Title: Restoration Economics: Investigating the Effect of AOC Remediation on Residential Housing Prices

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Abstract:

Areas of Concern (AOCs) are geographic areas within the Great Lakes Basin with a historical legacy of environmental degradation of a water body. Thirty-one sites were listed as AOCs in 1987, and only four have been delisted. AOC remediation is the goal of the Great Lakes Legacy Act (2002, hereafter GLLA), and it is a primary goal of the Great Lakes Restoration Initiative (2010, hereafter GLRI). This research studies the housing market impacts of three distinct features of the AOC program: listing, delisting, and remediation grant funding under the GLLA and GLRI. We develop spatially referenced data on housing transactions within 25 kilometers (km) of the AOC boundaries from 1977-2017. We also compile data on grant awards for AOC remediation projects under the GLLA and GLRI from 2004-2017. We develop three main findings. First, housing prices declined over 10% due to AOC listing for parcels within 15 km of the AOC boundary. Second, a similar analysis of delisting does not provide evidence of an effect on housing prices, perhaps due to the small number of parcel observations in the delisting analysis. Third, a dollar increase in cumulative grant dollars to an AOC resulted in an increase in the range of 0.0006-0.0009 dollar in housing prices for parcels within 20 km of the AOC boundary. These estimates translate into price increases at the mean of the data of $1,618 to $2,427 per parcel. These price effects represent increases in the market value of all parcels within 20 km of the AOCs.

Keywords: Area of concern, difference-in-differences estimators, Great Lakes restoration initiative, hedonic price theory, housing market effects

Executive Summary:

Areas of Concern (AOCs) are geographic areas within the Great Lakes Basin with a historical legacy of severe environmental degradation of a water body. Thirty-one sites in the United States were listed officially as AOCs under the binational Great Lakes Water Quality Agreement of 1987. Only four have been delisted. AOC remediation is the goal of the federal Great Lakes Legacy Act (2002, hereafter GLLA), and it is one of the five focal areas of the federal Great Lakes Restoration Initiative (2010, hereafter GLRI). Three AOCs have been delisted in recent years – Presque Isle Bay in Pennsylvania in 2013, White Lake in Michigan in 2014, and Deer Lake in Michigan, also in 2014.

This research studies the housing market impacts of three distinct features of the AOC program: listing, delisting, and remediation grant funding under the GLLA and GLRI. We compile data on housing market transactions within 25 kilometers (km) of the AOC boundaries from 1977-2017. The individual transaction parcels are spatially referenced such that each parcel's distance to the nearest AOC is established. Data from 1977-1997 are applied in the listing analysis. Data from ten years before delisting to 2017 are used in the delisting analysis. Data from 1995-2017 are used in the grants analysis. For the grants analysis, we also compile data on dollars awarded for remediation under GLLA and GLLA by AOC and by year. The data represent 406 projects and $523 million in funds awarded, all through competitive grant processes. We apply econometric methods for estimating causal effects to study the three program impacts.

We find statistically significant and economic meaningful effects of AOC listing in 1987. Here we organized the housing data in distance bins ranging from 0-5 km, 5-10 km, 10-15 km, and 15-20 km from the AOC boundary. In addition, a 0 km bin includes parcels that are inside or on the AOC boundary. Beginning with the 0 km bin, the estimated reductions in sales prices due to listing are: 13.2%, 16.6%, 13%, 10.3%, and 6%, respectively. These impacts are measured relative to any price decrease in the 20-25 km bin, but we developed evidence suggesting that the listing effect went to zero past 20 km. These results are interesting because the underlying environmental quality of AOCs did not change at the time of listing. The negative effects thus reflect dissemination of new public information about the extent of degradation of AOCs along with, potentially, a stigmatizing effect of being identified as one of the 31 AOCs in the U.S. waters of the Great Lakes.

A similar analysis of the delisting of three AOCs does not provide robust evidence of an effect on housing prices. The weak statistical evidence may result from the relatively small number of housing-parcel observations in the analysis, which follows from the fact that the delistings are both few in number and recent in time. An alternative perspective is that one might expect a null effect: while listing was a relatively abrupt event from the public's perspective, remediation and delisting have occurred in stages under the concerted public-information campaigns of the GLRI and GLLA administrative agencies. Thus a possibility is that, prior to delisting, housing prices in areas near AOCs have already rebounded from the sharp price decreases caused by delisting. A richer analysis of delisting awaits a better dataset, which is conditional on both additional AOC delistings and a longer time period post-delisting.

The third component of the research involves analysis of the effect of GLLA and GLRI grant dollars on housing prices. Here we assess the effect of $523 million in grants awarded to 406 AOC projects from 2004-2017. We find that a dollar increase in cumulative grant dollars to an AOC resulted in an increase in the range of 0.0006-0.0009 dollar (0.06-0.09 cents) in housing prices in the 0-20 km distance bin. These estimates translate into price increases at the mean of the data of $1,618 to $2,427 per parcel, i.e., the results are economically meaningful. These results can be applied to the entire housing stock within 20 km of every AOC as a basis to estimate the economic benefits of AOC remediation projects. In other words, the estimated price effects are correct to interpret as increases in the market value of all housing parcels within 20 km.

In conceptual terms, the estimates of the capitalization effects on property values provide lower bounds of the economic benefits of AOC remediation, since the economic benefits that accrue to people living within 20 kilometers of an AOC do not incorporate benefits accruing to those living farther away. These could be individuals who travel to the AOC for recreation or tourism as well as individuals deriving economic value simply from knowing that formerly degraded areas with the Great Lakes basin are being restored to places that are safe and pleasant for people, and that provide healthy habitats for fish and wildlife.

Policymakers and stakeholders have long thought that the GLRI provides economic benefits in addition to improving environmental and ecological conditions in the AOCs. The positive causal effects of grant dollars on housing prices now provides strong empirical evidence in support of these claims.

Restoration Economics: The Effect of the Great Lakes Area-of-Concern Program on Residential Housing Prices

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1. Introduction

The Great Lakes and their tributaries make up the largest surface freshwater system on the planet. They provide water to millions of people in the areas that surround them. Historically, the Great Lakes region served as the industrial heartland of the country for much of the 20th century. This activity left a legacy of toxic pollution, and to this day, chemicals such as fire retardants like polybrominated diphenyl ethers (PBDEs) are found in the water, air, sediment, wildlife, and people who live near the Great Lakes. Exposure to these chemicals comes at a hefty cost to health, resulting in thyroid disorders, birth defects, infertility, cancer and neuro-behavioral disorders (McCartney, 2017). Thus, efforts to clean up the pollutants have been extensive. Many vacated manufacturing plants were listed as Superfund sites, and previous research has estimated the housing market impacts of Superfund site remediation (Greenstone and Gallagher, 2008; Gamper-Rabindran and Timmins, 2013).

In tandem with these land-based sites, 31 water-based sites were listed in 1987 as Areas of Concern (AOCs) and designated for pollution remediation. These sites were viewed as the most environmentally degraded rivers, lakes, and bays connecting to the waters of the Great Lakes. Cleanup of these sites languished until two federal grants programs were established under the Great Lakes Legacy Act (2002, hereafter GLLA) and the Great Lakes Restoration Initiative (2010, hereafter GLRI). Almost $1.2 billion has been spent across more than 700 remediation projects under these two programs, and both programs continue to operate and fund new projects. To date, only four AOC sites have been delisted, where delisting signals that the area has been fully cleaned up.

We analyze three housing market impacts of the AOC program: listing, delisting, and grant dollars awarded to AOC remediation projects under the GLLA and GLRI. To the extent that delisting and grant dollars awarded are heuristics for water quality remediation, our method can provide suggestive evidence of consumers' valuation for improved water quality. A characteristic of the housing impacts is that they may vary as a function of distance from the AOC site, so we apply a spatially explicit hedonic price approach following, for example, Currie et al. (2015), Davis (2011), and Muehlenbachs, Spiller, and Timmins (2015).

To analyze AOC listing, we define multivalued treatments based on progressively longer distances from the AOC boundary, with distance bins of equal length used to demarcate the heterogeneous treatments. An econometric framework is applied that extends inverse propensity score weighting techniques to the context of multivalued treatments (Imbens, 2000). In particular, we use the efficient-influence-function (EIF) estimator for multivalued treatment effects, which is a doubly robust estimator (Cattaneo, 2010). By estimating differential treatment effects, we develop an arguably more accurate answer to the policy question: how do housing values change when consumers learn that their water quality is hazardous? We find effects that are significant both economically and statistically, with negative housing price impacts of 13.2% for homes within or directly adjacent to AOCs compared to those 20–25 km away. For homes 0–5, 5–10, 10–15, and 15–20 km away, we find a negative price effect of listing of 11.6%, 13.0%, 10.3%, and 6.0%, respectively, as compared to those 20–25 km away from the AOCs.

With such large effects, it is important to understand whether housing prices react to information about remediation of water quality. Delisting signals to consumers that water quality is no longer hazardous. Thus, we analyze the houses that experienced negative housing price shocks from the listing to determine if delisting would raise housing prices in those areas. We find no robust evidence of an effect from the delisting.

We next investigate the effect of grant dollars on housing prices. A conjecture is that listing and delisting are different in kind, with listing an abrupt change and delisting a gradual change that culminates a lengthy process of completing (typically) many individual remediation projects. As an investment in remediation, rewarding grant dollars for projects serves as a proxy for environmental quality improvement. Thus, such awards might be capitalized into housing values and manifest as housing price changes. We develop an econometric approach to identify the effect of AOC-level grant dollars awarded on sales price changes. House sales within 20 kilometers of the AOC are in the treatment group based on the finding that the treatment effect of listing goes to zero beyond 20 kilometers. House sales in the 20-25 kilometer distance bin are in the control group.

One concern is that the awarding of grants to AOCs could be non-random across AOCs and, conditional on grants being awarded, the amount awarded could be non-random across AOCs. Three different specifications of the estimator are developed to address this concern. We find a positive, statistically significant, and economically meaningful effect of grant dollars on housing prices.

Our work relates to previous research on the impacts of environmental quality and benefit-cost evaluation of environmental quality programs. A number of papers examine the impact of environmental quality changes that occur due to pollution generated at a fixed location. Two outcomes are studied most: infant health and residential housing prices.[1] Three common features of this literature are: (i) a source of pollution at a geographic point (i.e., smokestack, Superfund site, toxics plant, fracking well, water treatment plant, highway, toll booth, or airport) with a spatially delineated treatment area,[2] (ii) an estimated average treatment effect that applies uniformly across the treatment area (i.e., a binary treatment), and (iii) in some cases, a control area determined by finding the distance past which the effect goes to zero. Following previous research, we identify the treatment effect by comparing near impacts versus far impacts, before and after the treatment.

Benefit-cost comparisons are made possible by evaluating an entire program, and hedonic price theory has merit in this regard due to its foundation in welfare economics (Rosen, 1974; Greenstone, 2017). Studies of the Superfund program (Greenstone and Gallagher, 2008; Gamper-Rabindran and Timmins, 2013) and the grants program under the Clean Water Act

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[1] On infant health, see for example Currie et al. (2015); Currie and Walker (2015); Schlenker and Walker (2016). On the residential housing market, see Currie et al. (2015); Gamper-Rabindran and Timmins (2013); Greenstone and Gallagher (2008); and Muehlenbachs, Spiller, and Timmins (2015). Herrnstadt et al. (2016) study the effect on violent crime in Chicago of air pollution from major interstate highways that transect the city.

[2] The treatment area is typically defined as being “close” to the pollution source; however, there is heterogeneity across research studies regarding the distances considered to be “close” versus “far.”'

(Keiser and Shapiro, 2018) apply this approach to program evaluation in contexts similar to the AOC program.[3] Evidence on the Superfund program is mixed; Greenstone and Gallagher (2008) find that its benefits were substantially smaller than the costs, while Gamper-Rabindran and Timmins, 2013) find that benefits outweighed costs at 39 of 52 sites. Keiser and Shapiro (2018) show that, under the Clean Water Act, the grants program's benefits were substantially smaller than its costs.[4]

Utilizing data on individual housing transactions, we follow prior research that takes advantage of house parcels with a record of repeat sales, thereby permitting the specifications to control for house characteristics that remain fixed over time.[5] In all cases, we interpret these results with the caveat that changes in housing values are only one category of nonmarket benefits associated with environmental quality improvements, and thus they typically serve as a lower bound to an estimate of aggregate benefits of these programs. Keiser and Shapiro (2018) discuss this perspective on the approach thoroughly.

The report continues as follows. Section 2 describes the policy context for the AOC program, while Section 3 reports on the data and variables. Section 4 develops the econometric models for analysis of AOC listing and delisting, and Section 5 describes results on the effects of AOC listing and delisting on residential housing values. Section 6 develops the econometric models for analysis of grant dollars and describes results on the effects of grant dollars awarded on residential housing values. Section 7 concludes the report.

2. Policy Background: The AOC Program

The AOC program originated in 1987 as an amendment to the Great Lakes Water Quality Agreement (GLWQA) of 1972. The agreement – a binational agreement between Canada and the United States – identified AOC sites as the worst cases of environmental degradation in the Great Lakes coastal regions and directed the countries to remediate pollution at these sites. Forty-three AOCs were designated in 1987, with 31 in U.S. waters (Figure 1). Most of the AOC sites include a segment of a river as it enters a Great Lake or a segment of a river in tandem with a harbor on a Great Lake. Official boundaries of the AOC sites follow these basic parameters of encompassing water bodies and some adjacent land. The U.S. AOC program – which is the focus of our study – is administered by the Great Lakes National Program Office of the USEPA.

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[3] Several studies apply the hedonic price method to study residential property markets at particular AOCs (Braden et al. 2004; Braden et al. 20081; Braden et al. 2008b; and Isley et al. 2018). However, the studies apply cross-sectional methods, and not a quasi-experimental approach, and thus do not generate causal estimates. Chay and Greenstone (2005) pioneer the approach of combining quasi-experiments and hedonic theory to develop causal estimates for measuring the value of nonmarket environmental goods.

[4] Olmstead (2010) provides a thorough review of earlier benefit-cost analyses of the Clean Water Act and policy instrument choice in water quality regulation.

[5] Papers using this repeat sales model include Currie et al. (2015); Muehlenbachs, Spiller, and Timmins (2015); Balthrop and Hawley (2017); and Kuwayama, Olmstead, and Zheng (2018).

The environmental condition of each AOC is summarized by completing a checklist of fourteen "beneficial uses" of the water bodies. For the program, these are construed in negative terms by using the official label of Beneficial Use Impairments (BUIs) of an AOC site. BUIs are consequences of pollution, and they include such categories as fish and wildlife deformities, degradation of small plant and animal life in aquatic ecosystems, degradation of aesthetics, and beach closings due to public health concerns. Each AOC was evaluated, as a baseline, by assessing the number of BUIs that were present at the time of listing in 1987. Three AOCs had the maximum number of BUIs, fourteen, as their baseline condition. At the other extreme, one AOC had only two BUIs and three AOCs had three BUIs as baseline conditions. The mean number of BUIs was 8.2 per AOC. For each AOC, BUIs are removed incrementally over time by remediation projects. A scientific review is conducted to certify a successful remediation and, thus, an official "BUI removal."

The delisting process takes several steps after remediation (Carney, 2016). The process typically works sequentially by removing all BUIs, drafting a delisting document, and submitting the draft for review by stakeholders and the general public. A final delisting report is then prepared, transmitted to the U.S. Department of State, and placed into effect. The Oswego River AOC was the first delisting in the United States in 2006. Presque Isle Bay AOC was delisted in 2013, and White Lake AOC and Deer Lake AOC were delisted in 2014.

After initiating the AOC program in 1987, site remediation languished for the next fifteen years (International Joint Commission, 2003; U.S. General Accounting Office, 2002). This changed in 2002 with passage of the federal Great Lakes Legacy Act, which targeted cleanup of contaminated sediment at twelve sites. The act authorized \$50 million in annual federal expenditures through 2008, and it was reauthorized in 2008 under the same terms. Expenditures on projects began in 2005 and continue through the present time. \$586 million in aggregate was spent on 20 projects under the GLLA (Table 1). The Great Lakes Restoration Initiative – another federal initiative – sustained the funding of AOC site remediation. The GLRI started in 2010 and is now moving through a second phase that ends in 2019. Through 2016, \$590 million was spent on 691 AOC remediation projects under the initiative (see Table 2). Over this time, a total of 66 BUIs at 24 AOCs were removed, which is more than six times the total number of BUIs removed in the preceding 22 years. In tandem, the GLLA and GLRI combined for \$1.176 billion in remediation expenditures through 2016. This is an average of \$37.9 million per AOC site, which is very similar to the average expenditure of \$43 million per site for remediation of Superfund sites (Greenstone and Gallagher, 2008).

3. Data

Our housing data come from CoreLogic, Inc. and are county deed and tax records in the Great Lakes region. We keep only homes that were sold at least twice. We included distinct sales from the tax database (e.g. only one sale in deed data, but tax data has a prior sale with a different sale date), but only if they were 30 days apart from the corresponding sale in the deed database. We drop one home that supposedly sold 3766 times and another that supposedly sold 81 times. We dropped observations where year built predated effective year built and effective year built was after the 1987 GLWQA. Effective year built is a variable that represents either the year built or the last year in which major improvements were made. These improvements might inject noise into our estimates, which are based on repeat sales. We drop an observation if the year built is missing, because many of the observations for which the year built is missing are land rather than structures.

We dropped the observation if the sale amount, sale date, longitude or latitude was missing, or if there was an invalid latitude or longitude. We imputed the sale date if the days or months were missing in the following way. If a month was given but no day then we imputed to the 15th of the month. If no month was given then we imputed to June 15th of that year.

Unfortunately, which deed document types represent sales is not entirely clear in the CoreLogic data. We combed through 117 document types represented in the CoreLogic data, found definitions and descriptions of each one, and researched whether or not they were likely to indicate a sale. We dropped all document types that were not likely to indicate a sale. Definitions and decisions are available upon request. After that, we additionally excluded the tax deed document type because the values of properties sold under that code in our data were unreasonably low. The 90th percentile sale amount for the tax deed document type was just $25,500. We excluded the following transaction types: timeshares, refinances, construction loans, and nominal transactions. This left the following transaction types: resale, new construction, seller carryback. A seller carryback occurs when a seller acts as the bank or lender and carries a second mortgage on the subject property. This helps a cash-strapped buyer.

All prices are adjusted to be equivalent to May 2018 prices (most recently available as of 6/12/2018) using the CPI series "Consumer Price Index for All Urban Consumers" from the U.S. Bureau of Labor Statistics (2017). We dropped the top and bottom 1% of the inflation-adjusted sales prices to account for outliers.

We further restrict our sample by keeping only residential single family homes. We drop foreclosures and auctions. We also drop the top and bottom 1% of observations in square footage. We drop the observation if the census tract population was 0.

We first analyze the official listing of AOCs in 1987, pre-specifying six bins representing distances from the AOCs. Table 4 presents summary statistics for each of the distance bins. Each observation is a pair of sales, where one sale occurred before and the other occurred after the listing. Distance to water is monotonically increasing as we move farther from the AOC.

Figure 3 plots the average change in log price by distance bin. The average change in log price is increasing as we move farther away from the AOC. The fact that the average change is higher for 0–5 km than for 5–10 km is curious, but overall, the graph looks roughly monotonic.

To analyze the delisting of the AOCs, we select only the observations near three AOCs that were delisted: Deer Lake, Presque Isle Bay, and White Lake. We do not use Oswego River, because the Oswego River AOCs delisting occurred much earlier than the other three. We limit to sales up to 10 years before and after the delisting. We use two bins in our delisting analysis because we have far fewer observations. Table 8 presents summary statistics for the two bins. The bins are chosen based on results from our listing analysis, which is appropriate because the samples are mutually exclusive.

Figure 6 presents the average change in log price from before to after the delisting, and shows that the distance bin further from the AOCs experienced a larger change in log price. It is not clear why this sign pattern would arise. Standard error bars do not rule out equal changes in log price for the two distance bins. The relationship between change in log price and distance appears to be nonmonotonic.

For our grants analysis, we use project-level data compiled by the US EPA in its role as the lead federal agency in both the GLLA and GLRI. For each project, the data include information on AOC, dollars awarded, and year awarded. We cleaned the data by assuring that the project (1) focused on AOCs and not toxic substances (the label for Focus Area 1 under the GLRI is Toxic Substances and Areas of Concern, not simply AOCs), and (2) focused on a single AOC rather than multiple AOCs. We aggregated the data to the AOC level and then formed a variable that measures cumulative grant dollars awarded to the AOC by year, 2004-2017. The data represent 406 projects and $523 million in nominal dollars.

To match the housing price data – which are used to generate a variable change in price for a sales-pair of the same parcel – we form a variable for the change in cumulative grants dollars at the AOC to which the parcel is near. Table 13 reports summary statistics for these variables.

4. Listing and Delisting: Empirical Specification

Consider an event pertaining to the AOCs. We seek the effect of the event on prices varying distances away from AOCs. We specify the price as a function of distance bins:

$$P_{ijt} = \sum_{k=0}^{K} \alpha_k d_i^k \, Post_{jt} + \mu_i + \gamma_t + \epsilon_{ijt} \tag{1}$$

where P_{ijt} = log price of house *i* near AOC *j* at time *t*; $Post_{jt}$ = indicator variable equaling 1 if time *t* is after listing (or after delisting) at AOC *j*, and 0 otherwise; d_i^k = indicator variables equaling 1 if the parcel is in the *k*th distance bin from the nearest AOC, and 0 otherwise; μ_i = fixed effect for parcel *i*; γ_t = fixed effect for year *t*; ϵ_{ijt}= idiosyncratic error term; and α_k = parameters to estimate in the regression.

We use only repeat sales. We transform Equation (1) so that the dependent variable is the difference between two sales of housing parcels. When we restrict to the time periods just before and just after the events we consider,[6] it is rare to encounter more than two sales of a particular home, so this transformation is suitable in our context.

For two sales occurring at times *t* and *s*, where *t* > *s*:

$$\Delta_{st}P_{ij} = \sum_{k=0}^{K} \alpha_k d_i^k \, \Delta_{st}Post_j + \Delta_{st}\gamma_t + \omega_{ijts} \quad (2)$$

where $\Delta_{st}P_{ijt} = P_{ijt}$ - P_{ijs}; $\Delta_{st}Post_j = Post_{ijt} - Post_{ijs}$; $\Delta_{st}\gamma_t = \gamma_t - \gamma_s$; and $\omega_{ijts} = \epsilon_{ijt} - \epsilon_{ijs}$. We use only sales pairs such that *t* is after the event and *s* is before it, so $\Delta_{st}Post_j = 1$:

$$\Delta_{st}P_{ij} = \sum_{k=0}^{K} \alpha_k d_i^k + \Delta_{st}\gamma_t + \omega_{ijts} \quad (3)$$

Our specification follows the intuition of Muehlenbachs, Spiller, and Timmins (2015), who study the impact of proximity of shale gas wells by comparing prices for homes of different distances from wells before and after drilling.[7] Muehlenbachs, Spiller, and Timmins (2015) define three bins of unequal sizes by using a cross-sectional local polynomial regression to detect the distances at which effects of shale wells seem to have heterogeneous impacts before they estimate the effect of shale wells on price; this procedure was first proposed in Linden and Rockoff (2008). We choose not to use this procedure because we find that bin cutoffs are highly sensitive to choice of bandwidth in our context. Instead, we construct bins of equal sizes.

We suspect that the composition of homes differs across distance bins because of the way that AOCs generally are within, overlapping with, or adjacent to lakes. Thus, the homes in bins farther away from the AOC will likely also be farther away from the lake. To illustrate, we plot home locations and distance bins around the Green Bay/Fox River AOC in Figure 2. Homes in our sample are given by the pink dots in the figure. The Green Bay/Fox River AOC is outlined in red. Mechanically, if homes are uniformly distributed throughout the concentric rings, each subsequent ring will contain a larger proportion of homes that are farther away from the lake. Figure 2 also suggests that bin composition might differ drastically in terms of population, so a second variable on which we want to adjust the composition is Census Tract population.

These composition effects constitute a type of selection bias. In the standard setup, if the selection bias is time-invariant, then the most efficient estimator can be obtained by simply differencing it out in a linear framework (Heckman et al., 1998). However, an underlying assumption for that technique to be valid is that the treatment effect is expected to be similar for all treatment bins. Abadie (2005) points out that observed compositional differences between treated and control groups can cause non-parallel dynamics in the outcome variable, and proposes the use of semi-parametric weighting-based treatments as a potential solution.

[6] We use sales up to 10 years before and after the events we study. Our data starts in 1980 for the listing specification.

[7] Muehlenbachs, Spiller, and Timmins (2015) also compare the effects for groundwater and piped-water homes, so their estimator is a triple-difference estimator.

In our case, these observed compositional differences are a clear threat to identification: homes close to and far from water bodies are likely to experience differential impacts from AOC-related events such as listing and delisting, so distributions of potential outcomes for our bins can only be considered to be equivalent once we condition on distance to the nearest major lake. A further advantage of using a semi-parametric weighting-based approach over controlling for covariates linearly is that it does not make functional form assumptions for how the observed covariates relate to the treatment or the outcome, and thus avoids possible bias due to misspecification (Meyer, 1995; Abadie, 2005). Moreover, external validity considerations might also warrant our weighting approach.

To account for differences in the composition of homes across bins, we seek to weight observations closer to the water and observations that are in more heavily populated areas in our estimation. We use the Efficient Influence Function (EIF) estimator for multivalued treatment effects developed by Cattaneo (2010), who builds off of work by Rosenbaum and Rubin (1983) on semi-parametric weighting estimation using propensity scores, subsequent work by Hirano, Imbens, and Ridder (2003) on the comparatively favorable properties of using the inverse propensity scores, and work by Imbens (2000) on extending inverse propensity score weighting techniques to multivalued treatment contexts.[8,9]

The key assumption for identifying the α's in equation (3) is the ignorability assumption[10], which requires that after controlling for observed covariates such as the distance to the nearest major lake and Census Tract population, the potential outcome distributions for each distance bin are independent of the treatment level (or distance bin), ruling out a situation where some unobservable factor correlated with distance from AOC affects the potential-outcome distributions of the bins differentially. We ensure that our weights sum to 1, a procedure which is shown by Busso, DiNardo, and McCrary (2014) to produce better finite sample properties of weighting estimators in simulations and which is recommended by Cattaneo (2010) when working with the EIF estimator.

We consider the groups of potential variables in Table 3 in our EIF specifications. Group 2 contains distance to nearest water and tract population. Because we have a strong theoretical basis for thinking that distance to nearest water and tract population are important in balancing bin composition, we use these variables in our preferred specification. In our preferred specification, we use a parametric specification of the generalized propensity score (GPS), where the variables enter linearly into the link function. Our fixed effects for the first and second sale year enter linearly into our conditional expectation function for the outcome. So, we specify the outcome as a linear function of the distance to the AOC and the fixed effects.

[8] See Cattaneo, Drukker, and Holland (2013) for a practitioner's guide.
[9] The EIF estimator is recommended by Cattaneo, Drukker, and Holland (2013) because it is doubly robust, meaning that it is robust to misspecification of either the influence function or the generalized propensity score (GPS), but not both. This contrasts with the more commonly used Inverse Propensity Weighting (IPW) technique due to Hirano, Imbens, and Ridder (2003). For an example of a recent paper applying IPW in a difference-in-differences style context, see Deryugina, Kawano, and Levitt (2018).
[10] Ignorability encompasses what are commonly known as the selection-on-observables and common support assumptions.

5. Listing and Delisting: Results

We first present results from estimating a simple linear model (Table 5) for the purpose of comparison. Column 1 contains estimates from equation (3). Columns 2-4 present results from estimating (3) while including the variables that we considered for our GPS as linear controls. Table 5 is clustered on Census Tract. The excluded category is the 20–25 km distance bin. Effect magnitudes are not very different across the specifications, though estimates are higher in magnitude in all bins when we control for the groups of variables that might determine bin composition, and mostly continue to increase when we add more variables. This indicates that these variables that determine bin composition could be important determinants of price growth during this time period, and including them could give us more representative average treatment effects. We find that the change in log price for all of the closer distance bins are different from the 20–25 km bin in a statistically significant way for all of the specifications in Table 5.

We use EIF estimators for the rest of our results. Our preferred specification is chosen according to our intuition, and uses distance to water and tract population to model the GPS parametrically. The predicted probabilities are bounded below by 0.042. Intuitively, these probabilities will be closer to 0 the more bins we have, so they should be interpreted keeping the context of six bins in mind. We could not find a reference that presented a rule of thumb for the minimum predicted probabilities necessary for estimation using the EIF. However, the predicted probabilities in the guide to the EIF method by Cattaneo, Drukker, and Holland (2013) are as low as 0.032. Thus, we are confident that our predicted probabilities are at least consistent with current practice in implementing EIF estimation.

Table 6 presents the results of our preferred specification and Figure 4 presents the Potential Outcome Means (POMs). In Table 6, Column 1 presents the results clustered by Census Tract and Column 2 presents the results clustered by county. The difference in the change in log price for the homes inside or directly adjacent to the AOC is 13.2%, and it is 16.6%, 13%, 10.3% and 6% for the 0–5, 5–10, 10–15, and 15–20 km bins, respectively. The signs are all as expected: homes closer to the AOC experienced a more deleterious effect on housing prices than homes 20–25 km due to the listing. The results for each bin are statistically significant at the 1% level when we cluster on Census Tract. The 0 km and 0–5 km bins are statistically significant at the 1% and the rest of the closer bins are statistically significant at the 5% level when we cluster on county. This is surprising given that there are only 27 counties represented. It is worth noting that the estimates are higher than those we obtained from the linear specification, by up to 8 percentage points in some cases. This might indicate that weighting by the composition is necessary to ensure that we are comparing apples to apples when it comes to homes near AOCs.

We reproduce the results of our preferred specification using a flexible approach to the propensity score in Table 7; the POMs can be found in Figure 5. The algorithm, which maximizes the Bayesian Information Criterion (BIC), is applied to the Distance to Water and Tract Population variables and restricted to a second-order polynomial in these variables. The fully interacted model is chosen, containing variables Distance to Water, Tract Population, Distance to Water^2, Tract Population^2, and Distance to Water $\times$ Tract Population. This more flexible approach can mean that estimated propensity scores are closer to 0 or 1 for some individuals, which presents a problem for estimation. The predicted probabilities can be as low as 0.019. The ATEs are slightly larger, but approximately the same. We conclude that the parametric version is adequate.

Motivated by our significant results for the effect of listing, we combine the first 5 bins into one bin to analyze the delisting.[11] The question at hand is: for the area surrounding the AOC, which was hit harder than the periphery by the listing, does delisting improve property values?

We present the ATEs from the EIF estimation akin to our preferred specification for the listing in Table 10. The coefficient is negative, contrary to our expectations, as delisting might serve as a positive "news shock" about water quality. The estimate is not significant. For the graph of POMs, see Figure 7.

As robustness checks, we also present the results from the same linear difference-in-differences style specifications we presented for the listing analysis (Table 9) and from a nonparametric GPS specification (Table 11). We find that the signs of coefficients are not consistent across specifications for the linear specification, and standard errors for our coefficients of interest are large. The ATE is imprecisely estimated for the GPS specification and has a negative sign; for the graph of POMs see Figure 8. We interpret these results as indicating that the delisting does not have a robust effect on housing prices.

The presence of GLRI and GLLA grants means our estimates are more difficult to interpret. To the extent that AOCs were simultaneously cleaned up at the same time as delisting, our estimates will combine the effect of the news shock of delisting with the consumers' true valuation of the cleaner water. If we assume that consumers use delisting as a heuristic for understanding surface water quality near their home, then our delisting estimates should be positive because they should represent their preferences over clean water.[12]

However, delistings could negatively affect housing prices because they represent less grant funding for cleanup in the future. To the extent that consumers were aware of delistings, they might have perceived that cleanup – and its related expenditures – would be stalled by them. It is possible that the expected loss in future grant money cancelled out any positive effect of information about water quality that consumers received from the delisting news.

[11] This is out of necessity. Unfortunately, we have too few observations to estimate 6 bins using the EIF function. When we do not combine the bins in this way, the likelihood does not converge across several specifications because there are too few observations in certain bins.
[12] The Great Lakes Legacy Act was authorized in 2002 with the first appropriation in 2004, so we do not suspect that grants could bias our estimates of the effect of listing.

6. Grants: Empirical Specifications and Results

Now we turn to examining the effect of grants. We seek to understand whether grants awarded to AOCs raised housing prices. Grants awarded under the GLLA began in 2004, while grants awarded under the GLRI began in 2010. Over $350 million in federal dollars has been awarded under the GLLA, and over $600 million has been awarded to AOC remediation under the GLRI. The grants fund specific remediation projects or related technical assessment activities. Both programs are administered using a competitive grants process. USEPA administers the GLLA program, while five federal agencies administer the AOC area within the GLRI program (USEPA, National Oceanic and Atmospheric Administration, US Army Corps of Engineers, US Fish and Wildlife Service, and US Geological Survey).

6.1. Preliminary Evidence

We worry that the awarding of grants to areas could be non-random across AOCs, and conditional on grants being awarded, the amount awarded could be non-random across AOCs. It is unknown how the decision to seek/award grants might depend on trends in housing prices or on local economic conditions that affect housing prices. These concerns motivate us to start with a crude specification that includes only sale pairs where there were grants awarded in between the sales. In this specification, we simply compare the price change in the 0–20 km bin to that in the 20–25 km bin[13,14]:

$$\Delta P_{ist} = \alpha\, !\{0 - 20km\}_i + \varphi' X_i + \sigma_s + \tau_t + \epsilon_{ist} \qquad (4)$$

where $!\{0 - 20km\}_i$ is a dummy variable that equals 1 if the house is located in an AOC or less than 20 km away from it, and 0 otherwise; X_i is a vector of controls; σ_s is a dummy that equals 1 if the first sale occurred in year *s*; is a dummy that equals 1 if the second sale occurred in year *t*; ϵ_{ist} is an idiosyncratic error term. The specification in equation (4) equivalent to a difference-in-differences setup with the differences being near-far and before-after.

The parameter α represents the difference in the price change from before to after the grant in the 0–20 km bin and the 20–25 km bin. If the 0–20 km and 20–25 km distance bins would have had a similar price trend in the absence of the grant being administered, then α can be seen as the causal effect of obtaining a grant on the housing price. This supposes that the 20–25 km bin is not able to benefit from the grant. If houses in the 20–25 km bin are able to benefit from the grant, but our theory tells us that they benefit less from the grant, then they are also treated, albeit to a lesser extent. In this situation, α serves as a lower bound on the causal effect of obtaining a grant on housing prices.

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[13] These bins are motivated by our listing specification, where we saw a statistically insignificant effect after 20 km, and by data limitations that prevent us from using a finer-grained specification of the distance bins.
[14] We only use observations within 25 km of the AOCs.

We focus on the effects of grants on price, keeping both grants and price in levels rather than transforming to logarithms, because subsequent grant specifications will use continuous variation in the value of grants to exploit more variation in our dataset and we want to be able to compare effects in this baseline regression to effects in those. Not transforming the variables is preferable in our specifications with a continuous grant variable for a few reasons. First, we wish to know the capitalization in dollars of a dollar value of grants. We are not inherently interested in what the effect of a percent change in cumulative dollar value of grants is on the percent change in prices because it is not the statistic relevant to policymakers. Second, while we can take the coefficients from a regression of log price change on a series of dummy variables (as in Section 4) and transform them to get the price change in levels from listing or delisting, the same is not true when one regresses log price change on a continuous variable, and could introduce confounding (Ciani and Fisher, 2019). Nevertheless, we produce robustness checks using the logarithm of the prices for this specification, and we analogously produce robustness checks using the inverse hyperbolic sine of the grants and the inverse hyperbolic sine of the prices for the continuous specifications, because they sometimes include grant values of zero.

The specification in (4) is equivalent to a difference-in-differences with house fixed effects and year fixed effects, without the vector of controls X_i. With the vector of controls, we have additionally allowed for the price trend to depend on the controls. This would be equivalent to including controls interacted with a *Post* dummy in a difference-in-differences specification.[15]

Our vector of controls includes distance to water, tract population in 1980, and land square footage. These controls do not vary over the course of the sample, and are determined prior to the grants being administered; as such, they are not affected by treatment.

The purpose of these controls is twofold. First, preferences for these features could be changing over time. Because the distance bins have different compositions of these features, our regression could pick up a spurious relationship between distance to AOC and the price change if that were the case. For example, homes in the 0–20 km bin are within or close to AOCs, which are located on or adjacent to the shoreline, whereas fewer homes in the 0–25 km bin are close to the shoreline, and thus they tend to be farther away from water on average. If preferences for distance to water are changing over time, then that could be reflected in our estimate of α if we do not control for distance to water.

Second, the controls can add precision by soaking up residual variation in the change in price under the presence of heterogeneous price trends in these features over time, even if preferences for these features are completely uncorrelated with the AOC bins and even if there are similar distributions of features across bins.

Our sample only uses two observations per home for each grant, one of which occurred before and one of which occurred after the grant was administered. In real estate, this design has

[15] Note that by taking the difference of the two sale prices, we have already differenced out the individual home-specific house features, just as one would in a difference-in-differences specification.

advantages over generalized (panel) difference-in-difference models with time fixed effects that include observations with more than two or less than two observations per home, or even studies that restrict to two sales but allows homes to differ in whether those sale pairs occurred both before, one before and one after, or both after treatment. The reason is that in real estate, we generally do not observe homes every single year.[16] Therefore, in general, the composition of homes before and after the event can differ by treatment group if the frequency of sales changes due to the event (e.g., people sell immediately after the event in the near group because they realize the water quality is bad), if people sort across treatment groups due to the event (e.g., people decide to build a home farther away from the AOC than they originally would have), or simply because of over-time macroeconomic trends unrelated to treatment. These compositional effects would not be a problem if researchers observed a latent sale price for every home in every year. In our design, using only two observations per house means that the sample of homes is the exact same before and after the grant changes. In cases where there are multiple positive grant changes in a single AOC, the sample might include multiple observations for a single home, but for each grant we have one observation before and one observation after.

Because the same area could have more than one positive grant change and homes in a single census tract could experience correlated shocks to their price trends, we report standard errors that are robust to clustering at the census tract level to account for serial correlation and cross-house correlation within tracts.[17]

Notice that our estimates are an effect of treatment (getting a grant) on the treated. This means they do not necessarily capture what the effect would be for untreated areas or time periods for treated AOCs during which treatment did not occur. It is plausible that site selection bias (Allcott, 2015) would mean that our estimates do not represent the treatment effects that could be expected for AOCs that were not treated.[18]

The results are displayed in Table 14a. We find that, when including controls, we have a positive and statistically significant effect of grants on the areas that received them. The effect is not significant when not including controls, and is negative. We find that α is between 12,058.82 and 12,288.12. Because α can already be seen as a lower bound, these estimates seem to indicate that the average grant raised housing prices for the average home in the 0–20 km bin by at least \$12,058.82.

However, we urge caution when interpreting these estimates. This baseline approach falls prey to what Smith and Sweetman (2016) call the "common and prosaic practice of reducing heterogeneous programs and policies to binary indicator variables." It masks what we suspect are extremely heterogeneous treatment effects due to the wide range of grant values. Grants can range from \$57,442 to \$54,103,863, a difference of nearly 1000%. Furthermore, the grant variable is skewed. This could lead to imprecision in our estimates. Furthermore, it could mean

[16] If we observe a home every single year, it is being repeatedly flipped and should probably be excluded.
[17] We also report standard errors robust to clustering at the county (FIPS) level, as an additional robustness check.
[18] This is a conditional average treatment effect in the cases in which we use controls.

that our estimated effect is not necessarily close to the differential effect of an average-sized grant on near and far houses. Therefore, we conclude that Table 14a shows evidence that grants had a positive and statistically significant average treatment effect on the treated (ATET), but we are not confident in the interpretation of this average treatment effect on the treated in terms of policy parameters of interest.

6.2. Descriptive Evidence: An Upper Bound Estimate on The Effect of Grant Dollars

While the results of Table 14a are compelling, it would be interesting to understand the impact of a dollar of grants on homes within and adjacent to the AOC. For a descriptive estimate of the price change associated with the grants in the 0–20 km bin from before to after the grant, we regress the change in price from before to after the grant on the change in grant, including year effects for years *s* and *t*, only for sale pairs that saw a positive change in cumulative grants and only for homes in the 0–20 km bin. That is, we specify:

$$\Delta P_{ist} = \beta \Delta G_{ist} + \varphi' X_i + \sigma_s + \tau_t + \epsilon_{ist} \tag{5}$$

where G_{ist} is the cumulative dollar value of grants awarded to the AOC that home *i* is in the vicinity of. This amounts to the analogue of a continuous (intensity) difference-in-differences specification for these areas, where the two "differences" are before-after and the continuous (intensity) grant variable.[19, 20]

The specification given in equation (5) makes better use of variation in our data than the specification given in equation (4). However, it imposes a linear functional form assumption on the relationship between price and grants. This means that it might be sensitive to the influence of outliers if the true relationship is not linear.[21]

The specification in (5) differs from many continuous (intensity) difference in difference designs in an important way: in the typical continuous (intensity) difference-in-difference design, the variable that represents intensity of treatment is assigned before the treatment takes place. Here, however, we could have endogeneity of the intensity variable. Grant value could respond to changes in housing prices in areas.

The coefficient on ΔG_{ist} can only be interpreted causally if we assume that conditional on getting a grant, the dollar value of grants are randomly assigned.

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[19] We could think of these as before-after and high grant change-low grant change.

[20] This is equivalent to Keiser and Shapiro's (2018) hedonic specification, ignoring the controls and the fact that we are only using positive grant changes. The controls and sample differences do not change the fundamental argument that there is potential for endogeneity. Keiser and Shapiro (2018) additionally do a triple-differences (which is not their main hedonics specification) but the third difference is upstream versus downstream. They find nothing, but really it could just be that upstream and downstream properties are equally affected by the grants (why would they be differentially affected?). If upstream properties are affected *at all*, their estimate is a lower bound. So if they find a null effect, they cannot say there's no effect.

[21] For more on linearity, see section 6.3.

Note that Table 14a, by contrast, does not assume that conditional on getting a grant, the dollar values of grants are randomly assigned; we do not need to make any assumption on how dollar values of grants are assigned because the intensity of grants is not being utilized as variation.

Economic intuition can help us sign the endogeneity bias in equation (5). The payoff to seeking a grant is highest for areas where prices inside and close to the AOC are comparatively low and/or decreasing more rapidly (or increasing at a slower rate) than other areas, which could be due to idiosyncratic shocks to housing prices, which tend to be mean-reverting. If prices are simply lower on average for areas that sought larger grants, but also on the same growth path as prices for areas that sought smaller grants, then this average difference will be netted out when we difference over time. However, if areas where prices near and inside AOCs are decreasing more rapidly (or experiencing more sluggish growth) are more motivated to seek higher-magnitude grants than other areas[22], or if the grant-awarding mechanism awards more grants to places where housing values are decreasing more rapidly (or growing more sluggishly), then this mean reversion will result in an effect that is known as Ashenfelter's dip (Ashenfelter, 1978). Ashenfelter's dip causes difference-in-differences estimates to overstate the magnitude of the effect of treatment[23], and so our estimates can be seen as an upper bound on the effect of grant dollars on the areas directly adjacent to AOCs.

In conclusion, we find that an upper bound on the effect of a dollar of grants on the price change of homes that were located 0–20 km from an AOC is $0.0009. The average grant value for observations in the near bin is $2,696,439 (see Table 12a. Thus, the overall upper bound of the effect of the grant program on homes in the near bin is estimated to be 0.0009*2,696,439 = $2,428.

6.3 Causal Evidence: A Lower Bound Estimate on The Effect of Grant Dollars

Equation (4) lumps the effect of all grants together, even though grants vary in size from the tens of thousands to the tens of millions of dollars. Given this extreme heterogeneity, we would expect that the effect of grants might be estimated relatively imprecisely in Table 14a. And, given the endogenous nature of the grants, the treatment effect reported in Table 15a can only be seen as an upper bound. This motivates a specification where we explore the effect of a dollar of grants by examining the differential change over time for the near and far bins, again considering just those sale pairs that occurred just before and just after a positive change in the amount of cumulative grants awarded to an area:

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[22] One possible channel for using local economic information to influence GLRI and GLLA funding decisions is the public advisory committees associated with each AOC. These committees were prescribed for AOCs as part of the organizational structure of the program. They are composed of local stakeholders, including members of community groups, business leaders from different sectors, and key staff of local government agencies. Holifield and Williams (2019) assess the public advisory committees across the AOCs.

[23] The reasoning is as follows: the "rebound" to the mean effect is conflated with the effect of the treatment, leading researchers to falsely attribute the higher growth in the outcome variable for the treatment group relative to the control group to the treatment, even when the treatment did nothing.

$$\Delta P_{ist} = \beta Near_i \times \Delta G_{ist} + \gamma \Delta G_{ist} + \varphi' X_i + \tau_t + \sigma_s + \epsilon_{ist} \tag{6}$$

where β is the differential change in price associated with a dollar of grants for the near bin minus that same change for the far bin. The $\gamma \Delta G_{ist}$ term captures linear selection bias in the awarding of grants to AOCs that is common to both distance bins.

This regression uses the change in price associated with a one dollar increase in grants in the far bin as a counterfactual for the same change in the near bin. More precisely, it addresses the potential endogeneity of grant amounts in equation (5) by using the 20–25 km distance bin to net out AOC-specific factors that determine the magnitude of grants given. That is, even if grant dollars are endogenously awarded to AOCs (conditional on a grant being awarded in the first place) in equation (6), if the process is endogenous in a way that is uncorrelated with differential changes in prices over time in the near and far bins, then β can still be interpreted as a lower bound on the average causal effect of one dollar of grant money from the program on the homes in the 0–20 km distance bin (or a lower bound on the conditional average treatment effect on the 0–20 km group, when controls are included). Our approach would be invalidated if an AOCs motivation to seek larger grants depended on differential price trends between homes in the two distance bins.[24] One way that this assumption could be violated is if areas where the prices of waterfront homes or homes close to water have dipped the most seek larger grants for restoration of AOCs; this motivates the inclusion of our control for distance to water.

It is worthwhile to point out that because we only use sale pairs that occurred just before and just after a positive change in the cumulative grants awarded to an area, the specification in equation (6) is not quite a triple-difference specification, because there are no "untreated" observations in the Near bin. As such, we do not include the regressor $\Delta Near$.

The estimates associated with equation (6) are given in Table 16a. When we do not include controls, we get a very small and statistically insignificant effect. Including three different sets of controls, we find that the effects are all around 0.0006. We suspect that our controls serve important functions as described in Section 6.1, and thus our preferred specifications are given in columns 2 through 4. The effect sizes are economically significant in magnitude. The average grant value for observations in the near bin is $2,696,439 (see Table 12a). Thus, the overall effect of the grant program on homes in the near bin is estimated to be 0.0006*2,696,439 = $1,618.

We would expect the estimated treatment effect of a dollar of grants in equation (6), β, to be slightly lower than the β in equation (5). This is because β in (6) is the difference between the change in price associated with a dollar change in grants for the near bin and the far bin, whereas β in (5) is simply the raw change in price from before to after the grant change associated with a dollar change in grants. To the extent that the 20–25 km bin reaps some benefits of the grants,

[24] Note that because we only use sale pairs where there is a positive grant change between the sales, there is no threat of endogeneity due to areas being more likely to seek a grant in the first place if differential trends in housing prices in the two distance bins follow a certain pattern; however, arguments about site selection bias given in the first section still apply here.

this raw change should be higher. Indeed, we find that in all columns of Table 16a, the implied effect of a dollar of grants on average is less than the corresponding column in Table 15a. However, the magnitudes are of the same order, which is reassuring. We interpret the β from equation (5) to be an upper bound on the ATET and the β from equation (6) to be a lower bound on the ATET.

The specification in (6) uses a continuous grants variable and makes the same functional form assumption as we made in (5). This means that it also has the advantage of using more variation in the data and an easy-to-interpret estimator. However, like the specification given in (5), it suffers from potential misspecification bias and is sensitive to the inclusion of outliers if the true relationship between the change in price and the grant change is not linear.

As a robustness check on the assumption that the effect of grants on the change in price is linear, we show the results from grant-specific regressions. That is, we estimate equation (4) for each grant change. We want to see whether the relationship between the coefficients and the grant amounts is monotonic and linear. We use controls for distance to water and tract population. The results are produced in Figure 9. The purple dots are coefficients from specification (4), with each sample just including observations from a grant change of the same size. The sizes of the purple dots indicate how many observations from our sample are in each regression. The blue and orange dots are lower and upper bounds for the 95% confidence intervals from each regression. The green line is a fitted line; the slope and intercept are given in the lower left-hand corner of the figure.

It is reassuring that the general trend is upward and the relationship looks monotonic in Figure 9. This gives us confidence that grants to remediate areas around AOCs raised housing values. It is also reassuring that the relationship between the dollar effect of grants and the coefficient giving the differential change in price for near versus far bins looks roughly linear. The coefficient is 0.0008, which is similar to the coefficient of 0.0006 for column 3 (our preferred set of controls) in Table 16. The intercept is -1051.3182, which is close to 0 considering the scale of the grants, and not significant in this meta-regression.

To establish robustness of Figure 9, we follow the same procedure of producing a regression for each grant change, but vary the set of controls. The coefficients for the line of best fit vary from 0.0006 to 0.0008, in line with our previous estimates. The confidence intervals are tightest in general for the specification with the richest set of controls. The intercepts vary from -3398.415 to -835.2567. It is worthwhile to note that the intercept was not significant in any of these meta-regressions. Broadly, we take these robustness checks as evidence that the relationship between change in price and change in cumulative grants is roughly linear with an intercept of roughly 0, given the scale of the grants. Thus, we are confident in our linearity assumption in equation (6).

7. Conclusions

This research develops the first causal evidence of the housing-market impacts of the AOC program. We find robust evidence that the *listing* of the AOCs negatively impacted housing prices in the areas within 20 kilometers of the AOCs. This is interesting because the environmental quality of AOCs does not change at the point of listing; the negative effect thus reflects dissemination of new public information about the extent of degradation of AOCs along with, potentially, a stigmatizing effect of being identified as one of 31 AOCs in the U.S. waters of the Great Lakes.

A similar analysis of the *delisting* of three AOCs does not provide robust evidence of an effect on housing prices. The weak statistical evidence may result from the quite small number of housing-parcel observations in the econometric analysis, due to the fact that the delistings are both few in number and recent in time. An alternative perspective is that one might expect a null effect: while listing was a relatively abrupt event from the public's perspective, remediation and delisting have occurred in stages under the concerted public-information campaigns of the GLRI and GLLA administrative agencies. Thus a possibility is that, prior to delisting, housing prices in areas near AOCs have steadily rebounded from the sharp price decreases caused by delisting. A richer analysis of delisting awaits a better dataset, which is conditional on additional delistings and a longer time period post-delisting.

This set the stage for the analysis of the effect of GLLA and GLRI grant dollars on housing prices. We assess the effect of $523 million in grants awarded to 406 projects. We find that the effect of a dollar increase in cumulative grant dollars to an AOC resulted in a range of 0.0006-0.0009 dollar (0.06-0.09 cents) increase in housing sales prices. This applies to the housing stock within the 0-20 kilometer distance bin of parcels. These estimates translate into price effects at the mean of the data in the range of $1,618 to $2,427 per parcel.

The estimates of the grant effects provide lower bounds for a statistical reason. The estimated effects are relative to any price effects in the 20-25 kilometer bin. We found evidence that the effect of listing went to zero in this bin. However, if a positive effect persist in the 20-25 kilometer bin in the grants case, then the absolute price effects are larger in the 0-20 kilometer bin. In addition, for a conceptual reason, the estimates of the capitalization effects on property values provide lower bounds of the economic benefits of AOC remediation. Any economic benefits that accrue to people who do not live within 20 kilometers of an AOC are not incorporated into the housing market. These people could be individuals who travel to the AOC for recreation or tourism was well as individuals accruing existence value from knowing that formerly degraded areas with the Great Lakes basin are being restored to places that are safe and pleasant for people, and that provide healthy habitats for fish and wildlife.

Policymakers and stakeholders have long thought that the GLRI provides economic benefits in addition to improving environmental and ecological conditions in the AOCs. The positive causal effects of grant dollars on housing prices now provides strong empirical evidence in support of these claims.

Table 1: Funding for AOC Projects under GLLA (2004–2016)

State	Projects	Funding (Million $)
Illinois	0	0
Indiana	3	182
Michigan	6	94
Minnesota	1	2
Ohio	3	107
Pennsylvania	0	0
New York	1	43
Wisconsin	6	157
Total	20	586

Table 2: Funding for AOC Projects under GLRI (2010–2016)

State	Projects	Funding (Million $)
Illinois	25	10
Indiana	31	83
Michigan	152	146
Minnesota	60	27
New York	117	68
Ohio	58	36
Pennsylvania	1	1
Wisconsin	79	124
Multi-State	168	95
Total	691	590

Table 3: Groups of Potential GPS Variables

Group:	Potential Variables:
1	Distance to Nearest Major Water Body
2	Distance to Nearest Major Water Body, Tract Population
3	Distance to Nearest Major Water Body, Tract Population, Year Built

Table 4: Summary Statistics by Distance Bin, 10 Years Before and After Listing

Treatment Level	Mean	Std Dev	Min	Max	Obs
0 km					
Living Sqft (1000)	1.650	0.651	0.685	4.044	73278
Year Built	1949.540	23.648	1800.000	1987.000	73278
Full Baths	1.390	0.572	1.000	5.000	73278
Distance to Water	11.865	12.090	0.000	62.215	73278
Change in Log Price	0.080	0.442	-4.678	3.819	73278
Land Sqft (1000)	16.201	987.635	0.435	267258.469	73278
Tract Population (1980)	3887.672	1326.060	199.000	9077.000	73278
0–5 km					
Living Sqft (1000)	1.536	0.592	0.685	4.044	29464
Year Built	1947.041	24.200	1812.000	1987.000	29464
Full Baths	1.308	0.522	1.000	5.000	29464
Distance to Water	13.764	17.551	0.000	68.452	29464
Change in Log Price	0.100	0.462	-4.063	3.601	29464
Land Sqft (1000)	14.453	53.984	0.044	4356.000	29464
Tract Population (1980)	3968.519	1324.790	296.000	8702.000	29464
5–10 km					
Living Sqft (1000)	1.555	0.611	0.685	4.041	16306
Year Built	1955.519	23.670	1800.000	1987.000	16306
Full Baths	1.431	0.577	1.000	6.000	16306
Distance to Water	17.052	17.070	0.000	73.448	16306
Change in Log Price	0.088	0.534	-3.615	4.000	16306
Land Sqft (1000)	18.170	51.287	0.436	4356.000	16306
Tract Population (1980)	4142.802	1470.488	759.000	9220.000	16306
10–15 km					
Living Sqft (1000)	1.473	0.552	0.685	4.030	14166
Year Built	1951.117	25.409	1800.000	1987.000	14166
Full Baths	1.387	0.570	1.000	6.000	14166
Distance to Water	18.512	18.710	0.000	78.522	14166
Change in Log Price	0.112	0.572	-3.888	4.164	14166
Land Sqft (1000)	16.158	53.057	0.252	4705.351	14166
Tract Population (1980)	4258.479	1483.272	354.000	10874.000	14166
15–20 km					
Living Sqft (1000)	1.461	0.581	0.685	4.044	15685
Year Built	1955.040	22.985	1807.000	1987.000	15685
Full Baths	1.378	0.563	1.000	6.000	15685
Distance to Water	22.799	21.462	0.000	83.545	15685
Change in Log Price	0.144	0.520	-3.613	3.560	15685
Land Sqft (1000)	13.200	43.983	0.800	4377.475	15685
Tract Population (1980)	4312.957	1417.413	839.000	10874.000	15685
20–25 km					
Living Sqft (1000)	1.478	0.564	0.688	4.042	16601
Year Built	1945.141	28.486	1810.000	1987.000	16601
Full Baths	1.390	0.580	1.000	5.000	16601
Distance to Water	24.722	23.248	0.000	88.189	16601
Change in Log Price	0.187	0.526	-3.504	3.659	16601
Land Sqft (1000)	11.643	45.199	0.998	4298.283	16601
Tract Population (1980)	4310.704	1435.710	204.000	11173.000	16601

Notes: These are the summary statistics for each distance bin.

Table 5: Linear Diff-in-diff Style Specification, Listing

	Change in Log Price	Change in Log Price	Change in Log Price	Change in Log Price
0 km	−0.0980***	−0.1005***	−0.1060***	−0.1106***
	(0.0050)	(0.0052)	(0.0052)	(0.0052)
0–5 km	−0.0808***	−0.0829***	−0.0873***	−0.0897***
	(0.0056)	(0.0057)	(0.0057)	(0.0057)
5–10 km	−0.0965***	−0.0980***	−0.0997***	−0.1095***
	(0.0070)	(0.0070)	(0.0070)	(0.0070)
10–15 km	−0.0766***	−0.0779***	−0.0777***	−0.0836***
	(0.0071)	(0.0071)	(0.0071)	(0.0072)
15–20 km	−0.0436***	−0.0440***	−0.0438***	−0.0530***
	(0.0067)	(0.0067)	(0.0067)	(0.0068)
Distance to Water		−0.0002**	0.0000	−0.0001
		(0.0001)	(0.0001)	(0.0001)
Tract Population (1980)			−0.0000***	−0.0000***
			(0.0000)	(0.0000)
Year Built				0.0009***
				(0.0001)
Census Tract Clusters	59332	59332	59332	59332
Observations	164319	164319	164319	164319

Notes: This is a linear DID regression. We cluster on Census Tract (59,634 clusters). This specification includes sale year 1 and sale year 2 fixed effects. We include variables determining the composition of our distance bins, controlling for them linearly. Standard errors are in parentheses. Base outcome is 20-25 km bin.

Table 6: Table of ATEs, Parametric GPS (GPS Variables: Distance to Water, Tract Population), Listing

Dependent variable:	Change in Log Price	
	(1)	(2)
ATE of 0 km vs 20–25 km	-0.132^{***}	-0.132^{***}
	(0.006)	(0.037)
ATE of 0–5 km vs 20–25 km	-0.116^{***}	-0.116^{***}
	(0.006)	(0.034)
ATE of 5–10 km vs 20–25 km	-0.130^{***}	-0.130^{**}
	(0.007)	(0.057)
ATE of 10–15 km vs 20–25 km	-0.103^{***}	-0.103^{**}
	(0.008)	(0.045)
ATE of 15–20 km vs 20–25 km	-0.060^{***}	-0.060^{**}
	(0.007)	(0.030)
Dist to Water in GPS	Yes	Yes
Tract Pop in GPS	Yes	Yes
Year Built in GPS	No	No
Cluster on	Census Tract	County
Clusters	59634	27
Observations	165630	165630

Notes: In the first column, we cluster on Census Tract (59,634 clusters), and in the second, we cluster on county (27 clusters). This specification includes sale year 1 and sale year 2 fixed effects. We include variables determining the composition of our distance bins linearly in the logit link function to obtain our parametric GPS specification. Standard errors are in parentheses. Base outcome is 20-25 km bin.

Table 7: Table of ATEs, Nonparametric GPS (GPS Variables: Distance to Water, Tract Population), Listing

Dependent variable:	Change in Log Price	
	(1)	(2)
ATE of 0 km vs 20–25 km	−0.135***	−0.135***
	(0.006)	(0.045)
ATE of 0–5 km vs 20–25 km	−0.117***	−0.117***
	(0.006)	(0.044)
ATE of 5–10 km vs 20–25 km	−0.133***	−0.133**
	(0.007)	(0.065)
ATE of 10–15 km vs 20–25 km	−0.105***	−0.105*
	(0.008)	(0.055)
ATE of 15–20 km vs 20–25 km	−0.062***	−0.062*
	(0.007)	(0.034)
Dist to Water in GPS	Yes	Yes
Tract Pop in GPS	Yes	Yes
Year Built in GPS	No	No
Cluster on	Census Tract	County
Clusters	59634	27
Observations	165630	165630

Notes: In the first column, we cluster on Census Tract (59,634 clusters), and in the second, we cluster on county (27 clusters). This specification includes sale year 1 and sale year 2 fixed effects. We maximize the Bayesian Information Criterion using permutations of variables determining the composition of our distance bins up to a second-order polynomial to obtain our nonparametric GPS specification. Standard errors are in parentheses. Base outcome is 20-25 km bin.

Table 8: Summary Statistics by Distance Bin, 10 Years Before and After Delisting

Treatment Level	Mean	Std Dev	Min	Max	Obs
0–20 km					
Living Sqft (1000)	1.596	0.571	0.640	3.655	3034
Year Built	1955.555	32.169	1800.000	2012.000	3034
Full Baths	1.440	0.605	1.000	4.000	3034
Distance to Water	4.121	2.822	0.000	27.454	3034
Change in Log Price	0.041	0.576	-4.282	3.630	3034
Land Sqft (1000)	26.027	128.838	0.562	4356.000	3034
Tract Population (1980)	4437.833	1472.897	799.000	8313.000	3034
20–25 km					
Living Sqft (1000)	1.741	0.657	0.696	3.598	153
Year Built	1962.085	40.259	1850.000	2008.000	153
Full Baths	1.595	0.622	1.000	3.000	153
Distance to Water	12.889	8.767	0.305	31.284	153
Change in Log Price	0.047	0.617	-2.972	2.044	153
Land Sqft (1000)	146.289	300.480	5.998	2840.722	153
Tract Population (1980)	5429.935	1402.048	2469.000	7396.000	153

Notes: These are the summary statistics for each distance bin.

Table 9: Linear Diff-in-diff Style Specification, Delisting

	Change in Log Price	Change in Log Price	Change in Log Price	Change in Log Price
0–20 km	–0.0101	0.0808	0.0676	0.0481
	(0.0537)	(0.0631)	(0.0633)	(0.0637)
Distance to Water		0.0104***	0.0107***	0.0076**
		(0.0037)	(0.0037)	(0.0038)
Tract Population (1980)			–0.0000**	–0.0000**
			(0.0000)	(0.0000)
Year Built				0.0012**
				(0.0005)
Base Bin Change in Log Price	0.0498	0.0498	0.0498	0.0498
Census Tract Clusters	1693	1693	1693	1693
Observations	3036	3036	3036	3036

Notes: This is a linear DID regression. We cluster on Census Tract (1,698 clusters). This specification includes sale year 1 and sale year 2 fixed effects. We include variables determining the composition of our distance bins, controlling for them linearly. Standard errors are in parentheses. Base outcome is 20-25 km bin.

Table 10: Table of ATEs, Parametric GPS (GPS Variables: Distance to Water, Tract Population), Delisting

Dependent variable:	Change in Log Price	
	(1)	(2)
ATE of 0–20 km vs 20–25 km	−0.013	-0.013^{***}
	(0.075)	(0.004)
Distance to Water in GPS	Yes	Yes
Tract Pop in GPS	Yes	Yes
Year Built in GPS	No	No
Cluster on	Census Tract	County
Clusters	1693	2
Observations	3036	3036

Notes: We cluster on Census Tract (1,693 clusters). There were not enough counties represented to cluster on county in this specification. This specification includes sale year 1 and sale year 2 fixed effects. We include variables determining the composition of our distance bins linearly in the logit link function to obtain our parametric GPS specification. Standard errors are in parentheses. Base outcome is 20-25 km bin.

Table 11: Table of ATEs, Nonparametric GPS (GPS Variables: Distance to Water, Tract Population), Delisting

Dependent variable:	Change in Log Price	
	(1)	(2)
ATE of 0–20 km vs 20–25 km	−0.011	−0.011
	(0.076)	(.)
Distance to Water in GPS	Yes	Yes
Tract Pop in GPS	Yes	Yes
Year Built in GPS	No	No
Cluster on	Census Tract	County
Clusters	1693	2
Observations	3036	3036

Notes: We cluster on Census Tract (1,698 clusters). There were not enough counties represented to cluster on county in this specification. This specification includes sale year 1 and sale year 2 fixed effects. We maximize the Bayesian Information Criterion using permutations of variables determining the composition of our distance bins up to a second-order polynomial to obtain our nonparametric GPS specification. Standard errors are in parentheses. Base outcome is 20-25 km bin.

Table 12: Summary Statistics by Distance Bin, 1995–2017

(a) 0–20 and 20–25 km Bins

Treatment Level	Mean	Std Dev	Min	Max	Obs
0–20 km					
Living Sqft (1000)	1.585	0.657	0.664	4.104	1,000,163
Year Built	1955.598	29.372	1700.000	2016.000	1,000,163
Full Baths	1.424	0.597	1.000	6.000	1,000,163
Distance to Water	12.181	14.420	0.000	85.697	1,000,163
Change in Price	11708.786	94110.842	-966243.500	978733.250	1,000,163
Land Sqft (1000)	19.165	1325.345	0.001	642762.625	1,000,163
Tract Population (1980)	3998.088	1523.792	52.000	12233.000	1,000,163
Sale Amount, Sale 1	163698.518	124450.123	4211.503	1001871.000	1,000,163
Sale Amount, Sale 2	175407.304	124725.147	4211.503	1001292.688	1,000,163
20–25 km					
Living Sqft (1000)	1.583	0.639	0.664	4.104	91,371
Year Built	1953.679	34.872	1700.000	2016.000	91,371
Full Baths	1.500	0.629	1.000	6.000	91,371
Distance to Water	27.626	25.714	0.000	90.498	91,371
Change in Price	10304.389	114350.981	-884282.063	956049.188	91,371
Land Sqft (1000)	21.028	151.951	0.001	36162.203	91,371
Tract Population (1980)	4072.130	1612.818	204.000	11173.000	91,371
Sale Amount, Sale 1	203457.497	148683.986	4223.795	1001642.813	91,371
Sale Amount, Sale 2	213761.886	161012.880	4220.390	1001642.813	91,371

Notes: These are the summary statistics for each distance bin.

Table 13: Grant Statistics by Distance Bin, 1995–2017

(a) 0–20 and 20–25 km Bins

Treatment Level	Mean	Std Dev	Min	Max	Obs
0–20 km					
Cumulative Grants at Sale 1	1,829,715	11,221,672	0	145,206,621	1,005,335
Cumulative Grants at Sale 2	7,668,031	23,177,260	0	145,359,923	1,005,335
Δ Grant, Raw	6,368,189	20,389,494	0	145,359,923	921,685
Δ Grant, Endpoint=0	4,692,652	17,811,750	0	145,206,621	921,685
Δ Grant, No Endpoints	176,349	2,042,653	0	54,103,863	584,031
Δ Grant, > 0, Raw	15,838,803	29,732,055	6,187	145,359,923	370,575
Δ Grant, > 0, Endpoint=0	14,620,972	29,040,100	6,187	145,206,621	295,818
Δ Grant, > 0, No Endpoints	2,696,439	7,550,109	57,442	54,103,863	38,196
20–25 km					
Cumulative Grants at Sale 1	4,713,095	20,677,414	0	145,206,621	91,391
Cumulative Grants at Sale 2	19,643,502	41,128,499	0	145,359,923	91,391
Δ Grant, Raw	15,977,995	35,892,650	0	145,359,923	85,399
Δ Grant, Endpoint=0	12,727,943	31,797,762	0	145,206,621	85,399
Δ Grant, No Endpoints	217,781	2,352,498	0	53,611,634	49,084
Δ Grant, > 0, Raw	36,053,183	46,724,345	6,187	145,359,923	37,847
Δ Grant, > 0, Endpoint=0	35,453,003	44,841,044	6,187	145,206,621	30,659
Δ Grant, > 0, No Endpoints	5,847,687	10,758,233	57,442	53,611,634	1,828
Total					
Cumulative Grants at Sale 1	2,069,989	12,316,478	0	145,206,621	1,096,726
Cumulative Grants at Sale 2	8,665,956	25,383,716	0	145,359,923	1,096,726
Δ Grant, Raw	7,183,084	22,290,985	0	145,359,923	1,007,084
Δ Grant, Endpoint=0	5,374,031	19,521,903	0	145,206,621	1,007,084
Δ Grant, No Endpoints	179,561	2,068,363	0	54,103,863	633,115
Δ Grant, > 0, Raw	17,711,997	32,229,429	6,187	145,359,923	408,422
Δ Grant, > 0, Endpoint=0	16,577,279	31,462,304	6,187	145,206,621	326,477
Δ Grant, > 0, No Endpoints	2,840,365	7,753,500	57,442	54,103,863	40,024

Notes: These are the summary statistics for grants for each distance bin. "Raw" = raw difference between cumulative grants in the year of the second sale and cumulative grants in the year of the first sale, "Endpoint = 0" = if a sale occurs in a year a grant was administered, it is not counted in the grant difference, "No Endpoints" = the observation is missing if a sale occurs in a year a grant was administered, "> 0" = only positive grant amounts are used. Grants do not vary over distance bins for a single point in time, so variation across bins is coming from differential sale years.

Table 14: Linear Diff-in-diff Style Specification, Grants, Clustered on CT

(a) 0–20 and 20–25 km Bins

	Change in Price	Change in Price	Change in Price	Change in Price
0–20 km	-8255.3305^{***}	12058.8285^{***}	12139.7440^{***}	12288.1206^{***}
	(2089.1522)	(2683.4934)	(2685.0932)	(2700.3152)
Distance to Water		538.0064^{***}	536.1659^{***}	555.1489^{***}
		(39.7899)	(39.9536)	(40.2757)
Tract Population (1980)			−0.3318	-0.7776^{**}
			(0.3598)	(0.3499)
Year Built				-94.2308^{***}
				(21.4003)
Base Bin Change in Price	−21851.3876	−21851.3876	−21851.3876	−21851.3876
Grant Measure	> 0, No Endpoints	> 0, No Endpoints	> 0, No Endpoints	> 0, No Endpoints
Census Tract Clusters	23614	23614	23614	23614
Observations	38332	38332	38332	38332

Notes: This is a linear DID regression. This specification includes sale year 1 and sale year 2 fixed effects. We include variables determining the composition of our distance bins, controlling for them linearly. Standard errors are in parentheses. Base outcome is 20–25 km bin.

Table 15: Linear Diff-in-diff Style Specification, Grants

(a) 1 bin, Out to 20 km

	Change in Price	Change in Price	Change in Price	Change in Price
Δ Grant	0.0006***	0.0009***	0.0009***	0.0009***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Distance to Water		845.7360***	842.0356***	883.8192***
		(47.9115)	(48.2541)	(49.0886)
Tract Population (1980)			−0.3968	−0.9548***
			(0.3689)	(0.3574)
Year Built				−117.4083***
				(22.4977)
Grant Measure	> 0, No Endpoints	> 0, No Endpoints	> 0, No Endpoints	> 0, No Endpoints
Census Tract Clusters	22743	22743	22743	22743
Observations	36748	36748	36748	36748

Notes: This is a linear DID regression. This specification includes sale year 1 and sale year 2 fixed effects. We include variables determining the composition of our distance bins, controlling for them linearly. Standard errors are in parentheses. Base outcome is 20–25 km bin.

Table 16: Linear Diff-in-diff Style Specification, Grants

(a) 0–20 and 20–25 km Bins

	Change in Price	Change in Price	Change in Price	Change in Price
0–20 km × Δ Grant	0.0000	0.0006***	0.0006***	0.0006***
	(0.0001)	(0.0002)	(0.0002)	(0.0002)
Δ Grant	0.0007***	0.0003**	0.0003**	0.0003**
	(0.0001)	(0.0002)	(0.0002)	(0.0002)
Distance to Water		494.8259***	492.3014***	509.1770***
		(33.6938)	(33.9245)	(34.1573)
Tract Population (1980)			−0.3670	−0.8002**
			(0.3598)	(0.3496)
Year Built				−91.7310***
				(21.4209)
Base Bin Change in Price	−21851.3876	−21851.3876	−21851.3876	−21851.3876
Grant Measure	> 0, No Endpoints	> 0, No Endpoints	> 0, No Endpoints	> 0, No Endpoints
Census Tract Clusters	23614	23614	23614	23614
Observations	38332	38332	38332	38332

Notes: This is a linear DID regression. This specification includes sale year 1 and sale year 2 fixed effects. We include variables determining the composition of our distance bins, controlling for them linearly. Standard errors are in parentheses. Base outcome is 20–25 km bin.

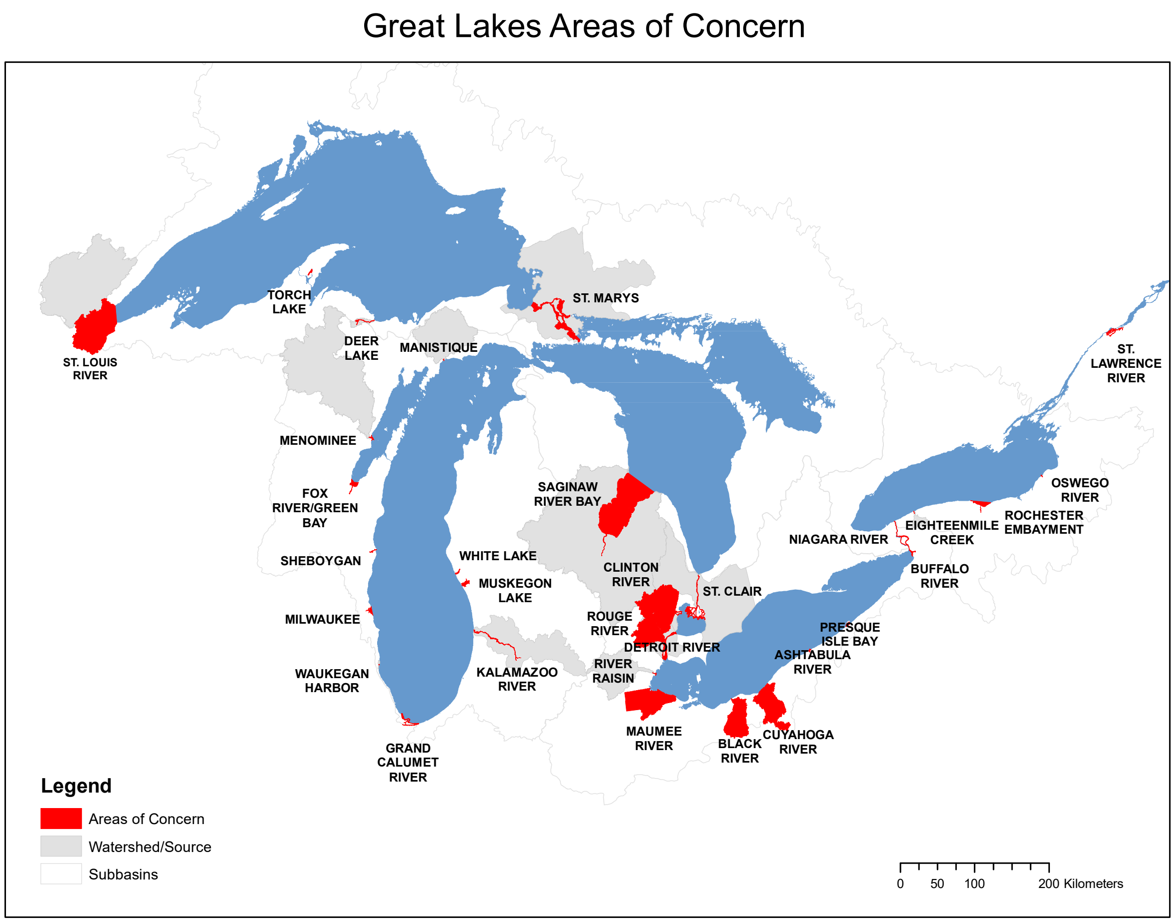


Figure 1: Map of the AOCs

Figure 2: Green Bay-Fox River AOC

Figure 3: Change in Log Price, by Distance from AOC, Listing

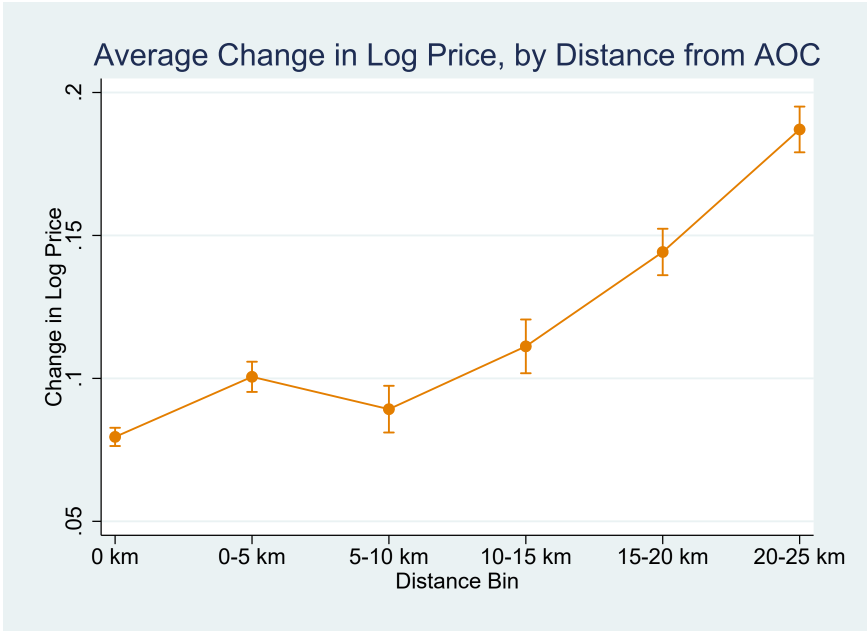


Figure 4: POMs, Parametric GPS (GPS Variables: Distance to Water, Tract Population), Listing

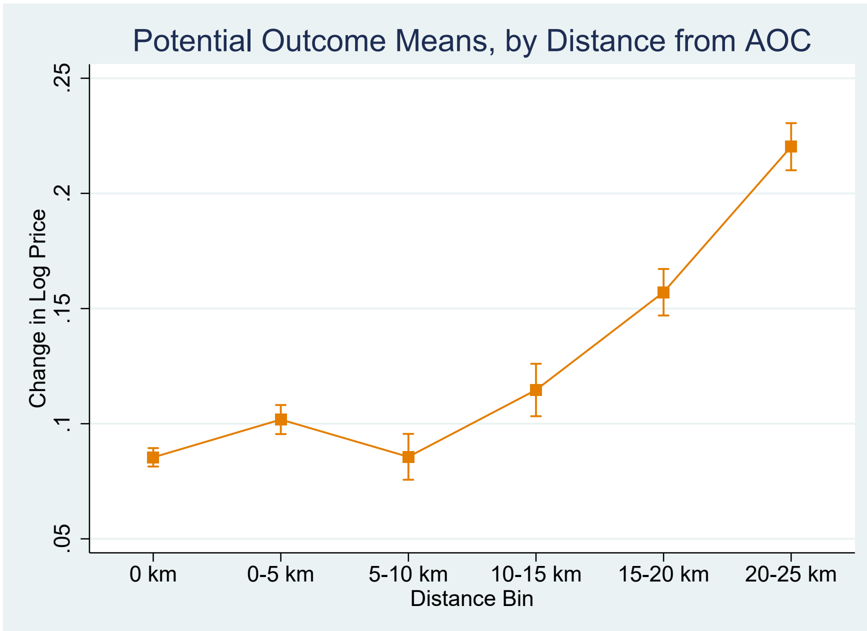


Figure 5: POMs, Nonparametric GPS (GPS Variables: Distance to Water, Tract Population), Listing

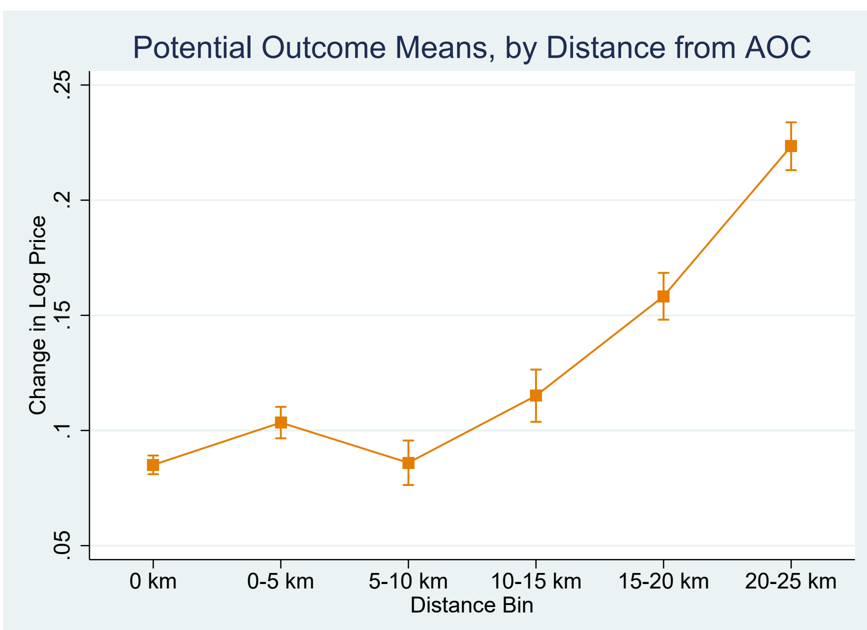


Figure 6: Change in Log Price, by Distance from AOC, Delisting

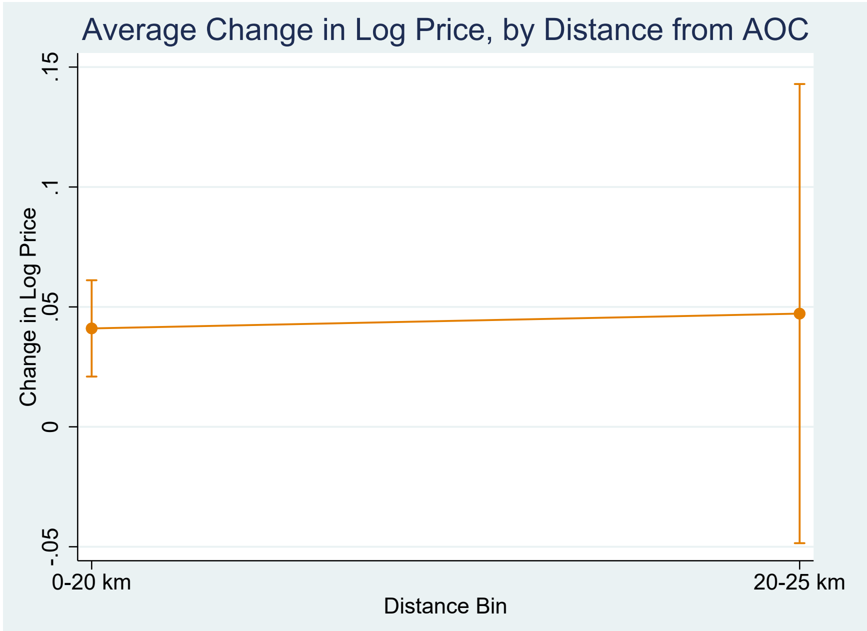


Figure 7: POMs, Parametric GPS (GPS Variables: Distance to Water, Tract Population), Delisting

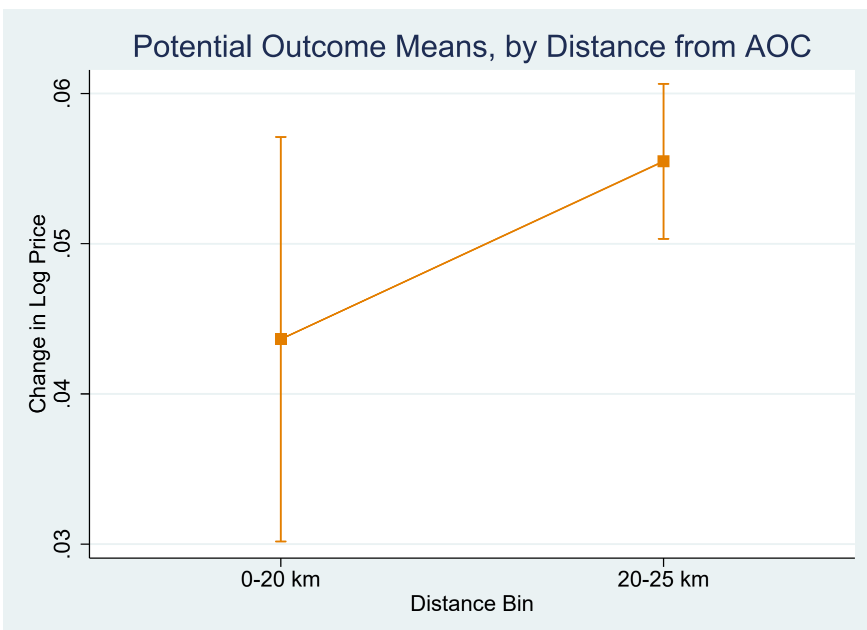


Figure 8: POMs, Nonparametric GPS (GPS Variables: Distance to Water, Tract Population), Delisting

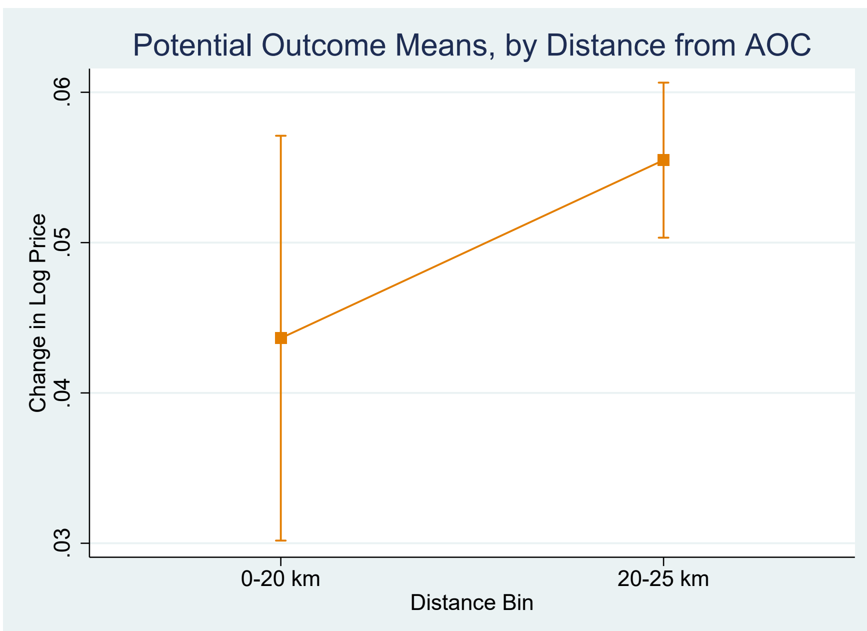
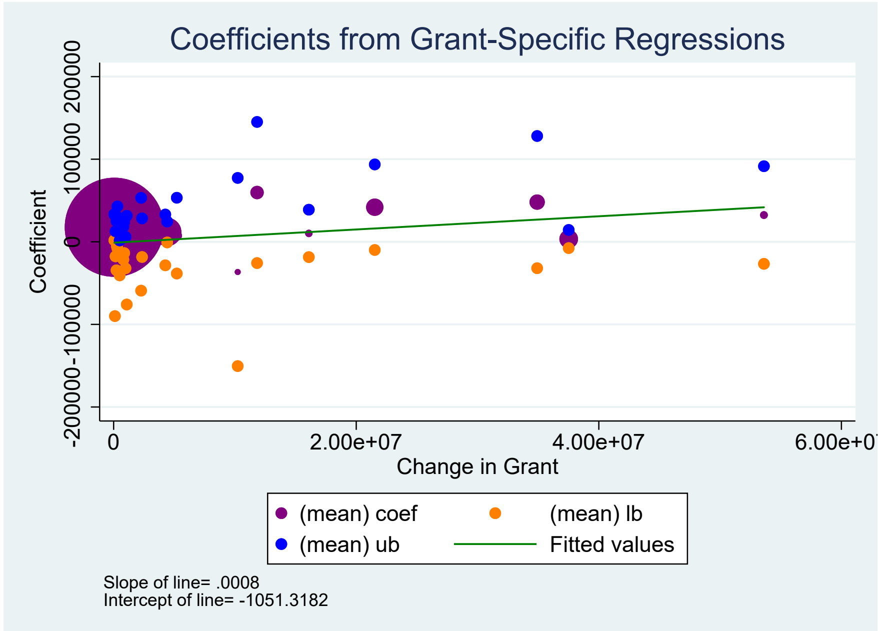


Figure 9: Change in Price Coefficients by Grant Magnitude



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