Property Values, Water Quality, and Benefit Transfer: A Nationwide Meta-analysis

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ABSTRACT *We construct a comprehensive, publicly available meta-dataset based on 36 hedonic studies that examine the effects of water quality on housing values in the United States. The meta-dataset includes 656 unique estimates and entails a cluster structure that accounts for price effects at different distances. Focusing on water clarity, we estimate reduced-form meta-regressions that account for within-market dependence, statistical precision, housing market and waterbody heterogeneity, publication bias, and methodological practices. Although we find evidence of systematic heterogeneity, the out-of-sample transfer errors are large. We discuss the implications for benefit transfer and future work to improve transfer performance.* (JEL Q51, Q53)

1. Introduction

The hedonic literature examining the effects of surface water quality on residential property values began over 50 years ago with David's (1968) report. Since then, the literature has evolved significantly. To assess this literature's suitability to support management decisions related to water quality, we use meta-analytic methods to synthesize and draw key conclusions from 36 unique studies in the United States. There are several existing meta-analyses of hedonic property value studies, including applications to air quality (Smith and Huang 1993, 1995), contaminated sites (Messer et al. 2006; Kiel and Williams 2007; Schütt 2021), open space (Mazzotta, Besedin, and Speers 2014), and noise (Nelson 2004). To our knowledge, this study is the first comprehensive and rigorous meta-analysis of the hedonic literature examining surface water quality.[1]

The results from meta-analyses can help make predictions for benefit transfer—where an analyst uses the predicted outcomes to infer ex ante or ex post effects of some policy action, in lieu of conducting a new original study. Analyses of public policies often rely on benefit transfer because original studies require a lot of time and money or are infeasible because of data constraints. In fact, benefit transfer is one of the most common approaches used to complete benefit-cost analyses at the U.S. Environmental Protection Agency (U.S. EPA 2010; Newbold et al. 2018a). Improving benefit transfer, as well as combining limited but heterogeneous information for surface water quality changes, remains a priority for policy makers (Newbold et al. 2018a).

Several meta-analyses of stated preference studies on water quality have been published

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[1] There are three notable unpublished studies. In her M.A. thesis, Fath (2011) conducted a limited meta-analysis of 13 hedonic studies. Ge et al. (2013) conducted a meta-analysis that combined contingent valuation, travel cost, and hedonic studies. They estimated one meta-regression using only hedonic studies for comparison (10 studies with 127 observations). Abt Associates (2015) estimated the capitalization effects of large-scale changes in water clarity of lakes using a simple weighted-average across nine hedonic studies. We are aware of one published meta-analysis that focused on urban rivers and property values, but it did not examine measures of water quality (Chen et al. 2019).

(e.g., Johnston, Besedin, and Stapler 2017; Newbold et al. 2018b; Johnston, Besedin, and Holland 2019; Moeltner et al. 2019), and these studies are the workhorse for benefit analyses of federal water quality policies (Griffiths et al. 2012; U.S. EPA 2015; Corona et al. 2020). Our meta-analysis complements these efforts. Hedonic property value studies provide a revealed preference-based estimate of values for a subset of households living close to a body of water and thus circumvent concerns related to the use of stated values based on hypothetical scenarios.

Our study aggregates the hedonic property value literature examining water quality and systematically calculates comparable within- and cross-study elasticity estimates by accounting for differences in functional forms, assumed price-distance gradients, and baseline water quality conditions. We convert the primary study coefficient estimates to common elasticity measures for waterfront and near-waterfront homes, and we use Monte Carlo simulations to estimate the corresponding standard errors. Each study yields numerous meta-observations because of multiple study areas, water quality metrics, and model specifications, leading to a meta-dataset that contains more than 650 unique observations. We find considerable differences across the studies in terms of how water quality is quantified, the type of waterbody studied, and the region of the United States examined. We often find it difficult to convert the disparate water quality measures to a common metric.

The analysis in this study focuses on water clarity, where there is a sufficient number of observations (n = 260) for regression analysis. We test for systematic heterogeneity in the housing price elasticities across different regions and types of waterbodies, and we account for best methodological practices and publication bias in the literature. Benefit transfer performance across the different models is compared using an out-of-sample transfer error exercise. Unit value transfers and the simplest weighted least squares (WLS) meta-regression models yield the lowest median transfer error, and we discuss the implications for benefit transfer.

Along with recommendations to practitioners, we provide guidance on combining our results with available data to assess local, regional, and national policies affecting water quality. We highlight gaps in the literature regarding the types of waterbodies and regions covered, and the disconnect between the water quality metrics examined by economists versus those by water quality modelers and policy makers.

2. Meta-dataset

Identifying Candidate Studies and Inclusion Criteria

In developing the meta-dataset, we followed recent meta-analysis guidelines for searching and compiling the literature (Stanley et al. 2013; Havránek et al. 2020).[2] We focused on studies examining the relationship between residential property values and measures of surface water quality.[3] In total, we identified 65 studies in the published and gray literature that were potentially relevant. To facilitate linkages between water quality models and economic valuation, and ultimately to perform more defensible benefit transfers for U.S. policies, focus was drawn to the 36 primary studies that examined surface water quality in the United States using objective water quality

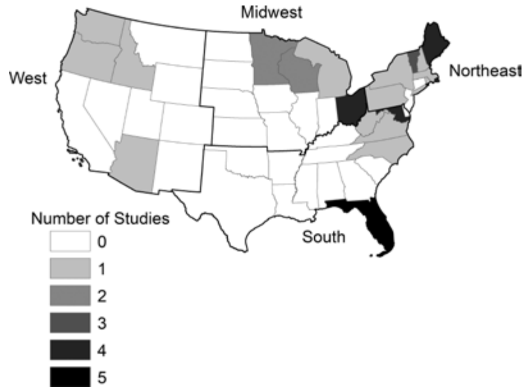
[2] The first author developed how the data would be coded with feedback from all authors. The fourth author did most of the data entry with quality checks by all authors throughout the process.

[3] The search began with reviewing reports (e.g., Van Houtven, Clayton, and Cutrofello 2008; Abt Associates 2016) or other literature reviews and meta-analyses on related topics (e.g., Crompton 2004; Braden, Feng, and Won 2011; Fath 2011; Alvarez and Asci 2014; Abt Associates 2015). The next step was to search a variety of databases and working paper series, which included Google Scholar, Environmental Valuation Reference Inventory, JSTOR, AgEcon Search, EPA's National Center for Environmental Economics Working Paper Series, Resources for the Future (RFF) Working Paper Series, Social Science Research Network (SSRN), and ScienceDirect. Keywords when searching these databases included all combinations of the terms: house, home, property, value, price, or hedonic with terms such as water quality, water clarity, Secchi disk, pH, aquatic, and sediment. Requests were also submitted to ResEcon and Land and Resource Economics Network. Seven additional studies were provided from the first request on October 24, 2014. One additional study was added from a second request on January 21, 2016. After this lengthy process, we attempted one final literature search through the U.S. EPA's internal library system.

measures.[4] A full list of the studies is provided in Appendix A. Although it was published after the construction of the meta-dataset, the list of identified studies was compared with an extensive literature review by Nicholls and Crompton (2018), which provided additional assurance that our identified set of studies is comprehensive. The final meta-dataset is publicly available on the U.S. Environmental Protection Agency's (EPA) Environmental Dataset Gateway.[5]

Figure 1

Number of Water Quality Hedonic Studies in Each U.S. State



Meta-dataset Structure and Details

From the selected 36 studies, 26 are published in peer-reviewed academic journals, 3 are working papers, 3 are master's or Ph.D. theses, 2 are government reports, 1 is a presentation, and 1 is a book chapter. The year of publication ranges from 1979 to 2017. The majority of primary studies examine freshwater lakes (24 studies), followed by estuaries (6 studies), rivers (2 studies), and small rivers and streams (3 studies). One study examines both lakes and rivers. As shown in Figure 1, spatial coverage is limited in the southwest, west-central, and parts of the southern United States, while the Northeast and some parts of the Midwest and South have the most studies.

The meta-dataset consists of a panel or cluster structure, where each study can contribute multiple observations. Individual studies may analyze multiple study areas, water quality metrics, distances, and model specifications. When selecting observations for inclusion in the metadata, researchers tend to follow one of two different approaches (Boyle and Wooldridge 2018). The first approach is to only include the "preferred" estimate from each study, but in the current context, and as described by Boyle and Wooldridge (2018) more generally, this leads to two practical issues. First, in many cases the primary study authors do not identify a preferred model. For example, among the 18 hedonic studies examining water clarity, in only three cases do the primary study authors identify a preferred model (Hsu 2000; Olden and Tamayo 2014; Zhang and Boyle 2010). Second, even in cases where a preferred estimate is explicitly claimed, the decision criteria differ across researchers and are often unknown to the meta-analyst.

To avoid introducing additional subjectivity and potential biases associated with choosing a single estimate (Viscusi 2015; Vesco et al. 2020), we take the second approach described by Boyle and Wooldridge (2018). We include all applicable observations in our meta-dataset, even in cases where the primary estimates do not differ in terms of population, water quality measure, and study area. Recent meta-analyses have taken this same approach (Havránek, Horvath, and Zeynalov 2016; Klemick et al. 2018; Jachimowicz et al. 2019; Johnston, Besedin, and Holland 2019; Penn and Hu 2019; Subroy et al. 2019; Brouwer and Neverre 2020; Vedogbeton and Johnston 2020; Vesco et al. 2020; Schütt 2021). Each primary study estimate, even if pertaining to the same commodity and population, provides a unique observation of the underlying data-generating process for which we want to estimate the parameters.[6]

[4] Specifically, 29 studies were dropped after further screening because an objective water quality measure was not used, the study area was outside of the United States, a working paper or other gray literature study became redundant with a later peer-reviewed publication in the meta-dataset, or the research was not a primary study (e.g., a literature review).

[5] U.S. EPA's Environmental Dataset Gateway: "Meta-dataset for property values and water quality," available at https://doi.org/10.23719/1518489.

[6] Although we disagree with the idea of limiting a meta-analysis to only a set of subjectively identified "preferred"

There are 30 different measures of water quality examined in the literature. To be fully transparent and provide the most information for practitioners to choose from when conducting benefit transfers, the meta-dataset includes all water quality measures. The pooling of estimates across different water quality measures, however, is not necessarily appropriate. Even when converted to elasticities, a 1% change in Secchi disk depth (i.e., how many meters you can see down into the water) means something very different from a 1% change in fecal coliform counts, pH levels, or nitrogen concentrations, for example.[7]

Formatting Comparable Elasticity Estimates

A key challenge in constructing any meta-dataset is to ensure that all the outcomes of interest are comparable across studies (Nelson and Kennedy 2009). By focusing on a single methodology, the outcome of interest itself is always the same—the price effects on residential property values. However, we must still account for two other factors that would otherwise diminish the comparability of results across studies, both of which pertain to assumptions in the original hedonic regression models.

The first form of cross-study differences is a common obstacle for meta-analysts. Differences in functional form lead to coefficient estimates that have different interpretations. In the hedonic literature, some studies estimate semi-log, double-log, and even linear models. Other studies include interaction terms between the water quality measure and various attributes of the waterbody (e.g., surface area) to model heterogeneity. To address these differences, we convert the coefficient estimates from the primary studies to common elasticity and semi-elasticity estimates based on study-specific model-by-model derivations, which are carefully detailed in Appendix A.2. These calculations sometimes include the mean transaction price and mean values of observed covariates, as reported in the primary study. These variables enter the elasticity calculations due to interaction terms or other functional form assumptions in the primary studies.

The second form of cross-study differences involves how the home price effects of water quality are allowed to vary with distance to the waterbody. In a meta-analysis of stated preference studies on water quality, Johnston, Besedin, and Holland (2019) point out that no published meta-regression studies in the valuation literature include a mechanism to incorporate the relationship between households' values for an environmental commodity and distance to the resource. Johnston, Besedin, and Holland (2019) account for this relationship by estimating the mean distance among the survey sample in each primary study, and then include that mean distance as a control variable in the right-hand side of their meta-regression models. We take a different approach that explicitly incorporates spatial heterogeneity into the structure of the meta-dataset.

In the hedonic literature, different primary studies make different functional form assumptions when it comes to the price-distance gradient with respect to water quality, including discrete distance bins and continuous gradients (e.g., linear, inverse distance, polynomial). In a recent meta-analysis of hedonic property value studies examining the price effects of proximity to waste sites, Schütt (2021) circumvents the issue of different distance gradient forms by simply excluding discrete distance specifications. In doing so, his meta-analysis disregards 32% of the otherwise eligible studies.

In contrast, we address this issue directly by including multiple observations from the

estimates and throwing out potentially valuable information, we note that the results of our later meta-regression analysis of water clarity are qualitatively similar. The sign and magnitude of the coefficient estimates are similar, but many of the estimates become insignificant when focusing on this limited sample, especially as the meta-regression models increase in complexity.

[7] That said, when a valid approach could be found, the primary study estimates are converted to a common water quality measure. Such a conversion is only undertaken for two hedonic studies where an appropriate conversion factor for the corresponding study area was available in the literature (Guignet et al. 2017; Walsh et al. 2017). In these cases, the meta-dataset includes unique observations corresponding to the inferred water quality measure (Secchi disk depth) and the original measure (light attenuation). To our knowledge, valid conversion factors or other approaches are not currently available for the other water quality measures and primary study areas.

same primary hedonic regression but where each meta-observation corresponds to house price effects at different distances from the resource. In other words, we calculate the elasticity estimates for "representative" homes at the same, predetermined distances across primary studies, but we do so based on the assumed form of the distance gradients in the original hedonic regressions.

This adds a novel dimension to the cluster structure of our meta-dataset. Except for internal meta-analyses by Klemick et al. (2018) and Guignet et al. (2018), our meta-analysis is the first to incorporate this distance dimension into the meta-dataset. In an internal meta-analysis, the researchers estimate the primary regressions themselves, and thus have the luxury of assuming consistent functional forms and distance gradients in their initial hedonic models. In the current meta-analysis, we do not have this advantage; adapting the elasticity estimates to be comparable across different distance gradient specifications in different studies is a unique challenge.

To minimize any potential sample selection bias corresponding to greater distances, we limit our meta-data and analysis to only price effects within 500 m of a waterbody. Although some studies have found evidence that water quality affects home values at greater distances (e.g., Walsh, Milon, and Scrogin 2011; Netusil, Kincaid, and Chang 2014; Klemick et al. 2018; Kung, Guignet, and Walsh 2022), 16 of the 36 studies in the meta-dataset exclusively analyze price effects on waterfront homes. It is unknown whether some primary studies limited the spatial extent of the analysis because no significant price effects were found or believed to be present at greater distances, or for other reasons (e.g., data or computational limitations). The same reasoning applies to why other studies decided to limit the spatial extent of the analysis at a certain distance.

We standardize the elasticities across different studies with different distance gradient functional form assumptions by "discretizing" distance into two bins: waterfront homes and nonwaterfront homes within 500 m. This allows us to calculate elasticities in a consistent fashion, no matter the form of the price-distance gradient assumed in the original hedonic regressions. If a primary study only examined waterfront homes, then it only contributes observations to the meta-dataset corresponding to the waterfront distance bin. If a study examined waterfront and nonwaterfront homes, then it contributes separate observations for each distance bin, even if the observations are derived from the same underlying regression coefficients.

The elasticity calculations for waterfront and nonwaterfront homes are model-specific and depend on the assumed specifications in the primary studies (see Appendix A.2 for details). Generally, for elasticity estimates corresponding to waterfront homes, any waterfront indicators are set to one, and a distance of 50 m is plugged into the study-specific elasticity derivations as needed. This assumed distance for a "representative" waterfront home is based on observed mean distances among waterfront homes across the primary studies. For nonwaterfront homes within 0–500 m, the midpoint of 250 m is plugged into the study-specific elasticity derivations when applicable.

Finally, meta-analysis often requires a measure of statistical precision around the outcome of interest, in this case, the inferred elasticity estimates. To obtain the corresponding standard error of those estimates, we conduct Monte Carlo simulations. The meta-dataset contains intermediate variables representing all relevant sample means, coefficient estimates, variances, and covariances from the primary studies. Often only the variance for the single coefficient entering the study-specific elasticity calculations is needed for these simulations, and it is common in the literature to report coefficient standard errors. However, some study-specific elasticity calculations include multiple coefficients, requiring both the variances and covariances among that set of coefficients. Hedonic studies do not usually report the full variance-covariance matrix. When needed, we contacted the primary study authors to obtain the covariance estimates required for the Monte Carlo simulations.[8] However, in 25 cases (from four

[8] We are extremely grateful to Okmyung Bin, Allen Klaiber, Tingting Liu, and Patrick Walsh for providing the variance-covariance estimates needed to complete the Monte

different studies), we assume the corresponding covariances are zero because we were unsuccessful in acquiring the information.[9] None of these cases pertain to water clarity, chlorophyll a, or fecal coliform, however, so this assumption does not affect our later unit value and meta-regression results.

Using the primary study coefficient, variance, and covariance estimates, the Monte Carlo simulations entail 100,000 random draws from the joint normal distributions estimated by each primary study. The simulations are carried out separately for each observation in the meta-dataset. After each draw of the relevant coefficients, the inferred elasticity is recalculated, resulting in an empirical distribution from which we obtain the elasticity standard deviation for each observation in the meta-dataset.

The set of 36 studies provide 665 observations for the meta-dataset. We focus on the subset of 598 observations where a house price elasticity and corresponding standard error could be inferred (see Appendix A.1 for details). Water clarity is by far the most common water quality measure analyzed in the literature (with 260 elasticity estimates), followed by fecal coliform (56) and chlorophyll a (36). Several other water quality measures have been examined in the literature and also contribute unique elasticity estimates to the meta-dataset (see Appendix B.2).

Mean Elasticity Estimates and Weighting

Mean elasticity estimates provide useful summary measures and can be used for benefit transfer when unit value transfers are deemed appropriate. Although the literature still generally finds function transfer approaches that explicitly account for various dimensions of heterogeneity preferable (Johnston and Rosenberger 2010), simpler unit value transfers have performed better in some contexts (Barton 2002; Lindhjem and Navrud 2008; Johnston and Duke 2010; Bateman et al. 2011; Klemick et al. 2018).

Table 1, column (1), displays the unweighted mean elasticity estimates for the three most common water quality measures in the hedonic literature: water clarity, chlorophyll a, and fecal coliform.[10] We present separate mean elasticities for waterfront homes and nonwaterfront homes within 500 m of a waterbody. The underlying elasticity estimates come from hedonic regressions that condition on other variables affecting house prices; therefore, the mean house price elasticities can be interpreted as the percent change in price, holding all other observables constant. We note that often the original hedonic regressions do not condition on other measures of water quality.[11] Our interpretation of the literature is that the included water quality measures are often understood to be an indicator or proxy for perceived quality in general (e.g., Taylor 2017).

The unweighted mean elasticities for chlorophyll a are seemingly counterintuitive, and only marginally significant at best. The unweighted mean elasticities with respect to fecal coliform counts are more in line with expectations. The unweighted mean elasticity with respect to water clarity among waterfront homes is positive, as expected, but it is surprising that it is statistically insignificant.

The unweighted mean elasticities can be misleading because of the clustered nature of the metadata. For example, a single primary study may include multiple regression specifications that estimate the price effects for the

Carlo simulations. We also thank Kevin Boyle for providing details on the functional form assumptions in Michael, Boyle, and Paterson (2000).

[9] This assumption could lead to an over- or underestimate for the variance of the corresponding elasticity, depending on the covariance between the two primary study coefficients. Consider the simple case where the elasticity estimate ε is the sum of two coefficients in the primary study hedonic regression, a and b. Then we have that $var(\varepsilon) = var(a+b) = var(a) + var(b) + 2cov(a,b)$. The need to account for multiple parameters often arises due to the inclusion of interaction terms with water quality in the original hedonic regression; and it is often unclear, a priori, what the sign of $cov(a,b)$ should be.

[10] Mean elasticity estimates for all 30 water quality measures examined in the literature are provided in Appendix B.2.

[11] E.g., only 2 (out of 36) of the price elasticity estimates with respect to chlorophyll a come from studies where the original hedonic regressions controlled for other measures of water or ecological quality. Similarly, only 34 (out of 56) and 57 (out of 260) of the price elasticity estimates with respect to fecal coliform and water clarity, respectively, are based on models that control for other quality measures.

Table 1

Mean Elasticity Estimates of the Three Most Frequently Examined Water Quality Measures

Water Quality Measure	Unweighted Mean (1)	Cluster-Weighted Mean (2)	Variance-Adjusted Cluster-Weighted Mean (3)	RES Cluster-Adjusted Weighted Mean (4)	n	Studies	Clusters (K_d)
Chlorophyll a (mg/L)							
Waterfront	0.737* [−0.044, 1.517]	0.324* [−0.036, 0.684]	−0.023*** [−0.028, −0.019]	−0.026*** [−0.031, −0.021]	18	3	3
Nonwaterfront within 500 m	0.005 [−0.201, 0.211]	0.01 [−0.085, 0.105]	0.008*** [0.005, 0.010]	0.009*** [0.006, 0.012]	18	3	3
Fecal Coliform (count per 100 mL)							
Waterfront	−0.018*** [−0.026, −0.011]	−0.037 [−0.088, 0.014]	−0.028 [−0.079, 0.023]	−1.3E-4*** [−1.8E-4, −0.7E-4]	36	4	4
Nonwaterfront within 500 m	−0.020*** [−0.034, −0.006]	−0.058* [−0.127, 0.010]	−0.061* [−0.129, 0.008]	−0.052** [−0.096, −0.008]	20	3	3
Water Clarity (Secchi disk depth, m)							
Waterfront	0.158 [−6.099, 6.416]	0.206 [−16.575, 16.987]	0.191 [−16.590, 16.972]	0.109*** [0.099, 0.118]	177	18	66
Nonwaterfront within 500 m	0.028*** [0.020, 0.036]	0.042*** [0.025, 0.059]	0.041*** [0.024, 0.058]	0.026*** [0.017, 0.034]	83	6	19

Note: Confidence intervals at the 95% level are displayed in brackets. Only elasticity estimates pertaining to the three most commonly examined water quality measures in the hedonic literature are presented, but the full suite of mean elasticity estimates are presented in Appendix B.2. We present the respective units for each water quality measure in parentheses just as a reference but emphasize that the elasticity estimates are unit-less. RES, random effect size.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

same waterbody and housing market, so the weight given to those estimates must be reduced accordingly (Mrozek and Taylor 2002). We next present cluster-weighted means, where we define each cluster as a unique study and housing market combination. Meta-observations estimated from a common transaction dataset in terms of the study area, time period, and waterbodies are really just different estimates of the same underlying "true" elasticity. No matter how many estimates are provided by a study for a specific location, each cluster as a whole is given the same overall weight. This holds regardless of whether the estimates for different clusters (i.e., housing markets) are from the same study or a different study. For example, Boyle, Poor, and Taylor (1999) estimate the effects of lake clarity on four different housing markets in Maine. These estimates are each given a weight of one and thus are weighted the same as if they were estimates for four different housing markets from four different studies.[12]

More formally, let $\hat{\varepsilon}_{idj}$ denote elasticity estimate i, at distance d, for cluster j, and k_{dj} is the number of elasticity estimates for distance bin d in each cluster j. The cluster-weighted mean elasticity for each distance bin d is

$$\bar{\varepsilon}_d = \frac{\sum_{j=1}^{K_d}\sum_{i=1}^{k_{dj}}\left\{\frac{1}{k_{dj}}\hat{\varepsilon}_{idj}\right\}}{K_d}, \quad [1]$$

where the same $\omega_{idj} = \frac{1}{k_{dj}}$ weight is given to each meta-observation in cluster j and for that distance bin. The total number of clusters in the meta-dataset for distance bin d is K_d.

The cluster-weighted mean elasticities are presented in Table 1, column (2). The results are generally similar, suggesting a marginally significant increase in waterfront home values in response to an increase in the concentration of chlorophyll a, and again an insignificant effect on the value of nonwaterfront homes. The negative price elasticity among waterfront homes with respect to fecal coliform is now insignificant. The mean elasticity with respect to fecal coliform counts for nonwaterfront homes is only marginally significant but larger in magnitude, suggesting a 0.06% decrease in value due to a 1% increase in fecal coliform counts. The cluster-weighted mean elasticities with respect to water clarity are similar to the unweighted means, suggesting a positive but insignificant effect on waterfront home prices, and a significant 0.04% increase in nonwaterfront home prices due to a 1% increase in Secchi disk depth.

We propose an adjustment to the above cluster weights that redistributes the weight given to each observation within a cluster and distance bin. Consider the incorporation of a reallocation parameter (r_{idj}) to the above cluster weights, as follows:

$$\omega_{idj} = \frac{1}{k_{dj}} r_{idj}, \quad [2]$$

where $0 \le r_{idj} \le k_{dj}$. This is a generalization of the above cluster weights, where $r_{idj} = 1$. This is also a generalization of the approach taken by some meta-analysts who select a single "preferred" estimate from each cluster, in which case $r_{idj} = k_{dj}$ for the preferred estimate and zero otherwise. As discussed, choosing a single preferred or best estimate can be challenging, disregards information, and introduces additional subjectivity to the meta-analysis.

Instead, we propose an adjustment to the cluster weights based on the inverse variance, or fixed effect size (FES) weights commonly used in the meta-analysis literature (Nelson and Kennedy 2009; Borenstein et al. 2010; Nelson 2015; Havránek, Horvath, and Zeynalov 2016; Vesco et al. 2020; Schütt 2021). Under the FES framework, each meta-observation is considered a draw from the same underlying population distribution, which makes sense in the context of multiple estimates from the same cluster (i.e., housing market). Our proposed variance-adjusted cluster (VAC) weights give more weight to more precise estimates within a cluster while ensuring that equal influence is given to each cluster (or housing market) examined in the literature. For example, Walsh, Milon, and Scrogin (2011) provide six elasticity estimates

[12] Ara (2007) statistically identified and separately analyzed several submarkets when estimating the housing price effects around Lake Erie. These submarkets sometimes overlap because of different statistical strategies, and so in our meta-analysis we treat all estimates from Ara (2007) as being from the same broader housing market.

with respect to Secchi disk depth for lakefront homes in Orange County, Florida. The initial one-sixth weight given to each of these estimates is now redistributed so that more weight is given to more precise estimates in that cluster. The cluster itself (i.e., the elasticity for lakefront properties in Orange County) is still given the same overall weight of one.

This is a specific case of the weights in equation [2], where the reallocation parameter is equal to the usual FES or inverse-variance weights, normalized to sum to one within each cluster and distance bin, and multiplied by k_{dj}:

$$r_{idj} = \left(\frac{\frac{1}{v_{idj}}}{\sum_{i=1}^{k_{dj}} \left(\frac{1}{v_{idj}} \right)} \right) k_{dj},$$

where v_{idj} denotes the variance of elasticity estimate i for homes in distance bin d, in cluster j. Plugging this into equation [2] and canceling out common terms yields our proposed VAC weights:

$$\omega_{idj} = \frac{\frac{1}{v_{idj}}}{\sum_{i=1}^{k_{dj}} \left(\frac{1}{v_{idj}} \right)}. \quad [3]$$

The VAC weighted mean elasticity for distance bin d is calculated as

$$\bar{\varepsilon}_d = \frac{\sum_{j=1}^{K_d} \sum_{i=1}^{k_{dj}} \left\{ \frac{\frac{1}{v_{idj}}}{\sum_{i=1}^{k_{dj}} \left(\frac{1}{v_{idj}} \right)} \hat{\varepsilon}_{idj} \right\}}{K_d}. \quad [4]$$

Under the VAC weighting scheme, every housing market and set of waterbodies analyzed in the literature is given equal influence. This is appropriate if one believes the primary study estimate(s) for a particular housing market and waters are a relatively accurate approximation, regardless of statistical precision. At the same time, statistical precision relative to multiple estimates within a cluster is still given consideration by giving more weight to more precise estimates.

The VAC weighted mean elasticities are presented in Table 1, column (3). The waterfront elasticity with respect to chlorophyll a now has the expected negative sign, suggesting a 0.02% decrease in price when chlorophyll a increases by 1%. The possibly counterintuitive positive elasticity for nonwaterfront homes remains, however, and is now statistically significant. The mean elasticities with respect to fecal coliform and water clarity are similar to the cluster-weighted means.

In contrast to the motivation behind our VAC weights, if one believes that estimates from certain studies, and for specific housing markets and waterbodies, should not be given equal weight because they are of poorer quality, then even the weight given to the cluster as a whole should be reduced. To accommodate this thought, we develop an alternative weighting scheme based on the commonly employed random effect size (RES) weights (Nelson and Kennedy 2009; Borenstein et al. 2010; Nelson 2015). The RES weighting scheme is preferred if the meta-observations are believed to be estimates of different "true" elasticities from different distributions (Harris et al. 2008; Borenstein et al. 2010; Nelson 2015), as is the case when considering variation across housing markets. One would expect the true home price elasticities with respect to water quality to be different across waterbodies that differ in size, baseline water quality, and the provision of recreational, aesthetic, and ecosystem services. Heterogeneity in terms of housing bundles and preferences and income of buyers and sellers would also lead to different elasticities across markets.

Our proposed RES cluster-adjusted (RESCA) weights take the conventional RES weights (w_{idj}^{RES}), which discount the weight given to elasticities estimated with relatively less precision compared with estimates in and across clusters, and then further discounts the weight given to observations where multiple estimates are provided for the same cluster. This is done by taking the product of the RES and inverse cluster weights: $\frac{w_{idj}^{\mathrm{RES}}}{k_{dj}}$. A similar weighting scheme was proposed by Van Houtven, Powers, and Pattanayak (2007), but they were forced to use primary study sample size as a proxy for statistical precision because of a lack of information on the estimated variances in their meta-dataset. For our study, we observe (or can infer) the variance for virtually all elasticity observations. Weights based on the inverse variance or standard error are recommended over those based on the inverse of the study sample size (Van Houtven, Powers, and Pattanayak 2007; Subroy et al. 2019). Details on the interpretation and derivation of the standard RES weights and our proposed RESCA weights are provided in Appendix B.1.

The RESCA weighted mean elasticity for distance bin d is calculated as follows, and the results are presented in Table 1, column (4):

$$\bar{\varepsilon}_d = \sum_{j=1}^{K_d}\sum_{i=1}^{k_{dj}} \frac{\frac{w_{idj}^{\mathrm{RES}}}{k_{dj}}}{\sum_{j=1}^{K_d}\sum_{i=1}^{k_{dj}}\left(\frac{w_{idj}^{\mathrm{RES}}}{k_{dj}}\right)} \hat{\varepsilon}_{idj}. \quad [5]$$

The mean elasticity estimates are now all statistically significant, and often of the expected sign. Similar to the VAC weights, we see a −0.026 price elasticity associated with chlorophyll a for waterfront homes; but still see a counterintuitive, small but positive elasticity corresponding to nonwaterfront homes. For both waterfront and nonwaterfront homes, we now see the expected negative and significant mean price elasticities corresponding to fecal coliform. Perhaps most striking, this is the first case where the mean price elasticity with respect to water clarity is statistically significant for waterfront homes, suggesting a 0.11% increase due to a 1% increase in Secchi disk depth. The price elasticity with respect to water clarity among nonwaterfront homes is similar to the previous mean calculations.

Water Clarity: Descriptive Statistics and Publication Bias

Water clarity is the most common water quality measure in the meta-dataset, with 260 elasticity estimates from 18 studies covering 66 different housing markets. This relatively large sample allows us to estimate meta-regressions for purposes of function transfers.

Descriptive statistics of the elasticity observations with respect to water clarity appear in Table 2. Of the 260 estimates, 56% correspond to water clarity in freshwater lakes or reservoirs, and the other 44% correspond to estuaries. About 68% of the observed elasticity estimates are for waterfront homes. The average of the mean baseline clarity levels reported in the primary studies is a Secchi disk depth of 2.34 m. Of course, this varies by waterbody type. Estuaries have a mean Secchi disk depth of only 0.64 m, whereas freshwater lakes have a mean Secchi disk depth of 3.68 m. Most estimates correspond to the South (48%) or Northeast (29%) regions of the United States, with the remainder corresponding to the Midwest (19%) or West (3%).[13]

Sociodemographics of the primary study areas were obtained from the U.S. Census Bureau by matching each observation to data for the corresponding jurisdictions and year of the decennial census. We chose the finest level of census geography possible while still ensuring that each primary study area was fully encompassed. The identified census jurisdictions are coarse, corresponding to counties, multicounty areas, or states. Median household income is, on average, \$59,078 in the primary study areas (2017\$). Interestingly, the percent of the population with a college degree is low (only 14%, on average), as is population density, suggesting an average of about 50 households per square kilometer. These statistics suggest that homes near lakes and estuaries generally tend to be in more rural areas. It is important to recognize the spatial coarseness of these sociodemographic measures. For example, in many cases income levels among waterfront property owners are likely higher than elsewhere in a county. Finally, mean house prices as reported in the primary studies were \$211,314, on average.

In terms of methodological choices, the assumed functional form of the primary study hedonic regressions varies considerably. Most use double-log specifications (43%), followed by linear-log (31%), log-linear (22%), and even linear (4%). As can be seen by the "no spatial methods" variable, 38% of the elasticity estimates with respect to water clarity

[13] Regions of the United States are displayed in Figure 1 and are defined following the U.S. Census Bureau's four census regions, available at https://www2.census.gov/geo/pdfs/maps-data/maps/reference/us_regdiv.pdf (accessed June 11, 2019).

Table 2
Descriptive Statistics of Observations Pertaining to Water Clarity

Variable	Mean	Std. Dev.	Min.	Max.
Dependent Variable				
Elasticity	0.1167	0.2543	−0.6478	1.7198
Study Area/Commodity Variable				
Waterfront[a]	0.6808	0.4671	0	1
Mean clarity (Secchi disk depth, m)	2.34	1.97	0.38	6.45
Lake or reservoir[a]	0.5615	0.4972	0	1
Estuary[a]	0.4385	0.4972	0	1
Waterbody size[b] (sq. km)	6.8846	5.3156	0.0002	20.8858
Median income (thousands, 2017$)	59.078	14.144	37.865	91.174
College degree (% population)	0.1366	0.0414	0.0768	0.2734
Population density (households/sq. km)	49.91	58.38	1.41	227.96
Mean house price (thousands, nominal $)	211.314	131.341	31.287	675.364
Northeast[a]	0.2885	0.4539	0	1
Midwest[a]	0.1923	0.3949	0	1
South[a]	0.4846	0.5007	0	1
West[a]	0.0346	0.1832	0	1
Methodological Variable				
Elasticity variance	1,228.159	18,704.52	9.03E-06	301,448.5
Unpublished[a]	0.1500	0.3578	0	1
Not in situ measure[a]	0.6115	0.4883	0	1
Other water quality variables[a]	0.2192	0.4145	0	1
Assessed values[a]	0.0538	0.2261	0	1
Study time period (years)	10.27	3.82	3	24
Time trend (0=1994 to 20=2014)	8.59	6.17	0	20
No spatial methods[a]	0.3808	0.4865	0	1
Double-log[a]	0.4308	0.4961	0	1
Linear-log[a]	0.3077	0.4624	0	1
Linear[a]	0.0385	0.1927	0	1
Log-linear[a]	0.2231	0.4171	0	1

Note: Unweighted descriptive statistics are presented for $n = 260$ elasticity estimates in the meta-dataset pertaining to water clarity. Estimates are based on 18 primary hedonic studies, corresponding to 66 study-housing market clusters.

[a]Independent variables that are dummy variables.

[b]Size of the waterbody (or waterbodies) examined in the primary studies was only available for $n = 79$ of the 260 observations in the meta-dataset pertaining to water clarity. If multiple waterbodies were examined in the primary study hedonic regression models, the average waterbody size is reported.

were derived from models that did not use econometric methods to account for spatial dependence (i.e., spatial fixed effects, spatial lag of neighboring house prices, or accounting for spatial autocorrelation via a formal spatial autocorrelation coefficient or cluster-robust standard errors). Although the majority of primary studies use in situ Secchi disk measurements (12 studies), we do see that 61% of the elasticity observations are based on hedonic regressions that used clarity measures other than in situ measures.[14] About 22% of the observed elasticities with respect to water clarity are from hedonic regressions that also control for other measures of water or ecological quality. A time trend variable, as reflected by the last year of transaction data in the primary study, is also included and ranges from 1994 to 2014. This is converted into an index representing the number of years since 1994, which

[14]This large proportion is primarily due to Ara (2007) and Walsh et al. (2017), who provide 30 and 112 elasticity estimates for homes around Lake Erie and the Chesapeake Bay, respectively. Those studies, along with Guignet et al. (2017), use water clarity values based on spatial interpolations. Two other studies use measurements predicted from water clarity models (Boyle and Taylor 2001; Liu et al. 2014), and one study uses satellite data (Horsch and Lewis 2009).

corresponds to the first study of water clarity in the meta-dataset.

We were able to identify and include three unpublished hedonic studies examining water clarity (15% of the observations), but publication bias is still a concern given our goal of obtaining an accurate estimate of the true underlying elasticities for purposes of benefit transfer. Following recommendations by Stanley and Doucouliagos (2012), we examine a series of funnel plots and implement more formal funnel-asymmetry and precision-effect tests. In doing so, we find clear evidence that publication bias is a concern with the metadata collected from this literature (see Appendix B.3 for details). In the meta-regression models discussed next, we include the inverse of the elasticity variances as a right-hand-side covariate to minimize such publication bias (Stanley and Doucouliagos 2012, 2014).

3. Meta-regression Methodology

Function transfers based on meta-regressions can be a useful approach for benefit transfer (Nelson 2015). The approach takes advantage of the full amount of information provided by the literature while accounting for key dimensions of heterogeneity in the outcome of interest. Consider the following reduced-form meta-regression model:

$$\hat{\varepsilon}_{idj} = \beta_0 + \boldsymbol{\beta}_1 \mathbf{x}_{idj} + \boldsymbol{\beta}_2 \mathbf{z}_{idj} + e_{idj}, \qquad [6]$$

where the parameters to be estimated are $\beta_0, \boldsymbol{\beta}_1$, and $\boldsymbol{\beta}_2$. The right-side moderator variables include a vector of characteristics of the primary estimate, study area, and corresponding waterbody ($\mathbf{x}_{idj}$) and a vector of methodological variables ($\mathbf{z}_{idj}$), which describe attributes of the primary study and model assumptions. The error term e_{idj} is assumed to be normally distributed and is allowed to be correlated within clusters.

The vector $\mathbf{x}_{idj}$ includes indicators of whether the elasticity estimate corresponds to waterfront homes, water quality in an estuary (as opposed to freshwater lakes),[15] and the mean baseline water clarity level corresponding to the respective waterbody or portion of the waterbody. The vector $\mathbf{x}_{idj}$ also includes characteristics of the study area and housing market, such as median income, proportion of the population with a college degree, mean house prices, and indicators denoting each of the four broad U.S. regions: the Northeast, Midwest, South, or West.

The vector $\mathbf{z}_{idj}$ captures differences in elasticities due to estimate quality and methodological choices made by the primary study authors. If particular values of $\mathbf{z}_{idj}$ denote best practices, then such information can be exploited when predicting values for purposes of benefit transfer (Boyle and Wooldridge 2018). The vector $\mathbf{z}_{idj}$ includes the variance of the corresponding elasticity estimate. Under the assumption that better-quality estimates have a lower variance, Stanley and Doucouliagos (2012, 2014) argue that this attribute should be set to zero in any subsequent benefit transfer exercise.

In addition, $\mathbf{z}_{idj}$ includes indicator variables denoting unpublished studies, whether a study used assessed values (as opposed to actual transaction prices), different functional forms, and when the model did not account for spatial dependence. If a primary study model did not include spatial fixed effects, a spatial lag of housing prices, nor account for spatial autocorrelation in some fashion, then the no spatial methods indicator is set to one, and is zero otherwise.

We also include a study year trend variable to possibly reflect changes in empirical methods, data, tastes and preferences, and awareness of water quality over time (Rosenberger and Johnston 2009). Time trends in meta-analyses of stated preference studies are typically based on the year the primary study survey was conducted, which is different from the year of publication (e.g., Van Houtven, Powers, and Pattanayak 2007; Rosenberger and Johnston 2009; Johnston, Besedin, and Holland 2019). For a hedonic meta-analysis, the choice is not as clear because the observed revealed preference data in a primary study

[15] As described in Section 2, the meta-dataset includes price elasticities corresponding to freshwater lakes, estuaries, rivers, and small rivers and streams. However, the hedonic studies in the meta-dataset that examine water clarity focus solely on lakes and estuaries.

often spans several years. To capture changes over time, we use the last year of transaction data in the primary study sample.[16]

When estimating equation [6], the observations are weighted according to the same VAC or RESCA weights discussed in Section 2, but an additional complication arises from the cluster structure of the metadata. There may be cluster-specific effects associated with a particular housing market and the waterbodies examined in that housing market. A fixed effect (FE) panel meta-regression model to directly estimate cluster-specific effects could be implemented, but this approach is not viable in the current context. First, the fixed effects would absorb much of the variation of interest and disregard a lot of observations. Many of the modifiers in the meta-regression do not vary within a cluster. Even when there is some within-cluster variation, it is often only among a subset of the observations. Second, out-of-sample inference for purposes of benefit transfer would not be valid because we cannot estimate the corresponding fixed effects for housing markets and waterbodies that are not in the current meta-dataset.

Conventional random effects (RE) panel models are sometimes recommended in cases when a meta-regression is estimated using multiple estimates from a primary study (Nelson and Kennedy 2009). However, the cluster-specific effect could be correlated with observed right-hand-side variables, which leads to inconsistent estimates (Wooldridge 2002). Stanley and Doucouliagos (2012) point out that the necessary assumptions for consistent estimates in a RE panel model will often be violated, especially when a measure of precision (e.g., estimate variance) is included on the right-hand side to control for publication bias. They recommend a simple WLS meta-regression that allows for cluster-robust standard errors. We follow this recommendation in our meta-regression analysis.[17]

[16] This proxy is not without possible error, however; e.g., Zhang et al. (2015) conduct a more recent analysis using older transaction data, and so our trend variable may not reflect methodological trends well in that case.

[17] As a robustness check, we estimate the corresponding RE panel models and find virtually identical results (see Appendix C). When benefit transfer is the primary objective, Boyle and Wooldridge (2018) suggest an alternative model first proposed by Mundlak (1978). The Mundlak model parametrically estimates the cluster-specific effects by including the cluster average of the relevant modifier variables in the right-hand side of the meta-regression. This model slightly relaxes the assumptions needed for consistent estimates from a RE panel model. It also has some advantages compared to a FE panel model because it does not disregard variation with respect to cluster-invariant variables and allows for out-of-sample inference (Boyle and Wooldridge 2018). In earlier versions of this meta-analysis, we estimated a series of meta-regressions following the Mundlak approach (Guignet et al. 2020), but we do not pursue these models in the current study for two reasons. First, the WLS models performed better when assessing out-of-sample transfer error. Second, few of the right-hand-side variables vary within a cluster, and those that do are mainly methodological variables. Therefore, the parametrically estimated cluster-specific effects based on cluster means would capture methodological choices and not cluster-specific effects associated with a particular housing market or waterbody, which is the primary interest for purposes of benefit transfer.

4. Results

Meta-regression Results

We first estimate the WLS meta-regressions using our VAC weights, which account for the relative statistical precision within each cluster, but we ultimately treat estimates for each housing market and waterbody as a unique and unbiased glimpse into how water clarity affects home prices in that area, regardless of the relative precision of the underlying estimates across clusters. Recognizing that such an interpretation might be overly naive and runs counter to conventional meta-analysis (Nelson and Kennedy 2009; Borenstein et al. 2010; Nelson 2015), we reestimate the models using the proposed RESCA weights, which discount the cluster as a whole if its elasticities are estimated with relatively less precision. The WLS meta-regression models are all estimated using cluster-robust standard errors, where the clusters are defined according to the 66 unique study-housing market combinations.

The VAC WLS results are presented in Table 3. Model 1 is our base meta-regression and includes only a constant term, an indicator of whether an elasticity estimate corresponds to waterfront homes, and the variance of that estimate to control for publication bias. The positive and statistically significant constant term suggests that, on average, a 1% increase

Table 3
Variance-Adjusted Cluster-Weighted Least Squares Meta-regression Results

Variable	(1)	(2)	(3)	(4)	(5)	(6)
Waterfront[a]	0.1457***	0.0170	−0.0677	0.1010**	−0.1126	0.1251***
	(0.046)	(0.023)	(0.052)	(0.039)	(0.086)	(0.042)
Midwest[a]		−0.1056	−0.1488*	−0.2059	−0.2561***	−0.3156***
		(0.087)	(0.082)	(0.132)	(0.096)	(0.116)
South[a]		−0.2248***	−0.2797***	−0.3209*	−0.2889***	−0.3945***
		(0.076)	(0.084)	(0.171)	(0.108)	(0.131)
West[a]		−0.1803	−0.1803	−0.1747	−0.3745**	−0.3772**
		(0.120)	(0.121)	(0.147)	(0.180)	(0.174)
Estuary[a]			−0.0401**		−0.1223***	
			(0.016)		(0.039)	
Waterfront × estuary			0.1274**		0.1721*	
			(0.055)		(0.088)	
Mean clarity				0.0375		0.0799**
				(0.030)		(0.035)
Waterfront × mean clarity				−0.0684**		−0.1061**
				(0.029)		(0.050)
Elasticity variance	8.97E-07***	6.26E-07**	6.26E-07**	5.10E-07	6.02E-07**	5.02E-07
	(1.52E-07)	(2.56E-07)	(2.57E-07)	(3.30E-07)	(2.56E-07)	(3.65E-07)
Time trend					0.0183***	0.0223***
					(0.006)	(0.006)
Linear-log[a]					0.1973	0.1987
					(0.161)	(0.159)
Linear[a]					0.2198**	0.1159
					(0.086)	(0.083)
Log-linear[a]					−0.0020	−0.0059
					(0.008)	(0.008)
Constant	0.0414*	0.2474***	0.3320***	0.3092*	0.1681	0.0493
	(0.021)	(0.077)	(0.090)	(0.182)	(0.122)	(0.180)
Observations	260	260	260	260	260	260
Adjusted R-squared	0.080	0.148	0.148	0.156	0.170	0.176

Note: The dependent variable is the home price elasticity with respect to water clarity (Secchi disk depth). Clustered-robust standard errors are in parentheses and are clustered according to the $K = 66$ study-housing market combinations. Weighted least squares regressions are estimated using the "regress" routine in Stata 16 and defining analytical weights equal to the variance-adjusted cluster weights (see equation [3]).
[a]Independent variables that are dummy variables.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

in water clarity leads to a 0.04% increase in the price of nonwaterfront homes within 500 m of the waterbody. As expected, the price elasticity with respect to clearer waters is significantly higher among waterfront homes (by 0.1457 percentage points). Together these estimates suggest that, on average, a 1% increase in water clarity leads to a 0.19% increase in waterfront home prices. The statistically significant coefficient corresponding to elasticity variance suggests that publication bias is a concern, but setting this variable to zero when predicting values for function transfers controls for such bias (Stanley and Doucouliagos 2012).

Model 2 in Table 3 includes indicator variables denoting the four regions of the United States, with the Northeast being the omitted category. The negative region coefficients suggest that housing price elasticities with respect to water clarity in the Midwest, South, and West tend to be less than those in the Northeast, but such differences in this model are only significant in the South. For example, a 1% increase in water clarity in the Northeast would lead to an average increase in value of 0.27% for waterfront homes and 0.25% for nonwaterfront homes within 500 m. In the South, the results suggest that a 1% increase in clarity corresponds to much smaller but still significant 0.04% and 0.02% increases in price among waterfront and nonwaterfront homes. Considering the average house price of $211,314, this suggests that a

1% increase in water clarity (an average of 2.34 cm or just under 1 in.) would increase the value of a waterfront and nonwaterfront home in the Northeast by \$559 (p = 0.000) and \$522 (p = 0.001), respectively. This same improvement for otherwise similar waterfront and nonwaterfront homes in the South would be only \$84 (p = 0.061) and \$48 (p = 0.041).

Models 3 and 4 assess potential heterogeneity in the housing price effects based on characteristics of the environmental commodity, in this case, the type of waterbody and whether the waters are already relatively clear. Model 3 includes an indicator denoting whether the elasticity estimates correspond to an estuary (as opposed to a freshwater lake or reservoir) as well as a corresponding interaction term with the waterfront indicator. The negative and statistically significant coefficient on *estuary* suggests that an increase in water clarity has a smaller effect on the price of homes near an estuary, compared with an increase in lake water clarity. Such a finding seems reasonable given that surrounding residents may not generally expect the water to be clear in estuaries because brackish waters are often naturally opaque. However, the opposite is found among waterfront homes, as suggested by the positive and statistically significant 0.0873 (p = 0.049) sum of the *estuary* and *waterfront* × *estuary* coefficients.

To better illustrate the implications of model 3, consider a 1% increase in clarity in an estuary in the Northeast. This would lead to a 0.35% increase in the value of a bayfront home and a 0.29% increase in the value of a nonbayfront home that was still within 500 m of the estuary. In contrast, the same 1% increase in water clarity would lead to a 0.26% increase among lakefront homes in the Northeast and a larger 0.33% increase among nonlakefront properties that are within 500 m. Although it may seem counterintuitive that the elasticity among lakefront properties is less, this does not necessarily violate the intuition that those closest to the resource should hold a higher value for an improvement. Baseline house values tend to be much larger among waterfront homes, so a smaller elasticity does not necessarily translate to a smaller marginal implicit price. For example, Walsh, Milon, and Scrogin (2011) report a mean sale price of \$452,646 and \$199,982 (2002\$) among lakefront and nonlakefront homes, respectively. Applying these mean values to the aforementioned elasticities suggests an implicit price for a 1% increase in clarity of \$1,197 for lakefront homes and \$664 for nonlakefront homes.

Model 4 explores whether the house price elasticity with respect to clearer waters tends to be systematically different depending on baseline water clarity.[18] The model includes the mean clarity level corresponding to each primary study estimate and an interaction term between mean clarity and the waterfront indicator. The main effect is statistically insignificant, as is the sum of the main effect and the coefficient on the *waterfront* × *mean clarity* interaction term. This suggests that the price elasticity for nonwaterfront and waterfront homes, respectively, does not vary with baseline water clarity.

In subsequent models not reported here, we find little evidence of systematic heterogeneity with respect to the other study area and commodity variables reported in Table 2, including household median income, percent of population with a college degree, mean house prices, and waterbody size. Although mean house prices and waterbody characteristics are taken from the primary studies, the lack of significant findings pertaining to household median income, education, and other census-derived sociodemographics may be partly attributed to the spatial coarseness of the county- or state-level variables and the resulting measurement error.

Models 5 and 6 build on the previous two models by controlling for methodological characteristics of the primary studies. The time trend variable is added, as well as indicators denoting the functional form of the hedonic price function that was assumed by the primary study authors (double-log is the omitted category). The positive and significant time trend suggests that, all else constant, the elasticity estimates have been increasing over time. The specification indicators are largely insignificant, with the exception of *linear* in model 5. This suggests

[18] Because of collinearity concerns, we do not account for both waterbody type and baseline water clarity in the same model.

that assuming a linear hedonic price function tends to yield higher elasticity estimates. This result is not robust to model 6, and there is otherwise little evidence that functional form assumptions yield significant differences in the elasticity estimates.

Including these methodological variables, particularly the time trend, strengthens the earlier findings pertaining to the study area and waterbody characteristics. All regional indicators are now negative and statistically significant, demonstrating that the price effects with respect to water clarity in other regions tend to be lower, compared with the Northeast. The negative coefficient on *estuary* is now larger in magnitude, suggesting that at least among nonwaterfront homes, water clarity in estuaries tends to have a lesser effect on house values. In model 6, the positive and now significant 0.0799 coefficient corresponding to mean water clarity implies that nonwaterfront homes surrounding waterbodies with already relatively clear waters experience larger increases in value in response to further improvements. This "pristine premium" seems to be isolated to nonwaterfront homes, however. The sum of the *mean clarity* and *waterfront* × *mean clarity* coefficients is insignificant, suggesting that among waterfront homes, the baseline average clarity levels are not associated with systematically higher or lower price effects due to further improvements.

Although not reported here, models including the other methodological variables in Table 2 revealed statistically insignificant effects (e.g., length of the study period, used assessed values, was unpublished, used water clarity data other than in situ measurements, and controlled for other measures of water or ecological quality). In particular, we find no significant differences in price elasticity estimates when the primary studies included spatial fixed effects, spatial autoregressive (SAR) models, or accounted for spatial autocorrelation using a formal spatial error model (LeSage and Pace 2009) or by allowing for geographically clustered standard errors.

We reestimate the six WLS meta-regressions using the proposed RESCA weights. The results are presented in Table 4 and are generally similar to the VAC WLS models. One notable difference is that the coefficient corresponding to the elasticity variance term is no longer significant in any of the six RESCA WLS models. This suggests that after accounting for relative statistical precision both within and across clusters, selection bias is no longer a concern. The RESCA WLS models also tend to predict slightly lower elasticity estimates. For example, model 1 in Table 4 predicts an elasticity of 0.1085 and 0.0257 among waterfront and nonwaterfront homes, respectively, compared with the 0.1871 and 0.0414 elasticity estimates from the corresponding VAC WLS model in Table 3.

To demonstrate how the meta-regression results can be used for function-based transfers, consider one of the most comprehensive meta-regressions, model 6 from Table 4.[19] For this illustration, we predict the elasticity estimates by plugging in the cluster-weighted mean values for the study area and waterbody characteristics,[20] but practitioners should plug in values specific to their policy site. In cases where best practices are clearly discerned, the corresponding values for methodological variables should be used when predicting for benefit transfer (Boyle and Wooldridge 2018). Following this guidance, we set the linear specification indicator to zero. Economic theory and simulation evidence suggest that assuming a linear hedonic price function is generally inappropriate (Bishop et al. 2020; Bockstael and McConnell 2006). A similar motivation lends itself to setting the elasticity variance to zero for our illustrative elasticity calculation (Stanley and Doucouliagos 2012). In contrast, among the remaining specifications observed in the metadata (double-log, linear-log, and log-linear), the most appropriate functional form is unclear. If there are no clear best practices for a methodological variable, then Boyle and Wooldridge (2018) suggest using the average value across the literature.[21] We plug in the unweighted sample

[19] Step-by-step guidance for a similar benefit transfer exercise is provided in Appendix D.2.

[20] See Appendix D.1 for the cluster-weighted mean values of all covariates.

[21] In the case of a linear meta-regression, this approach of plugging in the sample means is similar to a more generalizable procedure proposed by Moeltner, Boyle, and Paterson

Table 4
RES Cluster-Adjusted Weighted Least Squares Meta-regression Results

Variable	(1)	(2)	(3)	(4)	(5)	(6)
Waterfront[a]	0.0828***	0.0374*	−0.0356	0.0715*	0.0080	0.0829**
	(0.018)	(0.022)	(0.050)	(0.036)	(0.044)	(0.031)
Midwest[a]		−0.0204	−0.0462	−0.0475	−0.1565***	−0.1476***
		(0.040)	(0.036)	(0.048)	(0.032)	(0.039)
South[a]		−0.0833***	−0.1451***	−0.1096	−0.2600***	−0.2495***
		(0.031)	(0.043)	(0.081)	(0.037)	(0.044)
West[a]		−0.0607	−0.0607	−0.0595	−0.3200***	−0.4216***
		(0.097)	(0.098)	(0.107)	(0.111)	(0.077)
Estuary[a]			−0.0181		−0.0534***	
			(0.020)		(0.020)	
Waterfront × estuary			0.1019*		0.0582	
			(0.055)		(0.050)	
Mean clarity				0.0247		0.0601***
				(0.037)		(0.022)
Waterfront × mean clarity				−0.0332		−0.0317
				(0.032)		(0.024)
Elasticity variance	2.22E-05	2.05E-05	2.01E-05	2.01E-05	1.91E-05	1.86E-05
	(2.18E-05)	(2.04E-05)	(2.01E-05)	(2.01E-05)	(1.93E-05)	(1.89E-05)
Time trend					0.0121***	0.0158***
					(0.002)	(0.002)
Linear-log[a]					−0.0371	−0.0953*
					(0.040)	(0.049)
Linear[a]					0.0807	0.0493
					(0.091)	(0.052)
Log-linear[a]					−0.0023	−0.0001
					(0.005)	(0.005)
Constant	0.0257	0.1025***	0.1755***	0.1086	0.1577***	0.0034
	(0.016)	(0.031)	(0.055)	(0.098)	(0.034)	(0.063)
Observations	260	260	260	260	260	260
Adjusted R-squared	0.101	0.146	0.159	0.148	0.201	0.222

Note: The dependent variable is the home price elasticity with respect to water clarity (Secchi disk depth). Clustered-robust standard errors are in parentheses and are clustered according to the $K = 66$ study-housing market combinations. Weighted least squares regressions are estimated using the "regress" routine in Stata 16 and defining the analytical weights equal to the random effect size (RES) cluster-adjusted weights (see Section 2).
[a]Independent variables that are dummy variables.
* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

proportions across the remaining three specification indicators. More specifically, among the remaining 250 observations that used one of these specifications to estimate the price elasticity with respect to water clarity, we see that 45% were based on a double-log (the omitted category), 32% on a linear-log, and 23% on a log-linear specification. To infer an elasticity that is based on the most recent methods and data possible, the value for the time trend index is set to 20 (which corresponds to 2014, the most recent year observed in the metadata).

This illustrative exercise yields an "average" elasticity for waterfront homes of 0.2698, suggesting that a 1% increase in Secchi disk depth (an increase of 2.34cm, on average) leads to an average increase in waterfront home values of 0.27% ($p = 0.000$). A slightly smaller 0.2564 elasticity is estimated for the "average" nonwaterfront home ($p = 0.000$). Based on the average home price from Table 2, these results translate to a mean implicit price of \$570 ($p = 0.000$) and \$542 ($p = 0.000$) for waterfront and nonwaterfront homes, respectively. Overall, the literature yields plausible and statistically significant

(2007), where the meta-analyst identifies the set of all possible combinations of methodological variable values, assigns a probability to each, and then predicts the benefit transfer estimates for each methodological variable combination and takes the average across all combinations.

estimates of how water clarity is capitalized in surrounding home values, even after empirically controlling for key dimensions of heterogeneity, publication bias, and methodological assumptions.

Best-Performing Model for Benefit Transfer

To examine out-of-sample transfer error and assess the performance of the various models and weighting schemes, we iteratively leave out observations corresponding to the 66 housing market study clusters, and then reestimate the mean unit values and meta-regression models using the remaining sample. The predicted elasticities are then estimated for the excluded cluster. This is repeated by excluding the 66 clusters one at a time. After completing all 66 iterations, we calculate the median absolute transfer error. Similar out-of-sample transfer error exercises have been implemented in the literature (e.g., Lindhjem and Navrud 2008; Stapler and Johnston 2009).

We conduct this out-of-sample transfer exercise in two ways. In the first approach, we construct a synthetic observation for each distance bin d in cluster j and then compare the elasticity value for this synthetic observation to the predicted elasticity from the meta-regression models. The synthetic observation is constructed using the same VAC weights (i.e., an inverse variance weighted mean across all elasticity estimates for distance bin d in cluster j):

$$\hat{\varepsilon}^{s}_{dj} = \sum_{i=1}^{k_{dj}} \left(\frac{\frac{1}{v_{idj}}}{\sum_{i=1}^{k_{dj}} \frac{1}{v_{idj}}} \; \hat{\varepsilon}_{idj} \right).$$

The corresponding right-hand-side variables for the synthetic observations are calculated in the same fashion. Those variable values are then plugged into the estimated meta-regressions to yield a predicted elasticity $\hat{\hat{\varepsilon}}^{s}_{dj}$, which is compared to the "actual" elasticity for each synthetic observation $\hat{\varepsilon}^{s}_{dj}$. A similar exercise is done using the mean unit values, where the waterfront or nonwaterfront means are calculated each iteration, and then used as the unit value prediction $\hat{\hat{\varepsilon}}^{s}_{dj}$ for the excluded observations. The transfer error is calculated as the absolute value of the percent difference:

$$\left|\%TE_{dj}\right| = \left| \left(\frac{\hat{\hat{\varepsilon}}^{s}_{dj} - \hat{\varepsilon}^{s}_{dj}}{\hat{\varepsilon}^{s}_{dj}} \right) \times 100 \right|. \qquad [7]$$

Our synthetic observation approach for measuring out-of-sample transfer error weights the "actual" observed elasticity estimates and the sample used to parameterize the meta-regression models in the same way. When dealing with a panel- or cluster-structured meta-dataset, the more common practice of comparing predicted and observed elasticity estimates for all left-out observations within each iteration (e.g., Londoño and Johnston 2012; Fitzpatrick, Parmeter, and Agar 2017; Subroy et al. 2019) potentially inflates the transfer error. The parameterized meta-regressions, and hence the predicted elasticities $\hat{\hat{\varepsilon}}_{idj}$, would discount less precise estimates, but the excluded elasticity observations $\hat{\varepsilon}_{idj}$ that these are compared with in each iteration would all be treated equally when assessing the transfer error. This inconsistent weighting across the predicted and observed elasticities automatically puts the predictive performance of the meta-regression models at a disadvantage. Nonetheless, we carry out our transfer error exercise using this conventional approach and find similar results.

The median absolute transfer error results for each model and weighting scheme are presented in Table 5. The top panel shows the median transfer errors using our out-of-sample synthetic observation approach. The lower panel shows the median transfer errors when all excluded observations are treated equally and used for comparison. The results suggest a median absolute transfer error of 76%–119% under the synthetic observation comparison versus 83%–131% when comparing all excluded observations.

Although errors of this size are not unheard of, the transfer errors for this study are in the high range. Kaul et al. (2013) examined 1,071 transfer errors reported by 31 studies and report that the absolute value of the transfer errors ranged from 0% to 7,496%, with a median of 39%. Rosenberger (2015) summarized the results for 38 studies that statistically

Table 5
Out-of-Sample Transfer Error: Median Absolute Value of the Percent Difference in Predicted Elasticities

Weighing Scheme	Weighted Mean	WLS 1	WLS 2	WLS 3	WLS 4	WLS 5	WLS 6
Comparison with Synthetic Observations for Excluded Cluster (n=85)							
Variance-adjusted cluster (%)	92.5	90.5	87.0	108.1	93.9	119.0	104.2
RES cluster-adjusted (%)	76.0	76.3	82.7	89.4	89.4	90.7	78.9
Comparison with Excluded Cluster Observations (n=260)							
Variance-adjusted cluster (%)	130.5	127.5	89.8	100.5	99.9	120.3	121.4
RES cluster-adjusted (%)	82.7	83.4	83.1	89.5	90.4	93.7	87.2

Note: The out-of-sample transfer error is calculated by iteratively leaving out sets of observations pertaining to each of the $K = 66$ clusters, estimating the model with the remaining clusters' observations, and calculating the predicted elasticities and resulting transfer error for the synthetic observation or the actual observations corresponding to the excluded cluster. RES, random effect size; WLS, weighted least squares.

analyzed transfer errors and reported a median transfer error of 36% for function transfers. In their leave-one-study-out transfer error analysis, Londoño and Johnston (2012) report a 59% median transfer error using all available studies. Similar to our study, Subroy et al. (2019) used a leave-one-cluster-out approach and estimated a median transfer error of 21% for nonmarket values of threatened species.

Overall, considering the unit value transfers, all meta-regression models and weighting schemes, and both sets of out-of-sample comparisons, we find that the RESCA weighted models outperform the VAC weighted models. This is reasonable given that the RESCA weighting scheme gives less weight to imprecise elasticity observations, relative to other estimates both within and across markets and studies, whereas under the VAC weighting scheme, only within-cluster relative precision is considered. The VAC weighting scheme is more sensitive to less precise, possibly outlying elasticity estimates, because even if all elasticity values for a particular housing market and waterbody are imprecisely estimated, the cluster is still given the same overall weight as any other market and waterbody examined in the literature.

Among the RESCA-weighted estimates, the simplest unit value transfer and model 1 yield the lowest out-of-sample transfer error. Both the mean unit values and model 1 account for differences across waterfront and nonwaterfront homes, and model 1 adjusts for publication bias. Otherwise, these simple transfers do not account for any form of heterogeneity across study areas and the waterbodies being analyzed or any methodological choices made in the primary studies. Although simpler transfers have been found to perform better in some contexts (Barton 2002; Lindhjem and Navrud 2008; Johnston and Duke 2010; Bateman et al. 2011; Klemick et al. 2018), it is surprising that accounting for such (often statistically significant) heterogeneity does not improve transfer performance. In fact, Table 5 suggests that transfer performance generally decreases with model complexity. The one exception is RESCA model 6, which accounts for heterogeneity in baseline water clarity, study attributes, and across regions. Model 6 using the RESCA weights yields a median out-of-sample transfer error of 79% under our preferred synthetic observation comparison. This is just slightly worse than the 76% transfer error from the RESCA weighted unit value transfer or simple function transfer using model 1.

When using our meta-analysis results for benefit transfer, practitioners should balance the findings of our out-of-sample transfer error exercise against the potential need to account for heterogeneity across markets and the environmental commodity. Based on these considerations, we recommend function transfers based on the RESCA weighted models 1 and 6.[22] The most accurate benefit transfer approach, however, may well be case-specific.

[22] One can use the coefficient estimates in Table 4 for benefit transfer. The full variance-covariance matrix for RESCA WLS models 1 and 6 are presented in Appendix E. These are needed to derive the corresponding confidence intervals via the delta method (Greene 2003, 70) or Monte Carlo simulations. An illustrative step-by-step benefit transfer example based on RESCA WLS model 6 is provided in Appendix D.2.

When a specific policy context is in mind and resources are available, researchers should consider using our meta-dataset directly to tailor the set of studies to their particular context and conduct their own meta-analysis. One could simply compare the most relevant characteristics across the study and policy sites or pursue more sophisticated model search algorithms to identify the optimal subset of meta-observations to inform benefit transfer to a specific context (Moeltner and Rosenberger 2008, 2014; Johnston and Moeltner 2014; Moeltner et al. 2019).

5. Discussion

A primary objective of this study is to help practitioners make use of the large body of literature of hedonic property value studies examining surface water quality and ultimately facilitate ex ante and ex post assessments to better inform local, regional, and national policies. Based on the constructed meta-dataset, limited unit value transfers could be conducted to assess policies affecting one of several different water quality measures (e.g., chlorophyll a and fecal coliform). Given the limited number of studies on any one water quality measure, unit value transfers are often the only viable option. In the context of water clarity, a function transfer using meta-regression results may improve accuracy by catering the estimates to a particular policy and by adjusting for best methodological practices and publication bias. Although statistically significant heterogeneity in the property price effects of water clarity is identified, we find that accounting for such heterogeneity did not improve transfer performance.

As with any benefit transfer exercise, our results must be given appropriate caveats. The median out-of-sample transfer errors of even our best-performing unit value or function transfers are among the upper end of errors found in the meta-analytic literature valuing environmental commodities. Examining the distributions of transfer errors revealed no evidence of better transfer performance for different regions or types of waterbodies. The capitalization of water quality changes in surrounding housing values is a very local phenomenon. Surely local unobserved factors remain that affect the accuracy of any transferred estimates, at least in this general setting.[23]

In addition, when using reduced-form meta-regression models like ours for benefit transfer, one must consider the trade-offs between a potentially better model fit from a reduced-form specification versus the theoretical consistency of a more structural meta-regression model (Newbold et al. 2018b; Johnston and Bauer 2020). In the context of stated preference studies, Newbold et al. (2018b) and Moeltner (2019) have argued for more theoretically consistent meta-regression models, in particular for models that satisfy the adding-up condition (Diamond 1996). Formal incorporation of the results from the hedonic property value literature in benefit-cost analysis is a broader topic in need of research. Much of the applied hedonic literature, including all of the estimates in our meta-dataset, are of marginal price effects based on regressions of Rosen's (1974) first-stage hedonic model. As such, a welfare interpretation generally only holds at the margin (Kuminoff and Pope 2014). Although advancements have been made to infer formal nonmarginal welfare measures (e.g., Bartik 1987; Zabel and Keil 2000; Ekeland, Heckman, and Nesheim 2004; Bajari and Benkard 2005; Zhang, Boyle, and Kuminoff 2015; Bishop and Timmins 2018, 2019; Banzhaf 2020, 2021), such methods are not widely applied, and a commonly agreed-on "best" approach remains an open question (Bishop et al. 2020).

Nonetheless, our meta-analysis results, or the results of subsequent case-specific meta-analyses using our meta-dataset, can be combined with spatially explicit data of the relevant surface waterbodies, housing locations, baseline housing values, and the number of homes to project the total capitalization effects of a policy affecting water quality.

[23] In future work, with specific policy applications in mind, it may prove fruitful to identify the optimal scope (i.e., the subset of meta-observations that should be used for benefit transfer estimates) (Moeltner and Rosenberger 2008, 2014; Johnston and Moeltner 2014; Moeltner et al. 2019). The optimal subset of the metadata can be identified using model search algorithms (e.g., Moeltner 2019) but will vary across policy contexts.

Ideally, such a benefit transfer exercise can be carried out using detailed, high-resolution data on waterbodies and individual residential properties. In the absence of such data, one can combine our results with publicly available waterbody quality and location data provided by the National Water Quality Monitoring Council's Water Quality Portal and the National Hydrography Dataset, along with aggregated data on housing and land cover, from the U.S. Census Bureau and National Land Cover Dataset, for example.[24]

Our metadata development and meta-analysis can complement benefit transfer efforts based on stated preference studies, which are the workhorse for benefit analyses of federal water quality policies (Griffiths et al. 2012; U.S. EPA 2015; Corona et al. 2020). In fact, colleagues at the U.S. EPA plan to incorporate our meta-analysis in an integrated assessment model called the Benefits Spatial Platform for Aggregating Socioeconomics and H_2O Quality (or BenSPLASH), which is designed as a flexible, modular tool for water quality benefits estimation (Corona et al. 2020). Although stated preference methods are generally more comprehensive in capturing total values, hedonic studies provide a revealed preference estimate that circumvents concerns related to the use of stated values based on hypothetical scenarios.[25]

In future work, we hope to expand our meta-dataset in two ways. First, for tractability we decided early in the development of the meta-dataset to limit the distance bins to waterfront homes and nonwaterfront homes within 500 m of a waterbody. Based on our review of the literature, this seemed reasonable, although some studies are finding price effects farther away (Walsh, Milon, and Scrogin 2011; Netusil, Kincaid, and Chang 2014; Klemick et al. 2018; Kung, Guignet, and Walsh 2022). Adding meta-observations that pertain to farther distance bins will provide a more comprehensive meta-analysis in the future (but one must still consider the sample selection concerns discussed in Section 2).

Second, new studies should be periodically added to the meta-dataset. When conducting new hedonic studies, we encourage researchers to consider some of the gaps in the literature. Our review reveals limitations in the types of waterbodies and geographic areas covered. More hedonic studies examining surface water quality in the mountain states in the West, parts of the Midwest, and the South-Central portions of the United States are needed, as are studies examining how property values respond to water quality changes in estuaries, rivers, and streams. Such primary studies will facilitate nationwide coverage and ultimately more accurate benefit transfers.

Finally, our review highlights a disconnect between the water quality metrics used by economists and those by water quality modelers and policy makers. Water clarity is the most common metric in the hedonic literature. It is a convenient measure for nonmarket valuation because households are able to directly observe it. In certain cases, it also acts as a reasonable proxy for other measures of water quality (e.g., nutrients or sediments). Even so, water clarity is not a good measure of quality in all contexts (Keeler et al. 2012). For example, waters with low pH levels due to acid rain or acid mine drainage may be very clear but of poor quality. This disconnect between water clarity and quality is an issue in the nonmarket valuation literature more broadly (Abt Associates 2016).

Although the majority of hedonic studies focus on water clarity, water quality models such as the Soil and Water Assessment Tool (SWAT), Hydrologic and Water Quality System (HAWQS), and SPAtially Referenced Regressions on Watershed Attributes (SPARROW) tend to focus on changes in nutrients, sediments, metals, dissolved oxygen, and

[24] Website links to these publicly available data sources are as follows: Water Quality Portal, https://www.waterqualitydata.us/; National Hydrography Dataset, https://www.usgs.gov/core-science-systems/ngp/national-hydrography/; U.S. Census Bureau, https://www.census.gov/; National Land Cover Dataset, https://www.mrlc.gov/ (accessed February 20, 2019). The Lake Multiscaled Geospatial and Temporal Database is another useful data source specific to lake water quality, available at https://lagoslakes.org/projects/ (accessed February 10, 2021).

[25] One would not necessarily want to add estimates across these methods because of potential double counting. Although a large portion of total values derived by stated preference studies may reflect nonuse values (e.g., Freeman, Herriges, and Kling 2014; Moore et al. 2018), there is still likely overlap in the endpoints valued; e.g., both could capture use values from waterfront recreation affected by water quality.

organic chemicals (Tetra Tech 2018). There are some process-based water quality models and estimated conversion factors that can be used to calculate changes in Secchi disk depth, but such approaches require location-specific relationships and waterbody characteristics as an input (Hoyer et al. 2002; Wang, Linker, and Batiuk 2013; Park and Clough 2018); thus, deterring the broader application of these existing approaches to project water clarity changes resulting from a policy.

Further research is necessary to improve the link between water quality and economic models and ultimately better inform policy. Closing this gap can entail one of two things, or some combination of both. First, when choosing the appropriate water quality metric, economists should keep the application of their results in mind. Doing so will allow economic results to be more readily used to monetize the quantified policy changes projected by water quality models. Second, water quality modelers could develop models that directly project changes in water clarity or perhaps develop more robust conversion factors. Such a call is not a new idea. Desvousges, Naughton, and Parsons (1992, 682) recommended that, at the very least, analyses establish "the correlation between policy variables and variables frequently used as indicators of water quality." Developing such conversion factors would be challenging and would likely need to be watershed-, and perhaps even waterbody-, specific.

6. Conclusion

Despite the large number of studies of the capitalization of surface water quality into home values, this literature has not generally been used to directly inform decision-making in public policy. In fact, hedonic property value studies in general tend to not be quantitatively used in regulatory analyses of regional and nationwide regulations enacted by the U.S. EPA (Petrolia et al. 2021). In the water quality context, heterogeneity in local housing markets, the types of waterbodies examined, the model specifications estimated, and the water quality metrics used are key reasons the results of these local studies have not been applied to broader policies. This meta-analysis overcame these obstacles through the meticulous development of a detailed and comprehensive meta-dataset.

The relative out-of-sample transfer performance of our reduced-form meta-regression models suggests caution when conducting benefit transfers. The proper use of our results will depend on the relative accuracy necessary for decision-making (Bergstrom and Taylor 2006). Nonetheless, in the absence of resources for an original study, this meta-dataset and meta-analysis provide a path for practitioners to conduct benefit transfer and assess how improvements in water quality from local, regional, and even national policies are capitalized into housing values.

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