages’ persuasive intent is thought to be a key way that stories encourage persuasion (Moyer-Guse 2008, Moyer-Guse and Nabi 2010, Brusse et al. 2015); particularly among those for whom the message runs counter to existing attitudes, stories that are perceived as attempts to persuade might be processed in the same way as messages that are not structured as stories. This would limit the ability of stories to change attitudes and stereotypes more than non-story messages.

Existing work finds mixed evidence for the effects of narrative messages on intergroup attitudes. Several studies find that intensive forms of storytelling, including narrative fiction (Johnson et al. 2013), feature films (Czaja 2013), documentaries (Chattoo and Feldman 2017), and TV shows (Murrar and Brower 2018, study 1; Moyer-Gusé et al. 2019, can improve various forms of inter-group attitudes, though Paluck (2009) studies a serial radio drama in Rwanda and finds no effects on personal beliefs, though positive effect on behaviors and perceived social norms. While these findings are intriguing, these results are from particularly lengthy and immersive forms of storytelling, and one where real-world exposure is subject to significant self-selection.[[3]](#footnote-4) Kalla and Broockman (2020) employ a treatment closer to common forms of political communication, finding that canvassers and phone-bankers employing narrative messages can have significant and long-lasting effects on anti-immigrant and transphobic attitudes. However, they do not test whether these narrative messages are more or less effective than similar non-narrative messages.

Studies on less intensive forms of political communication, such as the news articles that we test in this paper, have more ambivalent findings. Murrar and Brauer (2018, study 2), find a positive effect of a four-minute YouTube clip that combined video, music, and images of Muslim Americans, but Beauvais and Stolle (2019) find no difference between the effects of narrative and non-narrative messages on attitudes towards indigenous peoples. Wojcieszak and Kim (2016) compare the effects of reading narrative and non-narrative comments on news stories about out-groups, but only measure the effect on subjects’ self-perceived attitude change, not an actual change in attitudes towards the outgroup. Oliver et al. (2012) examine the effect of narrative vs. non-narrative news stories on empathy towards out-groups and find an effect mediated by compassionate emotional reactions to the story, but do not appear to find a main effect of article format on empathetic attitudes.

**Hypotheses**

Drawing on the literature above, we evaluate two sets of hypotheses about the effect of storytelling on affect, stereotyping, social distance preferences, and policy attitudes related to the group portrayed in the story, as compared to a similar political message that does not contain a story. The first set of hypotheses reflects the expectation that storytelling will be more effective than non-story messages at changing attitudes towards a group.

*H1a: Messages that contain stories will improve affect towards the group portrayed in the message more than messages that do not contain stories.*

*H2a: Messages that contain stories will reduce negative stereotypes of the group portrayed in the message more than messages that do not contain stories.*

*H3a: Messages that contain stories will reduce preferences for social distance towards the group portrayed in the message more than messages that do not contain stories.*

*H4a: Messages that contain stories will increase favorability towards policies favorable towards the group portrayed in the message more than messages that do not contain stories.*

Further, because stories may be particularly able to overcome resistance to persuasion through mechanisms like counter-arguing, we expect that stories will be especially effective at changing attitudes among out-group members, relative to messages that do not contain stories. While these hypotheses posit a causal effect among the sub-group who does not identify with the target group, we do not interpret any difference in effects between this sub-group and the sub-group that identifies with the target group as being caused by this identification (Kam and Trussler 2017).

*H1b: Among people who do not identify with the group portrayed in the message, messages that contain stories will improve affect towards the group more than messages that do not contain stories.*

*H2b: Among people who do not identify with the group portrayed in the message, messages that contain stories will reduce negative stereotypes of the group more than messages that do not contain stories.*

*H3b: Among people who do not identify with the group portrayed in the message, messages that contain stories will reduce preferences for social distance towards the group more than messages that do not contain stories.*

*H4b: Among people who do not identify with the group portrayed in the message, messages that contain stories will increase favorability towards policies favorable towards the group more than messages that do not contain stories.*

**Method**

To evaluate the effect of a story on affect, stereotyping, social distance preferences, and policy attitudes related to the outgroup, we conducted an experiment that assigned subjects to read a news article about an out-group that began with either an anecdotal lead or with a traditional, non-narrative lead. Reporters often open news articles with anecdotal leads - brief stories about individuals caught up in the events the article describes. This technique is used to humanize complicated stories, or to allow journalists to inject emotion into a story without violating journalistic norms about objectivity (Harrower 2006; Craig 2006, Ch. 2; Wahl-Jorgenson 2013). Though controversial among journalists and communications scholars (Craig 2006, Ch 2; Schudson 2009), they are common particularly in feature writing (Wahl-Jorgenson 2013). Thus, the anecdotal lead is a common way for citizens to encounter stories in political communication.

*Treatments*

We created experimental treatments in the form a newspaper story about one of two groups. The first, undocumented immigrant youth who received deportation relief under the DACA program (so-called “Dreamers”), was chosen because it is likely to be a disliked out-group for most Republican subjects. The second, coal miners who supported Trump, was chosen because it is likely to be a disliked out-group for most Democrats in the sample. This allows us to examine the effects of storytelling about out-groups among subjects with varying political allegiances. It also allows us to compare the effects of storytelling among subjects who feel more or less affectively close to the target out-group. We created two versions of each article, one that opened with an anecdotal lead and one that opened with a straight, non-narrative lead.[[4]](#footnote-5) The treatment articles, and the anecdotes that formed the core of the anecdotal leads, were composites that drew on several articles about these groups from a variety of national and local sources (see Appendix A). To maintain mundane realism the anecdotes followed the form of common anecdotal leads, with the first five-to-six paragraphs of the article devoted to the anecdotal lead, followed by the core, non-anecdotal content of the article. To maintain comparability between the “story” and “non-story” conditions we made sure that factual content and group-identity cues contained in the anecdotal lead were mirrored in the non-anecdotal-lead article.

*Measures*

We measured the dependent variables for each hypothesis in the following manner:

* Transportation and Identification: We measure narrative transportation using a modified version of the Transportation Scale – Short Form (TS-SF) scale (Appel et al. 2015) (Krippendorff’s *α* = .66 - .73), and identification using the Experience-Taking scale from Kaufman and Libby (2012) (Krippendorff’s *α* = .91).
* Affect: We measure affect using a standard feeling thermometer for six different groups. First, we measure affect for the groups immediately implicated in the articles, described as “Immigrants who were brought into the US illegally as children,” and “rural, blue-collar Trump supporters.” Next, to see if any effect on attitudes generalizes beyond the specific group discussed in the article we measured affect and stereotypes for more general groups “Trump Supporters,” and “Immigrants” as well as groups with which these groups would be contested: “Trump Opponents” and “Americans.” Since these more general group feeling thermometers were also asked pre-treatment we analyze the change in relative favorability towards the general group portrayed in the article by subtracting the feeling thermometer for “Trump Opponents” and “Americans” from the post treatment feeling thermometer for “Trump Supporters,” and “Immigrants,” and then subtracting the pre-treatment measure of these differences from the post-treatment measure.
* Stereotypes: We measure group stereotyping using a battery of stereotype questions that asked subjects to rate groups on four seven-point trait scales (Violent – Peaceful, Untrustworthy – Trustworthy, Lazy – Hard-working, and Unintelligent – Intelligent) combined in an additive index (*α* = .89-.93 for all groups). As with the feeling thermometer measures, we measure stereotypes of the groups directly implicated in the story, and the more general groups.
* Social Distance Preferences: We measure these using two items for each group, one asking how willing the respondent would be to join a non-political group that included members of the group and the second asking how comfortable the respondent would be if a close relative was marrying a member of the group. These questions were asked with respect with the more general groups (“Immigrants,” and “Trump supporters”). We create an index for each group (*α* = .78), and analyze the difference between these indexes across conditions.
* We measure policy attitudes using an additive index of two questions that tap into general attitudes towards immigrants[[5]](#footnote-6) (*α* = .87), and two questions that measure support for coal mining[[6]](#footnote-7) (*α* = .9). [[7]](#footnote-8)

*Experimental Procedure*

The experiment was conducted using 877 subjects recruited from Amazon’s Mechanical Turk platform.[[8]](#footnote-9) While not representative of the adult US population, mTurk has been found to generally replicate findings from other, high-quality adult samples (Berinsky et al. 2012, Clifford et al. 2015, Coppock 2018). To ensure data quality, we included two attention check questions as well as three questions measuring comprehension of the articles and followed the procedures described by Kennedy et al. (2018) and Prims and Motyl (2018) to remove bots and non-US respondents using VPSs. We also measured the time subjects spent on treatment article. We dropped all subject who failed at least one of the attention-check questions, all of the comprehension questions, or spent less than ten seconds on the page containing the treatment article. The result was 613 subjects in the final analysis.[[9]](#footnote-10)

At the start of the experiment, subjects completed a battery of questions measuring identification with a variety of different groups. We use the single-item measure of Postmes et al. (2013), which asks subjects to agree or disagree with the statement “I identify with [group]” on a seven-point scale, repeated for five groups: Trump Supporters, Trump Opponents, Farmers, Blue-Collar Workers, and White-Collar Workers.[[10]](#footnote-11) As a pre-treatment measure of affect, we also asked a series of feeling thermometer questions about five groups: Trump Supporters, Trump Opponents, Americans, Immigrants, and Farmers.

The study employs a 2 (Immigrant vs. Coal Miner) x 2 (Anecdotal Lead vs. Straight News Lead) factorial design.[[11]](#footnote-12) Subjects were randomly assigned to experimental conditions using a block-randomized design. We created four blocks depending on pre-treatment measures that asked participants how much they agreed with the statements “I identify with Trump Supporters” and “I identify with Immigrants” on a seven-point scale and classify participants as “identifiers” with a group if they chose “strongly agree,” “agree,” or “slightly agree” with the statement. The first block included subjects who identified with Trump supporters and did not identify with immigrants, the second those who identified with both groups, the third those who did not identify with Trump supporters but did identify with immigrants, the fourth those who did not identify with either group.

After random assignment, subjects read two articles. The first article, read by all subjects, described a recent poll about farm subsidies. The second article contained the assigned experimental treatment (see Appendix A for treatment texts). After this, participants completed the dependent variable measures. Participants were then debriefed and thanked for their participation.

*Analysis*

All analyses were performed by calculating average treatment effects between conditions and using a randomization inference framework to produce confidence intervals and *p*-values (Gerber and Green 2012, Ch 3.; Keele et al. 2012). All analyses were conducted in R version 3.6.1; randomization inference was conducted using the *ri* package version 0.9 (Aronow and Samii 2012).

*Approval and Funding*

This research was determined exempt by the Human Subjects Review board at the BLINDED FOR REVIEW. Funding was provided by the author’s university-provided research funds.

**Results**

*Effects of an Anecdotal Lead*

We first examine the effect of the experiment’s primary manipulation: the presence or absence of a story. Here we pool together the secondary identity conditions to see if there was an effect of an anecdotal lead overall. We separate out these results by the out-group in question.

**Table 1 About Here**

Table 1 shows the effect of the manipulations on the degree to which subjects were transported into the story and the degree to which they identified with the subject of the story. These may be thought of as a manipulation check: if the anecdotal lead produced no greater narrative transportation or identification with the story’s subjects, it is likely not being processed as a story. We find that the coal-miner manipulation had a statistically significant impact on transportation and identification of roughly one-third of a standard deviation. We see slightly smaller (one-fifth of a standard deviation) and only marginally significant effects for the DACA manipulation significant effects on identification. These results suggest that the narrative structure succeeded in having the expected effects in terms of processing, though the coal miner manipulation was more successful. Tables 2 and 3 show that this general pattern of results held among those who did and did not identify with the group described in the article.

**Table 2 About Here**

**Table 3 About Here**

However, the top two rows of Table 4 shows that this did not generate an effect on feeling thermometer measures of either the group directly implicated in the story or the more general group. For the specific group (Childhood Arrivals/Rural, Blue-Collar Trump Supporters), we look at the feeling thermometer administered after the treatment. For the general group, we calculate the difference between the feeling thermometer for the group described in the article (Immigrants, Trump Supporters), before and after treatment and use the change in this score, which can be interpreted as the increase or decrease in relative favorability towards the group described in the story. Across these four measures, only one approaches statistical or substantive significance, the change in relative feeling thermometer towards immigrants.

**Table 2 About Here**

The middle rows of Table 2 show no effect of the story manipulation on an index of stereotypes, first of the group immediately implicated by the story and then again by the more general group, where higher numbers are more positive. Again, there is no meaningful difference between experimental conditions. The next row shows the result for an index of two measures of preferred social distance from the described group with higher scores indicating greater preferred distance. These indices are nearly identical across experimental conditions. Finally, we test the difference in an index of support for policies favorable to the group described in the story, with higher numbers indicating less support for the policies. Again, there is essentially no difference between condition.[[12]](#footnote-13)

**Table 3 About Here**

*Effects of the Anecdotal Lead on Group Identifiers/Non-Identifiers*

Potentially, these null results obscure meaningful treatment effects within subgroups of subjects, specifically those who identify with the group who are the subject of the story and those who do not identify with this group, as posited by *H1b – H4b*. If stories are particularly potent at reducing intergroup tension, we might expect that there would be little difference between story and non-story treatments among people who identify with the group depicted in the article, but we might expect that stories would have a greater positive effect, relative to the non-story treatment, on people who do not identify with the group depicted. To evaluate this, we repeat the above analyses for samples split by whether they report identifying with immigrants (for those assigned to an immigrant condition) or by whether they report identifying with Trump Supporters (for those assigned to a coal miner condition). These results are reported in Table 5 and 6. Again, we see no statistically or substantively significant effects.[[13]](#footnote-14)

**Table 4 About Here**

**Discussion and Conclusions**

In an age of polarization, finding forms of communication that can bridge intergroup divides is ever more important. Theories of narrative persuasion, as well as work from normative democratic theory, suggests that storytelling might be one such form. These theories suggest that stories are processed in a unique manner which involves mentally simulating the world and events described by the story from the perspective of the story’s characters; when the story’s characters come from an out-group, this might provide a way for people to share in the experiences of out-group members. If stories can serve this function, the ubiquity of stories in political communication, including the common journalistic practice of opening news articles with an anecdotal lead, might allow them to play an important role in bridging intergroup difference. Unfortunately, despite these theoretical hopes and some existing empirical work, we find no evidence that stories, in their common media form of an anecdotal lead, can serve this purpose. Stories appear to have no advantage over non-narrative communication in affecting intergroup relations.

These findings are not necessarily in contradiction with other recent work that does find a positive effect of narrative on intergroup attitudes, most prominently Broockman and Kalla (2020)’s finding that canvassers and phone bankers employing narrative messages reduce anti-immigrant and transphobic attitudes. First, while we compare narrative leads to standard leads of similar length and informational content, Broockman and Kalla (2020) compare narrative messages to a no-message placebo (Studies 2 and 3), or compare a message that contain a set of arguments to a message that contains the same set of arguments and adds an exchange of personal narratives (Study 1). Thus, while Broockman and Kalla (2020) find a positive effect of narrative messages, they can offer no evidence for whether narrative messages are more effective than non-narrative messages; our results suggest that they are not. Second, Broockman and Kalla (2020) test interventions that are unusually intensive even for canvassing or phone-banking campaigns. Thus, their results are in line with existing work which shows that lengthy, intensive forms of narrative can have a positive impact on intergroup attitudes (Johnson et al. 2013, Czaja 2013, Chattoo and Feldman 2017, Murrar and Brower 2018, Moyer-Gusé et al. 2019). In this context, our results and those testing interventions of similar intensity (e.g. Beauvais and Stolle 2019) can be thought of as setting scope conditions for the effect of narratives. In practical terms, this suggests that in many common forms of political communication, from news reports to campaign ads to brief social media message, narrative messages are unlikely to have an advantage over non-narrative messages.

What might account for the failure of stories to have any advantage over non-story communication in this experiment? We suggest several possibilities. First, stories about out-group members may be particularly difficult cases for theories of narrative persuasion. These theories depend on imaginatively simulating the story’s world and identifying with the story’s characters, a hard task when the audience knows little about the out-group’s world and might find adopting the out-group character’s perspective aversive. Second, media like news articles, perhaps particularly news articles about out-groups, might be inherently perceived as having persuasive intent, particularly since even those that begin with anecdotal leads generally also include a great deal of non-narrative content. Since awareness of persuasive intent can short-circuit narrative processing, stories presented in an explicitly political format may be processed in a similar manner to non-narrative messages.[[14]](#footnote-15) If this explains our null finding, then we would expect a similar null effect of stories other obviously persuasive media such as political advertisements.

One other explanation is the salience of the issues and groups used in this study. The out-groups selected here, DACA recipients and rural, blue-collar Trump supporters, were selected because they are groups about whom stories are often told in political discourse. DACA recipients have made their personal stories a centerpiece of their political campaign (Rhodan 2017), and the trope of “stories about rural white Trump supporters” became a cliché after the 2016 election (e.g. Skerrett 2018). In this way, all subjects in the study might have been pre-treated, such that a single article telling a by-now familiar story has little effect (cf. Gaines et al. 2007). Stories might be more effective when issues are new and not clearly defined in the minds of voters. Still, this would seem to limit the range of issues on which narrative persuasion could be expected to have an effect. Future work should examine these possibilities, but future attempts to bridge intergroup differences should be mindful of the potential limitations of storytelling.

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|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| DV | DACA (n = 305) | | Coal Miner (n = 307) | |
| Transportation Scale – Short Form | Story | No Story | Story | No Story |
| 5.09  (sd = 1.10) | 4.85  (sd = 1.06) | 4.86  (sd = 1.39) | 4.38  (sd=1.45) |
| ATE = 0.21 (-.02, .45)  *p* = .077 | | ATE = .43 (.12, .74)  *p* = .008 | |
| Identification | 4.84  (sd = 1.01) | 4.63  (sd = 1.02) | 4.17  (sd = 0.93) | 3.88  (sd = 1.00) |
| ATE = 0.21 (0.01, .44)  *p* = .064 | | ATE = 0.39 (.08, .50)  *p* = .006 | |
| Top row for each DV shows mean scores on the DV in each condition. Bottom row shows estimated average treatment effect and 95% confidence interval, along with two-sided *p* value for the test. | | | | |

**Table 1: Effect of Story Manipulation on Transportation and Identification Measures**

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| DV | Identifies with Immigrants (n = 82) | | Does not Identify with Immigrants (n = 225) | |
| Transportation Scale – Short Form | Story | No Story | Story | No Story |
| 5.55  (sd = 0.93) | 5.24  (sd = 0.92) | 4.91  (sd = 1.12) | 4.72  (sd=1.08) |
| ATE = 0.27 (-.14, .67)  *p* = .179 | | ATE = .19 (-.10, .48)  *p* = .194 | |
| Identification | 5.46  (sd = 1.09) | 5.13  (sd = 1.22) | 4.62  (sd = 1.42) | 4.13  (sd = 1.44) |
| ATE = 0.25 (-.22, .72)  *p* = .299 | | ATE = 0.50 (.13, .88)  *p* = .011 | |
| Top row for each DV shows mean scores on the DV in each condition. Bottom row shows estimated average treatment effect and 95% confidence interval, along with two-sided *p* value for the test. | | | | |

**Table 2: Effect of DACA Story Manipulation on Transportation and Identification Measures by Identification with Immigrants**

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| DV | Identifies with Trump Supporters (n = 90) | | Does Not Identify with Trump Supporters (n = 215) | |
| Transportation Scale – Short Form | Story | No Story | Story | No Story |
| 5.22  (sd = 0.90) | 4.93  (sd = 1.05) | 4.67  (sd = 1.03) | 4.50  (sd=0.99) |
| ATE = 0.28 (.-.13, .70)  *p* = .178 | | ATE = .18 (-.08, .45)  *p* = .184 | |
| Identification | 4.62  (sd = 0.75) | 4.23  (sd = 0.80) | 3.98  (sd = 0.94) | 3.72  (sd = 1.04) |
| ATE = 0.40 (.07, .72)  *p* = .018 | | ATE = 0.25 (--.02, .51)  *p* = .069 | |
| Top row for each DV shows mean scores on the DV in each condition. Bottom row shows estimated average treatment effect and 95% confidence interval, along with two-sided *p* value for the test. | | | | |

**Table 3: Effect of Coal Minder Story Manipulation on Transportation and Identification Measures by Identification with Trump Supporters**

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| DV | DACA (n = 307) | | Coal Miner (n = 305) | |
| Feeling Thermometer, Childhood Arrivals/Rural, Blue-Collar Trump Supporters | Story | No Story | Story | No Story |
| 67.6  (sd = 28.2) | 64.7  (sd = 27.3) | 43.3  (sd = 33.4) | 41.9  (sd=33.8) |
| ATE = 2.1 (-3.5, 67.7)  *p* = .463 | | ATE = 1.2 (-.3.6, 6.3)  *p* = .619 | |
| Feeling Thermometer  Immigrants – Americans (change pre-post)  Trump Supporters – Trump opponents (change pre-post) | 2.9  (sd = 12.8) | 0.4  (sd = 12.6) | 2.3  (sd = 12.8) | 1.7  (sd = 12.8) |
| ATE = 2.5 (-0.3, 5.3)  *p* = .084 | | ATE = 0.6 (-2.2, 3.5)  *p* = .678 | |
| Stereotype Index, Childhood Arrivals/Rural, Blue-Collar Trump Supporters | 21.2  (sd = 4.9) | 20.5  (sd = 4.8) | 16.9  (sd = 5.8) | 16.6  (sd=6.2) |
| ATE = 0.6 (-0.4, 1.6)  *p* = .267 | | ATE = 0.3 (-0.7, 1.3)  *p* = .525 | |
| Stereotype Index, Immigrants/Trump Supporters | 20.6  (sd = 4.8) | 20.1  (sd = 5.1) | 15.8  (sd = 6.2) | 15.7  (sd = 7.0) |
| ATE = 0.3 (-0.7, 1.3)  *p* = .560 | | ATE = 0.1 (-1.0, 1.1)  *p* = .894 | |
| Social Distance Index, Immigrants/Trump Supporters | 2.0  (sd = 1.2) | 2.1  (sd = 1.2) | 3.3  (sd = 1.4) | 3.3  (sd=1.5) |
| ATE = -.1 (-0.3, 0.1)  *p* = .421 | | ATE = -0.1 (-0.3, 0.2)  *p* = .703 | |
| Policy Index, Immigration/Coal Mining | 2.7  (sd = 1.3) | 2.7  (sd = 1.2) | 3.8  (sd = 1.3) | 3.6  (sd = 1.3) |
| ATE = -0.1 (-0.4, 0.1)  *p* = .400 | | ATE = 0.2 (-0.1, 0.4)  *p* = .17 | |
| Top row for each DV shows mean scores on the DV in each condition. Bottom row shows estimated average treatment effect and 95% confidence interval, along with two-sided *p* value for the test. | | | | |

**Table 4: Effect of Story Manipulation on Group Affect, Stereotyping, and Policy Attitudes, Full Sample**

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| DV | Identifies with Immigrants (n = 82) | | Does not identify with Immigrants (n = 225) | |
| Feeling Thermometer, Childhood Arrivals | Story | No Story | Story | No Story |
| 82.4  (sd = 17.1) | 79.3  (sd = 22.6) | 61.9  (sd = 29.6) | 59.6  (sd = 26.9) |
| ATE = 1.6 (-6.4, 9.7)  *p* = .687 | | ATE = 2.3 (-4.8, 9.3)  *p* = .529 | |
| Feeling Thermometer  Immigrants – Americans (change pre-post) | 1.6  (sd = 8.4) | 0.1  (sd = 8.6) | 3.4  (sd = 14.1) | 0.5  (sd = 13.8) |
| ATE = 1.5 (-2.2, 5.3)  *p* = .437 | | ATE = 2.8 (-0.9, 6.4)  *p* = .136 | |
| Stereotype Index, Childhood Arrivals | 23.7  (sd = 3.5) | 22.7  (sd = 4.7) | 20.3  (sd = 5.1) | 19.8  (sd = 4.6) |
| ATE = 0.7 (-1.0, 2.3)  *p* = .421 | | ATE = 0.5 (-0.7, 1.7)  *p* = .393 | |
| Stereotype Index, Immigrants | 23.3  (sd = 3.3) | 23.1  (sd = 4.4) | 19.5  (sd = 4.8) | 19.1  (sd = 4.9) |
| ATE = 0.2 (-1.5, 1.8)  *p* = .838 | | ATE = .4 (-0.9, 1.6)  *p* = .580 | |
| Social Distance Index, Immigrants | 1.5  (sd = 0.7) | 1.7  (sd = 0.7) | 2.2  (sd = 1.3) | 2.3  (sd = 1.3) |
| ATE = -.2 (-0.5, 0.1)  *p* = .170 | | ATE = -0.1 (-0.4, 0.3)  *p* = .706 | |
| Policy Index, Immigration | 1.9  (sd = 0.8) | 2.0  (sd = 0.9) | 3.0  (sd = 1.4) | 2.9  (sd = 1.2) |
| ATE = -0.0 (-0.3, 0.3)  *p* = .859 | | ATE = 0.1 (-0.2, 0.4)  *p* = .669 | |
| Top row for each DV shows mean scores on the DV in each condition. Bottom row shows estimated average treatment effect and 95% confidence interval, along with two-sided *p* value for the test, and the *p* values of one-sided tests comparing the ATE to *m* and *-m*. | | | | |

**Table 5: Effect of Story Manipulation on Affect, Stereotyping, and Policy Attitudes related to Immigrants by Identification with Immigrants**

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| DV | IDs with Trump Supporters (n = 90) | | Does not ID with Trump Supporters (n = 221) | |
| Feeling Thermometer, Rural, Blue-Collar Trump Supporters | Story | No Story | Story | No Story |
| 82.5  (sd = 17.1) | 81.4  (sd = 16.6) | 26.8  (sd = 23.2) | 25.4  (sd = 24.0) |
| ATE = 1.0 (-6.1, 8.1)  *p* = .783 | | ATE = 1.4 (-5.0, 7.7)  *p* = .676 | |
| Feeling Thermometer,  Trump Supporters – Trump opponents (change pre-post) | -0.4  (sd = 13.5) | 2.7  (sd = 11.5) | 3.3  (sd = 12.4) | 1.3  (sd = 13.3) |
| ATE = -2.5 (-7.8, 2.6)  *p* = .357 | | ATE = 1.9 (-1.5, 5.3)  *p* = .282 | |
| Stereotype Index, Rural, Blue-Collar Trump Supporters | 23.3  (sd = 3.5) | 23.1  (sd = 3.2) | 14.3  (sd = 4.3) | 13.9  (sd = 5.1) |
| ATE = 0.2 (-1.2, 1.7)  *p* = .760 | | ATE = 0.4 (-0.9, 1.7)  *p* = .570 | |
| Stereotype Index, Trump Supporters | 22.8  (sd = 3.8) | 23.5  (sd = 3.4) | 12.9  (sd = 4.5) | 12.5  (sd = 5.3) |
| ATE = -0.8 (-2.3, 0.7)  *p* = .318 | | ATE = .4 (-0.9, 1.7)  *p* = .538 | |
| Social Distance Index, Trump Supporters | 1.9  (sd = 0.8) | 2.0  (sd = 1.0) | 3.8  (sd = 1.3) | 3.9  (sd = 1.3) |
| ATE = -0.0 (-0.4, 0.4)  *p* = .985 | | ATE = -0.1 (-0.4, 0.3)  *p* = .704 | |
| Policy Index, Coal Mining | 2.7  (sd = 1.0) | 2.4  (sd = 0.8) | 4.3  (sd = 1.2) | 4.1  (sd = 1.2) |
| ATE = 0.3 (-0.1, 0.7)  *p* = .104 | | ATE = 0.1 (-0.2, 0.4)  *p* = .473 | |
| Top row for each DV shows mean scores on the DV in each condition. Bottom row shows estimated average treatment effect and 95% confidence interval, along with two-sided *p* value for the test. | | | | |

**Table 6: Effect of Story Manipulation on Affect, Stereotyping, and Policy Attitudes related to Trump Supporters by Identification with Trump Supporters**

1. This is conceptually distinct from the use of the term “identification” in the social identity literature. In this case, we mean the identification that develops between a story’s audience and an individual character in the story. [↑](#footnote-ref-2)
2. As Polletta et al. (2011) notes, while stories are often perceived as being powerful rhetorical tools, we also have a variety of cultural scripts for dismissing stories. “People call something ‘just a story’ to refer to its weak claim to credibility, and ‘just her story’ to refer to its one-sided and subjective character … we admonish lying children for ‘telling stories.’” (pg.110). [↑](#footnote-ref-3)
3. Further, Paluck 2009 reports more ambivalent results from radio dramas broadcast in Africa. [↑](#footnote-ref-4)
4. Our goal is the evaluate the relative effectiveness of narrative and non-narrative messages, so we do not employ a true control. As a result, we cannot offer evidence about the effectiveness of narrative messages relative to no message. [↑](#footnote-ref-5)
5. “How much should the number of immigrants permitted to come to the U.S. be increased or decreased, or should the number be the same as now?” and “On the whole, do you think that immigration is a good thing or a bad thing for the country today?” [↑](#footnote-ref-6)
6. “How much do you favor or oppose weakening environmental regulations in order to allow more coal power plants to stay open?” and “Thinking about coal power plants in the U.S., how many of these power plants do you think should be shut down over the next decade?.” [↑](#footnote-ref-7)
7. We also measure responsibility attributions, to evaluate whether these treatments acted as episodic and thematic frames (Iyengar 1994). We find no difference in responsibility attributions across story and non-story conditions. Appendix D reports the details of this analysis. [↑](#footnote-ref-8)
8. For recruitment details and CONSORT Flow Diagram see Appendix B. [↑](#footnote-ref-9)
9. Results are substantively unchanged if we re-run these analyses dropping only those subjects who quit the study before any dependent variables were collected. See Appendix F. [↑](#footnote-ref-10)
10. While somewhat less reliable than multi-item measures of group identification (Postmes et al. 2013, Reysen et al. 2013), this single-item measure achieves acceptable levels of reliability for the purpose of distinguishing subjects who identify more with immigrants than with Trump supporters (and vice versa). Further, it has the advantage of allowing subjects to express identification with groups to which they do not formally belong (e.g. US-born children and grandchildren of immigrants can say that they identify with immigrants.) [↑](#footnote-ref-11)
11. We included an additional manipulation to test whether the effect of the anecdotal lead is moderated by the degree of existing identification that the audience feels with the out-group being portrayed. To manipulate this moderator, we by describe the protagonist in the story as having a secondary identity that either is or is not shared by the subject. We found no effect of this manipulation, and thus pool these conditions in the analysis in the main text. We report the results of this manipulation in Appendix B. [↑](#footnote-ref-12)
12. We evaluate the degree to which these null findings can be interpreted as evidence of a negligible effect in Appendix E. [↑](#footnote-ref-13)
13. We repeat these analyses splitting the sample by party ID, dropping true independents. Notably, this is a post-treatment measure, but Party ID is known to be stable over time. We find no statistically significant effects in any subset of sample and can generally rule out substantively meaningful effects. See appendix G for the full results. [↑](#footnote-ref-14)
14. A related possibility is that our pre-treatment measures of identification with and affect towards the groups described in the articles indicated such persuasive intent, and thus contributed to the “short-circuiting” of narrative processing. While we cannot definitively rule out this possibility, we see it as unlikely for three reasons. First, these batteries asked about several groups that were not the primary target of persuasion and not inherently political (“farmers,” “blue-collar workers,” “white-collar workers,” and “Americans”). Second, these batteries were separated from the treatments by an unrelated article about farm subsidies. Finally, we view it as unlikely that these batteries provided respondents with a stronger indicator of persuasive intent then the content of the news stories themselves. The question of whether to measure covariates that will be used to estimate heterogeneous treatment effects before or after treatment is hotly debated (Montgomery et al. 2018, Klar et al. 2020); in this case, we chose pre-treatment measurement, though future work might attempt to replicate these results with post-treatment measurements of identity. [↑](#footnote-ref-15)