An Evaluation of Pittsburgh's Dynamically-Priced Curb Parking Pilot

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By

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Abstract

This paper analyzes a small, dynamically-priced curb parking pilot that took place in Pittsburgh, PA from 2013 to 2015. Dynamic pricing of curb parking is a recent innovation – one which is designed to manage parking congestion through price manipulation in order to optimize occupancy and reduce traffic congestion. I find that prices declined during the pilot, revenues rose and occupancies moved towards target ranges set by program administrators. In the scant few studies of such pricing schemes, disagreement has arisen amongst scholars as to whether elasticities are appropriate to measure the effect of price changes on driver behavior. This paper demonstrates the use of the regression discontinuity statistical model in estimating the effects of price change effects on driver behavior. Regression discontinuity models suggest that prices had the intended effect on driver behavior during the Pittsburgh pilot, but such effects took a couple weeks to develop.

Introduction

In October of 2014, Pittsburgh's City Council authorized the Pittsburgh Parking Authority to use a "dynamic pricing model" to set parking prices city-wide. The perceived success of a small pilot undertaken starting in January, 2013 was the basis for Pittsburgh's city-wide legislation (Zullo 2014). In theory, the dynamic pricing model works as follows: parking demand and supply will equilibrate so that there are always at least a few spaces available on each block, minimizing the time it takes to find parking and minimizing congestion and pollution (Shoup 2004). Pittsburgh joins San Francisco, Los Angeles and a few others in using this new way of pricing street parking. With few pilots completed, and fewer studied intensely, more information is needed to allow implementation of dynamic, demand-based parking with a thorough understanding of probable effects. Do the programs work as intended? Do they vary from place-to-place? How is demand affected by price changes? How frequently should prices change? By how much?

This study uncovers more evidence of the effects of market-based curb parking schemes by examining Pittsburgh's pilot program. I conclude that the pilot worked as intended- overall revenues rose, occupancy moved towards target ranges on most streets and price changes had the intended impact on driver behavior. Drivers' general price sensitivity was similar to that measured in other studies. Furthermore, this analysis demonstrates the utility of the regression discontinuity (RD) statistical model in analyzing the causal effects of price changes in the absence of robust controls. Furthermore, I use the RD model to show that the effect of price changes on driver behavior were larger several weeks after the changes took place – suggesting a lag in consumer response to price changes. However, the robustness of these findings is diminished by the fact that application of price changes by administrators was haphazard and resulted in decreased samples sizes in RD models.

By comparing and contrasting the dynamics of this pilot program to those of other pilots, specifically San Francisco's *SFPark*, a more nuanced picture of dynamic, demand-based parking pricing begins to emerge. This information is then used to form the basis for some observations about generalizability and program design and administration. I conclude by offering recommendations to those planners interested in creating and operating demand-based-priced parking programs.

Cruising - the impetus for demand-based pricing of on-street parking

For decades, planning scholars have argued that curb parking tends to be underpriced and therefore overcrowded. Donald Shoup summarized the history of literature on the topic in his tome *The High Cost of Free Parking* (2004)*:* When curbside parking spaces are overcrowded, and vacant spaces are unavailable, drivers "cruise" in search of parking. Cruising creates negative externalities including congestion, air pollution and unnecessary fuel consumption. Furthermore, congestion prompted the creation of minimum parking requirements for most types of zoning districts. Shoup argues persuasively (in *High Cost* and elsewhere) that these minimums are inefficient (Shoup 1999) (Pierce and Shoup 2013).

In various studies undertaken during the past 90 years, cruising has been observed to comprise a large proportion of overall city traffic. The average percentage of cruising as a proportion of overall traffic observed in 10 studies from 1927-2011 was 34% (Shoup 2004). As long ago as 1954, Nobel-winner William Vickrey suggested that curb prices could be manipulated in order to suppress or stimulate demand with the goal of ensuring enough parking availability to eliminate or decrease cruising (Vickrey 1954). Traffic engineers have determined that approximately 80-85% average occupancy represents an optimal point at which one space per block should be available (Shoup 2004) (Millard-Ball, Weinberger and Hampshire 2014). Arnott and Inci created several theoretical models of parking and congestion (2006). Their most "robust" model suggested that "it is efficient to raise the on-street parking fee to the point where cruising for parking is eliminated." The alternative would be to increase the amount of parking to achieve the same effect – an inefficient solution for reasons explained by Shoup (2004). Parking minimums create serious land market distortions and promote sprawl and automobile-centric development. Brooke, Ison and Quddus (2014) surveyed street parking-related literature. They suggest that there is agreement that setting adjacent on- and off-street parking to demand-based prices in concert could eliminate cruising.

Survey of existing pilot programs and related research

The emergence of digital parking meters now allows for demand-based price manipulation to achieve desired occupancy or vacancy levels. Furthermore, since modern kiosks accept credit cards, prices can be raised to levels which would have previously required drivers to carry onerous amounts of coinage. Recently, there have been several federally funded pilot programs demonstrating the dynamic pricing concept (US Department of Transportation 2015). Due to the novelty of such programs, research regarding the effects of demand-based price regimes is scant.

At present, academic literature appears to be limited to the cases of San Francisco, Seattle and Los Angeles. Pittsburgh's pilot program was conducted by two Carnegie Mellon professors as a demonstration, and as of this writing, has not been analyzed in a peer reviewed study.

The four pilots differed in small ways. They each used different heuristic occupancy targets, price change intervals and price increments. Some cities used tech tools to communicate rates to drivers. Some varied price by time of day. Some cities had sensors to measure occupancy, some used estimates based on transaction data. The parameters and characteristics of these four pilot programs can be compared in Figure 1.

City	Target Occupancy	Price Change Time Interval	Price Increment ($/hr)	Time-of-Day Pricing?	Consumer Price Information App?	Measured or Estimated Occupancy
San Francisco*	60-80%	10 changes (2011-13)	$0.25	Yes	Yes	Measured
Los Angeles**	70-90%	No more than once per month	$1 (or 150% base rate)	Yes	Yes	Measured
Seattle+	71-86%	*Not truly "dynamic" - only one change*	Variable by district	No	No	Estimated
Pittsburgh++	60-80%	No more than once per month	$0.25	No	No	Estimated

Figure 1. Characteristics of selected demand-based pricing pilot programs in the US. All of these programs except Pittsburgh's have been analyzed in peer-reviewed literature. Characteristics of particular programs reflect only the periods under study.
Information is drawn from the following sources:
* (Millard-Ball, Weinberger and Hampshire 2014), (Pierce and Shoup 2013); ** (Ghent 2015);
+ (Ottosson, et al. 2013); ++ (Fichman 2015)

These pilots have been studied with varying degrees of rigor. Seattle's pilot has been econometrically analyzed in the peer reviewed literature (Ottosson, et al. 2013), but was not truly "dynamic" as it had only one price change. The Ottosson, et al, study of Seattle's 2011 demand-based price corrections was done in order to opportunistically exploit a natural experiment brought about by a price change which applied to whole neighborhoods. Much of the heterogeneity amongst these diverse neighborhoods does not appear to have been persuasively controlled. Los Angeles' *LA Express Park* pilot has not yet been subject to an econometric analysis. San Francisco's *SFPark* program is the most thoroughly studied and most programmatically sophisticated of these program. The Pittsburgh pilot is in many ways influenced by, and similar to the *SFPark* design. This analysis will therefore discuss only *SFPark* and the Pittsburgh pilot in detail.

***SFPark* – design, study and criticism**

In 2011, the San Francisco Municipal Transportation Authority (SFMTA) adopted a pricing program known as *SFPark*. This program incorporated the dynamic pricing strategies advocated by Shoup and Vickrey. *SFPark* was funded in part by the US Department of Transportation (US Department of Transportation 2015). The general setup of the program was as follows:

> -256 on-street pilot blocks were established in commercial corridors, some blocks were set up as controls
>
> -On pilot blocks, prices were set for three different "time bands" and priced differently for weekends and weekdays
>
> - When average occupancy rates on a given block fell below 60%, prices were lowered $0.25, when rates rose above 80%, rates were raised $0.25. Rates were changed 10 times during a 2011-2013 study (Millard-Ball, Weinberger and Hampshire 2014).

There is disagreement amongst scholars about how *SFPark*'s prices affected driver behavior and whether the program brought about optimal outcomes. A 2013 paper by Pierce and Shoup reported that parking demand in *SFPark*'s first year tended to be relatively inelastic but varied by location, time, day, initial price and size of price change. Occupancy on over 2/3rds of blocks over or under 60-80% occupancy moved into the target range in Year 1 (Pierce and Shoup 2013). Such an analysis shows *SFPark*'s pricing regime quickly bringing parking occupancy into socially optimal equilibrium. Subsequently, several papers emerged that painted a less rosy picture.

Chatman and Manville conducted a study which logged repeated observations of *SFPark* blocks and found that although occupancy fell as prices rose, overall levels of *availability* (the time during which a space is available on a given block), did not increase (Chatman and

Manville 2014). This perhaps points to a weakness in the occupancy-driven heuristic basis of demand-based pricing models.

Millard-Ball, Weinberger and Hampshire published two papers about *SFPark* – both of which were critical of Pierce and Shoup's research. The first, published in 2014 in *Transportation Research Part A*, concluded that rate changes achieved the SFMTA's occupancy goal over the course of two years. However, they also asserted that Pierce and Shoup's (2013) one-year elasticities were overstated because of a failure to address endogeneity related to price changes. Essentially, they suggest that the circular relationship between price manipulation and demand biases any model used to derive elasticities. Subsequently, Millard-Ball, Weinberger and Hampshire (2014) submitted a published comment in response to the Pierce and Shoup, published in the *Journal of the American Planning Association* (JAPA). They expanded on their previous assertion that endogeneity is present dynamic-pricing elasticity estimations, adding an assertion that the changing conditions which drive traffic to one place or another introduce more bias into these models. Subsequently, they compared the one-year elasticities calculated by Pierce and Shoup (2013) using the *SFPark* data to a random simulation where price was designed to have no impact on demand and observed that Pierce and Shoup's findings were "largely spurious, caused by the statistical phenomenon of reversion to the mean." (Millard-Ball, Weinberger and Hampshire 2014)

One of the primary weaknesses in the *SFPark* design was the location of the control blocks. Most of the "control" areas are in completely different parts of the city from the pilot areas, leading to the possibility of spatial variations that cannot be effectively controlled. For example, the downtown core and touristy Fisherman's Wharf are hard to compare to control areas in low-rise residential Inner Richmond.

Millard-Ball, Weinberger and Hampshire (2014) used a "fuzzy" regression discontinuity model to disaggregate data by geographic area and by individual rate adjustment. Their results confirmed their skepticism that individual price changes in *SFPark* were influencing demand in the short term. The RD design has practical application for analysis of rate changes, particularly in Pittsburgh's pilot area, where the primary purpose was to demonstrate a tool to policy makers, not create an iron-clad experimental design and application of treatment effects was uneven.

As a result of Millard-Ball, Weinberger and Hampshire's demonstration, I decided to use the "sharp" RD model to evaluate the effects of treatment in Pittsburgh. The details of the RD design are discussed towards the end of the following section.

Pittsburgh Data, Empirical Setting and Methods

Description of Pilot Program

In 2012, the City of Pittsburgh raised parking rates in areas adjacent to Carnegie Mellon University (CMU) to $2/hour, doubling the previous price and driving parking occupancy to low levels adjacent to the university. This price change prompted distorted parking behavior, as commuters began parking in far-flung residential areas and began leaving CMU's curbsides under-occupied (Fichman 2015). In September of 2012, the City installed electronic kiosks, meaning that prices could now be more easily manipulated and drivers could pay for parking using credit cards, obviating the need to carry large quantities of coinage. In the Fall of 2012, Steven Spear and Mark Fichman, professors at the Tepper School at CMU persuaded the City of

Pittsburgh to let them administer a dynamic, demand-based pricing pilot on five streets near the University. Unlike the large-scale *SFPark* and *LA Express Park* pilots, this was not a Federally funded project. The pilot began in January, 2013 and ran until December, 2015. The purpose of the pilot was to demonstrate to the City that occupancy-driven pricing would produce better outcomes for both consumers and government than the City's single-price, time-limited regime (Fichman 2015). In late 2014, Pittsburgh City Council enabled dynamic pricing city-wide starting in February, 2015 (Zullo 2014) – spelling the beginning of the end for the demonstration pilot.

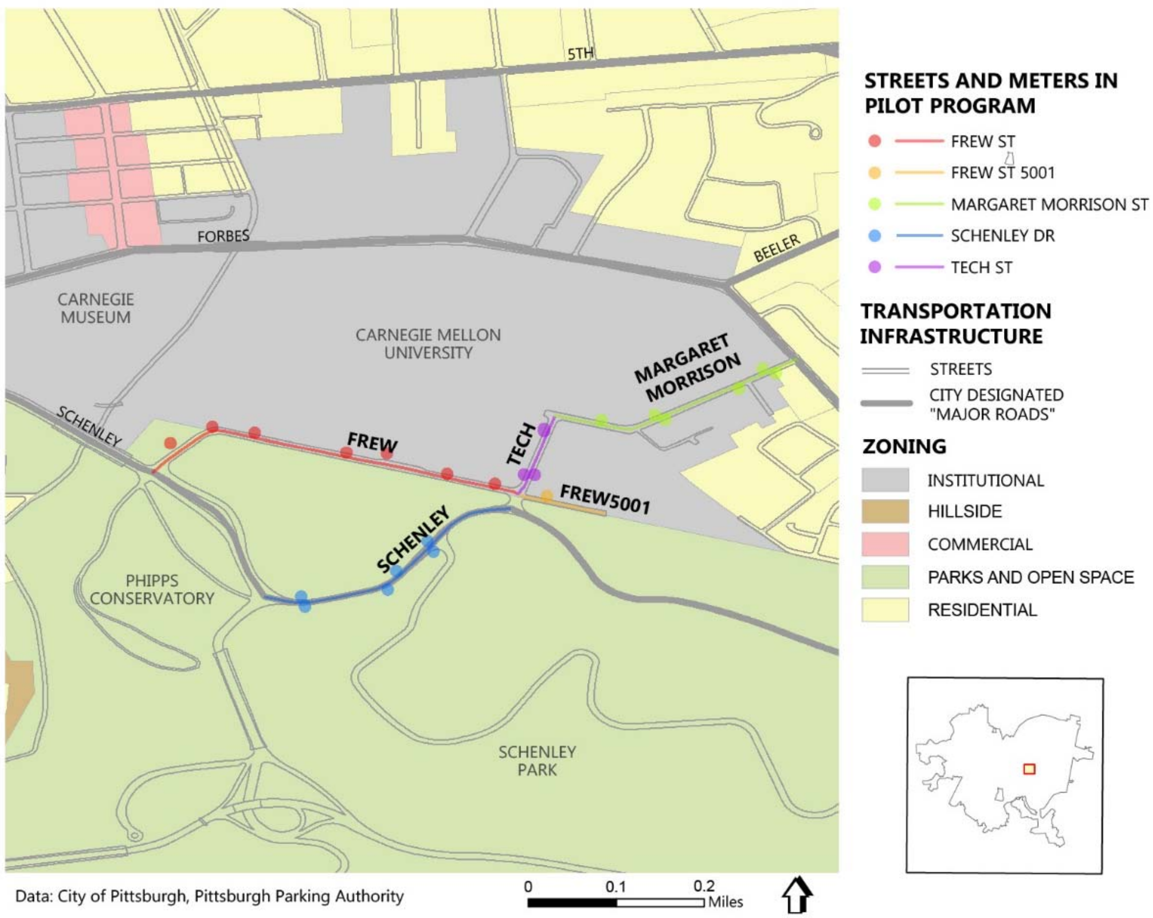


Figure 2. Study Area

Five streets were included in the original pilot- Tech Street, Margaret Morrison Street, Schenley Drive and two sections of Frew Street (one known in this study as "Frew 5001" for its sole meter ID). Because the pilot was designed for demonstration and not specifically for empirical study, there were no "control" meters or streets in the pilot area. However, there were some factors that made the area easy to study. Most pilot meters were not adjacent to any other on-street meters. The land use surrounding the meters was almost exclusively institutional, providing a reasonable amount of spatial homogeneity to the study area (Figure 2). The parking and traffic in the area are almost entirely related to the university – nearby residential areas have ample street parking. Schenley Drive presents the exception to this general homogeneity. Schenley Drive borders a 3-hour time-limited parking area as well as some free parking areas which are used by tourist visitors to Phipps Conservatory. It can be

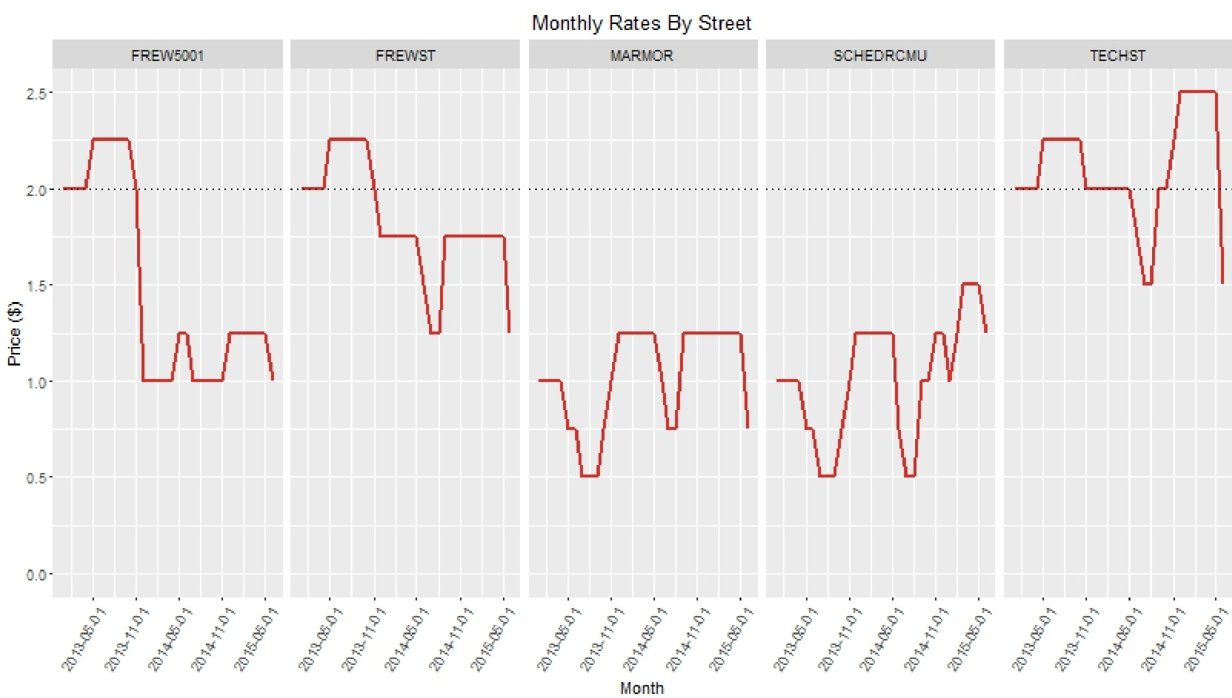


Figure 3. Monthly rates for each street in the pilot. The dashed line represents the $2 price which the PPA established in January, 2012.

argued that Schenley does not belong in the study, but it most certainly represents a part of the CMU parking “market” and its idiosyncrasies can be controlled using fixed effects.

The Pittsburgh pilot was inspired by the parameters of *SFPark*, but operated under a more fluid set of conditions. The city initially limited price changes to $0.25, with no more than one change per month. Each street had only one price, all day, each day for a month. Parking was metered for the hours 8AM-6PM. As time went on, the administrators made larger price changes at the end of the school year. After $0.25 reductions seemed too slow to correspond with drastically lower summer-time demand, the administrators began using larger decreases at the end of the school year and $0.50 increases at the end of the summer. A timeline of price changes for all five study streets can be found in Figure 3. One notable idiosyncrasy in the pricing regime is that the 5001 section of Frew St started off as a higher-priced “Premium” parking area, but under that pricing regime it saw occupancy in the 10-20% range. That block is extremely steep and perhaps seen as an undesirable parking location. Administrators switched it over to an “Economy” pricing regime by late 2013 – dropping its price from $2 to $1.

The administrators periodically estimated gross and mean daily occupancy, using samples of mid-week data, and changed prices based in part on a stated goal to keep mid-week, school-year occupancies between 60-80%. They also changed prices in reaction to repeated “maxing out” at certain locations (Fichman 2015).

Figure 4. Signage and consumer-facing information at the study site

Signs indicating “Economy” and “Premium” parking zones were posted to differentiate higher and lower rate areas and by graphic displays at the electronic kiosks, however drivers did not learn the actual price until visiting the curb-side kiosk (Figure 4). This is a notable difference with the consumer experience of *SFPark*, where drivers were able to examine prices using an internet application. A driver in Pittsburgh who didn’t like the price posted at a kiosk had to decide whether to pay or return to the car to find other parking. Given these circumstances, it is possible that some drivers made a quick decision to pay an undesirable price and use their newfound information about prices on their next visit. It is also possible that some drivers re-entered their vehicles and continued to cruise for parking.

Data

The City of Pittsburgh and the CMU administrators collected a data set that consisted of point-level kiosk transactions for the entire city and conveyed the data set to me upon request. The city-wide data set which was used for this study consisted of 13,348,153 point level observations. Each data point contained the following information: purchase time and date, minutes purchased (units), amount paid, kiosk ID (indicating Street Location and unique ID), paid interval start and end, masked payer ID and some extraneous information (Figure 5). The City also provided the latitude and longitude of all kiosks.

Item Name	Example
Purchase Date Local	11/21/2014 4:51
Terminal - Terminal ID	410153-MARMOR5103
Pay Unit – Name	Card
Ticket Number	12241
Amount	8
Units	514
Masked PAN	443057*8412
Pay Interval Start Local	11/21/2014 8:00
Pay Interval End Local	11/21/2014 14:24
Tariff Package – Name	Pgm42
Article Name	Article 1
Node	Oakland
Tariff Package – ID	42

Figure 5. Sample of raw data

Occupancy Estimation

I estimated occupancy by kiosk in fifteen minute intervals throughout each day of the study period. I then coalesced these kiosk estimations to the street-level. It's necessary to deal with occupancy at the street or block level because A) that is the level at which prices are determined and B) because a driver can pay at any kiosk on a given street - each individual kiosk may log a parking observation from anywhere on the street. My methods were designed to approximate the approach of the program administrators, so as to understand program dynamics from a similar standpoint.

There were numerous considerations and assumptions necessary to undertake these estimations. Unlike *SFPark* and *LA Express Park*, Pittsburgh does not use sensors to determine actual length of stay. I assumed that on average, a car occupies a spot for as long as time has been purchased. Perhaps this is unreasonable, but for lack of any other information about behavior related to departures, this is a necessary assumption.

It was challenging to estimate maxima in order to estimate percentage occupancies. With the exception of stretches of Frew Street, each street in the study area has parallel parking. Parallel parking areas can accommodate a varying number of vehicles depending on vehicle size and spacing on a given day. I surveyed the study site and observed parallel parking behavior. I considered this information alongside street length measurements done using Google Earth and estimates of average parallel parking space length described in other Pennsylvania design codes (Philadelphia Mayor's Office of Transportation and Utilities 2009). Pittsburgh’s zoning and subdivision regulations do not specify the length of an on-street space (City of Pittsburgh 2015). I estimated a parallel parking space to be approximately 18 feet in length and determined parking maxima along each street included in the study area (Figure 6).

Street (Pseudonym in Data Set)	**Maximum Occupancy**	**Price Regime**
Frew St. (FREWST)	170	Premium
Frew St West (FREW5001)	14	Economy
Tech Street (TECHST)	34	Premium
Schenley Drive (SCHEDRCMU)	107	Economy
Margaret Morrison St (MARMOR)	54	Economy

Figure 6. Characteristics of streets in study area

My algorithmic estimation of occupancy, written using the statistical software R, functioned roughly as follows:

1. For each meter, for each day, a 96-item vector represents the 96 fifteen-minute periods of the day. Consider each item in the vector as being a "bin."

2. Each time a driver logs a transaction, divide the number of minutes paid by fifteen. Consider each increment of fifteen minutes to be a "token." Let's say a driver purchases one hour of parking - that would be four tokens.

3. Given the start time of the transaction, rounded to the nearest fifteen minutes – drop the first token in the bin that corresponds to that time and one in each successive bin until there are no more tokens. So if a driver buys one hour of parking at 8AM, he is awarded four tokens - he drops a token into the 8:00-8:14 bin, one in the 8:15-8:29 and so on, until he's out of tokens.

4. Repeat this process for each transaction.

5. Sum the like bins for all meters on a street for the day.

6. Calculate the percentage occupancy in each street bin relative to the maximum occupancy, take the mean percentage occupancy for each street-day for the hours 8AM-6PM. If this figure is above 100% (as it can be given the fact that multiple drivers can pay for a space should one leave before his time is up), assign a 100% occupied or "maxed out" value to that day. These estimations are easily interpretable via a histogram which shows the occupancy of parked cars on a given day shown in Figure 7.

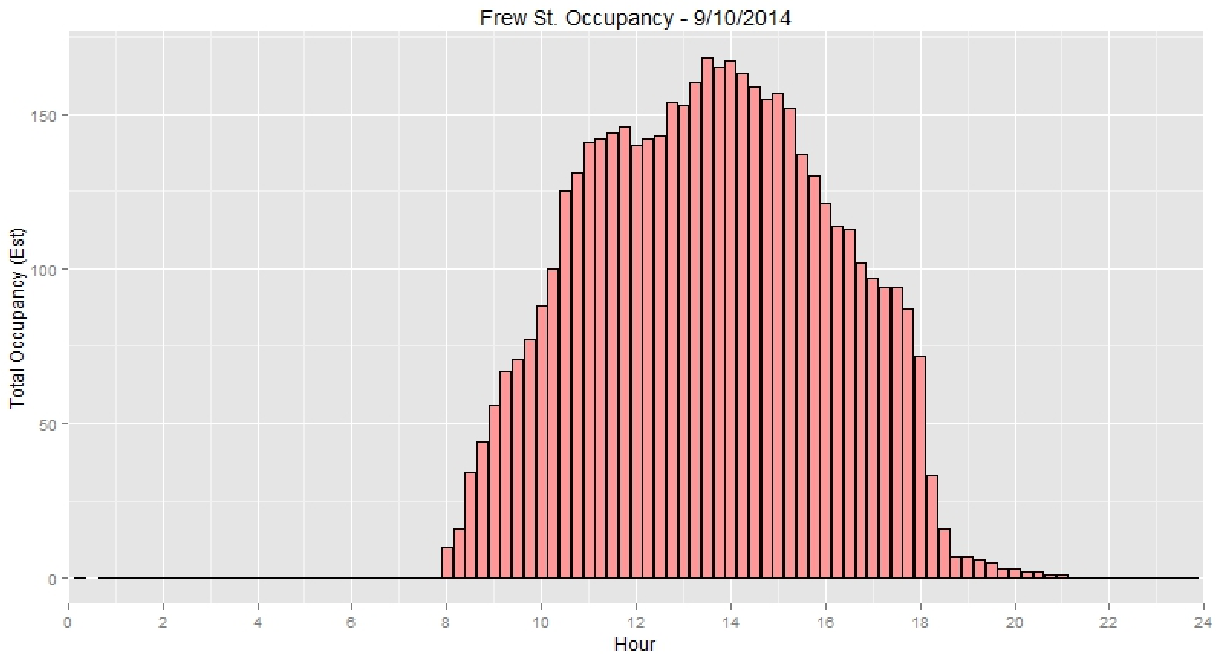


Figure 7. Estimated occupancy for Frew Street, September 10th, 2014

Additional Data Cleaning

The pilot program was designed to use pricing to manipulate occupancy during mid-week periods during the school year. Figure 8 shows the average daily occupancies for Frew St throughout the study period. It is noticeable that during the summer and winter break periods, occupancy is lower – with many days logging little to no occupancy. Similarly, there were days during the school year with almost no occupancy. All streets shared these seasonal characteristics. Recall that the conditions of the pilot only allowed for \$0.25 price changes, one price change per month. Despite the fact that the pilot implemented \$0.50 price changes at the beginning and end of summer in 2014, I reason that these months of extremely low

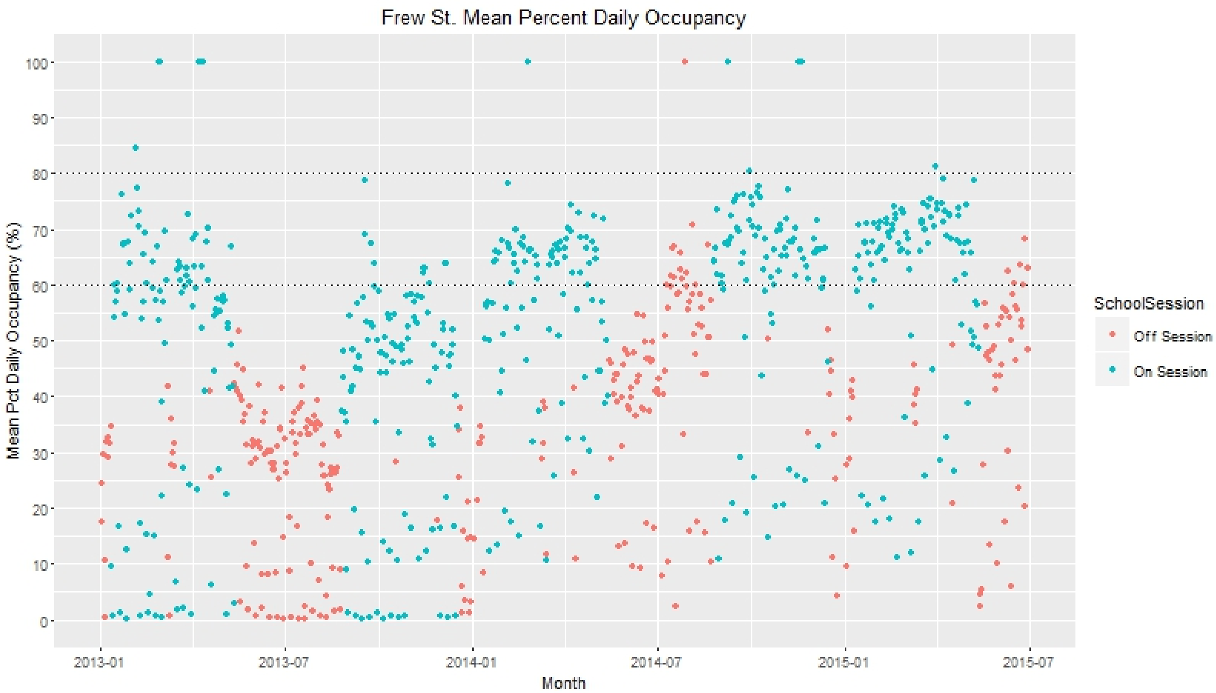


Figure 8. Average daily occupancies on Frew St. Dashed lines represent 60 and 80% estimated mean daily occupancy levels. Red observations represent days coded as having no class or examinations at CMU.

demand were subject to conditions inappropriate for such a constrained pricing regime. Given these conditions, I decided to exclude all data points from the months June, July and August and all Saturday and Sunday observations from regression models rather than just controlling for them. The remaining data set contains 3840 street-day observations. 711 of these fall within either 30 days before or 90 days after a school-year price change.

Variable Name	Description	Example
street	Street Name Fixed Effect	FREW5001
weekday	Day of Week Fixed Effect	Monday
date	Date using POSITX time convention	2014-10-20
off_session	No Classes Fixed Effect (1 for "no class/exams")	0
monthlyPrice	Price Charged in USD	1.00
monthDumb	Month Fixed Effect	OCT
yearDumb	Year Fixed Effect	YEAR13
avgOccupancy	Mean Daily Gross Occupancy	7.6
pctTrueOccupancy	Mean Percentage Occupancy	54%
prevOcc	Mean Mid-Week Occupancy in Previous Month	86%
trueMaximum	Maximum Block Occupancy	14
t	Month Prior To Price Change Fixed Effect	0
t_plus_1	Month 1 After Price Change Fixed Effect	0

Figure 9. Sample of raw and transformed data used to estimate regression models

I also examined Carnegie Mellon's publicly available academic calendars for the years included in the pilot study and coded all street-days without class or examinations with a dummy variable to control for "off session" university business in regression models. Figure 9 shows a sample of cleaned and transformed data used in the regression models in this study.

Estimation of Price Elasticities of Demand

In order to determine the effect of price changes I undertook two sets of regression models – first, an Ordinary Least Squares (OLS) regression to estimate the relationship between price and occupancy while controlling for several time and locational variables. Equation 1 describes the OLS model.

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \cdots + \beta_k x_k + \varepsilon$$

Equation 1. OLS multiple regression

The null hypothesis tested by an OLS model is that the coefficient β indicating the relationship of price with demand (occupancy) is statistically indistinct from zero. The alternative hypothesis would be that "all else equal," there is a statistically significant chance ($p<0.05$) that the magnitude of the β value associated with the independent variable "price" is not zero. The coefficients used to estimate regressions are shown in Figure 9.

Several assumptions must be met in order for OLS to correctly fit a model and determine the significance of the β value associated with price. First, the independent variables should have no strong multi-collinearity. Multi-collinearity can be assessed by measuring variance inflation factors (VIF) using the "car" package for the language R. Second, the relationship between the dependent and independent variables must be linear. Third, the error terms in the model (the difference between the observed points and the mean function) must be normally distributed. I assumed model error terms to be approximately normally distributed. Lastly, the variance or these error terms should be uncorrelated with the other variables (homoscedastic).

I estimated the price elasticities of demand D for price P and quantity Q at point E using Equation 2, which incorporates the β coefficient associated with price from Equation 1.

$$\mathrm{E_D} = (\Delta \mathrm{Q}/\Delta \mathrm{P}) * (\mathrm{P}/\mathrm{Q}) = \beta * (\mathrm{P}/\mathrm{Q})$$

Equation 2. Price Elasticity of Demand for Point E

Regression Discontinuity Models

Millard-Ball, Weinberger and Hampshire (2014) argue that the two-way relationship between price-changes and occupancy (e.g. high occupancy levels inducing price changes and price changes influencing occupancy) represents an endogenous force within an OLS model used to estimate elasticities. The RD model is a “quasi-experimental” design that offers a work-around to this problem. A RD design assigns a “treatment” effect in a non-experimental setting by disaggregating data points into those above and below a certain threshold for treatment (Thistlewaite and Campbell 1960), (Lee and Lemieux 2010). This design has recently increased in popularity and has become “a highly credible and transparent way of estimating program effects,” according to Lee and Lemieux (2010).

In the case of the parking pilot, ideally, when mean monthly occupancies dropped below 60% on a given block, they were to be subject to a price decrease in the subsequent month. Similarly, when occupancies exceeded 80%, they were to be subject to a price increase in the subsequent month. Hypothetically, this would allow for the application of RD models to estimate the effect of price changes on either side of the 60% and 80% boundaries during the month prior to a price change. Because the Pittsburgh pilot had no “control” blocks, the RD design is appealing. All else equal, one would assume observations with 59% and 61% occupancies in a given month to be very similar, except for the fact that the 59% block will have its price manipulated the following month. That allows one to detect possible effects.

The RD design which estimates effects on either side of a strict cutoff value is known as a "sharp" RD model. Such a model is idealized in Figure 10. In this figure, consider the relationship between month *t* and month *t+1*. If occupancy on a given block in month *t* is below 60%, by design, in month *t+1* the price on that block should be lower, presuming a strict

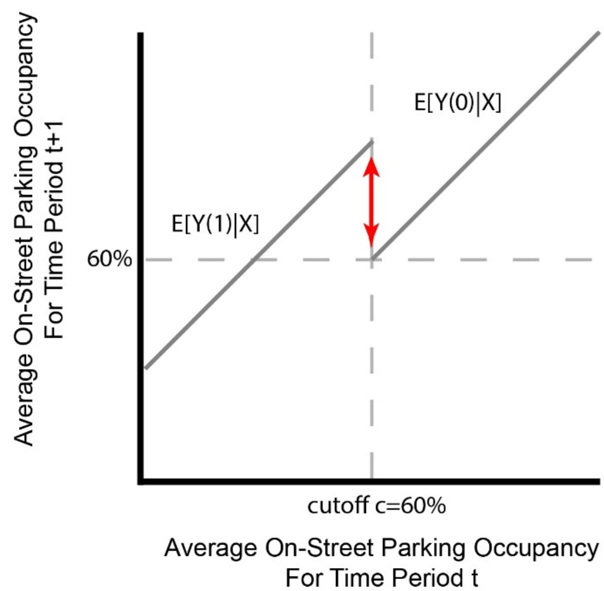


Figure 10. Graphical representation of hypothesized RD design at the 60% occupancy threshold. Red arrow represents the hypothetical occupancy effect of a $0.25 price change. E[Y(0)|X] is the conditional expectation of Y given X in the absence of the price change treatment effect. E[Y(1)|X] is the conditional expectation of Y given X in the presence of (or interacted with) the treatment effect (1).

application of the pricing regimen. Given an alternative hypothesis that the decrease in price in month *t+1* would be associated with a rise in occupancy relative to month *t*, one would expect a line fitted to the discontinuous function in Figure 10, where the treatment (price increase or decrease) is represented by a dummy variable *D* which affects the conditional expectation

E(Y|X) of outcomes on either side of the cutoff value *c* = 60% for a user-specified bandwidth *b* (Equation 3).

$$\mathrm{D} = 0 \; if \; x \leq \; c$$

$$\mathrm{D} = 1 \; if \; x > \; c$$

Equation 3. Assignment of dummy treatment variable D

Because the assignment variable X is independent of potential outcomes Y(0) and Y(1), the causal effect of the treatment is represented by the coefficient ρ – representing discontinuity between estimated functions on either side of the cutoff value (Equations 4-6).

$$\mathrm{E[Y(0)|X]} = \alpha \; + \beta X$$

$$\mathrm{Y(1)} = \mathrm{Y(0)} + \rho$$

Equations 4 & 5. Conditional expectation given lack of treatment variable.

$$Y = \; \alpha \; + \beta X + \rho \mathrm{D} + \eta$$

Equation 6. Combined regression terms

Equations 3-6 are adapted from Imbens and Lemieux (2008). The specification of bandwidth is important to prevent non-parametric functions (which function on larger scales in the data) from deceptively approximating jumps in value at the cutoff point (Angrist and Pischke 2015). In practice, the RD model can be used as a tool to estimate OLS regressions on either side of the discontinuity and compare their behavior at the cutoff. This is the method I utilize. However, it is also common practice to estimate a single model and assess the effect as the coefficient ρ associated with the interaction between the treatment fixed effect and each of the independent variables in order to reduce the magnitude of standard errors (Shaman 2016).

The RD design can be illustrative in implementing a “data-mining” approach, and comparing the effect of treatment on one-week, two-week, one-month and two-month timescales. This approach allows one to perceive the change or lag in the effect of the treatment on either side of the threshold over time, should such an effect exist. RD can be implemented using the base OLS regression models in R and graphically represented using the "lattice" package.

Some practical problems with the data limit the power of RD models in this instance. I examined the occupancy data and price change regime and determined that in practice, the administrators did not adhere to such strict rules regarding the 60% and 80% thresholds – instead working to minimize spikes in occupancy and generally maintain occupancies within the 60-80% range. This means that there were several circumstances in which streets that had mid-week mean occupancies of less than 80% had their prices raised, or, in one circumstance, for reasons I could not determine, Frew Street's "5001" block underwent a price hike despite mean

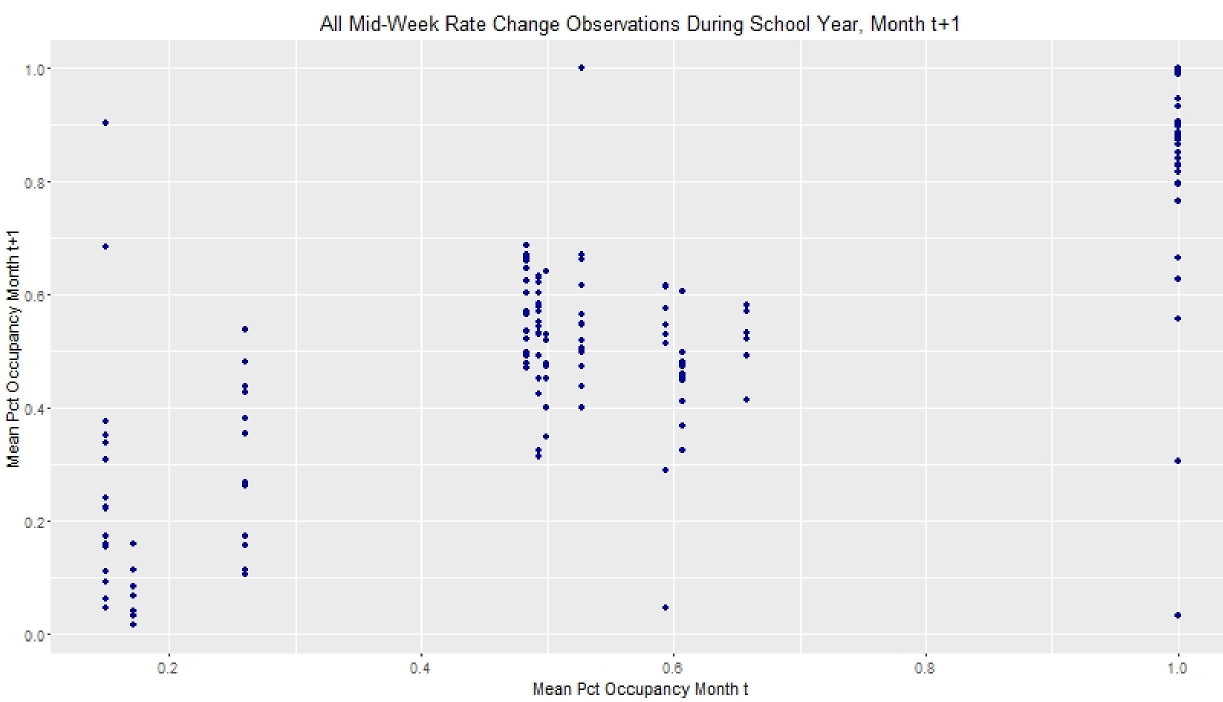


Figure 11. Observations of daily mean occupancy rates for midweek, school year kiosks as a function of previous occupancy in month t. This plot shows only observations which had experienced price changes the month prior.

occupancies near 20% - perhaps a response to several maxed out periods in tandem with coterminous areas on Tech Street and Frew's main drag. Administrators communicated to me that these uneven applications of treatment were in part a function of uncertainty regarding the occupancy estimations. They were also a product of administrative application of specialized local knowledge – for example, if idiosyncratic campus events like graduation led to inflated mean occupancies, these observations were disregarded. Also, the administrators were sampling days each month, rather than looking at the full data set – which might have led them to make decisions while missing information that is included in this study (Fichman 2015). Furthermore, because the data had to be trimmed down to omit summer-time observations, this means that after cleaning, little data remained near the 80% threshold (Figure 11) – certainly not enough to create any reliable estimates for effects near 80%. Most of the blocks which had school-year rate increases had experienced previous average occupancies of 100%.

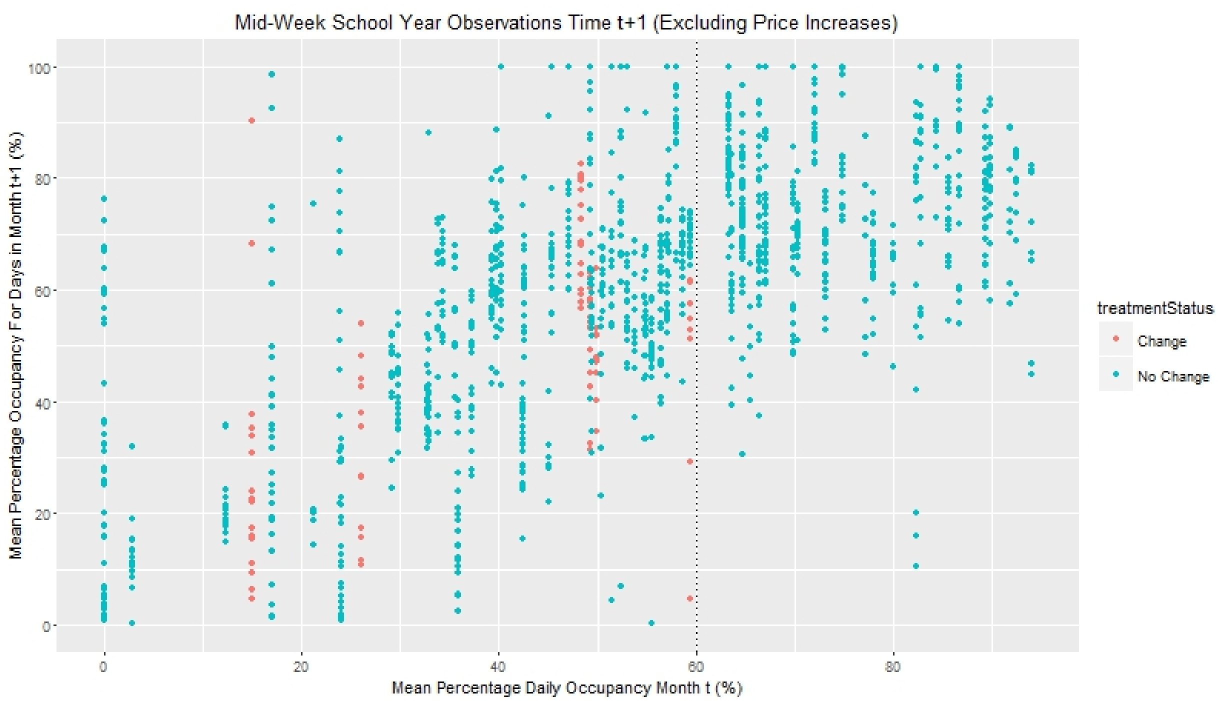


Figure 12. Observations of daily mean occupancy rates for midweek, school year kiosks as a function of previous occupancy in month t. Observations which followed a price change are shown in red.

The 60% threshold has a reasonable amount of data on either side, but for some months with less than 60% occupancy the treatment was not applied (Figure 12). This may be accounted for by using only treated blocks below c=60% to the exclusion of un-treated blocks below 60%.

In order to determine if removal of the untreated observations below 60% would unduly bias the sample, I performed a Welch, Independent Two-Sample t-test to test the null hypothesis that the mean mid-week, school-year occupancy in months of less than 60% occupancy prior to price decrease was statistically no different than mean occupancy in months of less than 60% occupancy which did not have their prices manipulated. Should the samples prove strongly distinct, one might argue that removing untreated observations in order to run an RD model would result in serious bias unless untreated and treated observations are essentially indistinct from one another. The Welch test is more appropriate than the Student's t-test in this circumstance because the samples have unequal sizes and variance, but the Welch test (sometimes known as the "Unequal Variance" t-test) performs well under such an assumption (Ruxton 2006). I failed to reject the null hypothesis that the means were statistically indistinct, suggesting no specific bias in applying the treatment (Figure 13). I believe that the non-application of treatment was not done with any specific goal of occupancy manipulation,

F-Test to Compare Variances

		95% Confidence Interval		df		
F-Statistic	p-value	Lower	Upper	Denominator	Numerator	Ratio of Variances
0.777	0.128	0.548	1.075	96	236	0.777

Welch's T-Test Assuming Unequal Variance and Sample Size

			95% Confidence Interval		Sample Means	
t-statistic	df	p (two tailed)	Lower	Upper	No Price Change	Price Change
0.238	160.16	0.813	-0.037	0.047	0.389	0.383

Figure 13. F-Test and Welch's T-Test performed on daily mean weekday, school-year occupancy observations below 60% occupancy for which there was and was not a price change in the subsequent month

but was rather a product of haphazard intervention brought about for reasons specified earlier. The probability of a sub 60% observation receiving the treatment was 0.27.

Results

Descriptive Analyses

During the course of the pilot, overall revenues increased (Figure 15). Total revenues for the five streets analyzed in this paper were $2,139,328 during the study period. Recall that for most streets in the pilot program, rates decreased during the study period (Figure 3) and all streets ended up having lower prices than the $2 City-imposed price which was the impetus for the pilot.

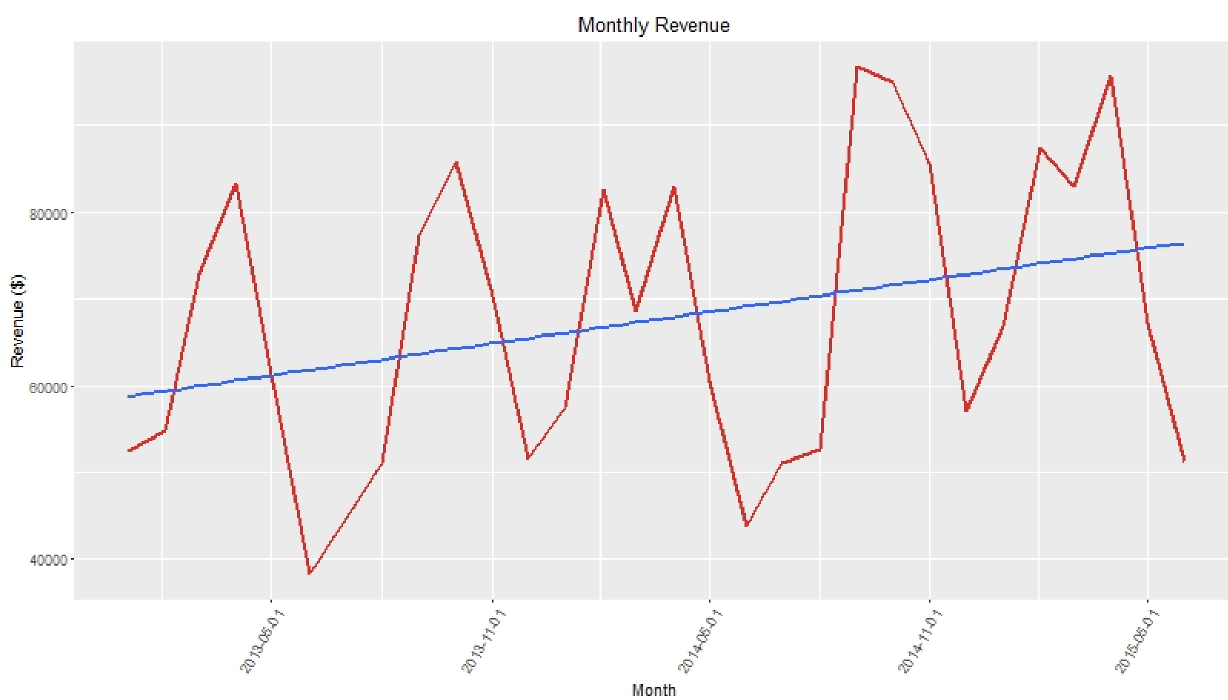


Figure 15. Gross monthly revenues and linear trend.

By the end of the pilot, occupancies for most streets moved towards the target 60-80% range specified by the administrators (Figure 16). The pilot seemed especially successful in elevating the occupancy of blocks which had previous occupancies below 60% and seemed to have little effect on blocks which had very high occupancies (Figure 17). The plot below shows a similar graphic to one published by Millard-Ball, Weinberger and Hampshire (2014). This plot is notably similar in that a fitted line does not show a distinct discontinuity near 60% or 80%. In the case of *SFPark*, this lack of discontinuity was taken by the authors to mean that price changes were not working. However, in the case of the Pittsburgh data, the haphazard application of price change treatments means that one should expect no such effect at those thresholds.

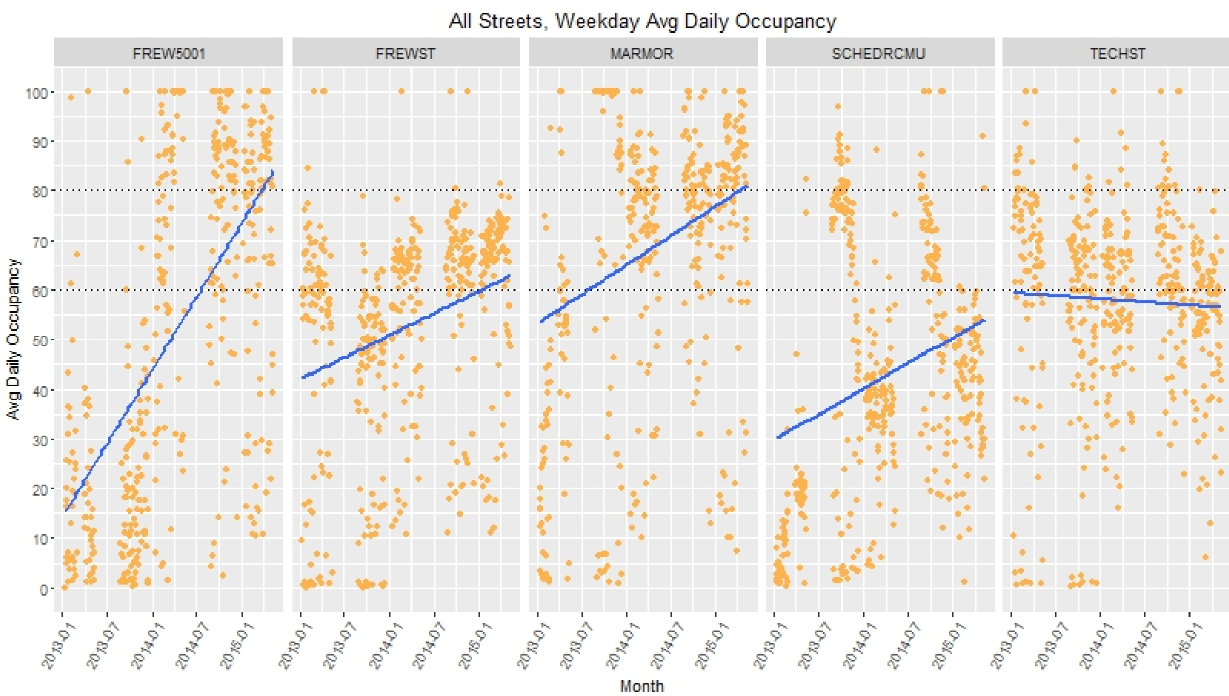


Figure 16. Street-by-street daily, on-session, midweek mean occupancies with linear trend

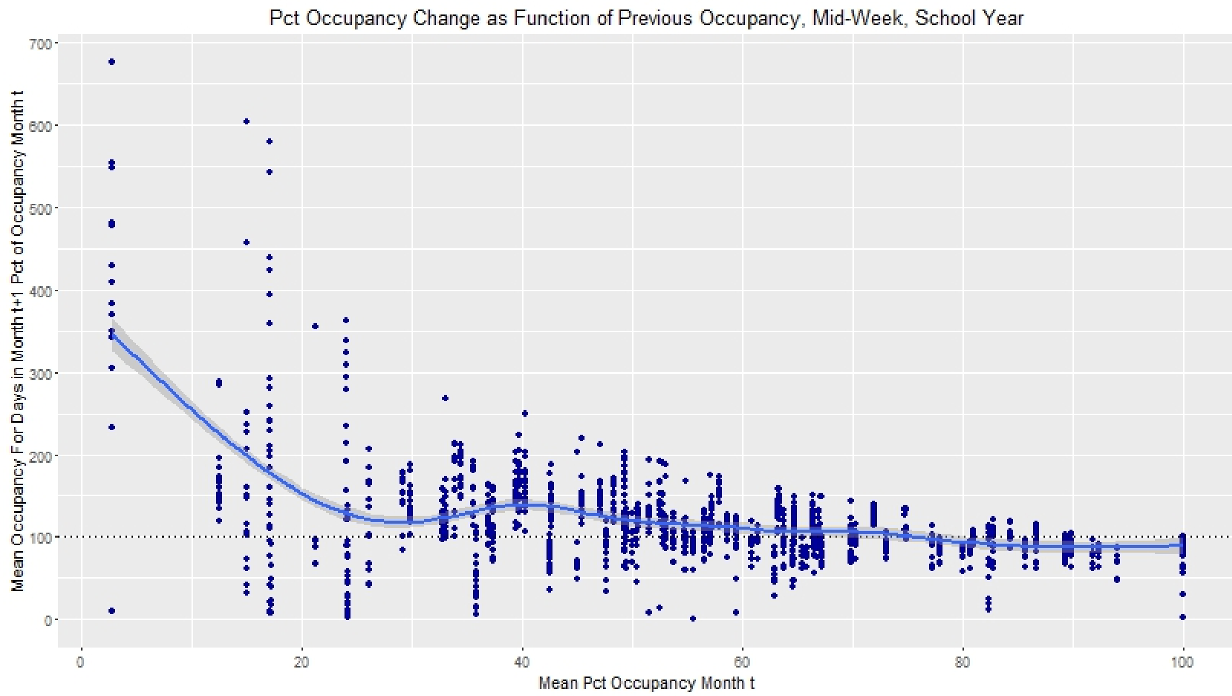


Figure 17. Observations of daily mean occupancy rates for midweek, school year street-days in month t+1 as a percentage of previous occupancy in month t. Loess estimated fit. Dotted line represents 100%. One outlier of 1000% change was removed

Price Elasticity of Demand

Street (Pseudonym in Data Set)	**Weekday Price Elasticity of Demand**
Frew St. (FREWST)	-0.728
Frew St West (FREW5001)	-0.299
Tech Street (TECHST)	-0.730
Schenley Drive (SCHEDRCMU)	-1.211
Margaret Morrison St (MARMOR)	-0.867

Figure 18. Price elasticities of demand for streets involved in the pilot

Figure 18 shows the Price Elasticities of Demand for all streets based on weekday observations during the entire study period. These elasticities were calculated based on OLS regression coefficients associated with on-street parking rate estimated using the model shown in Appendix I. The coefficients in these models represent the estimated effect of price on mean daily occupancy when controlling for street (in the case of the study-wide regression), day of week, year, and whether CMU class was in session. These factors explained a large proportion of the overall variance in occupancy. R-squared values ranged from 0.435 in the case of Schenley Drive to .699 for the 5001 block of Frew Street. It should be noted that whether or not school was in session had a very large effect on occupancy. For the entire data set, the "Off Session" fixed effect was associated with a 29.7% reduction in mean daily occupancy. I did not control for "month" of the year fixed effects because of strong multi-collinearity with the "Off Session" effect. Schenley Drive was the most elastic, which seems intuitive given its relative distance to main campus attractions and its relative proximity to some free on-street parking near Phipps Conservatory. The two premium areas, Frew St. and Tech St. had very similar elasticities, while the Economy-priced Margaret Morrison Street was slightly more elastic. Frew St.'s 5001 block was the least elastic by far. I compare these elasticities to those measured for San Francisco and Seattle in the discussion section of this study.

Regression Discontinuity

The RD models suggested that price changes were indeed having an effect (Figure 19). However, some problems with sample size limit the conclusiveness of these analyses and the results should be treated with some reserve. At the 60% threshold, there was a distinct discontinuity for observations in month t+1, estimated as a 12.17% change in occupancy associated with a $0.25 change in price (Figure 20). Splitting the discontinuity into weeks 1-2 and weeks 3-4 of month t+1 provides some interesting insight into the behavior of drivers. In weeks 1-2, the estimated effect of a $0.25 change in price was 6.19% (Figure 21), whereas the estimated effect in the month's last two weeks was 18.14% (Figure 22), implying that there was a lag in driver responsiveness to price changes.

The regression lines represent the fitted values of the OLS models shown in Appendix II.

Time Period	**Change in Occupancy at 60% Threshold**
Month t+1	12.17%
Month t+1, Days 1-14	6.19%
Month t+1, Days 14-End	18.14%

Figure 19. Estimated discontinuity at 60% threshold for three SRD models

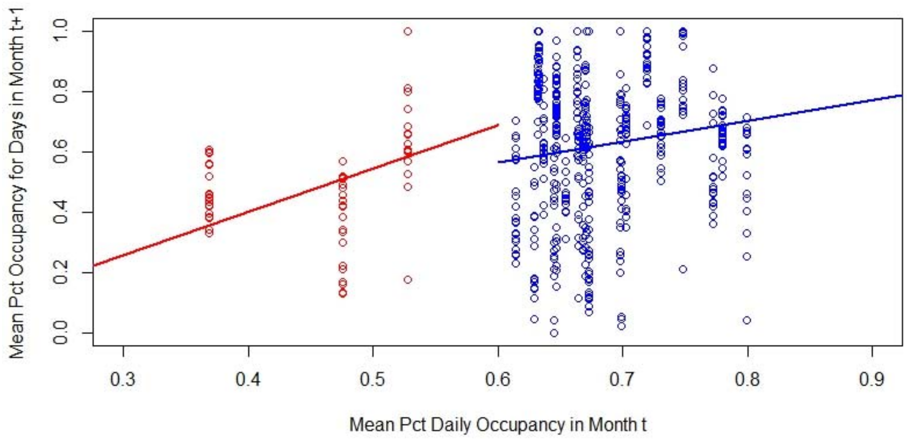
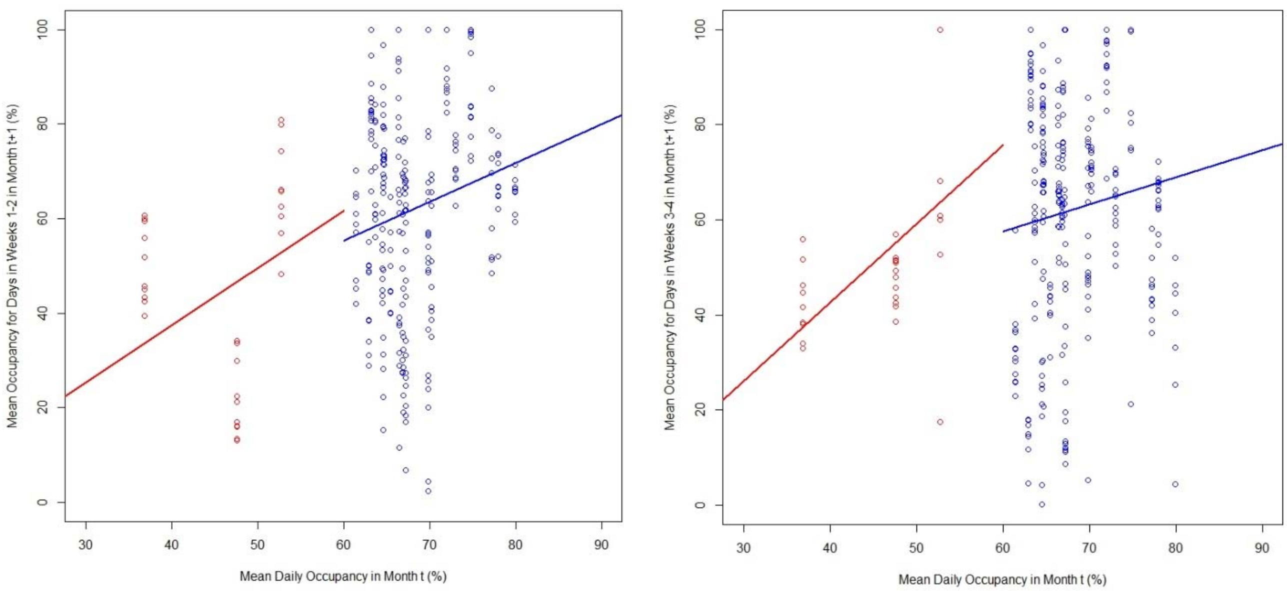


Figure 20. SRD plot showing discontinuity in occupancy in month t+1. Blocks which received price manipulation treatment below the 60% discontinuity threshold are shown in red.

Relatively small sample sizes caused some problems and should give some reason to take the SRD results with caution. First, there was too little data on the treatment side of the discontinuity to use a bandwidth smaller than that of the entire data-set without seeing an incredibly large discontinuity – over 50%. There is little to suggest that this linear discontinuity is actually tracing a non-linear function, but the function may be over-fitted. Second, the estimations of discontinuities for weeks 1-2 and weeks 3-4 of month t+1 have too few observations to include as many controls as were included with the estimation of the full month t+1 (Appendix II). Each of these estimations was carried out using a reduced number of controls to eliminate the strong multi-collinearity between street fixed effects and price in month t. In this sense, these bi-weekly regressions are not completely comparable to that of the whole month t+1.



Figures 21 and 22. RD plots showing discontinuity in occupancy in month t+1, weeks 1-2 (left) and 3-4 (right). Blocks which received price manipulation treatment below the 60% discontinuity threshold are shown in red.

Discussion

The dynamic pricing regime used during the pilot pushed occupancy towards target rates, increased revenues and lowered prices. Furthermore, RD models suggest that the price changes had the intended effect on driver behavior. The static rates the City had been using prior to the implementation of the pilot did not accurately price parking according to demand. Considering how relatively elastic driver response was during the study period, over-charging for parking during 2012 most likely created incentive for drivers to modify their parking behavior in ways that had potentially negative effects. Though it is difficult to discern where drivers parked outside the study area when prices were too high, it’s clear that behavior was affected. For example, the 5001 block of Frew Street had extremely low occupancy when prices were set at "Premium" levels early in the study period (at a similar rate to the pre-study rate of $2/hr), but saw increased occupancy when prices were reduced.

City	Estimated Elasticity
San Francisco (Pierce and Shoup, 2013)	-1.04 to -0.03
Seattle (Ottosson et al., 2013)	-0.4 (mean)
Pittsburgh	**-1.21 to -0.29**

Figure 23. Estimated elasticities for Pittsburgh in comparison to other published studies.

The range of elasticities estimated for streets in the pilot program roughly corresponds to estimates in both San Francisco and Seattle, (Figure 23). Unlike Seattle and San Francisco, this pilot did not estimate for blocks city-wide, and the absence of observations in central business areas probably created a picture of a more elastic parking market. For reasons discussed earlier, simple price elasticities of demand are probably not extremely useful in determining the specific effect of price manipulation because of issues with endogeneity. Furthermore, *positive* elasticities measured by Chatman and Manville (2014) associated with rate hikes suggest that issues with latent demand may further obscure the use of such a metric in measuring the effectiveness of a pricing regime. However, elasticities are useful for purposes of comparison considering how scant the published literature is at this juncture.

With caveats about sample size taken under consideration, RD models suggest that price changes do indeed have an effect on driver behavior which corresponds to a negative elasticity. However, these models also suggest that drivers did not respond instantaneously to price changes during the pilot. The existence of these lags implies that more precise communication of price may reduce lag-time in driver responsiveness by communicating information more quickly. Such communication could come in the form of a consumer-facing internet and phone application showing pricing information and perhaps occupancy estimations. Recall that during the pilot program, specific prices were not communicated directly to drivers except at the kiosk. Although there were street signs indicating "Economy" and "Premium" parking zones, a driver could not learn the specific price until he parked the car and approached the kiosk. By that time, the driver may be much less likely to get back in the car and choose another location and he may just pay for the transaction and apply new pricing knowledge on the next visit. For one-time visitors, rates may have little effect.

Dynamically-priced parking programs should be designed to allow for robust measurement of effects. This analysis demonstrates the potential utility of the regression discontinuity design in measuring the effect of price changes. The regression discontinuity model should allow planners to monitor the effect of dynamic pricing without worrying about the bias caused by myriad spatial and temporal factors, not to mention the endogeneity inherent in occupancy-driven price manipulations. In order to effectively use this tool, programs should adhere to one or more regimes of rule-based decision-making that can be carefully compared and measured. Furthermore, randomized control areas should be implemented in any pilot program to provide more basis for robust estimation of treatment effects.

In order for a demand-based-pricing program to function effectively, it is important to accurately measure occupancy. During the pilot, fundamental uncertainty about the accuracy of occupancy measures seems to have made administrative decisions prone to error. Knowledge of this uncertainty may have prompted administrators to implement rate changes conservatively. It is perhaps for this reason that positive rate changes came mostly as a result of maximum occupancy events, it was also for this reason that price reduction treatment was applied haphazardly.

Estimating occupancy is a difficult thing to do well – if sensors cannot be used to monitor true occupancy, it would be necessary to model arrival and departure rates in relation to occupancy to estimate when people have left their spaces. Spatial elements of parking might also be difficult to model – a block can support ten compact cars or perhaps six pickup trucks, with some margin for error based on how close drivers decide to park to one another. There is a way to reduce some uncertainty created by this spatial problem. When a driver in Pittsburgh pays for parking they input their license plate number, so that parking authorities can appropriately assign tickets to scofflaw drivers. This personal identifying information is masked in the data for privacy purposes. However, it seems conceivable that license plate information could be joined with information about the make and model of the identified vehicle, which could be associated with a database of known dimensions of these vehicle types. This would allow one to estimate the total length of all the vehicles parking at any given time. This naturally introduces some privacy concerns, so subsequent to this join of information, perhaps the ID, make and model of the car could be obscured before any administrators could view it.

The difficulty pilot administrators had in influencing behavior during summer months points to the importance of using lax constraints on price changes. The use of multiple "time-bands" would allow for more sensitive and flexible implementation of prices in order to accommodate fluctuations in parking patterns during the day, week or year. One of the reasons robust statistical analysis was difficult in this study was that summer observations were next to useless because prices could not be adjusted quickly enough to deal with changes in demand. Although administrators began using larger price changes during 2014, it was still too experimental and too slow to provide useful data wherein supply and demand were subject to the equilibrating forces of price manipulation.

Ultimately, demand-based pricing is designed to reduce traffic congestion by eliminating cruising. Occupancy and vacancies are only proxies of the real dependent variable of interest. To truly assess any demand-based parking program, traffic counts should be taken at strategic locations that allow for controlled observation of effects. In the case of the Pittsburgh pilot, the Pennsylvania Department of Transportation, Southwestern Pennsylvania Commission and Pittsburgh's city's planning department responded to my inquiries about traffic count inventories. As a group, they had taken fewer than a half-dozen traffic counts in the area near the university, none of them on the relevant streets. Purpose built counters or video monitors are cheap and easy to operate and should be integrated with any price manipulation scheme so as to measure the most important of all effects – road decongestion.

If demand-based programs are monitored such that prices can be manipulated effectively, and if rules are flexible enough to allow for administrators to effectively equilibrate supply and demand, this will bring parking a little closer to being a good which is non-excludable. Unlike a true "public good," it will still be rival in consumption with private parking garages. However, private garage owners already intuitively use the demand-based pricing strategy – if lots are empty, they are charging too much, and vice versa. If publicly owned parking can be more efficiently priced such that it competes effectively with private parking, private parking owners may cry foul, but their land will soon be purposed to higher and better uses – a net social benefit. A holistic transportation strategy, complete with dynamically priced parking and available, reliable public transportation, could create an environment in which to make a politically feasible push to reduce or eliminate minimum parking requirements in a zoning code and make the actual price of driving and parking transparent to consumers.

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Appendix – Regression tables

OLS Models to Estimate Elasticities

	Dependent variable: Percentage Mean Daily Occupancy (%)					
	All Streets	**FREW ST**	**FREW ST 5001**	**SCHENLEY DRIVE**	**TECH ST**	**MARGARET MORRISON ST**
Monday Fixed Effect	-5.551***	-3.193**	-6.763***	-1.248	-7.856***	-7.836***
Standard Error	1.049	1.543	2.494	2.392	1.706	2.551
Thursday Fixed Effect	0.094	1.581	1.217	0.675	-2.15	-0.608
Standard Error	1.027	1.473	2.451	2.348	1.678	2.525
Tuesday Fixed Effect	-1.29	0.261	-2.619	0.522	-5.172***	0.41
Standard Error	1.019	1.453	2.473	2.323	1.66	2.501
Wednesday Fixed Effect	-1.338	-0.535	-2.07	0.867	-3.869**	-1.756
Standard Error	1.024	1.461	2.475	2.374	1.671	2.474
Frew St Fixed Effect	19.537***					
Standard Error	1.111					
Margaret Morrison Fixed Effect	10.487***					
Standard Error	1.117					
Schenley Drive Fixed Effect	-16.646***					
Standard Error	1.118					
Tech St Fixed Effect	26.968***					
Standard Error	1.232					
Price (Dollars)	**-30.070***	**-21.251***	**-9.870***	**-49.593***	**-18.291***	**-57.791***
Standard Error	1.014	2.722	3.527	3.031	2.217	4.088
Year 2014 Fixed Effect	8.724***	2.258	38.447***	18.735***	-2.475*	28.012***
Standard Error	0.763	1.675	3.879	1.788	1.262	2.191
Year 2015 Fixed Effect	12.332***	4.067**	41.385***	22.715***	0.08	33.491***
Standard Error	0.882	1.764	3.616	2.518	1.499	2.544
Off Session Fixed Effect	-29.767***	-24.823***	-35.170***	-30.071***	-33.048***	-34.777***
Standard Error	0.688	1.01	1.641	1.692	1.152	1.83
Constant	95.616***	100.242***	49.806***	90.769***	108.331***	124.302***
Standard Error	1.962	6.013	7.479	3.491	5.103	4.298
Observations	2,942	596	562	590	610	584
R^2	0.548	0.578	0.699	0.435	0.584	0.45
Adjusted R^2	0.546	0.572	0.694	0.428	0.578	0.443
Residual Std. Error	17.592 (df = 2929)	11.377 (df = 587)	18.491 (df = 553)	17.939 (df = 581)	13.074 (df = 601)	19.171 (df = 575)
F Statistic	295.783*** (df = 12; 2929)	100.496*** (df = 8; 587)	160.333*** (df = 8; 553)	55.982*** (df = 8; 581)	105.325*** (df = 8; 601)	58.872*** (df = 8; 575)
Note:						*p **p ***p<0.01

Appendix I -OLS regressions used to estimate price elasticity of demand. Base cases for fixed effects are: Friday (Weekday), 2013 (Year) and Frew St 5001 (Street) and On-Session (Off-Session)

OLS Models to Estimate Regression Discontinuity

	Dependent variable: Mean Percentage Occupancy For Days in Time Period t+i					
	Whole Month t+1, 0-60% Previous Occupancy	Whole Month t+1, 60-80% Previous Occupancy	Week 1-2, Month t+1, 0-60% Previous Occupancy	Week 1-2, Month t+1, 60-80% Previous Occupancy	Week 3-4, Month t+1, 0-60% Previous Occupancy	Week 3-4, Month t+1, 60-80% Previous Occupancy
Previous Month Mean Daily Occupancy (%)	-0.463	0.207	-2.049***	0.075	0.542	0.349*
Standard Error	0.38	0.141	0.385	0.198	0.437	0.199
Monday Fixed Effect	-0.469	-2.189	-2.492	-1.703	1.914	-2.649
Standard Error	4.512	1.869	3.858	2.535	5.912	2.729
Thursday Fixed Effect	0.906	1.81	0.427	-0.521	-0.125	4.177
Standard Error	4.253	1.814	3.767	2.508	5.206	2.595
Tuesday Fixed Effect	2.227	-0.951	5.207	-1.934	-2.432	0.06
Standard Error	4.493	1.801	4.229	2.486	5.361	2.576
Wednesday Fixed Effect	-0.923	-0.899	0.847	-1.47	9.047	-0.607
Standard Error	4.526	1.748	3.775	2.483	6.444	2.439
Frew St Fixed Effect		12.742***		11.292*		12.667**
Standard Error		4.319		6.283		6.016
Margaret Morrison Fixed Effect		7.349***		2.437		11.146***
Standard Error		1.925		2.773		2.677
Schenley Drive Fixed Effect	22.469	-20.329***	67.647***	-23.260***	-42.416**	-17.990***
Standard Error	17.555	2.14	15.03	3.153	17.711	3.05
Tech St Fixed Effect	70.800***	25.022***	131.890***	26.261***	-15.295	22.005***
Standard Error	14.614	6.007	14.419	8.628	20.181	8.473
Price (Dollars)	-22.098	-39.465***	-17.138	-45.367***		-32.226***
Standard Error	15.163	6.518	10.402	9.397	0	9.197
Off Session Fixed Effect	-16.890***	-36.341***	-8.275**	-38.069***	-67.738***	-36.524***
Standard Error	4.022	1.356	3.841	2.129	12.546	1.879
Constant	72.046**	110.992***	80.616***	130.596***	62.946***	90.220***
	33.093	15.246	22.348	21.833	15.275	21.407
Observations	79	538	39	259	40	279
R^2	0.798	0.685	0.93	0.664	0.869	0.718
Adjusted R^2	0.772	0.678	0.909	0.649	0.835	0.706
Residual Std. Error	0.123 (df = 69)	0.132 (df = 526)	0.075 (df = 29)	0.128 (df = 247)	0.109 (df = 31)	0.135 (df = 267)
F Statistic	30.373*** (df = 9; 69)	103.832*** (df = 11; 526)	43.006*** (df = 9; 29)	44.461*** (df = 11; 247)	25.659*** (df = 8; 31)	61.754*** (df = 11; 267)
Note:						*p **p ***p<0.01

Appendix II - OLS regressions used to estimate regression discontinuities. Base cases for fixed effects are: Friday (Weekday), Frew St 5001 (Street) and On-Session (Off-Session)