

**The Missing Entrepreneurs. The Impact of Antidiscrimination on Entrepreneurship:
Evidence from LGBT Rights Laws**

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This study revisits the well-established claim that reducing discrimination spurs entrepreneurial entry. We propose that the effect of antidiscrimination initiatives on entrepreneurial foundings depends crucially on whether discrimination originates on the demand side or the supply side of the entrepreneurial process. The universal benefits of antidiscrimination practices in the context of entrepreneurial entry are based on the study of demand-side discrimination, or when prospective entrepreneurs face discrimination by key resource providers for a new venture (i.e., investors, banks, prospective employers, and customers). Yet we hypothesize the opposite effect on the supply side, or when prospective entrepreneurs face discrimination in the current workplace. Using evidence from the enactment of LGBT antidiscrimination policies, we show that workplace antidiscrimination initiatives deter entry into entrepreneurship because they increase the attractiveness of current employment and increase the opportunity cost of becoming an entrepreneur. Despite the reduction in the overall rates of entrepreneurship, however, the share of high-quality entrants increases, because antidiscrimination policies motivate the pursuit of highest-potential opportunities. Finally, these two effects are amplified in states with higher rates of discrimination, in general, where the benefits of antidiscrimination practices are most acute.

INTRODUCTION

A central question in organizational research is what conditions spur entrepreneurship, by inclining individuals to pursue a new venture (Aldrich and Ruef, 2006; Audia and Rider 2006; Kacperczyk, 2012; 2013; Kacperczyk and Younkin, 2017). Significant scholarship has equated successful participation in entrepreneurial activity with the absence of barriers, though, in many cases, to systematically undermine an individual's ability and motivation to pursue a new venture (Easley, 2016; Kouriloff, 2000; Thornton, 1999). Sociological research, in particular, has identified discrimination as key obstacle to broad participation in entrepreneurship, with evidence of bias as key barrier to an attempted entry by various minority groups, including women (Buttner and Rosen 1989, Thébaud 2010, Tinkler, Bunker Whittington et al. 2015), non-Whites (Younkin and Kuppuswamy, 2017; Fairlie and Robb, 2007; Blanchflower, Levine and Zimmerman 2003), immigrants (Teixeira and Truelove, 2007), or elderly (Zhang, 1978). A consensus is thus building that antidiscrimination practices and the resulting reduction in discrimination would democratize access to entrepreneurial opportunities, increasing the overall rates of new-venture foundings. In short, scholars have reached a general conclusion that, "supporting entrepreneurs during the life cycle of their business requires promoting equal opportunity and abolishing discrimination." (Asher, 2012: 10)

Despite the prominence of these arguments in the literature to date, however, the relationship between antidiscrimination initiatives and the rates of entrepreneurial entry is poorly understood. In prescribing antidiscrimination practices as a remedy to boost entrepreneurial activity, previous studies have solely considered the demand-side discrimination, which arises when prospective entrepreneurs face discrimination by key resource holders, including angel investors (Ewens and Townsend, 2017), venture capitalists (Brush, 2001; Brush et al., 2004), banks and loan officers (Blanchflower, Levine and Zimmerman 2003; Thebaud and Sharkey, 2014), start-up employees (Kacperczyk, Younkin, and Balachandran, 2017), crowds (Younkin and Kuppuswamy, 2017), and consumers (Borjas and Bronars 1989; Meyer 1990; Kawaguchi 2004). Whereas the focus in past research has been limited to the demand

side, claims regarding the benefits of reducing discrimination to boost entrepreneurial rates have been general.

Importantly, in making such universal claims, researchers have largely neglected the supply side of the entrepreneurial entry. Yet the decision to enter entrepreneurship is also driven by the supply-side factors, or the availability of individuals to occupy entrepreneurial roles (Thornton, 1999). Growing research has shown, in particular, that the opportunity structure in paid employment affects the supply of prospective entrepreneurs by influencing the decision to leave in pursuit of a new venture (Hellman, 2008; Kacperczyk, 2012; Sorensen and Sharkey, 2014). And whereas discrimination may also originate on the supply side, by constraining the availability of opportunities and resources within the workplace, prior research has not considered how antidiscrimination initiatives affect entrepreneurial entry when these programs pertain to the supply side.

In this study, we therefore shift the prevalent focus on antidiscrimination pertaining to the demand side, or efforts to reduce bias held by key resource providers (i.e., investors, banks, crowds, employees, or customers), to focus on antidiscrimination pertaining to the supply side, or efforts to reduce bias in the current workplace. We build on the growing line of work, which considers transition into entrepreneurship to be a career choice and a mobility process (Audia and Rider 2006, Gompers et al. 2005, Sorensen and Sharkey, 2014; Rosti and Chelli, 2005). A fundamental insight in this research is that entrepreneurial transitions are intertwined with one's career prospects elsewhere and that employees weigh entrepreneurial entry against continuing to work in paid employment (e.g., Kacperczyk 2012, Kacperczyk and Marx 2016, Sorensen and Sharkey 2014). Significant to the question at hand, this perspective posits that entry becomes less appealing when alternative prospects appear more attractive and more costly to give up (e.g., Amit et al., 1995; Kacperczyk and Marx, 2016). Building on this research, we propose that reducing discrimination on the supply-side, or by an employer, will *reduce* the overall rates of entrepreneurial entry. Consistent with the career perspective on entrepreneurship, we expect antidiscrimination initiatives to enhance the attractiveness of current employment relative to entrepreneurship. Specifically, given the high opportunity cost of leaving attractive employers (Jackson *et*

al., 1991; O'Reilly, Caldwell, and Barnett, 1989; O'Reilly and Chatman, 1986; Burbano, Mamer, and Snyder, 2013), we propose that employees will be generally less likely to transition into entrepreneurship, following an introduction of workplace antidiscrimination initiatives.

As further tests of our claims, we expect that antidiscrimination initiatives in the workplace will increase the threshold for choosing to explore a new opportunity through entrepreneurship. Given that only valuable opportunities will be worth the cost of giving up attractive employment, we expect workplace antidiscrimination policies to induce the foundings of growth-oriented ventures pursued, as manifested in higher rates of startups with patents and access to venture-capital funding. Moreover, we expect these relationships to be amplified for firms in states where discrimination is more common and more prevalent, in general, because employees in those states benefit the most from workplace antidiscrimination programs. Finally, our results imply that initiatives aiming to reduce workplace discrimination will incline individuals to stay with current employer rather than move. Hence, we expect that, following the introduction of antidiscrimination initiatives, the overall job creation and destruction in established firms will decrease, as employees are less likely to engage in inter-firm mobility.

The relationship between antidiscrimination interventions in the workplace and entrepreneurial rates is difficult to address empirically because such programs are likely endogenous with respect to entrepreneurship. In particular, finding a negative relationship between antidiscrimination practices and entrepreneurial-entry rates may be spurious if such relationship is driven by unobserved firm characteristics that enhance a firm's behavior with regards to both antidiscrimination policies and entrepreneurial spawning. This concern is particularly severe given that firm-level attributes, such as managerial style, while difficult to observe, are likely to drive a firm's investments in antidiscrimination programs and innovative activities likely to propel entrepreneurial spawning. Moreover, the relationship between antidiscrimination policies and entrepreneurial entry is subject to reverse causality concerns. For example, a negative correlation between workplace antidiscrimination policies and the rates of entrepreneurship may indicate that firms that generate fewer entrepreneurs might do so because of limited slack resources allocated to innovation; those resources might be, in turn, allocated to cater to the interests

of minority groups in order to reduce discrimination. In short, while empirically challenging, leveraging a research design that provides a clean causal estimate is central to understanding the impact of antidiscrimination initiatives in the workplace on entrepreneurial rates.

We address this empirical challenge by exploiting a quasi-natural experiment provided by the staggered enactment of Employment Non-Discrimination Acts (ENDAs) in twenty-one states, spanning the period of thirty-two years. We take advantage of this natural experiment for three reasons. First, such acts prohibit discrimination based on sexual orientation and gender identity, thus matching in importance some of the earlier acts against workplace discrimination on the basis of race, gender, religion, national origin, and physical disability. Second, there is ample evidence suggesting that these acts have significantly reduced workplace discrimination on the basis of sexual orientation or sexual identity (Flatt and Klawitter, 1998; Ragins and Cornwell, 2001; Barron, 2009; Barron and Hebl, 2013). Finally, because of their exogenous and staggered nature, the effects of these laws can be modeled using a difference-in-differences methodology—with the “treatment” group composed of states that are subject to these acts, and the “control” group composed of states that are not.

Our results show that the enactment of these laws leads to a significant decrease in the rates of entrepreneurship. At the same time, the average quality of new ventures founded increases, mainly because initiatives that reduce discrimination increase the threshold for giving up paid employment in pursuit of entrepreneurship. Together, the findings indicate that implementing initiatives to reduce discrimination, when the latter originates on the supply side, stifles the overall rates of entrepreneurship, with an exception of high-quality ventures for which founding rates increase.

THEORY

Antidiscrimination and Entrepreneurial Entry

Many studies have argued that discrimination, which involves stereotypes and biases toward minority groups, stifles participation in entrepreneurship by blocking some groups of aspiring entrepreneurs from access to opportunities and capital on which the entrepreneurial entry depends (Buttner and Rosen, 1988; Belcourt et al. 1991; Goffee and Scase 1983; Hisrich and Brush 1984; Humphreys and McClung 1981;

Reuber, et al. 1991; Stevenson 1986; Heilman and Chen 2003; Zhang, 1978). In general, scholars have argued that interventions that aim to reduce discrimination will foster the overall rates of participation in the entrepreneurial activity (e.g., Asher, 2012). And indeed, numerous studies on disadvantaged groups, including women and racial minorities, seem to have reached a similar conclusion about the value of antidiscrimination policies for boosting entrepreneurial activity.

First, studies on gender gap in entrepreneurship have commonly concluded that discrimination based on negative stereotypes about gender and cultural beliefs about the value of masculinity in the context of entrepreneurship, dramatically stifles the rates of entrepreneurial entry by preventing women from accessing opportunities and resources on which entrepreneurial entry depends (e.g., Carter and Shaw 2006; Cavalluzzo, Cavalluzzo and Wolken 2002; Thebaud, 2010; Thebaud and Sharkey, 2014). Because gender disparities in access to opportunities and resources tend to persist even after factors such as human capital, industry, and credit histories are taken into account, scholars have generally suggested that interventions to reduce negative bias would promote female participation in entrepreneurship, leading to an increase in entrepreneurial rates, in total (Jennings and Brush 2013 for a review). Similarly, in the context of racial minorities, scholars have advocated the key role of antidiscrimination interventions in facilitating entry into entrepreneurship by disadvantaged groups. Gold (2016), for example, argued that mitigating racial inequality would increase entrepreneurial opportunities for Blacks and therefore increase the overall rates of participation in entrepreneurship. Along the same lines, Younkin and Kuppuswamy (2017) offered a number of interventions to mitigate racial bias in order to increase the odds of raising capital on a crowd-funding platform and therefore increase entrepreneurship. Consistently, Blanchflower, Levine, and Zimmerman (2003) found that black-owned small businesses were more likely to be denied credit even after controlling for differences in creditworthiness. The authors attributed this gap to discrimination and argued that programs and regulatory incentives to reduce racial bias would increase minorities' access to bank loans, boosting entrepreneurial activity. Finally, echoing these findings, policy makers have emphasized the necessity to “create or change existing laws to reduce or eliminate

discrimination and to promote equality between people and social groups” (2013: 107).¹ Overall, then, scholars and practitioners alike have commonly claimed that there exists a positive relationship between interventions aiming to reduce discrimination and the rates of entrepreneurial entry.

Despite the prominence of these arguments, however, the relationship between antidiscrimination interventions and entrepreneurial rates has been poorly understood. Whereas previous studies have highlighted the general benefits of reducing discrimination in the context of entrepreneurial entry, these overarching conclusions have often been drawn from the study of demand-side discrimination alone, which arises when some groups of prospective entrepreneurs are put at a systematic disadvantage in an effort to access opportunities and resources critical for launching a new venture. A common implication of these studies is therefore the notion that reducing negative biases boosts entrepreneurial activity, because it increases access to resources held by professional investors, consumers, crowds, or employees (Ewens and Townsend, 2017; Brush, 2001; Brush et al., 2004; Blanchflower, Levine and Zimmerman 2003; Thebaud and Sharkey, 2014; Kacperczyk, Younkin, and Balachandran, 2017; Younkin and Kuppuswamy, 2017; Borjas and Bronars 1989; Meyer 1990; Kawaguchi 2004).

But whereas scholars have made general claims about the benefits of antidiscrimination for entrepreneurship, they have not examined the plausibility of this claim in the context of supply-side discrimination. Yet transition into entrepreneurship is also driven by the supply-side factors, or the availability of individuals to leave paid employment in order to occupy entrepreneurial roles (Thornton, 1999). Growing line of research suggests, for example, that whether an individual decides to enter entrepreneurship is partly a function of an employer’s characteristics that influence a worker’s decision to give up opportunities in paid employment and pursue entrepreneurship instead (Amit, 1995; Hellman, 2008; Kacperczyk, 2012; Sorensen and Sharkey, 2014; Audia and Rider, 2006). Despite the crucial importance of these supply-side factors in driving an individual’s decision to leave paid employment for entrepreneurial entry, prior studies have not considered how reducing discrimination on the supply side

¹ “The Missing Entrepreneurs. Policies for Inclusive Entrepreneurship in Europe.”

might impact entrepreneurial entry. In short, whereas the benefits of reducing discrimination have been commonly acknowledged, whether these claims continue to hold when discrimination subsides on the supply side (i.e., in the workplace) has not been tested.

Opportunities in Paid Employment and Transition into Entrepreneurship

A growing line of entrepreneurship research suggests that, since most entrepreneurs typically emerge from existing organizations (Bide, 1992; Audia and Rider, 2005), the supply-side factors, such as characteristics of current employment, play an important role in driving an individual's decision to pursue entrepreneurship (e.g., Audia and Rider 2005, Gompers et al. 2005; Sorensen and Fasiotto, 2011).

Scholars have claimed, for example, that entrepreneurship is intertwined with career options in paid employment and is therefore best examined as a career-mobility decision (Kacperczyk 2012, Kacperczyk and Marx 2016, Sorensen and Sharkey 2014). The argument follows that individuals evaluate entrepreneurial entry relative to other career options available within paid employment (Kacperczyk 2012, Kacperczyk and Marx 2016, Sorensen and Sharkey 2014). Importantly, employees are more likely to transition into entrepreneurship when entry is more compelling than alternative career options in paid employment (Kacperczyk 2012, Kacperczyk and Marx 2016, Sorensen and Sharkey 2014; Rider et al., 2016).

For example, past research has found that mutual fund managers were less likely to leave for entrepreneurship when opportunities for internal venture development were available in current employment, making attachment to current employment more attractive (Kacperczyk, 2012). Other research has similarly shown that software engineers were less likely to enter entrepreneurship when they were employed at large firms with more attractive opportunities related to promotion, wages, or internal resources, amongst others (Kacperczyk and Marx, 2016). Still others have argued that employees were more likely to enter entrepreneurship when their internal advancement options were blocked, or when they were provided with fewer, and less attractive, offers from other firms, conditional on mobility (Sorensen and Sharkey, 2014). Echoing these arguments, there is evidence that employees affiliated with more attractive employers are less inclined to self-select into entrepreneurship because the opportunity

cost of doing so is higher (e.g., Amit et al. 1995; Anton and Yao 1995; Hellman, 2007; Sorensen and Sharkey, 2015). For instance, there are consistent findings that workers are less likely to become entrepreneurs when they earn higher wages in paid employment (Amit et al., 1995), but more likely to do so when they experience disagreements with current employer (Klepper, 2009), are unable to pursue their own initiatives (Hellman, 2007), or are less likely to receive external offers from employers (Sorensen and Sharkey, 2014). In all cases, the opportunity cost of leaving current employment or joining another firm is sufficiently low to motivate individuals to pursue entrepreneurship in lieu of paid employment. Together, these studies underscore the role of options available in paid employment and the opportunity cost of leaving in shaping the propensity to become an entrepreneur.

Antidiscrimination and Entry into Entrepreneurship

There is a rationale to expect that reducing discrimination on the supply side (i.e., in the workplace) will affect entrepreneurial rates by enhancing the attractiveness of paid employment and the consequent opportunity cost of giving it up. To the extent that antidiscrimination initiatives lead to implementation of firm-level practices that protect disadvantaged groups from bias and promote openness to diversity, these initiatives will reduce entrepreneurial entry in two ways.

First, the attractiveness of advancement options within current employment as well as with other employers should significantly increase for minority groups, when discrimination in the workplace subsides. The argument follows that these kinds of initiatives provide disadvantaged groups with more equitable advancement options; for example, there is evidence that hiring and promotions of minorities increase dramatically as a result of diversity programs put in place in response to antidiscrimination legislation (Baron et al. 1991:1386; Donohue and Siegelman 1991; Kalev and Dobbin forthcoming; Leonard 1984; Skaggs 2001). As advancement options become more equitably distributed within organizations, minority members will be more inclined to join and remain in the firm, in expectation of better, more enticing career opportunities. Consistent with this claim, the supply-side approaches to labor-market discrimination have provided empirical evidence that women and minorities are more attracted to employers with formal personnel systems put in place to prevent discrimination (DiPrete 1989;

Barbulescu and Bidwell, 2013). Hence, these studies suggest that antidiscrimination policies and programs increase the perceived opportunity structure in the firm, making any given employer more appealing and more costly to leave behind. Importantly, this argument implies that those subject to discrimination will be more likely to keep their attachment to paid employment and less likely to transition into entrepreneurship, as antidiscrimination initiatives are adopted.

Second, diversity-promoting initiatives may reduce the rates of entrepreneurial entry by enhancing the perceived attractiveness of paid employment to other employees, even those not be subject to discrimination *per se*. Extensive research in sociology and strategy highlights the benefits of openness to diversity. The “creative class” argument, for example, posits that tolerance and openness to diversity play an important role in attracting valuable talent in knowledge industries (Jacobs, 1961; Florida, 2002a, 2002b; Page, 2007; Florida et al., 2008; Wedemeier, 2014). Florida (2014) argues that knowledge workers at highest risk of becoming entrepreneurs, find diversity-promoting organizations appealing because “creative class” tends to value diversity and environment in which everyone is included. Even if they are not members of minority groups themselves, high-human capital workers tend to self-sort into the kind of environments in which social benefits and opportunities extend to minority groups, including women, non-Whites, or gay (Florida, 2014).

A similar argument — that reducing discrimination in the workplace and promoting diversity enhances the value employees place on their employment —is also prevalent in the line of research on corporate social responsibility (CSR) (Burbano, Mamer, and Snyder, 2013; Flammer and Kacperczyk, 2015). Studies in this vein have suggested that CSR practices, many of which focus on promoting diversity, enhance employee commitment and job satisfaction, in general (Dimarco, 1975). This might be because firms perceived as being fair and caring towards *all* groups of employees are able to reap significant reputational benefits (Greening and Turban, 2000). Indeed, diversity practices have been found to boost workplace engagement, the feelings of inclusion, and trust climate (Downey, Werff, Thomas and Plaut, 2014). An employer’s efforts to promote diversity initiatives further enhance employee loyalty and work engagement because employees infer from firms’ engagement in diversity and other social

initiatives whether the managers and the organization are fair-minded (Aguilera *et al.*, 2007) and evaluate whether the employer is an accurate fit (Kim *et al.*, 2010). Presumably, loyal employees are less inclined to leave the current workplace and therefore less likely to transition to entrepreneurship. Importantly, because knowledge workers value practices aiming to increase inclusion and equality, firms are willing to strategically implement these programs when competing for talent (Flammer and Kacperczyk, 2017). For example, there is evidence that employers open to diversity are better positioned to attract (e.g., Albinger and Freeman, 2000; Burbano, 2016; Greening and Turban, 2000; Turban and Greening, 1996) and retain knowledge workers (e.g., Burbano, Mamer, and Snyder, 2013; Jackson *et al.*, 1991; O'Reilly, Caldwell, and Barnett, 1989; O'Reilly and Chatman, 1986).

Together, these studies suggest that sanctioning workplace discrimination and accommodating the needs of minority groups is key to enhancing the value of paid employment for workers, including members of minority groups and other employees alike. Accordingly, such initiatives should increase the attractiveness of paid employment and the corresponding opportunity cost of leaving current employer in pursuit of entrepreneurship. Following an adoption of antidiscrimination initiatives, workers will therefore be more likely to keep their attachment to paid employment and forgo entrepreneurial opportunities, contributing to lower rates of new-venture creation, overall.

If antidiscrimination enhances the attractiveness of paid employment and consequently increases the opportunity cost of leaving for entrepreneurship, the threshold for exploring entrepreneurial opportunities will also increase. Put differently, in instances where employees chose to sort into entrepreneurship and gave up compelling career options elsewhere, entrepreneurial opportunities were likely more promising and more enticing *ex-ante*, leading to the founding of ventures with higher-growth potential. Hence, if opportunity cost of entrepreneurship increases following an adoption of antidiscrimination initiatives, the average growth potential of new-ventures *ex-post*, will rise. This leads to the following predictions:

H1: Workplace antidiscrimination initiatives will reduce the rate of entrepreneurial foundings.

H2: Workplace antidiscrimination initiatives will increase the average quality of entrepreneurial foundings.

Mechanisms: State Heterogeneity and Effect on Employee Turnover in Established Companies

Our core argument suggests that the opportunity cost of transitioning into entrepreneurship increases following the adoption of antidiscrimination practices in the workplace, because opportunities in paid employment become more appealing for worker groups subject to discrimination as well as other employees. In what follows, we investigate this mechanism in greater detail by assessing whether our findings are systematically moderated under some conditions or whether they are associated with other outcomes.

First, we expect cross-sectional heterogeneity in new-venture foundings across states. If our supposition is plausible, the negative treatment effect of workplace initiatives to reduce discrimination will be most acute for firms located in states with higher levels of discrimination, in general. When discrimination of minority groups is more common and more prevalent, sanctioning discriminatory practices proves particularly effective. Consistent with this claim, Wald et al., (1996) argue that the treatment effect of gay rights legislation is weaker in areas where discrimination is lower prior to the enactment of antidiscrimination legislation because these areas are more accepting of gays and lesbians in the first place. Accordingly, we expect that a decrease in entrepreneurial foundings and an increase in the average quality of new ventures will be amplified in states that are more prone to discrimination, in general.

H3a: A reduction in the rate of entrepreneurial foundings due to workplace antidiscrimination initiatives will be amplified in states with higher discrimination levels.

H3b: An increase in the average quality of entrepreneurial foundings due to workplace antidiscrimination initiatives will be amplified in states with higher discrimination rates.

As a second test, we consider the effect of antidiscrimination initiatives on job reallocations, or the rate at which jobs are destroyed and created in established firms. Our argument posits that, in deciding between keeping their attachment to paid employment or pursuing entrepreneurship, individuals reveal a stronger positive bias toward the former than the latter, when the attractiveness of the current job

increases due to a more inclusive working environment. A further implication of our argument is that antidiscrimination initiatives in the workplace enhance the attractiveness of a current job relative not only to entrepreneurship but also to paid employment in another company. Given that any move, either to entrepreneurship or to a different employer, is associated with search and switching costs (e.g., Bidwell, 2011), workers are unlikely to switch to another employer when the attractiveness of current employment increases. When the tendency to switch employers decreases, following the introduction of antidiscrimination initiatives in the firm, fewer jobs will be created and destroyed, lowering job reallocation rates across established firms. As workers become less likely to leave current employer, fewer jobs will likely be destroyed within the current firm, since current employer will be less in need to hire replacement. Similarly, as workers become less likely to switch jobs, fewer jobs will likely be created within a new firm, since a new employer will be less in need to expand the firm and accommodate new talent. Overall, then, as workplace discrimination subsides, the total reallocation turnover of employees in established firms, as indicated by the rate at which new jobs are created and old jobs are destroyed within a firm, will decrease, which leads to the following hypothesis:

H4: Workplace antidiscrimination initiatives will decrease employee reallocation (turnover) in established businesses.

EMPIRICAL SETTING AND DATA

Our empirical analyses mirror the theoretical development. First, our main purpose is to assess the effect of anti-discrimination initiatives on entrepreneurial entry. Second, we aim to provide empirical evidence that supports the mechanisms behind the effect of interest that we suggested in our theoretical development.

Empirically, it is difficult to estimate how the adoption of initiatives that aim to reduce workplace discrimination affects the rate of organizational foundings. For instance, one could regress the rates of entrepreneurial entry on firm-level practices against workplace discrimination. Yet, such regression may be subject to a classic endogeneity problem. First, firm level characteristics might lead to spurious

correlation between antidiscrimination programs and entrepreneurial entry. For example, it could be that management quality—which is difficult to observe—drives both antidiscrimination practices and workers’ motivation to leave current employment for entrepreneurship. Moreover, a potential correlation between antidiscrimination programs and entrepreneurial entry could be subject to reverse causation. It could be that the lack of new ideas within the firm – typically correlated with entrepreneurial spawning and foundings – leads the firm to implement antidiscrimination practices so as to attract the “creative class” (Florida, 2002). To rule out these and other potential alternative explanations, it is necessary to leverage a research design that provides *exogenous* shifts in workplace antidiscrimination—such exogenous shifts would allow us to estimate the causal effect of workplace antidiscrimination on entrepreneurial rates. The source of exogenous variation we exploit in this paper is the staggered enactment of antidiscrimination laws in different moments in time across different U.S. states.

Specifically, we focus on a workplace antidiscrimination law, the Employment Non-Discrimination Act (ENDA), which protects workers from discrimination based on sexual orientation and gender identity. The District of Columbia was the first to pass such law in 1977, and by the end of 2009 twenty other states had followed suit (see table 1 for a list of the enactments). The enactment of these laws is suitable for our identification strategy for at least three reasons. First, there is substantial empirical evidence that discrimination on the basis of sexual orientation and gender identity has been an important challenge in the American workplace (e.g., Badgett et al., 2007). In fact, the U.S. still does not have any federal legislation that prohibits sexual orientation discrimination in the labor market. Yet, driven by individual actions and social movements pressure, a number of states have enacted laws that prohibit employment discrimination on the basis of sexual orientation. *De facto*, the protections provided in such acts have mirrored the earlier protections against workplace discrimination on the basis of race, gender, religion, national origin, and physical disability, and have allowed advocates to frame the sexual orientation protections as incremental additions to existing policies (Klawitter and Flatt, 1998).

Second, it has been largely accepted that ENDA’s enactment had a causal impact on reducing discrimination (Flatt and Klawitter, 1998; Ragins and Cornwell, 2001; Barron, 2009; Barron and Hebl,

2013). Scholars have argued that the laws had both instrumental and symbolic effects. The instrumental effect indicates that prejudiced employers discriminated less because such laws created an “expected cost” of a magnitude that equals the cost of law violation if caught (e.g., attorney’s fees, fines) times the probability of being caught (Landes, 1968). Along similar lines, others have suggested that 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 1984a; Skaggs 2001). The symbolic effect suggests that, even absent any possibility of tangible punishment, the legislation reduced a gay discrimination simply by designating it as illegal, criminal, or deviant (e.g., Tapp and Kohlberg, 1971; Zimring and Hawkins, 1971). For example, Helb et al., (2016) argue that the mere fact this discrimination is labeled as illegal (without the threat of enforcement) is sufficient to create a symbolic effect in changing workplace norms regarding the acceptability of prejudice and discrimination toward gays and lesbians. Consistent with this claim, the authors find that public awareness of sexual orientation antidiscrimination laws is greater in cities with such legislation relative to other cities. Similarly, other studies found that these antidiscrimination legislative mandates are linked to employee perceptions of lesser sexual orientation discrimination (Button, 2001; Griffith and Hebl, 2002; Ragins and Cornwell, 2001).

Finally, although the introduction of these laws does not allow for measuring discrimination directly, it nevertheless provides us with a unique opportunity to proxy for the *change* (i.e. decrease) in the level of discrimination in a given state after the enactment of the law. In the ideal experiment, states would “randomly” pass sexual orientation and gender workplace antidiscrimination laws. In reality, however, enacting these laws is not random—it may depend on, e.g., changes in economic conditions and political pressures. These, in turn, may affect workers’ inclination to leave paid employment in order to launch a new venture. Below (see the methodology subsection), we discuss the political economy of the “treatments” and describe how our difference-in-differences specification helps address this potential issue.

Dependent Variables

Entrepreneurial foundings. We obtained data on entrepreneurial foundings 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 approximately 4m establishments and 70m employees are included in the average year. The LBD includes complete accounting of very small firms and establishments, which are often excluded or sub-sampled 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. Entrepreneurial foundings is measured as the total number of new, stand-alone establishments in a given state and year. Data from LBD is available from 1977 to 2011.

Quality of entrepreneurial foundings. Evaluating – and thus measuring – the quality of entrepreneurial foundings (intended as the ability of new ventures to survive and grow) is challenging. Given this empirical challenge, we employ two different measures of the quality of entrepreneurial foundings, both of them based on solid streams of literature. First, we proxy for the quality of new ventures in a given state using the average amount of venture capital (VC) funding they received, calculated as the total amount of VC investments in a given-state year normalized by the number of new entrants in the same state-year. Ample empirical evidence shows that VC investments are an important milestone for a high-growth venture. Specifically, studies show that VC funding is associated with a strong “treatment” effect as well as a strong “selection” effect; venture capitalists spur higher growth of a new venture and only invest in *ex-ante* promising, potentially high-growth firms (see Da Rin, Hellmann, and Puri, (2011) for a review of related literature). Hence, if start-ups receive more funds from such specialized investors, their potential for growth and hence their quality is, on average, higher. The VC data (by location of investee company) was retrieved from Thomson Reuters’ VentureXpert.

The second measure of the quality of entrepreneurial foundings we consider is the propensity of young firms to file for patents, proxied for by the number of patent applications filed by *new* assignees in a given state-year normalized by the number of new entrants. New firms that file for patents are more

likely (a) to be endowed with superior technologies and (b) to be better able to capture the value that their technologies create. Several studies have highlighted the impact of patenting on the survival of SMEs. Cockburn and Wagner (2007), for example, test whether the patent portfolio of companies listed in Nasdaq influenced their survival after the burst of the dotcom bubble in the late nineties. Helmers and Rogers (2010) measure how the patent and trademark portfolios of the 162,000 LLC companies created in the United Kingdom in 2001 influenced their survival rate after 5 years. In both cases, less than half of companies did own patents, and these patent-owning companies have witnessed a significantly higher survival rate (of 34% in the first study, and 16% in the second one). Hence, we can argue that new entrants have a higher entrepreneurial quality, on average, when their propensity to patent is higher. Data on patents is retrieved from the NBER dataset.

In our theoretical development, we also provided some hypotheses on the mechanisms that underlie the expected effect of antidiscrimination laws on entrepreneurship. These hypotheses have alternative dependent variables.

Job reallocation in established businesses. Following the literature (Acs et al., 2009) for any state-year, we measure jobs reallocation as the sum of jobs destructed and created by established businesses. Prior studies have established a robust link between job reallocation rates and workers' flows across established firms, suggesting that workers move less across jobs when jobs are created and destroyed at lower rates (i.e., job reallocation rate decreases) (Davis and Haltiwanger, 1992). Consistent with this and other research, we measure the job destruction rate and the job creation rate based on the net employment change at the employing unit. In adopting these measures as a proxy for worker flows, we follow a number of studies (Gielen and Ours, 2006; Davis and Haltiwanger, 1999). These data are taken from the LBD database.

Treatment Dummy

As we mentioned, our treatment measures changes in the level of workplace discrimination in a given state. Hence, we exploit the staggered enactment of workplace antidiscrimination laws that promote the protection of gay and gender identity rights. Following the relevant related literature (e.g., Kerr and

Nanda, 2009; Flamer and Kacperczyk, 2015), our independent variable takes the value of 1 starting from the year *after* a given state has enacted an anti-discrimination law and zero before that for the treated states, and is always equal to zero for the control states, or those that did not implement the antidiscrimination law. A list of all the states that enacted the ENDA is provided in Table 1.

Moderating variable

State discrimination. To separate states with higher and lower levels of discrimination, we used a recent ranking that measures U.S. states' discrimination. The ranking is based on the discrimination complaints filed to the Office of Fair Housing and Equal Opportunity for 2006-2010 divided by 100,000 of residents. Discrimination by real estate agents and landlords directed against minority home seekers, including LGBT occurred and continues to occur throughout America's metropolitan areas. Following other studies, we use this measure to proxy for general discrimination levels by state (Ross and Galster, 2005). The ranking is based on percentiles: a state in the 90th percentile (with the least amount of complaints filed) was given 20 points; a state in the 80th percentile was given 18 points, etc.² For the purposes of our analyses, states in the bottom half of this ranking are considered high-discrimination states, whereas states in the top half of the ranking are considered low-discrimination states.

Control Variables

Because our treatment is likely exogenous with respect to the outcome, any potential confounders are likely to be balanced between the treatment and the control states. However, to the extent that this assumption might not hold in our setting, we mitigate this concern by including in our models additional covariates. First, in all specifications we include state- and year-fixed effects, to control for, respectively, constant differences across U.S. states and for aggregate changes over time due to business cycles, national policies, etc. In most of the specifications we also control for the number of active firms in the state (source: LBD database), and for GDP per capita (source: Census Bureau): both of these variables are related to the opportunities and need of individuals to transition to self-employment. Table 1 reports summary statistics for our main variables, and table A1 in the appendix reports bivariate correlations. In

² See <http://www.thedailybeast.com/ranking-the-most-tolerant-and-least-tolerant-states>. Accessed August 2017.

additional analyses, reported in table 3 in the appendix, we re-estimate models with additional state-level controls and find similar results.

Insert Tables 1 & 2 about here

METHODOLOGY

To examine the impact of workplace antidiscrimination on entrepreneurship following the passage of the antidiscrimination laws, we use a difference-in-differences (diff-in-diffs) methodology based on the 21 treatments listed in Table 1. The unit of analysis of our empirical analyses, therefore, is 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 form:

$$Y_{st} = f(\varphi_s + \tau_t + \beta_{AD}AD_{st} + \beta_{CV}CV_{st-1})$$

where AD is the "treatment dummy" - i.e., a dummy variable that equals one if the company is located in a state that has enacted the antidiscrimination law by year t . CV a vector of control variables, and φ_s and τ_t are state and year fixed effects. The functional form of $f()$ we implement depends on the nature of the dependent variable. Errors are always clustered at the state level, to address potential serial correlation concerns as highlighted by Bertrand et al. (2004). The coefficient of interest is β , which measures the effect of antidiscrimination law on our dependent variable. For instance, hypothesis 1 predicts that β should be negative and significant when Y is the number of entrepreneurial foundings.

We can illustrate how diff-in-diffs work with a simple example. Suppose we want to measure the effect of Maryland's 2001 enactment of the antidiscrimination law on entrepreneurial rates. We would compute the difference in the rates of entrepreneurship post-2001 versus pre-2001 for companies located in Maryland (a "treated state"). Yet, other events may have happened around 2001, potentially influencing changes in the entrepreneurial rates. For example, there may have been an economy-wide

boom that translates into more opportunities to create new ventures. To account for such contemporaneous effects, we use a control group. For example, we could consider as a “control state” Pennsylvania (knowing Pennsylvania did not introduce an ENDA) and compute the corresponding difference in the entrepreneurial rates post-2001 versus pre-2001. Computing the *difference between these two differences* provides an estimate of the effect of Maryland’s 2001 enactment of the workplace antidiscrimination law on the entrepreneurial rates, controlling for contemporaneous changes in the entrepreneurial rates that are due to changes in broad economic conditions. The difference between this example and our regression specification is that the latter accounts for the fact that the implementation of the antidiscrimination law is staggered over time across states. It follows that the composition of both the treatment and control groups changes over time as more states are progressively “treated.”

MAIN RESULTS

Rate of entrepreneurial Foundings

We begin by evaluating the effect of antidiscrimination laws on the number of new entrants. Since *Entrepreneurial foundings* is a count variable, we use a Poisson (fixed effects) regression. Table 3 reports the results of this basic estimation. In these analyses (and in most of the following ones), *control* states, i.e. states that never change their status with respect to anti-discrimination policies, are observed over the complete time frame of the data, i.e. from 1977 to 2011; by contrast, *treated* states – that is, states that enact new antidiscrimination policies – are observed for two different time-windows around the first year after the enactment: 5 years in model (1) and 10 years in model (2) of table 3. These specifications ensure that treated states are accounted for a limited and homogenous number of years. Given the staggered introduction of laws and the relatively long-time span of our dataset, we wish to minimize the possibility that the treatment dummies absorb other events or noise due to a substantial number of years following treatment. Overall then, the treated states have equal “weight” in our estimation.

Both specifications reported in table 3 show that the enactment of ENDAs has a negative and statistically significant effect on entrepreneurial foundings. These results thus lend support to hypothesis

1, and suggest that a decrease in discrimination in the workplace affects negatively new-firm entry: considering specification (1), introducing an antidiscrimination law diminishes entrepreneurial foundings by about 8%.

Insert Table 3 about here

Quality of entrepreneurial foundings

Hypothesis 2 suggested that antidiscrimination laws increase the average *quality* of new entrants. To test this hypothesis, we use a linear fixed-effects estimation as both of our proxies for the average quality of foundings are continuous variables. The estimates for entrepreneurial quality are reported in table 4, with VC funding as a measure of quality in Models 1-2, and patents as a measure of quality in Models 3-4. For both measures, we consider the time window of ten years before and after the treatment as well as five years before and after the treatment. Consistent with our predictions, we find that while the enactment of ENDAs reduces the overall new-firm entry, it nevertheless promotes the entry of high-quality startups. The parameter estimate of the treatment dummy is positive and statistically significant across all specifications with the exception of specification (4), where it is borderline significant at conventional levels. These results thus broadly support hypothesis 2, and suggest that the enactment of antidiscrimination laws increases the opportunity cost of entering self-employment by improving established workplaces. As a consequence, those who turn to entrepreneurship do so only when they perceive high-value, i.e. potentially high-growth entrepreneurial opportunities. In sum, we have found support for the key predictions of our study.

Insert Table 4 about here

Dynamic Effects

Our diff-in-diffs analyses rely on the so-called “parallel paths” assumption. The parallel path

assumption states that the average change in outcome for the treated in the absence of treatment should equal the average change in outcome for the non-treated. To verify that there were no divergent trends across states *before* the change in judicial selection – and also to assess how the effect of the ENDA occurs over time – we construct a *dynamic* difference-in-differences model, employing a set of dummies that measure the distance in years from the enactment of the law, before and after its introduction. Specifically, we replace the treatment dummy with a set of dummy variables up to five years before the treatment and up to five years following the treatment. The coefficients reported in models (1), (2) and (3) of table 5 indicate that there is no difference across states in the patterns of entry and quality of foundings *prior to* ENDA's enactment, thus suggesting that our results are not driven by diverging pre-treatment trends. By contrast, the post-change coefficients that relate to the number of entrants (specification (1)) are negative and statistically significant at conventional levels. At the same time, these results highlight that the effect of anti-discrimination laws is stronger in the first four years after their introduction. The coefficients that relate to the quality of foundings are positive and significant (see specifications (2) and (3), table 5), although we observe that the effect of the enactment of laws on the quality proxy based on venture capital funding presents a longer time lag. This is likely due to the fact that while in our dependent variable we measure total VC funds and entry in the same year, VC funding actually occurs later in the startup life-cycle.

Insert Table 5 about here

A graphical representation of these analyses is reported in figures 1, 2, and 3, that display dynamically the confidence interval of the parameter estimates of law enactment. Again, these figures show that there was no significant difference in the patterns of entrepreneurial activity between treated and control states before the actual enactments of the laws, reinforcing our confidence in the diff-in-diffs results. Importantly, the treatment effect equals zero at the time the law is implemented in each state. The

rates of entrepreneurial entry start declining one year following the treatment. Finally, the negative effect persists up to four years following the treatment and then it subsides around year five.

Insert Figures 1, 2 and 3 about here

ADDITIONAL RESULTS: STATE HETEROGENEITY AND EFFECT ON EMPLOYEE TURNOVER IN ESTABLISHED BUSINESSES

Having provided empirical evidence that shows that the enactment of antidiscrimination laws leads to lower rates of entrepreneurship (although of higher quality), we proceed to provide additional empirical evidence with the objective to assess the mechanisms. This evidence would further add confidence to our conclusions regarding the causal effect of antidiscrimination laws on entrepreneurship.

In particular, we argued that, if reduction of discrimination is the key mechanism to explain lower rates of entry, then our results on the effect of the enactment of ENDAs should be stronger in states most subject to discrimination. By the same token, we would expect the effect of ENDAs on the quality of new entrants to be stronger in states most prone to discrimination. Hence, we re-estimated the baseline specification used to test hypotheses 1 and 2, but now splitting the sample between high-discrimination states and low-discrimination states. Estimates reported in table 6 show that the magnitude of the coefficient is always higher in high-discrimination states; tests for the statistical significance of the difference confirm that the parameter estimates of antidiscrimination laws is higher in estimations in high-discrimination states that relate to entrepreneurial entry and the quality of start-up proxied with patents ($p=0.09$ and $p=0.01$, respectively), whereas it is not so for the quality measures using VC funding. Overall, however, these results lend some support to hypotheses 3a and 3b.

Our arguments also imply that the introduction of antidiscrimination laws reduces jobs' reallocation as the attractiveness of current paid employment increases, both for those subject to discrimination as well as for other constituencies. The results of the regressions reported in table 7 confirm this to be the case. The coefficient of antidiscrimination laws is negative and significant,

indicating they have a negative effect on total job reallocations; this is true when we consider a 20- as well as a 10-year window around the enactment year (see columns (1) and (2)). Further analyses show that the total effect results from both lower creation as well as job destruction (columns (3) to (6)). These results, therefore, support hypothesis 4 and more broadly our mechanisms.

Insert Tables 6 & 7 about here

Validity of the Identification Strategy

Our identification strategy needs to satisfy two requirements to be valid, i.e. the relevance condition and the exclusion restriction. First, the treatment (i.e., the enactment of the antidiscrimination laws) needs to trigger *relevant* changes in firm-level diversity policies. Second, the treatment needs to be *exogenous* with respect to entrepreneurial rates. In the following, we discuss both requirements.

Relevance condition

To satisfy the relevance condition, the implementation of the antidiscrimination law needs to bring about relevant changes in the firm-level practices toward homosexual minorities and diversity, in general. There are strong reasons to believe that ENDA's enactment fulfills the relevance criterion. As mentioned before, existing research has documented decreases in perceived discrimination among gay and lesbian employees in U.S. states with sexual orientation antidiscrimination legislation compared with areas without such legal protection (Ragins and Cornwell, 2001). This finding is further bolstered by strong complementary evidence at the organizational level: gay and lesbian employees also perceive less discrimination when organizational sexual orientation nondiscrimination policies are in place relative to when they are not (Button, 2001; Griffith and Hebl, 2002; Ragins and Cornwell, 2001). More generally, there is by now much evidence that the state-level enactment of antidiscrimination laws is typically followed by a firm-level implementation of antidiscrimination practices, which aim to reduce bias toward disadvantaged groups and promote openness to diversity, more generally. For example, scholars have

found that laws enforcement resulted in firms implementing numerous antidiscrimination practices, including formal hiring and promotion procedures, affirmative action plans, and employment and promotion tests to create objective selection criteria (Dobbin, Sutton, Meyer and Scott, 1993). Such initiatives have further been linked to remarkable declines in gender and racial segregation (Jacobs 1989a; King 1992; Tomaskovic-Devey et al. 2006; Kalev and Dobbin forthcoming; Leonard 1989; 1990; Skaggs 2001), and improvements in organizational diversity (Dobbin and Kelley, 2007).

Despite the strong evidence, we nevertheless investigate the relationship between the state-level enactment of antidiscrimination laws and firm-level policies that these laws should imply. To the extent that antidiscrimination laws impact firm practices, we should observe that firms are more likely to protect sexual minorities and more likely to promote diversity, following the implementation of the law. We re-estimate the baseline specification but now measure the outcome (i.e., practices) at the firm level. We use two measures to proxy for firm-level diversity policies using the KLD data. KLD is an independent social choice investment advisory firm that compiles ratings on the extent to which companies address the needs of their stakeholders. For each stakeholder group, including communities, environment, customers, diversity, and employees, strengths and concerns are measured to evaluate positive and negative aspects of corporate actions toward stakeholders. These ratings are compiled from multiple data sources, including annual questionnaires sent to companies' investor relations offices, firms' financial statements, annual and quarterly reports, general press releases, government surveys, and academic publications (see KLD, 2010). KLD ratings are widely used in past studies (e.g., Berman et al., 1999; Deckop, Merriman, and Gupta, 2006; Hillman and Keim, 2001; Waddock and Graves, 1997; Kacperczyk, 2009). Our theory predicts that, following the implementation of workplace antidiscrimination laws (i.e., laws protection gays and gender identity), firms will exhibit behaviors that are more favorable toward a diverse workplace. Hence, we first use the KLD score regarding a firm's pro-gay and pro-lesbian policies in a given year and headquartered in a given state. A higher score indicates that the company was evaluated by the KLD as more favorable towards homosexual employees. To the extent that this measure might not capture all diversity-promoting practices implemented by the firm in response to antidiscrimination laws,

we use an alternative measure that involves a firm's ratings with respect to all the diversity-promoting initiatives. Again, higher values of this measure indicate that companies receive higher ratings for their employee diversity initiatives.

To explain changes in diversity policies, we estimate linear models with firm size (measured as thousands of employees, source COMPUSTAT), leverage (long-term debt over total financing, source: COMPUSTAT), and capex (thousands of US\$, source: COMPUSTAT), as control variables as well as state-fixed effects, firm-fixed effects, and year fixed effects to alleviate concerns of unobserved heterogeneity.

Consistent with our theory, table 8 shows that firms that are headquartered in states that introduce ENDAs are more likely to introduce pro-gay and lesbian policies as well as to increase their Diversity score, as indicated by the KLD index.

Insert Table 8 about here

Exclusion restriction

Our identification strategy also assumes that the enactment of ENDAs is exogenous with respect to entrepreneurial activity in the state. In the following, we discuss potential identification concerns and explain how our econometric analyses help address them.

Reverse causality. A potential criticism is that firms or other constituencies may lobby for the passage of antidiscrimination laws in the workplace, and hence the treatment would not be exogenous to our treatment. Nevertheless, this concern is mitigated for the three following reasons. First, accounts of the political economy of these reforms suggest that their passage is exogenous to product markets, innovation and entrepreneurial activities, and depends instead on other variables unrelated to the economy (e.g., Haider-Markel and Meier, 2003; Gao and Zhang, 2016). Second, to rule out potential reverse causality concerns, we examine the dynamics of the treatment effect. If reverse causation explains our

results, then we would expect that the enactment of the antidiscrimination laws has a negative and significant “effect” already before the rejection occurs. However, we find no evidence for such pre-existing trends. Changes in entrepreneurial activity occur only *after* (not before or contemporaneous with) the enactment of the antidiscrimination law. Finally, we also verified empirically whether states’ economic and political conditions might have influenced the enactment of ENDAs. Table A2 in the appendix reports the results of linear probability models where the dependent variable is the anti-discrimination law dummy and the explanatory variables are the number of entrants and total number of firms in a given state-year, our proxies for the quality of new entrants, and GDP per capita. The estimated coefficients of the number entrants as well as of the quality of foundings are far from being statistically significant. These results thus suggest that entrepreneurial activities bear no significant influence on the introduction of the laws, thus reinforcing our confidence on the exogeneity of the changes in the legal protection of LGBT groups on the workplace – and, as a consequence, on the causal effect of anti-discrimination on entrepreneurship.

Unobserved differences between treated and control states. Another potential concern is that treated and control states may differ along unobservable characteristics, and that these differences may correlate with both state-level rates of entrepreneurial activity and states’ enactment of the antidiscrimination laws. Nevertheless, this concern is unlikely to explain our results, for several reasons. First, as discussed above, we find no evidence of pre-existing trends. This suggests that treated and control firms are on similar trends prior to the treatment. Second, to address the potential concern that omitted local trends may confound our results, we re-estimate our baseline regression including a larger set of state-level controls. More specifically, an important concern might be that the passage of antidiscrimination legislation and entrepreneurial foundings is correlated with the political trends in the state. For example, if Democratic states are more likely to promote antidiscrimination legislation and less likely to promote entrepreneurship, then our correlation might simply be driven by the state-level politics. To mitigate this concern, we re-estimate the baseline specifications with controls for state political trends. Specifically, we insert two dummy variables that take the value of “1” if the state had a Republican

majority, respectively, at the latest presidential elections and at the latest Governor elections, and a value of 0 if the state had a Democrat majority. We also control for taxation at the state level, an important driver of entrepreneurial activities, calculated as total revenues by state taxes divided by state GDP (source: Census Bureau).

There are no observable changes in the parameters of interest (see table A3 in the appendix). Third, given the staggered nature of the treatments, the eventually treated firms are first in the control group, and only later in the treatment group (i.e., once they have been treated). This feature allows us to re-estimate our difference-in-differences specification using only the eventually treated firms—which means that the control group consists only of firms that are eventually treated (for a similar test see, e.g., Bertrand and Mullainathan, 2003; Flammer and Kacperczyk, 2016). We also do exclude firms located in California because, compared to the other treated states, California might be an outlier in terms of population and GDP per capita. Restricting our sample to treated states only, which all chose to enactment the ENDA, and excluding California allows for reducing heterogeneity across states. Results in table A4 in the Appendix confirm unobserved heterogeneity of this kind not to be an issue, as the treatment effect on the number of startups (column 1) and their average quality – at least when measured considering their innovativeness (column 2) – remains largely consistent with our theory. Finally, our findings on the cross-sectional heterogeneity further mitigate concerns that our results may be driven by unobservables. Indeed, for our results to be spurious, the unobservable would need to be both correlated with the treatment *and* systematically related to quality of entrepreneurial firms, states with higher level of discrimination, and a decrease in job reallocation in incumbent firms (see the results section), which seems highly unlikely.

ALTERNATIVE EXPLANATIONS

Other Shocks. Although our main results are robust to a number of specifications and tests, alternative explanations might still exist. One possibility is that other events contemporaneous with the enactment of the antidiscrimination laws are responsible for the change in patterns of entrepreneurial entry. However, the evidence for the mechanisms, as discussed in the previous section, mitigates this

concern: if our results reflect other legal changes along this time, then we would not observe all other consequences that our theory implies, including those stated in hypotheses 3a/3b and 4 and the results at the firm level.

Still, to increase the confidence in our findings, we collected data on other legal reforms implemented in the U.S within the timeframe of our empirical analysis. We focus on the key legal changes that have either been shown to have an effect on entrepreneurship or one would expect them to influence entrepreneurial rates, in theory. These include changes in non-compete enforcement (Garmaise, 2011), changes in trade-secret protection via the Uniform Trade Secret Act (Png, 2012), and bank deregulation laws (Kerr and Nanda, 2009). Some of these reforms have taken place in our treated states or neighboring states. Hence, there might be a concern that these reforms have affected entrepreneurship, leading to spurious correlations in our estimates. To mitigate this challenge, we re-estimated the baseline specifications with additional controls for years in which these additional changes occurred. As the results reported in table 9 show, adding to our basic specification these additional covariates does not change the significance or the economic magnitude of our main estimates, both with respect to the number as well as the quality of foundings, neither statistically nor economically.

Insert Table 9 about here

Entrepreneurship Costs. An additional alternative explanation could be that antidiscrimination laws increase the costs for firms and for potential entrepreneurs by forcing them to implement costly procedures to mitigate discrimination. Allegedly, this would be the case if anti-discrimination 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”.³ For instance, in response to ENDA’s passage in the Senate, Speaker Boehner stated that he “believes this legislation will increase frivolous litigation and cost

³ See: <http://executivelp.com/laws-regulations/21st-century-effects-of-discrimination-do-we-still-need-anti-discrimination-laws/>
Accessed August 2017.

American jobs, especially small business jobs.” This implies that any cause of action might increase the legal costs of businesses by increasing the number of both meritorious and frivolous claims.⁴

To address this concern, we investigate the effect of our treatment on exit rates. If employees are less likely to become entrepreneurs because the expected costs of launching a new venture increase following the antidiscrimination laws, then we should not only observe a decrease in new-firm entry, but also an increase in exit. The increased costs of litigation should force small existing businesses out. We thus also collected data on firm exits from the LBD dataset and verified the effect of ENDAs on exit. The results reported in table 10 show that antidiscrimination laws do not have any effect on exit, even if we disaggregate data depending on firm size. This result is therefore not consistent with the idea of an increased cost of business due to antidiscrimination laws.

Insert Table 10 about here

DISCUSSION

Entrepreneurship scholars have long identified discrimination as an important obstacle to entrepreneurial entry (Thebaud and Sharkey, 2016; Younkin and Kuppaswamy, 2017; Fairlie and Robb, 2007). A consensus has therefore emerged that boosting entrepreneurial activity hinges on reducing bias through the implementation of antidiscrimination practices. But whereas scholars have made general claims about the benefits of antidiscrimination initiatives for entrepreneurship, they only have examined the demand-side discrimination, or instances when prospective entrepreneurs face discrimination by resource holders (i.e., investors, banks, customers, or employees). By contrast, in this study, we argue the well-established claims about the benefits of antidiscrimination initiatives for entrepreneurial entry neglect the role of the supply-side factors in driving the decision to become an entrepreneur. Although discrimination may also originate on the supply side, influencing the availability of individuals willing to leave paid employment

⁴ See <https://onlabor.org/the-employment-non-discrimination-act-an-explainer/?print=print>
Accessed August 2017.

in pursuit of new ventures (Thornton, 1999), and hence impacting the overall rates of entrepreneurship, few studies have accounted for these factors.

In this study, we draw on the career perspective on entrepreneurship (e.g., Audia and Rider, 2006; Sorensen and Sharkey, 2014; Kacperczyk and Marx, 2016) and theorize that reducing discrimination has a stifling effect on entrepreneurship, when such initiatives pertain to the supply side, that is, they are implement in the current workplace. Lending support to this theory, our findings indicate that antidiscrimination laws, which prevent workplace discrimination based on sexual orientation, have a negative effect on entrepreneurial entry. This result is consistent with our argument that antidiscrimination initiatives, when applied to the supply side, enhance the attractiveness of paid employment relative to entrepreneurship, increasing the opportunity cost of leaving to launch a new venture. As further evidence, our study shows that, as the threshold for giving up paid employment for entrepreneurship increases, the average growth potential of a new venture increases. That is, whereas individuals are overall less likely to pursue a new venture following the enactment of antidiscrimination acts, at the same time, those who transition to entrepreneurship are more likely to do so when entrepreneurial opportunities are more attractive and associated with high-growth potential.

Our analyses probed deeper into other mechanisms we theorized. First, consistent with the notion that reducing workplace discrimination enhances the attractiveness of paid employment relative to entrepreneurship, we find that the negative treatment effect of antidiscrimination laws is amplified in states with higher discrimination levels, in general. Moreover, lending support to our claims that individuals are more likely to substitute entrepreneurship with paid employment when the latter becomes more attractive (Hellman, 2007; Kacperczyk, 2012; Kacperczyk and Marx, 2015), we find that the treatment effect leads to a decrease in jobs' reallocation in established firms. Finally, our findings confirm the claim that pro-gay and pro-diversity policies are more prevalent in the workplace, following the enactment of the antidiscrimination laws, consistent with sociological literature which documented a robust link between antidiscrimination legislation and firm-level programs to reduce job segregation

(Jacobs 1989a; King 1992; Tomaskovic-Devey et al. 2006; Kalev and Dobbin forthcoming; Leonard 1989; 1990; Skaggs 2001).

Yet more research on entrepreneurship and antidiscrimination is needed. We have established a negative causal relationship between laws that reduce discrimination in the workplace and entrepreneurial entry. At the same time, we provide evidence that this relationship reverts when considering high-growth entrepreneurship (i.e., ventures with patents and VC funding). But the treatment effect of workplace antidiscrimination laws on entrepreneurship may also depend on other factors, such as career stage, human capital, demographics, or risk-taking propensity. For example, individuals with high risk-taking propensity or those who are younger may be less sensitive to changes in the implementation of antidiscrimination policies. Although our analyses take the first step to shed light on the relationship between antidiscrimination initiatives and transition into entrepreneurship, future research could profitably address how individual and organizational characteristics might modify the relationship between antidiscrimination laws and entrepreneurship, in a more nuanced way. Future studies might also examine the impact of such antidiscrimination initiatives of new-venture performance or founder's subsequent commitment. In this study, we focused on the decision to transition to entrepreneurship. Yet, future research may profitably address the impact of such legislation on new-venture performance subsequently. Specifically, more attention is required to understand how the motives to enter entrepreneurship might affect founder's commitment to grow new venture subsequently. Finally, future studies may want to investigate a net effect of antidiscrimination initiatives on entrepreneurship. On the one hand, reducing societal biases might facilitate entrepreneurship by opening access to resources and opportunities. On the other hand, reducing discrimination might hinder entrepreneurship by increasing the cost of leaving paid employment. Therefore, an intriguing path of inquiry might be examining the net effect of antidiscrimination practices on entry into entrepreneurship.

Overall, this study revisits the well-established claim that reducing discrimination promotes entrepreneurship. The majority of studies have made such general claims without taking the supply side factors—which are equally central to driving entrepreneurial entry (Thornton, 1999)—into consideration.

But because discrimination may also originate on the supply side, when individuals face bias in the workplace, understanding the relationship between antidiscrimination initiatives and entrepreneurial entry requires taking the supply side into consideration. We find that antidiscrimination laws have a negative impact on entrepreneurship, but that, conditional on entry into entrepreneurship, there is a positive treatment of effect of such laws on new-venture growth potential. Such research may have important policy implications. Conventional wisdom suggests that policies to promote new venture formation should focus on reducing biases that prevent minorities from accessing entrepreneurial opportunities and resources. However, these policies are less attentive to the fact that reducing bias may also enhance the attractiveness of paid employment relative to entrepreneurship. Hence, the impact of antidiscrimination policies on entrepreneurship may depend on whether such discrimination originates on the demand or the supply side. More generally, encouraging venturing activity and therefore economic growth may require reassessing existing policies to focus on the specific actors involved in discrimination. Together, our insights indicate that, although some antidiscrimination programs may boost entrepreneurial rates, others may have a more nuanced impact on entrepreneurship by influencing whether or not individuals are willing to give up paid employment in the first place.

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Figure 1: Dynamic effect of antidiscrimination laws on the rate of entrepreneurial foundings

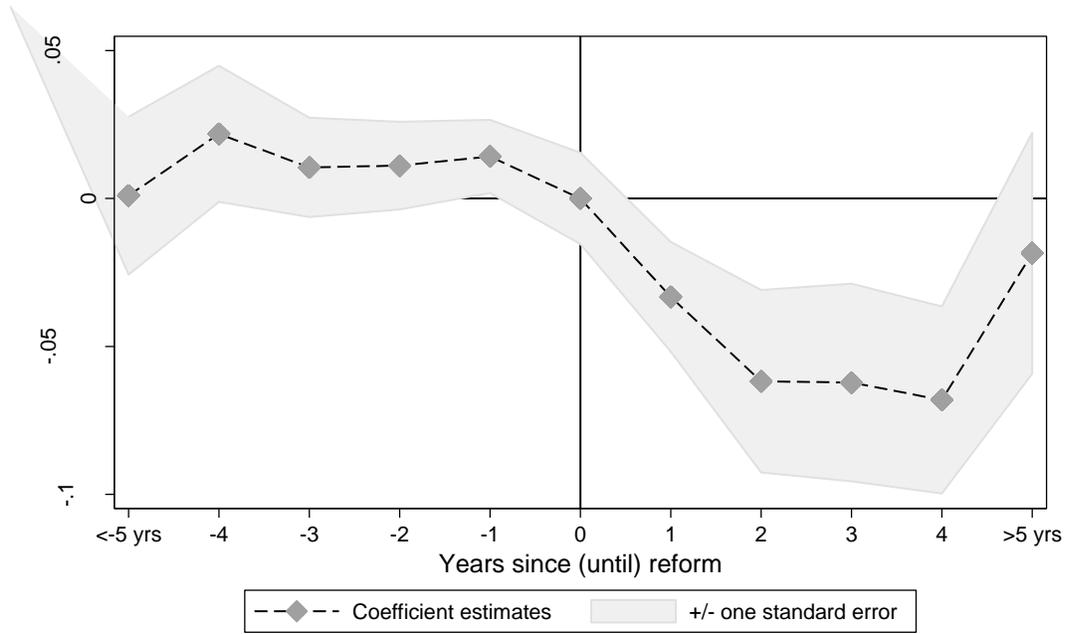


Figure 2: Dynamic effect of antidiscrimination laws on the average quality of entrepreneurial foundings (Innovation measure)

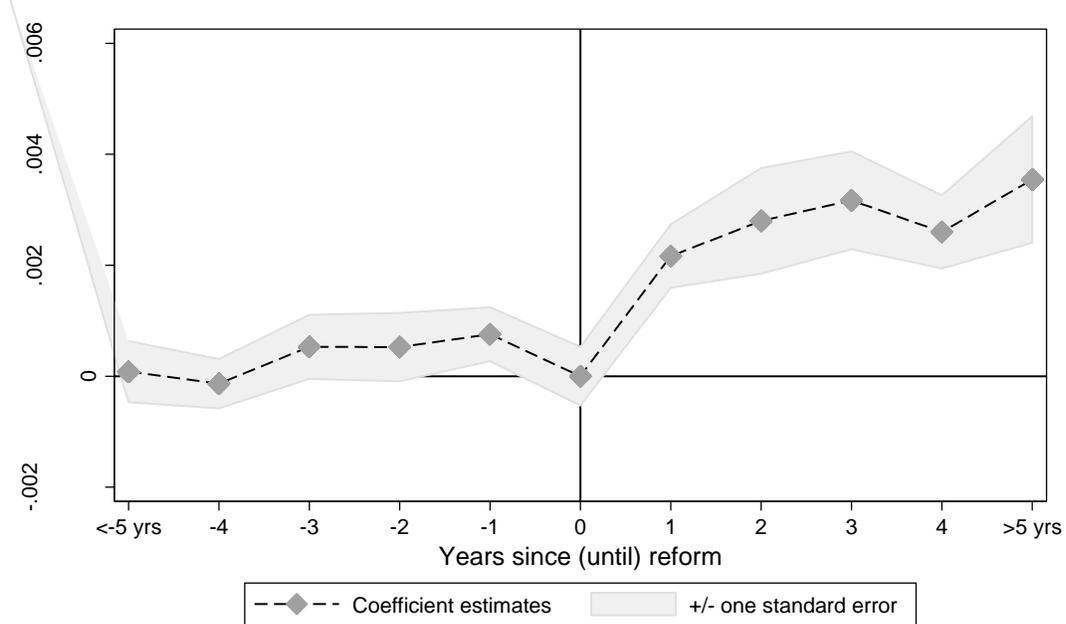


Figure 3: Dynamic effect of antidiscrimination laws on the average quality of entrepreneurial foundings (VC measure)

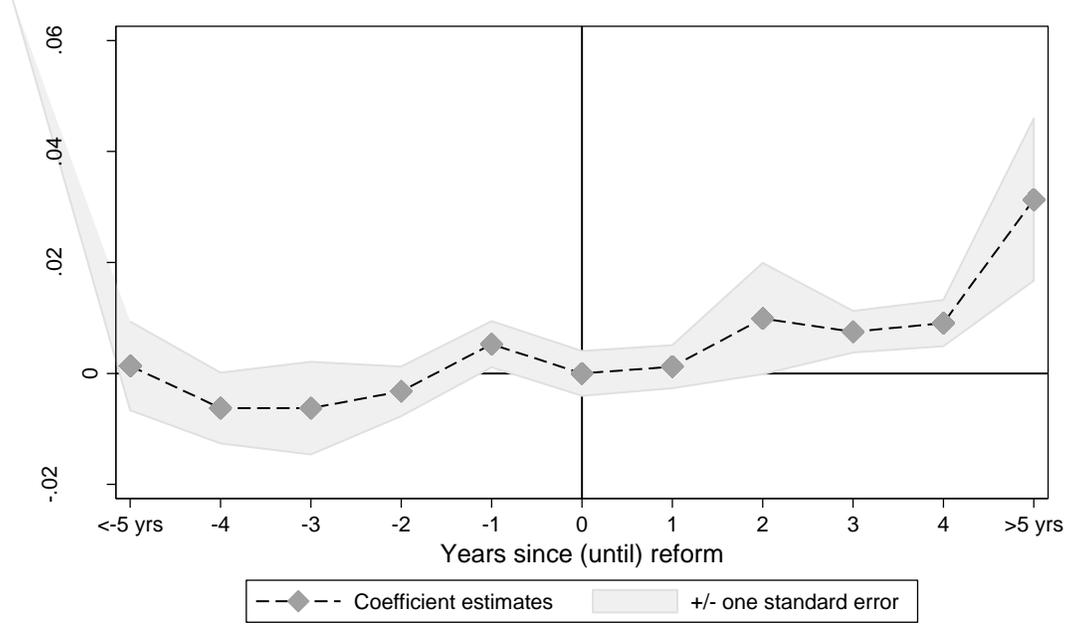


Table 1: Year of antidiscrimination law enactment – from 1977 to 2009

State	Year
District of Columbia	1977
Wisconsin	1982
Massachusetts	1989
California ^a	1991
Connecticut	1991
Hawaii	1991
Vermont	1991
New Jersey	1992
Minnesota	1993
Rhode Island	1995
New Hampshire	1998
Nevada	1999
Maryland	2001
New Mexico	2003
New York	2003
Maine	2005
Illinois	2006
Washington	2006
Colorado	2007
Iowa	2007
Delaware	2009

Source: Gao and Zhang (2016); Hunt (2012); Friedman (1992).

^aIn California the law was enacted in 1992; however, it was preceded in 1991 by a court decision (“Soroka vs. Dayton Hudson”) which interpreted labor code provisions as prohibiting all employers from discriminating against an employee on the basis of his or her sexual orientation (Friedman, 1992).

Table 2: Summary statistics

	Observations	Mean	Standard deviation	Min	Max
Entrepreneurial foundings	1399	9530.972	10617.02	950	70608
Average quality of foundings (innovation)	1113	.006692	.0044796	0	.0302532
Average quality of foundings (VC)	1335	.0167139	.0477684	0	.9960603
Antidiscrimination law	1399	.1315225	.3380918	0	1
GDP per capita	1399	26.96609	12.1094	6.487086	72.45419
Firms	1399	90.1859	92.46418	6.762	579.958
Job creation incumbents	1399	184387.1	207968	9251	1739920
Job destruction incumbents	1399	196927.9	221066.4	10591	1544524

Table 3: Effect of antidiscrimination laws on the rate of entrepreneurial foundings (Poisson specification)

VARIABLES	(1) Entrepreneurial foundings +/- 10 years	(2) Entrepreneurial foundings +/5 years
Antidiscrimination law	-0.080*** (0.031)	-0.072*** (0.027)
GDP per capita	0.018*** (0.003)	0.018*** (0.003)
Firms	0.002** (0.001)	0.002** (0.001)
Observations	1,399	1,236
State FEs	Yes	Yes
Year FEs	Yes	Yes
Log-likelihood	-60484.167	-54156.186

Note: Robust standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 4: Effect of antidiscrimination laws on average quality of foundings – (OLS specifications)

VARIABLES	(1) Average quality of foundings (Innovation) +/- 10 years	(2) Average quality of foundings (Innovation) +/- 5 years	(3) Average quality of foundings (VC) +/- 10 years	(4) Average quality of foundings (VC) +/- 5 years
Antidiscrimination law	0.003*** (0.001)	0.002*** (0.001)	0.023** (0.009)	0.008 (0.005)
GDP per capita	0.000*** (0.000)	0.000*** (0.000)	0.002** (0.001)	0.001*** (0.000)
Firms	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Observations	1,113	970	1,335	1,172
State FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
R-squared	0.629	0.622	0.335	0.433

Note: Robust standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 5: Effect of antidiscrimination laws on entrepreneurial foundings and average quality of foundings: dynamics (Poisson and OLS specifications)

VARIABLES	(1) Entrepreneurial foundings	(2) Average quality of foundings (Innovation)	(3) Average quality of foundings (VC)
t-5	0.032 (0.027)	0.000 (0.001)	0.003 (0.008)
t-4	0.037 (0.024)	-0.000 (0.000)	-0.005 (0.006)
t-3	0.021 (0.017)	0.001 (0.001)	-0.006 (0.008)
t-2	0.017 (0.015)	0.001 (0.001)	-0.003 (0.004)
t-1	0.014 (0.013)	0.001 (0.000)	0.005 (0.004)
t+1	-0.032* (0.017)	0.002*** (0.001)	0.001 (0.004)
t+2	-0.063** (0.026)	0.003*** (0.001)	0.009 (0.010)
t+3	-0.066** (0.028)	0.003*** (0.001)	0.007* (0.004)
t+4	-0.079*** (0.027)	0.003*** (0.001)	0.009** (0.004)
t+5	-0.046 (0.029)	0.003*** (0.001)	0.030** (0.015)
GDP per capita	0.019*** (0.003)	0.000* (0.000)	0.001*** (0.000)
Firms	0.002** (0.001)	0.000 (0.000)	0.000 (0.000)
Observations	1,355	1,076	1,291
State FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
Log-likelihood	-58572.203		
R-squared		0.884	0.574

Note: Robust standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 6: Effect of antidiscrimination laws depending on state discrimination

VARIABLES	(1) Entrepreneurial foundings Low-dis. States +/- 10 years	(2) Entrepreneurial foundings High-dis. States +/- 10 years	(3) Average quality of foundings (Innovation) Low-dis. States +/- 10 years	(4) Average quality of foundings (Innovation) High-dis. States +/- 10 years	(5) Average quality of foundings (VC) Low-dis. States +/- 10 years	(6) Average quality of foundings (VC) High-dis. States +/- 10 years
Antidiscrimination law	-0.016 (0.027)	-0.094*** (0.032)	0.001** (0.001)	0.004*** (0.001)	0.017* (0.008)	0.026* (0.014)
GDP per capita	0.010*** (0.003)	0.016*** (0.004)	0.000 (0.000)	0.000** (0.000)	0.001*** (0.000)	0.003 (0.002)
Firms	0.006*** (0.001)	0.002** (0.001)	0.000 (0.000)	0.000 (0.000)	0.000** (0.000)	0.000 (0.000)
Observations	-0.016 724	-0.094*** 664				
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Log-likelihood	-20805.578	-33638.101				
R-squared			0.604	0.676	0.516	0.294

Note: Robust standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.1
The discrimination measure is not available for District of Columbia.

Table 7: Effect of antidiscrimination laws on employee turnover in established businesses (Poisson specification)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Job reallocation +/- 10 years	Job reallocation +/-5 years	Job destruction +/-10 years	Job destruction +/-5 years	Job creation +/-10 years	Job creation +/-5 years
Antidiscrimination law	-0.113*** (0.026)	-0.074*** (0.020)	-0.116*** (0.026)	-0.063*** (0.015)	-0.111*** (0.029)	-0.084*** (0.028)
GDP per capita	0.009*** (0.003)	0.012*** (0.003)	0.009** (0.004)	0.011*** (0.004)	0.009*** (0.003)	0.013*** (0.003)
Firms	0.003*** (0.001)	0.003*** (0.000)	0.004*** (0.001)	0.005*** (0.001)	0.001 (0.001)	0.001* (0.001)
Employees	-0.000* (0.000)	-0.000* (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	0.000 (0.000)	0.000 (0.000)
Observations	1,399	1,236	1,399	1,236	1,399	1,236
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Log-likelihood	-1454042.609	-1176958.748	-1420682.580	-1108501.884	-1044078.212	-908477.267

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1

Table 8: Effect of Antidiscrimination laws on the working environment (OLS specification)

VARIABLES	(1) Gay Lesbian Policies	(2) Diversity Score
Antidiscrimination	0.046** (0.023)	0.124*** (0.043)
Firm size (employees)	0.000 (0.000)	0.002*** (0.000)
Capital expenditure	-0.000 (0.000)	0.000* (0.000)
Leverage	-0.021 (0.039)	0.083 (0.061)
Observations	22,027	29,488
Firm FEs	Yes	Yes
State FEs	Yes	Yes
Year FEs	Yes	Yes
R-squared	0.724	0.748

Note: Robust standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 9: Effect on the number and average quality of foundings controlling for other shocks (Poisson and OLS specification)

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Entrepreneurial foundings Control for trade secret shocks +/- 10 years	Entrepreneurial foundings Control for banking system shocks +/- 10 years	Average quality of foundings (innovation) Control for trade secret shocks +/- 10 years	Average quality of foundings (innovation) Control for banking system shocks +/- 10 years	Average quality of foundings (VC) Control for trade secret shocks +/- 10 years	Average quality of foundings (VC) Control for banking system shocks +/- 10 years
Antidiscrimination law	-0.071*** (0.026)	-0.091*** (0.032)	0.003*** (0.001)	0.003*** (0.001)	0.022** (0.009)	0.022** (0.009)
GDP per capita	0.019*** (0.003)	0.018*** (0.003)	0.000* (0.000)	0.000* (0.000)	0.002** (0.001)	0.002** (0.001)
Firms	0.001** (0.001)	0.002** (0.001)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Non-competes	0.226 (0.139)		-0.001 (0.001)		-0.055** (0.025)	
UTSA	0.084* (0.050)		-0.000 (0.000)		-0.014* (0.007)	
Bank Inter-state deregulation		0.025 (0.024)		-0.001** (0.000)		-0.011** (0.005)
Bank Intra-state deregulation		-0.048 (0.039)		0.000 (0.000)		-0.000 (0.004)
Observations	1,335	1,399	1,049	1,113	1,335	1,335
State FEs	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes
Log-likelihood	-53689.429	-59633.647				
R-squared			0.619	0.633	0.339	0.337

Note: Robust standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 10: Effect of antidiscrimination laws on firm exit (Poisson and OLS specifications)

VARIABLES	(2) Firm exit +/- 10 years	(2) Firm exit +/- 5 years	(3) Proportion of micro firms exiting (<5 employees) +/- 10 years	(4) Proportion of small- medium firms exiting (>5 and <250 employees) +/- 10 years	(5) Proportion of large firms exiting >250 employees +/- 10 years
Antidiscrimination law	-0.037 (0.030)	-0.004 (0.019)	0.001 (0.001)	-0.001 (0.001)	0.000 (0.000)
GDP per capita	0.003 (0.004)	0.002 (0.005)	-0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Firms	0.003*** (0.000)	0.003*** (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)
Observations	1,399	1,236	1,399	1,399	1,399
State FEs	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes
Log-likelihood	- 108342.688	- 100619.504			
R-squared			0.315	0.324	0.357

Note: Robust standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.1

APPENDIX

Table A1: Correlations

	1.	2.	3.	4.	5.	6.	7.	8.
1. Entrepreneurial foundings	1							
2. Average quality (innovation)	0.243 ^{***}	1						
3. Average quality (VC)	0.226 ^{***}	0.500 ^{***}	1					
4. Antidiscrimination law	0.0437	0.442 ^{***}	0.290 ^{***}	1				
5. GDP per capita	0.0306	0.380 ^{***}	0.236 ^{***}	0.314 ^{***}	1			
6. Firms	0.968 ^{***}	0.338 ^{***}	0.269 ^{***}	0.0886 ^{***}	0.136 ^{***}	1		
7. Job creation incumbents	0.968 ^{***}	0.336 ^{***}	0.292 ^{***}	0.0663 [*]	0.0963 ^{***}	0.981 ^{***}	1	
8. Job destruction incumbents	0.938 ^{***}	0.342 ^{***}	0.254 ^{***}	0.0769 ^{**}	0.143 ^{***}	0.975 ^{***}	0.960 ^{***}	1

*** p<0.01, ** p<0.05, * p<0.1

Table A2: Determinants of antidiscrimination law

VARIABLES	(1) Antidiscrimination enactment	(2) Antidiscrimination enactment	(3) Antidiscrimination enactment	(4) Antidiscrimination enactment
Entrepreneurial foundings	-0.000 (0.000)			-0.000 (0.000)
Firms	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.001)
GDP per capita	0.004*** (0.001)	0.003 (0.002)	0.004*** (0.001)	0.004 (0.002)
Average quality of foundings (innovation)		0.258 (2.321)		-0.014 (2.302)
Average quality of foundings (VC)			0.114 (0.250)	0.020 (0.265)
Observations	1,460	1,264	1,309	1,113
State FEs	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes
R-squared	0.056	0.052	0.056	0.052

Note: Robust standard errors clustered by state in parentheses. *** p<0.01, ** p<0.05, * p<0.1

TABLE A3: State level regressions with additional controls

VARIABLES	(1) Entrepreneurial foundings	(2) Average quality of foundings (Innovation)	(3) Average quality of foundings (VC)
Antidiscrimination law	-0.055** (0.025)	0.003*** (0.001)	0.022** (0.009)
GDP per capita	0.018*** (0.003)	0.000 (0.000)	0.002** (0.001)
Firms	0.001** (0.001)	0.000 (0.000)	0.000 (0.000)
Republican state (presidential elections)	0.056*** (0.016)	-0.001*** (0.000)	-0.002 (0.004)
Republican state (Governor elections)	0.005 (0.011)	0.000 (0.000)	-0.003 (0.005)
Tax rate	-5.080*** (1.627)	0.001 (0.015)	-0.177 (0.189)
Observations	1,388	1,102	1,326
State FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
R-squared		0.637	0.337
Log-likelihood	-56318.221		

Robust standard errors clustered by state in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table A4. Effect on entrepreneurial foundings and average quality of foundings on a subset of similar states (excluding California and considering only states passing the law)

VARIABLES	(1) Entrepreneurial foundings	(2) Average quality of foundings (Innovation)	(3) Average quality of foundings (VC)
Antidiscrimination law	-0.048* (0.027)	0.001** (0.000)	-0.006 (0.013)
GDP per capita	-0.008 (0.006)	-0.000 (0.000)	0.007** (0.003)
Firms	0.008*** (0.002)	-0.000 (0.000)	-0.001 (0.001)
Observations	356	280	354
State FEs	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes
R-squared		0.674	0.428
Log-likelihood	-6699.209		

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1