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The relationship between urban forests and income: A meta-analysis

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Abstract

Urban trees provide substantial public health and public environmental benefits. However, scholarly works suggest that urban trees may be unequally distributed among poor and minority urban communities, meaning that these communities are potentially being deprived of public environmental benefits, a form of environmental injustice. The evidence of this problem is not uniform however, and evidence of inequity varies in size and significance across studies. This variation in results suggests the need for a research synthesis and meta-analysis.

We employed a systematic literature search to identify original studies which examined the relationship between urban forest cover and income (n=61) and coded each effect size (n=332). We used meta-analytic techniques to estimate the average (unconditional) relationship between urban forest cover and income and to estimate the impact that methodological choices, measurement, publication characteristics, and study site characteristics had on the magnitude of that relationship. We leveraged variation in study methodology to evaluate the extent to which results were sensitive to methodological choices often debated in the geographic and environmental justice literature but not yet evaluated in environmental amenities research.

We found evidence of income-based inequity in urban forest cover (unconditional mean effect size = 0.098; s.e. = .017) that was robust across most measurement and methodological strategies in original studies and results did not differ systematically with study site characteristics. Studies that controlled for spatial autocorrelation, a violation of independent errors, found evidence of substantially less urban forest inequity; future research in this area should test and correct for spatial autocorrelation.

Keywords

Meta-analysis; urban forests; environmental inequity; environmental justice

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1. Introduction

Traditionally, quantitative environmental justice research has been concerned with the extent to which low-income and minority communities are exposed to environmental hazards and lack access to environmental amenities. As research increasingly considers the causes of and potential remedies for environmental inequity, important questions remain about the size and scope of the problem itself. While several reviews have been conducted of the environmental hazards literature (Ringquist, 2005; Mohai, Pellow, & Roberts, 2009), little synthesis has been conducted on the distribution of environmental amenities. We conducted a systematic review and meta-analysis of an important environmental amenity, urban forest cover, and its relationship to income.

Urban forests—the land in and around areas of intensive human influence which is occupied by trees and associated natural resources (definition modified from Strom, 2007)—provide many environmental and health benefits to urban residents (Rosenzweig et al., 2006; Kuo, 2001; Donovan & Butry, 2010). Over the last several decades, studies have considered the empirical distribution of the urban forest with respect to an array of socioeconomic characteristics. Findings across studies have been mixed with respect to income; most studies find a positive relationship between urban forest cover and income (Danford et al., 2014; Heynen et al., 2006; Landry & Chakraborty, 2009; Locke & Grove, 2014; Pham, Apparicio, Séguin, Landry, & Gagnon, 2012; Schwarz et al., 2015) but there are some exceptions (Pham, Apparicio, Landry, et al., 2013; Grove, Locke, & O'Neil-Dunne, 2014).

While some evidence suggests income-based urban forest inequity exists, the magnitude of estimates varies across studies and the average magnitude is unknown. Moreover, the city-specific nature of previous research and variation in methodological choices across studies raise questions about the source of differences—does variation in results reflect real differences between study sites, or is it a product of methodological choices? Answering these questions will inform current research on the drivers of urban forest cover inequity, methodological choices in environmental justice research, and ongoing efforts to increase forest cover in cities around the world (McPherson & Young, 2010). This project aggregated information from many city-specific studies to estimate the average effect size (average relationship) between urban forest cover and income. A companion paper examined the relationship between urban forest cover and race [redacted]. Our analysis also examined potential explanations for variation across studies by controlling for characteristics of the original studies such as their empirical strategies and study location. By quantifying the findings from the relevant literature, meta-analysis yields a more complete summary of the state of our collective knowledge as compared to a systematic review.

Meta-analysis is particularly useful in the case of urban forest equity because it synthesizes several literatures that might not normally interact. In addition to studies that are explicitly concerned with environmental justice and inequity, there are many studies that estimate the same relationship to achieve other research objectives, including to evaluate competing theories about drivers of urban land use (Boone, Cadenasso, Grove, Schwarz, and Buckley, 2010; Grove et al., 2006), draw insights about the choices of private citizens (Pham, Landry, Sequin, & Gagnon, 2013; Grove, Locke, & O'Neil-Dunne, 2014) or public servants (Landry

& Chakraborty, 2009), and improve urban forest cover measurement (Szantoi et al., 2008). This study diversity gave us a unique ability to evaluate the sensitivity of study results to methodological choices, a concern articulated by environmental justice scholars but not yet evaluated with respect to environmental amenities.

A note on terminology: scholars define and measure both urban forest cover and income in numerous ways. For example, some scholars include herbaceous cover (grass and shrubs) in their measure of urban forest cover, while others limit their measure strictly to trees or tree cover. For simplicity, we use *urban forest cover* as a catch-all term for our outcome of interest. We use *income* to describe measures of financial resources, including poverty rates or "high poverty" dichotomous indicators.

The remainder of this paper is organized as follows: the next section discusses our data collection process, including the literature search process and the inclusion criteria. We then discuss reasons that urban forest cover inequity may vary across studies according to the literature. We then discuss our coding of effect sizes and relevant covariates and the meta-analytic methods used in this analysis (forest plots and meta-regression). We then report results, discuss their implications for research and urban forest policy, and conclude.

2. Literature Search and Inclusion Criteria

2.1 Literature Search

We implemented this meta-analysis as outlined by Ringquist (2013) and Borenstein and colleagues (2009). First, in the scoping stage we refined and operationalized our research question and identified the focal predictors (see inclusion criteria below). Second, we populated a complete list of acceptable measures of the dependent variable (i.e. urban forest cover) and generated coding instruments.

We then conducted a systematic and exhaustive search of the existing literature to identify all original studies that have empirically tested the relationship between urban forest cover and sociodemographic characteristics. To identify appropriate studies, we identified (1) a set of search terms that would yield original studies that met our inclusion criteria and (2) relevant repositories that would contain original studies. In each repository, we conducted the same set of 16 unique searches—each search included the word "urban," one of four search terms related to the dependent variable urban forest cover, and one of four terms related to the distribution of that forest cover. The four dependent variable search terms were "*tree cover,*" *canopy*, *forest*, and *vegetation*. The four other terms were *socioeconomic*, *demographic*, *distribution*, and *equity*. We conducted these searches in the following academic research databases: Academic Search Premier, American Psychological Association (APA) PsycNET, Google Scholar, Google Books, JSTOR, National Bureau of Economic Research (NBER), ProQuest Dissertations and Theses Database (PQDT), Social Science Research Network (SSRN), and the local version of WorldCat (USDCat) for all articles and then again for books only.

Each search permutation (e.g. *urban "tree cover"* by *socioeconomic* using Google Scholar as the search engine) returned several study results, termed "hits." Using the title alone, we evaluated whether the study was *potentially relevant* by determining whether the study could satisfy our inclusion criteria (see below). If we determined from the title that a study was potentially relevant, we read the abstract. Using the title and abstract we determined whether each potentially relevant study was *relevant*, meaning that study could plausibly meet our inclusion criteria. Finally, if the study was relevant, we read the full text to determine whether it satisfied our inclusion criteria and was *acceptable.* We then coded each acceptable study (see study coding). We completed database searches on October 3, 2016.

In addition to database searches, we employed three strategies to identify all relevant studies, including conference proceedings and presentations, government reports, and white papers. First, we emailed the first three authors of each acceptable study, informed them of our project, noted their acceptable study(ies), and requested any additional relevant published or unpublished studies that they authored. Second, we conducted an ancestry and legacy search for each acceptable study. We reviewed each citation in the study (the study's ancestry) for potentially relevant titles and used Google Scholar's "cited by" function to find studies that had cited the acceptable study (the study's legacy). Finally, we sent a request for studies to subscribers of the Urban Forest Listserv, a listserv that facilitated discussion on theoretical and applied urban forest research (managed by the University of South Florida).

2.2 Inclusion criteria

The inclusion criteria listed non-negotiable requirements for any study to be included in our meta-analysis. If a study met the inclusion criteria, other study variation was tolerated.

The first inclusion criterion was that the outcome measure must have measured urban trees or urban vegetation (which must have included trees and can also include shrubs and grass). Outcome variables were typically the dependent variable in a regression analysis or one of the two variables in a correlation. We excluded studies that used other measures of urban environmental condition, including measures of herbaceous cover (grass and shrubs only), the distribution of parks, and measures of ecosystem services related to urban trees (e.g. atmospheric temperature, carbon storage). We also excluded tree species diversity.

The second inclusion criterion required the focal predictor to be a measure of either absolute or relative income, or race or ethnicity (as mentioned above, the focal predictor race was analyzed in a companion piece, [redacted]). Studies used different measures of income but the most common were median income (70 percent of effect sizes) and poverty rate (20 percent of effect sizes). We would have also included a measure of total wealth, but no such measure was used in any study. We excluded studies that used other socioeconomic proxies for income such as education, property value, or percent renters. We excluded effects that did not measure income independently of other factors. For example, Nielsen's PRIZM neighborhood segmentation data combined a set of neighborhood-level socioeconomic factors into one indicator, from which we could not isolate income.

Third, we included studies only if they had intra-city variation. Studies that exclusively compared urban forest cover between cities without any variation *within* cities were excluded (for example, Heynen & Lindsey, [2003]). We excluded studies without intra-city variation because this would likely mask locally-driven relationships between income and urban forest cover. For the purposes of this analysis, we also excluded any effects for which the independent and dependent variables were measured more than ten years apart (e.g. Locke & Baine, 2015) and any effects for which the dependent variable was a measure of change in the urban forest cover over time. These studies of the antecedents are important but have asked a different research question than most contemporaneous effects studies.

Fourth, to restrict the study to *urban* forests, the study site must have included an urban center. Study areas were often measured as a metropolitan statistical area or county/group of counties in the United States. A study area could, as most studies did, include the outlying suburban or rural areas around a city center. We excluded studies in which the area of interest was a large area such as a watershed, state, or country because the area was not predominantly urban.

Fifth, to quantitatively compare the relationships across studies, each study needed to conduct a statistical test with sufficient reported information to create a meta-analytic measure of effect size. Analysis required a sample size and either a coefficient and standard error, a test statistic value, or a p-value. A wide range of statistics and distributions were acceptable (e.g. t, z, χ^2, or F). In a few cases, authors of an original study performed a statistical test and reported the results of a test statistic and/or p-value, but did not report the sample size. In other cases, authors reported model statistics but no individual coefficients or p-values. In those cases we contacted the author(s) by email requesting missing information. In one case, the authors did not have the original data so could not provide us with sufficient information to include the study (Bigsby, McHale, & Hess 2014). Overall, we found and coded 61 acceptable studies with 332 effect sizes measuring the relationship between urban forest cover and income. Figure 1 summarizes the literature search process. All numbers in Figure 1 include our simultaneous search for the focal predictor race.

3. Explaining Variation in Urban Forest Cover

The relationship between urban forests and income is likely to vary based on an array of factors. After our review of the existing literature (see detailed search methods, above), we grouped hypothesized drivers of urban forest cover into three primary categories: methodological choices, measurement of the outcome and independent variables, and characteristics of the study site. We also examined other factors important to meta-analysis, such as publication status, publication outlet, and study focus. This section discusses how the literature has theorized these factors may impact the relationship between urban forest cover and income and how we coded these factors, typically as binary indicator variables.

3.1 Methodological choices

Meta-analysis is particularly well-suited to weigh-in on ongoing conversations in environmental justice research about the impact of methodology on findings. We examined three of the most commonly debated methodological approaches: accounting for spatial autocorrelation, including control variables (and potential over-controlling), and selecting the level of aggregation for the unit of analysis.

3.1.2 Accounting for spatial autocorrelation—Several scholars, particularly geographers, have argued that adjusting for spatially correlated errors is critical for correctly estimating the relationship between urban forest cover and sociodemographic characteristics (more precisely, for correctly estimating standard errors) (e.g. Schwarz et al., 2015). Tobler's first law of geography— "everything is related to everything else, but near things are more related than distant things"— suggests that neighboring geographic units (e.g. census tracts, parcels) are likely more similar to each other than to more distant geographic units (Chakraborty, 2011). "Neighborhoods" are often chosen based on data availability and reflect political boundaries rather than differences in physical and social space. Systematic similarities between neighboring units, spatial autocorrelation, means that the reported sample size overestimates the actual degree of certainty in estimates, represented by the sample size (and the use of sample size in estimating standard errors) and threatens the independence of observed observations, a key assumption in most statistical tests. As a result, test statistics and type I error rates will be inflated. We expected that studies that accounted for spatial autocorrelation may have reported lower t/Z/F statistics, and thus a smaller effect size. We constructed a variable to indicate whether the study controlled or corrected for spatial autocorrelation using either spatial error or spatial lag methods.

3.1.3 Control variables—Results are also likely to vary with the inclusion of covariates in regression models. It has become standard in the environmental justice literature to control for potential confounders expected to be related to both the outcome of interest and the environmental justice indicator. Inclusion of covariates has been considered an indicator of high study quality in both the environmental literature (Ringquist, 2005) and in the meta-analytic literature (Hedges, Tipton, & Johnson, 2010). We created a variable to indicate if the authors used a (weaker) correlation coefficient or bivariate regression. The base case is the use of multiple regression.

Including control variables allows authors to evaluate competing hypotheses about drivers of inequity. For example, scholars might include indicators of both neighborhood income and racial composition in the same model to tease out which factor has a stronger conditional relationship with neighborhood urban forest inequity. This strategy addresses a classic question in inequities research—whether inequity is a story about economic class, race, or both. Meta-analysis allowed us to evaluate the extent to which these choices effect observations of inequity.

While including control variables can help illuminate relationships, they can also muddle understanding. For example, some studies included both income and other indicators of socioeconomic status such as homeownership rates, home values, and education in the same models. These additional variables plausibly over-controlled for the relationship between income and tree cover and might have led to an underestimate of the "true" relationship between income and urban forest cover. At the very least, correlation between these indicators and income may have introduced multicollinearity which would have widened standard errors. To account for these factors, we coded indicators if there were no controls for race or ethnicity (the focal predictor in our companion paper), density (either population or unit density), housing age, homeownership status or home values, and education. Finally, we included a variable to indicate if the income measure was a measure of relative income (e.g. poverty) rather than absolute income (e.g. median income).

3.1.4 Level of aggregation—Findings from the environmental hazards literature suggest that evidence of inequity can vary with the level of aggregation used in analysis (Baden, Noonan, & Turaga, 2007; Ringquist, 2005). Studies have examined the distribution of urban forest cover using geographic units including census block groups (Schwarz et al., 2015; Landry & Chakraborty, 2009), census tracts (Heynen et al., 2006; Jenerette et al., 2007), plots (Conway & Bourne, 2013), and parks (Martin et al., 2004). Evidence that study results vary with the level of aggregation would be particularly problematic because it would suggest that a seemingly minor choice about aggregation can impact study conclusions through ecological fallacy, where inferences about individuals are made based on the group in which they belong.

The “correct” level of aggregation is unclear. Parcel- or household-level estimation does not suffer from the ecological fallacy. However, because trees provide benefits beyond the property on which they grow, small units of analysis cannot capture neighborhood-level benefits and so do not properly capture environmental amenity “access” ([redacted]). The “noisiness” of parcel-level estimates is also a concern. For example, parcel-level estimates might be more subject to measurement error and are more sensitive to highly-localized built environment constraints (e.g. certain locations, such as corner lots, are particularly suitable for trees). Higher levels of aggregation do not have the same measurement error issues, but variation of income and urban forest cover within a single unit of analysis may obscure a true relationship. To balance these competing interests, we coded whether the unit of analysis was small (a parcel or smaller), medium (U.S. census block or block group equivalent), or large (a U.S. census tract or larger equivalent). For official geographic units outside of the United States, we made coding decisions using the average size of those units. If unit size was unknown (as was the case in some non-U.S. studies), we coded it missing (e.g. Kirkpatrick, Daniels, & Davidson 2011; Hetrick, Chowdhury, Brondizio, & Moran, 2013).

3.2 Measurement

It is of keen interest whether the manner in which the urban forest cover is measured influences the observed relationship with income. Generating estimates of urban forest cover is often time and resource intensive. As a result, few studies have compared results using multiple data collection methods. Meta-analysis can provide insight into whether measurement choices impact measured inequity.

3.2.1 Top Down vs. Bottom Up—Studies have generally taken one of two approaches to measure urban forest cover: top-down strategies that use imagery from above (e.g. satellite imagery, aerial photographs) and bottom-up strategies that measure urban forest cover from the ground (e.g. tree inventories; property owner surveys). Environmental justice studies of urban forest cover have relied on the often unspoken assumption that the expected value of ecosystem services is the same across units of the urban forest. For example, studies that used satellite imagery have relied on the assumption that each square meter of canopy cover provides equal services; tree inventories require similar assumptions about tree stems. These assumptions fail if tree attributes are clustered in space in a way that relates to neighborhood characteristics. For example, it might be the case that there are fewer, but larger and better maintained trees in high-income neighborhoods, and many smaller and unwanted nuisance-trees in low-income neighborhoods. In this case, low income neighborhoods would have higher stem counts but lower canopy cover and we would observe less inequity (or even negative inequity) using stem counts than we would using canopy cover. We created a variable to indicate if the outcome variable was measured using a tree or stem inventory or the alternative (aerial or satellite imagery).

A number of other detailed measurement strategies might influence observed inequity, including the resolution of top-down strategies and the extent to which authors eliminated built environment footprints in their estimates of forest cover. While these strategies are relevant, we were unable to extract meaningful and reliable covariates from original studies to represent these strategies.

3.2.2 Vegetation types—The distribution of urban trees and urban vegetation more broadly might differ. Some studies measured only trees (e.g. using canopy cover or stems; Conway & Bourne, 2013), while others used trees and shrubs (woody vegetation) (Clark et al., 2013), and still others measured all vegetation or greenness (including grass; Tooke et al., 2010; Jenerette et al., 2011). Few studies have compared different measurement techniques (without also varying other characteristics of the outcome variable), but Conway & Bourne (2013) found evidence that inequity varied across three different measures of the urban forest (canopy cover, stem density, and species richness).

Although we did not have a strong a priori theory or robust literature to set expectations, considering relationships between vegetation types and features of the built environment yielded potential hypotheses. Both vegetation cover generally (trees, grass, and shrubs) and tree cover specifically are constrained by features of the built environment. We expected to see higher vegetation cover in suburban areas (with larger residential lots) than in the dense urban core (Pham et al., 2012; Troy et al., 2007). Given that suburban areas are often higher income, we expected to see a positive relationship between urban vegetation cover and income (without controlling for density or other built environment characteristics). The ratio of trees to other vegetation is likely higher in more dense urban centers, where for example, there are few residential lawns but there are tree pits along sidewalks. Findings from Pham and colleagues (2013) suggest this might be the case; the authors find stronger negative relationships between density and vegetation than with trees/shrubs both on all land and in backyards. The only positive relationship they find is between density and street tree cover (Pham et al., 2013). To the extent that urban density is related to income, we expected to observe less inequity in tree cover than we observed in overall vegetation.

Vegetation (trees, grass, shrubs) is likely more constrained by urban form than tree cover is. Grass, for example, will voluntarily grow across many non-impervious surfaces. Trees are mostly constrained to impervious areas, but the location of trees within available space is likely more influenced by human actors than the location of grass and shrubs. Actors might plant and cultivate trees in wealthier areas, or actors might compensate for built environment constraints in low income areas by planting in the limited planting space available and creating new planting locations in sidewalks and abandoned lots. In light of built environment constraints, we might see less inequity in tree cover than in overall vegetation.

A finding that differences in how vegetation was measured helped explain differences between studies would provide future researchers with some guidance on the tradeoffs of choosing between measures. We coded a variable to indicate whether the original author's measure of urban forest cover included both trees and herbaceous cover.

3.2.3 Publicness—We leveraged differences in the types of land studied to garner insights about drivers of and potential remedies for urban forest inequity. Urban trees grow on many types of land, such as residential property, along streets, in parks, near streams or waterways, and in abandoned lots. Some studies measured urban forest cover on all land in a city (Schwarz et al., 2015), while many restricted their study to urban forest cover on only residential land (Grove et al., 2014), in public right-of-ways (Landry & Chakraborty, 2009), in parks, (Martin et al., 2004) or in private gardens (backyards) (Kirkpatrick et al., 2007).

The relative influence of urban forest actors varies across these land use types; municipalities play a much larger role in managing urban forest cover on public land than they do on private land. More observed inequity on public land such as tree lawns and right-of-ways (Pham et al., 2012, for example) than on private land would suggest a public policy failure in the distribution of resources. A few recent studies on governmental and nonprofit tree planting provide some insight into the distributional results of tree planting programs, but findings conflict. Several studies found that trees from city government programs were more likely to be planted in higher income neighborhoods (Locke & Grove, 2014) and that homeowners were more likely to participate in planting programs (Donovan & Mills, 2014). [redacted] and colleagues found the opposite when looking at nonprofit plantings—lower income neighborhoods were more likely to have a tree planting. Another study found no relationship between tree requests and income or race (although they find positive and significant relationships with education and percent renter occupied housing; Locke & Baine, 2015). It is beyond the capacity of this meta-analysis to determine whether strategies relate to higher overall urban forest inequity. However, a finding of inequity on public land would suggest that changes in public policy and service provision are necessary to remedy existing inequity.

Evidence of higher inequity on private land would suggest unequal use of resources by residents and property owners. Given the constraints on municipalities from engaging on private land, potential remedies are less obvious, but could include subsidized tree-planting on private property ([redacted]). We included indicator variables if the sampling frame was private land only or if the sampling frame was mixed public/private land (a category that included studies that used all urban land). The base case was public land, which included pure public lands (parks, city property) and street trees and tree lawns.

3.3 Characteristics of the study sites

Most studies (though not all) restricted their analysis to a single city or a small number of neighboring cities, likely due to intensive data collection requirements. While results from single-city studies help local actors identify and address existing inequities, they provide limited generalizable information. Although there are a few cases in which multiple studies have been published about a single city (e.g. Baltimore; Boone, Cadenasso, Grove, Schwarz, & Buckley, 2010; Romolini, Grove, & Locke, 2013; Schwarz et al., 2015; Thornton et al., 2016), there are not enough of these clusters to leverage a city fixed effect. Relatedly, because of the small number of observations in study sites and repetition of study authors and datasets, socioeconomic factors that are constant within study-sites will confound the relationship. Given the limited ability to cluster by study site, we collected additional data on each study site. We categorized variables into socioeconomic and environmental factors.

3.3.1 Socioeconomic characteristics—We tested for the presence of a "city effect" using several city-level covariates. First, we included the population of the city (measured continuously in 100,000s, demeaned) to understand whether city size is related to inequity. We coded study site population from original studies if reported. When a study did not report population, we searched Google for the study site's population in the last year of urban forest cover data (regardless of the publication year). Population was not adjusted for the total land area in the research frame, but often included population of an entire MSA if included by the original authors.

We included measures of city-level racial segregation and income inequality, hypothesizing that cities with high racial segregation and income inequality may also have high levels of inequality in the distribution of urban forest cover. We surmised that a latent construct of inequality may be the driver of racial segregation, income inequality, and urban forest cover inequality. To construct our measure of racial segregation, we used a dissimilarity index from The University of Michigan's Population Studies Center (Farley, n.d.). The index used 2000 census tract data to estimate the distribution of racial groups across census tracts within a city—essentially how residentially segregated one racial group is from another racial group (0 indicates no residential segregation between two groups; 100 indicates absolute segregation). We collected the indices that estimated segregation between White and African American residents and between White and Hispanic/Latino/a residents. From the 100-point scale, we created a binary variable for each index where 0 indicated the dissimilarity index was in the bottom three quartiles of dissimilarity indices in the database. We chose this threshold because our studies over-represented cities with high residential segregation and the bottom three quartiles offered balance between 1s and 0s.

Income inequality was derived from Brookings Institution 2014 data for the 100 largest U.S. metropolitan areas using the 95/20 ratio, or the ratio of income of the wealthiest 5 percent of households to income of the poorest 20 percent of households (Holmes & Berube, 2016a; Holmes & Berube, 2016b). We generated a binary indicator that equaled 1 if a city's 95/20 ratio was higher than 9.7—the aggregate ratio for the 100 largest metro areas in 2014 (Holmes & Berube, 2016a).

3.3.2 Environmental characteristics—The urban forest is more robust in areas where the climate naturally supports woody vegetation (Nowak et al., 1996). In climates that naturally support trees, the urban forest is often a combination of planted trees, natural remnant forests, and trees that have regenerated on their own. Cities in climates that do not support trees naturally (e.g. the arid southwestern United States) rely more heavily on active tree planting (Nowak, 2012), which requires time and financial resources. We expected higher income-based inequity in cities whose climate dictates a higher reliance on costly tree planting.

We obtained climate information from an updated version of the Köppen-Geiger climate classification map (Kottek et al, 2006a, 2006b). Kottek and colleagues (2006a) classified climate types using global temperature and precipitation data from 1951 to 2000. With these data, they replicated the calculations from the commonly used, but older, Köppen-Geiger classification map (last updated in 1961). The climate classification system was designed with vegetation in mind. The three-letter climate classification code indicates three features: main climate, precipitation, and temperature. The scheme identified five main climates: equatorial, arid, warm temperate, snow, and polar. We obtained a shapefile that contained the climate classification map on a regular 0.5 degree latitude/longitude grid. Locations for cities and towns in the United States were obtained from ArcGIS Online's "USA Major Cities" layer pack (obtained 09/28/2016). In ArcMAP 10.4 we extracted the local climate classification to each city. From the three-letter climate classification codes we generated two binary indicators of climate which were most likely related to natural tree growth and regeneration and had sufficient variation in our sample. The first variable indicated that the main climate was arid (using both precipitation and temperature data), and the second indicated the precipitation class was not humid. We used these two operationalized climate variables in separate models. The base case in this category was a non-arid climate or humid climate.

The interaction between climate and land use (see the section on *publicness*, above) may also be an important factor in urban tree inequity because publicly maintained trees may be costlier to maintain in arid cities than areas where trees grow more easily. Thus, the public/private dynamic and climate together may mediate tree cover inequity.

3.4 Publication status and focus

Finally, findings might vary across the many research fields that study urban forest cover distribution. For example, studies in geography might be less likely to find evidence of inequity because the methods used in geography vary systematically from other studies. Environmental justice studies might face pressure to shelve insignificant quantitative findings, editors and reviewers may reject insignificant results, scholars may explore the question of environmental inequities in urban areas where racial injustice may be more prevalent or simply in cities with significant minority populations, or scholars might reframe a study when they find evidence of inequity. As a test for publication bias, we tested whether studies published in academic journals have systematically different results than other studies.

Second, we coded whether the field of study was geography. For published works we noted whether the publication outlet was a geography journal, such as GIScience & Remote Sensing, Spatial Demography, and Urban Geography. We coded unpublished work such as dissertations and conference proceedings based on the academic program completed or the conference attended. The comparison group included all other fields, which largely included ecology and interdisciplinary social science journals. We included a variable for whether a study's frame was environmental justice related. We coded this as 1 if the title or abstract included the word (in)equity, environmental justice, access, or the study generally discussed the unequal distribution of urban forests.

We also included an interaction term for whether the study had both an environmental justice focus and was not peer reviewed, suspecting that such studies may be systematically different from either of the other two categories.

A number of other detailed measurement strategies might influence observed inequity, including the resolution of top-down strategies and the extent to which authors eliminate built environment footprints in their estimates of forest cover. While these strategies are relevant, we were unable to extract meaningful and reliable covariates from original studies to represent these strategies.

4. Data Coding and Methods

Meta-analysis combines the results of numerous quantitative studies (*original studies*) that have previously examined a relationship of interest between a dependent variable (Y) and a *focal predictor* (X), otherwise known as the independent variable of interest. The unit of analysis in meta-analysis is the *effect size*, which is the standardized measure of each tested relationship between X and Y (often, the beta coefficient on the X variable in a regression, or a Pearson's / Spearman's correlation coefficient).

This section discusses how original studies were coded as well as some brief notes about the covariates described above. This section also details the meta-analytic methods employed in this paper: forest plots and meta-regression. Forest plots are a graphical illustration of the mean effect size for each quantitative study. Meta-regression serves two purposes. First, it allows us to examine the grand unconditional mean effect size using a technique like weighted least squares. This reports the effect size for all studies—the best estimate of the mean of the totality of the current literature—and is the intercept of a model with no control variables. Second, it allows us to condition the mean effect size on covariates that we suspect determine or influence the magnitude of effect sizes. Using meta-regression we both calculated the average relationship between urban forest cover and income and explored the reasons results may vary across studies. Finally, to further our exploration of publication bias, we employed a confunnel plot. More on the confunnel plot can be found in the results section.

4.1 Study coding

We coded studies after one of the authors read the full text of a study and deemed it acceptable according to our inclusion criteria. Data was then extracted from each study using a uniform coding instrument (available upon request). The primary variable of interest coded for this meta-analysis was the effect size, the relationship between urban forest cover and income. Many studies reported multiple effect sizes. To take an example, Heynen, Perkins, and Roy (2006) tested correlations between median household income for both residential canopy and all canopy cover in Milwaukee. We also coded information about the effect and characteristics of the outcome measures, focal predictors, research design, and more as detailed above.

Because each study reported the relationship between urban forest cover and income differently (e.g. Spearman's rho, regression coefficient), we standardized using an r-based measure, a measure rooted in Pearson's product-moment correlation coefficient. Pearson's r is bounded between −1 and +1 with 0 indicating no relationship and −1 and +1 indicating perfect linear relationships. In this paper, positive values indicate inequity (lower income populations have less urban forest cover) and negative values indicate negative inequity (lower income populations have more urban forest cover).

When precise information on test statistics or probability values was not reported, we defaulted to the use of statistical significance as denoted by stars or asterisks to conservatively calculate the effect size (this approach is conservative because a single asterisk denoting p<.05 might have a true p value of .011, just above the p<.01 mark). In cases where the test-statistic or standard error was not reported and a coefficient was not indicated as being statistically significant, we assumed the true effect size was zero.

Because an r measure based on Pearson's correlation has at least two problems (it is censored and heteroskedastic), we transformed this variable using the Fisher transformation to z, where z=0.5 ln[(1+r)/(1−r)]. This transformation makes the standard error a convenient $1/\sqrt{N-3}$. While z ranges from negative infinity to infinity, in most practical cases, z can be interpreted similarly to r as z becomes extremely small or large as r approaches −1 or 1 respectively. Fisher's z then is the effect size used in all subsequent analyses. Fisher's Z can be converted back to Pearson's r using the formula, $r=\frac{e^{2z}-1}{e^{2z}+1}$, but unless the value of either Z or r is greater than |0.4|, the values are within 0.02 of one other.

4.2 Covariates

The covariates in this meta-regression were factors that varied either across estimates within a study or between studies that might explain why effect sizes were different in size (see descriptions of covariates, above). Almost all covariates were binary indicator variables. As with binary variables in a traditional regression analysis, the meta-regression coefficients on these covariates can be interpreted as the additive effect of "turning on" a binary variable.

Because the unconditional intercept is meaningful in meta-analysis (it is the unconditional mean effect size), the conditional intercept can be meaningful and interesting as well. To take advantage of this, we entered variables in reverse. In other words, "low quality" practices were given a value of "1" while practices associated with high quality were given a value of "0." The intercept then reflects the mean effect size when the "higher quality" practices were undertaken. For several methodological and measurement covariates, there is no clear indicator of high quality. For those variables the intercept, although still meaningfully interpretable, loses this inherent indication about study quality. We examined numerous contingency tables to ensure that there was enough variation for each variable to observe a real effect, rather than capture the idiosyncratic effect of a small number of outliers.

4.3 Inter-coder reliability assessments

Because two researchers collected and coded information for this paper, we undertook two-stage inter-coder reliability assessments to verify we agreed on complex constructs. In the first stage, we evaluated whether both authors were similarly assessing the acceptability of original studies. There was 100 percent agreement between the two authors when assessing 30 studies, nine of which were deemed acceptable by both authors.

In the second inter-coder reliability assessment, we assessed levels of agreement in coding key details of effect sizes such as the coefficient, p-value, and test statistic, and whether the raw coefficient favored inequity or negative inequity. The two authors had a percent agreement of 99.6 percent, the lone difference being a typographical error (from 49 effects with 247 attributes). Both inter-coder reliability assessments were excellent. Figure 1 highlights the results of these inter-coder reliability assessments as well as their timing in the literature search process.

4.4 Forest plots

Forest plots allowed us to compare the within-study average effect size between all studies. To combine effects within studies, we multiplied each effect by its weight and then constructed an average weighted effect size (and standard error) for each study. Using these weighted averages, we calculated the overall mean and confidence interval (as well as a prediction interval) for all studies. This overall mean and confidence interval differed from the one reported by the (more accurate) meta-regression because forest plots employ a study-level average rather than the individual effect-size level average. The two averages tend to be similar.

4.5 Meta-regression

Meta-regression is similar to weighted least squares. Each effect coded from an original study is weighted based on its sample size. This gives more weight (or preference) to larger studies, reducing the impact of small sample outliers on our results. Unlike weighted least squares, however, meta-regression also adjusts its weight matrix for heterogeneity of the estimates using what is known as a random-effects framework. In social science meta-analyses, we typically accept that effect sizes are drawn from a distribution of effects. The core assumption of the random effects framework is that effects differ for reasons which cannot be attributed to sampling distribution alone. The assumption of a random effects estimator is in opposition to a fixed effects estimator, where the true population mean is fixed and effects are drawn from a distribution around that mean. The fixed effects framework is likely to be the correct assumption for a series of lab experiments using the same instrument.

To adjust for heterogeneity we included an estimate of it in each effect's weight. That estimate is represented by τ^2, which is an estimate of the dispersion of the distribution of the true effect. In other words, there is sampling error around the population parameter and there is a distribution of the true effect that cannot be explained by sampling error. τ^2 estimates the latter. The practical impact of including τ in the weight is to place more emphasis on smaller studies than they would receive in a fixed effects meta-regression.

We also reported I^2. I^2 is a measure of the amount of heterogeneity of the estimate which is explained by factors other than random sampling (Higgins, 2002) and is interpreted as the percent of variation owing to factors other than sampling error. The I^2 statistics in this analysis were large, around .9 or 90 percent in the meta-regression models, which is typical for meta-regressions in the social sciences. For each of these values of I^2, the p-value of a Q test (a test for whether the true distribution is fixed or random) would be less than .001, indicating the assumption of the random effects framework was the correct assumption.

We also accounted for non-independence of effect sizes within studies as effect sizes often use the same or similar data. To adjust for non-independence, we used cluster robust variance estimators (CRVE), as employed in other recent meta-analyses ([redacted]; Ringquist, 2013). However, one potential source of bias in parameter estimates (as opposed to the standard errors, for which CRVE is sufficient) in meta-regression coefficients is the repeated use of the same sample within studies. One option for dealing with this bias is to keep just one observation per study site per study. However, this strategy omits much of the variation we use to understand the difference between modeling choices. A test of the bias caused by repeated samples suggests that it is small (within the original margin of error; available upon request).

Model specifications and meta-regression tables mirrored the variable choices we described in detail above. We examined the unconditional mean effect size and added covariates to examine the impact of methodological and measurement choices in all studies (Table 2) and U.S. studies (Table 3). We then examined study-site characteristics such as environment and socioeconomic characteristics for U.S. studies (Table 4), and examined publication focus and outlets, including whether the study has been published in a peer reviewed journal (Table 5).

4.6 Tests for publication bias

Following Ringquist (2013), we tested for publication bias using the meta-regression coefficient for "not peer reviewed." A statistically significant coefficient would indicate that peer reviewed studies had sufficiently different results from studies that are not peer reviewed. Other tests of publication bias, which include Begg and Egger tests, examine an unweighted unconditional effect size. As a result, these tests are sensitive to errors, including non-independent error terms, heterogeneity, and the use of covariates. While both Begg and Egger tests (not reported) suggested publication bias at the $p<.01$ level, publication bias was not revealed by meta-regression, however. Thus, we did not conduct trim-and-fill or other correction procedures. Our meta-regression test for publication bias results along with a confunnel plot are discussed in more detail at the end of the results section.

5. Results

5.1 Descriptive statistics

Descriptive statistics for the control variables are reported in Table 1. We report descriptive statistics for all studies and for studies just in the United States, which contained about 80 percent of all effect sizes. We isolated U.S. studies because of the United States' unique history of the environmental justice movement and because some covariates (e.g. racial dissimilarity and income inequality) were only available for U.S. study sites. The mean study-site population in our sample is 2.2 million.

5.2 Forest plots

The forest plot in Figure 2 reports the average effect size for each study. The black dot for each study represents the mean effect size, the black horizontal line is the 95 percent confidence interval for that mean, and the gray box is the weight applied to each study. The same statistics are numerically presented in the right-hand columns. As Figure 2 demonstrates, there was significant heterogeneity in average effect size between studies, but there was a consistent positive relationship between income and urban forest cover. The bottom diamond in Figure 2 graphs the overall mean (.12) and 95 percent confidence interval (.10 to .14). However, because this overall mean was constructed from study means, it varied slightly from the mean presented using meta-regression; the meta-regression mean effect size was calculated correctly.

5.3 Meta-regression

Tables 2 through 5 report our meta-regression results. Tables are organized as follows: the first column of statistics reports the unconditional mean effect size, which is the average relationship between urban forest cover and income for all studies. Starting in column 2 we added additional covariates. Positive coefficient values indicate higher levels of urban forest inequity with respect to income (or less negative inequity) in the presence of the binary variable, values around zero indicate no influence of the predictor on observed inequity, and negative values suggest less urban forest inequity (or more negative inequity) in the presence of the binary variable.

Table 2 reports the unconditional mean effect size in column 1 and results from seven additional meta-regression models in columns 2 through 8 for all studies. The mean effect size coefficient is 0.098. Although this would be typically considered a weak or negligible relationship for Pearson's r, it is much larger than other recent meta-analyses in social sciences (see [redacted]). Moreover, hypothesis testing for this coefficient suggested that we reject the null hypothesis of zero at the $p<.001$ level. This suggests that, taken together, the urban forest literature shows a statistically significant and meaningful relationship between urban forest cover and income, and that higher income properties or neighborhoods have more urban forest cover. The remaining models attempt to explain this observed relationship.

The addition of the spatial-autocorrelation covariate (entered in reverse) suggests that studies that do not control for spatial autocorrelation are much more likely to find inequity (Column 2 of Table 2). In fact, the effect size for studies that do control for spatial autocorrelation is more than halved to 0.044. It remains statistically different from zero at the $p<.05$ level, however. The coefficient for no control for spatial autocorrelation tells a compelling story; studies (and models within studies) which do not use a spatial lag or employ spatial error correction have an average effect size which is larger by 0.071, a result that is statistically significant at the 0.01 level. This is a key finding. Studies appear to find a weaker relationship between urban forest cover and income when the error term is corrected for violations of independence. This finding is limited, however, by the fact that only about 20 percent of studies control for spatial autocorrelation, and because correcting for spatial autocorrelation is somewhat new, studies that correct for it are more recent than the typical study in our sample. It is possible that the addition of new studies which control for spatial autocorrelation would dampen the unconditional mean effect size.

Column 3 adds two variables which account for how the spatial unit of analysis was defined. We again find that these choices have a significant impact on the measured outcome. Studies which used a small spatial unit of analysis such as a parcel or household found a null effect for urban forest cover inequity (when added to the mean effect size). This should be compared to large tracts or medium-sized geographies (between parcel and tracts in size; reflected in the mean effect size), which show positive inequity. This suggests that our concerns about measurement error at the parcel level were warranted. The larger units of analysis (census tract size or larger) show no difference in estimated inequity from medium units of analysis. This suggests that census tracts are as plausible a level of neighborhood aggregation as the smaller blocks and block groups. The conditional mean effect size (the intercept) that focuses on spatial units at the census block or block group level is statistically different from zero and the coefficient is substantively meaningful though cannot be differentiated from larger spatial unit units (tract or larger).

Columns 4 and 5 examine the influence of modeling choices and use of covariates. In column 4, the intercept represents effects from multivariable regression models which control for racial minority population, density, and housing age. With these controls, the mean effect size decreases to .067 from .098, but it remains statistically different from zero (at the $p<.01$ level). This suggests that these controls are important potential confounders, but their inclusion does not eliminate the estimated relationship between urban forest cover and income. Column 5 adds potentially over-controlling variables: education and home value. As suspected, these controls decreased the mean effect size (and widen the standard error) such that the conditional mean effect size is no longer statistically different from zero. This suggests that issues of collinearity and over-controlling may indeed arise from the inclusion of other socioeconomic variables and may make it more difficult to estimate the relationship between income and urban forest cover, ceteris paribus.

Column 6 adds three measurement considerations: whether the outcome variable was measured as both tree and herbaceous cover, whether the outcome variable was measured as a stem count or inventory, and whether the focal predictor was a measure of relative income (e.g. poverty rates) rather than absolute income (e.g. median income). We did not find that these measurement choices have a meaningful impact on the measured relationship, either practically nor with respect to hypothesis testing. This should reassure researchers that these measurement choices do not appear to have a substantial impact on estimated results.

Last, in columns 7 and 8 we created two models for “best case” or “high quality” effects, one that contains a correction for spatial errors and one that does not. Both models have indicators for whether the spatial unit of analysis was smaller than a household or parcel, whether the study used multiple regression, and whether it employed control variables (which did not over-control). We also added one more covariate—whether the study was not peer-reviewed. Both models result in small but statistically significant conditional mean effect sizes as reported by the intercept. The intercept in the model which controls for corrections for spatial autocorrelation is once again smaller than in the model which does not. However, we find that even our high-quality studies which employ multiple regression, numerous controls, and possibly spatial error correction still find important levels of income-based inequity in urban forest cover.

Table 3 replicates the models in Table 2 for U.S. geographies only. This choice excluded 15 studies (24 percent) and 68 observations (20 percent). The results in Table 3 largely mimic those in Table 2 with one notable exception—the conditional mean effect size in columns 2 and 8, models that control for spatial error correction, are no longer statistically distinguishable from zero. The actual coefficients are not much different in magnitude, but the loss of some observations resulted in a larger standard error. However, given that other findings remain significant and magnitudes are similar, Table 3 suggests that this environmental inequity is neither uniquely American, nor particularly American as the mean effect size for U.S. geographies (.116) is comparable to all studies (.098).

Table 4 uses several city-level indicators to test whether observed inequity varied systematically with characteristics of the study site. Table 4 presents data for a sub-sample of U.S. study sites because the data, particularly measures of racial segregation and income inequality, were only available for single (and large) U.S. cities. Column 1 reports the unconditional mean effect size. Column 2 shows that (demeaned) population does not appear to be linearly related to effect size. In other words, the size of a study site does not seem to predict the level of inequity in this environmental amenity. Surprisingly, racial segregation and income inequality also do not appear to be meaningful predictors. The coefficient on income inequality is large and practically meaningful, but of the unanticipated sign—it suggests that areas of low income inequality nonetheless have higher levels of urban forest cover inequity. Columns 5 through 8 of Table 4 suggest that the natural environment has little predictive power with respect to the distribution of urban forest cover by income, contrary to our expectations. Neither climate nor precipitation predict our relationship of interest. There also is no statistically meaningful mediating effect of climate and precipitation on private and public/private mixed land uses. Taken together, these results somewhat surprisingly suggest that the inequity of urban forest cover does not vary across region/climate type nor a single type of land. Taken together with Tables 2 and 3, it appears that methodological choices are more important explanatory factors than socioeconomic or environmental features of a city.

Table 5 reports models that test hypotheses about publication status and study focus. Column 2 examines the impact of publication status by including an indicator for a study that was not peer-reviewed. The small coefficient is not statistically different from zero which suggests that publication bias is not an important factor in this analysis–studies that are published in peer-reviewed academic journals find roughly the same effect size as those that are not. We also find in column 3 that results from studies which frame their study around environmental justice are not statistically different from studies which do not have an environmental justice frame. The most common discipline (publication outlet and degree-granting program) was

Geography; in column 4 we also find this not to be a critical factor. Finally, columns 5 through 7 combine these indicators, culminating in an interaction term between EJ focus and non-peer reviewed. We find that none of these factors importantly explain the relationship between urban forest cover and income.

Figure 3 reports a confunnel plot. A confunnel plot examines the individual-level effect sizes, sorted by sample size. Effects with large samples are reported on the top of the confunnel; effects with small samples are closer to the bottom. Confunnel plots are used to visually examine publication bias, particularly for small studies. If observations appear to be missing for one of the bottom quadrants (bottom right or bottom left) it is typically interpreted as resulting from publication bias. In Figure 3, effects from peer reviewed studies are represented by black plus symbols. Effects from non-peer reviewed studies are represented by a gray X. The vertical black line indicates the mean effect size (from Table 2) and the shaded cones which give the plot its name are formed by the 90, 95, and 99 percent confidence intervals for effect sizes at different sample sizes. Dots along the same vertical axis are typically effect sizes from the same study. As the confunnel indicates, there appears to just be one peer reviewed study that has effect sizes (mostly) in the positive domain (Jenerette, Miller, Buyantuev, et al., 2013). This single study is the reason that Begg and Egger tests suggest publication bias but our meta-regressions do not. However, once sample size is used as a weight in meta-regressions, the small weight given to this small study suggests that there is no publication bias (as indicated by Table 5 and the coefficient for non-peer reviewed in Tables 2 and 3). Removing this single study (Jenerette, Miller, & Buyantuev, et al 2013) and re-running results does not qualitatively impact our results (available upon request). We conducted another robustness check (not shown, available upon request) by dropping a single large study which contained 32 of our 332 effects (Yngve, 2016). Our results were robust to the exclusion of this single large study; results are qualitatively similar to those in Tables 2 through 5.

6. Discussion

The tools of meta-analysis allowed for a more comprehensive and nuanced understanding of previous studies that have examined the distribution of the urban forest. When applied, we found positive but inconsistent evidence for income-based inequity in the distribution of the urban forest cover.

The unconditional mean effect size revealed significant income-based inequity in urban forest cover. Meta-regressions examining the impact of methodological characteristics, measurement characteristics, and study site characteristics provide at least two major insights. First, the results show that evidence of income-based inequity is sensitive to methodological choices. Perhaps most important are the findings related to spatial autocorrelation, a method that some scholars advocate is necessary to produce unbiased estimates (Grove et al., 2014). In both all studies (Table 2) and U.S. studies (Table 3), evidence of income-based inequity was significantly reduced when we accounted for correction of spatial autocorrelation.

The level of aggregation explained some variation in results. Estimates of income-based inequity were lower when the spatial unit of analysis was a parcel or household, suggesting concerns in the hazards literature that the results of environmental justice studies are sensitive to the level of aggregation (Noonan, 2008) might apply to environmental amenities as well. However, we found no difference in inequity between studies that measure census blocks and block groups (or equivalent) and census tracts. Above, we argued that parcel-level analysis might not accurately model access because urban trees have positive externalities that are experienced by individuals that do not live on the property. Our results suggest parcel-level analyses underestimate inequity.

We found very little evidence that study-site characteristics, peer review, or study focus were related to observed inequity. Thus, researchers should be more confident that their study-site selection does not appear to be as important as their methodological choices.

Implications for research and practice

Our results yield several significant implications for future research in urban forestry and environmental justice and for the practice of urban forestry. First, these results make clear that studies of urban forest distribution need to test and control for spatial autocorrelation, even for correlation matrices. Currently only a relatively small number of studies and effect sizes account for spatial autocorrelation—just 68 effect sizes (20 percent) from 10 studies in our sample—which suggests more research is needed in this area to understand whether the observed difference is from the spatial autocorrelation methods or from some other systematic similarity between this small group of studies.

Many studies (18 of 62) present only correlation coefficients, which are subject to multiple confounding factors. We suggest multiple regression at a minimum and preferably in combination with a correction for spatial autocorrelation. Our findings also suggest that control variables (racial and ethnic minorities, density, and housing age, for example) do reduce the measured effect size. Our findings speak to larger concerns that the results of environmental justice studies are sensitive to methodological choices (Noonan, 2008).

A note for future authors in this area: too many studies did not report their sample size and we had to request it from the authors. Sample size is essential in evaluating the relationship between two variables as it is part of the calculation of t values. Reporting sample size allows researchers to qualitatively evaluate the magnitude of the effect and separate the size of the effect from levels of statistical significance, because p-values alone are misleading and should not be confused with the magnitude of an effect (see Gerstner et al., 2017).

The findings of this meta-analysis also have several implications for the practice of urban forestry. Even with our cautions about the impact of spatial autocorrelation methods, we still found that low-income neighborhoods face inequity in urban forest cover, even if that relationship is no longer statistically significant. As a result, we argue that urban forestry policy is warranted in evaluating and potentially targeting low-income neighborhoods for urban forestry programs such as tree-planting programs. We find consistent evidence of inequity on both public land (including tree lawns/boulevards) and private land, suggesting that municipalities and local nonprofits should consider strategies that reduce inequity both on public and on private land. The tools available to these groups are more restricted on private land, but strategies like subsidies for planting, tree giveaways, and collaboration with neighborhood groups could increase local private tree cover ([redacted]).

Public actions to reduce urban forest inequity should include active engagement of low-income communities. Urban greening efforts, even those that *do* explicitly seek to remedy environmental injustices, can initiate a process described as “eco logical gentrification” or “environmental gentrification” (Dooling, 2009; Pearsall & Anguelovski, 2016). As environmental amenities are improved, neighborhoods that had been ignored by governments and investors become valued, investors transform properties, and low-income residents are displaced when they can no longer afford to stay. This process often results in displacing the very residents who advocated for those environmental amenities and/or were the intended beneficiaries (Dooling, 2009; Anguelovski, 2015). Urban greening done poorly can “create new forms of exclusion” (Anguelovski, 2015).

Ecological gentrification challenges conventional urban greening strategies (Dooling, 2009) and has led scholars and planners to consider “just green enough” strategies that improve environmental amenities without creating focal points for developers and for gentrification (Wolch et al., 2014). From this phenomenon has also emerged a new aspect of environmental justice mobilization “as the defense of the right to place and territory, the right to stay without being displaced, and the right to remain protected from waves of uncontrolled investment, land grabbing, environmental profit, speculation, and disinvestment” (Anguelovski, 2015). The work in this meta-analysis provides evidence of environmental inequity and we hope that it will contribute to conversations about how to meaningfully remedy such inequity.

This meta-analysis synthesized previous quantitative literature about the distribution of the urban forest with respect to income. It offered, to date, the most comprehensive statement of whether inequities exist and the magnitude of those inequities. However, this evaluation was limited by several factors. First, while we identified evidence of income-based inequity, the meta-analysis could not identify the *cause* of that inequity, either economic (demand for trees) or political (provision of trees). It is also important to note that the analysis did not describe (in)equity in access to the *benefits of* or the *quality of* the urban forest. In other words, urban forest cover estimates included in this paper do not assess the quality nor species diversity of trees. For example, using estimates of percent canopy cover implied that all tree cover offers equal benefits; however, this is not the case. For example, trees on abandoned lots and trees in the “fence-line forest” (comprised of trees that have grown along unmaintained fences), may pose more of a nuisance than a benefit (Heynen et al., 2006). The balance of benefits differs across tree species as well; flowering trees might provide relatively more aesthetic benefits per unit canopy cover than other trees, and taller trees might provide more of a storm water benefit per unit canopy than other trees.

Last, there were a few studies that we would have liked to include because of their relevance. However, we were not able to include coefficients from geographically weighted regressions because they offer no global coefficient estimates (e.g. Pearsall & Christman, 2012; Landry, 2013). Pearsall and Christman (2012) find that their geographically weighted regression models perform better than global and regional OLS models. Just as we find the relationship between income and urban forest cover varies between study sites, the results from Pearsall and Christman (2012) suggest that these relationships also vary *within* study sites.

7. Conclusion

This meta-analysis evaluated the relationship between urban trees and vegetation (the outcome variable) and income (the focal predictor). Our literature search found 61 studies with 332 total effect sizes which quantitatively evaluated this relationship. We used the tools of meta-analysis to quantitatively accumulate original studies into standardized effects. In particular, we reported a forest plot and meta-regressions. Using meta-regression, we conditioned the observed mean effect size on a number of theoretically important variables such as methodological choices, measurement choices, study-site characteristics, and publication focus.

Our results yielded several interesting findings which ought to inform both research and practice. First, future studies of urban forest inequity should be thoughtful in their methodological choices, given the sensitivity of income-based results to correction of spatially-correlated errors and the spatial unit of analysis. Second, while no individual control variables appeared to have a dramatic impact on the measured effect size, the use of control variables such as housing age, density, and racial composition, did dampen the relationship between urban forest cover and income and thus they may be important to include to estimate the underlying relationship (rather than one which might be confounded by other factors).

Optimistically, it does not appear that measurement choices or study site characteristics appear to have a dramatic impact on the results; including variables that measure herbaceous cover, using an inventory/stem count, relative income/poverty, and public vs. private land use did not seem to impact the estimated mean effect size. Insignificant effects of Köppen-Geiger codes, study-site population, and city-level racial/income segregation further suggest that the particularities of a chosen study site should also not impact results importantly. This suggests that future authors should not anticipate significant differences in the results given these choices. Last, publication lens or outlet did not impact findings; publication bias is not substantially important, a focus on environmental justice issues did not relate to effect sizes, and findings from geography are no different from those in other outlets.

Overall, after examining numerous relationship and covariates, there appear to be substantial levels of urban forest cover inequity with respect to income with the critical caveat that this relationship is significantly dampened by corrections for spatial autocorrelation. Future research in this area must take this threat to correctly estimating standard errors seriously and adjust accordingly.

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Research highlights

- Meta-analysis reveals significant income-based urban forest inequity.
- Inequity persists regardless of measurement and methods choices of original studies.
- Inequity appears smaller when models control for spatial autocorrelation.
- Urban forestry programs should consider program impact on urban forest equity.

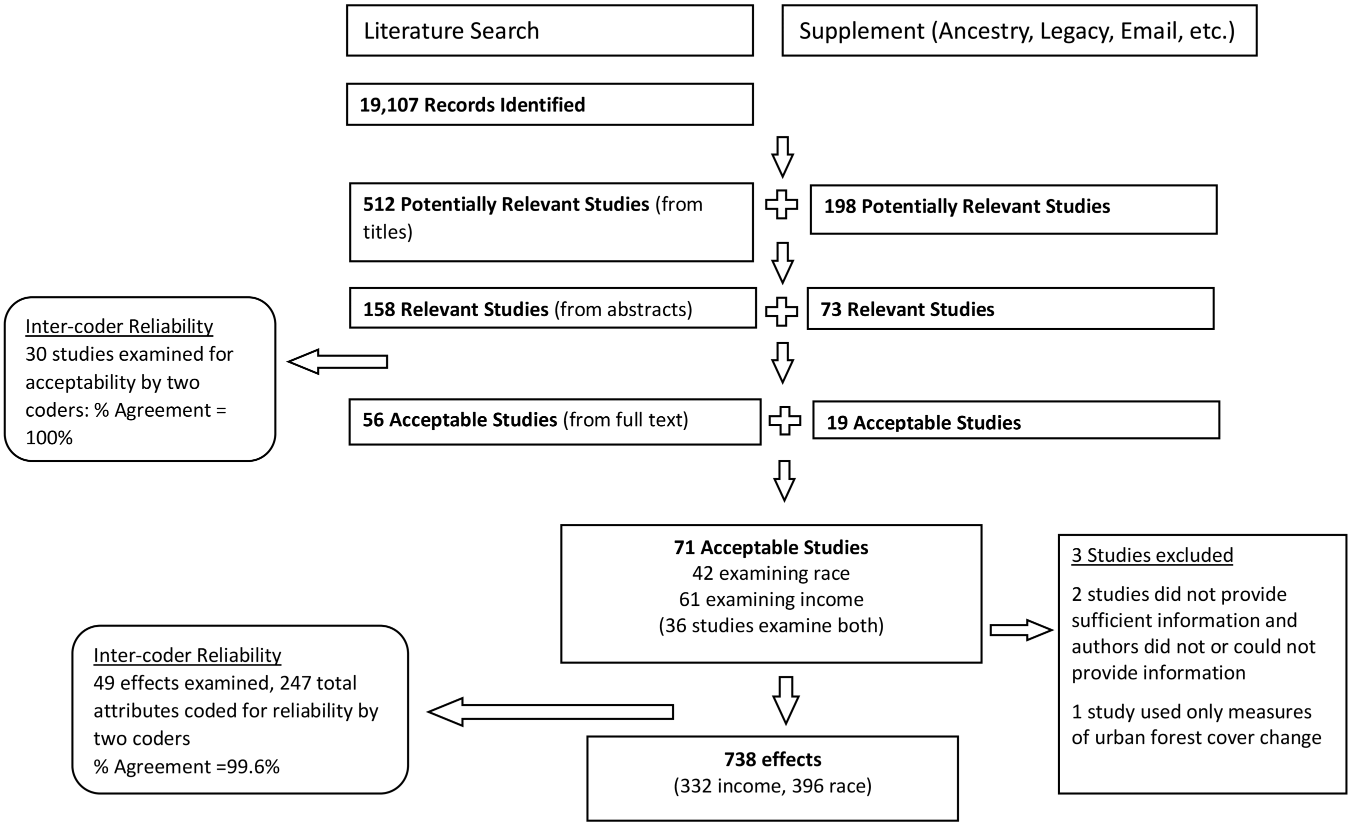

Figure 1. Flowchart of Literature Search Process and Inter-Coder Reliability Assessments

Results are from a combined search for studies that estimate the relationship between urban forest cover and either race or income. See [redacted] for results of race analysis.

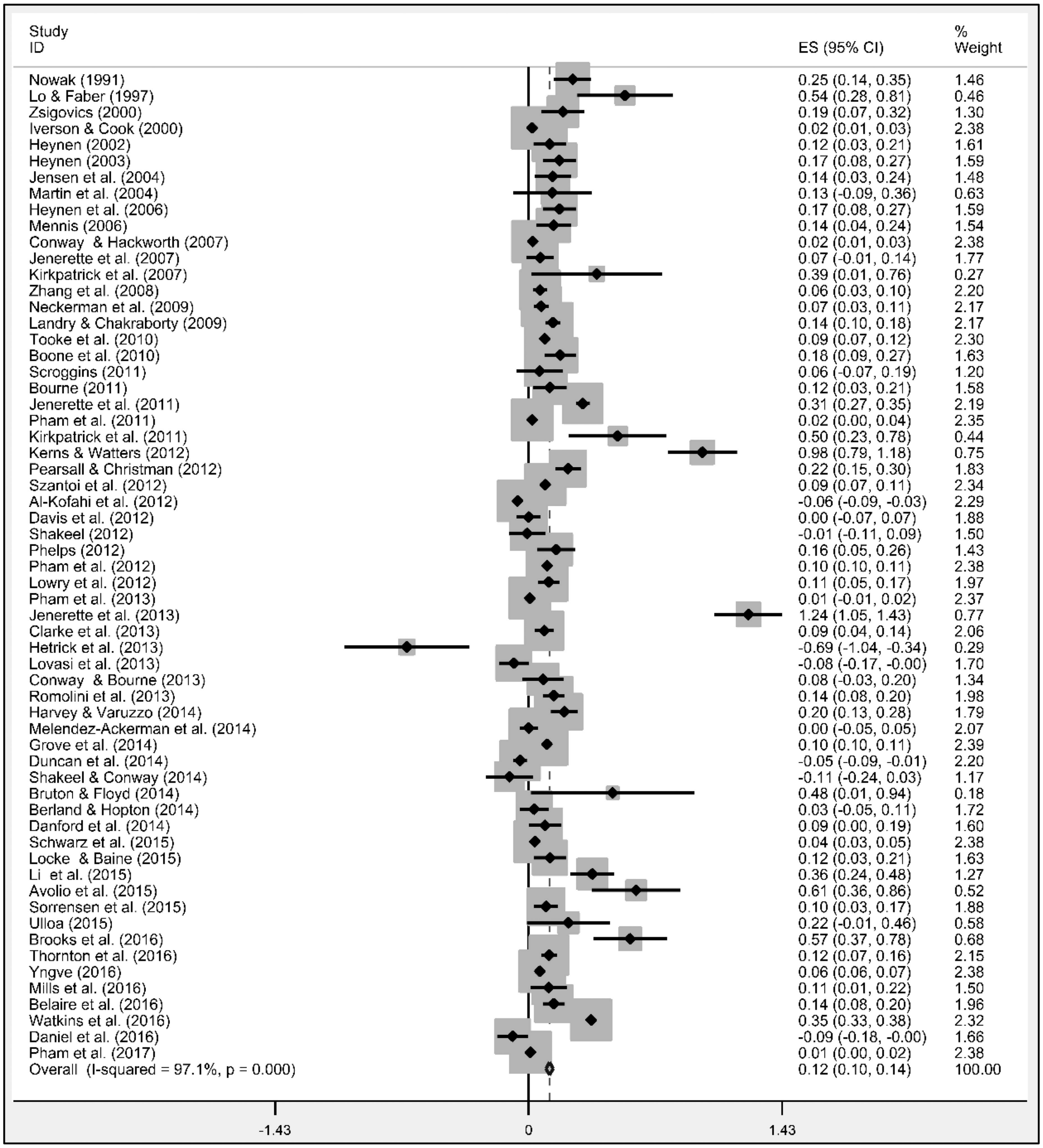

Figure 2. Forest Plot – Distribution of Urban Forests by Income

Black center dots represent the within-study mean effect size and horizontal bars are the within-study standard error. Gray boxes visualize the weight given to each study. Effects in positive space indicate positive inequity; effects in negative space indicate negative inequity. The same statistics are reported in the right two columns. The bottom diamond reports the overall mean effect size and its standard error (the standard error is represented by the left and right edges of the diamond). This overall mean effect will be slightly different than the one reported in meta-regressions due to within-study variation

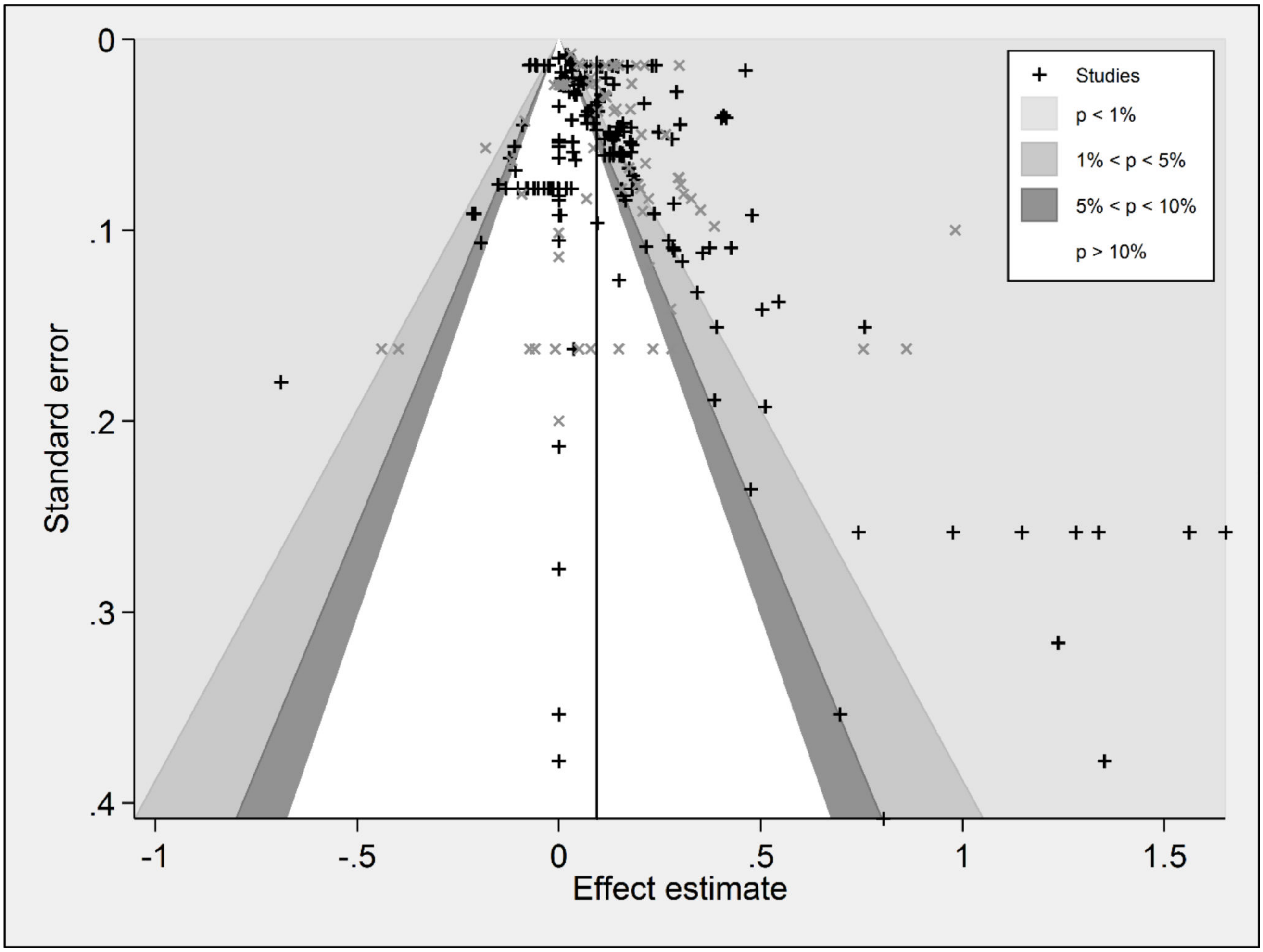

Figure 3. Confunnel Plot

Black plus symbols represent effect sizes (within studies) from peer reviewed publication. Gray Xs are from non-peer reviewed studies. Effect sizes are sorted by sample size; large samples are on reported on the top of the confunnel, small samples towards the bottom. The shaded cones are formed by the 90, 95, and 99 percent confidence intervals for effect sizes at the given sample size. Pluses and Xs on the same horizontal line are typically effect sizes from the same study. The vertical black line indicates the mean effect size.

Table 1

Descriptive Statistics

	All Studies			U.S. Studies		
	Mean	**Total**	**N**	**Mean**	**Total**	**N**
Effect size observations	1524.066	505990	332	1236.019	326309	264
No correction for spatial autocorrelation	0.789	262	332	0.818	216	264
Spatial Unit of analysis is census tract or larger	0.326	104	319	0.279	70	251
Spatial unit of analysis is a parcel or household	0.169	55	325	0.163	42	257
Method is correlation or bivariate OLS	0.482	160	332	0.485	128	264
No control for racial or ethnic minorities	0.750	249	332	0.727	192	264
No control for density	0.726	241	332	0.765	202	264
No control for housing age	0.762	253	332	0.807	213	264
No control for homeownership/home value	0.753	250	332	0.777	205	264
No control for education	0.825	274	332	0.856	226	264
Outcome measure is tree and herbaceous	0.392	130	332	0.333	88	264
Outcome variable is tree or stem inventory	0.108	36	332	0.117	31	264
Relative income measure (e.g. poverty rate)	0.292	97	332	0.280	74	264
Domain is private land only	0.211	70	332	0.208	55	264
Domain is mixed public/private land	0.518	172	332	0.511	135	264
Non-peer reviewed study	0.286	95	332	0.265	70	264
Population (in 100,000s)	20.9	6,891.6	329	22.6	5,917.3	262
Dissimilarity index white-black <75th percentile	0.351	88	251	0.337	83	246
Dissimilarity index white-Hispanic/Latino/a <75th percentile	0.386	97	251	0.374	92	246
High p95/p20 income inequality (2014)	0.770	174	226	0.770	174	226
Arid climate code (Köppen-Geiger)	0.206	51	248	0.206	51	248
Not humid precipitation code (Köppen-Geiger)	0.343	85	248	0.343	85	248
Study focus is Environmental Justice	0.572	190	332	0.545	144	264
Discipline is Geography	0.337	112	332	0.352	93	264

Notes: All variables, except for effect size observations and population, are binary variables. Mean can be interpreted as the proportion of observations coded as “1” and total is the total number of observations coded as “1.” Sample size (N) changes based on whether data was able to be coded (some non-U.S. geographies were coded as missing for various reasons).

Table 2

Meta-Regression: Methodology, Measurement, and Best Case Models for All Studies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mean Effect Size	0.098***	0.044*	0.109***	0.067**	0.053	0.090***	0.096**	0.045*	0.059***
	(0.017)	(0.019)	(0.017)	(0.022)	(0.027)	(0.021)	(0.033)	(0.020)	(0.017)
No correction for spatial autocorrelation		0.071**						0.062*	
		(0.026)						(0.026)	
Spatial unit of analysis is census tract or larger			0.001						
			(0.037)						
Spatial unit of analysis is a parcel or household			−0.105***					−0.125***	−0.117***
			(0.028)					(0.027)	(0.028)
Method is correlation or bivariate OLS				0.026	0.022			0.010	0.018
				(0.064)	(0.072)			(0.027)	(0.026)
No control for racial or ethnic minorities				−0.020	−0.023			−0.012	−0.008
				(0.036)	(0.032)			(0.025)	(0.025)
No control for density				0.051	0.045			0.050	0.060
				(0.050)	(0.050)			(0.032)	(0.035)
No control for housing age				−0.006	−0.016			−0.015	0.003
				(0.053)	(0.046)			(0.044)	(0.048)
No control for homeownership/home value					0.006				
					(0.046)				
No control for education					0.031				
					(0.032)				
Outcome measure is tree and herbaceous						0.029			
						(0.029)			
Outcome variable is tree or stem inventory						0.053			
						(0.030)			
Relative income measure (e.g. poverty rate)						−0.023			
						(0.040)			

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Domain is private land only							−0.059		
							(0.048)		
Domain is mixed public/private land							0.029		
							(0.039)		
Non-peer reviewed study								0.001	0.014
								(0.027)	(0.029)
Number of Observations	332	332	319	332	332	332	332	325	325
Number of Studies	61	61	59	61	61	61	61	60	60
R^2	0.000	0.031	0.080	0.025	0.029	0.014	0.040	0.151	0.133
Adj. R^2	0.000	0.028	0.074	0.013	0.011	0.005	0.034	0.133	0.116
τ	0.125	0.122	0.107	0.122	0.122	0.126	0.121	0.101	0.102
I^2	0.922	0.918	0.916	0.911	0.907	0.923	0.921	0.884	0.887

Notes:

* $p<0.05$

** $p<0.01$

*** $p<0.001$.

Coefficients are effects using Fisher's transformation of Pearson's r. They can be interpreted similarly to Pearson's r – on a scale of −1/+1. Cluster robust standard errors are in parentheses. Positive coefficients indicate inequity, coefficients near zero indicates equity, and negative coefficients indicate negative inequity.

Table 3

Meta-Regression: Methodology, Measurement, and Best Case Models for U.S. Studies

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mean Effect Size	0.116 ***	0.044	0.125 ***	0.085 **	0.083 *	0.102 ***	0.107 *	0.048	0.071 **
	(0.023)	(0.025)	(0.014)	(0.028)	(0.038)	(0.025)	(0.041)	(0.026)	(0.023)
No correction for spatial autocorrelation		0.089 *						0.073 *	
		(0.034)						(0.031)	
Spatial unit of analysis is census tract or larger			0.011						
			(0.058)						
Spatial unit of analysis is a parcel or household			−0.129 ***					−0.139 ***	−0.134 ***
			(0.030)					(0.034)	(0.036)
Method is correlation or bivariate OLS				0.030	0.029			0.013	0.019
				(0.077)	(0.087)			(0.027)	(0.027)
No control for racial or ethnic minorities				−0.019	−0.018			−0.012	−0.003
				(0.059)	(0.055)			(0.034)	(0.036)
No control for density				0.047	0.046			0.048	0.058
				(0.049)	(0.055)			(0.031)	(0.034)
No control for housing age				−0.009	−0.010			−0.022	−0.006
				(0.058)	(0.055)			(0.048)	(0.051)
No control for homeownership/home value					−0.000				
					(0.058)				
No control for education					0.004				
					(0.046)				
Outcome measure is tree and herbaceous						0.067			
						(0.044)			
Outcome variable is tree or stem inventory						0.043			
						(0.037)			
Relative income measure (e.g. poverty rate)						−0.036			
						(0.049)			

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Domain is private land only							−0.061		
							(0.061)		
Domain is mixed public/private land							0.044		
							(0.052)		
Non-peer reviewed study								0.018	0.033
								(0.030)	(0.035)
Number of Observations	264	264	251	264	264	264	264	257	257
Number of Studies	46	46	44	46	46	46	46	45	45
R^2	0.000	0.033	0.107	0.015	0.015	0.031	0.046	0.174	0.151
Adj. R^2	0.000	0.029	0.100	−0.000	−0.008	0.019	0.039	0.151	0.131
tau	0.144	0.140	0.117	0.142	0.143	0.144	0.140	0.111	0.112
I^2	0.928	0.920	0.922	0.915	0.914	0.926	0.928	0.893	0.895

Notes:

* $p<0.05$

** $p<0.01$

*** $p<0.001$.

Coefficients are effects using Fisher's transformation of Pearson's r. They can be interpreted similarly to Pearson's r – on a scale of −1/+1. Cluster robust standard errors are in parentheses. Positive coefficients indicate inequity, coefficients near zero indicates equity, and negative coefficients indicate negative inequity.

Table 4

Meta-Regression: Characteristics of U.S. Study Sites

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean Effect Size	0.113 ***	0.116 ***	0.102 ***	0.098 *	0.117 ***	0.114 ***	0.109 *	0.110 *
	(0.023)	(0.025)	(0.026)	(0.040)	(0.019)	(0.022)	(0.044)	(0.046)
Population (in 100,000s)		−0.001		−0.001				
		(0.001)		(0.001)				
Low dissimilarity index (White: African American)			0.042	−0.012				
			(0.056)	(0.063)				
Low dissimilarity index (White: Hispanic/Latino/a)			−0.014	−0.012				
			(0.044)	(0.050)				
High p95/p20 income inequality				0.086				
				(0.060)				
Arid climate code (Köppen-Geiger)					−0.025		0.000	
					(0.083)		(0.061)	
Not humid precipitation code (Köppen-Geiger)						−0.005		−0.004
						(0.055)		(0.041)
Domain is private land only							−0.066	−0.066
							(0.064)	(0.063)
Domain is mixed public/private land							0.034	0.035
							(0.053)	(0.053)
Number of Observations	248	248	243	223	248	248	248	248
Number of Studies	40	40	39	34	40	40	40	40
R^2	0.000	0.018	0.010	0.040	0.003	0.000	0.043	0.043
Adj. R^2	0.000	0.014	0.002	0.022	−0.001	−0.004	0.031	0.031
tau	0.141	0.139	0.140	0.134	0.141	0.141	0.138	0.138
I^2	0.917	0.915	0.918	0.920	0.917	0.917	0.915	0.914

Notes:

* $p<0.05$

** $p<0.01$

*** $p<0.001$.

Coefficients are effects using Fisher's transformation of Pearson's r. They can be interpreted similarly to Pearson's r – on a scale of −1/+1. Cluster robust standard errors are in parentheses. Positive coefficients indicate inequity, coefficients near zero indicates equity, and negative coefficients indicate negative inequity.

Table 5

Meta-Regression: Publication Lens and Focus

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Mean Effect Size	0.098 ***	0.089 ***	0.082 **	0.099 ***	0.084 *	0.081 *	0.073 *
	(0.017)	(0.021)	(0.029)	(0.023)	(0.032)	(0.032)	(0.034)
Non-peer reviewed study		0.031				0.042	0.082
		(0.030)				(0.046)	(0.051)
Study focus is Environmental Justice			0.027		0.028	0.025	0.039
			(0.035)		(0.035)	(0.034)	(0.040)
Geography				−0.002	−0.008	−0.029	−0.031
				(0.036)	(0.034)	(0.047)	(0.047)
EJ * not peer reviewed							−0.056
							(0.054)
Number of Observations	332	332	332	332	332	332	332
Number of Studies	61	61	61	61	61	61	61
R^2	0.000	0.006	0.006	0.000	0.006	0.015	0.019
Adj. R^2	0.000	0.003	0.003	−0.003	0.000	0.006	0.007
tau	0.125	0.124	0.123	0.126	0.124	0.123	0.122
I^2	0.922	0.920	0.922	0.921	0.921	0.921	0.907

Notes:

* $p<0.05$

** $p<0.01$

*** $p<0.001$.

Coefficients are effects using Fisher's transformation of Pearson's r. They can be interpreted similarly to Pearson's r – on a scale of −1/+1. Cluster robust standard errors are in parentheses. Positive coefficients indicate inequity, coefficients near zero indicates equity, and negative coefficients indicate negative inequity