Delivering Higher Pay? The Impacts of a Task-Level Pay Standard in the Gig Economy*

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February 2025

Abstract

How does a task-level minimum pay requirement for gig workers affect their earnings and employment? We study this question in the context of a January 2024 law in Seattle that establishes a per-task minimum pay standard for app-based delivery workers. Drawing on novel cross-platform, trip-level gig activity data, we compare earnings and employment trajectories around the implementation of the law for workers who were doing delivery work in Seattle before the reform against workers who had been active in other regions of Washington State. We find that the minimum pay law raised delivery pay per task, though the increases in base pay per task were partially offset by a substantial reduction in average tips, a major component of delivery pay. At the same time, the policy led to a reduction in the number of tasks completed by highly attached incumbent drivers (but not an increase in exit from delivery work), reflecting both lower demand for deliveries in Seattle and increased competition from labor market entrants. We find limited evidence of switching from delivery to ride-hailing work. In total, we find that the policy had no net impact on the monthly earnings of incumbent delivery drivers. These results highlight the challenges of raising pay in spot markets for tasks where there is free entry of workers.

*We gratefully acknowledge support from the Block Center for Technology and Society at Carnegie Mellon University. We thank the staff at the City of Seattle Office of Labor Standards for answering questions about policy details.

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1 Introduction

Minimum wage regulations that raise pay rates above competitive market-clearing levels often result in rationing, as the quantity of labor supplied to the market exceeds available job opportunities. Most analyses of the minimum wage focus on traditional labor markets with persistent employment relationships. In this case, rationing entails a division between employees, who benefit from higher wages (albeit perhaps with fewer paid hours), and unemployed individuals, who cannot find a covered job. However, in platform-mediated gig economy markets and other spot markets for tasks with free entry among workers, the impact of a minimum pay standard may differ substantially depending on how tasks are rationed. As a growing number of jurisdictions consider the adoption of minimum pay policies for platform-based gig work, it is important to understand who bears the benefits and costs of such regulations. In principle, gig economy pay regulations are intended to address low earnings levels among platform-based workers, who are not covered by standard minimum wage laws (Zipperer et al., 2022; New York City Department of Consumer and Worker Protection, 2022; Manzo, Petrucci and Bruno, 2022; Jacobs et al., 2024). A key question is whether such policies can effectively raise earnings for market participants in practice.

In this paper, we shed light on this question by examining the impacts of a new task-level minimum pay standard for platform-based gig work. Specifically, we study Seattle's App-Based Worker Minimum Payment (ABWMP) Ordinance, which set minimum base pay requirements for workers performing delivery tasks on gig-work apps. This law went into effect in January 2024 and applies only to deliveries starting or ending in the city of Seattle. We estimate the causal effects of the policy by comparing changes in outcomes for drivers with different degrees of pre-policy exposure to work in Seattle.

To evaluate the impacts of the pay ordinance, we draw on unique gig worker data from Gridwise Inc, which contains task-level information on workers' activities and associated revenues across major delivery and rideshare platforms. The data allow us to track individual drivers over time and provide fine geographic detail and task-level compensation broken out into base pay, tips, and other payments. Our sample covers the State of

Washington from August 2023 to July 2024, allowing us to observe gig work across all platforms before and after the law's implementation, both within and outside Seattle.

We use these data to implement a difference-in-differences design among workers who were active delivery drivers prior to the reform. Our core design compares drivers whose pre-reform delivery activity was concentrated in Seattle to drivers who drove elsewhere in Washington state before the policy was implemented. We focus primarily on workers who were highly active in the pre-policy period, as these workers are much more likely to continue to engage in delivery work afterwards. However, we also examine impacts on less-attached incumbent drivers as well. In addition, we conduct descriptive analysis to characterize post-period rates of entry into the market in each region.

First, we document that the minimum pay standard was binding and resulted in average base pay per task doubling in Seattle during a period when pay rates remained constant in the rest of the state. Crucially, however, although the policy only applies to base pay, tips are a major part of driver compensation in app-based delivery work and constitute the *majority* of total pay per task on average. We find that the Seattle pay standard led to an immediate drop in average tips per task, which may have been in part a result of changes in delivery app interfaces for Seattle-based customers. The decline in tips offsets over one-third of the increase in base pay.

Turning to the individual-level analysis, we find that while highly-attached drivers who were exposed to the reform benefited from higher total earnings per delivery task after the reform, this was offset by a decline in the number of monthly tasks beginning in the second month after the pay standard was implemented, reflecting an aggregate decline in the number of tasks completed in Seattle over the same period. Strikingly, combining both margins we find exposed workers experience *no* increase in total monthly earnings after the first month following policy implementation. We highlight that, despite the decline in tasks completed, exposed individuals experience an increase in total monthly *base* earnings—the component of pay targeted directly by the policy—but this is fully offset by the decline in tips. We additionally examine whether exposed workers respond to lower task demand by switching towards ride-hailing work, but find only small and statistically insignificant effects on ride-hailing tasks and earnings. When we examine less-attached workers, we find nearly identical increases in delivery earnings per task as for the more-attached workers, but find no reduction in delivery tasks completed per month. However, since the baseline number of monthly tasks completed by drivers in the less-attached sample is an order of magnitude smaller than in the more-attached sample, the net impacts on monthly earnings are small and not statistically significant.

Why did exposed incumbent workers not benefit on net from the minimum pay standard? On one hand, the decline in tips, which were not targeted by the policy, partially undermined the realized increases in base pay. However, the remaining increase in net earnings per task was exactly offset by declining tasks per month. This decline in tasks per driver reflects two simultaneous forces. First, higher delivery fees led to a reduction in overall demand for delivery tasks in Seattle. At the same time, higher pay per task may have attracted new entrants into delivery work. Because delivery apps allocate tasks across all active drivers, an increase in available drivers leads to longer wait times for trips. Consistent with this challenge, we find that the entire decline in aggregate Seattle delivery tasks was driven by a decline among incumbent workers who were active prior to the reform. By contrast, we find that the volume of deliveries performed by new entrants evolves nearly identically in Seattle as in other regions of Washington. Together, these results suggest that free entry of drivers led to relative influx of new workers responding to increased pay per task who became a larger share of the post-reform market and competed with incumbent workers for a shrinking pie. Consistent with Hall, Horton and Knoepfle (2023), our results suggest that free entry drives up queuing times and drives down expected earnings towards a fixed outside option.

Our paper contributes to a large empirical literature on the impacts of minimum wage policies, particularly more recent studies on the effects on city-level minimum wage laws (Jardim et al., 2022; Dube and Lindner, 2021; Karabarbounis, Lise and Nath, 2023).[1] Particularly relevant is the recent study by Jardim et al. (2022) examining the impacts of a city-level minimum wage in Seattle implemented 2015 that covered traditional employ-

[1] For earlier evidence and comprehensive reviews, see Card and Krueger (1995); Brown (1999); Neumark and Wascher (2010); Dube and Lindner (2024).

ment but not platform-based gig work. They find that Seattle-based workers exposed to the policy were no more likely to become unemployed than other workers in the state, but that the benefits of higher hourly wage rates were partially—but not fully—offset by lower hours for continuing workers. In contrast to our setting where tasks are distributed among all market participants, they find that the higher minimum wage reduced entry of new workers, who faced increased difficulty finding traditional jobs.

We also add to a growing literature documenting the importance of responses to the minimum wage on margins beyond employment and wages (Clemens, 2021; Liu et al., 2024; Davies, Park and Stansbury, 2024). In particular, we examine a setting in which tips are a major component of overall earnings, but were not directly covered by the minimum pay standard. We find that documenting adjustments on the tip margin is crucial to capturing the full effects of the policy on the intended beneficiaries themselves.

Our work is most directly related to recent work studying the impacts of pay policies and other regulations in the online platform-based gig economy. Koustas, Parrott and Reich (2020) study how the implementation of a minimum pay standard for ride-hailing app drivers in New York City in 2019 impacted demand for trips, finding a route-level price elasticity of -0.68. Horton (2025) studies an online labor market that randomly assigned minimum hourly wage rules to a subset of job postings, and finds that "employers" in this market became more selective about the types of workers they hired, and that reductions in hours worked per task largely offset the increases in hourly wage. Most directly related to our findings is a study by Hall, Horton and Knoepfle (2023) which documents that increases in pay rates per trip implemented by Uber led to an increase in entry and longer wait times for trips (lower utilization rates) that exactly offset, such that hourly earnings did not change for drivers in affected markets.

The paper proceeds as follows. Section 2 provides background on working on online delivery platforms and the Seattle minimum pay ordinance for app-based delivery workers. Section 3 describes the Gridwise data and details our research design. We present our descriptive results of aggregate market trends in Section 4, and our individual results in Section 5. Section 6 discusses on-going work and concludes.

2 Institutional Background

This section briefly summarizes the key features of working on online delivery platforms and the Seattle minimum pay ordinance for app-based delivery workers.

2.1 Working on Online Delivery Platforms

On-demand delivery services facilitated through online delivery platforms have experienced fast worldwide growth over the past decade, particularly since the onset of the COVID-19 pandemic.[2] This new type of service has generated substantial work opportunities for independent couriers who can self-schedule their delivery work, and has become one of the most important services in the gig economy.[3]

In the online delivery market, delivery platforms dispatch delivery tasks generated from customer orders to delivery workers, having workers pick up and deliver products from merchants to customers. On most platforms, when workers plan to work, they notify the platform of their availability, at which point the platform begins searching for and assigning delivery tasks. After a short period of time, workers are offered individual tasks, usually starting nearby, with task information on pickup and destination locations, time and distance, and total pay including estimated tips. Workers can either accept or deny the offered tasks. When they complete a task or when they no longer plan to work and end a driving session, they can observe total pay from the completed tasks, broken out into base pay and tips.

In contrast to ride-hailing work, where tips are a small part of total earnings, the *majority* of delivery compensation typically comes from tips. Appendix Figure A.2 shows the pay structure on major delivery and rideshare platforms in Washington State between

[2]For example, Statista reports that the global online food and grocery delivery market has generated $1.21 trillion in revenue in 2024, with rapid average annual growth of up to roughly 50% during the COVID-19 pandemic in 2020 and 2021. They forecast that this market is expected to achieve a compound annual growth rate of 9.33% from 2024 to 2029, expanding to a projected $1.89 trillion globally by 2029. See `https://www.statista.com/outlook/emo/online-food-delivery/worldwide` (accessed January 10, 2025). Garin et al. (2025) document substantial rise in platform-mediated delivery (and transportation) work from 2012 through 2023 in the United States, with a dramatic increase in delivery work and significant shift from ride-hailing to delivery work around the COVID pandemic.

[3]Garin et al. (2025) document that platform-mediated delivery and transportation work has been the largest component of gig work since 2017, and present evidence that most of the expansion of platform work since 2020 has been driven by delivery work.

August 2023 and December 2023.[4] Tips account for between 52 and 62 percent of base pay and tips per task on major delivery platforms, while they only make up about 12 percent on rideshare platforms, suggesting that delivery workers generally rely more on tips for a significant portion of their income than rideshare workers do.[5] This is a consequence of the tendency of delivery customers to make tips that scale with the value of the meal, rather than with the delivery fee. Given the generous tip amounts, apps can offer attractive jobs to workers while providing relatively low base pay.

2.2 Seattle Minimum Pay Ordinance for App-Based Delivery Workers

On May 31, 2022, the City of Seattle passed a minimum pay ordinance for app-based delivery workers, known as the App-Based Worker Minimum Payment (ABWMP) Ordinance (hereafter, the ordinance). This ordinance followed previous city-level initiatives that implemented substantial increases to the minimum wage and created minimum pay standards for ride-hailing work (but did not apply to platform-based delivery work).[6] The delivery pay ordinance mandated that the minimum base compensation for delivery tasks resulting in engaged time or engaged miles exceed the greater of i) $0.44 per minute plus $0.74 per mile, or ii) $5 per offer, for each offered task.[7,8] The ordinance came into effect on January 13, 2024.

[4] Authors' analysis of Gridwise data.

[5] Jacobs et al. (2024) analyzing Gridwise data over a two-week period in January 2022 in Los Angeles and San Francisco Bay metros and in Boston, Chicago, and Seattle metros document qualitatively similar findings. Gridwise's 2025 Annual Gig Mobility Report also supports these findings (Gridwise Analytics, 2025).

[6] See Jardim et al. (2022) for a discussion of the city-level minimum wage reform.

[7] "Engaged time" begins upon the app-based worker's acceptance of the offer and ends upon the app-based worker's completing performance of the offer, cancellation of the offer by a customer or the network company, or cancellation with cause of the app-based worker's acceptance of the offer. "Engaged miles" refer to miles traveled during engaged time. If an app-based worker accepts a new offer during performance of a previously accepted offer, and both offers are facilitated or presented by the same network company, engaged time and engaged miles accrued during any period of time in which performance of the offers overlaps shall be subject to the minimum compensation requirements for a single offer. Tips and incentives paid to an app-based worker do not count towards the minimum payment. For details, refer to https://library.municode.com/wa/seattle/codes/municipal_code?nodeId=TIT8LAST_CH8.37ASEWOMIPA (accessed August 6, 2024).

[8] See https://www.seattle.gov/laborstandards/ordinances/app-based-worker-ordinances/app-based-worker-minimum-payment-ordinance (accessed August 6, 2024).

The ordinance covers app-based delivery services performed in Seattle.[9] If the engaged time of a service begins in Seattle, the requirements of the ordinance apply, regardless of where the service terminates. If the engaged time begins outside of Seattle, the ordinance applies only for the portion of the service that occurs within Seattle. In other parts of Washington State outside Seattle, there are no minimum pay regulations for app-based delivery workers. More generally, during the period we study, there were no other major changes to city-level labor market regulation. The Seattle ride-hailing pay standard had initially been implemented on January 1, 2021, and was then superseded by a statewide regulation (State House Bill 2076), which took effect on January 1, 2023. Hence, the ride-hailing pay standard applied equally in all parts of Washington state throughout the period we study.[10] This policy variation motivates our research design below.

Following the ordinance's implementation, many delivery platforms responded by making changes to their apps to make costs induced by the ordinance salient to consumers and delivery workers. For example, DoorDash, Instacart, and Uber Eats imposed a flat $4.99 or $5 fee on Seattle orders. As shown in Figure 1, Seattle consumers now have to pay a new $4.99 regulatory response fee on DoorDash highlighted at checkout, while consumers in Spokane do not. Some platforms went further and no longer allowed customers in Seattle to tip at checkout. For example, on Uber Eats, Seattle consumers cannot provide a tip before delivery is complete, as shown in Figure 2, and can only add it after delivery.[11] By contrast, there are no such tip policy changes in Spokane.

3 Data and Research Design

An empirical analysis of minimum pay standards in the gig economy requires task-level gig activity and earnings data. As self-employed independent contractors, gig workers do not pay into state unemployment insurance (UI) systems, which means that gig workers'

[9]The ordinance covers services facilitated by network companies that mediate work performed by 250 or more app-based workers worldwide regardless of where those workers perform work.

[10]For details on the Washington State House Bill 2076, refer to https://lawfilesext.leg.wa.gov/biennium/2021-22/Pdf/Bills/Session%20Laws/House/2076-S.SL.pdf (accessed August 6, 2024).

[11]See https://www.uber.com/blog/uber-delivery-tip-policy-seattle/ (accessed August 6, 2024).

earnings do not appear in quarterly UI earnings records. Administrative tax return data provide broad coverage of gig-worker earnings, but do not provide the geographic or other detail required to study the Seattle minimum pay ordinance we focus on. Proprietary internal data from gig economy platforms, while fine-grained, only provide insights into labor supply of individuals on a particular gig economy platform, and do not speak to behaviors of these individuals on other platforms. To overcome these data limitations and facilitate research on minimum pay standards in the gig economy, we use unique data from Gridwise Inc. that tracks detailed worker activity and earnings across multiple gig platforms. This section first introduces the Gridwise data source and then details our research design.

3.1 Gridwise Data

Our main data set consists of task-level gig platform information collected by the Gridwise app. Gridwise is a third-party gig work assistance app that allows gig workers to link all of the various delivery and rideshare platforms they use and sync their gig driving activity in order to help them, for example, track earnings, mileage, and expenses, optimize activity across platforms, and prepare tax returns.[12] Through their app, Gridwise automatically collects users' real-time gig driving activity and earnings data. To date, Gridwise has collected data on over 720 million trips and $8.3 billion in driver earnings, with some metrics showing up to 98% correlation with key quarterly figures reported by major gig platforms (Gridwise Analytics, 2025). A unique aspect of Gridwise data is that it provides visibility to gig workers' activity and earnings across platforms, which presents comprehensive insights into the labor supply of gig workers.

We use data on all tasks performed by Gridwise users in Washington State between August 2023 and July 2024, giving us roughly six months of data before and after the Seattle minimum-pay ordinance went into effect. The data cover major delivery and rideshare platforms including DoorDash, Grubhub, Instacart, Uber Eats, Lyft, and Uber. The structure of the data collected by Gridwise varies by gig platform. For Grubhub,

[12]Gridwise offers a free version and a premium service, Gridwise Plus, with prices starting at $9.99 per month or $6 per month billed annually. See https://gridwise.io/plus/ (accessed January 10, 2025).

the core reporting unit of work is a single task involving one pickup and its associated drop-off. For Instacart, Uber Eats, Lyft, and Uber, the work unit is either a single task or a batch with multiple tasks offered as a single job to workers, while for DoorDash, the unit is a shift, typically a driving session, which can consist of multiple tasks and/or batches. For each unit of work ("Activity" hereafter), we observe the unique worker ID and platform ID, worker earnings including base pay, tips, bonuses, and total earnings, start and end time and locations (census block level), and number of tasks. In addition, the Gridwise data also include a *task-level* breakdown for platforms where the unit of work is a shift or a batch, which allows us to observe the components of pay for each individual delivery order but not the precise start and end times and locations of the delivery within the broader reporting unit.[13] The data include 2,844,465 tasks completed by 5,930 workers, generating a total of $35,366,044 in worker earnings. Among these, 4,492 delivery workers performed 1,939,592 tasks, earning a total of $18,899,728.

We use the unique worker IDs to aggregate across tasks to create a worker-level dataset, with associated monthly delivery and rideshare tasks and earnings (including base pay, tips, bonuses, and total earnings), and exposure to work in Seattle based on the share of delivery earnings from Seattle tasks prior to the Seattle minimum pay ordinance's implementation. In our analysis, we define months as 30-day periods relative to January 13, 2024. The following section details how we define our primary exposure measure.

3.2 Empirical Design

The primary goal of this paper is to estimate the causal impact of the minimum pay policy on individual workers. To that end, we follow workers who were active in delivery work during the pre-implementation period and measure differences in post-implementation outcomes for workers who were more vs. less exposed to the policy. As described in Section 2.2, Seattle's minimum pay ordinance applies to tasks rather than workers. The

[13] The start and end locations of individual tasks are at a much less granular level of Core Based Statistical Areas (CBSAs), which does not allow us to distinguish whether individual tasks start or end within the city of Seattle.

ordinance (fully or partially) covers tasks that start or end within the city's boundaries. We measure a worker's exposure to the policy based on the share of their pre-policy activity that occurred in Seattle.

Specifically, we define exposure as follows. First, because not all tasks in our data can be geocoded individually at the city level, we classify Activities as i) Definitely starting or ending in Seattle (denoted as S); ii) Definitely not in Seattle (starting and ending in Washington State outside King County (which contains Seattle); denoted as N); or iii) Residual (starting (ending) in other parts of King County outside Seattle and ending (starting) not in Seattle; denoted as R).[14] Second, for each worker, we define exposure as $S/(S+N)$, the share of *classifiable* pre-policy delivery earnings coming from Seattle activities.[15] We adopt this conservative approach to defining exposure to be confident that individuals with zero measured exposure did not drive in Seattle in the pre-implementation period. Since we exclude tasks that start and end in King County outside Seattle (R tasks) from this exposure measure, workers with 100% measured exposure may still do some of their pre-reform trips outside Seattle in this buffer region; in practice, we estimate that drivers with 100% exposure do roughly half of their pre-reform tasks in Seattle and half elsewhere in King County.

For simplicity, we use a discretized version of the exposure measure in our main analysis, in which we code a worker as exposed ("treated") if their exposure is greater than 80% and not-exposed ("control") if the exposure is less than 20%. In practice, this simplification is not restrictive, as over 95 percent of workers in our sample are distributed at extremes beyond these two thresholds, as shown in Appendix Figure A.3. In robustness tests, we show that our main results are qualitatively unchanged when we use the continuous exposure measure (see Appendix Tables B.1, B.2, and B.3).

Throughout our analysis, we present results separately for workers with higher or

[14] While we have census-block-level geographic information for tasks on gig platforms where the unit of work (an Activity) is a single task which helps identify whether a task starts (ends) within or outside Seattle's boundaries, for platforms where an Activity represents a shift or a batch we don't have such granular geographic information available for encompassed tasks. See discussion in footnote 13.

[15] Workers who *only* perform delivery work classified as Residual—primarily in King County outside Seattle—in the pre-policy period, are excluded from the less-/not-exposed sample and the analysis by construction under this definition in order to mitigate the risk of these workers switching to the Seattle market post-policy and potential policy spillovers.

lower degrees of attachment to the gig delivery market in the pre-policy period. We define more-attached workers as those who performed delivery tasks above the median in the pre-policy period (about 20 tasks per month). Our main analyses focus on this group, as attached workers are significantly more likely to continue to do delivery work in the post-reform period than incumbent drivers with low levels of attachment, and are therefore are most likely to be impacted by the policy. However, we also present impacts on incumbent drivers with lower attachment.

Table 1 presents means and standard deviations of key worker characteristics prior to policy implementation for the exposed (Seattle) and not-exposed (non-Seattle) worker samples (columns 1–4), and estimates of differences in characteristics between the two groups (columns 5-6). Panel A shows the statistics for the subsample of more-attached workers. In this subsample, Seattle's workers performed approximately 106 delivery tasks per month and worked for 4 out of 5 months, on average, before policy implementation, which implies an average of about 133 delivery tasks per *active* months during the pre-policy period. Within this same subsample of attached drivers, non-Seattle workers have similar earnings per task and days active per month as those in Seattle, but complete slightly fewer delivery tasks per month. Panel B presents summary statistics for the subsample of less-attached workers. Notably, the average earnings, tasks completed, and days active among this subsample in the pre-policy period are an order of magnitude smaller than the corresponding amounts for the more-attached workers.

To estimate the causal effects of the minimum-pay policy, we use a dynamic difference-in-differences design comparing the differential evolution of outcomes for exposed ("treated") and not-exposed ("control") workers before vs. after policy implementation. Specifically, we estimate the following specification:

$$Y_{it} = \sum_{k \neq -1} \beta_k \text{Treat}_i \times \mathbb{1}\{t = k\} + \alpha_i + \zeta_t + \epsilon_{it}, \tag{1}$$

where Y_{it} is one of several labor market outcomes for individual worker i in event month t, measured relative to January 13, 2024, the date when the Seattle minimum pay ordinance went into effect. In our main analysis, we examine four sets of individual-level outcomes on delivery work: average pay per task, any work, number of completed tasks, and earnings. The term Treat_i is an indicator for whether worker i was exposed to work in Seattle prior to policy implementation. α_i indicates individual worker fixed effects and controls for worker characteristics that are constant over time. ζ_t indicates event year-month fixed effects and controls for factors that are constant across workers but vary over time. Standard errors are clustered at the worker level. The coefficient of interest is β_k. $k = 0$ corresponds to the first event month after the date January 13, 2024 when the ordinance became effective. The event month before the effective date, $k = -1$, is omitted from the estimation in order for the model to be identified. Each β_k measures the difference between exposed ("treated") and not-exposed ("control") workers in a given event month relative to the difference in the event month prior to policy implementation. To the extent that identification assumptions hold (discussed below), the β_ks identify the average treatment effect on the treated (ATT) of the minimum pay ordinance on individual worker outcomes, in month k relative to ordinance implementation. In addition to the fully dynamic difference-in-differences specification, we also estimate a "static" specification by replacing the indicators for event months interacted with the term Treat_i in Equation (1) with a single indicator for the post-ordinance period to obtain the average effect of the ordinance. Results appear in Section 5.

The key identifying assumption of our difference-in-differences design is that outcomes for exposed ("treated") and not-exposed ("control") workers would have evolved in parallel in the absence of policy change. As a check, we test for differences in pre-reform trends across groups by examining estimates of β_k over the pre-policy period ($k < -1$) after estimating Equation (1). As shown in Section 5, we find no evidence of pretrend violations. In robustness tests, we also estimate a modified version of our main specification Equation (1), including a fixed set of pre-policy individual worker covariates—delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, during the pre-policy period—interacted with indicators for event months to control for potentially time-varying impact of baseline worker characteristics. As shown in Section 5, the results are robust to adding these additional controls. These tests support the identifying assumption required for the validity of the difference-in-differences research design.

4 Descriptive Analysis of Aggregate Trends

Before documenting the minimum pay policy's impacts on individual workers, we first present descriptive analysis of how the delivery market evolved in Seattle around the policy's implementation. To this end, we aggregate delivery tasks by app and market, assigning all tasks in the data (not just those done by incumbents) to Seattle or the rest of Washington outside King County based on the start and end location of their encompassing activity as described in Section 3.2.[16] We focus on two sets of aggregate outcomes on delivery work: average pay per task and the total number of completed tasks.

First, we find that base pay per task more than doubles in Seattle after the enactment of the policy. Figure 3 plots the time series of average base pay, tips, and total pay per task on the four largest delivery platforms, DoorDash, Grubhub, Instacart, and Uber Eats, in each event month relative to ordinance implementation, separately for tasks that are part of an activity that passes through Seattle and those that start and end in Washington State outside King County. Month 0 is the first event month post-ordinance, and month -1 is the event month before the ordinance. In each panel, outcomes in both regions are relatively stable across the pre-policy period. Following the implementation, there is an immediate increase in base pay per task in Seattle, as shown in Panel A. The effect persists over time and is qualitatively similar across platforms.[17] By comparison, no meaningful changes happened in the remainder of Washington outside King County. These results provide suggestive evidence that the minimum pay standard is binding,

[16]We exclude activities classified as Residual, those primarily in the King County market outside Seattle, from the comparison group and analysis in order to avoid potential policy spillovers.

[17]Average base pay per task on Instacart before the ordinance and its rise post-ordinance are both a bit larger than on other platforms, largely because Instacart's main businesses, grocery deliveries, usually take longer time to complete than food deliveries mainly offered by other platforms, due to additional in-store shopping time required by grocery deliveries.

that post-implementation pay rates are well above the five dollar lower bound, and that platforms are complying with the ordinance following its implementation. The lack of any effect outside of Seattle indicates that there are no discernible spillover effects of the ordinance to areas outside of King County.

At the same time, however, Panel B shows immediate declines in average tips per task in Seattle. While we observe a decline in tips per task across all platforms, the magnitude of the decline varies significantly across apps. In particular, the decline in tips per task is notably larger on Instacart and Uber Eats, the platforms that disabled up-front tipping in Seattle in response to the ordinance, as described in Section 2.2. As a result of the decline in tipping, the increases in *total* earnings per task are smaller than the increases in base pay, as shown in Panel C, with total earnings per task rising less on Uber Eats and Instacart than on DoorDash and Grubhub.

We next examine how the overall volume of tasks observable in the Gridwise data evolve around policy implementation. Figure 4 plots total monthly deliveries in Seattle and the rest of Washington State (excluding King County), respectively, normalizing all amounts by each region's respective pre-ordinance average monthly total tasks. The top lines in Figure 4 show that while the total number of tasks in each region trended similarly before the pay standard went into effect, the volume of tasks completed in Seattle persistently declined after its implementation, both in absolute terms and compared to trends in the rest of the state. The largest decline occurs in the second month after the policy implementation. These aggregate trends are consistent with higher costs per delivery resulting in reduced total demand for delivery tasks in Seattle compared to the remainder of Washington. An important caveat is the reported numbers reflect only tasks recorded among Gridwise users rather than all delivery tasks performed on each platform.

However, strikingly, Figure 4 shows that the decline in delivery tasks in Seattle is driven entirely by a reduction in tasks completed by incumbent drivers who were active prior to the reform. While tasks completed by incumbents fall sharply in Seattle after the ordnance went into effect, we find that the volume of deliveries performed by new entrants evolves nearly identically in Seattle as in other regions of Washington. Within three months of the reform, post-reform entrants account for a majority of all tasks in Seattle, while incumbents complete twice as many tasks as entrants elsewhere in the state. On the whole, these results suggest that even as the delivery market in Seattle contracted post-ordinance, there was a relative influx of new workers responding to increased pay per task who competed with incumbent drivers for a shrinking pie.

5 Individual Results

In this section, we examine the impacts of the ordinance on individual workers active in delivery work prior to implementation. We first discuss impacts on the high-attachment sample and subsequently examine effects on less-attached drivers. Figures 5, 6, and 7 plot estimates of β_k for the main outcomes from estimating Equation (1) for more-attached workers, and Appendix Figure A.6 plot estimates for less-attached workers. Tables 2, 3, and 4 summarize estimates of the respective average effects. We report both baseline results without including controls and results including the full set of controls, but will focus on discussing baseline results throughout the section, since the controls have minimal quantitative impact.

We first examine impacts on average earnings per task of highly-attached workers in Figure 5. In the legend of the figure, we show highly attached exposed workers earned an average of $4.87 base pay per delivery prior to ordinance implementation. We find that after implementation, these drivers saw an immediate, persistent increase in average base pay per delivery. Pooling all post-period months, base earnings per trip increased by $4.09 on average as reported by column (1) of Table 2 Panel (A). While this reflects a substantial increase over the pre-period average base pay rates, the magnitude of the increase is notably smaller than the increases in average base pay per task observed above in Figure 3 Panel (A). This difference arises due to our focus on exposed *individuals* as opposed to exposed *deliveries* in Figure 3. As discussed above in section 3.2, our exposure is constructed such that individuals with 100 percent exposure still do many trips in the "buffer region" of King County outside Seattle—in practice, 100% exposed drivers only do about half of their deliveries in Seattle in the pre-period. Thus, one should expect the worker-level changes in base pay per trip using this exposure definition to only be about half the magnitude of the increase observed for Seattle deliveries, since half of exposed workers' deliveries are not covered by the new pay standard.[18]

However, Figure 5 shows that the increases in average base pay per delivery were offset by declining tips following the ordinance. Column (3) of Table 2 Panel (A) reports that their average tips declined by \$1.5 from a pre-ordinance average of \$5.13 per task. This decline offsets over one-third of the increase in base pay per task reported in column (1). Overall, these effects result in a net increase in total pay per task of \$2.61 from a pre-ordinance average of \$10.09, as shown in column (5) and Figure 5.

We next examine how the reform impacted the number of deliveries completed by exposed drivers. To assess extensive-margin effects on continued participation in delivery work, Figure 6 Panel (A) examines an indicator for performing any delivery work. We find zero effect of the ordinance for attached workers. Thus, the ordinance did not lead to any significant exit from delivery work among highly attached workers (see also column (1) of Table 3 Panel (A)).

However, we find that the number of tasks completed by continuing workers fell after the reform. Figure 6 Panel (C) evaluates the number of delivery tasks completed per month in logs (defined only for months in which drivers completed at least one task). While we observe no decrease in trips completed in the first month after implementation (month 0), we find that continuing attached workers completed about 20%-30% fewer monthly tasks in each month afterwards. This dynamic pattern is consistent with the trends in Figure 4, which showed the biggest decline in Seattle tasks occuring in the second month after implementation. This lagged response contrasts with the immediate positive effect on pay per task after the ordinance went into effect, suggesting a gradual demand response. In the pooled specification, we estimate an average reduction of 26 percent in monthly tasks over the entire post-ordinance period (column (5) of Table 3 Panel (A)). Combining the extensive and intensive margins, Figure 6 Panel (B) displays effects on

[18] In addition, exposed drivers might reduce the percentage of their trips done in Seattle after the reform as the size of the market shrinks, as in Figure 4.

deliveries per month in levels, inclusive of zeros. The results are largely consistent with the effects in logs—we find a decline of 10-20 tasks per month from a pre-period average of 106.

Together, Figures 5 and 6 suggest that, for highly attached workers, the ordinance increased pay per delivery, but decreased the number of delivery tasks completed per month after first post-policy month. Strikingly, Figure 7 shows that when we combine these opposing effects and evaluate monthly total delivery earnings, there is *no* effect on total delivery earnings among highly attached workers after the first month. In the first month (month zero), monthly earnings rise as earnings per trip jump immediately before any significant decline in demand occurs. However, after the initial month, the two opposing effects described above offset, leading to no net impact on monthly delivery earnings. Pooling the entire post-ordinance period we estimate a null average effect on monthly delivery earnings among highly attached workers (see column (5) of Table 4 Panel A).

In Figure 7, we additionally break down the effect on monthly total earnings into the contributions from base earnings and from tips. Interestingly, we find that even with declining deliveries per month, the ordinance led to an increase in monthly total base pay for exposed workers, as intended. While the magnitude of the increase attenuates after the initial month, declining trips do not fully offset the increases in base pay per trip. Thus, if average tips per delivery had remained unchanged, the policy would have increased drivers' monthly earnings on net. However, since tips comprise the majority of delivery pay and *did* fall after the reform, the decline in monthly tip earnings exactly offsets the base earnings gains and drives the change in total earnings down to zero (see also columns (1) and (3) of Table 4 Panel A).

Up to now, we have seen no meaningful increases in delivery earnings among attached workers. In theory, workers can offset lower delivery tasks by engaging more in other gig work of similar nature, for example, rideshare work. Appendix Figure A.5 Panels (A), (B), and (C) examine, respectively, an indicator for any rideshare work, the number of rideshare tasks completed, and rideshare earnings. While we do find some evidence of a small increase in rideshare work, the associated rise in earnings is negligible and not statistically significant. On net, total earnings among attached workers remain approximately unchanged when incorporating the rideshare margin.

We next turn to the results for the sample of less-attached incumbent workers who did 20 or fewer monthly tasks over the five months leading up to the policy implementation. Panel (B) of Tables 2, 3, and 4 and Appendix Figure A.6 present the main results for this group of less-attached workers. In column (5) of Table 2 Panel (B) and Appendix Figure A.6 Panel (A), we find a nearly identical effects on per-task base pay, tips, and total earnings as for more-attached workers. Meanwhile, in contrast to the more-attached drivers, we find no significant declines in the number of delivery tasks completed per month in columns (3) and (5) of Table 3 Panel (B) and Appendix Figure A.6 Panels (B) and (C)—though it should be stressed that only a small minority of less-attached drivers remained active in the post-policy period at all, so the effects on logged outcomes conditional on activity should be interpreted with extreme caution. Overall, we find in column (5) of Table 4 Panel (B) and Appendix Figure A.6 Panel (D) that the dollar increase in total monthly delivery earnings for less-attached workers (inclusive of zeros) is small and positive although not statistically significant. One should note, however, that while these effect magnitudes are small, they represent a roughly 30 percent increase over baseline monthly earnings. Thus it is possible that although the effects are too small for us to detect in our sample, small earnings gains accrued across a large number of occasional delivery drivers, potentially including post-reform entrants and infrequent drivers who are less likely to sign up for the Gridwise app.[19]

Overall, this section suggests that, among attached workers, the ordinance led to higher total delivery earnings in the first event month and then no net increases in delivery earnings afterwards. This is driven by the following two facts. First, higher delivery base pay per task was offset by an immediate fall in tips per delivery task. Second, workers completed fewer delivery tasks per month after the initial event month. Less-attached workers experienced a similar increase in delivery pay per task, but no

[19] Fisher (2024) argues that small welfare gains distributed across a very large set of occasional gig economy drivers may amount to substantial welfare improvements in the aggregate.

decreases in delivery tasks completed per month. They achieved some gains in delivery earnings, but these gains are small and not statistically significant.

6 Discussion and Conclusion

As a growing number of jurisdictions consider extending minimum pay policies to workers in the gig economy who compete in a spot market for tasks, it is important to understand whether such policies make gig workers better off in practice. In this paper, we analyze the impacts of a task-level minimum pay ordinance for platform-based delivery workers in Seattle and find that the gains from higher base pay were offset by two main forces. First, while the Seattle pay standard only applied to base pay rates, app-based delivery workers derive a majority of their earnings from tips, and reductions in tipping substantially offset the increases in base pay. Second, a simultaneous decline in demand for deliveries following the reform and an increase in competition from labor market entrants reduced total monthly earnings, such that total monthly earnings did not increase on net.[20]

These results highlight the challenges of raising pay in settings where there is free entry of workers and no clear distinction between "insiders" who have covered jobs and "outsiders" who would like a covered job but cannot find one—particularly in cases like delivery work with free entry. In traditional markets the winners (those with covered jobs) and losers (those without) are distinct individuals, so the optimality of a minimum wage depends on how the jobs are rationed across individuals (Luttmer, 2007; Lee and Saez, 2012; Gerritsen, 2017). By contrast, in a platform market where tasks are rationed across all participants, the winners and losers are effectively the same individuals. Hence, with free entry and an unchanging outside option, a task-level pay standard may fail to raise expected pay for any individual driver. Without imposing barriers to entry—which would potentially undermine the flexibility that platform gig work offers to workers—it

[20] An alternative hypothesis is that the decline in tasks per month may be driven by voluntary reductions in labor supply in response to higher pay per task, consistent with an earlier literature on income targeting in taxi markets (although recent evidence from Buchholz, Shum and Xu (2025) suggests that what appears to be targeting behavior may be illusory and labor supply elasticities are generally non-negative) (Camerer et al., 1997; Farber, 2005, 2008; Thakral and Tô, 2021). While we think that income targeting is unlikely to account for the full reduction in tasks we observe, we are currently conducting further analysis to assess impacts on hours spent looking for tasks.

will likely be challenging to craft policies that ensure higher pay for gig workers.[21]

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[21] As an illustrative case, recent efforts in New York City to scale task-level minimum pay rates for ride-hailing drivers based on average utilization rates (i.e. time on trip per hour) have led to apps imposing "blackouts" that effectively limit entry into the market. See https://www.bloomberg.com/graphics/2024-uber-lyft-nyc-drivers-pay-lockouts/ (accessed March 20, 2025).

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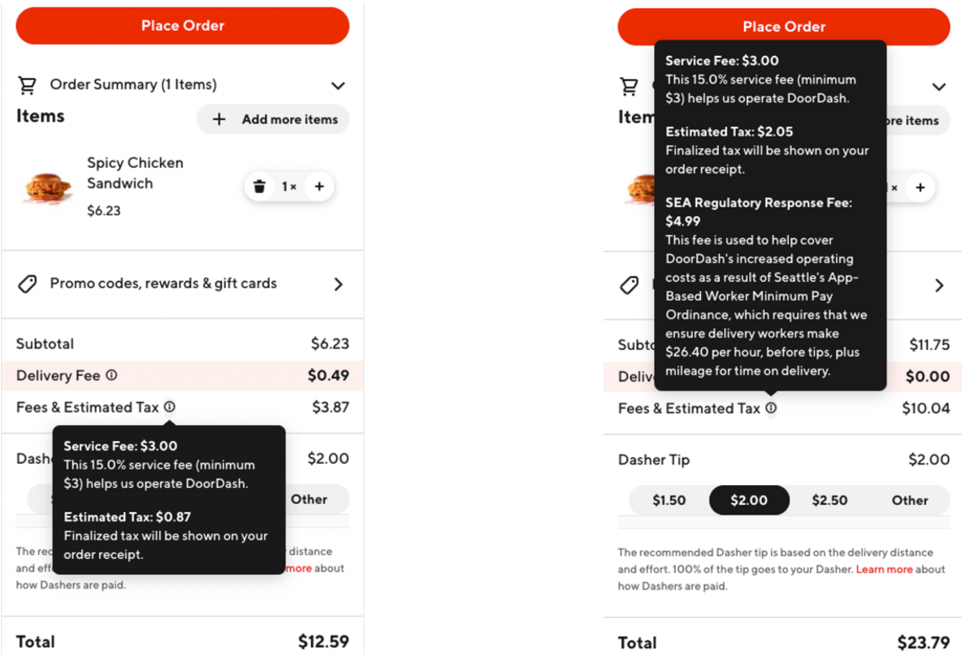
Figures

Figure 1: Fee Differences, by Region

(A) DoorDash, Spokane

(B) DoorDash, Seattle

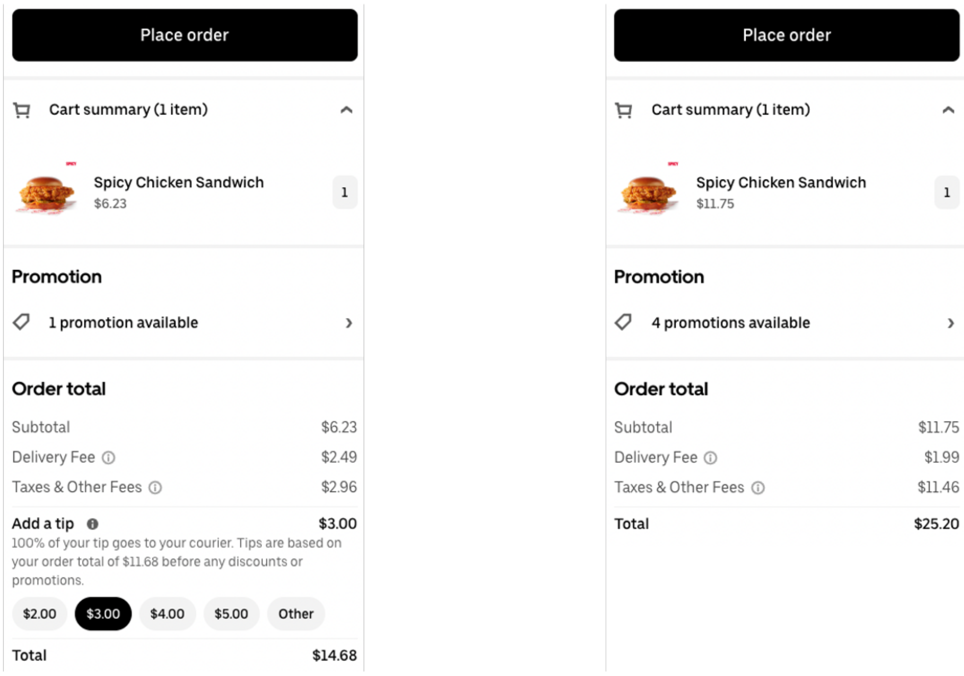


Notes: Panels (A) and (B) show screenshots of checkout pages for orders placed on the DoorDash app with shipping addresses in Spokane and Seattle, respectively. Text in the black boxes shows details of fees and estimated taxes of the respective orders. Screenshots recorded in the second half of 2024.

Figure 2: Tipping Differences, by Region

(A) Uber Eats, Spokane

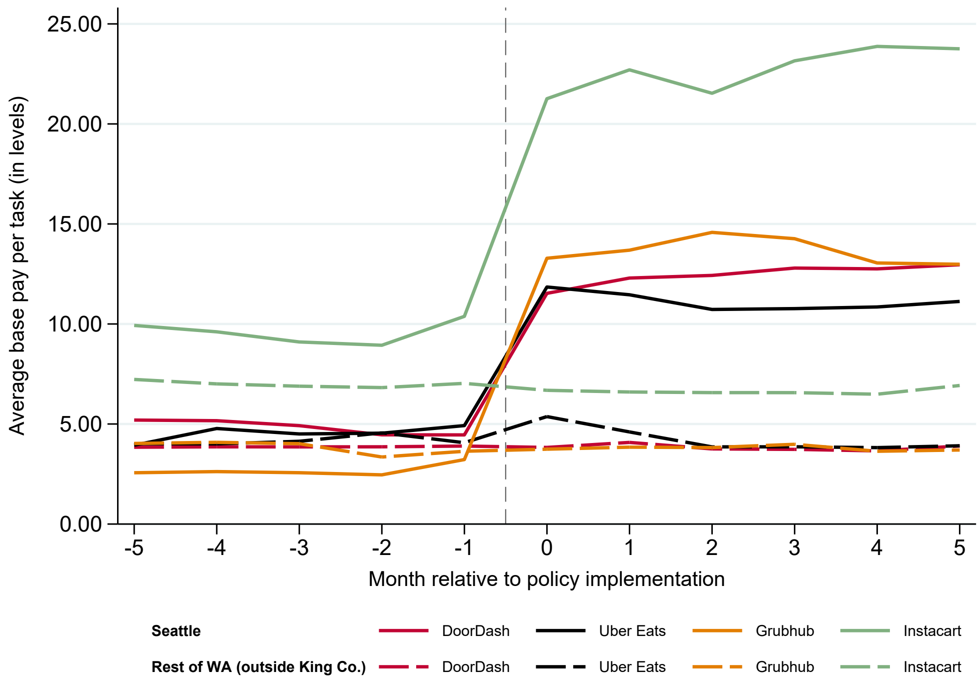
(B) Uber Eats, Seattle



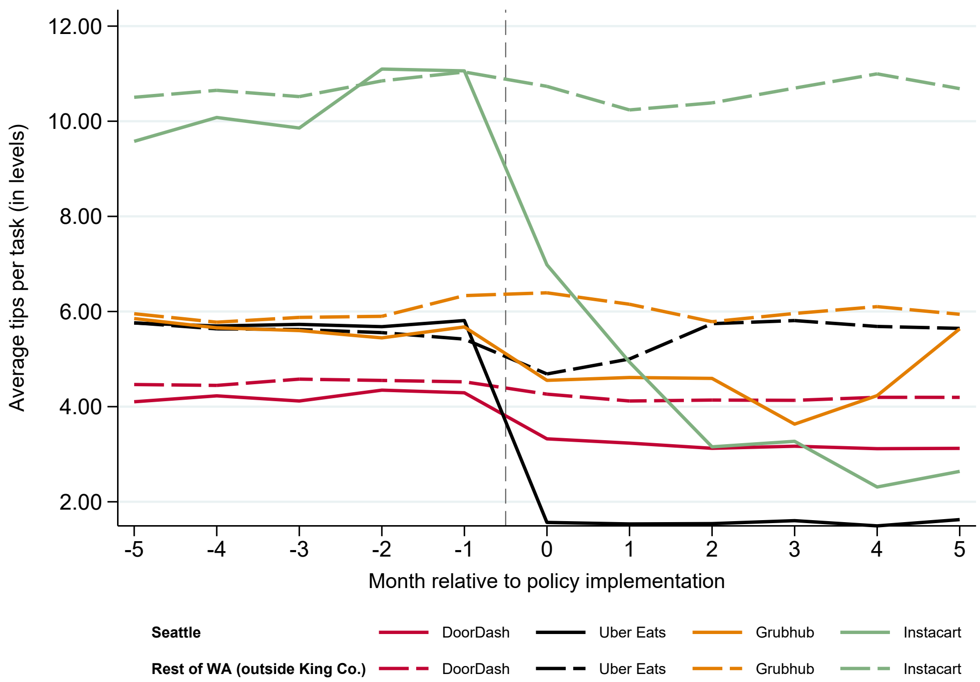
Notes: Panels (A) and (B) show screenshots of checkout pages for orders placed on the Uber Eats's app with shipping addresses in Spokane and Seattle, respectively. In Panel (A), the section labeled "add a tip" provides customers the option to choose a tip amount; this section is absent for Seattle in Panel (B). Screenshots recorded in the second half of 2024.

Figure 3: Average Pay Per Task, by Delivery Platform and Region

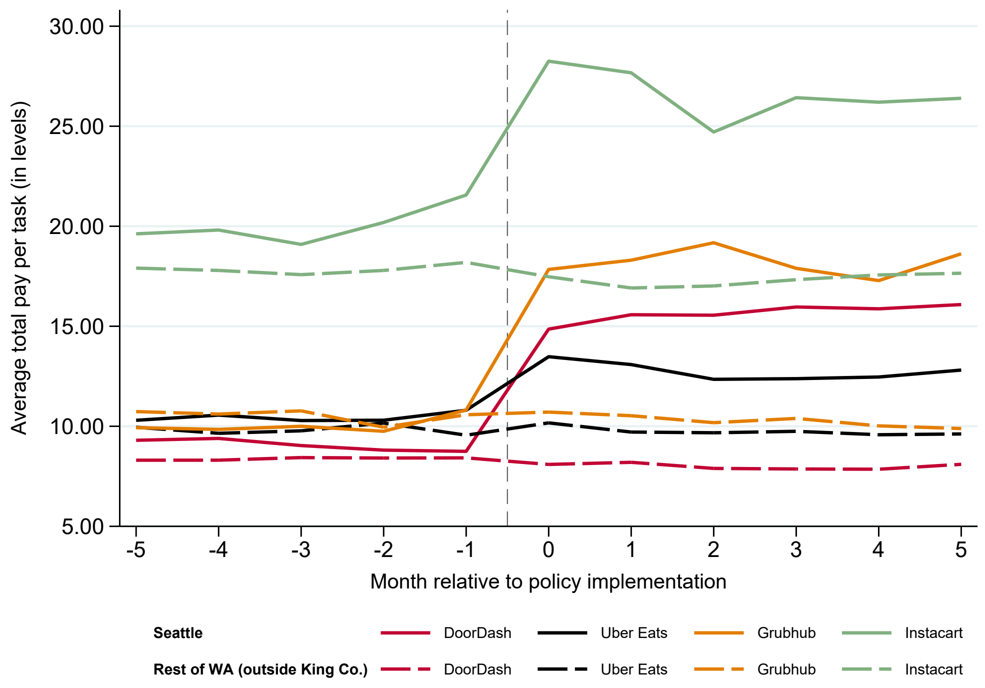
(A) Average Base Pay Per Task



(B) Average Tips Per Task



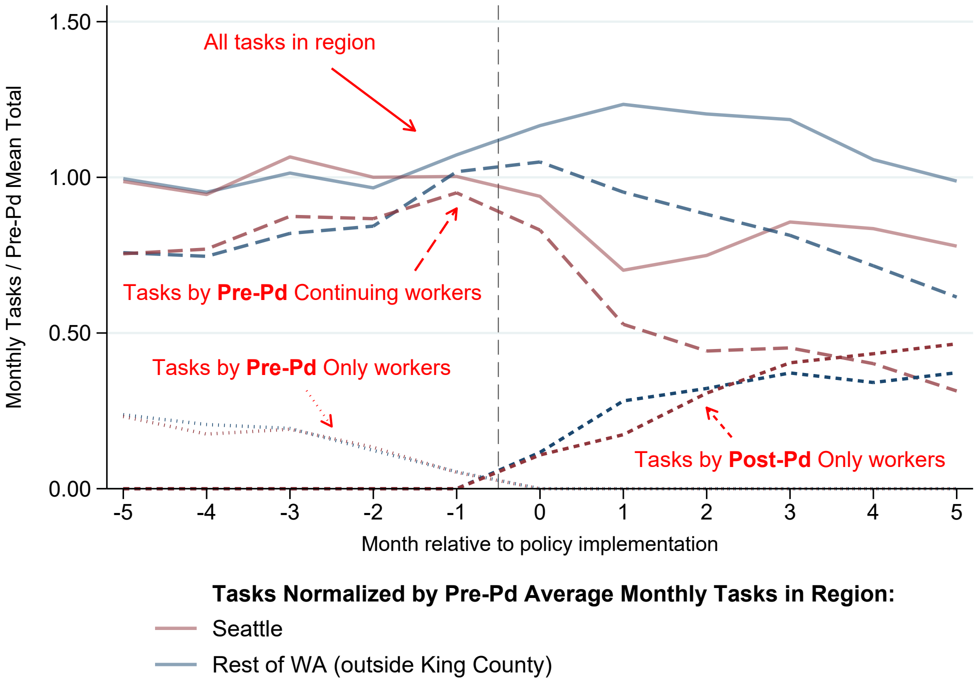
(C) Average Total Pay Per Task



Notes: Panel (A) plots the monthly time series of average base pay per task by delivery platform and region. Solid lines in red, orange, green, and black denote observations of delivery platforms DoorDash, Grubhub, Instacart, and Uber Eats in Seattle, respectively. Dashed lines denote the observations of corresponding delivery platforms in Washington State outside King County. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Panels (B) and (C) present corresponding plots for outcomes average tips per task and average total pay per task.

Source: Authors' analysis of Gridwise data.

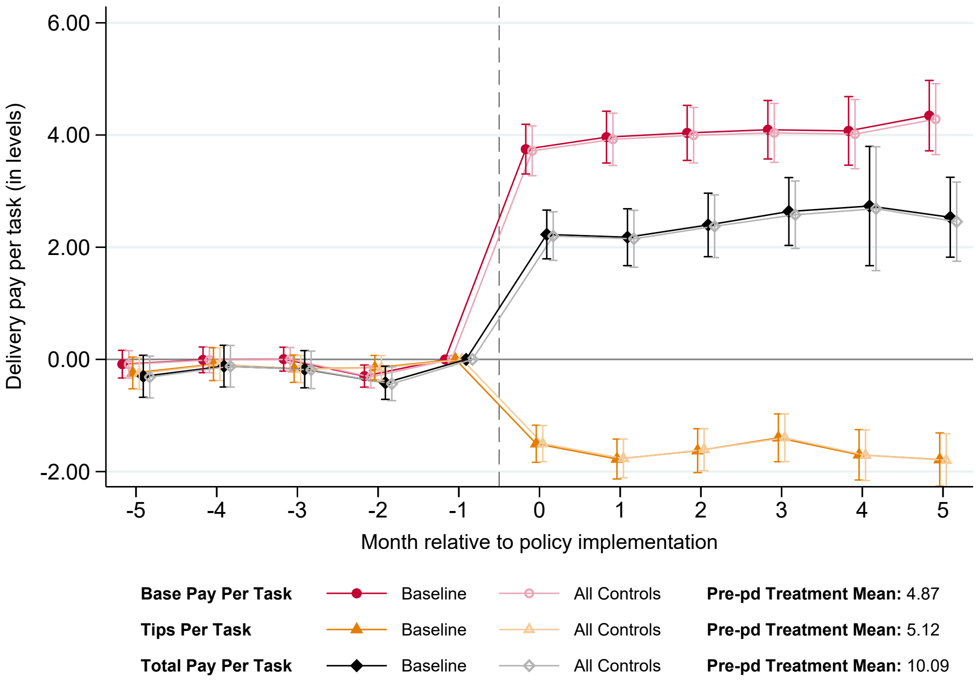
Figure 4: Delivery Tasks by All Workers, Incumbents, Workers Who Exit, and New Entrants by Region



Notes: This figure plots the monthly time series of delivery tasks completed by all workers, incumbents, workers who exit, and new entrants by region. All values are normalized by the pre-ordinance average monthly total delivery tasks completed by all workers in the respective regions. Solid lines in red and blue denote normalized monthly total delivery tasks completed by all workers in Seattle and Washington State outside King County, respectively. Longer-dashed lines denote normalized monthly delivery tasks completed by workers who were active in the pre-ordinance period and continued being active post-ordinance (incumbents) in corresponding regions. Shorter-dashed lines denote normalized monthly delivery tasks completed by workers who were not active in the pre-ordinance period and only started to be active post-ordinance (new entrants) in corresponding regions. Faint dotted lines denote normalized monthly delivery tasks completed by workers who were active in the pre-ordinance period but became inactive post-ordinance (workers who exit) in corresponding regions. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance.

Source: Authors' analysis of Gridwise data.

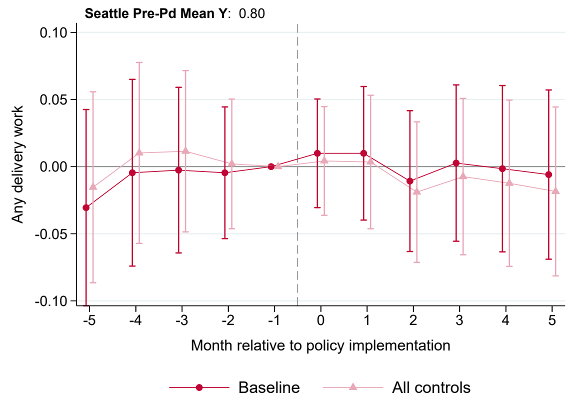
Figure 5: Delivery Pay Per Task



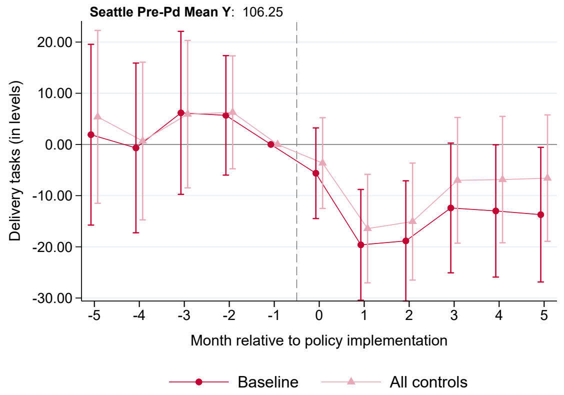
Notes: This figure plots estimates of β_k from Equation (1) using the sample of more-attached workers on three outcomes for delivery pay per task: delivery base pay per task (in red with circle markers), delivery tips per task (in orange with triangle markers), and delivery total pay per task (in black with diamond markers). All outcomes are calculated in levels. Outcomes corresponding to zero delivery tasks in a given event month are coded as missing in that month. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Worker fixed effects and event year-month fixed effects are included in the estimation. Plots in darker colors with solid markers are estimates from Equation (1) without including additional controls (Baseline). Plots in lighter colors with hollow markers are estimates from Equation (1) including the full set of additional controls (All Controls)—a set of pre-policy individual worker covariates interacted with indicators for event months. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. All samples are restricted to more-attached workers, defined as workers who performed delivery tasks above the median in the pre-policy period. The whiskers represent 95 percent confidence intervals. Standard errors are clustered at the worker level. Text on the right-hand side of the legend presents means of corresponding outcomes prior to the ordinance for exposed workers.

Figure 6: Delivery Tasks

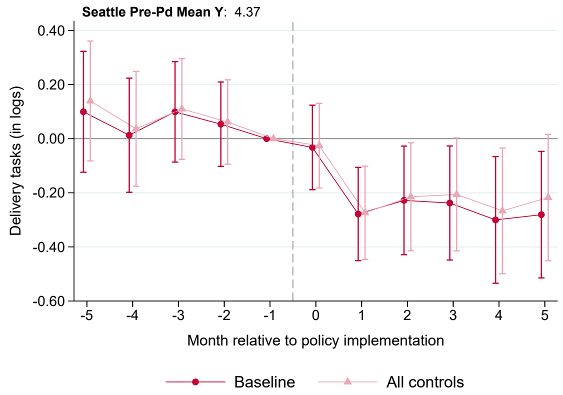
(A) Any



(B) Levels

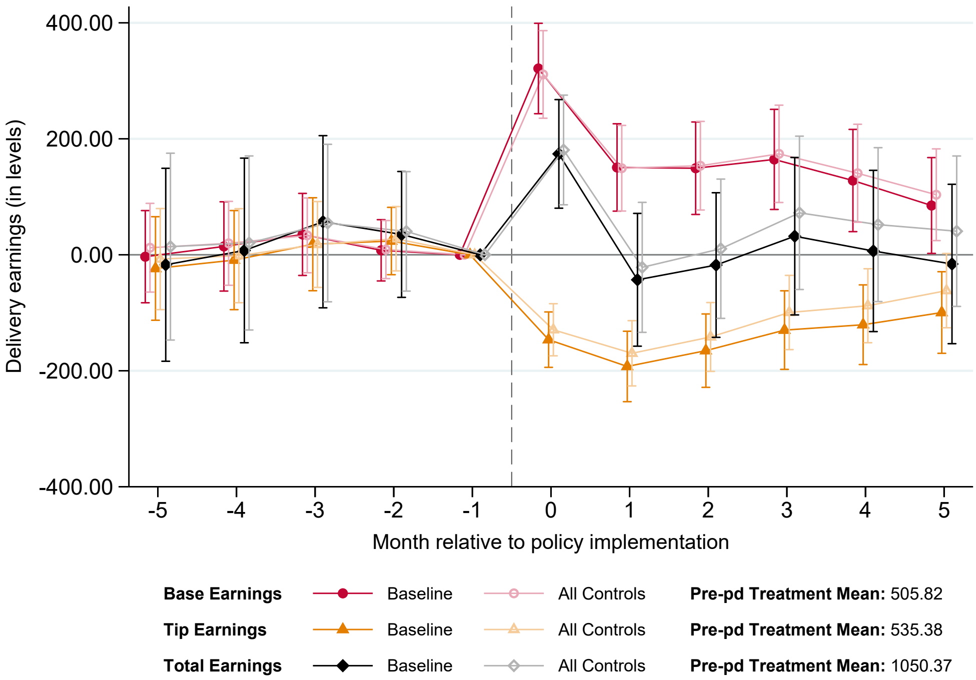


(C) Logs



Notes: Panels (A), (B), and (C) plot, respectively, estimates of β_k from Equation (1) using the sample of more-attached workers on three different forms of monthly completed delivery tasks: an indicator for performing any delivery tasks per month (Panel A), the number of monthly completed delivery tasks in levels (Panel B), and the number of monthly completed delivery tasks in logs (Panel C). Outcomes in levels are inclusive of zero values. Outcomes in logs corresponding to zero values in levels in a given event month are coded as missing in that month. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Worker fixed effects and event year-month fixed effects are included in the estimation. Plots in darker colors with circle markers are estimates from Equation (1) without including additional controls (Baseline). Plots in lighter colors with triangle markers are estimates from Equation (1) including the full set of additional controls (All controls)—a set of pre-policy individual worker covariates interacted with indicators for event months. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. All samples are restricted to more-attached workers, defined as workers who performed delivery tasks above the median in the pre-policy period. The whiskers represent 95 percent confidence intervals. Standard errors are clustered at the worker level. Text in the top left-hand corner in each panel presents the means of the corresponding outcome prior to the ordinance for exposed workers.

Figure 7: Delivery Earnings



Notes: This figure plots estimates of β_k from Equation (1) using the sample of more-attached workers on three outcomes for monthly delivery earnings: monthly delivery base earnings (in red with circle markers), monthly delivery tip earnings (in orange with triangle markers), and monthly delivery total earnings (in black with diamond markers). All outcomes are calculated in levels, inclusive of zero values. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Worker fixed effects and event year-month fixed effects are included in the estimation. Plots in darker colors with solid markers are estimates from Equation (1) without including additional controls (Baseline). Plots in lighter colors with hollow markers are estimates from Equation (1) including the full set of additional controls (All Controls)—a set of pre-policy individual worker covariates interacted with indicators for event months. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. All samples are restricted to more-attached workers, defined as workers who performed delivery tasks above the median in the pre-policy period. The whiskers represent 95 percent confidence intervals. Standard errors are clustered at the worker level. Text on the right-hand side of the legend presents means of corresponding outcomes prior to the ordinance for exposed workers.

Tables

Table 1: Descriptive Statistics

	Seattle workers		Non-Seattle workers		Diff. from Seattle workers	
	Mean	SD	Mean	SD	Δ	SE
	(1)	(2)	(3)	(4)	(5)	(6)
A. More-attached workers						
Number of delivery tasks (monthly average)	106.2	[99.7]	93.6	[83.4]	12.6**	(5.42)
Delivery earnings (monthly average)	1050.4	[990.9]	926.3	[859.1]	124.1**	(55.1)
Delivery earnings per task	9.91	[2.82]	10.1	[3.60]	-0.17	(0.21)
Delivery base earnings per task	4.81	[1.34]	4.40	[1.44]	0.41***	(0.087)
Delivery tip earnings per task	5.02	[1.93]	5.62	[2.69]	-0.61***	(0.15)
Months active in delivery	3.99	[1.28]	4.06	[1.20]	-0.072	(0.075)
Days active in delivery (monthly average)	12.6	[7.19]	12.6	[7.26]	-0.061	(0.44)
Rideshare earnings (monthly average)	77.3	[447.8]	70.9	[372.0]	6.41	(24.2)
N workers	377		908			
B. Less-attached workers						
Number of delivery tasks (monthly average)	5.99	[5.75]	5.61	[5.52]	0.38	(0.37)
Delivery earnings (monthly average)	61.9	[64.9]	58.4	[64.1]	3.50	(4.28)
Delivery earnings per task	11.6	[6.70]	11.2	[5.68]	0.35	(0.39)
Delivery base earnings per task	6.68	[6.45]	5.47	[3.51]	1.22***	(0.29)
Delivery tip earnings per task	4.77	[2.81]	5.63	[3.88]	-0.86***	(0.24)
Months active in delivery	1.98	[1.14]	2.07	[1.20]	-0.089	(0.079)
Days active in delivery (monthly average)	1.69	[1.72]	1.71	[1.62]	-0.019	(0.11)
Rideshare earnings (monthly average)	825.6	[1697.1]	138.9	[710.6]	686.8***	(68.0)
N workers	291		997			

Notes: This table reports means and standard deviations of key characteristics prior to the ordinance for Seattle workers (columns 1-2) and non-Seattle workers (columns 3-4), and estimated differences in characteristics and standard errors between the two groups (columns 5-6) from an equation regressing each characteristic on an indicator for Seattle workers. Seattle (non-Seattle) workers refer to exposed (not-exposed) workers in the text, defined as workers with intensity of exposure to Seattle above 80% (below 20%) as described in Section 3.2. Panels (A) and (B) calculate the statistics on the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 2: Effects on Delivery Pay Per Task

	Base pay per task		Tips per task		Total pay per task	
	(1)	(2)	(3)	(4)	(5)	(6)
A. More-attached workers						
Treated × Post	4.086***	4.046***	-1.501***	-1.493***	2.611***	2.578***
	(0.214)	(0.214)	(0.142)	(0.137)	(0.208)	(0.207)
N workers	1,265	1,265	1,265	1,265	1,265	1,265
Treatment pre-pd mean Y	4.875	4.875	5.126	5.126	10.093	10.093
B. Less-attached workers						
Treated × Post	4.204***	4.474***	-1.597***	-1.582***	2.621***	2.900***
	(0.452)	(0.474)	(0.279)	(0.291)	(0.446)	(0.480)
N workers	941	941	941	941	941	941
Treatment pre-pd mean Y	6.437	6.437	4.814	4.814	11.344	11.344
Controls		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for the post-ordinance period, on three outcomes for delivery pay per task: delivery base pay per task (columns 1-2), delivery tips per task (columns 3-4), and delivery total pay per task (columns 5-6). All outcomes are calculated in levels, inclusive of zero values. Outcomes corresponding to zero delivery tasks in a given event month are coded as missing in that month. Worker fixed effects and event month fixed effects are included in the estimation. Columns 1, 3, and 5 present estimates without including additional controls, and columns 2, 4, and 6 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with indicators for post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates on the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. The rows of "treatment pre-period mean Y" report means of corresponding outcomes prior to the ordinance for exposed workers. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 3: Effects on Delivery Tasks

	Any		Levels		Logs	
	(1)	(2)	(3)	(4)	(5)	(6)
A. More-attached workers						
Treated × Post	0.009	-0.010	-16.482**	-12.916**	-0.262***	-0.261***
	(0.024)	(0.022)	(5.171)	(4.346)	(0.066)	(0.065)
N workers	1,285	1,285	1,285	1,285	1,265	1,265
Treatment pre-pd mean Y	0.798	0.798	106.247	106.247	4.359	4.359
B. Less-attached workers						
Treated × Post	0.006	-0.021	-0.466	-0.781	0.064	0.162
	(0.022)	(0.021)	(1.276)	(1.415)	(0.132)	(0.129)
N workers	1,288	1,288	1,288	1,288	941	941
Treatment pre-pd mean Y	0.396	0.396	5.993	5.993	2.117	2.117
Controls		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for post-ordinance period, on three different forms of monthly completed delivery tasks: an indicator for performing any delivery tasks per month (columns 1-2), the number of monthly completed delivery tasks in levels (columns 3-4), and the number of monthly completed delivery tasks in logs (columns 5-6). Outcomes in levels are inclusive of zero values. Outcomes in logs corresponding to zero values in levels in a given event month are coded as missing in that month. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, and 5 present estimates without including additional controls, and columns 2, 4, and 6 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with indicators for post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates on the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. The rows of "treatment pre-period mean Y" report means of corresponding outcomes prior to the ordinance for exposed workers. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 4: Effects on Delivery Earnings

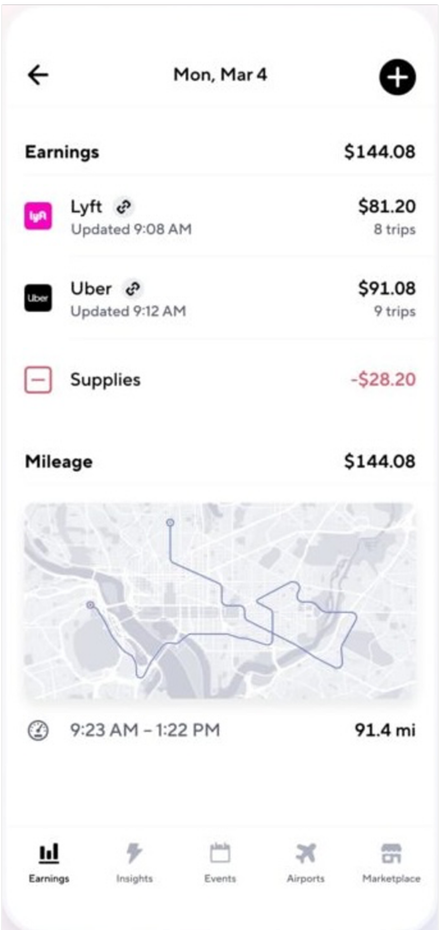
	Base earnings		Tip earnings		Total earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
A. More-attached workers						
Treated × Post	155.637***	157.126***	-144.194***	-122.304***	6.174	29.858
	(35.667)	(33.995)	(27.647)	(22.143)	(54.710)	(49.324)
N workers	1,285	1,285	1,285	1,285	1,285	1,285
Treatment pre-pd mean Y	505.818	505.818	535.381	535.381	1050.368	1050.368
B. Less-attached workers						
Treated × Post	33.224**	32.602**	-14.245**	-15.199*	18.752	17.046
	(10.789)	(11.883)	(5.293)	(5.968)	(15.255)	(16.917)
N workers	1,288	1,288	1,288	1,288	1,288	1,288
Treatment pre-pd mean Y	30.896	30.896	30.456	30.456	61.947	61.947
Controls		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for post-ordinance period, on three outcomes for monthly delivery earnings: monthly delivery base earnings (columns 1-2), monthly delivery tip earnings (columns 3-4), and monthly delivery total earnings (columns 5-6). All outcomes are calculated in levels, inclusive of zero values. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, and 5 present estimates without including additional controls, and columns 2, 4, and 6 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with indicators for post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates on the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. The rows of "treatment pre-period mean Y" report means of corresponding outcomes prior to the ordinance for exposed workers. * $p < .10$, ** $p < .05$, *** $p < .01$.

Online Appendices [Not for Publication]

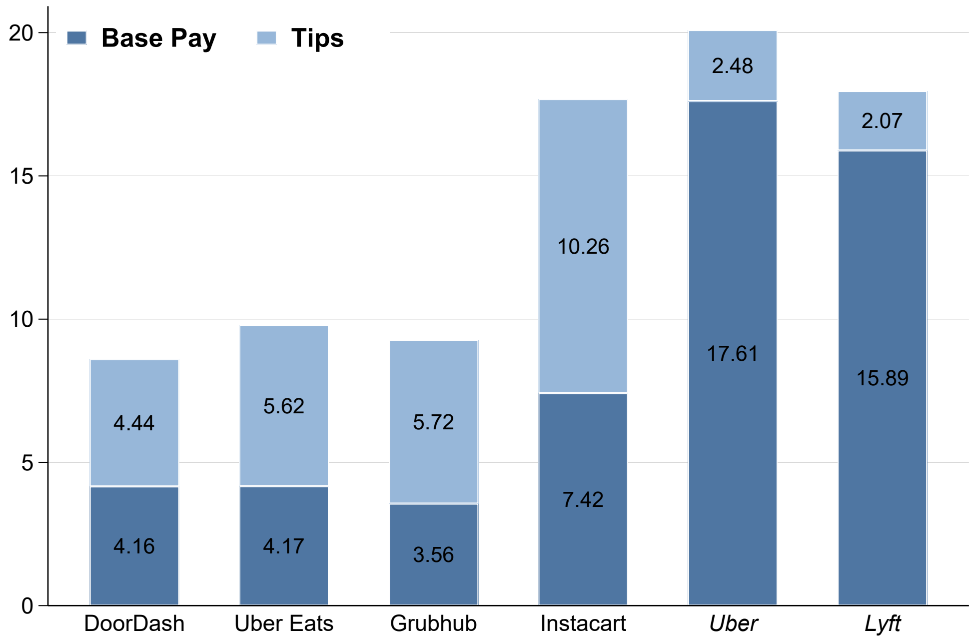
A Appendix Figures

Figure A.1: Gridwise App Screenshot



Notes: This figure shows an example screeshot of Gridwise app. Screenshot recorded in the second half of 2024.

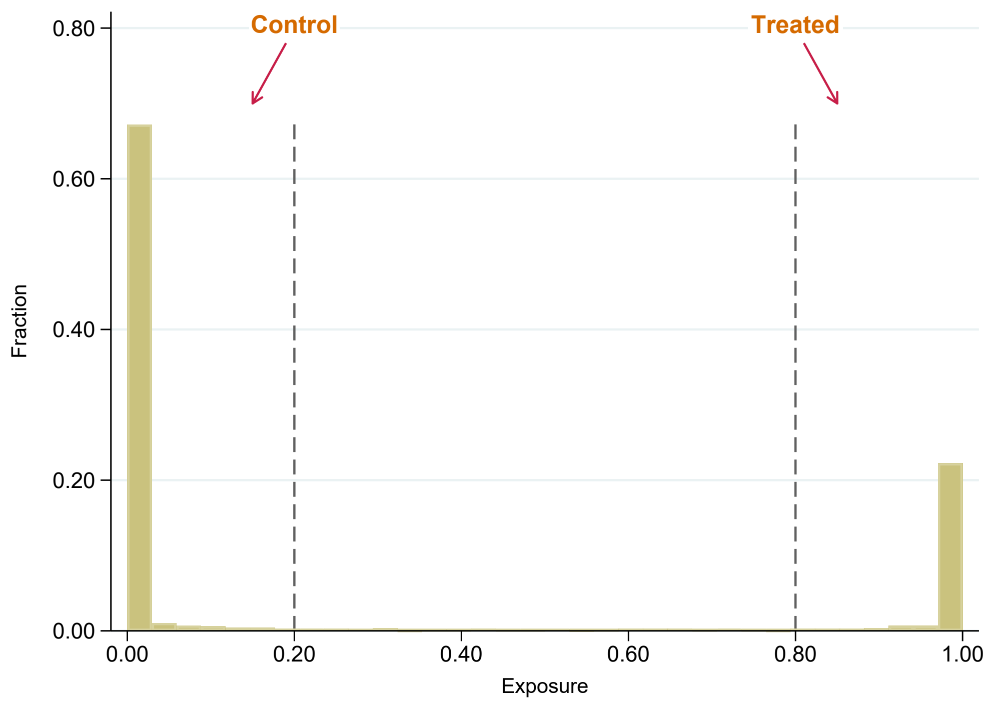
Figure A.2: Average Pay Per Task, Washington State (August 2023–December 2023)



Notes: This figure calculates average base pay per task and average tips per task for tasks completed in Washington State from August 2023 to December 2023 on delivery platforms DoorDash, Grubhub, Instacart, and Uber Eats and rideshare platforms Lyft and Uber. Numbers presented on the top of bars report average base pay and tips per task on corresponding platforms.

Source: Authors' tabulations using Gridwise data.

Figure A.3: Distribution of Exposure Measure across Workers



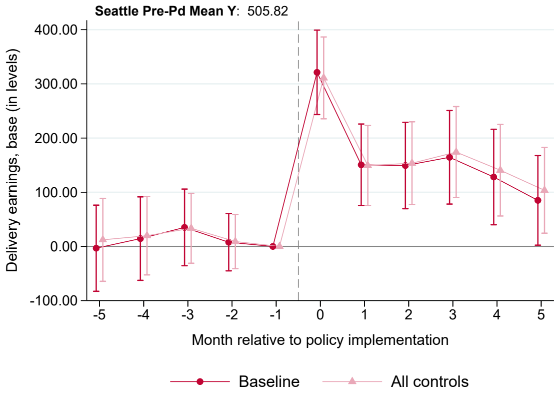
Notes: This figure presents the distribution of constructed measure of exposure to Seattle across workers in our sample. Exposure is calculated as the share of delivery earnings a worker made from performing Seattle tasks prior to the Seattle minimum pay ordinance's implementation. For a detailed description of the calculation, see Section 3.2.

Source: Authors' analysis of Gridwise data.

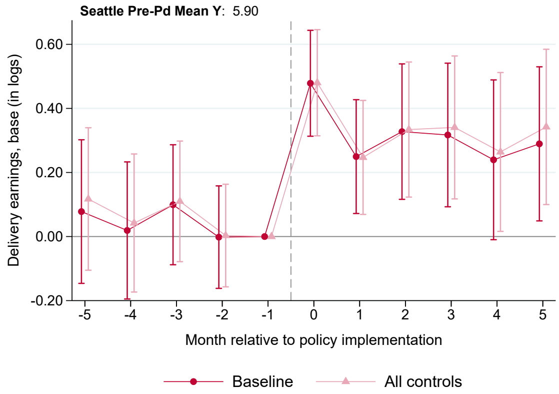
Figure A.4: Delivery Earnings – More-Attached Workers

(A) Base Earnings

(I) Levels

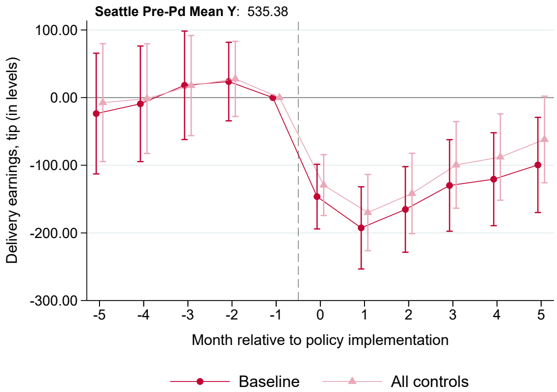


(II) Logs

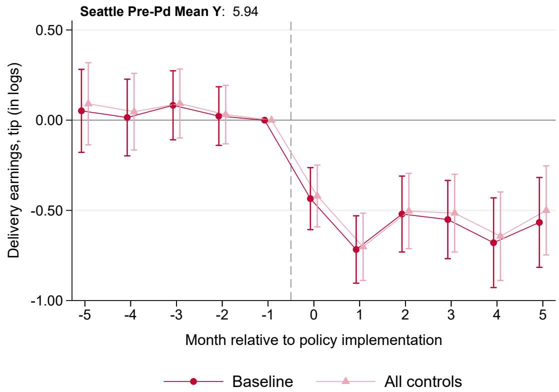


(B) Tip Earnings

(I) Levels

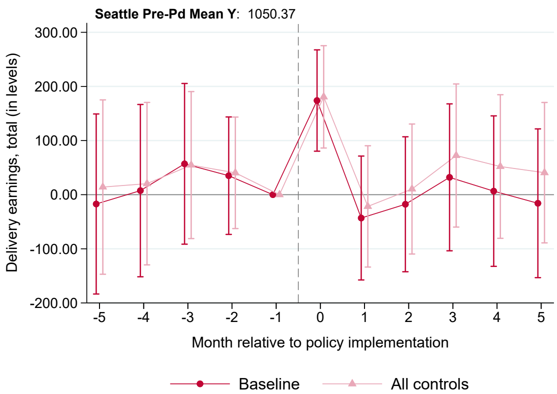


(II) Logs

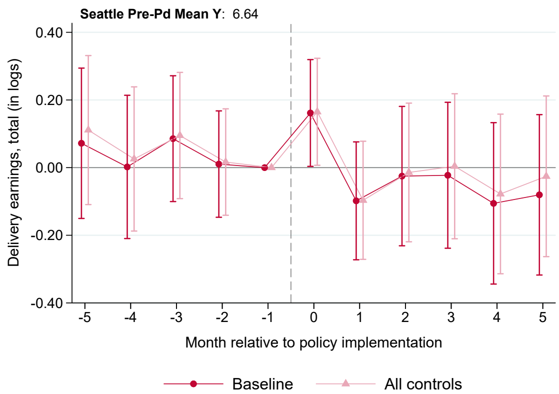


(C) Total Earnings

(I) Levels



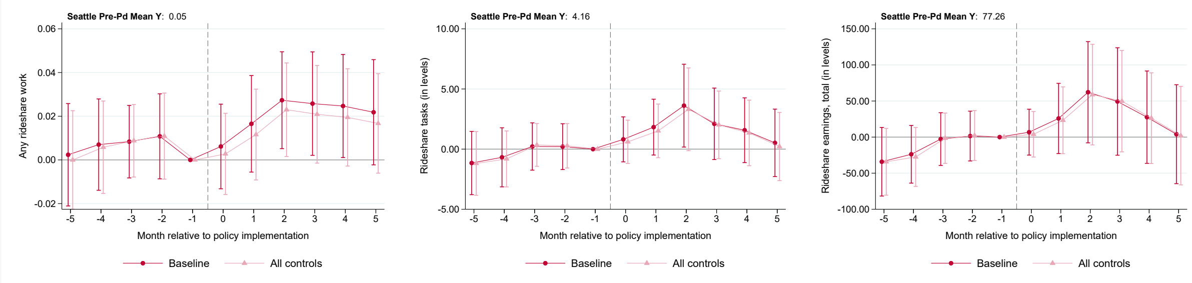
(II) Logs



Notes: Panels (A), (B), and (C) plot, respectively, estimates of β_k from Equation (1) using the sample of more-attached workers on three outcomes for monthly delivery earnings: monthly delivery base earnings (Panel A), monthly delivery tip earnings (Panel B), and monthly delivery total earnings (Panel C). In each panel, Panel (I) examines corresponding outcomes in levels, inclusive of zero values, and Panel (II) examines corresponding outcomes in logs, among which corresponding to zero values in levels in a given event month are coded as missing in that month. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Worker fixed effects and event year-month fixed effects are included in the estimation. Plots in darker colors with circle markers are estimates from Equation (1) without including additional controls (Baseline). Plots in lighter colors with triangle markers are estimates from Equation (1) including the full set of additional controls (All controls)—a set of pre-policy individual worker covariates interacted with indicators for event months. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. All samples are restricted to more-attached workers, defined as workers who performed delivery tasks above the median in the pre-policy period. The whiskers represent 95 percent confidence intervals. Standard errors are clustered at the worker level. Text in the top left-hand corner in each panel presents the mean of the corresponding outcome prior to the ordinance for exposed workers.

Figure A.5: Rideshare Work and Earnings

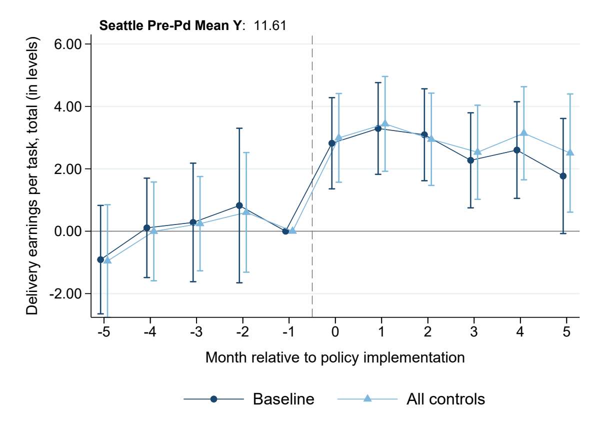
(A) Any Rideshare Work (B) # Rideshare Tasks (C) Rideshare Earnings



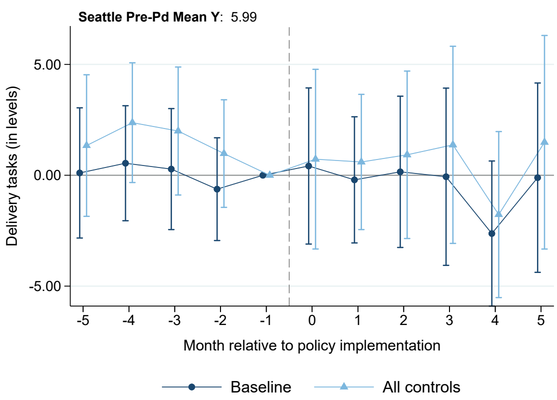
Notes: Panels (A), (B), and (C) plot, respectively, estimates of β_k from Equation (1) using the sample of more-attached workers on three outcomes for rideshare work and earnings: an indicator for performing any rideshare tasks per month (Panel A), the number of monthly completed rideshare tasks (Panel B), and monthly rideshare total earnings (Panel C). All outcomes are calculated in levels, inclusive of zero values. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Worker fixed effects and event year-month fixed effects are included in the estimation. Plots in darker colors with circle markers are estimates from Equation (1) without including additional controls (Baseline). Plots in lighter colors with triangle markers are estimates from Equation (1) including the full set of additional controls (All controls)—a set of pre-policy individual worker covariates interacted with indicators for event months. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. All samples are restricted to more-attached workers, defined as workers who performed delivery tasks above the median in the pre-policy period. The whiskers represent 95 percent confidence intervals. Standard errors are clustered at the worker level. Text in the top left-hand corner in each panel presents the mean of the corresponding outcome prior to the ordinance for exposed workers.

Figure A.6: Delivery Tasks and Earnings – Less-Attached Workers

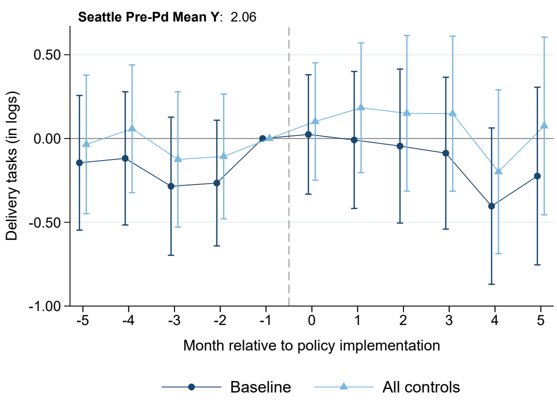
(A) Total Delivery Pay Per Task



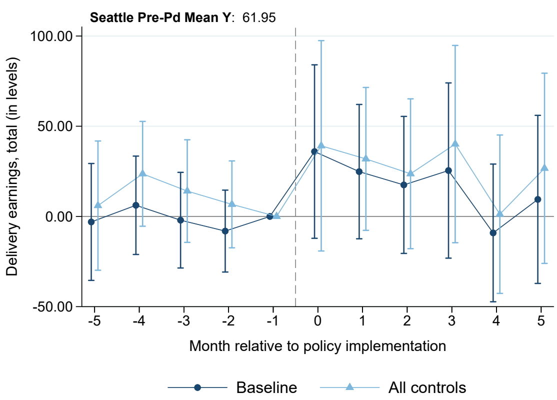
(B) # Delivery Tasks Including Zeros



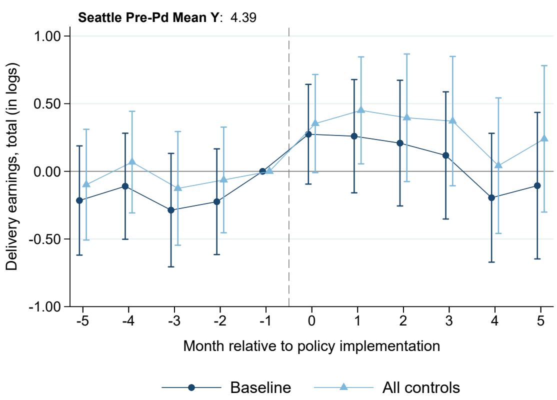
(C) Log(Delivery Tasks) if Positive



(D) Total Delivery Earnings Including Zeros



(E) Log(Total Delivery Earnings) if Positive



Notes: Panel (A) plots estimates of β_k from Equation (1) on total delivery pay per task using the sample of less-attached workers. The outcome is calculated in levels, among which corresponding to zero delivery tasks in a given event month are coded as missing in that month. Panels (B) and (C) present corresponding plots for the outcomes of number of monthly completed delivery tasks in levels and in logs, respectively. Panels (D) and (E) present corresponding plots for the outcomes of monthly total delivery earnings in levels and in logs, respectively. Outcomes in levels are inclusive of zero values. Outcomes in logs corresponding to zero values in levels in a given event month are coded as missing in that month. Month 0 denotes the first event month following ordinance implementation, and month -1 denotes the event month before the ordinance. Worker fixed effects and event year-month fixed effects are included in the estimation. Plots in darker colors with circle markers are estimates from Equation (1) without including additional controls (Baseline). Plots in lighter colors with triangle markers are estimates from Equation (1) including the full set of additional controls (All controls)—a set of pre-policy individual worker covariates interacted with indicators for event months. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. All samples are restricted to less-attached workers, defined as workers who performed delivery tasks below the median in the pre-policy period. The whiskers represent 95 percent confidence intervals. Standard errors are clustered at the worker level. Text in the top left-hand corner in each panel presents the mean of the corresponding outcome prior to the ordinance for exposed workers.

B Appendix Tables

Table B.1: Robustness – Effects on Delivery Pay Per Task, Continuous Exposure Effects

	Base pay per task		Tips per task		Total pay per task	
	(1)	(2)	(3)	(4)	(5)	(6)
A. More-attached workers						
Exposure × Post	4.111***	4.066***	-1.547***	-1.531***	2.582***	2.554***
	(0.212)	(0.211)	(0.140)	(0.136)	(0.210)	(0.209)
N workers	1,324	1,324	1,324	1,324	1,324	1,324
B. Less-attached workers						
Exposure × Post	4.243***	4.512***	-1.562***	-1.528***	2.693***	2.988***
	(0.449)	(0.465)	(0.278)	(0.287)	(0.438)	(0.466)
N workers	991	991	991	991	991	991
Controls		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for post-ordinance period and replacing the binary exposure measure Treat_i with a continuous exposure measure as discussed in Section 3.2, on three outcomes for delivery pay per task: delivery base pay per task (columns 1-2), delivery tips per task (columns 3-4), and delivery total pay per task (columns 5-6). All outcomes are calculated in levels, inclusive of zero values. Outcomes corresponding to zero delivery tasks in a given event month are coded as missing in that month. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, and 5 present estimates without including additional controls, and columns 2, 4, and 6 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with indicators for post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates on the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table B.2: Robustness – Effects on Delivery Tasks, Continuous Exposure Effects

	Any		Levels		Logs	
	(1)	(2)	(3)	(4)	(5)	(6)
A. More-attached workers						
Exposure × Post	0.004	-0.015	-16.626**	-13.156**	-0.266***	-0.265***
	(0.024)	(0.023)	(5.279)	(4.466)	(0.066)	(0.066)
N workers	1,344	1,344	1,344	1,344	1,324	1,324
B. Less-attached workers						
Exposure × Post	0.006	-0.022	-0.151	-0.464	0.026	0.142
	(0.023)	(0.021)	(1.394)	(1.515)	(0.133)	(0.130)
N workers	1,353	1,353	1,353	1,353	991	991
Controls		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for post-ordinance period and replacing the binary exposure measure Treat_i with a continuous exposure measure as discussed in Section 3.2, on three different forms of monthly completed delivery tasks: an indicator for performing any delivery tasks per month (columns 1-2), the number of monthly completed delivery tasks in levels (columns 3-4), and the number of monthly completed delivery tasks in logs (columns 5-6). Outcomes in levels are inclusive of zero values. Outcomes in logs corresponding to zero values in levels in a given event month are coded as missing in that month. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, and 5 present estimates without including additional controls, and columns 2, 4, and 6 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with indicators for post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates on the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table B.3: Robustness – Effects on Delivery Earnings, Continuous Exposure Effects

	Base earnings		Tip earnings		Total earnings	
	(1)	(2)	(3)	(4)	(5)	(6)
A. More-attached workers						
Exposure × Post	157.433***	160.422***	-146.743***	-124.353***	4.678	30.410
	(36.436)	(34.746)	(28.070)	(22.771)	(56.070)	(50.748)
N workers	1,344	1,344	1,344	1,344	1,344	1,344
B. Less-attached workers						
Exposure × Post	35.021**	34.082**	-13.364*	-13.983*	21.837	20.166
	(11.427)	(12.302)	(5.598)	(6.253)	(16.089)	(17.514)
N workers	1,353	1,353	1,353	1,353	1,353	1,353
Controls		✓		✓		✓

Notes: This table presents estimates of β from Equation (1) replacing the indicators for event months interacted with the term Treat_i with a single indicator for post-ordinance period and replacing the binary exposure measure Treat_i with a continuous exposure measure as discussed in Section 3.2, on three outcomes for monthly delivery earnings: monthly delivery base earnings (columns 1-2), monthly delivery tip earnings (columns 3-4), and monthly delivery total earnings (columns 5-6). All outcomes are calculated in levels, inclusive of zero values. Worker fixed effects and event year-month fixed effects are included in the estimation. Columns 1, 3, and 5 present estimates without including additional controls, and columns 2, 4, and 6 present estimates including the full set of additional controls—a set of pre-policy individual worker covariates interacted with indicators for post-ordinance period. Covariates consist of delivery (rideshare) tasks, months active in delivery (rideshare) work, share of earnings from rideshare versus delivery work, and log total earnings, all measured during the pre-policy period. Panels (A) and (B) present estimates on the samples of more-attached and less-attached workers, respectively, defined as workers who performed delivery tasks above and below the median in the pre-policy period. Standard errors in parentheses are clustered at the worker level. * $p < .10$, ** $p < .05$, *** $p < .01$.