

Countervailing Claims: Pro-Diversity Responses to Stigma by Association Following the Unite the Right Rally

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Abstract

Why do firms position on divisive social issues? I draw on theories of stigma by association to explain why firms' mere proximity with controversial political actors may lead stakeholders to presume firms silent on social issues are misaligned with their sociopolitical preferences. These firms, in turn, countervail these presumptions of misalignment by eschewing silence and claiming sociopolitical positions. I substantiate this theory in the context of employee recruitment following the 2017 Unite the Right white supremacist rally in Charlottesville, Virginia. I show that Charlottesville's employers combated presumptions that they shared demonstrators' *anti*-diversity positions by making countervailing *pro*-diversity claims in their online job postings. In supplementary analysis, I show the rally was associated with a newfound wage premium among Charlottesville's employers, but that this premium was lower in instances where employers made pro-diversity claims. This paper advances the idea of strategic sociopolitical positioning, whereby firms make calculated appeals to stakeholders.

Keywords: stakeholders, recruiting, labor markets, organizational theory, corporate social responsibility, diversity

1 Introduction

Why do firms position on divisive social issues? Despite some evidence that stakeholders reward firms that take positions aligned with their preferences (Chatterji and Toffel 2019, Wowak et al. 2022), growing research suggests firms face significant, offsetting motivations to stay silent. Not only does silence preclude accusations of hypocrisy (Wagner et al. 2009, Janney and Gove 2011, Carlos and Lewis 2018) and unwanted pressure from activists (Vogel 2005, Baron and Diermeier 2007), but it may even cultivate ‘false consensus,’ wherein stakeholders with different preferences presume silent firms align with their respective preferences (Burbano 2021, Hou and Poliquin 2022). In this telling, presumptions of sociopolitical alignment mean sociopolitical claims do little to enhance stakeholder perceptions that firms are aligned with their preferences while eliciting negative reactions from those misaligned with the stance. Closely related research in political science similarly demonstrates how silence can forestall scrutiny while allowing candidates to build larger, more diverse coalitions (Conover and Feldman 1982, Tomz and Van Houweling 2009, Hurst 2022).

But while this pessimistic perspective on the benefits of sociopolitical positioning largely supports “conventional wisdom” (Hambrick and Wowak 2021:p.33) that firms fare better when they remain silent (Friedman 1970, Hadani and Schuler 2013, Bhagwat et al. 2020), it raises questions as to *why* firms often fail to do so (Larcker et al. 2018). For example, in the wake of Texas’s Senate Bill 8, which codified a strongly pro-life stance on abortion, many Texas-based firms took highly-publicized, pro-choice positions (Paredes 2021). In the wake of Donald Trump’s victory in the 2016 presidential election, many U.S. firms made commitments to racial, gender, and LGBTQ+ diversity (In the Trump Era 2017). And, as I will examine in detail in this paper, in the wake of a large scale white supremacist rally in Charlottesville, Virginia in 2017, many employers in Charlottesville added pro-diversity claims to their job postings. When are these and other instances of firms trading silence for stance taking mostly a manifestation of the values of top managers or the result of imitative processes among peer firms (Marquis 2003, Briscoe et al. 2014, 2015)? And when, in contrast, do they reflect calculated, strategic appeals to stakeholders (Mohliver et al. 2021, Melloni et al. 2023)?

I draw on theories of stigma by association (Jonsson et al. 2009, Pontikes et al. 2010, McDonnell et al. 2021) to explain one reason why firms strategically position on sociopolitical issues. Central

to my theory is the notion that that stakeholders frequently generalize the positions of non-firm political actors (such as voters, politicians, legislatures, courts, or protesters) to firms they associate with these actors, often due merely to the fact that these political actors and firms are physically proximate. Among stakeholders whose preferences do not align with these actors’ positions, this generalization engenders “sociopolitical stigma,” which, drawing on Devers et al.(2009), I define as stakeholders’ negative evaluations of a firm due to a perceived lack of alignment between this firm and stakeholders’ preferred sociopolitical positions. Sociopolitical stigma by association undermines the feasibility of maintaining false consensus through silence by replacing stakeholders’ optimistic presumptions of sociopolitical alignment with pessimistic presumptions of sociopolitical *misalignment*.

Accordingly, firms associated with non-firm political actors that are viewed unfavorably by their stakeholders face incentives to eschew silence and combat this stigma by claiming sociopolitical positions aligned with their stakeholders’ preferences. Drawing on Werner’s (2012, p. 23-24) theory of private politics as defensive, strategic maneuvering, I propose that firms respond to these incentives by making “countervailing claims,” meaning sociopolitical claims meant to offset or counter the negative effects of stigma by association. In my telling firms take sociopolitical positions not to offset negative evaluations arising from their own actions (Elsbach 1994, Bansal and Clelland 2004, McDonnell and King 2013) or to counterposition vis-à-vis rival firms (Mohliver et al. 2021), but rather to distance themselves from controversial *non*-firm political actors with whom they are associated.

I substantiate this theory in the context of employee recruitment following the 2017 Unite the Right white supremacist rally in Charlottesville, Virginia. Using a dataset of 66.5 million online jobs postings, I show that in the two years following the rally, employers in Charlottesville significantly increased their use of pro-diversity claims in their job postings. Consistent with my theory, I show that the adoption of pro-diversity claims was not exceptionally widespread, but was most pronounced in instances where prospective applicants were plausibly (1) more inclined to generalize the white supremacists’ anti-diversity positions to Charlottesville’s employers and (2) held stronger pro-diversity preferences. In supplementary analysis, I show the rally was associated with a newfound wage premium, but that this premium was lower in instances where employers made pro-diversity claims. I motivate and supplement my quantitative analysis with qualitative

evidence from memoirs, media reports, and twenty-one original field interviews.

2 Theory

Firms increasingly position on divisive social issues. This positioning is distinct from the “pro-social” claim-making long-examined in the impression management literature inasmuch as it involves positions that are contested rather than “socially acceptable” (McDonnell and King 2013:p.388). Although nascent research in this area has tended to examine CEO activism (Chatterji and Toffel 2019, Hambrick and Wowak 2021, Wowak et al. 2022, Hou and Poliquin 2022), firms’ sociopolitical positioning is often not easily attributable to any single organizational member. Moreover, while some work narrowly considers position-taking with respect to issues unrelated to a firm’s business, in many cases a firm’s sociopolitical positioning *does* relate to business, including outdoor recreation companies taking pro-environmental positions, employers taking positions regarding their employees’ reproductive rights, or, more generally, firms with diverse workforces proclaiming the value of diversity (Larcker et al. 2018, Mohliver and Hawn 2019, Pardes 2021). In contrast with corporate social responsibility or other forms of non-market strategy, the distinguishing characteristic of sociopolitical positioning is that it can be achieved with little expenditure of company resources (Wowak et al. 2022:p.554). This is not to say that sociopolitical positioning cannot be complemented with more costly, substantive actions, but rather that the act of claiming a position is, in itself, a theoretical construct of interest. In making this distinction, organizational scholars mirror a long tradition in political science that treats positioning as theoretically distinct from actual policies undertaken to pursue these positions (Dewan and Shepsle 2011).

2.1 Stakeholder Responses to Sociopolitical Positioning

With little exception, emergent scholarly attention to firms’ sociopolitical positioning examines whether these actions benefit firms. Highlighting upsides, some work emphasizes that positioning requires little expenditure of firm resources, while increasing the extent to which stakeholders aligned with the espoused position reward the firm. On the first point, compared to more resource-intensive non-market strategies, such as self-regulation (Short and Toffel 2010), political donations (Werner 2017), corporate volunteer programs (Carnahan et al. 2017) or philanthropy (Luo et al. 2018), firms can position via simple statements in social media posts, television ads, or, as I will show

in this paper, job postings (Khosrowshahi 2020, Wowak et al. 2022). On the second point, workers (Carpenter and Gong 2015, McConnell et al. 2018, Wowak et al. 2022), consumers (Chatterji and Toffel 2019, Panagopoulos et al. 2020), and even investors (Bolton et al. 2020, Mohliver and Hawn 2019), may be more inclined to reward firms that take positions aligned with their preferences.

A separate set of studies has illustrated significant, offsetting downsides of sociopolitical positioning. This work emphasizes how positioning may incur unwanted public scrutiny as well as asymmetrically negative reactions from stakeholders misaligned with the espoused position. On the first point, Baron and Diermeier (2007) as well as Vogel (2005:p. 54-55) suggest that stance-taking attracts activists in favor of the claimed position who perceive the firm taking the position as sympathetic to their cause and consequently less likely to resist pressure to pursue costly actions consistent with the position. This downside was substantiated in the case of Starbucks, whose pro-environmental positioning drew attention from activists who in turn successfully pressured the firm to embrace costly changes to their coffee sourcing procedures (Argenti 2004). When scrutiny is increased, there is also an increased risk that a firm will be seen as hypocritical, which can significantly reduce stakeholder support (Vogel 2005, Wagner et al. 2009, Janney and Gove 2011). For example, many organizations that stated support for Black Lives Matter in the summer of 2020, including J.P. Morgan Chase (Jan et al. 2020), L’Oréal (Elan 2020), and the United Kingdom’s Royal Academy of Drama and Art (Bakare 2020), were subsequently accused of hypocrisy for actions inconsistent with these claims. Silence on sociopolitical issues prevents these accusations and affords latitude for actions that might otherwise be deemed hypocritical (Carlos and Lewis 2018).

On the second point, a growing body of evidence suggests that when a firm claims a sociopolitical position it elicits asymmetrically negative reactions from stakeholders misaligned with the position. Examining consumer reactions to corporate support for gun control in the wake of mass shootings, Hou and Poliquin (2022) demonstrate asymmetrically negative responses from consumers that oppose gun control. Similarly, Burbano (2021) presents evidence that taking a position on North Carolina’s trans-exclusionary bathroom bill failed to increase output among workers aligned with the position, but significantly reduced output among workers misaligned with the position. Burbano proposes these findings may be driven by false consensus bias, which is the tendency to overestimate the extent to which the unknown preferences of others match one’s own (Rosenhan and Messick 1966, Ross et al. 1977). These findings are also consistent with theories of negativity bias

and loss aversion, in which bad outcomes, in this case sociopolitical misalignment, have stronger behavioral implications than good outcomes, in this case sociopolitical alignment (Kahneman et al. 1991, Baumeister et al. 2001, Rozin and Royzman 2001). These findings suggest that claiming a sociopolitical position does little to improve perceptions of alignment among those aligned with this position, while eliciting out-sized negative reactions among those misaligned with the position.

The idea that claiming a sociopolitical position undermines stakeholders’ optimistic presumptions that the firm aligns with their preferences is consistent with related research in political science. By avoiding clear policy stances, a political candidate can often attract a wider range of voters since these voters presume the candidates’ positions align with their own, different preferences (Markus and Converse 1979, Conover and Feldman 1982, Tomz and Van Houweling 2009). Inasmuch as candidates take well-defined positions, they are incentivized to do so on only the few core issues on which voters expect to see a position taken (Krosnick 1990). This logic is especially germane to firms. If a firm fails to communicate quarterly earnings, the goods it sells, the salary it pays, or other information most core to business, this silence will unavoidably elicit negative attention from investors, customers, and employees respectively. Silence on divisive issues, however, may deflect unwanted scrutiny while leaving room for stakeholders to make optimistic presumptions regarding the firm’s stances on these issues. These strong motivations to remain silent perhaps explain why firms remain silent on the vast majority of social issues. They also raise questions regarding when, if ever, decisions to eschew silence and claim positions on these issues are strategic. They suggest that inasmuch as firms position on sociopolitical issues this action may simply reflect the values of top managers (Hambrick and Wowak 2021) or results from imitative processes among peer firms (Marquis 2003).

2.2 Combatting Sociopolitical Stigma with Countervailing Claims

I, however, propose conditions under which firms face strong incentives to eschew silence and strategically claim positions on divisive issues. My argument builds on Burbano’s (2021) observation that stakeholders make presumptions regarding the positions of silent firms. But whereas Burbano points only to *optimistic* presumptions arising from false consensus bias, I argue these presumptions may sometimes be *pessimistic*. Pessimistic presumptions of sociopolitical *misalignment* erode the benefits of sociopolitical silence and incentivize firms to combat these presumptions with

countervailing sociopolitical claims. In the following two subsections I detail this two-part process wherein (1) stakeholders generalize non-firm political actors' positions to firms associated with these actors and, consequently, (2) firms that depend on stakeholders whose preferences misalign with these positions combat this generalization with countervailing claims.

2.2.1 Sociopolitical Stigma

My argument starts from the premise that stakeholders generalize the positions of high-profile non-firm political actors to firms associated with these actors. Frequently, this association will arise due to the firm's location. For example, stakeholders observe a state's legislature pass left-leaning laws and then presume that firms in that state tend to hold left-leaning sociopolitical positions. These generalizations, however, might arise due to associations unrelated to location. For example, stakeholders might observe white supremacist rioters bearing Tiki Torches and then presume that the vendors or manufactures of Tiki Torches share the rioters' grievances. Critically, these presumptions arise from mere association and not because stakeholders are aware of actions by the firm consistent with these positions. When political actors misalign with stakeholders' preferences, this generalization engenders "sociopolitical stigma," which I define as stakeholders' negative evaluation of a firm due to a perceived lack of alignment between this firm and stakeholders' preferred sociopolitical position. This is a variation of Devers et al.'s (2009:p.165) general definition of stigma as "a label that evokes a collective perception that [an] organization is deeply flawed and discredited." With my construct, the discrediting "label" is distinctly sociopolitical: Stakeholders with a shared sociopolitical preference collectively perceive these stigmatized firms as misaligned with their preferences, and are consequently less inclined to reward them.

This argument is consistent with prior research demonstrating that individuals generalize the actions of focal entities to sets of associated entities (Adut 2005, Pontikes et al. 2010). The canonical example is Hollywood during the Red Scare, where audiences formed negative evaluations not only of actors that had been blacklisted as Communists, but also of actors that had not been listed as Communists but who had previously worked with blacklisted actors (Pontikes et al. 2010). This inclination to generalize is starkly illustrated by evidence that individuals can incur negative evaluations due simply to physical proximity with individuals with stigmatized characteristics (Hebl and Mannix 2003, Penny and Haddock 2007, Pryor et al. 2012). These generalization processes

play a critical role in how stakeholders evaluate organizations. Unable to collect detailed information on large numbers of individual organizations, stakeholders frequently make wider inferences based on the actions of a small number of organizations. These inter-organizational generalization processes have been documented in many contexts, including among organizations that share a parent organization (Jensen 2006, Piazza and Jourdan 2018), industry affiliation (Jonsson et al. 2009, Durand and Vergne 2015, Naumovska and Lavie 2021), partnership (McDonnell et al. 2021), or organizational form (Yue et al. 2013).

Through these same generalization processes, position-taking by political actors significantly shapes stakeholders’ presumptions regarding the positions of firms associated with these actors. These actors attract media attention, which coordinates generalization processes and results in “collective perceptions” of firms’ sociopolitical positions which, because large numbers of stakeholders hold them, become material to the firm (Devers et al. 2009). Other forces or actors plausibly shape stakeholders’ presumptions regarding firms’ sociopolitical positions. The prominence of political actors in the sociopolitical landscape, however, as well as their mandates to frequently take sociopolitical positions, means that they play a primary role.

2.2.2 Countervailing Claims

These presumptions create incentives for firms to either claim a sociopolitical position or maintain silence. On the one hand, if the firm is more reliant on stakeholders whose preferences are *further* from these actors’ positions, then the firm is more incentivized to combat the ensuing stigma with countervailing sociopolitical claims. In this case, the firm faces a greater burden of proof in demonstrating to stakeholders that, despite association with a political actor who holds a disliked position, it is, in fact, aligned with its stakeholders’ preferences. Borrowing Adut’s (2005, p.231) evocative language, such firms have become, in the eyes of their stakeholders, contaminated “third parties” who, for reasons unrelated to their own actions, face pressure “to signal resolve and rectitude” to their stakeholders’ preferences. In doing so, the firm still incurs costs associated with making these claims, such as unwanted attention from activists or accusations of hypocrisy, but these costs are more likely to be offset by the benefits that come with remedying stakeholders’ presumptions of misalignment. On the other hand, if the firm relies more heavily on stakeholders whose preferences are *closer* to the political actor’s position, then the firm benefits from presumptions of

sociopolitical alignment and thus has a greater incentive to remain silent.

My characterization of strategic positioning is consistent with Werner’s (2012) conceptualization of private politics, which modifies March’s behavioral theory of the firm to allow more room for “strategic calculation” (p.22-23). Central to both my argument and Werner’s conceptualization is the idea that firms monitor the social environment and seek to “protect themselves and their reputations from hostile public opinion” (p.24). In contrast to this and other prior work, however, I argue that sociopolitical positioning is neither a direct response to competitors (Mohliver et al. 2021), an effort to assuage general public distrust of corporations, nor meant to compensate for the focal firm’s own missteps (Elsbach 1994, Werner 2012, McDonnell and King 2013). Instead, countervailing claims arise as decision makers within firms come to believe that, due to their mere association with political actors who take positions disliked by their stakeholders, they may benefit by combating presumptions that they share these disliked positions. Note that this argument operates regardless of exactly *why* perceptions of sociopolitical misalignment make stakeholders less inclined to reward firms. Whether this disinclination is fundamentally “taste-based” (*i.e.*, due simply to ideological aversion) or, “statistical” (*i.e.*, due to concern that the sociopolitical position signals additional undesirable firm qualities), firms face incentives to remedy it by explicitly aligning with the preferences of those stakeholders on which they most depend.

3 Empirical Context: Unite the Right Rally in Charlottesville, VA

I examine this theory in the context of employer positioning on diversity issues in wake of the Unite the Right white supremacist rally in Charlottesville, Virginia on August 11-12, 2017. This context is ideal for two key reasons. First, the rally was exogenous to the actions of Charlottesville’s employers. Although political actors constantly position on divisive issues, this positioning is often endogenous in firms’ other actions. For example, firms donate to politicians, lobby for legislation, participate in lawsuits, and officially sponsor parades and other protests. At the very least, they may employ individuals who privately engage in these activities. This makes it difficult to infer whether firms’ sociopolitical positioning is in fact a response to the positions taken by political actors or is instead driven by some other firm characteristic. In the case of the Unite the Right rally, however, white supremacist protesters came, with almost no exception, from outside of Charlottesville, plausibly staining Charlottesville-area employers with an anti-diversity stigma that was

unrelated to their own actions and even the private behaviors of their employees. Second, Charlottesville is isolated in rural central Virginia. This isolation allows me to compare the responses of employers in this clearly-defined “treated” location to the behavior of employers in “control” locations. In the remainder of this section I describe the Unite the Right rally and provide preliminary, qualitative evidence from memoirs, media reports, and twenty-one original field interviews¹ that employers in Charlottesville sought to combat *anti*-diversity stigma arising from the rally with countervailing *pro*-diversity claims. This preliminary qualitative evidence is illustrative and suggestive and serves primarily to describe the setting while building intuition for the large-data, quantitative analysis that follows.

3.1 Sociopolitical Stigma for Charlottesville Employers

The Unite the Right rally was organized in response to the Charlottesville city council’s decision in February 2017 to remove a statue of confederate Civil War general Robert E. Lee. The rally attracted thousands of white supremacists from around the country to Charlottesville on August 11th and 12th. Through their clothing, protest signs, and verbal chants, the agitators manifested strongly anti-diversity positions. The event captured widespread attention, with striking images of mostly-male, Tiki-Torch-bearing white supremacists appearing across major media outlets (McAuliffe 2019, Signer 2020). Commentators have characterized the rally as the “largest demonstration of white supremacy in a generation” (Rowley 2018).

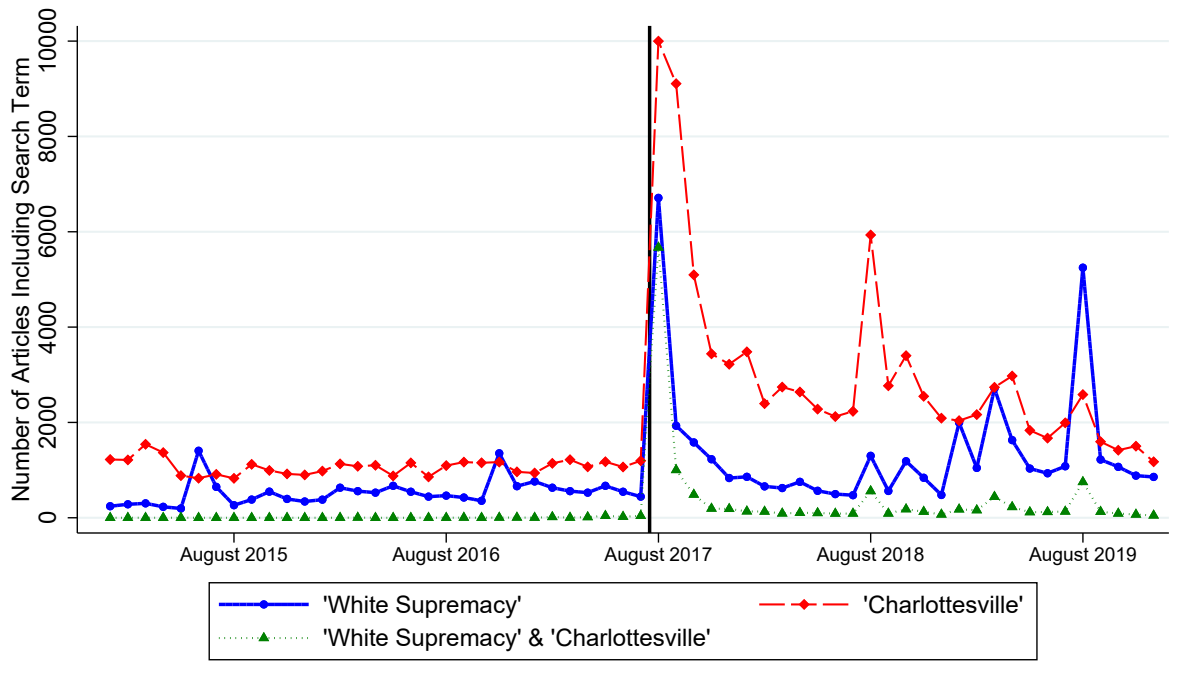
Multiple accounts describe how the rally stained Charlottesville with a newfound anti-diversity stigma. In his memoir of the events, then-Virginia Governor Terry McAuliffe characterized the rally as a “lightning bolt,” which “lit up the scourge of racism and hatred of others as it really is, in the here and now (McAuliffe 2019:p.158).” Then-Charlottesville mayor Michael Signer characterized the rally as an “earthquake” in which perceptions of Charlottesville were “fundamentally shaken.” Signer lamented that, due to the rally,

...Charlottesville would become synonymous with white supremacy and with terrorism. The city’s “brand” now comingled the pleasant contours of UVA and Monticello with swastikas, shields, swords, helmets, and that haunting chant: “Jews will not replace us” (Signer 2020:p.8).

Some Charlottesville residents lamented a disconnect between these perceptions and their experiences. One resident quoted in *The New York Times* characterized “the portrait of Charlottesville

¹I describe the sampling method for identifying these interviewees in the section titled “Interview Sampling Methodology” in the appendix.

Figure 1: Unite the Right created association between Charlottesville and white supremacy.



that emerged from 2017” as “exaggerated,” bemoaning “a misconception that we just have Klan members hanging around Charlottesville” (MacFarquhar 2021). Similarly, multiple interviewees described friends and family from out of town who had difficulty recognizing that the rioters, with almost no exception, were not from Charlottesville and thus formed an inaccurate belief that the rally displayed anti-diversity attitudes that were common among Charlottesville’s residents. One interviewee recalled that while attending college out of state, her son was introduced as coming from “Charlottesville, where the Nazis ride.” She added, “that’s just the way outsiders perceived it.”

To more systematically substantiate this newfound stigma by association, I documented media mentions of Charlottesville in the period before and after the rally. Using the Nexis Lexis media archives, which capture major print, audio, and television media reports, I documented the number of stories by month from 2015 through 2019 which contained the search terms “Charlottesville,” “white supremacy,” or both of these terms. These patterns are illustrated in Figure 1, and show strongly correlated upticks in each of these three search terms beginning in August 2017. The chart also shows correlated upticks in these search terms around the annual anniversary of the events (August 2018 and August 2019) suggesting this newfound stigma was not transitory.

My interviewees provided preliminary evidence that Charlottesville’s employers became sensitive to how this stigma by association shaped how they were perceived by prospective employees. A Charlottesville government official recalled,

I can remember specifically a conversation with a large company in town that, you know, they had visits planned and they had to change those. And then they had people kind of [pause] concerned...I think the local firms that were hiring, you know, significantly and looking to recruit people here, to take a job, had to kind of adjust their approach and be prepared to answer the question about [where they stood on the issue of diversity].

Reporting similar concerns, another interviewee commented,

Every time I try to hire someone at the highest level we have to talk about [the rally]. If they’re from Stanford, Duke, Berkeley, they’re like what the hell was that? Is that who you are?

Similar difficulties were mentioned by other interviewees. One expressed that the rally “came up repeatedly in interviews” and that “one African American applicant asked me explicitly about it and how [the employer] was resisting it.”

3.2 Countervailing Pro-Diversity Claims

The Unite the Right rally put Charlottesville’s employers in a difficult situation regarding whether they should speak up or remain silent on issues of diversity. Although the vast majority of Americans do not explicitly support white supremacy, there is surprisingly high levels of agreement with anti-diversity positions central to white supremacist ideology. In a national poll conducted a month after the rally, fewer than 8% of respondents expressed support for “white nationalism” or “neo Nazism,” but 31% agreed that the United States needed to “protect and preserve its White European heritage,” 39% agreed that “white people are under attack in this country,” and 57% said that Confederate monuments should remain in public spaces (New Poll 2017).

Disagreement regarding whether to respond to the Unite the Right rally with countervailing pro-diversity claims, moreover, reflected mainstream divisions in American society regarding the appropriateness of efforts to celebrate demographic diversity. Those on the political left are more inclined than those on the political right to believe that demographic diversity should be celebrated (Fingerhut 2018). This partisan division was manifest in the contrasting ways Democratic

and Republican politicians responded to the events. While Virginia’s Democratic Governor Terry McAuliffe immediately condemned the rally, Republican President Donald Trump equivocated before famously affirming that “there were very fine people on both sides” (McAuliffe 2019). The emergence of diversity as a divisive sociopolitical issue is further evident in efforts under the Trump administration and Republican-led state governments to eliminate diversity, equity, and inclusion (DEI) initiatives (Fuchs 2020), as well as heated debate in local school board meetings regarding the appropriateness of educational initiatives related to racism, LGBTQ+ equality, and antisemitism (Thompson and Press 2022). In this sense, my approach mirrors recent work that has leveraged the January 6, 2021 assault on the US capitol (Li and Disalvo 2022) or mass shootings (Hou and Poliquin 2022) to examine firms’ sociopolitical positioning. In these cases, as in the Unite the Right rally, sharply divided *mainstream* public opinion regarding the appropriate response to *fringe* extremists put firms in a difficult position regarding whether they should speak up.

Comments from a number of interviewees suggested that Charlottesville’s employers were aware of this tension as they transitioned from staying silent to taking pro-diversity positions. In the words of one employee:

Prior to that event, [my employer] had obviously had an advocacy group, which took positions on matters of public policy, but those were limited to its sort of sphere of, stay in our lane. We’re going to talk about regulation. We’re going to talk about market ethics. We’re going to talk about [redacted]. We’re not going to get out into broader social issues. And that changed for the organization as a result [of the rally], and there was some very robust internal discussion about where we should take positions on issues of social import.

Detailing this “robust internal discussion,” this interviewee went on to describe spirited back and forth regarding whether it was advisable for the firm to embrace a pro-diversity stance, remain silent, or even speak up in favor of the protesters’ first amendment right to free speech. Similar debate was described by an interviewee from a different employer who indicated that it took an entire year of internal debate, paired with growing recognition that the rally was creating difficulties for recruiting, before the employer ultimately adopted pro-diversity claims.

Additional interviews and media reports provided further examples of employers across Charlottesville responding to the rally by eschewing silence and taking pro-diversity positions. One interviewee explained:

I think one of the things that really came out of the events from Unite the Right is that organi-

zations I think went from a more passive support of social positions and sort of a tacit, we all got this right, to a much more active and vocal position on issues.

In an interview with the local newspaper *C’Ville Weekly*, Andrea Copeland of the Charlottesville Chamber of Commerce characterized this shift from silence to pro-diversity positioning as an effort by employers to combat presumptions that they shared the attitudes that had been on display at the rally. Her words closely mirror my theoretical argument:

A lot of companies, for good reason, have sat down and said, ‘What can we do to make things better, to make everyone feel welcome, to make this community aware that we don’t stand for exclusion, we don’t stand for racism, we don’t stand for what was on display August 11th and 12th (Pullinger 2019)?

An interviewee at a local financial services company reported that whereas speaking up on social issues “had not been [the employer’s] style in the first four years [the interviewee] worked there,” after the rally that changed as the employer “came out with statements [about diversity] and put up some kind of fluffy, but nice stuff on our website.” In another case, Jim Hall, CEO of WorldStrides, which sells educational trips for student groups, penned a letter titled “Our Hometown” on the company’s website in which he stated,

It was a disturbing and sad weekend here in WorldStrides’ hometown of Charlottesville...Today especially, I find solace in the work that we do as an organization. We expose young people to new places, new ideas, new people, and new cultures. Through that, I believe that we help them develop an understanding, appreciation, and love of diversity, and the richness that it brings to our lives.

Similar sentiments were expressed by Teachstone, a company that develops and markets online training resources. In a letter titled “A Message about our Home,” and signed “The Teachstone Family,” the company affirmed the importance of “developing true and real relationships among people of all races, ethnicities, religions, and backgrounds.” In another case, the Charlottesville-based Center for Non-Profit Excellence lamented in an open letter on its website that “we were invaded this weekend. [sic]By violent, extremist hate groups that don’t reflect our collective values or the way we view the world.” The University of Virginia (UVA), Charlottesville’s largest employer, responded to the events with a series of countervailing pro-diversity claims. A general communication from the University stated that, “Those who gather with the intent to strike fear and sow division do not reflect the University’s values and will not influence or diminish the Uni-

iversity’s commitment to inclusion, diversity and mutual respect.” Shortly thereafter, a similar statement was issued by then-UVA president Teresa Sullivan, who affirmed that “the ideologies and beliefs expressed by many of the groups that have converged on Charlottesville this weekend contradict our values of diversity, inclusion, and mutual respect.” Illuminating a key motivation behind these statements, an interviewee who works in recruiting at UVA explained that in the wake of the rally “we wanted to do everything we could to dispel the myth that this is who we are.” These many examples are consistent with my proposed theory: the rally created an *anti*-diversity stigma by association, which these employers felt compelled to combat via countervailing *pro*-diversity claims.

4 Econometric Analysis

These qualitative anecdotes are consistent with my theory, but they do not allow me to systematically demonstrate the proposed effect or address alternative explanations. In this section, I use a difference-in-differences design on a comprehensive dataset of online job postings in the United States to estimate the effect of the rally on Charlottesville-area-employers’ propensity to make pro-diversity claims in their recruiting efforts.

4.1 Data and Variables

Pro-Diversity Claims in Job Postings: I operationalize sociopolitical claims in terms of firms’ use of pro-diversity claims in job postings. Recent examples have appeared in postings by Chevron, which touts a commitment to “respect, diversity, and inclusion,” and Nike, which asserts it “thrives by embracing diversity.” Whereas white supremacy celebrates racial and cultural uniformity, pro-diversity claims carry a multicultural ideology that celebrates racial and cultural differences (Plaut et al. 2011, Dover et al. 2016). As described in the previous section, at this moment in United States history, pro-diversity claims marked, and continue to mark, a sociopolitical stance in opposition not only to a historical, white-dominated status quo, but political forces, especially on the right, which resist trends in growing diversity (Dover et al. 2016, Norris and Inglehart 2019, McVeigh and Estep 2019). Analysis of corporate sociopolitical stance-taking from this period have concluded that “diversity [was] the most frequently advocated issue” (Larcker et al. 2018:p.2), while experimental work demonstrates that pro-diversity claims in job postings carry

a strongly Democratic partisan connotation (Hurst 2021). Further illustrating the sociopolitical nature of pro-diversity claims, a CNN photographer at the Unite the Right Rally captured a now iconic image of Confederate-flag-bearing white supremacists marching far below a massive banner hung by Charlottesville residents emblazoned with the words “Diversity makes us stronger” (see Figure A1). Thus, although some past work has characterized pro-diversity claims simply as pro-social claims (McDonnell and King 2013:p.397), in this context they represent a position on one side of a divisive social issue.

Examining pro-diversity claims in the context of online job postings has two key advantages for evaluating my theorized relationship. First, job postings constitute a near-universal medium whereby firms can make sociopolitical claims. Whereas only a small subset of organizations have a meaningful media presence, nearly all organizations make online job postings. Second, geographic variation in job postings allows for fine-grained inference regarding the strategic behavior I theorize. In theory, firms can tailor job postings across time, locations, and labor pools in response to variation in sociopolitical stigma and worker preferences. Other mediums whereby firms make sociopolitical claims, such as social media posts, lack this location and labor pool specificity, making it more difficult to isolate my theorized mechanism.

I measure firms’ use of pro-diversity claims with a data-set of all online job postings in the United States in the four-year period straddling the rally. I purchased this data from Burning Glass Technologies. Burning Glass collects all job posting from approximately 45,000 company websites, online newspapers, and online job boards. They ensure that no more than 5% of vacancies found this way come from any one source and remove all duplicate postings across job boards. Third party analysis from 2014 estimated that Burning Glass captures all job postings in the United States that appear online (Carnevale et al. 2014). Analysis by Burning Glass from 2022 indicates that, dating back to 2013, the data captures 98.7% of jobs included in the United States Bureau of Labor Statistics’ Job Openings and Labor Turnover Survey (Representativeness Analysis 2022),² which is the primary measure of job openings in the United States and is used extensively by policy makers and other labor market analysts (Zanzalari 2022). The data suffers some underrepresentation for jobs where recruiting is less likely to occur online, such as those related to food preparation and serving, office and administrative support, and construction. Accordingly, I am

²This representativeness report is available in the supporting documentation.

only able to measure changes in employers’ use of pro-diversity claims in *online* recruiting.

I code a job posting as featuring a pro-diversity claim if it meets any one of the three following criteria: (1) It includes the word “diversity,” but not only within the words “diversityjobboard,” “diversityjobs,” “diversitynursing,” or “pharmadiversity.” (2) It includes the word “inclusion” but not the word “inclusion” followed by “in”, “of”, “onto” or “with.” (3) Or, it includes the word “inclusive” but not the phrase “all inclusive” or “inclusive of.” I detail the iterative process whereby I arrived at and validated this measure in the appendix. To summarize, this strategy is based on extensive manual analysis on random samples of job postings including the words “diversity,” “equity,” “inclusion,” “diverse,” “equitable,” or “inclusive.” I also examined a range of other words and phrases that might signal pro-diversity claims, including “belonging”, “bipoc,” “black lives matter,” “dei,” and “social justice,” but found that these were exceedingly rare. This approach yields not only a very high true *positive* rate (I estimate that 96% of postings coded as featuring pro-diversity claims actually feature pro-diversity claims) but also a very high true *negative* rate (I estimate that 97% of job postings coded as not featuring pro-diversity claims do not actually feature pro-diversity claims).

Additional Variables: I use additional variables at the job posting, employer, and geographic levels. At the job posting level, these include whether or not the job posts a salary (rather than an hourly wage or no information on compensation), the two-digit NAICS industry code, whether or not the job requires a bachelor’s degree, and the county in which the job is located. At the employer level, I use manual internet searches to identify employers that are not only hiring in Charlottesville but also based in or headquartered in Charlottesville. At the geographic level, I use the United States Census Bureau’s American Community Survey (ACS) to collect annual 5-year estimates of the county’s median age, the percent of residents that identify as non-Hispanic white, the population, the fraction of residents that hold a bachelor’s degree, residents’ average annual earnings and the vote share that went to Clinton in the 2016 election. I omit job postings that are missing values on any of these job or county-level variables or for which there is no employer listed.

4.2 Appropriate Level of Analysis

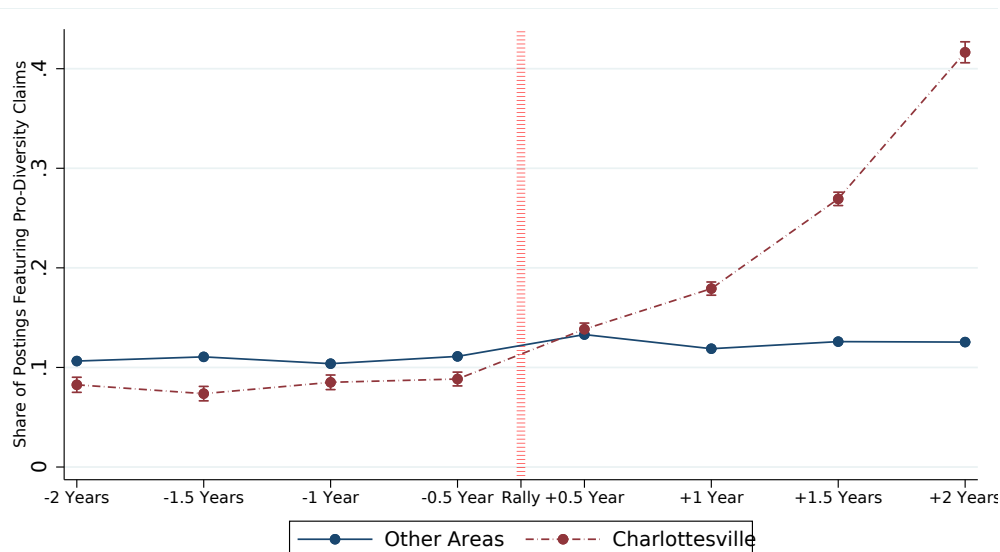
When it comes to making pro-diversity claims, employers may either tailor the language of individual postings or make one-off decisions whereby they standardize language across many or

all postings. This raises the question as to whether analyzing individual postings or individual employers is the appropriate strategy for capturing employers' decisions to use pro-diversity claims. I feature posting-level analysis as my main results for two related reasons. First, empirical patterns in my data suggest that universal, standardized use of pro-diversity claims is the exception rather than the rule. Figure A2 in the appendix shows that within the same employer, in the same county, in the same year, the share of job postings that feature pro-diversity claims varies significantly. Specifically, among employer-county dyads that make a pro-diversity claim in *any* of their postings in a given year, the percent of employers featuring pro-diversity claims in *all* of their postings is always below 30%, and is well below 25% in three of the four years in my sample. Insights gleaned from analyzing this within-employer, posting-level variation helps arbitrate between competing theoretical explanations (see Section 5 below).

Relatedly, by analyzing individual postings I capture the reality that, especially within larger employers, recruiting is not the purview of a single, central decision-maker but rather the particular department, team, or division that is hiring. For example, although nearly all job postings by the University of Virginia are coded by Burning Glass under a single employer, hiring is the purview of disparate academic departments, athletic teams, and administrative divisions within the university. This likely explains, in part, why some but not all postings by the University of Virginia across any six-month period in my data feature pro-diversity claims (see Figure A4 in the appendix). Treating employers as the unit of analysis would inaccurately characterize these larger employers as governed by a single decision maker or set of decision makers, giving them equal weight to smaller employers, such as single-location nail salons, restaurants, and laundromats, where this is in fact the case.

There are, however, two drawbacks to using job postings as the units of analysis. First, where employers make a single decision to include pro-diversity claims in large numbers of postings, posting-level analysis arguably puts too much weight on employers that post many jobs. Second, posting-level analysis involves comparing different populations of firms in the pre- and post-treatment periods. I use employer-county fixed effects to ensure that postings by employers that do not appear in the pre- and post-treatment periods do not directly contribute to the estimated treatment effects, but postings by these employers might indirectly shape this estimate through their influence on time fixed effects or coefficients on controls. To assuage these two concerns, I supplement my principal job-posting-level analysis, where the outcome is whether or not a given job

Figure 2: Posting-level trends show that in the years following the Unite the Right Rally there was an exceptionally large increase in the share of Charlottesville job postings featuring pro-diversity claims.



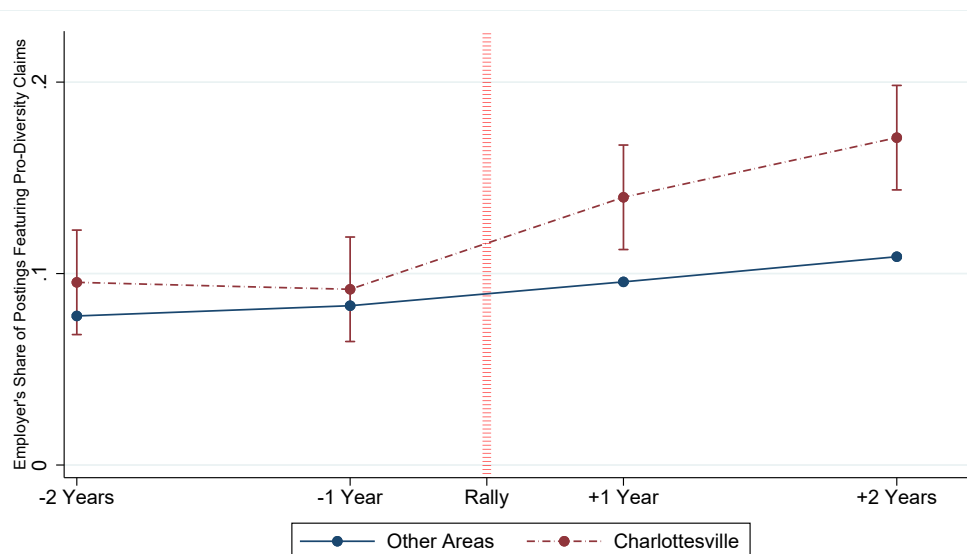
NOTES: Points indicate the share of job postings by six month periods that included pro-diversity claims in the Charlottesville MSA versus other areas, (*i.e.*, the rest of the United States). Six months periods are measured relative to the second day of the rally, August 12, 2017. The vertical dashed line marks the division between the pre and post-rally period. Points are accompanied by 95% confidence intervals, but given high precision these are not visible on the “Other Areas” points are only barely visible on the Charlottesville points.

posting features a pro-diversity claim, by analyzing a balanced-by-year panel of employer-county dyads, where the outcome variable is the share of job postings in a given employer-county-year that feature pro-diversity claims. Although neither my principal posting-level analysis nor this supplementary employer-level analysis flawlessly captures a decision to make a pro-diversity claim, I show my results are robust to either approach.

4.3 Summary Statistics

Table 1 presents summary statistics at the posting, employer, and county level. I present these for the full sample, the Charlottesville MSA, counties that are home to flagship state universities, and counties in which more than 50% of the vote share went to Hilary Clinton in the 2016 presidential election. The Charlottesville MSA includes the City of Charlottesville, Albemarle County, Fluvanna County, Greene County, Nelson County, and the incorporated town of Scottsville. 91% of job postings in the Charlottesville MSA are in the city of Charlottesville. My full sample includes 66.5 million job postings, 12% of which featured a pro-diversity claim. This low rate is consistent with the idea, central to my theory, that employers are sensitive to the downsides associated with

Figure 3: Employer-level trends show that in the years following the Unite the Right Rally, there was an exceptionally large increase in the average share of job postings in which Charlottesville’s employers featured pro-diversity claims.



NOTES: Points indicate the share of job postings, on average, in which employer-county dyads featured pro-diversity claims in their job postings in the Charlottesville MSA versus other areas, (*i.e.*, the rest of the United States). This reflects a balanced panel in the sense that it only includes employer-county dyads that post at least one job in each of the four years surrounding the rally. Years are measured relative to the second day of the rally, August 12, 2017. The vertical dashed line marks the division between the pre and post-rally period. Bars represent 95% confidence intervals, but given high precision these are not visible on the “Other Areas” points.

sociopolitical claims and thus tend to avoid them. Equal opportunity claims, in contrast, signal intentions for regulatory compliance, appear in a much higher 40% of postings. Consistent with my theorized relationship, Figure 2 shows that the share of Charlottesville-area job postings that featured pro-diversity claims increased 401% from 8.3 percentage points in the pre-treatment period to 41.6 percentage points in the second half of the second year following the rally, while the use rate in jobs outside of Charlottesville increased only slightly. Whereas in the six months prior to rally the Charlottesville MSA was in the 64th percentile of MSAs in terms of the share of job postings that featured pro-diversity claims, two year later it had risen to the 99% percentile (see Figure A3 in the appendix). Table A2 in the appendix reports summary statistics for the employer-level, balanced panel dataset, and Figure 3 shows a similar increase in the use of pro-diversity claims when using these employers, rather than postings, as the units of analysis.

4.4 Difference-in-Differences Analysis

I estimate a difference-in-differences model where I compare the difference in the use rate of pro-diversity claims among job postings in the Charlottesville MSA to the use rate of pro-diversity

Table 1: Summary Statistics

	(1) Full Sample	(2) Charlottesville MSA	(3) Flagship Counties	(4) Clinton Counties
Job-Level				
Pro-Diversity Claim	0.12 (0.32)	0.15 (0.36)	0.13 (0.34)	0.13 (0.33)
Equal Opportunity Claim	0.40 (0.49)	0.49 (0.50)	0.41 (0.49)	0.40 (0.49)
Job Requires Bachelor's Degree	0.29 (0.45)	0.39 (0.49)	0.32 (0.47)	0.34 (0.47)
Pay Annually Dummy	0.15 (0.35)	0.16 (0.37)	0.17 (0.38)	0.15 (0.36)
Moving Expenses Covered	0.05 (0.22)	0.03 (0.17)	0.05 (0.22)	0.05 (0.21)
Postings	66,498,855	62,074	6,080,342	42,044,363
Employer-Level				
Annual Jobs / Employer	29.05 (686.84)	11.78 (104.07)	17.02 (178.94)	25.10 (502.24)
Common Employers (Share)	Anthem (0.01) Lowe's (0.01) CACI (0.01) Marriott (0.01) HCA (<0.01)	UVA (0.19) CACI (0.10) Anthem (0.05) Sentara (0.02) UVA Health (0.02)	Amazon (0.01) Anthem (0.01) JP Morgan Chase (0.01) Lowe's (0.01) Accenture (0.01)	Anthem (0.01) CACI (0.01) Marriott (0.01) Accenture (0.01) Macy's (0.01)
Common Industries (Share)	Healthcare (0.19) Finance (0.10) Professional Ser. (0.10) Hospitality (0.09) Wholesale (0.09)	Education (0.23) Professional Ser. (0.15) Healthcare (0.13) Finance (0.09) Retail (0.08)	Healthcare (0.17) Professional Ser. (0.10) Education (0.10) Finance (0.10) Hospitality (0.08)	Healthcare (0.18) Professional Ser. (0.12) Finance (0.11) Hospitality (0.10) Retail (0.08)
Public Administration	0.03 (0.17)	0.04 (0.19)	0.03 (0.18)	0.03 (0.16)
Unique Employers	1,493,729	3,167	230,395	1,096,955
Annual Jobs / Employer-County	8.45 (62.04)	10.32 (97.13)	9.48 (81.51)	9.45 (74.77)
Unique Employer-County Dyads	5,034,136	3,708	407,585	2,874,805
County-Level				
Median Age	41.19 (5.33)	40.77 (5.77)	34.90 (4.77)	37.93 (5.13)
% White	77.03 (19.84)	74.64 (8.42)	70.23 (18.27)	50.39 (23.80)
Population	102,976.49 (32,9057.10)	38,137.37 (3,2325.66)	398,565.19 (444,815.83)	392,209.98 (777,446.08)
Average Annual Earnings	33,698.15 (6,334.39)	37,580.10 (5,695.84)	39,330.05 (6,117.48)	35,955.93 (9,847.74)
Clinton Vote Share in 2016	0.32 (0.15)	0.50 (0.16)	0.55 (0.13)	0.61 (0.09)
Unique Counties	3,141	6	54	432

Notes: Column 1 includes the full sample. Column 2 includes only the subset of postings from the Charlottesville MSA. Column 3 includes only those postings from counties that are homes to flagship state universities. Column 4 includes the subset of postings from counties where more than 50% of the 2016 presidential vote went to Hilary Clinton.

claims among all other job postings in the pre- versus post-shock periods. Specifically, I estimate

$$Diversity_{i,c,t} = \beta Charlottesville_{i,c,t} \times PostRally_{i,c,t} + X_{i,c,t} + \lambda_{i,c} + \lambda_t + \epsilon_{i,c,t} \quad (1)$$

where $Diversity_{i,c,t}$ is a dummy equal to one if a job posting by employer i in county c in year-month

Table 2: Difference-in-differences estimates at the level of job postings shows a significant increase in pro-diversity claims among Charlottesville job postings following the Unite the Right rally.

Sample:	(1) All	(2) Flagship Counties	(3) Clinton Counties	(4) Only UVA	(5) Excluding UVA
Charlottesville $\times$ Post-Rally	0.086*** (0.003)	0.086*** (0.007)	0.089*** (0.004)	0.215*** (0.036)	0.024*** (0.005)
Year Month Fixed Effects	✓	✓	✓	✓	✓
Employer County Fixed Effects	✓	✓	✓	✓	✓
Job-Level Controls	✓	✓	✓	✓	✓
County-Level Controls	✓	✓	✓	✓	✓
Observations	55,668,345	5,089,867	34,632,156	1,273,264	55,654,371

Notes: The units of analysis in all models are individual job postings. “Charlottesville $\times$ Post-Rally” is a dummy variable equal to one for job postings made in the Charlottesville area after August 11, 2017. The outcome in all models is a dummy variable equal to one if the job posting included a pro-diversity claim. All columns report estimates of Equation 1 and differ only in terms of the sample on which they are estimated. Column 1 includes the full sample of job postings. Column 2 includes the subset of job postings in the Charlottesville area and counties that were also home to flagship state universities. Column 3 includes the subset of job postings in counties where more than 50% of the 2016 presidential vote went to Hilary Clinton. Column 4 includes job postings from the University of Virginia and all other job postings by institutions of higher education outside of Charlottesville. Column 5 excludes job postings made by the University of Virginia. All estimates are linear probability models with robust standard errors clustered on county. Notice that the discrepancy in sample size in this table compared to Column 1 of Table 1 follows from the fact that I estimate these model’s with Stata’s *reghdfe* command, which drops singleton observations. $^{\dagger}p < 0.10$, $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

t included a pro-diversity claim, $Charlottesville \times PostRally$ is a dummy equal to one if the posting was made in the Charlottesville MSA after August 11th, 2017, X is a vector of the job and county-level controls listed above, $\lambda_{i,c}$ are employer-county fixed effects, λ_t are month-year fixed effects where months are measured in relation to the rally (*i.e.*, August 12-August 11), and ϵ is the error term. β is the coefficient of interest and represents the difference-in-differences estimate of the effect of the Unite the Right rally on the extent to which job postings in the Charlottesville MSA featured pro-diversity claims. I estimate this relationship using a linear probability model with robust standard errors clustered on county. Results are presented in Model 1 of Table 2. This illustrates that the estimated treatment effect of the Unite the Right rally on the share of Charlottesville-area job postings featuring pro-diversity claims was 8.6 percentage points ($p < 0.001$). Relative to the unconditional pre-treatment baseline rate of 8.3%, this represents a sizeable 104% increase.

4.5 Evaluating Plausibility of the Parallel Trends Assumption

A key identifying assumption for causal interpretation of this estimate is the parallel trends assumption. This means that in the absence of the Unite the Right rally the difference between the rate with which job postings in the Charlottesville area featured pro-diversity claims and the rate at which postings in all other areas featured pro-diversity claims would have remained constant from the pre- to post-treatment period. Although this assumption is not directly testable, I conduct

three exercises to examine its plausibility.

Placebo Thresholds: First, I re-estimate the treatment effect at pre-treatment, placebo dates. I limit the sample to all job postings made in the two years prior to the rally and then re-estimate Equation 1, replacing the true treatment threshold with placebo treatment thresholds at three, six-months-apart, pre-treatment dates (February 11, 2016; August 11, 2017; February 11, 2017). These estimates, along with the estimate of β at the true treatment threshold for the full sample, are charted in Figure A5 in the appendix. Although these three coefficients are significantly different from zero, they are all much smaller in magnitude than my estimated effect and in two of the three cases in the opposite direction.

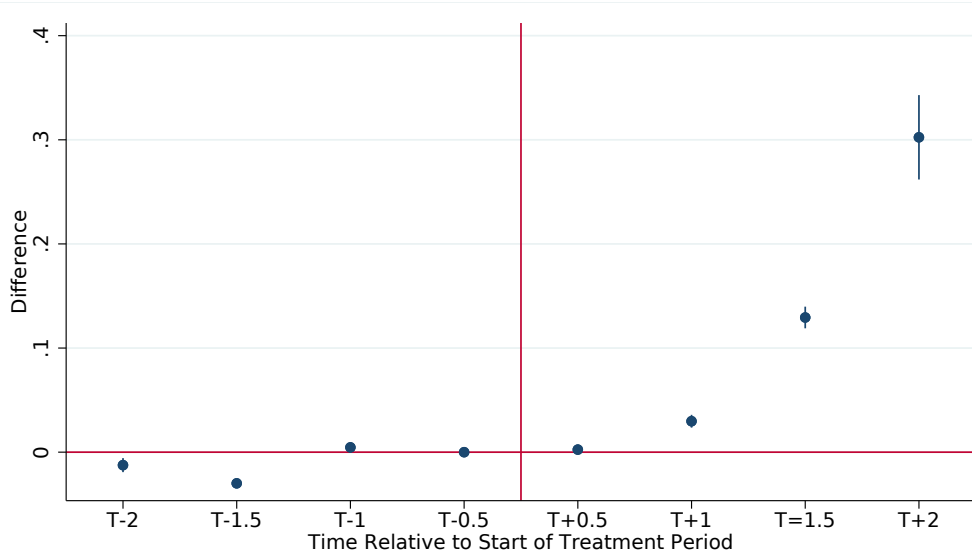
Time Specific Effects: As a second test, I estimate an event-study model to examine whether there was a non-zero trend in the difference-in-differences estimates prior to treatment. Specifically, I estimate

$$Diversity_{i,c,t} = \sum_{k=-3, k \neq 0}^{k=4} \delta_k Cville_{i,c,t} \times period_k + X_{i,c,t} + \lambda_{i,c} + \lambda_t + \epsilon_{i,c,t} \quad (2)$$

where *period* are year dummies equal to one if the posting was made in period k where $k \in [-3, 4]$ corresponds to each of the eight, 6-month periods in the study's time window (meaning $k = -3$ for entries made between August 12th, 2015 and February 11th, 2016, $k = 4$ for entries made between February 12th, 2018 and August 11th, 2019). The parameters δ_k represent the period-specific difference-in-differences estimates. Note that the dummy for $k = 0$ is omitted so that these estimates are relative to the six month period before the rally (February 12th, 2017 to August 11th, 2017). If there were no confounding pre-trends, then the test should not reject the null that $\delta_k = 0$ for all $k < 0$. Figure 4 shows these estimates, and shows that two of these coefficients are in fact significantly different from zero, but are very small in magnitude. I also estimate this event-style model while excluding postings from UVA. These results are shown in Figure A6 in the appendix. In this case the effects are smaller, but also climb in the post-treatment period.

Parallel Trends Sensitivity Analyses: Given evidence of possible non-zero differences in pre-trends shown in Figure 4, I conduct sensitivity analysis suggested in Roth (2021). Roth recommends estimating the robustness of results to the hypothetical non-zero difference in pre-trends that would be detected 50% and 80% of the time. Figures A7 and A8 in the appendix plot these hypothesized

Figure 4: Dynamic difference-in-differences estimates of δ_k from Equation 2 show the estimated effect grew throughout the post-treatment period.



NOTES: Each point represents the estimates of δ_k from Equation 2, where δ_{-3} corresponds to “T-2”, δ_{-2} corresponds to “T-1.5,” and so on. These represent the difference in predicted probability that, in the indicated time period, a job posting in Charlottesville versus a job posting outside of Charlottesville features a pro-diversity claim relative to this difference in the reference time period (δ_0 , which corresponds to “T-0.5”). The reference time period is the six-month period preceding the Unite the Right Rally (February 12, 2017 - August 11, 2017). Bars indicate 95% confidence intervals.

pre-trends relative to the estimates of δ_k and show that, even allowing for non-zero pre-trends of this size, the post-treatment coefficients $\hat{\delta}_2$ - $\hat{\delta}_4$ remain significant. Figures A9 and A10 show that the results excluding UVA (shown in Figure A6) are also robust to these hypothetical non-zero pre-trends. Together, these three analyses are consistent with the conclusion that the estimated effects cannot be attributed to non-parallel pre-trends.

4.6 Considering Alternative Control Geographies

In the preceding analysis, the control group includes any job posting made outside of the Charlottesville MSA. It could be the case, however, that there was a confounding trend or event, simultaneous with but separate from the Unite the Right rally, which systematically increased the use of pro-diversity claims in job postings in a subset of places similar to Charlottesville. To address this possibility, I identify alternative control geographies that, in different ways, are especially similar to Charlottesville.

Flagship State University Counties: First, I identify counties that, like Charlottesville, are home to flagship state universities. Table 1 shows these areas are more similar to Charlottesville on a number of observed covariates. They may also be more similar in important, less observable

ways, such as their cultural importance and sensitivity to diversity issues. I re-estimate Equation 1, limiting the sample to postings in this subsample of locations. Results are presented in Model 2 of Table 2. The estimated treatment effect in this case ($\hat{\beta} = 0.086$, $p < 0.001$) is very similar to the estimate from the main specification.

Clinton Counties: Second, I identify counties in which, like Charlottesville, a majority of voters supported Hilary Clinton in the 2016 election. This addresses the possibility that there may have been a general pro-diversity backlash to Donald Trump’s election in which employers in left-leaning areas augmented their use of pro-diversity claims. I again re-estimate Equation 1, limiting the sample to postings in this subsample of geographies. The results of this regression are presented in Model 3 of Table 2. The estimated treatment effect in this case ($\hat{\beta} = 0.089$, $p < 0.001$) is again very similar to the estimate from the main specification.

UVA Versus other Higher Education Institutions: Third, I re-estimate Equation 1 but exclude all Charlottesville-area job postings not made by UVA, and all job postings outside of Charlottesville that were not made by colleges, universities, and professional schools. This addresses the possibility that there was an exceptional upward trend in the use of pro-diversity claims among institutions of higher education during my time period of analysis, which, since UVA features so prominently in the Charlottesville labor market, might confound my results. These results are reported in Model 4 of Table 2. The effect in this case is large ($\hat{\beta}_3 = 0.215$, $p < 0.001$), representing a 140% increase from UVA’s pre-treatment use rate of pro-diversity claims. This demonstrates UVA was exceptional relative to other higher education institutions.

Excluding UVA: Fourth, I again exclude UVA job postings and then re-estimate Equation 1. These results are reported in model 5 of Table 2, which shows that the effect shrinks ($\hat{\beta}_3 = 0.024$), but is still highly significant ($p < 0.001$). Relative to the unconditional pre-treatment baseline rate of pro-diversity claims among postings by non-UVA Charlottesville employers (0.066) this still represents a sizeable 36% increase in the use of pro-diversity claims. This estimate, however, indicates that UVA has a large effect on my posting-level estimates. For completeness, I conduct the firm-level analysis detailed in Section 4.7 below.

Synthetic Control Comparison: Finally, I employ the synthetic control method proposed in Abadie et al. (2010), which involves comparing a treated unit, in this case Charlottesville, with a synthetic control unit created from a weighted average of the untreated units. Besides providing

a method for identifying the control group, this addresses the possibility that, because I have few treated units and many control units, errors are heteroskedastic. To execute this approach, I limit the comparison set to job postings in MSAs, collapse the data to the MSA-month level, and then calculate the weights for the synthetic control using the geographic-level controls from Equation 1. Table A7 demonstrates the improvement in balance achieved by this synthetic control method. Figures A11 and A12 illustrate the main result, showing that the difference in the use rate of pro-diversity claims in Charlottesville versus the synthetic Charlottesville rose in the years following the Unite the Right rally. I then conduct the randomization inference method developed by Abadie et al. (2010). I iteratively apply the synthetic control method to each untreated MSA. A sample of these placebo effects, along with the effects for Charlottesville, are plotted in Figure A13. I then calculate the average mean squared prediction error (MSPE) for each placebo geography across the pre-treatment period, do the same across the post-treatment period, and then calculate the percentage of placebo post- over pre-treatment MSPEs ratios that are greater than this ratio for Charlottesville. Figure A14 shows the distribution of this test statistic, which corresponds to a p-value of 0.010.

4.7 Employer-Level Analysis

So far, I have treated individual postings as the units of analysis. As I explain in section 4.2, this approach better captures decisions to make pro-diversity claims, but has the potential disadvantages of (1) over-weighting the decision of larger employers and (2) comparing different employers in the pre- and post-treatment periods. Accordingly, I repeat my main analysis at the employer level using a balanced-by-year panel of employer-county dyads. Stated differently, the panel includes employer-county-year combinations in which the employer posted at least one job in that county in each of the four years comprising my study’s period. Here, the outcome of interest is the share of job postings in a given year in which a given employer in a given county made a pro-diversity claim. I estimate a two-way fixed effects difference-in-differences model where

$$DiversityShare_{i,c,y} = \beta Charlottesville_{i,c,y} \times PostRally_{i,c,y} + \lambda_{i,c} + \lambda_y + \epsilon_{i,c,y} \quad (3)$$

where $DiversityShare_{i,c,y}$ is the share of job postings by employer c in county i in year y that feature a pro-diversity claim. Here, the parameter of interest β represents the effect of the Unite

the Right rally on the amount by which employers in Charlottesville, on average, changed the share of their job postings that included pro-diversity claims. Results are presented in Table A3 in the appendix. Here, the effect size is smaller ($\hat{\beta} = 0.039, p < 0.05$), but, relative to the pre-treatment unconditional baseline use rate of 9.3%, this represents a sizeable 42% increase. Table A4 in the appendix shows the effects by year, again showing that the effect grew with time. The estimated effect for the final year ($\hat{\beta} = 0.053, p < 0.01$) represents a 58% increase from the unconditional pre-treatment baseline mean. For robustness, I again re-estimate the effect but limit the control group to counties that feature flagship state universities, limit the control group to counties in which over half of voters supported Hilary Clinton in the 2016 election, limit the comparison to UVA versus other higher education institutions, and then remove UVA postings from the sample. Models 2-5 of Table A4 show that the results are robust in these alternative specifications.

4.8 A Brief Note on SUTVA Violations

Another identifying assumption in this context is the stable unit treatment value assumption (SUTVA). This means that in order for my estimates to be unbiased, the rally would have exclusively affected Charlottesville and not spilled into other geographies. This assumption is likely violated inasmuch as the rally received extensive media attention, plausibly resulting in a broader generalization process wherein job seekers became more inclined to presume anti-diversity stances among employers in locations besides Charlottesville. It is likely, however, that the bias resulting from this violation would attenuate estimated treatment effects inasmuch as, on balance, employers in these other locations, especially those in my alternative control subsets (college towns, employers in Democratic-leaning areas, and institutions of higher education), would similarly tend to rely on left-leaning workers and would thus similarly face incentives to combat any spillover anti-diversity presumptions of anti-diversity with countervailing pro-diversity claims.

5 Examining the Proposed Mechanism

As outlined in the theory section, I propose that this sizeable increase in employers' use of pro-diversity claims is a strategic, defensive response on the part of Charlottesville employers meant to countervail the anti-diversity stigma they incurred as a result of their proximity to the Unite the Right Rally. Nevertheless, outside of the qualitative anecdotes I present in Section 3.2, I

cannot *directly* observe whether this was in fact employers' motivation for adopting pro-diversity claims. It is unlikely, moreover, that my theorized mechanism was the *only* motivation for this behavior. In this section, however, I present a range of empirical patterns that are consistent with my proposed mechanism and less consistent with alternative explanations including isomorphic processes, increased salience of diversity issues, upper echelon attitudes, or efforts to comply with the Equal Opportunity Act. Table 3 summarizes these alternative explanations and empirical patterns I present that are less consistent with these explanations.

5.1 Examining Effects by Headquarters Location

If employers' adoption of pro-diversity claims was in fact strategic, as opposed to the result of a more general isomorphic process or simply a manifestation of the values of top managers, it should have been greatest among employers most at risk of incurring anti-diversity stigma by association. To test this, I examine whether adoption was larger for employers not only located in Charlottesville, but also headquartered in Charlottesville. This differentiates, for example, UVA and the CFA Institute, which are based in Charlottesville, from Bank of America, which has locations in Charlottesville but is headquartered in Charlotte, North Carolina. Since Charlottesville-based employers are more socially proximate to Charlottesville, they plausibly face greater risk of incurring stigma by association, and, accordingly, face an outsized incentive to combat this stigma with countervailing pro-diversity claims. I examine this by estimating

$$\begin{aligned} Diversity_{i,c,t} = & \beta_1 CVilleBased_{i,c,t} \times Charlottesville_{i,c,y} \times PostRally_{i,c,y} + \\ & \beta_2 CVilleBased_{i,c,t} + \beta_3 CVilleBased_{i,c,t} \times Charlottesville_{i,c,y} + X_{i,c,t} + \lambda_{i,c} + \lambda_t + \epsilon_{i,c,t} \end{aligned} \quad (4)$$

where *CvilleBased* is a dummy equal to one if the job posting was made by an employer based in Charlottesville. In this case β_1 is the coefficient of interest. These results are presented in Model 1 of Table 4, which shows that the effect of the Unite the Right rally on the use of pro-diversity claims in job postings was significantly larger for Charlottesville-based employers ($\hat{\beta}_1 = 0.176$, $p < 0.001$). To examine whether this effect may have been driven entirely by employers such as UVA that posted many jobs, I repeat this analysis using the balanced panel, estimating a version of Model 3 where I interact the treatment dummy with *CvilleBased*. These results are reported in Model 1 of Table A5 and again show that the effect is significantly larger for employers that are headquartered

Table 3: This table summarizes alternative explanations and accompanying empirical analysis.

Alternative Explanation		Patterns Inconsistent with Explanation
(1) <i>Isomorphism</i>	The effect chiefly reflected a general imitative process or an emergent norm of making pro-diversity claims that diffused across Charlottesville’s employers.	Adoption of pro-diversity claims was not exceptionally widespread (Section 5.6), but was concentrated in instances where firms faced greater incentives to do so, including where job seekers were more likely to presume employers held anti-diversity positions (Sections 5.1 & 5.3) and where job seekers held stronger pro-diversity preferences (section 5.2).
(2) <i>Issue Salience</i>	The effect chiefly reflected the fact that issues of diversity were suddenly very salient to Charlottesville’s workers, incentivizing employers to appeal to these preferences.	The effect was larger when targeting job seekers <i>outside</i> of Charlottesville (Section 5.3), and the effect emerged after a delay and then grew rather than appearing suddenly, which would be more consistent with a sudden spike in salience (Section 5.5).
(3) <i>Upper Echelon</i>	The effect chiefly reflected the personal values of top managers and/or the fact that top managers were suddenly paying greater attention to diversity issues.	Holding constant top managers, the increase in diversity claims was greater in Charlottesville compared to other geographies (Section 5.4). The delayed effect, as well as supplementary interviews, are consistent with the idea that increased use of pro-diversity claims reflected internal deliberation regarding strategic implications of stance taking rather than unilateral expression of top managers’ values (Section 5.5).
(4) <i>EO Compliance</i>	The effect was chiefly a byproduct of firms’ efforts to signal compliance with the Equal Opportunity Act by including equal opportunity claims in their jobs postings. Because equal opportunity claims often include pro-diversity claims, there was a mechanical increase in pro-diversity claims.	The increase in equal opportunity claims was much smaller in magnitude than the increase in pro-diversity claims (Section 5.7).

in Charlottesville ($\hat{\beta}_1 = 0.056$, $p < 0.01$).

5.2 Examining Effects by Target Applicant Pro-Diversity Preferences

As an additional test to arbitrate between my theorized strategic explanation and alternative, non-strategic explanations, I examine whether the effect was largest where prospective applicants held the strongest pro-diversity preferences. I test this by leveraging the fact that bachelor’s-holding job-seekers, on average, held significantly stronger pro-diversity views than their non-bachelor’s holding counterparts (Parker et al. 2019). This education-based division was starkly visible in the 2016 presidential election where non-college graduates went 50%/43% for Donald Trump, whose campaign became synonymous with anti-diversity positions and who was famously reticent to condemn the Unite the Right Rally, while college graduates went 57%/36% for Clinton (An Examination 2018).

I test whether the effect was larger in job postings targeting bachelor’s-holding workers by re-estimating Equation 4 but replacing *CvilleBased* with a dummy equal to one if the posting indicated that applicants must hold at least a bachelor’s degree. These results are presented in Model 2 of Table 4 and show that the effect was significantly larger ($\hat{\beta}_1 = 0.028$, $p < 0.001$) for

Table 4: Consistent with the proposed mechanism, adoption of pro-diversity claims following the rally was larger where targeted job seekers were more likely to generalize the rally’s anti-diversity stances and where targeted job seekers held stronger pro-diversity preferences.

	(1)	(2)	(3)
Charlottesville × Post-Rally × Charlottesville Headquarters	0.176*** (0.005)		
Charlottesville × Post-Rally × Requires Bachelor’s Degree		0.028*** (0.002)	
Charlottesville × Post-Rally × Moving Expenses Covered			0.165*** (0.009)
Year-Month Fixed Effects	✓	✓	✓
Employer-County Fixed Effects	✓	✓	✓
Job-Level Controls	✓	✓	✓
County-Level Controls	✓	✓	✓
Observations	55,668,345	55,668,345	55,668,345

Notes: The units of analysis in all models are job postings. “Charlottesville × Post-Rally” is a dummy variable equal to one for job postings made in the Charlottesville MSA after August 11, 2017. The outcome in all models is a dummy variable equal to one if the job posting included a pro-diversity claim. All models are versions of Equation 4. “Charlottesville Headquarters” is a dummy equal to one if the posting was made by an employer headquartered or based in Charlottesville. “Requires Bachelor’s Degree” is a dummy equal to one if the posting indicated that applicants must hold at least a bachelor’s degree. “Moving Expenses Covered” is a dummy equal to one if the posting included a commitment to cover moving expenses. For brevity, the table excludes the main effects and only shows the estimated interaction effect ($\hat{\beta}_1$). [†] $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

these bachelor’s-degree-requiring jobs. I again repeat this at the employer level using the balanced panel. These results are reported in Model 2 of Table A5 and show the effect was significantly larger ($\hat{\beta}_1 = 0.046$, $p < 0.001$) among employers that were in the top quartile of employer-county dyads in terms of the share of their jobs that required a bachelor’s degree, which corresponds to employers for which more than 39% of jobs required a bachelor’s degree.

5.3 Examining Effects by Geographic Scope of Recruiting

An alternative strategic explanation for the increased use of pro-diversity claims is that the main effects simply reflected firms’ efforts to appeal to Charlottesville’s workers for whom, due to their proximity to the rally, issues of diversity had suddenly become very salient. If this were the case, the effects should be relatively larger in instances where the geographic scope of recruiting was largely limited to Charlottesville and smaller in those instances where employer sought applicants outside of Charlottesville who lacked this salience-inducing proximity. My proposed explanation, in contrast, would suggest that these effects would be largest in instances where recruitment efforts focused on prospective employees *outside* of Charlottesville. This is because those outside of Charlottesville were less aware that the rally’s agitators were mostly from outside of Charlottesville

and were thus more susceptible to the generalization process I theorize above. The plausibility of this variation in perceptions was substantiated by multiple Charlottesville-based interviewees who expressed frustration that, due to the rally, their community had, in the eyes of outsiders, become inaccurately associated with anti-diversity positions.

To examine this possibility, I estimated heterogeneous treatment effects with respect to whether or not the job posting included a commitment to cover moving expenses. It is highly plausible that postings that featured these commitments were more likely to be seeking applicants outside of Charlottesville compared to those that did not. I re-estimate Equation 4 but replace *CvilleBased* with a dummy equal to one if the job posting included a commitment to cover moving expenses. These results are presented in Model 3 of Table 4 and show the effect in these cases is significantly larger ($\hat{\beta}_1 = 0.165$, $p < 0.001$). Again, I repeat this analysis at the firm level using the balanced panel. These results are reported in Model 3 of Table A5 and show the effect was larger ($\hat{\beta}_1 = 0.242$, $p < 0.10$) among employers for whom over half of their job postings included a commitment to cover moving expenses.³ Besides addressing this alternative salience explanation, this test is further evidence consistent with a strategic, rather than isomorphic or upper-echelon perspective, inasmuch as the adoption of pro-diversity claims was again larger where employer faced greater incentive to do so.

5.4 Examining Intrafirm Geographic Variation

Next, I conduct another test to arbitrate between my theorized strategic explanation and an upper echelon explanation that would attribute these patterns to an expression of top managers' pro-diversity beliefs. I examine whether, holding constant top managers, a given employer was more likely to adopt pro-diversity claims in its Charlottesville job postings versus its postings in other locations. I do so by estimating a version of Equation 1 where I include company-year fixed effects (in addition to month and company-county fixed effects). Note that this means postings from employers that ever only post in one county do not contribute directly to the estimated effect, which is why I do not feature this as my main analysis. These estimates are reported in Model 1 of Table 5 and show that in this case the effect is smaller but still significant ($\beta = 0.010$, $p < 0.01$).

³I use this threshold rather than the top quartile threshold I use for the firm-level BA-requirement analysis because, due to the rarity of commitments to cover moving expenses, employers in the top quartile include these commitments in only 3% of job postings.

This is consistent with the idea that employers selectively adjusted their recruiting language to countervail the anti-diversity stigma that had arisen uniquely in Charlottesville. As reported in Table A6, I repeat these analysis at the employer level and find a relationship that is of similar size ($\hat{\beta}_1 = 0.011$, $p = 0.16$) but not statistically significant.

5.5 The Delayed Effect

As shown in the posting-level and employer-level analysis in both descriptive statistics (see Figures 2 and 3) and in difference-in-difference estimates (see Figures 4 and A6 as well as Table A4), the increased use of pro-diversity claims did not arise immediately following the rally, but began to materialize after approximately six months and grew throughout the post-treatment period. If the effect had been driven primarily by an increase in the salience of diversity issues within Charlottesville or arisen due to newfound desire on the part of top managers to express their pro-diversity values, it would have been more likely to materialize immediately before stabilizing or tapering down as the issue decreased in salience and/or top managers' attention was drawn to other issues. The delayed effect, in contrast, is more consistent with the idea that the increased use of pro-diversity claims represented a calculated response on the part of employers. For one, the delay is consistent with a learning process wherein employers did not immediately appreciate the difficulties the rally had created for their recruiting efforts, but as prospective employees consistently mentioned the rally, began making pro-diversity claims. This learning process was substantiated by multiple interviewees who described their initial surprise when, following the rally, prospective employees began inquiring regarding employers' stances on diversity. Second, the delay is consistent with the idea that there was deliberation and debate preceding employers' decisions to adopt pro-diversity stances. This too, was substantiated by multiple interviewees.

5.6 Significant Non-Adoption

The increased use of pro-diversity claims represented a substantial shift relative to the pre-treatment use rate and elevated Charlottesville from the 64th to the 99th percentile of MSAs in terms of the share of postings with pro-diversity claims. Nevertheless, by the end of the study period, a significant majority (70%) of postings in Charlottesville continued to *not* feature pro-diversity-claims. This pattern also provides further support for my theorized mechanism. Universal or near-universal adoption of pro-diversity claims would be more consistent with an isomorphic

Table 5: Additional analysis shows patterns consistent with the theorized mechanism.

Outcome:	(1) Pro-Diversity	(2) EO Claim	(3) “Charlottesville”
Charlottesville $\times$ Post-Rally	0.010** (0.004)	0.035*** (0.004)	-0.018*** (0.001)
Year-Month Fixed Effects	✓	✓	✓
Employer-County Fixed Effects	✓	✓	✓
Employer-Year-Month Fixed Effects	✓		
Job-Level Controls	✓	✓	✓
County-Level Controls	✓	✓	✓
Observations	53,921,070	55,668,345	55,668,345

Notes: The units of analysis in all models are job postings. “Charlottesville $\times$ Post-Rally” is a dummy variable equal to one for job postings made in the Charlottesville MSA after August 11, 2017. Model 1 is identical to Equation 1 with the difference that it includes employer-year-month fixed effects. The outcome in Model 1 is a dummy variable equal to one if the job posting includes a pro-diversity claim. Models 2 and 3 differ from Equation 1 only in terms of the outcome variable. The outcome in Model 2 is a dummy variable equal to one if the job posting includes an equal opportunity claim. The outcome in Model 3 is a dummy variable equal to one if the job posting includes the word “Charlottesville.” $^{\dagger}p < 0.10$, $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

process wherein the Unite the Right rally caused a general diffusion of the practice or begat a general norm among Charlottesville employers to use these claims. Instead, I demonstrate that the adoption of the practice is selective and concentrated in instances where employers had the greatest incentive to do so, including instances where they (1) were more likely to be associated with the Unite the Right rally and (2) they were targeting job seekers with stronger pro-diversity preferences.

5.7 Examining Equal Opportunity Claims

A separate alternative explanation is that this increase in pro-diversity claims mostly resulted from efforts to signal compliance with the the Equal Opportunity Act (EOA), which prohibits employers from discriminating based on race, color, sex, religion, or national origin (Dobbin 2009). Employers may have feared that the Unite the Right Rally made employees, job seekers, regulators, or even the private bar more inclined to seek legal action for perceived discrimination, a possibility consistent with research illustrating social activism often shapes broader regulatory outcomes (Fremeth et al. 2022). Since EO claims are often accompanied by pro-diversity claims, an increase in equal opportunity claims motivated by a desire to signal compliance with the EOA may have mechanically led to a increase in pro-diversity claims. To examine this possibility, I re-estimate Equation 1 but replace the outcome *Diversity* with a dummy that is equal to one if the job posting includes an EO claim.⁴ Model 2 of Table 5 shows there was a significant increase in EO claims,

⁴I coded a job postings as featuring an EO claim if it featured the phrase “equal opportunity,” “equal employment

Table 6: The Unite the Right rally was associated with a newfound wage premium in Charlottesville’s job postings, but this premium was significantly lower among job postings that featured pro-diversity claims.

Outcome:	(1) Annual Salary	(2) Log Annual Salary	(3) Annual Salary	(4) Log Annual Salary
Charlottesville × Post-Rally	2500.027*** (503.652)	0.038*** (0.007)	2564.113*** (527.743)	.045*** (0.007)
Pro-Diversity Claim			3126.015*** (338.612)	0.054*** (0.004)
Charlottesville × Pro-Diversity Claim			-2827.620*** (499.297)	-0.063*** (0.006)
Year-Month Fixed Effects	✓	✓	✓	✓
Employer-County Fixed Effects	✓	✓	✓	✓
Job-Level Controls	✓	✓	✓	✓
County-Level Controls	✓	✓	✓	✓
Observations	7,028,116	7,028,116	7,028,116	7,028,116

Notes: The units of analysis in all models are job postings. “Charlottesville × Post-Rally” is a dummy variable equal to one for job postings made in the Charlottesville MSA after August 11, 2017. The outcome in Models 1 and 3 is the minimum annual salary advertised in the job posting. The outcome in models 2 and 4 is the log of the minimum annual salary advertised in the job posting. All models are estimated using ordinary least squares with robust standard errors clustered on county. The relatively low sample size in these models follows from the fact that many jobs do not list annual salaries. [†] $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

but that this effect was much smaller ($\hat{\beta}=0.035$) than the overall increase in pro-diversity claims ($\hat{\beta}=0.086$). Model 2 of Table A6 reports this same analysis at the employer-level and shows an effect that is statistically insignificant and even smaller relative to the employer-level estimate of the increase in pro-diversity claims.

5.8 Mentioning Charlottesville in Job Postings

Finally, I examine changes in the extent to which job postings featured the word “Charlottesville.” Although this pattern is somewhat distinct from my theory of countervailing claim making, a decrease in mentions would be consistent with my broader argument that firms recognized the liability created by their association with Charlottesville and sought to downplay this association in their recruiting efforts. Consistent with this idea, Model 3 in Table 5 shows that there was in fact a significant decrease ($\hat{\beta} = -0.017$, $p < 0.001$) in the extent to which Charlottesville area employers included the word “Charlottesville” in their job postings. Model 5 of Table A6

shows this effect is even larger ($\hat{\beta} = -0.041$, $p < 0.001$) when analyzing at the employer level.

6 Supplementary Wage Analysis

As a supplementary exercise, I examine whether the rally may have made it more difficult for Charlottesville’s employers to attract workers and whether these difficulties were ameliorated by pro-diversity claims. To examine whether the rally made it more difficult to attract workers, I examine whether it affected the wages offered by Charlottesville’s employers. A newfound wage premium would be consistent with the idea that employers in Charlottesville used higher wages to compensate for the fact that that job seekers became less inclined to work for them due their stigma by association with the rally. To estimate the wage premium, I re-estimate Equation 1 but replace the outcome *Diversity* with the annual salary listed in the job posting. Where the annual salary is expressed as a range, I use the lower bound of this range. These results are presented in Model 1 of Table 6 and show that the rally is associated with a wage premium of \$2,442.

I next examine whether this premium was lower in instances where employers used pro-diversity claims. A lower premium in the presence of pro-diversity claims would be consistent with the idea that these claims assuaged prospective employees’ concerns regarding the employers’ stances on diversity, allowing employers to substitute them for the higher wages they would otherwise pay to compensate for these concerns. In estimating this relationship, it is important to acknowledge that since pro-diversity claims were not randomized, it is possible that associations are caused by unaccounted-for third factors. The continued inclusion of a rich set of fixed effects and controls mitigates but does not eliminate this risk. It is also important to note that I cannot observe the relationship between different posting characteristics (*i.e.*, salary offered, diversity claims) and whether the employer successfully filled the advertised position. With these words of caution, I re-estimate a version of Equation 4 where I replace the outcome variable *Diversity* with the annual salary and replace the interacted variable *CVilleBased* with *Diversity*. These results are presented in Model 3 and show that the use of pro-diversity claims is in fact associated with a significant reduction of the wage premium. Table A8 show these patterns are robust to the exclusion of postings by UVA.

opportunity,” or “eo” as a stand-alone abbreviation.

7 Conclusion and Discussion

This paper provides a novel strategic explanation as to why firms position on divisive sociopolitical issues. I draw on theories of stigma by association to explain why firms' mere proximity with controversial political actors may lead stakeholders to presume firms silent on sociopolitical issues are misaligned with their sociopolitical preferences. Firms, in turn, combat these presumptions of misalignment by eschewing silence and making countervailing sociopolitical claims. I substantiate this theory in the context of employee recruitment following the 2017 Unite the Right white supremacist rally in Charlottesville, Virginia. I show that Charlottesville's employers combated presumptions that they shared the demonstrators' *anti*-diversity positions by adopting countervailing *pro*-diversity claims in their online job postings. Following the rally, the Charlottesville MSA rose from the 64th to 99th percentile in terms of the share of MSA job postings featuring pro-diversity claims. Consistent with my theory, however, I show that the adoption of pro-diversity claims was not exceptionally widespread, but was concentrated in instances where employers targeted prospective applicants who (1) were plausibly more inclined to generalize the white supremacists' anti-diversity positions and (2) held stronger pro-diversity preferences. I show, moreover, that the rally was associated with a newfound wage premium for Charlottesville-area job postings, but that this premium was lower for job postings that featured pro-diversity claims.

7.1 Strategic Sociopolitical Positioning

This paper advances nascent literature regarding *strategic* sociopolitical positioning, or the idea that firms' sociopolitical positioning represent calculated appeals to stakeholders. It differs from related work suggesting that firms may use social claims to combat negative evaluations arising from their *own* actions (Elsbach 1994, Bansal and Clelland 2004, McDonnell and King 2013) or to differentiate themselves from their competitors (Mohliver et al. 2021). Instead, I demonstrate that certain firms strategically differentiate themselves from *non-firm* political actors with whom they are associated but who are viewed unfavorably by key stakeholders. And whereas a growing body of both empirical (Burbano 2021, Hou and Poliquin 2022, Wowak et al. 2022) and theoretical (Mohliver et al. 2021, Melloni et al. 2023) work has sought to identify when sociopolitical positioning benefits firms, to my knowledge this paper provides the first empirical evidence of firms strategically claiming sociopolitical positions. Further elaboration of this strategic perspective

requires additional empirical studies that similarly treat firm behavior, rather than stakeholder reactions, as the key outcome of interest. In extending this research, future work may seek to theorize how strategic motivations, such as those outlined here and those outlined by Mohliver et al. (2021) and Melloni et al. (2023), interact with expressive motivations, such as those outlined by Hambrick and Wowak (2021). For example, it may be that top managers generally prefer to express their political beliefs, but carefully limit these expressions to instances where they are likely to benefit the firm.

By examining sociopolitical claims in the context of job postings, rather than in the public statements commonly examined in past research (Chatterji and Toffel 2019, Hou and Poliquin 2022), this paper also demonstrates how a firm’s positioning can be subtle and even tailored to particular stakeholder audiences. Future work might compare firms’ job posting language with messages to consumers, shareholders, or other stakeholders. Evidence of a focal firm making contrasting claims that correlate with stakeholders’ contrasting preferences would further enrich the strategic view of sociopolitical stance-taking. Such research will benefit by looking to novel data. Surveys by Hertel-Fernandez (2018), for example, suggest that in the United States nearly 50% of managers attempt to mobilize their workers politically and 25% of workers have had a manager try to mobilize them. Scholars able to analyze the internal communications whereby these mobilization efforts occur would provide important insights into sociopolitical positioning. More generally, a fuller account of strategic positioning might account for if and or when stakeholders’ reactions to perceptions of sociopolitical alignment is fundamentally “taste-based” (i.e., due simply to ideological aversion) or “statistical” (i.e., due to the belief that the sociopolitical position signals additional firm qualities). This might elucidate, for example, the extent to which firms need to back sociopolitical claims with evidence that they behave in a way consistent with these claims.

Future work may also examine how these practices evolve in the context of growing political polarization. On one hand, growing polarization may generate ever-stronger negative reactions to firms’ sociopolitical stance taking, which would seem to *dis-incentivize* stance taking. On the other hand, growing polarization may lead to greater sorting by political ideology, a possibility suggested by Bermiss and McDonald (2018). Sorting by political ideology would, it seems, *incentivize* sociopolitical positioning, since there would be greater political homogeneity and thus less risk of alienating those “on the other side.” The result of polarization may thus be an environment

where the shrinking number of firms with politically heterogeneous stakeholder bases deliberately maintain silence while the growing number of firms with politically homogeneous stakeholder bases increasingly speak up, especially when doing so combats presumptions that they are misaligned with these stakeholders’ sociopolitical preferences.

7.2 Human Capital Strategy

This paper makes secondary contributions to work on non-pecuniary human capital strategy. Prior research demonstrates how non-pecuniary firm characteristics can enhance potential applicants’ inclination to apply (Abraham and Burbano 2021), increase the amount of effort exerted by workers (Carpenter and Gong 2015, Burbano 2021), improve employee retention (Bode et al. 2015, Carnahan et al. 2017, Bermiss and McDonald 2018) and even reduce knowledge spillovers (Flammer and Kacperczyk 2019). To date, however, this literature is largely agnostic as to whether firms cultivate these characteristics in an attempt to reap these benefits. To my knowledge, this paper provides the first evidence of firms strategically adjusting their human capital strategy in an attempt to align with workers’ exogenously-given, non-pecuniary preferences. Moreover, although I provide preliminary evidence that employers tailor their pro-diversity language, and seem to do so in strategic ways, future work could do more to theorize and test the conditions under which employers standardize or tailor diversity-related recruitment strategies as well as the effects of these actions. Prior work, for example, illustrates how multi-location firms can benefit by delegating product development and advertising responsibilities to local offices who are better-positioned to respond to the tastes of local markets. Are there analogous benefits to delegating diversity or other political strategies to local human resources functions who may be similarly well-positioned to appreciate the political preferences within a given labor pool?

7.3 Diversity Strategy

By operationalizing sociopolitical positioning with employers’ use of pro-diversity language, this paper also relates to literature on workplace inequality and diversity in recruiting (Adam Cobb 2016, Tolbert and Castilla 2017, Dobbin and Kalev 2021, Hurst et al. 2022). Much of this research has examined organizational practices relating to *current* employees, such as diversity training, formalization of promotion criteria, and grievance systems (Dobbin 2009, Kalev et al. 2006, Castilla 2015). Diversity claims, in contrast, target *prospective* employees, with potentially large impacts on

who applies to and, ultimately, works at the firm. And while a number of lab and field experimental studies demonstrate that these claims may in fact enhance applicant pool diversity (Kang et al. 2016, Flory et al. 2021, Abraham and Burbano 2021), I provide, to my knowledge, the to-date most comprehensive estimates of the extent to which firms actually use these claims. I estimate that 12% of jobs in the US feature pro-diversity claims. Given the ease with which firms can include these claims as well as growing public and regulatory pressure for workplace diversity, why is this rate so low? Do employers eschew these claims to avoid accusations of hypocrisy? Do they fear they will be seen as too political? I, moreover, present findings consistent with the conclusion that employers perceived as hostile towards minorities may need to pay a wage premium, but that these perceptions and their accompanying wage costs can be reduced when employers make pro-diversity claims. Using the data and strategy for measuring pro-diversity claims that I present here, future work can address these questions and more fully illustrate when and to what effect employers use pro-diversity claims in their recruiting efforts.

7.4 Locational Stigma and Implications for Integrated Strategy

Finally, this paper advances research on stigma by association. Whereas past work demonstrates how stigmatizing generalization processes disseminate across organizations that share a parent organization (Jensen 2006, Piazza and Jourdan 2018), partnership (McDonnell et al. 2021), organizational form (Yue et al. 2013), or industry (Jonsson et al. 2009, Piazza and Perretti 2015, Naumovska and Lavie 2021), this paper considers how they may disseminate due to simply sharing a location with non-firm or even non-organizational actors. This possibility has significant implications for integrated strategy, or the idea that firms enhance the success of their market strategies by undertaking complementary non-market strategies (Holburn and Vanden Bergh 2014, Barber IV and Diestre 2019). When, for example, a firm opens offices or acquires a firm in a new location, it exposes itself to possible stigma arising from positions taken by political actors in that location. This study suggests conditions under which these location-based market strategies should be paired with sociopolitical claims in order to combat perceptions of sociopolitical misalignment among stakeholders.

These integrated strategies are visible in the U.S. in the case of corporate re-locations into right-leaning states. Firms including Toyota, Hewlett Packard, and Tesla have recently moved

headquarters from left-leaning states such as California to right-leaning Southern states such as Texas. These locations offer attractive tax advantages and a low cost of living. In entering these locations, however, firms risk acquiring sociopolitical stigma resulting from right-leaning positions established by, among other things, anti-trans bathroom bills and abortion bans. Underlining this tension, *Fortune* magazine reports that although these states provide “pro-enterprise climates,” they are “simultaneously rolling out [right-leaning] laws that are an automatic turnoff to many young, liberal tech workers and force companies into damage control mode” (Leonhardt 2021). Anecdotal, this “damage control” involves combating this *right*-leaning stigma with countervailing *left*-leaning sociopolitical claims (Pardes 2021, Gelles 2021). Future work might use this context to test whether firms systematically employ countervailing sociopolitical claims to combat sociopolitical stigma arising from mergers, acquisitions, or re-locations into right-leaning states.

7.5 A Note on Generalizability

The idiosyncratic nature of the Unite the Right rally raises questions regarding the generalizability of empirical results. Even while there is surprisingly widespread sympathy with many ideas core to white supremacy (New Poll 2017), dramatic manifestations such as the Unite the Right rally are rare. However, given the frequency with which politicians, legislatures, courts, electorates, and protesters take positions that are viewed unfavorably by large populations of stakeholders, the strategic behavior I theorize in this paper is plausibly ubiquitous. Numerous anecdotes suggest this is the case. In the wake of Texas’s Senate Bill 8, which codified a strongly pro-life stance on abortion, many Texas-based tech companies made countervailing commitments to women’s and reproductive rights (Pardes 2021). In the wake of Donald Trump’s victory in the 2016 presidential election, which many saw as revealing the ubiquity of anti-diversity sentiment in the U.S. electorate, many U.S. firms made countervailing commitments to racial, gender, and LGBTQ+ diversity (In the Trump Era 2017). These, and numerous similar contexts, however, present serious impediments credibly disentangling my theorized explanation from plausible alternative explanations. I overcome these impediments by exploiting idiosyncrasies of the Unite the Right Rally. Thus, similar to recent work by Favaron et al. (2022), I leverage idiosyncrasies of a somewhat unusual empirical context to credibly test a broadly generalizable theoretical mechanism. And while I consider the case of human capital and labor markets, my broader theoretical argument plausibly generalizes to

stakeholders in product and other factor markets who, past research shows, are similarly disposed to let their political preferences shape their decision to reward or spurn firms (McConnell et al. 2018, Chatterji and Toffel 2019).

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Online Appendix

for

Countervailing Claims:

Pro-Diversity Responses to Stigma by Association Following the Unite the Right Rally

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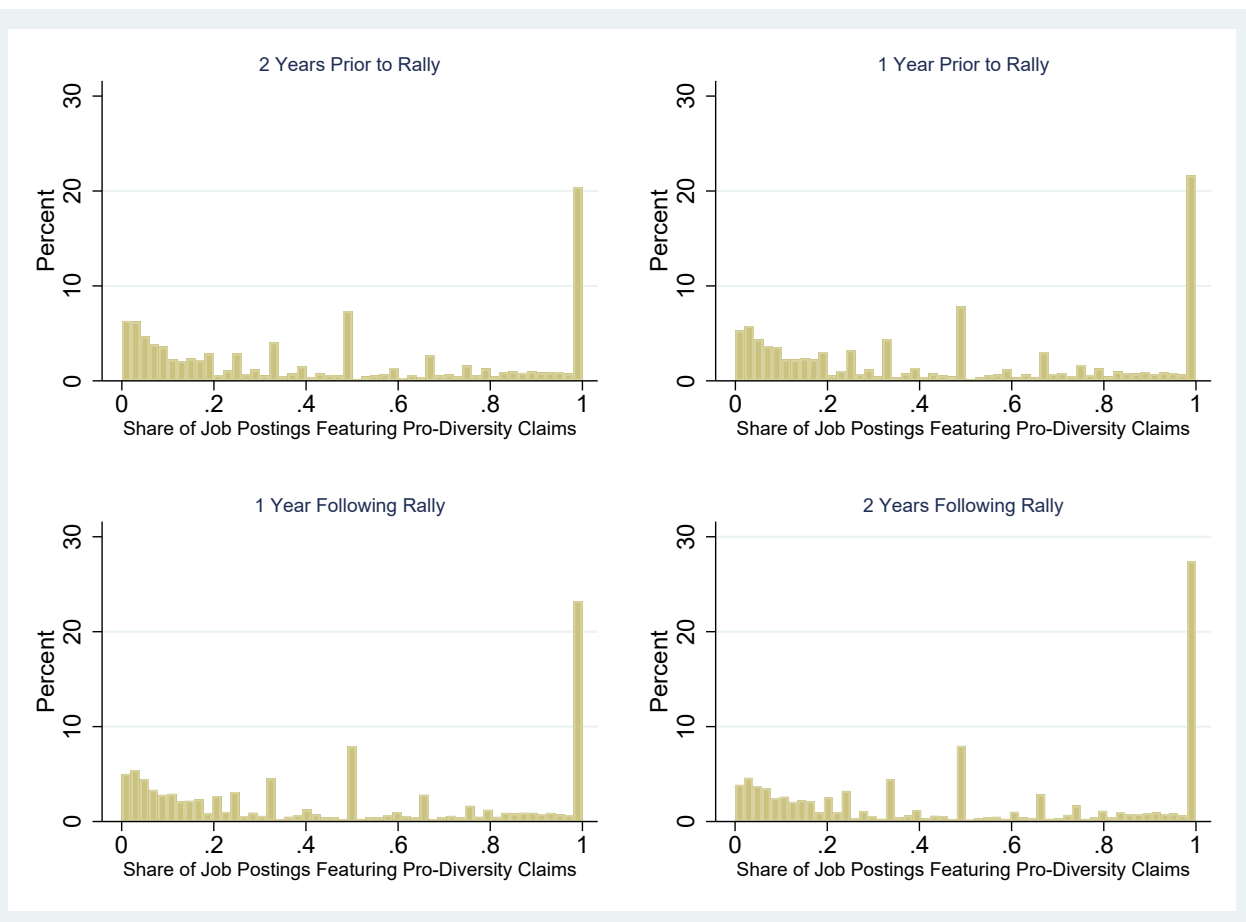
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Figure A1: A photo from the rally illustrates that pro-diversity claims represented a sociopolitical stance in opposition to the anti-diversity positions of white supremacists.



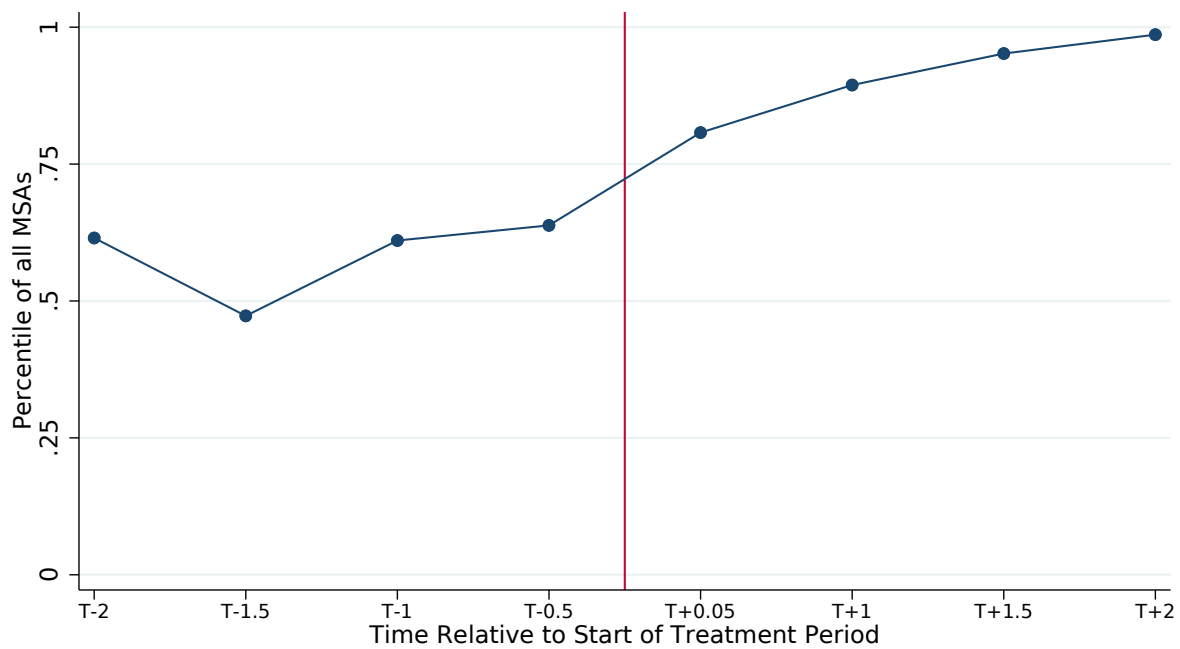
NOTES: This photo, featured in Reeve (2021), was taken in Charlottesville during the the Unite the Right Rally. It shows a prominently displayed banner bearing the pro-diversity claim “Diversity makes us stronger” above agitators bearing flags with anti-diversity symbols.

Figure A2: Among employer-county dyads that feature a pro-diversity claim in *any* of their job postings in a given year, the percentage of employer-county dyads that feature pro-diversity claims in *all* of their job postings never rose above 30% across the four years comprising the study's time frame.



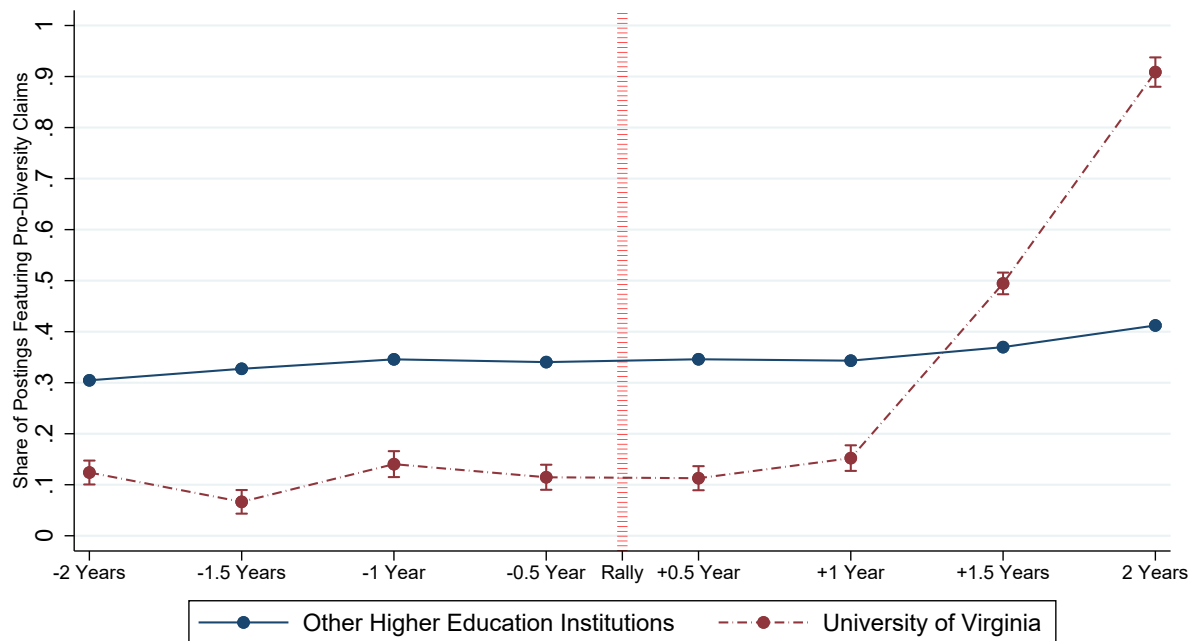
NOTES: This chart shows that, in most cases, employers that feature pro-diversity claims in their job postings do not do so in *all* of their job postings. Each of the four charts are histograms showing the percentage of employer-county dyads in the indicated year that featured pro-diversity claims in a given share of their job postings. Only employers that post more than one job in a given year are included, since only these can possibly vary their use of pro-diversity claims across postings. The sample is limited to employers that featured a pro-diversity claim in at least one job posting in that year. The purpose of this figure is to show that there is considerable variation in the extent to which employers feature pro-diversity claims in their job postings. If employers mostly make one-off decisions to include pro-diversity claims in *all* of their job postings, then all or most of the mass in these figures would be at 1 (*i.e.*, the far-right of the graph). Instead, the mass at 1 never rises above 30% and in the first three years is below 25%.

Figure A3: Following the rally, the Charlottesville MSA rose to the 99th percentile of all MSAs in terms of the share of job postings that featured pro-diversity claims.



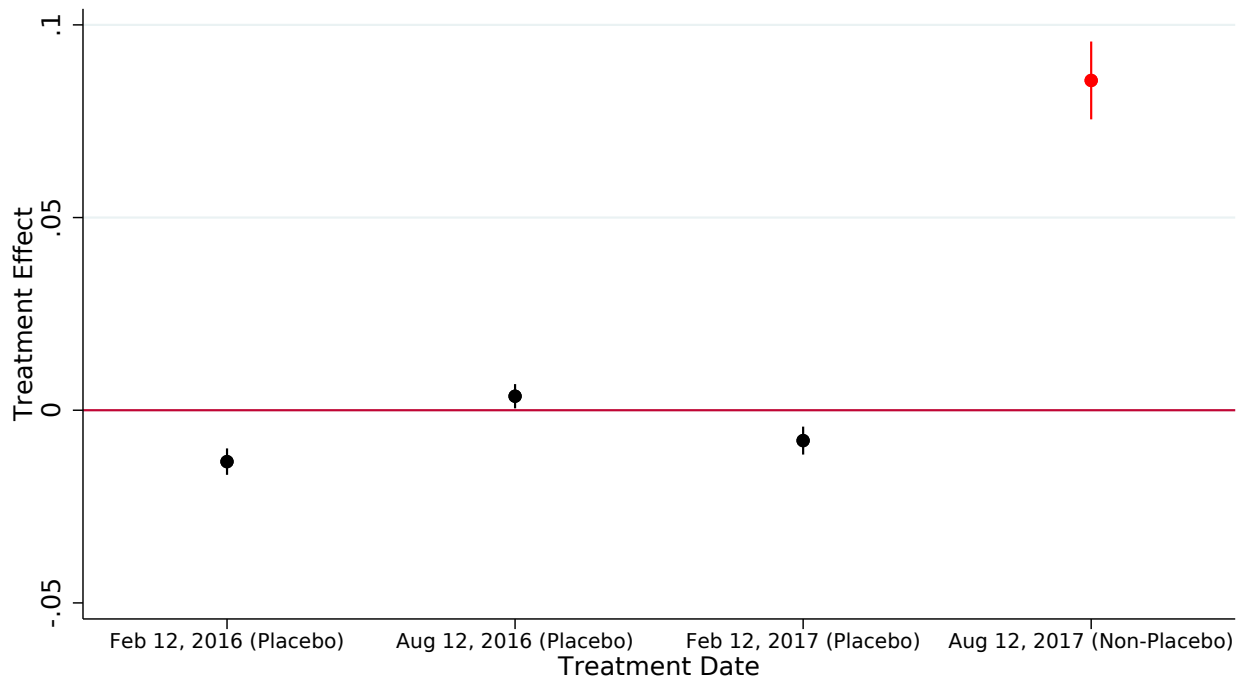
NOTES: Each point indicates, for the given 6-month period, the Charlottesville MSA's percentile relative to all other MSAs in terms of the share of job postings that featured pro-diversity claims.

Figure A4: Compared to job postings at other higher education institutions, the share of job postings made by the University of Virginia that featured pro-diversity claims increased significantly following the Unite the Right rally.



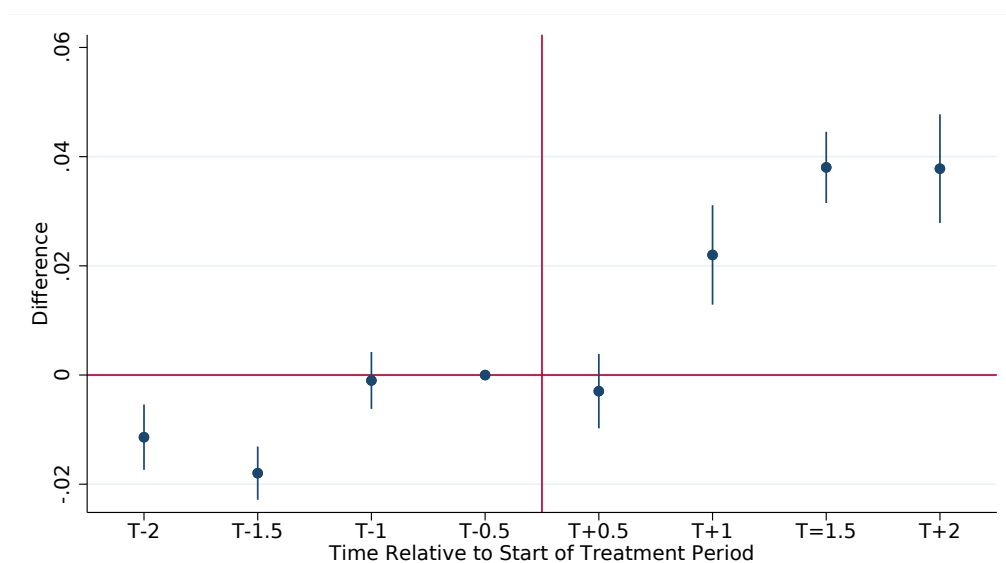
NOTES: Points indicate the fraction of job postings by year that included pro-diversity claims for the University of Virginia versus other higher education institutions in the United States. The vertical dashed line marks the division between the pre and post-rally period. Points are accompanied by 95% confidence intervals, but given high precision these are not visible on the “Other Higher Education Institutions” points.

Figure A5: The treatment effect estimated at the true treatment threshold (August 12, 2017) is significantly larger than, and in the opposite direction from two of the three, treatment effects estimated at placebo thresholds six months, one year, and eighteen months before treatment.



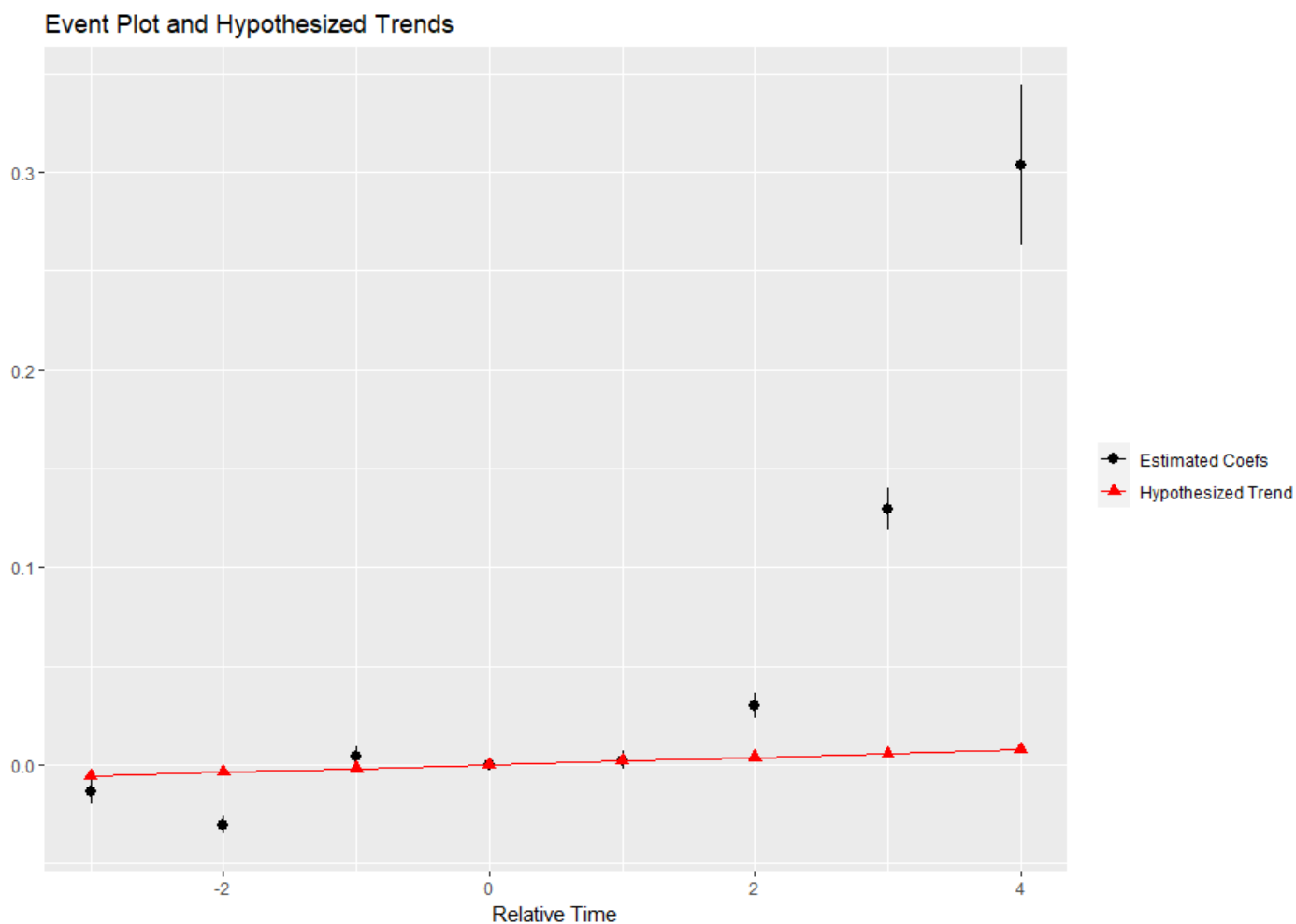
NOTES: Points are estimates of β from Equation 1, which differ in terms of the definition of the treatment threshold and sample of job postings. The first three points are for the placebo treatment thresholds labelled on the x-axis and exclude all observations in the post-shock period (in other words, include only those job postings in the two years leading up to the rally). The estimate on the far right, marked in red, is the estimated treatment effect with the full sample at the true treatment threshold.

Figure A6: Excluding job postings by the University of Virginia, dynamic difference-in-differences estimates of δ_k from Equation 2 show the estimated effect grew throughout the post-treatment period.



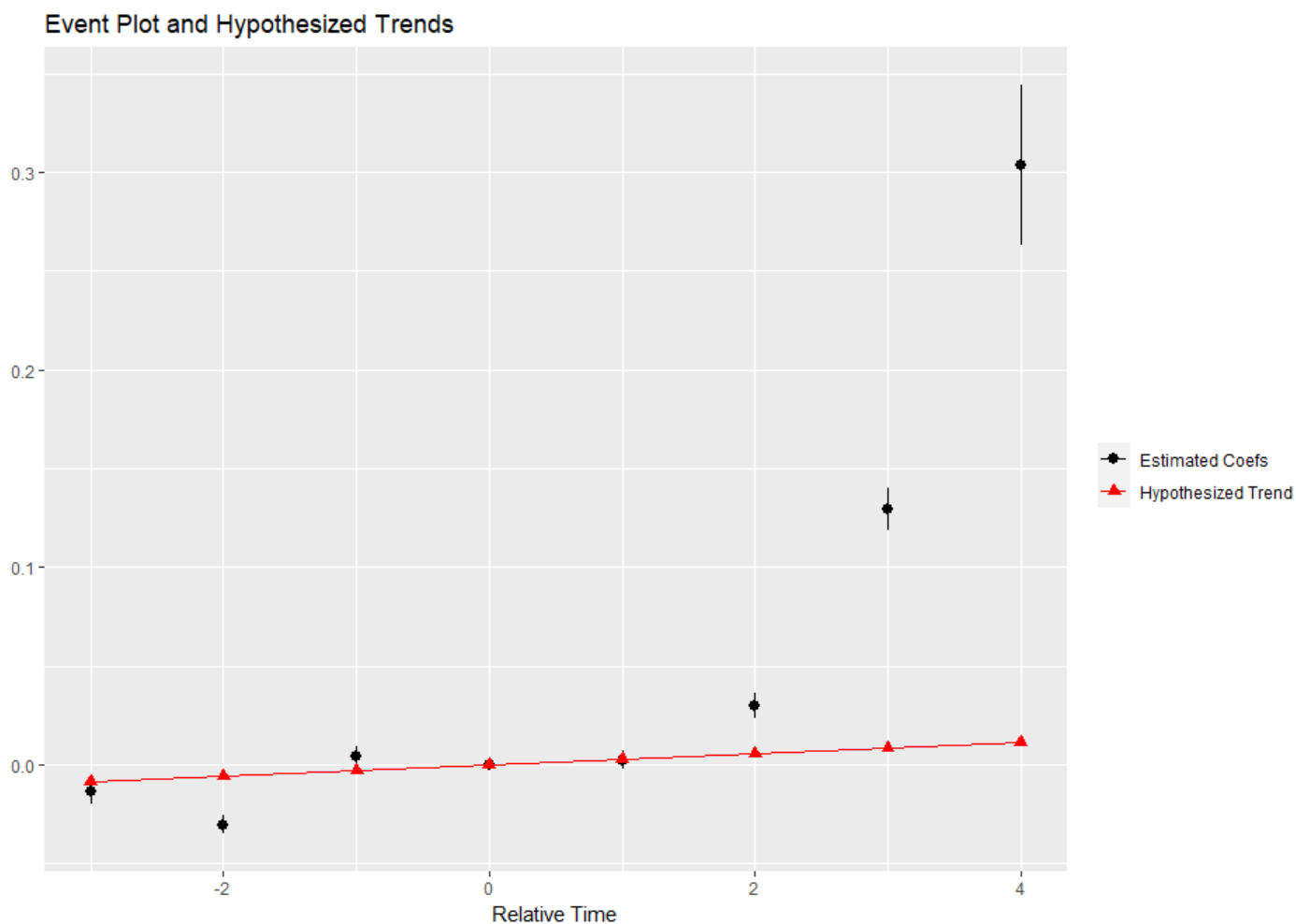
NOTES: This figure is analogous to Figure 4 but excludes postings by UVA. Each point represents the estimates of δ_k from Equation 2, which represent the difference in predicted probability that, in the indicated time period, a job posting in Charlottesville versus a job posting outside of Charlottesville features a pro-diversity claim relative to this difference in the reference time period (δ_0). The reference time period is the six month period preceding the Unite the Right Rally (February 12, 2017 - August 11, 2017). Bars indicate 95% confidence intervals. The region to the right of the vertical center line indicates the post-shock period.

Figure A7: Robustness analysis from Roth (2021) shows event-style estimates relative to the hypothetical non-zero pre-trend detectable at the 50% power level.



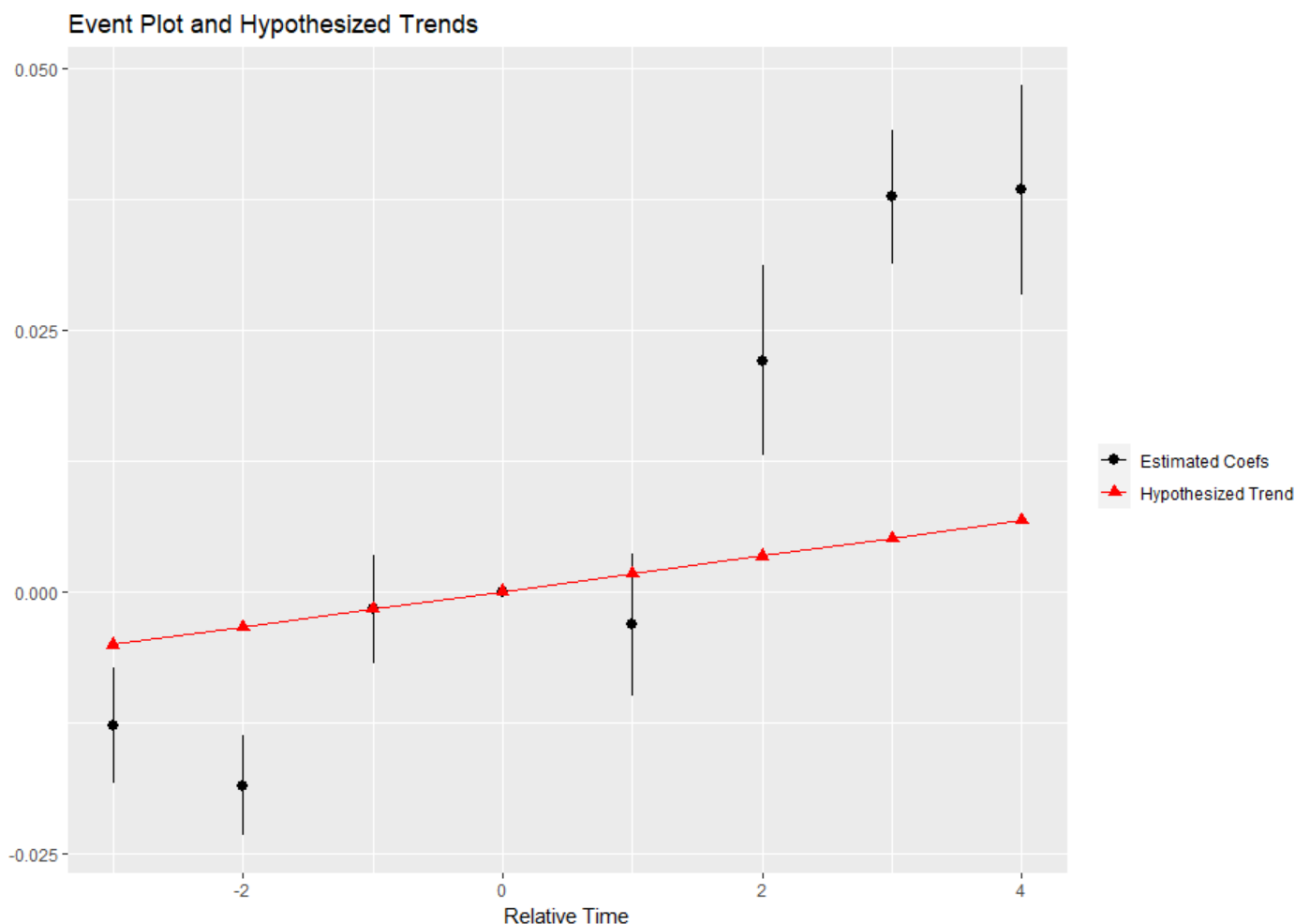
NOTES: The circular points and associated confidence intervals are the same time-specific difference-in-differences estimates presented in Figure 4. The triangular points and accompanying trend line indicates the hypothetical non-parallel pre-trend that would be detected at a power level of 50%. This is generated using the methods and software presented in Roth (2021). This line does not cross the 95% confidence intervals of the positive effects estimated in the post-treatment period (δ_2 , δ_3 , and δ_4 , or the final three points on the right), which is consistent with the conclusion that these estimates are robust to a violation of parallel pre-trends of this size.

Figure A8: Robustness analysis from Roth (2021) shows event-style estimates relative to the hypothetical non-zero pre-trend detectable at the 80% power level.



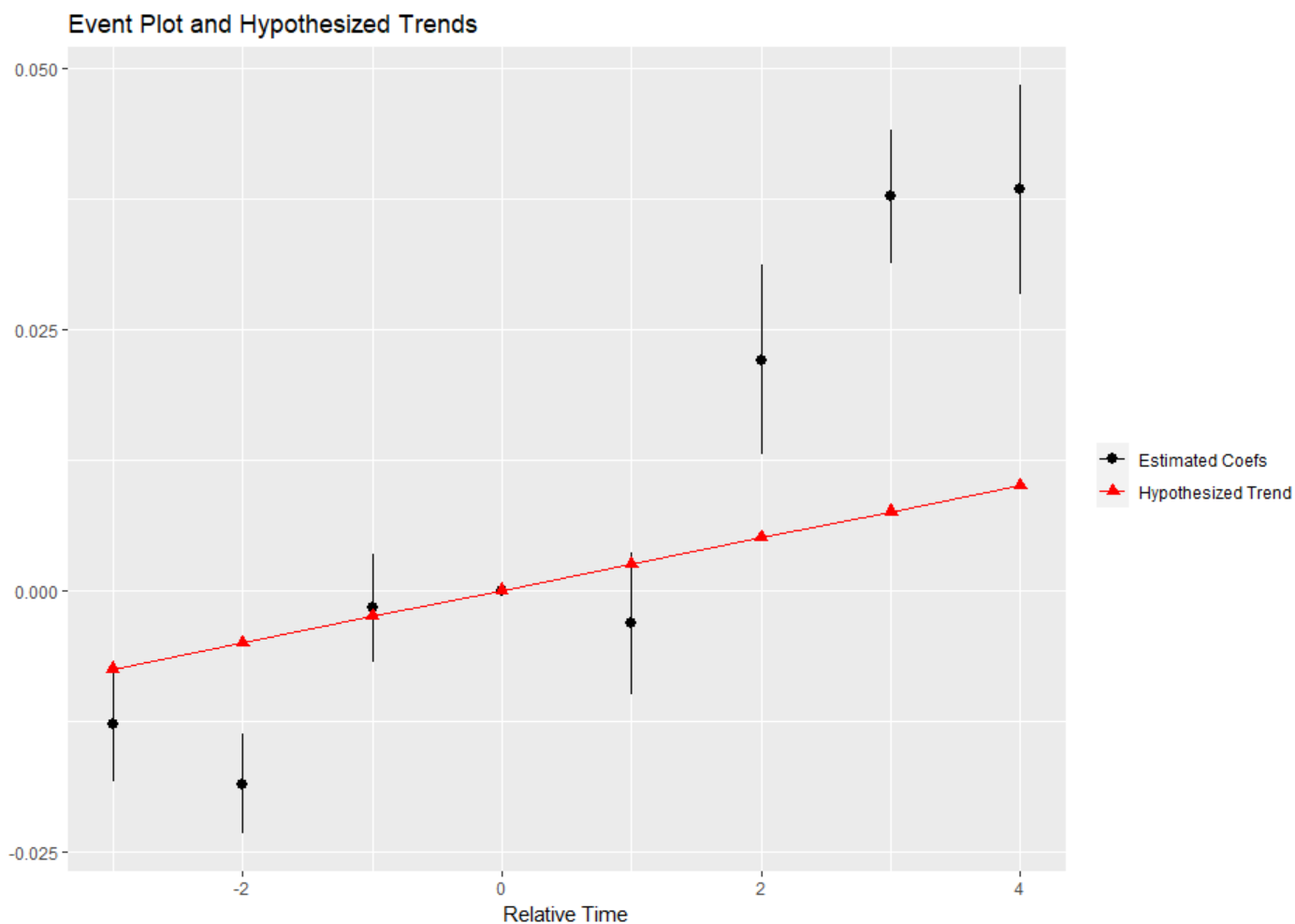
NOTES: The circular points and associated confidence intervals are the same time-specific difference-in-differences estimates presented in Figure 4. The triangular points and accompanying trend line indicates the hypothetical non-parallel pre-trend that would be detected at a power level of 80%. This is generated using the methods and software presented in Roth (2021). This line does not cross the 95% confidence intervals of the positive effects estimated in the post-treatment period (δ_2 , δ_3 , and δ_4 , or the final three points on the right), which is consistent with the conclusion that these estimates are robust to a violation of parallel pre-trends of this size.

Figure A9: Excluding postings by UVA, Robustness analysis from Roth (2021) shows event-style estimates relative to hypothetical non-zero pre-trend detectable at the 50% power level.



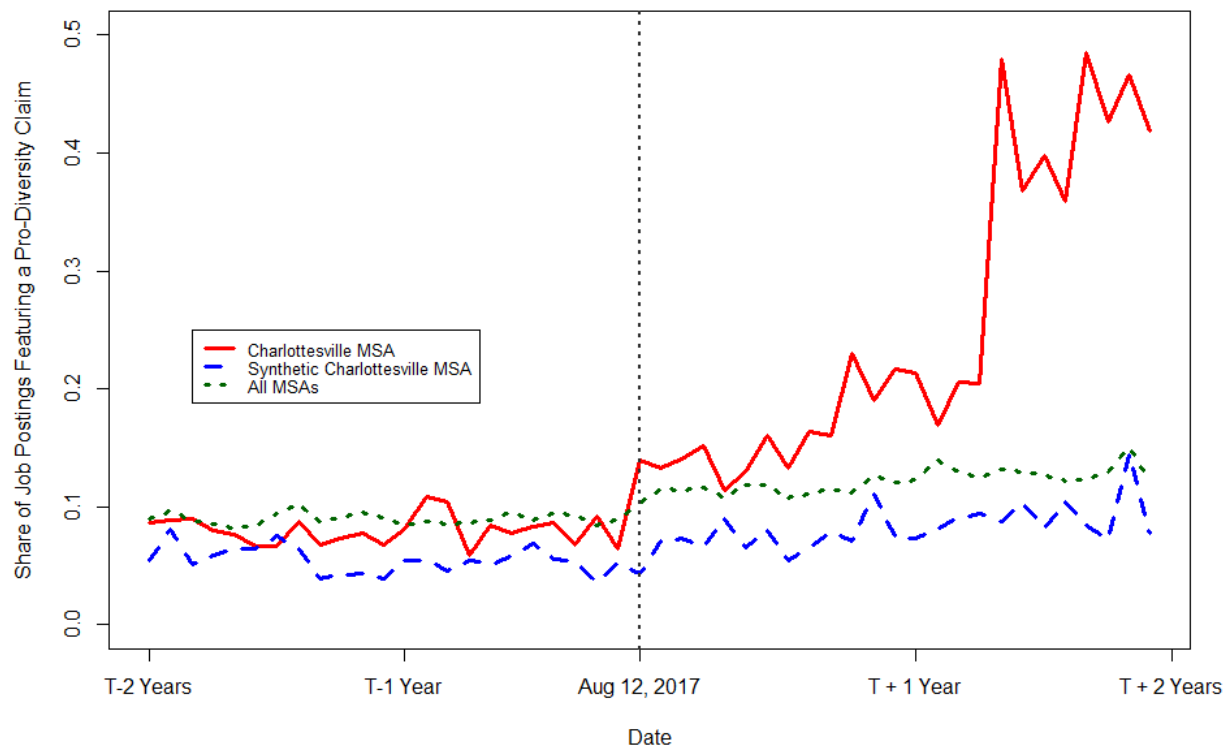
NOTES: The circular points and associated confidence intervals are the same time-specific difference-in-differences estimates presented in Figure A6. The triangular points and accompanying trend line indicates the hypothetical non-parallel pre-trend that would be detected at a power level of 50%. This is generated using the methods and software presented in Roth (2021). This line does not cross the 95% confidence intervals of the positive effects estimated in the post-treatment period (δ_2 , δ_3 , and δ_4 , or the final three points on the right), which is consistent with the conclusion that these estimates are robust to a violation of parallel pre-trends of this size.

Figure A10: Excluding postings by UVA, robustness analysis from Roth (2021) shows event-style estimates relative to hypothetical non-zero pre-trend detectable at the 80% power level.



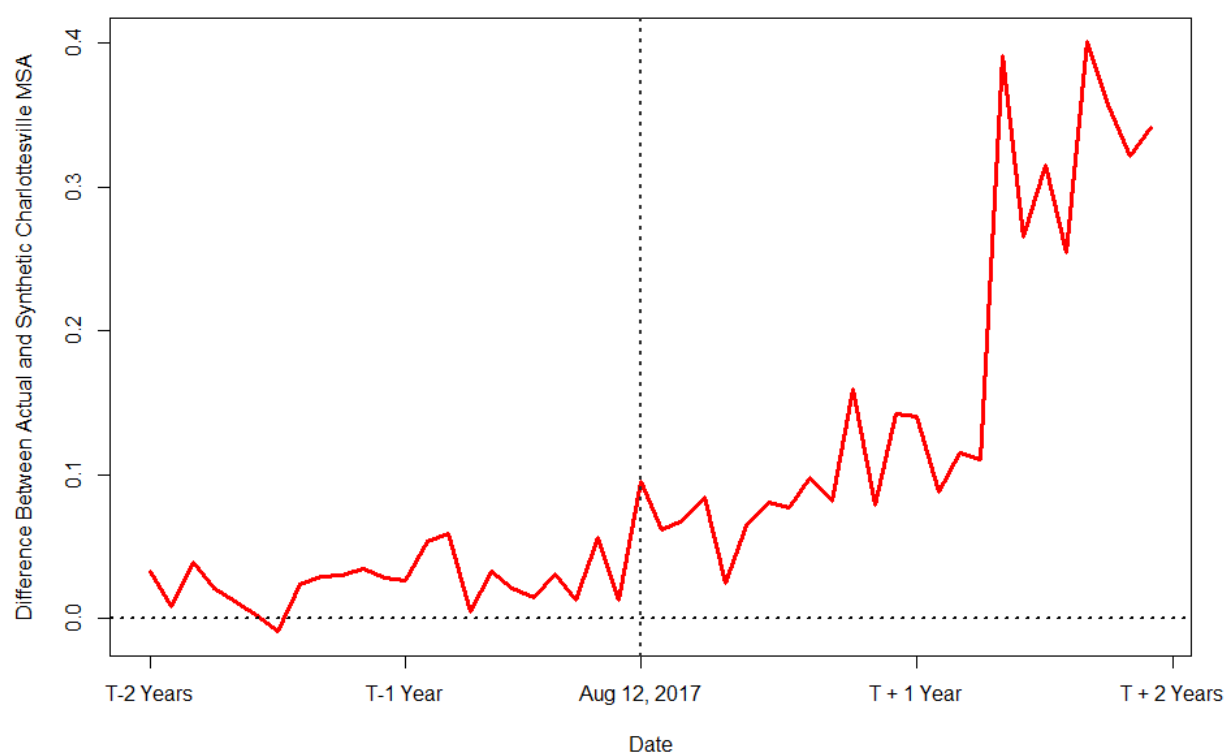
NOTES: The circular points and associated confidence intervals are the same time-specific difference-in-differences estimates presented in Figure A6. The triangular points and accompanying trend line indicates the hypothetical non-parallel pre-trend that would be detected at a power level of 80%. This is generated using the methods and software presented in Roth (2021). This line does not cross the 95% confidence intervals of the positive effects estimated in the post-treatment period (δ_2 , δ_3 , and δ_4 , or the final three points on the right), which is consistent with the conclusion that these estimates are robust to a violation of parallel pre-trends of this size.

Figure A11: The difference in the share of job postings that included pro-diversity claims in the Charlottesville MSA versus the synthetic Charlottesville MSA grew significantly following the rally.



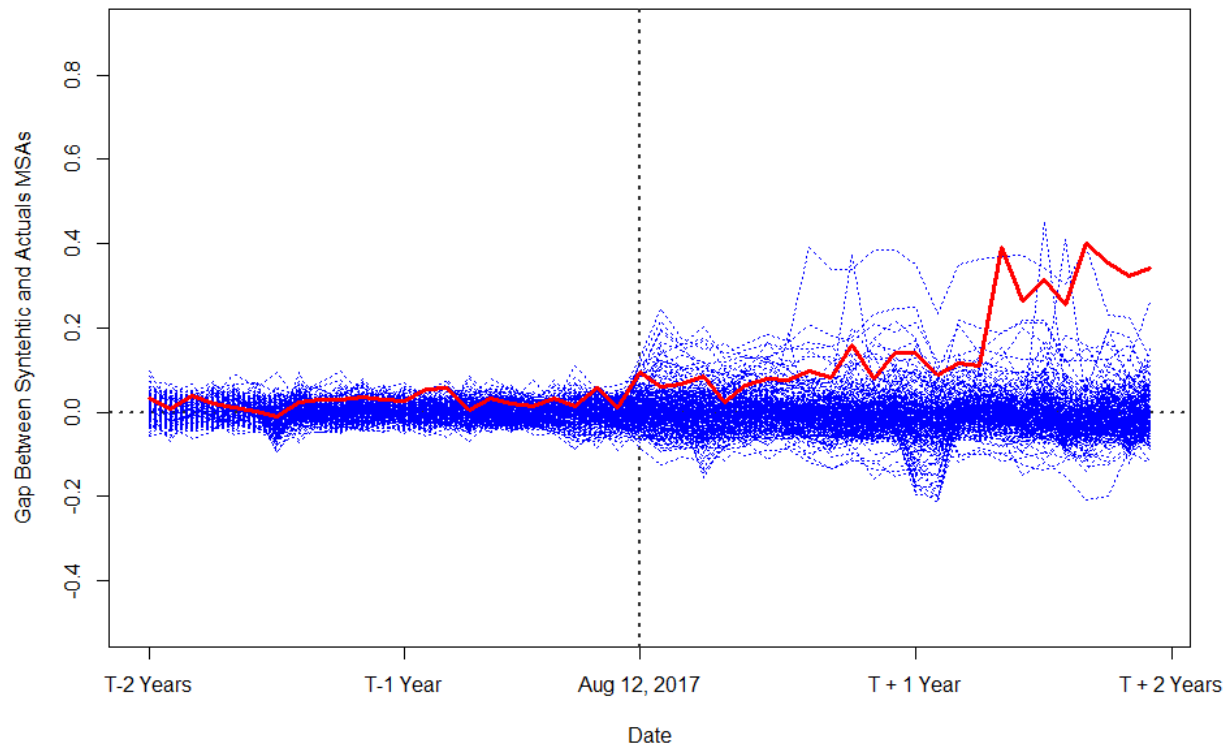
NOTES: Points indicate the monthly share of job postings featuring pro-diversity claims. The share for the synthetic Charlottesville MSA is calculated using the synthetic control method presented in Abadie et al. (2010). The region to the right of the vertical dashed line indicates the post-rally period.

Figure A12: The difference in the share of job postings that included pro-diversity claims in the Charlottesville MSA versus the synthetic Charlottesville MSA grew significantly following the rally.



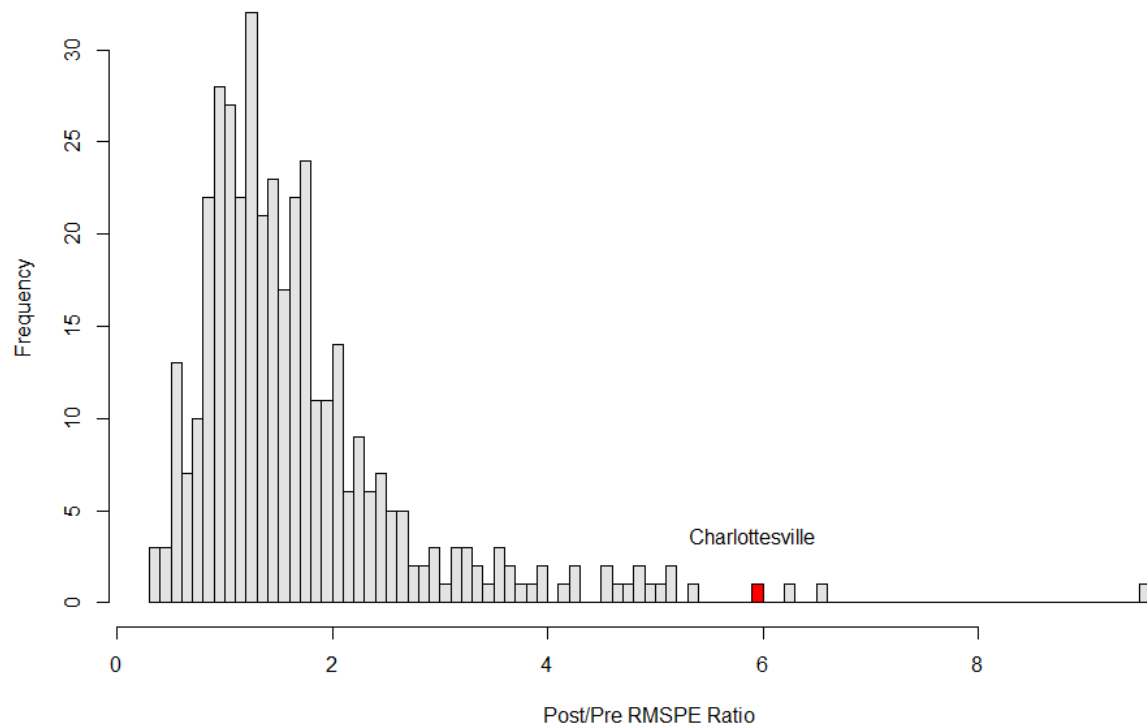
NOTES: Points indicate the difference, by month, in the use rates of pro-diversity claims for job postings in the Charlottesville area versus the synthetic control area. The region to the right of the vertical dashed line indicates the post-rally period. The dramatic increase about six months before the close of the study period reflects a significant increase in the use of pro-diversity claims by the University of Virginia that began at the start of 2019.

Figure A13: These lines illustrate the difference in the share of job postings featuring pro-diversity claims in the actual versus synthetic geographies for all MSAs for which pre-treatment root mean squared prediction error was equal to or less than that of Charlottesville. The Charlottesville value is shown by the solid red line.



NOTES: This charts the difference in use rates of pro-diversity claims between the actual and synthetic MSA for MSAs in the pool of untreated MSAs (the blue dashed lines), as well as Charlottesville (the bold, solid line marked in red). Consistent with recommendations from Abadie et al. (2010), to increase the readability of the graph, I include only those MSAs with pre-treatment RMSPEs that were less than or equal to that of Charlottesville. Again, this demonstrates that it is very unlikely that the effects detected in Charlottesville, presented in Figure A12, were due to chance.

Figure A14: The distribution of the ratio of pre- versus post-treatment root mean squared prediction error for all MSAs demonstrates it is very unlikely the observed effect is due to chance.



NOTES: This histogram shows the distribution of the the ratio of pre- versus post-treatment root mean squared prediction error for Charlottesville as well as the universe of placebo MSAs. This is the test statistic for the size of the effect of the Unite the Right rally where a larger value indicates a larger effect. The value of this statistics for Charlottesville, labelled and darkened on the far right, is exceptionally large, corresponding to an exact p-value of 0.010, meaning it is very unlikely that the treatment effect observed for Charlottesville, visible in Figure A12, was simply due to chance.

Table A1: This table differs from Table 2 only in that employer-county fixed effects have been replaced with employer fixed effects.

Sample:	(1) All	(2) Flagship Counties	(3) Clinton Counties	(4) Only UVA	(5) Excluding UVA
Charlottesville $\times$ Post-Rally	0.036*** (0.007)	0.044*** (0.007)	0.039*** (0.004)	0.169*** (0.047)	0.012*** (0.002)
Year Month Fixed Effects	✓	✓	✓	✓	✓
Employer Fixed Effects	✓	✓	✓	✓	✓
Job-Level Controls	✓	✓	✓	✓	✓
County-Level Controls	✓	✓	✓	✓	✓
Observations	56,900,934	5,149,846	35,254,610	1,279,906	56,886,915

Notes: The units of analysis in all models are job postings. “Charlottesville $\times$ Post-Rally” is a dummy variable equal to one for job postings made in the Charlottesville area after August 11, 2017. The outcome in all models is a dummy variable equal to one if the job posting included a pro-diversity claim. All columns report estimates of a version of Equation 1 that includes employer level rather than employer-county level fixed effect and differ only in terms of the sample on which they are estimated. Model 1 includes the full sample of job postings. Model 2 includes the subset of job postings in the Charlottesville area and counties that were also home to flagship state universities. Model 3 includes the subset of job postings in counties where more than 50% of the 2016 presidential vote went to Hilary Clinton. Model 4 includes job postings from the University of Virginia and all other job postings by institutions of higher education outside of Charlottesville. Model 5 excludes job postings made by the University of Virginia. The discrepancy in sample size in this table compared to Model 1 of Table 1 follows from the fact that I estimate these model’s with Stata’s *reghdfe* command, which drops singleton observations. $^{\dagger}p < 0.10$, $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Table A2: Summary Statistics for Balanced-by-Year Panel of Employee-County Dyads

	(1) Full Sample	(2) Charlottesville MSA	(3) Flagship Counties	(4) Clinton Counties
Pro-Diversity Claims	0.09 (0.25)	0.12 (0.29)	0.09 (0.25)	0.09 (0.24)
Equal Opportunity Claims	0.40 (0.43)	0.49 (0.45)	0.40 (0.43)	0.39 (0.43)
Number of Jobs	28.60 (134.81)	31.96 (199.53)	33.26 (177.89)	34.51 (165.41)
Share of Jobs Requiring BA	0.23 (0.27)	0.24 (0.29)	0.25 (0.27)	0.29 (0.29)
Share of Jobs with Moving Expenses Covered	0.06 (0.16)	0.05 (0.16)	0.06 (0.16)	0.06 (0.15)
Unique Employers	72,734	304	13,447	55,692
Unique Employer-County Dyads	390,942	322	32,142	215,023
Observations (Employer-County-Year)	1,563,768	1,288	128,568	860,092

Notes: The units of analysis are employer-county-years. These units comprise a panel of employer-county dyads balanced on the four years surrounding the treatment event.

Table A3: Difference-in-differences estimates at the employer-county level show a significant increase in pro-diversity claims among Charlottesville job postings following the Unite the Right rally.

Sample:	(1) All	(2) Flagship Counties	(3) Clinton Counties	(4) Only UVA	(5) Excluding UVA
Charlottesville $\times$ Post-Rally	0.039* (0.018)	0.043* (0.018)	0.028*** (0.002)	0.824*** (0.029)	0.039* (0.018)
Year Fixed Effects	✓	✓	✓	✓	✓
Employer-County Fixed Effects	✓	✓	✓	✓	✓
Observations	1,553,124	128,568	849,448	7,020	1,553,084

Notes: The units of analysis in all models are employer-county-years. These units comprise a panel of employer-county dyads balanced on the four years surrounding the treatment event. Years are measured relative to the treatment event. The outcome in all models is the share of job postings by a given employer in a given county in a given year that feature a pro-diversity claim. “Charlottesville $\times$ Post-Rally” is a dummy variable equal to one for employers in any of the counties comprising the Charlottesville MSA after August 11, 2017. All columns report estimates of Equation 1 and differ only in terms of the sample on which they are estimated. Model 1 includes the full sample of job postings. Model 2 includes the subset of job postings in the Charlottesville MSA and counties that were also home to flagship state universities. Model 3 includes the subset of job postings in the Charlottesville MSA and counties where more than 50% of the 2016 presidential vote went to Hilary Clinton. Model 4 includes job postings from the University of Virginia and all other job postings by institutions of higher education outside of Charlottesville. Model 5 excludes job postings made by the University of Virginia. All estimates are linear probability models with robust standard errors clustered on county. $^{\dagger}p < 0.10$, $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Table A4: Analyzing at the employer level, the estimated effect grew in the post-treatment period.

	(1)
Charlottesville $\times$ 2 Years Prior	0.009*** (0.001)
Charlottesville $\times$ 1 Year Following	0.035 (0.020)
Charlottesville $\times$ 2 Years Following	0.053** (0.016)
Year Fixed Effects	✓
Employer-County Fixed Effects	✓
Observations	1,553,124

Notes: The units of analysis are employer-county-years. These units comprise a panel of employer-county dyads balanced on the four years surrounding the treatment event. Years are measured relative to the treatment event. The outcome is the share of job postings by a given employer in a given county in a given year that feature a pro-diversity claim. “Charlottesville $\times$ Post-Rally” is a dummy variable equal to one for employers in any of the counties comprising the Charlottesville MSA after August 11, 2017. This table reports estimates of a version of Equation 1 where *Charlottesville* is instead interacted with dummies for each of the four years comprising the study period. The reference year is the year leading up to the rally. I estimate the equation with a linear probability model with robust standard errors clustered on county. $^{\dagger}p < 0.10$, $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Table A5: Consistent with the proposed mechanism, employer-level analysis shows that adoption of pro-diversity claims was larger where targeted job seekers were more likely to generalize the anti-diversity stance and where targeted job seekers held stronger pro-diversity preferences.

	(1)	(2)	(3)
Charlottesville × Post-Rally × Charlottesville Headquarters	0.056** (0.020)		
Charlottesville × Post-Rally × Employer Dependent on Bachelor's-Holding Workers		0.046*** (0.014)	
Charlottesville × Post-Rally × Employer Reliant on Out of Town Workers			0.242† (0.143)
Year Fixed Effects	✓	✓	✓
Employer-County Fixed Effects	✓	✓	✓
Observations	1,553,124	1,553,124	1,553,124

Notes: The units of analysis in all models are employer-county-years. These units comprise a panel of employer-county dyads balanced on the four years surrounding the treatment event. Years are measured relative to the treatment event. The outcome in all models is the share of job postings by a given employer in a given county in a given year that feature a pro-diversity claim. “Charlottesville × Post-Rally” is a dummy variable equal to one for employers in any of the counties comprising the Charlottesville MSA after August 11, 2017. All columns report estimates of a version of Equation 3 that includes an interaction term and differ only in terms of which variable is interacted. For brevity, I exclude from the table the main effects and only show the estimated interactions of interest. “Charlottesville Headquarters” is a dummy variable equal to one for job postings made in the Charlottesville area after August 11, 2017. “Employer Dependent on Bachelor's-Holding Workers” is a dummy equal to one for employers that are in the top 75% in terms of the total number of postings in the study period that required at least a Bachelor's degree. “Employer Reliant on Out of Town Workers” is a dummy equal to one for employers that, across the study period, offered to cover moving expenses in over half of their job postings. † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A6: Additional employer-level analysis is consistent with the proposed mechanism.

Outcome:	(1) Pro-Diversity	(2) Equal Opportunity	(3) “Charlottesville”
Charlottesville × Post-Rally	0.011 (0.008)	0.020 (0.012)	-0.042*** (0.002)
Year Month Fixed Effects	✓	✓	✓
Employer County Fixed Effects	✓	✓	✓
Employer-Year-Month Fixed Effects	✓	✓	✓
Observations	1,343,940	1,553,124	1,553,124

Notes: The units of analysis in all models are employer-county-years. These units comprise a panel of employer-county dyads balanced on the four years surrounding the treatment event. Years are measured relative to the treatment event. “Charlottesville × Post-Rally” is a dummy variable equal to one for employers in Charlottesville after August 11, 2017. Model 1 is identical to Equation 3 with the difference that it includes employer-year-month fixed effects. The outcome in Model 1 is the share of job postings by a given employer in a given county in a given year that feature a pro-diversity claim. The outcome in Model 2 is the share of job postings by a given employer in a given county in a given year that feature an equal opportunity claim. The outcome in Model 3 is the share of job postings by a given employer in a given county in a given year that feature the word “Charlottesville.” Models 2 and 3 differ from Equation 3 only in terms of the outcome variable. † $p < 0.10$, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table A7: The synthetic control method achieves better balance compared to full sample of MSAs.

	Charlottesville MSA	Synthetic Charlottesville MSA	All MSAs
Median Age	31	31	37
Population	44,751	75,234	377,849
Median Income (\$)	36,886	36,880	36,583
% White	66	66	69
Number of Jobs Per Month	1,250	1,209	3,247

Notes: The units of analysis are MSA-months where months are measured relative to the treatment date.

Table A8: Excluding job postings by the University of Virginia, the Unite the Right rally was associated with a newfound wage premium in Charlottesville job postings, but this premium was significantly lower among job postings that featured pro-diversity claims.

Outcome:	(1) Annual Salary	(2) Log Annual Salary	(3) Annual Salary	(4) Log Annual Salary
Charlottesville × Post-Rally	1867.845* (835.387)	0.040*** (0.012)	2116.398* (880.903)	0.044*** (0.012)
Pro-Diversity Claim			3129.730*** (338.735)	0.054*** (0.004)
Charlottesville × Pro-Diversity Claim			-2978.107*** (434.749)	-0.057*** (0.007)
Year-Month Fixed Effects	✓	✓	✓	✓
Employer-County Fixed Effects	✓	✓	✓	✓
Job-Level Controls	✓	✓	✓	✓
County-Level Controls	✓	✓	✓	✓
Observations	7,022,598	7,022,598	7,022,598	7,022,598

Notes: This table differs from Table 6 only in that it excludes postings by UVA. The units of analysis in all models are job posting. “Charlottesville × Post-Rally” is a dummy variable equal to one for job postings made in the Charlottesville area after August 11, 2017. The outcome in models 1 and 3 is the minimum annual salary advertised in the job posting. The outcome in Models 2 and 4 is the log of the minimum annual salary advertised in the job posting. All models are estimated using ordinary least squares with robust standard errors clustered on county. Notice that the relatively low sample size in these models follows from the fact that many jobs do not list annual salaries. [†] $p < 0.10$, $*p < 0.05$, $**p < 0.01$, $***p < 0.001$.

Methodology for Recruiting Interviewees

I identified interviewees through the alumni relations office of two universities in the United States. Both are consistently ranked in the top 50 of the *US News and World Report* college rankings.⁵ I undertook this recruiting strategy with the idea that these alumni, given the elite nature of the universities, were likely to hold managerial or other decision-making positions within their employers and were thus likely to have insight regarding if or how their employers adjusted recruiting practices in response to the Unite the Right rally. I identified and contacted all alumni whose current addresses, as per information available through the alumni relations offices, indicated they lived in Charlottesville. I also took a snowball approach where I asked the alumni that replied to my requests if they had colleagues or friends in the community that might be willing to chat with me. For those that were disposed to meet, I conducted the interviews via Zoom or in-person during a research trip I took to Charlottesville in the summer of 2022. In all, I was able to conduct 21 interviews.

⁵I abstain from further detail regarding these universities out of respect for interviewees' requests for anonymity in discussing the rally, which is still a sensitive topic in the Charlottesville community.

Methodology for Identifying Pro-Diversity Claims

As mentioned in the body of paper, I code a job posting as featuring a pro-diversity claim, if it meets any one of the following three criteria:

1. It includes the word “diversity,” but not only within the words “diversityjobboard,” “diversityjobs,” “diversitynursing,” or “pharmadiversity.”
2. It includes the word “inclusion” but not the word “inclusion” followed by “in”, “of”, “onto” or “with.”
3. It includes the word “inclusive” but not the phrase “all inclusive” or “inclusive of.”

Here, I lay out the process whereby I arrived at (Steps 1-8) and validated (Step 9) this strategy. This exercise was completed with the full Burning Glass data for the years 2015-2019.

1. **Preliminary analysis:** My first step in identifying pro-diversity claims was to read through a random sample of 500 job postings. In this preliminary, exploratory exercise I noted that employers made pro-diversity claims using the language of diversity, equity, and inclusion. I noted that pro-diversity claims seemed to most commonly feature the word “diversity,” with related language commonly appearing along with the word diversity, or, in a small number of cases, on its own. Informed by this preliminary analysis, I then systematically examined employers’ use of the nouns “diversity,” “equity,” and “inclusion” and their corresponding adjectives “diverse,” “equitable,” and “inclusive.” As detailed in steps 2-7 below, I first verified that the word “diversity” accurately captured pro-diversity claims and then checked the extent to which job postings that did not feature the word diversity but did feature these other words featured pro-diversity claims.

2. **Analyzing “diversity”:** I began by analyzing employers’ use of the word “diversity,” which appears in 9.81% of job postings. I took a random sample of 500 job postings that included the word diversity and, based on posting-by-posting manual analysis of all 500 of these postings, calculated that 93.98% of these were true-positives in the sense that they in fact contained pro-diversity claims. Frequently, these involved an employer’s assertion that it was “dedicated to,” “committed to,” “celebrates,” “values,” “believes in,” “thrives on,” or “embraces” diversity. I noticed in a small number of cases (<1%) that the only mention of diversity was in relation to the

name of the relevant job board, including “diversityjobboard,” “diversityjobs,” “diversitynursing,” or “pharmadiversity.” Accordingly, I did not code a job postings as featuring a pro-diversity claim if the word “diversity” only appeared in the context of one of these job board names.

3. Analyzing “equity”: I next examined employers’ use of the word “equity,” which appeared in 1.21% of job postings and 1.00% of jobs that did not feature the word “diversity.” I manually checked a random sample of 500 job postings that included the word equity but did not include the word diversity. Again based on on posting-by-posting manual analysis of all 500 of these postings, I calculated that only 9.16% were true positives. For the vast majority of false positives, the word equity referred to financial equity. Unfortunately, there was no common pattern among these false positives which allowed me to systematically identify them. Given this very high rate of false positives, at this step I did not code job postings that included the word equity but did not include the word diversity as containing pro-diversity claims.

4. Analyzing “inclusion”: I next examined employers’ use of the word “inclusion,” which appeared in 2.42% percent of job postings and fewer than 0.46% of job postings that did not include the word “diversity.” I manually checked a random sample of 500 job postings that included the word “inclusion” but did not include the word “diversity.” Again based on posting-by-posting manual analysis of all 500 of these postings, I calculated that 46% were true positives. In this case, however, 72.22% of false-positives were easily identifiable as those in which the word “inclusion” was followed by one of the prepositions “in,” “of,” “onto,” or “with.” Of the remaining job postings from this sample of 500, 74.75% were true positives. To double check the efficacy of this approach, I take a fresh random sample of 100 job postings that feature “inclusion,” but were not followed by “in,” “of,” “onto,” or “with” and did not include “diversity.” Consistent with my previous estimate, 76% of these are true positives. Given this high rate of true positives, I coded job postings that featured “inclusion” but did not feature inclusion followed by “in,” “of,” “on,” “onto,” or “with” as featuring pro-diversity claims. I also estimate that this captures about 20% of the true-positives featuring the word equity inasmuch as it detects job postings that featured some combination of the words “equity” and “inclusion” but not the word diversity.

5. Analyzing “diverse”: I next examined the word “diverse,” which appeared in 12.98% of job postings and 11.62% of job postings that did not include the word “diversity.” I manually checked a random sample of 500 job postings that included the word “diverse” but did not include the

word “diversity.” Again based on posting-by-posting manual analysis of all 500 of these postings, I calculated that 44% were true positives. In this case, however, there was no systematic way to identify false positives. The issue here was that in many cases “diverse” explicitly characterized the diversity of something that was not demographic diversity, such as distributors, product offerings, or job requirements. This problem also existed to a small extent with “diversity,” but was much more prevalent here. I also noticed at this point, however, that over 30% of the true positives included the word “inclusive,” a detail I return to in Step 7, below. Given this high rate of false positives, at this point I did not code job postings that included the word “diverse” but did not include the word “diversity” as including pro-diversity claims.

6. Analyzing “equitable”: I next examined the word “equitable,” but since this word was extremely rare (appearing in only 0.28% of job postings and fewer than 0.22% of job postings that did not include the word “diversity”), I did not manually check for false positives or otherwise use it to identify pro-diversity claims.

7. Analyzing “inclusive”: Finally, I examined the word “inclusive,” which appeared in 4.82% of job postings and 3.79% percent of job postings that did not include the word diversity. I manually checked a random sample of 500 job postings that included the word “inclusive” but did not include the word “diversity.” Again based on posting-by-posting manual analysis of all 500 of these postings, I calculated that 59.64% were true positives. These true positives very frequently referenced the employer’s inclusive “environment” or “workplace.” In this case, 97.03% of false positives were systematically identifiable as those that include the phrase “all inclusive,” which was used to indicate that a list of applicant requirements or job responsibilities was not exhaustive, and “inclusive of,” which preceded a list of applicant requirements or job responsibilities. After removing these false positives, the true positive rate rises to 97%. To double check the efficacy of this approach, I took a fresh random sample of 100 job postings that feature “inclusive,” but did not include the word “diversity” or the phrases “all inclusive” or “inclusive of” and find that 98% of these were true positives. Accordingly, I code all such job postings as featuring pro-diversity claims.

It is also important to note that, as mentioned in Step 5, by capturing these true-positive uses of “inclusive” I capture a significant number of the true-positive cases that include the word “diverse.” In fact, I estimate that by including these postings, the true positive rate for the remaining job

postings that feature “diverse” but not “diversity” falls even further from 44% to 35%. This provides further justification for my decision in Step 5 to not code job postings that feature “diverse” but not “diversity” as including pro-diversity claims.

8. Considering additional diversity-related words: Finally, I examined a range of other diversity-related words, but found that these were very rare. These include “belonging” (appearing in fewer than 0.01% of all job postings), “bipoc” (appearing in 0.01% of all job postings and fewer than 0.01% that did not include the word “diversity”), “black lives matter” (appearing in fewer than 0.001% of all job postings), “dei” (appearing in fewer than 0.001% of all job postings), and “social justice” (appearing in 0.01% of all job postings but far fewer than 0.01% of postings that did not include the word “diversity”).

9. Validating Final Measure: As a final check, I estimated the rate of false positives and false negatives. To check for false positives, I took a random sample of 500 job postings that I had coded as featuring pro-diversity claims. I examined these manually and found that 96% of these were true positives. To check for false negatives, I took a random sample of 500 job postings that I had coded as *not* featuring pro-diversity claims. I examined these manually and found that 97% of these were true negatives.