AN EXPERIMENT IN HIRING DISCRIMINATION VIA ONLINE SOCIAL NETWORKS [*]

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Abstract. Online services offer workers and firms a new channel for job market searching and matching. However, little is known about how job candidates' online information influences employers' hiring decisions. Leveraging and advancing a methodology common in résumé studies, we investigate how the hiring behavior of U.S. employers is affected by their online search activity and the information they find online about job candidates. We create profiles for job candidates on popular online social networks, manipulating information that is protected under either federal or state laws but that may be inferred from individuals' online presences. We submit job applications on behalf of the candidates to over 4,000 U.S. employers, and compare interview invitations for a Muslim candidate relative to a Christian candidate, as well as a gay candidate relative to a straight candidate. We estimate that a minority of U.S. employers searched online for the candidates' information. We find that discrimination against the Muslim candidate relative to the Christian candidate significantly varies with employer characteristics. Employers in areas with higher proportions of Republican voters were significantly less likely to call back the Muslim candidate. The results are robust to using either state- or county-level data, to controlling for firm, job, and geographical characteristics, and to various model specifications. The findings suggest that hiring discrimination via online searches of candidates may not be widespread, but online disclosures of personal traits can significantly influence the hiring decisions of a self-selected set of employers.

Keywords: discrimination, field experiments, online social networks, privacy

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1. Introduction

The rise of Internet and social media services such as blogs and online social networks has produced a new channel through which employers and potential employees can find information about each other. That information can facilitate and improve the matching between firms and workers, to mutual satisfaction. However, many job seekers also reveal online personal information that is not easily discovered in personal interviews, and which may even be illegal for employers to request or use in the hiring process. For instance, in most of the United States, if an employer were to ask about a job applicant's religious affiliation, it would risk being sued for discrimination under Equal Employment Opportunity laws. Seeking this information online, instead, reduces employers' costs in a variety of ways, including lowering the risk of detection. Thus, although online communications may facilitate labor market matching, they may at the same time create new arenas for labor market discrimination.

To date, no field data have revealed the actual frequency of employers' searches for prospective employees, or whether information found online can impact a firm's hiring decisions. Self-report surveys of employers suggest that U.S. firms frequently use various online services to research job candidates.[1] However, the Equal Employment Opportunity Commission (EEOC) has cautioned against risks associated with searching online for protected characteristics,[2] and in so doing it may have dissuaded some organizations from using social media in the hiring process. Some states have even drafted bills limiting employers' ability to access candidates' online information.[3] In fact, at least one recent survey suggests that only a minority of employers actually search job candidates online.[4] Thus the actual extent of online searches for job applicants among U.S. employers is unclear.

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[1] Various surveys of employers conducted in recent years have found self-reported evidence that online searches are part of the hiring process. See "Employers Using Social Networks for Screening Applicants," accessed June 24, 2014, http://wikibin.org/articles/employers-using-social-networks-for-screening-applicants.html; "Ponemon Institute HR Special Analysis, May 2007," International Association of Privacy Professionals, accessed June 24, 2014, https://www.privacyassociation.org/publications/2007_12_ponemon_institute_littler_mendelson_study; Stuart J. Johnston, "Microsoft Survey: Online 'Reputation' Counts," Internet News, last modified January 27, 2010, accessed June 24, 2014, http://www.internetnews.com/webcontent/article.php/3861241/Microsoft+Survey+Online+Reputation+Counts.htm.

[2] See Theodore Claypoole, "EEOC Regulations Spotlight Social Media," Womble Carlyle, last modified May 24, 2011, accessed June 24, 2014, http://www.wcsr.com/Insights/Alerts/2011/May/EEOC-Regulations-Spotlight-Social-Media. On the potential risks of using social media in hiring for firms, see Harpe (2009).

[3] For instance, Texas S.B. 118 aims to prohibit an employer from "requiring or requesting access to the personal accounts of employees and job applicants through electronic communication devices; establishing an unlawful employment practice." See S.B. 118, 83rd Leg., Reg. Sess. (Tex. 2013).

[4] See "Threading the Needle: Employment Background Screening in an Age of Increased Litigation & Legislation," EmployeeScreenIQ, accessed November 10, 2013, http://www.employeescreen.com/threading_the_needle.pdf, which provides lower estimates of employers' search probability than the surveys cited in Footnote 1.

In addition, according to the employers, only job-relevant information is being sought online: firms admit to searching blogs or online profiles for evidence of professional or unprofessional traits and behaviors.[5] However, much more private information can be gleaned, directly or indirectly, from the online presences of many prospective hires. A status update can reveal a candidate's place of worship. A blog post can suggest sexual orientation. A comment on a social media profile can show family status. Whether U.S. employers react to such personal traits, rather than merely to the professional information they may look for online, is also not known.

We present a randomized field experiment that tests the joint hypothesis that firms search online for information about job applicants, and change their hiring activities based on the information they find. Leveraging and advancing a methodology common in résumé studies, we use social media profiles of fictional job candidates to manipulate information – namely, religious affiliation or sexual orientation – that may be hard to discern from résumés and interviews, and that may be protected either under federal or some state laws (henceforth referred to as "protected information"),[6] but which may be obtainable online. We design résumés and online profiles for four prospective job candidates – a Muslim versus a Christian candidate, and a gay versus a straight candidate – using material revealed online by actual members of popular social networking sites and job seeking sites. Across conditions, we manipulate the candidates' personal information exclusively via their online profiles, rather than their résumés, to highlight the desired religious affiliation or sexual orientation. Candidates' professional background and résumés are kept constant across conditions. We complement the field experiment with an online experiment, whose goals include testing the quality of our manipulated social media profiles, estimating the effects of our manipulations on hypothetical hiring behavior, and guiding the formation of hypotheses to test in the field experiment.

The online experiment contains roughly 750 subjects (both with and without previous hiring experience). We randomly assign the manipulated online information to online survey subjects and ask them to evaluate the candidate. The main dependent variables are hypothetical willingness to call the candidate for an interview and perceptions of the candidate's suitability for the job. The results from the online experiment show significant heterogeneity in responses to our

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[5] See Footnote 1.

[6] Different types of personal information enjoy different levels of protection across U.S. states. Some personal traits cannot even be inquired about in interviews, while others cannot be used in the hiring decision. Some are protected across all states, and others only in some states. For simplicity, we refer to information about these traits collectively as "protected" information, but investigate state-level differences in the degree of their protection through our empirical analysis.

manipulations. Among other things, we find no bias against the gay candidate relative to the straight candidate; we find bias against the Muslim candidate relative to the Christian candidate; and we find greater bias among subjects with self-reported Republican Party identification.

The randomized field experiment, by contrast, consists of applications submitted to job openings of over 4,000 U.S. employers, with a single application sent to each employer. We submit résumés and cover letters on behalf of the candidates and capture employers' responses. The résumés and letters contain no references or links to the candidates' manipulated personal information. To be treated by our manipulation of online profiles, the employer must independently choose to search online for information about the candidate using the name indicated on the submitted résumé.

The main dependent variable in the field experiment is the number of interview invitations each candidate receives (i.e., callbacks). We compare the callback rate for the Christian candidate to that for the Muslim candidate, and the callback rate for the straight candidate to that for the gay candidate. We control for independent variables used in relevant résumé studies (Bertrand and Mullainathan 2004; Tilcsik 2011), including firm characteristics, job characteristics, and geographical characteristics of the job's location. We also track a number of ancillary variables that provide us with an estimate of the frequency with which employers search candidates online.

We find, first, that roughly one-tenth to one-third of the employers in the field experiment searched online for the candidates' information. Second, we find that the results of the field experiment regarding callbacks are similar to those of the online experiment. We detect no evidence of lower callback rates for the gay candidate compared to the straight candidate. We find 13% fewer callback rates for the Muslim candidate compared to the Christian candidate, although across the entire sample the difference is not statistically significant. As prior work and as our own online experiment predict, however, we find evidence that discrimination varies according to other variables, particularly political party support. Following the literature, we measure political party support in the states and counties where the firms are located. We find evidence of discrimination against the Muslim candidate compared to the Christian candidate among employers in states and counties with a higher proportion of Republican voters. In the ten states with the highest percentage of votes for the Republican candidate in the 2012 U.S. Presidential election, we find a significant and robust difference in callback rates for the Muslim and Christian candidates. The callback rates for the Muslim and Christian candidates are, respectively, 2.3% and 17.3%. The bias in politically mixed and Democratic states is significantly smaller than in Republican states.

The results are robust with respect to the inclusion of state fixed effects in county-level data, firm characteristics (including firm size and ownership status), and additional state or county controls (including median income, unemployment rates, percent college-educated, urbanicity, percent non-white, percent Evangelical Protestant, percent Muslim, percent foreign-born, state-level policies to protect against discrimination on the basis of religion and sexual orientation, and an index capturing degrees of social media penetration), as well as to different categorizations of Republican, mixed, and Democratic states or counties based on electoral results and Gallup Organization surveys. We analyze potential mechanisms for the findings in Section 6.4.

The fact that only a minority of U.S. employers in our sample searched online for the candidates may explain the moderate magnitude of the effects we found in the whole sample. However, the findings also hint at a potentially strong effect of the manipulated information, conditional on the employer searching the candidate online. Considered together, the results suggest that hiring discrimination via Internet searches may not be widespread (for the types of jobs and companies included in the study). And yet, social media and online tools may have a significant effect on the hiring decisions of a self-selected set of employers who search candidates online.

The findings provide field evidence for, and quantification of, employers' usage of a new channel for gathering information about candidates. In addition, they illustrate the possibility of discrimination based on protected information via online channels, highlighting an emerging tension between state or federal regulation and modern information technology. The former aims to preclude certain information from being used in the hiring process; the latter can effectively bypass legislation by allowing individuals to make their information openly available to others online. This paper also introduces a novel methodology - the manipulation of Internet profiles - for field experiments on discrimination. One advantage of this method, which we exploit in this paper, is its suitability for investigating discrimination associated with traits that may be protected under a country's laws or that may be difficult to realistically manipulate in résumé-only or audit studies (as most job candidates may refrain from revealing certain types of personal information on résumés). Such traits include not only the ones we tested in this experiment, but a host of others, including marital and family status, the size and composition of one's social networks, social skills, or, in countries where photographs are not included on résumés, physical attributes such as beauty or height. In fact, our methodology (creating online presences to test dynamics in the offline world) may be used not only in studies of discrimination in the job market, but also in comparable studies of bias in access to credit, housing, or educational opportunities. Finally, our findings relate to the growing empirical (Miller and Tucker 2009; Goldfarb and Tucker 2011) and theoretical (Taylor 2004) literature on the economics of privacy (Stigler 1980; Posner 1981), by illustrating potential trade-offs arising from the public disclosure of personal information.

Economists have long been interested in the role of information (Stigler 1962) and signaling (Spence 1973) in job market matching. Recent economic literature has highlighted how hiring mechanisms that *reduce* candidates' information available to employers (for example, by making the candidate anonymous) may actually increase interview opportunities for certain categories of applicants (Goldin and Rouse 2000; Aslund and Skans 2012), or may raise social welfare (Taylor and Yildirim 2011). The rise of Internet and social media platforms, which transform content users into content creators, conversely makes more candidate information available to employers – potentially *too much* information.[7] The extent to which this new information channel will improve labor search efficiency (Kroft and Pope 2014) and reduce labor market frictions by allowing better matching of employers and employees, or will in fact lead to more discrimination, is likely to be an issue of increasing public policy relevance.

2. Background

2.1 Public Disclosures, Social Media, and Social Networking Sites

The rise of social media has fueled, and has been fueled by, an arguably unprecedented amount of public sharing of personal information. The shared information frequently includes disclosures and revelations of a personal, and sometimes surprisingly candid, nature. In certain cases, personal information is revealed through fully identified profiles.[8] Other times, users provide personal information under pseudonyms that may still be identified.[9] Some social media users take

[7] Even in competitive markets, firms may overinvest and collect an "excessive" amount of information in equilibrium, because of a contrast between social incentives and firms' data-gathering incentives (see Burke, Taylor, and Wagman 2012). In a labor market search model, Wagman (2014) finds that firms may search for "bad news" about applicants and end up collecting too much information, resulting in applicants inefficiently matched with firms of lower productivity. Seabright and Sen (2013) examine how reductions in the cost of applying for jobs may increase the number of applications to a degree that adversely affects firms.
[8] For instance, an overwhelming majority of Facebook users in a sample of North-American college students surveyed by Acquisti, Gross, and Stutzman (2011) used real first and last names on their profiles.
[9] Numerous examples exist of techniques through which seemingly pseudonymous online profiles can be re-identified across a variety of platforms and scenarios. See, for instance, Narayanan and Shmatikov (2009).

advantage of privacy settings to manage and restrict their online audiences.[10] Others think they do, but actually fail to protect their information.[11]

Employers can access shared personal information in many ways. Some job candidates make their online profiles (on Facebook, Twitter, or blogging platforms) openly accessible to strangers.[12] Others are more selective, but sensitive information such as religious affiliation, sexual orientation, or family status may still be indirectly inferable from seemingly more mundane data.[13] Finally, some employers engage in social engineering (such as using friends of friends connections to view a candidate's profile), or even ask for the candidates' passwords to access their profiles.[14] Although the legal consensus seems to be that seeking information online about job candidates (or employees) may not violate U.S. law, such searches do raise privacy and legal issues, such as the discriminatory behavior that may result from the searches (Sanders 2011; Sprague 2011; Sprague 2012).

Various surveys[15] suggest that U.S. employers have been using social media to screen prospective job candidates. However, no prior controlled experiment has measured the frequency of firms' usage of online profiles in hiring decisions, and how profile information actually affects those decisions. The closer study to our experiment (Manant, Pajak, and Soulié 2014) is investigating the impact of different search costs for online candidates' information in the French labor market. Bohnert and Ross (2010) used a survey-based experiment to investigate how the content of social networking profiles can influence evaluations of job candidates. Garg and Telang (2012) analyzed how job applicants can use LinkedIn to find connections that may lead to a job offer. Kluemper, Rosen, and Mossholder (2012) assessed the relationship between the content of a person's Facebook profile and her future job performance.

[10] See Stutzman, Gross, and Acquisti (2012).
[11] Consider the gap between stated and actual privacy settings of online social network users reported by Acquisti and Gross (2006). Similar results have been subsequently found by Madejski, Johnson, and Bellovin (2012).
[12] For instance, using data collected for this study (see Section 4.2), we estimate that, in 2011, 42% of all profiles members of the Facebook network of a major North-American college shared "likes" publicly (where a like could be an interest, book, movie, music, or a TV program). Data reported in Acquisti, Gross, and Stutzman (2011) also shows that a majority of Facebook members in a North American city used facial images in their primary profile photos (which are public by default). Facebook data analyzed by Johnson, Egelman, and Bellovin (2012) indicates that around 54% of Facebook members made available to strangers at least some of their profile information.
[13] For instance, Jernigan and Mistree (2009) show that a Facebook member's sexual orientation may be inferable from knowledge of her friends' network.
[14] Cases in which employers asked job candidates to provide passwords to their online profiles have been reported in the media. See Joanna Stern, "Demanding Facebook Passwords May Break Law, Say Senators," ABC News, last modified March 26, 2012, accessed June 26, 2014, http://abcnews.go.com/Technology/facebook-passwords-employers-schools-demand-access-facebook-senators/story?id=16005565#.T7-fVMWQMQo.
[15] See Footnote 1.

2.2 Discrimination and Résumé Studies

Until recently, methodological hurdles limited randomized field experiments investigating real labor market discrimination. One method, the traditional audit study, involves having actors apply to positions. These studies have produced evidence of discrimination against minorities and women, but suffer from various limitations stemming from the use of actors (Altonji and Blank 1999).[16] A more recent development entails sending fictitious résumés and applications to real employers, where the applications are manipulated and randomly assigned (Bertrand and Mullainathan 2004). The advantage of this method over prior audit studies is that it avoids the pitfalls associated with the use of actors. Following Bertrand and Mullainathan (2004), experiments using written applications to real employers have found evidence of discrimination against people with various traits. Of particular relevance to our study, prior literature has found discrimination against job candidates with Muslim rather than Swedish names in Sweden (Carlsson and Rooth 2007), against job candidates who openly signal Muslim beliefs (relative to other religious beliefs) on their résumés in New England (Wright et al. 2013), against job candidates who explicitly or implicitly signal same-sex sexual orientation (Weichselbaumer 2003; Ahmed and Hammarstedt 2009; Tilcsik 2011),[17] but no evidence of discrimination against Muslims (and little systematic discrimination by caste in new economy sectors) in India (Banerjee et al. 2009).[18]

Our experiment follows Bertrand and Mullainathan (2004), but is designed for a world in which employers can use online resources to find information that job applicants did not explicitly or implicitly make available in their résumés. Hence, one difference between existing résumé studies and our approach is that we focus on employers independently choosing to search online for the candidates' information. A second difference is that we focus on candidates revealing information in public (and yet personal) online social profiles, rather than volunteering personal traits in a professional context. This approach allows the investigation of discrimination based on protected information that candidates do not frequently provide in their résumés or during interviews.

[16] See Bertrand and Mullainathan (2004) for a review of these arguments.
[17] Other studies of sexual orientation's impact on hiring discrimination include Drydakis (2009) and Hebl et al. (2002).
[18] In addition, résumé studies have investigated discrimination against women and indigenous people in Peru (Galarza and Yamada 2012) and discrimination based on physical attractiveness (Ruffle and Shtudiner 2014; Bóo, Rossi, and Urzúa 2013). Furthermore, Ahmed and Hammarstedt (2008) used Internet ads to find vacant rental apartments in Sweden in a study of discrimination in the rental market. Their experimental manipulation relied on names to signal ethnicity and gender.

3. Specification

The economic literature has investigated employers' job market search strategies and incentives, highlighting employers' use of formal or informal information networks (Rees 1966), and their reliance on intensive or extensive searches (Barron, Bishop, and Dunkelberg 1985). Online tools offer employers new channels for finding candidates' professional information, but also their personal information, creating opportunities for discrimination. As with other biases, discrimination based on religious affiliation and sexual orientation may be driven by animus or beliefs about productivity. The literature has discussed sophisticated methods for empirically separating these mechanisms (Mobius and Rosenblat 2006; Neumark 2012; Ewens, Tomlin, and Wang 2014). The focus of the experiment presented in this paper differs from those studies. We test for the existence of hiring discrimination that stems from two steps: first, each employer decides whether to search online for a candidate's information; and, second, each employer who finds the candidate's profile is subject to the experimental treatment. Thus, we test a joint hypothesis that employers use online tools and discriminate on the basis of their findings. More specifically, using a between-subjects design, we test for an effect of random assignment to treatment conditions on callbacks:

$$Callback_i = ß_0 + ß_1 * A_i + \boldsymbol{x_i}'\boldsymbol{\gamma} + \boldsymbol{z_i}'\boldsymbol{\delta} + \varepsilon_i \qquad (1)$$

where A_i is an indicator of random assignment of employer i to either the Muslim condition ($A_i = 1$) compared to the Christian condition ($A_i = 0$) or to the gay condition ($A_i = 1$) compared to the straight condition ($A_i = 0$), $ß_0$ and $ß_1$ are unknown parameters, $\boldsymbol{x_i}$ and $\boldsymbol{z_i}$ are vectors of, respectively, observed and unobserved regressors capturing employer i's traits, $\boldsymbol{\gamma}$ and $\boldsymbol{\delta}$ are vectors of unknown parameters, and ε_i is an error term. $Callback_i$ equals one if employer i contacts the candidate for an interview and zero if there is no response or an explicit rejection. Assuming successful randomization of A_i, the estimate of $ß_1$ will be an unbiased estimate of the effect of manipulated online profiles on callbacks. It is analogous to an intent-to-treat effect, but we refer to it as an assignment effect to emphasize that the main goal of our design is testing the joint effect of i) choosing to search (and thus unknowingly self-selecting into our experiment's treatment) and ii) being treated, whereas researchers in the intent-to-treat literature are primarily interested in isolating the treatment effects.

We do not expect search rates to differ across our condition assignments, because employers are treated by our manipulated profiles after they choose to search.[19] However, it is likely that both search and discrimination probabilities may vary with employer characteristics,[20] and that employer characteristics may affect the probability of searching differently than the probability of discriminating. For instance, certain types of firms – say, firms in IT – may be more likely to search but less likely to discriminate based on the traits we manipulate. We test for heterogeneous assignment effects in our regression analysis by including interactions between the random assignment and regressors that prior literature predicts may interact significantly with our manipulations. When we find a difference in assignment effects between sub-samples of employers with different background characteristics, we expect it to reflect the differences between these sub-samples of employers in the joint effect of search rates and reactions to the randomized manipulations.

3.1 Dependent and Independent Variables

Our experimental design manipulates personal information that is unlikely to be volunteered by candidates in résumés and that may be risky under federal or state law for employers to inquire about during interviews, but which may be obtained online. Building on résumé studies by Weichselbaumer (2003), Carlsson and Rooth (2007), and Tilcsik (2011), we focus on sexual orientation and religious affiliation.

Our main dependent variable is equivalent across the online and the field experiments. The online experiment captures subjects' self-reported hypothetical willingness to call the candidate for an interview and their perceptions of the candidates' employability. The field experiment captures real interview invitations that the candidate receives from actual employers (either by email, phone call, or letter).

[19] As further discussed in Section 4, submitted résumés were held constant across candidates except for the names, which were designed (and successfully tested in two different studies, reported in Sections 4 and 5) so as not to elicit differential perceptions of employability or of other traits manipulated in the experiment. Thus, by design, we would expect (and tested for, to the extent that our field data allowed) similar search probabilities across the candidates, independent of the experimental condition they represented.
[20] For instance, screening and hiring procedures differ significantly across firms, with some firms doing everything in-house and others outsourcing screening to mathematical algorithms or external firms (include emerging services specializing in online screenings, such as HireRight.com). Hiring procedures used will influence a given employer's experimental treatment probability. We are agnostic regarding the specific hiring procedures used within a given firm. However, we assume this heterogeneity in screening procedures to be similarly distributed across experimental conditions.

In both experiments, we base our choice of independent variables on prior résumé studies. In addition, we use prior literature and results from the online experiment to form hypotheses for the analysis of the results of the field experiments.

In the online experiment, our analysis uses four sets of variables. The first set includes variables that may capture socioeconomic status, such as age, sex, race, income, and education (at least one prior study suggests that people with higher socioeconomic status may discriminate more; see Milkman, Akinola, and Chugh 2014). The second set consists of a dummy variable that indicates whether or not a subject has hiring experience. Such a variable is important for comparing online and field results, because whereas our online subjects come from a sample that includes diverse demographics (with and without hiring experience), the field experiment strictly samples from a population with hiring experience. The third set of variables, which includes religion and being U.S. born, is intended to capture social group loyalty, which may include animus toward or biased beliefs about people who are different from oneself (Brewer and Miller 1996, Akerlof and Kranton 2000). A final, related variable captures political party identification. Prior literature has investigated the role of political ideology in discrimination (Gaertner 1973; Wetherell, Brandt, and Reyna 2013). Ample evidence from social survey data suggests that attitudes to Muslims are more negative among Republicans than Independents or Democrats (Cox et al. 2011; Ogan et al. 2014). Gift and Gift (2014) find that political leaning influences hiring (in a résumé study, employers called back Democratic applicants at higher rates in a highly liberal county and Republican applicants at higher rates in a highly conservative county). Similarly, an influential résumé study on discrimination against gay men includes controls for political party support (Tilcsik 2011).

In the field experiment, we control for variables used in comparable résumé studies, particularly Bertrand and Mullainathan (2004) and Tilcsik (2011), including employer characteristics (such as industry or number of employees), job characteristics (such as job-specific requirements), and regional characteristics based on location of the job (such as the variation in state and local policies to protect gay people). We cannot directly control for certain traits that we captured in the online experiment (for instance, the political identification of the individual making the decision whether to call the candidate for an interview). However, following Tilcsik (2011), who finds that treatment effects were significantly more favorable to the gay candidate in New York, California, and Pennsylvania compared to Texas, we capture regional variation and test for interactions between geographical dummies and our assignment dummy. In addition, we include controls that are specific to our experimental design's online dimension, such as a measure of Facebook penetration across states (a proxy for the likelihood that an individual in a particular state may be inclined to search online for the candidate's social media profile). Finally, we use the results of the online experiment to guide our specifications in the field experiment: in addition to the aforementioned control variables, in the analysis of the field experiment we include interactions that were significant in the online experiment, and test the robustness of those interactions with respect to several model specifications.

3.2 Interpreting a Null Result

Given that our design tests jointly for online searching and for discrimination on the basis of manipulated online profiles, a null effect in the field experiment may signal low levels of either of these behaviors. The experiment could fail to reject the null hypothesis in a number of additional ways as well.

First, employers may search the candidates online but fail to find our profiles. Our design addresses this concern by using name-selection criteria that produce high-ranked search results, and by checking, for the duration of the experiment and across a variety of platforms, that candidate name searches produce the desired results (see Section 4). Another possibility is that the manipulations may fail to signal the traits we intended to manipulate. To address this possibility, prior to conducting the field experiment, we used the online experiment to test whether we successfully manipulated beliefs about the candidates' personal traits. A further concern is that the candidates may be either under- or over-qualified, creating floor or ceiling effects in call-back rates. We addressed this possibility by designing the résumés and professional backgrounds of the candidates in concert with human resources (HR) professionals, and testing their quality during the online experiment. An additional possibility is that employers may not pursue the candidates because of suspicions that they are fictional. Consequently, we designed résumés and profiles using information that real individuals (including members of job search sites and members of the same social network the candidates were purported to belong to) had also made publicly available on their profiles or on job search sites. Furthermore, we chose privacy and visibility settings that were common among the profiles of real members. The online experiment tested the success of our efforts at creating realistic profiles by asking questions aimed at discerning whether online subjects had doubts about the veracity of the candidates (see Section 5). Finally, a null effect may be consistent with scenarios in which employers may search, but at a later stage in the hiring process (for instance, after candidates interviews). Our design would not capture this behavior, and would not be able to distinguish this scenario from one with little or no discrimination.

3.3 Limitations

A number of limitations are inherent to the experiment's design.

First, as noted above, a null effect should not be interpreted as an absence of discrimination, because a number of potential explanations may exist - only some of which we can exclude with our ancillary analysis.

Second, as noted above, if we find heterogeneity in the assignment effect according to firm characteristics, we cannot identify whether it is due to differences in search rates, differences in the treatment effect, or both.

Third, the results stem from candidates with certain characteristics (young, with graduate degrees, and with technical backgrounds), applying to certain jobs (a combination of technical, managerial, and analyst positions), with certain employers (private sectors employers spread across the United States whose job openings could be found through online job sites). The results may not necessarily apply to other types of candidates, positions, or organizations.

Fourth, we measure employers' traits such as political orientation based on state or county level data. Hence, we do not capture the political party identification of the actual employers or human resource decision makers: we merely measure the political party support in the states and counties where the firms are located. Additionally, given that local political party support is not randomly assigned in the experiments, we cannot identify causal links between political party support and discrimination.

Finally, a potential concern with the experimental design is that discrimination on the basis of information posted online may stem not only from effects of the communicated trait (i.e., beliefs about productivity and animus), but also from the signal that presenting such information online sends about a candidate's judgment or character outside of that inherent in the trait itself. For instance, someone who chooses to post information about her religion online may be viewed as strongly religious, which may have positive or negative effects on her employability. However, the disclosures we investigate in this paper take place in personal profiles that employers must actively seek, rather than in résumés in which candidates intentionally reveal potentially sensitive information to employers. Such an online social presence may be seen as either positive or negative, depending on the employer. More importantly, we created balanced and realistic profiles. By this, we mean two things. First, each profile included a balanced combination of condition-specific information (that is, traits associated with the experimental condition) and "generic" information (that is, traits not specifically related to a given experimental condition); both types of information were extracted from actual profiles of social network users who are demographically similar to the candidates, using an approach discussed in detail in Section 4.2. Second, we disclosed the same amount of condition-specific information about each candidate and the same amount of generic information about each candidate; therefore, the Christian and Muslim candidates' profiles (and the gay and straight candidates' profiles) present equivalent information about the strength of their religion or sexual orientation (see, again, Section 4.2). Furthermore, we mimicked the amount of self-disclosure that demographically similar real users of the same social network post online. We did this by selecting the number of fields to complete and make available online, and how much information to provide in those fields, based on an analysis of actual social networks users' profiles. In addition to making our profiles more natural, this procedure minimized the chances that employers may interpret the profiles as "over-sharing" by candidates.

4. Design

In both the online and the field experiments, we implemented a between-subjects design in which we assigned each subject (an online participant, or an actual employer) to one of four treatment conditions, composed of résumés and social media profiles for a single job candidate. We manipulated religious affiliation (a Christian versus a Muslim male) and sexual orientation (a gay versus a straight male).[21] Thus, the experimental conditions represent a range of traits that include federally protected information (religious affiliation)[22] and information protected only in certain states (sexual orientation).[23] While these traits enjoy different levels of protection under U.S. law, job candidates can signal this information online both in an explicit manner (e.g., self-descriptions

[21] Both the Christian and Muslim profiles are presented as straight. Neither the gay or straight profile provide information about the candidate's religious affiliation.
[22] Under Title VII of the Civil Rights Act of 1964, companies with 15 or more employees may not inquire about a candidate's religious beliefs.
[23] In several, but not all, U.S. states, employers cannot ask job candidates questions regarding their sexual orientations. See "Laws Enforced by EEOC," U.S. Equal Employment Opportunity Commission., accessed June 26, 2014, http://www.eeoc.gov/laws/statutes/.

on a profile indicating one's sexual orientation) and an implicit one (e.g., a primary profile photo suggesting the individual's religious affiliation).

For each of the four candidates, we designed: i) a résumé; ii) a profile on LinkedIn (a popular online professional network commonly used by human resource professionals and job seekers, henceforth referred to as "PN," as in "Professional Network"); and iii) a profile on Facebook (a popular social networking site commonly used for socializing and communicating, henceforth referred to as "SN," as in "Social Network").[24] We designed candidates' résumés and PN profiles to show identical information across conditions, except for the candidates' names. In contrast, the candidates' SN profiles manipulated information about the candidates' personal traits using various profile fields, including those that some employers admit to using during the hiring process in semi-structured interviews (Llama et al. 2012; see Section 4.2). Each applicant's name corresponds to an experimental condition; the names link together a candidate's résumé, PN profile, and SN profile.[25] Because a single name links all of these materials, we could submit just one application to each employer in our sample.[26] Submitting multiple candidates' applications to the same employer would have increased the risk of an employer detecting social media profiles which have identical photos and generic information, yet come from candidates with different names and condition-specific information.

4.1 Design Approach and Priorities

We designed profiles that i) were realistic and representative of the population of SN users with the traits we were manipulating, and ii) held constant direct signals of productivity outside of beliefs stemming from the trait itself. As described in the rest of this section, we populated the résumés and online profiles using existing information posted online by actual SN members demographically similar to the candidates, and by individuals who listed their résumés on job searching sites. The SN profiles are the vehicles for the manipulations, and responses to these profiles comprise our study's core. The remainder of this section discusses our design in more detail.

[24] In addition, and for completeness, we created candidates' accounts on Google+ and Google Sites. These profiles were identical across conditions and did not include professional and candidate-specific information.
[25] We took down the various online materials we had created only after the experiment's completion, which included candidates' online profiles and the sites of companies that had been created as part of the design.
[26] Many traditional résumé studies use multiple applications to each employer, but for an exception see Ahmed, Andersson, and Hammarstedt (2013), who sent one application per employer.

4.2 Candidates' Information

Names

We designed first and last names representing a U.S. male for each of the conditions used in the experiments. We chose first names common among U.S. males in the same age group as our candidates, and assigned identical first names to candidates in matching pairwise conditions (that is, the gay and straight candidates had the same first name, and the Muslim and Christian candidates had the same first name). We then designed last names by altering letters of existing but similarly low-frequency U.S. last names. The last names were chosen to have the same number of syllables, identical lexical stress, and similar sounds across matching pairwise conditions.[27]

We iteratively tested several combinations of first and last names until we found a subset that satisfied three criteria. The first criterion was that the exact first and last name combination would be unique on SN and PN: no other profile with that name should exist on the network. The second criterion was that SN and PN profiles designed under each name would appear among those names' top search results conducted with the most popular search engines (Google, Bing, and Yahoo), as well as searches conducted from within the SN and the PN social networks. We continuously monitored the fulfillment of these criteria for the experiment's duration.[28] The third and most critical criterion was that the names alone should not elicit statistically significant differences in perceptions of the manipulated traits; in a way, our design relied on names that achieved a goal opposite to the one achieved in Bertrand and Mullainathan (2004). To test our ability to meet this criterion, we ran two tests: one test ("names only") focused only on names, and tested that names did not elicit different perceptions of the traits manipulated in the profiles (its design and results are presented in the rest of this sub-section). The other test ("No SN links") combined names, résumés, and professional online profiles, and tested that names and professional information did not elicit differential propensity to invite a candidate to an interview (its design and results are described in Section 5.3).

[27] Further details about this process are presented in the Supplementary materials A.

[28] The experiment was not started until search engines had indexed the social media profiles. After the experiment began, and on a weekly basis throughout the experiment's duration, we monitored the search results for the candidates' names. We conducted searches through proxy servers so that they appeared as coming from foreign IP addresses, which enabled us to avoid contaminating the actual employer search data that we hoped to gather from services such as Google Keyword Tool trends (see Section 6.2).

We conducted a between-subjects "names only" experiment to test perceptions of the names. We recruited subjects from Amazon Mechanical Turk (or AMT: a popular online platform that facilitates matching between researchers and potential subjects).[29] Each subject was presented with one first and last name combination, randomly chosen from a list of possible names that satisfied the other two criteria, and was asked to fill out a questionnaire about what traits that name elicited. We focused on the traits we actually manipulated in the online and field experiments (religious affiliation and sexual orientation). The four names we selected for the actual experiments were those that did not elicit statistically significant differences in perceptions of traits in a sample of 496 subjects (also satisfying the additional criteria discussed above).[30] Within pairs of equally constructed names, each name was then randomly assigned to a candidate.

As noted, by design candidates had common U.S. first names and modifications of U.S. last names. Text and photos on their social media profiles showed them to be Caucasian, U.S. born, and English speaking.[31] Thus, in the religious manipulation conditions, our candidates were presented as a Caucasian U.S. born Christian male, and as a Caucasian U.S. born Muslim male. One advantage of this design is that it isolates religion from ethnicity. Another advantage is that, if an effect of the treatment were to be detected, it may be causally linked to employers seeking information online and discovering the candidate's religion from his social media presence, rather than their inferring his ethnicity from his name on the résumés. A potential disadvantage consists in the relative rarity of Caucasian Muslims in the United States. However, roughly 35% of the about 2.35 million American Muslims are, like our candidate, US-born; many have Anglo-Saxon names, which they have not changed to an Arabic version.[32]

Email addresses and telephone numbers

For inclusion in the résumé and cover letters sent to companies, we designed email addresses using a consistent format across the profiles and registered a telephone number for the applicants. We used automated message recordings for the number's voice messaging service. This message was identical across conditions and recorded with the voice messaging service's standard greeting.

[29] See Section 5 for more details about AMT.
[30] See Supplementary materials Table S1.
[31] For realism, our Muslim candidate's social network profile presented him as speaking both English and Arabic.
[32] See "Muslim Americans: Middle Class and Mostly Mainstream," Pew Research Center, last modified November 10, 2013, accessed June 30, 2014, http://pewresearch.org/pubs/483/muslim-americans.

We also used the same postal address for all candidates, corresponding to a residential area of a mid-size North-American city.

Résumés

The résumés contained information depicting a professional and currently employed candidate. Each résumé contained the candidate's contact information, educational background, work experience, as well as technical skills, certifications, and activities. The résumés were held constant across conditions, except for the names of the applicants and their email addresses.[33] Hence, professional and educational backgrounds did not vary across experimental conditions, thus holding constant the candidates' job market competitiveness. The information included in the résumé was modeled after résumés found on websites such as Monster.com and Career.com for job seekers demographically similar (in terms of age and educational background) to the candidates. The résumé represented a candidate with a bachelor degree in computer science and a Master's degree in Information Systems.[34] Creating a single SN and PN profile per candidate constrained the types of jobs to which we could apply. Hence, all résumés needed to be consistent with the constant information provided in the candidates' social media profiles (for instance, all candidates' profiles exhibited the same Master's degree in Information Systems). Therefore, the openings (and respective résumés) tended to be of technical, managerial, or analytic nature. Two human resource recruiters vetted the résumés for realism, professionalism, and competitiveness, before they were tested in the online experiment. A design objective (also tested during the online experiment) was to create a sufficiently competitive candidate for stimulating the level of interest necessary to generate an online search, but not so competitive as to outweigh any potential effect arising from an employer's perusal of the candidate's online profile.

The résumés did not include links to the candidates' personal profiles or references to the personal traits we manipulated on the SN. The experimental design relied entirely on the possibility

[33] Research assistants blind to the experimental conditions were allowed to add up to two technical skills (for instance, Java) or certifications (for instance, CISSP certification) to a résumé if the job description required those skills or certifications as pre-conditions. This fine tuning always occurred *before* a candidate's name was randomly added to the résumé, and therefore before the candidate was assigned to a job application.

[34] We prepared 10 versions of the same résumés, focusing on slightly different sets of expertise: web development, software development, quality assurance, project or product management, medical/healthcare information, information systems, information security, business intelligence, business development, and analytics. A sample résumé is presented in Supplementary materials B.

that employers would autonomously decide to seek information about applicants online, either by searching for their names on popular search engines or directly on popular social networks.

PN ("Professional Network") profiles

We designed PN profiles for each name, maintaining identical profile information across conditions. The content of the profiles reflected the information provided in the résumés.[35] To increase realism, we also designed additional PN profiles for other fictional individuals and connected them to the candidates' profiles, so that they would become "contacts" for our candidates. We took great care to avoid any possible linkage between the actual candidates' profiles.[36] The number of PN connections was identical across all the profiles and chosen to be as close as possible to the median number of connections of a US-based profile on the PN professional networking site at the time of the experiment.

SN ("Social Network") profiles

SN profiles served as the principal manipulation vehicle in the field experiment. We paired each candidate with a SN profile created under the candidate's name and populated the profile with information extracted from an analysis of the public disclosures mined from real profiles on the same social network.

First, we downloaded the public profiles of 15,065 members of the same Facebook college network from which the candidates purportedly graduated. The vast majority of those profiles belonged to individuals with a similar age range (their 20s), current location (a North-American mid-size city), and educational background as our candidates. As further detailed below, among that set of profiles we then paid specific attention to those with the same gender (male) as our candidates, and who self-disclosed traits identical to the ones manipulated in the experimental conditions (that is: Christian, Muslim, straight, or gay).

We designed several components of each SN profile based on the information we had mined from real online profiles: personal information (such as current location, location of birth, education, and employment history); closed-ended text fields (such as interests, activities, books,

[35] Supplementary materials C presents copies of the information provided in the PN profiles.
[36] We also created websites, email accounts, and PN presence for some of the companies reported in the candidates' résumés, as well as other workers at those companies and potential letter-writers, in order to have a complete, believable background should anyone search. The candidates were also registered as alumni from the institution that granted their degrees. As noted, we took down these materials after the completion of the experiment.

movies, or TV shows); open-ended text fields (such as those providing a summary self-description of the individual and his favorite quotations); friends list; primary profile photo and secondary photos; and background image. Based on literature that emerged during the design phase of the experiments,[37] however, we decided not to make all this information publicly available and therefore visible to an HR professional. To avoid potential confounding effects from over-disclosure, we refrained from showing certain fields that only a minority of users of the network actually publicly show, such as posts, status updates, and friends list.

By design, some of these profile components were constant across the different profiles and thus, across the different experimental conditions. Other components were manipulated across conditions. We discuss each type of component in the following sub-sections.[38]

Information kept constant across SN profiles

By design, we made the candidates' primary profile photo, secondary photos, current location, hometown, age, education, employment history, and friends list constant across conditions. Basic personal information that could be gleaned from the résumés was also constant across the conditions.

To find suitable primary photos, we first recruited non-professional models to submit a photo portfolio using an online classifieds service. Next, we tested the photos we received with a sample of online survey subjects in order to select a model that would meet our needs. About forty subjects (recruited via AMT) were asked to rate all models along 7-point Likert scales, indicating their perceived attractiveness and professionalism. We selected one male model that received median perceived attractiveness and professionalism ratings. We then asked this selected model to submit additional photos for inclusion as primary and secondary photos in the SN profiles. As noted, those photos, and therefore the face of the candidate, were held constant across all profiles and conditions. Basic personal information (such as the SN member's current location and city of birth, educational background, employment history, hometown, and age) was made publicly visible on

[37] Johnson, Egelman, and Bellovin (2012) report that only 14.2% of the users of a popular social network whose profile information they mined had a public wall (that is, visible status updates and comments). That noted, we reiterate that the profiles used in the experiment contained equivalent amounts of information across conditions, so that any difference in callback rates could not be merely imputed to a higher amount of disclosure by one type of candidate over another. Furthermore, we note that several fields (such as name, gender, primary photo, profile photo, and networks) are mandatorily public on the network we used for the experiment.

[38] Supplementary materials C presents copies of the information provided in the SN profiles for the four candidates.

the profile and kept constant across the conditions. That information reflects the content of the résumé.

We designed numerous "friends" profiles to connect to the candidates' profiles. Again, the number and identities of friends were identical across conditions. The set of friends displayed a mix of names, backgrounds, and profile visibility settings. However, the list of friends remained publicly visible only during the online experiment. (In preparation for the field experiment, we set the friends list to "private" – that is, not visible to an employer - because novel research had emerged suggesting that the overwhelming majority of members of the network do not publicly disclose their friends lists.)[39]

Information manipulated across SN profiles

Some fields in the profiles were manipulated across conditions: close-ended text fields (e.g., interests), open-ended text fields (e.g., quotations), and the background image. Furthermore, we manipulated the candidates' sexual orientation by filling out the field "interested in" (either male interested in females, or interested in males). We identified religious affiliation through the "religion" field, either Christian or Muslim, with no specific denomination.

We followed a number of principles in designing the manipulated information. First, we constructed "baseline" profile data, which was constant across all conditions, including information such as specific interests and activities statistically common among the over 15,000 SN profiles we had previously mined ("baseline information"). We then augmented the profiles with additional data (such as additional interests or activities) specific to the traits that we wanted to manipulate ("treatment information"). Both "baseline" and "treatment" information were extracted from real and existing SN profiles. The profiles therefore represented realistic pastiches of *actual* information retrieved, combined, and remixed from existing SN profiles.[40] In all cases, we took care to avoid confounding the trait we were manipulating with signals of worker quality beyond signals inherent in the manipulated trait itself. Furthermore, as noted, the different candidates' profiles were

[39] See Johnson, Egelman, and Bellovin (2012). Although we made the list of friends private, we did not delete the friend profiles because 1) some of those friends' comments appeared on the candidates' profile photos, adding realism to the profiles (again, comments and photos were held identical across conditions); 2) the presence of a network of friends decreased the probability that the candidates profiles could be identified as fake and deactivated by the network (none of the candidates profiles was deactivated during the duration of the experiment).

[40] Technical limitations inherent in designing an experiment via online social networks precluded us the possibility of randomizing the entire content of the candidates' profiles by creating thousands of different profiles for each candidate. On the other hand, our approach gains in ecological realism by consisting of existing profile data.

designed to disclose the same amount of personal information, including the same quantity and types of data revealing their sexual orientation or religious affiliation, so as not to create profiles with unbalanced condition-specific disclosures. For similar reasons, we ensured that the information revealed about the candidates would not be construed as over-sharing, or as forced caricatures of what a Muslim (or Christian, or gay, or straight) profile should look like. The combination of baseline and treatment information of true existing profiles was one of the means we used to meet this goal; using only information existing in other SN profiles, and making certain fields publicly inaccessible, were two others. The online experiment, with its open-ended questions about the profiles, tested whether our goal was reached (Section 5). .

Close-ended text fields such as interests and activities were extracted using a combination of statistical and manual analysis from real SN profiles of people who shared demographic characteristics with the candidate (for the "baseline information") and who exhibited the same characteristics we manipulated (for "treatment information"). For example, when text involved countable objects such as one's favorite book or movie, we calculated the most popular items listed by the population of SN profiles with the candidate's trait and used that as the "baseline information" for the candidate. Then, we repeated the operation, focusing on the subset of the public disclosures of over 15,000 SN profiles that displayed the same traits we wanted to manipulate in order to create the "treatment information." For instance, we based part of the profile text indicating activities and interests for the Christian male profile on a statistical analysis of the entire sample of over 15,000 profiles that included non-Christian ones; the remaining part of the profile text about his activities and interests was the result of the statistical analysis of the sub-sample of profiles of single Christian males at his university. If our sample did not offer enough information (for instance, movies) for individuals with a given trait, we complemented the statistical analysis with qualitative, manual analysis of other SN profiles. We extracted open-ended fields (such as personal self-descriptions, personal quotations, or non-textual fields like profile background images) through manual analysis of existing profiles, since the extreme variety of style and content across open-ended texts made a statistical approach unfeasible.[41]

4.3 Tracking Employers' Searches

[41] The profile background images consist of large landscape pictures, placed at the top of a profile, which many SN members use for self-presentation and representation. The background images we used for the profiles were also based on the background images of existing SN profiles that publicly displayed the traits we manipulated in the experiments.

Ideally, we would track and identify which employers visited the manipulated profiles. However, the personal social network we used in the experiment does not allow this kind of tracking. Thus, with only a handful of exceptions, we were not able to observe *individual* employers' search activity. However, we were able to infer indirect evidence of employers' search in the aggregate using a combination of metrics. We used Google's Keyword Tool trends to capture aggregate estimates of the number of times the candidates' names were searched for on Google from IP addresses within the U.S.; we also tracked visits to additional "professional profiles" that we created on PN.[42] Details and results are presented in Section 6.1.

5. Online experiment

The online survey experiment consisted of a randomly assigned questionnaire with a between-subjects design. The survey was meant to complement the field experiment in various ways. First, the online experiment tested whether the treatment conditions successfully manipulate relevant beliefs, such as the religious affiliation or sexual orientations of the candidates. Second, its open-ended questions tested the perceived realism of the profiles. Third, the online experiment, which was conducted prior to the field experiment, provided guidance for hypotheses to be tested in the field data. Fourth, the online experiment helped us to test whether, in absence of the manipulated personal profiles (PN), the candidates' names, résumés, and professional profiles elicited differential propensities to invite the candidate for an interview. Lastly, the online experiment enabled a comparison between methods with substantially different pros and cons.[43]

We recruited survey subjects using Amazon Mechanical Turk. Although this sample is not necessarily nationally representative, studies of this platform have shown that results from AMT samples can be similar to those obtained from nationally representative samples (Paolacci, Chandler, and Ipeirotis 2010). Furthermore, judgment and decision-making experiments using AMT samples have replicated results found in traditional subject samples (Goodman, Cryder, and

[42] LinkedIn, the professional social network we used in our study, allows members to purchase "premiere" accounts. Such accounts provide some information about other members of the professional network who are visiting their profiles.
[43] An advantage of the field experiment is that it captures hiring behavior by employers who face actual incentives and are unaware that we are observing their behavior. However, it samples a narrow segment of the U.S. labor market. The online experiment is potentially more broadly representative, but the responses are hypothetical, un-incentivized, and given by subjects who know they are under observation.

Cheema 2013). Although our AMT sample is not representative of the sample of employers in the field experiment, a subset reports having hiring experience – a factor we leverage in the analysis.[44]

5.1 Survey Design and Structure

In the online experiment, AMT subjects were randomly assigned to one of four conditions (gay, straight, Christian, or Muslim candidate), and were provided links to one candidate's résumé, PN profile, and SN profile.[45] As a reminder, PN profiles were identical across conditions, whereas SN profiles contained the treatment information.[46]

The survey instrument had four elements: i) introduction and randomized manipulation; ii) measurement of perceived employability; iii) measurement of beliefs for the purpose of manipulation check; and iv) open-ended questions and demographic characteristics.[47]

After passing an attention-check screening question and reading an introduction,[48] the survey asked subjects to peruse a candidate's résumé and online PN and SN profiles, which were linked from the survey. Subjects were then invited to imagine being an HR professional at a firm to which the candidate was applying, and asked whether they would call back the individual for an interview. In subsequent pages, subjects responded to an additional set of questions on the suitability of the candidate for the job to which he was applying. We intended these questions to capture the role of standard productivity concerns in perceived suitability for the job.[49] Following these questions, subjects were asked a series of manipulation-check questions, aimed at establishing whether the subject thought that the candidate he or she viewed was male or female, professional or unprofessional, attractive or unattractive, married or single, straight or gay/lesbian, Caucasian or

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[44] Uhlmann et al. (2013) recently used AMT for a study on how assumptions about the appropriateness of referencing non-work roles while in work settings creates impressions that affect professional outcomes. As with our experiment, the authors controlled for AMT subjects with and without HR and hiring experience.

[45] In the online experiment and in the field experiment we do not compare candidates crosswise vis-à-vis the manipulated trait (for instance, a gay candidate versus a Muslim candidate), both for lack of ex ante theoretical grounds, and because the names of the candidates were designed to be used in pairwise comparisons (see Section 4.2).

[46] In Section 5.3, we describe a set of four additional conditions we ran as controls.

[47] See Appendix A for a complete wording of relevant survey questions.

[48] Attention-check questions have been used as effective means of increasing quality of answers on AMT (Aust et al. 2013; Buhrmester, Kwang, and Gosling 2011; Oppenheimer, Meyvis, and Davidenko 2009).

[49] The questions were:

1. I believe that this candidate would be a great hire for such a position. His or her qualifications are excellent and are well-matched to the position.
2. I believe that the candidate is not sufficiently qualified for the position and/or does not seem to have enough promise for success in the position.
3. I believe that the candidate is over-qualified for the position.
4. I believe that the candidate would not fit into the workplace or would not be a good fit for the position.

The response options for these questions were presented on a Likert scale from 1 (strongly disagree) to 7 (strongly agree).

African-American, Christian or Muslim, and had kids or did not have kids. Next, subjects were asked open-ended comment questions about the candidates, their résumés, and their online profiles. Finally, subjects were asked to provide their demographic information, including their gender, age, education, job, hiring experience, religious affiliation, and political identification. We use answers to these questions as covariates in our regression analysis.

5.2 Results

Collection for the online experiment started in April 2012 and continued for approximately two months, with 776 observations assigned to four experimental conditions (Muslim, Christian, gay, and straight).[50]

Appendix Table A1 presents summary statistics for the variables used in the analysis of the online experiment: 42.23% percent of our subjects are male; 40.40% are over 30 years of age; 48.74% have college degrees; 48.28% have annual household incomes over $45,000; 16.79% are Republicans; and 43.03% are Democrats.[51] Finally, 49.41% of the sample reports some degree of hiring experience (either having hired an employee or worked in HR).

The online experiment captures whether the treatments significantly affect subjects' perceptions of the manipulated traits, whether the subject would call the candidate for an interview (*Selfcall*), and an attitudinal scale of perceived employability of the candidate (*Employability scale*).

First, we tested whether we successfully manipulated the perceptions as intended. The randomization of a trait (Muslim male versus Christian male, gay male versus straight male) had a significant effect at the one-percent level on corresponding beliefs about that trait (see Appendix Table A3).

Second, we tested the effects of the treatments on *Selfcall* and *Employability Scale*. Tables Ia and Ib present, respectively, summary statistics for our main dependent variables (*Selfcall*, *Employability Scale*) for the entire sample and among subjects who have hiring experience. *Selfcall* and *Employability Scale* had non-responses resulting in sample sizes of 770 and 766, respectively.

[50] We recruited US-based AMT workers with high approval rates (over 95% approval). Eighteen percent of recruited subjects failed the attention-check question at the start of the questionnaire and were not allowed to continue the survey. Among those who passed the attention-check, we excluded about 9% from the analysis when we detected a second or higher attempt to take the survey from the same IP address.

[51] Among the demographic variables we tracked, no statistically significant differences existed across pairwise conditions, except non-whites, one income category, and one age category in the Muslim/Christian conditions; and unemployed, one income category, and one age category in the gay/straight conditions. We control for those (and other) variables in the regression analysis reported in the rest of this section.

Table Ia shows that no significant treatment effects exist in the whole sample. Table Ib presents these statistics among the half of the sample with hiring experience. These subjects more closely represented the real employers who, in the field experiment, evaluated the candidates. Among subjects with hiring experience, religion has a significant effect on the scale of perceived employability, with a lower score for the Muslim candidate.[52] There are no significant differences between the gay and the straight candidate in either measure.

Table II presents OLS regressions of perceived employability on the treatment conditions, control variables, and various interactions. On the basis of prior research (see Section 3.2) we predicted heterogeneous responses to the treatment conditions according to certain traits. The controls include hiring experience, age, gender, race, employment status, college degree, income, political party identification, foreign-born, and religious affiliation. The regressions in Table II also include interactions that were significant in separate regressions (presented in Appendix Table A2), where each regression includes the treatment dummy, one other regressor, and the interaction between those two.

Column (1) of Table II presents results for the religion manipulation. This regression includes the Muslim versus Christian treatment dummy, all controls (not reported, but see notes to Table II for a list of included variables), as well as interactions significant in column (1) of Appendix Table A2. Column (1) shows that the interactions with foreign-born and income are significant at the ten-percent level, but the hiring experience interaction is no longer significant after adding additional controls and interactions.

Column (2) presents the results for the sexual orientation manipulation. This regression includes the gay versus straight treatment dummy, all controls (the same ones included in column (1)), as well as interactions significant in column (2) of Appendix Table A2. The interaction between the assignment dummy and the dummy for non-white is positive and significant at the five-percent level. The interactions between the assignment dummy and the age dummies are insignificant.

Column (3) shows the results for the pooled sample. This regression includes a treatment dummy that equals one if the candidate is either Muslim or gay, and zero if the candidate is either Christian or straight, as well as the same controls as the previous columns and interactions

[52] The effect on the *SelfCall* measure is significant at the ten-percent level (Fisher's one-way exact test). As shown by the table, almost all subjects said they would invite the candidate for an interview, resulting in very little variation in this variable.

significant in column (3) of Appendix Table A2. The interactions between the treatment dummy and the dummies for being non-white, independent, and a Democrat are significant at the ten-percent level, with the same signs presented in Appendix Table A2. The interaction between the treatment dummy and the dummy for being non-white is not significant.

The results in Tables I and II suggest some bias against the Muslim candidate compared to the Christian candidate among subjects with hiring experience. In addition, Table II points to heterogeneity of bias based on income, race, employment status, being foreign-born, and political party identification, which we further test in the field experiment.

5.2.1 Perceived Realism of the Profiles and the Candidates

In addition to analyzing hypothetical interview invitations and perceived employability of the candidates, we examined open-ended answers to questions about the candidates' résumés, PN, and SN profiles. A research assistant, blind to the experimental conditions, hand-coded the answers. The answers revealed that subjects across all conditions believed that the candidate was a real person. Only 0.34% of the subjects wrote comments suggesting doubts about the veracity of the profiles. Most subjects, instead, provided comments referring to their searching and finding more information online about the candidate, or even wishing the candidate "best of luck" in his or her job search.

5.3 No SN Links Experiment

In addition to the online experiment, we tested four additional "*No SN Link*" conditions in February 2013, right before starting the field experiment. Unlike the other conditions in the online experiment, subjects in this "*No SN Links*" experiment were not provided links to the SN profiles of the candidate. Our rationale for the *No SN Link* conditions was to check (in addition to the testing of candidates' names already discussed in Section 4.2) that no significant differences would result from the combination of the candidates' names, their résumés, and their PN profiles.[53]

We assigned 982 AMT subjects to four experimental conditions (Muslim, Christian, gay, and straight) but only provided links to the résumés and the PN profiles for the candidates, which

[53] In addition, the *No SN Links* experiment reflected some changes we made to the candidates' information in preparation for the field experiment, which were based on the feedback we gathered during the online experiment and literature about online social networks that had in the meanwhile emerged. As noted above (Section 4.2), the changes consisted of making the SN status updates and friends list invisible, and selecting new, easier-to-search names for the candidates (the successful testing of these names was described in Section 4.2).

are identical across conditions. Because these subjects were not given links to the SN (that is, personal) profiles of the candidates, they would not infer the candidates' sexual orientation or religious affiliation. Neither the *Selfcall* rates nor the *Employability Scale* were significantly different across conditions. This was the case in the overall sample and (unlike in the conditions with links to the SN profiles) in the sample with hiring experience too. Unlike in the conditions in which links to the SN profiles were provided, we did not detect differences in perceptions of the manipulated traits in the *No SN Link* condition.[54] In the absence of SN profile information, therefore, we find no evidence of discrimination against the Muslim or gay candidates based on names alone (Section 4.2), nor do we find evidence that the names, résumés, or the PN profiles alone elicited differential perceptions of the traits we manipulated in the SN profiles (this section).

6. Field Experiment

The field experiment was a between-subjects design in which an employer was randomly assigned to receive one job application from either the Muslim, Christian, gay, or straight candidate. We applied to 4,183 actual job openings in the U.S. and captured employers' responses to the applications. Ten applications failed to meet criteria we had defined in our procedures for acceptable and complete applications, leaving us with 4,173 usable applications. The job application period lasted from February to the end of July of 2013, when we achieved our goal of a sample size of at least 1,000 job applications for each candidate.[55]

The experiment involved extracting and submitting applications to job openings from Indeed.com, an online job search site that aggregates jobs from several other sites.[56] Companies were selected from a pool of firms offering jobs related to the candidates' backgrounds. As mentioned above, we adjusted the original résumés tested during the online experiment to fit 10 different job types. The job types cover a combination of technical, managerial, and analytic positions, since search and discrimination probabilities may differ not only by firms' characteristics, but also by job types (see Fernandez and Friedrich 2011). We focused on private sector firms, and on positions that required either a graduate degree or some years of work experience to fit the candidates' résumé background.

[54] See Supplementary materials Tables S4 and S3.
[55] The target sample size was chosen based on similar studies, such as Betrand and Mullainathan (2004).
[56] Supplementary materials D lists the search terms used to find different type of jobs.

We defined several criteria that jobs and companies had to pass for us to apply to them. Primarily, the job had to be related to the candidates' background and level of experience, although we also included (and controlled for) positions for which the candidates could be considered slightly over- or under-qualified.[57] In addition, we avoided sending two applications to the same company, or to companies that were likely to share HR resources such as databases of applicants (for instance, parent companies of firms to which we already applied).[58] We also excluded staffing companies, companies located in the same geographic region as the candidates' reported current location, and companies with 15 or fewer employees (to limit the costs imposed on them by the process of searching for the candidates online).[59] All applications (résumés and cover letters) were submitted online, either by email or through employer-provided web forms.

We recorded the city and state listed on job postings, and, when possible, we recorded the city and state where the job would be located. When job location was not provided, we used the location of the company's headquarters. We obtained this measure for all observations and employed it for our state level analysis. From the company name, we were able find the street address of the company headquarters for all but a few hundred observations. We used ArcGIS[60] to match this street address to its county. We then merged our data with county level data from the American Community Survey[61] based on the county where the company headquarters is located.

6.1 Results: Employer Search Trends

One of our research objectives was to determine roughly the frequency of employers' online searches of job candidates. The SN we used in the experiment does not allow its members to track or count visitors to their profiles. Hence, we cannot observe how many employers were directly exposed to the experimental manipulation. In theory, *all* employers who received applications as

[57] We had two rationales for including such positions. First, we wanted to investigate whether discriminatory bias may be more likely to surface when a candidate is not a close match for an available position. Second, as noted, our experimental design faced a dilemma: creating résumés sufficiently competitive for likely interview consideration, but not so competitive as to eclipse the content of the SN profiles. Ex ante, it would be hard to gauge this kind of ideal "level" of résumé quality.
[58] As noted above, multiple candidates' applications to the same firm were not tolerable in our design because they would pose the risk that employers might see applications and social media profiles with identical photos and overlapping information from seemingly different candidates. We also took great care to avoid cross-linkage of the candidates by the same employer through other means. For instance, employers could not navigate from one of the candidates to the other using their (shared) friends, and profile images were subtly modified with random-noise (invisible to the human eye) in order to avoid being listed as similar images by Google's reverse image search functions.
[59] Small firms were excluded from the experiment in agreement with our institution's IRB. It is worth noting that these firms may not be constrained by EEOC laws, and may be more likely to discriminate. The exclusion of these firms may thus yield more conservative estimates of discrimination.
[60] See website for ArcGIS; https://www.arcgis.com/features/.
[61] See website for the United States Census Bureau, American Community Survey; https://www.census.gov/acs/www/.

part of the experiment may have searched for the candidates directly on the SN; in practice, this appears quite unlikely. While we cannot precisely capture the number of employers' searches, however, we can obtain noisy estimates. We used a combination of two data sources to learn about the frequency with which employers searched for the candidates online:

1. Google AdWords "Keyword Tool" statistics, which capture the number of times the names of the candidates were searched on Google from U.S. IP addresses;

2. PN "Premiere" account statistics, which provide a count of visits to the PN profiles, and, in a small handful of cases, the actual identity of the visitors.

Although each of these data sources is imperfect, considered together they cast some light on the proportions of employers searching the candidates online. (We present assumptions, calculations, and raw results in Appendix B.) We estimate that the likely proportion of employers who searched for the profiles ranged from 10.33% to 28.82%. As mentioned above, by and large we cannot identify the specific employers visiting the profiles. Thus, these proportions are sample-wide aggregate estimates. Search rates may differ by type of job, industry, or employer location, although, due to randomization, they should not differ by experimental condition. Furthermore, these numbers reflect the proportion of employers who searched in our particular study. They are not estimates of the percentage of employers in the U.S. labor market who regularly search for candidates online. That noted, the estimates appear consistent with the results of a 2013 CareerBuilder survey of 2,291 hiring managers and HR managers, according to which 24% claimed to "occasionally" use social media sites to search for candidates' information; 8% answered "frequently;" and only 5% answered "always."[62]

6.2 Field Results: Employer Callbacks

Our primary dependent variable is a callback dummy that equals one if the candidate was contacted for an interview and zero if the candidate received a rejection, no response, or a scripted

[62] See Michael Erwin, "More Employers Finding Reasons Not to Hire Candidates on Social Media, Finds CareerBuilder Survey," CareerBuilder, last modified June 27, 2013, accessed June 30, 2014, http://www.careerbuilder.com/share/aboutus/pressreleasesdetail.aspx?sd=6%2f26%2f2013&siteid=cbpr&sc_cmp1=cb_pr766_&id=pr766&ed=12%2f31%2f2013.

request for more information. Of the 4,173 observations from both manipulations, 11.20% were callbacks, 15.86% were rejections, 69.29% had no response, and 3.32% were scripted requests for more information.

Timing of Callbacks

Figure I shows the geographical distribution of applications across the United States. Figure II provides a distribution of the timing of responses we received across all candidates, where a response is an interview invitation, a rejection, or a request for more information. This figure does not include the 70% of employers who did not respond to the application. Most responses came within the first 14 days.[63] Roughly 77% of responses arrived within one month from the application, and roughly 90% within two months. The distribution of timing of responses follows the same patterns across the four candidates.[64]

Callback Rates

Table III presents callback rates in the four condition assignments. Results for the religion manipulation are presented in Panel A. The callback rates for the Christian and Muslim candidates are 12.64% and 10.96%, respectively, but this difference is not statistically significant. Callback rates for the sexual orientation manipulation are presented in Panel B. We find no main effect in this manipulation either, with callback rates of 10.63% and 10.69% in the straight and gay manipulations, respectively.

As previously discussed, prior literature as well as our online experiment results suggest that some types of employers may be significantly more biased than others (see Section 3 and Section 5.2). As a result, in our empirical analysis we consider interactions between our condition assignments and certain traits, and also control for other variables used in similar résumé studies. From the prior literature and our analysis of the online experiment, political party identification of the evaluator emerged as a key variable. In the field experiment, we cannot identify the individual making the callback decision, so his or her political party identification is unobservable. Following Tilcsik (2011), we measure political party support in the firm's geographical area. Figure III shows callback rates for each manipulation in the ten most Republican states, the ten most Democratic

[63] We did not apply to job openings that had been posted for over 30 days. It is possible that some job openings may have been filled or were otherwise inactive. We expect these cases to be uncorrelated with the condition assignments.
[64] Unreported analysis; available from the authors upon request.

states, and the remaining states. In defining these groups of states, we follow the Gallup Organization, which regularly produces lists of the ten most Republican and ten most Democratic states based on self-reported political party identification. In our main analysis, we use actual voting results from the 2012 Presidential election. We define the ten most Republican and Democratic states as those with, respectively, the highest and lowest percentages of voters voting for Mitt Romney in the 2012 presidential election. We refer to the remaining states as politically mixed.[65]

The top panel of Figure III shows that in Republican states 17.31% of applications by the Christian candidate received interview invitations compared to only 2.27% for the Muslim candidate. This difference is striking, but compatible with our estimates of search rates.[66] It is also significant at the five-percent level. In contrast, the bottom part of Figure III shows no difference between gay and straight interview invitations within each group of states. Despite obvious group size differences (the number of applications in Republican states is small), this remains a robust and significant effect. It is robust not only with respect to multiple specifications (see *Analysis* section, below), but also to using larger Republican sub-samples (see county-level results, Table IV), using alternative definitions of Republican states (see Appendix Table A8), and other robustness checks (see Section 6.3).

Analysis

Appendix Table A4 presents summary statistics for control variables used in regression analysis of the field experiment data.[67] As noted, we use control variables that are analogous to

[65] We use Presidential election data from Leip (2013), which is compiled, where possible, from final election results from official sources (e.g., certified election results posted by state election boards). We found that election data from news organizations is often not updated to include final election results. In Section 6.3 we show that the results are robust with respect to using the Gallup Organization's classification of Republican and Democratic states for the year of 2012, according to daily tracking of self-reported political party identification.

[66] Consider three types of employers. The first type rejects the application and does not search the candidate online; the second type considers the candidate further and searches online; the third type considers the candidate further but does not search online. Suppose, for instance, that 65% of the employers are of the first type, 30% of the second type, and 5% of the third type (the assumption of a 30% employer search rate is based on the results presented in Section 6.1). To reduce the number of unknowns, we assume that the callback rate among employers of type 3 equals the average of two callback rates among employers of type 2 - namely, the average of the callback rate for the Muslim candidate and the callback rate for the Christian candidate among employers who search. We now have two equations with two unknowns: $.65*0+.3M+.05(M+C)/2=.02$ and $.65*0+.3C+.05(M+C)/2=.17$, where M and C are the callback rates for the Muslim and Christian candidates among employers who search. Sample-wide callback rates of 2% for the Muslim candidate and 17% for the Christian candidate can be explained by callback rates among employers who search of 2% for the Muslim candidate and 52% for the Christian candidate, and a callback rate when employers consider the candidate but do not search of 27%. (If the results presented in Section 6.1 underestimated actual employers' search rates, or if employers in Republican states have a higher than average search rate, then in this example we can assume a higher fraction of employers who search, which will yield a smaller callback bias for employers of second type.)

[67] We find no statistically significant differences across pairwise conditions among all the controls we tracked for this study, except for the measure of Facebook penetration and the measure of religious protection policy in the Muslim/Christian conditions

those included in prior literature that used comparable (race: Betrand and Mullainathan 2004) or similar (sexual orientation: Tilcsik 2011) treatments in the US labor market. In addition to geographical, job, and firm controls, we also allow interactions between our assignment dummies and control variables in cases where prior literature and our online experiment predict a hypothesis to test. Specifically, in our field manipulation of religion, we test interaction effects that were significant in the online experiment either in the religion manipulation (column (1) of Table II) or in the pooled sample (column (3) of Table II). Thus, we interact state or county measures of political party support, percent foreign born, natural log of median income, and 2012 unemployment rates with the randomized assignment dummy. Similarly, in our field manipulation of sexual orientation, we test interaction effects that were significant in the online experiment either in the sexual orientation manipulation (column (2) of Table II) or in the pooled sample (column (3) of Table II). The variables interacted with our gay versus straight assignment dummy are the state or county measures of political party support, 2012 unemployment rates, and percent non-white.

In regression analysis we consider both state-level and county-level definitions of Republican, politically mixed, and Democratic areas. Figure III used a state-level definition of political areas, which has two drawbacks. First, taking the two manipulations together, there are 1,745 employers in 10 Democratic states or districts (including Washington DC), 2,244 in 31 mixed states, and just 184 in 10 Republican states. Second, we cannot fully control for state-level covariates with state-level definitions of political areas. We address these issues with a county-level definition of Republican, Democratic, and politically mixed areas, and by estimating additional regressions with county-level geographical controls and state-fixed effects. We define the Republican, Democratic, and politically mixed counties by the range of percentages of 2012 Romney voters observed in the state groups. In the county-level measure, the sample sizes in the Republican and Democratic areas are 228 and 2,475, respectively.[68] Finally, in Section 6.3, we test for robustness with respect to changes in estimators and types of standard errors, and with respect to additional specifications and measures of political areas.[69]

(at the 5% level), the two job types (web design, business development), field experience, and public firm (at the 10% level). We control for those – and other – variables in the regression analysis presented further below in this section.

[68] The county-level sample is smaller because we were unable to identify counties for all of our observations. We lose a few additional observations in the county-level regressions because of missing county-level demographic data.

[69] In unreported results (available upon request), we also test for any effect of the application channel (email application versus web application) or the date of the job opening. We fail to find any significant impact of those variables on callback rates.

Table IV presents OLS regressions of callbacks on the Muslim assignment dummy, control variables, and the interactions in the religion manipulation listed above.[70] Column (1) presents a baseline regression that includes a dummy for assignment to the Muslim condition, dummies for the politically mixed states and Democratic states (Republican states are the omitted category)[71], the percent of the state population that is foreign-born, the natural log of state-level median income, state unemployment rates, and interactions between the assignment dummy and each of the other independent variables. Continuous regressors are centered on their means.

The estimated callback rate for the Christian candidate in the default category – namely, Republican states, evaluated at the sample means of the continuous regressors – is .181 (see constant). In the default category, the estimated callback rate in the Muslim condition is significantly lower compared to the Christian condition (see coefficient in first row). This coefficient is -.185, yielding an estimated callback rate for the Muslim candidate of zero, and is significant at the one-percent level. The estimated callback rate for the Christian candidate in politically mixed and Democratic states is lower than it is in Republican states, but not significantly so.

The interactions between the Muslim assignment dummy and the political area dummies are significant. Recall Figure III: there, the difference between the Muslim assignment effect in the Democratic states compared to Republican states is 0.152. In column (1) of Table IV, this interaction effect is estimated to be 0.206 and is significant at the five-percent level. The effect of assignment to the Muslim candidate compared to the Christian candidate is also significantly more positive in politically mixed states than in Republican states, by a substantial amount (0.153). None of the other interaction effects in column (1) are significant.

Column (2) is identical to column (1), except that the political area dummies represent the groups of counties instead of groups of states. The effect of the Muslim assignment dummy in Republican counties, evaluated at the means of the county-level demographic interaction variables, is -0.155 and significant at the five-percent level. In this column the estimated callback rates from employers assigned to the Christian candidate in politically mixed and Democratic counties are significantly lower, at the ten-percent level, than they are in Republican counties. County level

[70] Table IV presents robust standard errors with no clustering. The results are robust with respect to different specifications of the model, as discussed in Section 6.3 (see Appendix Table A5 for probit estimates and Appendix Table A6 OLS estimates with clustered standard errors).

[71] In columns (1), (4), and (6), the politically mixed and Democratic state dummies represent the groups of states presented in Figure III (see Appendix Table A7 for a list of these states).

unemployment rate, percent foreign-born, and natural log of median income do not have significant effects on callback rates. The interactions of the Muslim assignment effect with the politically mixed and Democratic areas are positive and significant. None of the other interactions with the Muslim assignment dummy are significant.

Including state fixed effects in the county-level specification again yields strong results. Column (3) is identical to column (2), except that it includes state fixed effects. The interaction effects are virtually unchanged from column (2). Looking across the first three columns of Table IV, one can see that the county-level political area interaction effects are somewhat smaller in magnitude than the state-level interaction effects. Nonetheless, these interaction effects are remarkably similar, considering that we are comparing between-states estimates in column (1) to within-states estimates in column (3). Recall as well that the fraction and absolute number of applications to Republican counties is somewhat larger than the fraction and number of applications to Republican states (see Appendix Table A4).

Column (4) is the same as column (1), except that it includes additional state-level control variables – namely, the percent of the state population that is non-white, possessing a college degree or higher, Evangelical Protestant, Muslim and urban, as well as Facebook penetration and legal protection from religious discrimination. The Muslim assignment effect and the interaction effects are virtually unchanged from column (1).

Column (5) is the same as column (3), except that it includes additional county-level control variables – namely, the percent of the county population that is non-white, possessing a college degree or higher, Evangelical Protestant, Muslim and dummies for county rural-urban continuum codes. The interaction effects between the Muslim assignment dummy and the political area dummies are significant at the five-percent level for politically mixed counties and at the one-percent level for Democratic counties.

For robustness, we added firm- and job-level controls to the regressions (see columns (6) and (7)). The results obtained by including these variables are consistent with previous state- and county-level regressions discussed above (see columns (4) and (5)). Firm-level controls include dummies indicating whether firms are women and/or minority owned, public, have over 500 employees,[72] or are federal contractors. Job-level controls include 9 dummies for field of

[72] Fewer than 500 employees is a widely used cutoff standard for identifying small firms in U.S. Small Business Administration programs.

employment, a dummy indicating whether the job is an entry-level position, and dummies indicating whether the job application requires references, listing a preferred salary, a master's degree, one or more years of experience required, and multiple fields of experience. The results from the state-level regression with the firm- and job-level controls (column (6)) are consistent with the results from the other state-level regressions (columns (1) and (4)). The results of county-level regressions with firm- and job-level controls (column (7)) are consistent with the results of the other county-level regressions (columns (2), (3), and (5)). The political area interaction effects in both the state- and county-level regressions with the additional controls (columns (6) and (7)) remain significant at levels similar to the prior columns.

Table V presents regression results for our sexual orientation manipulation.[73] There are no noteworthy effects in this table. In particular, there is no effect of the gay assignment dummy compared to the straight assignment dummy. Furthermore, in sharp contrast to the results of the religious affiliation manipulation, there are no significant interactions between the assignment dummy and the political area dummies.

In sum, the field experiment produced results comparable to the online experiment, with no bias against the gay candidate relative to the straight candidate, and stronger bias against the Muslim candidate relative to the Christian candidates among employers located in Republican states or counties, compared to those located in politically mixed or Democratic areas. Our results differ from those of Tilcsik (2011), who found an effect of sexual orientation. However, they are consistent with more recent results presented by Bailey, Wallace, and Wright (2013), who did not find evidence of discrimination against gay men or lesbians. In addition to possible explanations for null results discussed in Section 3.2, changing attitudes toward gay people may explain the differences between our results and Tilcsik's (2011), whose experiment was conducted in 2005. According to our analysis of General Social Survey data,[74] acceptance of gay marriage increased among self-identified Republican, Independent, and Democratic respondents from 2006 to 2012.

6.3. Robustness checks

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[73] The null results of Table V are robust with respect to different specifications of the model. See Section 6.3.

[74] Tom W. Smith, Peter V. Marsden, Michael Hout, Jibum Kim, *General Social Surveys, 1972-2012* [machine-readable data file]. Principal Investigator, Tom W. Smith; Co-Principal Investigators, Peter V. Marsden and Michael Hout, NORC ed. Chicago: National Opinion Research Center, producer, 2005; Storrs, CT: The Roper Center for Public Opinion Research, University of Connecticut, distributor. 1 data file (55,087 logical records) and 1 codebook (3,610 pp). Results are available upon request.

While the regressions we have presented so far have a binary dependent variable, we used OLS because of issues concerning estimates of interaction effects in probit regressions (Ai and Norton 2003). Appendix Table A5 is identical to Table IV, except that it presents probit instead of OLS results. Although the interpretation differs (Buis 2010), the probit coefficients provide evidence of robustness in the results.

Table IV reported unclustered standard errors. Appendix Table A6 is identical to Table IV, except that it presents clustered standard errors (at the state or county level, as appropriate) instead of unclustered errors. The results are consistent with those in Table IV.[75]

Table IV presented county-level regressions with state fixed effects to address potential weaknesses associated with state-level data. Here, we present additional *state*-level robustness checks. Recall that our state-level analyses so far have used the ten most Republican states, ten most Democratic states, and politically mixed states defined according to percentages of voters who voted for Romney in the 2012 U.S. Presidential election. We refer to this categorization of states henceforth as the *Romney vote list* of Republican, politically mixed, and Democratic states. To test whether the results are sensitive to using the Romney vote list, we test for robustness using two additional definitions of Republican, Democratic, and politically-mixed states. The first is the ten most Republican and Democratic states in 2012, according to Gallup survey measures of self-reported political party identification (Saad 2013). We categorize the remaining states as politically mixed. Hereafter we refer to this categorization of states as the *Gallup list*.[76] The second additional definition is a combination of the Romney vote list and the Gallup list, hereafter referred to as the *Combined list*. The combined list of Republican states includes any state on either the Romney vote list or the Gallup list of Republican states. Similarly, the list of Democratic states includes any state on either the Romney vote list or Gallup list of Democratic states. The remaining states are politically mixed according to the Combined list.

Appendix Table A7 presents the lists of states according to these three definitions. The Romney vote and Gallup lists of Republican states differ in three cases. The Romney vote list includes Arkansas, Kentucky, and West Virginia; whereas, the Gallup list excludes these states but includes Alaska, Montana, and North Dakota. Similarly, the Romney vote list of Democratic states

[75] We conducted the same robustness analysis for Table V (the sexual orientation conditions) that we conducted for Table IV. There were no significant results in Table V and no noteworthy changes in the robustness analysis.

[76] Gallup produced this list of states from a sample of 321,233 respondents surveyed by Gallup Daily tracking over the course of the year at the rate of 1,000 respondents per day.

includes California and New Jersey, whereas the Gallup list excludes these but includes Connecticut and Illinois. The Combined list of Republican states is the union of Republican states listed in columns (1) and (2), totaling 13 states. The Combined list of Democratic states is the union of Democratic states, totaling 12 states.

Appendix Table A8 presents regression results using the Gallup list and Combined list of Republican and Democratic states. It shows that the results are robust with respect to the different definitions of Republican and Democratic states. Using the Gallup list (column 1) the estimated difference in the callback rate for the Muslim assignment compared to the Christian assignment is 18.0 percentage points higher in politically mixed states than in Republican states. This interaction effect is significant at the five-percent level. The estimated effect of the Muslim assignment dummy is 19.3 percentage points higher in Democratic states compared to Republican states. This interaction effect is also significant at the five-percent level. Using the Combined list (column (2)) the interactions between the Muslim assignment dummy and the politically mixed states and the Democratic states are 0.186 and 0.243, respectively, and are both significant at the one-percent level.

The rest of the table uses the Combined list for additional robustness checks. Columns (3) and (4) are the same as column (2), except that they show the smallest and largest interaction with the Democratic states that we could find by deleting each of the 50 states (including Washington, DC), one regression at a time. Column (3) removes Alaska from the sample and column (4) removes Vermont. The results remain robust with respect to dropping one state at a time. Even the smallest interaction with the Democratic states dummy is significant at the five-percent level. Columns (5) and (6) are the same as column (2) except that they add, respectively, the additional controls included in columns (4) and (6) of Table IV. Again, the results are robust with respect to the inclusion of these additional controls.

Taking the regression evidence as a whole, it appears that the interaction effects between the Muslim assignment dummy and the political area dummies are robust with respect to estimation choices, measurement of political area dummies, and specification of regressors.

6.4 Understanding hiring bias

The field experiment highlighted a robust association between callback bias and state- and county-level political party support: both politically mixed and Democratic areas have significantly higher callbacks for the Muslim candidate, compared to the Christian candidate, than Republican areas. While our experimental design cannot support conclusive causal claims, a number of possible mechanisms that might be driving the results. Political party identification is associated with a large number of variables (including economic self-interest and a wide range of non-pecuniary preferences); we consider whether any of these might potentially help explain why callback bias varies according to political party support.

A first possibility is that, with Republican party support being stronger in areas with more Evangelical Protestants, callback bias may be driven by animus on the part of Evangelical Protestants. We find no support for this hypothesis.[77] A second possibility is that the fraction of the population that is African American – a strong predictor of Democratic party support – might predict lower callback bias. We find no support for this hypotheses, or related hypotheses, either.[78] A third possibility relates to the idea that norms or preferences against (hiring) Muslims are stronger in Republican areas. Consider that, of all of the demographic variables included as control variables in our main analysis, only one had a significant interaction with our Muslim assignment dummy: the percentage of the county population that is Muslim. The effect is such that callback bias against the Muslim candidate decreases significantly (at the five-percent level) in the percentage of the county population that is Muslim. The result is robust to a variety of specification changes.[79] One possible explanation for this interaction is that because of animus, norms, or other social factors, Muslims may tend not to live in highly Republican areas, and employers in Republican areas may prefer not to hire Muslims. A related possibility is that employers in Republican areas, regardless of their own political party identification, may be concerned that Muslim employees would not be

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[77] In post-hoc analysis, we tested for interactions between the Muslim assignment dummy and the fractions of the state and county populations that are Evangelical Christian, and we found no such interaction. We tried various specifications that mimic our definition of Republican, politically mixed and Democratic areas – namely, with dummies to indicate areas with particularly high and low fractions of Evangelical Christians. The fraction of Christians more broadly also fails to interact significantly with the Muslim assignment dummy. Results are available upon request.

[78] In post-hoc analysis, we find no evidence of an interaction between our Muslim assignment dummy and a variety of measures that might capture this phenomenon, including percentages of the state and county populations that are black, non-white, and foreign-born. Results are available upon request.

[79] For instance, we estimated a regression of callbacks on the Muslim assignment dummy, the percentage of the county population that is Muslim, the interaction between these two variables, state fixed effects, and a variety of controls. The interaction between the Muslim assignment dummy and the percentage of the county population that is Muslim is positive and significant at the five-percent level in all specifications that include state fixed effects. Appendix Table A9 presents county-level specifications that are similar to the county-level regressions in Table IV, except that the interacted variables are the percentage of the county population that is Muslim and the politically mixed and Democratic counties. The magnitude of the interaction with the county-level percentage Muslim is about 0.37 (when state fixed effects are included), implying that if the county-level percentage Muslim were to increase by one tenth of a percentage point, then the difference in the callback rate (i.e., the callback bias against the Muslim candidate) would decrease by 0.037 percentage points.

well received by their current employees (and thus may not be as productive). Indeed, a repeated cross-section of Americans surveyed by the Arab American Institute from 2003-2012 shows that Democratic voters are consistently much more likely to say that their "overall opinion" about Muslims or Arabs is "favorable" compared to "unfavorable".[80] Interestingly, respondents who personally know a Muslim or Arab are substantially more likely to report "favorable" opinions of Muslims.[81] Attitudinal differences may or may not correspond to behavioral differences. Perhaps survey respondents are merely reporting differences in perceptions of what is socially desirable. Nonetheless, the correspondence between attitudes of residents and behavior of employers is striking. Seventy-nine percent of the Republican counties in the field experiment have an undetectable (i.e., zero) percentage of Muslims in their population compared to 13% in Democratic counties.[82] Thus, the unfavorable views about Muslims among Republican voters and those who do not know any Muslims suggests that significantly stronger callback bias against the Muslim candidate in Republican counties, which tend to have few Muslims, may relate to group loyalty or animus among county residents. This interpretation of the data follows Luttmer (2001), who finds compelling evidence of racial group loyalty at the local level in the U.S.

7. Conclusion

We set out to answer the question: Do hiring decision makers seek online information which should not be used in the hiring process, and, if so, how do they react to the information they find? We focused on the effects of religious affiliation and sexual orientation. We used randomized experiments to answer those questions, capturing discrimination both in behaviors of real employers and attitudes of survey subjects.

The online experiment suggests that survey subjects with hiring experience judge the Muslim candidate to be significantly less employable than the Christian candidate. We also find

[80] "The American Divide: How We View Arabs and Muslims," Arab American Institute, August 23, 2012, accessed September 26, 2014, http://b.3cdn.net/aai/4502fc68043380af12_oum6b1i7z.pdf. For instance, in 2012, Romney voters reported views toward Muslims that were "unfavorable" in 57% of responses and "favorable" in 25% of responses. The views of Obama voters were "unfavorable" 29% of the time and "favorable" 53% of the time. See Section 3.1 for additional sources on differences in views toward Muslims by political party identification.

[81] Of people who respond "yes" to the question: "Do you personally know anyone who is Arab or Muslim?", 57% say they have "favorable" opinions about Muslims and 36% say they have "unfavorable" opinions. Of those who say they do not personally know an Arab or Muslim person, 32% say they have "favorable" opinions and 46% say they have "unfavorable" opinions. "The American Divide: How We View Arabs and Muslims," Arab American Institute, August 23, 2012, accessed September 26, 2014, http://b.3cdn.net/aai/4502fc68043380af12_oum6b1i7z.pdf.

[82] The percentage of the county population that is Muslim has a highly significant negative association with the county-level percentage Romney vote, both with and without state fixed effects. Results available upon request.

greater bias among subjects who self-reported Republican political party identification. We find little evidence of discrimination based on sexual orientation. These results are robust with respect to demographic controls including income, age, race, religion, and being U.S. born.

The results from the field experiment are broadly consistent with the online experiment. First, the field experiment indicates that a substantial minority of U.S. employers we applied to searched for the candidates online. Second, in the religion manipulation, discrimination against the Muslim candidate relative to the Christian candidate is significantly higher among employers who are located in Republican states or counties compared to those located in politically mixed or Democratic states or counties. Taken together, these findings suggest that while hiring discrimination via Internet searches and social media may not be widespread for the companies and jobs we considered in this experiment, revealing certain traits online may have a significant effect on the hiring behavior of self-selected employers who search for the candidates online.

This work suggests fruitful directions for future research. As discussed in the introduction, there is a tension between a potentially efficiency-enhancing effect of online information on labor market search and a potential for that same information to increase labor market discrimination. To the extent that online markets facilitate not just matching but also discrimination, people in disadvantaged categories may face a dilemma: if they censor their online activity, they may be able to protect their privacy, but a limited online presence might, by itself, signal that there is something to hide, or that something is missing. Imagine a charismatic Muslim candidate with both a strong social network in his religious community and many professional contacts. If he were to reduce his online presence to hide his friendships in his religious community, at least three problems arise. The first is a “lemons” effect: people may assume that those with a restricted online presence belong to less preferred categories. The second is what we may term an “online network exclusion effect”: by protecting his privacy, the candidate is deprived of the opportunity to present professionally relevant positive traits about himself. The third effect is a bundled traits effect: candidates may have some traits that employers find less desirable and some traits that are more desirable, and online mechanisms sometimes bundle together information about undesirable and desirable traits. For the charismatic Muslim candidate described above, information about personal traits that are potentially less desirable to employers may be bundled with information about traits that are more desirable to employers, including the strength of his professionally relevant social networks. This presents additional challenges for disadvantaged candidates who may wish to protect information about traits that are disadvantageous in the labor market while promoting their positively viewed traits. This dilemma may motivate design of mechanisms that facilitate communication only about job-relevant characteristics. Thus, a few multi-faceted questions for future research arise, which could be asked descriptively using the methodology in this paper: Does the amount of online self-disclosure affect labor market success? (For instance, in an age of self-disclosure, is the absence of social media presence a neutral, positive, or even negative signal? Do employers pay attention to the size of a candidate' personal social network?) Will market mechanisms develop to solve some of these problems? It may also be interesting to ask these questions normatively: How should online self-disclosure affect labor market success? How should markets be structured to address these problems?

Some employers openly admit to using social media in the hiring process (Llama et al. 2012); at the same time, legal professionals warn firms about the risks of doing so (Harpe 2009). Both the EEOC and state legislators have become interested in the role of the Internet and social media in the job search process; at the same time, companies like HireRight.com and Rapleaf.com have started offering related services – arguably shielding firms from the risks associated with doing the search themselves. In sum, the rise of the Internet and social media has increased the amount of personal and professional information available to employers about a job candidate. Our paper offers a first insight and a methodological path towards understanding the extent to which this new information channel may reduce labor market frictions, or lead to more discrimination.

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APPENDIX

Appendix A. Online Experiment: Employability Study Questionnaire

Q1.1 Before we start the task, we have a little test to identify people who are paying attention to the instructions of the survey. In the next page, you will see a photo of three people. Then, you will be asked to state how many people you see in the photo. We want you to answer "three" even though you will see four people in the photo. This is to make sure that only M-Turkers who pay attention to these instructions continue to our task. In other words, in order to continue to the rest of the study and be paid for your time, you must answer incorrectly by choosing the "three" option. All right? Ok, please proceed to the next page!
<PAGE BREAK>

[Photo of four people here]
Q1.2 How many people can you see in this picture? (0; 1; 2; 3; 4; 5)
[If 3 Is Not Selected, Then Skip To End of Survey]
Q2.1 Dear [Participant] – Thank you for participating in our task. In the following pages, you will be presented with the résumés of one or more individuals who are applying for managerial level positions in mid to large technology and consulting firms, or for technology related positions in other industries. Links to the applicants' online social network profiles, if available, will also be provided. We did receive approval to show the candidate's information as part of this study. HR personnel at those firms will be making actual hiring decisions about these applicants. We want to compare the hiring advice of [platform participants] to those of professional HR personnel who are receiving the same job applications. Specifically, we would like your opinion on whether you would call the applicant back for an interview if you were an HR person at one of the firms to which he or she is applying. You should form your decision based on perusal of the résumé of the applicant and, if available, the other linked information. You must be 18 years or older to participate in this task. The task is expected to take around 20 minutes. Your answers will not be linked to your [platform name] accounts for payment. Have you read and understood these instructions? Please briefly summarize this task in your own words (that is, without copying and pasting the text above; providing a correct description of the instructions is a condition to receive your payment on [platform name]). When you are done, please click on "Next."
<PAGE BREAK>

[Instructional Text]
The URLs below contain links to the online résumé and (if available) the online profiles of an individual who is applying for a managerial level position in a mid-size technology firm based in the US. Please take as much time as needed to study and vet the background of this individual. In the following pages, you will be asked questions about him/her. After you are done perusing the documents of this job applicant, please click on the "Next" button. You can click on the links below, or copy and paste the links into your browser:
Q3.1 [Instructional Text for No SN conditions]
Résumé of the applicant in pdf: [link]
Link to the applicant's profile on PN: [link]
<PAGE BREAK>

[Instructional Text for other conditions]
Résumé of the applicant in pdf: [link]
Link to the applicant's profile on PN: [link]
Link to the applicant's profile on SN: [link]
<PAGE BREAK>

Q119 What was the name of the candidate?
<PAGE BREAK>

Q77 The individual whose background and experience you just examined is applying for a managerial level position in a mid-size technology firm based in the US. Consider being an HR professional at the firm to which this candidate is applying. You can assume that the individual is applying for a position in his or her area of professional expertise, and that you can make your decision for this applicant independently of any information about other candidates who may also be applying for the same position. Would you call back this individual for an interview? [Yes, I would call this individual back for an interview; No, I would not call this individual back for an interview]
<PAGE BREAK>

Q21.1 Now, we would like to ask you to predict whether or not the applicant will in fact be called back for an interview by the firm to which he or she is applying. After the hiring season has ended, and the answer to this question is known, we will check which [participants] made the right prediction. If your prediction is correct, you will be entered into a lottery for a $100 prize among other participants in this task. If you are a winner, we will contact you through [the survey platform]. Just your best guess: do you think that this individual will be called back for an interview by the firm he or she is applying to? [Yes, I think that he or she will be called back for an interview; No, I think that he or she will not be called back for an interview]
<PAGE BREAK>

[Note: Responses for Likert scale in questions below are: Strongly Disagree; Disagree; Somewhat Disagree; Neither Agree nor Disagree; Somewhat Agree; Agree; Strongly Agree]
Q21.2 We would like to learn more about your evaluation of this job applicant. Please tell us how you would evaluate this applicant in the following dimensions:
Q21.3 I believe that this candidate would be a great hire for such a position. His or her qualifications are excellent and are well-matched to the position. [Response categories: Likert Scale]
Q21.4 I believe that the candidate is not sufficiently qualified for the position and/or does not seem to have enough promise for success in the position. [Response categories: Likert Scale]
Q21.5 I believe that the candidate is over-qualified for the position. [Response categories: Likert Scale]
Q21.6 I believe that the candidate would not fit into the workplace or would not be a good fit for the position. [Response categories: Likert Scale]
Q21.7 I would like to gather more information about this candidate. [Response categories: Likert Scale]
Q21.8 I would like to gather more information about the opening this candidate is applying to. [Response categories: Likert Scale]
Q21.9 How interested would you be in meeting this candidate as a social acquaintance or contact if such an opportunity were available? [Response categories: Likert Scale]
Q21.10 How interested would you be in meeting this candidate as a professional acquaintance or contact if such an opportunity were available? [Response categories: Likert Scale]
Q21.11 Please describe any other considerations you made when you decided whether or not to interview the candidate.
<PAGE BREAK>

Q21.12 When you studied the job applicant's résumé and background, which characteristics came to mind? Select the point on each of the following rows which comes closest to the characteristics this name evokes. If no characteristic comes to mind, please select the select "I do not know."
Q21.13 1 [Male; Female; I do not know]
Q21.14 1 [Professional; Unprofessional; I do not know]
Q21.15 1 [Attractive; Unattractive; I do not know]
Q21.16 1 [Married; Single; I do not know]
Q21.17 1 [Straight; Gay or Lesbian; I do not know]
Q21.18 1 [Caucasian; African American; I do not know]
Q21.19 1 [Christian; Muslim; I do not know]
Q21.20 1 [Has kids; Does not have kids; I do not know]
<PAGE BREAK>

Q21.21 Do you have other comments about the job applicant's résumé?
Q21.22 Did you use Google or other search engines to find additional information about the job applicant? [Yes; No]
Q21.23 If yes, can you please explain why you used a search engine, and what you found?
Q21.24 Did you use PN to find information about the job applicant? [Yes; No]
Q21.25 If yes, can you briefly comment on the job applicant's PN profile, and how it affected (if at all) your decision?
Q21.26 Did you use SN to find information about the job applicant? [Yes; No]
Q21.27 If yes, can you briefly comment on the job applicant's SN profile, and how it affected (if at all) your decision?
Q80 Did you log on to SN to view the candidate's profile? [I checked the profile but without logging in; I checked the profile after logging in; I did not check the profile; I do not have a SN account]

Q21.28 If you believe that the firm would make a different decision than yours about the candidate (e.g., if you believe that the firm will call the candidate for an interview but you would not, or vice versa), can you please briefly explain the reason for your belief?
Q21.29 Do you have any other final comment about this questionnaire?
<PAGE BREAK>

Q21.30 Have you ever taken this study before (that is, an MTurk task in which you were provided links to the résumé and online profiles of a candidate, and asked whether you would call that candidate for an interview)? Don't worry, you will get paid for your task regardless of your answer; however, please reply honestly because otherwise the results of this study will be corrupted. [Yes; No; I do not know]
<PAGE BREAK>

Q22.1 We are almost done. Here are some final demographic questions.
Q22.2 How old are you? [20 or younger; 21 – 30; 31 – 40; 41 – 50; 51 – 60; 61 or older; Prefer not to answer]
Q22.3 Are you male or female? [Male; Female; Prefer not to answer]
Q22.4 What is your marital or relationship status? Please check all that apply. [Single; Happily involved with a serious girlfriend or boyfriend; Happily Married; Involved in an unhappy relationship or marriage; Divorced or Separated; Widowed; Prefer not to answer]
Q22.5 What is the highest level of education that you have obtained? [Some high school or no high school; High school graduate; Trade school/Associate degree; Some college; College graduate; Advanced degree; Prefer not to answer]
Q22.6 What is your race? [African American; Non-American African Descent; Caucasian; Hispanic; Asian/Pacific Islander; Prefer not to answer; Other (please specify)]
Q22.7 Have you ever had hiring responsibilities in your professional life? [Never; Once or Twice; Sometimes; Often; I work, or worked, in Human Resources (HR)]
Q22.8 What is your current work situation? Please check the category that best represents you. [Student; Stay at home caregiver or homemaker; Unemployed; Part-time employed; Full-time employed; Self-employed; Retired; Not working for some other reason (e.g., disability, illness, etc.); Prefer not to answer; Other (please specify)]
Q22.9 What industry do you currently work in? [Natural Resources and Mining; Construction; Manufacturing; Trade, Transportation, and Utilities; Information; Financial activities; Professional and Business Services; Education and Health Services; Leisure and Hospitality; Public Administration; Arts; Other]
Q22.10 We would like to know more about your occupation. Please check all of the boxes below that describe the work that you do: [Blue collar; White collar; Service worker; Administrative support worker; Lower management; Middle management; Upper management; Other; Prefer not to answer]
Q22.11 Which country were you born in? [Response categories: "Prefer not to answer" and all 196 countries]
Q22.12 Which country do you live in now? [Response categories: "Prefer not to answer" and all 196 countries]
Q22.13 If you live in the US, what is your state/territory of residence? [Response categories: "Prefer not to answer", the U.S. states, the District of Columbia, and the unincorporated commonwealths]
Q22.14 Now some final questions. They relate to politics and income and your answers would greatly help our study. However, you do not need to answer them if you do not feel comfortable doing so (as a reminder, this survey is anonymous).
Q22.15 What is your religious affiliation? [Prefer not to answer; Christianity; Islam; Judaism; African Traditional & Diasporic; Agnostic; Atheist; Baha'I; Buddhism; Cao Dai; Chinese traditional religion; Hinduism; Jainism; Juche; Neo-Paganism; Nonreligious; Rastafarianism; Secular; Shinto; Sikhism; Spiritism; Tenrikyo; Unitarian-Universalism; Zoroastrianism; primal-indigenous]
Q22.16 Have you had an experience that convinced you of the existence of God? [1 = Definitely no; 7 = Definitely yes; Prefer not to answer]
Q22.17 How many children do you have? [1; 2; 3; 4; 5 or more; Prefer not to answer]
Q22.18 How dedicated do you feel to your own work, profession or career on a scale from 1 to 5? [1 = Very dedicated; 5 = Not dedicated; 6 = Prefer not to answer]
Q22.19 Which political party do you most identify with? [Republican, Democrat, Independent, Prefer not to answer, Other (please specify)]
Q22.20 How would you rate your political leanings? [1 = Left/Liberal; 10 = Conservative/Right; Prefer not to answer]
Q22.21 What is your annual household income before taxes? [$0 - $20,000; $20,001 - $45,000; $45,001 - $70,000; $70,001 - $100,000; More than $100,000; Prefer not to answer]

Appendix B. Field Experiment: Search Results

We used two data sources to estimate the percentages of employers who searched for the candidates online. One data source consisted of Google AdWords "Keyword Tool" statistics. These publicly accessible statistics captured the number of times a certain term was searched on Google from various locations. We used this tool to estimate the number of times the exact names of the candidates were searched from U.S. IP addresses. The second data source consisted of statistics provided by the PN network (LinkedIn) via so-called "Premiere" accounts. If a user subscribes to a Premiere account, that user will be able to get information such as the count of visits to its PN profiles, and in some cases the actual identity of the visitors. We subscribed to Premiere accounts for each of our candidates' profiles in order to track visits to those profiles.

Each of these sources of data is imperfect. Google Keyword Tool statistics provide aggregate monthly means for searches of a given term, rather than raw data. Furthermore, if the mean is higher than zero but below 10 searches per month, no exact count is provided.[83] Similarly, "Premier" PN accounts do not actually track all visits to that account (for instance, in our tests, visits from subjects that had not logged onto the network went undetected and uncounted; visitors who only viewed the summary profile of the candidate, rather than his full profile, also were not detected or counted; in addition, certain premiere accounts may allow "invisible" views of other LinkedIn profiles).[84] However, considered together, these data sources offer a rough estimate of the proportions of employers searching the candidates online.

The most conservative number of Google searches we documented was 240, or 5.74% of employers we applied to. This number is almost certainly too conservative, since it is based only on a subset of searches: Google began phasing out in the summer of 2013 the specific metrics we were using from its Keyword Tool website, right near the end of the phase of the experiment in which we stopped submitting applications to job openings, but before all employers' responses were received; as a result, we were unable to update information for two of the candidates.[85] Up to that point, the statistics for the search of all candidates followed similar trends. Assuming that the trend for the two candidates with incomplete search data would continue in the same way as the trend for the other candidates, the overall search rate would have been an average of 108 searches for each of our candidates, or about 10.33% of the employers we applied to.

In addition, while Google's Keyword Tool statistics can tell us that at least a certain number of searches were performed for the candidates, they do not necessarily provide us with the total number of Google searches. Furthermore, and importantly, the data refer only to Google searches, and not searches on Bing or other search engines, or searches conducted directly inside the social networks (both SN and PN), which may be frequent. While we cannot capture searches done inside the SN, we can attempt some extrapolations. First, as noted above, we did gather some data by purchasing "Premiere" accounts that provide information about visitors to the PN profiles. We counted 94 such visits (or about 2.26% of our applications).[86] As noted above, however, these numbers should also be taken with caution, as

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[83] We used Google Keyword Tool statistics to capture monthly means of the total number of searches for the candidates' names over the previous 12 months. Since the 12 months period included months before the start of the project – when searches were zero – as time progressed, the estimates rose and become more precise. While useful for the purpose of attaining order-of-magnitude estimates, the data has limitations: we do not know the exact algorithm used in Keyword Tool trends data, but we know that the number is not absolute - it is based on the number of searches for a given term, relative to the total number of searches done on Google over time. See "Where Trends Data Comes From," Google, accessed July 22, 2014, https://support.google.com/trends/answer/92768?hl=en&ref_topic=13975. Furthermore, the data is normalized. See "How Trends Data is Normalized," Google, accessed July 22, 2014, https://support.google.com/trends/answer/87284?hl=en&ref_topic=13975.

[84] Only for a small subset of application were we able to identify specific employers visiting the candidates' profiles – for instance, because someone visited the PN of our candidates from an account linked to a company to which we had recently applied. By and large, employers who search should not be considered recognizable within our sample. For this reason, we cannot reliably estimate differences in search rates by location or industry or job type.

[85] As noted above, the aggregate trend data is imprecise for low levels of search, having just two categories below 10 (zero and zero to ten) compared to a record for each search above 10. Thus, for two candidates the number was greater than zero, but below ten, meaning we have imprecise data for them; and for two candidates the number of searches reached ten, meaning we have more precise data for them. Furthermore, our tests indicate that Google Trend data appeared to be irregularly updated (that is, not all search terms were updated at the same time). As part of our tests, we tracked the trends of a fictional, nonexistent, and unpronounceable name ("Adam Yujdkdebbnbaopl") and those for a famous actor with a rare last name ("Jake Gyllenhaal"). While mean searches for Adam Yujdkdebbnbaopl were reported as zero for the duration of the experiment, as expected, searches for Jake Gyllenhaal were large, of course (673,000 per month) – but this number did not change during the several weeks period of our test.

[86] Whereas Google search data is not comparable across conditions due to the sudden termination of the service, we can compare the number of PN visits across conditions. The differences are not statistically significant ($p > .1$).

they most likely underestimate the number of actual visits to the PN profiles. Furthermore, we can weight data we obtained from Google Keyword Tool with third-party data showing the degree of overlap or relative proportions between searches via different tools, in order to provide some overall estimates of the online searches conducted for our candidates during the period the experiment was ran. Based on analyses of search engine market shares,[87] and assuming that the ratio of Bing's market share to Google's correlates with the search volume on each candidate, one may conjecture that for each single Google search, 0.25 Bing searches of the candidates were also performed. By similar logic, using data available from surveys of employers' usage of Internet tools for candidate searches, for every Google search of the candidates one could expect between 0.66 to 0.81 searches occurred directly within the SN, and approximately 0.73 searches occurred directly within the PN.[88] Naturally, it is not possible to know whether these would be *additional* employers than those performing Google searches, or the same employers. By combining the abovementioned statistics and proportions we can produce optimistic estimates of aggregate Internet searches for our candidates.[89] Using the proportions indicated above, and adding together searches from each search engine or network, we find a higher estimate of 28.82% of employers who searched for the candidates.[90]

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[87] See Adam Lella, "comScore Releases February 2013 U.S. Search Engine Rankings," comScore, last modified March 13, 2013, accessed July 22, 2014, http://www.comscore.com/Insights/Press_Releases/2013/3/comScore_Releases_February_2013_U.S._Search_Engine_Rankings.

[88] These ratios are calculated based on data found in the following surveys of employers: "Employers Using Social Networks for Screening Applicants," accessed June 24, 2014, http://wikibin.org/articles/employers-using-social-networks-for-screening-applicants.html; "Ponemon Institute HR Special Analysis, May 2007," International Association of Privacy Professionals, accessed June 24, 2014, https://www.privacyassociation.org/publications/2007_12_ponemon_institute_littler_mendelson_study; and Stuart J. Johnston, "Microsoft Survey: Online 'Reputation' Counts," Internet News, last modified January 27, 2010, accessed June 24, 2014, http://www.internetnews.com/webcontent/article.php/3861241/Microsoft+Survey+Online+Reputation+Counts.htm.

[89] The estimate is optimistic since it assumes no overlap among employers searching, for instance, on Google and SN.

[90] The calculation starts from a baseline proportion of 10.33% employers searching on Google, and adds weighted proportions for Bing, SN, and PN searches.

Appendix Table A1.
Online Experiment: Selected Summary Statistics. Numbers Shown are Means (Standard Deviations in Parentheses) for Christian, Muslim, Gay, and Straight Conditions Pooled

	Mean (s.d.)	N
Muslim or gay treatment	0.497 (0.500)	776
No Hiring Experience	0.506 (0.500)	761
Republican	0.168 (0.374)	667
Democratic	0.430 (0.495)	667
Independent	0.402 (0.491)	667
Foreign-born	0.064 (0.245)	752
Non-Christian	0.515 (0.500)	687
No college degree	0.513 (0.500)	753
Female	0.578 (0.494)	753
Non-white	0.187 (0.390)	743
Unemployed	0.083 (0.276)	748
Income ≤ $20k	0.187 (0.390)	696
$20k < Income ≤ $45k	0.330 (0.471)	696
$45k < Income ≤ $70k	0.249 (0.432)	696
$70k < Income ≤ $100k	0.134 (0.340)	696
Income > $100k	0.101 (0.301)	696
Under 21 years old	0.105 (0.306)	755
21-30 years old	0.491 (0.500)	755
31-40 years old	0.223 (0.416)	755
41-50 years old	0.111 (0.315)	755
51-60 years old	0.056 (0.229)	755
Over 60 years old	0.015 (0.120)	755

Appendix Table A2.
Online Experiment: Differences in Treatment Effects According to Background Characteristics of Subjects

	(1) Religion Manipulation	(2) Sexual orientation manipulation	(3) Religion and sexual orientation manipulations pooled
Treatment*No hiring experience	0.444**	-0.009	0.230
	(0.203)	(0.206)	(0.144)
Treatment*Political independent[a]	0.451	0.342	0.391*
	(0.279)	(0.327)	(0.215)
Treatment*Democrat[a]	0.399	0.478	0.439**
	(0.271)	(0.333)	(0.215)
Treatment*Foreign-born	1.277***	0.093	0.619**
	(0.479)	(0.291)	(0.274)
Treatment*Non-Christian	0.300	0.067	0.168
	(0.205)	(0.215)	(0.149)
Treatment*No college degree	0.215	0.202	0.219
	(0.204)	(0.207)	(0.144)
Treatment*Female	-0.098	0.348	0.142
	(0.207)	(0.213)	(0.148)
Treatment*Non-white	0.199	0.494*	0.368*
	(0.326)	(0.271)	(0.203)
Treatment*Unemployed	-0.249	-0.436	-0.393*
	(0.297)	(0.326)	(0.209)
Treatment*Y≤$20k[b]	0.975**	-0.386	0.219
	(0.410)	(0.422)	(0.309)
Treatment*$20k< Y≤$45k[b]	0.297	-0.497	-0.164
	(0.352)	(0.380)	(0.276)
Treatment*$45k< Y ≤$70k[b]	0.288	-0.072	0.026
	(0.397)	(0.414)	(0.303)

Appendix Table A2
(continued)

	(1) Religion Manipulation	(2) Sexual orientation manipulation	(3) Religion and sexual orientation manipulations pooled
Treatment*$70k< Y ≤$100k[b]	0.821**	-.101	0.318
	(0.393)	(0.409)	(0.299)
Treatment*under 21 years old[c]	0.344	0.469*	0.402
	(0.764)	(0.272)	(0.411)
Treatment*21-30 years old[c]	0.303	0.167	0.205
	(0.728)	(0.203)	(0.383)
Treatment*31-40 years old[c]	0.333	0.268	0.289
	(0.750)	(0.267)	(0.403)
Treatment*41-50 years old[c]	0.312	-0.267	0.002
	(0.754)	(0.366)	(0.421)
Treatment*51-60 years old	0.827	0.558	0.681
	(0.800)	(0.417)	(0.458)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Numbers presented are OLS coefficients (standard error in parentheses). Each OLS coefficient is the interaction effect in a regression that includes a constant, the treatment dummy, a background characteristic, an interaction between the treatment, and the background characteristic. Unless noted otherwise, there is one such regression for each of the control variables in the analysis. The treatment dummy equals: 1 if the candidate is Muslim and zero if Christian in the religion manipulation; 1 if the candidate is gay and zero if straight in the sexual orientation manipulation; and 1 of the candidate is either Muslim or gay, and zero if either Christian or straight in the pooled data.

[a] These interaction effects are from a single regression that includes these interactions, the three associated first order effects, and a constant.

[b] These interaction effects are from a single regression that includes these interactions, the five associated first order effects, and a constant.

[c] These interaction effects are from a single regression that includes these interactions, the six associated first order effects, and a constant.

Appendix Table A3.
Online Experiment: Manipulation Checks. OLS Regressions of Beliefs on Treatment Dummies when Link to Facebook Profiles (i.e. SN manipulation) Was Provided

	(1) Believes candidate is Muslim (versus Christian)	(2) Believes candidate is gay (versus straight)
Muslim treatment (versus Christian)	0.766*** (0.053)	
Gay treatment (versus straight)		0.904*** (0.046)
Constant	0.000 (0.035)	0.026 (0.036)
Observations	111	96
R^2	0.654	0.801

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Numbers reported are OLS coefficients (standard errors in parentheses). The dependent variable is coded as 1 if the respondent believes the candidate is Muslim in columns (1) or (2), or gay in columns (3) and (4). It is coded as zero if the respondent believes the candidate is Christian or straight. "Don't know" responses are coded as missing.

Appendix Table A4.
Field Experiment: Selected Summary Statistics

	Religious affiliation conditions		Sexual orientation conditions	
	Mean (s.d.)	N	Mean (s.d.)	N
Panel A: State-level variables[a]				
Politically mixed state	0.534 (0.499)	2082	0.541 (0.498)	2091
Democratic state	0.420 (0.494)	2082	0.417 (0.493)	2091
Facebook penetration of state	0.519 (0.067)	2082	0.518 (0.060)	2091
State sexual orientation protection policy			0.984 (.0931)	2091
State prohibits pre-employ. inquiries about religion	0.622 (0.485)	2082		
Ln(median income)	10.915 (0.134)	2082	10.914 (0.133)	2091
Unemployment rate	8.218 (1.615)	2082	8.233 (1.604)	2091
% Bachelor's degree or more	0.312 (0.051)	2082	0.311 (0.049)	2091
% Non-white	0.276 (0.104)	2082	0.275 (0.104)	2091
% Foreign-born	0.152 (0.079)	2082	0.151 (0.080)	2091
% Evangelical Protestant	9.647 (6.384)	2082	9.690 (6.433)	2091
% Islamic	0.048 (0.029)	2082	0.049 (0.031)	2091
% Urban population in state	84.508 (10.467)	2082	84.077 (10.739)	2091
Panel B: County-level variables[b]				
Politically mixed area	0.352 (0.478)	2057	0.338 (0.473)	2071
Democratic area	0.586 (0.493)	2057	0.613 (0.487)	2071
2012 Local area unemployment rate	7.507 (1.739)	2060	7.492 (1.696)	2074

Appendix Table A4
(continued)

	Religious affiliation conditions		Sexual orientation conditions	
	Mean (s.d.)	N	Mean (s.d.)	N
Ln(median income in local area)	11.029 (0.252)	2050	11.029 (0.245)	2066
% Bachelor's degree or more	0.389 (0.109)	2050	0.396 (0.108)	2066
% Non-white	0.325 (0.146)	2048	0.330 (0.150)	2064
% Evangelical Protestant	8.135 (6.362)	2060	7.939 (5.880)	2074
% Islamic	0.059 (0.078)	2060	0.060 (0.089)	2074
Counties in metro areas with pop. 250K to 1M	0.136 (0.343)	2060	0.135 (0.342)	2074
Counties in metro areas with pop. < 250K	0.035 (0.184)	2060	0.037 (0.188)	2074
Urban pop. of >=20K, adjacent to metro area	0.015 (0.122)	2060	0.009 (0.095)	2074
Urban pop. of >=20K, not adjacent	0.002 (0.049)	2060	0.006 (0.076)	2074
Urban pop. <20K, adjacent to metro area	0.006 (0.079)	2082	0.006 (0.079)	2091
Urban pop. <20K, not adjacent to metro area	0.003 (0.058)	2082	0.005 (0.069)	2091
Panel C: Firm-level variables[c]				
Public firm	0.196 (0.397)	2082	0.178 (0.383)	2091
Firm size: 500 employees or more	0.279 (0.449)	2082	0.295 (0.456)	2091

Appendix Table A4
(continued)

	Religious affiliation conditions		Sexual orientation conditions	
	Mean (s.d.)	N	Mean (s.d.)	N
Firm Women/Minority owned	0.061 (0.240)	2009	0.063 (0.243)	2011
Federal contractor	0.401 (0.490)	2082	0.409 (0.492)	2091
Entry level job	0.130 (0.337)	2082	0.129 (0.335)	2091
References required	0.100 (0.300)	2082	0.103 (0.304)	2091
Salary required	0.230 (0.421)	2082	0.228 (0.420)	2091
Master degree required	0.167 (0.373)	2082	0.172 (0.378)	2091
Work experience required	0.827 (0.379)	2082	0.833 (0.373)	2091
Multiple fields required	0.146 (0.354)	2082	0.170 (0.376)	2091
Job in information systems	0.213 (0.409)	2082	0.218 (0.413)	2091
Job in analytics	0.127 (0.333)	2082	0.116 (0.320)	2091
Job in business development	0.034 (0.180)	2082	0.036 (0.186)	2091
Job in business intelligence	0.081 (0.272)	2082	0.080 (0.271)	2091
Job in health care	0.023 (0.150)	2082	0.029 (0.167)	2091
Job in information security	0.056 (0.229)	2082	0.045 (0.208)	2091
Job in product development	0.130 (0.336)	2082	0.132 (0.338)	2091
Job in quality assurance	0.075 (0.264)	2082	0.070 (0.256)	2091
Job in software engineering	0.183 (0.387)	2082	0.196 (0.397)	2091

Appendix Table A4
(continued)

	Religious affiliation conditions		Sexual orientation conditions	
	Mean (s.d.)	N	Mean (s.d.)	N
Job in web design	0.079 (0.270)	2082	0.079 (0.270)	2091

[a] Sources for Panel A:
Politically mixed and Democratic states: Defined by official state results from the 2012 Presidential election. Voting data assembled by Leip (2013). See Appendix Table A7, column 1 for lists of states.
State sexual orientation protection policy: States with neither an anti-discrimination law nor a prohibition against pre-employment inquiries are coded as 0. States that have either anti-discrimination laws or a prohibition against pre-employment inquiries, but not both, are coded as 1. States with both anti-discrimination laws and a prohibition against pre-employment inquiries are coded as 2. See "State Laws on Employment-Related Discrimination," National Conference of State Legislatures, last modified October 2010, accessed July 28, 2014, http://www.ncsl.org/documents/employ/DiscriminationChart-III.pdf.
State-level prohibition against pre-employment inquiries about religion: States were coded as either yes (inquiries prohibited) or no. Discrimination based on religion is federally prohibited in the United States under Title VII of the Civil Rights Act of 1964, so there is no state-level variation. See individual state policies listed at: http://www.blr.com/HR-Employment/Discrimination/Religious-Discrimination.
Facebook penetration: Author calculations using Facebook and CPS data. We obtained Facebook data by using each state as a potential advertising target market on Facebook, which gives data on exactly how many Facebook users are in each state. The number of Facebook users was divided by CPS state population data. See "Advertise on Facebook," Facebook, accessed July 21, 2014, https://www.facebook.com/ads/create/?campaign_id=433405496710037&placement=bkmk_adcr&extra_1=campaign. See "State Totals: Vintage 2012," United States Census Bureau, accessed July 21, 2014, http://www.census.gov/popest/data/state/totals/2012/index.html.Geographical variables: 2012 American Community Survey, U.S. Census, and Infogroup Religion 2010 survey, all accessed from http://www.socialexplorer.com/.
[b] Sources for Panel B:
Politically mixed and Democratic counties: Defined by official county results from the 2012 Presidential election. Voting data assembled by Leip (2013).
Geographical variables: 2012 American Community Survey, U.S. Census, and Infogroup Religion 2010 survey, all accessed from http://www.socialexplorer.com/.
Sources for Panel C:
Firm characteristics: Hoovers.com and ReferenceUSA.com
Job characteristics: Authors' data.

Appendix Table A5.
Field Experiment: Probit Analyses in the Christian-Muslim Manipulation. Dependent Variable Is Callbacks

	(1) State	(2) County	(3) County	(4) State	(5) County	(6) State	(7) County
Muslim Condition	-1.228**	-0.742**	-0.786**	-1.236**	-0.912***	-1.164**	-0.732**
	(0.502)	(0.319)	(0.339)	(0.505)	(0.350)	(0.516)	(0.371)
Politically mixed state or county	-0.217	-0.373*	-0.492**	-0.280	-0.446*	-0.156	-0.279
	(0.230)	(0.200)	(0.221)	(0.242)	(0.232)	(0.253)	(0.248)
Democratic state or county	-0.285	-0.382*	-0.577**	-0.431	-0.491*	-0.279	-0.375
	(0.308)	(0.209)	(0.243)	(0.357)	(0.277)	(0.376)	(0.297)
Percentage Foreign-born	-1.584	0.025	0.724	-2.296	0.803	-3.161**	0.565
	(1.176)	(0.654)	(0.823)	(1.508)	(0.939)	(1.579)	(0.994)
Median income (ln)	0.685	-0.152	-0.596*	1.632*	-0.375	1.494	-0.514
	(0.563)	(0.270)	(0.339)	(0.984)	(0.420)	(1.045)	(0.448)
Unemployment rate	0.047	-0.022	-0.095*	0.078	-0.041	0.071	-0.013
	(0.047)	(0.036)	(0.053)	(0.054)	(0.061)	(0.057)	(0.065)
Muslim*Politically mixed state or county	1.068**	0.607*	0.658*	1.071**	0.787**	1.014**	0.537
	(0.487)	(0.333)	(0.355)	(0.490)	(0.366)	(0.501)	(0.388)
Muslim*Democratic state or county	1.332**	0.765**	0.830**	1.347**	0.951**	1.213**	0.755*
	(0.576)	(0.343)	(0.364)	(0.578)	(0.374)	(0.595)	(0.397)
Muslim*Percentage foreign-born	0.672	0.270	0.105	0.745	0.258	1.198	0.743
	(1.739)	(0.945)	(0.959)	(1.763)	(0.967)	(1.849)	(1.046)
Muslim* median income (ln)	-0.796	-0.194	0.267	-0.924	-0.299	-0.973	-0.630
	(0.805)	(0.392)	(0.399)	(0.814)	(0.400)	(0.862)	(0.433)
Muslim* unemployment rate	-0.068	-0.008	-0.005	-0.077	-0.011	-0.096	-0.037
	(0.069)	(0.051)	(0.053)	(0.070)	(0.053)	(0.073)	(0.056)
State fixed effects	No	No	Yes	No	Yes	No	Yes
More Geo. controls	No	No	No	Yes	Yes	Yes	Yes
Firm and job controls	No	No	No	No	No	Yes	Yes
Constant	-0.913***	-0.794***	-0.945	-0.811***	0.502	-1.216***	-0.237
	(0.242)	(0.189)	(0.591)	(0.269)	(0.858)	(0.316)	(0.913)
Observations	2082	2048	2011	2082	2010	2009	1939

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Numbers reported are probit coefficients (standard errors in parentheses). This table is otherwise identical to Table IV. See notes to Table IV.

Appendix Table A6.
Field Experiment: OLS Regressions (with clustered errors) in the Christian-Muslim Manipulation. Dependent Variable Is Callbacks

	(1) State	(2) County	(3) County	(4) State	(5) County	(6) State	(7) County
Muslim condition	-0.185***	-0.155**	-0.153**	-0.187***	-0.176**	-0.171***	-0.140*
	(0.047)	(0.068)	(0.070)	(0.045)	(0.070)	(0.050)	(0.071)
Politically mixed state or county	-0.050	-0.094	-0.116	-0.065*	-0.111	-0.040	-0.077
	(0.035)	(0.066)	(0.071)	(0.035)	(0.075)	(0.034)	(0.072)
Democratic state or county	-0.066	-0.096	-0.133*	-0.098*	-0.121	-0.068	-0.098
	(0.053)	(0.068)	(0.074)	(0.049)	(0.082)	(0.048)	(0.078)
Percentage Foreign-born	-0.327	0.002	0.142	-0.495*	0.155	-0.615**	0.102
	(0.219)	(0.124)	(0.152)	(0.289)	(0.178)	(0.287)	(0.158)
Median income (ln)	0.146	-0.029	-0.114*	0.363*	-0.081	0.310	-0.108
	(0.095)	(0.041)	(0.062)	(0.190)	(0.084)	(0.192)	(0.089)
Unemployment rate	0.010	-0.004	-0.017	0.017**	-0.007	0.015*	-0.002
	(0.008)	(0.009)	(0.011)	(0.008)	(0.012)	(0.008)	(0.013)
Muslim*Politically mixed state or county	0.153***	0.130*	0.129*	0.153***	0.153**	0.141***	0.111
	(0.044)	(0.070)	(0.073)	(0.043)	(0.073)	(0.046)	(0.075)
Muslim*Democratic state or county	0.206***	0.160**	0.160**	0.209***	0.182**	0.183**	0.145*
	(0.066)	(0.072)	(0.075)	(0.061)	(0.076)	(0.070)	(0.076)
Muslim*Percentage foreign-born	0.150	0.058	0.051	0.169	0.073	0.233	0.126
	(0.276)	(0.163)	(0.162)	(0.276)	(0.160)	(0.269)	(0.159)
Muslim* median income (ln)	-0.161	-0.037	-0.057	-0.194	-0.056	-0.196	-0.089
	(0.117)	(0.068)	(0.070)	(0.116)	(0.070)	(0.126)	(0.063)
Muslim* unemployment	-0.014	-0.002	-0.003	-0.016	-0.004	-0.019*	-0.007
	(0.011)	(0.010)	(0.011)	(0.011)	(0.010)	(0.011)	(0.011)
State fixed effects	No	No	Yes	No	Yes	No	Yes
More Geo. controls	No	No	No	Yes	Yes	Yes	Yes
Firm and job controls	No	No	No	No	No	Yes	Yes
Constant	0.181***	0.216***	0.245***	0.204***	0.578***	0.148***	0.488**
	(0.037)	(0.066)	(0.069)	(0.040)	(0.207)	(0.043)	(0.226)
Observations	2082	2048	2048	2082	2046	2009	1974
R^2	0.005	0.006	0.030	0.009	0.035	0.043	0.069

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Numbers reported are OLS coefficients (clustered standard errors in parentheses). Cluster level is at the geographical level indicated in the column headings. This table is otherwise identical to Table IV. See notes to Table IV.

Appendix Table A7.
Field Experiment: Lists of States Used in Measures of Most Republican and Most Democratic States

(1) RomneyVote List: states categorized by % Romney vote in the 2012 U.S. Presidential election	(2) Gallup List: states categorized by the Gallup Organization using Daily tracking data on political party identification from Jan. 1 – Dec. 31 of 2012	(3) Combined List: Republican and Democratic states are the union of Republican and Democratic states in columns (1) and (2)
Panel A: Three lists of the most Republican states resulting from different definitions		
AL	AL	AL
ID	ID	ID
KS	KS	KS
NE	NE	NE
OK	OK	OK
UT	UT	UT
WY	WY	WY
AR	AK	AK
KY	MT	MT
WV	ND	ND
		AR
		KY
		WV
Panel B: Three lists of the most Democratic states resulting from different definitions		
DC	DC	DC
DE	DE	DE
HI	HI	HI
MA	MA	MA
MD	MD	MD
NY	NY	NY
RI	RI	RI
VT	VT	VT
CA	CT	CT
NJ	IL	IL
		CA
		NJ

We construct the lists shown in column (1) from final official state-level voting results posted by states and compiled by Leip (2013). The Gallup lists shown in column (2) are the ten states with the weakest and strongest Democratic advantage, defined as the percentage of subjects who identify as Democratic minus the percentage who identify as Republican (Saad 2013).

Appendix Table A8.
Field Experiment: OLS Regressions in the Christian-Muslim Manipulation. Dependent Variable is Callbacks. Geographical Control Variables Are at the State Level and Include Alternative Measures of Republican, Politically Mixed, and Democratic State Groups

	(1)	(2)	(3)	(4)	(5)	(6)
Muslim condition	-0.193***	-0.221***	-0.200***	-0.246***	-0.232***	-0.207***
	(0.071)	(0.072)	(0.072)	(0.071)	(0.071)	(0.073)
Politically mixed state	-0.085	-0.081	-0.063	-0.092	-0.115*	-0.089
	(0.066)	(0.058)	(0.057)	(0.058)	(0.061)	(0.062)
Democratic state	-0.092	-0.107	-0.081	-0.145*	-0.206**	-0.162*
	(0.068)	(0.078)	(0.076)	(0.075)	(0.091)	(0.094)
Percentage Foreign-born	-0.394*	-0.332	-0.312	-0.293	-0.523*	-0.644**
	(0.227)	(0.230)	(0.230)	(0.229)	(0.297)	(0.300)
Median income (ln)	0.132	0.186	0.145	0.250*	0.495**	0.417*
	(0.112)	(0.141)	(0.138)	(0.137)	(0.236)	(0.241)
Unemployment rate	0.012	0.014	0.011	0.019*	0.028**	0.024*
	(0.010)	(0.012)	(0.012)	(0.011)	(0.013)	(0.013)
Muslim*Politically mixed state	0.180**	0.186***	0.168***	0.199***	0.192***	0.177***
	(0.074)	(0.065)	(0.065)	(0.065)	(0.065)	(0.066)
Muslim*Democratic state	0.193**	0.243***	0.217**	0.285***	0.261***	0.219**
	(0.078)	(0.092)	(0.092)	(0.091)	(0.091)	(0.093)
Muslim*Percentage foreign-born	0.343	0.209	0.188	0.164	0.220	0.289
	(0.306)	(0.319)	(0.320)	(0.319)	(0.323)	(0.330)
Muslim* median income (ln)	-0.089	-0.205	-0.163	-0.275	-0.259	-0.226
	(0.150)	(0.179)	(0.179)	(0.176)	(0.178)	(0.179)
Muslim* unemployment	-0.015	-0.020	-0.017	-0.026*	-0.024	-0.024
	(0.014)	(0.015)	(0.015)	(0.015)	(0.015)	(0.015)
States dropped from sample?	None	None	AK	VT	None	None
More Geo. controls	No	No	No	No	Yes	Yes
Firm and job controls	No	No	No	No	No	Yes
Constant	0.210***	0.216***	0.195***	0.238***	0.274***	0.212***
	(0.064)	(0.064)	(0.062)	(0.063)	(0.070)	(0.075)
Observations	2082	2082	2079	2077	2082	2009
R^2	0.005	0.006	0.005	0.007	0.011	0.045

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Callbacks = 1 if the employer contacted the candidate for an interview, zero if there was no invitation from a complete application. Numbers reported are OLS coefficients (robust standard errors in parentheses). Column 1 uses state political grouping from Gallup list shown in column (2) of Table A7. Columns (2)-(6) use state political grouping from combined list shown in column (3) of Table A7. Additional state-level controls in column (5): state-level Facebook penetration, legal protection from religious discrimination, % non-white, % college educated or more, % Evangelical Protestant, % Islamic, and % urban. Firm-level controls in column (6): dummies for women and minority owned, public firm, large firm (with 500 employees or more), federal contractor. Job controls in column (6) are 9 dummies for field of employment and dummies for entry level position, references required, preferred salary required, master's degree required, one or more years of experience required, and multiple fields of experience required. The continuous state and county variables – namely, Facebook penetration,

unemployment, percentage non-white, income (ln), % foreign-born, % college educated or more, % Evangelical Protestant, % Islamic, and % urban population – are centered on their means. The omitted categories for dummies capturing variables with more than two categories are: Republican states or counties, counties with less than 20,000 people, and jobs in information systems. The remaining variables are binary.

Appendix Table A9
OLS regressions of callbacks in the Christian-Muslim manipulation. All regressions use county-level demographic data.

	(1)	(2)	(3)	(4)
Muslim condition	-0.148**	-0.143**	-0.164**	-0.128**
	(0.061)	(0.065)	(0.066)	(0.065)
Politically mixed state or county	-0.095*	-0.101*	-0.106*	-0.072
	(0.055)	(0.060)	(0.064)	(0.063)
Democratic state or county	-0.095*	-0.090	-0.112	-0.088
	(0.054)	(0.060)	(0.069)	(0.068)
Percentage Islamic	-0.161	-0.285**	-0.278**	-0.260**
	(0.100)	(0.114)	(0.119)	(0.122)
Muslim*Politically mixed state or county	0.128**	0.123*	0.147**	0.104
	(0.065)	(0.069)	(0.070)	(0.069)
Muslim*Democratic state or county	0.149**	0.145**	0.165**	0.129*
	(0.064)	(0.068)	(0.069)	(0.068)
Muslim*Percentage Islamic	0.317*	0.371**	0.367**	0.383**
	(0.177)	(0.179)	(0.181)	(0.183)
State fixed effects?	No	Yes	Yes	Yes
Constant	0.216***	0.215***	0.569***	0.477**
	(0.052)	(0.058)	(0.182)	(0.192)
Observations	2057	2057	2046	1974
R^2	0.006	0.030	0.036	0.070

* p < 0.10, ** p < 0.05, *** p < 0.01. Callbacks = 1 if the employer contacted the candidate for an interview, zero if there was no invitation from a complete application. Numbers reported are OLS coefficients (robust standard errors in parentheses). Additional county-level controls in columns (3) and (4) are:% foreign-born, median income (ln), unemployment rate, % non-white, % college educated or more, % Evangelical Protestant, and rural-urban continuum code dummies. Firm-level controls in column (4) are: dummies for women and minority owned, public firm, large firm (with 500 employees or more), and federal contractor. Job controls in column (4) are 9 dummies for field of employment and dummies for entry level position, references required, preferred salary required, master's degree required, one or more years of experience required, and multiple fields of experience required. The continuous county variables – namely, % foreign-born, median income (ln), unemployment rate, % non-white, % college educated or more, % Evangelical Protestant, and % Islamic – are centered on their means. The omitted categories for dummies capturing variables with more than two categories are: Republican states or counties; counties with less than 20,000 people; and jobs in information systems. The remaining variables are binary.

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TABLES AND FIGURES

Table Ia
Online Experiment: Hypothetical Callback Ratios and Perceived Employability among Survey Subjects

	Hypothetical interview invitation		Employability scale[a]	
PANEL A: Religion manipulation				
	Christian	Muslim	Christian	Muslim
Invitation rate	93.68%	93.94%	0.046	-0.044
N	190	198	188	197
	Two-sided Fisher's exact p-value: 1.000		Two-sided t-test p-value: 0.379	
PANEL B: Sexual orientation manipulation				
	Straight	Gay	Straight	Gay
Invitation rate	94.95%	94.57%	-0.052	0.056
N	198	184	197	184
	Two-sided Fisher's exact p-value: 1.000		Two-sided t-test p-value: 0.294	

[a]The employability scale is the first principal component (from principal component analysis) of our four questions on perceived employability, with questions signed to increase in perceptions that the candidate is a good hire, and standardized to s.d. = 1, mean=0. The scale is computed and standardized separately for each manipulation.

Table Ib
Online Experiment: Hypothetical Callback Ratios and Perceived Employability among Survey Subjects with Hiring Experience

	Hypothetical interview invitation		Employability scale[a]	
PANEL A: Religion manipulation				
	Christian	Muslim	Christian	Muslim
Invitation rate	96.15	90.53	0.063	-0.230
N	104	95	104	95
	Two-sided Fisher's exact p-value: 0.151		Two-sided t-test p-value: 0.042	
PANEL B: Sexual orientation manipulation				
	Straight	Gay	Straight	Gay
Invitation rate	95.60	94.19	-0.135	-0.014
N	91	86	91	86
	Two-sided Fisher's exact p-value: 0.742		Two-sided t-test p-value: 0.413	

[a]See notes to Table Ia.

Table II
Online Experiment: OLS Regressions in Religion Manipulation, Sexual Orientation Manipulation and Pooled Sample. Dependent Variable is Perceived Employability.

	(1) Religion manipulation	(2) Sexual orientation manipulation	(3) Pooled Sample
Muslim and/or gay treatment	-0.875**	-0.417	-0.563**
	(0.408)	(0.279)	(0.224)
Treatment*No hiring Experience	0.379	0.044	0.271
	(0.237)	(0.280)	(0.166)
Treatment*Foreign-born	1.101*		0.441
	(0.623)		(0.405)
Treatment*Y≤\$20k	0.698		
	(0.440)		
Treatment*\$20k<Y≤\$45k	0.281		
	(0.402)		
Treatment*\$45k<Y≤\$70k	0.400		
	(0.462)		
Treatment*\$70k<Y≤\$100k	1.006**		
	(0.461)		
Treatment*Non-white		0.885**	0.419
		(0.400)	(0.310)
Treatment*under 21 yrs old		0.692	
		(0.497)	
Treatment*21-30 yrs old		0.407	
		(0.359)	
Treatment*31-40 yrs old		0.351	
		(0.397)	
Treatment*41-50 yrs old		0.046	
		(0.479)	

Table II
(continued)

	(1) Religion manipulation	(2) Sexual orientation manipulation	(3) Pooled Sample
Treatment*51-60 yrs old		0.839	
		(0.523)	
Treatment*Independent			0.424*
			(0.237)
Treatment*Democrat			0.422*
			(0.240)
Treatment*Unemployed			-0.454*
			(0.239)
Additional controls?	Yes	Yes	Yes
Constant	0.049	0.052	-0.052
	(0.538)	(0.356)	(0.340)
Observations	283	271	554
R^2	0.149	0.092	0.082

* p < 0.10, ** p < 0.05, *** p < 0.01. Numbers reported are OLS coefficients (robust standard errors in parentheses). The employability scale is the first principal component of our four questions on perceived employability, with questions signed to increase in perceptions that the candidate is a good hire, and standardized to s.d. = 1, mean=0. The scale is computed and standardized separately for each manipulation. Additional controls are dummies for hiring experience, political independent, Democrat, foreign-born, non-Christian, no college degree, female, non-white, unemployed, four income categories, and five age categories. Omitted categories for dummies capturing variables with more than two values are: Republican; income > $100,000; and over 60 years of age.

Table III
Field Experiment: Callback Ratios from Employers

PANEL A: Religion manipulation		
	Christian male	Muslim male
Invitation rate	12.64%	10.96%
N	1060	1022
Two-sided Fisher's exact p-value	0.249	

PANEL B: Sexual orientation manipulation		
	Straight male	Gay male
Invitation rate	10.63%	10.69%
N	1025	1066
Two-sided Fisher's exact p-value	1.000	

Table IV
Field Experiment: OLS Regressions in the Christian-Muslim Manipulation. Dependent Variable is Callbacks. Column Headings Refer to Whether Geographical Characteristics Are Measured at the State or County Level

	(1) State	(2) County	(3) County	(4) State	(5) County	(6) State	(7) County
Muslim condition	-0.185***	-0.155**	-0.153**	-0.187***	-0.176**	-0.171**	-0.140**
	(0.070)	(0.064)	(0.068)	(0.069)	(0.069)	(0.071)	(0.068)
Politically mixed state or county	-0.050	-0.094*	-0.116*	-0.065	-0.111*	-0.040	-0.077
	(0.057)	(0.056)	(0.061)	(0.059)	(0.064)	(0.060)	(0.063)
Democratic state or county	-0.066	-0.096*	-0.133**	-0.098	-0.121*	-0.068	-0.098
	(0.075)	(0.057)	(0.063)	(0.080)	(0.070)	(0.083)	(0.069)
Percentage Foreign-Born	-0.327	0.002	0.142	-0.495	0.155	-0.615*	0.102
	(0.249)	(0.142)	(0.165)	(0.318)	(0.188)	(0.320)	(0.185)
Median income (ln)	0.146	-0.029	-0.114*	0.363*	-0.081	0.310	-0.108
	(0.127)	(0.055)	(0.068)	(0.220)	(0.083)	(0.225)	(0.083)
Unemployment rate	0.010	-0.004	-0.017*	0.017	-0.007	0.015	-0.002
	(0.011)	(0.008)	(0.010)	(0.012)	(0.011)	(0.012)	(0.011)
Muslim*Politically mixed state or county	0.153**	0.130*	0.129*	0.153**	0.153**	0.141**	0.111
	(0.065)	(0.067)	(0.071)	(0.065)	(0.072)	(0.067)	(0.071)
Muslim*Democratic state or county	0.206**	0.160**	0.160**	0.209**	0.182**	0.183**	0.145**
	(0.091)	(0.068)	(0.073)	(0.089)	(0.074)	(0.092)	(0.073)
Muslim*Percentage foreign-born	0.150	0.058	0.051	0.169	0.073	0.233	0.126
	(0.347)	(0.193)	(0.197)	(0.349)	(0.197)	(0.356)	(0.195)
Muslim* median income (ln)	-0.161	-0.037	-0.057	-0.194	-0.056	-0.196	-0.089
	(0.162)	(0.076)	(0.079)	(0.162)	(0.079)	(0.163)	(0.078)
Muslim* unemployment rate	-0.014	-0.002	-0.003	-0.016	-0.004	-0.019	-0.007
	(0.014)	(0.010)	(0.010)	(0.014)	(0.010)	(0.014)	(0.010)
State fixed effects	N/A	No	Yes	N/A	Yes	N/A	Yes
More Geo. controls	No	No	No	Yes	Yes	Yes	Yes
Firm and job controls	No	No	No	No	No	Yes	Yes
Constant	0.181***	0.216***	0.245***	0.204***	0.578***	0.148**	0.488**
	(0.061)	(0.054)	(0.060)	(0.065)	(0.181)	(0.071)	(0.192)
Observations	2082	2048	2048	2082	2046	2009	1974
R^2	0.005	0.006	0.030	0.009	0.035	0.043	0.069

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Callbacks = 1 if the employer contacted the candidate for an interview, zero if there was no invitation from a complete application. Numbers reported are OLS coefficients (robust standard errors in parentheses). The additional state-level controls in columns (4) and (6) are: state-level Facebook penetration, legal protection from religious discrimination, % non-white, % college educated or more, % Evangelical Protestant, % Islamic, and % urban. Additional county-level controls in columns (5) and (7) are: % non-white, % college educated or more, % Evangelical Protestant, % Islamic, and rural-urban continuum code dummies. Firm-level controls in columns (6) and (7) are: dummies for women and minority owned, public firm, large firm (with 500 employees or more), and federal contractor. Job controls in columns (6) and (7) are 9 dummies for field of employment and dummies for entry level position, references required, preferred salary required, master's degree required, one or more years of experience required, and multiple fields of experience required. The continuous state and county variables – namely, Facebook penetration, unemployment, percentage non-white, income (ln), % foreign-born, % college educated or more, % Evangelical Protestant, % Islamic, and % urban population – are centered on their means. The omitted categories for dummies capturing variables with more than two categories are: Republican states or counties; counties with less than 20,000 people; and jobs in information systems. The remaining variables are binary.

Table V
Field Experiment: OLS Regressions in Gay-Straight Manipulation.
Dependent Variable is Callbacks. Column Headings Refer to Whether Geographical Characteristics Are Measured at the State or County Level

	(1) State	(2) County	(3) County	(4) State	(5) County	(6) State	(7) County
Gay condition	-0.001	0.010	-0.005	-0.002	-0.009	-0.007	-0.040
	(0.080)	(0.063)	(0.067)	(0.080)	(0.067)	(0.084)	(0.069)
Politically mixed state or county	-0.039	0.030	0.034	-0.025	0.003	-0.037	-0.015
	(0.061)	(0.044)	(0.049)	(0.064)	(0.049)	(0.068)	(0.052)
Democratic state or county	-0.011	0.021	0.039	0.035	-0.015	0.032	-0.032
	(0.067)	(0.047)	(0.054)	(0.082)	(0.057)	(0.086)	(0.060)
Unemployment rate	-0.010	-0.009	-0.006	-0.014	-0.017*	-0.016	-0.016
	(0.008)	(0.006)	(0.007)	(0.010)	(0.010)	(0.010)	(0.011)
Percentage non-white	-0.003	0.053	0.019	0.083	0.278**	0.087	0.223
	(0.136)	(0.087)	(0.112)	(0.206)	(0.139)	(0.208)	(0.142)
Gay*Politically mixed state or county	-0.000	-0.033	-0.022	0.001	-0.017	0.012	0.010
	(0.080)	(0.064)	(0.069)	(0.080)	(0.068)	(0.085)	(0.070)
Gay*Democratic state or county	0.002	0.007	0.024	0.003	0.030	-0.005	0.062
	(0.087)	(0.068)	(0.072)	(0.088)	(0.072)	(0.092)	(0.074)
Gay*Unemployment rate	0.007	0.006	0.007	0.008	0.009	0.009	0.009
	(0.011)	(0.009)	(0.009)	(0.011)	(0.009)	(0.011)	(0.010)
Gay*Percentage non-white	-0.042	-0.082	-0.097	-0.049	-0.108	-0.039	-0.105
	(0.173)	(0.126)	(0.128)	(0.179)	(0.128)	(0.182)	(0.130)
State fixed effects	No	No	Yes	No	Yes	No	Yes
More Geo. controls	No	No	No	Yes	Yes	Yes	Yes
Firm and job controls	No	No	No	No	No	Yes	Yes
Constant	0.132**	0.083*	0.070	0.113*	0.257*	0.090	0.252*
	(0.061)	(0.043)	(0.049)	(0.065)	(0.132)	(0.074)	(0.146)
Observations	2091	2061	2061	2091	2061	2011	1983
R^2	0.003	0.002	0.021	0.006	0.030	0.035	0.060

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Callbacks = 1 if the employer contacted the candidate for an interview, zero if there was no invitation from a complete application. Numbers reported are OLS coefficients (robust standard errors in parentheses). The additional included geographical controls in columns (4) and (6) are: state-level Facebook penetration, state anti-discrimination policies for sexual orientation, % foreign-born, natural log of median income, % college educated or more, % Evangelical Protestant, % Islamic, and % urban. The additional geographical controls in columns (5) and (7) are county level % foreign-born, natural log of median income, % college educated or more, %

Evangelical Protestant, % Islamic, and rural-urban continuum code dummies. In columns (6) and (7), firm-level controls are dummies for women and minority owned, public firm, large firm (with 500 employees or more), federal contractor. Job controls in columns (6) and (7) are 9 dummies for field of employment, dummies for entry level position, references required, preferred salary required, master's degree required, one or more years of experience required, and multiple fields of experience required. The continuous state and county variables – namely, Facebook penetration, unemployment, percentage non-white, income (ln), % foreign-born, % college educated or more, % Evangelical Protestant, % Islamic, and % urban population – are centered on their means. The omitted categories for dummies capturing variables with more than two categories are: Republican states or counties; counties with less than 20,000 people; and jobs in information systems. State anti-discrimination policies for sexual orientation is 0, 1, or 2 for levels of protection that are low, medium, and high, respectively (see Appendix Table A4 notes for sources and coding details). The remaining variables are binary.

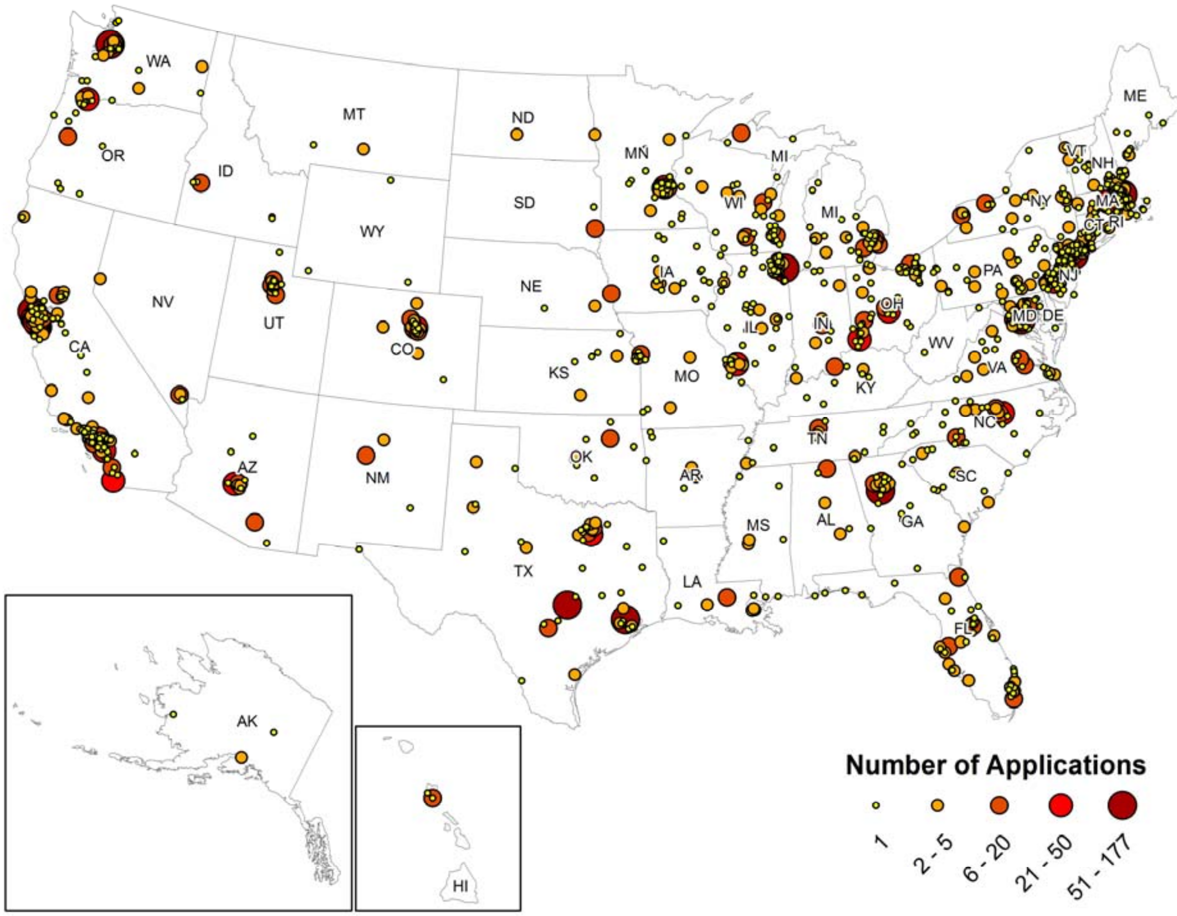

Figure I

Field Experiment: Geographical Distribution of Applications across the United States

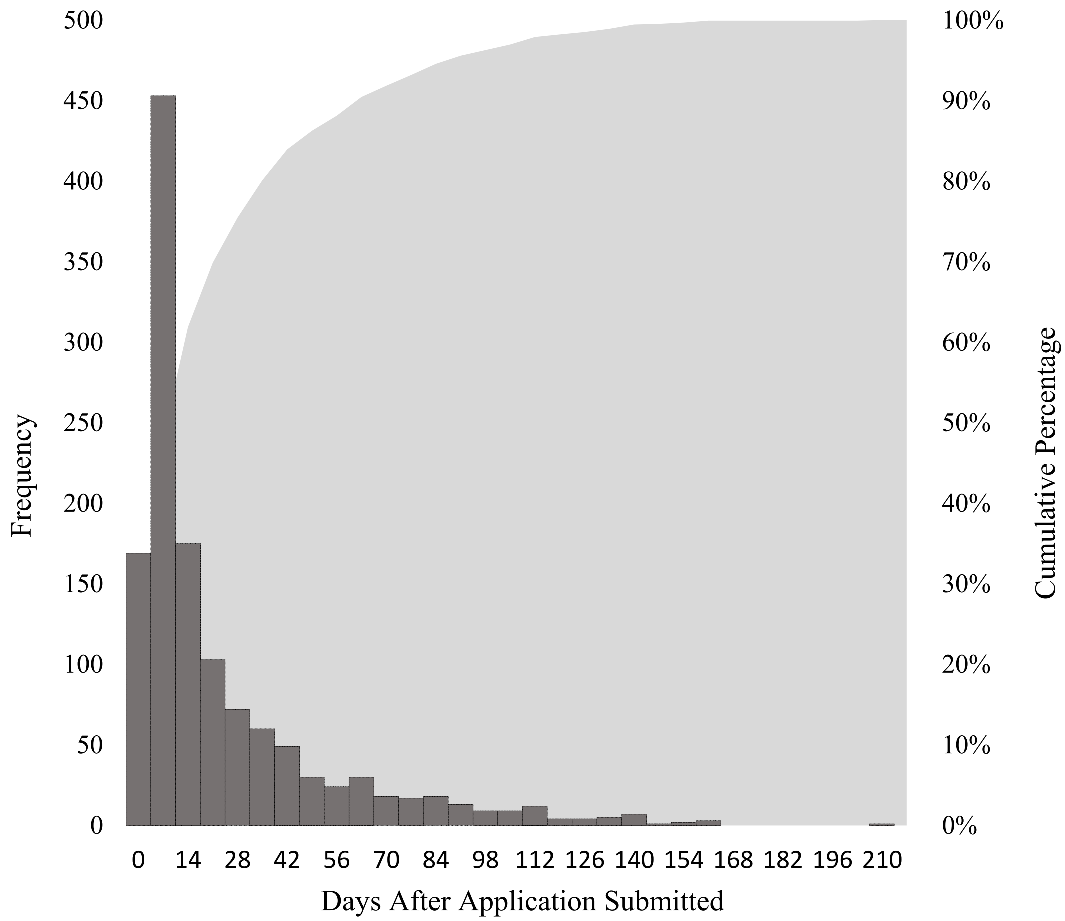

Figure II

Field Experiment: Distribution of Employers' Responses over Time

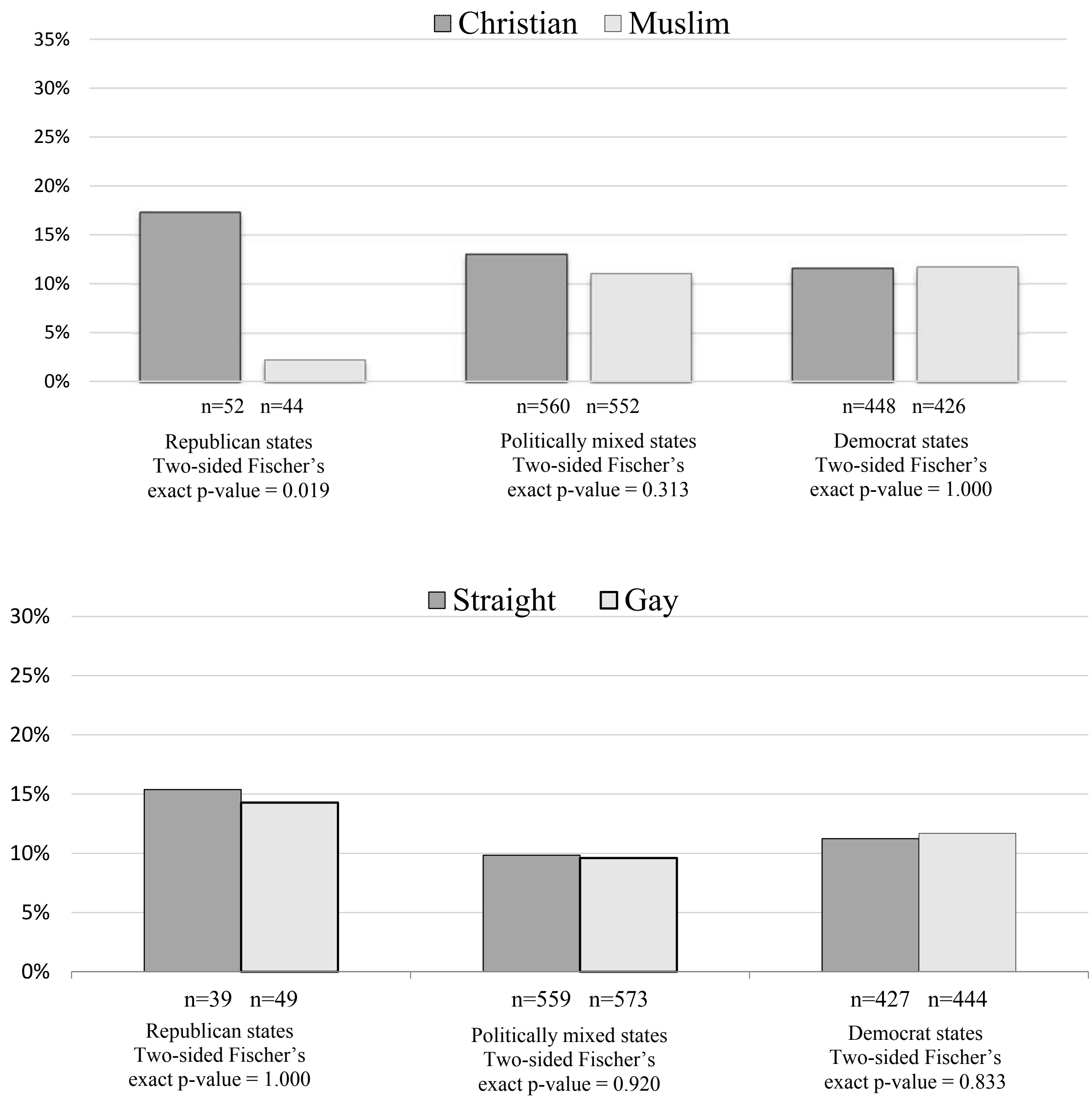

Figure III

Field Experiment: Callback Rates by Political Leaning of the State