

Populism as Dog-Whistle Politics: Anti-Elite Discourse and Sentiments toward Minorities

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Abstract

Radical right-wing candidates frequently rely on populist and ethno-nationalist claims to mobilize support for their political campaigns. While studies have examined the predictors of radical-right support and the consequences of populism for party politics, they have not considered the effects of populist discourse on voters' attitudes—and particularly, how populism interacts with ethno-nationalism to increase the salience of exclusionary beliefs. Using online survey experiments, we examine whether exposure to populist political claims increases out-group antipathy in the United States. In Study 1, we randomly assign respondents to three conditions featuring vignettes constructed from existing political speeches: a morally neutral argument, populist discourse targeting political elites, and a moral critique of immigration. We then assess respondents' sentiments toward minority groups using feeling thermometers. Our results demonstrate that exposure to populist discourse alone results in less favorable views of immigrants and racial minorities (as well as elites) and that this effect is only partly moderated by partisan identification. In Study 2, we use a similar design to evaluate the link between economic populism common in radical-left politics and economic nationalism: the populist condition features a critique of economic elites, the nationalist condition presents moral critiques of countries competing for U.S. jobs, and the outcome variables measure sentiments toward these countries. While economic nationalism does lead to greater chauvinism, we find no comparable effects of economic populist language. These results suggest that the observed spillover effects in Study 1 are not a result of pre-existing attitudinal associations but rather of the bundling of anti-elite talk with ethno-nationalism in public discourse, which has turned populism into a form of dog-whistle politics. By demonstrating that populist appeals have the capacity to activate hostility toward minority groups, our study contributes to research on the mobilization of political disaffection and ethnoracial resentment in contemporary politics.

Research on the consequences of populism has been primarily concerned with its effects on policy (Minkenberg 2001; Pappas, Mednez, and Herrick 2009; Zaslove 2004), governance (Akkerman and de Lange 2012; Kaltwasser and Taggart 2015; Pappas 2013), and the structure of party politics (Art 2007; de Lange 2012; Fallend and Heinisch 2015). Less attention has been paid to the effects of populist discourse on the beliefs of everyday voters, particularly those who actively support populist parties and politicians. Given that populism is based on moral critiques of elites and the glorification of “the people,” it is likely to affect trust in democratic institutions and the perceived legitimacy of elected officials, civil servants, and experts. But populist discourse may also have consequences less directly related to its anti-elite orientation. Among radical-right candidates, anti-elite rhetoric has been frequently accompanied by ethno-nationalist hostility toward immigrants and minorities (Ivarsflaten 2008; Mudde 2007; Rydgren 2008), as evidenced by recent political campaigns in the United States, France, the Netherlands, the United Kingdom, and elsewhere. The bundling of anti-elite and ethnoracially exclusionary claims—and the overarching moral dualism inherent in populist discourse—raises the possibility of spillover effects across cognitive domains, whereby anti-elite talk alone may prime greater antipathy toward immigrants and minorities. This paper examines whether this is the case in the United States. Does populism targeting political elites effectively function as a form of ethnoracial dog-whistle politics?¹ And if so, is this effect present among audiences across the political spectrum or primarily those on the right, where ethno-nationalist populism has been most prevalent?

¹ “Dog-whistle politics” refers to the use of ostensibly innocuous discursive cues that prime more insidious out-group hostilities, particularly among those who share the ideological predilections of the speaker. In practice, such coded language often evokes racially charged attitudes, but it has also been used in religious and anti-immigrant discourse. The metaphor is a reference to high-pitched dog-training whistles that use frequencies inaudible to humans.

Finally, does populism focused on economic elites have comparable spillover effects vis-à-vis economic nationalism, in the form of negative sentiments toward countries often blamed for the offshoring of American manufacturing jobs?

These questions are important for both substantive and theoretical reasons. Right-wing populist parties have drawn support from segments of the population strongly opposed to immigration and hostile toward minorities (Ivarsflaten 2008; Rydgren 2008). But these sentiments are not new—they have been part of the cultural landscape of most Western countries for decades (Smith 1997; Bonikowski 2017; Bonikowski and DiMaggio 2016). It is the ability of the radical-right to effectively activate these beliefs that has fueled its recent successes. What is less clear is whether this newfound resonance is solely a result of explicitly ethno-nationalist discourse or if it is also reinforced by populist anti-elite talk. If the latter were true, this would suggest that populism itself can stand in for the host ideologies to which it is attached.

Using a survey experiment design, our paper seeks to determine whether populist political discourse increases out-group antipathy in the United States. In Study 1, we presented respondents with one of three vignettes, featuring morally neutral claims, anti-elite discourse targeting politicians, and anti-immigrant discourse. Following exposure to the vignettes, respondents were asked a series of attitudinal questions, including feeling-thermometer items measuring their sentiments toward specific social groups, including immigrants and minorities.

We supplement these data with a second study, which follows a similar design, but examines economic populism and economic nationalism. The three vignettes include a morally neutral condition, an anti-elite condition targeting economic leaders, and an economic nationalist condition vilifying United States' export-oriented trade partners. The outcomes of interest

measure sentiments toward countries frequently perceived as competing for Americans' jobs, including China, Mexico, India, and Vietnam, again using feeling thermometers.

Results from Study 1 suggest that populist talk alone does activate greater out-group hostility, as demonstrated by lower scores in the populist condition compared to the control condition on feeling thermometers toward immigrants, African Americans, Latinos, and Asian Americans (as well as wealthy elites, who are more directly associated with populist claims). An interaction analysis suggests that only some of these associations differ significantly by respondents' party affiliation. We posit that the spillover effects of populism on out-group attitudes are either a result of the bundling of anti-elite talk with ethno-nationalism by political authority figures, which once routinized, charges populism with out-group hostility at the level of individual attitudes, or of respondents' propensity to generalize morally binary sentiments across domains regardless of the content of political discourse.

The results of Study 2, on economic populism, suggest that the issue bundling mechanism is predominant. Whereas a subset of respondents exposed to economic-nationalist claims reports greater antipathy toward countries associated with the offshoring of U.S. jobs² than respondents in the control condition, economic populism alone does not produce comparable effects (neither among the full sample nor among Democrats, who are more likely to have had prior exposure to economic populist claims in the public sphere).³ General propensity toward domain-independent binary moral thinking, therefore, appears not to be sufficient for cross-domain spillover effects. Unlike populism targeting political elites, economic populism has

² Among Democrats, this effect is present only among the 70.4 percent of respondents who do not express disagreement with the economic nationalism vignette.

³ In direct contrast to Study 1, economic populism also fails to generate greater antipathy toward minority and immigrant groups. We do not foreground this result, however, because it represents a less conservative test of the posited mechanisms, given that ethno-nationalism is not a salient feature of contemporary radical-left political discourse in the United States.

not been prominently combined with economic nationalism or with ethno-nationalism in recent political discourse and as a result, respondents have not developed strong associations between the corresponding attitudes.

Our study documents the capacity of seemingly innocuous anti-elite talk, which gains its legitimacy from the appearance of speaking truth to power, to heighten out-group hostility in other domains of social life and to mobilize political support among ethno-nationalist segments of national polities. In so doing, we focus specifically on the activation of attitudinal associations across cognitive domains, following the dual insight from cultural sociology that meaning is inherently relational and that associations between beliefs are shaped by repeated exposure to patterned environmental cues (Martin 2004; Goldberg and Stein 2018). By combining this perspective with previously separate literatures on populism and ethnoracial exclusion, our paper contributes to the sociological understanding of radical-right populist politics, which have been in ascendance across contemporary democracies.

Populism

Populist politics have been a topic of growing attention in Europe and the United States, as radical parties and candidates on both sides of the political spectrum have mobilized growing segments of national electorates using anti-elite appeals (Berezin 2009; Brubaker 2017; Mudde 2007; Oliver and Rahn 2016). The academic literature on this topic has been concerned with defining the phenomenon (e.g., Deegan-Krause and Haughton 2009), understanding the sources of support—both attitudinal and structural—for populist parties (e.g., Ivarsflaten 2008), and examining the consequences of the rise of populism for policy and party politics (e.g., de Lange 2012). In so doing, studies often conflate populism with the political ideologies to which it is attached, so that in effect, populism, ethno-nationalism, and, in some cases, welfare chauvinism

or Euroskepticism are treated as a single ideological complex used to classify parties. More recently, however, scholars have called for the analytical separation of populism from associated political ideologies in order to study predictors and consequences specific to populism itself (Aslanidis 2016; Bonikowski 2017; Moffit and Tormey 2013; Roodijn and Akkerman 2015). Our paper builds on this approach by examining whether populist discourse has implications for influencing or triggering specific social attitudes at the micro level.

How do we conceptualize populism? While scholars have framed populism as an ideology and a form of political practice, we rely on a discursive definition of the phenomenon (Bonikowski and Gidron 2016; Hawkins and Kalwasser 2017). From this perspective, populism is a form of political claims-making predicated on a moral opposition between corrupt elites and the virtuous people, with the latter viewed as the only legitimate source of political power. Populist discourse seeks to mobilize support for political campaigns and policy initiatives by capitalizing on institutional distrust and anti-elite sentiments among politically disaffected segments of the population (Akkerman, Mudde, and Zaslove 2013; Rico, Guinjoan, and Anduiza 2017; Schulz et al. 2017; Spruyt, Keppens, and Droogenbroeck 2016).

The discursive approach to populism research is sensitive to context, because it assumes that the same political actor may use populism in one setting but not another. More importantly for our purposes, it also suggests that populism should be located not in the ideological statements of parties, but rather in the content of political speech itself. It is at the point of contact between politicians and citizens that populism is most potent.

As past research has shown, populism can be found on both sides of the political spectrum. On the left, populism tends to take an economic form, with the vilified elites consisting of business leaders and their political allies. On the right, populism frequently targets

elected officials and civil servants, who are viewed as having forsaken the common good in favor of their own interests. At the same time, right-wing populism frequently lays blame for social and economic problems on ethnic, racial, and religious minorities and immigrants, who are seen as receiving special favors from out-of-touch politicians. This has led some scholars to conclude that right-wing populism tends to be exclusionary (Mudde and Kaltwasser 2013), while left-wing populism is more likely to be inclusive (Madrid 2008). It is important to note, however, that this is more a statement of empirical regularity than of theoretical principle: there is no reason to assume that anti-business populism on the left, for instance, couldn't resort to exclusionary ethno-nationalist claims. In fact, there are historical precedents for this, as in the case of the U.S. labor movement (Olzak 1989). Nonetheless, in contemporary politics, categorical exclusion appears to be primarily associated with populism on the radical right.

The connection between anti-elite discourse and ethnoracial exclusion is not limited to the messaging of parties and politicians—it extends to the beliefs of these parties' supporters as well. Researchers have demonstrated that, on average, those susceptible to right-wing populist claims, both in the United States and Europe, tend to hold more negative attitudes toward immigrants and racial, ethnic, and religious minorities (Ivarsflaten 2008; Oliver and Rahn 2016; Rydgren 2008; Oesch 2008).⁴ It is plausible then that when supporters of right-wing populist candidates are exposed to populist claims in defense of “the people,” they interpret such claims as code for “people like us”—that is, white and native born, and more often than not, lacking college degrees and residing outside of major urban centers (Cramer 2016; Gidron and Hall 2017). For many, the increased salience of in-group identification combined with the activation

⁴ This does not preclude the possibility that populism also exploits some voters' political disaffection, economic insecurity, and anxiety about cultural change without associating those grievances with out-group hostility.

of out-group resentments is likely to serve as a powerful source of political mobilization, both in the streets and at the ballot box (Tajfel 1983).

The coupling of anti-elite discourse and ethno-nationalism among right-wing politicians raises the question of whether cross-domain spillover effects of populism—if any—will be more prevalent among respondents on the right than the left. Even though supporters of right-wing candidates and parties are the primary audience for such claims, this type of discourse is likely to reach those on the left as well, possibly making them susceptible to similar cross-domain spillover effects. Because our data include pre-treatment measures of political preferences, we can investigate these possibilities by performing a moderation analysis that interacts party identification with exposure to populist discourse as predictors of sentiments toward out-groups.

Racial Resentment

The sources of out-group antipathy toward minorities have been extensively studied by social psychologists. Most of this work has focused on the role of perceived racial threat in increasing racial resentment and on the consequences of these processes for social interaction and support for exclusionary social policies. This line of inquiry is grounded in group position theory (Blumer 1958; Bobo 1999; Bobo and Hutchings 1996), which posits that among members of dominant groups within a racial status hierarchy, perceived threats to group position tend to trigger increased resentment towards members of out-groups. Negative racial attitudes triggered by perceived racial threat, in turn, have been shown to have wide ramifications, including strengthened commitment to conservative ideology and the Republican Party (Craig and Richeson 2014a), discrimination against ethnic minority and foreign-born welfare claimants (Ford 2016), racially biased definitions of academic merit (Samson 2013), and greater propensity to support the Tea Party (Willer, Feinberg, and Wetts 2016).

One major source of perceived racial threat is demographic change. Experimental studies have shown that when exposed to information about white demographic decline or increased racial diversity, white respondents express more negative attitudes towards other racial groups (Craig and Richeson 2014b; Enos 2014; Outten et al. 2012). As Abascal (2015) argues, group members in an established racial hierarchy tend to respond to threatening primes by “prioritizing the most privileged identity to which they can plausibly lay claim and which also excludes the growing group” (p. 789). It is not coincidental then that leaders of radical-right populist parties and movements frequently seize on alarmist imagery of nations besieged by immigrants and minorities, who allegedly threaten the dominance of the white, native-born majority. By vilifying members of ethnoracial groups as morally suspect while denigrating liberal elites who allegedly cater to the former’s “special interests,” ethno-nationalist populists are able to activate racial resentments and mobilize them for political ends (Ivarsflaten 2008; Schmuck and Matthes 2015).

Researchers have also assessed the attitudinal effects of perceived political threat from racial minority groups. Messing, Jabon, and Plaut (2015), for instance, demonstrate that exposure to political advertisements featuring images of President Obama with artificially darkened complexion trigger negative racial stereotypes of African Americans among white respondents. Moreover, the same study finds that such manipulated imagery is routinely employed in actual campaign communications. Willer, Feinberg, and Wetts (2016) use images of President Obama to show that the political salience of race increases perceived racial threat in a manner similar to information about demographic change and economic competition, and increases racial resentment. Thus, racial hostilities can result not only from concrete information that highlights inter-group resource competition, but also from an increase in the salience of race in politics.

Our study examines racial antipathy, but it also extends the analysis to other out-group attitudes, including those targeting religious minorities as well as documented and undocumented immigrants. We do so, because the resentments mobilized by radical-right actors are not solely racial. By engaging with ethno-nationalism more generally, this form of politics makes salient essentialist definitions of nationhood that are just as likely to evoke xenophobia, religious intolerance, and cultural anxiety as they are to emphasize racial symbolic boundaries (Bonikowski and DiMaggio 2016). For this reason, we conceptualize the hypothesized spillover effects as involving out-group sentiments in general. Nonetheless, our analyses are able to distinguish between respondents' dispositions toward specific groups, which allows us to treat potential variation in the experimental effects across target groups as an empirical question.

Dog-Whistle Politics

The use of subtle visual cues in research on racial resentment reinforces a broader experimental finding that racial threat need not be communicated explicitly in order to provoke out-group hostility. Indeed, U.S. politics is rife with examples of coded language intended to mobilize white voters through subtle appeals to racial resentment, as evidenced by Nixon's Southern Strategy and its imagery of the "silent majority," or the "law and order," "tough on crime" rhetoric of the 1990s (Haney López 2014). Populism itself has had a troubled history of subtly veiled racial appeals in the United States, as noted in Kazin's (1998) account of nineteenth century agrarian politics: "[T]he Populists continued to assume, as had their Jeffersonian and Jacksonian forebears, that the 'plain people' meant those with white skin and a tradition of owning property on the land or in the craft. Not surprisingly, most blacks did not accept the Populists' circumscribed offer" (p. 41).

Experimental research has confirmed that multivocal appeals have the capacity to effectively communicate coded messages, particularly to those predisposed to recognize them. This is the case, for instance, with subtle religious undertones that mobilize support among devout voters but are largely invisible to those who do not hold strong religious beliefs (Albertson 2015; Calfano and Djupe 2009). Similar effects have been found for racial dog-whistle discourse, which is able to increase perceptions of racial threat that in turn lead to greater levels of racial resentment (Hurwitz and Peffley 2005; Valentino, Hutchings, and White 2002; White 2007). Unlike in the case of religious language, however, the coded meaning of racial dog-whistles is typically not lost on the minority groups targeted by them or on other audiences who have gained sufficient familiarity with such discursive strategies. In this sense, the dog-whistle metaphor has drifted from its original referent and has come to signify coded exclusionary language in general, and not just exclusionary language imperceptible to anyone but its intended audience.

Nevertheless, the propensity of audiences to be effectively mobilized by the cues implicit in dog-whistle discourse may depend on their sharing similar worldviews and biases as the speaker (Ford 2016; Schmuck and Matthes 2015), as well as on the absence of disconfirming counter-stereotypical information (Valentino, Hutchings, and White 2002). This suggests that if populism does generate spillover effects into the domain of ethno-nationalist beliefs, it may do so primarily among those respondents who are already predisposed toward out-group hostility.

This is not to say, however, that dog-whistle appeals are simply redundant with audiences' pre-existing beliefs. On the contrary, by increasing the salience of pre-existing out-group resentments, subtly coded discourse can effectively channel those beliefs into support for exclusionary politics. Moreover, even if onlookers do recognize dog-whistle discourse for what it

is, its coded content gives politicians and their supporters plausible deniability if challenged: these actors are likely to reject charges of exclusionary predilections by referring to the innocuous nature of the messages in question (Haney López 2014).

Similar implications may extend to political discourse that combines explicit ethno-nationalist content with populist appeals, as was the case with Donald Trump's presidential campaign. The joint use of populism and ethno-nationalism may allow politicians and their supporters to emphasize their anti-elite tendencies while downplaying racial resentment, even as the populist appeals subtly cue both sets of beliefs (of course, not all supporters will deny racially based motivations; some are likely to explicitly embrace them). Trump had frequently employed such bait-and-switch tactics in his campaign, endorsing exclusionary claims at one moment and backtracking shortly thereafter, while all along staying on-message about the corruption of political elites.

Taken together, research on populism, racial resentment, and dog-whistle politics suggests that populist claims may serve as subtle cues that activate people's antipathy toward ethnoracial minorities and immigrants. In examining this possibility, we focus on four hypotheses. The first concerns the presence of spillover effects in the full sample of respondents:

Hypothesis 1 (Dog-whistle effect of generic populism): Respondents exposed to populist discourse targeting political elites will express less positive views of immigrants and racial minorities than respondents assigned to the control condition.

An aggregate effect of populism on out-group hostility may mask underlying moderating effects of respondents' social and political attributes. In particular, respondents who have been most frequently exposed to—and have favorable views of—political discourse that couples anti-elite and ethno-nationalist claims may be most likely to respond to populist dog-whistle

messages. Given that exclusionary populist claims are most common on the political right, we may find the strongest spillover effects among Republicans:

Hypothesis 2 (Moderation of dog-whistle effect by party): The differences in respondents' sentiments toward immigrants and racial minorities observed in the right-wing populist and control conditions will be larger among Republicans than Democrats.

Finally, if we do observe spillover effects, these could stem from two distinct mechanisms: the bundling of anti-elite and ethno-national talk in public discourse, which primes respondents to perceive these claims as mutually substitutable, or, alternatively, respondents' general propensity to associate moral binary classifications across domains.

Without longitudinal data that predates the 2016 election, it is difficult to directly adjudicate between these alternative scenarios. It is possible, however, to gain some insight into this problem by considering the type of populist discourse, typically found on the political left, that targets economic elites and the possible spillover of its moral dualism onto chauvinist attitudes toward countries perceived as competing for American jobs, particularly in manufacturing and in geographically mobile segments of the service sector (such as call centers). This is the objective of our second study.

The logic of the experiment in Study 2 is based on the observation that Democratic respondents are likely to be susceptible to both economic populist claims (i.e., those targeting self-serving economic elites) and economic nationalist claims (i.e., those targeting countries associated with the offshoring of U.S. jobs). These claims, however, have not been coupled explicitly in recent left-wing political discourse, including the campaigns of leading Democratic presidential candidates in 2016. Therefore, if we observe a spillover of moral dualism from the domain of economic populism to the domain of economic nationalism among Democratic respondents, we can conclude that this is a result of their propensity to connect those two domains

independently of political campaigns and related public commentary.⁵ On the other hand, if we do not observe such spillover—that is, if economic populist primes do not result in greater antipathy toward countries associated with the offshoring of U.S. jobs—then we can conclude that this absence is due to the lack of bundling of these two issues in the public sphere.⁶

Hypothesis 3 (Mechanism 1: propensity toward morally binary beliefs):

Democratic respondents exposed to populist discourse targeting economic elites will express greater antipathy toward countries associated with the offshoring of U.S. jobs than respondents assigned to the control condition.

Hypothesis 4 (Mechanism 2: issue bundling in public discourse): Democratic respondents exposed to populist discourse targeting economic elites will *not* express greater antipathy toward countries associated with the offshoring of U.S. jobs than respondents assigned to the control condition.

Data and Methods

To test these hypotheses, we carried out online survey experiments, in which respondents were exposed to excerpts from political speeches and asked a series of questions about their social and political attitudes, including their sentiments toward specific minority groups (in Study 1) and other countries (in Study 2). We randomly sampled respondents from online panels managed by the Harvard Digital Lab for the Social Sciences (DLABSS) and Qualtrics in August 2016 (Study 1) and the summer of 2018 (Study 2). We relied on these two recruitment channels to maximize variation in partisan identification (which was measured prior to experimental treatment): the DLABSS panel consists primarily of Democratic respondents, while Qualtrics was able to oversample Republican respondents. DLABSS promoted the survey through email lists, website

⁵ For economic populism to serve as a dog-whistle for economic nationalism, Democratic respondents would need to be susceptible to the effects of economic nationalism in the first place. Given that Democratic respondents are divided in their support for economic nationalist claims (28.4 percent of those assigned to the economic nationalist condition disagree with the experimental vignette), we treat this variable as a moderator in the analysis.

⁶ In the interest of direct comparison with Study 1, we also examined associations between economic populism and antipathy toward immigrants and minority groups, with the latter typically absent from left-wing discourse.

ads, and tweets, whereas Qualtrics relied on their existing panel of regular survey participants. Both recruitment efforts targeted U.S. residents 18 years of age or older. Potential participants were invited to take part in a research project investigating the persuasiveness of political messaging and were informed that their participation would consist of evaluating ideas commonly expressed by candidates in U.S. elections, as well as answering questions about their own political views. We subsequently eliminated from the samples respondents who were not U.S. citizens and who are likely to have sped through the survey without closely reading the content (i.e., those whose survey completion time was 75 percent faster than the median).

The survey data collection yielded 935 respondents for Study 1 (288 self-identified Democrats, 478 Republicans, 136 independents, and 33 respondents with missing party identification data) and 958 respondents for Study 2 (567 self-identified Democrats, 344 Republicans, and 47 independents). In each experiment, the respondents were randomly assigned to three conditions. In the control condition (Study 1 $N = 318$; Study 2 $N = 318$), which was identical across the two studies, participants read a vignette that used morally neutral language to describe difficulties in direct communication between elected representatives and their constituencies. The problem was framed in technical terms, no individuals or groups were assigned blame, and a vaguely technocratic solution to the problem was proposed. The remaining conditions differed between Study 1 and Study 2.

In the populist condition in Study 1 ($N = 300$), participants were presented with a speech excerpt containing morally charged language accusing elite politicians of benefiting themselves at the expense of “the people.”⁷ In the anti-immigrant condition in Study 1 ($N = 317$), the

⁷ The vignette was designed to be politically multivocal, mixing excerpts from left- and right-wing populist speeches. In a separate analysis on Mechanical Turk, respondents who were asked to identify the source of the vignette attributed it to both Bernie Sanders and Donald Trump—and to a lesser degree, to Hillary Clinton and Marco Rubio. The results are available upon request.

vignette used incendiary language to criticize undocumented immigrants, making reference not only to their legal status but also to their fundamental moral failings. The vignette for the populist condition in Study 2 (N = 313) targeted economic elites, blaming Wall Street, billionaires, and corporate leaders for rigging the political system in their own favor. Finally, the economic nationalist condition in Study 2 (N = 327) accused China and “other countries” of not playing by the rules, taking advantage of the United States, and decimating the American middle class.

To maximize the vignettes’ plausibility, we selected excerpts from actual political speeches made by U.S. political candidates, edited them for maximum impact, and combined them into the five distinct passages. We made sure that the vignettes were of equal length, totaling 107 words each. Participants were informed that they were reading actual speeches made by U.S. political candidates. The vignettes are presented in Table 1.

It is important to note that the vignettes’ varied intensity is intentional, not only because it reflects real political discourse, but also because it facilitates the comparisons in our analysis. For instance, the strong language in the anti-immigrant condition in Study 1 serves as a baseline for examining the magnitude of possible dog-whistle effects in the right-wing populism condition. The tone of the economic nationalism condition in Study 2, on the other hand, is milder, to prevent socially desirable responses by Democratic respondents who may be averse to explicitly inegalitarian frames. Despite these differences, our vignettes represent relatively weak primes compared to the intensity and frequency of populist, ethno-nationalist, and economic nationalist claims-making in contemporary U.S. political discourse. Therefore, any effects we observe in our study are likely to underestimate the actual impact of such claims in everyday political life.

Table 1. Vignettes used in Study 1 and Study 2

Study 1 and Study 2, Condition 1: Control

The current political system makes it really difficult for the government to connect with its constituents. The institutions are built in such a way that the government's attention is often distracted by many competing pressures, no matter how hard it tries to focus on representing the voters. To fix the system requires a lot of effort, in terms of both policy design and active participation on the part of citizens. There should be new channels of communication that would make it easier for voters to make their voices heard. At the same time, voters should be more actively engaged in reaching out to those who represent them.

Study 1, Condition 2: Right-Wing Populist Discourse

People across this country, ordinary folks with bills to pay and kids to raise, know that politicians and bureaucrats in Washington don't really work for us. The government is run by powerful elites who are fully controlled by the lobbyists and the donors. These corrupt politicians make decisions that benefit themselves and their rich friends, and ignore our wellbeing. They represent themselves only, not us. This is how hard-working American taxpayers end up being ruled by special interests in Washington. This is why we, everyday people, feel we have lost control of our own government and that Washington insiders have become our masters instead of our servants.

Study 1, Condition 3: Anti-Immigrant Discourse

The thousands of immigrants who come in illegally to steal our jobs are intolerable. They flood in, go on welfare, commit crimes, exploit our country, and pay no consequences for it. America can no longer afford to be soft on them. The government has tried to make policies that go after these immigrants, but its efforts have been limited because the current political system makes it really difficult to create real change. The institutions are built in such a way that the government's attention is often distracted by many competing pressures, no matter how hard it tries to focus on the issues that voters care about most.

Study 2, Condition 2: Economic Populist Discourse

The greed of Wall Street and corporate America is destroying the lives of everyday folks like us. Throughout history, the billionaires and corporate leaders have rigged our government in their favor, leaving us far behind. Our voices have been drowned out by powerful corporations and big finance. The business elites get whatever they want from policymakers, we get absolutely nothing. What kind of democracy are we living in? Democracy in our country is for the wealthy few, and ordinary taxpayers like us, people who work hard to make ends meet, are being crushed by the top one percent. Their corporate interests dominate everything; our interests are ignored.

Study 2, Condition 3: Economic Nationalist Discourse

People across this country, ordinary folks with bills to pay and kids to raise, are suffering because China and other countries are not playing by the rules. They are taking our jobs by cutting labor standards, stealing our intellectual property, and dumping their cheap products on the American market. They are taking the dollar hostage and they are trying to change global trade rules so that take can keep taking advantage of us. China gets richer and richer while the American middle class collapses. This cannot go on anymore. We need to stand up for hard-working Americans by negotiating trade deals that benefit us, not other countries.

After reading the vignettes, respondents answered a series of attention-check questions and then completed a survey concerning their social and political attitudes, including a series of items assessing their favorability toward minority groups, elites, and other countries. The minority groups of particular interest in Study 1 include African Americans, Latino/a Americans, Asian Americans, undocumented immigrants, documented immigrants, Muslims, Jews, and wealthy people in the top one-percent of the income distribution. The countries in Study 2

comprise China, Mexico, India, Vietnam, Russia, and Canada (we included Russia and Canada for purposes of comparison, the former having a controversial reputation in the domain of politics but not economic competition and the latter being largely uncontroversial). Responses to these items were recorded using feeling thermometers ranging from 0 to 100. Finally, respondents were asked a set of standard demographic questions. Table 2 reports descriptive statistics for both studies.

Results

The analysis of Study 1 data proceeds in two steps. First, we compare the mean feeling thermometer values across the three conditions for the sample as a whole. This tests Hypothesis 1, whether populism targeting political elites has general spillover effects that are not moderated by any particular sociodemographic variables. Second, we use regression models with interaction terms to examine the moderating effect of party identification. This allow us to evaluate Hypothesis 2, which posited that populism’s dog-whistle effects may be concentrated among those respondents who have been most frequently exposed to, and are most favorable toward, political discourse that connects anti-elitism with racial resentment. Given the prominence of such claims among Republican politicians and media personalities, we can expect the spillover effects to be strongest among Republican respondents.

To identify the mechanisms potentially driving the observed spillover effects—that is, to distinguish between issue bundling in public discourse and respondents’ propensity to connect binary moral claims across domains—we use data from Study 2, which examines economic populism and economic nationalism, strategies more commonly observed in left-wing politics. We first determine whether economic nationalism is associated with negative sentiment toward

Table 2. Descriptive statistics for Study 1 and Study 2

Variable	Study 1 (N = 935) Proportion / mean	Study 2 (N = 958) Proportion / mean
Race: White	0.81	0.74
Race: Black	0.08	0.13
Race: Latino/a	0.07	0.09
Race: Asian	0.03	0.04
Race: Other	0.03	0.01
Gender: Female	0.64	0.68
Education: High school or less	0.32	0.34
Education: Associate or college degree	0.48	0.54
Education: Graduate degree	0.19	0.13
One or both parents born in the U.S.	0.93	0.93
English as primary language at home	0.99	0.99
Party: Democrat	0.32	0.59
Party: Republican	0.53	0.36
Party: Independent	0.15	0.05
Age (mean)	51.67	55.54

countries associated with the offshoring of U.S. jobs by comparing country feeling thermometer values between the economic nationalism and control conditions in the sample as a whole, among Democratic respondents, and among a subset of Democrats who are not strongly opposed to economic nationalist discourse. In assessing the significance of the experimental effects, we rely on regression models, first without and then with an interaction term between experimental group assignment and opposition to economic nationalism. Second, we compare the country feeling thermometer values between the economic populism condition and control condition, again using regression models. Significant differences in means between the two conditions can be interpreted as evidence for respondents' general propensity toward moral binary classification in the absence of issue bundling in public discourse (i.e., Hypothesis 3), while the lack of such differences would suggest that issue bundling in public discourse is necessary for the activation of attitudinal links between populism and nationalism (i.e., Hypothesis 4).

Table 3. Group feeling thermometer mean values; control, populism, and immigration conditions

	1. Control (N=318)	2. Populism (N=300)	3. Immigration (N=317)	1 vs. 2 (<i>t</i> / <i>p</i> - value)	1 vs. 3 (<i>t</i> / <i>p</i> - value)	2 vs. 3 (<i>t</i> / <i>p</i> -value)
African Americans	75.4	69.8	71.2	-2.85 (0.002)	-2.19 (0.015)	0.68 (0.248)
Latino/a Americans	72.7	69.3	70.7	-1.67 (0.048)	-1.00 (0.159)	0.66 (0.255)
Asian Americans	75.1	71.7	73.5	-1.78 (0.038)	-0.83 (0.202)	1.00 (0.159)
Undocumented immigrants	40.1	34.6	35.9	-2.29 (0.011)	-1.75 (0.04)	0.54 (0.294)
Documented immigrants	72.6	67.8	71.8	-2.26 (0.012)	-0.37 (0.355)	1.95 (0.026)
Muslims	51.7	49.1	50.5	-1.06 (0.145)	-0.51 (0.305)	0.56 (0.288)
Jews	74.1	72.2	71.8	-0.96 (0.17)	-1.15 (0.126)	-0.21 (0.418)
The wealthy top 1%	49.1	42.1	45.7	-3.04 (0.001)	-1.45 (0.074)	1.54 (0.062)

Table 3 presents the means of the feeling thermometers in Study 1 across the three conditions (control, right-wing populist, and anti-immigration). Consistent with the expectation that populism would be associated with stronger anti-elite sentiments, respondents in the populist condition evaluated wealthy Americans more negatively than those in the control condition (49.9 vs. 43.5). Moreover, exposure to populist rhetoric appears to generate greater antipathy toward African Americans (lowering favorability toward this group by 5.6 points, or 7 percent), Latino/a Americans (by 4 percent), Asian Americans (by 5 percent), undocumented immigrants (by 15 percent), and documented immigrants (by 5 percent). Sentiments toward Muslims and Jews are also less positive in the populist condition, but the differences are not statistically significant.

The magnitude of the dog-whistle effects of populism is similar to that of explicit (and strong) anti-immigrant discourse, as demonstrated by the non-significant differences in means between these two conditions. In fact, the post-treatment decrease in favorability toward immigrants with legal status in the United States is significantly greater in the populism condition than in the anti-immigrant condition (the vignette in the latter focuses specifically on

undocumented migration). These results lend support to Hypothesis 1 and suggest that populism does function as a form of dog-whistle politics: absent ethno-nationalist cues, the vilification of political elites affects respondents' dispositions toward ethnic and racial minority groups.

The fact that we observe any significant differences after respondents' single exposure to a brief vignette—what we consider a conservative test of our conceptual framework—is substantively and theoretically relevant. The effects of frequent exposure to more extensive political messaging in less artificial settings are likely to be much greater. This is further supported by the finding that populism appears to have similar effects on out-group antipathy compared to incendiary anti-immigrant discourse, the political relevance of which is well documented (e.g., Matthes and Schmuck 2017; Schemer 2012). Nonetheless, in an effort to assess the magnitude of the observed effects, we compared them to associations between feeling thermometer responses and salient political issue preferences in the 2016 American National Election Study, after controlling for sociodemographic covariates. The results (available upon request) suggest that the effect sizes in our study, conservative though they are, are meaningful. For instance, a similar difference in antipathy toward African Americans to that observed between the populist and control conditions lowers the probability of support for government involvement in improving racial equality by 11 percent in the ANES data, while a comparable difference in antipathy toward undocumented migrants increases the probability of favoring lower levels of immigration by 3 percent.⁸

Interestingly, while the differences between the control and anti-immigrant conditions are consistent with a priming effect of anti-immigrant discourse, they reach significance for only two

⁸ These estimates are based on the 2016 ANES sample limited to white respondents with covariate values set to male, Democrat (reference category: Republican), mean age, and high school education (reference category: less than a high school degree).

groups: undocumented migrants and African Americans. The former group is directly targeted in the anti-immigrant vignette, but the latter is not. This suggests that xenophobia and racism are strongly linked in the minds of the respondents and that anti-immigrant discourse may itself serve as a dog-whistle for racial politics.

Next, we investigate the possibility that the dog-whistle effect is moderated by party identification. As we outline in Hypothesis 2, the cuing of out-group antipathies by populist claims may be strongest among the intended audience for this dog-whistle strategy, that is self-identified Republicans. Given their distinct policy preferences, Democrats may be unaffected by populist dog-whistling (i.e., not noticing the dog whistle, in keeping with the original meaning of the metaphor) or they may even recoil from it (i.e., noticing the dog-whistle but recognizing it for what it is). The latter possibility would likely result in more positive post-treatment sentiments toward minority groups among Democrats, which could dampen the aggregate dog-whistle effects in the full sample. Alternatively, the cross-domain spillover could extend to Democrats as well, resulting in no significant moderation effect, either due to Democratic respondents' repeated exposure to political discourse that ties populism with ethno-nationalism or due to their general tendency toward binary moral thinking.

To investigate these possibilities, Table 4 reports the condition-specific means disaggregated by party, along with an interaction-based test for the moderating role of party identification.⁹ Among Republicans, the differences in means between the control and populism conditions are roughly consistent with those for the sample as a whole. In addition to intensifying anti-elite attitudes, populism appears to increase antipathy toward African Americans, Latino/a

⁹ We exclude from this and subsequent analyses involving party identification respondents who identify as independents leaning toward the Democratic or Republican Party. Given their ambiguous political identity, the expectations for these respondents are less obvious and their exclusion leads to more conservative tests of our moderation hypotheses.

Table 4. Group feeling thermometer mean values; control and populism conditions by party

	Democrats			Republicans			Interaction <i>t</i> / <i>p</i> -value ¹
	Control (N=99)	Populism (N=91)	Diff. <i>t</i> / <i>p</i> - value ¹	Control (N=158)	Populism (N=159)	Diff. <i>t</i> / <i>p</i> - value ²	
African Americans	79.7	73.7	-1.67 (0.024)	75.0	68.9	-2.19 (0.007)	-0.02 (0.245)
Latino/a Americans	74.3	74.2	-0.05 (0.241)	72.3	68.3	-1.43 (0.039)	-0.84 (0.101)
Asian Americans	74.9	74.9	-0.01 (0.247)	76.6	72.0	-1.70 (0.022)	-1.03 (0.076)
Undocumented immigrants	54.3	52.5	-0.45 (0.163)	29.8	22.8	-2.29 (0.006)	-1.04 (0.074)
Documented immigrants	73.4	73.7	0.09 (0.233)	72.1	65.5	-2.22 (0.007)	-1.43 (0.039)
Muslims	63.6	63.7	0.02 (0.246)	43.0	40.3	-0.83 (0.102)	-0.52 (0.15)
Jews	74.4	71.9	-0.69 (0.122)	75.1	73.1	-0.72 (0.118)	0.11 (0.228)
Wealthy top 1%	38.7	33.1	-1.35 (0.045)	56.1	49.0	-2.25 (0.006)	-0.31 (0.189)

¹ One-tailed significance tests from OLS regression predicting country feeling thermometer with condition

assignment, party identification, and interaction between Republican Party identification and populism condition

² One-tailed significance tests from OLS regression predicting country feeling thermometer with condition

assignment, party identification, and interaction between Democratic Party identification and populism condition

Americans, Asian Americans, and undocumented and documented immigrants. Among

Democrats, the differences in means are significant for sentiments toward African Americans,

but not any of the other groups (the post-treatment mean for sentiments toward undocumented

migrants is lower as well, but the difference fails to reach statistical significance). The interaction

term for party identification, however, is only significant for one of the eight feeling

thermometers: that toward documented immigrants. As a result, for the other seven dependent

variables, we cannot reject the null hypothesis of no consistent partisan differences in the dog-

whistle effects of populism. The evidence for an effect irrespective of party is strongest for

sentiments toward African Americans, which are significantly decreased by populist discourse

among both Democrats and Republicans. Thus, Hypothesis 2 receives mixed support: cross-

domain spillover may be more consistent among Republicans, but Democrats are not immune

from its effects.

We now turn to analyses of the possible mechanisms driving the observed dog-whistle effects. If cross-domain spillover between populism and nationalism is generated by a tendency toward binary moral classification, we should observe comparable patterns of nationalist sentiment (in this case targeting other countries) among respondents exposed to economic populism in Study 2. If, on the other hand, spillover is a result of the bundling of populism and nationalism in public discourse, there should be no observable spillover effect for economic populism, given the absence of such issue bundling in the political campaigns of prominent Democratic candidates, such as Hillary Clinton and Bernie Sanders, or in left-wing media.¹⁰

First, however, we need to establish whether exposure to economic nationalism itself results in greater out-group antipathies. We may observe such effects in the sample as a whole or primarily among Democratic respondents, given the long-standing interest on the left in fair trade practices and the protection of U.S. labor markets (which were only recently co-opted on the right by the Trump campaign). Table 5 presents the mean feeling thermometer values for the sample as a whole, along with significance tests for the differences between the control and economic nationalism conditions. The out-groups of interest here are other countries, particularly those that are frequently vilified in economic nationalist discourse as responsible for large-scale job losses in the United States.

Although the feeling thermometer means for China, Mexico, India, and Vietnam are consistently lower in the economic nationalist condition than in the control group, the only post-

¹⁰ We focus on left-wing political discourse, because populist claims vilifying economic elites are typical of Democratic candidates and are rarely found among their Republican counterparts (Author 2016). In further confirmation of this assumption, a sample of Mechanical Turk respondents asked to identify the source of the economic populist vignette overwhelmingly attributed it to Bernie Sanders and, to a lesser degree, to Hillary Clinton. For the same reason, we focus the analysis of Study 2 data on Democratic respondents. This constitutes a conservative test of Hypotheses 3 and 4: failure to observe spillover effects among this “best-case” sample would constitute strong evidence against the general propensity to link binary moral claims across domains and in favor of issue bundling in political discourse as mechanisms driving the dog-whistle effects in Study 1.

Table 5. Country feeling thermometer mean values by condition; full sample

	1. Control (N=318)	2. Economic nationalism (N=327)	1 vs. 2 (<i>t</i> / <i>p</i> -value)
China	44.3	39.9	-2.12 (0.017)
Mexico	55.1	53.1	-0.88 (0.19)
India	51.3	49.9	-0.70 (0.243)
Vietnam	47.0	46.2	-0.37 (0.356)
Russia	32.1	29.4	-1.36 (0.087)
Canada	79.1	79.0	-0.09 (0.464)

treatment difference that reaches statistical significance is that for China. Conversely, the condition-specific means are most similar for Canada, a country rarely vilified in economic nationalist discourse (at least prior to President Trump’s attempts to renegotiate the NAFTA trade agreement in late 2018).

Based on these results, we could limit our analysis of the potential dog-whistle effects of economic populism to anti-China sentiments. It is possible, however, that economic nationalist claims have more pervasive consequences for chauvinism among Democrats than among the full sample. Given that Democrats are the primary audience for economic populist discourse, they are also the most relevant group for our analysis of its spillover effects.

In examining the consequences of exposure to economic nationalism among Democratic respondents, it is important to take into account variation in support for such political claims. 20.1 percent of Democrats assigned to the economic nationalism condition express disagreement and 8.3 percent express strong disagreement with the experimental vignette. The effects of economic nationalism on chauvinist attitudes are likely to be stronger among the remaining 71.6

Table 6. Country feeling thermometer mean values; control and economic nationalism conditions; Democrats only

	Base model			Moderation by support for economic nationalism			
	Control (\bar{x})	Economic nationalism (\bar{x})	Difference t / p -value ¹	Control (\bar{x} , supporters) ²	Economic nationalism (\bar{x} , supporters) ²	Difference t / p -value ²	Interaction effect t / p -value ²
China	48.3	44.8	1.28 (0.101)	49.0	41.7	-2.09 (0.019)	1.80 (0.037)
Mexico	63.5	61.3	0.84 (0.21)	63.1	56.6	0.84 (0.027)	0.84 (0.014)
India	55.7	52.8	1.22 (0.123)	55.2	49.7	1.22 (0.04)	1.22 (0.079)
Vietnam	51.0	49.4	0.72 (0.261)	51.0	46.1	0.72 (0.073)	0.72 (0.056)
Russia	28.5	26.0	0.92 (0.166)	29.8	26.2	0.92 (0.136)	0.92 (0.301)
Canada	85.0	84.3	0.36 (0.374)	83.5	81.0	0.36 (0.172)	0.36 (0.105)

¹ One-tailed significance tests from OLS regression predicting country feeling thermometer with condition assignment.

² Means and one-tailed significance tests from OLS regression predicting country feeling thermometer with condition assignment, evaluation of economic nationalist prime (imputed), and an interaction term reflecting effect of disagreement with economic nationalist frame in the economic nationalist condition

percent of Democratic respondents. Consequently, we include respondents' evaluation of the vignette as a moderator in the analysis.^{11,12}

Table 6 compares country feeling thermometer values between the economic nationalist and control conditions among Democratic respondents. The results for the base model include condition-specific means and t -tests, while those for the moderation model include means for the

¹¹ Conditioning on post-treatment variables is often inadvisable, because it can bias experimental results if the variable in question is affected by the treatment (Montgomery, Nyhan, and Torres 2018). In this case, however, we are interested in evaluations of the experimental treatment itself, which are elicited immediately after respondents are exposed to the priming vignette. Since this is not a distal variable with independent meaning outside of the post-treatment context, its use as a moderator poses no inherent problems for our analysis.

¹² For the cross-group comparison to be meaningful, we need data on support for economic nationalism among respondents assigned to both the experimental and control conditions, in order to avoid comparing favorably disposed respondents in the experimental condition with both favorably and unfavorably disposed respondents in the control condition. Our survey does not contain this information for the control condition, because to obtain it, we would have had to expose respondents to the economic nationalist vignette, thereby compromising our experimental design. Instead, we only asked respondents to evaluate the vignettes specific to the conditions to which they were randomly assigned. To overcome this limitation, we use chained multiple imputation to predict the evaluation of economic nationalism for respondents assigned to all conditions. This strategy takes advantage of the fact that the data are "missing completely at random" (Rubin 1987). Having imputed respondents' evaluation of economic nationalism, we are able to use this variable as a moderator in the remaining analyses.

70.4 percent of respondents (across all conditions) who are not unfavorable to the economic nationalist prime, *t*- and *p*-values for main between-condition differences in the country feeling thermometers, and *t*- and *p*-values for the interaction term between condition assignment and disagreement with the economic nationalist vignette. Given that our hypotheses are unambiguously directional, we use one-tailed tests to maximize statistical power.

The difference in the results across the two subsamples is striking: none of the differences in feeling thermometer values between the control and economic nationalism conditions reach statistical significance in the base model, but once opposition to economic nationalism is taken into account in the moderation models, exposure to economic nationalism produces significantly greater antipathy toward China, Mexico, and India compared the control condition. Moreover, the interaction terms are significant for two of those three feeling thermometers: those measuring sentiments toward China and Mexico. It appears, therefore, that a majority of—but not all—Democrats are susceptible to economic nationalism, and that among these respondents, economic nationalism breeds chauvinism toward countries often vilified for poaching U.S. jobs.¹³

Having identified the subset of respondents particularly susceptible to the impact of economic nationalism on sentiments toward other countries, we can use this subsample to answer our final question, whether economic populism serves as a dog-whistle for economic nationalism, even in the absence of the bundling of these two discourses in public discourse. Failure to observe this effect among Democratic respondents susceptible to economic nationalist claims themselves can be interpreted as supporting evidence for the centrality of issue bundling,

¹³ Our attempts to maximize the effects of economic nationalism should not be misunderstood as the cherry picking of results. In identifying a subset of respondents particularly susceptible to the effects of economic nationalism, we are ensuring that our subsequent analysis of the dog-whistle effects of economic populism is conservative. Failure to observe dog-whistle effects among a subsample most likely to exhibit them offers a more compelling test of possible mechanisms than doing so among a subsample of respondents only weakly responsive to economic nationalism.

as opposed to inherent attitudinal predispositions, in linking populist and nationalist attitudes, which would aid in the interpretation of the findings from Study 1. To assess the potential spillover effects in Study 2, we compare the mean country feeling thermometer values between respondents exposed to the economic populism vignette and those assigned to the control condition, using a model that accounts for the previously established moderating effect of the vignette evaluation.

In testing this hypothesis, however, we cannot interpret failure to observe a statistically significant dog-whistle effect as evidence for the absence of such an effect, since one cannot prove a null hypothesis. Instead, we must swap our null and alternative hypotheses and test for the equivalence of country feeling thermometer means across the control and economic populism conditions (Streiner 2003). Under this test design, the null hypothesis posits that there is a meaningful difference between the means across the two conditions, with “meaningfulness” defined in terms of an absolute value δ , expressed in the units of the dependent variable, that generates an interval around the observed difference (Tryon and Lewis 2008). A statistically significant *t*-test for a given feeling thermometer would allow for the rejection of the null hypothesis of difference at a given level of significance (we use a combined $\alpha = 0.05$ for two one-tailed tests) and serve as evidence for the alternative hypothesis, which posits that the condition-specific means of a dependent variable are equal within the interval defined by δ .

In evaluating equivalence, we rely on $\delta = 6$, meaning that the null hypothesis posits the difference in means between the two conditions to be equal within an interval of +/- 6 points on the 100-point scale of the feeling thermometer. This is the lowest threshold at which our sample size is sufficiently powered to detect a statistically significant effect.¹⁴ Moreover, given the size

¹⁴ This threshold also represents a smaller between-condition difference than those observed for China, Mexico, and India in the previous analysis of the economic nationalism and control conditions.

Table 7. Country feeling thermometer mean values; control and economic populism conditions; Democrats only

	Control, supporters (\bar{x}) ¹	Economic populism, supporters (\bar{x}) ¹	Difference t / p -value ¹	Equivalence ($\delta = 6$) left-tail t / p -value ²	Equivalence ($\delta = 6$) right-tail t / p -value ²
China	48.9	45.7	0.40 (0.345)	3.02 (0.001)	1.04 ³ (0.149)
Mexico	63.1	64.9	0.09 (0.463)	1.92 (0.028)	2.29 (0.011)
Russia	29.7	26.7	0.45 (0.327)	3.21 (0.001)	1.09 ⁴ (0.138)
India	55.2	56.2	0.18 (0.427)	2.05 (0.021)	2.30 (0.011)
Vietnam	51.0	51.6	0.33 (0.37)	2.06 (0.020)	2.16 (0.016)
Canada	83.5	86.0	-0.48 (0.315)	1.92 (0.028)	3.56 (0.000)

¹ Means and one-tailed significance tests from OLS regression predicting country feeling thermometer with condition assignment, evaluation of economic nationalist prime (imputed), and an interaction term reflecting effect of disagreement with economic nationalist frame in the economic nationalist condition.

² Two one-tailed significance tests with $\delta = 6$, limited to subset of respondents who did not express disagreement with the economic nationalist vignette (imputed values).

³ Indeterminate result at $\delta = 6$; reaches significance at $\delta = 9$.

⁴ Indeterminate result at $\delta = 6$; reaches significance at $\delta = 8$.

of the standard errors and the degrees of freedom in our sample, under no conditions could a null hypothesis of difference be rejected at $\delta < 5$ (Dinno 2017), which makes the chosen threshold reasonably conservative. It is important to stress that we consider the equivalence-test results alongside traditional hypothesis tests, which place the equivalence of condition-specific means within the null hypothesis. None of the latter tests produce statistically significant results, so equivalence cannot be ruled out for any of the country feeling thermometers (and, more directly, that significant differences between thermometer means can in fact be ruled out).

Table 7 presents the results of the comparison of country feeling thermometer values between the control and economic populism conditions. In light of the moderating effects demonstrated in the previous analysis, the condition-specific means and significance tests of difference in the table are calculated based on the results of a regression model with an interaction term between condition assignment and the imputed evaluation of the economic nationalist vignette. The t -tests of equivalence were calculated with the `testt` package for Stata

(Dinno 2017) using a sample restricted to respondents who did not disagree with the economic nationalism vignette. Non-significant results for the tests of difference combined with significant results of the tests of equivalence can be interpreted as strong evidence of equivalence, while failure to reject both null hypotheses indicates an indeterminate solution due to the sample size lacking sufficient statistical power (Tryon and Lewis 2008).

For four out of the six feeling thermometers, the results provide strong evidence for the equivalence of means across the control and economic populism conditions at $\alpha = 0.05$ and $\delta = 6$. This is the case for Mexico, India, Vietnam, and Canada. For China and Russia, the null hypothesis that the mean feeling thermometer value is greater in the economic populism condition cannot be rejected at $\delta = 6$, but the t -score reaches significance at $\delta = 8$ and $\delta = 9$, respectively. Given the non-significance of the tests of difference for all of the feeling thermometers and the significance of the tests of equivalence within the given intervals, we conclude that there is no compelling evidence for economic populism serving as a dog-whistle for economic nationalism among Democratic respondents.¹⁵ This is consistent with Hypothesis 4, but not Hypothesis 3: populism spillover effects appear to be primarily generated by issue bundling in public discourse, not by pre-existing tendencies toward binary moral cognition.

Discussion

This study has demonstrated that exposure to populist claims targeting political elites can activate respondents' ethno-nationalist sentiments, increasing their antipathy toward immigrants and ethnoracial minorities. For some target groups, like documented immigrants, this effect is limited to Republican respondents, but for most other target groups, there are no statistically

¹⁵ In the interest of a direct comparison with Study 1, we also analyzed the relationship between economic populism and ethno-nationalism using the data from Study 2. None of the mean differences in feeling thermometers toward minority groups were significantly different between the economic populist and control conditions.

significant differences by party. Indeed, with respect to one group in particular—African Americans—both Democratic and Republican respondents express greater antipathy following exposure to populist claims that vilify political elites.

To better understand whether the dog-whistle effect of populist discourse is a function of a pre-existing tendency among respondents to associate binary moral claims across domains or a result of the bundling of populism and nationalism in public discourse, we examined the relationship between economic populism and economic nationalism (i.e., the tendency to disparage countries associated with the offshoring of U.S. jobs). The primary effects of economic nationalism on antipathy toward other countries were modest for the sample as a whole, but Democratic respondents exposed to economic nationalist claims reported significantly lower favorability toward China, Mexico, and India; these associations were moderated by support for economic nationalism: for the 29.6 percent of respondents who opposed this type of discourse, the effect of economic nationalism was much weaker and failed to reach statistical significance.

Even when favorability toward economic nationalism is taken into account, however, we find no evidence of economic populism serving as a dog-whistle for economic nationalism among Democrats. Given that populism and nationalism were not explicitly linked by the 2016 presidential campaigns of Hillary Clinton or Bernie Sanders nor do such claims frequently co-occur in mainstream media, we conclude that it is the absence of issue bundling in public discourse that precludes Democratic respondents from exhibiting strong associations between these sentiments. This stands in contrast to the routine invocation of both anti-statist populism and ethno-nationalism by the Trump campaign and right-wing media, which appears to have effectively turned anti-elite claims into proxies for ethnoracial resentment.

The notion that attitudes must be connected by an authority, such as a political party, in order to exhibit constraint (that is, strong association) at the individual level can be traced back to the seminal work of Converse (1964), and its subsequent elaboration by Martin (2002). This approach argues that there is no inherent logical connection between most beliefs and that instead, beliefs become cognitively associated once people observe their routine co-occurrence in the discourse and practice of high-status individuals in their social groups—and all the more so when such associations are institutionalized by authoritative organizations. Once such patterns are sufficiently reinforced, departures from them are rare, because the decoupling of tightly coupled beliefs is likely to generate cognitive dissonance and be censured within ideologically homophilous social networks (Goldberg and Stein 2018; Martin 2002).

Our results are consistent with this perspective, but they suggest that constraint itself is insufficient for producing cross-domain spillover effects. Respondents in Study 1 tended to hold both politically populist and ethno-nationalist views, while a majority of Democratic respondents in Study 2 held both economic populist and economic nationalist views, but only among the respondents in Study 1 did populism activate nationalism. If it is the case that these divergent results are a consequence of the bundling of populism and nationalism by prominent Republican political and media personalities and the absence of such bundling by their Democratic counterparts, this would suggest that existing belief constraint must be accompanied by joint belief activation by political authority figures in order to generate cross-domain spillover. In other words, it is not sufficient that both beliefs are co-present in people's cognitive schemas—the mutual substitutability of the public frames corresponding to those beliefs must also be encoded in cultural scripts that are repeatedly performed by actors in positions of cultural authority.

In addition to this general theoretical insight, our study makes three contributions specific to research on radical politics. First, we show that the analytical distinction between populism and nationalism made in recent work (Bonikowski 2017; Mudde 2007; Rooduijn and Akkerman 2017) can be operationalized in empirical research, so that the distinct effects of each phenomenon and the relationship between them can be systematically evaluated. Isolating populism and nationalism in experimental vignettes allowed us to demonstrate the tight coupling between these ideational constructs in one sub-sample and their independence in another.

Second, our findings suggest that while exclusionary forms of populism may be concentrated in right-wing discourse (Judis 2016; Mudde and Kaltwasser 2013), their effects extend beyond their primary audience. Democrats in our study were not immune from the effects of populism targeting political elites or of economic nationalism: exposure to the former increased their anti-elite sentiments, while exposure to the latter increased their chauvinist attitudes toward other countries. Moreover, when primed with populism in Study 1, Democratic respondents expressed stronger out-group hostilities toward African Americans, even though ethno-nationalism is largely absent from Democratic political discourse.

Finally, the results in Study 1 suggest that the targets of the out-group hostility cued by populism aimed at political elites are not limited to the minority groups explicitly vilified by prominent political campaigns. Even though the Trump campaign focused its vitriol on immigrants and Muslims and portrayed African Americans in a paternalistic rather than overtly derogatory manner (Lamont, Park, and Ayala-Hurtado 2017), exposure to populist claims resulted in greater antipathy toward African Americans as well, among both Republican and Democratic respondents. While this may be a function of Trump's paternalism toward African Americans serving as a racial dog-whistle in its own right, it is also likely to stem from the broad

exclusionary scope of ethno-nationalism. Those who define the nation in ascriptive terms tend to endorse a wide range of restrictive criteria of legitimate national membership, including birth in the country, religion, ancestry, and race (Bonikowski and DiMaggio 2016). Once a link between populism and ethno-nationalism is established, it is likely that the nationalist schemas made salient in the process activate not only the out-group antipathies directly referenced in political speech, but also a range of other, related out-group sentiments.

Although our study is not without limitations—we rely on non-representative survey samples, for instance, and make strong assumptions about the comparability of mechanisms across experiments—it marks an important starting point for research on the ideational consequences of populist discourse. Rather than assuming populism and nationalism to be either conceptually coterminous or empirically inseparable, our paper demonstrates the possibility of precisely defining and operationalizing these constructs and rigorously studying the relationship between them. It is our hope that future research will build on this approach to better understand how populist discourse shapes and activates political attitudes, mobilizes political behavior, and interacts with partisan commitments. Such work can serve as an important complement—and perhaps even a corrective—to the large party-based literature on the causes of populist electoral success and the consequences of populist governance.

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