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Launching with a parachute: The gig economy and new business formation☆

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ABSTRACT

We utilize the staggered arrival of Uber and Lyft—large sources of on-demand, platform-enabled gig opportunities—in U.S. cities to examine the effect of the arrival of flexible gig work opportunities on new business formation. The introduction of gig opportunities is associated with an increase of ~5% in the number of new business registrations in the local area, and a correspondingly-sized increase in small business lending to newly registered businesses. Internet searches for entrepreneurship-related keywords increase ~7%. These effects are strongest in locations where proxies for ex ante economic uncertainty regarding the viability of new businesses are larger. Our findings suggest that the introduction of the gig economy creates fallback opportunities for would-be entrepreneurs that reduce risk and encourage new business formation.

1. Introduction

As far back as Knight (1921), scholars have argued that bearing risk is one of the essential characteristics of entrepreneurship. Because the capital markets provide too little capital to entrepreneurs as a result of moral hazard and adverse selection problems (e.g., LeRoy and Singell, 1987), entrepreneurs must finance themselves and bear the risk of failure. Empirical research on the relationships between wealth constraints and entrepreneurship (Jensen et al., 2014), job protection and entrepreneurial activity (Gottlieb et al., 2018), and unemployment insurance and new business formation (Hombert et al., 2020) in countries outside the U.S. are consistent with this view. Under a Knightian perspective, the relaxation of the personal liquidity constraint through the provision of a channel for income supplement and/or through the provision of employment fallbacks that serve as insurance for failed entrepreneurs should encourage additional entrepreneurial entry. In this paper, we argue that the arrival of the platform-enabled, on-demand gig economy, with its flexible work hours and low entry barriers, provides just such insurance against entrepreneurial-related income volatility.

We empirically explore the effect of gig opportunities on the emergence of new entrepreneurial ventures utilizing the staggered rollout of a major source of gig

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work opportunities: ridehailing platforms. This analysis is motivated by the Knight (1921) view that potential entrepreneurs consider the returns of alternative employment opportunities when choosing to start new business ventures. Applying this framework to the gig economy, the arrival of on-demand, platform-based gig opportunities dramatically reduced the riskiness of the fallback option for would-be entrepreneurs, thereby fostering the launch of new entrepreneurial activity. This does not imply per se that all entering entrepreneurs will be gig economy workers, of course. Rather, the gig economy provides insurance and peace of mind in knowing that it is there, if needed, and as such, affects expectations in the entry decision.

We focus on incorporated business starts, as the factors that drive entry into entrepreneurship likely differ across the various types of entrepreneurship. While the income opportunities option provided by the gig economy may entice risk-bearing would-be entrepreneurs to launch new companies, at the same time, for individuals engaged in ad hoc self-employment, the gig economy offers the potential for a steadier "employment"-like opportunity. Though technically every gig economy worker is, in fact, self-employed, many of these individuals, particularly those who drive for platforms such as Uber or Lyft, self-classify as "working for Uber (Lyft)" and do not report themselves as self-employed in self-reported survey measures. Burtch et al. (2018) utilize this fact to show that with the entry of the gig economy, self-reported self-employment from the Current Population Survey—primarily unincorporated self-employment—goes down, consistent with the notion of some self-employed workers transitioning to working in the gig economy (and consequently classifying themselves as "working for Uber (or Lyft)" rather than being self-employed). The effect of the availability of gig economy opportunities on new (incorporated) business launches—which differ considerably from low-quality self-employment, which is the source of the impacts in Burtch et al. (2018)—remains, however, unexplored.

Our empirical analysis utilizes a relatively novel dataset of actual new business registrations in a local region, provided by the Startup Cartography Project (SCP) (http://www.startupcartography.com). Because a new company must not only incorporate in a state jurisdiction (which may not be the state they operate in), but also register there to do business with their local Secretary of State (where the business actually operates), and because such registrations provide an actual operating address for the new company, utilizing business registration data allows us to observe the full universe of newly incorporated businesses. The SCP dataset provides us with counts, by zip-code and quarter, of all new for-profit businesses, allowing us to observe entrepreneurial entry at the micro-level.

We utilize the arrival of ridehailing platforms, Uber and Lyft, as they represent two of the first large-scale app-based gig economy opportunities to roll out across the U.S., providing thousands of drivers in cities opportunities at any hour of the day or night to work for as long or as short as they wished. Ridehailing platforms such as Uber and Lyft allow drivers, once approved, to use their own or rented cars to offer rides whenever they choose. There are no minimum hour requirements and only modest constraints on maximum hours. Thus, drivers can work whenever they want to. Moreover, the arrival of these platforms often heralds the arrival of other gig economy platforms such as food delivery, errand running, or package delivery.

Utilizing incorporated business registrations rather than measures of "self-employment" both allows us to capture the type of entrepreneurial entry we are most interested in (businesses who have taken a form required for possible growth) as well as avoid the concern that any increase in measures of "self-employment" may simply be capturing ridehailing workers, who by definition are contractors and therefore self-employed. Since, according to sources at Uber, individual drivers on ridehailing platforms in the U.S. rarely, if ever, incorporate,[1] the newly registered incorporated businesses (and associated SBA loans) should not be reflecting drivers incorporating to drive for ridehailing companies as individuals.

A natural concern is that ridehailing platforms did not launch in specific cities randomly. This is concerning for our identification approach, for example, if ridehailing platforms specifically entered into "entrepreneurial" cities first. This does not appear to be the case. Using a hazard model approach, we document that the rollout timing of ridehailing platforms into cities is, as expected, predicted by per-capita income, population size, and unemployment levels. However, it appears to not be predicted by the levels of entrepreneurial activity within a city. Thus, the identifying assumption for a difference-in-differences analysis, namely, that the treatment is unrelated to the outcome at baseline appears to hold.

Accordingly, we utilize a difference-in-differences (D.D.) specification with fixed effects for location and time (quarter-year) as well as location-specific linear trends. Our D.D. specification allows us to capture macroeconomic changes, such as the Great Recession, technological improvements, as well as city-specific conditions such as city topology, industry mix, and so forth. The location-specific time trend captures location-specific pre-trends in our outcome variables that existed prior to the arrival of ridehailing. To capture potential time-and-city varying confounders, such as population changes or increases in employment or income, we further control for population levels and per capita income. Our results are robust to the inclusion of a variety of additional controls as well as location-specific quadratic trends and hold for different pre-period lengths as well as when we restrict the sample solely to ever-treated locations. We find an increase of 4–6% in new business registrations following the arrival of the gig economy in a city. The parallel trends observed in the data further suggest that we are not simply picking up differential trends in new business formation in the treated cities in the pre-period.

Presumably, if the increase in new business launches is driven by the existence of gig economy income fallbacks, then the intensity of ridehailing adoption in a city should be related to the documented increase in our outcome

[1] This fact is a primary consideration in much of the discussion over how such drivers should be classified—as contractors or as employees. If drivers tended to incorporate, the contractual relationship assumed between the driver-provider and the platform would be clear (contractor).

variables. We proxy for the strength of ridehailing take-up in a city using the intensity of Google searches for terms such as "Uber" and "Lyft" in the treatment cities, a proxy that has been shown by past literature (e.g., Cramer and Krueger, 2016) to correlate strongly with adoption of the platforms. When we substitute the treatment indicator for post-ridehailing-city with our ridehailing adoption intensity proxy for the city, we obtain similar results to those in our main specifications, with entrepreneurial entry increasing in the intensity of adoption of gig opportunities in the city.

Having established the basic positive relation between the availability of ridehailing platform gig opportunities and new business formation, we next proceed to examine the financing channel for new businesses. As documented by Guzman and Stern (2019), the vast majority of new business launches are "traditional business entrepreneurship" (TBE) of the type described by Knight (1921).[2] In contrast to innovation-driven entrepreneurship (IDE) ventures, which are typically financed via equity by angel and venture capital investors who bear the primary risk associated with the venture, TBE ventures are typically financed through entrepreneur wealth or through some form of debt, particularly small business lending (for example, Bothelho, Fehder, and Hochberg (2021) provide a discussion of TBE versus innovation-driven entrepreneurship and their financing). Thus, we focus our attention on SBA loans. We match businesses registered in the prior 6 (or 12) months to data on SBA loans made under the SBA's 7(a) programs. Consistent with our findings of a 4–6% increase in realized business registrations, we document a corresponding increase of similar magnitude in small business lending to newly registered businesses after the arrival of the gig economy.

So far, the measures we have employed measure realized entrepreneurial activity. We next proceed to explore whether the presence of gig economy income opportunities can also be seen in indicators of interest in the possibility of launching a business. We measure *entrepreneurial interest* (expression of interest in entrepreneurship) using google searches for terms related to entrepreneurship, such as "how to start a business" or "how to incorporate." By utilizing searches, as opposed to realized new venture starts, our intent is to capture an alternative measure of changes in expectations regarding the possibility of entering into entrepreneurship. Consistent with the notion that the availability of gig-work as a fallback spur potential interest in entry into entrepreneurial activity, the DD specification documents an approximate 7–12% increase in entrepreneurial interest surrounding the arrival of ridehailing platforms in a city.

If our premise holds that the availability of ridehailing platform gig opportunities facilitates new business formation by reducing the uncertainty associated with entry into entrepreneurial activity, then the availability of new gig opportunities in the form of ridehailing platforms should be more valuable in locations where ex-ante economic and entrepreneurship-related uncertainty is higher. To capture this notion, we focus on four proxies of ex-ante economic uncertainty. First, we measure the variance in wage growth across industries in the area measured over the period 2000 to 2010, at the city level, as a proxy for earnings volatility in the area. While we would ideally like to measure the volatility of entrepreneurial earnings, this data is not available. Conceptually, we can think of economic profits as reflecting demand shocks to industries, which in turn also lead to variation in wage growth. We acknowledge, however, that this proxy is not tied closely to entrepreneurial income. We thus next turn to looking at a measure that better reflects the specific uncertainty associated with launching a new business: the volatility of business income. We construct two measures: (i) the volatility of zip-level business income in the CBSA in 2010, pre-ridehailing entry; and (ii) the volatility of historical zip-level business income in a CBSA over the five-year period of 2005 to 2010. Finally, we turn to a measure that proxies for downside risk of launching a new business: the business bankruptcy rate in the county the city is located in.

We then interact our post-ridehailing variable with these proxies for uncertainty. Across all four proxies for economic and entrepreneurial uncertainty, we observe that the relation between the arrival of ridehailing platforms (and their associated gig work opportunities) and new business formation is more pronounced in locations where our proxies for uncertainty are higher ex-ante. Specifically, we find a 3 percentage point larger effect in cities with a standard deviation higher wage growth volatility, a 2–4 percentage point larger effect in areas with a standard deviation higher business income volatility, and a 1 percentage point larger effect in areas with a standard deviation higher business bankruptcy rate.

Importantly, we show that the pattern of where in the city these businesses open (geographic HHI) does not change post-gig economy arrival, suggesting that we are not merely picking up an increase in business opportunities due to the opening of new neighborhoods to transportation via ridehailing. Moreover, we find that the mix of new business types (traditional business versus innovation-driven business) also does not appear to be significantly altered by the arrival of the gig economy. Finally, while our D.D. specification with city-specific linear trends is designed to explicitly control for growth patterns in the city, we provide further evidence that the effect we document is not simply a manifestation of differential overall economic growth patterns. Specifically, we show that average weekly wages do not increase following the arrival of the gig economy, while our entrepreneurial activity measures do.

We conclude our analysis descriptively by exploring heterogeneity in our outcomes across the city characteristics of education level, race, and credit constraints. We

[2] In contrast, the high-growth, innovation-driven entrepreneurial activity that is typically financed by venture capitalists conforms more closely to the non-constraint view of Schumpeter. Schumpeter (1934, 1942) argues that the functions of the entrepreneur and the capitalist are separate. The entrepreneur plays the role of identifying potential arbitrage opportunities in the economy, while the part of modern capital markets is to find a capitalist willing to bear the risk for the entrepreneur. See Botelho et al. (2021) for a review of the innovation-driven entrepreneurship (IDE) literature and a discussion of the differences between TBE and IDE.

find that our effects are largest in areas with lower education levels, higher fractions of Hispanic population, lower fractions of African-American population. When we look at credit constraints at the city level, we find a U-shaped pattern suggesting the effects are larger both when the population of a location is extremely credit-constrained and in locations where they face much lower constraints. This is consistent on the supply side with a loosening of the credit constraint and with increases in demand in less constrained areas.

Our study offers several contributions to the existing literature. First and foremost, our results speak to a growing literature on the factors that drive entry into entrepreneurship. Recently, there has been a great deal of concern regarding a decline in entrepreneurial entry and business dynamism (e.g., Decker et al., 2016), given the importance of entrepreneurial activity for economic growth (e.g., Haltiwanger et al., 2013). Manso (2011, 2016) noted that tolerance for failure is a key driver of entrepreneurial entry; here, the gig opportunities provided by ridehailing platforms arrival provides the safety net that makes experimentation "safe" to explore. Our findings are consistent with those found in other contexts and countries when liquidity or credit constraints are relaxed, job protection is extended, or income fallbacks are provided: for example, Jensen et al. (2014) show that a Danish mortgage reform that increases credit by \$30 K leads to an increase in entry, while Gottlieb et al. (2018) show that extended job-protected maternity leave in Canada increases the likelihood of entry, Bellon et al. (2019) show that personal wealth windfalls from fracking increase entry into self-employment, and Hombert et al. (2020) show that provision of unemployment insurance to those entering into entrepreneurship increases new business formation. More broadly, our paper relates to a growing literature on entrepreneurial entry barriers, including personal wealth, government regulation, tax policy, and banking systems (see, e.g., Evans and Jovanovic, 1989; Gentry and Hubbard, 2000; Hurst and Lusardi, 2004; Klapper et al., 2006; Cagetti and de Nardi, 2006; Aghion et al., 2007, and many more).

Relatedly, the ridehailing entry events studied in this paper could be considered shocks to the non-pecuniary benefits to alternative employment—most notably, work flexibility. We expect there to be an effect on business formation decisions if marginal entrepreneurs value the flexibility either directly or as a means of insurance. This contrasts with existing evidence on non-pecuniary benefits in entrepreneurship, which focuses on how these aspects of entrepreneurial jobs motivate or sustain entrepreneurship (e.g., Hurst and Pugsley, 2015 or Bellon et al., 2021).

Second, our study further contributes to a growing literature on the spillovers of the gig-economy on traditional business entrepreneurship and employment effects. Our work complements several closely related studies such as Koustas (2018), Fos et al. (2019), and Jackson (2019), who demonstrate that the gig economy can serve as an income fallback in down states of the world such as unemployment or job loss. Our finding complements prior work by showing how gig opportunities for income fallbacks during down states of the world not only spur less reliance on unemployment benefits or lower duration of unemployment spells but also drive entry into entrepreneurship.

Finally, our work contributes to the growing literature on the economics of the gig-economy and, more specifically, ridehailing. Prior work in this literature, such as (Hall and Krueger, 2017) and (Chen, Chevalier, Rossi and Oehlsen, 2017), explore the importance of the flexibility provided by these platforms to the direct providers of driver services.[22] Our findings suggest that there are not only benefits to those who offer services for gig economy on-demand platforms but also for those outside the platforms, as the existence of such opportunities may provide insurance against the uncertainty and risk associated with entry into entrepreneurial activity. Moreover, our findings, which document increased entry of incorporated businesses in the wake of ridehailing platform launches, stand in contrast to Burtch et al. (2018), who examine the effect of the gig economy on Kickstarter projects and self-reported self-employment measures. While their findings suggest a move from ad hoc unincorporated self-employment into working for gig economy platforms, our findings on incorporated businesses reinforce the distinction between self-employment and incorporated entrepreneurial businesses and how they may differentially be affected by the advent of app-enabled gig platforms.

The paper proceeds as follows. Section 2 provides an overview of the gig economy and outlines our conceptual framework. Section 3 describes our data and sample. Section 4 presents our empirical results. Section 5 concludes.

2. The gig economy and new business formation

2.1. The app-enabled gig economy

The advent of the smartphone and the complementary technological advancement have reshaped the commercial landscape, providing consumers new ways to access the retail marketplace and providing workers with easy access to a new source of flexible work opportunities. The collection of markets that match providers to consumers on a gig (or job) basis, in support of on-demand commerce, has been coined "the gig economy." Companies such as Uber, Lyft, DoorDash, and Task Rabbit are prime examples of companies in this category that have arisen from such innovation.

In the basic business model of the gig economy, gig workers serve as contractors to an on-demand company, providing services to the company's clients (Donovan et al., 2016). Prospective clients request services through an online platform or smartphone application that allows them to search for providers or to specify jobs. Providers (i.e., gig workers) engaged by the on-demand company then provide the requested services and are compensated for the jobs they perform. Importantly, in contrast to the fixed-shift temp work of old, the new app-enabled gig platforms provide unprecedented flexibility to work only when the

[22] Other work shows the potential negative externalities of ridehailing on society (see Barrios et al (2019) for the effects on traffic fatalities and congestion).

worker wishes, and only for as long as they wish to work—be that one minute, one hour, or more.

While specific business models vary across the companies that control such platforms, with few exceptions, on-demand platform companies do not view their service providers as employees but rather as independent contractors that utilize the platforms to obtain referrals and communicate with clients. In addition, on-demand platform-companies offer providers the ability to select or refuse jobs, set their hours and level of participation, and control other aspects of their work. As a result, in some ways, the gig economy can be viewed as an expansion of traditional freelance work (i.e., ad hoc self-employed workers who generate income through a series of jobs and projects). Gig jobs, however, do differ from traditional freelance jobs in a number of ways. The user interface and brand built by the tech-platform company attracts clients, eliminating or reducing entry costs for providers (gig workers). These platforms may also attract potential service providers that have a wider variety of demographic, skill, and career characteristics. Because gig workers do not need to invest in establishing a company and marketing to a consumer base, operating costs may be lower. As a result, participation in the gig market is often more transitory than the traditional freelancing market of old.

The advent of app platforms such as Uber, Lyft, and others makes it easy for prospective providers to engage in gig work. These low barriers to entry allow gig work to substitute for other employment in down states of the world (Fos et al., 2019) or provide supplemental income opportunities.

2.2. Ridehailing

Ridehailing platforms were among the first app-enabled gig economy platforms to launch in the U.S. at a significant scale. Uber was the first ridehailing firm in the United States, launching in San Francisco in May 2010, and was followed two years later by Lyft and Sidecar. Ridehailing then expanded rapidly across the country. By the end of 2014, ridehailing firms operated in 80% of U.S. cities with a population of 100,000 or more. Much of the spread in ridehailing was driven by the convenience for users, stemming from new technology easing the matching of riders and drivers and enabling seamless payment through an app. This spread was facilitated by ridehailing firms' exemptions from (or willful disregard for) taxi and livery restrictions, which allowed them to expand supply during periods of high demand and adjust prices to encourage more riders and drivers to participate in the market.[3] Because ridehailing platforms were among the first gig platforms launched, their introduction into a city represents a shock to the supply of flexible gig work. It is this shock that we utilize for our empirical design.

2.3. Ridehailing gig opportunities and new business formation

To better understand the potential effects of gig employment on entrepreneurial activity, we take the Knightian view that an individual's decision to enter entrepreneurship versus full-time wage-employment is determined by the relative expected returns offered by the two choices. Returns confer utility, and agents choose the option that maximizes their expected utility (Lucas, 1978; Kihlstrom and Laffont, 1979; Jovanovic, 1982). Importantly, the decision relies on expected returns, and as such, this view does not necessarily suggest that entrepreneurs will have to engage in gig work per se, so much as that the option value of being able to access gig work opportunities in the event of failure or in low states of the world situations will affect their ex-ante decision to enter.[4] Put differently, a potential entrepreneur faces a more attractive choice for entering into entrepreneurship when gig economy opportunities exist for fallback purposes than when they do not.[5] Moreover, the existence of gig opportunities may enable a would-be entrepreneur to launch a business that would not provide sufficient income in the absence of supplemental gig income. Importantly, the "insurance" that the ready availability of gig opportunities provides to a would-be entrepreneur should be more valuable in particular when uncertainty regarding the viability or longevity of their proposed business is higher, or when more generally, economic uncertainty is higher.[6] Importantly, the ridehailing entry events studied in this paper could be considered shocks to the non-pecuniary benefits to alternative employment—most notably, work flexibility. Under this view, we would expect to see an effect on business formation decisions if would-be entrepreneurs value such non-pecuniary benefits (e.g. flexibility) either directly or as a means of insurance. We empirically investigate the effect of ridehailing platform gig opportunities on new business formation from this viewpoint.

[3] Many major ridehailing companies adjust pricing in real time to better match supply and demand, charging higher "surge pricing" fares during periods with high demand.

[4] Importantly, it is not unreasonable to think that income from fulltime job employment is unlikely to be substantially affected by the advent of the gig economy, or ridehailing. For example, a person working full time has limited hours to earn via gigs. The income that one earns as an entrepreneur, either in its ability to supplement entrepreneurial income or as a fallback in a failure state, however, should be more likely to be affected by the existence of gig opportunities, as these may serve to supplement income during slow times or in failure.

[5] Alternatively, one could assume that both fulltime and entrepreneurial income might be positively affected by the gig economy, but the entrepreneurial income is likely to be more affected—and we should also thus see an increase in ridehailing activity leading to an increase in new business launches.

[6] An alternative or additional mechanism that has been suggested by users of ridehailing services is that driving for ridehailing platforms also provides a way for drivers to market their other side businesses. We acknowledge that there may be other channels beyond income fallbacks at play which we cannot test. That said, our results support a first-order role for reduction in uncertainty/risk as (at least) a partial driver of any effect.

3. Data and sample

Our sample consists of all incorporated "places"[7] in the continental United States5F with population greater than or equal to 10,000 in 2010.[8] Our full sample covers the period 2000 to 2016; all results are robust to employing shorter pre-RH sample windows. The sample stops in 2016 due to that being the last year of availability in the SCP data. Our list of incorporated places is obtained from the Census Bureau and covers all self-governing cities, boroughs, towns, and villages in the United States.[9] (For ease of interpretation, we interchangeably refer to these as "cities" or "locations" throughout the text.) Our observations are measured at the quarterly level. The full sample contains 201,212 quarterly observations on 2959 "places" from 2000 to 2016, among which 1193 adopt RH prior to 2016. Fig. 3 shows the diffusion of RH across the United States, by cities and population. Diffusion of RH across U.S. cities began slowly, accelerating rapidly after 2013. Diffusion by population follows a standard S-curve, consistent with general historical patterns of new technology diffusion.[10]

3.1. Ridehailing launch and driver enrollment intensity

Data on RH launch dates for each city are obtained directly from Uber and Lyft.[11] The companies provided dates of service launch for each type of service launched: (i) UberBlack/UberTaxi, which allows customers to hail a livery or taxi vehicle; (ii) UberX/Lyft, which allow customers to hail regular cars driven by driver-partners; and (iii) UberPool/Lyft Line, which allow customers to share a hailed vehicle with others. We merge these dates with Census Bureau's incorporated place directory in 2010.

While data on driver enrollment and usage is not publicly available, other researchers have shown a strong correlation between Google trends for searches for RH keywords and actual driver uptake (Cramer and Krueger, 2016). To measure the intensity of RH adoption, we thus follow the spirit of the work of Cramer and Krueger (2016) and Hall et al. (2018) and use Google searches for the terms "Uber," "Lyft," and "rideshare."[12] The standard Google Trends index, which scales results from 0 to 100 based on the most popular term entered, does not easily allow comparisons across geographic areas and time periods. Instead, we use data from the Google Health Trends API, which describes how often a specific search term is entered relative to the total search volume on Google's search engine within a geographic region and time range and returns the probability of a search session that includes the corresponding term for that region and time period. This makes comparisons across locations and time feasible. We track trends for searches for these terms using the Google Health Trends API for all Nielsen Designated Market Areas (DMAs) at a monthly frequency from January 2004 to December 2016. We aggregate the data to the quarter level and match the DMAs to Census incorporated places using a crosswalk provided by Nielsen. Thus, in specifications that use log search share as a proxy for driver enrollment intensity, we interpret the coefficients in terms of percentage change in search share.

3.2. Entrepreneurial measures

We utilize three main outcome measures for our analysis of entrepreneurial activity. The first of these measures captures new business launches. The second provides a measure of financing for the types of new businesses we would expect to see launched under the Knightian-inspired conceptual model: small business loans to new businesses. Finally, we explore whether increased interest in entrepreneurship is apparent more generally, utilizing internet search share for terms related to starting a new business, a measure we term *entrepreneurial interest.*

3.2.1. New business launches

Our first outcome measure is the quantity of new business launches, measured by location and time period using business registrations. For this purpose, we obtain data on new, for-profit business registrations from the Startup Cartography Project (SCP, Guzman and Stern, 2019). The SCP leverages business registration records, which are public records created when an individual register's a new business as a corporation, LLC or partnership. Importantly, as noted by Guzman and Stern (2019), while it is possible to found a new business without business registration (e.g., a sole proprietorship), the benefits of registration are substantial, and include limited liability, various tax benefits, the ability to issue and trade ownership shares, and credibility with potential customers. Furthermore, all corporations, partnerships, and limited liability companies must register with a Secretary of State or equivalent to take advantage of these benefits: registering the firm triggers the legal creation of the company. As such, these records reflect the population of incorporated businesses operating in a location (which may differ from their state of incor-

[7] We use incorporated places, rather than Census Designated Places (CDPs), because CDP annual population estimates are not readily available, except by individual place download, whereas population data is available for incorporated places for mass download through the census.

[8] Some places in our sample had lower populations than 10,000 during the sample period, most notably during the period of 2001–2010. We impose the cutoff on population as measured in 2010. As an example, consider Hutto, Texas, a suburb of the Austin-RoundRock metro area. In 2001, Hutto had a population of 3,030, the lowest in our sample. By 2010, it had grown to over 14,000, mimicking the growth of the Austin metro area. As it has population above 10,000 in 2010, it is included in our sample. Our results are robust to permutations to this cutoff.

[9] https://www.census.gov/content/dam/Census/data/developers/understandingplace.pdf

[10] In the Online Appendix, we further demonstrate the robustness of our results to using shorter pre-sample periods.

[11] In this version, we use the exact cities indicated by Uber and Lyft, even if we suspect or believe that the launch covered adjacent cities as well (e.g., San Francisco launched in 2010, and there is no separate launch date for San Jose or Palo Alto). Since this means some places we include in our control may in fact be treated in later years in the sample as service expands slowly out beyond original boundaries, we are biasing against finding an effect of treatment.

[12] We use the freebase identifiers for term "Uber" (/m/0gx0wlr) and "Lyft" (/m/0wdpqnj). Freebase identifiers denote all searches that were classified to be about this topic.

poration) that take a form that is a practical prerequisite for growth.

The SCP dataset provided to us covers 49 states plus the District of Columbia over the period 2001 to 2014, and 47 States plus the District of Columbia from 2015 to 2016. For each state, the SCP data includes records on the complete population of firms satisfying one of the following two conditions: (i) a for-profit firm physically located in the local jurisdiction, or (ii) a for-profit firm whose jurisdiction is in Delaware but whose principal office address is in the local state.

The SCP dataset provides a number of variables of interest to entrepreneurship researchers. We focus here on two specifically: (i) the quantity of new business registrations in a Census incorporated place in a given year and quarter, and (ii) an Entrepreneurial Quality Index (EQI), which is a measure of average quality within any given group of firms, and represents a prediction for the probability of a growth outcome for a firm within a specified population of start-ups in a specific period (More information on this measure can be found in Guzman and Stern 2019).

3.2.2. Lending to new businesses

Our second outcome measure is the volume of lending under the Small Business Administration's (SBA) 7(a) loan program. The Small Business Administration (SBA) 7(a) Loan Guarantee program is one of the most popular loan programs offered by the agency. Under the program, a 7(a) loan guarantee is provided to lenders to make them more willing to lend money to small businesses with weaknesses in their loan applications, such as new businesses startups that lack the cash flow history to provide a lender with the assurance of continued ability to pay back a loan. 7(a) loans may be used for such business purposes as purchasing land or buildings, equipment, machinery or supplies; for long-term or short-term working capital; for refinancing; or for the purchase of an existing business. They are limited to a maximum of $2 million, with an SBA loan guarantee of 75%. The terms of SBA 7(a) loans are up to 25 years for real estate and equipment and seven years for working capital, and interest rates are set and capped based on the prime rate, the size of the loan, and the maturity of the loan.

We utilize data on SBA lending under the 7(a)-guarantee program that is released quarterly under the Freedom of Information Act. The Startup Cartography Project then matched the SBA loans to their business registration records, providing us with business registration data for approximately half the loans in the dataset. As each business registration contains a date of registration, for each location and quarter, we can then calculate two measures: the number of loans made to new businesses registered in the prior six months, and the number of loans made to new businesses registered in the prior twelve months.[14]

3.2.3. Entrepreneurial interest

Ideally, we would observe not only actual entry into entrepreneurship, but also underlying expectations regarding the uncertainty associated with entrepreneurial entry. Unfortunately, to our knowledge, no survey data regarding entrepreneurial expectations at the city or county level exists. Instead, we construct a measure that is meant to capture the level of general interest in entrepreneurial activity in the area, as measured by searches for entrepreneurship-related terms online. Our measure, which we dub *Entrepreneurial Interest*, utilizes the google Health Trends interface to extract data on searches for entrepreneurship related terms such as “how to start a business” and “how to incorporate.”[15] As previously noted, the Google Health Trends API describes how often a specific search term is entered relative to the total search volume on Google search engine within a geographic region and time range, and returns the probability of a search session that includes the corresponding term, which makes comparisons across locations and time feasible.[13] We track trends for searches for these terms using the Google Health Trends API for all Nielsen Designated Market Areas (DMAs) at monthly frequency from January 2004 to December 2016. We aggregate the data to the quarter level and match the DMAs to Census incorporated places using a crosswalk provided by Nielsen.

Using this data, we then define three outcome measures: (i) whether a city is in the top quartile of cities for probability of search for entrepreneurship-related terms in that period; (ii) whether a city is in the bottom quartile of cities for probability of search for entrepreneurship-related terms in that period; and (iii) actual search share.

3.3. Measures of economic and entrepreneurial uncertainty

If the gig work opportunities provided by the entry of ridehailing platforms serve to reduce the uncertainty associated with entry into entrepreneurship, we would expect to see that the value of these opportunities would be higher in areas where ex ante economic or entrepreneurial uncertainty is higher. We utilize four proxies for general economic and entrepreneurship-specific uncertainty.

3.3.1. Wage growth volatility

Our first proxy attempts to capture labor market uncertainty using variation in wage growth. We construct a measure of the volatility in wage growth in each location in our sample. We utilize data from the Bureau of Labor Statistics (BLS) Quarterly Census of Employment and Wages (QCEW) for this purpose. Wage growth volatility is computed as the sum of the variances and covariances of the wage growth rate in the various industry sectors, weighted by the employment share of each individual sector. We compute this measure at the county level.

[14] We use the Census 2010 zip-to-place crosswalk to match the zip of the SBA borrower to census places. In situations where the zip of the borrower locates in multiple census places, we use the borrower city information to refine the matching. Using this process, we are able to accurately match 95% of SBA loans to census places.

[15] Specifically, we use the terms: “start a business,” “start your own business,” “start a company,” “how to incorporate,” “entrepreneurship,” and “become an entrepreneur.”

[13] These probabilities are calculated on a uniformly distributed random sample of 10%-15% of Google web searches. Mathematically, the numbers returned from the Google Trend API can be officially written as: $Value_{[time,termrestriction]} = P(term - restriction|timeandgeo - restriction)$.

For our computations, we derive a variance-covariance matrix from a trend-adjusted time series of county-industry employment data from 2000 to 2010. Mathematically, the measure of wage growth volatility for the portfolio of industries in a given city is then expressed as:

$$\sigma_p^2 = \sum_i w_i^2 \sigma_i^2 + \sum_{i \neq j} \sum_{i \neq j} w_i w_j \sigma_{ij}$$

where w_j denotes the proportion of total employment in industry j, σ_j^2 denotes the variance of wage growth rate in industry j, and p denotes city.

3.3.2. Volatility of zip-level business income

In addition to general labor market uncertainty, we next turn to looking at a measure that is more closely tied to the volatility of entrepreneurial earnings in an area. Specifically, we collect data from the Internal Revenue Service (IRS) that measure both the number and amount of business or professional net income filed. These data are based on administrative records of individual income tax returns (Forms 1040) from the IRS Individual Master File system and are reported at the zip code level in the Statistics of Income dataset.

Specifically, we construct two measures: (i) the volatility of business income in the CBSA in 2010, the year before ridehailing entry (we refer to this measure as a cross-sectional volatility measure); and (ii) the volatility of historical business income in a CBSA over a five-year period prior to 2010 (we refer to this measure as a time-series volatility measure). Ideally, to measure the volatility of business income in an area, we would use data on the business income of each entrepreneur in that area, and then take the standard deviation. However, the highest level of geographic granularity reported by IRS is at the zip code level. Hence, we first calculate the average business income for each entrepreneur in a zip code, and then calculate the volatility of business income across all zip codes in a CBSA.

Below we describe the detailed steps to construct the two variables. First, we collect variables A00900 and N00900 from the IRS SOI dataset. These variables measure the amount and number of business or professional net income (less loss) filed, respectively. Second, we divide the amount of business or professional net income by the number of filings in a zip code to obtain the average business income per filing. Third, we calculate the standard deviation of the average business income per filing across all zip codes in a CBSA. Fourth, we calculate the standard deviation of the average business income per filing across all zip codes in a CBSA across a five-year period from 2005 to 2010 (we skip year 2008 and years before 2005 because the variable that measures the number of business income filings are not consistently available for those years.)

3.3.3. Business bankruptcy rates

Finally, we construct a measure that relates more directly to the failure risk of launching a new business: the business bankruptcy rate in an area. Specifically, we obtain data on the county-year counts of business bankruptcy cases from U.S. Courts, Report F-5A. The U.S. Courts first started to report this statistic during 12-month period ending on March 31, 2013. That is the year of the data we use. We then match the cities in our sample to counties using crosswalks from the U.S. Census.

We then normalize the number of bankruptcies in a county by the number of business income filings in the county, reported by IRS SOI. While we would ideally want to measure only entrepreneurial failure rate, rather than overall business failure rate, no such comprehensive data is available at the city level. Still, we view business bankruptcies as a useful proxy for failure risk in its absence.

3.4. Control variables and city characteristics

We use several measures to explore heterogeneity by city characteristics and as control variables in our models. We obtain annual city population estimates and population density from the U.S. Census and annual county income per capita from the Bureau of Economic Analysis. Controlling for population, per capita income, and unemployment rate—which vary by time and location—are of first order importance as they provide a proxy for specific concerning confounders.

To examine how variation in individuals' credit constraints affect the impact of the gig economy on entrepreneurship, we use a dataset of anonymized individual credit bureau records in 2010 to further construct several ex ante proxies for income and credit constraints. The credit bureau data contain a 1% representative sample of all U.S. residents selected based on the last two digits of their social security number. This sampling procedure produces a random sample of individuals because the Social Security Administration sequentially assigns the last 4 digits of social security numbers to new applicants regardless of geographical location. We calculate the annual average personal income and credit score for each city, as well as the fraction of low income and subprime borrowers. Following the cutoff used by the credit bureau, we identify an individual as a subprime borrower if his or her credit score is below 660. Approximately 44% of individuals in our sample are subprime.

To explore heterogeneity by demographic characteristics, we develop city-level measures of education levels using the Census Bureau's 5-year American Community Survey data. We obtain both the fraction of individuals with a high-school degree and the fraction of individuals with a bachelor's degree for each city in 2010. We also calculate county-level racial and ethnic composition measures, such as the fraction of Hispanic population and the fraction of Black and African American population, using Census Bureau's 2010 county population estimates.

3.3. Summary statistics

Table 1, Panel A presents summary statistics for the places in our sample over the sample period. The places average 54,348 in population and have an income per capita of $39,300. Prior to the advent of ridehailing in 2010, 44.1% of borrowers in our sample places were subprime, 49% were low income, 85.6% have at least a high school degree, and 28.6% have at least a Bachelors degree.

Table 1
Summary Statistics.

Variable	Mean	SD	P10	Median	P90
Business Registration	133.7	391.2	5.0	47.0	283.0
SBA Loans to New Businesses (Count)	0.4	1.7	0.0	0.0	1.0
SBA Loans to New Businesses (Amount $K)	127.6	718.4	0.0	0.0	215.0
Google Search Share	713.0	382.2	443.1	631.7	1109.8
Population	54,348.2	199,878.5	11,224.0	23,398.0	93,807.0
Income Per Capita	39.3	12.2	26.5	37.1	55.1
Credit Score	669.5	33.6	626.8	668.7	713.7
Fraction of Subprime Borrowers (%)	44.1	13.5	26.2	44.3	61.2
Fraction of Low Income (%)	49.1	12.1	32.4	49.6	64.4
Fraction of High School Degree (%)	85.6	9.3	74.1	87.4	95.4
Fraction of Bachelor Degree (%)	28.6	15.1	12.9	24.7	50.6
Fraction of Black and African American Population (%)	11.9	12.2	1.1	7.7	27.3
Fraction of Hispanic Population (%)	15.8	16.3	2.2	9.1	40.9
Wage Growth Volatility (%)	0.4	0.3	0.2	0.3	0.8
Business Income Volatility (Cross-Sectional)	6912.3	4496.6	2482.5	5633.9	12,290.8
Business Income Volatility (Time-Series)	7237.2	4334.0	2845.1	5845.6	14,350.8
Business Bankruptcy Rate (%)	3.3	7.0	0.1	0.9	8.1
Entrepreneurial Quality Index	0.0006	0.0015	0.0002	0.0004	0.0008

Notes: The sample contains 201,212 quarterly observations on 2959 census incorporated places from 2000 to 2016. *Business Registration* measures the number of new business registrations in a city-quarter. *SBA Loan to New Businesses (Count)* measures the total number of SBA 7(a) loans issued to businesses registered within 12 months. *SBA Loan to New Businesses (Amount $K)* measures the total amount of SBA 7(a) loans issued to businesses registered within 12 months. *Google Search Share* measures the share of google search volume for the terms such as "how to start a business". *Subprime Borrowers* measures the fraction of borrowers in a city-quarter that has below 660 credit score. *Wage growth volatility* is the weighted sum of the variances and covariances of wage growth rate in the sectors of the economy, weighted by the employment share of each individual sector. Business Income Volatility (Cross-Sectional) is the cross-zip standard deviation of IRS-measured business income in a CBSA in 2010. Business Income Volatility (Time-Series) is the cross-zip, cross-year standard deviation of IRS-measured business income in a CBSA from 2005 to 2010. Business Bankruptcy Rate is the county-year counts of business bankruptcy cases reported by U.S. Courts divided by the number of business filings reported by IRS, measured in 2013. *Entrepreneurial Quality Index* measures average entrepreneurial quality in a given city-quarter, as defined in Guzman and Stern (2019). More detailed explanations of the variable constructions can be found in the Data and Sample section of the paper.

As can be seen from the distributional statistics in the table, there is wide variation across all these characteristics across the sample. The table further presents summary statistics on our entrepreneurial activity measures over the sample period.

4. Empirical analysis

To assess the impact of the insurance against entrepreneurial income-related volatility on entrepreneurial activity, we employ a standard generalized difference-in-differences approach. We index cities by c and time by t. We estimate models of the following form:

$$\log(1 + outcome_{t,c}) = \propto_c + \gamma_t + \beta' X_{t,c} + \theta_c t + \delta POST_t * TREATED_c + \varepsilon_{t,c},$$

where $outcome_{t,c}$ is one of our measures of entrepreneurial interest or activity in city c in quarter t, $\propto_c$ is a city fixed effect, γ_t is quarter-year fixed effect, $X_{t,c}$ is a vector of time-varying, city specific control variables (lagged one quarter), and $\theta_c t$ is a city-specific linear time trend.[13]

We use robust standard errors clustered at the city level. Our observations are at the quarterly level and cover the first quarter of 2001 through the fourth quarter of 2016. For each outcome measure, we present estimates for models estimated on the full sample (2001–2016), a subsample using a shorter pre-period (2005–2016), and restricting the sample only to those cities that are ever-treated by ridehailing during our sample period. This last specification is meant to assuage concerns that our estimates may be driven solely by the differences between never-treated and ever-treated cities.

4.1. New business registration

We begin by exploring new business launches, using the registrations of new companies. Table 2 employs our DD specification, where our outcome measure is the natural logarithm of one plus the number of new business registrations in the city/quarter. For brevity, we report only the coefficient on the variable of interest—$POST_t * TREATED_c$ in the table. We report OLS specifications, but our results remain robust to the use of count models instead (though we note that interpretation of interaction terms in such models is not straightforward and cannot simply be determined by the sign of the coefficient—see Ai and Norton 2003). We estimate four models: column (1) presents estimates from the full sample period, column (2) shortens the sample pre-period to post 2005, column (3) restricts to solely ever-treated cities, and column (4) uses only ever-treated cities, but with the sample post-2005. The second pair of models are meant to assuage concerns

[16] For robustness, we also estimate all our models with the inclusion of a location-specific quadratic trend as well, with qualitatively similar results.

Table 2
Gig Economy and New Business Registration.

	Log (1+New Business Registration)			
	(1) >2000	(2) >2005	(3) Treat = 1	(4) >2005 & Treat=1
Treat X Post	0.0389***	0.0676***	0.0527***	0.0594***
	(0.0112)	(0.0121)	(0.0105)	(0.0108)
Log Pop	0.7358***	0.3212***	0.7164***	0.1987*
	(0.0928)	(0.1087)	(0.1189)	(0.1094)
Log Income Per Capita (lag)	0.5212***	0.5094***	0.2297***	0.0262
	(0.0572)	(0.0668)	(0.0715)	(0.0728)
Unemployment Rate (lag)	0.0004	−0.0052**	−0.0125***	−0.0186***
	(0.0018)	(0.0021)	(0.0021)	(0.0023)
Observations	195,446	139,225	114,384	81,761
R-squared	0.9590	0.9592	0.9665	0.9683
City FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes

Notes: This table presents results from generalized difference-in-difference regressions. The dependent variable, Log *(1+New Business Registration)*, is the natural logarithm of one plus the number of new business registrations in a city-quarter. *Treat X Post* is a dummy variable that equals one if city c adopted at least one ridehailing service (proxy for gig economy arrival) at time t. Control variables in the regressions include the natural logarithm of population, income per capita (lagged one quarter), and unemployment rate (lagged one quarter). Standard errors, adjusted for clustering at the city level, are reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

that any results might be driven solely by differences between ever-treated and never-treated cities.

Across all four models, we observe a similar pattern. The coefficient on $POST_t * TREATED_c$ ranges from 0.03 to 0.06, depending on the sample employed, consistent with the arrival of the gig economy being associated with an increase of approximately 3 to 6% in new business registrations. Fig. 2 graphs the coefficients at the annual level around the entry point; the graph suggests that the parallel trends assumption holds.

Appendix Table A3 and A4 and Figures A3 and A4 further present robustness to adjustments for staggered D-in-D models suggested by Goodman-Bacon (2021) and Borusyk and Jaravel (2017). Borusyk and Jaravel (2017) explain that staggered difference-in-difference estimates can subject to under-identification issues. Our results are robust to including a large set of control cities, which mitigates the concern. However, because we include city-specific linear trends in our main specification, the issue can arise again. Following Borusyk and Jaravel (2017) suggestion, in Appendix Table A3, we estimate difference-in-difference models where time effects are identified solely from the control cities. Specifically, we use a two-step process. In the first stage, we manually detrend outcome and control variables using the means of the variables estimated from the control cities. In the second stage, we run our main specification using the detrended variables, excluding time fixed effects. In Figure A4, we present the dynamic difference-in-difference coefficients omitting the relative time coefficients from the year before treatment and the first year available, following the suggestion of Borusyk and Jaravel (2017). Inferences remain unchanged.

To help give more context on the empirical design, we also run the Goodman-Bacon decomposition and analyze the weights underlying our staggered DiD regressions. As Goodman-Bacon (2021) notes, the two-way fixed-effect (TWFE) estimator is a weighted average of all potential 2 × 2 DiD estimates where the weights are based on both the group size and the variance in the treatment. The decomposition results are presented in Table A4 and Figure A3. The first component is "Earlier Treatment vs. Later Control", which compares the cities that adopted RH earlier to the cities that have not yet adopted RH. In Panel A, the average coefficient estimate derived from this source of variation is −0.006 and has a weight of 0.176. The second component is "Later Treatment vs. Earlier Control", which compares the cities that adopted RH later to the cities that have already adopted RH. The average coefficient estimate derived from this source of variation is −0.02 and has a weight of 0.014. The third component is "Treatment vs Never Treated", which compares cities that adopted RH at some point during the sample period and those that did not. The average estimate derived from this source of variation is 0.02 and has a weight of 0.81.

The Goodman-Bacon DD decomposition with the inclusion of control variables is only derived for a "two-group" comparison level (the regression coefficient relating two timing groups across the whole panel), as opposed to our "2 × 2 DD" comparison level (the regression coefficient relating two timing groups only on the subset of periods where one switches). As a result, we can only see the aggregate of the four groups in a model with no control variables, and as a result, these estimates will differ somewhat from our main model estimates, as our main models do include controls. The Goodman-Bacon decomposition exercise, however, allows the reader a further window into variations that may exist among comparison groups. We also note that the comparisons in the Goodman-Bacon

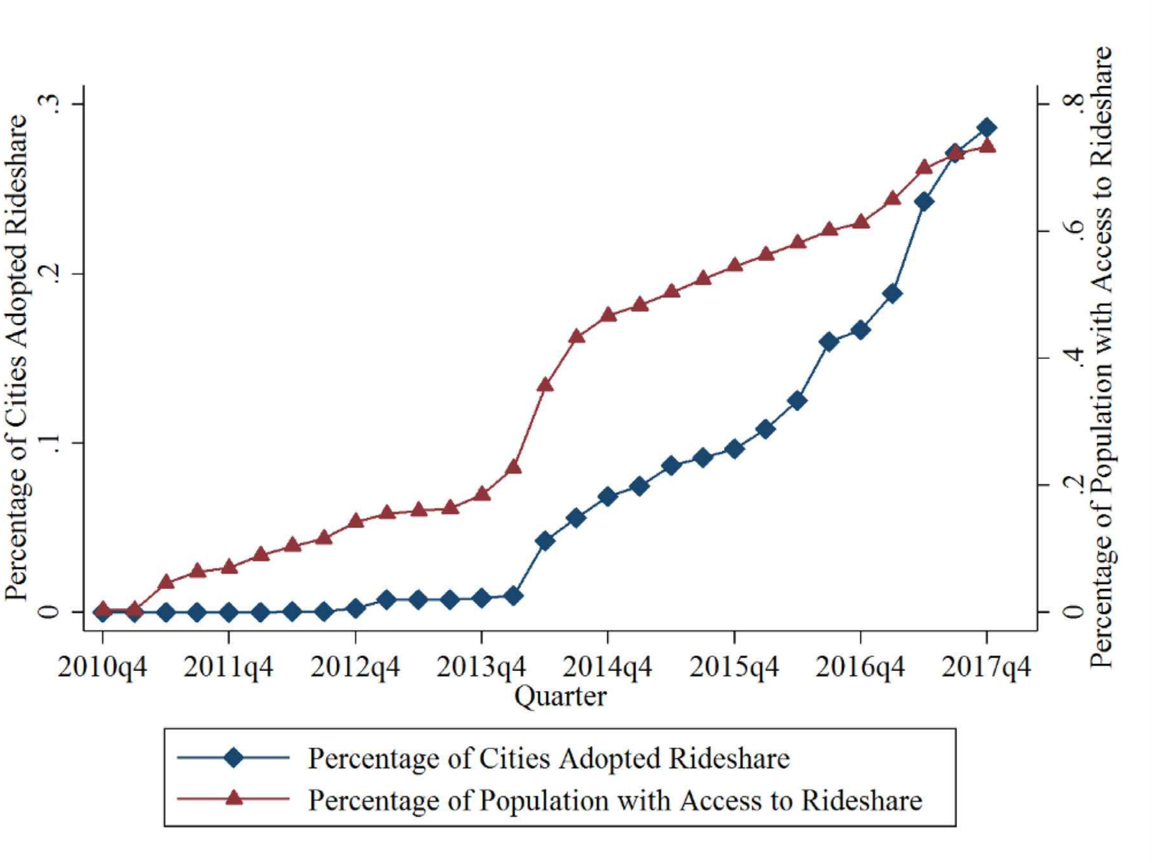

Fig. 1. Ridehailing Diffusion.
This figure shows the diffusion of ridehailing across the U.S. by cities and population. The sample consists of all census incorporated places in the United States. The navy (red) line graphs the percentage of cities (population) that adopted ridehailing in each quarter between the fourth quarter of 2010 and the fourth quarter of 2017.

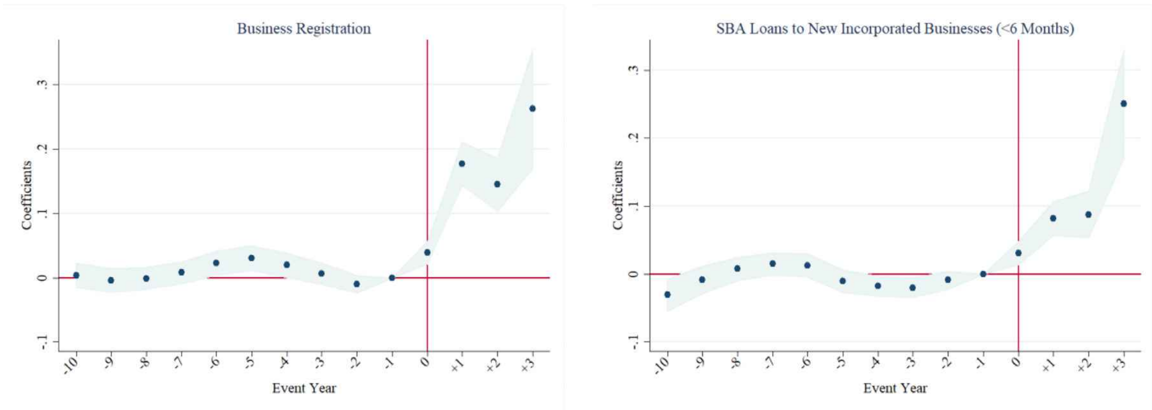

Fig. 2. Difference-in-Difference Estimators.
This figure displays the regression coefficient estimates for our three main outcomes and two-tailed 95% confidence intervals based on standard errors clustered at the city level. The outcome variables in Panel A is the natural logarithm of one plus new business registrations. The outcome variable in Panel B is the natural logarithm of one plus the number of SBA loans issued to newly-registered business. To map out the pattern in the counterfactual treatment effects, we regress the outcome variables on the lag and lead indicators (bunched by four quarters) of the ridehailing entry. The sample includes all rideshare cities in years after 2005 (the specifications used in the Column (4) of Table 2 and Table 5A). The control variables include the natural logarithm of population, income per capita (lagged one quarter), and unemployment rate (lagged one quarter). The vertical red line indicates the quarter of entry.

Table 3
Intensity of Gig Economy Adoption and New Business Registration.

	Log (1+New Business Registration)			
	(1) >2000	(2) >2005	(3) Treat = 1	(4) >2005 & Treat=1
Treat X Post X Log (Ridehailing-Related Search Share)	0.0772***	0.0765***	0.0660***	0.0664***
	(0.0109)	(0.0111)	(0.0111)	(0.0113)
Log Pop	0.4114***	0.2942***	0.3274***	0.1712
	(0.0981)	(0.1087)	(0.1047)	(0.1097)
Log Income Per Capita (lag)	0.4415***	0.4587***	−0.0303	−0.0262
	(0.0630)	(0.0672)	(0.0702)	(0.0723)
Unemployment Rate (lag)	−0.0000	−0.0034	−0.0135***	−0.0164***
	(0.0021)	(0.0021)	(0.0023)	(0.0023)
Observations	151,061	139,225	88,629	81,761
R-squared	0.9598	0.9593	0.9685	0.9684
Lower Order Interaction Terms	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes

Notes: This table shows how the effect of ridehailing on new business registrations vary with the intensity of ridehailing service. The dependent variable, Log *(1+New Business Registration)*, is the natural logarithm of one plus the number of new business registrations in a city-quarter. Log *Ridehail Google Search Share* is the natural logarithm of Google search share for the terms "Uber," "Lyft," and "rideshare." *Treat X Post* is a dummy variable that equals one if city c adopted at least one ridehailing service (proxy for gig economy arrival) at time t. Control variables in the regressions include the natural logarithm of population, income per capita (lagged one quarter), and unemployment rate (lagged one quarter). Standard errors, adjusted for clustering at the city level, are reported in parentheses. ∗∗∗, ∗∗, and ∗ represent statistical significance at the 1%, 5%, and 10% levels, respectively.

decomposition that involve "early Treated" vs "later Control" may attenuate the estimated effect, as there could be positive drift in entrepreneurial activities for the not-yet-treated cities after treatment. This would bias the coefficient downwards—potentially even making it even negative—depending on the sample period length. To put more color on it, cities treated in future years are used as controls in comparing the early treated cities compared to later treated cities. But these later-treated cities are only observed in the control (untreated) state for a limited amount of time (until treated). In our sample, we have cities experiencing entry between 2010 and 2016. As a result, the early cities only get a few quarters of "clean" comparisons with the later treated cities in most cases, with the remaining comparisons being of treated to treated, effectively. A similar issue arises when comparing later treated to all previously treated cities. The treated cities are used as controls, and to the extent that there is any dynamics in the treatment effects, that will attenuate the estimated coefficient. As a result, there is a tradeoff between the potential bias from this sort of contamination versus using never-treated cities which potentially may be different in some other fundamental way. We attempt to give the reader data to draw their own inferences on this tradeoff by presenting the adoption models and hazard models to try to rule out potential differences, as well as including control variables such as population and income.

On the plus side, having a large group of never-treated cities is advantageous, conditional on never-treated cities being similar to eventually-treated cities, as the regression then puts less weight on the "problematic" 2 × 2 DDs that use already treated units as controls (Barrios, 2021; Goodman-Bacon, 2021). For both outcome variables, the weight given to treatment versus never treatment is high. This helps us get away from bias in the treatment effect heterogeneity among the cohorts. For example, when analyzing the new business registrations, we see that the comparisons between "Earlier Treatment vs. Later Control" and "Later Treatment vs. Earlier Control" gives us negative estimates. This is consistent with dynamically changing treatment effects biasing the coefficient from the TWFE estimate that we use.[17]

To add further perspective to the negative coefficient on "Earlier Treatment vs. Later Control" in the Goodman-Bacon decomposition, we conduct two untabulated exercises[18] to assess the statistical power of this estimate relative to the "Treated vs. Never-Treated" coefficient in the decomposition exercise without controls. First, we construct a confidence interval of the Goodman-Bacon coefficient on Treated vs Never Treated using the individual 2 × 2 estimates from this group, and show that the coefficient on "Earlier Treatment vs. Later Control" lies somewhere between the 15th and 20th percentile of the left-tail distribution. The confidence interval analysis supports the interpretation that the estimate of −0.006 is not from an extreme end of the distribution and not statistically different from that of the coefficient of treated vs. never treated.

[17] To summarize, the weights underlying staggered DiD regressions come from the size of each subgroup (what share of units—cities—are in the treatment and control group for a given pair, and what share of time periods are used in a given 2x2 subsample), and the variance of treatment (how close to the beginning/end of the subsample window does treatment turn on). In our specific setting, most of the weight for our estimates for both outcomes come from variation between the treated cities and the never treated cities.

[18] Available from authors upon request.

Table 4
Modeling Ridehail Adoption: Cox Proportional Hazard Model.

	(1) All Cities	(2) Rideshare Cities	(3) All Cities	(4) Rideshare Cities
Annual% Change in Business Registration	0.9961	0.9941		
	(0.0056)	(0.0056)		
Annual% Change in Business Registration Per Capita			0.9961	0.9942
			(0.0056)	(0.0056)
Annual% Change in Pop	1.1977***	0.9191	1.1976***	0.9189
	(0.0430)	(0.0566)	(0.0430)	(0.0566)
Annual% Change in Income	1.2015***	1.1599***	1.2015***	1.1599***
	(0.0483)	(0.0553)	(0.0483)	(0.0553)
Annual% Change in Unemployment Rate	0.9657	0.8009**	0.9657	0.8009**
	(0.0774)	(0.0790)	(0.0774)	(0.0790)
Log Pop	1.7464***	1.3980***	1.7464***	1.3980***
	(0.0437)	(0.0332)	(0.0437)	(0.0332)
Log Income Per Capita (lag)	1.2486***	1.1739***	1.2486***	1.1739***
	(0.0417)	(0.0382)	(0.0417)	(0.0382)
Unemployment Rate (lag)	1.3246***	1.1979***	1.3246***	1.1979***
	(0.0485)	(0.0445)	(0.0485)	(0.0445)
Observations	41,664	23,950	41,664	23,950

Notes: This table presents results from proportional cox hazard model estimations. The reported coefficient estimates are hazard ratios. We collapse observations at the city-year level to calculate annual percentage changes in business registration, business registration per capita, population, income, and unemployment rate. All variables are standardized to have a mean of 0 and standard deviation of one to facilitate comparison between estimated hazard ratios. In Columns (1) and (3), we include all cities in our sample. Columns (2) and (4) limit the analysis to cities that adopted rideshare during our sample period. Standard errors, adjusted for clustering at the city level, are reported in parentheses. $***$, $**$, and $*$ represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Second, we conduct a simulation exercise, whereby we obtain the 2 × 2 "Treated vs. Never-Treated" coefficient estimates and construct subsamples with similar weight to that shown by the Goodman-Bacon decomposition (0.176). For each subsample, we compute the implied Goodman-Bacon coefficient estimate, and plot the histogram of the distribution of these implied estimates. Consistent with the inferences of the confidence intervals calculated above, the coefficient −0.006 is located between the 20th and 30th percentile of the distribution of the estimated coefficients, further supporting the interpretation that the estimate −0.006 is not from an extreme end of the distribution. Overall, these exercises support the overall conclusion from our main analysis.

Appendix Tables A1 and A2 and Figure A1 present robustness to restricting the sample to post 2010 and adding a quadratic trend. The coefficients in Table A1 are, on average, smaller than our main coefficient estimates (Table 2 and Table 5). This discrepancy can be a result of a few factors. First, as Goodman-Bacon (2021) notes, when we drop the pre-period for both treatment and control cities, we effectively change the weights based on the variance of the treatment for the observation in the model. In this case we have trimmed most of the pre-period. Additionally, when we drop the pre-period observations in models with a linear trend, we are changing the estimated trend—it is now being generated off a much larger proportion of the post-period. This is the point made by Wolfers (2006) and addressed by Borsyuk and Jaraval (2017). In other words, in the post-2010 sample, where we have a shorter sample, we are now mainly using post-periods to estimate the linear trend and thus biasing our estimates down. This issue will be especially exacerbated with the SBA loans outcome variable, as it contains many zeros (relative to the business registration data). Therefore, estimating a linear trend requires more data points and, hence, a longer sample. Thus, removing the pre-periods can have a larger downward pull on the estimate. In Appendix Figure A2, we further examine the sensitivity of our coefficient estimates by removing one state at a time and re-estimating the specification. As the plotted coefficients suggest, our inferences remain unchanged.

In the main models presented in Table 2, we employ the first launch of a RH service, irrespective of the type of service, as our treatment date. Take-up of these services, however, is likely to intensify over time. To explore this issue, we interact our $TREATMENT$ indicator with the intensity of Google searches for ridehailing-related terms measure and re-estimate our models. Table 3 replicates the models in Table 2, but with additional interaction with this adoption intensity proxy. The resulting estimates are consistent with an increase in business registrations following an increase in our Google Trends adoption intensity measure. For all four models, the coefficient estimates on $TREAT * POST * INTENSITY$ is positive and statistically significant. Thus, as our proxy for gig economy adoption intensity (Google trends search share for ridehailing keywords) increases, so do new business launches.[19]

[19] An ideal additional test would be to look at U.S. cities where RH was introduced and then withdrawn. Unfortunately, these cities are few, and the circumstances do not allow for the types of tests we would want. For example, Uber and Lyft both withdrew from the Austin market at one point in 2016 in a regulatory dispute, but at least five other RH services were still operating and took up the slack. Uber and Lyft then returned to the Austin market within a year, after Texas passed HB100, creating

Table 5
Gig Economy and Small Business Loans to Newly Registered Businesses.

Panel A: Firms Registered Within 6 Months

	Log (1+ SBA Loans to Newly Registered Firms (<6 M))			
	(1) >2000	(2) >2005	(3) Treat = 1	(4) >2005 & Treat=1
Treat X Post	0.0662***	0.0892***	0.0421***	0.0566***
	(0.0082)	(0.0095)	(0.0093)	(0.0103)
Log Pop	0.1493***	0.4185***	0.1710***	0.5124***
	(0.0414)	(0.0835)	(0.0533)	(0.1101)
Log Income Per Capita (lag)	0.1573***	0.3872***	0.2024***	0.4891***
	(0.0400)	(0.0487)	(0.0632)	(0.0787)
Unemployment Rate (lag)	−0.0017	−0.0024*	−0.0022	−0.0034
	(0.0012)	(0.0014)	(0.0018)	(0.0022)
Observations	198,238	142,017	115,024	82,401
R-squared	0.3942	0.4132	0.4578	0.4739
City FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes

Panel B: Firms Registered Within 12 Months

	Log (1+SBA Loans to Newly Registered Firms (<12 M))			
	(1) >2000	(2) >2005	(3) Treat = 1	(4) >2005 & Treat=1
Treat X Post	0.0838***	0.1136***	0.0552***	0.0759***
	(0.0092)	(0.0107)	(0.0103)	(0.0115)
Log Pop	0.1769***	0.5245***	0.1960***	0.6084***
	(0.0465)	(0.1070)	(0.0612)	(0.1418)
Log Income Per Capita (lag)	0.2182***	0.5346***	0.2856***	0.6675***
	(0.0446)	(0.0581)	(0.0712)	(0.0958)
Unemployment Rate (lag)	−0.0028**	−0.0043***	−0.0038*	−0.0058**
	(0.0013)	(0.0016)	(0.0021)	(0.0025)
Observations	198,238	142,017	115,024	82,401
R-squared	0.4291	0.4485	0.4934	0.5094
City FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes

Notes: This table presents results from generalized difference-in-difference regressions. The dependent variable is the natural logarithm of one plus the number of SBA loans to firms that are incorporated in less than 6 months (Panel A) or 12 months (Panel B). *Treat X Post* is a dummy variable that equals one if city c adopted at least one ridehailing service at time t. Control variables in the regressions include the natural logarithm of population, income per capita (lagged one quarter), and unemployment rate (lagged one quarter). Standard errors, adjusted for clustering at the city level, are reported in parentheses. ∗∗∗, ∗∗, and ∗ represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Certainly, ridehailing companies' choice of cities to launch in first was probably not random. The main concern for our identification approach then centers around whether Uber and Lyft were specifically selecting cities in which to roll out services based on the trends in entrepreneurial activity and business registration in that city. To interpret our estimates with an eye towards causality, business registrations themselves would ideally not be a predictor of entry. Table 4 presents estimates from a Cox proportional hazards model for ridehailing entry into cities. The reported coefficient estimates are hazard ratios. We collapse observations at the city-year level to calculate annual percentage changes in business registration, business registration per capita, population, income, and unemployment rate. As can be seen from the table, while population and income strongly predict entry timing, while there is no statistically significant loading on trends in new business registrations.

4.2. Loans to newly registered businesses

Next, we turn to our second outcome measure based on the financing channel for new businesses. We do this using SBA 7(a) small business loans to newly registered businesses, as small, traditional businesses represent the vast majority of new business starts (as opposed to innovation-

looser statewide rules that superseded Austin's (their return led to immediate massive drops in volume for the competitors that sprung up in their absence). In Las Vegas, the other city we are aware of, RH was introduced, then outlawed after only one month of service.

Table 6
Gig Economy and Entrepreneurial Interest (Search Share).

	Log (1+Google Search Share)			
	(1) >2000	(2) >2005	(3) Treat = 1	(4) >2005 & Treat=1
Treat X Post	0.1136***	0.0677***	0.1237***	0.0619***
	(0.0121)	(0.0094)	(0.0148)	(0.0108)
Observations	153,853	142,017	89,269	82,401
R-squared	0.6140	0.6663	0.5875	0.6473
Controls	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes

Notes: This table presents the effect of gig economy arrival on entrepreneurial intent, measured using google search share for entrepreneurship-related phrases, such as "start a business", "how to incorporate", and "become an entrepreneur". The outcome variable is the natural log of one plus google search share. Control variables in the regressions include the natural logarithm of population, income per capita (lagged one quarter), and unemployment rate (lagged one quarter). Standard errors, clustered at the city level, are reported in parentheses. ∗∗∗, ∗∗, and ∗ represent statistical significance at the 1%, 5%, and 10% levels, respectively.

driven startups, which are typically funded by venture capital and are much rarer).

Table 5 presents estimates from models similar to those in Table 2, using the SBA loan counts to new businesses registered in the last 6 (Panel A) or 12 months (Panel B). Here, once again, we see that the emergence of the gig economy, in the form of entry of a RH platform, is associated with an increase in loans to new businesses, consistent with—and of a magnitude corresponding to—the increase in new business registrations suggested by the models in Table 2. Thus, across both of the outcome measures—realized starts and financing—we observe a consistent pattern: the arrival of the gig economy is associated with an increase in entrepreneurial entry activity, consistent with the hypothesis that the gig economy serves as an income supplement and/or insurance against entrepreneurial-related income volatility.

4.3. Entrepreneurial interest

As we have seen in the prior two subsections, the arrival of the platform-enabled gig economy appears to be associated with a significant increase in entrepreneurial entry. We next turn to the measure which serves as our proxy interest in considering entrepreneurial entry more broadly: internet searches for terms and phrases directly related to launching a business—which we term entrepreneurial interest. In Fig. 3, we explore the relationship between entrepreneurship search share and business registrations. Fig. 3 presents a scatter plot of business registrations against search share for entrepreneurial terms, for the pre-ridehail subsample and the post-ridehail subsample. For both subsamples, the relationship is, as expected, upwards sloping. In the post-ridehail subsample, however, the slope of the relationship steepens.[20]

Table 6 employs the natural logarithm of one plus the search share for entrepreneurship-related terms as the outcome measure for entrepreneurial interest. As before, column (1) presents estimates using the full sample, column (2) restricts the sample to post-2005, column (3) restricts the sample to the ever-treated sample of cities, and column (4) imposes both the post-2005 and ever-treated filters. Regardless of specification, we observe a similar pattern of increase in search for entrepreneurial-related terms after the arrival of the gig economy: the estimates suggest an increase in the range of 7% to 13% in the share of searches for entrepreneurship-related terms.

4.4. Reduction in uncertainty

We next proceed to dig deeper into the plausibility of our conceptual framework, by exploring whether the effects we document are in fact larger in areas where ex ante economic uncertainty or entrepreneurship-related uncertainty are greater, as would be expected. In Table 7, we employ our four proxies for general and entrepreneurship-related economic uncertainty to test this relation. We present the estimates from fully interacted models which include the interaction of each of the four measures with the $TREATMENT * POST$ variable. We standardize these measures to have a mean of zero and a standard deviation of one to ease the interpretation of the coefficients. Because these proxies are measured once-per-city for the pre-period rather than at the annual level, the lower order terms for the uncertainty measures themselves are absorbed in the city FE. In Panel A, the outcome measure is new business formation, and in Panel B, it is entrepreneurial interest (search share for entrepreneurship-related terms).

Both Panels present estimates for models with interactions with our four proxies of ex ante economic uncertainty. In column (1), we use our proxy for labor market uncertainty, the variance in wage growth across industries in the city measured over the period of 2000 to 2010. Consistent with the notion that gig work opportunities offered by ridehailing platforms are more valuable in areas where ex ante economic and entrepreneurial uncertainty

[20] The figure estimates are presented in tabular form in the appendix, Table A5.

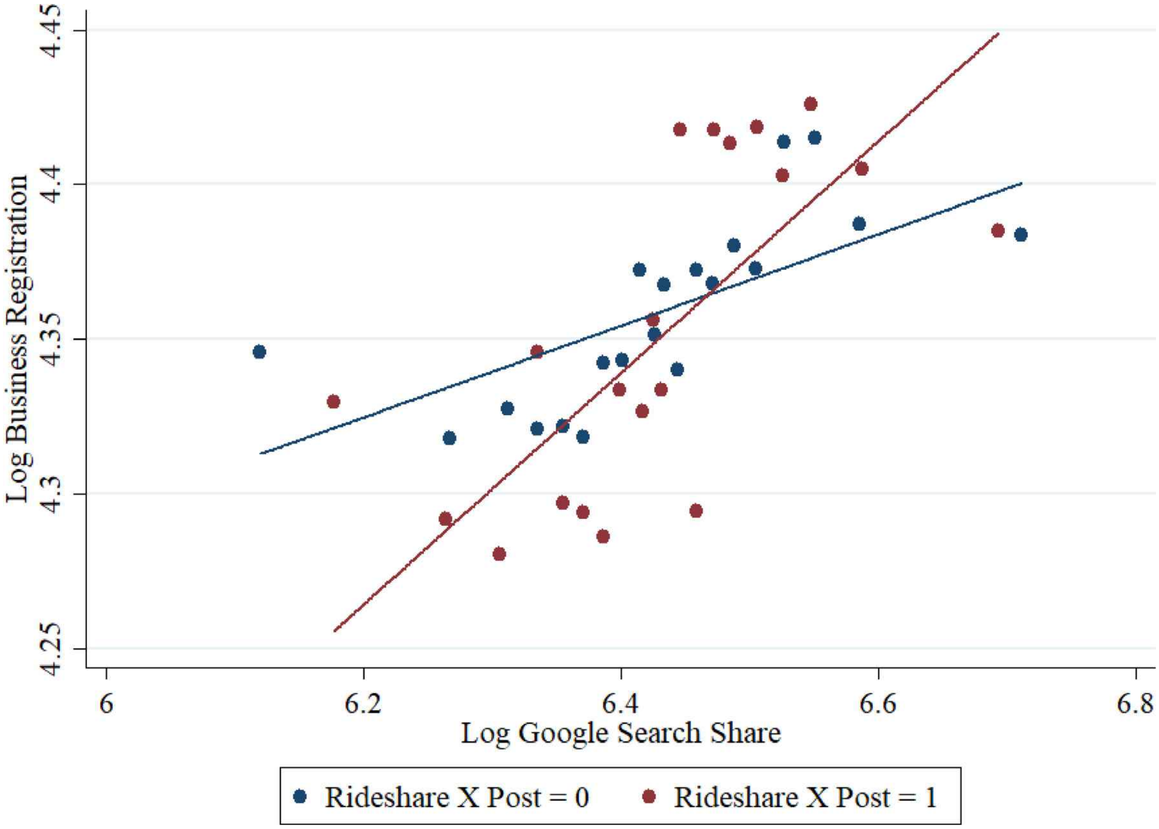

Fig. 3. Relation between Search Activity and New Business Registration.
This figure plots the relation between new business registration and entrepreneurship-related Google search share before and after the entry of ridehailing. Blue points represent pre-ridehailing city-quarter observations. Red points represent post-ridehailing city-quarter observations.

are higher, we observe that the effects are higher in areas with more ex ante labor market uncertainty. For example, a one standard deviation increase in the wage growth volatility in a city is associated with a 2.9 percentage point increase in new business registrations, and a 1.5 percentage point increase in entrepreneurial interest, on top of the main $TREATMENT * POST$ effect.

In columns (2) and (3), we turn to our two proxies for business income uncertainty. Once again, for both the cross-sectional and time-series volatility versions of the measures, we observe that the effects of a shock to the supply of gig work hours in the form of the arrival of ridehailing is higher in areas with higher ex ante business income uncertainty. A one standard deviation increase in business income volatility is associated with a 3 to 4 percentage point increase in new business registrations, and a about 9 percentage point increase in entrepreneurial interest, on top of the main $TREATMENT * POST$ effect.

Finally, in column (4), we use our proxy for the risk of business failure, the business bankruptcy rate in the county in which the city is located in. Once again, consistent with the notion that the availability of gig work opportunities provides valuable fallback options for would-be entrepreneurs that encourage them to venture into new business formation, we find that the effects of the arrival of ridehailing platforms are higher in areas where the business bankruptcy rate is higher ex ante. A one standard deviation increase in the business bankruptcy rate is associated with a 1 percentage point increase in new business registrations, and a 1 percentage point increase in entrepreneurial interest, on top of the main $TREATMENT * POST$ effect. Taken together, the results across all four of our proxies for the various elements of economic uncertainty further bolster our notion of gig-work providing fallback opportunities to would be entrepreneurs.[21]

While the analysis in Table 7 supports an insurance mechanism driving the relation between entry of ridesharing and new business starts, in Appendix Table A7, we conduct additional analysis to decompose the nature of the insurance effect further. While we use various proxies for economic uncertainty, the measures are conceptually slightly different. For example, low business volatility

[21] In the Appendix Table A6 Panel A, we present additional evidence consistent with the notion that the effect of the arrival of the gig economy may be larger in areas where an income supplement or fallback may be most valuable: areas with low personal income pre-gig economy arrival. Consistent with this, we observe that the effects are higher in low-income areas. In Panel B, we show that wage growth is not increasing faster in low-income areas, alleviating the concern that areas with lower income are just growing faster and thus having more new business registrations. We caveat that these estimates may suffer from a reflection problem that we cannot fully address.

Table 7
Mechanisms for Growth in Entrepreneurial Entry.

Panel A: New Business Registration

	Log (1+New Business Registration)			
	(1)	(2)	(3)	(4)
Treat X Post X Wage Growth Volatility	0.0293***			
	(0.0067)			
Treat X Post X Business Income Volatility (Cross-Sectional)		0.0382***		
		(0.0094)		
Treat X Post X Business Income Volatility (Time-Series)			0.0430***	
			(0.0086)	
Treat X Post X Business Bankruptcy Rate				0.0096**
				(0.0047)
Treat X Post	0.0339***	0.0240*	0.0215*	0.0374***
	(0.0115)	(0.0129)	(0.0126)	(0.0116)
Observations	195,446	195,446	195,446	195,446
R-squared	0.9590	0.9590	0.9591	0.9590
Controls	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes

Panel B: Google Search Share

	Log (1+Google Search Share)			
	(1)	(2)	(3)	(4)
Treat X Post X Wage Growth Volatility	0.0148***			
	(0.0056)			
Treat X Post X Business Income Volatility (Cross-Sectional)		0.0926***		
		(0.0126)		
Treat X Post X Business Income Volatility (Time-Series)			0.0902***	
			(0.0122)	
Treat X Post X Business Bankruptcy Rate				0.0118***
				(0.0045)
Treat X Post	0.1111***	0.0806***	0.0819***	0.1116***
	(0.0123)	(0.0140)	(0.0141)	(0.0125)
Observations	153,853	153,333	153,593	153,801
R-squared	0.6140	0.6163	0.6146	0.6140
Controls	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes

Notes: This table presents the heterogeneous effects of ridehailing on new business registration (Panel A) and entrepreneurial interest (Panel B) by several measures of uncertainty. The dependent variable in Panel A, Log *(1+New Business Registration),* is the natural logarithm of one plus the number of new business registrations in a city-quarter. The dependent variable in Panel B, Log *(1+Google Search Share),* is the natural logarithm of one plus google search share for entrepreneurship-related phrases, such as "start a business", "how to incorporate", and "become an entrepreneur". *Wage growth volatility* is the standardized weighted sum of the variances and covariances of wage growth in the sectors of the economy, weighted by the employment share of each individual sector as measured up until 2010. *Business Income Volatility (Cross-Sectional)* is the cross-zip standard deviation of IRS-measured business income in a CBSA in 2010. *Business Income Volatility (Time-Series)* is the cross-zip, cross-year standard deviation of IRS-measured business income in a CBSA from 2005 to 2010. *Business Bankruptcy Rate* is the county-year counts of business bankruptcy cases reported by U.S. Courts divided by the number of business filings reported by IRS, measured in 2013. All interacted variables are standardized to have a mean of 0 and standard deviation of 1. More detailed explanations of the variable constructions can be found in the Data and Sample section of the paper. Control variables in the regressions include the natural logarithm of population, income per capita (lagged one quarter), and unemployment rate (lagged one quarter). Standard errors, clustered at the city level, are reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

areas may not benefit as much from the insurance effect of the gig economy because entrepreneurs there are already insured. Additionally, it could also be the case that the outside option benefits from the gig economy are larger in areas with higher labor income uncertainty. The former is about opportunities within the formed businesses, whereas the latter is about the opportunity cost/fallback of starting a business. To test this potential differential, we include interaction for both measures in Appendix Table A7, and find that both views play a role in generating our inferences.

Table 8
Gig Economy and the Nature of Entrepreneurial Activity.

Panel A: Entrepreneurship Quality

	Entrepreneurship Quality Index			
	(1) >2000	(2) >2005	(3) Treat = 1	(4) >2005 & Treat=1
Treat X Post	0.000004	−0.000015	0.000012	0.000006
	(0.000021)	(0.000020)	(0.000021)	(0.000019)
Log Pop	0.000113	0.000000	0.000025	−0.000084
	(0.000095)	(0.000123)	(0.000090)	(0.000116)
Log Income Per Capita (lag)	0.000135	0.000156	0.000144	0.000193
	(0.000119)	(0.000143)	(0.000172)	(0.000201)
Unemployment Rate (lag)	−0.000000	0.000002	−0.000002	−0.000000
	(0.000004)	(0.000005)	(0.000004)	(0.000006)
Observations	188,117	134,317	113,201	81,043
R-squared	0.2849	0.3272	0.2685	0.2891
City FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes

Panel B: Entrepreneurship Concentration

	Business Registration Zip HHI			
	(1) >2000	(2) >2005	(3) Treat = 1	(4) >2005 & Treat=1
Treat X Post	0.0036**	−0.0009	0.0032*	0.0008
	(0.0018)	(0.0020)	(0.0018)	(0.0019)
Log Pop	−0.0513***	−0.0285	−0.0421**	−0.0413**
	(0.0138)	(0.0191)	(0.0180)	(0.0197)
Log Income Per Capita (lag)	−0.0226**	−0.0077	−0.0025	0.0017
	(0.0110)	(0.0120)	(0.0110)	(0.0120)
Unemployment Rate (lag)	0.0017***	0.0013**	0.0001	0.0001
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Observations	195,446	139,225	114,384	81,761
R-squared	0.8675	0.8805	0.9328	0.9441
City FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes

Notes: This table presents results from generalized difference-in-difference regressions. The dependent variable in Panel A is the average entrepreneurial quality index defined in Guzman and Stern (2019). The dependent variable in Panel B measures the concentration of new business registration in a city-quarter using an HHI index that equals to the sum of the zip shares of business registration in a city squared. *Treat X Post* is a dummy variable that equals one if city c adopted at least one ridehailing service at time t. Control variables in the regressions include the natural logarithm of population, income per capita (lagged one quarter), and unemployment rate (lagged one quarter). Standard errors, adjusted for clustering at the city level, are reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

4.5. Nature of entrepreneurial activity

Next, we explore whether the gig economy has compositional effects on the type of business launched. As noted by Levine and Rubinstein (2017, 2018), Guzman and Stern (2019), and Bothelho, Fehder, and Hochberg (2021), there is considerable heterogeneity in both the goal of entrants into entrepreneurship and in the types of companies they launch. These range from small business entrepreneurs who undertake entrepreneurship for non-pecuniary reasons, such as leisure or flexibility (Hurst and Pugsley, 2011), to entrepreneurs like Mark Zuckerberg or Peter Thiel, who launch innovation-driven startups with the goal of high growth. Guzman and Stern (2015, 2019) combine the comprehensive business registration data used earlier in this paper with predictive analytics to compute entrepreneurial "quality" estimates over time. For the purposes of our analysis, SCP provided us with their Entrepreneurial Quality Index (EQI)—which measures the predicted probability that a new business launched in a location and time period will have a high growth outcome—computed at the county-quarter level. We can then use the EQI measure to assess compositional effects: if EQI increases post-RH arrival, this suggests that the share of innovation-driven startups in a treated location goes up post-gig economy arrival. If EQI decreases, it suggests the share of traditional, small business entrepreneurship has gone up.

In Table 8, Panel A, we estimate our models using EQI as the outcome variable. As can be seen from the models

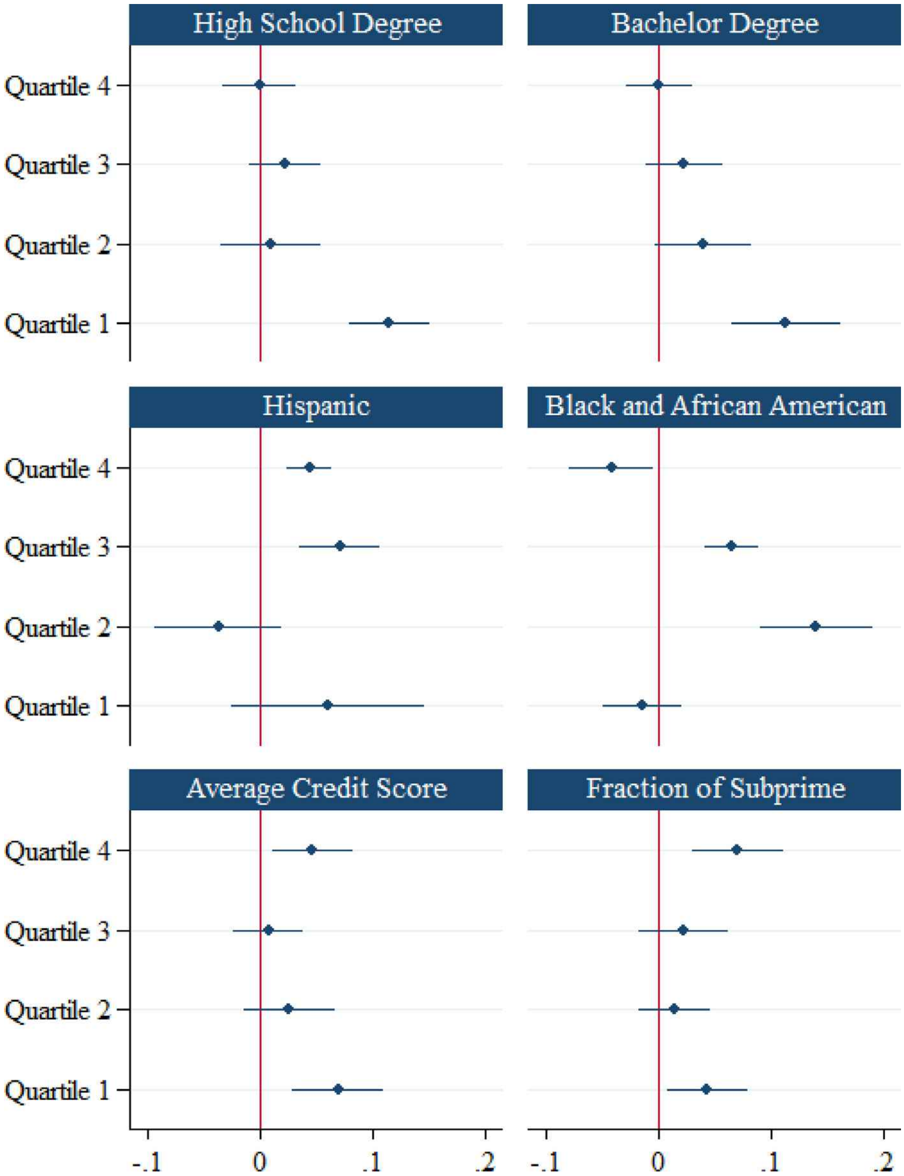

Fig. 4. Heterogeneity by Demographics.
This figure displays the regression coefficient estimates in Table 10 and two-tailed 95% confidence intervals based on standard errors clustered at the city level. We break out the effect of rideshare entry by the fraction of population in a city with high school degrees, the fraction of population in a city with bachelor's degrees, the fraction of Hispanic population in a city, the fraction of black and African American population, average credit score, and the fraction of subprime borrowers, i.e. borrowers with credit scores below 660. The outcome variable for all panels is the natural log of new business registrations.

in the table, we observe no significant change in EQI in the treated cities post-gig economy arrival, suggesting that the gig economy does not significantly alter the composition of the types of entrepreneurs in a city.

In Panel B, we explore another aspect of the new entrepreneurial activity: geographic dispersion across the city. One concern is that our prior estimations are picking up not a general effect of the gig economy, but rather a specific effect of ridehailing, namely the ability of this new transportation mode to open opportunities for businesses in new neighborhoods that previously suffered from a lack of easy transportation access. In Panel B of Table 8, we estimate our DD models using as an outcome measure the Herschman-Herfindahl Index by zip code within the city, as a measure of geographic dispersion of where businesses launch. More specifically, the dependent variable in Panel B measures the concentration of new business registration in a city-quarter, measured using an HHI index that equals to the sum of the zip shares of business registration in a city squared. We observe no significant change in geographic concentration in the treated cities post-gig economy arrival—if anything, we see a slight in-

Table 9
Is the Increase in Entrepreneurship Due to City Growth?

	Log Average Weekly Wage			
	(1) >2000	(2) >2005	(3) Treat = 1	(4) >2005 & Treat=1
Treat X Post	−0.0093***	−0.0045***	−0.0079***	−0.0032***
	(0.0009)	(0.0009)	(0.0011)	(0.0009)
Log Pop	0.0262***	0.0576***	0.0250**	0.0646***
	(0.0095)	(0.0132)	(0.0123)	(0.0157)
Log Income Per Capita (lag)	0.2597***	0.2202***	0.2603***	0.1865***
	(0.0140)	(0.0131)	(0.0144)	(0.0114)
Unemployment Rate (lag)	−0.0033***	−0.0029***	−0.0022***	−0.0021***
	(0.0002)	(0.0002)	(0.0003)	(0.0003)
Observations	198,238	142,017	115,024	82,401
R-squared	0.9818	0.9793	0.9813	0.9785
City FE	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes

Notes: This table presents results from generalized difference-in-difference regressions. The dependent variable is the natural logarithm of average weekly wage in a city in a given quarter. *Treat X Post* is a dummy variable that equals one if city c adopted at least one ridehailing service at time t. Control variables in the regressions include the natural logarithm of population, income per capita (lagged one quarter), and unemployment rate (lagged one quarter). Standard errors, adjusted for clustering at the city level, are reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 10
Who are the new entrepreneurs?

	Education		Race		Credit Score	
	(1) High School	(2) Bachelor	(3) Hispanic	(4) Black and African American	(5) Level	(6) Fraction of Subprime
Treat X Post X Q1	0.1148***	0.1131***	0.0604	−0.0139	0.0690***	0.0435**
	(0.0185)	(0.0246)	(0.0434)	(0.0179)	(0.0206)	(0.0178)
Treat X Post X Q2	0.0100	0.0399*	−0.0371	0.1398***	0.0262	0.0147
	(0.0227)	(0.0216)	(0.0290)	(0.0255)	(0.0204)	(0.0162)
Treat X Post X Q3	0.0218	0.0224	0.0707***	0.0651***	0.0074	0.0223
	(0.0162)	(0.0174)	(0.0185)	(0.0124)	(0.0159)	(0.0203)
Treat X Post X Q4	0.0000	0.0005	0.0441***	−0.0419**	0.0465***	0.0700***
	(0.0165)	(0.0149)	(0.0102)	(0.0193)	(0.0180)	(0.0206)
Observations	195,446	195,446	195,446	195,446	195,379	195,379
R-squared	0.9590	0.9590	0.9590	0.9591	0.9590	0.9590
Controls	Yes	Yes	Yes	Yes	Yes	Yes
City FE	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes
City Linear Trend	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table presents heterogeneous effects of ridehailing on entrepreneurship by city characteristics. The dependent variable, Log (1+New Business Registration), is the natural logarithm of one plus the number of new business registrations in a city-quarter. Column (1) breaks out the effect by the fraction of population in a city with high school degrees, column (2) by the fraction of population in a city with bachelor's degrees, column (3) by the fraction of Hispanic population in a city, column (4) by the fraction of black and African American population, column (5) by average credit score, and column (6) by the fraction of subprime borrowers, i.e. borrowers with credit scores below 660. *Treat X Post* is a dummy variable that equals one if city c adopted at least one ridehailing service at time t. *Q1, Q2, Q3,* and *Q4* are indicator variables that take a value of one if a city's characteristics is in the respective quartile of distributions. Control variables in the regressions include the natural logarithm of population, income per capita (lagged one quarter), and unemployment rate (lagged one quarter). Standard errors, adjusted for clustering at the city level, are reported in parentheses. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively.

crease in concentration—suggesting that the gig economy does not significantly alter the geographic concentration of entrepreneurs in a city.

4.6. Are we just capturing city growth?

One concern is that despite our strict specification, perhaps somehow, we are still capturing different economic growth trends in cities. We can assuage these concerns directly. If our findings were driven solely by improvements in economic conditions that are not captured by the D.D. specification, we would expect to see a similar effect if we replaced our business registrations outcomes with a measure such as local employment wages. Table 9 estimates models using average weekly wage as the outcome measure and similar specifications to the previous tables. Not only do we observe no corresponding increase in average weekly wages, but the coefficients are, in fact, negative, suggesting that our observed increases in entrepreneurial activity are not driven by general growth in economic activity.

4.7. Heterogeneity by demographics

Finally, we explore whether the gig economy differentially bolsters entrepreneurial entry in cities with different underlying demographic and socioeconomic characteristics. Understanding any heterogeneity present in the empirical patterns may provide further insights or avenues for exploration for those interested in boosting entrepreneurial activity amongst specific populations. Here, we focus on education, race, credit constraints and income. In Table 10, we break out our results across a variety of city characteristics. For each characteristic, we assign cities to quartiles based on the measures for each characteristic calculated in 2010. We take these measures primarily from the American Community Survey. We then re-estimate our models, interacting $POST * TREATMENT$ with the four quartile indicators for each city characteristic. The specifications include location and year-quarter fixed effects, a location-specific linear time trend, and control variables. We also present the estimates graphically in Fig. 4.

For education levels, we observe the gig economy effect on entrepreneurial entry is concentrated in cities with a low fraction of population having obtained a high school (column (1)) or bachelor degree (column (2)). This would be consistent with the gig economy insurance effect being more valuable for lower education entrepreneurs. When we look at race, we find the effect is higher in cities with a higher fraction of Hispanic population (column (3)) and in cities in the middle of the distribution for Black and African American population (column (4)). The effect is actually negative and significant coefficient in cities in the top quartile of Black and African American population share. We take no stance on the mechanism for the observed heterogeneity for race; future research may wish to explore these patterns in more detail.

We next turn to socioeconomic characteristics. In columns (5) and (6), we look at credit constraints, measured as the average credit score in the city (column (5)) and the fraction of subprime borrowers (credit score below 660) in the city (column (6)). Our observed effects on entrepreneurial entry are concentrated in the lowest *and* highest quartiles of credit score: the large effect in the lowest quartile of credit score and highest quartile of the fraction of subprime borrowers are consistent with the Knightian view of risk-bearing in entrepreneurship. We observe an equally large in the least constrained areas (Q4 credit score, Q1 subprime fraction) where the demand effect could play a role.

5. Conclusion

Economists since Adam Smith have emphasized the importance of entrepreneurs and new business formation to the economy. Policymakers continuously seek for ways to stimulate entrepreneurial activity in their local regions. In this paper, we shed light on a development in the digital economy that has positive spillover effects on entrepreneurial activity: the advent of on-demand gig economy platforms. Utilizing the shock to gig opportunities provided by the launch of ridehailing platforms across U.S. cities, our findings suggest that the insurance that gig work opportunities can provide against entrepreneurial-related economic uncertainty serves to increase entrepreneurial activity and galvanize would-be entrepreneurs to engage in new business formation.

While much of the literature on the effects of the gig economy focuses on its direct impact on gig workers, our work joins an emerging literature exploring the spillover effects from the advent of large-scale gig platforms (e.g., Koustas, 2018; Fos et al., 2019). Our findings suggest that the gig economy plays a substantial role in spurring entrepreneurial entry by providing a form of insurance against entrepreneurial related-income volatility, potentially reducing the risk of launching a new business. This benefit appears to be particularly strong in cities with worse socioeconomic conditions, where policymakers may be especially interested in encouraging new entrepreneurial activity.

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Supplementary materials

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