# 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. Contrary to popular belief, recessions do not spawn superstar firms.

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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 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 selection 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), whereby it is the startups whose prospects are relatively more affected by the recession that will tend to wait for the 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 the business cycle.

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 inventor to commercialize her 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. We interpret this group of "always-takers" 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 39.3% and 16.1% of our sample, respectively.

Intention-to-treat effects have a causal interpretation as long as the invitation to treatment is randomly assigned, a condition that prior work shows is plausible in our setting.[1] ITT effects are a lower bound on the causal effects of the recession on innovative startups. Much of the

[1]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).

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 quartile 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 "ness" (Kelly et al. 2021).[2] 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 principal channels through which a recession can affect a startup's development: a funding channel and a labor-market channel. Suppose the supply of funding to startups becomes tighter in a recession.[3] 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, there is 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.[4] Using variation

[2]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.

[3]Between 2007 and 2009, VCs reduced their funding of startups by 27.2% (see `https://nvca.org/recommends/111997-2/`), and 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).

[4]Consistent with this conjecture, we document that labor mobility among inventors in the U.S. economy declined sharply during the recession, from around 0.7% a month in 2006 to around 0.5% a month in 2009.

in 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 work at. Better retention, larger R&D teams, and higher 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.[5] 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).[6] Procyclical changes in aggregate demand can permanently affect a startup's ability to grow 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 startups that is driven entirely by lower startup mortality linked to an improved ability to

[5]Howell et al. (2020) show that venture funding is procyclical, resulting in lower quality innovation in recessions. 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.

[6]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.

retain founding inventors and attract more productive R&D workers. We find no evidence that innovative startups born in the Great Recession faced worse funding conditions than their (only randomly different) expansion peers, suggesting that prior evidence of recession-induced funding constraints, and the negative firm-level consequences they lead to, may not generalize to 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.[7] While Hacamo and Kleiner do not use the term, they too estimate intention-to-treat effects.[8] We go two steps further, estimating local average treatment effects and using an Angrist and Pischke (2009) decomposition to show that positive and negative selection effects coexist. Specifically, forced entrepreneurs are considerably more numerous in our sample (16.1%) than startups that begin life in the recession in compliance with the invitation to be treated (9.4%). The high incidence of "always-takers" supports the role of low-quality forced entrepreneurs in Ghatak, Morelli, and Sjöström's (2007) general equilibrium model.

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 cycle, while a slow examiner may cause the startup to be born at a propitious time.

[7] 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.

[8] 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 $\beta_{OLS}$ 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 three institutional features of the PTO's patent examination process: (i) the PTO assigns patent applications quasi-randomly to examiners (Lemley and Sampat 2012), (ii) examiners differ systematically in their review speed (Hegde, Ljungqvist, and Raj 2022), and (iii) the PTO has a substantial backlog of applications that results in multi-year waits for a decision on an application.[9] Given these institutional features,

[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

two inventors in the same technology field who apply for a patent at the same time will differ in whether their patent is issued in a future recession or a future expansion, depending on their respective randomly assigned examiner's review speed. 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 $Z_{1,i} = 0$). We refer to always-takers as forced entrepreneurs, because they start operations in a recession regardless of whether they have received a patent. 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 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 $\delta_{ITT}$ equals $E[Y_i|Z_{1,i} = 1] - E[Y_i|Z_{1,i} = 0]$, i.e., the difference in average

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 $\beta_{OLS} = \beta_{LATE} + \pi^{at}_{D_i=1}(E[Y_i^{at}|D_i = 1] - E[Y_i^c|D_i = 1, Z_{1,i} = 1]) + \pi^{nt}_{D=0}(E[Y_i^{nt}|D_i = 0] - E[Y_i^c|D_i = 0, Z_{1,i} = 0])$, where $\pi^{at}_{D=1}$ and $\pi^{nt}_{D=0}$ 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.

observed outcomes among those invited to be treated and those not invited. The ITT effect has two desirable properties: it has a causal interpretation, assuming nothing more than that $Z_1$ is randomly assigned; and it is a lower bound on the local average treatment effect, 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?*

Even though patent applications are assigned randomly to examiners, observed review times could correlate with the characteristics of applicants, their applications, or the underlying inventions in such a way 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 $\delta_{ITT}$.

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 processes effectively rule out such lobbying: until the examiner issues a decision, her identity is unknown to the applicant.[12] 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

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

[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.

(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 historical 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 $\delta_{ITT}$ in equation (5) as the bias-corrected intention-to-treat effect.

### *1.2.3. Local Average Treatment Effects*

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

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 (i.e., that there are no startups that would only start operations in a recession if they received a patent in an expansion and that would only start operations in an expansion if they received a patent in a recession). The monotonicity condition cannot be tested, but it seems plausible that it would hold 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. Quasi-random assignment of patent applications to examiners who differ in their predicted review speeds goes a long way towards ensuring that the exclusion restriction holds: it is difficult to see how a randomly assigned invitation to be treated in a future recession rather than a future expansion would affect the startup's future outcomes directly rather than through the prevailing macroeconomic conditions at the time the invitation is received in future. It is also worth noting that applicants do not know the identity of their examiner until the examiner issues a decision on their application and that review times are highly dispersed.[14] Startups thus cannot easily predict what macroeconomic conditions will prevail when they eventually receive news of their patent. It is therefore 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. By implication, it seems unlikely that the instrument would correlate with omitted variables that could drive a startup's future success differently depending on when it receives an invitation to treatment that is not even observable to the startup ahead of time.

If these three additional identifying assumptions hold, $\beta_{LATE} = \beta_{IV} = \delta_{ITT}/\gamma$, where $\gamma$ is the first-stage coefficient on the instrument (or equivalently, the share of compliers).

#### *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. Our empirical design differs from theirs in that 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

[14]Figure IA.1 in the Internet Appendix illustrates the wide dispersion in review times within technology field and application year.

examiner. 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. 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 $\beta$. 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 $E[Y_i|D_i = 1] - E[Y_i|D_i = 0]$. Consider application year $t-1$. Applications randomly assigned to fast examiners are assigned to the recession (with effect on outcome $Y$ of $\beta$) 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 ($-\lambda$): $E[Y_i|D_i = 0] = -\lambda$. Thus, $E[Y_i|D_i = 1] - E[Y_i|D_i = 0] = \beta + 2\lambda$. And similarly for application years $t-2$ and $t-3$. The next table summarizes these effects:

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

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, $\beta$, identified as the coefficient on $D$ in a regression of $Y$ on

$D$.[15] This is shown in the final row marked "Combined." 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.

### *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 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.[16] Their inclusion controls for time-varying industry-level demand and technology-related shocks 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,

[15] In the presence of application-year fixed effects, $\beta$ is identified even if the distribution of applications varies over time. All our empirical specifications include application-year fixed effects.

[16] 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.

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.[17]

# 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. 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]

[17]In some subsample tests, the sample size can become so small that attrition due singletons becomes a problem. In these cases, we cluster the standard errors at the slightly coarser art unit *group* level.

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

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 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,

[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%.

[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 patents filed between 2002 and 2009, 78.9% were granted in 5 years or less. Our results are not sensitive to the five-year cutoff.

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. 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 our estimates, 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 assume that startups are born randomly over the business cycle and so ignore selection biases. 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 that die are assigned a growth rate of $-100\%$, 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. Recall that we include art-unit-by-application-year fixed effects, which allows us to compare firms seeking patent protection for their invention in

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

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 that choose to get going 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.

## *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 of the first-action decision, $Z_1$. 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.[23]

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 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 appears to primarily work through a reduced mortality rate.

## *3.3. Robustness*

Table IA.3 in the Internet Appendix shows 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).

[23] Reassuringly, 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.

By assuming that the recession treatment $D$ is binary, our empirical design implicitly makes no distinction between slowdowns and recoveries. In Table IA.4 in the Internet Appendix, we find no evidence that our baseline ITT effects of the recession 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.[24]

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.5 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-

[24]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 ($p = 0.093$).

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 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.[25]

## *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).[26] 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 ($p = 0.012$).[27] Figure 3 reports quantile ITT ef-

[25]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.

[26]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).

[27]As a side note, traditional citation-based metrics of patent quality do not pick this up; see columns 4 and 5.

fects, 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.

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

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*

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 has only 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 source of potential funding comes is patent sales. While the patent transfer market is quite active,[28] little is known about the prices at which patents are sold. In a competitive market, a patent would sell for the expected net present value of its future cash flows. If so, selling a patent simply exchanges a future stream of cash flows for a lump sum of equal value today. If the patent is instead sold at a discount, its sale will lower the value of the firm. Moreover, for a given sale price, a patent sale may reduce the value of the firm's remaining patent

[28] Serrano (2010) reports that 13.5% of patents are traded at least once, rising to 23.9% of patents granted to "small inventors" such as the ones we focus on.

portfolio, to the extent that firms use complementary patents or patent "thickets" to protect their intellectual property. The costs and benefits of funding a startup's operations through patent sales are thus a priori unclear. We find that 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 source of funding 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 are economically large increases in the likelihood of going public, 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 small 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 among recession startups.

Table 6 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).[29] Consistent with the extensive-margin results in Table 5, there is little evidence that the Great Recession affects funding choices on the intensive margin, except that startups post patents with a higher breakthroughness rank as collateral ($p = 0.052$) and that when they do sell patents, they sell a larger number ($p = 0.073$).

## 4.2. *Labor Channel*

Table 7 reports bias-corrected intention-to-treat effects of the Great Recession on inventor mobility, hiring, and separation. Panels A through C provide robust evidence that recession

[29] These intensive-margin subsamples can get so small that power becomes an issue in the first-stage weak-instrument test. We report results for the five-year horizon, for which $Z_2$ is an at least marginally strong instrument for $Z_1$ in all three subsamples.

startups are better 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 labor-market 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 7, 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 8 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 7 and 8 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.[30] 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.[31] Finally, we measure outcomes over windows of 3, 5, and 7 years.

Table 9, 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

[30] 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).

[31] 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.

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 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 9 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 9 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. Unusually, the tough one of these turns out to be the relevance condition: 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.[32] The first-stage coefficient has an economic interpretation: it implies that only 2.4% of the 6,946 startups in our sample "comply" with the invitation to treatment by starting operations in the year they receive a positive decision on their patent application. By implication, only a subset of inno-

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

vative 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 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 10 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. A priori, this restricted sample includes compliers and never-takers (firms that receive a first-action decision in a recession but wait till the recovery to be born), alongside one type of always-takers: startups that take the treatment when assigned to the treatment (by being born in the recession if that is when they receive their first-action decision). By excluding pre-first-action births, the sample excludes the other type of always-takers: those that take the treatment when assigned to the control group (i.e., startups that choose to be born in the recession without yet having received a first-action decision). We investigate the effects of this sample restriction shortly.

Panel A reports naïve OLS estimates for the restricted sample, which can be compared to the full-sample naïve OLS estimates reported in Table 2. We find positive and statistically significant effects on survival and growth in the short-run that persist in the long-run. The effects are considerably larger than their full-sample counterparts in Table 2, which is intuitive: dropping startups that start operations before their first-action decision removes a type of always-taker that can be thought of as particularly desperate forced entrepreneurs.

Panel B reports the first-stage, which 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 C. Consistent with the full-sample ITT effects reported in Table 3, we see little effect in the short-run, but over the full seven-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, $p = 0.017$). These estimates can be interpreted as the average causal effects of the Great Recession on compliers (i.e., the LATE) under the identifying assumption that the monotonicity and exclusion restrictions hold.[33] Conditional on survival, the recession has no effect on long-term growth (Panel D), consistent with our full-sample ITT results.

We can use the estimates in Table 10 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. We follow the approach outlined in Angrist and Pischke (2009, Section 4.4.4). The fraction of compliant startups equals the first-stage of the Wald estimator.[34] The fractions of compliers among treated startups and untreated startups are calculated using the probabilities that the instrument assigns a startup to the Great Recession and that a startup starts operations in the Great Recession. The following table presents the fractions of treated startups that comply, untreated startups that comply, never-takers, and always-takers in the restricted sample:

| | | Randomized invitation to treatment ($Z_2$) 0 | 1 |
|---|---|---|---|
| Recession treatment ($D$) | 0 | Compliers 35.2% | Never-takers 39.3% |
| | 1 | Always-takers 16.1% | Compliers 9.4% |

The table shows that non-compliance is rampant. Forced entrepreneurs who opt into the recession are numerous in this sample (16.1%), more numerous in fact than startups that begin life in the recession in compliance with the instrument (9.4%). The high incidence of always-takers implies that our instrument purges a large negative selection bias. The table further shows that never-takers constitute 35.2% of this sample, suggesting that the tendency to defer the start of operations until the economy recovers is widespread among innovative startups. 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

[33] Consistent with ITT effects being a lower bound on LATE, the second-stage estimates in Table 10, Panel C are around three times as large as the corresponding ITT estimates in Table 3.

[34] We estimate the first-stage of the Wald estimator without covariates. The fraction of compliers among the treated is calculated as the Wald first-stage times the probability that the instrument equals 1 divided by the probability of being treated.

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, our results in Table 10 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 average complier to the average startup in the restricted sample using the test described in Section 4.4.4 of Angrist and Pischke (2009). The results are reported in Table IA.7 in the Internet Appendix. Compliers look very similar to the average startup in terms of founding-inventor characteristics and funding status at the time of the first-action decision.[35] 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.

Table IA.6 in the Internet Appendix considers what happens when we relax the restriction on pre-first-action births. Using a symmetric window of +/−1 year around the first-action year allows for the possibility of really desperate forced entrepreneurs who opt into the recession. A comparison of the results in Tables 10 and IA.6 is instructive. Consistent with always-takers who opt into the recession indeed being low-quality forced entrepreneurs, the naïve OLS estimates in Panel A are only half as large when we allow for pre-first-action births. The share of compliers, as measured by the first-stage coefficient on the instrument $Z_2$ in Panel B, is a third lower at 16.5%, as one would expect if the symmetric window brought in more always-takers. The second-stage estimates, shown in Panel C, are smaller, consistent with the always-takers being low-quality entrepreneurs. Moreover, the second-stage estimates are noisier, suggesting that the outcomes of always-takers who opt into the recession are particularly dispersed relative to the outcomes of compliers and never-takers. A relatively large variability in outcomes seems intuitive if always-takers really are forced entrepreneurs.

[35] Noisily, startups with weak founding inventors (those in the bottom 25% of productivity) are underrepresented among compliers ($p = 0.094$), as are startups whose founding inventors are associated with a prior top-decile breakthrough patent ($p = 0.065$). Perhaps weak inventors opt out of compliance because they have to (e.g., they struggle to raise funding in a recession), while inventors with a track record of breakthrough inventions opt out because they can (e.g., they delay resigning from their current employer to found their startup when the economy recovers).

# 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.

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 positive and negative selection effects co-exist, and that once they are purged, the causal effects of the Great Recession on "compliers" are positive.

As every recession is 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 VentureXpert. |
| $\mathbb{1}$(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 | 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. |
| Productivity of founding inventors | 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 by founding inventor) | Indicator set equal to 1 if a founding inventor was granted a patent ranking in the top decile of the breakthroughness distribution before filing the focal patent. |
| **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 $\infty$. 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 $\infty$. 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. |
| 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. |

| Variable | Definition |
| --- | --- |
| 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. |
| 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. |

| Variable | Definition |
|---|---|
| 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. |

# 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 report 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 |
| Mean percentile breakthroughness rank | 5 years | 0.54 | 0.56 | 0.26 |
| Citations to 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) | 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 years | 0.59 | 0.00 | 0.89 |
| | 3 years | 0.75 | 0.00 | 0.95 |
| | 5 years | 0.91 | 0.50 | 0.99 |
| | 7 years | 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 years | −0.37 | 0.00 | 1.06 |
| | 5 years | −0.40 | 0.00 | 1.11 |
| | 7 years | −0.42 | 0.00 | 1.13 |
| Hiring rate of non-founding inventors | 1 year | 0.12 | 0.00 | 0.26 |
| | 3 years | 0.29 | 0.00 | 0.48 |
| | 5 years | 0.40 | 0.00 | 0.65 |
| | 7 years | 0.47 | 0.00 | 0.79 |
| Separation rate of non-founding inventors | 1 year | 0.02 | 0.00 | 0.13 |
| | 3 years | 0.10 | 0.00 | 0.39 |
| | 5 years | 0.18 | 0.00 | 0.57 |
| | 7 years | 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.

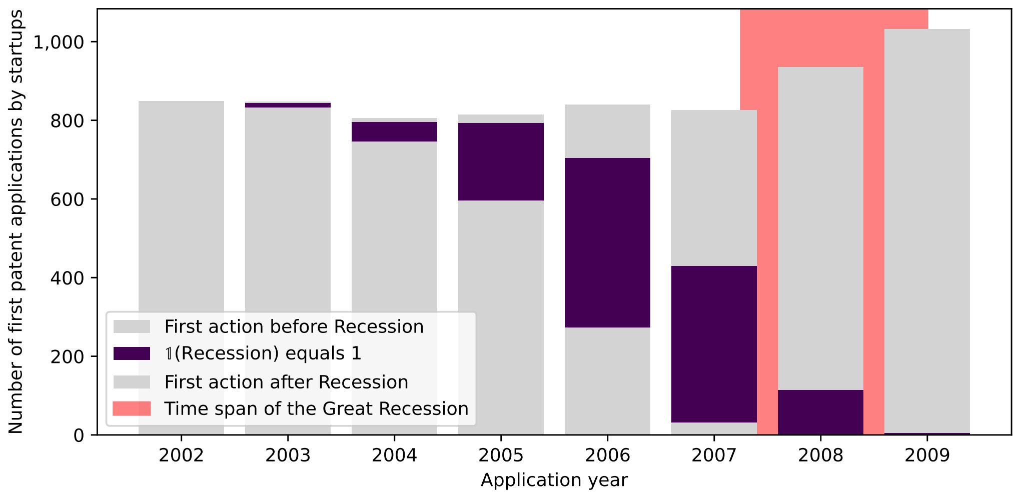
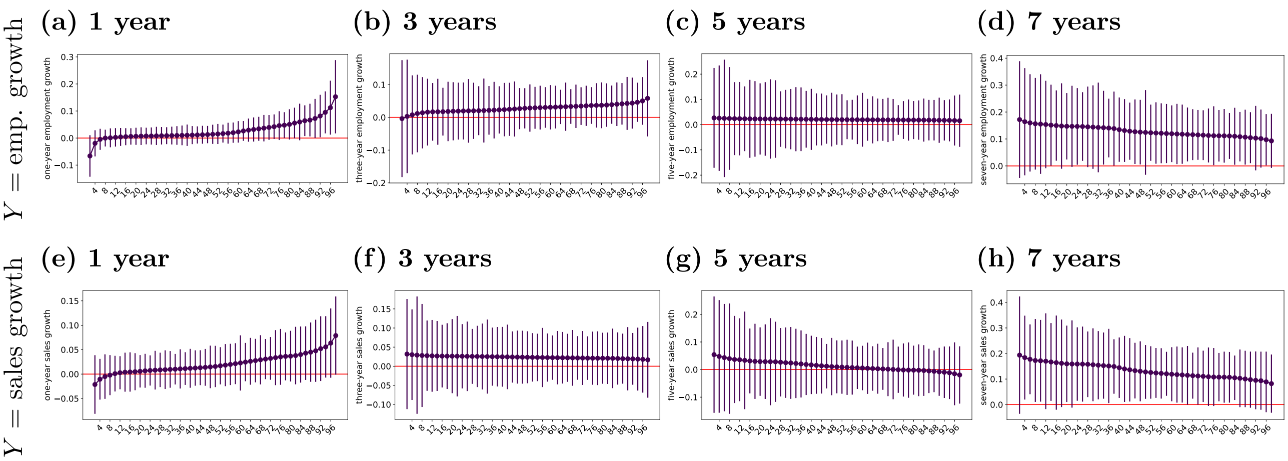


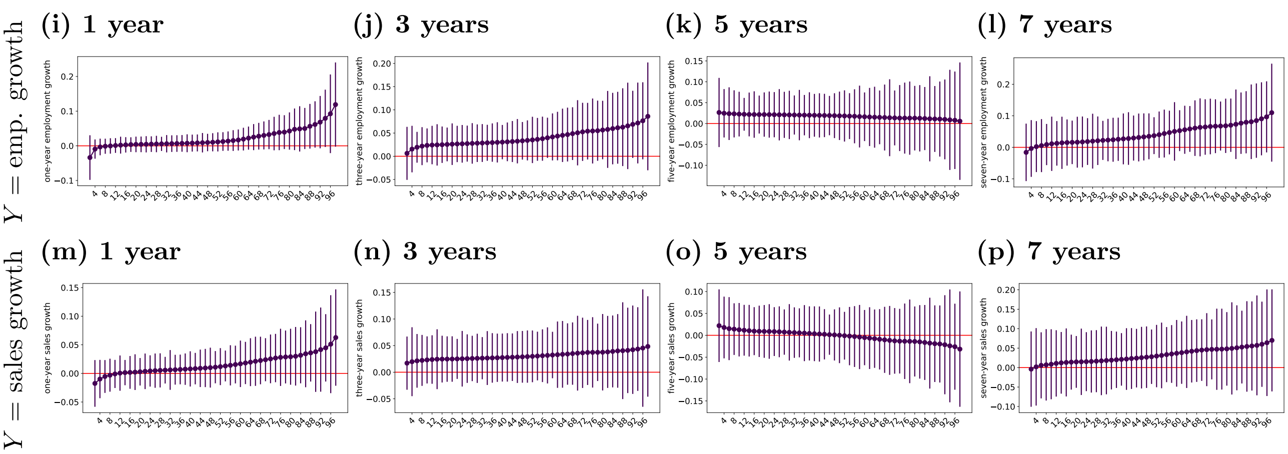
**Figure 2. Startup Growth: Quantile ITT Effects.**

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 $Z_2$)



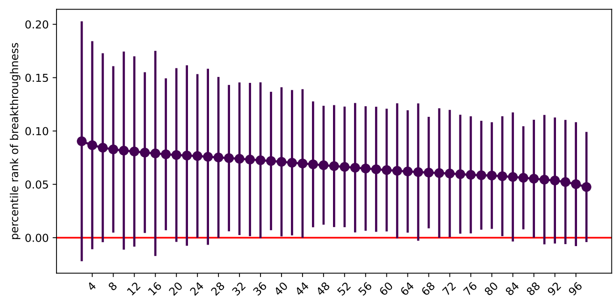

### B. Bias-corrected intention-to-treat ($Y$ on $Z_2$), conditional on survival



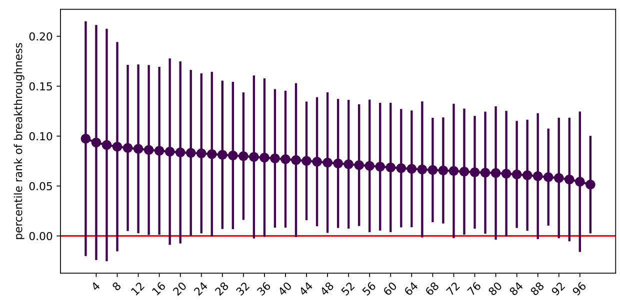
## Figure 3. Follow-on Innovation: Quantile ITT Effects.

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**

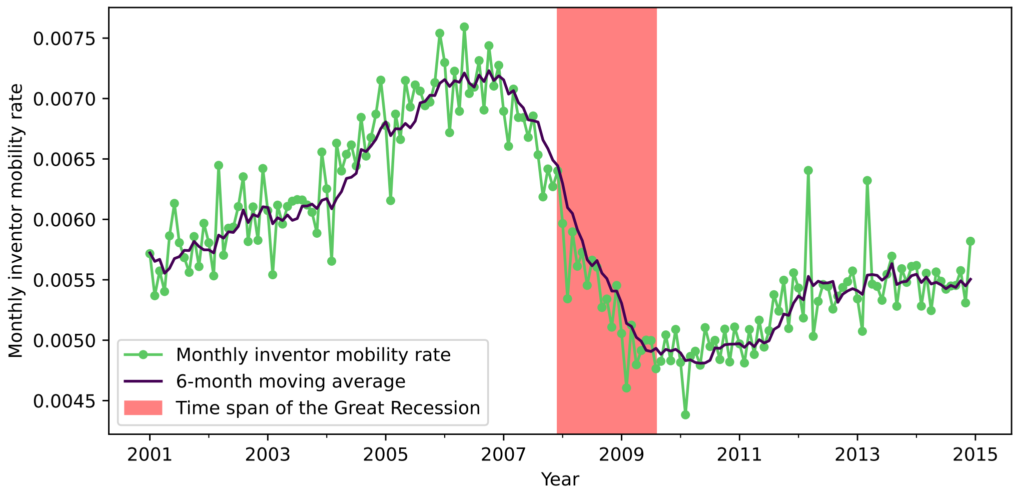



**(b) Conditional on survival**



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

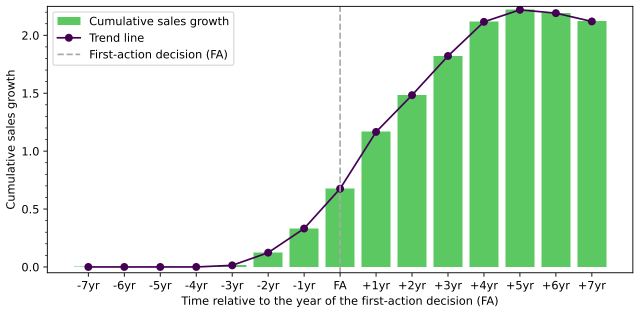
The figure shows the monthly mobility rate of U.S. inventors from 2001 to 2015. We compute the monthly mobility rate as the number of inventors moving from one firm to another firm divided by the number of inventors employed by U.S. firms in a given month. To measure inventor mobility between 2001 and 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**

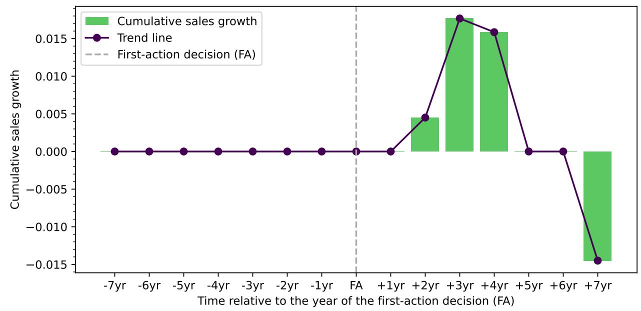


**Table 1. Summary Statistics: Recession vs. Expansion Startups.**

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** |
| Productivity of founding inventor | 7.29 | 8.08 | 2.86 | 7.51 | 8.24 | 2.68 | 0.73 |
| $\mathbb{1}$(Prior breakthrough patent by founding inventor) | 0.24 | 0.00 | 0.43 | 0.27 | 0.00 | 0.44 | −0.76 |

## 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 sales and employment to zero 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 | 3 years | 5 years | 7 years |
| | | (1) | (2) | (3) | (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 Growth and Survival: 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 |

**Continued on next page**

**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<br>(1) | 3 years<br>(2) | 5 years<br>(3) | 7 years<br>(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 |

**Continued on next page**

**Table 5**
**Continued**

| | Startup funding over | | | |
|---|---|---|---|---|
| | 1 year<br>(1) | 3 years<br>(2) | 5 years<br>(3) | 7 years<br>(4) |
| **E. $\mathbb{1}$(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. $\mathbb{1}$(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. $\mathbb{1}$(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. $\mathbb{1}$(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. $\mathbb{1}$(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. $\mathbb{1}$(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. 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). 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 7. 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<br>(1) | 3 years<br>(2) | 5 years<br>(3) | 7 years<br>(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 7**
**Continued**

| | Horizon | | | |
|---|---|---|---|---|
| | 1 year<br>(1) | 3 years<br>(2) | 5 years<br>(3) | 7 years<br>(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 8. 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 9. Startup Growth and Survival: 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 7 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 10. Startup Growth and Survival: 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. Panel A reports the results of estimating equation (1), that is, naïve OLS regressions of $Y$ on D. The remaining panels 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 B reports the first-stage, $D$ on $Z_2$. The weak-instrument $F$-test uses the Kleibergen-Paap $rk$ statistic. Panels C and D 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 | 3 years | 5 years | 7 years |
| | | (1) | (2) | (3) | (4) |
| **A. Naïve OLS ($Y$ on $D$)** | | | | | |
| #1 | $Y = \mathbb{1}$(Survival) | 0.015** | 0.052** | 0.102*** | 0.120*** |
| | | *0.008* | *0.021* | *0.032* | *0.033* |
| | $R^2$ | 14.9% | 19.6% | 22.7% | 22.7% |
| | No. of obs. | 1,878 | 1,878 | 1,878 | 1,878 |
| #2 | $Y$ = Emp. growth | 0.061** | 0.153*** | 0.209*** | 0.255*** |
| | | *0.030* | *0.055* | *0.077* | *0.083* |
| | $R^2$ | 21.7% | 20.6% | 21.7% | 20.9% |
| | No. of obs. | 1,878 | 1,878 | 1,878 | 1,878 |
| #3 | $Y$ = Sales growth | 0.074** | 0.148*** | 0.199** | 0.267*** |
| | | *0.030* | *0.055* | *0.078* | *0.084* |
| | $R^2$ | 21.1% | 19.5% | 21.5% | 21.3% |
| | No. of obs. | 1,878 | 1,878 | 1,878 | 1,878 |
| **B. 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 |
| **C. 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 |

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**Table 10**
**Continued**

| | | Startup survival and growth over | | | |
|---|---|---|---|---|---|
| | | 1 year | 3 years | 5 years | 7 years |
| | | (1) | (2) | (3) | (4) |
| **D. 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

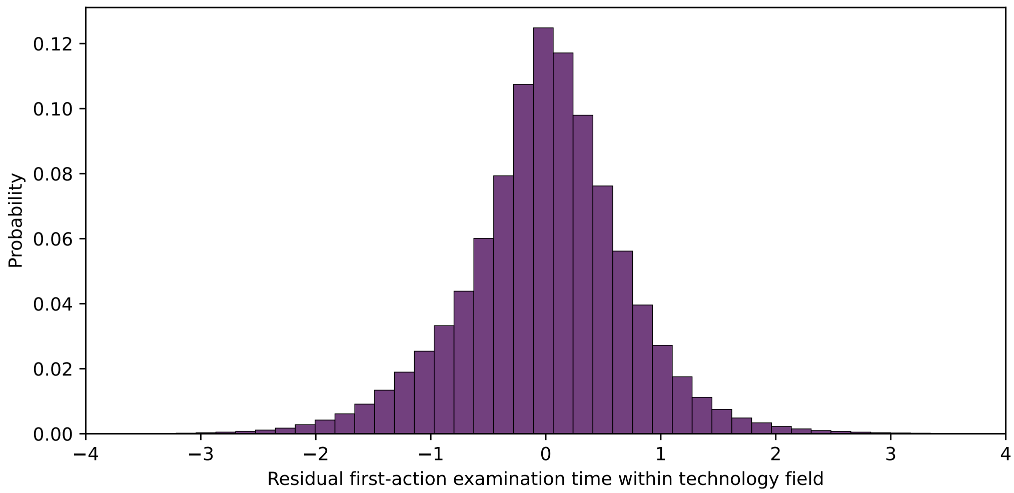
Great Recession Babies:

How Are Startups Shaped by Macro Conditions at Birth?

(NOT INTENDED FOR PUBLICATION)

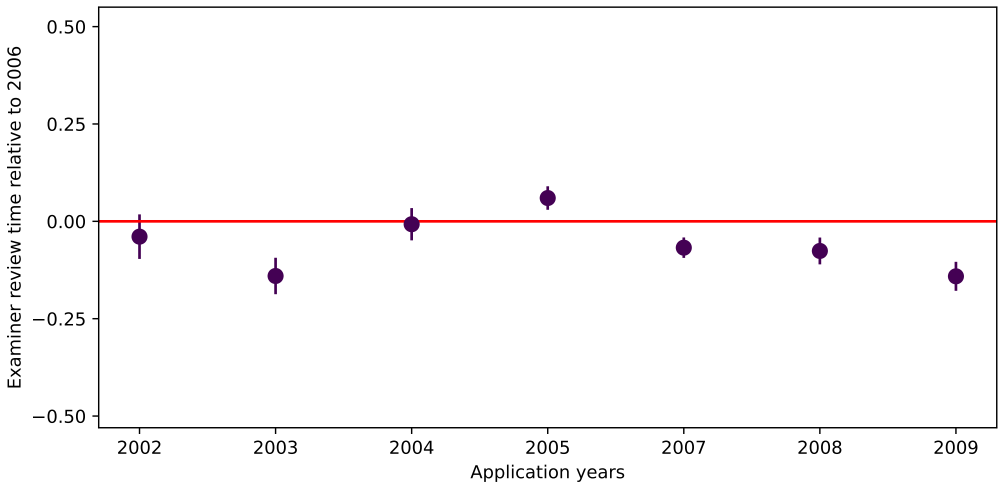
## Figure IA.1. Residual First-Action Examination Time.

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 years) 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 by founding inventor) | 0.26 | 0.55 | 0.59 | 1.09 | 1.02 | 1.16 | ** |

## Table IA.2. Summary Statistics: Recession vs. Expansion Startups Based on $\widehat{Z}_1$.

The table compares 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 |
| Productivity of founding inventor | 7.47 | 8.12 | 2.67 | 7.48 | 8.26 | 2.70 | 1.27 |
| $\mathbb{1}$(Prior breakthrough patent by founding inventor) | 0.29 | 0.00 | 0.46 | 0.26 | 0.00 | 0.44 | −0.39 |

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

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 controlling for the effects of review speed. Panel A reports the results of estimating equation (2), that is, $Y$ on $Z_1$, controlling for first-action examination time $t_{examination_i}$. Panel B 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 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 $Z_1$ 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<br>(1) | 3 years<br>(2) | 5 years<br>(3) | 7 years<br>(4) |
| **A. Intention-to-treat ($Y$ on $Z_1$)** | | | | | |
| #1 | $Y = \mathbb{1}(\text{Survival})$ | −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** | 0.009 | 0.088* | 0.183*** |
| | | *0.016* | *0.035* | *0.051* | *0.057* |
| | $R^2$ | 23.9% | 25.7% | 25.6% | 26.7% |
| | No. of obs. | 6,160 | 6,160 | 6,160 | 6,160 |
| #3 | $Y$ = Sales growth | 0.027 | −0.019 | 0.065 | 0.196*** |
| | | *0.017* | *0.035* | *0.052* | *0.060* |
| | $R^2$ | 23.1% | 25.2% | 25.8% | 26.7% |
| | No. of obs. | 6,074 | 6,074 | 6,074 | 6,074 |
| **B. First-stage ($Z_1$ on $Z_2$)** | | | | | |
| #1 | $Z_1 = \mathbb{1}(\text{Recession})$ | 0.349*** | 0.349*** | 0.349*** | 0.349*** |
| | | *0.025* | *0.025* | *0.025* | *0.025* |
| | $F$-test: IV = 0 | 188.1 | 188.1 | 188.1 | 188.1 |
| | No. of obs. | 6,160 | 6,160 | 6,160 | 6,160 |

**Continued on next page**

**Table IA.3**
**Continued**

| | | Startup survival and growth over | | | |
|---|---|---|---|---|---|
| | | 1 year<br>(1) | 3 years<br>(2) | 5 years<br>(3) | 7 years<br>(4) |
| **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 | 0.074 | 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 |
| **D. 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 Growth and Survival: 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.4**
**Continued**

| | | | 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.4**
**Continued**

| | | | Startup growth and survival over | | | |
|---|---|---|---|---|---|---|
| | | | 1 year<br>(1) | 3 years<br>(2) | 5 years<br>(3) | 7 years<br>(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.5. Startup Growth and Survival: ITT Effects using Continuous Growth.

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. Unlike in Table 3, we use continuous growth rates. 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 | 3 years | 5 years | 7 years |
| | | (1) | (2) | (3) | (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 | 0.026** | 0.013 | 0.055 | 0.107*** |
| | | *0.011* | *0.024* | *0.034* | *0.039* |
| | $R^2$ | 24.3% | 26.3% | 25.8% | 26.9% |
| | No. of obs. | 6,160 | 6,160 | 6,160 | 6,160 |
| #3 | $Y =$ Sales growth | 0.028* | −0.013 | 0.034 | 0.133*** |
| | | *0.017* | *0.029* | *0.039* | *0.046* |
| | $R^2$ | 23.5% | 25.8% | 25.6% | 26.3% |
| | 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 | 0.044 | 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 |

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**Table IA.5**
**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 | 0.039 | 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.6. Startup Growth and Survival: LATE With Pre-First-Action Births.**

The table reports the results of relaxing the restriction on pre-first-action births in Table 10 by including firms born in the year before their first-action. The sample consists of 3,379 startups born in the first-action year, the year before, or the year after. 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. Panel A reports the results of estimating equation (1), that is, naïve OLS regressions of $Y$ on D. The remaining panels 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 B reports the first-stage, $D$ on $Z_2$. The weak-instrument $F$-test uses the Kleibergen-Paap *rk* statistic. Panels C and D 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 3,379 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<br>(1) | 3 years<br>(2) | 5 years<br>(3) | 7 years<br>(4) |
| **A. Naïve OLS ($Y$ on $D$)** | | | | | |
| #1 | $Y = \mathbb{1}$(Survival) | 0.009** | 0.040*** | 0.050** | 0.059*** |
| | | *0.004* | *0.013* | *0.020* | *0.022* |
| | $R^2$ | 10.0% | 13.2% | 15.9% | 14.7% |
| | No. of obs. | 3,222 | 3,222 | 3,222 | 3,222 |
| #2 | $Y =$ Emp. growth | 0.046*** | 0.126*** | 0.121** | 0.130** |
| | | *0.017* | *0.035* | *0.051* | *0.057* |
| | $R^2$ | 13.6% | 13.3% | 14.6% | 13.8% |
| | No. of obs. | 3,222 | 3,222 | 3,222 | 3,222 |
| #3 | $Y =$ Sales growth | 0.057*** | 0.126*** | 0.133*** | 0.159*** |
| | | *0.018* | *0.035* | *0.051* | *0.057* |
| | $R^2$ | 13.5% | 12.7% | 14.9% | 14.2% |
| | No. of obs. | 3,222 | 3,222 | 3,222 | 3,222 |
| **B. First-stage ($D$ on $Z_2$)** | | | | | |
| #1 | $D = \mathbb{1}$(Recession) | 0.165*** | 0.165*** | 0.165*** | 0.165*** |
| | | *0.033* | *0.033* | *0.033* | *0.033* |
| | $F$-test: IV = 0 | 25.7 | 25.7 | 25.7 | 25.7 |
| | No. of obs. | 3,222 | 3,222 | 3,222 | 3,222 |

**Continued on next page**

**Table IA.6**
**Continued**

| | Startup survival and growth over | | | |
|---|---|---|---|---|
| | 1 year<br>(1) | 3 years<br>(2) | 5 years<br>(3) | 7 years<br>(4) |
| **C. LATE ($Y$ on $\widehat{D}$)** | | | | |
| #1 $Y = \mathbb{1}$(Survival) | 0.018 | 0.210** | 0.101 | 0.351* |
| | *0.033* | *0.093* | *0.148* | *0.180* |
| No. of obs. | 3,222 | 3,222 | 3,222 | 3,222 |
| #2 $Y$ = Emp. growth | −0.072 | 0.406* | 0.152 | 0.675 |
| | *0.127* | *0.230* | *0.366* | *0.443* |
| No. of obs. | 3,222 | 3,222 | 3,222 | 3,222 |
| #3 $Y$ = Sales growth | −0.002 | 0.455* | 0.205 | 0.750* |
| | *0.129* | *0.232* | *0.369* | *0.455* |
| No. of obs. | 3,222 | 3,222 | 3,222 | 3,222 |
| **D. LATE ($Y$ on $\widehat{D}$), conditional on survival** | | | | |
| #1 $Y$ = Emp. growth | −0.116 | −0.090 | −0.185 | −0.340 |
| | *0.107* | *0.176* | *0.283* | *0.309* |
| No. of obs. | 3,205 | 3,037 | 2,637 | 2,184 |
| #2 $Y$ = Sales growth | −0.044 | −0.021 | −0.097 | −0.187 |
| | *0.111* | *0.182* | *0.283* | *0.309* |
| No. of obs. | 3,205 | 3,037 | 2,637 | 2,184 |

## Table IA.7. Characteristics of Compliers.

The table reports the relative likelihood that a startup with a certain characteristic actually starts operations during the Great Recession. Following the approach of Angrist and Pischke (2009, Section 4.4.4), the baseline likelihood of a startup starting operations during the Great Recession is estimated via the first-stage of the Wald estimator ($D$ on $Z_2$) in the sample of 2,017 firms that are born in the first-action year or the year after (as in Table 10). The likelihood of a startup starting operations during the Great Recession is estimated via the first-stage of the Wald estimator ($D$ 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) | 0.45 | 0.45 | 0.45 | 1.00 | 0.89 | 1.12 | |
| $\mathbb{1}$(Founding inventor's first patent filing) | 0.44 | 0.45 | 0.48 | 1.08 | 0.97 | 1.18 | |
| $\mathbb{1}$(Founding inventor productivity in bottom 25%) | 0.09 | 0.45 | 0.34 | 0.77 | 0.45 | 1.06 | * |
| $\mathbb{1}$(Founding inventor productivity in bottom 50%) | 0.21 | 0.45 | 0.41 | 0.92 | 0.74 | 1.08 | |
| $\mathbb{1}$(Founding inventor productivity in top 50%) | 0.79 | 0.45 | 0.46 | 1.03 | 0.98 | 1.08 | |
| $\mathbb{1}$(Founding inventor productivity in top 25%) | 0.59 | 0.45 | 0.47 | 1.06 | 0.98 | 1.14 | |
| $\mathbb{1}$(Prior breakthrough patent by founding inventor) | 0.26 | 0.45 | 0.37 | 0.83 | 0.65 | 1.01 | * |
| $\mathbb{1}$(Reg. D private placement) at first-action | 0.02 | 0.45 | 0.19 | 0.43 | −0.39 | 1.31 | |
| $\mathbb{1}$(VC funding) at first-action | 0.03 | 0.45 | 0.56 | 1.26 | 0.60 | 1.84 | |