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

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Editor’s Note

Sandra Rosenbloom

In the Winter 2013 issue (Vol. 79, No. 1), *JAPA* published an article by Gregory Pierce and Donald Shoup, “Getting the Prices Right: An Evaluation of Pricing Parking by Demand in San Francisco,” that evaluated the first year of an innovative parking pricing scheme in San Francisco. The program, *SFpark*, raised and lowered parking prices over time to respond to demonstrated demand for curb parking; the program sought to ensure that parking prices reflected actual demand in ways that reduced cruising for empty spaces. Pierce and Shoup concluded that *SFpark* had in fact changed driver behavior in the ways sought: It had reduced demand for parking in most treated neighborhoods.

A few months later another set of researchers, Adam Millard-Ball, Rachel Weinberger, and Robert Hampshire, submitted a rejoinder to the Pierce and Shoup article, arguing that the observed differences in parking behavior may have been due not to changes in parking prices, but to relatively random changes in demand for parking, changes having nothing to do with varying prices set by the program. In this context, Millard-Ball et al. also contested the methods Pierce and Shoup had used. I sought five double-blind reviews of the Millard-Ball et al. manuscript and shared that (blinded) manuscript with Pierce and Shoup.

The reviews posed a problem for me. While all of the reviewers felt that Millard-Ball et al. made some valid points, some reviewers suggested that the normal way to respond to such a situation is to write a new article using different methods or data and specifically challenging prior research (and in fact Millard-Ball et al. have subsequently published their own research on *SFpark* [Millard-Ball, Weinberger, & Hampshire, “Is the Curb 80% Full or 20% Empty? Assessing the Impacts of San Francisco’s Parking Experiment,” *Transportation Research Part A: Policy and Practice*, 2014]). Indeed, the most common way in which our understanding of a variety of planning issues grows and changes is through one set of scholars challenging or building on (and sometimes both) research previously published by other scholars.

I decided in the end to publish both the Millard-Ball et al. rejoinder to the original article and a response from Pierce and Shoup for two major reasons. First, planners are often challenged to prove the efficacy and impact of different planning interventions in a relatively short period of time with limited resources. It seems useful to suggest the potential and the dangers of short-term assessments of any kind of program designed to change people’s behaviors in specific ways. Second, and related, the methodological issues under discussion are not particularly sophisticated in themselves; many readers may have used or commissioned or depended on analyses based on such methods. To the extent that is so, it may be helpful to understand the strengths and weaknesses of such approaches. Finally, I also found both manuscripts to be well written and well argued. I would be interested in reader views on the value of such an exchange, which follows below.

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

Adam Millard-Ball, Rachel Weinberger, and Robert Hampshire

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Pierce and Shoup’s article (2013) provides a long-awaited analysis of San Francisco’s *SFpark*, the most sophisticated parking pricing program in the United States, 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. They compute a mean elasticity for curb parking of $-0.40$ (i.e., that every 10% increase in parking prices would lead to a 4% decrease in demand for parking spaces).

In principle, a performance-based parking system such as *SFpark*, 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. Pierce and Shoup’s findings suggest that the *SFpark* program has had a remarkable impact in an extremely short time, an impact that is substantially faster and greater than that shown in other analyses, including our own (Millard-Ball, Weinberger, & Hampshire, 2014) and others (e.g., 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), specifically the possibility that fluctuations in demand trigger price changes under SFpark's rate adjustment rules.

In this comment, we simulate parking demand as a purely random process, with price having no impact on demand. In 10,000 simulations, we recreate price elasticities similar 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 feel that it is too soon to draw firm conclusions about the impact of performance-based parking pricing programs such as SFpark. The policy takeaway 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 (2013) results do not primarily reflect the impact of price changes on occupancy, it is first necessary to understand how SFpark adjusts rates. (For more general background, the reader is referred to their article, or to the SFpark website at www.sfpark.org.) The San Francisco Municipal Transportation Agency (SFMTA) established a target occupancy range of 60% to 79% on each metered block. If occupancy is 80% or higher, meter rates increase by 25 cents per hour, up to a maximum of $6. If occupancy is less than 60%, then meter rates decrease by 25 cents, or by 50 cents if occupancy is less than 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, see Wooldridge (2012). 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 rate increases if occupancy reaches 80% or more. The rate is increased according to SFpark's rules, but demand would have fallen back toward 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 ranges 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 applies to any block with equilibrium occupancy in SFMTA's target range. Any random fluctuation that pushes occupancy outside the 60% to 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% to 79% range in subsequent months.

Another example of endogeneity occurs when unrelated events drive occupancy. 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.1 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 SFpark vary not only by block, but also by six timebands (before noon; noon to 3 p.m.; and after 3 p.m. on weekdays and weekends). Following Pierce and Shoup, we calculate midpoint elasticities for each of the 5,332 price changes2 in the first year of SFpark 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 25 cents per hour.
For each block and timeband, the SFMTA provides six data points representing the average occupancies in the run-up to each of the six rate adjustments, plus one additional data point following the August 2012 rate adjustment. For each block and timeband, we calculate the mean and standard deviation of these seven data points.

We then simulate “before” and “after” occupancy for each block and 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:

$$\text{Before O}_{it} = \mu_{it} + \varepsilon_{it}$$

$$\text{After O}_{it} = \mu_{it} + \rho \varepsilon_{it} + \delta_{it}$$

where $\mu_{it}$ is the mean occupancy for block $i$ in timeband $t$; $\rho$ is the serial autocorrelation term; $\varepsilon_{it}, \delta_{it} \sim N(0, \sigma_{it}^2)$; and $\sigma_{it}$ is the estimated standard deviation.

The before rate is the pre-SFpark 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 1–3 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 important, we derive even more elastic responses from purely random simulations. Our overall simulated elasticity is $-0.56$, compared with Pierce and Shoup’s $-0.40$. One possible explanation for the difference is that the other forms of endogeneity discussed here, such as new destinations opening on particular blocks, may be biasing Pierce and Shoup’s results toward 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 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).
The somewhat counterintuitive patterns by price change in Figure 3 deserve particular attention. The response is most elastic for the adjustment of –50 cents, and virtually zero for the adjustment of –25 cents. This pattern can be explained as a function of the distribution of occupancies and the rate adjustment regime. Recall that a rate adjustment of –50 cents 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 –50 cents 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.

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 important, their reported average elasticity of –0.40 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 –0.30, with a general range of –0.10 to –0.60. This means that according to Pierce and Shoup, SFpark elasticities lie in the top 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 (Manville & Williams, 2012; Shoup, 2011). Moreover, under SFpark, 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 SFpark program. Further, the presence of latent demand is likely to temper the impacts of price increases on blocks that are almost 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 –0.40, especially in the short term, warrants close scrutiny.

An Alternative Empirical Approach: Regression Discontinuity

Any analysis of SFpark and similar programs clearly needs to explicitly consider the design of the rate adjustment process. Here, we present an alternative method—regression discontinuity—that is widely used to address similar problems of endogeneity. Since the 30%, 60%, and 80% thresholds for rate changes are determined exogenously, blocks just below a threshold can be compared with blocks just above the threshold. If rate changes had 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 with blocks that were at 80% occupancy (where rates increased by 25 cents). 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 top 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—rate changes are discontinuous at each threshold by design—but the purpose here is simply to illustrate how regression discontinuity analysis can work.

The bottom 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 downward at each of the three thresholds. No such discontinuities are evident in the bottom panel, indicating that there is no
discernable short-run occupancy impact of the SFpark rate adjustments. This is confirmed by the regression discontinuity coefficients shown in Table A-1 of the Technical 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 Table A-1 are based on more than two years of SFpark rate adjustment data, and use data through November 2013.6 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.

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, particularly 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 below, 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 SFpark’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 SFpark 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.7 Other elements of the SFpark 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 SFpark 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, 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 policymakers need to commit for the longer haul.

Notes
2. Pierce and Shoup (2013) 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.
3. Pierce and Shoup (2013) do not report exact numbers, but Figure 9 of their article 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.
4. A related design that could be used is the matching design discussed by Dehejia and Wahba (2002).
5. For a discussion of the assumptions behind regression discontinuity designs and applications to the transportation and planning contexts, see Washington, Karlaftis, and Manning (2011) and Deng and Freeman (2011).
7. A similar view has been expressed by the manager of SFpark (Bialick, 2011).

References

Technical Appendix
Table A-1 presents the estimated short-run impacts of rate changes on parking occupancy. 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-cent adjustment. For the 25-cent decrease, it indicates the impact of moving from a −25-cent to a zero adjustment. For the 50-cent decrease, it indicates the impact of moving from a −50-cent to a −25-cent 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.

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.

Each row in Table A-1 represents a separate regression using the subsample where “before” occupancy is within 10% of the threshold. The regression is of the cubic form:

\[ y_i = \gamma + \beta_1 X_i + \beta_2 X_i^2 + \beta_3 X_i^3 + \epsilon_i \]

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
- \( \beta_1, \beta_2 \) and \( \beta_3 \) are estimated coefficients (not reported)
- \( \epsilon_i \) is the error term.
Table A-1. Estimated short-run impacts of rate changes.

<table>
<thead>
<tr>
<th>Rate change threshold</th>
<th>Coefficient</th>
<th>Robust standard error</th>
<th>t statistic</th>
<th>p</th>
<th>N</th>
</tr>
</thead>
<tbody>
<tr>
<td>25-cent increase</td>
<td>1.750</td>
<td>1.243</td>
<td>1.408</td>
<td>0.159</td>
<td>5,771</td>
</tr>
<tr>
<td>25-cent decrease</td>
<td>–2.808</td>
<td>2.145</td>
<td>–1.310</td>
<td>0.190</td>
<td>3,828</td>
</tr>
<tr>
<td>50-cent decrease</td>
<td>7.127</td>
<td>6.611</td>
<td>1.078</td>
<td>0.281</td>
<td>1,268</td>
</tr>
</tbody>
</table>

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 cents) or ceiling ($6), or where a rate change is not made for other reasons. (Inclusion of these observations does not change the results.)

Response to Millard-Ball et al.:
Parking Prices and Parking Occupancy in San Francisco

Gregory Pierce and Donald Shoup

Gregory Pierce (gspierce@ucla.edu) is a doctoral student in the Department of Urban Planning at the University of California, Los Angeles. Donald Shoup, FAICP (shoup@ucla.edu), is distinguished professor in the Department of Urban Planning at the University of California, Los Angeles.

We appreciate Millard-Ball, Weinberger, and Hampshire’s (this issue) explanation of endogenous price changes in the SFpark program, and their observation that regression to the mean may be driving changes in parking occupancy. Further, we agree that the price elasticities estimated from SFpark should be treated with caution. In our article (Pierce & Shoup, 2013), we warned that the price elasticities estimated from SFpark cannot predict how prices affect parking occupancy:

The data show that the price elasticity of demand for curb parking is far from uniform. Elasticity varies according to location, time of day, day of the week, initial price, and date of the price change. The data also show astonishing variation in the price elasticity of demand at the block level…. The wide range of price elasticities suggests, as one would expect, that many variables other than price affect parking demand. In many cases the price elasticity was positive, which means that occupancy either rose after prices rose or fell after prices fell. Higher prices do not cause higher occupancy, and lower prices do not cause lower occupancy, so other factors must have overwhelmed the effects of prices on occupancy in the cases of positive price elasticity. The wide range of elasticity at the block level also suggests that circumstances on individual blocks vary so greatly that planners will never be able to develop a robust theoretical model to predict the correct prices needed to achieve the target occupancy for every block. Instead, the best way to achieve target occupancy is to do what SFpark does: adjust prices in response to the observed occupancy. This simple trial-and-error method mirrors how other markets establish prices, so it should also work in the market for on-street parking. (Pierce & Shoup, 2013, pp. 73–75)

In addition to explaining why planners cannot estimate a single value for the price elasticity of demand for curb parking, we emphasized that the elasticities are not useful for predicting the right prices. Figure 9 in our 2013 article shows that the estimated elasticities ranged widely all the way from below –7 to above +7. Indeed, the elasticity was positive in 36% of the cases. Regression to the mean does not cause positive price elasticities. The many positive elasticities therefore conflict with the surmise that regression to the mean produced SFpark’s results. Many factors other than price affect parking occupancy. Using the positive elasticities to measure the effects of prices would give the impression that SFpark was making things worse, not better.

We agree that random fluctuations in demand, regression to the mean, and endogenous price changes could have produced the parking occupancy results during the first year of SFpark. But did they? Fortunately, the additional evidence from SFpark’s second year strongly suggests that its benefits are real and not a statistical artifact. The strongest evidence comes from the convergence of parking occupancy rates to the target occupancy range.

Movement Toward the Target Occupancy

The research question we posed in our article was, “Did SFpark change drivers’ behavior in the right direction?” Perhaps the clearest evidence of SFpark’s effectiveness is the