# **Evaluating the Impacts of Performance-Based Parking** Comment on Pierce and Shoup

# June 2014

This is an Accepted Manuscript of an article published in the

Journal of the American Planning Association, 79(4): 330-336

available online: http://www.tandfonline.com/10.1080/01944363.2014.918481

#### Adam Millard-Ball

Assistant Professor, Environmental Studies Department University of California, Santa Cruz Santa Cruz, California 95064 adammb@ucsc.edu

### Rachel R. Weinberger

Director of Research and Policy Strategy Nelson\Nygaard Consulting Associates New York, New York 11217 rweinberger@nelsonnygaard.com

### **Robert C. Hampshire**

Assistant Professor of Operations Research and Public Policy Heinz College Carnegie Mellon University Pittsburgh, Pennsylvania 15217 hamp@andrew.cmu.edu

#### Introduction

Gregory Pierce and Donald Shoup (2013) provide a long-awaited analysis of San Francisco's SFpark – the most sophisticated parking pricing program in the U.S., if not the world. They analyze the impact of performance-based rate changes during the first year of SFpark's operations, and conclude that there have been substantial, measurable impacts on parking occupancy. The average price elasticity they compute is -0.4, meaning that a 10% increase in price is associated with a 4% reduction in demand.

In principle, a performance-based parking system such as SF*park*, which adjusts prices in a bid to achieve a target occupancy for curb parking, is an excellent way to reduce congestion and to improve the driver experience. Their finding represents a remarkable impact of the SF*park* program in an extremely short time, and an impact that is substantially faster and larger than shown in other analyses, including our own (Millard-Ball et al. 2014) and others (Chatman & Manville 2014).

We agree with Pierce and Shoup's policy recommendations, which build on much of Shoup's earlier work (e.g. Shoup 2005). However, their empirical analysis ignores the endogeneity of prices (defined and discussed below), and specifically the possibility that fluctuations in demand trigger price changes under SF*park*'s rate adjustment rules.

In this note, we simulate parking demand as a purely random process, with price having no impact on demand. In 10,000 simulations, we recreate similar price elasticities to those of Pierce and Shoup. Thus, we conclude that their findings are largely spurious, caused by the statistical phenomenon of reversion to the mean. Using an alternative method – the regression discontinuity design – that is more robust in this type of situation, we show that there is no evidence of short-run impacts on occupancy from individual rate changes.

We believe that it is too soon to draw firm conclusions about the impact of performancebased parking pricing programs such as SF*park*. The takeaway for practice is that short-run elasticities are likely to be substantially less than those reported by Pierce and Shoup. After one year, fluctuations in demand may be influencing price more than price is influencing short-run demand. Similar evaluations need to explicitly account for endogeneity and the design of the rate-change mechanism.

### **Elasticities as a Statistical Artifact**

To understand why Pierce and Shoup's results do not primarily reflect the impact of price changes on occupancy, it is first necessary to understand how SF*park* adjusts rates. (For more general background, the reader is referred to their article, or to the SF*park* website at www.sfpark.org.) The San Francisco Municipal Transportation Agency (SFMTA) established a target occupancy range of 60-79% on each metered block. If occupancy is 80% or greater, meter rates increase by 25 cents per hour, up to a maximum of \$6. If occupancy is below 60%, then meter rates decrease by 25 cents, or by 50 cents if occupancy is below 30%, to a minimum of 25 cents. In general, rates are adjusted every two months.

Immediately, it is evident that the relationship between price and demand runs in both directions. Parking demand affects prices (by design), and prices are expected to affect parking demand. In statistical terms, prices are "endogenous" because there is a two-way relationship between prices and the variable of interest, parking occupancy. For a more detailed discussion of endogeneity from a statistical perspective, the reader is referred to a text such as Wooldridge (2012), but we illustrate the basic problem here with an example.

Consider a block that is in equilibrium at 75% occupancy, just within SFMTA's target range. There is some random variation in demand, and so in some rate adjustment periods, occupancy exceeds 80%, but expected occupancy in the following rate adjustment period is still the equilibrium level of 75%. Now recall that SFMTA's rate adjustment rules call for rates to be increased if occupancy reaches 80% or more. The rate is increased according to SF*park*'s rules, but demand would have fallen back towards its equilibrium level of 75% regardless of the price increase. It is an example of reversion to the mean – the tendency of extreme values to revert to their normal range on subsequent observations. Yet, it could appear from the data that the reversion to the long-run equilibrium is a behavioral response to the price change.

Indeed, this story applies to any block with equilibrium occupancy in SFMTA's target range. Any random fluctuation that takes occupancy outside the 60-79% range will be followed by a price change (because of SFMTA's rate adjustment rules), and more likely than not, a reversion to the 60-79% range in subsequent months.

Another example of endogeneity occurs when occupancy is driven by unrelated events. Suppose that a new restaurant opens on a block, and grows in popularity over time. Occupancy is likely to increase in each rate adjustment period (because of the restaurant), but meter prices will also increase over time (because of SFMTA's rate adjustment rules). In this example, elasticities might well be positive, giving rise to the erroneous conclusion that higher prices increase demand. As with the case of random fluctuations in demand, a naïve analysis that does not account for endogeneity runs the risk of bias.

### **Simulating Parking Elasticities**

We use the same data as Pierce and Shoup.<sup>1</sup> First, we replicate their results (with some minor inconsistencies). Second, we simulate random changes in demand, based on the average observed demand and corresponding standard deviations, and calculate the simulated elasticities.

Prices under SF*park* vary not only by block, but also by six timebands (before noon; noon to 3pm; and after 3pm, on both weekdays and weekends). Following Pierce and Shoup we calculate midpoint elasticities for each of the 5,332 price changes<sup>2</sup> in the first year of SF*park* operations; this encompasses six rate adjustments between August 2011 and August 2012. In a further 2,872 cases, prices remained unchanged, either because occupancy was in the target range or to maintain the minimum price of \$0.25/hour.

Our random simulation of parking demand proceeds as follows. For each block and timeband, the SFMTA spreadsheet provides six datapoints representing the average occupancies in the run up to each of the six rate adjustments, plus one additional datapoint following the August 2012 rate adjustment. For each block and timeband, we calculate the mean and standard deviation of these seven datapoints.

<sup>&</sup>lt;sup>1</sup> The data are available on the SFMTA website: http://sfpark.org/resources/meter-rate-adjustment-spreadsheet-april-2013/, last accessed December 19, 2013.

<sup>&</sup>lt;sup>2</sup> Pierce and Shoup report 5,294 price changes; the slight discrepancy may be caused by dropping certain blocks with incomplete observations. Note that this total only includes rate changes for which elasticities can be computed; it excludes observations where 'after' occupancy data are missing.

We then simulate 'before' and 'after' occupancy for each block/timeband as a random deviation from the mean occupancy. The random deviation is drawn from a mean-zero normal distribution using the block- and timeband-specific standard deviation. We include a serial autocorrelation term ( $\rho = 0.22$ , estimated from the SFMTA data) to account for the correlation between 'before' and 'after' observations that would be expected given that there may be block-specific time trends. Formally, we simulate occupancy as follows:

 $\begin{array}{l} Before\_Occ_{it} = \mu_{it} + \epsilon_{it} \\ After\_Occ_{it} = \mu_{it} + \rho\epsilon_{it} + \delta_{it} \end{array}$ 

where  $\mu_{it}$  is the mean occupancy for block *i* in timeband *t*;  $\rho$  is the serial autocorrelation term;  $\epsilon_{it}$ ,  $\delta_{it} \sim N(0, \widehat{\sigma_{it}})$ ; and  $\widehat{\sigma_{it}}$  is the estimated standard deviation.

The 'before' rate is taken to be the pre-SF*park* rate, which is \$2, \$3 or \$3.50 per hour depending on the neighborhood. We calculate the rate adjustment using SFMTA's rules, based on the 'before' occupancy. The rate adjustment has no impact on the 'after' occupancy, which is purely a random departure from the mean. We average the elasticities for each block and timeband over 10,000 simulations.

Figures 1 to 3 compare the estimates reported by Pierce and Shoup, our replication of their work, and our random simulation of parking demand. With the exception of the results by price change, we replicate their results almost precisely using the same dataset. More importantly, we derive even more elastic responses from purely random simulations. Our overall simulated elasticity is -0.56, compared to Pierce and Shoup's -0.40. One possible explanation for the difference is that the other forms of endogeneity discussed above, such as new destinations opening on particular blocks, may be biasing Pierce and Shoup's results towards zero.

In common with Pierce and Shoup, our simulation yields positive elasticities on some individual blocks – i.e., blocks where an increase in price is followed by an increase in demand, or vice versa. In our simulation, this is simply due to the random nature of occupancy changes, and captures the reality (emphasized by Pierce and Shoup) that many other factors affect parking demand. However, regression to the mean ensures that these positive elasticities are countered

out by an even greater number of negative elasticities, giving a mean elasticity of -0.56 (in our simulations) or -0.40 (in Pierce and Shoup's results).<sup>3</sup>

The somewhat counterintuitive patterns by price change in Figure 3 deserve particular attention. The response is most elastic for the adjustment of -\$0.50, and virtually zero for the adjustment of \$-0.25. This pattern can be explained as a function of the distribution of occupancies and the rate adjustment regime. Recall that a rate adjustment of -\$0.50 is reserved for blocks that fall below 30% occupancy. As shown in Figure 4, relatively few blocks have an average (equilibrium) occupancy below 30%. Thus, most of the rate changes of \$-0.50 are due to large random fluctuations on blocks with higher equilibrium occupancies, which subsequently revert to the mean and generate spurious elasticities.

Figure 4 also shows the pattern of simulated 'elasticities' by equilibrium occupancy. The most elastic demand occurs at 70% occupancy. As discussed above, this is because any large departure (10% or more) will result in a rate adjustment, but occupancy will revert to the mean independently of the rate change.

<sup>&</sup>lt;sup>3</sup> Pierce and Shoup do not report exact numbers, but Figure 9 of their paper and our replication of their results indicate that more than 35% of elasticities are positive. Our simulations result in only 19% of elasticities being positive. This provides further evidence that other forms of endogeneity, such an increase in demand from a new restaurant leading to subsequent price increases, which would be expected to yield positive elasticities, are biasing Pierce and Shoup's results.



Figure 1 Elasticity by Time of Day



Figure 2 Elasticity by Neighborhood



Figure 3 Elasticity by Price Change



Figure 4 Distribution of Occupancies and Simulated Elasticities

### Too Good to be True?

Even before considering the simulation results, there are some signs that should raise questions as to the validity of the Pierce and Shoup conclusions. Most importantly, their reported average elasticity of -0.4 is similar to or more elastic than those reported in other contexts. A review by Vaca and Kuzmyak (2005) suggests a typical parking price elasticity of -0.3, with a general range of -0.1 to -0.6. This means that according to Pierce and Shoup, SF*park* elasticities lie in the upper part of the range reported in the literature.

Yet, the San Francisco program might be expected to generate relatively inelastic responses. Many on-street parking spaces are occupied by disabled placard holders, who do not pay for parking and thus would not respond directly to any price change (Shoup 2011; Manville & Williams 2012). Moreover, under SF*park*, parking prices are not readily apparent to drivers who do not first park and check the meter, or research the prices online. It is unclear how many drivers are actually aware of the price differentials, even if they are aware of the broader SF*park* program. Further, the presence of latent demand is likely to temper the impacts of price increases on blocks that are close to fully occupied, as newly available spaces are taken up by drivers who previously could not find a space on that block. For all these reasons, an elasticity of -0.4, especially in the short term, warrants close scrutiny.

# An Alternative Empirical Approach: Regression Discontinuity

Any analysis of SF*park* and similar programs clearly needs to explicitly consider the design of the rate adjustment process. In this section, we present an alternative method – regression discontinuity – that is widely used to address similar problems of endogeneity.<sup>4</sup> Since the 30%, 60% and 80% thresholds for rate changes are determined exogenously, blocks just below a threshold can be compared to blocks just above the threshold. If rate changes were having an impact, one would expect to see "jumps" or discontinuities in occupancy changes at this threshold. For example, blocks that were at 79% occupancy in the "before" period (and thus did not experience a rate change) should see little change in occupancy compared to blocks that were

<sup>&</sup>lt;sup>4</sup> A related design that could be used is the matching design discussed by Dehejia & Wahba (2002).

at 80% occupancy (where rates increased by 25 cents).<sup>5</sup> Such a regression discontinuity design relies on the assumption that blocks immediately above and immediately below the threshold are similar in all ways except for the different rate changes.

Figure 5 provides a graphical representation of the regression discontinuity analysis. The x-axis indicates occupancy in the "before" period, and the vertical lines indicate the rate adjustment thresholds. The upper panel shows the percentage *rate* change between the "before" and "after" periods. The impact of the thresholds is clearly visible through the discontinuity in the solid line. The conclusions are somewhat obvious – by the design of SF*park*, rate changes are discontinuous at each threshold – but the purpose here is simply to illustrate how regression discontinuity analysis can work.

The lower panel of Figure 5 shows a similar analysis for the percentage *occupancy* change. If response to price changes were large, as Pierce and Shoup suggest, then there would be similar discontinuities in the solid line. The percentage occupancy change should drop sharply downwards at each of the three thresholds. No such discontinuities are evident in the lower panel, indicating that there is no discernable short-run occupancy impact of the SF*park* rate adjustments. This is confirmed by the regression discontinuity coefficients shown in the appendix, which represent the impact of moving from a "before" occupancy just below a threshold to just above a threshold. The coefficients are neither uniformly negative nor statistically significant.

The results shown in Figure 5 and the appendix are based on more than two years of SF*park* rate adjustment data, and use data through November 2013.<sup>6</sup> Almost identical results are obtained using only the first year of data, as in the Pierce and Shoup analysis. We also obtain similar results when disaggregating by geographic area and by individual rate adjustment. In no case is there evidence of any short-run impact of rate changes on driver behavior.

<sup>&</sup>lt;sup>5</sup> For a discussion of the assumptions behind regression discontinuity designs and applications to the transportation and planning contexts, see Washington et al. (2011) and Deng & Freeman (2011).

<sup>&</sup>lt;sup>6</sup> The data file is available at http://sfpark.org/resources/meter-rate-adjustment-spreadsheet-november-2013/, last accessed December 19, 2013.



Figure 5 Regression Discontinuity Analysis

These regression discontinuity results have several important limitations (all of which are common to the Pierce and Shoup analysis). First, we are unable to distinguish between the different ways in which drivers may respond to a change in price – in particular, the extent to which they choose to shift their parking location versus changing mode or the decision to make the trip. Second, the size of the elasticity will likely depend on the rate differential with neighboring blocks. An increase from \$2 to \$2.25 may have an effect if rates on neighboring

blocks are \$1, but not if neighborhood blocks are priced at \$5. Third, as we emphasize in the conclusion to this paper, we only examine the short-run impacts of rate adjustments, and do not account for longer-term responses by drivers following multiple rate changes over a one- to two-year period.

## Conclusions

Pierce and Shoup interpret their elasticities as demonstrating the large effects of parking price changes on driver behavior. However, similar results follow from random fluctuations in demand, which subsequently trigger changes in price according to SF*park*'s rate adjustment rules. If similar elasticities can be produced through random behavior, then claims that parkers in aggregate are changing their behavior in response to price may be unfounded. Moreover, using an alternative method that accounts for certain limitations in Pierce and Shoup's analysis, we find that the short-run elasticity is indistinguishable from zero. Pierce and Shoup conclude that individual price changes are influencing short-run demand, but our results call into question the direction of the causal relationship. Fluctuations in demand may be influencing price more than individual 25-cent changes in price are influencing demand.

Our results do not necessarily mean that SF*park* is failing to achieve its stated goals of improving parking availability and reducing cruising. In common with Pierce and Shoup, we analyze only the short-run impacts following individual rate changes. It is plausible that parkers do not react to each 25-cent change, but do adjust their behavior in the longer term as awareness of price differentials increases, and as the cumulative rate changes mount up.<sup>7</sup> Other elements of the SF*park* program, such as real-time occupancy information, improved payment options and adjustments to off-street parking rates, may also affect on-street parking demand.

Indeed, in our other work (Millard-Ball et al. 2014) we find that the overall impacts of SF*park* have grown over time; not until the second year did measurable changes to parking occupancy and cruising occur. Negligible impacts after individual rate adjustments should not come as a surprise given the time needed for drivers to understand the system. Moreover,

<sup>&</sup>lt;sup>7</sup> A similar view has been expressed by the manager of SF*park* (Bialick 2011).

potentially high levels of consumer surplus, with willingness to pay exceeding initial parking rates, might mean that large rate adjustments are needed before drivers respond.

Planners should treat the elasticities reported by Pierce and Shoup with caution. They should not expect shifts in parking demand in response to relatively small price changes, at least in the short-run. If the aim is to affect curb parking occupancy in a context such as San Francisco, then price changes may need to be large enough to be immediately noticeable, or else policy makers need to commit for the longer haul.

#### References

- Bialick, A., 2011. Jay Primus: Too Early to Evaluate Results of SFPark. Streetsblog. Available at: http://sf.streetsblog.org/2011/12/16/jay-primus-too-early-to-evaluate-results-of-sfpark/.
- Chatman, D.G. & Manville, M., 2014. Theory versus implementation in congestion-priced parking: An evaluation of SFpark, 2011-2012. *Research in Transportation Economics*, in press.
- Dehejia, R.H. & Wahba, S., 2002. Propensity Score-Matching Methods for Nonexperimental Causal Studies. *Review of Economics and Statistics*, 84(1), pp.151–161.
- Deng, L. & Freeman, L., 2011. Planning for Evaluation: Using Regression Discontinuity to Evaluate Targeted Place-Based Programs. *Journal of Planning Education and Research*, 31(3), pp.308–318.
- Manville, M. & Williams, J.A., 2012. The Price Doesn't Matter If You Don't Have to Pay: Legal Exemptions and Market-Priced Parking. *Journal of Planning Education and Research*, 32(3), pp.289–304.
- Millard-Ball, A., Weinberger, R. & Hampshire, R., 2014. Is the curb 80% full or 20% empty? Assessing the impacts of San Francisco's parking experiment. *Transportation Research Part A: Policy and Practice*, 63, pp.76–92.
- Pierce, G. & Shoup, D., 2013. Getting the Prices Right. *Journal of the American Planning Association*, 79(1), pp.67–81.
- Shoup, D., 2011. Ending the Abuse of Disabled Parking Placards. Access, 39, pp.38-40.
- Shoup, D.C., 2005. The high cost of free parking, Washington, DC: Planners Press.
- Vaca, E. & Kuzmyak, J.R., 2005. Parking Pricing and Fees. Traveler Response to Transportation System Changes. TCRP Report 95, Ch. 13, Washington, DC.
- Washington, S., Karlaftis, M.G. & Mannering, F.L., 2011. *Statistical and Econometric Methods* for Transportation Data Analysis, Boca Raton: CRC Press.
- Wooldridge, J.M., 2012. *Introductory Econometrics: A Modern Approach* 5th ed., Mason, OH: Cengage Learning.

# **Technical Appendix: Estimated Short-Run Impacts of Rate Changes**

		Robust			
Rate change threshold	Coefficient	standard error	t statistic	р	Ν
25-cent increase	1.750	1.243	1.408	0.159	5771
25-cent decrease	-2.808	2.145	-1.310	0.190	3828
50-cent decrease	7.127	6.611	1.078	0.281	1268

#### Notes:

(1) Coefficients refer to the estimated impact of crossing each rate adjustment threshold. For the 25-cent increase, it indicates the impact of moving from a zero to a +25¢ adjustment. For the 25-cent decrease, it indicates the impact of moving from a -25¢ to a zero adjustment. For the 50-cent decrease, it indicates the impact of moving from a -50¢ to a -25¢ adjustment. One would expect all three coefficients to be negative, as rate changes increase (and thus occupancy changes would be expected to decrease) across each threshold.

(2) Estimated elasticities could be calculated by dividing the coefficient (i.e. estimated occupancy change) by the average percentage rate change in the sample. We do not do this since the coefficients are indistinguishable from zero.

(3) Each row represents a separate regression using the subsample where "before" occupancy is within 10% of the threshold. The regression is of the cubic form:

 $y_i = \propto +\gamma T_i + \beta_1 X_i + \beta_2 X_i^2 + \beta_3 X_i^3 + \varepsilon_i$ 

where:  $y_i$  is percentage occupancy change

 $T_i$  is a dummy variable indicating whether observation *i* has "before" occupancy greater than or equal to the threshold

 $\gamma$  is the coefficient of interest (reported in the table)

 $X_i$  is occupancy in the "before" period

 $\propto$ ,  $\beta_1$ ,  $\beta_2$  and  $\beta_3$  are estimated coefficients (not reported)

 $\varepsilon_i$  is the error term

(4) Qualitatively identical results are obtained using alternative specifications, such as a polynomial of degree 4 or 5, or a local linear regression model. We drop observations where a rate change is not possible because the "before" rate is at the floor  $(25\phi)$  or ceiling (\$6.00), or where a rate change is not made for other reasons. (Inclusion of these observations does not change the results.)