Great Recession Babies:

How Are Startups Shaped by Macro Conditions at Birth? *

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Abstract

We combine novel micro data with quasi-random timing of patent decisions over the business cycle to estimate the effects of the Great Recession on innovative startups. After purging ubiquitous selection biases and sorting effects, we find that recession startups experience better long-term outcomes in terms of employment and sales growth (both driven by lower mortality) and future inventiveness. While funding conditions cannot explain differences in outcomes, a labor market channel can: recession startups are better able to retain their founding inventors and build productive R&D teams around them.

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Recessions are frequently viewed as a time when creative companies are born, aptly captured in the aphorism "necessity is the mother of innovation." Indeed, the list of prominent startups that began life in a recession is long. Well known examples from the Great Recession of 2007 to 2009 include GitHub (founded in February 2008), airbnb (August 2008), Pinterest (October 2008), Slack (January 2009), WhatsApp (February 2009), and Uber (March 2009). Yet the list of prominent startups born in an expansion is long too, and most likely longer. Are recessions a good time or a bad time to start a business? The answer is not obvious. A priori, recessions could either hinder or help startups. For example, a contraction in funding availability may make it more difficult for a startup to get off the ground in a recession, while a reduction in competition for critical inputs such as skilled labor may make it easier.

To identify the economic effects of the Great Recession on U.S. startups, we combine novel data with an identification strategy that helps disentangle the causal effects of the recession from the selection effects that arise as potential founders who differ in their entrepreneurial skills and the quality of their ideas endogenously choose whether and when to start a business. Firms that begin life in a recession are likely systematically different from firms that begin life at other times. For example, if recessions are a challenging time to raise funding, we expect startups that nonetheless choose to get going in a recession to be of higher average quality, resulting in selection bias. Of course, the bias need not be positive: if individuals with low skills are more likely to become entrepreneurs after losing their jobs in a recession (Evans and Leighton 1990), recession startups may instead be of lower average quality (Ghatak, Morelli, and Sjöström 2007). Moreover, if the sensitivity to macro conditions at birth varies across startups, we expect sorting effects (Heckman 2001), such that it is startups whose prospects are more affected by recession that will tend to wait for a recovery before starting operations.

The ideal experiment is clearly not feasible: we cannot randomize when startups are born. To get as close to random assignment as possible, we narrow our focus to innovative startups, specifically, to startups that are founded to exploit a technological innovation that can be protected by a patent. Innovative startups are considered to play an outsize role in productivity growth and economic welfare (Acemoglu et al. 2018). Narrowing the focus to innovative startups allows us to exploit two lottery-like features of the patent examination process at the U.S. Patent and Trademark Office (PTO) to quasi-randomize whether an innovative startup receives its first patent during or outside a recession. First, the PTO assigns applications in each technology field to patent examiners randomly with respect to the characteristics of the applicant, the application, and the underlying invention (Lemley and Sampat 2012). Second, examiners in a given technology field vary systematically in their review speeds (Hegde, Ljungqvist, and Raj 2022). Combined with multi-year waits for a decision as a result of a backlog exceeding half a million unexamined applications each year, these two features of the PTO's examination process quasi-randomize the timing of a startup's patent decision relative to the business cycle.

To illustrate our empirical design and the causal inference it permits, consider the following stylized example. At $t = 0$, an inventor applies for a patent, not knowing that in year $t = 2$, a recession will occur. There are three types of patent examiners: those who take 1, 2, or 3 years to issue a decision. If the application is randomly assigned to a type 1 or type 3 examiner, the patent will randomly issue in the year before or after the recession. If the inventor randomly draws a type 2 examiner, the patent will randomly issue in the recession. Random assignment of applications to examiners who differ in their review speed thus ensures that the time at which the inventor is granted the patent is random with respect to future realizations of stochastic business-cycle conditions.

Our empirical design compares startups that randomly receive a patent in a recession to those (in the same technology field applying in the same year) that randomly receive a patent at other times. The resulting estimates are intention-to-treat (ITT) effects because receiving a patent in a recession does not oblige the startup to commercialize its invention then. In the language of randomized control trials, the patent grant is an "invitation" to be treated (i.e., to begin life in the recession). There are two forms of endogenous non-compliance. First, startups can decline the invitation (i.e., defer the start of operations until the economy recovers). This group of "never-takers" begins life in an expansion regardless of when the patent is granted. Second, startups can opt into treatment absent an invitation. Such "always-takers" are often viewed as forced entrepreneurs: they start operations in the recession whether or not their patent is granted then. Empirically, we estimate that both groups are present and sizeable, with never-takers and always-takers accounting for 54.3% and 20.1% of our sample, respectively.

ITT effects have a causal interpretation as long as the invitation to treatment is randomly assigned (Angrist and Pischke 2009). We show that this condition plausibly holds in our setting, given that we exploit exogenous variation in examiner review speed in combination with when a future recession occurs (something that is difficult to predict years ahead). ITT effects are a lower bound on the causal effects of the recession on innovative startups. Much of the evidence we report is in the form of ITT effects. If we are willing to make additional identifying assumptions (discussed in Section 1.2.3), we can use the randomly assigned invitation to treatment as an instrument for being born in a recession, which allows us to estimate the causal effect of the recession on compliers (the local average treatment effect or LATE).

We utilize a rich data set that combines administrative data from the PTO's internal databases with data on four types of firm-level outcomes: (a) startup survival, sales growth, and employment growth; (b) follow-on innovation and patent originality; (c) fundraising through private placements of equity or debt securities under Regulation D, venture capital raises, loans secured against a patent, patent sales, or initial public offerings on a stock market; and (d) the mobility and productivity of founding inventors and new R&D personnel. Our sample consists of 6,946 startups that file their first successful patent application between 2002 and 2009 and receive a decision on their application by 2012. We track these startups through 2019.

Naïve OLS estimates show that compared to expansion startups, recession startups experience marginally faster employment and sales growth over 1 to 3 years, with no difference in long-run growth over 5 to 7 years. These estimates could over- or underestimate the causal effects of the Great Recession on startups, and even the positive sign may not be right, though it turns out to be: the ITT effects reveal that the Great Recession has large positive effects on innovative startups in the long-run (though not in the short-run). We find that a startup invited to be born in the Great Recession is 12.1% more likely to survive to its seventh anniversary than the average startup invited to be born at another time in the 2002-2012 window. Over its first 7 years of operations, the average recession startup grows its employment and sales by a cumulative 35.2 and 35.7 percentage points faster, respectively, than the average expansion startup. Contrary to the idea that recessions spawn superstar firms, we find (using quantile regressions estimated in two-percentile increments) that the growth-boosting effect of the Great Recession decreases monotonically across the growth distribution, with top-decile recession startups experiencing no significant difference in growth rates over 7 years.

As noted, owing to non-compliance, our ITT estimates are lower bounds on the causal effect on the treated (the LATE). Exploiting random assignment of patent grants over the business cycle, we estimate that the LATE is considerably larger, with a 31.1 percentage-point increase in the seven-year survival rate, an 82.8 percentage-point difference in the cumulative employment growth rate over 7 years, and a 90.4 percentage-point difference in the cumulative sales growth rate over 7 years. These growth boosts are driven by the difference in survival rates: conditional on survival, the Great Recession has no effect on startup growth.

Besides survival and growth, we also study inventiveness. While the Great Recession has no effect on the quantity of follow-on innovation startups produce after their first patent, it does positively affect a measure of the originality and hence likely economic value of their follow-on innovation: its "breakthroughness" (Kelly et al. 2021).[1] To illustrate, the average recession startup produces follow-on patents whose breakthroughness rank is 16.5 percentiles higher than that of startups born at other times, and 19.1 percentiles higher conditional on survival.

We investigate two channels through which a recession can affect a startup's development: a funding channel and a labor-market channel. Prior work shows that the supply of funding to startups becomes tighter in a recession.[2] According to received wisdom, startups may then struggle to survive, and if they do survive, they may struggle to invest in the foundations of their long-term growth, be it personnel, product development, or customer acquisition. While we see some evidence of short-term financial stress, in that recession startups do not pay their suppliers as promptly in their first year of operation, we find no evidence that the Great Recession has adverse transitory or permanent effects on funding. On the contrary, the recession produces more startups that eventually list on the stock market (a milestone the entrepreneurial finance literature views as a marker of success). This finding sits well with our baseline results, insofar as more recession startups survive to grow to a sufficient size to satisfy listing requirements.

The labor-market channel explains the positive long-run effects of the Great Recession well. We find robust evidence that recession startups are significantly better able to retain their founding inventors, especially over the short- to medium-term. For example, the unconditional likelihood of one or more founding inventors leaving within 3 years of patent grant is 43%; among recession startups, it is as much as 25 percentage points lower. We conjecture that the markedly higher retention rate among recession startups reflects reduced labor mobility at a time when incumbents reduced or ceased hiring during the Great Recession.[3] Using variation in

[1]Breakthrough patents are identified based on the textual similarity to previous and subsequent patents. A breakthrough patent has a low textual similarity with previous patents and a high textual similarity with subsequent patents.

[2]Between 2007 and 2009, VCs reduced their funding of startups by 27.2% (see `https://nvca.org/recommends/111997-2/`). Housing collateral, often viewed as a key source of funding for small firms (Adelino, Schoar, and Severino 2015), declined in value by around 10% (Mian and Sufi 2014).

[3]Consistent with this conjecture, we document that labor mobility among inventors in the U.S. economy

labor-market demand for R&D workers in a startup's technology field as an instrument for its founding-inventor retention rate, we show that greater retention early in a startup's life predicts performance later in its life. We also find (statistically more marginal) evidence that recession startups grow their R&D teams faster and that they hire more productive R&D workers, perhaps because they can take advantage of reduced demand for R&D workers elsewhere in the economy, or perhaps because retaining founding inventors with a record of winning at least one patent makes them a more attractive place for external hires to join. Better retention, larger R&D teams, and higher R&D productivity in turn help explain why recession startups produce more impactful follow-on innovations, survive, and manage to list on the stock market.

Our study contributes to the literatures on business cycles, innovation, and entrepreneurial finance. Much prior work considers startup growth to be procyclical, due to either a funding channel, a labor channel, or a demand channel. Recessions are characterized by reduced venture funding (Nanda and Rhodes-Kropf 2013) and by tighter lending, especially to small, opaque, and risky firms (Bernanke, Gertler, and Gilchrist 1996) and to entrepreneurs relying on their housing wealth as collateral (Schmalz, Sraer, and Thesmar 2017). Innovative startups such as the ones we focus on tend to be particularly adversely affected by funding contractions.[4] The labor market can induce procyclicality if the quality pool of entrepreneurs worsens in a recession as low-skill workers lose their jobs and become self-employed (Ghatak, Morelli, and Sjöström 2007), or if risk-averse would-be founders are less willing to take on startup risk in a recession (Rampini 2004).[5] Procyclical changes in aggregate demand can permanently affect a startup's ability to grow (Moreira 2016), for example if being born in a recession leads firms to choose a niche rather than mass product as in Sedláček and Sterk's (2017) model calibration.

We contribute to this literature by providing (arguably causal) micro evidence that the Great Recession had a positive and therefore counter-cyclical effect on the growth of innovative

declined sharply during the recession, from around 0.7% a month in 2006 to around 0.5% a month in 2009.

[4]Howell et al. (2020) show that venture funding is procyclical, resulting in lower quality innovation in recessions. Our design holds quality constant. Bernstein, McQuade, and Townsend (2021) show that recessions lower inventors' productivity as their housing wealth declines. Albert and Caggese (2020) show that funding constraints during a financial crisis have a more negative effect on high-growth than low-growth startups. Granja and Moreira (2022) show that lower credit supply during the Great Recession constrained the ability of firms in the consumer sector to introduce product innovations. Babina, Bernstein, and Mezzanotti (2022) show that reduced credit supply during the Great Depression of the 1930s decreased innovation by independent inventors.

[5]In Rampini's (2004) model of occupational choice, the less risk averse become entrepreneurs and the more risk averse seek salaried employment. Wealth effects make risk aversion counter-cyclical such that entrepreneurial activity increases in expansions. Relatedly, Bernstein, Townsend, and Xu (2020) show empirically that high-quality job-seekers favor incumbents over startups in a recession.

startups that is driven entirely by lower startup mortality linked to an improved ability to retain founding inventors and attract more productive R&D workers. We find no evidence of financial "scarring": innovative startups born in the Great Recession face no worse funding conditions going forward than their (only randomly different) expansion peers. Prior evidence of recession-induced funding constraints, and the negative firm-level consequences they lead to, may thus not generalize to our research design and/or the innovative startups we focus on.

Our finding that innovative startups benefit from getting their start in the Great Recession tallies well with Hacamo and Kleiner (2022), who show that firms founded by students who graduate from college during periods of high unemployment are more likely to survive, innovate, and receive venture backing. In their occupational-choice model, this corresponds to a positive selection effect.[6] While Hacamo and Kleiner do not use the term, they too estimate intention-to-treat effects.[7] We go two steps further, estimating local average treatment effects and using an Angrist-Pischke (2009) decomposition to show that sorting into and out of treatment coexist. Specifically, we show that 15.9% of sample startups endogenously opt to be born in the recession, while 11.4% opt to wait for a recovery. Based on observables, startups that sort into the recession look strong on average, suggesting they may not be founded by forced entrepreneurs.

Finally, we contribute to the literature on the growth-boosting effects of patents. Farre-Mensa, Hegde, and Ljungqvist (2020) provide causal evidence that receiving a legal property right over an invention enables startups to grow employment and sales substantially faster, holding constant the economic benefits startups derive from the underlying invention. In our setting, all sample startups receive a patent. The question we consider is thus not whether but when over the business cycle sample startups receive their first patent. Our focus on this intensive margin allows us to examine how the growth boost Farre-Mensa, Hegde, and Ljungqvist document varies over the business cycle. In so doing, we provide nuance to Hegde, Ljungqvist, and Raj's (2022) finding that patent grant delays harm startup growth: a fast examiner may cause a startup to be born at an inopportune time in the business cycle, while a slow examiner may cause the startup to be born at a propitious time.

[6]Other empirical studies consistent with positive selection effects include Babina (2020), who shows that financial distress at incumbent firms induces higher-quality employees to leave to set up better firms than typical entrepreneurs, and Ates and Saffie (2021), who show that positive selection by lenders resulted in fewer but higher quality firms being born in Chile's financial crisis of 1998.

[7]Their estimates are ITT because a high unemployment rate at graduation only serves as an exogenously assigned invitation to entrepreneurship—an invitation some graduates will endogenously non-comply with (for example, by going to graduate school, taking a gap year, or choosing the relative safety of a government job).

1. Empirical Design

1.1. Identification Challenge

We are interested in the effects of being born in the Great Recession on firm-level outcomes such as survival, growth, and future inventiveness. We use a potential-outcomes framework to formalize our empirical design. Let $D_i = \mathbb{1}(Recession)_i$ be an indicator set equal to 1 if startup i is born in the recession and 0 otherwise. Denote by Y_{1i} startup i's outcome if $D_i = 1$ and by Y_{0i} its outcome if $D_i = 0$. Only one of these potential outcomes is observed. Write startup i's observed outcome as $Y_i = Y_{0i} + (Y_{1i} - Y_{0i})D_i$. The difference in potential outcomes, $Y_{1i} - Y_{0i}$, is the causal effect of the recession on startup i. Next, consider the following regression:

$$Y_i = E(Y_{0i}) + (Y_{1i} - Y_{0i})D_i + (Y_{0i} - E(Y_{0i})) = \alpha + \beta D_i + \eta_i \tag{1}$$

where we ignore covariates to simplify the exposition and assume, for now, that the recession has a homogeneous effect on all startups: $Y_{1i} - Y_{0i} = \beta$. Estimating equation (1) by OLS yields $\beta_{OLS} = E[Y_i|D_i = 1] - E[Y_i|D_i = 0]$, i.e., the observed difference in average outcomes between startups born in the recession and startups born at other times. It is easy to show that β_{OLS} equals the average treatment effect of interest plus a selection bias: $\beta_{OLS} = \beta_{ATE} + (E[\eta_i|D_i = 1] - E[\eta_i|D_i = 0])$. The selection bias will be non-zero if startups born in the recession and startups born at other times face different potential outcomes absent the recession. In our setting, selection bias would be positive if, for example, only startups of above-average quality could raise funding in a recession. It would be negative if, for example, below-average workers who lost their jobs chose to become entrepreneurs in larger numbers in a recession.

1.2. Identification Strategy

To deal with selection biases, we combine two independent sources of random variation. The first builds on three institutional features of the PTO's patent examination process: (i) the PTO assigns patent applications quasi-randomly to examiners,[8] (ii) examiners differ systematically in their review speeds (Hegde, Ljungqvist, and Raj 2022), and (iii) the PTO has a substantial

[8] See Cockburn, Kortum, and Stern (2002), Lichtman (2004), Sampat and Lemley (2010), Lemley and Sampat (2012), Gaulé (2018), Sampat and Williams (2019), Farre-Mensa, Hegde, and Ljungqvist (2020), and Hegde, Ljungqvist, and Raj (2022), among others.

backlog of applications that results in multi-year waits for a decision on an application.[9] The second comes in the form of the stochastic arrival of a future recession. Combining these two independent sources of random variation with technology-field-by-application-year fixed effects ensures that startups in the same technology field that apply for a patent at the same time will not differ systematically whether their patent is issued in a future recession or a future expansion.

Formally, let $Z_{1,i} = 1$ if startup i receives a positive decision on its first patent application during the recession, and zero otherwise. Write startup i's observed treatment status as $D_i = D_{0i} + (D_{1i} - D_{0i})Z_{1,i}$. We next discuss two properties of Z_1 that are essential to our ability to identify the effect of D on Y.

1.2.1. Non-Compliance and Invitation to Treatment

Receiving a positive decision on a patent application in a recession, $Z_{1,i} = 1$, does not guarantee that the startup will be born in the recession. Startups can choose not to comply with the assignment to treatment, resulting in heterogeneous treatment effects for compliers (those for which $D_i = 1$ if $Z_{1,i} = 1$ and $D_i = 0$ if $Z_{1,i} = 0$), always-takers ($D_i = 1$ whether $Z_{1,i} = 1$ or $Z_{1,i} = 0$), and never-takers ($D_i = 0$ whether $Z_{1,i} = 1$ or $Z_{1,i} = 0$). Always-takers start operations in a recession regardless of whether they have received a patent, perhaps because they are forced entrepreneurs. We think of never-takers as startups that sort into the recovery: if they have received a patent during the recession, they delay the start of their operations until the recovery. The causal treatment effect of interest is the local average treatment effect on compliers, $\beta_{LATE} = E[Y_i^c|D_i = 1, Z_{1,i} = 1] - E[Y_i^c|D_i = 0, Z_{1,i} = 0]$.[10]

Given endogenous non-compliance, $Z_{1,i} = 1$ should be viewed as an *invitation* to be treated (i.e., to be born in the recession). As long as the invitation is randomly assigned, we can

[9] An application spends much of this multi-year wait unexamined in the examiner's queue. While PTO examiners are provided incentives to handle applications in date-order priority, they also have conflicting incentives to meet production quotas (see Hegde, Ljungqvist, and Raj 2021 for further discussion). Actual examination time varies by technology field and examiner seniority but is comparatively short, averaging 23 hours in 2009 (Marco et al. 2017).

[10] With heterogeneous treatments, OLS estimates $\beta_{OLS} = \beta_{LATE} + \pi^{at}_{D_i=1}(E[Y_i^{at}|D_i = 1] - E[Y_i^c|D_i = 1, Z_{1,i} = 1]) + \pi^{nt}_{D_i=0}(E[Y_i^{nt}|D_i = 0] - E[Y_i^c|D_i = 0, Z_{1,i} = 0])$, where $\pi^{at}_{D_i=1}$ and $\pi^{nt}_{D_i=0}$ are the shares of always-takers among the treated and of never-takers among the non-treated, respectively. Thus, the bias in OLS is a function of how much better or worse always-takers do compared to compliers under the treatment and of how much better or worse never-takers do compared to compliers absent treatment. Whether OLS over- or underestimates the LATE is thus an empirical question: $\beta_{OLS} - \beta_{LATE}$ cannot be signed a priori unless one can rule out either always-takers or never-takers.

estimate an intention-to-treat (ITT) effect by regressing Y on Z_1,

$$Y_i = \kappa + \delta_{ITT} Z_{1,i} + \epsilon_i \tag{2}$$

where the ITT effect δ_{ITT} equals $E[Y_i|Z_{1,i} = 1] - E[Y_i|Z_{1,i} = 0]$, i.e., the difference in average observed outcomes among those invited to be treated and those not invited. The ITT effect has three desirable properties: it has a causal interpretation, assuming nothing more than that Z_1 is randomly assigned (Angrist and Pischke 2009, p. 163); it has the same sign as the local average treatment effect, enabling us to sign the effect of the Great Recession on compliant startups with much milder identifying assumptions (i.e., random assignment); and it is a conservative lower bound on the LATE, as intention-to-treat ignores the fact that those who would benefit the least from treatment (or be harmed the most by it) will endogenously non-comply.[11]

1.2.2. Is Z_1 As Good As Randomly Assigned?

Recall that we exploit a double randomization: random assignment to examiners who differ in their review speed and the random arrival of a future recession. The main potential violation of double randomization would be if examiners selectively adjusted their review speed based on application or applicant characteristics once the macroeconomic state of the world is realized, such that certain types of applications are more likely to be reviewed in a recession. If so, Z_1 would not be as good as randomly assigned and equation (2) would not identify the causal intention-to-treat effect δ_{ITT}.

There are two potential ways in which Z_1 could fail to be randomly assigned. The first is that certain types of applicants "lobby" their examiner to conclude the examination of their application more speedily in a recession (perhaps in the hope of increasing their chances of receiving funding in an otherwise tough market). The PTO's review process effectively rules out such lobbying: until the examiner issues a decision, her identity is unknown to the applicant.[12]

[11] With full compliance, $D_i = Z_{1,i}$ for all i and δ_{ITT} thus equals the local average treatment effect β_{LATE}. With non-compliance, $\delta_{ITT} < \beta_{LATE}$.

[12] At various points in time, the PTO has offered accelerated-review programs that were open to a small and narrowly drawn set of applicants satisfying strict eligibility criteria. Importantly, applicants could not petition for accelerated review post-application. We can therefore rule out that startups selectively sought to influence the timing of their patent review post-application as the economy slowed down or entered recession. Startups that filed their patent application with a petition for accelerated review during the recession can be viewed as always-takers (they wish to be born in the recession) and so do not pose a challenge to our empirical design.

Hence, only actions taken by the examiner can affect the timing of the decision relative to the state of the business cycle. Suppose some examiners prioritize applicants of below-average quality in a recession.[13] If so, the pool of startups receiving a positive decision on their patent application in a recession would be skewed towards below-average-quality firms, resulting in equation (2) estimating a downward-biased ITT effect. In Section 3.2, we report evidence consistent with weaker applicants receiving time-priority during the Great Recession.

To fix this problem, we instrument Z_1 by predicting whether or not each startup's patent decision is issued in the recession based on the sum of the application date, the docket time lag (the application-specific administrative lag from the time the application is filed to the time it is docketed with an examiner), and the examiner's average historic review speed:

$$\widehat{t}_{decision_i} = t_{application_i} + t_{docket-time-lag_i} + \bar{t}_{review-speed_{ij}}. \tag{3}$$

where i indexes startups as before and j indexes examiners. The resulting instrument, which we denote Z_2, equals 1 if the predicted decision date coincides with the Great Recession, and 0 otherwise:

$$Z_{2,i} = \begin{cases} 1 \text{ if Dec 1, 2007} \leq \widehat{t}_{decision_i} \leq \text{June 30, 2009}, \\ 0 \text{ otherwise.} \end{cases} \tag{4}$$

As we will see, Z_2 turns out to be a strong instrument for Z_1, allowing us to correct potential biases induced by examiner-induced departures from time-priority by estimating

$$Y_i = \mu + \delta_{ITT} \hat{Z}_{1,i} + \psi_i \tag{5}$$

where we instrument Z_1 using Z_2. We refer to δ_{ITT} in equation (5) as the bias-corrected intention-to-treat effect.

1.2.3. Local Average Treatment Effects

Much of our evidence is in the form of bias-corrected ITT effects. If we are willing to make additional identifying assumptions, we can use the randomly assigned invitation to be treated,

[13] We stress that such behavior would not reflect policy: the PTO is supposed to be "fair," that is, blind with respect to applicant characteristics.

Z, as an instrument for D. As Angrist and Pischke (2009, Section 4.4.3) show, the causal effect of the recession on compliers (the LATE) can be consistently estimated if we instrument the endogenously selected treatment, D, with a randomly assigned invitation to treatment that satisfies the relevance condition, the monotonicity condition, and the exclusion restriction. If these three additional identifying assumptions hold, $\beta_{LATE} = \beta_{IV} = \delta_{ITT}/\gamma$, where γ is the first-stage coefficient on the instrument (or equivalently, the share of compliers).

The relevance condition requires that there are enough compliers (i.e., that enough startups start operations once they receive a patent), or equally, that the first-stage regression of D on Z is significant. Whether the relevance condition holds is an empirical question.

The monotonicity condition assumes that there are no "defiers." In our case, violations of the monotonicity condition require that a startup systematically and consistently chooses to defy treatment, by *only* starting operations in a recession if it received its patent in an expansion and by *only* starting operations in an expansion if it received its patent in a recession. Such behavior seems (to us) unlikely. As we report later, a Dobbie, Goldin, and Yang (2018) test fails to detect violations of the monotonicity condition in our setting.

The exclusion restriction requires that the instrument only affect outcomes through its effect on when a firm is born and not directly or through another channel. As we report later, a Angrist, Lavy, and Schlosser (2010) test fails to detect violations of the exclusion restriction in our setting. For now, we discuss the exclusion restriction in light of our empirical strategy.

A priori, double randomization goes a long way towards a plausible exclusion restriction. To reiterate, the first randomization takes the form of quasi-random assignment of patent applications to examiners who differ in their predicted review speeds (but whose identity is only revealed when the examiner issues a decision on the patent application). The second randomization takes the form of the stochastic nature of the business cycle: given multi-year review lags at the patent office, startups cannot plausibly know, at the time of application, in what stage of the business cycle they might eventually be granted a patent.[14]

Double randomization makes it difficult to see how a randomly assigned invitation to be treated in a random future recession rather than a random future expansion would affect

[14]Moreover, given that the examiner's identity is unknown until the examiner issues a decision and given that review times are highly dispersed (see Figure IA.1 in the Internet Appendix), expectations of what macroeconomic conditions might prevail when a startup eventually receives news of its patent are surely very noisy.

the startup's future outcomes directly rather than through the difficult-to-predict prevailing macroeconomic conditions at the future time the invitation is received. Similarly, double randomization makes it difficult to see how startups that will receive their patent news in a future recession might today take unobserved actions that would cause them to differ systematically from startups that will receive their patent news in a future expansion.

1.2.4. Disentangling the Effects of Recessions and Patent Review Delays

Hegde, Ljungqvist, and Raj (2022) use random assignment to fast and slow examiners to show that patent review delays harm a startup's growth prospects. As equation (4) makes clear, our empirical design differs from theirs in that it combines exogenous variation in review speed across randomly assigned patent examiners with when a future recession occurs. As a result, review speed does not have a monotonic effect on treatment in our setting: depending on the patent application date, a startup can be born in the Great Recession as a result of its application having been assigned to either an ex ante fast or an ex ante slow examiner. There is thus no reason to expect that our results are confounded by either review speed or any other examiner habit that correlates with review speed.[15] The following stylized example illustrates why our results are robust.

Suppose patents are randomly assigned to three types of examiners: slow (with a review time of 3 years), average (2 years), and fast (1 year). A slow review has a negative effect on outcome Y of $-\lambda$, while a fast review has a positive effect of $+\lambda$. (Symmetry is without loss of generality.) The recession takes place in year t. The causal effect of the recession on outcomes is β. The table below illustrates how variation in review speed assigns startups to the recession:

Application year	Slow examiner	Average examiner	Fast examiner
$t-3$	$\mathbb{1}(Recession) = 1$	$\mathbb{1}(Recession) = 0$	$\mathbb{1}(Recession) = 0$
$t-2$	$\mathbb{1}(Recession) = 0$	$\mathbb{1}(Recession) = 1$	$\mathbb{1}(Recession) = 0$
$t-1$	$\mathbb{1}(Recession) = 0$	$\mathbb{1}(Recession) = 0$	$\mathbb{1}(Recession) = 1$

Abstracting (without loss of generality) from selection effects, OLS estimates $E[Y_i|D_i =$

[15] As a practical matter, our results are virtually unchanged when we allow for review delays, suitably identified, to directly affect startup growth as in Hegde, Ljungqvist, and Raj (2022). The same is true for other examiner habits, including scope leniency (the tendency for an examiner to grant broad rather than narrow patents).

$1] - E[Y_i|D_i = 0]$. Consider application year $t-1$. Applications randomly assigned to fast examiners are assigned to the recession (with effect on outcome Y of β) and benefit from a fast review ($+\lambda$). Hence, $E[Y_i|D_i = 1] = \beta + \lambda$. Applications randomly assigned to slow and average examiners are assigned to the expansion, with the former suffering from a slow review ($-\lambda$): $E[Y_i|D_i = 0] = -0.5\lambda$. Thus, $E[Y_i|D_i = 1] - E[Y_i|D_i = 0] = \beta + 1.5\lambda$. And similarly for application years $t-2$ and $t-3$. The next table summarizes these effects:

Application year	Estimated recession effect
$t-3$	$\beta + (-\lambda) - 0.5\lambda = \boldsymbol{\beta - 1.5\lambda}$
$t-2$	$-(-0.5\lambda) + \beta - 0.5\lambda = \boldsymbol{\beta}$
$t-1$	$-(-0.5\lambda) + \beta + \lambda = \boldsymbol{\beta + 1.5\lambda}$
Combined	$\frac{1}{3}(\beta - 1.5\lambda) + \frac{1}{3}\beta + \frac{1}{3}(\beta + 1.5\lambda) = \boldsymbol{\beta}$

As long as the number of applications and the distribution of slow, average, and fast examiners are (fairly) stable over time, the effects of fast and slow review speed cancel out, leaving the true effect of the recession, β, identified as the coefficient on D in a regression of Y on D.[16] This is shown in the final row marked "Combined."

1.3. Empirical Implementation

We follow Farre-Mensa, Hegde, and Ljungqvist (2020) and Hegde, Ljungqvist, and Raj (2022) in taking as the examiner's first positive decision on a startup's patent application the "first office action on the merits" decision rather than the eventual patent grant. The first-action decision is the examiner's preliminary ruling on the application. While the predicted timing of the first-action decision relative to the business cycle in equation (4) is a function of the randomly assigned examiner's review speed, the timing of the final grant is almost surely endogenous: the delay between first-action and final decision is determined, in large part, by how long the applicant takes to respond to concerns the examiner raises at first-action, which in turn depends on the applicant's resources and the economic benefits it expects to derive from the patent. Farre-Mensa, Hegde, and Ljungqvist find that first-action decisions are

[16] As an empirical matter, these conditions hold in our sample. Figure IA.2 in the Internet Appendix confirms that review times are indeed fairly stable within technology field over time, varying in a one-quarter range for applications submitted between 2002 and 2009. The number of applications is fairly constant in 2002-2007 and increases by less than 10% in 2008 and 2009.

highly predictive of final patent grants and thereby resolve much of the uncertainty about the patentability of an invention. They could thus plausibly trigger a startup to start operations, as required for a significant first-stage.

As Lemley and Sampat (2012) argue, assignments of applications to examiners are only random conditional on technology field and application year. To capture this, we follow prior work and include art unit by application year fixed effects.[17] Their inclusion controls for time-varying demand and technology-related shocks within each narrowly defined technology field that could affect both the processing of patent applications and firm outcomes. In addition, we include headquarter-state fixed effects to control for geographical differences in conditions that could affect outcomes (say, a greater availability of venture funding in California).

We follow the NBER's Business Cycle Dating Committee and consider the Great Recession to have started on December 1, 2007 and to have ended on June 30, 2009. In a robustness test, we allow for differential effects in the slowdown (the four quarters before the recession) and the recovery (the four quarters after the recession).

We consider both the short-term and the long-term effects of the recession by measuring outcomes Y over windows extending from 1 to 7 years.

To allow for common shocks affecting startups in a given technology field, we cluster the standard errors at the art unit level.

2. Sample and Data

2.1. Outcome Data

Being privately held, the startups in our sample are not covered in standard financial databases such as Compustat. Our principal source of data on firm outcomes is the National Establishment Times Series (NETS) database, from which we obtain data on survival and growth in employment and in sales. NETS, which is assembled by Walls & Associates from archival Dun & Bradstreet data, is similar to the U.S. Census Bureau's Longitudinal Business Database (LBD) in that it aims to cover the universe of business establishments in the U.S.

[17] An art unit is an administrative unit at the PTO consisting of patent examiners who specialize in a narrowly defined technology field, such as "liquid crystal cells, elements, and systems" (art unit 2871). There are over 900 art units at the PTO.

Unlike the LBD, NETS does not require special permission for access. We use the 2020 version of NETS, which covers 78 million establishments in the U.S. between 1990 and 2019.

Absent common identifiers, linking patent assignees to NETS (and to other databases) requires matching on firm names and locations. A key practical problem is that many startups change their names (and some move locations) over time. To help us address this problem, Walls & Associates have provided us with a non-public file containing historic time series of business names, trade names, and locations for each establishment in NETS.[18] After standardizing names and locations, our record linkage approach uses exact and tf-idf matching of names within geographic blocks composed of counties and states. We are able to match 89.1% of all patents granted between 1989 and 2016 to firms in NETS—a substantially higher match rate than that achieved by studies using the Census Bureau's data.[19]

We supplement the NETS data with data on (i) follow-on patents and citations (obtained from the PTO's PatentsView database), (ii) a measure of breakthrough patents constructed as in Kelly et al. (2021), (iii) data on various forms of funding, including private placements of debt or equity under Regulation D (from the SEC's EDGAR service), venture capital (from Thomson Reuters VentureXpert), the use of patents as collateral or their sale (from the USPTO Patent Assignment database), and IPOs (from Thomson Reuters SDC), (iv) the labor-market mobility of inventors (following the approach of Marx, Strumsky, and Fleming 2009), and (v) inventor productivity (constructed using data from the PTO's PatentsView database).

2.2. Sample Construction

We construct our sample of innovative startups as follows. Our starting point is the set of 23,088 distinct NETS firms (using HQ DUNS) that file their first patent application between 2002 (the first year after the 2001 recession) and 2009 (the ending year of the Great Recession) and that receive their first-action decision no later than 2012 (allowing us to track outcomes for the next 7 years in the current release of the NETS database). We then drop patent assignees that are universities, hospitals, associations, or foundations and firms that are spin-offs from

[18] We are grateful to Don Walls for granting access to this file.

[19] Balasubramanian and Sivadasan (2011) are able to match 63.7% of patent assignees to firm names in the Census Bureau's Business Register, often considered the "gold standard" for its coverage of the entire population of U.S. business establishments with paid employees filing taxes with the Internal Revenue Service. Kerr and Fu (2008) report a match rate of about 70%.

established companies.[20] Not all of the 17,269 NETS firms that remain after these filters are startups, as some file their first patent application in "old age." To screen out "old" firms, we limit our sample to the 6,946 startups that are at most 5 years old at the time of grant.[21]

2.3. Summary Statistics

Of the 6,946 startups in our sample, 17% receive their first-action decision on their first patent application during the Great Recession. Figure 1 graphs, for each application year between 2002 and 2009, the number of sample startups receiving a first-action decision before, during, or after the recession. The annual number of applications is fairly constant in 2002-2007, averaging 868 a year, and increases to 935 in 2008 and 1,032 in 2009. Reflecting multi-year delays at the PTO, applications that receive a first-action decision during the recession were, in the main, filed years earlier. For example, 24.3% of the 814 applications filed in 2005 and 51.5% of the 839 applications filed in 2006 received a first-action decision in the recession.

Table 1 compares a variety of observable characteristics (measured at birth and holding art unit and application year constant) between startups that are born in the Great Recession ($D = 1$) and those born at other times ($D = 0$). For variable definitions and details of their construction see Appendix A. Consistent with a negative selection effect, we see that recession startups have significantly fewer employees and are more likely to struggle to pay their bills on time according to Dun & Bradstreet's PayDex score, a measure of credit risk.[22] Their founding inventors are less likely to have previously obtained a patent, more often work alone, and when they do not, are part of smaller teams. Recession startups are also significantly more likely to represent themselves before the PTO ("pro se"), rather than use a patent attorney or patent agent. Such behavior is associated with inexperienced, less sophisticated inventors (Gaudry 2012). These systematic differences imply that a naïve regression of Y on D will compare apples and oranges, and to the extent that recession startups are indeed weaker on average, will underestimate the effect of the recession on startups.

To allow the reader to gauge the economic significance of the estimates reported in the next

[20] We also drop 4,866 firms with missing data on their headquarter state, the founding year, the first-action date, or the art unit in which their application is examined. Further, we drop a small handful of firms whose first patent is assigned to an examiner who investigated fewer than 10 prior patents.

[21] We estimate that of the 701,888 patent applications filed between 2002 and 2009, 78.9% were granted in 5 years or less. Our results are not sensitive to the five-year cutoff.

[22] A PayDex score above 80 indicates that the firm pays its bills on time.

section, we report in Appendix B summary statistics for all our outcome variables.

3. The Effects of the Great Recession on Startups

3.1. Naïve OLS Estimates

We begin by reporting OLS estimates of equation (1) that are naïve in the sense that they ignore selection biases by assuming startups are born randomly over the business cycle. The outcome variables, Y, are survival, cumulative growth in employment, and cumulative growth in sales, in each case measured over periods of 1, 3, 5, and 7 years from birth. We report two growth measures. The first is constructed such that firms are assigned employment and sales of zero when they die, thereby combining the intensive growth margin with the extensive survival margin. The second measures growth conditional on survival. The variable indicating birth relative to the business cycle, D, is set equal to 1 if the startup's founding year coincides with the Great Recession, and 0 otherwise.[23] Recall that we include art-unit-by-application-year fixed effects, which allow us to compare firms seeking patent protection for their invention in the same technology field at the same time.

Table 2 reports the naïve OLS estimates. The observed differences in survival and growth between startups that choose to be born in the Great Recession and those that choose to be born at other times are economically small and mostly statistically insignificant. In Panel A, which includes dead firms, the observed differences, where they are statistically significant, are positive: startups founded in the recession are 1.6 percentage points more likely to survive over 3 years (relative to a sample average of 92%) and grow employment 2.3 and 5.3 percentage points faster and sales 4.1 and 5.9 percentage points faster over 1 and 3 years, respectively. Conditional on survival, the results in Panel B are similar in the short-term, but in the long-term, we see a significant difference in the seven-year employment growth rate, which is 6.5 percentage points *lower* among recession startups.

[23] To code when a startup is born, we prefer using the founding year rather than the incorporation year (as many startups begin life as LLCs and only incorporate later) or the first year of positive sales or employment (as those are outcome variables). Still, there surely is measurement error in D: when is a firm really born? Using an instrument, as we will do later, helps fix the attenuation bias that measurement error can lead to.

3.2. *Intention-To-Treat Effects*

Table 3 reports intention-to-treat effects. Panel A regresses Y on Z_1, the indicator capturing a startup's actual first-action date relative to the recession. Like the naïve OLS estimates, the ITT estimates are positive. They are also larger. Startups receiving their first-action decision in the recession are 6.9 percentage points more likely to survive for 7 years ($p = 0.002$), which is economically meaningful relative to the sample average of 70%. They grow employment faster, by 3.2 percentage points over 1 year ($p = 0.046$), 9.1 percentage points over 5 years ($p = 0.077$), and 18.4 percentage points over 7 years ($p = 0.001$). Sales growth is no different in the short-term, but over 7 years, it is faster by a cumulative 19.7 percentage points ($p = 0.001$).

Whether these estimates can be viewed as causal, and thus as lower bounds on the local average treatment effects on the treated (the LATE), depends on whether the invitation to treatment Z_1 is as good as randomly assigned. As noted, patent examiners may selectively depart from strict time-priority in ways that induce correlation between applicant characteristics and the timing of the first-action decision relative to the business cycle. Table IA.1 in the Internet Appendix uses the approach described in Section 4.4.4 of Angrist and Pischke (2009) to show that applications that are handled according to strict date-order priority (i.e., those for which predicted and actual examination time coincide) are systematically stronger than the average sample startup: they are more likely to involve a team of founding inventors rather than a single inventor ($p = 0.081$) and their founding inventors more often have prior patenting experience ($p = 0.089$), high productivity ($p < 0.05$), and a track record of producing breakthrough inventions ranking in the top decile of U.S. patents ($p = 0.012$). By implication, when examiners depart from strict date-order priority, they favor weaker inventors on average.

To fix endogenous departures from date-order priority, we use the predicted time of the first-action decision, Z_2, as an instrument for the actual time, Z_1. Panel B reports the first-stage, regressing Z_1 on Z_2. The instrument predicts the actual time very well. The F-test is 187.7, well above the rule-of-thumb value of 10 required for the instrument to be strong.[24]

Table 3, Panel C reports the second-stage results of Y on $\hat{Z}_1$, which we refer to as bias-corrected intention-to-treat effects and which we view as our core estimates. Over periods of up to 5 years, startups invited to be born in the recession have statistically similar outcomes as

[24]Reassuringly, the balance test in Table IA.2 in the Internet Appendix shows that when assigned based on Z_2, treated and controls do not differ significantly on observables, as expected given random assignment.

startups invited to be born in an expansion. Over 7 years, on the other hand, recession startups are 12.1 percentage points more likely to survive ($p = 0.076$) and grow their employment and sales a cumulative 35.2 and 35.7 percentage points faster, respectively (both $p < 0.05$). The fact that the bias-corrected ITT effects in Panel C are almost twice as large as the corresponding estimates in Panel A is consistent with examiners favoring unobservably (to us) weaker applications in the recession. Given random assignment of Z_2, the bias-corrected ITT estimates in Panel C have a causal interpretation. They are therefore a lower bound on LATE.

Table 3, Panel D reports bias-corrected ITT effects conditional on survival. Within the sample of survivors, we find no statistically significant differences in growth rates at any horizon. This implies that the differences in growth rates in Panel C are largely driven by the difference in survival. In other words, the positive causal effects of the Great Recession on startups appear to primarily work through a reduced mortality rate.

3.3. Robustness

To avoid any confusion, we reiterate that our instrument, Z_2, is not examiner review speed, but an indicator that equals 1 if the startup's predicted patent decision coincides with the Great Recession as a result of the randomly assigned examiner's review speed in combination with when a future recession occurs. Because review speed does not have a monotonic effect on either the instrument or the treatment (see Section 1.2.4), it is not surprising that our baseline ITT results are virtually unchanged when we allow for patent review delays, suitably identified, to directly affect startup growth as in Hegde, Ljungqvist, and Raj (2022) (see Table IA.3 in the Internet Appendix). For the same reason, there is no reason to expect that our results are confounded by other examiner habits that correlate with review speed. A notable example is grant leniency: while fast examiners tend to grant patents with broader property rights, controlling for scope leniency, suitably instrumented as in Hegde, Ljungqvist, and Raj, leaves our results unchanged (see Table IA.4 in the Internet Appendix).[25]

By assuming that the recession treatment D is binary, our empirical design implicitly makes

[25] To address any remaining concerns that review speed may correlate with unobserved examiner habits that could affect outcomes of interest in unexpected ways, we offer two further robustness tests. First, we remove the examiner from the construction of the instrument, Z_2, by replacing the examiner's historic review speed in equation (3) with the average historic review speed in the art unit. Second, we include examiner fixed effects in our baseline model, to control for unobserved time-invariant examiner habits. Neither affects our results; see Tables IA.5 and IA.6 in the Internet Appendix.

no distinction between slowdowns and recoveries. In Table IA.7, we find no evidence that our results change when we allow slowdowns and recoveries to affect startups differently. Recession startups continue to be more likely to survive ($p = 0.023$) and to experience faster growth in employment ($p = 0.009$) and sales ($p = 0.009$) over their first 7 years.[26]

Our growth rate measures use a definition that has become standard in the literature on firm dynamics: $g_{it} = (Y_{it} - Y_{it-1})/[\frac{1}{2}(Y_{it} - Y_{it-1})]$ (see Davis, Haltiwanger, and Schuh 1996 for a discussion). As Table IA.8 in the Internet Appendix shows, our findings are virtually identical using a continuous growth measure instead.

3.4. Superstar Firms

The ITT estimates in Table 3 capture causal effects on the *average* firm invited to be born in the recession. To test whether the recession has differential effects in the cross-section of firms, and in particular in the superstar right tail of the distribution, we estimate quantile bias-corrected ITT regressions. To get as granular a set of estimates as possible, we report estimates for quantiles 2 to 98 in increments of 2.

Figure 2, Panel A graphs the quantile ITT estimates along with 95% confidence intervals. Contrary to the absence of a short-term effect for the average startup reported in Table 3, we find significant short-term effects in the right tail of the distribution, especially for employment: over a one-year horizon, recession startups in the top quintile of the employment-growth distribution experience significantly faster growth than expansion startups. For sales growth, the shape looks similar but it is only in the 98th percentile that the difference between recession and expansion startups is statistically significant. The boosts to employment and sales growth among the fastest growing startups attenuate over 3 years and disappear over 5 years.

Over 7 years, firms invited to be born in the recession grow their employment and sales faster the *slower* their growth. This inverse relation suggests that the recession benefits slower-growing firms more than faster-growing ones in the long-term. For employment growth, the quantile ITT estimates are generally statistically significant except in the two tails; for sales

[26] While allowing for differential effects of slowdowns and recoveries leaves our ITT estimates of the recession unchanged, the results for slowdowns and recoveries are of independent interest. We find that startups born in the slowdown preceding the Great Recession experience slower short-term growth in employment ($p = 0.035$) and sales ($p = 0.098$), while startups born in the subsequent recovery enjoy faster short-term employment growth ($p = 0.093$).

growth, they are generally statistically significant except in the right tail. Overall, we see little evidence to suggest that superstar firms benefit especially from being born in the recession.

Figure 2, Panel B shows that we find no significant quantile ITT effects at any horizon once we condition on survival, consistent with the absence of significant effects for the average firm, conditional on survival, reported in Table 3.[27]

3.5. Follow-on Innovation of Startups

We next investigate how the Great Recession affects an innovative startup's ability to continue innovating. Table 4 reports bias-corrected ITT effects, estimated either unconditionally (Panel A) or conditional on survival (Panel B). We find that the recession has no effect on either the propensity to continue innovating or the quantity of follow-on innovation, both unconditionally and conditional on survival. Specifically, the likelihood that an innovative startup is subsequently granted one or more patents (column 1) does not differ between recession and expansion startups, nor does the number of follow-on patents (column 2).

What is affected is the originality (and hence likely economic value) of follow-on inventions. To measure originality, we use the "breakthroughness" measure of Kelly et al. (2021), who classify a patent as a breakthrough patent if it has a low textual similarity with previous patents (suggesting it does something highly novel) and a high textual similarity with subsequent patents (suggesting it influences future innovation by others).[28] In column 3, we see that recession startups are subsequently granted patents whose average rank in the breakthroughness distribution is significantly higher, by 16.5 percentiles unconditionally ($p = 0.022$) and 19.1 percentiles conditional on survival ($p = 0.012$).[29] Figure 3 reports quantile ITT effects, showing that recession startups produce higher-impact follow-on inventions throughout the breakthroughness distribution, including in the very right tail. Conditioning on survival makes very little difference to this finding.

[27]Intriguingly, Panel B adds nuance to the findings in Panel A by showing that the positive long-term growth differentials in the left tail disappear once we condition on survival. This suggests that for low-growth startups, the main benefit of being (invited to be) born in the recession is an improved chance of survival.

[28]Unlike in the tables investigating survival and growth, we study follow-on innovation for the next 5 years. The reason is that the breakthrough measure is based on forward similarity with future granted patents that are applied for after the focal patent. Owing to reporting lags at the PTO, 2017 is the last year for which patent applications are available without truncation bias (Hall, Jaffe, and Trajtenberg 2001). This limits us to a five-year window from 2012 (the last year during which sample startups can receive a first-action decision on their first patent application).

[29]As an aside, traditional citation-based metrics of patent quality do not pick this up; see columns 4 and 5.

4. What Drives the Effects of the Great Recession on Startups?

The ITT results reported in the previous section show that the Great Recession had positive effects on the survival and growth prospects of innovative startups, once we hold the underlying quality of the business idea constant via random assignment. What drives these counter-cyclical effects? In this section, we investigate two principal channels through which being born in a recession can affect a startup's future development: a funding channel and a labor-market channel.

4.1. Funding Channel

Much prior work considers startup growth to be procyclical. A popular explanation is that funding dries up in recessions, but two questions remain: does funding dry up even when holding the startup's underlying quality constant (as our empirical design does)? and if so, is the effect transitory or does it permanently scar the startup?

Table 5 reports bias-corrected intention-to-treat effects of the Great Recession on startup funding. Panel A considers Dun & Bradstreet's PayDex score, a measure of credit risk; a score above 80 indicates that the firm pays its bills on time. On this measure, recession startups experience significant financial stress in their first year of operation: they are 19.8 percentage points less likely to pay their bills on time than are expansion startups ($p = 0.038$). Over horizons beyond 1 year, the differential becomes economically smaller and eventually disappears after 7 years, suggesting that the recession only has a transitory effect on startups' credit risk.

The remainder of Table 5 investigates how startups finance their operations. We find no evidence that the recession impairs a startup's ability to raise funding through private placements of equity or debt securities under Regulation D (Panels B and C), from venture capitalists (Panels D and E), or via loans secured against their patent portfolio (Panels F and G), either in the short-term or in the long-term.

Another potential source of funding is patent sales.[30] The costs and benefits of funding a startup's operations through patent sales are a priori unclear. While a patent sale provides

[30] The patent transfer market is quite active. Serrano (2010) reports that 13.5% of patents are traded at least once in their lifetime, rising to 23.9% of patents granted to "small inventors" such as the ones we focus on.

short-term funding, the startup's value will fall if the patent is sold at a discount to the net present value of the future cash flows the startup is expected to earn from it. Empirically, we find that recession startups avoid patent sales. Over a five-year horizon, recession startups are 8.3 percentage points less likely to raise funding by selling their first patent ($p = 0.091$ in Panel H) and 9.6 percentage points less like to sell any patent in their portfolio ($p = 0.064$ in Panel I). Though noisily estimated, these are large effects relative to the unconditional likelihoods of 16% and 20%, respectively.

The final funding source we consider is initial public offerings on a stock market, reported in Panel J. Recession startups are substantially more likely to raise funding from the stock market compared to expansion startups. Specifically, their likelihood of going public is 1.4 percentage points higher over 3 years ($p = 0.050$), 1.3 percentage points higher over 5 years ($p = 0.074$), and 3.4 percentage points higher over 7 years ($p = 0.005$). These effects are economically large given that so few U.S. startups go public: the unconditional likelihood of a startup listing on a stock market ranges from only 0.3% over 3 years to 0.8% over 7 years. Given such a low IPO rate, we view the positive effect of the recession on a startup's likelihood of going public as a consequence—rather than a cause—of the higher growth rates we see throughout the distribution of recession startups.

None of the results reported in Table 5 suggests that funding dries up in the Great Recession once we hold startup quality constant, nor that recession startups are permanently scarred in their ability to raise funding.[31] We view these null results as reassuring: given that we find counter-cyclical effects of the Great Recession on surival and growth, it would have been surprising to find recession startups facing greater financial constraints than expansion startups.

4.2. Labor Channel

Table 6 reports bias-corrected ITT effects of the Great Recession on inventor mobility, hiring, and separation. Panels A through C provide robust evidence that recession startups are better

[31] These conclusions continue to hold on the intensive margin. Table IA.9 in the Internet Appendix briefly considers 12 intensive funding margins, such as the number of VC rounds a startup receives, how many patents it posts as collateral when it borrows, and the breakthroughness rank of the patents it sells, in each case estimated in subsamples consisting of firms that obtain VC funding (Panel A), post a patent as collateral (Panel B), or sell at least one patent (Panel C). Consistent with the extensive-margin results in Table 5, there is little evidence that the Great Recession affects funding choices on the intensive margin at all, and when it does, it does so positively: startups post patents with a higher breakthroughness rank as collateral ($p = 0.052$), and when they do sell patents, they sell a larger number ($p = 0.073$).

able to retain their founding inventors. Over a one-year horizon, the likelihood that a founding inventor departs is 14.8 percentage points lower at a recession startup than at an expansion startup ($p = 0.050$ in Panel A), an effect that is large compared to the unconditional likelihood of 16%. Switching from the inventor level to the startup level, we see a similar picture: the likelihood that a startup loses one or more of its founding inventors over a one-year horizon is 22.3 percentage points lower at recession startups ($p = 0.028$ in Panel B), compared to an unconditional likelihood of 20%. The separation rate, shown in Panel C, is correspondingly lower as well ($p = 0.020$). These effects persist beyond a startup's first year of operation and continue to be economically large (and marginally statistically significant) 7 years out.

A plausible (to us) explanation for the beneficial effects of the Great Recession on founding-inventor retention is that competition for R&D workers decreased in 2007-2009. Figure 4 plots the monthly mobility rate of inventors in the U.S., using the universe of inventors, over the period 2001 to 2015. Mobility declined sharply during the Great Recession, falling from around 0.7% of inventors moving to a new employer a month in 2006 to around 0.5% a month in 2009.

Table 6, Panel D shows that recession startups grow their R&D teams faster compared to expansion startups, by 33.7 percentage points more over 1 year ($p = 0.079$) and 38.3 percentage points more over 3 years ($p = 0.092$). This differential growth in R&D team size is driven by the greater retention of founding inventors reported in Panels A through C: in Panels D and E, we find no difference in the hiring and separation rates of non-founding inventors.

While recession startups do not hire more non-founding R&D workers, they hire more productive ones. Table 7 considers a measure of productivity based on sorting R&D workers employed at sample startups into deciles by the citations to their past patents, using the universe of inventors. Recession startups hire R&D workers who are ranked 1.8 deciles higher on average than those hired by expansion startups in their first year of operation ($p = 0.060$); in their first 3 years of operation, they hire R&D workers who are ranked 1.5 deciles higher ($p = 0.095$).

The results in Tables 6 and 7 are consistent with a labor-market channel helping to explain why recession startups perform better than expansion startups, insofar as the Great Recession enabled startups to retain their founding inventors and build productive R&D teams around them. To investigate the labor-market channel further, we next examine how a startup's ability to retain its founding inventors early in its life affects its subsequent chances of survival and growth in employment and sales. The identification challenge in this test is that unobserved factors may affect both the startup's founding-inventor separation rate and the startup's later performance. For example, it is likely that startups with better prospects (unobserved to the econometrician) both find it easier to retain their founding inventors early on and perform better down the road.

To get a step closer to causality, we instrument a startup's founding-inventor separation rate early in its life with a proxy for the economy-wide demand for R&D workers in the startup's technology field at that time. The idea is that low demand for R&D workers specializing in the startup's technology field will make it easier to retain its founding inventors, and vice versa (relevance). The exclusion restriction requires that changes in the demand for R&D workers in the startup's technology field early in its life do not affect the startup's later-in-life performance other than through their effect on the startup's ability to retain its founding inventors early on. We discuss potential challenges to the exclusion restriction after presenting the results.

We implement this labor-market channel test as follows. We measure a startup's founding-inventor separation rate (defined as in Table 7, Panel C) over the first 2 years from the startup's first-action date.[32] We instrument the separation rate using the change in labor demand for R&D workers in the startup's technology field over the same period, measured as the two-year difference in the mobility rate of R&D workers whose latest patents were granted in the startup's art unit group.[33] Finally, we measure outcomes over windows of 3, 5, and 7 years.

Table 8, Panel A reports the first-stage estimate of the effect of the change in labor demand on the startup's founding-inventor separation rate. As expected, the effect is positive. It is also statistically significant with an F-statistic of 14.2, comfortably in excess of the rule-of-thumb value of 10 required for the instrument to be strong. The first-stage coefficient suggests that a one-standard-deviation fall in the demand for R&D workers in the startup's technology field reduces the rate at which founding inventors leave the startup during its first 2 years by 11.5 percentage points, from the unconditional mean of 59% to 47.5%. Panel B reports the second-stage estimates for our three outcome variables. While the founding-inventor separation

[32]Exploring different windows, we find that the sensitivity of the separation rate to changes in labor demand decreases beyond 2 years. This aligns with prior findings that non-pecuniary match factors such as distance to work or interactions with coworkers (Card et al. 2018) become more important with tenure, at the expense of the kinds of pecuniary match factors that vary with general labor-market conditions (see, for example, Lentz, Piyapromdee, and Robin 2022).

[33]Mobility rates are constructed analogously to Figure 4, which plots the mobility of R&D workers in the U.S. without conditioning on technology field.

rate has no effect on survival or growth over 3 years, it does have a large negative effect over 5 and 7 years. To illustrate, the 11.5 percentage-point fall in a startup's early-life separation rate induced by a one-standard-deviation fall in demand for R&D workers in the startup's technology field increases the startup's chances of surviving for 7 years by 5.4 percentage points ($p = 0.002$) and its growth in employment and sales by 12.6 ($p = 0.010$) and 13.1 percentage points ($p = 0.014$), respectively.

A causal interpretation of the estimates in Table 8 requires that the exclusion restriction holds. Any challenge to the exclusion restriction needs to be able to explain why a fall in demand for the type of R&D workers who patented the startup's founding invention later benefits the startup for reasons other than the startup's improved ability to retain its founding inventors. This causal chain rules out challenges based on the idea that reductions in demand for R&D workers in the startup's technology field portend poor investment opportunities in that technology field: if so, the startup should not perform better down the road. With the caveat that other types of challenges are possible, we view the results in Table 8 as supporting a labor-market channel by which startups benefit from being born in the Great Recession.

5. From ITT to LATE

As noted in Section 1.2.3, we can move beyond intention-to-treat effects of the Great Recession on startup performance to local average treatment effects if we are willing to make additional identifying assumptions, namely that the predicted time of the first-action decision over the business cycle (Z_2) not only is as good as randomly assigned, but that it also satisfies the relevance condition, the monotonicity condition, and the exclusion restriction.

The relevance condition turns out to be challenging: in the sample as a whole, the first-stage regression of D on Z_2 is weak, with a coefficient on Z_2 of 0.024 and a standard error of 0.023.[34] The first-stage coefficient has an economic interpretation: it implies (noisily) that only 2.4% of the startups in our sample "comply" with the invitation to treatment by starting operations in the year in which they are predicted to receive a positive decision on their patent application. By implication, only a subset of innovative startups are responsive to the instrument, Z_2, namely those firms for which a strong signal about patentability is (close to) a necessary condition for

[34] Recall that a weak first-stage is not a problem for the ITT effects we have presented so far: all they require in order to be interpreted causally and as a correctly signed lower bound on the LATE is random assignment.

starting operations. Figure 5 illustrates this fact by showing that the average sample startup starts to generate sales around 2 years before its first-action year, while the median sample startup starts generating sales 2 years after its first-action year. To make progress, we need to restrict the sample to firms that are responsive to the invitation to treatment (with which they can then endogenously choose to comply or not to comply).

Table 9 restricts the sample to firms that are responsive to the invitation to treatment in the sense that they are born around the time of their first-action decision, specifically, in the first-action year or the year after. Panel A shows that the first-stage in the restricted sample is strong, with an F-statistic of 46.5. The first-stage coefficient implies that 25.5% of the startups in the restricted sample comply with the invitation to be treated. The second-stage estimates are reported in Panel B. Consistent with the full-sample ITT effects reported in Table 3, we see little effect in the short-run, but over the full 7-year window, the Great Recession has large positive effects on survival (+31.1 percentage points, $p = 0.040$), employment growth (+82.8 percentage points, $p = 0.027$), and sales growth (+90.4 percentage points, $p = 0.017$). Conditional on survival, the recession has no effect on long-term growth (Panel C), consistent with our full-sample ITT results.

5.1. *Testing the Identifying Assumptions*

The estimates in Table 9, Panel B can be interpreted as the average causal effects of the Great Recession on compliers (i.e., the LATE) as long as the exclusion restriction and monotonicity condition hold.[35] Do these identifying assumptions plausibly hold?

Angrist (2022) describes a "no first stage, no reduced form" test of the exclusion restriction first proposed by Angrist, Lavy, and Schlosser (2010). The intuition is simple. The exclusion restriction requires that the instrument only affect outcomes through its effect on when a firm is born; the instrument would be invalid if it (or something it correlates with) had a direct effect on outcomes, *regardless of when a firm is born.* Finding a direct (i.e., a reduced-form) effect in samples in which the instrument has no effect on when a firm is born (i.e., no first-stage) thus indicates that the exclusion restriction is violated.

Table IA.10 in the Internet Appendix considers two such samples. The first is the full sample

[35] Consistent with ITT effects being a lower bound on LATE, the second-stage estimates in Table 9, Panel C are around three times as large as the corresponding ITT estimates in Table 3.

mentioned earlier, that is, the sample in which only 2.4% of the startups comply with the invitation to treatment by starting operations in the year in which they are predicted to receive a positive decision on their patent application. Panel A confirms that there is no significant first-stage in the full sample. Panel B shows that in this no-first-stage sample, the reduced-form effects of the instrument on survival and growth in employment and sales over horizons from 1 to 7 years are economically small and, with a single exception, statistically indistinguishable from zero. In other words, the timing of a startup's doubly-randomly assigned invitation to treatment relative to the business cycle has no independent effect on its future performance.

The second no-first-stage sample is Table 9's subsample of untreated firms ($D = 0$). If the instrument did correlate with something that affects outcomes regardless of treatment, then in the reduced-form, the instrument should affect outcomes even in the untreated group. As Panel C shows, this is not the case: the reduced-form is zero, implying that the instrument has no direct effect on outcomes (as required for the instrument to be valid).

We next turn to the monotonicity condition. As Dobbie, Goldin, and Yang (2018) note, a testable implication of monotonicity is that the first-stage estimates should be non-negative in all subsamples formed based on observables: no-defiers implies that whatever pre-treatment characteristic we stratify the sample by, the instrument should always affect firms in the same direction. In Table IA.11 in the Internet Appendix, we stratify the sample based on the founding inventor team's size, its prior experience, its productivity, its track record of producing breakthrough inventions, and a measure of its sophistication (whether it applies for its patent pro-se or has attorney representation). Panel A reports the first-stages of the Wald estimator while Panel B includes our full set of fixed effects (which reduces the sample size due to singletons, occasionally severely so). In Panel A, the first-stage is always positive, economically sizeable, and statistically significant. In Panel B, the first-stage is always positive and generally significant. In sum, these findings are line with the monotonicity assumption.[36]

[36] Readers familiar with the Frandsen, Lefgren, and Leslie (2023) joint test of monotonicity and the exclusion restriction may wonder why we do not report it. The reason is that their test is derived for a setting in which monotonicity is a restriction on the observable behavior of the assign*ors*-to-treatment: if a lenient judge (to use their setting) sentences a defendant to pre-trial detention, so would a harsher judge have done. This literal monotonicity implies restrictions on the observed outcome data that their test is designed to exploit. In our setting (as in any randomized control trial), monotonicity is a restriction on the unobservable behavior of the assign*ee*-to-treatment: does the subject defy assignment to treatment by consistently choosing treatment when assigned to the control arm and vice versa? The Dobbie, Goldin, and Yang (2018) test, by contrast, allows us to look for evidence of defiers indirectly, by studying compliance behavior in subsamples based on assignees' observable characteristics.

5.2. *Profiling Compliers and Non-compliers*

We can use the estimates in Table 9 to quantify the presence of compliers and non-compliers, which in turn sheds light on the extent of selection biases and sorting effects in our setting. Using the approach outlined in Angrist and Pischke (2009, Section 4.4.4), Figure 6 plots the fractions of compliers and non-compliers. As we already know from the first-stage reported in Table 9, compliers account for 25.5% of the restricted sample; never-takers account for 54.3% and always-takers for 20.1%. In other words, non-compliance is rampant and mostly takes the form of avoiding to start operations in a recession.

The following table provides a breakdown of compliers and non-compliers by invitation to treament Z_2 and realized treatment D:

	Randomized invitation to treatment (Z_2): 0	Randomized invitation to treatment (Z_2): 1
Recession treatment (D): 0	compliers (20.2%) and never-takers (42.8%)	never-takers (11.4%)
Recession treatment (D): 1	always-takers (15.9%)	compliers (5.4%) and always-takers (4.2%)

Roughly 80% of the compliers are in the expansion treatment and 20% in the recession treatment. That makes intuitive sense, given a fairly constant application rate over time and the fact that the Great Recession accounts for 2 of the 11 calendar years in the sample. The vast majority of always-takers opt into the recession: 15.9% of sample startups choose to start operations in the recession ($D = 1$) even though they are not assigned to it ($Z_2 = 0$). By contrast, a minority of never-takers, accounting for 11.4% of the startups in the sample, when assigned to the recession, delay the start of their operations and so opt out of the recession. Such behavior is not inconsistent with the positive treatment effects we find: because our estimated treatment effects are local (applying to the compliant sub-population), never-takers would not be better off on average had they begun life in the recession. Their decision to wait until the recovery is a form of sorting on the expected sensitivity of their prospects to the recession.

Because LATE is specific to the subpopulation of compliers for the instrument used, the results in Table 9 will only generalize to other populations of interest to the extent that they share similar characteristics as our compliant subpopulation. To get a sense of the external validity of our LATE estimates, we compare the pre-treatment characteristics of compliers to the average startup in the restricted sample. While individual compliers cannot be identified, it is possible to describe their observable characteristics using the approach of Marbach and Hangartner (2020), which is identified under weaker assumptions than LATE.[37] Figure 6 plots means along with 95% confidence intervals, holding technology field, application year, and headquarter location constant. Compliers look no different from the average sample startup in terms of the observables we consider. Given these findings, our LATE estimates should generalize to populations of startups for which positive news about their patent application plausibly triggers the start of operations.

While never-takers look little different from the average sample startup (except that they use more experienced attorneys), always-takers stand out in two regards: they are more likely to have produced a breakthrough invention before ($p = 0.007$) and they use less experienced patent attorneys ($p = 0.010$). These patterns provide nuance to the interpretation that always-takers are "forced entrepreneurs": while some may be (as suggested by their use of less experienced attorney), others may not be (as suggested by their track record of producing breakthrough inventions).

6. Conclusions

We investigate empirically how startups are shaped by the macroeconomic conditions at their birth. To deal with biases arising from the fact that startups can choose when they are born, we exploit the quasi-random timing of patent decisions over the business cycle in the years around the Great Recession. To the extent that recessions leave a permanent mark on startups, we find that it is a positive one: after purging ubiquitous selection biases and sorting effects, recession startups experience better long-term outcomes in terms of employment and sales growth (both driven by lower mortality) and future inventiveness. Contrary to popular belief, recessions do not spawn superstar firms especially: the beneficial long-term effects of the Great Recession are evident throughout the distribution of firms, and they are strong among both low-growth and high-growth firms.

[37]Specifically, the approach requires random assignment and monotonicity but not the exclusion restriction.

Our finding that the Great Recession left a positive long-term mark on startups contrasts with the negative long-term "scarring" effects documented for individual graduates entering the labor market in a recession (Oyer 2006; Kahn 2010; Oreopoulos, von Wachter, and Heisz 2012; Borgschulte and Martorell 2018; Schwandt and von Wachter 2019; Rothstein 2021). We trace the positive effects on startups to a reduction in competition for talented R&D workers during the Great Recession. Specifically, we show that recession startups are better able to retain their founding inventors and to build productive R&D teams around them. Linking retention and performance directly, we find that a greater retention rate early in a startup's life (suitably instrumented) predicts performance later in its life.

Methodologically, our empirical design compares the future outcomes of startups applying for a patent in the same narrow technology field at the same time as a function of when over the business cycle they receive a positive decision about their patent application. By virtue of random assignment of patent applications to patent examiners who differ in their review speeds, the timing of the patent decision is quasi random with respect to the business cycle. But random assignment is not sufficient to ensure that the effect of the recession on the treated can be estimated consistently. The reason is that while the exogenous timing of the patent decision randomly assigns startups to the recession treatment and the expansion control group, startups can opt out of these random assignments, by endogenously delaying the commercialization of a patent issued in a recession ("never-takers") or by commercializing an invention during a recession before the patent has been granted ("always-takers"). We estimate that such non-compliance is rampant, show that endogenous sorting into and out of the recession coexist, and establish that once the selection effects are purged, the causal effects of the Great Recession on "compliers" are positive.

As every recession is likely different in some way, we leave the question whether our findings generalize beyond the Great Recession to future research.

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A. Variable Definitions

Variable	Definition
A. Treatment, assignment to treatment, and instrumental variable	
D: $\mathbb{1}$(Recession)	Indicator set equal to 1 if the startup is founded in 2008 or 2009, and 0 otherwise. Source: NETS.
Z_1: $\mathbb{1}$(Recession)	Indicator set equal to 1 if the startup receives the first-action decision on its first successful patent application during the Great Recession (December 1, 2007 to June 30, 2009), and 0 otherwise. Source: USPTO Patent Application Information Retrieval (PAIR).
Z_2: $\mathbb{1}$(Recession)	Indicator set equal to 1 if the startup is predicted to receive the first-action decision on its first successful patent application during the Great Recession (December 1, 2007 to June 30, 2009), and 0 otherwise. We predict the first-action date based on the sum of the application date, the docket lag, and the examiner's average historical review speed. Source: USPTO Patent Application Information Retrieval (PAIR).
First-action examination time	The time between a startup's patent application date and the first-action date, in years. Source: USPTO Patent Application Information Retrieval (PAIR).
Examiner review speed	The average first-action examination time (in years) of a startup's patent examiner, computed using all patents the examiner examined prior to the startup's application date. Examiner review speed is calculated as of the focal patent's first-action date. Source: USPTO Patent Application Information Retrieval (PAIR).
B. Firm characteristics at birth and at first-action	
Employees at birth	The number of employees at the startup in its founding year. Source: NETS.
$\mathbb{1}$(PayDex score $\geq$ 80) at birth	Indicator set equal to 1 if the startup has a minimum PayDex score of at least 80 (indicating it pays bills timely) throughout its founding year, and 0 otherwise. Source: NETS.
Age at first-action	The startup's first-action year minus the startup's founding year. Source: USPTO PatentsView and NETS.
Employees at first-action	The number of employees at the startup in its first-action year. Source: NETS.
Sales at first-action	The startup's sales in the first-action year, deflated to U.S. dollars of 2012 purchasing power using the GDP deflator. Source: NETS.
$\mathbb{1}$(PayDex score $\geq$ 80) at first-action	Indicator set equal to 1 if the startup has a minimum PayDex score of at least 80 (indicating it pays bills timely) throughout its first-action year, and 0 otherwise. Source: NETS.
$\mathbb{1}$(Reg. D private placement) at first-action	Indicator set equal to 1 if the startup has filed a Regulation D form before its first-action date, and 0 otherwise. Source: EDGAR.
$\mathbb{1}$(VC funding) at first-action	Indicator set equal to 1 if the startup has raised VC funding before its first-action date, and 0 otherwise. Source: Thomson Reuters VenturеXpert.
$\mathbb{1}$(Founding inventor's first patent filing)	Indicator set equal to 1 if the focal patent is the founding inventor's first patent filing, and 0 otherwise.
Years since founding inventor's first patent	The number of years since the filing of the inventor's first successful patent application, measured either relative to the startup's birth year or its first-action year. Source: USPTO PatentsView.

Variable	Definition
$\mathbb{1}$(Single founding inventor)	Indicator set equal to 1 if the startup's first (eventually successful) patent is filed by a single inventor, and 0 otherwise. Source: USPTO PatentsView.
No. of founding inventors	The number of inventors listed on the startup's first (eventually successful) patent application. Source: USPTO PatentsView.
Founding inventor productivity	We measure founding inventor productivity by sorting founding inventors into deciles by the citations to their past patents. To define the decile breakpoints, we rank the universe of inventors in the U.S. every quarter by the average standardized number of citations to patents granted to them over the previous 10 years. To account for technology-specific time trends, we standardize a patent's citations by the mean citations in a given grant year and technology class. We divide the standardized citations by the patent's number of inventors. For each patent, we count citations in the 5 years after its grant date. Founding inventors who receive zero citations are assigned to the bottom decile. Source: USPTO PatentsView.
$\mathbb{1}$(Prior breakthrough patent)	Indicator set equal to 1 if a founding inventor filed a patent ranking in the top decile of the breakthroughness distribution before filing the focal patent.
Breakthroughnewss rank of prior patents	The mean percentile breakthroughness rank of a founding inventor's patents filed before the focal patent.
$\mathbb{1}$(Pro se applicant)	Indicator set equal to 1 if a founding inventor files the startup's first patent on her own (without a patent attorney or agent).
$\mathbb{1}$(No. of attorney's prior applications)	The (mean) number of previous applications the applicant's patent attorney or patent agent has filed.
C. Survival and growth	
$\mathbb{1}$(Survival)	Indicator set equal to 1 if NETS reports employment data for the startup in year $t+k$ or any subsequent year, where t is the first-action year and $k = 1, 3, 5, 7$, and 0 otherwise. Source: NETS.
Employment growth	Following Davis, Haltiwanger, and Schuh (1996), employment growth after the first-action decision is defined as $\frac{employment_{t+k} - employment_t}{\frac{1}{2}(employment_{t+k} + employment_t)}$, where t is the first-action year and $k = 1, 3, 5, 7$. Note that this definition measures decreases and increases in employment symmetrically between -2 (the startup ceases to exist) and $+2$ (the startup adds its first employees), whereas a conventional growth rate ranges from -1 to ∞. Source: NETS.
Sales growth	Following Davis, Haltiwanger, and Schuh (1996), sales growth after the first-action decision is defined as $\frac{sales_{t+k} - sales_t}{\frac{1}{2}(sales_{t+k} + sales_t)}$, where t is the first-action year and $k = 1, 3, 5, 7$. Note that this definition measures decreases and increases in sales symmetrically between -2 (the startup ceases to exist) and $+2$ (the startup earns its first revenue), whereas a conventional growth rate ranges from -1 to ∞. Source: NETS.
D. Follow-on innovation	
$\mathbb{1}$(Follow-on patent)	Indicator set equal to 1 if the startup files a successful patent application after the first-action date of its first patent application, and 0 otherwise. Source: USPTO PatentsView.
Patents	Number of follow-on patents granted to the startup over the 5 years from the first-action decision on startup's first patent application. Source: USPTO PatentsView.

Variable	Definition
Breakthroughness rank	The mean percentile breakthroughness rank of the startup's follow-on patents granted over the 5 years from the first-action decision on its first patent application. Following Kelly et al. (2021), breakthroughness is measured using a patent's one-year forward similarity scaled by its five-year backward similarity. Source: Own calculation.
Citations to follow-on patents	The total number of citations received by the startup's follow-on patents over the 5 years from each follow-on patent's grant date. Source: USPTO PatentsView.
Mean citations per follow-on patent	The total number of citations divided by the number of follow-on patents filed by the startup (missing if the startup files no eventually successful follow-on patent applications in the first 5 years after the first-action on its first patent application). Source: USPTO PatentsView.
E. Funding	
$\mathbb{1}$(PayDex score $\geq$ 80)	Indicator set equal to 1 if the startup has a minimum PayDex score of at least 80 (indicating it pays bills timely) in year k following its first-action date t, and 0 otherwise. Source: NETS.
$\mathbb{1}$(Reg. D private placement)	Indicator set equal to 1 if the startup files one or more Regulation D forms in the k years following its first-action date t, and 0 otherwise. Source: EDGAR.
$\mathbb{1}$(First Reg. D private placement)	Indicator set equal to 1 if the startup files its first Regulation D form in the k years following its first-action date t, and 0 otherwise. The variable is set to missing for a startup that filed its first Regulation D form before its first-action date. Source: EDGAR.
$\mathbb{1}$(VC funding)	Indicator set equal to 1 if the startup raises VC funding in the k years following its first-action date t, and 0 otherwise. Source: Thomson Reuters VentureXpert.
$\mathbb{1}$(First VC funding)	Indicator set equal to 1 if the startup raises VC funding for the fist time in the k years following its first-action date t, and 0 otherwise. The variable is set to missing for a startup that raised VC funding before its first-action date. Source: Thomson Reuters VentureXpert.
$\mathbb{1}$(First patent as collateral)	Indicator set equal to 1 if the startup uses its first patent as collateral in the k years following its first-action date t, and 0 otherwise. Source: USPTO Patent Assignment database.
$\mathbb{1}$(Any patent as collateral)	Indicator set equal to 1 if the startup uses any of its patents as collateral in the k years following its first-action date t, and 0 otherwise. Source: USPTO Patent Assignment database.
$\mathbb{1}$(Sale of first patent)	Indicator set equal to 1 if the startup reassigns its first patent in the k years following its first-action date t, and 0 otherwise.
$\mathbb{1}$(Sale of any patent)	Indicator set equal to 1 if the startup reassigns any of its patents in the k years following its first-action date t, and 0 otherwise.
$\mathbb{1}$(IPO fundraising)	Indicator set equal to 1 if the startup raises funding via an initial public offering on a U.S. stock exchange in the k years following its first-action date t, and 0 otherwise. Source: Thomson Reuters SDC.
F. Funding — intensive margin	
Number of VC funding rounds	Number of VC funding rounds the startup raises in the k years following its first-action date t. Source: Thomson Reuters VentureXpert.
VC funding amount	Total amount of VC funding the startup raises in the k years following its first-action date t. Source: Thomson Reuters VentureXpert.

Variable	Definition
Mean VC funding amount per round	Total amount of VC funding divided by number of VC funding rounds raised by the startup. Source: Thomson Reuters VentureXpert.
Time to VC funding round	Time in years until the startup raises VC funding following the first-action date. Source: Thomson Reuters VentureXpert.
Number of collateralized loans	Number of patent reassignments with the conveyance type "security" in the k years following the startup's first-action date t. Source: USPTO Patent Assignment database.
Number of patents used as collateral	Number of patents reassigned in transactions with conveyance type "security" in the k years following the startup's first-action date t. Source: USPTO Patent Assignment database.
Breakthroughness rank of patent collateral	Mean percentile breakthroughness rank of the patents the startup uses as collateral. Source: USPTO Patent Assignment database.
Time to collateralized loan	Time in years from the startup's first-action date t until one or more of the startup's patents are used as collateral for the first time. Source: USPTO Patent Assignment database.
Number of patent sales	Number of patent reassignments involving one or more of the startup's patents in the k years following its first-action date t. We exclude transactions related to collateral borrowing (i.e., conveyance types "security" and "release"). Source: USPTO Patent Assignment database.
Number of sold patents	Number of the startup's patents that are reassigned in the k years following its first-action date t. We exclude transactions related to collateral borrowing (i.e., conveyance types "security" and "release"). Source: USPTO Patent Assignment database.
Breakthroughness rank of patents sold	Mean percentile breakthroughness rank of the startup's patents that are reassigned. Source: USPTO Patent Assignment database.
Time to patent sale	Time in years from the startup's first-action date t until one or more of the startup's patents are reassigned for the first time. Source: USPTO Patent Assignment database.
G. Employment of founding and non-founding inventors	
$\mathbb{1}$(Founding inventor departs)	Indicator set equal to 1 if one of the startup's founding inventors leaves for another firm in the k years following its first-action date t, and 0 otherwise. Measured either at the inventor level or at the startup level. Source: USPTO PatentsView.
Separation rate of founding inventors	We measure a startup's founding-inventor separation rate after its first-action decision as $\frac{\text{departing founding inventors}_{t,t+k}}{\frac{1}{2}(\text{founding inventors}_{t+k}+\text{founding inventors}_{t})}$, where t is the first-action year and $k = 1, 3, 5, 7$. Source: USPTO PatentsView.
Growth rate of founding and non-founding inventors	We measure the growth rate of a startup's team of inventors after its first-action date as $\frac{\text{inventors}_{t+k}-\text{inventors}_{t}}{\frac{1}{2}(\text{inventors}_{t+k}+\text{inventors}_{t})}$, where t is the first-action year and $k = 1, 3, 5, 7$. Source: USPTO PatentsView.
Hiring rate of non-founding inventors	We measure a startup's non-founding inventor hiring rate after its first-action decision as $\frac{\text{hired non-founding inventors}_{t,t+k}}{\frac{1}{2}(\text{inventors}_{t+k}+\text{inventors}_{t})}$, where t is the first-action year and $k = 1, 3, 5, 7$. Source: USPTO PatentsView.
Separation rate of non-founding inventors	We measure a startup's non-founding inventor separation rate after its first-action decision as $\frac{\text{departing non-founding inventors}_{t,t+k}}{\frac{1}{2}(\text{inventors}_{t+k}+\text{inventors}_{t})}$, where t is the first-action year and $k = 1, 3, 5, 7$. Source: USPTO PatentsView.

Variable	Definition
H. Productivity of founding and non-founding inventors	
Inventor productivity	We measure inventor productivity by sorting inventors employed at sample startups into deciles by the citations to their past patents. To define the decile breakpoints, we rank the universe of inventors in the U.S. every quarter by the average standardized number of citations to patents granted to them over the previous 10 years. To account for technology-specific time trends, we standardize a patent's citations by the mean citations in a given grant year and technology class. We divide the standardized citations by the patent's number of inventors. For each patent, we count citations in the 5 years after its grant date. Inventors who receive zero citations are assigned to the bottom decile. Source: USPTO PatentsView.
I. Labor demand for R&D workers	
Change in labor demand for R&D workers	We measure the change in labor demand for R&D workers in a startup's technology field as the difference in the mobility rates of inventors in that technology field between month $t+24$ and month t, where t is the month of a startup's first action date. We take a startup's technology field to be the art unit group in which the startup's first patent was granted. We compute the monthly mobility rate of inventors in a technology field as the number of inventors moving from one firm to another scaled by the number of inventors employed by U.S. firms in that technology field and month. We then smooth the series by taking a six-month moving average, which we annualize by multiplying by 12. To measure inventor mobility between 2001 and 2015, we follow the approach of Marx, Strumsky, and Fleming (2009) and use the universe of granted patents from 1976 to 2020. We assign inventors to a technology field in a given month based on the art-unit group of their most recent patent filing. Source: USPTO PatentsView.
J. Patent scope and scope leniency	
Patent scope	The number of independent claims in a startup's granted patent application. Source: USPTO Patent Application Information Retrieval (PAIR).
Examiner scope leniency	The average number of independent claims granted by a startup's patent examiner in prior patent applications, computed using all patents the examiner examined prior to the startup's application date. Examiner scope leniency is calculated as of the focal patent's first-action date. Source: USPTO Patent Application Information Retrieval (PAIR).

B. Summary Statistics: Outcome Variables

The table reports summary statistics. Panels A, B, and C report summary statistics for the 6,946 startups in the main sample. Panel D reports summary statistics for the 713 startups that receive VC financing, the 745 startups that use at least one patent as collateral, and the 1,392 startups that sell at least one patent over the subsequent 5 years. Panel E reports summary statistics for the 14,348 founding inventors who produce a startup's first patent. We compute employment spells for those inventors who file at least one more patent over the subsequent 7 years and departure likelihoods for the inventors who are employed by the startup at first-action. Panels F and G reports summary statistics for the 3,218 startups for which we observe at least one employed inventor at first-action. For variable definitions and details of their construction see Appendix A.

	Window	Mean	P50	SD
A. Survival and growth				
$\mathbb{1}$(Survival)	1 year	1.00	1.00	0.07
	3 years	0.92	1.00	0.26
	5 years	0.82	1.00	0.39
	7 years	0.70	1.00	0.46
Employment growth	1 year	0.06	0.00	0.33
	3 years	−0.00	0.00	0.73
	5 years	−0.20	0.00	0.99
	7 years	−0.44	0.00	1.15
Sales growth	1 year	0.05	0.00	0.35
	3 years	−0.01	0.00	0.75
	5 years	−0.19	0.00	1.02
	7 years	−0.41	−0.01	1.18
B. Follow-on innovation				
$\mathbb{1}$(Follow-on patent)	5 years	0.40	0.00	0.49
Number of follow-on patents	5 years	5.00	2.00	11.98
Breakthroughness rank of follow-on patents	5 years	0.54	0.56	0.26
Citations of follow-on patents	5 years	31.95	4.00	165.13
Mean citations per follow-on patent	5 years	3.94	2.00	7.44
C. Funding				
$\mathbb{1}$(PayDex score $\geq$ 80)	1 year	0.32	0.00	0.47
	3 years	0.29	0.00	0.46
	5 years	0.27	0.00	0.44
	7 years	0.27	0.00	0.44
$\mathbb{1}$(Reg. D private placement)	1 year	0.09	0.00	0.29
	3 years	0.16	0.00	0.36
	5 years	0.18	0.00	0.38
	7 years	0.18	0.00	0.39
$\mathbb{1}$(First Reg. D private placement)	1 year	0.05	0.00	0.21
	3 years	0.08	0.00	0.28
	5 years	0.10	0.00	0.30
	7 years	0.11	0.00	0.31
$\mathbb{1}$(VC funding round)	1 year	0.06	0.00	0.24
	3 years	0.09	0.00	0.29
	5 years	0.10	0.00	0.30
	7 years	0.11	0.00	0.31
$\mathbb{1}$(First VC funding)	1 year	0.02	0.00	0.13
	3 years	0.03	0.00	0.18
	5 years	0.04	0.00	0.19

	Window	Mean	P50	SD
	7 years	0.04	0.00	0.20
𝟙(First patent as collateral)	1 year	0.02	0.00	0.15
	3 years	0.07	0.00	0.25
	5 years	0.10	0.00	0.30
	7 years	0.13	0.00	0.34
𝟙(Any patent as collateral)	1 year	0.02	0.00	0.15
	3 years	0.07	0.00	0.25
	5 years	0.11	0.00	0.31
	7 years	0.14	0.00	0.34
𝟙(Sale of first patent)	1 year	0.03	0.00	0.16
	3 years	0.10	0.00	0.29
	5 years	0.16	0.00	0.37
	7 years	0.21	0.00	0.41
𝟙(Sale of any patent)	1 year	0.04	0.00	0.20
	3 years	0.12	0.00	0.33
	5 years	0.20	0.00	0.40
	7 years	0.25	0.00	0.43
𝟙(IPO fundraising)	1 year	0.00	0.00	0.03
	3 years	0.00	0.00	0.05
	5 years	0.01	0.00	0.07
	7 years	0.01	0.00	0.09
D. Funding — intensive margin				
Number of VC funding rounds	5 years	2.98	3.00	2.06
VC funding amount ($ million)	5 years	27.68	14.46	44.57
VC funding amount per round ($ million)	5 years	1.11	0.00	4.83
Time to VC funding round (years)	5 years	1.14	0.84	1.06
Number of collateralized loans	5 years	1.63	1.00	1.43
Number of patents used as collateral	5 years	4.28	2.00	9.28
Breakthroughness rank of patent collateral	5 years	0.49	0.49	0.28
Time to collateralized loan (years)	5 years	2.33	2.25	1.43
Number of patent sales	5 years	1.99	1.00	3.34
Number of sold patents	5 years	2.60	1.00	4.48
Breakthroughness rank of patents sold	5 years	0.49	0.49	0.28
Time to patent sale (years)	5 years	2.43	2.36	1.41
E. Founding inventors — inventor level				
𝟙(Founding inventor departs)	1 year	0.16	0.00	0.37
	3 years	0.36	0.00	0.48
	5 years	0.44	0.00	0.50
	7 years	0.48	0.00	0.50
F. Employment of founding and non-founding inventors — startup level				
𝟙(Founding inventor departs)	1 year	0.20	0.00	0.40
	3 years	0.43	0.00	0.49
	5 years	0.51	1.00	0.50
	7 years	0.55	1.00	0.50
Separation rate of founding inventors	1 year	0.34	0.00	0.73
	2 year	0.59	0.00	0.89
	3 year	0.75	0.00	0.95
	5 year	0.91	0.50	0.99
	7 year	1.00	0.67	1.04
Growth rate of founding and non-founding inventors	1 year	−0.17	0.00	0.78

	Window	Mean	P50	SD
	3 year	−0.37	0.00	1.06
	5 year	−0.40	0.00	1.11
	7 year	−0.42	0.00	1.13
Hiring rate of non-founding inventors	1 year	0.12	0.00	0.26
	3 year	0.29	0.00	0.48
	5 year	0.40	0.00	0.65
	7 year	0.47	0.00	0.79
Separation rate of non-founding inventors	1 year	0.02	0.00	0.13
	3 year	0.10	0.00	0.39
	5 year	0.18	0.00	0.57
	7 year	0.25	0.00	0.71
G. Productivity of founding and non-founding inventors				
Productivity of founding inventors	1 year	7.70	8.75	2.55
	3 years	7.65	8.50	2.56
	5 years	7.49	8.00	2.58
	7 years	7.35	8.00	2.56
Productivity of non-founding inventors	1 year	7.00	7.71	2.62
	3 years	6.38	7.00	2.73
	5 years	5.81	6.00	2.68
	7 years	5.43	5.67	2.57
Productivity of all inventors	1 year	7.35	8.00	2.46
	3 years	6.99	7.50	2.47
	5 years	6.60	7.00	2.46
	7 years	6.26	6.50	2.42

Figure 1. Sample Distribution over Time.

The figure shows the number of sample firms by year of patent application. The sample consists of 6,946 startups that file their first (eventually successful) patent application between 2002 (the first year after the 2001 recession) and 2009 (the ending year of the Great Recession) and that receive their first-action decision no later than 2012. The dates of the Great Recession (December 1, 2007 to June 30, 2009) are shaded in red. We distinguish between patent applications that receive their first-action decision before, during, and after the Great Recession. 17% of sample startups receive the first-action decision during the Great Recession. For variable definitions and details of their construction see Appendix A.

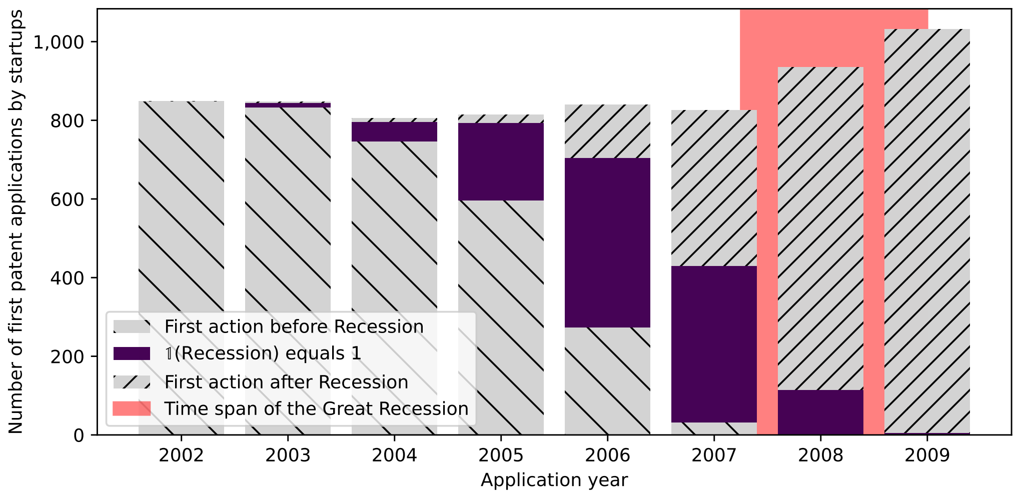
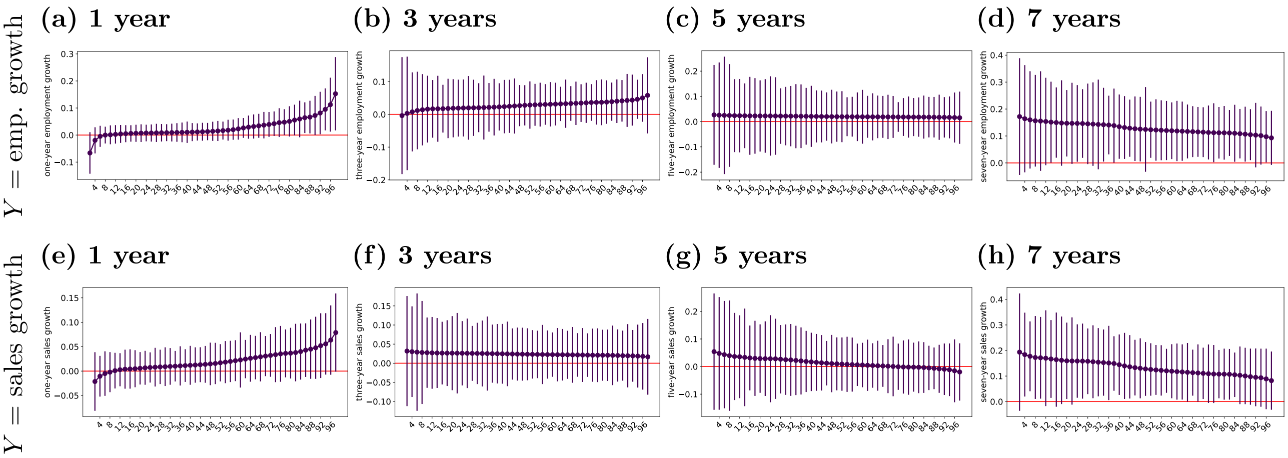


Figure 2. Startup Growth: Quantile ITT Effects.

The figure plots bias-corrected quantile intention-to-treat (ITT) estimates of the effects of being born in the Great Recession on a startup's growth in employment and sales over windows of 1, 3, 5, and 7 years following the startup's first-action date, along with 95% confidence intervals based on bootstrapped standard errors clustered at the art unit level. We estimate bias-corrected intention-to-treat effects (Y on Z_2) for quantiles 2 to 98 in increments of 2. Panel A considers all startups (setting sales and employment to zero for dead firms), while Panel B considers only surviving startups. All specifications include art-unit-by-application-year fixed effects and indicators for startups headquartered in California or Massachusetts. In addition, the specifications for employment growth control for log employment in the year of first-action, while those for sales growth control for log sales in the year of first-action. For variable definitions and details of their construction see Appendix A.

A. Bias-corrected intention-to-treat (Y on Z_2)



B. Bias-corrected intention-to-treat (Y on Z_2), conditional on survival

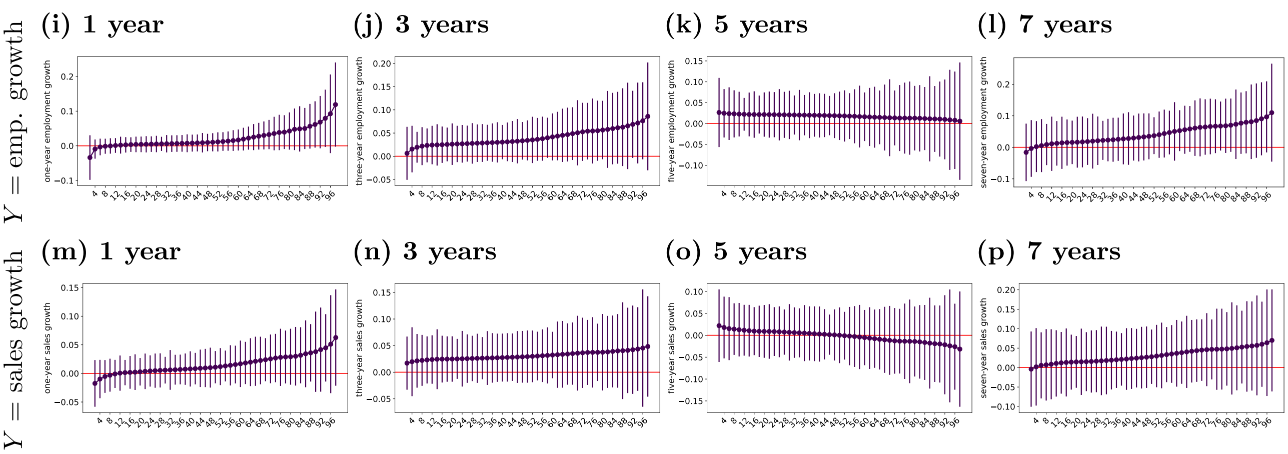
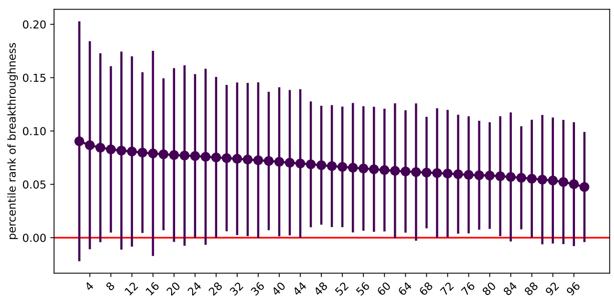


Figure 3. Follow-on Innovation: Quantile ITT Effects.

The figure plots bias-corrected quantile intention-to-treat (ITT) estimates of the effect of being born in the Great Recession on the "breakthroughness" of a startup's follow-on inventions over the 5 years from the startup's first first-action date, along with 95% confidence intervals based on bootstrapped standard errors clustered at the art unit level. The unit of observation is a follow-on patent and the dependent variable is the follow-on patent's percentile rank in the breakthroughness distribution considering all patents granted in the U.S. over our sample period. We estimate bias-corrected intention-to-treat effects (Y on Z_2) for quantiles 2 to 98 in increments of 2. Panel (a) considers all startups, while Panel (b) considers only surviving startups. Both specifications include art-unit-by-application-year fixed effects and indicators for startups headquartered in California or Massachusetts. For variable definitions and details of their construction see Appendix A.

(a) Full sample



(b) Conditional on survival

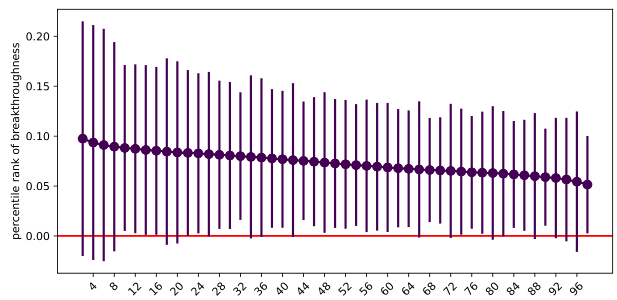


Figure 4. Monthly Mobility Rate of U.S. Inventors.

The figure shows the monthly mobility rate of U.S. inventors from 2001 to 2015. We compute the monthly mobility rate as the number of inventors moving from one firm to another firm divided by the number of inventors employed by U.S. firms in a given month. To measure inventor mobility between 2001 and 2015, we use the universe of granted patents from 1976 to 2020 and follow the approach of Marx, Strumsky, and Fleming (2009). The dates of the Great Recession (December 1, 2007 to June 30, 2009) are shaded in red.

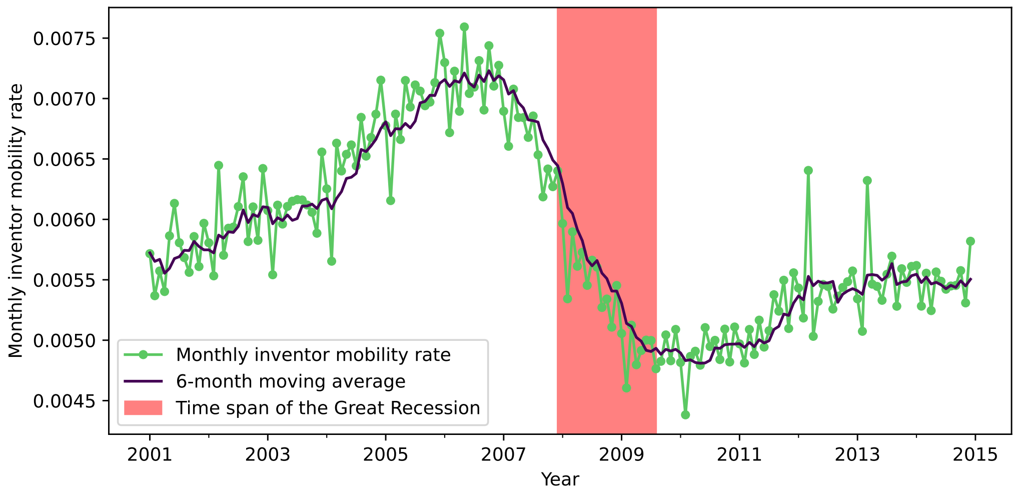
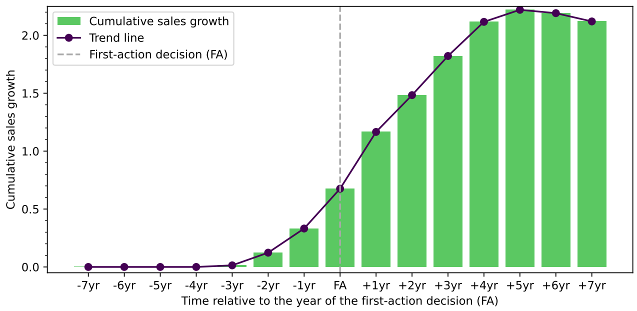


Figure 5. Startup Sales Growth Around the First-Action Decision

The figure shows startups' annual sales growth from up to 7 years before to up to 7 years after the year of the first-action decision on a startup's first successful patent application. In each year, we calculate a conventional sales growth rate as $\frac{sales_t - sales_{t-1}}{sales_{t-1}}$. We set a startup's sales growth to zero in the year(s) before NETS reports positive sales for the first time. The sample consists of surviving firms (which is why cumulative sales growth can appear to decline over time).

(a) Mean



(b) Median

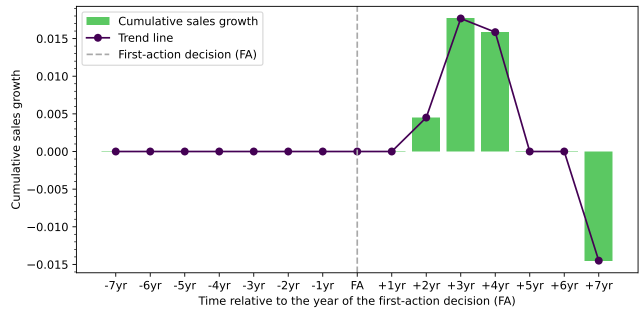
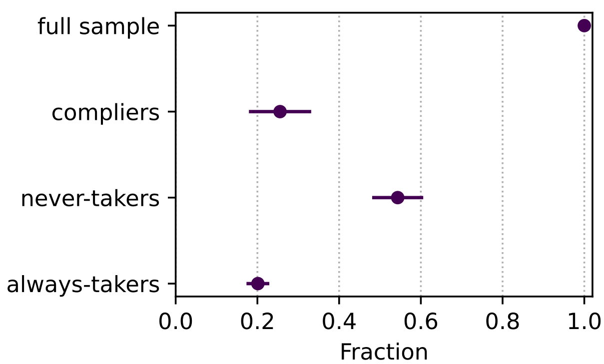


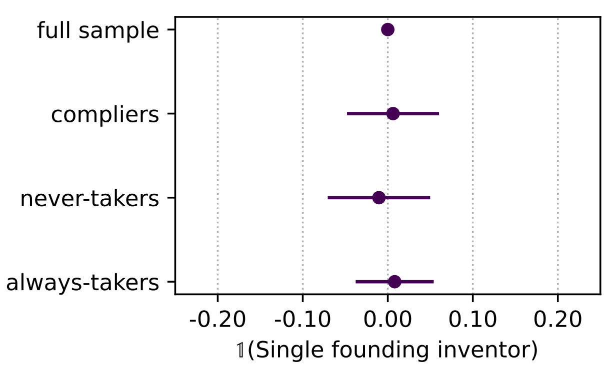
Figure 6. Profiling Compliers and Noncompliers.

The figure plots estimated fractions and mean characteristics for the complier, never-taker, and always-taker subpopulations for the 2,017 firms born in the first-action year or the year after (as used in Table 9). To estimate the fractions, we follow the approach outlined in Angrist and Pischke (2009, Section 4.4.4) and estimate the first-stage used in Panel B of Table 9. To estimate the mean characteristics, we residualize the characteristics using art-unit-by-application-year and state fixed effects and use the residualized characteristics as inputs for the approach of Marbach and Hangartner (2020), which requires random assignment of the instrument and that there are no defiers. The horizontal lines indicate 95% confidence intervals based on 1,000 bootstraps clustering standard errors at the art unit level. For variable definitions and details of their construction see Appendix A.

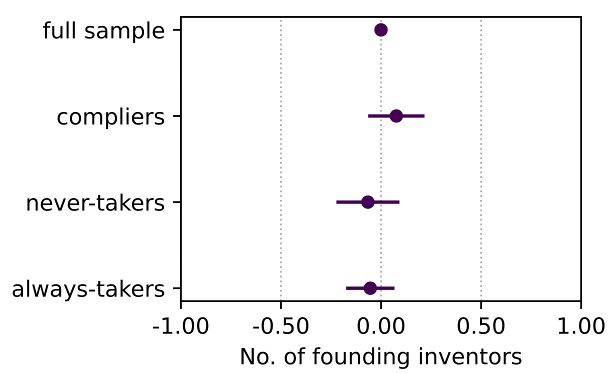
(a) Fractions of compliers and non-compliers



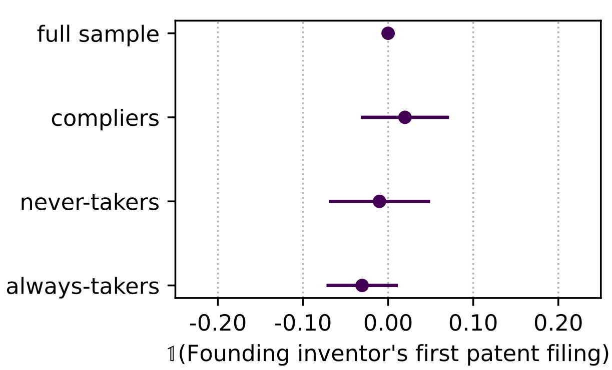
(b) $\mathbb{1}$(Single founding inventor)



(c) No. of founding inventors



(d) $\mathbb{1}$(Founding inventor's first patent filing)



(e) Founding inventor productivity

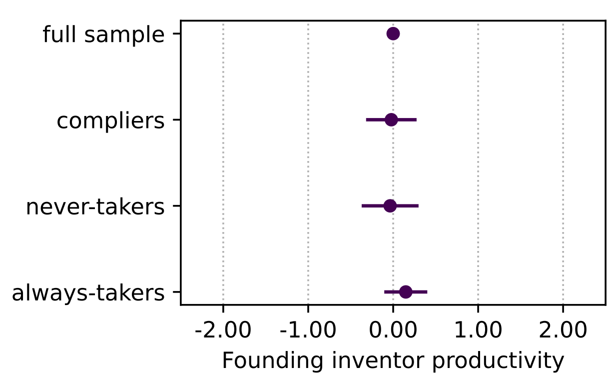
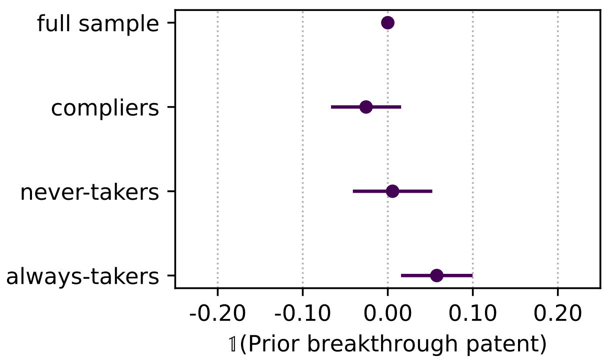
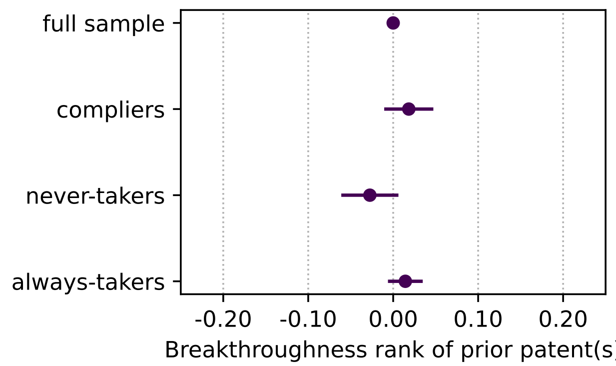


Figure 6
Continued

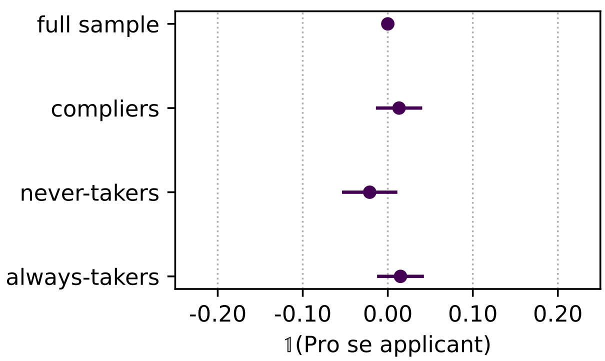
(f) 𝟙(Prior breakthrough patent)



(g) Breakthroughness rank of prior patents



(h) 𝟙(Pro se applicant)



(i) Log(No. of attorney's prior applications)

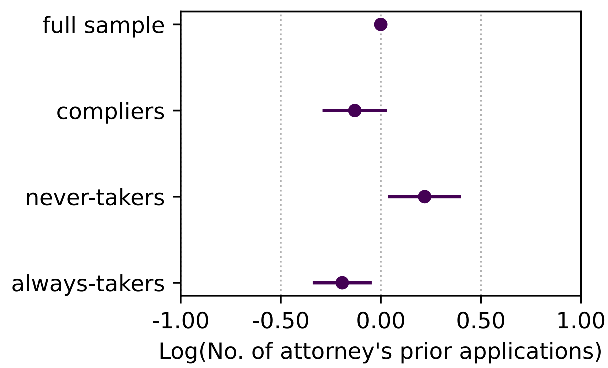


Table 1. Summary Statistics: Recession vs. Expansion Startups.

The table reports summary statistics for the 1,354 startups born in the Great Recession ($D = 1$) and the 5,592 startups born at other times ($D = 0$). For variable definitions and details of their construction see Appendix A. To test whether recession and expansion startups differ on observables, we use a t-test of equal means after controlling for art-unit-by-application-year fixed effects and clustering the standard errors at the art unit level. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

	Recession ($D = 1$)			Expansion ($D = 0$)			Adj. differences
	Mean	P50	SD	Mean	P50	SD	t-stat
Employees at birth	5.34	2.00	22.80	13.72	3.00	237.20	–1.77*
$\mathbb{1}$(PayDex score $\geq$ 80) at birth	0.63	1.00	0.49	0.45	0.00	0.50	2.14**
$\mathbb{1}$(Founding inventor's first patent filing)	0.47	0.00	0.50	0.43	0.00	0.50	2.26**
Years since founding inventor's first patent	5.75	1.00	7.82	5.59	2.00	7.51	–0.50
$\mathbb{1}$(Single founding inventor)	0.47	0.00	0.50	0.44	0.00	0.50	2.20**
No. of founding inventors	2.01	2.00	1.37	2.08	2.00	1.38	–2.38**
Founding inventor productivity	7.29	8.08	2.86	7.51	8.24	2.68	0.73
$\mathbb{1}$(Prior breakthrough patent)	0.24	0.00	0.43	0.27	0.00	0.44	–0.76
Breakthroughness rank of prior patent(s)	0.52	0.52	0.23	0.54	0.54	0.24	0.94
$\mathbb{1}$(Pro se applicant)	0.09	0.00	0.28	0.09	0.00	0.29	–1.98**
Log(No. of attorney's prior applications)	7.13	7.31	1.59	6.90	7.02	1.55	–1.29

Table 2. Naïve OLS Effects of the Great Recession on Startup Survival and Growth.

The table reports naïve OLS estimates of the effects of being born in the Great Recession on a startup's likelihood of survival, its employment growth, and its sales growth over windows of 1, 3, 5, and 7 years following the startup's birth. Panel A considers all startups (setting growth rates to -100% for dead firms), while Panel B considers only surviving startups. All specifications include art-unit-by-application-year and headquarter-state fixed effects. In addition, the specifications for survival and employment growth control for log employment in the year of birth, while those for sales growth control for log sales in the year of birth. The number of observations falls short of 6,946 startups due to singletons; in the sales-growth specifications, it is further reduced due to missing sales data in NETS. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

		Startup survival and growth over			
		1 year (1)	3 years (2)	5 years (3)	7 years (4)
A. Naïve OLS (Y on D)					
#1	$Y = \mathbb{1}$(Survival)	0.001	0.016**	0.004	0.004
		0.001	*0.007*	*0.013*	*0.016*
	R^2	20.4%	23.1%	25.0%	25.6%
	No. of obs.	6,160	6,160	6,160	6,160
#2	Y = Emp. growth	0.023*	0.053**	−0.018	−0.030
		0.012	*0.022*	*0.037*	*0.044*
	R^2	27.4%	25.9%	24.5%	26.1%
	No. of obs.	6,160	6,160	6,160	6,160
#3	Y = Sales growth	0.041***	0.059**	−0.010	0.004
		0.013	*0.023*	*0.038*	*0.044*
	R^2	27.1%	24.8%	24.8%	26.1%
	No. of obs.	6,074	6,074	6,074	6,074
B. Naïve OLS (Y on D), conditional on survival					
#1	Y = Emp. growth	0.022*	0.017	−0.034	−0.065**
		0.012	*0.018*	*0.023*	*0.029*
	No. of obs.	6,159	6,036	5,580	4,739
#2	Y = Sales growth	0.039***	0.023	−0.024	0.007
		0.013	*0.019*	*0.022*	*0.028*
	No. of obs.	6,073	5,951	5,501	4,675

Table 3. Startup Survival and Growth: ITT Effects.

The table reports intention-to-treat (ITT) estimates of the effects of being born in the Great Recession on a startup's likelihood of survival, its employment growth, and its sales growth over windows of 1, 3, 5, and 7 years following the startup's first-action date. Panel A reports the results of estimating equation (2), that is, Y on Z_1. The remaining panels allow for Z_1 not to be as good as randomly assigned by using the predicted time of the first-action decision, Z_2, as an instrument for the actual time of the first-action decision, Z_1. Panel B reports the first-stage, Z_1 on Z_2. The weak-instrument F-test uses the Kleibergen-Paap *rk* statistic. Panels C and D report bias-corrected ITT effects (equation (5)) in the full sample and in the sample of surviving startups, respectively, estimated via 2SLS using Z_2 to instrument for Z_1. All specifications include art-unit-by-application-year and headquarter-state fixed effects. In addition, the specifications for survival and employment growth control for log employment in the year of first-action, while those for sales growth control for log sales in the year of first-action. The number of observations falls short of 6,946 startups due to singletons; in the sales-growth specifications, it is further reduced due to missing sales data in NETS. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

		Startup survival and growth over			
		1 year (1)	3 years (2)	5 years (3)	7 years (4)
A. Intention-to-treat (Y on Z_1)					
#1	$Y = \mathbb{1}$(Survival)	−0.002	−0.007	0.031	0.069***
		0.004	*0.013*	*0.020*	*0.022*
	R^2	20.0%	24.8%	26.1%	26.6%
	No. of obs.	6,160	6,160	6,160	6,160
#2	Y = Emp. growth	0.032**	0.013	0.091*	0.184***
		0.016	*0.035*	*0.051*	*0.057*
	R^2	23.9%	25.4%	25.5%	26.7%
	No. of obs.	6,160	6,160	6,160	6,160
#3	Y = Sales growth	0.027	−0.016	0.067	0.197***
		0.018	*0.035*	*0.052*	*0.060*
	R^2	23.1%	25.0%	25.8%	26.7%
	No. of obs.	6,074	6,074	6,074	6,074
B. First-stage (Z_1 on Z_2)					
#1	$Z_1 = \mathbb{1}$(Recession)	0.349***	0.349***	0.349***	0.349***
		0.025	*0.025*	*0.025*	*0.025*
	F-test: IV = 0	187.7	187.7	187.7	187.7
	No. of obs.	6,160	6,160	6,160	6,160
C. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$)					
#1	$Y = \mathbb{1}$(Survival)	0.010	−0.009	0.005	0.121*
		0.013	*0.035*	*0.059*	*0.068*
	No. of obs.	6,160	6,160	6,160	6,160
#2	Y = Emp. growth	0.073	0.072	0.037	0.352**
		0.054	*0.103*	*0.151*	*0.167*
	No. of obs.	6,160	6,160	6,160	6,160
#3	Y = Sales growth	0.063	0.063	0.016	0.357**
		0.058	*0.107*	*0.152*	*0.170*
	No. of obs.	6,074	6,074	6,074	6,074

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Table 3
Continued

		Startup survival and growth over			
		1 year	3 years	5 years	7 years
		(1)	(2)	(3)	(4)
D. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$), conditional on survival					
#1	Y = Emp. growth	0.056	0.112	0.037	0.113
		0.045	*0.070*	*0.092*	*0.111*
	No. of obs.	6,125	5,609	4,835	4,003
#2	Y = Sales growth	0.046	0.089	−0.023	0.068
		0.050	*0.077*	*0.099*	*0.125*
	No. of obs.	6,039	5,527	4,764	3,947

Table 4. Follow-on Innovation: ITT Effects.

The table reports bias-corrected intention-to-treat (ITT) estimates (equation (5)) of the effects of being born in the Great Recession on five measures of a startup's follow-on innovation measured over the 5 years following the startup's first-action date. Panels A and B report results for the full sample and for the sample of surviving startups, respectively, estimated via 2SLS using Z_2 to instrument for Z_1. The first-stage estimates are not shown to conserve space. The weak-instrument F-tests use the Kleibergen-Paap *rk* statistic. All specifications include art-unit-by-application-year and headquarter-state fixed effects. The number of observations in column 1 falls short of 6,946 startups due to singletons; the remaining columns show intensive-margin results for the subsample of startups with follow-on patents. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

	Follow-on patents		Breakthroughness	Citations	
	$\mathbb{1}$(Follow-on patent)	ln(patents)	Mean rank	ln(total)	ln(mean)
	(1)	(2)	(3)	(4)	(5)
A. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$)					
ITT: $\widehat{Z}_1$	0.073	−0.132	0.165**	−0.090	0.075
	0.073	*0.288*	*0.071*	*0.600*	*0.373*
F-test: IV = 0	187.5	54.8	52.0	49.4	49.4
No. of obs.	6,160	1,964	1,878	1,454	1,454
B. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$), conditional on survival					
ITT: $\widehat{Z}_1$	0.089	−0.201	0.191**	−0.206	0.114
	0.093	*0.304*	*0.076*	*0.660*	*0.414*
F-test: IV = 0	131.4	49.1	47.0	38.7	38.7
No. of obs.	4,835	1,774	1,694	1,316	1,316

Table 5. Funding: ITT Effects.

The table reports bias-corrected intention-to-treat (ITT) estimates (equation (5)) of the effects of being born in the Great Recession on 10 measures of startup funding over windows of 1, 3, 5, and 7 years following the startup's first-action date. All specifications are estimated via 2SLS using Z_2 to instrument for Z_1. The first-stage estimates are not shown to conserve space. The weak-instrument F-tests use the Kleibergen-Paap rk statistic. All specifications include art-unit-by-application-year and headquarter-state fixed effects. In addition, we include an indicator set equal to 1 if the startup had a PayDex Score of at least 80 in the first-action year (Panel A) the log number of Regulation D private placements before first-action (Panel B), and the log number of VC funding rounds completed before first-action (Panel D). The number of observations in Panel A is constrained by data availability in NETS. In the remaining panels, it falls short of 6,946 startups due to singletons. Panels C and E use the subsamples of startups without a Regulation D private placement and without venture funding prior to first-action, respectively. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

	Startup funding over			
	1 year (1)	3 years (2)	5 years (3)	7 years (4)
A. $\mathbb{1}$(PayDex Score $\geq$ 80)				
ITT: $\widehat{Z}_1$	−0.198**	−0.144	−0.096	0.001
	0.095	*0.106*	*0.086*	*0.088*
F-test: IV = 0	71.5	71.5	71.5	71.5
No. of obs.	1,770	1,770	1,770	1,770
B. $\mathbb{1}$(Reg. D private placement)				
ITT: $\widehat{Z}_1$	−0.006	−0.021	−0.047	−0.035
	0.037	*0.044*	*0.050*	*0.050*
F-test: IV = 0	187.3	187.3	187.3	187.3
No. of obs.	6,160	6,160	6,160	6,160
C. $\mathbb{1}$(First Reg. D private placement)				
ITT: $\widehat{Z}_1$	−0.021	−0.055	−0.069	−0.052
	0.027	*0.039*	*0.049*	*0.049*
F-test: IV = 0	170.2	170.2	170.2	170.2
No. of obs.	5,147	5,147	5,147	5,147
D. $\mathbb{1}$(VC funding)				
ITT: $\widehat{Z}_1$	0.016	0.027	0.021	0.018
	0.030	*0.033*	*0.033*	*0.034*
F-test: IV = 0	186.8	186.8	186.8	186.8
No. of obs.	6,160	6,160	6,160	6,160

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Table 5
Continued

	Startup funding over			
	1 year (1)	3 years (2)	5 years (3)	7 years (4)
E. 𝟙(First VC funding)				
ITT: $\widehat{Z}_1$	−0.005	0.015	0.011	0.007
	0.019	*0.027*	*0.027*	*0.028*
F-test: IV = 0	173.8	173.8	173.8	173.8
No. of obs.	5,471	5,471	5,471	5,471
F. 𝟙(First patent as collateral)				
ITT: $\widehat{Z}_1$	0.031	0.002	0.002	0.011
	0.023	*0.036*	*0.047*	*0.048*
F-test: IV = 0	187.5	187.5	187.5	187.5
No. of obs.	6,160	6,160	6,160	6,160
G. 𝟙(Any patent as collateral)				
ITT: $\widehat{Z}_1$	0.026	0.007	0.002	0.013
	0.023	*0.037*	*0.047*	*0.049*
F-test: IV = 0	187.5	187.5	187.5	187.5
No. of obs.	6,160	6,160	6,160	6,160
H. 𝟙(Sale of first patent)				
ITT: $\widehat{Z}_1$	−0.016	−0.024	−0.083*	−0.039
	0.022	*0.043*	*0.049*	*0.059*
F-test: IV = 0	187.5	187.5	187.5	187.5
No. of obs.	6,160	6,160	6,160	6,160
I. 𝟙(Sale of any patent)				
ITT: $\widehat{Z}_1$	−0.037	−0.038	−0.096*	−0.071
	0.028	*0.047*	*0.051*	*0.063*
F-test: IV = 0	187.5	187.5	187.5	187.5
No. of obs.	6,160	6,160	6,160	6,160
J. 𝟙(IPO fundraising)				
ITT: $\widehat{Z}_1$	0.004	0.014**	0.013*	0.034***
	0.004	*0.007*	*0.007*	*0.012*
F-test: IV = 0	186.4	186.4	186.4	186.4
No. of obs.	6,160	6,160	6,160	6,160

Table 6. Inventor Mobility, Hiring, and Separation: ITT Effects.

The table reports bias-corrected intention-to-treat (ITT) estimates (equation (5)) of the effects of being born in the Great Recession on inventor mobility, hiring, and separation at startups over windows of 1, 3, 5, and 7 years following the startup's first-action date. The unit of observation in Panel A is a founding inventor; in the remaining panels, the unit of observation is a startup. All specifications are estimated via 2SLS using Z_2 to instrument for Z_1. The first-stage estimates are not shown to conserve space. The weak-instrument F-tests use the Kleibergen-Paap *rk* statistic. All specifications include art-unit-by-application-year and headquarter-state fixed effects. In addition, Panel A controls for a founding inventor's productivity and the log number of years since her first patent, Panels B and C for the log number of founding inventors and their mean productivity at first-action, and Panels D, E, and F for the log number of inventors and their mean productivity at first-action. The number of observations falls short of 6,946 startups due to data requirements to construct inventors' employment spells based on their patenting activities and because some inventors leave their startup before the first-action decision; it is further reduced due to singletons. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

	Horizon			
	1 year (1)	3 years (2)	5 years (3)	7 years (4)
A. $\mathbb{1}$(Founding inventor departs) — inventor level				
ITT: $\widehat{Z}_1$	−0.148**	−0.145	−0.121	−0.200*
	0.075	*0.100*	*0.106*	*0.108*
F-test: IV = 0	84.2	84.2	84.2	84.2
No. of obs.	4,494	4,494	4,494	4,494
B. $\mathbb{1}$(Founding inventor departs) — startup level				
ITT: $\widehat{Z}_1$	−0.223**	−0.250**	−0.185	−0.216*
	0.101	*0.123*	*0.136*	*0.129*
F-test: IV = 0	88.0	88.0	88.0	88.0
No. of obs.	2,192	2,192	2,192	2,192
C. Separation rate of founding inventors				
ITT: $\widehat{Z}_1$	−0.437**	−0.397*	−0.256	−0.552*
	0.186	*0.229*	*0.256*	*0.295*
F-test: IV = 0	88.0	88.0	88.0	88.0
No. of obs.	2,192	2,192	2,192	2,192
D. Growth rate of founding and non-founding inventors				
ITT: $\widehat{Z}_1$	0.337*	0.383*	0.396	0.351
	0.191	*0.227*	*0.259*	*0.260*
F-test: IV = 0	109.4	109.4	109.4	109.4
No. of obs.	2,379	2,379	2,379	2,379

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Table 6
Continued

	Horizon			
	1 year (1)	3 years (2)	5 years (3)	7 years (4)
E. Hiring rate of non-founding inventors				
ITT: $\widehat{Z}_1$	−0.030	0.056	0.042	−0.005
	0.068	*0.108*	*0.137*	*0.154*
F-test: IV = 0	109.4	109.4	109.4	109.4
No. of obs.	2,379	2,379	2,379	2,379
F. Separation rate of non-founding inventors				
ITT: $\widehat{Z}_1$	0.023	0.058	0.038	0.097
	0.044	*0.069*	*0.081*	*0.106*
F-test: IV = 0	109.4	109.4	109.4	109.4
No. of obs.	2,379	2,379	2,379	2,379

Table 7. Inventor Productivity: ITT Effects.

The table reports bias-corrected intention-to-treat (ITT) estimates (equation (5)) of the effects of being born in the Great Recession on the productivity of non-founding inventors hired over windows of 1, 3, 5, and 7 years following the startup's first-action date. All specifications are estimated via 2SLS using Z_2 to instrument for Z_1. The first-stage estimates are not shown to conserve space. The weak-instrument F-tests use the Kleibergen-Paap *rk* statistic. All specifications include art-unit-group-by-application-year and headquarter-state fixed effects. In addition, they control for the log number of founding and non-founding inventors and their mean productivity at first-action. The number of observations falls short of 6,946 startups due to data requirements to construct inventors' employment spells based on their patenting activities and because some startups do not hire any non-founding inventors; it is further reduced due to singletons. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

	Productivity of non-founding inventors hired at startups over			
	1 year (1)	3 years (2)	5 years (3)	7 years (4)
ITT: $\widehat{Z}_1$	1.775*	1.498*	1.242	0.684
	0.940	*0.896*	*0.935*	*1.014*
F-test: IV = 0	32.8	38.6	34.1	25.8
No. of obs.	991	1,198	1,103	841

Table 8. Startup Survival and Growth: Testing the Labor-Demand Channel.

The table reports 2SLS estimates of the effect of losing one or more founding inventors early in a startup's life on the startup's subsequent likelihood of survival and its growth in employment and sales. The variable of interest is the startup's founding-inventor separation rate, defined as in Table 6 and measured over the 2 years from the startup's first-action date. (When measured over shorter periods, results are qualitatively similar but considerably noisier.) Outcomes are measured over windows of 3, 5, and 7 years. We instrument the separation rate using the change in labor demand for R&D workers in the startup's technology field during the 2 years from its first-action date. Panel A reports the first-stage estimate of the effect of the change in labor demand on the startup's founding-inventor separation rate. Panel B reports the second-stage estimates for our three outcome variables. All specifications include art-unit-by-application-year and headquarter-state fixed effects. In addition, the specifications for survival and employment growth control for log employment in the year of first-action, while those for sales growth control for log sales in the year of first-action. The weak-instrument F-test uses the Kleibergen-Paap *rk* statistic. The number of observations falls short of 6,946 startups due to data requirements to construct inventors' employment spells based on their prior patenting activities and because some inventors leave their startup before the first-action decision; it is further reduced due to singletons. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

		Startup survival and growth over		
		3 years (1)	5 years (2)	7 years (3)
A. First-stage				
#1	Y = Separation rate	7.184***	7.184***	7.184***
		1.906	*1.906*	*1.906*
	F-test: IV = 0	14.2	14.2	14.2
	No. of obs.	2,193	2,193	2,193
B. Second-stage				
#1	$Y = \mathbb{1}$(Survival)	−0.068	−0.448***	−0.472***
		0.054	*0.146*	*0.151*
	No. of obs.	2,193	2,193	2,193
#2	Y = Emp. growth	−0.020	−0.942**	−1.099**
		0.185	*0.366*	*0.426*
	No. of obs.	2,193	2,193	2,193
#3	Y = Sales growth	−0.000	−1.006**	−1.143**
		0.196	*0.398*	*0.462*
	No. of obs.	2,163	2,163	2,163

Table 9. Startup Survival and Growth: LATE.

The table reports local average treatment (LATE) estimates of the effects of being born in the Great Recession on a startup's likelihood of survival, its employment growth, and its sales growth over windows of 1, 3, 5, and 7 years following the startup's birth. To estimate LATE effects, we restrict the sample to the 2,017 firms that are born in the first-action year or the year after. We use the predicted time of the first-action decision, Z_2, as an instrument for the actual time of the startup's birth, D. Panel A reports the first-stage, D on Z_2. The weak-instrument F-test uses the Kleibergen-Paap *rk* statistic. Panels B and C report LATE effects in the full sample and in the sample of surviving startups, respectively, estimated via 2SLS using Z_2 to instrument for D. All specifications include art-unit-group-by-application-year and headquarter-state fixed effects. In addition, the specifications for survival and employment growth control for log employment in the year of birth, while those for sales growth control for log sales in the year of birth. The number of observations falls short of 2,017 startups due to singletons; in the sales-growth specifications, it is further reduced due to missing sales data in NETS. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

	Startup survival and growth over			
	1 year (1)	3 years (2)	5 years (3)	7 years (4)
A. First-stage (D on Z_2)				
#1 $D = \mathbb{1}$(Recession)	0.255***	0.255***	0.255***	0.255***
	0.037	*0.037*	*0.037*	*0.037*
F-test: IV = 0	46.5	46.5	46.5	46.5
No. of obs.	1,878	1,878	1,878	1,878
B. LATE (Y on $\widehat{D}$)				
#1 $Y = \mathbb{1}$(Survival)	0.021	0.158*	0.104	0.311**
	0.033	*0.083*	*0.143*	*0.151*
No. of obs.	1,878	1,878	1,878	1,878
#2 Y = Emp. growth	0.034	0.336	0.309	0.828**
	0.100	*0.211*	*0.353*	*0.374*
No. of obs.	1,878	1,878	1,878	1,878
#3 Y = Sales growth	0.086	0.379*	0.366	0.904**
	0.107	*0.216*	*0.356*	*0.376*
No. of obs.	1,878	1,878	1,878	1,878
C. LATE (Y on $\widehat{D}$), conditional on survival				
#1 Y = Emp. growth	−0.012	−0.040	0.091	0.060
	0.075	*0.149*	*0.196*	*0.207*
No. of obs.	1,861	1,732	1,447	1,180
#2 Y = Sales growth	0.042	−0.001	0.146	0.157
	0.084	*0.156*	*0.210*	*0.224*
No. of obs.	1,861	1,732	1,447	1,180

INTERNET APPENDIX

for

Great Recession Babies:

How Are Startups Shaped by Macro Conditions at Birth?

(NOT INTENDED FOR PUBLICATION)

Figure IA.1. Residual First-Action Examination Time.

The figure shows the distribution of the time from patent application to the “first office action on the merits” (first-action) decision within technology field and application year. The figure plots the distribution of residual first-action examination time estimated on the universe of 2,878,069 patent applications filed between 2002 and 2009, controlling for art-unit-by-application-year fixed effects. For variable definitions and details of their construction see Appendix A.

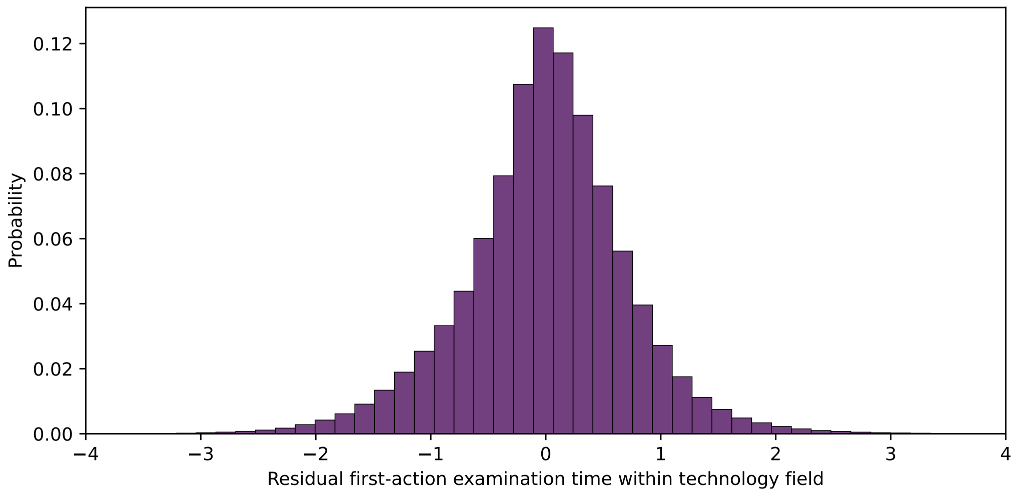

Figure IA.2. Examiner Review Speed by Application Year.

The figure shows plots regression coefficients of examiner review speed (in months) on indicator variables for applications filed in 2002, 2003, 2004, 2005, 2007, 2008, and 2009. The omitted reference group is applications filed in 2006. The OLS regression is estimated on the universe of 2,878,069 patent applications filed between 2002 and 2009 and controls for art unit fixed effects. Standard errors are clustered at the art unit level. The vertical lines indicate 95% confidence intervals. For variable definitions and details of their construction see Appendix A.

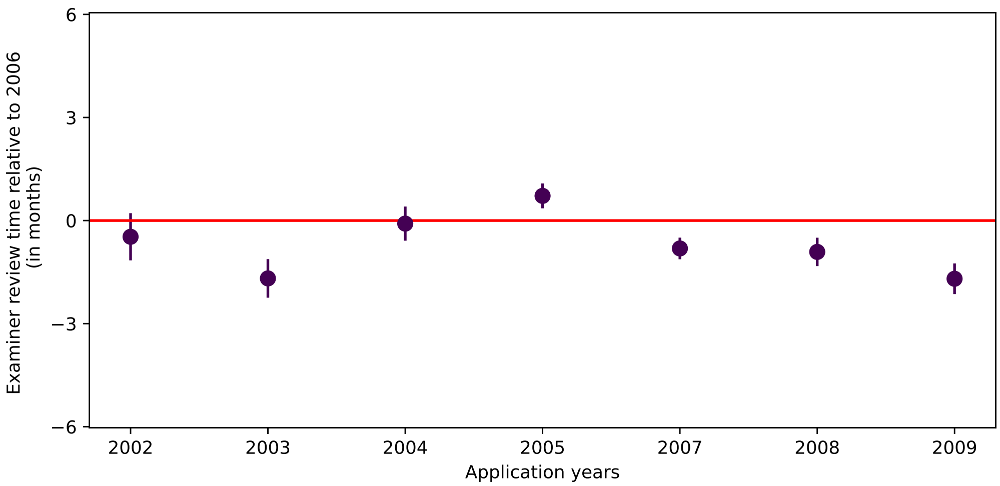

Table IA.1. Examination Practices During the Great Recession.

The table reports the relative likelihood that an examiner handles the patent application of a startup with a certain characteristic according to date-order priority during the Great Recession. Following the approach of Angrist and Pischke (2009, Section 4.4.4), the baseline likelihood of an examiner handling applications in date order is estimated via the first-stage of the Wald estimator (Z_1 on Z_2) in the full sample of 6,946 startups. The likelihood of an examiner handling patent applications with a certain characteristic in date order is estimated via the first-stage of the Wald estimator (Z_1 on Z_2) in the subsample of startups with that characteristic. The relative likelihood is then computed as the ratio of the first-stage estimates in the subsample and the full sample. To test whether the relative likelihood is statistically different from 1, we construct non-parametric confidence intervals based on 1,000 bootstraps clustering standard errors at the art unit level. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

		First-stage of Wald estimator			Non-parametric test		
	Mean	Full sample	Subsample	Relative likelihood	95% confidence interval		Significance level
$\mathbb{1}$(Single founding inventor)	0.44	0.55	0.52	0.95	0.90	1.01	*
$\mathbb{1}$(Founding inventor's first patent filing)	0.44	0.55	0.52	0.96	0.91	1.01	*
$\mathbb{1}$(Founding inventor productivity in bottom 25%)	0.09	0.55	0.44	0.80	0.66	0.95	**
$\mathbb{1}$(Founding inventor productivity in bottom 50%)	0.20	0.55	0.48	0.88	0.79	0.96	**
$\mathbb{1}$(Founding inventor productivity in top 50%)	0.80	0.55	0.57	1.03	1.01	1.06	**
$\mathbb{1}$(Founding inventor productivity in top 25%)	0.62	0.55	0.57	1.05	1.01	1.09	**
$\mathbb{1}$(Prior breakthrough patent)	0.26	0.55	0.59	1.09	1.02	1.16	**
$\mathbb{1}$(Pro se applicant)	0.09	0.55	0.52	0.96	0.84	1.09	

Table IA.2. Balance Test: Recession vs. Expansion Startups Based on $\widehat{Z}_1$.

The table reports a balance test comparing sample startups according to $\widehat{Z}_1$. $\widehat{Z}_1$ distinguishes the 708 startups that receive the first-action decision on their first patent application in the Great Recession ($Z_1 = 1$) and are predicted to receive the first-action decision in the Great Recession based on the examiner's historic review speed ($Z_2 = 1$) to the 5,323 startups that receive the first-action decision on their first patent application in the expansion ($Z_1 = 0$) and are predicted to receive the first-action in the expansion given the examiner's historic review speed ($Z_2 = 0$). For variable definitions and details of their construction see Appendix A. To test whether startups in the two groups differ on observables, we use a t-test of equal means after controlling for art-unit-by-application-year fixed effects and clustering the standard errors at the art unit level. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

	Recession ($\widehat{Z}_1 = 1$)			Expansion ($\widehat{Z}_1 = 0$)			Adj. differences
	Mean	P50	SD	Mean	P50	SD	t-stat
Age at first-action	1.96	2.00	1.40	2.02	2.00	1.33	0.50
Employees at first-action	12.67	3.00	97.28	24.68	3.00	610.92	–0.93
Sales at first-action ($ million)	2.31	0.28	19.81	8.55	0.31	369.31	–1.09
$\mathbb{1}$(PayDex score $\geq$ 80) at first-action	0.36	0.00	0.48	0.34	0.00	0.47	–0.64
$\mathbb{1}$(Reg. D private placement) at first-action	0.15	0.00	0.36	0.14	0.00	0.35	–0.09
$\mathbb{1}$(VC funding) at first-action	0.11	0.00	0.32	0.09	0.00	0.29	0.27
$\mathbb{1}$(Founding inventor's first patent filing)	0.43	0.00	0.50	0.44	0.00	0.50	0.67
Years since founding inventor's first patent	5.60	2.00	7.49	5.62	1.67	7.59	–1.12
$\mathbb{1}$(Single founding inventor)	0.42	0.00	0.49	0.45	0.00	0.50	0.69
No. of founding inventors	2.13	2.00	1.42	2.06	2.00	1.36	–0.65
Founding inventor productivity	7.47	8.12	2.67	7.48	8.26	2.70	1.27
$\mathbb{1}$(Prior breakthrough patent)	0.29	0.00	0.46	0.26	0.00	0.44	–0.39
Breakthroughness rank of prior patent(s)	0.54	0.55	0.24	0.53	0.54	0.23	–0.80
$\mathbb{1}$(Pro se applicant)	0.10	0.00	0.29	0.09	0.00	0.28	1.19
Log(No. of attorney's prior applications)	6.97	7.08	1.54	6.93	7.05	1.57	–0.87

Table IA.3. Startup Survival and Growth: ITT Effects Controlling for Review Speed.

The table reports bias-corrected intention-to-treat (ITT) estimates of the effects of being born in the Great Recession on a startup's likelihood of survival, its employment growth, and its sales growth over windows of 1, 3, 5, and 7 years following the startup's first-action date controlling for the effects of review speed. Panel A reports the first-stage, Z_1 on Z_2, controlling for the first-action examination time $t_{examination_i}$. The weak-instrument F-test uses the Kleibergen-Paap *rk* statistic. Panels B and C report bias-corrected ITT effects (equation (5)) in the full sample and in the sample of surviving startups, respectively, estimated via 2SLS using Z_2 to instrument for Z_1 and the examiner's historic review speed plus the application-specific time between application and docket to instrument for the first-action examination time $t_{examination_i}$. All specifications include art-unit-by-application-year and headquarter-state fixed effects. In addition, the specifications for survival and employment growth control for log employment in the year of first-action, while those for sales growth control for log sales in the year of first-action. The number of observations falls short of 6,946 startups due to singletons; in the sales-growth specifications, it is further reduced due to missing sales data in NETS. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

		Startup survival and growth over			
		1 year	3 years	5 years	7 years
		(1)	(2)	(3)	(4)
A. First-stage (Z_1 on Z_2)					
#1	$Z_1 = \mathbb{1}$(Recession)	0.349***	0.349***	0.349***	0.349***
		0.025	*0.025*	*0.025*	*0.025*
	F-test: IV = 0	188.3	188.3	188.3	188.3
	No. of obs.	6,160	6,160	6,160	6,160
B. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$)					
#1	$Y = \mathbb{1}$(Survival)	0.010	−0.009	0.005	0.121*
		0.013	*0.035*	*0.059*	*0.068*
	No. of obs.	6,160	6,160	6,160	6,160
#2	Y = Emp. growth	0.074	0.073	0.037	0.352**
		0.054	*0.104*	*0.151*	*0.168*
	No. of obs.	6,160	6,160	6,160	6,160
#3	Y = Sales growth	0.063	0.063	0.016	0.356**
		0.058	*0.107*	*0.152*	*0.171*
	No. of obs.	6,074	6,074	6,074	6,074
C. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$), conditional on survival					
#1	Y = Emp. growth	0.057	0.113	0.039	0.115
		0.045	*0.070*	*0.093*	*0.112*
	No. of obs.	6,125	5,609	4,835	4,003
#2	Y = Sales growth	0.046	0.090	−0.023	0.066
		0.050	*0.077*	*0.100*	*0.126*
	No. of obs.	6,039	5,527	4,764	3,947

Table IA.4. Startup Survival and Growth: ITT Effects Controlling for Patent Scope.

The table reports bias-corrected intention-to-treat (ITT) estimates of the effects of being born in the Great Recession on a startup's likelihood of survival, its employment growth, and its sales growth over windows of 1, 3, 5, and 7 years following the startup's first-action date controlling for the effects of patent scope. Panel A reports the first-stage, Z_1 on Z_2, controlling for patent scope. The weak-instrument F-test uses the Kleibergen-Paap *rk* statistic. Panels B and C report bias-corrected ITT effects (equation (5)) in the full sample and in the sample of surviving startups, respectively, estimated via 2SLS using Z_2 to instrument for Z_1 and the examiner's historic scope leniency for patent scope. All specifications include art-unit-by-application-year and headquarter-state fixed effects. In addition, the specifications for survival and employment growth control for log employment in the year of first-action, while those for sales growth control for log sales in the year of first-action. The number of observations falls short of 6,946 startups due to singletons and missing patent claim data needed to construct patent scope; in the sales-growth specifications, it is further reduced due to missing sales data in NETS. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

		Startup survival and growth over			
		1 year (1)	3 years (2)	5 years (3)	7 years (4)
A. First-stage (Z_1 on Z_2)					
#1	$Z_1 = \mathbb{1}$(Recession)	0.345***	0.345***	0.345***	0.345***
		0.025	*0.025*	*0.025*	*0.025*
	F-test: IV = 0	184.2	184.2	184.2	184.2
	No. of obs.	6,044	6,044	6,044	6,044
B. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$)					
#1	$Y = \mathbb{1}$(Survival)	0.010	−0.006	0.017	0.133*
		0.013	*0.036*	*0.060*	*0.073*
	No. of obs.	6,044	6,044	6,044	6,044
#2	Y = Emp. growth	0.070	0.068	0.049	0.372**
		0.055	*0.104*	*0.153*	*0.179*
	No. of obs.	6,044	6,044	6,044	6,044
#3	Y = Sales growth	0.060	0.065	0.028	0.372**
		0.059	*0.108*	*0.157*	*0.186*
	No. of obs.	5,959	5,959	5,959	5,959
C. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$), conditional on survival					
#1	Y = Emp. growth	0.051	0.092	0.018	0.093
		0.046	*0.072*	*0.094*	*0.116*
	No. of obs.	6,009	5,498	4,739	3,922
#2	Y = Sales growth	0.042	0.077	−0.033	0.063
		0.051	*0.079*	*0.102*	*0.130*
	No. of obs.	5,924	5,417	4,669	3,868

Table IA.5. Startup Survival and Growth: Robustness to Unobserved Examiner Habits.

The table reports bias-corrected intention-to-treat (ITT) estimates of the effects of being born in the Great Recession on a startup's likelihood of survival, its employment growth, and its sales growth over windows of 1, 3, 5, and 7 years. We investigate the concern that the examiner's predicted review speed (of which our instrument, Z_2, is a non-monotonic function) potentially correlates with unobserved examiner habits that could affect outcomes of interest in unexpected ways We do so by replacing the examiner's predicted review speed with the art unit's average review speed in the construction of the instrument. with the art unit's average review speed when constructing the instrument. Specifically, we predict whether or not each startup's patent decision is issued in the recession based on the sum of the application date, the application-specific administrative lag from the time the application is filed to the time it is docketed with an examiner, and (unlike in Table 3) the average historical review speed across all examiners in the art unit. Panel A reports the first-stage. The weak-instrument F-test uses the Kleibergen-Paap *rk* statistic. Panels B and C report bias-corrected ITT effects (equation (5)) in the full sample and in the sample of surviving startups, respectively, estimated via 2SLS using the alternative version of Z_2 to instrument for Z_1. All specifications include art-unit-by-application-year and headquarter-state fixed effects. In addition, the specifications for survival and employment growth control for log employment in the year of first-action, while those for sales growth control for log sales in the year of first-action. The number of observations falls short of 6,946 startups due to singletons; in the sales-growth specifications, it is further reduced due to missing sales data in NETS. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

		Startup survival and growth over			
		1 year (1)	3 years (2)	5 years (3)	7 years (4)
A. First-stage (Z_1 on $Z_{2,alternative}$)					
#1	$Z_1 = \mathbb{1}$(Recession)	0.341***	0.341***	0.341***	0.341***
		0.028	*0.028*	*0.028*	*0.028*
	F-test: IV = 0	150.8	150.8	150.8	150.8
	No. of obs.	6,160	6,160	6,160	6,160
B. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$)					
#1	$Y = \mathbb{1}$(Survival)	0.023*	0.049	0.081	0.222***
		0.012	*0.038*	*0.071*	*0.079*
	No. of obs.	6,160	6,160	6,160	6,160
#2	Y = Emp. growth	0.095**	0.274**	0.225	0.595***
		0.045	*0.112*	*0.169*	*0.188*
	No. of obs.	6,160	6,160	6,160	6,160
#3	Y = Sales growth	0.084*	0.280**	0.245	0.661***
		0.050	*0.115*	*0.173*	*0.198*
	No. of obs.	6,074	6,074	6,074	6,074
C. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$), conditional on survival					
#1	Y = Emp. growth	0.047	0.188**	0.065	0.166
		0.044	*0.073*	*0.098*	*0.139*
	No. of obs.	6,125	5,609	4,835	4,003
#2	Y = Sales growth	0.035	0.182**	0.053	0.196
		0.049	*0.080*	*0.107*	*0.147*
	No. of obs.	6,039	5,527	4,764	3,947

Table IA.6. Startup Survival and Growth: Robustness to Time-Invariant Examiner Characteristics.

The table reports bias-corrected intention-to-treat (ITT) estimates of the effects of being born in the Great Recession on a startup's likelihood of survival, its employment growth, and its sales growth over windows of 1, 3, 5, and 7 years following the startup's first-action date. We investigate the concern that the examiner's predicted review speed (of which our instrument, Z_2, is a non-monotonic function) potentially correlates with unobserved examiner habits that could affect outcomes of interest in unexpected ways by including examiner fixed effects. Panel A reports the first-stage, Z_1 on Z_2. The weak-instrument F-test uses the Kleibergen-Paap *rk* statistic. Panels B and C report bias-corrected ITT effects (equation (5)) in the full sample and in the sample of surviving startups, respectively, estimated via 2SLS using Z_2 to instrument for Z_1. All specifications include art-unit-by-application-year, headquarter-state, and patent-examiner fixed effects. In addition, the specifications for survival and employment growth control for log employment in the year of first-action, while those for sales growth control for log sales in the year of first-action. The number of observations falls short of 6,946 startups due to singletons; in the sales-growth specifications, it is further reduced due to missing sales data in NETS. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

		Startup survival and growth over			
		1 year (1)	3 years (2)	5 years (3)	7 years (4)
A. First-stage (Z_1 on Z_2)					
#1	$Z_1 = \mathbb{1}$(Recession)	0.380***	0.380***	0.380***	0.380***
		0.041	*0.041*	*0.041*	*0.041*
	F-test: IV = 0	84.9	84.9	84.9	84.9
	No. of obs.	4,311	4,311	4,311	4,311
B. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$)					
#1	$Y = \mathbb{1}$(Survival)	0.007	0.024	0.006	0.178
		0.019	*0.058*	*0.110*	*0.123*
	No. of obs.	4,311	4,311	4,311	4,311
#2	Y = Emp. growth	0.073	0.209	0.128	0.581**
		0.082	*0.153*	*0.272*	*0.287*
	No. of obs.	4,311	4,311	4,311	4,311
#3	Y = Sales growth	0.067	0.204	0.127	0.564*
		0.095	*0.166*	*0.286*	*0.307*
	No. of obs.	4,232	4,232	4,232	4,232
C. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$), conditional on survival					
#1	Y = Emp. growth	0.061	0.239**	0.173	0.349
		0.067	*0.116*	*0.164*	*0.250*
	No. of obs.	4,286	3,840	3,155	2,441
#2	Y = Sales growth	0.055	0.251*	0.121	0.050
		0.082	*0.136*	*0.188*	*0.295*
	No. of obs.	4,207	3,764	3,088	2,391

Table IA.7. Startup Survival and Growth: ITT Effects Distinguishing Expansion, Slowdown, Recession, and Recovery.

The table reports bias-corrected intention-to-treat (ITT) estimates of the effects of being born in the year before the Great Recession ("slowdown"), during the Great Recession, or in the year after the Great Recession ("recovery") on a startup's likelihood of survival, its employment growth, and its sales growth over windows of 1, 3, 5, and 7 years following the startup's first-action date. The omitted reference group is the expansion period from January 2002 to November 2006. Panel A reports the three first-stages, Z_1 on Z_2. The weak-instrument F-tests use the Kleibergen-Paap rk statistic. Panels B and C report bias-corrected ITT effects (equation (5)) in the full sample and in the sample of surviving startups, respectively, estimated via 2SLS using $Z_{2,slowdown}$, $Z_{2,recession}$, and $Z_{2,recovery}$ to instrument for $Z_{1,slowdown}$, $Z_{1,recession}$, and $Z_{1,recovery}$, respectively. All specifications include art-unit-by-application-year and headquarter-state fixed effects. In addition, the specifications for survival and employment growth control for log employment in the year of first-action, while those for sales growth control for log sales in the year of first-action. The number of observations falls short of 6,946 startups due to singletons; in the sales-growth specifications, it is further reduced due to missing sales data in NETS. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

			Startup growth and survival over			
			1 year (1)	3 years (2)	5 years (3)	7 years (4)
A. First-stages (Z_1 on Z_2)						
#1	$Z_1 = \mathbb{1}$(Slowdown)	$Z_2 = \mathbb{1}$(Slowdown)	0.250***	0.250***	0.250***	0.250***
			0.031	*0.031*	*0.031*	*0.031*
		F-test: IV = 0	65.7	65.7	65.7	65.7
		No. of obs.	6,160	6,160	6,160	6,160
#2	$Z_1 = \mathbb{1}$(Recession)	$Z_2 = \mathbb{1}$(Recession)	0.349***	0.349***	0.349***	0.349***
			0.025	*0.025*	*0.025*	*0.025*
		F-test: IV = 0	187.7	187.7	187.7	187.7
		No. of obs.	6,160	6,160	6,160	6,160
#3	$Z_1 = \mathbb{1}$(Recovery)	$Z_2 = \mathbb{1}$(Recovery)	0.226***	0.226***	0.226***	0.226***
			0.023	*0.023*	*0.023*	*0.023*
		F-test: IV = 0	95.1	95.1	95.1	95.1
		No. of obs.	6,160	6,160	6,160	6,160

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Table IA.7
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			Startup growth and survival over			
			1 year (1)	3 years (2)	5 years (3)	7 years (4)
B. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$)						
#1	$Y = \mathbb{1}$(Survival)	$\widehat{Z}_1$: $\mathbb{1}$(Slowdown)	0.002	0.080	0.233**	0.194
			0.012	*0.067*	*0.097*	*0.127*
		$\widehat{Z}_1$: $\mathbb{1}$(Recession)	0.010	−0.003	0.041	0.151**
			0.010	*0.033*	*0.059*	*0.066*
		$\widehat{Z}_1$: $\mathbb{1}$(Recovery)	−0.001	−0.057	−0.018	−0.020
			0.021	*0.071*	*0.082*	*0.103*
		F-test: IV = 0	29.9	29.9	29.9	29.9
		No. of obs.	6,160	6,160	6,160	6,160
#2	Y = Emp. growth	$\widehat{Z}_1$: $\mathbb{1}$(Slowdown)	−0.144**	−0.038	0.371	0.315
			0.068	*0.165*	*0.242*	*0.312*
		$\widehat{Z}_1$: $\mathbb{1}$(Recession)	0.068	0.054	0.109	0.414***
			0.052	*0.096*	*0.151*	*0.158*
		$\widehat{Z}_1$: $\mathbb{1}$(Recovery)	0.135*	−0.086	0.074	0.067
			0.080	*0.172*	*0.203*	*0.250*
		F-test: IV = 0	29.9	29.9	29.9	29.9
		No. of obs.	6,160	6,160	6,160	6,160
#3	Y = Sales growth	$\widehat{Z}_1$: $\mathbb{1}$(Slowdown)	−0.119*	−0.028	0.369	0.335
			0.072	*0.168*	*0.239*	*0.299*
		$\widehat{Z}_1$: $\mathbb{1}$(Recession)	0.061	0.042	0.092	0.432***
			0.055	*0.101*	*0.153*	*0.163*
		$\widehat{Z}_1$: $\mathbb{1}$(Recovery)	0.139	−0.128	0.104	0.139
			0.089	*0.177*	*0.210*	*0.264*
		F-test: IV = 0	28.9	28.9	28.9	28.9
		No. of obs.	6,074	6,074	6,074	6,074

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Table IA.7
Continued

			Startup growth and survival over			
			1 year (1)	3 years (2)	5 years (3)	7 years (4)
C. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$), conditional on survival						
#1	Y = Emp. growth	$\widehat{Z}_1$: $\mathbb{1}$(Slowdown)	−0.148**	−0.229*	−0.190	−0.110
			0.066	*0.122*	*0.157*	*0.205*
		$\widehat{Z}_1$: $\mathbb{1}$(Recession)	0.049	0.078	0.023	0.104
			0.044	*0.067*	*0.092*	*0.113*
		$\widehat{Z}_1$: $\mathbb{1}$(Recovery)	0.139**	0.013	0.141	0.198
			0.070	*0.108*	*0.139*	*0.175*
		No. of obs.	6,125	5,609	4,835	4,003
#2	Y = Sales growth	$\widehat{Z}_1$: $\mathbb{1}$(Slowdown)	−0.123*	−0.224*	−0.175	−0.044
			0.070	*0.128*	*0.164*	*0.205*
		$\widehat{Z}_1$: $\mathbb{1}$(Recession)	0.043	0.048	−0.033	0.080
			0.048	*0.071*	*0.096*	*0.128*
		$\widehat{Z}_1$: $\mathbb{1}$(Recovery)	0.143*	−0.039	0.178	0.391*
			0.078	*0.120*	*0.158*	*0.215*
		No. of obs.	6,039	5,527	4,764	3,947

Table IA.8. Startup Survival and Growth: ITT Effects using Continuous Growth.

The table reports bias-corrected intention-to-treat (ITT) estimates of the effects of being born in the Great Recession on a startup's likelihood of survival, its employment growth, and its sales growth over windows of 1, 3, 5, and 7 years following the startup's first-action date. Unlike in Table 3, we use continuous growth rates. Panel A reports the first-stage, Z_1 on Z_2. The weak-instrument F-test uses the Kleibergen-Paap rk statistic. Panels B and C report bias-corrected ITT effects (equation (5)) in the full sample and in the sample of surviving startups, respectively, estimated via 2SLS using Z_2 to instrument for Z_1. All specifications include art-unit-by-application-year and headquarter-state fixed effects. In addition, the specifications for survival and employment growth control for log employment in the year of first-action, while those for sales growth control for log sales in the year of first-action. The number of observations falls short of 6,946 startups due to singletons; in the sales-growth specifications, it is further reduced due to missing sales data in NETS. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

		Startup survival and growth over			
		1 year	3 years	5 years	7 years
		(1)	(2)	(3)	(4)
A. First-stage (Z_1 on Z_2)					
#1	$Z_1 = \mathbb{1}$(Recession)	0.349***	0.349***	0.349***	0.349***
		0.025	*0.025*	*0.025*	*0.025*
	F-test: IV = 0	187.7	187.7	187.7	187.7
	No. of obs.	6,160	6,160	6,160	6,160
B. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$)					
#1	$Y = \mathbb{1}$(Survival)	0.010	−0.009	0.005	0.121*
		0.013	*0.035*	*0.059*	*0.068*
	No. of obs.	6,160	6,160	6,160	6,160
#2	Y = Emp. growth	0.044	0.075	0.038	0.248**
		0.039	*0.075*	*0.102*	*0.110*
	No. of obs.	6,160	6,160	6,160	6,160
#3	Y = Sales growth	0.059	0.076	0.021	0.265**
		0.056	*0.092*	*0.118*	*0.126*
	No. of obs.	6,074	6,074	6,074	6,074
C. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$), conditional on survival					
#1	Y = Emp. growth	0.039	0.105	0.044	0.161
		0.037	*0.067*	*0.096*	*0.116*
	No. of obs.	6,125	5,609	4,835	4,003
#2	Y = Sales growth	0.052	0.100	−0.006	0.122
		0.053	*0.088*	*0.123*	*0.153*
	No. of obs.	6,039	5,527	4,764	3,947

Table IA.9. Intensive Funding Margins: ITT Effects.

The table reports bias-corrected intention-to-treat (ITT) estimates (equation (5)) of the effects of being born in the Great Recession on 12 intensive funding margins over the 5 years following the first-action date, estimated in subsamples consisting of firms that obtain VC funding (Panel A), post a patent as collateral (Panel B), or sell at least one patent (Panel C). We focus on the five-year horizon because the intensive-margin subsamples can get so small that power becomes an issue in the first-stage weak-instrument test. For the five-year horizon, Z_2 is an at least marginally strong instrument for Z_1 in all three subsamples. All specifications are estimated via 2SLS using Z_2 to instrument for Z_1. The first-stage estimates are not shown to conserve space. The weak-instrument F-tests use the Kleibergen-Paap *rk* statistic. All specifications include art-unit-group-by-application-year and headquarter-state fixed effects. In addition, Panel A controls for the log number of VC funding rounds completed before first-action. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

	Intensive margin of startup funding over 5 years			
	(1)	(2)	(3)	(4)
A. VC funding				
Y=	**ln(No. rounds)**	**ln(Amount)**	**ln(Amount per rd.)**	**ln(Time to funding)**
ITT: $\widehat{Z}_1$	−0.509	−1.231	−0.379	−0.099
	0.347	*2.310*	*2.075*	*1.040*
F-test: IV = 0	9.9	9.9	9.9	9.9
No. of obs.	585	585	585	585
B. Collateral lending				
Y=	**ln(No. loans)**	**ln(No. patents)**	**ln(Percentile rank$_{bs}$)**	**ln(Time to loan)**
ITT: $\widehat{Z}_1$	0.320	0.767	0.357*	0.477
	0.390	*0.544*	*0.182*	*0.608*
F-test: IV = 0	13.4	13.4	13.5	13.4
No. of obs.	603	603	602	602
C. Patent sales				
Y=	**ln(No. sales)**	**ln(No. patents)**	**ln(Percentile rank$_{bs}$)**	**ln(Time to sale)**
ITT: $\widehat{Z}_1$	0.571*	0.049	−0.040	0.357
	0.317	*0.347*	*0.123*	*0.463*
F-test: IV = 0	25.8	25.8	25.4	25.8
No. of obs.	1,295	1,295	1,283	1,291

Table IA.10. Testing the Exclusion Restriction.

The table reports the test of the "no first stage, no reduced form" restriction described in Angrist (2022) and applied by Angrist, Lavy, and Schlosser (2010). The exclusion restriction implies that reduced-form effects in samples for which the first-stage is zero should be zero as well. We test this implication in two samples. The first sample is the sample in which only 2.4% of the startups "comply" with the invitation to treatment by starting operations in the year in which they are predicted to receive a positive decision on their patent application. Panel A presents the first-stage estimates and Panel B the reduced-form estimates. The second sample is the subsample of the 2,017 firms born in the first-action year or the year after that are born outside the Great Recession (i.e., $D = 0$, as used in Table 9). Panel C presents the reduced-form estimates. All specifications include art-unit-group-by-application-year and headquarter-state fixed effects. In addition, the specifications for survival and employment growth control for log employment in the year of birth, while those for sales growth control for log sales in the year of birth. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coefficient estimates. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

		Startup survival and growth over			
		1 year	3 years	5 years	7 years
		(1)	(2)	(3)	(4)
A. First-stage (D on Z_2) estimated in the full sample					
#1	$D = \mathbb{1}$(Recession)	0.024	0.024	0.024	0.024
		0.022	*0.022*	*0.022*	*0.022*
	F-test: IV = 0	1.2	1.2	1.2	1.2
	No. of obs.	5,382	5,382	5,382	5,382
B. Reduced-form (Y on Z_2) estimated in the full sample					
#1	$Y = \mathbb{1}$(Survival)	–0.000	0.010	–0.008	0.026
		0.004	*0.009*	*0.015*	*0.019*
	R^2	6.5%	8.7%	10.2%	10.2%
	No. of obs.	5,382	5,382	5,382	5,382
#2	Y = Emp. growth	–0.031**	–0.007	–0.050	0.036
		0.015	*0.027*	*0.040*	*0.049*
	R^2	9.2%	9.1%	9.5%	9.8%
	No. of obs.	5,382	5,382	5,382	5,382
#3	Y = Sales growth	–0.019	0.002	–0.037	0.040
		0.016	*0.028*	*0.041*	*0.050*
	R^2	8.5%	8.4%	9.7%	10.0%
	No. of obs.	5,382	5,382	5,382	5,382
C. Reduced-form (Y on Z_2) estimated in the sample used in Table 9, conditional on $D = 0$					
#1	$Y = \mathbb{1}$(Survival)	–0.012	0.024	0.012	0.058
		0.012	*0.032*	*0.052*	*0.056*
	R^2	18.5%	25.1%	27.4%	26.8%
	No. of obs.	1,372	1,372	1,372	1,372
#2	Y = Emp. growth	–0.035	0.019	0.038	0.161
		0.040	*0.086*	*0.137*	*0.143*
	R^2	26.0%	24.2%	26.7%	25.6%
	No. of obs.	1,372	1,372	1,372	1,372
#3	Y = Sales growth	–0.017	0.046	0.066	0.189
		0.045	*0.088*	*0.139*	*0.144*
	R^2	24.6%	23.1%	26.0%	25.6%
	No. of obs.	1,372	1,372	1,372	1,372

Table IA.11. Testing the Monotonicity Condition.

The table reports the test of the monotonicity condition introduced by Dobbie, Goldin, and Yang (2018). Monotonicity implies that the first-stage estimates should be non-negative in all subsamples formed based on observable startup characteristics. We test this implication in subsamples of the estimation sample used for the LATE estimates reported in Table 9. Panel A reports the first-stage of the Wald estimator, while Panel B reports the first-stage including fixed effects as in Table 9. The number of observations is smaller in Panel B than in Panel A due to singletons. For variable definitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors are clustered at the art unit level. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level, respectively.

	First-stage			
	Coef.	Std. error	Significance level	No. of obs.
	(1)	(2)	(3)	(4)
A. First-stage of Wald estimator				
$\mathbb{1}$(Single founding inventor)=1	0.45	0.04	***	907
$\mathbb{1}$(Single founding inventor)=0	0.45	0.04	***	1,110
$\mathbb{1}$(Founding inventor's first patent filing)=1	0.48	0.04	***	878
$\mathbb{1}$(Founding inventor's first patent filing)=0	0.42	0.03	***	1,139
$\mathbb{1}$(Founding inventor productivity in bottom 25%)=1	0.34	0.09	***	190
$\mathbb{1}$(Founding inventor productivity in bottom 25%)=0	0.46	0.03	***	1,827
$\mathbb{1}$(Founding inventor productivity in bottom 50%)=1	0.41	0.05	***	426
$\mathbb{1}$(Founding inventor productivity in bottom 50%)=0	0.46	0.03	***	1,591
$\mathbb{1}$(Founding inventor productivity in top 50%)=1	0.46	0.03	***	1,591
$\mathbb{1}$(Founding inventor productivity in top 50%)=0	0.41	0.05	***	426
$\mathbb{1}$(Founding inventor productivity in top 25%)=1	0.47	0.03	***	1,198
$\mathbb{1}$(Founding inventor productivity in top 25%)=0	0.41	0.04	***	819
$\mathbb{1}$(Prior breakthrough patent)=1	0.37	0.06	***	519
$\mathbb{1}$(Prior breakthrough patent)=0	0.47	0.03	***	1,498
$\mathbb{1}$(Pro se applicant)=1	0.45	0.07	***	170
$\mathbb{1}$(Pro se applicant)=0	0.45	0.03	***	1,847
B. First-stage of fixed-effects model				
$\mathbb{1}$(Single founding inventor)=1	0.28	0.06	***	757
$\mathbb{1}$(Single founding inventor)=0	0.24	0.06	***	965
$\mathbb{1}$(Founding inventor's first patent filing)=1	0.35	0.06	***	750
$\mathbb{1}$(Founding inventor's first patent filing)=0	0.26	0.05	***	986
$\mathbb{1}$(Founding inventor productivity in bottom 25%)=1	0.22	0.48		50
$\mathbb{1}$(Founding inventor productivity in bottom 25%)=0	0.29	0.04	***	1,685
$\mathbb{1}$(Founding inventor productivity in bottom 50%)=1	0.18	0.09	**	283
$\mathbb{1}$(Founding inventor productivity in bottom 50%)=0	0.30	0.04	***	1,448
$\mathbb{1}$(Founding inventor productivity in top 50%)=1	0.30	0.04	***	1,448
$\mathbb{1}$(Founding inventor productivity in top 50%)=0	0.18	0.09	**	283
$\mathbb{1}$(Founding inventor productivity in top 25%)=1	0.28	0.05	***	1,048
$\mathbb{1}$(Founding inventor productivity in top 25%)=0	0.21	0.07	***	698
$\mathbb{1}$(Prior breakthrough patent)=1	0.10	0.10		359
$\mathbb{1}$(Prior breakthrough patent)=0	0.30	0.04	***	1,366
$\mathbb{1}$(Pro se applicant)=1	0.26	0.31		41
$\mathbb{1}$(Pro se applicant)=0	0.26	0.04	***	1,721