Price Caps on Platforms Using Dynamic Pricing: Theory and Evidence from Rideshare*

Andrew Bibler
University of Nevada, Las Vegas

Mark J. Tremblay
University of Nevada, Las Vegas; CESifo

March 26, 2026

Abstract

Dynamic pricing has become synonymous with rideshare, such as Lyft and Uber, who popularized the usage of such pricing strategies. While technological advancements have improved the ability of platforms to implement dynamic pricing, little is known about the welfare implications of the now commonly used strategy or policies that limit the use of dynamic pricing strategies. We develop a simple model of price caps on platforms that match demand and supply, showing that while the price control can be welfare enhancing by reducing platform markups, they can harm welfare by reducing efficiency as seen through decreases in output. Using a sample of rideshare receipts from users across the United States, we estimate the effects of a policy change that eliminated a base fare price cap on rideshare in Nevada, thereby enabling the use of dynamic pricing, on market prices, quantities, and welfare. Using a difference-in-differences approach, we find that eliminating the price cap led to an immediate and persistent increase in rideshare prices of 11-17% and increased quantities by 13-16%. Combining the empirical results with our model generates three key insights: (i) comparable with prior literature, we find that $\epsilon^d < -4.7$ and $\epsilon^s \in (0.74, 1.14)$; (ii) eliminating the price cap increased welfare by up to $130,000-145,000 per day in Nevada (a 9-17% increase in welfare), without accounting for wait times, and $330,000-680,000 (or 21-43%) using anecdotal changes in wait times; and (iii) an alternative policy that imposes a commission fee cap of 15%, commonly used for food delivery services, would have increased welfare by 30% relative to the price cap (or 33-52% with wait times).

Keywords: Surge Pricing, Uber, Lyft, Price Regulations, Price Ceiling, Commission Fee

*We acknowledge funding from the Regional Transportation Commission (RTC) of Southern Nevada.

1 Introduction

Dynamic pricing — adjusting prices in response to changes in demand or supply — has become ubiquitous across a wide range of markets, including online travel agencies, airlines, insurance, hotels, car rentals, electricity markets, theme parks, gas stations, secondary ticketing platforms, home-sharing, ride-sharing, online advertising, cloud computing, delivery and logistics services, toll roads, and ski resorts.[1] Its growing prevalence has coincided with heightened policy attention to complex pricing practices, particularly regarding price transparency and price gouging (Tran et al., 2025). Perhaps the most visible and culturally salient example of dynamic pricing — and its perceived harm to consumers (Elías et al., 2025) — is "surge pricing" implemented by Uber and Lyft, which has become a common reference point in popular media and online commentary.[2]

The increased usage and awareness of dynamic pricing has prompted policy consideration across the world. Concerns about price increases were prominently discussed leading up to the 2024 U.S. presidential election.[3] Many jurisdictions impose emergency-specific restrictions on price increases. Further restrictions are under consideration at the city, state, and national levels. For example, India imposes regulations that limit surge pricing;[4] the State of Washington has considered temporary rideshare caps for the upcoming World Cup;[5] and New York City has debated banning dynamic pricing altogether following public controversy over its proposed adoption in Wendy's fast-food chains.[6]

These debates highlight the need for a clearer understanding of the effects of dynamic pricing and related regulations on market efficiency, and for empirical evidence that informs optimal implementation of such policies. In markets characterized by the presence of two-

[1] Stamatopoulos et al. (2025) find that the introduction of digital price tags in grocery stores has so far not led to surge pricing.

[2] For example, when consumers face surge prices, they assign worse reviews to drivers Xu et al. (2025).

[3] https://www.bbc.com/news/articles/czxv65245kgo

[4] https://www.business-standard.com/industry/news/us-europe-supermarkets-surge-pricing-electronic-shelf-labels-125072900531_1.html

[5] https://www.king5.com/article/news/local/seattle-lawmaker-proposes-price-cap-rideshares-during-major-events-world-cup/281-ab411f1a-8475-421f-b9df-933e3c169b8d

[6] https://www.nytimes.com/2024/02/27/business/wendys-prices-inflation.html

sided platforms, such as rideshare markets, determining how policy constraints on dynamic pricing affect market outcomes is complicated by the market structure, which is defined by the presence of platforms that posses some degree of market power. Price caps are one potential policy that may be used to stifle the use of dynamic pricing by platforms. However, there is little to no research describing the effects of price ceilings in markets dominated by platforms.

To address this, we develop a simple model to describe the effect of price caps in rideshare markets. In the model, platforms match demand and supply and collect a fee for each transaction. The transaction fee creates a wedge between rider demand and driver supply and generates deadweight loss relative to the perfectly competitive benchmark. We consider the effects of imposing a price ceiling in this market, showing that it can be welfare enhancing or welfare reducing, depending on how restrictive the ceiling is. Price ceilings that are relatively close to the unrestricted equilibrium price improve welfare by reducing platform markups. The range of welfare enhancing price ceilings is defined by a threshold value; price ceilings below the threshold lead to excess demand and are welfare reducing. Importantly, we show that a price cap harms welfare if and only if the control reduces market quantities relative to the unregulated market. Consequently, estimating the effect of a price cap on market quantities produces a straightforward test of whether the price cap increases or decreases welfare. This ambiguity underscores the need for careful evaluation of dynamic pricing regulations, particularly as their use, and political salience, continue to expand.

We use a policy change in Nevada that removed a base fare price cap on the rideshare market to estimate the effects of change on welfare. The policy effectively eliminated surge and forced rideshare platforms to use a standard pricing formula based on the time and distance of the trip. We focus on the abrupt removal of a price control which reinstated the use of dynamic pricing. We use a difference-in-differences estimation strategy to compare outcomes in Nevada with outcomes in control states across the U.S., around the time of the policy change. Removing the price cap increased rideshare prices by 11-17%, and increased

the number of rides by 13-16%. Interpreting the empirical results in the context of the model, the positive effect of re-instating dynamic pricing on both prices and quantities implies that the price cap was welfare reducing, meaning that the cap was set below the range of welfare-enhancing price ceilings.

In addition, we provide estimates of the demand and supply elasticities, and welfare benefits from eliminating the price cap. The supply elasticity is point-identified under an assumption of a constant platform fee, i.e., the fee, in percentage terms, does not respond to the policy change. Under this assumption, we estimate a supply elasticity of 0.74 to 1.14, depending on the empirical specification. We infer an upper bound on the elasticity of demand of -4.7, suggesting that rideshare demand is relatively elastic. Further, we find that eliminating the price cap increased welfare in Nevada across drivers, riders, and the platforms by $130,000-145,000 per day or 9-17%. While the baseline estimates suggest that the price cap led to a sizable transfer to consumers due to lower prices, supplemental evidence suggests that these are a largely or totally offset by longer wait times. Finally, we estimate that a hypothetical alternative policy that caps platform fees at 15%, instead of a control on consumer prices, would have increased welfare by 30%.

2 Literature

Our primary contribution is that we are the first to analyze a price ceiling in markets where a platform inserts a commission fee when connecting demand and supply. In addition, we extend the literature on the welfare effects of policy regulation in rideshare markets by providing estimates, or bounds, of elasticities and welfare effects of a policy that confounds dynamic pricing through setting an effective price ceiling in a platform-dominated market.

Several related papers also consider effects of dynamic pricing regulations on market participants. These papers generally compare outcomes under unrestricted dynamic pricing with alternative policies that limit (i) the use of dynamic pricing but allow a single-price optimum or (ii) the size of the commission fees that platforms can charge. For policies that

restrict to a single-optimized price, Cachon et al. (2017) uses a stylized model to highlight the welfare benefits of surge pricing in rideshare markets.[7] Notably, both Williams (2022) (for airlines) and Castillo (2025) (for Uber) show that restricting to a single-optimized price creates winners and losers but reduces aggregate welfare.[8] In contrast, we focus on the effects of price ceilings which can be more restrictive than the single-optimized price regulation and find that a price ceiling can be welfare enhancing or welfare reducing, compared to the unrestricted dynamic pricing equilibrium, but that it reduced welfare in the case of the Nevada policy.

For policies that restrict platform commission fees, note that, relative to restricting consumer prices, commission caps are a more direct way of limiting platform markups. Prior work analyzes the effects of commission caps in the food delivery industry (Sullivan, 2025; Li and Wang, 2025). Through our empirical exercise, we compare outcomes under the price ceiling policy with the welfare results under an alternative commission cap policy, finding that the price cap substantially reduced welfare, relative to a commission cap, which better aligns platform profit maximization and market efficiency. Indeed, it is trivial to show that for every price ceiling, there exists some commission fee cap that is Pareto improving.

Our work also contributes to the empirical work on price ceilings which is limited and largely focuses on non-binding price ceilings that facilitate price coordination or higher markups (Knittel and Stango, 2003; Genakos et al., 2018). While Gatsios et al. (2026) find this for non-binding gas stations in Greece, they also find that the price ceiling is binding for some stations implying a welfare tradeoff. In our case, the price ceiling was clearly binding and we use both price and quantity data to determine the welfare implications from a binding price cap.[9]

[7] Garg and Nazerzadeh (2022) reveal that the platform should allocate surge benefits to drivers in an additive fashion opposed to a multiplicative one.

[8] Aizawa and Ko (2025) find that stricter regulations on dynamic pricing in long-term care insurance markets reduce insurer profits and increases market concentration, but provide limited benefits to consumers.

[9] Tripathi et al. (2025) consider quantity restrictions in the Paris home sharing market, an alternative to price regulations. While they find that occupancy falls in response to the quantity control, prices did not respond to the policy, making it difficult to disentangle welfare implications.

The Nevada rideshare policy, which effectively instituted a base fare price cap prior to the laissez-faire dynamic pricing environment, allows us to be the first to empirically investigate the effects of limiting dynamic pricing through price caps. In doing so, we also contribute to the literature on the estimation of demand and supply elasticities in rideshare markets. We estimate an upper bound on the demand elasticity of -4.7. This level of demand sensitivity is comparable to elasticities estimated by Christensen et al. (2025), who find elasticities between -4.4 and -5.1 for Uber trips using experimental price discounts, and Buchholz et al. (2025), who estimate demand elasticities of -4.3 and -5.5 for daytime and night time ride-hailing services from a large platform in Prague. Other studies document less elastic demand in ridesharing markets. For example, Cohen et al. (2016) find a demand elasticity for Uber rides between -0.4 and -0.6 during the platforms early years comparing ride acceptance rates of riders who received different surge offers due to slight differences in rider circumstances, e.g., time or geography.[10] Using a similar approach and time period for Lyft, Goldszmidt et al. (2020) find a demand elasticity of -0.59. Using tax introductions in Chicago, and assuming a 25% take rate by the rideshare platforms, Tarduno (2025) estimates a demand elasticity of -0.34.

There may be several reasons that demand is more price sensitive in this setting, relative to some prior work. First, we estimate demand during more recent time period. If platform markups increased over time, this could contribute to increasing demand sensitivity over time. Second, we use a salient policy change that alleviated a restrictive price control. In particular, lifting the price control likely (and anecdotally) led to large decreases in wait times. The resulting price increases may be offset by declines in wait times, such that the price effects overstate the change in the cost of rideshare services.

Further, we estimate supply elasticities ranging from 0.74 to 1.14 under the assumption of a constant platform fee in percentage terms. While the level fee is likely to respond to the rideshare policy, it does not rule out a constant fee in percentage terms. Prior work

[10]Prior to 2017, Uber offered temporary price increases in 10% increments, which were referred to as surge prices.

also finds relatively elastic supply of Uber rides. Tarduno (2025) estimate an elasticity of supply of 2.52 for Uber in Chicago, Chen et al. (2019) find a supply elasticities between 1-3 on Uber, and Angrist et al. (2021) estimate a supply elasticity of 1.2.

The apparent variation in market elasticities may stem from multiple sources. First, our estimates are specific to the rideshare market in Nevada, which may differ from other geographies. Second, the elasticities of demand and supply may have evolved over time. Given the sudden rise of rideshare markets and eventual establishment as a commonly used ride service, the different estimates may reflect the differential time frames of the empirical studies.

Our estimated welfare effects are related to prior literature on welfare in rideshare markets specifically. For example, Cachon et al. (2017) highlights the welfare benefits of surge pricing in rideshare markets.[11] Using data from Uber and structural estimation techniques, Castillo (2025) finds that that surge pricing increases total welfare in Uber markets by 2.15% of gross revenue, relative to the single price option. We contribute to these welfare findings by instead investigating the effects of a price cap, not a single optimized pricing, on dynamic pricing. Finding that the elimination of the base fare price cap increased welfare by 9-17% suggests that a base fare price cap is less efficient than a single-price policy.

Finally, through our empirical exercise, we consider the effects of an alternative commission cap policy. This comparison is valuable when considering the different policy options that impose pricing restrictions in platform-dominated markets, and directly relates to prior literature on the effects of commission cap policies. For example, using a counterfactual exercise, Sullivan (2025) finds that moderate commission caps would increase welfare in the food delivery industry. In a separate work on food delivery commission caps, Li and Wang (2025) find that caps imposed on local restaurants, not their competing chains, can have negative welfare consequences. In the context of the Nevada rideshare market, we find that capping rideshare fees to 15%, as done in food delivery, would increase welfare by 19% relative to the

[11] Garg and Nazerzadeh (2022) reveal that the platform should allocate surge benefits to drivers in an additive fashion opposed to a multiplicative one.

unregulated market and 30% relative to the base fare price cap.

3 The Model

In this section, we develop a model to analyze the effects of imposing a price cap in the rideshare market. For simplicity, we consider a static model, comparing the price cap policy equilibrium with that of the unregulated market. While the static model is a more natural fit for the price cap subgame where adjusting to demand and supply is limited, we view the static problem as an aggregation across different periods with relatively high and low prices in the unrestricted equilibrium. In addition, this approach aligns with our empirical strategy, which is to focus on comparing outcomes in the restricted and unrestricted states of the world.

3.1 The Market for Rideshare

Consider a monopolistic platform that connects demand and supply. We assume linear demand and supply functions. To reflect the rideshare setting, we assume that the platform collects a fee, f, from suppliers. That is, consumers pay the posted price, p, and suppliers receive $p - f$, which produces the following inverse demand and supply functions:[12]

$$D: \quad p = a - bq, \tag{1}$$

$$S: \quad p - f = c + dq, \tag{2}$$

where $a, b, c, d > 0$. In this case, the platform's profit is given by:

$$\Pi = f \cdot q(f). \tag{3}$$

[12]We focus on the single platform case, but our model extends to monopolistic competition among platforms that each manage their own demand and supply. As more platforms enter, the demand and supply for each platform's product become more elastic.

3.2 The Laissez-Faire Equilibrium

In an unregulated market, the platform uses dynamic pricing to optimally adjust prices and fees to changes in demand and supply (i.e., changes in the a, b, c, and d parameters). Setting demand equal to supply and solving for $q(f)$ implies that

$$q(f) = \frac{a - c - f}{b + d}.$$

Substituting this into Equation (3) and maximizing platform profits implies the following laissez-faire equilibrium:

$$f^* = \frac{a-c}{2}, \qquad p^* = \frac{a(b+2d)+bc}{2(b+d)},$$
$$q^* = \frac{a-c}{2(b+d)}, \qquad p^* - f^* = \frac{ad+(2b+d)c}{2(b+d)}.$$

3.3 The Price Cap Equilibrium

We focus on binding price caps, denoted by $\overline{p}$, which are below the Laissez-Faire equilibrium price, p^*, such that $\overline{p} < p^*$. In such cases, the platform chooses the optimal fee by solving the following problem:

$$\max_f \Pi \text{ s.t. } \overline{q} \leq \min\{q^d, q^s\},$$

where $q^d = \frac{a-\overline{p}}{d}$ is the quantity demanded under the price cap and $q^s = \frac{\overline{p}-c-f}{d}$ the quantity supplied under the price cap.

This generates two types of equilibria, depending on the level of $\overline{p}$. For $\overline{p}$ sufficiently close to p^*, the distortion is minor and demand still equates supply in equilibrium. Instead, if $\overline{p}$ is sufficiently low, there will be excess demand in equilibrium. These results, and the corresponding equlibria, are formalized in the following lemma:

Lemma 1. *There exists a $\tilde{p} \in (p_c, p^*)$, where p_c is the competitive equilibrium price given by*

$p_c = p(f=0) = \frac{ad+bc}{b+d}$, *such that the equilibrium for* $\overline{p} \in (\tilde{p}, p^*)$ *is given by*

$$\overline{q} = q^s = q^d = \frac{a-\overline{p}}{d} := \overline{q}_H, \qquad \overline{f} = \overline{p} - c - \frac{d}{b}(a-\overline{p}) := \overline{f}_H.$$

Instead, if the price cap is lower, $\overline{p} < \tilde{p}$*, then the equilibrium is given by*

$$\overline{q} = q^s = \frac{\overline{p}-c}{2d} := \overline{q}_L \leq q_L^d = \frac{a-\overline{p}}{b}, \qquad \overline{f} = \frac{\overline{p}-c}{2} := \overline{f}_L.$$

All proofs are in Appendix A.

Lemma 1 describes the ambiguous relationship between the price cap and total welfare. For moderate price caps, $\overline{p} \in (\tilde{p}, p^*)$, reducing the price cap increases welfare $\left(\frac{dW}{d\overline{p}} < 0 \text{ since } \frac{d\overline{q}_H}{d\overline{p}} < 0\right)$. However, when $\overline{p} < \tilde{p}$, reducing the price cap decreases welfare $\left(\frac{dW}{d\overline{p}} > 0 \text{ since } \frac{d\overline{q}_L}{d\overline{p}} > 0\right)$.

3.4 The Impact of a Price Cap

Imposing a price cap generates several meaningful implications. First, Lemma 1 shows that shortages may or *may not* exist depending on the level of the price cap. If $\overline{p} > \tilde{p} > p_c$, then demand equates supply, and no shortage exists (as in the laissez-faire equilibrium). Instead, if $\overline{p} < \tilde{p}$, excess demand creates a shortage.[13]

Second, a price cap reduces the platform's fee, relative to the unrestricted equilibrium:

Corollary 1. *Imposing a price cap reduces the platform's fee:* $f^* > \overline{f}$ *for all* $\overline{p} < p^*$.[14]

This result will be useful when we turn to welfare, after the empirical analysis, as it provides insight on the supply curve. With that in mind, our model also allows for a welfare comparison across the two regimes:

Proposition 1. *In regard to welfare, if* $\overline{p} \in [p_c, p^*)$*, then* $\overline{q} \geq q^*$ *and total welfare increases relative to the laissez-faire equilibrium. Instead if* $\overline{p} < p_c$*, then* $\overline{q} < q^*$ *and total welfare*

[13] In rideshare markets, this could lead to longer wait times. While outside the scope of our simple model, we discuss this further in Section 5.

[14] It is straightforward to show that $f^* \geq \overline{f}_H$ for all $\overline{p} \in (\tilde{p}, p^*)$ and that $f^* \geq \overline{f}_L$ for all $\overline{p} < \tilde{p}$.

decreases relative to the laissez-faire equilibrium.

Proposition 1 describes the differential welfare effects of moderate vs. restrictive price caps. Moderate price caps reduce platform markups and increase welfare, whereas price caps that are too low restrict the market and decrease welfare. This highlights the importance of distinguishing between policies that cap prices and those that allow for single-price profit maximization in place of dynamic pricing, as studied by Williams (2022) and Castillo (2025). Single-price profit maximization policies limit surplus creation and reduce aggregate welfare. While this force also exists with price caps, price caps limit markups, allowing for the possibility of welfare increasing price caps.

It is important to note the threshold difference between Corollary 1 and Proposition 1 implies that a price cap-induced shortage does *not* necessarily imply that the price cap is welfare reducing. Furthermore, the model provides a straightforward empirical test to determine whether the given price cap increased or decreased welfare since welfare is increasing with the price cap if and only if it increases market quantity. We test this by estimating the reduced form effects of the removal of a de facto price cap. If removing the price restriction increases quantity, then the price cap decreased welfare relative to the laissez-faire equilibrium.[15]

4 Nevada's Policy and Data

We focus on a Nevada policy that eliminated a temporary price cap on rideshare across the state. States across the U.S., including Nevada, limited business operations at the start of the COVID-19 pandemic in March 2020. Starting in the summer of 2020, Nevada gradually increased business capacity. While COVID related policies and restrictions were primarily determined at the state level, numerous states followed a similar pattern with a dramatic initial decline in activity in March 2020, followed by a gradual re-opening at the end of

[15]It is also worth noting that the model does not explicitly account for consumer costs from increased wait times, which we expect to be longer when a shortage exists. As a result, the model may understate welfare losses from price caps that lead to shortages.

2020 that accelerated in early 2021 due to widespread vaccine availability and increasing vaccination rates. For rideshare in Nevada,[16] the initial policy established the base fare equation as the maximum that could be charged during the emergency, which effectively eliminated dynamic pricing during that time.[17] This was not lifted until May 28th, 2021,[18] well after vaccine availability in the United States, and reinstated the ability for rideshare platforms to use dynamic pricing. Given this timeline, we focus on the elimination of the price cap, instead of the initial implementation which coincided with the COVID-19 shutdown.

Our main analysis focuses on a 21 week window around the removal of the price restriction on May 28th, 2021, using 1.2 million rideshare trips derived from email receipts across 150,000 U.S. users over this time period. Table 1 includes summary statistics for the entire sample, the state of Nevada, and other states. The first column includes averages over the entire sample. The average ride cost consumers $20.80, lasted 15.6 minutes, and traveled 6.8 miles. Columns 2-3 contain the average prices, travel time, and distance in Nevada for the period before and after Nevada eliminated the rideshare price cap. The analogous averages for all other states are contained in columns 4-5. In addition, columns 6-7 include summary statistics for a restricted set of control states that expanded business capacity on a similar timeline as Nevada in 2021.[19] Due to the overlap of lifting the price restriction and expanding to full business operations, we use both the full set of controls as well as the restricted set of controls in our analysis.

Table 1 provides suggestive evidence that the price cap limited rideshare prices in Nevada. The average price in Nevada increased by 18.2% after removing the price regulation ($17.95 to $21.24), while prices in other states were relatively stable ($20.61 to $20.99, or 1.8%). The

[16]State of emergency provisions are state-specific. Massachusetts and Nevada are the only two that included rideshare price restrictions. We focus on Nevada, as Massachusetts removed their price cap in October 2020, which was early in the reopening process and before vaccinations were publicly available.

[17]See NAC 706A.290 (2.), NRS 706A.170 (6.), and Senate Bill 279, which was introduced on March 18, 2021, to remove the Nevada Transportation Authority's ability to set a maximum fare that ride-hailing companies can charge in an emergency. As further support that this was a price cap, not an optimized single-price fare, we show in Figure B.3 that lifting the price restriction increased prices for every 6 hour time period within the week.

[18]See the announcement for lifting the rideshare emergency restrictions.

[19]These states are California, Connecticut, Illinois, Maryland, Michigan, and New Jersey.

average price increase in states that opened on a similar timeline was 5% ($20.53 to $21.54), which is higher than average across all controls but significantly lower than the change in Nevada. Comparing observation counts in Table 1 also provides suggestive evidence that lifting the price cap increased the number of rides. The number of rides per week increased by about 27% in Nevada, but only 8% in the rest of the sample and 12% in the restricted control group.[20]

Figure B.1 displays the natural log of averages prices (Panel A) and natural log of quantities (Panel B) in Nevada and other states at the weekly level. Each dot in Panel A is the natural log of the average price in Nevada in a given week. Prices increased dramatically in Nevada starting in the first week of the treatment. The dashed line represents the weekly average price across all other states. While prices trend upward over the sample period, the prices in other states evolved smoothly over the sample period. Panel B includes the natural log of the average number of rides per day. Each dot represents the average in Nevada, which shows a slightly delayed increase in the number of rides after the treatment date. The delay could be due to supply-side frictions, leading to a delayed reaction in quantities. However, as the dynamic pricing technology was already in place, prices could quickly adjust to the removal of the price restriction. Altogether, using the implications of the model, the apparent positive effect of dynamic pricing on prices and quantities suggests that the price cap was welfare reducing.

Finally, we compare rideshare markets in 2021 with those from a typical year in Appendix Table B.1 which includes summary statistics for the same samples around the same calendar date in 2022 and 2023. The average ride in the full sample in 2022-2023 cost $21.15 (compared to $20.80), lasted 15.9 minutes (compared to 15.6), and traveled about 7 miles (compared to 6.8 miles). In 2022-2023, the average price in Nevada decreased from $20.69 before May 28th to $19.75 after May 28th. There was a slight decline in prices after May 28th across all

[20]The pre-period includes 10 weeks and the post period includes 11 weeks (including the first week of treatment). So the sample includes $10{,}517/10 \approx 1{,}051$ rides per week in Nevada during the pre-period and $14{,}726/11 \approx 1{,}338$ rides per week in Nevada during the post-period.

other states and slight increase in the sample of restricted control states. In 2022-2023, the number of rides per week declined by 1.8%, 3%, and 1.2% after May 28 in Nevada, the full sample, and restricted control sample, respectively. The stable prices and quantities in the rideshare market over the same calendar date in 2022 and 2023, relative to 2021, supports the claim that the Nevada policy change in 2021 had a significant impact on the market.

5 Estimation and Results

5.1 Event Studies

We present changes in average prices and quantities in Nevada, relative to other states, using event studies. We use the following equation to produce event studies for price changes:

$$ln(price)_{iszt} = \delta_z + \delta_t + \sum_{j=-10}^{10} \beta_j \, \mathbf{1}(\text{event}_{st} = j) + \varepsilon_{iszt}, \tag{4}$$

where $ln(price)_{iszt}$ refers to the natural log of the total price for ride i that occurred in state s and zip code z on date t. We include zip code and date fixed effects represented by δ_z and δ_t. The summation indexes weeks around the initial treatment week. We omit the third week prior to treatment, $j = -3$, so that each $\widehat{\beta}_j$ represents a week-specific difference between prices in Nevada and other states, relative to the difference in the omitted week.[21]

We estimate the following conditional expectation specification for the number of rides using Poisson QMLE to produce event studies for the number of rides per day:

$$E[Rides|X]_{szt} = exp[\delta_z + \delta_t + \sum_{j=-10}^{10} \beta_j \, \mathbf{1}(\text{event}_{st} = j)] \tag{5}$$

where $Rides_{szt}$ refers to the number of completed rides in state s and zip code z on date

[21] In supplementary analyses, we include comparable event studies for years 2022-2023. In those years, the EDC music festival occurred in Nevada approximately one week prior to May 28th. While the festival did not take place over our 2021 sample period, we omit the third week before treatment for all event studies for consistency. For DDD estimates, we exclude the two weeks prior to May 28th from the sample for all years.

t. Like the event study specification for prices in Equation (4), we include zip code and date fixed effects, and estimate week-specific differences in the outcome between Nevada and other states, relative to the omitted week. To estimate Equation (5), we aggregate the number of rides to the zip code-date level.

The event studies are presented in Figure 1. Panel (a) of Figure 1 compares the average $ln(price)$ of rides in Nevada with all US states. Similar to the comparison of means in Figure B.1, the event study suggests the policy change led to an immediate increase in the average price of rides in Nevada of approximately 10 to 20%. Panel (b) restricts the control group to states that opened on a similar timeline to Nevada. The positive coefficients are slightly attenuated when restricting the control group, with week-specific estimates in the 5 to 15% range. Comparing Panels (a) and (b) suggests that part of the price increase in Nevada around the policy change may be due to the state's expansion to full business capacity, but that the majority of the increase in prices is not explained by the re-opening.

Panels (c) and (d) of Figure 1 show event studies for *rides* at the zip code level when comparing to the full set of control states and the restricted set of control states, respectively. Both panels show a slightly delayed quantity increase in Nevada, relative to other states, that reaches an approximate 15 to 20% increase that persists throughout the sample period. Overall, event study estimates resemble the changes in the raw averages presented in Figure B.1. While removing the temporary price cap increases both prices and quantities, prices react relatively quickly. The relatively rapid price response, compared to quantities, is consistent with platforms quickly adjusting price-setting behavior, and a lag in adjustments in driver supply.

5.2 Main Estimates

5.2.1 Methods

We start by estimating the main effects of re-instating dynamic pricing on $ln(price)$ using the following difference-in-differences specification:

$$ln(price)_{iszt} = \delta_z + \delta_t + \beta \cdot 1[NV \times Post]_{st} + \Gamma X_{iszt} + \varepsilon_{iszt}, \tag{6}$$

which is comparable to the event study specification used in Equation (4). We estimate a single treatment effect, β, from the coefficient on the interaction between the treated state and post-treatment indicators, $1[NV \times Post]_{st}$. The estimated effects summarize the conditional change in the price differential between Nevada and control states from the pre- to post-treatment period. In some specifications we include a vector of controls, X_{iszt}, which includes trip duration in minutes, trip distance in miles, and the ride type.[22] All specifications include date fixed effects, δ_t, and zip code fixed effects, δ_z. In addition, we include estimated effects on *price* to compare estimates in level and percentage terms. Tables 2 and 3 provide estimated effects of removing the price cap on prices (Table 2) and quantities (Table 3).

There are two primary threats to the validity of the difference-in-differences estimator. The first is due to the timing of the policy as it relates to other policies surrounding the COVID-19 pandemic. Because the initial restriction on dynamic pricing was a result of the state of emergency declared by the state of Nevada, not surprisingly, lifting the price restriction occurred as business operations returned to full capacity in Nevada. Consequently, the DiD estimator may capture changes due to the general relaxation of COVID-related restrictions in addition to the effect of the removing the rideshare pricing restriction. Multiple features of the state-level return to full business operations help to mitigate this concern. First, states gradually returned to full capacity with many, like Nevada, begining to reopen in 2020. We consider a 21 week period around May 28, 2021, more than a year after the start

[22] Ride types include UberX, UberXL, Black, Comfort, Lyft, Lyft XL, and Pink.

of the pandemic. The return to full capacity marks a marginal increase in activity, rather than a drastic shift from complete shut down to full operations. Second, the widespread availability of vaccinations occurred similarly across the country several months before the Nevada rideshare pricing policy change. Nevertheless, to address this concern, we compare results from two sets of controls groups: one using all states as controls and a second using a restricted control group that consists of states that opened on a similar timeline to the state of Nevada. With the restricted control group, the difference-in-differences estimator compares outcomes in Nevada with other states that opened on a similar timeline, such that the effects of expanding business operations are differenced out.[23]

The second major threat to the identification strategy is differential seasonality in the ride sharing market in Nevada, relative to the control states. Because the difference-in-differences estimator compares outcomes in Nevada and other states within a 21 week window, the estimates may also reflect differences in the seasonal patterns of rideshare prices and quantities. For example, travel to Nevada may fluctuate over the summer months due to changes in temperatures, i.e., higher temperatures in August, which coincides with the latter portion of the sample period. We address this concern by incorporating a triple difference estimator. Intuitively, we compare the difference-in-differences estimates from 2021 with analogous differences-in-differences estimates in other years in which there was no major policy change in the ridesharing market. Estimates in other years reflect typical seasonal patterns in Nevada, relative to the control states. By comparing the 2021 estimates with estimates from 2022-2023, we effectively remove the portion of the main estimates that is attributable to differences in seasonal patterns. We estimate this directly using the following specification:

$$ln(price)_{iszyt} = \delta_{zy} + \delta_t + \beta_1 \cdot 1[NV \times Post]_{st} \cdot 1[2021]_y + \beta_2 \cdot 1[NV \times Post]_{st} + \Gamma X_{iszyt} + \varepsilon_{iszyt}, \quad (7)$$

where δ_{zy} represents zip code-year fixed effects, and δ_t refers to date fixed effects. Now, $Post_{st}$

[23]The restricted control group includes California, Illinois, Michigan, Connecticut, New Jersey, and Maryland, all of which returned to full capacity in our sample period.

refers to the dates after May 28th within the calendar year and β_1 describes the difference between the the 2021 difference-in-differences estimator and the 2022-2023 difference-in-differences estimator. That is, the estimates describe differences in outcomes between Nevada and control states around the same calendar date in 2021 and other years. We include the estimates and analogous event studies in the appendix. We find similar results using the DiD and DDD methods, suggesting that the main estimates are unlikely to be driven by differences in seasonal ridesharing patterns between Nevada and control states.

We use a similar approach to estimate the effects of lifting the price restriction on market quantities. The main difference in estimation of quantity effects is that we aggregate ride level data to the zip code by day level to estimate the effects on quantities. First, we estimate the following conditional expectation using Poisson QMLE:

$$E[Rides|X]_{szt} = exp[\delta_z + \delta_t + \beta 1[NV \times Post]_{szt}] \tag{8}$$

where $Rides_{szt}$ refers to the number of completed rides at the zip code (z) by day (t) level. To start, we include zip code-day observations with zero rides in sample. To do this, we create a balanced panel of rides at the zip code-day level by aggregating to the zip code-day level and filling in zeros for zip code-days with no rides. In doing so, there are a significant number of observations with zero rides. We use the Poisson QMLE to estimate the effects in percentage terms while including observations with zero rides. To address the first identification threat of coinciding policy changes, we present estimates using both control groups: (1) all states and (2) the restricted set of states that opened on a similar timeline to Nevada. In addition, we estimate level effects by estimating the analogous specification by OLS.

The second panel in Table 3 presents the estimated effects on quantities when excluding the zip code by day observations with zero rides. We do this to test whether there is significant within-zip code movement on the extensive margin. We estimate the percentage effects when excluding zeros by OLS using $ln(rides)_{st}$ as the outcome, and estimate level

effects by OLS using $rides_{st}$ as the outcome.

Finally, to address the potential concern of differential seasonality, we presents results of the triple difference estimator for quantities, comparing DiD estimates in 2021 when the policy change took place with DiD estimates for 2022-2023 when there was no policy change. To estimate the triple-difference equation for quantities, we us the analogous specification. Like the price equation, we include zip code-year fixed effects and date fixed effects. The post-period refers to the period after May 28th in each calendar year. The estimated effect is the difference between the within year change in the number of rides (or natural log of the number of rides) in Nevada, relative to control states, in 2021, and the within year differences in outcomes in other years between Nevada and the control states around the same calendar date. Triple difference estimates are included in the appendix. Similar to the price results, we find little evidence that the main estimates are driven by differential seasonal patterns in Nevada, compared to the control states.

5.2.2 Results

Column 1 of Table 2 includes the difference-in-differences estimate on the natural log of prices with zip code and day fixed effects when including all states as controls. Column 2 limits the set of control states to those that re-opened a similar timeline to Nevada in 2021. When comparing to all states, we find that the policy change increased prices by 14%. Limiting the set of control states to those that opened on a similar timeline, we estimate that lifting the price restriction increased prices by 11%, which suggests that some portion of the price increase may be due to relaxing COVID restrictions around the same time as the rideshare policy changed.

Column 3 includes estimates that condition on travel time, travel distance, and the ride type (e.g., UberX, Uber XL, Lyft). Conditional on ride characteristics, we estimate that lifting the price restriction increased prices by 17% on average. The increase in the coefficients from columns 2 and 3 suggest that the policy is correlated with ride level characteristics. We

present estimated effects on trip duration and trip distance in Appendix Table B.2. When comparing with the restricted set of control states in Columns 2 and 4 of Table B.2, we find that dynamic pricing decreased the average travel time by 1 minute and reduced the average trip distance by approximately 0.5 miles. Conditioning on ride characteristics helps adjust for changes in the composition of the type of rides that occurred after the policy change. Because the average trip duration and distance decreased after lifting the price restriction, and both features are positively correlated with prices, conditioning increases the estimated effect on average prices.

Columns 4-6 of Table 2 include estimated effects on level prices. The results are consistent when using the level price or the natural log of the price as the outcome and show that dynamic pricing increased average prices by \$2.47 to \$3.97 in Nevada, relative to other states. Overall, the results are consistent across specifications and suggest that eliminating the price cap increased prices in the rideshare market in Nevada by 11-17%, meaning that the price cap was binding. All estimated price effects in Table 2 are statistically significant at the 1% level.[24]

Next, we turn to estimating the effects on quantity (number of rides). Note that because there are many observations at the zip code by date level with zero rides, we form a balanced panel of zip code by day level rides, including observations with zero rides in a given zip code-day. Columns 1-2 of Table 3, Panel A includes the Poisson QMLE estimates when including zip code-day level observations with zero rides. Aggregating to the zip code by day level allows for controlling for time invariant characteristics at the zip code level. We find that the policy change increased the number of rides by 16% when using all states as controls and by 13% when using the restricted set of control states. Both estimates are statistically significant at the 10% level or better with standard errors clustered at the zip code level. In last two columns of Table 3, Panel A we present the estimated effects in level terms, finding that lifting the policy restriction increased the number of rides by 0.27

[24]In all specifications, we use standard errors clustered at the zip code level.

to 0.3 per zip code-day. The level estimates are 53% (0.306/0.58) and 35% (0.267/0.76) of the sample means, suggesting a significant impact on the quantity of trips in the rideshare market.

In Panel B of Table 3, we report estimated effects on the quantity of rides in percentage and level terms, which are estimated by OLS. We estimate a 5 to 6% increase in the number of rides when using $ln(rides)_{szt}$ as the outcome, and 0.6 to 0.7 rides per zip code-day when using $rides_{szt}$ as the outcome. The estimated level changes represent increases of 27% ($0.704/2.59 \approx 0.27$) and 26% ($0.651/2.52 \approx 0.26$) of the sample means. Three of the four estimates are statistically significant at the 10% level or better when clustering at the zip code level. Comparing the quantity estimates when excluding observations with no rides with the estimated effects in Panel A suggests that there is significant movement on the extensive margin, perhaps due to using a subset of the population of rideshare trips. Overall, while there is some variation in the magnitudes, the estimated quantity effects consistently imply that lifting the price restrictions had a meaningful impact on the number of trips.

Indeed, the evidence presented in Tables 2 and 3 suggests that the price cap, i.e., disabling dynamic pricing, imposed meaningful restrictions on the rideshare market prior to its elimination. The policy change increased prices (rides) by 11-17% (13-16%), and the changes are statistically significant across a variety of specifications. In addition to changes in the prices and quantities, there may be other, unobserved effects on consumers and drivers, such as rider wait times. While we do not observe wait times, the Las Vegas Sun reported extremely long wait times at the Las Vegas airport the week before the price cap was eliminated[25] and the Nevada Transportation Authority reported increased wait times of 225% and fewer drivers (less than 40%) relative to pre-pandemic numbers.[26] This aligns with the literature on surge pricing and wait times in rideshare markets, where Yan et al. (2020) reveal a reduction in wait times may be the main benefit of surge pricing, and Castillo et al. (2025)

[25] https://lasvegassun.com/news/2021/may/23/extended-wait-times-for-uber-lyft-are-raising-conc/

[26] https://news3lv.com/news/local/sisolak-signs-regulation-letting-ride-shares-bring-back-surge-pricing

find that occasional occurrences of high prices prevent extended periods of long wait times.

5.2.3 DDD Estimates

We supplement our main analysis with a triple difference estimator. Intuitively, we compare the difference-in-differences estimator in 2021 with the analogous difference-in-differences estimator for year 2022-2023 when no policy change occurred. A 2022-2023 DiD estimator describes differential changes in the Nevada rideshare market around the same date in a typical year, relative to other states. For example, if there are differential seasonal patterns between Nevada and control states, we may mistakenly interpret seasonal patterns as an effect of the relaxation of price restrictions in Nevada in 2021.

To start, we provide event studies that compare the prices and quantities in Nevada and control states around May 28th in 2022-2023 in Appendix Figure B.2. Panel (a) includes the difference in the average $ln(price)_{iszt}$ and panel (c) includes the difference in average $rides_{szt}$. Figure B.2 shows relatively comparable trends in prices and rides, but also shows a spike in prices in Nevada in the two weeks before May 28th in 2022-2023. The relatively high prices in Nevada in the two weeks before May 28th in 2022-2023 are driven by the Electric Daisy Carnival (EDC), which took place soon before May 28th in both 2022 and 2023. The festival takes place in Las Vegas, Nevada, and attracted approximately 500,000 attendees each year. The 2021 festival took place in October, well after our sample period. Panels (b) and (d) of Figure B.2 include triple difference event studies, comparing the outcomes in Nevada in 2021 and 2022-2023 with the analogous changes in the restricted set of control states. The EDC festival in 2022-2023 leads to a significant negative price coefficients in the two weeks prior to treatment when using the DDD estimator. For this reason, we omit the last two weeks before treatment for all years when estimating the effects of dynamic pricing using the DDD estimator.

Table B.3 includes the DDD estimates for prices, which are comparable to the main DD estimates presented in Table 2. When using the restricted set of control states, we find

that dynamic pricing increased prices by 12.6% unconditionally (compared to 11.2%), and 17.5% when conditioning on ride characteristics (compared to 17.3%). Similarly, using the DDD estimator, we find a 14.2 to 15.8% increase in rides (compared to 12.8 to 16.3%). The similarity in the DD and DDD estimators suggest that the main estimate are not driven by typical differential patterns in the rideshare market in Nevada and other states.

5.3 Heterogeneity in Price Effects

Given that the policy change effectively re-instated the use of dynamic pricing, we expect the effects of the policy change to be higher in periods of high demand. In these periods, riders are more likely to experience temporarily higher prices when dynamic pricing is allowed. However, in the period before the policy change, prices could not increase during these periods. To test this, we present estimated price effects using the difference-in-differences estimator with the restricted set of control states for 28 ($= 7 \times 4$) 6 hour blocks across the week which we present in Figure B.3. Each dot represents a treatment effect specific to a 6 hour block on a particular day of the week, e.g., Monday from midnight to 6 a.m., Monday from 6 a.m. to noon, etc.). The estimates are larger in magnitude during expected periods of high demand, particularly during the periods of Friday and Saturday evening (and into the following morning hours), with the two largest estimates occurring at 6 p.m. to midnight on Saturday and midnight to 6 a.m. on Sunday. The estimates also appear generally higher on weekends and in the evening time, and relatively small in magnitude during most morning hours. To summarize, we present the average estimated price effects for two subsamples, rides after 5pm on all days, and weekend rides, in Table B.5. From Panels A and B, we estimate that removing the price restriction increased prices by 16 to 23% for rides after 5 p.m., and by 13 to 20% for weekend rides, both of which fall above the analogous estimated average effects of 11 to 17% in the full sample. The price heterogeneity shows that the effects are concentrated in periods that we expect relatively high demand, which are the periods that we expect price controls to create greater distortions when dynamic pricing is

not available.

6 Policy Implications

Interpreting the empirical results in the context of the model provides several insights on the welfare effects of price restrictions in the rideshare market. By Proposition 1, increases in both price and quantity following the elimination of the price cap implies that $\overline{p} < p_c$ and that the price cap was welfare reducing. Given this, we use the model to estimate, or bound, supply and demand elasticities, and to bound the welfare benefits from eliminating the base fare price cap.

6.1 Elasticities and Welfare Comparisons

We focus on the empirically observed case in which eliminating the price cap increases quantities. Figure 2a describes the observed equilibrium under a price cap $(\overline{q}, \overline{p})$, and the observed equilibrium after removing the price cap (q^*, p^*) — the large bullets in Figure 2a represent observed equilibria. The possible locations of the competitive equilibrium shown in Figure 2a are implied by Proposition 1. Estimating the supply elasticity only requires an assumption on the ratio of the commission fee to the demand price. Assuming that the fee is a constant percentage of the demand price means that the implied elasticity from the reduced form results on the price and quantity correctly identifies the elasticity of supply. Note that this assumption is consistent with the model prediction that the fee decreases with the price cap in level terms. In this case, the supply elasticity is directly estimable using results in Tables 2 and 3. Using the reduced form estimates in Column 2 of Tables 2 and 3, which include the restricted sample of controls, implies an elasticity of $\varepsilon_s = 0.128/0.112 \approx 1.14$.[27] The lowest implied elasticity of supply that we find uses the estimated price effect conditional on ride characteristics, from Column 3 of Table 2, which implies and elasticity of supply of

[27]The implied elasticity is similar when based on results using the full set of controls from Column 1 of Tables 2 and 3, $\varepsilon_s = 0.163/0.144 \approx 1.13$.

$\varepsilon_s = 0.128/0.173 \approx 0.74$.

Next, we turn to bounding the demand elasticity and welfare changes. First, we determine a linear equation for the supply function that is consistent with our estimates by imposing an assumption on the size of the commission fee as a fraction of the demand price. While the reduced form effects on prices and quantities can identify the elasticity of supply, the assumption about the magnitude of the fee relative to the price pins down the location of the supply function. Anecdotal evidence suggests that rideshare platform fees are approximately 50% of the price that consumers pay in the laissez-faire equilibrium.[28] We label the supply function $S_{50\%}$ in Figure 2c to convey the added assumption. Next, we find the steepest demand functions that is consistent with the $S_{50\%}$. We include a general graphic depiction in Figure 2c and the resulting plausible supply and demand curves are presented in Figure 3b.

We construct the supply and demand functions presented in Figure 3b in the following way. The assumption that the fee is equal to 50% of the price allows us to calculate two points on the supply function. First, we use the pre-treatment average price and quantity from Table 1 to determine the price-quantity combination under the price ceiling $(\bar{q}, 0.5\bar{p}) = (150, 0.5 \times \$17.95)$. From Table 1, $\bar{p} = \$17.95$. Similarly, 10,517 rides occurred in Nevada in the 10 week pre-period, so we use $\bar{q} = 150$, which is the pre-period average rides per day. Second, we use the estimated effects on prices and quantities to determine a second point on the supply function, $(q^*, 0.5p^*) = (175, 0.5 \times \$21.04)$. We determine the equilibrium quantity using the reduced form estimated effect on quantity of 16.3%, so that $q^* = 150 \times (1+0.163) \approx 175$. We use the estimated effect on prices to determine $p^* = \bar{p} + \$3.09 = \$21.04$ (Table 2 Column 4). These two points imply $\overline{S}$ is given by $p - f = -0.26 + 0.0616q$.

Using $S_{50\%}$, we determine the steepest possible demand curve ($\overline{D}$) from the restrictions on the location of the competitive equilibrium from Figure 2a; this generates Figure 2c. To determine the demand function that corresponds to this supply function, we find two

[28] As seen by data from FairFare at `https://www.youtube.com/watch?v=1zGlYNS2qGk`.

points on the demand function. The first is $(q^*, p^*) = (175, \$21.04)$. The second comes from using the result that $\overline{p} < p_c$, so that $\overline{p}$ gives the lowest possible value of p_c. Assuming that $p_c = \overline{p}$, along with the implied supply function, means that the competitive equilibrium would occur at $(q_c, p_c) = (296, \$17.95)$.[29] Now, the two points correspond to the steepest demand function that is consistent with empirical findings and applied assumptions: $\overline{D}$ is given by $p = 25.51 - 0.0255q$. This is true because any alternative p_c, such that $p^* > p_c > \overline{p}$ implies a competitive equilibrium occurs at higher quantity and a flatter demand function.[30] From this demand function, we determine an upper bound on the elasticity of demand, $\epsilon^d < -4.7$.[31]

The model also allows us to determine upper and lower bounds for the increase in welfare following the removal of the base fare price cap (Figures 2b and 2d). The minimum increase in welfare due to the price cap removal is found by considering the flattest possible demand and supply curves $(\underline{D}, \underline{S})$, as shown in Figure 2b.[32] Note that the sample represents approximately 0.2% of the total market.[33] Thus, we inflate the bounds using level estimates by a factor of 500 to approximate the dollar value of the total change in welfare at the state level. In Figure 3a, we determine the minimum increase in welfare: $\underline{W} = (21.04 - 10.52) \cdot (175 - 150) \cdot 500 \approx \$130{,}000$; a 16.7% increase in welfare.[34] To determine the maximum change in welfare, we use the steepest plausible demand curve under the assumption of a constant 50% platform fee, which is described above and presented in

[29]From the supply equation and setting $f = 0$, implies $q = (17.95 + 0.26)/0.0616 \approx 296$.

[30]If demand is steeper, then the perfectly competitive equilibrium is below the price cap equilibrium which is not possible.

[31]This is the elasticity calculated at (q^*, p^*), the unobstructed platform equilibrium: $\overline{\varepsilon^d} = (\delta q/\delta p) \times (p^*/q^*) \approx (1/-0.0255) \times (21.04/175) \approx -4.7$. Alternatively, the implied demand elasticity at the competitive equilibrium is $\overline{\varepsilon^d} = (\delta q/\delta p) \times (p_c/q_c) \approx (1/-0.0255) \times (17.95/296) \approx -2.4$

[32]For the given change in q, $q^* - \overline{q}$, welfare is increasing in the steepness of the demand or supply functions.

[33]We determine this by comparing our sample size to the publicly available samples for Chicago, New York City, and the Las Vegas airport.

[34]In Figure 3a, the \$17.95 comes from the pre-period average price in Nevada in Table 1, which increases by \$3.09 to \$21.04 based on Table 2 (Column 4); this gives \$10.52 = (0.50) × 21.04. Table 1 reveals that 10,517 rides occurred in Nevada in the 10 week pre-period, which amounts to 150 per day; Table 3 (Column 1) reveals a 16.3% increase which gives the 175.

Figure 3b. We find that $\overline{W} \approx \$145{,}000$[35]; a 9% increase in welfare.

Under the scenario in Figure 3b, which better mirrors results from the literature, we can determine welfare changes for buyers, sellers, and the platform. The price cap increased consumer surplus by 116%, reduced producer surplus by about 26%, and reduced platform fees by about 27%. While total welfare increased by 9%, the combined change in producer surplus and platform fees accounts for more than double that change. There is an offsetting reduction in consumer surplus, meaning that the price cap policy reduced welfare, but also acted as a sizable transfer to consumers, although the gains accrued to a relatively small number of market participants.

6.2 Accounting for Wait Times

So far, we have used our price and quantity estimates from the Nevada price cap shock to better understand demand and supply elasticities as well as the welfare effects from the price cap. While we do not observe wait times, Ghili et al. (2026) show that wait times are longer in thinner rideshare markets. The Regional Transportation Authority of Southern Nevada stated that wait times were up 225% prior to the removal of the cap,[36] Anecdotally, wait times typically vary between 5-10 minutes, suggesting that the price cap policy increased the average wait time by 11.25 to 22.5 minutes. Further, Goldszmidt et al. (2020) and Buchholz et al. (2025) estimates riders are willing to pay \$19 and \$13.21 per hour to reduced wait times, respectively). Combining the estimated increase in wait times, along with the estimated value of decreasing wait times, we consider a back-of-the-envelope calculation for welfare that includes the increase in wait times under the base fare price cap. Applying the 225% increase, as well as value of time estimates from Goldszmidt et al. (2020) and Buchholz et al. (2025), generates a conservative wait time cost per rider under the price cap of $5 \times 2.25.21/60 = \$2.48$ and a more aggressive wait time cost per rider under the price cap

[35] $500 \times \int_{\overline{q}}^{q^*} (\overline{D}(q) - \overline{S}(q))dq$

[36] https://news3lv.com/news/local/sisolak-signs-regulation-letting-ride-shares-bring-back-surge-pricing

of $10 \times 2.25 \times 19/60 = \$7.13$. By including wait times, this implies that the \$3.09 average savings per rider under the price cap amounts to a best case savings of \$0.61 and a worst case cost of \$4.04.

Extending these results to our welfare estimates implies that the 116% increase in consumer surplus from the price cap, actually amounts to anywhere from a 22% increase in consumer surplus to a 43% decrease in consumers surplus. Similarly, instead of a welfare increase of \$145,000 (or 9%), including the cost of wait times implies that the removal of the base fare price cap actually increased welfare by \$330,000-680,000 (or 21-43%). These findings highlight how the price restriction, in addition to distorting prices, harms rideshare app efficiency and prevents platforms from connecting riders and drivers in a timely fashion which exacerbates welfare losses.

6.3 Comparison with a Fee Cap Policy

In other settings, regulators have favored direct regulation of platform commission fees. For example, a number of jurisdictions imposed a 15% commission fee cap on food and grocery deliveries during the COVID pandemic, some of which have persisted; Apple conceded a 15% commission for small app producers, down from 30%, as a part of the Apple vs Epic litigation; and the state of Washington is considering capping rideshare commissions directly.[37] Capping platform commissions, rather than directly regulating price, offers policy makers an alternative pricing regulation which is worth investigating. Importantly, while Proposition 1 shows that price caps can be welfare reducing, capping commissions is never welfare reducing relative to the laissez-faire equilibrium. Unlike price caps, commission fee caps do not inhibit dynamic pricing and focus solely on limiting platform markups. For comparison, we consider the implication of a 15% commission fee cap in the rideshare market under Figure 2c where $\overline{D}$ is given by $p = 25.51 - 0.0255q$ and $\overline{S}$ is given by $p - f = -0.26 + 0.0616q$. Under such a commission fee cap, the equilibrium is given by $p = 18.79$, $(1-f)p = 15.97$, and $q = 264$, and

[37] https://www.king5.com/article/news/local/seattle-lawmaker-proposes-price-cap-rideshares-during-major-events-world-cup/281-ab411f1a-8475-421f-b9df-933e3c169b8d

welfare increases by 30% (19%) relative to the price cap (laissez-faire) equilibrium when the cost of wait times is not included. By including wait times, welfare under the commission fee cap increase by 33-52% relative to the base fare price cap (assuming no additional benefits from shorter wait times under the commission fee cap).

7 Conclusion

In a simple model of rideshare where a platform connects demand with supply, we show that price caps can increase or decrease welfare relative to the unregulated market, ultimately making the success of a price cap an empirical question. By estimating changes in prices and quantities following the elimination of a rideshare price cap in Nevada, we show that the price cap was too low so that prices, quantities, and welfare increase following the return to the dynamic pricing equilibrium that is commonly seen in rideshare.

References

Aizawa, N. and Ko, A. (2025). Dynamic pricing regulation and welfare in insurance markets. *Journal of Political Economy*, 133(8):2371–2413.

Angrist, J. D., Caldwell, S., and Hall, J. V. (2021). Uber versus taxi: A driver's eye view. *American Economic Journal: Applied Economics*, 13(3):272–308.

Buchholz, N., Doval, L., Kastl, J., Matejka, F., and Salz, T. (2025). Personalized pricing and the value of time: Evidence from auctioned cab rides. *Econometrica*, 93(3):929–958.

Cachon, G. P., Daniels, K. M., and Lobel, R. (2017). The role of surge pricing on a service platform with self-scheduling capacity. *Manufacturing & Service Operations Management*, 19(3):368–384.

Castillo, J. C. (2025). Who benefits from surge pricing? *Econometrica*, 93(5):1811–1854.

Castillo, J. C., Knoepfle, D., and Weyl, E. G. (2025). Matching and pricing in ride hailing: Wild goose chases and how to solve them. *Management Science*, 71(5):4377–4395.

Chen, M. K., Rossi, P. E., Chevalier, J. A., and Oehlsen, E. (2019). The value of flexible work: Evidence from uber drivers. *Journal of Political Economy*, 127(6):2735–2794.

Christensen, P., Nino, G., and Osman, A. (2025). The demand for mobility: Evidence from an experiment with uber riders. *Journal of Political Economy*.

Cohen, P., Hahn, R., Hall, J., Levitt, S., and Metcalfe, R. (2016). Using big data to estimate consumer surplus: The case of uber. Technical report, National Bureau of Economic Research.

Elías, J., Lacetera, N., and Macis, M. (2025). Is the price right? the role of economic trade-offs in explaining reactions to price surges. *Management Science*.

Garg, N. and Nazerzadeh, H. (2022). Driver surge pricing. *Management Science*, 68(5):3219–3235.

Gatsios, G., Genakos, C., and Papadokonstantaki, S. (2026). The impact of price ceiling regulation: Evidence from the retail gasoline market. *Available at SSRN*.

Genakos, C., Koutroumpis, P., and Pagliero, M. (2018). The impact of maximum markup regulation on prices. *The Journal of Industrial Economics*, 66(2):239–300.

Ghili, S., Kumar, V., and Teng, F. (2026). Spatial distribution of access to service: Theory and evidence from ride-sharing. *Management Science*.

Goldszmidt, A., List, J. A., Metcalfe, R. D., Muir, I., Smith, V. K., and Wang, J. (2020). The value of time in the united states: Estimates from nationwide natural field experiments. Technical report, National Bureau of Economic Research.

Knittel, C. R. and Stango, V. (2003). Price ceilings as focal points for tacit collusion: Evidence from credit cards. *American Economic Review*, 93(5):1703–1729.

Li, Z. and Wang, G. (2025). Regulating powerful platforms: Evidence from commission fee caps. *Information Systems Research*, 36(1):126–140.

Stamatopoulos, I., Sanders, R. E., and Bray, R. (2025). Electronic shelf labels have not led to surge pricing in us grocery retail, despite regulator concerns. *Available at SSRN*.

Sullivan, M. (2025). Fee optimality in a multi-sided market. *Working Paper*.

Tarduno, M. (2025). Elasticities and tax incidence in urban ridesharing markets: Evidence from chicago. *Journal of Urban Economics*, 150:103809.

Tran, K. D., Madio, L., Rossi, M., and Tremblay, M. (2025). Cleanin'it up: Unshrouding hidden fees on a peer-to-peer platform. *CESifo Working Paper*.

Tripathi, S., Kyriakou, H., and Petropoulos, G. (2025). Assessing the impact of algorithmic quantity regulations on sharing platforms: Evidence from airbnb in paris. *CESifo Working Paper*.

Williams, K. R. (2022). The welfare effects of dynamic pricing: Evidence from airline markets. *Econometrica*, 90(2):831–858.

Xu, L., Wei, Y. and Zhu, Y. (2025). Customer voice on two-sided platforms: The effect of surge pricing on customer complaints. *Management Science*, 71(8):6729–6745.

Yan, C., Zhu, H., Korolko, N., and Woodard, D. (2020). Dynamic pricing and matching in ride-hailing platforms. *Naval Research Logistics (NRL)*, 67(8):705–724.

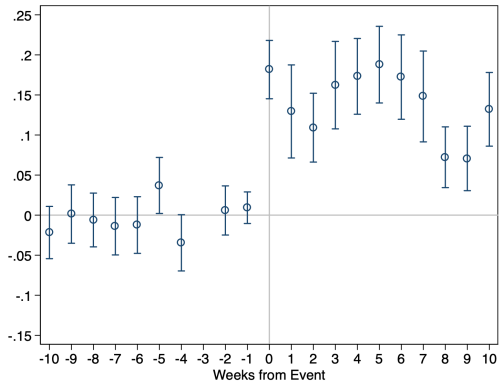
Tables and Figures

Table 1: The Main Summary Statistics (2021)

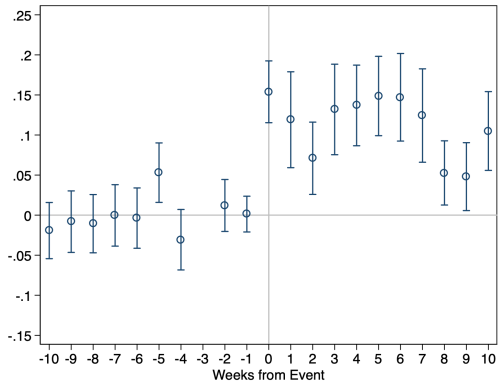
	Full Sample	NV Sample		All Other States		Restricted	
		Pre	Post	Pre	Post	Pre	Post
Total Price	20.80	17.95	21.24	20.61	20.99	20.53	21.54
	(12.86)	(8.06)	(10.63)	(12.69)	(13.09)	(12.58)	(13.19)
Duration (Min.)	15.58	15.02	14.34	15.41	15.75	15.46	16.01
	(10.24)	(8.26)	(7.32)	(10.15)	(10.40)	(10.40)	(10.79)
Distance (Mi.)	6.76	6.55	5.99	6.66	6.87	7.10	7.31
	(6.78)	(5.19)	(4.88)	(6.72)	(6.88)	(7.14)	(7.31)
Ride Type							
Black	0.01	0.00	0.00	0.01	0.01	0.01	0.01
	(0.10)	(0.01)	(0.01)	(0.10)	(0.10)	(0.11)	(0.10)
Comfort	0.01	0.02	0.01	0.01	0.01	0.01	0.01
	(0.11)	(0.15)	(0.11)	(0.11)	(0.11)	(0.11)	(0.10)
Lyft	0.32	0.40	0.43	0.31	0.33	0.34	0.35
	(0.47)	(0.49)	(0.49)	(0.46)	(0.47)	(0.47)	(0.48)
Lyft XL	0.01	0.07	0.03	0.01	0.01	0.01	0.01
	(0.11)	(0.26)	(0.18)	(0.10)	(0.11)	(0.08)	(0.10)
UberX	0.59	0.41	0.44	0.60	0.58	0.58	0.57
	(0.49)	(0.49)	(0.50)	(0.49)	(0.49)	(0.49)	(0.49)
UberXL	0.03	0.07	0.05	0.03	0.03	0.02	0.02
	(0.17)	(0.26)	(0.22)	(0.17)	(0.18)	(0.13)	(0.15)
Pink	0.03	0.03	0.03	0.03	0.03	0.03	0.03
	(0.16)	(0.16)	(0.16)	(0.17)	(0.16)	(0.18)	(0.17)
Observations	1,283,241	10,517	14,726	574,858	683,140	161,397	198,880

Notes: Trip level summary statistics from 2021. The first column includes the full 2021 sample. Columns 2-3 include observations in Nevada before and after the policy change. Columns 4-5 include all other states before and after the policy change. The last two columns include a restricted set of controls state (CA, CT, IL, MD, MI, and NJ).

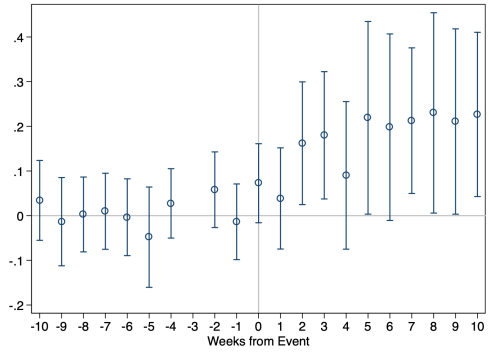
Figure 1: Event Studies on the Effects of Dynamic Pricing



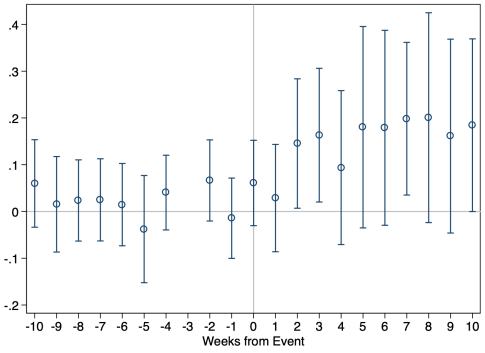
(a) Effects on prices, all controls



(b) Effects on prices, restricted controls



(c) Effect on rides, full controls



(d) Effect on rides, restricted controls

Notes: The top panels include event studies for the effect of dynamic pricing on log prices estimated using OLS. The unit of observation is a completed ride. The bottom two panels include event studies for the effect of dynamic pricing on the the number of rides at the zip-day level and are estimated using Poisson regression. Coefficients represent week-specific estimates. Panels (a) and (c) includes observations from all US States. Panels (b) and (d) include observations from Nevada and the restricted set of control states (CA, CT, IL, MD, MI, and NJ). All condition on zip code and day fixed effects. Standard errors are clustered at the zip code level.

Table 2: The Effect on Prices

	Full Sample	Restricted Controls		Full Sample	Restricted Controls	
	Ln(Price)	Ln(Price)	Ln(Price)	Price	Price	Price
NV × Post	0.144***	0.112***	0.173***	3.087***	2.467***	3.974***
	(0.015)	(0.015)	(0.020)	(0.342)	(0.348)	(0.468)
Duration (Min.)			0.017***			0.317***
			(0.001)			(0.015)
Distance (Mi.)			0.034***			0.998***
			(0.001)			(0.019)
Observations	1,285,393	385,272	385,272	1,285,393	385,272	385,272
Zip Code FE	x	x	x	x	x	x
Day FE	x	x	x	x	x	x
Ride Type FE			x			x

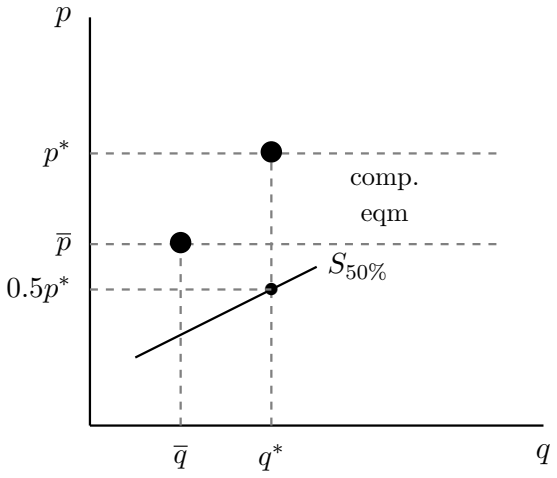
Notes: Difference-in-Differences estimates of the effect of dynamic pricing on the natural log of price (Columns 1-3) and on prices (Columns 4-6). Each observation is a completed rideshare trip. The sample includes the 21 week window around the NV rideshare policy change. Columns 1 and 4 include observations from all US States and D.C. as controls. Columns 2-3 and 5-6 use observations from CA, CT, IL, MD, MI, and NJ as the control group. All regressions include zip code and date fixed effects. Columns 3 and 6 condition on the duration of the trip, measured in minutes; the distance of the trip, measured in miles; and the ride type (7 distinct ride types displayed in Table 1). Standard errors are clustered at the zip code level.

Table 3: The Effect on Quantities

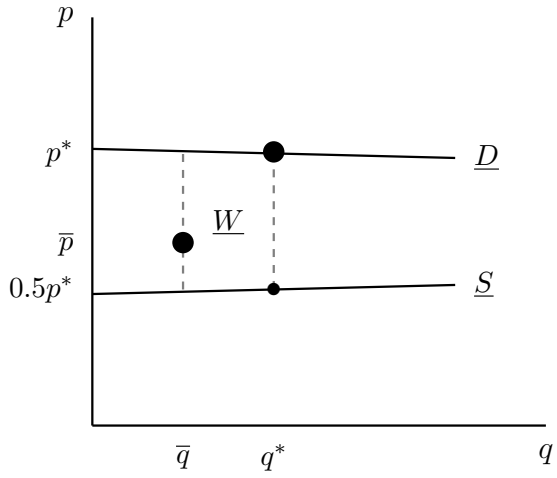
	Full Sample	Restricted Controls	Full Sample	Restricted Controls
Panel A: Including Zeros				
	Ln(Rides)	Ln(Rides)	Rides	Rides
NV × Post	0.163**	0.128*	0.306*	0.267
	(0.074)	(0.075)	(0.165)	(0.165)
Mean	0.68	0.86	0.58	0.76
Observations	1,891,302	446,586	2,208,822	505,239
Panel B: Excluding Zeros				
	Ln(Rides)	Ln(Rides)	Rides	Rides
NV × Post	0.059**	0.051*	0.704*	0.651
	(0.028)	(0.029)	(0.417)	(0.418)
Mean	2.59	2.52	2.59	2.52
Observations	496,679	153,057	496,679	153,057
Zip Code FE	x	x	x	x
Day FE	x	x	x	x

Notes: Difference-in-Differences estimates of the effect of dynamic pricing on the number of rides in percentages (Columns 1-2) and levels (Columns 3-4). Observations are at the zip code by day level. The outcome is the number of completed trips in the zip code by day. Columns 1-2 of Panel A are estimated using Poisson QMLE and all other regressions are estimated by OLS. The sample includes the 21 week window around the NV rideshare policy change. Columns 1 and 3 include observations from all US States and D.C. as controls. Columns 2 and 4 use observations from CA, CT, IL, MD, MI, and NJ as the control group. Panel A includes one observation for every zip code by date combination. That is, we rectangularize the data set to a balanced panel of zip code by day observations, including observations zip code days with zero rides. Panel B includes only zip code by day observations with a positive number of rides. All regressions include zip code and date fixed effects. Standard errors are clustered at the zip code level.

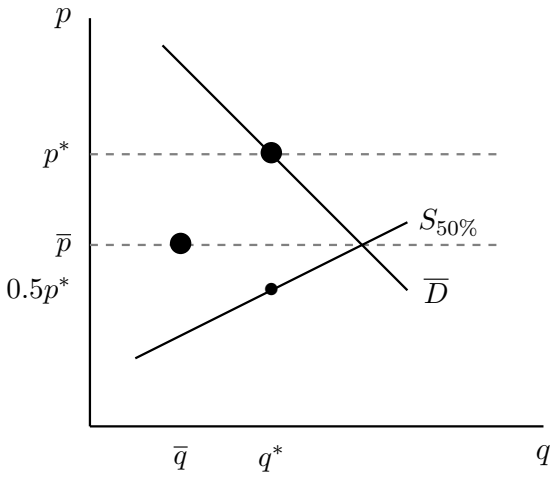
Figure 2: The Case Where Eliminating the Price Cap Increases Welfare



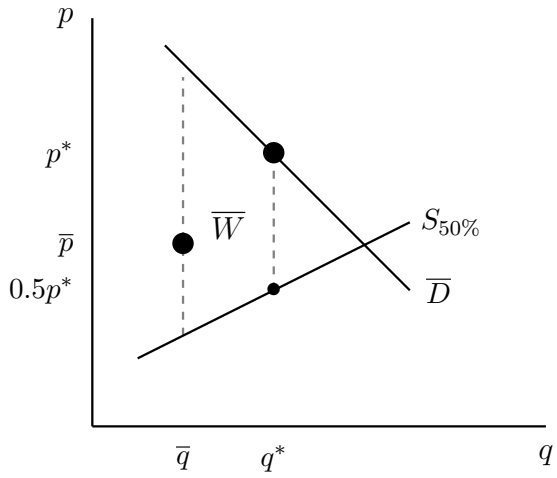
(a) Observed Equilibria



(b) Minimum Increase in Welfare

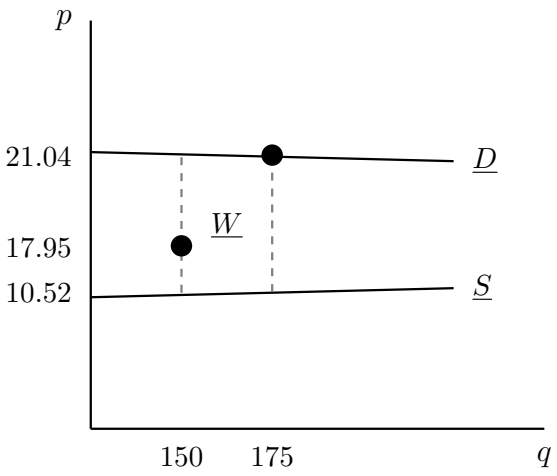


(c) Steepest Supply and Demand

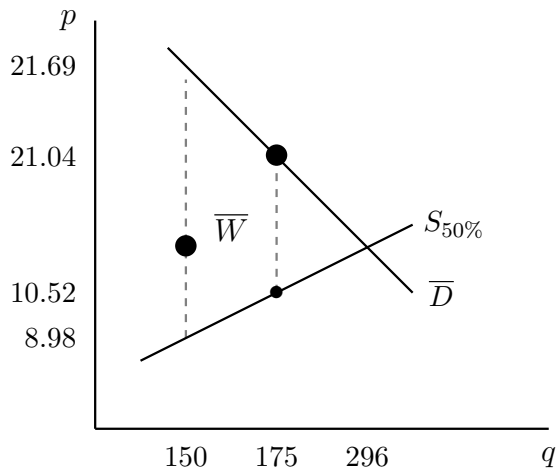


(d) Maximum Increase in Welfare

Figure 3: Applying the Levels Estimates to the Model



(a) Minimum Increase in Welfare



(b) Maximum Increase in Welfare

A Appendix of Proofs

Proof of Lemma 1: Start by considering the second case of the lemma, denoted with the subscript L, where $q^s > q^d$ so that $q_s = \frac{\overline{p}-c-f}{d} > q^d$ allows the platform to optimize over f and we therefore have that the platform maximizes Equation (3) with respect to f and subject to $q(f) = \frac{\overline{p}-c-f}{d}$. In this case we get $\overline{f}_L = \frac{\overline{p}-c}{2}$, $\overline{q}_L = q^s = \frac{\overline{p}-c}{2d}$, $\overline{p} - \overline{f}_L = \frac{\overline{p}+c}{2}$, and $q^d_L = \frac{a-\overline{p}}{b}$ which is less than $q^s = \overline{q}_L$ if and only if

$$\overline{p} < \frac{2ad + bc}{b + 2d} := \tilde{p}.$$

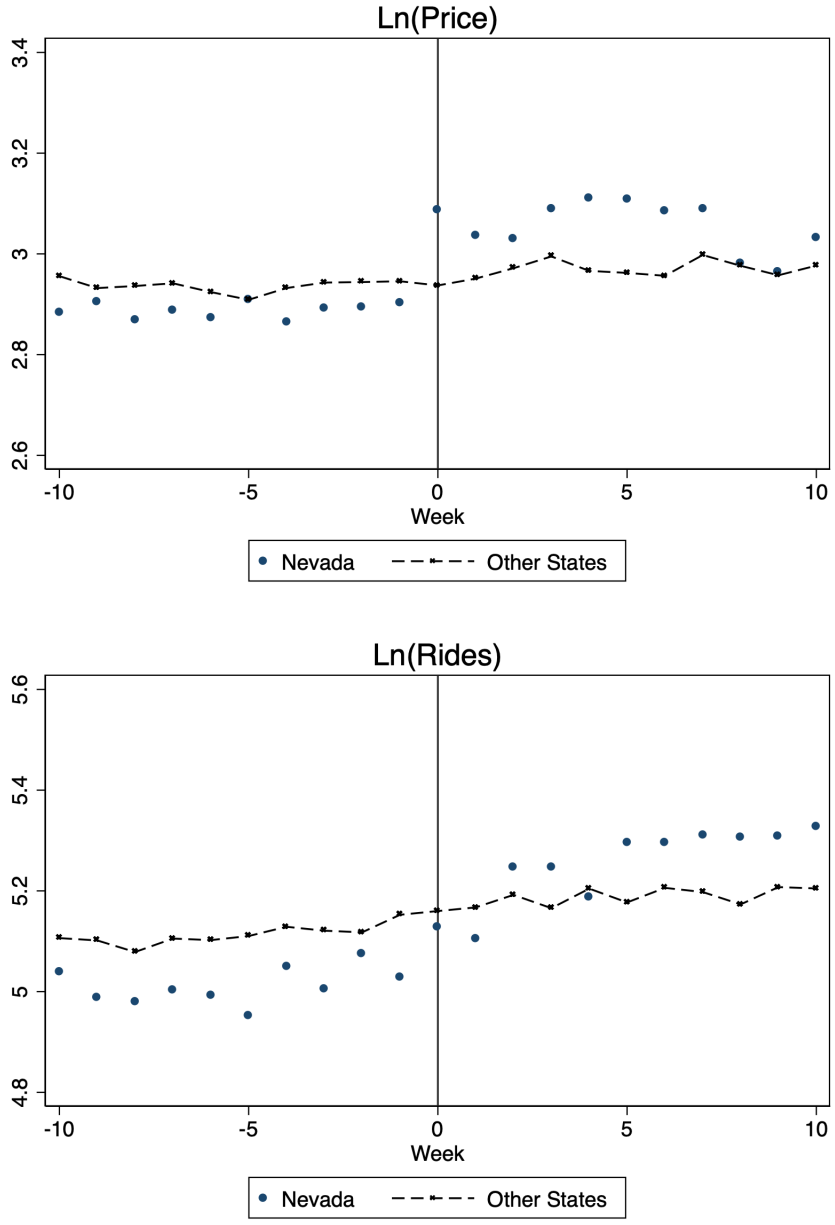
Note that it is straightforward to see that $\tilde{p} \in (p_c, p^*)$.

Otherwise, if $\tilde{p} \in (p_c, p^*)$, then we have that $\overline{q}_H = q^s = q^d = \frac{a-\overline{p}}{d}$ so that $q_s = \frac{\overline{p}-c-f}{d} = q^d\overline{q}_H$ implies that $\overline{f}_H = \overline{p} - c - \frac{d}{b}(a - \overline{p})$. □

Proof of Proposition 1: Comparing quantities for higher levels of p we have that $q^* < \overline{q}_H$ for $\overline{p} \in (\tilde{p}, p^*)$. Instead, we see that $q^* > \overline{q}_L$ if and only if $(a-\overline{p})d > (\overline{p}-c)b$ which simplifies to $\overline{p} < \frac{ad+bc}{b+d} = p_c$. □

B Additional Tables and Figures

Figure B.1: Prices and Rides, Nevada vs. Other States



Notes: The two panels display the natural log of the number of rides and the natural log of the mean price in Nevada and other states at the weekly level.

Table B.1: Summary Statistics (2022-2023)

	Full Sample	NV Sample		All Other States		Restricted	
		Pre	Post	Pre	Post	Pre	Post
Total Price	21.15	20.69	19.75	21.33	21.03	21.07	21.16
	(14.39)	(10.70)	(9.81)	(14.51)	(14.47)	(14.53)	(14.66)
Duration (Min.)	15.88	14.39	14.04	15.95	15.90	16.12	16.20
	(10.79)	(7.60)	(7.33)	(10.90)	(10.83)	(11.39)	(11.46)
Distance (Mi.)	6.96	5.62	5.40	6.96	7.03	7.26	7.32
	(7.15)	(4.61)	(4.49)	(7.17)	(7.25)	(7.54)	(7.63)
Ride Type							
Black	0.01	0.00	0.00	0.01	0.01	0.01	0.01
	(0.10)	(0.00)	(0.00)	(0.11)	(0.10)	(0.12)	(0.11)
Comfort	0.02	0.02	0.03	0.02	0.02	0.02	0.02
	(0.15)	(0.14)	(0.16)	(0.15)	(0.14)	(0.14)	(0.14)
Lyft	0.30	0.28	0.27	0.30	0.31	0.31	0.31
	(0.46)	(0.45)	(0.45)	(0.46)	(0.46)	(0.46)	(0.46)
Lyft XL	0.01	0.01	0.02	0.01	0.01	0.01	0.01
	(0.09)	(0.12)	(0.14)	(0.09)	(0.09)	(0.08)	(0.09)
UberX	0.62	0.63	0.62	0.62	0.62	0.62	0.62
	(0.49)	(0.48)	(0.48)	(0.49)	(0.49)	(0.49)	(0.49)
UberXL	0.03	0.05	0.05	0.03	0.03	0.02	0.02
	(0.17)	(0.22)	(0.22)	(0.17)	(0.17)	(0.14)	(0.15)
Pink	0.01	0.00	0.00	0.01	0.01	0.01	0.01
	(0.08)	(0.07)	(0.06)	(0.09)	(0.08)	(0.09)	(0.08)
Observations	3,402,649	44,874	48,571	1,600,922	1,708,282	472,262	513,228

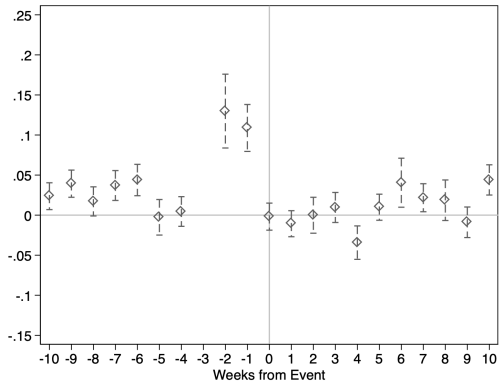
Notes: Trip level summary statistics from 2022-2023. The first column includes the full 2022-2023 sample. Columns 2-3 include observations in Nevada before and after the the date of the 2021 policy change (May 28th). Columns 4-5 include all other states before and after the date of the 2021 policy change. The last two columns include a restricted set of controls state (CA, CT, IL, MD, MI, and NJ).

Table B.2: Effects of Dynamic Pricing on Trip Duration and Distance

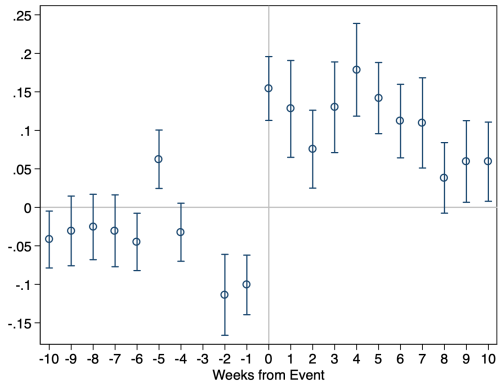
	Full Sample	Restricted Controls	Full Sample	Restricted Controls
	Duration (Min.)	Duration (Min.)	Distance (Mi.)	Distance (Mi.)
NV × Post	-0.783***	-1.038***	-0.429***	-0.491***
	(0.154)	(0.163)	(0.123)	(0.128)
Observations	1,285,393	385,272	1,285,393	385,272
Zip Code FE	x	x	x	x
Day FE	x	x	x	x

Notes: Difference-in-Differences estimates of the effect of dynamic pricing on trip duration, measured in minutes (Columns 1-2), and trip distance, measured in miles (Columns 3-4). Each observation is a completed trip. The sample includes the 21 week window around the NV rideshare policy change. Columns 1 and 3 include observations from all US States and D.C. as controls. Columns 2 and 4 use observations from CA, CT, IL, MD, MI, and NJ as the control group. All regressions include zip code and date fixed effects. Standard errors are clustered at the zip code level.

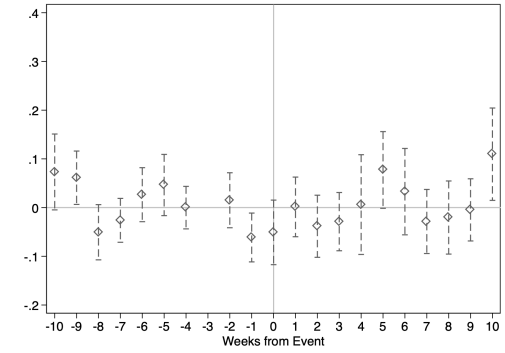
Figure B.2: Event Studies from 2022-2023 and DDD



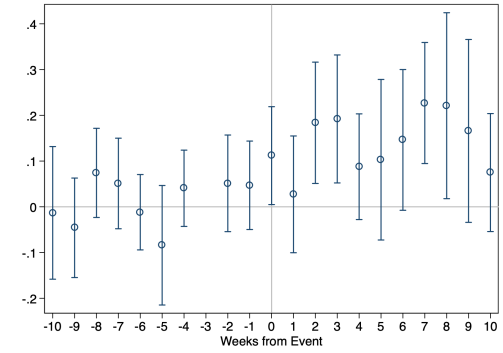
(a) 2022-2023 Price ES, restricted controls



(b) DDD on prices, restricted controls



(c) 2022-2023 Rides ES, restricted controls



(d) DDD on rides, restricted controls

Notes: The top two panels include event studies for ln(prices). Panel (a) is an event study centered around the same calendar date of the 2021 treatment (May 28th) for 2022-2023 when no policy change took place. Panel (b) includes a triple difference event study on prices, describing the difference between the 2021 DD estimator and the 2022-2023 DD estimator. The unit of observation is a completed ride. The bottom two panels include event studies for rides. Panel (c) includes the event study around May 28th in 2022-2023, and Panel (d) includes the triple difference estimator. The observations are at the zip-day level and are estimated using Poisson regression. Coefficients represent week-specific estimates. All panels include observations from Nevada and the restricted set of control states (CA, CT, IL, MD, MI, and NJ). All condition on zip code and day fixed effects. Standard errors are clustered at the zip code level.

Table B.3: DDD Effects on Prices

	Full Sample	Restricted Controls		Full Sample	Restricted Controls	
	Ln(Price)	Ln(Price)	Ln(Price)	Price	Price	Price
NV × Post × 2021	0.137***	0.126***	0.175***	3.038***	2.840***	4.108***
	(0.016)	(0.016)	(0.021)	(0.366)	(0.377)	(0.504)
NV × Post	0.010***	-0.011***	0.000	0.061	-0.383***	-0.095
	(0.004)	(0.004)	(0.003)	(0.090)	(0.099)	(0.080)
Duration (Min.)			0.018***			0.324***
			(0.000)			(0.012)
Distance (Mi.)			0.035***			1.106***
			(0.000)			(0.013)
Observations	4,250,882	1,325,454	1,325,454	4,250,882	1,325,454	1,325,454
Zip Code-Year FE	x	x	x	x	x	x
Day FE	x	x	x	x	x	x
Ride Type FE			x			x

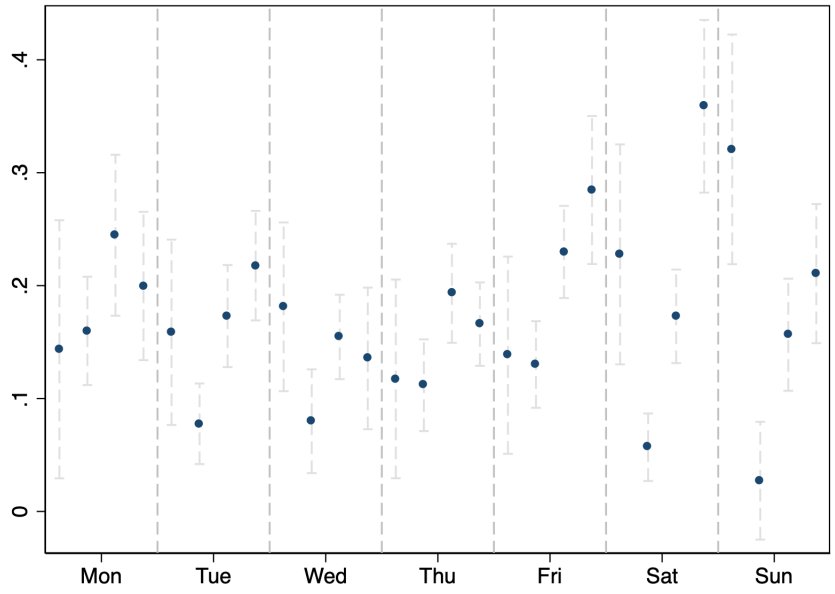
Notes: Triple difference estimates of the effect of dynamic pricing on the natural log of price (Columns 1-3) and on prices (Columns 4-6). Each observation is a completed rideshare trip. The sample includes the 21 week window around the NV rideshare policy change in 2021, and the same dates in the 2022 and 2023 calendar years. The last two weeks prior to the treatment date are excluded for every year. Columns 1 and 4 include observations from all US States and D.C. as controls. Columns 2-3 and 5-6 use observations from CA, CT, IL, MD, MI, and NJ as the control group. All regressions include zip code and date fixed effects. Columns 3 and 6 condition on the duration of the trip, measured in minutes; the distance of the trip, measured in miles; and the ride type (7 distinct ride types displayed in Table 1). Standard errors are clustered at the zip code level.

Table B.4: DDD Effects on Quantities

	Full Sample	Restricted Controls	Full Sample	Restricted Controls
Panel A: Including Zeros				
	Rides	Rides	Rides	Rides
NV × Post × 2021	0.158**	0.142**	0.356**	0.328*
	(0.066)	(0.067)	(0.170)	(0.171)
NV × Post	0.009	-0.012	-0.041	-0.053
	(0.022)	(0.022)	(0.053)	(0.053)
Mean	0.907	1.128	0.710	0.967
Observations	4,694,767	1,175,587	5,995,374	1,371,363
Panel B: Excluding Zeros				
	Ln(Rides)	Ln(Rides)	Rides	Rides
NV × Post × 2021	0.057*	0.056*	0.741*	0.738*
	(0.032)	(0.033)	(0.424)	(0.425)
NV × Post	0.002	-0.004	-0.015	-0.069
	(0.015)	(0.016)	(0.117)	(0.118)
Observations	1,496,771	460,137	1,496,771	460,137
mean	2.840	2.880	2.840	2.880
Zip Code-Year FE	x	x	x	x
Day FE	x	x	x	x

Notes: Triple Difference estimates of the effect of dynamic pricing on the number of rides in percentages (Columns 1-2) and levels (Columns 3-4). Observations are at the zip code by day level. The outcome is the number of completed trips in the zip code by day. Columns 1-2 of Panel A are estimated using Poisson QMLE and all other regressions are estimated by OLS. The sample includes the 21 week window around the NV rideshare policy change in 2021, and the same calendar dates for years 2022 and 2023. The last two weeks prior to the treatment date are excluded for every year. Columns 1 and 3 include observations from all US States and D.C. as controls. Columns 2 and 4 use observations from CA, CT, IL, MD, MI, and NJ as the control group. Panel A includes one observation for every zip code by date combination. That is, we rectangularize the data set to a balanced panel of zip code by day observations, including observations zip code days with zero rides. Panel B includes only zip code by day observations with a positive number of rides. All regressions include zip code by year fixed effects and date fixed effects. Standard errors are clustered at the zip code level.

Figure B.3: Price Effects by Time and Day of Week



Notes: Each dot represents a DD estimate on $ln(price)$ specific to a 6 hour block, e.g., Monday from midnight to 6 a.m. is the first estimate. Standard errors are clustered at the zip code level and 95% CIs included.

Table B.5: Heterogeneous Effects on Prices

	Full Sample	Restricted Controls		Full Sample	Restricted Controls	
	Ln(Price)	Ln(Price)	Ln(Price)	Price	Price	Price
Panel A: After 5pm						
NV × Post	0.198***	0.160***	0.230***	4.334***	3.580***	5.295***
	(0.017)	(0.018)	(0.025)	(0.351)	(0.365)	(0.549)
Observations	456,883	134,799	134,799	456,883	134,799	134,799
Panel B: Weekends						
NV × Post	0.173***	0.132***	0.206***	3.903***	3.080***	4.938***
	(0.015)	(0.015)	(0.022)	(0.378)	(0.385)	(0.538)
Observations	435,948	130,351	130,351	435,948	130,351	130,351
Panel C: Uber						
NV × Post	0.141***	0.112***	0.166***	2.890***	2.321***	3.669***
	(0.018)	(0.018)	(0.019)	(0.423)	(0.426)	(0.463)
Observations	823,430	235,853	235,853	823,430	235,853	235,853
Panel D: Lyft						
NV × Post	0.153***	0.116***	0.180***	3.432***	2.688***	4.268***
	(0.019)	(0.019)	(0.023)	(0.397)	(0.413)	(0.534)
Observations	460,727	149,179	149,179	460,727	149,179	149,179
Zip Code-Year FE	x	x	x	x	x	x
Day FE	x	x	x	x	x	x
Ride Type FE			x			x

Notes: Heterogneity in price effects. Each observation is a completed rideshare trip. The sample includes the 21 week window around the NV rideshare policy change in 2021, and the same dates in the 2022 and 2023 calendar years. The last two weeks prior to the treatment date are excluded for every year. Columns 1 and 4 include observations from all US States and D.C. as controls. Columns 2-3 and 5-6 use observations from CA, CT, IL, MD, MI, and NJ as the control group. All regressions include zip code and date fixed effects. Columns 3 and 6 condition on the duration of the trip, measured in minutes; the distance of the trip, measured in miles; and the ride type (7 distinct ride types displayed in Table 1). Standard errors are clustered at the zip code level.