When the end is the beginning: the effect of criminal infractions and post-deportation outcomes on perceived fairness and blame attribution in deportation cases

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This paper is dedicated to the memory of Juan Manuel Sandoval Sandoval. He was deported, but like so many others, crossed again. He died in the Arizona desert in 2017. Rest in peace.

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Abstract

Deportation is a commonly used policy tool to address the issue of undocumented immigration. Associated with this is the prevalence of criminality narratives applied to undocumented migrants, tethering them to serious and violent criminal infractions. In reality, most deportations are based on the removal of undocumented immigrants who have no criminal record or are guilty of very minor legal infractions, such as driving with a broken taillight. Further, most deportation narratives ignore realities of post-deportation experiences, experiences that frequently put deportees in high precarity. In this paper, we ask two questions. First, does the kind of legal infraction prompting deportation impact judgment about deportation? Second, does information about what happens to a deportee after removal effect attitudes about deportation? Herein, we find that major infractions compared to minor infractions, in general, increases support for deportation as well as increasing levels of immigrant blaming. However, this increase can be counteracted if information about a negative outcome experienced by the deportee after removal—operationalized as the migrant’s death—is provided. Using two distinctly different groups of respondents, we show these results hold in both studies. Further, we show evidence that information about legal infractions and the migrant’s death are strongly moderated by party, but not by beliefs related to system justification, individualizing values, and ideology. Our results suggests that interventions in the narratives surrounding deportation may work to reduce support for such policies.

Keywords

deportation, undocumented immigration, criminality, partisanship
Introduction

Journalist Sarah Stillman tells the story of Laura S., an undocumented immigrant living in Texas in 2009. While driving home one night, she was pulled over, allegedly for driving between lanes on a Texas highway. When asked for proof of residence, she had none. Panicking, Laura S. pleaded with the police officer that she could not be returned to Mexico because she had a protection order against her ex-husband. Despite her pleas, the police officer called U.S. Border Patrol (hereafter USBP). After being detained, she was deported. A week later, her ex-husband strangled her, dousing her body in gasoline (Stillman 2018). The infraction of driving between lanes led to her deportation. Her deportation led to her death.

Deportation is a central feature of U.S. immigration policy. As Slack (2019) notes, it is among the most powerful expressions of state authority, giving it the power to determine who stays and who must go. Despite this, most Americans likely pay little attention to how deportation policies are implemented, are likely to be personally unaffected by deportation, and may be content with deportation policies because of the seemingly compelling logic that only those “who break the law” are removed. While for most Americans, deportation marks a final ending, for deportees, their families, and communities, this is just the beginning.

In this paper, we are interested in understanding how support for deportation and attributions of blame about responsibility for deportation are related to narratives about criminality and post-deportation outcomes. We demonstrate support for deportation and tendencies to blame immigrants are strongly related to legal infractions as well as informational cues about what happens to the deportee after removal. Using multiple survey experiments, we find, serious legal infractions compared to less serious infractions induce greater support for removal and higher rates of immigrant blaming; however, when post-deportation information is provided, support for removal and levels of immigrant blaming decrease. We also find attitudes about system justification, fairness and harm, as well as party affiliation and ideology impact judgment.

Deportation and criminality narratives

Between 1892 and 2019, the United States deported 8.7 million individuals. Of this, 6.1 million have been deported since 2001, amounting to 70 percent of the total (Department of Homeland
Security, 2019). Yet despite these staggering numbers, the apparatus of deportation policy is largely unseen. To the extent deportation policy attracts attention, it has mostly centered on DACA and Sanctuary City issues. But at least in the case of DACA, the narratives around deportation are highly specialized. As Sati (2020) notes, DACA recipients are often portrayed as the “good immigrants” juxtaposed with all the other “bad immigrants.” As such, Americans’ opposition to deporting the “good” immigrants tells us little about their opinion on removal of non-DACA recipients, a much larger and heterogenous group.

In contrast, issues surrounding Sanctuary Cities have also prompted discussions about deportation, discussions often driven by vivid narratives about acts of violence by undocumented migrants residing in them (Collingwood and Gonzalez O’Brien, 2019). This focus on extreme criminality is a common trope among anti-immigrant politicians. More generally, linking serious legal infractions committed by deportees to American victims is a calculated use of pathos designed to increase anxiety about immigrants, decrease empathy toward them, and generate support for the widespread use of deportation (Goodman, 2020). This sort of populist rhetoric pitting “us” versus “them” is what Hameleers, Box, and de Vreese (2017) refer to as “emotionalized blame attribution,” the rhetorical strategy of making it “easy” for individuals to blame the outgroup. Nonetheless, most deportees have committed no criminal offense outside of unauthorized entry or visa overstaying (Amuedo-Dorantes, Puttitanum, and Martinez-Donate, 2019). According to Transactional Records Access Clearinghouse (TRAC) data, 67% of deportations in FY 2020 were of migrants with no criminal record while fewer than 10% of deportees were guilty of violent infractions.

Even so, tethering deportees to violent criminality likely makes it easy for many to justify removal: “the immigrant was here illegally and murdered someone. Of course he should go.” The simplistic narrative resolves ambiguity making the removal of the “criminal alien” a justifiable end to an unpleasant chain of events. The blame for the migrant’s fate lies squarely on the deportee, not the system doing the removal. Problematically, the reality of deportation policy is far more complicated than the simple narrative that the “bad ones” go because they did “bad things here.” Deportation policies often do not return a deportee safely home (c.f. Goodman, 2020; Rozo, Anders, and Raphael, 2021; Slack, 2019). Apart from the fact that “home” may be fraught with violence, deportation policy by design makes post-deportation life as difficult as possible. For example, in 2011, the USBP implemented the Consequence Delivery System (CDS) which, as Slack (2019) points out
“involves a suite of programs representing unique punishment” for deportees (p. 25).

Moreover, deported migrants are often put into a state of even higher precarity after deportation, one frequently lethal (Buckinx and Filindra, 2015). Slack (2019) notes mass deportation in the Obama Administration coincided with expansion of the volatility of Mexico’s drug war, a combination increasing the chances a deportee would be murdered or disappeared. With respect to violence associated with deportation, Rozo et al (2021) found homicide rates in Mexican municipalities close to repatriation points were significantly higher than for municipalities less close. They suggest increases in violent crime could be due to victimization of deportees as well as deportees turning to criminal enterprises because of the lack of integration with the repatriation point.

Unfortunately, the consequences of U.S. deportation policy in terms of what happens to deportees after removal is largely hidden. The perpetual linkage of deportees to serious criminal offenses likely makes it “easy” to support removal. Further, given the simplistic narratives regarding deportation (“only the bad ones go”), when individuals assess blame for why deportations happen, they may more readily engage in dispositional blaming as versus situational blaming. In other words, the tendency to blame the deportee (dispositional) for what happens may be higher than for blame attributed to the U.S. immigration system (situational).

If rates of immigrant blaming are high, support for changing the system leading to mass removal may be low. Indeed, as Goodman (2020) argues, the justification for the use of mass deportation is dependent on a narrative the country is being “invaded” or “overrun.” In turn, these threatening narratives may create fear and anxiety about immigration, immigrants (c.f. Albertson and Gadarian, 2015; Brader, Valentino, and Suhay, 2008; Branton, Cassese, Jones, and Westerland, 2011; Ferris and Mohamed, 2013), and racial and ethnic minorities writ large (Craig and Richeson, 2014). Ascribing to such narratives may invoke a sense of powerlessness—our jobs are being taken”, “our country is being overrun”—such that mass removal is deemed necessary to preserve the status quo. Thus, powerlessness may motivate legitimization and defense of a system producing perceived stability (van der Toorn et al., 2015; Jost, 2020).

Individuals who may know little about deportation but are inculcated with threatening narratives are likely to see the basic features of deportation policies as inherently justifiable, thus leading to satisfaction with apparatuses leading to mass removal. Because the ubiquity of criminality narratives of immigrants coupled with the absence of information about post-deportation outcomes, it
becomes natural to consider two questions: 1) How does the severity of a legal infraction committed by an undocumented immigrant impact one’s judgment about deportation? and 2) How is this judgment impacted if one learns about negative post-deportation outcomes? These questions lead to two hypothesis that we refer to as the “infraction” and “information” hypotheses.

The infraction hypothesis addresses the criminality component to deportation, predicting that when an unauthorized migrant is deported because of a serious infraction, levels of support for removal as well as dispositional blame will be higher compared to a minor infraction. Given the linkage of deportees to serious crime coupled with the fact most deportations do not occur because of serious infractions, assessing how judgment varies by level-of-infraction is a natural first step.

The information hypothesis predicts that when individuals are given information about an adverse outcome experienced by a deportee after removal—operationalized below as the violent death of the migrant—levels of support for removal as well as dispositional blame will be lower compared to when this information is absent. Our expectation is this information humanizes the deportee, thus prompting individuals to consider the existential consequences of deportation policy leading to decreased support and victim blaming.

Further, learning of the deportee’s death may be tantamount to learning about a failure of the state’s deportation policy. In this sense, then, the expectations of the information hypothesis are somewhat akin to Malhotra and Kuo’s (2008) finding regarding governmental failure in the wake of Hurricane Katrina. They found that when given contextual cues regarding who was responsible when government actors failed, individuals gave unbiased attributions of blame (i.e. partisans overcame partisan bias and could accurately blame co-partisans for failed action). In our context, for those predisposed to dispositional blaming, the degree of immigrant-blaming may decrease when given the contextualizing information about the migrant’s death.

More generally, we think the informational cue fits within Weiner, Perry, and Magnusson’s (1988) conception of “onset-uncontrollable” stigma. The “stigma” associated with being an undocumented migrant deported because of a legal infraction is onset by the actions of the migrant; however, the death of the migrant is an outcome possibly uncontrollable by the migrant. In other words, the migrant’s actions led to removal, but the migrant’s death was out of the migrant’s control. Weiner, Perry, and Magnusson (1988) found that onset-uncontrollable stigmas were linked to feelings of “pity, liking, and no anger, as well as with behavioral judgments of help-giving” (p. 745).
somewhat similar vein, being informed about the migrant’s death after deportation may invoke the 
agent-harms-patient template (c.f. Gray and Wegner, 2009; Schein, Goranson, and Gray, 2015; Schein and Gray, 2015). That is, support for deportation policy as well as the degree of dispositional 
blaming may decrease when one associates the action of the state (the “agent”) with the death of 
the migrant (the “patient”).

Yet while our expectation is that contextualizing information about the migrant’s death is 
predicted to decrease support for removal and dispositional blaming, it may be the case this kind of 
information is irrelevant to judgments about deportation. For example, conservatives, high “system 
justifiers,” (Jost, 2020), and those who strongly endorse just world beliefs may see the migrant’s 
death as largely an irrelevant byproduct of the migrant’s actions. Support for the apparatus of 
deportation may override any concern about unfortunate outcomes, especially if the apparatus is 
perceived as maintaining the status quo and serving a protective role. The death of the migrant is 
an outcome brought on by the migrant’s transgressions against the system.

We return to these issues in the latter part of the paper. For now, our overriding concern, centers 
on how changes to the typical narrative of deportation impacts judgment about deportation. The 
two major outcomes we are interested in are support for deportation as well as blame attribution. 
With respect to support for deportation, this is an explicit statement about legitimacy of the state’s 
action. From our perspective, understanding how support or opposition varies across legal infractions 
is important given the context of deportation policy and the rhetoric surrounding it. In our studies, 
we are interested in the relative difference in how people support deportation policies when they 
are asked to consider a more likely type of legal infraction (i.e. a minor infraction) but a type of 
infraction less likely to arise in deportation narratives, compared to the less likely type of legal 
infraction (i.e. a major infraction) that is more likely to arise in deportation narratives.

The second outcome measure, blame attribution, is also important. Attributions of blame can 
establish the boundaries of the policy space in which debate and discussion is possible. For example, 
if one is prone to immigrant blaming, then it is unlikely any policies alleviating stressors on the 
undocumented or making it easier for the undocumented to get regularized legal status will be

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1Parenthetically, work in the domain of terror management theory has found that reminders of personal mortality 
salience seem to be related to increased victim blaming (Hirschberger, 2006). If reminders of the migrant’s mortality 
evoke thoughts of personal mortality (an issue we do not explore herein) then it possibly could be the case that 
individuals are more prone to blame the victim when they learn of the victim’s death.
possible. On the other hand, punitive and restrictive policies like fast-track removal or making Sanctuary Cities illegal are likely to be appealing to the dispositional blamer.

However, if one is less prone to blame the immigrant and more prone to blame the system producing deportation, then policies such as path-to-citizenship or guest worker programs may be appealing because they more readily address deficiencies in the system institutionalizing “illegality” and giving rise to mass deportation. On the other hand, a situational blamer is less likely to see punitive and restrictive policies as appealing. Understanding how criminal infractions and information about post-deportation experiences increase or decrease dispositional attributions of blame thus is a policy-relevant question and one our research design can address.

To summarize, our research expectations are displayed in Figure 1. The upper left panel shows the predicted direction of the infraction hypotheses, specifically suggesting that support for deportation as well as dispositional blaming are predicted to increase when one is exposed to information about a high-grade infraction compared to a low-grade infraction. The upper right panel shows the predicted direction of the information hypothesis implying support and dispositional blaming are predicted to decrease when one is exposed to information about the migrant’s death, as shown in the upper right panel.

Taken together, this leads to consideration of a $2 \times 2$ research design, shown in the lower right panel. Here, cell A denotes exposure to a low-grade infraction without any information about the migrant’s death; cell B denotes exposure to a high-grade infraction without any information about the migrant’s death; cell C denotes exposure to a low-grade infraction with information about the migrant’s death; and cell D denotes exposure to a high-grade infraction with information about the migrant’s death. Given the predictions of the infraction and information hypotheses, hypotheses about four contrasts can be made, as shown illustrated in the lower right panel of Figure 1.

Regarding the infraction hypothesis, exposure to information about a high-grade infraction (cells B and D) will result in greater support and dispositional blaming compared to exposure to information about a low grade infraction (cells A and B). Symbolically, then, $B - A > 0$ and $D - C > 0$. Regarding the information hypothesis, exposure to information about the migrant’s death (cells C and D) will result in reduced levels of support and dispositional blame compared to non-exposure (cells A and B). This implies $C - A < 0$ and $D - B < 0$. This logic leads to the conclusion that maximal support and dispositional blaming should be in the high-infraction/no death information
Figure 1: This figure shows the predicted direction of the relationship of support for removal and dispositional blaming as a function of the migrant’s infraction and whether or not information about the migrant’s death was given.

cell (B) and the lowest levels being in the low-infraction/death information cell (C). When it comes
to deportation narratives, the most common portrayal of deportees entails high criminality threat
absent any contextualizing information. In short, in the “real world,” most people most of the time
are “in” cell B. We now turn to our research design. Below, we make use of the lettering convention
in Figure 1 to facilitate interpretation.

Overview of studies

Our analysis is based on four survey experiments conducted in 2021. Study 1 was administered to
undergraduates in the Political Science subject pool at a large public university. Studies 2a and
2b were a respondent-driven survey initially distributed by college students enrolled in a Political
Science class. Studies 3a and 3b were a combination of a respondent driven survey as well as use of a
college student subject pool. Study 4 used an online panel, partnering with CloudResearch (Chandler
et al., 2019). Thus, studies 1-3 differed from study 4 in terms of research participant composition.
Study 4 is an older, more conservative, and more geographically diverse sample compared to the studies 1-3. We demonstrate both the infraction and information hypotheses are supported in all studies, but that outcomes in study 4 show higher rates of support for removal and greater levels of dispositional blame. Since all of these studies had considerable overlap, we describe the basic features of them.

In the studies, we assess how severity of the infraction and information about the immigrant’s fate impacts judgement with respect to: 1) supporting the decision to deport the immigrant and 2) willingness to blame the immigrant for what happened. In all of the studies, research participants learn of a fictitious migrant named Miguel Lopez (or Maria Lopez in study 2a), a citizen of El Salvador, who unlawfully crossed the border into the U.S. In the vignettes, research participants learn Lopez was involved in either a high-level infraction, defined as being involved in a drunk driving accident leaving another motorist injured, or a low-level infraction, defined as driving with a broken taillight (studies 1, 2a, 2b, 3b, and 4) or being caught up in a workplace raid (study 3a only).

In each of the studies, research participants were then randomly assigned to either receive information about what happened to Lopez after deportation, or receive no information. The specific information participants received is that after deportation, Lopez was found murdered in El Salvador, a victim of gang violence. Thus, the core feature of each of the studies follows a $2 \times 2$ design where we manipulate severity of the legal infraction (low v. high) and post-deportation information (informed of Lopez’ death or not). Studies 3a, 3b and 4 differ from 1, 2a, and 2b because of inclusion of a true control condition (where no information about the legal infraction or Lopez’ death is given).[2]

We selected the infractions of drunk driving, driving with a broken taillight, and getting caught in an immigration raid for several reasons. With respect to the drunk driving cue, it is commonplace to associate deportees with severe criminal infractions that injure or kill Americans. This is especially true with respect to drunk driving incidents (Nowrasteh and Howard, 2021). Despite the fact there is no evidence that Americans residing in areas with large numbers of undocumented migrants are more likely to be victims of drunk driving incidents, elite rhetoric connecting the undocumented to drunk driving incidents is prevalent (Nowrasteh and Howard, 2021).

[2]In the online appendix, the full text of each vignette is given.
With respect to the low-grade infractions of driving with a broken taillight or getting caught in a workplace raid, we selected these infractions for two reasons. First, most deportations are of individuals who have engaged in no criminal behavior. Neither of these infractions constitute a criminal offense and both are a typical way undocumented are apprehended. Indeed, Clarissa Martinez of the National Council of La Raza, referred to the prevalence of deportations due to minor infractions as “broken taillight syndrome” (NPR 2010). Second, there are no obvious external victims of either of these two kinds of infractions: the narrative an American has been victimized does not apply.

After exposure, all respondents were asked a series of questions regarding support for Lopez’ removal, how much Lopez deserved what happened, and perceived fairness in how Lopez was treated. The support-for-removal question was a seven-point Likert-type item worded as “Do you support or oppose the decision to deport Miguel Lopez?” with response options of 1=strongly oppose to 7=strongly support. Perceived deservedness of the decision to deport Lopez was assessed with the item “Because Miguel Lopez was in the country unlawfully, he got what he deserved” with 1=strongly disagree and 7=strongly agree. Fairness was assessed with the question “The U.S. immigration system treated Miguel Lopez fairly” with 1=strongly disagree and 7=strongly agree. The deservedness and fairness items were then combined to form a two-item, 14-point scale we refer to below as “Justness.” For studies 1-3, Cronbach’s $\alpha$ was 0.80; for study 4, $\alpha = 0.78$, indicating acceptable levels of reliability.

Following this, participants in studies 1-3 were asked to ascribe blame for Lopez’ deportation. Blame attribution was measured in two ways. First, respondents selected which was most to blame for what happened to Lopez. In studies 1-3, these options were: Lopez; U.S. immigration system; or El Salvador’s government. In study 4, research participants selected between Lopez or the U.S. immigration system. After this, research participants were then asked to allocate the amount of blame for Lopez, the U.S. immigration system, and El Salvador’s government (in study 4, blame allocation was split between Lopez and the U.S. immigration system). The total amount of allocated blame summed to 100, so if someone fully blamed Lopez, the score would be “100.”

Following this, research participants were asked to offer open-ended responses to two questions.

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3In study 4, we were interested in the pairwise judgment between the American immigration system and Lopez, thus juxtaposing the system doing the removal with the individual subjected to the system.
The first asked respondents to explain why they supported or opposed Lopez’ removal; the second asked to explain their choice of what was most to blame for Lopez’ removal. These open-ended responses are referenced below but systematic analysis of these data will be done in a separate paper. Prior to exposure to the experimental conditions, research participants in all studies were asked about their party affiliation, racial identification, ideological position (i.e. liberal to conservative), as well as a series of questions measuring system justifying beliefs and individualizing values. These measures are also discussed below.

Analysis plan

To simplify our presentation, we combine the survey experiments in the following way. Separate sets of analyses were done for studies 1, 2a/b, 3a/b, and 4. Studies 1-3 produced virtually identical results. Across all outcome measures, estimated treatment effects exhibited only slight variation among the studies. This provided justification for combining these studies. Regarding the gender condition (Maria v. Miguel Lopez; studies 2a and 2b) and the low-infraction condition (immigration raid v. broken taillight; studies 3a and 3b), we found no appreciable gender differences nor any differences across low-infraction conditions. As such, combining (or pooling) studies 1, 2a/b, and 3a/b was justified on statistical grounds. We did however, find that study 4 differs from the other studies.

For presentational purposes, we refer to the combination of studies 1, 2a/b, and 3a/b as “Study A” and study 4 as “Study B.” The total number of research participants who were exposed to experimental conditions in Study A was 1,710. Of these, 65% identified as female, 33% identified as male, and 2% identified as other. Regarding racial identification, 20% identified as Asian, 4% identified as black/African American, 35% identified as Latina/o, and 33% identified as being white. With respect to party affiliation, 79% identified as being a Democrat (or independent leaning Democrat) and 10% identified as Republican (or leaning Republican). In Study B, the total number of respondents exposed to experimental conditions was 1,200. Of these, 55% identified as female, 44% identified as male, and 0.25% identified as other. In terms of race, 4.6% identified as Asian, 13% identified as black/African American, 6% identified as Latina/o, and 74% identified as white. In the online appendix, all analysis conducted separately by study is presented along with a statistical test showing no study differences in treatment effects. In the supplemental appendix, we address how survey “speeding” was handled.
as white. Regarding partisanship, 44% identified as Democrat and 56% identified as Republican. Study B differed markedly from Study A: it has a much whiter, more Republican, and more male composition.

The effect of infraction and post-deportation information

To assess the infraction and information hypotheses, we regressed the outcome measures on a factor-level variable denoting experimental condition. These conditions corresponded to: 1) broken taillight/immigration raid with no information about Lopez’ death (labelled “Low/none” in the figures below); 2) drunk driving with no information about Lopez’ death (labelled “High/none”); 3) broken taillight/immigration raid with information provided about Lopez’ death (labelled “Low/death”); and 4) drunk driving with information provided about Lopez’ death (labelled “High/death”). In terms of our research expectations, “Low/none” corresponds to cell A in Figure 1, “High/none” corresponds to cell B, “Low/death” corresponds to cell C, and “High/death” corresponds to cell D. Those receiving no information about the legal infraction and no death information constitute the control condition.

The four outcome measures were: 1) support for Lopez’ deportation; 2) the scaled measure of the justness of Lopez’ removal; 3) the degree to which research participants viewed Lopez as most to blame for his removal; and 4) the amount of blame (on a 100-point scale) allocated to Lopez. Each outcome measure was rescaled to lie between 0 and 1 (for the “Lopez most to blame” item, the measure is binary, 1=Lopez was most to blame; 0=not). High scores on the outcome measures imply one is supportive of Lopez’ removal, sees the removal as just, and attributes a higher share of blame to Lopez.

To test the hypotheses, we estimated a regression model of the form

\[
\hat{O} = \beta_0 + \beta_1 \text{Low/none} + \beta_2 \text{High/none} + \beta_3 \text{Low/death} + \beta_4 \text{High/death} + \delta_0 \text{StudyB} + \delta_1 \text{Low/none} \times \text{StudyB} + \delta_2 \text{High/none} \times \text{StudyB} + \delta_3 \text{Low/death} \times \text{StudyB} + \delta_4 \text{High/death} \times \text{StudyB},
\]

(1)

where \(\hat{O}\) corresponds to the outcome measures discussed above. Here, \(\beta_0\) gives the baseline (control condition) estimates for Study A, the \(\beta_k\) give the exposure effects for the for experimental conditions.
for Study A, $\delta_0$ gives the offset in $\hat{O}$ for Study B, and $\delta_1 - \delta_4$ give the difference in exposure effects for Study B compared to Study A (i.e. the interactions of Study B with experimental conditions). Estimation, then, of the $\delta_k$ allow us to fully account for differences between Studies A and B.

The regression estimates and standard errors from this model are reported in Table 1. Two main points emerge. First, Study B respondents score on average, significantly higher on each outcome measure compared to Study A respondents (i.e. $\delta_0$ shows a significant increase in baseline estimates). This is not surprising: Study B intentionally oversampled Republican identifiers who (as we show later) are also much more likely to approve of deportation and more likely to attribute blame to Lopez. Second, however, the basic pattern of results generally hold for both Study A and Study B research participants and are in the predicted direction. This point is made clearer visually.

Table 1: Regression estimates of exposure effects for each outcome measure

<table>
<thead>
<tr>
<th>Outcome variables</th>
<th>Support for removal</th>
<th>Justness of removal</th>
<th>Most to blame</th>
<th>Amount of blame</th>
</tr>
</thead>
<tbody>
<tr>
<td>Study A ($\beta_0$)</td>
<td>0.392 (0.027)</td>
<td>0.384 (0.024)</td>
<td>0.143 (0.036)</td>
<td>0.207 (0.024)</td>
</tr>
<tr>
<td>Low/none ($\beta_1$)</td>
<td>-0.096 (0.031)</td>
<td>-0.083 (0.027)</td>
<td>0.031 (0.042)</td>
<td>0.005 (0.028)</td>
</tr>
<tr>
<td>High/none ($\beta_2$)</td>
<td>0.165 (0.031)</td>
<td>0.024 (0.028)</td>
<td>0.391 (0.043)</td>
<td>0.263 (0.028)</td>
</tr>
<tr>
<td>Low/death ($\beta_3$)</td>
<td>-0.210 (0.031)</td>
<td>-0.200 (0.028)</td>
<td>-0.048 (0.043)</td>
<td>-0.061 (0.028)</td>
</tr>
<tr>
<td>High/death ($\beta_4$)</td>
<td>-0.068 (0.031)</td>
<td>-0.121 (0.028)</td>
<td>0.112 (0.043)</td>
<td>0.091 (0.028)</td>
</tr>
<tr>
<td>Study B ($\delta_0$)</td>
<td>0.269 (0.034)</td>
<td>0.270 (0.030)</td>
<td>0.545 (0.046)</td>
<td>0.473 (0.030)</td>
</tr>
<tr>
<td>Low/none×Study B ($\delta_1$)</td>
<td>0.085 (0.043)</td>
<td>0.084 (0.038)</td>
<td>-0.066 (0.058)</td>
<td>-0.099 (0.038)</td>
</tr>
<tr>
<td>High/none×Study B ($\delta_2$)</td>
<td>-0.059 (0.043)</td>
<td>0.055 (0.038)</td>
<td>-0.317 (0.058)</td>
<td>-0.188 (0.038)</td>
</tr>
<tr>
<td>Low/death×Study B ($\delta_3$)</td>
<td>0.050 (0.043)</td>
<td>0.030 (0.038)</td>
<td>-0.045 (0.058)</td>
<td>-0.048 (0.038)</td>
</tr>
<tr>
<td>High/death×Study B ($\delta_4$)</td>
<td>0.003 (0.043)</td>
<td>0.021 (0.038)</td>
<td>-0.113 (0.058)</td>
<td>-0.142 (0.038)</td>
</tr>
</tbody>
</table>

Observations | 2,901 | 2,899 | 2,879 | 2,890
Adjusted R$^2$ | 0.250 | 0.286 | 0.243 | 0.362

Note: Table entries are regression estimates with standard errors in parentheses. The $\beta$ and $\delta$ correspond to the parameters from equation 1.

Briefly, consider Figure 2. Here, we generated fitted values, $\hat{O}$, and plotted them along with the 95% confidence intervals around this prediction. The estimated effects reported here are quite similar to the predictions made in Figure 1. Under the infraction hypothesis, support, perceived justness, and dispositional blame should increase for those exposed to a high-grade infraction compared to those exposed to a low-grade infraction (i.e. $B > A$ and $D > C$ from the figure). The differences in fitted values between the “Low/none” and “High/none” and the “Low/death” v. “High/death” conditions, as shown in Figure 2, are consistent with this expectation, although generally the differences are greater for Study A respondents compared to Study B. Now consider the information hypothesis.
Evidence in *support* of the information hypothesis would suggest the predicted outcomes should *decrease* when information about Lopez’ death is provided, thus implying (from Figure 1) that $C < A$ and $D < B$). Again, the results, at least with respect to the fitted values, are consistent with our expectations. However, we’re less interested in fitted values than in the *differences* in effects within each relevant contrast. As Greenland et al. (2016) note, overlapping *prediction intervals* are not prima facia evidence for a null effect.

Figure 2: This figure gives the predicted $\hat{O}$ for the four outcome measures for Study A (orange line) and Study B (blue line). Numerical entries denote the 95% prediction interval for each experimental condition.

A more efficient way to visualize the results are to examine the differences in marginal effects within each of the relevant contrasts, including comparisons to the control condition. To do this, we looked at the contrasts (from the lower right panel of Figure 1) of $B \text{ v. } A$, $D \text{ v. } C$, $C \text{ v. } A$, and $D \text{ v. } B$, as well as $A, B, C, \text{ & } D \text{ vs. }$ the control condition. These differences along with the 95%
Confidence intervals for the differences, are reported in Figure 3.

**Figure 3:** This figure gives the confidence intervals for differences in estimates for a given contrast. $\Delta y$ corresponds to the change in the outcome measure in original units for the outcome measure. For example, a “1.5” implies a 1.5 point difference between experimental conditions. The percentage given corresponds to the percentage change on the scale for each outcome. For example, “20%” implies there is a 20% increase in $y$ within the contrast. The $p$-value corresponds to the two-tail $p$ for the difference in experimental conditions. Entries in bold type imply statistically significant results ($p < 0.05$).
Study A differences are shown in the left column and Study B differences are in the right column. Regarding the infraction hypothesis, the relevant information is shown in the top two rows of the figure which respectively give the “Low/none” and “High/none” contrast (row 1; i.e. $B \text{ v. } A$) and the “Low/death” and “High/death” contrast (row 2; i.e. $D \text{ v. } C$). Support for the infraction hypothesis would imply the difference in marginal effects should be positive (i.e. $B - A > 0$ and $D - C > 0$).

To facilitate interpretation of the figure, consider the top row, which shows the confidence interval for the difference between the “High/none” and “Low/none” conditions for the support of Lopez’ removal measure among Study A respondents. The point estimate corresponds to the difference in marginal effects between these conditions and is about 0.26 (from Table 1, this is obtained as $[\beta_0 + \beta_1] - [\beta_0 + \beta_2]$) with a standard error for the difference of 0.02. The number “1.6” corresponds to the point increase in the support for Lopez’ removal measure between conditions, rescaled into original units on the seven-point scale. The percentage of “23%” corresponds to the percent increase on the scale (i.e. $\frac{1.6}{7} \times 100$) that the point increase corresponds to. The $p$-value gives the level-of-significance for the difference in contrasts. For this contrast, the 95% confidence interval does not contain 0 and so we would conclude the high infraction condition (with no death information) is associated with a significant increase in support for Lopez’ removal relative to those exposed the low infraction condition (with no death information), a result consistent with the infraction hypothesis.

For both Studies A and B, the differences in marginal effects between the “High/none” v. “Low/none” and “High/death” v. “Low/death” contrasts for each of the outcome measures provide evidence for the infraction hypothesis. With or without information regarding Lopez’ death, predicted levels of support, perceived justness, and, especially, dispositional blame are significantly higher in the drunk driving condition. Substantively, this suggests that the rhetorical strategy of linking deportees to serious legal infractions—a strategy commonly used—induces greater support for removal of migrants and the willingness to blame migrants for their removal. In other words, linkage of deportation to the oft-used criminality narrative seems to legitimize deportation, despite the fact that in reality, most deportations are based on minor (or no) legal infractions [Amuedo-Dorantes, Puttitanum, and Martinez-Donate 2019].

In general, the estimated effects are larger for Study A than for Study B. Study B consists of a much whiter, more male, more Republican and more conservative sample, making this sample, in our view, a “hard case” for our hypothesis. Attitudes among many of these respondents exhibit
very high rates support, perceived justness, and dispositional blaming; showing any movement due to experimental treatments, we think, is an interesting result and one we return to in detail later.

Thus there seems to be evidence the framing of a migrant’s infraction impacts judgment. However, a natural follow-up question to ask is ‘how is judgment impacted when one learns about what happens to Lopez after removal?’. This is the basis of the information hypothesis. As noted, we argue that if one learns Lopez was a victim of murder, support for Lopez’ removal will decrease, as this information better contextualizes possible post-deportation outcomes. To assess the information hypothesis, the relevant rows in Figure 3 are rows 3 and 4. The third row gives the difference in marginal effects between the “Low/death” v. “Low/none” (C v. A) conditions and the fourth row gives these differences for the “High/death” v. “High/none” (D v. B). If information about Lopez’ death decreases support, perceived justness, and dispositional blame, then the difference in marginal effects should be negative (i.e. to the left of 0 implying from Figure 1 that $C - A < 0$ and $D - B < 0$).

Again, for both Studies A and B, we find generally strong support for the information hypothesis. Compared to experimental conditions where information of Lopez’ death is not provided, when it is given, support, perceived justness, and dispositional blaming is reduced. Indeed, even in the context of a serious legal infraction–drunk driving that left a motorist injured–information about how Lopez’ removal led to Lopez’ death serves to decrease support, perceived justness, and dispositional blaming relative to individuals who learn of the criminal offense but do not learn of Lopez’ death. Contextualizing deportation, at least in the context of providing information about the death of the migrant, seems to temper beliefs about the legitimacy of deportation.

But how do the informational cues of Lopez’ infraction and Lopez’ death impact judgment relative to the control condition? These contrasts are shown in rows 5-8 in Figure 3. Several remarks can be made, starting with Study B respondents. It is clear there are no differences in attitudes when Study B respondents are given information about a low-grade infraction with no death information (see right column, row 5) compared to the control condition. There is substantial overlap with 0 in all the confidence intervals. This suggests that at least for Study B participants, learning of a low grade infraction without the death cue does not impact attitudes beyond what we would observe when no information is given, a result explored more below. For Study A participants, there is evidence that provision of information about the low-grade infraction (without death information)
decreases support and perceived justness, but has no impact on blame attribution.

However, in general, across most of the outcome measures, there is evidence that if one learns about a high-grade infraction (with no death information given) there is a significant \textit{increase} in support, perceived justness, and dispositional blaming compared to the control condition. Conversely, when information about Lopez’ death is given, for either the low-infraction condition or the high-infraction condition, support, justness, and dispositional blame significantly decrease relative to research participants in the control condition. There are two exceptions to this. For Study A respondents who learn of Lopez’ death in the high-infraction condition, despite the fact that support, perceived justness, and the \textit{amount} of blame allocated to Lopez decreases relative to the control condition, beliefs that Lopez is \textit{most} to blame increase relative to control. For Study B participants in this same condition, there are no differences in who is most to blame for those exposed to information about Lopez’ death compared to control.

\textbf{Discussion}

There are two key substantive points. First, the rhetorical strategy of linking undocumented migrants to serious criminal infractions, unfortunately, “works.” We demonstrate support and perceived justness of removal as well as levels of dispositional blame significantly increase when a deportee is linked to a high-level criminal infraction. However, we also show that if the high criminality narrative is \textit{not} used, then support, perceived justness and dispositional blaming decreases.

Second, when information about negative outcomes experienced by the deportee is supplied, support for and perceived justness of removal significantly declines. We see this as an important finding: information about the harmful outcomes that can befall deportees is very \textit{rarely} offered (c.f. Slack 2019). As such, most Americans are not likely to ever have the need to think about these sorts of negative outcomes. When research participants in the two studies learn of these outcomes, however, support, perceived justness, and rates of dispositional blame tend to decrease.

With respect to the composition of the studies, some remarks are in order. As noted, Study A is far more Democratic, liberal, female, younger, and less white than Study B participants, who are more Republican, conservative, more male, much older, and much whiter. Given this, we think both groups provide a hard case for showing movement in attitudes. For Study A, demonstrating any \textit{decrease} in support, perceived justness, and dispositional blaming is a high hurdle because baseline
levels of support for deportation policy were already very low. For Study B, demonstrating any *increase* in support, perceived justness, and dispositional blaming is equally a high hurdle because baseline levels of support for deportation were already very high.\(^6\)

So despite the fact that most Americans likely know very little about the details of deportation, most Americans have strong opinions about immigration writ large, given the persistent saliency of the issue ([Hajnal and Rivera, 2014](#)). Moreover, given the close connection between immigration, cultural values and sociotropic concerns ([Hainmueller and Hopkins, 2014](#), [Solodoch, 2021](#)), immigration attitudes are likely to be strongly held, stable attitudes. Indeed, recent work by [Kustov, Laaker, and Reller (2021)](#) demonstrate the temporal stability of immigration attitudes not only in the U.S., but in other countries as well. Thus, given immigration attitudes may be strongly held, they may also be relatively immutable. From our perspective then, showing that attitudes about one feature of immigration–deportation–seem susceptible to movement would have some important implications, which we discuss below.

Given support for the infraction and information hypotheses in both studies, then, it is natural to consider how other dispositional factors may impact judgment about deportation. Earlier, we considered the possible roles of system justifying beliefs and beliefs about fairness and harm may play in affecting opinions about immigration policy. Moreover, given widespread evidence of ideological and partisan differences in attitudes about undocumented immigration (c.f. [Oskooii, Dreier, and Collingwood, 2018](#)), exploring the role of these factors on support and blame in the context of deportation seems important. We turn to this analysis next.

**Party affiliation, ideology, system justifying beliefs, and individualizing values**

The issue of undocumented immigration is highly polarized on partisan grounds. Regarding deportation, [Hammer and Kafura (2019)](#) found that 82% of Republicans but only 29% of Democrats thought deportation was an extremely or somewhat effective policy of addressing undocumented immigration. Ironically, with respect to punitive immigration policy—including the widespread use of mass deportation—Presidents from *both* political parties have ubiquitously used deportation as a policy tool (c.f. Goodman 2020; Slack 2019).

\(^6\)Related to compositional differences in each of the studies, in the supplemental appendix, we demonstrate that the results presented here hold for both white and nonwhite respondents as well as female and male respondents.
Nonetheless, the rhetoric used on the undocumented immigration issue substantially varies in tone by party, differences reflected in mass public opinion. In terms of our analysis, we suspect that support and blame attributions will vary between Republican and Democratic party identifiers: the former will be more supportive and more likely to engage in dispositional blaming than the latter. Bringing party affiliation into the analysis may help us to better understand variation in the information and infraction hypotheses, especially among Study B participants, which has a large number of Republican respondents.

Relatedly, we might expect that ideological variation among partisans may additionally impact judgment regarding Lopez’ removal. Given the close connection between conservatism and anti-immigrant attitudes, variation in ideological placement among partisans may impact the effect of the infraction and information cues. In the analysis to come, we incorporate ideology using a seven-point scale where a “1” denoted “very liberal” and “7” denoted “very conservative.” We rescaled this measure to range from 0 to 1. Underscoring differences in the two studies, for Study A participants, mean ideology was 0.27 (s.d.=0.25); for Study B, the mean was 0.60 (s.d.=0.31). Political ideology is also closely related to other potentially important factors. As discussed previously, receptivity to the infraction and information cues may be impacted by beliefs related to system justification and individualizing values.

Jost’s system justification theory (c.f. Jost, Banaji, and Nosek 2004; Jost 2020) we think, may help account for one’s support for deportation as well as degree of dispositional blaming. Under system justification theory, individuals have a strong need to defend the status quo as inherently legitimate. This need is in part driven by a desire to maintain certainty, order, safety, and security in the face of perceived challenges and threats to the system. Thus, if one sees the implementation of some kind of policy as inherently just and, importantly, necessary to maintain order and safety, then one may be motivated to support the policy, even if that policy leads to injustice, negative externalities, and even harm. High system justifiers may be unpersuaded by countervailing information about how deportation results in a migrant’s death. The negative outcome, from this perspective, emanates from the deportee’s behavior, not from the state. As such, blame attribution is very easy: bad things happen because of bad things the immigrant did. The system bears no blame and in fact, may be lauded for such outcomes. The predictions made by system justification theory thusly leads to an additional hypothesis we call the system justification hypothesis. Specifically, high sys-
tem justifiers (compared to low system justifiers) will be more likely to support deportation as well as engage in dispositional blaming invariant to infraction or to provision of post-deportation death information.

Anecdotally, there is some evidence respondents in our study thought along these lines. After respondents indicated their support or opposition to Lopez’ removal and gave attributions of blame, they were asked to provide an open-ended response giving the reason why they supported Lopez’ removal and blamed Lopez (or did not blame Lopez). Some responses underscore the arguments made above.

“Miguel broke our laws by entering unlawfully. He further broke a law by driving under the influence of alcohol. He has now broken two of our laws. He is NOT a law abiding citizen and should have been deported. He is a drain on our society by injuring other people. What happened to Miguel is his own doing.”

“I believe in actually following laws. It is illegal to enter the country and live here without citizenship. Why should people get a pass? Laws are made for a reason. It would be a danger to legal citizens if people could live here unchecked and benefit from our taxes.”

“I am sorry he died but he should have come in the right way.”

Implicit in these responses is the presumption that the removal of Lopez was because of Lopez’ actions. And as implicated in the third quote, despite Lopez’ death, the U.S. immigration system operated as “it should.” Indeed, quotes similar to these were frequent and we think underscores the coupling of system legitimacy to attributions of dispositional blame. In the analysis below, system justifying beliefs are measured using a seven-item battery of agree/disagree statements designed to measure general system justification (Kay and Jost 2003; Jost 2020). Responses were coded such that high scores denoted high system justifiers and low scores denoted low system justifiers. The scale was recoded to range between 0 and 1. For Study A, Chronbach’s $\alpha = 0.78$ ($M=0.32 \ [0.18]$). For Study B, $\alpha = 0.68$ ($M=0.52 \ [0.18]$). In the supplemental materials, the specific questions used for this scale are given.

7The original general system justification scale consisted of an eighth item (“In general, you find society to be fair.”). We did not include this item for three reasons: first, in survey pretests, this item had a very low correlation with the other items (thus reducing reliability); second, of the eight items, this one specifically references the individual (i.e. “you”); third, because the item fared poorly compared to the others and because we wanted to minimize survey length to improve overall response quality, we eliminated the question.
Yet apart from evoking judgments about the legitimacy of the political system, deportation surely taps into an additional set of considerations related to notions of fairness and harm. These considerations are sometimes collectively described as “individualizing” values (Graham, Haidt, and Nosek 2009) in the context of moral foundations theory. In the context of deportation, these considerations center on one’s judgment about how the deportee was treated and what (if any) harm came to the individual. In this sense, values related to fairness and harm, as Niemi and Young (2016) contend, fit well within the paradigm in moral psychology of the agent-harms-patient template (c.f. Gray and Wegner 2009, 2011; Schein, Goranson, and Gray 2015; Schein and Gray, 2015), a template we think is important to consider with respect to judgment about deportation. As Strupp-Levitsky et al. (2020) show, beliefs about fairness and harm are cognitively motivated from empathic concern and so serve as a contrast to system justifying beliefs, which are cognitively motivated from epistemic and existential needs (c.f. Jost 2020).

Those strongly endorsing conceptions of fairness and harm may see the actions of the state to be unjustifiable because in so-doing, harm is brought upon the individual. Clearly, the issue of deportation raises a number of considerations about how unauthorized migrants should be treated. Unlike high system justifiers, those strongly endorsing individualizing values may be influenced by information regarding the migrant’s death, as this is the ultimate form of harm brought about by the agent. This line of thinking leads to an additional hypothesis we refer to as the individualizing hypothesis. Specifically, those high in individualizing values are likely to be less supportive of deportation and less likely to engage in dispositional blaming compared to those who weigh these values as less important. As in the case of system justifiers, several respondents offered open-ended responses emblematic of the concepts of fairness and harm.

“He was in extreme danger. Yes he wasn’t smart to drink and drive and hurt someone else, which isn’t okay. But sending someone to their death is almost worse.”

“If it will cost him his death as he stated then they are meant to protect him from any harm regardless of whether he came lawfully or unlawfully.”

“The US deporting him was sending him to his death. Miguel Lopez has the right to life.”

To measure the individualizing values of fairness and harm, we used a subset of items from the Moral Foundations Questionnaire battery of questions (Graham et. al 2011). The specific questions
used are provided in the supplemental appendix. In Study A, the individualizing values scale was based on a series of six-items and in Study B, eight-items were used (i.e. two additional questions were added). The reliability as given by $\alpha$ was not large in either study. For Study A, $\alpha = 0.52$ ($M=0.74 \ [0.15]$). For Study B, $\alpha = 0.64$ ($M=0.68 \ [0.17]$).

The effect of system justifying beliefs, individualizing values, partisanship, and ideology

In the following analysis, we rely on data from Study B. Among other things, we are interested in the role partisanship plays in judgment. Only 10% of Study A participants identified as being Republican (including leaners; $n = 174$); however, in Study B, we put quotas on the CloudResearch panel to ensure it was 55% Republican and 45% Democrat (we did not include Independents). This produced a sample with 667 Republican identifiers (56%) and 533 Democrat identifiers (44%). Moreover, the reliability of the individualizing values scale was minimally acceptable in Study B, but unacceptably low in Study A.

To assess the role of the experimental conditions along with partisanship, ideology, individualizing values, and system justifying beliefs, we proceeded in the following way. We estimated a model including a factor-level variable denoting experimental condition, party affiliation (Republican=1 or Democrat=0), and the system justification, individualizing values and ideological placement scales. Initially, we included the following two-way and three-way interactions: $party \times condition$, $party \times scales$, $scales \times condition$, and $party \times scales \times condition$. For all outcome measures, we found no evidence of any significant three-way interactions nor did we find any evidence that the effect of the experimental conditions were moderated by any of the scale measures (i.e. the $scales \times condition$ interactions did not hold). However, we did find strong evidence that party affiliation moderated the exposure effects in the experiment and that party affiliation moderated the relationship between system justification, individualizing values, and ideology. Thus the reported

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8In the online appendix, we reproduce the analysis below including Study A research participants. The main conclusions presented below, apply to that study but there is considerably more error in predictions for Republican identifiers.
model is of the form

$$\hat{O} = \beta_0 + \beta_1 \text{Low/none} + \beta_2 \text{High/none} + \beta_3 \text{Low/death} + \beta_4 \text{High/death}$$

$$+ \delta_0 \text{Party} + \delta_1 \text{Low/none} \times \text{Party} + \delta_2 \text{High/none} \times \text{Party}$$

$$+ \delta_3 \text{Low/death} \times \text{Party} + \delta_4 \text{High/death} \times \text{Party}$$

$$+ \gamma_1 SJ + \gamma_2 IV + \gamma_3 I + \gamma_4 SJ \times \text{Party} + \gamma_5 IV \times \text{Party} + \gamma_6 I \times \text{Party},$$

(2)

where $SJ$ denotes the system justifying beliefs scale, $IV$ denotes the individualizing values scale, and $I$ denotes ideological placement. Thus, $\beta_{1-4}$ give the treatment effects for Democrats relative to the control condition ($\beta_0$), $\delta_0$ gives the offset in the control conditions between Republicans and Democrats, $\delta_{1-4}$ give the treatment effects for Republicans relative to the Republican control condition ($\beta_0 + \delta_0$), and $\gamma_{1-6}$ give the estimates for the values scales and the interaction of them by party.

The full set of estimates for each outcome are presented in Table 2. However, given the models have a number of two-way interactions, simply inspecting coefficients in a table is not illuminating. To interpret the results, we rely on several plots. Of most interest to us is the extent to which partisanship interacts with the infraction and informational cues. Recall that in the previous analysis, Study B research participants were much older, whiter, and Republican compared to the Study A participants. Given the composition of Study B, then, we have the opportunity to see how ample numbers of individuals from both parties are impacted (or not) by the experimental treatments.

We start with Figure 4 which shows the prediction intervals for the $\hat{O}$ by experimental condition and party affiliation. Three points emerge, which we elaborate on below. First, for both Republican and Democratic identifiers, the predicted direction of the infraction cue is in the correct direction for the outcome measures related to support for and perceived justness of Lopez’ removal (i.e. from Figure 1, $B > A$ and $D > C$). That is, when given information Lopez was involved in a high-grade infraction, support and perceived justness increases.

Second, regarding the information hypothesis, for these same outcome measures (support and justness), both Republican and Democrats respond in the predicted direction when given information about Lopez’ death (i.e. from Figure 1, $C < A$ and $D < B$). That is, when one learns of Lopez’ death, in either the high-grade or low-grade infraction, support and perceived justness decreases.
Table 2: Outcome measures as a function of experimental conditions, party, system justification and individualizing values

<table>
<thead>
<tr>
<th>Outcome measures</th>
<th>Support for removal</th>
<th>Justness of removal</th>
<th>Most to blame</th>
<th>Amount of blame</th>
</tr>
</thead>
<tbody>
<tr>
<td>Democrat (β₀)</td>
<td>0.554 (0.082)</td>
<td>0.425 (0.072)</td>
<td>0.366 (0.123)</td>
<td>0.488 (0.085)</td>
</tr>
<tr>
<td>Low/none (β₁)</td>
<td>0.012 (0.040)</td>
<td>0.027 (0.035)</td>
<td>0.050 (0.060)</td>
<td>0.027 (0.042)</td>
</tr>
<tr>
<td>High/none (β₂)</td>
<td>0.204 (0.041)</td>
<td>0.153 (0.036)</td>
<td>0.330 (0.061)</td>
<td>0.192 (0.042)</td>
</tr>
<tr>
<td>Low/death (β₃)</td>
<td>−0.111 (0.040)</td>
<td>−0.090 (0.035)</td>
<td>−0.001 (0.059)</td>
<td>−0.104 (0.041)</td>
</tr>
<tr>
<td>High/death (β₄)</td>
<td>−0.051 (0.039)</td>
<td>−0.074 (0.035)</td>
<td>0.090 (0.059)</td>
<td>−0.042 (0.040)</td>
</tr>
<tr>
<td>Republican (δ₀)</td>
<td>0.098 (0.113)</td>
<td>0.182 (0.099)</td>
<td>0.237 (0.169)</td>
<td>0.098 (0.117)</td>
</tr>
<tr>
<td>Party ×Low/none (δ₁)</td>
<td>−0.046 (0.054)</td>
<td>−0.051 (0.047)</td>
<td>−0.165 (0.080)</td>
<td>−0.052 (0.055)</td>
</tr>
<tr>
<td>Party ×High/none (δ₂)</td>
<td>−0.187 (0.053)</td>
<td>−0.150 (0.047)</td>
<td>−0.462 (0.080)</td>
<td>−0.217 (0.055)</td>
</tr>
<tr>
<td>Party ×Low/death (δ₃)</td>
<td>−0.093 (0.053)</td>
<td>−0.150 (0.047)</td>
<td>−0.159 (0.080)</td>
<td>−0.006 (0.055)</td>
</tr>
<tr>
<td>Party ×High/death (δ₄)</td>
<td>−0.016 (0.054)</td>
<td>−0.043 (0.047)</td>
<td>−0.151 (0.080)</td>
<td>0.002 (0.055)</td>
</tr>
<tr>
<td>System justification (γ₁)</td>
<td>0.342 (0.068)</td>
<td>0.402 (0.059)</td>
<td>0.438 (0.101)</td>
<td>0.303 (0.070)</td>
</tr>
<tr>
<td>Fairness &amp; harm (γ₂)</td>
<td>−0.369 (0.085)</td>
<td>−0.284 (0.075)</td>
<td>−0.249 (0.127)</td>
<td>−0.178 (0.088)</td>
</tr>
<tr>
<td>Ideology (γ₃)</td>
<td>0.096 (0.049)</td>
<td>0.165 (0.043)</td>
<td>0.096 (0.072)</td>
<td>0.032 (0.050)</td>
</tr>
<tr>
<td>Party ×SJ (γ₄)</td>
<td>−0.379 (0.098)</td>
<td>−0.378 (0.086)</td>
<td>−0.550 (0.146)</td>
<td>−0.448 (0.101)</td>
</tr>
<tr>
<td>Party ×FH (γ₅)</td>
<td>0.330 (0.111)</td>
<td>0.240 (0.097)</td>
<td>0.357 (0.165)</td>
<td>0.225 (0.114)</td>
</tr>
<tr>
<td>Party ×I (γ₆)</td>
<td>0.166 (0.072)</td>
<td>0.115 (0.063)</td>
<td>0.263 (0.107)</td>
<td>0.322 (0.074)</td>
</tr>
<tr>
<td>Observations</td>
<td>1,195</td>
<td>1,195</td>
<td>1,190</td>
<td>1,191</td>
</tr>
<tr>
<td>Adjusted R²</td>
<td>0.275</td>
<td>0.349</td>
<td>0.158</td>
<td>0.221</td>
</tr>
</tbody>
</table>

Table entries are regression estimates with standard errors in parentheses. The β and δ correspond to the parameters from equation 1.

Third, there are substantial differences due to party in the tendency to engage in Lopez’ blaming. Republican identifiers, simply put, are high Lopez blamers, invariant to infraction or information cues. This is shown in the bottom two panels of Figure 4 where, with few exceptions, there is substantial overlap in the prediction intervals for both of the blame measures. Thus, while there is evidence that Republican identifiers’ support for and perceived justness of Lopez’ removal is directionally related to the infraction and information cues, the tendency to blame the migrant is unaffected by this information.

However, to better visualize the relationships between the outcome measures, party, and experimental conditions, we report the differences in the marginal effects for each of the relevant contrasts, results shown in Figure 5. Since our hypotheses are explicitly tied to specific contrast comparisons, Figure 5 more clearly shows these differences and may be interpreted identically as Figure 3, the only difference being that results are shown by party here. In Figure 5, rows 1 and 2 give information relevant to the infraction hypothesis; rows 3 and 4 give information relevant to the information hypothesis; and rows 5-8 given the contrasts between the infraction/information conditions and the control condition.
To elaborate, consider first the infraction hypothesis for Republican identifiers, denoted in the left panels in rows 1 and 2. With respect to support for Lopez’ removal as well as perceived justness of the removal (top two rows), there seems to be strong evidence for the infraction hypothesis, for the “Low/death” v. “High/death” contrast (row 2) but no strong evidence for the infraction hypothesis for the “Low/none” v. “High/none” contrast (row 1). In the latter contrast, support and perceived justness “move” in the predicted direction, but intervals for the pairwise comparison overlap 0. Thus, absent any information about Lopez’ murder, Republican support for and perceived justness of Lopez’ removal are very similar.

Indeed, provision of information about Lopez’ infraction without information about Lopez’ death yields predicted levels of support and perceived justness indistinguishable from the control condition.
**Figure 5:** This figure gives the confidence intervals for differences in estimates for a given contrast. ∆y corresponds to the change in the outcome measure in original units for the outcome measure. For example, a “1.5” implies a 1.5 point difference between experimental conditions. The percentage given corresponds to the percentage change on the scale for each outcome. For example, “20%” implies there is a 20% increase in y within the contrast. The p-value corresponds to the two-tail p for the difference in experimental conditions. Entries in bold type imply statistically significant results (p < 0.05).

(see the left column, rows 5 and 6, which give the contrasts with the control condition). Given Republican identifiers’ high baseline level of support for deportation in the general public (Hammer and Kafura, 2019), it seems it does not “much matter” what Lopez did to get deported: he broke the law and his deportation is highly approved of and is a fair and just outcome. Republican respondents seemed mostly unmoved by the type of infraction when information about Lopez’ death...
was not provided.

But when it is, Republican support and perceived justness *does show change*. As shown in the left column on row 2, for both measures, levels of support and perceived justness significantly *increase*. Or in other words, there is a significant infraction “gap” that only holds when Lopez’ death information is given. For Republican research participants who learn Lopez was deported for high grade infraction and was then subsequently murdered, predicted levels of support and perceived justness are significantly higher compared to Republican identifiers who only learn of Lopez’ death in the broken taillight infraction. Alternatively one could interpret this as levels of support and perceived justness are significantly lower in the “Low/death” v. “High/death” condition. In other words, information about Lopez’ legal infraction matter *but only* when information about his death is given.

With respect to Republicans and the information hypothesis, the relevant plots are shown in the left column on rows 3 and 4. Republican support of Lopez’ removal as well as perceived justness of the removal significantly decreases when information about Lopez’ death is given, a result consistent with the hypothesis and implying $C < A$ and $D < B$. Thus, it seems Republican identifiers respond to information about Lopez’ death, decreasing levels of support and perceived justness of his removal. This is, we think, an important result that we elaborate on below, but ultimately, we think we can demonstrate that for Republicans, support for removal and perceived justness of deportation is potentially moveable *if* the removal is contextualized with information about a negative post-deportation outcome.

However, regardless of this, Republican identifiers are, bluntly put, *high immigrant blamers*. For the two blame attribution measures, there is no strong evidence that the death cue impacts assessments of blame. One exception to this is the contrast of “Low/death” v. “Low/none” (row 3), where the amount of blame allocated to Lopez decreases by about 8 points (on the 100 point scale), a result significant at $p = 0.02$. But for the other blame attribution measures, there is substantial overlap with 0 in the confidence intervals. For the most part, then, invariant to why Lopez was deported or what happened to him after deportation, Republican respondents overwhelmingly blame Lopez the most and allocate the bulk of the blame to him.

With respect to Democrat identifiers, consider the infraction hypothesis shown in rows 1 and 2 in right column of Figure 5. Similar to Republican identifiers, we find support for the infraction
hypothesis, but unlike Republicans, this support is found only for the “Low/none” v. “High/none” contrasts (row 1). Thus, $B > A$ but $D \approx C$. Democratic identifiers are strongly impacted by information about Lopez’ legal infraction, but only if information about Lopez’ death is not provided. In this sense, Democrats are mirror images of Republicans, who only showed differences by infraction when Lopez’ death was reported. Support for Lopez’ removal as well as perceived justness of his removal substantially increases when Democrat respondents learn the infraction was a drunk driving incident leaving another motorist injured but do not learn anything about what happened to Lopez compared to Democrats in the low infraction (no death) condition.

Thus, juxtaposed to Republicans who, in the absence of information about Lopez’ death, indicated high approval of deportation invariant to Lopez’ legal infraction, changes in attitudes for Democrats seem strongly impacted by the infraction. This, we think, has important implications which we discuss below. With respect to the information hypothesis, we find strong evidence that for Democrats, information about Lopez’ death significantly lowers levels of support and perceived justness. Indeed, for Democrats (and again, exactly opposite of Republicans), the type legal infraction does not significantly impact judgement if information about Lopez’ death is provided (as denoted by the differences in the “Low/death” v. “High/death” contrast). In other words, it doesn’t matter what Lopez did to get deported if information about his death is given.

As such, among Democrats, we find strong support for the information hypothesis (as we did for Republicans) and qualified support for the infraction hypothesis: it holds only when information about Lopez’ death is not provided. Thus, for Democrats, support for removal and perceived justness of deportation is potentially moveable in both directions. If the removal is contextualized with information about a negative post-deportation outcome, support and perceived justness decrease. However, if only given information about a serious legal infraction, absent any contextualizing information, Democratic approval and beliefs about the justness of deportation increase. As noted earlier, it is precisely this kind of narrative that dominates deportation rhetoric. Relating this back to Figure 1, for Democrats “in” cell B, they show significant increases in their willingness to legitimize deportation.

A similar story emerges regarding Democrat attributions of blame. Predicted levels of Lopez blaming substantially increase when Democrats only learn Lopez was involved in a drunk driving incident. However, when information about Lopez’ death is provided, dispositional blaming signif-
icantly decreases to the point where, once again, the type of legal infraction makes no difference. Thus, for Democrats but not for Republicans, we can show that the amount of blame attributed to Lopez substantially decreases when information about his death is provided. Thus, regarding partisanship, informational cues “work” for both parties, but work in different ways, the implications of which we turn to below.

Lastly, we turn attention to our measures of system justifying beliefs, perception of fairness and harm (i.e. individualizing values), and ideology. As noted above, we found no evidence that these measures moderated the experimental treatments; however, we did find evidence the relationship between these items was moderated by party affiliation. In Figure 6, we plot the estimated relationship for each outcome measure as a function of system justification, individualizing values, and ideology. The way the plot is oriented, the \( y \)-intercept corresponds to the control condition in the experiment; however, since the scales do not moderate the treatments, the estimated relationship is isomorphic (i.e. offset only by differences in experimental treatments).

Several points emerge from Figure 6. First, variation in attitudes for either system justifying or individualizing values seem mostly unrelated to Republican opinion on any of the outcome measures. The estimated relationships, shown in the upper left and right panels of Figure 6 are essentially 0 when accounting for error in the predicted \( \hat{O} \). Second, among Democrats, there is a strong relationship between both system justifying beliefs and conceptions of fairness and harm in attitudes on the four outcome measures. Regarding system justification, there is a relatively strong and positive relationship between increased endorsement of system justifying beliefs and support for perceived justness of Lopez’ removal as well as increased levels of Lopez blaming. Indeed, for high Democratic system justifiers, support, perceived justness, and dispositional blame significantly increase, approaching levels observed among Republican respondents. With respect to individualizing values, shown in the upper right panel of Figure 6 we can show that for Democrats who strongly endorse individualizing values, support, perceived justness, and dispositional blame significantly decreases. Thus for Democrats, but not Republicans, our expectations regarding system justifying beliefs and individualizing values are consistent with the results.

The absence of any relationship between system justifying beliefs among Republicans was unexpected. However, there are some plausible reasons why this may be the case. First, the simple fact is that baseline levels of support for deportation policies as well as the tendency to blame
immigrants is extremely high to begin with and after accounting for the experimental treatments, there is little variation remaining to account for attitudes. Second, and more directly related to the system justification measure, definitions of exactly what the meaning of the “system” is might have considerable heterogeneity among Republicans. There is evidence to suggest this may be the case. [Azevedo, Jost, and Rothmund (2017)] present survey evidence from the 2016 Presidential election showing that system justifying beliefs could be multifaceted, remarking that “there is more than one sense in which someone may be said to accept or reject the societal status quo” (p.235). In their analysis, they found that while Republican Trump supporters were very likely to embrace capitalist
values and gendered division-of-labor, they were less likely to embrace general notions of system justification compared to non-Trump supporters.

We fielded Study B in the particularly fraught period of 2021 in the context where Trump support had become synonymous with Republican identification, information about the violence and destruction of the January 6 insurrection was rampant, and Republican support for the new Biden administration was in single digits. In the context of the measure of general system justification, heterogeneous definitions of what the “system” is may have contributed to the absence of a relationship among Republicans. It is worth noting that in Study A, although there were far fewer Republican identifiers, the exact same pattern of results reported in Figure 6 was found. It is equally worth noting that Study A Democratic respondents reproduced the same pattern of results found in Study B. The supplemental materials presents the full set of results.

And so if system justifying beliefs and individualizing values are unrelated to Republicans’ judgments about deportation, what is related? The lower left panel of Figure 6 shows the estimated relationship between ideological placement and attitudes for the four outcome measures. For Republicans, increased conservatism is strongly associated with high levels of support, perceived justness, and dispositional blaming. Interestingly, political conservatism has been found to be highly correlated with system justifying beliefs. Underscoring the argument made above regarding heterogenous definitions among Republicans of what “the system” is, in our studies, ideology was weakly correlated with system justification. For Study B Republicans, \( r = 0.11 \) (and in Study A, \( r = 0.08 \)). Thus, high levels of conservatism among Republicans seems fairly strongly associated with deportation attitudes even as system justifying beliefs (as measured by the general system justification scale) seem not to have much of a relationship (we think for reasons outlined above). Among Democratic respondents, the relationship between ideology and attitudes is weak.

Discussion

There are several takeaway points from this analysis, most of them dealing with partisan differences in receptivity to infraction and death information cues. For Republicans, the kind of infraction prompting removal makes no difference unless contextualizing information about Lopez is given. Thus, Republicans show receptivity to the death cue. For Democrats, the infraction only matters if information about Lopez’ death is not given. Consider the implications of this last point. Our
results suggests that Democratic identifiers are “moveable” toward increased endorsement of the legitimacy of deportation and the willingness to engage in immigrant blaming when presented with the oft-used criminality narrative discussed above. That is, the narrative that deportation is largely due to an undocumented immigrant’s involvement in a major criminal act. Absent any follow-up information—which in reality, is typically absent from deportation narratives—Democratic identifiers start to “look” like Republicans in their attitudes. In short, the typical narrative of deportation seems to, unfortunately, work very well in terms of inducing greater perceived legitimacy of deportation.

**General discussion**

One central takeaway point here is the extent to which individuals, especially Democratic identifiers, are readily impacted by what we have called the criminality narrative. For this group, absent contextualizing information regarding Lopez’ death, support, perceived justness, and immigrant blaming approaches levels observed among Republicans. Republicans, on the other hand, are largely unmoved by the infraction and as such, absent contextualizing information, they display extraordinarily high rates of support, perceived justness, and dispositional blaming.

From a normative perspective, we think this is a bad and dangerous outcome. Mass removals in the U.S. are almost exclusively driven by the deportation of individuals with no criminal record, or if so, minor infractions of the law. “Broken taillight” syndrome, as Clarissa Martinez of the National Council of La Raza, referred to it, is real. Equating the undocumented, and certainly the undocumented who are under orders-of-removal, with violent criminality distorts reality in ways that we show increases support and perceived justness of deportation as well as dispositional blaming of migrants, even among those ostensibly predisposed to have less punitive views about immigration (i.e. Democrats).

Contextualizing deportation in terms of what may befall a deportee, however, seems to counteract these tendencies. That respondents show some sensitivity to information about Lopez’ death in terms of decreasing support, perceived justness, and dispositional blaming, is normatively encouraging to us. This is particularly the case with respect to Republican identifiers who ostensibly are predisposed to have more punitive views about immigration. These results, we think, should lead us to consider interventions that help to change the narratives about deportation.
In these studies, we show that deportation attitudes are potentially moveable (in both directions). Importantly, we are not asserting here that we have fundamentally changed the minds of individuals about their deportation attitudes—in a cross-sectional design, that is impossible to tell—but what we can assert is that there is observable and significant evidence that opinions about deportation are receptive to contextualizing information regarding deportation outcomes. Also, we want to be clear that we’re not asserting, especially with respect to Republican identifiers, that exposure to information about Lopez’ death is “moving” them from pro- to anti-deportation stances. However, we can show that relative to the typically high levels of support for mass deportation evinced among Republicans, contextualizing information regarding the death of the migrant can reduce these levels. In turn, this gives us some hope that further thinking about ways to contextualize deportation is a useful enterprise.

One effort to do this is already underway. The plight of many deportees is so dangerous and punitive that in 2017, Robert Irwin began the Humanizando la Deportación (Humanizing Deportation) Project. Through recorded audio/video, Irwin and his colleagues have documented the stories of over 300 migrants (Irwin, 2020). In vivid detail, many of these stories convey the realities of post-deportation life, a life unseen by most Americans. Ultimately, further exploration into fundamentally changing deportation narratives is crucial.

However, changing narratives about deportation is not sufficient to forestall or prevent negative outcomes deportees may face after deportation. In other words, even if narratives change such that full-throttle endorsement of the legitimacy of deportation is reduced (normatively, a good thing), if mass removal continues to take place, then invariant to any sort of changed narrative, the lives of the deported, their families, and communities will still be irreparably harmed in ways outlined by Goodman (2020) and Slack (2019), among others. Thus, we agree with Buckinx and Filindra (2015)’s normative conclusion that the principal of what they call jus noci—or the right not to be harmed—must factor into the decisions made by government entities about the decision to remove individuals from the U.S. That is, extant conditions in repatriation points should be a part of the decision making process when it comes to deportation proceedings.

We are not optimistic about this actually happening, especially in light of the COVID-era use of the Title 42 policy and the persistent and relentless policy choices made by Department of Homeland Security to make the lives of deportees as difficult as possible (Slack, 2019). However, discussions
regarding the standard of *jus noci* need to be in the policy space. By extension, similar discussion invoking this principle needs to be had with respect to undocumented migrants residing *within* the U.S. For example, racist, cavalier “stunts” in 2022 where Governors Ron DeSantis (FL) and Greg Abbott (TX) transported migrants under false pretenses to locations in the country’s interior, underscore the issue that it is not just the deported who face potentially existential consequences of removal. Extraordinarily negative outcomes occurring to migrants can be found well within the U.S.’ borders. This is further underscored with the persistent and ceaseless usage of mass detention of undocumented migrants. These, of course, are issues far beyond the scope of this paper; however, they are relevant to the motivation of this paper.

**Conclusion**

This paper had two straightforward goals. First, does information about a legal infraction impact judgment about deportation? Second, does learning about a deportee’s death after deportation mitigate or reduce support for deportation and tendencies to engage in immigrant blaming? We find affirmative answers to both questions, the answers to which are highly dependent upon partisan identification. We acknowledge that much future work needs to be done. For example, we chose the outcome of the migrant’s death to constitute the contextualizing information. In part, we reasoned if we cannot show any movement when given information about the ultimate negative outcome—death—then it would not be likely we would find movement for *any* outcome. Of course, this may or may not be the case and future work should explore different sorts of outcome narratives apart from the migrant’s death. However, as far as we are aware, this is the first study to demonstrate how attitudes about deportation can be impacted by post-deportation narratives. Finding that such narratives can reduce support for deportation is an encouraging first step. However, equally important is the unfortunate result that the typical criminality narrative surrounding deportation works, and works well. Thus, as discussed above, interventions to counteract this narrative are needed.
References


Supplemental materials for When the end is the beginning: the effect of criminal infractions and post-deportation outcomes on perceived fairness and blame attribution in deportation cases

October 14, 2022
Overview

In these supplemental materials we do the following: Section 1 gives the question wording for each experimental condition; Section 2 shows how we handled survey speeding as well as demonstrating that inclusion of speeders has no appreciable impact on our conclusions; Section 3 provides justification for pooling studies 1, 2a/b, and 3. Additionally, Section 3 demonstrates that the gender manipulation as well as the alternative low-infraction condition (immigration raid) had no impact on our conclusions. Section 4, shows the results of the treatment effects accounting for race and for gender. In Section 5, we report the specific question wordings used for the system justification and individualizing values measures. In Section 6 we report the results of a fully pooled model including Study A respondents.

1 Vignette wording

In this section, we give the wording for each of the experimental treatments. In Study 2a, “Miguel Lopez” was replaced with “Maria Lopez.”

Text for the control condition:

“Miguel Lopez came to the U.S. from Apopa, El Salvador. He illegally entered the U.S. by crossing the U.S.-Mexico border. Not long after entering, immigration officials discovered he was in the country illegally. After several months in detention, Lopez was deported to El Salvador.”

Text for the “Low/none” condition referencing a broken tail light:

“Miguel Lopez came to the U.S. from Apopa, El Salvador. He illegally entered the U.S. by crossing the U.S.-Mexico border. Not long after entering, he was pulled over by the police while driving with a broken tail light. When Lopez was unable to provide identification, the police called immigration officials. After several months in detention, Lopez was deported to El Salvador.”

Text for the “Low/none” condition referencing an immigration raid:

“Miguel Lopez came to the U.S. from Apopa, El Salvador. He illegally entered the U.S. by crossing the U.S.-Mexico border. Not long after entering, he was arrested after the place where he worked was subject to an immigration raid. When Lopez was unable to provide identification, he was placed in detention. After several months in detention, Lopez was deported to El Salvador.”

Text for the “High/none” condition:

Miguel Lopez came from Apopa, El Salvador. He illegally entered the U.S. by crossing the U.S.-Mexico border. Not long after entering, he was apprehended by the police after being involved in a car crash that left another driver injured. It was later determined Lopez was driving while drunk. While in detention,
he was turned over to immigration officials. After several months in detention, Lopez was deported to El Salvador.

Text for the “Low/death” condition referencing a broken tail light (the text referencing Lopez’ death is identical for the immigration raid condition):

“Miguel Lopez came to the U.S. from Apopa, El Salvador. He illegally entered the U.S. by crossing the U.S.-Mexico border. Not long after entering, he was pulled over by the police while driving with a broken tail light. When Lopez was unable to provide identification, he was turned over to immigration officials. Lopez pleaded with immigration officials that deportation would result in his death because of threats made against him by gangs in El Salvador. Despite his pleas, Lopez was deported. A few days after his return, his body was found by Salvadoran police. It was determined he died by gunshot wounds. His death was attributed to gang violence.”

Text for the “High/death” condition:

“Miguel Lopez came from Apopa, El Salvador. He illegally entered the U.S. by crossing the U.S.-Mexico border. Not long after entering, he was apprehended by the police after being involved in a car crash that left another driver injured. It was later determined Lopez was driving while drunk. While in detention, he was turned over to immigration officials. Lopez pleaded with immigration officials that deportation would result in his death because of threats made against him by gangs in El Salvador. Despite his pleas, Lopez was deported. A few days after his return, his body was found by Salvadoran police. It was determined he died by gunshot wounds. His death was attributed to gang violence.”

2 Survey speeding

Because these studies were online survey experiments, attention was paid to the duration of time research participants took to complete the study. Online surveys are susceptible to so-called “survey speeding” (?); that is, the tendency of some respondents to quickly “click through” survey items, thus failing to read survey questions or experimental stimuli. To handle the issue of survey speeding, we computed the median completion time for each of the individuals studies. We defined a survey speeder as any respondent who completed the survey faster than 50% of the median time. Thus if the median completion time was, say, 14 minutes, then a survey speeder would be defined as someone having a completion time of 7 minutes or less. Speeders were flagged and eliminated from the analysis.

In Table 1, we give the number of “starters” (those who agreed to informed consent and started the survey), the number of “dropouts” (those who broke off the survey prior to exposure to experimental stimuli), and the number of “speeders” for each study. The last column corresponds to the number of respondents who were eligible for inclusion in the analysis.
Table 1: Sample sizes for each study after handling dropouts and speeders

<table>
<thead>
<tr>
<th>Study</th>
<th>Starters</th>
<th>Dropouts</th>
<th>Speeders</th>
<th>Observations</th>
</tr>
</thead>
<tbody>
<tr>
<td>Study 1</td>
<td>461</td>
<td>13</td>
<td>21</td>
<td>427</td>
</tr>
<tr>
<td>Study 2a</td>
<td>415</td>
<td>61</td>
<td>32</td>
<td>322</td>
</tr>
<tr>
<td>Study 2b</td>
<td>387</td>
<td>49</td>
<td>26</td>
<td>312</td>
</tr>
<tr>
<td>Study 3a</td>
<td>421</td>
<td>65</td>
<td>19</td>
<td>337</td>
</tr>
<tr>
<td>Study 3b</td>
<td>392</td>
<td>57</td>
<td>23</td>
<td>312</td>
</tr>
<tr>
<td>Study 4</td>
<td>1569</td>
<td>185</td>
<td>184</td>
<td>1200</td>
</tr>
</tbody>
</table>

“Dropouts” are research participants who opted out of the survey prior to exposure to the experimental conditions. In study 4, dropouts also included respondents who did not pass screening questions filtering out non-partisans.

Figure 1: This figure gives the predicted $\hat{O}$ for the four outcome measures for Study A (orange line) and Study B (blue line) including survey speeders. Numerical entries denote the 95% prediction interval for each experimental condition.

To show exclusion of speeders has no impact on conclusions reported in main body of paper, we estimated the treatment effects including speeders. The results are reported in Figure 1. This
figure shows the differences in fitted values across each experimental condition.

In Figure 2, we report the results excluding speeders (i.e. using the set of respondents used in the main paper). The differences between the two plots are indistinguishable indicating our criteria for excluding speeders has no impact on our conclusions.

![Figure 2](image)

Figure 2: This figure gives the predicted $\hat{O}$ for the four outcome measures for Study A (orange line) and Study B (blue line) excluding speeders. Numerical entries denote the 95% prediction interval for each experimental condition.

### 3 Justification for pooling studies 1-3

In the analysis, we opted to combine Studies 1, 2a/b, and 3a/b. This section provides evidence justifying the decision to combine these studies into “Study A.” In Figure 3, we give the estimated treatment effects of each individual study. The top three panels correspond to Studies 1–3 and the bottom corresponds to Study 4. The key takeaway point is that Studies 1–3 are indistinguishable in terms of predicted outcomes for each of the dependent measures. Study 4 is distinctly different from Studies 1–3.
Figure 3: Effects of information about Lopez on beliefs about support for removal and dispositional blame.

Regarding the gender manipulation (“Miguel” v. “Maria”), the second row of Figure 3 presents the estimates separately for each condition. While it is the case that estimated predicted effects were generally lower in the “Maria Lopez” condition, these differences are tiny and mostly indistinguishable from 0. This provides justification for pooling the gender conditions. Similar remarks apply to the low-infraction manipulation, shown in row three. The differences between the broken taillight condition and the immigration raid condition are minimal and mostly indistinguishable from 0.

To statistically assess dissimilarities among studies 1-3, we regressed the outcome measures on the experimental condition factor as well as a factor-level variable indentifying studies 1, 2a, 2b, 3a, and 3b. The results are reported in Table 2. The key point is that the indicator variables for
each of the studies indicate no difference from the baseline (study 1). Further, the coefficients show no distinct differences across Studies 2–3 in terms of the difference-from-baseline. This leads to the conclusion that studies 1–3 are indistinguishable from one another, thus justifying pooling. This constitutes the justification for Study A.

Table 2: Regression estimates for Studies 1-3

<table>
<thead>
<tr>
<th>Dependent variables</th>
<th>Support for removal</th>
<th>Justness of removal</th>
<th>Most to blame</th>
<th>Amount of blame</th>
</tr>
</thead>
<tbody>
<tr>
<td>Low/none</td>
<td>-0.095</td>
<td>-0.075</td>
<td>0.014</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.032)</td>
<td>(0.028)</td>
<td>(0.042)</td>
<td>(0.026)</td>
</tr>
<tr>
<td>High/none</td>
<td>0.166</td>
<td>0.032</td>
<td>0.374</td>
<td>0.261</td>
</tr>
<tr>
<td></td>
<td>(0.032)</td>
<td>(0.028)</td>
<td>(0.043)</td>
<td>(0.026)</td>
</tr>
<tr>
<td>Low/death</td>
<td>-0.210</td>
<td>-0.192</td>
<td>-0.066</td>
<td>-0.063</td>
</tr>
<tr>
<td></td>
<td>(0.033)</td>
<td>(0.028)</td>
<td>(0.043)</td>
<td>(0.026)</td>
</tr>
<tr>
<td>High/death</td>
<td>-0.065</td>
<td>-0.111</td>
<td>0.094</td>
<td>0.089</td>
</tr>
<tr>
<td></td>
<td>(0.033)</td>
<td>(0.028)</td>
<td>(0.043)</td>
<td>(0.026)</td>
</tr>
<tr>
<td>Study 2a</td>
<td>-0.028</td>
<td>-0.012</td>
<td>0.003</td>
<td>-0.009</td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td>(0.020)</td>
<td>(0.030)</td>
<td>(0.018)</td>
</tr>
<tr>
<td>Study 2b</td>
<td>0.009</td>
<td>0.011</td>
<td>0.045</td>
<td>0.028</td>
</tr>
<tr>
<td></td>
<td>(0.023)</td>
<td>(0.020)</td>
<td>(0.030)</td>
<td>(0.019)</td>
</tr>
<tr>
<td>Studies 3a</td>
<td>-0.014</td>
<td>0.008</td>
<td>-0.015</td>
<td>0.003</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.020)</td>
<td>(0.031)</td>
<td>(0.019)</td>
</tr>
<tr>
<td>Study 3b</td>
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<td>0.020</td>
<td>-0.006</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(0.024)</td>
<td>(0.020)</td>
<td>(0.031)</td>
<td>(0.019)</td>
</tr>
<tr>
<td>Constant</td>
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<td>0.371</td>
<td>0.154</td>
<td>0.205</td>
</tr>
<tr>
<td></td>
<td>(0.033)</td>
<td>(0.028)</td>
<td>(0.043)</td>
<td>(0.027)</td>
</tr>
</tbody>
</table>

| Observations        | 1,704               | 1,702               | 1,687         | 1,697          |
| Adjusted $R^2$      | 0.149               | 0.082               | 0.136         | 0.182          |

4 Analysis by race and gender

About half of the Study A participants identified as nonwhite and about 30% of Study B participants identified as nonwhite. To see if there were substantial differences in treatment effects due to race, we estimated the model described in equation 1 in the main paper but included a dummy variable recorded as “1” if the respondent identified as racially white and “0” if not. We interacted this dummy variable with each variable in equation 1. The results are visually reported in Figure 4.

In general, the pattern of results reported in the main paper hold for both white respondents and non-white respondents. In other words, conclusions drawn about the infraction hypothesis (given by the differences between the “Low/none” v. “High/none” and “Low/death” v. “High/death” conditions) seem to hold for both subgroups). Similar remarks apply regarding the information
hypothesis.

To see if there were substantial differences in treatment effects due to gender, we estimated the model described in equation 1 in the main paper but included a dummy variable recorded as “1” if the respondent identified as female and “0” if male. We interacted this dummy variable with each variable in equation 1. The results are visually reported in Figure 5. As with race, there are no appreciable differences in conclusions if we account for gender.

5 Question wording of scales

For the system justification scale, respondents were given the prompt “For the next set of questions, we are interested in how much you agree or disagree with each idea. You can work quickly; your
Figure 5: This figure gives the predicted $\hat{O}$ for the four outcome measures by gender for Study A (orange line) and Study B (blue line). Numerical entries denote the 95% prediction interval for each experimental condition.

first feeling is generally best.” Responses were on a seven-point scale coded 1=strongly disagree to 7=strongly agree. The statements were: In general, the American political system operates as it should; The United States is the best country in the world to live in; Most policies serve the greater good; Everyone has a fair shot at wealth and happiness; Our society is getting worse every year (R); and Society is set up so that people usually get what they deserve. Thus, high scores on this scale denote high system justifiers; low scores denote low system justifiers.

For the individualizing values scale, in Study A, respondents were given the following prompt: “In the next few questions, we are interested in how much you agree or disagree with each idea. You can work quickly; your first feeling is generally best.” Responses were scored on a seven-point scale coded such that 1=strongly disagree to 7=strongly agree. The statements were: “One of the worst things a person could do is hurt a defenseless animal”; “It can never be right to kill a human
being”; “Compassion for those who are suffering is the most crucial virtue”; “If we are to fight crime effectively, some people’s rights will have to be violated” (R); “Justice, fairness and equality are the most important requirements for a society”; and “When the government makes laws, the number one principle should be ensuring that everyone is treated fairly.” In Study B, in addition to the six statements given above, responses were recorded on two additional statements: “Our laws should first and foremost aim to reduce harm to victims”; “I think it’s morally wrong that rich children inherit a lot of money while poor children inherit nothing.”

6 Fully pooled analysis

In the main paper, we rely on Study B data to estimate the party effects as well as the relationship between system justification, individualizing values, and ideology. In this section, we report the fully pooled results. These results are not reported in the main paper for several reasons, chief among them is that in Study A, there are few Republican identifiers; however, we also point out that the fairness and harm scale had very low reliability (and, for Study A, was based on two fewer questions than for Study B respondents).

To proceed, we estimated the model reported in equation 2 in the main text but interacted each term in that model with a dummy variable denoting Study A respondents. Doing this permits us to separate the results of Study A from Study B. In Table 3 we report the results. Because of the number of two-way and three-way interactions, we report the results as figures.

In Figure 6, the treatment effects for each experimental condition and each outcome measure are reported by party and study. Study A is in the left column and Study B is in the right column. The confidence intervals for the Republican estimates in Study A indicate a great deal of uncertainty, owing to the small number of Republican identifiers ($n = 174$). Regarding support and perceived justness of Lopez’ removal, however, even with a small number of Study A Republicans, the pattern is in the predicted direction for both the infraction and information hypotheses. For Democrats, the pattern of estimates is very similar for Study A respondents compared to Study B, with two exceptions.

First, for Study A Democrats, baseline levels of support, perceived justness, and dispositional blaming are lower than for Study B Democrats. This is due to composition of the two studies,
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<tbody>
<tr>
<td>Support for removal</td>
<td>0.246 (0.091)</td>
<td>-0.097 (0.029)</td>
<td>0.188 (0.029)</td>
<td>-0.232 (0.029)</td>
<td>-0.060 (0.029)</td>
<td>0.291 (0.155)</td>
<td>0.353 (0.052)</td>
<td>-0.199 (0.055)</td>
<td>0.325 (0.044)</td>
<td>0.109 (0.046)</td>
<td>0.016 (0.047)</td>
<td>0.120 (0.046)</td>
<td>0.009 (0.046)</td>
<td>-0.193 (0.186)</td>
<td>-0.011 (0.080)</td>
<td>-0.170 (0.094)</td>
<td>-0.220 (0.062)</td>
<td>-0.028 (0.082)</td>
<td>-0.232 (0.082)</td>
<td>-0.049 (0.081)</td>
<td>-0.088 (0.082)</td>
<td>-0.232 (0.127)</td>
<td>0.005 (0.133)</td>
<td>0.167 (0.116)</td>
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<tr>
<td>Justness of removal</td>
<td>0.149 (0.077)</td>
<td>-0.083 (0.025)</td>
<td>0.025 (0.025)</td>
<td>-0.212 (0.025)</td>
<td>-0.110 (0.025)</td>
<td>0.297 (0.132)</td>
<td>0.356 (0.044)</td>
<td>-0.148 (0.047)</td>
<td>0.291 (0.038)</td>
<td>0.111 (0.040)</td>
<td>0.128 (0.040)</td>
<td>0.122 (0.039)</td>
<td>0.036 (0.039)</td>
<td>-0.115 (0.159)</td>
<td>0.046 (0.068)</td>
<td>-0.137 (0.081)</td>
<td>-0.126 (0.053)</td>
<td>0.008 (0.070)</td>
<td>-0.100 (0.070)</td>
<td>-0.104 (0.069)</td>
<td>-0.122 (0.070)</td>
<td>-0.370 (0.109)</td>
<td>-0.037 (0.113)</td>
<td>0.227 (0.100)</td>
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<td>Most to blame</td>
<td>0.414 (0.138)</td>
<td>0.006 (0.043)</td>
<td>0.410 (0.044)</td>
<td>-0.059 (0.044)</td>
<td>0.102 (0.044)</td>
<td>0.380 (0.235)</td>
<td>0.381 (0.078)</td>
<td>-0.038 (0.083)</td>
<td>0.319 (0.067)</td>
<td>0.053 (0.070)</td>
<td>-0.080 (0.071)</td>
<td>0.058 (0.070)</td>
<td>-0.012 (0.069)</td>
<td>-0.143 (0.282)</td>
<td>0.058 (0.121)</td>
<td>-0.212 (0.143)</td>
<td>-0.223 (0.095)</td>
<td>0.140 (0.125)</td>
<td>-0.221 (0.124)</td>
<td>-0.055 (0.122)</td>
<td>0.004 (0.123)</td>
<td>-0.439 (0.192)</td>
<td>-0.055 (0.201)</td>
<td>0.109 (0.178)</td>
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<tr>
<td>Amount of blame</td>
<td>0.390 (0.087)</td>
<td>-0.009 (0.027)</td>
<td>0.269 (0.028)</td>
<td>-0.080 (0.028)</td>
<td>0.104 (0.028)</td>
<td>0.210 (0.148)</td>
<td>0.281 (0.049)</td>
<td>-0.073 (0.052)</td>
<td>0.224 (0.043)</td>
<td>0.036 (0.044)</td>
<td>-0.077 (0.045)</td>
<td>0.024 (0.044)</td>
<td>-0.146 (0.044)</td>
<td>-0.112 (0.178)</td>
<td>0.021 (0.077)</td>
<td>-0.105 (0.090)</td>
<td>-0.192 (0.060)</td>
<td>0.114 (0.079)</td>
<td>-0.078 (0.079)</td>
<td>0.048 (0.077)</td>
<td>-0.087 (0.078)</td>
<td>-0.297 (0.122)</td>
<td>0.002 (0.127)</td>
<td>0.053 (0.112)</td>
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Observations: 2,706 2,704 2,686 2,696
Adjusted R²: 0.499 0.556 0.367 0.516
with Study B being far whiter, older, and more conservative than Study A participants, who were mostly young college students. Second, in the main paper, we found that for Democrats, there were no strong differences between the “Low/death” and “High/death” conditions. For Study A, there is evidence that these two conditions differ, suggesting that the infraction hypothesis holds with respect to the comparison between “Low/death” and “High/death.” For Study B, we concluded this hypothesis did not hold, though the direction of the differences were in the predicted direction. Nonetheless, we do not think this substantially counters our main conclusions. Apart from the fact that Study B is a more diverse sample, for Study A Democrats in the “Low/death” condition, levels of support, perceived justness, and dispositional blame are extremely low compared to Study B. Given this, there is simply more “room to move” from the low baseline.

In comparing the estimates for system justification, individualizing values, and ideology, consider
Figure 7 Here we plot the estimates of these items from the fully pooled regression model reported in Table 3. As is clear, apart from the fact that Study B participants have higher baseline levels of support and the confidence intervals are much wider for Republicans in Study A (because there are so few), perceived justness, and dispositional blame compared to Study A respondents, the conclusions from the main paper with respect to Study B are comparable to results found for Study A.

Figure 7: This figure gives the estimated relationship between system justification (upper left), individualizing values (upper right) and ideology (lower left) along with the 95% confidence intervals for each outcome measure by study. Republicans are denoted as red and Democrats as blue.