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Notching for Free: Do Cyclists Reveal the Value of Time?

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RESOURCES FOR THE FUTURE

Notching for Free: Do Cyclists Reveal the Value of Time?*

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Abstract

We explore a nonlinear, "notched" pricing structure in a novel market—urban bike-sharing—to identify how inframarginal price changes affect consumer behavior. By observing cyclists extending their trips to avoid a discontinuous price increase, we are able to estimate a time-for-money trade-off directly for both commuting and recreational trips. Although our estimation strategy reveals an estimate of consumers' opportunity cost of time under neoclassical assumptions, we find that a 400% price increase does not affect behavior in a meaningful way. This result suggests consumers respond more strongly to quantity signals, which has direct implications for nonlinear pricing in other settings.

JEL codes: Q50, D12, R20, L91

Keywords: notches, bunching, opportunity cost of time, consumption splitting, bike-sharing, nonlinear pricing

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Different constraints are decisive for different situations, but the most fundamental constraint is limited time.

— Gary S. Becker, Nobel Lecture (Becker, 1993)

1 Introduction

Taxes, prices, and policies that create nonlinearities in consumers' choice sets have received increasing attention by economists in recent years. Facing large incentives, such as changes in income tax rates, consumers tend to bunch on the favorable side of tax schedules (Kleven and Waseem, 2013). In other cases consumers appear to be unaware of small incentives, such as jumps in the marginal price of electricity within a billing period (Ito, 2014). Understanding how consumers respond to nonlinear incentives, what that behavior reveals about the trade-offs they make, and how those trade-offs can inform economic thinking remain open questions.

In this paper, we uncover consumer responses to a price "notch" (i.e., a price that introduces a discontinuity in a consumer's budget set) in a novel market: cycling via an urban bikesharing program. By exploiting a discrete jump in the cost of a bicycle trip beyond 30 minutes in Denver's B-cycle program, we identify novel consumer bunching behavior in which a cyclist avoids a price discontinuity by going out of her way to check-in and check-out a bicycle at an intermediate station along her route. Splitting consumption in such a way reveals a consumer's willingness to trade off her time to avoid the price increase, thus illuminating the opportunity cost of her time. Recreational users spend more time avoiding the price notch than commuters, suggesting important heterogeneity in labor-leisure time trade-offs.

Methodologically, we use a two-stage matching design paired with parametric estimation for causal inference (e.g., Heckman et al. (1997); Smith and Todd (2005); Ferraro and Miranda (2017); Wichman and Ferraro (2017)). In doing so, we identify bunching in response to nonlinear prices relative to an observable counterfactual distribution, as opposed to estimating a counterfactual density or observing spatial discontinuities across comparison units. Specifically, we pre-process our data set using covariate matching to construct an observationally similar counterfactual for each trip that we identify as a "daisychain" trip (i.e., a trip that has been split to avoid the price discontinuity). We then apply parametric estimators to our balanced data set to control for unobserved heterogeneity that might further bias our estimates. Comparing the time difference from a daisychain trip to its corresponding comparison trip provides an estimate of the time cost consumers are willing to incur to avoid the price notch, which provides an upper bound on their value of time.

Value of time (VOT) estimates are used broadly to justify public projects and value nonmarket goods. For example, the VOT has been used to: (i) estimate the value of a statistical life (Ashenfelter and Greenstone, 2004), (ii) calculate search costs for home production (Aguiar and Hurst, 2007), and (iii) value lost recreational benefits within the Deepwater Horizon oil spill settlement (Deepwater Horizon Natural Resource Damage Assessment Trustees, 2016). Our estimates of VOT, derived from inframarginal price changes, are generally smaller than those of existing studies and those recommended for benefit-cost analyses provided by federal agencies. Notably, our paper is the first to our knowledge to identify within-sample user type and temporal heterogeneity that reveals larger VOTs for commuters and lower values for recreational users. By virtue of design, we are able to isolate consumer behavior from vehicle choice better than previous work (e.g., each trip observation in our sample is taken on a bicycle with exactly the same specifications).

We also provide evidence that the discontinuity itself drives consumer behavior, rather than the price increase. We show that consumers do not alter behavior in a meaningful way in response to a 400% increase in the price of a trip beyond 30 minutes, from $1 to $5. This result suggests that pricing policies designed to target easy-to-understand quantities (e.g., miles driven per hour, the length of a shower, megabytes of cellular data used to upload a video, and so forth) as focal points may induce behavior that accords with existing economic models, and can improve the effectiveness and predictive qualities of taxes, prices, and policies that create nonlinear budget sets.

More practically, our results provide novel evidence on price responsiveness, and the corresponding mechanism driving such responsiveness, of consumers in the burgeoning "sharing" economy. As such, our paper complements recent work on demand sensitivity in peer-to-peer transit markets (Cohen et al., 2016; Cramer and Krueger, 2016). Our estimates help to understand implicit trade-offs in revealed preference indicators for valuing public transportation projects that need to incorporate differences in motor-vehicle and bicycle traffic, as the share of bicycle commuters has increased by 90% between 1990 and 2012 in the 70 largest cities in the U.S.[1] Further, our results provide important takeaways for the growing bikesharing industry. First, we provide strong evidence that bicyclists, like other consumers, respond to their pricing structure. Utilizing prices to manage scarcity (e.g., dynamic prices) could improve system efficiency, and including price sensitivity in demand projections can improve the financial solvency of urban transit programs. Second, public funding for bikesharing programs often relies on a coarse measurement of demand: trips. Our present paper explores nuances in how current reporting practices may overstate the usage of bikesharing

[1] Statistics obtained from the U.S. Census American Community Survey, summarized by the League of American Bicyclists, `http://www.bikeleague.org/commutingdata`. (Last Accessed: October 19, 2016).

programs by not incorporating strategic consumer behavior.

1.1 Background on bikesharing

Bikesharing is an urban transit system in which members can use bicycles from stations in public places and return them to other stations when their ride is complete. Modern systems require members to purchase a membership for a specified time (e.g., one day, three days, one month, and one year). Members use a key to unlock a bicycle at any station, and they can return it to an empty dock at a station near their end destination. Generally, the marginal cost of a trip completed within a given amount of time (typically 30 minutes) is zero, while trips that last longer than that are priced according to an increasing tiered schedule (see Figure 1 for an example of a pricing structure, and Table 1 for the price schedule analyzed in this analysis).

Bikesharing systems are growing rapidly in North America and providing new transportation opportunities for residents and visitors in major cities (Martin and Shaheen, 2014). There are more than 600 bikesharing systems with 600,000 bikes internationally. The first U.S. programs started in 2010 in Denver, Washington, Minneapolis, and Des Moines. Several cities have adopted systems in years since, with ridership increasing each year. Users in the largest program in the U.S., New York's CitiBike, logged over 10 million rides in 2015.[2] Worldwide, the industry is projected to generate revenues of four to six billion U.S. dollars by 2020.[3]

Bikesharing systems are intended to encourage short-to-medium distance rides. Ideally, these systems would complement existing public transit, providing an alternative to walking to and from a major transit center, or linking two routes that do not overlap (Pucher and Buehler, 2005). Shaheen (2012) notes several potential local benefits of bikesharing: increased mobility, consumer transportation cost savings, reduced transportation infrastructure costs, reduced traffic congestion (Hamilton and Wichman, 2017), reduced fuel use, increased use of public transit (Martin and Shaheen, 2014), public health improvements, and greater environmental awareness.

A review of the literature returns very few papers that study outcomes of bikesharing programs. Davis et al. (2015) analyze spatial and temporal variation in ride patterns using data from Bay Area Bikeshare. Martin and Shaheen (2014) evaluate how bikeshare programs integrate into existing transit systems, and they use surveys in Washington, DC, and Melbourne to assess modal shift to bikesharing. Fishman et al. (2015) research the institutional

[2] CitiBike trip data available from `https://www.citibikenyc.com/system-data`.

[3] We thank Roland Berger Strategy Consultants for providing us with their 2015 study titled "Bike Sharing 4.0".

and demographic factors affecting bikeshare membership. Hamilton and Wichman (2017) estimate the causal impact of bikeshare stations on local traffic congestion in Washington, DC. Shaheen (2012) and Shaheen et al. (2014) provide more comprehensive overviews of the institutional design and usage trends in North America. We are aware of no studies that consider price responsiveness of cyclists within these programs.

1.2 Transportation and the opportunity cost of time

The value of time (VOT) has been a topic of interest to economists for several decades, with applications such as valuing recreational amenities and evaluating public policies related to the productive benefits of transportation. The conceptual foundation of the VOT was formed by seminal papers from Becker (1965) and DeSerpa (1971), who formalized how consumers allocate resources when time is scarce. Beesley (1965) complemented these theories in an empirical paper within a travel time context.

Many of the early papers used travel cost to value recreational benefits, imputed by using the time spent to travel to a park, lake, or other recreational site. Stated preference studies for recreation demand literature include Bockstael et al. (1987), Feather and Shaw (1999), and Lew and Larson (2005). Transportation research has myriad useful applications for the VOT. A number of papers use toll road choices to infer travel cost (see, e.g., Bhat (1995), Brownstone and Small (2005), Small et al. (2005), Steimetz and Brownstone (2005), and Fosgerau et al. (2010)). Fezzi et al. (2014) analyze behavior on a series of routes to recreational sites with different levels of tolls. Deacon and Sonstelie (1985) exploit a natural experiment induced by wait times and gasoline prices, where drivers revealed their VOT by trading off time waiting in a queue for cheaper gas. Wolff (2014) uses speed and gasoline data to estimate whether drivers reduce their driving speed when gas prices are high to conserve gasoline.

Typical estimates of the VOT have a wide range of values, usually less than the wage rate. Cesario (1976) estimates a value as low as 33% of the wage rate. Wolff (2014) finds a rate closer to half the prevailing wage using a strategy that identifies revealed preference measures on an intensive margin. Fezzi et al. (2014) estimate a VOT at 70-80% of the individual-specific wage rate, rather than inferring the wage from annual median household income, as suggested by U.S. Department of Transportation guidelines (U.S. Department of Transportation, 2014). And, Deacon and Sonstelie (1985) and Small et al. (2005) produce values of 80-90%. Ashenfelter and Greenstone (2004) use the full wage rate to estimate how the public trades off wealth for mortality risks associated with higher traffic speed limits.

Taken at face value, our paper makes several contributions to the VOT literature. We provide revealed preference estimates using a sample of bicyclists by exploiting the fact that an alternate route can be taken to avoid an increase in price. Second, we have a classification of users (registered and casual) that allows us to see differences in the VOT within groups based on differences in trip purpose. Third, we directly assess temporal heterogeneity to explore the role of time-varying time constraints. Lastly, our design isolates both the mechanism by which consumers reveal their value of time and the vehicle attributes that may interact with consumer preferences. More broadly, we contend that within our setting, and other information-constrained settings, consumers may not be fully attentive to the price schedule, and rather than placing undue confidence in our VOT estimates, we rest the importance of this paper on identifying novel strategic consumer behavior that suggests an alternative mechanism through which consumers react to complicated pricing schemes.

2 Notches, avoidance behavior, and the value of time

Notches were first defined formally in Blinder and Rosen (1985) as a discontinuity in the budget constraint, which results in a finite sum of benefits being lost when the notch threshold is crossed. There are numerous recent studies that analyze whether consumers bunch at notches in tax systems; many are summarized by Slemrod (2013). Kleven and Waseem (2013) analyze bunching behavior at income tax schedule notches, where there are discrete changes in tax liability at different levels of income in Pakistan. Bunching at threshold levels in taxation has also been analyzed in the context of home and property sales (Kopczuk and Munroe, 2015; Best and Kleven, 2013), business earnings and size (Hsieh and Olken, 2014; Onji, 2009), savers taxes (Ramnath, 2013), and vehicle fuel standard taxes (Sallee and Slemrod, 2012).

Several papers observe temporal bunching, where consumption occurs in a more favorable tax year. This temporal shift is documented for changes in the U.S. capital gains tax (Burman and Randolph, 1994) and estate tax (Kopczuk and Slemrod, 2003). Temporal bunching is also observed in the absence of a changing tax structure. LaLumia et al. (2015) find an increase in U.S. births near the end of December, and a corresponding decrease in early January, attributed to the earned income tax credit, which increases with the number of children within a family. Ito and Sallee (2014) explore attribute-based regulations in the context of fuel economy, where the policy standard is a nonlinear function of vehicle weight. Further, state and local taxes have been found to induce spatial bunching near borders. Individuals near state borders, where there is a significant change in the sales tax rate, will buy on the favorable side of the border. These cases are considered for general sales taxes (Agrawal, 2013) and cigarette taxes (Lovenheim, 2008).

The taxation literature also provides insight on kinks (i.e., nonlinearities in budget sets), which can induce bunching behavior. Responses to kinked budget sets are considered in the cases of income taxes (Saez, 2010; Bastani and Selin, 2014), wealth taxes (Seim, 2013), and business taxes (Carrillo et al., 2012; Chetty et al., 2011). A corresponding literature focusing on kinked price schedules analyzes demand for electricity and water (Ito, 2014; Wichman, 2014; Szabó, 2015). For consumer goods and services in these contexts, where inattention and imperfect information plays an important role (Sallee, 2014), bunching does not appear to be prevalent.

Responses to notches outside of the taxation literature, however, are less common. The only paper that specifically analyzes behavior in a non-tax setting is Mbiti and Weil (2013), who analyze bunching in e-money withdrawal amounts, where a nominal fee is charged based on the bracket in which the withdrawal amount falls.

One relevant strategic response to notched pricing structures is "splitting" consumption. Onji (2009), for example, analyzes the effect of a tax reform in Japan that allowed firms with annual sales less than 500 million yen to opt out of the VAT system into a simpler, more favorable tax program. Onji observes an increase in the density of the distribution under the threshold, which he attributes anecdotally to companies splitting into multiple smaller companies that each have revenues below the 500 million yen threshold. This example will return results different from traditional bunching examples because individuals well beyond the notch can cheat under the notch, and the data generated by this form of cheating will not necessarily bunch right below the notch (e.g., a 650 million yen company, well above the notch, might split into two 325 yen companies, now well below the notch). Bunching and holes may be less prevalent near the threshold, but the density of these distributions should increase on the more favorable side of the notch and decrease on the less favorable side. We identify a parallel concept in individual behavior, where consumers split bicycle trips into multiple shorter trips to avoid a discontinuous jump in price.

There are several primary takeaways from this literature for the present analysis. First, nonlinear incentives induce relatively predictable behavior in theory. In practice, however, strategic manipulation of consumption or income in response to nonlinear thresholds is observed when reporting is manipulable and the payoffs are large (e.g., self-reporting income taxes). When individuals have less precise control over their behavior, such as with electricity consumption, bunching is less likely to occur. As a result, there is little empirical evidence of bunching around nonlinear incentives for the consumption of consumer goods and services.

2.1 A simple model of consumption splitting

To highlight the role of nonlinear incentives for consumption splitting and its usefulness in measuring consumers' VOT, we present a stylistic model below. Consider a bikeshare user i who chooses a route between station θ and θ', with an expected trip duration of $E[d(\theta, \theta')]$. The consumer faces the following notched price schedule,

$$p(d) = \begin{cases} 0 & \text{if } d(\theta, \theta') < k \\ p & \text{if } d(\theta, \theta') \geq k \end{cases} \tag{1}$$

where k is a notch in the elapsed duration of the trip, and $p(d)$ is the full cost of the trip (i.e., not marginal).

If the consumer anticipates that $E[d(\theta, \theta')]$ will exceed k, she has the option to split her trip at a marginal time cost of $A(\beta) > 0$ into two legs by stopping at a central location θ^S, where $E[d(\theta, \theta^S) + d(\theta^S, \theta')] - E[d(\theta, \theta')] = \beta$ such that $\beta \geq 0$ and $E[d(\theta, \theta^S)], E[d(\theta^S, \theta')] < k$. That is, the duration of the combination of both legs is weakly greater than that of the direct route and the duration of each leg is anticipated to be shorter than the notch, k. The consumer therefore faces a per-trip expenditure of $p(d)$ if she does not split her trip into multiple legs and $A(\beta)$ if she does. In the context of bikesharing, consumption splitting is colloquially referred to as 'daisychaining,' and we adopt that terminology.[4]

Several predictions fall out of the structure of this choice problem directly. First, a consumer will only choose to split consumption if $A(\beta) \leq p(d)$. Although intuitively obvious, this prediction implies that consumers will equate the marginal value of their time spent daisychaining with the expected marginal cost of a trip. This prediction, then, allows us to observe behavior within a trip in response to inframarginal price changes to estimate consumers' value of time directly.

Second, the likelihood of consumption splitting jumps at k, increases as p increases, and decreases as β increases. Intuitively, this is the result of the piecewise notch in trip expenditures and the change in relative costs of alternatives. This prediction serves as the foundation of our instrumental variables strategy discussed below, in which we instrument for the likelihood of daisychaining with indicators representative of the rate schedule.

Third, more precision in consumer expectations implies a greater degree of sorting around k. In other words, a consumer with a better sense of her travel time, and the cost of her alternatives, is more likely to locate in an optimal region of the price schedule. We incorporate

[4] The act of docking bicycles to keep trip length under the limit where overage prices are incurred is known as daisychaining or dock-surfing in the bikeshare community. See the use in blog posts at http://www.virtuousbicycle.com/BlogSpace/getting-ready-for-citibike-bike-share/ and https://brooklynspoke.com/2013/06/03/citi-bike-101-dock-surfing/

these predictions empirically by relying on consumer-specific heterogeneity present in our data. Specifically, we observe whether a trip is taken by a "registered" (i.e., annual) or "casual" (i.e., 3-day) member. Further, we allow the possibility that A varies over time to account for urgency (Bento et al., 2014) or temporal shifts in time constraints. As such, we examine how our parameter of interest, the empirical estimate of β, varies between weekdays and weekends, as well as different commuting times within the day.

As a graphical example, we present three alternative distributions of consumer behavior in response to a notch threshold k in Figure 2. The solid blue line represents a counterfactual in which the consumer has no ability to manipulate her trip time. The dashed red line represents classical bunching behavior in response to the nonlinear incentives in the spirit of Kleven and Waseem (2013). This behavior, indicative of consumer sorting on an intensive margin, could represent self-reported income above or below an income-tax threshold. In the bikeshare example, the only margins on which consumers could exert this behavior is by cycling faster along a given route, or to end a trip early and walk the remainder of the distance to the end destination. Finally, the dotted black line represents cumulative trip durations under a model of splitting behavior. As shown, the density moves smoothly through the discontinuity in the price schedule, and displays bunching on the *less* favorable side of the threshold because consumers are linking multiple shorter trips get from their starting destination to their end destination. In this way, daisychaining bikeshare trips can be viewed as sorting along an extensive margin.

3 Data

We use trip data provided by Denver B-cycle (henceforth, "B-cycle") to analyze the patterns of bikeshare users.[5] B-cycle began in 2010 and, as of Fall 2016, had approximately 90 stations with more than 700 bicycles throughout Denver, Colorado. For each trip taken, we observe the start and end destination and their geographic coordinates; the start and end time of the trip, providing its duration; a unique subscriber ID; and an indicator for whether the user is a registered (annual) member or casual (3-day) member. For each route defined by its start and end locations, we construct an estimate of time needed to bike the route and the distance of the optimal route using the Google Maps Directions API.

To identify what we define as daisychaining (i.e., consumption splitting in the context of bikesharing programs), we use B-cycle trip data from January 2013 through December 2016,

[5]Denver B-cycle provides publicly available trip level data at `https://denver.bcycle.com/company`. Station metadata can be found in the page source for the station map at `https://denver.bcycle.com/`. Denver B-cycle provided us with information on retired stations and other helpful information about the data via email.

inclusive.[6] We use the subscriber ID to isolate each member's trips, sorting from oldest to newest, and we flag pairs where the second trip began three minutes or less after the first trip ended. We are then able to combine each group of two or more consecutive trips into one multi-segment trip, and view the aggregate trip time, starting station, and ultimate ending station.[7] In the interest of finding trips where the user stopped at an intermediate station between their true start and end destination, we include other restrictions to eliminate observations that might confound our modeling approach: (i) we eliminate round trips, where a user's start and end destination are identical; (ii) we eliminate trips over 150 minutes from the data, because they are often caused by failure to dock the bike properly; (iii) we eliminate daisychain trips where one or more segments exceeded 65 minutes; and (iv) we remove daisychain trips if the two segments went in opposing directions.[8]

Further, we take advantage of changes in the pricing structure that affect incentives to daisychain. At the beginning of 2015, costs for the daily and annual passes increased (from \$8 to \$9 and \$80 to \$90, respectively), which should only affect virtual income and should not affect the distribution of trip time or the proportion of trips that were split.[9] In October of 2015, B-cycle increased the overage charges (see Table 1). Previously, the first overage charge was \$1, and each half hour afterwards was \$4. The new overage price fee is \$5 for the first overage charge, and \$5 for every 30 additional minutes. Within this analysis, we analyze "pre-Oct. 2015" observations and "post-Oct. 2015" observation separately. Our primary results are drawn from the pre-Oct. 2015 sample, although we analyze the effect of the 400% increase in the price of exceeding the free ride allotment on the distribution of trip time and frequency of trip splitting by comparison with the post-Oct. 2015 sample.

After imposing these restrictions, we identify roughly 20,000 daisychain trips taken by more than 2,100 registered and 6,600 casual users within the universe of trips between 2013 and 2016. 28 percent of all registered users had daisychained at least once, as well as 7 percent of casual users.

[6] 2013 was the first year that a unique subscriber ID was provided in the trip data.

[7] For our analysis, we only use two-segment daisychains, although we identified a small number of trips with three or more segments.

[8] We identify trips where the first segment and second segment point in opposing directions using the bearing (angle from North) of each trip. If the linear direction for the second segment is larger than 135 degrees from the linear direction from the first segment, we drop the entire trip from our sample.

[9] At the same time, B-cycle introduced an "Annual Plus Membership," which allowed 60 minutes of free ride time instead of 30. The price difference between this membership and the annual membership that only provides 30 minutes free per trip was \$10. Significant migration to the annual plus membership would change the distribution of annual member trip times and reduce split trips. Because of these differential incentives, we remove Annual Plus members from our analysis. In the calendar year of 2016, annual memberships were no longer available for purchase. Thus, we only observe registered users in our sample in 2016 if they had purchased an annual membership in 2015. Marginal trip costs change for all users simultaneously regardless of when the membership was purchased.

As shown in Table 2, mean trip time for trips in our full sample (n=930,694) in 2013–2016 was 14.52 minutes, with a standard deviation of 12.93 and a median of 10. For registered users (n=652,918), the mean is 10.35 with a standard deviation of 7.73 and median of 8. For casual users (n=277,653), the mean is 24.32 with standard deviation of 16.82 and a median of 20.

4 Empirical strategy

4.1 Evidence of consumption splitting

In Figure 3, we plot the distribution of trip time over all years and member types in our raw B-cycle trip data for both registered and casual users. As shown, registered users have a smaller mean and the majority of the mass of the distribution is below the 30-minute mark. Casual users have a higher mean, and a slightly more uniform distribution of trip time. The raw data show virtually no bunching near the 30-minute notch.

To determine whether there is strategic avoidance of the notch price at 30 minutes, we append individual trips that meet our criteria for a daisychain trip. In Figure 4, we plot the trip time distributions for each individual segment of a trip that was daisychained by user type. For both registered and casual users, we begin to see apparent sorting around the 30-minute notch. Users appear to end their first segment before the notch, but not immediately so, contrary to typical bunching in the presence of notches (Kleven and Waseem, 2013). This strategic behavior occurs because the second segment is also subject to a 30-minute notch and, hence, cyclists can link two shorter trips together to avoid the notch.

In Figure 5, we append both segments for each daisychain trip in our data and plot the resulting distribution. The result is a set of trips that displays more obvious sorting around the notch point. For casual users, the distribution is roughly symmetric, peaking between 35 and 40 minutes. There appears to be a small, but abrupt, discontinuity in the distribution at the 30-minute mark. The distribution for registered users is more irregular. There is substantially more mass below the 30-minute notch and a relatively smooth change across the notch point. Between 35 and 40 minutes, however, there is a dramatic drop in the distribution, which suggests that consumers may add multiple short trips together to bunch directly on the unfavorable side of the notch.

4.2 Confronting selection and simultaneity

The consumer's choice problem is simple: Do I choose to daisychain to avoid the price notch? Two straightforward identification problems arise in this context. First, if daisychain trips are not representative of all bikeshare trips, then we have a sample selection issue and any estimate of the time elasticity of prices will be inconsistent. Second, because the cost of a trip is a function of trip duration, the choice to daisychain is made concurrently with the trip duration. If there is some unobserved component of choice that affects both the extensive margin (whether to daisychain) and the intensive margin (duration of the trip), then any econometric estimate of consumers' value of time will embed simultaneity bias.

We approach each of these empirical concerns as follows. First, we adopt a two-step covariate matching algorithm to construct an observationally similar comparison sample prior to applying any parametric estimator. This strategy, by balancing "treated" (i.e., daisychain) and "comparison" (i.e., direct) trips on observable characteristics of the route and user, effectively eliminates the selection concern and, importantly, reduces model dependence (Ho et al., 2007). We do not contend that our matching approach solves the potential simultaneity problem. Rather, we contrast different parametric approaches to controlling for omitted variables that may influence the decision to daisychain. By including individual-specific fixed effects, we remove time-invariant characteristics associated with a user's propensity to daisychain (including, e.g., her income, ability, and risk preferences). Alternatively, by including route-specific fixed effects, we are able to eliminate attributes that comprise the geography of a trip. Lastly, we instrument the choice to daisychain using information from the price schedule. All models produce similar results.

4.3 Matching

Our primary empirical concern in estimating the time cost of daisychaining is selection: trips that are likely to be daisychained are not representative of all direct trips. For example, a short trip that is expected to take less than 10 minutes does not serve as a good counterfactual for a trip that is likely to exceed 30 minutes. This fact is borne out in the data by a simple comparison of means for direct and daisychain routes, indicating that daisychain trips are 16–18 minutes longer in duration and 1.2–1.9 miles longer in distance (see Tables 3, 6, and 7). This estimate almost certainly is biased upward by sample selection. The proper comparison is the time difference between daisychain trips and direct trips that are of similar distances, on similar routes, during similar times, and so forth. We contend that for trips along similar dimensions in our observed covariates, any omitted variables that affect the propensity to daisychain are also balanced. We use matching methods to construct a balanced comparison group.

Specifically, we use one-to-one Mahalanobis covariate matching without replacement with calipers (equal to one standard deviation) to reweight our comparison sample. That is, for each daisychain observation in our data set, we search for a direct route that is within the caliper width for each variable using the Mahalanobis distance metric. If a daisychain trip does not have a match within the caliper width, it is dropped from our sample. We use our matching algorithm to construct frequency weights that are passed on to our parametric models described below. The covariates used for matching are: distance of the trip returned by Google Maps' API, start station latitude and longitude, end station latitude and longitude, month of sample, hour of day, and a suite of hourly weather variables.

Rather than matching on categorical variables, we implement our matching algorithm for subsets of our full data set, including: (i) whether the trip was taken by a casual or registered user, (ii) whether the trip was taken on a weekday or weekend, and (iii) whether the trip was taken before the October 2015 rate change. Primary results for each of our 4 user-time combinations are presented for pre-Oct. 2015 rate change, although post-Oct. 2015 results are replicated in the appendix. Definitions and sources for our primary variables are presented in Table 4. Our matched sample thus returns a comparison group of direct trips that are virtually identical to daisychain trips, the only difference being the choice to daisychain and the trip's corresponding difference in duration.

4.4 Econometric models

On our matched samples, we apply several alternative models. To control for confounding factors that influence trip time, and to increase the precision of our estimates, we estimate

$$\text{Trip Time}_{ijt} = \alpha + \beta \text{Daisy}_{ijt} + \mathbf{R}_j' \gamma_1 + \mathbf{A}_i' \gamma_2 + \mathbf{W}_t' \gamma_3 + \lambda_t + \varepsilon_{ijt} \tag{2}$$

where Trip Time_{ijt} is the elapsed duration of a trip in minutes for subscriber i, on route j, at time t. Daisy_{ijt} is a dummy variable equal to one if the trip is identified as a daisychain trip rather than a direct trip. $\mathbf{R}_j$ is a vector of route-specific controls that are fixed over time, including distance and, in some specifications, route fixed effects. $\mathbf{A}_i$ is a vector of subscriber-specific controls that are fixed over time, including, in some specifications, subscriber fixed effects. $\mathbf{W}_t$ is a vector of hourly weather conditions, and λ_t is a set of hour-of-day and month fixed effects.

We consider two alternative sets of fixed effects specifications. The first incorporates subscriber-level effects. For this specification, we identify β by observing variation in the choice to daisychain within a given subscriber. The notion here is that for similar routes we observe a single bicyclist multiple times—sometimes she rides a direct route and sometimes she daisychains, and the difference in trip times conditional on other covariates is our estimate of β. The subscriber-level effects control for individual characteristics such as income, cycling speed, ability, preferences for risk, and so forth. Inclusion of subscriber fixed effects are important because we do not have detailed individual-level data on subscribers. Limitations of this approach, however, are that we have fewer repeated observations for casual users than registered users and our identifying assumptions imply that an individual's VOT might vary over time.

An alternative specification controls for route-level fixed effects. For this specification, we identify our parameter of interest by fixing the route (i.e., traveling from point A to point B) and observing multiple cyclists using the same start and end stations, but some cyclists split their trip into two trips. The identifying assumption is that for a given route variation in the choice to daisychain for similar individuals provides an estimate of the additional time it takes to daisychain. Allowing the propensity to daisychain to vary across individuals appeals to the intuitive notion that an individual's value of time is fixed, however it allows for the possibility that individual-level unobserved heterogeneity may bias estimates of β.

Our final specification addresses our omitted-variables problem by instrumenting for the dichotomous Daisy_{ijt} variable. We contend that the conditional choice to daisychain is explained entirely by the overage fee incurred by cycling longer than 30 minutes. In other words, in the absence of an increase in the cost of a trip after 30 minutes, the likelihood of daisychaining is zero. Thus, Daisy_{ijt} is a function of the *expected* trip time, which only affects Daisy_{ijt} through the rate schedule. Our empirical measure of expected trip time is the cycling duration between two points returned by Google Maps' API. Intuitively, *ex ante* expected trip time is not correlated with the unobserved component of trip time, ε_{ijt}, because it is a deterministic function of distance traveled, urban street layout, elevation change, stop lights, and so forth.

For instrumental variables, we construct a set of 10-minute bins for whether a trip's expected duration falls within the specified range. Specifically, we estimate the first-stage regression,

$$\text{Daisy}_{ijt} = \delta + \sum_b \omega_b 1[\text{Time Bin}_j^b] + \mathbf{R}_j'\hat{\gamma}_1 + \mathbf{A}_i'\hat{\gamma}_2 + \mathbf{W}_t'\hat{\gamma}_3 + \lambda_t + \mu_{ijt}, \tag{3}$$

where each Time Bin_j^b is equal to one if Google Maps' API returns a trip time estimate for trip j within bin b and zero otherwise. We include 5 time bins, ranging in minutes from $[10, 20)$, $[20, 30)$, $[30, 40)$, $[40, 50)$, and greater or equal to 50 minutes. Expected trip durations less than 10 minutes serve as our omitted category. Given the discontinuous increase in the cost of a trip beyond 30 minutes, we anticipate the likelihood of daisychaining to increase monotonically across bins. Because our matching approach balances the probability of daisychaining at 50%, we are comfortable estimating Equation 3 linearly.

Further, we examine heterogeneity by user groups that likely possess different knowledge of the bikesharing infrastructure. We run each of our econometric models on registered (i.e., annual) and casual (i.e., sub-annual) members. We contend, generally, that annual users will have better knowledge of where intermediate stations are located and they will be representative of cyclists who use B-cycle for regular commuting. Casual users, on the other hand, are more likely to be tourists with less knowledge of urban infrastructure and, importantly, different constraints on their time. To segment our sample further along these dimensions, we analyze differences between weekday and weekend travel for each user type. Further, we examine time-of-day heterogeneity to explore the importance of temporally distinct values of time.

Finally, we explore the impact of a 400% increase in the cost of a trip beyond 30 minutes. We exploit a change in rate structure on October 1st, 2015, that increased overage fees from \$1 to \$5. By using the price increase as a treatment indicator, we implement a simple difference-in-difference design to estimate both the change in the likelihood of daisychaining as well as the change in the trip time associated with daisychaining. Specifically, we estimate

$$\text{Daisy}_{ijt} = \alpha + \delta_1 1\{\text{Post Oct. 2015}\} + \mathbf{R}_j'\gamma_1 + \mathbf{A}_i'\gamma_2 + \mathbf{W}_t'\gamma_3 + \lambda_t + \varepsilon_{ijt} \tag{4}$$

where the $1\{\text{Post Oct. 2015}\}$ equals one if the trip takes place after October 1, 2015. The coefficient δ_1 provides an estimate of the increase in the likelihood of daisychaining in response to the price increase. We also re-estimate Equation 2 with an interaction between Daisy_{ijt} and $1\{\text{Post Oct. 2015}\}$ to provide an estimate of how much additional time spent daisychaining can be attributed to the October 2015 price increase.

5 Results and discussion

5.1 Empirical results

Our empirical goal is to estimate the additional time spent daisychaining for a trip relative to an otherwise identical trip. We can construct a simple lower bound using expected bicycle trip time estimates returned from Google Maps. Using the B-cycle data, we create a variable equal to the difference between a Google time estimate for a direct trip and the sum of the time estimate for the two segments that make up a daisychain route for all observed trips in our raw data. Table 5 shows that the median and mean of this variable are between 2.5 and 3.8 minutes. We expect this estimate to be low because the times do not necessarily reflect the time cost of getting into and out of the flow of traffic and docking the bicycle.

Turning to our trip data, we provide a simple comparison of means to highlight the empirical difference in time for daisychain trips and direct trips, as well as an obvious source of bias in our raw data set. As shown in Table 3, we display trip time statistics for our unweighted "full sample" for each of our user-time combinations (i.e., registered-weekday, casual-weekday, registered-weekend, casual-weekend). A naïve comparison suggests that daisychain trips take 16.6–18.3 minutes longer than direct routes. Of course, this estimate is skewed upwards because daisychain trips are weakly longer in distance than direct routes by definition. After reweighting our sample using one-to-one Mahalanobis matching without replacement on a set of matching covariates that include the distance of the trip, start and end location, time-of-day and month of the trip, segmented by our four user-time combinations, we reduce the difference in trip time estimates substantially. On the matched sample (Table 3), the data suggest that a daisychain trip is approximately 7.7–11.2 minutes longer than an observationally similar direct trip. This comparison also reveals important distinctions within our user-time groups. Registered users on weekdays have the smallest estimate, while casual users have the largest. This initial result is sensible: registered users on weekdays are more likely to be commuters, who value time and urgency, whereas casual users are more likely to use the bikeshare for recreational purposes.

Before discussing our parametric results, we first consider how well our matching algorithm performs. Tables 6 and 7 highlight common balance statistics for our matched samples (Lee, 2013; Ferraro and Miranda, 2017; Wichman and Ferraro, 2017). We report results from one-to-one, nearest-neighbor matching using the Mahalanobis metric without replacement. Our results, however, are remarkably robust to other matching approaches, including propensity score matching, $1:m$ matching (with $m = 3, 5$), matching with replacement, and the exclusion or addition of matching covariates.[10] Notably, if we include the Google Maps' trip distance as the only matching covariate, we obtain qualitatively similar balance results on trip time and all other covariates. We settled on our matching approach because of its transparency, comprehensiveness, and its strong degree of balance.

Each of our covariates is nearly perfectly balanced on all measures, with the exception of Google Distance. For this variable, the standardized mean difference in each of our four samples improves drastically (by a factor of 10 in some cases), but remains at a value of approximately 7–12, which can be considered a moderately large positive difference. To provide a sense that balance on this covariate is adequate, we present k-densities for both unweighted and weighted samples for each primary covariate for each of our user-time combinations before and after the rate change in Figures A.1–A.4. The plotted densities reveal that the weighted density for Google Miles for daisychain trips tracks the distribution of

[10] Results from matching with an alternative caliper width (caliper $= 0.25\sigma$) are presented in the appendix. Other results are available from the authors upon request.

direct trips quite closely, although the direct trips have trivially more mass on the "shorter" side of trips.

Our matched k-densities for trip time between daisychain and direct routes, presented in Figure 6, illustrate our primary identification strategy. Because all daisychain and direct trips are matched to be similar along observable route characteristics—and thus, by assumption, balanced on unobservable characteristics that may affect the decision to daisychain—the matched distribution of direct trips provides a proper, observed counterfactual density for daisychain trips. As shown in Figure 6, we contend that the distribution of daisychain trips (solid gray line) would look exactly like the weighted distribution of direct trips (dashed red line) in the absence of a price discontinuity at 30 minutes. These figures reveal substantially more mass on the unfavorable side of the of the price discontinuity for daisychain trips than that of direct trips. Such sorting around the discontinuity is enabled by the low-cost alternative to paying the overage free by splitting consumption of one trip into two smaller trips.

In our parametric models, we take advantage of consumer sorting around the notch point at 30 minutes and the observed counterfactual for daisychain trips. This approach is distinct from recent applications of counterfactual estimation in the presence of notches in the taxation (Chetty et al., 2011; Kleven and Waseem, 2013) and fuel economy (Ito and Sallee, 2014) domains. Our approach is also distinct in that we are interested in what that behavior reveals about our policy parameter of interest: consumers' VOT.

In our first naïve model, we regress trip time on a binary variable for whether the trip was a daisychain trip, an estimate of trip distance returned by Google Maps' API, and a suite of weather variables that vary by hour. We apply each of our econometric models to each of our four user-time groups. We report robust standard errors, clustered at the route level in most specifications, and as suggested by Ho et al. (2007) we do not account for any variation that may be introduced by preprocessing our set of comparison trips.

In Table 8, results from a simple linear model improve the precision of our estimates for the coefficient on Daisy relative to a comparison of means, but the point estimates remain largely unchanged—daisychain trips for registered users on weekdays take approximately 7.4 minutes longer than direct trips, and this coefficient is larger (approximately 9–12 minutes) for weekend trips or trips taken by a casual user. This pattern of coefficients is preserved when we apply the same estimator to our matched sample (Table 8, Panel B). The weighted estimates are all smaller than those of the unweighted sample, suggesting that positive sample selection bias may be present in our most rudimentary models. But, the change in coefficients is relatively small.

In Table 9, we apply linear panel data estimators to our matched sample to explore the possibility that unobservable, time-invariant characteristics of users or routes affect the likelihood to daisychain. In panels A and B, we eliminate subscriber-specific and route-specific heterogeneity, respectively. These models produce smaller estimates than in Table 8. Models with subscriber fixed effects possess larger coefficients for casual users, with much larger estimates of variance, than do models with route fixed effects, potentially due to fewer repeated observations within each casual subscriber. In models with route fixed effects, coefficients on Daisy for registered users on weekdays are significantly smaller than weekends (6.1 minutes vs. 8.0 minutes, $p < 0.01$); the same statistic for casual users is slightly greater than 9 minutes for both weekdays and weekends and these estimates are statistically similar.

We also instrument for the choice to daisychain using information from the price schedule, as described in Equation 3. Because trip time and daisychaining are unconditionally positively correlated (i.e., the likelihood of daisychaining increases with expected trip time), we would expect that correcting for this simultaneity would produce smaller parameter estimates. As shown in Panel C of Table 9, however, the point estimates are generally larger for the IV models than the route fixed effects models in Panel B. None of the coefficients, however, is statistically different.

Comparing the three sets of results in Table 9 suggests that our matching approach reduces model dependence (Ho et al., 2007) to the point where different model specifications and identification assumptions do not have a qualitatively important impact on our estimated parameters. Indeed, within each user-time group, none of the specifications produces significantly different estimates by conventional standards. Our preferred estimates are those with route fixed effects without instrumental variables (Panel B) primarily because they are estimated with the most precision, and they rely on the most sensible variation in the available data.

Using our preferred model specification, we explore how consumer VOT estimates vary throughout the day. Other recent research has found within-day traffic variation to be of substantial policy importance (e.g., Anderson (2014) and LaRiviere et al. (2016)). We decompose our average effects for each user-time group by interacting Daisy with indicators for morning, afternoon, evening, and night. By doing so, we hope to further isolate commuting behavior from recreational behavior. In Table 10, we observe a consistent pattern across user-time groups: morning periods have smaller point estimates than the relatively homogenous estimates at other times of the day. This difference is most striking for registered users on weekdays, which accords with the notion that these are most likely commuters who value prompt arrival at their workplace. The coefficient for this group is significantly smaller ($p < 0.05$) than all other coefficients for registered-weekday trips. Although other user-time groups display similar intra-day patterns, the estimates are not estimated precisely enough to reveal statistical differences.

Finally, we explore consumer behavior in response to a 400% increase in the price of a trip beyond 30 minutes, from $1 to $5 in October 2015. Using a simple difference-in-difference model, we ask two questions: did the price increase increase the likelihood to daisychain? And, conditional on the choice to daisychain, did the price increase increase the time spent daisychaining? The results for both of these extensive and intensive margin models are in Table 11. The answers are simple: no. In Panel A, the probability of daisychaining increased by a small amount in two of the models, but the standard errors are large relative to the point estimates. Further, in Panel B, an interaction between Daisy and the Post October 2015 dummy variable suggests that registered cyclists spent more time daisychaining during the week in response to the price increase, but this difference is not statistically different from the time spent daisychaining during earlier months in the sample.[11] Although we observe fewer daisychain trips after October 2015, we do have sufficient power to identify the additional effect of the price change.

5.2 Implications for VOT

In official federal guidance on using VOT estimates in economic analyses (U.S. Department of Transportation, 2014), the only direct guidance for bicycling states: "Personal time spent...bicycling, should be evaluated at 100 percent of hourly income, with a range of 80 to 120 percent to reflect uncertainty" [pp. 13]. This guidance is not, to our knowledge, informed by any revealed preference estimate. The same guidance suggests valuing VOT using the median hourly income, constructed as median household income for a given geographic region divided by the number of working hours in a year: 2,080. We present relevant statistics for our application—median household income for residents in the U.S., Colorado, Denver, and for B-cycle members—in Table 12.

In the most recent years available, B-cycle members reported annual household income ($99,000 in 2012) approximately one-half larger than that of Denver residents ($67,000 in 2014). This estimate suggests that our sample of bikeshare users is skewed towards relatively wealthy individuals. Demographic surveys[12] for similar bikeshare programs reach the same

[11] Note that our primary matched samples were constructed for trips that occurred prior to October 2015. We construct a new matched sample using the post-October of 2015 observations only. As such, the matched sample examining the price increases is comparing apples to apples (i.e., trips that were taken under the same pricing regime).

[12] See, e.g., the 2016 Capital Bikeshare Member Survey Report, `https://d21xlh2maitm24.cloudfront.net/wdc/CABI-2016MemberSurveyReport-FINAL.pdf?mtime=20170302144201`. Last accessed: July 7, 2017.

conclusion: bikeshare users are more likely to be male, younger, white, and wealthier than the population in the same urban area.

How do our estimates translate to an estimate of consumers' VOT? As mentioned previously, the most common revealed preference estimates for VOT for automobile drivers range approximately from 50% of the hourly wage (Wolff, 2014) to nearly 100% (Small et al., 2005). Using B-cycle's reported data, 50% of the prevailing wage for our sample is \$23.8 per hour. We previously asserted that a consumer can split her trip for a marginal time cost of $A(\beta)$ to avoid possibly paying an overage fee of k. Our empirical analysis provides an estimate of β attributable to daisychaining. Our argument is as follows: 1) the only reason to daisychain is to avoid the overage fee k, and 2) the only reason to avoid paying k is if the user can incur a smaller cost $A(\beta)$. This logic reveals an implied value of time (upper bound) defined as $\text{VOT} = 60(k/\beta)$.

In Table 13, we present implied VOT estimates for various estimates of β and values of k that reflect the prevailing pricing structures. Our primary VOT estimates vary from \$6.47–\$9.87 per hour for our four user-time combinations prior to Oct. 2015. These estimates are approximately the level of the minimum wage, ranging from 13.6–20.7% of the B-cycle members' hourly wage. Further, our time-of-day results suggest substantial differences, which is consistent with a higher value for urgency (Bento et al., 2014) or heterogeneity in the recreational vs. commuting populations. Registered users on weekday mornings, who are most likely commuters, value their time at approximately \$18.09 per hour, or 37.9% of the hourly wage.[13] Our results suggest that casual users on weekend nights, in contrast, value their time at only 11.1% of the hourly wage. These results emphasize the importance of incorporating temporal and user type heterogeneity when valuing bicycle transit for personal and recreational use. If taken directly, these results suggest that official federal guidance may overestimate the value of time spent bicycling, in some cases by a factor of eight.

Notably, while our estimates appear generally lower than that of previous analyses, our best measure of the value of time spent commuting (i.e., registered users on weekday mornings) comports with the majority of revealed preference estimates from automotive data. This result, therefore, suggests that the value of improving bicycle infrastructure can be assessed on equal footing with improvements of public and private urban transit options.

These implications, as do those from all revealed preference estimates of VOT, are predicated on the assumption that consumers optimize perfectly with respect to their time-for-money trade-off. Wolff (2014) argues that drivers trade off speed for fuel efficiency, Small et al. (2005) and Fezzi et al. (2014) model the driver's decision to pay a toll for congestion-free

[13]A figure form Denver B-cycle's 2012 Member survey showed that 43.6% of annual members use their membership for commuting to or from work.

travel, and Deacon and Sonstelie (1985) evaluate the trade-off between time spent waiting in line for cheaper gasoline. Our VOT estimates, in contrast, assume that consumer behavior responds to a discontinuity in the cost of a bicycle trip. But, by exploiting a 400% increase in that price, we find large magnitudes of VOT, ranging from 58–114% of the hourly wage. These estimates are implausible primarily because they are generated by behavior that is statistically identical to behavior generated by a much smaller price level. The conclusion we arrive at, given this information, is that individuals are not responding to the change in price, rather they are responding to the more salient information in the price structure: the quantity notch at 30 minutes.

Relative to other papers in this literature that exploit variation in highway tolls or gasoline prices, which tend to be salient signals (i.e., typically, this price information is advertised on bright highway signs or roadside billboards), our application relies on a not-entirely-intuitive price structure (see Figure 1). Thus, consumers might know that the price increases from $p = 0$ to $p > 0$ at 30 minutes, but they might not know the level of p. Because of the peculiarity in our price structure within transportation, it is unclear whether the behavior we identify in this paper is useful for other transportation applications.

We do, however, believe our results have policy import in the context of nonlinear pricing for electricity, water, cellular phone usage, and other consumer goods where inattention and imperfect information play an important role (see, e.g., Sexton (2014); Wichman (2017); Finkelstein (2009); Chetty et al. (2009); Li et al. (2014); Grubb (2014)). Because our results suggest that consumers may respond more strongly to easily identifiable and familiar information (e.g., a quantity of 30 minutes), perhaps increasing-block price structures intended to encourage conservation of electricity or water could be improved by highlighting familiar quantity signals. Rather than telling consumers, for example, that the marginal price of water increases from \$2.14 to \$2.75 after 400 cubic feet per month, one could instruct consumers that shortening showers to no more than 10 minutes could reduce monthly water bills by an equivalent cost savings. Data overage charges for cell-phone use operate under a similar premise. As a consumer reaches an pre-determined quantity, they are told via text message of the percentage of data remaining before they are charged a fee. This insight suggests that efforts to "teach" consumers how bills or taxes are calculated under complicated rate structures (Kahn and Wolak, 2013; Chetty and Saez, 2013) might be misguided. Simplifying and isolating relevant information for use in decision making may be a more promising line of research to induce expected outcomes than would attempting to affect consumer behavior in a way that corresponds with our economic models.

5.3 Caveats, confounders, and conclusions

Naturally, our estimates are subject to several caveats. Most glaringly, our analysis is an evaluation of a single bicycle-sharing program on a subset of relatively wealthy users. Other bikesharing programs, however, comprise similarly select populations, so our sample is likely representative of behavior in other programs. More specifically, nearly all U.S.-based bikesharing programs, and many more globally, share commonality with the rate structure we analyze here. One also might contend that even within our select sample, we analyze a distinct set of trips that are not representative of overall bikeshare user behavior. One response to such a criticism is noting that opting in to ride-sharing programs (Cohen et al., 2016), car-pooling (Bento et al., 2013), and HOV lanes (Small and Yan, 2001), for example, are equally select samples within their respective domains.

An alternative concern might be that bicycling is associated with other benefits in addition to commuting, such as health benefits from exercising. We reiterate that all of our estimated effects are relative to the counterfactual of cycling for a slightly smaller amount of time. Therefore, any relative benefits of daisychaining are likely trivial. Further, we contend that cycling better isolates the margins on which these benefits might arise unlike myriad potential benefits and costs (e.g., comfort, privacy, prestige) that arise in automobile comparisons.

Our identification strategy relies upon the assumption that conditional on observable characteristics, any unobservable components that affect the choice to daisychain are balanced. One threat to this strategy is that a short trip, in distance, may embed other characteristics associated with daisychaining that are not balanced but, in principle, unobservable. For example, a 1-mile direct route may not be a good counterfactual for a daisychain trip with the same start and end destinations if there are potential recreational, sightseeing benefits nearby. That is, a direct route would be shorter than a meandering daisychain trip on the same route, which would bias our coefficient estimates away from zero. In Table A.4, we interact the choice to daisychain with trip distance bins. Our primary results are robust to this stratification for trips 4 miles long or less.

There are two additional confounding factors that could alter the interpretation of our results: redocking a bicycle mid-trip because of a mechanical failure and falsely assigning daisychain status to a trip when there was actually a brief layover at an intermediate destination. For the former, we cannot identify this relationship statistically and it is possible that we are partially attributing the daisychain response to riders going out of their way to swap their bicycle for a better one. That said, it is difficult to justify a revealed disamenity value for a squeaky bicycle seat at a sizable fraction of the prevailing wage. Second, because we identify daisychain trips as trips that are checked-in and checked-out of the same stations within 3 minutes, perhaps cyclists dock their bicycle on their way to a dinner party to purchase a bottle of wine. We cannot rule out this behavior, but it seems unlikely that such short intermediate stops are representative of all bikeshare trips in our sample. Further, Table 6 reveals apparent sorting around the 30-minute price notch. There is no rationale for why mechanical failures or wine retailers should respond to discontinuities in the price structure of a bicycle trip.

A final concern is whether we have sufficiently purged our data of various forms of endogeneity. We contend that the consonance of our estimates in Table 9, using different identification assumptions, provides strong evidence that our matching procedure has reduced our dependence on model choice and specification. A comparison of means on the matched sample provides qualitatively similar results to our preferred econometric specification. It is true that we have not formally modeled the decision to daisychain in our econometric specification—that choice problem is not the focus of our analysis, nor would the structure of that choice problem yield anything of broad value for economists and policymakers. The present paper dispenses with all major sources of bias to provide a plausibly causal estimate of consumers' opportunity cost of time using reduced-form methods. Should 'plausibly causal' seem too strong of a statement, recall that we have also estimated reasonable lower and upper bounds for our parameters through a comparison of Google Maps' travel time estimates (Table 5) and a comparison of means weighted by our matching approach (Table 3), respectively.

Overall, this paper provides evidence of novel consumer bunching in a novel market—cycling within urban bikesharing programs. Our empirical results identify strategic behavior that is applicable to other markets. We estimate revealed preference measures of the value of time for bicyclists that depend on trip purpose and temporal heterogeneity. Although some of our estimates largely comport with previous VOT estimates for commuters, we find that individuals do not respond to a dramatic change in price. Our results, then, provide justification for an attempt to better understanding how consumers assimilate information within complicated rate structures into their decision-making process.

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Table 1: B-cycle membership and overage prices

Dates Effective	24-hour	7-day	30-day	Annual	Annual Plus Fee	1st Overage Fee	2nd(+) Overage
Jan. 2012–Dec. 2014	8	20	30	80	N/A	1	4
Jan. 2015–Sep. 2015	9	N/A	15	90	100	1	4
Oct. 2015–Dec. 2015	9	N/A	15	90	100	5	5
Jan. 2016–Dec. 2016	9	N/A	15	N/A	135	5	5

Notes: All amounts are nominal U.S. Dollars. B-cycle has an "Annual Plus" membership that allows registered members to receive an allotment of 60-minutes free per trip. We drop annual plus members from our sample.

Table 2: Trip time statistics by year and user type

	Obs.	Mean	Std. Dev.	Median
2013	198,539	14.81	13.48	11
2014	301,456	14.26	13.02	10
Jan. 2015–Sep. 2015	224,818	15.03	13.24	11
Oct. 2015–Dec. 2015	47,838	12.94	11.17	10
2016	157,920	14.38	12.01	10
Casual	277,653	24.32	16.82	20
Registered	652,918	10.35	7.73	8
Full sample	930,571	14.52	12.93	10

Table 3: Mean trip time statistics by user-time groups and daisychain vs. direct trips for raw and matched samples

	Raw data			Matched data		
	Direct	Daisychain	Diff.	Direct	Daisychain	Diff.
Registered, Weekday	9.70	26.72	–17.02	18.71	26.39	–7.68
	(7.13)	(12.76)		(10.07)	(12.78)	
Casual, Weekday	23.22	41.55	–18.33	29.95	41.18	–11.23
	(16.77)	(15.94)		(16.60)	(15.95)	
Registered, Weekend	12.40	30.20	–17.80	20.71	29.99	–9.28
	(9.01)	(13.28)		(10.48)	(13.18)	
Casual, Weekend	24.78	41.42	–16.64	30.64	41.24	–10.60
	(17.05)	(15.60)		(16.46)	(15.58)	

Notes: Trip time means are presented along with standard deviations (in parentheses). Differences are presented as Direct–Daisychain. Matched data presents the weighted sample after 1:1 Mahalanobis matching without replacement. All differences are significant at the $p<0.01$ level as indicated by a two-sided t-test. All observations are pre-Oct. 2015 price change.

Table 4: Variable Descriptions

Variable	Units	Varies at	Source
Daisy	Binary	Subscriber-by-trip level	Author constructed
Google Distance	Miles	Route level	Google Maps API
Trip Time	Minutes	Subscriber-by-trip level	
Start Latitude	Decimal	Route level	
Start Longitude	Decimal	Route level	
End Latitude	Decimal	Route level	
End Longitude	Decimal	Route level	Denver B-cycle
Month	Integer	Trip level	
Weekday	Binary	Trip level	
Start Hour	Integer	Trip level	
Member Type	Integer	Subscriber level	
Precipitation Intensity	Inches per hour	Hourly	
Precipitation Probability	Proportion	Hourly	
Temperature	Degrees Fahrenheit	Hourly	
Dew Point	Degrees Fahrenheit	Hourly	
Humidity	Proportion	Hourly	
Wind Speed	Miles per hour	Hourly	Dark Sky API
Visibility	Miles	Hourly	
Cloud Cover	Proportion	Hourly	
Fog	Binary	Hourly	
Rain	Binary	Hourly	
Snow	Binary	Hourly	

Table 5: Google Maps' bicycle trip time differences between daisychain and direct trips

	Obs.	Mean	Std. Dev.	Median
All "daisychain" rides	15,353	3.16	3.74	2.50
Positive difference only	13,344	3.80	3.55	2.76

Notes: Positive difference in the second row looks only at trip time differences where the segmented trip is expected to take at least as long as the direct route. The difference between some trips is negative due to Google Maps returning suboptimal routes for a subset of our sample.

Table 6: Covariate balance for registered users from 1:1 nearest neighbor matching without replacement

	Weekday		Weekend	
	Full Sample	Matching (Caliper = 1σ)	Full Sample	Matching (Caliper = 1σ)
Trip Time (minutes)				
Mean difference	17.021	7.626	17.801	9.161
Std. mean difference	133.445	59.672	134.055	69.615
Mean raw eQQ difference	17.016	7.628	17.79	9.162
Variance ratio (Treat/Comp.)	3.2	1.594	2.173	1.547
Google Distance (miles)				
Mean difference	1.857	0.117	1.599	0.146
Std. mean difference	102.317	6.882	111.48	10.497
Mean raw eQQ difference	1.856	0.117	1.598	0.146
Variance ratio (Treat/Comp.)	4.084	1.048	2.023	1.06
Start Latitude				
Mean difference	-0.004	0	-0.001	0
Std. mean difference	-20.257	0.021	-7.951	1.096
Mean raw eQQ difference	0.004	0	0.003	0
Variance ratio (Treat/Comp.)	2.23	0.995	1.728	1.001
Start Longitude				
Mean difference	0.003	0	0.001	0
Std. mean difference	14.898	-0.108	2.987	-0.84
Mean raw eQQ difference	0.004	0	0.003	0
Variance ratio (Treat/Comp.)	1.57	0.999	1.453	1.005
End Latitude				
Mean difference	-0.007	0	-0.002	0
Std. mean difference	-30.135	-0.185	-10.791	0.201
Mean raw eQQ difference	0.007	0	0.004	0
Variance ratio (Treat/Comp.)	2.852	1.004	1.84	1.006
End Longitude				
Mean difference	0.006	0	0.002	0
Std. mean difference	29.888	0.305	7.638	0.323
Mean raw eQQ difference	0.007	0	0.004	0
Variance ratio (Treat/Comp.)	1.867	0.985	1.666	1.018
Month				
Mean difference	0.023	-0.015	-0.145	-0.005
Std. mean difference	0.885	-0.599	-5.626	-0.187
Mean raw eQQ difference	0.198	0.042	0.145	0.067
Variance ratio (Treat/Comp.)	0.888	1.038	0.939	1.055
Start Hour				
Mean difference	1.169	-0.022	-0.192	0.031
Std. mean difference	26.874	-0.518	-5.411	0.872
Mean raw eQQ difference	1.17	0.102	0.449	0.078
Variance ratio (Treat/Comp.)	0.876	1.028	0.813	1.034

Notes: For each covariate, we evaluate the improvement in covariate balance in four ways (Lee, 2013): (i) difference in means; (ii) standardized mean difference (Rosenbaum and Rubin (1985) suggest that a standardized difference greater than 20 should be considered large); (iii) eQQ mean difference, a nonparametric measure that evaluates rank rather than the precise value of the observations (Ho et al., 2007); and (iv) variance ratio between treated and untreated units (Sekhon, 2008). All observations are pre-Oct. 2015 price change.

Table 7: Covariate balance for casual users from 1:1 nearest neighbor matching without replacement

	Weekday		Weekend	
	Full Sample	Matching (Caliper = 1σ)	Full Sample	Matching (Caliper = 1σ)
Trip Time (minutes)				
Mean difference	18.337	11.354	16.636	10.586
Std. mean difference	115.017	72.108	106.828	68.472
Mean raw eQQ difference	18.331	11.355	16.655	10.641
Variance ratio (Treat/Comp.)	0.904	0.901	0.834	0.86
Google Distance (miles)				
Mean difference	1.282	0.164	1.225	0.178
Std. mean difference	89.219	11.964	83.209	12.456
Mean raw eQQ difference	1.281	0.164	1.224	0.178
Variance ratio (Treat/Comp.)	1.543	1.061	1.418	1.063
Start Latitude				
Mean difference	-0.002	0	-0.002	0
Std. mean difference	-9.696	0.461	-13.216	0.326
Mean raw eQQ difference	0.002	0	0.002	0
Variance ratio (Treat/Comp.)	1.425	0.994	1.357	0.988
Start Longitude				
Mean difference	0.003	0	0.004	0
Std. mean difference	13.581	-0.086	18.181	0.003
Mean raw eQQ difference	0.003	0	0.004	0
Variance ratio (Treat/Comp.)	1.379	0.994	1.333	0.993
End Latitude				
Mean difference	-0.002	0	-0.001	0
Std. mean difference	-10.882	-0.731	-6.932	-0.394
Mean raw eQQ difference	0.002	0	0.001	0
Variance ratio (Treat/Comp.)	1.38	0.987	1.265	0.993
End Longitude				
Mean difference	0.002	0	0.002	0
Std. mean difference	12.411	0.272	9.584	-0.015
Mean raw eQQ difference	0.003	0	0.003	0
Variance ratio (Treat/Comp.)	1.413	1.003	1.367	1
Month				
Mean difference	-0.11	0.007	0.012	-0.013
Std. mean difference	-4.878	0.302	0.515	-0.55
Mean raw eQQ difference	0.121	0.046	0.057	0.066
Variance ratio (Treat/Comp.)	0.997	1.054	1.003	1.066
Start Hour				
Mean difference	-0.361	0.004	-0.476	-0.006
Std. mean difference	-10.535	0.117	-15.115	-0.191
Mean raw eQQ difference	0.617	0.048	0.518	0.07
Variance ratio (Treat/Comp.)	0.746	1.022	0.822	1.056

Notes: For each covariate, we evaluate the improvement in covariate balance in four ways (Lee, 2013): (i) difference in means; (ii) standardized mean difference (Rosenbaum and Rubin (1985) suggest that a standardized difference greater than 20 should be considered large); (iii) eQQ mean difference, a nonparametric measure that evaluates rank rather than the precise value of the observations (Ho et al., 2007); and (iv) variance ratio between treated and untreated units (Sekhon, 2008). All observations are pre-Oct. 2015 price change.

Table 8: Regression results from linear models on full and weighted samples

	Weekday		Weekend	
	Registered	Casual	Registered	Casual
Panel A: Linear models on full sample				
Daisy	7.399	12.208	9.096	10.819
	(0.441)	(0.367)	(0.300)	(0.354)
Google distance	5.075	4.849	5.367	4.645
	(0.064)	(0.112)	(0.059)	(0.106)
Obs.	410,935	126,283	89,259	103,130
R-squared	0.454	0.190	0.425	0.172
Panel B: Linear models on weighted sample				
Daisy	7.117	10.466	8.628	10.037
	(0.304)	(0.430)	(0.339)	(0.429)
Google distance	4.093	4.385	4.610	4.495
	(0.200)	(0.174)	(0.136)	(0.189)
Obs.	9,008	8,164	4,559	8,056
R-squared	0.425	0.243	0.384	0.258

Notes: Dependent variable in all models is trip time (in minutes). All observations are pre-Oct. 2015 price change. All models include controls for weather, month fixed effects, and hour-of-day fixed effects. Panel A presents results from linear models on the full sample. Panel B presents results from linear models on a weighted sample using 1:1 Mahalanobis matching without replacement. Robust standard errors clustered at the route level are presented in parentheses.

Table 9: Regression results from panel models on weighted samples

	Weekday		Weekend	
	Registered	Casual	Registered	Casual
Panel A: Subscriber fixed effects on weighted sample				
Daisy	5.733	12.325	7.215	10.921
	(0.693)	(1.987)	(0.670)	(2.357)
Google distance	4.920	4.240	4.644	4.966
	(0.179)	(0.733)	(0.256)	(0.705)
Obs.	9,008	8,164	4,559	8,056
R-squared	0.724	0.927	0.748	0.941
Panel B: Route fixed effects on weighted sample				
Daisy	6.079	9.096	8.027	9.270
	(0.404)	(0.688)	(0.621)	(0.667)
Google distance	6.011	6.704	5.444	6.484
	(0.755)	(1.284)	(1.243)	(0.975)
Obs.	9,008	8,232	4,559	8,056
R-squared	0.750	0.626	0.776	0.649
Panel C: IV models with route fixed effects on weighted sample				
Daisy	6.981	8.522	10.123	11.413
	(1.359)	(1.821)	(1.473)	(1.768)
Google distance	5.532	6.881	4.362	5.435
	(0.903)	(1.453)	(1.049)	(1.135)
First-stage F-stat	173.92	28650.30	81.34	90.34
Obs.	7,737	7,167	3,426	7,198
R-squared	0.209	0.158	0.235	0.174

Notes: Dependent variable in all models is trip time (in minutes). All observations are pre-Oct. 2015 price change. All models include controls for weather, month fixed effects, and hour-of-day fixed effects. All panels present results from linear panel models on a weighted sample using 1:1 Mahalanobis matching without replacement. In Panel C, Daisy is instrumented for using indicators for 10-minute bins of expected trip duration as described in Equation 3. Robust standard errors clustered at the subscriber level (Panel A) and route level (Panels B and C) are presented in parentheses.

Table 10: Regression results with route fixed effects and time-of-day heterogeneity

	Weekday		Weekend	
	Registered	Casual	Registered	Casual
Daisy × Morning	3.316	9.032	5.360	8.758
	(0.637)	(2.501)	(2.090)	(3.202)
Daisy × Afternoon	6.931	9.194	8.130	9.437
	(0.707)	(0.942)	(0.899)	(0.870)
Daisy × Evening	6.720	9.399	8.276	8.774
	(0.590)	(0.990)	(1.022)	(1.045)
Daisy × Night	6.523	7.751	8.553	11.315
	(0.982)	(2.005)	(1.645)	(2.107)
Google distance	6.024	6.506	5.429	6.471
	(0.741)	(1.266)	(1.241)	(0.975)
Obs.	9,008	8,164	4,559	8,056
R-squared	0.752	0.627	0.776	0.649

Notes: Dependent variable in all models is trip time (in minutes). All observations are pre-Oct. 2015 price change. All models include controls for weather, month fixed effects, and hour-of-day fixed effects. Morning = 1 if the trip began between 6:00AM and 9:59AM. Afternoon = 1 if the trip began between 10:00AM and 2:59PM. Evening = 1 if the trip began between 3:00PM and 7:59PM. Night = 1 if the trip began at or after 8:00PM. Results are from linear panel models on a weighted sample using 1:1 Mahalanobis matching without replacement. Robust standard errors clustered at the route level are presented in parentheses.

Table 11: Regression results from linear probability models using 400% price increase beginning on October 1, 2015 as treatment

	Weekday		Weekend	
	Registered	Casual	Registered	Casual
Panel A: Linear probability model on weighted sample with route fixed effects				
1{Post October 2015}	0.003	0.005	-0.042	-0.032
	(0.018)	(0.021)	(0.041)	(0.053)
Google distance	0.494	0.555	0.482	0.583
	(0.058)	(0.048)	(0.076)	(0.078)
Obs.	11,372	11,222	5,619	4,838
R-squared	0.366	0.450	0.516	0.610
Panel B: Route fixed effects on weighted sample with interactions				
Daisy	6.058	9.641	8.062	8.984
	(0.381)	(0.608)	(0.586)	(0.626)
Daisy $\times$1{Post October 2015}	0.556	-0.500	-0.523	-0.804
	(0.606)	(0.849)	(1.194)	(0.877)
Google distance	6.506	6.400	6.014	6.255
	(0.709)	(0.899)	(1.128)	(0.846)
Count of Daisy=1 $<$ Oct. 2015	4,564	3,976	2,297	3,973
Count of Daisy=1 $\geq$ Oct. 2015	1,196	1,681	532	1,764
Obs.	11,372	11,222	5,619	11,401
R-squared	0.727	0.593	0.748	0.605

Notes: Dependent variable in Panel A is whether a trip was daisychain. Dependent variable in Panel B is trip time (in minutes). All models include controls for weather, month fixed effects, and hour-of-day fixed effects. Results are from linear panel models on a weighted sample using 1:1 Mahalanobis matching without replacement. Robust standard errors clustered at the route level are presented in parentheses.

Table 12: Income statistics

	Median HH Income		% HHs less than $35K		Minimum Wage	
	2012	2014	2012	2014	2012	2014
United States	$52,970	$53,657	28.1	26.4	$7.03	$7.25
Colorado	$58,532	$61,303	24.2	21.6	$7.41	$8
Denver	$63,366	$66,870	—	—	$7.41	$8
B-cycle annual members	$99,214	—	9.7	5.5	—	
B-cycle casual users	—	—	12.6	18.3	—	—

Notes: All dollar figures are in 2014 adjusted USD. % Households with income less than $35,000 is based on nominal amounts. Median household income is estimated using the table on page 7 of the Denver B-cycle Member demographics report, 2012 (`https://denver.bcycle.com/docs/librariesprovider34/default-document-library/denver-b-cycle-demography_2011-2012.pdf?sfvrsn=2`). The percent of households with income under $35,000 comes from page 8 of the 2010-14 report (`https://denver.bcycle.com/docs/librariesprovider34/default-document-library/denver-b-cycle-demography_2010-2014.pdf?sfvrsn=2`). Household median incomes for the United States, Colorado, and Denver metro area come from `http://www.deptofnumbers.com/income/colorado/denver/`. Estimates of the percent of households with income less than $35,000 were constructed using the the 1-year ACS samples, which can be downloaded from `https://usa.ipums.org/usa`. Minimum wage adjusted to 2014 USD.

Table 13: Implied estimates of consumers' value of time ($/hour)

	Weekday		Weekend	
	Registered	Casual	Registered	Casual
Primary average effects				
(Table 9, Panel B)	9.87	6.60	7.47	6.47
	(20.7%)	(13.8%)	(15.7%)	(13.6%)
Time-of-day results				
(Table 10)				
Morning	18.09	6.64	11.19	6.85
	(37.9%)	(13.9%)	(23.5%)	(14.4%)
Afternoon	8.66	6.53	7.38	6.36
	(18.2%)	(13.7%)	(15.5%)	(13.3%)
Evening	8.93	6.38	7.25	6.84
	(18.7%)	(13.4%)	(15.2%)	(14.3%)
Night	9.20	7.74	7.02	5.30
	(19.3%)	(16.2%)	(14.7%)	(11.1%)
After 400% price increase				
(Table A.8, Panel B)	54.59	27.82	43.75	32.16
	(114.4%)	(58.3%)	(91.7%)	(67.4%)

Notes: VOT is computed as $60(k/\beta)$, where β is our estimated coefficient on Daisy and k is the notch price. All VOT estimates are in 2014 adjusted USD. Percentages in parentheses represent VOT as a proportion of the hourly wage, defined as $99,214 / 2080.

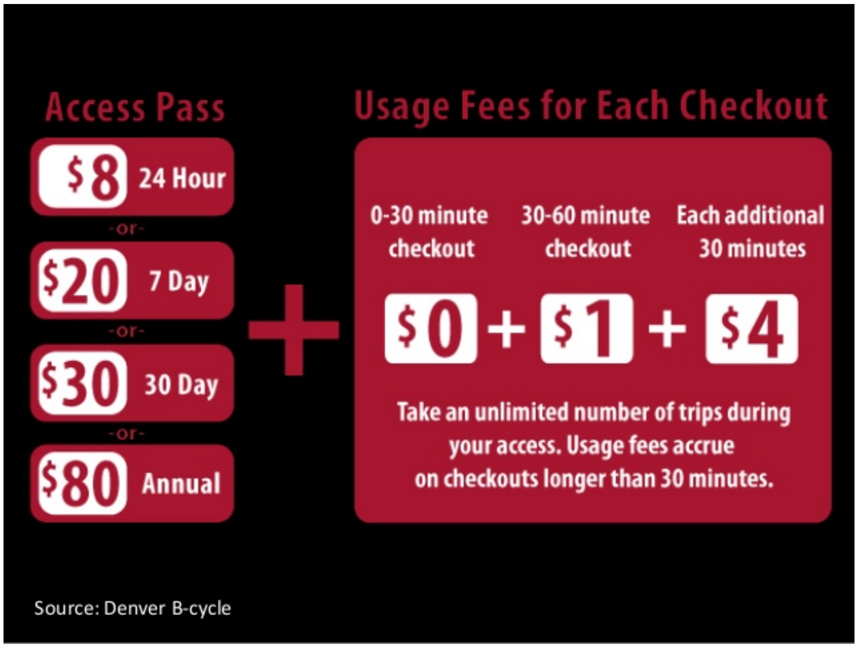

Figure 1: Example of Denver B-cycle's pricing structure in the fall of 2016.

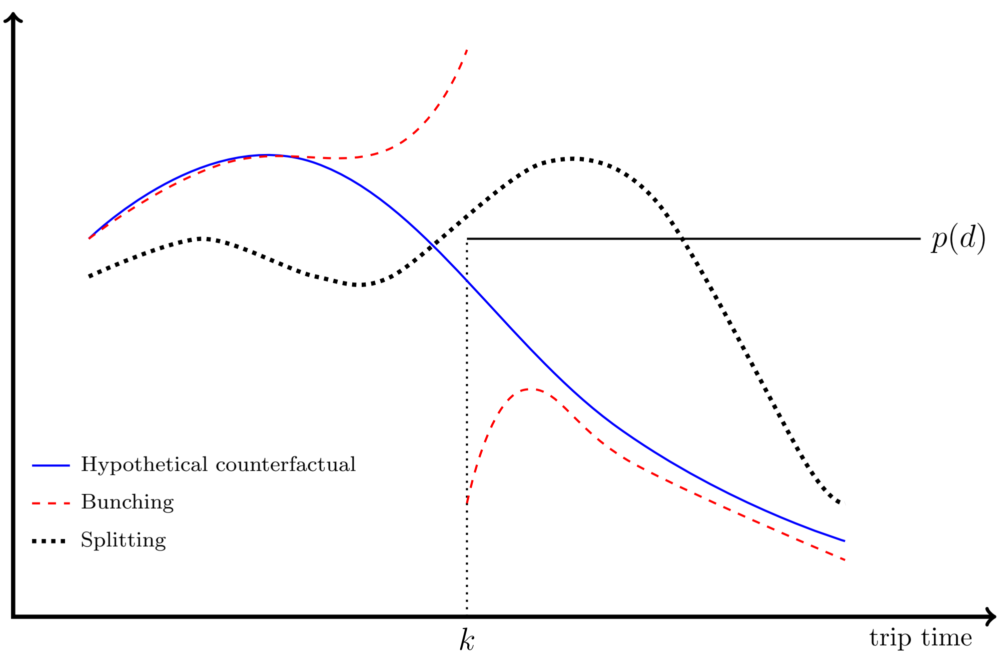

Figure 2: Stylized densities for alternative models of consumer behavior in response to a notched price schedule $p(d)$.

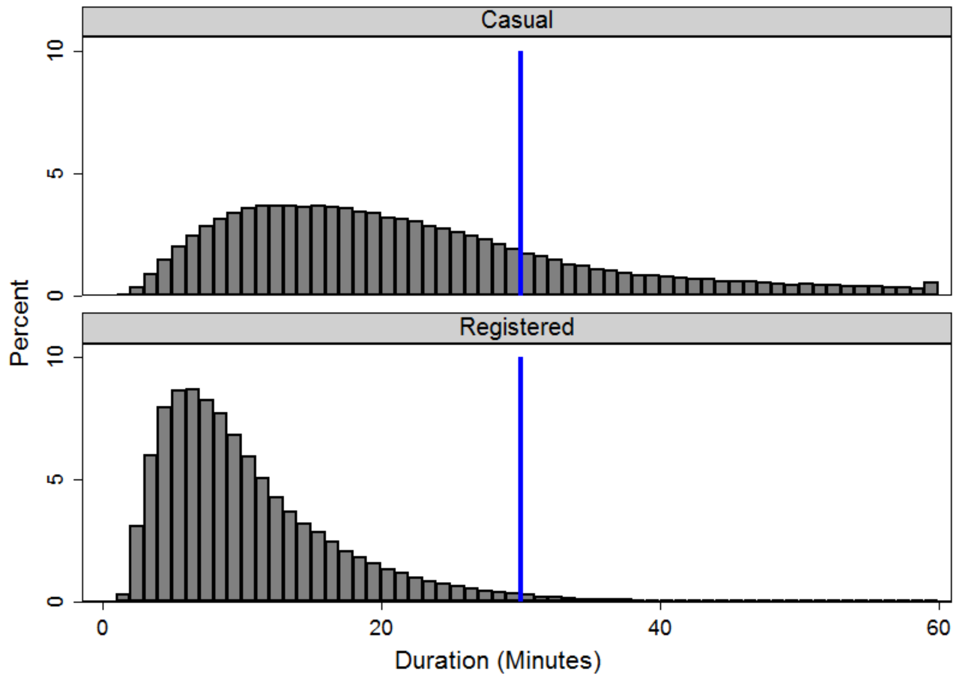

Figure 3: Distributions of trip time by member type from raw data. Each distribution is truncated at 60 minutes. All observations are pre-Oct. 2015 price change.

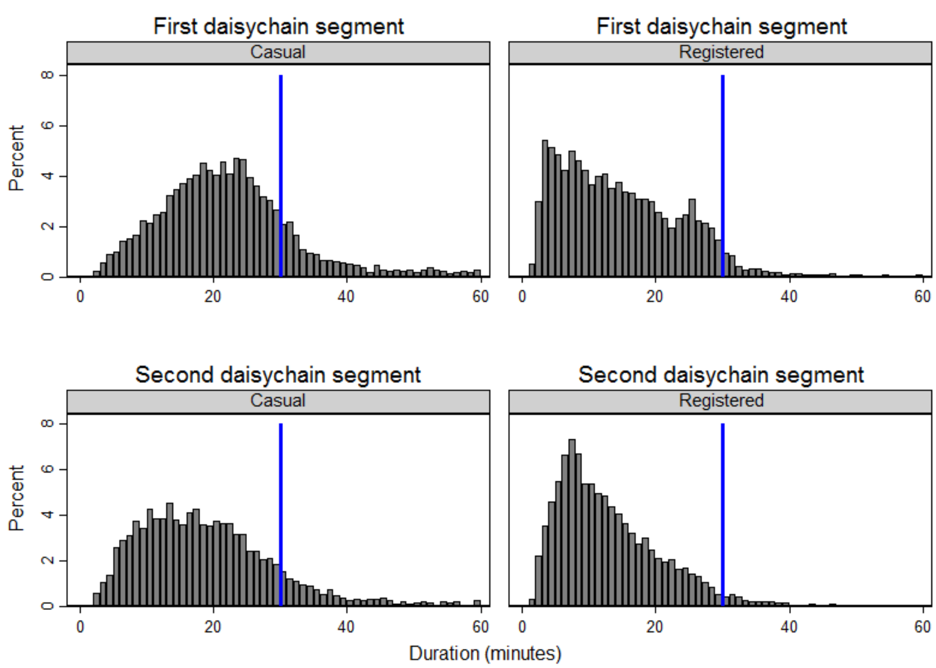

Figure 4: Distributions of trip time by daisychain segment and member type. Each distribution is truncated at 60 minutes. All observations are pre-Oct. 2015 price change.

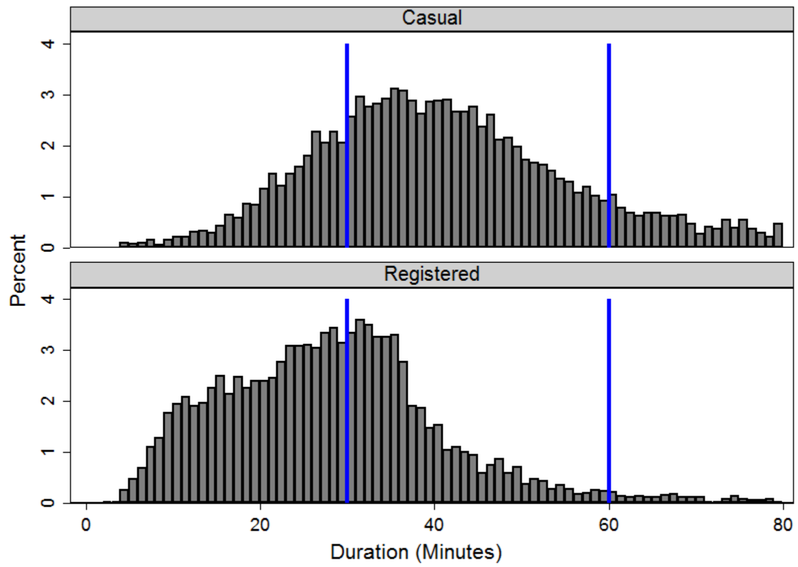

Figure 5: Distributions of daisychain trip time by member type. Each distribution is truncated at 80 minutes. All observations are pre-Oct. 2015 price change.

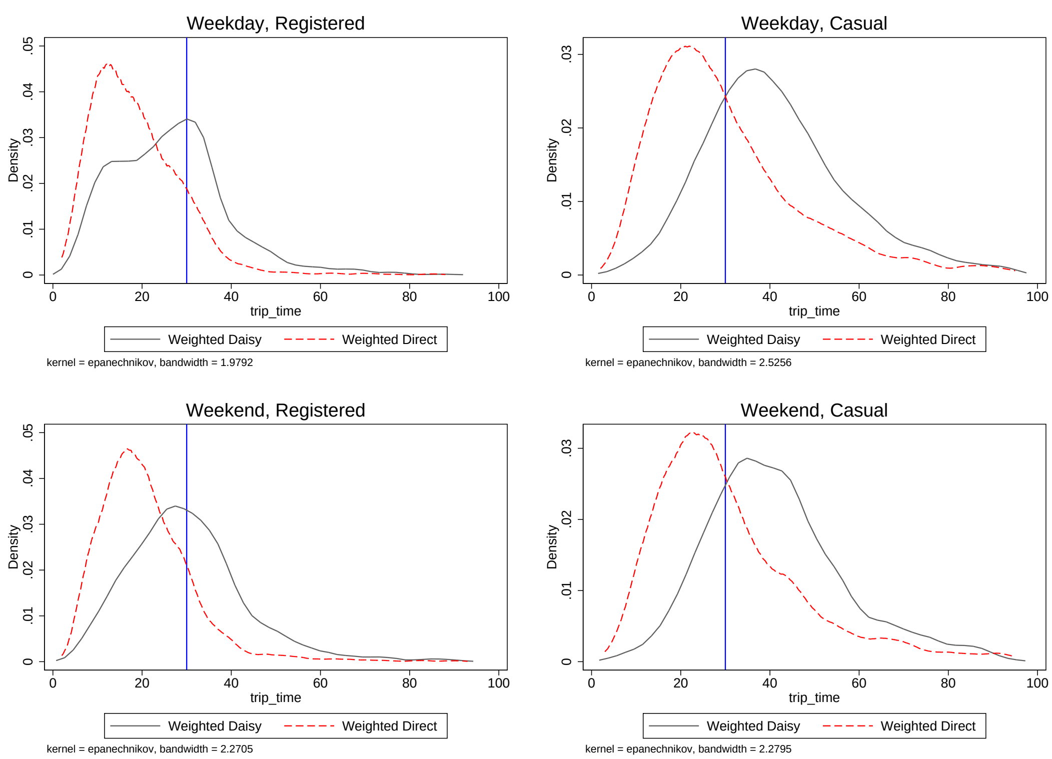

Figure 6: k-density distributions for trip time for each user-time combination after 1:1 Mahalanobis matching without replacement for key variables. All observations are pre-Oct. 2015 price change.

A Appendix A

FOR ONLINE PUBLICATION:

Additional results for:

Notching for free: Do cyclists reveal the value of time?

Casey J. Wichman and Brandon Cunningham

September 6, 2017

Table A.1: Covariate balance for registered users from 1:1 nearest neighbor matching without replacement with calipers equal to 0.25 standard deviations

	Weekday		Weekend	
	Full Sample	Matching (Caliper = 0.25σ)	Full Sample	Matching (Caliper = 0.25σ)
Trip Time (minutes)				
Mean difference	16.893	6.933	17.790	8.075
Std. mean difference	131.405	56.520	134.157	63.906
Mean raw eQQ difference	16.887	6.933	17.779	8.103
Variance ratio	3.283	2.422	2.206	1.646
Google Distance (miles)				
Mean difference	1.822	0.045	1.599	0.050
Std. mean difference	101.001	3.722	111.669	4.569
Mean raw eQQ difference	1.822	0.046	1.598	0.053
Variance ratio	4.102	0.990	2.032	1.004
Start Latitude				
Mean difference	-0.004	< 0.001	-0.001	< 0.001
Std. mean difference	-20.054	-0.325	-7.788	-0.084
Mean raw eQQ difference	0.004	< 0.001	0.003	< 0.001
Variance ratio	2.205	0.993	1.735	0.995
Start Longitude				
Mean difference	0.003	< 0.001	0.001	< 0.001
Std. mean difference	15.014	-0.296	2.727	-0.217
Mean raw eQQ difference	0.004	< 0.001	0.003	< 0.001
Variance ratio	1.575	0.997	1.440	0.996
End Latitude				
Mean difference	-0.007	< 0.001	-0.002	< 0.001
Std. mean difference	-29.814	-0.381	-11.925	0.079
Mean raw eQQ difference	0.007	< 0.001	0.004	< 0.001
Variance ratio	2.804	0.994	1.872	0.990
End Longitude				
Mean difference	0.006	< 0.001	0.002	< 0.001
Std. mean difference	29.924	0.164	8.878	-0.440
Mean raw eQQ difference	0.007	< 0.001	0.004	< 0.001
Variance ratio	1.869	0.996	1.657	0.999
Month				
Mean difference	-0.082	0	-0.158	0
Std. mean difference	-3.088	0	-5.942	0
Mean raw eQQ difference	0.228	0	0.157	0
Variance ratio	0.876	1	0.947	1
Start Hour				
Mean difference	1.184	0.028	-0.195	0
Std. mean difference	27.178	0.639	-5.507	0
Mean raw eQQ difference	1.185	0.055	0.452	0
Variance ratio	0.881	1.014	0.81	1

Notes: For each covariate, we evaluate the improvement in covariate balance in four ways (Lee, 2013): (i) difference in means; (ii) standardized mean difference (Rosenbaum and Rubin (1985) suggest that a standardized difference greater than 20 should be considered large); (iii) eQQ mean difference, a nonparametric measure that evaluates rank rather than the precise value of the observations (Ho et al., 2007); and (iv) variance ratio between treated and untreated units (Sekhon, 2008). All observations are pre-Oct. 2015 price change.

Table A.2: Covariate balance for casual users from 1:1 nearest neighbor matching without replacement with calipers equal to 0.25 standard deviations

	Weekday		Weekend	
	Full Sample	Matching (Caliper = 0.25σ)	Full Sample	Matching (Caliper = 0.25σ)
Trip Time (minutes)				
Mean difference	18.308	10.689	16.574	9.594
Std. mean difference	115.400	65.934	107.093	62.946
Mean raw eQQ difference	18.302	10.695	16.589	9.720
Variance ratio	0.903	0.938	0.828	0.824
Google Distance (miles)				
Mean difference	1.276	0.084	1.249	0.080
Std. mean difference	89.378	7.948	84.763	7.036
Mean raw eQQ difference	1.275	0.084	1.248	0.081
Variance ratio	1.532	0.998	1.421	0.994
Start Latitude				
Mean difference	-0.001	< 0.001	-0.003	< 0.001
Std. mean difference	-9.255	-1.070	-14.954	-0.418
Mean raw eQQ difference	0.002	< 0.001	0.003	< 0.001
Variance ratio	1.400	0.992	1.398	0.981
Start Longitude				
Mean difference	0.003	< 0.001	0.004	< 0.001
Std. mean difference	13.459	-0.618	19.238	0.187
Mean raw eQQ difference	0.003	< 0.001	0.004	< 0.001
Variance ratio	1.370	1.002	1.326	0.997
End Latitude				
Mean difference	-0.002	< 0.001	-0.001	< 0.001
Std. mean difference	-10.890	-1.242	-8.542	-0.295
Mean raw eQQ difference	0.002	< 0.001	0.001	< 0.001
Variance ratio	1.396	0.989	1.288	0.997
End Longitude				
Mean difference	0.002	< 0.001	0.002	< 0.001
Std. mean difference	12.335	0.283	10.779	-0.334
Mean raw eQQ difference	0.003	< 0.001	0.003	< 0.001
Variance ratio	1.417	0.998	1.387	1.001
Month				
Mean difference	-0.036	0	0.091	0
Std. mean difference	-1.533	0	3.674	0
Mean raw eQQ difference	0.062	0	0.115	0
Variance ratio	1.028	1	1.027	1
Start Hour				
Mean difference	-0.366	0	-0.488	0
Std. mean difference	-10.753	0	-15.658	0
Mean raw eQQ difference	0.627	0	0.527	0
Variance ratio	0.740	1	0.809	1

Notes: For each covariate, we evaluate the improvement in covariate balance in four ways (Lee, 2013): (i) difference in means; (ii) standardized mean difference (Rosenbaum and Rubin (1985) suggest that a standardized difference greater than 20 should be considered large); (iii) eQQ mean difference, a nonparametric measure that evaluates rank rather than the precise value of the observations (Ho et al., 2007); and (iv) variance ratio between treated and untreated units (Sekhon, 2008). All observations are pre-Oct. 2015 price change.

Table A.3: Regression results from panel models on weighted samples with caliper width equal to 0.25 standard deviations

	Weekday		Weekend	
	Registered	Casual	Registered	Casual
Panel A: Subscriber fixed effects on weighted sample				
Daisy	5.126	7.645	6.414	13.203
	(0.769)	(4.171)	(1.331)	(6.235)
Google distance	5.300	4.712	5.194	4.361
	(0.382)	(3.055)	(0.743)	(3.270)
Obs.	4,612	3,074	1,416	2,812
R-squared	0.718	0.968	0.883	0.977
Panel B: Route fixed effects on weighted sample				
Daisy	5.220	9.539	7.495	9.201
	(0.536)	(1.178)	(1.002)	(1.127)
Google distance	10.526	6.928	1.733	6.727
	(2.363)	(5.084)	(4.737)	(4.547)
Obs.	4,612	3,074	1,416	2,812
R-squared	0.724	0.636	0.784	0.646
Panel C: IV models with route fixed effects on weighted sample				
Daisy	4.741	9.458	8.640	12.501
	(2.063)	(3.053)	(2.305)	(4.372)
Google Distance	11.279	7.050	-0.492	1.955
	(3.647)	(6.151)	(5.362)	(6.865)
First-stage F-stat	57.30	1410.72	25.26	66.84
Obs.	3,952	2,670	1,022	2,487
R-squared	0.197	0.162	0.265	0.149

Notes: Dependent variable in all models is trip time (in minutes). All observations are pre-Oct. 2015 price change. All models include controls for weather, month fixed effects, and hour-of-day fixed effects. All panels present results from linear panel models on a weighted sample using 1:1 Mahalanobis matching without replacement. In Panel C, Daisy is instrumented for using indicators for 10-minute bins of expected trip duration as described in Equation 3. Robust standard errors clustered at the subscriber level (Panel A) and route level (Panels B and C) are presented in parentheses.

Table A.4: Regression results with route fixed effects and distance heterogeneity

	Weekday		Weekend	
	Registered	Casual	Registered	Casual
Daisy × 1{0–2 miles}	5.730	10.684	7.907	11.658
	(0.626)	(1.589)	(1.414)	(1.378)
Daisy × 1{2–4 miles}	5.754	7.831	6.938	7.619
	(0.939)	(1.505)	(1.428)	(1.712)
Daisy × 1{4+ miles}	0.881	10.784	8.038	1.851
	(0.919)	(3.072)	(4.040)	(2.915)
Google distance	11.938	7.600	1.718	8.234
	(2.368)	(5.001)	(4.779)	(4.390)
Obs.	4,612	3,074	1,416	2,812
R-squared	0.728	0.637	0.784	0.651

Notes: Dependent variable in all models is trip time (in minutes). All observations are pre-Oct. 2015 price change. All models include controls for weather, month fixed effects, and hour-of-day fixed effects. Interactions with Daisy are for routes that are between 0 and 2 miles, 2 and 4 miles, and greater than 4 miles, respectively. Results are from linear panel models on a weighted sample using 1:1 Mahalanobis matching without replacement. Caliper width is one standard deviation. Robust standard errors clustered at the route level are presented in parentheses.

Table A.5: Covariate balance for registered users from 1:1 nearest neighbor matching without replacement (post Oct. 2015 price change observations only)

	Weekday		Weekend	
	Full Sample	Matching (Caliper = 1σ)	Full Sample	Matching (Caliper = 1σ)
Trip Time (minutes)				
Mean difference	17.021	7.626	17.801	9.161
Std. mean difference	133.445	59.672	134.055	69.615
Mean raw eQQ difference	17.016	7.628	17.79	9.162
Variance ratio (Treat/Comp.)	3.2	1.594	2.173	1.547
Google Distance (miles)				
Mean difference	1.857	0.117	1.599	0.146
Std. mean difference	102.317	6.882	111.48	10.497
Mean raw eQQ difference	1.856	0.117	1.598	0.146
Variance ratio (Treat/Comp.)	4.084	1.048	2.023	1.06
Start Latitude				
Mean difference	-0.004	0	-0.001	0
Std. mean difference	-20.257	0.021	-7.951	1.096
Mean raw eQQ difference	0.004	0	0.003	0
Variance ratio (Treat/Comp.)	2.23	0.995	1.728	1.001
Start Longitude				
Mean difference	0.003	0	0.001	0
Std. mean difference	14.898	-0.108	2.987	-0.84
Mean raw eQQ difference	0.004	0	0.003	0
Variance ratio (Treat/Comp.)	1.57	0.999	1.453	1.005
End Latitude				
Mean difference	-0.007	0	-0.002	0
Std. mean difference	-30.135	-0.185	-10.791	0.201
Mean raw eQQ difference	0.007	0	0.004	0
Variance ratio (Treat/Comp.)	2.852	1.004	1.84	1.006
End Longitude				
Mean difference	0.006	0	0.002	0
Std. mean difference	29.888	0.305	7.638	0.323
Mean raw eQQ difference	0.007	0	0.004	0
Variance ratio (Treat/Comp.)	1.867	0.985	1.666	1.018
Month				
Mean difference	0.023	-0.015	-0.145	-0.005
Std. mean difference	0.885	-0.599	-5.626	-0.187
Mean raw eQQ difference	0.198	0.042	0.145	0.067
Variance ratio (Treat/Comp.)	0.888	1.038	0.939	1.055
Start Hour				
Mean difference	1.169	-0.022	-0.192	0.031
Std. mean difference	26.874	-0.518	-5.411	0.872
Mean raw eQQ difference	1.17	0.102	0.449	0.078
Variance ratio (Treat/Comp.)	0.876	1.028	0.813	1.034

Notes: For each covariate, we evaluate the improvement in covariate balance in four ways (Lee, 2013): (i) difference in means; (ii) standardized mean difference (Rosenbaum and Rubin (1985) suggest that a standardized difference greater than 20 should be considered large); (iii) eQQ mean difference, a nonparametric measure that evaluates rank rather than the precise value of the observations (Ho et al., 2007); and (iv) variance ratio between treated and untreated units (Sekhon, 2008).

Table A.6: Covariate balance for casual users from 1:1 nearest neighbor matching without replacement (post Oct. 2015 price change observations only)

	Weekday		Weekend	
	Full Sample	Matching (Caliper = 1σ)	Full Sample	Matching (Caliper = 1σ)
Trip Time (minutes)				
Mean difference	18.337	11.354	16.636	10.586
Std. mean difference	115.017	72.108	106.828	68.472
Mean raw eQQ difference	18.331	11.355	16.655	10.641
Variance ratio (Treat/Comp.)	0.904	0.901	0.834	0.86
Google Distance (miles)				
Mean difference	1.282	0.164	1.225	0.178
Std. mean difference	89.219	11.964	83.209	12.456
Mean raw eQQ difference	1.281	0.164	1.224	0.178
Variance ratio (Treat/Comp.)	1.543	1.061	1.418	1.063
Start Latitude				
Mean difference	-0.002	0	-0.002	0
Std. mean difference	-9.696	0.461	-13.216	0.326
Mean raw eQQ difference	0.002	0	0.002	0
Variance ratio (Treat/Comp.)	1.425	0.994	1.357	0.988
Start Longitude				
Mean difference	0.003	0	0.004	0
Std. mean difference	13.581	-0.086	18.181	0.003
Mean raw eQQ difference	0.003	0	0.004	0
Variance ratio (Treat/Comp.)	1.379	0.994	1.333	0.993
End Latitude				
Mean difference	-0.002	0	-0.001	0
Std. mean difference	-10.882	-0.731	-6.932	-0.394
Mean raw eQQ difference	0.002	0	0.001	0
Variance ratio (Treat/Comp.)	1.38	0.987	1.265	0.993
End Longitude				
Mean difference	0.002	0	0.002	0
Std. mean difference	12.411	0.272	9.584	-0.015
Mean raw eQQ difference	0.003	0	0.003	0
Variance ratio (Treat/Comp.)	1.413	1.003	1.367	1
Month				
Mean difference	-0.11	0.007	0.012	-0.013
Std. mean difference	-4.878	0.302	0.515	-0.55
Mean raw eQQ difference	0.121	0.046	0.057	0.066
Variance ratio (Treat/Comp.)	0.997	1.054	1.003	1.066
Start Hour				
Mean difference	-0.361	0.004	-0.476	-0.006
Std. mean difference	-10.535	0.117	-15.115	-0.191
Mean raw eQQ difference	0.617	0.048	0.518	0.07
Variance ratio (Treat/Comp.)	0.746	1.022	0.822	1.056

Notes: For each covariate, we evaluate the improvement in covariate balance in four ways (Lee, 2013): (i) difference in means; (ii) standardized mean difference (Rosenbaum and Rubin (1985) suggest that a standardized difference greater than 20 should be considered large); (iii) eQQ mean difference, a nonparametric measure that evaluates rank rather than the precise value of the observations (Ho et al., 2007); and (iv) variance ratio between treated and untreated units (Sekhon, 2008).

Table A.7: Regression results from linear models on full and weighted samples (post Oct. 2015 price change observations only)

	Weekday		Weekend	
	Registered	Casual	Registered	Casual
Panel A: Linear models on full sample				
Daisy	8.416	14.248	9.511	11.929
	(0.360)	(0.455)	(0.550)	(0.489)
Google distance	4.996	5.080	5.120	4.846
	(0.078)	(0.112)	(0.080)	(0.126)
Obs.	120,212	86,369	22,219	39,804
R-squared	0.441	0.299	0.477	0.247
Panel B: Linear models on weighted sample				
Daisy	7.378	11.484	9.080	10.424
	(0.394)	(0.518)	(0.655)	(0.613)
Google distance	5.099	4.341	4.086	4.128
	(0.192)	(0.259)	(0.270)	(0.243)
Obs.	2,364	4,308	1,060	4,001
R-squared	0.435	0.316	0.380	0.268

Notes: Dependent variable in all models is trip time (in minutes). All models include controls for weather, month fixed effects, and hour-of-day fixed effects. Panel A presents results from linear models on the full sample. Panel B presents results from linear models on a weighted sample using 1:1 Mahalanobis matching without replacement. Robust standard errors clustered at the route level are presented in parentheses.

Table A.8: Regression results from panel models on weighted samples (post Oct. 2015 price change observations only)

	Weekday		Weekend	
	Registered	Casual	Registered	Casual
Panel A: Subscriber fixed effects on weighted sample				
Daisy	5.781	11.508	7.884	14.343
	(0.746)	(2.314)	(1.362)	(2.388)
Google distance	4.933	4.501	2.951	4.369
	(0.415)	(0.699)	(0.649)	(0.827)
Obs.	2,364	4,308	1,060	4,001
R-squared	0.753	0.868	0.804	0.896
Panel B: Route fixed effects on weighted sample				
Daisy	5.496	10.785	6.857	9.328
	(0.699)	(0.924)	(2.281)	(1.264)
Google distance	9.131	6.916	8.622	2.564
	(1.867)	(1.677)	(8.092)	(2.005)
Obs.	2,364	4,346	1,060	4,001
R-squared	0.828	0.758	0.876	0.761
Panel C: IV models with route fixed effects on weighted sample				
Daisy	10.136	12.963	9.978	18.495
	(1.735)	(2.180)	(2.380)	(3.273)
Google distance	6.735	5.705	6.349	-4.428
	(1.503)	(1.642)	(4.587)	(3.248)
First-stage F-stat	36.42	36.92	12.13	25.04
Obs.	1,642	3,430	558	3,346
R-squared	0.181	0.246	0.313	0.085

Notes: Dependent variable in all models is trip time (in minutes). All models include controls for weather, month fixed effects, and hour-of-day fixed effects. All panels present results from linear panel models on a weighted sample using 1:1 Mahalanobis matching without replacement. In Panel C, Daisy is instrumented for using indicators for 10-minute bins of expected trip duration as described in Equation 3. Robust standard errors clustered at the subscriber level (Panel A) and route level (Panels B and C) are presented in parentheses.

Table A.9: Regression results with route fixed effects and time-of-day heterogeneity (post Oct. 2015 price change observations only)

	Weekday		Weekend	
	Registered	Casual	Registered	Casual
Daisy × Morning	4.440	10.451	7.059	3.988
	(1.574)	(2.991)	(6.048)	(6.840)
Daisy × Afternoon	7.030	11.145	5.441	9.883
	(1.680)	(1.338)	(3.922)	(1.687)
Daisy × Evening	5.623	10.462	8.325	8.778
	(0.906)	(1.478)	(3.430)	(1.537)
Daisy × Night	4.151	14.441	3.900	13.195
	(1.718)	(3.894)	(6.474)	(4.084)
Google distance	8.920	6.820	9.024	2.483
	(1.871)	(1.690)	(8.210)	(2.009)
Obs.	2,364	4,308	1,060	4,001
R-squared	0.829	0.759	0.877	0.762

Notes: Dependent variable in all models is trip time (in minutes). All models include controls for weather, month fixed effects, and hour-of-day fixed effects. Morning = 1 if the trip began between 6:00AM and 9:59AM. Afternoon = 1 if the trip began between 10:00AM and 2:59PM. Evening = 1 if the trip began between 3:00PM and 7:59PM. Night = 1 if the trip began at or after 8:00PM. Results are from linear panel models on a weighted sample using 1:1 Mahalanobis matching without replacement. Robust standard errors clustered at the route level are presented in parentheses.

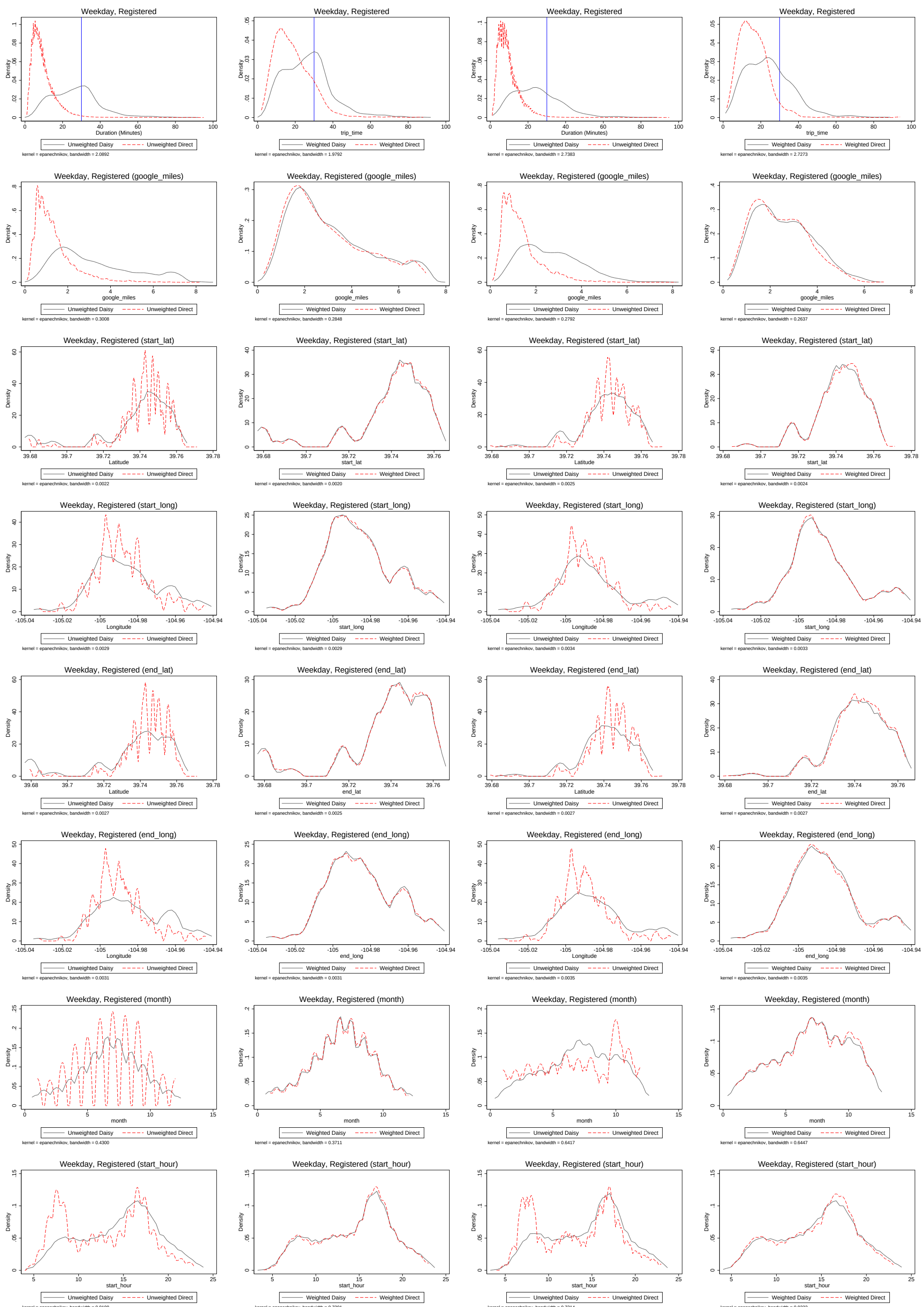

Figure A.1: k-density distributions for **registered users on weekdays** before and after 1:1 Mahalanobis matching without replacement for key variables. Repeated observations are taken into account with frequency weights. First column is raw data for pre-Oct. 2015. Second column is matched data for pre-Oct. 2015. Third column is raw data for post-Oct. 2015. Fourth column is matched data for post-Oct. 2015.

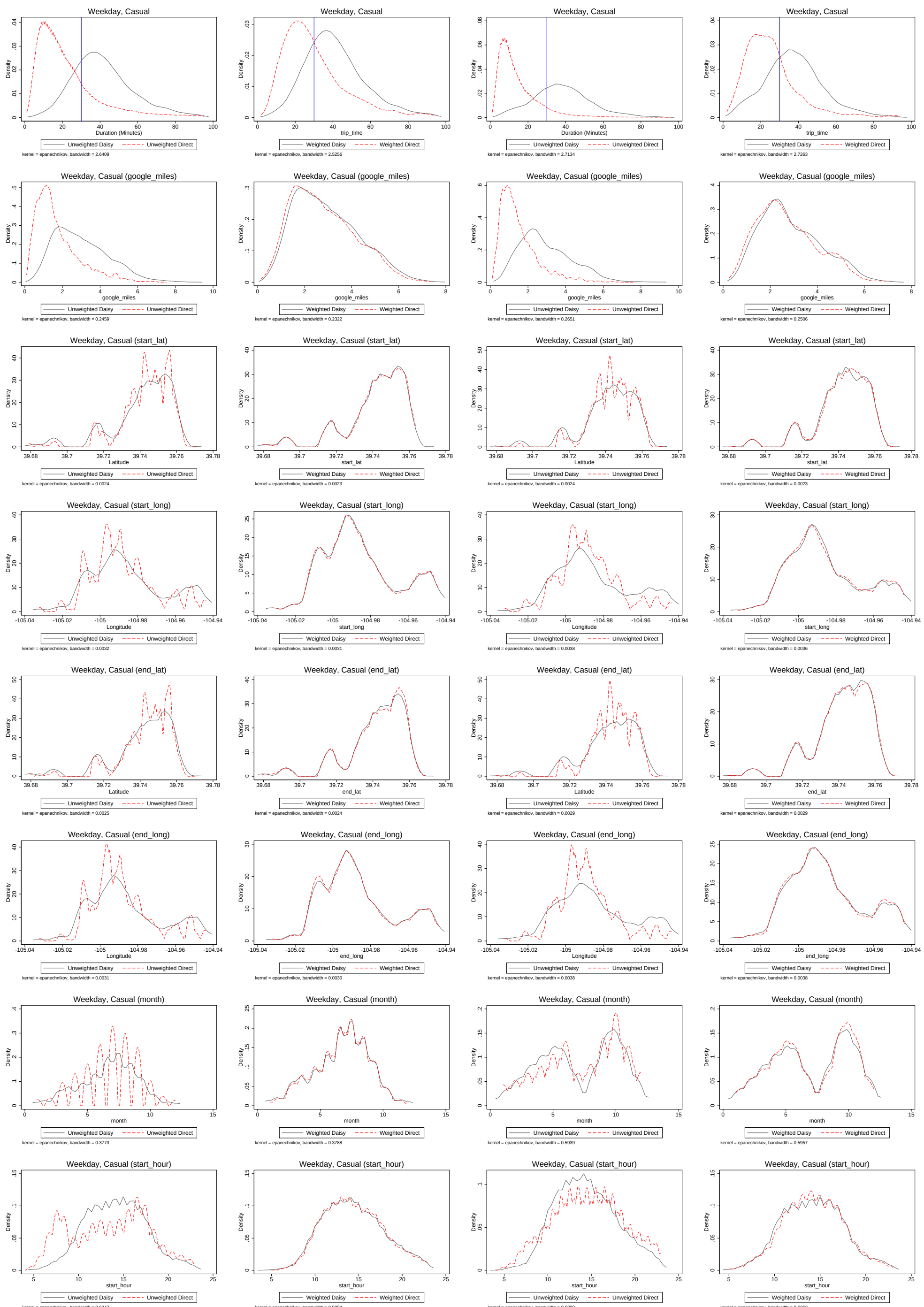

Figure A.2: k-density distributions for **registered users on weekends** before and after 1:1 Mahalanobis matching without replacement for key variables. Repeated observations are taken into account with frequency weights. First column is raw data for pre-Oct. 2015. Second column is matched data for pre-Oct. 2015. Third column is raw data for post-Oct. 2015. Fourth column is matched data for post-Oct. 2015.

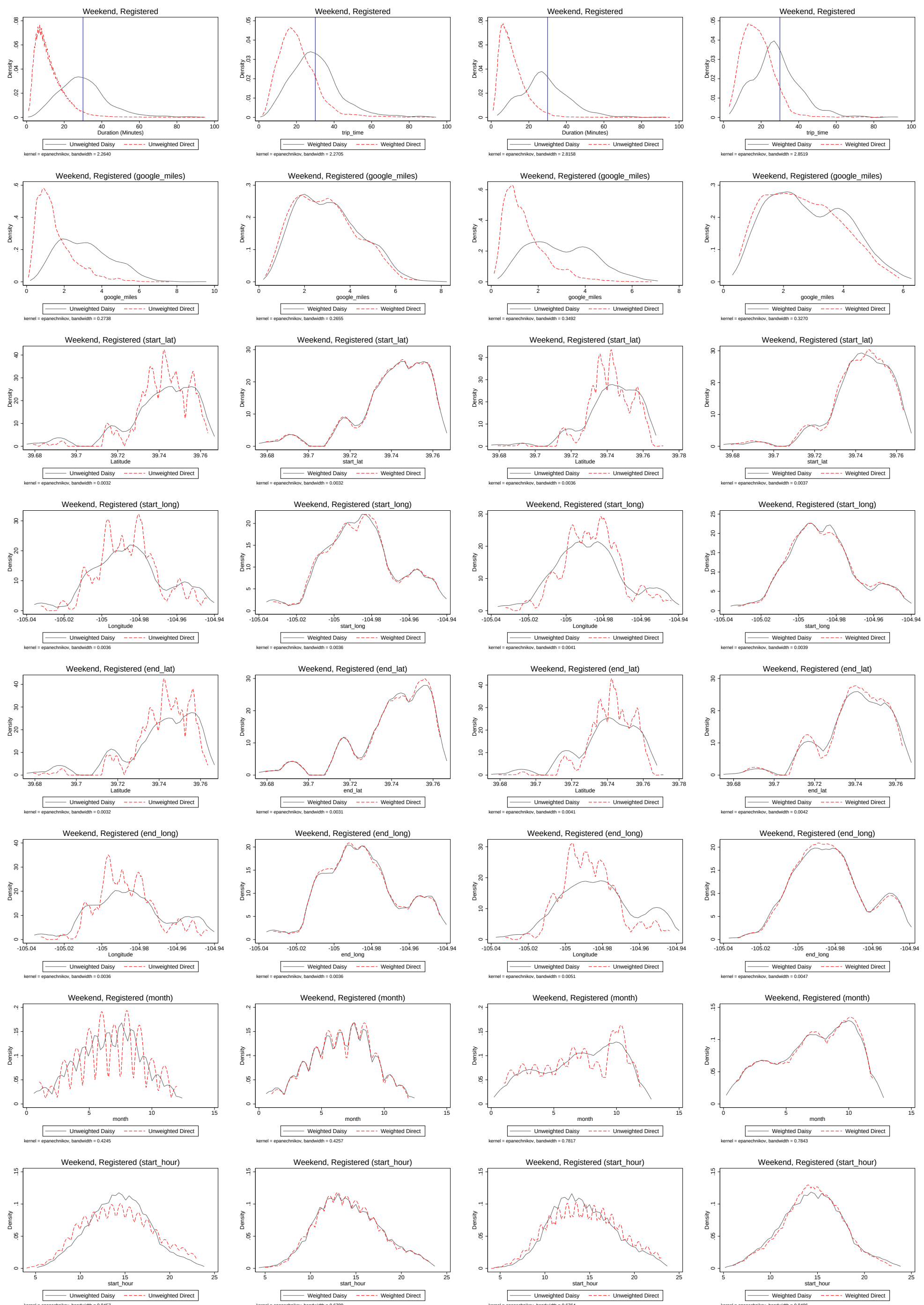

Figure A.3: k-density distributions for **casual users on weekdays** before and after 1:1 Mahalanobis matching without replacement for key variables. Repeated observations are taken into account with frequency weights. First column is raw data for pre-Oct. 2015. Second column is matched data for pre-Oct. 2015. Third column is raw data for post-Oct. 2015. Fourth column is matched data for post-Oct. 2015.

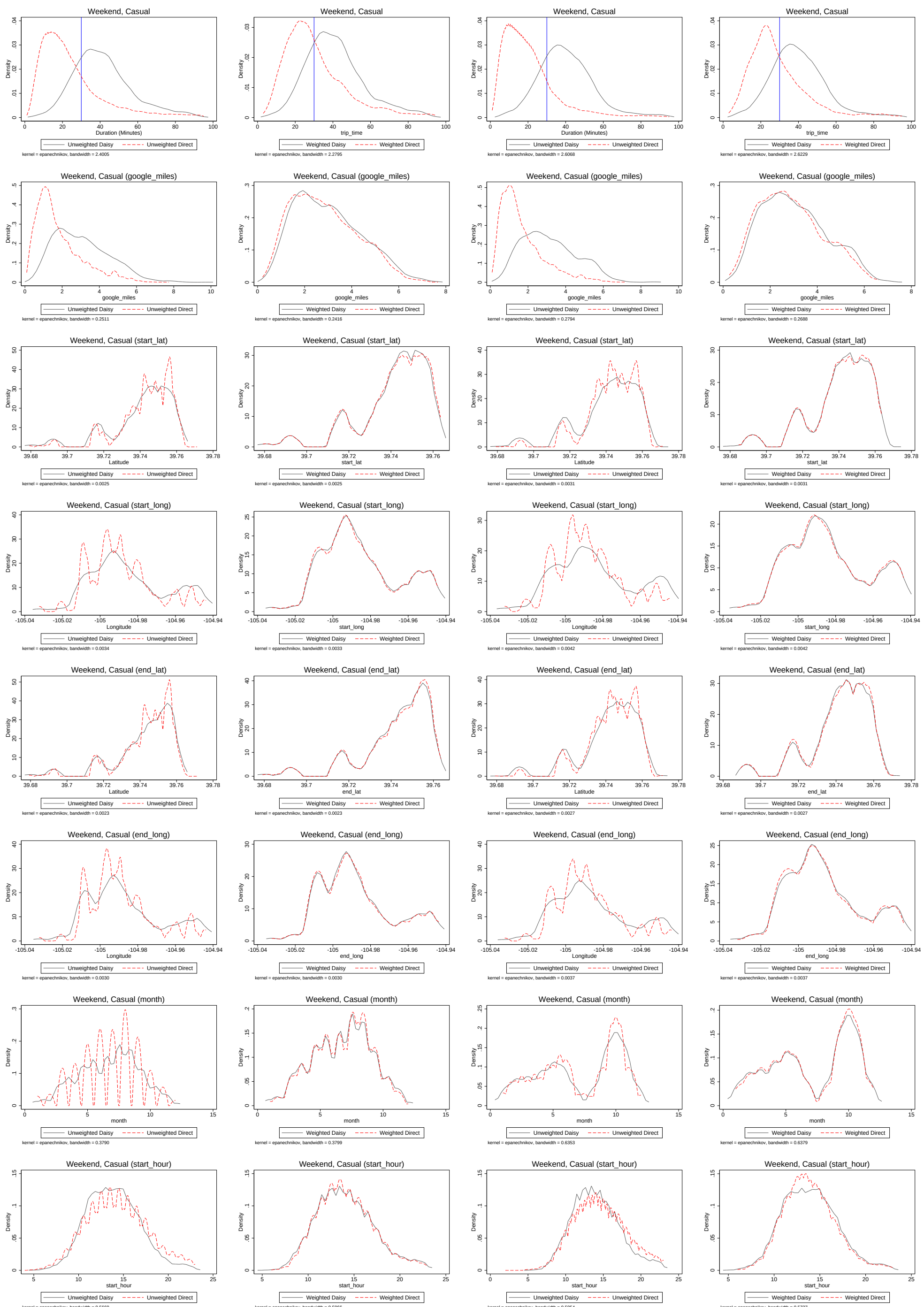

Figure A.4: k-density distributions for **casual users on weekends** before and after 1:1 Mahalanobis matching without replacement for key variables. Repeated observations are taken into account with frequency weights. First column is raw data for pre-Oct. 2015. Second column is matched data for pre-Oct. 2015. Third column is raw data for post-Oct. 2015. Fourth column is matched data for post-Oct. 2015.