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

Gordon Burtch
Carlson School of Management
University of Minnesota

Seth Carnahan
Stephen M. Ross School of Business
University of Michigan

Brad N. Greenwood
Fox School of Business
Temple University

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Gordon Burtch
Carlson School of Management
University of Minnesota

Seth Carnahan
Ross School of Business
University of Michigan

Brad N Greenwood
Fox School of Business
Temple University
brad.n.greenwood@gmail.com

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 set of natural experiments, 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.

Keywords: *gig economy, digital platforms, innovation, crowdfunding, entrepreneurship, difference in difference, natural experiment*

* 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, Chatterjee 2001, 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, LendingClub, Postmates, Uber, TaskRabbit). Collectively referred 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 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 2015, Zervas et al. 2015). In this work, we extend this literature by offering the first consideration of how the entry of these platforms influence local entrepreneurial activity[1].

With industry disruption comes the expectation of eventual economic growth, innovation, and entrepreneurship (Gans et al. 2002, Ireland et al. 2003, McAfee and Brynjolfsson 2008). However, existing observations provide little evidence of changes in entrepreneurial activity deriving from the gig-economy.[2] Traditional economic measures of employment and

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[1] When referring to "entrepreneurial activity" we exclude employment on the gig-economy platform itself. For example, when examining the effect of Uber on local entrepreneurial activity, we treat employment as an Uber driver as out of scope. Recent class action law suits filed against the platforms (e.g. http://uberlawsuit.com/, http://www.uberlitigation.com/) support the view many drivers view themselves as employees of the firm rather than independent contractors.

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

productivity are often coarse and subject to lags, making it difficult to capture changes stemming from relatively recent developments (Sundararajan 2014).[3] Further, the *a priori* relationship between the gig-economy and entrepreneurial activity is unclear, with both the popular press and the scholarly community providing competing arguments. 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 their 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 then to set their own schedules while earning stable pay (Hall and Krueger 2015).[4,5] On the other hand, many researchers have noted that un- and under-employment are significant drivers of entrepreneurial activity. This is because people who do not believe they have other, acceptable, employment options may choose to engage in entrepreneurial activity because they have low opportunity costs (Acs and Armington 2006, Armington and Acs 2002, Fairlie 2002, Storey 1991). If this is the case, the arrival of gig-economy platforms may slow entrepreneurial activity by providing alternate employment opportunities for these lower quality entrepreneurs (Block and Koellinger 2009).

These two logics offer competing predictions for 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

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

exploit a set of natural experiments: the entry of the ride-sharing service Uber and the entry of the on-demand delivery service Postmates into local markets. We examine 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. This econometric strategy offers us two notable benefits. First, because the rollout of Uber and Postmates is staggered both temporally and geographically, i.e. the services enter different locations at different times, we are able to exploit a difference in difference design that mitigates many of the endogeneity concerns which are present when studying entrepreneurial entry. Second, by focusing on Kickstarter campaign launches we are able to capture an early stage of 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).

Findings indicate that the entry of each service, viz. Uber and 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 may choose to work in the gig-economy rather than trying to pursue entrepreneurial projects of relatively low quality. The identified effects are also pronounced. For example, 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].

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[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.

Three notable contributions stem from this work. 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, and our findings suggest a decline in local Kickstarter projects (particularly lower quality projects) following the introduction of Uber X and Postmates. We thus 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. 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). Moreover, our work also has implications for the sustainability of crowdfunding platforms. Because the primary effect of gig-economy platform entry is to reduce the volume of low-quality campaigns, 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. Campaigns on crowdfunding platforms must compete for attention and capital, and thus the presence of low quality campaigns is likely to increase search costs for potential campaign backers or redirect funds that might have been better spent elsewhere. By facilitating a reduction in lower quality projects, the gig-economy can enable campaign backers to focus their attention on the high 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). Inasmuch as the increased digitization of daily life has brought with it both negative (Chan and Ghose 2014, Chan et al. 2015) and positive (Burtch et al. 2013, Greenwood and Wattal 2015) externalities, it is incumbent upon researchers to continue breaking open this black box both by considering both the overall effects of platform entry, as well the differential effects wrought the Internet (Edelman and Luca 2014, Greenwood and Agarwal 2015, Rhue 2015). Our work highlights one potentially positive effect: gig-economy platforms may provide job opportunities for individuals who otherwise would engage in lower quality 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 2015, 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 amongst lower-tier hotels. Greenwood and Wattal (2015) study the effect of Uber entry on DUI fatalities and arrive at a similar conclusion when they observe that gig-economy services are most likely to affect price-sensitive consumers.

However, a relative dearth of work has considered the supply side of these markets, in particular, who the suppliers of these services are likely to be. Given that a rigorous examination of supply is necessary to comprehend how these markets function, it is critical that scholars begin to explore these questions and understand the long term implications of the answers that may arise. It should be noted that three marked exceptions to the predominant focus on the demand side of this equation exist: Edelman and Luca (2014), who examine how racial bias affects AirBNB hosts, Rhue (2015), who examines the effect of racial bias on fundraising success, and Morse (2015), who provides a conceptual review of the impacts that P2P lending markets are having on consumer lending.

In this work, we consider how the entry of gig-economy platforms might 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), the predominant motive for service-provider participation in these markets remains an open empirical question. Examining how gig-economy platforms influence entrepreneurial activity in local areas allows us to speak to whether the individuals who provide their labor to these platforms might otherwise be engaged in other forms of entrepreneurial activity.

Slack Resources and Entrepreneurship

Why might the entrance of gig-economy platforms increase the amount of local entrepreneurial activity? Extant literature offers two arguments. 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 have been argued to offer service providers a combination of scheduling flexibility and stable income, their entry may enable would-be entrepreneurs to strategically re-allocate 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 (George 2005, Shah and Tripsas 2007, Voss et al. 2008). In principle, gig-economy employment may 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 localized 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, significant anecdotal evidence underscores the wild success of Google and 3M's "free time policies," which have led to company innovations ranging from the Post-It[7] to Google's secret research lab, Google X[8].

In the context of the gig-economy, this logic is compelling and would suggest that the inherent flexibility of contract based employment would allow the nascent entrepreneur to optimize her time in such a way that she could strategically redeploy time and other assets to a

[7] http://www.historyextra.com/article/feature/velcro-viagra-10-products-were-invented-accident

[8] http://www.bloomberg.com/bw/articles/2013-05-22/inside-googles-secret-lab; note that Google has significantly restricted this policy in recent years.

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 who enforce 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 entrepreneurial activity 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 growth, 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), extant research 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) when they cannot find work or are overqualified for their current position based on education or experience (Åstebro et al. 2011). It is this second class of entrepreneur who may opt *not* to pursue an entrepreneurial opportunity when presented with the employment the gig-economy offers.

Why might someone who is un- or under-employed no longer pursue entrepreneurship after the entry of a gig-economy platform? 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 under-employment) or because the entrepreneurial 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 gig-economy may change the internal calculus for these would-be entrepreneurs. Because the gig-economy appears to offer significant employment opportunities (for example, according to Hall & Kreuger (2015), Uber employed about 150,000 US-based drivers at the start of 2015; and according to the GAO, about 7.9% of US workers worked in contingent employment as of 2010[9]), it is plausible that entrepreneurial activity may fall as a result of the gig-economy, as 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 local entrepreneurial activity may *fall* as the gig-economy grows.

The presence of compelling theoretical arguments on both sides of this debate creates a natural tension in the literature. 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 empirical analyses to determine the predominant effect.

Methods

Data & Descriptive Statistics

We draw on data from multiple sources to execute our estimations. The first, the volume of crowdfunding campaigns launched on Kickstarter over time, are collected via Kickstarter's API[10] through daily queries of active projects from September of 2013 through March of 2015. By recursively paging through the active campaign list, we capture every campaign that was live at

[9] http://www.gao.gov/products/GAO-15-168R
[10] Kickscraper: http://syntaxi.net/2013/03/24/let-s-explore-kickstarter-s-api/

any given point in time. In turn, we identify the amount of money sought and ultimately raised by each campaign. As our analyses focus on the staggered entry of two gig-economy platforms within the United States, Uber X and Postmates, we further collect data on the entry pattern of these platforms. Uber, founded in 2009, is a mobile smartphone application that allows consumers to 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[11]. Couriers purchase and deliver goods to customers, on demand, from any retail location (restaurant or store) in a city. Data on Uber entry and Postmates entry are retrieved directly from the Uber Blog[12] and from the Postmates website[13], respectively. Finally, we incorporate dynamic socioeconomic data from the Area Resource File[14].

Econometric Specification

The primary econometric specification we employ is a multi-site entry difference-in-difference (DD) relative time model (Angrist and Pischke 2008). Intuitively, 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 data structure 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

[11] https://postmates.com/about
[12] https://blog.uber.com/
[13] https://postmates.com/
[14] http://ahrf.hrsa.gov/

analysis divides the United States into 172 unique economic areas based on shared economic activity (in particular, commuting patterns).[15] Our main dependent variable is the number of Kickstarter campaigns launched in EA *i,* during month *t.* In our initial analyses, the primary independent variable of interest is the dichotomous treatment variable, *Uber X*, which indicates that the ride-sharing service *Uber X* has entered EA *i* at time *t*. Consistent with prior work examining the effect of *Uber*'s entry on a locale (Greenwood and Wattal 2015), we focus on *Uber X,* as opposed to the premium service *Uber Black,* due to the significantly lower startup costs and larger network of drivers[16]. *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 treatment, *Postmates,* also a dichotomous indicator, captures whether *Postmates* has entered EA *i* at 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. Finally, 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.

We employ a relative time model, as opposed to a traditional DD estimation, because it enables us to evaluate the parallel trends assumption. As discussed by Angrist and Pischke (2008), the chief assumption of the DD estimation is that there is no pre-treatment heterogeneity in the trends between treated and untreated groups. If trends in the dependent variable are heterogeneous over time, this presents a problem, because it implies that the untreated group cannot serve as a valid control, i.e. reflection of what would have happened in the absence of

[15] http://www.rita.dot.gov/bts/sites/rita.dot.gov.bts/files/publications/federal_register/1995/html/bts_19950310.html
[16] Replication of our estimations using Uber Black yields no significant correlation.

treatment. Extensively used in extant literature (Autor 2003, Bapna et al. 2015, Chan and Ghose 2014, Greenwood and Wattal 2015), this estimation incorporates a second set of time dummies that indicate the chronological distance between an observation period, *t*, and the timing of treatment in EA *i*. Thus, this approach not only allows us to ensure that there is no pretreatment heterogeneity between the treated and untreated EAs, it also lets us determine how long it takes for significant effects to manifest following treatment. Our final model specification is expressed in Equation (1):

$$y_{it} = \alpha_i + \tau_t + \sum_j \beta_j (u_i * \phi) + \epsilon_{it} \qquad (1)$$

In this specification, y_{it} takes two forms. In the first set of estimations, it is the raw number of campaigns launched in EA *i* at time *t*. In the second set, it is the ln (+1) of the number of campaigns launched in EA *i* at time *t*. α_i represents the vector of EA fixed effects, *u* is an indicator whether or not the platform will ever enter *i*, and φ is the vector of relative time dummies. We initially model *u* as entry of *Uber* into an EA due to 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 effect, and second, as a combination of a year fixed effect and a quarter fixed effect (capturing within-year seasonal trends). For the log specifications we employ an OLS estimator to allow for easily interpretable coefficients. For each of the non-logged specifications, we employ a panel 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).

In contrast to the fixed effects negative binomial (NB) estimator, a PPML estimator enables us to obtain consistent, robust standard errors (clustered by EA), even under conditions of over-dispersion (Wooldridge 1997). Additionally, 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. 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, because a log-OLS specification in count data can produce severely biased estimates (O'Hara and Kotze 2010), particularly under conditions of heteroscedasticity, as a result of Jensen's inequality (Silva and Tenreyro 2006).

Results

Results in Table 3 reveal several interesting findings. When considering the log model in Columns 1 and 2, we witness some evidence of a pre-treatment difference (notable in Rel Time $_{(t-6)}$). 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 2 years after implementation. When considering the results of the non-logged model (Columns 3 and 4), a similar and more robust story emerges. Not only is there no significant evidence of a pre-treatment trend in these regressions, but a negative and significant post treatment trend manifests and stabilizes more quickly, roughly one year after implementation. Specifically, estimates in Model 3 suggest that the number of active Kickstarter projects declines about 14% in treated EAs in the fourth quarter after Uber X's entry. Taken in sum, these estimates provide strong and significant evidence that the entry of gig-economy platforms reduces the number of Kickstarter campaigns in local areas.

Additional analyses and robustness tests

Our initial estimations suggest that there is a decrease Kickstarter campaign launches as a result of platform entry. We next consider an extensive set of falsification tests to ensure robustness. In these tests we continue to use the relative time model, to allow for an evaluation of the parallel trends assumption (Angrist and Pischke 2008, Autor 2003). Further, due to the concerns raised by O'Hara and Kotze (2010), as well as Silva and Tenreyro (2006, 2011), we focus exclusively on the PPML count estimator. These tests include, but are not limited to, examining how other covariates may influence the relationship between the gig-economy platform entry and Kickstarter campaign launches, the validity of the control group, the possibility of false significance and spurious effects arising from autocorrelation (Bertrand et al. 2002), and accounting for the possibility of model dependence and ensuring precision of the estimated treatment effects by repeating our estimations on a reduced, matched sample (Iacus et al. 2012).

Selection Model

While our results from Table 3 provide preliminary evidence that the entrance of gig-economy platforms into local areas has a negative effect on the number of Kickstarter launches, we are mindful that Uber does not randomly select markets for entry. While the absence of a pre-treatment trend in our previous estimations argues against a selection interpretation of our results, we add control variables that help to account for factors that might make a market attractive to Uber while also influencing the rate of local entrepreneurial activity. We include additional regressors in equation 1 which capture changes in local employment and wages. These 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. Results, in Table 4, corroborate our previous estimates. Not only do we see an absence of a pre-treatment trend in both Column 1 and 2, but the effect becomes stable and significant 1 year after *Uber X* enters a market.

Coarsened Exact Match

The next concern we address relates to model dependence and heterogeneity between the treated and control groups. To the extent that unobserved and randomly distributed factors, such as changes in the wealth or population of a local area which were not captured in the selection model, may influence the likelihood of gig-economy platforms (and *Uber* in particular) entering the local market[17], it is important to minimize these potential differences. To execute this next text we pre-process our data, applying a Coarsened Exact Match (CEM) (Blackwell et al. 2009, Iacus et al. 2009, 2012), based on three covariates: population in the local area (to account for market size), average weekly wage (to account for the differences in average local opportunity costs), and current period.

The benefits of pre-processing the data to enforce matching are four-fold. First, as discussed, this limits the degree of ex-ante heterogeneity between the treated and control groups, thereby increasing the strength of any causal claims (Overby and Forman 2014). Second, the match considers both univariate and multivariate imbalance between the treated and control groups, thereby making it significantly more flexible than a traditional propensity score match (Iacus et al. 2009, 2012). Third, ignoring heterogeneity between treated and control groups exacerbates the potential that our results are dependent on our choice of model specification (Iacus et al. 2011). Enforcing matching in the data in terms of control variables helps to ensure that the treatment indicator is independent of the other covariates. Fourth, and finally, because the CEM determines the matching buckets algorithmically, the researcher does not need to estimate them based on her own priors. Inasmuch as our own perceptions of an appropriate match may be biased, as a result of numerous personal confounding factors, this eliminates a significant concern. Results, Table 5, indicate support for our previous findings. The entry of gig-economy platforms significantly reduces the number of Kickstarter campaign launches 9-12

[17] Recall that as we include EA fixed effects we are controlling for the time invariant heterogeneity between EAs. However, as socio-demographic characteristics may change over time, it is important to consider these dynamics.

months after entry.

Random Implementation Test

Although our results have shown striking consistency across a variety of estimations, it is equally important to consider potential problems with the standard errors which may result from the structure of the data. 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." Although our initial estimates (Table 3) cluster on the EA to help account for this dependence, we implement one of the two major tests suggested by the authors, a random implementation test[18], to ensure the robustness of our findings.

To execute this test, we randomly apply our treatment to 1,440 observations in our data, (which corresponds to the number of treated observations), in order to create a pseudo (placebo) treatment. We replicate our estimation from Equation 1 (once again constrained only to *Uber X* and regressing the dependent variable upon the constructed treatment indicators and the same set of fixed effects that entered into our main analyses). We then store the coefficient of this pseudo treatment and replicate the procedure 1,000 times. Conceptually, this test provides two major benefits. The first is that it 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). The second is that it provides a reliable check against outliers. Results are presented in Table 6 and indicate i) that the estimated average β associated with the randomly assigned treatment is not significantly different from zero, suggesting that our main results are reliably estimated, and

[18] The other, collapsing the data into pre and post periods with single observations, is infeasible because our treatment is applied in different locations at different times.

ii) the estimated β is quite small and not driven by serial correlation in the standard errors.

Empirical Extensions

Results of our robustness checks strongly suggest a relationship between gig-economy platform introduction and a reduction in overall Kickstarter campaigns. We next consider additional analyses that help to identify the mechanism underlying this effect, which we argue is a shift away from lower quality entrepreneurial projects and towards employment 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 (inasmuch as the vehicles have specific requirements and can have no cosmetic damage). We therefore expand our analysis to consider the introduction another platform, *Postmates*, an on-demand courier and delivery service. Three significant benefits come from this approach. First, following Goldfarb and King (2015), we respond to active calls to safeguard our results against spurious correlation by replicating them using multiple datasets. Second, 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 for service providers. *Postmates* requires only that a delivery person have a bicycle, while *Uber X* requires that a driver have an automobile in relatively good condition. Third, 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, we must rule out this potential alternate explanation (Younkin and Kashkooli 2013, 2016).

Bearing this in mind, we replicate our relative time model, this time focusing on *Postmates* entry. Results are in Table 7 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 *UberX* (this is confirmed at Rel Time $_{(t+5)}$ by a set of pairwise Wald tests, with values of 2.35 and 2.42 respectively, $p < 0.05$).

Taken in sum these results indicate the resolution of two significant concerns which might undermine the argument that the change in entrepreneurial activity is a result of lower quality entrepreneurs selecting out of the market. First, to the degree that *Postmates* has significantly lower startup costs, we see that a platform with a lower fixed cost of entry creates a stronger reduction in entrepreneurial activity. Overall, this is consistent with the idea that individuals with lower opportunity costs are shifting their efforts away from entrepreneurship and towards gig-economy employment. Second, inasmuch as it is possible that *Uber* and *Postmates* are substituting as a means for the nascent entrepreneur to acquire excess capital, it is important that the lower paying platform (Postmates) have the larger effect.[19] In order 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.[20]

Failed vs. Successful Campaigns

Our next concern relates to the quality of campaigns which have been launched, which we proxy with fundraising outcomes. To the extent that the gig-economy is reducing entrepreneurial

[19] 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/

[20] Unreported analysis also suggests that the marginal campaign size is decreasing post platform entry, further corroborating the fact that the platforms are not being substituted for (because it is not smaller campaigns that are selecting off the crowdfunding platform).

activity by shifting effort away from lower quality projects, we anticipate that the effect of gig economy platform entry is stronger (weaker) amongst crowdfunding campaigns that experience lesser (greater) fundraising outcomes.

To execute this test we discretize our dependent variable into four buckets and replicate our estimations. The first, Unfunded Campaigns, represents campaigns 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 marginally 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, amongst campaigns that are of lower quality and thus ultimately performed less well in fundraising. Results are in Table 8. As expected, we note that the lion's share of the effect accumulates to campaigns which received no funding (Columns 1 and 2). Further, we note in Columns 3-6 that there is a marginal decrease in both partially funded campaigns and funded campaigns, but the effect is intermittent. Strikingly, in Columns 7 and 8, we note that there is no change amongst hyperfunded campaigns (those that top 200% of their funding goal). Taken in sum, these results appear to confirm our conclusion that gig-economy platforms reduce entrepreneurial activity by allowing individuals to pursue gig-economy employment rather than lower quality entrepreneurial projects.

Total Dollars Pledged

One, further, possible alternative explanation for these patterns 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 Kickstarter funding is often provided by local individuals (Agrawal et al. 2010), a local economic downturn may reduce capital available to Kickstarter entrepreneurs while also attracting platform entry.

However, it is perhaps useful to note that some recent work has demonstrated that a significant portion of funding on Kickstarter may be 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 accounting for 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 available that is available for investment or pledging. Results in Table 9 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 do not have an effect on total capital pledged by Kickstarter funders.

Changes in Self-Reported Profession

To further enhance the robustness of our results, we examine whether the entry of Uber has a measurable effect on local employment in the taxi and chauffeur industry. Our argument is that individuals are working for Uber instead of engaging in lower quality entrepreneurial activity; while we lack individual-level data that allow us to show this directly, it is helpful to show that Uber's entry shifts local employment towards its industry. To do this, we replicate our main analysis using the Integrated Public Use Microdata Series Current Population Survey (IPUMS-CPS), the US's largest publically available census of individual level microdata[21]. These data

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

derive from quarterly surveys that allow us to track the self-reported occupation of anonymous individuals in a particular geographic area. The survey design provides weights that allow the data to reflect the overall US population. We regress a dichotomous indicator of whether an individual self-reports being a paid driver or chauffer as their primary occupation (occupation code 53-3041) on the dichotomous indicator of *Uber X* entry, time fixed effects and EA fixed effects. Because our dependent variable is dichotomous, we perform this estimation initially using a logit. Owing to the incidental parameters problem with fixed effects in nonlinear models, we then replicate the estimation with a linear probability model. We expect a positive and significant relationship between platform entry and individuals' self-reporting as a paid driver. Results in Table 10 reveal the expected correlation. In both the LPM (Column 2) and the logit (Column 1) specifications, significantly more individuals report that paid driver or chauffer is their primary occupation after the entry of *Uber X*, further supporting our proposed mechanism that these gig-economy platforms are shifting employment patterns. Results are robust to the inclusion or exclusion of survey weights.

Discussion

An interesting tension that arises when one considers how the rise of the gig-economy might influence entrepreneurial activity in other sectors. On the one hand, gig-economy employment might provide individuals with flexibility and resources, thereby increasing entrepreneurial activity. On the other, the presence of gig-economy platforms may direct individuals' efforts towards employment on the platform, thereby reducing entrepreneurial activity. Using a multi-treatment difference-in-difference specification, our analyses indicate a consistent, negative effect of gig-economy platform entry (e.g. *Uber X* and *Postmates*) on the volume of crowdfunding campaign launches in a geographic area. We find that the effect is stronger when the fixed cost of entry for platform service providers (e.g., Uber drivers, Postmates couriers) is lower, and we find that the reduction in campaigns mainly stems from a reduction in lower quality projects. Our interpretation of these results is that gig economy platforms allow individuals with lower opportunity costs to shift their efforts away from lower quality entrepreneurial projects and towards gig-economy 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 relatively unpromising entrepreneurial activities. While many 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 2015)), the supply side of the market remains notably understudied. Further, although we do not observe an increase in entrepreneurial activity, 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 offers rich opportunities for future work which might explore alternative measures that capture these 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 how the supply side of these platforms function, and that future work can build on this analysis to develop a more holistic understanding of participation in the gig-economy.

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. 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)).

Third, the differential effect on more vs. less successful campaigns is particularly interesting, because it 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. To the degree that this reduces the cognitive burden and search costs imposed on crowd-financers, by elevating the overall level of quality on the platform and thereby reducing adverse selection, this should help to ensure the sustainability of the crowdfunding marketplace, because the crowd, facing a budget constraint, can focus its attention and wealth on higher quality campaigns. In contrast, in the absence of the treatment, it is more likely that capital will be temporarily tied up in campaigns that are of insufficient quality to ultimately succeed, cannibalizing potential contributions from more deserving campaigns.

We must also acknowledge that each of the aforementioned mechanisms may be at play, in tandem. In some cases, gig-economy platforms might supply would-be entrepreneurs with the slack resources they require to pursue their passion, whereas in other cases these platforms might provide gainful employment to the under- and unemployed. Our claim is that the latter mechanism dominates. Future work might explore these issues further, considering 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 incredibly valuable.

Finally, this work offers notable insights for policy makers who are currently debating the legality of services like Uber and Postmates. Although our results do not capture potential reductions in employment that may occur 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 benefit when deciding whether to accommodate these platforms. In this same vein, our work contributes to the blossoming 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). To the degree that much of this work is designed to inform policy, either through a change in the broad understanding of digital phenomena (Greenwood and Wattal 2015, Pang et al. 2014), or by highlighting the differential effects which accrue to different groups (Rhue 2015), our work highlights the need to continue down the important path of providing robust empirical evidence which informs extant debate.

This work is, of course, subject to a number of limitations, which offer potentially fruitful avenues 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. Data limitations prevent us from testing this assumption directly. Second, although our results indicate that failed projects decrease more than successful projects, we cannot make substantive comment about the quality of the campaigns which were not initiated or the overall public welfare changes which result from them not being initiated. 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. Finally, because gig-economy platforms are a rather recent development, we are unable to examine the longer term consequences of their entry on entrepreneurial activity. Future work, which has access to longer panels of data, might examine the relationship between the entry of Uber X and the formation and growth of incorporated businesses.

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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

	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	7620000000	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	ln(Campaigns)	ln(Campaigns)	Campaigns	Campaigns
Rel Time $_{(t-6)}$	0.233***	0.220***	0.244***	0.221***
	(0.0764)	(0.0502)	(0.0581)	(0.0550)
Rel Time $_{(t-5)}$	-0.144	-0.0956	-0.0122	-0.0409
	(0.0953)	(0.100)	(0.0709)	(0.0643)
Rel Time $_{(t-4)}$	-0.0299	0.0392	-0.0447	0.00392
	(0.0488)	(0.0476)	(0.0520)	(0.0579)
Rel Time $_{(t-3)}$	0.0508	0.0349	0.0477	0.0147
	(0.0407)	(0.0389)	(0.0551)	(0.0519)
Rel Time $_{(t-2)}$	0.0154	0.00262	0.0522	0.0173
	(0.0344)	(0.0333)	(0.0376)	(0.0414)
Rel Time $_{(t-1)}$		Omitted		
Rel Time $_{(t0)}$	0.0354	0.00182	0.0394	-0.00524
	(0.0363)	(0.0345)	(0.0302)	(0.0263)
Rel Time $_{(t+1)}$	0.0499*	0.0633**	0.0352	-0.00369
	(0.0289)	(0.0294)	(0.0342)	(0.0355)
Rel Time $_{(t+2)}$	0.0466	0.0568	0.00376	-0.0358
	(0.0378)	(0.0362)	(0.0432)	(0.0489)
Rel Time $_{(t+3)}$	0.0306	0.0210	-0.0257	-0.0818
	(0.0451)	(0.0412)	(0.0571)	(0.0673)
Rel Time $_{(t+4)}$	-0.0234	0.00990	-0.143**	-0.168***
	(0.0449)	(0.0441)	(0.0581)	(0.0648)
Rel Time $_{(t+5)}$	-0.0314	-0.0249	-0.120*	-0.150**
	(0.0460)	(0.0462)	(0.0642)	(0.0645)
Rel Time $_{(t+6)}$	-0.0464	-0.0128	-0.196**	-0.216**
	(0.0590)	(0.0657)	(0.0764)	(0.0860)
Rel Time $_{(t+7)}$	-0.0938	-0.0838	-0.189**	-0.222**
	(0.0746)	(0.0740)	(0.0841)	(0.0879)
Rel Time $_{(t+8)}$	-0.160**	-0.219***	-0.315***	-0.398***
	(0.0648)	(0.0641)	(0.0773)	(0.0787)
Rel Time $_{(t+9)}$	-0.191***	-0.0935	-0.359***	-0.330***
	(0.0678)	(0.0703)	(0.0881)	(0.0949)
Rel Time $_{(t+10)}$	-0.356***	-0.364***	-0.408***	-0.450***
	(0.0782)	(0.0784)	(0.0965)	(0.0990)
Economic Area 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
R-Squared	0.154	0.171		
Number of Groups	172	172	172	172

Dependent Variable ln(Campaigns) indicates the DV is the log (+1) of the number of Kickstarter Campaigns launched in *i t*. 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. Estimator is an OLS for Columns 1 and 2 and a PPML for Columns 3 and 4. Standard errors are clustered on the EA for Columns 1 and 2. All standard errors, in parentheses, are robust and clustered on the EA- *** p<0.01, ** p<0.05, * p<0.1

Table 4: Relative Time Model of the Effect of Uber X Entry on Kickstarter Campaigns with Employment Controls

	(1)	(2)
DV	Campaigns	Campaigns
Rel Time $_{(t-6)}$	0.238***	0.219***
	(0.0615)	(0.0568)
Rel Time $_{(t-5)}$	-0.0233	-0.0455
	(0.0685)	(0.0644)
Rel Time $_{(t-4)}$	-0.0543	-0.000623
	(0.0529)	(0.0592)
Rel Time $_{(t-3)}$	0.0421	0.0128
	(0.0567)	(0.0528)
Rel Time $_{(t-2)}$	0.0498	0.0167
	(0.0396)	(0.0422)
Rel Time $_{(t-1)}$	Omitted	
Rel Time $_{(t0)}$	0.0311	-0.00832
	(0.0308)	(0.0283)
Rel Time $_{(t+1)}$	0.0249	-0.00759
	(0.0333)	(0.0360)
Rel Time $_{(t+2)}$	-0.00505	-0.0389
	(0.0446)	(0.0501)
Rel Time $_{(t+3)}$	-0.0350	-0.0849
	(0.0592)	(0.0686)
Rel Time $_{(t+4)}$	-0.162**	-0.175**
	(0.0654)	(0.0707)
Rel Time $_{(t+5)}$	-0.150**	-0.162**
	(0.0732)	(0.0714)
Employment Controls	Yes	Yes
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 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 5: Coarsened Exact Match Relative Time Model of the Effect of Uber X Entry on Kickstarter Campaigns

	(1)	(2)
DV	Campaigns	Campaigns
Rel Time $_{(t-6)}$	0.199***	0.231***
	(0.0752)	(0.0839)
Rel Time $_{(t-5)}$	-0.0503	-0.0386
	(0.0961)	(0.106)
Rel Time $_{(t-4)}$	-0.0877	-0.00782
	(0.0730)	(0.0870)
Rel Time $_{(t-3)}$	0.110	0.0442
	(0.0753)	(0.0775)
Rel Time $_{(t-2)}$	0.0118	-0.00894
	(0.0392)	(0.0465)
Rel Time $_{(t-1)}$	Omitted	
Rel Time $_{(t0)}$	-0.000814	-0.0374
	(0.0298)	(0.0294)
Rel Time $_{(t+1)}$	-0.000433	-0.0482
	(0.0397)	(0.0469)
Rel Time $_{(t+2)}$	-0.0113	-0.0651
	(0.0406)	(0.0505)
Rel Time $_{(t+3)}$	-0.0481	-0.122*
	(0.0509)	(0.0670)
Rel Time $_{(t+4)}$	-0.129**	-0.192***
	(0.0547)	(0.0632)
Rel Time $_{(t+5)}$	-0.139**	-0.205***
	(0.0608)	(0.0704)
EA Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	No
Seasonal Effects	Yes	No
Quarter Effects	No	Yes
N	2,895	2,895
Number of Groups	170	170

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. Coarsened Exact Match is executed on population of the EA, average weekly wage, and time period. 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

Dependent Variable Campaigns indicates the DV is the number of Kickstarter Campaigns launched in *i t*.

Table 7: 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 8: 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 that 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 9: Relative Time Model of the Effect of Uber X Entry on Kickstarter Pledges

	(1)	(2)
DV	Dollars Pledged	Dollars 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 10: Difference in Difference Estimate of Change In Probability of Self-Reported Occupation After Uber X Entry

	(1)	(2)
DV	Driver	Driver
Estimator	Logit	LPM
Uber X	0.217***	0.000684**
	(0.0693)	(0.000337)
Constant	-4.916***	0.00198***
	(0.0873)	(0.000219)
Year Fixed Effects	Yes	Yes
Month Effects	Yes	Yes
EA Fixed Effects	Yes	Yes
N	1,861,144	1,657,292

Dependent Variable 0 / 1 indicator of whether or not individual self-reports as a driver or chauffer (Occupation code 53-3041). Year fixed effects indicates a time fixed effect applied at the yearly level. Month fixed effect indicates the application of an additional monthly fixed effect at the quarter level. Standard errors, in parentheses, are robust and clustered by EA – *** p<0.01, ** p<0.05, * p<0.1