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Can You Gig it? An Empirical Examination of the Gig-Economy and Entrepreneurial Activity

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Abstract

We examine how the entry of gig-economy platforms influences local entrepreneurial activity. On one hand, such platforms may reduce entrepreneurial activity by offering stable employment for the un- and under-employed. On the other hand, such platforms may enable entrepreneurial activity by offering work flexibility that allows the entrepreneur to re-deploy resources strategically in order to pursue her nascent venture. To resolve this tension, we exploit a natural experiment, the entry of the ride-sharing platform Uber X and the on-demand delivery platform Postmates into local areas. We examine the effect of each on crowdfunding campaign launches at Kickstarter, the world's largest reward-based crowdfunding platform. Results indicate a negative and significant effect on crowdfunding campaign launches, and thus local entrepreneurial activity, after entry of Uber X or Postmates. Strikingly, the effect appears to accrue primarily to un-funded and under-funded projects, suggesting that gig-economy platforms predominantly reduce lower quality entrepreneurial activity by offering viable employment for the un- and under-employed. We corroborate our findings with US Census data on self-employment, which indicate similar declines following the entry of Uber X, and with a small scale survey of gig-economy participants.

Keywords: *gig economy, digital platforms, crowdfunding, entrepreneurship, difference in difference, natural experiment, necessity-based entrepreneurship*

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* Author's names are in alphabetical order. Each author contributed equally to this work.

Introduction

The introduction of new business models spawned by digital platforms has captured the attention of scholars and policy makers for decades (Bakos and Bailey 1997, Parker and Van Alstyne 2005). While classic examples, such as eBay and Amazon.com (Brynjolfsson et al. 2003, Dellarocas and Wood 2008, Forman et al. 2008), continue to generate billions in revenue annually, new models of platform-enabled, peer-to-peer businesses have recently come to the fore (e.g. AirBNB, Postmates, Uber, TaskRabbit). Collectively referred to as the collaborative-, sharing-, or gig-economy; businesses of this type are anticipated to comprise a substantial portion of the economy in the coming years, with serious economic implications (Sundararajan 2014); including the disruption of long-standing industries (Morse 2015) and the displacement of incumbents (Zervas et al. 2015). Indeed, these platforms are already estimated to comprise a roughly $26 billion market (Malhotra and Van Alstyne 2014) and numerous studies have explored patterns of demand for services provided by gig-economy platforms (Edelman and Luca 2014, Greenwood and Wattal 2016, Zervas et al. 2015). In this work, we extend this literature by offering the first consideration of how the entry of gig-economy platforms, formally defined as digital on-demand platforms which enable a flexible work arrangement, influence local entrepreneurial activity[1].

With industry disruption comes the expectation of economic growth, innovation, and entrepreneurship (Gans et al. 2002, McAfee and Brynjolfsson 2008). Yet, little evidence of changes in entrepreneurial activity deriving from the gig-economy have emerged to date.[2] While the absence of any apparent changes may be due in part to the fact that traditional economic measures of employment and productivity are often coarse and subject to lags, making it difficult to capture changes stemming from relatively recent developments (Sundararajan 2014),[3] it may also be due, in part, to countervailing effects

[1] When referring to “entrepreneurial activity” we exclude employment on the gig-economy platform itself, for a number of reasons. As an example, recent class action lawsuits filed against Uber (e.g. http://uberlawsuit.com/, http://www.uberlitigation.com/) indicate that many service providers view themselves as employees of the firm, rather than independent contractors. Moreover, as reported in our discussion section, we completed a small-scale survey of service-providers, the overwhelming majority of whom reported that they did not view themselves as entrepreneurs.

[2] http://www.wsj.com/articles/proof-of-a-gig-economy-revolution-is-hard-to-find-1437932539

[3] http://www.citylab.com/work/2013/10/rise-invisible-work/7412/

in the relationship between the gig-economy and entrepreneurial activity.

To this point, both the popular press and the scholarly community have provided competing arguments of the effect gig-economy platforms will have on entrepreneurial activity. On the one hand, the introduction of flexible *ad hoc* employment may lead to greater entrepreneurial activity because it affords the nascent entrepreneur the ability to strategically optimize her time in order to garner the necessary resources to initiate a project or start a firm (Agrawal et al. 2015, Douglas and Shepherd 2000). Indeed, both scholarly work and the popular press have repeatedly noted that gig-economy businesses provide workers with an unprecedented degree of flexibility, allowing them to set their own schedules while earning stable pay (Hall and Krueger 2015).[4,5] On the other hand, researchers have noted that un- and under-employment are themselves significant drivers of entrepreneurial activity. This is because people who do not believe they have acceptable employment options may choose to engage in entrepreneurial activity as a result of their low opportunity costs (Acs and Armington 2006, Armington and Acs 2002, Fairlie 2002, Storey 1991). If this is the case, the arrival of the gig-economy may slow entrepreneurial activity by providing alternate employment opportunities for these entrepreneurs (Block and Koellinger 2009).

These two logics offer competing predictions of the effect gig-economy platform entry will have on rates of local entrepreneurial activity, a tension we aim to resolve. More formally, we ask the following question: *What is the effect of gig-economy platform introduction on the rate and characteristics of entrepreneurial activity in a given locale?* To resolve this tension, we exploit a natural experiment, the entry of the ride-sharing service Uber X and the entry of the on-demand delivery service Postmates into local markets. We study the relationship between the entry of these services and the volume of local crowdfunding campaigns launched on Kickstarter, the world's largest crowdfunding platform (Burtch et al. 2013, 2015, Rhue 2015), over a 21-month period between 2013 and 2015.

[4] http://www.nationaljournal.com/next-economy/big-questions/how-airbnb-uber-are-changing-nature-work
[5] http://venturebeat.com/2014/08/17/inside-the-sharing-economy-workers-find-flexibility-and-19-hour-days/

This econometric strategy offers us two notable benefits. First, because the rollout of Uber and Postmates is staggered temporally and geographically, i.e. the services enter different locations at different times, we are able to exploit a difference in difference design that allows us to account for time-invariant, location-specific heterogeneity, as well as broader macroeconomic trends that might otherwise influence our results. Second, by focusing on Kickstarter campaign launches we are able to capture early stage entrepreneurial activity that should respond more quickly to the introduction of gig-economy platforms, as compared to more traditional measures like firm founding and patenting (Bessen and Hunt 2007, Sundararajan 2014). Moreover, focusing on Kickstarter campaigns has the added benefit of enabling an operationalization of entrepreneur quality (e.g., proxied by fundraising performance). Our analysis, therefore, depends on key assumptions, the most notable being i) that the entry and timing of gig-economy platforms are exogenous with respect to Kickstarter campaign launch volumes, conditional on our controls, and ii) that Kickstarter campaign volume is a valid indicator of entrepreneurial activity.

To establish the robustness of our results we leverage a wide variety of falsification tests, including a consideration of alternative empirical specifications, estimators, and measures, the application of matching techniques, and replication using a series of different independent and dependent variables. For example, we evaluate the robustness of Kickstarter campaigns as a measure of entrepreneurial activity by replicating our analysis on reports of self-employment from the US Census Bureau's Current Population Survey (CPS). Collectively, findings indicate that the entry of each service, i.e., Uber X or Postmates, into a geographic region results in a significant decrease in the volume of campaign launches on Kickstarter, and thus a decline in entrepreneurial activity. The effect is driven primarily by a reduction in unsuccessful campaigns, suggesting that some individuals choose to work in the gig-economy rather than pursue entrepreneurial projects of relatively low quality. The identified effects are also pronounced. Results indicate that Uber X's entry into a location resulted, on average, in a 14% decline in the volume of campaigns launched on Kickstarter one year later. Economically, this translates to a decrease of more than $7.5 million in fundraising requests across the United States over the 21-month period of our study.[6]

This paper makes four notable contributions. First, our results speak to an important debate about how gig-economy platforms influence local entrepreneurial activity. There are compelling theoretical reasons to believe that gig-economy platforms might either increase or decrease activity. We provide initial evidence that gig-economy jobs may, on average, substitute for lower quality entrepreneurial activity rather than act as a complement to higher quality entrepreneurial activity.

Second, we consider a novel measure of entrepreneurial activity: the rate and volume of crowdfunding campaign launches (e.g. Mollick, 2014). Unlike traditional measures of entrepreneurship and innovation (e.g., patenting), crowdfunding and crowdsourced activity provides a more transparent, short-term bellwether of the rate and scale of entrepreneurship in a given geography. Indeed, recent work has already begun to recognize the importance of harnessing the crowd when discussing innovation (Bockstedt et al. 2015). By demonstrating that our results are robust to the use of a more traditional measure of entrepreneurship (i.e. CPS self-employment), we add to a growing literature that makes a compelling case for the use of Kickstarter as a measure of entrepreneurial activity (Mollick and Kuppuswamy 2014).

In this same vein, our work has implications for the sustainability of crowdfunding platforms. Our findings suggest that the advent of the gig-economy is particularly important for platforms like Kickstarter, where sustainability and growth is tied to the crowd's efficient and successful identification of high quality projects. Because campaigns on crowdfunding platforms must compete for attention and capital, the presence of low quality campaigns is likely to increase search costs for potential campaign backers and redirect funds that might have been better allocated elsewhere. By facilitating a reduction in lower quality projects, the gig-economy can enable campaign backers to focus their attention on the high

[6] This calculation is based on an observed median campaign request amount of $5,200, 1,440 EA-quarter observations in which *Uber X* was active, and a conservative estimate of *Uber X*'s average marginal effect (-0.010) on the logged count of campaign launches. This calculation was performed as follows: exp(0.010) campaigns per period * 1440 periods * $5,200 = $7,563,255.65.

quality, high potential campaigns, resulting in more efficient capital allocation (Shane 2009).

Third, there are clear implications for policy. Our results suggest that gig-economy jobs may be particularly attractive to un- and under-employed individuals, who might otherwise have pursued low quality entrepreneurial projects. While the introduction of these disruptive platforms may speed the demise of incumbents (Seamans and Zhu 2013), possibly eliminating some jobs in the process, our results suggest that individuals with weak attachments to the labor market may in fact benefit from this creative destruction. This detail may be important for policy makers to consider as they tackle the regulation and legality of these platforms.

Finally, our work contributes to the burgeoning literature examining societal level impacts of information systems and digital platforms (Bapna et al. 2012, Chan and Ghose 2014, Chan et al. 2015, Parker et al. 2016, Rhue 2015, Seamans and Zhu 2013). The increasing digitization of daily life may bring both negative (Chan and Ghose 2014, Chan et al. 2015) and positive (Greenwood and Wattal 2016) externalities. As such, it is incumbent upon researchers to explore the consequences that IT artifacts have on society and the economy, as well as the heterogeneous impacts on different sections of the population, to better inform their management and related policy. Our work highlights one potentially positive effect: gig-economy platforms may provide job opportunities for individuals who otherwise would engage in lower quality, perhaps less promising, entrepreneurial activity.

Related Work

Academic research on the gig-economy has proceeded along a number of fronts, ranging from platform design and user response (Fradkin 2013, Fradkin et al. 2014), to the effect on labor movements (Friedman 2014, Milkman and Ott 2014), to broader economic and societal effects (Edelman and Luca 2014, Greenwood and Wattal 2016, Zervas et al. 2015). The latter effects, in particular, have received considerable attention. Zervas et al. (2015), for example, examine the impact of AirBNB’s entry on the Texas hotel industry, finding strong evidence of cannibalization, particularly among lower-tier hotels. Greenwood and Wattal (2016) study the effect of Uber entry on DUI fatalities and arrive at a similar conclusion; they observe that gig-economy services are most likely to affect price-sensitive consumers.

However, less work has considered the supply side of these markets, in particular, who the suppliers of these services are likely to be. Understanding the nature of supplier characteristics can help researchers and policymakers to better understand how these markets operate, thus it behooves the scholarly community to begin to explore these questions. Three notable exceptions to the predominant focus on the demand side exist: Edelman and Luca (2014), who examine how racial bias affects AirBNB hosts, Rhue (2015), who examines racial bias in crowdfunding, and Morse (2015), who discusses the impacts that P2P lending markets are having on consumer lending.

In this work, we consider how gig-economy platforms may influence entrepreneurial activity. Although research, thus far, has focused on the economic benefits that the gig-economy may produce, in terms of flexible employment and micro-entrepreneurship (Sundararajan 2014), it is still unclear why individuals choose to supply labor to these platforms. Relatedly, it is also unclear what activities these individuals might have engaged in had the platform not been available. Determining the answers to these questions is critical to understanding the macro-level tradeoffs that accompany the operation of gig-economy platforms.

Slack Resources, Experimentation, and Entrepreneurship

Why might the entrance of gig-economy platforms increase the amount of local entrepreneurial activity? The prior literature offers two mechanisms. First, scholars have argued that entrepreneurship depends on the availability of slack resources, i.e. resources which can be directly re-assigned to entrepreneurial endeavors (Agrawal et al. 2015, Richtnér et al. 2014). Because gig-economy platforms purportedly offer service providers a combination of scheduling flexibility and stable income, their entry may enable the would-be entrepreneur to strategically re-allocate a constrained sets of resources in order to push a nascent idea forward (Agrawal et al. 2015). Second, and related to the first point, the nascent entrepreneur, unburdened by earlier resource constraints, may be encouraged to explore or exploit new opportunities as they emerge (Shah and Tripsas 2007, Voss et al. 2008). In principle, gig-economy employment may therefore enable experimental sampling of potential opportunities because sufficient resources are available to afford it (Greve 2007, Kerr et al. 2014, Shah and Tripsas 2007).

Significant evidence of both behaviors can be found in the empirical literature and the popular press. Agrawal et al. (2015), for example, find that when prestigious universities release their students for breaks, i.e. winter or summer vacations, there is a dramatic uptick in the amount of entrepreneurial activity around those universities; a notion that is corroborated by Facebook and Microsoft both being founded when Harvard was in winter session (Graham 2012). Similarly, Voss et al. (2008) find, in the context of professional theatre companies, that slack resources tend to be redeployed to explore nascent opportunities. On the practitioner side, anecdotal evidence underscores the success of Google's, Hewlett-Packard's, and 3M's "free time policies," which have led to a number of top products.

In the context of the gig-economy, this logic is compelling, and suggests that the flexibility of gig-economy employment would allow the nascent entrepreneur to optimize her resources in such a way that she could redeploy time and other assets to a budding venture (Douglas and Shepherd 2000). Put another way, because gig-economy firms like Uber or Postmates allow the entrepreneur to set her own hours (Hall and Krueger 2015), they offer a significant advantage over traditional employers with less flexible schedules (Swarns 2014). If this is the case, the entrepreneur may be able to exploit this flexibility for her own gain, and devote resources to the venture without losing financial security.

Opportunity Costs and Entrepreneurship

Although the literature on slack resources would suggest that the entrance of gig-economy platforms might facilitate entrepreneurial entry, other work provides the opposite prediction. While many would-be entrepreneurs might depart traditional employment to pursue higher wages (Braguinsky et al. 2012), or increased flexibility (DeMartino and Barbato 2003, Sørensen 2007), past work also suggests that individuals may pursue entrepreneurship as a means of resolving un-employment or under-employment (Block and Koellinger 2009, Fairlie 2002, Storey 1991).

That is, received research has argued that un- and under-employed individuals pursue entrepreneurial activity because they have significantly lower opportunity costs than someone who is fully employed (Block and Koellinger 2009, Fairlie 2002, Storey 1991). As a result, they may pursue entrepreneurial activity because they have excess time (due to partial employment) or because such activity has a higher expected value than their wage employment opportunities (Acs and Armington 2006, Armington and Acs 2002). The entrance of employment opportunities via the gig-economy may change the internal calculus for these would-be entrepreneurs. As the gig-economy may offer significant employment opportunities (Uber employed roughly 150,000 drivers in the United States as of 2015 (Hall and Krueger 2015); and 7.9% of US workers worked in contingent employment as of 2010 (GAO 2015)), it is plausible that entrepreneurial activity may fall as a result of the gig-economy, because entrepreneurs who are un- or under-employed select into this new, viable alternative. Put another way, if individuals with lower opportunity costs pursue employment in the gig-economy, rather than pursuing entrepreneurial activity, then it is plausible that total local entrepreneurial activity may *fall* as the gig-economy grows.

The presence of persuasive theoretical arguments on both sides of this debate reveals a compelling tension. On the one hand, entrepreneurial activity may rise because individuals can use the slack resources that are freed up via employment in the gig-economy to pursue new projects. On the other, if the gig-economy predominantly provides economic opportunities to the un- or under-employed, entrepreneurial activity may decline as those individuals redeploy their efforts away from entrepreneurial activity, towards gig-economy employment. Therefore, in the absence of a compelling *a priori* expectation, we look to our empirical analyses to determine the predominant effect.

Methods

Data & Descriptive Statistics

We draw on data from multiple sources to execute our estimations. To construct our control variables, we obtain dynamic socioeconomic data, across locations, from the Area Resource File[7]. To construct our dependent variable, we create a measure of the volume of crowdfunding campaigns launched on Kickstarter over time, in each location. The campaign data are collected via Kickstarter's API[8] through daily queries of active projects from September of 2013 through March of 2015. By paging through the active campaign list, we capture every campaign that was live at any given point in time. In turn, we

[7] http://ahrf.hrsa.gov/

[8] Kickscraper: http://syntaxi.net/2013/03/24/let-s-explore-kickstarter-s-api/

identify the amount of money sought, and ultimately raised, by each campaign.

We use Kickstarter campaign launches to measure entrepreneurial activity in our primary estimations for several reasons. First, Kickstarter activity is a responsive measure of entrepreneurial activity, unlike other measures, such as VC financing or patent production, which are subject to significant delays. VC financing, for example, can take years if not decades to obtain (Gompers and Lerner 1999). Kickstarter activity, in contrast, has the potential to shift relatively quickly in response to changes in the local marketplace. Further, the timing of Kickstarter activity can be measured precisely, because one can observe the exact day when a campaign is posted.

Second, Kickstarter has very low barriers to entry. Whereas Kickstarter used to employ a manual vetting process before allowing campaigns to be posted on the site, the process was automated in 2014, making it relatively easy to launch a campaign. This stands in stark contrast to the market for VC, where less than 1% of new firms receive capital (Robb and Robinson 2012), and in even further contrast to patent possession (which a firm might never pursue). Because VC funding is allocated to only the highest quality ventures, any measure of entrepreneurial activity constructed on the basis of VC funding would be subject to a significant selection bias, and thus measurement error. Kickstarter, on the other hand, reflects attempts at fundraising initiated both high and low quality ideas. Finally, because we can observe the fundraising outcomes for all projects, we are able to assess the apparent quality of entrepreneurs and their ideas, as judged by the market (i.e. the crowd). Other measures of entrepreneurship, such as reports of self-employment on the US Census Bureau's CPS (which we use as a robustness test below) do not provide clear markers of the quality of the entrepreneurial idea[9].

We operationalize the entry of the gig-economy into a local area based on the staggered entry of Uber X and Postmates. Uber, founded in 2009, is a smartphone application that allows consumers to

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[9] While some US Census Bureau products (i.e. the Annual Social and Economic Supplement) allow for the measurement of dollars earned in self-employment, which might be a reasonable proxy for quality, these measures are only available on a yearly basis. Given the novelty of gig-economy platforms, yearly data does not provide a sufficient estimation panel. Data on the whether or not a worker is self-employed, without reference to dollars earned, is available on a monthly basis (see below).

submit a trip request, which is then routed to Uber drivers who use their own cars to fulfill the request. Uber X is Uber's low-cost option. Postmates, founded in 2011, defines itself as an "urban logistics and on-demand delivery platform," which connects customers with local couriers.[10] Couriers purchase and deliver goods to customers, on demand, from any retail locations in a city. Data on Uber entry and Postmates entry are retrieved directly from the Uber Blog and from the Postmates website, respectively.

We focus on Uber X and Postmates for several reasons. First, both Uber and Postmates are rolled out on a city-by-city basis, thus the expansion of each platform is geographically and temporally dispersed. In contrast, many other platforms, such as AirBNB, were rolled out without geographic restriction, a fact that prevents the identification of a plausible control group; without which it would be difficult to reliably estimate the effect of platform entry. Second, Uber and Postmates are two of the most highly valued companies in the gig economy. Uber, in particular, is the largest and most heavily utilized platform, with a valuation of nearly $70B dollars (as of early 2016). Large platforms are desirable for our analyses because they are most likely to have an effect on labor dynamics in local markets.[11] Moreover, Postmates provides a useful point of comparison for Uber, because our theory suggests that the influence of gig economy platforms should vary with the cost of participation faced by service providers. As an on-demand courier service, workers for Postmates need only own a bicycle. In contrast, providing services in the Uber marketplace, even for Uber X, requires that individuals own a relatively new automobile, in good condition. We rely on these differences to help identify the mechanisms that drive our results. Third, Uber and Postmates, unlike many of the other gig-economy platforms that have engaged in staggered rollouts, are particularly systematic about publishing the dates and locations of rollouts, which has the benefit of clear measurement. Finally, and perhaps most importantly, these platforms purposefully enable a flexible work arrangement, thereby meeting our formal definition of the gig-economy. This stands in contrast to platforms like Etsy, which would more appropriately be classified as online retailers.

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[10] https://postmates.com/about

[11] An explicit examination of other ridesharing services (e.g. Lyft, Sidecar, Uber Black) is reported in Table 8.

Econometric Specification

The primary econometric specification we employ is a multi-site entry difference-in-difference (DD) relative time model (Angrist and Pischke 2008). This model allows us to conduct a quasi-experiment using secondary data because the treatments, i.e. the entry of *Uber* and *Postmates*, are applied in different locations at different times, in a plausibly exogenous manner. The longitudinal nature of the data allows us to examine the existence of pre-treatment trends in Kickstarter campaign activity. This approach further enables us to include location and time fixed effects, which effectively control for static heterogeneity across locations, as well as any unobserved temporal trends or shocks (e.g., seasonality).

Our unit of analysis is the Economic Area (EA) – month. The Bureau of Economic Analysis divides the United States into 172 unique economic areas based on shared economic activity (in particular, commuting patterns).[12] Our main dependent variable is the number of Kickstarter campaigns launched in EA *i*, during month *t*. In our initial analyses, the independent variable of interest is the treatment indicator, *Uber X*, which indicates that the ride-sharing service *Uber X* has entered EA *i* as of time *t*. Consistent with prior work examining the effect of Uber's entry on a locale (Greenwood and Wattal 2016), we focus on Uber X, rather than the premium service Uber Black, due to the significantly lower startup costs and larger network of drivers.[13] Uber Black requires a driver to use a "black car" or limousine, while an Uber X driver can utilize a broader range of lower cost, personal vehicles. Our second independent variable, *Postmates*, also a treatment indicator, captures whether Postmates has entered EA i as of time t. Both of these variables are coded as one during the first full month of implementation. A full list of Uber and Postmates entries by location is available in Table 1. To complete the difference in difference specification, we include vectors of EA and time fixed effects. In total, our sample includes data on 75,115 campaigns, launched across 172 EAs, over 21 months. Table 2 provides the descriptive statistics for our sample. Figure 1 is a histogram of Uber X entries[14].

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[12] http://www.rita.dot.gov/bts/sites/rita.dot.gov.bts/files/publications/federal_register/1995/html/bts_19950310.html

[13] Replication of our estimations using Uber Black yields no significant correlation. These results are explored further below.

[14] A formal test of the skewness on the entry of Uber X into local areas is insignificant (p=0.155), suggesting that the

We employ a relative time model, as opposed to a traditional difference in difference estimation, because it allows us to evaluate the parallel trends assumption and observe any dynamics in the effect of gig economy entry. As discussed by Angrist and Pischke (2008), the chief assumption of the DD estimation is that of parallel pre-treatment trends. If trends in the dependent variable exhibit significant differences, prior to treatment, this implies that the untreated group cannot serve as a valid control, i.e. a reflection of what would have happened to the treatment group in the absence of treatment.

The relative time specification of the difference in difference model now sees relatively common use in the IS literature (Autor 2003, Bapna et al. 2015, Chan and Ghose 2014, Greenwood and Wattal 2016). This estimation incorporates a second set of time dummies that indicate the chronological distance, *j*, of an observation period, *t*, with respect to the timing of treatment in that location, *i*. Our primary model specification is expressed in linear form, for the sake of simplicity, in Equation (1). Here, y_{it} is the number of campaigns launched in EA *i* at time *t*, α_i represents the vector of EA fixed effects, ϕ is the vector of relative time dummies, τ is a vector of absolute time dummies, and *X* is a series of dynamic location-specific controls.

(1) $$y_{it} = \alpha_i + \tau_t + \sum_j \beta_j(\varphi_{it}) + X_{it} + \varepsilon_{it}$$

The relative time dummies (φ) initially reflect only the timing of Uber's entry into an EA. This is because of the greater number of such treatments that appear in our sample (72), as compared with *Postmates* (31). Time fixed effects (τ) are modeled in two ways: first, as year-quarter effects, and second, as a combination of a year fixed effects and quarter fixed effects[15]. As noted previously, it is unlikely that Uber randomly selects markets for entry. While an evaluation of the parallel trends assumption helps us to assess the validity of our identification strategy, it remains possible that Uber selects into locations based on some dynamic factors. To account for this possibility, we also incorporate a number of location-

implementations are sufficiently distributed over time. A QQ plot of the normality of entry times corroborates this test. We thank the anonymous reviewer for suggesting these tests.

[15] Results using month-year fixed effects, as well as those using a yearly fixed effect with monthly seasonality dummies are consistent for both Uber X and Postmates. These estimations are available from the authors upon request.

specific dynamic control variables (*X*), which capture factors that might make a market attractive to Uber while also influencing the rate of local entrepreneurial activity (i.e., factors which, if omitted, might lead to the identification of spurious effects). These controls include the log of the number of employed people working in the EA, the average weekly wage within the EA, and total quarterly wages within the EA.

Our primary estimator is a fixed effects Poisson Pseudo-Maximum Likelihood (or Quasi-Maximum Likelihood) estimator (PPML or PQML) (Azoulay et al. 2010, Burtch et al. 2014, Greenwood and Gopal 2015, Simcoe 2007), although we note that the results are robust to a logged-OLS and fixed effect negative binomial (NB) specification. Further, although a NB estimator is typically prescribed in cases of over-dispersed count data, as discussed by Allison and Waterman (2002), no true fixed effect estimator has yet been proposed in the NB case, whereas a conditional fixed effect Poisson estimator is available. Moreover, the PPML estimator enables us to obtain consistent, robust standard errors (clustered by EA), even under conditions of over-dispersion (Wooldridge 1997). The PPML estimator has also been shown to significantly outperform a log-OLS specification when data contains many zeroes (Silva and Tenreyro 2011), as in our sample, and to provide more reliable estimates. This is because a log-OLS specification in count data can produce severely biased estimates (O'Hara and Kotze 2010), particularly under conditions of heteroscedasticity (Silva and Tenreyro 2006).

Results

Results in Table 3 reveal several interesting findings. When considering the base model in Columns 1 and 2, we witness some evidence of a pre-treatment difference (Rel Time $_{(t\text{-}6)}$). However, as these are the only significant time periods, with no evidence of significant differences in the remainder of the pre-treatment periods, the data appear to support the parallel trends assumption. Further, we see that the negative effect of *Uber X* entry on the number of Kickstarter campaigns becomes stable and significant roughly one year after implementation. Specifically, estimates in Model 1 suggest that the number of active Kickstarter projects declines about 14% in treated EAs in the fourth quarter after Uber X's entry. Results in Column 3 and 4, which utilize different combinations of control variables, corroborate these estimates. The fact that the estimates do not change much with the inclusion of different controls for time and employment suggests a robust effect. The effect becomes stable and significant one year after *Uber X* enters a market (consistent with previous estimates of the effect of Uber (Greenwood and Wattal 2016, Mulholland and Dills 2016)). Graphical representations of the estimates are depicted in Figures 2 and 3.[16] Broadly speaking, these estimates provide strong and significant evidence that the entry of gig-economy platforms reduces the number of Kickstarter campaigns being launched in local areas.[17]

Additional Analyses and Robustness Tests

We next consider an extensive set of robustness checks, which are summarized in Table 4. In these tests we continue to use the relative time specification, for the sake of consistency, and to allow for a continued evaluation of the parallel trends assumption (Angrist and Pischke 2008, Autor 2003). We begin by applying a Coarsened Exact Match (CEM), in an effort to rule out model dependence as a driver of our results, and also to ensure the precision of our estimates (Iacus et al. 2012). We then address the possibility that our results are spurious, deriving from serial correlation in the dependent variable (Bertrand et al. 2002), via a random implementation test. Third, we explore the robustness of our results to the inclusion of entry by competing ride-sharing services, such as Lyft and Curb, as well as the effect of Uber Black. Fourth, we consider additional controls in our relative time specification, namely location-specific time trends. Finally, we examine whether the results are robust to an alternative measure of entrepreneurial activity, namely reports of self-employment as recorded by the US Census Bureau's CPS. Following these analyses, which are intended to establish the robustness of the primary results, we conduct additional tests (such as a consideration of the entry of Postmates and the quality of the Kickstarter campaigns) which allow us to explore the mechanisms driving the main findings.

Coarsened Exact Match

An issue with the prior estimations is that they rely on parametric control variables to create comparability between the treated EAs and untreated EAs. To help address this issue, we pre-process our

[16] Although relative time dummies before t-6 and after t+5 are not displayed they are estimated. Replication of the estimations collapsing these variables into a single dummy are consistent. We thank the anonymous reviewer for this suggestion.
[17] It should be noted that all subsequent analyses are robust to the inclusion of the employment controls in Table 3.

data, applying a CEM (Blackwell et al. 2009, Iacus et al. 2009, 2012), which discards EAs that are not comparable to other EAs on the set of chosen covariates. Models 1 and 2 of Table 5 apply this match on a monthly basis using three covariates. We match on population to create parity in the relative sizes of the local markets, and by extension the probability of Uber entering the market (see Table 1). We match on the average weekly wage, as our theory directly implicates un- or under-employment as the primary mechanism by which the would be entrepreneur will forego her Kickstarter campaign to drive for the service. Finally, we match on time period, to ensure homogenous macroeconomic conditions. It should further be noted that the inclusion of additional matching criterion, such as those used in Table 3 (e.g. total quarterly wages) result in substantially fewer matches, rendering the estimation underpowered. However, our results remain robust to the inclusion of these controls.

Model 3 takes a slightly different approach, matching treated EAs to untreated EAs (i.e. EAs that never receive Uber X) in the month prior to Uber X's entry and enforcing that match for the entire estimation panel. If an untreated EA is matched to multiple treated EAs, we retain the earliest match. In this model, we match on population density and the number of Kickstarter campaign launches leading up to the observation period[18], each measured at the EA level. Results in Table 5, indicate support for our previous findings. Again, we see that the entry of gig-economy platforms significantly reduces the number of Kickstarter campaign launches 9-12 months after entry.

Random Implementation (Shuffle) Tests

Next, we consider the potential for false significance in our estimates due to spurious relationships or serial correlation in our dependent variable. As discussed by Bertrand et al. (2002), "[m]ost Difference-in-Difference (DD) papers rely on many years of data and focus on serially correlated outcomes. Yet almost all these papers ignore the bias in the estimated standard errors that serial correlation introduces." Because a large portion of the market entries observed in our data are concentrated in early 2014 (Figure 1), it is possible that something idiosyncratic about these cities is driving the observed effect (such as a particular

[18] We thank the anonymous reviewer for suggesting these matching criteria.

reaction to the 2014 economic recovery from the Great Recession). Although our initial estimates (Table 3) cluster standard errors by EA, and thus account for these dependencies to some degree, it is useful to implement several of the tests suggested by the Bertrand et al. (2002)[19], to ensure robustness.

In the first analysis, a random implementation test, we reassign our treatment indicators at random in our data. That is, we shuffle the 1,440 indicators of Uber X's presence in our original sample to a randomly selected set of (new) observations, thereby creating a pseudo (placebo) treatment. We then estimate a standard difference-in-difference model (i.e., an Uber presence dummy) with time and location fixed effects. We store the coefficient of this pseudo treatment and replicate the procedure 1,000 times. This test helps to assess the spuriousness of any significant results obtained in the main analyses arising from autocorrelation in the dependent variable (Bertrand et al. 2002).

Results are presented in Table 6 and indicate three key findings. First, the estimated average β associated with the randomly assigned treatment indicator is not significantly different from zero, suggesting that the main results are reliably estimated. Second, the estimated β is quite small and not driven by serial correlation in the standard errors. Third, the finding is not significantly driven by outliers. Moreover, in unreported tests, we replicate our estimates excluding major cities (e.g. New York, San Francisco, Boston), to help rule out the possibility that our results are driven by an intensive response amongst a few large markets.[20] Although there is a substantial concentration of Kickstarter campaigns in locations like Los Angeles, New York, and San Francisco, our results from these estimations suggest that, while the size of the effect does decrease when major metropolitan areas are excluded, they remain qualitatively similar (insofar as the results remain negative and significant one year after implementation).

The second major test, also a random implementation model, addresses concerns related to the concentration of city rollouts in early 2014. Following Greenwood and Agarwal (2016), we implement

[19] We opt for this approach over the alternative suggestion by Betrand et al (2002) to collapse panels into pairs of pre and post observations, because our treatments are staggered in time.
[20] We thank the anonymous reviewer for this suggestion

this test by swapping the implementation vectors across cities that receive the Uber treatment (i.e. Philadelphia receives the implementation vector for New York, which receives the vector of Tampa, which receives the vector of Miami, and so on). We then replicate our estimation procedure from Equation (1) 500 times and store the coefficients. This test allows us to examine whether there are structural differences between cities that receive Uber and those that do not, which could have yielded a drop in Kickstarter campaigns even if Uber had not arrived. Results are in Table 7 and suggest that the average treatment effect is indistinguishable from zero once the vectors are swapped. Taken in sum, these tests suggest that neither outliers, serial correlation, or unobserved macro-economic trends around the bulk of Uber's expansions are driving the results.

Other Ride-Sharing Services

Thus far, our results have focused on the effect of Uber X. However, other ridesharing services are in operation during the sample period, which provides an opportunity to extend our analyses and ensure that the omission of these other services in our primary estimation is biasing our estimates. It is plausible, for example, that other *discount* ridesharing services may evoke the same theoretical tension as Uber X for the would-be entrepreneur. If this were the case, the would be entrepreneur might begin driving for Lyft, as opposed to Uber X. Moreover, *premium* ridesharing services, i.e. Uber Black, are also in operation, providing an opportunity to evaluate our proposed theoretical mechanism, i.e., necessity based entrepreneurship. The costs of participating as a driver for Uber Black (which requires a black car limousine) are significantly greater than those of Uber X. As a result, we should observe a significantly smaller, if any, effect from Uber Black, relative to Uber X, if our proposed mechanism is correct.

To address the effect of other discount ridesharing services we first gathered the implementation schedules of Uber X's main competitors: Lyft, Sidecar, Flywheel, and Curb. Uber appears to have been the first to market in all but 15 economic activity areas. In 13 of the 15 EAs where Uber X was not the first to arrive, it was at most 2 months behind the other services in its arrival. In the remaining 2 EAs, it arrived 8 months later (Los Angeles, where Lyft's expansion was much of the impetus for Uber's nationwide rollout of Uber X) and 19 months later (Lincoln, NE, where Uber has recently launched).

We take two empirical approaches to assess the effects of these other discount ridesharing services. First, following Greenwood and Wattal (2016), we replicate our estimations after omitting any observation where a competitor ride sharing service is operating alongside Uber X. This approach is preferable to creating additional vectors of relative time dummies, because the similarity in entry times across services creates significant multicollinearity issues. This strategy allows us to measure the effect of Uber X exclusively. Results are in Columns 1 and 2 of Table 8.

Second, because the other discount services have the potential to trigger the same decision-making scenario for a would-be entrepreneur of necessity, we consider a replication in which we adjust our definition of the relative time dummies, such that they reflect the first implementation of any ridesharing service in a location (whether Uber X, or a competitor, e.g., Lyft, Curb). Results of this second analysis are presented in Columns 3 and 4 of Table 8. Finally, to explore any possible influence of *premium* ridesharing services, we gather data on the implementation schedule of Uber Black (again from the Uber blog). We then replicate the estimation of Equation (1) using a set of relative time dummies that reflect the rollout of Uber Black. These results are presented in Columns 5 and 6 of Table 8.

The estimates add interesting nuance to our previous findings. First, our subsample analyses excluding observations in which competitors were active (Columns 1 and 2), indicate that the negative and significant relationship persists between Uber X and Kickstarter campaigns launches in Rel $\text{Time}_{(t+4)}$. Note that, once we drop observations where Uber X operated in tandem with a competitor, relative time dummies for t+5 and later are no longer identified. This is because a competitor enters no more than four quarters after Uber in every location. Second, we see in Columns 3 and 4, under our adjusted definition of treatment (i.e., the entry of *any* discount ride-sharing service), that the broader negative relationship between service entry and the number of launched campaigns persists, though the magnitude of the effect is marginally reduced and takes an additional period to manifest. Finally, we see in Columns 5 and 6 that there is no significant effect of Uber Black entry on the volume of Kickstarter campaign launches. Given that Uber Black does not present an easily accessible employment alternative for an average necessity-based entrepreneur (given Uber Black's reliance on professional limo drivers), this null result is consistent with our proposed mechanism, namely a reduction in necessity based entrepreneurship.

EA Specific Time Trends

Although findings suggest that neither the temporal sequencing of Uber's rollout pattern nor the locations where Uber has elected to enter are endogenous with respect to our outcome variable, it is possible that idiosyncratic time trends exist across economic areas which our previous analyses have failed to account for. To resolve this concern we replicate our estimations substituting EA specific linear time trends in lieu of EA fixed effects. The benefit of this model is that, although it sacrifices power and a varying base level of entrepreneurship (because the intercept is assumed to be common), it allows for varying trajectories in entrepreneurial activity across EAs. The results are presented in Table 9, where we see broad consistency with our main set of estimates.

Alternative Measure of Entrepreneurial Activity

Our next concern is that Kickstarter is a relatively novel measure of entrepreneurial activity. It might be argued, therefore, that our results are attributable to shifts in patterns of crowdfunding use, rather than shifts in entrepreneurship more generally. For example, it is possible that gig economy employment simply allows entrepreneurs to obtain funds via an avenue different than Kickstarter, meaning that Kickstarter campaigns might decrease without an overall decline in entrepreneurial activity. To rule out this alternative explanation, we next replicate our analyses using a more traditional measure of entrepreneurial activity, reported instances of self-employment on the US Census Bureau's CPS, obtained from the Integrated Public Use Microdata Series Current Population Survey (IPUMS-CPS) dataset (Ruggles et al.), the US's largest publically available census of individual level microdata.[21] These data have been used extensively in prior empirical research (Fairlie 2013, Lazear 2004). As such, if we are able to replicate our findings on this alternative measure, it would alleviate concerns that Kickstarter is not representative of entrepreneurship.

[21] https://usa.ipums.org/usa/

CPS data derive from monthly surveys that allow us to track, among other information, the self-reported occupation of individuals in a particular geographic area, including whether an individual is "self-employed." [22] Further, the survey data incorporates weights that allow us to adjust the estimates to obtain results reflective of the overall US population. We leverage the same econometric specification expressed by Equation 1, replacing the count of Kickstarter campaigns with the count of individuals reporting self-employment. Additionally, we draw on other measures from the IPUMS-CPS to control for the number of employed individuals (whether self-employed or in wage work) in the EA, the unemployment rate, and the percentage of bachelor degree holders. While the IPUMS-CPS does not contain wage information, the latter two variables should account for local labor market conditions in the way wage information did in our prior analyses. Above and beyond providing an additional measure of entrepreneurial activity, these data allow us to increase the robustness of our results by expanding our panel to cover a larger time period than the Kickstarter analysis (the sample includes 2010 through 2015).

Results are in Table 10. Note that this analysis includes 144 EAs (as compared to 172 in the main analyses), because 26 EAs do not contain a Core Based Statistical Area that is large enough to meet the non-disclosure confidentiality requirements of the IPUMS-CPS. Our first two models exclude individuals who list their primary occupation as a professional driver or chauffer, though results are consistent if these individuals are included (Columns 3 and 4).[23] Column 1 includes EA, year, and seasonal fixed effects, as well as EA-level controls. Column 2 incorporates quarter fixed effects. In each model, we observe limited evidence of a pretreatment trend, aside from an isolated uptick in self-employment five quarters before Uber X enters. In the quarter of Uber X's entry, self-employment declines about 2%, and this effect increases to about 9% by quarter six (which is consistent with the results from Kickstarter). Results across the remaining models yield consistent estimates, which we depict visually in Figure 4.[24]

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[22] https://cps.ipums.org/cps-action/variables/CLASSWKR#description_section

[23] It should be noted that the CPS's "Contingent Worker Survey Supplement," aimed (in part) at better measurement of gig-economy work, is not included in these results as the program does not launch until May 2017.

[24] Results when collapsing the relative time dummies prior to t-6 and after t+6 into single covariates yield consistent results and

Empirical Extensions

We next consider analyses that (in tandem with our Uber Black results above) help to better identify the mechanism underlying the observed effect, i.e. necessity based entrepreneurship, which should result in a shift away from lower quality entrepreneurial projects and towards participation in the gig-economy.

Postmates

Our earlier results suggest that Uber X generates a clear, negative and statistically significant, effect on the number of Kickstarter campaign launches. However, startup costs for Uber X, while significantly lower than the premium car service Uber Black, are still non-trivial (vehicles having specific age, body style, and cosmetic requirements). We therefore expand our analysis to consider the introduction another platform, Postmates, an on-demand courier and delivery service. If the driving force behind the effect is a reduction in entrepreneurial activity attributable to individuals who face lower opportunity costs, we would anticipate that larger effects should be realized from platforms that bear lower fixed costs of entry. Postmates requires only that a delivery person have a bicycle, while Uber X requires that a driver have a car in relatively good condition. Moreover, as the platform may be acting as a substitute means for acquiring startup capital, i.e. the entrepreneur is raising money via the gig-economy instead of Kickstarter, this analysis helps to examine this potential alternate explanation, since working on the Postmates platform is relatively less lucrative than Uber (Younkin and Kashkooli 2013, 2016).

We replicate our relative time model, this time focusing on Postmates entry. Results are in Table 11 and contain several notable findings. First, consistent with previous estimations we see a negative and significant effect emerge post platform entry. Second, we see that the effect of Postmates manifests more quickly (3 months after implementation), and that the coefficients are significantly larger, as compared to Uber X (this is confirmed at Rel Time $_{(t+5)}$ by a set of pairwise Wald tests, with values of 2.35 and 2.42 respectively ($p < 0.05$)).[25] As signing on as a courier with Postmates requires relatively little in the way

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are available upon request. We thank the anonymous reviewer for this suggestion.

[25] The inclusion of both vectors of relative time dummies (Uber X and Postmates), in a single specification, yields estimates consistent with the above. That is, neither vector exhibits a notable pre-treatment trend, the negative effect of Postmates entry manifests more quickly, and the effect of Postmates at t+5 is significant stronger than that of Uber X.

of startup costs when compared to driving for Uber X, the fact that Postmates' entry exhibits a stronger effect on the volume of entrepreneurial activity suggest individuals with lower opportunity costs are shifting their efforts away from entrepreneurship and towards the platform.

Further, similar to our previous analysis using data from the CPS, if gig economy employment and Kickstarter are merely substituting for each other as a means for acquiring capital, we would expect that the lower paying platform (Postmates[26]) would have the smaller effect. But this is not the case. In other words, to the extent that a gig-economy worker might be substituting part-time work for the crowdfunding campaign (Hall and Krueger 2015) in order to acquire the capital necessary to launch a *de novo* venture, these results provide an important check against this explanation.[27]

Failed vs. Successful Campaigns

Our next concern relates to heterogeneity in the effects of gig-economy entry, based on entrepreneur quality. To execute these tests, we proxy quality using Kickstarter fundraising outcomes, thereby allowing the market, i.e. the crowd, delineate between high and low quality ideas. If the gig-economy is reducing entrepreneurial activity among necessity-based entrepreneurs, who are generally of lower quality, we would anticipate the lion's share of the effect to accrue among crowdfunding campaigns with lesser fundraising success. To execute this test, we split Kickstarter campaigns into four buckets based on the percent of target raised by the entrepreneur. We then replicate our estimations on each subsample. The first bucket, Unfunded Campaigns, represents campaigns launched which received zero funding. The second, Partially Funded Campaigns, represents campaigns that received some funding but did not reach their funding goal. The third, Funded Campaigns, represents campaigns that met their funding goal and the fourth, Hyperfunded Campaigns, represents campaigns that achieved at least double their funding goal. We anticipate stronger effects toward the bottom of the distribution, among campaigns of lower

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[26] Hall and Krueger (2015) indicate Uber X drivers average salaries range between $17/hr and $29/hr while Postmates couriers make a median of $19/hr during peak hours - http://techcrunch.com/2015/05/04/hitting-2-million-deliveries-postmates-ceo-bastian-lehmann-says-profitability-is-possible-in-2016/

[27] Unreported analysis also suggests that the marginal campaign size is decreasing post platform entry, further corroborating the fact that Kickstarter and the gig-economy platforms are not substitutes (because it is given that smaller campaigns that are selecting off the crowdfunding platform).

quality which ultimately underperform in terms of fundraising. Results of this analysis are in Table 12.

Consistent with the idea that gig-economy work shifts effort away from lower quality entrepreneurial projects, the largest share of the effect accrues to campaigns which received no funding (Columns 1 and 2). Further, we note in Columns 3-6 that there is a more moderate decrease in both partially funded campaigns and funded campaigns, though the effect is intermittent. Finally, in Columns 7 and 8, we observe no statistically significant change in the volume of hyperfunded campaigns (those that top 200% of their funding goal). Taken in sum, these results support the idea that gig-economy platforms reduce entrepreneurial activity by allowing individuals to pursue gig-economy employment rather than lower quality entrepreneurial projects (indicative of necessity-based entrepreneurship).

Total Dollars Pledged

One further alternative explanation for the observed effects is that these platforms are choosing to enter downtrodden economic areas where they know that they will be able to attract labor supply. To the extent that capital is often provided by local individuals on crowdfunding platforms (Agrawal et al. 2010), a local economic downturn may reduce capital available to Kickstarter entrepreneurs while also attracting gig-economy platform entry.

However, contrary to the results of Agrawal et al. (2010), it is perhaps useful to note that some recent work has demonstrated that a significant portion of funding on Kickstarter is in fact being supplied by non-local parties (Younkin and Kashkooli 2016). For example, Madsen and McMullin (2015) observe that the average Kickstarter project attracts backers from 22.4 distinct US cities and 6.7 states (2014). Moreover, our controls for employment and wages help account for this alternative explanation by incorporating labor patterns in the EA. Nonetheless, we replicate our relative time estimations using the total dollars pledged as our dependent variable, as opposed to the number of campaigns launched. If total dollars pledged remain stable after platform entry, it suggests that our results are driven by a shift in the campaigns that are launched following platform entry, rather than the amount of capital that is available from the market. Results in Table 13 indicate no significant pre- or post-treatment trend in total dollars pledged on Kickstarter, suggesting that gig-economy platforms reduce the number of projects on the platforms, but are not significantly correlated with the total capital supplied by funders.

Changes in Self-Reported Profession

In our final empirical extension, we examine whether the entry of Uber X has a measurable effect on local employment in the taxi and chauffeuring industry. Our argument is that individuals are working for Uber instead of engaging in lower quality entrepreneurial activity. However, we lack the individual-level data that would allow us to show this directly. It is therefore helpful to examine whether Uber's entry shifts local employment towards its industry, i.e. driving. To do this, we once again draw upon the IPUMS-CPS dataset, performing an analysis that is identical to that for self-employment. Specifically, at the EA-month level, we regress the number of individuals who report their primary occupation as being a paid driver or chauffer on the entry of Uber, in a manner similar to Table 10. We expect a positive and significant relationship between Uber X entry and individuals' self-reporting as a paid driver.

Results in Table 14 reveal the expected correlation. Notably, we observe an absence of pre-treatment trends, indicating that, conditional on fixed effects and controls, Uber X does not appear to enter into cities where the taxi and limousine industries are already growing. We see that within one year the number of individuals reporting their primary occupation as paid driver increases by about 32%, which suggests that Uber X has a significant impact on driving industry employment, as we would expect. It is useful to compare this effect size to that which we observe in the self-employment results, where we see that Uber X is associated with a 5% drop in self-employment one year after the introduction of the platform. While we cannot directly compare these elasticities, the fact that the increase in driver employment is qualitatively larger in magnitude than the decrease in self-employment gives us some confidence that our effect sizes are reasonable. Uber X will undoubtedly draw workers away from activities other than self-employment, so if we observed a stronger effect on self-employment than on driver employment, we might worry that an unobserved third factor (other than Uber X) might be driving the downward trend in self-employment. Note that the number of observations in this analysis is smaller than the self-employment analysis due to a few EAs reporting no individuals listing driver occupations during the time period.

Discussion

An interesting tension arises when one considers how the rise of the gig-economy might influence entrepreneurial activity. On the one hand, gig-economy employment might provide individuals with flexibility and resources that enable them to engage in the creation of a new venture, thus increasing total entrepreneurial activity. On the other, the presence of gig-economy platforms may provide a desirable employment alternative for would-be necessity-based entrepreneurs, thus reducing total entrepreneurial activity. Exploiting a multi-treatment difference-in-difference specification around the entry of gig economy platforms into various locations over time, we find consistent evidence of a negative effect of gig-economy platform entry (e.g. Uber X and Postmates) on entrepreneurial activity, as measured by the volume of crowdfunding campaign launches and the volume of individuals reporting self-employment on the US Census Bureau's Current Population Survey. Further, we observe that the effect of entry is stronger when the entering platform bears lower fixed cost of participation (e.g., Uber Black requires a black car, Uber requires a standard vehicle, Postmates requires a bicycle), and the effect manifests primarily as a reduction in low quality crowdfunding campaigns. The findings suggest that gig economy platforms provide necessity-based entrepreneurs with a preferable, alternative source of employment.

At least four notable contributions stem from this work. First, our results provide a glimpse into the supply-side of gig-economy markets, suggesting that individuals who take up jobs on these platforms may be directing their efforts away from underperforming entrepreneurial activities. While researchers to date have considered the effect of platform entry on the demand side of markets (e.g. on competitors (Seamans and Zhu 2013, Zervas et al. 2015) or consumers (Edelman and Luca 2014, Greenwood and Wattal 2016)), the supply side of the market remains notably understudied.

Second, as alluded to previously, we offer a novel measure for entrepreneurial activity: the rate and volume of crowdfunding campaign launches. This measure offers several benefits over other measures of entrepreneurship and innovation. For example, both failed and successful campaigns are immediately visible, offering researchers insights both into which entrepreneurs were successful and which were unsuccessful, enabling study of the antecedents and consequences of either. This is a sharp departure from traditional datasets like VentureXpert or Y Combinator (Aggarwal et al. 2012, Greenwood and Gopal 2015, Sorenson and Stuart 2001), where the ability to capture both funded and unfunded entrepreneurs is often lacking. Further, this measure is quick to respond to changes in markets, and offers researchers immediate visibility into both campaign and funding dynamics in the marketplace (unlike more slowly moving metrics like patenting (Sundararajan 2014)). Finally, it is important to note that our results are robust to the use of more traditional measures of entrepreneurship, namely reports of self-employment on the US Census Bureau's Current Population Survey; a consistency that speaks to the validity of Kickstarter's campaign volumes as a proxy for entrepreneurial activity in a region.

A third contribution applies to the crowdfunding literature. The differential effect on more versus less successful campaigns suggests positive spillover benefits for crowdfunding marketplaces. The entry of gig-economy platforms appears to help separate wheat from chaff, reducing 'noise' in the crowdfunding marketplace. Any decline in low-quality campaigns might be expected to reduce the cognitive burden and search costs imposed on crowd-financers, mitigating adverse selection, which, in turn, should help to ensure the sustainability of the crowdfunding marketplace. With the elimination of many low-quality campaigns, the crowd, facing a budget constraint, can shift its focus and wealth to higher quality campaigns and, in theory, achieve greater returns. In contrast, in the absence of the treatment, capital is more likely to be tied up in campaigns with poor prospects and a lower likelihood of achieving their fundraising targets, cannibalizing potential contributions from more deserving campaigns.

Finally, this work offers notable insights for policy makers who are currently debating the legality of services like Uber and Postmates, as well as for the growing literature on the broader societal impacts of information systems (Bapna et al. 2012, Chan and Ghose 2014, Chan et al. 2015, Parker et al. 2016, Rhue 2015). Although our results do not capture potential reductions in employment that may occur among incumbent firms in the industries that are disrupted by the entry of a gig-economy platform (e.g. taxis, established courier services, hotels (Zervas et al. 2015), or newspapers (Seamans and Zhu 2013)), our results provide evidence that these platforms may provide employment opportunities for un- or under-employed individuals in other sectors. Policy makers may find it valuable to consider this potential effect when deciding whether to accommodate these platforms. However, it is important to note that the long-term implications and dynamics of the effects we observe remain unknown. As noted above, some of the entrepreneurship that is obviated by the entry of a gig-economy platform might have otherwise blossomed into viable businesses that would have provided jobs in the local community, and perhaps led to useful innovations. More work is therefore needed to better understand the full implications of shifting labor away from lower quality entrepreneurship and towards gig-economy employment.

This work is subject to a number of limitations, which offer rich opportunities for future work. First, an important assumption in our arguments is that individuals who would have attempted fundraising campaigns on Kickstarter are instead choosing to work for Uber and Postmates. Limitations of our archival data prevent us from testing this assumption directly. We have attempted to alleviate this issue by supplementing our archival analyses with a survey of Uber and Lyft drivers, wherein we asked drivers to indicate i) whether they viewed themselves as entrepreneurs, ii) whether they had ever considered starting a small business, iii) whether they currently operate a small business, in parallel to their work as a driver, and iv) how they would have gone about raising money for a small business. Unfortunately, we were only able to obtain 20 responses, an admittedly small number. Nonetheless, those responses largely aligned with the findings of our archival analyses.[28]

First, just 2 respondents (10%) reported viewing themselves as entrepreneurs. This result is consistent with our claim that there are significant differences between more traditional forms of entrepreneurship (e.g., starting a small business) and employment in the gig-economy. Second, 15 (75%) respondents indicated that they had considered the launch of a small business in the past, while 8 (40%) indicated that they currently operate a small business, in addition to their work as a driver. These two

[28] Unfortunately, gaining access to the Uber and Lyft driver populations proved exceedingly difficult. One of the authors posted a series of survey solicitations to the most prominent Uber discussion forums, including the uberdrivers subreddit, uberforum.com and uberpeople.net. However, in all three cases, the posts were quickly removed and the author was expelled / banned from the forum.

results suggest that a substantial portion of current drivers have previously considered creating a business, yet only a fraction ultimately followed through. This means that a large proportion of potential entrepreneurs opted instead to only drive for a ridesharing service. Finally, 3 respondents (15%) indicated that in funding a small business, they would look to use crowdfunding, and 1 respondent (5%) even reported that they had previously organized a crowdfunding campaign. While we hesitate to extrapolate from this small-set of responses, the proportions do align well with our archival results.

Second, although our results indicate that the volume of failed projects decreases more than that of successful projects, we cannot make substantive comment about the quality or features of the campaigns that might otherwise have been initiated. Thus we cannot draw conclusions about the overall public welfare changes that result from their absence. As noted above, it is possible that some campaigns which may have been started in the absence of Uber X would have been extremely successful, resulting in large positive economic spillovers. Relatedly, because gig-economy platforms are a rather recent development, we are unable to examine the longer term consequences on entrepreneurial activity from their entry.

Third, while we find that, on average, gig-economy platforms tend to reduce entrepreneurial activity, it is possible that in some situations these platforms do in fact supply some would-be entrepreneurs with the slack resources they require to pursue their passion (the 8 respondents in our survey who indicated operating a small business in parallel to driving for a ridesharing service are a testament to this). Future work might therefore explore the boundary conditions of our findings, where perhaps entrepreneurship is positively stimulated, e.g., identifying specific geographies or business domains where firm founding is most critically dependent upon the availability of slack resources (such as scheduling flexibility). Individual-level data which allows researchers to track who engages in platform employment and who engages in entrepreneurial activity, simultaneously, would be valuable in this effort.

Finally, it remains possible that the scheduling flexibility afforded by employment in these marketplaces may enable individuals to pursue other, non-entrepreneurial, activities that are also subject to scheduling constraints or require other forms of slack resources, e.g., pursuing an education, being a caregiver, or job seeking. This observation suggests that there may be rich opportunities for future work exploring the gig-economy's effects on various non-entrepreneurial activities, to better understand why individuals supply labor to these platforms. More generally, it is our hope that this work presents a first step into examining how the supply side of these platforms functions, and that future work can build on this analysis to develop a more holistic understanding of participation in the gig-economy.

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Table 1: EAs Experiencing Uber X and Postmates Entry

EA	Uber X	Postmates	EA	Uber X	Postmates
Boston, MA	6/1/2013	7/2/2014	St. Louis, MO	10/9/2014	9/17/2015
New York, NY	8/1/2013	5/30/2013	Kansas City, MO	5/1/2014	9/17/2015
Philadelphia, PA	10/15/2015	8/7/2014	Des Moines, IA	9/1/2014	
Washington, DC	8/1/2013	12/10/2013	Madison, WI	3/1/2014	
Richmond, VA	8/1/2014		Minneapolis, MN	9/1/2013	5/14/2015
Greensboro, NC	8/1/2014		Omaha, NE	5/1/2014	
Raleigh-Durham, NC	6/1/2014	9/17/2015	North Platte, NE	8/1/2014	
Norfolk, VA	7/1/2014	8/3/2015	Wichita, KS	8/1/2014	
Charlotte, NC	9/1/2013	5/14/2015	Tulsa, OK	3/1/2014	
Charleston, SC	6/1/2014		Oklahoma City, OK	9/1/2013	9/17/2015
Jacksonville, FL	5/1/2014		Dallas-Fort Worth, TX	11/1/2013	2/3/2015
Orlando, FL	6/1/2014		Austin, TX	8/1/2014	1/5/2014
Miami, FL	6/1/2014	9/17/2015	Houston, TX	2/1/2014	2/3/2015
Tampa, FL	4/1/2014		Corpus Christi, TX	6/1/2014	
Atlanta, GA	10/1/2013	5/1/2015	San Antonio, TX	3/1/2014	4/15/2015
Knoxville, TN	8/1/2014		Lubbock, TX	6/1/2014	
Lexington, KY	6/1/2014		Denver, CO	10/1/2013	9/30/2014
Cincinnati, OH	3/1/2014		Spokane, WA	5/1/2014	
Dayton OH	8/1/2014		Boise, ID	10/1/2014	
Columbus, OH	2/1/2014	9/17/2015	Salt Lake City. UT	5/1/2014	
Pittsburgh, PA	2/1/2014	9/17/2015	Las Vegas, NV		9/30/2014
Cleveland OH	4/1/2014		Flagstaff, AZ	9/1/2014	
Toledo, OH	6/1/2014		Albuquerque, NM	5/28/2014	
Detroit, MI	10/1/2013		El Paso, TX	6/1/2014	
Grand Rapids, MI	7/1/2014		Phoenix, AZ	8/1/2013	3/12/2015
Milwaukee, WI	3/1/2014	9/17/2015	Tucson, AZ	2/1/2014	
Chicago, IL	4/1/2013	3/18/2014	Los Angeles, CA	9/1/2013	5/15/2014
Fort Wayne, IN	8/1/2014		San Diego, CA	5/1/2013	9/12/2014
Indianapolis, IN	9/1/2013		Fresno, CA	2/1/2014	
Louisville, KY	4/1/2014		San Francisco, CA	7/1/2012	12/15/2011
Nashville, TN	12/1/2013	8/3/2015	Sacramento, CA	11/1/2013	8/13/2015
Memphis, TN	4/1/2014		Eugene, OR	7/1/2014	
Birmingham, AL	8/1/2014		Portland-Salem, OR	7/1/2014	3/12/2015
Montgomery, AL	8/28/2014		Seattle, WA	4/1/2013	3/6/2013
New Orleans, LA	4/16/2015		Anchorage, AK	9/16/2014	
Baton Rouge, LA	7/1/2014		Honolulu, HI	6/1/2014	

EA Names are shortened to the largest city in the EA in the interest of space

Table 2: Summary Statistics
N - 3612 EA-months

	Variable	Mean	Std. Dev.	1	2	3	4	5	6	7
1	Campaigns	6.575695	28.1841							
2	Uber X	0.0990917	0.2987953	0.4527						
3	Postmates	0.0261492	0.1595843	0.5627	0.3593					
4	Employment	643894.7	1089218	0.5732	0.27	0.346				
5	Average Wage	687.3171	92.0522	0.3202	0.2535	0.2578	0.4116			
6	Quarterly Wage	7620.00	1.50E+10	0.5934	0.2902	0.4029	0.9897	0.4465		
7	Population	3516222	2.36E+07	0.2032	0.0086	0.0281	0.1341	0.1525	0.1296	
8	Dollars Pledged	57026.06	332493.2	0.6555	0.223	0.4188	0.532	0.2722	0.5519	0.1277

Table 3: Relative Time Model of the Effect of Uber X Entry on Kickstarter Campaign Launches

	(1)	(2)	(3)	(4)
DV	Campaigns	Campaigns	Campaigns	Campaigns
Rel Time $_{(t-6)}$	0.244***	0.221***	0.238***	0.219***
	(0.0581)	(0.0550)	(0.0615)	(0.0568)
Rel Time $_{(t-5)}$	-0.0122	-0.0409	-0.0233	-0.0455
	(0.0709)	(0.0643)	(0.0685)	(0.0644)
Rel Time $_{(t-4)}$	-0.0447	0.00392	-0.0543	-0.000623
	(0.0520)	(0.0579)	(0.0529)	(0.0592)
Rel Time $_{(t-3)}$	0.0477	0.0147	0.0421	0.0128
	(0.0551)	(0.0519)	(0.0567)	(0.0528)
Rel Time $_{(t-2)}$	0.0522	0.0173	0.0498	0.0167
	(0.0376)	(0.0414)	(0.0396)	(0.0422)
Rel Time $_{(t-1)}$		Omitted		
Rel Time $_{(t0)}$	0.0394	-0.00524	0.0311	-0.00832
	(0.0302)	(0.0263)	(0.0308)	(0.0283)
Rel Time $_{(t+1)}$	0.0352	-0.00369	0.0249	-0.00759
	(0.0342)	(0.0355)	(0.0333)	(0.0360)
Rel Time $_{(t+2)}$	0.00376	-0.0358	-0.00505	-0.0389
	(0.0432)	(0.0489)	(0.0446)	(0.0501)
Rel Time $_{(t+3)}$	-0.0257	-0.0818	-0.0350	-0.0849
	(0.0571)	(0.0673)	(0.0592)	(0.0686)
Rel Time $_{(t+4)}$	-0.143**	-0.168***	-0.162**	-0.175**
	(0.0581)	(0.0648)	(0.0654)	(0.0707)
Rel Time $_{(t+5)}$	-0.120*	-0.150**	-0.150**	-0.162**
	(0.0642)	(0.0645)	(0.0732)	(0.0714)
Employment Controls	No	No	Yes	Yes
EA Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	Yes	No
Seasonal Effects	Yes	No	Yes	No
Quarter Effects	No	Yes	No	Yes
N	3,612	3,612	3,612	3,612
Number of Groups	172	172	172	172

Dependent Variable Campaigns indicates the DV is the number of Kickstarter Campaigns launched in *i t*. Year fixed effects indicates a time fixed effect applied at the yearly level. Seasonal fixed effect indicates the application of an additional seasonal fixed effect at the quarter level. Quarter fixed effect indicates a fixed effect for the three month period of time the observation resides in. Rel Time t-x indicates the number of quarters prior to, or after, the implementation of Uber X is from the focal time period. Coefficients for relative time periods prior to t-5 are estimated but not included in the interest of space. Employment controls indicates the log of the number of employed people, average weekly wage, and total quarterly wages within the EA. Estimator is a PPML. Standard errors, in parentheses, are robust and clustered by EA – *** p<0.01, ** p<0.05, * p<0.1

Table 4: Summary of Robustness Tests

Concern	Test	Finding	Location
Serial correlation in standard errors	Random Implementation (Shuffle) Test	Placebo treatments yield no correlation with campaign volume	Table 6
Cities receiving treatment experiencing different economic trajectory	Replicate estimations with EA specific time trends	Results remain consistent	Table 9
	Shuffle Test swapping across treated cities	No significant correlation with campaign volume	Table 7
Outliers driving the results (large cities)	Replicate estimations without New York, Boston, and San Francisco	Results remain consistent	Available upon request
Lack of comparability between treated and control groups	Coarsened Exact Match	Results remain consistent	Table 5
Kickstarter not representative of entrepreneurship	Replicate estimations using CPS-IPUMS data	Results remain consistent	Table 10, Figure 4
Alternate ridesharing services driving effect	Replicate estimations omitting other discount ridesharing services	Results remain consistent	Table 8
	Replicate estimations including other discount ridesharing services	Results remain consistent	
Reduction in campaigns not a function of necessity entrepreneurship	Replicate with lower startup cost platform (Postmates)	Results increase in size	Table 11
	Replicate with higher startup cost platform (Uber Black)	No significant correlation with campaign volume	Table 8, Models 5 and 6
Inability to track individual drivers and entrepreneurs	Small scale survey of current Uber drivers	Non-trivial portions of drivers indicate that they have considered launching a venture	See discussion section
	Replicate estimates with drivers and chauffeurs in local area as DV	Number of local people claiming to drive professionally increases	Table 14
Incorrect functional form of time controls	Replicate with month and month-year time controls	Results remain consistent	Available upon request
	Collapse Rel Time controls before t-6 and after t+6 into single estimates	Results consistent using Kickstarter and Self Employment DVs	Available upon request
Functional form of model is not Poisson	Replicate using logged OLS	Results remain consistent	Available upon request

Table 5: Coarsened Exact Match Relative Time Model of the Effect of Uber X Entry on Kickstarter Campaigns

	(1)	(2)	(3)
DV	Campaigns	Campaigns	Campaigns
Rel Time $_{(t-6)}$	0.199***	0.231***	0.278***
	(0.0752)	(0.0839)	(0.0517)
Rel Time $_{(t-5)}$	-0.0503	-0.0386	-0.186
	(0.0961)	(0.106)	(0.160)
Rel Time $_{(t-4)}$	-0.0877	-0.00782	-0.0495
	(0.0730)	(0.0870)	(0.0620)
Rel Time $_{(t-3)}$	0.110	0.0442	-0.0103
	(0.0753)	(0.0775)	(0.0418)
Rel Time $_{(t-2)}$	0.0118	-0.00894	0.0319
	(0.0392)	(0.0465)	(0.0344)
Rel Time $_{(t-1)}$		Omitted	
Rel Time $_{(t0)}$	-0.000814	-0.0374	0.0125
	(0.0298)	(0.0294)	(0.0369)
Rel Time $_{(t+1)}$	-0.000433	-0.0482	0.0373
	(0.0397)	(0.0469)	(0.0362)
Rel Time $_{(t+2)}$	-0.0113	-0.0651	0.0334
	(0.0406)	(0.0505)	(0.0414)
Rel Time $_{(t+3)}$	-0.0481	-0.122*	-0.0131
	(0.0509)	(0.0670)	(0.0479)
Rel Time $_{(t+4)}$	-0.129**	-0.192***	-0.127*
	(0.0547)	(0.0632)	(0.0739)
Rel Time $_{(t+5)}$	-0.139**	-0.205***	-0.102*
	(0.0608)	(0.0704)	(0.0591)
EA Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	No	No
Seasonal Effects	Yes	No	No
Quarter Effects	No	Yes	Yes
N	2,895	2,895	3,045
Number of Groups	170	170	145

Dependent Variable Campaigns indicates the DV is the number of Kickstarter Campaigns launched in *i t*. Year fixed effects indicates a time fixed effect applied at the yearly level. Seasonal fixed effect indicates the application of an additional seasonal fixed effect at the quarter level. Quarter fixed effect indicates a fixed effect for the three month period of time the observation resides in. Rel Time t-x indicates the number of quarters prior to, or after, the implementation of Uber X is from the focal time period. Coefficients for relative time periods prior to t-6 and after t+5 are estimated but not included in the interest of space. Estimator is a PPML. In Models 1 and 2, Coarsened Exact Match is executed on a monthly basis and includes population of the EA, average weekly wage, and time period. In Model 3, Coarsened Exact Match is executed in month prior to UberX entry and includes EA population density and volume of Kickstarter campaigns. Robust standard errors, clustered by EA, in parentheses - *** p<0.01, ** p<0.05, * p<0.1

Table 6: Random Implementation Test

Estimation	Campaigns with Seasonal and Year Fixed Effects	Campaigns with Quarter Fixed Effects
μ of Random β	-0.00007	0.00006
σ Random β	0.03482	0.03443
Estimated β (Rel Time t-4)	-0.143	-0.168
Replications	1000	1000
Z-Score	-4.105291	-4.881488
P-Value	p<0.001	p<0.001

Table 7: Random Implementation Swapping City Vectors

	(1)	(2)
DV	Campaigns	Campaigns
μ of Rel Time $_{(t-6)}$	-0.0564817	-0.085929
σ of Rel Time $_{(t-6)}$	0.2699605	0.2683418
μ of Rel Time $_{(t-5)}$	-0.0814847	-0.0318059
σ of Rel Time $_{(t-5)}$	0.1273561	0.1249748
μ of Rel Time $_{(t-4)}$	-0.0754988	-0.0131784
σ of Rel Time $_{(t-4)}$	0.0742611	0.0709079
μ of Rel Time $_{(t-3)}$	0.0126101	-0.0094269
σ of Rel Time $_{(t-3)}$	0.0590545	0.0511089
μ of Rel Time $_{(t-2)}$	0.007137	-0.0051064
σ of Rel Time $_{(t-2)}$	0.0487959	0.0432838
Rel Time $_{(t-1)}$	Omitted	
μ of Rel Time $_{(t0)}$	0.0278718	0.0009403
σ of Rel Time $_{(t0)}$	0.0560146	0.0552895
μ of Rel Time $_{(t+1)}$	-0.0096916	0.0030407
σ of Rel Time $_{(t+1)}$	0.0520722	0.0509672
μ of Rel Time $_{(t+2)}$	-0.007911	0.0010768
σ of Rel Time $_{(t+2)}$	0.063213	0.0633114
μ of Rel Time $_{(t+3)}$	0.0112026	0.0092805
σ of Rel Time $_{(t+3)}$	0.0757703	0.0789072
μ of Rel Time $_{(t+4)}$	-0.0114517	0.0184322
σ of Rel Time $_{(t+4)}$	0.0876446	0.0900819
μ of Rel Time $_{(t+5)}$	0.0063007	0.0217177
σ of Rel Time $_{(t+5)}$	0.1038831	0.1109839
Replications	500	500
EA Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	No
Seasonal Effects	Yes	No
Quarter Effects	No	Yes
N	3,612	3,612
Number of Groups	172	172

Table 8: Consideration of Other Ridesharing Services

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Others Omitted	Others Omitted	Others Included	Others Included	Uber Black	Uber Black
DV	Campaigns	Campaigns	Campaigns	Campaigns	Campaigns	Campaigns
Rel Time $_{(t-6)}$	0.226***	0.243***	0.186***	0.200***		
	(0.0625)	(0.0567)	(0.0643)	(0.0607)		
Rel Time $_{(t-5)}$	-0.0695	-0.0893	-0.0373	-0.0475	0.107**	0.228***
	(0.0709)	(0.0690)	(0.0661)	(0.0633)	(0.0502)	(0.0433)
Rel Time $_{(t-4)}$	-0.0547	0.0549	-0.0545	0.0193	-0.0224	0.0453
	(0.0560)	(0.0583)	(0.0601)	(0.0611)	(0.139)	(0.0964)
Rel Time $_{(t-3)}$	0.0416	0.0478	0.0207	0.0371	-0.0951	-0.0688
	(0.0587)	(0.0504)	(0.0539)	(0.0465)	(0.0689)	(0.0627)
Rel Time $_{(t-2)}$	0.0328	0.0380	0.0451	0.0201	-0.0682*	-0.0369
	(0.0306)	(0.0297)	(0.0313)	(0.0288)	(0.0400)	(0.0508)
Rel Time $_{(t-1)}$			Omitted			
Rel Time $_{(t0)}$	-0.0252	-0.0139	0.0172	0.0216	0.121	0.127
	(0.0423)	(0.0369)	(0.0385)	(0.0321)	(0.0944)	(0.0778)
Rel Time $_{(t+1)}$	0.0306	0.0340	0.0501	0.0201	0.0573	0.0685
	(0.0628)	(0.0396)	(0.0411)	(0.0361)	(0.0839)	(0.0736)
Rel Time $_{(t+2)}$	0.0329	0.0259	0.0188	0.0193	0.000328	0.000633
	(0.0768)	(0.0683)	(0.0409)	(0.0383)	(0.0713)	(0.0642)
Rel Time $_{(t+3)}$	0.00611	0.0373	0.0367	0.00835	-0.0152	0.0288
	(0.0502)	(0.0555)	(0.0515)	(0.0445)	(0.104)	(0.0814)
Rel Time $_{(t+4)}$	-0.102*	-0.162**	-0.0722	-0.0940	0.0200	-8.00e-05
	(0.0531)	(0.0747)	(0.0646)	(0.0685)	(0.0951)	(0.0782)
Rel Time $_{(t+5)}$			-0.103	-0.104*	0.0123	0.0428
			(0.0656)	(0.0580)	(0.0917)	(0.0812)
Rel Time $_{(t+6)}$			-0.165**	-0.186**	0.0563	0.0304
			(0.0690)	(0.0728)	(0.107)	(0.0862)
Rel Time $_{(t+7)}$			-0.198**	-0.175**	0.0229	0.0219
			(0.0825)	(0.0763)	(0.106)	(0.0899)
Rel Time $_{(t+8)}$			-0.237***	-0.271***	-0.0813	-0.0808
			(0.0830)	(0.0861)	(0.109)	(0.100)
EA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	Yes	No	Yes	No
Seasonal Effects	Yes	No	Yes	No	Yes	No
Quarter Effects	No	Yes	No	Yes	No	Yes
Observations	3,058	3,058	3,612	3,612	3,612	3,612
Number of Groups	170	170	172	172	172	172

Dependent Variable Campaigns indicates the DV is the number of Kickstarter Campaigns launched in *i t*. Sample Others Omitted indicates observations where Lyft, Sidecar, Curb, and Flywheel are present are omitted from the sample. Sample Others Included indicates that relative time dummies are defined based on the first existence of either Uber, Lyft, Sidecar, Curb, and Flywheel. Sample Uber Black indicates relative time dummies based on Uber Black implementation. Year fixed effects indicates a time fixed effect applied at the yearly level. Seasonal fixed effect indicates the application of an additional seasonal fixed effect at the quarter level. Quarter fixed effect indicates a fixed effect for the three month period of time the observation resides in. Rel Time t-x indicates the number of quarters prior to, or after, the implementation of Uber X is from the focal time period. Coefficients for relative time periods prior to t-5 are estimated but not included in the interest of space. Others omitted sample indicates observations with other discount ridesharing services are omitted. Others included sample indicates that relative time dummies are reoperationalized based on the entry of any discount service. Uber Black indicates relative time dummies are based on Uber Black entry. Estimator is a PPML. Standard errors, in parentheses, are robust and clustered by EA – *** p<0.01, ** p<0.05, * p<0.1

Table 9: Replication of Relative Time Model with EA Specific Linear Trends

	(1)	(2)
DV	Campaigns	Campaigns
Rel Time $_{(t-6)}$	0.239*	0.0231
	(0.141)	(0.149)
Rel Time $_{(t-5)}$	-0.0157	0.0380
	(0.172)	(0.179)
Rel Time $_{(t-4)}$	0.0391	0.00460
	(0.0553)	(0.0655)
Rel Time $_{(t-3)}$	0.0882*	-0.0273
	(0.0500)	(0.0556)
Rel Time $_{(t-2)}$	0.0527	-0.0691
	(0.0399)	(0.0463)
Rel Time $_{(t-1)}$	Omitted	
Rel Time $_{(t0)}$	0.0144	0.0837*
	(0.0434)	(0.0502)
Rel Time $_{(t+1)}$	-0.0299	0.0438
	(0.0460)	(0.0513)
Rel Time $_{(t+2)}$	-0.0669	-0.0261
	(0.0575)	(0.0615)
Rel Time $_{(t+3)}$	-0.120	0.0545
	(0.0795)	(0.0812)
Rel Time $_{(t+4)}$	-0.244**	-0.0283
	(0.101)	(0.100)
Rel Time $_{(t+5)}$	-0.290**	-0.139
	(0.126)	(0.126)
Rel Time $_{(t+6)}$	-0.350**	-0.260
	(0.155)	(0.161)
Rel Time $_{(t+7)}$	-0.417**	-0.420**
	(0.198)	(0.200)
Year Fixed Effects	Yes	No
Seasonal Effects	Yes	No
Quarter Effects	No	Yes
N	3,612	3,612
Number of Groups	172	172

Dependent Variable Campaigns indicates the DV is the number of Kickstarter Campaigns launched in *i t.* Year fixed effects indicates a time fixed effect applied at the yearly level. Seasonal fixed effect indicates the application of an additional seasonal fixed effect at the quarter level. Quarter fixed effect indicates a fixed effect for the three month period of time the observation resides in. Rel Time t-x indicates the number of quarters prior to, or after, the implementation of Uber X is from the focal time period. Coefficients for relative time periods prior to t-6 and after t+7 are estimated but not included in the interest of space. Estimator is a PPML. Estimation includes EA specific time trends in lieu of EA fixed effects. Robust standard errors, clustered by EA, in parentheses - *** p<0.01, ** p<0.05, * p<0.1

Table 10: Relative Time Model of the Effect of Uber X Entry on Change in Self-Reported Occupation in the US Census Bureau's IPUMS-CPS data

	(1)	(2)	(3)	(4)
DV	Self-Employment - Driver Not Primary Occupation	Self-Employment - Driver Not Primary Occupation	All Self Employment	All Self Employment
Rel Time $_{(t-6)}$	-0.000602	0.00796	0.00250	0.0109
	(0.0227)	(0.0228)	(0.0225)	(0.0224)
Rel Time $_{(t-5)}$	0.0496**	0.0531***	0.0520**	0.0551***
	(0.0206)	(0.0205)	(0.0206)	(0.0206)
Rel Time $_{(t-4)}$	0.000657	0.00108	0.00340	0.00344
	(0.0223)	(0.0219)	(0.0222)	(0.0217)
Rel Time $_{(t-3)}$	-0.00250	0.00325	-0.00354	0.00197
	(0.0219)	(0.0236)	(0.0215)	(0.0230)
Rel Time $_{(t-2)}$	-0.00572	-0.000229	-0.00579	-8.33e-05
	(0.0146)	(0.0152)	(0.0144)	(0.0151)
Rel Time $_{(t-1)}$		Omitted		
Rel Time $_{(t0)}$	-0.0260*	-0.0313*	-0.0231	-0.0285*
	(0.0154)	(0.0161)	(0.0152)	(0.0160)
Rel Time $_{(t+1)}$	-0.0500**	-0.0555**	-0.0483**	-0.0537**
	(0.0217)	(0.0229)	(0.0214)	(0.0227)
Rel Time $_{(t+2)}$	-0.0582**	-0.0553**	-0.0564**	-0.0533**
	(0.0227)	(0.0234)	(0.0228)	(0.0237)
Rel Time $_{(t+3)}$	-0.0485**	-0.0455*	-0.0447*	-0.0414*
	(0.0235)	(0.0253)	(0.0234)	(0.0252)
Rel Time $_{(t+4)}$	-0.0551**	-0.0629**	-0.0543**	-0.0614**
	(0.0275)	(0.0285)	(0.0275)	(0.0285)
Rel Time $_{(t+5)}$	-0.0880***	-0.0966***	-0.0862***	-0.0946***
	(0.0293)	(0.0317)	(0.0288)	(0.0312)
Rel Time $_{(t+6)}$	-0.0932**	-0.0991**	-0.0888**	-0.0937**
	(0.0365)	(0.0390)	(0.0360)	(0.0386)
Employment Controls	Yes	Yes	Yes	Yes
EA Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	Yes	No
Seasonal Effects	Yes	No	Yes	No
Quarter Effects	No	Yes	No	Yes
N	10,115	10,115	10,115	10,115
Number of Groups	144	144	144	144

Dependent Variable Campaigns indicates the DV is the number of individuals self-reporting as self-employed. Columns 3 and 4 includes Drivers and Chauffers in this estimation. Year fixed effects indicates a time fixed effect applied at the yearly level. Seasonal fixed effect indicates the application of an additional seasonal fixed effect at the quarter level. Quarter fixed effect indicates a fixed effect for the three month period of time the observation resides in. Rel Time t-x indicates the number of quarters prior to, or after, the implementation of Uber X is from the focal time period. Coefficients for relative time periods prior to t-6 and after t+6 are estimated but not included in the interest of space. Estimator is a PPML. Robust standard errors, clustered by EA, in parentheses - *** p<0.01, ** p<0.05, * p<0.1

Table 11: Relative Time Model of the Effect of Postmates Entry on Kickstarter Campaigns

	(1)	(2)
DV	Campaigns	Campaigns
Rel Time $_{(t-6)}$	0.0784	0.0795
	(0.0637)	(0.0545)
Rel Time $_{(t-5)}$	0.000571	0.00645
	(0.0529)	(0.0452)
Rel Time $_{(t-4)}$	0.0155	0.0459
	(0.0364)	(0.0362)
Rel Time $_{(t-3)}$	0.0311	0.0507*
	(0.0313)	(0.0277)
Rel Time $_{(t-2)}$	0.0532	0.0618**
	(0.0366)	(0.0248)
Rel Time $_{(t-1)}$	Omitted	
Rel Time $_{(t0)}$	-0.0118	0.00244
	(0.0344)	(0.0464)
Rel Time $_{(t+1)}$	-0.145***	-0.125**
	(0.0411)	(0.0567)
Rel Time $_{(t+2)}$	-0.0965	-0.0937**
	(0.0636)	(0.0399)
Rel Time $_{(t+3)}$	-0.171***	-0.160***
	(0.0340)	(0.0415)
Rel Time $_{(t+4)}$	-0.112***	-0.0997**
	(0.0408)	(0.0455)
Rel Time $_{(t+5)}$	-0.345***	-0.385***
	(0.0706)	(0.0724)
EA Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	No
Seasonal Effects	Yes	No
Quarter Effects	No	Yes
N	3,612	3,612
Number of Groups	172	172

Dependent Variable Campaigns indicates the DV is the number of Kickstarter Campaigns launched in i t. Year fixed effects indicates a time fixed effect applied at the yearly level. Seasonal fixed effect indicates the application of an additional seasonal fixed effect at the quarter level. Quarter fixed effect indicates a fixed effect for the three month period of time the observation resides in. Rel Time t-x indicates the number of quarters prior to, or after, the implementation of Postmates is from the focal time period. Coefficients for relative time periods prior to t-6 and after t+5 are estimated but not included in the interest of space. Estimator is a PPML. Standard errors, in parentheses, are robust and clustered by EA – *** p<0.01, ** p<0.05, * p<0.1

Table 12: Relative Time Model of the Effect of Uber X Entry on Kickstarter Campaigns By Campaign Success

DV	(1) Unfunded Campaigns	(2) Unfunded Campaigns	(3) Partially Funded Campaigns	(4) Partially Funded Campaigns	(5) Funded Campaigns	(6) Funded Campaigns	(7) Hyperfunded Campaigns	(8) Hyperfunded Campaigns
Rel Time $_{(t-6)}$	0.00465	-0.0334	0.280***	0.251***	0.133	0.125	0.0923	0.0915
	(0.220)	(0.238)	(0.0993)	(0.0889)	(0.0913)	(0.0937)	(0.193)	(0.195)
Rel Time $_{(t-5)}$	-0.0118	-0.0523	-0.0189	-0.0552	-0.151	-0.162	0.360	0.356
	(0.173)	(0.152)	(0.0700)	(0.0708)	(0.144)	(0.139)	(0.222)	(0.229)
Rel Time $_{(t-4)}$	-0.188	-0.0842	-0.0635	-0.00501	-0.0262	-0.00963	0.132	0.151
	(0.118)	(0.117)	(0.0569)	(0.0626)	(0.0721)	(0.0749)	(0.132)	(0.133)
Rel Time $_{(t-3)}$	0.0168	-0.0535	0.0581	0.0166	-0.00352	-0.0139	0.183	0.176
	(0.126)	(0.112)	(0.0657)	(0.0619)	(0.0548)	(0.0540)	(0.137)	(0.138)
Rel Time $_{(t-2)}$	0.0744	-0.00241	0.115***	0.0671	-0.0493	-0.0577	-0.0314	-0.0436
	(0.100)	(0.0907)	(0.0440)	(0.0481)	(0.0627)	(0.0639)	(0.105)	(0.108)
Rel Time $_{(t-1)}$					Omitted			
Rel Time $_{(t0)}$	0.0580	-0.0381	0.0819**	0.0215	-0.0411	-0.0531	-0.0718	-0.0846
	(0.0855)	(0.0671)	(0.0342)	(0.0318)	(0.0397)	(0.0367)	(0.0628)	(0.0666)
Rel Time $_{(t+1)}$	0.0743	-0.00370	0.0574	0.00987	-0.0343	-0.0478	0.0310	0.0192
	(0.0866)	(0.0773)	(0.0461)	(0.0460)	(0.0395)	(0.0410)	(0.0853)	(0.0889)
Rel Time $_{(t+2)}$	-0.0296	-0.106	0.0262	-0.0209	-0.0191	-0.0336	-0.0996	-0.112
	(0.0849)	(0.0879)	(0.0482)	(0.0548)	(0.0512)	(0.0527)	(0.106)	(0.108)
Rel Time $_{(t+3)}$	-0.0999	-0.204*	-0.00556	-0.0738	-0.0448	-0.0644	-0.0535	-0.0708
	(0.104)	(0.114)	(0.0677)	(0.0799)	(0.0574)	(0.0588)	(0.120)	(0.125)
Rel Time $_{(t+4)}$	-0.261**	-0.315**	-0.112	-0.141*	-0.110*	-0.118*	-0.0455	-0.0515
	(0.126)	(0.139)	(0.0687)	(0.0753)	(0.0639)	(0.0659)	(0.145)	(0.145)
Rel Time $_{(t+5)}$	-0.251*	-0.314**	-0.0664	-0.105	-0.0958	-0.104	-0.0695	-0.0804
	(0.142)	(0.143)	(0.0727)	(0.0762)	(0.0718)	(0.0720)	(0.177)	(0.178)
EA Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No
Seasonal Effects	Yes	No	Yes	No	Yes	No	Yes	No
Quarter Effects	No	Yes	No	Yes	No	Yes	No	Yes
N	3,444	3,444	3,612	3,612	3,549	3,549	3,171	3,171
Number of Groups	164	164	172	172	169	169	151	151

Dependent Variable definitions related to the number of Kickstarter Campaigns launched in i t. Unfunded Campaigns indicates zero funding received. Partially Funded Campaigns indicate less than 100% funding received. Funded Campaigns indicate funding goal was met. Hyperfunded Campaigns indicate 200% of funding goal reached. Year fixed effects indicates a time fixed effect applied at the yearly level. Seasonal fixed effect indicates the application of an additional seasonal fixed effect at the quarter level. Quarter fixed effect indicates a fixed effect for the three-month period of time the observation resides in. Rel Time t-x indicates the number of quarters prior to, or after, the implementation of Uber is from the focal time period. Coefficients for relative time periods prior to t-6 and after t+5 are estimated but not included in the interest of space. Estimator is a PPML. Standard errors, in parentheses, are robust and clustered by EA – *** p<0.01, ** p<0.05, * p<0.1

Table 13: Relative Time Model of the Effect of Uber X Entry on Kickstarter Pledges

	(1)	(2)
	Dollars	Dollars
DV	Pledged	Pledged
Rel Time $_{(t-6)}$	0.0521	0.0569
	(0.175)	(0.174)
Rel Time $_{(t-5)}$	-0.171	-0.164
	(0.191)	(0.189)
Rel Time $_{(t-4)}$	0.112	0.103
	(0.124)	(0.123)
Rel Time $_{(t-3)}$	0.108	0.114
	(0.0946)	(0.0966)
Rel Time $_{(t-2)}$	0.0208	0.0264
	(0.101)	(0.102)
Rel Time $_{(t-1)}$	Omitted	
Rel Time $_{(t0)}$	0.0782	0.0859
	(0.0586)	(0.0630)
Rel Time $_{(t+1)}$	-0.000292	0.00817
	(0.0660)	(0.0695)
Rel Time $_{(t+2)}$	-0.0822	-0.0731
	(0.0728)	(0.0719)
Rel Time $_{(t+3)}$	-0.0637	-0.0513
	(0.0774)	(0.0811)
Rel Time $_{(t+4)}$	-0.0163	-0.0112
	(0.0918)	(0.0919)
Rel Time $_{(t+5)}$	-0.0601	-0.0530
	(0.109)	(0.108)
EA Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	No
Seasonal Effects	Yes	No
Quarter Effects	No	Yes
N	3,612	3,612
Number of Groups	172	172

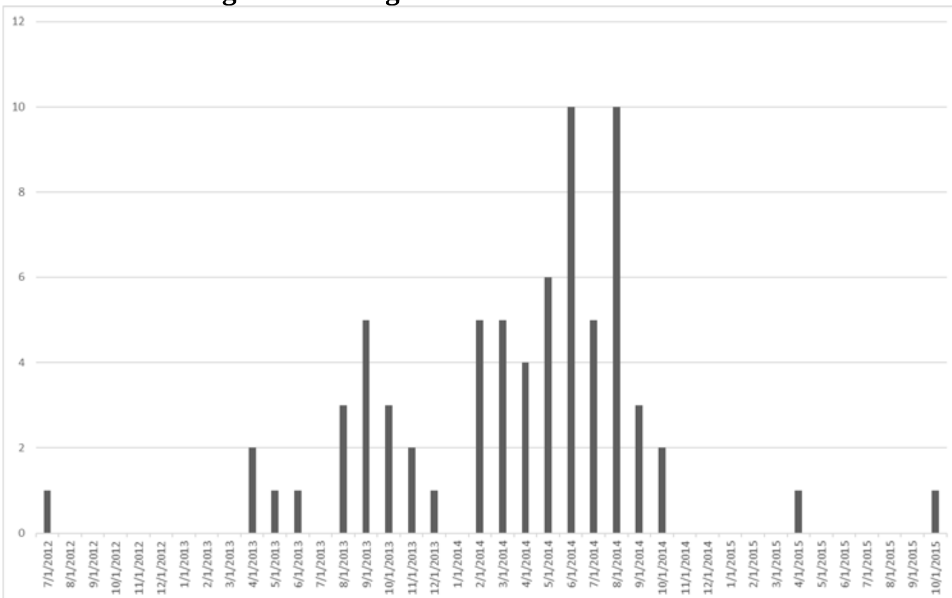
Dependent Variable is the number of dollars donated to Kickstarter Campaigns in i t. Year fixed effects indicates a time fixed effect applied at the yearly level. Seasonal fixed effect indicates the application of an additional seasonal fixed effect at the quarter level. Quarter fixed effect indicates a fixed effect for the three month period of time the observation resides in. Rel Time t-x indicates the number of quarters prior to, or after, the implementation of Uber is from the focal time period. Coefficients for relative time periods prior to t-6 and after t+5 are estimated but not included in the interest of space. Estimator is a PPML. Standard errors, in parentheses, are robust and clustered by EA – *** p<0.01, ** p<0.05, * p<0.1

Table 14: Relative Time Model of the Effect of Uber X Entry on Self Reporting as Driver in iPUMS

	(1)	(2)
	Driver or Chauffer	Driver or Chauffer
DV	as Primary Occ.	as Primary Occ.
Rel Time $_{(t-6)}$	0.0739	0.0988
	(0.161)	(0.178)
Rel Time $_{(t-5)}$	0.0251	0.0224
	(0.129)	(0.141)
Rel Time $_{(t-4)}$	-0.0596	-0.0928
	(0.123)	(0.133)
Rel Time $_{(t-3)}$	-0.118	-0.135
	(0.135)	(0.142)
Rel Time $_{(t-2)}$	0.0155	0.00446
	(0.103)	(0.125)
Rel Time $_{(t-1)}$	Omitted	
Rel Time $_{(t0)}$	0.121	0.146
	(0.111)	(0.125)
Rel Time $_{(t+1)}$	0.142	0.163
	(0.132)	(0.142)
Rel Time $_{(t+2)}$	0.121	0.122
	(0.136)	(0.134)
Rel Time $_{(t+3)}$	0.154	0.155
	(0.123)	(0.128)
Rel Time $_{(t+4)}$	0.217*	0.284**
	(0.111)	(0.121)
Rel Time $_{(t+5)}$	0.251*	0.339**
	(0.150)	(0.148)
Rel Time $_{(t+6)}$	0.343**	0.425***
	(0.135)	(0.145)
Employment Controls	Yes	Yes
EA Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	No
Seasonal Effects	Yes	No
Quarter Effects	No	Yes
N	10,115	10,115
Number of Groups	144	144

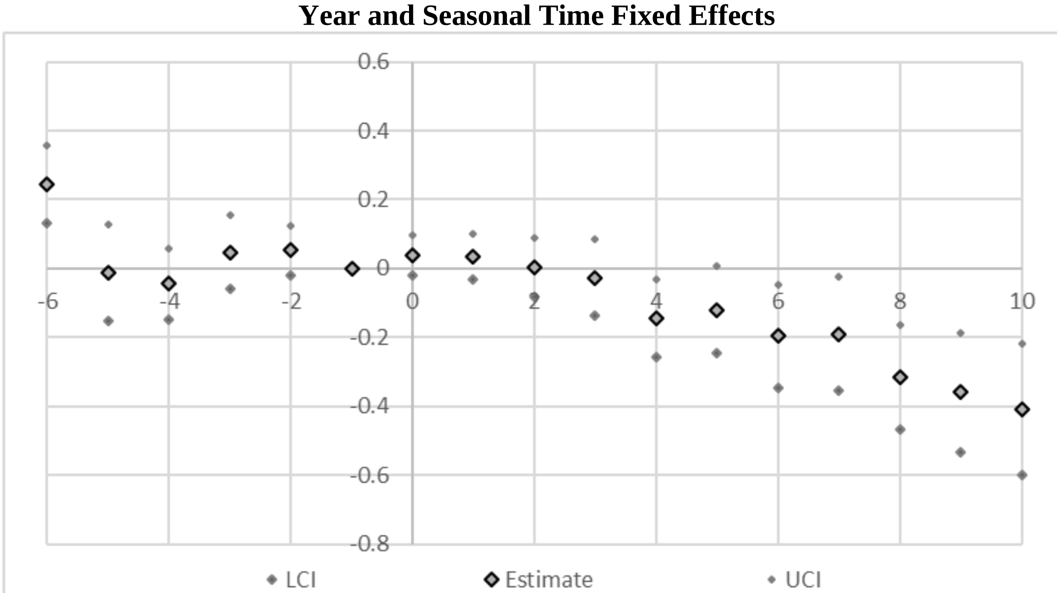
Dependent Variable is the number of people self reporting as drivers or chauffers in CPS-IPUMS. Year fixed effects indicates a time fixed effect applied at the yearly level. Seasonal fixed effect indicates the application of an additional seasonal fixed effect at the quarter level. Quarter fixed effect indicates a fixed effect for the three month period of time the observation resides in. Rel Time t-x indicates the number of quarters prior to, or after, the implementation of Uber is from the focal time period. Estimator is a PPML. Standard errors, in parentheses, are robust and clustered by EA – *** p<0.01, ** p<0.05, * p<0.1

Figure 1: Histogram Uber X Platform Entries



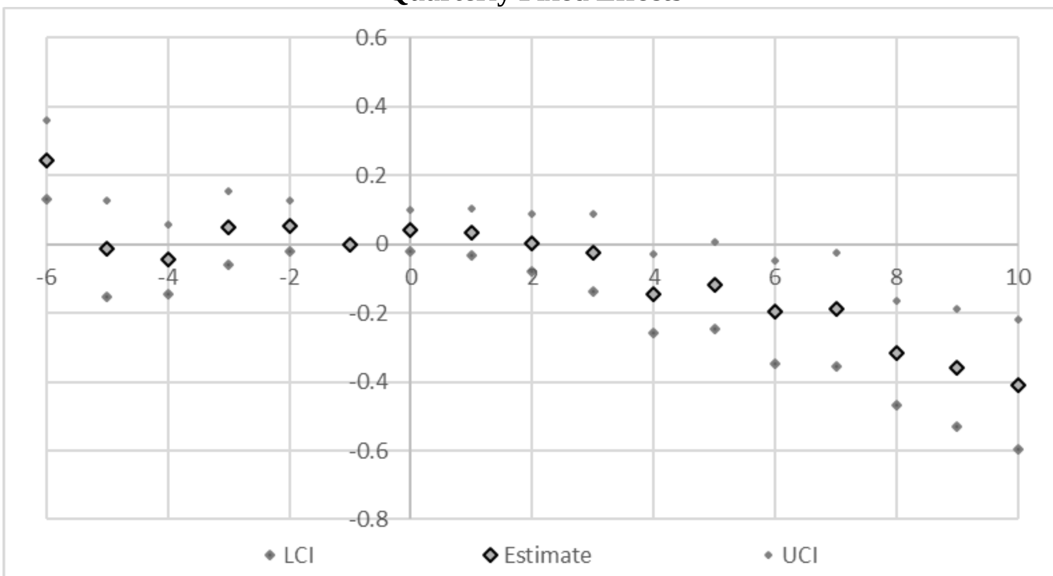
X Axis – Time (Months) / Y-Axis – Number of Entries

Figure 2: Effect of Uber X on Kickstarter Campaign Launches
Year and Seasonal Time Fixed Effects



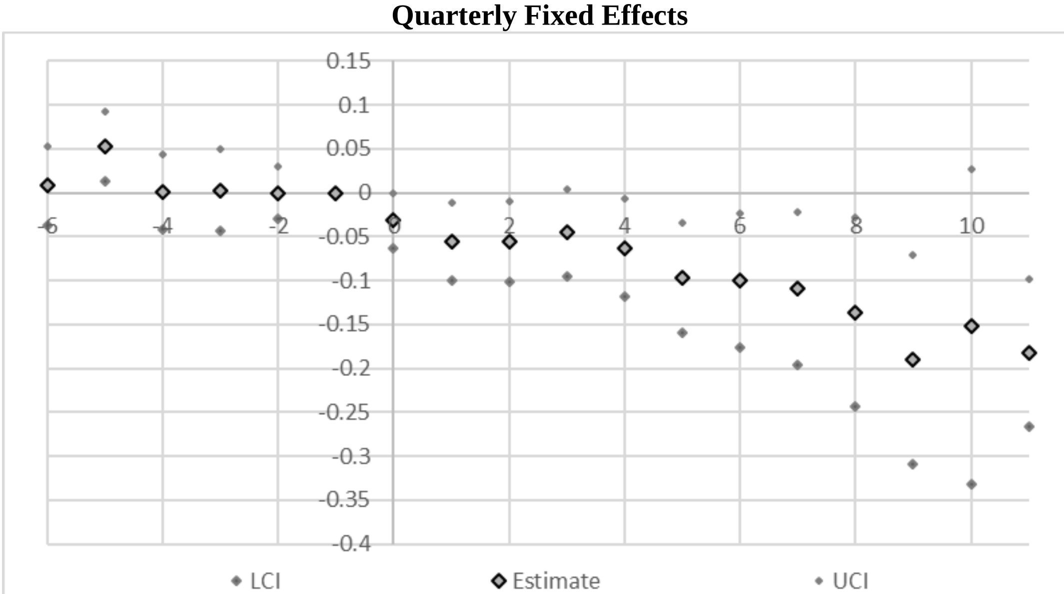
X Axis – Time (Quarters) / Y-Axis – Change in Kickstarter Campaign Launches
LCI and UCI and 95% Confidence intervals of the effect. Estimate is effect size.

Figure 3: Effect of Uber X on Kickstarter Campaign Launches
Quarterly Fixed Effects



X Axis – Time (Quarters) / Y-Axis – Change in Kickstarter Campaign Launches
LCI and UCI and 95% Confidence intervals of the effect. Estimate is effect size.

Figure 4: Effect of Uber X on Self Employment in US Census IPUMS-CPS Data
Quarterly Fixed Effects



X Axis – Time (Quarters) / Y-Axis – Change in Number of Self Employed Individual in Economic Area
LCI and UCI and 95% Confidence intervals of the effect. Estimate is effect size.