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Bridging the Gap: Legal Doctrine and Immigration Detention

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Abstract

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Does learning about legal violations in immigration detention affect U.S. public approval of the practice? If so, does awareness of violations of U.S. constitutional law or international law have a greater effect? Despite increased emphasis on domestic politics in the legal community and on immigration attitudes in the field of political science, scholars remain divided over whether and how legal criticism of a country's practices affects public opinion and behavior. Moreover, while public support is essential to the legitimacy of policies in a democracy, research remains largely observational and only recently have experimental studies tested why some citizens seek to narrow the "rights gap," while others "backlash" against criticism. In the real world, proponents of immigration detention often defend the need to protect national security and domestic sovereignty, but reformists argue that immigration detention is unjust when the U.S. breaches its legal obligations. Upon employing a Lucid survey experiment, I predict that referencing a U.S. constitutional violation will decrease public approval for immigration detention and increase action for reform. I hypothesize that referencing an international violation will have the same effects, but to a weaker extent than a U.S. constitutional violation. My findings carry important implications as to how legal norms could bridge the gap between rights and reality, and whether immigration detention policy truly reflects the will of the American people.

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I. Introduction

“We are doing everything possible for ourselves, making masks out of socks, but we know that this is not enough. We need someone to listen to us please. We are only asking for help. We are only fighting for our lives.” – Woman detained at Georgia’s Irwin Detention Center in 2020.

“What worries me is that we are restrained from our freedom as human beings. Our children are crying.” – Father participating in hunger strike at Texas’ Karnes Detention Center in 2018.¹

The detention of immigrants in isolated facilities across the United States is a pressing yet divisive issue that calls into question human dignity, civil rights, and democratic politics. While government officials often justify immigration detention as ensuring national security and sovereignty, activists demand compliance with the rule of law even under these circumstances. In 2018, the U.S. detained nearly 400,000 people. In 2019, the number of people detained reached over 500,000 people, breaking both national and global records (GDP 2017; TRAC 2021).

The U.S. government has long used detention to isolate and control particular immigrant groups. In the wake of the Chinese Exclusion Acts, Chinese and other Asian immigrants at Angel Island Immigration Station were interrogated, confined in dormitories, and supervised by escort guards. Amidst fears of invasion during WWII, people of Japanese descent were placed in internment camps as “enemy aliens” (*Korematsu v. United States* 1944; Longazel et al. 2016). Yet the mistreatment of immigrants has continued in recent decades.

¹ Both of the above testimonials are featured in a report authored by Setareh Ghandehari, Luis Suarez, and Gabriela Viera of Detention Watch Network (DWN) (2021).

After the Bracero contract labor program employed Mexican immigrants, the government detained and deported thousands of them across the border through Operation Wetback. With increases in Caribbean refugees in the 1980s, along with the War on Drugs, the Immigration and Naturalization Service (INS) apprehended migrants from Haiti and eventually all other countries and placed them in detention centers that still stand today (GDP 2017; Longazel et al. 2016).

After 9/11, when the INS became the Department of Homeland Security (DHS), the government began to legally justify detaining immigrants because they pose a “danger to the community” (8 C.F.R. § 1236). For example, in *Trump v. Hawaii* (2018), the Supreme Court deemed national security to be a “legitimate purpose” of what many considered a racially-motivated immigration policy: the Muslim travel ban. While the government may defend immigration detention in court cases and policy statements, breaking established U.S. legal obligations may draw criticism and disapproval among citizens.

Civil and human rights advocates claim that U.S. immigration detention is an unjust practice that violates fundamental laws and treaties that this country ratified long ago. Thousands of immigrants in the U.S. have reported cruel, inhuman, and degrading treatment or punishment in detention. Among 47,145 grievances gathered by ICE’s reporting line and Human Rights Watch’s (HRW) complaint system, 67% referred to limited access to legal counsel and to basic immigration case information and 12.5% related to insufficient access to the outside world (Ryo and Peacock 2018). Based on data the HRW and the ACLU collected from five detention centers, thirty-nine adults died and twelve adults committed suicide in custody or right after release (Cho et al. 2020). A national study found, among 7,000 detained asylum-seekers, 61.8% reported inadequate food and water, 34.5% described a lack of facility hygiene, and 45.6% shared their inability to sleep due to overcrowding or frigid temperatures (Saadi et al. 2020).

Finally, the DHS received 33,126 complaints of sexual and physical abuse from 2010 to 2016, though it investigated only 570 of them, and only 57% of solitary confinement sentences had a recorded reason (Saadi et al. 2020). The practice of immigration detention allegedly protects national safety and security, yet risks the welfare of immigrants themselves.

Immigration detention policy offers a window into a larger puzzle: whether a state practice considered in violation of a country's law(s) affects the attitudes and action of *informed* citizens. International legal studies and political science research have yet to assess the role of U.S. constitutional and international legal norms in the context of immigration detention. Most studies informing citizens about legal violations center around wartime practices or economic policies (i.e., drone strikes, trade barriers), although two researchers recently analyzed opinions on solitary confinement and refugee quotas (Chilton 2014; Cope and Crabtree 2020). Immigration detention is different from, though related to, civil confinement and immigration restrictions. In addition, prior studies informing participants about legal violations solely focus upon international treaty obligations. Only one study informed participants of how the practice of torture violates both U.S. constitutional law and international law (Chilton and Versteeg 2016). Thus, I compare and contrast the salience of two sources of law, the International Covenant on Civil and Political Rights (ICCPR) and the U.S. Constitution, each of which prohibit the cruel treatment of detainees in the United States.

The 7th Article of the International Covenant on Political Rights (ICCPR) expressly prohibits "torture or cruel, inhuman or degrading treatment or punishment" (UN General Assembly 1966). The U.S. ratified the ICCPR in 1992, when the U.S. Senate gave its consent and President Bush implemented the international treaty via domestic legislation. Because the

ICCPR underwent this domestic implementation process, it is the law of the land and binds all 50 United states and territories.

However, the U.N. Special Rapporteur found that U.S. immigration detention “violates the spirit of international laws and... the actual letter of those instruments” when the “availability of effective alternatives renders [detention] unnecessary” (UN Human Rights Council 2008, 25). In 2014, the Human Rights Committee, the ICCPR’s treaty-monitoring body, suggested that the U.S. ensure that detained immigrants “have access to legal representation” and “access to adequate health care” (cited in GDP 2017). In the 2020 Universal Periodic Review, which assesses compliance with the ICCPR and other treaties, many countries advised the U.S. to change its immigration practices too. Thailand called upon the U.S. to “increase efforts to protect migrants... by seeking alternatives to detention [and] ensuring access to basic services,” meanwhile Zambia and Ecuador advised the U.S. to use “alternatives to the present immigration detention system” that “meet basic human rights standards” (cited in GDP 2017). The ICCPR is not the first and only legal norm that the United States violates with the ongoing practice of immigration detention.

Like the 7th Article of the ICCPR, the 8th Amendment of the U.S. Constitution guarantees that the U.S. government shall not subject anyone to “cruel and unusual punishment” (U.S. Const. amend. VIII). This amendment was adopted by Congress and ratified by the states in 1791 as part of the U.S. Bill of Rights, making it the law of the land. While immigration detention has not yet been deemed a civil rights violation or unconstitutional per se in federal courts, lower courts have edged towards conferring noncitizens certain rights and liberties before and during detention. While *Zadvydas v. Davis* (2001) only made a statutory interpretation of the Immigration and Nationality Act (INA), the Supreme Court did construe the federal statute as

limiting immigration detention *after* a final removal order because making detention indefinite raised “serious constitutional concerns.” However, in *Demore v. Kim* (2003), the Supreme Court ruled that it is constitutional for the INA to subject immigrants to detention *before* a removal order, or while they are in proceedings. Supreme Court cases on immigration detention mainly focus upon the length of detention (Smith 2019), but it is possible that one day the high court will review a challenge to the cruel treatment of immigrants in detention.

Informing citizens about a violation of either international law or of U.S. constitutional law could persuade them to support immigration detention reform, but justifications of national security and sovereignty may make them resist international legal standards, especially in the post-Trump era. All of the previous survey experiments informing U.S. citizens about legal violations were fielded before the 2016 election cycle (Chaudoin 2014; Chilton 2014; Chilton 2015; Kreps and Wallace 2016; Wallace 2013; Wallace 2014). Since 2016, the Trump Administration employed anti-immigration rhetoric, implemented restrictive immigration policies (i.e., the Muslim travel ban), and withdrew from several international relations (e.g., the W.H.O. and the U.N. Human Rights Council). Research in other countries indicates that citizens who already hold negative views towards international governmental organizations or who worry about international scrutiny are more likely to backlash in defense of their country’s practices (Chapman and Chaudoin 2020; Lupu and Wallace 2019). Given the shift in U.S. immigration affairs and international relations, public opinion is in need of updated assessment.

The primary goal of this research study is to elucidate the extent to which awareness of legal violations affects approval of U.S. immigration detention and action to phase out the practice. As I discuss below, legal and political science scholars propose competing theories about whether and how a country’s legal commitments impact citizens’ responses to alleged violations.

Moreover, research studies are often either observational or, more recently, experimental and only test either why citizens seek to close the “gap” between policy and practice or “backlash” against criticism of inconsistent policies – but not whether certain groups in a sample may backlash, while others advocate for change. Through a survey experiment, I inform two treatment groups that a court condemns the United States for violating its ratified law. I vary the type of law allegedly in violation, either the U.S. Constitution or the ICCPR, to disentangle whether one evokes more support for immigration detention and its reform. Specifically, I measure 1) citizens’ approval of ongoing immigration detention and immigration detention reform and 2) citizens’ willingness to sign a petition to Congress and contact their representative. I also perform exploratory analyses about the interactive effects between the treatments and partisanship and attentiveness, respectively.

The remainder of this paper proceeds as follows. The first section highlights the turn to domestic politics in the study of law and international relations, as well as the measurement of immigration attitudes in the field of political science. I also explain why immigration detention reform offers a fruitful avenue for studying whether awareness of legal violations brings citizens to shift their attitudes and take action. Next, I evaluate competing theories about citizens addressing the legal *rights gap* and engaging in a *backlash* against legal criticism, which led me to develop my three hypotheses. I then discuss my survey experimental research design, including its strengths and limitations. After I share the descriptive statistics and regression results of my data, I conclude by discussing my study’s implications and areas for future research.

II. Literature Review

Citizens' Responses to Rights Violations

Public Opinion in Survey Experiments

In the fields of international relations and political science, scholars have measured support for human rights reform using both behavioral and attitudinal outcome questions. Firstly, some researchers studying the impact of information about legal violations discovered its significant, yet modest treatment effects on citizens' attitudes. Often times, after reading survey vignettes, researchers have participants answer follow-up questions about the extent to which they agree with *criticized practices* (Chilton 2014; Chilton and Versteeg 2016; Kreps and Wallace 2016; Lupu and Wallace 2019; McEntire et al. 2015; Wallace 2013, 2014). Wallace observed a significant negative effect of learning about international violations on support for torture itself (2013), while Chilton and Versteeg found that awareness of U.S. constitutional violations decreased support for torture (2016), though the differences between their control group and treatment groups was not statistically significant. Kreps and Wallace found that information that drone strikes are "inconsistent with international legal principles" significantly decreased approval of the wartime practice (2016). The type of actor alleging the violation made a difference. Learning about illegal drone strikes from the government was less persuasive than reading the same information from the UN or an NGO, perhaps because international organizations frame information with a humanitarian goal, unlike government statements considered as regular updates (Kreps and Wallace 2016).

In other studies, researchers ask questions to capture the extent to which participants support *reforms aimed at improving criticized practices* (Anjum et al. 2021; McEntire et al. 2015). Public support for reforms has also been statistically significant, regardless of the framing of

legal information (informational, motivational, and personal, see McEntire et al. 2015) or the type of legal reform proposed (e.g., increasing female quotas, raising minimum marriage age, repolling locations where women cast few votes, and repealing laws, see Anjum et al. 2021).

Therefore, my study includes not only attitudinal questions asking participants about their level of support for immigration detention practices, but also about their willingness to advocate for immigration detention reform.

However, other researchers have found little to no significant change in public opinion about criticized practices after participants learned that their country violated the law. Chilton and Versteeg (2016) did not find statistically significant differences between their control group and treatment groups informed that U.S. torture practices breach legal obligations. However, in Wallace's (2013) study, which Chilton and Versteeg (2016) replicated, he obtained statistically significant results. Chilton and Versteeg attributed their null findings to their unique sample demographics, as their independents were 10% more supportive of torture than Wallace's moderates to begin with (2013). Thus, it may have been harder for the treatment to make Chilton and Versteeg's participants significantly less likely to support torture than Wallace's participants, who might be more easily persuaded to shift their opinions. Furthermore, researchers have suggested that other factors may trump the effects of legal norms, including participants' prior policy preferences (Chaudoin 2014; Tomz and Van Houweling 2010) and dislike for international scrutiny into domestic affairs (Chapman and Chaudoin 2020; Cope and Crabtree 2020; Lupu and Wallace 2019). My study seeks to elucidate the puzzling findings of these studies and whether awareness of certain legal violations does or does not significantly affect citizens' responses to immigration detention.

Political Behavior in Survey Experiments

More recently, a small group of studies measured not only public opinion about a country's practices, but also people's willingness to engage in reform through various action-oriented questions. After viewing survey vignettes about a legal violation, participants have been asked how likely they would be to add their name to a petition to elected officials (Anjum et al. 2021; McEntire et al. 2015), to vote for a party or candidate (Anjum et al. 2021), or to pay extra and donate funds (Anjum et al. 2021; Hertel et al. 2009) – all to improve existing practices. In all of these studies, mobilization to act did significantly increase after learning about illegal practices. However, McEntire et al. noted that “action mobilization” is more difficult than getting an attitudinal consensus that a practice is wrong (2015, 26). It is possible that motivating a participant to take direct action requires a greater treatment effect from the survey vignettes. Choosing to change one's behavior or commit to a greater cause goes beyond simply having a negative attitude toward a practice. My study thus incorporates behavioral questions pertaining to willingness to advocate for immigration detention reform replacing the practice with community-based case programs.

Public Opinion On Immigration in Surveys and Survey Experiments

In the political science community, scholars have long fielded empirical studies that measure attitudes about immigrants and immigration policy in the United States. Rigorously-tested explanations of anti-immigration views include feeling economically or culturally threatened by immigrants, particularly those from certain countries or of the male gender (Enns and Ramirez 2018; Hainmueller and Hopkins 2014; Sniderman et al. 2004). More recent studies illustrate the demographic factors that significantly influence citizens' attitudes toward specific immigration policies, such as the zero tolerance policy that separated families (Rowatt et al. 2020), sanctuary policies that prohibit cooperation between local law enforcement and federal immigration

authorities (Casellas and Wallace 2018), and policies that allow private carceral facilities to be used for immigration detention (Enns and Ramirez 2018). I discuss these demographic factors in the *Balancing Confounding Variables* and *Research Design* sections below.

Notably, there are no academic studies or national polls that focus *solely* upon citizens' responses to U.S. immigration detention. Some national polls assess opinions about Immigration and Customs Enforcement (ICE). For example, a 2018 Pew Research Center study of 1,007 U.S. citizens indicated polarized opinions about ICE, with 47% of Americans viewing it unfavorably and 47% taking the other side (Doherty et al. 2018). Also, 77% of conservative Republicans viewed ICE favorably, while a large majority of liberal Democrats (82%) viewed ICE unfavorably. There are significant gender differences. Fewer men viewed ICE unfavorably (41%) than women (53%) (Doherty et al. 2018). However, ICE is a government agency that manages immigration detention centers as well as immigration court cases and homeland security investigations, so public attitudes from these surveys may reflect opinions about a wide range of practices.

More specifically, an NPR/Ipsos online poll of 1,176 U.S. adults (a probability sample) showed that 68% of Democrats want to end ICE's for-profit detention centers and 60% of Independents offered support for this reform. However, only 44% of Republicans support the proposal (Newall 2021). Also, a national web survey based on a probability sample of U.S. citizens assessed general sentiments on U.S. immigration detention and found that 59% of likely 2020 voters support suspending new detentions and 51% support limiting the detention of newly arrived immigrants (Chapin 2020). The more recent surveys focus more upon attitudes about immigration detention, possibly indicating the greater salience of the practice among citizens and the political polarization of the issue as well. However, these survey polls differ in methodology

from my survey experiment, which can make causal inferences about support for immigration detention and its reform (e.g., due to legal norms, partisanship, etc.).

Information About Rights Violations

Treaty Implementation in Observational Studies

When a country violates the obligations in its legal agreement(s), how does the country's population react? International legal scholars began answering this question with macro-level studies, finding that signing human rights treaties may correlate with the realization of rights within a country, although that relationship is stronger when a country has judicial independence, domestic legislatures, and/or a more active civil society advocating for human rights (Davis et al. 2012; Lupu 2013; Neumayer 2005; Powell and Staton 2009; Risse and Sikkink 2009). Moreover, the realization of human rights varies by which international treaties are under study. Ratifying the International Covenant on Civil and Political Rights (ICCPR) is associated with greater freedom of association and speech, although not better physical integrity rights (e.g., freedom from torture), while the Convention Against Torture (CAT) coincides with increased torture practices (Hill 2010; Lupu 2013).

However, such observational studies face endogeneity issues. Ratifying an agreement may not be the sole reason for changes in a country's human rights record, but countries that sign particular agreements and those who do not might be different in ways that cannot be controlled. For example, potential common causes of treaty ratification and improved human rights include membership in international organizations, which often require states to sign onto treaties to become members (Davis et al. 2012). Furthermore, studies that assess the impact of several ratified treaties likely capture the "ease of ratification" (Simmons 2010, 289), rather than a country's commitment to a specific agreement (Hill 2010; Lupu and Wallace 2019). Thus, I

conduct a more parsimonious survey experiment that focuses upon the specific effect of one human rights treaty, the International Covenant on Civil and Political Rights, in one country, the United States.

Violations of International Law in Survey Experiments

Increasingly, research scholars have begun employing experimental methods to analyze the impact of international human rights treaties on public opinion and willingness to advocate for reform. Survey experiments proliferated the field of international legal studies in the last decade to heed the call for empirical tests that can make causal inferences about the impact of legal norms (Chilton 2017; Chilton and Tingley 2013; Kertzer and Tingley 2018). Despite methodological progress, however, most studies narrowly focus on wartime practices, including interrogation techniques, torture, drone strikes, and bombing campaigns (Chapman and Chadouin 2020; Chilton and Versteeg 2016; Kreps and Wallace 2016; Lupu and Wallace 2019; McEntire et al. 2015; Wallace 2013; Wallace 2014) or economic practices related to trade and labor (Chaudoin 2014; Hertel et al. 2009; Putnam and Shapiro 2017). More recent survey experiments have centered around countries setting restrictive refugee quotas and using solitary confinement on prisoners (Cope and Crabtree 2020; Chilton 2014). However, I conduct further research, as suggested by McEntire et al. (2015), to determine what may “work better to mobilize consensus and action around some human rights issues but not others (such as disappearances), or on behalf of some types of victims” (33). My study is the first survey experiment to focus upon how different legal norms impact mobilization around the issue of immigration detention, building upon research about issues relating to incarcerated individuals and non-citizens.

Violations of Constitutional Law in Survey Experiments

Furthermore, a few scholars have compared the effects of awareness of violations of international law *and* U.S. constitutional law. Chilton and Versteeg (2016) found that U.S. constitutional violations decreased support for torture relative to control, although the difference between the experimental and control groups was just shy of statistical significance. Chapman and Chaudoin recently noted that many studies only "rely on general indicators of a population's support for international law... when considering whether an intervention would enjoy local support" but that they leave out "likely reactions to a domestic investigation" (2020, 1307). After comparing the impact of domestic versus foreign investigations into legal violations, the scholars found that the public was more supportive of international interference in other countries rather than into their own country's domestic affairs (Chapman and Chaudoin 2020). Taking into account these inconsistent findings and the potentially different responses to domestic and international scrutiny, I compare citizen responses to alleged violations of the ICCPR *and* the U.S. Constitution.

Legal Vignettes in Survey Experiments

In practice, prior survey experiments included carefully worded vignettes to make participants aware of the way in which their country's practice(s) violates the law. Specifying the legal provisions in violation, rather than making generic reference to a violation of "human rights" or "international treaties," proves to have a stronger effect on decreasing public support for practices (Chilton 2014, 135; Chilton and Versteeg 2016, 3; Lupu and Wallace 2019, 7; Wallace 2013, 118; Wallace 2014, 505). Not only can this design choice improve treatment effects, but it also avoids deception when choosing practices actually prohibited in legal instruments. For example, Lupu and Wallace's (2019) survey "shamed" a country for forms of repression prohibited in the ICCPR and Anjum et al. (2021) shared proposals from a UN

CEDAW committee report. Furthermore, citing a credible legal authority that criticizes the U.S. can confer more legitimacy to a legal allegation because institutions may be perceived to have more relevant knowledge and public trust. Prior studies have shown that shaming from international non-governmental organizations, like Human Rights Watch and Amnesty International, make respondents more likely to pressure their elected leaders than shaming from activists or entities without humanitarian recognition (Murdie and Bhasin 2011; Davis et al. 2012; Kreps and Wallace 2016). International governmental organizations (IGOs), like the UN Security Council, UN Special Rapporteur, UN treaty-monitoring bodies, and the International Criminal Court (ICC) (Anjum et al. 2021; Chapman and Chaudoin 2020; Chapman and Reiter 2004; Kreps and Wallace 2016; Thompson 2006), are also considered legitimate critics providing a “second opinion” on abuses (Grieco et al. 2011, 564). Thus, my study is among the first to attribute legal allegations to a U.S. federal court and an international court in general.

Declining to reference the name of a specific legal institution (e.g., the U.S. Supreme Court, the Eastern District Court of New York, the ICC, the International Court of Justice) that monitors compliance with the international law or U.S. constitutional law has allowed researchers to focus upon their key independent variable: legal norms. As other scholars note, the international law treatment should be made “purposely broad, rather than naming individual institutions, to capture the effect of general attitudes toward international law, rather than... strong prior beliefs with respect to individual institutions” so that “any backlash effect [is due to] broad negative perceptions of international law” (Lupu and Wallace 2019, 7). Not specifying a judicial institution that has actually investigated a country’s practices is also beneficial to create a hypothetical scenario. For example, Chapman and Chaudoin’s design choice to cite a court (the ICC) that has never had jurisdiction over the country of study (Kyrgyzstan) sought to “simulate

the proposal of a possible investigation.... rather than [capture] attitudes that have already hardened or been influenced by mobilization campaigns" by certain legal institutions (2020, 1306). Powell and Staton (2009) also called upon researchers to link theoretical arguments about domestic courts, such as judicial legitimacy, to international bodies. I examine whether judicial decisions that the U.S. violates the law subject to a court's jurisdiction affects citizen responses to immigration detention.

Balancing Confounding Variables

Nativity, Race, and Gender

I pull several variables from the immigration research literature above to ensure that all of the randomized groups in my survey experiment share characteristics other than the independent variable of interest. I did so to also distinguish treatment effects among certain demographic groups in my sample, such as Republican participants, as discussed in the *Research Design* and *Results* sections. Surveys show that foreign born people who identify as Latinx are less supportive of the punitive immigration policies (Casellas and Wallace 2018; Enns and Ramirez 2018). White males strongly support certain freedoms (e.g., fair labor), but not those freedoms that protect minorities (Hertel et al. 2009).

Relatedly, gender might influence public responses to restrictive immigration practices, including immigration detention, as fewer men view ICE unfavorably (41%) than women (53%) (Doherty et al. 2018). Women have also been more likely to indicate support for reforming labor policies and sleep deprivation policies than men (Hertel et al. 2009; McEntire et al. 2015). As a result, survey experimental studies can benefit from controlling for nativity, race or ethnicity, and gender to ensure that the only difference across the groups is whether they receive a particular experimental treatment.

Partisanship and Political Ideology

In addition, survey analyses of partisanship and anti-immigrant attitudes indicate that Republicans are more supportive of restrictive immigration policies than Democrats (Casellas and Wallace 2018). A U.S. national public opinion poll found that liberals are three times more likely than non-liberals to support human rights, reflective of support for international legal agreements among left-leaning societal coalitions and leftist supporters (Hertel et al. 2009). In Wallace's causal experiment, liberals and conservatives responded in systematically different ways to legal appeals (2013). Liberals tended to focus upon "principles of equality and community," which coincides with "multilateralism and international institutions," while conservatives held more intense skepticism of such institutions and promoted greater self-reliance (Wallace 2013, 115). It is of interest to study whether conservative, Republican citizens also resist the imposition of international legal instruments upon domestic immigration detention practices.

Region, Education, Income, and Age

While living in areas with a growing Latino population and in border states did not have a significant effect in one study (Casellas and Wallace 2018), other studies suggest that support for rights abuses increases among residents in regions more likely to be subject to investigation. For example, residents' "proximity to areas where abuses took place" may breed fear of the "resumption of hostility" due to legal scrutiny from the International Criminal Court (Chapman and Chaudoin 2020, 1307). However, living in a state with pro-immigrant sanctuary policies or with greater numbers of immigrants could make participants in such a state less supportive of detention.

Another important factor in immigration opinions is education and income level. A lower degree of education, correlated with earning lower wages, is proven to lead to greater perceptions of immigrants as a threat (Hainmueller and Hopkins 2014; Sniderman et al. 2004) and to greater support for restrictive immigration policies (Casellas and Wallace 2018; Enns and Ramirez 2018). While income did not have a significant effect upon opinions about sanctuary policies (Casellas and Wallace 2018), it may have a significant influence in the context of immigration detention policies because those most vulnerable to detention and/or with immigrant heritage tend to have lower socioeconomic statuses.

Finally, several studies disentangled the impact of age upon public support for rights abuses. Referencing specific legal provisions causes older veterans to decrease their support for torture (Wallace 2014), although older Americans (over 60) are less willing than their younger counterparts to pay more to improve practices (Hertel et al. 2009). Overall, the variables I use to create balance across groups are race, Hispanic ethnicity, nativity, gender, partisanship, U.S. region, education level, household income, and age.

This Study's Contributions

This law and social science study contributes to existing literature in several respects. More recent survey experiments have tested whether opposition to a practice increases upon learning about prohibitions against torture, solitary confinement, drone strikes, attacking civilians, the use of force, and restrictive refugee quotas. Scholars have also experimentally tested whether and how awareness of a country's unethical economic practices influences perceptions about the government and human rights. However, I contribute new research in this area by calling people's attention to the rights at issue in immigration detention.

Earlier studies on the impact of legal agreements upon the implementation of rights were historically observational, comparing the behavior of countries before and after they ratified a treaty. However, survey experimental methodology has proliferated the legal scene due to its greater internal validity from randomly assigning citizens into treatment groups and balancing out the groups based on control variables. Thus, I join the increasing number of legal researchers who use survey experiments to test political theories about compliance with the rule of law and examine heterogenous treatment effects (i.e., partisanship) in the survey sample.

Specifically, I manipulate my survey vignettes to give precise citations of the U.S. Constitution and the ICCPR to confer more legitimacy to the norms in violation. While observational studies suffered from endogeneity due to baseline differences between countries that ratify laws and those countries that do not, I randomize who receives information about legal norms and whether those norms are constitutional or international. Also, keeping the legal institutions cited in vignettes intentionally vague prevents treatment effects from factors other than the legal norm in violation. Thus, I take advantage of the ability to reference imaginary or vague institutions in survey vignettes. I cite information just as it would appear in a real-world court case or news story, with a court of law criticizing a country for violating the law.

In addition, I incorporate into my study the novel design choices of the few recent survey experiments in the fields of international law and political science. Chilton and Versteeg's (2016) study compared opinions about how torture violates international law and U.S. constitutional law, so I also use two treatments about how immigration detention violates international law and U.S. constitutional law. Furthermore, I assess not only citizens' attitudes, but also their potential real-world behaviors, selecting the types of reform that seemed to best suit immigration detention advocacy: petitioning for change and lobbying to members of Congress.

III. Theories and Hypotheses

Two bodies of scholarship contribute competing theories as to how and why citizens respond to their country's legal violations, which offers the opportunity to explore which theory applies to immigration detention. Reasons of "safety and security" may make some citizens view a practice as "necessary or at least justified... to safeguard from threats," yet the value of "justice and compliance with the law" may make other citizens condemn existing practices (Lupu and Wallace 2019, 4). Attitudes and actions towards reform may depend upon whether a citizen considers international or U.S. constitutional law more legitimate and how they choose to react to alleged violations of either source of law.

Rights Gap Theory

The Power of Legal Norms

On one hand, citizens may desire to narrow the gap between their country's constitutional and/or international promises and its actual practices. Legal theorists consider the law to be a "unique form of social organization" that is consistent and reliable because it provides "rules and the consequences of violating those accepted terms" (Wallace 2013, 111). The law determines what is considered appropriate government action, raises the value placed on rights, and mobilizes the populace to improve practices. In the field of international law, scholars view legal norms as following a "spiral model" of domestic implementation, in which international actors "from above" mobilize citizens "below" to demand respect for the guarantees of ratified agreements (Davis et al. 2012, 205; Risse and Sikkink 2009, 26; Simmons 2010, 35). Thus, ratifying a law is like "a pledge of a nation's reputation" because ratification is "embedded in a broader system of socially constructed... rule-making, normatively linked by the principle of *pacta sunt servanda*, in that agreements of a legally obligatory nature must be observed"

(Simmons 2010, 8, 9). It is well-established among legal scholars that formal agreements engender legitimacy and socialize countries to conform to legal standards, yet political science theories add nuance by illuminating the role of domestic politics in enforcing rules.

Political scientists contend that, when citizens learn about government abuses, their approval of the government decreases. More to the point, citizens might call for reform and punish leaders for breaking legal norms (Davis et al. 2012; Risse and Sikink 2009) because of the harm a violation causes “a country's global legitimacy, a leader's competency, and domestic audiences' normative ideals of upholding the law” (Simmons 2010, 9). Such punishment from citizens, known as domestic audience costs, can take the form of protests against the government or the removal of incumbent leaders. Citizens might also use the law “in litigation directly... cited as an authoritative legal source” and thus a “tool in local court cases” against the government (Simmons 2010, 41). To avoid these political and legal risks, government officials tend to make “tactical concessions” to avoid further undesirable criticism, such as stopping visible abuses or confirming their commitment to legal agreements (Davis et al. 2012, 206; Risse and Sikink 2009, 12). Therefore, citizens may demand respect for constitutional and/or international law and express their disapproval of immigration detention practices that violate either source of law.

For example, Lupu and Wallace's “International Legal Constraints” hypothesis predicted that “violations of international human rights law decrease public support for the government” (2019, 6). In order to test this theory about the importance of compliance and consistency with the law, my two “Rights Gap” Hypotheses are:

H1: Awareness of a U.S. Federal Court deciding that immigration detention violates the U.S. Constitution will decrease support for immigration detention and increase support for reform, compared to control.

H2: Awareness of an International Court deciding immigration detention violates the International Covenant on Civil and Political Rights will decrease support for immigration detention and increase support for reform, compared to control.

Backlash Theory

Resistance to International Law

On the other hand, citizens might justify that their government has control over its domestic immigration policy and that international law has little to no bearing on whether practices are acceptable. When legitimate actors shame countries for violating international law, it might “increase nationalism and backlash against criticism” because shamed countries engage in “norm-offending behavior to counter a sanctioning agent, such as an international organization” (Terman 2020 cited in Chapman and Chaudoin 2020, 1308). In other words, countries and their citizens might develop an identity from their stigmatized status and continue to break legal norms, such as “Donald Trump, [who] famously violated a number of hallowed norms,” and “political movements – from Brexit to the Alt-Right” (Terman 2020, 619). Political psychologists also describe how exposure to information contradicting one's prior beliefs can cause a "backfire" effect, in which individuals stick to their views more strongly than before (Nyhan and Reifler 2010, 307). Thus, Risse and Sikkink's “spiral model” mentioned above, in which legal institutions influence domestic politics (2009, 26), might only apply when a country's citizens do not feel threatened by international legal norms or identify with isolationist domestic policy.

Because the U.S. government historically claims that immigration detention is necessary to protect homeland security and to exercise U.S. independence, citizens might continue to approve practices and uphold the status quo. A shamed government might employ “norms of sovereignty

and nonintervention to argue against allegations” (Davis et al. 2012, 208), such as that immigration detention protects against “dangers to the community” or “national security” (8 C.F.R. § 1236; *Trump v. Hawaii* 2014). Just as pro-compliance actors can gather support for reform, so too might other actors “who oppose compliance[,] rally public support to their side by accusing an institution of bias” or by warning about civil unrest (Chapman and Chaudoin 2020, 1309). For example, some Americans use “protectionist rhetoric to justify creating trade barriers, while others have pointed to the challenge of global competition as a justification for reluctance to markedly raise wages or improve working conditions” (Hertel et al. 2009, 455). The use of torture has also been conditionally accepted when said to only be used against enemy combatants and terrorists (Chilton and Versteeg 2016; Wallace 2013). Given these arguments and theories, some citizens may defend detention for its use against undocumented immigrants.

Furthermore, research suggests that more conservative citizens may drive results in which international legal norms have a weaker effect upon public opinion or behavior than constitutional legal norms. Through a causal experiment, Wallace found that liberals tended to focus upon “principles of equality and community,” which coincides with “multilateralism and international institutions,” while conservatives held more intense skepticism of such institutions and promoted greater self-reliance (2013, 115). For these reasons, Lupu and Wallace proposed a “International Legal Backlash” hypothesis, testing whether “violations of international human rights law increase public support for the government,” as an alternative to their “International Legal Constraints” hypothesis mentioned above (2019, 6). I additionally propose that international law will be considered a threat to U.S. control over its domestic affairs, which are primarily guided and enforced by the U.S. Constitution and other documents internal to the United States (not the ICCPR or international treaties). In the wake of the isolationist political

agenda of the Trump Administration, U.S. citizens may consider international legal standards to be less legitimate than norms deriving from the U.S. Constitution. Thus, I expected greater resistance to international law than constitutional law with my third “Backlash” hypothesis, which compared the magnitude of the effects from *H1* and *H2*:

H3: Compared to awareness of U.S. constitutional violations, awareness of international violations will have weaker effects on support for immigrant detention and support for reform.

In sum, competing theories about narrowing the rights gap and backlashing against international norms offer possible ways in which citizens might respond to information about immigration detention violating the U.S. Constitution and the ICCPR. Both treatment groups made aware of the rights gap between ratified law and the reality of immigration detention may be more likely to disapprove of detention and advocate for reform. However, learning about U.S. constitutional violations might lead to greater disapproval of immigration detention and action toward its reform than international standards, which might appear to interfere in U.S. domestic affairs.

IV. Experimental Design

Study Population and Survey Platform

I administered my survey experiment online through the survey platform, Lucid Theorem (or Lucid), obtaining a sample size of 1,388 participants. The study population of interest was foreign-born or U.S.-born adult citizens, 18 years or older. The sampling frame was demographically proportionate to the U.S. population in terms of gender, race/ethnicity, age, region, etc.² Research also indicates that, compared to MTurk, another online survey platform, Lucid generates a “much larger pool of subjects” and a sample more similar to U.S. “demographic, political, and psychological profiles” (Coppock and McClellan 2019, 12). Therefore, although several of the survey experiments I build upon used MTurk (Chaudoin 2014; Chilton 2014; Kreps and Wallace 2016; McEntire et al. 2015), I chose Lucid because of its more representative national probability sample that would have greater external validity.

Specifically, compared to MTurk samples, Lucid samples prove to more closely match the gender, education, age, income, and voter registration and turnout of participants in the 2012 American National Election Survey (ANES) (Coppock and McClellan 2019). Cited throughout this paper, the 2012 ANES is a reliable survey source in the political science field and I pull several of my demographic questions from the instrument. In terms of politics and ideology, Lucid tends to have participants who are most interested in politics, with 2012 ANES having the second most politically interested participants and MTurk having the least politically interested participants. Lucid participants also poll more conservatively than MTurk participants, who are younger and more liberal (Berinsky et al. 2012; Coppock and McClellan 2019), so Lucid participants may be relatively less skewed to the political left. Also, more conservative,

² See *Appendix B* for demographic comparison of my Lucid sample and the U.S. population.

Republican-affiliated citizens are precisely those who might backlash and continue to support immigration detention. I wanted to capture this key demographic group and test whether the information in my survey could shift their attitudes and actions toward reform too.

Lucid also addresses several issues with MTurk over the years. Scholars worry that MTurk workers are unrealistic survey experts who focus on the quantity of surveys they complete and on the money they earn. In 2018, the academic community learned that many MTurk respondents were “fraudulent bots” or were individuals who formed a community online that shared survey-taking shortcuts online (e.g., Turkernation) (Coppock and McClellan 2019, 2). Although Captcha screener responses could be used to screen out non-human participants, Lucid can avoid the above internal validity concerns because “the risk of cooperation among subjects is minimal given their diverse sources” in separate online spaces and “subjects are less professionalized” (Coppock and McClellan 2019, 12). Lucid participants take an average of 4.28 surveys per month, with 98% of participants taking fewer than one survey per day and 2.43 surveys per month. Also, 94% of Lucid participants prove to take their surveys at home and receive an average of \$1 per completed response, which is well above the average payment on MTurk (Coppock and McClellan 2019, 4). My Lucid survey experiment may have slightly more internal validity than an MTurk-based study, which would raise greater concerns about survey corruption.

Given that an online survey sample is the most efficient use of my limited resources, I intentionally selected Lucid because of its well-researched reputation in social science studies. Each participant read background information, a follow-up paragraph of legal text (if in the treatment group), a series of outcome questions, demographic questions, and a debrief. The estimated time to complete the survey was 6-8 minutes, presenting a minimal time burden.

Experimental Variables

I tested the relationship between legal norms and U.S. public opinion and behavior. The independent variable is awareness about a legal violation, which I operationalized through a treatment vignette with a citation of how immigration detention violates either the International Covenant on Civil and Political Rights (ICCPR) or the U.S. Constitution. The dependent variable is public responses to immigration detention, measured as the degree to which U.S. citizens approve of immigration detention and its reform and how likely they are to engage in reform. Specifically, participants self-reported their attitudes about immigration detention itself and immigration detention reform, and expressed their likelihood of signing a petition to Congress and contacting a member of Congress.

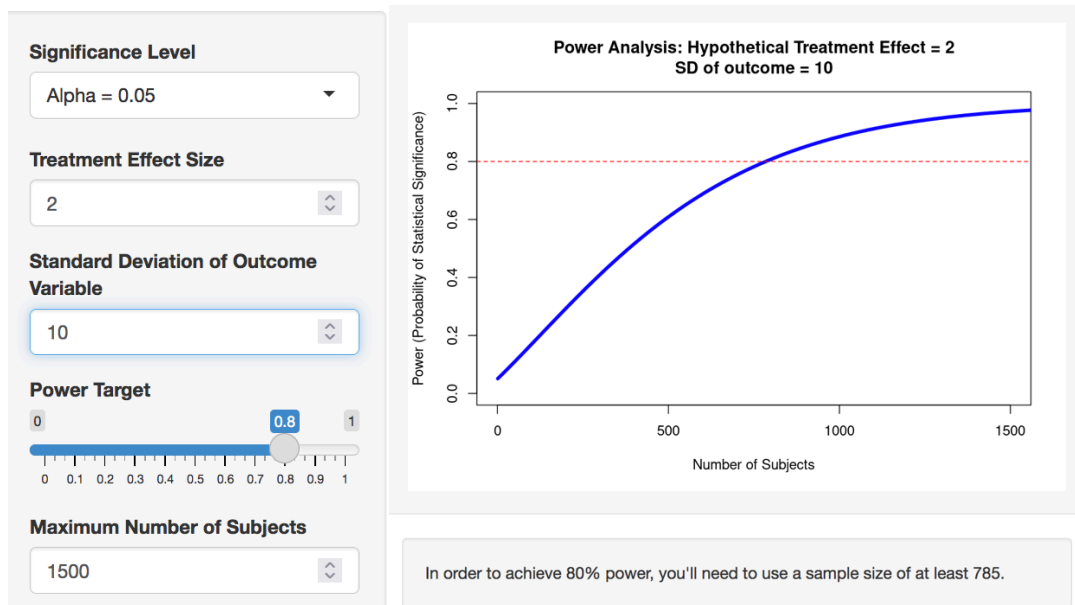
Although experimental randomization eliminates the need for control variables because there should be no significant differences between experimental groups, I decided to include certain covariates both to aid in explaining variation in the outcomes and to improve precision in the estimation of treatment effects. All Lucid participants provide their race, ethnicity, nativity, partisanship, ideology, census region, 5-digit zip code, education level, household income, gender, and age before taking part in any study. I created two additional demographic variables of participant nativity and parent nativity because of the potential effect of immigration heritage upon immigration policy preferences. Lucid's demographic variables have been used in model studies, as discussed in the *Literature Review* section, so I believe they are both reliable and valid. The above covariates allowed me to isolate the causal effect of my treatment and to look closely at the interaction between the treatments and specific covariates later on, including partisanship and attentiveness.

Power Calculations

Using Coppock's *Evidence in Governance and Politics (EGAP) Power Calculator*, I calculated the power for my survey experiment (2013). I specified a few aspects of my experimental design to do so. First, I used a significance level at the standard $\alpha = 0.05$. I kept the power target at the standard 0.8. I made the maximum number of subjects I would recruit as 1,500 based on the maximum grant funding I could obtain. Then, I estimated the treatment effect size and standard deviation of my outcome variable. The relationship between these two important measures can be thought of in terms of standardized effects: the treatment effect size divided by the standard deviation.

Standardized effects of 0.2 are considered small-medium sized effects and are usually the benchmark used for power calculations in survey experiments. Other survey experiments similar to mine, such as Chilton (2014) and Wallace's (2013), found standardized effects of between 0.2-0.4, so I used a standardized effect of 0.2 in my power analysis. I made the treatment effect size equal to 2 and the standard deviation equal to 10 ($2/10 = 0.2$), resulting in a standardized effect of 0.2, which was my best guess for the size of the treatment effects in this study. With this information, the *EGAP Power Calculator* stated that I needed about 393 subjects per group and, if I were comparing only two groups (treatment and control), I needed a sample size of at least 785 (see *Figure 1* below). However, because I had a total of three comparison groups, two treatments and one control, I needed at least 1179 subjects (393×3) for sufficient power at the 0.8 target level.

Figure 1: Power Calculation Graph



Methods

I designed a survey experiment using Qualtrics, a popular online survey tool, which was distributed to participants who were recruited by Lucid. The survey began by providing participants with information about the survey itself, to ensure participants made an informed decision about whether to consent to participate. For those who gave their consent, the survey continued by asking one demographic question and one screening question, which recorded their level of attentiveness. Extensive research illustrates the power of “attention checks” to prevent null results largely from uncommitted participants by requiring subjects to demonstrate that they are carefully reading the prompts and survey questions (Berinsky et al. 2014, 2016, 2021). I included another screening question right after a demographic question in a later section of the survey to measure participants’ level of attentiveness throughout the survey. I did not screen out survey participants who got both screening questions incorrect because I planned assess the interaction effect between attentiveness and the treatments. Thus, I analyzed the results at different levels of attentiveness, or getting 0, 1, or 2 (all) of the screening questions correct.

Survey Instrument

The following section outlines my survey instrument.³ I put a demographic question about age with an answer choice text box allowing only numeric values from 18 to 100, since only U.S. adults over 18 years old could participate in my survey. All participants then read a question with an intentionally lengthy, detailed prompt to check whether they read the questions carefully. I modeled the “attention check” question after those rigorously tested as a valid measure of attentiveness (Berinsky et al. 2014, 2016, 2021). I placed the question about age before the “attention check” question to make the latter, which was about their feelings, less obvious to participants, since both questions seemed to be about their personal information.

After completing the first attention check sequence, participants proceeded to the sections of the survey experiment directly relevant to immigration detention. All participants read background information about immigration detention, which came directly from a credible source, the Bipartisan Policy Center (Sullivan and Mason 2019). Using the neutral language prevented priming participants in any way or appealing to the beliefs of a certain political party or ideology.

The last two sentences of the background information replicated the survey vignettes of Wallace (2013) and Chilton and Versteeg (2016) on the subject of torture, and of Chilton (2014) on the subject of solitary confinement. Chilton’s background vignette stated the following: “Supporters of the use of solitary confinement argue that its use is necessary to maintain prison discipline and ensure the safety of prisoners and guards alike... Critics of the use of solitary confinement argue that it should be eliminated...” (2014, 135). I shifted the focus of my background vignette to immigration detention and common justifications of its use from the

³ For an exact copy of the survey instrument, including the consent portion, see *Appendix D*.

government, private detention corporations, and other authorities (immigrants are a danger to the community, 8 C.F.R. § 1236; immigrants are a national security threat, *Trump v. Hawaii* 2018). I placed a 20 second timer on the survey's background section before participants could proceed to the next part of the survey to ensure that participants took the time to read the information carefully. Otherwise, participants could speed through and miss the context of the background section, which is especially important when the control group does not read any further information.

Table 1: Number of Respondents per Group

	Control	Constitutional	International
Number of subjects	471	453	464

Next, participants were randomly assigned to one of three groups: either the control group or one of the two experimental groups (see *Table 1*). While the control group only read the initial background prompt, remaining participants were randomized into one of the experimental groups and given additional information about how immigration detention practices violate ratified U.S. law. Across the two treatment groups, I varied the particular source of law that a court decides is at issue in immigration detention. One treatment group read that immigration detention violates the U.S. Constitution, while the other treatment group read that immigration detention violates the ICCPR. In the vignettes that both of the treatment groups read, I included a reference to the title of a legal instrument, the specific provision of that legal instrument pertaining to cruel treatment in confinement, and the authoritative source with subject matter jurisdiction over that legal instrument. The only variation in the treatment vignettes was whether the norm in violation is international law or U.S. constitutional law. I included a shorter, 10 second timer on each of the treatment vignettes before participants could proceed to the next part

of the survey for the same reasons stated above, though I accounted for the briefer nature of this section.

After reading the background information and relevant prompts, participants in all groups were asked a series of outcome questions. Firstly, they indicated their level of approval of immigration detention on a five-point Likert scale. Answer choices ranged from strongly approve to strongly disapprove, just as Chilton (2014), Chilton and Versteeg (2016), Cope and Crabtree (2020), and Wallace (2013) included in their surveys. I allowed participants to give the middling response of “neither approve nor disapprove” to offer a wider range of options and to relieve any pressure to give a strong response on either end.

Then, participants in all groups were given background information about a proposed immigration detention reform, specifically community-based case management, which offers alternative housing arrangements while immigrants await court decisions. The first sentence in this background information section replicated a portion of Chilton’s 2014 survey vignette, although I changed the subject of the reform. Chilton informed his participants that “American lawmakers have been considering reforms that would eliminate the use of solitary confinement...” (2014, 135). I focused upon an alternative to detention program for which human rights organizations, political representatives, and grassroots activists, such as HRW, Amnesty International, and Detention Watch Network, have advocated over the years (i.e., Senator Booker and Rep. Jayapal’s *Dignity for Detained Immigrants Act*).

The second sentence in this background information section came from the “background” section of the final report of the DHS Office of the Inspector General on the Family Case Management Program, which was a piloted and quite successful alternative to detention program (FCMP) (2017, 2). However, I did not mention ICE by name, as the official government

document does, just as I do not include the name of particular judicial institutions in the treatment vignettes, to prevent priming participants who may have certain associations with them. If I did name ICE, participants might apply their prior beliefs towards the agency, especially since polls indicate that attitudes toward the agency itself are heavily polarized, as explained in the *Literature Review* section.

While the background information about the proposed alternative to detention remained on the survey page, participants answered follow-up questions. Firstly, participants expressed their degree of approval for shifting immigrants from detention to alternative housing arrangements while they await court decisions with a five-point Likert scale. Answer choices ranged from strongly approve to strongly disapprove, just as Chilton (2014), Chilton and Versteeg (2016), Cope and Crabtree (2020), and Wallace (2013) included in their surveys. I allowed participants to give the middling response of “neither approve nor disapprove” for the same reasons described above.

Next, participants in all groups were asked about their willingness to take action in support of immigration detention reform. They indicated how likely they were to sign a petition urging Congress to approve of the reform and/or to individually contact their Congressional representative to support the reform. Potential answer choices ranged from not at all likely to extremely likely, just as Anjum et al. (2021), Hertel et al. (2009), and McEntire et al. (2015) included in their survey experiments. I allowed participants to give the middling response, “likely” in this case, for the same reasons described above. However, prior researchers asked participants about their willingness to act through voting, donating, and signing petitions to improve various rights. I chose two reform efforts that vary in level of commitment and that describe real-world advocacy options. While signing a petition is quicker and offers group

solidarity, contacting Congressional representatives is a more individual and time-consuming task. I also do not know whether one form of action might resonate more strongly with a survey participant, so I provide two options in the case that one does.

Next, all participants answered the second and last screening question, which was surrounded by demographic questions to make the attention check less evident. I asked a demographic question about gender. I took inspiration for this question from the 2012 American National Election Survey (ANES), which asked participants, “Are you male or female?” (self_gender variable, ANES 2014, 737). The second attention sequence follows that asks for one’s favorite color, as used by Berinsky et al. (2014, 2016, 2021). I also asked additional demographic questions about each participant’s birthplace and their parents’ birthplace, since Lucid requires participants only to provide their race, partisanship, census region, education level, household income, gender, age, and Hispanic ethnicity. I pulled the two new questions directly from the 2012 ANES too (dem_nativity and dem_parent variables, ANES 2014, 686). Asking participants about their nativity and their parent(s) nativity likely does not make participants worry about personal privacy or security concerns, as when asked about their citizenship status.

Finally, I included a debrief section at the end of my survey to elucidate the hypothetical, experimental nature of my study and to share the ways in which participants can affect immigration detention reform in the real world. Amnesty International has a campaign in support of the *Dignity for Detained Immigrants Act*, the bill I describe in the survey’s background prompt. Thus, I provided citizens with the organization’s online petition and a template to use to contact one’s congressional representatives. I hyperlinked these resources in the survey.

Strengths and Limitations

Notably, one of the greatest strengths of survey experimental research is the high degree of internal validity, which allowed me to make causal claims due to randomized treatment assignments. Survey experiments are easy to implement and, because of the establishment of cause and effect, they “can provide firmly grounded inferences about real-world political attitudes and behavior,” as compared to conventional survey research (Gaines et al. 2006, 2). The survey component also allows researchers to more easily recruit larger numbers of participants and incorporate experimental manipulations into national surveys. As mentioned, Lucid costs very little per participant and generates samples with a demographic makeup similar to a national sample. Furthermore, random assignment to any one treatment enables comparison across groups who are balanced on all other observed control variables and unobserved factors. Unlike observational studies about the impacts of ratifying constitutions or international laws, my study gauged the causal relationship between raising awareness of legal violations and public opinion. Randomization also facilitates more valid inferences, in that any differences between the groups are caused by the treatments and not any pretreatment differences.

However, I do recognize the limitations of experimental surveys like mine. Key threats to inference include external validity, or generalizability, and social desirability bias. Firstly, although there is a good ability to generalize from my representative Lucid sample to the U.S. population,⁴ there is evidence that the effects of experimental survey manipulations may not endure for long in real life (Barabas and Jerit 2010). However, for the purpose of this survey experiment on a largely under-researched topic, I am more concerned with whether an effect exists at all and less with the duration of that possible effect. Also, my study’s results are limited

⁴ See *Appendix B* for Demographic Comparison Table 10, which illustrates the impressive similarity between my Lucid sample and the U.S. population.

to the United States because the language of the vignettes and the common arguments for and against immigration detention are unique to the government agencies and policies in this country. Finally, survey experiments can sometimes provide too clean a stimulus compared to the “noisy” environment that individuals face, oversimplifying human issues that are changing and complicated. For example, during the pandemic, immigration policy changed greatly across various presidential administrations. Immigration detention is also only one dimension of immigration policy and practice in the United States.

Social desirability bias is a concern in my survey research, wherein participants may not be honest about their true attitudes or actions when asked about sensitive topics. Instead, they may complete the survey with the knowledge that they are experimental subjects (known as the Hawthorne Effect). Other researchers have illustrated this phenomenon with regard to sexual activities and racism (Krumpal 2013). Immigration detention is a similarly politicized subject and, in highlighting the alleged cruel and unusual treatment of immigrants in detention facilities, it may be upsetting to learn about as well. However, the nature of my experimental conditions imply that social desirability will likely be present and balanced across all participants. I am more interested in whether there are meaningful differences between the treatment groups. While the presence of social desirability bias likely exists in my research design, it does not hinder me from making modest conclusions about whether legal norms impact support for certain practices and likelihood to promote reform. In fact, my survey experiment may serve as a tougher test of this relationship, showing how modest experimental effects may translate into greater real-world effects.

Furthermore, simply testing whether participants express disapproval of a practice and whether they would advocate for reform in an anonymous, online survey may not accurately

measure whether participants would do so in real life. Participants may just be stating that they hold certain attitudes or would engage in reform when, in reality, they do not do so or would not do so (Hertel et al. 2009; McEntire et al. 2015). Over-reporting of support for human rights issues is especially possible when there is little commitment to following through on one's self-reported support or opposition to a cause. However, the behavioral measures I include and the in-between answer choice for participants to select (i.e., neither approve nor disapprove) can demonstrate more apathy toward or disapproval of detention. Participants who indicate that they are likely to petition or call upon Congress and who mark more extreme responses in opposition to immigration detention do go a step above the average citizen. I also include a debrief section that offers survey participants opportunities and resources to actually impact immigration detention reform through a template for contacting their representative(s) and a petition for urging Congress to enact an alternative to detention. Future research could conduct in-depth, qualitative follow-up interviews into whether participants actually take action after a survey experiment like mine.

Nonetheless, while the effect size may not ultimately be very large, small changes in public opinion have a comparatively large policy impact. As Gilens (2005) notes, the relationship between public opinion and policy impact is S-shaped, where the key inflection point for public influence is around 50%. Similarly, Page and Shapiro (1983) explain that small treatment effects can greatly increase whether official policy will become more congruent with public sentiment, further suggesting the substantive impact of legal norms on immigration detention policy attitudes and behavior. The absolute size of the expected effect is similar across experimental studies on international law that use the same 6-point Likert scale of approval for a practice, such as Wallace (2013; 6%), Chilton (2014; 4%), and Lupu and Wallace (2019; 4-8%).

Although a treatment effect in either direction may not be enormous, such changes can have disproportionate implications in situations where public support is relatively divided. Overall, the beneficial outcomes of my survey experiment trump the limitations when observational, macro-level studies and recent survey experiments have not conducted a socially relevant, multi-faceted survey experiment about immigration detention.

V. Results

In this section, I will present the descriptive statistics of my sample, the main regression results, the two-sample z-tests for a backlash effect, and the interaction effects of attentiveness and partisanship to show whether and how legal norms influenced public opinion and political behavior in regard to immigration detention.

Descriptive Statistics

Firstly, I discuss the descriptive statistics of the full sample (N = 1,388). Taking one outcome measure at a time, I interpret *Table 2* and *Figure 2* together. I describe the central tendency (mean) and the overall sample distribution of the full sample, as well as whether and how there are apparent differences in the distributions across the four outcome measures.

Table 2: Basic Descriptive Statistics for Each Outcome Measure⁵

Group	N	Mean	Standard Deviation
Detention Support⁶			
Full Sample	1,388	3.30	1.25
Control Group	471	3.38	1.25
Constitutional Group	453	3.24	1.25
International Group	464	3.28	1.26
Reform Support⁶			
Full Sample	1,388	3.24	1.28
Control Group	471	3.28	1.31
Constitutional Group	453	3.31	1.24
International Group	464	3.15	1.26
Petition Support⁷			
Full Sample	1,388	2.34	1.30
Control Group	471	2.36	1.32
Constitutional Group	453	2.42	1.34
International Group	464	2.26	1.24

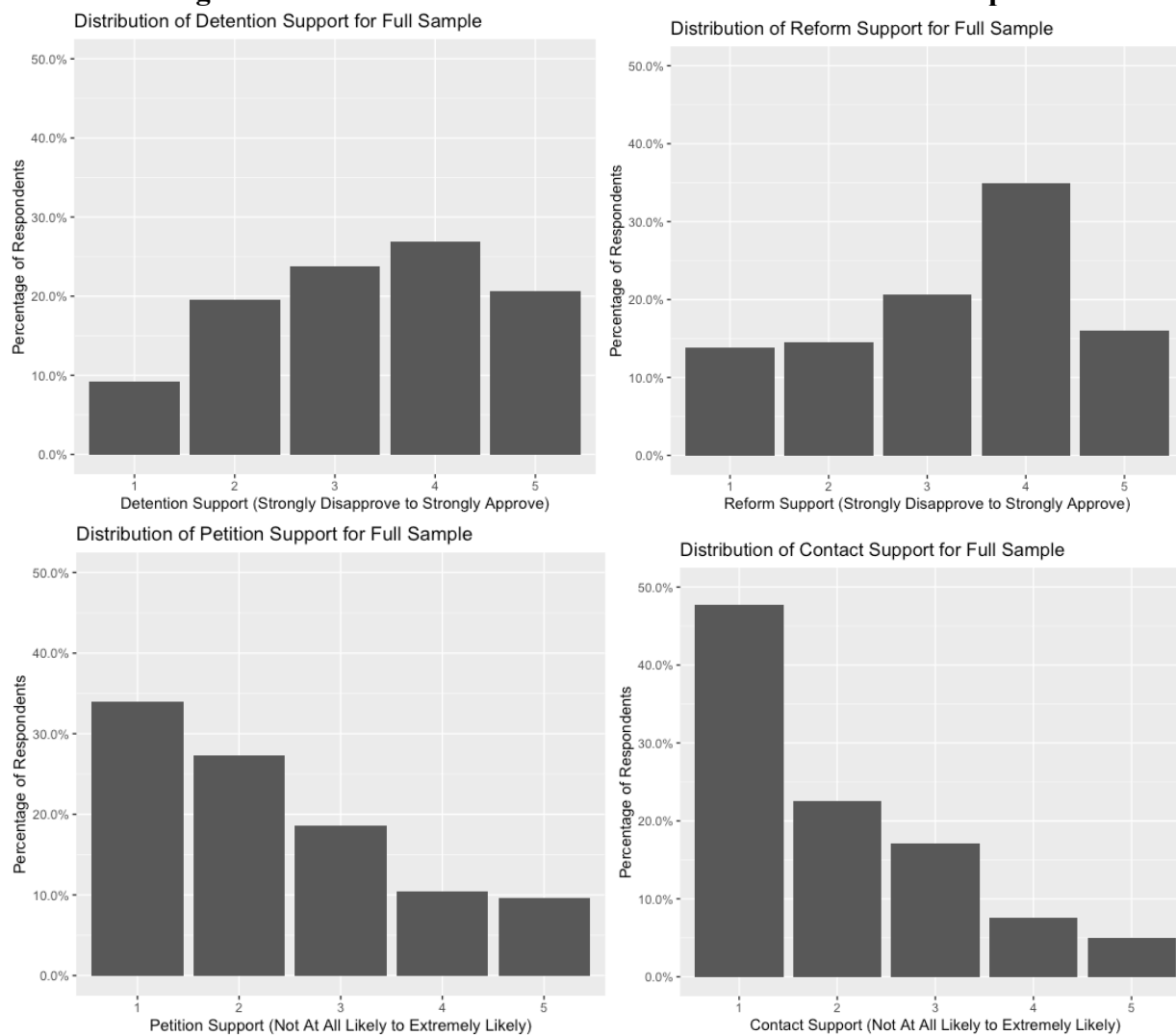
⁵ See *Appendix A* for Covariate Balance Table 8 and F-tests with p-values indicating balance across experimental groups. Also in *Appendix A* are Figures 6-9, illustrating the sample distribution of each group (control, constitutional, and international).

⁶ The Detention Support and Reform Support outcome measures are coded on a five-point Likert scale from 1 to 5, or from *Strongly Disapprove* to *Strongly Approve* coded in a reverse order to how they appear in the survey answer choices).

⁷ The Petition Support and Contact Representative Support outcome measures are coded on a five-point Likert scale from 1 to 5, or from *Not At All Likely* to *Extremely Likely*.

Contact Representative Support ⁷			
Full Sample	1,388	1.99	1.18
Control Group	471	1.97	1.15
Constitutional Group	453	2.06	1.22
International Group	464	1.96	1.19

Figure 2: Distribution of Each Outcome Measure for Full Sample



In the full sample, the sample distributions for the two attitudinal outcome measures are quite evenly divided when, on average, most respondents were divided in their support for immigration detention and its reform (see *Figure 2* above). Meanwhile, the sample distributions for the two behavioral outcome measures are skewed to the right and responses are clustered lower on the Likert scale, as most respondents were not very likely to engage in reform efforts.

The average Likert scale response for Detention Support is 3.30, as most respondents *Neither Approve Nor Disapprove* of detention. The average Likert scale response for Reform Support is 3.24, as most respondents *Neither Approve Nor Disapprove* of reform (a community-based alternative to detention). Finding that latent public opinion about immigration detention and its reform clusters around the mid-way point of the Likert scale (50%) in my study reflects other national probability survey results. For example, in one of the few polls about immigration detention attitudes, 59% of likely 2020 voters supported suspending new detentions and 51% supported limiting the detention of newly arrived immigrants (Chapin 2020). Though I discuss the role of partisanship in my sample later in this section, another national public opinion poll found that 68% of Democrats want to close detention centers and 60% of Independents offered support for this reform, while only 44% of Republicans did so (Newall 2021).

Furthermore, the average Likert scale response for Petition Support is 2.34, as most respondents are *Somewhat Likely* to sign a petition to replace detention with a community-based alternative. The average Likert scale response for Contact Representative Support is 1.99, as most respondents are *Somewhat Likely* (and nearly *Not At All Likely*) to contact their representative to replace detention with a community-based alternative. The more non-committal responses to the behavioral outcome measures reflect what McEntire et al. (2015) also found in their survey experiment: “action mobilization” requires a greater investment of time and energy, which many respondents do not engage in because there are few direct, personal benefits from helping detained immigrants. The lower likelihood of participating in more time-intensive reform efforts further explains why more respondents would sign a petition (mean of 2.34), a quick and easy-to-join group effort, than contact political officials (mean of 1.99), a more individualized task of sharing one’s contact information and particular views.

Covariates and Balance

I controlled for the following demographic covariates in my analysis: race, nativity, partisanship, region, education level, household income, gender, age, and Hispanic ethnicity. These characteristics were coded according to the information provided by Lucid based on the demographic data it collects from all respondents before they take surveys. In addition, I asked respondents about their nativity and their parent's nativity (place of birth) with the same three answer choice options for both questions: a U.S. territory, a U.S. state or D.C., or another country. I created a balance table to assess covariate balance across treatment groups,⁸ looking across groups to see if differences stand out and imply that there is a greater proportion of certain individuals in one group than in another. Ultimately, I confirmed that the experimental randomization worked as intended and that I did not have different types of respondents in different groups.

F-statistic tests are a more succinct way to interpret information that a balance table simply illustrates in proportions and percentages for each covariate. F-tests regress a treatment indicator on all of the covariates to see if any predict the treatment. The null hypothesis for each of my F-tests was that the coefficients on the covariates are each equal to zero, meaning I expected that none of the covariates should predict treatment. Indeed, balance was supported by failing to reject the null at the 5% significance level and by finding no significant differences across the experimental groups.⁹

Rights Gap Hypotheses

⁸ See *Appendix A* for Covariate Balance Table 8.

⁹ See *Appendix A* for F-tests with p-values indicating no statistically significant differences across groups.

To test my two Rights Gap Hypotheses, I utilized multivariate regression because it allowed me to control for the covariates listed above and to measure the degree to which my independent variables (legal norms and other covariates) are related to my dependent variables (four outcome measures). The associated regression specification for *H1* and *H2* is:

$$Outcome = \beta_0 + \beta_1 \text{constitutional violation} + \beta_2 \text{international violation} + \gamma X + \epsilon$$

where β_1 and β_2 are the estimated average treatment effects (ATEs), *constitutional violation* is an indicator for receiving the constitutional legal violation treatment and *international violation* is an indicator for receiving the international legal violation treatment, X refers to a vector of the covariates, and ϵ refers to the error. I ran this model specification four times for the four outcomes of interest: support for immigration detention and reform (an alternative to detention) as well as likelihood to sign a petition and contact one's representative.

For the Constitutional Rights Gap Hypothesis (*H1*), the estimand of interest (β_1) is an ATE based on comparing the group that received the treatment with the constitutional legal violation to the control group that received no legal information. I predicted that awareness that immigration detention violates the U.S. Constitution would decrease support for immigration detention and increase support for reform, compared to control. For the International Rights Gap Hypothesis (*H2*), the estimand of interest (β_2) is an ATE based on comparing the group that received the treatment with the international legal violation to the control group that received no legal information. I predicted that awareness that immigration detention violates the ICCPR would decrease support for immigration detention and increase support for reform, compared to control. I expected the signs on β_1 and β_2 to be negative for the immigration detention support outcome and positive for the reform support outcome. I also expected the signs on β_1 and β_2 to

be positive for the two behavioral outcome measures, likelihood to sign a petition and to contact one's representative.

Figure 3: Main Coefficient Plot, Including Covariates

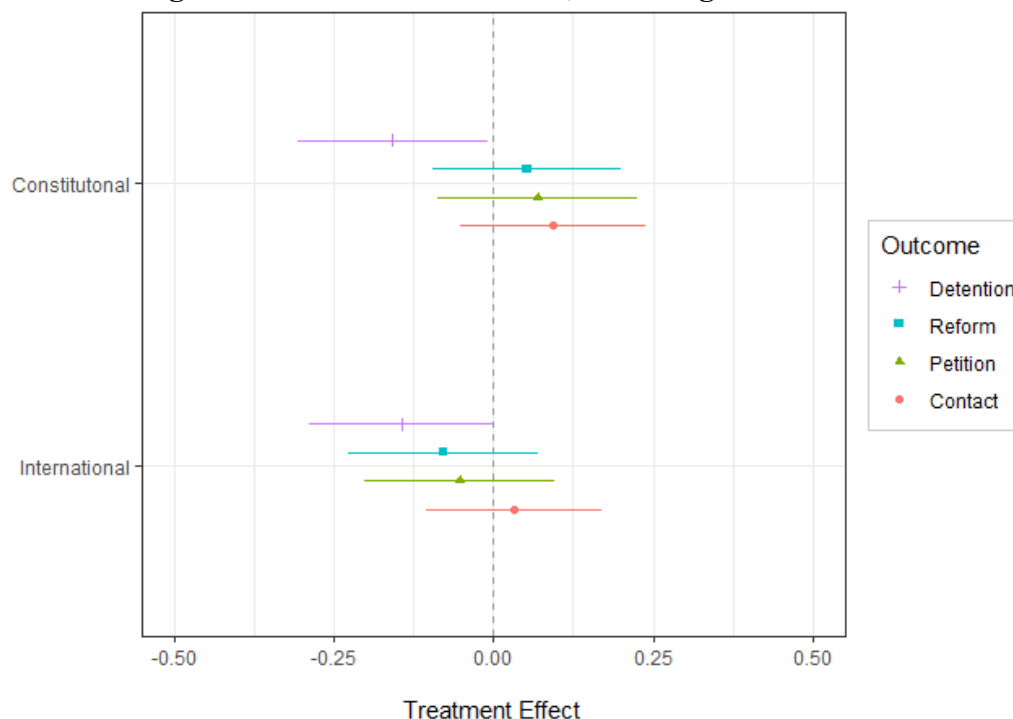


Table 3: Main Regression Results, Including Covariates¹⁰

	Detention Support (1)	Reform Support (2)	Petition (3)	Contact Rep (4)
Constitutional	-0.158** (0.076)	0.052 (0.076)	0.069 (0.080)	0.094 (0.074)
International	-0.144** (0.074)	-0.078 (0.076)	-0.052 (0.076)	0.032 (0.070)
Age ¹¹	0.012***	0.015***	-0.017***	-0.014***

¹⁰ See Table 10 in *Appendix C* for main regression results with three-level partisanship (Democrats, Republicans, Independents). This *Results* section contains the main regression results with all ten of Lucid's partisanship categories, as I committed to in my Pre-Analysis Plan on *Open Science Framework*. The results are similar in magnitude and statistically significant in both Table 2 and Table 10.

¹¹ While there are interesting and statistically significant results for several other covariates (Age, Gender, Nativity, and Region), this paper does not analyze them in detail. My Pre-Analysis Plan on *Open Science Framework* focused upon the heterogeneous effects of Partisanship and Attentiveness, which related most to my theories about what characteristics might drive people to narrow the rights gap or backlash legal criticism.

	(0.002)	(0.002)	(0.002)	(0.002)
Female Gender	-0.318***	0.248***	0.169***	-0.028
	(0.063)	(0.063)	(0.065)	(0.060)
Other Gender	-0.199	1.019**	1.063*	0.649
	(0.750)	(0.475)	(0.611)	(0.643)
US Territory Born	0.153	0.111	0.008	0.152
	(0.185)	(0.181)	(0.184)	(0.181)
Foreign Born	-0.288*	0.206	0.350**	0.174
	(0.162)	(0.168)	(0.174)	(0.155)
US Territory Born Parent	0.237	-0.058	0.059	0.191
	(0.194)	(0.189)	(0.186)	(0.193)
Foreign Born Parent	0.190	-0.104	-0.033	0.118
	(0.134)	(0.142)	(0.143)	(0.132)
Strong Democrat	-0.364***	0.855***	0.886***	0.676***
	(0.112)	(0.102)	(0.115)	(0.101)
Not Very Strong Democrat	-0.184	0.375***	0.414***	0.217*
	(0.123)	(0.115)	(0.128)	(0.112)
Independent Leaning Democrat	-0.481***	0.713***	0.459***	0.296**
	(0.124)	(0.116)	(0.134)	(0.122)
Independent Leaning Republican	0.319***	-0.307**	-0.131	-0.093
	(0.122)	(0.133)	(0.124)	(0.110)
Other - Leaning Democrat	-0.089	0.425	0.638*	0.436
	(0.334)	(0.354)	(0.358)	(0.292)
Other Party	-0.202	-0.073	0.011	0.015
	(0.138)	(0.142)	(0.145)	(0.131)
Other - Leaning Republican	0.494***	-0.270	-0.412**	-0.017
	(0.188)	(0.184)	(0.179)	(0.186)
Not Very Strong Republican	0.308**	0.001	-0.151	-0.077
	(0.134)	(0.140)	(0.127)	(0.109)
Strong Republican	0.815***	-0.586***	-0.337***	-0.007
	(0.108)	(0.128)	(0.120)	(0.112)
Midwest	-0.179*	0.257***	0.138	0.108
	(0.094)	(0.096)	(0.097)	(0.093)
South	-0.115	0.220**	0.208**	0.185**
	(0.085)	(0.087)	(0.088)	(0.082)
West	-0.271***	0.111	0.038	-0.043
	(0.095)	(0.100)	(0.101)	(0.094)
Black	0.322***	0.084	0.009	0.276***
	(0.113)	(0.103)	(0.111)	(0.104)
American Indian or Alaska Native	-0.610**	-0.518*	0.117	0.104

	(0.291)	(0.291)	(0.285)	(0.338)
Asian or Pacific Islander	0.067	-0.160	-0.099	0.046
	(0.157)	(0.167)	(0.173)	(0.157)
Other Race	0.035	-0.024	-0.108	-0.168
	(0.139)	(0.128)	(0.138)	(0.126)
Middle Income	0.049	0.107	0.015	-0.117
	(0.077)	(0.073)	(0.078)	(0.071)
High Income	0.207**	0.039	-0.069	-0.109
	(0.089)	(0.089)	(0.093)	(0.085)
Other Income	-0.227	0.181	-0.189	-0.068
	(0.201)	(0.184)	(0.166)	(0.163)
Hispanic	-0.068	-0.038	0.186	0.111
	(0.124)	(0.129)	(0.139)	(0.125)
Some college or Associate Degree	-0.071	-0.039	0.013	0.056
	(0.085)	(0.086)	(0.088)	(0.081)
Bachelor's Degree	-0.157*	0.022	-0.032	0.015
	(0.085)	(0.084)	(0.088)	(0.082)
Graduate Degree	-0.016	0.061	0.065	0.240**
	(0.111)	(0.116)	(0.126)	(0.122)
Other Degree	-0.170	0.183	0.012	0.372
	(0.521)	(0.446)	(0.580)	(0.548)
Attentiveness	0.049	-0.048	-0.061	-0.126***
	(0.043)	(0.043)	(0.045)	(0.042)
Constant	2.997***	3.467***	2.745***	2.436***
	(0.159)	(0.160)	(0.168)	(0.157)
N	1,388	1,388	1,388	1,388
R2	0.196	0.234	0.203	0.174
F Statistic (df = 34; 1353)	9.695***	12.133***	10.127***	8.356***

*p < .1; **p < .05; ***p < .01

Notes: With robust SEs, including covariates

Table 4: Simple Regression Results, Excluding Covariates¹²

Detention Reform Petition Contact

¹² When excluding the covariates (*Table 4*), the same coefficients are no longer statistically significant and smaller in magnitude than when including covariates (*Table 3*). However, this difference is normal. Because my results are barely under 0.05 significance, removing the precision gain from the covariates increased uncertainty (the standard errors and p-values) in the covariate unadjusted model (Gerber and Green 2012). Removing covariates also changed the composition of the reference group to include everyone in the control group, decreasing the magnitude of the treatment effects. In line with conventional research papers, my Pre-Analysis Plan prepared to *include covariates*.

	Support (1)	Support (2)	(3)	Rep (4)
Constitutional	-0.135 (0.082)	0.029 (0.084)	0.052 (0.087)	0.087 (0.078)
International	-0.100 (0.082)	-0.134 (0.084)	-0.107 (0.084)	-0.007 (0.076)
Constant	3.378*** (0.057)	3.285*** (0.061)	2.363*** (0.061)	1.968*** (0.053)
N	1,388	1,388	1,388	1,388
R2	0.002	0.003	0.003	0.001
F Statistic (df = 2; 1385)	1.456	2.135	1.775	0.896

*p < .1; **p < .05; ***p < .01

Notes: With robust SEs, excluding covariates

Table 3 and *Figure 3* present the main regression results of the four outcome measures while controlling for covariates. There are statistically significant treatment effects for some, but not all, of the outcome measures. Support for immigration detention for those who received either treatment differed significantly from those who received no treatment ($p < 0.05$). On average, those in the constitutional treatment group are less supportive of detention than those in the control group by about 0.158 points on the Likert scale.¹³ The predicted average score for Detention Support in the constitutional treatment group is 2.52 on the Likert scale relative to 2.68 in the control group, setting all covariates to their samples means or modes.¹⁴ This translates to decreased Detention Support by 0.13 standard deviations after receiving the constitutional treatment. On average, those in the international treatment group are less supportive of detention than those in the control group by about 0.144 points on the Likert scale. The predicted average

¹³ The five-point Likert Scale for Detention Support is coded from 1 to 5, or from *Strongly Disapprove* to *Strongly Approve* of detention (higher value associated with increasing approval of detention). The mid-level, neutral response of Neither Approve Nor Disapprove of detention is coded as 3. See *Appendix D* for the “Outcome Detention” question (coded in a reverse order to how it appears in the survey answer choices).

¹⁴ Notably, the predicted average scores were lower on the Detention Support Likert scale than the actual averages in *Table 2* because setting the covariates to their sample means for the predicted average scores over-represented White, Strong Democrats (highest-average categories for race and partisanship).

score for Detention Support in the international treatment group is 2.54 on the Likert scale relative to 2.68 in the control group, setting all covariates to their samples means or modes.¹⁴ This translates to decreased Detention Support by 0.12 standard deviations after receiving the international treatment.

However, it is important to assess whether my *statistically* significant treatment effects when including covariates are *substantively* significant, given that the decrease in Detention Support in both treatment groups is a small fraction of the five-point Likert scale. Firstly, standardized effects of 0.20 are considered small-medium sized effects and are usually the benchmark used for power calculations in similar survey experiments. Other researchers such as Chilton (2014) and Wallace (2013), whose survey experiments I partially replicated, calculated standardized effects of between 0.20-0.40. The standardized effects of Detention Support for both my treatment groups (0.13 and 0.12, see above paragraph) are below 0.20, suggesting that they are substantively small. However, Chilton and Wallace fielded their survey experiments at different times in U.S. history and focused upon solitary confinement and torture practices, respectively. Comparatively, it may be more difficult to move people's opinions on as high-profile, partisan, and controversial a topic as immigration detention in the wake of the Trump administration, so my treatment effects may still be considered realistic and substantive.

Table 4 shows that there are no statistically significant differences in support for reform, likelihood to sign the petition for reform, or likelihood to contact one's representative to urge reform between either of the treatment groups relative to the control group. In line with the findings of McEntire et al., it is more difficult to move citizens to engage in "action mobilization" (2015, 26). Advocating for reform may be more costly to citizens, requiring them to change the status quo rather than simply state their views on immigration detention. Thus,

only one attitude-based outcome measure (Detention Support) had statistically significant differences between each treatment group relative to the control group, while the three action-oriented outcome measures did not.

Assessing Statistical Significance

I used Huber-White heteroskedasticity-robust standard errors and assessed statistical significance at the 5% alpha level (0.05) for all of the regression models above. Regular standard errors assume homoskedasticity, or that the error term from regression has a constant variance, but that may not be the case. Heteroskedasticity is another word for non-constant and, with Huber-White's robust standard errors that assume heteroskedasticity, my models and estimates accounted for the treatment potentially affecting the variance of the outcome or the covariates being related to the variance of the outcome (Freedman 2006). Finally, robust standard errors are often larger than regular standard errors and thus provide wider, more conservative confidence intervals. Because I obtained significant results using robust standard errors, I am even more confident about detecting a true treatment effect. For the primary regression used to assess *H1* and *H2*, I reported the nominal p-values.

Backlash Hypothesis

To test my Backlash Hypothesis, I conducted two-sample z-tests comparing the β_1 and β_2 coefficients in the associated regression equation for *H1* and *H2* (see page 41). With the two-sample z-tests, I could compare the average responses for the two attitudinal measures and the two behavioral outcome measures across two treatment groups. The estimands of interest are average treatment effects (ATEs) for the group that received the treatment with the international legal violation and for those who received the treatment with the constitutional legal violation.

I predicted that awareness that immigration detention violates the ICCPR would have weaker effects on support for immigrant detention and support for reform than awareness that immigration detention violates the U.S. Constitution. I expected statistically significant differences ($p < 0.05$) between the international and constitutional treatment groups for each of the four outcome measures because some citizens may consider international law a threat to U.S. control over its domestic affairs, which are primarily guided and enforced by the U.S. Constitution. In the wake of the isolationist and anti-immigration policies of the Trump administration, U.S. citizens may consider international treaties, like the ICCPR, to be less legitimate than norms deriving from the U.S. Constitution. Thus, the constitutional treatment might have had a greater expected treatment effects than the international treatment.

Table 5: Backlash Z-Test Results, Including Covariates

Outcome Measures				
	Detention Support	Reform Support	Petition	Contact Rep
P-Values	0.862	0.084*	0.121	0.397

* $p < .1$; ** $p < .05$; *** $p < .01$

Notes: With robust SEs, including covariates

However, as depicted in *Table 5*, there are no statistically significant differences between the international and constitutional treatment outcomes to indicate a “backlash” against international norms. Each of the p-values for the z-tests comparing the outcome measures across the treatment groups was greater than 0.05, meaning I failed to reject the null hypothesis that there is no statistically significant difference in support or action between the international and constitutional treatment groups. Reform Support differed across the two treatment groups with nearly conventional statistical significance (0.084, $p < 0.1$). The differences across the two treatment groups for the other three outcome measures (Detention Support, Petition Support, Or Contact Representative Support) are not statistically significant either. The null findings for the backlash effect may indicate that constitutional law is not generally considered more legitimate

than international law. Respondents neither resist nor accept international standards more than constitutional norms. Based on the statistical significance of the Detention Support outcome measure, which supported part of my two Rights Gap Hypotheses, violations of constitutional and international law may both provide enough evidence to make some respondents recognize the rights gap between legal promises and immigration detention practices.

Interaction Effects

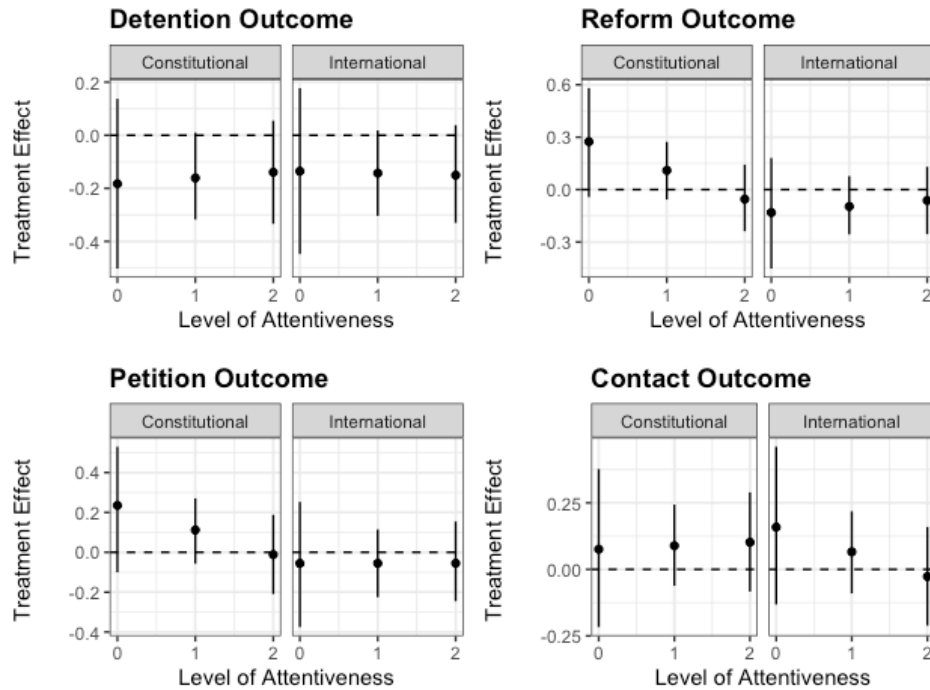
Attentiveness

I studied the interaction between attentiveness and each treatment (β_4 and β_5) to explore whether treatment effects are stronger among respondents who paid more attention to the rule of law referenced. The associated regression specification by attentiveness for $H1$ and $H2$ is below.

$$\begin{aligned} Outcome = & \beta_0 + \beta_1 \text{constitutional violation} + \beta_2 \text{international violation} + \beta_3 \text{attentiveness} \\ & + \beta_4 \text{constitutional violation} \times \text{attentiveness} + \beta_5 \text{international violation} \times \text{attentiveness} + \\ & \gamma X + \epsilon \end{aligned}$$

Extensive research shows that “attention checks” can prevent null results from uncommitted participants by requiring subjects to demonstrate that they carefully read the prompts and survey questions (Berinsky et al. 2014, 2016, 2021). Thus, I expected the signs on β_4 and β_5 to be negative for the immigration detention support outcome and positive for the reform support outcome. More attentive respondents might have stronger negative reactions to immigration detention than those who do not pay as close attention to either treatment because the legal citation in the treatment vignette confers more legitimacy when read in detail. I also expected the signs on β_4 and β_5 to be positive for the two behavioral outcomes, both contacting a Congressional representative and signing a petition for Congress. Greater willingness to advocate for change might reflect stronger treatment effects among the more attentive respondents, who would be more moved to act after reading information about alleged legal violations.

Figure 4: Post Estimation Graph of Treatment Effect x Level of Attentiveness

Table 6: Interaction Results for Attentiveness¹⁵

	Detention Support (1)	Reform Support (2)	Petition (3)	Contact Rep (4)
Constitutional	-0.187 (0.165)	0.285* (0.157)	0.225 (0.161)	0.073 (0.148)
International	-0.134 (0.158)	-0.131 (0.162)	-0.064 (0.160)	0.154 (0.154)
Attentiveness	0.044 (0.072)	-0.002 (0.078)	-0.024 (0.075)	-0.103 (0.069)
Constitutional x Attentiveness	0.022 (0.105)	-0.172* (0.101)	-0.115 (0.106)	0.015 (0.098)
International x Attentiveness	-0.007 (0.101)	0.036 (0.103)	0.007 (0.104)	-0.088 (0.099)
N	1,388	1,388	1,388	1,388

¹⁵ See Appendix C (Tables 11-13) for sub-setted results with respondents of each level of attentiveness (0, 1, or 2 attention check questions correct).

R2	0.196	0.236	0.204	0.174
F Statistic (df = 36; 1351)	9.146***	11.616***	9.609***	7.923***

*p < .1; **p < .05; ***p < .01

Notes: With robust SEs, including covariates

However, as illustrated in *Table 6* and the *Figure 4* post-estimation interaction plots, there are no statistically significant differences between the treatment effect sizes of each level of attentiveness (0, 1, or 2 attention check questions correct). When there are differences in treatment effect size, such as in the Reform outcome measure for the constitutional treatment group, Petition outcome measure for the constitutional treatment group, and Contact outcome measure for the international treatment group, more attentive respondents are less likely to be affected by the treatments (with an effect size closer to zero). Similarly at the 0.1 significance level, on average, those in the constitutional treatment group have a 0.285 point increase in reform support among those least attentive (0 correct, β_1), a 0.113 point increase in reform support among those partly attentive (1 correct, $\beta_1 + \beta_4$), and a 0.041 point decrease in reform support among those most attentive (2 correct, $\beta_1 + 2(\beta_4)$). Again, attentiveness had an opposite effect than expected, as greater attentiveness may diminish treatment effects.

My unexpected findings raise questions about how attentiveness moderated treatment effects in this context. Researchers at the forefront of attention check literature, who reviewed and conducted national surveys with the same attention checks, acknowledge the “great variability in responses to Screeners,” specifically that “passage on any single item does not perfectly predict passage on other Screeners on the same survey” (Berinsky et al. 2014, 745). It is possible that respondents who pass the attention checks may not actually be more attentive to my survey, but may be aware of the expected answers to common attention checks used in experiments (known as the Hawthorne Effect). Many attention checks are fielded in online samples from Amazon

MTurk and Survey Sampling International (SSI) (Berinsky et al. 2012, 2014, 2016, 2021), so the Lucid respondents who seem to be the most attentive may just be more experienced survey takers. More attentive respondents may have needed more information than one legal citation to resist or reform an established U.S. practice or may have resisted the treatment because they have firmer prior opinions about immigration detention based on the news information they usually consume. On the other hand, the less attentive respondents in the constitutional treatment group (significance at the 0.1 level), who largely contribute to the statistically significant main regression results, may have only needed to quickly skim a legal citation to support reform to a greater degree than those in the control group. The less attentive respondents may have had prior opinions that were not as firmly grounded, so they were swayed more easily. Also, less attentive respondents may illustrate the Hawthorne Effect issue, responding by changing their behavior in the direction they think the researchers want because they know they are being studied.

Partisanship

I also studied the interaction between partisanship and each treatment (β_4 and β_5) to test whether there are weaker treatment effects among respondents who are “Strongly Republican,” “Not Very Strongly Republican,” or “Independent Leaning Republican” (based on demographic data collected by Lucid). The associated regression specification by partisanship for $H1$ and $H2$ is below.

$$\text{Outcome} = \beta_0 + \beta_1 \text{constitutional violation} + \beta_2 \text{international violation} + \beta_3 \text{Republican} + \beta_4 \text{constitutional violation} \times \text{Republican} + \beta_5 \text{international violation} \times \text{Republican} + \gamma X + \epsilon$$

Republicans may be more committed to current restrictive immigration policies commonly supported by their political party, compared to Democrats and Independents. Research suggests that liberals tend to support “multilateralism and international institutions,”

such as human rights, while conservatives hold more intense skepticism of such institutions and promote greater self-reliance (Hertel et al. 2009; Wallace 2013, 115). Republicans are also more supportive of immigration policies, including Secure Communities (287(g) agreements) and other ICE programs, than Democrats (Casellas and Wallace 2018; Doherty et al. 2018; Newall 2021). Therefore, I expected the signs on β_4 and β_5 to be positive for the immigration detention support outcome and negative for the reform support outcome. Democrats and Independents might be more likely to have stronger negative reactions to legal violations than Republicans and may more strongly disapprove of immigration detention and push for its reform. I also expected the signs on β_4 and β_5 to be negative for the two behavioral outcomes, likelihood to sign a petition and to contact one's representative.

Figure 5: Post Estimation Graph of Treatment Effect x Republican or Not

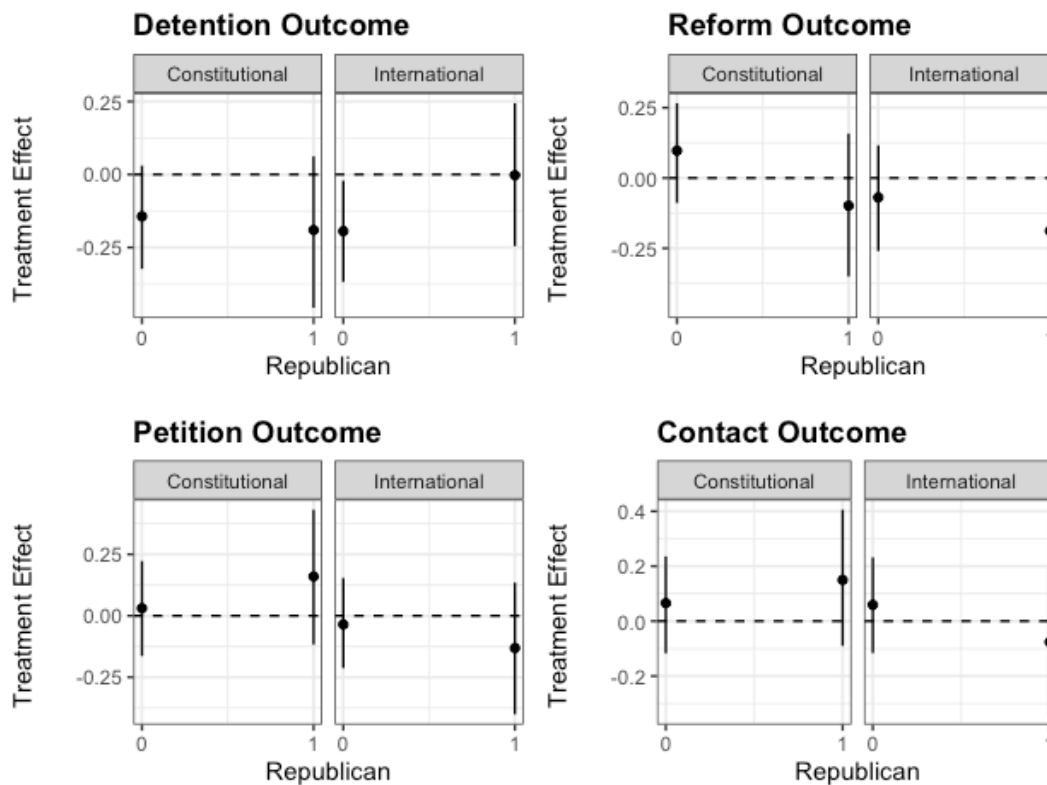


Table 7: Interaction Results for Republican Partisanship¹⁶

	Detention Support (1)	Reform Support (2)	Petition (3)	Contact Rep (4)
Constitutional	-0.144 (0.098)	0.100 (0.091)	0.030 (0.105)	0.063 (0.093)
International	-0.196** (0.094)	-0.066 (0.093)	-0.038 (0.101)	0.060 (0.091)
Republican	0.696*** (0.109)	-0.675*** (0.127)	-0.638*** (0.114)	-0.333*** (0.099)
Constitutional x Republican	-0.048 (0.159)	-0.192 (0.174)	0.129 (0.166)	0.083 (0.153)
International x Republican	0.191 (0.152)	-0.124 (0.174)	-0.099 (0.153)	-0.133 (0.139)
N	1,388	1,388	1,388	1,388
R2	0.165	0.163	0.144	0.138
F Statistic (df = 28; 1359)	9.605***	9.436***	8.162***	7.773***

*p < .1; **p < .05; ***p < .01

Notes: With robust SEs, including covariates

For all four of the outcome measures, *Table 7* shows the highly statistically significant differences (at the 0.01 level) between the Republican and non-Republican respondents (Democrats and Independents) in the control group. Republican respondents in the control group are, on average, more supportive of detention than non-Republican respondents by about 0.696 points on the Likert scale and less supportive of reform than non-Republican respondents by about 0.675 points on the Likert scale. Also, Republican respondents in the control group are, on average, less supportive of signing a petition than non-Republican respondents by about 0.638 points on the Likert scale and less supportive of contacting one's representative for reform than non-Republican respondents by about 0.333 points on the Likert scale.

¹⁶ See *Appendix C* (Table 14) for sub-setted results with Republican respondents only.

Moreover, *Table 7* and the *Figure 5* post-estimation interaction plots illustrate one statistically significant interaction effect between Republican partisanship and both treatments. At the 0.05 significance level, on average, within the international treatment group, there was a 0.20 decrease in support for immigration detention (Detention Support) among non-Republicans (β_2) relative to control. There was practically no reduction in support among Republicans ($\beta_2 + \beta_5$). These effects are also represented in the Detention Outcome plot. Overall, as expected, non-Republican respondents, or Democrats and Independents, decreased their support for immigration detention after learning that it violates international law, likely because of the support for “multilateralism and international institutions,” such as human rights, among liberals as aforementioned (Wallace 2013, 115). Interestingly, the constitutional treatment did not have a statistically significant effect upon non-Republicans or Republicans, which may be because leaders of all political parties have over-used constitutional arguments for salient, controversial issues like immigration detention. For example, one side has argued that immigrants do not deserve constitutional rights, while an opposing side states that constitutional protections extend to non-citizens. This may have made evidence of legal violations lose its novelty and strength in this policy area.

VI. Discussion and Conclusion

My survey experiment explored how awareness of U.S. legal violations impacts public attitudes and behaviors toward immigration detention and its proposed reform. Over the years, observational studies suggested that ratification of certain international agreements do improve state practices and survey experiments obtained mixed findings about how participants react to legal criticism depending on the criticized practice and the national context. However, previous studies have not directly tested exactly *why* informing citizens about legal promises might shift their perspectives about questionable policies. This paper sought to answer this question through a survey experiment fielded upon 1,388 Lucid respondents, a sufficient sample size according to my calculations (see *Power Calculations*). Impressively, the demographic makeup of my Lucid sample is similar to the U.S. population in many ways,¹⁷ including the percent who identified as Female, Male, Other Gender, Black, White, Asian and Pacific Islander, American Indian or Alaska Native, Democrat, and Republican (Jones 2022; U.S. Census Bureau 2020; Wilson and Meyer 2021). As a result, there is strong evidence that both my statistically significant and null findings are externally valid and can provide inspiration for future research.

Some results confirmed my Constitutional Rights Gap Hypothesis and my International Rights Gap Hypothesis. Awareness that immigration detention violates constitutional law *and* international law each had a statistically significant impact on attitudinal support for immigration detention. Notably, the constitutional law vignette had a slightly larger treatment effect (0.158 point decrease on the Likert scale) than the international law vignette (0.144 point decrease on the Likert scale) relative to control. While my treatment effect sizes are not very large, the relationship between public opinion and policy change is often S-shaped, where the key

¹⁷ See *Appendix B* for Demographic Comparison Table 9 of my Lucid sample and the U.S. population.

inflection point for public influence is around 50% (Gilens 2005). In other words, when public opinion is divided on an issue, a small shift in the responses clustered around the middle of a distribution is substantively significant. In my study, when I estimated the predicted average scores and set all covariates to their sample means or modes, I can see such a shift near the center of the five-point Likert scale for Detention Support. The predicted average score moved from 2.68 in the control group to 2.52 in the constitutional treatment group and to 2.54 in the international treatment group. The mean from my descriptive statistics shows that average Detention Support went from 3.38 on the Likert scale in the control group to 3.24 in the constitutional treatment group and to 3.28 in the international treatment group (see *Table 2*). Thus, legal norms decreased the percentage of respondents who approved of detention, shifting people away from the center of the scale (or 3, *Neither Approve Nor Disapprove* of detention). Understanding the legal violations taking place in immigration detention could make people reconsider their acceptance of the status quo and make them more willing to discuss what needs to be reassessed, even if they may not be fully committed to engaging in reform just yet.

However, there are no statistically significant differences between either treatment group and the control group in support for reform, likelihood to sign a petition, or likelihood to contact one's representative. Because there are statistically significant treatment effects for only one attitudinal outcome measure (Detention Support), it is possible that legal norms mainly influence public opinion and do not move people to invest their time in changing the system. The null findings for all of my behavioral outcome measures across treatment groups may illustrate the difficulty of mobilizing citizens to act, as McEntire et al. found (2015), because signing a petition or contacting one's representative is more costly than just expressing one's attitudes.

These time-intensive tasks may require more persuasive information than either the type or style of evidence I presented in my survey vignettes.

The results also rejected my Backlash Hypothesis, as there are no statistically significant differences between the international treatment group effect and the constitutional treatment group effect in any of the outcome measures. The null findings for the backlash effect imply that constitutional law is not considered much more legitimate than international law. While the Trump administration employed isolationist foreign policy and withdrew from several international agreements affecting migrants and refugees, the new Biden administration has reversed many of Trump's efforts (e.g., re-joining the W.H.O and the U.N. Human Rights Council, ending the Muslim travel ban). Thus, because I fielded my survey experiment in the post-Trump era, U.S. constituents may now be just as skeptical about the legitimacy of constitutional principles as they are about international standards.

However, while some of my results were statistically significant, the small differences between each treatment group relative to the control group may or may not be substantively significant because the literature does not show consistent, statistically significant treatment effects among participants informed that a practice violates international or constitutional law. For instance, Chilton found that information that “solitary confinement ‘violates international human rights treaties that the United States has signed’ had a modest but statistically significant effect on public opinion ($p = 0.03$) [which] translates into roughly a 4 percent increase in support for reform [on a six-point Likert scale binarized into a support/do not support measure for a logistic regression]... consistent with the 6 percent change that Wallace found in his study [using the same scale] (2013)” (2014). On the other hand, Chilton and Versteeg's results discovered, “[a]lthough the respondents given the international law treatment did express lower support for

the use of torture than the control group, the difference between the two groups is not statistically significant ($p = 0.54$) [and] the difference between the Control group and the Con. Law group is not statistically significant ($p = 0.08$)... using the full six-point response variable” (2016).

Furthermore, Lupu and Wallace illustrated that the statistical significance of legal norms remains unclear outside of the United States too. The two researchers “rescaled the values for the approval outcome variable to range between 0 and 1, so that effect sizes indicate... percentage change in government approval” to find that international legal allegations had an effect size of “nearly zero” among Argentinian respondents and increased the “level of government approval by about 4%, but not in a statistically significant manner ($p = 0.05$)” among Israeli respondents (2016). The lack of substantive significance in Argentina and Israel may be attributed to their historical resistance against human rights institutions. Meanwhile, Indian respondents are “significantly less likely to approve of the government, with an effect size of about 8%” on the same 0 to 1 scale (2016). Thus, the direction and magnitude of the treatment effects of legal norms may vary based on national context. While the above studies use a similar survey methodology and international legal treatment to my study, they were fielded in different countries and/or informed respondents of different rights abuses, so it is difficult to compare the statistical significance of those studies to my U.S. study about immigration detention. Further research can replicate my study to validate whether the difference in Detention Support between my treatment and control groups is substantively significant.

Also, there are no statistically significant interaction effects between any level of attentiveness and either of the treatments. While these interaction effects are inconsistent and not significant, increasing levels of attentiveness correlated with smaller treatment effects closer to zero for some behavioral outcome measures (Reform Outcome for constitutional group, Petition

Outcome for constitutional group, and Contact Outcome for international group). My attention check questions may have captured the level of expertise and experimental awareness among Lucid participants, rather than their attentiveness to my particular survey. Furthermore, more attentive respondents may not have been as easily swayed by my brief survey vignettes as those who quickly completed the survey because they needed stronger evidence about the legal violations. More attentive respondents may have firmer prior opinions, compared to less attentive respondents, due to their close consideration of the information they choose to consume – especially in the post-Trump era filled with fears of misinformation and “fake news.” Just one legal citation may not have been persuasive enough to make more attentive respondents change their views, compared to less attentive but more malleable respondents.

Yet, there was one statistically significant interaction effect between Republican partisanship and the treatments. Non-Republican respondents in the international treatment group are more likely to decrease their support for immigration detention than those in the control group, potentially demonstrating the tendency for liberals to support international cooperation (Hertel et al. 2009; Wallace 2013, 115). There was practically no change in support for detention among Republicans. Meanwhile, the constitutional treatment did not have a statistically significant effect upon non-Republicans or Republicans. It is possible that members of all political backgrounds, whether they identify as Democratic, Republican, or Independent, have become de-sensitized to constitutional arguments related to immigration or do not consider constitutional standards to be very applicable to non-citizens in detention. While legal norms may hold weight in the courtroom and among legal practitioners, they may not be a very useful tool in political discussions and debates.

Notably, there are several methodological limitations to my research design, which offer potential avenues for improvement and further study. Firstly, making the legal vignettes more detailed may increase the magnitude of the treatment effects. For example, a longer paragraph could provide additional background on the constitutional or international law in violation, such as its historical precedence in the United States and if it has been cited in other cases. Also, describing how the experiences of immigrants in detention qualify as the “cruel and unusual” treatment according to the U.S. Constitution and the ICCPR could make the treatments more impactful. When crafting my survey vignettes,¹⁸ I did consider detailing the rights abuses in immigration detention (i.e., solitary confinement, medical neglect), but decided against it due to the risk of the emotional evidence or particular abuses driving the treatment effects rather than the legal norms I wanted to test. However, it is possible that providing only neutral background information did not sufficiently portray the legal violations occurring in immigration detention, especially when it is a less widely-known practice (compared to, say, solitary confinement).

Using a different approach to relay the background information and legal vignettes, such as videos or audio recordings, rather than survey text, might also keep respondents more engaged and result in greater treatment effects. Moreover, constructing the vignettes with a layout similar to an actual court case, with a bold header and a citation number associated with the court decision, might make the legal allegations seem more realistic. Asking whether respondents would contribute to immigration detention reform in other ways may also produce different treatment effects, as other studies have analyzed how likely respondents are to vote for a (hypothetical) political candidate, join a (hypothetical) political party, or to pay extra and donate funds to support one side of an issue (Anjum et al. 2021; Hertel et al. 2009). However, when

¹⁸ For an exact copy of the legal survey vignettes, see *Appendix D*.

considering the additions I propose, researchers must keep ethical conduct in mind because of the greater level of deception required to convince respondents that the vignettes are entirely factual and not hypothetical in nature.

Also, creating more unique questions may ensure that respondents' answer choices reflect their opinions and behaviors, rather than their prior survey experience. For example, the attention check questions could be more original, unlike those used in rigorously tested surveys (Berinsky et al. 2014, 2016, 2021), to prevent professional survey takers from getting them correct out of familiarity or awareness of the experiment. In addition, the screener questions could be related and tailored to the subject matter of my survey, law and immigration, rather than about a participant's favorite color and feelings. Putting a timer on the outcome questions, as I did with the treatment vignettes, could further ensure that quicker, more experienced survey takers do not rush through but instead take time to check their responses or re-read information while waiting for the arrow button that allows them to proceed.

Several non-methodological ideas for future research could also continue to fill the gap I identified in the literature. I uniquely chose to focus upon the issue of immigration detention to build upon similar survey experiments about Turkey's refugee quota policies (Cope and Crabtree 2020) and U.S. solitary confinement of terrorist detainees (Chilton 2014). Other studies could explore U.S. public opinion and behavior regarding other under-studied immigration policies, such as "zero-tolerance" prosecution of asylum seekers, which allegedly violates international refugee law, and "secure communities" agreements, considered in violation of racial discrimination laws. Furthermore, while I fielded my study in the United States, future researchers can gather data from citizens in other parts of the world. For example, Turkish and Israeli respondents have been shown to backlash against international legal criticism of their

country's practices, which has been attributed to their negative views toward international institutions (Cope and Crabtree 2020; Lupu and Wallace 2016). Perhaps citizens in countries well-known for their cooperation with international organizations and migrant protocols, such as Canada and Switzerland, would be interesting to study.

With these limitations and areas of improvement in mind, my study still makes important contributions. Learning about legal doctrine is not a panacea that automatically results in support for reform but may bring some citizens to firstly recognize the "rights gap" in immigration detention and become more willing to discuss the legitimate issue. Awareness of constitutional and international legal violations can make citizens more disapproving of immigration detention, but it does not mobilize them to take initiative by signing community petitions and contacting political representatives on their own. Thus, it may remain in the hands of policymakers and legal advocates to put immigration detention at the top of the agenda and to hold conversations about immigration detention reform, which could incorporate more formalized evidence of legal violations as well as detailed descriptions of the abuses taking place. Perhaps then the United States can bridge the gap between survey attitudes and real-world actions, between immigration detention and more humane alternatives.

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VIII. Appendices

Appendix A

Table 8: Covariate Balance

	Control Group Mean or Proportion	Constitutional Group Mean or Proportion	International Group Mean or Proportion
Attentiveness	1.34	1.35	1.39
Male Gender	0.50	0.47	0.48
Female Gender	0.50	0.53	0.52
Other Gender	0.00	0.01	0.00
Age	43.7	45.4	46.3
Strong Democrat	0.23	0.23	0.23
Not very strong Democrat	0.08	0.11	0.12
Independent leaning Democrat	0.11	0.09	0.10
Independent leaning Republican	0.09	0.14	0.10
Independent – neither	0.16	0.14	0.14
Other - leaning Democrat	0.01	0.01	0.01
Other - neither	0.07	0.07	0.07
Other - leaning Republican	0.03	0.03	0.2
Not very strong Republican	0.09	0.05	0.07
Strong Republican	0.23	0.18	0.22
White	0.72	0.72	0.73
Black	0.11	0.13	0.13
American Indian or Alaska Native	0.01	0.01	0.02
Asian or Pacific Islander	0.05	0.04	0.05
Other Race	0.11	0.09	0.08
Hispanic	0.14	0.12	0.10
Not Hispanic	0.86	0.88	0.90
US-Born	0.81	0.83	0.84
US-Territory Born	0.10	0.10	0.09
Foreign-Born	0.09	0.08	0.06
US-Born Parent	0.77	0.79	0.80
US-Territory Born	0.08	0.10	0.09
Foreign-Born Parent	0.15	0.11	0.11
Low Income	0.32	0.38	0.33
Middle Income	0.41	0.36	0.41
High Income	0.25	0.24	0.25
Other Income	0.02	0.02	0.02
High School Graduate or Less	0.30	0.30	0.30
Some College or Associate Degree	0.29	0.27	0.28
Bachelor's Degree	0.31	0.29	0.31
Graduate Degree	0.10	0.13	0.11
Other Degree	0.00	0.00	0.00
Northeast	0.22	0.21	0.19
Midwest	0.18	0.19	0.18

South	0.36	0.36	0.40
West	0.24	0.25	0.22

F-Tests

To also assess the successfulness of randomization and covariate balance, I conducted two F-tests. The p-value for the constitutional treatment was 0.905, which fails to reject the null hypothesis that the covariates jointly do not predict treatment assignment, suggesting randomization was successful and balance was achieved.

The p-value for the international treatment was 0.381, which fails to reject the null hypothesis that the covariates jointly do not predict treatment assignment, suggesting randomization was successful and balance was achieved.

Figure 6: Sample Distribution of Detention Support

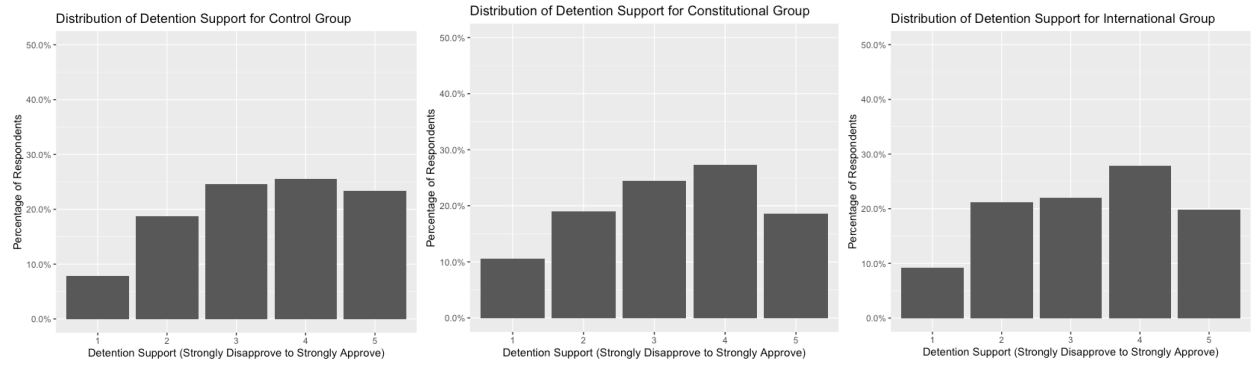


Figure 7: Sample Distribution of Reform Support

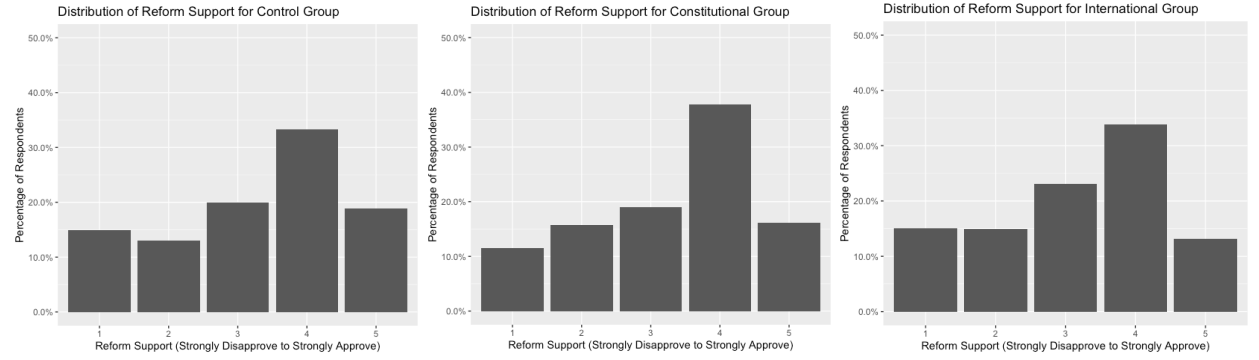


Figure 8: Sample Distribution of Petition Support

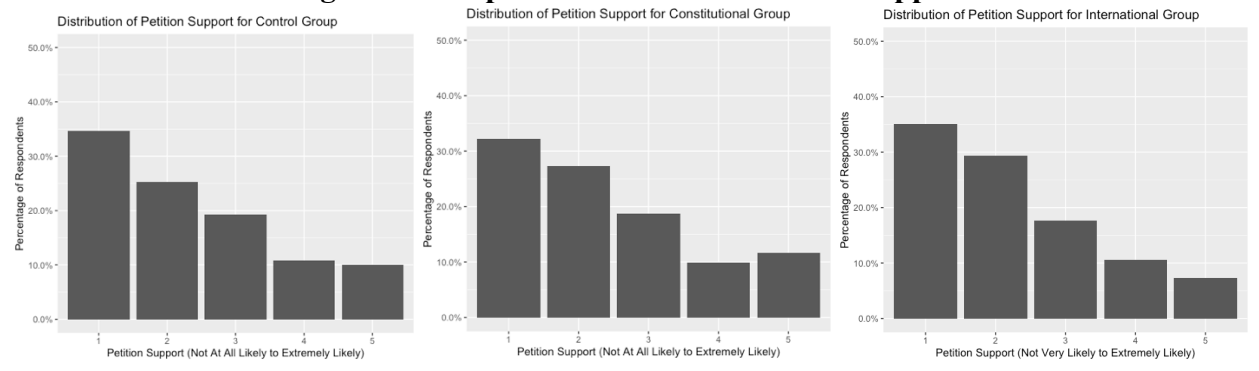
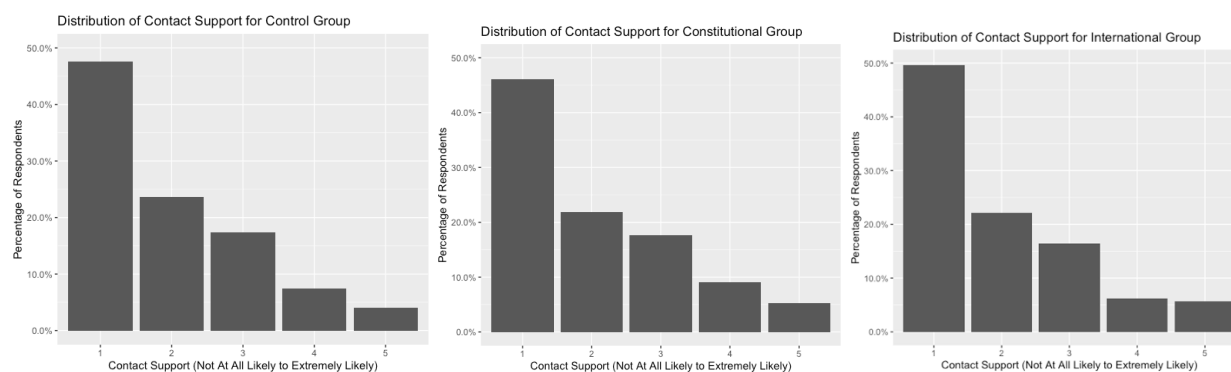


Figure 9: Sample Distribution of Contact Representative Support**Appendix B****Table 9: Demographic Comparison Between Lucid Sample and U.S. Population¹⁹**

	Lucid Sample Mean or Proportion (N = 1,388)	U.S. Population Mean or Proportion (N = 331,893,745)
Male Gender	0.48	0.49
Female Gender	0.51	0.51
Other Gender	0.004	0.0036 (non-binary)
Age	45.10	38.2 (as of 2018)
Democrat (Strong Democrat and Not very Strong Democrat)	0.32	0.29
Independent leaning Democrat	0.09	0.17 (Democrat-leaning independent)
Non-leaning Independent	0.15	0.08
Independent leaning Republican	0.11	0.17 (Republican-leaning independent)
Republican (Strong Republican and Not very Strong Republican)	0.21	0.27
White	0.72	0.76
Black	0.12	0.13
American Indian or Alaska Native	0.01	0.01
Asian or Pacific Islander	0.05	0.06
Hispanic	0.12	0.19
Foreign-Born	0.08	0.14 (naturalized and not naturalized)
High School Graduate or Less	0.30 (18 and older)	0.27 (25 and older)
Some College or Associate Degree	0.28 (18 and older)	0.20 (25 and older)
Bachelor's Degree	0.30 (18 and older)	0.20 (25 and older)
Graduate Degree	0.12 ((18 and older)	0.13 (25 and older)

¹⁹ U.S. Population demographic percentages from the U.S. Census Bureau (2020), Gallup (Jones 2022), and from a UCLA Law study (Wilson and Meyer 2021).

Appendix C

Table 10: Alternative Main Regression Results with 3-Level Partisanship

	Detention Support (1)	Reform Support (2)	Petition (3)	Contact Rep (4)
Constitutional	-0.156** (0.076)	0.031 (0.075)	0.065 (0.080)	0.085 (0.074)
International	-0.136** (0.074)	0.106 (0.076)	-0.069 (0.076)	0.017 (0.070)
Age	0.013*** (0.002)	-0.015*** (0.002)	-0.017*** (0.002)	-0.014*** (0.002)
Female Gender	-0.310*** (0.063)	0.249*** (0.063)	0.171*** (0.065)	-0.025 (0.060)
Other Gender	-0.200 (0.786)	0.981* (0.505)	1.065* (0.612)	0.662 (0.640)
US Territory Born	0.152 (0.182)	0.151 (0.176)	0.028 (0.183)	0.176 (0.182)
Foreign Born	-0.290* (0.163)	0.222 (0.169)	0.355** (0.175)	0.184 (0.157)
US Territory Born Parent	0.263 (0.191)	-0.103 (0.185)	0.029 (0.185)	0.168 (0.195)
Foreign Born Parent	0.176 (0.133)	-0.107 (0.142)	-0.007 (0.144)	0.122 (0.132)
Democrat	-0.344*** (0.083)	0.731*** (0.078)	0.677*** (0.085)	0.449*** (0.075)
Republican	0.530*** (0.082)	-0.325*** (0.090)	-0.209** (0.084)	-0.072 (0.077)
Midwest	-0.184* (0.095)	0.261*** (0.098)	0.136 (0.097)	0.103 (0.093)
South	-0.086 (0.085)	0.199** (0.088)	0.193** (0.089)	0.183** (0.082)
West	-0.266*** (0.096)	0.117 (0.101)	0.038 (0.101)	-0.039 (0.094)
Black	0.309*** (0.114)	0.119 (0.102)	0.065 (0.111)	0.313*** (0.104)
American Indian or Alaska Native	-0.585* (0.301)	-0.518* (0.310)	0.158 (0.299)	0.140 (0.336)
Asian or Pacific Islander	0.076 (0.159)	-0.210 (0.164)	-0.159 (0.170)	-0.013 (0.159)
Other	0.011 (0.139)	-0.028 (0.130)	-0.116 (0.139)	-0.190 (0.124)
Middle Income	0.060	0.103	0.011	-0.119*

	(0.078)	(0.075)	(0.078)	(0.072)
High Income	0.219**	0.033	-0.089	-0.125
	(0.089)	(0.090)	(0.093)	(0.085)
Other Income	-0.248	0.254	-0.152	-0.051
	(0.202)	(0.187)	(0.170)	(0.166)
Hispanic	-0.078	-0.031	0.207	0.127
	(0.127)	(0.129)	(0.138)	(0.124)
Some college or Associate Degree	-0.057	-0.060	-0.019	0.036
	(0.085)	(0.087)	(0.089)	(0.081)
Bachelor's Degree	-0.170**	0.022	-0.040	0.001
	(0.085)	(0.084)	(0.088)	(0.082)
Graduate Degree	-0.015	0.073	0.070	0.247**
	(0.113)	(0.117)	(0.128)	(0.124)
Other Degree	-0.244	0.114	0.005	0.367
	(0.462)	(0.442)	(0.562)	(0.542)
Attentiveness	0.057	-0.053	-0.060	-0.123***
	(0.043)	(0.043)	(0.045)	(0.042)
Constant	2.928***	3.475***	2.740***	2.448***
	(0.147)	(0.150)	(0.155)	(0.143)
N	1,388	1,388	1,388	1,388
R2	0.175	0.211	0.183	0.158
F Statistic (df = 27; 1360)	10.691***	13.486***	11.316***	9.487***

*p < .1; **p < .05; ***p < .01

Notes: With robust SEs, including covariates

Table 11: Sub-Setted Results, 2 Correct Attention Check Respondents

	Detention Support (1)	Alternative Support (2)	Petition (3)	Contact Rep (4)
Constitutional	-0.113 (0.105)	-0.085 (0.105)	-0.017 (0.116)	0.085 (0.105)
International	-0.171* (0.104)	-0.046 (0.104)	-0.045 (0.108)	0.004 (0.098)
N	721	721	721	721
R2	0.241	0.289	0.203	0.164
F Statistic (df = 33; 687)	6.597***	8.471***	5.303***	4.085***

*p < .1; **p < .05; ***p < .01

Notes: With robust SEs, including covariates

Table 12: Sub-Setted Results, 1 Correct Attention Check Respondents

	Detention Support (1)	Reform Support (2)	Petition (3)	Contact Rep (4)
Constitutional	-0.286** (0.143)	0.219 (0.147)	0.221 (0.147)	0.193 (0.133)
International	-0.034 (0.136)	-0.080 (0.146)	-0.025 (0.135)	0.005 (0.127)
N	448	448	448	448
R2	0.204	0.251	0.244	0.222
F Statistic (df = 33; 414)	3.208***	4.215***	4.039***	3.572***

*p < .1; **p < .05; ***p < .01

Notes: With robust SEs, including covariates

Table 13: Sub-Setted Results, 0 Correct Attention Check Respondents

	Detention Support (1)	Reform Support (2)	Petition (3)	Contact Rep (4)
Constitutional	-0.113 (0.207)	0.097 (0.200)	0.080 (0.195)	0.030 (0.182)
International	-0.137 (0.196)	-0.183 (0.198)	-0.193 (0.210)	0.158 (0.204)
N	219	219	219	219
R2	0.264	0.194	0.258	0.266
F Statistic (df = 31; 187)	2.165***	1.449*	2.103***	2.190***

*p < .1; **p < .05; ***p < .01

Notes: With robust SEs, including covariates

Table 14: Sub-Setted Results, Republican Respondents

	Detention Support (1)	Reform Support (2)	Petition (3)	Contact Rep (4)
Constitutional	-0.133 (0.157)	-0.159 (0.184)	0.096 (0.168)	0.346** (0.162)

International	-0.014 (0.132)	-0.175 (0.172)	-0.190 (0.138)	-0.013 (0.132)
N	296	296	296	296
R2	0.189	0.236	0.227	0.236
F Statistic (df = 25; 270)	2.516***	3.341***	3.167***	3.338***

*p < .1; **p < .05; ***p < .01

Notes: With robust SEs, including covariates

Appendix D

Survey Consent

Consent_Section

Emory University

Consent to be a Research Subject

Introduction You are being asked to be in a research study. This form is designed to tell you everything you need to think about before you decide to consent (agree) to be in the study or not to be in the study. **It is entirely your choice. If you decide to take part, you can change your mind later on and withdraw from the research study. You can skip any questions that you do not wish to answer.**

Study Overview The purpose of this study is to understand public opinion about the detention of immigrants in the United States. The study is funded by the Department of Political Science at Emory University. This study will take about 6-8 minutes to complete.

Procedures If you join, you will be asked to read information about the detention of immigrants in the United States, provide your reactions, and then answer a few demographic questions about yourself. You will be asked to respond to questions in the form of an online survey. At the end, additional information about the purpose and goals of the study will be provided.

Risks and Discomforts The risks due to participation in this study are that you may find some of the information presented to be sensitive. As with any online data entry, there is minimal risk associated with providing information from a personal IP address. To minimize these risks, no member of the research team will have access to the link between survey participants and survey responses. Anonymized survey responses may be stored on the researchers' password-protected computers for the purpose of data analysis.

Benefits This study is not designed to benefit you directly. However, participation will give greater knowledge about the experiences of immigrants. You will also have a platform to share your thoughts and ideas about immigration in the United States.

Confidentiality Study records can be opened by court order. They may also be produced in response to a subpoena or a request for production of documents. Certain offices and people other than the researchers may look at study records. Government agencies and Emory employees overseeing proper study conduct may look at your study records. These offices include the Emory Institutional Review Board and the Emory Office of Research Compliance. Emory will keep any research records we create private to the extent we are required to do so by law. A study number rather than your name will be used on study records wherever possible. Your name and other facts that might point to you will not appear when we present this study or publish its results.

Withdrawal from the Study You have the right to leave a study at any time without penalty.

Contact Information

If you have questions about this study, your part in it, your rights as a research participant, or if you have questions, concerns or complaints about the research you may contact the following:

Contact Beth Reingold at 404-727-6569

Contact the Emory Institutional Review Board at 404-712-0720 or toll-free at 877-503-9797 or by email at irb@emory.edu

You may also let the IRB know about your experience as a research participant through the Research Participant Survey at <https://tinyurl.com/ycewgkke>.

Consent

By checking the box below, you acknowledge the information above and consent to participate in the survey.

I acknowledge that I have read and consent to the information above.

Demographic_Section1

First, please respond to the following questions about yourself.

Demo_Gender What is your gender?

- Male
 Female
 Other

Demo_Age What is your age?

Attention_Check_1

Before we proceed, we have a question about how you're feeling.

To help us understand how people make decisions, we are interested in information about you. Specifically, we are interested in whether you take the time to read the directions; if not, some results may not tell us very much about decision-making in the real world. To show that you have read the instructions, please check only the "none of the above" option as your answer.

Please check all words that describe how you are currently feeling.

- Interested
 Excited
 Upset
 Enthusiastic
 Irritable
 Alert
 Inspired
 Nervous
 Determined
 Attentive
 Active
 None of the above

Background_Section

Treatment_Background

Please read the following excerpt about immigration detention from the Bipartisan Policy Center (2019).

Immigration detention in the United States is the practice of holding individuals in government custody for immigration violations, such as illegal entry or visa overstay, during their removal proceedings. The average daily detained population has been steadily increasing for decades over multiple administrations.

Supporters of immigration detention argue that it is necessary to ensure national security, public safety, and court attendance. Opponents argue that immigration detention is cruel and unjust.

Treatment1_Section
Constitutional

Now, please read the following information carefully before moving on.

A United States Federal Court decided that immigration detention in the United States violates its obligations under the 8th Amendment of the U.S. Constitution, which prohibits "cruel and unusual punishment."

Treatment2_Section
International

Now, please read the following information carefully before moving on.

An International Court decided that immigration detention in the United States violates its obligations under the 7th Article of the International Covenant on Civil and Political Rights, which prohibits "cruel, inhuman or degrading treatment."

Outcome

Detention Do you approve or disapprove of the practice of immigration detention?

- Strongly approve
- Approve
- Neither approve nor disapprove
- Disapprove
- Strongly disapprove

Outcome_Background

Please read the following excerpt about immigration detention reform.

Congress has been considering reforms that would shift immigrants from detention to alternative housing arrangements while they await court decisions.

Community-based immigration programs allow immigrants to live outside of detention centers, while case managers ensure immigrants comply with legal obligations, such as checking in with immigration officials and attending court hearings.

Alternative

Do you approve or disapprove of shifting immigrants from detention to alternative housing arrangements while they await court decisions?

- Strongly approve

- Approve
- Neither approve nor disapprove
- Disapprove
- Strongly disapprove

Petition

How likely are you to sign a petition urging Congress to enact a bill that shifts immigrants from detention to alternative housing arrangements while they await court decisions?

- Not at all likely
- Somewhat likely
- Likely
- Very likely
- Extremely likely

Contact How likely are you to contact your Congressional representative to enact a bill that shifts immigrants from detention to alternative housing arrangements while they await court decisions?

- Not at all likely
- Somewhat likely
- Likely
- Very likely
- Extremely likely

Demographic_Section2

Now, please respond to the following questions about yourself.

Nativity In what state, country, or territory were you born?

- A U.S. state or D.C.
- A U.S. territory
- Another country

Parent_Nativity Where were your parents born?

- A U.S. state or D.C.
- A U.S. territory
- Another country

Attention_Check_2

We have a question about your preferences.

Individual preferences and knowledge, along with situational variables can greatly impact the decision process. To demonstrate that you read this much, just go ahead and select both red and green among the alternatives below, no matter what your favorite color is. Yes, ignore the question below and select both of those options.

What is your favorite color?

- White

- Black
- Red
- Pink
- Green
- Blue

Debrief Section

Debrief

The information provided to you is part of a study about whether the American public might think or act differently after learning about how immigration detention practices allegedly violate constitutional law or international law.

While some survey takers read real, textual citations of the U.S. Constitution or the International Covenant on Civil and Political Rights (ICCPR), we told them about a *hypothetical* decision from a U.S. Federal Court or an International Court. Neither court has held that the U.S. government violates the prohibition against cruel and unusual punishment or treatment contained in the U.S. Constitution and the ICCPR. The court cases were instead created by a team of researchers to serve as “vignettes” that do not reflect actual legal decisions, but that do serve as a reminder of the legal norms and obligations at issue with U.S. immigration detention.

You can still sign a real petition to Congress and contact your representative to advocate for dignity for detained immigrants across the country.

Please view the following resources for more information and opportunities to contribute to immigration detention reform:

[Sign Your Petition to Congress](#)

[Contact Your Members of Congress](#)