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Institutional protection of minority employees and entrepreneurship: Evidence from the LGBT Employment Non-Discrimination Acts

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Research Abstract

A diverse workforce has long been associated with multiple firm benefits, but this is sometimes difficult to achieve due to employer discrimination. Although multiple institutional arrangements have been put in place to ban discriminatory behavior, the effects of such regulations remain relatively unexplored, often neglecting start-ups. We propose that institutional changes aiming to outlaw employment discrimination will trigger two main effects: they will (a) depress start-up founding rates through enhancement of wage-work appeal, and (b) increase the average start-up quality due to a higher threshold for leaving wage-work. We test our predictions by exploiting the staggered enactment of Employment Non-Discrimination Acts in the U.S. Consistent with our theory, we find that this institutional protection reduced the quantity of entrepreneurship but increased its quality.

Managerial Summary: Do laws that protect traditionally oppressed groups from discrimination in the workplace affect the propensity of workers to leave their company and start an entrepreneurial venture? Our study shows that legislative changes that ban the discrimination against members of the LGBT community

in the labor market have a two-fold effect on entrepreneurship. First, they reduce the workers' propensity to start a new venture (thus reducing the loss of human capital) because of an increased relative appeal of the workplace. Second, they increase the overall quality of the startups founded in the region, increasing the overall welfare. Additionally, we find that these effects are more likely to be present when litigation against employment discrimination is more frequent and when the protected minorities are more prevalent.

KEYWORDS

discrimination, entrepreneurship, gender, institutions, start-ups

1 | INTRODUCTION

Transition into entrepreneurship is a common occupational experience, with many individuals starting and running their own business at some point in their life. Accordingly, scholars in strategy and entrepreneurship continue to debate when individuals choose to become founders (e.g., Campbell, Agarwal, & Seth, 2012; Kacperczyk, 2012). In addressing this question, ample research has recognized the critical importance of the institutional environment—in terms of regulatory changes and policies—in facilitating or hindering entrepreneurship across regions and nations (Armour & Cumming, 2008; Castellaneta, Conti, & Kacperczyk, 2020; Conti & Valentini, 2018; Eberhart, Eesley, & Eisenhardt, 2017; Eesley, 2016; Hiatt, Sine, & Tolbert, 2009; Sine & David, 2010). Yet despite this extant work, our understanding of how the institutional environment impacts entrepreneurship, influencing its quantity and quality, remains sorely incomplete.

For the most part, researchers have focused on the impact of institutions targeting *prospective founders*. For example, scholars have evaluated the impact of regulations designed to facilitate entrepreneurship, by removing barriers to key resources on which entrepreneurial entry depends (e.g., Eberhart et al., 2017; Hiatt et al., 2009; Sine, Haveman, and Tolbert, 2005). Before they access resources, however, individuals must first decide whether to forgo paid employment and become founders. Indeed, recent advances in entrepreneurship theory indicate that entrepreneurial entry is intertwined with individual mobility dynamics because employees evaluate the decision to launch a new venture against the backdrop of conditions they face in paid employment (e.g., Kacperczyk, 2012; Kacperczyk & Marx, 2016; Sørensen & Sharkey, 2014; Stenard & Sauermann, 2016). Thus, institutions targeting *employees* and affecting employment conditions may dramatically change an individual's propensity to become a founder and, hence, they need to be considered more fully.

In this study, we thus shift the focus from regulations that target *founders* and aim to improve their access to resources, to regulations that target *employees*, and aim to improve their conditions in wage work. Drawing on ample research documenting the key role of institutional protection against employment discrimination in increasing the appeal of the workplace

(Dobbin & Kelly, 2007; Dobbin, Sutton, Meyer, & Scott, 1993; Kalev & Dobbin, 2006; Kelly & Dobbin, 1998), we develop a simple theoretical framework that relates regulatory changes that prohibit employment discrimination to entrepreneurship. We propose that, as the regulatory environment becomes hostile to employment discrimination, this will impact entrepreneurship in two ways: (a) it will depress the founding rates through enhancement of the relative appeal of wage work, and (b) it will increase the average new-venture quality and survival due to a higher threshold for leaving wage work. We further expect that these effects will operate both directly, by reducing departures to entrepreneurship among the protected class members (i.e., individuals targeted by antidiscrimination laws), and indirectly, by reducing departures for entrepreneurship among nonprotected minority groups, as well as nonminority employees, more generally. Overall, we expect that institutional protection targeting disadvantaged workers—as opposed to protection targeting founders—will have an unexpected, so-far overlooked impact on startup creation and quality.

To test our theoretical predictions, we focus on the enactment of Employment Non-Discrimination Acts (ENDA) in U.S. This legislation has several notable advantages for our study. First, it aimed to prohibit employment discrimination based on sexual orientation and gender identity, thus matching the importance of earlier acts against employment discrimination on the basis of race, religion, national origin, or physical disability (Dobbin et al., 1993; Kelly & Dobbin, 1998, 1999). Second, its enactment represents a plausibly exogenous shock with respect to entrepreneurship, offering a fortuitous opportunity for causal identification. Finally, the legislation has been progressively introduced in 15 U.S. states over the period of 1980–2006, allowing for the use of a staggered difference-in-differences approach. We find support for our claims that institutional change aiming to ban employment discrimination affects entrepreneurial entry: it depresses entry rates, on the one hand, but increases quality of new ventures, on the other hand. In summary, our results support the notion that institutions that target employees—by protecting them against employer discrimination—represent an overlooked factor driving the rate and quality of entrepreneurship. Our results are further robust to a battery of robustness tests. At the same time, we provide strong empirical evidence consistent with the underlying mechanisms we theorize. Finally, supplemental analyses refute alternative explanations for our main findings, such as an increase in start-up costs due to the new laws, contemporary institutional changes, and others.

2 | THEORY

2.1 | Past research

What motivates individuals to launch new ventures and become founders is the subject of long-standing inquiry in entrepreneurship and strategy literature (e.g., Kacperczyk, 2012; Kacperczyk & Marx, 2016; Stenard & Sauermann, 2016). Whereas the vast majority of studies have documented the role of individual and organizational factors, researchers have increasingly linked entrepreneurial entry with the characteristics of the institutional environment and the critical role of changes in institutions, deemed to regulate entry (e.g., Eesley, 2016; Sine & David, 2010; Sine, Haveman, & Tolbert, 2005). For example, differences in regulatory reforms have been found to account for variation in entrepreneurship rates across regions and countries (Armour & Cumming, 2008). In addition, institutional changes exert an important influence on the individuals' motivation and ability to transition into entrepreneurship, by enhancing their

willingness to strike out on their own (e.g., Eberhart et al., 2017; Eesley, 2016). However, despite rich research inquiry into changes in regulatory institutions, our understanding of their effect on entrepreneurship remains incomplete. In examining the impact of the institutional environment, the lion's share of attention has focused on institutions that target prospective founders—by facilitating access to critical resources needed to launch new ventures. For example, the preponderance of research has considered institutional changes designed to reduce the barriers to entrepreneurship in terms of financial or human capital, and thus to increase founding rates (e.g., Castellaneta et al., 2020; Eberhart et al., 2017; Eesley, 2016).

Yet transition into entrepreneurship is not solely a function of access to resources. Prospective founders also need to decide whether to leave wage work for entrepreneurship in the first place. Indeed, entry into entrepreneurship can be characterized as a career-mobility process, given that the majority of entrepreneurs proceed from paid employment (e.g., Audia & Rider, 2005; Sørensen & Fassiotto, 2011). From this perspective, the decision to become a founder is not a function of access to resources alone; rather, it also depends on the appeal of workplace conditions or the attractiveness of wage work, in general (Kacperczyk, 2012; Sørensen & Sharkey, 2014). Empirically, studies find that individuals exhibit greater tendency to strike out on their own when options in wage employment are blocked or unattractive, making entrepreneurship more appealing in relative terms (e.g., Hellmann, 2007; Kacperczyk, 2012; Kacperczyk & Marx, 2016). For example, workers have been found to favor entrepreneurship over paid employment when launching a startup is more lucrative (Amit & Muller, 1995), when promotion chances available in wage work are less abundant or less accessible (Kacperczyk & Marx, 2016; Sørensen & Sharkey, 2014), or when accommodating employees' own initiatives in wage work becomes difficult or unlikely (Hellmann, 2007; Klepper, 2007; Shah, Agarwal, & Echambadi, 2019). In all these instances, the opportunity cost of leaving paid employment is sufficiently low to entice the pursuit of entrepreneurship in lieu of being a wage worker. Together, these studies underscore the profound impact of career options available in the workplace, and the cost of giving them up, in shaping the propensity of becoming a founder.

If workplace conditions affect entrepreneurial mobility decisions, then institutional changes that target current employees and affect workers' career choices may also significantly influence entrepreneurial rates. Indeed, ample research in sociology and strategy indicates that institutional employment protection—especially including regulation that prohibits employer discrimination against minority workers—is not only prevalent, but also consequential. Although an inclusive, diverse workplace has long been documented to enhance a firm's performance and sustainable competitive advantage, diverse work environments are difficult to foster and maintain because employers may indulge their "taste for discrimination," by limiting opportunities of certain groups due to conscious and unconscious bias against minorities (Becker, 1957, 1971; Borjas & Ramey, 1995). Importantly, given the significant societal and firm-related costs that arise because of discrimination, multiple institutional protections have been put in place to counter discrimination in how employers hire, promote, and fire workers (e.g., Kelly & Dobbin, 1998). For example, in the United States alone, beginning with the Civil Rights Act of 1964, antidiscrimination law has been progressively developed to outlaw employment discrimination on the basis of societally disadvantaged traits such as race, color, creed, or national origin. Indeed, these regulative changes—which aim to prohibit employment discrimination and increase workplace diversity—have been commonly considered the primary means by which individuals are protected from disadvantageous treatment in wage work (Rubin, 2019).

Despite its prevalence, past studies have not examined how institutional protection of minority workers affects entrepreneurship. A small number of studies have considered the

impact of discrimination—and of the practices designed to counter negative bias on the basis of race or gender—on entrepreneurial rates (e.g., Fairlie & Robb, 2007; Younkin & Kuppuswamy, 2017) and the subsequent growth of entrepreneurial firms (e.g., Blanchflower, Levine, & Zimmerman, 2003; Freeland & Keister, 2016; Kacperczyk, Younkin, & Rocha, 2021). There is additional evidence that discrimination is a central hindrance to entrepreneurship because stereotypes and negative biases can suppress access to key resources, such as capital, on which the founding of a new venture depends (e.g., Buttner & Rosen, 1989). Despite its valuable insight, however, this work has been limited to discriminatory behaviors (and their influence) that arise on the part of resource holders in entrepreneurship, including investors, suppliers, and banks (Carter & Shaw, 2006; Thébaud & Sharkey, 2016), as well as startup employees (Kacperczyk et al., 2021). By contrast, institutional protection of minority workers has been mostly neglected, leaving its potential impact on the rate of creation and quality of startups unclear. With few notable exceptions (Hwang, 2021; Hwang & Phillips, 2020), the relationship between labor market discrimination and entrepreneurship has received little attention. Hence, the need to unpack the impact of institutional career constraints on entrepreneurship provides a motivation for our study. In what follows, we therefore examine how the legal compulsion not to discriminate and to ensure that members of traditionally-oppressed groups are treated in the same way as other employees may alter the relative benefits of leaving paid employment for entrepreneurship.

2.2 | The twofold effect of institutional protection against employment discrimination on entrepreneurship

There is strong reason to expect that protection of historically-oppressed groups will affect entrepreneurship in two ways: by (a) depressing the rates of new-venture creation, and (b) enhancing the average quality of new ventures. Following Coleman's (1994) diagram, which has become the standard way of representing micro–macro links, we stipulate that macro-level shifts in institutional employment protection will result in macro-level changes in startup rates and quality via the micro-level dynamics unfolding in firms and affecting employees (cf., Figure 1). First, as institutional protection against employment discrimination increases, this will lead to micro-level initiatives introduced by employers to comply with the law and protect certain minority workers from discrimination (macro-to-micro link 1). These initiatives will, in turn, enhance the appeal of wage work relative to entrepreneurship, mainly among the employees of the protected class, but also among other minority workers as well as for some of their nonminority colleagues (micro-to-micro link 2). Finally, as the attractiveness of wage

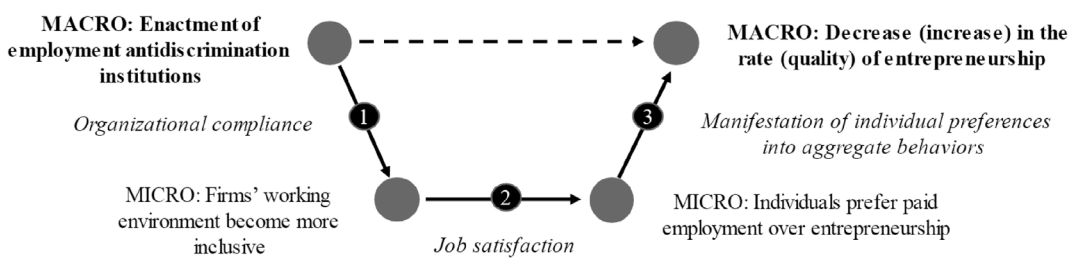

FIGURE 1 Theoretical model

work among these different groups increases, employees will become less inclined to leave for entrepreneurship but more prone to found high-quality ventures, leading to lower founding rates but higher startup quality at the macro level (micro-to-macro link 3). Figure 1 summarizes the causal pathways we envision, which connect institutional protection and entrepreneurship.

2.2.1 | The effect of institutional protection against employment discrimination on founding rates

We first turn to the impact of institutional protection against employment discrimination on founding rates. There is a strong reasons to expect that, following regulative changes that extend institutional protection to historically-discriminated groups, the protected class will likely become shielded from employment discrimination and experience better overall treatment in wage work as employers start complying with new regulations—either because conscious, negative actions subside, or because discriminated groups can more easily seek redress for acts of discrimination through lawsuits or grievance procedures, which are often costly for employers (Rubin, 2019). And although evidence for the efficacy of diversity policies remains mixed and may critically depend on the specific initiatives introduced (e.g., Kalev & Dobbin, 2006), institutions prohibiting employment discrimination generally improve the status of newly-protected groups in terms of their earnings or career advancement (e.g., Collins, 2003). Better access to previously-out-of-reach opportunities will, in turn, enhance the attractiveness of wage work among newly protected groups and, by default, reduce the relative appeal of entrepreneurship. Consistent with this claim, research shows that disadvantaged groups find employers with formal personnel systems in place to prevent discrimination (Barbulescu & Bidwell, 2013) or those perceived as less discriminatory at the point of hire or post-hire (e.g., Pager & Pedulla, 2015) more appealing. Along similar lines, formerly incarcerated individuals are more likely to turn to entrepreneurship to avoid labor-market discrimination (Hwang & Phillips, 2020), while scientists facing early-career immigration constraints, which prevent them from integrating into the labor market, increase the likelihood of later career transitions of such immigrants to the founding of incorporated ventures (Agarwal, Ganco, & Raffie, 2021). Finally, anecdotal evidence suggests that minority groups place greater weight on launching and operating their own venture than on wage work, preferring the former to the latter when they experience discrimination in the labor market. For example, LGBT workers exhibit a stronger preference for self-employment when discrimination based on sexual orientation becomes more prevalent in wage work. As one LGBT freelancer stated: "if you freelance...your exposure to discriminatory colleagues might be minimal" (Jacobs, 2016). Similarly, following the removal of LGBT protection in Kansas, one LGBT entrepreneur commented: "I am glad to be self-employed."[1] Together, these examples highlight that entrepreneurship will become a less attractive option relative to wage work when the institutional protection against employment discrimination intensifies. Members of newly protected groups who would otherwise be more inclined to become founders, will thus been more likely to keep their attachment to wage work, leading to a decline of founding rates.

Second, as the institutional protection of certain groups from employment discrimination increases and the working environment becomes more inclusive, significant spillovers to other minority groups not included in the protected class may follow, diminishing the relative attractiveness of entrepreneurship among a wider group of traditionally-disadvantaged workers. Other disadvantaged individuals, beyond the newly-protected class alone, will witness

[1]https://www.tapatalk.com/groups/l_anon/kansas-governor-removes-protections-for-lgbt-worke-t4562353.html.

significant improvements in their own standing. For example, employers may extend protection to a broader circle of disadvantaged workers, given that these individuals share the common experience of being discriminated against (Gaertner & Dovidio, 2000; Hutchinson, 1997; Urban & Miller, 1998; Wildman, 1996), and thus can be classified as belonging to the "minority" category (Zhu, Shen, & Hillman, 2014).[2] In addition, increases in the institutional protection of historically oppressed groups will signal to other disadvantaged workers that the employer is committed to treating all employees equitably (Griffith & Hebl, 2002; Ragins & Cornwell, 2001). Together, these different mechanisms indicate that protecting certain disadvantaged groups from employment discrimination will enhance the perception of wage work among the protected class as well as among other members of disadvantaged groups, diminishing the relative appeal of entrepreneurship as an occupational choice.

Finally, institutional protection from employment discrimination may also enhance the perception of wage work among nondisadvantaged workers, leading to further spillover effects. Following institutional changes that aim to ban discrimination of disadvantaged groups in paid employment, even nondisadvantaged individuals may find wage work marginally more attractive and will thus be less willing to leave for entrepreneurship. As Li and Nagar (2013) put it, "an organization's policies toward its employees, whether an inclusive healthcare policy or discriminatory promotion or hiring policy send latent signals to the entire organization (...). Such signals might then impact all employees" (p. 543). And even if *some* employees may oppose antidiscrimination policies, workers will, on average, consider equitable treatment of groups burdened by negative stereotypes a desirable societal goal. Indeed, the act of extending protections to historically oppressed groups, similar to other corporate social responsibility initiatives (Burbano, Mamer, & Snyder, 2013; Flammer & Kacperczyk, 2016; Greening & Turban, 2000; Kacperczyk, 2009), is often associated with higher levels of workplace engagement and employee job satisfaction (Dimarco, 1975; Downey, van der Werff, Thomas, & Plaut, 2015; Flammer & Kacperczyk, 2019; Greening & Turban, 2000) and lower employee turnover (Bode, Singh, & Rogan, 2015; Flammer & Kacperczyk, 2019). Hence, following this accumulated research, we expect that, in the aftermath of the enactment of institutions protecting minority groups from employment discrimination, the reputation of the employer as equitable and fair will increase, resulting in greater job satisfaction and lower propensity to leave for entrepreneurship among nonminorities.

Overall, we anticipate that regulative changes that ban employment discrimination will reduce the rates of entrepreneurship because of the direct impact on workers belonging to the protected class, and the indirect impact on other minorities, as well as at least some nonminority workers.

> **Hypothesis (H1).** *Increases in institutional protection from employment discrimination will lead to a decline in the rates of entrepreneurial foundings.*

2.2.2 | The effect of institutional protection against employment discrimination on new-venture quality

Our previous arguments suggest that institutional protection from employment discrimination will suppress the rates of entrepreneurial entry by increasing the relative appeal of wage work vis-à-vis

[2]Research shows that different types of discrimination, including racism and sexism, tend to all be perceived as instances of generalized discrimination (Hutchinson, 1997; Wildman, 1996).

entrepreneurship. A direct corollary of our argument is that the opportunity cost of leaving wage work for entrepreneurship will increase, as disadvantaged groups gain institutional protection and discriminatory practices in the labor market become outlawed. From a worker's perspective, this will increase the threshold to give up paid employment, encouraging employees to incur the cost of leaving only when expected payoffs from new ventures appear more promising. Indeed, numerous studies report that startups exhibit a higher growth potential and stronger survival chances when entry threshold increases, and new ventures are thus unlikely to reflect a necessity or lack of satisfying employment options (Fairlie and Fossen, 2017). For example, women become more inclined to pursue high-growth ventures when family-friendly amenities, which women often value, tend to be more easily accessible in paid employment, making any potential move to entrepreneurship more costly (Thébaud, 2015). Similarly, enforcement on noncompete covenants, which increases the hiring cost for founders and thus reduces the relative appeal of entrepreneurship vis-à-vis paid work, has been found to result in the creation of higher-growth, longer-surviving ventures (Starr, Balasubramanian, & Sakakibara, 2018). Taken together, these studies imply that workers will make career detours to entrepreneurship primarily in cases when new-venture opportunities are more promising or more valuable than staying put in paid employment. Importantly, extending this logic to protection against employment discrimination, it follows that the average quality of new ventures will increase, when institutional protection of minority workers intensifies. Hence, we predict that:

> **Hypothesis (H2).** *Increases in institutional protection from employment discrimination will lead to an increase in startup quality.*

2.2.3 | Institutional protection against employment discrimination and entrepreneurship: mechanisms

Our model posits that institutional protection against discrimination results in lower employment discrimination of certain protected groups via the enactment of corporate antidiscrimination policies. To avoid being sued, employers will be more likely to enact these policies in environments where the protected minorities are more prevalent and, hence, the chances of legal recourse due to employer discrimination is higher (which reinforces link 1 in Figure 1). In addition, following a reduction in employer discrimination, increases in employee job satisfaction will be higher in these environments (cf., link 2 in Figure 1), given that the protected employees are especially sensitive to firm policies that promote inclusion. Consistent with this claim, past studies find that the presence of supportive LGBT policies within an organization is associated with gay and lesbian employees' greater job satisfaction (Day & Schoenrade, 2000; Griffith & Hebl, 2002) and fewer discrimination reports by LGBT employees (e.g., Ragins & Cornwell, 2001). It follows, therefore, that minority groups that gain institutional protection from employers' discriminatory acts will be more likely to favor wage work over entrepreneurship. Accordingly, we expect the decline in entrepreneurial foundings and the increase in the share of high-quality ventures to be amplified when the protected groups are more prevalent. Hence,

> **Hypothesis (H3a).** *Following increases in institutional protection from employment discrimination, decreases in startup foundings will be amplified when the protected minorities are more prevalent.*

Hypothesis (H3b). *Following increases in institutional protection from employment discrimination, increases in startup quality will be amplified when the protected minorities are more prevalent.*

A further implication of our model is the claim that increases in institutional protection against discrimination will be more consequential for employers when the protected groups exhibit a higher tendency to mobilize regulatory institutions—via legal complaints and grievances—to enforce their rights and benefits. Importantly, when those most subject to employer discrimination can more easily make use of institutional protections by seeking redress for discriminatory acts, employers will respond by complying to a larger degree with the regulatory changes in order to avoid financial or reputational costs of litigation. For example, studying a number of high-profile sex and race discrimination lawsuits settled against publicly traded companies in the United States, Hirsh and Cha (2018) found that legal pressures produced significant diversity gains by increasing the representation of women and nonwhites in managerial ranks. More generally, lawsuits against employers will have an equity-enhancing effect, generating policy changes that improve minority conditions and reduce the prevalence of discriminatory acts in wage work. Hence, when minorities exhibit a greater tendency to mobilize institutions in an effort to fend off discrimination in paid employment, institutional protection will be particularly effective in reducing discrimination in the workplace (which strengthens link 1 in Figure 1), and increasing employee job satisfaction (consistent with link 2 in Figure 1). It follows, therefore, that the macro effects we predict—the decline in the founding rates of new businesses and the concurrent increase in their quality—will be additionally amplified when litigation against employment discrimination is more prevalent. Hence:

Hypothesis (H4a). *Following increases in institutional protection from employment discrimination, decreases in startup foundings will be amplified when litigation against employment discrimination is more prevalent.*

Hypothesis (H4b). *Following increases in institutional protection from employment discrimination, increases in startup quality will be amplified when litigation against employment discrimination is more prevalent.*

3 | EMPIRICAL SETTING AND DATA

To examine the impact of employment antidiscrimination institutions on entrepreneurship, we exploit the passage of legislation that reduced employers' discriminatory acts, taking advantage of the staggered enactment of such laws in different moments in time across different U.S. states. Specifically, we consider state-level enactment of the Employment Non-Discrimination Act (ENDA), which shields workers from employment discrimination based on sexual orientation and gender identity. The District of Columbia was first to pass such law in 1977, and by the end of 2011, 21 states had followed suit (see Table A1). These enactments are suitable for our identification strategy for at least three reasons. First, substantial empirical evidence shows that discrimination based on sexual orientation and gender identity has been an important challenge in the workplace (e.g., Badgett, Lau, Sears, & Ho, 2007). The United States still has no federal legislation in place to prohibit discrimination in the labor market based on sexual orientation. Yet, driven by individual actions and pressure from social

movements, a number of states have enacted laws prohibiting employment discrimination. The protections they provide mirror earlier protections against discrimination based on race, gender, religion, national origin, and physical disability, and they have allowed advocates to frame sexual orientation protections as incremental additions to existing policies (Klawitter & Flatt, 1998). Second, despite being possibly driven by social movements, the enactment of antidiscrimination legislation is unlikely to be related to any economic characteristics of the state, including the rate or quality of entrepreneurship. Accounts of the political economy of these reforms suggest their passage is exogenous to product markets, innovation, and entrepreneurial activities, and depends instead on other variables unrelated to the economy (Gao & Zhang, 2017; Haider-Markel & Meier, 2003), an assumption which we empirically validate in our setting, by assessing whether the trend in the outcome variables differs between treated and control states—cf., Table 2—and whether the past levels of the outcome variables predict the enactment of the antidiscrimination legislation—cf., Table A2.

Furthermore, it is largely accepted that ENDA's enactment reduces employment discrimination (Barron & Hebl, 2013; Klawitter & Flatt, 1998). Scholars have shown that ENDA has both instrumental and symbolic effects. Instrumentally, biased employers discriminate less in hiring and rewarding workers because ENDA creates an "expected cost," which incorporates violating the law if caught (e.g., attorney's fees, fines) multiplied by the probability of being caught (Landes, 1968). Perceived risk of litigation and financial loss, as well as negative publicity and harm to firm reputation, are key factors responsible for the impact of antidiscrimination legislation on firm behavior (Leonard, 1984). The symbolic effect suggests that, even absent the possibility of tangible punishment, ENDA reduces discrimination on the basis of sexual orientation simply by designating such discrimination as illegal, criminal, or deviant (e.g., Tapp & Kohlberg, 1971; Zimring and Hawkins, 2010). Indeed, the mere fact that this discrimination becomes prohibited (without the threat of enforcement) is sufficient to create the symbolic effect in changing workplace norms about the acceptability of prejudice and discrimination toward gays and lesbians (Hebl et al., 2016). Accordingly, this antidiscrimination law has been linked to employee perceptions of reduced discrimination (Griffith & Hebl, 2002; Ragins & Cornwell, 2001), while others have documented the positive effects of ENDA on innovation (Gao & Zhang, 2017) and firm performance (Nguyen, Kecskés, & Mansi, 2020).

4 | STATISTICAL APPROACH

To examine the impact of employment antidiscrimination legislation on entrepreneurship, we use a difference-in-differences methodology based on the 15 "treatments" occurring between 1980 and 2006 (cf., Table A1). Our unit of analysis is the state-year. Our methodology follows Bertrand and Mullainathan's (2003) application of the difference-in-differences methodology in the presence of staggered treatments at the state level. Specifically, our main specifications will take the following form:

$$Y_{st} = f\left(\varphi_s + \tau_t + \beta_{AD}\mathrm{AD}_{st} + \boldsymbol{\beta}_{CV}\mathbf{CV}_{st-1}\right) \quad (1)$$

where AD is the antidiscrimination law "treatment dummy"—equal to 1 if the state has enacted the antidiscrimination law by year t—and φ_s and τ_t are state and year fixed effects to control for, respectively, constant differences across U.S. states and aggregate changes over time due to business cycles, national policies, and so forth. **CV** is a parsimonious

vector of control variables coherent with previous literature (e.g., Conti & Valentini, 2018), which includes real GDP per capita (source: Census Bureau), the net job creation rate, or the job employment growth rate (source: LBD), state taxation level (Census Bureau), the prevalence of minorities in the population, as measured by the proportion of black people and women (source: Census Bureau), and the state population education level, as proxied by the proportion of people with at least a college degree (source: Census Bureau). Besides controlling for the economic and social factors traditionally correlated with workers' opportunities, we also control for state political trends because the political orientation of a state may plausibly influence both the adoption of antidiscrimination legislation and entrepreneurship, leading to spurious correlations. Hence, we include a dummy variable that takes the value of "1" if the state had a Republican Governor and "0" otherwise. Errors are clustered at the state level, to address serial correlation concerns (Bertrand, Duflo, & Mullainathan, 2004).

The coefficient of interest is β_{AD}, which measures the effect of antidiscrimination law on our dependent variable. To verify our theoretical predictions, we consider different dependent variables Y in Equation (1), related to the rate and quality of entrepreneurial ventures. First, we assess the effect of antidiscrimination laws on the number of *entrepreneurial foundings*, using data from the Longitudinal Business Database (LBD). The LBD provides data on annual employments for every U.S. private sector establishment with a payroll. The underlying data are sourced from U.S. tax records and Census Bureau surveys and span the period between 1977 and 2011. The LBD includes complete accounting even of small firms and establishments often excluded or subsampled in typical corporate surveys. The LBD also lists physical locations of establishments rather than states of incorporation. Following Kerr and Nanda (2009), for each establishment we define its year of entry as the first year of positive employment.[3] Our main dependent variable—entrepreneurial foundings—is measured as the total number of new, stand-alone establishments in a given U.S. state and year. Based on Hypothesis (H1), we expect β_{AD} to be negative. In addition, we expect the coefficient of antidiscrimination law to be even more negative in states where LBGT minorities are more present (based on H3a), and where discrimination is higher (based on H3b).

We further estimate the effect of the antidiscrimination law on startup quality. First, we measure startup quality as the propensity of young firms to file for patents, proxied by the number of patent applications filed by *new* assignees in a given state-year normalized by the number of new entrants.[4] New firms that file for patents are more likely (a) to be endowed with superior technologies and (b) to capture the value their technologies create. Several studies highlight the impact of patenting on the survival of new ventures. Cockburn and Wagner (2007), for example, found that for companies listed in the Nasdaq, having a patent portfolio increased their survival after the dotcom bubble burst in the late 1990s. Helmers and Rogers (2010) similarly show that for the 162,000 limited-liability companies created in the United Kingdom in 2001, patent and

[3]Prior research indicates that any potential lag between founding and hiring the first employee is often negligible (i.e., under 1 year) (Dahl and Klepper, 2015; Fairlie and Miranda, 2016), making our measure a suitable proxy for founding a new venture.

[4]Since we are interested in identifying inventions generated by individuals and used by the same individuals to found a new venture, we assigned patents to states based on *both* the location of the (new) assignee *and* the location of the first inventor. Hence, we exclude from our sample patents for which the two locations do not coincide, as these are likely to be instances where individual inventors collaborate with or sell ideas to a company, rather than founding a new business. Using alternative approaches (e.g., considering the location of the assignee *or* the location of the inventor) produces similar findings (available upon request).

trademark portfolios increased their survival rate after 5 years. In both cases, less than half the companies owned patents; companies that owned patents had a significantly higher survival rate (34% in the first study, 16% in the second). These and other studies provide compelling evidence that new ventures are of higher quality when their propensity to patent increases. Data on patents were retrieved from the USPTO Patentsview dataset.

Second, we proxy for quality of new ventures in a given state using the average amount of venture capital (VC) funding a new venture received, computed as the total amount of VC investments in a given state-year (available from 1980), normalized by the number of new entrants in the same state-year. VC investments are an important milestone for a high-growth venture (Guzman & Stern, 2016) and VC funding is associated with strong treatment and selection effects, whereby venture capitalists spur higher growth of new ventures, while investing in ex-ante promising startups (Da Rin, Hellmann, & Puri, 2011). Hence, if startups receive more funds from such specialized investors, their growth potential and quality tends to be higher. The VC data (by location of investee company) were retrieved from Thomson Reuters' VentureXpert.

Finally, we measure startup quality as proxied by their survival chances as the proportion of startups created in a given state-year that were able to survive for at least 5 years (Kerr & Nanda, 2009). Because our LBD ends in 2011, for firms founded in 2006, only a 5-year survival can be measured. Overall, due to data limitations, we can consider and measure *all* our dependent variables between 1980 and 2006—and this is the time frame for which we estimate our models. Based on Hypothesis (H2), we expect β_{AD} to be positive for all our measures of new-venture quality and survival. Further, we expect the coefficient of antidiscrimination law to be even more positive in states where LBGT minorities are more present (based on Hypothesis (H3b)), and where discrimination levels are higher (based on Hypothesis (H4b)). Table 1 reports summary statistics for our main state-level variables, whereas Table A2 reports pairwise correlations for state-level variables.[5]

5 | EMPIRICAL RESULTS

5.1 | Validity of the identification strategy

We begin by assessing the validity of our identification strategy, providing at the same time some initial results. First, a difference-in-differences approach relies on the parallel trend assumption, which states that the average change in outcome for the treated in the absence of treatment should equal the average change in outcome for the nontreated. To verify that there

[5]As can be seen in Table 1, the majority of key state-level covariates that pertain to economic and social conditions (e.g., GDP per capita, proportion of women, proportion of black people, and proportion of people with a college degree) are similar across control and treated states, before the treatment. Although we find some covariates to be different (e.g., the rate of job creation), this is fairly common. For example, in their seminal study, Bertrand and Mullainathan (2003) similarly find differences between "eventually-treated" and "never-treated" states (e.g., with the former having have larger plants in terms of headcount). Following their arguments, we consider such differences immaterial for our identification strategy, for two reasons. First, because the enactment of the law is staggered over time, most states that enact antidiscrimination law enter in both the "treated" and the "control" groups—mitigating the concern that unobserved differences between the two groups may drive our results (Bertrand & Mullainathan, 2003). Second, the identification assumptions of the difference-in-differences regression do not require that the treated and control states exhibit similar levels in the covariates or outcome variables before the treatment. Rather, the difference-in-differences approach relies on the similar trend in the outcome variable between the treated and control states: and, as we will show, our data meet this assumption.

TABLE 1 Summary statistics

	Average All	Average Treated	Average Treated before	Average Treated after	Average Never treated
Entrepreneurial foundings	9,906.618	11,132.07	10,592.5	12,347.73	9,089.649
Startup quality (patents)	.0069179	.4786922	.4757194	.48539	.4634275
Startup quality (VC)	.0164056	.0087506	.0071599	.0123346	.005696
Startup quality (survival)	.4695334	.0288156	.0133765	.0636001	.0081323
Antidiscrimination	.122963	.3074074	0	1	0
GDP per capita	33,105.41	35,832.87	33,371.07	41,379.34	31,287.1
Net job creation rate	2.064519	2.026667	2.174332	1.693976	2.089753
Proportion black	.0838422	.0580104	.0634592	.0457344	.1010634
Proportion women	.5143248	.5137519	.5134066	.5145299	.5147067
Proportion college	.1419066	.163042	.147968	.197004	.1278162
Red governor	.477037	.4814815	.4090909	.6445783	.4740741
Taxation level	5.48947	5.666971	5.464953	6.122122	5.371136

Note: GDP per capita is in thousand 2012 US dollars. The number of firms is measured in thousands. Startup quality (VC) is in million US dollars. Data on taxation not available for the District of Columbia.

were no divergent trends across states before the change in protection from employment discrimination—and to assess how the ENDA's effect occurs over time—we construct a dynamic difference-in-differences model, using a set of dummies that measure the distance in years from the enactment of the law, before and after its introduction. The coefficients reported in Models 1–4 of Table 2 indicate that there is no difference across states in the patterns of entry, quality, and survival of ventures founded prior to the ENDA enactments, thus confirming that our results are not driven by diverging pretreatment trends. By contrast, in the postchange period, the coefficients that relate to the number of entrants (specification 1) become negative and are estimated with more precision. Similarly, the coefficients for the effect of ENDA on all three measures of startup quality show a meaningful effect only after the law enactment (see specifications 2–4, Table 2). Further, these estimates lead to two important conclusions. First, the effect of antidiscrimination laws is immediate for most outcomes, occurring at $t + 1$ or 1 year following the treatment. However, for VC funding, the treatment effect is delayed until $t + 3$ or 3 years following the treatment. Such delay can be explained by the fact that venture capitalists tend to heavily rely on key milestones (i.e., revenue), which are not achieved until later in the startup life-cycle. Second, there is a negative pretrend in the model considering the survival of startups, at $t - 1$ (Model 4)—which is likely to reduce the economic significance of our results. Hence, when assessing the effect of the antidiscrimination laws on the survival of startups, it is appropriate to control for state linear pretrend.

Figures 2–5 display dynamically the confidence interval of the parameter estimates of law enactment. Again, these figures show no significant difference in the patterns of entrepreneurial activity between treated and control states before enactment of the laws, reinforcing our confidence in the difference-in-differences results.

The patterns of the dynamic treatment effect, as shown above, mitigate the legitimate concern regarding the exogeneity of the shock. However, to additionally alleviate this concern, we conducted a supplemental analysis to investigate whether states' economic and political conditions might influence the enactment of ENDA. Table A3 reports results of linear probability models where the

TABLE 2 Effect of employment antidiscrimination laws on the rate of entrepreneurial foundings and startup quality: dynamics

	(1) Entrepreneurial foundings	(2) Startup quality (patents)	(3) Startup quality (VC)	(4) Startup quality (survival)
$t \leq -5$	0.019	−0.000	0.003	0.006
	(0.032)	(0.000)	(0.011)	(0.008)
$t - 4$	0.008	−0.000	−0.006	0.008
	(0.026)	(0.000)	(0.008)	(0.008)
$t - 3$	0.008	0.001	−0.006	0.007
	(0.021)	(0.000)	(0.009)	(0.007)
$t - 2$	0.009	0.001	−0.001	0.002
	(0.018)	(0.000)	(0.006)	(0.005)
$t - 1$	0.008	0.001	0.003	−0.001
	(0.013)	(0.000)	(0.005)	(0.004)
$t + 1$	−0.030	0.002	−0.004	0.004
	(0.014)	(0.001)	(0.006)	(0.004)
$t + 2$	−0.055	0.002	0.012	0.008
	(0.019)	(0.001)	(0.014)	(0.004)
$t + 3$	−0.051	0.002	0.009	0.011
	(0.023)	(0.001)	(0.005)	(0.006)
$t + 5$	−0.056	0.001	0.009	0.020
	(0.029)	(0.001)	(0.007)	(0.010)
$t \geq 5$	−0.047	0.003	0.044	0.007
	(0.024)	(0.001)	(0.025)	(0.006)
GDP per capita	0.000	0.000	0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Job creation rate	0.006	−0.000	0.000	−0.000
	(0.002)	(0.000)	(0.000)	(0.000)
Proportion black	−0.627	0.008	0.046	0.245
	(0.782)	(0.012)	(0.276)	(0.122)
Proportion women	3.688	−0.020	1.200	0.108
	(2.701)	(0.025)	(0.813)	(0.401)
Proportion college	−0.908	0.002	0.730	−0.321
	(1.392)	(0.014)	(0.385)	(0.205)
Red governor	0.003	0.000	−0.001	−0.001
	(0.015)	(0.000)	(0.004)	(0.002)
Taxation level	−0.050	−0.000	−0.004	0.004
	(0.014)	(0.000)	(0.003)	(0.002)

TABLE 2 (Continued)

	(1) Entrepreneurial foundings	(2) Startup quality (patents)	(3) Startup quality (VC)	(4) Startup quality (survival)
Observations	1,350	1,350	1,350	1,350
State FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
R-squared		.867	.540	.724
Log-likelihood	−53,658.517			

Note: Robust standard errors clustered by state are in parentheses. Poisson model in Model 1; Log-linear OLS model in Models 2–4.

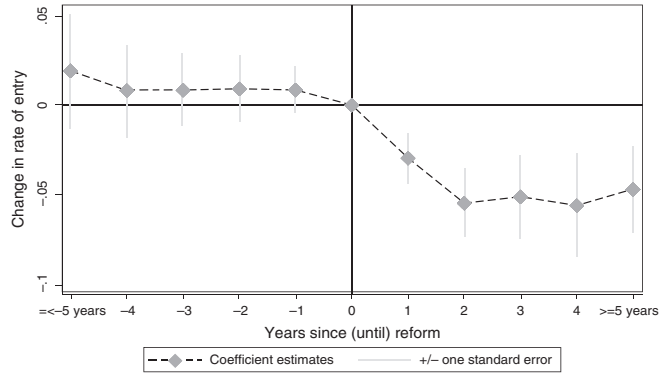

FIGURE 2 Dynamic effect of employment antidiscrimination laws on the rate of entrepreneurial foundings

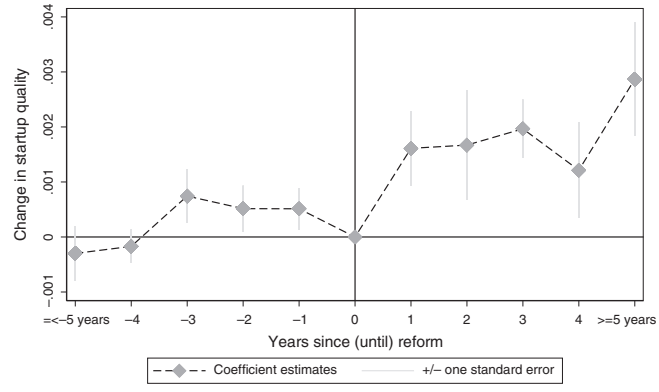

FIGURE 3 Dynamic effect of employment antidiscrimination laws on startup quality (innovation measure)

dependent variable is the antidiscrimination law dummy, and the explanatory variables are the number of entrants in a given state-year, our proxies for startup quality, and GDP per capita. As can be seen, the number of startups and their quality do not predict the enactment of antidiscrimination laws, while firm survival is negatively associated with ENDA enactment. This suggests that the

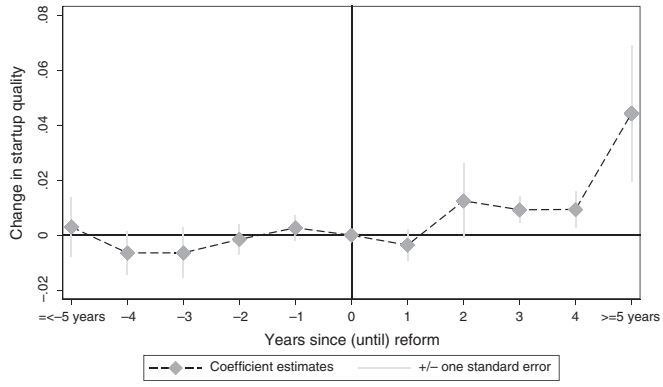

FIGURE 4 Dynamic effect of employment antidiscrimination laws on startup quality (venture capital measure)

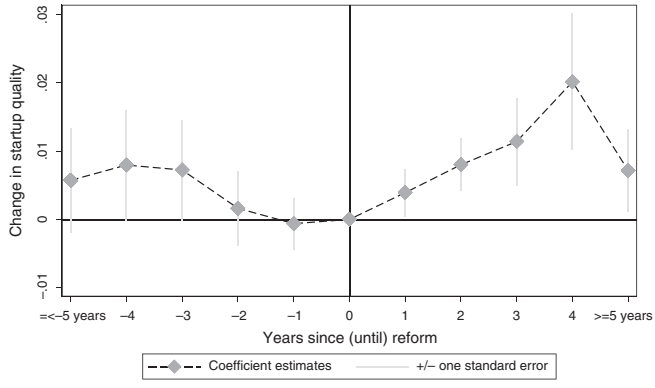

FIGURE 5 Dynamic effect of employment antidiscrimination laws on startup quality (survival measure)

positive treatment effect of antidiscrimination laws on the survival of new ventures represents a conservative estimate. But overall, this analysis reinforces our confidence regarding the exogeneity of the changes in legal protection of LGBT groups with respect to entrepreneurial foundings.

We also note that our approach follows the classic approach as in Bertrand and Mullainathan (2003) and, as such, due to the staggered nature of the treatment, the control group includes *all* states (that is, not only states that are never treated, but also states that are always or eventually treated in the time period considered). In this regard, one concern may be that our estimates entail a comparison between late-treated states and early-treated states, which might be problematic when the effect of treatment is "heterogeneous," that is, it increases or decreases over time.[6] Although plausible, this concern is mitigated in our study because our

[6]Goodman-Bacon (2020) shows that, when the treatment is staggered over time, the diff-in-diff estimator is a weighted average of all possible diff-in-diff estimators that compare different group of units to each other, including: (a) treated with never treated units as controls; (b) treated with always treated units as controls; (c) early treated with late treated units (before treatment of the latter) as controls; (d) late treated versus early treated units (after treatment of the latter) as controls. This latter comparison is problematic when the treatment effect is heterogenous (i.e., it changes over time), as it might bias the estimates and due to "negative weights" in the computation of the average treatment effect.

sample includes multiple states (i.e., 36) that are *never* treated in the considered time period. Hence, the estimation of our treatment derives mostly from the comparison of treated versus never-treated states. Moreover, as suggested by Goodman-Bacon (2020), we decompose the diff-in-diff estimator, to assess the relative weight of each comparison. As expected, about 85% of variance to estimate our treatment derives from the comparison between treated versus never- or always-treated states. About 10% of the variance comes instead from the comparison between early treated states versus. Later treated states (before they have been treated) as controls By contrast, only 5% derives from the comparison between later treated versus earlier treated states (after they have been treated) as controls, which is the comparison deemed problematic.

5.2 | Rate of entrepreneurial Foundings

Our results so far corroborate the validity of our identification approach and provide a set of initial findings that are fully consistent with our theory. We next turn to evaluate in more detail the effect of antidiscrimination laws on each dependent variable of interest, starting from the number of new entrants. Because *entrepreneurial foundings* is a count variable, we estimate a Poisson (fixed-effects) specification. However, as a robustness check, we also estimate a log-linear OLS model. Table 3 reports the results of this basic estimation. The Poisson specifications show that enactment of ENDA has a negative effect on entrepreneurial foundings (Models 1–3 of Table 3). These results thus lend broad support to Hypothesis (H1) and suggest that the institutional protection of minority employees decreases new-firm entry by about 6% in specification 1, Table 3 (p-value equal to .047). The economic magnitude of the effect is similar if we restrict the time window around the treatment to 10 years (as in specification 2) and 5 years (as in specification 3). This relatively large economic significance likely reflects spillovers to other minority groups as well as to nonminorities. Indeed, past studies find effects of comparable magnitude: For example, Gao and Zhang (2017) document a 7.6% impact of pro-LGBT legislation on patenting rates and a 10.6% impact on patent citations. Similarly, Vakili and Zhang (2018) show that the legalization of same-sex marriages increases the number of patents by 6.2% and the number of patenting organizations by 8.5%. Finally, Nguyen et al. (2020) demonstrate that LGBT antidiscrimination laws increase a firm's market-to-book ratio by 3%. In all these studies, spillovers to other social groups are also likely to explain the relatively large economic effects. Such effects are found to be even larger when scholars consider pro-LGBT initiatives at the firm-level. For example, a corporate equality index, which measures the level of sexual orientation diversity, increases the stock market valuation by 16% (Wang & Schwarz, 2010), and employee productivity (Shan, Fu, & Zheng, 2017) by 4%, while same-sex domestic partnership benefits increase annualized excess returns by 14% and return on assets by 20% (Li & Nagar, 2013). Further, there is direct evidence that pro-LGBT corporate initiatives enhance the workplace attractiveness even among nonminority workers (Badgett, Durso, & Schneebaum, 2013; Day & Schoenrade, 2000; Tejeda, 2006), further substantiating the indirect effect of pro-LGBT legislation due to the impact on non-LGBT minorities as well as nonminority groups. Finally, as a robustness check, Models 4–6 of Table 3 are estimated with an OLS specification where the dependent variable is the log of the rate of entrepreneurial foundings. Even in this case, our results are largely recovered: implementation of ENDA diminishes the rate of entrepreneurial foundings by 7%.

TABLE 3 Effect of employment antidiscrimination laws on the rate of entrepreneurial foundings

	(1) Entrepreneurial foundings	(2) Entrepreneurial foundings ±10 years	(3) Entrepreneurial foundings ±5 years	(4) (Log of) entrepreneurial foundings	(5) (Log of) entrepreneurial foundings ±10 years	(6) (Log of) entrepreneurial foundings ±5 years
Antidiscrimination	−0.059	−0.056	−0.068	−0.065	−0.072	−0.074
	(0.029)	(0.029)	(0.028)	(0.050)	(0.040)	(0.034)
GDP per capita	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Job creation rate	0.006	0.008	0.008	0.009	0.010	0.011
	(0.002)	(0.002)	(0.002)	(0.002)	(0.001)	(0.002)
Proportion black	−0.687	−0.705	−0.586	−0.053	0.099	0.336
	(0.771)	(0.872)	(0.976)	(0.793)	(0.990)	(1.058)
Proportion women	3.733	−0.411	−2.184	2.793	0.220	−0.791
	(2.624)	(2.496)	(2.789)	(2.843)	(1.978)	(1.872)
Proportion college	−0.985	0.312	1.165	−1.039	−0.054	0.879
	(1.325)	(1.353)	(1.586)	(1.789)	(1.460)	(1.553)
Red governor	0.003	0.008	0.020	0.021	0.019	0.018
	(0.015)	(0.015)	(0.014)	(0.014)	(0.014)	(0.013)
Taxation level	−0.049	−0.057	−0.068	−0.055	−0.054	−0.051
	(0.015)	(0.015)	(0.015)	(0.011)	(0.014)	(0.015)
Observations	1,350	1,124	981	1,350	1,124	981
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
R-squared				.433	.428	.447
Log-likelihood	−53,394.538	−42,085.942	−36,841.086			

Note: Robust standard errors clustered by state are in parentheses. Poisson model in Models 1–4. Log-linear OLS model in Models 5–8.

5.3 | Startup quality

H2 posits that antidiscrimination laws will increase the quality of new entrants. To test this hypothesis, we estimate fixed-effects linear models. Table 4 reports estimates for quality, as proxied by startup patenting propensity in Models 1–3, VC funding per startup in Models 4–6, and the proportion of firms surviving at least 5 years in Models 7–9. We estimate our models on the full time period, but for robustness, we also limit the time window to either 10 or 5 years before and after treatment. Finally, as previously mentioned, our models of the effect of ENDA on startup survival include a state linear trend, to control for the different (negative) pretrend between treated and control states. Consistent with our predictions, we find that, while the enactment of ENDA reduces the overall new-firm entry, it also promotes entry of higher-quality startups. The parameter estimate of the treatment dummy is positive across all specifications (p-value is equal to .002 in specification 1 and 2 and to .01 in specification 3; to .007, .036, and .021, respectively, in specifications 4, 5, and 6; to .042, .173, and .063, respectively, in specifications 7, 8, and 9).

As for the economic significance of the effects, ENDA exerts a limited—but not negligible—effect on the proportion of firms surviving at least 5 years, which increases by about .01: this represents a 2% increase of the baseline proportion of surviving new ventures, which is about .47. Furthermore, ENDA has a strong effect on startups' patenting propensity and VC funding: the sample means of the two variables are 0.009 and 0.018, respectively. The parameter estimates of Models 1 and 4 imply that enactment of ENDA increases patenting propensity and VC funding by 0.002 and 0.025, respectively, that is, approximately 30 and 140% of the sample average. These results broadly support Hypothesis (H2), and suggest that enactment of antidiscrimination laws increases the opportunity cost of entering entrepreneurship by enhancing the attractiveness of paid employment. As a consequence, when discrimination subsides, workers turn to entrepreneurship only to pursue attractive opportunities that result in higher-quality ventures. In summary, we find strong support for the key predictions of our study.

5.4 | Testing the mechanism

The previous analysis confirms the presence of a (macro-to-macro) link between institutional protection from employment discrimination and entrepreneurship. That is, we show that the former leads to lower rates of entrepreneurship but a higher quality of new ventures. We next verify the mechanisms underlying these effects—or the macro-to-micro links depicted Figure 1—by assessing how the relationship between antidiscrimination institutions and entrepreneurship is affected by the contingencies we specified.

To begin with, as suggested by Hypothesis (H3a) and (H3b), the treatment effect should be amplified in environments where the presence of the minority groups targeted by the antidiscrimination legislation is more substantial. Indeed, in these environments, links 1 and 2 in Figure 1 will be stronger because employers are more likely to implement antidiscrimination initiatives to comply with the regulation, and, at the same time, the well-being of the protected class is particularly responsive to firm pro-inclusion policies. To assess this claim, we first consider whether the effect of ENDA tends to vary with the total LGBT population present in a state, re-estimating our baseline models within two subsamples: above and below the median levels of the U.S. LGBT population by state (we collected data on states' LGBT populations from

TABLE 4 Effect of employment antidiscrimination laws on startup quality

	(1) Startup quality (patents)	(2) Startup quality (patents) ±10 years	(3) Startup quality (patents) ±5 years	(4) Startup quality (VC)	(5) Startup quality (VC) ±10 years	(6) Startup quality (VC) ±5 years	(7) Startup quality (survival)	(8) Startup quality (survival) ±10 years	(9) Startup quality (survival) ±5 years
Antidiscrimination	0.002	0.002	0.002	0.025	0.026	0.009	0.013	0.012	0.008
	(0.001)	(0.001)	(0.001)	(0.014)	(0.012)	(0.007)	(0.006)	(0.008)	(0.004)
GDP per capita	0.000	0.000	0.000	0.000	0.000	0.000	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Job creation rate	−0.000	−0.000	−0.000	0.001	0.001	−0.000	0.000	0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Proportion black	0.014	0.009	0.010	0.110	0.158	0.020	−0.071	−0.166	−0.209
	(0.010)	(0.010)	(0.008)	(0.253)	(0.244)	(0.123)	(0.139)	(0.158)	(0.176)
Proportion women	−0.012	−0.032	−0.030	1.345	1.437	0.383	0.303	0.100	0.008
	(0.025)	(0.031)	(0.028)	(0.872)	(0.972)	(0.377)	(0.358)	(0.414)	(0.482)
Proportion college	0.009	0.013	0.017	0.866	0.599	0.250	−0.634	−0.488	−0.230
	(0.013)	(0.017)	(0.013)	(0.404)	(0.452)	(0.150)	(0.195)	(0.231)	(0.240)
Red governor	0.000	0.000	0.000	−0.001	−0.004	−0.003	−0.001	0.001	0.001
	(0.000)	(0.000)	(0.000)	(0.005)	(0.006)	(0.002)	(0.002)	(0.002)	(0.002)
Taxation level	−0.000	−0.000	−0.000	−0.002	−0.003	−0.003	−0.001	−0.001	−0.001
	(0.000)	(0.000)	(0.000)	(0.003)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Observations	1,350	1,124	981	1,350	1,124	981	1,350	1,124	981
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	No	No	Yes	Yes	Yes
R-squared	.570	.532	.500	.364	.350	.452	.632	.648	.670

Note: Robust standard errors clustered by state are in parentheses. OLS models.

the 2005 American Community Survey). Table 5 presents estimates for these models.[7] Consistent with our expectations, the effect of antidiscrimination initiatives—concerning both the number of new foundings and their quality—is stronger in states where the gay population is above the median. The economic difference across coefficients is evident. The *p*-values of the test for the differential effect of antidiscrimination laws in high versus low gay population states are presented in Table A4, Column 1: the *p*-value is equal to .011 for entrepreneurial foundings, .002 for the patent-based measure of quality, .067 for the VC measure of quality, and .000 for the survival-based measure of quality.

Based on Hypothesis (H4a) and (H4b), we would further expect that antidiscrimination laws will prove particularly effective in areas where leveraging antidiscrimination laws is more common among minority employees, since firms will comply to a larger degree with the institutional protection of minority workers in order to avoid financial or reputational costs of litigation—which would strengthen link 1 in Figure 1. To separate states with higher and lower propensity to leverage antidiscrimination institutions, we collected litigation data on civil rights suits relating to jobs, available from the Federal Judicial Centre Integrated databases, from 1970 to the present.[8] For each state-year in our data, we computed the count of civils suits. To avoid endogeneity with our antidiscrimination legislation, we considered the number of grievances between 1970—the first year when the information is available—and 1977—the year where ENDA has been enacted for the first time in DC. For the purpose of our analyses, states that fall above the median in terms of the annual number of civil right job suits are high-litigation states, and states that fall below the median in terms of the annual number of civil right job suits are high-litigation states. As expected, Table 6 shows that the magnitude of the coefficient is persistently higher in high-litigation states. Tests for the statistical significance of the difference between low- and high-litigation states are presented in Table A4, Column (2). The *p*-value is equal to .073 for entrepreneurial foundings, .011 for the patent-based measure of quality, .061 for the VC measure of quality, and .203 for startup life chances. Whereas the statistical accuracy of differences varies across measures, and we cannot informedly speculate on the drivers of such differences besides possible measurement noise, results point strongly in the direction of our theory.

5.5 | Supplemental mechanism tests

5.5.1 | LGBT occupations

To provide additional evidence for the mechanism we hypothesized, we assessed changes in the proportion of self-employed individuals by occupation, following treatment. If our results reflect greater retention of LGBT workers within wage employment, then we should observe that the proportion of self-employed individuals, in the post-ENDA period, declined most in occupations with a larger share of LGBT workers. Presumably, occupations with a higher concentration of gay individuals would be most affected by our treatment, thus providing more direct evidence for the mechanisms behind ENDA.

To test this prediction, we used the Current Population Survey (CPS) data from 1995 to 2019. For any state and year, we computed the proportion of self-employed individuals in the top 15 occupations/industries with the highest LGBT representation, as defined by Tilcsik, Anteby, and

[7]For split-sample analyses, we do not cluster the standard errors at the state level, as the number of clusters is too low and could bias the estimates (Angrist & Pischke, 2009).

[8]Data are available at: https://www.fjc.gov/research/idb.

TABLE 5 Effect of employment antidiscrimination laws according to state gay population

	(1) Entrepreneurial foundings ± 10 years low gay population states	(2) Entrepreneurial foundings ± 10 years high gay population states	(3) Startup quality (patents) ± 10 years low gay population states	(4) Startup quality (patents) ± 10 years high gay population states	(5) Startup quality (VC) ± 10 years low gay population states	(6) Startup quality (VC) ± 10 years high gay population states	(7) Startup quality (survival) ± 10 years low gay population states	(8) Startup quality (survival) ± 10 years high gay population states
Antidiscrimination	0.026	−0.071	0.001	0.003	0.018	0.041	0.001	0.020
	(0.036)	(0.014)	(0.000)	(0.000)	(0.006)	(0.011)	(0.005)	(0.005)
GDP per capita	0.000	0.000	0.000	−0.000	0.000	−0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Job creation rate	0.010	0.004	−0.000	−0.000	0.000	0.002	−0.000	0.000
	(0.002)	(0.003)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Proportion black	3.349	−1.205	0.020	0.029	0.001	0.589	0.231	−0.287
	(0.720)	(0.348)	(0.009)	(0.007)	(0.065)	(0.187)	(0.190)	(0.109)
Proportion women	1.908	4.185	−0.035	0.112	−0.111	5.431	−0.075	1.818
	(1.479)	(2.351)	(0.024)	(0.035)	(0.162)	(1.644)	(0.275)	(0.436)
Proportion college	−3.292	−0.697	0.003	0.015	0.107	1.279	−0.423	−0.869
	(0.806)	(0.764)	(0.011)	(0.012)	(0.070)	(0.346)	(0.193)	(0.197)
Red governor	0.070	−0.006	0.000	0.000	−0.002	−0.002	−0.003	−0.001
	(0.011)	(0.008)	(0.000)	(0.000)	(0.001)	(0.004)	(0.002)	(0.002)
Taxation level	−0.071	−0.036	−0.000	−0.000	−0.001	0.002	0.000	−0.004
	(0.010)	(0.013)	(0.000)	(0.000)	(0.001)	(0.004)	(0.002)	(0.003)
Observations	675	675	675	675	675	675	675	675
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	No	No	Yes	Yes
R-squared			.748	.899	.458	.627	.765	.837
Log-likelihood	−15,969.132	−34,969.132						

Note: Robust standard errors are in parentheses. Poisson model in Models 1–2; Log-linear OLS model in Models 3–8.

TABLE 6 Effect of employment antidiscrimination laws according to state employment antidiscrimination litigation levels

	(1) Entrepreneurial foundings ± 10 years low litigation states	(2) Entrepreneurial foundings ± 10 years high litigation states	(3) Startup quality (patents) ± 10 years low litigation states	(4) Startup quality (patents) ± 10 years high litigation states	(5) Startup quality (VC) ± 10 years low litigation states	(6) Startup quality (VC) ± 10 years high litigation states	(7) Startup quality (survival) ± 10 years low litigation states	(8) Startup quality (survival) ± 10 years high litigation states
Antidiscrimination	−0.023	−0.074	0.002	0.003	0.018	0.061	0.011	0.016
	(0.024)	(0.015)	(0.000)	(0.000)	(0.005)	(0.016)	(0.005)	(0.006)
GDP per capita	0.000	0.000	0.000	−0.000	0.000	0.000	−0.000	−0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Proportion black	0.009	0.003	−0.000	−0.000	−0.000	0.001	−0.000	0.001
	(0.002)	(0.003)	(0.000)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
Job creation rate	−0.053	−1.251	0.003	0.028	−0.063	0.874	−0.105	−0.126
	(0.413)	(0.475)	(0.007)	(0.007)	(0.051)	(0.214)	(0.196)	(0.116)
Proportion women	3.832	4.208	−0.056	0.100	0.046	5.186	−0.084	1.323
	(1.445)	(2.157)	(0.023)	(0.032)	(0.197)	(1.674)	(0.262)	(0.542)
Proportion college	−2.749	−0.900	−0.007	0.027	0.156	0.830	−0.570	−0.683
	(0.725)	(0.801)	(0.010)	(0.011)	(0.076)	(0.336)	(0.166)	(0.262)
Red governor	0.044	−0.001	0.000	−0.000	−0.002	−0.004	−0.003	0.000
	(0.010)	(0.008)	(0.000)	(0.000)	(0.002)	(0.004)	(0.002)	(0.002)
Taxation level	−0.065	−0.024	−0.000	−0.000	0.000	0.006	0.000	−0.007
	(0.008)	(0.012)	(0.000)	(0.000)	(0.001)	(0.006)	(0.002)	(0.003)
Observations	675	675	675	675	675	675	675	675
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State trend	No	No	No	No	No	No	Yes	Yes
R-squared			.836	.904	.533	.620	.757	.835
Log-likelihood	−17,951.125	−32,951.125						

Note: Robust standard errors are in parentheses. Poisson model in Models 1–2; Log-linear OLS model in Models 3.

Knight (2015) and the proportion of self-employed individuals in other occupations.[9] Finally, we estimated the effect of our treatment on these proportions. Although given CPS' sampling procedure and the reduced number of years, results should be treated with caution, as can be seen in Table A5, the results are consistent with our prediction: Model (1) shows that, within the subsample of occupations with high LGBT concentration, the share of self-employed workers—conditional on year and states fixed effects—declined significantly after the treatment. The estimated effect is also economically sizable, as the proportion of self-employed in high LGBT occupation is equal to about 0.002. Model (2) estimates the same baseline specifications, but within the subsample of occupations with a low concentration of LGBT workers. In this case, the treatment dummy is positive, suggesting that the observed effect is indeed stronger among jobs in which LGBT workers tend to be more highly represented. Because self-employed individuals are the ones most likely to be pushed out of wage employment due to discrimination, these estimates lend additional support to our claims.

5.5.2 | Minority founders

As a further test of the mechanisms, we directly assess the effect of antidiscrimination institutions on the share of startups founded by minorities. Based on our theory, we expect that all minorities—even the ones not specifically targeted by the new legislation—will be less exposed to discrimination at work and thus less likely to transition to entrepreneurship, relative to nonminorities. This will be reflected in a lower share of new ventures founded by minorities, in general. We gather data on new ventures launched by minorities from the National Establishment Time-Series (NETS) database, which provides information on new establishments founded by minorities starting from 1989. Using these data, it is possible to identify new ventures founded by women as well as by other minorities. Results in Table A6, estimated using OLS, confirm our expectations: antidiscrimination legislation reduces the share of startups founded by minorities, and this result holds when minority groups are combined (Model 1) or assessed separately (Models 2 and 3). Our results (available upon request) are also robust to the use of a fractional logit model.

5.5.3 | Mediation effect

To the extent that antidiscrimination laws affect firm practices, we should further observe that employers are more likely to implement firm-level policies to protect sexual minorities and promote diversity, following the enactment of such laws (cf., link 1 in Figure 1). In turn, these firm-level policies should reduce the entrepreneurship rate but increase the new-venture quality.

To verify if this is the case, we re-estimate our baseline specification, but now measure the adoption of firm-level practices and assess whether a firm headquartered in a given state introduced pro-diversity policies in a given year. Our main proxy for firm-level adoption of diversity policies is the LGBT antidiscrimination programs, or whether the focal firm has in place progressive

[9]Following Tilcsik et al. (2015), we code the following occupations as having high concentration of LGBT individuals: Psychologists; Training and development specialists and managers; Social and community service managers; Technical writers; Occupational therapists; Massage therapists; Urban and regional planners; Producers and directors; Postsecondary teachers; Probation officers and correctional treatment specialists; Morticians, undertakers, and funeral directors; Physical therapists and exercise physiologists; Computer and information systems managers; Lawyers, judges, magistrates, and other judicial workers; Web developers.

LGBT policies, that is, polices aimed at the minority targeted by the antidiscrimination laws. Using the KLD data, we construct a dummy variable coded as "1" if, in a reference year, a firm had enacted LGBT antidiscrimination programs, and "0" otherwise. As another dependent variable, we model Diversity-promoting programs and measure the presence of corporate initiatives to promote other minorities, including women and racial minorities. Finally, we consider CSR programs, or the aggregated KLD score a firm receives for policies favoring different stakeholder groups (i.e., the community, employees, diversity, the natural environment, and customers).

We estimate OLS models controlling for firm size (measured as thousands of employees, source: COMPUSTAT), leverage (long-term debt over total financing, source: COMPUSTAT), cash holdings (source: COMPUSTAT), capital expenditure (source: COMPUSTAT), firm fixed effects, and year fixed effects. Consistent with our expectations, Table A7 shows that firms operating in states that enacted ENDA are more likely to introduce pro-gay/lesbian policies (Model 1), as well as diversity-promoting programs (Model 2). Strikingly, we also find an increase in firms' overall CSR scores, as indicated by the KLD index, following the enactment of ENDA (Model 3). In summary, these supplemental analyses lend support to our main mechanism, whereby state-level policies that prohibit discrimination lead to firm-level adoption of initiatives and programs to prevent discrimination in the labor market and to cater to a broad range of stakeholders (e.g., customers, employees, and communities).

The previous analysis confirms that the enactment of the antidiscrimination laws has induced companies to implement antidiscrimination corporate programs. However, to further establish that the effect of antidiscrimination institutions on entrepreneurship is mediated by antidiscrimination corporate programs, it is further necessary to establish that the latter programs have, in turn, reduced the rate of entrepreneurial foundings while increasing their quality and survival. This seems to be the case, based on the results presented in Table A8. Models (1)–(4)—estimated by a simple linear regression—show a positive coefficient on Gay-Lesbian Antidiscrimination Policies except for firm survival in Model 4. Because these results are suggestive but not definitive evidence for mediation, we additionally follow the approach by Shaver (2005) and estimate an instrumental variable regression, where we use ENDA as an instrument for corporate antidiscrimination, which is equivalent to assuming full mediation.[10] As can be seen in Models (5)–(8), when we instrument Gay-Lesbian Antidiscrimination Policies with the enactment of ENDA, we find similar effects: antidiscrimination policies are negatively associated with entrepreneurial and positively associated with the new-venture quality.[11]

5.5.4 | Individual-level experimental analysis

So far, our analyses have been conducted at the state level. However, our theory implies changes in individual-level job satisfaction, following the enactment of antidiscrimination policies. In turn, this increased job satisfaction will result in a lower individual likelihood of

[10]One limitation of these estimates, however, is the inability to test for whether the mediating effect is partial or full. One empirical approach would involve including in the same equation the mediated variable and the mediator. Although recent research (e.g., Shaver, 2005) has clearly shown this approach may bias estimates, for the sake of completeness, we have also taken this approach and our estimates—which, given all the endogeneity problem should be taken with the grain of salt—suggest the presence of partial mediation. Given endogeneity problems potentially affecting our estimates, we have decided not to include these last results in the current manuscript.

[11]As KLD data on corporate antidiscrimination programs are only available since 1991, we substitute missing values in previous years with zero. Using a linear extrapolation method to impute missing values gives us similar results.

transitioning to entrepreneurship. To validate the theorized micro-level links depicted in Figure 1, which cannot be tested for directly in observational data, we next turn to an experimental vignette study. Specifically, we examine whether the enactment of antidiscrimination policies in wage work enhances its attractiveness, inclining our subjects to forgo entrepreneurship. Using a sample of 424 online workers currently employed in the U.S. and holding at least a bachelor degree, we attempted to validate this argument in an online experiment. To obtain variation in antidiscrimination laws, we used three different scenarios, two of which are control groups and one is a treatment group.[12] The first control group received a generic description with no reference to any laws ("baseline"). The treatment group received the same description as in the baseline, but with supplemental information describing the enactment of the employment antidiscrimination laws. To account for the treatment group receiving more information than the control group, we used a second control group in which we supplemented the baseline scenario with additional generic information about the enactment of state laws unrelated to the decision to leave paid employment or to become an entrepreneur. After receiving all the information, subjects were then asked about: (a) their willingness to leave current employment to become an entrepreneur and (b) how satisfied they will be working for their current employer. Table A9 shows the phrasing of the four messages, as well as the complete script of the experiment.[13] Table A10 reports the means across the three conditions to validate random assignment and establish that the key respondent characteristics were balanced across groups.

The results are presented in Table A11. As can be seen, participants in the treatment group (Model 3) were less willing to found their own business (mean = 2.18) than participants in the baseline control group (mean = 2.62; Model 1) or length-matched control group (mean = 2.57; Model 2). Further, participants in the treatment group (Model 3) report greater perceived attractiveness of wage employment (mean = 3.88) than participants in the baseline control group (mean = 3.38; Model 1) or length-matched control group (mean = 3.49; Model 2).[14] Together, these results provide supportive evidence that initiatives designed to prohibit employment discrimination enhance workplace appeal and thus reduce the attractiveness of leaving for entrepreneurship (link 2 in Figure 1).

Overall, our extensive set of empirical results provide broad support for our model reported in Figure 1 and its causal pathways, linking minority protection and entrepreneurship. First, we found strong support for (H1) and (H2), the macro-to-macro link—and we have shown (H3 and H4) that this effect is stronger where it is expected to be so if the mechanism is the envisioned one. We also have shown that the enactment of ENDA has led to higher organizational compliance (link 1, Figure 1), and that organizational compliance leads to lower levels of

[12]We only included in our sample participants who passed the attention check. Participants who failed or did not complete the survey were removed from the sample.

[13]An important concern in survey design is the possibility of social desirability bias, or the tendency of participants to present themselves in a socially acceptable way. In our case, social desirability bias may motivate the respondents to express favorable opinions about diversity initiatives. We mitigated this concern in two ways. First, we administered the survey in an anonymous manner and informed the respondents that their identity would remain undisclosed. Second, the respondents were told that they would receive compensation regardless of their views. Finally, we implemented the technique of "indirect questioning," which has been shown to reduce social desirability bias. Specifically, we presented the respondents with a hypothetical scenario and asked them to make decisions regarding entrepreneurship.

[14]We additionally find that the treatment condition is associated with lower willingness to found a business for both historically discriminated participants as well as for non-minorities. This lends additional support to the spillover effects to non-minorities, as hypothesized.

entrepreneurship—in particular for minorities (link 2, Figure 1). While this evidence is strongly supporting our theory, we also consider alternative explanations— and rule them out empirically.

5.6 | Alternative explanations

5.6.1 | Entrepreneurship Costs

An important alternative explanation could be that antidiscrimination laws increase the costs of founding a new venture, due to additional training requirements or potential lawsuits. Allegedly, this would be the case if antidiscrimination laws "raise the cost of doing business, which makes everyone suffer, because businesses pass those costs on to consumers in the form of higher prices."[15]Although plausible, this concern is unlikely for several reasons. First, nonpartisan sources such as the Congressional Budget Office of 2013 estimated the ENDA costs to the private sector to be relatively small, and mostly administrative.[16] Second, our effect is amplified in states with greater representation of LGBT individuals, states with greater levels of discrimination, and occupations with a higher concentration of LGBT workers. These findings would be difficult to explain with increases in costs alone. Nonetheless, we conduct supplemental analyses to inspect this alternative explanation even further.

First, we examine whether our treatment is weaker in states (California, Connecticut, Delaware, Illinois, Iowa, Maryland, Massachusetts, Nevada, New Hampshire, New Mexico, New York, Rhode Island, and Washington) that allows smaller firms to be exempted from compliance.[17] If our effect is driven by an increase in costs for entrepreneurs, we would expect that the negative effect we observe is mitigated in those states—presumably because entrepreneurs would be less concerned about the costs when truly small firms (often startups) are exempted from complying to the legislation. To assess this cross-sectional heterogeneity, we re-estimate the baseline specification but model the effect of ENDA when they apply to all firms versus when they apply to only larger firms separately. As Table A12 shows, the coefficients of the antidiscrimination law are similar (in terms of the magnitude or statistical significance) regardless of whether the law applies to any firm or alternatively only to firms larger than a certain size threshold. If anything, the effect of the antidiscrimination laws is stronger in states that exempt small firms from compliance. This finding provides some reassurance that costs, which could potentially be imposed on entrepreneurs following the passage of ENDA, are an unlikely driver of our effects.

Second, we investigate the effect of our treatment on exit rate. If employees are less likely to become entrepreneurs because the expected costs of launching a new venture increase, we should observe not only a decrease in new-firm entry, but also an increase in firm exit, based on the presumption that the increased costs of litigation will force small existing businesses out. We thus collected data on firm exit from the LBD dataset and verified the effect of ENDA on exit. Table A13 shows that antidiscrimination laws have no effect on exit. This result is not consistent with the idea that antidiscrimination legislation leads to increased business costs of new ventures.

[15]See: http://executivelp.com/laws-regulations/21st-century-effects-of-discrimination-do-we-still-need-anti-discrimination-laws/. Accessed August 2017.

[16]The Congressional Budget Office of 2013 estimated that changes to comply with ENDA would be "relatively minor and would be made in the course of other routine updates" (Congressional Budget Office. S. 815, Employment Non-Discrimination Act of 2013. September 11, 2013. http://cbo.gov/sites/default/files/cbofiles/attachments/s815.pdf).

[17]The minimum firm size threshold below which the law does not apply might vary from 3 employees (as in Connecticut) to 15 employees (as in Illinois or Nevada).

5.6.2 | Economic expansion or downturn

The passage of pro-LGBT legislation might trigger changes in the economy, leading to either expansion or downturn. Such changes, might, in turn, lead to lower rates of entrepreneurship and higher rates of high-quality ventures. As mentioned before, however, prior research finds positive effects of pro-LGBT legislation on firm productivity and innovation (e.g., Nguyen et al., 2020; Vakili & Zhang, 2018), thus mitigating a concern that economic downturn might drive our results. Our findings are similarly unlikely to be driven by economic expansion because the estimates are robust to including state-level GDP per capita in our models. However, to additionally, mitigate this concern, we re-estimate the baseline specifications from Table A14, but now include a control for GDP growth rate (in addition to GDP per capita), which can capture economic growth more precisely, and our results are fully recovered.

5.6.3 | Simultaneous shocks

One might be concerned that events contemporaneous with the enactment of antidiscrimination laws are responsible for the change in patterns of entrepreneurial entry. However, evidence for the mechanisms, as discussed in the previous section, mitigates this concern: if our estimates reflect other legal changes around this time, then we would not observe a number of outcomes, as our theory predicted. Furthermore, the staggered enactment of the laws makes the occurrence of exactly simultaneous unobserved events less likely. Still, to increase confidence in our findings, we collected data on other legal reforms implemented in the United States within the timeframe of our empirical analysis. We focus on key legal changes either shown to have an effect on entrepreneurship or expected to influence entrepreneurial rates and quality in theory. These include changes in trade-secrets protection via the Uniform Trade Secrets Act (UTSA; Castellaneta, Conti, & Kacperczyk, 2017; Png, 2012), bank deregulation laws (Kerr & Nanda, 2009), and variation in judicial selection methods, which affect the independence of the judiciary from incumbent businesses (Conti & Valentini, 2018). Some of these reforms occurred in our treated states or in neighboring states. Hence, one might be concerned that these reforms affected entrepreneurship, leading to spurious correlations in our estimates. To mitigate this challenge, we re-estimate the baseline specifications with additional controls for years in which these changes occurred. As shown in Table A15, adding these additional covariates to our basic specification does not change the significance of our main estimates, with respect to either the number or quality of new ventures.

6 | DISCUSSION

Although ample research investigates the individual and organizational antecedents of an individual's entry into entrepreneurship (e.g., Kacperczyk, 2012), factors responsible for this transition are not fully understood—given that returns to entrepreneurship tend to be lower than earnings in wage work (e.g., Hamilton, 2000; Moskowitz & Vissing-Jørgensen, 2002). The lion's share of research has documented the critical importance of the institutional environment in driving entrepreneurship, but the extant studies have predominantly focused on institutional changes that target *prospective founders*, by facilitating access to resources. Drawing on research that considers entrepreneurship to be a mobility decision (e.g., Hellmann, 2007; Kacperczyk, 2012; Sørensen & Sharkey, 2014), we argue and find support for the claim that institutional changes that target

current employees, by enhancing their work conditions and reducing employer discrimination play a critical role in influencing entrepreneurship, for two reasons. First, as discrimination subsides, wage work becomes more appealing, depressing founding rates. Second, because the threshold for giving up wage work rises, such declines result in the higher growth potential of new ventures launched. Overall, workers respond to declines in employer bias by being less willing and less motivated to found new ventures, but those who nevertheless transition to entrepreneurship do so primarily to explore high-growth opportunities.

Whereas past research on the impact of institutional protections of minority workers on entrepreneurial firms remains scant, we theorize and find empirical support for the claim that institutions that prohibit employment discrimination are likely to trigger two opposite effects on entrepreneurship. First, such regulations are expected to suppress entry into entrepreneurship, by making the workplace more attractive to the protected class, other minorities, and some nonminority workers alike. Lending support to this expectation, we find that the enactment of ENDA, which banned employment discrimination on the basis of sexual orientation and gender identity, had a negative effect on entrepreneurial foundings, leading to lower rates of new-venture creation. Second, although the rate of new-venture creation will fall, the quality of new ventures is expected to rise, given that institutional protection from discrimination will increase the threshold for giving up paid employment. Together, therefore, these main findings lend support to our theoretical framework, documenting the profound and so-far neglected impact of institutional protection from employment discrimination on the founding rates and quality of new ventures.

Our analyses probed deeper into the mechanisms we theorized. First, consistent with the notion that entry into entrepreneurship fluctuates vis-à-vis the attractiveness of paid employment, we find that our effects are amplified in states with higher representation of the protected minority workers. In addition, we find that the negative effect of the institutional protection of minority employees on entrepreneurship, including the rate of entry and quality, is amplified in states where litigation against employers regarding workplace discrimination is more common or more prevalent. In tandem, these cross-sectional, heterogenous effects lend additional evidence to the mechanisms we theorize: that the relative appeal of paid employment increases, as institutional protection of disadvantaged groups intensifies.

Our work is also defined by some limitations. First, as in most quasi-experimental studies using institutional variation for identification and despite a battery of robustness tests, we cannot indisputably claim that the regulatory change represents a fully exogenous source of variation; hence, our results should be interpreted with caution even if they remain consistent with the vignette experiment in which we leverage random assignment. Second, we conceptualized the mechanisms underlying our macro-to-macro link (see Figure 1) and offered additional tests to probe these processes deeper. But further opportunities exist to test these micro-level processes with greater precision. For example, future studies could focus on more fine-grained, employer-employee data to model individual transitions from paid employment into entrepreneurship as well as additional experimentally-collected data. With such data at hand, researchers may want to further investigate whether the effects we document vary systematically across employees occupying different positions or organizational ranks within organizations.

Despite these limitations, our results carry relevant implications for theory and practice. First, our study contributes to the vibrant body of work on the determinants of entrepreneurial entry (e.g., Kacperczyk, 2012; Moskowitz & Vissing-Jørgensen, 2002; Stenard & Sauermann, 2016). Whereas ample research has documented the influence of the institutional environment in driving entrepreneurial rates (e.g., Eberhart et al., 2017; Hiatt et al., 2009; Sine, Haveman, and Tolbert, 2005); past work has primarily considered the impact of regulations targeting *prospective*

founders. By contrast, we shift the focus of these debates to consider regulations targeting *current employees*, given that entrepreneurship can be conceptualized as a mobility process (e.g., Kacperczyk & Marx, 2016; Sørensen & Sharkey, 2014). We thus bring these recent conversations with research on institutional protection from employer discrimination (e.g., Kelly & Dobbin, 1999) to develop and evaluate a new explanation for entrepreneurial entry: institutional protection from employer discrimination. Our study thus extends the line of work on entrepreneurship determinants (e.g., Campbell et al., 2012; Carnahan et al., 2012; Kacperczyk, 2012), while also contributing to the growing scholarly interest in institutional constraints imposed on individuals' careers and their role in shaping the decision to become a founder (Hwang & Phillips, 2020; Agarwal et al., 2021). By documenting that entrepreneurial entry rates and new-venture quality are influenced by labor market institutions, we thus establish the unintended effects of such institutions on startup creation and quality.

In addition, we extend the scope of the strategy and organizational research on the effects of institutional protection from employer discrimination on firm performance and competitive advantage (e.g., Negro & Olzak, 2019; Nguyen et al., 2020; Gao & Zhang, 2017; Vakili & Zhang, 2018; Shan et al., 2017; Wang & Schwarz, 2010). Whereas past research has documented the beneficial impact of such initiatives on established, mature firms, we show that institutional protection against discrimination affects entrepreneurial ventures alike, by impacting wage workers and their perception of paid work. In short, our study offers evidence that protecting minorities from employer discrimination can have unintended consequences on the creation and the quality of new ventures.

From a policy perspective, our findings illuminate the critical importance of institutional protection from employment discrimination for economy-wide outcomes, beyond mature, established firms alone. This further suggests possible labor market interventions that can have major policy implications. For example, interventions aimed at increasing the quality of entrepreneurship might focus on reducing discrimination in paid work, but these measures should also target regions and states with a higher representation of the discriminated groups and greater prevalence of litigation against employment discrimination ex-ante. Our findings suggest that it is in those cases that the effects of institutional protection from employment discrimination on entrepreneurship are most pronounced because discriminated groups will be most likely to leverage institutional mechanisms to fend off discriminatory acts.

In summary, our study makes important contributions to theories and empirical work on institutional protection against discrimination in wage employment and research on entrepreneurship. Importantly, whereas the predominant focus in past research has been on the institutional factors that target *prospective founders*, we advance current theories by developing a framework to understand the impact of institutional factors that target *employees* on entrepreneurship, its creation, and quality. Our study shifts away from the traditional approach by suggesting work on the importance of the institutional environment needs to move beyond considering initiatives designed to increase entrepreneurial rates. Incorporating initiatives implemented with employees and their welfare in mind into the current theories and frameworks is the next step toward a clearer understanding of the impact of institutions on the creation of new firms.

DATA AVAILABILITY STATEMENT

The study uses a mix of data derived from public domain resources and data with embargo due to third party restrictions. Research data are not shared.

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