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

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Abstract

There is ample evidence that urban trees benefit the physical, mental, and social health of urban residents. The environmental justice hypothesis posits that environmental amenities are inequitably low in poor and minority communities, and predicts these communities experience fewer urban environmental benefits. Some previous research has found that urban forest cover is inequitably distributed by race, though other studies have found no relationship or negative inequity. These conflicting results and the single-city nature of the current literature suggest a need for a research synthesis. Using a systematic literature search and meta-analytic techniques, we examined the relationship between urban forest cover and race. First, we estimated the average (unconditional) relationship between urban forest cover and race across studies (studies = 40; effect sizes = 388). We find evidence of significant race-based inequity in urban forest cover. Second, we included characteristics of the original studies and study sites in meta-regressions to illuminate drivers of variation of urban forest cover between studies. Our meta-regressions reveal that the relationship varies across racial groups and by study methodology. Models reveal significant inequity on public land and that environmental and social characteristics of cities help explain variation across studies. As tree planting and other urban forestry programs proliferate, urban forestry professionals are encouraged to consider the equity consequences of urban forestry activities, particularly on public land.

Keywords

Meta-analysis; Environmental equity; Environmental racism; Urban vegetation; Street trees

1. Introduction

In the face of urbanization and global climate change, an international movement to "green" cities has emerged. This movement has encouraged both metaphorical greening activities to reduce consumption (e.g. energy efficiency improvements, public transportation investments) and physical greening activities that cultivate urban vegetation. Prominent in this second set of activities are city tree-planting initiatives that collectively aim to plant

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millions of trees globally (such as MillionTreesNYC, [www.milliontreesnyc.org; (Fisher et al., 2015)]).

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 benefits to the physical, mental, and social health of urban residents (Haluza et al., 2014; Hartig et al., 2014; Lee and Maheswaran, 2011; Westphal, 2003) and improve local environmental conditions (Armson et al., 2012; Nowak et al., 2013; Zhang et al., 2012). In addition to their contributions to mitigating climate change (Nowak, 1993), new planted trees promise to provide local benefits to the communities in which they are planted. However, early evidence cautions that urban forestry programs have the potential to create or exacerbate inequity by planting in areas with higher existing canopy cover, higher income (Donovan and Mills, 2014; Locke and Grove, 2016), and with fewer minority residents (Watkins et al., 2016). Even were these programs to plant in low-income and minority neighborhoods, they might yield unintended consequences such as ecological gentrification —increasing property values and forcing low-income renters to relocate (Dooling, 2009; Pearsall and Anguelovski, 2016).

Unequal access of low income and minority residents to urban forests implies unequal access to the physical, mental, and social health benefits that urban forests provide—an environmental injustice. Scholars who have empirically examined the relationship between urban forest cover and race or ethnicity have found conflicting results—studies have found positive, negative, and no relationship between minority populations and urban forest cover (Danford et al., 2014; Flocks et al., 2011). These studies tend to be of a single city, however, potentially hindering the generalizability of results. In light of mixed findings, it is still unclear whether concerns of systematic inequity are substantiated by the existing research. Furthermore, there is little understanding of why we observe mixed findings across studies. Do observed differences across studies stem from differences between study sites (cities), or do they stem from methodological choices?

To address these lingering questions, we conducted a meta-analysis of the relationship between urban forest cover and race. A companion paper examined the relationship between urban forest cover and income (Gerrish and Watkins, 2017). We aggregated information from existing studies to estimate the unconditional mean effect size (the average relationship) between urban forest cover and race. The environmental justice (alternatively, environmental racism) hypothesis predicts that people and communities of color will have less access to environmental amenities; in this case, it predicts that people of color will live in areas with disproportionately low urban forest cover. While variation across studies complicates the comparison of the existent literature, it yields a rich opportunity for meta-analysis. We examined potential explanations for variation across studies by controlling for characteristics of the original studies, their empirical strategies, and their study sites using meta-regression, a tool of meta-analysis.

A note about terminology in this paper: for simplicity, in this paper we use *urban forest cover* as a catch-all term for a study's measure of urban trees and herbaceous plants, regardless of how it was operationalized in the original study. Many of the studies in this meta-analysis drew indicators from Census data to measure the percent of a population that is White, African American, Hispanic/Latinx (pronounced La-teen-ex), or another group. Studies often referred to these as measures of race, although some considered Hispanic an indicator of ethnicity. Given the complexity of racial and ethnic identity and the simplicity of the census indicators, this paper uses *race* to refer to a study's independent variable, regardless of how the original study identified it.

Meta-analysis is particularly useful in the case of urban forest equity because it can synthesize several literature that might not otherwise interact. In addition to including studies that are explicitly concerned with environmental justice and mapping and estimating inequity, our meta-analysis captured studies that described urban land use and land use change (Boone et al., 2010; Grove et al., 2006, 2014), study environmental stewardship choices by individuals (Grove et al., 2014; Pham et al., 2013) or public servants (Landry and Chakraborty, 2009), and advance methods for measuring urban forest cover (Szantoi et al., 2008).

Of note, we are constrained in our ability to examine the intersectionality of environmental inequity by the model specifications used in existing studies. We speak briefly to the intersectionality of race and class in our models and discussion, but acknowledge the limitations of this meta-analysis's contributions to a critical approach to environmental justice in this vein (Pellow, 2016) (we again refer readers to a companion study on income, Gerrish and Watkins, 2017). For example, a quantitative study might interact income and race variables to explore whether one variable moderates the other. Because the original studies in this meta-analysis do not conduct such tests, we cannot examine these relationships. Additionally, 35 of the 40 studies analyzed in this study are from the United States; a lack of English-language studies testing our hypotheses in other countries limits the generalizability of this work outside of the US.

To our knowledge, no meta-analyses have been done on municipal service provision equity and only one exists on environmental justice and environmental hazards (Ringquist, 2005; see also Mohai et al., 2009 for a review). Only a few meta-analyses have been conducted on topics in urban greening, and most of them are ecological studies; topics include amenity valuation (Brander and Koetse, 2011), intra-urban biodiversity (Beninde et al., 2015), local plant extinction (Duncan et al., 2011), organic material and environmental outcomes (Scharenbroch, 2009), and street tree survival (Roman and Scatena, 2011). Calls for synthesis of the environmental justice literature in urban forestry across many cities have been made (e.g. Frey, 2016).

This article is organized as follows: first we examine some of the theoretical reasons why access to urban forest cover may vary by race. Second, we explicate the literature search protocol, coding process, inter-coder reliability checks, tests for publication bias, and the methods for conducting meta-regressions. Third we examine the results of meta-regressions. Finally we discuss the implications for policy and research and conclude.

1.1. Understanding variation in urban forest cover

From the current literature, we hypothesized that estimates have varied across studies for four reasons: methodological choices, measurement choices for race, measurement choices for urban forest cover, and characteristics of the study site such as climate.

1.2. Methodological choices

Ongoing discourse in the environmental justice and urban forestry literature suggests differences in model selection and specification might yield differences in findings. Three conversations are particularly prevalent: whether to estimate unconditional or conditional effects, the importance of accounting for spatial autocorrelation, and the extent to which evidence of inequity varies with the size of the unit of analysis (see Noonan, 2008 for a discussion of these concerns with respect to environmental hazards).

1.2.1. Control variables—Results are 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, and inclusion of covariates is one indicator of a high study quality (Ringquist, 2005).

Including control variables allows authors to prevent spurious conclusions. For example, scholars might include indicators of both race and income in the same model (see Pham et al., 2012). This strategy addresses an enduring question in inequities research—-whether inequity is about race or about class or both (Mohai et al., 2009).

Moreover, urban forestry scholars use multiple covariates to compare competing theories. Findings suggest that features of the built environment such as terrain (Berland et al., 2015), street characteristics (Pham et al., 2017), construction age (Pham et al., 2017; Steenberg et al., 2015), vacant land (Nowak et al., 1996); or available planting space (Shakeel, 2012) help to explain urban forest distribution, and might explain variation better than social characteristics of a neighborhood (Berland et al., 2015; Pham et al., 2017; although see Melendez-Ackerman et al., 2014 for contrasting findings). Because features of the built environment are collinear with socio-demographic characteristics, we expect studies that control for built environment features to find weaker evidence of race-based urban forest inequity.

1.2.2. Accounting for spatial autocorrelation—Researchers, particularly Geographers, argue that adjusting for spatially correlated errors is critical for correctly estimating the relationship between urban forest cover and sociodemographic characteristics (more accurately, to correctly estimate standard errors) (e.g. Schwarz et al., 2015). According to Tobler's first law of geography—"everything is related to everything else, but near things are more related than distant things"—neighboring geographic units are likely more similar to each other than to more distant geographic units (Chakraborty, 2011). Non-independence of observations results in spatial error correlation, violating an ordinary least squares (OLS) assumption. Spatial autoregressive models (SAR) account for this autocorrelation by either introducing a spatial lag term or correcting standard error calculations. These strategies help capture unobserved historical and ecological factors that drive spatial patterns (Pham et al., 2012). Studies that have compared estimates from OLS and SAR models generally have found that SAR models demonstrate less inequity than OLS models (Schwarz et al., 2015).

1.2.3. Level of aggregation—Findings from environmental justice literature in other contexts suggest that evidence of inequity can vary by the size of the unit of analysis (Baden et al., 2007; Noonan, 2008; Ringquist, 2005; Tan and Samsudin, 2017). Urban forestry studies have used a variety of geographic units, including plots (Conway and Bourne, 2013), parcels (Shakeel, 2012), census block groups (Landry and Chakraborty, 2009; Schwarz et al., 2015) and census tracts (Heynen et al., 2006; Jenerette et al., 2007). Evidence that results vary with the level of aggregation would suggest that a seemingly minor choice, often made for convenience, can impact conclusions.

1.3. Measurement

Meta-analysis allows us to determine whether estimates of environmental inequity are sensitive to measurement choices (Mohai and Saha, 2006 discussed this concern with respect to environmental hazards). Some studies isolated individual groups (e.g. African American, Asian). Others measured a disambiguated minority population or the inverse, White population, or they measured visible minority or the inverse (in Canadian studies, e.g. Conway and Bourne, 2013). Studies measured urban forest cover in various ways as well. Some only included trees (Conway and Bourne, 2013), others trees and shrubs or woody vegetation (Clarke et al., 2013), and finally all vegetation or greenness (Jenerette et al., 2011; Szantoi et al., 2008, 2012; Tooke et al., 2010).

Urban forest cover data most commonly comes from on-the-ground inventories or from satellite or aerial imagery, which is then used to operationalize forest cover differently. For example, some studies defined tree cover using the percent of land area that is covered with tree cover. Others counted the number of trees per unit area. Other studies measured vegetation using wavelength intensity and the Normalized Difference Vegetation Index (NDVI) (Szantoi et al., 2012). The use of differing measures may contribute to variation in findings. However, few within-study comparisons of measurement techniques exist (without also varying other study characteristics). Conway and Bourne (2013) found some evidence that measurement might contribute to differences across studies. In their study, evidence of inequity varied across measures of canopy cover, stem density, and species richness. Shakeel (2012) found evidence that the relationship between urban forest cover and both features of the built environment and management had different directions when urban forest cover was measured as tree density and canopy cover. Another study found no significant difference across measures because no significant relationship was uncovered (Meléndez-Ackerman et al., 2014).

1.3.1. Domain—Urban trees grow on many types of land, including on 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 others restricted their study by looking at only urban forest cover on residential land (Grove et al., 2014), in public right-of-ways (Landry and Chakraborty, 2009), and in parks (Martin et al., 2004). Urban forest distribution might differ across these domains. For simplicity, we will refer to land types—including ecological, physical, and political categorizations—as the *domain.*

Urban forests in the United States are largely managed at the municipal level (Profous and Loeb, 1990) and municipalities determine the extent to which homeowners are responsible for the trees in front of their property on public land (in some cities, residents have sole responsibility for those trees; see Donovan and Butry, 2010). In addition, public officials or contracted arborists make many decisions related to urban forestry including where to plant, maintain, and remove trees. Thus variation in inequity across domains would illuminate drivers of inequity and appropriate remedies. For example, a larger proportion of the trees on residential land have been planted, compared to other land-use types (Nowak, 2012), and so evidence of inequity on residential land would suggest tree *planting* as a driver and avenue for redress.

Previous studies offer some evidence about the distributional results of municipal and nonprofit urban forestry programs. Watkins et al. (2016) found that nonprofit tree-planting programs were more likely to occur when the proportions of African American and Hispanic/Latinx residents were smaller in a neighborhood (although they found a negative relationship between planting and income) and another study found no relationship between tree requests and the percent of neighborhood residents who were White (Locke and Baine, 2015). If inequity is found to be higher on public land than on private land (see Pham et al., 2012 for example), it more directly implicates the behavior of public and nonprofit actors. Policy levers to address inequity will vary depending on whether inequity exists on public lands, private lands, or both.

1.4. Study sites

Previous *inter*-city research has found city-level characteristics are related to urban environmental conditions, including urban forest cover (Nowak et al., 1996). While single-city studies assist local actors in identifying and addressing existing inequities, using them to generalize about urban environments should be done with caution. Meta-analysis can help identify whether social and environmental city-level characteristics might drive within-city urban forest cover distribution.

1.4.1. Environmental conditions—We expect for there to be more robust urban forest cover in areas where the climate naturally supports woody vegetation (Nowak et al., 1996). In these climates, the urban forest is comprised of natural remnant forests, trees that have regenerated on their own, and planted trees (one in three trees is planted, on average). Cities in climates that do not support trees naturally, including grasslands and deserts, rely more heavily on active tree planting (Nowak, 2012), which requires time and financial resources. The potential unequal effect of tree-planting programs might be stronger in cities with non-supportive climates that rely more heavily on tree-planting activities, particularly on public land that is most often the target of planting programs. Several studies that have found higher canopy cover in African American neighborhoods posit that this might be a result of fence-line forests that grow unmanaged and unwanted (Heynen et al., 2006). If this hypothesis is true, we would expect to observe this relationship only in cities that have naturally-supportive climates.

1.4.2. Social inequity—Findings from some previous studies suggest socioeconomic inequality may be related to environmental inequity and poor environmental conditions and thus we examine these impacts in this article as well. For example, Morello-Frosch and Jesdale (2006) find both total cancer risk from air toxics and disparities in cancer risk were higher in more segregated metropolitan areas. A recent review by Cushing et al. (2015) found social inequity related to degraded air and water quality across cities. In a nationwide study, Jesdale et al. (2013) found a relationship between residential segregation and urban land-cover and that city characteristics—population, ecoregion, and rainfall—mediated this relationship.

1.5. Study lens

Finally, findings might vary across the many research fields studying urban forest cover distribution. For example, studies in geography might be less likely to find evidence of inequity because they employed spatial autoregressive models. Papers framed around environmental justice might have faced pressure to shelve non-significant findings, editors and reviewers may have rejected insignificant results, or the studies may select study sites with more prevalent racial injustice or significant minority populations.

2. Material and methods

We conducted this meta-analysis as defined by Ringquist (2013) and Borenstein et al. (2009). Meta-analysis combines the results of multiple quantitative studies (*original studies*) that examined the relationship between a particular dependent variable (urban forest cover) and a focal predictor (race). The unit of analysis in meta-analysis is the *effect size*, which is here a measure of the relationship between race and urban forest cover standardized across analyses, typically a regression coefficient on the covariate, race.

After an exhaustive literature search detailed in section 2.1, we conducted our analysis in three parts. First, we employed forest plots, a graphical illustration of the mean effect size for each quantitative study. Second, we examined the grand unconditional mean effect size using *meta-regression*, a technique similar to Weighted Least Squares. For each study and/or effect size we also code independent variables that we suspect influenced the magnitude of effect sizes, drawn from the theoretical constructs in section 1. We again used meta-regression to examine the impact of covariates.

2.1. Literature search

We conducted a systematic search of the existing literature to identify all original studies that had empirically tested the relationship between urban forest cover and race, including published manuscripts, conference papers and presentations, government reports, and white papers. To complete the search, we first refined and operationalized our research question and identified the focal predictors (independent variables of interest; see inclusion criteria below). We then populated a complete list of acceptable measures of the dependent variable urban forest cover, and generated coding instruments. To identify appropriate studies, we (1) defined a set of search terms that would yield original studies that met our inclusion criteria and (2) identified relevant document repositories that would contain original studies. In each repository, we conducted the same set of 16 searches—each search included the word "urban," one of four search terms related to the dependent variable ("*tree cover*," *canopy*, *forest*, and *vegetation*) and one of four related to the distribution of those trees either by race or income (*socioeconomic*, *demographic*, *distribution*, and *equity*). We conducted these 16 searches in the following databases: Academic Search Premier, American Psychological Association (APA) PsycNET, Google Scholar, Google Books, JSTOR, National Bureau of Economic Research database (NBER), ProQuest Dissertations and Theses Database (PQDT), Social Science Research Network (SSRN), and WorldCat (all documents then books only). We finished database searches on October 3, 2016.

Each unique search returned document titles, or "hits." We read each title and evaluated whether the study was *potentially relevant*. If so, we read the abstract and determined whether the potentially relevant study was *relevant*. In cases where a search yielded fewer than 300 hits, we reviewed the titles of all hits. In cases where a search yielded more than 300 hits, we reviewed up to 300 hits *or* searched at least 30 hits beyond the last "potentially relevant" hit, whichever came later. If we could not determine that a study was *not* relevant from the abstract, we made a conservative choice and marked it as relevant. We then read the full text of each relevant study to determine whether it satisfied all inclusion criteria and was *acceptable*. We then coded each acceptable study.

We employed three additional strategies to identify relevant studies. First, we emailed the first three authors of each acceptable study with a request for any additional relevant published or unpublished studies they had authored. Second, we conducted an ancestry and legacy search for each acceptable study; we reviewed each study citation (ancestry) and used Google Scholar to find studies that had cited the acceptable study (legacy). Finally, we sent a request for studies to subscribers to the Urban Forest Listserv, a listserv that facilitated discussion on theoretical and applied urban forest research (managed by the University of South Florida). We also received some unsolicited contributions from authors who knew of our ongoing research.

2.2. Inclusion criteria

For a study to be coded as acceptable and to be included in this meta-analysis, it must meet a predetermined set of inclusion criteria. First, the outcome measure must have been a measure of urban trees or vegetation (including trees, shrubs, and grass). 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). Second, the study must have had a measure of race as a right-hand side variable. To make valid comparisons between studies, we set a few additional restriction criteria. First, we excluded effects that did not measure race independently of other factors. For example, PRIZM data combined a set of neighborhood-level socioeconomic factors into one indicator, from which we could not isolate race.

We restricted our sample to studies that contained *intra*-city variation. Studies that exclusively compared urban forest cover between cities were excluded (for example, Heynen and Lindsey, 2003). To restrict the study to urban forests the study area must have included an urban center (similar to a metropolitan statistical area in the United States), though the study area could have included some outlying areas. Studies in which the area of interest was a larger area like a watershed, state, or country were excluded because the area was not predominantly urban.

Studies also needed a sufficient statistical test (e.g. compared against the distribution of t, z, χ^2, or F) to create an r-based measure. Finally, studies must have been available in English. Fig. 1 shows the search for potentially relevant documents (including duplicates) to the 42 acceptable studies used in this analysis. All numbers in Fig. 1 include data for our simultaneous search for the focal predictor income.

2.3. Study coding

From each study, we coded information about the effect size (the relationship between our outcome and focal predictors) and characteristics of the outcome measures, focal predictors, research design, and more. Overall, we coded 42 acceptable studies and 396 effect sizes.

Because original studies reported effect sizes using either Pearson’s r, Spearman’s r, or a regression coefficient, the relationship must be standardized. As in many social sciences meta-analyses, we chose an r-based measure, a measure rooted in Pearson’s product-moment correlation coefficient (r). Pearson’s r is bounded between −1 and +1 with 0 indicating no relationship and −1 or +1 indicating perfect linear relationships. In this study, effects in positive space indicate inequity (larger minority populations are associated with less urban forest canopy) and negative numbers are associate with negative inequity (minority populations are associated with more urban forest cover).

Some studies did not report sufficient information for us to calculate a precise effect size (for example, studies reporting coefficients but no standard error). In these cases, we took several strategies to accurately estimate the effect size. If the coefficient was statistically significant, we used statistical significance stars to calculate the most conservative effect size. In cases where the test-statistic or standard error was not reported, and a coefficient was not marked as statistically significant, we made the assumption that the effect size was zero.

Because an r measure based on Pearson’s correlation has two problems (it is both censored and heteroskedastic), we transformed it using the Fisher transformation to z, where $z = 0.5 \ln[(1 + r)/(1 - r)]$. This transformation also made the standard error convenient to calculate as $1/\sqrt{(N\ 3)}$. In practice, the transformation to z has very little practical impact on the interpretation of results if z is less than $|0.4|$ as is the case in most social policy research. z is our effect size. The average effect size (weighted by the standard error) is interesting in its own right, but can also be conditioned on study- and effect-level covariates to help explain why the effect size varies in the literature. The covariates coded for this paper are detailed below.

Although included in our searches, we excluded from this analysis any effects for which the independent and dependent variables were measured more than ten years apart (e.g. Boone et al., 2010; Locke and Baine, 2015) and any effects for which the dependent variable was a measure of change in the urban forest cover (e.g. Heynen, 2006) over time. These studies addressed different research questions than ours. We also could not include coefficients from geographically weighted regressions (e.g. Landry, 2013) because they offered no global coefficient estimates.

2.4. Study site data

Most studies provided little systematic study-site information, so we collected data from several additional data sources to investigate the extent to which environmental and social city characteristics drive variation. Many of the factors we might expect to relate to urban forest cover distribution, like history of residential segregation, historical development of the city, or total urban forestry budget either are not available or would be extraordinarily labor intensive to obtain for our sample. For cities in the US, we collected information on available proxies: city population, racial residential segregation, income inequality, and climate classification.

For study-site analyses, we limited our sample to coefficients from models of a single, U.S. city. Two studies included a single geographic location with boundaries larger than a single city. In these cases, we assigned the study site characteristics for the focal city: Miami for Miami-Dade County, Florida (Szantoi et al., 2012, 2008); and Minneapolis for Minneapolis and St. Paul (Kerns and Watters, 2012).

We obtained city-level racial residential segregation and racial composition data from the Racial Residential Segregation Measurement Project from the Population Studies Center at the University of Michigan (Farley, n.d.). We obtained income inequality estimates from Holmes and Berube (2016a) of the Brookings Institute.

We obtained climate information from an updated version of the Köppen-Geiger climate classification map (Kottek et al., 2006a, 2006b). Kottek et al. (2006a) classified climate types using global temperature and precipitation data from 1951 to 2000. With these data, they replicated the calculations from the older, very commonly used Köppen-Geiger classification map (last updated in 1961). The climate classification system was designed with vegetation in mind.

We obtained a shapefile that contained the climate classification map on a regular 0.5° 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 for each city.

2.5. Covariates

Based on the potential explanations for variation in urban forest cover discussed in section 1, we introduced a number of variables which we used to condition the effect size. Variables were dummy indicators, unless otherwise noted. In meta-regression, the unconditional intercept represents the average mean effect size. In multivariable meta-regression we can meaningfully interpret the intercept; the intercept is the average effect of the focal predictor when covariates are zero. Covariates can be coded so that the intercept represents a "best case" interpretation. Thus, to retain intercept meaning, we coded variables in reverse such as not peer reviewed and an absence of controls. We grouped covariates into five categories: measures of race and ethnicity, methodological choices, characteristics of outcome measures, publication characteristics, and study site characteristics.

2.5.1. Measures of race and ethnicity—We coded a set of dummy variables to indicate how the independent variable was measured: race classifications were Black or African American, Hispanic or Latinx, Asian, and a variable for Multiple Minorities (two or more racial or ethnic minority groups or White population and visible minority population). Estimates for the first three race classifications cannot be considered to be "pure" effects because many studies included other race classifications as covariates in the same model so the base case was not "all other individuals."

2.5.2. Methodological choices—The first variable we coded was an indicator variable for whether the effect was derived from a correlation coefficient or bivariate regression. We expected effects from correlation or bivariate regression to be larger than the effects from multivariate models due to confounders.

We coded *no income control* to indicate that the study did not control for income. Without controlling for income, a study may find evidence of racial disparities, when the distribution of trees might better be explained by (omitted) income, though we know these two factors are related. We also coded for features of the build environment using two variables. *No density control* indicates an effect size did not have a control for housing, street, or population density and *no age control* indicated a study did not control for the age of the housing stock or neighborhood. We expected effect sizes in studies that controlled for the built environment to be smaller than effect sizes in studies without these controls.

We coded *no SAR* to indicate that the authors did not control for spatial lags nor correct for correlated spatial errors. As discussed above, spatial autocorrelation underestimates standard errors and might make otherwise small effects statistically significant. We expected the studies which did not account for spatial autocorrelation to have larger average effect sizes.

We coded for the level of aggregation of the unit of analysis. A larger unit potentially contains more dissimilar urban forest cover and racial variation (Pham et al., 2017). However, larger geographies may have less measurement error compared to smaller geographies. We defined "large" geographies to be as large as or larger than U.S. census tracts. We classified units of analysis outside of the United States based on their relative size to U.S. Census geographies; if the size was unclear, we coded this variable missing.

Significance of this variable would suggest that estimates are sensitive to the level of aggregation though we do not have expectations for the sign of the coefficient.

2.5.3. Characteristics of outcome measures—We coded several characteristics of the outcome variable. *Vegetation* indicated that the outcome variable measured both trees and other vegetation. *Not % cover* indicated it was not measured using a measure of percent canopy cover. We coded for whether a study restricted its focus to *private land* (via parcel boundaries, for example) or included *mixed land* that is both private and public. The comparison case was studies that only studied urban forest cover on public land. We were more interested in access to the benefits of trees than to actual land ownership, so we considered studies that focused on land buffers along streets regardless of land ownership to be studies of public land.

2.5.4. Publication characteristics—We coded *ej lens* to indicate studies whose title or abstract included the word (in)equity, environmental justice, access or generally expressed concern about the unequal distribution of urban forests. We coded a suite of variables to indicate the field of study of the publication, including the most common, *geography*. For published works, we used the field of study of the journal. For dissertations, we used the field of study of the author. For other papers, we made a judgment call based on the publishing organization or author affiliation. *Non-peer-reviewed* indicated a study was not published in a peer-reviewed journal.

2.5.5. Study site characteristics—We coded study site population from original studies if reported. When studies did not report population, we searched Google for the city's population in the last year of urban forest cover data in the original study. We measured population in hundreds of thousands.

We measured racial residential segregation using the index of dissimilarity from the Racial Residential Segregation Measurement project from the Population Studies Center at the University of Michigan (Farley, n.d.). The index used 2000 census tract data to estimate the distribution of racial groups across census tracts within a city for the largest 250 cities in the United States– essentially how segregated a racial group was from another racial group. An index value of zero, the minimum, indicated no residential segregation between the two groups, and a value of 100, the maximum, indicated absolute segregation. For example, if a city had an index of dissimilarity for White and African American residents of 54, it would mean that either 54 percent of White residents or 54 percent of African American residents would have to move from one census tract to another to produce an even distribution.

We collected the index of dissimilarity between White individuals and individuals from four minority groups as measured in the U.S. Census – Black or African American, American Indian or Alaska Native, Asian, and Hispanic. From these data, we created a binary variable where 1 indicated a site's dissimilarity index was in the top quartile (top twenty five percent) of dissimilarity indices in the University of Michigan database of 250 cities. We chose the top quartile because our studies over-represented cities with high residential segregation and the top quartile offered sufficient balance between 1s and 0s.

We measured income inequality using the 95/20 ratio—the ratio of household income of the wealthiest 5 percent of households to household income of the poorest 20 percent of households. These estimates were calculated for the largest cities in 97 large U.S. metropolitan areas using the 2014 American Community Survey and obtained from Holmes and Berube (2016a). To illustrate, in Boston in 2014, the bottom 20 percent of households earned on average $14,942 per year and the top five percent earned $266,224 per year on average. The 95/20 ratio for Boston was 17.8, the highest in the country. The ratio for the United States was 9.3 and for the aggregated largest metro areas it was 9.7 (Holmes and Berube, 2016b). We generated a binary indicator that equaled 1 if a study city's 95/20 ratio was lower than 9.7.

The Köppen-Geiger climate classification uses a three-letter code to indicate three features: main climate, precipitation, and temperature (see http://koeppen-geiger.vu-wien.ac.at/present.htm). The scheme identifies five main climates: equatorial, arid, warm temperate, snow, and polar. After extracting the climate classification for each city from the shapefile, we created a suite of binary variables that indicated each main climate, precipitation, and temperature code. From these, we operationalized favorable growing conditions using a binary indicator that the climate precipitation code was *humid* and operationalized the alternative (unfavorable growing conditions) using a binary indictor that the climate code was *arid*. In the United States, most of the Eastern United States is classified as Cfb, meaning the climate is "warm temperate," the precipitation is "fully humid," and the temperature is "warm summer." The southwest is mostly arid climate, with patches of warm temperate climate. Los Angeles differs from the Eastern U.S. only in precipitation, with the code Csb and "summer dry" precipitation designation. Miami's climate is unlike most other study cities: it is equatorial and its precipitation is monsoonal.

2.6. Inter-coder reliability assessments

We conducted two inter-coder reliability assessments to evaluate our agreement on the acceptability of original studies and study coding. The first inter-coder reliability assessment measured whether the authors were similarly marking studies as acceptable after reading the full text. In that inter-coder reliability assessment, 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 effect sizes and several other important details of coding effect sizes such as the coefficient, p-value and test statistic, and whether the raw coefficient favored inequity or negative inequity. We also compared our coding of whether data collection from aerial/satellite imagery or an inventory as well as whether there was a control for housing age. The two authors had agreement of 99.6 percent, the lone difference being a typographical error (n = 247). Both assessments are considered "excellent" using typical rules of thumb. While percent agreement is sometimes limited in its applicability, we found that the high agreement rate obviated the need for further analysis using Cohen's Kappa or similar measures. Fig. 1 highlights the results of these inter-coder reliability assessments as well as their timing in the literature search process.

2.7. Descriptive statistics

Descriptive statistics for the control variables can be found in Table 1. We report the proportion of observations coded as 1 (the mean), the total number of observations coded as 1 out of the 388 total effect sizes, and the total number of observations.

2.8. Forest plots

Forest plots compare the average effect size between studies, creating a (weighted) average for each study so that all studies can be compared directly. To combine effects within a study, we multiplied each effect by its weight and then constructed an average weighted effect size (and standard error). A forest plot can also calculate the overall mean and confidence interval (as well as a prediction interval) for all studies. This overall mean and confidence interval will differ from the one found by the (more accurate) meta-regression because forest plots employ a study-level average rather than the individual effect-size level average, though the two averages tend to be similar.

2.9. Meta-regression

Meta-regression was the primary tool we used to examine and report meta-analytic results. Meta-regression allowed us to properly weight the unconditional mean effect size (the average relationship between urban forest cover and race) as well as condition the average effect size on (mostly) binary covariates. As with binary variables in a traditional regression analysis, these coefficients can be interpreted as the additive effect of "turning on" the binary variable.

Meta-regression involved a few more steps compared to ordinary least squares regression. First, each effect coded from original studies was weighted based on its sample size. This gave more weight (or preference) to studies which were estimated more efficiently, which muted the effects of statistical outliers from small samples on our results. The second step adjusted for heterogeneity of the estimates. In non-laboratory and non-experimental meta-analyses in particular, we often believe that our effects are drawn from a distribution of effects which are different for reasons other than sampling error alone: a random effects framework. In this framework, constructs such as the study location will have important impacts on the estimated effect. A random effects estimator is in opposition to using fixed effects, where the true population mean is fixed and effects are drawn from a distribution around that mean. To handle heterogeneity, we included an estimate of it in the effects' weights, $\boldsymbol{\tau}^2$ (and $\boldsymbol{\tau}$). $\boldsymbol{\tau}^2$ is an estimate of the dispersion of the distribution around a true effect. In other words, there are two components of the distribution of the mean effect size–a distribution of the true effect (rather than a population parameter) and sampling error. Including $\boldsymbol{\tau}^2$ attempted to decompose those two effects. The practical impact of including $\boldsymbol{\tau}$ in the weight was to place more emphasis on smaller studies than they would receive in a fixed effects meta-regression.

We report both $\boldsymbol{\tau}$ and the I^2 statistic. I^2 is a measure of the amount of heterogeneity of the estimate which is explained by factors other than random sampling (Higgins and Thompson, 2002). The I^2 statistic is large, roughly around .9 or 90 percent, which is common for meta-analyses in the social sciences, but would be highly unusual for lab experiments or randomized trials. For each of these values of I^2, the p-value of the chi-squared Q test would be less than .001, indicating that the random effects framework is preferable to fixed effects.

We also accounted for non-independence of effect sizes. Effects from social science research are often not drawn from independent samples, unlike for many meta-analyses in the sciences. Some studies have many estimates using the same data and our sample was no different. The largest study in our sample had 84 effects. Because of this non-independence, we used cluster robust variance estimators (CRVE), as employed in other recent meta-analyses (Gerrish, 2016; Ringquist, 2013).

2.10. Meta-regression model specifications

We tested a number of meta-regression specifications to examine our stated hypotheses. First, we estimated the unconditional mean effect size using a model with only the intercept. Next, we estimated the unconditional mean effect size for each operationalization of race and ethnicity.

We then specified three models to test our methodological hypotheses. The first controlled for studies that used correlation or bivariate OLS and for studies that did not account for spatial autocorrelation. The intercept of this model estimated inequity for studies with at least one control variable and that accounted for spatial autocorrelation. The second model added a variable for tract or larger. The intercept of this model estimated inequity for models that had at least one control variable, that accounted for spatial autocorrelation, and that used a unit of analysis smaller than a U.S. census tract. The third tested our hypotheses about the inclusion of specific control variables by including indicators that a study did not control for income, density, or neighborhood age.

We then estimated a model that controlled for outcome variables that measured both trees and herbaceous cover and outcome measures that were not percent canopy cover. The intercept estimated inequity in studies that measured percent tree cover.

Our fifth model tested our hypotheses about land ownership; it controlled for whether a study measured urban forest cover only on private land and on mixed private/public land. The intercept estimated inequity on public land only. Our sixth model combined effects of measurement and land type.

We then estimated a “best case” model (from the models above), in which the intercept measured inequity in studies that controlled for income, density, and neighborhood age; that accounted for spatial autocorrelation; that focused on public land only; and were peer-reviewed. Given the significance of land type in this model, we then estimated the same best case model without indicators of land type. The models described in the previous few paragraphs can be found in Table 3. In addition, considering the robust literature and particular interest in questions of environmental justice in the United States and the small number of non-US studies identified during the literature search, we re-estimated methodology, measurement, and domain models using only studies conducted in the United States (Table 4).

A second suite of models tested our hypotheses related to study lens and publication outlet (Table 5). We ran three bivariate meta-regressions to test the impact of non-peer review, study lens, and geography. We then combined ej lens and geography in one model and then all three study features in an additional model. The intercept of this model estimated inequity in studies that did not have an environmental justice lens, were not published in a geography outlet, and were peer-reviewed. In our final model in this set we added the interaction of environmental justice and non-peer-review. We re-estimated these models with only effects from studies of the United States (see appendix).

Next, we examined the presence of a "city effect" in seven models (Table 6). The first estimated the effect of city population (demeaned). The intercept estimated inequity when city population was at the sample mean. Then we tested whether residential racial segregation is related to variation in residential segregation by including indicators that a city had medium or low dissimilarity indices between White and African American residents and White and Hispanic/Latinx residents. The intercept estimated inequity in cities with high residential segregation between White and African American residents and White and Hispanic residents. In an appendix we also tested the robustness of these results by adding controls for the percent minority, the percent African American, and the percent Hispanic.

Our third study-site model combined population, residential segregation, and income inequality measures; the intercept of this model estimated inequity in cities with high residential segregation between African American and White residents, high income inequity, and when population was at the sample mean.

Four models examined whether climate influences urban forest inequity. We estimated whether observed inequity differed between arid climates and non-arid climates and estimated whether inequity differed between humid climates (intercept) and non-humid climates. We then added private and mixed land to these models to examine whether climate effects vary across types of land ownership.

In an appendix, we re-estimated the models described above with subsamples based on each distinct race classification. These models illuminate whether findings were systematic across groups or were driven by one or two particular race classifications.

2.11. Publication bias—Publication bias occurs when there is pressure to find statistical evidence that supports a particular conclusion that result in stuffing contradictory results in the file drawer and is an important concern for results of meta-analyses. We used two tests for publication bias, one visual and one statistical.

Fig. 2 displays our visual test, a confunnel plot. The funnel shape is formed by the standard errors from sample size, with large studies towards the top of the plot. The shaded cones are formed by the 90, 95, and 99 percent confidence intervals. We have graphed both peer-reviewed and non-peer-reviewed studies on the same plot. Black plus symbols represent peer-reviewed publications and gray Xs represent non-peer-reviewed studies. In the absence of publication bias, points would be fairly symmetric around the mean. Publication bias is evident in a confunnel plot if there is an absence of (typically) peer-reviewed studies in the lower left or right quadrant, suggesting studies have been shelved. Aside from a single small study that has a large positive effect (an outlier), it does not appear that publication bias is a significant concern because there are peer-reviewed and non-peer-reviewed studies in most quadrants.

Our statistical test used a dummy variable for non-peer-reviewed studies in meta-regressions, both unconditional and conditional on other factors.

3. Results

3.1. Forest plots

The forest plot in Fig. 3 compares the average effect size between studies. There appears to be significant heterogeneity between studies; markedly some studies find negative inequity, on average. The values in the rightmost columns report the statistics visualized in the body of the forest plot—the mean effect, 95 percent confidence intervals, and study weight. The bottom diamond in Fig. 3 reports the overall mean (diamond center) and confidence interval (diamond width). Because this is a mean constructed from study means, the mean in the forest plot varies slightly from the mean estimated in the meta-regressions below, which leveraged all individual effects within studies. See the appendix for forest plots for each unique race classification.

3.2. Meta-regression

Though forest plots are useful in comparing mean effect sizes between studies, they may mask methodological heterogeneity and they condense many effects (from correlation or regression) into a single average effect by study. Meta-regression, in contrast, allowed us to examine why effects vary between and within studies.

Tables 2–6 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 race across all relevant studies. Starting in column 2 and continuing to the right we added additional covariates as described above. Coefficient values around zero indicate no relationship between the variable and observed urban forest inequity. Positive coefficient values indicate the variable is related to observing higher inequity (or observing less negative inequity). Negative values suggest the variable is related to observing less inequity (or more negative inequity).

Estimating the unconditional mean effect size across all studies, we find a positive and significant relationship between race and urban forest cover (effect size = .050; s.e. = .024) signaling race-based inequity (Table 2). Effect sizes can be interpreted similarly to Pearson's correlation coefficient, r, bounded between −1 and +1. Recent meta-analyses using meta-regression suggest that observed effect sizes typically range between 0 and ±0.20 (Gerrish, 2016). This effect size can be interpreted as small to modest in policy/practical size. In models restricted to one race classification (Table 2), we find significant inequity in studies that examine Hispanic/Latinx populations (0.069) and studies that examine Multiple Minorities together (0.106; a rather large observed effect size in the authors' experience). We find no significant inequity for studies that focused on African American or Asian populations. When we focused on studies in the United States, the unconditional mean effect size is marginally larger (.051; s.e. = 0.027) but no longer statistically significant; race classification-specific model results are consistent in U.S.-only models (see appendix).

Results present some evidence that methodological choices explain variation across studies (Table 3 Models 2, 3, and 4); none of the coefficients of methodological variables are statistically significant, but the intercept (mean effect size) is also no longer statistically significant once methodological choices are accounted for. This suggests that studies that include at least one control variable, account for spatial autocorrelation, and use larger units of analysis do not find evidence of inequity. There is little evidence that measurement influences study results. The coefficients on *vegetation* and *not percent cover* are not significant and the intercept is positive and significant; studies that measure urban forest cover as the percent tree canopy cover find significant evidence of inequity (Table 3, Model 5).

In Table 3 Model 6 we report strong evidence that the magnitude of inequity varies across domain. The coefficient on private land shows a strong negative and significant relationship with urban forest cover (−0.158). The positive and significant intercept (0.097) reveals substantial inequity on public land. These relationships are consistent and are slightly larger when we combine measurement and domain variables in Model 7.

In our “best case” model (Table 3 Model 8), a significant coefficient on no income control suggests higher inequity in studies that do not control for income. However, the effect of no income control is not very robust and disappears when we remove domain from the best case model (see Model 9). The significant effect of private land remains in the best case model. Its significance across specifications suggests that evidence of inequity on public land is robust. The results are similar (with small changes in coefficient size) when we focused on studies from the United States (see Table 4).

Regarding publication bias, we find no significant difference in observed inequity between peer-reviewed and non-peer-reviewed studies (Table 5, Model 2), studies with and without an environmental justice lens (Table 5, Model 3), or between geography and non-geography studies (Table 5, Model 4). Studies without a focus on environmental justice found on average no significant evidence of inequity. The same holds in models 5 and 6 that combined study features. Adding the interaction of lens and peer-review (Model 7) revealed significantly higher evidence of inequity in non-peer-reviewed environmental justice studies.

Significant effects of population size, racial residential segregation, and climate suggest that the notion of a “city effect” is founded (Table 6). We find that cities with larger populations have significantly less urban forest cover inequity and cities with the mean population in our sample have, on average, significant inequity in urban forest cover (Table 6, Model 2). Contrary to our expectations, we find consistent evidence of higher inequity in cities with lower residential segregation of White and African American residents and no relationship between inequity and segregation between White and Hispanic/Latinx residents (Table 6, Model 3). These results hold when we controlled for population and income inequality (Table 6, Model 4) and when we controlled for city-level demographics (see appendix). We find no significant relationship between income inequality and race-based urban forest cover inequity (Table 6, Model 4).

Table 6 also reports a significant relationship between climate and inequity for both measures of climate (arid climate and humid climate). As expected, we find evidence of significantly higher inequity in arid cities (Model 5) and significantly lower inequity in humid cities (Model 6), even when controlling for domain characteristics (Model 7 and 8). Consistent with the results in Table 3, the coefficient on private land is negative and significant. Significant intercepts in Models 7 and 8 also reveal significant inequity in urban forest cover on public land across climate specifications (with one exception in Model 5).

3.3. Results by race classification

Tables 3–6 reveal significant study and site variables. Given the differences in unconditional mean effect sizes across race classifications reported in Table 2, we tested our hypotheses using a sub-sample of each race classification (results in appendix). We also reported a forest plot for each race classification.

These models illuminate whether the observed relationships are driven by inequity for a specific racial group or are consistent across groups. Insufficient sample size for Asian-only effects (effect sizes n = 46; studies n = 5) prevents discussion here. For the other effect sizes, we have substantial variation for most variables (i.e. enough 1s and 0s). We make note where this is not the case.

In models with all effect sizes discussed above we find no significant methodology variables and no significant intercepts. In race-specific models, no control for spatial autocorrelation is positive and significant in Hispanic/Latinx only and Multiple Minorities models and the level of aggregation is large and significant in African American models. We find negative inequity for African Americans when models control for income, density, and housing age.

When we account for whether a study controls for income, we find no evidence of inequity for any race classification, and African American studies that control for income and do not control for other races (i.e. they compare African Americans to the rest of the population) find *negative* inequity.

Results of our race-specific models about measurement and domain help identify particular areas of inequity. Unlike in the all-effects models, our race-specific models find that outcome variable measurement matters. We find significantly lower inequity for African Americans and higher inequity for Hispanics/Latinx when forest cover includes herbaceous cover. These two relationships seem to cancel each other out in the all-effects model. We also find higher inequity for African Americans when forest cover is not measured as percent cover.

Inequity of urban forest cover on public land is present across sub-samples, even after controlling for measurement and domain variables. A large, negative, and significant coefficient on private land in the African American model suggests substantial negative inequity.

Studies with an environmental justice lens that focus on African Americans find significant inequity, whereas studies without an environmental justice focus find negative inequity. The interaction term between environmental justice and peer-reviewed explains some of the effect, but the effect of environmental justice lens remains significant. These models do not control for other characteristics of studies that might be correlated with study lens.

African American effects seem to be driving much of the study-site related findings. African Americans experience higher urban forest inequity in cities with lower residential segregation between White and African American residents, experience significantly less inequity in humid climates, and significantly less inequity on private and mixed land (this last relationship is also significant in multiple minority models). African Americans experience very large and significant inequity on public land in non-humid climates.

4. Discussion

Using the tools of meta-analysis, our intent was to paint a more precise and nuanced portrait of previous research that had examined urban forest distribution. We completed a comprehensive search of the literature to identify all effect sizes that test the relationship between urban forest cover and race, however measured.

We find mixed evidence of race-based inequity in urban forest cover though we find systematic inequity in the unconditional mean effect size, and in studies that examined Hispanic/Latinx populations and disambiguated minority populations (at least two racial/ethnic minority groups). The results for Hispanic/Latinx and Multiple Minority populations are robust to controls for whether the model included other variables that indicated race or ethnicity, measurement differences, and land type. However, evidence of inequity disappears when we account for methodological choices such as controlling for spatial autocorrelation or when the study controlled for income.

Our tests for whether methodological characteristics, measurement characteristics, and study site characteristics explain variation provide interesting conditional results. We find mixed evidence for the effect of methodological choices. In race specific models, significant inequity disappears when models control for spatial autocorrelation or when models control for income (In a companion meta-analysis, we found evidence of income-based inequity [Gerrish and Watkins, 2017]). In this paper, we find that income appears to mediate the relationship between race and urban tree cover. Combined, our findings suggest that the story of urban forest inequity is likely driven more by income than race.

Importantly, when we tease out locations of inequity we find significant evidence of race-based inequity on public land. Inequity on public land is even higher in non-humid climates; the largest inequity in this study is in models that examine African American access to urban forests on public land in non-humid climates. We also find that tree cover on private land has a positive relationship with minority population, particularly for African American residents. This meta-analysis cannot speak directly to *why* we observe these differences, but our findings can speak to the relative validity of hypotheses in the literature about why urban forest cover might differ systematically. These hypotheses are about public service provision, the built environment, residential preferences, legacy effects, and social stratification.

Our finding that race-based inequity exists primarily on public but not on private land suggests that inequity is at least partially inequity in *public service provision* and in part driven by the choices of municipal policy makers and public agents. The influence of public policy and municipal agents are more constrained on private land, where private property rights protect the individual choices of property owners.

The *built environment* hypothesis expects that a positive relationship between population density and minority population drives urban forest inequity. Our finding of negative inequity on private land cannot be explained by this hypothesis. An economic perspective might argue that urban vegetation reflects the *preferences* of urban residents, either manifest by cultivating vegetation or by moving to areas with vegetation that align with their preferences. According to this hypothesis, our finding of a positive relationship between private land vegetation and minority population suggests that minority residents have a stronger preference for vegetation than other groups. Pham et al. (2012) posited that the positive relationship between backyard trees and visible minorities they observe in Canada might reflect preferences for gardening among immigrants. Furthermore, if vegetation on private land (where residents have more direct control over their land) reflects stronger preferences for vegetation by people of color, than evidence of inequity on public land even more strongly suggests inequity in public service provision. A preferences lens might also interpret this finding to suggest that residents are compensating for low public urban forest cover by cultivating higher urban forest cover on their private land.

It is important to note for units of analysis larger than a parcel, we cannot discern which residents have higher vegetation in these neighborhoods; for example, we cannot say for sure that people of color are compensating for low public forest cover because our observations at the neighborhood level could be driven by their White neighbors. This limitation is not very important when the concern is about access to urban forests benefits that are more diffuse (e.g. cooling, air purification) because residents of color are still exposed to the benefits of neighboring trees. But in the case of more localized benefits, such as aesthetics, and in the case of interpreting causal mechanisms, it is.

In contrast, a *legacy* hypothesis posits that the preferences of White city residents dictate the vegetative environment of people of color. One form of this hypothesis posits that suburbanization and White flight left behind large, stately street trees in neighborhoods now occupied by minority residents (Boone et al., 2010), leading to negative inequity (see Battaglia et al., 2014 for an example of the opposite). Our finding of urban forest inequity on public land does not support this legacy hypothesis. If this phenomenon is occurring, then the influences of municipal activity and public policy or other historical factors are even stronger.

A *social stratification* or *luxury effect* hypothesis, specific to private land, posits that vegetation is a reflection of wealth (Hope et al., 2003; Mennis, 2006). Wealthier residents are able to move to areas with more vegetation, invest in vegetation on their properties, and/or attract higher public investment. We find evidence that income explains part of, but not all of, the story of inequity. When we control for income, we observe inequity on public land in all-effects models but not in race-specific models.

Finally, previous work has suggested that the "*fence-line forest*," comprised of nuisance trees that have grown along unmaintained fences, might explain negative inequity on private land (Heynen et al., 2006). This might be accompanied by vegetation growing on abandoned lots. No original studies examined private land in non-humid cities where we expect volunteer tree regeneration to be relatively low, so we cannot speak directly to this hypotheses.

We cannot determine from our results which of these hypotheses explain(s) inequity in urban vegetation. We can note that our results do not support the claim that people of color prefer less urban vegetation. They do support the claim the actions of public agents and or city policy contribute to inequity in urban forests, particularly on public lands.

Given its home in multiple disciplines, urban forestry research offers a unique opportunity to assess the extent to which the lens of environmental justice was related to published or reported outcomes. Collectively, we find no evidence that peer review or discipline is related to inequity. We find some evidence that study lens is related to observed inequity and that this effect is likely driven by non-peer-reviewed studies. When we examine this hypothesis with race-classification subsamples, we find that there are significantly higher findings of inequity in both published and unpublished studies with an environmental justice lens for African American effects.

This variation may come from a number of factors; it could be the case that there are other unaccounted for differences (in methodology, or study site) between studies with and without an environmental justice lens; that scholars are more likely to test environmental justice concerns in cities where they suspect there is inequity; or that scholars that find inequity in their results are more likely to then frame a narrative in their paper that focuses on inequity. It might be the case that authors are more likely to submit or editors are more likely to accept publications that find significant evidence of race-based inequity for African Americans. Our analysis cannot tell us whether any of, or which of, these hypotheses explains the variation we observe.

In addition to the evidence that study characteristics explain effect size variation, we find fairly strong and robust evidence of a "city effect." We find a relationship with population, residential segregation, and local climate. Contrary to expectation, we find more evidence of inequity in cities that have low or medium racial residential segregation between White and African American residents, a result that is robust to controlling for population and income inequality, and to controlling for population demographics. Consistent with our hypothesis, we also find higher inequity in climates that are less supportive for tree success (i.e. climates that require more time and financial resources to provide tree cover); of the race-specific models, this relationship is strongest for African American effects.

These study site models are not intended to identify the precise features of a city that determine the distribution of its urban forests. Rather, they serve as an indication that race-based inequity varies significantly across *studies* because race-based inequity varies significantly across *cities*.

4.1. Implications for research and practice

The results of this meta-analysis offer several implications for research and practice. First, our results suggest instances of inequity are not consistent in magnitude across racial and ethnic minority groups and across cities. Scholars should be intentional and transparent in the way they measure minority groups and studies should be written and read with this limited external validity in mind. Relatedly, when possible, studies that evaluate the distributional outcomes of urban forestry programs should first describe the current distribution of the urban forest in the study city. This will help the authors and readers interpret the extent to which urban forestry programs will remedy, create, or exacerbate inequity.

We tested the influence of methodological choices and found mixed evidence of their importance. Controlling for income and features of the built environment reduced observed inequity. We find moderate evidence that controlling for income changes estimates of race-based inequity, which suggests the story of urban forest inequity is more about socioeconomic class than about discrimination or different urban forest preferences. Scholars should be thoughtful about the hypothesis they are interested in answering and justify their use of control variables accordingly. If scholars are interested in describing the lived experiences of people of color, a control for income might over-control and cloud the “true extent” of urban forest access. If scholars are interested in drivers of inequity, or *why* we observe a certain urban forest distribution, controlling for other potential explanations (like income or physical neighborhood features) is necessary to estimate the “true effect” of race-based discrimination on urban forest distribution. We suggest scholars run models both with and without income to determine the extent to which it influences their particular case.

Our mixed findings about spatial autocorrelation and level of aggregation suggest that decisions about these methods are worth making carefully but are not highly consequential to a study’s results. In the companion piece to this analysis, we find spatial autocorrelation controls to significantly impact results related to income-based urban forest inequity (Gerrish and Watkins, 2017). Because studies often examine both race and income, we suggest scholars use spatial autocorrelation adjustments and employ multiple strategies as robustness checks.

That studies with an environmental justice lens find more evidence of inequity supports our claim that synthesis across disciplines is important. Similar research methods with different results might have different research framing – combining studies across disciplines will yield a more complete picture of the state of the world.

Effect of methodology, measurement, and study sites are not consistent across racial groups, and we tended to be more accurate in our predictions for African American effect sizes than for others. The urban greening literature should continue to study the urban forest experiences of Hispanic/Latinx and other minority residents to strengthen hypotheses for these groups.

Our results yield two important findings for the practice of urban forestry. First, wide variation across studies suggests that urban forest policy and management should be informed by city-specific analyses of patterns of race-based inequity. These analyses are a ripe area for collaboration between scholars and municipal and nonprofit urban forestry practitioners. Our results also suggest that less data-intense approaches (e.g. using NDVI) produce fairly similar results to exhaustive approaches, suggesting that resource-constrained cities might get a pretty accurate picture of their urban forest with less data-intensive approaches.

Perhaps most importantly, our study finds significant evidence of urban forest cover inequity on public land. Because the location of urban trees is the result of a complex process that involves the actions of multiple management agents over time (Landry, 2013; Pham et al., 2017), patterns in today's urban forest cannot be easily ascribed to a few explicit actions of individuals, neighborhoods, or city governments. The suburbanization of cities in the United States privileged White Americans with clean and inexpensive environments, eroded quality of life in dense urban areas, and relegated African American communities to areas that were unattractive to White city-dwellers (Pulido, 2000). Urban "revitalization" now threatens to do the opposite (Pearsall and Anguelovski, 2016). Current access to the urban forest is a snapshot in a long process of urbanization, suburbanization, and re-urbanization. Although observed inequity is unlikely to be the result of intentional acts of discrimination by a few select individuals, evidence of environmental injustice and racism need not be the result of intentional actions. Unjust outcomes from race-neutral decision making are sufficient evidence of environmental racism (Pulido, 2000; Sicotte, 2014). Given the evidence presented in this paper that access to public urban canopy cover is disproportionately lower for people of color, and regardless of the process that produced that inequity, there is a clear need for municipalities and nonprofits to evaluate the equity consequences of urban forest policy and management. This evaluation should particularly consider the values and preferences of individual neighborhoods in crafting just and successful programs (Ordóñez Barona, 2015). A broader set of policy tools is available for urban forest activity on public land so while our finding of inequity on public land is troubling, it also suggests modifying public policy and the behavior of public agents might offer remedies to inequity.

This meta-analysis synthesizes previous quantitative literature about the distribution of the urban forest with respect to race and ethnicity. It offers, to date, the most comprehensive statement of whether inequities exist and the magnitude of those inequities. However, in the cases where we find inequity, the meta-analysis does not tell us the *cause* of that inequity, nor does the analysis illuminate (in)equity in access to the *benefits of* or the *quality of* the urban forest. Environmental justice studies of urban forest cover rely on the often unspoken assumption that the expected value of ecosystem services from each unit of the urban forest is the same. However, tree benefits vary with condition, domain, species, and resident preferences. Even more fundamentally, many of the papers in this meta-analysis rely on the assumption that trees have universal net positive value and unequal forest cover is an injustice to be remedied. The assumption may not be universally true (Battaglia et al., 2014). For example, canopy cover estimates include trees on abandoned lots and along fences which might not be appealing or desired by residents and damaged trees that pose risks to residents. More attention to the quality and desirability of trees will improve the body of research.

5. Conclusion

In this meta-analysis, we examined studies which had estimated the relationship between urban trees and vegetation (the outcome variable) and race (the focal predictor). We employed the techniques of meta-analysis—forest plots and meta-regressions—which allowed us to quantitatively accumulate original studies into standardized effects. Using meta-regression, we conditioned the observed mean effect size on a number of theoretically important variables. We tested hypotheses related to methodology, measurement, domain, publication features, and study site characteristics.

We find evidence of race-based inequity, but that best methodological practices reduce the magnitude and significance of this evidence. We find consistent and significant urban forest inequity on public land, suggesting a clear need for urban forest policy and practitioners to consider the equity implications of current practices and policy.

Supplementary Material

Refer to Web version on PubMed Central for supplementary material.

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Appendix A. Supplementary data

Supplementary data related to this article can be found at https://doi.org/10.1016/j.jenvman.2017.12.021.

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* Indicates the study was coded and included in the meta-analysis.

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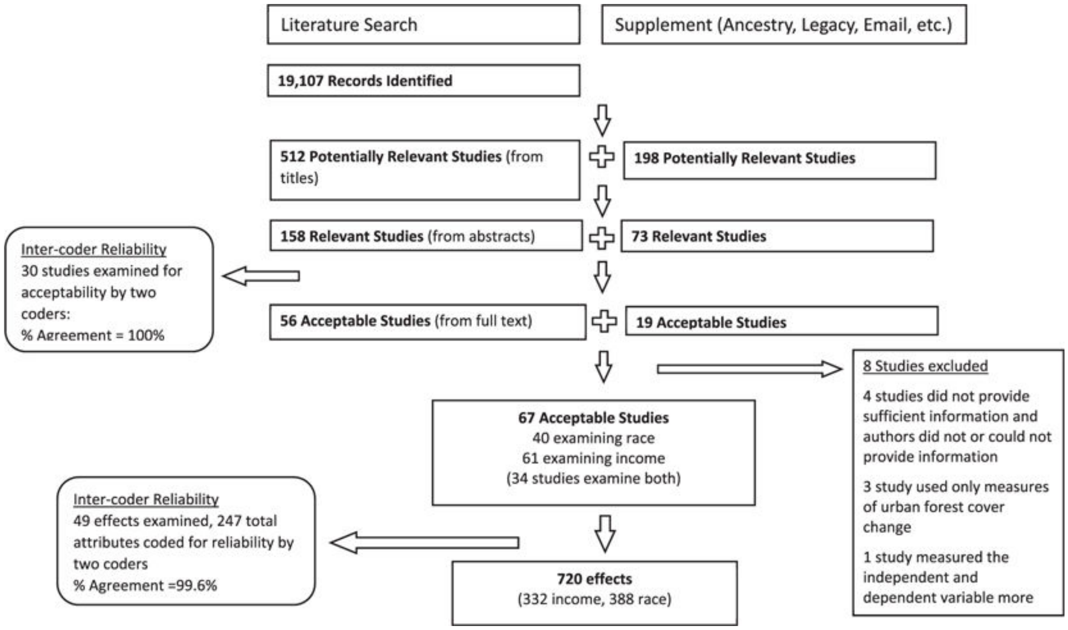

Fig. 1. Flowchart of Literature Search Process and Inter-Coder Reliability Assessments

Results are from a combined search for studies that estimate the relationship between urban forest cover and either race or income. See Gerrish and Watkins, 2017 for results of income analysis.

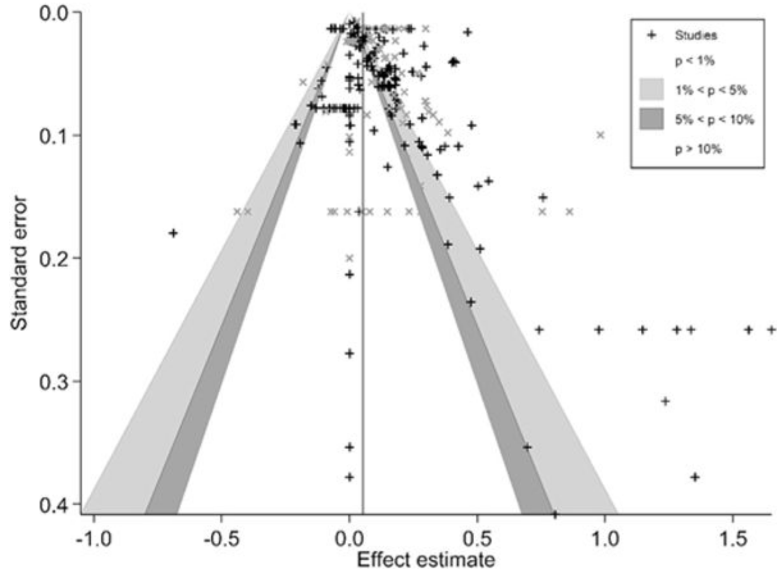

Fig. 2.

Black plus symbols represent effect sizes from peer-reviewed publication. Gray Xs are from non-peer-reviewed studies. Effect sizes are sorted by sample size; large samples are 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 horizontally aligned are typically effect sizes from the same study. The vertical black line indicates the mean effect size.

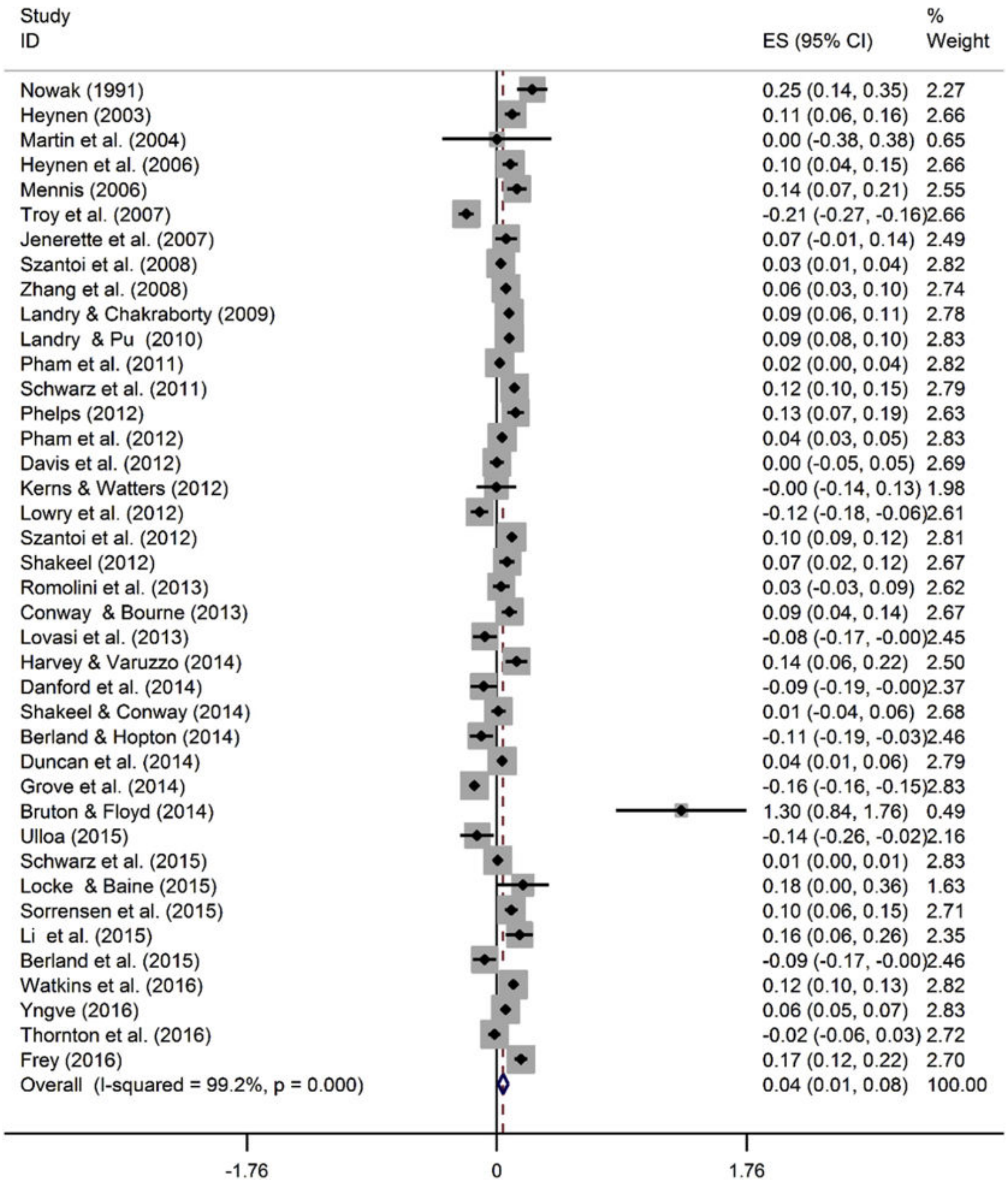

Fig. 3.

Forest Plot. Notes: black center dots (horizontal bars) represent a study's mean effect size (95 percent confidence interval). The size of each gray box visualizes the study's weight. The same statistics are reported in the right two columns. The bottom diamond reports the overall mean effect size and its standard error Berland and Hopton, 2014; Bruton and Floyd, 2014; Davis et al., 2012; Duncan et al., 2014; Harvey and Varuzzo, 2013; Heynen, 2003; Landry and Pu, 2010; Li et al., 2015; Lovasi et al., 2013; Lowry et al., 2012; Nowak, 1991; Perkins et al., 2004; Pham et al., 2011; Phelps, 2012; Romolini et al., 2013; Schwarz et al., 2011; Shakeel and Conway, 2014; Sorrensen et al., 2015; Thornton et al., 2016; Troy et al., 2007; Ulloa, 2015; Yngve, 2016; Zhang et al., 2008.

Table 1

Descriptive statistics.

	Mean	Total	Total Obs.
Number of observations	1344.477	521,657	388
Correlation coefficient or bivariate OLS	0.454	176	388
No control for spatial error or lag	0.704	273	388
Spatial Unit of analysis is census tract or larger	0.316	122	386
Spatial unit of analysis is a parcel or a household	0.054	21	388
No control for income poverty or wealth	0.665	258	388
No control for density	0.665	258	388
No control for housing age	0.668	259	388
Outcome measure is both trees and herbaceous	0.173	67	388
Outcome measure is NOT % cover	0.209	81	388
outcome measure is tree or stem inventory	0.057	22	388
Treatment variable measures African American or Black[a]	0.304	118	388
Treatment variable measures Hispanic or Latinx[a]	0.276	107	388
Treatment variable measures Asian[a]	0.119	46	388
Treatment variable measures disambiguated minority[a]	0.289	112	388
Survey frame is private land only	0.160	62	388
Survey frame is mixed public/private land	0.570	221	388
Study has a focus on Environmental Justice	0.760	295	388
Discipline is Geography	0.302	117	388
Study is non-peer-reviewed	0.299	116	388
Population in 100000s	16.246	6271.113	386
Low dissimilarity index (White: African American)	0.301	102	339
Low income inequality	0.128	40	312
Arid climate (Köppen-Geiger)	0.082	28	343
Humid climate (Köppen-Geiger)	0.685	235	343

Notes: 388 total effect sizes. All variables, except for effect size are binary variables. Mean reports the proportion of observations coded as “1” and Total reports the total number of observations coded as “1.” Effects derived from 40 studies with 521,657 total observations.

[a]Or inverse.

Table 2

Unconditional mean effect size by race classification.

	All effects	African American	Hispanic/Latinx	Asian	Multiple Minority
Mean Effect Size	**0.050* (0.024)**	**−0.012 (0.046)**	**0.069* (0.028)**	**0.038 (0.021)**	**0.106*** (0.023)**
Number of Observations	388	118	107	46	112
Number of Studies	40	20	19	5	23
R2	0.000	0.000	0.000	0.000	0.000
Adj. R2	0.000	0.000	0.000	0.000	0.000
Estimate of τ	0.124	0.135	0.095	0.066	0.112
I^2	0.950	0.947	0.885	0.728	0.858

Notes:

* $p < .05$

** $p < .01$

*** $p < .001$. 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 in parentheses. Positive coefficients indicate inequity.

Table 3

Meta-regression: Methodology, measurement, domain, and best case models.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Mean effect size	0.050* (0.024)	0.007 (0.019)	−0.004 (0.021)	−0.023 (0.027)	0.054* (0.024)	0.097** (0.031)	0.107** (0.031)	0.036* (0.016)	−0.021 (0.021)
Correlation coefficient or bivariate OLS		0.042 (0.042)	0.039 (0.042)						
No control for spatial error or lag		0.034 (0.037)	0.035 (0.035)					0.018 (0.026)	−0.015 (0.040)
Spatial unit of analysis is census tract or larger			0.037 (0.033)						
No control for income poverty or wealth				0.043 (0.024)				0.064* (0.026)	0.045 (0.025)
No control for density				0.094 (0.052)				0.035 (0.036)	0.096 (0.064)
No control for housing age				−0.026 (0.036)				−0.043 (0.029)	−0.025 (0.034)
Outcome measure is trees *and* herbaceous					−0.025 (0.058)		−0.008 (0.035)		
Outcome measure is NOT % cover					0.004 (0.041)		−0.022 (0.034)		
Survey frame is private land only						−0.158* (0.070)	−0.163* (0.067)	−0.151** (0.046)	
Survey frame is mixed public/private land						−0.034 (0.039)	−0.041 (0.037)	−0.030 (0.020)	
Study is non-peer-reviewed								0.024 (0.023)	0.019 (0.032)
Number of Observations	388	388	386	388	388	388	388	388	388
Number of Studies	40	40	39	40	40	40	40	40	40
R2	0.000	0.051	0.069	0.126	0.005	0.147	0.151	0.237	0.129
Adj. R2	0.000	0.046	0.062	0.119	−0.000	0.143	0.142	0.223	0.118
Estimate of τ	0.124	0.121	0.119	0.114	0.124	0.111	0.111	0.104	0.114
I^2	0.951	0.946	0.946	0.932	0.946	0.928	0.923	0.908	0.931

Notes:

* $p < .05$

** $p < .01$

*** $p < .001$. 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 in parentheses. Positive coefficients indicate inequity.

Table 4

Meta-regression: Methodology, measurement, domain, and best case models; U.S. ONLY.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8	Model 9
Mean effect size	0.051 (0.027)	0.004 (0.021)	−0.006 (0.022)	−0.023 (0.029)	0.054* (0.026)	0.101** (0.033)	0.118*** (0.028)	0.058* (0.022)	−0.021 (0.023)
Correlation coefficient or bivariate OLS		0.047 (0.044)	0.043 (0.044)						
No control for spatial error or lag		0.037 (0.039)	0.035 (0.038)					0.033 (0.024)	−0.016 (0.044)
Spatial unit of analysis is census tract or larger			0.046 (0.037)						
No control for income poverty or wealth				0.042 (0.028)				0.079* (0.032)	0.043 (0.029)
No control for density				0.096 (0.055)				0.003 (0.033)	0.096 (0.069)
No control for housing age				−0.024 (0.035)				−0.057 (0.030)	−0.024 (0.034)
Outcome measure is trees *and* herbaceous					−0.023 (0.078)		0.012 (0.043)		
Outcome measure is NOT % cover					0.005 (0.051)		−0.045 (0.034)		
Survey frame is private land only						−0.181* (0.079)	−0.200* (0.078)	−0.199*** (0.047)	
Survey frame is mixed public/private land						−0.039 (0.042)	−0.052 (0.036)	−0.039 (0.022)	
Study is non-peer-reviewed								0.031 (0.022)	0.030 (0.033)
Number of Observations	354	354	352	354	354	354	354	354	354
Number of Studies	35	35	34	35	35	35	35	35	35
R2	0.000	0.057	0.082	0.127	0.003	0.169	0.180	0.266	0.134
Adj. R2	0.000	0.052	0.074	0.120	−0.003	0.164	0.171	0.251	0.122
Estimate of τ	0.129	0.125	0.122	0.118	0.129	0.113	0.114	0.105	0.118
I^2	0.952	0.946	0.945	0.930	0.945	0.932	0.926	0.911	0.930

Notes:

* $p < .05$

** $p < .01$

*** $p < .001$. 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 in parentheses.
Positive coefficients indicate inequity.

Table 5

Meta-regression: Publication characteristics.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Mean effect size	0.050* (0.024)	0.037* (0.017)	−0.030 (0.052)	0.043* (0.021)	−0.032 (0.051)	−0.041 (0.051)	−0.007 (0.045)
Study is non-peer-reviewed		0.046 (0.041)				0.045 (0.030)	−0.089 (0.057)
Study has a focus on Environmental Justice			0.105 (0.058)		0.104 (0.061)	0.105 (0.061)	0.056 (0.051)
Geography				0.023 (0.041)	0.010 (0.043)	−0.008 (0.033)	0.003 (0.024)
EJ* not peer-reviewed							0.171* (0.064)
Number of Observations	388	388	388	388	388	388	388
Number of Studies	40	40	40	40	40	40	40
R2	0.000	0.022	0.105	0.005	0.106	0.124	0.181
Adj. R2	0.000	0.019	0.103	0.003	0.102	0.118	0.172
Estimate of τ	0.124	0.124	0.115	0.124	0.115	0.114	0.109
I^2	0.950	0.949	0.933	0.949	0.931	0.931	0.924

Notes:

* $p < .05$

** $p < .01$

*** $p < .001$. 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 in parentheses. Positive coefficients indicate inequity.

Table 6

Meta-regression: Study site characteristics.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7	Model 8
Mean effect size	0.053 (0.028)	0.058** (0.018)	0.003 (0.035)	0.035* (0.013)	0.047 (0.029)	0.104*** (0.016)	0.100** (0.034)	0.153*** (0.035)
Demeaned city population (in 100,000)		−0.002** (0.001)		−0.002* (0.001)				
Low dissimilarity index (White: African American)			0.094*** (0.024)	0.053** (0.017)				
Low dissimilarity index (White: Hispanic/Latinx)			0.045 (0.031)					
Low income inequality				0.006 (0.027)				
arid climate (Köppen-Geiger)					0.074* (0.028)		0.055* (0.024)	
Humid climate (Köppen-Geiger)						−0.074* (0.034)		−0.059* (0.024)
Survey frame is private land only							−0.177* (0.083)	−0.172* (0.083)
Survey frame is mixed public/private land							−0.042 (0.042)	−0.058 (0.041)
Number of Observations	343	343	339	312	343	343	343	343
Number of Studies	32	32	32	29	32	32	32	32
R2	0.000	0.223	0.140	0.259	0.018	0.056	0.181	0.202
Adj. R2	0.000	0.221	0.135	0.252	0.015	0.053	0.174	0.195
Estimate of τ	0.127	0.106	0.107	0.095	0.126	0.122	0.110	0.108
I^2	0.949	0.914	0.929	0.917	0.947	0.942	0.925	0.924

Notes:

* $p < .05$

** $p < .01$

*** $p < .001$. 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 in parentheses. Positive coefficients indicate inequity.