From Kids to Counties:

New Directions in Implicit Social Cognition

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A dissertation

submitted in partial fulfillment of the

requirements for the degree of

Doctor of Philosophy

University of Washington

2016

Reading Committee:

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Program Authorized to Offer Degree:

Psychology

University of Washington

Abstract

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Due to the limitations of self-report measures, psychologists have developed alternative measurement instruments – often called “implicit” measures - that do not rely on reports from participants. Using the most widely used implicit measure - the Implicit Association Test (IAT) - this dissertation contributes to two emerging areas of research: (1) using implicit measures with child participants and (2) testing whether implicit measures are sensitive to features (e.g., racial diversity) that vary across geographic units (e.g., U.S. counties or states). Chapter 2 presents 5 studies demonstrating that a child-adapted version of the IAT has adequate test-retest reliability young children. Results testing whether the IAT was predictive of children’s behavior were more mixed. Chapter 3 shows that Black-White racial attitudes indexed via the Race Attitude IAT are sensitive to differences in the proportion of Black residents across U.S. counties and states. Higher proportions of Black residents were associated with higher levels of pro-White/anti-Black implicit attitudes for White residents, but lower levels of pro-White/anti-Black implicit attitudes

for Black residents. Chapter 4 demonstrates that scores from the Race Attitude IAT are independently predicted by contact with Whites, contact with Blacks, and the proportions of Blacks (but not Whites) residing in one's county. For both White and Black respondents, having contact with Whites (e.g., as a close friend or romantic partner) or living in counties with higher proportions of Blacks was associated with stronger pro-White/anti-Black implicit attitudes, whereas contact with Blacks was associated with weaker pro-White/anti-Black implicit attitudes. Overall, this work suggests that the IAT is not only practical for use with young children, but also useful for testing the effects of features that vary across geographical units.

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Chapter 1

Introduction

To assess people's thoughts, feelings, and beliefs, psychologists typically rely on self-report (Robins, Tracy, & Sherman, 2009). Though widely used in psychology, self-reports suffer from (at least) two limitations (Nosek, Hawkins, & Frazier, 2011): First, self-report measures assume that people have introspective access to their psychological attributes (e.g., beliefs). Second, self-report measures assume that people are willing to provide accurate descriptions of their attributes. Much previous research casts doubt on these assumptions. On one hand, people are notoriously poor at introspecting (Nisbett & Wilson, 1977; Pronin, 2009; Wilson, 2002). For example, people fail to recognize how irrelevant features (e.g., the order in which items are presented) affect their evaluations (Nisbett & Wilson, 1977), how the presence of others influences their decision to help another person (Latane & Darley, 1970), or the source of a cue that enabled them to solve a problem (Maier, 1931). On the other hand, self-reports are sensitive to self-presentation concerns. People may engage in various strategies (e.g., denial, defensiveness, exaggerating) to present a positive image to others (Brown, 2007) or the self (Paulhus & Vazire, 2009), and any one of these strategies may be at play when people provide self-reports. For example, egalitarian social norms discourage the expression of bias towards racial minorities, and people's degree of concern with appearing prejudiced governs the extent to which they report unfavorable attitudes toward racial minorities (Hall & Payne, 2010; see also Crandall, Eshleman, & O'Brien, 2002). Taken together, introspective limits and self-presentation concerns can undermine the utility of self-report measures to obtain valid accounts of people's psychological attributes (cf. Lucas & Baird, 2006).

To overcome the limitations of self-report measures, measures have been developed that (a) do not require introspection and (b) afford individuals limited control over their responses (e.g., by requiring rapid judgments), thus restricting the opportunity for respondents to implement self-presentation strategies (Gawronski & De Houwer, 2014; Nosek, Hawkins, & Frazier, 2011). These measurement instruments are often referred to "implicit" measures, whereas self-report measures are commonly referred to as "explicit" measures. As the use of implicit measures has become widespread in virtually every area of psychology (Payne & Gawronski, 2010), extensive research has investigated the psychometrics, interpretations, and mechanisms of these measures (e.g., Nosek, Greenwald, & Banaji, 2007). In contrast, this dissertation seeks to make contributions to relatively new applications for implicit measures. More specifically, using the most common implicit measure- the Implicit Association Test (IAT; Greenwald, Schwartz, & McGhee, 1998) - this work investigates (a) the properties of the IAT when administered in samples of young children and (b) whether features of one's geographical environment (e.g., racial diversity in one's county or state) predict scores on a version of the IAT measuring Black-White racial attitudes (hereafter, Race Attitude IAT). Before describing the existing work within these two emerging areas, I briefly discuss the history of implicit measures in psychology and describe the structure/interpretation of the IAT.

Implicit Measures In Psychology

Implicit measures were developed by cognitive psychologists interested in studying memory without relying on participant self-report (e.g. Jacoby & Dallas, 1981; Jacoby & Witherspoon, 1982; for review, see Roediger, 1990). For example, Jacoby and Dallas (1981) found that even when participants could not discriminate previously seen words from novel words, participants were quicker (i.e., primed) to recognize practiced words relative to novel words. In the mid-1980s, social psychologists adopted priming techniques to study social psychological constructs such as attitudes (e.g., Fazio, Sanbonmatsu, Powerll, & Kardes, 1986) and stereotypes (e.g., Devine, 1989; Dovidio, Evans, & Tyler, 1986; Gaertner & McLaughlin, 1983). However, priming effects were unreliable (thus, sensitive to only group differences) and often small in magnitude (Payne & Gawronski, 2010; Teige-Mocigemba, Klauer, & Sherman, 2010). Commenting on the shortcomings of existing implicit measures, Greenwald and Banaji (1995) called for their refinement and predicted that "when such measures do become available, there should follow the rapid development of a new industry of research on implicit cognitive aspects of personality and social behavior" (p. 20).

The Implicit Association Test. Consistent with the prediction made by Greenwald and Banaji (1995), the introduction of the IAT, which produced reliable and large effects, led to a flurry of new interest in using implicit measures to assess constructs such as attitudes, stereotypes, and self-esteem (e.g., Farnham & Greenwald, 2000; McConnell & Leibold, 2001; Rudman & Lee, 2002). The IAT is a latency-based task administered via personnel computer in which participants use two computer keys to categorize items from two target categories (e.g., "Black people" and "White people") and two attribute categories (e.g., "good" and "bad"). In one set of trials, participants categorize items from all categories using one target-attribute pairing for each key (i.e., "Black people" + "bad" with one response key; "White people" + "good" with the other response key). In a second set of trials, participants categorize items using the alternative target-attribute pairing for each key (i.e., "White people" + "bad" with one response key; "Black people" + "good" with the other response key). The logic behind the IAT is that ease of responding corresponds to the strength to which paired target and attribute categories are associated; items from associated categories are paired more quickly (and more accurately) with the same response key, relative to when items from unassociated categories share the same response key. Therefore, the IAT provides an index of the *relative* associations between targets and attributes by comparing how fast a participant completes each set of trials.

Established and Emerging Areas of Research in Implicit Cognition

Though implicit measures have been applied widely within psychology, there are several *well-established* areas of research. A great deal of research has investigated questions such as the association between implicit and explicit measures (Hofmann, Gawronski, Gschwendner, Le, & Schmitt, 2005), predictive validity of implicit measures (Cameron, Brown-Iannuzzi, Payne, 2012; Greenwald, Poehlman, Uhlmann, & Banaji, 2009), and the mechanisms/processes indexed by implicit measures (Sherman, Klauer, & Allen, 2010). However, this dissertation seeks to make contributions within two relatively *emerging* areas of implicit cognition research: First, implicit cognition research is becoming increasingly interdisciplinary (Payne & Gawronski, 2010), and one area of particular growth has come from the increasing use of implicit measures in developmental research (Olson & Dunham, 2010). Second, while some scholars suggested that individual differences in implicit social cognition might be difficult to change (e.g., Wilson, Lindsey, & Schooler, 2000), subsequent laboratory research demonstrated that scores on implicit measures are sensitive to contextual factors (Blair, 2002 Devine, 2001). More recently, research has begun investigating how differences between more macro-level contexts, such as those between countries (e.g., Nosek et al., 2009), influence individual differences on implicit measures. Initial findings from these research areas are reviewed below.

Developmental Research. Since the initial publication using the IAT with children[1] in 2005 (Rutland et al., 2005), the IAT has been increasingly used in developmental research

[1] For the purpose of this dissertation, I am considering developmental samples as those with a mean age below 12.0 years.

(Dunham & Emory, 2014). Indeed, the IAT has been used to assess children's implicit associations within a number of domains such as such as gender identity (e.g., Cvencek, Meltzoff, & Greenwald, 2011), racial attitudes (e.g., Baron & Banaji, 2006; Newheiser & Olson, 2012), self-esteem (Cvencek, Greenwald, & Meltzoff, 2016), math and gender stereotypes (e.g., Cvencek, Kapur, & Meltzoff, 2015), among others (e.g., Grumm, Hein, Fingerle, 2011; Heiphetz, Spelke, & Banaji, 2013). In addition to providing an opportunity test the early formation of implicit cognition, developmental research also provides the opportunity to adjudicate between competing theoretical predictions about the formation and development of implicit cognition constructs (Hailey & Olson, 2014; Olson & Dunham, 2010). For example, some accounts have suggested that implicit attitudes are formed through an extended period of social learning (Wilson, Lindsay, & Schooler, 2000). However, implicit attitudes in young children are similar in magnitude to those held by adults (e.g., Baron & Banaji, 2006; Dunham, Baron, & Banaji, 2008; Newheiser & Olson, 2012), which appears to suggest that an extended learning period is not necessary to develop strong implicit associations.

Despite the interest among developmental researchers to use the IAT and the value in using child samples to assess the origins of implicit cognition, there is surprisingly little evidence about whether the IAT is reliable across testing administrations in children (test-retest reliability) and/or predictive of behavior child participants (Olson & Dunham, 2010). This oversight in the developmental literature is especially striking when considering the extensive work investigating the psychometric properties of the IAT in adult samples (e.g., Egloff et al., 2005; Greenwald et al., 2009; Nosek et al., 2007). Critically, extensive developmental change occurs across many of the cognitive skills necessary to complete the IAT throughout childhood, such as task switching and response inhibition (Dunham & Emory, 2014). As such, it cannot be assumed that the IAT test-retest reliability and predictive validity estimates documented in adults will be similar in samples of young children (Olson & Dunham, 2010). Lastly, the test-retest reliability and predictive validity evidence that does exist (reviewed below) is quite mixed.

Test-Retest Reliability. The IAT was developed to index individual differences in implicit cognition (Greenwald et al., 1998). Despite the fact that test-retest reliability is a key index for sensitivity to individual differences (Greenwald & Nosek, 2001), only five published reports have examined the test-retest reliability of the IAT in children. Further, the estimates that are available from these reports are quite variable. On one hand, some developmental research has found that the IAT has test-retest reliability on par with that found in adults (median r = .50; Nosek et al., 2007). For example, Schultz and Bruni (2010) and Bruni (2007) created an IAT measuring identification with nature, and when administered twice in the same testing session, test-retest correlations were .62 and .45, respectively. On the other hand, other developmental work has found substantially weaker evidence for the test-retest reliability of the IAT in children. Indeed, Corenblum and Armstrong (2012) assessed implicit Black-White racial attitudes and self-esteem across a 1-year lag time and found that IAT scores across testing administrations on the Race Attitude IAT were weakly correlated (r = .18), and scores on the Self-Esteem IAT (associations of self with positive or negative valence) were negatively correlated (r = −.17). In total, despite the assumption among developmental researchers that the IAT indexes stable individual differences (e.g., Newheiser et al., 2014), there is relatively weak evidence for the test-retest reliability of the IAT in children.

Predictive Validity. Despite the considerable research testing whether the IAT is predictive of behavior in adults (Greenwald et al., 2009), relatively few studies have tested the predictive validity of the IAT in child samples. Further, paralleling the wide range of test-retest reliability estimates for the IAT in child samples, published predictive validity estimates are also quite variable. Indeed, Cvencek and colleagues (2011) found that children's implicit gender attitudes were significantly correlated with parental reported gendered-behavior (r = .52). Similarly, using a minimal groups paradigm (Tajfel, 1970), Dunham and colleagues (2011) found that children's implicit attitudes towards novel groups predicted zero-sum (i.e., whatever gained by one party is lost by the other) resource allocations between ingroup members and outgroup members. In contrast, Pieters et al. (2010) found that implicit attitudes towards alcohol was unrelated to self-reported alcohol consumption (r = .02). Similarly, van Goethem, Scholte, and Wiers (2010) found that implicit bullying attitudes were unrelated to self-reported, peer-reported, and teacher-rated bullying behaviors (all r's $\leq$ .10). When taken together, it is unclear whether the IAT is useful for predicting behavioral outcomes in children.

While estimates from previous research investigating the test-retest reliability and predictive validity of the IAT in children have been quite variable, isolating the factors that explain why the IAT is more/less reliable or valid with child samples is impossible. Indeed, this previous work differed in many respects, such as age of respondents, domain (e.g., nature identity, gender stereotypes, self-esteem, alcohol attitudes), structure of the IAT (e.g., number of trials, response method, and modality of stimulus presentation), and for studies investigating test-retest reliability, the time between testing administrations (e.g., same day vs. 1-year). Critically, test-retest reliability and predictive validity estimates often came from reports that varied several of these factors (e.g., both age of respondents and IAT structure), which makes it impossible to isolate the relevant factor/s that explains why test–retest reliability differed across these studies. This question is addressed in Chapter 2 by examining the test-retest reliability and predictive validity of the IAT by either holding constant or systematically varying two critical factors that complicate interpretation of the previously reviewed literature: domain and lag-time.

Contextual Effects on Implicit Cognition. A second emerging line of research comes from investigations of how differences across macro-level contexts shape individual differences in implicit cognition. While implicit associations were initially thought to be resistant to contextual influences (i.e., stable across situational factors; e.g., Wilson et al., 2000), subsequent laboratory research found that implicit attitudes and stereotypes– primarily in the domain of race- were highly context dependent (Devine, 2001). Indeed, performance on implicit attitude and stereotype measures are influenced by the context in which stimuli are encountered (e.g., Dasgupta & Greenwald, 2001; Gawronski, Hu, Rydell, Vervliet, & De Houwer, 2015; Wittenbrink, Judd, & Park, 2001; Rudman & Lee, 2002), or the social role or situation that a person is embedded in while completing the measure (Lowery, Hardin, &Sinclair, 2001; Richeson & Ambady, 2001; 2003; Sinclair, Lowery, Hardin, & Colangelo, 2005). Thus, there is evidence that more immediate social context (i.e., the type that can be manipulated in the laboratory) can affect responding on implicit measures (for reviews, see Blair, 2002; Devine, 2001; Sheppard, 2011).

Broader Social Context. More recently, research has begun investigating how one's broader social context impacts implicit cognition by conducting field studies and large-scale correlational analyses. In the domain of implicit gender-career stereotypes (associations of gender categories with either "leader" or "supporter"), Dasgupta and Asgari (2004) found that female students exposed to counter-stereotypical exemplars through attendance at a women's college (e.g., female professors and deans) had lower male-leader/female-follower stereotypes than female students at a comparable coeducational college. Similarly, national-level differences in implicit gender-science stereotypes (i.e., male-math/female-reading) have been shown to be related with higher disparities between 8th grade boys and girls on a standardized test assessing math and science achievement (Nosek et al., 2009) and lower levels of enrollment of females in tertiary science education (Miller, Eagly, & Linn, 2014). Finally, Marini and colleagues (2013) found that national-level obesity (e.g., Body Mass Index, percentage of overweight residents) was associated with increased implicit pro-thin/anti-obese attitudes. Interestingly, the association between obesity and implicit weight attitudes were opposite at the national and individual-levels, such that overweight individuals tended to have weaker implicit pro-thin/anti-obese attitudes. In total, this initial work provides some preliminary evidence that implicit gender-stereotypes and weight attitudes are sensitive to differences existing at the national-level.

Despite a growing number of investigations testing how country-level social contexts covary with differences in implicit cognition, to date, there have been no investigations of how implicit racial attitudes may vary between macro-level units. This omission from the literature is striking for (at least) two reasons. First, many of the previously described contextual effects on IAT scores have come in the domain of race, which suggests that Black-White implicit race attitudes may be sensitive to contextual factors found at higher-levels of analysis (e.g., countries or states). Second, extensive interdisciplinary research from political science, sociology, and psychology has investigated the association between within-country racial demographics (e.g., across U.S. states) and *explicit* racial attitudes (e.g., Fossett & Kiecolt, 1989; Giles, 1977; Giles & Buckner, 1993; Glaser, 1994; Quillian, 1995; 1996; Taylor, 1998; Wagner, Christ, Pettigrew, Stellmacher, & Wolf, 2006; Wagner, Van Dick, Pettigrew, & Christ, 2003; Wilcox & Roof, 1978). Thus, this previous work provides an interesting backdrop with which to compare how racial demographics covary (if at all) with implicit racial attitudes. For these reasons, Chapters 3 and 4 of this dissertation investigates whether Black-White racial demographics in U.S. states and counties are associated with scores on the Race Attitude IAT

Overview

This dissertation explores the use of the IAT in two emerging areas of research. Previous research in children has found limited and mixed evidence for the test-retest reliability and predictive validity of the IAT, and disentangling the conditions under which the IAT is more/less reliable or valid in child samples is nearly impossible as previous studies have differed in many respects (e.g., domain, lag-time, etc.). Chapter 2 systematically varied domain (gender vs. race) and lag-time (10-minute vs. 1-month vs. 1-year) to determine the effects of these factors on the test-retest reliability and predictive validity of children's IAT scores. A second area of emerging research considers how features of the broader social environment are associated with individual differences in implicit cognition. Chapter 3 investigates how racial diversity across U.S. counties and states, a factor shown to be predictive of explicit racial attitudes, impacts implicit racial attitudes among White and Black respondents. Chapter 4 follows up on findings from Chapter 3 by investigating the simultaneous effects of racial diversity and a factor theorized to be important in reducing intergroup conflict – contact between groups.

Chapter 2

Test-Retest Reliability and Predictive Validity of the Implicit Association Test in Children

Rae, J.R. & Olson, K.R. (under review).

The Implicit Association Test (IAT) is increasingly used in developmental research despite minimal evidence that children's IAT scores are reliable across time or predictive of behavior. When test-retest reliability and predictive validity have been assessed, the results have been mixed, and because these studies have differed on many factors (e.g., age of respondents, domain, etc) simultaneously, it is difficult to discern when the IAT with child samples is more or less reliable and valid. Five studies (total *N* = 513) investigated whether two of these factors – domain and lag-time - affected the test-retest reliability and predictive validity of IAT scores for children 6 to11 years old. An internal meta-analysis of these studies found that children's IAT scores were similarly correlated across domains and lag-times, but that the IAT was more predictive of behavior in the domain of racial attitudes compared to gender identity. These studies demonstrate domain differences in the development of implicit cognition and the importance of future research testing the performance of the IAT in child samples.

Test-Retest Reliability and Predictive Validity of the Implicit Association Test in Children

The Implicit Association Test (IAT; Greenwald, Schwartz, & McGhee, 1998) is the most widely used measure of implicit cognition (Payne & Gawronski, 2010), and is increasingly used in developmental research (Dunham & Emory, 2014). Indeed, a search of the Google Scholar and the PsychInfo databases (using terms such as "IAT", "implicit association test", "children", "childhood", etc.) and cross-referencing published articles yielded 65 reports, papers, and theses/dissertations reporting IAT results with child samples (defined as mean age of under 12 years).[2, 3] The number of identified reports per year is plotted in Figure 1, demonstrating a clear upward trajectory. However, despite the practical and theoretical implications of this work (Gonzalez, Steele, & Baron, in press; Olson & Dunham, 2010), we know surprisingly little about whether this increasingly utilized measure is reliable or valid when used with young children. In the current work, we assess whether the IAT is reliable across testing administrations (test-retest reliability) and whether the IAT is predictive of behavior (predictive validity)—cornerstones of good assessment tools.

Why Does Test-Retest Reliability and Predictive Validity Matter?

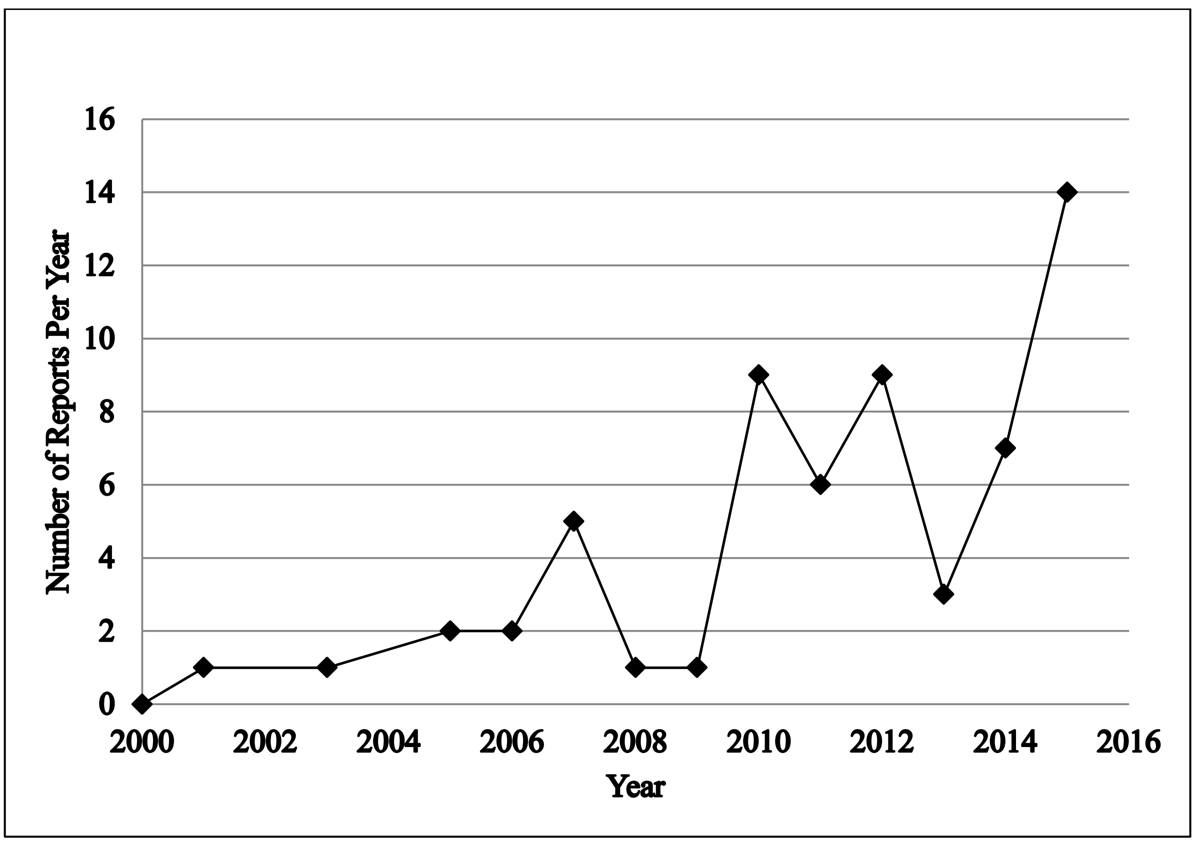
Test- Retest Reliability. Establishing adequate test–retest reliability is critical insofar as researchers believe themselves to be assessing meaningful, stable individual differences, rather than momentarily-accessible associations. Indeed, the IAT was developed as an individual difference measure of implicit cognition in adults (Greenwald et al., 1998), and in the literature to date, developmental researchers have often assumed that the IAT with children *is* a reliable

[2] For our literature search, we included any report containing a sample with a mean age below 12.0 years. For longitudinal research, we considered the mean age of the sample at first time point. We also included reports from samples in which the mean was not reported, but the midpoint (or weighted average) of the age range was below 12.0 years. For example, if a report indicated that twenty 11-year olds, five 12-year-olds, and five 13-year-olds were in the sample, this study would be included in that the weighted average of the age range is (.5×10) + (.25×12) + (.25×13) = 11.25 years.

[3] The cut-off date for the search was October 1, 2016. See Supplemental Material for a list of all identified reports.

indicator of a trait-like individual difference. For example, several lines of research have documented how children's racial attitudes (indexed via the IAT) vary as a function of their social status (e.g., Dunham, Newheiser, Hoosain, Merrill, & Olson, 2014) or preference for high-status groups (e.g., Newheiser, Dunham, Merrill, Hoosain, & Olson, 2014). This work – and much other work using the IAT with child samples - assumes that there is meaningful child-specific variance in IAT scores that can be explained by individual difference factors (e.g., experience or preferences).

Figure 1. Number of publications, reports, and theses using the IAT in child samples (12 years or younger) plotted against the publication year (2000-2015).



Despite the fact that developmental researchers have treated the IAT as an individual difference measure and that test–retest reliability is the primary test for sensitivity to individual differences (Greenwald & Nosek, 2001), to date there have been only a small handful of studies examining test-retest reliability using the IAT with children. Unfortunately, the findings are rather mixed and difficult to interpret. Table 1 presents the nine test–retest reliability coefficients from five reports using the IAT with children reported in previous developmental research. Critically, these values range from −.17 (Corenblum & Armstrong, 2012) to +.62 (Bruni & Schultz, 2010). Further, directly comparing these results is nearly impossible as the studies themselves drastically differed in the structure and presentation of the IAT (e.g., number of trials, response type, stimulus presentation), domain of study, age of participants, and many other factors (e.g., sample-related differences). Therefore, it is difficult to discern what differences may explain variability in test–retest reliability estimates.

Predictive Validity. Predictive validity is another key feature of good measures (Kimberlin & Winterstein, 2008), and is particularly crucial for the IAT because a key motivation behind the use of implicit measures is that they guide behavior (e.g., McConnell & Leibold, 2001). Implicit cognition is thought to be less influenced by social desirability concerns than more explicit measures are and as such, are supposed to be especially predictive in more socially sensitive domains (e.g., intergroup behavior) wherein people may be hesitant to express bias or undesirable preferences. Indeed, a meta-analysis with large numbers of studies with adults have generally found that the IAT is in fact more predictive of behavior than more explicit measures in predicting Black-White interracial behavior and intergroup behavior more generally (e.g., Greenwald, Poehlman, Uhlmann, & Banaji, 2009; cf. Oswald, Mitchell, Blanton, Jaccard,

& Tetlock, 2013). Likely developmental psychologists have similar goals, and as such, not surprisingly, often have utilized the IAT in domains such as race.

However, again, as in the case of test-retest reliability, we have relatively little evidence about the predictive validity of the IAT in child participants, and what evidence we have is quite mixed. Table 1 presents the 17 reports of predictive validity of the IAT in the developmental literature. As the table indicates the literature has found results ranging from no relationship between the IAT and behavior (r =.02; Pieters, van der Vorst, Engels, & Wiers, 2010; van Goethem et al., 2010) to a strong positive relationship between the IAT and behavior (r =.52, Cvencek, Meltzoff & Greenwald, 2011).[4] Again, however, interpretation is difficult because of a number of study factors (structure of the IAT, age of respondents, etc.) varied substantially across studies reporting predictive validity estimates.

What Explains Variability in Test-Retest Reliability and Predictive Validity Estimates?

As alluded to above, the variation observed in past work on test-retest reliability and predictive validity could be explained by a wide range of factors, which we explore below.

Domain Differences. Studies differing in test–retest reliability and predictive validity estimates in Table 1 have come from very different domains. Within adult populations, domain has been found to relate to both test-retest reliability and predictive validity. For example, political attitudes have much stronger test-retest reliability than attitudes toward the self (i.e., self-esteem; Bar-Anan & Nosek, 2014). Similarly, while scores on the Race Attitudes IAT have been shown to predict interracial behavior (e.g., McConnell & Leibold, 2001), scores on the Self-Esteem IAT have not been found to correlate with theoretically

[4] Bruni & Schultz (2010) and Vander Heyden and colleagues (2016) did not report the magnitude of null IAT-criterion associations, which leaves open the possibility that the lower-bound on IAT-criterion correlations could be lower than +.02 (or even negative).

Table 1. Authors, publication year, sample size, domain, age, number of IAT trials, and parameter estimates from identified articles, reports, and theses that reported either test-retest reliability or predictive validity estimates of the IAT in child samples.

Authors (year)	***N***	**Domain**	**Age (years)**	**Trials (N)**	**Test-retest reliability (r)**	**Lag- Time**	**Predictive Validity ($\lvert r \rvert$)**
Bruni (2007) Study 1	52	Nature identity	M = 9.69	168	.45	< 1 day	
Bruni and Schultz (2010) Study 3[1]	30	Nature identity	10-12	168	.62	< 1 day	.41, .45, .36[2]
Pieters et al. (2010) Study 1	99	Alcohol attitudes	M = 10.17				.02
Pieters et al. (2010) Study 2	35	Alcohol attitudes	M = 11.36				.39[3]
van Goethem et al. (2010) - Version 1	240	Bullying attitudes	M = 11.41				.07, .02, −.06
van Goethem et al. (2010) - Version 2	240	Bullying attitudes	M = 11.41				.07, .06, .10
Cvencek et al. (2011) Study 2	75	Gender attitudes	M = 4.46				.52
Dunham et al. (2011) Study 1	33	Intergroup attitudes	M = 5.40				.50
Dunham et al. (2011) Study 2	43	Intergroup attitudes	M = 5.50				.41
Grumm et al. (2011)	115	Aggression identity	M = 9.70				.23[4]
Corenblum and Armstrong (2012)	196	Race attitudes	7.11- 10.54	180	−.17[5]	1-year	
O'Connor et al. (2012)	376	Alcohol attitudes	M = 11.10				.06
Galdi et al. (2014) - Females[6]	120	Math-Gender stereotypes					.27
Galdi et al. (2014) - Males	120	Math-Gender stereotypes					.05
Cvencek et al. (2015)	299	Math Self-concept Math-Gender stereotypes Gender identity	7.37 - 11.38				.15, 16, .06
Diesendruck and Menahem (2015)	48	Ethnic attitudes	M = 6.50				.51[7]
Lemmer et al. (2014)[8]	574	Aggression identity	11.67	220	.14 - .36[9]	5-months – 1.5-years	
	317	Aggression identity	11.61				.20, .20, .19, .17,.14, .12, .06, .11
Leeuwis et al. (2015)	330	Self-esteem	M = 11.20	80	0.29	1-year	
Meyer and Gelman (2016)	77	Gender stereotypes	M = 6.45				.10
Vander Heyden et al. (2016)	237	Gender stereotypes	M = 10.82				ns[10]

(Table 1 continued)

Notes:

1. Pilot data in this report was collected from fifth graders in the U.S. Although age was not reported for the sample, this study was included in the review because students at this grade-level are typically below our age cutoff (10-11 years). Also, participants were randomized to take an IAT with words or pictures, but exact sample sizes in each condition were not reported.
2. There were nine null IAT-behavior associations, though the magnitudes of the associations are not reported.
3. Pieters et al. (2010) reported that alcohol consumption was higher for children that associated alcohol with negative (rather than positive) valence. This effect is opposite to that found with older children and adolescents (12-15 years) in which alcohol consumption was higher for those that associated alcohol with positive valence (Thush & Wiers, 2007).
4. Estimate is a standardized regression coefficient.
5. Correlations were between "error-free" latent variable estimates.
6. Mean age for the entire sample was 6.47 years. Age by gender was not reported.
7. The authors reported a significant correlation for male participants, but did not report the magnitude of a non-significant correlation for female participants.
8. Only a subset of the sample completed measures used for tests of predictive validity.
9. The authors assessed stability of the IAT across four time points, but only reported the range of estimates.
10. The authors reported that IAT-criterion correlations $\leq .18$ for male participants, and did not report the magnitude of any correlations for female participants. All correlations were non-significant.

relevant criterion variables (e.g., Bosson, Swann, & Pennebaker, 2000; Buhrmester, Blanton, & Swann, 2011). Thus, domain differences may explain variability in the test-retest reliability and predictive validity estimates presented in Table 1.

***Age of Respondents*.** Similar discrepancies in results across the reviewed studies could be driven by differences in the age of respondents. While all studies reported in Table 1 had a mean age of less than 12 years, there was still substantial age variability across reports with mean ages ranging from 4.46 years (Cvencek et al., 2011) to 11.61 years (Lemmer, Gollwitzer, & Banse, 2011). As such, differences across studies could be driven by developmental changes in the cognitive abilities that the IAT relies upon, such as task switching, response inhibition, and reaction time (Dunham & Emory, 2014). Admittedly, there has been no direct evidence that these early developmental changes affect performance on the IAT, however, this hypothesis seems especially plausible when taken with evidence that developmental changes later in life (age-related slowing among elderly participants) must be taken into account when analyzing and interpreting IAT scores (Hummert, Garstka, O'Brien, Greenwald, & Mellott, 2002).

Structure of IAT. Another factor that differs dramatically across the studies in Table 1 and that likely affects the degree of reliability and validity of children's IAT scores is the structure of the IAT administered in any given report. Despite using the same name (the IAT), researchers have taken a number of liberties in the design of the IAT. As just a sample of these differences, the number of trials (168 vs. 144 vs 70)[5], the modality of stimulus presentation (visual vs audio vs. both audio and visual), scoring algorithm, and response type (e.g., paper and pencil, keyboard press, external buttons, computer mouse movements, etc) differed across studies. Previous work with adult samples has suggested that even trivial procedural differences can impact the reliability and validity of the IAT (Bar-Anan & Nosek, 2014). Lastly, while not reliant on the IAT structure per se, past developmental research has used IAT scoring algorithms that differ in the criterion used to remove trial latencies deemed as outliers (e.g., Bruni & Schultz, 2010; Newheiser et al., 2014) and/or participants with too many error responses (e.g., Cvencek et al., 2011; Newheiser et al., 2014).

***Lag Time*.** Within the studies exploring test-retest reliability, the amount of time between testing administrations varies considerably (as can be seen in Table 1). The strongest test-retest reliability, perhaps not surprisingly, occurs in studies in which IATs were completed on the same day (e.g., Bruni & Schultz, 2010) while weaker evidence comes from work in which administrations were separating by one-year (e.g., Corenblum & Armstrong, 2012; see Table 1). This result is not surprising in that lower reliability coefficients are to be expected with longer

[5] All else being equal, IATs with a greater number of trials should have stronger test-retest reliability. Results presented in Table 1 do not conform to this expectation (e.g., test-retest coefficients were comparable from IATs with the fewest and greatest number of trials). However, the small number of available reports prevents drawing any strong conclusions about the role of IAT length in influencing previous test-retest reliability estimates.

lag times (Ozer, 1999). However, lag time may especially impact test–retest reliability in developmental samples, as changes in IAT scores over a one-year period could reflect problems with the measure, but could also reflect real developmental changes in the underlying construct (rather than unreliability of IAT scores; Carmines & Zeller, 1979).

Can Specific Factors Be Isolated? While we have made the case that four factors may explain variability in test–retest reliability and predictive validity estimates, the critical question is whether the unique effects of any one factor can be isolated. For example, if two estimates of test–retest reliability come from reports varying only on domain assessed while the other three factors (e.g., age of respondents, structure of the IAT, lag time) are held constant, then there is at least preliminary evidence that domain differences can explain some of the observed differences. In contrast, if more than one factor differs across reports (e.g., domains assessed and lag time), such inferences are not possible. Unfortunately, this latter scenario exemplifies the research reviewed here. For example, studies providing strong (e.g., Bruni & Schultz, 2010) and weak (e.g., Corenblum & Armstrong, 2012) evidence of test–retest reliability differ on nearly every dimensions—domain (nature identification vs. racial attitudes), age of participants (10-12 years vs. 7.11- 10.54 years), length (168 trials vs. 180 trials), stimulus presentation (moving vertically from the top of the screen to the bottom vs. presented center screen), lag time between testing administration (one day vs one year), etc. As such, identifying the key factor that explains why test–retest reliability differed across these studies is impossible.

Overview of the Present Research

Across five studies, we sought to evaluate test-retest reliability and predictive validity of the IAT with young children by either holding constant or systematically varying (across studies) the factors (e.g., lag-time, constructs assessed, age of subjects, and structure of the IAT) that complicate interpretation of the previously reviewed literature. In all studies, we used one version of the child IAT (based on Newheiser & Olson, 2012) with the same exclusion criteria and tested only children between 6 and 11 years of age (at the time of the first administration). We obtained results for two domains (gender identity and race attitudes), and at three time lags (10-minutes, 1-month, and 1-year [gender-identity only]). Comparing results across lag times in the same domain allowed us to estimate the pure effects of time on test-retest reliability, while comparing across studies in different domains allowed us to evaluate the effect of content domain on both predictive validity and test-retest reliability. Study 1 assessed the predictive validity and test-retest reliability of the Gender Identity IAT across a lag time of 10-minutes. In Study 2 (also using the Gender Identity IAT), we extended the lag time between test and retest to 1-month. Studies 3 and 4 replicated Studies 1 and 2 in the domain of Black-White race attitudes. Finally, Study 5 tested the test-retest reliability of the Gender Identity IAT across 1-year.

Study 1:

Implicit Gender Identity across a 10 minute lag

Study 1 investigated test-retest reliability of the Gender Identity IAT and an explicit gender identity measure across a 10-minute lag. This short lag-time provides an upper limit on the test-retest reliability of Gender IAT scores. In addition, we tested whether the IAT and/or an explicit gender identity measure would be predictive of gender-related behavior.

Method

Participants

One hundred and five participants completed the study at a research lab located at the University of Washington. Only children between 6 and 11 years of age were eligible to participate (also true of all subsequently presented studies), but due to experimenter error, three participants older than 12 years participated in the study. Data from these three participants are excluded from all analyses. Partial data is reported from one six-year-old who failed to complete either IAT due to difficulty reading word stimuli in the IAT (see below), two other participants that requested to discontinue the testing session, and one participant whose IAT data from retest was removed via established exclusion criteria (described on pg. 30). Table 2 shows the age and gender characteristics of the sample for this study.

Measures

Explicit Gender Identity. Gender identity was assessed via two sequential questions in which children were shown a picture of a White boy and a White girl (of approximately equal age in middle childhood) on laptop computer (Cvencek, Meltzoff, & Greenwald, 2011). While pointing at each picture, the experimenter explained the name and gender of each child (e.g., “On the left is Paul. He is a boy” and “On the left is Amanda. She is a girl.”). Children were first asked to indicate which child they were more like (e.g., “Are you more like Amanda or are you more like Paul?”). After making a selection, the experimenter then covered the unselected child with his/her hand and pointed to two circles (one small and one large) located below the selected child and asked the child to indicate the degree of their similarity with the selected target (e.g., “How much like [Paul/Amanda] are you? A little [while pointing at the small circle] or a lot [while pointing at the large circle]?”). At retest, participants rated different male and female children (Emily and David, also White). The order in which the different photo sets were rated, as well as the side of the screen in which the male and female targets were presented, was counterbalanced across participants. The measure was scored from 1 (indicating participant felt “a lot” like the male target) to 4 (indicating participant felt “a lot” like the female target).

Implicit Gender Identity. We used a child-adapted version of the IAT (from Newheiser & Olson, 2012) to assess implicit gender identity. The IAT is a latency-based measure that indexes the relative association between two concepts (a target and an attribute). For example, the Gender IAT measures the speed with which participants pair pictures of males and females with "Me" words (e.g., I, me, mine, myself) and "Not me" words (e.g., others, them, theirs, they). The logic of this IAT is that the quicker that participants pair "female" with "Me" items and "male" with "Not me" items (relative to their speed in pairing "female" with "Not me" items and "male" with "Me" items), the stronger they implicitly identify as female.

To create a measure appropriate for use with children, the child-adapted IAT used here (but also used in previous research; see Olson, Key, & Eaton 2015) differed from the traditional (adult) IAT in several ways. First, instead of photographs of adults, we used photographs of both male and female children. Additionally, the IAT used in the present research reduced task demands for participants in two ways. First, completing the IAT can be taxing (requiring task switching, response inhibition, etc.), so to avoid fatiguing participants we reduced the length of the IAT to 70 trials (the traditional IAT may contain over 200 trials; Greenwald et al., 1998). Second, we marked response keys with bright yellow stickers so that participants did not need to retain response key assignment information throughout the task (for a similar approach, see Cvencek, Greenwald, et al., 2011).

The Gender Identity IAT used here consisted of five blocks, and pictures of male and female children along with the words "Me" and "Not me" served as category labels in all blocks. In two initial practice blocks (10 trials each), participants used two marked computer keys (the "D" and "K" keys on a Qwerty keyboard) to practice discriminating target (Me/Not me) and attribute items (male/female; see Table 3). In the third block, items from the practice blocks were combined such that participants categorized items from both targets and attribute (20 trials). Items from one target-attribute pairing (e.g., male + Me) were classified with one response key and items from the other target-attribute pairing (e.g., female + Not me) were classified with the other response key. After practice reversing the key assignments for the attribute categories (10 trials), target and attribute items were again classified using the alternative target-attribute pairings (e.g., male + Not me; female + Me) for each response key (20 trials). The order of target-attribute pairings in combined tasks was counterbalanced across participants.

A summary score for the IAT (called a *D*-score) that is *conceptually* similar to a Cohen's *d* is calculated by dividing the mean latency difference between the third and fifth block by the "inclusive" standard deviation (i.e., all retained latencies completed within the third and fifth block). Consistent with the recommended scoring algorithm, all trials greater than 10,000-ms were removed and data from participants completing more than 10% of trials in under 300-ms was removed (Greenwald et al., 2003). The IAT was scored such that positive values indicate higher identification with female relative to male. The split half reliabilities (Spearman-Brown corrected) of the Gender Identity IAT at test and retest were .73 and .82, respectively.[6]

Table 2. Structure of child-adapted Gender Identity IAT used in Studies 1, 2, and 5. The Race IAT used in studies 3 and 4 presented the target discrimination task in block 1 (Black/White), attribute discrimination task in block 2 (Good/Bad), and block 5 was a reverse target discrimination task.

			Response key assignment	
Block	***N* trials**	**Task**	**Left key ('D')**	**Right key ('K')**
1	10	Attribute discrimination	Male	Female
2	10	Target discrimination	Me	Not Me
3	20	Initial combined task	Male + Me	Female + Not Me
4	10	Reverse attribute discrimination	Female	Male
5	20	Reversed combined task	Female + Me	Male + Not Me

[6] *D*-scores were computed on two subsets (10 trials each) of IAT trials and the correlation between the *D*-scores was adjusted via the Spearman-Brown prophecy formula.

Gender Related Behavior – Coloring Page Prize. To assess gender-related behavior, participants were able to choose a coloring page as a prize at the conclusion of the testing session. The experimenter randomly placed four coloring pages on the table in front of the participant. Coloring pages were chosen via a pilot study such that they differed in degree of perceived masculinity/femininity.[7] In order from pages perceived to be most masculine to most feminine, participants could choose between a truck, an astronaut, a horse and carriage, and a unicorn. Coloring book pages were coded with values from 1 (truck) to 4 (unicorn).

Gender Related Behavior - Clothing. As a second measure of gender-related behavior, two experimenters rated participant's clothing (what they wore to the appointment without advance notice that it would be coded) on a scale from 1 = "very boy-y" to 5 = "very girl-y". An examine outfit that would be scored 1 was a football jersey with cargo shorts, black socks and charcoal gray shoes. An examine outfit that would be scored 5 would be a tutu paired with a sparkly pink shirt and ballet slippers. We assumed that at these ages, children likely play a non-trivial role in the selection of their outfits—an assumption supported by asking a handful of parents. Interrater agreement was excellent (interclass correlation coefficient [ICC] = .93).[8]

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[7] Twenty children (M = 8.34 years, SD = 1.60 years; 10 female) participated in the pilot study. Participants were presented with 10 coloring book pages and asked to select the page that they thought boys would like most. After making a selection, the experimenter removed the selected page and asked the participant to make the same judgement from the remaining options. This procedure was completed for a third trial. Then, all removed pages were put back and participants completed the same procedure in which they selected the pages they thought girls would like most. We calculated masculinity ratings by assigning 3 points to the page for every participant that chose it on the first trial (when making selections for boys), 2 points for each selection on the second trial, 1 point for selections on the third trial, and 0 points for non-selected pages. Means were calculated for each page, and found participants were more likely to think that boys would like the truck coloring page (M= 2.15, SD =.88) than the coloring page of an astronaut (M = .80, SD = .95, $t(18) = 4.67$, $p < .001$). Further, the mean rating for the astronaut page was significantly higher than those unicorn and a horse and carriage (both M's & SD's =0, $t(18) = 3.67$, $p <.001$). Point assignments were assigned in the same way for pages selected for girls (3 points assigned to the first selection, etc.). The means indicated that participants were more likely to think that girls would like the unicorn coloring page (M= 1.90, SD =1.21) than the coloring page of a horse and carriage (M = .85, SD = 1.14, $t(18) = 2.83$, p =.007). Further, scores for the page with a horse and carriage was significantly higher than the page with a truck, $t(18) = 2.75$, p =.009, and astronaut, $t(18) = 3.09$, p =.003.

[8] Due to experimenter error, 2 participants did not receive any clothing ratings. An additional 5 participants only had a rating from one experimenter.

Procedure

All participants were tested individually and completed the study tasks in a fixed order (also true of Studies 2 through 4). After obtaining consent from parents as well as verbal assent from children (and written assent for those age 9 and above), participants completed the explicit gender identity measure and the Gender Identity IAT. After a five-minute filler task, participants completed both measures for a second time. Due to the time it took to complete each measure, there were approximately 10 minutes between the start of the first explicit identity measure and the start of the second identity attitude measure, and approximately 10 minutes between the start of the first IAT and the start of the second IAT. After completing both measures for a second time, participants were given the opportunity to choose a coloring book page as a prize for study participation. The content of the selected coloring book page (e.g., masculine or feminine content) served as a measure of gender behavior. The testing session concluded with an opportunity for the participants to ask questions about the research.

Analytic Strategy

Test-retest reliability was assessed via the zero-order correlation among scores the IAT and explicit gender identity measure collected before and after the filler task. To test whether the stability of implicit or explicit gender identity changed across participant age, we correlated age with the absolute difference in IAT scores and the explicit gender identity measure between test and retest. Evidence for predictive validity also came from zero-order correlations between identity measures and masculinity/femininity ratings of both the participant's clothes during the experiment and of the coloring book page selected at the conclusion of the experiment. Importantly, we also used multiple regressions to test two further questions. First, to investigate whether implicit and explicit gender identity *independently* predict gender- related behavior, we simultaneously regressed scores of each behavioral measure on both IAT scores and scores on the explicit gender identity measure. These analyses allowed us to test whether IAT scores and scores on the explicit identity measure were significant predictors when included in the same model. Second, we further added participant gender and age into the multiple regression model containing IAT scores and scores on the explicit gender identity, which allowed us to test whether implicit and explicit gender identity predicts gender related behavior above and beyond other demographic variables.

Results

Degrees of freedom vary across statistical test due to incomplete data on various study measures as described above.

Descriptive Statistics

Table 3 shows overall means and standard deviations and zero-order correlations among both demographic and non-demographic study variables. Participant gender (but not age) was highly correlated with both explicit and implicit gender identity measures and measures of gender- related behavior (all *r*'s $\geq .51$). Also, implicit and explicit gender identity measures (from test, retest, and the average score from test and retest) were significantly correlated (*r*'s ranging from .35 to .90). Lastly, the masculinity/femininity of the coloring book page selected as a prize at the conclusion of the study was associated with the masculinity/femininity of the clothing the participant wore during the testing session ($r = .67$).

Table 4 shows differences in non-demographic variables between male and female participants. Recall that each variable was scored such that higher values indicate stronger

Table 3. Zero-order correlations among demographic variables and measured variables in Study 1. *N*'s for means and standard deviations ranged from 95 to 102. Correlations and associated *p*-values were computed with *N*'s ranging from 93 to 102.

	Variable	**Mean**	**SD**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
1	Gender (Female = 1)	.46	.50	-	-	-	-	-	-	-	-	-
2	Age	9.18	1.69	−.04	-	-	-	-	-	-	-	-
3	Self-report Identity - Test	2.39	.79	.77***	−.09	-	-	-	-	-	-	-
4	Self-report Identity - Retest	2.49	.74	.68***	−.05	.56***	-	-	-	-	-	-
5	Self-report Identity (average)	2.45	.67	.82***	−.08	.89***	.88***	-	-	-	-	-
6	IAT - Test	−.04	.55	.51***	−.06	.45***	.35***	.48***	-	-	-	-
7	IAT - Retest	−.03	.57	.63***	.06	.48***	.48***	.56***	.61***	-	-	-
8	IAT (average)	−.03	.51	.64***	.00	.52***	.47***	.58***	.90***	.90***	-	-
9	Coloring Book Selected	2.51	1.17	.70***	−.08	.55***	.60***	.65***	.41***	.44***	.48***	-
10	Clothing	2.81	1.15	.90***	−.15	.67***	.62***	.73***	.46***	.53***	.56***	.67***

Note: *** = $p < .001$ (two-tailed)

Table 4. Mean and standard deviations of the six non-demographic variables in male and female participants in Study 1. The *p*-values are from two-tailed t-tests (*df* ranging from 72.08 to 97.01). Effect sizes are presented as Cohen's *d*s.

Variable	Females	Males	*t*	*p*	*d*
Self-report Identity - Test	3.04 (.51)	1.84 (.50)	12.02	< .001	2.39
Self-report Identity - Retest	3.02 (.61)	2.02 (.49)	9.01	< .001	1.82
Self-report Identity (average)	3.03 (.36)	1.94 (.41)	14.14	< .001	2.84
IAT - Test	.27 (.52)	−.30 (.44)	5.85	< .001	1.19
IAT - Retest	.37 (.45)	−.34 (.44)	7.83	< .001	1.60
IAT (average)	.33 (.37)	−.32 (.42)	7.99	< .001	1.65
Coloring Book Selected	3.40 (.82)	1.77 (.85)	9.44	< .001	1.94
Clothing	3.94 (.60)	1.87 (.38)	20.09	<.001	4.21

association with female than male (identity measures) or more feminine (a) clothing worn during the study or (b) choice of coloring book page at the end of the study. As expected, and consistent with the high correlations among participant gender and both gender identity and behavior measures Table 3, males had lower scores than females on all measures, and all differences were large (all Cohen's *d*s ≥ 1.19). Taken together, the descriptive statistics and zero-order correlations validate the choice of measures used in Study 1.

Test-Retest Reliability

Scores on the Gender Identity IAT were significantly correlated across the 10-minute lag time, $r(96) = .61$, $p < .001$. Scores on the explicit gender identity measure were significantly correlated across the 10-minute lag time, $r(99) = .56$, $p = < 001$.[9] Age was not correlated with the absolute difference between IAT scores, $r(96) = .10$, $p = .31$, or explicit gender identity scores, $r(95) = .03$, $p = .74$, from test and retest.

Predictive Validity

Table 3 provides initial evidence of the predictive validity of both the Gender Identity IAT and the explicit gender identity measure. Indeed, IAT scores at test and retest were positively associated with scores on the gender-related behavior measures (*r*'s ranging from .41 to .56), which was also true explicit gender identity measure (*r*'s ranging from .55 to .73).

Table 5 presents the results of multiple regression models predicting the masculinity/femininity of the coloring book page that participants selected at the end of the study. Table 6 presents parallel analyses in which masculinity/femininity of the clothing that children wore in the testing session served as the outcome variable. An examination of Model 1

[9] Using Spearman-Brown prophecy formula, the expected test-retest reliability of the *average* score on the IAT and explicit measure (from test and retest) was .76 and .72, respectively.

Table 5. The unstandardized coefficients, standard error of the unstandardized coefficients, and standardized coefficients (β) of hierarchical multiple regression models regressing *femininity/masculinity of coloring book selection* on (a) gender identity measures (from test, retest, and the average between test and retest) and (b) demographic variables in Study 1.

	Model 1			Model 2			Model 3			Model 4			Model 5			Model 6		
Variable	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β
Self-report Identity - Test	.68***	.15	.46	-	-	-	-	-	-	.01	.18	.01	-	-	-	-	-	-
IAT - Test	.39†	.21	.18	-	-	-	-	-	-	.03	.20	.02	-	-	-	-	-	-
Self-report Identity - Retest	-	-	-	.81***	.15	.50	-	-	-	-	-	-	.35*	.15	.22	-	-	-
IAT - Retest	-	-	-	.40*	.19	.19	-	-	-	-	-	-	−.11	.19	−.05	-	-	-
Self-report Identity (average)	-	-	-	-	-	-	1.00***	.17	.58	-	-	-	-	-	-	.36	.22	.21
IAT (average)	-	-	-	-	-	-	.33	.23	.14	-	-	-	-	-	-	−.07	.23	−.03
Gender (1= Female)	-	-	-	-	-	-	-	-	-	1.59***	.29	1.37	1.43***	.26	1.2	1.35***	.33	1.15
Age	-	-	-	-	-	-	-	-	-	−.06	.05	−.08	−.06	.05	−.09	−.06	.05	−.08
R^2		.33			.40			.44			.49			.55			.52	

Note: *** = p < .001, ** = p < .01, * = p < .05, † = p < .10.

Table 6. The unstandardized coefficients, standard error of the unstandardized coefficients, and standardized coefficients (β) of hierarchical multiple regression models regressing *femininity/masculinity of clothing worn during testing session* on (a) gender identity measures (from test, retest, and the average between test and retest) and (b) demographic variables in Study 1.

	Model 1			Model 2			Model 3			Model 4			Model 5			Model 6		
Variable	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β
Self-report Identity - Test	.86***	.12	.59	-	-	-	-	-	-	−.10	.10	−.07	-	-	-	-	-	-
IAT - Test	.45**	.17	.22	-	-	-	-	-	-	.01	.10	.00	-	-	-	-	-	-
Self-report Identity - Retest	-	-	-	.73***	.13	.48	-	-	-	-	-	-	.02	.09	.01	-	-	-
IAT - Retest	-	-	-	.65***	.17	.32	-	-	-	-	-	-	−.06	.11	−.03	-	-	-
Self-report Identity (average)	-	-	-	-	-	-	1.03***	.14	.60	-	-	-	-	-	-	−.02	.13	−.01
IAT (average)	-	-	-	-	-	-	.56**	.19	.25	-	-	-	-	-	-	−.08	.13	−.05
Gender (1= Female)	-	-	-	-	-	-	-	-	-	2.18***	.16	1.91	2.07***	.15	1.82	2.14***	.19	1.87
Age	-	-	-	-	-	-	-	-	-	−.09**	.03	−.14	−.09**	.03	−.13	−.09**	.03	−.13
R^2		.49			.46			.56			.83			.82			.82	

Note: *** = p < .001, ** = p < .01, * = p < .05, † = p < .10

(scores at *test* as predictors), Model 2 (scores at *retest* as predictors), and Model 3 (averages of scores from test and retest as predictors) estimates from both Tables 5 and 6 reveals that higher associations of self with female (on either identity measure) was associated with more feminine clothing/coloring book selection. Two exceptions were that IAT scores obtained at test ($\beta = .18$, $p = .070$) and the average from test and retest ($\beta = .14$, $p = .154$) did not predict coloring page selection (see Models 1 and 3 in Table 5). Standardized regression coefficients of explicit gender identity scores were roughly twice as large as those of IAT scores (see Models 1-3 in Tables 5 and 6). This suggests that although both IAT scores and explicit gender identity scores uniquely predicted behavior, explicit gender identity was the stronger predictor. Lastly, implicit and explicit gender identity alone explained an impressive proportion of the variance in gender related behavior (R^2's from Models 1-3 in Tables 5 and 6 ranged from .33 to .56).

Models 4 through 6 (Tables 5 and 6) tested whether implicit and explicit gender identity predicted gender related behavior above and beyond participant's gender and age. Other than a significant association between explicit gender identity at retest and coloring book page selected (see Model 5 in Table 5), the near zero estimates for IAT scores and scores on the explicit gender identity measure indicates that both implicit and explicit gender identity was generally not predictive of behavior after controlling for participant demographics – especially sex (see Tables 5 and 6).

Discussion

Study 1 found that the test-retest coefficient for the Gender Identity IAT across a 10-minute lag significantly different from zero ($r = .61$). Comparing this estimate to previously reported test-retest reliability coefficients using child samples (presented in Table 1) reveals that the estimate from Study 1 is the strongest test-retest coefficient reported with children so far.

Further, the magnitude of the test-retest coefficient in Study 1 is even stronger than the typical test-retest coefficient reported in adult samples (median r = .56; Nosek et al., 2007). Taken together, results from Study 1 provide promising evidence that the Gender Identity IAT used here may individual differences in gender identity. However, with such a short lag time between test and retest (10-minutes), our results are poorly positioned to evaluate whether the Gender Identity IAT assesses trait-like associations that are stable over time. This limitation is taken up in Study 2.

Study 1 also investigated the predictive validity of the Gender Identity IAT. Results found that scores on the IAT (at both test and retest) were predictive of gender-related behavior (r's ranging from .41 to .56). Comparing these correlations to the predictive validity estimates from previous developmental research (Table 1) reveals that the strongest IAT-criterion association from Study 1 (r =.56) is stronger in magnitude than estimates reported in previous research. Further, scores on the IAT were predictive of gender related behavior even while controlling for explicit gender identity, although explicit gender identity was a stronger predictor of behavior. While we generally found that neither IAT scores nor scores on the explicit gender identity measure were predictive of behavior after controlling for participant demographics, these results must be interpreted cautiously due to high correlations among predictors. Indeed, high correlations among predictors can make coefficient interpretations difficult. For example, after controlling participant gender and age in Model 5 (Table 5), the IAT regression weight was *negative*, though the sign of the correlation between the IAT and the criterion is *positive*. This statistical artifact (called "net suppression"; Darmawan & Keeves, 2006; Krus & Wilkinson, 1983) limits the ability to assess the contribution of implicit and explicit gender identity in predicting behavior while controlling for participant demographics.

Lastly, participant age was not related to any other demographic or non-demographic variable. Further, discrepancies between IAT scores and scores on the explicit gender identity measure from test and retest were not related to participant age. Taken together, participant age does not appear to explain the *magnitude* or degree of *stability* of scores on either gender identity measure.

Study 2:

Implicit Gender Identity across One-Month Lag

In Study 2, we extended the lag time between test and retest of the Gender IAT to approximately 1-month. By holding all other factors hypothesized to affect test-retest reliability and predictive validity (e.g., domain, age of participants, IAT structure) constant, we can compare estimates from Studies 1 and 2 to assess the effects of time on test-retest reliability of the Gender Identity IAT. Finally, Study 2 also provided an opportunity to replicate the predictive validity findings from Study 1.

Method

Unless otherwise noted, all methods in Study 2 were identical to those used in Study 1.

Participants

One hundred and six participants between the ages of 6 and 11 years participated in the study at either a research lab located at the University of Washington (N = 8) or a quiet testing space in their school (N = 98; all schools located in Washington state). Incomplete data occurred for a variety of reasons. Due to experimenter error, IAT data from the first test administration was lost for nine participants. Additionally, three participants requested to skip part of the study procedure. We had several problems with regard to clothing rating scores as well: For 18 participants these scores were not recorded at either test or retest due to experimenter error and

12 others were not recorded because they wore a required school uniform during the experiment. Finally, due to absence (school participants) or inability to re-contact participants for a second lab visit, an additional six participants provided no data at retest. Data from participants with incomplete data are reported whenever possible for the measures they did complete. Table 6 shows the age and gender characteristics of the sample.

Procedure

In Study 1, test and retest administrations of the explicit gender identity measure and the Gender Identity IAT was separated by a 5-minute filler task. In Study 2, participants completed the explicit and implicit gender identity measures, and approximately 3 to 4 weeks later (M = 27 days, SD = 3 days), we either returned to participants' schools or participants came back into the lab and completed the measures for a second time.

Reliability of Measures

The Spearman-Brown correct split half reliability of the Gender Identity IAT at test and retest was .80 and .72, respectively. Interrater agreement of clothing masculinity/femininity scores was excellent at both test (ICC = .89) and retest (ICC = .93).[10]

Results

Degrees of freedom vary across statistical test due to incomplete data on various study measures as described above.

Descriptive Statistics

Table 7 shows overall means, standard deviations, and zero-order correlations among all study variables. Results closely aligned with those reported in Study 1: participant gender (but not age) was highly correlated with all gender identity measures and measures of gender related

[10] Due to having only one experimenter present during the testing session, six participants only received one rating at test and two others had only one rating at retest.

Table 7. Zero-order correlations among demographic variables and measured variables in Study 2. *N*'s for means and standard deviations ranged from 72 to 106. Correlations and associated *p*-values were computed with *N*'s ranging from 64 to 104.

	Variable	**Mean**	**SD**	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**
1	Gender (Female = 1)	.50	.50	-	-	-	-	-	-	-	-	-	-
2	Age	8.21	1.59	−.23**	-	-	-	-	-	-	-	-	-
3	Self-report Identity - Test	2.57	.84	.72***	−.10	-	-	-	-	-	-	-	-
4	Self-report Identity - Retest	2.66	.86	.76***	−.09	.76***	-	-	-	-	-	-	-
5	Self-report Identity (average)	2.65	.79	.78***	−.11	.93***	.94***	-	-	-	-	-	-
6	IAT - Test	.05	.55	.69***	−.05	.45***	.55***	.54***	-	-	-	-	-
7	IAT - Retest	−.01	.56	.53***	−.01	.44***	.36***	.42***	.56***	-	-	-	-
8	IAT (average)	.00	.49	.69***	−.02	.52***	.50***	.53***	.88***	.89***	-	-	-
9	Coloring Book Selected	2.59	1.19	.74***	−.17†	.57***	.63***	.63***	.54***	.41***	.52***	-	-
10	Clothing - Test	2.88	1.22	.92***	−.28**	.78***	.77***	.82***	.60***	.52***	.63***	.77***	-
11	Clothing - Retest	3.11	1.34	.93***	−.18	.73***	.77***	.79***	.65***	.49***	.63***	.78***	.92***

Note: *** = $p < .001$, ** = $p < .01$, * = $p < .05$ (two-tailed)

Table 8. Mean and standard deviations of the six non-demographic variables in male and female participants in Study 2. The *p*-values are from two-tailed t-tests (*df* ranging from 69.82 to 101.82). Effect sizes are presented as Cohen's *d*s.

Variable	Females	Males	*t*	*p*	*d*
Self-report Identity - Test	3.17 (.61)	1.96 (.56)	10.48	< .001	2.05
Self-report Identity - Retest	3.30 (.58)	2.0 (.55)	11.43	< .001	2.31
Self-report Identity (average)	3.24 (.52)	2.01 (.48)	12.11	< .002	2.47
IAT - Test	.45 (.40)	−.31 (.40)	9.21	< .001	1.89
IAT - Retest	.29 (.50)	−.30 (.46)	6.06	< .001	1.23
IAT (average)	.37 (.38)	−.32 (.34)	8.92	< .001	1.90
Coloring Book Selected	3.44 (.80)	1.68 (.80)	10.50	< .001	2.20
Clothing - Test	3.99 (.54)	1.76 (.44)	21.78	<.001	4.51
Clothing - Retest	4.28 (.54)	1.80 (.46)	21.22	<.001	4.96

behavior (all r's ≥ .53), implicit and explicit gender identity measures (from test, retest, and the average score from test and retest) were correlated (r's ranging from .36 to .94), and femininity/masculinity ratings of participant's clothing closely aligned with femininity/masculinity of the coloring book page that participants selected as a prize. Table 8 shows differences in gender identity and behavior measures between male and female participants. These results also paralleled results from Study 1 such that boys had much lower scores than girls on all variables (all Cohen's ds ≥ 1.23).

Test-Retest Reliability

Scores on the Gender Identity IAT were significantly correlated across the 1-month lag time, $r(88) = .56$, $p = < .001$. Scores on the explicit gender identity measure were significantly correlated across the 1-month, $r(94) = .76$, $p = < .001$.[11] Age was not correlated with the absolute difference between IAT scores, $r(88) = .00$, $p = .98$, or explicit gender identity scores, $r(94) = -.03$, $p = .75$, from test and retest.

Predictive Validity

The zero-order correlations between IAT scores and gender-related behavior measures in Table 7 provide evidence for the predictive validity of the Gender Identity IAT. Interestingly, IAT scores not only predicted gender behavior measured later in the same testing session (e.g., IAT scores from test predicting clothing ratings from test), but also gender behavior even a month later (r's = .54, .65). Explicit gender identity was also a strong predictor of gender behavior (r's ranging from .57 to .82).

Table 9 presents the results of multiple regression models predicting the masculinity/femininity of the coloring book page that children chose as a prize at the conclusion

[11] The expected test-retest reliability of the average score on the IAT and explicit measure was .72 and .86, respectively.

Table 9. The unstandardized coefficients, standard error of the unstandardized coefficients, and standardized coefficients (β) of hierarchical multiple regression models regressing *femininity/masculinity of coloring book selection* on (a) gender identity measures (from test, retest, and the average between test and retest) and (b) demographic variables in Study 2.

	Model 1			Model 2			Model 3			Model 4			Model 5			Model 6		
Variable	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β
Self-report Identity - Test	.56***	.14	.40	-	-	-	-	-	-	.14	.15	.10	-	-	-	-	-	-
IAT - Test	.79***	.21	.36	-	-	-	-	-	-	.15	.23	.07	-	-	-	-	-	-
Self-report Identity - Retest	-	-	-	.78***	.12	.56	-	-	-	-	-	-	.23	.16	.17	-	-	-
IAT - Retest	-	-	-	.49*	.19	.23	-	-	-	-	-	-	.12	.18	.06	-	-	-
Self-report Identity (average)	-	-	-	-	-	-	.75***	.15	.50	-	-	-	-	-	-	.25	.18	.16
IAT (average)	-	-	-	-	-	-	.70**	.24	.29	-	-	-	-	-	-	.12	.26	.05
Gender (1= Female)	-	-	-	-	-	-	-	-	-	1.52***	.31	1.27	1.40***	.30	1.17	1.43***	.35	1.20
Age	-	-	-	-	-	-	-	-	-	−.03	.06	−.04	−.02	.06	−.02	−.03	.08	−.04
R^2		.41			.43			.45			.56			.56			.56	

Note: *** = $p < .001$, ** = $p < .01$, * = $p < .05$.

Table 10. The unstandardized coefficients, standard error of the unstandardized coefficients, and standardized coefficients (β) of hierarchical multiple regression models regressing *femininity/masculinity of clothing worn during testing session* on (a) gender identity measures from the *same* testing session and (b) demographic variables in Study 1. Average scores on gender identity measures from test and retest were used to predict the average clothing rating from test and retest. Results were unchanged using identity measures collected at test to predict clothing worn at retest.

	Model 1			Model 2			Model 3			Model 4			Model 5			Model 6		
Variable	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β
Self-report Identity - Test	.89***	.12	.61	-	-	-	-	-	-	.23*	.10	.16	-	-	-	-	-	-
IAT - Test	.59**	.17	.27	-	-	-	-	-	-	−.05	.13	−.02	-	-	-	-	-	-
Self-report Identity - Retest	-	-	-	1.05***	.12	.25	-	-	-	-	-	-	.12	.12	.08	-	-	-
IAT - Retest	-	-	-	.59**	.18	.67	-	-	-	-	-	-	.09	.13	.04	-	-	-
Self-report Identity (average)							1.01***	.12	.28	-	-	-	-	-	-	.16	.11	.10
IAT (average)							.71***	.20	.64	-	-	-	-	-	-	.07	.14	.03
Gender (1= Female)	-	-	-	-	-	-	-	-	-	1.91***	.19	1.56	2.25***	.23	1.68	2.06***	.20	1.65
Age	-	-	-	-	-	-	-	-	-	−.07*	.03	−.10	−.02	.05	−.02	−.04	.04	−.06
R^2		0.64			.65			.72			.86			.86			.91	

Note: *** = $p < .001$, ** = $p < .01$, * = $p < .05$.

of the study. Table 10 presents parallel analyses in which masculinity/femininity of the clothing that children wore in the testing session served as the outcome variable. Results were similar to Study 1 in that scores on the Gender Identity IAT and explicit gender identity measure independently predicted scores on the gender behavior measures and explained a high proportion of the variance (R^2's ranging from .41 to .72; see Models 1-3 in Tables 9 and 10). With the exception of explicit identity predicting masculinity/femininity of clothing worn during testing session at test (Model 4 in Table 10), identity measures were *not* significant predictors of gender-related behavior after controlling for participant sex and age (see Models 4-6 in Tables 9 and 10).

Discussion

Study 2 tested the test-retest reliability of the Gender Identity IAT across a 1-month lag and found that the test-retest coefficient was significantly differed from zero (r = .56), which suggests that the Gender Identity IAT tested here assesses trait-like associations in young children. The magnitude of this correlation is not only similar in magnitude that that observed in adults (median r = .56 reported in adult samples; Nosek et al., 2007), but is also indistinguishable from the point estimate of the test-retest coefficient obtained in Study 1. Indeed, the test-retest coefficient 95% confidence interval (CI) in Study 2 extends from .40 to .68, which clearly captures the point estimate from Study 1 observed with a 10-minute lag (r= .63). This result is interesting when taken with findings that that lag time did not affect the strength of IAT test-retest coefficients observed in adults (Nosek et al., 2007). Results from Study 2 suggest the possibility that children's implicit gender cognitions may be invariant over extended periods of time (a point we return to in Study 5). Study 2 also replicated results from Study 1 in which both implicit and explicit gender identity predicted scores on the gender behavior measure, though not after controlling for participant sex and age. However, like Study 1, we again found evidence that "net suppression" may complicate interpretation of our presented regression weights (compare IAT estimates from Models 1 and 3 in Table 10). Finally, replicating results from Study 1, Study 2 similarly found that participant age was not related to either the magnitude or degree of stability of scores on either gender identity measure.

Study 3:

Implicit Racial Attitudes across 10 minute Lag

While Studies 1 and 2 provided initial evidence for the test–retest reliability and predictive validity of children's Gender Identity IAT scores, it is unclear whether these results hold for IATs assessing other constructs. As previously discussed, research from adult subjects shows clear domain differences in the test–retest reliability and predictive validity of IAT scores (e.g., Bar-Anan & Nosek, 2014; Greenwald et al., 2009). In Study 3, we tested this possibility for young children by investigating the test-retest reliability (across a 10-minute lag time) and predictive validity of a Race Attitude IAT. With the exception of domain, all other factors were held constant from Study 1 (e.g., age of respondents, structure of the IAT, lag time), which afforded an opportunity to test whether domain differences affected the test-retest reliability and predictive validity estimates.

Method

Unless otherwise noted, all methods in Study 3 were identical to those used in Study 1.

Participants

One hundred and eight White American children between the ages of 6 and 11 years participated in in the study at a research lab located at the University of Washington. One participant was identified by parental report as Asian-American and another did not complete any of the study procedures. Data from these participants are not reported in any analyses. An additional five participants failed to complete all study measures and an IAT score from one other was removed due to the established exclusion criteria. Nevertheless, the other data from these participants are reported whenever possible. Table 11 shows the demographic characteristics of the sample.

Measures

Race-Related Behavior. We devised a new race-related behavioral measure. Participants were told a cover story that children visiting the lab space often colored pictures while waiting for their activities to start. Participants were told of an ongoing art contest open to children that had previously visited the lab and asked if they would be willing to vote for a winner between two finalists. All participants agreed to vote. Participants were then presented two pictures, both of which were the same template of a house taken from a coloring book. Each house was colored differently but with the same colors (one house had a red roof and purple walls while the other house had purple walls and a red roof). Children younger than 8-years-old were shown a more basic house template, while children 8-years and older were shown a more elaborate house taken from the same coloring book.

On the art contest entries, we paper-clipped non-professional (approximately 3 x 3 inch size) colored photographs of White and Black children, with the White child attached to one entry and the Black child paper-clipped to the other entry. The photographs were approximately matched to the participant's gender and age. Combinations of child's race and house pictures were counterbalanced across participants. While presenting the art contest entries, the experimenter identified the children as the creator of each picture by pointing at the target before nonchalantly unclipping the photo and placing it next to the picture. Finally, participants were asked which child should win the art contest and receive a prize.

Explicit Racial Attitudes. Participants were asked to indicate how much they liked 8 children depicted serially in photographs on a laptop computer. Four of the photographs were of Black children and the other four were of White children (race of evaluated child alternated on each trial). All photographs were in color, edited so that each child's head and shoulders were presented on a white background, and approximately matched to the participant on both gender and age.[12] Participants made evaluations using a 6-point smiley-face scale (1= "really don't like"*;* 6 = "really like"), and an explicit preference index for Whites relative to Blacks was computed by subtracting the mean liking of the four photographs of White children from the mean liking of the four photographs of Black children. Scores greater than zero indicate preference for Whites over Blacks, scores near zero indicate no preference, and scores less than zero indicate preference for Blacks over Whites.

Implicit Racial Attitudes. Implicit racial attitudes were assessed with the same child-adapted IAT as described in Study 1. However, the target concepts of "Me" and "Not Me" were replaced with "Black" and "White", and the attribute concepts of "male" and "female" were replaced with the categories of "good" and "bad." The target concepts were represented by photographs of Black and White children (boys and girls), and two additional photographs served as the category labels. The attribute concepts were represented by the photographs of pleasant (e.g., a wrapped gift, a gumball machine, a litter of puppies, and a portion of ice cream)

[12] A racially and ethnically diverse group of undergraduate research assistants (a) located the photographs used in the explicit attitude measure and (b) rated all of these photographs on dimensions of perceived age, warmth, and attractiveness. Each prospective photograph was rated by between 8 and 15 raters and the internal consistencies for both the age (α = .97) and warmth ratings (α = .917) were excellent, while internal consistency for the attractiveness ratings was acceptable (α = .65). Two sets of photographs were created for each gender and age group by assembling photographs of White and Black children that were similar with respect to each dimension. Given the low internal consistency of the initial attractiveness ratings, three additional undergraduate research assistants (that did not provide the initial evaluations) rated the White and Black children within each set as equally attractive both *within* and *between* the final sets of photographs. Participants rated different sets of photographs at test and retest for the explicit attitude measure. The order in which participants rated the two sets of photographs was counterbalanced across participants. One photograph set presented a White child as the first photograph while the other set presented a Black child as the first photograph.

and unpleasant stimuli (e.g., a house on fire, a car crash, a tarantula, and a broken house-window). The logic of this IAT was that children that prefer Whites over Blacks should respond more rapidly when photographs of White children are paired with pleasant images relative to unpleasant images. The IAT was scored such that positive values indicate higher identification with female relative to male, and Spearman-Brown corrected split half reliability at test and retest was .59 and .64, respectively.

Procedure

After obtaining consent, an experimenter requested the participant vote in an “art contest” (the race-related behavioral measure) before beginning their activities. The art contest was portrayed as unrelated to the research study for which the lab visit was scheduled. Next, a second experimenter (in order maintain the appearance that the art contest was unrelated to the research study) entered testing room and administered all study measures.[13]

Results

Degrees of freedom vary across statistical test due to incomplete data on various study measures as described above.

Descriptive Statistics

Table 11 shows overall means and standard deviations among all study variables. Table 11 also presents results showing whether non-demographic variables differed from either zero or chance responding. Scores on the explicit racial attitude measure were different from zero at test and when averaged across test and retest, indicating that participant’s self-reported liking of White children was higher than their self-reported liking of Black children. However, explicit racial attitude scores were not different from zero at retest, and examining the effect sizes of the

[13] For two participants, one experimenter administered all study measures in that second experimenter was not available.

scores (from test, retest, and the average from test and retest) indicates that participant's explicit preference for Whites over Blacks was small (Cohen's d's $\leq .27$). Similarly, the proportion of participants selecting the White child as the winner on the art contest behavioral measure did not differ from chance (Proportion = .44, $p = .29$), and the effect size was small (Cohen's $h = .12$; Cohen, 1988). In contrast, scores on the Race Attitude IAT at test, retest, and averaged across test and retest indicated a large effect such that participant's implicitly preferred Whites over Blacks (Cohen's d's $\geq .96$).

Table 11 also shows the zero-order correlations among both demographic and non-demographic variables. Replicating results from previous research (Dunham, Baron, & Banaji, 2006; Dunham, Baron, & Carey, 2011), scores on the IAT and explicit racial attitude measure were not correlated at either test or retest (r's $\leq .01$), and girls were more likely than boys to choose the White child as the winner of the art contest ($r = .22$). Further, participant age was negatively associated with scores on the explicit attitude measures (at test and retest) and performance on the art contest, but uncorrelated with scores on the IAT (see Table 11).

Test-Retest Reliability

Scores on the Race Attitude IAT were significantly correlated across the 10-minute lag time, $r(100) = .33$, $p = < 001$. Scores on the explicit racial attitude measure were significantly correlated across the 10-minute, $r(101) = .63$, $p = < 001$.[14] Age was not correlated with the absolute difference between IAT scores, $r(100) = -.02$, $p = .81$, or explicit gender identity scores, $r(101) = -.09$, $p = .35$, from test and retest.

[14] The expected test-retest reliability of the average score on the IAT and explicit measure was .50 and .77, respectively.

Table 11. Zero-order correlations among demographic variables and measured variables in Study 3. *N*'s for means and standard deviations ranged from 72 to 106. Correlations and associated *p*-values were computed with *N*'s ranging from 106 to 101.

	Variable	**Mean**	**SD**	***t***	***d***	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**
1	Gender (Female = 1)	.46	.50	-	-	-	-	-	-	-	-	-	-
2	Age	8.78	1.98	-	-	−.11	-	-	-	-	-	-	-
3	Self-report Attitude - Test	.15	.81	1.87	.19	−.03	−.53***	-	-	-	-	-	-
4	Self-report Attitude - Retest	.23	.84	2.78**	.27	.16	−.41***	.63***	-	-	-	-	-
5	Self-report Attitude (average)	.18	.75	2.47*	.24	.11	−.52***	.90***	.90***	-	-	-	-
6	IAT - Test	.46	.43	11.20***	1.07	.33***	.16	.01	−.01	.01	-	-	-
7	IAT - Retest	.43	.45	9.72***	.96	.19†	.15	−.10	−.17†	−.15	.33***	-	-
8	IAT (average)	.45	.35	12.93***	1.29	.31**	.17†	−.05	−.12	−.09	.80***	.83***	-
9	Art Winner (White child = 1)	.44	.50	1.14	.12	.22*	−.28**	.24*	.19†	.24*	.23*	.26**	.30**

Note: *** = $p < .001$, ** = $p < .01$, * = $p < .05$, † = $p < .10$

1. The percentage of White children chosen to win the art contest was tested from chance using a one-sample proportion test. The test-statistic reported for the test is a χ^2.
2. The reported effect size for the proportion of children choosing a White child as the winner on the art contest (from chance responding) is Cohen's *h*.

Table 12. Estimates, standard errors, and 95% confidence intervals (CI) of standardized odds ratios (all predictors other than gender were standardized) of hierarchical multiple logistic regression models regressing race of the child selected as the winner on the art contest (White = 1; Black = 0) on (a) racial attitude measures from the *same* testing session and (b) demographic variables in Study 3.

	Model 1			Model 2			Model 3			Model 4			Model 5			Model 6		
Variable	OR	SE (OR)	95% CI	OR	SE (OR)	95% CI	OR	SE (OR)	95% CI	OR	SE (OR)	95% CI	OR	SE (OR)	95% CI	OR	SE (OR)	95% CI
Self-report Identity - Test	1.67*	1.24	1.11-2.63	-	-	-	-	-	-	1.20	1.30	.72-2.04	-	-	-	-	-	-
IAT - Test	1.69*	1.25	1.11-2.68	-	-	-	-	-	-	1.71*	1.28	1.06-2.86	-	-	-	-	-	-
Self-report Identity - Retest	-	-	-	1.72*	1.26	1.12-2.78	-	-	-	-	-	-	1.17	1.30	.70-2.00	-	-	-
IAT - Retest	-	-	-	2.05**	1.27	1.31-3.37	-	-	-	-	-	-	2.13**	1.29	1.32-3.66	-	-	-
Self-report Identity (average)	-	-	-	-	-	-	1.82**	1.26	1.18-2.96	-	-	-	-	-	-	1.18	1.32	.68-2.06
IAT (average)	-	-	-	-	-	-	2.19**	1.28	1.38-3.69	-	-	-	-	-	-	2.35**	1.31	1.41-4.22
Gender (1= Female)	-	-	-	-	-	-	-	-	-	2.49*	1.59	1.02-6.32	2.23†	1.62	.88-5.86	1.84	1.63	.71-4.86
Age	-	-	-	-	-	-	-	-	-	.52*	1.31	.30-.86	.44**	1.32	.25-.74	.44**	1.33	.24-.76
Nagelkerke's R^2	.14			.17			.21			.25			.30			.31		

Note: *** = $p < .001$, ** = $p < .01$, * = $p < .05$, † = $p < .10$

Predictive Validity

Table 11 provides initial evidence of the predictive validity of both the Race Attitude IAT and the explicit racial attitude measure. Indeed, IAT scores at test, retest, and averaged across test and retest were positively associated with performance on the art contest behavior measure (r's = .23 - .30). In contrast, performance on the art contest significantly correlated with explicit attitude scores at test and averaged across test and retest (both r's = .24), but not scores from retest (by conventional definitions of statistical significance; r =.19).

Table 12 presents the results of multiple logistic regression models predicting whether participants selected the White child as the winner of the art contest as a function of scores on the Race Attitude IAT and explicit attitude measure (both standardized) at test, retest and averaged across test and retest (Models 1-3, respectively). Estimates were transformed into odds ratios (exponentiated via the natural log), and values significantly bigger than one indicate a positive association between the predictor and criterion. Models 1 through 3 were similar such that the coefficients for the IAT and explicit attitude measure were significantly larger than one, indicating that higher implicit and explicit preference for Whites over Blacks was associated with an increased probability of choosing the White child as the art contest winner. These predictors explained a modest proportion of the variance (R^2's of .14 and .21; see Table 12).

Models 4 through 6 present results in which age and sex were added to the multiple logistic regression models containing implicit and explicit attitudes (see Table 12). Results were similar when using scores from test, retest, or those averaged between test and retest. While IAT scores remained positive and significant predictors of choosing a White child as the winner on the art contest in Models 4 through 6, the explicit attitude coefficients were non-significantly different from one. Additionally, age was a significant predictor and significantly below the reference value of one, which indicates that increased participant age was associated with a *lower* probability of choosing the White child on the art contest.

A more intuitive sense of the predictive utility of the IAT was obtained by comparing the predicted probability of choosing the White child to win the art contest at ± 1 standard deviation on the IAT (while holding other values at their respective means and fixing gender as female). For a participant at −1.0 standard deviations below the IAT mean, the predicted probabilities of choosing a White art contest winner was 40.9% ,36.0%, and 31.3% for scores at test, retest, and averaged across test and retest, respectively. In contrast, the predicted probability of choosing a White art contest winner for participants at +1.0 standard deviation above the IAT mean was 66.8%, 72.1%, and 71.6% at test, retest, and averaged across test and retest, respectively.

Discussion

Study 3 investigated the test – retest reliability of the Race Attitudes IAT in children and found that scores across a 10-minute lag time were positively correlated (r = .33). However, the relatively modest correlation between IAT scores is somewhat unexpected when considered in the context of the test-retest coefficients reported in Studies 1 and 2 (r's > .50). However, is the estimate obtained in Study 3 meaningfully different than those obtained in Studies 1 and 2? The 95% CI on the test-retest point estimate from Study 3 extends from .14 to .49, which indicates that scores on the Race Attitude IAT are weaker than those observed for the Gender Identity IAT. This result may indicate a clear difference in the trait-like association that young children hold between racial groups and valence and themselves and gender categories. A different possibility is that differences in test-retest reliability coefficients from Studies 1 and 2 and the estimate obtained in Study 3 is due to characteristics of the samples – specifically, the fact that Studies 1 and 2 sampled respondents from both focal groups of the Gender Identity IAT (i.e., male and females) while Study 3 only had respondents from one focal group of the Race Attitude IAT (i.e., Whites). We return to this point after presenting the results for Study 5.

Paralleling the results of Studies 1 and 2 in the domain of gender, Study 3 found that IAT scores and scores on the explicit attitude measure were predictive of race-related behavior. However, there were several key differences in the results from Study 3. First, implicit and explicit racial attitudes predicted a smaller proportion of the variance in the criterion variable than corresponding gender identity measures in Studies 1 and 2. Second, Studies 1 through 3 were aligned in their findings in that both implicit and explicit measures independently predicted behavior, but the results from Study 3 diverged in that IAT scores predicted behavior above and beyond demographic covariates, while explicit measures did not. This result is consistent with research from adults showing that the IAT outperforms corresponding explicit measures in predicting behavior in social sensitive domains (e.g., intergroup and interracial relations; Greenwald et al., 2009).

Finally, replicating results from Studies 1 and 2 in the domain of gender identity, Study 3 found that participant age was not related to the degree of stability of scores on either racial attitude measure. However, results from Study 3 did differ from those reported in Studies 1 and 2 in that participant age was related to the magnitude of explicit (but not implicit) racial attitudes. Indeed, our finding that older children tended to report weaker preferences for Whites relative to Blacks replicates previous work demonstrating that explicit preference for one's group declines throughout childhood but that implicit preference stays constant (Baron & Banaji, 2006).

Study 4:

Implicit Racial Attitudes across One-Month Lag

In Study 4 we extended the lag time between test and retest of the Race Attitude IAT to 1-month. We also used Study 4 as an opportunity to replicate our findings from Study 3 in which scores on the Race Attitude IAT predicted race-related behavior.

Method

Unless otherwise noted, all methods in Study 4 were identical to those used in Study 3.

Participants

One hundred and nine White American children between the ages of 6 and 11 years old participated in the study at either in a quiet space in their school (*N*=96) or a research lab (*N*=13) located at the University of Washington. At test, three participants were absent and two others failed to complete the task. At retest, five participants were absent and one other failed to complete all IAT procedures. Data from these participants is reported whenever possible for the other measures. Table 13 shows the age and gender characteristics of the sample for the study.

Procedure

After obtaining consent, participants completed the explicit and implicit attitude measure. Approximately 3 to 4 weeks later (*M* = 28 days, *SD* = 4 days), we either returned to participant's school or the participant came back into the lab and again completed the implicit and explicit attitude measure. Finally, a second experimenter administered the art contest.

Measures

Explicit Racial Attitudes. We created an explicit attitude measure in this study to match the relative nature of the IAT. Across eight trials, participants were shown pairs of children (one White and the other Black) and asked to choose they liked better between two (e.g. "We're going to do an activity where we want you to tell us which of these kids you like better. They will be in pairs but you can only choose one kid."). All presented pairs of children were (a) gender-matched to the participant and (b) approximately the same age in middle childhood. We used the same pre-rated photographs in Study 1. To index relative preference for White over Blacks, we summed the number of trials in which the participant indicated liking the White child more than the Black child. Thus, values ranged from zero to eight.

Implicit Racial Attitudes. The Spearman-Brown correct split half reliability of the Race Attitude IAT at test and retest was .66 and .61, respectively.

Results

Degrees of freedom vary across statistical test due to incomplete data on various study measures.

Descriptive Statistics

Table 13 shows (a) overall means and standard deviations among all study variables and (b) results showing whether non-demographic variables differed from either zero, the midpoint of the scale or chance responding. Scores on the explicit racial attitude measure at both test, retest, and the average of scores from test and retest were significantly above the midpoint, indicating that across the eight test trials, participants on average liked the White child more often than the Black child. The effect size for participant's preference for White over Black was medium with Cohen's *d*'s of .57, .45, and .58 from test, retest, and the average from test and retest, respectively. Similar to the results from Study 3, participant's IAT scores strongly differed from zero (Cohen's *d*'s $\geq$ .96), indicating a strong implicit preference for Whites over

Blacks. Also, the proportion of participants selecting the White child as the winner on the art contest behavioral measure did not differ from chance (Proportion = .51, p = .84), and the effect size was near zero (Cohen's h = .02).

Table 13 also shows the zero-order correlations among both demographic and non-demographic variables. Unlike the results from Study 3, scores on the IAT and explicit racial attitude measures were generally correlated (r's = .18 - .41), and girls were no more likely than boys to choose the White child as the winner of the art contest (r = .15, p = .138). However, we did replicate our findings from Study 3 in which participant age was negatively associated with scores on the explicit attitude measures (at test and retest) and performance on the art contest, but uncorrelated with scores on the IAT (see Table 13).

Test-Retest Reliability

Scores on the Race Attitude IAT were significantly correlated across the 1-month lag time, $r(97)$= .25, p = 014. Scores on the explicit racial attitude measure were significantly correlated across the 1-month lag time, $r(97)$= .65, p = < 001.[15] Age was not correlated with the absolute difference between IAT scores, $r(97)$= −.01, p = .94, or explicit gender identity scores, $r(97)$= .01, p = .49, from test and retest.

Predictive Validity

Table 13 presents the zero-order correlations among attitude measures and scores on the art contest (White child as winner = 1). Unlike Study 3, we found that IAT scores from test (r =.10), retest (r = .17), or averaged across test and retest (r = .17) were *not* associated with choosing the White child to win the art contest. In contrast, favoring Whites over Blacks on the

[15] The expected test-retest reliability (Spearman-Brown corrected) of the average score on the IAT and explicit measure (from test and retest) was .40 and .79, respectively.

Table 13. Zero-order correlations among demographic variables and measured variables in Study 4. *N*'s for means and standard deviations ranged from 99 to 109. Correlations and associated *p*-values were computed with *N*'s ranging from 98 to 109.

	Variable	**Mean**	**SD**	***t***	***d***	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**
1	Gender (Female = 1)	.47	.50	-	-	-	-	-	-	-	-	-	-
2	Age	8.65	1.89	-	-	.07	-	-	-	-	-	-	-
3	Self-report Attitude - Test	5.10	1.94	27.13***	.57	.18†	−.35***	-	-	-	-	-	-
4	Self-report Attitude - Retest	4.94	2.10	23.70***	.45	.13	−.35***	.65***	-	-	-	-	-
5	Self-report Attitude (average)	5.04	1.79	27.92***	.58	.18†	−.40***	.90***	.92***	-	-	-	-
6	IAT - Test	.43	.45	9.64***	.96	.12	.04	.26**	.18†	.26**	-	-	-
7	IAT - Retest	.48	.43	11.43***	1.12	.22*	−.04	.36***	.26**	.35***	.25*	-	-
8	IAT (average)	.45	.35	12.94***	1.29	.20*	−.03	.41***	.30**	.40***	.80***	.78***	-
9	Art Winner (White child = 1)	.51	.50	.04	.02	.15	−.42***	.25*	0.26**	.27**	.10	.17†	.17†

Note: *** = $p < .001$, ** = $p < .01$, * = $p < .05$, † = $p < .10$

1. The percentage of White children chosen to win the art contest was tested from chance using a one-sample proportion test. The test-statistic reported for the test is a χ^2.
2. The reported effect size for the proportion of children choosing a White child as the winner on the art contest (from chance responding) is Cohen's *h*.

Table 14. Estimates, standard errors, and 95% confidence intervals (CI) of standardized odds ratios (all predictors other than gender were standardized) of hierarchical multiple logistic regression models regressing race of the child selected as the winner on the art contest (White = 1; Black = 0) on (a) racial attitude measures from the *same* testing session and (b) demographic variables in Study 4.

	Model 1			Model 2			Model 3			Model 4			Model 5			Model 6		
Variable	OR	SE (OR)	95% CI	OR	SE (OR)	95% CI	OR	SE (OR)	95% CI	OR	SE (OR)	95% CI	OR	SE (OR)	95% CI	OR	SE (OR)	95% CI
Self-report Identity - Test	1.72*	1.27	1.09-2.79	-	-	-	-	-	-	.93	1.34	.52-1.66	-	-	-	-	-	-
IAT - Test	1.06	1.25	.69-1.64	-	-	-	-	-	-	1.30	1.28	.81-2.14	-	-	-	-	-	-
Self-report Identity - Retest	-	-	-	1.66*	1.25	1.08-2.61	-	-	-	-	-	-	1.23	1.29	.75-2.06	-	-	-
IAT - Retest	-	-	-	1.28	1.25	.83-2.00	-	-	-	-	-	-	1.24	1.28	.78-2.04	-	-	-
Self-report Identity (average)	-	-	-	-	-	-	1.75*	1.27	1.10-2.88	-	-	-	-	-	-	1.04	1.35	.58-1.86
IAT (average)	-	-	-	-	-	-	1.19	1.27	.75-1.91	-	-	-	-	-	-	1.38	1.29	.83-2.32
Gender (1= Female)	-	-	-	-	-	-	-	-	-	2.32†	1.64	.88-6.20	2.04	1.62	.80-5.40	1.84	1.64	.70-5.00
Age	-	-	-	-	-	-	-	-	-	.32***	1.35	.17-.55	.38***	1.30	.22-.62	.33***	1.35	.18-.57
Nagelkerke's R^2	.09			.11			.12			.31			.30			.32		

Note: *** = $p < .001$, ** = $p < .01$, * = $p < .05$, † = $p < .10$

explicit attitude measures (at test retest, and averaged across test and retest) was associated with choosing a White child to win the art contest (*r*'s = .25 -.27).

Table 14 presents the results of multiple logistic regression models (estimates transformed into odds ratios) predicting whether participants selected the White child as the winner of the art contest as a function of scores on the Race Attitude IAT and explicit attitude measure (both standardized) at test, retest, and averaged across test and retest (Models 1 through 3, respectively). Models 1 through 3 were similar such that the coefficients for the explicit attitude measure were significantly larger than one, which indicates that even while controlling for differences in implicit preference for Whites over Blacks, higher explicit preference for Whites over Blacks was associated with an increased probability of choosing the White child as the art contest winner. In contrast, the coefficient for IAT scores was not significantly different from one in Models 1 through 3. These predictors were a very modest proportion of the variance (R^2's of .09 and .12; see Table 14).

Models 4 through 6 present results in which age and sex were added to the multiple logistic regression models containing implicit and explicit attitudes. Results were similar across both models: Age was a significant predictor and significantly below the reference value of one, which indicates that increased participant age was associated with a lower probability of choosing the White child on the art contest. All other coefficients did not significantly differ from one (see Table 14).

Though IAT scores were not associated with the race of the child selected to win the art contest, to provide a sense of the size of the IAT effect that was observed in Study 3, we again compared the predicted probability of choosing the White child to win the art contest at ± 1 standard deviation on the IAT (hold other predictors at their respective means and fixing gender as female). For a participant at −1.0 standard deviations below the IAT mean, the predicted probabilities of choosing a White art contest winner was 56.2%, 56.0%, and 52.9% for scores collected at test, retest, and averaged across test and retest, respectively. In contrast, the predicted probability of choosing a White art contest winner for participants at +1.0 standard deviation above the IAT mean was 68.3%, 66.2%, and 68.1% at test, retest, and averaged across test and retest, respectively.

Discussion

Study 4 investigated the test – retest reliability of the Race Attitudes IAT in children and found that scores across a 1-month lag time were positively correlated (r = .25). This estimate was similar to the test-retest coefficient for the Race Attitudes IAT in Study 3 (r = .33). Study 3 also found that IAT scores and scores on the explicit attitude measure were predictive of race-related behavior. In contrast, in Study 4 we found that scores on the explicit attitude measure (but not IAT) predicted scores on the race-related behavior measure – though not while controlling for participant age and sex. However, while the IAT scores from test and the average from test and retest were not significantly correlated with scores on the race-related behavior measure (r = .17, p = .09, 95% CI = −.02; .35), the 95% CI encompasses the estimated correlations between IAT scores (from test, retest, and the average across test and retest) and the race-related behavior measure in Study 3 (r’s =.23 - .30). Thus, at least for IAT scores collected at test in Study 4, the magnitude of the association between scores on the race-related behavior measure appears comparable with Study 3.

As the IAT and art contest were identical in Studies 3 and 4, the failure to find correspondence between the race-related behavior measure and IAT scores in Study 4 may have been due to a procedural variation between studies. One such procedural change that seems especially relevant is the timing of the art contest. In Study 3, participants completed the art contest prior to completing any attitude measures, while the art contest was the last task completed by participants in Study 4. Thus, completing the attitude measures prior to completing the art contest in may have cast doubt on our cover story that the task was unrelated to the study measures, affected participant responding, and in turn, attenuated correspondence between IAT scores and the behavior measure. However, this explanation seems unlikely in that the proportion of White children chosen to win the art contest was actually higher in Study 4 (Study 3: 44% vs. Study 4: 51%), whereas socially desirable responding should presumably led to a decrease in White children chosen to win the art contest.

The modest and variable estimates between scores on the Race Attitude IAT on a measure of race-related behavior in Studies 3 and 4 demonstrates a well-known link between measure reliability and validity. Indeed, reliability affects validity by setting an upper bound on the association between a measure and a criterion variable (Nunnally, 1978). In other words, if we replicated Study 3 or 4 many times and on each replication calculated the correlation between IAT scores on scores on the behavior measure, we would expect (on average) for IAT scores to correlate more strongly with scores from another administration of the same IAT than with scores on the race-related behavior measure. As such, due to the modest test-retest coefficient for the Race Attitude IAT (r = .25), associations between IAT scores and scores on the behavior measure should also be expected to be modest and variable (hence, the low correlation).

We also replicated the results of Study 3 by finding that participant age was associated with the magnitude of scores on the explicit racial attitude measure (but not the IAT), but not the stability of scores on either attitude measure. Lastly, we found that scores on the Race Attitude IAT were significantly correlated with scores on the explicit race attitude measure in Study 4 (but not Study 3). One possible explanation for this finding is that the structure of the explicit attitude measure used in Study 4 (a relative measure that is more similar to the IAT) is responsible for enhanced agreement among measures. Indeed, conceptual correspondence has been found to improve correlations between scores on the IAT and corresponding explicit measures (Hofmann, Gawronski, Gschwendner, Le, & Schmitt, 2005).

Study 5:

Implicit Gender Identity across One-Year Lag in Gender Diverse Sample

In Study 5, we sought to test whether the more reliable of the two versions of the IATs tested here, the Gender Identity IAT, would be reliable when assessed over a longer time period (1-year). Further, we used this opportunity to test whether implicit gender identity would be reliable across time in an especially diverse group—not only gender “typical” children, but we also included children who identify as transgender or gender nonconforming. Examining this population allowed us to assess how generalizable our findings might be. Demonstrating adequate test-retest reliability across such a long lag time and within a more diverse sample would provide especially strong evidence that the Gender Identity IAT assesses trait-like associations.

Method

Data were collected as part of an ongoing longitudinal study of gender development in socially supported transgender children.[16] In the current study, only IAT scores were analyzed; other measures collected in the longitudinal study are the focus of other reports.

[16] Gender diverse children represent a small portion of the population, which makes them especially at-risk for being identified. Thus, to protect the identities of participants in this study, data from Study 5 is not posted on OSF.

Participants

Participants in the current study were (1) children who were themselves "gender diverse" (which included transgender children as well as children who identify male AND female, and children who defy traditional gender stereotypes) (*N*=32), (2) siblings of children who are gender diverse (*N*=22), and (3) unrelated gender-typical children (*N*=31) who are used in the longitudinal studies as a control group, for a total of 85 participants. The latter two groups were combined in analyses as they were all gender "typical". Participants completed the study measures at either conferences, support meetings, their family's home, or at a research lab at the University of Washington. Only children who first completed the Gender Identity IAT when they were between the ages of 6 and 11 years (*M* = 8.91 years, *SD* = 1.68 years) were eligible to inclusion in this study. Additionally, children had to have returned for a follow-up IAT before July 16, 2016 in order to be included (this study is ongoing). Due to experimenter error, IAT data from the second administration of the IAT was lost for nine participants that met our inclusion criteria.

Procedure

In Study 5, participants completed the Gender Identity IAT, and approximately 1-year later (*M* = 431 days, *SD* = 93 days), participants completed the measures for a second time.

Measures

Implicit Gender Identity. The Spearman-Brown correct split half reliability of the Gender Identity IAT at test and retest was .73 and .80, respectively.

Results

Descriptive Statistics

Table 15 shows overall means and standard deviations among IAT scores (from both test and retest) broken down by participant's natal sex (assigned at birth) and whether they were gender diverse or gender-typical. Although the small cell sizes preclude testing for statistical significance across groups, the means suggest when comparing children by their natal sex, gender diverse children have very different IAT scores than their gender "typical" peers.

Table 15. Mean and standard deviations of IAT scores (both test and retest) of participants in Study 5 stratified by natal sex and gender identity (gender typical vs. gender diverse).

Natal Sex	Male		Female	
Variable / Gender Status	Gender Typical (N = 24)	Gender Diverse (N = 18)	Gender Typical (N = 30)	Gender Diverse (N = 13)
IAT - Test	−.42 (.43)	.42 (.42)	.32 (.41)	−.22 (.44)
IAT - Retest	−.33 (.44)	.35 (.48)	.40 (.42)	−.27 (.53)

Test-Retest Reliability

Scores on the Gender Identity IAT were significantly correlated across the 1-year lag time, $r(83)=.50$, $p<.001$.[17] Age was not correlated with the absolute difference between IAT scores from test and retest, $r(83)=-.19$, $p = .08$.

Discussion

Results from Study 5 indicate that scores on the Gender Identity IAT are reliable even when tested over a lag time of 1-year and within a sample that has considerable gender diversity. This finding provides additional evidence that the measure is tapping into strong trait-like

[17] The expected test-retest reliability (Spearman-Brown corrected) of the average score on the IAT (from test and retest) was .67.

associations that are stable over long periods of time. Further, as in Studies 1 and 2, we found that participant age did not predict the stability of scores on the IAT.

Comparing Studies 1 through 5

As previously mentioned, sampling differences might explain why we found stronger test-retest reliability and predictive validity evidence for the Gender Identity IAT compared to the Race Attitude IAT. Recall that children from both focal groups of the Gender Identity IAT (i.e., male and female) completed the measure (as well as children who have non-binary identities, in Study 5), whereas children from only one of the IAT focal groups (i.e., White children) completed the Race Attitude IAT. As such, evaluating the Race Attitude IAT in a racially homogenous sample (opposed to a racially diverse sample) may have led to weaker test-retest coefficients and predictive validity estimates (e.g., due to a restricted range of scores; Sackett & Yang, 2000). To test this possibility, we performed an internal meta-analyses on Studies 1 through 5 to estimate the overall *within*-focal group (i.e., for individuals within each IAT target group) effect size of the test-retest reliability and predictive validity of the IAT. Further, we tested whether domain or lag-time moderated the strength of test-retest reliability, and whether domain moderated the strength of association between IAT and criterion variables.

Method

Correlations were computed separately for both males and females (Studies 1, 2, & 5)[18], as well as White children (Studies 3 & 4), which meant we had test-retest estimates from 8 independent samples and predictive validity estimates from 6 independent samples (recall predictive validity was not assessed in Study 5). To prevent dependency among our predictive validity estimates (from using several outcome variables taken from the same sample of

[18] To see results of Studies 1 and 2 disaggregated by participant sex, see Appendix A. In Study 5, test-rest correlations for the IAT for males, $r(42)=.48$, $p=.001$, and females, $r(41)=.58$, $p<.001$, were different from zero.

participants), for studies using the Race Attitude IAT we used correlations of average IAT scores (between test and retest) and the art contest behavior measure. For studies using the Gender Identity IAT, we used correlations between average IAT scores and the average of standardized scores on (a) masculinity/femininity of the coloring book page children selected at the conclusion of the experiment and (b) masculinity/femininity of the clothing that participants wore during the experiment.[19] Correlations were normalized via Fisher's *z*-transformation (as per the recommendation of Borenstein, Hedges, Higgins, & Rothstein, 2009), and then transformed back to the *r* metric for presentation. To assign more weight to samples yielding more precise estimates, we weighted estimates by the inverse of their variance (Borenstein et al., 2009). We used random effects fit by maximum likelihood estimation. Lag-time (measured in days) and domain (dummy coded with gender identity =0; race attitudes =1) were tested as moderators.

Results

Test-Retest Reliability

Figure 2 presents estimated within-focal group test-retest coefficients (along with 95 % CIs) for each sample along with the mean effect size across all samples ($\bar{r}$). Overall, children's IAT scores were moderately correlated across test and retest, $\bar{r}$ =.36, z = 8.02, p < .001. Moreover, we found IAT correspondence between test and retest did not differ as a function of domain, $B = -0.14$, $z = -1.47$, p = .142, or lag-time, B = .00, z = 1.37, p = .171.

Predictive Validity

Figure 3 presents estimated within-focal group predictive validity estimates as well as the mean effect size across samples. Overall, children's IAT scores were associated with behavioral

[19] For children in Study 2 that had two clothing ratings, we took the average of these scores before standardizing.

Figure 2. Shows within-focal group (e.g., males or Whites) *test-retest* coefficients (along with 95% CIs) for the studies using the Gender Identity IAT (Studies 1, 2, & 5) and Race Attitude IATs (Studies 3 & 4). The mean effect size represents the weighted average across all samples (N = 472). All estimates are scaled on the r metric.

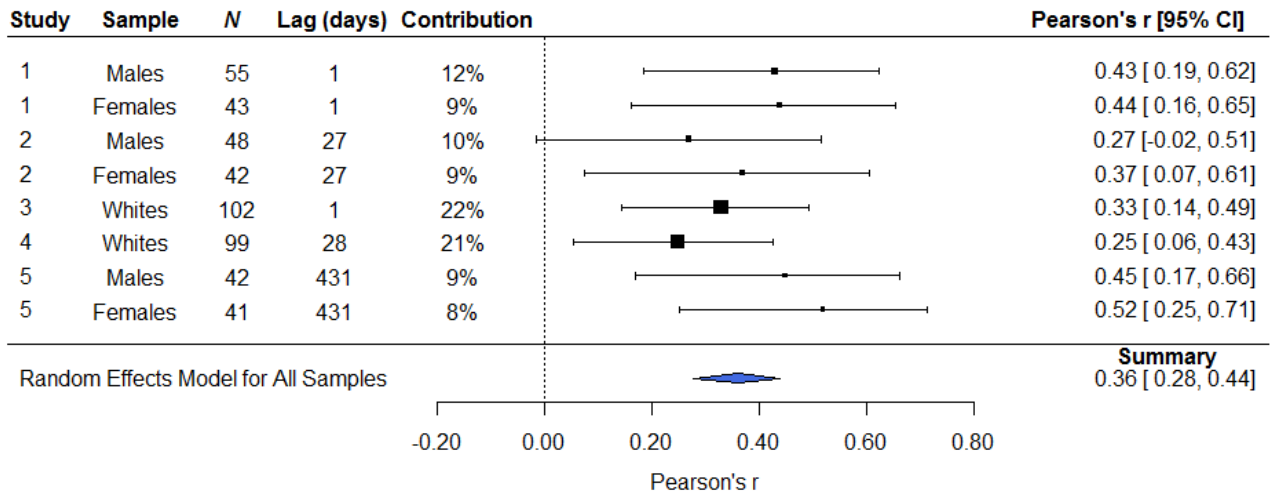
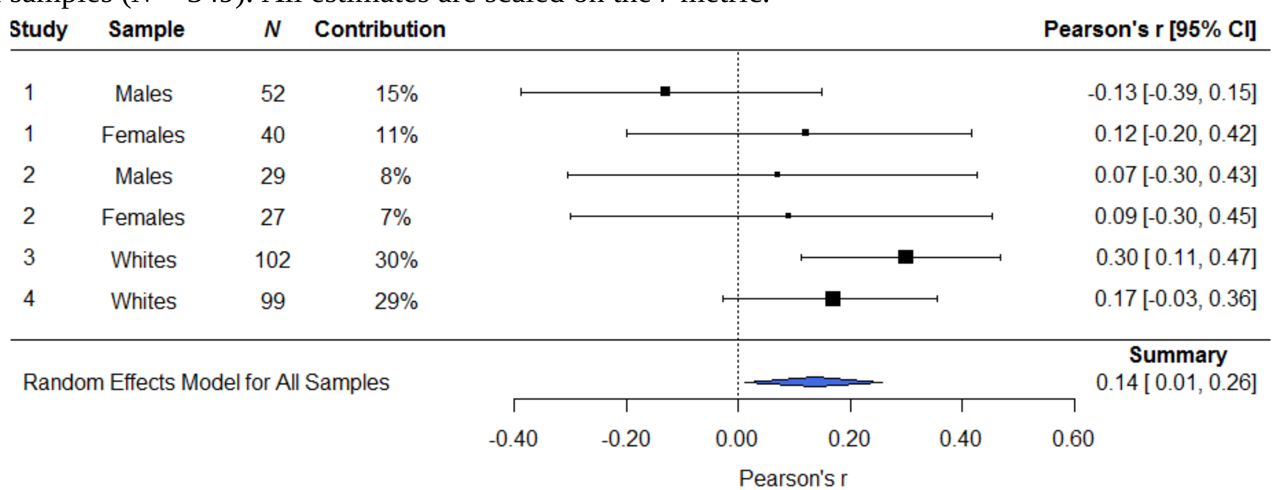


Figure 3. Shows within-focal group (e.g., males or Whites) *predictive validity* estimates (along with 95% CIs) for the studies using the Gender Identity IAT (Studies 1 & 2) and Race Attitude IATs (Studies 3 & 4). The mean effect size represents the weighted average across all samples (N = 349). All estimates are scaled on the r metric.



measures, $\bar{r}$ =.14, z = 2.13, p = .033. However, we found that domain moderated the association of IAT scores with behavioral measures such that IAT scores were *more* predictive of behavior in the domain of race attitudes (compared to gender identity), B = .23, z = 2.03, p = .043. In fact, IAT scores did not predict behavior in the domain of gender identity, B = .02, z = .18, p = .861.

Discussion

An internal meta-analysis of the results of Studies 1 through 5 produced two interesting findings. First, even within-focal groups, the IAT correlated with itself and with behavioral measures, such that mean effect sizes for test-retest reliability and predictive validity were greater than zero. Second, while there was no evidence that domain or lag-time moderated correspondence between IAT scores between test and retest, we did evidence that the Race Attitude IAT was more predictive of a behavioral than the Gender Identity IAT. However, due to the small number of samples considered here, these results should be taken cautiously.

General Discussion

We conducted five studies (with over 500 children) to investigate the effects of two factors that have varied in previous research investigating the test-retest reliability and predictive validity of the IAT in children: *lag time* (10-minutes vs. 1-month vs. 1-year) and *domain* (gender identity vs. race attitudes). At first glance, our results seemed to suggest that scores on the Gender Identity IAT had stronger test-retest reliability and predictive validity than the Race Attitude IAT. In Studies 1 through 4, we not only found that scores on the Gender Identity IAT had a test–retest reliability coefficient roughly twice as large as that of the Race Attitude IAT, but we also found that Gender Identity IAT scores had stronger bivariate associations with behavioral measures than the Race Attitude IAT. However, while participants in studies that used the Gender Identity IAT came from both IAT focal groups (i.e., both males and females), we only included White children in our studies testing the effects of the Race Attitude IAT. An internal meta-analysis that estimated *within*-group test-retest reliability and predictive validity revealed that (a) the mean test-retest reliability effect size was different from zero (r= .36) and did not significantly differ across domain or lag-time and (b) the mean predictive validity effect size was different from zero (r = .14), but that IAT scores better predicted behavior in the domain of race attitudes.

While we anticipated that lag-time might play a role in affecting the test-retest reliability of children's IAT scores, our results demonstrating the negligible impact of lag-time is particularly interesting in that it suggests that the IAT indexes trait-like associations in children. Often researchers only use test-retest coefficients from one lag-time to conclude that a construct is stable (Fraley & Roberts, 2005), which has been true for previous IAT test-retest reliability estimates from child samples (e.g., Bruni, 2007). However, data from at least two different lag-times (preferably more) are needed to assess whether the strength of test-retest reliability estimates decay over time. For example, while children's IAT scores are stable across 10-minutes, this coefficient could decrease to zero as test-retest interval increases (Fraley & Roberts, 2005). Another possibility is that coefficients might approach a non-zero asymptote as the test-retest interval increases, which would indicate the presence of a durable "trait" (Fraley & Roberts, 2005). As the results presented here indicate that the strength of the test-retest coefficient for IAT scores did not significantly differ across lag-time, it seems likely that the IAT is indexing a stable trait for child participants. Moreover, given that lag time had little impact on the test-retest reliability of the IAT in two domains (i.e., gender identity and racial attitudes), it seems possible that children as young as 6-years-old already hold trait-like implicit associations in numerous domains. In light of these findings, work might try to go even further by parceling out child-specific, situation-specific, and random- error in children's IAT scores using approaches such as a latent state-trait analysis (Steyer, Schmitt, & Eid, 1999; see also Schmukle & Egloff, 2004).

The findings and methodological approach used here might serve as a springboard for future research. Indeed, while we found that including members of both focal groups from the IAT increased test-retest reliability of the IAT in the domain of gender identity, future work might test whether correspondence between IAT scores is similarly increased when tested in samples of both White and Black respondents. As the IATs used here varied on both *construct* (attitude vs. identity) and *social category* (race vs. gender), future work might disentangle these factors by using IATs measuring different constructs but with the same social categories (e.g., gender attitude vs. gender identity), or alternatively, comparing IATs assessing the same construct but towards different social categories (e.g., gender attitude vs. race attitude). Finally, future research might test the effects of age and IAT structure, two factors that we did not manipulate here, on the test-retest reliability and predictive validity of the IAT. Indeed, despite the fact that participant age was not associated with stability of scores on either of the IATs tested here, perhaps children's IAT scores may become even more stable and predictive of behavior later development (i.e., through adolescence and into adulthood). Further, other IAT structures adapted for use with child samples may produce more reliable scores than the one used here (e.g., additional practice trials or test blocks). Future work testing these possibilities might make use the strategy employed here in which factors predicted to affect IAT performance– in this case age and IAT structure – are systematically varied either within or between studies.

Finally, while we replicated previous work showing that the magnitude of IAT scores is stable across age (e.g., Baron & Banaji, 2006), we also found that age was not associated with stability of IAT scores over time. That is, correspondence of IAT scores across test and retest did not vary as a function of age. In both cases, it is important to point out that these results do *not* imply that children of different ages performed equivalently while completing the IAT. Indeed, we found that older children in our studies tended to have faster and less variable trial latencies, as well as fewer errors. However, a virtue of the IAT scoring algorithm is that the latency difference *between* critical blocks for each participant is adjusted by their variability of responses *within* the critical blocks. Thus, while younger children tend to have longer response latencies (relative to older children), these differences are "cancelled out" by increased variability in response latencies. In total, the IAT scoring algorithm adjusts for individual difference factors – such as age, task switching, working memory - that correlate with response-speed (Greenwald et al., 2003), which likely explains why participant age had negligible effects in our IAT analyses.

Conclusion

The IAT is increasingly used in developmental research. Overall, the present work illustrates that the IAT indexes individual differences in implicit cognition that are consistent across time and that correlate with behavior (with some exceptions). While we found that IAT scores generally indexed stable traits in young children, and were even reliable of time periods up to 1-year, evidence of predictive validity was more variable across the domains we tested. Thus, we look forward to future work that will continue to understand when and where the IAT will be most or least useful with child populations moving forward.

Chapter 3

Exposure to Racial Out-Groups and Implicit Race Bias in the United States

Rae, J.R., Newheiser, A., & Olson, K.R. (2013). *Social Psychological and Personality Science*.

The industrialized world is becoming more ethnically diverse. Research in several disciplines has suggested that exposure to racial outgroups may be associated with more positive *and* more negative intergroup attitudes. Given that U.S. states are often at the center of debate regarding diversity-related public policy, we examined how exposure to outgroups is associated with state-level implicit and explicit race bias among White and Black Americans. We found that larger proportions of Black residents across U.S. states were associated with stronger implicit and explicit ingroup bias among both White and Black respondents. State-level bias was predicted by proportions of Black residents even when controlling for (a) state-level demographic control variables (e.g., median income); (b) proportions of non-Black minorities; and (c) historical membership in the Confederacy. Our results convey the importance of investigating why diversity may not always have the positive impact on intergroup relations that one might hope it to have.

Exposure to Racial Outgroups and Implicit Race Bias in the United States

Ethnic diversity is on the rise in virtually all industrialized countries (Coleman, 2006), and researchers are increasingly interested in understanding the consequences and correlates of exposure to members of racial outgroups. Greater diversity is associated with benefits such as improved group problem solving (Page, 2007) and enhanced educational outcomes (Chang, 1999). However, diversity may also have *downsides*, including decreased trust and increased social isolation (Putnam, 2007). Within psychology, research on the relationship between diversity and explicit intergroup bias has similarly reached opposing conclusions, suggesting that exposure to racial outgroups may be associated with either more positive or more negative intergroup relations (e.g., Sigelman & Welch, 1993; cf. Bobo, 1988). In the present research, we investigated whether exposure to racial outgroups is positively or negatively associated with intergroup bias measured at the *implicit* (i.e., more indirect or automatic; Fazio & Olson, 2003) and explicit (i.e., more direct and controlled) levels.

Consistent with theories arguing that exposure to outgroups heightens perceptions of threat and competition (LeVine & Campbell, 1972; Riek, Mania, & Gaertner, 2006; Stephan & Stephan, 2000), greater exposure to racial minorities is often associated with *increased* explicit bias. For example, among White respondents sampled from comparable cities in the U.S. North and South, those in the South expressed stronger explicit anti-Black bias; and within the South, anti-Black bias was greater in regions with more African-American residents (Pettigrew, 1959). Similarly, larger proportions of African-American residents within communities have also been linked to lower support for racial integration and greater perceived threat among White residents (Fossett & Kiecolt, 1989; Taylor, 1998; see also Ayers, Hofstetter, Schnakenberg, & Kolody, 2009, for similar effects with exposure to Latino residents).

However, other work argues that exposure to outgroups *reduces* threat and intergroup bias (Allport, 1954; Pettigrew & Tropp, 2006). This research has found that greater exposure to outgroups is associated with *lower* explicit bias among majority-group members. For instance, relative to living in East Germany, residing in West Germany (that has more non-German residents) predicted lower explicit prejudice among native Germans (Wagner, Van Dick, Pettigrew, & Christ, 2003). Moreover, as the proportion of non-German residents within German districts increased, negative explicit evaluations of non-Germans among native Germans decreased (Wagner, Christ, Pettigrew, Stellmacher, & Wolf, 2006). Similarly, greater community-level ethnic diversity in the United Kingdom was associated with perceptions that ethnic diversity was respected and groups of different backgrounds interacted positively (Lawrence, 2009).

Thus, exposure to racial outgroups can predict either *greater* or *lower* intergroup bias measured at the explicit level. However, given that implicit measures are increasingly relied upon in intergroup research (Fazio & Olson, 2003) and may be more strongly influenced (relative to explicit measures) by one's cultural environment (e.g., observing local status differences; Rudman, 2004; see also Newheiser & Olson, in press), in the present work we primarily considered the relationship between exposure to racial outgroups and *implicit* race bias. To date, most evidence suggests that exposure to racial outgroups may be an effective method of reducing implicit bias (Pearson, Dovidio, & Gaertner, 2009), such that outgroup exposure and the quantity of intergroup interaction predict more positive implicit outgroup attitudes (Prestwich, Kenworthy, Wilson, & Kwan-Tat, 2008; Turner, Hewstone, & Voci, 2007). However, given that implicit bias is sensitive to environmental associations and societal evaluations of social groups (Karpinski & Hilton, 2001; Rudman, 2004), the direction of the relationship between exposure to outgroups and implicit bias may depend on whether the exposure is primarily positive or negative. Indeed, while positive (vs. negative) exposure to outgroup members may be more prevalent, negative exposure may have a more potent influence on intergroup processes (Graf, Paolini, & Rubin, in press). Thus, we explored whether exposure to outgroups is positively or negatively associated with implicit bias at the level of U.S. states.

We examined implicit bias as indexed by the most commonly used implicit attitude measure, the Implicit Association Test (IAT; Greenwald, McGhee, & Schwartz, 1998). Specifically, we analyzed *state-level* implicit race bias using data aggregated from over 890,000 race IATs completed via Project Implicit (see Xu, Nosek, & Greenwald, 2014, for information about this publicly available dataset). For comparison, we also analyzed aggregated explicit bias, available from the same database. Because few studies have investigated the relationship between exposure to outgroups and minority-group members' attitudes (see Shelton, 2000), we examined implicit and explicit bias among both Black and White respondents.[20] We focused on the state level because states are often at the center of debate about diversity-related policy (e.g., immigration; Archibold, 2010), and investigated whether the proportion of Black residents within a U.S. state predicted aggregated intergroup bias among residents of the state (see Christ et al., 2014, for a similar strategy). Understanding how proportions of minority-group and majority-group residents relate to group-level attitudes is helpful in contextualizing the environment within which diversity-related policy and politics occur. Prior work has employed a similar approach in the domains of gender disparities in math and science performance (Nosek et al., 2009) and weight bias (Marini et al., 2013).

[20] We restricted the scope of our investigation to White and Black respondents because these are the focal groups for the race IAT on the Project Implicit website.

Method

Participants

Volunteers completed the race IAT via Project Implicit (https://implicit.harvard.edu/implicit/). To match the available population estimates we employed as predictor variables (see below) while maximizing sample size, we considered data collected from 2008 to 2012 (the full dataset contains IAT scores from 2002 to 2012). [21]Because geographic location was a primary predictor, the data set (>3.6 million observations) was restricted to participants within the U.S. with complete IAT data (*N*=1,104,428). The sample was further restricted to participants with adequate IAT performance (using conventions established by Greenwald, Nosek, & Banaji, 2003, and Nosek et al., 2009): Data from participants who made errors on >30% of trials or had reaction times <300 milliseconds on >10% of trials were excluded (approximately 2% of the sample). Finally, excluding respondents who did not self-identify as Black or White yielded a total of *N*=893,387 observations. For ease of reporting and because of the large sample size contributing to our state-based analyses, we performed separate analyses on subsamples of White (*n*=759,755) and Black (*n*=133,632) respondents.

As indicated by demographic information provided on the site, our White and Black samples were highly educated, typically visited the Project Implicit website to fulfill an educational requirement (~40% of participants), and were similar to each other with regard to education level, religiosity, and political orientation (see Appendix B for details on sample demographics). The actual population of a state was strongly correlated with the number of people who completed the IAT in that state, $r(48)=.91$, $p<.001$ (also true at the county level, mean $r=.85$, $SD=.21$).

[21] Using the full dataset yields the same pattern of results; details are available from the first author.

Measures

Implicit Bias. We analyzed *D* scores (Greenwald et al., 2003) that were coded to represent ingroup bias: For White respondents, scores above 0 indicated a bias favoring Whites over Blacks and scores below 0 indicated a bias favoring Blacks over Whites; for Black respondents, scores above 0 indicated a bias favoring Blacks and scores below 0 indicated a bias favoring Whites.

Explicit Bias. Participants also responded to an item assessing explicit racial attitudes (Nosek et al., 2007). Specifically, participants indicated their relative preference for White and Black Americans on a 7-point scale (1=*I strongly prefer African Americans to European Americans*; 4=*I like European Americans and African Americans equally*; 7=*I strongly prefer European Americans to African Americans*). Scores were recoded to indicate ingroup preference and centered at 0 so that possible values ranged from -3 to +3.

Ratio of Black to White Residents within States. Estimates of the proportion of residents from different racial/ethnic backgrounds within states were obtained via the American Community Survey (ACS), conducted by the U.S. Census Bureau. We used the ACS 5-year averages of data collected between 2008 and 2012. Because the race IAT measures implicit bias favoring Whites over Blacks (or vice versa), we created a ratio of the number of Black to White residents in each state (with higher numbers indicating more Black residents) to measure intergroup exposure across U.S. states. Similar to prior work using this technique (Alba, Rumbaut, & Marotz, 2005), values were natural-log transformed because this ratio was small in states with few Black residents.[22] However, for ease of interpretation, in descriptive statistics and figures the index refers to the untransformed number of Black residents for every 100 White residents, which ranged from 0.47 to 62.3 (median=9.28).

[22] Using the untransformed values yielded the same pattern. These results are available from the first author.

The ratio of Black to White residents may seem incomplete as an index of exposure to racial outgroups insofar as it ignores the possibility that apparently diverse locales may be divided into homogeneous subregions (Holloway, Wright, & Ellis, 2012). Therefore, to validate this index, we investigated its relationship with racial/ethnic *segregation*. Specifically, we considered the interaction index, which represents the chances of a Black resident encountering a White resident (or vice versa) in a given spatial unit (Massey & Denton, 1988). Interaction index scores range from 0 to 1, with scores near 0 indicating that Black and White residents in an area are unlikely to meet (high segregation, low diversity) while scores near 1 indicate that encounters between individuals from different racial groups are likely (low segregation, high diversity). Because segregation is typically measured at the level of relatively small geographical units (e.g., the U.S. Census tract level; e.g., Iceland & Scopilliti, 2008), we correlated the interaction index for White and Black residents and the ratio Black to White residents at the level of U.S. *counties* (see below for more information about our county-level analyses), again using ACS 5-year estimates. The correlation was strong: White residents, $r(813)=.97$, $p<.001$; Black residents, $r(813)=-.71$, $p<.001$. These results validate the ratio of Black to White residents as an index of outgroup exposure.

Results

We estimated state-level implicit and explicit bias by averaging IAT and explicit bias scores within each state, separately for White and Black respondents (see Nosek et al., 2009). We then investigated whether the ratio of Black to White residents within states predicted state-level implicit and explicit bias among White and Black respondents.[23] Given recent concerns about covariates (Simmons, Nelson, & Simonsohn, 2011), in the main text we report results

[23] Although we used ordinary least-squares estimation in all analyses reported below, we considered several analytic methods, including weighted least-squares estimation; all methods yielded the same pattern. All results are available from the first author.

without any adjustment variables; however, in Appendix B we report analyses including covariates used in previous research (Putnam, 2007; Taylor, 1998). In *all* cases, the association between both implicit and explicit bias and outgroup exposure was unchanged in the presence of covariates. All data-analytic decisions, including exclusion criteria, selection of control variables, and our use of both state-level and county-level data, were made *a priori* on conceptual grounds before any data were analyzed.

State-Level Implicit Bias

Validating the State-Level Approach. We sought to validate the state-level IAT scores by correlating them with another state-level measure of racial bias: percentage of racially charged Google search queries. Specifically, we correlated the average state-level IAT scores for White and Black respondents with the relative percentage of Google search queries of the pejorative word "nigger" or its plural for each state between 2004 and 2007 (i.e., the state with the highest percentage of racially charged search queries was assigned a score of 100; for details, see Stephens-Davidowitz, 2012). The correlation was positive and strong: White respondents, $r(48)=.78$, $p<.001$; Black respondents, $r(48)=.50$, $p<.001$, lending validity to the aggregated IAT scores.

Primary Analyses. On the national level (i.e., collapsing across states), the mean IAT score among Black respondents was 0.04 (SD=0.44); among White respondents, it was 0.41 (SD=0.40), mirroring patterns observed in laboratory studies (e.g., Ashburn-Nardo, 2010). For the Black sample, the number of IAT scores per state ranged from 36 to 9,461, with aggregated IAT scores ranging from -0.10 to 0.13 (SD=0.04). For the White sample, the number of IAT scores per state ranged from 894 to 56,800, with aggregated IAT scores ranging from 0.35 to 0.45 (SD=0.03). White and Black respondents' state-level IAT scores were positively correlated,

Figure 4. White respondents' state-level IAT scores (left pane) and explicit bias scores (right pane) as predicted by the natural log of the ratio of Black to White residents within each state, with a fitted line from a simple linear regression model. Each data point represents a U.S. state.

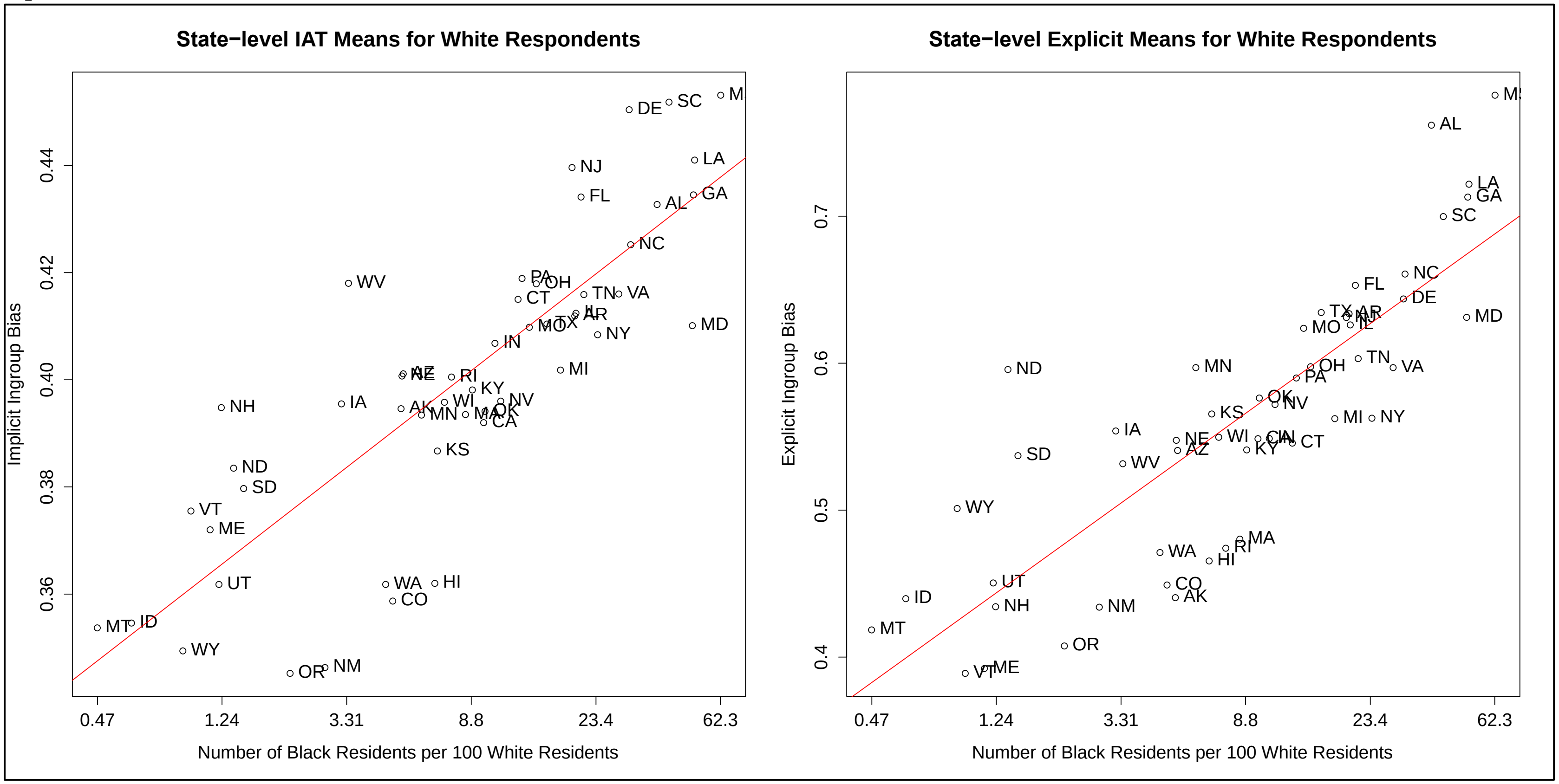
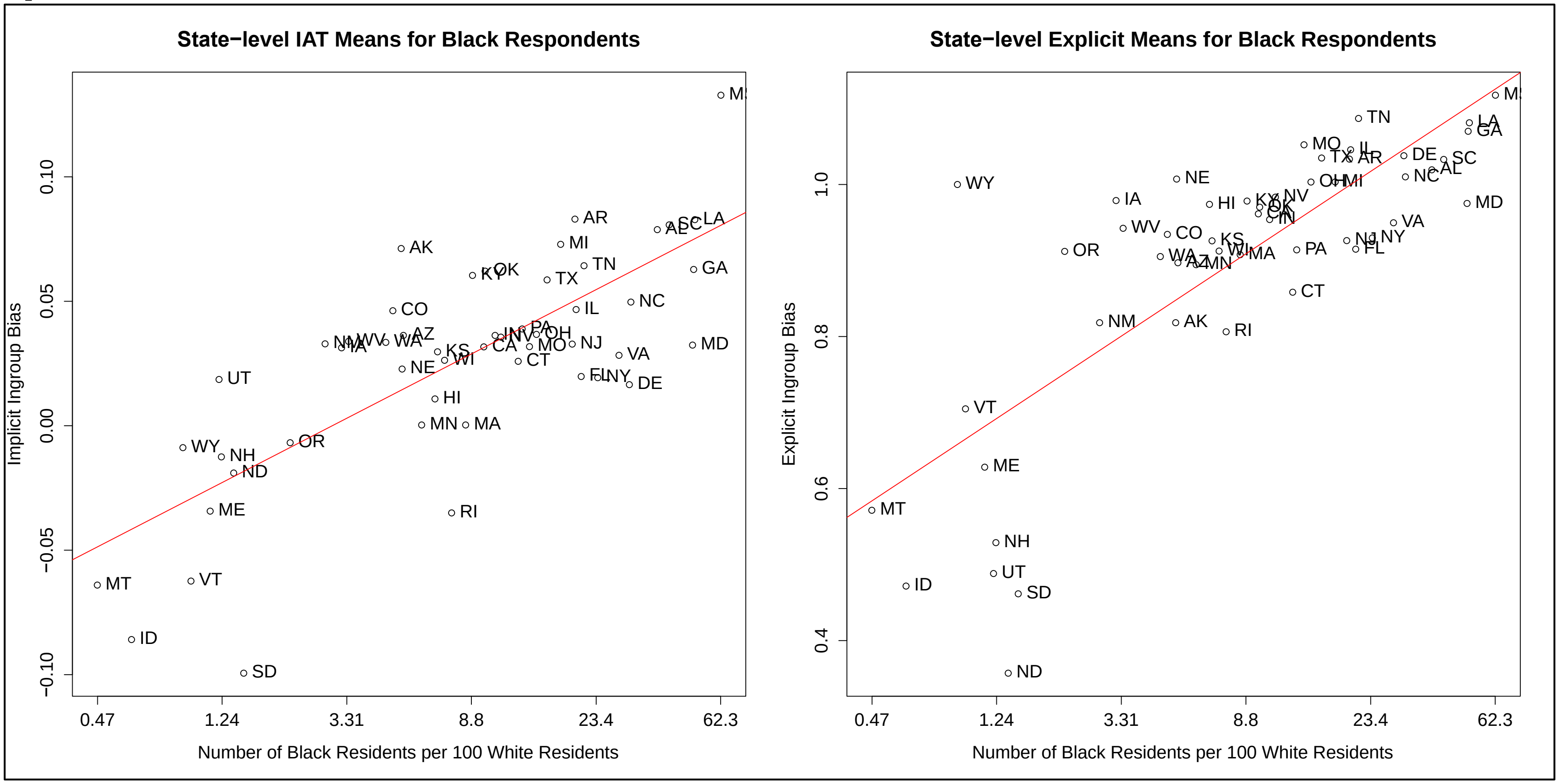


Figure 5. Black respondents’ state-level IAT scores (left pane) and explicit bias scores (right pane) as predicted by the natural log of the ratio of Black to White residents within each state, with a fitted line from a simple linear regression model. Each data point represents a U.S. state.



r(48)=.59, *p*<.001, such that states with stronger implicit ingroup bias among White respondents also had stronger implicit ingroup bias among Black respondents.

Central to testing the relationship between exposure to racial outgroups and implicit bias, the ratio of Black to White residents within a state (with higher numbers indicating more Black residents) was strongly positively correlated with implicit bias among both White respondents, *r*(48)=.83, *p*<.001 (Figure 4, left pane), and Black respondents, *r*(48)=.76, *p*<.001 (Figure 5, left pane). Thus, as the ratio of Black to White residents increased, state-level implicit ingroup bias among both White and Black respondents increased.

Replicating and Extending the State-Level Analyses

Having found evidence of a positive relationship between exposure to racial outgroups and implicit ingroup bias, we next sought to replicate this pattern of results across different units of analysis and using additional control variables. Our aim was to document that the relationship between outgroup exposure and implicit bias was not spurious or reducible to other variables. The details of these analyses are reported in Appendix B.

County-Level Analysis. A potential limitation to the results reported above is our focus on large regions—U.S. states—which could appear more or less diverse simply because of their inclusion of urban centers with high concentrations of Black residents and rural areas with high concentrations of White residents. Accordingly, we also analyzed the data by U.S. *counties* (which represent the smallest geographical unit available in the public-use IAT dataset, with an average population of approximately 100,000 residents; Xu et al., 2014). We only considered counties with at least 10 observations from White and Black respondents, yielding a sample of 815 counties (mostly located in states high in racial diversity). Although these counties do not represent a sample generalizable to the entire U.S., our county-level findings mirrored our state-level findings: The ratio of Black to White residents in a county positively predicted county-level IAT scores in both the White sample, β=.33, p<.001, and the Black sample, β=.38, p<.001.

Overall Racial/Ethnic Diversity. Because the IAT available in the public-use dataset (Xu et al., 2014) measures implicit bias favoring Whites over Blacks (or vice versa), our primary predictor was the ratio of these two groups. However, it is possible that *overall* racial/ethnic diversity, rather than the ratio of Black to White residents specifically, predicts implicit bias. We investigated this possibility by using the ratio of non-Black racial/ethnic minority residents (e.g., Asian, Hispanic, and multiracial residents) to White residents as an additional predictor. For White respondents, state-level IAT scores were not significantly associated with the ratio of non-Black racial/ethnic minority residents to White residents at the bivariate level, r(48)=-.24, p=.100. When considered in the full model (together with the ratio of Black to White residents and all control variables), this index predicted lower state-level implicit bias, β=-.36, p=.005. Importantly, however, the predictive strength of the ratio of Black to White residents was substantially higher, β=.77, p<.001. The ratio of non-Black racial/ethnic minority residents did not predict implicit bias among White residents at the county level. For Black respondents, the ratio of non-Black racial/ethnic minority residents to White residents was not associated with state-level implicit bias in either bivariate or multivariate analyses. The same pattern emerged at the county level.

Southern versus Northern States. Observing that most studies that have revealed a negative relationship between diversity and interracial relations in the U.S. have surveyed intergroup attitudes in the South, Wagner et al. (2006) hypothesized that unique features of the South may explain this relationship. Our implicit bias results do not align with this hypothesis: The ratio of Black to White residents was positively associated with stronger implicit ingroup bias among White and Black respondents across *all* U.S. states. In addition, even after excluding the 11 former Confederate states from the analysis, the ratio of Black to White residents remained a positive predictor of implicit bias among both Black, $\beta=.68$, $p<.001$, and White respondents, $\beta=.74$, $p<.001$. However, we further tested the possibility that lingering historical biases particular to, or especially pronounced in, Southern states could explain our findings. We included a binary variable indicating whether a state was a member of the Confederate States of America to the saturated state-level model that included both Black and non-Black ethnic/racial diversity and all covariates (see Appendix B). In a subsequent model, we added the interaction between this binary variable and the ratio of Black to White residents. Neither the main effect nor the interaction predicted implicit bias for either sample. A similar pattern emerged at the county level.[24]

State-Level Explicit Bias

The ratio of Black to White residents predicted a similar pattern of results for explicit state-level bias. This ratio correlated strongly and positively with explicit ingroup bias among both White respondents, $r(48)=.83$, $p<.001$, and Black respondents, $r(48)=.78$, $p<.001$ (see Appendix B for detail). However, explicit bias did differ from implicit bias in that state-level explicit bias among Black respondents ($M=0.90$, $SD=0.18$) was *higher* than that reported by White respondents ($M=0.56$, $SD=0.10$), $t(49)=16.25$, $p<.001$ (see right panes in Figures 4 and 5). This pattern aligns with prior lab results in which White participants typically express stronger implicit ingroup bias than Black participants but Black participants often express stronger explicit ingroup bias than White participants (e.g., Ashburn-Nardo, 2010; Livingston, 2002).

[24] Approximately 50% of the 815 counties in our analysis were located in the 11 former Confederate states. There was a main effect of Confederate state on implicit bias at the county level for White respondents and a marginally significant interaction for Confederate state on implicit bias at the county level for Black respondents. See Appendix B for detail.

Discussion

We aggregated 890,000 observations collected via Project Implicit to investigate the relationship between exposure to members of racial outgroups and group-level implicit and explicit race bias across U.S. states and counties. The pattern of results for implicit and explicit bias was the same: Higher proportions of Black residents in a state or county predicted stronger ingroup bias among both White *and* Black respondents from that state or county. Thus, states like Mississippi and South Carolina that have a higher proportion of Black residents tended to have stronger aggregated ingroup bias, relative to states like Montana and Vermont that have few Black residents.

While the differences in state-level IAT scores were modest, with aggregated *D* scores ranging from 0.35 to 0.45 among White respondents and from -0.10 to 0.13 among Black respondents, even small IAT effects can be associated with large and very consequential societal effects (e.g., group-based discrimination in a variety of domains; Greenwald, Banaji, & Nosek, in press). Furthermore, the degree of ingroup bias separating Black and White respondents *within* states was large. For example, the difference in implicit ingroup bias for White respondents (D=0.45) and Black respondents (D=0.13) from Mississippi was 0.58 (considered a large effect; the smallest difference of implicit bias scores within states was 0.24). Moreover, ingroup bias aggregated at the state level revealed a pattern consistent with previous reports that have found that implicit ingroup bias is stronger among White (vs. Black) respondents, while explicit ingroup bias is stronger among Black (vs. White) respondents (e.g., Ashburn-Nardo, 2010; Livingston, 2002).

Despite the clear pattern in our primary analysis, it is unclear *why* greater proportions of Black residents are associated with higher ingroup bias. One possibility was that our findings were not specific to the presence of Black residents, and ingroup bias was rather driven by racial/ethnic diversity more generally. For example, states and counties with high proportions of racial/ethnic minority groups may *also* have higher proportions of Black residents, which in turn increases overall bias against all racial/ethnic outgroup members among both Black and White respondents. In this case, a third variable (overall proportions of racial/ethnic minority residents) would be driving our pattern of results. If this were the case, controlling for the proportions of non-Black racial/ethnic minority residents should diminish, or even eliminate, the relationship between ingroup bias and the proportion of Black residents. However, we found that whereas the proportion of non-Black racial/ethnic minority residents did predict implicit bias among White respondents when controlling for state-level covariates (though not at the bivariate level), this variable predicted *lower* levels of implicit bias and had no effect on the relationship between implicit bias and proportions of Black residents. In addition, greater proportions of non-Black minority residents did not predict bias among Black respondents. This pattern indicates that our results are uniquely tied to the relative proportions of Black and White residents specifically.

Another possible explanation for our results was that historical or structural contexts unique to or more pronounced in the U.S. South could explain the pattern (see Wagner et al., 2003). For example, lingering effects of the slavery era may not only have contributed to high proportions of Black residents in the South relative to other parts of the U.S., but also created higher levels of historical tension between White and Black residents in the region. Therefore, this historical confound could have driven our results by producing a spurious correlation between proportions of Black residents and ingroup bias. However, we found that historical membership in the Confederacy did not moderate the relationship between proportions of Black residents and implicit bias. Further supporting our interpretation, the proportion of Black residents remained a positive predictor of state-level implicit bias even when we excluded the 11 former Confederate states from the analysis. Interestingly, *explicit* ingroup bias was stronger among White respondents from Southern states and counties, perhaps indicating differences in perceived expression norms (e.g., Crandall, Eshleman, & O'Brien, 2002).

One concern that could be raised is that states are too large of a unit of measurement to sufficiently index outgroup exposure. Exposure computed at the state level averages across urban centers with higher proportions of Black residents and rural areas with higher proportions of White residents, potentially failing to represent respondents' immediate environments. Therefore, it may be that the association between exposure to racial outgroups and ingroup bias is only found at the level of U.S. states and does not hold at more fine-grained levels of analysis. However, we found the same pattern of results even at the smaller unit of counties.

Another concern may be that there is an association between the proportion of Black residents and state-level aggregated IAT scores, but that these measures are not optimal indexes of exposure to racial outgroups and group-level implicit bias, respectively (e.g., see Tetlock & Mitchell, 2009, for a criticism of the IAT as a measure of "implicit bias"). This is an important issue, and future research might seek to replicate our pattern of results using alternative measures. While prior research has utilized both the ratio of Black to White residents (Alba et al., 2005) and aggregated IAT scores (within the domains of gender-science stereotypes and weight bias; Marini et al., 2013; Nosek et al., 2009), we attempted to further validate the measures used in the present study. We found that the ratio of Black to White residents in a locality was strongly correlated with the interaction index, a measure designed to assess the likelihood that county residents from different racial groups actually encounter each other, providing some validity for our exposure measure. We also report evidence that lends validity to our aggregated state-level IAT scores: These scores were strongly correlated with the percentage of racially charged Google search queries performed in each state. Given that users feel relatively anonymous online, racially charged Google search queries likely index undisclosed attitudes toward race.

Whereas we ruled out some possible reasons for the relationship between outgroup exposure and ingroup bias, we did not demonstrate *why* this relationship exists. Future research will benefit from testing potential explanations, such as the ways in which *group status* may interact with outgroup exposure in shaping implicit bias. Implicit bias is sensitive to environmental cues, such as societal evaluations of social groups (Newheiser & Olson, in press; Rudman, 2004), which likely explains the overall pattern of higher implicit ingroup bias among White (higher-status group) relative to Black (lower-status group) respondents. Critically, outgroup exposure may reinforce or undermine intergroup status differentials, which may explain differences in implicit ingroup bias *within* groups and the association between outgroup exposure and implicit bias. For example, Black respondents living in areas with low proportions of Black residents experience relatively high exposure to White residents, and as a result may notice status differentials more. As the proportion of Black residents increases, Black respondents have relatively less exposure to White residents (perhaps further exacerbated by ongoing racial segregation; Holloway, Wright, & Ellis, 2012), likely making low ingroup status less salient. In contrast, White respondents living in areas with few Black residents have relatively few encounters with the low-status group, and therefore their high ingroup status may not be chronically salient. But for White respondents living in areas with high proportions of Black residents, high ingroup status may indeed be chronically salient, and may bolster ingroup bias. This reasoning would predict the pattern of results we observed in our primary analysis, in which states and counties with higher proportions of Black residents showed stronger ingroup bias among both White and Black respondents.

Although our results contribute to a more comprehensive understanding of the relationship between exposure to outgroups and intergroup bias, we acknowledge important limitations. First, our data are correlational and causal relationships cannot be inferred. Second, participants self-selected into our sample by completing the race IAT via the Project Implicit website. Our sample was highly educated and the most common reason why participants visited Project Implicit was to fulfill an educational requirement (approximately 40% of our sample). While between-group differences do not appear to be driving our results (see Appendix B), our sample is not representative of the U.S. more broadly. Lastly, while there were clear reasons why we focused on aggregated data, this necessarily meant that we averaged out meaningful individual-level variation that can and should be the focus of additional research.

In conclusion, aligning with findings from political science (Putnam, 2007) and sociology (Quillen, 1995), we found that greater proportions of Black, relative to White, residents in U.S. states and counties predicted stronger ingroup bias among both White and Black Americans. Although we attempted to isolate the relationship between outgroup exposure and race bias (e.g., by using control variables and replicating the pattern across units of analysis), it remains unclear exactly why this pattern emerged. Our results underscore the importance of further investigation into why exposure to racial outgroups—a feature that we strive to increase in our schools, workplaces, and communities—may not always have the beneficial impact it is hoped to deliver.

Chapter 4

Contact and Exposure to Racial Ingroups and Outgroups Independently Predict Racial Bias

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In independent lines of research, contact and exposure to racial outgroups has been associated with ingroup bias. In a large sample of White and Black U.S. residents (*N* ~ 95,000), this research simultaneously tested the effects of contact and exposure to both racial outgroup members and racial ingroup members on racial bias. We found that respondents that had higher contact with Whites or lived in counties with higher exposure to Blacks tended to have stronger pro-White/anti-Black bias, whereas those with higher contact with Blacks tended to have weaker pro-White/anti-Black bias. These results suggest the importance of examining the role that contact and exposure to both the ingroup and outgroup may have in shaping intergroup relations.

Contact and Exposure to Racial Ingroups and Outgroups Independently Predict Racial Bias

Racial and ethnic diversity is increasing in the U.S. By 2043 the U.S. is projected to become a "majority-minority nation," such that non-Hispanic Whites will comprise less than 50% of the population (U.S. Census Bureau, 2012). In light of this increasing diversity, it is important to consider how these population changes might impact racial bias, and ultimately intergroup relations. To this end, previous intergroup relations research has investigated how increased contact and exposure to racial outgroups (e.g., living in the same area as outgroup members) – two likely outcomes of increasing racial and ethnic diversity – are associated with racial bias. While the effects of exposure and contact to *outgroup* members have typically been studied in isolation (Christ & Wagner, 2012; Pettigrew, 1998; Wilder & Thompson, 1980), the current research investigates how contact and exposure to both *ingroup* members and *outgroup* members might act in concert to predict explicit (e.g., self-report) and implicit (e.g., automatic or unconscious) racial bias.

Contact Theory and Group Threat Theory

Contact theory contends that contact between ingroup and outgroup members can be effective in reducing bias (Allport, 1954), and that the bias reducing effects of intergroup contact occur via numerous mechanisms (e.g., learning about the outgroup, reducing anxiety in intergroup interactions; for reviews, see Dovidio, Gaertner, & Kawakami, 2003; Pettigrew, 1998). In the past half century, this hypothesis has been the focus of extensive research (Brown & Hewstone, 2005), and a meta-analysis of this work (with over 700 independent samples) testing the effects of contact with numerous outgroups (e.g., age, ability, race) revealed a significant inverse relationship between intergroup contact and ingroup bias (mean $r = -.21$; Pettigrew & Tropp, 2006). Thus, insofar that increasing racial diversity also increases contact with members of racial outgroups (Stein et al., 2000; Wagner et al., 2003, 2006), increasing racial diversity should be associated with *lower* levels of racial bias.

Group threat theory contends that intergroup bias is precipitated by (actual or perceived) group-competition for resources, status, and power (e.g., Blalock, 1967; Esses, Jackson, & Armstrong, 1998; Key, 1949). For majority group members, threat of group-competition is linked to the size of the minority group, such that bias among majority group members increases as the relative size of the minority group increases (e.g., Blalock, 1967; Quillian, 1996). Research from disciplines such as political science and sociology has provided substantial support for this prediction, such that increased exposure to outgroup members has been found to be positively associated with ingroup bias (e.g., Ayers, Hofstetter, Schnakenberg, & Kolody, 2009, Fossett & Kiecolt, 1989; Giles, 1977; Giles & Buckner, 1993; Glaser, 1994; Quillian, 1995; 1996; Taylor, 1998; but for examples in which outgroup exposure has been associated with lower levels of ingroup bias, see Wagner, Christ, Pettigrew, Stellmacher, & Wolf, 2006; Wagner Van Dick, Pettigrew, & Christ, 2003). Thus, to the extent that increasing racial diversity increases exposure to racial outgroups, it should be associated with *higher* levels of racial bias.

Limitations of Previous Research

Despite the strong evidence that has accumulated for both contact theory and group threat theory, there are several limitations to this previous work. We discuss these in turn.

Testing Contact and Exposure in Isolation. Though much research has demonstrated that intergroup contact and outgroup exposure are associated with ingroup bias when considered in isolation (Christ & Wagner, 2012), little research has tested the simultaneous effects of these variables. However, insofar that intergroup contact and outgroup exposure covary, examining the effects of intergroup contact in the absence of outgroup exposure (or vice versa) may provide a biased estimate of the strength of association between intergroup contact and ingroup bias (a scenario referred to as omitted variable bias; Bareto & Howland, 2006). Indeed, omitted variable bias is a concern when an omitted predictor is associated with both the outcome variable and a predictor variable included in a regression model. Thus, as intergroup contact and outgroup exposure have both been found to predict ingroup bias (as argued in the previously reviewed research) and positively covary (e.g., Ayers et al., 2009)[25], assessing the effects intergroup contact and outgroup exposure in tandem may provide an opportunity to more accurately gauge of how each variable relates to ingroup bias.

Reliance on Explicit Measures of Ingroup Bias. A second limitation of previous research testing the effects of contact and exposure is that this work has typically relied upon *explicit* measures of ingroup bias, thus, there is little evidence that contact or exposure is associated ingroup bias on *implicit* measures. Explicit racial bias measures index conscious biases that respondents are willing to disclose, whereas implicit measures rely on indirect assessment techniques to infer biases that respondents may be unable or unwilling to self-report (De Houwer, Teige-Mocigemba, Spruyt, & Moors, 2009). For example, the most commonly used measure of implicit racial attitudes, the Implicit Association Test (IAT; Greenwald, Schwartz, & McGhee, 1998), indexes attitudes by the speed with which participants categorize pictures of members of two racial groups and good and bad words. While egalitarian norms often prevent the expression of bias on explicit measures (Crandall, Eshleman, & O'Brien, 2002; Plant & Devine, 1998), the IAT is less susceptible to these social desirability concerns. Indeed, the IAT is especially useful in socially sensitive domains (e.g., stereotyping and prejudice; Blair, 2002; Greenwald et al., 1998), such that individual differences on the IAT are more predictive of

[25] In a study of immigration policy preferences, Ayers et al. (2009) found that residents living in Census tracts with higher proportions of Latinos also had higher intergroup contact with Latinos (r = .16).

intergroup behavior than parallel explicit measures (Greenwald, Poehlman, Uhlmann, & Banaji, 2009; cf. Oswald, Mitchell, Blanton, Jaccard, & Tetlock, 2013).

Though implicit bias may have a larger downstream influence on intergroup behavior, only a handful of studies have tested the association between intergroup contact and implicit bias. Moreover, these studies have yielded mixed findings such that some studies have shown that intergroup contact is associated with reduced implicit ingroup bias (e.g., Aberson & Haag, 2007; Aberson, Shoemaker, & Tomolillo, 2004), other studies have failed to find a significant association between intergroup contact and implicit bias (Jelenec & Steffens, 2002; Teachman & Brownell, 2001). Similarly, research investigating the effects of outgroup exposure has not investigated whether implicit ingroup bias may vary as a function of outgroup exposure. The one exception is Rae, Newheiser, and Olson (2015), who reported the effects of outgroup exposure on both implicit and explicit ingroup bias among White and Black Americans. In a large sample of over 850,000 participants, Rae et al. found that greater proportions of Black (relative to White) residents in U.S. states and counties were associated with higher levels of implicit and explicit ingroup bias among both White and Black respondents.

Measuring Exposure at Distal Units of Analysis. A third limitation of previous work testing the effects of outgroup exposure is that outgroup exposure has often been measured only at large contextual units of analysis (e.g., the proportions of outgroup members in a respondent's city, county, or state; e.g., Rae et al., 2015). Thus, it could be possible that self-reported exposure that indexes one's more proximal exposure to outgroups (e.g., outgroup members in one's school or neighborhood) is differentially associated with ingroup bias compared to exposure measured at more distal units of analysis (e.g., county or state). Consistent with this possibility, Oliver and Wong (2003) found that while living in a racially diverse neighborhood was associated more positive perceptions of racial outgroups, living in diverse metropolitan areas was associated with more negative perceptions of racial outgroups.

Focusing on (or Confounding) the Role of the Outgroup. Another limitation of previous work testing the effects of outgroup exposure is that this work has often focused on the role of outgroups (e.g., Taylor, 1998), or even confounded the effects of ingroup and outgroup exposure. Indeed, Rae et al. (2015) combined proportions of Black and White residents into a single predictor by creating a ratio of Black to White residents within states and counties. Thus, it is unclear whether results reported by Rae et al. were driven by proportions of White residents, Black residents, or both. Moreover, research testing the effects of intergroup contact has similarly focused on how contact with outgroup members may be effective in reducing ingroup bias, which has meant neglecting the potential role of ingroup contact. One notable exception came from Wilder and Thompson (1980), who demonstrated that contact with ingroup members (students of the college in which participants were enrolled) was associated with *increased* ingroup bias, but that contact with outgroup members (students enrolled in a nearby college) was associated with reduced ingroup bias. In another example, Levin and colleagues (2003) used a longitudinal design to test how college student's friendships with racial ingroup members and racial outgroup members in the second and third year of college predicted their racial attitudes at the conclusion of college. Their results indicated that friendships with members of racial outgroups were associated with reduced racial ingroup bias, but that friendships with racial ingroup members predicted increased racial ingroup bias.

Testing Effects among Majority Group Members. One final limitation is that majority group members have typically been the focus of research testing the effects of contact and exposure – and intergroup research more broadly (Shelton, 2000). For example, while there is evidence that the negative association between intergroup contact and *explicit* bias may be weaker for minority group members compared to majority group members (Tropp & Pettigrew, 2005), to our knowledge, no research has tested whether intergroup contact is associated with lower levels of *implicit* bias among minority group members. Moreover, there have been few studies testing the effects of exposure in minority group samples (but see Ha, 2010; McClain et al., 2006).

Current Work

The current research used a large sample of White and Black U.S. residents (*N* ~ 95,000) to test the simultaneous effects of contact and exposure with Whites and Blacks on racial bias (*N* ~ 95,000). Our approach had several key strengths: First, we were able to directly compare the simultaneous effects of contact versus exposure, while also being able to differentiate the effects of contact with/exposure to ingroup members versus contact with/exposure to outgroup members. Second, we measured respondent's exposure outgroups in more proximal settings (e.g., as classmates or neighbors) and at a more distal level (e.g., proportions of outgroup members in one's county of residence), which enabled us to compare the effects of exposure across different units of analysis. Third, we tested our effects using both explicit and implicit measures of racial bias, whereas previous research has almost exclusively relied on explicit measures. Fourth, we were able to test the generality of our findings by analyzing data from both White and Black respondents.

Method

Respondents

Respondents were volunteers that completed the Race Implicit Association Test (IAT; Greenwald, Schwartz, & McGhee, 1998) via the Project Implicit Demonstration site (https://implicit.harvard.edu/implicit/). We analyzed data from respondents that completed the IAT between February 2, 2015 (when items measuring contact and exposure to Whites and Blacks were introduced) and June 31, 2016. Only respondents who (a) racially identified as White or Black (the focal groups on the Race IAT), (b) resided in the U.S. and self-reported their state and county of residence[26], (c) fully completed the IAT procedure, (d) demonstrated adequate performance on the IAT (IAT exclusion criteria are described below on pg. 115), and (e) had complete data for items measuring either contact *or* exposure to Whites and Blacks (described below on pg. X) were included in our analyses. Using these exclusion criteria, data was available from 10,981 Black and 84,245 non-Hispanic White U.S. residents. Other than having no Black respondents residing in New Hampshire, respondents were drawn from all 50 states and the District of Columbia, and represented over 900 counties (950 counties for Black respondents and 2,460 counties for White respondents).

Respondents tended to be *female* (Whites: 61%; Blacks: 67%)[27], *young* (Whites: *M* = 29.12 years, *SD* = 13.59 years; Blacks: *M* = 27.86 years, *SD* = 12.30 years)[28], *educated* (on a percentage metric; Whites: *M* = 70.74%; *SD* = 21.61%; Blacks: *M* = 69.57%; *SD* = 21.53%)[29], and *liberal* (political ideology scale with 1 = "strongly conservative", 7= "strongly liberal"; Whites: *M* = 4.72; *SD* = 1.76; Blacks: *M* = 4.87; *SD* = 1.43). Further, White respondents reported an average yearly income of over $70,000 (*M*=7.72, *SD* = 5.96; scale with 1= "Up to $10,000 per year" and 21 = "More than $200,000 per year") and an average wealth of between

[26] The District of Columbia was treated as a state in our analyses.
[27] We coded respondents according to their gender identity. Thus, cisgender females and transgender females were grouped together, and cisgender males and transgender males were grouped together.
[28] We screened out implausible values by computing descriptive statistics only for respondents with reported ages of less than 120. We retained these respondents in subsequent analyses.
[29] As the scale of education variable was numeric and differed for subsets of the sample, we put scores on a percentage metric using the procedure described by Cohen, Cohen, Aiken, & West (1999). In this coding scheme, a value of 50% indicates the participant selected the midpoint of the available response options (a response for someone in their last year of high school or with some college education) whereas a value of 100% indicates they selected the highest response option (either holds or is pursuing an advanced degree).

\$30,000 and \$40,000 (*M* = 5.97, *SD* = 3.27; defined as assets minus debts with 1= "\$0 to \$5,000" and 14 = "More than \$50 Million"). Black respondents reported an average yearly income of over \$50,000 (*M* = 5.75, *SD* = 5.11) and an average wealth of between \$20,000 and \$30,000 (*M* = 4.67, *SD* = 3.09).[30]

Individual-Level Variables

Implicit Association Test. The Race IAT is a computerized task that assesses associations between Whites and Blacks (target categories) and valence (attribute categories). In two initial practice blocks, respondents used two computer keys to practice classifying target (photographs of Blacks and Whites) and attribute (good and bad words) items. Across two critical blocks, these tasks were combined such that respondents classified items from one target-attribute pairing with one response key (e.g., Black + good), and classified items from the alternative target-attribute pairing with the second response key (e.g., White + bad). After practice reversing the key assignments for attribute categories, respondents completed two additional critical blocks with the alternative target-attribute pairings (e.g., Black + bad; White + good) from the initial critical blocks. The target-attribute pairings within critical blocks were completed in a counterbalanced order across respondents.

A summary measure for the IAT was computed using the *D*-algorithm (Greenwald et al., 2003), which eliminates response latencies longer than 10,000-ms and respondents that complete more than 10% of trials in less than 300-ms. The resulting index (a *D*-score) can take on values between +2 and -2, and in the current work, was scored such that higher values indicate stronger implicit pro-White/anti-Black bias.

[30] Respondents indicated how they were supported financially. If they indicated they were self-supported, the income and wealth values reported here refer to their person income and wealth. If they selected any other means of support, the income and wealth values are those of the person most responsible for their support.

Explicit Bias. Explicit racial bias was assessed with a composite of three self-report items. Specifically, we took the average of (a) standardized scores from a single item that assesses liking for Whites relative to Blacks on a 7-point scale (−3 =“I strongly prefer Blacks to Whites”; 0=“I like Blacks and Whites equally”; 3=“I strongly prefer Whites to Blacks”)[31] and (b) the difference score between an items measuring warmth towards Whites and Blacks (response options for both groups: 0 = “coldest feelings”; 5 = “neutral”; 10 = “warmest feelings”).

Contact with Whites and Blacks. Contact with Whites and Blacks was assessed with eight items. Specifically, respondents indicated if they have/had Black or White *family members* (3 items; “I have a parent who is…”, “I have a close family member of my own generation who is…”, “I have a close family member younger than my generation who is…”), *friends* (2 items; “I had a strong childhood friendship with a person who is…”, “I have had (or now have) a strong post-childhood friendship with a person who is…”), *romantic relationships* (1 item; “I have had (or now have) a romantic relationship with a person who is…”), or *admired teachers* (2 items; “I had a highly admired preschool or elementary school teacher who is…”, “I had a highly admired middle or high school teacher who is…”). We created a count variable using these items. For example, if a respondent indicated they had a White parent, this was considered a White contact experience and coded as a “+1”. We similarly coded the remaining seven items and created a single index by summing the number of White contact experiences. Thus, White contact scores ranged from 0 to 8 with higher scores indicating more contact with Whites. We used the same coding scheme to index contact with Blacks.

Exposure to Whites and Blacks. Exposure to Whites and Blacks was assessed in two ways. First, as a more proximal index of exposure (hereafter referred to as “community-level

[31] The raw item was originally scored with all positive values (1=“I strongly prefer Blacks to Whites”; 4=“I like Blacks and Whites equally”; 7=“I strongly prefer Whites to Blacks”). We subtracted four from each score so that the mid-point of the scale was centered at zero.

exposure”), we used six self-report items. Using four relevant response options of “minority White”, “majority White”, “minority Black” and “majority Black”, respondents indicated the racial composition of their *neighborhood* (2 items; “Appropriate descriptions of the neighborhood I lived longest in before age 10 are…”, “Appropriate descriptions of the neighborhood I lived longest in between ages 10 and 18 are”…), *teachers* (2 items; “Appropriate descriptions of the teachers I had in elementary and middle school are…”, “Appropriate descriptions of the teachers I had in high school are…”), and *classmates* (2 items; “Appropriate descriptions of my classmates in elementary and middle school are…”, “Appropriate descriptions of my classmates in high school are…”). To create a count of the number of contexts in which respondents had high exposure to Whites, we assigned responses of “minority White” a “0” and “majority White” responses a “+1”, and summed the recoded responses across all six exposure items.[32] We used the same coding scheme to index exposure to Blacks, such that self-reported exposure to both Whites and Blacks could take on values from 0 to 6.

As a more distal index of exposure, we calculated the percentage of Whites and Blacks within a respondent’s county of residence (hereafter referred to as “county-level exposure”). Estimates were obtained from the American Community Survey (ACS), which is disseminated yearly by the U.S. Census Bureau. In an effort to balance (a) using ACS data that most accurately characterizes the states and counties of respondents when they completed the study and (b) minimize data loss through an inability to provide exposure estimates for respondents[33], we used 5-year averages of data collected between 2010 and 2014.

[32] Some respondents provided impossible responses on self-reported exposure items (e.g., living in both a minority Black and majority Black neighborhood). Implausible responses were omitted.
[33] Geographical units of 20,000 or fewer are eligible for 5-year ACS estimates only. For more detail see: https://www.census.gov/programs-surveys/acs/technical-documentation/data-suppression.html

Covariates. There are a number of individual difference predictors that have shown to be predictive of implicit and explicit bias (e.g., Nosek et al., 2007). In order to accurately assess the effects of our focal predictors (exposure and contact) we present their effects with and without controlling for a number of covariates, including age (in years), gender identity (0 = “female” or “transgender female; 1 = “male” or “transgender male”), education level, political orientation, income, and wealth.

Analytic Approach

Respondents completing the Race IAT via Project Implicit could either receive self-report items pertaining to contact with Whites and Blacks *or* community-level exposure to Whites and Blacks (but not both). Thus, two separate analyses were needed to test the effects of contact and community-level exposure on implicit and explicit racial attitudes. County-level exposure variable were used as predictors in both analyses. For our analyses testing the effects of contact to Whites and Blacks, we had data from 9,082 Black and 51,361 non-Hispanic White respondents (total N = 60,443). We used multivariate multiple regression to simultaneously test the effects of predictors on both the implicit and explicit attitude measures, which were in turn followed-up with univariate multiple regression models. We used a two-step forward modeling approach. In Step 1, we fit a model that included (a) contact with Whites, (b) contact with Blacks, (c) county-level exposure to Whites, and (d) county-level exposure to Blacks. Further, in Step 1 we included interactions between race (dummy coded with White = 0 and Black = 1) and each contact and exposure variable, which allowed us to test whether their effects varied by participant race. In Step 2 we introduced covariates (age, sex, education, political orientation, income and wealth) and interactions between covariates and participant race into the model. For our analyses testing the effects of community-level exposure, we had data from 1,899 Black and

32,884 non-Hispanic White respondents (total N = 34,783). We used the same modeling approach except that the predictors of contact with Whites and Blacks were replaced with community-level exposure to Whites and Blacks. All continuous variables were grand-mean centered. For brevity, we limit our discussion to contact and exposure variables (though regression tables report all estimates).

Results

Descriptive Statistics

Table 16 shows overall means, standard deviations, and zero-order correlations among all study variables. Overall, respondents showed a preference for Whites over Blacks both implicitly (IAT M = .31, SD = .44; Cohen's d =.71) and explicitly (M = .21, SD = .87, d =.24), and implicit and explicit racial bias measures were positively correlated, r(93,122) = .304, 95% CI [.298, .310]. Out of eight possible contact experiences, participants reported having frequent contact with Whites (M = 6.96, SD = 1.80), and less frequent contact with Blacks (M =2.45, SD =2.29), and contact with Whites and Blacks was negatively correlated, r(60,441) =−.620, 95% CI [−.625, −.615]. Respondents had high community-level (M =5.86, SD= .60) and county-level exposure to Whites (M = 73.81%, SD = 16.12%), but these two indexes of exposure to Whites were weakly correlated, r(34,030) =.066, 95% CI [.055, .077]. In contrast, respondents had low community-level (M =2.00, SD = 2.63) and county-level exposure to Blacks (M= 13.08%, SD = 13.29%), but

Table 16. Sample sizes, means, standard deviations, and zero-order correlations among all study variables.

	Variable	***N***	***M***	***SD***	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**
1	Implicit Attitudes	95,266	.31	.44	–	–	–	–	–	–	–	–	–	–	–	–	–	–
2	Explicit Attitudes	93,124	.21	.87	.304	–	–	–	–	–	–	–	–	–	–	–	–	–
3	Race (0= White; 1 = Black)	95,266	.12	.32	−.298	−.420	–	–	–	–	–	–	–	–	–	–	–	–
4	Contact with Whites	60,443	6.96	1.80	.283	.433	−.780	–	–	–	–	–	–	–	–	–	–	–
5	Contact with Blacks	60,443	2.45	2.29	−.316	−.480	.745	−.620	–	–	–	–	–	–	–	–	–	–
6	Community Exposure to Whites	34,032	5.86	.60	.027	.062	−.120	NA	NA	–	–	–	–	–	–	–	–	–
7	Community Exposure to Blacks	2,049	2.00	2.63	−.338	−.458	.804	NA	NA	−.730	–	–	–	–	–	–	–	–
8	County Exposure to Whites	95,266	73.81	16.12	.082	.142	−.230	.220	−.260	.066	−.375	–	–	–	–	–	–	–
9	County Exposure to Blacks	95,266	13.08	13.29	−.074	−.116	.244	−.230	.293	−.030	.426	−.792	–	–	–	–	–	–
10	Age (years)	41,641	29.06	13.46	−.019	−.040	−.030	$.002^{\delta}$	$−.010^{\delta}$	$.029^{\delta}$	.236	−.081	.032	–	–	–	–	–
11	Gender (0 = F; 1 = M)	87,683	.38	.49	.065	.084	−.040	.075	−.050	$.013^{\delta}$	$−.084^{\delta}$	$−.010^{\delta}$	−.010	$.015^{\delta}$	–	–	–	–
12	Education	93,900	70.61	21.60	−.024	−.040	−.020	.017	$.006^{\delta}$	.031	.179	−.138	.103	.440	−.022	–	–	–
13	Political Orientation	92,875	4.74	1.73	−.111	−.230	.027	$−.010^{\delta}$	.070	$.000^{\delta}$	$−.087^{\delta}$	−.165	.072	.138	−.101	.138	–	–
14	Income	78,983	7.49	5.90	.066	.074	−.110	.131	−.120	.026	−.244	−.114	.021	.081	.093	−.040	−.050	–
15	Wealth	72,083	5.82	3.28	.081	.112	−.130	.139	−.150	.031	−.246	−.018	−.030	.140	.088	−.080	−.070	.663

Note. *N*’s for means, standard deviations, and zero-order correlations ranged from 921 to 95,226. This wide range can be explained by the fact that estimates for some variables were available for the entire sample due to exclusion criteria (e.g., we included only respondents that had an IAT *D*-score and reported their race), while few respondents had complete data on the measure of community-level exposure to Blacks ($N = 2{,}049$). Recall that respondents only received contact items or self-reported exposure items, which meant that correlations could not be computed among these variables (correlations between these variables are labeled “NA” in Table 1). Due to the large sample size, most zero-order correlations were statistically different from zero. Thus, only correlations that were *not* significant with $p < .001$ are subscripted (δ) in Table 1.

community-level and county-level indices of exposure to Blacks were moderately correlated, $r(2{,}047) = .426$, 95% CI [.390, .461]. Finally, indices of community-level exposure to White and Blacks, $r(1, 296) = -.729$, 95% CI [−.753, −.702], and county-level exposure to Whites and Blacks were negatively correlated, $r(95, 224) = -.792$, 95% CI [−.794, −.790].

Analysis 1: Effects of Contact and County-Level Exposure to Whites and Blacks

In our first analysis, we tested the effects of contact and county-level exposure to both Whites and Blacks on implicit and explicit attitudes. As shown in Table 16, there were positive bivariate associations between contact with Whites and both implicit, $r(60{,}441) = .283$, 95% CI [.276, .290], and explicit pro-White/anti-Black bias, $r(59{,}133) = .433$, 95% CI [.426, .440]. County-level exposure to Whites was also positively (but weakly) associated with scores on both implicit, $r(95{,}224) = .082$, 95% CI [.076,.088], and explicit measures, $r(93{,}122) = .142$, 95% CI [.136, .148]. In contrast, contact with Blacks was negatively associated with both implicit, $r(60{,}441) = -.316$, 95% CI [−.323, −.309], and explicit pro-White/anti-Black bias, $r(59{,}133) = -.481$, 95% CI [−.487, −.475]. County-level exposure to Blacks was also negatively associated with scores on both implicit, $r(95{,}224) = -.074$, 95% CI [−.080, −.068], and explicit measures, $r(93{,}122) = -.116$, 95% CI [−.122, −.110]. To assess the simultaneous association between racial bias and both contact and county-level exposure variables, as well as how these effects might differ between White and Black participants, we next conducted multiple regression analyses using the previously described two-step model building approach.

Step 1: Multivariate Effects of Race, Contact, and County-Level Exposure. An initial multivariate regression model was fit to assess the simultaneous effects of contact (with both Whites and Blacks) and county-level exposure (to Whites and Blacks) on implicit and explicit racial attitudes. We also included two-way interactions between each contact and exposure variable and participant race. As race was dummy coded (White = 0; Black =1), main effects are estimates for White respondents (the reference group) and interaction coefficients are the estimated difference of an effect (e.g., having contact with Whites) on racial bias between Whites and Blacks. Table 17 shows that each variable had a significant multivariate main effect, and with the exception of contact with Blacks, each multivariate effect was qualified by a significant interaction with participant race.

***Univariate Analyses*.** Table 18 shows the effects of predictors included in Step 1 on implicit bias. Each variable had a significant main effect such that Black respondents ($\beta = -.55$, $p < 2.2^{-16}$) and respondents with higher contact with Blacks ($\beta = -.16$, $p < 2.2^{-16}$) tended to have *weaker* implicit pro-White/anti-Black bias, while respondents that had higher contact with Whites ($\beta = .06$, $p = 2.3^{-11}$) or that had higher county-level exposure to Whites ($\beta = .03$, $p = 1.1^{-5}$) or Blacks ($\beta = .04$, $p = 2.4^{-10}$) tended to have *stronger* implicit pro-White/anti-Black bias. Interestingly, there was not a significant interaction between participant race and contact with Whites, contact with Blacks, or county-level exposure to Whites (all $|\beta\text{'s}| \leq .03$, $p \geq .056$; see Table 18). In contrast, the main effect of county-level exposure to Blacks was qualified by an interaction with participant race ($\beta = -.04$, $p = .009$). Though the main effect of county-level exposure to Blacks shows that higher exposure to Blacks is associated with increased implicit pro-White/anti-Black bias for White respondents, a simple slopes analysis indicated that county-level exposure to Blacks was not associated with implicit bias among Black respondents ($\beta = .00$, $p = .892$). Predictors in Step 1 explained over 12% of the variance in implicit bias (see Table 18).[34]

[34] We also ran a model that included explicit bias and the interaction between participant race and explicit bias as predictors. Other than finding that county-level exposure to Whites was no longer a significant predictor ($\beta = .01$, $p = .077$), results were unchanged.

Table 19 shows the effects of predictors included in Step 1 on *explicit* bias. Each variable had a significant main effect (with the same sign as the coefficients from the results with the implicit measures) such that Black respondents ($\beta = -.15$, $p = 1.5^{-7}$) and respondents with higher contact with Blacks ($\beta = -.32$, $p < 2.2^{-16}$) tended to have *weaker* explicit pro-White/anti-Black bias, while respondents that had higher contact with Whites ($\beta = .13$, $p < 2.2^{-16}$) or that had higher county-level exposure to Whites ($\beta = .08$, $p < 2.2^{-16}$) or Blacks ($\beta = .09$, $p < 2.2^{-16}$) tended to have *stronger* explicit pro-White/anti-Black bias. There was not a significant interaction between participant race and either contact with Blacks or county-level exposure to Blacks (both $|\beta\text{'s}| \leq .02$, $p \geq .137$; see Table 19). There were significant interactions between participant race and both contact with Whites ($\beta =.10$, $p < 2.2^{-16}$) and county-level exposure to Whites ($\beta =.05$, p .002). In both cases, the significant (and positive) coefficients for both main effects and interactions indicate that while contact and county-level exposure to Whites was associated with increased explicit pro-White/anti-Black bias among White respondents, these variables had an even stronger (positive) effect on explicit pro-White/anti-Black bias among Black respondents. Step 2 predictors explained over 26% of the variance in explicit bias.[35]

Step 2: Adding Covariates. In addition to the predictors included in Step 1, covariates (age, gender, political orientation, education, wealth, and income) and interactions between covariates and participant race were added as predictors in Step 2. There were three differences between the multivariate effects in Step 2 and those reported in Step 1. First, while county-level exposure to Whites and the interaction between participant race and county-exposure to Whites had significant multivariate effects in Step 1, neither were significant in Step 2 (see Table 17).

[35] We also ran a model that included implicit bias and the interaction between participant race and implicit bias as predictors. Other than finding a significant interaction between participant race and county-level exposure to Blacks ($\beta = .03$, $p = .045$), results were unchanged.

Table 17. Multivariate multiple regression results testing the effects of contact and county-level exposure on implicit and explicit bias.

	Step 1 (N = 59,135)				Step 2 (N = 15,899)			
Variables	Wilks' Λ	F	df	p	Wilks' Λ	F	df	p
Race	.74	10245.50	2	$< 2.2^{-16}$	.72	3106.7	2	$< 2.2^{-16}$
Contact with Whites	.98	705.60	2	$< 2.2^{-16}$	.97	206.2	2	$< 2.2^{-16}$
Contact with Blacks	.94	1824.40	2	$< 2.2^{-16}$	.94	494.6	2	$< 2.2^{-16}$
County Exposure to Whites	1.00	16.20	2	9.11^{-8}	1.00	1.0	2	.384
County Exposure to Blacks	1.00	114.90	2	$< 2.2^{-16}$	1.00	32.4	2	8.7^{-25}
Race × Contact with Whites	1.00	45.70	2	$< 2.2^{-16}$	1.00	6.6	2	.001
Race × Contact with Blacks	1.00	.50	2	.605	1.00	3.4	2	.033
Race × County Exposure to Whites	1.00	5.40	2	.004	1.00	.3	2	.739
Race × County Exposure to Blacks	1.00	5.50	2	.004	1.00	4.9	2	.008
Age					1.00	10.7	2	2.4^{-5}
Gender					1.00	35.2	2	5.4^{-16}
Political Orientation					.95	377.9	2	2.2^{-16}
Education					1.00	12.5	2	3.9^{-6}
Income					1.00	3.2	2	.040
Wealth					1.00	11.3	2	.012
Race × Age					1.00	5.3	2	.005
Race × Gender					1.00	14.0	2	.000
Race × Political Orientation					1.00	9.2	2	.000
Race × Education					1.00	6.3	2	.002
Race × Income					1.00	3.1	2	.044
Race × Wealth					1.00	.3	2	.783

Table 18. The unstandardized coefficients (B), standard error of the unstandardized coefficients (SE B), and standardized coefficients (β) of multiple regression models predicting *implicit* pro-White/anti-Black bias from contact and county-level exposure.

	Step 1 (N = 59,135)				Step 2 (N = 15,899)			
Variables	*B*	*SE B*	β	*p*	*B*	*SE B*	β	*p*
Race	−.248	.014	−.55	$< 2.2^{-16}$	−.252	.029	−.56	$< 2.2^{-16}$
Contact with Whites	.014	.002	.06	2.3^{-11}	.015	.004	.06	2.1^{-4}
Contact with Blacks	−.031	.001	−.16	$< 2.2^{-16}$	−.029	.002	−.15	$< 2.2^{-16}$
County Exposure to Whites	.001	.000	.03	1.1^{-5}	.000	.000	.02	.234
County Exposure to Blacks	.001	.000	.04	2.4^{-10}	.001	.000	.05	.001
Race × Contact with Whites	−.003	.003	−.01	.284	.004	.006	.02	.531
Race × Contact with Blacks	.004	.003	.02	.194	.012	.006	.06	.055
Race × County Exposure to Whites	−.001	.001	−.03	.056	−.001	.001	−.04	.212
Race × County Exposure to Blacks	−.001	.001	−.04	.009	−.001	.001	−.04	.188
Age					.000	.000	.00	.902
Gender					.037	.008	.08	6.0^{-7}
Political Orientation					−.022	.002	−.08	$< 2.2^{-16}$
Education					.000	.000	−.01	.198
Income					.000	.001	.03	.959
Wealth					.004	.002	.00	.013
Race × Age					.000	.001	.01	.823
Race × Gender					−.055	.020	−.12	.006
Race × Political Orientation					.018	.006	.07	.005
Race × Education					.000	.000	.00	.851
Race × Income					.001	.002	.01	.623
Race × Wealth					−.002	.004	−.02	.533
R^2	.123				.138			

Table 19. The unstandardized coefficients (*B*), standard error of the unstandardized coefficients (SE *B*), and standardized coefficients (β) of multiple regression models predicting *explicit* pro-White/anti-Black bias from contact and county-level exposure.

	Step 1 (N = 59,135)				Step 2 (N = 15,899)			
Variables	*B*	*SE B*	β	*p*	*B*	*SE B*	β	*p*
Race	−.135	.026	−.15	1.5^{-7}	−.309	.051	−.34	1.5^{-9}
Contact with Whites	.064	.004	.13	$< 2.2^{-16}$	.074	.007	.15	2.2^{-16}
Contact with Blacks	−.124	.002	−.32	$< 2.2^{-16}$	−.117	.004	−.30	2.2^{-16}
County Exposure to Whites	.004	.000	.08	$< 2.2^{-16}$	.000	.001	.00	.825
County Exposure to Blacks	.006	.000	.09	$< 2.2^{-16}$	.003	.001	.04	.002
Race × Contact with Whites	.050	.006	.10	$< 2.2^{-16}$	.040	.011	.08	1.7^{-4}
Race × Contact with Blacks	.003	.006	.01	.660	.019	.011	.05	.099
Race × County Exposure to Whites	.003	.001	.05	.002	.003	.002	.06	.060
Race × County Exposure to Blacks	.001	.001	.02	.137	.005	.002	.07	.014
Age					−.001	.001	−.01	.078
Gender					.021	.013	.02	.124
Political Orientation					−.090	.004	−.17	$< 2.2^{-16}$
Education					−.001	.000	−.02	.016
Income					−.003	.001	.04	5.3^{-5}
Wealth					.011	.003	−.02	.047
Race × Age					.007	.002	.10	5.3^{-6}
Race × Gender					.119	.035	.13	7.2^{-4}
Race × Political Orientation					−.026	.011	−.05	.020
Race × Education					−.003	.001	−.08	3.0^{-4}
Race × Income					−.008	.004	−.05	.833
Race × Wealth					.001	.007	.01	.041
R^2	.267				.314			

Second, while the interaction between participant race and contact with Blacks was not significant in Step 1, there was a significant multivariate effect in Step 2. Notably, missing data on covariates drastically reduced the sample size from Step 1 (N=59,135) to Step 2 (N = 15,899).

Univariate Analyses**.** Table 18 shows the effects of predictors included in Step 2 on implicit bias. With the exception of county-level exposure to Whites, which was not a significant predictor of implicit bias in Step 2 (β = .02, p = .531), all other main effect estimates were similar to those obtained in Step 1 (see Table 18). Also replicating results from Step 1, we also found that there were no significant interactions between participant race and contact with Whites, contact with Blacks, or county-level exposure to Whites (all $|\beta\text{'s}| \leq .06$, $p \geq .055$). Lastly, while different from zero in Step 1, the coefficient for the interaction between participant race and county-level exposure to Blacks was not different from zero in Step 2 ($\beta = -.04$, p = .188).[36]

Table 19 shows the effects of predictors included in Step 2 on explicit bias. Similar to the results on the implicit measure, all main effects other than county-level exposure to Whites (β =.00, p = .825) remained significant in Step 2 (see Table 19). Also replicating results from Step 1, we also found that there was a significant interaction between participant race and contact with Whites (β =.08, $p = 1.7^{-4}$). The positive interaction coefficient indicates that contact with Whites continued to be an even stronger predictor for Black respondents than for White respondents. Unlike the results in Step 1, the coefficient for the interaction between participant race and county-level exposure to Whites was not different from zero in Step 2 (β =.06, p = .060).

[36] We ran the model after (a) excluding covariates that did not have correlations greater than or equal to .05 with either outcome variables or predictors included in Step 1 (participant age and education; see Table 16) and (b) using explicit bias and the interaction between participant race and explicit bias as predictors. There were two differences in the results: First, we found that contact with Whites was no longer a significant predictor (β =.01, p = .233). Second, we found a significant interaction between participant race and county-level exposure to Blacks (β =−.04, p = .025). A simple slopes analysis indicated county-level exposure to Blacks was not associated with implicit bias among Black respondents (β =−.02, p = .268).

However, there was a significant interaction between participant race and county-level exposure to Blacks that was different from zero in Step 2 (but not Step 1; β =.07, p = .014). The positive and non-zero main effect of county-level exposure to Blacks indicates that higher exposure to Blacks is associated with greater explicit pro-White/anti-Black bias among White respondents (the reference group), and the positive non-zero interaction term indicates that this effect was even stronger for Black respondents.[37]

Analysis 2: Effects of Community-Level and County-Level Exposure to Whites and Blacks

Recall that the effects of contact and community-level exposure could *not* be tested in the same analyses due to the fact that respondents completing the Race IAT via Project Implicit could receive self-report items pertaining to contact or community-level exposure (but not both). Thus, we now examine the associations between community-level exposure and implicit and explicit racial bias. As shown in Table 16, there were positive (but weak) bivariate associations between community-level exposure to Whites and both implicit, r(34,030) =.027, 95% CI [.016, .038], and explicit pro-White/anti-Black bias, r(33,261) =.062, 95% CI [.051, .073]. In contrast, community-level exposure to Blacks was negatively associated with both implicit, r(2,047) =−.338, 95% CI [−.376, −.299], and explicit pro-White/anti-Black bias, r(2,005) = −.458, 95% CI [−.492, −.423]. To assess the simultaneous association between racial bias and both community-level and county-level exposure variables for White and Black respondents, we employed the same two-step model building approach used to test contact effects in Analysis 1.

[37] We ran the model after (a) excluding covariates that did not have correlations greater than or equal to .05 with either outcome variables or predictors included in Step 1 (participant age and education; see Table 16) and (b) using implicit bias and the interaction between participant race and implicit bias as predictors. There were three differences in the results: First, we found that county-level exposure to Whites was a significant predictor (β =.02, p = .008). Second, we found a significant interaction between participant race and county-level exposure to Whites (β =.05, p = .014). Third, we found that the interaction between participant race and county-level exposure to Blacks was no longer significant (β =.03, p = .068).

Step 1: Main Effects of Race, Contact, and County Exposure. A multivariate regression model was fit to assess the simultaneous effects of community-level and county-level exposure. We also included two-way interactions between each exposure variable and participant race. Table 20 shows that only participant race and community-level exposure to Whites had significant multivariate effects. However, due to missing data on self-report exposure variables, the sample size for community-level exposure analyses were modest (N = 1,281).

***Univariate Analyses*.** Table 21 shows the effects of predictors included in Step 1 on implicit bias. While Black respondents tended to have lower implicit pro-White/anti-Black bias than White respondents ($\beta = -1.02$, $p < 2.2^{-16}$), all other coefficients were not significantly different from zero (all $|\beta\text{'s}| \leq .10$, $p \geq .294$; see Table 21). These variables explained approximately 10% of the variance in implicit bias.[38]

Table 22 shows the effects of predictors entered in Step 1 on explicit bias. There were three significant main effects such that Black respondents and ($\beta = -.75$, $p = 7.5^{-7}$) and those with higher *county-level* exposure to Blacks ($\beta = .09$, $p = .027$) tended to have stronger explicit pro-White/anti-Black bias, whereas *community-level* exposure to Blacks was associated with weaker explicit pro-White/anti-Black bias ($\beta = -.29$, $p = .016$). The effect of community-level exposure to Blacks was qualified by a significant interaction with participant race ($\beta = .44$, $p =$ .023). The simple slope for Black respondents indicated that community-level exposure to Blacks was *not* associated with explicit bias ($\beta = .14$, $p = .330$).[39]

[38] We also ran a model using explicit bias and the interaction between participant race and explicit bias as predictors. Results were unchanged.
[39] We also ran a model using implicit bias and the interaction between participant race and implicit bias as predictors. The only difference in the results was that there was a significant interaction between participant race and community-level exposure to Whites, such that community-level exposure to Whites was not associated with explicit bias for White respondents ($\beta = -.01$, $p = .675$), but was associated with greater explicit pro-White/anti-Black bias among Black respondents ($\beta = .07$, $p = .034$).

Table 20. Multivariate multiple regression results testing the effects of community-level and county-level exposure on implicit and explicit bias.

	Step 1 (N = 1,281)				Step 3 (N = 329)			
Variables	Wilks' Λ	F	df	p	Wilks' Λ	F	df	p
Race	.78	179.98	2	$< 2.2^{-16}$	.75	51.4	2	$< 2.2^{-16}$
Community Exposure to Whites	.99	4.20	2	.015	.96	5.9	2	.003
Community Exposure to Blacks	1.00	1.02	2	.362	1.00	.2	2	.794
County Exposure to Whites	1.00	.37	2	.689	1.00	.2	2	.813
County Exposure to Blacks	1.00	1.46	2	.232	1.00	.4	2	.683
Race × Community Exposure to Whites	1.00	.89	2	.411	.99	.9	2	.391
Race × Community Exposure to Blacks	1.00	2.50	2	.083	.99	1.2	2	.312
Race × County Exposure to Whites	1.00	1.46	2	.232	1.00	.3	2	.714
Race × County Exposure to Blacks	1.00	.61	2	.544	1.00	.6	2	.571
Age					.99	.9	2	.410
Gender					.97	5.5	2	.005
Political Orientation					.91	15.9	2	.000
Education					1.00	.4	2	.660
Income					.99	2.1	2	.123
Wealth					.99	2.3	2	.100
Race × Age					1.00	.6	2	.543
Race × Gender					.99	1.0	2	.362
Race × Political Orientation					.98	2.6	2	.079
Race × Education					1.00	.5	2	.618
Race × Income					.98	3.7	2	.027
Race × Wealth					.99	.9	2	.402

Table 21. The unstandardized coefficients (*B*), standard error of the unstandardized coefficients (SE *B*), and standardized coefficients (β) of multiple regression models predicting *implicit* pro-White/anti-Black bias from community-level and county-level exposure.

	Step 1 (*N* = 1,281)				Step 2 (*N* = 329)			
Variables	*B*	*SE B*	β	*p*	*B*	*SE B*	β	*p*
Race	−.455	.076	−1.02	$< 2.2^{-16}$	−.603	.198	−1.34	.002
Community Exposure to Whites	.014	.016	.02	.390	−.014	.034	−.02	.684
Community Exposure to Blacks	−.003	.023	−.02	.898	.004	.050	.02	.936
County Exposure to Whites	−.001	.001	−.02	.642	.001	.003	.05	.604
County Exposure to Blacks	.000	.002	.00	.953	.002	.003	.08	.440
Race × Community Exposure to Whites	−.036	.035	−.05	.294	−.141	.081	−.19	.081
Race × Community Exposure to Blacks	−.017	.037	−.10	.646	−.140	.092	−.82	.133
Race × County Exposure to Whites	−.001	.004	−.03	.844	.002	.010	.07	.848
Race × County Exposure to Blacks	−.001	.004	−.02	.856	.002	.011	.05	.867
Race × Age					−.006	.008	−.19	.424
Age					.000	.003	.01	.914
Gender					.067	.052	.15	.193
Political Orientation					−.042	.015	−.16	.005
Education					.000	.001	.01	.866
Income					−.003	.005	−.03	.624
Wealth					.016	.011	.11	.145
Race × Gender					.015	.164	.03	.928
Race × Political Ideology					.048	.067	.18	.472
Race × Education					.008	.006	.42	.137
Race × Income					−.030	.037	.77	.016
Race × Wealth					.059	.024	−.24	.375
R^2	.102				.154			

Table 22. The unstandardized coefficients (*B*), standard error of the unstandardized coefficients (SE *B*), and standardized coefficients (β) of multiple regression models predicting *explicit* pro-White/anti-Black bias from community-level and county-level exposure.

	Step 1 (N = 1,281)				Step 2 (N = 329)			
Variables	*B*	*SE B*	β	*p*	*B*	*SE B*	β	*p*
Race	-.675	.136	−.75	7.5^{-7}	−.824	.350	−.92	.019
Community Exposure to Whites	-.007	.029	.00	.817	.061	.060	.04	.309
Community Exposure to Blacks	-.100	.041	−.29	.016	−.045	.088	−.13	.614
County Exposure to Whites	.003	.002	.05	.181	.000	.005	.01	.939
County Exposure to Blacks	.006	.003	.09	.027	.005	.006	.08	.349
Race × Community Exposure to Whites	.113	.062	.07	.066	.243	.143	.16	.090
Race × Community Exposure to Blacks	.150	.066	.44	.023	.089	.164	.26	.587
Race × County Exposure to Whites	.001	.007	.02	.891	−.039	.018	−.70	.054
Race × County Exposure to Blacks	-.008	.007	−.12	.270	−.034	.019	−.52	.072
Age					−.001	.005	−.02	.763
Gender					.122	.092	.14	.186
Political Orientation					−.124	.026	−.24	3.9^{-6}
Education					−.001	.003	−.03	.642
Income					.003	.010	.10	.150
Wealth					.027	.019	.02	.788
Race × Age					−.020	.014	−.29	.171
Race × Gender					.388	.291	.43	.184
Race × Political Orientation					−.240	.119	−.46	.045
Race × Education					.001	.010	.02	.932
Race × Income					−.047	.043	−.31	.275
Race × Wealth					.059	.065	.21	.370
R^2	.195				.335			

Step 2: Adding Covariates. Along with the predictors included in Step 1, covariates and interactions between covariates and participant race were included as predictors in Step 2. Similar to the results of the multivariate multiple regression model fit in Step 1, we found that both participant race and community-level exposure to Whites had significant multivariate main effects (see Table 20). However, note that the sample size was again reduced from that in Step 1 due to missing data on covariate predictors (N = 329).

***Univariate Analyses*.** Table 21 shows that results from Step 2 were similar to those from Step 1, such that other than participant race ($\beta = -1.34$, $p = .002$), no other regression coefficients (for exposure or interactions between participant race and exposure) were significantly different from zero (all $|\beta\text{'s}| \leq .82$, $p \geq .081$).[40]

Table 22 shows the effects of predictors from Step 2 on explicit bias. Like Step 1, we found that Black participants tended to have weaker explicit pro-White/anti-Black bias than White respondents ($\beta = -.92$, $p = .019$). However, unlike the results found in Step 1, we found no significant main effects of exposure or two-way interactions between participant race and exposure (all $|\beta\text{'s}| \leq .70$, $p \geq .054$; see Table 22).[41]

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[40] We ran the model after (a) excluding covariates that did not have correlations greater than or equal to .05 with either outcome variables or predictors included in Step 1 (participant age and education; see Table 16) and (b) using explicit bias and the interaction between participant race and explicit bias as predictors. Results were unchanged.

[41] We ran the model after (a) excluding covariates that did not have correlations greater than or equal to .05 with either outcome variables or predictors included in Step 1 (participant age and education; see Table 16) and (b) using implicit bias and the interaction between participant race and implicit bias as predictors. Results differed such that the main effects of community-level exposure to Blacks ($\beta = -.38$, $p = .016$), county-level exposure to Blacks ($\beta = .11$, $p = .028$), and the interaction between participant race and community-level exposure to Blacks ($\beta = .57$, $p = .012$) that were significant in Step 1 were also significant in Step 2. The other difference in the results is that the interaction between participant race and community-level exposure to Whites was no longer significant ($\beta = .07$, $p = .107$).

Discussion

In a sample of White and Black responds residing the U.S., this study tested the effects of contact with/ exposure to Whites and Blacks on implicit and explicit racial bias. Summarizing results that were consistent both with and without including covariates, our analyses testing the effects of contact found that contact with Whites, contact with Blacks, and county-level exposure to Blacks were durably associated with both implicit and explicit racial bias. Specifically, we found that respondents that had higher contact with Whites or lived in counties with higher exposure to Blacks tended to have *stronger* pro-White/anti-Black bias, whereas those with higher contact with Blacks tended to have *weaker* pro-White/anti-Black bias. Moreover, we replicated these effects across both implicit and explicit measures. Interestingly, other than finding that contact with Whites consistently had a stronger effect on explicit bias for Black respondents compared to Whites respondents, we found little evidence that participant race moderated the effects we observed. In contrast, we found no durable effects of community-level exposure to Whites or Blacks, though due to high proportions of missing data, the size of the sample used to test community-level exposure was *much* smaller than that used to test the effects of contact.

Contributions of the Current Work

Integrating Contact and Exposure. One contribution of this work is the demonstration that two variables often considered in isolation – contact and exposure- are both influential predictors when considered in tandem. Indeed, we found that contact and exposure can have independent effects on racial bias, which suggests that the importance of both individual experiences (contact with Whites and Blacks) and the social context in which these experiences occur (the degree of outgroup exposure in a county). While we assessed the *main effects* of contact and exposure, future research might go even further by considering whether the effects of contact differ across contexts with more/less outgroup exposure (i.e., an *interaction* between contact and exposure). A priori, it seems plausible that contact could be either more or less helpful in areas characterized by varying degrees of outgroup exposure. On one hand, if areas with high outgroup exposure are also characterized by social norms discouraging intergroup interactions (Pettigrew, 1998), or perhaps elevated status differences between group members (Allport, 1954), contact with outgroup members may be less beneficial in these localities. On the other hand, if these features are more characteristic of low exposure environments, contact with outgroup members may be more beneficial in these areas.

Effects of Contact. Another contribution of this work is the demonstration that contact with *outgroup* members may have implicit bias reducing effects for both majority and minority group members. Though several studies have found that intergroup contact may be associated with reduced implicit ingroup bias among majority group members (e.g., Aberson et al., 2004; MacInnis et al., 2016; Prestwich, Kenworthy, Wilson, & Kwan-Tat, 2008), our sample of White respondents provides support for this hypothesis in the largest sample to date (N > 51,000). Moreover, to our knowledge, this is the first study demonstrating the intergroup contact may be effective in reducing implicit bias among minority group members. Like most work testing the effects of intergroup contact (Pettigrew & Tropp, 2006), the correlational nature of our data prevents us from concluding that intergroup contact plays a causal role in reducing implicit bias. In other words, it could true that respondents with lower levels of ingroup bias seek out more contact with outgroup members rather than the alternative discussed here, which is that contact with outgroup members leads to lower levels of ingroup bias. That being said, various strategies for reducing implicit bias have been ineffective in producing long-term effects (Lai et al., 2016), and intergroup contact has *not* been one of the tested intervention strategies. Thus, the correlational evidence presented here seems to suggest that intergroup contact may play a causal role in reducing implicit bias.

Another contribution of this research was our strategy to separately assess the effects of contact with Whites and Blacks. In this way, we were not only able to assess respondent's contact with outgroup members (e.g., contact with Whites among Blacks), which is typically the focus of research testing contact effects, but we were also able to measure respondent's contact with ingroup members (e.g., contact with Blacks among Blacks). By finding that contact with Whites and Blacks were simultaneous predictors of racial bias for both White and Black respondents, our results suggest that ingroup contact predicts racial bias above and beyond outgroup contact. This finding is particularly interesting insofar that contact theory clearly specifies the routes by which contact with outgroup members may decrease ingroup bias, but is silent with regards to why contact with ingroup members might be associated with increased ingroup bias (Levin et al., 2003). Again, an obvious explanation is that respondents with high ingroup bias tend to seek out more contact with ingroup members. However, are there any explanations for why ingroup contact might lead to more ingroup bias? One possibility could be that ingroup contact increases the perceived entitativity (the perceived coherence and unity of a group; Hamilton & Sherman, 1996) of the ingroup, which in turn, is associated with increased ingroup bias (Gaertner & Schopler, 1998). Another possibility is that ingroup contact may increase ingroup favoritism via cognitive consistency principles (e.g., Greenwald et al., 2002; Heider, 1958; Osgood & Tannenbaum, 1955). For example, Balanced Identity Theory (Greenwald et al., 2002) argues that two concepts will become associated with both are linked to a third concept. As such, given the general tendency to associate the self (concept A) with positive valence (concept B), ingroup contact may strengthen the association of self with the ingroup (concept C), which in turn increases the association of the ingroup with positive valence.

Effects of Exposure. Previous research has commonly examined how ingroup bias may change as a function of outgroup exposure, or even combined outgroup and ingroup exposure into a single predictor (Rae et al., 2015). As the present work separately tested the effects of exposure to both Whites and Blacks, we were able to pinpoint that racial bias was predicted by county-level proportions of Black (but not White) residents- an effect true for both White and Black respondents. In other words, we found that increased *outgroup* exposure was associated with higher levels of ingroup bias among Whites, and that lower *ingroup* exposure was associated with lower levels of ingroup bias among Blacks. This finding in part contradicts previous findings reported by Rae et al. Indeed, Rae et al. found that state-level implicit and explicit pro-White/anti-Black bias was highest for Whites - but lowest for Blacks- in states with higher ratios of Black to White residents (e.g., Mississippi). Further, Rae et al. found a similar effect using counties (rather than states) as the unit of analysis. While results for White respondents reported here are consistent with those reported by Rae et al., we found the opposite pattern of results for Black respondents such that pro-White/anti-Black bias increased as a function of exposure to Black residents.

What might explain discrepancy in the findings reported by Rae et al. and those described here? One explanation is that the analyses reported here and those reported by Rae et al. tested the effects of exposure using different units of analysis. Indeed, Rae et al. tested how *aggregated* bias scores at the state and county-levels was associated with state and county-level exposure, whereas we examined the association of county-exposure on *individual's* racial bias. Variables can have different – and even opposite- associations at different units of analysis (Robinson, 1950). Another explanation is that sampling differences are responsible for discrepancies for results reported here and by Rae et al. One particularly salient difference is sample size: Rae et al. analyzed data from over 890,000 respondents, whereas we tested the effects of exposure to Whites and Blacks in a smaller sample (*N* ~ 59,000). In total, future work should seek to more firmly establish the association between exposure and ingroup bias among minority group members.

Limitations

First, it is worth reiterating that our analyses were all correlational, which precludes drawing causal interpretations from any of the results reported here. Second, although we sought to test the effects of community-level exposure on racial bias, missing data prevented us from securing a large enough sample to adequately test the effects of community-level. Thus, we consider the reported results of community-level exposure to be tentative (at best). Another limitation involves our dataset. Although collecting data through Project Implicit allowed us to target a far larger sample than we otherwise could have, respondents did choose to access the Project Implicit website and self-selected to complete the Race IAT, thus these respondents may meaningfully differ from the general population (e.g., with regards to demographic characteristics, interest in the topic; Nosek et al., 2002). Finally, our results might be both group-specific (i.e., Black/White relations) and context-specific (i.e., the U.S.), such that our results may have looked different had we assessed racial bias towards different groups (i.e., Asian/White relations in the U.S.) or in a different context (i.e., Black/White relations in South Africa).

Conclusion

As the U.S. becomes increasingly diverse, residents will inevitably have more frequent exposure to members of other groups. This work suggests that this exposure to Black residents may increase threat and ingroup bias for White residents, and decrease ingroup bias for Black residents. For Whites, our results suggest that intergroup contact has the potential to buffer against these effects, thus reducing ingroup biases. We believe that this work provides an important foundation for understanding how contact and exposure – both to ingroups and outgroups- work in concert to predict ingroup biases. Moreover, we hope that this work stimulates future investigation of how intergroup contact can be encouraged within the diverse social contexts in which it is needed the most.

Chapter 5

General Discussion

Psychologists most often rely on self-report measures to index people's thoughts, feelings, and beliefs. However, implicit measures are increasingly used as windows into portions of the mind that people are unwilling or unable to self-report. This dissertation used the IAT in two relatively new areas of implicit cognition research. Chapter 2 investigated the test-retest reliability and predictive validity of a child-adapted version of the IAT. Overall, children's IAT scores were stable over time, and to a lesser extent, predictive of behavior. Indeed, relative to scores on the Gender Identity IAT, scores on the Race Attitude IAT were more predictive of behavior. Chapters 3 and 4 investigated whether scores on the Race Attitude IAT were associated with differences in Black-White racial demographics across U.S. states and counties. At both the aggregate-level (i.e., mean IAT scores for counties and states; Chapter 3) and individual-level (i.e., a person's IAT score; Chapter 4), results indicated that areas with more Black residents were associated with higher levels of pro-White/anti-Black implicit attitudes for White residents. However, associations between Black-White racial demographics and implicit race attitudes among Black respondents were more mixed: In Chapter 4, areas with greater of proportions of Black residents tended to have *lower* levels of pro-White/anti-Black implicit attitudes among Blacks. In contrast, proportions of Black residents were associated with *higher* levels of pro-White/anti-Black implicit attitudes for Blacks in Chapter 5. Chapter 4 also found that racial demographics and contact with both with ingroup and outgroup members were independent predictors of implicit race attitudes. Below I discuss these results in more detail and consider how results from this dissertation contribute to previous research in implicit cognition.

Developmental Research: Performance of the IAT

Previous studies investigating the test-retest reliability and predictive validity of the IAT in children have differed on many factors (e.g., domain, lag-time, IAT structure, age of respondents), making it difficult to identify the conditions under which children's IAT scores are more/less reliable and valid. Chapter 2 presented the results of five studies in which two of these factors – domain and lag-time– were held constant or varied to assess their impact on the test-retest reliability and predictive validity of children's IAT scores. Surprisingly, results indicated that children's IAT scores were stable over time, and that neither domain nor lag-time moderated the strength of test-retest reliability. However, IAT test-retest coefficients reported in previous developmental research have been quite variable (ranging from $r = -.17$ to $r = .62$). Thus, as the results reported here suggest that domain and lag-time may not moderate strength of test-retest reliability of the IAT in children, what other factors might explain the variability in test-retest coefficients observed in previous work? Two factors worthy of consideration are those that were held constant in the current work but have varied across previous studies: IAT structure and age of respondents. Moreover, future research might also consider the similarity of conditions under which children take the IAT at both test and retest. Indeed, previous work (reviewed in Chapter 1) and the current work (results from Chapters 3 and 4) demonstrate how implicit cognition is sensitive to contextual factors. In Chapter 2, conditions between test and retest were as similar as possible; at both testing occasions, participants were administered the IAT by the same experimenter, in the same location, and often during the same time of day. Reducing the variability that may have arisen had participants completed the IATs in different contexts (e.g., administered the task by a different experimenter; Lowery et al., 2001) might partially explain why we found relatively similar test-retest coefficients across all studies.

While neither domain nor lag-time moderated the degree to which children's IAT scores were correlated across test and retest, an internal meta-analysis of the studies in Chapter 2 found that IAT was more predictive of behavior in the domain of racial attitudes compared to gender identity. This finding should serve as a springboard for future research exploring *why* these domain differences occur. On one hand, differences in our results could be explained by the fact that the Race Attitude IAT and Gender Identity IAT used here assessed different *constructs* (attitude vs. identity). On the other hand, differences in our results could be explained by the fact that these IATs also assessed implicit associations towards different *social categories* (gender vs. race). To disentangle these possibilities, future work might benefit from testing the differences in IATs measuring different constructs but with the same social categories (e.g., gender attitude vs. gender identity), or alternatively, comparing IATs assessing the same construct but towards different social categories (e.g., gender attitude vs. race attitude).

Context Effects of Implicit Cognition: Broader Social Environment

A handful of studies have shown that features of the broader social environment can be useful predictors of implicit weight attitudes (Marini et al., 2013) and implicit gender stereotypes (Miller et al., 2014; Nosek et al., 2009). Chapters 3 and 4 investigated how racial diversity, a factor shown to be predictive of explicit racial attitudes (e.g., Taylor, 1998), is associated with implicit and explicit Black-White racial attitudes among both White and Black respondents. At the aggregate-level, Chapter 3 found that Black and White residents in U.S. states with higher proportions of Black residents (e.g., Mississippi) tended to have higher levels of implicit and explicit ingroup favoritism than Black and White residents living in states with lower proportions of Black residents (e.g., Idaho). In other words, Whites tended to have stronger pro-White/anti-Black attitudes and Blacks tended to have weaker pro-White/anti-Black attitudes as a function of the proportion of Black residents residing in their state – at least at the aggregate-level. This effect was also true at the level of U.S. counties. Chapter 4 replicated this effect at the individual-level for White respondents, such that proportions of Black residents in one's county was positively associated with implicit and explicit pro-White/anti-Black attitudes, even when accounting for respondent's contact with Whites and Blacks. However, failing to replicate the findings from Chapter 3, proportions of Black residents was also associated with more pro-White/anti-Black attitudes among Blacks in Chapter 4.

One strength of this research is its focus on how features of the natural environment may influence implicit cognition. Indeed, previous research has often used laboratory manipulations to study how context can influence responding on implicit measures (e.g., Dasgupta & Greenwald, 2001; Rudman & Lee, 2002), and laboratory studies can often have limited use in understanding real-world behavior (Mehl, 2013). Thus, examining how racial diversity – an environmental feature that characterizes the spaces that people inhabit everyday- can influence implicit race attitudes is a particular strength of the papers presented in Chapter 3 and 4. Future work should investigate whether group proportions at the state and county-levels are similarly associated with implicit attitudes towards other social groups (e.g., Asians or Hispanics).

Despite providing a demonstration (Chapter 3) and replication (Chapter 4) of an association between proportions of Black residents and implicit racial attitudes among Whites, it is still unclear *why* Whites living in areas with higher proportions of Black residents tend to have higher implicit pro-White/anti-Black attitudes. One possibility is that living in areas with higher proportions of Black residents is threating, which in turn, leads to higher levels of liking for the ingroup (e.g., Blalock, 1967). However, many types of threat (e.g., realistic threat, symbolic threat, etc.) have been identified in previous work (for reviews, see Riek et al., 2006; Stephan &

Stephan, 2000), so it is unclear which type/s of threat might be triggered by increased proportions of Black residents. Therefore, future work might measure different threat concerns along with the Race Attitude IAT, and test which type of threat mediates the association between larger proportions of Black residents and increased levels of pro-White/anti-Black attitudes for White respondents. Further it is unclear why proportions of Blacks residents were associated with weaker pro-White/anti-Black attitudes among Blacks in Chapter 3, but higher pro-White/anti-Black attitudes among Blacks in Chapter 4. Thus, future research should continue to investigate the association between racial demographics and racial attitudes among Blacks.

Several limitations of this work must be acknowledged. First and foremost, analyses in Chapters 3 and 4 utilized observational data, which precludes drawing any directional and causal conclusions. For example, results in Chapter 4 indicated that increased contact with Blacks was associated with lower levels of implicit and explicit pro-White/anti-Black attitudes among Whites. While this result might be taken to mean that contact with Blacks causes lower levels of pro-White/anti-Black attitudes, the opposite could also be true such that Whites with lower levels of pro-White/anti-Black attitudes are those that choose to engage in contact with Blacks. Secondly, analyses in in Chapter 3 aggregated IAT scores across U.S. states and counties, which meant averaging out variability at the individual-level. Further, it is inappropriate to draw inferences about individual-level (i.e., psychological) processes from aggregated analyses. Another limitation of Chapter 3 is that a ratio of Black to White respondents within counties and states was used to predict aggregated IAT scores. While this technique was used to match the relative nature of the IAT and has been used in previous research (Alba et al., 2005), it is unclear from this measure whether proportions of Whites, proportions of Blacks, or some combination of both are responsible for the association with implicit and explicit ingroup bias. However, analyses used in Chapter 4 addressed these concerns such that county-level proportions of Whites and Blacks were used to predict individual-level implicit and explicit attitudes. Lastly, data in Chapters 3 and 4 came from respondents that volunteered to complete the IAT, which means that self-selection issues (e.g., Nosek, Banaji, & Greenwald, 2002) may have biased the results.

Conclusion

Implicit measures are increasingly being used in psychology. This dissertation suggests that implicit measures- specifically the IAT – may be useful in looking at the development of implicit cognition in early childhood, as well as testing for how individual differences in implicit cognition might be influenced by one's broader social context.

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Appendix A

Results from Study 1 Disaggregated by Participant Sex

Table A1. Zero-order correlations among males for variables in Study 1. *N*'s ranged from 52 to 55.

	Variable	1	2	3	4	5	6	7	8
1	Age	-	-	-	-	-	-	-	-
2	Self-report Identity - Test	−.11	-	-	-	-	-	-	-
3	Self-report Identity - Retest	−.14	.09	-	-	-	-	-	-
4	Self-report Identity (average)	−.18	.74***	.74***	-	-	-	-	-
5	IAT - Test	−.07	.14	.00	.17	-	-	-	-
6	IAT - Retest	−.11	−.07	−.15	−.12	.43**	-	-	-
7	IAT (average)	−.10	.04	−.09	.02	.85***	.85***	-	-
8	Coloring Book Selected	.02	−.18	.33*	.10	−.14	−.07	−.13	-
9	Clothing	−.13	−.21	.11	−.08	−.03	−.06	−.06	.08

Note: *** = $p < .001$, ** = $p < .01$, * = $p < .05$.

Table A2. The unstandardized coefficients, standard error of the unstandardized coefficients, and standardized coefficients (β) of hierarchical multiple regression models regressing *femininity/masculinity of coloring book selection* on (a) gender identity measures (from test, retest, and the average between test and retest) and (b) demographic variables (excluding sex) in Study 1. Presented results are for male participants only.

	Model 1			Model 2			Model 3			Model 4			Model 5			Model 6		
Variable	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β
Self-report Identity - Test	−.25	.25	−.17	-	-	-	-	-	-	−.25	.25	−.17	-	-	-	-	-	-
IAT - Test	−.22	.31	−.10	-	-	-	-	-	-	−.22	.32	−.10	-	-	-	-	-	-
Self-report Identity - Retest	-	-	-	.55*	.23	.35	-	-	-	-	-	-	.57*	.24	.36	-	-	-
IAT - Retest	-	-	-	−.04	.27	−.02	-	-	-	-	-	-	−.02	.27	−.01	-	-	-
Self-report Identity (average)	-	-	-	-	-	-	.25	.32	.14	-	-	-	-	-	-	.26	.33	.15
IAT (average)	-	-	-	-	-	-	−.32	.35	−.14	-	-	-	-	-	-	−.32	.35	−.14
Age	-	-	-	-	-	-	-	-	-	.00	.08	.00	.04	.07	.05	.01	.08	.02
R^2		.04			.11			.03			.04			.11			.03	

Note: *** = $p < .001$, ** = $p < .01$, * = $p < .05$.

Table A3. The unstandardized coefficients, standard error of the unstandardized coefficients, and standardized coefficients (β) of hierarchical multiple regression models regressing *femininity/masculinity of clothing worn during testing session* on (a) gender identity measures (from test, retest, and the average between test and retest) and (b) demographic variables (excluding sex) in Study 1. Presented results are for male participants only.

	Model 1			Model 2			Model 3			Model 4			Model 5			Model 6		
Variable	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β
Self-report Identity - Test	−.16	.11	−.11	-	-	-	-	-	-	−.17	.11	−.12	-	-	-	-	-	-
IAT - Test	.00	.12	.00	-	-	-	-	-	-	−.01	.12	.00	-	-	-	-	-	-
Self-report Identity - Retest	-	-	-	.08	.11	.05	-	-	-	-	-	-	.07	.11	.04	-	-	-
IAT - Retest	-	-	-	−.04	.13	−.02	-	-	-	-	-	-	−.05	.13	−.02	-	-	-
Self-report Identity (average)	-	-	-	-	-	-	−.08	.15	−.05	-	-	-	-	-	-	−.11	.15	−.06
IAT (average)	-	-	-	-	-	-	−.04	.15	−.02	-	-	-	-	-	-	−.06	.15	−.03
Age	-	-	-	-	-	-	-	-	-	−.04	.03	−.06	−.03	.03	−.04	−.04	.03	−.06
R^2		.04			.01			.01			.07			.03			.03	

Note: *** = $p < .001$, ** = $p < .01$, * = $p < .05$.

Table A4. Zero-order correlations among females for variables in Study 1. *N*'s ranged from 41 to 47.

	Variable	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**
1	Age	-	-	-	-	-	-	-	-
2	Self-report Identity - Test	−.07	-	-	-	-	-	-	-
3	Self-report Identity - Retest	.06	.07	-	-	-	-	-	-
4	Self-report Identity (average)	.00	.67***	.78***	-	-	-	-	-
5	IAT - Test	−.04	.06	−.03	.01	-	-	-	-
6	IAT - Retest	.28†	.10	.32*	.30†	.44**	-	-	-
7	IAT (average)	.10	.09	.18	.19	.87***	.82***	-	-
8	Coloring Book Selected	−.28†	.28†	.19	.31*	.21	−.01	.13	-
9	Clothing	−.45**	.02	−.02	.00	.05	−.09	−.01	.21

Note: *** = p < .001, ** = p < .01, * = p < .05.

Table A5. The unstandardized coefficients, standard error of the unstandardized coefficients, and standardized coefficients (β) of hierarchical multiple regression models regressing *femininity/masculinity of coloring book selection* on (a) gender identity measures (from test, retest, and the average between test and retest) and (b) demographic variables (excluding sex) in Study 1. Presented results are for female participants only.

	Model 1			Model 2			Model 3			Model 4			Model 5			Model 6		
Variable	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β
Self-report Identity - Test	.45†	.24	.31	-	-	-	-	-	-	.40†	.24	.27	-	-	-	-	-	-
IAT - Test	.34	.24	.16	-	-	-	-	-	-	.30	.24	.14	-	-	-	-	-	-
Self-report Identity - Retest	-	-	-	.20	.22	.12	-	-	-	-	-	-	.19	.21	.12	-	-	-
IAT - Retest	-	-	-	−.11	.30	−.05	-	-	-	-	-	-	.04	.30	.02	-	-	-
Self-report Identity (average)	-	-	-	-	-	-	.49†	.29	.28	-	-	-	-	-	-	.47	.28	.27
IAT (average)	-	-	-	-	-	-	.18	.30	.08	-	-	-	-	-	-	.21	.29	.09
Age	-	-	-	-	-	-	-	-	-	−.10	.07	−.15	−.15*	.07	−.22	−.14*	.06	−.21
R^2		.12			.02			.09			.17			.14			.19	

Note: *** = p < .001, ** = p < .01, * = p < .05.

Table A6. The unstandardized coefficients, standard error of the unstandardized coefficients, and standardized coefficients (β) of hierarchical multiple regression models regressing *femininity/masculinity of clothing worn during testing session* on (a) gender identity measures (from test, retest, and the average between test and retest) and (b) demographic variables (excluding sex) in Study 1. Presented results are for female participants only.

	Model 1			Model 2			Model 3			Model 4			Model 5			Model 6		
Variable	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β
Self-report Identity - Test	.03	.18	.02	-	-	-	-	-	-	.00	.17	.00	-	-	-	-	-	-
IAT - Test	.06	.18	.03	-	-	-	-	-	-	.04	.16	.02	-	-	-	-	-	-
Self-report Identity - Retest	-	-	-	−.01	.17	.00	-	-	-	-	-	-	.00	.16	.00	-	-	-
IAT - Retest	-	-	-	−.13	.23	−.06	-	-	-	-	-	-	.05	.22	.03	-	-	-
Self-report Identity (average)	-	-	-	-	-	-	−.01	.25	.00	-	-	-	-	-	-	.00	.23	.00
IAT (average)	-	-	-	-	-	-	−.01	.24	−.01	-	-	-	-	-	-	.07	.22	.03
Age	-	-	-	-	-	-	-	-	-	−.15**	.05	−.22	−.15**	.05	−.22	−.15*	.05	−.22
R^2		.00			.01			.00			.21			.19			.19	

Note: *** = p < .001, ** = p < .01, * = p < .05.

Results from Study 2 Disaggregated by Participant Sex

Table A7. Zero-order correlations among males for variables in Study 2. *N*'s ranged from 30 to 53.

	Variable	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**
1	Age	-	-	-	-	-	-	-	-	-
2	Self-report Identity - Test	$.45^{***}$	-	-	-	-	-	-	-	-
3	Self-report Identity - Retest	$.40^{**}$	$.52^{***}$	-	-	-	-	-	-	-
4	Self-report Identity (average)	$.49^{***}$	$.87^{***}$	.88	-	-	-	-	-	-
5	IAT - Test	−.01	−.12	.03	−.01	-	-	-	-	-
6	IAT - Retest	.15	.16	−.04	.03	$.27^{\dagger}$	-	-	-	-
7	IAT (average)	.13	.12	−.01	.02	$.75^{***}$	$.84^{***}$	-	-	-
8	Coloring Book Selected	.15	.15	.2	.21	−.02	−.01	−.01	-	-
9	Clothing - Test	−.06	$.25^{\dagger}$	.01	.12	−.13	.15	.07	.21	-
10	Clothing - Retest	.01	.23	.32	.36*	−.21	.08	−.06	.12	$.64^{***}$

Note: *** = $p < .001$, ** = $p < .01$, * = $p < .05$.

Table A8. The unstandardized coefficients, standard error of the unstandardized coefficients, and standardized coefficients (β) of hierarchical multiple regression models regressing *femininity/masculinity of coloring book selection* on (a) gender identity measures (from test, retest, and the average between test and retest) and (b) demographic variables (excluding sex) in Study 2. Presented results are for male participants only.

	Model 1			Model 2			Model 3			Model 4			Model 5			Model 6		
Variable	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β
Self-report	.21	.22	.15	-	-	-	-	-	-	.15	.25	.10	-	-	-	-	-	-
IAT - Test	−.03	.31	−.01	-	-	-	-	-	-	−.05	.32	−.02	-	-	-	-	-	-
Self-report	-	-	-	.31	.24	.22	-	-	-	-	-	-	.26	.27	.19	-	-	-
IAT - Rete	-	-	-	.03	.27	.01	-	-	-	-	-	-	.01	.28	.00	-	-	-
Self-report	-	-	-	-	-	-	.36	.27	.24	-	-	-	-	-	-	.31	.31	.20
IAT (avera	-	-	-	-	-	-	−.01	.38	.00	-	-	-	-	-	-	−.02	.39	−.01
Age	-	-	-	-	-	-	-	-	-	.05	.08	.07	.04	.08	.05	.03	.09	.04
R^2		.02			.04			.05			.03			.05			.05	

Note: *** = *p* < .001, ** = *p* < .01, * = *p* < .05.

Table A9. The unstandardized coefficients, standard error of the unstandardized coefficients, and standardized coefficients (β) of hierarchical multiple regression models regressing *femininity/masculinity of clothing worn during testing session* on (a) gender identity measures from the *same* testing session and (b) demographic variables (excluding sex) in Study 2. Average scores on gender identity measures from test and retest were used to predict the average clothing rating from test and retest. Presented results are for male participants only.

	Model 1			Model 2			Model 3			Model 4			Model 5			Model 6		
Variable	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β
Self-report	.36	.27	.18	-	-	-	-	-	-	.33*	.16	.23	-	-	-	-	-	-
IAT - Test	−.01	.38	−.03	-	-	-	-	-	-	−.03	.16	−.01	-	-	-	-	-	-
Self-report	-	-	-	.34†	.18	.22	-	-	-	-	-	-	.35†	.18	.23	-	-	-
IAT - Rete	-	-	-	.13	.18	.05	-	-	-	-	-	-	.15	.19	.06	-	-	-
Self-report	-	-	-	-	-	-	.37†	.20	.24	-	-	-	-	-	-	.44†	.22	.28
IAT (avera	-	-	-	-	-	-	.06	.21	.02	-	-	-	-	-	-	.10	.21	.04
Age	-	-	-	-	-	-	-	-	-	−.06	.04	−.08	−.03	.07	−.03	−.06	.06	−.07
R^2		.05			.11			.11			.12			.12			.14	

Note: *** = *p* < .001, ** = *p* < .01, * = *p* < .05.

Table A10. Zero-order correlations among males for variables in Study 2. *N*'s ranged from 31 to 53.

	Variable	1	2	3	4	5	6	7	8	9
1	Age	-	-	-	-	-	-	-	-	-
2	Self-report Identity - Test	−.28*	-	-	-	-	-	-	-	-
3	Self-report Identity - Retest	−.09	.46***	-	-	-	-	-	-	-
4	Self-report Identity (average)	−.23	.87***	.84***	-	-	-	-	-	-
5	IAT - Test	.29†	−.01	0.02	.01	-	-	-	-	-
6	IAT - Retest	.12	.06	−.07	.00	.37*	-	-	-	-
7	IAT (average)	.23	.03	−.07	−.02	.78***	.87***	-	-	-
8	Coloring Book Selected	−.16	.06	.12	.10	.14	.10	.07	-	-
9	Clothing - Test	−.39**	.26†	.33*	.35*	−.11	−.01	−.06	.34*	-
10	Clothing - Retest	−.04	−.02	−.01	−.02	.31†	.06	.14	.36*	.26

Note: *** = $p < .001$, ** = $p < .01$, * = $p < .05$.

Table A11. The unstandardized coefficients, standard error of the unstandardized coefficients, and standardized coefficients (β) of hierarchical multiple regression models regressing *femininity/masculinity of coloring book selection* on (a) gender identity measures (from test, retest, and the average between test and retest) and (b) demographic variables (excluding sex) in Study 2. Presented results are for female participants only.

	Model 1			Model 2			Model 3			Model 4			Model 5			Model 6		
Variable	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β
Self-report	.07	.20	.05	-	-	-	-	-	-	−.04	.20	−.03	-	-	-	-	-	-
IAT - Test	.29	.32	.13	-	-	-	-	-	-	.47	.33	.22	-	-	-	-	-	-
Self-report	-	-	-	.16	.21	.11	-	-	-	-	-	-	.13	.21	.10	-	-	-
IAT - Rete	-	-	-	.18	.24	.08	-	-	-	-	-	-	.22	.24	.10	-	-	-
Self-report	-	-	-	-	-	-	.13	.25	.09	-	-	-	-	-	-	.01	.25	.01
IAT (avera	-	-	-	-	-	-	.16	.36	.07	-	-	-	-	-	-	.31	.36	.13
Age	-	-	-	-	-	-	-	-	-	−.17†	.09	−.23	−.09	.09	−.12	−.16	.10	−.22
R^2		.02			.02			.01			.11			.05			.09	

Note: *** = p < .001, ** = p < .01, * = p < .05.

Table A12. The unstandardized coefficients, standard error of the unstandardized coefficients, and standardized coefficients (β) of hierarchical multiple regression models regressing *femininity/masculinity of clothing worn during testing session* on (a) gender identity measures from the *same* testing session and (b) demographic variables (excluding sex) in Study 2. Average scores on gender identity measures from test and retest were used to predict the average clothing rating from test and retest. Presented results are for female participants only.

	Model 1			Model 2			Model 3			Model 4			Model 5			Model 6		
Variable	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β	B	SE (B)	β
Self-report	.21	.13	.15	-	-	-	-	-	-	.16	.13	.11	-	-	-	-	-	-
IAT - Test	−.15	.20	−.06	-	-	-	-	-	-	−.05	.21	−.02	-	-	-	-	-	-
Self-report	-	-	-	−.01	.16	.00	-	-	-	-	-	-	.00	.16	.00	-	-	-
IAT - Rete	-	-	-	.06	.18	.03	-	-	-	-	-	-	.06	.18	.03	-	-	-
Self-report	-	-	-	-	-	-	.07	.18	.04	-	-	-	-	-	-	.05	.13	.03
IAT (avera	-	-	-	-	-	-	.06	.13	.03	-	-	-	-	-	-	.09	.18	.04
Age	-	-	-	-	-	-	-	-	-	−.11*	.06	−.14	−.02	.07	−.02	−.06	.06	−.07
R^2		.08			.00			.01			.15			.01			.05	

Note: *** = p < .001, ** = p < .01, * = p < .05.

Literature Review Citations

Below are the 62 articles, reports, theses/dissertations identified in our search for work using the IAT in child samples. For our literature search, we included any report containing a sample with a mean age below 12.0 years. For longitudinal research, we considered the mean age of the sample at first time point. We also included reports from samples in which the mean was not reported, but the midpoint (or weighted average) of the age range was below 12.0 years. For example, if a report indicated that twenty 11-year olds, five 12-year-olds, and 5 13-year-olds were in the sample, this study would be included in that the weighted average of the age range is (.5×10) + (.25×12) + (.25×13) = 11.25 years. The cut-off date for the search was June 15, 2016.

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32. Hauser, J. C. (2010). *Understanding explicit and implicit anti-fat attitudes and their relations to other prejudiced attitudes, controllability beliefs and social desirability in children, adolescents, and young adults* (Doctoral dissertation, Bowling Green State University).

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Appendix B

Exposure Variables

Using American Community Survey (ACS) 5-year estimates, we computed relative proportions of Black to White residents (primary analyses) and non-Black to White residents (exploratory analyses) in U.S. states and counties. See operationalization below.

Control Variables

In additional models, we statistically adjusted for state- and county-level demographic variables that may influence state-level implicit bias, as previous studies have done (Putnam, 2007; Taylor, 1998). These control variables were derived from ACS 5-year estimates from 2008 to 2012 (*state education level, state income, percentage of citizens, economic inequality, and population density)* and 2012 Presidential election results (*political orientation*; "Election 2012: Results," 2012). See operationalization below.

List and Operationalization of Variables in Bivariate Correlation and Regression Tables

66. <u>Blacks to Whites:</u> The natural log-transformed ratio of the number of Black to White residents in each locality.
67. <u>Education:</u> The percentage of residents in a locality holding a bachelor's degree or higher.
68. <u>Median Income:</u> The median household income in a locality in 2012 inflation-adjusted dollars.
69. <u>Percent Citizens</u>: The percentage of residents in a locality holding U.S. citizenship.
70. <u>Gini Coefficient</u>: Index of economic inequality in a locality (0=*complete equality*; 1=*complete inequality*).
71. <u>People Per Sq. Mile</u>: The number of residents per square-mile in a locality.
72. <u>2012 % Democrat</u>: The percentage of voters in a locality endorsing the 2012 Democratic presidential candidate.
73. <u>Non-Blacks to Whites</u>: The natural log-transformed ratio of the number of non-Black minority residents to White residents in each locality.
74. <u>Confederate State</u>: A binary variable indicating whether a state was a member of the Confederate States of America.
75. <u>Confederate * Blacks to Whites:</u> An interaction term between the binary variable indicating whether a state was a member of the Confederate States of America and the natural log-transformed ratio of the number of Black to White residents in each locality.

Bivariate Correlations

Table B1. Zero-order among predictor variables (state and county-level) used in all analyses.

Zero-Order Correlations Among Variables Predicting State-level IAT Scores.

	Predictor	1	2	3	4	5	6	7	8	9
1	Blacks to Whites	1								
2	Education	.16	1							
3	Median Income	-.02	.58***	1						
4	Percent Citizens	-.28	-.29*	-.54***	1					
5	Gini Coefficent	.64***	.27	-.21	-.37**	1				
6	People Per Sq. Mile	.35*	.67***	.54***	-.49***	.38**	1			
7	2012 % Democrat	.18	.60***	.53***	-.49***	.18	.54***	1		
8	Non-Blacks to Whites	.10	.02	.53***	-.70***	.03	.20	.37**	1	
9	Confederate State	.59***	-.27	-.39**	.02	.47***	-.09	-.25	-.16	1

Note: df= 48. **p*<.05 ***p*<.01 ****p*<.001

Zero-Order Correlations Among Variables Predicting County-level IAT Scores.

	Predictor	1	2	3	4	5	6	7	8	9
1	Blacks to Whites	1								
2	Education	-.29***	1							
3	Median Income	-.33***	.69***	1						
4	Percent Citizens	.04	-.38***	-.32***	1					
5	Gini Coefficent	.33***	.12***	-.43***	-.20***	1				
6	People Per Sq. Mile	.12***	.22***	.11**	-.36***	.23***	1			
7	2012 % Democrat	.16***	.21***	.03	-.28***	.25***	.30***	1		
8	Non-Blacks to Whites	.14***	.34***	.30***	-.74***	.12***	.32***	.35***	1	
9	Confederate State	.57***	-.30***	.32***	.10**	.18***	-.11**	-.28***	-.14***	1

Note: df = 815 for all correlations except for those with 2012% Democrat (df =782). **p*<.05 ***p*<.01 ****p*<.001

Table B2. Summary of Regression Analysis for Variables Predicting State-level IAT Scores among White Respondents

	Model 1			Model 2			Model 3			Model 4			Model 5		
Predictor	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β
Blacks to Whites	0.018	0.002	.83***	0.019	0.002	.84***	0.018	0.002	.83***	0.018	0.002	.79***	0.017	0.002	.77***
Education				0.001	0.002	.05	- 0.002	0.002	-.15	-0.001	0.002	-.12	-0.001	0.002	-.09
Median Income				0.000	0.000	-.12	0.000	0.000	.11	0.000	0.000	.09	0.000	0.000	.06
Percent Citizens				0.002	0.001	.20	0.000	0.001	.03	0.000	0.001	.03	0.000	0.001	-.02
Gini Coefficient				-0.095	0.229	-.06	0.001	0.208	.00	-0.042	0.217	-.03	-0.087	0.225	-.06
People Per Sq. Mile				0.000	0.000	.28*	0.000	0.000	.22*	0.000	0.000	0.23*	0.000	0.000	.24*
2012 % Democrat				0.000	0.000	-.12	0.000	0.000	-.04	0.000	0.000	-.04	0.000	0.000	-.05
Non-Blacks to Whites							-0.014	0.004	-.38**	-0.013	0.002	-.36**	-0.013	0.004	-.36**
Confederate State										0.005	0.007	.18	-0.005	0.015	-.18
Confederate*Blacks to Whites													0.008	0.011	.38
R^2		.690			.775			.822			.823			.823	
Change in R^2		-			.085			.047			.001			.004	

Note: IAT responses were coded such that higher scores indicated greater ingroup bias. All predictors, with the exception of Confederate State, are centered at their mean.
†*p*<.10.**p*<.05. ***p*<.01. ****p*<.00

Table B3. Summary of Regression Analysis for Variables Predicting State-level Explicit Attitude Scores among White Respondents

	Model 1			Model 2			Model 3			Model 4			Model 5		
Predictor	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β
Blacks to Whites	0.068	0.007	.83***	0.073	0.007	.90***	0.073	0.007	.90***	0.068	0.009	.83***	0.064	0.009	.79***
Education				0.004	0.005	.10	0.003	0.006	.07	0.005	0.006	.12	0.008	0.006	.19
Median Income				0.000	0.000	-.14	0.000	0.000	-.10	0.000	0.000	-.13	0.000	0.000	-.20
Percent Citizens				0.001	0.003	.05	0.001	0.004	.02	0.001	0.004	.02	-0.002	0.004	-.08
Gini Coefficient				-0.182	0.761	-.03	-.126	0.776	-.02	-0.404	0.799	-.07	-0.778	0.802	-.14
People Per Sq. Mile				0.000	0.000	.01	0.000	0.000	-.02	0.000	0.000	0.00	0.000	0.000	.01
2012 % Democrat				-0.003	0.001	-.29**	-0.003	0.001	-.27**	-0.003	0.001	-.26*	-0.003	0.001	-.29*
Non-Blacks to Whites							-0.008	0.015	-.06	-0.003	0.016	-.02	-0.003	0.015	-.02
Confederate State										0.033	0.026	.32	-0.054	0.054	-.52
Confederate*Blacks to Whites													0.071	0.038	.87†
R^2		.692			.813			.815			.822			.836	
Change in R^2		-			.121			.002			.007			.014	

Note: Explicit attitude responses were coded such that higher scores indicated greater ingroup bias. All predictors, with the exception of Confederate State, are centered at their mean.
†*p*<.10.**p*<.05. ***p*<.01. ****p*<.001.

Table B4. Summary of Regression Analysis for Variables Predicting State-level IAT Scores among Black Respondents

	Model 1			Model 2			Model 3			Model 4			Model 5		
Predictor	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β
Blacks to Whites	0.026	0.003	.76***	0.029	0.004	.83***	0.029	0.004	.83***	0.033	0.004	.95***	0.033	0.004	.96***
Education				-0.005	0.003	-.25†	- 0.005	0.003	-.30†	-0.007	0.003	-.40*	-0.007	0.003	-.41*
Median Income				0.000	0.000	.30	0.000	0.000	.35†	0.000	0.000	.40*	0.000	0.000	.42*
Percent Citizens				0.002	0.002	.14	0.001	0.002	.10	0.001	0.002	.09	0.001	0.002	.11
Gini Coefficient				0.482	0.394	.21	0.514	0.401	.22	0.722	0.403	.31†	0.751	0.403	.32†
People Per Sq. Mile				-0.001	0.000	-.26†	0.000	0.000	-.27*	0.000	0.000	-.31*	0.000	0.000	-.31*
2012 % Democrat				0.000	0.000	-.14	-0.001	0.001	-.124	-0.001	0.000	-.15	-0.001	0.001	-.14
Non-Blacks to Whites							-0.005	0.008	-.08	-0.008	0.008	-.10	-0.008	0.008	-.15
Confederate State										-0.025	0.013	-.57†	-0.018	0.028	-.41
Confederate*Blacks to Whites													-0.005	0.020	-.16
R^2		.577			.728			.730			.753			.753	
Change in R^2		-			.171			.002			.023			.000	

Note: IAT responses were coded such that higher scores indicated greater ingroup bias. All predictors, with the exception of Confederate State, are centered at their mean.
†p<.10.*p<.05. **p<.01. ***p<.001.

Table B5. Summary of Regression Analysis for Variables Predicting State-level Explicit Attitude Scores among Black Respondents

	Model 1			Model 2			Model 3			Model 4			Model 5		
Predictor	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β
Blacks to Whites	0.085	0.010	.78***	0.097	0.014	.89***	0.097	0.014	.89***	0.108	0.016	.99***	0.111	0.016	1.02***
Education				0.004	0.010	.08	0.005	0.011	.09	0.000	0.011	0.00	-0.002	0.012	-.04
Median Income				0.000	0.000	-.26	0.000	0.000	-.28	0.000	0.000	-.23	0.000	0.000	-.19
Percent Citizens				-0.006	0.006	-.17	-0.006	0.007	-.16	-0.006	0.007	-.17	-0.004	0.007	-.10
Gini Coefficient				-1.724	1.397	-.23	-1.749	1.427	-.24	-1.216	1.467	-.17	-0.896	1.522	-.12
People Per Sq. Mile				0.000	0.000	-.13	0.000	0.000	-.13	0.000	0.000	-.16	0.000	0.000	-.16
2012 % Democrat				0.001	0.002	.09	0.001	0.002	.08	0.001	0.002	.07	0.001	0.002	.08
Non-Blacks to Whites							0.004	0.029	.02	-0.006	0.028	-.04	-0.006	0.029	-.03
Confederate State										-0.064	0.047	-.46	0.011	0.028	-.08
Confederate*Blacks to Whites													-0.060	0.020	.55
R^2		.606			.651			.651			.666			.672	
Change in R^2		-			.045			.000			.015			.006	

Note: Explicit attitude responses were coded such that higher scores indicated greater ingroup bias. All predictors, with the exception of Confederate State, are centered at their mean.
†*p*<.10.**p*<.05. ***p*<.01. ****p*<.001.

Table B6.Summary of Regression Analysis for Variables Predicting County-level IAT Scores among White Respondents

	Model 1			Model 2			Model 3			Model 4			Model 5		
Predictor	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β
Blacks to Whites	0.012	0.001	.33***	0.014	0.001	.38***	0.015	0.002	.41***	0.012	0.002	.33***	0.012	0.002	.32***
Education				-0.029	0.029	-.06	- 0.021	0.030	-.05	-0.025	0.030	-.05	-0.025	0.030	-.05
Median Income				0.000	0.000	.08	0.000	0.000	.07	0.000	0.000	.09	0.000	0.000	.09
Percent Citizens				0.000	0.001	.04	0.000	0.001	-.08	0.000	0.000	.00	0.000	0.001	.00
Gini Coefficient				-0.005	0.074	.00	-0.031	0.076	-.02	-0.028	0.075	-.02	-0.028	0.075	-.02
People Per Sq. Mile				0.000	0.000	-.09	0.000	0.000	-.09	0.000	0.000	-.05	0.000	0.000	-.05
2012 % Democrat				0.000	0.000	-.10**	0.000	0.000	-.09*	0.000	0.000	-.05	0.000	0.000	-.05
Non-Blacks to Whites							-0.012	0.003	-.10†	-0.005	0.004	-.07	-0.005	0.004	-.07
Confederate State										0.011	0.005	.23*	0.011	0.005	.22*
Confederate*Blacks to Whites													0.001	0.003	.03
R^2		.108			.148			.152			.157			.158	
Change in R^2		-			.040			.004			.005			.001	

Note: IAT responses were coded such that higher scores indicated greater ingroup bias. All predictors, with the exception of Confederate State, are centered at their mean.
†*p*<.10.**p*<.05. ***p*<.01. ****p*<.001.

Table B7.Summary of Regression Analysis for Variables Predicting County-level Explicit Attitude Scores among White Respondents

	Model 1			Model 2			Model 3			Model 4			Model 5		
Predictor	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β
Blacks to Whites	0.050	0.004	.40***	0.062	0.004	.50***	0.065	0.005	.53***	0.053	0.006	.43***	0.045	0.007	.37***
Education				0.074	0.091	.05	0.100	0.091	.06	0.085	0.091	.05	0.087	0.091	.06
Median Income				0.000	0.000	.07	0.000	0.000	.06	0.000	0.000	.09	0.000	0.000	.09
Percent Citizens				0.002	0.002	.05	-0.001	0.002	-.02	0.001	0.002	.03	0.000	0.002	.01
Gini Coefficient				0.396	0.229	.09†	0.309	0.233	.07	0.325	0.231	.07	0.325	0.231	.07
People Per Sq. Mile				0.000	0.000	-.13*	0.000	0.000	-.12†	0.000	0.000	-.08	0.000	0.000	-.05
2012 % Democrat				-0.003	0.000	-.23***	-0.003	0.000	-.22***	-0.002	0.000	-.17***	-0.002	0.000	-.19***
Non-Blacks to Whites							-0.021	0.011	-.10*	-0.014	0.011	-.06	-0.014	0.011	-.07
Confederate State										0.047	0.014	.29*	0.044	0.014	.28**
Confederate*Blacks to Whites													0.020	0.010	.16†
R^2		.161			.273			.277			.287			.291	
Change in R^2		-			.112			.004			.010			.004	

Note: Explicit attitude responses were coded such that higher scores indicated greater ingroup bias. All predictors, with the exception of Confederate State, are centered at their mean.
†*p*<.10.**p*<.05. ***p*<.01. ****p*<.001.

Table B8. Summary of Regression Analysis for Variables Predicting County-level IAT Scores among Black Respondents

	Model 1			Model 2			Model 3			Model 4			Model 5		
Predictor	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β
Blacks to Whites	0.025	0.002	$.38^{***}$	0.023	0.003	$.34^{***}$	0.023	0.003	$.35^{***}$	0.025	0.003	$.37^{***}$	0.020	0.004	$.30^{***}$
Education				-0.148	0.052	$-.17^{**}$	- 0.145	0.053	$-.17^{**}$	-0.144	0.053	$-.17^{**}$	-0.143	0.053	$-.17^{**}$
Median Income				0.000	0.000	.05	0.000	0.000	.05	0.000	0.000	.05	0.000	0.000	.05
Percent Citizens				0.001	0.001	.06	0.001	0.001	.05	0.001	0.001	.04	0.001	0.001	.03
Gini Coefficient				0.285	0.132	$.11^{*}$	0.276	0.135	$.11^{*}$	0.274	0.135	$.11^{*}$	0.274	0.134	$.11^{*}$
People Per Sq. Mile				0.000	0.000	-.02	0.000	0.000	-.03	0.000	0.000	-.04	0.000	0.000	-.01
2012 % Democrat				-0.001	0.000	$-.14^{**}$	-0.001	0.000	$-.13^{***}$	-0.001	0.000	$-.14^{***}$	-0.001	0.000	$-.17^{***}$
Non-Blacks to Whites							-0.002	0.006	-.02	-0.003	0.006	-.03	-0.003	0.006	-.03
Confederate State										-0.005	0.008	-.06	-0.006	0.008	-.07
Confederate*Blacks to Whites													0.011	0.006	$.17^{\dagger}$
R^2		.142			.208			.208			.208			.225	
Change in R^2		-			.054			.000			.000			.001	

Note: IAT responses were coded such that higher scores indicated greater ingroup bias. All predictors, with the exception of Confederate State, are centered at their mean.
†p<.10.*p<.05. **p<.01. ***p<.001.

Table B9.Summary of Regression Analysis for Variables Predicting County-level Explicit Attitude Scores among Black Respondents

	Model 1			Model 2			Model 3			Model 4			Model 5		
Predictor	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β	*B*	*SE B*	β
Blacks to Whites	0.061	0.006	.35***	0.060	0.007	.34***	0.063	0.007	.36***	0.064	0.009	.37***	0.067	0.011	.39***
Education				0.077	0.140	.04	0.107	0.141	.05	0.108	0.142	.05	-0.107	0.142	.05
Median Income				0.000	0.000	-.05	0.000	0.000	-.06	0.000	0.000	-.06	0.000	0.000	.-.06
Percent Citizens				0.006	0.002	.10*	0.002	0.003	.04	0.002	0.003	.04	0.002	0.003	.04
Gini Coefficient				-.119	0.355	-.02	-.223	.360	-.03	-0.224	0.361	-.03	-.224	0.361	-.03
People Per Sq. Mile				0.000	0.000	.02	0.000	0.000	.02	0.000	0.000	.02	0.000	0.000	.01
2012 % Democrat				0.000	0.001	.01	0.000	0.001	.02	0.000	0.001	.01	0.000	0.001	.02
Non-Blacks to Whites							-0.025	0.016	-.08	-0.026	0.017	-.09	-0.026	0.017	-.09
Confederate State										-0.004	0.022	-.02	-0.003	0.022	-.01
Confederate*Blacks to Whites													-0.008	0.016	-.04
R^2		.124			.132			.135			.135			.135	
Change in R^2		-			.008			.003			.000			.000	

Note: Explicit attitude responses were coded such that higher scores indicated greater ingroup bias. All predictors, with the exception of Confederate State, are centered at their mean.
†p<.10.*p<.05. **p<.01. ***p<.001.

Table B10. Demographic characteristics of White and Black respondents from Project Implicit public use dataset (Years: 2008-2012)

Characteristic	Total (*N* = 893,387) *N*	%	White Respondents (*n* = 759,755) *N*	%	Black Respondents (*n*=133,632) *N*	%
Gender						
Female	525,346	58.80	436,620	57.47	88,726	66.40
Male	363,984	40.74	319,655	42.07	44,329	33.17
Missing/Other	4,057	0.45	3,480	0.46	577	0.43
Age in Years (M±SD)	27.65 ± 11.95		27.37 ± 12.03		29.30 ± 11.36	
Education						
Less than High School Graduate	122,343	13.69	108,609	14.30	13,743	10.28
High School Graduate	79,042	8.85	67,418	8.87	11,624	8.70
Some College/College Graduate	525,782	58.85	437,325	57.56	88,457	66.19
Some Graduate School/Advanced Degree	159,914	17.90	141,116	18.57	18,798	14.07
Missing	6,316	0.71	5,287	0.70	1,019	0.76
Political Identification (M±SD)	4.31 ± 1.65		4.28 ± 1.68		4.67 ± 1.47	
Religiosity						
Not at all Religious	94,013	10.52	85,162	11.21	8,851	6.62
Slightly Religious	229,113	25.65	201,099	26.47	28,014	20.96
Moderately Religious	250,429	28.03	198,171	26.08	52,258	39.10
Strongly Religious	114,151	12.78	83,828	11.03	30,323	22.69
Missing	205,681	23.02	191,495	25.20	14,195	10.62
Reason for Visiting Project Implicit Website						
Assignment for School	357,090	39.97	293,673	38.65	63,417	47.46
Recommendation of Teacher	46,406	5.19	38,022	5.00	8,384	6.27
Assignment for Work	18,155	2.03	14,534	1.91	3,621	2.71
Recommendation of Friend	23,496	2.63	20,795	2.74	2,701	2.02
Link from Media Site	22,958	2.57	20,933	2.76	2,025	1.52
Other	164,600	18.42	144,246	18.99	20,354	15.23
Missing	260,682	29.18	227,552	29.95	33,130	24.79

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