Great Recession Babies:

How Are Startups Shaped by Macro Conditions at Birth? □

Daniel Bias[1] and Alexander Ljungqvist[2]

[1]Owen Graduate School of Management, Vanderbilt University

[2]Stockholm School of Economics, Swedish House of Finance, CEPR, ABFER, and IFN

May 23, 2023

lower bound on the causal effects of the recession on innovative startups. Much of the evidence we report is in the form of ITT effects. If we are willing to make additional identifying assumptions (discussed in Section 1.2.3), we can use the randomly assigned invitation to treatment as an instrument for being born in a recession, which allows us to estimate the causal effect of the recession on compliers (the local average treatment effect or LATE).

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

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

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

Besides survival and growth, we also study inventiveness. While the Great Recession has no e ect on the quantity of follow-on innovation startups produce after their rst patent, it does positively a ect a measure of the originality and hence likely economic value of their follow-on innovation: its \breakthroughness" (Kelly et al. 2021).[1]

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

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

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

---

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

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

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

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

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

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

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

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

1. Empirical Design

1.1. Identification Challenge

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

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

where we ignore covariates to simplify the exposition and assume, for now, that the recession has a homogeneous effect on all startups: $Y_{1i} - Y_{0i} = \beta$. Estimating equation (1) by OLS yields $\beta_{OLS} = E[Y_i|D_i = 1] - E[Y_i|D_i = 0]$, i.e., the observed difference in average outcomes between startups born in the recession and startups born at other times. It is easy to show that β_{OLS} equals the average treatment effect of interest plus a selection bias: $\beta_{OLS} = \beta_{ATE} + (E[\varepsilon_i|D_i = 1] - E[\varepsilon_i|D_i = 0])$. 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 backlog of applications that results in multi-year waits for a decision on an application.[9] The second comes in the form of the stochastic arrival of a future recession. Combining these two independent sources of random variation with technology-field-by-application-year fixed effects ensures that startups in the same technology field that apply for a patent at the same time will not differ systematically whether their patent is issued in a future recession or a future expansion.

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

1.2.1. *Non-Compliance and Invitation to Treatment*

Receiving a positive decision on a patent application in a recession, $Z_{1;i} = 1$, does not guarantee that the startup will be born in the recession. Startups can choose not to comply with the assignment to treatment, resulting in heterogeneous treatment effects for compliers (those for which $D_i = 1$ if $Z_{1;i} = 1$ and $D_i = 0$ if Z_1

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

$$Y_i = \alpha + \beta_{ITT} Z_{1;i} + \varepsilon_i \tag{2}$$

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

1.2.2. Is Z_1 As Good As Randomly Assigned?

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

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

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

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

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:

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

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

$$Y_i = \alpha + \beta_{ITT}\hat{Z}_{1,i} + \varepsilon_i \tag{5}$$

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

1.2.3. *Local Average Treatment Effects*

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

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

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

1.2.4. Disentangling the Effects of Recessions and Patent Review Delays

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

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

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

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

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

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

Application year	Estimated recession effect
$t-3$	$+ (- \;) - 0.5 \; = \beta - 1.5\lambda$
$t-2$	$-(-0.5\;) + \; - 0.5 \; = \beta$
$t-1$	$-(-0.5\;) + \; + \; = \beta + 1.5\lambda$

highly predictive of nal 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 signi cant rst-stage.

As Lemley and Sampat (2012) argue, assignments of applications to examiners are only random conditional on technology eld and application year. To capture this, we folm5dp7we Thian conrolsn for tme-y

ryingr dem(and)-89((and)-89((tec)27(hnolog-rel

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

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

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

2.2. *Sample Construction*

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

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

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

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

2.3. Summary Statistics

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

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

3. The Effects of the Great Recession on Startups

3.1. Naïve OLS Estimates

We begin by reporting OLS estimates of equation (1) that are naïve in the sense that they ignore selection biases by assuming startups are born randomly over the business cycle. The outcome variables, Y, are survival, cumulative growth in employment, and cumulative growth in sales, in each case measured over periods of 1, 3, 5, and 7 years from birth. We report two growth measures. The first is constructed such that firms are assigned employment and sales of zero when they die, thereby combining the intensive growth margin with the extensive survival margin. The second measures growth conditional on survival. The variable indicating birth relative to the business cycle, D, is set equal to 1 if the startup's founding year coincides with the Great Recession, and 0 otherwise.[23]

3.2. Intention-To-Treat Effects

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

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

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

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

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

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

Our growth rate measures use a definition that has become standard in the literature on firm dynamics: $g_{it} = (Y_{it} - Y_{it-1})/[\frac{1}{2}(Y_{it} - Y_{it-1})]$ (see Davis, Haltiwanger, and Schuh 1996 for a growth, they are generally statistically significant except in the right tail. Overall, we see little evidence to suggest that superstar firms benefit especially from being born in the recession.

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

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

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

4.1. Funding Channel

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 e ect 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 ($p = 0.$

factors may affect both the startup's founding-inventor separation rate and the startup's later performance. For example, it is likely that startups with better prospects (unobserved to the econometrician) both find it easier to retain their founding inventors early on and perform better down the road.

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

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

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

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

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

rate has no e ect on survival or growth over 3 years, it does have a large negative e ect 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 eld increases the startup's chances of surviving for 7 years by 5.4 percentage points (p = 0:002) and its growth in employment and sales by 12.6 (p = 0:010) and 13.1 percentage points (p = 0:014), respectively.

A causal interpretation of the estimates in Table 8 requires that the exclusion restriction holds. Any challenge to the exclusion restriction needs to be able to explain why a fall in demand for the type of R&D workers who patented the t(y)1tenention

bn(e(ts)-003(th)1(e)-003)27(t(yb)-990(for)-003creaonsb)-990o(thrs)-003(th)1ansteb startup'sovdsityobits

hoarsThih causal chins challengsl the thattiosldemand

for R&D workers in the startup's technoloh eld prateld pprs inesymentpraunitiers in

that technotheg(yel)-87[ashpu(eflomd)3858(b)-27(tthrs)-87[dront the rofWiwth the

hatthatthrs typst challengslossiabl1, w thereultsl Tablepraving

r-maorkt, chanel, byhicvt(ys,)-326(b)-27(n(e(,)-326fromy)-)27(b)-27(ving)-326(b)-27orny thehat

/nkttagenat forthee ecs7whereqtioed ar:,

5.2. Profiling Compliers and Non-compliers

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

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

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

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

Because LATE is specific to the subpopulation of compliers for the instrument used, the results in Table 9 will only generalize to other populations of interest to the extent that they

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

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

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

References

Acemoglu, Daron, Ufuk Akcigit, Harun Alp, Nicholas Bloom, and William Kerr. 2018. \Innovation, Reallocation, and Growth." *American Economic Review*, 108(11): 3450{ 91.

Adelino, Manuel, Antoinette Schoar, and Felipe Severino. 2015. \House Prices, Col-

Bernstein, Shai, Timothy McQuade, and Richard R. Townsend. 2021. \Do Household Wealth Shocks A ect Productivity? Evidence from Innovative Workers During the Great Recession." *Journal of Finance*, 76(1): 57{111.

Borgschulte, Mark, and Paco Martorell. 2018. \Paying to Avoid Recession: Using Reenlistment to Estimate the Cost of Unemployment." *American Economic Journal: Applied Economics*, 10(3): 101{27.

Card, David, Ana Rute Cardoso, Joerg Heining, and Patrick Kline. 2018. \Firms and Labor Market Inequality: Evidence and Some Theory." *Journal of Labor Economics*, 36(S1): S13{S70.

Cockburn, Iain M., Samuel Kortum, and Scott Stern. 2002. \Are All Patent Examiners Equal? The Impact of Examiner Characteristics." NBER Working Paper No. 8980.

Davis, Steven J., John C. Haltiwanger, and Scott Schuh. 1996. *Job Creation and Destruction.* MIT Press, Cambridge, Mass.

Dobbie, Will, Jacob Goldin, and Crystal S. Yang. 2018. \The E ects of Pretrial Detention on Conviction, Future Crime, and Employment: Evidence from Randomly Assigned Judges." *American Economic Review*, 108(2): 201{40.

Evans, David S., and Linda S. Leighton. 1990. \Small Business Formation by Unemployed and Employed Workers." *Small Business Economics*, 2(4): 319{330.

Farre-Mensa, Joan, Deepak Hegde, and Alexander Ljungqvist. 2020. \What Is a Patent Worth? Evidence from the U.S. Patent \Lottery"." *Journal of Finance*, 75(2): 639{682.

Frandsen, Brigham, Lars Lefgren, and Emily Leslie. 2023. \Judging Judge Fixed Effects." *American Economic Review*, 113(1): 253{77.

Gaudry, Kate S. 2012. \The Lone Inventor: Low Success Rates and Common Errors Associated with Pro-Se Patent Applications." *PLOS ONE*, 7(3): 1{11.

Gaule, Patrick. 2018. \Patents and the Success of Venture-Capital Backed Startups: Using Examiner Assignment to Estimate Causal E ects." *Journal of Industrial Economics*, 66(2): 350{376.

Ghatak, Maitreesh, Massimo Morelli, and Tomas Sjöström. 2007. \Entrepreneurial Talent, Occupational Choice, and Trickle Up Policies." *Journal of Economic Theory*, 137(1): 27{48.

Granja, João, and Sara Moreira. 2022. \Product Innovation and Credit Market Disruptions." *Review of Financial Studies*.

Marbach, Moritz, and Dominik Hangartner. 2020. \Pro ling Compliers and Noncompliers for Instrumental-Variable Analysis." *Political Analysis*, 28(3): 435{444.

Marco, Alan C., Andrew Toole, Richard Miller, and Jesse Frumkin. 2017. \USPTO Patent Prosecution and Examiner Performance Appraisal." *USPTO Economic Working Paper No. 2017-08*.

Marx, Matt, Deborah Strumsky, and Lee Fleming. 2009. \Mobility, Skills, and the Michigan Non-Compete Experiment." *Management Science*, 55(6): 875{889.

Mian, Atif, and Amir Su . 2014. \What Explains the 2007{2009 Drop in Employment?" *Econometrica*, 82(6): 2197{2223.

Moreira, Sara. 2016. \Firm Dynamics, Persistent E ects of Entry Conditions, and Business Cycles." *Persistent E ects of Entry Conditions, and Business Cycles (October 1, 2016)*.

Nanda, Ramana, and Matthew Rhodes-Kropf. 2013. \Investment Cycles and Startup Innovation." *Journal of Financial Economics*, 110(2): 403{418.

Oreopoulos, Philip, Till von Wachter, and Andrew Heisz. 2012. \The Short- and Long-Term Career E ects of Graduating in a Recession." *American Economic Journal: Applied Economics*, 4(1): 1{29.

Oyer, Paul. 2006. \Initial Labor Market Conditions and Long-Term Outcomes for Economists." *Journal of Economic Perspectives*, 20(3): 143{160.

Rampini, Adriano A. 2004. \Entrepreneurial Activity, Risk, and the Business Cycle." *Journal of Monetary Economics*, 51(3): 555{573.

Rothstein, Jesse. 2021. \The Lost Generation? Labor Market Outcomes for Post Great Recession Entrants." *Journal of Human Resources*, 0920{11206R1.

Sampat, Bhaven, and Heidi L. Williams. 2019. \How Do Patents A ect Follow-On Innovation? Evidence from the Human Genome." *American Economic Review*, 109(1): 203{36.

Sampat, Bhaven N., and Mark A. Lemley. 2010. \Examining Patent Examination." *Stanford Technology Law Review*, 2010: 2.

Schmalz, Martin C., David A. Sraer, and David Thesmar. 2017. \Housing Collateral and Entrepreneurship." *Journal of Finance*, 72(1): 99{132.

Schwandt, Hannes, and Till von Wachter. 2019. \Unlucky Cohorts: Estimating the Long-Term E ects of Entering the Labor Market in a Recession in Large Cross-Sectional Data Sets." *Journal of Labor Economics*, 37(S1): S161{S198.

Sedlacek, Petr, and Vincent Sterk. 2017. \The Growth Potential of Startups Over the Business Cycle." *American Economic Review*, 107(10): 3182{3210.

Serrano, Carlos J. 2010. \The Dynamics of the Transfer and Renewal of Patents." *RAND Journal of Economics*, 41(4): 686{708.

A. Variable Definitions

Variable	Definition
$\mathbb{1}$(Single founding inventor)	Indicator set equal to 1 if the startup's first (eventually successful) patent is filed by a single inventor, and 0 otherwise. Source: USPTO PatentsView.
No. of founding inventors	The number of inventors listed on the startup's first (eventually successful) patent application. Source: USPTO PatentsView.
Founding inventor productivity	We measure founding inventor productivity by sorting founding inventors into deciles by the citations to their past patents. To define the decile breakpoints, we rank the universe of inventors in the U.S. every quarter by the average standardized number of citations to patents granted to them over the previous 10 years. To account for technology-specific time trends, we standardize a patent's citations by the mean citations in a given grant year and technology class. We divide the standardized citations by the patent's number of inventors. For each patent, we count citations in the 5 years after its grant date. Founding inventors who receive zero citations are assigned to the bottom decile. Source: USPTO PatentsView.
$\mathbb{1}$(Prior breakthrough patent)	Indicator set equal to 1 if a founding inventor filed a patent ranking in the top decile of the breakthroughness distribution before filing the focal patent.
Breakthroughnewss rank of prior patents	

Variable	Definition
Breakthroughness rank	The mean percentile breakthroughness rank of the startup's follow-on patents granted over the 5 years from the first-action decision on its first patent application. Following Kelly et al. (2021), breakthroughness is measured using a patent's one-year forward similarity scaled by its five-year backward similarity. Source: Own calculation.
Citations to follow-on patents	The total number of citations received by the startup's follow-on patents over the 5 years from each follow-on patent's grant date. Source: USPTO PatentsView.
Mean citations per follow-on patent	The total number of citations divided by the number of follow-on patents filed by the startup (missing if the startup files no eventu-

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

B. Summary Statistics: Outcome Variables

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

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

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

Figure 1. Sample Distribution over Time.

The gure shows the number of sample rms by year of patent application. The sample consists of 6,946 startups that le their rst (eventually successful) patent application between 2002 (the rst year after the 2001 recession) and 2009 (the ending year of the Great Recession) and that receive their rst-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 rst-action decision before, during, and after the Great Recession. 17% of sample startups receive the rst-action decision during the Great Recession. For variable de nitions and details of their construction see Appendix A.

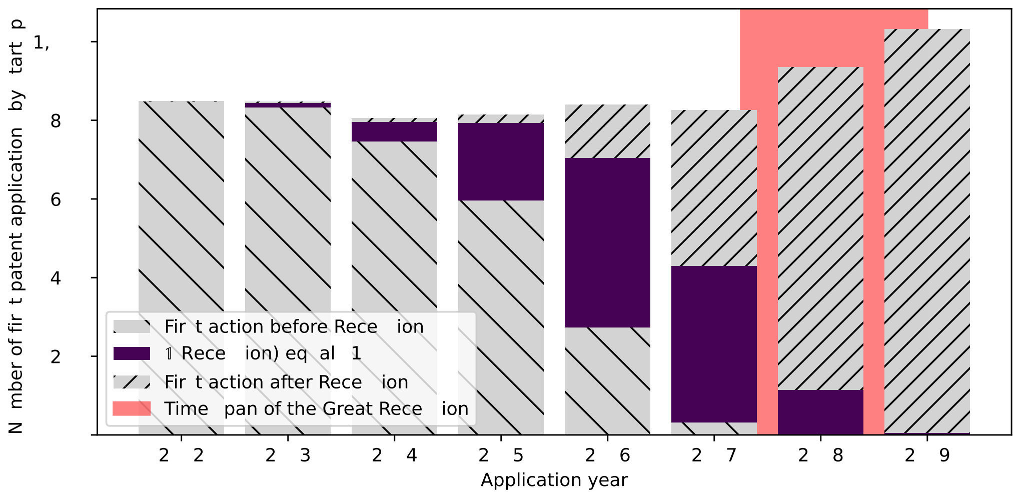

Figure 3. Follow-on Innovation: Quantile ITT E ects.

The gure plots bias-corrected quantile intention-to-treat (ITT) estimates of the e ect 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 rst

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

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 $\frac{sales_t - sales_{t-1}}{}$

Figure 6. Profiling Compliers and Noncompliers.

The figure plots estimated fractions and mean characteristics for the complier, never-taker, and always-taker subpopulations for the 2,017 firms born in the first-action year or the year after (as used in Table 9). To estimate the fractions, we follow the approach outlined in Angrist and Pischke (2009, Section 4.4.4) and estimate the

Figure 6
Continued

(f) 𝟙(Prior breakthrough patent)

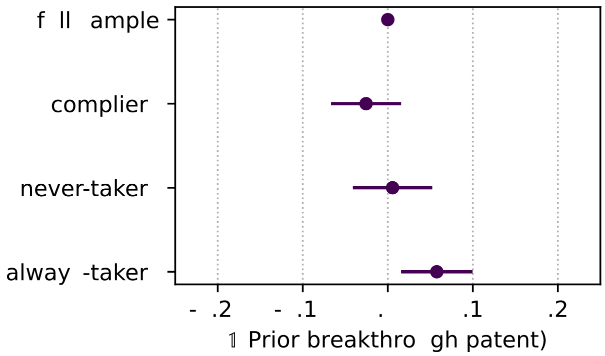

Table 1. Summary Statistics: Recession vs. Expansion Startups.

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

Table 3. Startup Survival and Growth: ITT E ects.

The table reports intention-to-treat (ITT) estimates of the e ects 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 rst-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 rst-action decision, Z_2, as an instrument for the actual time of the rst-action decision, Z_1. Panel B reports the rst-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 e ects (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 speci cations include art-unit-by-application-year and headquarter-state xed e ects. In addition, the speci cations for survival and employment growth control for log employment in the year of rst-action, while those for sales growth control for log sales in the year of rst-action. The number of observations falls short of 6,946 startups due to singletons; in the sales-growth speci cations, it is further reduced due to missing sales data in NETS. For variable de nitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coe cient estimates. We use ***, **, and * to denote signi cance 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 $\hat{Z}_1$)					
#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

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	3 years	5 years	7 years

Table 5
Continued

	Startup funding over			
	1 year	3 years	5 years	7 years
	(1)	(2)	(3)	(4)
E. $\mathbb{1}$(First VC funding)				
ITT: $\hat{\beta}_1$	−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: $\hat{\beta}_1$	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: $\hat{\beta}_1$	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: $\hat{\beta}_1$	−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: $\hat{\beta}_1$	−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: $\hat{\beta}_1$	0.004	0.014**	0.013*	0.034***
	0.004	*0.007*	*0.007*	*0.012*
F-test: IV = 0	186.4	186.4	186.4	186.4
No. of obs.	6,160	6,160	6,160	6,160

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

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

	Horizon			
	1 year (1)	3 years (2)	5 years (3)	7 years (4)
A. $\mathbb{1}$(Founding inventor departs) | inventor level				
ITT: $\hat{Z}_1$	−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: $\hat{Z}_1$	−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: $\hat{Z}_1$	−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: $\hat{Z}_1$	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

Continued on next page

Table 6
Continued

	Horizon			
	1 year (1)	3 years (2)	5 years (3)	7 years (4)
E. Hiring rate of non-founding inventors				
ITT: $\hat{\beta}_1$	−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: $\hat{\beta}_1$	0.023	0.058	0.038	0.097
	0.044	*0.069*	*0.081*	*0.106*
F-test: IV = 0	109.4	109.4	109.4	109.4
No. of obs.	2,379	2,379	2,379	2,379

Table 7. Inventor Productivity: ITT Effects.

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

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

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

The table reports 2SLS estimates of the effect of losing one or more founding inventors early in a startup's life on the startup's subsequent likelihood of survival and its growth in employment and sales. The variable of interest is the startup's founding-inventor separation rate, defined as in Table 6 and measured over the 2 years from the startup's first-action date. (When measured over shorter periods, results are qualitatively similar but considerably noisier.) Outcomes are

INTERNET APPENDIX

for

Great Recession Babies:

How Are Startups Shaped by Macro Conditions at Birth?

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

The gure shows the distribution of the time from patent application to the \ rst o ce action on the merits" (rst-action) decision within technology eld and application year. The gure plots the distribution of residual rst-action examination time estimated on the universe of 2,878,069 patent applications led between 2002 and 2009, controlling for art-unit-by-application-year xed e ects. For variable de nitions and details of their construction see Appendix A.

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

The gure shows plots regression coe cients of examiner review speed (in months) on indicator variables for applications led in 2002, 2003, 2004, 2005, 2007, 2008, and 2009. The omitted reference group is applications led in 2006. The OLS regression is estimated on the universe of 2,878,069 patent applications led between 2002 and 2009 and controls for art unit xed e ects. Standard errors are clustered at the art unit level. The vertical lines indicate 95% con dence intervals. For variable de nitions and details of their construction see Appendix A.

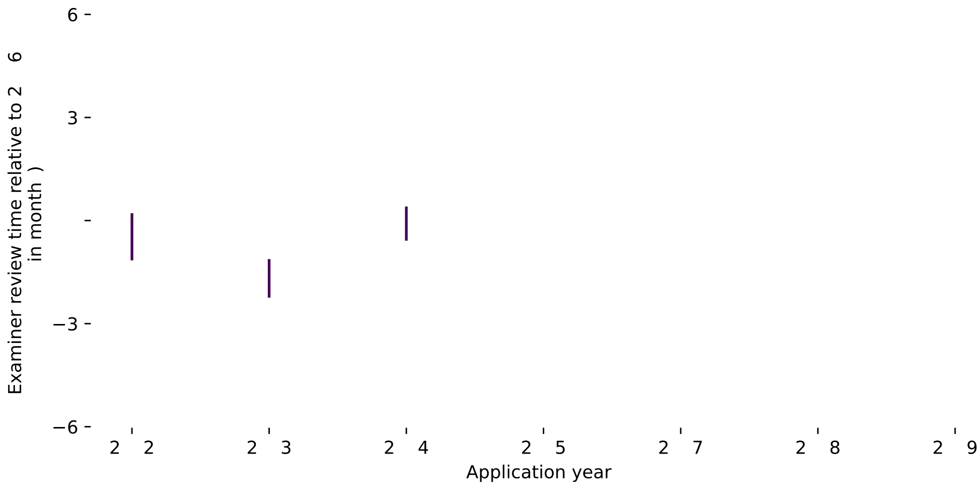

Table IA.1. Examination Practices During the Great Recession.

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 rst-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 rst-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 rst-stage estimates in the subsample and the full sample. To test whether the relative likelihood is statistically di erent from 1, we construct non-parametric con dence intervals based on 1,000 bootstraps clustering standard errors at the art unit level. We use ***, **, and * to denote signi cance at the 1%, 5%, and 10% level, respectively.

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

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

The table reports a balance test comparing sample startups according to $\hat{Z}_1$. $\hat{Z}_1$ 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

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

The table reports bias-corrected intention-to-treat (ITT) estimates of the e ects of being born in the Great

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

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

	Startup survival and growth over			
	1 year (1)	3 years (2)	5 years (3)	7 years (4)
A. First-stage (Z_1 on Z_2)				
#1 $Z_1 = \mathbb{1}$(Recession)	0.345***	0.345***	0.345***	0.345***
	0.025	*0.025*	*0.025*	*0.025*
F-test: IV = 0	184.2	184.2	184.2	184.2
No. of obs.	6,044	6,044	6,044	6,044
B. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$)				
#1 $Y = \mathbb{1}$(Survival)	0.010	−0.006	0.017	0.133*
	0.013	*0.036*	*0.060*	*0.073*
No. of obs.	6,044	6,044	6,044	6,044
#2 Y = Emp. growth	0.070	0.068	0.049	0.372**
	0.055	*0.104*	*0.153*	*0.179*
No. of obs.	6,044	6,044	6,044	6,044
#3 Y = Sales growth	0.060	0.065	0.028	0.372**

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

The table reports bias-corrected intention-to-treat (ITT) estimates of the e ects of being born in the Great Recession on a startup's likelihood of survival, its employment growth, and its sales growth over windows of 1, 3, 5, and 7 years. We investigate the concern that the examiner's predicted review speed (of which our instrument, Z_2, is a non-monotonic function) potentially correlates with unobserved examiner habits that could a ect outcomes of interest in unexpected ways We do so by replacing the examiner's predicted review speed with the art unit's average review speed in the construction of the instrument. with the art unit's average review speed when constructing the instrument. Speci cally, we predict whether or not each startup's patent decision is issued in the recession based on the sum of the application date, the application-speci c administrative lag from the time the application is led to the time it is docketed with an examiner, and (unlike in Table 3) the average historical review speed across all examiners in the art unit. Panel A reports the rst-stage. The weak-instrument F-test uses the Kleibergen-Paap rk statistic. Panels B and C report bias-corrected ITT e ects (equation (5)) in the full sample and in the sample of surviving startups, respectively, estimated via 2SLS using the alternative version of$Z$

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

The table reports bias-corrected intention-to-treat (ITT) estimates of the e ects of being born in the Great

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

The table reports bias-corrected intention-to-treat (ITT) estimates of the e ects 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 rst-action date. The omitted reference group is the expansion period from January 2002 to November 2006. Panel A reports the three rst-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 e ects (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 speci cations include art-unit-by-application-year and headquarter-state xed e ects. In addition, the speci cations for survival and employment growth control for log employment in the year of rst-action, while those for sales growth control for log sales in the year of rst-action. The number of observations falls short of 6,946 startups due to singletons; in the sales-growth speci cations, it is further reduced due to missing sales data in NETS. For variable de nitions and details of their construction see Appendix A. Heteroskedasticity-consistent standard errors clustered at the art unit level are shown in italics underneath the coe cient estimates. We use ***, **, and * to denote signi cance 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

Continued on next page

Table IA.7
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 $\hat{Z}_1$)						
#1	$Y = \mathbb{1}$(Survival)	$\hat{Z}_1$: $\mathbb{1}$(Slowdown)	0.002	0.080	0.233**	0.194
			0.012	*0.067*	*0.097*	*0.127*
		$\hat{Z}_1$: $\mathbb{1}$(Recession)	0.010	−0.003	0.041	0.151**
			0.010	*0.033*	*0.059*	*0.066*
		$\hat{Z}_1$: $\mathbb{1}$(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	$\hat{Z}_1$: $\mathbb{1}$(Slowdown)	−0.144**	−0.038	0.371	0.315
			0.068	*0.165*	*0.242*	*0.312*
		$\hat{Z}_1$: $\mathbb{1}$(Recession)	0.068	0.054	0.109	0.414***
			0.052	*0.096*	*0.151*	*0.158*
		$\hat{Z}_1$: $\mathbb{1}$(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	$\hat{Z}_1$: $\mathbb{1}$(Slowdown)	−0.119*	−0.028	0.369	0.335
			0.072	*0.168*	*0.239*	*0.299*
		$\hat{Z}_1$: $\mathbb{1}$(Recession)	0.061	0.042	0.092	0.432***
			0.055	*0.101*	*0.153*	*0.163*
		$\hat{Z}_1$: $\mathbb{1}$(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

Continued on next page

Table IA.7
Continued

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

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

	Startup survival and growth over			
	1 year (1)	3 years (2)	5 years (3)	7 years (4)
A. First-stage (Z_1 on Z_2)				
#1 $Z_1 = \mathbb{1}$(Recession)	0.349***	0.349***	0.349***	0.349***
	0.025	*0.025*	*0.025*	*0.025*
F-test: IV = 0	187.7	187.7	187.7	187.7
No. of obs.	6,160	6,160	6,160	6,160
B. Bias-corrected intention-to-treat (Y on $\widehat{Z}_1$)				
#1 $Y = \mathbb{1}$				

Y $\widehat{Z}$ Y

Table IA.9. Intensive Funding Margins: ITT E ects.

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

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

Table IA.10. Testing the Exclusion Restriction.

The table reports the test of the \no rst stage, no reduced form" restriction described in Angrist (2022) and applied by Angrist, Lavy, and Schlosser (2010). The exclusion restriction implies that reduced-form e ects in samples for which the rst-stage is zero should be zero as well. We test this implication in two samples. The rst sample is the sample in which only 2.4% of the startups \comply" with the invitation to treatment by starting operations in the year in which they are predicted to receive a positive decision on their patent application. Panel A presents the rst-stage estimates and Panel B the reduced-form estimates. The second sample is the

Table IA.11. Testing the Monotonicity Condition.

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