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Research Article

Do Street-Level Bureaucrats Discriminate Based on Religion? A Large-Scale Correspondence Experiment among American Public School Principals

Abstract: *Although public administration scholars have long studied discrimination on the basis of race/ethnicity, class, and gender, little to no research exists on whether street-level bureaucrats provide differential services based on the religious identity of their constituents. This article reports the results from a large-scale correspondence study of street-level bureaucrats in the American public school system. The authors emailed the principals of a large sample of public schools and asked for a meeting, randomly assigning the religious (non)affiliation of the family. To get at potential causal mechanisms, religious belief intensity was also randomly assigned. The findings show evidence of substantial discrimination against Muslims and atheists on a par with, and sometimes larger than, the racial discrimination found in previous studies. These individuals are substantially less likely to receive a response, with discrimination growing when they signal that their beliefs are more intense. Protestants and Catholics face no discrimination unless they signal that their religious beliefs are intense.*

Evidence for Practice

- Although legal protections and social norms are in place to protect public school students from religious discrimination, religious discrimination is large and widespread in American public schools. Groups such as Muslims and atheists face substantial barriers to equal treatment in the public domain.
- Public school officials and other policy makers should prioritize testing and implementing new policies and practices that protect both religious believers and nonbelievers. These policies should be designed with the understanding that unequal treatment is driven, in part, by differences in perceived costs attached to students from social minority groups.
- In a context of heightened scrutiny toward social out-groups at all levels of government, public schools play a vitally important role in ensuring that social minorities actually receive the equal treatment they are afforded under the law.

Street-level bureaucrats (SLBs) have substantial discretion in their actions as "ultimate policymakers" (Lipsky 1971, 1980, 2010; see also Brodkin 2011a, 2011b; Maynard-Moody and Musheno 2012; Pressman and Wildavsky 1984; Sowa and Selden 2003).[1] Despite being a core focus of public administration research over the past four decades (e.g., Brodkin 2012; Jensen and Vestergaard 2016; Jilke and Tummers 2018; Jilke, Van Dooren, and Rys 2018; Keiser 2010; Brodkin and Majmundar 2010; Maupin 1993; Monnat 2010; Moynihan and Herd 2010; Prottas 1979; Riccucci 2005a, 2005b; Tummers et al. 2015; Wenger and Wilkins 2009), whether SLBs choose to use their discretion in ways that aide or impede disadvantaged clients remains a vitally important open question.[2] Indeed, there are many important gaps in this literature (Brodkin 2011a, 2011b, 2012). For example, most of the literature on the decision-making processes of SLBs has focused on three client characteristics: race (e.g., Adida, Laitin, and Valfort 2010; Butler and Broockman 2011; Costa 2017; Einstein and Glick 2017), gender (e.g., Kalla, Rosenbluth, and Teele 2018), and class (e.g., Carnes and Holbein 2018). The search for bias induced by these characteristics has yielded many valuable insights into the drivers and consequences of bureaucratic action (e.g., Dubois 2016; Harrits 2019; Herd, Mitchell, and Lightman 2005; Riccucci and Saidel 1997; Watkins-Hayes 2011). At the same time, the relatively narrow focus has restricted our understanding of the breadth of bureaucratic discretion's influence. As a result, we are left with a limited understanding of the extent of discriminatory practices among SLBs.

In addition, despite being a core area of observational research, comparatively little experimental research has been conducted on the extent, nature, and mechanisms of potential biases among frontline

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bureaucrats.[3] This is unfortunate since observational research often struggles to fully address concerns about endogeneity and reverse causation (Jilke, Van de Walle, and Kim 2016; Van de Walle and Bouckaert 2003) and to identify causal mechanisms (Bullock, Green, and Ha 2010). Finally, when taken as a whole, research in this area suggests that when legal institutions (e.g., Brodkin 2012), professional norms (e.g., Weissert 1994), established managerial hierarchies (e.g., Keiser 2010; Maynard-Moody and Portillo 2010; Scott 1997), and active public oversight (e.g., Holbein 2016; Holbein and Hassell 2018; Moynihan 2008; Moynihan and Ingraham 2003; Moynihan, Pandey, and Wright 2011) are present, equitable representation is more likely to follow. However, most studies have examined the role of each of these factors in isolation; as a result it remains unclear is whether discriminatory behavior can still occur even when all of these factors are present.

In this article, we begin to fill these gaps. To do so, we conducted a large-scale correspondence experiment that tested whether SLBs discriminate on the basis of religion—a core individual characteristic that plays a key role in modern American society (as we describe later) but has yet to be explored thoroughly in the representative bureaucracy literature.[4] We focus on public schools in the United States, for three reasons. First, this is a social context in which all of the forces thought to promote equal treatment are present. In public schools, legal protections, professional norms of equitable treatment, established managerial hierarchies, and active public oversight are present to a degree not often seen in other contexts.[5,6] Second, public schools play a key role in local communities, where they serve as one of the most common touch points between citizens and their government (Holbein 2016; Holbein and Hassell 2018; McDonnell 2013; Soss 1999; Soss and Schram 2007). Third, as we describe later, public schools are at the center of fundamental debates about how the state and religion can, do, and should interact. Studying the behavior of SLBs in public schools thus allows us to expand our understanding of whether the factors thought to promote equitable treatment are actually sufficient to do so in this vitally important policy arena.

In our experiment, we emailed the principals of more than 45,000 public PK–12 schools in 33 U.S. states. In our emails, we manipulated the identity of a parent who was considering enrolling his or her child in that school and asked for a meeting with the principal. We randomly assigned the religious affiliation/nonaffiliation of the family (no information given, Protestant, Catholic, Muslim, or atheist). We held the names of the parent and child constant so that we could separate the effects of race/ethnicity and class—which have been studied previously in other contexts—from the effect of religious affiliation. To go an extra step beyond previous correspondence experiments, we explored a potential causal mechanism by also randomizing the intensity of the signaled beliefs: low (identification only), medium (identification + compatibility inquiry), or high (identification + accommodation request). This allows us to experimentally explore a key mechanism that might be driving any discriminatory effect: the perceived costs attached to the enrollment of religious adherents or atheists. We then observed whether principals replied to our email. This constitutes our outcome of interest.

Compared with baseline emails, which provide no information about religious background, we found high levels of discrimination against Muslims and atheists. We found that Muslim and atheist parents are discriminated against for merely revealing their beliefs in the signature part of their emails. Signaling membership in these groups decreased the probability of a reply by 4.6 and 4.7 percentage points, respectively. This difference is statistically and substantively meaningful; it is only slightly smaller than (but not statistically distinct from) the discriminatory effects of race/ethnicity shown in previous correspondence studies (Butler and Broockman 2011). For Muslims and atheists, discriminatory effects are present *regardless* of whether enrollment costs are explicitly signaled. Moreover, discrimination against Muslim and atheist parents increases *dramatically* if they inquire about the compatibility of the school with their beliefs or ask for religious accommodations, with such signals reducing response rates by 8.7 and 13.8 percentage points, respectively.

These strikingly lower response rates for higher levels of request intensity suggest that an important mechanism behind the discriminatory effects we find is the degree to which these individuals' belief systems are seen to be imposing a cost on public officials. Response rates for Protestant and Catholic parents are indistinguishable from the no information baseline in the low-intensity condition; discrimination only appears when parents inquire about the compatibility of the school with their beliefs or ask for accommodation of their beliefs. Finally, exploiting our purposefully large sample size, we show that discriminatory effects are systemic in the public education system. Many tests for treatment effect heterogeneity are woefully underpowered (Blair et al. 2018; Fink, McConnell, and Vollmer 2014), but our large sample size allows us to show with a great degree of precision that discrimination is remarkably consistent across the racial/ethnic composition of the school, the school type (primary, middle, or high), the median household income/poverty rates, the share of adults holding a bachelor's degree, Republican vote shares in the 2012 presidential elections, and the religious adherence rates of the surrounding community. Discrimination against citizens with nonmainstream beliefs about religion seems to be widespread in the American public school system.

Although research on SLBs has proliferated since Lipsky's seminal research on this topic (Lipsky 1971, 1980, 2010), important questions remain unanswered. Our article contributes in at least four important ways to this core area of public administration research. First, it improves our knowledge about the scope of inequitable treatment in the public domain. A large majority of studies on discriminatory behavior by SLBs focus narrowly on race, class, and gender (Maynard-Moody and Portillo 2010). While these social dimensions are vitally important, until now, the literature has (largely) ignored clients' religious affiliation. In performing their duties, the race, gender, and class of the client are not the only—and perhaps not even the most important—heuristics that SLBs use in deciding whether to respond to a request for help. As such, our research shows that public administration scholars would do well to expand the list of potential social cleavages reinforced by unequal treatment in the public domain.

Second, our research constitutes an important step forward in how we study the potential biases of SLBs. While a vast literature in public administration has studied the roots, nature, and extent

of inequities in public service provision, much of this research is observational, leaving open the possibility of confounding bias. To be sure, ours is not the first correspondence study involving public officials. However, most existing correspondence experiments focus on *elected* officials, while ours focuses on *unelected* SLBs. This distinction is important given the different incentive structures of these two types of actors (Dropp and Peskowitz 2012; Einstein and Glick 2017; Hemker and Rink 2017; Jilke, Van Dooren, and Rys 2018; Porter and Rogowski 2018). Moreover, even when we consider previous experimental tests of bias among SLBs, our study is unique. Ours is one of the (if not the) largest correspondence studies of SLBs to date.[7] This intentional design feature decreases the likelihood of Type M (magnitude) and S (sign) errors in our estimates (Gelman and Carlin 2014). It also allows us to experimentally evaluate a potential causal mechanism while minimizing false discovery risks when testing for treatment effect heterogeneity—something that previous experiments have not been able to accomplish.

Third, public administration research has long focused on the factors that drive the actions of SLBs. However, previous research has not clarified whether the conditions mentioned earlier are sufficient to ensure equitable treatment. In this article, we provide evidence that *even when* norms of equal treatment are actively extolled and incentivized, discrimination is against the law, SLBs are overseen by a strong and well-established hierarchy, and SLBs are watched by an active public—as is the case in the U.S. public education system—substantial inequitable treatment may still occur. This result suggests that in order for equal service provision to be realized, additional effort may be necessary.

Finally, our research addresses a topic that is rapidly gaining salience due to growing levels of hostility toward religious out-groups in the United States. In recent years, minority religious groups in the United States have faced heightened opposition and scrutiny. With the rise of Donald Trump and the broader social forces that led to his election, religious minorities such as Muslims have faced harsh rhetoric, violence, travel bans, and unfavorable policy environments.[8] Moreover, there is qualitative and survey-based evidence that minority religious groups as well as atheists face substantial hurdles.[9] While scholars have long documented the importance of religion in the public realm (e.g., Norris and Inglehart 2004; Putnam and Campbell 2010), little research with a compelling identification strategy exists quantifying the extent of discrimination against minority religious groups and atheists in the public domain. Exploring this topic is of vital importance given the foundational principles of religious freedom and neutrality in the United States. Our research demonstrates that religious equality is not currently present in American public schools. In so doing, it prompts practitioners to do more to ensure that this vital goal is fully realized.

The Micro-Foundations of Bureaucratic Responsiveness

What drives the behavior of SLBs? A vast literature in public administration seeks to answer this question, showing that a wide variety of forces shape bureaucratic action. These include—but are not limited to—the ethos of the profession in which the bureaucrat works, the personal beliefs that the bureaucrat holds, the resource constraints that the bureaucrat faces, the leadership structure that oversees the bureaucrat's actions, the peers with which the bureaucrat interacts, the overall composition of the constituency that the bureaucrat serves, and the identity of the individual constituents with whom the bureaucrat interacts (e.g., Brodkin 2012; Jilke and Tummers 2018; Kosar 2011; Lipsky 1980; Maynard-Moody and Portillo 2010; Stensöta 2011; Tummers et al. 2015; Weissert 1994). As Maynard-Moody and Portillo (2010, 21) succinctly put it, "the expression of street-level agency occurs in the context of three core relationships: with the immediate supervisor, with peers, and with clients and citizens."

Two factors that have been especially prevalent in explanations of how bureaucrats make decisions about how to spend their (finite) time in serving the public are convictions and costs.[10] The first explanation asserts that bureaucratic behavior is driven primarily by the individual beliefs, identities, and prejudices that bureaucrats possess. The second explanation takes the view that bureaucratic behavior is driven primarily by the needs, demands, and characteristics of the requesting client. As many scholars have discussed, distinguishing between these two factors is incredibly difficult (e.g., Broockman and Soltas 2018; Guryan and Charles 2013). In our view, it is likely that bureaucrats are driven by both of these factors simultaneously. Bureaucrats choose which constituents to represent based on the internal beliefs/prejudices that they hold as well as their intuitions about how much time, energy, and effort it will take to do so. Ultimately, it is not our goal to completely tease apart these competing explanations. Our primary goal is to explore the first-order question of whether religious discrimination exists in the domain we study. That being said, we do attempt to shed some light on what mechanisms might be in play. In our experiment (described more fully later), we manipulate the perceived costliness of constituents' requests. While our experiment does not allow us to test the role of convictions (which are tricky to manipulate experimentally, especially in the field), it does shine light on the extent to which costs play a role.

What drives the behavior of bureaucrats specifically in the education domain? As is true with other SLBs, public school principals face many competing demands on their time in their role as frontline bureaucrats. Principals serve as a hub in working with students, teachers, administrators, parents, and other community members. Principals play a key role in making sure that schools function well and that social divisions do not arise in a way that harms learning. Given the time constraints they face, public school principals have to be strategic in how they allocate their time; inevitably, they are forced to make decisions about whose needs to prioritize. Although principals are not elected by the public, they have a strong incentive to cater to their constituents' needs and wants. Doing so is vital for principals since in many states school officials' positions are quite precarious, especially under current performance management regimes (Holbein 2016; Holbein and Hassell 2018; Moynihan 2008; Moynihan and Ingraham 2003; Moynihan, Pandey, and Wright 2011; Smith and Larimer 2004).

The Potential Roots of Religious Discrimination

Exploring the extent to which SLBs discriminate on the basis of religion is vitally important not only because such discrimination is illegal but also because the role of religion in American society is changing. The United States stands out among Western

democracies not only for its relatively high level of religiosity but also for its religious diversity. In striking contrast to the citizens of many other Western democracies, an overwhelming majority of Americans continue to profess religious belief. At the same time, the religious landscape has been reshaped in recent decades as mainline groups have declined, religious diversity has increased, and a growing share of Americans identify as nonbelievers (Putnam and Campbell 2010; Sherkat 2014). These changes raise questions about the ability of SLBs—particularly in the education domain—to observe the civil rights of religiously diverse American families.

Citizens of all religious creeds as well as committed nonbelievers enjoy formal protection from harassment and equal rights in the public domain. In practice, however, the religious liberties of citizens appear to be frequently violated (Lippy 2006; Peek 2010). One can cite many examples of religious discrimination in public schools. In 2004, for instance, the Department of Justice sued a school for prohibiting a Muslim girl from wearing a headscarf (*Hearn and United States v. Muskogee Public School District*). In 2007, a high school student was kicked off the women's basketball team for refusing to take part in the Lord's Prayer (*Smalkowski v. Hardesty Public School District*). In 2012, a high school student was subject to harassment after asking that a prominently displayed prayer banner be removed from a public school (*Ahlquist v. Cranston*).

Despite these stark examples, far too little research exists detailing the extent and causes of religious discrimination.[11] Most of the existing research on diversity and discrimination in American public education—and on the services provided by SLBs more generally—focuses on ethnic and racial disparities in performance and enrollment (e.g., Johnson, Crosnoe, and Elder 2001; Kao and Thompson 2003; Roscigno 1998). At present, we simply do not know whether cases of religious discrimination such as the ones cited here represent exceptional incidents or merely the tip of the iceberg. Given the lack of systematic, rigorous evidence it is impossible to gauge the extent of religious discrimination in American public institutions in general and public schools in particular.

Potential Sources of Religious Discrimination in American Public Education

Why might principals discriminate on the basis of religion? Here, we briefly introduce three theoretical perspectives that motivate our hypotheses. These perspectives focus on the influence of secularism, Judeo-Christian nationalism, and civil religion. They originate in the sociology of religion (e.g., Norris and Inglehart 2004; Swatos and Christiano 1999; Wald and Wilcox 2006) and work on religion within public administration research (e.g., Garcia-Zamor 2003; Houston, Freeman, and Feldman 2008; King 2007)—literatures from which sociologists, psychologists, and political scientists frequently draw when studying the role of religion in American society. Together, these perspectives allow us to make explicit predictions about the effects of signaling (non)religious beliefs and the role that a key mechanism—the perceived costs attached to the enrollment of students from households with intense beliefs—plays in prompting discrimination.

We pause here to briefly note that we rely on these theoretical perspectives to help us understand the possible extent and shape of religious discrimination in American public schools. Our goal is *not* to test one theory in isolation or to conduct a "horse race" between different theories. Given that multiple forces are almost certainly at play, doing so would be far too simplistic. The theories that motivate our experiment are *not* mutually exclusive, but instead complement each other in helping us formulate multifaceted expectations for whether, where, and how religious bias among SLBs might operate.

Secularism

Secularization theory proposes that modernization propels the decline of religion at the level of institutions, attitudes, and beliefs (Norris and Inglehart 2004; Swatos and Christiano 1999; Wald and Wilcox 2006). Although the United States has long been considered an exception to the secularization thesis, recent developments suggest that secularization processes are starting to unfold in the United States (Garcia-Zamor 2003; Houston, Freeman, and Feldman 2008; King 2007; Putnam and Campbell 2010; Sherkat 2014; Voas and Chavez 2016).

Clashes over the secular nature of the public school system, the limits of religious accommodation, and state support for religious activities are nothing new (Alexander and Alexander 2012; Garcia-Zamor 2003; Houston, Freeman, and Feldman 2008; Justice and Mcleod 2016; King 2007; Matzke 2015). Historically, Protestant churches exerted substantial influence over public schooling. Religious discrimination toward minority faith communities and newcomer religions was commonplace (Reese 2011). Although campaigns to extend church-state separation faced substantial resistance from religious conservatives, they were remarkably successful. Despite popular religiosity, American public education has secularized more rapidly and thoroughly than the education systems of many other Western democracies (Houston, Freeman, and Feldman 2008; Maryl 2016).

The result is a tension between a procedural secularism, which guarantees that public institutions hold no religious preference, and a programmatic secularism, which insists that the public sphere admit no religion (Garcia-Zamor 2003; Houston, Freeman, and Feldman 2008; King 2007; Williams 2015). This tension generates unease about addressing religion in school (Justice and Mcleod 2016; Hartmann et al. 2011, 330). School officials are trained to be zealous guardians of church-state boundaries and to embrace secular norms in public education. Essex's (2016) widely used *School Law and the Public Schools: A Practical Guide for Educational Leaders* is very clear in this regard. It instructs principals and administrators that the law "compels public schools as state agencies to maintain a neutral position in their daily operations regarding religious matters" (17) and insists that they are legally obligated to refrain from endorsing religious symbols, devotions, and expressions at school and to avoid supporting students' religious activities (16–47).

The secularism perspective predicts that principals will react negatively to parents who reveal their religious beliefs in their emails. This prediction is captured in our first hypothesis:

> **Bias against religious clients hypothesis (hypothesis 1):** Parents who signal their religious beliefs will be less likely to receive a reply.

Judeo-Christian Nationalism

Secularization notwithstanding, religion continues to play a central role in establishing the boundaries of American national identity (Bonikowski and DiMaggio 2016; Hartmann et al. 2011). Christianity is especially important in subjectively defining "legitimate" membership in the American nation (Gerteis 2011; Wakefield 1976). In a representative sample of Americans, 65 percent of respondents reported that Christianity was a "fairly" or "very important" criterion for being considered "truly American" and nearly half (48 percent) said it was "very important" (Bonikowski and DiMaggio 2016, 955).

Christian identity has expanded since the mid-twentieth century, with intellectuals and politicians drawing heavily on explicitly "Judeo-Christian" religious discourses to construct the moral boundaries of America (Neuhaus 1986). Judeo-Christian political ideology was particularly useful in helping to integrate generations of White immigrants from Southern and Eastern Europe. While compared with the sectarianism of the past, the Judeo-Christian formulation was inclusive, it was inclusive only up to a point (Douthat 2013). With the acceleration of immigration from non-European countries that began in the late 1960s, the American religious landscape became far more diverse. In practice, newcomer religions have quickly adapted to American denominationalism (Berger 2007; Hirschman 2004). Nevertheless, from the 1990s onward, conservatives prominently reasserted claims about America as a Judeo-Christian nation (Hartmann, Zhang, and Wischstadt 2005; Wilcox and Robinson 2011). Conservative Protestantism thrives, at least in part, because its leaders portray it as the embattled defender of "true" American values (Lindsay 2007; Sutton 2014).

The limits of inclusion are apparent in the unease of many Americans toward members of unusual religious groups and religious newcomers (Bonikowski and DiMaggio 2016; Edgell, Gerteis, and Hartmann 2006; Tremblay-Boire and Prakash 2019). Anti-Muslim discourse resonates with many Americans, particularly the more than 30 percent who identify with conservative Protestantism (Pew Research Center 2015) and the quarter of Americans who can be classified as ardent nationalists (Bonikowski and DiMaggio 2016). In the context of a Judeo-Christian understanding of national identity and moral belonging, Muslim Americans pose a special problem, particularly in the wake of the 9/11 terrorist attacks and the war on terrorism.

In their analysis of American nationalism, Bonikowski and DiMaggio (2016) found that those Americans they classify as "ardent" nationalists are overwhelmingly White conservative Protestants who are prone to exclude religious minorities from those they consider truly American. In 2016, Donald Trump, who received about 80 percent of the White conservative Protestant vote, made the depiction of Muslims as outsiders a prominent theme in his campaign (Braunstein 2017). Hence, a theoretical reason for bias against Muslims may be moral judgment. In the context of the politicization of Islam as a supposed threat to American society and values, principals might be reluctant to assist Muslim families in enrolling in their schools either because of their own moral bias or because of what they consider to be prevailing community standards. This prediction is outlined in our second hypothesis:

> **Bias against religious minorities hypothesis (hypothesis 2):** Parents who signal their Muslim beliefs will be less likely to receive a reply.

Civil Religion

In American society, religion has long been an important source of conceptions about political community and social belonging. Public opinion data show that Americans across the political and racial spectrum remain notably hostile in their attitudes toward nonbelievers in roles of political leadership or as appropriate marriage partners for their children (Cragun et al. 2012; Edgell, Gerteis, and Hartmann 2006; Edgell et al. 2016).

Many Americans see the United States as a religious country (Noll 2002). Scholars of civil religion argue that this is envisioned not in terms of an established church or favoritism toward a particular denomination but as a consensus about the importance of religion for society. While a majority of Americans tolerate religious diversity, the explicit rejection of religion is intolerable to them (Edgell, Gerteis, and Hartmann 2006; Edgell et al. 2016). Edgell, Gerteis, and Hartmann (2006, 230) report that "[a] theists are at the top of the list of groups that Americans find problematic in both public and private life" (see also Cragun et al. 2012). Survey research finds that anti-atheist bias in the United States is "persistent, durable, and anchored in moral concern" (Edgell et al. 2016, 629). Recent experimental research reveals that people intuitively judge atheists as immoral (Gervais 2014) and regard them as lacking prosocial values (Simpson and Rios 2017). Consequently, atheists are strongly associated with immorality and contempt for common values—they are seen as "moral outsiders" in American society (Edgell, Gerteis, and Hartmann 2006, 227).

Distrust toward nonbelievers extends to attitudes about schools. More than one-third of Americans in recent General Social Surveys say that atheist teachers should be fired (Sherkat 2014, 159). Many Americans appear to believe that, by openly rejecting religion, atheists are rejecting the normative foundations of community and the broader civic good. This suggests that the mechanism producing bias against atheists is moral judgment. If parents identify themselves as atheists they may invite suspicion from principals who fear that atheists and their children would be immoral, ideologically strident, and likely to opt out of civil rituals (such as the Pledge of Allegiance). Principals might be reluctant to assist atheist families in enrolling in their schools either because of their own moral bias or because of what they consider to be prevailing community standards. This prediction is outlined in our third hypothesis:

> **Bias against atheist clients hypothesis (hypothesis 3):** Parents who signal their atheist beliefs will be less likely to receive a reply.

Intensity of Beliefs

Finally, in line with our previous discussion, we expect discrimination to increase if the intensity with which beliefs are held increases. If parents explicitly mention their religious or atheist beliefs in their emails to inquire about the compatibility of the school with their beliefs or request the accommodation of their beliefs, principals might perceive them as more costly clients,

making it more likely that they will ignore their request for help. This prediction is outlined in our fourth hypothesis:

> **Bias against costly clients hypothesis (hypothesis 4):** Parents who inquire about the compatibility of the school with their beliefs or request the accommodation of their beliefs will be less likely to receive a reply.

Previous Experimental Research on Discrimination

Now that we have laid out why SLBs might discriminate on the basis of religion, we turn to discussing how we can test for discriminatory behavior. Doing so is not easy. As Lipsky (2010, 112) notes, "it is difficult to assess equity of treatment [by SLBs]. In some instances it is against the law to collect data that would be necessary to demonstrate patterns of bias. In other instances there are no sound indicators of service quality, so it is [often] impossible to assess workers in this respect."

Despite these challenges, over the last decade, public administration scholars across a variety of disciplines have advanced the study of discrimination by using correspondence experiments to address the well-known limitations of surveys and observational studies in demonstrating bias (Bertrand and Duflo 2016; Costa 2017). Correspondence experiments measure discrimination by estimating how the willingness of public officials to respond to requests for help or information is affected by variation in putative clients' characteristics such as race, gender, or class. If we detect differences in response rates across such randomly assigned "treatment" groups, we can infer the presence of discrimination. Correspondence studies, which focus on officials' willingness to respond to messages from strangers, cannot capture all forms of discrimination. However, their advantage relative to Implicit Association Tests, resentment scales, or other means of measuring bias at the individual level is that randomization allows us to rule out the presence of confounding factors. Moreover, they demonstrate discriminatory *behavior*, not just discriminatory attitudes or beliefs.

Correspondence experiments have been used to examine whether bias occurs in response to group-based identification on the basis of race, class, ethnicity, gender, or sexual orientation (e.g., Blommaert, Coenders, and van Tubergen 2014; Butler 2014; Butler and Broockman 2011; Carnes and Holbein 2018; Gaddis 2015; Jilke, Van Dooren, and Rys 2018; Kalla, Rosenbluth, and Teele 2018; Neumark 2012; Pager and Shepherd 2008; Pedulla 2016; Porter and Rogowski 2018). A few experimental studies have also examined the potential for religious discrimination in the workplace. In an influential set of articles, Wright et al. (2013) and Wallace, Wright, and Hyde (2014) found that U.S. job applicants expressing a religious identity were less likely than those who did not to receive a response from a potential employer, with minorities such as Muslims and atheists suffering the greatest bias and evangelical Christians and Jews suffering little or no discernible bias. The United States does not appear to be unique in this regard. In France, not only do Muslims have lower incomes than matched Christian households, but a Muslim job candidate is about 2.5 times less likely to receive a job interview callback than a racially similar Christian counterpart (Adida, Laitin, and Valfort 2010).

A small but rapidly growing literature examines various biases among *elected* public officials (e.g., Broockman 2013; Butler 2014; Butler and Broockman 2011; Costa 2017).[12] Previous research, however, has paid less attention to the question of whether *appointed* public officials exhibit bias. Moreover, studies involving both elected and appointed public officials primarily focus on partisan and racial discrimination (e.g., Einstein and Glick 2017; White, Nathan, and Faller 2015). To our knowledge, no experimental research exists on religious biases in SLBs such as American public school principals.[13] Finally, because of the challenges involved in conducting correspondence studies involving political elites, such studies rarely have the ability to experimentally explore theoretically driven potential mechanisms or to investigate treatment effect heterogeneity with sufficient statistical power.

Research Design and Data

We use a large-scale correspondence experiment to investigate religious discrimination by PK–12 principals.[14] Our experimental sample consists of regular, operational, noncharter public PK–12 schools in 33 U.S. states. We included all states for which we were able to acquire principals' email addresses either by contacting state department of education or by downloading contact information from the websites of those institutions. Within these 33 states, we dropped all schools with missing principal contact information. We also excluded schools that could not be uniquely matched to NCES (National Center for Education Statistics) data and schools with missing covariate data in the NCES or American Community Survey (ACS). Based on state and NCES data, we dropped inactive, private, charter, nontraditional, adult, and virtual schools, as well as schools serving restricted populations such as schools for the blind and deaf and schools located on military bases. We also excluded schools with less than 100 students, schools that are majority American Indian, and schools that offer prekindergarten or kindergarten as the highest grade. If several schools shared a principal we only kept one of the schools, chosen randomly. If several schools were located at the same physical address we only kept one of the schools, also chosen randomly.[15]

Based on these selection criteria our sample size equaled 47,550 schools. When we conducted our correspondence experiment some of our emails could not be delivered due to misspelled or outdated principal email addresses.[16] Hence, our final sample size equals 45,710 schools.[17] The reason for our large sample size relative to previous correspondence studies is that we desire to (1) precisely estimate the effect of multiple main treatment conditions (i.e., religious affiliation/nonaffiliation), (2) precisely estimate experimentally assigned second-order conditions (i.e., intensity of beliefs) to evaluate a theoretically driven potential mechanism, and (3) precisely estimate heterogeneous treatment effects across a host of contextual variables. Each of these—especially (2) and (3)—requires higher statistical power than previous correspondence studies. Indeed, recent research shows that tests for heterogeneous treatment effects are often woefully underpowered (Blair et al. 2018; Fink, McConnell, and Vollmer 2014). As can be seen in our results, our estimates are precise, but not so precise as to suggest that our study's sample size is exorbitant.

As in all correspondence studies, our outcome of interest is whether an individual responds to our inquiry. While this measure is not

perfect, the justification for using it is that it gives us a glimpse into real-world behavior, thereby offering an improvement over survey-based measures of bias. Seeking to learn even more from correspondence studies, some readers might suggest that in addition to exploring whether principals respond, we should measure characteristics of their responses such as how helpful they were. The problem with doing this is that such an approach implicitly conditions on a post-treatment variable (getting a response in the first place), thus risking post-treatment bias (Montgomery, Nyhan, and Torres 2018). Using measures that are only defined for the subset of subjects who responded "'de-randomizes' an experiment in the sense that the resulting treatment and control groups no longer have potential outcomes that are in expectation equivalent" (Coppock 2019), turning an experiment into a poorly designed observational study. For this reason, we follow other correspondence studies in focusing on response rates.

We observe a number of covariates drawn from the NCES (2013), ACS (2012), and the Religious Congregations and Membership Study (RCMS) (2010). From the NCES, we observe the share of Asian, Hispanic, Black, and White students at the school level. We also observe the share of students eligible for free or reduced price lunches, the share of male students, the school size, and the pupil/teacher ratio. From the ACS, we observe the median household income, the share of adults holding a bachelor's degree, and the share of residents with income below the poverty line at the county level. We also observe county-level Republican vote shares in the 2012 presidential elections. From the RCMS, also at the county level, we have the rates of Black Protestant, evangelical Protestant, mainline Protestant, Catholic, Muslim, and total adherents per 1,000 capita.[18]

The plots in Figures A2–A4 in the Supporting Information compare our sample to the NCES population of 78,348 regular, noncharter public schools without missing NCES data in the 48 contiguous U.S. states. While our experimental sample is not truly a random sample from this NCES universe, it tracks the NCES population rather well in terms of observed covariates.

We randomly assign the *religious affiliation/nonaffiliation* of the parent (no information given, Protestant, Catholic, Muslim, or atheist). To unpack treatment effects and explore the mechanisms behind religious biases, we also randomize the *intensity* with which beliefs are communicated: low (identification), medium (identification + compatibility inquiry), or high (identification + accommodation request). This allows us to explore whether discrimination is indeed driven by higher perceived costs associated with families' greater intensity of belief, as we have theorized.

As a secondary part of our design, we also randomize the parent's and child's gender to rule out the possibility that our causal inferences about religious affiliation/nonaffiliation or intensity of belief are limited to a particular gender or gender combination. We signal the parent's and child's gender by using different names: Isaac and Rebekah Adam for the parents and Jonah and Sarah for the children.[19] We chose these first and last names because they frequently appear in the Old Testament, an important religious text for both Christians and Muslims.[20] As each of these names is relatively common in the United States, atheists with these names are also not unusual. Given the large number of emails we had to send out, we used several email accounts to contact principals. We sent emails during a one-week period in April 2016; the order in which principals were contacted was randomized. The text of our emails is shown in figure 1.

We include a Catholic treatment in our experiment to ensure that discrimination against Christians is not being driven by political hostility toward conservative Protestants. Catholicism is liturgically and theologically distinct from Protestantism and readily culturally identifiable. As a religious group, contemporary Catholics are a good benchmark because they are ethnically diverse and close to the U.S. mean on many demographic characteristics including education, income, and political preferences. In terms of the American religious spectrum, Catholics, on average, identify as religious moderates (Sherkat 2014). They furnish a better reference category than "mainline" Protestants because liberal Protestants are, on average, similar to the unchurched in attitudes and values. Accordingly, a mainline Protestant identification as a religious signal would not be as resonant as Catholicism.

The literature on religious discrimination has often focused on the Jewish experience (Davidson and Pyle 2011). When designing our study we decided to focus on Islam rather than Judaism as our non-Christian minority religion of interest. Obviously, we are not claiming that Jews do not experience religious discrimination. Rather, our decision was informed by the fact that, at the time of our study, survey evidence consistently showed that Americans were more favorable toward Jews than toward any other religious group and that anti-Semitism was widely disapproved of (Rebhun 2016).[21]

As an improvement over many other correspondence studies, we designed our experiment to also test a causal mechanism that is potentially causing discrimination. To be abundantly clear, we are not claiming that it is the only possible mechanism. Given the likely presence of unobservable mediators (Bullock, Green and Ha 2010), any such claim would be imprudent. Because of our large sample size, we were able to include in our design treatments that test the hypothesis that principals expect families with more intense beliefs to be more costly. This allows us to explore the role of one of the core mechanisms theorized to drive bureaucratic behavior (as we discussed earlier). To do so, we experimentally signal religious identity and the intensity with which beliefs are held in the following way. The low-intensity condition signals religious affiliation/nonaffiliation only through an email signature at the bottom of the email, in purple color. The email signature contains a modified version of a Richard Dawkins quote ("[. . .] teaches that life is precious and beautiful. We should live our lives to the fullest, to the end of our days"). This quote is sufficiently bland (and obscure) that it could be reasonably attributed to virtually any source. We substitute "Christianity," "Catholicism," "Islam," or "atheism" into the quote, depending on the religious affiliation/nonaffiliation treatment. We also change the purported author of the quote to the Reverend Billy Graham, Pope Benedict, the Prophet Muhammad, or Richard Dawkins, again depending on the religious affiliation/nonaffiliation treatment.

The medium-intensity condition keeps the signature but adds the following sentence, which is designed to signal the desire for

Subject: School visit?

Dear principal,

Hello. My family and I will be moving into the area sometime this summer. Right now, we are deciding where exactly to move and are looking at schools for our [son/daughter], [Jonah/Sarah]. Before we pick a place to live, we would like to meet with you or a member of your staff and chat a bit about your school. Would that be possible?

[A] [One of the reasons we would like to meet with you is that we are raising [Jonah/Sarah] to be a good [Christian/Catholic/Muslim/Atheist Humanist] and want to make sure that this would be possible at your school.]

[B] [One of the reasons we would like to meet with you is that we are raising [Jonah/Sarah] to be a good [Christian/Catholic/Muslim/Atheist Humanist] and want to protect [him/her] from anything that runs counter to our beliefs. We want to make sure that this would be possible at your school.]

Sincerely,
[Isaac Adam/Rebekah Adam]

[C] [Catholicism/Christianity/Islam/Atheism teaches that life is precious and beautiful. We should live our lives to the fullest, to the end of our days. – Pope Benedict/Rev. Billy Graham/The Prophet Muhammad/Richard Dawkins]

Note: Emails revealing no information about the parent's religious affiliation/nonaffiliation exclude text blocks A, B, and C. Among emails that do reveal religious affiliation/nonaffiliation, low-intensity requests include C (but not A or B), medium-intensity requests include A and C (but not B), and high-intensity requests include B and C (but not A).

Figure 1 Email to Principals.

compatibility between the school and the beliefs of the family: "One of the reasons we would like to meet with you is that we are raising [Jonah/Sarah] to be a good [Christian/Catholic/Muslim/Atheist Humanist] and want to make sure that this would be possible at your school."[22] The high-intensity condition likewise keeps the signature but adds the following sentence, which is designed to signal a request for the accommodation of the family's religious beliefs: "One of the reasons we would like to meet with you is that we are raising [Jonah/Sarah] to be a good [Christian/Catholic/Muslim/Atheist Humanist] and want to protect [him/her] from anything that runs counter to our beliefs. We want to make sure that this would be possible at your school." The no information given condition only contains the gender treatments.

Treatments were randomly assigned within blocks defined by state; shares of Asian, Hispanic, Black, and White students; percentage of students eligible for free or reduced price lunches; median household income; share of adults holding a bachelor's degree; share of residents with income below the poverty line; and Republican vote share in the 2012 presidential elections. Table A1 in the Supporting Information shows that our sample is well balanced.[23] We also compute an omnibus randomization inference *p*-value that tests for joint balance across all 18 covariates. This *p*-value equals .90, confirming that the blocked randomization procedure was successful in balancing observables.

We sent a single email to each principal with no follow-up in case of nonresponse. We then observed whether principals replied to our email within a 14-day window from the time the email was

sent.[24] Automatic replies such as out-of-office replies were discarded. We use receipt of a nonautomated reply email as our binary outcome variable.[25] We recognize of course that nonresponse results from many sources besides bias. For example, each principal's responsiveness is undoubtedly affected by factors such as his or her work load. That being said, we are interested in systematic differences across randomly assigned groups of principals exposed to different emails. While we cannot interpret nonreply by any individual principal as a sign of prejudice, the presence of systematic differences in responsiveness between randomly assigned treatment groups *does* constitute evidence of discrimination (Bertrand and Duflo 2016; Butler and Broockman 2011).

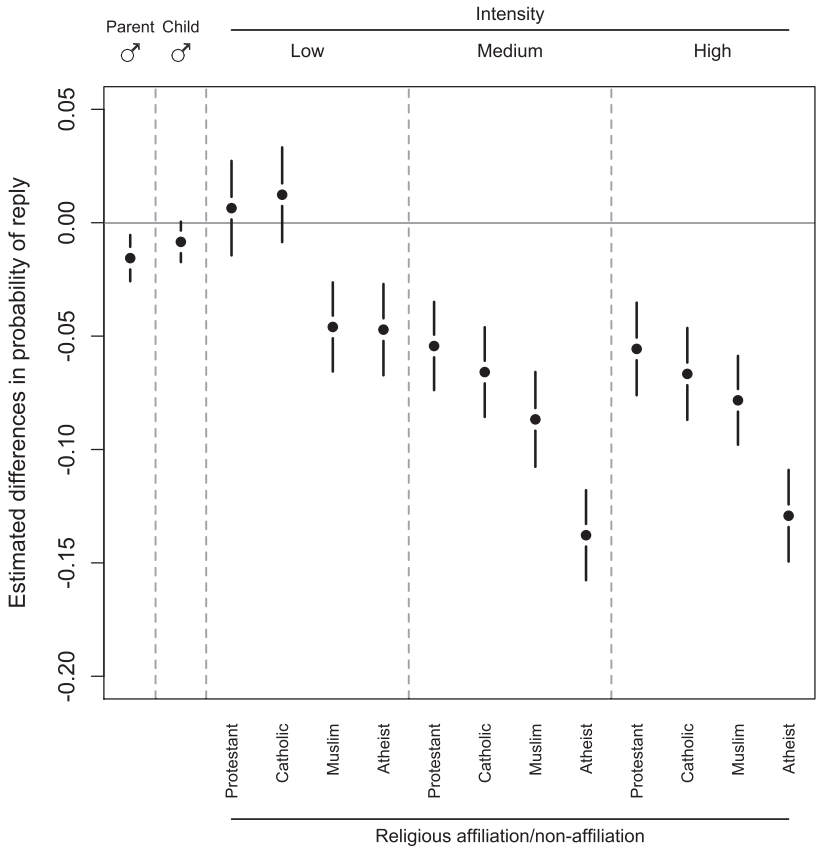
Empirical Results

Among the 45,710 subjects, 19,691 sent at least one nonautomated reply email within 14 days, for a response rate of 43.1 percent. This response rate is in line with response rates from other internet correspondence experiments with elected and appointed public officials (Costa 2017).[26]

Table A2 in the Supporting Information shows results from a probit model. Because we are interested in the interaction between the religious affiliation/nonaffiliation treatment and the intensity treatment, we include dummy variables representing all *combinations* of the religious affiliation/nonaffiliation and intensity treatment levels in the model. The model also includes dummy variables for parent's and child's gender as well as fixed effects for the email accounts we used to send emails (coefficient estimates not shown). Robust standard errors are clustered at the school district level.

We use a plot to visualize the main empirical results of our experiment. Based on the probit estimates in table A2 in the Supporting Information, figure 2 plots treatment effects (i.e., differences in probabilities). Treatment effects of male names are in comparison to female names. Treatment effects for the 12 religious affiliation/nonaffiliation and intensity combinations are in comparison to the baseline condition in which we do not provide any information about the religious affiliation/nonaffiliation of the family.[27]

For the religious affiliation/nonaffiliation and intensity treatments and comparing with the baseline (no information) condition (while averaging over the gender factors), we find the following patterns. For the religious affiliation/nonaffiliation treatment paired with the low-intensity condition, the effects of Protestant and Catholic affiliation are slightly positive but not statistically significant at the .10 level. This suggests that simply signaling membership in a mainstream religious group has no effect on the probability of getting a reply—principals do not discriminate against families belonging to these mainstream religious groups when costs of enrollment are not signaled.



Note: The plot shows estimated differences in probabilities of receiving a reply (i.e., treatment effects) and 95% confidence intervals based on the probit model in table A2 in the Supporting Information. Robust standard errors are clustered at the school district level.

Figure 2 Estimated Treatment Effects Based on Model in Table A2 in the Supporting Information.

An affiliation with Islam, on the other hand, even if signaled solely through the email signature (and not the text of the email itself), reduces the probability of reply by 4.6 percentage points, an effect that is highly statistically significant ($p < .001$). The effect size for atheist email signatures is very similar, reducing the probability of reply by 4.7 percentage points compared with the baseline condition ($p < .001$). These effects are substantively important, amounting to just over 10 percent of the overall response rate. They are slightly smaller (but not statistically distinct from) the race/ethnicity effects reported in a correspondence study of state legislators (Butler and Broockman 2011). The results demonstrate that a clear bias exists against these minority groups and that (as best we can tell) this bias is of the order of the large race/ethnic biases found in studies of elected officials. This bias is present even when costliness is not explicitly signaled.

Our results are both substantively and statistically identical if we additionally control for block fixed effects (table A3 in the Supporting Information). Using linear probability models instead of probit also does not affect our results (table A4 in the Supporting Information). The same is true when we control for the covariates listed in table A1 (table A4 in the Supporting Information). Finally, our findings are also completely unaffected by dropping Massachusetts from the sample (see discussion in note 17; table A5 in the Supporting Information). Finally, female parents and female children are slightly more likely to receive a reply, but the differences are substantively small and not always statistically significant.[28]

Potential Mechanism: Perceived Costs of Intense Beliefs

To unpack these effects, we next examine the intensity treatments that randomly assign principals to higher perceived costs either by parents' inquiring about the compatibility of the school with the family's beliefs or by requesting accommodation of the family's beliefs. If the religious discrimination that we have observed is driven by perceived costs, we would expect to see even larger effects for these treatment conditions.

In practice, this is exactly what we find. In the medium-intensity condition, in which parents inquire about the compatibility of the school with their beliefs, discrimination increases for all four religious affiliations/nonaffiliations. Compared with the low-intensity condition, effect estimates increase by $-4.6 - (-8.7) = 4.1$ percentage points for Muslims ($p < .002$) and $4.7 - (-13.8) = 9.1$ percentage points for atheists ($p < .001$). This suggests that the SLBs in our sample discriminate against Muslims and atheists (in part) because they perceive that serving such families would impose costs on them. Such costs could arise because these families are perceived to make demands on schools that are illegitimate or difficult to accommodate or because other members of the school community might object to their presence, causing conflicts that principals would prefer to avoid completely.

Interestingly, we also observe discrimination against mainstream religious groups when they signal a greater intensity of belief. For Protestants, the estimated increase in discrimination is $0.1 - (-5.4) = 5.5$ percentage points ($p < .001$); for Catholics, it equals $1.2 - (-6.6) = 7.8$ percentage points ($p < .001$). The increase in discrimination is much larger for atheists than for Muslims (and also somewhat larger than for Catholics and especially Protestants), suggesting that the perceived costs of dealing with parents inquiring about the compatibility of the school with their atheist beliefs are particularly high, which fits with the view that atheists are the most problematic "moral outsiders" in American society. At the same time, even mainstream religious groups are seen as problematic when they explicitly raise the question of the compatibility of their religious beliefs with the school.

Looking at the levels of discrimination (i.e., the decrease in responsiveness compared with the baseline condition) in the medium-intensity condition, discrimination against Muslims is somewhat larger than discrimination against Protestants and Catholics ($\chi^2_2 = 6.80, p = 0.03$) and discrimination against atheists is substantially larger than discrimination against Muslims ($\chi^2_1 = 17.72, p < .001$). While mainstream religious groups are penalized for beliefs of greater intensity and the accompanying perception that they are costly to deal with, Muslims and especially atheists are punished even more.[29]

Effect estimates for the high-intensity condition are very similar to estimates for the medium-intensity condition. We cannot reject the null hypothesis that effects in the medium and high-intensity conditions are the same ($\chi^2_4 = 0.95, p = 0.92$).

In the Supporting Information, we investigate treatment effect heterogeneity. Surprisingly, we find little evidence that treatment effects vary with the social context in which principals are embedded. We also formally generalize our results to the NCES population of 78,348 regular, noncharter public schools in the 48 contiguous U.S. states without missing data. Results are virtually identical.

Discussion

Consistent with hypothesis 3, we find discrimination against atheists even in the low-intensity condition. That is, merely revealing parents' atheist beliefs without actually mentioning them in the text of the emails is sufficient to induce a sizable drop in responsiveness. In line with hypothesis 2, the same is true for Muslims, for which we also find that principals are less likely to reply even in the low-intensity condition, where the only difference between Muslim parents' emails and the control emails is in the email signature. Contrary to hypothesis 1, we do not detect any discrimination against Protestant or Catholic parents if such parents signal their religious beliefs merely through their email signatures. In stark contrast to how atheist and Muslim parents are treated, Protestant and Catholic parents who do not explicitly mention their religious beliefs in the text of their emails do not experience a drop in responsiveness. This suggests that the effect we observe for Muslim parents is not due to their religiousness as such (as hypothesis 1 would predict) but the (to many Americans) unfamiliar and foreign nature of their religious beliefs and the popular association between Islam and extremism.

Finally, as predicted by hypothesis 4, we find that discrimination against both believers and atheists increases starkly as we move from the low-intensity condition to the medium-intensity condition in which parents explicitly inquire about the compatibility of the school with their beliefs. Unexpectedly, moving from medium intensity to high intensity does not further increase the extent of discrimination. It is possible that differences in language between the medium and high-intensity conditions were not sufficiently

large to induce additional discrimination.[30] Alternatively, it is conceivable that the language we used for the medium-intensity condition already implied a future request for religious accommodation, so that principals refrained from responding to these emails at the same rate as to emails that explicitly raised the question of religious accommodation.

Overall, we find marked differences in the extent of discrimination between the low-intensity condition, on the one hand, and the medium- and high-intensity conditions, on the other. This result supports our argument that one causal mechanism behind the discrimination we observe is the perceived costs of dealing with parents who hold strong religious or atheist beliefs. Parents who are seen as more "difficult" to deal with by SLBs are less likely to receive a reply to their requests for help in enrolling their children in public school.

Limitations

Correspondence studies, and randomized trials more generally, are designed to identify the average causal effects of specific treatments—in our study, signals of religious affiliation/nonaffiliation and intensity of belief embedded in parents' emails. Our experimental design allowed us to test one of the most theoretically compelling mechanisms (i.e., perceived costs) that could explain our results, but our findings suggest that cultural mechanisms may also be in operation. Nor does our correspondence study speak directly to the many other situations in which principals (or other SLBs) might engage in religious discrimination. For example, it is possible that parents who contact principals with a straightforward request for application forms might experience less discrimination than the parents in our experiment, who were asking for a more costly service (a meeting with the principal) without firmly committing to enroll their children in school.[31] Our study also does not speak directly to the question of whether students of different religious backgrounds are treated differently once they are enrolled in public school. That is a different research question that correspondence experiments cannot address. Our study shares these limitations with other correspondence studies (Guryan and Charles 2013).[32] While it is important to note these limitations, our article makes an important contribution by documenting for the first time that significant religious discrimination takes place when American parents interact with public school principals. Moreover, our results suggest that one mechanism driving this discrimination is the perceived costliness of dealing with religious parents and "moral outsiders."

Conclusion

In this article, we provide empirical evidence that even when norms of equal treatment are actively extolled and incentivized, discrimination is against the law, SLBs are overseen by a strong and well-established hierarchy, and SLBs are watched by an active public—as is the case in the U.S. public school system—inequitable treatment may still occur. Our correspondence experiment involving the principals of more than 45,000 public PK–12 schools provides clear evidence that religious discrimination is taking place in American public schools. Such discrimination appears to be driven, at least in part, by a perception that religious believers and atheists are prone to making illegitimate demands that impose costs on SLBs. For Muslims and atheists but not Protestants and Catholics, we document sizable discrimination even when they do not mention their beliefs in the text of their emails at all.

Our results are fundamentally important. First, they demonstrate that qualitative and survey-based evidence of religious discrimination is, indeed, evidence of a broader pattern of discrimination. Second, our results show that in administering their duties, the race, gender, and class of the client are not the only heuristics that SLBs use in deciding whether to respond to a request for help, a fact that some may (mistakenly) infer given the field's relatively narrow focus in the past. Finally, our results suggest that the forces commonly thought to preserve equitable treatment may be insufficient. In order to assure equal treatment, public systems must have more than legal protections, extolled and incentivized norms of equity, and public oversight.

Finally, our research speaks to practitioners in important ways. First and foremost, it lays out the importance and need for those who work within and oversee public schools to do more to protect students who hold religious or atheist beliefs. Practitioners and public administration scholars need to realize that the current state is not equitable. Religious discrimination is sizable and systemic. Acknowledging this fact alone is an important first step for administrators to take. Next, practitioners and scholars alike must act to come up with new ways to ensure that families are, in practice, given the protections they are legally entitled to. Public school principals, as vitally important SLBs, occupy a challenging role as mediators in the communities in which they work. However, this does not excuse them from doing more to ensure that religious discrimination is eliminated from public schools. The standard levers of oversight, extolled and incentivized norms, and legal protections may not be enough. Policies and programs designed to reduce the costs (real and perceived) that school officials face in catering to the needs of (non)religious minorities may be especially promising given the results that we have documented here.

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Notes

1. Lipsky (1980, 3) defines SLBs as "public service workers who interact directly with citizens in the course of their jobs, and who have substantial discretion in the execution of their work."
2. On the one hand, Sowa and Selden (2003, 700) argue that "administrators who perceive themselves as possessing significant discretion … are more likely to enact policy outcomes that favor minority interests." On the other, Maynard-Moody and Portillo (2010, 23) counter that "a system that depends on discretion will always retain potential for abuse, especially abuses based on institutionalized and culturally embedded stereotypes."

3. For notable exceptions, see Carnes and Holbein (2018); Harrits (2019); Jakobsen, Jacobsen, and Serritzlew (2019); Jilke and Tummers (2018); and Porter and Rogowski (2018). None of these studies explore whether there are religious biases in public service delivery.
4. By religious discrimination we refer to differential treatment based on religious affiliation or nonaffiliation. In line with the literature (e.g., Butler 2014), we use the terms "bias" and "discrimination" interchangeably.
5. The Constitution of the United States prohibits the state from discriminating against individuals or groups based on their religious identification. Beyond the Constitution, Title IV and Title IX of the Civil Rights Act of 1964 prohibit discrimination against students based on their faith. Case law clarifies that students can religiously identify at school and can take part in religious activities of their own devising. State constitutions also generally either make public education a fundamental right or contain protection clauses or their equivalents that prohibit religious discrimination (Alexander and Alexander 2012, 46).
6. To be absolutely clear, we are *not* arguing that public schools are perfectly equitable. Abundant research has shown that this is not the case (e.g., Reardon 2013). What we *are* arguing is that public school officials extol, and encourage, equity to a degree not realized in many other public sectors.
7. For comparison, Butler and Broockman (2011): n = 4,859; Hemker and Rink (2017): n = 408; Kalla, Rosenbluth, and Teele (2018): n = 8,189; and Carnes and Holbein (2018): n = 4,492.
8. See "U.S. Muslims Concerned about Their Place in Society, but Continue to Believe in the American Dream," Pew Research Center, July 26, 2017, https://www.pewforum.org/2017/07/26/findings-from-pew-research-centers-2017-survey-of-us-muslims/ (accessed July 31, 2020).
9. See "Americans Express Increasingly Warm Feelings Toward Religious Groups," Pew Research Center, February 15, 2017, https://www.pewforum.org/2017/02/15/americans-express-increasingly-warm-feelings-toward-religious-groups/ (accessed July 31, 2020).
10. In economics, these two explanations play out in discussions about taste-based versus statistical discrimination (Guryan and Charles 2013).
11. For exceptions in other social contexts (many of them outside the United States), see Adida, Laitin, and Valfort (2010) (in France); Gaikwad and Nellis (2017) (in India); McClendon and Riedl (2016) (in Kenya); Lajevardi and Abrajano 2019 (in the United States); and Lajevardi 2018 (in the United States).
12. Also see Broockman and Soltas (2018) for an innovative example of research on racial discrimination *against* elected officials (i.e., delegates).
13. The closest exception is Lajevardi (2018), which tests for religious bias among elected state legislators.
14. Our experiment has been approved by our Institutional Review Board. We discuss the ethics of our experiment in appendix S1 in the Supporting Information. Replication data are available upon request.
15. We dropped public charter schools from our sample since the process of applying to such schools tends to be different. Some public charter schools for example employ lottery systems. Since our emails could not be tailored toward the specific admission process and admission requirements at each school, principals at public charter schools might have found our emails to be suspicious.
16. We dropped all schools with bounced emails from the experimental sample. This is unproblematic because invalid or outdated email addresses are orthogonal to treatment assignment by virtue of randomization. Our results are unchanged if we treat principals with bounced emails as nonreplies.
17. The following 33 states make up the experimental sample (with number of schools in parentheses): AL (851), AR (761), CA (5892), CO (832), DE (135), FL (1902), GA (1636), IA (864), ID (389), IL (2519), IN (1486), LA (821), MA (1314), MI (1979), MN (813), MO (1412), MS (702), NC (2027), ND (161), NE (571), NH (326), NJ (1759), NM (412), NY (2904), OH (1974), RI (189), SC (930), TN (1067), TX (4723), VA (1427), VT (179), WA (1336), WI (1417). One complication arose during our experiment. In Massachusetts, our emails coincided with a malware attack targeting public schools. At least one principal thus forwarded our email to the Massachusetts State Police, which contacted all Massachusetts public school principals warning them that our emails were probably spam. We chose to keep Massachusetts in our sample since this warning only occurred one week after we had emailed principals; many Massachusetts principals had already replied by this point. Our results are entirely unchanged if we drop all Massachusetts schools. We have included this set of results in the Supporting Information.
18. Using Amelia II (Honaker, King, and Blackwell 2012), missing data in RCMS variables have been multiply imputed using the NCES and ACS variables listed earlier, the outcome variable, and an additional set of 18 ACS variables plausibly prognostic of religious adherence or missingness. All standard errors and statistical tests have been adjusted to account for multiple imputation.
19. Holding name constant allows us to separate out the effect of religion from ethnicity, which is frequently signaled by manipulating the name of the sender.
20. Of course, Muslims might be more likely to have Arabic versions of these names; "Jonah," for example, might be rendered as "Yunus." However, if we had used different names for different religious affiliations we would have conflated signals of religious affiliation with signals of race/ethnicity.
21. We designed and conducted our study well in advance of the 2016 presidential election and failed to foresee the reappearance of anti-Semitic tropes during the election campaign and, after the election, among the so-called alt-right. Our experimental design was informed by Wright et al. (2013) and Wallace, Wright, and Hyde (2014), who find minimal discrimination against Jews in the labor market. In hindsight, the inclusion of Jewish families would have been valuable.
22. We used "atheist humanist" as opposed to merely "atheist" in our emails since atheism as such does not have any ethical content. It would have sounded odd if parents had announced their intention to raise children to be "good Atheists." Moreover, we used "Christian" as opposed to "Protestant" in our emails since American Protestants typically refer to themselves as "Christian" and not "Protestant." In order to ensure that respondents would recognize the Protestant treatment in the signature line we attributed it to Billy Graham, one of the most famous Protestant clergymen of the late twentieth century.
23. We approximated exact randomization-based p-values using 1,000 randomly chosen blocked treatment assignments. The test statistic is the maximum Kolmogorov-Smirnov statistic across all two-way comparisons of treatment groups. The p-value is the fraction of test statistics at least as large as the test statistic in our sample.
24. Most principals responded within three business days, so a 14-day window is conservative.
25. We randomly sampled and read 500 reply emails. In almost all of them, principals either asked for times that we would be able to meet or proposed times for a meeting. In a few emails, principals asked us to provide additional information such as our moving date or our child's grade level. In eight emails, principals informed us that their schools did not offer school tours at the moment, typically because state testing was currently taking place. Seventeen reply emails were from former principals who suggested contacting the current principal, almost always either providing contact information for or copying the current principal. After discarding automatic replies, we thus feel confident in treating the receipt of a reply email as indication of a principal's willingness to meet with us. A typical reply was something like "Sure! When can you come in?" We should also point out that none of the replies suggested that principals found our emails suspicious in any way. As a descriptive exercise, we had originally planned to use text analysis tools to analyze and code the content and tone of reply emails, but the reply emails proved to be too uniform in content and too terse to make such an endeavor worthwhile. The major variation in responses is thus between getting a reply email and not getting a reply email.
26. A small number of principals sent several emails, typically to update times they

had mentioned in a previous email during which they would be available for a meeting.

27. Probability estimates are simulated using the observed values approach. Estimated probabilities for a given factor or factor combination average over the remaining factors. Figure A1 in the Supporting Information displays the underlying probability estimates.
28. There is no evidence of an interaction between parent's gender and child's gender: $\chi^2_1 = 0.19$, *p*-value = .67. There is also no evidence of interactions between the gender treatments and the religious affiliation/nonaffiliation or intensity treatments: $\chi^2_{24} = 26.73$, *p*-value = .32, suggesting that the discriminatory effects we observe are uniform across gender (a fact consistent with the other treatment heterogeneities we explore later). All Wald tests are two-sided.
29. Like most correspondence studies, our design does not throw light on whether this perception of increased costs is itself rooted in anti-Christian, anti-Muslim, or anti-atheist sentiment on the part of the principals. This distinction does not matter for our finding of religious discrimination but would be important for policy interventions designed to reduce such discrimination.
30. When designing the experiment we considered even more strident language for the high intensity condition but decided against it in order to safeguard the realism of our emails and the internal and external validity of our experiment.
31. We thank an anonymous reviewer for this insight.
32. Take correspondence studies of wage discrimination for example. Bertrand and Mullainathan (2004) show that Black and White job applicants with otherwise identical resumes are treated differently. It is possible of course that employers discriminate against Black workers when they review resumes but that Black workers are treated the same as white workers once they are hired. This possibility does not negate the fact that employers engage in illegal discrimination in at least one domain.

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Supporting Information

A supplemental appendix can be found in the online version of this article at http://onlinelibrary.wiley.com/journal/10.1111/(ISSN)1540-6210.