Spring 5-15-2020

Who Benefits from Park and Recreation Improvements in San Francisco? A case study of recreation center renovations in San Francisco

Safia Belayadi
sbelayadi@dons.usfca.edu

Who Benefits from Park and Recreation Improvements in San Francisco?*

A case study of recreation center renovations in San Francisco

Safia Belayadi
Advisor: Jessie Anttila-Hughes
International and Developmental Economics
University of San Francisco
2020

ABSTRACT

The purpose of this study is to examine the efficacy of city policies undertaking public goods investments to benefit disadvantaged communities in San Francisco. Namely, is the process of improving the quality of public goods serving targeted populations, or does it lead to unintended consequences such as gentrification? I take advantage of the timing of city recreation center renovations and the synthetic control method to capture any difference in the proportion of users that is poor before and after the renovation date. I use San Francisco Recreation and Park Department registrant data containing user zip codes and census demographic data at the tract-level to create a blended average control for each of the six treated recreation centers that have been renovated in the city. I assign each recreation center to an analysis neighborhood and use free and reduced lunch eligibility across neighborhoods as a proxy for whether or not a recreation center user is poor. In general, we see a higher proportion of poor users in treated centers post renovation in the long-term relative to the synthetic control. Considering existing literature pointing to the positive impacts of parks and recreation services on health and other outcomes of users, evaluating policies that strive to close these disparities needs to be prioritized.

* I would like to thank my advisor Jessie Antilla-Hughes for supporting my research idea from the very beginning, and the IDEC students and professors for all of their sincere feedback. Thank you to the San Francisco Recreation and Park Department staff for generously providing me with recreation center data and other information relevant to my work. Finally, a huge thank you to my family and friends who continuously motivate and push me in ways I cannot do for myself.

Table of Contents

Introduction

Parks and recreation services are shown in the economics literature and other disciplines to have heterogeneous effects across groups and cities in the United States. Parks and recreation services- hereafter PRS- are public goods that include but are not limited to playgrounds, parks, forests, trails, and recreation centers. Early neighborhood effects via exposure to these public goods in childhood have been shown to have positive long-term socio-economic outcomes on users (Chetty et al. 2014; Bell et. al 2018). PRS impact on health, wellbeing, and community engagement has also been given considerable mention in the literature (Heckman 2006; Pryor et al. 2014). As a result, large public investments in these services by city governments have been poured into areas to improve their quality and access. An analysis of whether these improvements actually benefit targeted communities has been explored in various contexts to examine any distributional consequences (Kazmierczak 2013; Jeffres et al. 2009; Banzhaf et at 2006; Kahn 2009). In particular, whether PRS improvements unintentionally attract more affluent users and lead to gentrification and displacement via increasing rental prices has been considered for informing public policy.

While the literature on the effects of public goods focuses considerably on the role of access, this study assesses the role quality can play on diverse distributional effects. In particular, it looks at general outcomes of user demographic composition following public goods improvements. Using city recreation center renovations by the San Francisco Recreation and Parks Department as a proxy for quality change, this paper applies the synthetic control method (SCM) to measure any considerable change in the proportion of poor users before and after a renovation. The status of a recreation center user as poor is predicted by merging individual user zip code data with the proportion of school children eligible for free and reduced lunch by zip code. Given free and reduced lunch eligibility depends on if a child's household income is below 185 percent[1] of the poverty level or if it receives SNAP or TANF (U.S. Department of Agriculture, National School Lunch Program), this acts as a sufficient proxy for overall low-income status. The sample includes 6 renovated recreation centers, each of which are compared to a synthetic control consisting of a weighted average of 6 non-renovated recreation centers. The weight assigned to each of the control recreation centers depends on how well it mimics the characteristics of the treated recreation center before the renovation, given a set of predictors and outcome trends.

Results show that for 5 out of 6 of the recreation centers over time, the proportion of users that is poor is higher than the synthetic control. This gap is shown to increase with time for these, highlighting the role time plays in measuring renovation effects. Only for one of the recreation centers do we see a lower proportion of poor users over time in the recreation center post-renovation relative to its synthetic control. Results from this study show an increase in poor RC users overall, hinting at the efficacy of city public investment projects targeting public goods in disadvantaged communities. When considering theories pointing to unintended policy consequences like gentrification and displacement, results suggest that the opposite may be happening- needy communities are in fact benefiting from these improvements.

[1] A child from a household with an income 130 percent below the federally mandated poverty level is eligible for free meals. A child from a household with an income between 130 and 185 percent below the federally mandated poverty level is eligible for reduced meals. These guidelines are determined federally by the U.S. Department of Agriculture via the National School Lunch Program (U.S. Department of Agriculture Food and Nutrition Service, National School Lunch Program Fact Sheet, page 1).

It is important to note that the sample size of this study only includes 12 recreation centers with a time range spanning from 2007 to 2020. The small sample size limits the power of our test, while limited pre-renovation data for recreation centers renovated closely after 2007 affects the strength of the fit between renovated centers and the synthetic control. This in turn reduces the confidence in stating that results for the recreation centers with limited pre-renovation data are statistically significant. This is apparent in the results which show a much better fit between the treated and control unit outcome trajectories for the recreation centers renovated much later than 2007. Expanding this analysis to recreation centers across cities in the Bay Area would allow us to determine whether these results are in fact consistent. In addition, more demographic data on recreation center users would improve the predicted effect of renovations across different groups of users (rather than relying on demographic tract level and zip code level data to infer these characteristics).

The structure of the paper is as follows: first, I include a brief literature review on the general relationship between public goods and social welfare; second, a background on the San Francisco Recreation and Parks Department and its recreation centers is given; third, data on recreation center users and various neighborhood characteristics is presented in the Data section; fourth, the Empirical Model section describes the use of the synthetic control and difference-in-difference models to measure renovation effects; fifth, results are laid out in the Results section followed by robustness checks in the Inference and Placebo Tests section; sixth, I summarize results from both empirical models in the Summary of Results section; finally, I finish with concluding remarks and a brief note on the limitations of this study.

Literature Review

I. Public Goods and Long-term Neighborhood Effects

Exposure and access to adequate public goods has been shown by many in the literature to result in positive long-term outcomes. Bell et al. (2018) - in their study measuring the effects of one's childhood environment on later adult socio-economic outcomes- reveal that individuals who grow up in neighborhoods with higher innovation exposure are more likely to be inventors relative to those from neighborhoods lacking in technology access. More so, they (2018) find that those who move to higher income neighborhoods with higher invention rates are more likely to invent than those who remain in their original neighborhoods. Applying these findings to the context of PRS, improvements in these public goods may significantly impact the trajectory of individuals who use them, especially relative to pre-improvement conditions. Similarly, Chetty et al. (2014) observe a positive correlation between intergenerational mobility and social capital, where the latter is defined as including social networks and engagement in local community organizations. Recreation centers and parks, as centers for community engagement and development, may thus be critical to breaking down the barriers to intergenerational mobility characteristic of the U.S. Finally, Heckman (2006) emphasizes the role of early childhood skill development on later social and economic outcomes, stressing that a lack of the former places individuals at an early disadvantage relative to individuals who receive more cognitive and noncognitive stimulation in their childhood (Heckman 2006). He finds that the rate of return to human investment on disadvantaged children earlier on is greater than at later stages, especially investment in noncognitive skills (Heckman 2006). This is because the traits that are developed by children from such investments- like motivation and perseverance- help them succeed economically and socially later on as adults.The noncognitive skills recreation centers and parks offer neighborhoods- as spaces for creativity, learning, and physical exercise, among others- can amount to substantial benefits later on for society.

II. Public Goods Quality and Networking

For inner cities and large metropolitan areas, PRS serve to foster the social and community networks crucial to outcomes of physical and mental health. Aleksandra Kazmierczak (2013), in her study of three UK inner cities, looks at the contribution PRS have on social networks- the latter of which have largely deteriorated due to the nature and structure of urban areas. Kazmierczak (2013) also stresses the importance of PRS quality in nurturing substantial long-term social interaction, highlighting the differential impacts of public goods based on quality. In other words, simply assessing whether or not a public good exists does not tell us much about whether or not a given neighborhood is provided for. The quality of that good determines whether or not people actually use the resource in the long term, motivating the topic for this paper: the effects of PRS renovations. Similarly, Pryor et al. (2014) explain how impactful PRS such as the YMCA are on marginalized inner-city youth. These spaces instill in youth who use them a sense of hope, increased self-efficacy, and community (Pryor et al. 2014). In the context of disadvantaged communities, having these recreational spaces as a resource to counter social and economic marginalization is crucial. In their national survey asking individuals to identify spaces in their neighborhoods where they feel a sense of community, Jeffres et al. (2009) list PRS among these spaces. They find that those in inner cities are less likely to mention having these spaces than those living in the suburbs or small towns- highlighting the lack of these spaces in larger metropolitan areas (Jeffres et al. 2009). Hypothesizing that individuals with access to spaces of community engagement and support will report higher levels of life quality, Jeffres et al. (2009) see evidence of this in their data, showing a negative correlation between the lack of the former and the latter. Similar to Kazmierczak (2013), Jeffres et al. (2009) encourage future research to focus on the quality of public goods when assessing lasting impacts. Whether PRS have lasting effects on specific populations like at-risk youth- as opposed to simply being short-term distractions from street life- is a question several researchers, such as Witt et al. (1996), ask. Instead of focusing on the police force to handle the issue of inner-city crime in the hands of youth, evidence of positive PRS impacts in this context is argued by Witt et al. (1996) to be better in informing public policy. However, more extensive research by way of PRS evaluations needs to happen in order to better measure what makes PRS successful in specific contexts and which neighborhood issues they help mitigate.

Examining factors such as PRS quality, proximity, and acreage- as recommended by Rigolon (2017)- is important for answering questions regarding disproportionate PRS use among youth and minority communities. Policies focusing solely on proximity show low-income and minority communities to be closer than more affluent communities to PRS (Rigolon 2017)- making it appear as though access is not an issue. However, as explained by (Rigolon 2017), affluent communities have more access to PRS acreage per person, to better quality PRS, and to safer PRS relative to disadvantaged communities. Thus, to fully comprehend PRS use disparities between communities of different ethno-racial and economic backgrounds in metropolitan cities such as San Francisco, researchers and policy makers must consider all of these factors that encompass PRS use. As mentioned by Formoso (2012), spaces such as PRS are institutional resources at the neighborhood level which deeply influence child outcomes later on. As such, maximizing the benefit of PRS for disadvantaged neighborhoods -through successful equity policies- can be paramount.

III. Public Good Investments, Gentrification and Displacement

Policies intending to improve public goods quality in disadvantaged communities need sufficient evaluation, especially in light of evidence they may be unintentionally spurring gentrification. This is in part due to the housing and consumption changes that may result from more affluent groups coming into an improved space. Coupled with potential displacement due to housing loss, these two phenomena motivate suspicion towards public goods urban improvements. Several researchers- such as Banzhaf et al. (2010)- have started to challenge these policies which historically turned to public goods investment as a solution to inequality and segregation. They (2010) look at the impact of public goods and their locations- which they consider to be exogenous- on group segregation levels. They find that location specific interventions in marginalized high minority areas tend to attract higher-income minorities post-intervention- increasing racial/ethnic segregation (Banzhaf et al. 2010). This contradicts the gentrification framework defined above in some aspects, which hypothesizes both racial and income composition effects in former minority spaces following public good investments, which in turn leads to minority displacement. Though this finding explains the entrance of higher-income individuals following public goods investment, Banzhaf et al (2010) reveal that these individuals actually belong to the minority group. They (2010) conclude that the spatial distribution of public goods, household income, and household tastes do nothing to curb segregation levels, posing a huge task for public policy makers regarding anti-segregation agendas.

Similarly, in their study looking at compositional changes in neighborhoods following pollution cleanup- generally referred to as environmental gentrification- Banzhaf et at (2006) find a statistically significant income compositional effect but a weak racial composition effect. In other words, richer households are shown to move into newly-cleaned areas- presenting an issue if the benefactors of these environmental quality policies are not poor individuals who tend to suffer from pollution the most but instead the richer households that relocate to these areas. Also examining the effects of public goods investment on later demographic composition, Kahn (2009) studies how improved public transportation access affects population sorting of major cities following transit expansions. He finds heterogeneous effects of transit expansion on gentrification (near station locations) across his sample of cities by type of transit station: those that involve driving to the station to park your car before riding (longer commute distance) are more associated with resulting poverty in the surrounding area, versus stations closer to city centers that involve a short walking distance before riding (Kahn 2009). Based on the above-mentioned findings, more extensive research needs to be made considering the interplay of distance to public goods, their type, and the unique contexts of the cities they are located in.

While gentrification and development often go hand-in-hand and may even benefit affected residents by galvanizing economic investment, it is important to consider the distribution of benefits on different resident groups. As the displacement literature points out, mobility patterns in the face of gentrification are heterogenous across groups. Ding et al (2016) in their study of gentrification and displacement in Philadelphia, emphasize examining the quality of moving or not moving for less advantaged groups, as opposed to focusing only on whether they move. They (2016) find that in general, less advantaged groups in Philadelphia are no more likely to move out of their gentrifying neighborhoods than the same demographic group in non-gentrified neighborhoods; if they do end up out-migrating, it is to more disadvantaged neighborhoods. In the context of this paper, findings from Ding et al. (2016) show that the majority of less advantaged groups that end up remaining within gentrifying areas may still benefit from the quality improvement of public goods in their neighborhood. As argued by Formoso et al. (2010), understanding the conditions under which gentrification positively and negatively impacts groups may help urban planners push for policies that maximize benefits for both groups. Identifying the general patterns of gentrification stages- such as visible ones mentioned by Grier et al. (2018) with the turn up of new coffee shops, dog parks, specialized businesses, and bike lanes- and resulting changes in consumption patterns is a start. Recognizing past barriers to PRS access (and the low quality of PRS that were available) for minority groups in the country and their lasting effects must be taken into account, as argued by Byrne (2012). For city planners seeking to develop PRS in disadvantaged communities- with the intention of increasing PRS use by those in these communities- it is paramount as stressed by Bryne (2012) that PRS characteristics align with the socio-cultural practices and preferences of said communities. If not, these policies become exclusionary and fail to do what they are supposed to do- increase the welfare of these communities by giving them spaces to thrive and foster lasting community bonds.

San Francisco Recreation and Parks Department

All city-owned parks and recreation centers- hereafter RC-in San Francisco are governed by the San Francisco Recreation and Parks Department (SFRPD). RCs offer a variety of programs to San Francisco residents of all age groups including but not limited to art, aquatics, programs for seniors and tots, science and technology, fitness and dance, and youth after-school programs and sports. RCs generally consist of outdoor play areas and indoor gyms, pools, and community rooms made available to the neighborhood for events. Centers that specialize in the arts offer art and photography studios, while some like Randall RC include a hands-on science museum. Individuals can enroll for Fall, Winter, Spring, and Summer programs. Drop-in activities are also available for those not enrolled in a session. In 2005, the City enacted what is now a yearly Capital Plan that uses funding primarily from bonds to renew and improve parks and recreation facilities. San Francisco residents vote to pass these bonds, and the bond amount is in the hundreds of millions. In addition to bond funds, RC renovations also rely on a combination of grants, donations, and City funding to meet project costs. These projects generally involve making infrastructural improvements to buildings, playgrounds, pools and outdoor restrooms, and prioritize facilities that pose public safety concerns.

After the approval of Proposition B in 2016, SFRPD implemented an Equity Strategy requiring the agency to consider equity in its allocation of resources to city PRS.[2] Identifying which census blocks in the city were disadvantaged was conducted to develop strategies to increase equity in PRS quality and access across SF. SFRPD adopted the California Protection Agency's definition of "disadvantaged" communities to determine its new resource allocation priorities. Cal-EPA scores census blocks across the country based on ten population characteristics (asthma, cardiovascular disease, low birth weight, department visits, linguistic isolation, poverty, educational attainment, housing burden, unemployment and household income) and ten pollution burden indicators (ozone concentrations, PM2.5 concentrations, diesel PM emissions, drinking water quality, pesticide use, toxic releases from facilities, traffic density, cleanup sites, groundwater threats, hazardous waste, impaired water bodies, and solid waste sites and facilities) pulled from the 2010 Census (Faust et al. 2017). The top 25 percent highest scoring census blocks are designated as disadvantaged- an index Cal-Epa has named EnviroScreen. While EnviroScreen does not include race, ethnicity or age as indicators for disadvantaged census blocks, SFRPD's Equity Strategy adds age and a non-white indicator to those already in EnviroScreen, as well as a quarter-mile buffer zone from the permiter of equity zones (SFRPD 2018). The Equity Strategy designates the top 20 percent highest scoring census blocks (relative to other census blocks in SF) as disadvantaged- amounting to 39 census blocks and 89 parks in fiscal year 2018/2019 (SFRPD

[2] Section 16.107 (a) of the Park, Recreation and Open Space Fund of the Charter states: "The Department embraces socio-economic and geographic equity as a guiding principle and commits to expending the funds across its open space and recreational programs to provide park and recreational access to all of San Francisco's diverse neighborhoods and communities."

2018). Figure A in the Data section below presents a map displaying the disadvantaged census blocks in the city shaded in red, while the blue symbolizes a ¼ mile buffer zone.

Data

The data used in this study comes from SFRPD registrant data from 2007-2020 containing 159,486 entries across all of the RCs in the sample. SFRPD does not keep detailed data on the usage of their city parks and other open areas, but does have RC data. Given these RCs offer a large and diverse choice of programs year round which cater towards all age groups and types, I consider them to be a sufficient public good proxy. Data includes registrant information for each of the following RCs: Betty Ann Ong RC, Glen Park RC, Hamilton RC, Palega RC, Randall RC, Sunset RC, Bernal Heights RC, Eugene Friend RC, Mission RC, Potrero Hill RC, St. Mary's RC, and Tenderloin RC. Data on renovation dates is also provided by SFRPD. A total of six RCs have before and after renovation data- forming the treatment group- and are matched with six RCs that have never been renovated- forming the control group. Other recreation facilities in the city have undergone renovations prior to 2007 but have been excluded from the sample given there is no data from SFRPD for this time period. Registrant information consists of registrant age, registrant gender, registrant home zip code, RC zip code, activity enrollment, and the session and year. Registrant street addresses are missing from the data given it is considered to be personally identifiable information.

The poverty status of registrants is not available in the registrant data. Relying on registrant zip code data to infer this information in a city as polarized as San Francisco is unsound, where groups from either extremes of the socio-economic ladder may reside in a single zip code. To compensate for this, I proxy for low-income status using the proportion of school children by zip code eligible for free or reduced price meals (FRPM). This eligibility is federally mandated by the U.S. Department of Agriculture as part of the National School Lunch Program. Children from households with incomes 130 percent below the federally mandated poverty level are eligible for free meals, while those with incomes between 130 and 185 percent are eligible for reduced meals[3]. Given the majority of RC users are children and the fact that FRPM eligibility is a good indicator of household low-income status, using this to predict whether an RC user is poor is sufficient. FRPM data comes from the California Work Opportunity (CalWORKS) program data (1988 - 2003) and the California Department of Education (CDE) through the California Longitudinal Pupil Achievement Data System (CALPADS) (2004 - 2019). Yearly FRPM eligibility for both free and reduced meals is combined and then averaged over all schools within a given zip code to get the average eligibility for that zip code. This yearly average is then matched with individual registrant zip code data for each RC to construct the outcome variable: the proportion of RC users that is poor.

I retrieve demographic data from the Census Bureau, the American Community Survey (ACS), and DataSF at the tract and year level to proxy for average neighborhood characteristics- data that form my set of predictors. Data on median household income and rent, age and race distribution, public transportation use and commute time, and various low-income measures used to calculate Metropolitan Transportation Commission (MTC)

[3] U.S. Department of Agriculture Food and Nutrition Service, National School Lunch Program Fact Sheet, page 1.

Communities of Concern (COC) is retrieved from the Census Bureau (2000 Census and 2010 Census) and the American Community Survey (2010-2018 ACS). Data on eviction notices, crime, the proportion of affordable housing, property values, and active businesses is retrieved from DataSF (2007-2020). Voter turnout for municipal elections comes from the San Francisco Department of Elections (2007 - 2019).

Demographic tract level data for my predictor set (covariates) is grouped by analysis neighborhood. There are 41 analysis neighborhoods in San Francisco which the Planning department groups by 2010-year census tracts. Tracts by analysis neighborhood were adjusted to 2000-year census tracts for data collected from the 2000 census- this was used for predictor data prior to 2010. As shown in Table A below, the 12 RC locations are assigned to 11 different analysis neighborhoods. A map of the city's analysis neighborhoods is included in the Appendix . Table A also lists all of the RCs used in my sample, whether they are in the treatment group (T) or control (C), the analysis neighborhood they are located in, the year they were renovated, and the number of years before and after renovation. I use the year RCs close as my treatment year to be conservative in estimating any changes in my outcome variable, rather than the year RCs open to the public. Figure A is a map showing where the RCs are located in the city relative to designated equity zones and larger zones constructed within a ¼ mile buffer. Those that have been renovated are represented in yellow while those that have not are represented in black.

Table A: Sample Summary

REC CENTER	ANALYSIS NEIGHBORHOOD	GROUP	TIME RANGE	TREATMENT YEAR	PRE-YEARS	POST-YEARS
Hamilton Recreation Center	Japantown	T	2007-2020	2008	1	12
Betty Ann Ong Recreation Center	Nob Hill	T	2007-2020	2010	3	10
Sunset Recreation Center	Sunset/Parkside	T	2007-2020	2010	3	10
Palega Recreation Center	Portola	T	2007-2020	2011	4	9
Glen Park Recreation Center	Glen Park	T	2007-2020	2015	8	5
Randall Museum	Castro/Upper Market	T	2007-2020	2016	9	4
SOMA/Eugene Friend Rec Center	South of Market	C	2007-2020	.	.	.
Potrero Hill Recreation Center	Potrero Hill	C	2007-2020	.	.	.
Tenderloin Recreation Center	Tenderloin	C	2007-2020	.	.	.
Bernal Heights Recreation Center	Bernal Heights	C	2007-2020	.	.	.
Mission Recreation Center	Mission	C	2007-2020	.	.	.
St. Mary's Rec Center	Bernal Heights	C	2007-2020	.	.	.

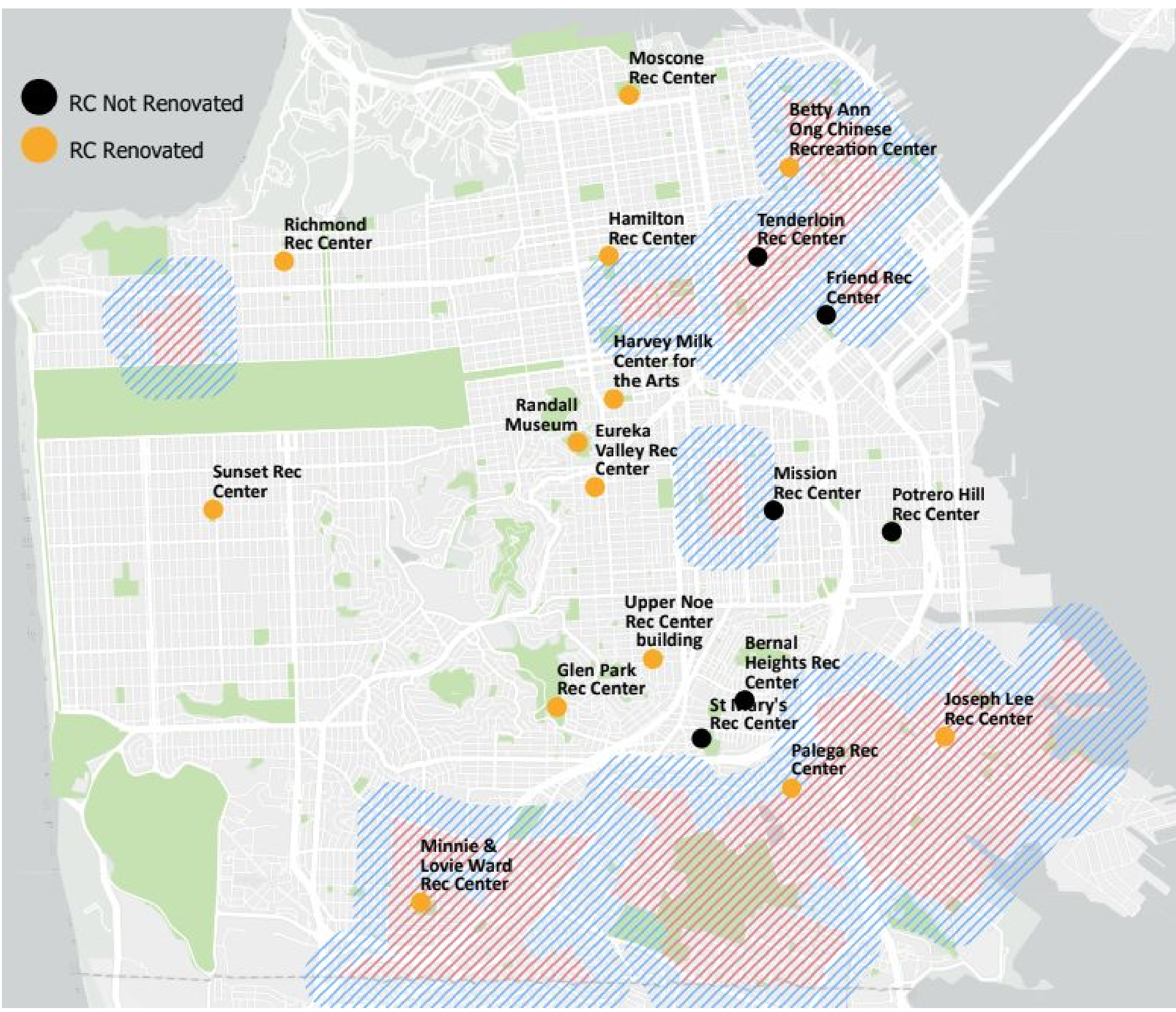


Figure A: Map of recreation center locations and SFRPD established equity zones

Empirical Model

I. Synthetic Control Method

I use the synthetic control method (SCM) to identify any causal impact of RC renovations on the proportion of users that is poor. I use SCM as an identification strategy given my small sample of 6 treated and 6 control RCs. It is because of this that I do not use an event-study analysis, where such a small sample size would significantly decrease the power of the test. The treatment group consists of RCs that have received a renovation- Randall RC, Glen Canyon RC, Palega RC, Sunset RC, Betty Ann Ong RC, and Hamilton RC. Those that have not been renovated are considered the control units, which are the same for each treated RC- Bernal Heights RC, St. Mary's RC, Mission RC, Potrero Hill RC, South of Market RC, and Tenderloin RC. My outcome of interest is the proportion of RC users that is poor- matching zip code level child eligibility for free and reduced lunch with RC registrant zip code locations as a proxy. I use the predictors mentioned in the data section above at the tract level to compare across RCs. Borrowing from Abadie et al. (2010):

$$
\begin{aligned}
Y_{it} &= Y_{it}^N + \alpha_{it} D_{it}. \\
Y_{it}^N &= \delta_t + \theta_t Z_i + \lambda_t \mu_i + \varepsilon_{it},
\end{aligned}
\qquad
D_{it} = \begin{cases} 1 & \text{if } i = 1 \text{ and } t > T_0, \\ 0 & \text{otherwise.} \end{cases}
$$

Y_{it} represents the proportion of RC users in analysis neighborhood *i* at year *t*. D_{it} indicates whether an RC received treatment (1) or not (0) in analysis neighborhood *i* at year *t.* The estimator, α_{it}, is the effect of the renovation on an RC in analysis neighborhood *i* at year *t*. Y_{it} is equal to the sum of the treatment effect ($\alpha_{it}D_{it}$) and Y_{it}^N, the unobserved counterfactual. Y_{it}^N is a factor model containing: δt (an unobserved common time-dependent factor), Zi is a (1 × r) vector of observed covariates, θt is a (r × 1) vector of unknown parameters, λt is a (1 × F) vector of unknown common factors, μi is a (F × 1) vector of unknown factor loadings, and εit are unobserved transitory shocks.[4]

For each treated RC, the sum of the weighted average ($w^*_2, \ldots, w^*_{i+1}$) of non-treated RCs (equal to 1) that best mimics the characteristics of the treated RC is constructed - hereafter the synthetic control (Abadie and Gardeazabal 2003):

$$\sum_{j=2}^{J+1} w_j^* Y_{j1} = Y_{11}, \ \ldots \ , \sum_{j=2}^{J+1} w_j^* Y_{jT_0} = Y_{1T_0}, \quad \text{and} \quad \sum_{j=2}^{J+1} w_j^* Z_j = Z_1.$$

The synthetic control is created by closely matching the treated RCs and non-treated RCs on outcomes and predictor variables of the pretreatment period. Assuming close matching, any difference between the treated RCs and the synthetic control group after treatment is taken as the impact. According to Abadie et al. (2010), if the standard condition is as follows:

$$Y_{1t}^N - \sum_{j=2}^{J+1} w_j^* Y_{jt}$$

this will equal to 0 if the amount of pre-renovation periods is large relative to the scale of εit. It follows then that the unbiased estimator of α_{it} is:

$$\widehat{\alpha}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}$$

4 https://yiqingxu.org/teaching/17802/synth.pdf

Using SCM for this study is valid if we hold the following assumptions: the demographic composition of non-treated RC users is not affected by renovations of treated RCs; second, there is no effect on RCs selected for treatment before the renovation actually begins; and third, the counterfactual RC can be created using a fixed amount of RCs from the synthetic control (McClelland et al. 2017). SCM is useful if the treatment and synthetic control do not follow parallel trends (as demanded by the difference-in-differences approach).

I use data from 2007 to 2020 as the period of interest based on the availability of RC registrant data. The pretreatment year range varies across the treated RCs, which spans from 2007 up to the treatment year. I match each treated RC to a weighted average of non-treated RCs (which sum to 1) based on predictor values averaged over the entire pretreatment period. The following predictors are averaged over the pretreatment period: (1) whether or not an RC is assigned to an analysis neighborhood designated as an equity zone (dummy variable), (2) RC distance to the nearest BART station and the average distance to a school, (3) total population, (4) racial distribution, (5) share of population under 20, 20-44, and over 75, (6) median gross rent, (7) median household income, (8) public transit use, (9) commute time if over an hour, (10) single parent households, (11) level of English proficiency, (12) low-income households (below 200% poverty level), (13) disabled households (14), rent-burdened households, (15) zero-vehicle households, (16) property values, (17) businesses opened, (18) affordable housing units, (19) total housing units, (20) number of police reports (as a proxy for crime), (21) eviction count, (22) voter turnout in municipal elections and (23) trends in the outcome variable.

These predictors were chosen for their ability to show variation across neighborhoods in San Francisco and their role in explaining the outcome variable. Given the short pretreatment range over which I can average predictor values (Randall RC allowing for the longest range of 9 years while Hamilton RC only providing us with 1 pretreatment year), I use a large number of predictors to strengthen the goodness of fit between the treated RC and its synthetic control (Abadie 2019)- with the goal of controlling for as much as possible. I also match on outcome variable trends as it is more important to have similar trends than it is to have similar averages. By matching on outcome trends, this helps to account for any significant predictors I may be missing from my model. RCs with earlier renovation dates do not include all of the predictors in the set given these dates precede available predictor data. The SCM creates neighborhood-level weights for each RC to form the synthetic control, which depends on the weights placed on the predictor variables (McClelland et al. 2017). No similar treatment has occurred in the synthetic control RCs during this time period by SFRPD. RCs that received treatment prior to 2007 are dropped given lack of pretreatment data.

I. Difference-in-Difference Method

The difference-in-difference method (DID) is an identification strategy used in econometrics to estimate causal effects of treatment when there are two periods (pretreatment and post-treatment) and assignment groups (treatment and control group). DID uses a natural experiment (treatment taken as-if-random) where the control group is untreated in both periods while the treated group receives treatment in the second period.

DID assumes treatment and control groups follow parallel trends in the pretreatment period, where the unit that received treatment could not have been on an upward trajectory regardless of the treatment. If this is not the case, results estimated can be biased. A DID estimator of the treatment effect is constructed which takes the difference between the treatment and control groups before and after the treatment year. This method removes the bias that results from simply taking the difference between the treatment and control groups that could be due to systematic differences between the two, as well as the bias from comparing the treatment group to itself over time- which could be due to trends (simple differencing).

Using the DID method to measure RC improvements has its caveats, however, if we consider that the process of choosing which RCs get renovations is endogenous. In other words, if the city government of San Francisco decides to renovate RCs that are specifically located in poorer neighborhoods, the DID estimator is biased in that the parallel trends assumption does not hold. If the renovation never happened for a RC that is in a poor neighborhood, one can easily see that it would lead to a negative difference in differences. This is because the neighborhoods that RCs are located in differ systematically in terms of the distribution of wealth. Another caveat of using the DID approach is that it is more useful to explain the effects of a policy for a short time window- as a result of its parallel trends assumption. If we want to measure the long-term effects of renovations on the income status of RC users, however, relying on this method is insufficient and may bias estimates downward.

Given the nature of this paper's panel data, I include time-varying covariates and RC specific time trends to relax the parallel trends assumption characteristic of the traditional ordinary least squares DID estimation. Assuming homogeneity within RCs, below is the DID equation used to estimate the effect of a renovation on the RC's proportion of poor users with time fixed effects and group fixed effects:

$$Y_{RC,Year} = \beta_0 + \beta_1(D_{RC}) + \beta_2(T_{Year}) + \delta(D_{RC} \times T_{Year}) + Z_{RC,Year}'\theta + \varepsilon_{RC,Year} \quad (1)$$

where $Y_{RC,Year}$ is the proportion of poor users in RC at year T, D a dummy variable which takes on the value of 1 if the RC is a treated RC (0 otherwise), T_{Year} a dummy variable which takes on the value of 1 if the year is the treatment year (0 otherwise), $D_{RC} \times T_{Year}$ an interaction term of the former two terms where δ is the estimator of interest (effect of renovation), $Z_{RC,Year}'$ the set of predictors used for pretreatment matching, and $\varepsilon_{RC,Year}$ the error term.

Results

I. Results: Synthetic Control Method

Below are the results from each of the six treated RCs relative to their synthetic control. The latter is a weighted average of the control units matched on pre-treatment outcome trends and various predictors. The control units are the same for each treated RC, which include Bernal Heights RC (Bernal Heights), St. Mary's RC (Bernal Heights), Mission RC (Mission), Potrero Hill RC (Potrero Hill), South of Market RC (South of Market), and

Tenderloin RC (Tenderloin). Optimal weights are determined so as to minimize the root mean squared prediction error (RMSPE) in the pretreatment period of the proportion of RC users that is poor (a good fit). The graphs are scaled in that the outcome variable is normalized so that its last pretreatment period outcome is equal to 1. For each RC, the first graph represents the trend in proportion of RC users that is poor in the treated RC (solid line) and its synthetic control (dotted line) for the period 2007-2020. If there is little difference between the two lines in the pretreatment period, it means the synthetic control is a good fit to act as a counterfactual, allowing for differences in the post-treatment period to be attributed to an impact from the renovation. The second graph shows differences in the renovation effect between the treated RC and its synthetic control for the entire time range. For Randall RC, Glen Canyon RC, Palega RC, Sunset RC, and Betty Ann Ong RC, a portion of the pretreatment period is used as a training period (where predictor outcomes for each of these years are added to the model) while the remaining periods form the validation period.

I. Randall RC (Castro/Upper Market)

Figure 1 plots the proportion of users that is poor for Randall RC and its synthetic control, with the renovation year in 2016 and a total of 9 pretreatment years. The first 5 periods of the pretreatment period are used as the training period, while the remaining pretreatment periods are the validation period. We observe that the trajectory of synthetic Randall closely follows the pathway of Randall RC for the entire pretreatment period- with an RMSPE of .0411 (see Appendix). Synthetic Randall is slightly higher than Randall RC until about 2014, where we then see a much smaller difference between the two during the last 2 pretreatment years. The largest difference between the two is around 2013. After the renovation in 2016, we observe that Randall RC has a slightly higher ratio of poor RC users relative to the synthetic control and they are both decreasing. In 2018, the two trajectories diverge dramatically, where Randall RC becomes positive, while synthetic Randall continues on its downward trend.

Figure 1.1 graphs the differences between Randall RC and synthetic Randall, where the post-treatment period is taken as the effect. For the entire post-treatment period, we observe that Randall RC has greater effects from its renovation than its synthetic control, and this effect increases on the outcome variable significantly over time. By 2020, the effect of the renovation is about 50% greater on Randall RC than it is for its synthetic control. This suggests that for Randall RC, the effects from the renovation are more pronounced over time, shown in the substantial divergence relative to its synthetic that occurs after 2018.

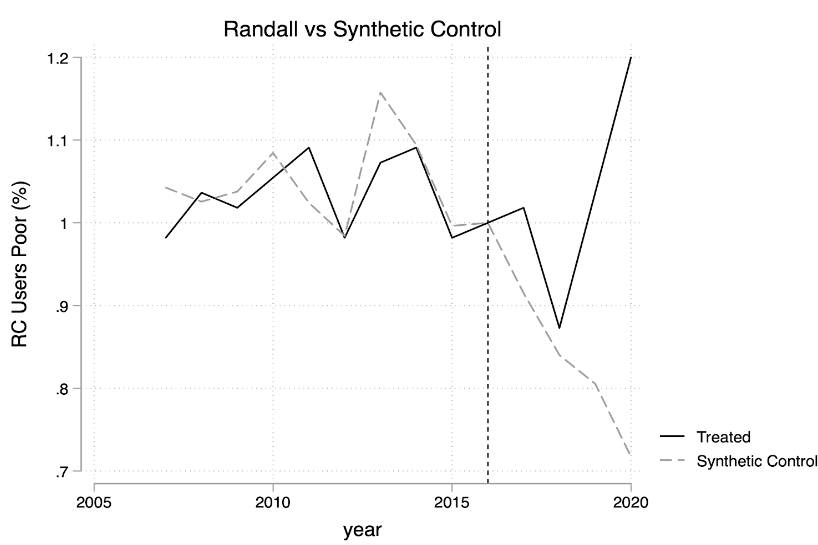


Figure 1: Randall RC vs. Synthetic Randall (scaled)

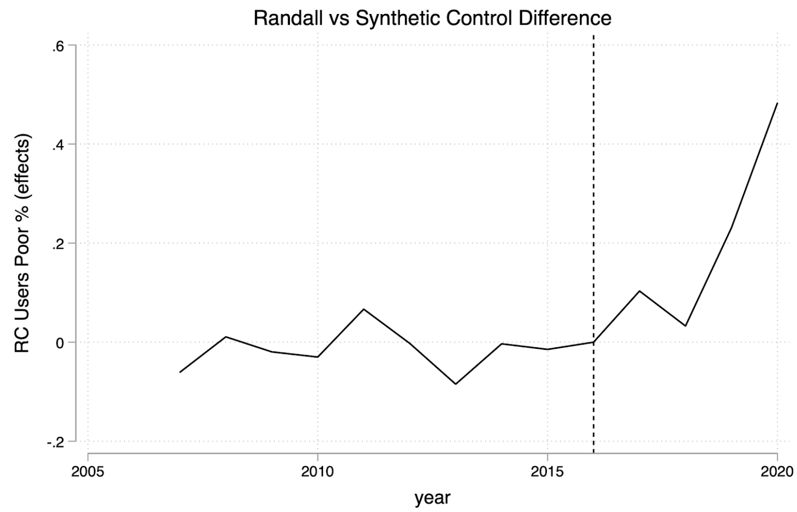


Figure 1.1: Outcome Gap between Randall RC vs. Synthetic Randall (scaled)

II. Glen Canyon RC (Glen Park)

Figure 2 plots the proportion of users that is poor for Glen Canyon RC and its synthetic control, with the renovation year in 2015 and a total of 8 pretreatment years. The first 4 of the 8 pretreatment periods are used as the training period, while the remaining periods are the validation period. We see that synthetic Glen Canyon's trajectory follows Glen Canyon RC very closely in the pretreatment period- with a RMSPE of .0227. In the first post-treatment period, there is almost no difference between Glen Canyon RC and its synthetic control. In 2016, Glen Canyon RC drops more than synthetic Glen Canyon and both follow a downward trajectory. In about 2018, both trajectories switch to a positive slope, and Glen Canyon RC's proportion of poor users increases significantly and surpasses synthetic Glen Canyon throughout the post-treatment period. In 2019, both trajectories switch to a downwards slope albeit at different levels.

Figure 2.1 graphs the differences between Glen Canyon RC and synthetic Glen Canyon, where the post-treatment period is taken as the effect. In the year before the renovation, Glen Canyon RC's effect is smaller, though the difference disappears leading up to the renovation. In the post-treatment period, Glen Canyon RC's effect is smaller than its synthetic control until about 2018. From 2018 onward, the effect from the renovation on the proportion of poor users is larger on Glen Canyon RC than its synthetic control. By 2020, the effect of the renovation on Glen Canyon RC is about 10% greater than on its synthetic control. This shows that positive effects on the proportion of poor users for Glen Canyon RC from the renovation do not appear until about four years after the renovation, and this effect is relatively large considering fluctuations of about .05 above and below 0 in the outcome for prior years.

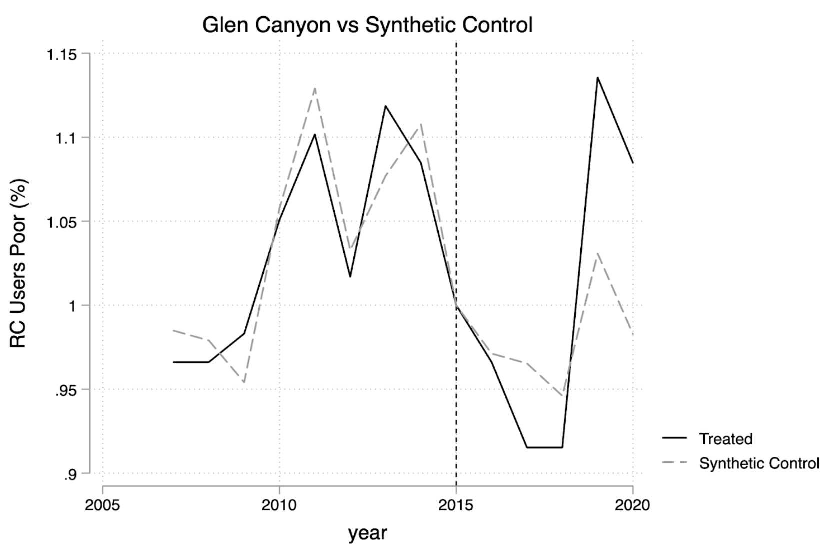


Figure 2: Glen Canyon RC vs. Synthetic Glen Canyon (scaled)

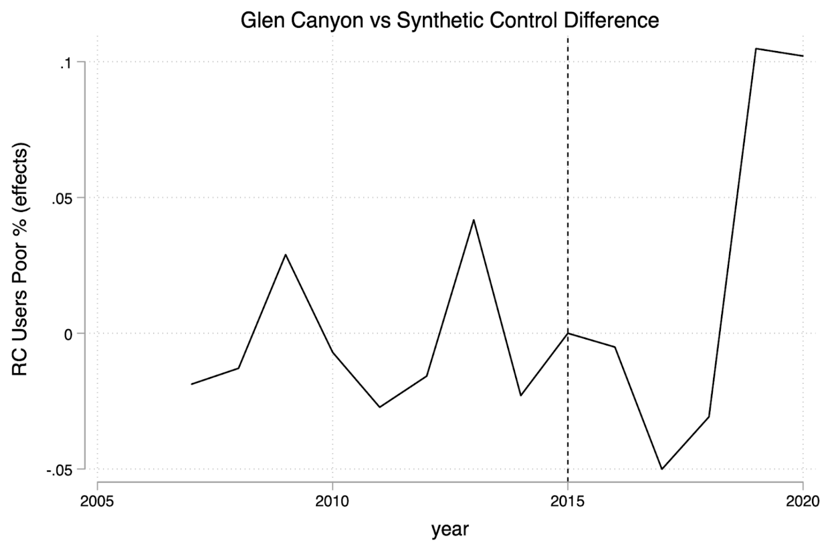


Figure 2.1: Outcome Gap between Glen Canyon RC vs. Synthetic Glen Canyon (scaled)

III. Palega RC (Portola)

Figure 3 plots the proportion of users that is poor for Palega RC and its synthetic control, with the renovation year in 2011 and a total of 4 pretreatment years. The first 2 of the 4 pretreatment periods are used as the training period, while the remaining periods are the validation period. Given the smaller range of pretreatment periods, achieving a good fit between Palega RC and its synthetic control is more difficult- with a RMSPE of .0176. We see that Palega RC has a constant positive trend leading up to the renovation, while synthetic Palega starts off with a negative trajectory, starts to increase in 2009, and then drops in the last pretreatment period. For the postreament period, the proportion of poor users for Palega RC remains lower than synthetic Palega until after 2018. From 2011 to 2012, both trajectories increase significantly then decrease in 2014 until about 2018, where synthetic Palega keeps its ratio of poor RC users higher than Palega RC. From 2018 onward Palega RC's outcome variable is higher than the synthetic control- though they follow similar trajectories.

Figure 3.1 graphs the differences between Palega RC and synthetic Palega, where the post-treatment period is taken as the effect. Until 2018, Palegas RC's effect from the renovation is smaller than the synthetic control. From 2018 to the end of the post-treatment period, the effect is larger on Palega RC. By 2020, the effect of the renovation is about 8% greater on Palega RC than for its synthetic control. This suggests that positive effects on Palega RC's proportion of poor users from the renovation happen over time.

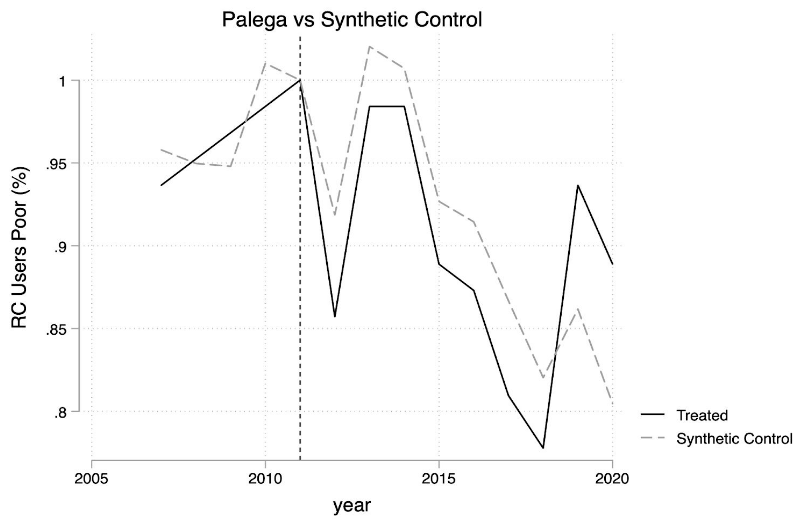


Figure 3: Palega RC vs. Synthetic Palega (scaled)

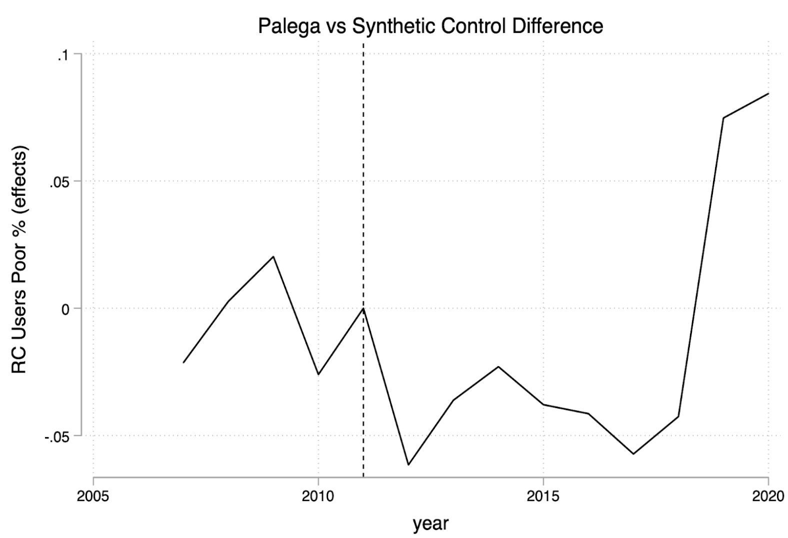


Figure 3.1: Outcome Gap between Palega RC vs. Synthetic Palega (scaled)

IV. Sunset RC (Sunset/Parkside)

Figure 4 plots the proportion of users that is poor for Sunset RC and its synthetic control, with the renovation year in 2010 and a total of 3 pretreatment years. The first 2 of the 3 pretreatment periods are used as the training period, while the remaining period is the validation period. Given the even smaller range of pretreatment periods, achieving a good fit between Sunset RC and its synthetic control is difficult. We observe that the difference between them here is much larger than for the above treated RCs- with a RMSPE of .0611 (see Appendix). For the first pretreatment year, Sunset RC has a positive trajectory in the outcome variable, then proceeds to have a downward slope from 2008 to 2009 before remaining constant for the last pretreatment period. Synthetic Sunset, however, starts off with a negative trajectory up until 2009, and switches to a positive trajectory in the last pretreatment period meeting the same level with Sunset RC. In the first year of the postreatement period, we observe a very similar trajectory for and synthetic Sunset. After 2011, synthetic Sunset's proportion of poor users drops more dramatically than does the proportion for Sunset RC. For the remainder of the post-treatment period, Sunset RC's outcome is higher than synthetic Sunset, though they follow similar trajectories.

Figure 4.1 graphs the differences between Sunset RC and synthetic Sunset, where the post-treatment period is taken as the effect. For the entire pretreatment period, the effect of the renovation on Sunset RC's outcome is larger relative to the effect on the synthetic control, experiencing a peak of 0.1. In the year immediately following the renovation, Sunset RC's effect from the renovation is slightly smaller than for synthetic control. For the remainder of the post-treatment period, however, the effect remains larger on Sunset RC though it does not follow a clear trajectory. By 2020, the effect of the renovation is about 13 % greater on Sunset RC than for its synthetic control.

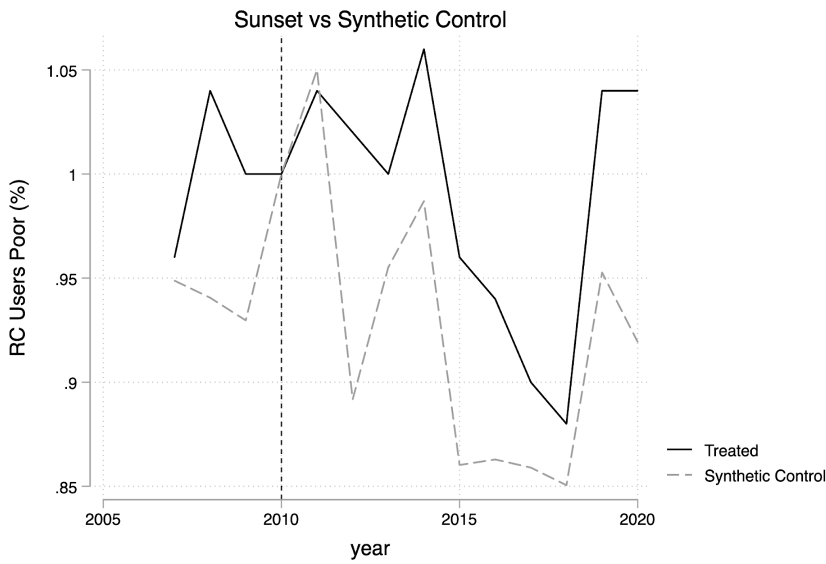


Figure 4: Sunset RC vs. Synthetic Sunset (scaled)

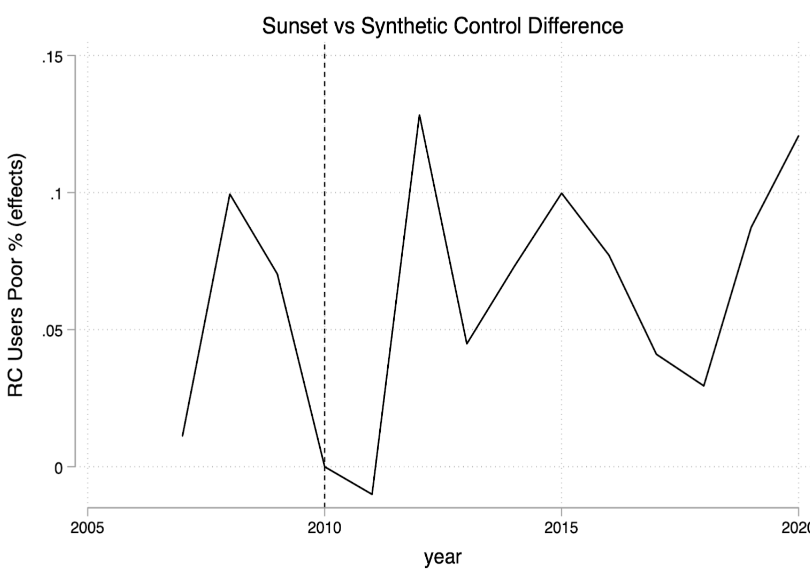


Figure 4.1: Outcome Gap between Sunset RC vs. Synthetic Sunset (scaled)

V. Betty Ann Ong RC (Nob Hill)

Figure 5 plots the proportion of users that is poor for Betty Ann Ong RC and its synthetic control, with the renovation year in 2010 and a total of 3 pretreatment years. The first 2 of the 3 pretreatment periods are used as the training period, while the remaining period is the validation period. Given the even smaller range of pretreatment periods, achieving a good fit between Betty Ann Ong RC and its synthetic control is difficult- with a RMSPE of .0561 (see Appendix). For the first pretreatment year, both Betty Ann Ong RC and synthetic Betty Ann Ong have a negative trajectory in the outcome variable, though Betty Ann Ong RC has a greater negative slope. In 2008, Betty Ann Ong RC's trajectory switches to positive until 2009, then remains constant for the year immediately preceding renovation. For its synthetic control, the downward trajectory in the pretreatment period shifts to positive shortly after in 2009, which continues up to the renovation year. In the postreatement period, Betty Ann Ong RC's proportion of RC users that is poor is higher than synthetic Betty Ann Ong two years after renovation. Betty Ann Ong RC's trajectory remains higher than the synthetic control for the remainder of the post-treatment period.

Figure 5.1 graphs the differences between Betty Ann Ong RC and synthetic Betty Ann Ong, where the post-treatment period is taken as the effect. The effect of the renovation on the outcome variable for Betty Ann Ong RC is negative in the year after the renovation relative to the synthetic control. After 2011, however, the effect on Betty Ann Ong RC's proportion of poor users is greater than for its synthetic control. This positive effect on the outcome variable for Betty Ann Ong RC increases over time. By 2020, the effect of the renovation on Betty Ann Ong RC is 29% greater than the effect on its synthetic control.

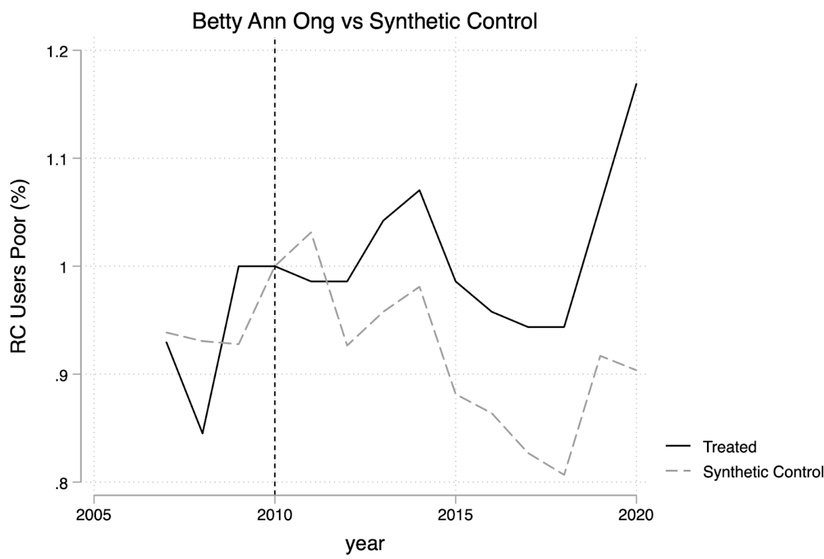


Figure 5: Betty Ann Ong RC vs. Synthetic Betty Ann Ong (scaled)

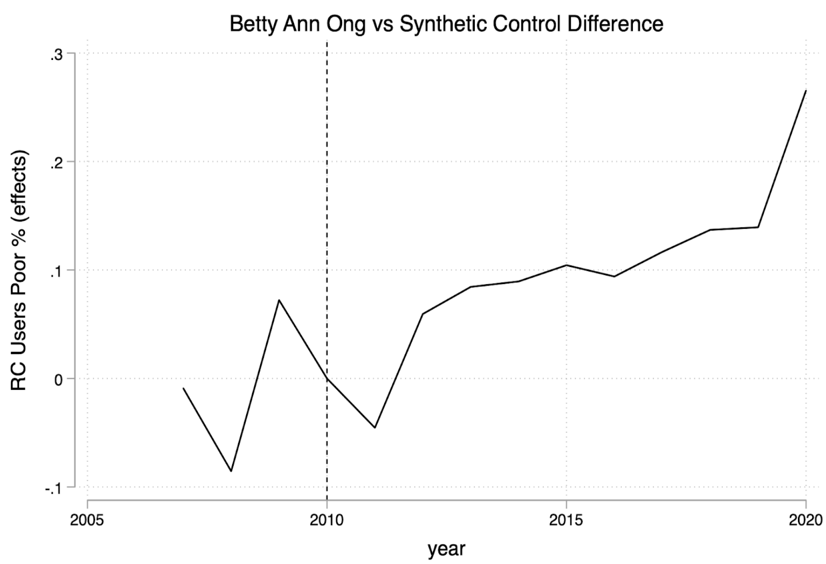


Figure 5.1: Outcome Gap between Betty Ann Ong RC vs. Synthetic Betty Ann Ong (scaled)

VI. Hamilton RC (Japantown)

Figure 6 plots the proportion of users that is poor for Hamilton RC and its synthetic control, with the renovation year in 2008 and a total of 1 pretreatment year. We see that the trajectories for Hamilton RC and its synthetic control are close in the single pretreatment period, with a pretreatment RSME of .0057 (see Appendix). However, synthetic Hamilton in this period has a slightly higher proportion of poor RC users than Randall RC. In the post-treatment period, the outcome trajectories for both Hamilton RC and the synthetic control fluctuate. Between 2010 -2012, 2017-2019, and after 2019, synthetic Hamilton's proportion of poor RC users is higher than Hamilton RC. Both follow relatively similar trajectories albeit have different peaks and troughs in the outcome variable. After 2014, synthetic Hamilton's outcome drops significantly more than Hamilton RC's level, though it is immediately followed by an upward trend until about 2019. While after 2019 both trajectories follow a downtown trend, Hamilton RC's proportion of poor users has a more negative slope. In the last year of the post-treatment period, synthetic Hamilton has a slightly higher proportion of poor users than Hamilton RC- a difference of about 2 %. This suggests that over the long term, the RCs that did not not receive a renovation have a higher proportion of poor users relative to renovated Hamilton RC.

Figure 6.1 graphs the differences between Hamilton RC and synthetic Hamilton, where the post-treatment period is taken as the effect. In the post-treatment period, Hamilton RC's effect from the renovation is greater than its synthetic control in the periods 2008-2009, between 2011 and 2016, and briefly in 2019. After 2019, however, the effect of the renovation on the proportion of poor RC users for Hamilton RC is less than for its synthetic- a difference of about 2%. While the effect from the renovation on Hamilton RC's outcome is positive in the immediate years following renovation and throughout the middle of the post-treatment period, we observe that this effect on Hamilton RC becomes less than its synthetic towards the end of the post-treatment. In other words, Hamilton RC's renovation has less of a positive effect on it's proportion of poor users than it does for its synthetic.

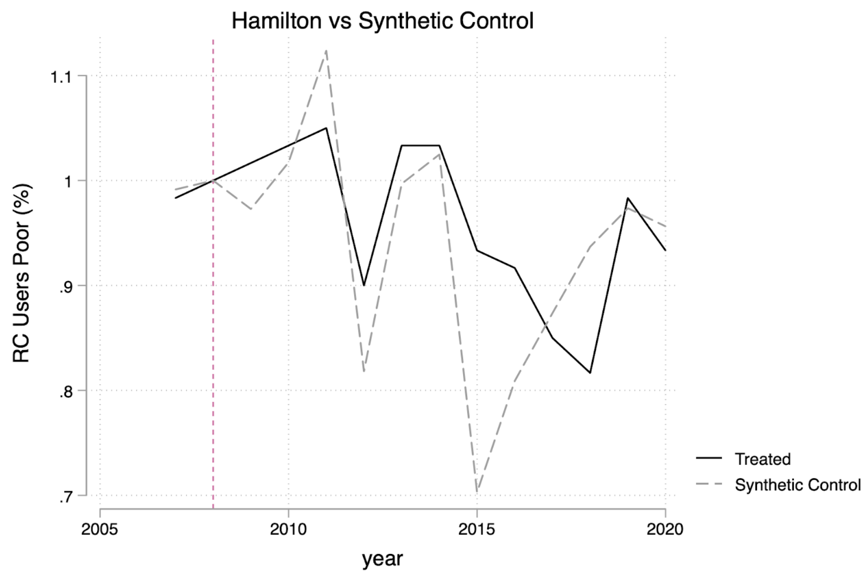


Figure 6: Hamilton RC vs. Synthetic Hamilton (scaled)

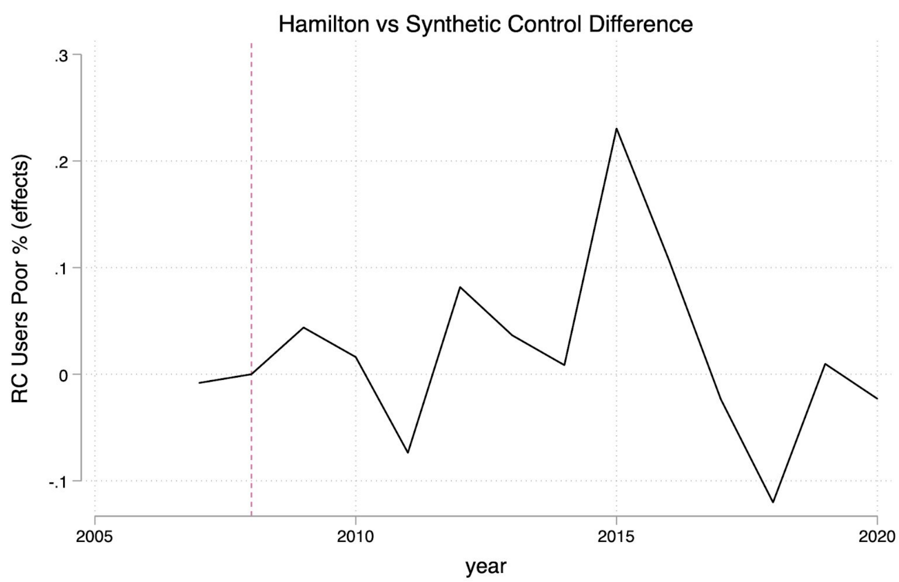


Figure 6.1: Outcome Gap between Hamilton RC vs. Hamilton (scaled)

I. Results: Difference in Difference Method

I ran each of the 6 treated RCs through a difference-in-differences (DID) model for the sake of comparing results to that of the synthetic control method (SCM). I include time-varying covariates and RC specific time trends to relax the parallel trends assumption characteristic of the traditional ordinary least squares DID estimation. I assume homogeneity within RCs. The covariates included in this model are from the same list of predictors used for the SCM. However, several of these predictors were omitted in the DID model due to collinearity. For each of the treated RCs, below are the DID regression results of the estimated effect of the renovation on the proportion of RC users that is poor. The first table includes a model that regresses the outcome variable on the treated RC, on the treatment period, and their interaction, as well as a model that includes covariates that were not dropped because of collinearity. The second table shows the effects of renovation on a treated RC over time with RC and year interactions. Because adding covariates to the model with RC and year interactions causes the majority of the terms to be omitted due to collinearity, I do not include them in the model.

I. Randall RC (Castro/Upper Market)

In the DID model without covariates, we observe a positive effect from the renovation on Randall RC's proportion of poor users. As shown in Table A, the model estimates that the renovation increases the proportion of poor users by about 6.3 % . This effect, however, is not statistically significant. When we control for a set of covariates that are relevant for comparing across RCs, the effect from the renovation on the proportion of poor users increases to 10.5%. Once again, this effect is not statistically significant.

When looking at the effects from the renovation across time by interacting Randall RC with each year from the post-treatment period in Table A2, we observe positive effects for all periods except period 1. The last period of the post-treatment was omitted because of collinearity. The first period post-treatment has a statistically significant effect of .0663 at the 1% significance level. Similarly we see a statistically significant estimated effect for period 3 of .15. Over time, the effect is much greater towards the end of the post-treatment (.15) than it is for the beginning (.066). This highlights the important role time plays in measuring the effect on Randall RC from the renovation.

VARIABLES	(1) % Poor RC Users	(2) % Poor RC Users
Randall	-0.110***	-0.501
	(0.00975)	(0.581)
Treated	-0.0625	0.00466
	(0.0385)	(0.0566)
Randall x Treated	0.0630	0.105
	(0.0385)	(0.0889)
Equity Zone		-0.624
		(0.585)
Distance to Bart		-0.000283
		(0.000201)
Distance to School		-7.19e-05
		(4.70e-05)
Total Population		-1.64e-05
		(1.26e-05)
Age < 20		-2.271
		(3.814)
20 > Age > 44		-0.633
		(1.180)
White		3.52e-06
		(8.05e-06)
Black		1.45e-05
		(0.000128)
Native		4.03e-05
		(0.000277)
Asian		-2.14e-05
		(2.98e-05)
Median Gross Rent		0.000142
		(0.000267)
Median HH Income		-4.63e-06
		(4.17e-06)
Public Transit		1.025
		(1.410)
Commute > 1 hour		-0.787
		(1.307)
Property Value		2.29e-10
		(1.53e-09)
Businesses Opened		0.000260
		(0.000440)
Affordable Housing		-1.14e-05
		(0.000703)
Housing Units		-5.56e-05
		(6.67e-05)
Crime		5.97e-06
		(4.29e-05)
Evictions		-0.000397
		(0.000390)
Constant	0.677***	2.332**
	(0.00975)	(1.071)
Observations	98	49
Neighborhoods	7	7

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A: % Poor RC Users regressed on treated RC, year, & interaction. The second column includes a model with covariates.

VARIABLES	(1) % Poor RC Users
Randall	-0.110***
	(0.0101)
Period 0	-0.0733***
	(0.0140)
Period 1	-0.0800**
	(0.0342)
Period 2	-0.0400
	(0.0517)
Period 3	-0.0567
	(0.0666)
Period 4	-
Period 0 x Randall	0.0663***
	(0.0140)
Period 1.x Randall	-0.00700
	(0.0342)
Period 2 x Randall	0.0430
	(0.0517)
Period 3 x Randall	0.150**
	(0.0666)
Period 4 x Randall	0
	(0)
Constant	0.677***
	(0.0101)
Observations	98
Neighborhoods	7

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table A2: Effect of Treatment on % Poor RC Users across time, with RC and year interactions.

II. Glen Canyon RC (Glen Park)

In the DID model without covariates, we observe a positive effect from the renovation on Glendale's proportion of poor users. As shown in Table B, the model estimates that the renovation increases the proportion of poor users by about 5.13 % . This effect is statistically significant at the 10% level. When we control for a set of covariates that are relevant for comparing across RCs, the effect from the renovation on the proportion of poor users increases to 19.3%. This effect, however, is not statistically significant.

When looking at the effects from the renovation across time by interacting Glen Canyon RC with each year from the post-treatment period in Table B2, we observe positive effects. The last period of the post-treatment was omitted because of collinearity. Only in period 3 do we see a statistically significant effect of .108 from the renovation on Glen Canyon RC's proportion of poor users. From period 2 to period 3, there is a dramatic increase in the estimated effect of the renovation on the outcome, from .0176 to .108. We observe that over time, Glen Canyon RC's effect fluctuates though at the end of the post-treatment period, the effect is much larger (.0946) than the beginning (.0259).

VARIABLES	(1) % Poor RC Users	(2) % Poor RC Users
Glen Canyon	-0.0743***	-0.157
	(0.00961)	(0.343)
Treated	-0.0681**	-0.135*
	(0.0283)	(0.0774)
Glen Canyon x Treated	0.0513*	0.193
	(0.0283)	(0.151)
Equity Zone		0.148
		(0.217)
Distance to Bart		9.61e-05
		(0.000185)
Distance to School		1.51e-05
		(4.26e-05)
Total Population		1.18e-05
		(1.07e-05)
Age < 20		-2.422
		(2.418)
20 > Age > 44		-0.0911
		(0.973)
White		-2.76e-06
		(6.54e-06)
Black		2.63e-05
		(8.27e-05)
Native		7.82e-05
		(0.000211)
Asian		-7.78e-05***
		(2.59e-05)
Median Gross Rent		0.000460
		(0.000296)
Median HH Income		-2.39e-06
		(2.35e-06)
Public Transit		0.834
		(0.833)
Commute > 1 hour		-1.842*
		(1.118)
Property Value		3.62e-10
		(9.93e-10)
Businesses Opened		0.000405
		(0.000277)
Affordable Units		-0.000138
		(0.000656)
Housing Units		1.82e-05
		(9.33e-05)
Crime		-9.39e-05
		(7.68e-05)
Evictions		-0.000545**
		(0.000257)
Constant	0.683***	0.458
	(0.00961)	(1.164)
Observations	98	49
Neighborhoods	7	7

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B: % Poor RC Users regressed on treated RC, year, & interaction. The second column includes a model with covariates.

VARIABLES	(1) % Poor RC Users
Glen Canyon	-0.0743***
	(0.0101)
Period 0	-0.0648***
	(0.0160)
Period 1	-0.0798***
	(0.0134)
Period 2	-0.0865***
	(0.0333)
Period 3	-0.0465
	(0.0514)
Period 4	-0.0631
	(0.0665)
Period 5	-
Period 0 x Glen Canyon	0.0259
	(0.0160)
Period 1 x Glen Canyon	0.0109
	(0.0134)
Period 2 x Glen Canyon	0.0176
	(0.0333)
Period 3 x Glen Canyon	0.108**
	(0.0514)
Period 4 x Glen Canyon	0.0943
	(0.0665)
Period 5 x Glen Canyon	0
	(0)
Constant	0.683***
	(0.0101)
Observations	98
Neighborhoods	7

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table B2: Effect of Treatment on % Poor RC Users across time, with RC and year interactions.

III. Palega RC (Portola)

In the DID model without covariates, we observe a positive effect from the renovation on Pelaga's proportion of poor users. As shown in Table C, the model estimates that the renovation increases the proportion of poor users by about 1.3 % . This effect, however, is not statistically significant. When we control for a set of covariates that are relevant for comparing across RCs, the effect from the renovation on the proportion of poor users increases to 3.81%. Once again, this effect is not statistically significant.

When looking at the effects from the renovation across time by interacting Palega RC with each year from the post-treatment period in Table C2, we observe positive effects except for period 0, period 5 and period 6. The last period of the post-treatment was omitted because of collinearity. We see no statistically significant effects. We can see that there are fluctuations in the estimated effects over time. At the end of the post-treatment period, Palega RC's effect on the outcome is much larger (.0293) than the beginning (-.000667).

VARIABLES	(1) % Poor RC Users	(2) % Poor RC Users
Palega	-0.0893***	0.0206
	(0.0130)	(0.564)
Treated	-0.0630***	-0.0967**
	(0.0190)	(0.0392)
Palega x Treated	0.0130	0.0381
	(0.0190)	(0.0880)
Equity Zone		-0.367
		(0.342)
Distance to Bart		0.000104
		(0.000148)
Distance to School		1.76e-05
		(3.39e-05)
Total Population		-5.29e-06
		(4.83e-06)
Age < 20		-2.735
		(3.791)
20 > Age > 44		-1.324
		(1.634)
White		2.78e-06
		(1.13e-05)
Black		-5.25e-05
		(7.69e-05)
Native		-0.000203
		(0.000205)
Asian		-5.07e-06
		(2.57e-05)
Median Gross Rent		-2.85e-05
		(0.000300)
Median HH Income		-3.91e-06
		(2.79e-06)
Public Transit		0.615
		(0.777)
Commute > 60		0.0339
		(1.573)
Property Value		-1.89e-10
		(8.77e-10)
Businesses Opened		0.000391
		(0.000323)
Affordable Housing		9.99e-05
		(0.000696)
Housing Units		-6.61e-05
		(8.42e-05)
Crime		1.59e-05
		(4.98e-05)
Evictions		-6.97e-05
		(0.000289)
Constant	0.699***	1.885
	(0.0130)	(1.440)
Observations	98	49
Neighborhoods	7	7

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table C: % Poor RC Users regressed on treated RC, year, & interaction. The second column includes a model with covariates.

VARIABLES	(1) % Poor RC Users
Palega	-0.0893***
	(0.0143)
Period 0	-0.0693**
	(0.0353)
Period 1	0.00733
	(0.0274)
Period 2	0.00900
	(0.0164)
Period 3	-0.0927*
	(0.0501)
Period 4	-0.0810***
	(0.0277)
Period 5	-0.0960***
	(0.0164)
Period 6	-0.103***
	(0.0304)
Period 7	-0.0627
	(0.0509)
Period 8	-0.0793
	(0.0668)
Period 9	-
Period 0 x Palega	-0.000667
	(0.0353)
Period 1 x Palega	0.00267
	(0.0274)
Period 2 x Palega	0.001000
	(0.0164)
Period 3 x Palega	0.0427
	(0.0501)
Period 4 x Palega	0.0210
	(0.0277)
Period 5 x Palega	-0.00400
	(0.0164)
Period 6 x Palega	-0.0173
	(0.0304)
Period 7 x Palega	0.0427
	(0.0509)
Period 8 x Palega	0.0293
	(0.0668)
Period 9 x Palega	0
	(0)
Constant	0.699***
	(0.0143)
Observations	98
Neighborhoods	7

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table C2: Effect of Treatment on % Poor RC Users across time, with RC and year interactions.

IV. **Sunset RC (Sunset/Parkside)**

In the DID model without covariates, we observe a positive effect from the renovation on Sunset RC's proportion of poor users. As shown in Table D, the model estimates that the renovation increases the proportion of poor users by about 3.88 % . This effect is statistically significant at the 10% significance level. When we control for a set of covariates that are relevant for comparing across RCs, the effect from the renovation on the proportion of poor users drops to 1.38%. This effect, however, is not statistically significant.

When looking at the effects from the renovation across time by interacting Sunset RC with each year from the post-treatment period in Table D2, we observe positive effects except for period 0 and period 2. The last period of the post-treatment was omitted because of collinearity. We see a statistically significant effect on the outcome for periods 1, 5, and 6 at the 1% significance level. We can see that there are fluctuations in the estimated effects over time. At the end of the post-treatment period, Sunset RC's effect on the outcome is much larger (.0908) than the beginning (-.0225).

VARIABLES	(1) % Poor RC Users	(2) % Poor RC Users
Sunset	-0.191***	1.630
	(0.0146)	(1.718)
Treated	-0.0448*	-0.0596
	(0.0229)	(0.0415)
Sunset x Treated	0.0388*	0.0138
	(0.0229)	(0.0525)
Equity Zone		-0.578
		(0.559)
Distance to Bart		-0.000211
		(0.000262)
Distance to School		-5.46e-05
		(6.03e-05)
Total Population		-1.15e-05*
		(6.81e-06)
Age < 20		-3.146
		(4.647)
20 > Age > 44		-0.789
		(1.124)
White		8.12e-07
		(8.68e-06)
Black		1.96e-05
		(8.61e-05)
Native		-3.43e-05
		(0.000234)
Asian		-1.06e-05
		(1.40e-05)
Median Gross Rent		0.000335
		(0.000397)
Median HH Income		-5.67e-06
		(4.32e-06)
Public Transit		0.676
		(1.103)
Commute > 1 hour		-2.062
		(2.240)
Property Value		1.93e-10
		(1.24e-09)
Businesses Opened		0.000353
		(0.000359)
Affordable Housing		-0.000147
		(0.000547)
Housing Units		-6.19e-05
		(6.11e-05)
Crime		-6.66e-06
		(4.63e-05)
Evictions		-0.000478
		(0.000478)
Constant	0.691***	2.323
	(0.0146)	(1.476)
Observations	98	49
Neighborhoods	7	7

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D: % Poor RC Users regressed on treated RC, year, & interaction. The second column includes a model with covariates.

VARIABLES	(1) % Poor RC Users
Sunset	-0.191***
	(0.0162)
Period 0	0.0425
	(0.0340)
Period 1	-0.0608*
	(0.0363)
Period 2	0.0158
	(0.0247)
Period 3	0.0175
	(0.0192)
Period 4	-0.0842*
	(0.0488)
Period 5	-0.0725***
	(0.0254)
Period 6	-0.0875***
	(0.0210)
Period 7	-0.0942**
	(0.0373)
Period 8	-0.0542
	(0.0579)
Period 9	-0.0708
	(0.0739)
Period 10	-
Period 0 x Sunset	-0.0225
	(0.0340)
Period 1 x Sunset	0.0708*
	(0.0363)
Period 2 x Sunset	-0.0158
	(0.0247)
Period 3 x Sunset	0.0125
	(0.0192)
Period 4 x Sunset	0.0642
	(0.0488)
Period 5 x Sunset	0.0425*
	(0.0254)
Period 6 x Sunset	0.0375*
	(0.0210)
Period 7 x Sunset	0.0342
	(0.0373)
Period 8 x Sunset	0.0742
	(0.0579)
Period 9 x Sunset	0.0908
	(0.0739)
Period 10 x Sunset	0
	(0)
Constant	0.691***
	(0.0162)
Observations	98
Neighborhoods	7

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table D2: Effect of Treatment on % Poor RC Users across time, with RC and year interactions.

V. Betty Ann Ong RC (Nob Hill)

In the DID model without covariates, we observe a positive effect from the renovation on Betty Ann Ong RC's proportion of poor users. As shown in Table E, the model estimates that the renovation increases the proportion of poor users by about 9.5 % . This effect is statistically significant at the 1% significance level. When we control for a set of covariates that are relevant for comparing across RCs, the effect from the renovation on the proportion of poor users increases to 13.1%. This effect, however, is not statistically significant.

When looking at the effects from the renovation across time by interacting Betty Ann Ong RC with each year from the post-treatment period in Table E2, we observe positive effects except for the first period. The last period of the post-treatment was omitted because of collinearity. We see statistically significant effects for all periods except the first period at 1% and 5% significance levels. We can see that there are fluctuations in the estimated effects over time, and from period 8 to 9 we see an almost doubling in the size of the effect. At the end of the post-treatment period, Betty Ann Ong RC's effect on the outcome is much larger (.231) than the beginning (-.0125).

VARIABLES	(1) % Poor RC Users	(2) % Poor RC Users
Betty Ann Ong	-0.0208	0.184
	(0.0146)	(0.214)
Treated	-0.0448*	-0.0430
	(0.0229)	(0.0461)
Betty Ann Ong x Treated	0.0948***	0.131
	(0.0229)	(0.0804)
Equity Zone		-0.498
		(0.521)
Distance to Bart		-0.000252
		(0.000204)
Distance to School		-6.39e-05
		(4.61e-05)
Total Population		-1.10e-05
		(9.11e-06)
Age < 20		-3.903
		(4.024)
20 > Age > 44		-1.053
		(1.358)
White		6.74e-06
		(7.79e-06)
Black		-2.98e-07
		(9.70e-05)
Native		-7.96e-05
		(0.000233)
Asian		-3.71e-05*
		(2.08e-05)
Median Gross Rent		0.000488
		(0.000392)
Median HH Income		-6.88e-06
		(4.42e-06)
Public Transit		0.702
		(1.015)
Commute > 1 hour		-2.350
		(1.512)
Property Value		8.05e-10
		(1.16e-09)
Businesses Opened		0.000264
		(0.000300)
Affordable Housing		-9.72e-05
		(0.000436)
Housing Units		-6.12e-05
		(5.30e-05)
Crime		-1.27e-05
		(3.81e-05)
Evictions		-0.000585
		(0.000424)
Constant	0.691***	2.597**
	(0.0146)	(1.302)
Observations	98	49
Neighborhoods	7	7

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table E: % Poor RC Users regressed on treated RC, year, & interaction. The second column includes a model with covariates.

VARIABLES	(1) % Poor RC Users
Betty Ann Ong	-0.0208
	(0.0162)
Period 0	0.0425
	(0.0340)
Period 1	-0.0608*
	(0.0363)
Period 2	0.0158
	(0.0247)
Period 3	0.0175
	(0.0192)
Period 4	-0.0842*
	(0.0488)
Period 5	-0.0725***
	(0.0254)
Period 6	-0.0875***
	(0.0210)
Period 7	-0.0942**
	(0.0373)
Period 8	-0.0542
	(0.0579)
Period 9	-0.0708
	(0.0739)
Period 10	-
Period 0 x Betty Ann Ong	-0.0125
	(0.0340)
Period 1 x Betty Ann Ong	0.0908**
	(0.0363)
Period 2 x Betty Ann Ong	0.0542**
	(0.0247)
Period 3 x Betty Ann Ong	0.0725***
	(0.0192)
Period 4 x Betty Ann Ong	0.114**
	(0.0488)
Period 5 x Betty Ann Ong	0.0825***
	(0.0254)
Period 6 x Betty Ann Ong	0.0875***
	(0.0210)
Period 7 x Betty Ann Ong	0.0942**
	(0.0373)
Period 8 x Betty Ann Ong	0.134**
	(0.0579)
Period 9 x Betty Ann Ong	0.231***
	(0.0739)
Period 10 x Betty Ann Ong	0
	(0)
Constant	0.691***
	(0.0162)
Observations	98
Neighborhoods	7

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table E2: Effect of Treatment on % Poor RC Users across time, with RC and year interactions.

VI. Hamilton RC (Japantown)

In the DID model without covariates, we observe a positive effect from the renovation on Hamilton RC's proportion of poor users. As shown in Table F, the model estimates that the renovation increases the proportion of poor users by about 0.8 % . This effect, however, is not statistically significant. When we control for a set of covariates that are relevant for comparing across RCs, the effect from the renovation on the proportion of poor users becomes negative, -1.04 %. Once again, this effect is not statistically significant.

When looking at the effects from the renovation across time by interacting Hamilton RC with each year from the post-treatment period in Table F2, we observe an even number of positive and negative effects. The last period of the post-treatment was omitted because of collinearity. We only see a statistically significant effect from the renovation on the outcome for the first period, with an effect of .02 at 1 % significance level. We can see that there are fluctuations in the estimated effects over time. At the end of the post-treatment period, Hamilton RC's effect on the outcome is slightly larger (.0283) than the beginning (.02).

VARIABLES	(1) % Poor RC Users	(2) % Poor RC Users
Hamilton	-0.0883***	0.184
	(0.0157)	(0.351)
Treated	-0.0286	0.0240
	(0.0198)	(0.0534)
Hamilton x Treated	0.00861	-0.0104
	(0.0198)	(0.0323)
Equity Zone		-0.190
		(0.313)
Distance to Bart		-0.000186
		(0.000170)
Distance to School		-5.15e-05
		(3.90e-05)
Total Population		-1.38e-05*
		(7.80e-06)
Age < 20		0.441
		(0.439)
20 > Age > 44		0.171
		(0.409)
White		9.10e-06*
		(5.36e-06)
Black		6.84e-05
		(5.67e-05)
Native		0.000139
		(0.000174)
Asian		1.11e-05
		(2.69e-05)
Property Value		0
		(5.03e-10)
Businesses Opened		-2.39e-05
		(8.98e-05)
Affordable Housing		-5.78e-05
		(0.000485)
Housing Units		-3.86e-05***
		(1.05e-05)
Crime		-4.89e-06
		(1.52e-05)
Constant	0.683***	1.028**
	(0.0157)	(0.400)
Observations	98	53
Neighborhoods	7	7

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table F: % Poor RC Users regressed on treated RC, year, & interaction. The second column includes a model with covariates.

VARIABLES	(1) % Poor RC Users
Hamilton	-0.0883***
	(0.0180)
Period 0	-0.00500
	(0.00693)
Period 1	0.0350***
	(0.00980)
Period 2	0.0500
	(0.0338)
Period 3	-0.0533
	(0.0394)
Period 4	0.0233
	(0.0260)
Period 5	0.0250
	(0.0198)
Period 6	-0.0767
	(0.0528)
Period 7	-0.0650**
	(0.0285)
Period 8	-0.0800***
	(0.0217)
Period 9	-0.0867**
	(0.0369)
Period 10	-0.0467
	(0.0591)
Period 11	-0.0633
	(0.0755)
Period 12	-
Period 0 x Hamilton	0.0200***
	(0.00693)
Period 1 x Hamilton	-0.0100
	(0.00980)
Period 2 x Hamilton	-0.0150
	(0.0338)
Period 3 x Hamilton	-0.00167
	(0.0394)
Period 4 x Hamilton	0.00167
	(0.0260)
Period 5 x Hamilton	-9.93e-09
	(0.0198)
Period 6 x Hamilton	0.0417
	(0.0528)
Period 7 x Hamilton	0.0200
	(0.0285)
Period 8 x Hamilton	-0.00500
	(0.0217)
Period 9 x Hamilton	-0.0183
	(0.0369)
Period 10 x Hamilton	0.0417
	(0.0591)
Period 11 x Hamilton	0.0283
	(0.0755)
Period 12 x Hamilton	0
	(0)
Constant	0.683***
	(0.0180)
Observations	98
Neighborhoods	7

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table F2: Effect of Treatment on % Poor RC Users across time, with RC and year interactions.

Inference and Placebo Tests

Below are the placebo estimates for each of the 6 treated RCs for the same treatment period but on all the 6 control units that form their synthetic controls. The first set of figures graph the outcome gaps for each treated RC (black line) and all of the individuals control RCs that make up the synthetic control- as if they had received treatment (white lines). The vertical red dotted line represents the renovation year.

P-values are provided comparing the estimated main effect on the treated RC to the distribution of placebo effects to determine the degree to which effects estimated are due to chance. The treatment effects are estimated by matching on trends in the outcome variable. For Tables 1-6, the first column represents the per-period effects for the post-treatment period regarding the outcome for each RC minus the outcome of its synthetic control. The second column provides the proportion of placebo effects per period that is at least as large as the main effect for each post-treatment period. The last column is the proportion of placebo standardized effects that are at least as large as the main standardized effect for each post-treatment period. The second set of figures are a graphical representation of the standardized p-values. The x-axis indicates the number of years after the renovation year, while the y-axis indicates the probability that the effects from the renovation estimated are due to chance (standardized p-values).

I. Randall RC (Castro/Upper Market)

As shown in Figure 1.2 below, the trajectories of Randall RC and the controls (placebos) in the pretreatment period are similar with the exception of Tenderloin RC. In the post-treatment period, the trajectories of Randall RC, Tenderloin RC, and South of Market RC diverge from the rest of the group that keeps a similar trajectory to the pretreatment period. The proportion of placebos that have a post-treatment RMSPE at least as large as the average for Randall RC is .167. The proportion of placebos that have a ratio of post-treatment RMSPE over pretreatment RMSPE at least as large as the average ratio for Randall RC is 0. The proportion of placebos that have a pretreatment RMSPE at least as large as the average of Randall RC is .834. This proportion is large and thus concerning as a measure of fit. Given we specified a training period, the proportion of placebos that have a RMSPE for the validation period at least as large as the average of Randall RC- also a measure of fit- is .66.

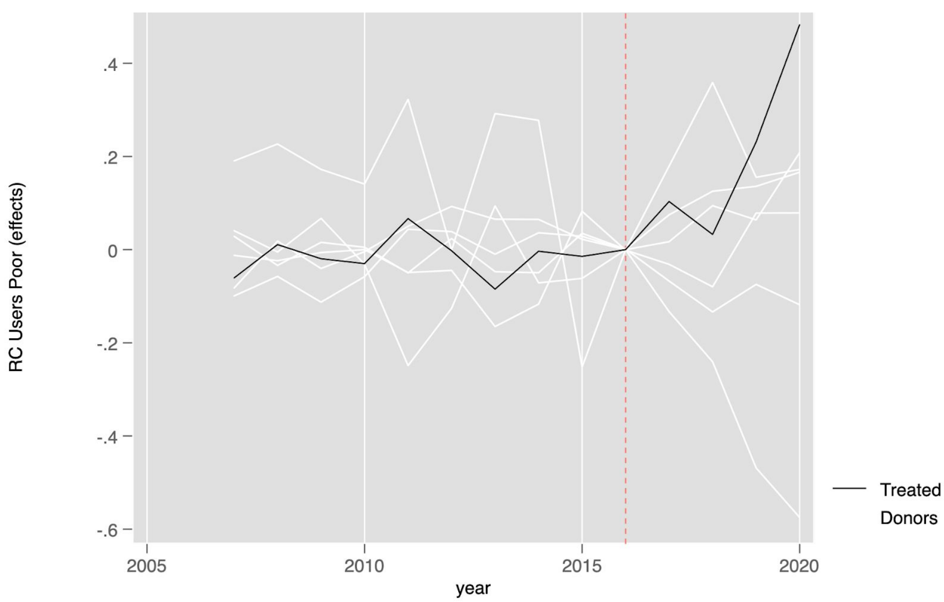


Figure 1.2: Outcome Gaps between Randall RC vs Donor Pool (scaled)

The standardized p-values for Randall RC as indicated by the table and graph below is 0 for all the years following renovation with the exception of 2018. This means that on average the probability that the treatment effect is due to chance is close to 0. This confirms that the estimated positive renovation effect on the proportion of RC users that is poor is statistically significant for Randall RC.

	estimates	pvals	pvals_std
c1	.1033289	.3333333	0
c2	.0326745	1	1
c3	.2310158	.1666667	0
c4	.4828175	.1666667	0

Table 1: Post-treatment Results: Effects, P-values, and Standardized P-values (scaled)

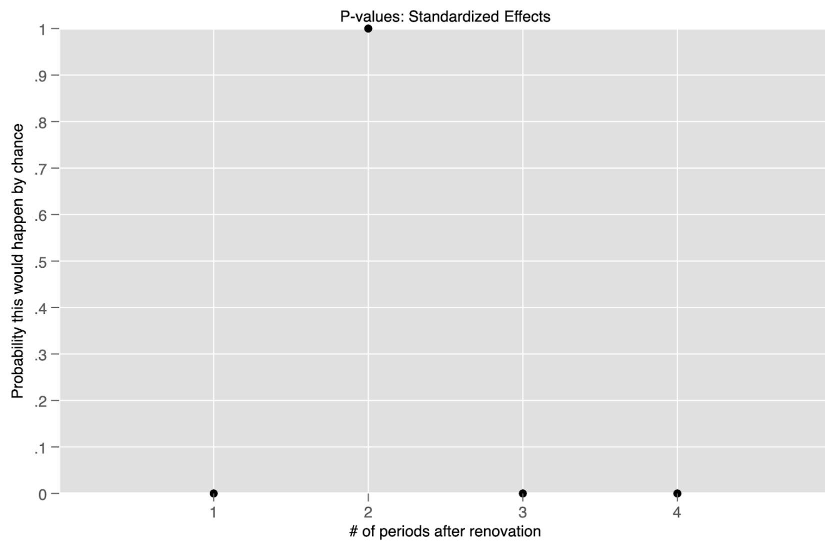


Figure 1.3: Standardized P-values for Randall RC

II. Glen Canyon RC (Glen Park)

As shown in Figure 2.2 below, the trajectories of Glen Canyon RC and the controls (placebos) in the pretreatment period are similar with the exception of Tenderloin RC. In the post-treatment period, the trajectories of Tenderloin RC and South of Market RC differ significantly from the rest of the group. The proportion of placebos that have a post-treatment RMSPE at least as large as the average for Glen Canyon RC is .66. The proportion of placebos that have a ratio of post-treatment RMSPE over pretreatment RMSPE at least as large as the average ratio for Glen Canyon RC is .167. The proportion of placebos that have a pretreatment RMSPE at least as large as the average of Glen Canyon RC is 1. This proportion is large and thus concerning as a measure of fit. Given we specified a training period, the proportion of placebos that have a RMSPE for the validation period at least as large as the average of Glen Canyon RC- also a measure of fit- is .834.

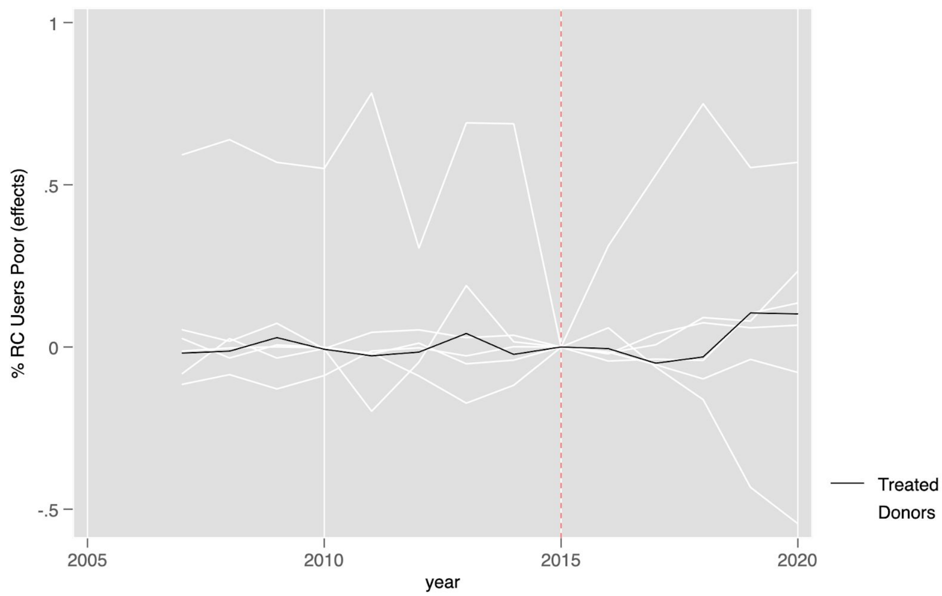


Figure 2.2: Outcome Gaps between Glen Canyon RC vs Donor Pool (scaled)

The standardized p-values for Glen Canyon RC do not show a clear trajectory over time. The p-value is high the first year after the RC receives renovation, drops to 0 the following year, increases to 0.5 in the third year, and drops back down to 0 in the fourth year before increasing to 0.33 in the final year. An average of these p-values over time suggests that there is a 36.6% chance the treatment effect estimated for Glen Canyon RC is due to chance.

	estimates	pvals	pvals_std
c1	-.0050829	1	1
c2	-.0500688	.5	0
c3	-.0307765	1	.5
c4	.1048239	.3333333	0
c5	.1020842	.6666667	.3333333

Table 2: Post-treatment Results: Effects, P-values, and Standardized P-values (scaled)

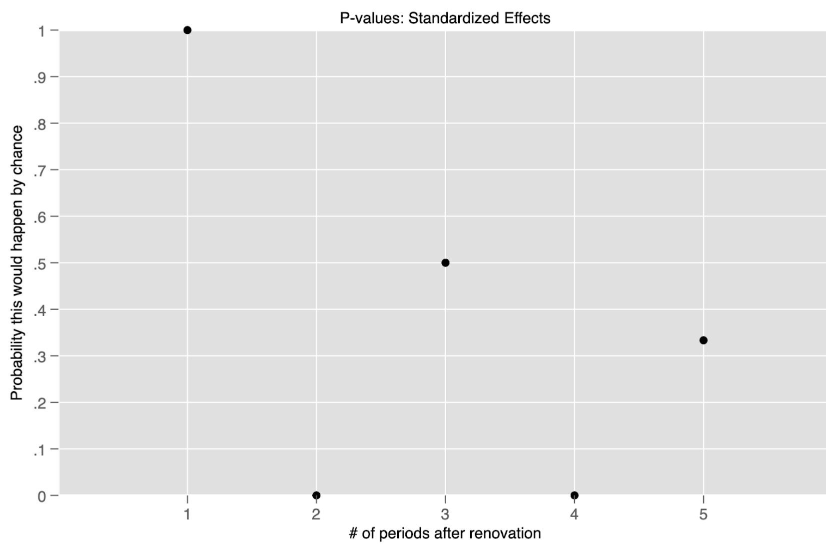


Figure 2.3: Standardized P-values for Glen Canyon RC

III. Palega RC (Portola)

As shown in Figure 3.2 below, the trajectories of Palega RC and the controls (placebos) in the pretreatment period are similar prior to 2011, with the exception of South of Market RC. After 2011 until the year of renovation, the effect trajectories diverge, with South of Market RC and Tenderloin RC showing the largest gaps. In the post-treatment period, the trajectories of Palega RC and the controls are also not similar. This can pose a problem regarding the selection of controls for Palega RC, but should render the effect of the renovation null. The proportion of placebos that have a post-treatment RMSPE at least as large as the average for Palega RC is .834. The proportion of placebos that have a ratio of post-treatment RMSPE over pretreatment RMSPE at least as large as the average ratio for Palega RC is .5. The proportion of placebos that have a pretreatment RMSPE at least as large as the average of Palega RC is .834. This proportion is large and thus concerning as a measure of fit. Given we specified a training period, the proportion of placebos that have a RMSPE for the validation period at least as large as the average of Palega RC- also a measure of fit- is .66.

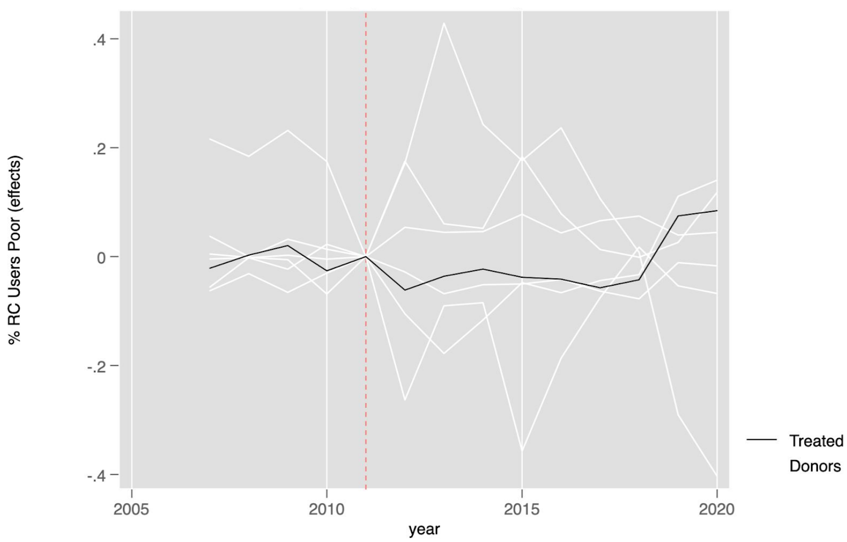


Figure 3.2: Outcome Gaps between Palega RC vs Donor Pool (scaled)

The standardized p-values for Palega RC show a downward trend over time. In the year following the renovation, we see a p.value of 0.5. This increases to .83 for the following year and holds for another year before beginning its downward decline. Seven years following renovation, the p-value increases slightly before leveling down to .167 for the last two years of the post-treatment. An average of these p-values over time suggests that there is a 46.2% chance the treatment effect estimated for Glen Canyon RC is due to chance.

	estimates	pvals	pvals_std
c1	-.0615177	.6666667	.5
c2	-.0361176	1	.8333333
c3	-.0229593	1	.8333333
c4	-.037892	1	.6666667
c5	-.041417	1	.5
c6	-.0572551	.6666667	.1666667
c7	-.0425557	.3333333	.3333333
c8	.0747377	.3333333	.1666667
c9	.0843201	.5	.1666667

Table 3: Post-treatment Results: Effects, P-values, and Standardized P-values (scaled)

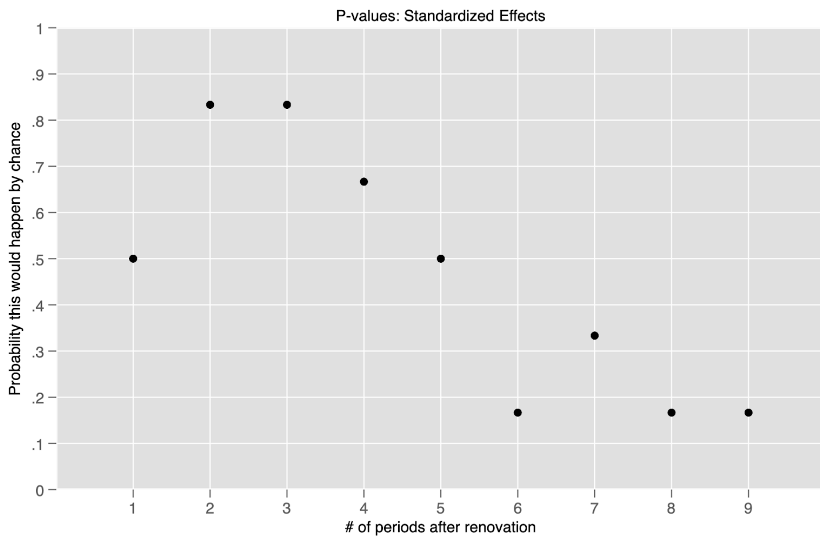


Figure 3.3: Standardized P-values for Palega RC

IV. Sunset RC (Sunset/Parkside)

From Figure 4.2 below, the trajectories of Sunset RC and the controls (placebos) in the pretreatment period are similar. In the post-treatment period, however, the trajectories of Sunset RC and the controls differ significantly. Sunset RC's trajectory is on the higher end of the spectrum regarding % poor RC user levels. The proportion of placebos that have a post-treatment RMSPE at least as large as the average for Sunset RC is .834. The proportion of placebos that have a ratio of post-treatment RMSPE over pretreatment RMSPE at least as large as the average ratio for Sunset RC is 1. The proportion of placebos that have a pretreatment RMSPE at least as large as the average of Sunset RC- a measure of fit- is 0. Given we specified a training period, the proportion of placebos that have a RMSPE for the validation period at least as large as the average of Sunset RC- also a measure of fit- is 0.

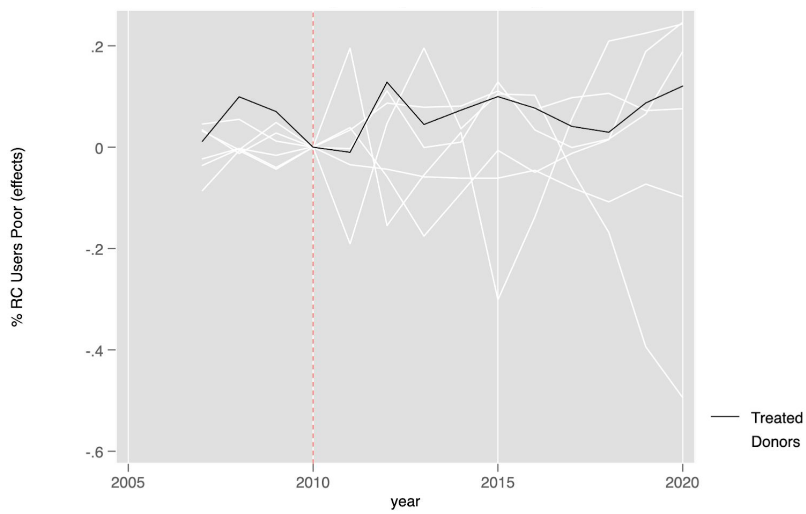


Figure 4.2: Outcome Gaps between Sunset vs Donor Pool (scaled)

The standardized p-values for Sunset RC follow a relatively constant trajectory over time. On average, the p-value over all of the time periods for the outcome is about .83. This suggests that there is about an 83% chance the positive treatment effect estimated for Sunset RC is due to chance. This may be due to the low number of pretreatment years we have to match on for Sunset RC and its synthetic control. Thus, we can not claim that the incline in the proportion of poor RC users for Sunset RC following the renovation is significant.

	estimates	pvals	pvals_std
c1	-.0100728	.8333333	.8333333
c2	.1282929	.1666667	.6666667
c3	.0448248	.8333333	.8333333
c4	.073015	.3333333	.6666667
c5	.0997764	.6666667	.8333333
c6	.0771234	.3333333	.8333333
c7	.0410214	.6666667	.8333333
c8	.029493	.6666667	.8333333
c9	.0873037	.5	.8333333
c10	.1206784	.6666667	1

Table 4: Post-treatment Results: Effects, P-values, and Standardized P-values (scaled)

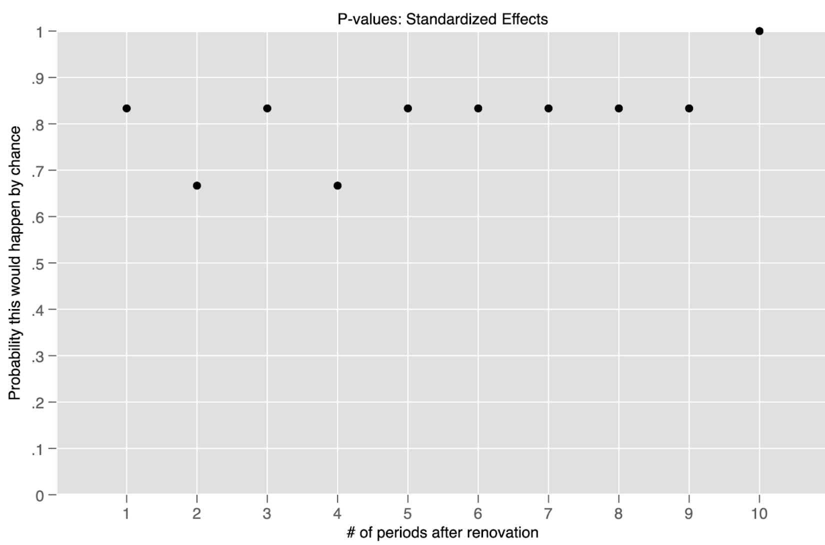


Figure 4.3: Standardized P-values for Sunset RC

V. **Betty Ann Ong RC** (**Nob Hill**)

From Figure 5.2 below, the trajectories of Betty Ann Ong RC and the controls (placebos) in the pretreatment period are similar. In the post-treatment period, their trajectories differ significantly, with Betty Ann Ong RC's trajectory on the higher end of the spectrum regarding positive renovation effects on the outcome. The proportion of placebos that have a post-treatment RMSPE at least as large as the average for Betty Ann Ong RC is .33. The proportion of placebos that have a ratio of post-treatment RMSPE over pretreatment

RMSPE at least as large as the average ratio for Betty Ann Ong RC is .834. The proportion of placebos that have a pretreatment RMSPE at least as large as the average of Betty Ann Ong RC- a measure of fit- is 0. Given we specified a training period, the proportion of placebos that have a RMSPE for the validation period at least as large as the average of Betty Ann Ong RC- also a measure of fit- is 0.

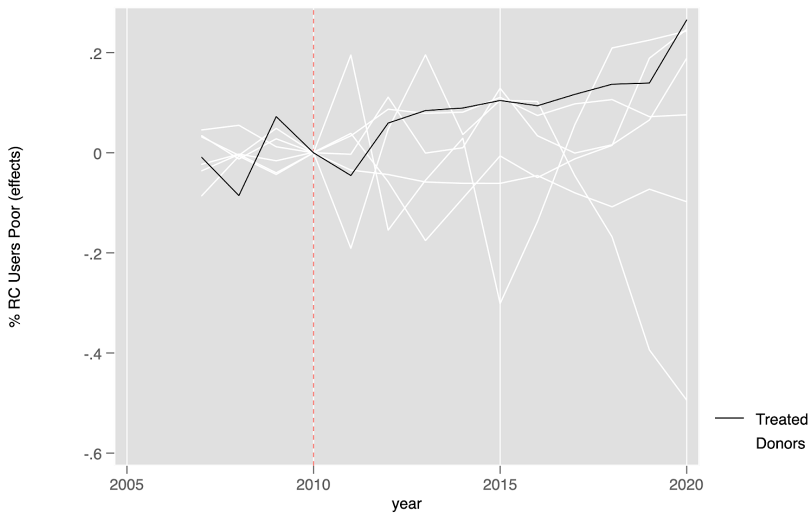


Figure 5.2: Outcome Gaps between Betty Ann Ong RC vs Donor Pool (scaled)

The standardized p-values for Betty Ann Ong RC do not follow a clear trajectory over time. On average, the p-value over all of the time periods for the outcome is also about .83. This may be due to the low number of pretreatment years we have to match on for Betty Ann Ong RC and its synthetic control. Thus, we can not claim that the incline (on average) in the proportion of poor RC users for Betty Ann Ong RC following the renovation is significant.

	estimates	pvals	pvals_std
c1	-.0454449	.3333333	.8333333
c2	.059441	.5	1
c3	.0844188	.3333333	.8333333
c4	.0894495	.1666667	.5
c5	.104426	.6666667	.8333333
c6	.0939928	.3333333	.8333333
c7	.1166711	0	.3333333
c8	.1369894	.3333333	.6666667
c9	.1393771	.5	.8333333
c10	.2654406	.1666667	.5

Table 5: Post-treatment Results: Effects, P-values, and Standardized P-values (scaled)

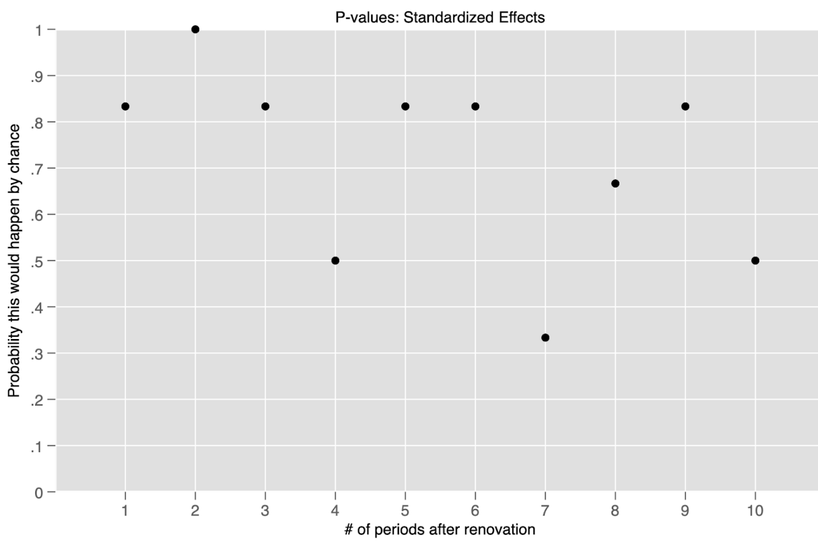


Figure 5.3: Standardized P-values for Betty Ann Ong RC

VI. Hamilton RC (Japantown)

From Figure 6.2 below, the trajectories of Hamilton RC and the controls (placebos) in the pretreatment period are similar. In the post-treatment period, their trajectories differ significantly. The proportion of placebos that have a post-treatment RMSPE at least as large as the average for Hamilton RC is 0.5. The proportion of placebos that have a ratio of post-treatment RMSPE over pretreatment RMSPE at least as large as the average ratio for Hamilton RC is .167. The proportion of placebos that have a pretreatment RMSPE at least as large as the average of Hamilton RC is 1. This proportion is large and thus concerning as a measure of fit.

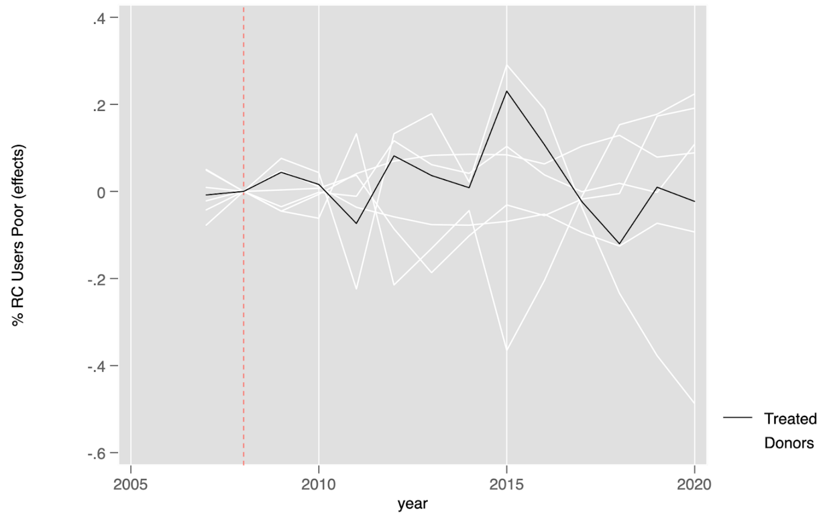


Figure 6.2: Outcome Gaps between Hamilton RC vs Donor Pool (scaled)

As shown in Figure 6.3 below, the standardized p-values for Hamilton RC in the last two periods of the post-treatment period have higher p-values in the outcome variable relative to the four years that preceded. On average however, they are close to 0 over time. Thus, we can claim that the estimated effect's trajectory on the proportion of poor RC users for Hamilton RC from the renovation is significant.

	estimates	pvals	pvals_std
c1	.0438058	.6666667	0
c2	.0161072	.3333333	.1666667
c3	-.0736391	.3333333	0
c4	.0817408	.6666667	0
c5	.0364665	1	.3333333
c6	.0085182	1	.6666667
c7	.2304454	.3333333	0
c8	.1076149	.3333333	0
c9	-.0236073	.5	0
c10	-.1202423	.6666667	0
c11	.0097133	.8333333	.8333333
c12	-.0229954	1	.5

Table 6: Post-treatment Results: Effects, P-values, and Standardized P-values (scaled)

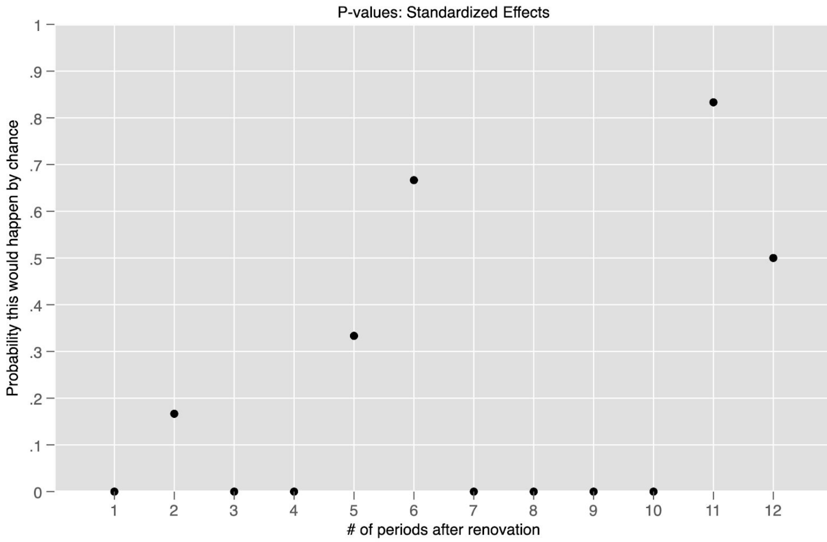


Figure 6.3: Standardized P-values for Hamilton RC

Summary of Results

I. Summary: Synthetic Control Method

Of the 6 treated RCs, those with larger pretreatment periods achieve a better fit between the RC and its constructed synthetic control in the pretreatment period. For 5 out of the 6 RCs- Randall RC, Glen Canyon RC, Palega RC, Sunset RC, and Betty Ann Ong RC- the effect from the renovation on the proportion of poor users on the RC is greater over time than it is for the synthetic control. While the levels in the proportion of poor users for the treated RCs are higher, the trajectories between the treated RC and its synthetic control are similar (with the exception of Randall RC where trajectories clearly diverge in the later part of the post-treatment period). For Randall RC, Glen Canyon RC, Palega RC, and Betty Ann Ong RC positive effects follow a positive trajectory that increases with time, particularly in the last two years of the post-treatment. For Sunset RC, while the effect from the renovation is positive for the majority of the post-treatment period, instead of having a clearly positive trajectory over time, the proportion of poor users fluctuates. It is only for Hamilton RC do we see a smaller proportion of poor users following the renovation over time than its synthetic.

The standardized p-values for Sunset RC, Betty Ann Ong RC, and Hamilton RC are higher than they are for Randall RC, Glen Canyon RC, and Palega RC - suggesting that the probability that the estimated effects from renovations on the proportion of poor users in these RCs is due to chance is higher. This is not a surprise considering that these two groups of RCs differ in the amount of pretreatment years available for constructing the synthetic control. In other words, we see a lower likelihood that the estimated effects from the renovation are due to chance in the RCs that have more pretreatment data available (Randall RC, Glen Canyon RC, and Palega RC) relative to those with very little (Sunset RC, Betty Ann Ong RC, and Hamilton RC). Randall RC- the center with the most amount of pretreatment years (9 years)- has a very low likelihood on average in the chance that its estimated effects are random. With Glen Canyon RC and Palega RC, we observe a downward trend over time in the standardized p-values for the outcome variable, highlighting the role time plays in measuring the effects of the renovations on the proportion of poor users for these RCs. Even for the RC sample with few pretreatment data, we observe that attributing estimated effects to chance is lower towards the end of the post-treatment period.

When determining the goodness of fit for the synthetic controls, assessing the gap between the pretreatment and post-treatment renovation effect on the proportion of poor users for the treated RCs and the controls is essential. For Randall RC, Glen Canyon RC and Hamilton RC, the proportion of controls that have at least as large of a gap as the average for the RC is lower than for Palega RC, Sunset RC, and Betty Ann Ong RC. For Randall RC, Glen Canyon RC and Hamilton RC, these proportions are 0, .167, and .167 respectively. For Palega RC, Sunset RC, and Betty Ann Ong RC, these proportions are .5, 1, and .834 respectively. This suggests that for the former group, the synthetic control is a better fit for the RC than for the latter group, and the treatment effect for the former is relatively more statistically significant. Forcing *synth* to match on pretreatment trends in the outcome variable was used in order to minimize the RMSPE between the treated RC and the controls. Without matching on trends, the same results do not hold regarding goodness of fit, preventing us from concluding that the estimated renovation effects are statistically significant.

II. Summary of Results: Difference-in-Differences Model

For 5 out of the 6 treated RCs, we see a positive effect overall from the renovation on the RC's proportion of poor users in the model that controls for the various predictors. It is only for Hamilton RC that we see a negative effect from treatment over time relative to its synthetic. In the models that do not include covariates, the renovation effect on the outcome is statistically significant for Glen Canyon RC, Sunset RC, and Betty Ann Ong RC. When we control for time, all RCs have one or more statistically significant RC and year effects except for Hamilton RC. Regarding the role time plays in the magnitude of the renovation effect on the RC's proportion of poor users, there is a large gap in the effect at the end of the post-treatment period relative to the beginning- except for Hamilton RC. In other words, for 5 out of the 6 RCs, the effect of the renovation on the proportion of poor users is more positive at the end than it was in the start. Betty Ann Ong RC has the largest difference in effect size if you compare the end of the post-treatment with the beginning- an estimated 24.35%. Hamilton RC, on the other hand, has the smallest difference- an estimated .83%.

The results from the DID method are similar to those from the synthetic control method in that we only see a negative renovation effect on the proportion of poor users for Hamilton RC. The trajectories for the RCs regarding the renovation effects, however, differ between the two models. This is most likely due to the covariates that were omitted in the DID model, which in turn can bias the estimated effects.

Conclusion

The purpose of this study is to examine whether improving city-owned RCs in San Francisco leads to any changes in the demographic composition of users. Are these public investments benefitting targeted communities in need, or are these improved spaces attracting more affluent users? For the majority of the RCs over time, we see a higher proportion of poor users in treated centers after they have been renovated relative to those that have not. This gap is shown to increase in the long term. Motivation for this research stems from literature pointing to the heterogenous distributional effects of large public investments on different socio-economic groups. The repercussions of improving public goods in growing metropolitan areas - like changes in housing and consumption- have shown to stimulate and/or exacerbate phenomena such as gentrification and displacement. Results from this study showing an increase in poor RC users overall suggest that the opposite may be happening- needy communities are in fact benefiting from these improvements. The hope is this helps inform public policy regarding proper evaluation of urban equity policies, public goods investment, and the distribution of social welfare . The long-term benefits that have been shown to accrue from early childhood exposure to neighborhood public goods such as RCs highlight the role public good quality has on decreasing disparities across groups. By focusing on RC renovations, this study adds to the literature on public goods access by including the role of quality and its diverse distributional effects.

Limitations and Future Study

My sample is limited to the period between 2007 and 2020. For RCs that were renovated shortly after 2007, their lack of pretreatment data hinders achieving a good synthetic fit. In addition, several observations from the SFRPD registrant data were excluded due to missing values and/or inconsistency. Missing registrant data in the outcome variable for a small number of observations was interpolated and/or extrapolated. The fact that drop-in RC users are missing from the SFRPD registrant data prevents us from fully capturing the effects of renovations on everyone who uses these spaces. Important registrant demographic data such as race and ethnicity is missing from the data which, if made available, can be crucial for measuring effects across diverse groups. Also, including a low-income status indicator in the registrant data would better explain the outcome variable, instead of using a proxy such as the proportion of free and reduced lunch by zip code as I do in this study. The *synth* and *synth_runner* packages on STATA also present issues in that the options for both do not always carry over. While in the latter a user may match on trends and include a training and validation period, these options are not available in *synth*. However, if one wants information on unit weights and the predictor balance, *synth* must be used. As a result, using the latter to retrieve unit and predictor weighting to explain the model generated with *synth_runner* is not fully accurate.

Extending this assessment to other parks and recreation services in the city will tell us more about public goods investments and the long-term effects on the demographic composition of users. Expanding the study further to other cities is also important to determine whether similar results hold in cities across and outside the Bay Area. This is with the goal of guiding public policy to which public investments work in terms of reaching needy communities, as well as the differential impacts from these investments across groups and neighborhoods.

References

Abadie, Alberto, and Javier Gardeazabal. "The economic costs of conflict: A case study of the Basque Country." *American economic review* 93.1 (2003): 113-132.

Abadie, Alberto, Alexis Diamond, and Jens Hainmueller. "Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program." *Journal of the American statistical Association* 105.490 (2010): 493-505.

Abadie, A. "Using synthetic controls: Feasibility, data requirements, and methodological aspects." *Journal of Economic Literature* (2019).

Bell, Alex, et al. "Who becomes an inventor in America? The importance of exposure to innovation." *The Quarterly Journal of Economics*134.2 (2018): 647-713.

Banzhaf, H. Spencer, and Randall P. Walsh. *Segregation and Tiebout sorting: Investigating the link between investments in public goods and neighborhood tipping*. No. w16057. National Bureau of Economic Research, 2010.

Banzhaf, H. Spencer and Walsh, Randall P., Do People Vote with Their Feet? An Empirical Test of Environmental Gentrification (March 1, 2006). RFF Discussion Paper No. 06-10. Available at SSRN: https://ssrn.com/abstract=901657 or http://dx.doi.org/10.2139/ssrn.901657

Bryson, Jeremy. "Brownfields gentrification: redevelopment planning and environmental justice in Spokane, Washington." *Environmental Justice* 5.1 (2012): 26-31.

Byrne, Jason. "When green is White: The cultural politics of race, nature and social exclusion in a Los Angeles urban national park." *Geoforum* 43.3 (2012): 595-611.

Chetty, Raj, et al. "Where is the land of opportunity? The geography of intergenerational mobility in the United States." *The Quarterly Journal of Economics* 129.4 (2014): 1553-1623.

Ding, Lei, Jackelyn Hwang, and Eileen Divringi. "Gentrification and residential mobility in Philadelphia." *Regional science and urban economics* 61 (2016): 38-51.

Formoso, Diana, Rachel N. Weber, and Marc S. Atkins. "Gentrification and urban children's well-being: Tipping the scales from problems to promise." *American journal of community psychology* 46.3-4 (2010): 395-412.

Fraser, James C. "Beyond gentrification: Mobilizing communities and claiming space." *Urban Geography* 25.5 (2004): 437-457.

Gharehgozli, Orkideh. "Synthetic Control and Dynamic Panel Estimation: A Case Study of Iran." (2018).

Grier, Sonya A., and Vanessa G. Perry. "Dog parks and coffee shops: Faux diversity and consumption in gentrifying neighborhoods." *Journal of Public Policy & Marketing* 37.1 (2018): 23-38.

Heckman, James J. 2006. "Skill Formation and the Economics of Investing in Disadvantaged Children." Science, 312 (5782): 1900{1902.

Ho, Ching-Hua, et al. "Parks, recreation and public health." *Parks & Recreation (Ashburn)* 38.4 (2003): 18-27.

Iasonidou, Sofia. "Estimating the effect of the 2008 financial crisis on GNI in Greece and Iceland: A synthetic control approach." (2016).

Jeffres, Leo W., et al. "The impact of third places on community quality of life." *Applied Research in Quality of Life* 4.4 (2009): 333.

Kahn, Matthew E. "Gentrification trends in new transit-oriented communities: evidence from 14 cities that expanded and built rail transit systems." *Real Estate Economics* 35.2 (2007): 155-182.

Kaźmierczak, Aleksandra. "The contribution of local parks to neighbourhood social ties." *Landscape and urban planning* 109.1 (2013): 31-44.

Kumar, Anil, and Che-Yuan Liang. "Labor Market Effects of Credit Constraints: Evidence from a Natural Experiment." (2018).

McClelland, Robert, and Sarah Gault. "The synthetic control method as a tool to understand state policy." *Washington, DC: Urban-Brookings Tax Policy Center* (2017).

Pryor, Brandy N. Kelly, and Corliss W. Outley. "Just spaces: Urban recreation centers as sites for social justice youth development." *Journal of Leisure Research* 46.3 (2014): 272-290.

Rigolon, Alessandro. "Parks and young people: An environmental justice study of park proximity, acreage, and quality in Denver, Colorado." *Landscape and Urban Planning* 165 (2017): 73-83.

Witt, Peter A., and John L. Crompton. "The at-risk youth recreation." *Journal of Park and Recreation Administration* 14.3 (1996): 1-9.

Zuk, Miriam, et al. "Gentrification, displacement, and the role of public investment." *Journal of Planning Literature* 33.1 (2018): 31-44.

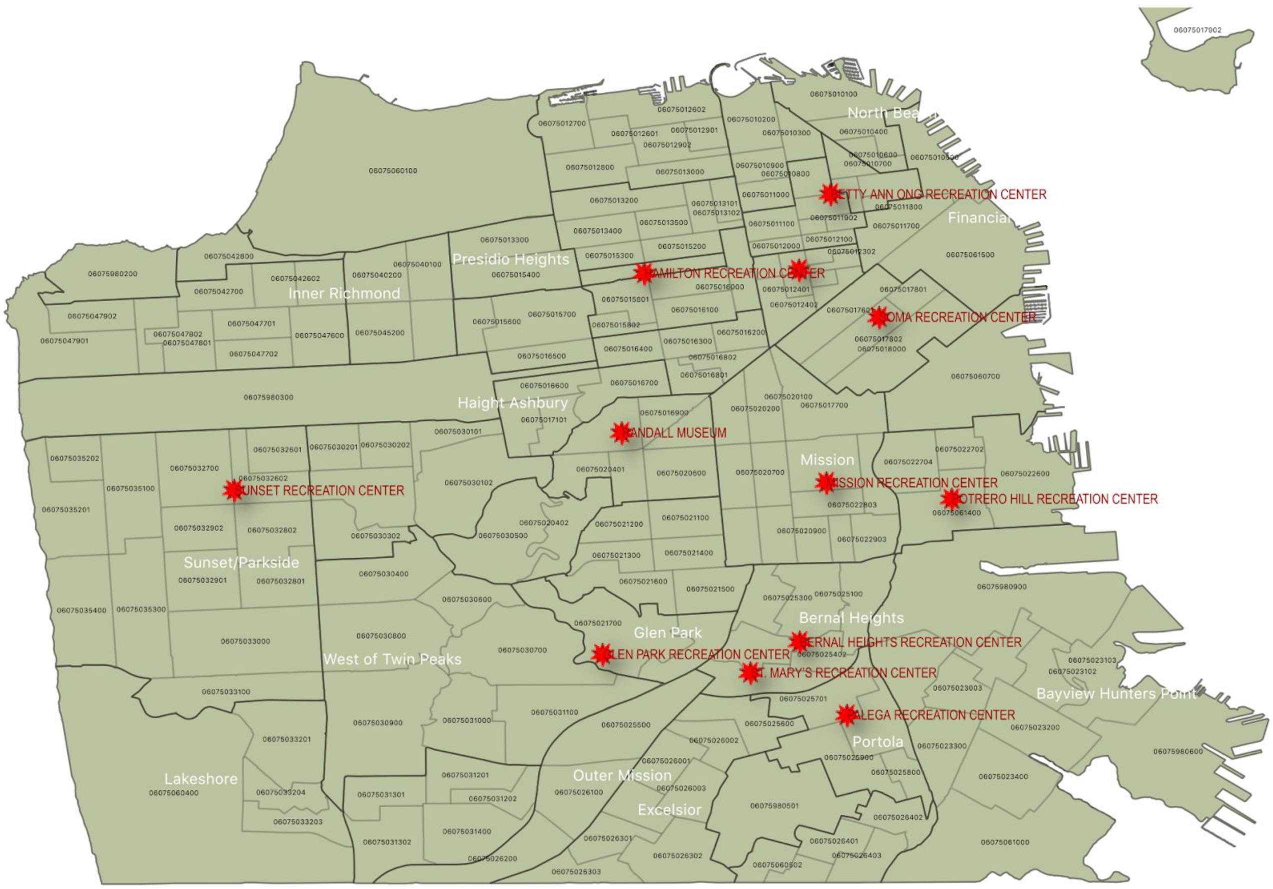
https://sfrecpark.org/wp-content/uploads/Equity-Analysis-and-Metrics-FY18-PROSAC-draft.pdf

http://www.cedlas-er.org/sites/default/files/aux_files/slides-aboal_0.pdf

https://yiqingxu.org/teaching/17802/synth.pdf

https://www.fns.usda.gov/nslp

Appendix



Map 1: San Francisco analysis neighborhoods grouped by 2010 Census tracts alongside RC locations

Pretreatment and Post-treatment: Root Mean Squared Prediction Error

The below tables contain the pretreatment match quality- in terms of RMSPE- for each of the treated RCs. The RMSPE is a measure of how good of a fit the synthetic control is to the treated unit. The tables also contain a measure of the post-treatment effect.

I. Randall RC (Castro/Upper Market)

Neighborhood	RC	Pre-RMSPE	Post-RMSPE
Bernal Heights	Bernal Heights RC	0.0338226	0.10236882
Bernal Heights2	St. Mary's RC	0.05906578	0.12980504
Castro/Upper Market	**Randall RC**	**0.04113844**	**0.27305016**
Mission	Mission RC	0.05379913	0.11905924
Potrero Hill	Potrero Hill RC	0.0721448	0.07021201
South of Market	South of Market RC	0.1013229	0.39541367
Tenderloin	Tenderloin RC	0.21617392	0.23164375

Table A: Pretreatment and Post-treatment RMSPE for Randall RC

II. Glen Canyon RC (Glen Park)

Neighborhood	RC	Pre-RMSPE	Post-RMSPE
Bernal Heights	Bernal Heights RC	0.02744969	0.06410445
Bernal Heights2	St. Mary's RC	0.07142446	0.05596882
Glen Park	**Glen Canyon RC**	**0.02276355**	**0.07055376**
Mission	Mission RC	0.04159731	0.1179546
Potrero Hill	Potrero Hill RC	0.07556822	0.0826649
South of Market	South of Market RC	0.09775075	0.32167706
Tenderloin	Tenderloin RC	0.5814057	0.56040084

Table B: Pretreatment and Post-treatment RMSPE for Glen Canyon RC

III. Palega RC (Portola)

Neighborhood	RC	Pre-RMSPE	Post-RMSPE
Bernal Heights	Bernal Heights RC	0.02286171	0.05039914
BernalHeights2	St. Mary's RC	0.045070589	0.05612655
Portola	**Palega RC**	**0.017611736**	**0.0543159**
Mission	Mission RC	0.02900039	0.10095898
Potrero Hill	Potrero Hill RC	0.00324757	0.10428874
South of Market	South of Market RC	0.18149179	0.26189891
Tenderloin	Tenderloin RC	0.030848097	0.17013082

Table C: Pretreatment and Post-treatment RMSPE for Palega RC

IV. Sunset RC (Sunset/Parkside)

Neighborhood	RC	Pre-RMSPE	Post-RMSPE
Bernal Heights	Bernal Heights RC	0.02301622	0.06993795
Bernal Heights2	St. Mary's RC	0.02698088	0.08433651
Sunset/Parkside	**Sunset RC**	**0.06114772**	**0.08033813**
Mission	Mission RC	0.04832226	0.08396089
Potrero Hill	Potrero Hill RC	0.0148169	0.11970858
South of Market	South of Market RC	0.02924247	0.23013017
Tenderloin	Tenderloin RC	0.03624188	0.18232596

Table D: Pretreatment and Post-treatment RMSPE for Sunset RC

V. Betty Ann Ong RC (Nob Hill)

Neighborhood	RC	Pre-RMSPE	Post-RMSPE
Bernal Heights	Bernal Heights RC	0.02301622	0.06993795
Bernal Heights2	St. Mary's RC	0.02698088	0.08433651
Nob Hill	**Betty Ann Ong RC**	**0.05614379**	**0.12756512**
Mission	Mission RC	0.04832226	0.08396089
Potrero Hill	Potrero Hill RC	0.0148169	0.11970858
South of Market	South of Market RC	0.02924247	0.23013017
Tenderloin	Tenderloin RC	0.03624188	0.18232596

Table E: Pretreatment and Post-treatment RMSPE for Betty Ann Ong RC

VI. Hamilton RC (Japantown)

Neighborhood	RC	Pre-RMSPE	Post-RMSPE
Bernal Heights	Bernal Heights RC	0.0345854	0.0736254
Bernal Heights2	St. Mary's RC	0.0300728	0.0792104
Japantown	**Palega RC**	**0.0057727**	**0.0894473**
Mission	Mission RC	0.054936	0.0610483
Potrero Hill	Potrero Hill RC	0.0065301	0.1020442
South of Market	South of Market RC	0.0358209	0.2352599
Tenderloin	Tenderloin RC	0.0161034	0.1754262

Table F: Pretreatment and Post-treatment RMSPE for Hamilton RC

Synthetic Control Method and *Synth* Package

I use the *synth* and *synth_runner* packages in Stata developed by Alberto et al. (2010)- for their study on the effects of California's tobacco control program on consumption- for optimal weight choice that allows for a good synthetic fit (minimum squared prediction error) for the pretreatment period. The results in the Results section above were achieved via matching treated and control RCs on outcome trends in the pretreatment period, and dividing this period into training and validation sections using *synth_runner*. This method, however, does not generate the weights control units and predictors receive for creating a good match- only *synth* generate these weights. *Synth* does not have the option to add a training period however, so the results and weights generated do not capture completely what the main results above (using *synth_runner*) express. They do however give us an insight of what the weight distribution for the control units and predictors generally looks like.

Below are the predictor balances, unit weights, and RMSPEs for each of the 6 treated RCs after running the model using the *synth* package. For Randall RC and Glen Canyon RC, the entire predictor list was included in the model. For Palega RC, Sunset RC, Betty Ann Ong RC, and Hamilton RC, the share of the population over 75, single parent household, level of English proficiency, poverty under the federal 200 % level, disabled, rent-burdened household, and zero vehicle household predictors were dropped due to these RCs having earlier renovation dates. Median gross rent, median household income, public transit, and commute time over an hour were also dropped for Hamilton RC given its even earlier renovation date. All of the RCs were also matched on outcome trends (scaled) in the pretreatment period exception of Sunset RC, which includes the lagged outcome from 2008 as an additional control. For all of the RCs, I use the fully nested optimization procedure available in *synth* that searches among all diagonal positive semidefinite V-matrices and sets of W-weights for the best fitting convex combination of the control units. This method produces convex combinations that achieve an even lower MSPE than what the default generates. In addition, I also use the *allopt* feature in *synth* (for all but Randall RC) to achieve even more robust results. This method runs the nested optimization three times using three different starting points to find the best result of the three. These robustness tools in *synth* considerably change the weights assigned to each control unit relative to when they are not used.

I. Randall RC (Castro/Upper Market)

Predictors	Randall	Synthetic
Equity Zone	0	0
Distance Bart station	1594.14	1945.33
Distance School	3088.92	3653.62
Total Population	20701.13	12566.25
Age < 20	.0723925	.1377401
20 > Age > 44	.514811	.5518547
White	16737.38	8464
Black	564.875	923.75
Native	93.75	159.5
Asian	1966.25	1782.125
Median Gross Rent	1764.286	2026.964
Median HH Income	88701.81	117975.4
Public Transit	.399881	.2361429
Commute > 60 min	.114961	.1047315
Age > 75	953	527
Single Parent HH	293	591
Level English Proficiency	315	551
Poverty < 200%	2758	2471
Disabled	2180	991
Rent Burdened HH	893	366
Zero Vehicle HH	3113	689
Property Value	748541.2	2000803
Businesses opened	247.1	220.7
Affordable Housing Units	4.5	11.2
Housing Units	41.9	91
Crime	779.9	4030.9
Evictions	60.3	22
Voter Turnout	.42788	.33822
Outcome Trend	1.030909	1.083051

Table 1A: Predictor Balance for Randall RC vs Synthetic (nested)

Neighborhood	Unit Weight
Bernal Heights	0
Bernal Heights2	0
Mission	0
Potrero Hill	1
South of Market	0
Tenderloin	0

Table 1B: Neighborhood Weights for Controls

RMSPE	.0812404

Table 1C: RMSPE for Randall RC

II. Glen Canyon RC (Glen Park)

Predictors	Glen Canyon	Synthetic
Equity Zone	0	0
Distance Bart station	701.7	1495.467
Distance School	3682.36	4455.637
Total Population	8100.714	19226.74
Age < 20	.1593318	.1667633
20 > Age > 44	.3704508	.4946015
White	5660.429	12057.31
Black	424.2857	1148.833
Native	34	148.8624
Asian	1157.714	3076.643
Median Gross Rent	1600.5	1746.99
Median HH Income	69579.5	88727.74
Public Transit	.3259167	.2711781
Commute > 60 min	.0959462	.099964
Age > 75	436	847.415
Single Parent HH	201	573.286
Level English Proficiency	152	1196.519
Poverty < 200%	1042	4198.115
Disabled	486	1651.107
Rent Burdened HH	181	518.653
Zero Vehicle HH	380	1027.129
Property Value	409557.1	3111337
Businesses Opened	57.33333	195.9097
Affordable Housing Units	.4444444	6.505222
Housing Units	4.333333	52.09689
Crime	678.1111	2265.996
Evictions	7.888889	31.71411
Voter Turnout	.44514	.3542251
Outcome Trend	1.032015	1.042188

Table 2A: Predictor Balance for Glen Canyon RC vs Synthetic (nested allopt)

Neighborhood	Unit Weight
Bernal Heights	0
Bernal Heights2	.521
Mission	0
Potrero Hill	.479
South of Market	0
Tenderloin	0

Table 2B: Neighborhood Weights for Controls

RMSPE	.0547066

Table 2C: RMSPE for Glen Canyon RC

III. Palega RC (Portola)

Predictors	Palega	Synthetic
Equity Zone	1	0
Distance Bart station	2173.35	1238.156
Distance School	4914.93	4914.372
Total Population	14778.67	22703.28
Age < 20	.228829	.1864252
20 > Age > 44	.3477342	.4656836
White	4175	13849.56
Black	958	1397.451
Native	35.33333	171.9307
Asian	7960.667	3667.856
Median Gross Rent	1456.25	1499.717
Median HH Income	45109.13	69846.04
Public Transit	.246875	.2872397
Commute > 60 min	.0809443	.0871747
Property Value	1024675	5424350
Businesses Opened	52.2	149.239
Affordable Housing Units	.2	2.705
Housing Units	8.8	25.6148
Crime	1545.4	1162.077
Evictions	14	31.2162
Voter Turnout	.3404	.3450511
Outcome Trend	.968254	.9107724

Table 3A: Predictor Balance for Palega RC vs Synthetic (nested allopt)

Neighborhood	Unit Weight
Bernal Heights	0
Bernal Heights2	.819
Mission	0
Potrero Hill	.181
South of Market	0
Tenderloin	0

Table 3B: Neighborhood Weights for Controls

RMSPE	.057257

Table 3C: RMSPE for Palega RC

IV. Sunset RC (Sunset/Parkside)

Predictors	Sunset	Synthetic
Equity Zone	0	0
Distance Bart station	5234.73	1081.87
Distance School	5692.91	5193
Total Population	75666.5	24975.5
Age < 20	.1841693	.1993088
20 > Age > 44	.3823008	.4505671
White	29407.5	14707
Black	723	1493.5
Native	128.5	164
Asian	41797.5	4110
Median Gross Rent	1568.929	1386
Median HH Income	62898.93	63461.83
Public Transit	.2730714	.3105
Commute > 60 min	.1288452	.0799205
Property Value	5443802	7606890
Businesses Opened	368	130.5
Affordable Housing Units	2.75	2
Housing Units	12.5	12.5
Crime	3969.75	626.75
Evictions	72.75	32.75
Voter Turnout	.30665	.3074
Outcome 2008	.52	.63

Table 4A: Predictor Balance for Sunset RC vs Synthetic (nested allopt)

Neighborhood	Unit Weight
Bernal Heights	0
Bernal Heights2	1
Mission	0
Potrero Hill	0
South of Market	0
Tenderloin	0

Table 4B: Neighborhood Weights for Controls

RMSPE	.1406236

Table 4C: RMSPE for Sunset RC

V. Betty Ann Ong RC (Nob Hill)

Predictor.	Betty Ann	Synthetic
Equity Zone	1	.312
Distance Bart station	1152.43	1185.805
Distance School	4350.8	4148.663
Total Population	25562	18039.98
Age < 20	.0803479	.1449964
20 > Age > 44	.5296873	.5012125
White	13977.5	9977.986
Black	571	1675.591
Native	102.5	142.304
Asian	9385	3640.743
Median Gross Rent	1183.429	1425.234
Median HH Income	46926.14	65809
Public Transit	.2418571	.289092
Commute > 60 min	.0712329	.1053992
Property Value	8.05e+07	1.22e+07
Businesses Opened	174.75	188.0535
Affordable Housing Units	3	42.6845
Housing Units	56.25	298.3445
Crime	592.25	1411.296
Evictions	40	40.5185
Voter Turnout	.29475	.2790272
Outcome Trend	.943662	.9583231

Table 5A: Predictor Balance for Betty Ann Ong RC vs Synthetic (nested allopt)

Neighborhood	Unit Weight
Bernal Heights	0
Bernal Heights2	.406
Mission	0
Potrero Hill	.282
South of Market	.312
Tenderloin	0

Table 5B: Neighborhood Weights for Controls

RMSPE	.039379

Table 5C: RMSPE for Betty Ann Ong RC

VI. Hamilton RC (Japantown)

Predictors	Hamilton	Synthetic
Equity Zone	1	0
Distance Bart station	1916.12	1081.87
Distance School	3988.04	5193
Total Population	3591	24952
Age < 20	.0935673	.2022842
20 > Age > 44	.4138123	.4656341
White	1691	12762
Black	639	1728
Native	6	149
Asian	1057	4293
Property Value	3985668	1.40e+07
Businesses Opened	29.5	133.5
Affordable Housing Units	3	2.5
Housing Units	81	21
Crime	604.5	621.5
Evictions	4	33.5
Voter Turnout	.3422	.3903
Outcome Trend	.9916666	1

Table 6A: Predictor Balance for Hamilton RC vs Synthetic (nested allopt)

Neighborhood	Unit Weight
Bernal Heights	0
Bernal Heights2	1
Mission	0
Potrero Hill	0
South of Market	0
Tenderloin	0

Table 6B: Neighborhood Weights for Controls

RMSPE	.0353553

Table 6C: RMSPE for Hamilton RC