Causal inference, high-frequency data, and the recreational value of water quality

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July 10, 2024

Abstract

Motivated by recent calls to incorporate causal inference techniques in recreation demand analyses, we introduce a method that leverages high-frequency recreation data to embed panel data causal inference techniques, such as difference-in-differences, event study designs, and synthetic controls, within travel cost random utility models. Our model allows site mean utilities to vary over time, which preserves panel variation for a second-stage linear regression. We apply our model to value the welfare losses caused by a series of water-quality-induced beach closures at Lake St. Clair in the summer of 2022. We estimate that the beach closures caused aggregate welfare losses of around $150,000. Damages can persist weeks after water quality returns to swimmable levels, and losses are larger on weekends and hotter days, on average. Our approach is particularly useful for valuing the welfare impacts caused by resource shocks, like harmful algal blooms or wildfires, and the increasing availability of high-frequency recreation datasets make it broadly applicable.

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1 Introduction

New datasets are capturing more information about recreation behavior than ever before. Cellphone-based mobility data provide continuous monitoring at thousands of recreation sites, and many park systems now record and digitize detailed administrative records. These emerging recreation datasets often contain rich temporal variation, commonly providing visitation information at the daily or weekly level. Survey-based datasets, which have traditionally been used to estimate recreation demand models, typically capture temporal variation less precisely. An emerging literature has begun using these cutting-edge data in recreation demand analyses (Newbold et al., 2022; Gellman et al., 2023; Knittel et al., 2023).

Developing methods to exploit this temporal variation is critical for the recreation demand literature. Many recreation demand analyses use cross-sectional variation to identify preferences for site attributes, which can leave estimates susceptible to omitted variables bias. Ji et al. (2020) discuss how omitted variables bias can undermine reliability of recreation demand analyses and suggest that, "future work that finds ways to improve estimates of the causal effect of water quality on recreation will be fruitful." In their best practices paper, Lupi et al. (2020) make similar suggestions, writing that "researchers should seek out opportunities to use modern identification methodologies, such as natural experiments, panel data approaches, and instrumental variable models to improve causal inference in recreation analysis." Our proposed method provides a tool that will help researchers address these recommendations.

This paper provides a method that exploits high-frequency data to apply panel data causal inference techniques within travel cost random utility models (RUM). We adapt the traditional travel cost RUM by allowing site mean utilities to vary daily—i.e., we include a full panel of site-by-date fixed effects. These mean utilities can be "unpacked" and written as functions of site attributes. Following Murdock (2006), we estimate the model in two stages. In stage 1, we estimate a subset of the parameters with maximum likelihood estimation, using a contraction mapping to solve for the panel of site mean utilities. In stage 2, we estimate counterfactual mean utilities—the mean utilities the site would have provided under different environmental or management conditions. Given a suitable natural experiment, these counterfactual mean utilities can be estimated using any popular panel data causal inference technique, such as difference-in-differences, an event study design, or the synthetic control method. We calculate the welfare impact of a resource change by comparing the expected utility given observed mean utilities to the expected utility given counterfactual mean utilities.

We apply our method to value the recreational welfare losses from beach closures caused by poor water quality. We focus on a series of beach closures at Lake St. Clair Metropark between July 21 and September 15, 2022. Over this time period, elevated bacteria levels forced on-and-off closures for 33 days in all. To estimate the welfare losses of these closures, we combine high-frequency administrative visitation data with daily beach closure records. Our visitation data contain visitors' home ZIP code and the minute of entry for over 1 million visits to the Huron-Clinton Metroparks in southeast Michigan. For simplicity, we focus on annual passholders, because a unique household identifier allows us to observe each household's full visitation history for the season. This restriction leaves us with 740,000 visits (roughly 70 percent of all visits) made by 115,000 annual passholders.

In the first estimation stage, we recover a full panel of site-by-date mean utilities. These estimates describe the mean utility a visitor receives from visiting a given park on a given day under observed conditions in the summer of 2022—with the beach closures. In the second estimation stage, we use the synthetic control method to estimate counterfactual mean utilities for Lake St. Clair Metropark—i.e., the mean utility Lake St. Clair Metropark would have provided if there were no beach closures. We calculate the welfare impact of the closure by comparing expected utility under observed conditions (with closures) and counterfactual conditions (no closures).

We estimate that the 2022 beach closures caused aggregate welfare losses of $150,000 for annual passholders, about $14 per lost trip. Applying an event study design in the second stage produces similar estimates. We also find evidence that the closures cause a stigma effect; welfare losses can continue to accumulate weeks after a closure ends. Intuitively, losses tend to be larger on weekends and hotter days, when the swimming beach is likely most popular.

We compare our estimates to two more traditional models with a cross-section of alternative-specific constants and an indicator variable equal to one on days when Lake St. Clair is impacted by the closures. This traditional approach is highly sensitive to assumptions on the duration of stigma effects. If we assume Lake St. Clair is impacted every day after the first closure, then the aggregate welfare loss estimate is similar to the estimate from our proposed approach, only about

9 percent smaller. If we assume Lake St. Clair is only impacted on days when the beach is closed—i.e., there is no stigma effect—then the aggregate welfare loss is roughly one-third of our preferred estimates. Unlike the traditional approach, our method requires minimal assumptions regarding stigma effects. Instead, we specify the initial closure date and allow the data to reveal when Lake St. Clair is impacted.

Our work makes three main contributions. First, we contribute to the literature focused on random utility modeling and estimation methods. Much of this research emerged in industrial organization settings and proposed linearizing the RUM estimation problem using an inversion or contraction mapping (Berry, 1994). Linearizing the estimation problem often reduces the computational burden, and it enables researchers to apply instrumental variables techniques to identify model parameters. Indeed, Berry (1994), Berry et al. (1995), and Nevo (2001), all use linearization to apply instrumental variables and avoid bias from omitted product attributes when estimating the price coefficient. Murdock (2006) and Timmins and Murdock (2007) adapt these linearization techniques to improve identification in travel cost RUMs. They apply the Berry contraction mapping to estimate a full cross-section of mean utilities, then regress these mean utilities on site attributes in a second-stage, linear regression. Because travel costs vary across individuals, preferences for travel costs are not subsumed by site mean utilities. Thus, including mean utilities controls for unobserved site attributes when estimating the travel cost coefficient (which is analogous to the price coefficient) and solves the primary identification concern from the industrial organization literature without an instrument. Omitted site attributes can still bias estimates for other site attributes, however, because preferences for site attributes are estimated using a cross-sectional regression. Timmins and Murdock show that instrumental variables can be used to avoid omitted variables bias in the second-stage regression.

We provide a blueprint for applying a broad suite of panel data causal inference techniques to estimate preferences in a RUM. Intuitively, our method is similar to the classic, industrial organization papers that linearize the RUM estimation problem to leverage instrumental variables, a technique originally developed for reduced-form settings. Methodologically, our two-stage estimation procedure closely follows Timmins and Murdock. The key difference is that we estimate a panel of mean utilities rather than a cross-section, preserving panel variation for the second-stage regression. Once again, it is worth noting that the second-stage regression is linear, so researchers can leverage any panel data causal inference technique available in reduced-form settings.

Second, we contribute to the recreation demand, nonmarket valuation literature by providing a method optimized to value shocks in resource quality. Much of the recreation demand literature applying travel cost RUMs values marginal changes in average resource quality (Egan et al., 2009; Keiser, 2019; Kuwayama et al., 2022; Kim, 2023). Yet, many resource changes are nonmarginal and difficult to capture in average measures, e.g., harmful algal blooms, wildfires, and infrastructure improvements. Fewer papers use revealed preference methods to value shocks like these. One exception is English et al. (2018) who value the recreational welfare impacts of the *Deepwater Horizon* oil spill. English et al.'s model allows mean utilities to vary between non-spill and spill conditions; this is similar to how we allow mean utilities to vary over time. The key difference between our approach and English et al.'s is that they estimate their model in one stage, while we estimate our model in two stages. We prefer our two-stage approach for two reasons. First, it allows us to capture substantial temporal heterogeneity in the magnitude and timing of welfare impacts. This temporal heterogeneity is often averaged over in a one-stage approach, because the nonlinearity makes it difficult to estimate a large number of parameters characterizing the heterogeneity. Second, our method allows researchers to leverage causal inference techniques typically applied in reduced-form settings, allowing researchers to base their identification strategies on widely used assumptions, such as parallel trends. Threats to these identification strategies are often well-understood. Furthermore, identification assumptions can often be assessed—e.g., by checking for pre-trends.

Third, our application valuing the impacts of beach closures contributes a policy-relevant welfare estimate. One of the Clean Water Act's primary goals is to achieve "swimmable" water quality, but elevated bacteria levels and harmful algal blooms often make waterbodies temporarily unfit for swimming. In our empirical setting, each year almost 25 percent of Michigan's swimming beaches experience a closure from elevated bacteria levels. One group of studies uses stated preference methods to value hypothetical beach closures (Roberts et al., 2008; Boudreaux et al., 2023). Other work uses revealed preference to value beach closures, but this literature typically proxies for beach closures with full site closures or historical and seasonal summary statistics—e.g., the average number of beach closures over the last three years (Murray et al., 2001; Parsons et al., 2009; Wolf et al., 2019). To our knowledge, only Lew and Larson (2005) estimate the impact of an observed beach closure using within-season variation in closure conditions. Counterintuitively, their estimate of the beach closure coefficient is not statistically significant. Like Lew and Larson, we value the impacts of observed beach closures without proxying with summary statistics or a full site closure. However, we find that beach closures cause significant, negative welfare impacts that can persist weeks after the closure ends.

The remainder of the paper proceeds as follows. Section 2 and Section 3 describe our model and two-stage panel estimation procedure. Section 4 applies our approach to estimate the welfare impacts of the 2022 Lake St. Clair beach closures, and Section 5 concludes.

2 Model

Like many recreation demand analyses, we adopt a repeated travel cost RUM framework. In this model, individuals repeatedly decide where to recreate, selecting the alternative from their choice set that provides them with the highest utility at each choice occasion. The choice set consists of recreation sites, indexed $j = \{1, ...J\}$, as well as an outside option, indexed $j = 0$. To fix ideas, we assume each individual faces daily choice occasions—i.e., each individual makes a recreation choice every day.

We specify the utility individual i receives from visiting site j at choice occasion t as

$$U_{ijt} = \delta_{jt} + \alpha^{TC} TC_{ij} + \alpha^{X} X_{ijt} + \epsilon_{ijt}, \tag{1}$$

where TC_{ij} is the travel cost, X_{ijt} contains demographic characteristics, and ϵ_{ijt} is an idiosyncratic error term. We call the δ_{jt} parameters "mean utilities". They are site-by-choice occasion fixed effects that represent the mean utility an individual receives from accessing site j at choice occasion t after controlling for travel costs and the X_{ijt} variables. They capture the utility provided by both observable and unobservable site attributes. Many recreation demand papers include time-invariant mean utilities and refer to them as alternative- or site-specific constants.

Existing studies sometimes decompose mean utilities as functions of site attributes, such as water quality measures or fish catch rates, following Murdock (2006) and Timmins and Murdock (2007). Decomposing mean utilities with a variety of attributes is also feasible in our model, but it may not fully leverage the mean utilities' temporal variation. Many attributes will not vary meaningfully within a recreation season and including many attributes may obscure sources of identifying variation.

Rather, our model's relative strength is that its mean utilities can be decomposed to isolate variation from natural experiments. Researchers can decompose mean utilities to leverage any popular panel data causal inference technique, including difference-in-differences and event study designs. For example, we may seek to understand how some resource shock, like the beach closures from our application, impacts a site's mean utility. We could model the impact of a resource shock, indicated by D_{jt}, on site j's mean utility using a two-way fixed effects specification:

$$\delta_{jt} = \phi_j + \xi_t + \beta D_{jt} + \nu_{jt}, \tag{2}$$

where ϕ_j is a site fixed effect, ξ_t is a choice occasion fixed effect, D_{jt} equals one if site j is impacted at choice occasion t and zero otherwise, and ν_{jt} is an error term. The coefficient β captures the average effect of the resource change on the impacted sites' mean utilities, an average treatment effect on the treated sites.[1]

We can then define counterfactual mean utilities, or the mean utilities that each site would have provided if there were no resource shock

$$\delta_{jt}^0 = \begin{cases} \delta_{jt} - \beta & \text{if } j \in \mathcal{D} \ \& \ t \geq \tau \\ \delta_{jt} & \text{otherwise} \end{cases} \tag{3}$$

where $\mathcal{D}$ denotes the set of impacted sites and τ denotes the first post-shock choice occasion.

In order to make the model estimable and to conduct welfare analysis, we make an assumption regarding the distribution of the idiosyncratic error term. In particular, we assume the error term, ϵ_{ijt}, follows a Generalized Extreme Value distribution that implies a two-nest structure, where one nest contains only the outside option and the second nest contains all the recreation sites included in the choice set. This assumption yields the closed-form choice probabilities:

[1] For simplicity, we abstract from concerns regarding staggered treatment timing and treatment effect heterogeneity. In practice, researchers can specify mean utilities to leverage any causal inference technique appropriate for their empirical setting. Our two-way fixed effects model is simply one potential mean utility decomposition.

$$P_{ijt} = \begin{cases} \dfrac{\exp(V_{i0t})}{\exp(V_{i0t}) + \left[\sum_{j=1}^{J} \exp\left(\frac{V_{ijt}}{\lambda}\right)\right]^{\lambda}}, & \text{if } \; j = 0 \\ \dfrac{\exp\left(\frac{V_{ijt}}{\lambda}\right)}{\sum_{j=1}^{J} \exp\left(\frac{V_{ijt}}{\lambda}\right)} \dfrac{\left[\sum_{j=1}^{J} \exp(\frac{V_{ijt}}{\lambda})\right]^{\lambda}}{\exp(V_{i0t}) + \left[\sum_{j=1}^{J} \exp\left(\frac{V_{ijt}}{\lambda}\right)\right]^{\lambda}}, & \text{if } \; j \in \{1, ..., J\} \end{cases} \tag{4}$$

where $V_{ijt} \equiv \delta_{jt} + \alpha^{TC} TC_{ij} + \alpha^{X} X_{ijt}$ is the deterministic portion of the utility from equation 1. The dissimilarity coefficient, λ, captures the similarity of alternatives in the "visit" nest. The model is consistent with utility maximizing behavior when the dissimilarity coefficient lies between 0 and 1, and values closer to 1 indicate alternatives in the "visit" nest are more dissimilar (McFadden, 1978).

We calculate the welfare impact of the resource change using the log-sum formula for compensating variation—the monetary compensation needed to equate an individual's expected maximum utility under observed and counterfactual resource conditions. The compensating variation is given by

$$CV_{it} = -\frac{1}{\beta^{TC}} \left[\ln \left(\exp(V_{i0}) + \left[\sum_{J=1}^{J} \exp(V_{ijt}^{1}/\lambda) \right]^{\lambda} \right) - \ln \left(\exp(V_{i0}) + \left[\sum_{j=1}^{J} \exp(V_{ijt}^{0}/\lambda) \right]^{\lambda} \right) \right] \tag{5}$$

where V_{ijt}^{1} represents the deterministic portion of utility under observed conditions, and V_{jt}^{0} represents the deterministic portion of utility under counterfactual conditions:

$$V_{jt}^{0} = \delta_{jt}^{0} + \alpha^{TC} TC_{ij}. \tag{6}$$

Note that our model, like all standard repeated RUMs, assumes that the Generalized Extreme Value error terms are independent across individuals and choice occasions. This simplification abstracts from several plausible features of recreational behavior. For instance, it assumes that recreators do not have consistently high (or low) unobservable affinity for certain sites and that a recreator's unobservable affinity for a site yesterday is uncorrelated with their unobservable affinity for the site today. The precise temporal variation captured by many emerging datasets makes relaxing these assumptions and estimating dynamic models increasingly feasible. Yet, fully dynamic models are rare in the recreation demand literature (see Provencher and Bishop, 1997, for one exception), and developing and estimating a fully dynamic model is beyond the scope of this paper.

Nonetheless, our method can be adapted to incorporate some aspects of dynamic behavior. Swait et al. (2004) describe discrete choice models capturing temporal dependence as a continuum. Models assuming independent choices over time and across individuals are at one extreme and fully dynamic models are at the other. Swait et al. and Moeltner and Englin (2004) both adopt "middle ground" approaches. They allow individuals' past decisions to affect their current choice, while maintaining the assumption that error terms are independent across individuals and choice occasions. Our model can be adapted to incorporate similar elements of dynamic behavior. Past decisions can be included as covariates in X_{ijt}, and the mean utility provided by a site can be written as a function of past conditions.

3 Estimation

We estimate the model in two stages, similar to Murdock (2006). To begin, we estimate the parameters in equation 1 using maximum likelihood estimation. The log-likelihood of observing individual i's visitation history is

$$\mathcal{L}_i\left(\delta, \alpha, \lambda\right) = \sum_{t=1}^{T} \sum_{j=0}^{J} y_{ijt} \ln\left(P_{ijt}\right), \tag{7}$$

where P_{ijt} is defined by equation 4 and y_{ijt} equals 1 if individual i chooses alternative j at occasion t and 0 otherwise. Summing $\mathcal{L}_i$ across individuals yields the objective function for the optimization routine.

Rather than estimate the mean utilities directly, we recommend applying the Berry (1994) contraction mapping to solve for the unique mean utilities that match the daily visitation shares predicted by the model to the daily visitation shares observed in the data. Estimation with the contraction mapping produces same estimates as maximum likelihood estimation with respect to δ, α, and λ, but it allows the optimization routine to search along fewer dimensions. Even with a relatively small choice set, the model in our application contains over 700 mean utility parameters.

Therefore, we suspect the contraction mapping substantially reduces the computational burden of the maximum likelihood estimation. After the first stage, we possess estimates of the α coefficients from equation 1, the dissimilarity coefficient, λ, and the mean utilities, δ.

The estimated mean utilities capture all observable and unobservable site attributes, which means they prevent unobservable site attributes from biasing the other first stage coefficient estimates. Applying the contraction mapping to estimate mean utilities requires normalizing the mean utility provided by the outside option to equal zero on each date. Thus, raw mean utility estimates should be interpreted relative to that day's outside option. On days when visiting recreation sites becomes more appealing relative to the outside option, such as weekends or holidays, mean utilities will increase. Note that fluctuations in the quality of the outside option can be controlled for by including a site-by-date fixed effect in the mean utility decomposition, so these fluctuations are generally not an identification concern.

In the second stage, we estimate counterfactual mean utilities. As mentioned in section 2, researchers can apply any panel data econometric technique to achieve this goal, including difference-in-differences, event study designs, or the synthetic control method. Continuing with the difference-in-differences example from equation 2, we would estimate mean utilities simply by regressing the first stage mean utility estimates on site indicator variables, choice occasion indicator variables, and a treatment indicator variable. The identification assumptions required to consistently estimate second-stage parameters are analogous to those required in reduced-form settings. Difference-in-differences and event study designs require some form of the parallel trends assumption, and just like in reduced-form settings, researchers can check the plausibility of a parallel trends assumption by checking for pre-trends.

Identification also requires a stable unit treatment value assumption (SUTVA), which can be violated if the treatement spills over and impacts control sites. To understand SUTVA in the context of our second-stage regression, it is helpful to note that our second-stage regression differs a reduced-form regression with visitation as the outcome. In such a reduced-form regression, SUTVA is likely to be violated, because a resource change at one site can cause inter-site substitution, creating visitation spillovers at control sites. By embedding causal inference techniques within a RUM, we explicitly model inter-site substitution, which mitigates this concern. Mean utilities, not visitation, are the outcome variable in the second stage. Thus, our version of SUTVA requires that a resource change at one site does not impact mean utilities at other sites. This version of SUTVA is more likely to hold than the reduced-form equivalent, because mean utilities at control sites are determined by their own attributes, rather than attributes at other sites. Nonetheless, SUTVA should not be ignored completely. For example, it is possible that a resource change at one site could affect congestion at control sites or that a resource change may lead visitors to visit new sites, learn, and update their preferences. The importance of these concerns will depend on the empirical setting.

4 Application

4.1 Background & Data

We demonstrate our method by estimating the recreational welfare losses caused by beach closures at Lake St. Clair Metropark during the summer of 2022. Lake St. Clair Metropark lies on the shores of Lake St. Clair, about 20 miles northeast of Detroit. It is part of the Huron-Clinton Metroparks system, a collection of thirteen parks throughout southeast Michigan. Like many of the metroparks, Lake St. Clair features a variety of recreational amenities, including walking trails, kayak rentals, a swimming pool, and a mini-golf course. If the swimming beach is closed, visitors are free to enjoy these other amenities.

Our recreation data are administrative visitation records collected by Huron-Clinton Metroparks staff at park entrance booths. The data log the minute-of-entry for over 1 million visits between May 15 and October 15, 2022. They also contain each visitor's residential ZIP code, allowing us to estimate a travel cost model. To enter a park, visitors must display an annual pass ($40) or purchase a daily pass ($10). When visitors purchase a daily pass, the staff ask for their home ZIP code and the payment system records the time of entry. The annual pass is a windshield sticker. Park staff scan a unique barcode on the sticker, which documents both the time of entry and the pass identification number. Thus, we observe a detailed visitation history for annual passholders—i.e., trip diary-style data.

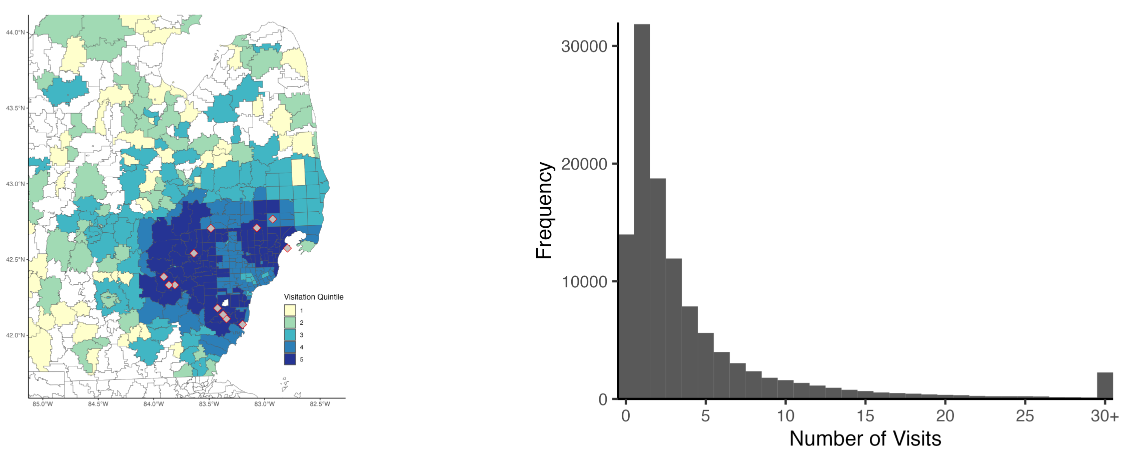
We restrict our focus to annual passholders and drop all day pass visits simplify the exposition of our method. This restriction preserves around 740,000 visits made by 115,000 annual passholders (about 70 percent of all visits). The accuracy of these visitation records depends on reliable entry booth staffing. To check this reliability, we identify all "reported zeros"—days where we do not observe any visitors to a park. These days most likely result from a lack of staffing rather than zero attendance. Appendix figure A.1 shows that three parks regularly report zero visitation and that reported zeros become more common from late-September through mid-October. Therefore, we focus our analysis from May 15 to September 19, 2022. Additionally, we keep the six parks with no reported zeros during this time period as distinct alternatives the choice set, and we group the other seven parks with the outside option.

The metroparks are popular recreation sites in southeast Michigan. Stony Creek, Kensington, and Lake St. Clair Metroparks each received more than 100,000 visits from annual passholders between May 15 and October 15, 2022 (table A.1). Figure 1a shows metropark locations, as well as the frequency of visitation by ZIP code. ZIP codes with the highest visitation tend to be near the metroparks, often in the Detroit suburbs. Figure 1b shows how many times annual passholders visited any of the six parks in our choice set. The distribution of these visitation frequencies is skewed to the right. Passholders visited these parks 4.5 times on average, and 90 percent of passholders visited fewer than ten times. A subset of passholders visited much more frequently; 2,251 passholders (2 percent) made at least 30 visits.

Figure 1: Visitation frequencies by ZIP code and passholder

(a) Visitation by ZIP Code

(b) Number of visits by annual passholder



Panel (a) shows Metroparks (gray-diamonds) and visitation by ZIP code (darker shading means higher visitation). High visitation ZIP codes tend to be nearby Metroparks, often in the Detroit suburbs. Panel (b) shows how many times annual passholders visited one of the six parks in our choice set. The mass of annual passholders with zero visits visited at least one of the parks we omit from the choice set.

One weakness of this dataset is that the visitation records do not contain any demographic information. In the absence of individual-level demographic variables, we assign each passholder the demographic information associated with their home ZIP code. Table 1 contains a full list of demographic variables.

Table 1 also shows statistics describing passholders' travel costs. Our travel cost calculations follow standard practices in the recreation demand literature. Specifically, we calculate passholder i's round-trip travel costs of accessing park j as

$$TC_{ij} = 2 \cdot \left(p^M \ Miles_{ij} + p_{it}^T \ Time_{ij}\right) \tag{8}$$

where $Miles$ represents the one-way driving distance in miles and $Time$ represents the one-way driving time in hours. The p^M term represents the per-mile, "out-of-pocket" cost of driving, which includes fuel, maintenance, and depreciation costs. The p_{it}^T term represents the opportunity cost of time. We calculate driving mileages and times from residential ZIP codes to metroparks using the Open Source Routing Machine, and we calculate a per-mile, "out-of-pocket" driving cost of 34.8 cents per mile using the 2022 AAA Your Driving Costs report. It is standard practice to assume an individual's opportunity cost of time is 1/3 of their hourly wage rate, so we assume a passholder's opportunity cost of time equals 1/3 of the median hourly wage rate in the their residential ZIP code. We calculate each ZIP code's median hourly wage rate by dividing its annual median household income by 2080 (= 52 weeks $\times$ 40 hours per week).

We complement these visitation data with water quality test results and beach closure records from the Michigan Department of Environment, Great Lakes, and Energy's (EGLE) BeachGuard monitoring program. EGLE coordinates water quality tests that track *E. coli* bacteria levels at 450 swimming beaches throughout Michigan. State law sets one-day and 30-day mean standards for bacteria levels, and a beach is closed to swimming if bacteria levels exceed either the one-day or 30-day mean threshold. County health officials test the water at Lake St. Clair on Mondays and Wednesdays from mid-April through the end of September. If bacteria levels exceed the daily threshold, the water is retested the following day. If bacteria levels still exceed the threshold, testing returns to its normal schedule. In the summer of 2022, elevated bacteria levels closed the beach for 33 days in all, and the first closure occurred July 21.

Table 1: Descriptive Statistics

Variable	Mean	Std dev	Min	Max
Travel variables (683,820 passholder-park pairs)				
Driving distance (miles, one-way)	38.1	18.7	0.8	182.3
Driving time (hours, one-way)	0.87	0.38	0.04	3.85
Travel cost ($s, round-trip)	50.8	25.2	1.7	269.9
Demographic variables (113,970 passholders)				
Median household income	85,758	24,390	21,180	164,250
$50k < median household income < $75k	0.329	0.470	0	1
$75k < median household income < $90k	0.210	0.407	0	1
Median household income > $90k	0.418	0.493	0	1
Share Black	0.084	0.124	0	0.957
Share Hispanic	0.036	0.033	0	0.710
Share other non-white race	0.099	0.071	0	0.506
Share with bachelor's degree	0.227	0.077	0.023	0.417
Share with graduate degree	0.158	0.100	0	0.531
Share with child	0.400	0.053	0	0.621

Note: Descriptive statistics for travel variables exclude the outside option. All demographic variables are ZIP code-level statistics from the 2021 American Community Survey 5-year data. We assign each individual the demographic information associated with their home ZIP code. We split race into three categories, white, Black, and other, and we classify ethnicity as either Hispanic or non-Hispanic. Share with bachelor's degree represents the share of the population whose highest degree is a bachelor's degree.

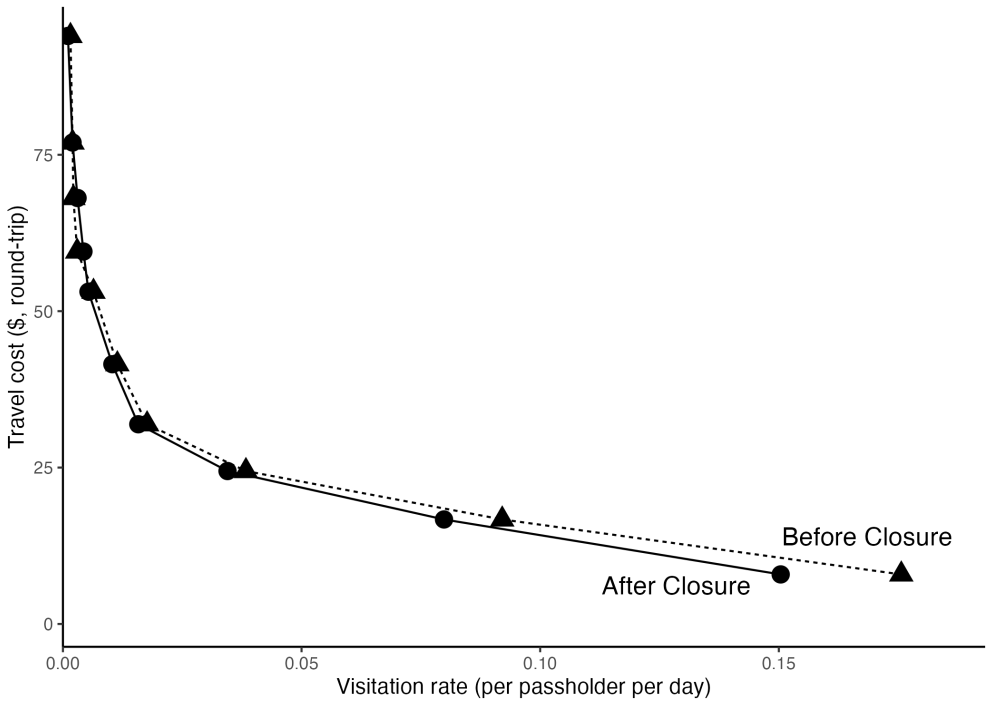
Potential visitors may become aware of beach closures through several channels. EGLE posts water quality test results, beach closures, and advisories for the general public on the BeachGuard website, and state and local news outlets regularly publish beach closure lists and updates. Closures are not rare, 23 percent of monitored beaches experienced a closure in 2018 (EGLE 2019), so many visitors are aware of a beach's status before taking their trip.

The raw visitation data reveal some evidence of the Lake St. Clair beach closures (figure 2). Visitation rates from low travel cost deciles are slightly lower the two weekends just after the first closure than they are the two weekends just before the first closure. In the lowest travel cost decile, the difference in visitation rates amounts to about 75 fewer visits each day. There is little discernible difference between pre- and post-closure visitation rates at high travel cost deciles, at least in part because Lake St. Clair receives fewer visitors from these distant locations.

4.2 Applying the method

We model demand for the metroparks using the repeated RUM with daily choice occasions presented in section 2. We apply our estimation procedure by first estimating daily mean utilities for each of

Figure 2: Lake St. Clair visitation rates by travel cost decile



Note: Figure 2 shows visitation rates on the two weekends immediately before and the two weekends immediately after the first beach closure. Each point represents the visitation rate (number of visits to Lake St. Clair per passholder per day) for a different travel cost decile. The vertical axis coordinate is the average travel cost for each decile.

the six parks in our choice set, then using the synthetic control method to estimate the causal effect of the beach closures on Lake St. Clair Metropark's mean utilities. The synthetic control method is frequently applied in settings with a small number of individuals, units, or, in this case, parks, and with only one treated unit, making it well-suited for our setting (Abadie & Gardeazabal, 2003; Abadie, 2021).

To apply the synthetic control method, we construct synthetic mean utilities for Lake St. Clair Metropark as a weighted average of other parks' mean utilities. We choose the weights minimize the error between the synthetic mean utilities and the observed mean utilities prior to the first beach closure. More formally, we select the weights by solving

$$\begin{aligned} &\arg\min_{w} \sum_{t<\tau} \left(\hat{\delta}_{1t} - \sum_{j=2}^{J} w_j \hat{\delta}_{jt} \right)^2 \\ &\text{subject to} \quad \sum_{j=2}^{J} w_j = 1 \end{aligned}$$

where Lake St. Clair Metropark is indexed by $j = 1$ and τ represents the date of the first beach closure. We assume these synthetic mean utilities represent Lake St. Clair's counterfactual mean utilities, and we calculate the aggregate welfare loss by summing equation 5 across all passholders and all dates after the first closure.

4.3 Results

Table 2 reports first stage parameter estimates for all variables except the panel of mean utilities, which we omit for brevity. The travel cost coefficient is negative and statistically significant, as expected. The dissimilarity coefficient is between zero and one, implying the model is consistent with utility-maximizing behavior. We interact several demographic variables with the outside option, and the coefficient estimates on these interaction terms produce multiple findings. First, the income coefficients indicate that passholders from high income ZIP codes are more likely to visit a metropark than passholders from lower income ZIP codes, all else equal. Similarly, the race coefficients indicate that passholders from ZIP codes with a higher share of Black and other non-white residents are more likely to visit metroparks than passholders from ZIP codes with a higher share of white residents. On the other hand, passholders from ZIP codes with higher shares of Hispanic residents and higher shares of residents with bachelor's degrees, graduate degrees, and children are more likely to choose the outside option. These coefficients suggest the metroparks may be attractive outdoor recreation sites to underrepresented racial minorities and less educated people, which is potentially noteworthy for research focused on increasing outdoor recreation participation rates from these groups. However, we recommend interpreting these demographic coefficients with caution, because we lack individual-level demographic data and focus only on annual passholders.

Figure 3 compares Lake St. Clair's observed and synthetic mean utilities. Peaks tend to correspond to weekends and holidays, where visiting a park is more appealing relative to the outside

Table 2: First Stage Estimates

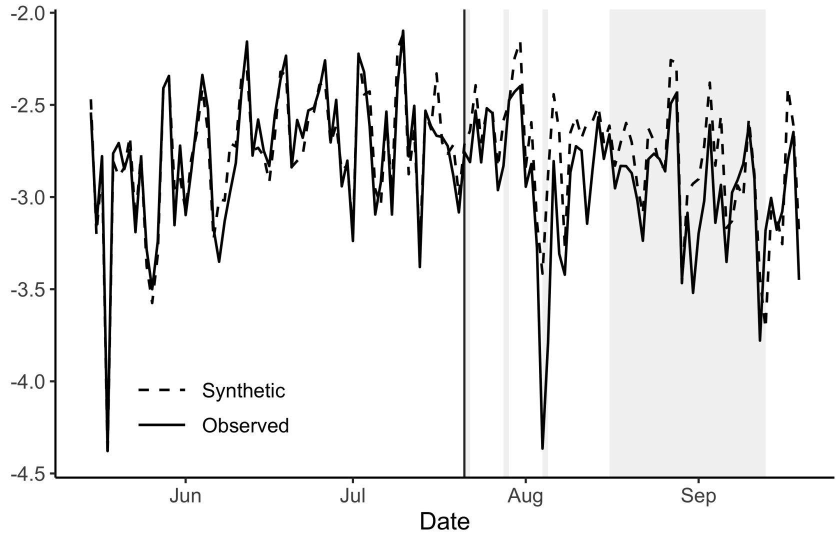
	Estimate	Standard Error
Travel cost ($10s)	-0.4406	0.0118
Dissimilarity coefficient	0.4373	0.0110
Interacted with outside option		
$50k < median household income < $75k	-0.3123	0.0029
$75k < median household income < $90k	-0.3460	0.0024
Median household income > $90k	-0.6031	0.0030
Share Black	-0.0699	0.0007
Share other non-white race	-0.2717	0.0004
Share Hispanic	0.2877	0.0002
Share with bachelor's degree	1.1612	0.0004
Share with graduate degree	0.5516	0.0005
Share with child	0.4444	0.0003
Log-likelihood	-2,492,800.4	

Note: The table shows parameter estimates and standard errors from the first-stage, maximum-likelihood estimation. The omitted race category is white, and the omitted ethnicity category is non-Hispanic. Education variables represent the share of ZIP code residents whose highest educational attainment is a bachelor's degree or a graduate degree. The omitted education category is "less than a bachelor's degree".

option. The vertical line marks July 21, 2022, the date of the year's first beach closure. Synthetic mean utilities match observed mean utilities closely before the first closure, suggesting that synthetic Lake St. Clair provides reasonable estimates of counterfactual mean utilities. After the first closure, observed mean utilities are consistently lower than the synthetic mean utilities. Figure 4 shows this more clearly by plotting the gap between the observed and synthetic mean utilities. This gap represents the causal effect of the beach closures on Lake St. Clair's mean utilities.

The beach closures decrease Lake St. Clair's mean utility on many days after the first closure, not only on days when the beach is closed. This persistent decrease is particularly apparent after the August 4th beach closure. The mean utility provided by Lake St. Clair drops sharply during the closure and takes several weeks to return to the counterfactual level. This delayed recovery suggests that the beach closures cause a stigma effect. There could be several factors contributing to the stigma. It is possible that some recreators are not be aware that the beach has reopened, in which case more complete information on beach reopenings might help limit welfare losses. On the other hand, even recreators that know the beach has reopened may prefer to avoid a site that recently experienced elevated *E. coli* levels. Indeed, Boudreaux et al. (2023) find that recreators are hesitant to return to beaches even when they know the water is swimmable again. Using a discrete

Figure 3: Observed and Sythetic Mean Utilities

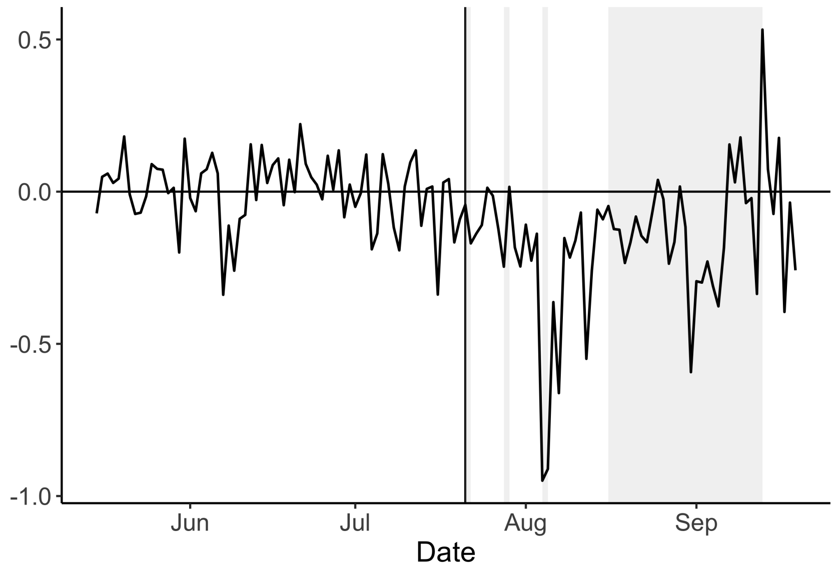


Note: The figure shows observed (solid) and synthetic (dashed) mean utilities for Lake St. Clair Metropark. The synthetic mean utilities represent the mean utilities that Lake St. Clair would have provided in absence of the beach closures. Grey shading indicates dates when Lake St. Clair experienced a beach closure.

choice experiment, they estimate that recreators are willing to drive 77 miles, on average, to avoid a site that experienced elevated bacteria levels six days ago. Given their result, it seems likely that the persistent decline in mean utilities is not completely driven by imperfect information, but rather that imperfect information could exacerbate stigma effects. Investigating how water quality testing and communication procedures can help mitigate stigma effects may be a fruitful path for future research.

Following standard practice, we execute a placebo test to gauge the significance of the drop in Lake St. Clair's mean utilities. We estimate the impact of the beach closure on each control park's mean utilities, repeatedly creating synthetic control parks and plotting the gap between each control park's synthetic and observed mean utilities. Figure 5 displays the gaps for control parks in grey and the gap for Lake St. Clair (the same as in figure 4) in black. Although several control parks have short spells with mean utility gaps similar to Lake St. Clair's, no park exhibits such a sustained change in mean utilities. We confirm this visual observation by computing the root-mean-squared error (RMSE) between observed and synthetic mean utilities for each park before and after the first closure. Lake St. Clair's ratio of post-closure RMSE to pre-closure RMSE is

Figure 4: The difference between observed and synthetic mean utilities



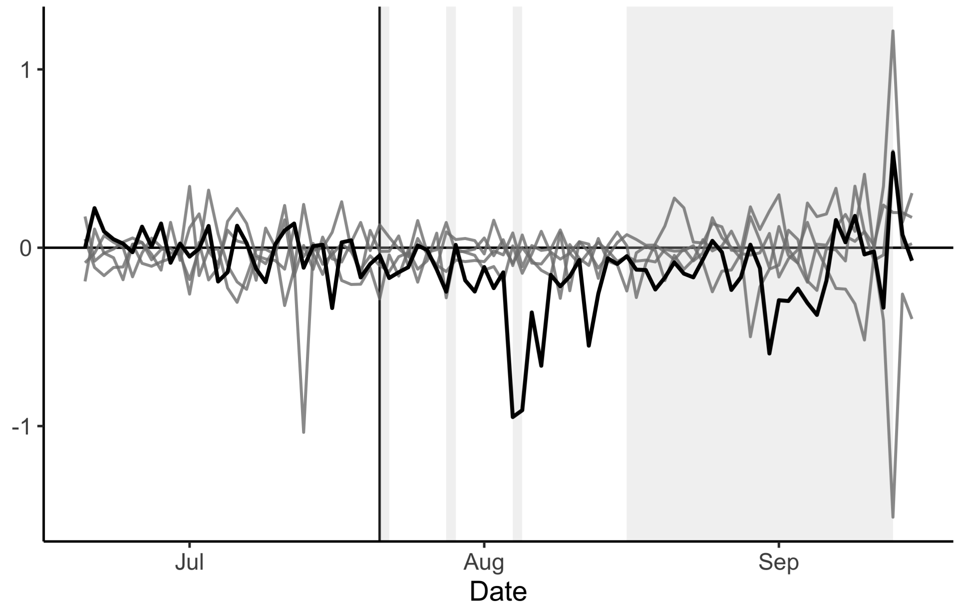
Note: The figure shows the differences between the observed and synthetic mean utility provided by Lake St. Clair Metropark. Grey shading indicates dates when Lake St. Clair experienced a beach closure.

2.45, while control parks' RMSE ratios range between 0.71 and 1.96. These ratios suggest that the decrease in mean utilities at Lake St. Clair is more dramatic than the change in mean utilities at any other park, and thus, our results pass this placebo test. Abadie (2021) provides a test of statistical significance based on these RMSE ratios. With only five control parks, we cannot obtain a statistical significant p-value according to any common threshold, but we do obtain the smallest possible p-value for our empirical context, 1/6 or 0.17.

Figure 5 also helps to assess potential SUTVA violations. As discussed in section 3, spillovers that impact control sites' mean utilities may violate SUTVA and pose a threat to identification. If control parks' mean utilities were contaminated by spillover effects of the beach closure, then the park(s) experiencing the greatest spillovers would likely appear treated in figure 5. Yet, no park exhibits a consistent change in its mean utility after the beach closure other than Lake St. Clair. While this is not conclusive evidence that SUTVA holds, it does provide additional support for our identification strategy.

Lake St. Clair's decreased mean utilities translate to recreational welfare losses. Our two-stage synthetic controls approach estimates the aggregate welfare loss (summed across all days after the initial closure and all annual passholders) at $152,500 and the loss per lost trip at $14.40. Dividing

Figure 5: Placebo test



Note: The figure shows the gaps between synthetic and observed mean utilities for Lake St. Clair (black) and each control park (grey). Because the mean utilities provided by the control parks are not impacted by the beach closure at Lake St. Clair, the gaps between synthetic and observed mean utilities for control parks serve as placebo tests. It is reassuring that no other parks' mean utilities appear impacted by the closure at Lake St. Clair. We remove the gap between synthetic and observed mean utilities for Stony Creek Metropark, because its mean utility is consistently higher than all other parks. Therefore, Stony Creek's mean utilities are poorly predicted by a convex combination of other parks', and it has a pre-closure root-mean-squared error four times larger than Lake St. Clair's and more than twice as large as any other park.

the aggregate loss by the number of days when the beach is closed, 33, yields a welfare loss per closure day of $4,600. We compare these estimates to those produced by several alternative models. The first uses our two-stage estimation approach but estimates counterfactual mean utilities using an event study design with weekly coefficients. Figure A.2 shows the coefficient plot from this second-stage regression, which reveals negative beach closure impacts consistent with our synthetic controls results. The two-stage event study approach produces an aggregate welfare loss estimate of $144,700, about 5 percent smaller than the two-stage synthetic control estimate.

We also estimate the welfare impact of the closures using two more traditional, "baseline" models. These baseline models include a travel cost variable, a cross-section of park mean utilities, and an indicator variable equal to one when Lake St. Clair is impacted by a beach closure (the indicator variable equals zero at all dates for other parks). We use two definitions to indicate when

Table 3: Welfare Losses by Model

	Baseline 1	Baseline 2	Synthetic Control	Event Study
Aggregate loss ($1,000s)	48.8	139.7	152.5	144.7
Loss per closure day ($)	1,479	4,234	4,622	4,386
Loss per day after initial closure ($)	800	2,291	2,500	2,373
Loss per lost trip ($)	14.82	14.91	14.40	14.69

Note: The table shows estimates of the welfare loss caused by the 2022 Lake St. Clair Metropark beach closures for different models. The "Aggregate Loss" row shows estimates of the total welfare loss. "Loss per closure day" divides the aggregate loss by the number of days where the beach was closed, 33. "Loss per day after initial closure" divides the aggregate loss by the number of days after the first beach closure. "Loss per lost trip" divides the aggregate loss by the number of lost trips to Lake St. Clair Metropark, which varies by model.

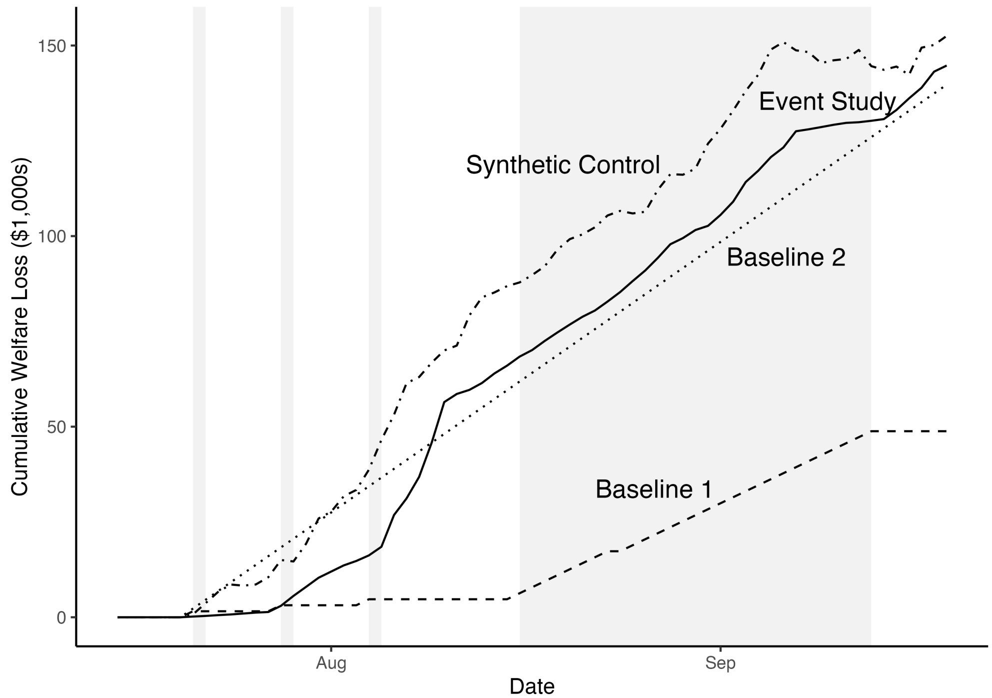
Lake St. Clair is "impacted by a beach closure". The first, labeled "Baseline 1," sets the indicator variable equal to one only on days when the beach is closed. The second, labeled "Baseline 2," sets the indicator variable equal to one on all days after the first closure. Appendix B describes the baseline models in more detail.

Welfare loss estimates from the baseline models are sensitive to how we define the beach closure indicator variable. The aggregate welfare loss estimate from the Baseline 1 model is $48,800, while the aggregate welfare loss estimate from the Baseline 2 model is $139,700, nearly three times larger. Again, one benefit of our two-stage approach it only requires us to specify the initial closure date. We allow the data to reveal when Lake St. Clair is impacted.

The aggregate welfare loss estimate from the Baseline 2 model is only 9 percent smaller than the two-stage synthetic control estimate. The fact that these approaches produce similar estimates is somewhat surprising, given that the Baseline 2 model averages over temporal variation in the mean utilities and the magnitude of beach closure impacts. Although this is just one comparison, the similarity of the baseline and two-stage estimates provides some degree of convergent validity for previous studies using the baseline model, at least when the timing of welfare impacts has been properly defined.

While total welfare loss estimates from the Baseline 2 and two-stage panel models are similar, the two-stage panel approach uncovers rich temporal variation in welfare impacts. This variation may be of interest to park managers and policy-makers, seeking to understand how a site's recreational value varies over time. For example, managers may find it useful to know the benefits of rushing to complete a project before a holiday weekend. Figure 6 shows that welfare losses grow quickly in late

Figure 6: Cumulative welfare loss across different model specifications



Note: The figure shows the cumulative welfare losses caused by the Lake St. Clair Metropark beach closures estimated by several different models. "Synthetic controls" (dot-dash) represents our preferred two-stage synthetic controls estimates. "Event Study" (solid) represents the two-stage event study estimates. "Baseline 1" (dashed) represents the traditional approach that assumes Lake St. Clair is impacted only when its beach is actually closed, and "Baseline 2" represents the traditional approach that assumes Lake St. Clair is impacted every day after the first closure. Grey shading indicates dates when Lake St. Clair experienced a beach closure.

July and early August, corresponding to the time when the beach closures cause the largest decrease in mean utilities. Losses continue accumulating steadily until around September 1, they then grow more quickly and finally flatten off. Note that the baseline models capture no heterogeneity in the timing of welfare impacts.

To gauge when welfare losses are largest, we regress the daily welfare loss estimates on the temperature, a rainy day indicator, and a weekend/holiday indicator (table 4). This regression is descriptive; we are no longer estimating causal effects or structural parameters. Welfare losses tend to increase with temperature. Daily welfare losses are around $1,000 larger when the temperature increases ten degrees, holding precipitation and the weekend indicator fixed. The sign of the rainy day coefficient depends on the second-stage model. Welfare losses from the two-stage event study model tend to be lower on rainy days, matching our prior expectation, but welfare losses from

Table 4: Regressing welfare losses on weather and a weekend dummy variable

	Synthetic Control	Event Study
Constant	1,698.0***	2,440.4***
	(429.2)	(337.0)
Daily max temperature (F)	115.6	99.2
	(80.7)	(63.3)
Rainy day	527.7	-1,447.1*
	(926.4)	(727.4)
Weekend or holiday	2,297.9***	544.8
	(708.9)	(556.6)
Observations	61	61
Adjusted R^2	0.139	0.051

Note: The table shows estimates from regressions of daily welfare losses on a weekend/holiday dummy variable and weather variables. The first column uses welfare loss estimates produced by applying the synthetic control method in the second stage, and the second column uses welfare loss estimates produced by applying an event study with weekly coefficients in the second stage. "Daily max temperature (F)" is the demeaned daily maximum temperature in degrees Fahrenheit. "Rainy Day" equals one if there was greater than 0.1 inches of rain and zero otherwise. "Weekend or holiday" equals one on weekends and holidays (e.g., July 4th, Labor Day) and zero otherwise.

the two-stage synthetic control model are actually larger on rainy days. Although, the coefficient estimate has a large standard error. The uncertainty in this estimate could be caused by our simplistic rainy day indicator variable, which does not capture variation in the time of day, duration, or intensity of rain showers. Finally, both models find that welfare losses are larger on the weekends and holidays than on weekdays, holding weather conditions fixed. In sum, these results are intuitive. A swimming beach likely provides more surplus for more recreators on a warm, sunny Saturday than a cool Tuesday, for example, and so it is reasonable that the beach closures cause larger welfare losses on warmer days and weekends.

5 Conclusion

Emerging recreation datasets provide rich temporal variation that has not been fully leveraged in recreation demand analyses. Our proposed method exploits this temporal variation to embed popular causal inference techniques, such as difference-in-differences or synthetic controls, within travel cost RUMs. These techniques have improved the credibility and reliability of empirical analyses in many economic subfields. It seems likely that they can improve the reliability of valuation estimates produced by recreation demand analyses.

We demonstrate how to apply our method by valuing the recreational welfare losses caused by beach closures at Lake St. Clair Metropark. Our method produces similar aggregate welfare loss estimates to a more traditional approach, but it requires fewer assumptions on the duration of stigma effects. It also reveals temporal heterogeneity in the magnitude of welfare impacts, and we find that welfare losses are larger on weekends and hotter days. This application provides a blueprint for valuing resource shocks and non-marginal amenity changes more broadly. Given the growing availability of high-frequency recreation data, our method may be useful in many empirical settings.

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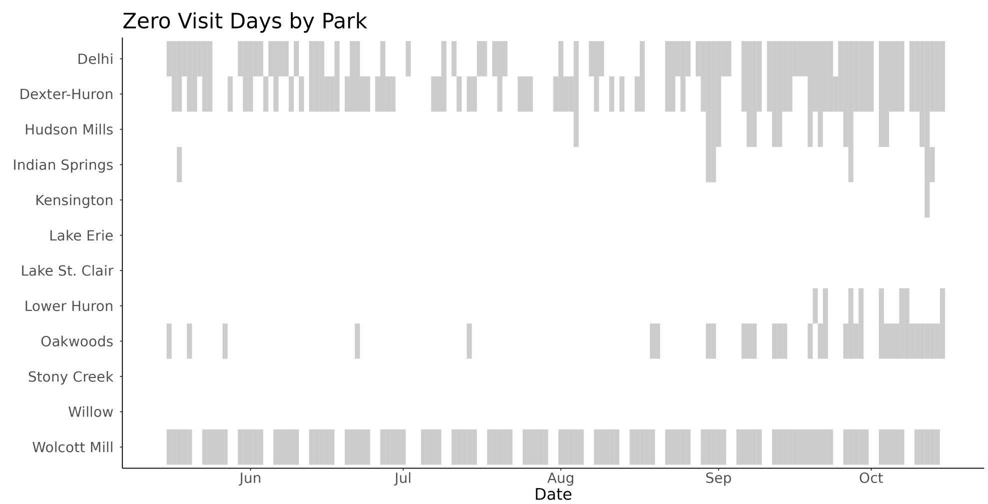
A Supplemental Figures and Tables

Table A.1: Visitation by Metropark 2022

Metropark	Visitation	Beach	Kayak Rental	Pool or Splash Pad	Hiking
Stony Creek	201,882	Y	Y		Y
Kensington	197,122	Y	Y	Y	
Lake St. Clair	117,376	Y	Y	Y	
Lower Huron	56,678		Y	Y	Y
Willow	52,218			Y	Y
Lake Erie	37,722			Y	Y
Hudson Mills	35,629		Y	Y	Y
Indian Springs	27,280			Y	Y
Oakwoods	5,175		Y		Y
Dexter-Huron	4,411		Y		Y
Delhi	1,880		Y		
Wolcott Mill	785				Y

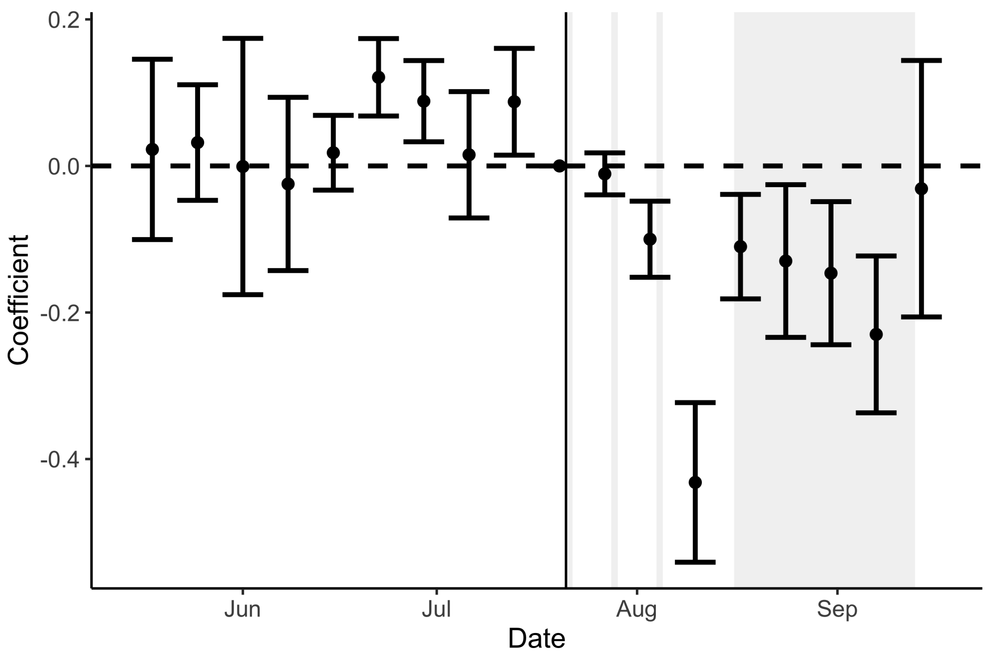
Note: The table shows total visitation by annual passholders between May 15 and October 15, 2022 and whether each park has a few of the common amenities. We do observe visitation records for Huron Meadows Metropark, and we exclude it from the table.

Figure A.1: Zero visit days



Note: The figure shows all zero visit days shaded in grey – i.e., days where a park recorded no visitors. This indicates days when entry booths were not staffed, rather than days when parks truly received zero visits. We group parks with zero visit days during our sample period (May 15 through September 19) with the outside option. We include Kensington, Lake Erie, Lake St. Clair, Lower Huron, Stony Creek, and Willow Metroparks, which all report no zero visit days in our sample period, as distinct alternatives in the choice set.

Figure A.2: Second stage event study coefficient plot



Note: The figure shows coefficient estimates from a second stage event study regression, where mean utilities are the outcome variable. A separate coefficient is estimated for each week relative to the initial beach closure. Grey shading indicates dates when Lake St. Clair experienced a beach closure.

Table A.2: Weights for Synthetic Lake St. Clair Metropark

Park	Weight
Kensington Metropark	0.003
Lake Erie Metropark	0.040
Lower Huron Metropark	0.462
Stony Creek Metropark	0.475
Willow Metropark	0.020

B Baseline Model

We compare welfare estimates produced by our model to those produced by a more traditional, "baseline" approach. This baseline approach largely follows English et al. (2018), who value the welfare impacts of the *Deepwater Horizon* oil spill. This appendix provides more details on the model and estimation procedure that produce our baseline estimates.

Assume the choice set and choice occasions are the same as in our primary model, but specify the utility individual i receives from visiting site j at choice occasion t as

$$U_{ijt} = \delta_j + \beta Closed_{jt} + \alpha^{TC} TC_{ij} + \alpha^{X} X_{ijt} + \epsilon_{ijt} \tag{9}$$

There are two differences between this specification and our preferred specification (equation 1). First, this model includes a cross-section of alternative-specific mean utilities, δ_j, rather than a panel. Second, this model includes the *Closed* indicator variable, which equals to one on days when site j is impacted by a beach closure. All other parameters and variables are defined as in equation 1.

We estimate all parameters in the baseline model using maximum likelihood estimation. Because the beach closure coefficient can be identified separately from site mean utility, there is no need to apply the two-stage estimation procedure we use for our primary model. Furthermore, given the relatively small choice set in our empirical setting, it is computationally efficient to estimate the cross-section of mean utilities directly, rather than apply a contraction mapping to match predicted and observed visitation shares for the entire sample period.

Ex ante, it is unclear how to define $Closed_{jt}$ for Lake St. Clair. If there is a stigma effect, then Lake St. Clair could be impacted even on days when its beach is open. However, it may be difficult to detect this stigma effect before estimating the model. Therefore, we choose two ways of defining the $Closed_{jt}$ variable. First, we set $Closed_{jt}$ equal to one only on days when the beach at Lake St. Clair is closed. We call the results produced by this specification the "Baseline 1" estimates. Second, we set $Closed_{jt}$ equal to one every day after the initial beach closure. We call the results produced by this specification the "Baseline 2" estimates.