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

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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), completely offsetting increased pay per task and leading to zero effect on monthly earnings. We find evidence that drivers experienced more unpaid idle time and longer distances driven between tasks, but find no evidence that drivers reduced their total time working on delivery apps and only limited evidence of switching from delivery to ride-hailing work. Using a simple model of the labor market for platform delivery drivers, we show that our evidence is consistent with free entry of drivers into the delivery market driving down the task-finding rate until expected earnings return to their pre-reform level. These findings highlight the challenges of raising pay in spot markets for tasks where there is free entry of workers.

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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 that 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 asso-

ciated 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 against 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.

Despite the lack of substantial long-run effects on earnings, drivers might still have been better off if their work hours declined or if they were able to maintain earnings while exerting less effort and incurring fewer costs associated with driving. To study these outcomes we use data from subsets of apps providing information on hours spent engaged with delivery apps and the locations and timing of tasks. We find no evidence that hours spent working on apps declined (though these estimates are somewhat imprecise) but we do find evidence of statistically significant and substantial declines in the number of tasks completed per hour. We also find that the minimum pay policy reduced the utilization rate (share of on-app time spent performing revenue-generating delivery tasks versus unpaid idle time) and increased wait times between tasks. Further, we find that the distance driven between subsequent tasks increased along with wait time, implying that workers continued to incur effort and vehicle costs during the additional unpaid idle time. In all, we find no evidence that incumbent drivers benefit through non-earnings margins.

Why did exposed incumbent drivers not benefit on net from the minimum pay standard? To help answer this question we interpret our empirical findings through the lens of a simple model of the app-based delivery market, closely following Hall, Horton and Knoepfle (2023) and Fisher (2024*a*). The key prediction from the model is that a trip-level minimum pay standard will only increase drivers' earnings in settings where there is not free entry of drivers and where the market-level elasticity of demand for drivers

is sufficiently small. Our finding of no earnings effect suggests that one or both of these conditions is violated in the Seattle gig delivery market. Descriptive evidence documenting a growing share of activity captured by new entrants in Seattle post-reform suggests that free entry plays an important role. Without strict barriers to entry, any increase in pay per trip will be partly offset by the entry of additional drivers, leading to increased wait times to find tasks and reducing pay per hour. With free entry (i.e. an infinite labor supply) this process continues until the effective hourly wage equals that in the wider labor market and the increased pay per task is fully offset, as we find empirically.

Our paper contributes to a large empirical literature on the impacts of minimum wage policies, including 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 employment 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.

Our work is most directly related to recent research 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

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

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 Sections 5 and 6. Section 7 interprets our findings through the lens of the model and discusses policy implications.

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 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

[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.

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, who pick up and deliver products from merchants to customers. On most platforms, workers begin by opening the driver-facing app and indicating that they are available to complete delivery tasks, at which point the app begins searching for available nearby tasks. After a wait, workers are offered individual delivery tasks, which they can either accept or reject (either proactively or by letting the offer time out). Offers generally contain information about the pickup location (typically close to the driver's current location), drop off location, estimated time and distance, and total pay. On most apps, the guaranteed pay rate for the trip is *inclusive* of tips, which are commonly made by customers upon placement of their orders.[4] Drivers can turn down tasks, but doing so typically leads to them being offered fewer and less desirable tasks in the future. At the conclusion of a delivery task, workers must decide whether to remain at their previous drop-off destination or to drive towards a desired pick-up spot. Because tasks are assigned to drivers near pick-up locations, and since drop-off destinations are often in residential areas, drivers are encouraged to drive towards "hot" zones with more restaurant activity in order to receive more frequent and more desirable offers—though they must do so at their own expense.[5]

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 August 2023 and December 2023.[6] 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

[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.

[4] 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.

[5] Drivers may also opt to drive towards their initial pick-up location to avoid drifting too far from their home location.

[6] Authors' analysis of Gridwise data.

significant portion of their income than rideshare workers do.[7] 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).[8] 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.[9,10] The ordinance came into effect on January 13, 2024.

The ordinance covers app-based delivery services performed in Seattle.[11] 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,

[7] 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).

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

[9] "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).

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

[11] 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.

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.[12] 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.[13] 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' 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

[12]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).

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

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.[14] 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. 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.

Gridwise is the only available source we are aware of that contains consistent information on all the delivery and rideshare trips done by individuals over time and across apps. Still, a limitation of the data is that it only covers the minority of drivers who are enrolled on the Gridwise app. Gridwise reports that quarterly trends in their data tightly follow the aggregates reported by the apps, with up to 98% correlation with key quarterly figures reported by major gig platforms (Gridwise Analytics, 2025). While there is no authoritative data on the number of drivers active on all apps, we compare the Gridwise sample sizes against the 2023 Census Nonemployer Statistics (the most recent available

[14] 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).

year), which tabulates the number of self-employed taxpayers by state and industry.[15] This comparison suggests that about 10% of platform drivers are covered by the Gridwise app. Since installing the app requires some effort and foresight, workers who do more gig work are likely to be over-represented relative to those who do gig work only intermittently. We note, however, that these more-intensive drivers are a primary focus of our individual analysis, both because they these are most likely to be impacted by the policy and are the drivers the policy reforms aim to benefit. We do not expect there to be any difference in coverage between treatment and control regions, nor do we expect the policy to impact coverage rates in either the treatment or control region.

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, 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, request, start, and end times, start and end 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.[16] The data include 2,844,465 tasks completed

[15]The 2023 Census Nonemployer Statistics for Washington state report 38,993 self-employed couriers and 22,800 self-employed drivers with income tax filings (U.S. Census Bureau, 2025). Note that this figure likely undercounts gig workers who do not file a Schedule C, but it also includes all couriers and taxi/limousine drivers, possibly leading to an overcount.

[16]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.

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 containing monthly delivery and rideshare tasks and earnings (including base pay, tips, bonuses, and total earnings) and exposure to work in Seattle based on the driver's share of pre-ordinance delivery earnings from Seattle. We define months as calendar-month periods relative to January 13, 2024.[17] Gridwise separately provides data on payments made by platforms to drivers that are not associated with a single task, which are either incentive payments for achieving a target or adjustment payments to ensure after-the-fact compliance with regulations or promises made to the worker.[18] We allocate these payments to a given worker-month, though cannot attribute them to specific trips. The following subsection details how we define our primary exposure measure.

The Gridwise data contains information about when and where delivery tasks took place, but the information is inconsistently reported across apps. Ideally we would observe both total on-app time and the start and end time/location of each delivery task, which would enable us to directly measure revenue-generating time separately from unpaid time cruising or idle while waiting for tasks. In practice, though, we observe total on-app time (the best available measurement of total hours worked) for DoorDash (but not the start and end time/locations of specific trips), while for Uber Eats and Grubhub, we observe the start and end time of individual trips but not overall on-app time. In order to create measure of total hours worked across apps, we develop a procedure to use the available information to impute total on-app time across all apps. In addition, we construct more detailed measures of time use that are only defined for activities on certain apps. In particular, for Uber Eats and Grubhub trips, we use the request and drop-off time/location data to measure average time and distance between subsequent delivery tasks and to calculate the utilization rate as the share of total monthly hours

[17] Months in our analysis go from the 13th of each month through the 12th of the following month.

[18] For instance, if a task takes longer than expected (for instance, due to restaurant delays) and the guaranteed pay for a delivery task that was calculated based on an initial estimate of task distance and duration turns out to be less than required by the minimum pay standard, the app may make a separate adjustment remittance to the driver.

worked that were revenue-generating trip time, following Jacobs et al. (2024).[19]

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 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).[20] Second, for each worker, we define exposure as $S/(S+N)$, the share of *classifiable* pre-policy delivery earnings coming from Seattle activities.[21] 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

[19] Details on the imputation method and evidence that the results are robust to alternative imputation assumptions will appear in a subsequent revision.

[20] 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 16.

[21] 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.

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.4. In robustness tests, we show that our main results are qualitatively unchanged when we use the continuous exposure measure (see Appendix Tables B.2, B.3, and B.4).

Throughout our analysis, we present results separately for workers with higher or 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 ten sets of individual-level outcomes on delivery work: average pay per task, any work, number of completed tasks, earnings, hours worked, active days, average number of tasks completed per hour, utilization rate, average task wait time, and average distance between tasks. 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

Sections 5 and 6.

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.[22] 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 trends in average base pay, tips, and total pay per task on the four largest delivery platforms, DoorDash, Grubhub, Instacart, and Uber Eats, in

[22]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.

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 throughout 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.[23] 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, 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.

To provide further detail about the shift in base pay rates after the minimum pay reform, Figure 4 plots the distribution of task-level base pay amounts in Seattle during the pre-reform and post-reform periods, respectively. The figure confirms that while a majority of trips had base pay less than five dollars before the pay standard went into place, essentially no trips had base pay levels below the five-dollar minimum afterwards. Notably, however, there is very little bunching of base pay at the five-dollar minimum in the post-period; instead, most of the base pay distribution shifts to higher dollar amounts. This finding suggests that the formulary minimum pay rates based on mileage and distance generally bind, rather than the five-dollar absolute minimum.

At the same time, however, Panel B of Figure 3 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

[23] 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.

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. In Appendix Figure A.3, we likewise find that the shift in the distribution of *total* payment amounts per trip is more muted than the shift in base pay rates shown in Figure 4.

We next examine how the overall volume of tasks observable in the Gridwise data evolve around policy implementation. Figure 5 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 5 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 5 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 Effects on Individual Earnings

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 6, 7, and 8 plot estimates of β_k for the main outcomes from estimating Equation (1) for more-attached workers, and Appendix Figure A.10 plots 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 for highly-attached workers in Figure 6. 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 in 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 descriptive increases in average base pay per task observed in Figure 3 Panel (A). This difference arises due to our focus on exposed *individuals* in Figure 6 as opposed to exposed *deliveries* in Figure 3. As discussed in Section 3.2, our exposure measure 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.[24]

However, Figure 6 shows that the increases in average base pay per delivery were

[24]In addition, exposed drivers might reduce the percentage of their trips done in Seattle after the reform as the size of the market shrinks (Figure 5).

offset by declining tips following the ordinance. Column (3) of Table 2 Panel (A) reports that their average tips declined by $1.51 from a pre-ordinance average of $5.12 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 $3.56 from a pre-ordinance average of $10.37, as shown in column (7) of Table 2 Panel (A) and Figure 6.

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 7 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, while the probability of completing *any* tasks was unaffected by the reform, we find that it reduced the *number* of tasks completed by continuing workers. Figure 7 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 5, 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 and/or a delay in the entry of new drivers. 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 7 Panel (B) displays effects on 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 6 and 7 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 8 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 tasks occurs. However, after the initial month, the opposing effects of increased pay per task and reduced number of tasks 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 (7) of Table 4 Panel A).

In Figure 8, 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.9 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.10 present the main results for this group of less-attached workers. In Table 2 Panel B and Appendix Figure A.10 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)-(6) of Table 3 Panel (B) and Appendix Figure A.10 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 (7) of Table 4 Panel (B) and Appendix Figure A.10 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.[25]

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

[25] Fisher (2024*b*) 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.

6 Effects on Individual Time Use and Implications for Workers

While the minimum pay law had no substantial long-run effects on driver earnings, it may still have improved driver well-being if drivers were able to maintain their earnings levels while reducing their hours spent on the app, enjoying more down time, or reducing their expenses associated with delivery driving. Importantly, though we observe a reduction in tasks completed per month, this does not necessarily indicate a reduction in total time spent working—if tasks become scarce, utilization rates (the share of working time spent on revenue-generating tasks) may fall as drivers spend more time driving in search of delivery offers (Jacobs et al., 2024). While we do not observe comprehensive data on driver time use or expenses, this section examines the available evidence.

We first study whether the minimum pay standard led to a reduction in the total hours that drivers spent doing delivery work. In theory, it is possible that higher pay rates would lead to a reduction in hours worked, due either to strong income effects or income-targeting behavior.[26] The empirical challenge, however, is that we do not consistently observe the total time that drivers spend engaged in delivery work. Ideally we would observe both revenue-generating hours and unpaid hours spent waiting or driving in search of new tasks. In practice, though, we observe *total* on-app time (the best available measurement of total hours worked) only for DoorDash sessions; for other apps, we only observe time on revenue generating trips. Accordingly, we examine effects on hours using two approaches: using a measure of hours worked constructed by imputing missing information and using only the hours worked on DoorDash, with both approaches yielding similar results.[27]

Table 5 shows the effects of the minimum pay law on total monthly hours spent en-

[26] An earlier literature on labor supply in taxi markets has raised the potential that taxi drivers stop working after hitting daily target earnings, (Camerer et al., 1997; Farber, 2005, 2008; Thakral and Tô, 2021), although recent evidence from Buchholz, Shum and Xu (2025) suggests that what appears to be targeting behavior may be in fact reflect the dynamics of search process and that labor supply elasticities are generally nonnegative.

[27] Details on the imputation method and evidence that the results are robust to alternative imputation assumptions will appear in a subsequent revision.

gaged with delivery apps. Columns (1)-(4) cover all apps, using the imputation, and columns (5)-(8) are restricted to DoorDash, for which on-app time is directly observed. As an additional proxy for time spent engaged with delivery apps, columns (9)-(12) study the number of days with delivery app activity during the month. Across all outcomes, and whether scaling outcomes in levels or logs, we find no evidence of substantial declines in hours worked. For more-attached workers, the point estimates are small (less than one hour in the level specification and less than 7 percent in the log specification) and statistically insignificant in all cases. Meanwhile, for less-attached workers, the point estimates are all positive and statistically insignificant. Across measures and specifications, there is no strong evidence for meaningful declines in hours as a result of the minimum pay policy, though the large standard errors limit the certainty with which we can rule out hours changes in either direction.

However, despite the lack of precision in our findings for hours, Table 6 documents a clear, statistically significant decline in the number of tasks completed per hour engaged with delivery apps. In columns (1)-(4), we find that drivers completed 0.3 to 0.4 fewer trips per active hour across both more- and less-attached drivers and for both hours measures. For Uber Eats and Grubhub, we observe information on each trip's start and end time and location, allowing us to calculate additional measures of drivers' time use and driving behavior. First, we calculate the utilization rate, defined as total monthly on-trip hours as a share of monthly active on-app hours, following Jacobs et al. (2024). Columns (5)-(6) estimate that utilization rates fell by 11 percentage points in the sample of more-attached drivers (17.4 percent of the post-period treatment mean utilization rate), and by 19 percentage points in the sample of less attached drivers (29 percent of the post-period treatment mean utilization rate), implying that drivers incurred more unpaid time per completed delivery after the policy. Similarly, columns (7)-(8) report that unpaid wait time between tasks rose by roughly 5 minutes, which is a substantial increase relative to average wait times (6–7 minutes and 12–13 minutes for treatment drivers in the pre- and post-periods, respectively). While the sample of drivers for Uber Eats and Grubhub is limited and may not be representative of the full sample, we nonetheless find consistent

evidence that the policy increased the share of their engaged time spent looking for tasks without being paid.

If workers spend more time waiting for new delivery tasks, should one consider that to be leisure time or work time? On one hand, if workers are idle while waiting for new tasks, they may spend that time engaged in personal activities; moreover, they may save on gas and other driving-related expenses if they spend more time idle. Alternatively, if trips become harder to find, workers may incur monetary and time costs when driving to areas with relatively more delivery activity in search of offers.[28] We can partially test how drivers use their idle time between tasks by examining the effects on the average distance between the prior task's drop-off location and the subsequent task's pick-up location for trips on Uber Eats and Grubhub. The results, presented in Columns (9) and (10) of Table 6, indicate that treated workers did in fact end up driving further between tasks after the implementation of the minimum pay standard. For more-attached workers, the straight line distance between tasks rose by 0.25–0.3 miles per task. These findings suggest that drivers continued to exert costly effort and incur vehicle-related costs during the additional idle time between trips, rather than consuming additional leisure time.

Taken together, the findings in this section show that as the number of trips per worker falls post-policy, there is no evidence that workers benefit from reduced working time, increased leisure, or reduced expenses associated with driving. Given the lack of monthly earnings increase after the first month post-policy, the evidence suggests that incumbent delivery drivers did not benefit from the minimum pay law in the long run. Instead, when the per-task pay rate increased, tasks became more scarce and drivers had to spend more time on-app searching for tasks, offsetting the increase in pay per task.

[28] Based on the authors' own experience driving on multiple apps, both the probability of getting an offer and the quality of offers improves continuously as one drives towards a central business district after dropping off a delivery in a residential neighborhood. During busy periods, attractive offers begin arriving sooner as one approaches the business district.

7 Discussion and Conclusion

Why did the minimum pay law fail to increase drivers' earnings and seemingly have minimal effect on their welfare? To help answer this question, in Appendix C we present a simple model of the app-based delivery market, closely following the model of the ride-sharing market introduced by Hall, Horton and Knoepfle (2023) and extended by Fisher (2024*a*). We assume that the gig delivery market is small in comparison to the rest of the labor market and allow for workers to have idiosyncratic preferences for working in each market. The variation in these preferences determines the labor supply elasticity to the delivery market and allows us to consider cases with frictional or free worker entry to the market. Rather than modeling the delivery platform's price setting problem as in Fisher (2024*a*), we make the simple but realistic assumption that increases in driver pay per task lead to a reduction in quantity of delivery tasks demanded. We then study how a regulation that raises the per-task pay rate affects the availability of tasks and drivers' earnings.

The key result is that a trip-level minimum pay standard will only increase individual drivers' earnings in settings where there is not free entry of drivers (i.e. a finite labor supply elasticity) and where the market-level elasticity of demand for drivers is sufficiently small (i.e. a demand elasticity with absolute value less than one). Intuitively, if the gig delivery market faces free entry of drivers, a binding minimum pay regulation will have no effect on drivers' per period earnings, as the equilibrium value of doing delivery work must always remain equal to the fixed, constant outside option available to all workers. In that case, driver entry will continue until the reduction in available tasks per worker precisely offsets the increase in pay per task. The condition on entry highlights a key tension in regulating gig work: increased pay necessitates the imposition of barriers to entry, which will tend to undermine the flexibility that the gig economy offers to workers. The demand elasticity condition is more general—it is similar to the effect of a traditional minimum wage in a competitive labor market, in which a higher minimum wage lowers aggregate payroll if the absolute elasticity of demand is greater than one. However, in the traditional setting, there is a well-defined group of job holders who benefit from higher

wages even when demand is highly elastic, with any reduction in demand manifesting in reduced employment levels. By contrast, in the platform gig-work setting where tasks are allocated across all job seekers, every *individual* experiences a reduction in expected pay when demand is sufficiently elastic.

Seen through the lens of the model, our finding of a lack of earnings growth suggests that at least one of these two conditions is not met. In the month following the regulation entering into force, we find that incumbent drivers saw an increase in monthly earnings, but those earnings subsequently fell back to their pre-treatment level, due to a reduction in the tasks available to incumbent drivers (Figures 7 and 8). We find no evidence that hours participating in gig delivery work fell substantially (Table 5), consistent with earnings returning to baseline due to drivers struggling to find delivery tasks. These results could be driven either by unit elastic demand for deliveries or free entry of drivers congesting the market. However, the descriptive evidence in Figure 5 suggests that free entry plays an important role in our setting—in particular, we find that the share of tasks done by new entrants grows in Seattle relative to the rest of the state after the reform, simultaneously with a sharp reduction in tasks completed by incumbents.

These findings are of direct relevance to policymakers seeking to raise pay for gig delivery drivers. If the market for drivers is indeed subject to nearly free entry, minimum pay policies will struggle to improve drivers' earnings without imposing some form of entry barrier, such as those traditionally governing entry to non-platform taxi markets in many cities.[29] However, even with entry barriers, the qualitative effect of a per-task minimum pay regulation will depend on the elasticity of demand for delivery services. If that demand is elastic, a per-task minimum pay regulation will still reduce drivers' earnings even with barriers to entry.

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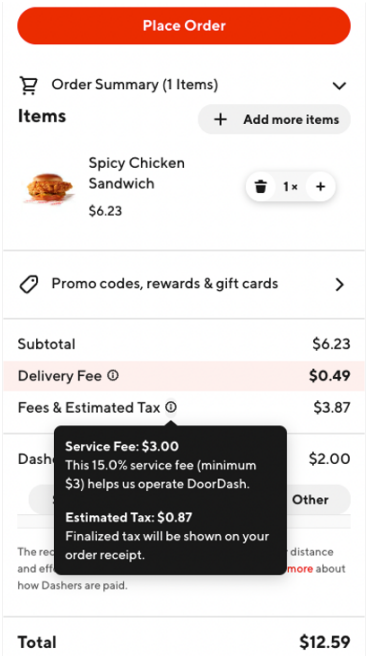
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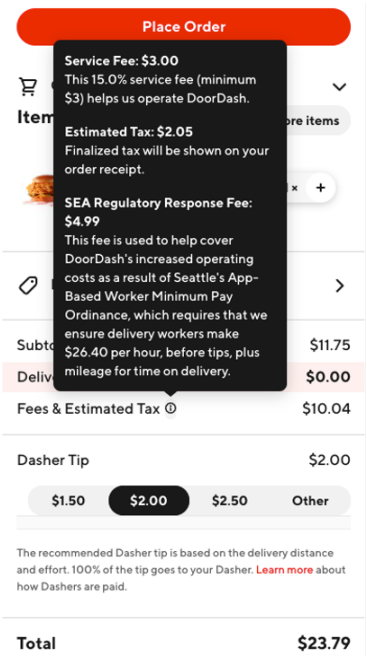
Figures

Figure 1: Fee Differences, by Region

(A) DoorDash, Spokane



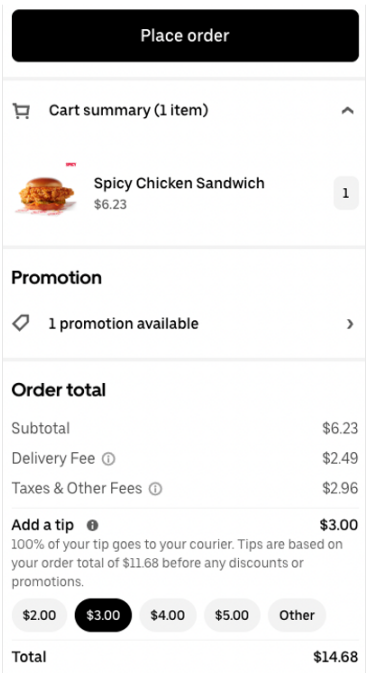
(B) DoorDash, Seattle



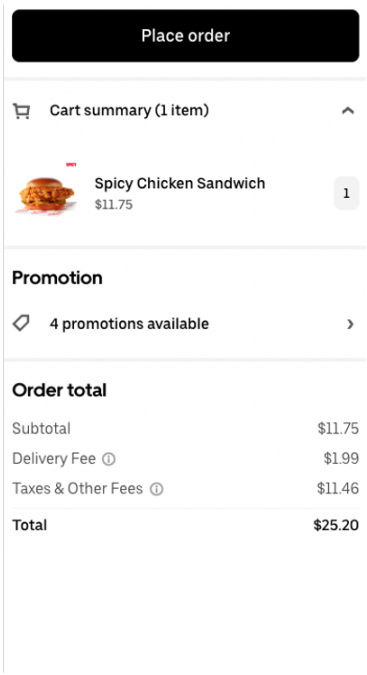
Notes: Panels (A) and (B) show screenshots of checkout pages for orders placed on the DoorDash app with shipping addresses in Spokane and Seattle, respectively. Text in the black boxes shows details of fees and estimated taxes of the respective orders. Screenshots recorded in the second half of 2024.

Figure 2: Tipping Differences, by Region

(A) Uber Eats, Spokane



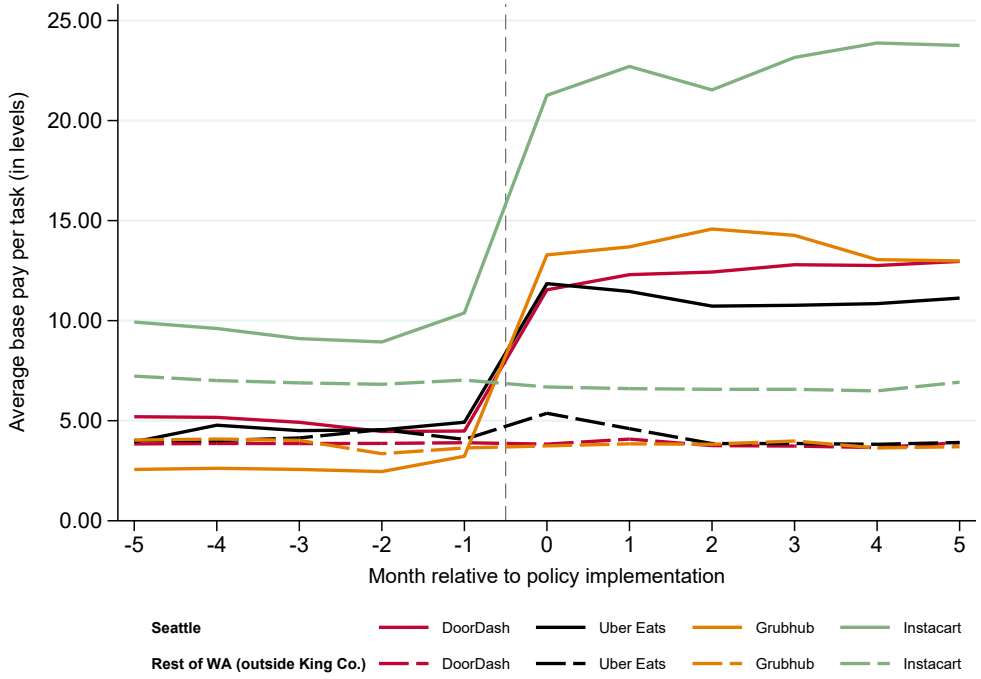
(B) Uber Eats, Seattle



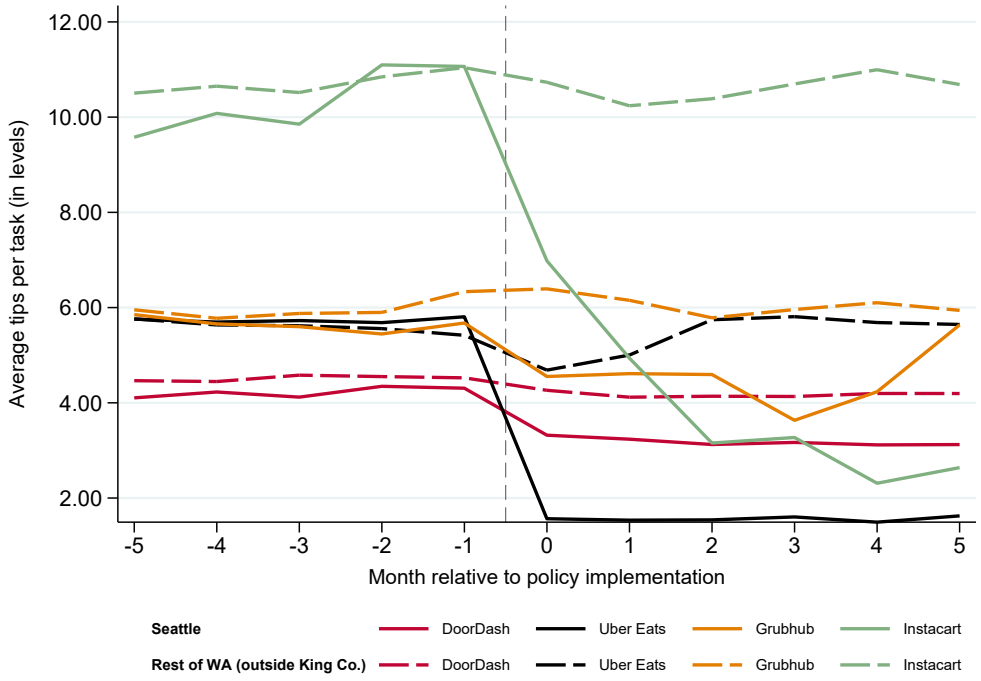
Notes: Panels (A) and (B) show screenshots of checkout pages for orders placed on the Uber Eats's app with shipping addresses in Spokane and Seattle, respectively. In Panel (A), the section labeled "add a tip" provides customers the option to choose a tip amount; this section is absent for Seattle in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

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

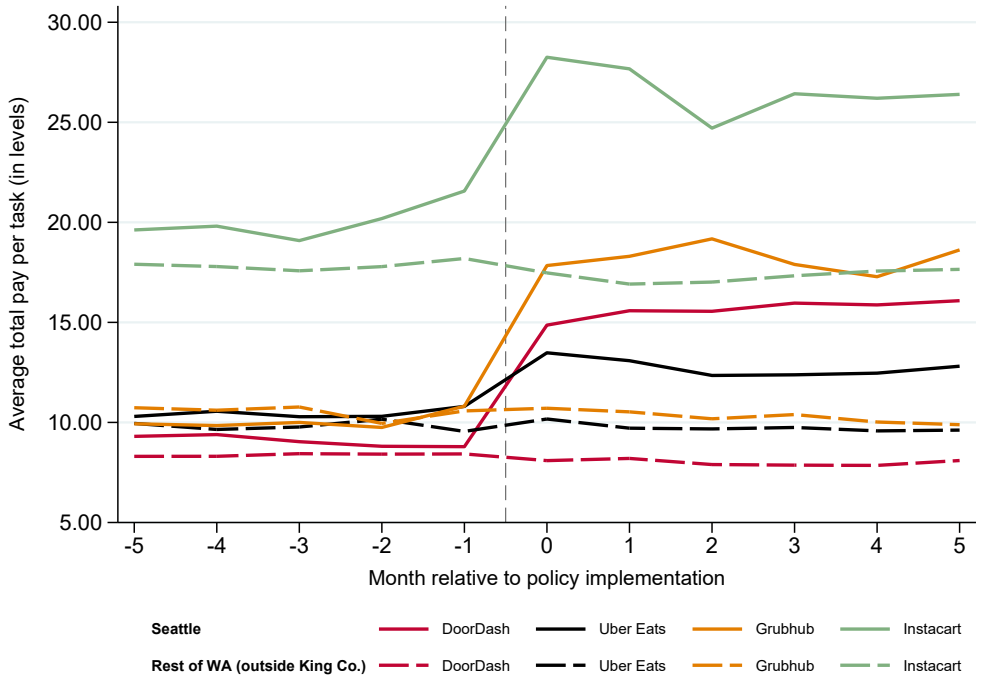
(A) Average Base Pay Per Task



(B) Average Tip Pay Per Task



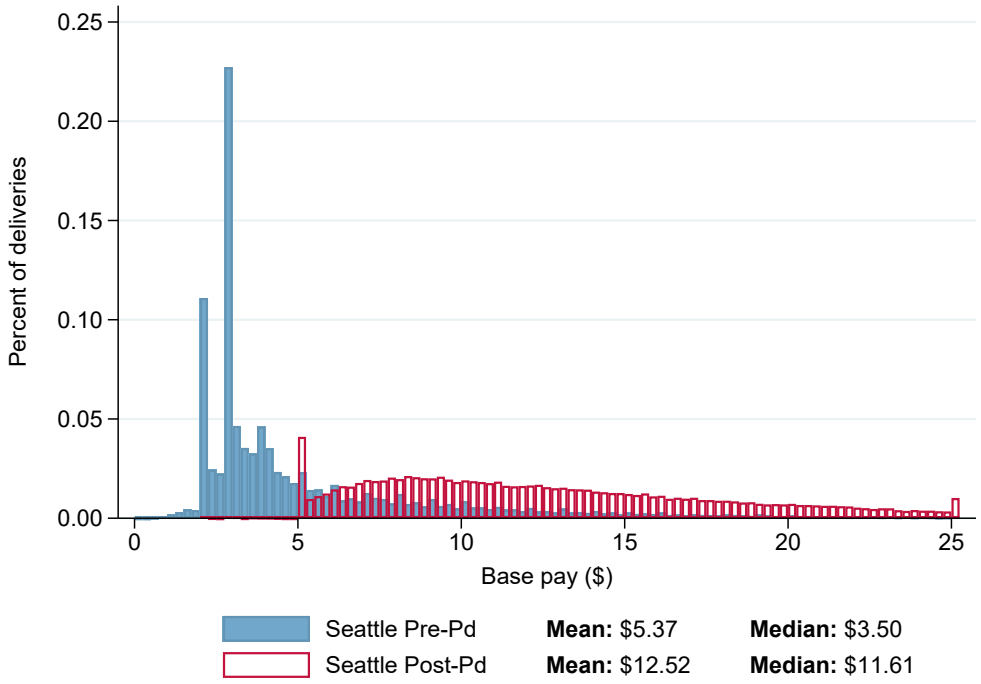
(C) Average Total Pay Per Task



Notes: Panel (A) plots the monthly trends in 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 tip pay per task and average total pay per task, where total pay includes base pay, tip pay, and bonus pay.

Source: Authors' analysis of Gridwise data.

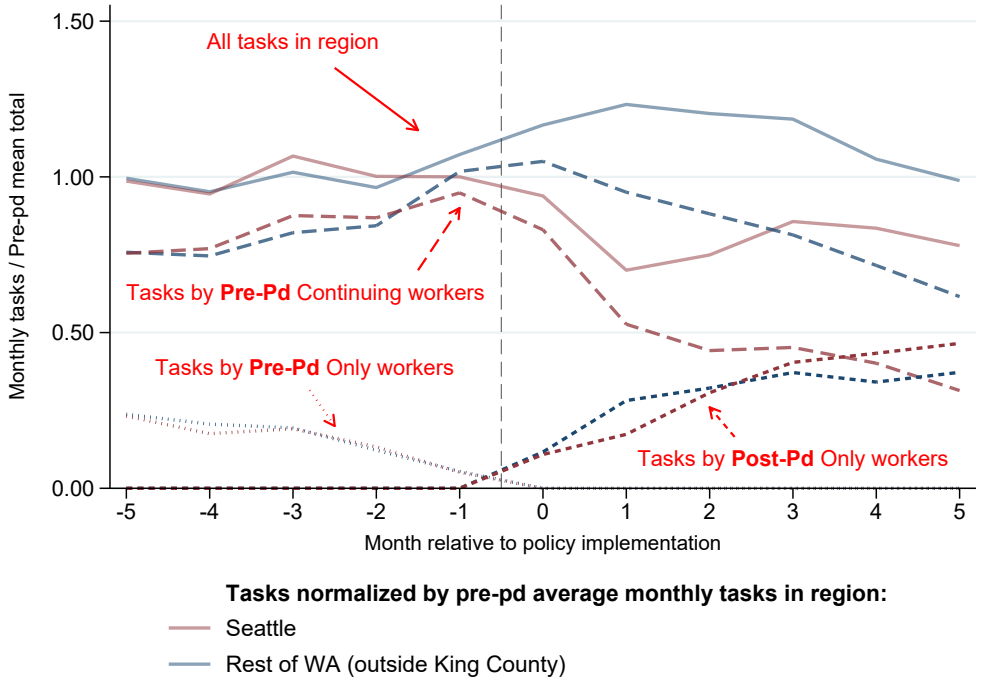
Figure 4: Distributions of Base Pay across Deliveries in Seattle, by Period



Notes: This figure presents the distributions of base pay across deliveries completed in Seattle in the pre-ordinance period and post-ordinance period, respectively. Blue solid bars denote the distribution of base pay across deliveries completed in Seattle in the pre-ordinance period. Red hollow bars denote the distribution of base pay across deliveries completed in Seattle in the post-ordinance period. Each bin represents $0.25. The sample is restricted to the 95th percentile of base pay across deliveries completed in Seattle for ease of visualization. Text on the right-hand side of the legend presents means and medians of base pay across deliveries completed in the pre-ordinance period and post-ordinance period in the sample.

Source: Authors' analysis of Gridwise data.

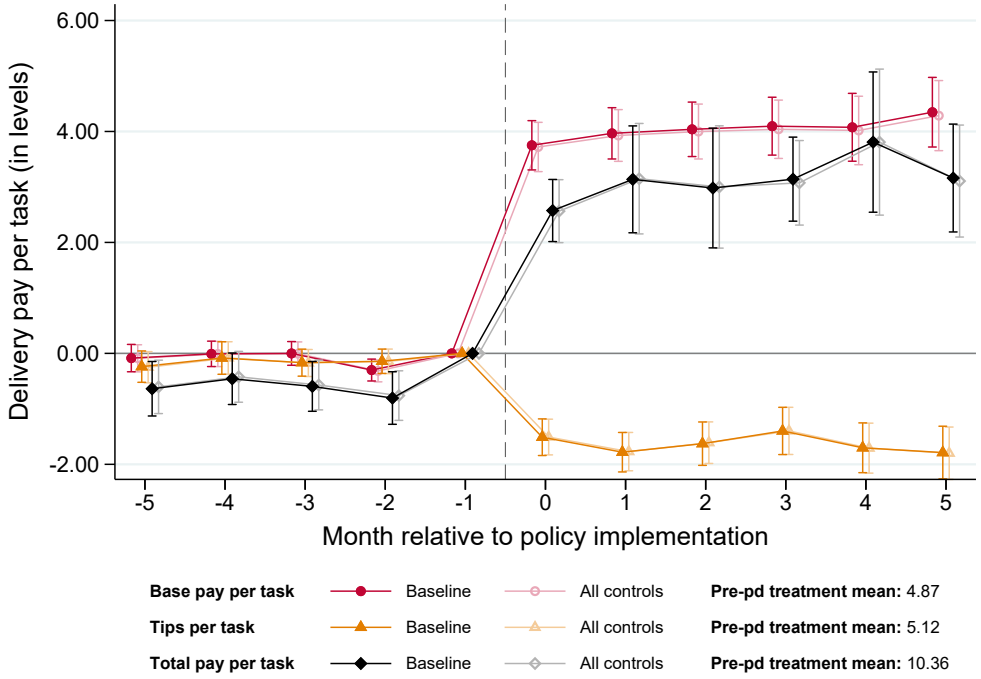
Figure 5: Trends in Delivery Tasks by All Workers, Incumbents, Workers Who Exit, and New Entrants, by Region



Notes: This figure plots the monthly trends in 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.

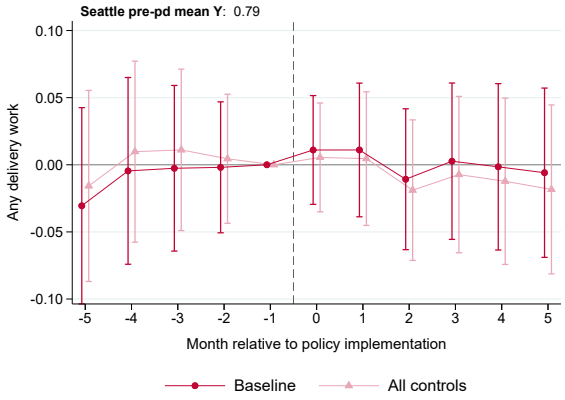
Figure 6: Effects on Delivery Pay Per Task



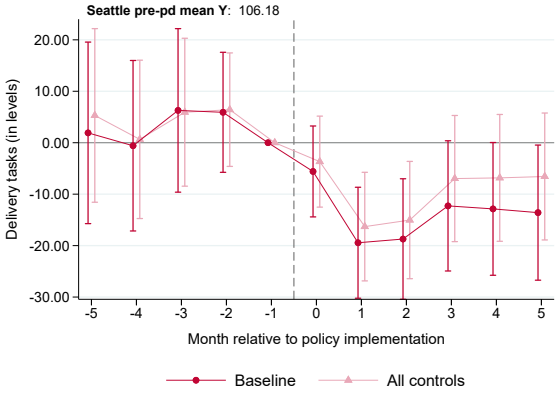
Notes: This figure plots estimates of β_k from Equation (1) using the sample of more-attached workers on three outcomes for delivery pay per task: delivery base pay per task (in red with circle markers), delivery tips per task (in orange with triangle markers), and delivery total pay per task (in black with diamond markers), where total pay includes base pay, tip pay, bonus pay, incentive pay, and adjustment pay. 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 7: Effects on Delivery Tasks

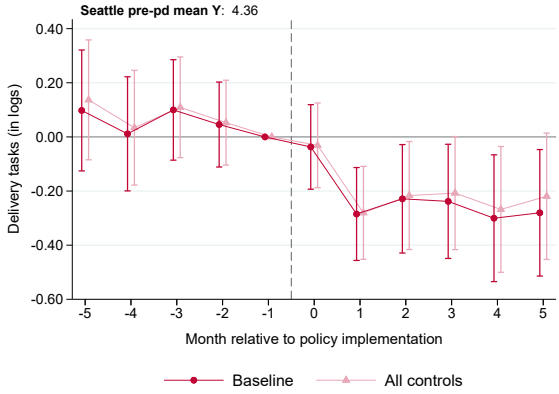
(A) Any



(B) Levels

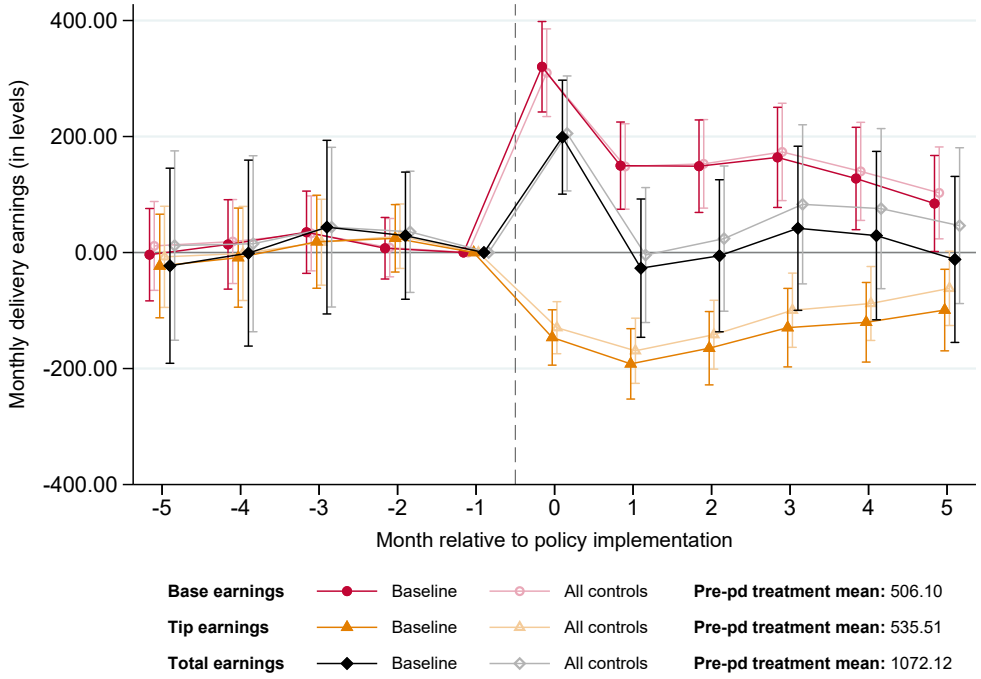


(C) Logs



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 8: Effects on 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), where total earnings include base earnings, tip earnings, bonus earnings, incentive earnings, and adjustment earnings. 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	[99.6]	93.6	[83.4]	12.6**	(5.42)
Delivery earnings (monthly average)	1050.8	[991.1]	926.5	[859.2]	124.3**	(55.1)
Delivery earnings per task	9.93	[2.82]	10.1	[3.60]	-0.16	(0.21)
Delivery base earnings per task	4.81	[1.34]	4.40	[1.44]	0.41***	(0.086)
Delivery tip earnings per task	5.02	[1.93]	5.62	[2.69]	-0.60***	(0.15)
Months active in delivery	3.99	[1.28]	4.06	[1.20]	-0.069	(0.075)
Days active in delivery (monthly average)	12.5	[7.18]	12.6	[7.26]	-0.075	(0.44)
Rideshare earnings (monthly average)	77.3	[447.8]	70.9	[372.1]	6.42	(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)	62.0	[64.9]	58.5	[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	6.69	[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]	-0.015	(0.11)
Rideshare earnings (monthly average)	826.0	[1697.8]	138.9	[711.2]	687.1***	(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		Tip pay per task		Bonus+incentive+adjustment pay per task		Total pay per task	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. More-attached workers								
Treated × Post	4.091***	4.050***	-1.506***	-1.498***	0.918***	0.909***	3.560***	3.521***
	(0.213)	(0.213)	(0.142)	(0.137)	(0.173)	(0.177)	(0.297)	(0.301)
N worker-months	9,906	9,906	9,906	9,906	9,906	9,906	9,906	9,906
N workers	1,265	1,265	1,265	1,265	1,265	1,265	1,265	1,265
Treatment pre-pd mean Y	4.877	4.877	5.128	5.128	0.330	0.330	10.365	10.365
Treatment post-pd mean Y	9.046	9.046	3.546	3.546	1.162	1.162	13.837	13.837
B. Less-attached workers								
Treated × Post	4.198***	4.468***	-1.595***	-1.579***	0.599	0.799	3.238***	3.713***
	(0.452)	(0.474)	(0.279)	(0.290)	(0.444)	(0.497)	(0.623)	(0.675)
N worker-months	4,160	4,160	4,160	4,160	4,160	4,160	4,160	4,160
N workers	941	941	941	941	941	941	941	941
Treatment pre-pd mean Y	6.445	6.445	4.811	4.811	0.494	0.494	11.793	11.793
Treatment post-pd mean Y	9.532	9.532	3.348	3.348	0.805	0.805	13.742	13.742
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 four outcomes for delivery pay per task: delivery base pay per task (columns 1-2), delivery tip pay per task (columns 3-4), delivery bonus, incentive, and adjustment pay per task (columns 5-6), and delivery total pay per task (columns 7-8), where total pay includes base pay, tip pay, bonus pay, incentive pay, and adjustment pay. All outcomes are calculated in levels. 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, 5, and 7 present estimates without including additional controls, and columns 2, 4, 6, and 8 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. The rows of "treatment post-period mean Y" report means of corresponding outcomes posterior 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	-16.450**	-12.891**	-0.262***	-0.261***
	(0.024)	(0.022)	(5.163)	(4.341)	(0.066)	(0.065)
N worker-months	14,135	14,135	14,135	14,135	9,906	9,906
N workers	1,285	1,285	1,285	1,285	1,265	1,265
Treatment pre-pd mean Y	0.799	0.799	106.181	106.181	4.356	4.356
Treatment post-pd mean Y	0.610	0.610	62.031	62.031	3.884	3.884
B. Less-attached workers						
Treated × Post	0.006	-0.021	-0.466	-0.779	0.069	0.166
	(0.022)	(0.021)	(1.276)	(1.414)	(0.132)	(0.128)
N worker-months	14,168	14,168	14,168	14,168	4,160	4,160
N workers	1,288	1,288	1,288	1,288	941	941
Treatment pre-pd mean Y	0.396	0.396	5.994	5.994	2.117	2.117
Treatment post-pd mean Y	0.233	0.233	7.548	7.548	2.546	2.546
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. The rows of "treatment post-period mean Y" report means of corresponding outcomes posterior to the ordinance for exposed workers. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 4: Effects on Delivery Earnings

	Base earnings		Tip earnings		Bonus+incentive+adjustment earnings		Total earnings	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. More-attached workers								
Treated × Post	155.311***	156.893***	-144.256***	-122.330***	17.514***	16.553***	27.768	50.346
	(35.658)	(33.994)	(27.643)	(22.138)	(5.249)	(4.945)	(56.732)	(51.494)
N worker-months	14,135	14,135	14,135	14,135	14,135	14,135	14,135	14,135
N workers	1,285	1,285	1,285	1,285	1,285	1,285	1,285	1,285
Treatment pre-pd mean Y	506.110	506.110	535.518	535.518	28.364	28.364	1072.123	1072.123
Treatment post-pd mean Y	548.647	548.647	224.643	224.643	42.486	42.486	817.126	817.126
B. Less-attached workers								
Treated × Post	33.223**	32.619**	-14.247**	-15.193*	3.779***	3.801**	22.619	21.064
	(10.788)	(11.881)	(5.292)	(5.966)	(1.030)	(1.235)	(15.795)	(17.524)
N worker-months	14,168	14,168	14,168	14,168	14,168	14,168	14,168	14,168
N workers	1,288	1,288	1,288	1,288	1,288	1,288	1,288	1,288
Treatment pre-pd mean Y	30.911	30.911	30.467	30.467	1.890	1.890	63.477	63.477
Treatment post-pd mean Y	71.701	71.701	24.730	24.730	4.549	4.549	101.034	101.034
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 four outcomes for monthly delivery earnings: monthly delivery base earnings (columns 1-2), monthly delivery tip earnings (columns 3-4), monthly delivery bonus, incentive, and adjustment earnings (columns 5-6), and monthly delivery total earnings (columns 7-8), where total earnings include base earnings, tip earnings, bonus earnings, incentive earnings, and adjustment earnings. 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, 5, and 7 present estimates without including additional controls, and columns 2, 4, 6, and 8 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. The rows of "treatment post-period mean Y" report means of corresponding outcomes posterior to the ordinance for exposed workers. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 5: Effects on Delivery Hours and Active Delivery Days

	Total hours, with imputation				DoorDash hours				Total active days			
	Levels		Logs		Levels		Logs		Levels		Logs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. More-attached workers												
Treated × Post	-0.952	0.170	-0.067	-0.069	-0.881	-0.056	0.026	0.020	0.296	0.223	-0.014	-0.021
	(2.705)	(2.449)	(0.065)	(0.064)	(2.278)	(2.133)	(0.085)	(0.085)	(0.451)	(0.421)	(0.044)	(0.044)
N worker-months	14,135	14,135	9,905	9,905	14,135	14,135	6,388	6,388	14,135	14,135	9,906	9,906
N workers	1,285	1,285	1,265	1,265	1,285	1,285	880	880	1,285	1,285	1,265	1,265
Treatment pre-pd mean Y	54.380	54.380	3.709	3.709	34.687	34.687	3.527	3.527	11.742	11.742	2.443	2.443
Treatment post-pd mean Y	38.185	38.185	3.420	3.420	26.352	26.352	3.424	3.424	8.637	8.637	2.320	2.320
B. Less-attached workers												
Treated × Post	0.864	0.782	0.235	0.345**	0.588	0.725	0.160	0.224	0.337	0.273	0.132	0.210*
	(0.793)	(0.876)	(0.134)	(0.134)	(0.590)	(0.668)	(0.213)	(0.200)	(0.234)	(0.243)	(0.093)	(0.094)
N worker-months	14,168	14,168	4,160	4,160	14,168	14,168	1,493	1,493	14,168	14,168	4,160	4,160
N workers	1,288	1,288	941	941	1,288	1,288	371	371	1,288	1,288	941	941
Treatment pre-pd mean Y	3.336	3.336	1.501	1.501	1.679	1.679	1.812	1.812	1.531	1.531	1.064	1.064
Treatment post-pd mean Y	5.030	5.030	2.114	2.114	2.803	2.803	2.320	2.320	1.826	1.826	1.556	1.556
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 hours and active delivery days: imputed monthly total hours on all delivery platforms (columns 1-2 for levels and columns 3-4 for logs), monthly total hours on platform DoorDash (columns 5-6 for levels and columns 7-8 for logs), and monthly active days on all delivery platforms (columns 9-10 for levels and columns 11-12 for logs). 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, 5, 7, 9, and 11 present estimates without including additional controls, and columns 2, 4, 6, 8, 10, and 12 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. The rows of "treatment post-period mean Y" report means of corresponding outcomes posterior to the ordinance for exposed workers. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table 6: Effects on Delivery Time Use

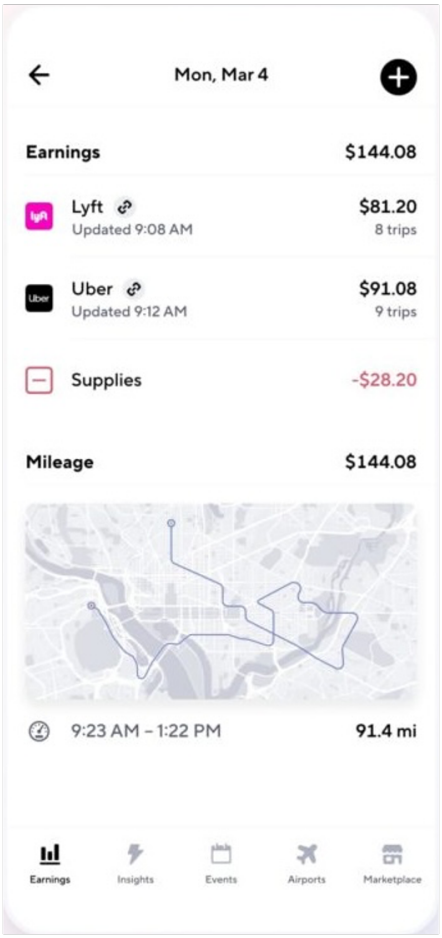
	Tasks per hour				Utilization rate		Task wait time (min)		Distance between tasks (mi)	
	All apps, with imputation		DoorDash only							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. More-attached workers										
Treated × Post	-0.340***	-0.333***	-0.340***	-0.341***	-0.109***	-0.108***	5.415***	5.363***	0.274***	0.296***
	(0.027)	(0.026)	(0.037)	(0.036)	(0.023)	(0.022)	(0.601)	(0.622)	(0.069)	(0.071)
N worker-months	9,905	9,905	6,388	6,388	3,727	3,727	4,461	4,461	4,377	4,377
N workers	1,265	1,265	880	880	591	591	656	656	644	644
Treatment pre-pd mean Y	1.981	1.981	1.970	1.970	0.759	0.759	6.920	6.920	1.464	1.464
Treatment post-pd mean Y	1.666	1.666	1.620	1.620	0.637	0.637	13.521	13.521	1.763	1.763
B. Less-attached workers										
Treated × Post	-0.278***	-0.333***	-0.420***	-0.411***	-0.193**	-0.193**	5.250***	4.251**	0.194	0.099
	(0.072)	(0.070)	(0.098)	(0.092)	(0.064)	(0.067)	(1.359)	(1.454)	(0.194)	(0.233)
N worker-months	4,160	4,160	1,493	1,493	1,453	1,453	1,549	1,549	1,523	1,523
N workers	941	941	371	371	365	365	386	386	378	378
Treatment pre-pd mean Y	1.974	1.974	1.914	1.914	0.882	0.882	5.663	5.663	1.440	1.440
Treatment post-pd mean Y	1.652	1.652	1.465	1.465	0.666	0.666	12.661	12.661	1.778	1.778
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 four outcomes for monthly time use in delivery: the monthly average number of delivery tasks completed per hour (columns 1-2 for monthly total tasks completed on all delivery platforms divided by imputed monthly total hours spent on all delivery platforms and columns 3-4 for monthly total tasks completed on platform DoorDash divided by monthly total hours spent on platform DoorDash), the monthly utilization rate (columns 5-6), monthly average wait minutes per task (columns 7-8), and monthly average miles traveled between tasks (columns 9-10). Utilization rates, task wait times, and distances between tasks are calculated for Uber Eats and Grubhub, given the data availability. All outcomes are calculated in levels. Outcomes corresponding to zero tasks completed on the outcome-corresponding delivery platform(s) 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, 5, 7, and 9 present estimates without including additional controls, and columns 2, 4, 6, 8, and 10 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. The rows of "treatment post-period mean Y" report means of corresponding outcomes posterior to the ordinance for exposed workers. * $p < .10$, ** $p < .05$, *** $p < .01$.

Online Appendices [Not for Publication]

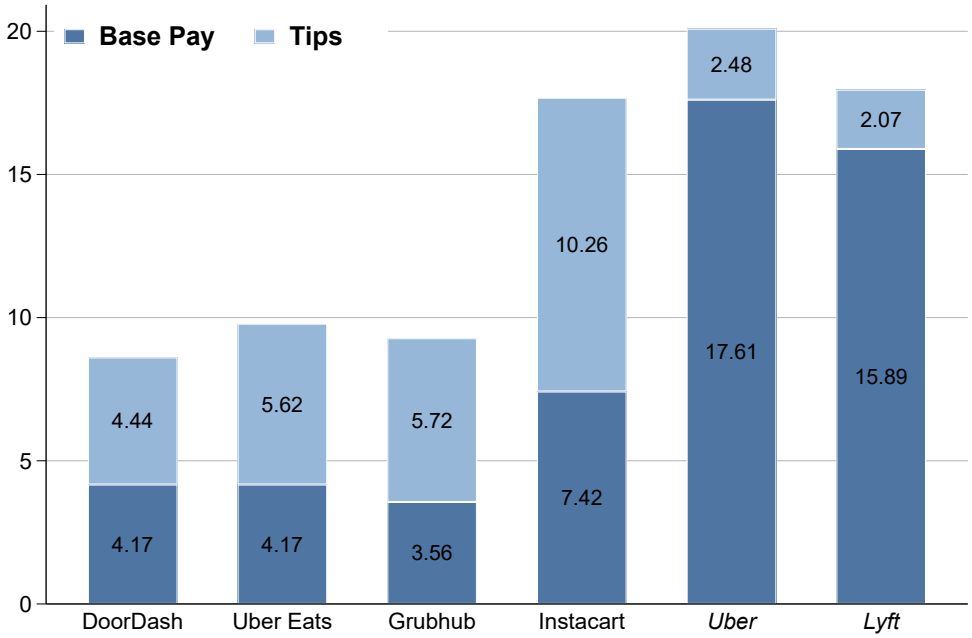
A Appendix Figures

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)

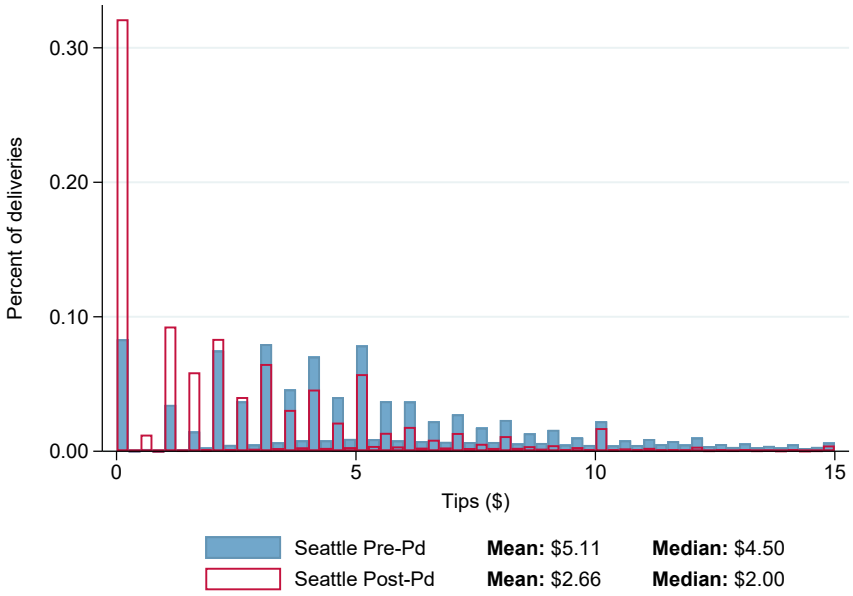


Notes: This figure calculates average base pay per task and average tips per task for tasks completed in Washington State from August 2023 to December 2023 on delivery platforms DoorDash, Grubhub, Instacart, and Uber Eats and rideshare platforms Lyft and Uber. Numbers presented on the top of bars report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

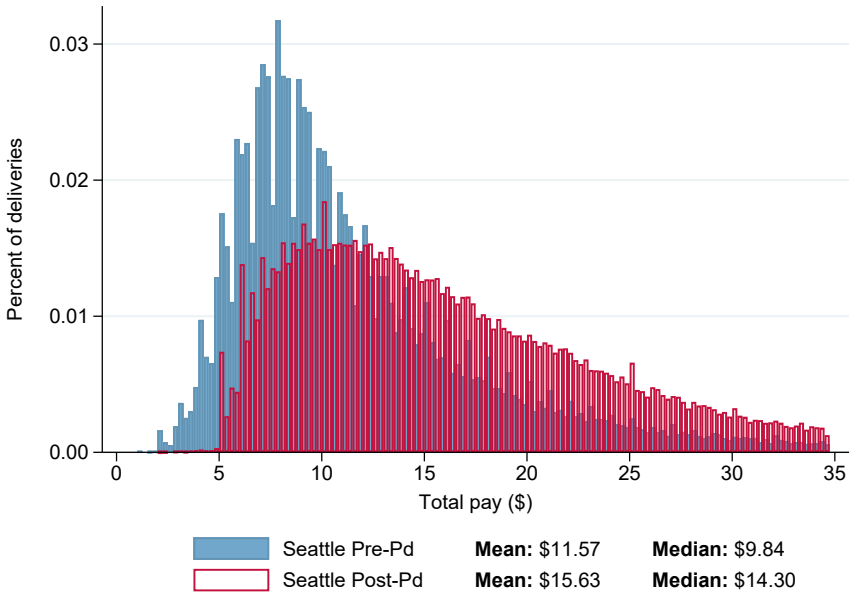
Source: Authors' tabulations using Gridwise data.

Figure A.3: Distributions of Pay across Deliveries in Seattle, by Period

(A) Tip Pay



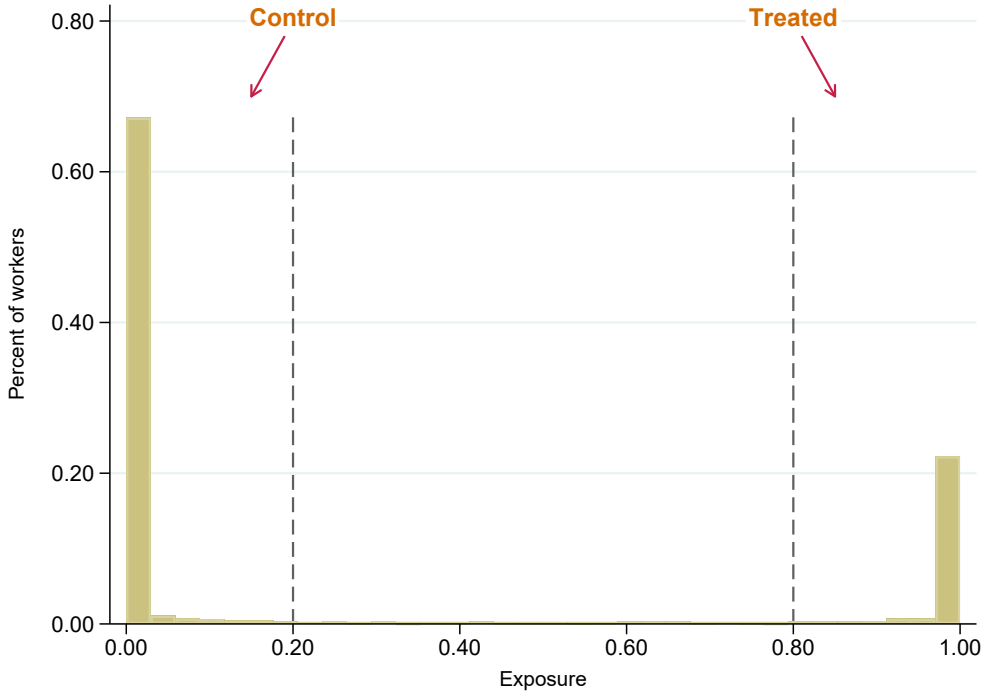
(B) Total Pay



Notes: Panel (A) presents the distributions of tip pay across deliveries completed in Seattle in the pre-ordinance period and post-ordinance period, respectively. Blue solid bars denote the distribution of tips across deliveries completed in Seattle in the pre-ordinance period. Red hollow bars denote the distribution of tips across deliveries completed in Seattle in the post-ordinance period. Each bin represents $0.25. The sample is restricted to the 95th percentile of tips across deliveries completed in Seattle for ease of visualization. Text on the right-hand side of the legend presents means and medians of tips across deliveries completed in the pre-ordinance period and post-ordinance period in the sample. Panel (B) presents the corresponding plot for total pay, where total pay includes base pay, tip pay, and bonus pay. Similarly, the sample used in Panel (B) is restricted to the 95th percentile of total pay across deliveries completed in Seattle for ease of visualization.

Source: Authors' analysis of Gridwise data.

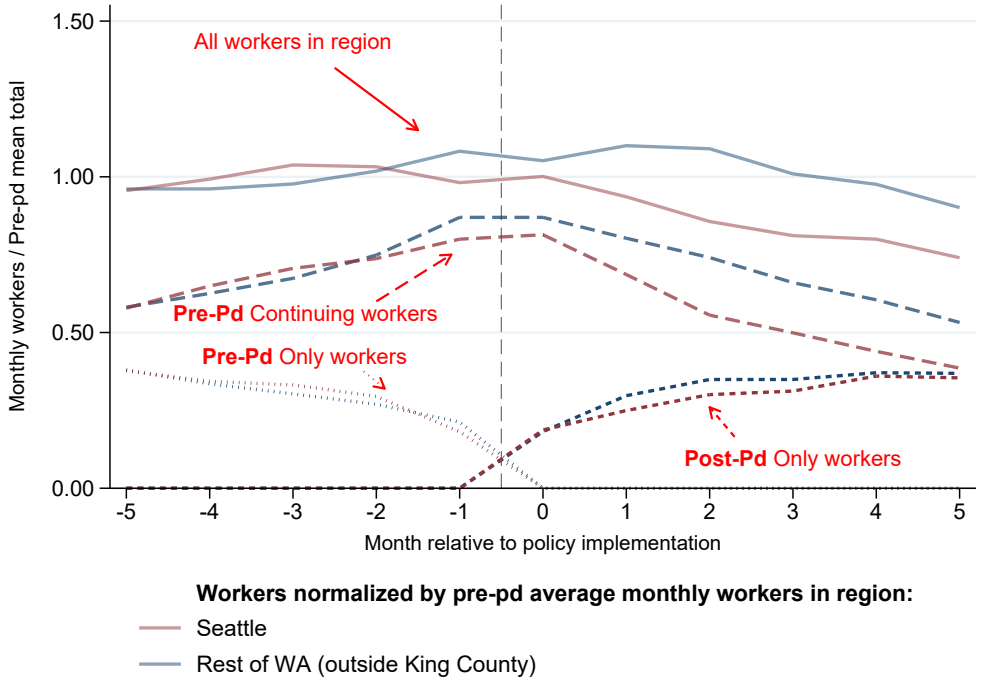
Figure A.4: Distribution of Exposure Measure across Workers



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.5: Trends in Delivery Workers – All Workers, Incumbents, Workers Who Exit, and New Entrants, by Region



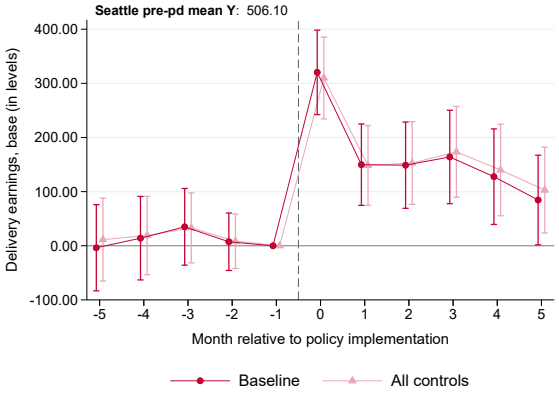
Notes: This figure plots the monthly trends in delivery workers including all workers, incumbents, workers who exit, and new entrants by region. All values are normalized by the pre-ordinance average monthly total numbers of delivery workers in the respective regions. Solid lines in red and blue denote normalized monthly total delivery workers in Seattle and Washington State outside King County, respectively. Longer-dashed lines denote normalized monthly delivery 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 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 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.

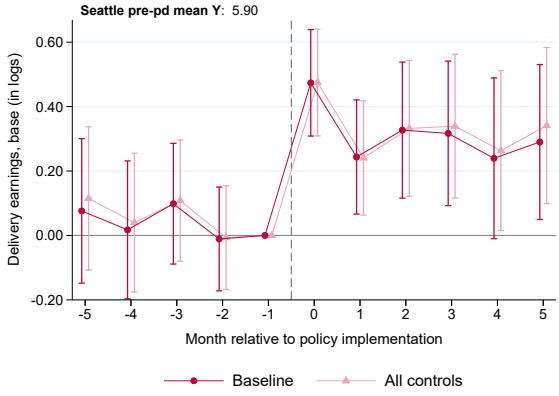
Figure A.6: Effects on Delivery Earnings – More-Attached Workers

(A) Base Earnings

(I) Levels

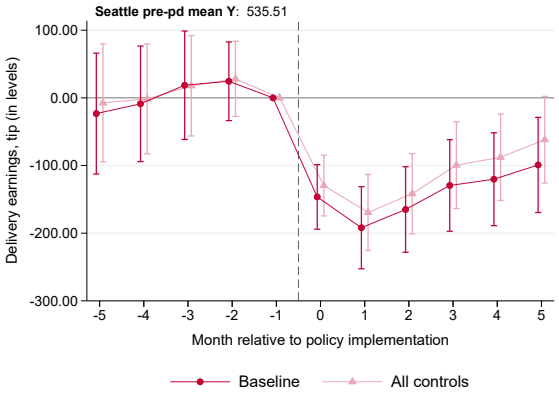


(II) Logs

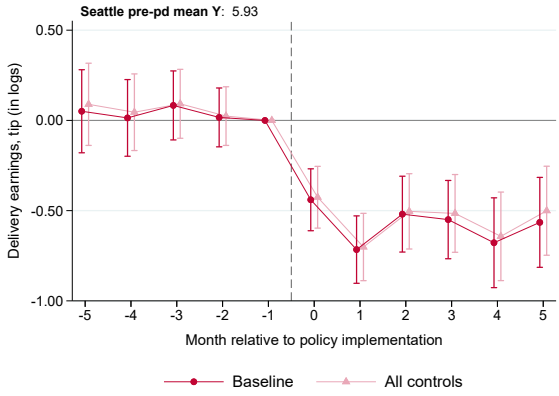


(B) Tip Earnings

(I) Levels

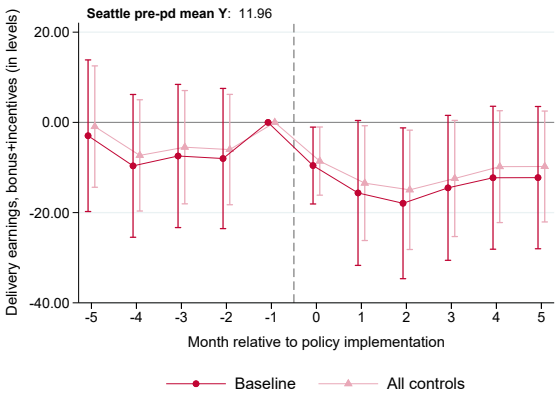


(II) Logs

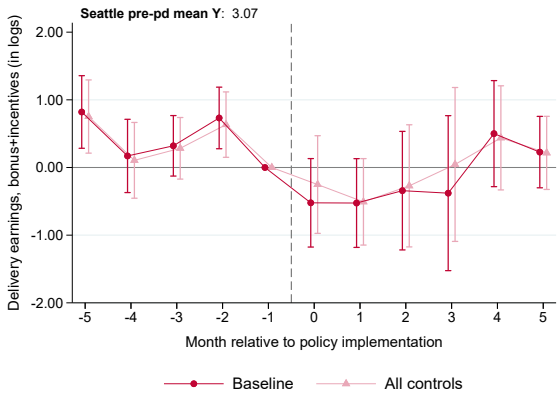


(C) Bonus & Incentive Earnings

(I) Levels

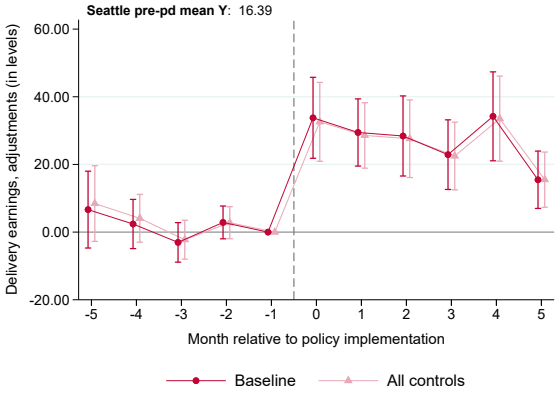


(II) Logs

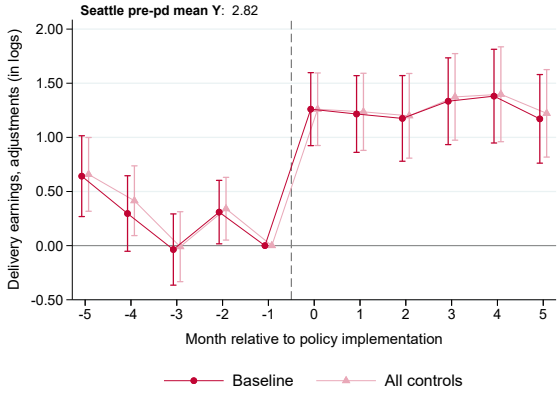


(D) Adjustment Earnings

(I) Levels

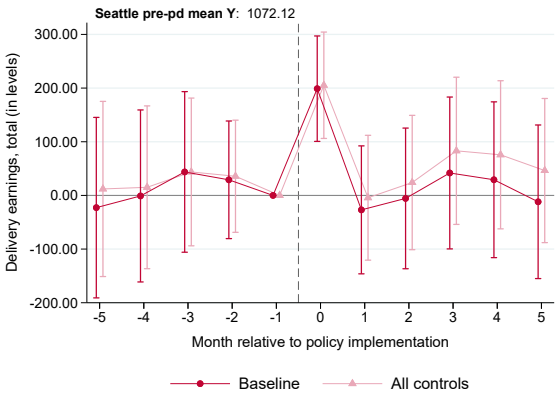


(II) Logs

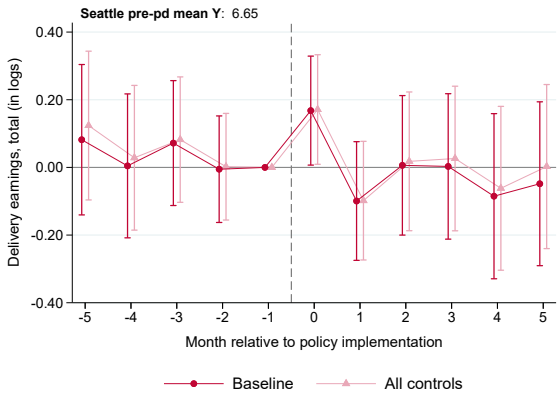


(E) Total Earnings

(I) Levels

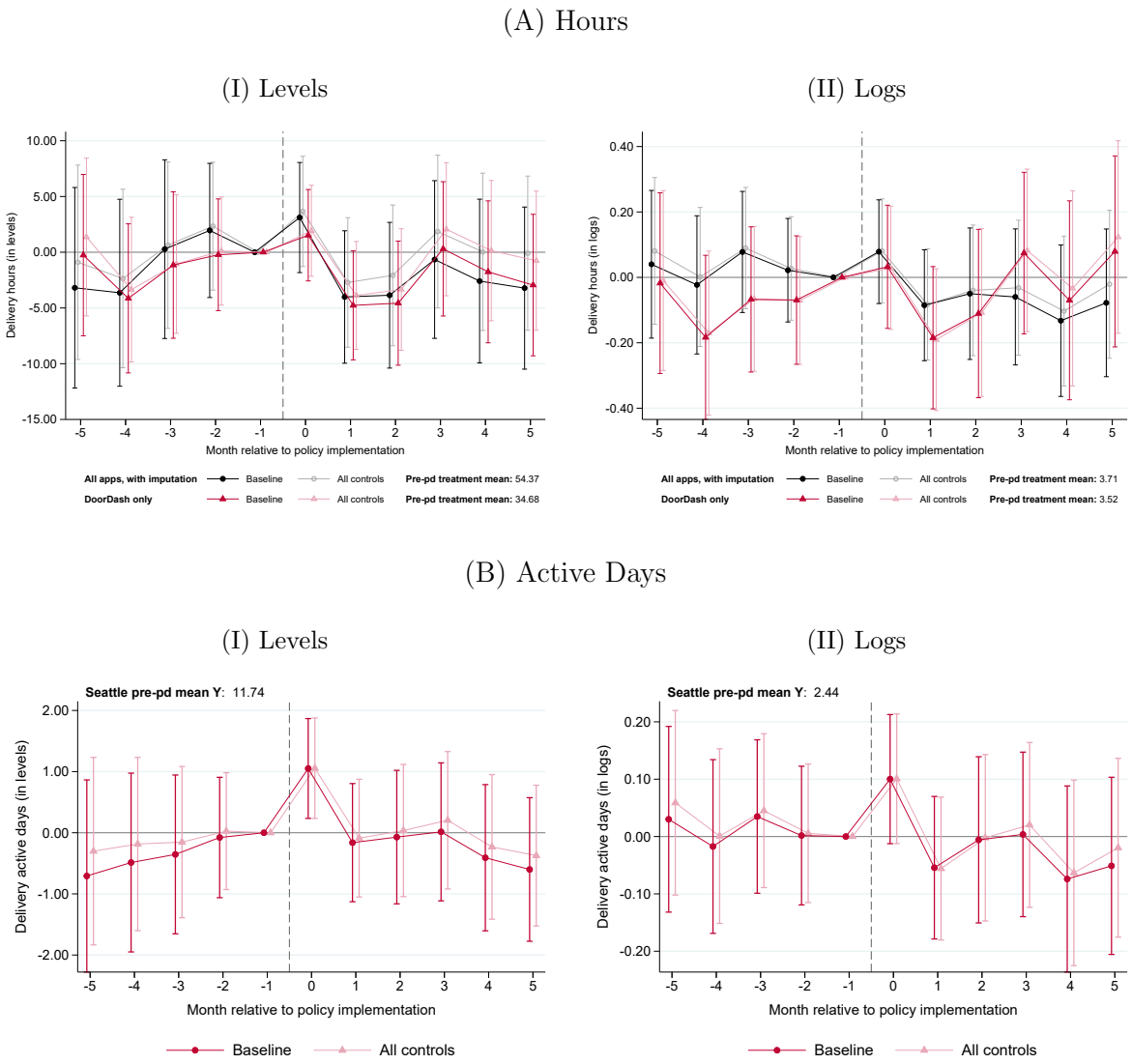


(II) Logs



Notes: Panels (A), (B), (C), (D), and (E) plot, respectively, estimates of β_k from Equation (1) using the sample of more-attached workers on five outcomes for monthly delivery earnings: monthly delivery base earnings (Panel A), monthly delivery tip earnings (Panel B), monthly delivery bonus and incentive earnings (Panel C), monthly delivery adjustment earnings (Panel D), and monthly delivery total earnings (Panel E), where total earnings include base earnings, tip earnings, bonus earnings, incentive earnings, and adjustment earnings. 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.7: Effects on Delivery Hours and Active Delivery Days – More-Attached Workers

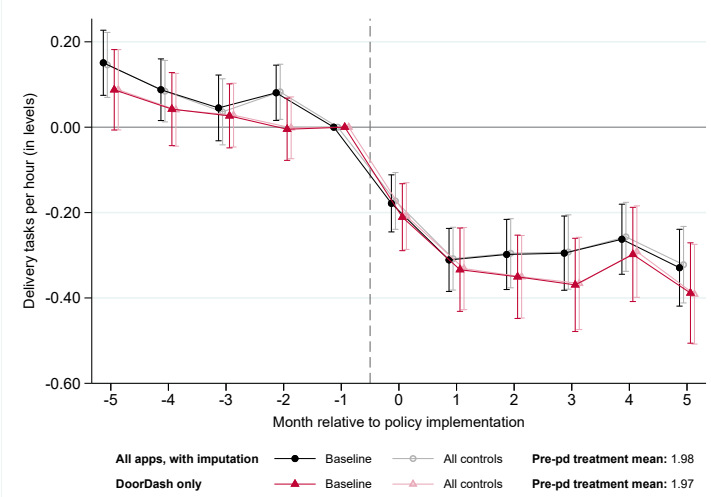


Notes: Panels (A) and (B) plot, respectively, estimates of β_k from Equation (1) using the sample of more-attached workers on monthly delivery hours (Panel A) and monthly active days in delivery (Panel B). In Panel (A), plots in black with circle markers are estimates from Equation (1) on imputed monthly total hours on all delivery platforms. Plots in red with triangle markers are estimates from Equation (1) on monthly total hours on platform DoorDash. In Panel (B), plots are estimates from Equation (1) on monthly active days on all delivery platforms. 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 are estimates from Equation (1) without including additional controls (Baseline). Plots in lighter colors 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 in each panel of Panel (A) presents means of corresponding outcomes prior to the ordinance for exposed workers. Text in the top left-hand corner in each panel of Panel (B) presents the mean of the corresponding outcome prior to the ordinance for exposed workers.

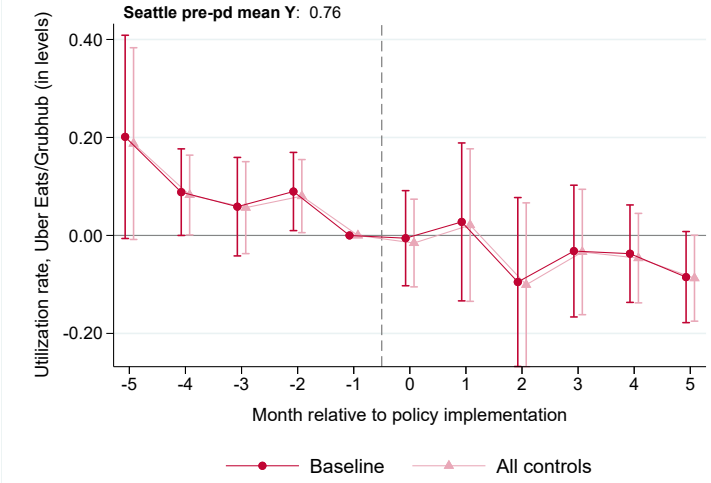
Source: Authors' analysis of Gridwise data.

Figure A.8: Effects on Delivery Time Use – More-Attached Workers

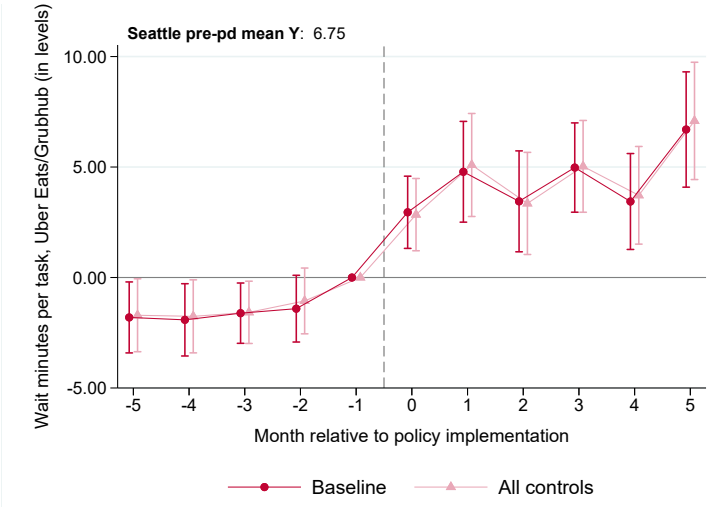
(A) Tasks Per Hour



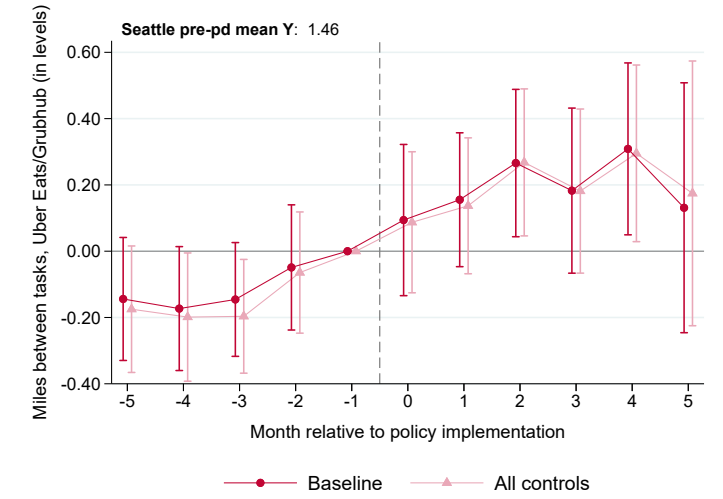
(B) Utilization Rate



(C) Task Wait Time (in Minutes)

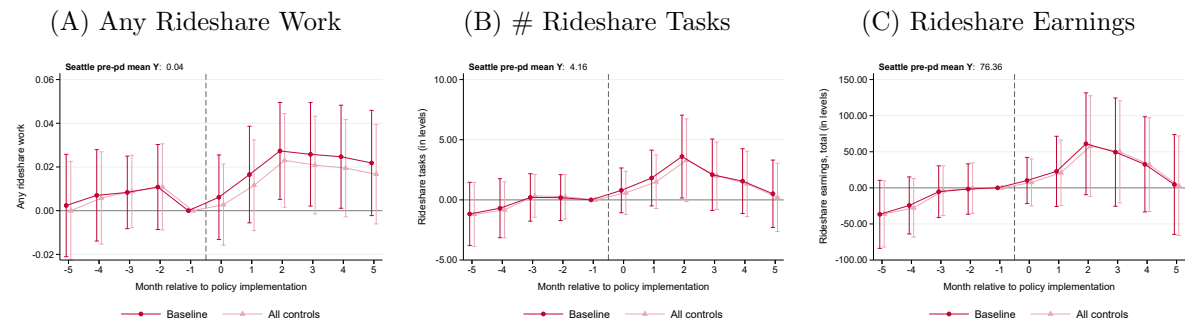


(D) Distance Between Tasks (in Miles)



Notes: Panels (A), (B), (C), and (D) plot, respectively, estimates of β_k from Equation (1) using the sample of more-attached workers on four outcomes for monthly time use in delivery: the monthly average number of delivery tasks completed per hour (Panel A), the monthly utilization rate (Panel B), monthly average wait minutes per task (Panel C), and monthly average miles traveled between tasks (Panel D). In Panel (A), plots in black with circle markers are estimates from Equation (1) on monthly total tasks completed on all delivery platforms divided by imputed monthly total hours spent on all delivery platforms. Plots in red with triangle markers are estimates from Equation (1) on monthly total tasks completed on platform DoorDash divided by monthly total hours spent on platform DoorDash. Given the data availability, utilization rates in Panel (B), task wait times in Panel (C), and distances between tasks in Panel (D) are calculated for Uber Eats and Grubhub. All outcomes are calculated in levels. Outcomes corresponding to zero tasks completed on the outcome-corresponding delivery platform(s) 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 are estimates from Equation (1) without including additional controls (Baseline). Plots in lighter colors 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 in Panel (A) presents means of corresponding outcomes prior to the ordinance for exposed workers. Text in the top left-hand corner in Panels (B), (C), and (D) presents the mean of the corresponding outcome prior to the ordinance for exposed workers.

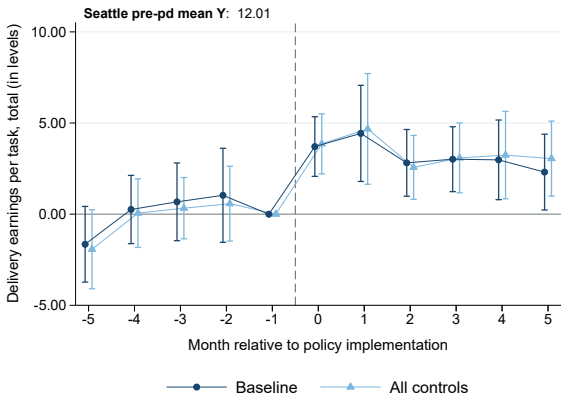
Figure A.9: Effects on Rideshare Work and Earnings



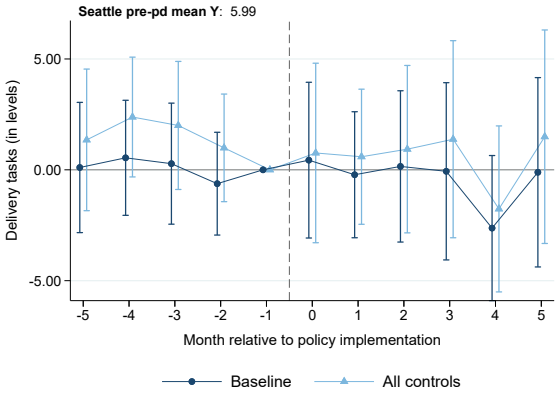
Notes: Panels (A), (B), and (C) plot, respectively, estimates of β_k from Equation (1) using the sample of more-attached workers on three outcomes for rideshare work and earnings: an indicator for performing any rideshare tasks per month (Panel A), the number of monthly completed rideshare tasks (Panel B), and monthly rideshare total earnings (Panel C), where total earnings include base earnings, tip earnings, bonus earnings, incentive earnings, and adjustment earnings. 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.10: Effects on Delivery Pay Per Task, Tasks, and Earnings – Less-Attached Workers

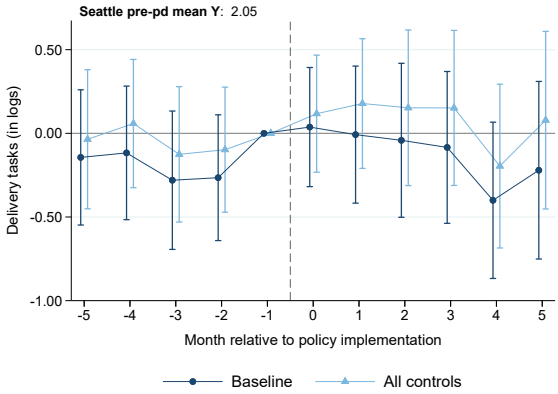
(A) Total Delivery Pay Per Task



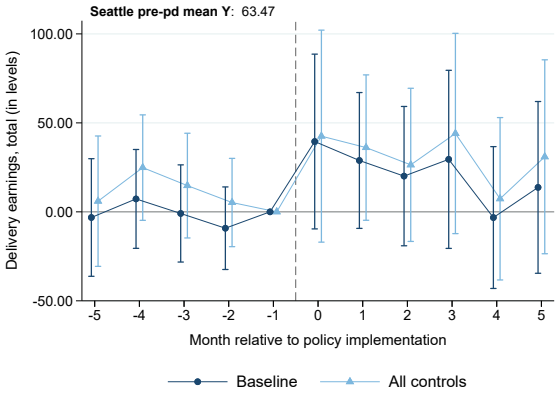
(B) # Delivery Tasks Including Zeros



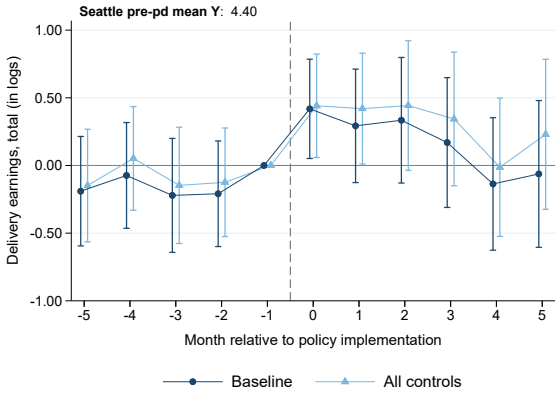
(C) Log(Delivery Tasks) if Positive



(D) Total Delivery Earnings Including Zeros



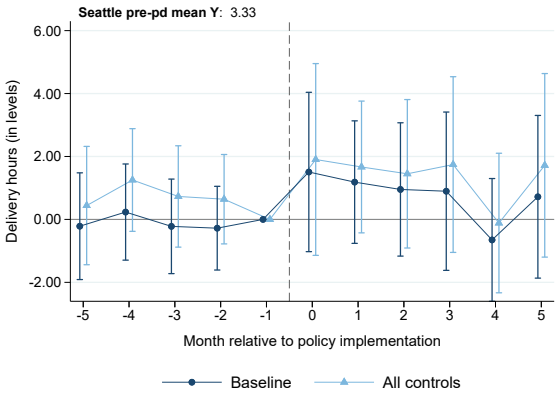
(E) Log(Total Delivery Earnings) if Positive



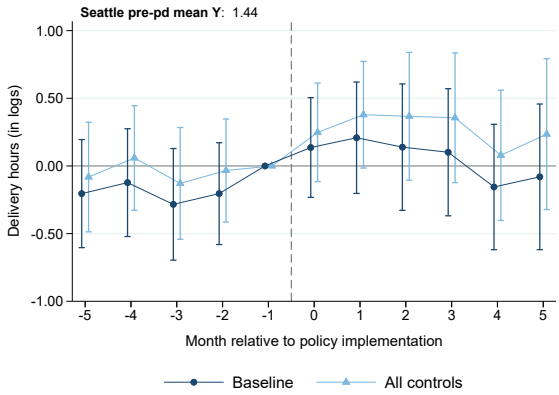
Notes: Panel (A) plots estimates of β_k from Equation (1) on total delivery pay per task using the sample of less-attached workers, where total pay includes base pay, tip pay, bonus pay, incentive pay, and adjustment pay. The outcome is calculated in levels, among which corresponding to zero delivery 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, where total earnings include base earnings, tip earnings, bonus earnings, incentive earnings, and adjustment earnings. 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.

Figure A.11: Effects on Delivery Hours and Active Delivery Days – Less-Attached Workers

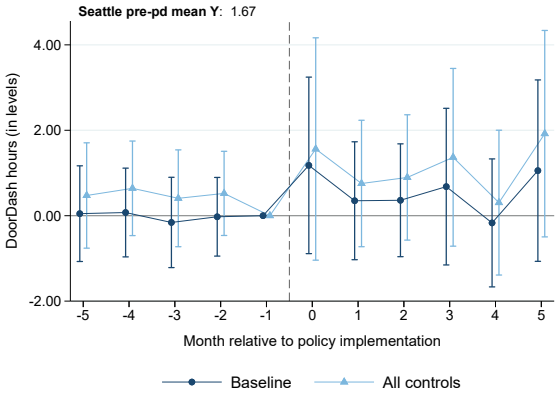
(A) Delivery Hours Including Zeros, All Apps with Imputation



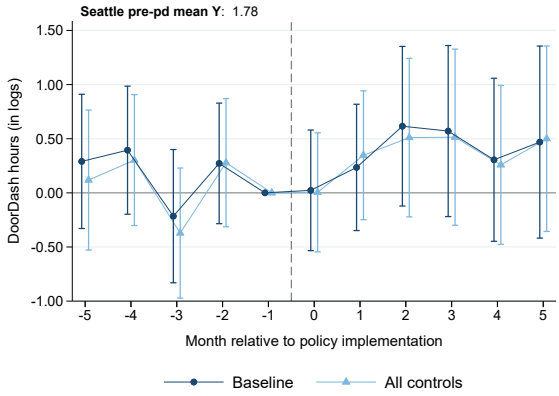
(B) Log(Delivery Hours) if Positive, All Apps with Imputation



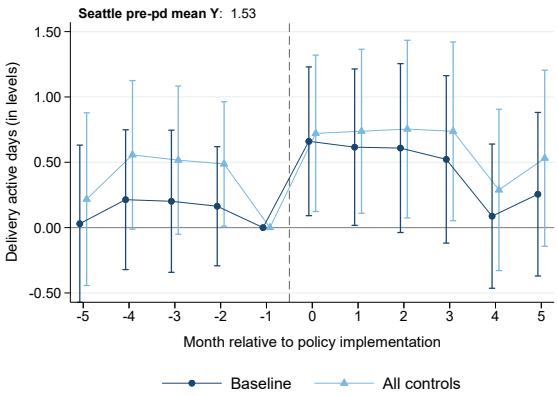
(C) Delivery Hours Including Zeros, DoorDash Only



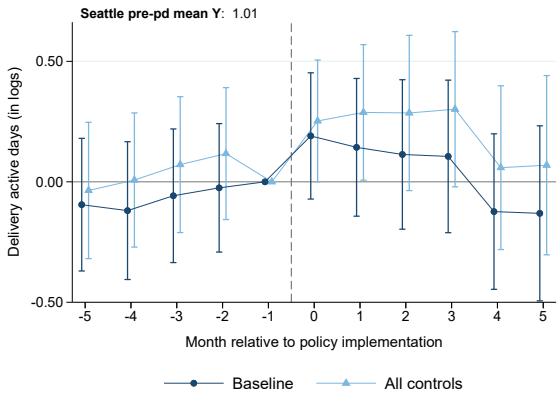
(D) Log(Delivery Hours) if Positive, DoorDash Only



(E) Active Delivery Days Including Zeros



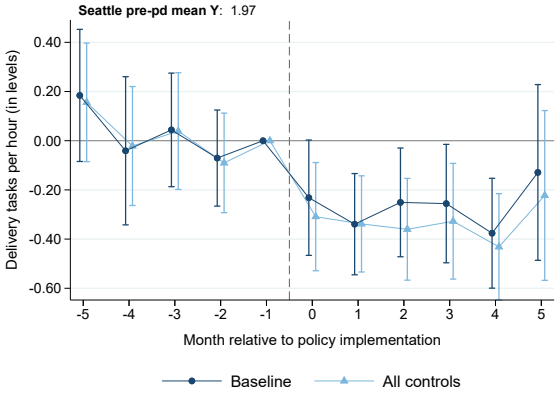
(F) Log(Active Delivery Days) if Positive



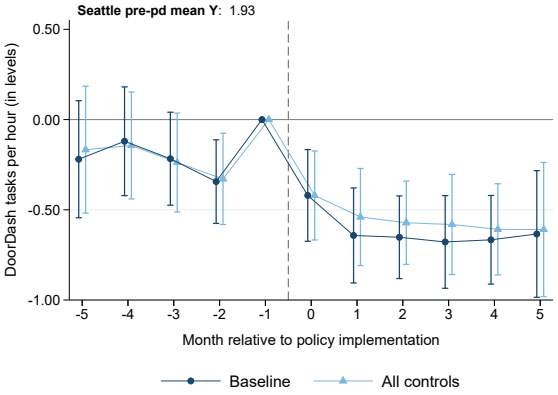
Notes: Panels (A) and (B) plot, respectively, estimates of β_k from Equation (1) on imputed monthly total hours on all delivery platforms in levels and in logs using the sample of less-attached workers. Panels (C) and (D) present corresponding plots for the outcomes of monthly total hours on platform DoorDash in levels and in logs, respectively. Panels (E) and (F) present corresponding plots for the outcomes of monthly active days on all delivery platforms 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.

Figure A.12: Effects on Delivery Time Use – Less-Attached Workers

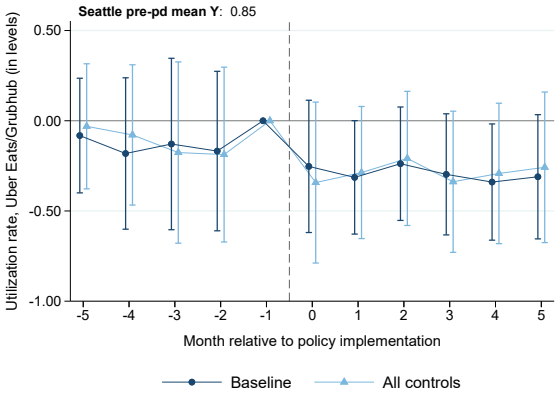
(A) Tasks Per Hour, All Apps with Imputation



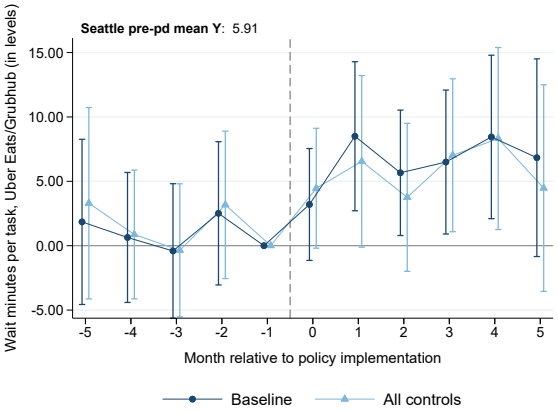
(B) Tasks Per Hour, DoorDash Only



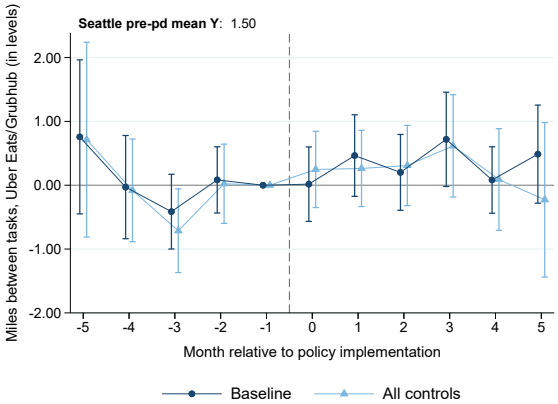
(C) Utilization Rate



(D) Task Wait Time (in Minutes)



(E) Distance Between Tasks (in Miles)



Notes: Panels (A), (B), (C), (D), and (E) plot, respectively, estimates of β_k from Equation (1) using the sample of more-attached workers on four outcomes for monthly time use in delivery: the monthly average number of delivery tasks completed per hour (Panel A for monthly total tasks completed on all delivery platforms divided by imputed monthly total hours spent on all delivery platforms and Panel B for monthly total tasks completed on platform DoorDash divided by monthly total hours spent on platform DoorDash), the monthly utilization rate (Panel C), monthly average wait minutes per task (Panel D), and monthly average miles traveled between tasks (Panel E). Given the data availability, utilization rates in Panel (C), task wait times in Panel (D), and distances between tasks in Panel (E) are calculated for Uber Eats and Grubhub. All outcomes are calculated in levels. Outcomes corresponding to zero tasks completed on the outcome-corresponding delivery platform(s) 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 Hours, Alternative Imputation Methods

	Total hours, with 1.25× imputation				Total hours, with 0.75× imputation			
	Levels		Logs		Levels		Logs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. More-attached workers								
Treated × Post	-0.642	0.465	-0.056	-0.057	-1.261	-0.124	-0.080	-0.082
	(2.714)	(2.459)	(0.064)	(0.063)	(2.697)	(2.440)	(0.066)	(0.066)
N worker-months	14,135	14,135	9,905	9,905	14,135	14,135	9,905	9,905
N workers	1,285	1,285	1,265	1,265	1,285	1,285	1,265	1,265
Treatment pre-pd mean Y	54.631	54.631	3.718	3.718	54.128	54.128	3.700	3.700
Treatment post-pd mean Y	38.627	38.627	3.447	3.447	37.743	37.743	3.389	3.389
B. Less-attached workers								
Treated × Post	0.946	0.849	0.237	0.348**	0.783	0.715	0.233	0.344*
	(0.804)	(0.885)	(0.131)	(0.131)	(0.782)	(0.866)	(0.139)	(0.138)
N worker-months	14,168	14,168	4,160	4,160	14,168	14,168	4,160	4,160
N workers	1,288	1,288	941	941	1,288	1,288	941	941
Treatment pre-pd mean Y	3.413	3.413	1.563	1.563	3.259	3.259	1.423	1.423
Treatment post-pd mean Y	5.176	5.176	2.179	2.179	4.884	4.884	2.037	2.037
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 the outcome imputed monthly total hours on all delivery platforms using two alternative imputation methods: multiplying the calculated average hours per trip in non-singleton supershifts by 1.25 (columns 1-2 for levels and columns 3-4 for logs) and by 0.75 (columns 5-6 for levels and columns 7-8 for logs) as hours per singleton supershift when imputing monthly hours of singleton supershifts and therefore monthly total hours. For a detailed description of the imputation procedure, see Section 3.1. 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, 5, and 7 present estimates without including additional controls, and columns 2, 4, 6, and 8 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. The rows of "treatment post-period mean Y" report means of corresponding outcomes posterior to the ordinance for exposed workers. * $p < .10$, ** $p < .05$, *** $p < .01$.

Table B.2: Robustness – Effects on Delivery Pay Per Task, Continuous Exposure Effects

	Base pay per task		Tip pay per task		Bonus+incentive+adjustment pay per task		Total pay per task	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. More-attached workers								
Exposure × Post	4.115***	4.070***	-1.552***	-1.536***	0.804***	0.783***	3.423***	3.374***
	(0.211)	(0.211)	(0.140)	(0.136)	(0.147)	(0.147)	(0.277)	(0.279)
N worker-months	10,311	10,311	10,311	10,311	10,311	10,311	10,311	10,311
N workers	1,324	1,324	1,324	1,324	1,324	1,324	1,324	1,324
B. Less-attached workers								
Exposure × Post	4.238***	4.506***	-1.559***	-1.525***	1.186*	1.419*	3.895***	4.419***
	(0.449)	(0.466)	(0.277)	(0.287)	(0.562)	(0.602)	(0.750)	(0.786)
N worker-months	4,448	4,448	4,448	4,448	4,448	4,448	4,448	4,448
N workers	991	991	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 four outcomes for delivery pay per task: delivery base pay per task (columns 1-2), delivery tip pay per task (columns 3-4), delivery bonus, incentive, and adjustment pay per task (columns 5-6), and delivery total pay per task (columns 7-8), where total pay includes base pay, tip pay, bonus pay, incentive pay, and adjustment pay. All outcomes are calculated in levels. 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, 5, and 7 present estimates without including additional controls, and columns 2, 4, 6, and 8 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 Tasks, Continuous Exposure Effects

	Any		Levels		Logs	
	(1)	(2)	(3)	(4)	(5)	(6)
A. More-attached workers						
Exposure × Post	0.004	-0.015	-16.588**	-13.123**	-0.266***	-0.266***
	(0.024)	(0.023)	(5.270)	(4.461)	(0.066)	(0.066)
N worker-months	14,784	14,784	14,784	14,784	10,311	10,311
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	-0.461	0.030	0.146
	(0.023)	(0.021)	(1.394)	(1.515)	(0.133)	(0.130)
N worker-months	14,883	14,883	14,883	14,883	4,448	4,448
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.4: Robustness – Effects on Delivery Earnings, Continuous Exposure Effects

	Base earnings		Tip earnings		Bonus+incentive+adjustment earnings		Total earnings	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. More-attached workers								
Exposure × Post	157.151***	160.248***	-146.780***	-124.352***	17.516***	16.594***	26.870	51.517
	(36.426)	(34.746)	(28.066)	(22.764)	(5.235)	(4.972)	(58.146)	(52.955)
N worker-months	14,784	14,784	14,784	14,784	14,784	14,784	14,784	14,784
N workers	1,344	1,344	1,344	1,344	1,344	1,344	1,344	1,344
B. Less-attached workers								
Exposure × Post	35.022**	34.101**	-13.365*	-13.977*	4.598***	4.651***	26.369	24.863
	(11.425)	(12.301)	(5.596)	(6.251)	(1.145)	(1.321)	(16.694)	(18.160)
N worker-months	14,883	14,883	14,883	14,883	14,883	14,883	14,883	14,883
N workers	1,353	1,353	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 four outcomes for monthly delivery earnings: monthly delivery base earnings (columns 1-2), monthly delivery tip earnings (columns 3-4), monthly delivery bonus, incentive, and adjustment earnings (columns 5-6), and monthly delivery total earnings (columns 7-8), where total earnings include base earnings, tip earnings, bonus earnings, incentive earnings, and adjustment earnings. 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, 5, and 7 present estimates without including additional controls, and columns 2, 4, 6, and 8 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.5: Robustness – Effects on Delivery Hours and Active Delivery Days, Continuous Exposure Effects

	Total hours, with imputation				DoorDash hours				Total active days			
	Levels		Logs		Levels		Logs		Levels		Logs	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. More-attached workers												
Exposure × Post	-1.197	0.016	-0.061	-0.066	-0.547	0.297	0.044	0.037	0.289	0.231	-0.010	-0.017
	(2.798)	(2.541)	(0.066)	(0.066)	(2.344)	(2.196)	(0.086)	(0.085)	(0.455)	(0.427)	(0.044)	(0.044)
N worker-months	13,959	13,959	9,795	9,795	14,784	14,784	6,587	6,587	14,784	14,784	10,311	10,311
N workers	1,269	1,269	1,249	1,249	1,344	1,344	913	913	1,344	1,344	1,324	1,324
B. Less-attached workers												
Exposure × Post	1.093	0.976	0.195	0.317*	0.491	0.600	0.055	0.133	0.341	0.273	0.103	0.189*
	(0.838)	(0.911)	(0.136)	(0.136)	(0.567)	(0.645)	(0.208)	(0.198)	(0.241)	(0.249)	(0.095)	(0.095)
N worker-months	14,344	14,344	4,270	4,270	14,883	14,883	1,576	1,576	14,883	14,883	4,448	4,448
N workers	1,304	1,304	957	957	1,353	1,353	389	389	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 outcomes for monthly delivery hours and active delivery days: imputed monthly total hours on all delivery platforms (columns 1-2 for levels and columns 3-4 for logs), monthly total hours on platform DoorDash (columns 5-6 for levels and columns 7-8 for logs), and monthly active days on all delivery platforms (columns 9-10 for levels and columns 11-12 for logs). 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, 5, 7, 9, and 11 present estimates without including additional controls, and columns 2, 4, 6, 8, 10, and 12 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.6: Robustness – Effects on Delivery Time Use, Continuous Exposure Effects

	Tasks per hour				Utilization rate		Task wait time (min)		Distance between tasks (mi)	
	All apps, with imputation		DoorDash only							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. More-attached workers										
Exposure × Post	-0.352***	-0.346***	-0.346***	-0.347***	-0.112***	-0.110***	5.785***	5.713***	0.294***	0.314***
	(0.027)	(0.027)	(0.037)	(0.037)	(0.023)	(0.022)	(0.614)	(0.636)	(0.071)	(0.073)
N worker-months	9,795	9,795	6,587	6,587	3,953	3,953	4,707	4,707	4,625	4,625
N workers	1,249	1,249	913	913	632	632	698	698	686	686
B. Less-attached workers										
Exposure × Post	-0.274***	-0.325***	-0.422***	-0.412***	-0.206***	-0.217***	6.014***	5.091***	0.197	0.127
	(0.071)	(0.069)	(0.094)	(0.089)	(0.062)	(0.065)	(1.335)	(1.420)	(0.195)	(0.231)
N worker-months	4,270	4,270	1,576	1,576	1,574	1,574	1,680	1,680	1,647	1,647
N workers	957	957	389	389	393	393	416	416	407	407
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 four outcomes for monthly time use in delivery: the monthly average number of delivery tasks completed per hour (columns 1-2 for monthly total tasks completed on all delivery platforms divided by imputed monthly total hours spent on all delivery platforms and columns 3-4 for monthly total tasks completed on platform DoorDash divided by monthly total hours spent on platform DoorDash), the monthly utilization rate (columns 5-6), monthly average wait minutes per task (columns 7-8), and monthly average miles traveled between tasks (columns 9-10). Utilization rates, task wait times, and distances between tasks are calculated for Uber Eats and Grubhub, given the data availability. All outcomes are calculated in levels. Outcomes corresponding to zero tasks completed on the outcome-corresponding delivery platform(s) 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, 5, 7, and 9 present estimates without including additional controls, and columns 2, 4, 6, 8, and 10 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. * $p < .10$, ** $p < .05$, *** $p < .01$.

C Model

In this appendix, we study the effects of a per-task minimum pay regulation in a simple model of the app-based delivery market, which for brevity we refer to as the "gig delivery" or "platform delivery" market. We closely follow the model of the ride-sharing market introduced by Hall, Horton and Knoepfle (2023) and extended by Fisher (2024*a*), deriving similar results in a somewhat simplified setting.

We assume throughout that the gig delivery market is small compared to the rest of the labor market and find that the minimum pay regulation can leave unchanged, increase, or decrease drivers' earnings depending on whether or not there is free entry and depending on the elasticity of demand for delivery services. If the gig delivery market faces free entry of drivers, a binding minimum pay regulation will have no effect on drivers' per period earnings, as the equilibrium value of doing delivery work must always remain equal to the fixed, constant outside option available to all workers (Hall, Horton and Knoepfle, 2023). Without free entry, the effect of a binding minimum pay regulation on drivers' earnings depends on the elasticity of demand for delivery services, consistent with the results in Fisher (2024*a*). Drivers' per-period earnings decrease when this demand curve is elastic, as the decline in trips per period exceeds the increase in pay per trip. Inversely, per-period earnings increase when demand is inelastic, as trips decline less than the increased in pay per trip.

C.1 Labor supply to the delivery market

Consider a market for gig delivery drivers where a single platform sets a per-delivery pay rate equal to w.[30] All workers, indexed by i, inelastically supply one period of full-time labor and can freely choose whether to spend the period doing platform delivery work or pursuing an outside option, which idiosyncratically varies across workers. This outside option includes both the value of work outside the gig delivery market and the value of non-work time.

If a worker chooses to drive in the gig delivery sector, their per-period earnings, $\omega^g = \theta w$, depend both on the platform-determined pay rate w for time spent completing delivery tasks and the share θ of the period spent engaged in those tasks. Worker j values the per-period outside option at ω_j^0 and chooses to do delivery work if $\omega^g > \omega_j^0$. If the value of the outside option is distributed according to some distribution $F(\cdot)$, the share of workers doing delivery work is $F(\omega^g)$. Assuming outside options follow a logistic distribution with scale parameter $\rho = 1/\sigma$ (where high ρ and low σ indicate greater variance in outside options), the share of workers doing delivery work is

$$\pi^g = Pr[\omega_j^0 < \omega^g] = \frac{e^{\sigma \ln \omega^g}}{1 + e^{\sigma \ln \omega^g}}. \tag{2}$$

The elasticity of this choice probability with respect to gig delivery earnings (given a fixed distribution of outside options) is

$$\frac{\partial \ln \pi^g}{\partial \ln \omega^g} = \sigma - \frac{1}{1 + e^{\sigma \ln \omega^g}} \cdot \frac{\partial}{\partial \ln \omega^g}(1 + e^{\sigma \ln \omega^g}). \tag{3}$$

[30] The single platform assumption is not crucial here, so long as the minimum pay standard is binding in what follows.

Focusing on the last term,

$$\frac{\partial}{\partial \ln \omega^g}(1 + e^{\sigma\omega^g}) = \sigma e^{\sigma \ln \omega^g}$$

Plugging this back in to (3) yields

$$\frac{\partial \ln \pi^g}{\partial \ln \omega^g} = \sigma(1 - \pi^g). \tag{4}$$

Letting $\overline{L}$ be the total amount of labor across both markets, the quantity of labor supplied to the gig delivery market in each period is

$$S(\omega^g) = \overline{L}\pi^g,$$

and the associated labor supply elasticity is

$$\varepsilon^S \equiv \frac{\partial \ln S(\omega^g)}{\partial \ln \omega^g} = \frac{\partial \ln \pi^g}{\partial \ln \omega^g} = \sigma(1 - \pi^g).$$

We assume that the delivery market is small in comparison to the overall labor market, such that $\pi^g \to 0$ and the labor supply elasticity facing the market is the constant σ. This is intuitive – as workers weigh wages more strongly than their idiosyncratic preferences, their labor supply will be more responsive to delivery market wages. The small market assumption also implies that changes in the gig delivery market have no effect on earnings in the outside market, ω^0, making the partial derivative in (3) applicable even in equilibrium.

C.2 Labor demand under a minimum pay standard

In contrast to the ride-hailing markets analyzed in Hall, Horton and Knoepfle (2023) and Fisher (2024*a*), the payments made to drivers in the gig delivery market are not directly paid by consumers.[31] Rather, consumers are presented with a cost of delivered items that reflects the price of food or merchandise charged by the restaurant or retailer and fees collected by the platform that are used both to compensate drivers and generate platform revenues. For the purposes of our analysis, it is not necessary to fully derive the determinants of demand for delivery drivers, which will depend jointly on consumer, restaurant, and platform behavior. Nor is it necessary to provide a full account for the objective functions that guide the platform (or the set of competing platforms) in their choice of w (see Fisher (2024*a*) for such a model). Instead we simply assume:

1. That there is *some* no-intervention equilibrium pay rate w^e chosen by the platform to maximize its profits, that is below the minimum rate $\bar{w}$ set by policy, and

2. For $\bar{w} > w^e$, the total demand for deliveries in the market is given by the downward-sloping demand curve $D(w)$, which is a reduced-form object reflecting the pass-through of costs to consumers and restaurants and any resulting decline in quantity demanded for deliveries completed by gig drivers.

[31] Recently, some ride-hailing platforms appear to have changed their driver payment practices to match gig delivery more closely, but this change emerged after Hall, Horton and Knoepfle (2023) and Fisher (2024*a*) were written.

C.3 Market equilibrium

Importantly, the per-trip pay rate (which is set by the firm or by a binding minimum policy rate) does not in itself determine worker earnings ω^g, which also depend on the endogenous fraction of time the worker spends doing deliveries during a period. For simplicity, we assume the platform distributes jobs evenly across workers, so that the share of the period spent completing delivery tasks is

$$\theta = \frac{D(w)}{S(\omega^g)} = \frac{D(w)}{S(w \cdot \theta)}$$

Re-arranging yields the equilibrium market-clearing condition, which is a simplified version of the corresponding conditions in Hall, Horton and Knoepfle (2023) (their equation (1)) and Fisher (2024*a*) (their equation (2)).

$$D(w) = \theta \cdot S(w \cdot \theta) \tag{5}$$

Since this condition does not equate supply and demand for delivery tasks, the pay rate w is not pinned down by this equilibrium condition, which depends upon both w and θ. Instead, the pay rate is determined by platform optimization and competition or by a binding minimum pay rate. Equation (5) should be understood as a constraint that specifies what the equilibrium value of θ will be for any specified choice of w. In particular, equation (5) implicitly defines the equilibrium market tightness (i.e. share of the period spent completing paying trips) as a function of the wage, $\theta(w)$. Totally differentiating equation (5), we can solve for $\theta'(w)$.

$$D'(w) = \theta'(w) \cdot S(w \cdot \theta(w)) + \theta(w) \cdot S'(w \cdot \theta(w))(\theta(w) + w \cdot \theta'(w))$$

Dividing through by $D(w)/w$ and noting $D(w)/w = \theta(w)S(w\theta(w))/w$,

$$\frac{D'(w)}{D(w)/w} = \frac{\theta'(w)}{\theta(w)/w} + \frac{S'(w\theta(w))w\theta'(w)}{S(w\theta(w))/w} + \frac{S'(w\theta(w))}{S(w\theta(w))/w\theta(w)}.$$

Multiplying the numerator and denominator of the middle term by $\theta(w)$,

$$\frac{D'(w)}{D(w)/w} = \frac{\theta'(w)}{\theta(w)/w} + \frac{S'(w\theta(w))}{S(w\theta(w))/w\theta(w)} \cdot \frac{\theta'(w)}{\theta(w)/w} + \frac{S'(w\theta(w))}{S(w\theta(w))/w\theta(w)}.$$

Defining elasticities as $\varepsilon^H = \frac{H'(x)}{H(x)/x}$,

$$\varepsilon^D = \varepsilon^\theta + \varepsilon^S \cdot \varepsilon^\theta + \varepsilon^S$$

$$\varepsilon^\theta = \frac{\varepsilon^D - \varepsilon^S}{1 + \varepsilon^S} = \frac{\varepsilon^D - \sigma}{1 + \sigma}$$

where $\varepsilon^D < 0$ is the elasticity of trip demand with respect to the per-trip pay rate, $\varepsilon^S = \sigma > 0$ is the elasticity of labor supply to per period earnings, and $\varepsilon^\theta < 0$ is the elasticity of θ to the per-trip pay rate. Notably, if both the elasticities of demand and supply are constant, then ε^θ is constant.

C.4 The effects of a minimum pay standard on per-period earnings

Given that earnings per period are $\omega^g = \theta w$, the elasticity of earnings to the binding per-trip minimum pay rate is

$$\varepsilon^{\omega} = 1 + \varepsilon^{\theta}.$$

Importantly, since $\varepsilon^{\theta} < 0$, this elasticity can be negative—that is, a higher per-trip minimum pay standard can lead to lower daily earnings if trips per period decline sufficiently. More specifically, the model with free entry implies the following key result:

Increasing the minimum pay standard increases driver daily pay if and only if

$$\varepsilon^{\theta} = \frac{\varepsilon^{D} - \sigma}{1 + \sigma} > -1$$

and has no effect if

$$\varepsilon^{\theta} = \frac{\varepsilon^{D} - \sigma}{1 + \sigma} = -1.$$

A few special cases are worth highlighting:

1. If worker mobility is perfect (no dispersion in outside options) such that $\sigma = \infty$, then $\varepsilon^{\theta} = -1$ and minimum pay standards have no impact on driver earnings. Intuitively, with free entry and no dispersion in outside options, the equilibrium value of doing delivery work must always remain equal to the fixed, constant outside option available to all workers. Any increase in $\bar{w}$ will lead to an increase in drivers that causes a decline in θ exactly offsetting the impact on pay per trip.

2. With a finite supply elasticity, higher minimum pay standards lead to lower driver earnings whenever the elasticity of demand is less than -1. Intuitively, when demand is elastic, the decline in trips per period exceeds the increase in pay per trip. This

3. Similarly, with a finite supply elasticity, higher minimum pay standards lead to higher daily driver earnings whenever the elasticity of demand is greater than -1. Intuitively, when demand is inelastic, the decline in trips per period is smaller than the increase in pay per trip.

In summary, a trip-level minimum pay standard will only increase driver earnings in settings where there is not free entry of drivers ($\sigma <$) and where the market-level elasticity of demand for drivers is sufficiently small ($|\varepsilon^{D}| < 1$). The first condition highlights a key tension in regulating gig work: increased pay necessitates the imposition of barriers to entry, which will tend to undermine the flexibility that the gig economy offers to workers. The second condition is more general—it is similarly the case in the traditional analysis of minimum wages in competitive labor markets that higher minimum wages will lower aggregate payroll if the absolute elasticity of demand is greater than one. However, in the traditional setting, there is a well-defined group of job holders who benefit from higher wages even when demand is highly elastic, with any reduction in demand manifesting as reduced employment levels. By contrast, in the platform gig work setting where tasks are allocated across all job seekers, every *individual* experiences a reduction in expected pay.

C.5 Discussion

Our empirical analysis of the Seattle minimum pay regulation finds a lack of earnings effect. In the month following the regulation entering into force, incumbent drivers saw an increase in monthly earnings, but those earnings subsequently fell back to their pre-treatment level, due to a reduction in the tasks available to incumbent drivers (Figures 7 and 8). We find no evidence that hours participating in gig delivery work fell substantially, consistent with earnings returning to baseline due to drivers struggling to find delivery tasks. In the context of the model, the lack of earnings effect implies that $\varepsilon^{\theta} = -1$, which could be driven either by free entry or unit-elastic demand. However, the descriptive evidence in Figure 5 suggests that free entry drives our findings, since the post-regulation share of tasks done by new entrants grows more in Seattle than in the comparison region.

A few important assumptions of the model are worth emphasizing. The model does not incorporate the pecuniary or effort costs of driving, and we implicitly assume that workers do not derive utility from idle time waiting for new tasks. These two assumptions imply that a larger value of θ is unambiguously negative for drivers – it implies lower earnings for a given per-task pay rate without any offsetting benefit. These assumptions align with our empirical finding that drivers face longer wait times and distances between trips under the minimum pay law (Table 6). Rather than saving on transportation costs or enjoying leisure time while waiting for new tasks, drivers appear to spend more time driving back to high-demand locations to obtain new tasks after the regulation went into effect.

Overall, in the context of the model, our findings suggest the the gig delivery market is subject to approximately free entry of drivers. This market feature implies that if policymakers seek to improve gig delivery drivers' earnings, they will struggle to do so without imposing some form of entry barrier, such as those traditionally governing entry to non-platform taxi markets in many cities. However, even with entry barriers, the qualitative effect of a per-task minimum pay regulation will depend on the elasticity of demand for delivery services; if that demand is elastic, the minimum pay regulation will still reduce drivers' earnings.