

Punctuated Entrepreneurship (Among Women)

Matt Marx[†]

Abstract: The gender gap in entrepreneurship is explained in part by employee non-compete agreements. Exploiting exogenous state-level variation in non-compete policy, I find that women more strictly subject to non-competes are 11-17% more likely to start companies after their employers dissolve. This result is not explained by the incidence of non-competes or lawsuits; however, women face higher relative costs in defending against potential litigation and in returning to paid employment after abandoning their ventures. Thus entrepreneurship among women appears “punctuated” in that would-be female founders are throttled by non-competes, their potential unleashed only by the failure of their employers.

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[†] Boston University Questrom School of Business, mattmarx@bu.edu / 617-353-4278. I am grateful to John Bauer Esq., Russell Beck Esq., Niles Eldredge, Michael Ewens, Benjamin Fink Esq., Lee Fleming, Aleksandra Kacperczyk, Sarah Kaplan, David Pardue Esq., and Timothy Simcoe for feedback as well as to seminar participants at the 2017 Kauffman Foundation Entrepreneurship Scholars Conference and Copenhagen Business School. Any opinions and conclusions expressed herein are those of the author and do not necessarily represent the views

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INTRODUCTION

Although the percentage of U.S. businesses owned by women is rising,¹ the persistence of a gender gap in entrepreneurship generally, coupled with a wider disparity among high-growth startups (Brush et al., 2014), has led scholars and policymakers alike to search for addressable factors affecting rates of entrepreneurship among women. Several theories have been advanced, including differences in risk-aversion and overconfidence (Eckel and Grossman, 2008); lack of exposure to networks, and investors (Belcourt, Bucket, and Lee-Gosselin, 1991; Loscoccoco, 1991); stereotyping (Thebaud, 2010, Gupta and Bhawe, 2007); and taste-based discrimination (Brooks, et al., 2014; Ewens and Townsend, 2017).

Except perhaps for college students starting companies, the decision to found a firm is made in the context of a worker's prior employment. Industry experience can be a potent asset, both in generating ideas as well as assembling both human and financial capital to overcome the liability of newness in nascent ventures (Klepper, 2009). But the knowledge and training gained in the workplace may arrive with restrictions—specifically, *post-employment* restrictions, which are governed by the state. One such post-employment restriction is a non-compete (NC), by which a worker covenants neither to found nor join a rival for a period of time following separation from the employer. NCs specify a length of time the restriction is valid (typically 1-2 years) as well as a field of expertise, which may list specific competitors but is typically construed to include the emergence of new rivals, including startups founded by ex-employees. Approximately 18% of U.S. workers across occupations are subject to an NC (Prescott et al,

¹ The 2012 Survey of Business Owners reports 35.8% of businesses are women-owned, vs. 26% in 1997.

2016), a figure which rises to nearly half in technical occupations (Marx, 2011).

Starting a business while subject to an NC adds legal risk to business risk because one's former employer may sue to enjoin the founder from continuing with a rival venture. An ex-employee subject to an enforceable NC must decide whether she can afford to undertake a legal defense, or whether it will have been worth starting the firm if she needs to abandon the venture due to a NC lawsuit and thus re-enter paid employment at an existing firm. These expected costs may act as a sharper brake on entrepreneurship among women, for at least two reasons.

First, anticipated costs of defending against potential litigation may be disproportionately high for women and the businesses they found. Typically, both the worker and company are targeted in a lawsuit, which may drain both personal and corporate reserves. Although more than one-third of businesses in the U.S. are owned by women, just 2% of venture capital investments go to women-owned firms (Azevedo, 2017), which may indicate that female entrepreneurs have fewer financial resources to draw upon in the event of a lawsuit. If women are less well equipped to defend themselves against a potential lawsuit, they may be less eager to enter to launch a rival venture when subject to an NC.

Second, even the financial expense of mounting a legal defense does not appear insurmountable, there is nonetheless the possibility of failing to prevail in court. In such a case, the entrepreneur will be enjoined from continuing to work at the rival firm she founded, either by shutting down the venture or by resigning and leaving it in the hands of other founders or employers. Either way, she may need to re-enter paid employment with an existing firm. Women may face greater workforce re-entry barriers than men due to a perceived lack of career commitment (McRae, 1993), which might raise the cost of re-entering paid employment after abandoning entrepreneurship and thus deter women disproportionately from founding new

ventures. Moreover, either of these factors could be exacerbated by the frequent finding that risk aversion tends to be higher among women (for an overview of related work, see Bertrand, 2011).

“Punctuated” entrepreneurship

If women are subject to higher relative costs of engaging in entrepreneurship when subject to tightly enforceable NCs, it may be that female would-be founders will be *more* likely to start a rival venture when they are *not* subject to such contractual obligations. One situation in which workers are released from post-employment restraints is the dissolution of their employer.

Entrepreneurial aspirations of women subject to NCs may be throttled for an extended time and only unleashed by their employer’s dissolution.

The theory of “punctuated equilibria” was pioneered by Eldredge’s (1971) examination of the fossil record and further elaborated in subsequent work with Gould (1972). Eldredge found that Darwin’s (1859) claim that natural selection operated gradually was inconsistent with long periods of species-level stasis followed by dramatic episodes of speciation. Darwin, not unaware of the discrepancy, had sought to lay blame at the incompleteness of the fossil record (Gawne, 2015) whereas Eldredge responded by amending the theory.

Similarly, rhetoric regarding entrepreneurship frequently invokes Schumpeter’s (1942) notion of the “*perennial* gale of creative destruction...*incessantly* revolutioniz[ing] the economic structure from within, *incessantly* destroying the old one, *incessantly* creating a new one” [emphases mine] (pp. 82-83). The cycle of creative destruction may seem “perennial” or in constant flow for those without industry experience or who live in states such as California, which proscribes NCs. But for women subject to NCs, who may anticipate particularly high costs of defending themselves against potential lawsuits and/or in returning to paid employment after

abandoning their ventures, entrepreneurship may appear less perennial and more punctuated.

I test the whether entrepreneurship is punctuated among women by NCs using employer-employee matched records from the U.S. Census Bureau for 24 states and the District of Columbia. The ability of companies to enforce NCs is governed at the state level and not by any national statute. For this study, variation in is drawn from two state Supreme Court rulings regarding NCs in Vermont and South Carolina as well as a misleadingly-worded constitutional amendment in Georgia via which citizens inadvertently voted to tighten NC enforcement policy.

Following the dissolution of their employers, women are generally less likely than men to found new firms. However, women are *more* likely to found firms following their employers' dissolution if NC enforcement was recently tightened. This effect obtains only among firms formed in the same industry as the prior employer, where post-employment restraints have force; moreover, those who found firms in the same industry as their former employers almost always do so in the same state (such that the local NC policy still applies). The result is robust to various sampling and treatment assumptions. Finally, NC policy does not appear to materially affect the likelihood of firm failure.

I then examine possible mechanisms underlying this finding. Reviewing more than 11,000 defendants in NC lawsuits since 2003, I find that the occupation-adjusted hazard of being sued regarding an NC is only half as great for women as for men. Given that women appear to be more highly impacted by the tightening of NC policy even though they are sued less often, it may be that there is a greater "chilling effect" on women, who thus violate NCs less often.

Finally, I examine whether women face higher relative costs of defending themselves against potential lawsuits and also in returning to paid work following the abandonment of an

entrepreneurial venture. To the first point, women who become entrepreneurs do so sooner in their careers than men and with substantially lower cumulative earnings, indicating that they have fewer financial resources with which to mount a legal defense. To the second point, women who abandon entrepreneurial ventures and return to paid employment earn less than they did before starting their company whereas men are not similarly penalized. Both of these results indicate why women may be more reluctant to found companies while subject to NCs, and thus we see more entrepreneurial activity among women following the dissolution of their employers.

Related work

Numerous articles have examined the theoretical and empirical implications of NCs, but to my knowledge none has examined gender. My results are perhaps most related to papers that explore the impact of NCs on entrepreneurial activity. At the firm level, Stuart and Sorenson (2003) find a negative association between NC policy and within-industry firm foundings following the completion of an IPO or acquisition by a biopharmaceutical firm, positing that NCs discourage executives and key technical personnel of those companies from leaving to form rivals. Essential to their analysis is that NCs are typically written to be transferrable to the acquiring firm, which is the opposite of the case of firm dissolution examined here. Also at the firm-level, Samila and Sorenson (2011) find that more investments by venture capitalists yield more business starts, instrumenting the former using average returns to university endowments.

At the individual level, Starr et al. (2017) find that less intra-industry entrepreneurship occurs in states with tighter NC policy, as compared to law firms which are unaffected by NCs. Like Starr et al. (2017), I use employer-employee matched data and find that the effects of NCs are limited to within-industry entrepreneurs. However, the longer time-series available to me coupled with an extended set of state-level reversals reported by Ewens & Marx (2017) facilitate

a difference-in-differences analysis based on three arguably exogenous changes to NC policy. Babina (2018) uses Census data to examine the connection between corporate financial distress and employee exit, finding that firms subject to industrywide negative shocks spawn more startups (especially in states where non-compete policy is less strict). Jeffers (2018) applies several NC policy shocks to LinkedIn data, finding a negative effect of NCs on entrepreneurship. In sum, although a connection has been drawn between NC policy and entrepreneurial activity, gender has not been considered as a possible factor.

EMPIRICAL APPROACH

I follow prior literature (Marx, Strumsky, and Fleming, 2009; Garmaise, 2011; Belenzon and Schankerman, 2012; Conti, 2015; Younge, Tong, and Fleming, 2015; Starr, 2018; Jeffers, 2018) in leveraging exogenous state-level policy shifts to identify the impact of NCs.² However, I depart from prior literature by examining post-dissolution job choices, which has two advantages. First, in the case of a firm dissolution, any employee previously subject to an NC is no longer bound. Second, examining post-dissolution choices also eliminates ambiguity between voluntary and involuntary turnover, which can be difficult to distinguish.

I use worker-level observations from the U.S. Census Bureau's Longitudinal Employer

² The unavailability of broad, longitudinal data regarding NC use makes it difficult to know whether a particular worker was subject to an NC. The only known longitudinal data on NC use is limited to CEOs (Kinia, et al., 2018), and the broadest survey data regarding NC use is for a single year (Prescott, et al. 2016). That said, NCs not only restrict the careers of those who sign them but impose negative externalities on those who have not (Starr, Frake, and Agrawal, 2018).

Household Dynamics (LEHD) database for 24 states and the District of Columbia.³ These data include quarterly wages for each worker from as early as 1986 through 2011 or 2012. Age, education, and gender are available for each worker; see Abowd et al. (2005) for a detailed description of the LEHD data. State-level records are linked by State Employer Identification Number to national Employer Identification Number in order to track movement across states.

Given that the data cover only approximately half of the U.S., I am only able to observe employment transitions within the states available to me. Workers may move from an available state to an unavailable state, or from an unavailable state to an available state. In the former case, I know whether the worker left a firm following its dissolution but not whether they subsequently founded a firm. In the latter case, I know whether a worker founded a firm but not whether their former employer dissolved. I thus eliminate from the dataset any observation where I do not observe the worker in the prior year. I also eliminate any observation where I cannot observe the worker in the subsequent year. Workers who are employed simultaneously by multiple firms, or who move back and forth between two firms, are also dropped.

At the firm level, Census provides establishment-year observations nationwide from 1976 until 2012. Firm dissolution is captured as a dummy variable set to one when all establishments of a firm cease reporting. Analysis is limited to observations of the next job after a worker's employer dissolves; transitions from a non-dissolved firm to another are not considered. There

³ Coverage of LEHD data for my project includes MD since 1986; CO/IL/IN/LA/MO/WA since 1990; OR/PA since 1991; GA since 1994; NM/RI/TX since 1995; HI/ME since 1996; DE/IA/NV/SC/TN since 1998; UT since 1999; OK/VT since 2000; DC since 2002; AR since 2003. CO/IL/TX are available only through 2011.

are about 1,230,000 such transitions (Census disclosure rules mandate rounding.)

The dependent variable $ENTREP_{it}$ reports whether the worker's next job following the dissolution of their employer involves joining an established firm (0) vs. founding a new firm (1). The LEHD does not denote the legal founders of the firm, so I infer these as follows. For each newly-founded firm, I record the workers who had earnings during the quarter when the firm first paid wages. (More than 85% of new firms had five or fewer employees during their first wage-paying quarter. If a firm has more than 50 employees during its first wage-paying quarter, I do not consider the firm to be a startup.) Of those who had earnings in the firm's first wage-paying quarter, I set $ENTREP_{it}$ to 1 for the top three earners (results are robust to considering the top five earners).⁴

Exogenous variation in state-level NC policy

My empirical strategy relies on plausibly exogenous variation in state-level NC policy. Ewens and Marx (2017) document 14 such shifts. Of these, five⁵ fall within the timeline of my LEHD data, and four are potentially usable for analysis. Louisiana's 2001 change is inapplicable

⁴ Note that this may fail if founders' wages are discounted vs. other early employees. I cannot distinguish this using the LEHD, but as a robustness check I set $ENTREP_{it}$ to 1 only for the >85% of firms that paid wages to five or fewer workers in their initial quarter (as opposed to 50). If for example founders accept zero wages in the firm's first quarter and appear in the LEHD only in subsequent quarters, then my results also might be interpreted in part as discouraging workers from joining new startups as well as being an official founder.

⁵ Colorado, Texas, and Illinois changed non-compete policy in 2011. However, these changes cannot be used as 2012 data is unavailable for those states.

because it applied only to workers who joined an existing firm, not those who started their own firm. This leaves Vermont (2005), Oregon (2008), South Carolina (2010) and Georgia (2010).

The policy change in Oregon did not result from a court decision but rather was enacted by the legislature. As a consequence, it is difficult to label Oregon's reform exogenous, as it was widely debated and perhaps anticipated by firms and workers. Moreover, its provisions did not apply retroactively to already-signed NCs, only those signed in the following year. Firms certainly could ask workers to sign revised NCs, but because the new law *loosened* restrictions on NCs, incentives favored firms leaving existing, tighter NCs in place.

Like Oregon, the change in Georgia was not enacted by the court; it involved an amendment to the state Constitution. Although this amendment was ratified by popular vote, there are reasons to treat this as an exogenous event (at least for workers). The ballot summary of the amendment read as follows: *Shall the Constitution of Georgia be amended so as to make Georgia more economically competitive by authorizing legislation to uphold reasonable competitive agreements?* The effect of the Georgia law was to give judges greater discretion in choosing to enforce NCs that were otherwise invalid under state law. For example, if state law limits NCs to a term of one year following separation of employer and employee, but the employee's contract contained a two-year NC, the judge can reduce the term of the contract to one year and enforce it. Doing so not only converts invalid NCs into valid ones but may give firms an incentive to be careless or even strategic in crafting NCs. Many employees will not know the details of NC law and may accept the contract at face value, and if a dispute lands in court the judge may simply rewrite the overbroad contract in a valid form.

The summary of the Georgia constitutional amendment did not mention NCs, so voting citizens could not have been expected to understand that the law would tighten NC policy in the

state. Indeed, Atlanta Attorney David Pardue criticized the amendment as “unconstitutional” for having misled the public regarding its actual purpose (Pardue, 2011). Moreover, unlike the Oregon change, the fact that the Georgia change *tightened* enforcement gave firms an incentive to require current employees to sign new NCs that reflect the new law. Said Benjamin Fink, partner at the Atlanta firm of Berman Fink Van Horn LLP, “[Law f]irms definitely issued alerts when the new law went into effect, and many employers revised their employment and restrictive covenant agreements to take advantage of the new law” (Fink, 2017). Van Horn’s contention is further supported by the fact that Georgia, in further contrast to Oregon, does not require additional firms to compensate existing employees for signing new or revised NCs; rather, continued to remain employed at the firm suffices as “consideration.” For all of these reasons, I treat Georgia’s change as exogenous.

South Carolina’s 2010 policy adjustment also involved judicial latitude in rewriting overbroad NC contracts to fit state law, but it differed in two respects. First, the SC change was the result of a state Supreme Court decision (*Poynter v. Century Builders*), which as Jeffers (2018) argues was likely unanticipated and immediately binding on any non-compete regardless of when it was signed. Second, whereas Georgia gave judges additional power to rewrite such constraints, in *Poynter* the state Supreme Court rejected the notion that judges should be able to or rewrite otherwise-invalid NC contracts.

The *Summits 7 v. Kelly* decision, handed down by the Supreme Court of Vermont in 2005, was likewise unanticipated and binding on existing NCs. Prior to *Summits 7*, Vermont lacked a clear position on whether continuing employment constituted sufficient consideration for an NC entered into after a worker started their job. Given evidence that approximately half of workers are asked to sign a NC on or after their first day at work (Marx, 2011), not needing to

give workers a bonus or raise in exchange for signing an NC after stating at the job could impose restraints on workers. Indeed, Starr (2018) finds lower wages among workers in states without consideration requirements (as in post-2005 VT).

Empirical specification

I estimate the following equation for the job taken by workers immediately following the dissolution of their employer. For treated states, only the observations in a symmetric window surrounding the treatment year are included. For untreated states, all years are included. The estimating equation is

$$ENTREP_{it} = \rho_0 + \rho_1 TIGHTEN_NC_{st} + \rho_2 FEMALE_i + \rho_3 TIGHTEN_NC_{st} * FEMALE_i + \rho_4 X_i + \gamma t + \vartheta_s$$

where i , t , and s index worker, year, and state. The dependent variable $ENTREP_{it}$ is 1 if the job immediately following the dissolution of one's employer involved starting a new company. $TIGHTEN_NC_{st}$ is set to 1 if state s tightened NC enforcement in year t and to -1 if enforcement was loosened in year t . In most models, this variable is set to 1 for two years after the law changed. X_i is a set of individual demographics including age, education, and earnings. For education, a dummy variable indicates whether the worker completed college (instead indexing whether the worker only completed high school returns similar results). Earnings are calculated in the year prior to the law change, if any (and otherwise simply for the previous year), in state s . Year and state fixed effects are also included, and robust standard errors are clustered at the worker level.

RESULTS

Descriptive statistics for approximately 1,230,000 workers whose employers dissolved are in

Panel A of Table 1. About 5.5% of workers start a company after their former employer dissolves; 3.8% of workers do so in the same industry as the former employer (as measured by 6-digit North American Industry Classification System (NAICS) codes). Slightly more than one-third of workers are female, and slightly more than two-thirds lack a college degree. Workers are 41 years old on average.

Table 1 about here

Next, I turn to model-based output in Table 2. In column (1), women are considerably less likely than men to start a company following the dissolution of their employer. Thus it does not appear to be the case that displaced women are pursuing entrepreneurship for lack of available paid employment.⁶ Those who lack a college degree are more likely to enter into entrepreneurship. Older workers are less likely to start a firm after their employer dissolves. Higher earners are considerably more likely to found a firm.

Table 2 about here

Turning to the explanatory variable that captures shifts in how tightly employee NCs are enforced, the coefficient on *TIGHTEN_NC* is estimated rather imprecisely. In column (2) I interact *TIGHTEN_NC* with *FEMALE*.⁷ Introducing the covariate for the tightening of NC

⁶ That said, if women are more likely to leave the workforce following the dissolution of their employer, I cannot detect this in the LEHD. Due to the missing 26 states, I do not know whether a worker moved to a state for which I do not have data or left the workforce entirely.

⁷ Interactions with education, age, and earnings were also calculated but are not shown. The results shown do not depend on their exclusion.

enforcement reverses the trend of entrepreneurial activity among women: the estimated coefficient for *TIGHTEN_NC * FEMALE* is positive with statistical significance at the 5% level. This indicates that even though women are generally less likely to start a firm following the dissolution of their employer, they are more likely to do so when subject to tighter NCs. Predicted probabilities of entrepreneurial activity indicate that, following the dissolution of their employers, women are 17.0% more likely to found a new firm in states where NC laws have tightened and 11.2% less likely where laws have loosened. In other words, the dissolution of women's employers appears to unleash entrepreneurial capacity which had previously been throttled by enforceable NCs.

In column (3), the dependent variable represents founding not just any startup but only in the same 6-digit NAICS code as the worker's former employer (i.e., an intra-industry spinoff). The estimated coefficient on *TIGHTEN_NC * FEMALE* retains its magnitude and statistical significance when restricting to entrepreneurship within the same industry. Column (4) further indicates that this effect is limited to entrepreneurial activity within the same industry, which is where we would expect post-employment restraints such as NCs to have their strongest effect (Garmaise, 2011; Starr, et al., 2017). Here, the dependent variable is limited to starting a company in a different 4-digit NAICS code than one's former employer. As such, it appears that only within-industry entrepreneurship among women is "punctuated" by firm failure when more tightly subject to NCs.

Table 3 contains robustness checks. Column (1) confirms that the results in prior columns do not depend on a 2-year treatment window. In column (2) of Table 3, I adopt a more conservative definition of founding a firm. Whereas the foregoing analysis took the top three wage-earners in the firm's first wage-paying quarter to be the founders of the firm, not

considering as startups new firms with more than 50 employees in that first quarter, in column (3) I limit the definition of a startup to the 85% of firms that had five or fewer employees in the first quarter. This stricter definition of entrepreneurial activity yields consistent results.

The remaining columns of Table 3 show that the results are robust to a more conservative sampling policy where I limit the sample to workers for whom their entire career history is available to me. Unlike in the prior models, where I eliminate the worker's first or last record in a given state that is not followed by another record in a state available to me, in columns (3) and (4) of Table 3 such workers are eliminated entirely. In particular, I delete any worker with more than a one-year gap in their employment record before the age of 50.⁸ Doing so approximately halves the sample. Results are robust in this smaller sample, whether using a two- or twenty-year window surrounding the policy reversals.

Table 3 about here

Additional robustness tests are performed in undisclosed results. First, I confirm that the results are robust to the inclusion of Oregon as a treated state. This check cannot be displayed due to Census disclosure-avoidance regulations, as showing models that differ only by the inclusion of Oregon might facilitate inferences about that state. Second, I find no effect when limiting the analysis in Table 2 to industries where NCs are ineffectual, including law, education,

⁸ I also considered eliminating workers whose employment record starts after age 30 or 35, possibly evidencing that their earlier career was not in one of the 24 states in these data. Given concerns that doing so might artificially censor women who entered the workforce after childbearing years, I do not take this step in columns (3-4) of Table 3, but the results are robust to this elimination.

religious services, and medical fields for states that ban NCs for physicians:

AR/CO/DE/IL/MA/NM/TN/TX. My findings are also robust to conducting a triple-differences analysis: treated women in non-affected industries are no more likely than men to found rival firms following their employers' dissolution. These results are not shown because there are too few workers in unaffected industries whose employers dissolved to satisfy Census disclosure requirements.

Identifying assumptions

Estimation of the regression equation will be confounded if assumptions underlying the analysis do not hold. In particular, it is important to establish 1) the absence of pre-trends 2) external validity of the treated states and workers, 3) whether there is a “home bias” in where workers engage in entrepreneurial activity, and 4) whether shifts in NC policy affect the propensity of firms to fail. I consider each of these in turn.

Pre-trends. Important to establish is that the results seen in Tables 2 and 3 are not an artifact of patterns preceding the three exogenous changes in NC policy. In Figure 1, I plot coefficients from an unreported model that interacts *FEMALE* with a series of interactions of *TIGHTEN_NC* and indicators for the treatment year, 1 year prior to treatment, 2 years prior to treatment, 1 year following treatment, and 2 years following the treatment. As is visible in Figure 1, the estimated coefficients prior to the treatment are not significantly different from zero. However, estimated coefficients for the two years following the treatment year are positive, statistically significant, and growing in magnitude.

Figure 1 about here

External validity. Do treated states differ from untreated states? Panel B of Table 1

shows difference-of-means tests by treatment. The first line of Table 1, Panel B, indicates that there are not substantial differences between rates of post-dissolution entrepreneurship in treated vs. untreated states: 5.9% vs. 5.4%, with statistical significance at the 8% level. Differences in same-industry entrepreneurship are somewhat more distinct. There are slightly more post-dissolution women in treated states (37.9% vs. 36.2%), and workers are slightly older (42.3 vs. 41.4 years). No significant differences in education are evident.

Additionally, do workers who “go down with the ship” by staying with the firm until the year of its dissolution differ from those who leave earlier? As noted by Kacperczyk and Marx (2015), it is possible that workers with higher opportunity costs leave struggling firms earlier. If so, then my results would apply primarily to less talented workers. Although the LEHD does not have information on occupations or roles within the firm, earnings may serve as a within-firm indicator of relative value. In undisclosed results, I compare the maximum annual earnings for workers who stayed until the firm’s final year vs. those who left earlier. Earnings for those who stayed until the end are no lower, and even somewhat higher, indicating that the workers I study do not appear to be substantively “worse” than those who left failing firms earlier.

“Home bias” in entrepreneurship. Do workers start new firms in the same state where their employer dissolved? If such entrepreneurs tend to cross state lines, then the effect of a shift of NC policy in the ex-employer’s state may be more difficult to tie to entrepreneurial activity. Evidence from employer-employee matched data in Denmark shows that Danish entrepreneurs do not move far when starting new ventures (Dahl & Sorenson, 2012), so this may be a reasonable assumption. However, Marx et al. (2015) find that a tightening of NC policy in a focal state may lead workers to migrate to states such as California, where NCs are proscribed, which could affect my findings.

In the LEHD data available to me, of the workers who found a firm following the dissolution of their employer, 99.06% do so in the same state. Of those who subsequently found a firm in the same industry as their former employer, that percentage rises to 99.56%. This contrast is even more evident in Table 4, which estimates the likelihood that a worker's job subsequent to the dissolution of their employer is in the same state. In column (1), women as well as older workers, less-educated workers, and wealthier workers are all more likely to find another job in the same state following the dissolution of their employer. Displaced workers who start a company are more likely to stay in the same state than those who take a job with an existing firm.

Columns (2-4) of Table 4 indicate that the "home bias" for entrepreneurs is stronger when the worker finds a company in the same industry as the prior, dissolved employer, as is evident from the magnitudes on the estimated coefficients on *ENTREP (same industry as prior employer)* vs. *ENTREP (different industry from prior employer)* in column (4); the two are statistically distinct. Given that post-dissolution entrepreneurs, and particular those who found in the same industry, are more likely to stay in state, it seems plausible that such transitions would be influenced by state-level changes in NC policy.

Table 4 about here

NC policy and firm failure. Does NC policy affect firm failure? If so, then baseline rates of employer dissolution may be skewed in treated states. Garmaise (2011) finds no connection between NCs and firm performance, but Younge and Marx (2016) find a short-term rise in Tobin's q following a tightening of NC policy. The ability to pay lower wages (Garmaise, 2011; Starr, 2018) and retain employees (Marx, et al. 2009) may influence firm survival. Several authors have found a connection between NCs and entrepreneurial entry, which may threaten

incumbent firms (Stuart and Sorenson, 2003; Samila and Sorenson, 2011; Starr, et al. 2017).

I investigate this possibility in Table 5 using a 10% random sample of firms from the Longitudinal Business Database. Columns (1) and (2) estimate the likelihood that a given firm will fail in any of the states available to me in the LEHD, based on the same NC policy shifts analyzed above. Although older firms as well as those with more employees are less likely to fail, I find no statistically significant impact of changing NC policy on firm failure. (Nor are significant results obtained in undisclosed results on split samples of larger vs. smaller firms, or when interacting *TIGHTEN_NC* with firm size.) Thus firm failure is not substantially affected by the state-level policy shifts used for identification.

The Longitudinal Business Database is available for all states from 1976-2012, so I can test firm failure using NC changes not just in three states but more than a dozen.⁹ These estimates, presented in columns (3) and (4), likewise do not indicate a sharp effect of NC policy on firm failure. Finally, the coefficient on *TIGHTEN_NC* is estimated no more precisely when including 2-digit NAICS fixed effects (columns 2 & 4).

Table 5 about here

Mechanisms

What explains the effect of NC policy on post-dissolution entrepreneurship among women? I

⁹ These are: MI (1985), FL (1996), LA (2001), OH (2004), VT (2005), OR(2008), ID (2008), WI (2009), SC (2010), GA (2010), CO (2011), IL (2011), TX (2011). Note that the Louisiana change, which does not affect entrepreneurial entry, might nonetheless affect firm performance and thus can be utilized in this test.

investigate three possibilities. First, perhaps substantially more women sign NCs. Second, independent of how often women sign NCs, firms might for some reason pursue legal action against female ex-employees more aggressively. Third, even if women sign NCs at rates similar to men and are no more likely to be sued for violating NCs, there may be a stronger “chilling effect” whereby women are more likely to honor the agreement they have signed. A reluctance to violate the NC among women might be explained by higher expected costs of litigation or venture abandonment, either of which could be exacerbated by risk aversion. I consider each potential mechanism in turn.

Do more women sign NCs? If women are more likely to sign NCs, their entrepreneurial potential might be throttled until their employer dissolves and they are no longer bound by the contract. Three papers to my knowledge contain information on the prevalence of NCs by gender. First, Lavetti et al. (2017) utilize data from a survey of 2000 physicians in five states asking “*If you left your practice, would you be under a non-compete?*” They find that physician gender has no bearing on having signed an NC. Similarly, Marx (2011) conducts a survey of 1,029 engineers in a variety of industries. When controlling for rank, he finds no gender differences either in whether a worker is asked to sign an NC or whether the worker complies by signing the NC. Starr et al. (2017) conduct an online survey with 11,505 respondents across a variety of occupations, estimating that 18.8% of men are currently subject to an NC vs. 17.3% of women. When controlling for wages, education, and other observables, it appears that women are 3 percentage points more likely to sign. Taken together, these surveys do not conclude that women are subject to substantially more NCs. Hence, it seems unlikely that the incidence rate of NC contracts among women can largely account for my results.

Are women at higher risk for being sued regarding an NC? Even if women are not

subject to substantially more NCs than men, they may nonetheless be at a greater risk of being sued when they violate the terms of the contract. This mechanism is difficult to evaluate using traditional legal databases such as Westlaw, Lexis, or Bloomberg as these contain only published court decisions and not every case filed. However, the Courthouse News Service (CNS) curates a database of *all* cases filed in a large subset of district courts across the U.S. I extracted 7,931 lawsuits from January 2003 through August of 2017 that referenced “non-compete” “post-employment restraints”, or similar terms.

Among the 11,975 defendants were 2,459 first names, 87% of which could be definitively classified as male or female according to genderize.io—a website that generates country-specific probabilities that a given name is male or female. I assigned names to a particular gender when genderize.io returned a confidence score of >80% for that name being male or female in the U.S. For the remaining 415 defendants where their gender could not be reliably classified based on first name, I searched for profiles on LinkedIn, Facebook, and company websites. I assigned gender only when the name and company of the defendant could both be confirmed and when personal pronouns, suffixes (i.e., “Jr.”) or photos gave clues as to gender. Of the 11,975 defendants, 59 remained unclassified.

Only 26.7% percent of defendants were classified as female. Whether this is higher or lower than expected depends on occupation. For example, if NCs were used only in the construction industry, nearly all defendants should be male as the Current Population Survey reports a 93% male workforce. I thus classified occupation for the 2,096 defendants where CNS had this field, shown in Table 6.

Table 6 about here

The next step in constructing a prior expectation of male vs. female defendants is to input the gender distribution for defendants' occupations. For example, the CPS reports that 49% of salespeople are female, whereas that figure is 14.2% of engineers and 92.4% of hairstylists. I then multiply the percentage of lawsuits in each occupation by the percentage of workers in each industry who are female and then sum these to yield an overall expected percentage of female defendants. I further adjust this forecast given the prevalence of NCs in each occupation. The most comprehensive data regarding NC use by industry is from Starr et al, 2017; which reports NC usage by occupation. Each projection is adjusted according to that occupation's prevalence of NCs and is then multiplied by 1.03 to reflect the aforementioned slight overrepresentation of women among NC signers. These adjustments raise the expected percentage of female defendants to 50.2%.

Given that occupation is available only for about one-fifth of the CNS cases, I recalculate this estimate given the 755 NC-related decisions published by Lexis during 2003-2006, where every decision describes the defendants in detail. (Again, only a subset of filed lawsuits go to court, are not settled, and thus are reported in Lexis). The distribution of occupations is generally similar, though lawsuits involving CEOs are more likely to appear in Lexis. Using Lexis cases in Table 6 instead of CNS results in an estimated 49.1% of defendants being female, quite similar to the 50.2% obtained using CNS data. Given that only 26.7% of actual CNS defendants appear to be female, this indicates a nearly 2x overrepresentation of men as defendants in NC lawsuits. Thus the targeting of women by firms for lawsuits seems an unlikely explanation.¹⁰

¹⁰ It is possible that women and men violate non-competes at similar rates, but that women are more responsive to pre-lawsuit threats such as cease-and-desist letters, which can precede legal

Are women less likely to violate NCs due to higher expected costs? Given that the incidence of NCs is roughly similar by gender, and that men are substantially overrepresented as defendants in NC lawsuits, perhaps a more plausible explanation for the stronger effect of NCs on female entrepreneurship is that women are less likely to violate post-employment contracts. Women may be more inclined to honor NCs given both higher relative costs of responding to a lawsuit or greater risk aversion even given similar costs.

An extensive literature in psychology and economists provides evidence on gender differences in tolerance for risk (Bertrand, 2011). Charness and Gneezy (2012) review dozens of experimental studies not designed to study gender differences but that still find higher risk-aversion among women. Although lab experiments tend to be conducted among college students, Dohmen et al. (2005) find similar risk differences in a survey of 22,000 German adults and moreover replicate a lab study with a subset of 450 of the respondents. Observational studies of investment patterns (Jianakoplos and Bernasek, 1998), securities trading (Barber and Odean, 2001), and white-collar crime (Steffensmeier, et al, 2013) are moreover consistent with these findings. If women are less eager to add legal risk to the inherent business risk of starting a new ventures, they may hesitate to engage in entrepreneurial activity when NC enforcement is tighter.

However, literature on gender and risk aversion has been criticized as emphasizing statistically significant differences where magnitudes are small (Nelson, 2014; see also Eckel and Grossman (2008)). Even if risk-aversion profiles do not differ substantially, NCs may nonetheless have a stronger impact on women if they face higher expected costs in undertaking

action. This possibility is difficult to rule out given my data but would not be inconsistent with the “chilling effect” discussed above.

entrepreneurial activity in the shadow of a possible lawsuit. For example, female entrepreneurs may have fewer financial resources with which to mount a legal defense. Of approximately 345,000 entrepreneurs in the LEHD for whom their entire career history is available to me,¹¹ men have considerably higher cumulative earnings before starting their first firm (\$247,200 vs. \$145,900 in deflated 1982 wages). In the first two columns of Table 7, these differences are robust to controlling for age, education, and the number of years before starting one's first company. Column (2) adds fixed effects at the 6-digit NAICS level. I do not observe the percentage of earnings saved, but unless women have much higher savings rates they are likely to have fewer resources with which to defend themselves against a potential lawsuit.¹² Of course, resources may be pooled across co-founders, so if female entrepreneurs have larger founding teams this disadvantage may be offset; in undisclosed results, however, I find the opposite to be the case in the LEHD.¹³

¹¹ Any worker with a gap in their employment record is eliminated from this dataset. It is possible that some workers began their employment in a state unavailable to me and then spent the rest of their career in available states, which could introduce noise into the calculation of pre-entrepreneurial earnings.

¹² Typically, both the ex-employee and her or his startup are named in the lawsuit.

¹³ Personal financial resources may be augmented by startup fundraising, which is unobservable in the LEHD. In unreported results, using Crunchbase data through June of 2016 I find that when controlling for cohort, country, and industry, women-owned businesses raise less capital—both debt and equity—in their first year of operation (when the firm is at highest risk of being sued over an NC).

Table 7 about here

If women entrepreneurs experience higher relative costs of defending themselves against a lawsuit from their former employers, or if they pre-emptively abandon the business rather than fight in court, they may face higher costs in returning to paid employment. Abandoning a startup may involve either resigning—allowing co-founders or subsequent employees to continue with the firm—or shutting down the company entirely. Scholars have documented difficulties in women returning to the workforce after attending to family responsibilities (see for example McRae, 1993), and it is possible that similar stereotypes apply to female entrepreneurs. For example, prospective employers may question whether a job applicant pursued entrepreneurship for the sake of job flexibility and thus question her commitment to the company.

For approximately 100,000 entrepreneurs who 1) worked in another firm before starting their own, 2) abandoned their startup—either by resigning or after its demise, but not following an acquisition, and 3) returned to paid employment, I estimate the ratio of their (1982 deflated) earnings in the first year after abandoning their startup vs. their average earnings prior to founding a firm. This ratio is above 1 for men and below 1 for women, a difference which is robust to controlling for demographics and experience in the remaining columns of Table 7. Column (4) estimates this pre-vs-post entrepreneurship earnings ratio with fixed effects at the 6-digit NAICS level, indicating that women pay a “re-entry” penalty when returning to paid employment following the abandonment of an entrepreneurial venture. This may explain why women subject to a NC may be less eager to start a rival firm and risk it being blocked by a lawsuit from their former employer.¹⁴ Note that these factors should operate independent of risk-

¹⁴ In undisclosed results, these ratios do not differ significantly for women and men who return

aversion profiles but could be exacerbated if women are indeed more risk-averse.

CONCLUSION

The persistence of a gender gap in entrepreneurial activity has been attributed to many factors, including individual preferences such as risk aversion as well as discrimination by resource providers including venture capitalists. This paper establishes that employee non-compete agreements (NCs) disproportionately throttle women's entrepreneurial ambitions, which are unleashed by the dissolution of their employers. I find that women subject to tighter NC policy are 11-17% more likely to start a business in the same industry once their employer dissolves. This result uses employer-employee matched data from 24 states and the District of Columbia, leveraging exogenous changes in NC policy from three states. The underlying mechanisms do not include a higher incidence of NCs or NC-related lawsuits among women; rather, it appears that women may be less likely to violate NCs. This reluctance may be due both to female founders having fewer financial resources as well as a wage penalty for women who re-enter paid employment at established firms after abandoning their own entrepreneurial ventures. Both of these are observable in the employer-employee matched data.

Because the state governs the use of NC agreements and other post-employment restraints, public policy might be changed to spur female entrepreneurship. Given that women may be less willing found new firms due to the chilling effect of *potential* litigation, given their lesser financial resources and the apparent wage penalty of re-entering the labor market after abandoning a venture, policymakers should consider not only whether NCs will be *enforced* in a court of law but whether they can be *used* at all. Currently, only Illinois bans the use of NCs (for

to paid employment following the acquisition of their startup.

low-wage workers) and empowers its Attorney General to bring civil action against firms for asking such workers to sign NCs unlawfully. Similar approaches in other states may help to even the playing field for female would-be founders, perhaps de-punctuating the entrepreneurial process.

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Table 1: Descriptive statistics and difference-of-means tests

Panel A: Sample statistics. $N=1,230,000$

	Mean	"Median"	Std. Dev.
<i>ENTREP</i> (any industry)	.05486	0	.2277
<i>ENTREP</i> (same industry as prior employer)	.03758	0	.1902
<i>EARNINGS</i>	9.972	6.962	.6589
<i>FEMALE</i>	.3619	0	.4805
<i>AGE</i>	41.38	40.49	10.76
<i>NO COLLEGE DEGREE</i>	.6984	1	.4589

Notes: data are job transitions after the dissolution of the worker's former employer, collected from 24 states and the District of Columbia (AR CO DC DE GA HI IL IN IA LA ME MD MO NV OK OR PA RI SC TN TX UT VT WA). Census disclosure-avoidance rules mandate rounding of observations to the nearest 10,000 and the truncation of reported statistics to four significant figures. Neither minimum nor maximum values can be reported. "Median" is the mean of observations in the 49th, 50th, and 51st percentiles.

Panel B: Difference-of-means, treated vs. untreated states. $N=1,230,000$

	Untreated	Treated	s.e.	p<
<i>ENTREP</i> (any industry)	.05483	.05889	.002329	.0807
<i>ENTREP</i> (same industry)	.03755	.04144	.001946	.0453
<i>EARNINGS</i>	9.972	9.944	.006743	.0001
<i>FEMALE</i>	.3617	.3799	.004917	.0002
<i>AGE</i>	41.38	42.34	.1101	.0001
<i>NO COLLEGE DEGREE</i>	.6984	.7022	.004696	.4202

Notes: data are job transitions after dissolution of the worker's former employer, collected from 24 states and the District of Columbia. Difference of means are calculated based on observations for states that were ever treated (SC VT GA) as opposed to never treated (AR CO DC DE HI IL IN IA LA ME MD MO NV OK PA RI TN TX UT WA).

Table 2: Employee non-compete (NC) policy and transition to entrepreneurship following the dissolution of a worker's employer. *N* is within 10,000 of 1,230,000.

<i>DV = worker founds a new firm</i>	(1)	(2)	(3)	(4)
<i>TIGHTEN_NC</i>	0.0351 (0.0558)	-0.0545 (0.0669)	-0.1159 (0.0798)	0.0376 (0.1308)
<i>TIGHTEN_NC * FEMALE</i>		0.2503 (0.0995)	0.2805 (0.1157)	0.1408 (0.2069)
<i>FEMALE</i>	-0.0911 (0.0088)	-0.0920 (0.0088)	0.0377 (0.0103)	-0.3871 (0.0174)
<i>NO COLLEGE DEGREE</i>	0.1265 (0.0091)	0.1265 (0.0091)	0.1724 (0.0109)	0.0187 (0.0171)
<i>AGE</i>	-0.0012 (0.0004)	-0.0012 (0.0004)	-0.0021 (0.0005)	0.0008 (0.0007)
<i>EARNINGS</i>	0.5534 (0.0056)	0.5534 (0.0056)	0.6228 (0.0064)	0.3407 (0.0108)
Constant	-8.334 (0.0701)	-8.334 (0.0701)	-9.488 (0.0809)	-7.459 (0.1345)
<i>DV = same industry as prior employer?</i>	either	either	yes	no

Notes: Estimates of the likelihood that workers whose employers shut down subsequently started a company instead of taking a job with an existing firm. Observations are for 24 states and the District of Columbia (AR CO DC DE GA HI IL IN IA LA ME MD MO NV OK OR PA RI SC TN TX UT VT WA), eliminating the first observation in each state following a gap in the employment record and the final observation in each state prior to a gap in the employment record. Census disclosure-avoidance rules mandate rounding of observations to the nearest 10,000 for samples greater than 100,000 and the truncation of reported statistics to four significant figures (not including leading zeroes). All models have year and state fixed effects. *TIGHTEN_NC* is a dummy variable set to 1 in state-years where NC policy had tightened within the previous two years and -1 where it had been loosened. State-level policy shifts include tightening in both VT (2005) and GA (2010) and loosening in SC (2010).

Table 3: Employee non-compete (NC) policy and transition to entrepreneurship following the dissolution of a worker's employer: Robustness tests

<i>DV = worker founds a new firm</i>	(1)	(2)	(3)	(4)
<i>TIGHTEN_NC</i>	-0.0230 (0.0704)	-0.1044 (0.0925)	-0.1213 (0.1259)	-0.0337 (0.1137)
<i>TIGHTEN_NC * FEMALE</i>	0.2762 (0.1002)	0.2852 (0.1306)	0.5136 (0.1994)	0.4247 (0.1647)
<i>FEMALE</i>	0.0246 (0.0094)	0.1995 (0.0114)	0.0601 (0.0146)	0.0571 (0.0141)
<i>NO COLLEGE DEGREE</i>	0.1662 (0.0099)	0.2514 (0.0127)	0.0627 (0.0147)	0.0574 (0.0141)
<i>AGE</i>	-0.0024 (0.0004)	-0.0060 (0.0005)	-0.0022 (0.0010)	-0.0018 (0.0010)
<i>EARNINGS</i>	0.6297 (0.0057)	0.2704 (0.0070)	0.6640 (0.0087)	0.6633 (0.0084)
Constant	-10.70 (1.014)	-6.221 (0.0900)	-9.875 (0.1288)	-9.877 (0.1253)
Observations (rounded to nearest 10,000)	1,490,000	1,230,000	600,000	650,000
Window surrounding treatment year	20	2	2	20
Max employees in first quarter for startup	50	5	50	50

Notes: Estimates of the likelihood that workers whose employers shut down subsequently started a company instead of taking a job with an existing firm. Observations are for 24 states and the District of Columbia (AR CO DC DE GA HI IL IN IA LA ME MD MO NV OK OR PA RI SC TN TX UT VT WA). The samples in columns (1-2) omit the first observation in each state following a gap in the worker's employment record as well as the final observation in each state prior to a gap in the employment record. In columns (3-4), any worker with a gap in their employment record is omitted entirely. Samples in columns (1) and (4) are slightly larger than those in (2) and (3) given that the window surrounding policy shocks is 20 years instead of 2. Finally, in column (2) only new firms with fewer than five employees in their first wage-paying quarter are considered to be startups. Census disclosure-avoidance rules mandate rounding of observations to the nearest 10,000 for samples greater than 100,000 and the truncation of reported statistics to four significant figures (not including leading zeroes). All models have year and state fixed effects. *TIGHTEN_NC* is a dummy variable set to 1 in state-years where NC policy had tightened within the previous two years and -1 where it had been loosened. State-level policy shifts include tightening in VT (2005) and GA (2010) and loosening in SC (2010).

Table 4: Estimates of the likelihood that the next job for a worker whose employer dissolved is in the same state as their former employer. *N* is within 10,000 of 1,230,000.

<i>DV = subsequent job in same state</i>	(1)	(2)	(3)	(4)
<i>ENTREP</i> (any industry)	0.9889 (0.0400)			
<i>ENTREP</i> (same industry as prior employer)		1.729 (0.0708)		1.733 (0.0708)
<i>ENTREP</i> (different industry from prior employer)			0.1796 (0.0510)	0.2107 (0.0510)
<i>EARNINGS</i>	0.5828 (0.0101)	0.5818 (0.0101)	0.6012 (0.0101)	0.5808 (0.0101)
<i>FEMALE</i>	0.6215 (0.0129)	0.6176 (0.0129)	0.6175 (0.0129)	0.6187 (0.0129)
<i>AGE</i>	0.0111 (0.0005)	0.0111 (0.0005)	0.0110 (0.0005)	0.0111 (0.0005)
<i>NO COLLEGE DEGREE</i>	0.3969 (0.0119)	0.3957 (0.0119)	0.3994 (0.0119)	0.3958 (0.0119)
Constant	-3.121 (0.0996)	-3.106 (0.0995)	-3.270 (0.0993)	-3.099 (0.0995)

Notes: Estimates of the likelihood that workers whose employers dissolved took their subsequent job in the same state as their prior employer. Observations are for 24 states and the District of Columbia (AR CO DC DE GA HI IL IN IA LA ME MD MO NV OK OR PA RI SC TN TX UT VT WA), removing the first observation in each state following a gap in the worker's employment record and the final observation in each state prior to a gap in the employment record. Census disclosure-avoidance rules mandate rounding of observations to the nearest 10,000 for samples greater than 100,000 and the truncation of reported statistics to four significant figures (not including leading zeroes).

Table 5: Effect of employee non-compete (NC) policy on firm failure

<i>DV = firm failure</i>	(1)	(2)	(3)	(4)
<i>TIGHTEN_NC</i>	0.0091 (0.0162)	0.0097 (0.0163)	0.0092 (0.0079)	0.0101 (0.0079)
<i>FIRM_AGE</i>	-0.0230 (0.0003)	-0.0215 (0.0003)	-0.0233 (0.0001)	-0.0218 (0.0001)
<i>FIRM_EMPLOYMENT</i>	-0.0052 (0.0008)	-0.0054 (0.0009)	-0.0062 (0.0004)	-0.0058 (0.0004)
Constant	-2.036 (0.2279)	-2.242 (0.3029)	-1.278 (0.1788)	-1.541 (0.1896)
Observations (rounded)	999,000	999,000	1,380,000	1,380,000
States	LEHD only	LEHD only	all LBD	all LBD
2-digit NAICS fixed effects	no	yes	no	yes

Notes: Estimates of the likelihood that firm dissolution is affected by state-level NC policy. Observations are a 10% random sample of firms in all U.S. states and the District of Columbia from 1976-2012.

Columns (1-2) further restricts the sample to states available to me in the LEHD (AR CO DC DE GA HI IL IN IA LA ME MD MO NV OK OR PA RI SC TN TX UT VT WA). In these columns, *TIGHTEN_NC* is a dummy variable set to 1 in state-years where NC policy had tightened within the previous two years (VT in 2005 and GA in 2010) and -1 where it had been loosened (SC in 2010). Columns (3-4) include the full LBD sample of U.S. states since 1976, adding the following state-level policy changes: MI (1985), FL (1996), LA (2001), OH (2004), OR(2008), ID (2008), WI (2009), CO (2011), IL (2011), TX (2011). Census disclosure-avoidance rules mandate rounding of observations to the nearest 10,000 for samples greater than 100,000 and to the nearest 1,000 for samples greater than 10,000 but smaller than 100,000 and the truncation of reported statistics to four significant figures (not including leading zeroes).

Table 6: Expected gender of defendants in employee non-compete lawsuits

occupation	# CNS defendants	% of CNS defendants	BLS estimates of % female in occupation	Expected percentage of female defendants	Non-compete usage by occupation (mean = 1)	Projected percentage of female defendants (adj.)
sales	727	34.7%	49.0%	17.0%	0.8	14.0%
manager	554	26.4%	39.1%	10.3%	1.6	17.0%
engineer	214	10.2%	14.2%	1.4%	1.9	2.8%
doctor	182	8.7%	38.2%	3.3%	1.0	3.4%
salon	124	5.9%	92.4%	5.5%	1.0	5.6%
broker	83	4.0%	52.4%	2.1%	1.2	2.6%
ceo	39	1.9%	27.3%	0.5%	1.6	0.8%
coach	34	1.6%	34.2%	0.6%	1.0	0.6%
real estate	37	1.8%	55.5%	1.0%	1.0	1.0%
construction	39	1.9%	3.0%	0.1%	0.6	0.0%
financial advisor	37	1.8%	52.5%	0.9%	1.2	1.1%
physical therapy	15	0.7%	69.3%	0.5%	1.4	0.7%
customer care	8	0.4%	65.0%	0.2%	1.0	0.3%
artist	3	0.1%	56.6%	0.1%	1.2	0.1%
TOTAL	2096			43.5%		50.2%

Notes: Table shows the occupations in which employee non-compete lawsuits are filed between 2003 and 2017. The number and percentage of defendants who are female is then shown for all lawsuits reported by the Courthouse News Service (CNS). The percentage of workers in each occupation that are female (from the Current Population Survey) is presented and multiplied by the percentage of lawsuits in each occupation and non-compete usage by occupation to predict the final column, expected percentage of female defendants in each occupation.

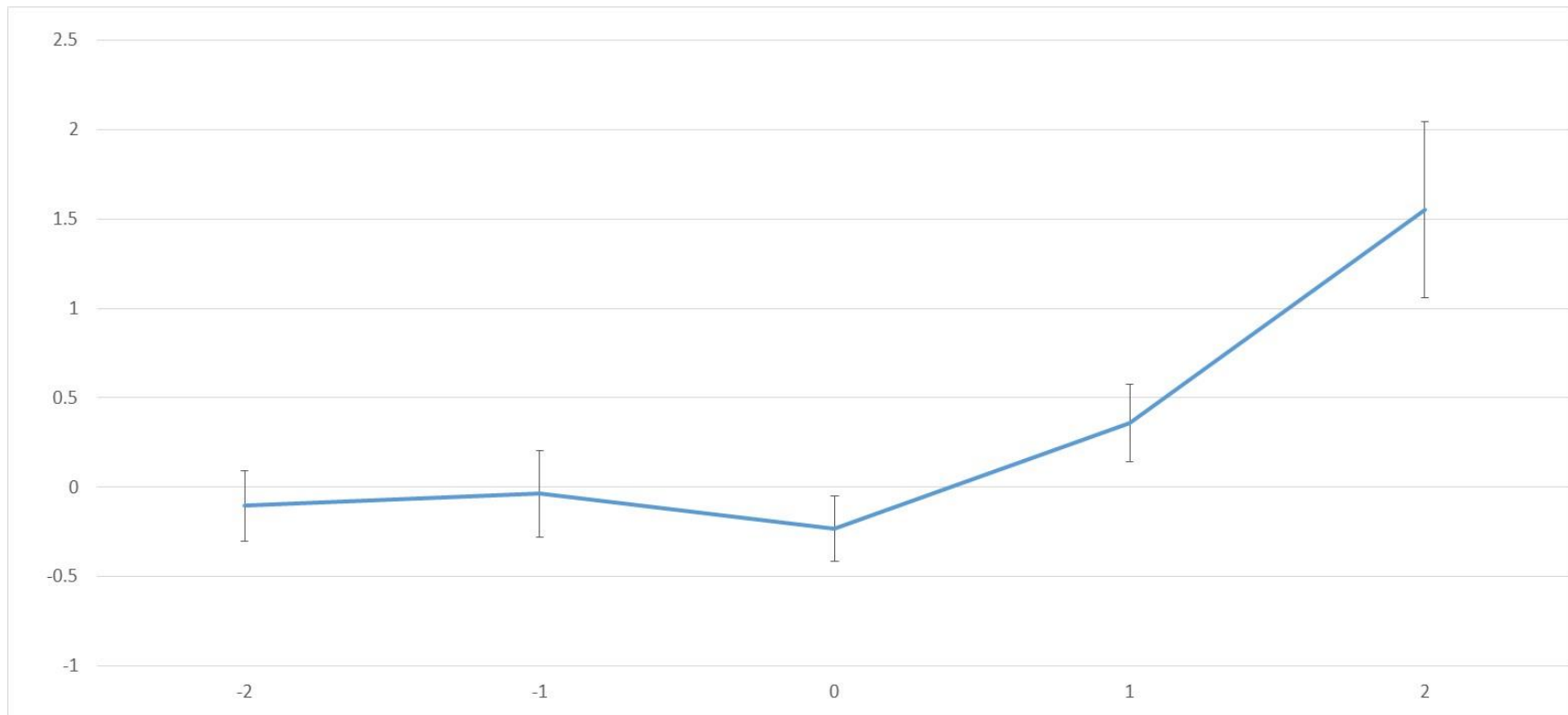
Table 7: Pre-entrepreneurial and post-entrepreneurial earnings

	<i>DV = cumulative earnings before first startup</i>		<i>DV = earnings ratio, post-startup vs. pre-startup</i>	
	(1)	(2)	(3)	(4)
<i>AGE</i>	0.0183 (0.0001)	0.0175 (0.0001)	-0.0105 (0.0007)	-0.0101 (0.0005)
<i>NO COLLEGE DEGREE</i>	-0.3990 (0.0026)	-0.3116 (0.0027)	-0.1275 (0.0185)	-0.0677 (0.0206)
<i>YEARS BEFORE FOUNDING FIRST STARTUP</i>	0.2392 (0.0003)	0.2376 (0.0003)	-0.0283 (0.0033)	-0.0296 (0.0036)
<i>FEMALE</i>	-0.2587 (0.0027)	-0.3406 (0.0029)	-0.0639 (0.0149)	-0.0876 (0.0145)
Constant	10.17 (0.0215)	10.27 (0.0245)	1.081 (0.1094)	0.9344 (0.0972)
6-digit NAICS fixed effects	no	yes	no	yes
Observations (rounded to nearest 1000)	345,000	345,000	100,000	100,000

Notes: Estimates of pre-entrepreneurship earnings (columns 1 & 2) as well as the ratio of post-entrepreneurship earnings vs. pre-entrepreneurship earnings (columns 3 & 4). Earnings are calculated as 1982 deflated wages as recorded in the LEHD. For columns (1-2), the dependent variable is the sum of all reported earnings (if any) prior to the worker founding her first entrepreneurial venture. For columns (3-4), the dependent variable is calculated as the worker's average annual earnings for all years prior to founding their first entrepreneurial venture divided by their annual earnings in the first year after abandoning their venture and returning to paid employment with an existing firm. Abandoning an entrepreneurial venture entails either a firm a worker founded, either after its demise or when it continues as a going concern (departing after an acquisition is not included, though in unreported results no statistically-significant differences in pre-vs-post wage rations are visible for men vs. women).

Observations are for 24 states and the District of Columbia (AR CO DC DE GA HI IL IN IA LA ME MD MO NV OK OR PA RI SC TN TX UT VT WA). Census disclosure-avoidance rules mandate rounding of observations to the nearest 10,000 for samples greater than 100,000 and to the nearest 1,000 for samples greater than 10,000 but smaller than 100,000 and the truncation of reported statistics to four significant figures (not including leading zeroes).

Figure 1: Pre-trends for the impact of non-compete (NC) policy on women engaging in entrepreneurship following employer dissolution



Notes: Observations are for 24 states and the District of Columbia (AR CO DC DE GA HI IL IN IA LA ME MD MO NV OK OR PA RI SC TN TX UT VT WA). Coefficients plotted are from an unreported regression similar to Table 2 but where *TIGHTENED_NC * FEMALE* is replaced by a series of dummies for the year of treatment and each of the two years surrounding the treatment year.