Gig Labor: Trading Safety Nets for Steering Wheels*

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Using administrative data on unemployment insurance matched with the credit profiles for individuals in the U.S., 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 exploits both the staggered entry of Uber across cities and the differential benefit of Uber's entry across car owners based on car age, a key eligibility requirement of the platform. We conclude that the introduction of Uber had a profound effect on labor markets, changing the way employees respond to job loss.

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Abstract

Using administrative data on unemployment insurance matched with the credit profiles for individuals in the U.S., 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 exploits both the staggered entry of Uber across cities and the differential benefit of Uber's entry across car owners based on car age, a key eligibility requirement of the platform. We conclude that the introduction of Uber had a profound effect on labor markets, changing the way employees respond to job loss.

1. Introduction

The idea that an individual prefers a smooth consumption stream to a lumpy one serves as the foundation of microeconomics (Keynes, 1936; Modigliani and Brumberg, 1954; Friedman, 1957; Hall, 1978). This key insight underpins the expected welfare gains from efficient intertemporal 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 (Diamond, 1990; Herkenhoff et al., 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 a 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 reemployment (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 et al., 2013; Mian and Sufi, 2014; Bernstein, 2021). Each tool acts as a half-measure because neither is able to address the root issue: frictions that prevent a newly unemployed individual from readily being rematched in the labor market.

In this paper we examine whether the introduction of the gig economy serves as a substitute for these other responses 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 3.8% less likely to receive UI benefits. Back-of-the-envelope calculations suggest a yearly reduction of about $1.53B in UI benefits distributed by government agencies as a result of a universal roll-out of ride sharing platforms. We find analogous effects when considering credit usage, where laid-off workers experience a relative decrease in total outstanding balances of $789,

or 1.2% of the average individual's debt burden.[1] Finally, effects of the ride-sharing platform extend to credit performance, with workers experiencing a relative decrease in delinquencies of 3.8%. To the extent that credit delinquencies are associated with negative welfare implications, this last result is consistent with a transition towards gig-based labor supply that allows some laid-off workers to avoid costs associated with job loss.

To identify these effects, we first leverage the disaggregated employment data to identify the set of job separations outside of a worker's control (a layoff). Our empirical strategy is based on a triple difference-in-difference approach where 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 is present. The final difference captures the differential effect of Uber's entry following job loss based on a worker's car age, which determines whether a car meets eligibility requirements specified by Uber. This final dimension reflects heterogeneity in individuals' ability to readily participate on the ride-sharing platform to earn income following a job separation. The high frequency, granular nature of the data allows us to strip out any location-specific economic trends with high-density 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 an eligible car owner relative to a noneligible car 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 eligible car-owners and non-eligible car owners would be the same for areas with and without Uber.

We present several pieces of evidence to support our identifying assumption. We find no evidence of a change in the composition of laid-off workers around the time ride-sharing enters a market, which is largely dictated by market size (Buchak, 2018). To further address

1. Given the nature of our data, as we discuss in more detail below, we are unable to decompose this reduction into the effect of the gig-economy on debt rollover rates and on consumption rates.

concerns of an omitted variable, we decompose our effects into event time around Uber's entry into a market, finding no evidence of an effect leading up to entry. 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 reemployment elsewhere.[2] Consistent with the short-term treatment of the ride-sharing platform, accounts of government workers moonlighting in the gig economy during recent government shutdowns were widely publicized by the media and policy makers alike (Little, 2013; Halsey and Aratani, 2019). Importantly, this mechanism represents a structural shift in labor markets likely to benefit a broad group of laid-off workers. Alternatively, workers may treat labor prospects associated with the ride-sharing platform as ubiquitous long-term job opportunities, with Uber's entry simply increasing the overall supply much in the same way a traditional employer would. Anecdotal evidence notwithstanding, we perform a series of tests to shed light on the channel through which Uber's entry affects labor markets.

First, we consider a subset of workers less likely to view Uber as a long-term employment prospect. After restricting the sample to those who regain formal employment at another firm within 12 months of the initial layoff, we continue to find that the effects of Uber's entry remain economically significant. Next, we consider workers from above-median income ZIP codes, and find similar results. Laid-off workers residing in such areas, which have an average yearly household income of over $134k, are arguably less likely to view Uber as an alternate form of permanent reemployment. We also 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 tradeoff is likely to be influenced by the potential UI benefits available.

2. Alternatively, a worker may have participated on the ride-sharing platform prior to job loss, originally treating Uber as a secondary source of income (Koustas, 2018). In such instances, our results are also consistent with an increase in the intensity of participation on the platform while seeking long-term employment elsewhere.

We find that the effects of Uber's entry are stronger in states with less generous UI benefits. Finally, in evaluating the horizon over which the ride-sharing platform had an impact on labor markets, we find that the effects of Uber remain significant two-plus years after Uber's entry into a market.

Overall, this series of tests suggests that Uber alters labor market dynamics by increasing the pool of easily accessible short-term jobs. Whereas our analysis focuses on Uber, the economic mechanism identified in this paper is likely to extend to a broad class of gig-labor firms that offer laid-off workers an ability to supply a discretionary amount of labor almost instantaneously (e.g., TaskRabbit, Thumbtack, etc.). Interestingly, the significant change these firms bring to labor markets is not the result of massive investments in fixed assets. Rather, the distinguishing feature of a gig-labor firm is that it merely changes the matching process of workers and tasks.

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 et al. (2018), while East and Kuka (2015) document 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. 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 et al., 2007; Schmieder et al., 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 ex-

amining 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, 2021; Mian and Sufi, 2014; Bethune, 2015), consumption (Mian et al., 2013), and housing prices (Mian and Sufi, 2011). This fragility is accentuated following a job loss; Gerardi et al. (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 et al. (2018) highlight the role of UI as a housing market stabilizer, helping individuals avoid foreclosure and the associated deadweight loss. We find that delinquency rates 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 et al., 2017; Buchak, 2018). Additional evidence documents the increase in competitive pressure faced by taxi drivers following Uber's entry (e.g., Hall et al., 2017; Berger et al., 2018). Burtch et al. (2018) document a negative and significant relationship between ride-sharing and entrepreneurial activity. Further, Barrios et al. (2018) show that the arrival of ride-sharing is associated with an increase in the number of motor vehicle fatalities and fatal accidents.[3]

Our paper is closely related to Koustas (2018) and Jackson (2019). Using account-level data from an online account aggregator, Koustas (2018) finds that the ride-sharing platform serves as a flexible second job, allowing a worker to buffer a negative shock to primary job income, likely due to a reduction in hours worked. We compliment this result by examining a worker's response to a complete loss of her primary source of income, demonstrating the role the ride-sharing platform plays in reducing a worker's demand for other means of weathering a layoff (i.e., UI participation and increased credit utilization). Using annual tax

3. 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.

filings, Jackson (2019) examines the short- and medium-term impact of the gig economy on traditional employment and earned income. The paper finds that among individuals receiving UI benefits, access to the gig economy increases one-year income while two- to four-year income lags expected levels, as individuals forgo traditional reemployment. Using individual-level data on layoffs linked to administrative UI data and consumer credit histories, our paper compliments this work by highlighting the effect of the gig economy on UI participation rates, credit utilization, and delinquencies. Importantly, the effect of the gig economy on delinquency rates we document suggests that the change in credit performance among laid-off workers is an important ingredient which should be considered when assessing the overall impact of the gig economy.

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. We discuss potential economic mechanisms and present tests designed to distinguish between potential candidates in Section 5. Finally, external validity and potential implications of our findings are discussed in Section 6, while Section 7 concludes.

2. Data

This section describes the data 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 four different data sets: 1) Uber product rollout dates across different geographic areas, 2) job separations and UI claims, 3) disaggregated credit data provided by Equifax Inc, one of the three credit bureaus involved in collection and transmission of credit and employment data within the U.S., and 4) car registration data collected by R.L. Polk & Company, also provided by Equifax.

2.1. Uber Introduction Data

We obtain the comprehensive set of product launch dates that occurred between June 2012 and February 2016 from Uber. The data covers approximately 120 Core-based Statistial Areas (CBSAs) 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 focus on the introduction of "UberX," a product for which the largest share of the population has access to a qualifying vehicle.[4]

[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 relative 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). 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, and UI

The data from Equifax Inc., contains anonymized individual-level information across the following three dimensions: credit, job separation events, and UI participation. The first of

4. 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.

these, consumer credit histories, contains credit line-level information for all individuals with some form of credit history in the U.S., and includes information such as account type (e.g. credit card, home loan), borrower location, account age, total borrowing, account balance, any missed or late payments, and defaults. These data are available at a monthly frequency between January 2010 through December 2017. To the best of our knowledge, we are the first to use the credit panel linked to individual-level job separation and UI participation data from Equifax Inc. We next describe these data in detail.

Job separation data are disseminated to Equifax by self-reporting employers who subscribe to UI management services provided by the company. When a UI claim is filed, government agencies reach out to the ex-employer to acquire information on the terms of separation in order to verify UI eligibility.[5] Many states require employers to respond to all such government requests to facilitate the efficient administration of UI claims. In order to adhere to such requirements, participating employers subscribe to the UI management services from Equifax which manages all such governmental requests on their behalf. As a result, participating employers report data related to all incidences of job separation to the company. The job separations data includes close to 20% of all separations reported in the Bureau of Labor Statistics (BLS)'s Job Openings and Labor Turnover Survey (JOLTS) data over our sample period.[6] Using this anonymized data, for each job separation, we are able to observe the date of the job separation and the reason for the separation.

In addition, Equifax Inc receives the UI participation and benefit data directly from relevant federal and state agencies. The company manages all communications with the governmental agencies on behalf of subscribing employers and, as part of such communications, receives administrative data related to all monetary disbursements received by laid-off workers. We begin by identifying instances of involuntary job loss (i.e., layoffs) using the job separations data, and merge it to UI data and anonymized credit histories that allow us to

5. One of the eligibility requirements require that claimants must have separated from the employer involuntarily due to no fault of their own.

6. The JOLTS program from BLS provides data on job openings, hires, and separations.

examine a large set of outcomes for laid-off individuals. In a later part of the analysis, we also utilize a separate employment data set from Equifax that covers 30 million individuals employed at 5,000 employers in the U.S. These employers subscribe to a different service (employment and income verification) provided by the company, and report information of their employees on a payroll-to-payroll basis.

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. State unemployment programs also vary over the determination of benefit amounts, maximum benefit caps, and the duration of benefit payments. For instance, as of January 2014, the maximum amount of weekly UI benefits provided in Massachusetts was $679, compared to $235 offered in Mississippi. We exploit this source of variation in state-level benefit generosity in later tests. At the same time, UI benefits falling below the benefit caps typically amount to approximately 50% of a worker's pre-layoff wages. In contrast, Hall and Krueger (2018) estimate that individuals participating on the ride-sharing platform earn approximately $19 per hour as of October, 2015. Moreover, while guidelines vary across states, UI benefits are typically offset by income earned during the unemployment spell after weekly earnings pass a low threshold (e.g., $50 in Georgia, Maryland, Tennessee, and Virginia). Taken together with the added flexibility of a worker being able to provide labor when her time-varying reservation wage is lowest (Chen et al., 2017), it is plausible that a recently laid-off worker may view participating on the platform as a preferred option to the reception of unemployment benefits.

2.3. Car Registration

The final source of data used in our primary analysis is car registration information collected by R.L. Polk, currently owned and maintained by IHS Markit, and provided by Equifax. The data is based on car registrations filed with state motor vehicle authorities

(e.g., DMVs and RMVs). Each observation in the data reflects a transfer of automobile ownership. The data includes all transactions involving both new and used cars for 48 states in the U.S., occurring after January 2010.[7] Among other information, the registration data includes vehicle identification numbers (VINs), from which we are able to identify key vehicle characteristics including make, model, and year of manufacture. The full sample consists of approximately 385M transactions involving 177M unique individual owners between 2010 and 2017.

2.4. 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 credit and UI participation decisions. For instance, it is plausible that an individual intending to quit her current job will reduce credit utilization prior to the event of job loss. A worker would also be ineligible for UI benefits in this instance, as UI eligibility requirements generally require that a worker lose the job through no fault of her own. Thus, the inclusion of individuals who experience a voluntary job separation may result in biased estimates. Instead, an ideal setting would consider unanticipated job losses, unrelated to a worker's actions or labor productivity.

Fortunately, the UI and job separation dataset lists the employer-reported reason for a job loss. We use this description field to identify separations that are plausibly unanticipated by the worker. Specifically, we identify individuals who 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. Our empirical strategy, outlined in more detail in Section 3, exploits variation in car-age-based eligibility to operate on Uber's platform. The intersection of the full sample of layoffs and individuals with a car within 5 years of the eligibility requirement yields a sample of 1.6M layoffs. We

7. New Hampshire and Pennsylvania are not included in the data. In addition, the data from Arkansas, Hawaii and Washington are only available beginning 2015.

then restrict the sample to layoffs that occur between January 2011 and December 2016, ensuring at least 12 months of data before the first layoff and after the last layoff in the sample. This restriction leaves us with approximately 700k layoffs. Next, we restrict the sample to CBSAs which experience the introduction of Uber at some point during our sample period.[8] Finally, we restrict our sample to individuals present in our credit data (i.e., those with some credit history) leaving us with slightly more than 263k layoffs.

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 25-month period surrounding the month that an individual is laid off. The panel indicates that the monthly probability of a UI benefit reception is 2.643%. As we are going to see in table 3, this probability sharply increases from 0.79% during the prelayoff period to 4.49% during the post-layoff period. Overall, in our sample about 13.4% of workers receive UI benefits following a layoff (note that the post-layoff period spans 12-month period regardless of UI benefit reception in a given month). This statistic differs from traditional UI take-up rates (e.g., Anderson and Meyer, 1997) for a number of possible reasons. For instance, since we are unable to determine a laid-off worker's UI eligibility (e.g., because of state-level tenure requirements) our estimates of UI take-up rate understate the actual take-up rate among eligible workers. Moreover, our sample also includes laid-off workers who quickly find reemployment, for whom UI participation may not be viable. Since such individuals are likely not to show up in unemployment statistics, this will result in a downward bias of our estimated UI take-up rate relative to traditional estimates of the UI take-up rate. Conditional on receiving UI benefits, the average worker receives \$474 per month (\$12.546/0.02643). The workers also have a non-negligible average outstanding debt balance of roughly \$66k, with a median of \$18.2k. Finally, workers are delinquent on their debt obligations in a nonnegligible portion of the sample, with a 12.5% likelihood of being delinquent on at least one line of credit at any given point in time.

8. Untabulated robustness test shows that our results are not driven by this restriction.

[Insert Table 1 Near Here]

To provide more insight into our final sample, Figure 2 illustrates the time-series variation in layoffs. The figure 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 four-fold. This increase coincides with the passage of the Federal Unemployment Insurance Integrity Act, which placed the burden of UI information verification requests on employers and expanded the set of employers using Equifax's UI management services.[9] 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]

3. Empirical Strategy

Does the rise of the gig economy 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.

The staggered nature by which Uber entered different geographic regions constitutes the first dimension we use in our empirical strategy. To illustrate this, suppose individuals experience a change in an outcome of interest (e.g., the likelihood of experiencing a credit

9. Results remain qualitatively unchanged if we restrict the sample to post December 2012 layoffs.

delinquency) following a job layoff. Then, if well identified, the difference-in-difference estimator made up of the interaction of an indicator variable for a job layoff and the presence of Uber in a CBSA at a point in time represents the unconditional effect of Uber's entrance on the change in outcome following job loss.

Given our use of staggered entry 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. Figure OA.2 supports this assertion, plotting the relation between 2012 CBSA-level census population estimates and Uber entry. Consistent with Buchak (2018), we find a strong downward-sloping relationship in which Uber enters larger markets first, 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. However, given this relation, one potential concern is that a worker's response to job loss also varies by market size. Our empirical specifications are informed by this possibility, in which multiple controls are allowed to vary at the CBSA-level, which we describe in more detail below.

While the introduction of the ride-sharing platform may provide temporary reemployment prospects, it is unlikely that all individuals are equally likely to benefit. Instead, one must first have access to a vehicle that meets the eligibility requirements mandated by the ride-sharing platform. The easiest way to meet this criterion is for an individual to already possess such a vehicle. This idea motivates our preferred empirical model. Specifically, while eligibility requirements exhibit some variation over time, and across markets and products, a qualifying vehicle must generally be a four-door sedan less than 15 years of age. In similar spirit to Buchak (2018), our empirical strategy exploits this latter requirement, leveraging car registration data to compare individuals with eligible vehicles between 10 and 15 years of age (*eligible car owners*) to those with vehicles between 15 and 20 years old who do not qualify (*noneligible owners*).

As our empirical strategy combines variation in eligibility with spatial variation in entry

dates, one concern might be that geographic regions exhibit varying degrees of car eligibility. Appendix Table OA.1 reports the average rate of car eligibility in our sample by state. Instead, eligibility shares appear to be quite homogeneous across states. Nevertheless, while our primary strategy exploits variation within car owners based on eligibility requirements to help alleviate identification concerns, it does so by focusing on a narrow subset of laid off workers. We revisit this tradeoff between identification and generalizability in Section 6, which considers a more general sample using a related identification strategy.

The result is a "triple diff-in-diff" estimator, which we formally describe in Equation (1). For each worker i, for which we classify her job loss as being out of her control (a *layoff*), we denote the month of the job separation by S_i. We retain the 25-month period surrounding this event, starting in month $S_i - 12$ and extending through month $S_i + 12$.[10] Given variation in layoff date, the result is an unbalanced panel with respect to calendar time. With this panel, we estimate empirical specifications of the following form:

$$\begin{aligned} y_{ijt} = {} & \eta \times \mathit{Layoff}_{ijt} \times \mathit{Uber}_{jt} \times \mathit{EligibleOwner}_{ijt} + \beta_{jt} \times \mathit{Layoff}_{ijt} \\ & + \delta_j \times \mathit{Layoff}_{ijt} \times \mathit{CarAge}_{ijt} + \mu_j \times \mathit{Uber}_{jt} \times \mathit{CarAge}_{ijt} + \phi_i + \varepsilon_{ijt}, \end{aligned} \tag{1}$$

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 $t \geq S_i$. 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. $\mathit{EligibleOwner}_{ijt}$ is an indicator that takes on a value of one if the age of individual i's car in the month prior to layoff is less than 15 years old. The sample is limited to car owners those vehicles are between 10 and 20 years old. Our variable of interest is the triple interaction of these three terms: *Layoff*, *Uber*, and *EligibleOwner*. This interaction captures the differential effect of Uber's presence on eligible car owners relative to noneligible car owners following job loss. There are two

10. In an extremely small number of cases a worker in our sample experiences two distinct layoffs. For such individuals, we retain the 25-month period around both layoffs.

things to note about our empirical strategy. First, to the extent that another ride-sharing platform operates in a CBSA prior to Uber's entry, a subset of eligible car owners will be incorrectly assigned to the control group. The result is an attenuation in the point estimate of the triple-interaction term. Second, we are unable to observe the propensity of a laid-off worker we classify as being an eligible car owner (or noneligible car owner) to participate on the platform. As such, η is an estimate of the unconditional differential effect of Uber's entry across all laid-off workers we classify as having an eligible car. We revisit this observation when interpreting the economic magnitude of our point estimates below.

As noted above, Uber's entry was strongly correlated with population counts, which may also impact an individual's response to layoffs. To account for this, and other effects driven by local economic conditions, we allow the general response to layoff β to vary at the CBSA-month level. Doing so also subsumes one double-interaction term generally included in a triple diff-in-diff ($Layoff \times Uber$). Our strategy also exploits variation in the age of a worker's vehicle. Given the plausible correlation between the ownership of a younger vs. older vehicle and other characteristics (e.g., wealth), it is plausible that the response to job loss might also vary with the choice of vehicle age. To this end, we interact $CarAge_{ijt}$, a vector of indicator variables corresponding to the possible yearly values of car age for individual i, with *Layoff*. We allow this control (which subsumes $Layoff \times EligibleOwner$) to vary across CBSAs, again reflecting the nonrandom entry of Uber across regions. To account for the final double interaction ($Uber \times EligibleOwner$), similar to the previous term, we interact $Uber_{jt}$ with the vector of car-age indicators, again allowing for the effects to vary across CBSA. Finally, we include an individual fixed effect, ϕ_i, to absorb individual time-invariant unobservable characteristics which could correlate with the reliance on credit and delinquency rates.[11]

Before continuing, we note that while prelayoff ownership of an eligible car is perhaps the most likely means of participating on the ride-sharing platform, this requirement may

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

be met by a number of alternative means. First, a worker may purchase a qualifying vehicle following job loss in order to participate on the platform. However, in contrast to the unconditional increase in auto purchases following Uber's entry documented by Buchak (2018), the newly unemployed likely face significantly larger frictions when attempting to purchase a vehicle relative to the general population. Consistent with this conjecture, unreported results indicate no evidence of a change in the likelihood of purchasing a qualifying vehicle in the months surrounding job loss pre- and post-Uber entry. Second, a recently laid-off worker may participate in a car rental program provided by Uber. However, the earliest such program was initiated in a limited number of markets in August 2015 (Uber, 2019), six months before the final cohort of laid-off workers we consider. Third, a noneligible car owner may participate on the platform by partnering with someone with an eligible car. While this possibility exists, we are unable to observe such instances and cannot comment on the frequency with which they occur. Importantly, these possibilities and the resulting misclassification of car eligibility will attenuate point estimates and bias against finding an effect.

The identifying assumption underlying this empirical strategy is as follows: the timing of Uber's entry into a market is orthogonal to omitted variables that would 1) influence the outcome of interest for a worker owning an eligible car relative to the owners of noneligible cars, and 2) that the change would not be present during employment and only materialize post separation. Put differently, in the absence of Uber's entry, the difference in outcome variables around layoffs for owners of younger versus older vehicles would be the same for areas with and without Uber. Before continuing, we discuss several aspects of this identifying assumption. Section 4.3 presents additional results in support of this identifying assumption.

One plausible concern is that Uber's entry might change incentives to continue working for individuals with an eligible car relative to owners of noneligible cars. While more relevant if studying all job separations, rather than our focus on *layoffs*, we briefly explore this possibility in case layoffs are being misclassified. Using our sample of job separations, we

construct the likelihood of a worker being laid off in a given quarter. Using this panel, Figure OA.3 explores a potential change in the likelihood of an eligible car owner being laid off relative to a noneligible car owner. The figure reports coefficients from a modified version of Equation (1) where we regress an indicator for being laid off on the interaction of *EligibleOwner* and a vector of event time dummies corresponding to Uber's entry into a market. The figure does not reflect any noticeable change in the relative likelihood of an eligible car owner being laid off around Uber's entry.

In broader terms, one challenge to our identification strategy is that Uber's entry coincides with a shift in the *type* of worker being laid. Specifically, if such differences were correlated with owning a newer versus older vehicle, this might bias our triple-difference estimator. To evaluate this concern, we consider the change in observable characteristics (e.g., credit score) for eligible vs. noneligible car owners around Uber's entry. For each layoff event, we collect the set of worker characteristics, measured in the month prior to job separation. As we now have one observation per layoff event, we modify Equation (1) to exclude all terms containing $Layoff$ as well as the individual fixed effect, while still including CBSA-month fixed effects. This yields a traditional difference-in-difference specification for eligible vs. noneligible car owners, pre- and post-Uber (defined using the layoff month for each worker). Table 2 presents the results, which are inconsistent with a change in observable characteristics across the two groups following Uber's entry into an area. Following Uber's entry, there is no statistically significant change in credit score (Column 1), amount of debt outstanding (Columns 2 through 4), or delinquency rates (Columns 5 through 7).

[Insert Table 2 Near Here]

Before continuing to the main results, we briefly examine the response of each outcome of interest to job loss in a simplified setting. Panel A of Table 3 shows that (perhaps unsurprisingly) after layoffs debt balances, the likelihood of credit delinquencies, and the likelihood of receiving UI benefits sharply increase.[12] Panel B of Table 3 reports qualitatively

12. Positive pre-layoff UI benefit values are consistent with a worker being laid off from a previous job,

similar responses to job loss when considering subsamples partitioned on a single dimension, either Uber's presence or car eligibility. Finally, Panel C of Table 3 shows the results for the four subsamples from the interaction of car eligibility and Uber's presence. Individuals increase the reliance on UI benefits, experience higher delinquency rates, and increase total debt balances following a job loss in each group.

The table reveals that owners of eligible cars have higher debt balances than owners of non-eligible cars. Importantly, such a difference does not pose a specific challenge to our identification assumption. Instead, as discussed above, such a difference must exhibit a change due to a factor correlated with Uber's entry to pose a threat to our empirical approach. We revisit this point in Section 4.3 below.

[Insert Table 3 Near Here]

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? We begin by examining each outcome of interest through the lens of our primary empirical strategy, beginning with the effect on unemployment insurance participation. We follow this up with a focus on credit usage performance. Next, we present graphical evidence which decomposes the effect in event time with respect to the layoff event. Finally, we report results of several tests in support of our identifying assumption.

4.1. Primary Results

We begin by studying the effect of the gig economy on the propensity to turn to unemployment insurance following job loss. Table 4 presents the results of OLS regressions of the form detailed in Equation (1). Standard errors are reported in parentheses, clustered at the zipcode of the worker's residence.

receiving UI benefits, becoming reemployed at a firm in our sample, and subsequently being laid off again.

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, scaled by 100 for ease of interpretation. Here the triple-interaction term captures the differential effect of Uber's introduction on eligible car owners relative to noneligible car owners following job separation. The coefficient of -0.171, significant at the 5% level, indicates that the likelihood of an eligible car owner receiving UI benefits (relative to a noneligible car owner) in a given month following job loss decreases by 17 basis points when Uber operates in the individual's CBSA. This represents a 3.8% relative decrease in the unconditional probability of receiving benefits following layoff (0.17% / 4.49%). Recall, our data do 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 meets 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 UI eligibility systematically varies for eligible car owners relative to noneligible car owners in a way that changed after Uber's entry.[13]

[Insert Table 4 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 -1.829, statistically significant at the 1% level, indicates that an eligible car-owning individual receives \$1.829 less per month following Uber's introduction into an area relative to a noneligible car owner. It is important to note that this corresponds to the

13. An individual who participates on the platform is required by law to report any earned income to the state unemployment insurance agency. However, because independent contractors receive a Form 1099 rather than a Form W-2, it is unlikely that a state UI agency is able to verify a worker's contemporaneous gig economy income while applying for UI benefits. For this reason, while failing to report earned income is generally considered fraud, we cannot rule out the possibility that some individuals participate on the ride-sharing platform, earn supplement income that is not reported to authorities, and still file a claim for UI benefits. At the same time, this possibility will result in a downward-bias in the estimated effect on UI take-up rates.

unconditional effect across all individuals, regardless of if they apply for UI benefits, rather than the intensive margin of UI usage. The coefficient on the triple-interaction represents a 8.1% relative decrease in this estimate of average UI usage for eligible car owners following layoff (-1.829 / -22.524).[14] The Department of Labor projects total benefits paid in 2018 to be \$28.8B.[15] In a back-of-the-envelope calculation, assuming a car ownership rate of 88% (Pew 2015) and an eligible ownership rate among car owners of 75%[16], a 8.1% reduction in total UI benefits paid out across all car owners equates to an approximate savings of \$1.53B per year ($\$28.8B \times 0.88 \times 0.75 \times 0.081$). While this calculation makes simplifying assumptions, the results suggest that our documented effects are economically meaningful.

While Table 4 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 downturns, this option is not costless. The limited-liability nature of consumer credit may lead to increased dead-weight costs due to moral hazard. For instance, Bernstein (2021) documents a reduction in labor supplied by households experiencing debt overhang. In addition, Mian et al. (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 reduce such costs in a counterfactual 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 5, which estimates OLS regressions of the form laid out in Equation (1) where the outcome is an individual's outstanding credit balance. In the

14. 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 who receive zero dollars in UI benefits, which make up the majority of our sample.

15. https://goo.gl/hghbCB

16. This is similar to the eligible ownership rate within our sample of 73.7%.

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 eligible car owners relative to noneligible car owners decreases by roughly $789 following Uber's entry into a local market. The point estimate on our variable of interest represents a 1.2% decrease relative to the average debt balance following layoff ($789 / $67,962).[17] Interestingly, when we consider each type of credit separately, we find a small change in the balance on credit cards (significant at 10% level). Instead, the final specification indicates the effect is predominately driven by a relative decrease in the balance of home loans ($507) which includes home equity loans, home equity line of credit (HELOC), second mortgages etc. It is important to note that we are unable to decompose a change in outstanding balances into the component attributed to debt rollover and that due to additional spending or consumption. However, it is unclear why the entrance of Uber would result in a *decrease* in the consumption of eligible car owners relative to noneligible car owners following a layoff.

[Insert Table 5 Near Here]

Panel B of Table 5 turns to the effects of Uber's introduction on the number of open accounts for an individual. The panel presents results consistent with Panel A, with eligible car owners experiencing a decrease in credit lines relative to noneligible car owners following Uber's entry into an area.

As a whole, the results presented in Table 5 suggest that eligible car owners are less likely to tap into credit reserves following job loss when Uber operates in the 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

17. 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.

attenuate the disincentive to work caused by debt overhang (Bernstein, 2021). We now seek evidence of a more direct channel through which Uber's entry may affect local economic conditions: delinquency rates.

Table 6 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 line of credit. The coefficient on the triple-interaction term suggests that Uber's entry reduces the change in delinquency rates for eligible car owners by 0.516 percentage points, or 3.8% of the mean delinquency rate following job loss (13.688%). Moving to credit card performance in the second specification, we find a similar sized effect in absolute terms of -0.514 percentage points. However, compared to the lower unconditional likelihood of being delinquent on a credit card following job loss (7.263%), this effect constitutes a relative decrease of 7.1%. In the final specification, we turn to the effect on mortgage delinquency rates. We find a reduction of 1.2 basis points in delinquencies among eligible car owners relative to non-eligible car owners following Uber's entry. Relative to the unconditional probability of mortgage delinquency of 2.267% following job loss, this represents a relative decrease of 0.53%.

[Insert Table 6 Near Here]

4.2. Graphical Evidence

Next, we examine the time-series dynamics of the effects presented in Section 4.1 in event time around a worker's job separation. To do this, we first construct the vector of 25 indicator variables which map to the 25 months around the layoff event, spanning -12 to 12. Next, we modify Equation (1) and replace the indicator for being laid off, $Layoff$, with this set of event-time indicator variables. We omit the observation three months prior to separation as the baseline. Our focus is on the triple interaction of $Uber$, $EligibleOwner$, and the vector of event-time dummies.

Figure 3 graphically presents the results from this approach. Panel A of Figure 3 focuses on the likelihood that an individual receives UI benefits in a given month. The negative

coefficient on the $t = 0$ interaction term indicates that following Uber's entry, eligible car owners are approximately 2 percentage points less likely to receive UI benefits relative to their noneligible car owner counterparts in the month they suffer a job loss. This difference persists for approximately six months (corresponding to the maximum duration of UI benefits for the majority of states), before starting to converge back to zero. Importantly, the fact that the coefficient remains negative in later months suggests that the introduction of Uber does not simply postpone a household's uptake of UI until a few months after being laid off. 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 eligible car owners relative to noneligible car owners.

[Insert Figure 3 Near Here]

Next, we examine the change in household leverage outcomes. Panel C of Figure 3 reports the results where the outcome is an individual's total amount of debt outstanding. The figure depicts a gap in the post-Uber difference in outstanding debt for eligible vs. non-eligible car owners. This gap continues to widen for three months, at which point eligible car owners have a relative decrease in outstanding balances of slightly less than $1k, before stabilizing. 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 credit obligation. While there is no relative difference in delinquencies prior to job loss, this pattern does not hold following separation. Instead, following Uber's entry into a market, eligible car owners experience lower delinquency rates relative to noneligible car owners. 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 3 are consistent with the results in Section 4.1, while showing no signs of a change in worker behavior prior to being laid off.

4.3. Pre-existing Trends

The results presented in Section 4 up to this point are consistent with the gig economy having a significant effect on household behavior following job loss. At the same time, it is possible that an alternative force is instead at play. Perhaps the most plausible concern is the endogenous entry of Uber into an area experiencing economic growth that disproportionately benefits owners of eligible cars relative to owners of noneligible cars. In other words, the timing of Uber's entry into markets may coincide with the realization of a preexisting, time-varying factor that differentially affects an eligible car owner's reemployment prospects relative to a noneligible car owner.

If this alternative is at work, one would expect that the difference in outcomes between eligible and noneligible car owners following a layoff would manifest prior to Uber's entry into the market. To examine this possible preexisting effect, we make a slight modification to Equation (1). First, we define the quarter that Uber first entered CBSA j by U_j. Following this, we replace the indicator variable *Uber* with a vector of indicator variables that correspond to the event time around Uber's entry, spanning an 8-quarter period around Uber's entry into an area. We assign any quarter occurring outside this range to their respective 'bookend' event time dummies. Thus, this vector of dummies allows us to estimate a separate coefficient of *Layoff* $\times$ *Eligible* in event time around Uber's entry into a CBSA.

[Insert Figure 4 Near Here]

Panel A of Figure 4 illustrates the estimation results when the outcome is $BenefitReceived$, an indicator that takes on a value of one if an individual receives UI benefits in a given month. The figure reports the estimated coefficient of *Layoff* $\times$ *Eligible* interacted with each of the event time indicator variables, with corresponding 95% confidence bands. The estimation results do not support the alternative hypothesis of a preexisting trend prior to Uber's introduction into a market. In contrast, the point estimate on *Layoff* $\times$ *Eligible* is stable and indistinguishable from zero before Uber first enters a market. Panel B presents similar

results when examining the difference in the average dollar amount of UI benefits received by eligible car owners relative to noneligible car owners. Finally, we consider the possibility of preexisting trends when examining the effect on credit balances (Panel C), overall delinquency rates (Panel D), and mortgage delinquencies (Panel E). None of the panels reveal any noticeable signs of a preexisting trend in the months leading up to Uber's entry into a market.

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 credit usage following job loss. Specifically, those individuals most readily able to participate on the ride-sharing platform receive relatively fewer UI benefits, do not draw down as much on untapped credit lines, 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 these outcomes. 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 response to job loss. 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 a broad set of 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. This is consistent with Figure 4, which estimates the differential response of eligible owners in event time around Uber's entry into a market. Recall, the final point estimate includes all observations eight or more quarters after Uber's entry, with statistically significant effects across each outcome we consider.

Alternatively, Uber may simply increase the supply of long-term employment prospects available to a laid-off worker, much in the same way a large generic firm would upon entry into an area. We now consider additional tests to provide better insight into the channel at

play.

First, if Uber simply produces an additional measure of long-term jobs, the firm's entry may lead to a new equilibrium with a lower average level of unemployment. We explicitly consider this possibility by augmenting Equation (1) with an additional variable designed 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 BLS. We allow the effect of unemployment to vary for eligible and noneligible car owners and to vary pre- and post-layoff. In unreported results, we find point estimates remain virtually unchanged from those presented in Section 4 following the inclusion of this control. However, 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 consider a collection of tests motivated by the argument that recently laid-off workers are likely to view the platform as a temporary solution to be used while pursuing superior long-term prospects in the traditional labor market, rather than a permanent form of reemployment. This is consistent with labor market episodes in which the gig economy garnered the attention of both the media and lawmakers in the wake of recent government shutdowns. Coinciding with the October 2013 shutdown, the household errand platform *TaskRabbit* saw a spike in participation with 13k+ applications in one day (Little, 2013). 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). Importantly, traffic controllers and government employees are arguably not likely to view the gig economy as a permanent employment prospect taken in lieu of traditional employment. Instead, these accounts are consistent with the gigeconomy providing easily available short-term employment following an unexpected income shock, supporting the

conjecture that the gig economy changes the structure of labor markets rather than merely producing additional, ubiquitous jobs. Such accounts are also consistent with Koustas (2018), which finds that (non-laidoff) workers supplement lost hours with additional income from gig economy work.

Historical accounts aside, we now seek empirical evidence to support our conjecture. First, to ensure that the previous results are not being driven by individuals who are permanently exiting the traditional work force, we focus on a subsample of workers for which this is not the case. Specifically, we restrict our sample to the set of workers which we observe being reemployed within 12 months of layoff by another firm in our employment data set. This subsample consists of slightly more than 135k workers.[18]

[Insert Table 7 Near Here]

Table 7 repeats the analysis from Section 4 using this restricted sample. The effects of Uber's entry remain economically and statistically significant, with the exception of home delinquencies. Moreover, while point estimates are generally smaller in magnitude relative to our baseline analysis, this is consistent with a reduced hardship of job loss for an individual who is able to more quickly find re-employment, and thus a smaller potential benefit of the gig economy.

Next, we consider a second subset of the population that is less likely to view the ride-sharing platform as a long-term solution to unemployment. Using ZIP code level yearly income statistics from the SOI division of the IRS, we restrict the sample to individuals residing in above-median income ZIP codes. Table OA.2 repeats the previous analysis on this subsample, where the average income is $134k, yielding similar inferences to Table 7. Overall, these results are consistent with a reduction in labor market frictions for a

18. Note, we are unable to observe reemployment in instances where the individual receives a new job at a firm which does not subscribe to Equifax's employment and income verification services. Thus, the excluded sample also includes workers that find reemployment within 12 months at noncovered firms. For this reason, inferring reemployment rates from the change in sample size understates the likelihood that a worker finds traditional reemployment within 12 months. See Kalda (2019) for a detailed description and discussion on representativeness of this employment data.

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 treated as a short-term means of buffering an income shock, this suggests that a recently laid off worker is substituting away from other short-term options (e.g., claiming UI). With this channel, we would expect a laid-off worker to evaluate ride-sharing participation against her outside option, which includes benefits received from UI. 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 as motivation for a test which exploits differences in the maximum amount of weekly UI benefits paid out across states. If laid-off workers view the ride-sharing platform as a substitute for income provided by UI, a worker is likely to choose Uber more often when expected UI benefits are lower. This implies that the effects we document above should be stronger when UI benefits are less generous.

To test this idea formally, we collect the maximum amount of UI benefits paid across states for each year in our sample. 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. We then partition the sample based on the median value of weekly benefits across states for each year.

Table 8 presents the results. Panel A of the table focuses on UI participation rates. When examining the receipt of any UI benefits (columns 1 and 3) and the dollar amount of UI benefits (columns 2 and 4), the point estimates are significant in states with below-median maximum UI benefits. In contrast, the point estimates are statistically insignificant in states with more generous UI limits. In Panel B of Table 8, we turn to the effects on credit usage and find point estimates that suggest a larger effect of Uber's introduction in states where

expected UI benefits are lower. Finally, Panel C of Table 8 reveals that when we examine the effects on credit delinquencies, the point estimates imply statistically significant effects of Uber's entry for eligible car owners only in the subsample of states with a smaller expected benefit.[19]

[Insert Table 8 Near Here]

Taken together, the results in this section are consistent with gig labor changing the structure of labor markets, increasing the liquidity provided by short-term jobs rather than simply increasing the supply of generic long-term jobs.

6. External Validity and Potential Implications

This section begins by presenting results from a similar empirical strategy that allows us to consider a broader sample of the population. We follow this with a brief set of analysis and discussion on potential implications for labor market outcomes.

6.1. External Validity

Our primary identification strategy allows us to exploit differential responses of laid-off workers to Uber entry among individuals with access to a vehicle between 10 and 20 years of age. Yet, doing so trades off better identification by focusing on a select subset of our sample. We now briefly present results from a related empirical strategy that allows us to consider a broader share of the population.

Rather than relying on car registration data to identify ownership of a vehicle between 10 and 20 years of age, we instead contrast the response of laid off workers for which we proxy as more broadly having access to a vehicle. Specifically, we use an individual's credit history in an attempt to identify car owners. We classify anyone with an auto loan or

19. Formal test indicates a difference in coefficients across the two subsamples that is statistically significant near traditional levels for credit usage and delinquency outcomes, with p-values ranging from 0.04 to 0.11. In contrast, we cannot rule out similar sized effects for UI outcomes from the first panel.

lease between 2000 and the month before getting laid off as being a car owner, *Owner*. By considering all individuals with an observable credit history, rather than those who own a car within a specific car age range, we are able to consider a much wider set of laid-off workers. From the initial set of 19.6M identified layoffs with credit records, we randomly select 1M layoffs, necessitated by computational restrictions put in place by the host site where the data resides. After restricting the sample to our time period and treated CBSAs, we are left with 495k layoff events. With this new sample, the only modification we make to our estimating equation is to replace *EligibleOwner* with *Owner*, yielding the following estimating equation:

$$\begin{aligned} y_{ijt} = \eta \times Layoff_{ijt} \times Uber_{jt} \times Owner_{ijt} + \beta_{jt} \times Layoff_{ijt} \\ + \delta_j \times Layoff_{ijt} \times Owner_{ijt} + \mu_j \times Uber_{jt} \times Owner_{ijt} + \phi_i + \varepsilon_{ijt}. \end{aligned} \tag{2}$$

With this regression specification, we reestimate our baseline findings, now examining the effect of Uber's entry on car owners relative to nonowners following a layoff. Panel A of Table 9 considers the effect on a worker's propensity of claim UI benefits. In the first specification, the coefficient of -0.30, significant at the 1% level, indicates that likelihood of a car owner receiving UI benefits (relative to a nonowner) in a given month following job loss decreases by 30 basis points when Uber operates in the individual's CBSA. For reference, the average post-layoff likelihood of a car owner receiving benefits in this sample is 3.24%. Thus, the triple-interaction estimate represents a 9.5% relative decrease. The second specification reports consistent results when considering the change in the dollar amount of UI benefits received per month, with Uber's entry being associated with a \$3.55 average decrease for car owners.

Panel B of Table 9 turns to the second means by which an individual may weather a job loss, increased credit usage. When considering all forms of credit in the first specification, the point estimate indicates a relative decrease in car owner response of roughly \$886 following Uber's entry into a local market. The remaining specifications consider individual types of

credit, with consistent results to those in Table 5. Namely, there is a modest reduction in credit card balances, while mortgage-related balances exhibit a larger response.

Finally, Panel C turns to the effect on credit delinquencies. When considering all forms of credit in the first specification, the triple-interaction coefficient indicates a 0.48pp relative decrease in post-layoff delinquencies of car owners. Benchmarked against the average post-layoff delinquency rate for car owners in this sample of 18.04pp, this represents a 2.6% relative decrease. When distinguishing between types of credit, we see that Uber's entry reduced relative post-layoff delinquency rates of car owners for both credit cards and home-related credit claims.

In summary, the previous table presents results consistent with the findings presented in Section 4 when considering a more general sample of laid-off workers. Given the change in both sample and workers posited as being more or less able to participate in a ride-sharing platform, we repeat the exercises designed to validate our empirical approach, with results reported in the Online Appendix. Specifically, we do not find a change in the characteristics of laid-off car owners relative to nonowners (Table OA.3), nor a noticeable differences in the layoff rate across the two groups (Figure OA.4) following Uber's entry. Additionally, Figure OA.5 does not indicate a change in the response of car owners relative to nonowners leading up to Uber's entry into an area (pre-trend test).

In the next test, we consider a plausible concern associated with our broader sample, in which there is a general difference between car owners and nonowners and that this inherent difference manifests itself after Uber enters a local market. In light of this, we consider two sets of closely related individuals who both have access to a vehicle: those financing their car through 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 likely will impair a lessee's ability to operate on a ride-sharing platform in any significant capacity, providing a useful placebo

group.

[Insert Table 10 Near Here]

Table 10 presents the results when bisecting 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* subgroup. In Panel A, we find an effect of Uber's entry on UI participation rates for individuals in the *Without Auto Lease* subgroup which is approximately 50% larger in magnitude relative to the sample of workers belonging to the *With Auto Lease* subgroup. The second set of specifications indicates a larger difference when examining the nominal amount of benefits received.[20]

In the final two panels we consider the effects on credit usage (Panel B) and performance (Panel C). Across the two panels, when jointly considering all forms of credit, the differential response appears to be larger for individuals without auto leases, with a statistically significant difference in coefficients at traditional levels. In contrast, we do not find differences when considering mortgage-related delinquency rates, with statistically insignificant effects in both subgroups. However, this final result may be due to limited power in the subsamples, as the previous table demonstrates a statistically significant effect when not distinguishing between the two subgroups.

Ultimately, the results based on this alternative strategy provide evidence consistent with the findings presented in Section 4 in a more general sample.

6.2. Potential Implications

Taken together, the results presented thus far are consistent with some laid-off workers substituting away from UI programs and credit consumption following the introduction of the gig economy into an area. If traditional responses to job loss (e.g., UI participation)

20. A formal test of the difference in coefficients yields p-values of 0.11 and 0.19, respectively.

are imperfect means by which workers buffer lost income, and some workers view the gig economy as providing a more suitable replacement, it is plausible that the introduction of the gig economy also alters a worker's behavior in seeking reemployment. More precisely, if workers view the gig economy as reducing the cost of being unemployed, its introduction would allow a recently laid-off worker to extend her unemployment spell in search of more suitable job.

We now briefly examine two outcomes related to this conjecture. Specifically, using our employment data, we examine the differential effect of Uber's entry on the average wage and unemployment spell of laid-off workers. As this sample consists of a single observation per unemployment spell, we modify Equation (1) to exclude all terms including $Layoff$, yielding a traditional difference-in-differences estimator.

Table 11 presents results from the difference-in-differences framework, where the variable of interest is the interaction of $Uber$ and $EligibleOwner$. We begin by considering the monthly income earned by a laid-off worker upon reemployment in the first specification. The coefficient on the interaction term indicates that following Uber's entry, the average monthly income of eligible car owners increases by \$80.92 relative to that of noneligible car owners. This represents a 2.6% increase when contrasted against the average monthly income of approximately \$3,103 in this sample. In the second specification, we turn to the duration of a laid-off worker's unemployment spell, measured in months. Consistent with an increase in search times, the duration of the unemployment spell increases by 0.24 months for eligible car owners following Uber's entry, or 3.26% of the unconditional mean in the sample.

[Insert Table 11 Near Here]

The previous results are consistent with the gig economy altering the job-seeking behavior of laid-off workers, potentially allowing workers to search for a higher quality match. However, we caution in drawing strong inferences from these results. In similar fashion to Table 7, one caveat to note is that we are unable to observe reemployment in instances where the individual receives a new job at a firm which does not subscribe to Equifax's employment

and income verification services. Thus, the excluded sample also includes workers who find reemployment within 12 months at noncovered firms. To the extent this sample selection correlates with car age or Uber's entry, it may affect our findings.

7. Conclusion

This paper highlights the role of 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 eligible car owners are 3.8% less likely to lean on UI programs following job loss in areas where the ride-sharing firm is present. Moreover, the introduction of Uber has a significant effect on household leverage outcomes. Eligible car owners increase their outstanding debt balances by 1.2% less than they otherwise would following Uber's entry, while relative delinquency rates fall by 3.8%. In support of our identify assumption, we do not find evidence of a pretrend in which eligible car owners outcomes deviate from noneligible car owners in the quarters leading up to Uber's entry into an area.

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 systematic evidence for this economic channel from a series of empirical tests. Our results hold when restricting the sample to laid-off workers whom we observe as being reemployed in the traditional workforce within 12 months of layoff, and when restricting the sample to high-income areas, both groups are less likely to view Uber as a long-term job prospect. Moreover, the effects of Uber's entry are stronger in states with less generous UI benefits. This is consistent with workers weighing the tradeoff between two short-term options, the gig economy and UI.

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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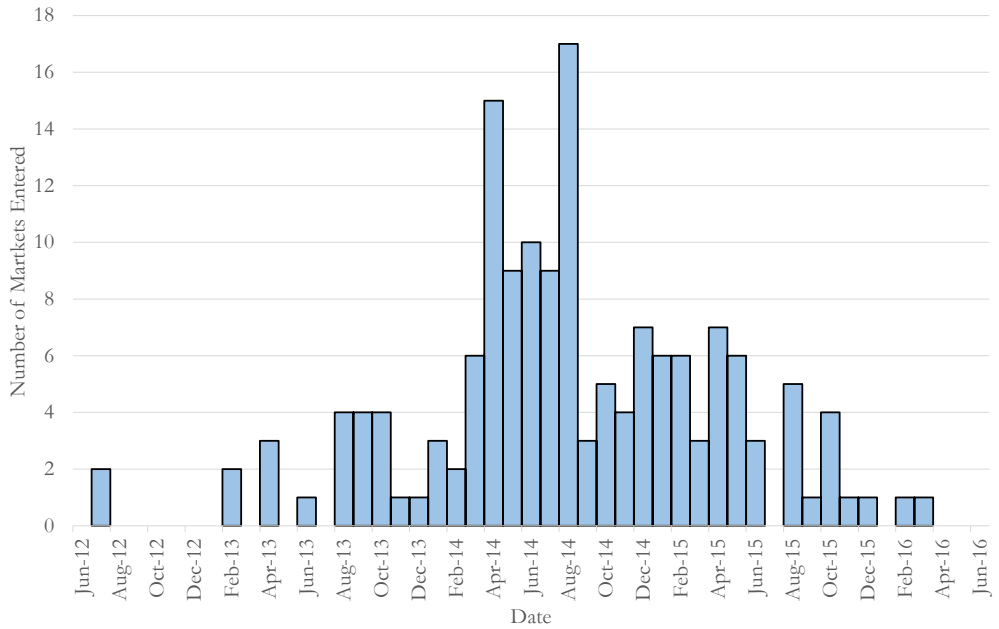
Panel A: Uber Entry through Time



Panel B: Uber Entry across States

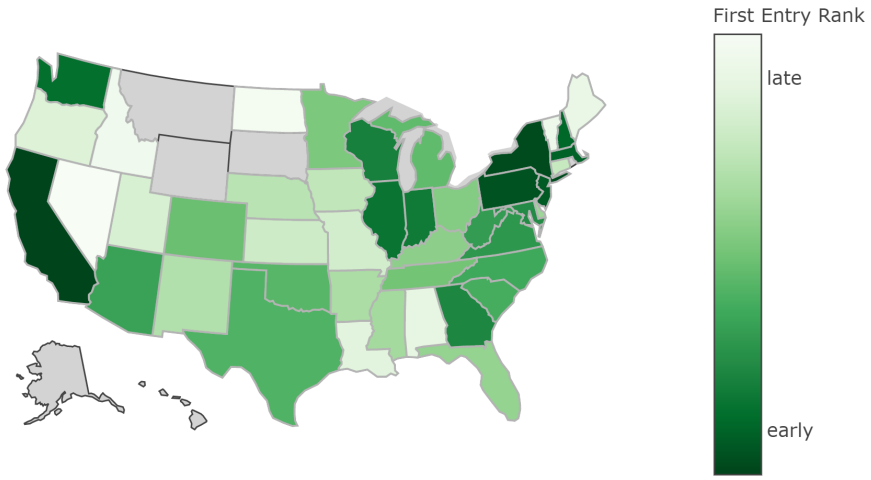


Fig. 1. Timing of Uber Entry
This figure illustrates the variation in Uber's entry into markets. Panel A reports the number of markets entered through time. Panel B presents a choropleth (geographic heat map) of the order of Uber's first entry into a state.

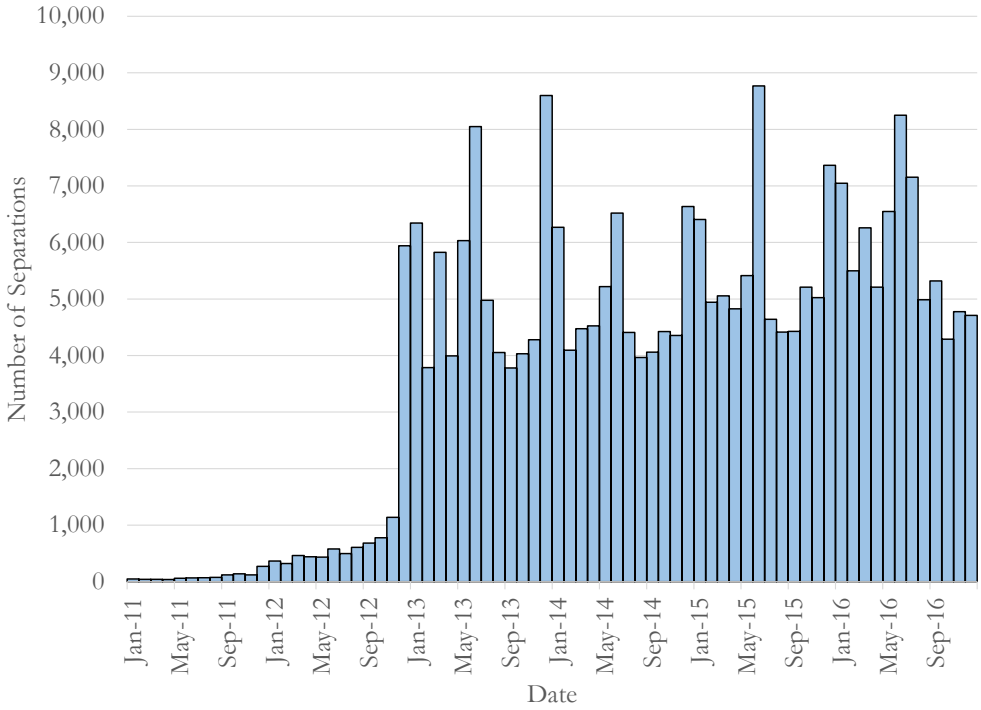


Fig. 2. Layoffs through Time

This figure illustrates the time-series variation in our sample of worker layoffs. The figure reports the monthly total of job separations we classify as a layoffs for workers matched to the car registration data. The totals reflect all sample restrictions.

Panel A: Reception of UI Benefit

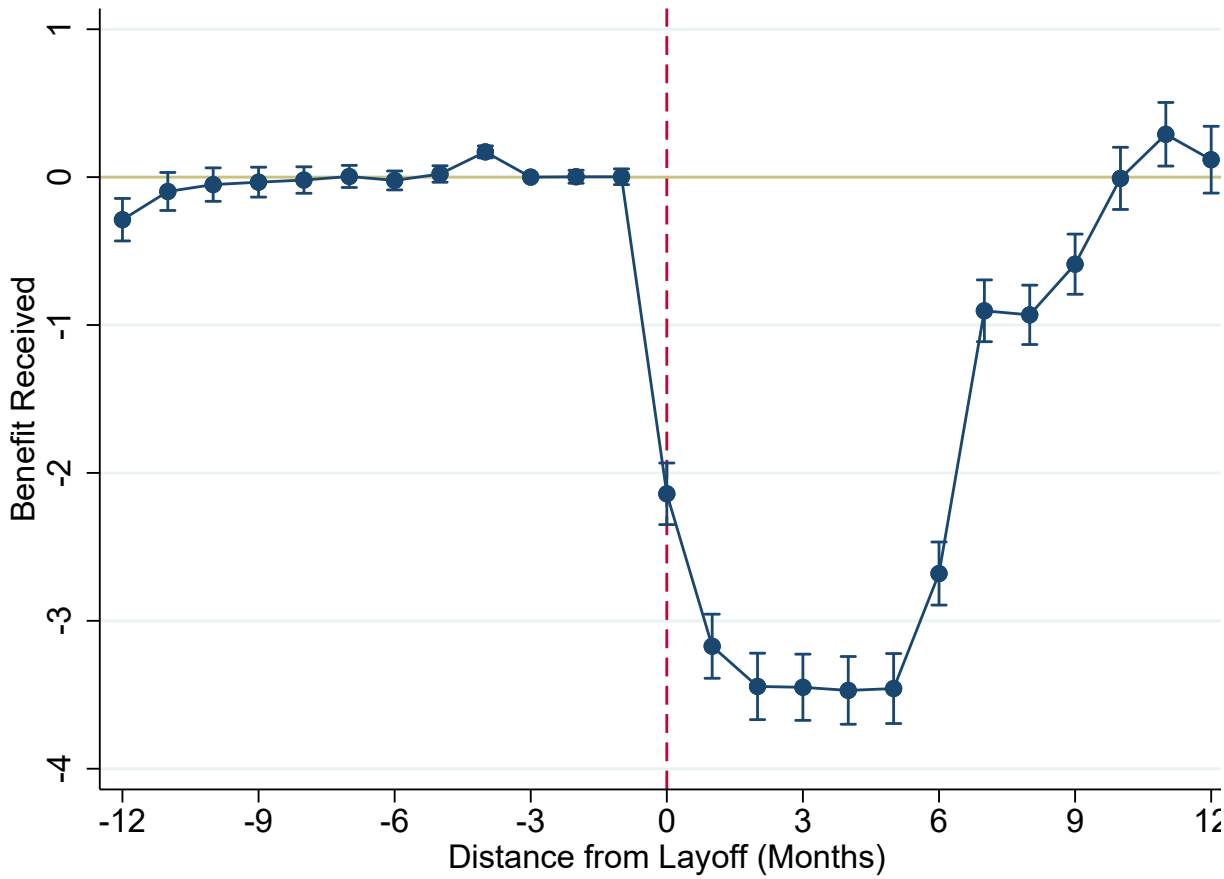
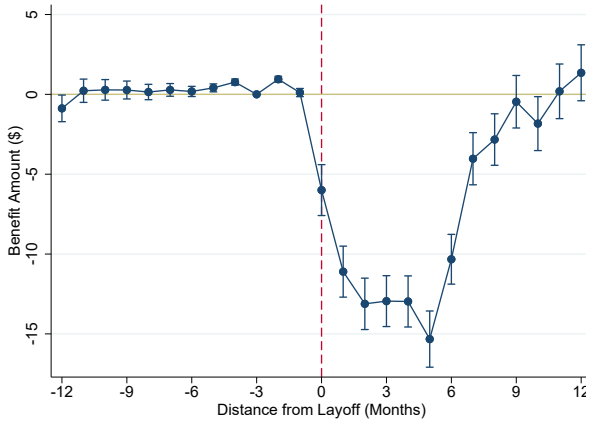
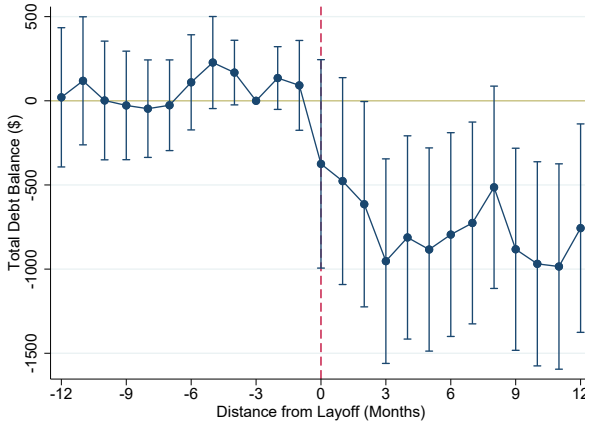


Fig. 3. Changes in Outcomes around Job Separations
This figure reports the difference in outcome variables for eligible car owners relative to non-eligible owners around job separation. The figure is based on a modified version of Equation (1), where the indicator for being laid off is replaced with a vector of dummies corresponding to event-time (in months) around being laid off. The figure reports coefficients of the triple interaction of *EligibleOwner*, *Uber*, and the event-time dummies, with corresponding 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).

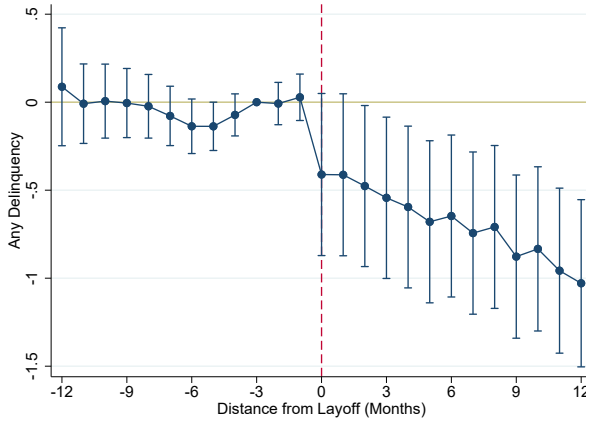
Panel B: UI Benefit Amounts



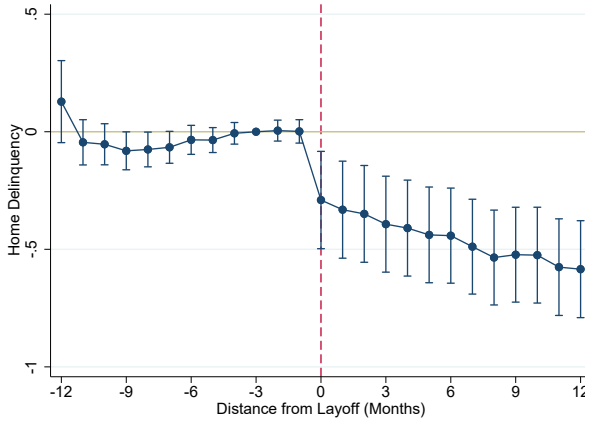
Panel C: Outstanding Credit Balances



Panel D: Delinquency on any Credit Type



Panel E: Delinquency on Mortgage Loans



Panel A: Reception of UI Benefit

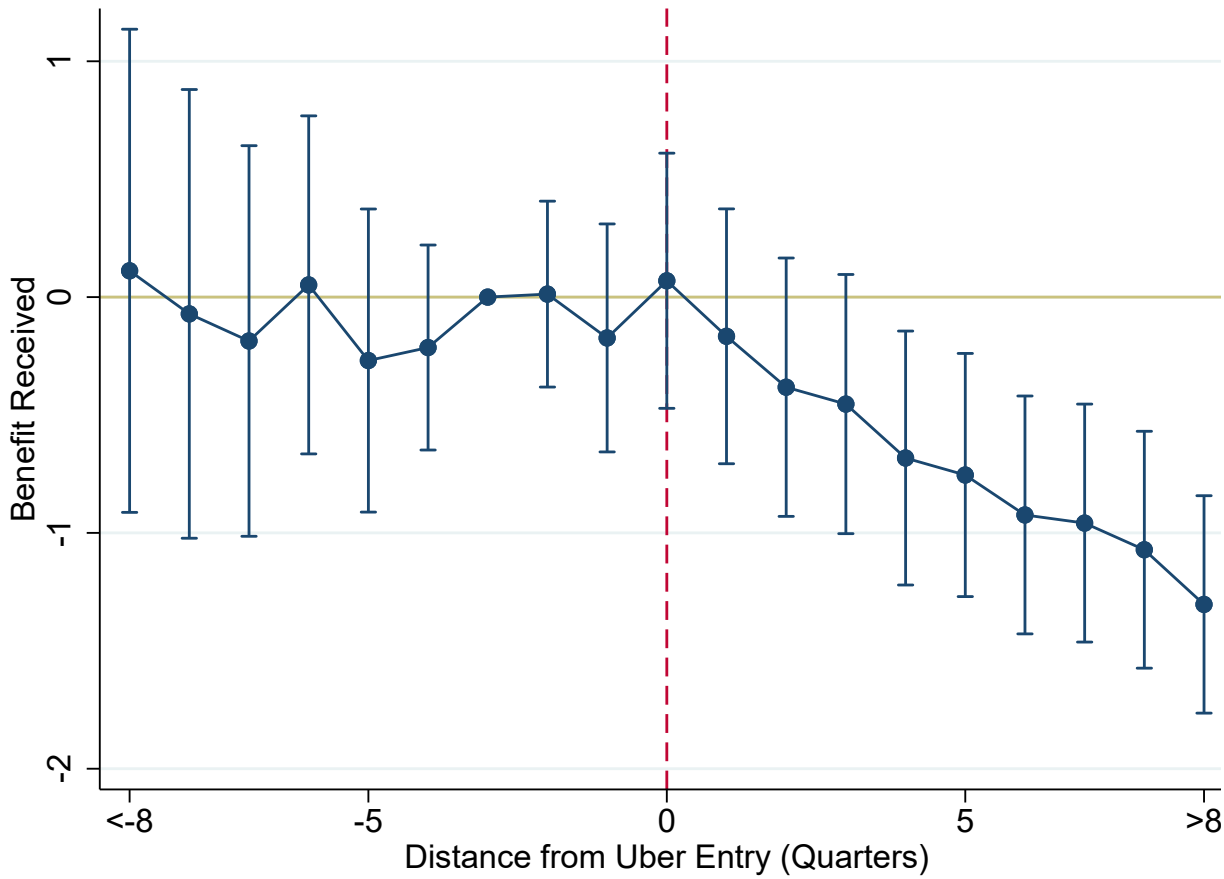
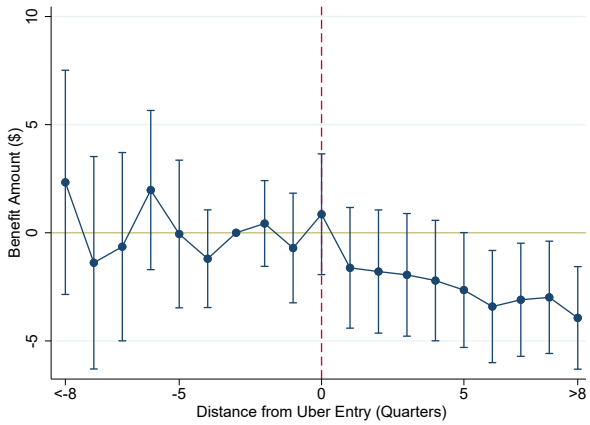
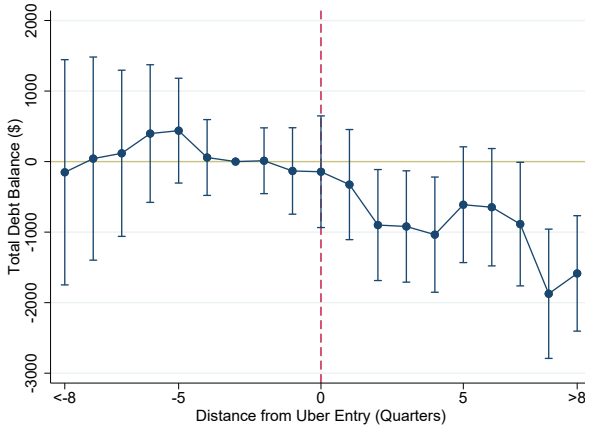


Fig. 4. Changes in Outcomes around Uber's entry
This figure reports the estimated effect of *Layoff* × *Eligible* on outcome variables in the months around Uber's entry into a market. We define the month that Uber first entered CBSA j by U_j. Following this, we remove the indicator variable $Uber_{jt}$, denoting Uber's presence in CBSA j as of month t from Equation (1). In its place, we include a vector of 17 indicator variables that correspond to the event time around Uber's entry (in quarters), ranging from $U_j - 8$ to $U_j + 8$. We assign any month occurring prior to $U_j - 8$, and any month subsequent to $U_j + 8$, to their respective "book-end" event time fixed effects. 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).

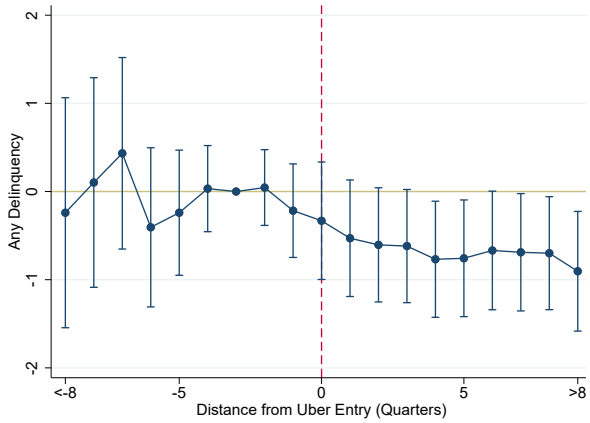
Panel B: UI Benefit Amounts



Panel C: Outstanding Credit Balances



Panel D: Delinquency on any Credit Type



Panel E: Delinquency on Mortgage Loans

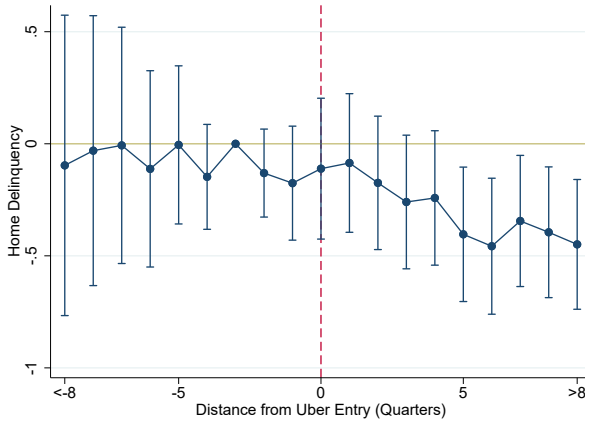


Table 1. Summary Statistics
This table reports summary statistics for our main sample.

Statistic	N	Mean	St. Dev.	Min	Median	Max
Benefit Received (%)	6,535,412	2.643	16.041	0.000	0.000	100.000
Benefit Amount ($)	6,535,412	12.546	82.657	0.000	0.000	618.050
Number of Accounts	6,535,412	7.875	6.065	0.000	7.000	28.000
Number of Credit Card Accounts	6,535,412	2.272	2.498	0.000	2.000	11.000
Number of Home Loans	6,535,412	0.612	0.993	0.000	0.000	4.000
Total Debt ($)	6,535,412	66,310	102,840	0	18,209	495,663
Credit Card Debt ($)	6,535,412	2,876	6,079	0	350	34,325
Home Loans ($)	6,535,412	44,891	91,201	0	0	427,954
Credit Score	6,535,412	621.187	109.555	300.000	611.000	839.000
Any Delinquency (%)	6,535,412	12.556	33.135	0.000	0.000	100.000
Credit Card Delinquency (%)	6,535,412	6.462	24.585	0.000	0.000	100.000
Home Loans Delinquency (%)	6,535,412	1.826	13.387	0.000	0.000	100.000

Table 2. Observable Characteristics at the Time of Layoffs.
This table reports the results of OLS regressions when modifying Equation (1) to exclude all interaction terms involving $Layoff$. The sample is made up of one observation per laid-off worker, measuring the worker's characteristics for the month prior to layoff. 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:	Credit Score	Debt ($)			Past Delinquencies (%)		
		Total	Credit Card	Home Loans	Total	Credit Card	Home Loans
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Eligible × Uber	0.110	1,105	58	753	0.096	0.131	-0.051
	(1.043)	(864)	(55)	(766)	(0.251)	(0.166)	(0.065)
N	263,591	263,591	263,591	263,591	263,591	263,591	263,591
	0.086	0.099	0.058	0.089	0.048	0.037	0.029
Fixed effects:							
Car age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 3. Changes in Outcome Variables around Layoffs

This table reports averages of main outcome variables before and after layoffs in various sub-samples. Each panel reports averages during the pre-layoff period (12 months prior to layoff) and post-layoff period (layoff month and the following 12 months). Panel A reports full sample results. Panel B reports the results for one-dimensional sample cuts based on car eligibility and Uber's entry. Panel C reports the results for two-dimensional sample cuts based on car eligibility and Uber's entry.

Dependent variable:	N	Benefit		Debt ($)			Delinquency (%)		
		Received (%)	Amount ($)	Total	Credit Card	Home Loans	Total	Credit Card	Home Loans
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Full Sample									
Pre-layoff	3,262,618	0.790	3.189	64,664	2,803	43,356	11.427	5.664	1.385
Post-layoff	3,272,794	4.490	21.874	67,962	2,949	46,431	13.688	7.263	2.267
Panel B: One-dimensional Cuts									
1(Uber)=1									
Pre-layoff	1,956,188	0.475	1.925	66,270	2,877	44,643	11.069	5.583	1.209
Post-layoff	2,601,594	4.268	21.168	71,398	3,111	49,119	12.365	6.529	1.715
1(Uber)=0									
Pre-layoff	1,306,430	1.262	5.081	58,439	2,516	38,369	12.814	5.974	2.066
Post-layoff	671,200	5.349	24.611	62,817	2,706	42,405	15.670	8.362	3.094
1(Eligible)=1									
Pre-layoff	2,425,486	0.796	3.206	67,780	2,939	45,903	11.557	5.626	1.415
Post-layoff	2,434,416	4.593	22.524	71,235	3,089	49,105	13.664	7.172	2.297
1(Eligible)=0									
Pre-layoff	837,132	0.775	3.139	55,616	2,408	35,961	11.050	5.773	1.299
Post-layoff	838,378	4.189	19.987	58,479	2,543	38,681	13.758	7.527	2.182
Panel C: Two-dimensional Cuts									
1(Eligible)=1, 1(Uber)=1									
Pre-layoff	1,450,479	0.483	1.956	69,491	3,021	47,301	11.174	5.538	1.240
Post-layoff	1,934,976	4.380	21.873	75,018	3,268	52,125	12.354	6.465	1.747
1(Eligible)=1, 1(Uber)=0									
Pre-layoff	975,007	1.260	5.066	61,148	2,621	40,489	13.041	5.966	2.091
Post-layoff	499,440	5.420	25.047	65,607	2,822	44,613	15.615	8.223	3.115
1(Eligible)=0, 1(Uber)=1									
Pre-layoff	505,709	0.452	1.837	56,917	2,459	36,929	10.765	5.715	1.120
Post-layoff	666,618	3.944	19.123	61,016	2,660	40,496	12.398	6.713	1.624
1(Eligible)=0, 1(Uber)=0									
Pre-layoff	331,423	1.267	5.127	50,563	2,212	32,205	12.155	5.998	1.991
Post-layoff	171,760	5.141	23.343	54,608	2,364	35,912	15.833	8.769	3.034

Table 4. Effects on UI Participation

This table reports the results of OLS regressions of the form described in Equation (1). $1(ReceivedBenefit)$ 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. *Eligible* is an indicator variable which takes on a value of one for workers whose cars are less than 15 years in age during the month prior to 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
	(1)	(2)
Layoff × Eligible × Uber	-0.171**	-1.829***
	(0.081)	(0.456)
Obs.	6,532,733	6,532,733
Adj. R^2	0.30	0.28
Fixed effects:		
Individual FE	Yes	Yes
City-Month-Layoff FE	Yes	Yes
Car age-City-Layoff FE	Yes	Yes
Car age-City-Uber FE	Yes	Yes

Table 5. Effects on Credit Outcomes
This table reports the results of OLS regressions of the form described in Equation (1). Panel A examines the outstanding balance on credit lines, while Panel B examines the number of open lines of credit for an individual. All other variables are described in 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 Account Balances

	Total Debt (1)	Credit Card (2)	Home Loans (3)
Layoff × Eligible × Uber	-789.853***	-41.754*	-507.505**
	(280.535)	(23.665)	(252.958)
Obs.	6,532,733	6,532,733	6,532,733
Adj. R^2	0.90	0.87	0.89
Fixed effects:			
Individual FE	Yes	Yes	Yes
City-Month-Layoff FE	Yes	Yes	Yes
Car age-City-Layoff FE	Yes	Yes	Yes
Car age-City-Uber FE	Yes	Yes	Yes

Panel B: Effect on Number of Open Accounts

	All Accounts (1)	Credit Card (2)	Home Loans (3)
Layoff × Eligible × Uber	-0.053***	-0.030***	-0.010***
	(0.019)	(0.007)	(0.003)
Obs.	6,532,733	6,532,733	6,532,733
Adj. R^2	0.93	0.93	0.93
Fixed effects:			
Individual FE	Yes	Yes	Yes
City-Month-Layoff FE	Yes	Yes	Yes
Car age-City-Layoff FE	Yes	Yes	Yes
Car age-City-Uber FE	Yes	Yes	Yes

Table 6. Effects on Delinquency Rates
This table reports the results of OLS regressions of the form described in Equation (1). 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 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.

	Any Delinquency (1)	Credit Card (2)	Home Loans (3)
Layoff × Eligible × Uber	-0.516**	-0.514***	-0.012*
	(0.227)	(0.172)	(0.007)
Obs.	6,532,733	6,532,733	6,532,733
Adj. R^2	0.55	0.53	0.48
Fixed effects:			
Individual FE	Yes	Yes	Yes
City-Month-Layoff FE	Yes	Yes	Yes
Car age-City-Layoff FE	Yes	Yes	Yes
Car age-City-Uber FE	Yes	Yes	Yes

Table 7. Re-employed within 12 Months
This table reports the results of OLS regressions of the form described in Equation (1). The sample is restricted to individuals obtaining a new job at a firm in our sample within 12 months of the initial job loss. All outcome variables are described in Tables 4, 5, and 6. 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) (1)	Benefit Amount (2)	Total Debt (3)	Home Loans (4)	Any Delinquency (5)	Home Delinquency (6)
Layoff × Eligible × Uber	-0.107**	-0.504**	-608.236***	-446.507*	-0.277***	0.174
	(0.046)	(0.227)	(217.885)	(229.520)	(0.114)	(0.155)
Obs.	2,498,576	2,498,576	2,498,576	2,498,576	2,498,576	2,498,576
Adj. R^2	0.307	0.279	0.901	0.894	0.549	0.473
Fixed effects:						
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
City-Month-Layoff FE	Yes	Yes	Yes	Yes	Yes	Yes
Car age-City-Layoff FE	Yes	Yes	Yes	Yes	Yes	Yes
Car age-City-Uber FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 8. Cross-Sectional Effects: UI Benefits
This table reports the results of OLS regressions of the form described in Equation (1). Considered are above and below median sample splits based on the maximum amount of weekly UI benefits paid across states. 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 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 Uptake

Max UI Benefits:	Below Median		Above Median	
	1(Received Benefit)	Benefit Amount	1(Received Benefit)	Benefit Amount
	(1)	(2)	(3)	(4)
Layoff × Eligible × Uber	-0.318*	-1.600*	-0.213	-0.988
	(0.169)	(0.847)	(0.262)	(1.438)
Obs.	3,322,148	3,322,148	3,209,404	3,209,404
Adj. R^2	0.283	0.262	0.319	0.300
Fixed effects:				
Individual FE	Yes	Yes	Yes	Yes
City-Month-Layoff FE	Yes	Yes	Yes	Yes
Car age-City-Layoff FE	Yes	Yes	Yes	Yes
Car age-City-Uber FE	Yes	Yes	Yes	Yes

Panel B: Effect on Account Balances

Max UI Benefits:	Below Median		Above Median	
	Total Debt (1)	Home Loans (2)	Total Debt (3)	Home Loans (4)
Layoff × Eligible × Uber	-1,519***	-907*	-408	-292
	(512)	(464)	(379)	(341)
Obs.	3,322,148	3,322,148	3,209,404	3,209,404
Adj. R^2	0.906	0.898	0.895	0.888
Fixed effects:				
Individual FE	Yes	Yes	Yes	Yes
City-Month-Layoff FE	Yes	Yes	Yes	Yes
Car age-City-Layoff FE	Yes	Yes	Yes	Yes
Car age-City-Uber FE	Yes	Yes	Yes	Yes

Panel C: Effect on Delinquency Rates

Max UI Benefits:	Below Median		Above Median	
	Any Delinquency (1)	Home Delinquency (2)	Any Delinquency (3)	Home Delinquency (4)
Layoff × Eligible × Uber	-0.987***	0.102	-0.266	-0.165
	(0.319)	(0.140)	(0.378)	(0.177)
Obs.	3,322,148	3,322,148	3,209,404	3,209,404
Adj. R^2	0.547	0.473	0.550	0.481
Fixed effects:				
Individual FE	Yes	Yes	Yes	Yes
City-Month-Layoff FE	Yes	Yes	Yes	Yes
Car age-City-Layoff FE	Yes	Yes	Yes	Yes
Car age-City-Uber FE	Yes	Yes	Yes	Yes

Table 9. Evidence from Broader Sample
This table reports the results of OLS regressions of the form described in Equation (2). The dependent variables considered include UI related outcomes (Panel A), credit usage (Panel B), and credit delinquency rates (Panel C). *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. All other variables are defined in the previous tables. 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)
Obs.	12,277,167	12,277,167
Adj. R^2	0.24	0.18
Fixed effects:		
Individual FE	Yes	Yes
City-Month FE	Yes	Yes

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)
Obs.	12,277,167	12,277,167	12,277,167
Adj. R^2	0.92	0.79	0.92
Fixed effects:			
Individual FE	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes

Panel C: Effect on Delinquency Rates

	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)
Obs.	12,277,167	12,277,167	12,277,167
Adj. R^2	0.57	0.60	0.51
Fixed effects:			
Individual FE	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes

Table 10. 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 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 Uptake

	Without Auto Lease Accounts		With Auto Lease Accounts	
	1(Received Benefit)	Benefit Amount	1(Received Benefit)	Benefit Amount
Layoff × Carowner × Uber	-0.47***	-5.54***	-0.12	0.24
	(0.09)	(1.45)	(0.10)	(1.51)
Layoff × Carowner	1.08***	21.05***	1.25***	22.67***
	(0.09)	(1.31)	(0.09)	(1.35)
Layoff × Uber	-0.02	5.97***	0.09	7.29***
	(0.09)	(1.05)	(0.09)	(1.09)
Carowner × Uber	0.06	-1.49*	-0.33***	-7.59***
	(0.06)	(0.88)	(0.07)	(0.92)
Layoff	4.43***	47.98***	4.60***	50.84***
	(0.10)	(1.17)	(0.11)	(1.23)
Obs.	9,164,395	9,164,395	9,404,960	9,404,960
Adj. R^2	0.28	0.22	0.27	0.22
Fixed effects:				
Individual FE	Yes	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes	Yes

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	-650.47***	-547.64***	-543.16***	-135.90
	(208.13)	(104.61)	(174.48)	(97.13)
Layoff × Carowner	-780.04***	-445.36***	384.21***	-246.24***
	(180.67)	(93.11)	(146.61)	(87.08)
Layoff × Uber	127.75*	64.86	164.83**	25.58
	(73.43)	(39.85)	(64.07)	(38.10)
Carowner × Uber	-525.19***	11.05	233.35**	-3.52
	(138.27)	(91.61)	(114.25)	(81.11)
Layoff	490.68***	212.57***	-204.17***	108.49***
	(67.12)	(35.47)	(57.06)	(34.57)
Obs.	8,464,016	8,464,016	8,596,328	8,596,328
Adj. R^2	0.92	0.92	0.93	0.92
Fixed effects:				
Individual FE	Yes	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes	Yes

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	-1.09***	-0.041**	0.13	0.04
	(0.18)	(0.02)	(0.17)	(0.07)
Layoff × Carowner	1.48***	0.001	0.78***	-0.04
	(0.16)	(0.06)	(0.14)	(0.06)
Layoff × Uber	0.266***	0.025	0.002	0.04
	(0.09)	(0.03)	(0.09)	(0.03)
Carowner × Uber	0.75***	-0.04	0.14	-0.04
	(0.15)	(0.05)	(0.15)	(0.06)
Layoff	-0.51***	-0.03	-0.31***	-0.03
	(0.09)	(0.03)	(0.08)	(0.03)
Obs.	8,945,785	8,945,785	8,714,693	8,714,693
Adj. R^2	0.59	0.54	0.59	0.55
Fixed effects:				
Individual FE	Yes	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes	Yes

Table 11. Labor Market Outcomes
This table reports the results of OLS regressions when modifying Equation (1) to exclude all interaction terms involving $Layoff$. The sample is made up of one observation per laid-off worker, measuring the worker's labor outcomes in the month of joining a new job within covered firms. 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.

	Income ($) (1)	Time to Next Job (2)
Eligible × Uber	80.92***	0.239**
	(24.03)	(0.111)
Eligible	48.61	-0.107
	(35.31)	(0.116)
Obs.	135,132	135,132
Adj. R^2	0.208	0.123
Fixed effects:		
City-Month FE	Yes	Yes

Gig-Labor: Trading Safety Nets for Steering Wheels

Online Appendix

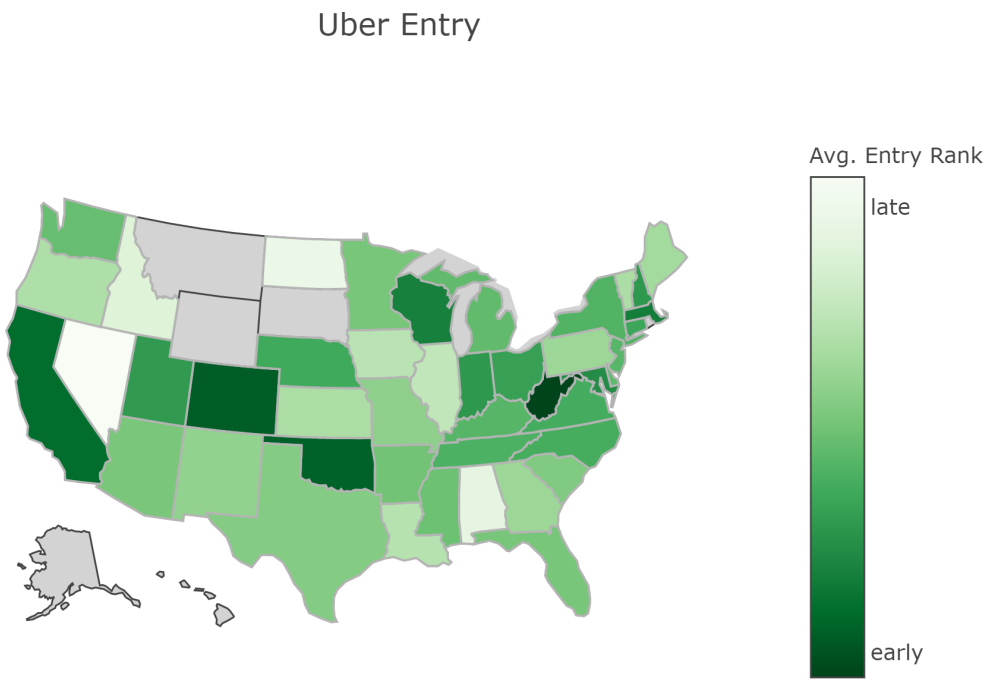


Figure OA.1. Uber Entry across States

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.

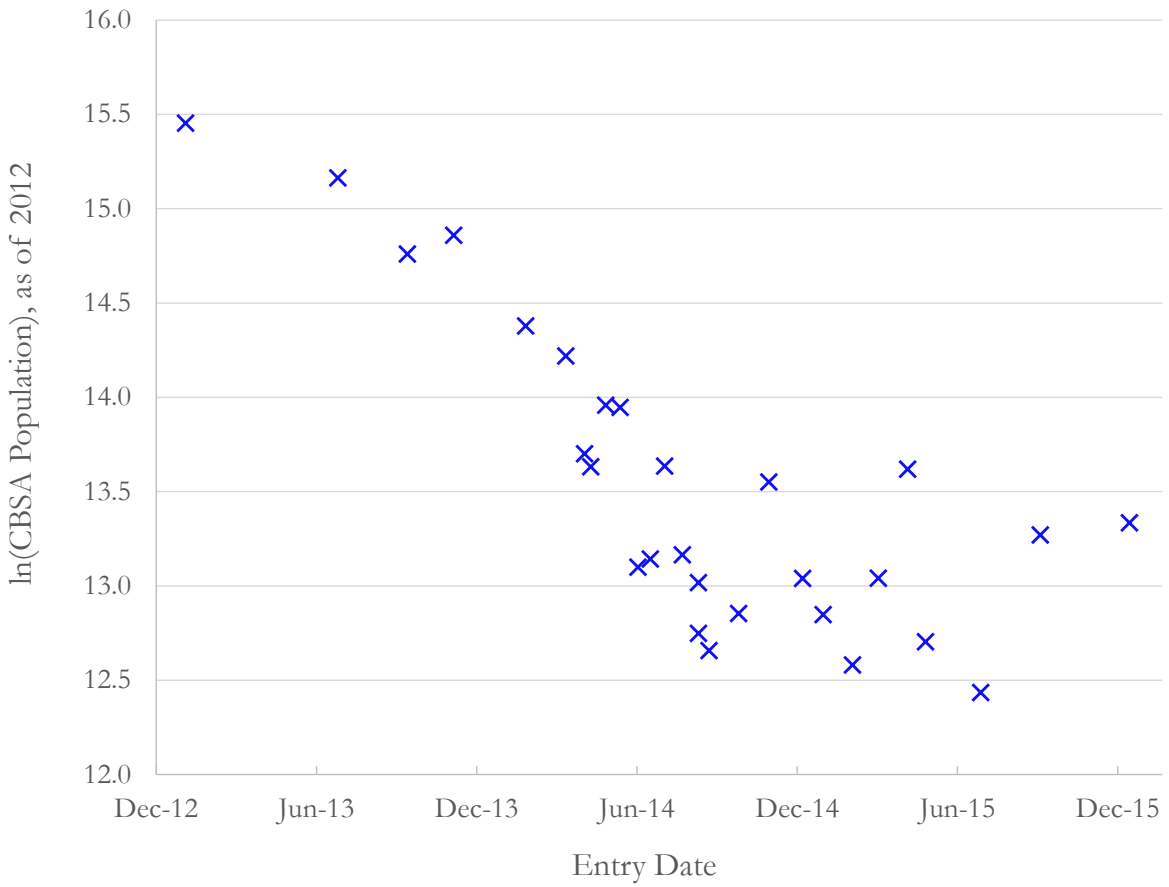


Figure OA.2. Uber Entry Dates and Area Population

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.

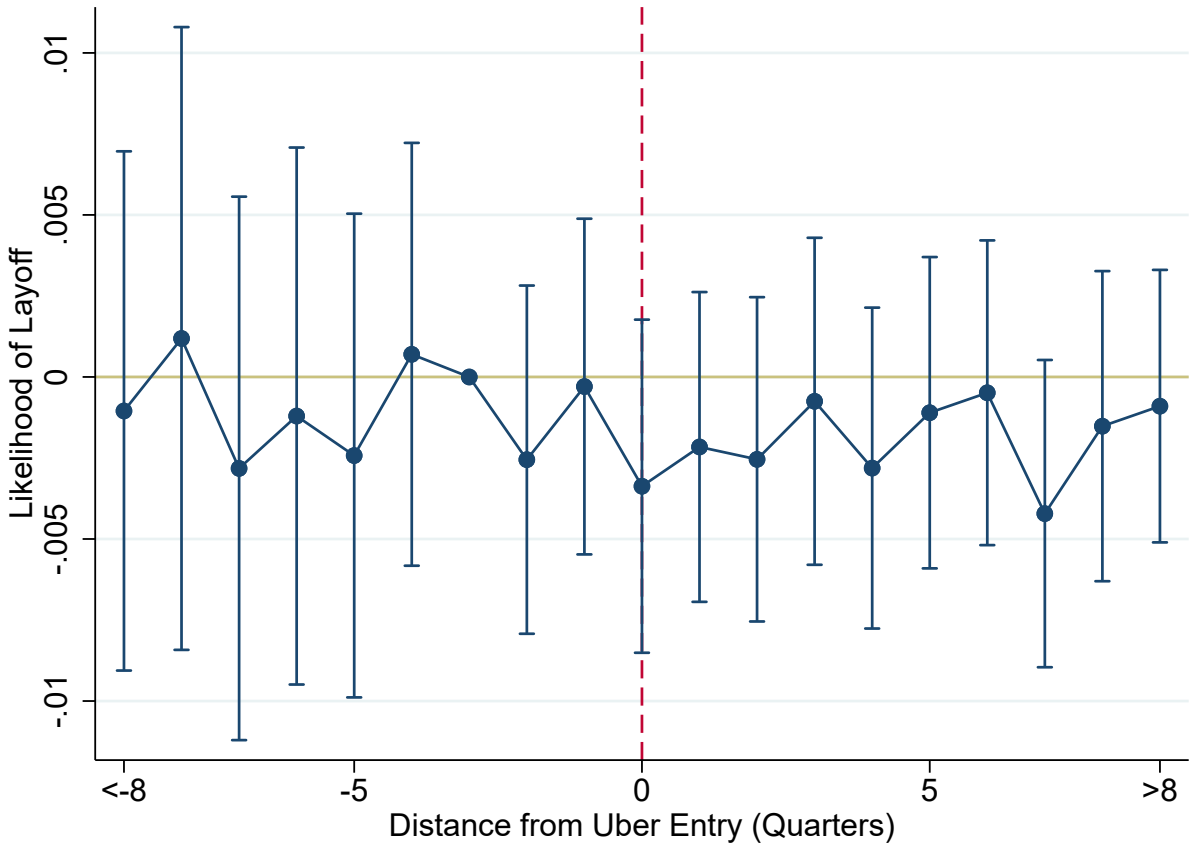


Figure OA.3. Layoff Likelihood around Uber's Entry

This figure reports the estimated effect of *Layoff* × *Eligible* on the likelihood of being laid off in the months around Uber's entry into a market. 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 from Equation (1). In its place, we include a vector of 17 indicator variables that correspond to the event time around Uber's entry (in quarters), ranging from $U_j - 8$ to $U_j + 8$. We assign any month occurring prior to $U_j - 8$, and any month subsequent to $U_j + 8$, to their respective "book-end" event time fixed effects. The figure also reports 95% confidence bands.

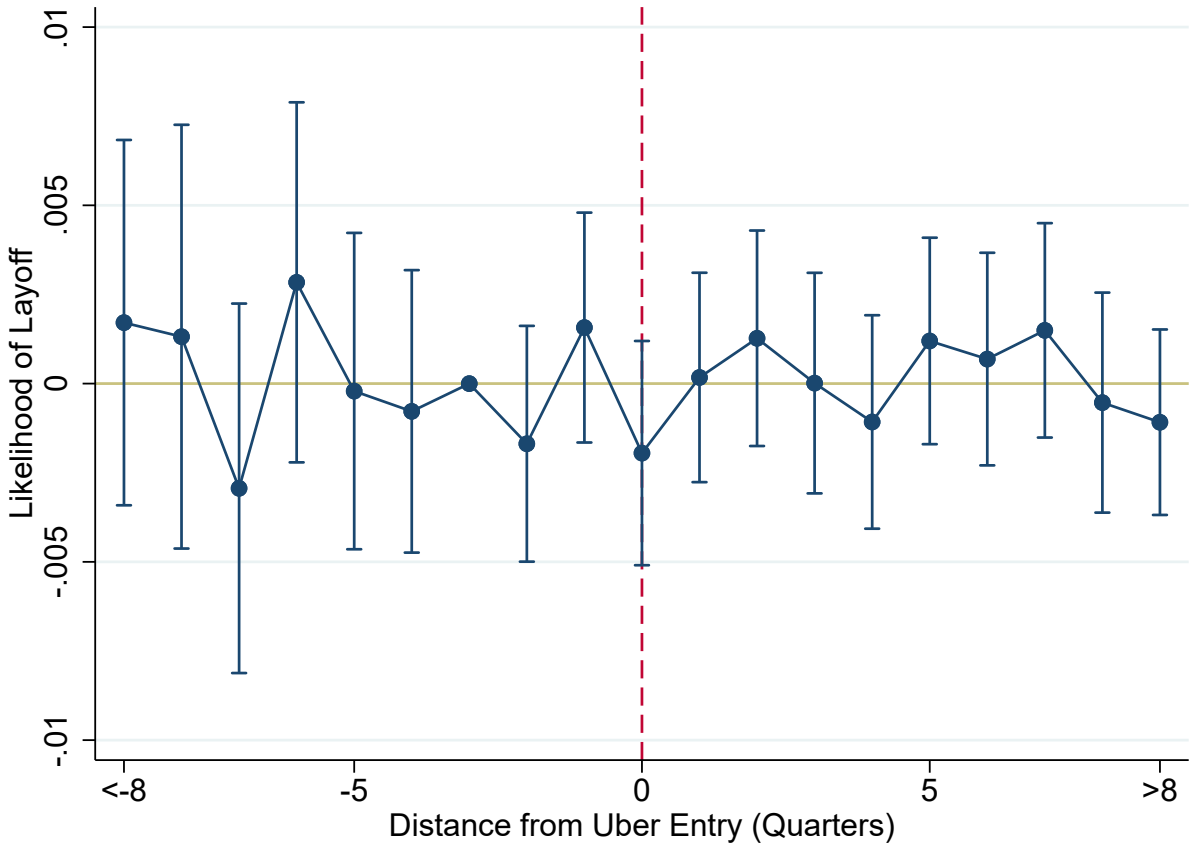


Figure OA.4. Layoff Likelihood around Uber's Entry, Broad Sample

This figure reports the estimated effect of $Layoff \times Ownership$ on the likelihood of being laid off in the months around Uber's entry into a market. 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 from Equation (2). In its place, we include a vector of 17 indicator variables that correspond to the event time around Uber's entry (in quarters), ranging from $U_j - 8$ to $U_j + 8$. We assign any quarter occurring prior to $U_j - 8$, and any quarter subsequent to $U_j + 8$, to their respective "book-end" event time fixed effects. The figure also reports 95% confidence bands.

Panel A: Reception of UI Benefit

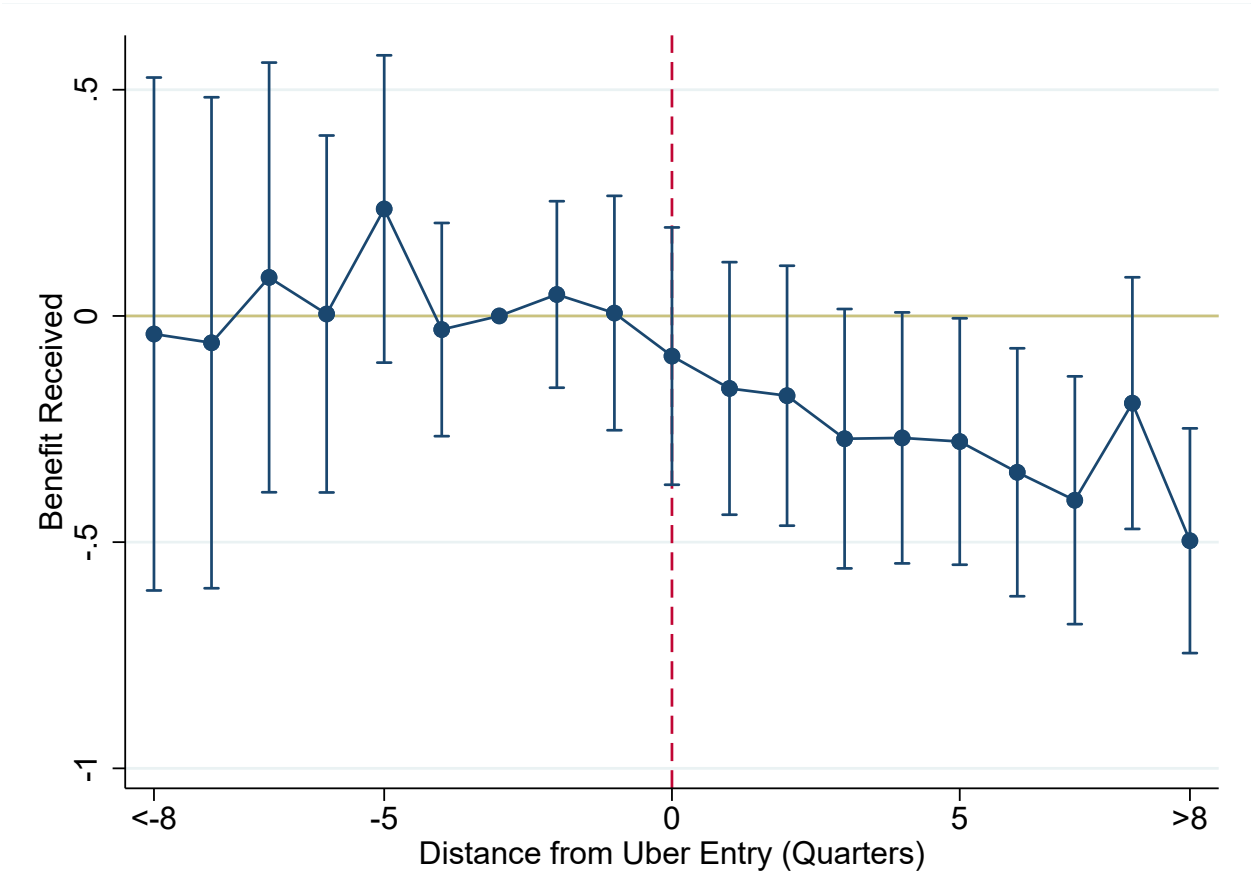
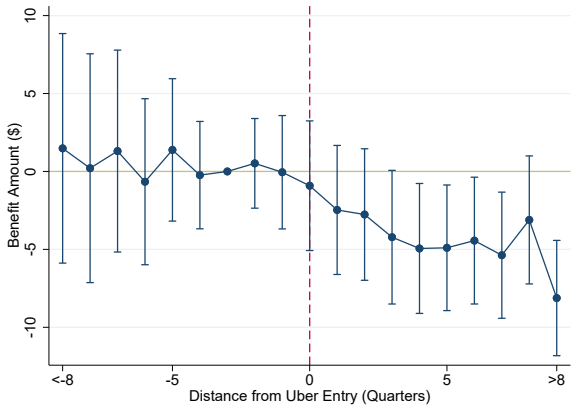


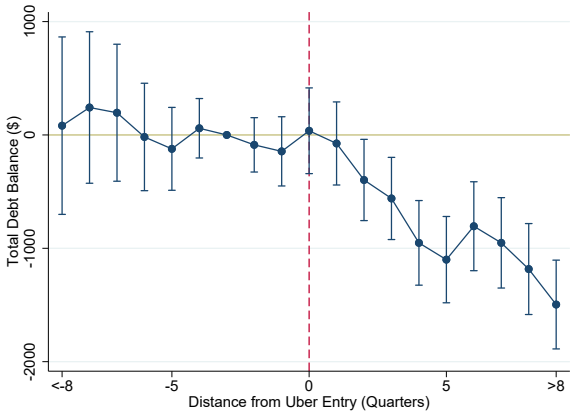
Figure OA.5. Changes in Outcomes around Uber's entry, Broad Sample

This figure reports the estimated effect of $Layoff \times Ownership$ on outcome variables in the months around Uber's entry into a market. 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 from Equation (2). In its place, we include a vector of 17 indicator variables that correspond to the event time around Uber's entry (in quarters), ranging from $U_j - 8$ to $U_j + 8$. We assign any quarter occurring prior to $U_j - 8$, and any quarter subsequent to $U_j + 8$, to their respective "book-end" event time fixed effects. 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).

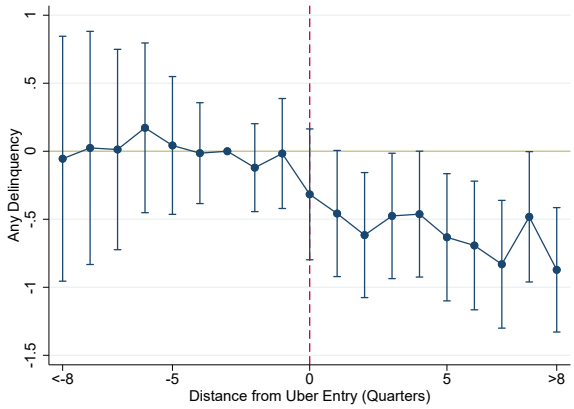
Panel B: UI Benefit Amounts



Panel C: Outstanding Credit Balances



Panel D: Delinquency on any Credit Type



Panel E: Delinquency on Mortgage Loans

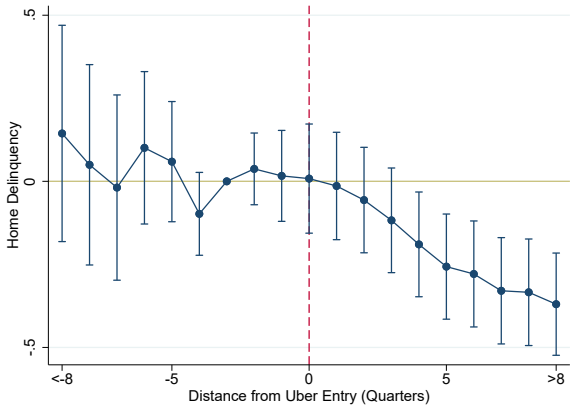


Table OA.1. Car Eligibility across States
This table reports the share of workers by state with a car between 10 and 15 years old in the month prior to layoff, relative to all workers with a car between 10 and 20 years of age. States are sorted in ascending order by share of eligible car owners.

State	Pct. Eligible	State	Pct. Eligible
OR	0.663	NC	0.739
ID	0.680	GA	0.739
DC	0.693	AR	0.740
WA	0.699	IL	0.741
ND	0.709	NH	0.741
HI	0.716	OH	0.743
NM	0.719	FL	0.743
WI	0.720	CO	0.743
NV	0.722	OK	0.744
KS	0.724	KY	0.746
CA	0.726	VA	0.748
MS	0.727	MD	0.750
NE	0.728	MI	0.755
MO	0.729	PA	0.755
IN	0.729	CT	0.757
UT	0.730	NJ	0.760
MN	0.733	NY	0.762
WV	0.737	VT	0.763
AZ	0.737	TX	0.766
TN	0.737	SC	0.774
LA	0.737	MA	0.786
IA	0.737	DE	0.791

Table OA.2. High income

This table reports the results of OLS regressions of the form described in Equation (1), while considering ZIP codes 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 4, 5, and 6. 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) (1)	Benefit Amount (2)	Total Debt (3)	Home Loans (4)	Any Delinquency (5)	Home Delinquency (6)
Layoff × Eligible × Uber	-0.522**	-1.254*	-3,617**	-2,443**	-1.547**	-0.029
	(0.249)	(0.692)	(1,687)	(1,213)	(0.689)	(0.447)
Obs.	656,804	656,804	656,804	656,804	656,804	656,804
Adj. R^2	0.340	0.322	0.900	0.893	0.593	0.508
Fixed effects:						
Individual FE	Yes	Yes	Yes	Yes	Yes	Yes
City-Month-Layoff FE	Yes	Yes	Yes	Yes	Yes	Yes
Car age-City-Layoff FE	Yes	Yes	Yes	Yes	Yes	Yes
Car age-City-Uber FE	Yes	Yes	Yes	Yes	Yes	Yes

Table OA.3. Observable characteristics at the time of layoffs.
This table repeats the analysis performed in Table 2 for the broad sample, examining a potential change in characteristics of laid-off workers following Uber's entry into an area. The sample is made up of one observation per laid-off worker, measuring the worker's characteristics for the month prior to layoff. 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:	Credit Score	Debt ($)			Past Delinquencies (%)		
		Total	Credit Card	Home Loans	Total	Credit Card	Home Loans
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Eligible × Uber	0.110	1,105	58	753	0.096	0.131	-0.051
	(1.043)	(864)	(55)	(766)	(0.251)	(0.166)	(0.065)
Obs.	263,591	263,591	263,591	263,591	263,591	263,591	263,591
Adj. R^2	0.086	0.099	0.058	0.089	0.048	0.037	0.029
Fixed effects:							
Car age FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes