Gig-Labor: Trading Safety Nets for Steering Wheels*

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This paper shows that the introduction of the "gig-economy" changes the way employees respond to job loss. Using a comprehensive set of Uber product launch dates and employee-level data on job separations, we show that laid-off employees with access to Uber are less likely to apply for UI benefits, rely less on household debt, and experience fewer delinquencies. Our empirical strategy is based on a triple difference-in-difference empirical model, comparing the difference in outcome variables 1) pre- and post-layoff, 2) before and after Uber enters a market, and 3) between workers with and without the ability to participate on the ride-sharing platform (car-owners inferred from auto credit histories). In support of our identification strategy, we find no apparent pre-existing difference in outcomes in the months leading up to Uber's entry into a market. Moreover, the effects are severely attenuated for workers with an auto lease, for whom the viability of participating on the ride-sharing platform is significantly reduced. Overall, our findings show that the introduction Uber had a profound effect on labor markets.

JEL classification: D10, E24, H53, J23, J65

keywords: gig-economy, labor markets, unemployment insurance, household debt, credit delinquencies

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Abstract

This paper shows that the introduction of the "gig-economy" changes the way employees respond to job loss. Using a comprehensive set of Uber product launch dates and employee-level data on job separations, we show that laid-off employees with access to Uber are less likely to apply for UI benefits, rely less on household debt, and experience fewer delinquencies. Our empirical strategy is based on a triple difference-in-difference empirical model, comparing the difference in outcome variables 1) pre- and post-layoff, 2) before and after Uber enters a market, and 3) between workers with and without the ability to participate on the ride-sharing platform (car-owners inferred from auto credit histories). In support of our identification strategy, we find no apparent pre-existing difference in outcomes in the months leading up to Uber's entry into a market. Moreover, the effects are severely attenuated for workers with an auto lease, for whom the viability of participating on the ride-sharing platform is significantly reduced. Overall, our findings show that the introduction Uber had a profound effect on labor markets.

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1. Introduction

The idea that an individual prefers a smooth consumption stream to a lumpy one serves as the foundation of micro-economics (Keynes (1936), Modigliani and Brumberg (1954), Friedman (1957), Hall (1978)). This key insight underpins the expected welfare gains from efficient inter-temporal risk sharing. These gains both act as motivation for government policy, such as the broad deployment of unemployment insurance, and serve as a theoretical justification for a consumer's increased reliance on credit during unemployment spells (Herkenhoff, Phillips, and Cohen-Cole (2016), Sullivan (2008)). Given that a large fraction of households do not have precautionary savings (Federal Reserve Board (2018)), unemployment insurance and access to credit are critical in helping household smooth consumption during unemployment spells.

Yet, both options are less than desirable from the viewpoint of economic efficiency. The presence of a social safety net may distort an individual's incentive to seek re-employment (Baily (1978), Flemming (1978), Ljungqvist and Sargent (1998)). At the same time, an increased reliance on credit may amplify negative economic shocks during downturns, possibly through the limited-liability nature of credit contracts, as evidenced in the recent financial crisis (Mian and Sufi (2011), Mian, Rao, and Sufi (2013), Mian and Sufi (2014), Bernstein (2016)). Each tool acts as a half-measure because neither is able to address the root issue: the existence of frictions that prevent a newly unemployed individual from readily being re-matched in the labor market.

In this paper we examine whether the introduction of the "gig-economy" reduces such frictions, altering the way employees respond to job loss. Using a comprehensive set of Uber product launch dates and employee-level data on job separations, we show that laid-off employees with access to Uber are less likely to rely on unemployment insurance and untapped credit. Following Uber's entry into a market, workers with access to the ride-sharing platform are 4.8% less likely to receive UI benefits. Moreover, they experience a relative decrease in total outstanding balances of $544, or 1.3% of the average individual's debt burden. Finally, we find that the effects of the ride-sharing platform extend to credit performance, with workers experiencing a relative decrease in delinquencies of 2.9%.

To identify these effects, we first leverage the dis-aggregated employment data to identify the set of job separations out of a worker's control (a layoff). From this, we construct an unbalanced panel that tracks each worker for a 25-month period around her layoff. Our empirical strategy is based on a triple difference-in-difference approach. For each layoff, the first difference captures changes in outcome variables following the layoff. The second difference captures heterogeneity in individuals' responses to layoffs based on whether Uber operates in the area. Finally, we compare changes in outcome variables for individuals we classify as possessing a vehicle prior to being laid-off relative to those without a vehicle. This third difference captures the heterogeneity in individuals' ability to earn income by participating on the ride-sharing platform. The high frequency, granular nature of the data allows us to strip out any location specific macro-economic trends and time-invariant heterogeneity across workers with location-time and worker fixed effects.

For the evidence to have a causal interpretation, our empirical strategy requires the following identifying assumption: the timing of Uber's entry into a market is orthogonal to omitted variables that 1) differentially affect the outcome of interest for a car-owner relative to a non-owner, and 2) that the resulting difference is not present during employment but instead materializes post-separation. That is, in the absence of Uber's entry, the difference in outcome variables around layoffs for car-owners and non-owners would be the same for areas with and without Uber.

We present several pieces of evidence to support our identifying assumption. We begin by showing that the timing of Uber's entry is largely dictated by market size (see also Buchak (2018)). To further address concerns of an omitted variable, we decompose the difference between car-owners and non-owners to job loss into the event time around Uber's entry into a market. Consistent with our identifying assumption, we find no evidence of an effect in the months leading up to entry, while the difference is realized almost immediately upon

Uber's entry into a market. Finally, we seek validation for our identification assumption by exploiting the potential difference between those workers with an auto loan versus an auto lease. If our results are driven by differential responses of car-owners and non-owners to layoffs, one would expect to find similar results across both sub-groups of car-owners. In contrast, if workers are turning to Uber then the typical mileage limitation written into auto lease contracts likely reduces the viability of participating on the ride-sharing platform and/or results in fewer miles driven. Consistent with the latter interpretation, we find that our results are primarily concentrated among those car-owners who do not have a car lease. Taken together, these results suggest that our main findings are not being driven by an omitted variable that is correlated with the timing of Uber entries.

Two economic channels stand out as chief candidates to explain our results. First, workers may view Uber as a short-term alternative for a recently lost job, to be used while seeking gainful re-employment elsewhere. Importantly, this mechanism represents a structural shift in labor markets likely to benefit laid-off workers for an extended period of time following Uber's entry. Alternatively, workers may view Uber in the same light as a traditional firm, whose entry yields similar labor market effects to that of a generic employer.

Recent historical episodes suggest that many workers experiencing an income shock treat the ride-sharing platform as a short-term option. Following the two most recent government shutdowns, accounts of government workers moon-lighting in the gig-economy were widely publicized by the media and policy makers alike (Little (2013), Halsey and Aratani (2019)). Anecdotal evidence not withstanding, we perform a series of tests to shed light on the channel through which Uber's entry affects labor markets.

First, we focus on the horizon of Uber's effects on the local economy. If Uber affects labor markets like a typical firm, it's entry may simply provide new job vacancies that recently unemployed workers are able to fill. This would suggest a transient effect which subsides as vacancies are filled. In contrast, we obtain very similar estimates when excluding the initial two-year period following Uber's entry into a local economy. In a second test, we focus on a subset of our sample that is less likely to view Uber as a long-term employment prospect. Using ZIP code level reported income from the IRS, we repeat our analysis on workers from above-median income neighborhoods. Our findings indicate the effects of Uber's entry remain economically and statistically significant when focusing on this sub-group. Finally, we exploit geographic variation in UI benefit generosity across states. If a worker treats Uber as a short-term solution and potential substitute for UI, this trade-off is likely influenced by the potential UI benefits available. On the other hand, if Uber is viewed as long-term employment then the generosity of short-term UI benefits should not impact the decision to participate on the platform. We find that the effects of Uber's entry are stronger in states with smaller expected UI benefits.

Overall, this series of tests suggest that Uber alters labor market dynamics by increasing the pool of easily accessible short-term jobs. Importantly, this mechanism represents a structural shift in labor markets likely to benefit laid-off workers for an extended period of time following Uber's entry.

Our paper contributes to several strands of literature. First, our paper adds to the expansive literature examining different facets of unemployment insurance programs, beginning with early works examining labor market implications and optimality (Flemming (1978), Baily (1978), Mortensen (1977)). Gruber (1997) highlights the consumption smoothing benefit associated with UI programs, which is examined in more depth using micro-level data by both Ganong and Noel (2019) and Kolsrud, Landais, Nilsson, and Spinnewijn (2018), while East and Kuka (2015) documents the effects of UI during the 1970s. While we do not examine consumption directly, to the extent that laid-off workers substitute UI benefits with wages earned on the ride-sharing platform, revealed preferences implied by our results suggest the introduction of Uber allows individuals to achieve a more desirable consumption path. In contrast, there exists an equally large body of work studying the chief cost associated with UI, the disincentive to seek re-employment (see Katz and Meyer (1990), Meyer (1990), Card, Chetty, and Weber (2007), Schmieder, Von Wachter, and Bender (2012) among others). In relation, our paper highlights the important role that the gig economy plays in reducing labor market frictions, and thus, the degree to which moral hazard plays a role.

Our paper also speaks to a second distinct, yet often related, strand of literature examining the effects of consumer credit decisions on the local economy. For instance, in the context of the recent financial crisis, household leverage choices have been linked to employment (Bernstein (2016), Mian and Sufi (2014), Bethune (2015)), consumption (Mian, Rao, and Sufi (2013)), and housing prices (Mian and Sufi (2011)). This fragility is accentuated following a job loss; Gerardi, Herkenhoff, Ohanian, and Willen (2017) documents the rise in mortgage defaults following job loss while Herkenhoff and Ohanian (2012) examines a worker's propensity to skip mortgage payments as a form of "informal" unemployment insurance. At the same time, Hsu, Matsa, and Melzer (2018) highlight the role of UI as a housing market stabilizer, helping individuals avoid foreclosure and the associated deadweight loss. In contrast, we find that delinquencies fall even further following Uber's entry into a market, suggesting the introduction of the gig economy is better able to insulate a local area from the propagation of economic shocks.

Finally, our paper contributes to a growing literature on the role of ride-sharing companies in labor and product markets. Several studies document how Uber's entry affected purchases of new cars and vehicle utilization rates (*e.g.,* Cramer and Krueger (2016), Gong, Greenwood, and Song (2017) , Buchak (2018)). Additional evidence documents the increase in competitive pressure faced by taxi drivers following Uber's entry (*e.g.,* Hall, Horton, and Knoepfle (2017), Berger, Chen, and Frey (2018)). Burtch, Carnahan, and Greenwood (2018) document a negative and significant relationship between ride-sharing and entrepreneurial activity. Further, Barrios, Hochberg, and Yi (2018) show that the arrival of ride-sharing is associated with an increase in the number of motor vehicle fatalities and fatal accidents.[1] Our paper contributes to this growing body of the literature by showing that Uber's entry changes an employee's response to job loss. Specifically, our results indicate that laid-off em-

1. In a recent paper, Ostrovsky and Schwarz (2018) develop a theoretical framework to study implications of the gig-economy for the efficiency of transportation markets.

ployees are less likely to apply for UI benefits, to increase household debt, and to experience a delinquency. Thus, our paper is the first to point out that access to ride-sharing can serve as a private unemployment insurance.

The remainder of this paper is organized as follows: Section 2 discusses the data we use, and the final sample we consider, while our empirical strategy is described in 3. We present our primary findings and additional analysis supporting our identification strategy in Section 4. Finally, we discuss potential economic mechanisms and present tests designed to distinguish between potential candidates in Section 5, while Section 6 concludes.

2. Data

This section describes the data sources used in the analyses, discusses our sample selection process, and presents summary statistics for the final sample considered. The bulk of our empirical tests rely on the intersection of two data sources: Uber product roll-out dates across different geographic areas, and disaggregated credit, un-employment, and UI data provided by Equifax Inc, one of the three credit bureaus involved in collection and transmission of credit and employment data within the US.

2.1. Uber Introduction Data

We obtain the comprehensive set of product launch dates that occurred between June 2012 and February 2016. The data covers roughly 160 geographic regions (typically representing a CBSA) and four product lines offered by the firm (*e.g.,* "UberBLACK"). While the ride-sharing company offers multiple products, which vary in both car quality (standard vs. luxury) and capacity (traditional vs. larger vehicles), we restrict our analysis to the introduction of "UberX" in each region. While UberX represents the first service introduced in a majority (79%) of the regions in our sample, in the remaining instances its introduction lags behind UberBLACK by an average of 7 months. However, to the extent that an individual is able to buffer their labor provision following a job loss by driving for the ride-sharing company, this ability is likely confined to those individuals able to quickly begin driving upon job separation (e.g., individuals already in possession of a qualifying car). Given that the eligibility requirements needed for a car to qualify for UberBLACK are substantially greater than UberX, we focus on the latter product in which a much larger portion of the population may participate.

[Insert Figure 1 Near Here]

Figure 1 illustrates the variation in Uber's entry across different markets. Panel A of Figure 1 reports a histogram of the monthly count of markets in which UberX is introduced over time. The panel demonstrates a considerable degree of time-series variation in market entry. We extend this analysis to the spatial dimension in a second panel. Panel B of Figure 1 illustrates the relatively timing of Uber entry across different states. Specifically, we sort states by the earliest entry date of Uber in any of the state's markets. The panel reports a heat map (choropleth map) of the percentile rank across all represented regions (where a lower percentile corresponds to an earlier introduction date). Consistent with the priors of many people, the figure indicates that Uber entered traditionally large, coastal regions first, followed by more central areas. Appendix Figure OA.1 reports a similar heat map when first ranking individual CBSAs by order of entry, and then displaying the state-level average percentile across all CBSAs in the state. The figure presents similar patterns to that of Figure 1.

2.2. Credit, Employment, & UI

The second data source considered in the analysis is furnished by Equifax Inc., and contains anonymized individual-level data across the following three dimensions: credit, unemployment events, and UI participation. The first of these, the consumer credit histories, contains loan-level information for all individuals with some form of credit history in the US, and requires little explanation given their growing popularity in the literature as of late.

In contrast, we are one of the first to use detailed individual-level job separation and UI participation data that deserves a more detailed discussion.

Both job separation and UI participation data are disseminated to Equifax Inc by self-reporting employers. In order to efficiently administer UI benefits, law mandates that an employer respond to all government requests to verify information provided during the UI claims process. In order to adhere to this, participating employers subscribe to a service offered by Equifax Inc which manages all such claims by governmental bodies on the their behalf. As a result, participating employers report data related to all incidences of job separation to the company. Using this anonymized data, for each job separation, we are able to observe information on the date of the job separation, the reason for the separation, and all future monetary disbursements to those individuals who later apply for UI benefits. We begin by identifying instances of job loss among individuals using this data and merge it to anonymized credit histories that allows us to examine a comprehensive set of outcomes.

Unemployment insurance benefits for the typical worker we observe are administered under the Federal-State Unemployment Insurance Program. While national guidelines are established by the Department of Labor, each state administers its own benefit program with a set of state-specific parameters governing eligibility, the determination of benefit amounts, and the duration of benefit payments. Unfortunately, while the state-level eligibility requirement for minimum hours worked or wages earned is publicly available, our data does not contain such information for workers prior to a job loss. Thus, we are unable to classify workers into those eligible to receive UI and those who are not. We discuss the implications of this data shortcoming on our empirical design below.

2.3. Final Sample

The focal event being studied in this paper is an individual's job separation. However, a worker may lose her job for a number of reasons which may influence her credit and UI participation decisions. For example, UI eligibility requirements generally require that a worker lose her job through no fault of her own. Moreover, it is plausible that an individual intending to quit her current job will reduce her credit utilization prior to the event of job loss. Thus, the ideal setting would consider unanticipated job losses, unrelated to a worker's actions or labor productivity.

Fortunately, the UI and job separation data lists the employer-reported reason for a job loss. We use this description field to identify separations that are plausibly unanticipated by the worker and also unrelated to labor quality. Specifically, we identify individuals that were separated from their employer either because of lack of work or firm level conditions (e.g. cash shortage). Using this approach, we identify a total of 19.6 million such "layoffs" in our data. For computational reasons, we randomly select 1 million layoffs from this data. We then confine our sample to layoffs that occur between January 2011 and December 2016 because this allows us to have at least 12 months of data before first layoff and after last layoff in the sample. This restriction leaves us with 834,741 layoffs. Finally, we restrict the sample to only job losses in CBSAs which experience the introduction of Uber at some point during our sample period. This restriction allows us to compare relatively similar CBSAs leaving slightly less than 495k layoffs.[2]

Panel A of Table 1 reports summary statistics for key outcomes of interest in the final sample. The table reports observations at a monthly level, and includes the 24-month period surrounding the month that an individual is laid-off. The panel suggests that UI benefit reception is a relatively rare event, with a monthly probability of approximately 1.66%. Conditional on receiving UI benefits, the average worker receives $1,134 per month ($18.86/0.0166). The workers also have a non-negligible average outstanding debt balance of roughly $32k, with a median of $12.8k. Finally, workers are delinquent on their debt obligations in a non-negligible portion of the sample, with a 16% likelihood of being delinquent on at least one line of credit at any given point in time.

[Insert Table 1 Near Here]

2. We conduct robustness tests to ensure that our results are valid even in the absence of this restriction.

Given the self-reported nature of the data, we now explore the representative nature of our final sample across time and geography in Figure 2. We begin by examining the time-series variation in layoffs. Panel A of Figure 2 reports a histogram of layoffs by month for the final sample. The sample demonstrates very few layoffs from January 2011 until December 2012, after which the frequency of layoffs increases by roughly three-fold. This increase is likely due to the passage of the Federal Unemployment Insurance Integrity Act, which placed the burden of UI information verification requests on employers. Beginning in January 2013, the arrival of job separations appears to be relatively uniform while also exhibiting some seasonality throughout a calendar year.

[Insert Figure 2 Near Here]

Next, we consider the spatial variation in our sample. Panel B of Figure 2 illustrates the representativeness of the final sample across geographies. The figure presents a heat map of the number of layoffs present in the final sample. Each total is first scaled by the state's 2016 job separation total as reported by the J2J Data from the U.S. Census. To aid in interpretation, we then normalize each state-level value by the median value across all states. The figure indicates relatively uniform coverage of the data, with slightly above average representation in Nevada, Arizona, Georgia, and Maryland, among others. However, it is unclear where this is due to an over-representation of participating employers from those states, or simply an above average turnover rate among employees.

Ultimately, the final sample of layoffs considered displays a considerable amount of variation, both across space and time. Our empirical strategy is designed to exploit both of these sources of variation, as we now describe.

3. Empirical Strategy

Does the rise of the gig economy reduce labor market frictions, and in turn, curtail an individual's reliance on unemployment insurance and consumer credit following job separation? We answer this question by exploiting the staggered entry of Uber across different geographic regions over time. If the entry of the ride-sharing platform into an area reduces labor market frictions, the ability of a worker to buffer her labor provision by participating on the ride-share platform may reduce the hardship associated with job loss. For a worker experiencing an unemployment spell, this may result in both a) a decrease in the propensity to claim unemployment insurance benefits and b) a reduction in the reliance on credit in order to maintain pre-separation consumption levels during the spell.

To test this set of hypotheses, we begin with a traditional difference-in-difference empirical model, which will serve as a baseline from which we will further build. Recall that our final sample consists of individuals experiencing a job loss which we classify as being out of the worker's control (a *layoff*). For each worker i, we denote the month of the job separation by S_i. For each individual, we retain the 25-month period surrounding this event, starting in month $S_i - 12$ and extending through month $S_i + 12$.[3] Thus, our sample is best described as an un-balanced panel with respect to calendar time. We start from the following estimating equation:

$$y_{ijt} = \beta \times \mathit{Layoff}_{ijt} + \lambda \times \mathit{Layoff}_{ijt} \times \mathit{Uber}_{jt} + \phi_i + \nu_{jt} + \varepsilon_{ijt}, \tag{1}$$

where y_{ijt} represents the outcome of interest (*e.g.*, credit delinquencies) for individual i, in CBSA j, in calendar month t. Layoff_{ijt} is an indicator variable that takes on a value of one for individual i for all months $t \geq S_i$. Thus, β represents the first difference, capturing the change in y associated with an individual transitioning from an employed to laid-off state. Uber_{jt} is an indicator variable that takes on a value of one for CBSA j if Uber operates in the area as of month t. λ represents the difference-in-difference estimator of the change in y for an individual post-separation (relative to pre-separation), in an area where Uber

3. Here, we opt for a slight abuse of notation for the sake of illustrative ease. In an extremely small number of cases a worker in our sample experiences two separate layoffs. For such individuals, we retain the 25-month period around both layoffs.

operates relative to an area where Uber is not yet present.

The regression includes CBSA-month fixed effects (ν_{jt}) to account for local economic conditions possibly correlated with both the outcome being considered (*e.g.*, credit delinquencies) and the timing of job layoffs. The inclusion of this fixed effect subsumes the indicator capturing the presence of Uber in an area ($Uber_{jt}$). Finally, we include an individual fixed effect, ϕ_i, to absorb individual-invariant unobservable characteristics which could correlate with the reliance on credit and delinquency rates.[4] Therefore this empirical framework relies on within-individual variation when estimating the model coefficients.

The difference-in-difference estimator of λ in Equation (1) represents the unconditional effect of Uber's entrance on the outcome following job loss. However, it is unlikely that all individuals are equally likely to benefit from the introduction of the ride-sharing platform. After all, in order to earn income by driving for Uber one must first have access to a qualifying vehicle. The easiest way to meet this criteria is for an individual to already possess a vehicle. We use this idea to motivate our preferred empirical model. Specifically, we use an individual's credit history in an attempt to identify car owners. We classify anyone with an auto loan or lease between 2000 and the month before getting laid-off as being a car owner. We posit that these individuals are most readily positioned to take advantage of Uber during job loss.

While this is perhaps the most likely means by which someone can easily transition into driving for the ride-sharing firm, it is also the case we can potentially identify in the data. However, this requirement may be met by a number of alternative means which we briefly discuss. Buchak (2018) examines one such path, documenting an increase in auto sales in a local market following Uber's entrance. However, while this up-tick in auto purchases represents an unconditional response, the individuals we study (the newly unemployed) likely face significantly larger frictions when attempting to purchase a vehicle relative to an employed individual. Even still, a non car-owner may still be able to participate on

4. For the few individuals experiencing two job separation episodes, the same individual fixed effect is used for all observations across both layoff events.

the platform by partnering with a car-owner. While this possibility exists, we are unable to observe such instances and cannot comment on the frequency with which they occur. Importantly, this measurement error reduces our chances of finding significant results.

While the results do not serve as a specific challenge to our identifying assumptions, we briefly examine variation in identified car ownership in our sample along two dimensions. Panel A of Figure 3 plots the time-series variation in car ownership for our sample. Specifically, for each month we report the proportion of individuals classified as a car owner experiencing a layoff in that month. The panel indicates a slight increase in ownership through our sample period, with ownership rates ranging from approximately 40% at the start of the sample to slightly more than 50% in the final period. Next, we turn to the spatial variation in our sample. Panel B of Figure 3 illustrates the car ownership rate across states in our sample. The panel suggests there exists a larger degree of variation across states than exists through time. In broad terms, ownership rates appear to be higher in the central region of the U.S, including Texas among others. In contrast, ownership rates in New York are among the lowest in our sample at slightly less than 30%.

[Insert Figure 3 Near Here]

With this, we extend Equation (1) to incorporate the differential effect on individuals we classify as car-owners relative to non-owners. Thus, our primary tests are structured as a "triple diff-in-diff" of the following form:

$$\begin{aligned} y_{ijt} = {} & \alpha \times Owner_{ijt} + \beta \times Layoff_{ijt} + \lambda \times Layoff_{ijt} \times Uber_{jt} + \delta \times Layoff_{ijt} \times Owner_{ijt} \\ & + \mu \times Uber_{jt} \times Owner_{ijt} + \eta \times Layoff_{ijt} \times Uber_{jt} \times Owner_{ijt} + \phi_i + \nu_{jt} + \varepsilon_{ijt}, \end{aligned} \tag{2}$$

where y_{ijt} again represents the outcome of interest for individual i, in CBSA j, in calendar month t. Relative to Equation (1), $Owner_{ijt}$ is the only variable yet to be defined. This indicator takes on a value of one if an individual is classified as a car-owner in the month prior to job separation. The coefficient associated with the product of *Layoff*, *Uber*, and

Owner represents the triple-interaction, capturing the differential effect of Uber's presence on car-owners relative to non-owners following job loss.

Our empirical approach hinges on the staggered entry of Uber across markets. It is useful to understand what drives the timing of this entry. Buchak (2018) argues that Uber's entry into a market is largely dictated by population size and prevalence of smart phones. We examine this assertion in Figure 4, which plots the relation between local population count and Uber entry. Specifically, for each CBSA we collect the population estimate from the census as of 2012, the start of our sample. We then rank CBSAs by their Uber entry date. The figure reports the logged population estimate against the entry date, averaged across "buckets" of five CBSAs.[5] Consistent with Buchak (2018), we find a strong downward-sloping relationship in which Uber enters larger markets fist, gradually expanding the ride-sharing to smaller markets over time. By the end of our sample period, Uber is present in 69.1% of all CBSAs by population count.

[Insert Figure 4 Near Here]

Uber entry can pose a potential threat to causal inference. Whereas the inclusion of CBSA-month fixed effects (ν_{jt}) mitigates concerns about omitted variables that vary at the location-time level, the remaining concern is that Uber's entry is driven by factors correlated with the benefits of car ownership during job loss. For instance, Uber may enter areas experiencing economic growth that disproportionately affects car owners. If this is the case, it may be easier for a car owner to go through the spell of unemployment than for a non-owner. In addition, the entrance of Uber might affect firms that are displaced by Uber and lead to layoffs by these firms. For instance, the introduction of Uber may lead to the closure of firms that hire car owners (*e.g.*, transportation services). Laid off employees, however, may immediately start working for Uber and therefore accumulate less debt and reduces

5. We aggregate the data across groups of five CBSAs so as not to disclose the specific entry date of a particular location without the data provider's permission.

UI consumption. Whereas this is also a causal effect of Uber entrance, it represents a very different economic mechanism.

Our empirical strategy therefore requires the following identifying assumption: the timing of Uber's entry into a market is orthogonal to omitted variables that would 1) influence the outcome of interest for a car-owner relative to a non-owner, and 2) that the difference would not be present during employment and only materialize post-separation. That is, in the absence of Uber's entry, the difference in outcome variables around layoffs for car-owners and non-owners would be the same for areas with and without Uber. Section 4.3 reports results of several tests in support of this identifying assumption.

4. Main results

Does the introduction of the gig-economy into an area ease labor market frictions, reducing one's need to offset lost wage income with other sources? In answering this question, we first present informal graphical evidence for each outcome of interest that suggests the answer is *yes*. We then examine each outcome with more depth and rigor, beginning with the effect on unemployment insurance participation. We follow this up with a focus on credit usage performance. Finally, we report results of several tests in support of our identifying assumption.

4.1. Graphical Evidence

We begin by illustrating the effect of the gig-economy on each outcome of interest in the event-time around an individual's job loss. To do this, we first construct the vector of 25 indicators variables which map to the 25 months around the month a worker is laid-off, spanning −12 to 12. Next, we partition our sample into the four mutually exclusive groups that correspond to the outer product of car ownership and Uber status. Finally, we regress an outcome of interest on the set of indicator variables interacted with the four groups, yielding a set of conditional means across event time for each group. In addition, we include

CBSA-month fixed effects to account for local economic trends.

Figure 5 graphically presents the results from this approach. For ease of illustration, we present the time-series differences between car-owners and non-owners. The blue line represents this difference prior to Uber's entry into the area, while the black line plots the outcome following Uber's entry. Panel A of Figure 5 focuses on the likelihood that an individual receives UI benefits in a given month. The difference car owners relative to non-owners jumps in the month following job-loss, indicating that car owners are more likely to receive UI benefits after being laid-off. Of more interest to us, the difference following Uber's entry into the market (black line) is smaller than the difference prior to Uber's entry (blue line) for each of the 12 months considered. Moreover, the fact that the difference in UI uptake for *Uber* remains smaller in later months suggests that the introduction of Uber does not simply delay a household's uptake of UI. Panel B repeats the previous analysis when considering the dollar amount of benefits received per month. The panel closely mimics the previous panel, confirming the relation between Uber's entry and the difference in UI usage by car owners relative to non-owners.

[Insert Figure 5 Near Here]

Next, we examine the change in household leverage outcomes. Panel C of Figure 5 reports the results where the outcome is an individual's total amount of debt outstanding. The figure depicts a widening gap between the two series immediately following job separation. Within two months of job separation, the difference between car owners' and non-owners' outstanding balance is roughly $500 less in instances where Uber is operating. Reassuringly, we find no evidence of a differential trend in the two series prior to job-loss. Finally, we consider the effect on credit performance. Panel D considers a worker's delinquency on any of her credit obligations. While both series exhibit relatively similar delinquency rates prior to job-loss, this pattern does not hold following separation. More precisely, the delinquency rate among car owners relative to non-owners increases at a faster rate prior to Uber's entry into a local area. Given its role in the recent financial crisis, in the final panel we consider the delinquency rate of home mortgages. The results resemble those of the previous panel, albeit with smaller magnitudes.

Overall, the graphical evidence in Figure 5 suggests that Uber's entry into a market reduces labor market frictions for car owners relative to non-owners. We now turn to the empirical strategy described in Section 3 to evaluation the effects of the ride-sharing platform.

4.2. Primary Results

We begin by studying the effect of the gig-economy on the propensity to turn to unemployment insurance. Table 2 presents the results of OLS regressions of the form detailed in Equation (2). Standard errors are reported in parentheses, clustered at the zipcode of the worker's residence.

We first examine the differential effect on the extensive margin of UI usage. Specifically, the dependent variable in the first specification is *Benefit Received*, an indicator variable that takes on a value of one if an individual receives UI benefits in a given month. We scale each point estimate by 100 for ease of interpretation. The key variable of interest is the triple-interaction term, which captures the differential effect of Uber's introduction on car owners relative to non-owners following job separation. The coefficient of -0.28, significant at the 1% level, indicates that likelihood of a car owner receiving UI benefits (relative to a non-owner) in a given month following job loss decreases by 28 basis points when Uber operates in the individual's CBSA. To aide in comparison, the change in the estimated likelihood of a car owner receiving benefits in a month following a layoff is 5.78% ($Layoff + Carowner$). Thus, the estimated effect associated with the triple-interaction term represents a 4.8% relative decrease in the unconditional probability of receiving benefits following layoff. Recall, our data does not allow us to perfectly identify workers meeting state UI eligibility requirements. While job loss descriptions allow us to reasonably identify a separation through no fault of the worker, we cannot ensure that the worker meet minimum wage or length of employment requirements prior to job loss. While this adds noise to the outcome of interest, it does not introduce a bias in the estimate unless eligibility systematically varies for car-owners relative to non-owners in a way that changed after Uber's entry.

Whereas only the triple-interaction coefficient has a causal interpretation, another result of note in this specification is the positive coefficient of 1.18 for $Layoff \times Carowner$, indicating a greater propensity for a car-owner to receive UI benefits following layoff. One explanation consistent with this positive relation is if there exists a cost, either in terms of time, effort, or another unspecified cost, associated with filing for and receiving UI benefits. In the presence of such a cost, individuals would only file for UI if the benefits exceeded the cost. It is plausible that individuals who own a car and likely face a higher average debt burden stand to gain more from filing for UI, thus resulting in a higher average take-up rate.

[Insert Table 2 Near Here]

To estimate the possible reduction in government expenditures, in the second column we instead consider the effect of Uber on the monthly dollar amount of UI disbursements. The coefficient of -2.16, statistically significant at the 5% level, indicates that a car-owning individual receives \$2.16 less per month following Uber's introduction into an area relative to a non-owner. It is import to note that this corresponds to the unconditional effect across all individuals, regardless of if they apply for UI benefits, rather than the intensive margin of UI usage. To add economic perspective, the estimated change in the dollar amount of UI benefits received per month for a car owner following job loss is \$73.15 (\$51.08 + \$22.07).[6] Therefore, the coefficient on the triple-interaction represents a 2.96% relative decrease in UI benefits. The Department of Labor projects total benefits paid in 2018 to be \$28.8B.[7] In a back-of-the-envelop calculation, assuming a car ownership rate of 88% (Poushter (2015)), a 2.96% reduction in total UI benefits paid out across all car owners equates to an approximate savings of \$750M per year ($\$28.8B \times 0.88 \times 0.0296$).

6. While this value may seem low, it is important to note that it does not condition on applying for UI benefits. Thus, it also incorporates workers that receive zero dollars in UI benefits, which make up the majority of our sample.

7. https://goo.gl/hghbCB

While Table 2 focuses on an individual's decision to apply for UI benefits, this is not the only means by which a household may smooth consumption during unemployment spells. Alternatively, an individual may lean on existing or new credit lines to mitigate the effects of a wage shock suffered from job loss. While the ability of an individual to increase her household leverage allows her to smooth consumption during downtimes, this option is not costless. The limited-liability nature of consumer credit may lead to increased dead-weight costs due to moral hazard. Bernstein (2016) documents a reduction in labor supplied by households experiencing debt overhang. In addition, Mian, Rao, and Sufi (2013) show that areas with larger increases in household leverage prior to the financial crisis also experienced slower rates of recovery in subsequent years.

In contrast, it may be possible to avoid such costs in a counter-factual where labor market frictions are reduced through the introduction of the gig-economy. We explore this possibility by studying the credit response of individuals following a layoff. We begin with Panel A of Table 3, which estimates OLS regressions of the form laid out in Equation (2) where the outcome is the number of open accounts for an individual. In the first specification, we study the broad effect of Uber's entry across all types of credit. The coefficient on the triple-interaction term indicates that the change in all open account for car owners relative to non-owners following layoff decreases by 0.13 accounts once Uber enters a market. To add economic content to this estimate, the average change in open accounts following layoff across all individuals is 0.23. Thus, the estimated effect represents 56.52% of the unconditional average change following layoff. We find a similar effect when we consider individual credit types. The panel documents a reduction in open lines of credit for car owners relative to non-owners once Uber enters a market, with the strongest effect coming from the number of credit card accounts.

[Insert Table 3 Near Here]

While the results in the first panel suggest an effect on the number of open credit lines, this is not evidence in and of itself that individuals reduce their reliance on credit during layoffs when the option to participate on the ride-sharing platform is available. To this end, Panel B of Table 3 turns to the effects of Uber's introduction on outstanding credit balances. In the first specification, we consider the effect on all credit types. The coefficient on the triple-interaction term indicates that the difference in the post-layoff change in credit balances for car owners relative to non-owners decreases by roughly \$544 following Uber's entry into a local market. The median individual in our sample has a total outstanding balance of \$12,833 (the distribution is right skewed, with a mean of approximately \$42k). Thus, the point estimate on our variable of interest represents a 4.2% decrease relative to the median worker in our sample.[8] Interestingly, when we consider each type of credit separately, we find no statistical difference in the balance on credit cards. Instead, the final specification indicates the effect is predominately driven by a relative decrease in the balance of home loans (\$325). As a whole, the results presented in Table 3 suggest that a car owner is less likely to tap into her credit reserves following job-loss when Uber operates in her area. At a superficial level, this is beneficial from the standpoint of a household, helping a newly laid-off worker to avoid a generally expensive means of smoothing consumption. More importantly, this decreased reliance on household leverage may also have implication for the propagation of shocks through the local economy. Specifically, the decreased use of consumer credit may result in a reduction in economic fragility (Mian and Sufi (2011), Mian and Sufi (2014)) or attenuate the disincentive to work caused by debt overhang (Bernstein (2016)). We now seek evidence of a more direct channel through which Uber's entry may effect local economic conditions: delinquency rates.

Table 4 estimates OLS regressions where the outcome is a credit delinquency. In the first specification, we examine the change in the probability of being delinquent on any lines of credit. The coefficient on the triple-interaction term suggests that Uber's entry reduces the change in delinquency rates for car owners by 0.49 percentage points, or 2.9% of the mean delinquency rate following job loss (16.8%). Moving to credit card performance in

8. Consistent with Bethune (2015), in a un-tabulated test we find that the average worker in our sample experiences a decrease in her outstanding balance following a layoff.

the second specification, we find a similar sized effect in absolute terms of -0.56 percentage points. However, compared to the lower unconditional likelihood of being delinquent on a credit card following job loss (6.61%), this effect constitutes a relative decrease of 8.47%. In the final specification, we turn to the effect on mortgage delinquency rates. We find a reduction of 10 basis points in delinquencies among car owners relative to non-owners following Uber's entry. Relative to the unconditional probability of delinquency of 1.16% following job loss, this represents a relative decrease of 8.6%.

[Insert Table 4 Near Here]

4.3. Addressing Challenges to Identification Strategy

The results presented in the Section 4.2 are consistent with the gig-economy having a significant effect on household behavior following job loss. At the same time, it is plausible that an alternative force is instead at play. To this end, we discuss two such alternatives and present a set of empirical tests specifically designed to distinguish between the possible explanations.

Perhaps the most plausible concern is the endogenous entry of Uber into an area experiencing economic growth that disproportionately benefits car owners. Specifically, the timing of Uber's entry into markets may coincide with the realization of a pre-existing, time-varying factor that differentially affects a car owner's re-employment prospects relative to a non-owner. This possibility, if true, serves as a significant challenge to our identification strategy. Yet, if true, one would expect an increase in the rate of vehicle purchases in the months leading up to Uber's entry into the market. Buchak (2018) finds no evidence of a pre-trend in which car sales increase prior to Uber's entry. Nevertheless, we now consider additional tests to evaluate this possible explanation for our results.

If this alternative is at play, one would expect that the difference in outcomes between car owners and non-owners following a layoff would manifest prior to Uber's entry into the market. To examine this possible pre-existing effect, we make a slight modification to

Equation (2). First, we define the month that Uber first entered CBSA j by U_j. Following this, we remove the indicator variable $Uber_{jt}$, denoting Uber's presence in CBSA j as of month t. In its place, we include a vector of 25 indicator variables that correspond to the event time around Uber's entry, ranging from $U_j - 12$ to $U_j + 12$. We assign any month occurring prior to $U_j - 12$, and any month subsequent to $U_j + 12$, to their respective "book-end" event time fixed effects. Thus, this vector of dummies allows us to estimate a separate coefficient of *Layoff* $\times$ *Ownership* for each "event time" around Uber's entry into a CBSA.

[Insert Figure 6 Near Here]

Panel A of Figure 6 illustrates the estimation results when the outcome is $BenefitReceived$, an indicator that takes on a value of one if an individual receives any benefits in a given month. The figure reports the estimated coefficient of *Layoff* $\times$ *Ownership* interacted with each of the event time indicator variables, with corresponding 95% confidence bands. The estimation results do not seem to support the alternative hypothesis of a pre-existing trend prior to Uber's introduction into a market. In contrast, the point estimate on *Layoff* $\times$ *Ownership* falls sharply in the month that Uber first enters a market, consistent with the findings presented in Table 2. Panel B presents similar results, both regarding a lack of pre-trends and a decrease following entry by Uber, when examining the difference in the average dollar amount of UI benefits received by car owners relative to non-owners. Finally, we consider the possibility of pre-existing trends when examining the effect on credit balances (Panel C), overall delinquency rates (Panel D), and mortgage delinquencies (Panel E). Neither panel reveals any noticeable signs of a pre-existing trend in the months leading up to Uber's entry into a market.

Figure 6 suggests that Uber's entry is not coinciding with another unobservable factor that is driving our results. Yet, this raises a related concern which is interesting in its own right. Suppose Uber strategically enters markets that are more likely to experience future economic growth. In this case, it is possible that the potential benefits of the gig-economy may not extend to an area experiencing a local economic downturn. Understanding the validity of this concern is especially important when considering our results' implications for the propagation and amplification of shocks resulting from the negative externalities imposed by a household's financial distress (Maturana and Nickerson (2019)).

To examine this idea, we repeat our primary analysis on areas we identify as experiencing a local economic downturn. Specifically, using data from the Bureau of Labor Statistics, we first compute the 6-month nominal change in the unemployment rate for each CBSA-month in our sample. We then re-estimate our primary analysis on the 10% of CBSA-months experiencing the largest 6-month rise in unemployment rates. Table 5 report the effects of Uber's entry on car-owners in this subsample for each outcome of interest.

[Insert Table 5 Near Here]

We begin by studying the change in unemployment insurance participation following a layoff. The first specification indicates that following Uber's entry, there is a 1.54pp reduction in a car-owner's monthly likelihood of receiving UI benefits relative to non-owners. The effect is economically significant, representing a 18.5% decrease relative to that of a car-owner following job loss when Uber isn't present ($Layoff \times Carowner + Layoff$). For comparison, the point estimate in the analogous specification estimated on the full sample (reported in Table 2) suggest a 28bp decrease in the monthly likelihood. Similar inferences can be drawn from the second specification, where the outcome is the dollar amount of benefits received.

We find similar results when turning to the effect on credit usage in the third and fourth specifications. For instance, the results indicate a relative decrease of $880 in total debt outstanding for car-owners following Uber's entry (the estimated effect in the full sample is a $543 decrease). Finally, we consider the effect on credit delinquencies in the final two specifications. The fifth specifications shows a significant decrease in total delinquencies of nearly 1pp, while we do not find a statistically significant effect on home delinquencies in the final specification. In sum, these findings suggest that the gig-economy provides a more attractive alternative to unemployment insurance and credit consumption when local economic conditions are declining.

Recall that the timing of Uber's entry in an area is strongly correlated with the area's population. Therefore, *Uber* (and thus the triple-interaction term) will take on a value of one disproportionately more often in larger CBSAs. A plausible concern left un-addressed by the previous analysis is that car owners in large cities face a relatively easier time in regaining employment when compared to smaller cities. If true, this alternative might explain the primary results of Section 4.2. To address this concern, Appendix Table OA.1 repeats our primary analysis when including a vector of fixed effects that vary at the CBSA-Layoff-Ownership level. Their inclusion accounts for any time-invariant differences across CBSAs in the behavior of car owners relative to non-owners following job loss. The test results yield similar inferences to the previous analysis, helping to alleviate the concern that the previous finding is the result of time-invariant differences across CBSAs which coincide with the order of Uber's entry.

Finally, we consider the alternative that car-owning individuals are inherently different from non-owners, and that this difference manifests itself once Uber enters a local market. To examine this alternative, we consider two sets of closely related individuals who both own cars: those financing their car thorough a loan and those with a car lease. Intuitively, while both groups have access to an automobile, those who lease likely face additional constraints imposed by their terms of usage agreement that prevent excessive use of their vehicle. Such restrictions typically limit the number of miles allowed per year. These constraints are likely to impair a lessee's ability to operate on a ride-sharing platform in any significant capacity, providing a nice placebo group.

[Insert Table 6 Near Here]

Table 6 presents the results when bisecting the set of car-owners based on the choice of loan versus lease. Specifically, we assign all individuals from the *car-owner* group who do not have a car lease into the *Without Auto Lease* sub-group. All remaining individuals from the *car-owner* group are assigned to the *With Auto Lease* sub-group. In Panel A, we find a statistically significant effect of Uber's entry on UI participation rates for individuals in the

Without Auto Lease sub-group. In contrast, when considering workers belonging to the *With Auto Lease* sub-group, the effect of Uber's entry is statistically indistinguishable from zero at conventional levels. Panel B continues by examining the effects on credit usage. Consistent with the previous panel, we find stronger effects of Uber's entry among workers that fall into the *Without Auto Lease* sub-group. Finally, Panel C presents similar results when focusing on credit performance. Taken together, these results suggest that the potential benefits of Uber's entry are concentrated among individuals with a car loan, precisely those posed to best take advantage of the ride-sharing platform.

Overall, the results support our identifying assumptions and imply that Uber's entry has a causal effect on laid-off employees.

5. Economic Mechanism

The results presented in Section 4 support the conjecture that workers use Uber as a substitute for unemployment insurance and an increased reliance on credit following job loss. Specifically, those individuals most readily able to participate on the ride-sharing platform receive fewer UI benefits, do not draw down on untapped credit lines as much as non-owners, and are less likely to experience a delinquency following Uber's entry into their area.

Yet, the previous tests are silent on the economic mechanism driving the results observed. While they don't make up the entire set of possibilities, two channels stand out as chief candidates to explain our results. First, workers may view Uber as a short-term alternative for a recently lost job. In this case, Uber increases the availability of easily accessible short-term jobs thereby increasing the *liquidity* in labor markets. Importantly, this mechanism represents a structural shift in labor markets likely to benefit laid-off workers for an extended period of time following Uber's entry. Accordingly, this channel suggests that the effects described in Section 4 should persist over time.

Alternatively, Uber may simply represent the entry of another large employer, yielding labor market effects similar to that of a generic firm. That is, Uber's entry could create a one-time increase in the demand for labor. In this case, the beneficial effects of Uber's entry documented in Section 4 will likely be transient in nature, subsiding after the initial job openings are filled.

In this section, we present evidence that sheds light on the channel through which Uber's entry affects labor markets. We begin by examining the persistence of the effect documented in Section 4. If Uber affects labor markets like a typical firm, it's entry may simply provide new job vacancies that recently unemployed workers are able to fill. In this case, it is plausible that the effects would subside over time as the initial vacancies are filled. In contrast, if Uber alters labor market dynamics, the reduction in search frictions should persist over time.

To this end, we repeat the previous analysis when excluding the initial period following Uber's entry into a local economy. Specifically, Table 7 performs OLS regressions similar to those of Tables 2, 3, and 4 when excluding the first 24 months following Uber's entry. Thus, *Uber* now captures the difference between an outcome prior to Uber entering and 24 months following Uber's entry into a market. We obtain very similar estimates, suggesting that the first 24 months following Uber's entry do not drive our results, with the effects instead being persistent in nature.

[Insert Table 7 Near Here]

Related to the previous test, Uber's entry may simply represent an additional employer, whereby the firm's entry may lead to a new equilibrium with a lower average level of unemployment. We explicitly consider this possibility in an alternate test. To do this, we augment Equation (2) with an additional variable intended to control for local unemployment rates. Specifically, we include *unemployment* which is the unemployment rate for the CBSA, reported at a monthly frequency by the Bureau of Labor Statistics. We allow the effect of unemployment to vary for drivers and non-drivers, and to vary pre- and post-layoff. In untabulated results, the results remain virtually unchanged from those presented in Section 4 following the inclusion of this control. However, arguably the inclusion of this additional variable may constitute a “bad control” which is influenced by the treatment (Uber’s entry). To this end, we do not lean heavily on this result, and instead seek additional support regarding the economic mechanism at work by examining the cross-section of the effect.

Next, we argue that recently laid-off workers are less likely to treat the ride-sharing platform as a permanent re-employment prospect than they are to view it as a temporary solution to be used while pursuing superior long-term prospects in the traditional labor market. We begin by discussing a historical labor market episode that supports our argument. Recently, the gig-economy has garnered the attention of both the media and lawmakers in the wake of governmental shutdowns. For instance, the household errand platform *TaskRabbit* observed a spike in participation coinciding with the October 2013 shutdown, resulting in more than 13k applications in one day (Little (2013)). Even more recently, law makers and union representatives expressed concern that critical federal employees such as air traffic controllers were taking up a second job as a driver on ride-sharing platforms to buffer the income shock due to the January 2019 shutdown (Halsey and Aratani (2019)). These accounts are consistent with the gig-economy providing easily available short-term employment following an unexpected income shock. Importantly, traffic controllers and government employees are arguably not likely to view the gig-economy as a permanent employment prospect. These accounts support our conjecture that the gig-economy changes the structure of labor markets rather than merely producing new jobs.

To examine this idea in more detail, we focus on a subset of the population that is less likely to view the ride-sharing platform as a long-term solution to unemployment. Specifically, using the yearly income statistics for each ZIP code from the SOI division of the IRS, we restrict the sample to individuals residing in above-median income ZIP codes. In our sample, this corresponds to individuals with a reported household annual income of approximately $75k.

[Insert Table 8 Near Here]

Table 8 repeats the analysis from Section 4 on this above-median income subsample. Our findings indicate the effects of Uber's entry remain economically and statistically significant, with the exception for home delinquency rate. For instance, the coefficient in the total debt regression is $544 in the main specification and $794 in this above-median income subsample. Overall, these results are consistent with a reduction in labor market frictions for a group of individuals not likely to view Uber as a feasible long-term alternative to traditional employment.

The distinction between the ride-sharing platform as a short- and long-term employment prospect also offers up another testable prediction. If the recently unemployed view Uber as a short-term means of buffering an income shock, this implies that a car-owner is willing to offer up labor in exchange for wages following a job separation rather than turning to other means (e.g., claiming UI). Thus, with this channel we would expect a laid-off worker to weigh the option to participate on the ride-sharing platform against the alternative of receiving UI benefits. In contrast, if the recently laid-off view Uber as a long-term job, variation in temporary benefits provided by UI should not affect a worker's decision to participate on the platform.

We use this trade-off to motivate the final test we consider. Specifically, we exploit differences in the maximum amount of weekly UI benefits paid out across states and their duration as a source of variation in this trade-off. If laid-off workers are evaluating the decision to drive for Uber against supplementary income provided by UI, a worker is likely to choose Uber more often when the expected UI benefits are lower. This would imply that the effects we document above should be stronger when UI benefits are less generous if workers view Uber as a short-term solution.

Table 9 formally examines this idea by considering heterogeneous effects on "sample-splits." For each year, we collect the maximum amount of UI benefits paid across states and their duration. Overall, the sample exhibits a considerable amount of state-level variation in benefit caps. For instance, as of January 2014, the maximum amount of weekly UI benefits provided in Massachusetts was $679, compared to $235 offered in Mississippi. While most states offer UI benefits for 26 weeks, in January 2014 the duration of benefits ranged from 16 weeks (Florida) to 30 weeks (Massachusetts). We then partition the sample based on the median value of the product of these two variables across states for each year. Panel A of the table focuses on UI participation rates. When examining the receipt of any UI benefits (columns 1 and 3), the point estimates suggest nearly a fourfold increase of the effect when transitioning from a larger expect benefit to a smaller expected benefit. When considering the effect on the dollar amount of UI benefits (columns 2 and 4), the difference is also pronounced. Moreover, while the point estimates in the triple-interactions are statistically significant in states with below-median maximum UI benefits, they are not in states with more generous UI limits.

[Insert Table 9 Near Here]

In Panel B of Table 9, we turn to the effects on credit usage. While we generally find point estimates that suggest a larger effect of Uber's introduction in states where expected UI benefits are lower, the differences are less stark than the previous panel. Finally, Panel C of Table 9 reveals that when we examine the effects on credit delinquencies, the point estimates imply a greater effect of Uber in states with low expected benefits. Again, we only find statistically significant effects of Uber's entry on car-owners in the subsample of states with a smaller expected benefit.

Overall, our findings are consistent with gig-labor changing the structure of labor markets by making them more liquid rather than generating a transitory supply of jobs.

6. Conclusion

This paper highlights the dramatic effect played by the gig economy in reshaping the landscape of labor markets and worker response to job separations. Using the staggered introduction of Uber across geographic regions, we find that car-owning workers are 4.8% less likely to lean on UI programs following job loss following the ride-sharing firm's entry.

Moreover, the introduction of Uber has a significant effect on household leverage outcomes. Car-owning workers increase their outstanding debt balances by 1.3% less than they otherwise would following Uber's entry, while relative delinquency rates fall by 2.9%. In support of our identify assumption, we do not find evidence of a pre-trend in which car-owners outcomes deviate from non-owners in the months leading up to Uber's entry into an area. Moreover, results are severely attenuated when restricting the car-owner set to lessees, for whom mileage limits likely impair the ability of participate on the ride-sharing platform in a meaningful way.

Anecdotal evidence from recent government shutdowns suggest many income-shocked workers view the gig-economy as a short-term solution to buffer consumption. We find additional support for this economic channel from a series of empirical tests. Our results hold when restricting the sample to laid-off workers from high-income areas, a group less likely to view Uber as a long-term job prospect. Moreover, evidence suggests that the effects of Uber are stronger in states with less generous UI benefits. This is consistent with workers weighing the trade-off between two short-term options, the gig-economy and UI.

Finally, results suggest that the effects are even more pronounced in areas experiencing local economic downturns. This last finding has implications for the propagation of shocks through local economies, and is material to the policy debate regarding potential economic stabilizers.

Taken together, our results demonstrate the substantial role the gig-economy plays in reducing labor market frictions, and the ensuing effect on a worker's response to job loss.

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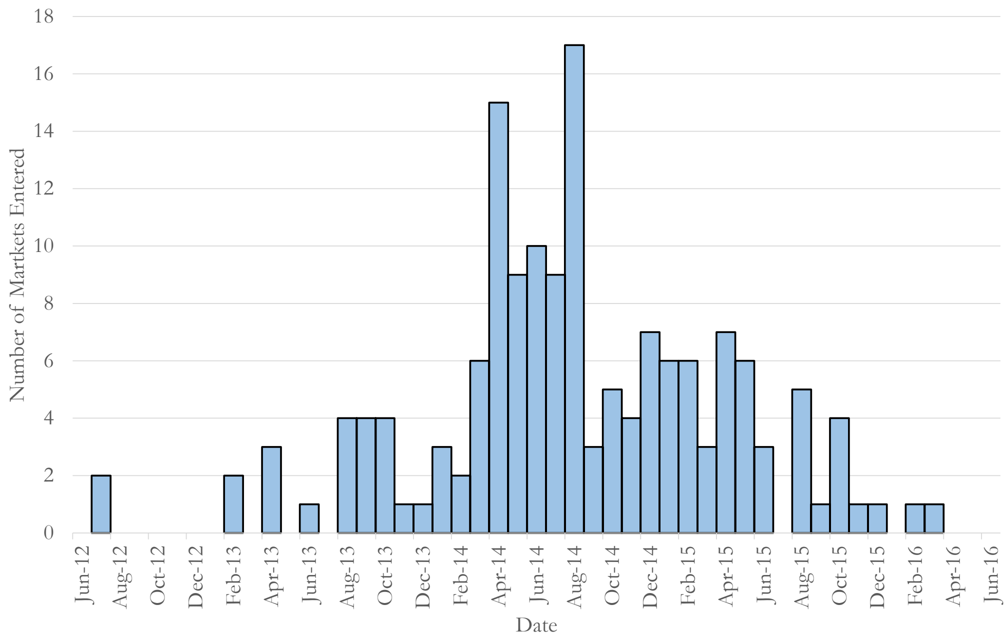
Panel A: Uber Entry through Time



Panel B: Uber Entry across States

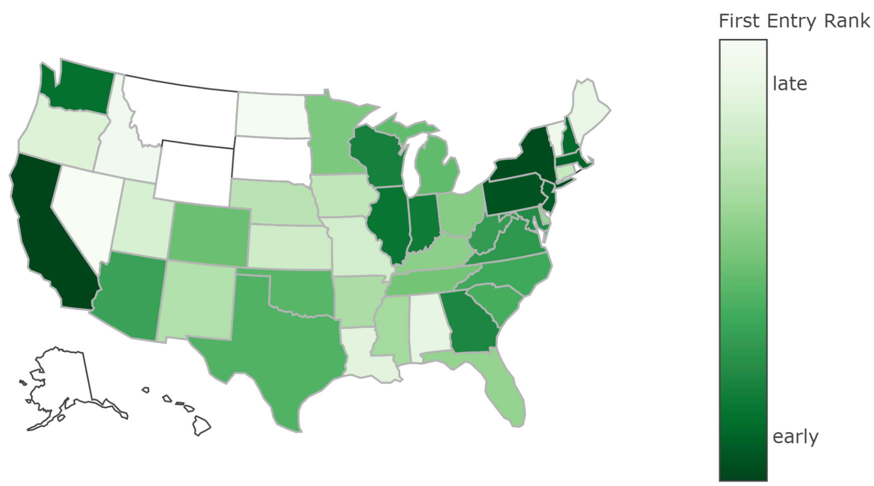
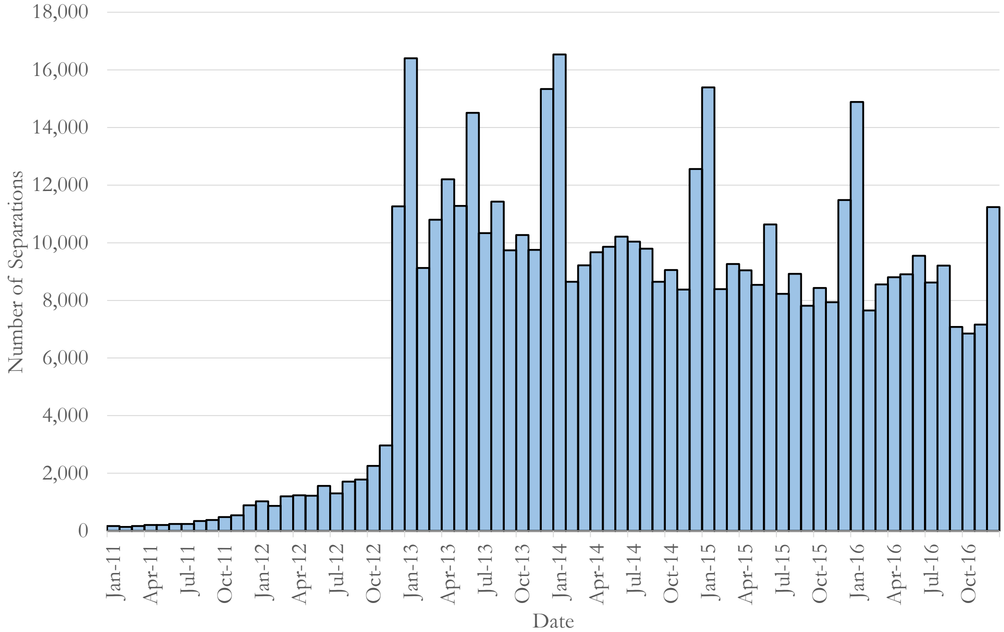


Fig. 1. Timing of Uber Entry
This figure illustrates the variation in Uber's entry into markets. Panel A reports the number of markets entered through time. Panel B presents a choropleth (geographic heat map) of the order of Uber's first entry into a state.

Panel A: Layoffs through Time



Panel B: Layoffs across States

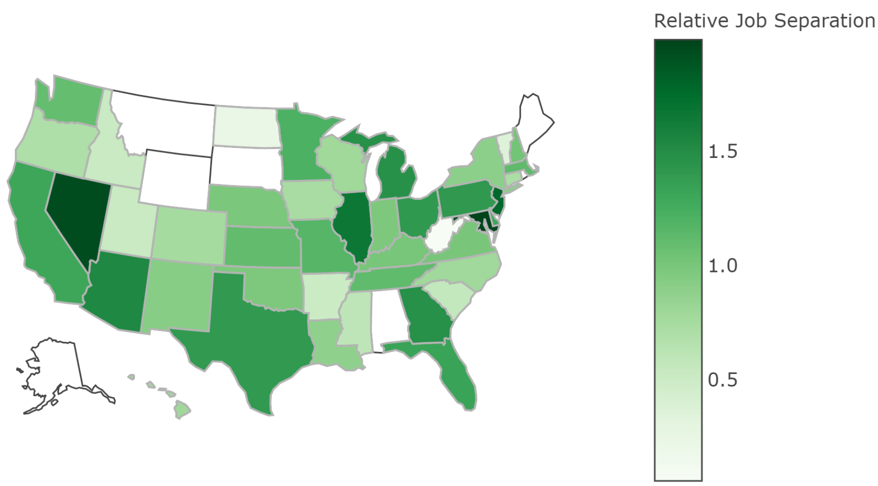
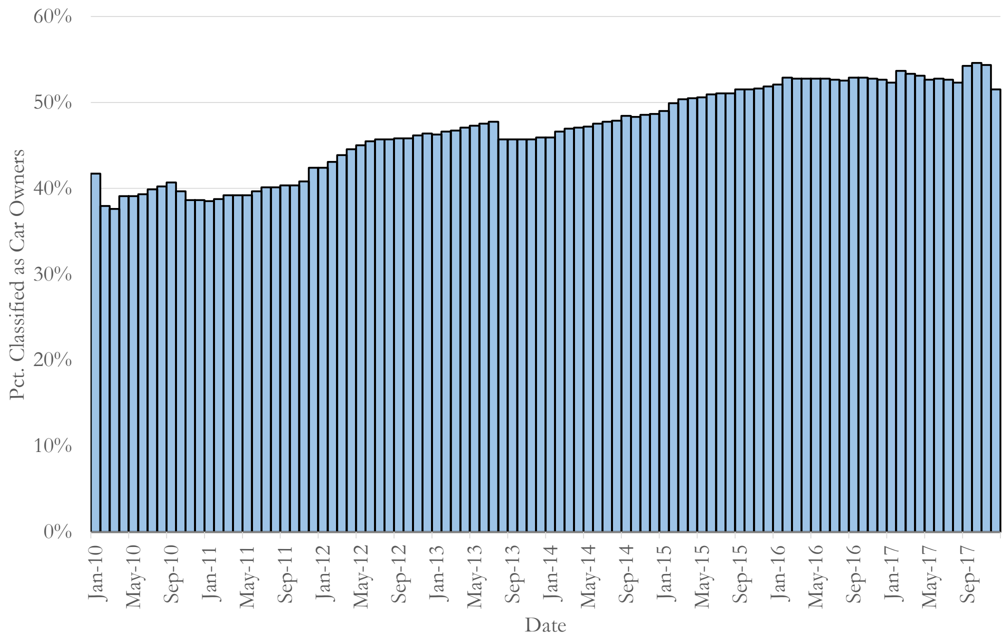


Fig. 2. Variation in Layoff Sample
This figure illustrates the variation in our sample of worker layoffs. Panel A reports the number of layoffs through time. Panel B presents a choropleth (geographic heat map) of the state-level representation of our sample. Each state-level total is first scaled by the number of 2018 job separations reported in the J2J Data from the Department of Labor. We then normalize the values so the median value across all states is one.

Panel A: Car Ownership through Time



Panel B: Car Ownership across States

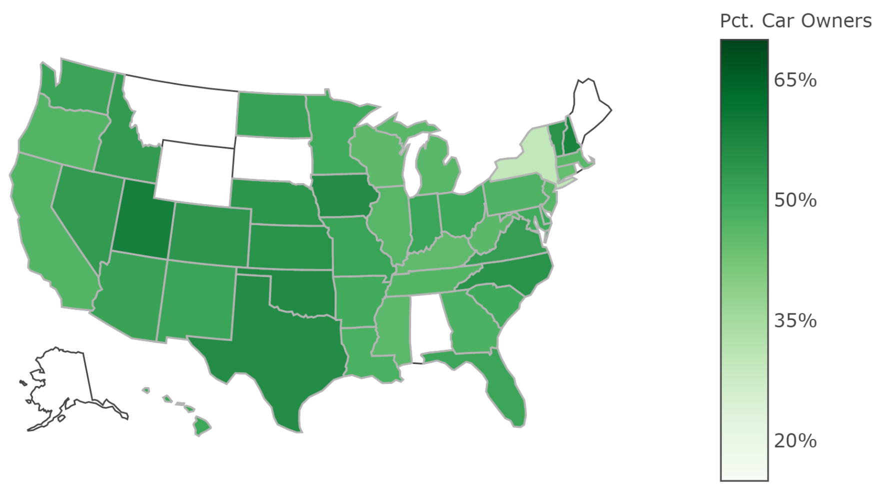


Fig. 3. Car Ownership Rates
This figure illustrates the variation in our sample of workers we classify as being car owners. Panel A reports the percentage of car owners through time. Panel B presents a choropleth (geographic heat map) of the state-level car ownership rate in our sample.

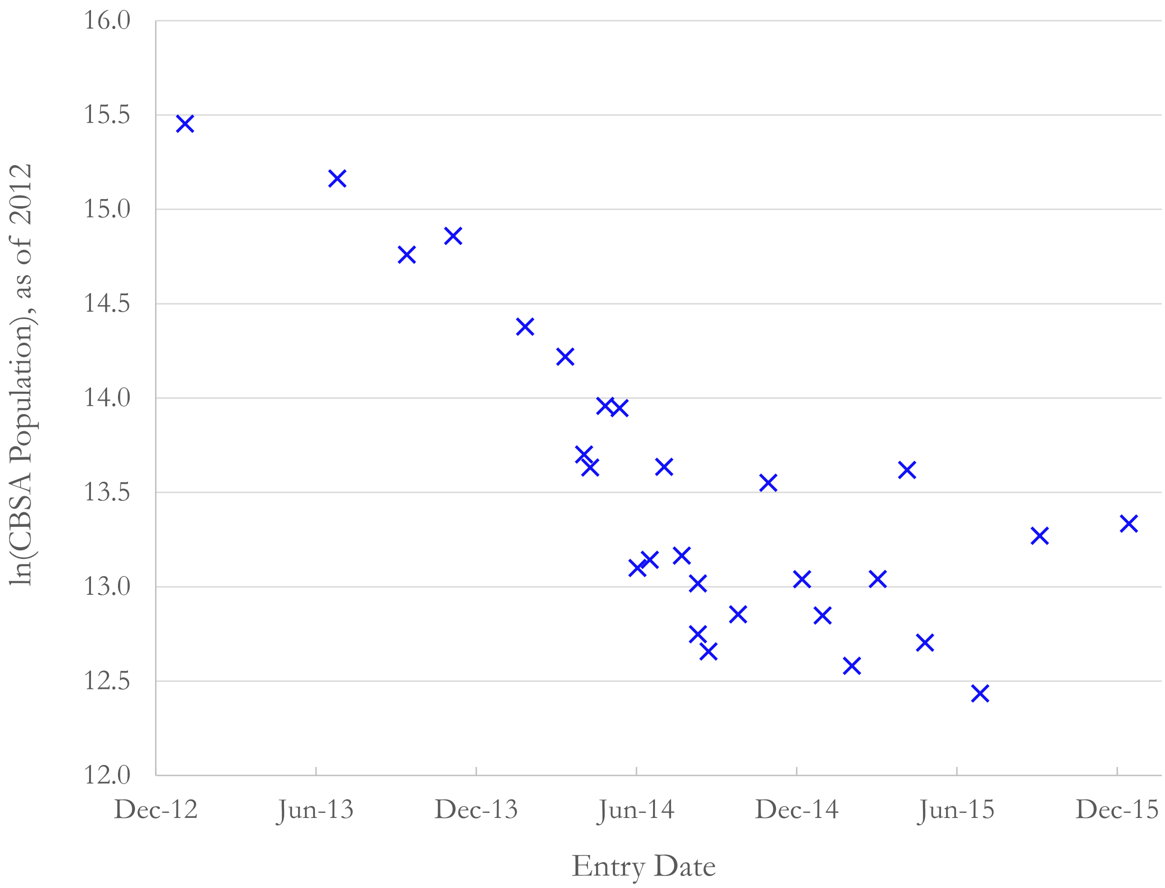


Fig. 4. Uber Entry Dates and Area Population

This figure shows the relation between the order of Uber's entry into markets and market size. Population size is taken from the 2012 census. Reported is the average of the natural log of population size across buckets of five CBSAs.

Panel A: Reception of UI Benefit

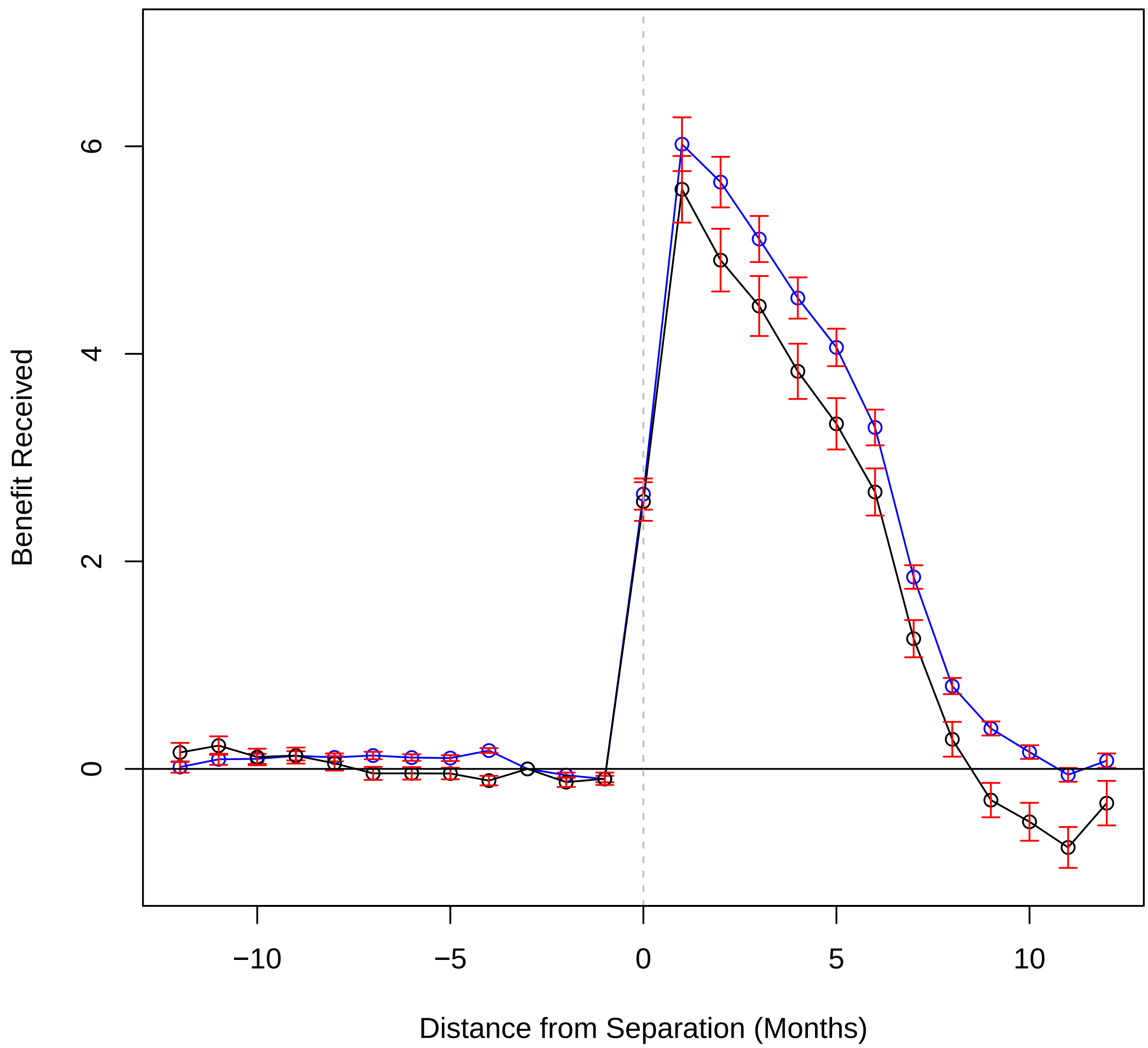
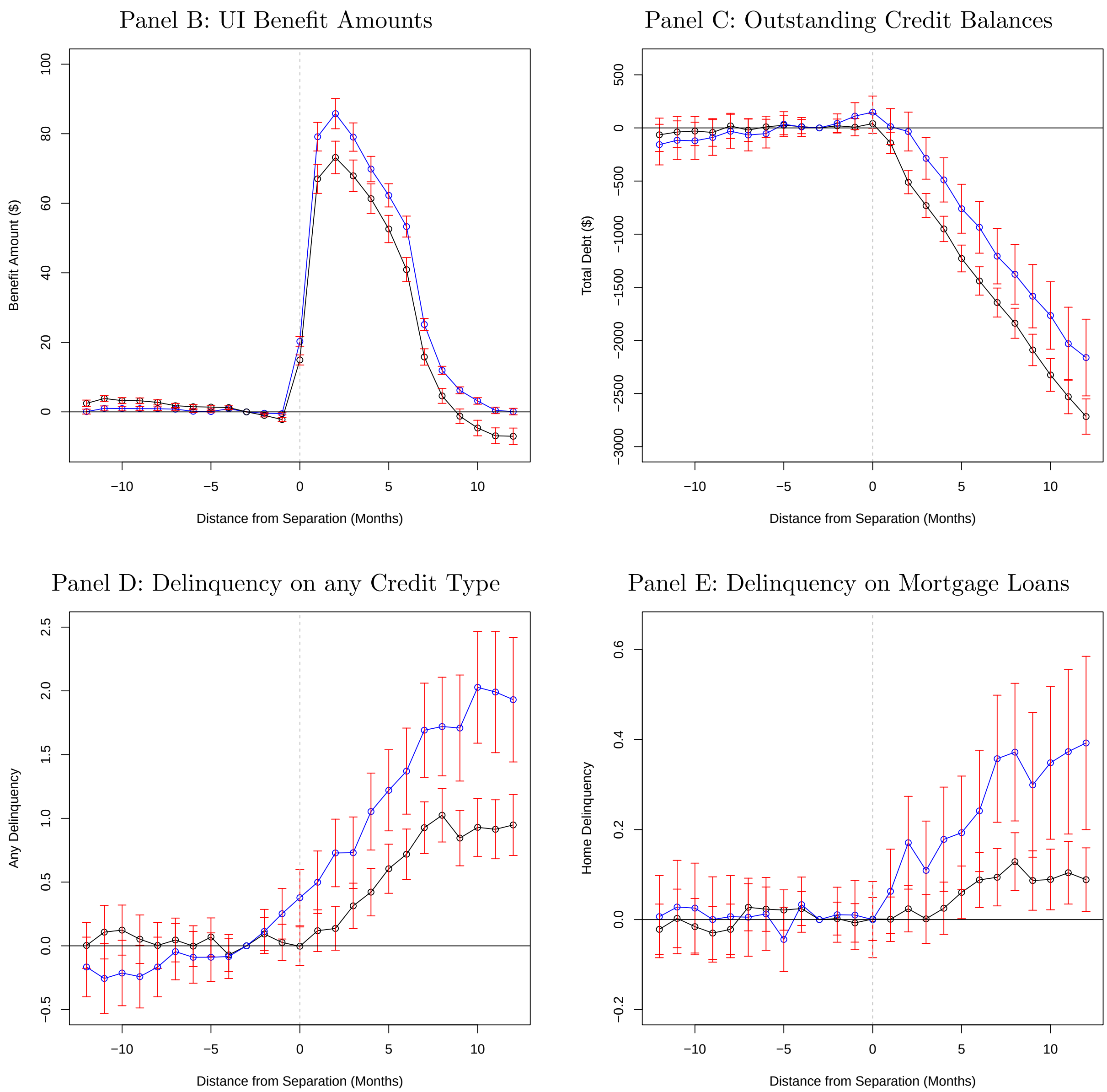


Fig. 5. Changes in Outcomes around Job Separations
This figure reports the difference in outcome variables for car owners relative to non-owners around job separation. Reported is the difference when Uber is present in a market (black line), and when Uber has yet to enter the market (blue line). The figure also reports 95% confidence bands and includes CBSA-month fixed effects. Outcomes include the monthly probability of receiving UI benefits (Panel A), dollar amount of benefits received (Panel B), outstanding credit balance (Panel C), overall credit delinquency rate (Panel D), and mortgage credit delinquency rate (Panel E).



Panel B: UI Benefit Amounts
Benefit Amount ($)
Distance from Separation (Months)
Panel C: Outstanding Credit Balances
Total Debt ($)
Distance from Separation (Months)
Panel D: Delinquency on any Credit Type
Any Delinquency
Distance from Separation (Months)
Panel E: Delinquency on Mortgage Loans
Home Delinquency
Distance from Separation (Months)

Panel A: Reception of UI Benefit

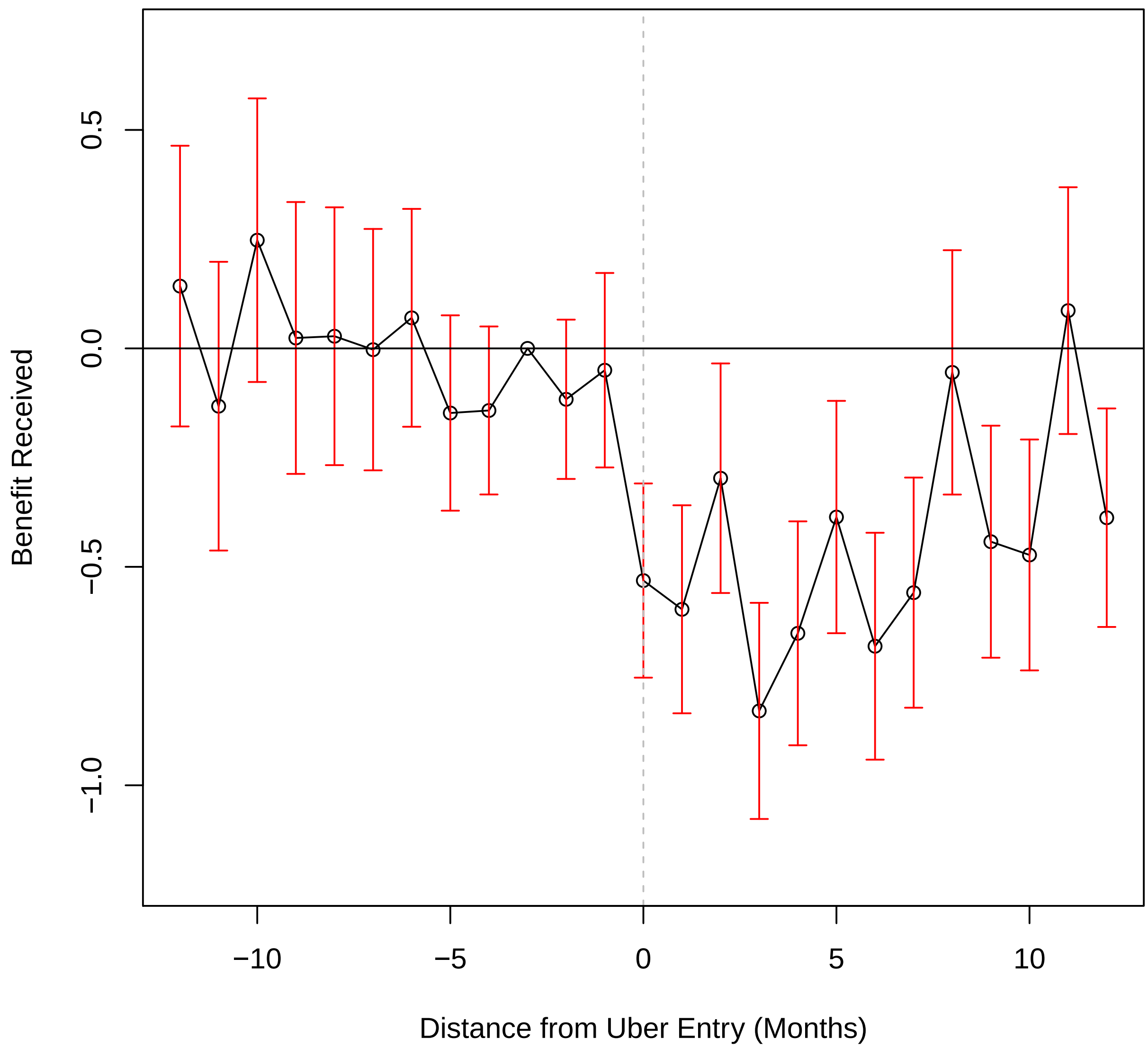
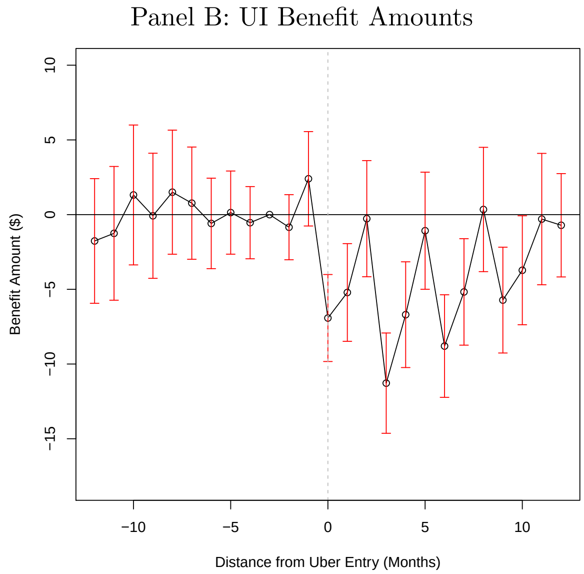
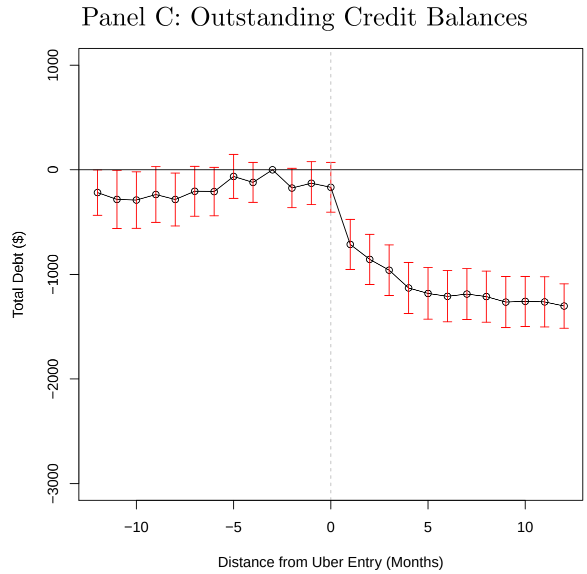
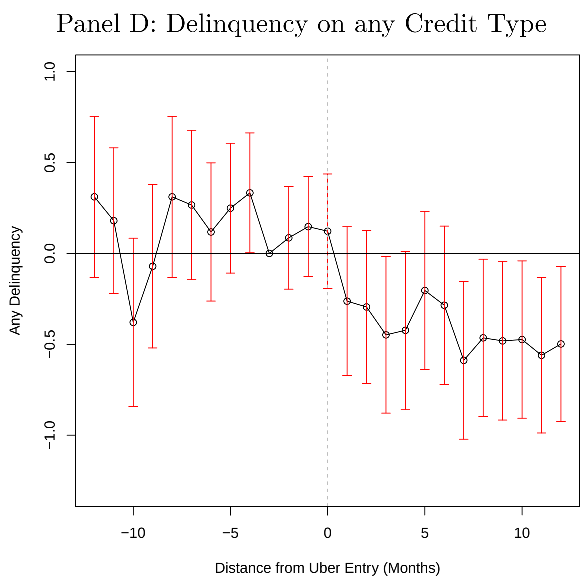


Fig. 6. Changes in Outcomes around Uber's entry
This figure reports the estimated effect of *Layoff* × *Ownership* on outcome variables in the months around Uber's entry into a market. The figure also reports 95% confidence bands. Outcomes include the monthly probability of receiving UI benefits (Panel A), dollar amount of benefits received (Panel B), outstanding credit balance (Panel C), overall credit delinquency rate (Panel D), and mortgage credit delinquency rate (Panel E).


Panel B: UI Benefit Amounts
Benefit Amount ($)
Distance from Uber Entry (Months)


Panel C: Outstanding Credit Balances
Total Debt ($)
Distance from Uber Entry (Months)


Panel D: Delinquency on any Credit Type
Any Delinquency
Distance from Uber Entry (Months)

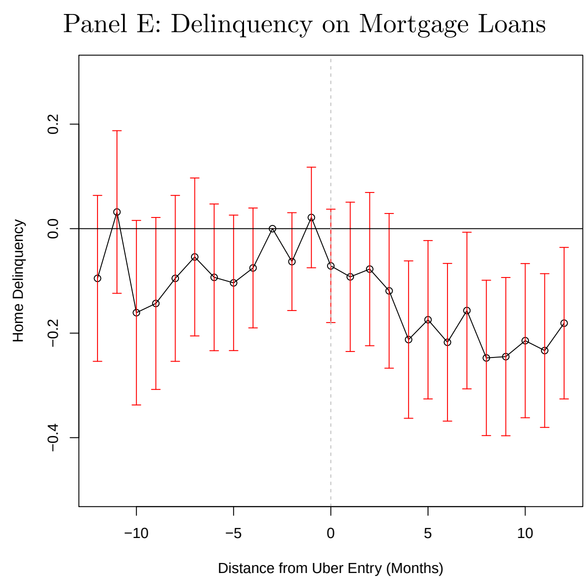

Panel E: Delinquency on Mortgage Loans
Home Delinquency
Distance from Uber Entry (Months)

Table 1. Summary Statistics
This table reports summary statistics for our main sample.

Statistic	N	Mean	St. Dev.	Min	Median	Max
Benefit Received (%)	12,277,167	1.663	12.788	0	0	100
Benefit Amount ($)	12,277,167	18.9	181.5	0	0	2,844
Total Debt ($)	12,277,167	31,991	45,596	0	12,833	202,996
Home Loans ($)	12,277,167	11,653	34,116	0	0	161834
Credit Score	12,277,167	621	113	300	613	839
CC Utilization	7,157,375	0.324	0.333	0	0.19	1
Any Delinquency (%)	12,277,167	16.33	36.964	0	0	100
Credit Card Delinquency (%)	12,277,167	6.341	24.37	0	0	100
Auto Loans Delinquency (%)	12,277,167	3.799	19.117	0	0	100
Home Loans Delinquency (%)	12,277,167	1.048	10.185	0	0	100

Table 2. Effects on UI Participation
This table reports the results of OLS regressions of the form described in Equation (2). $1(ReceivedBenefit)$ is an indicator variable which takes on a value of one if an individual receives a non-zero amount of UI benefits in a given month, while $BenefitAmount$ is the dollar amount of benefits received. *Layoff* is an indicator variable taking on a value of one in the months following job separation. $Carowner$ is an indicator variable which takes on a value of one if an individual has a car loan the month prior to job separation. Finally, $Uber$ is a dummy variable capturing Uber's presence in a CBSA at a given point in time. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by zipcode of the worker's residency. ***p<0.01, **p<0.05, *p<0.1.

	1(Received Benefit)	Benefit Amount
Layoff × Carowner × Uber	-0.28***	-2.16**
	(0.08)	(1.01)
Layoff × Carowner	1.18***	22.07***
	(0.08)	(1.08)
Layoff × Uber	0.02	6.84***
	(0.09)	(1.04)
Carowner × Uber	-0.14***	-4.85***
	(0.05)	(0.74)
Layoff	4.60***	51.08***
	(0.11)	(1.22)
Individual FE	Yes	Yes
City-Month FE	Yes	Yes
Obs.	12,277,167	12,277,167
Adj. R^2	0.24	0.18

Table 3. Effects on Credit Outcomes
This table reports the results of OLS regressions of the form described in Equation (2). Panel A examines the number of open lines of credit for an individual, while Panel B examines the outstanding balance on credit lines. All other variables are described in Table 2. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by zipcode of the worker's residency. ***p<0.01, **p<0.05, *p<0.1.

Panel A: Effect on Number of Open Accounts

	All Accounts	Credit Card	Home Loans
Layoff × Carowner × Uber	-0.13***	-0.05***	-0.02***
	(0.01)	(0.004)	(0.001)
Layoff × Carowner	-0.11***	0.001	0.0001
	(0.01)	(0.003)	(0.001)
Layoff × Uber	0.06***	0.02***	0.01***
	(0.01)	(0.002)	(0.001)
Carowner × Uber	0.03***	0.02***	0.01***
	(0.01)	(0.003)	(0.001)
Layoff	0.05***	0.001	0.001***
	(0.004)	(0.002)	(0.0005)
Individual FE	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes
Obs.	12,277,167	12,277,167	12,277,167
Adj. R^2	0.93	0.93	0.94

Panel B: Effect on Account Balances

	Total Debt	Credit Card	Home Loans
Layoff × Carowner × Uber	-543.84***	-4.36	-324.57***
	(101.31)	(5.97)	(75.80)
Layoff × Carowner	-710.11***	14.79***	-342.06***
	(88.57)	(4.94)	(68.11)
Layoff × Uber	202.07***	1.43	90.43**
	(55.17)	(3.31)	(40.85)
Carowner × Uber	-139.57	15.71***	4.15
	(86.29)	(5.17)	(63.01)
Layoff	322.21***	-19.17***	227.35***
	(48.51)	(2.77)	(37.00)
Individual FE	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes
Obs.	12,277,167	12,277,167	12,277,167
Adj. R^2	0.92	0.79	0.92

Table 4. Effects on Delinquency Rates
This table reports the results of OLS regressions of the form described in Equation (2). All outcomes are indicator variables that take on a value of one if a worker is delinquent on a line of credit of the specified type. All other variables are described in Table 2. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by zipcode of the worker's residency. ***p<0.01, **p<0.05, *p<0.1.

	Any Delinquency	Credit Card	Home Loans
Layoff × Carowner × Uber	-0.49***	-0.56***	-0.10**
	(0.14)	(0.10)	(0.05)
Layoff × Carowner	1.12***	0.65***	-0.03
	(0.12)	(0.08)	(0.05)
Layoff × Uber	0.23**	0.22***	0.03
	(0.09)	(0.06)	(0.03)
Carowner × Uber	0.42***	0.44***	-0.04
	(0.12)	(0.09)	(0.04)
Layoff	-0.60***	-0.33***	-0.02
	(0.08)	(0.05)	(0.03)
Individual FE	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes
Obs.	12,277,167	12,277,167	12,277,167
Adj. R^2	0.57	0.60	0.51

Table 5. Local Recessions
This table reports the results of OLS regressions of the form described in Equation (2), while restricting the sample to CBSA-months observations we identify as experiencing a local economic downturn. We use data from the Bureau of Labor Statistics to compute the 6-month nominal change in the unemployment rate for each CBSA-month in our sample. An area is considered to experience a local economic downturn if the 6-month nominal change in the unemployment rate exceeds the 90th percentile in our sample. All outcome variables are described in Tables 2, 3, and 4. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by zipcode of the worker's residency. ***p<0.01, **p<0.05, *p<0.1.

Dependent variable:	1(Received Benefit)	Benefit Amount	Total Debt	Home Loans	Any Delinquency	Home Delinquency
Layoff × Carowner × Uber	-1.54***	-23.02***	-880.35**	-617.23**	-0.97**	0.02
	(0.27)	(3.70)	(370.96)	(250.82)	(0.44)	(0.15)
Layoff × Carowner	2.21***	37.32***	-800.20***	-427.69*	1.30***	0.03
	(0.26)	(3.51)	(282.73)	(219.86)	(0.38)	(0.14)
Layoff × Uber	-2.98***	-29.17***	332.40	127.09	0.32	0.11
	(0.27)	(3.55)	(209.61)	(164.22)	(0.34)	(0.10)
Carowner x× Uber	0.87***	11.05***	-27.09	53.34	0.44	-0.03
	(0.22)	(3.35)	(396.53)	(280.58)	(0.50)	(0.19)
Layoff	6.10***	61.65***	455.52**	442.70***	-0.95***	-0.14
	(0.28)	(3.39)	(195.13)	(142.50)	(0.30)	(0.09)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	1,155,139	1,155,139	1,058,413	1,064,460	1,096,374	1,096,374
Adj. R^2	0.22	0.16	0.92	0.92	0.58	0.50

Table 6. Cross-Sectional Effects: Auto Leases
This table reports the results of OLS regressions of the form described in Equation (2). Considered are individuals with and without auto lease accounts. The table examines the effects on UI uptake (Panel A), outstanding credit balances (Panel B), and delinquency rates (Panel C). All independent variables are described in Table 2. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by zipcode of the worker's residency. ***p<0.01, **p<0.05, *p<0.1.

Panel A: Effect on UI Uptake

	Without Auto Lease Accounts		With Auto Lease Accounts	
	1(Received Benefit)	Benefit Amount	1(Received Benefit)	Benefit Amount
Layoff × Carowner × Uber	-0.42***	-5.60***	-0.16	0.87
	(0.11)	(1.59)	(0.11)	(1.64)
Layoff × Carowner	0.75***	16.54***	1.62***	27.76***
	(0.10)	(1.40)	(0.10)	(1.45)
Layoff × Uber	-0.12	6.34***	0.14	7.25***
	(0.11)	(1.37)	(0.10)	(1.13)
Carowner × Uber	0.12*	-0.53	-0.39***	-8.91***
	(0.06)	(0.96)	(0.07)	(1.00)
Layoff	4.92***	55.10***	4.28***	47.01***
	(0.13)	(1.47)	(0.11)	(1.28)
Individual FE	Yes	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes	Yes
Obs.	6,081,196	6,081,196	6,195,971	6,195,971
Adj. R^2	0.24	0.18	0.23	0.18

Panel B: Effect on Account Balances

	Without Auto Lease Accounts		With Auto Lease Accounts	
	Total Debt	Home Loans	Total Debt	Home Loans
Layoff × Carowner × Uber	-648.00***	-581.66***	-544.99***	-104.03
	(201.65)	(116.45)	(177.35)	(99.68)
Layoff × Carowner	-375.34**	-181.72*	-981.53***	-488.73***
	(174.04)	(104.30)	(142.81)	(90.33)
Layoff × Uber	131.24	106.53*	313.04***	85.00*
	(103.50)	(63.55)	(82.84)	(51.39)
Carowner × Uber	26.62	107.27	-225.11*	-73.03
	(151.23)	(97.94)	(123.07)	(82.07)
Layoff	273.72***	225.79***	341.33***	226.09***
	(90.12)	(57.51)	(76.40)	(46.89)
Individual FE	Yes	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes	Yes
Obs.	5,493,714	5,457,778	5,525,582	5,590,453
Adj. R^2	0.92	0.92	0.92	0.92

Panel C: Effect on Delinquency Rates

	Without Auto Lease Accounts		With Auto Lease Accounts	
	Any Delinquency	Home Delinquency	Any Delinquency	Home Delinquency
Layoff × Carowner × Uber	-0.53***	-0.13**	-0.37*	-0.02
	(0.17)	(0.06)	(0.22)	(0.07)
Layoff × Carowner	2.24***	(0.01)	(0.03)	-0.03
	(0.15)	(0.07)	(0.19)	(0.06)
Layoff × Uber	0.32***	0.05	0.15	0.01
	(0.11)	(0.04)	(0.15)	(0.04)
Carowner × Uber	0.93***	-0.01	-0.10	-0.08
	(0.16)	(0.06)	(0.17)	(0.05)
Layoff	-1.20***	-0.05	-0.01	0.00
	(0.09)	(0.04)	(0.14)	(0.03)
Individual FE	Yes	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes	Yes
Obs.	5,780,434	5,780,434	5,907,494	5,907,494
Adj. R^2	0.59	0.52	0.55	0.51

Table 7. Exclusion of the Initial Period following Uber's Entry
This table reports the results of OLS regressions of the form described in Equation (2), while excluding the first 24 months following Uber's entry. All outcome variables are described in Tables 2, 3, and 4. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by zipcode of the worker's residency. ***p<0.01, **p<0.05, *p<0.1.

Dependent variable:	1(Received Benefit)	Benefit Amount	Total Debt	Home Loans	Any Delinquency	Home Delinquency
Layoff × Carowner × Uber	-0.38***	-3.47**	-564.33***	-385.51***	-0.65***	-0.10**
	(0.09)	(1.46)	(203.36)	(86.42)	(0.16)	(0.05)
Layoff × Carowner	1.17***	21.54***	-744.69***	-352.51***	1.18***	-0.01
	(0.08)	(1.09)	(132.18)	(70.61)	(0.12)	(0.05)
Layoff × Uber	-0.49***	3.00**	275.56***	149.41***	0.18	0.02
	(0.11)	(1.39)	(84.12)	(46.85)	(0.11)	(0.03)
Carowner × Uber	-0.22***	-7.69***	-351.60**	-19.65	0.84***	0.04
	(0.08)	(1.22)	(164.92)	(90.86)	(0.17)	(0.06)
Layoff	4.39***	48.04***	291.96***	191.84***	-0.57***	0.004
	(0.11)	(1.22)	(61.35)	(38.19)	(0.09)	(0.03)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	7,723,959	7,723,959	6,930,476	6,947,671	7,350,155	7,350,155
Adj. R^2	0.23	0.18	0.92	0.92	0.58	0.52

Table 8. High income

This table reports the results of OLS regressions of the form described in Equation (2), while considering Zipcodes with high family income. We use the yearly income statistics for each ZIP code from the SOI division of the IRS and restrict the sample to individuals residing in above-median income ZIP codes. All outcome variables are described in Tables 2, 3, and 4. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by zipcode of the worker's residency. ***p<0.01, **p<0.05, *p<0.1.

Dependent variable:	1(Received Benefit)	Benefit Amount	Total Debt	Home Loans	Any Delinquency	Home Delinquency
Layoff × Carowner × Uber	-0.24**	-5.72***	-793.72***	-420.88***	-0.51***	-0.003
	(0.13)	(2.06)	(208.21)	(126.02)	(0.18)	(0.07)
Layoff × Carowner	1.40***	24.78***	-1,047.40***	-446.03***	0.81***	0.02
	(0.12)	(1.91)	(210.39)	(116.23)	(0.17)	(0.07)
Layoff × Uber	0.05	7.20***	387.25***	163.98**	0.41***	0.04
	(0.13)	(1.76)	(122.97)	(71.63)	(0.13)	(0.04)
Carowner × Uber	-0.25***	-6.24***	-305.44**	-96.5	0.43***	-0.10*
	(0.07)	(1.15)	(148.85)	(100.04)	(0.15)	(0.06)
Layoff	4.91***	61.86***	408.52***	232.42***	-0.62***	-0.07*
	(0.15)	(2.04)	(123.81)	(66.37)	(0.12)	(0.04)
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
Obs.	6,127,072	6,127,072	5,169,348	5,180,192	5,836,698	5,836,698
Adj. R^2	0.23	0.18	0.92	0.92	0.6	0.52

Table 9. Cross-Sectional Effects: UI Benefits
This table reports the results of OLS regressions of the form described in Equation (2). Considered are above and below median sample splits based on the expected UI benefits, which equal to the product of the maximum amount of weekly UI benefits paid across states and the maximum duration of these benefits. The table examines the effects on UI uptake (Panel A), outstanding credit balances (Panel B), and delinquency rates (Panel C). All independent variables are described in Table 2. Reported standard errors in parentheses are heteroscedasticity-robust and clustered by zipcode of the worker's residency. ***p<0.01, **p<0.05, *p<0.1.

Panel A: Effect on UI Uptake

Max UI Benefits:	Below Median		Above Median	
	1(Received Benefit)	Benefit Amount	1(Received Benefit)	Benefit Amount
Layoff × Carowner × Uber	-0.54***	-5.27***	-0.16	-3.34
	(0.11)	(1.25)	(0.15)	(2.36)
Layoff × Carowner	0.98***	14.95***	1.44***	31.84***
	(0.09)	(0.98)	(0.14)	(2.28)
Layoff × Uber	0.45***	10.33***	-0.53***	1.03
	(0.12)	(1.25)	(0.15)	(1.97)
Carowner × Uber	0.19***	0.42	-0.37***	-7.07***
	(0.07)	(0.89)	(0.07)	(1.20)
Layoff	4.10***	40.09***	5.18***	63.20***
	(0.12)	(1.13)	(0.20)	(2.37)
Individual FE	Yes	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes	Yes
Obs.	6,028,550	6,028,550	6,138,895	6,138,895
Adj. R^2	0.22	0.16	0.25	0.20

Panel B: Effect on Account Balances

Max UI Benefits:	Below Median		Above Median	
	Total Debt	Home Loans	Total Debt	Home Loans
Layoff × Carowner × Uber	-630.99***	-325.26***	-472.80**	-278.19**
	(176.68)	(104.90)	(196.63)	(113.32)
Layoff × Carowner	-528.56***	-356.79***	-839.45***	-356.53***
	(179.45)	(91.16)	(179.06)	(104.65)
Layoff × Uber	255.27***	32.14	149.50*	53.89
	(97.83)	(55.55)	(86.62)	(61.86)
Carowner × Uber	-36.05	-15.39	-181.6	24.64
	(123.75)	(87.92)	(116.30)	(92.59)
Layoff	266.67***	228.94***	404.10***	253.39***
	(84.40)	(49.11)	(86.45)	(57.49)
Individual FE	Yes	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes	Yes
Obs.	5,516,531	5,534,441	5,405,514	5,416,383
Adj. R^2	0.92	0.92	0.92	0.92

Panel C: Effect on Delinquency Rates

Max UI Benefits:	Below Median		Above Median	
	Any Delinquency	Home Delinquency	Any Delinquency	Home Delinquency
Layoff × Carowner × Uber	-0.67***	-0.10**	-0.22	0.003
	(0.19)	(0.05)	(0.21)	(0.07)
Layoff × Carowner	0.98***	-0.03	1.23***	0.03
	(0.16)	(0.06)	(0.19)	(0.07)
Layoff × Uber	0.12	0.05	0.34**	-0.01
	(0.13)	(0.04)	(0.14)	(0.04)
Carowner × Uber	0.36**	-0.04	0.53***	-0.06
	(0.16)	(0.06)	(0.17)	(0.06)
Layoff	-0.44***	-0.06*	-0.66***	0.01
	(0.11)	(0.03)	(0.13)	(0.04)
Individual FE	Yes	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes	Yes
Obs.	5,739,428	5,739,428	5,843,569	5,843,569
Adj. R^2	0.57	0.52	0.58	0.51

Gig-Labor: Trading Safety Nets for Steering Wheels

Online Appendix

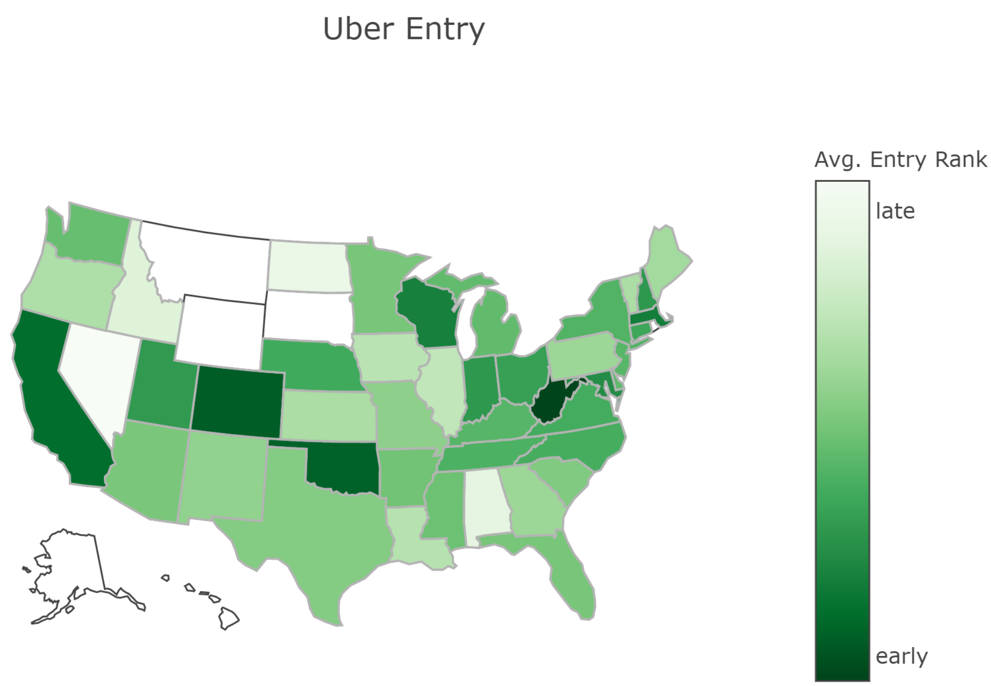


Figure OA.1. Uber Entry across States

This figure illustrates the variation in Uber's entry into markets. The figure presents a choropleth (geographic heat map) of the state-level percentile of Uber's relative entry date, averaged across all entered CBSAs in the state.

Table OA.1. Robustness: *City-Ownership-Layoff* fixed effects
This table reports the results of OLS regressions of the form described in Equation (2), when also including a fixed effect that varies at the *City-Ownership-Layoff* level. Panel A reports the results for UI benefits (as Table 2). Panel B reports the results for account balances (as Table 3). Panel C reports the results for delinquency rates (as Table 4). Reported standard errors in parentheses are heteroscedasticity-robust and clustered by zipcode of the worker's residency. ***p<0.01, **p<0.05, *p<0.1.

Panel A: Effect on UI benefits

	1(Received Benefit)	Benefit Amount
Layoff × Carowner × Uber	-0.28***	-5.98***
	(0.07)	(1.08)
Layoff × Uber	-0.94***	-7.81***
	(0.06)	(0.73))
Carowner × Uber	-0.26***	-3.79***
	(0.04)	(0.64)
Individual FE	Yes	Yes
City-Month FE	Yes	Yes
City-Ownership-Layoff FE	Yes	Yes
Obs.	12,277,167	12,277,167
Adj. R^2	0.26	0.2

Panel B: Effect on account balances

	Total Debt	Credit Card	Home Loans
Layoff × Carowner × Uber	-542.86***	4.8	-278.80***
	(128.75)	(6.14)	(82.75)
Layoff × Uber	207.17***	-4.97	76.18*
	(68.64)	(3.68)	(44.91)
Carowner × Uber	-183.81**	16.88***	-27.04
	(92.42)	(4.69)	(68.96)
Individual FE	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes
City-Ownership-Layoff FE	Yes	Yes	Yes
Obs.	12,277,167	12,277,167	12,277,167
Adj. R^2	0.92	0.79	0.92

Panel C: Effect on delinquency rates

	Any Delinquency	Credit Card	Home Loans
Layoff × Carowner × Uber	-0.23**	-0.53***	-0.11**
	(0.11)	(0.11)	(0.05)
Layoff × Uber	0.19*	0.28***	-0.01
	(0.11)	(0.07)	(0.03)
Carowner × Uber	0.37***	0.52***	-0.08*
	(0.13)	(0.10)	(0.04)
Individual FE	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes
City-Ownership-Layoff FE	Yes	Yes	Yes
Obs.	12,277,167	12,277,167	12,277,167
Adj. R^2	0.57	0.60	0.51