Relative exposure to negative economic shocks, racial animus, and voting [*]

Daniel B. Jones[1], Erica Owen[1], and Rena Sung[2]

[1] *University of Pittsburgh*
[2] *Kyung Hee University*

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Abstract

In this paper, we examine how relative exposure to negative local economic shocks across racial groups impacts racial animus and voting. Informed by group position theories of racism, we explore how the relative distribution of those shocks along racial lines exacerbates racial animus. We examine this in the context of the China shock. Using data from U.S. commuting zones between 2000 and 2020, we measure relative exposure as the gap in import exposure between white and Black workers. We show that negative economic shocks that disproportionately affect white workers relative to Black workers lead to increased expressions of anti-Black racial animus and also increase the Republican presidential vote share, even when controlling for the overall level of import exposure. Taken together, these findings suggest that it is economic decline *relative* to another group that generates racial animus and outwardly racist behavior, as well as influencing political behavior.

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1 Introduction

Across the U.S. and other advanced economies, we see increasing support for populist and far-right parties. A substantial literature links these political trends to significant structural changes in the economy in recent decades driven by increasing globalization and automation (e.g. Autor et al. 2020; Baccini and Weymouth 2021; Broz, Frieden, and Weymouth 2021a; Colantone and Stanig 2018b; Walter 2021). While both of these phenomena benefit some workers and harm others, together, globalization and automation have contributed to the decline in manufacturing jobs (e.g. Acemoglu et al. 2014; Autor and Dorn 2013; Pierce and Schott 2016) and of middle-skill, middle-class jobs (Acemoglu and Autor 2011). Notably, these shocks have not been distributed equally across space: certain regions have been much more negatively affected than others (e.g. Autor, Dorn, and Hanson 2016).

However, the distributional impacts of these shocks are also not felt equally *within* labor markets. In particular, given racial and ethnic occupational and industrial segregation in U.S. labor markets (Ard and Smiley 2022; Del Río and Alonso-Villar 2015), a localized economic shock will not equally impact the labor market outcomes of all racial groups within the area (Kahn, Oldenski, and Park 2022). For instance, in an area with an industry that is highly exposed to rising import competition or automation, it may predominantly be white and not Black workers employed in that industry. Thus, a negative economic shock and its effects become a racialized phenomenon with implications for politics.

Recent work has begun to consider the role of race in shaping the political economy of negative economic shocks, in particular emphasizing the role of group status threat. For instance, Baccini and Weymouth (2021) find that the effect of layoffs on voters' behavior differs by who is affected: white manufacturing layoffs reduced Democratic party vote share, while non-white manufacturing layoffs increased support for the Democratic party. The authors suggest that manufacturing layoffs trigger group status threat among white workers as a potential channel through which these effects occur.[1] In a similar vein, Mutz (2018) argues that changes in dominant group status threat,

[1]Indeed, their individual-level analysis shows that white and non-white voters respond differently to manufacturing job losses. White workers affected by manufacturing layoffs were less likely to vote for Clinton than non-white workers (558).

including along racial lines, from 2012 to 2016 were more directly associated with shifts towards support for Republican candidates than were changes in individual's economic standing alone.

Yet important questions remain about whether and how economic shocks trigger group status threats, and what the political consequences are. One implication of the argument invoking threat to group position as an explanation for the impacts of a local economic shock is that what may matter most in driving changes in attitudes and behavior is the exposure of one group *relative* to another. To examine this possibility, we bring together the literature on the "China shock," which has documented the effects of local labor market shocks on individuals' attitudes and voting behaviors, with the literature on how the perceived threat to one's own racial/ethnic group's relative position could be a potential driver of racial animus (e.g., Bobo and Hutchings 1996; Quillian 1995).

We propose that *relative* exposure to a local negative economic shock impacts expressions of racial animus, and, ultimately voting patterns. By relative exposure, we mean white workers' exposure to economic shocks relative to Black workers in the same area. We argue that a negative economic shock can exacerbate racial animus when that negative economic shock disproportionately affects one group. In doing so, we provide some of the first direct evidence linking the "China shock" to racial attitudes and animus. We also consider how these relative economic shocks influence partisan preferences and thus voting patterns. Drawing on work that documents that racial rhetoric by the Republican Party increased during the Obama years and also that – perhaps as a consequence – voters' racial resentment and animus became more predictive of support for Republican candidates during the same period (e.g., Tesler 2016), we anticipate that greater relative exposure will be associated with an increase in Republican Party presidential vote share.

Our empirical approach proceeds as follows. Consistent with previous studies, we take advantage of the rise of China in the global economy to generate variation across areas in local labor market conditions in the U.S. Starting in 2000, imports from China increased drastically with the normalization of trade relations via the U.S.-China Relations Act and China's entry into the World Trade Organization in 2001. The China shock can be considered a "natural experiment" in that places impacted are "as though" randomly assigned because local industrial specialization was determined well before the sudden rise of Chinese imports. We draw on two dimensions of racial animus, capturing anti-Black attitudes at the individual level with measures of implicit bias test scores from the Harvard Implicit Association Test (IAT) and feeling thermometers (also from the IAT). At the commuting zone level, we look at anti-Black hate crimes as another measure of racial animus. Finally, we link these relative economic shocks to the two-party vote share in U.S. presidential voting between 2008 and 2020.

We find support for our hypotheses. Across two different datasets and conceptualizations of racial animus, we find that higher import exposure among white workers relative to Black workers is associated with greater anti-Black racial prejudice. We also find that a relative gap in exposure to imports for white workers compared to Black workers leads to a decrease (increase) in the Democratic (Republican) party presidential vote share. Further, the relationship between the relative shocks and vote share becomes stronger and more negative over time. All of our results are robust to controlling for the overall (i.e. absolute) level of import exposure. Thus, our findings demonstrate that it is the relative shock that explains variation in the attitudinal and behavioral outcomes, rather than the absolute level of the import shock.

This paper makes several contributions. First, we shift the focus from the absolute level of local economic shocks to consider their relative impact on racial and ethnic groups. Our findings suggest that white racial animus will be more severe if the exposure is greater for white workers than for Black workers, even though import shocks can impact both groups. Second, we investigate a range of outcomes associated with racial animus and show that they are impacted by relative economic shocks, including racial resentment, implicit and explicit anti-Black racism, and anti-Black hate crimes. By examining these diverse outcomes, and leveraging the advantages and disadvantages of different measures of animus, we offer a broad understanding of how relative exposures to import shocks influence various aspects of political outcomes and the underlying racial dynamics that drive changes in attitudes and behaviors. Finally, in contrast to existing literature on economic shocks and U.S. presidential elections, we demonstrate that it is relative exposure, rather than overall exposure, that is associated with an increase in support for the Republican party beginning in 2012.

2 Related literature

In this paper, we incorporate insights from the racial animus literature in American politics into the literature on the political effects of negative economic shocks.

A large body of research examines the political and economic effects of the China shock, both as a causal identification strategy and to understand the political economy effects of globalization. In seminal work, Autor, Dorn, and Hanson (2013) find that from 2000 on, areas in the U.S. more 'exposed' to Chinese import competition experienced higher unemployment, lower labor force participation, and lower wages. These economic effects are persistent at least through 2019 (Autor, Dorn, and Hanson 2021). Many studies have demonstrated the influence of these local economic shocks on political attitudes and behavior. For instance, these shocks increase the electoral success of either more extreme Democratic or Republican representatives (depending on local partisan composition), while removing moderate representatives from the office (Autor et al. 2020). Accounting for both winners and losers from trade exposure, Jensen, Quinn, and Weymouth (2017) find that more workers in low-skill manufacturing are associated with a lower presidential vote share for the incumbent party, whereas more high-skilled workers in (high-wage) manufacturing and services are associated with an increase in the incumbent party vote share. Outside of the U.S., Chinese import-exposed areas in the UK are associated with higher Brexit vote shares (Colantone and Stanig 2018a) and an increase in support for far-right parties in Europe (Colantone and Stanig 2018b). Others have looked at deindustrialization and economic decline more generally. For instance, Broz, Frieden, and Weymouth (2021b) find that a decline in manufacturing employment share led to greater support for the Republican presidential candidate between 2012 and 2016. Scholars continue to debate whether the impact of these economic shocks on voting is due to economic impacts (i.e. "left-behind" hypothesis) or group status threat (e.g. Baccini and Weymouth 2021; Mutz 2018).

To better understand how race can shape the political effects of economic shocks on politics, including the role of group status threat, it is useful to consider a large body of work in American politics that examines the determinants of racial prejudice and its impacts on politics (Bobo 1999; Jardina 2019). Traditionally, scholars took an individual-oriented and psychological approach(Allport, Clark, and Pettigrew 1954), where outgroup hostility is a "stable" characteristic, shaped by negative stereotypes and feelings at an individual level. In contrast, other scholars emphasize the role of group position in understanding racial prejudice. This approach stems from group position theories in which white hostility and prejudice arise in response to the perception that other racial/ethnic groups are challenging their status or resources (Blumer 1958; Bobo and Hutchings 1996). Scholars from this tradition view racism as group competition and argue that feelings of isolation and threat stem from long-term experiences that a racial group has faced in society (Bobo and Hutchings 1996). Stratification economics directly applies these group position theories to analyze economic inequality across different groups based on race, ethnicity, and gender (Darity Jr 2022). Jardina (2019) provides empirical support for the group position theory in the context of Trump's election, arguing that when whites perceive threats, they become more aware of their racial identity; this can increase anti-Black animus and influence political behavior, leading white Americans to act in ways that seek to preserve racial hierarchy.

Recent work has begun to explore various dimensions of the intersections of race, racial attitudes, economic shocks, and voting outcomes. Baccini and Weymouth (2021) demonstrate that white manufacturing layoffs generated greater support for the Republican party in the 2016 election, while job losses for non-white workers were associated with greater support for the Democratic party. In their study, white group status threat is the mechanism posited to lead to greater support for Trump and the Republican party, although it is not directly examined in the article. Looking at attitudes instead of voting, Ferrara (2023) finds that general (i.e. non-race specific) exposure to China import shocks drives negative feelings toward ethnic minorities and positive feelings toward in-group members for white male respondents using ANES data. Interestingly, Ferrara finds a negative association between general import exposure at the local level on attitudes toward Hispanics and Asians, and no effect on attitudes toward Black Americans. Ballard-Rosa et al. (2021) argue large that negative economic shocks generate more authoritarian values because historically dominant groups experience negative effects on social identity. Using survey data from the U.S., they find that those regions that are both more substantially exposed to the China shock and that are more diverse have more authoritarian values. In an analysis of attitudes and voting in the U.K., Green, Hellwig, and Fieldhouse (2022) argue that votes for populism are based on group-based economics; voters assess the economy by comparing their group or community to "others." Analyzing survey data of the intention to vote for Brexit, they find that perceptions that the (racial/ethnic) out-group is doing better are positively associated with a greater likelihood of voting for Brexit (an absolute effect) and that perceptions that the (racial/ethnic) in-group is doing better relative to the out-group are associated with a lower likelihood of voting for Brexit.[2]

As we will lay out in the next section, this paper offers a theory of how relative group position – conceptualized and measured as the relative distribution of exposure to negative economic shocks between racial groups at the local level – influences racial animus and ultimately presidential voting.

3 Theory

In this section, we present our theoretical framework about how and when a local labor market shock may influence racial attitudes and voting. To summarize, we build our argument as follows. First, we introduce the concept of relative racial labor market shocks to describe how these shocks may differentially impact different groups within an area. Second, through the lens of group position theories, we outline how we expect relative labor market shocks to impact and exacerbate racial animus – and why it is important to consider the distinct effects of relative rather than absolute shocks. Finally, we link these expected dynamics to downstream impacts on voting behavior.

3.1 Relative economic shocks

A key innovation of our theory is the focus on how labor market shocks (e.g. import exposure, robot exposure, deindustrialization) affect different groups of workers in relative terms, rather than the absolute effects of said negative economic shock. We build our argument around one prominent case, the China import shock. By absolute shock, we refer to standard measures of exposure to Chinese imports at the local level, most commonly measured for workers overall, and in a few circumstances, by group. By contrast, a relative economic shock captures the difference in exposure between groups. Specifically, in this paper we consider the exposure of white workers relative to Black workers.

[2] In a separate analysis, they consider the role of geography, looking at economic perceptions of the gap between the local economy and the London economy.

We present a numerical example in Table 1 to illustrate how the racial composition of exposure to shocks can vary across locales and the difference between absolute and relative shocks. In this simple example, there are two counties, A and B, and two sectors, textiles and services. Textiles face a 40% increase in imports from China while services are unaffected. In County A, 10 percent of Black workers are employed in textiles and 90 percent are in services, while 60 percent of white workers are employed in textiles and 40 percent are services. The level of exposure to Black workers in County A is 0.04 ($= 10 * .4 + 90 * 0$)) and for white workers is 0.24 ($= 60 * .4 + 40 * 0$)). White workers are more exposed to the negative shock relative to Black workers. In County B, where white and Black workers are employed in textiles and services at the same rate, there is no difference in the level of exposure between groups. Thus, in absolute terms, white workers are exposed to the shock at the same level in both counties, but they are relatively more exposed in County A.

Table 1: Numerical example of relative import shock

	County A			County B		
	% Textiles	% Services	Exposure	% Textiles	% Services	Exposure
Black	10	90	0.04	60	40	0.24
White	60	40	0.24	60	40	0.24
Exposure Gap			0.20			0

3.2 Relative shocks and racial animus

We draw on group position theories (Blumer 1958; Bobo and Hutchings 1996) to form predictions about how changes in racial attitudes may differ in labor markets with different *relative* economic shocks – like County A and County B in the example above. The primary insight from group position theory is that a historically dominant group's racial animus is generated or exacerbated by the perception that another group is challenging their status. Although perceived challenges could emerge in several ways (e.g., population size, political power), we focus on economic challenges to perceived group status, operationalized here as a relative economic shock. Specifically, we posit that a gap in exposure – with white workers more exposed to an import shock than Black workers – leads to an increase in expressions of white anti-Black racism. While others have posited a link between relative economic decline and status threat, we address this explicitly – both in our theory and empirics.

We conceptualize racial animus by drawing on the American politics literature. Much previous work has focused on *racial resentment* as the primary measure of racial prejudice. In particular, the racial resentment scale, developed by Kinder and Sanders (1996) in the 1980s, has served as the dominant measure of racism in political science. This scale measures whether people believe racial inequality arises from individual attributes and behaviors or structural discrimination. However, recent work by Peyton and Huber (2021) and others, suggests that a potential limitation of the racial resentment concept is that it captures two distinct but related values: prejudicial racial attitudes and moral values related to social conservatism (1829). This contrasts with explicit measures of anti-Black racial prejudice which are delinked from policy associations and thus isolate more directly the willingness to discriminate based on race. As discussed below in the measurement section, we utilize both explicit and implicit measures of anti-Black racial animus.[3]

We expect that when white workers are relatively more affected by the China shock than Black workers in the same locale, expressions of anti-Black racism will be exacerbated. Thus, in reference to Table 1, the standard China shock literature would anticipate effects on attitudes and voting in both counties. In contrast, we expect to find an increase in animus only in County A – or at least that the effect of the shock would be more pronounced in County A. Our, therefore, is among the first to show that economic relative group position directly exacerbates racial animus.

3.3 Impact on presidential election outcomes

Finally, we examine variation in presidential voting outcomes as a function of the racial gap in local import exposure. Although partisan preference is not inherently linked to racial attitudes and perceived group position threat, it can become linked via rhetoric and position-taking of political entrepreneurs.

Political entrepreneurs can activate feelings of threat (Blumer 1958; Bobo and Hutchings 1996) and embolden those with prejudices to act in line with those beliefs (e.g. Newman et al. 2021). Previous research has shown that in the wake of the election of Obama, some politicians such as Trump used strong racial rhetoric to gather political support (Cramer 2020; Tesler 2016; Tesler

[3]We do not look at a racial resentment index due to data limitations.

and Sears 2010). For instance, the 2012 campaign featured the emergence of Trump as a political figure directly invoking identity in questioning Obama's birthplace. Indeed, the rightward shift of the Republican Party started during the Obama era and Trump accelerated this movement (Hopkins 2022). Notably, scholars have documented an increase in white identity (Jardina 2019) and a growing link between racial attitudes, preferences on non-racial policies, and views of the presidency during the Obama administration (Parker and Barreto 2014; Tesler 2012, 2016). Research suggests that this is because the Obama presidency generated a threat to the dominance of white Americans (for review, see Stephens-Dougan 2021).

In the U.S. context, racial attitudes are a significant predictor of policy and political preferences (e.g. Valentino, Neuner, and Vandenbroek 2018).[4] This is likely to have impacts on voting patterns because of the ability of political entrepreneurs to play on concerns around relative group position (e.g. Cramer 2020). Further, white voters experiencing threat due to relative economic decline will also be more likely to support parties and politicians that are expected to implement policies that preserve the racial hierarchy and status quo (Baccini and Weymouth 2021; Jardina 2019; Mutz 2018).[5]

Overall, we anticipate that a greater relative threat to white workers will decrease (increase) the Democratic (Republican) party vote share. One implication in the shift in the rhetoric and policy positions of the Democratic and Republican parties over time is that the link between relative import exposure to perceived group position threat may not be constant over time. Rather, relative import shocks may be increasingly linked to partisan preferences via rhetoric and position-taking of elites during the Obama administration and in subsequent years.

[4]For example, the presence of an African American president has had spillover effects of racial resentment to other policy areas that Obama emphasized such as health policy (Henderson and Hillygus 2011) and climate change (Benegal 2018). Abrajano and Hajnal (2015) demonstrate that racial resentment is positively correlated with anti-immigration sentiment. Hooghe and Dassonneville (2018) also argue that general racial resentment and anti-immigration sentiments are important determinants of a Trump vote.

[5]This is consistent with intuition of stratification economics, which suggests that when "the subordinate group is catching up–or has caught up or moved ahead–leads to an intensification of the desire to restore the subordinate group to its proper place" (Darity Jr 2022, 11).

3.4 Empirical expectations

Table 2 summarizes our empirical expectations. We hypothesize that where there is a large gap in exposure that negatively affects white workers relative to Black workers (Cell B), this will be associated with greater racial animus. We also expect greater electoral success for the Republican Party in presidential elections in those locales, compared to locales that are not exposed (Cell A), or both groups are equally exposed (Cell D).

To some extent, Table 2 also illustrates the difference between our theory and those focused on absolute shocks. The standard finding in the literature is that any negative shock to the local market (overall) leads white voters to vote in favor of right-wing parties. Thus, the literature predicts that where (white) workers are exposed, we should expect a greater impact on voting, regardless of differences in exposure between groups (i.e. in both Cells B and D). When white people lose manufacturing jobs due to import shocks, for example, they change their behavior in ways that support right-wing parties. Notably, the implicit or explicit mechanism in many cases is the focus on the dominant group status threat. What distinguishes our argument from the existing literature is that when the level of exposure to the negative shock is the same for both Black and white workers, we should not expect to see a change in the level of racial animus or voting patterns (Cell D). Cell A represents those locales that are not exposed to the import shock.

Table 2: Relative exposure, racial animus, and voting

	White Workers Not Exposed	White Workers Exposed
Black Workers Not Exposed	No change (A)	Exacerbate expressions of Anti-Black racism Increased voting for Republican Party (B)
Black Workers Exposed	(Few observations) (C)	No change (D)

An implicit assumption of our argument is that it is white worker losses relative to the experience of “nearby“ Black workers that generate greater racial animus. We implicitly assume that threat to group position is made salient when there are localized losses to white people but not to Black people in the same area. According to this logic, losses to White people in Alabama would not be “counteracted by” losses to Black people in Oregon. Put differently, the group position threat with distant regions is not salient to citizens or political elites. A further implication is that local relative group position threat is only salient where there are members of both groups living in the locale as we discuss further below.

4 Empirical Approach

In this section, we outline our empirical approach. For causal identification, we leverage the "China shock": the global rise in Chinese exports in some industries, which accelerated in the 2000s. The basic idea, which we outline briefly here, is that China's rise in the global economy occurred rapidly, but that it also impacted only a subset of industries (largely in manufacturing). As such, when focusing on local economies in the importing countries, only areas that were relatively specialized in the same industries at the beginning of China's rise were impacted. The localized "shock" is typically measured as the interaction of local industrial composition in a base year and year-by-industry changes in nationwide imports from China. Thus, the "shock" creates local variation in exposure to imports, and in turn, in exposure to disruption to the local economy – variation that, by virtue of not being measured based on actual year-to-year *localized* changes in employment, is otherwise exogenous to local demographic, economic, or political changes.

We follow the standard empirical approach in the literature on the China shock in several respects. First, we capture changes in outcomes in some year y relative to a base year. Our outcome measures are regressed on changes in localized import exposure stemming from Chinese imports during the same time period. Second, as in some other work (e.g. Autor, Dorn, and Hanson 2013; Autor et al. 2016; Baccini and Weymouth 2021), we take the year 2000 as our base year, which is early in China's entry into the global economy. Two major changes in trade policy occurred around that time. In 2000, the U.S.-China Relations Act was passed, granting China permanent normal trade relations status with the United States. Subsequently, in 2001, China was granted entry into the WTO. Further, a simple examination of the total value of exports from China to the U.S. shows a clear acceleration after 2000. To provide some sense of the acceleration: the value of exports from China to the U.S. increased by 44 billion dollars from 1992 to 2000, but increased a further *200* billion dollars in the years from 2000 to 2008. The growth has continued: the value of exports from China to the U.S. in 2021 was roughly eleven times what it was in the year 2000.[6] Finally, like many other studies, the "localized area" we focus on is the commuting zone – a collection of counties roughly coinciding with metropolitan statistical areas, but with the benefits of being defined for all counties in the U.S. and also being defined explicitly based on local labor markets. There are 741 commuting zones.

Our analysis highlights how a localized import shock will not equally impact all racial groups within the area. Thus, unlike most previous work, we construct distinct measures of localized import exposure by race group.[7] For each area, we construct a distinct measure of white import exposure and Black import exposure. We describe the data used in these measures in more detail in the following section. As we are primarily interested in the impacts of a *relative* economic shock which may in turn create a perceived threat to group position to the historically dominant group, we construct a measure of the white relative import exposure, measured as *[White Import Exposure - [Other Group] Import Exposure]*. Specifically, we focus on the relative gap in exposure between white and Black workers. When this number is positive, it means that local white workers are disproportionately exposed relative to the other group to the rise in imports from China. When this number is zero, it means that both groups are equally exposed (or that no one is very exposed). When this number is negative, white workers are less exposed.[8]

Per our theoretical framework, we anticipate that racial animus, as well as votes in favor of the Republican Party, will be greater when *[White Import Exposure - Black Import Exposure]* is positive. When *[White Import Exposure - Black Import Exposure]* is zero (meaning import exposure is either positive but equally experienced across groups or negligible) or negative, there is no (perceived) salient threat to group position; thus we would expect no shift in behavior or attitudes. As such, the gap between white import exposure and Black workers' import exposure is

[6]Source: World Bank, World Integrated Trade Solution.

[7]A recent working paper from Kahn, Oldenski, and Park 2022 also constructs group-specific exposure to import shocks and documents subsequent group-specific impacts on labor market outcomes. In contrast, our focus is on the relative import exposure measure that we construct based on the group-specific measures.

[8]Note that the gap measure could also be largely positive or negative if the local is extremely homogeneous in terms of the population share. We address this further below.

our main independent variable.

Note that the same basic features that create some exogeneity in the impact of the generic "China shock" on localized economic outcomes operate here as well; the relative shifts in group-specific outcomes that we will measure are based on the interaction of (1) pre-determined (race-specific) industrial composition of an area as of the year 2000 and (2) the post-2000 rise of China in some industries in the global economy. We elaborate on both of these features in the next section.

With all of that in mind, our empirical analysis draws on imports data, Census data, and a variety of outcome measures from the years 2000 to 2020. We estimate models of the following form:

$$y_{ct} = \beta[Wht.I.E. - Blk.I.E.]_{ct} + \delta_t + \gamma_c + \epsilon_{cy}$$

where *[Wht. I.E. - Blk. I.E.]* is the gap between white and Black Import Exposure in commuting zone c in a given year t relative to the year 2000 – and is equal to zero in the year 2000. y_{ct} is an outcome measure (measures of racial attitudes, presidential vote share, etc.) in that commuting zone and year. δ_t are year-fixed effects; γ_c are commuting zone fixed effects. We therefore capture within-commuting zone changes in our outcomes of interest as a function of within-commuting zone changes in *[Wht. I.E. - Blk. I.E.]* while also controlling for any year-specific shocks to the outcomes.

One concern in our setting is: if racial gaps in import exposure (*[Wht. I.E. - Blk. I.E.]*) are correlated with overall levels of import exposure, then it is less clear that we are capturing a substantially different phenomenon from papers focusing only on the overall import exposure. As such, in our key specification of interest, we present results controlling for both the gap and the level of import exposure:

$$y_{ct} = \beta_1[Wht.I.E. - Blk.I.E.]_{ct} + \beta_2 I.E._{ct} + \delta_t + \gamma_c + \epsilon_{cy}$$

$I.E._{ct}$ captures the generic overall import exposure in commuting zone c in year t as more typically constructed. In that model, β_1 remains the primary coefficient of interest, as it captures the *distinct* effect of a difference in relative exposure of imports experienced by white workers, in this case holding fixed the overall level of exposure. We also present models that are a variation on this, where instead of adding a control for overall import exposure, we add a control specifically for the level of white import exposure.

In our main analyses, we restrict the sample to commuting zones above the median of the Black population share. This drops commuting zones with very low Black population shares (less than approximately 3 percent). Despite dropping half of all commuting zones, we are dropping only roughly 13% of the U.S. population. We do this for two reasons. First, our theoretical framework puts forth that *salient local* perceived threat to group position drives changes in white workers' attitudes and behaviors; if the Black population is very small, any differential import exposure is unlikely to be sufficiently salient to drive changes. Second, the construction of our measures (discussed in detail in the next section) are based on a sample of the Census data. Thus, when the Black population share is small, our measure is less likely to reliably capture the phenomenon we aim to capture. To document that our results are not driven by this decision, in the appendix, we present similar results when using the full sample. Our results are also similar when using the full sample with regressions weighted by Black population shares as an alternative approach to reducing the influence of observations with very low Black population shares.

We also account for two other concerns in robustness tests in the appendix. First, one concern is that time-varying features of commuting zones may evolve in parallel with our primary measures of interest, and it may be that those other features drive our results. To address this, for each of our analyses, we include specifications that add a small set of relevant time-varying controls. The first addresses the possibility that import exposure is compensated and counteracted by shifts to non-tradable industries. We construct "Bartik shock" measures to account for changes in employment in industries not impacted by Chinese imports (Bartik 1991). The construction of that measure is similar to the (non-race specific) import exposure measure; we take the base year of 2000 and measure local workers' industrial composition in all industries *not* represented amongst those with any imports from China (i.e. largely non-manufacturing industries). We then interact that local base year industrial composition with nationwide growth in employment in each industry. Additionally, we include time-varying controls for: percent of the population who are Black, percent of the population who are foreign-born, and percent of employment in manufacturing. Results are generally robust to the inclusion of this battery of controls. One concern with this approach is that some controls may evolve as a response to treatment, introducing bias in our estimate of the impact of our main variable of interest. Our next robustness test is immune to that concern.

A different type of concern is that areas that ultimately experience larger white-Black gaps in import exposure differ in the base year (2000) in some important ways relative to areas with smaller gaps. [9] Our specifications include commuting zone fixed effects, so we do not in fact require "more" vs. "less" treated areas to be similar in the pre-treatment period; instead our strategy assumes that treatment is exogenous conditional on fixed effects and controlling for overall import exposure. However, commuting zone fixed effects can only account for time-invariant differences across commuting zones. To allow for the possibility that areas with different base-year characteristics may evolve differently with regards to our key outcome measures (a phenomenon that would not be captured by commuting zone fixed effects), we include some specifications where we interact a set of base-year characteristics with linear time trends. Specifically, the base-year controls we include are: share of population that is college educated, share of population that is foreign born, share of employment in manufacturing, a Gini coefficient to capture inequality, and share of the population that is Black. Those are all measured in the year 2000. We also include a control for the commuting-zone level vote share for George Wallace in 1968 as a proxy for pre-existing anti-Black racial animus. Our results are robust to this approach as well.

5 Data and measurement

In this section, we discuss our independent variable, the gap in local exposure to imports between white workers and Black workers. We then discuss three different measures of racial animus. Finally, we describe the presidential election voting data. Our analyses cover the United States from 2000 to 2020.

5.1 Race-Specific Localized Economic Shocks

We first explain the construction of our race-specific measures of localized exposure to imports, which in turn allows for the construction of our main independent variable: *[White Import Exposure - Black Import Exposure]*. To calculate localized (and race-specific) exposure to import

[9] See Table A1.

penetration, we require two types of data: first, the (nationwide) year-to-year industry-specific changes in imports from China and, second, the industrial composition of workers (by race) in each commuting zone in a base year, which we take to be the year 2000.

For the first, we draw on imports data from Schott (2008), who obtained the data from the Census Bureau. The data capture the dollar value of imports to the U.S. from China by year and industry. The Census Bureau describes this figure as "the Customs and Border Protection-appraised value of merchandise—generally, the price paid for merchandise for export to the United States."[10] We use these data to measure industry-specific changes in imports from China to the U.S. in any given year relative to the year 2000. Specifically, we calculate: $\Delta M_{tk} = (M_{tk} - M_{2000k})/M_{2000k}$, where M_{tk} is the dollar value of imports to the U.S. from China in industry k and year t. Although this trade data is available at a relatively disaggregated level (using SIC codes), to merge these data with some of the other data that we work with, we follow the industry coding of Autor, Dorn, and Hanson (2019) to aggregate to a smaller number of industry codes corresponding to those reported in the Census. Ultimately, we calculate changes over time in imports to the U.S. for 80 unique industries.[11]

Next, to calculate the expected impact of import penetration in local labor markets, we calculate the share of workers in each industry in each commuting zone in the year 2000. To do so, we draw on the 2000 Census data and, as noted above, use industry coding from Autor, Dorn, and Hanson (2019). For each industry k, we observe the share of workings in a commuting zone working in that industry: $share_{ck} = n_{ck}/n_c$, where n_{ck} is the number of workers in industry k in the commuting zone c and $n_c = \sum_k n_{ck}$.

At this point, we can construct a measure of import exposure that a commuting zone faces across the years in a manner that is fairly standard to the literature:

$$IE_{ct} = \sum_k \Delta M_{tk} \times share_{ck}$$

[10] https://www.census.gov/foreign-trade/Press-Release/current_press_release/ft900.pdf

[11] To provide a sense of granularity: the industry category with the largest change from 2000 to 2010 is a combination of textile, fabric finishing, coating, and knitting mills.

In short, this measure captures that local labor markets with a larger share of workers *initially* employed in industries that were prominent amongst new Chinese imports would face larger economic effects than areas with fewer workers employed in impacted industries.

Our paper, however, moves a step beyond that measure. We construct separate measures by race/ethnic group. To do so, we construct race-specific employment shares by industry and commuting zone: $share_{ckr} = n_{ckr}/n_{cr}$, where here n_{ckr} is the number of workers from race group r in commuting zone c and industry k.

Thus, we separately calculate:

$$WhiteIE_{ct} = \sum_k \Delta M_{tk} \times share_{ck,white}$$

$$BlackIE_{ct} = \sum_k \Delta M_{tk} \times share_{ck,Black}$$

and could continue this for other groups as well.

To provide initial evidence that these individual measures successfully predict distinct economic impacts by race group, we draw on the five-year sample of the American Community Survey from 2016-2020.[12] The data are restricted to Black and white workers who report being in the labor market. We regress individuals' logged annual earnings and working status (a dummy equal to one if employed) on White Import Exposure (*White I.E.*), Black Import Exposure, and both of these interacted with whether the individual is Black. Both import exposure measures are divided by their standard deviations to allow for easier interpretation. Results are presented in Table 3. Notably, White I.E. has a negative impact on white workers' earnings and working status, while Black I.E. does not have a negative impact on white workers. Specifically, for white workers, a one standard-deviation increase in white workers' import exposure is associated with earnings that are roughly 12.5 percent lower. The Black I.E. X Black interaction documents that the Black I.E. measure *does* predict negative impacts on earnings and working status for Black workers.

The analysis in Table 3 demonstrates that the measures of exposure for Black and white workers capture distinct local, racialized impacts of the same trade shock. As a reminder, the standard

[12]In appendix Table A3, we include a table using an earlier five-year sample – 2008-2012. The results are similar.

Table 3: Labor Outcomes

VARIABLES	(1) ln(Earnings)	(2) Working
White I.E.	-0.125***	-0.002***
	(0.034)	(0.001)
Black I.E.	0.007	-0.000
	(0.026)	(0.001)
White I.E. X Black	0.044***	0.000
	(0.017)	(0.002)
Black I.E. X Black	-0.071***	-0.005**
	(0.021)	(0.002)
Observations	3,536,354	3,755,738

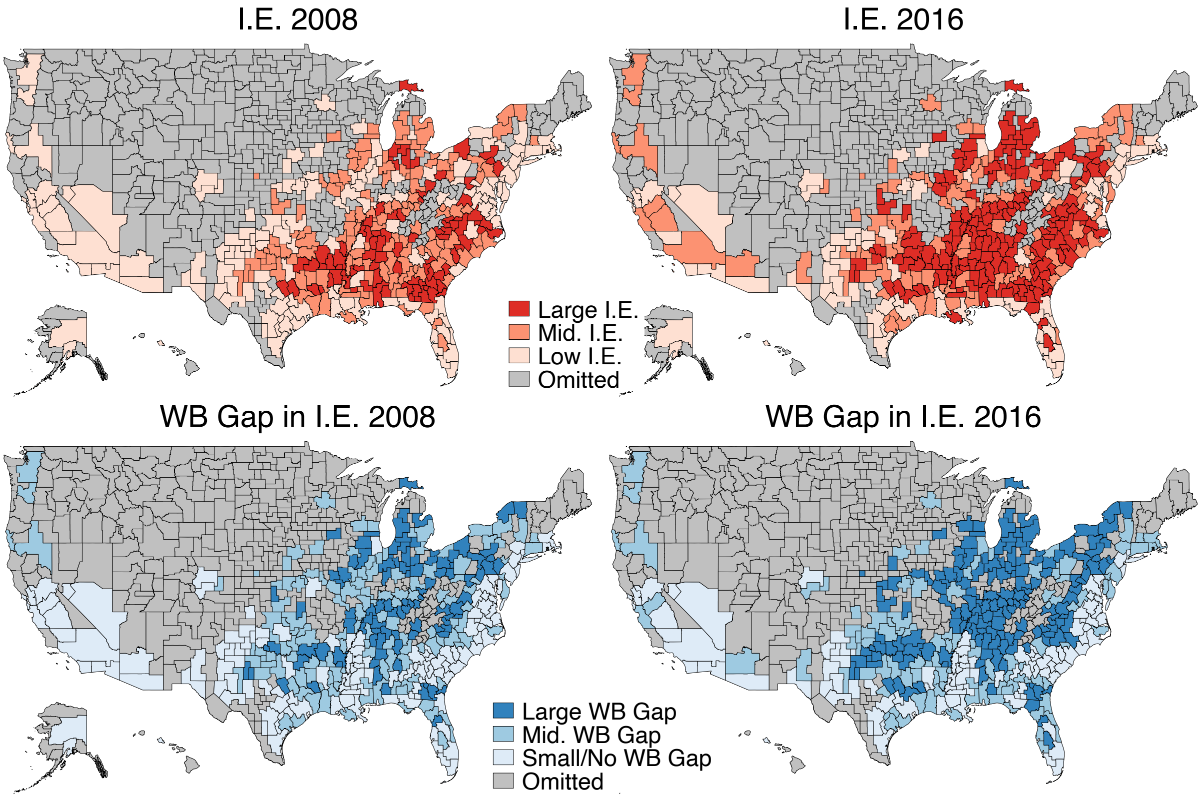
Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "[Race] I.E." is race-specific import exposure. Both import exposure measures have been divided by their standard deviations. "Blk. X [Race] I.E." is interaction with race of Census respondent and captures differential impact of [Race] I.E. on Black respondents. All specifications include state-race FEs, sex-race FEs, and race-age controls. Data draws on the 2016-2020 5-year sample of the American Community Survey.

overall import exposure measure captures that an area with a large share of workers in, say, textile manufacturing will be more impacted than an area with fewer workers in that industry. However, two areas may have the same share of workers in the textile industry, but different racial compositions of workers in the industry (and in others). Our theory suggests that these two areas will experience different downstream effects of the import exposure on the main outcomes of interest in our paper. We capture that most specifically, as noted in the prior section, by measuring the gap in import exposure between white workers and Black workers: *[Wht. I.E. - Blk. I.E.]*.

In Figure 2, we provide a descriptive geographic account of both overall import exposure and the relative racial import exposure gap (*[Wht. I.E. - Blk. I.E.]*). The top two maps in the figure depict which commuting zones fall in the highest, middle, and lowest terciles of overall import exposure in 2008 and 2016. The bottom two maps depict which commuting zones fall in the top, middle, and bottom terciles of *[Wht. I.E. - Blk. I.E.]*. The darkest shade in each map captures the highest tercile; for the bottom two maps, that is where there is the largest difference between white workers' import exposure and Black workers' exposure. We anticipate the largest changes in racial attitudes and voting patterns in these areas. In both sets of maps, the greyed-out areas represent the commuting zones that are below median in Black population share. As noted in the prior section,

Figure 1: Geographic Dispersion of Generic Import Exposure and White-Black Relative Import Exposure Gap



The top two maps plot the level of general/overall import exposure by commuting zone in 2008 and 2016 respectively. The bottom two maps plot the White-Black Relative Import Exposure Gap, as defined in text, at the commuting zone level. In both cases, the data are divided into terciles. For the top two maps, “High I.E.” is top tercile, where there is the largest degree of import exposure. In the bottom two, “Large WB Gap” is the highest tercile – or areas where white workers were substantially more exposed to imports than Black workers. “Small/No WB Gap” is the lowest tercile, which includes both areas with minimal gap and also the very small number of areas where Black workers are more exposed than white workers.

these are omitted from our sample for our main analysis.

Two patterns stand out. First, although there is some correlation between areas with high overall import exposure and large differences in white-Black import exposure, there is not perfect spatial overlap in these two measures. For instance, while there is substantial overall import exposure across most of the Southeast in 2008 and 2016, only certain areas within that region experienced high white-Black gaps in import exposure. Conversely, the white-Black import gap is large across almost all of the upper Midwest, which is less true of the overall import exposure level.

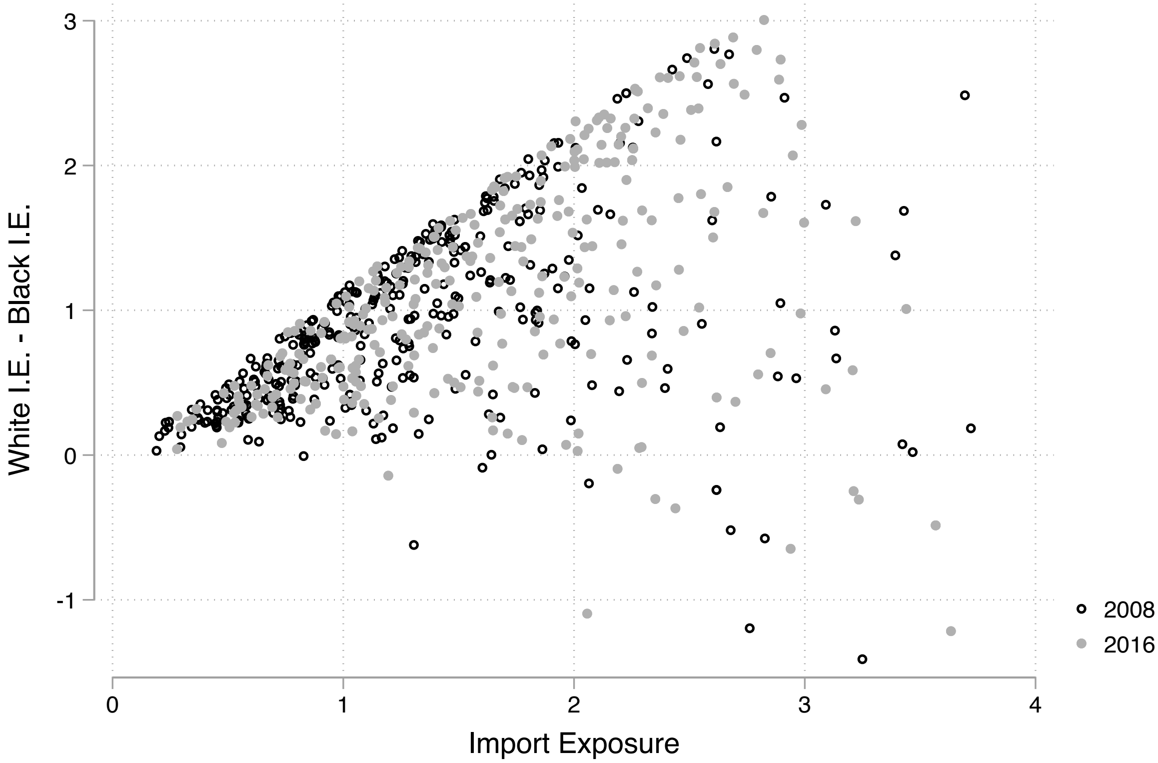
Second, both overall import exposure and white-Black gaps in import exposure vary over time, with both becoming more intense from 2008 to 2016. In the appendix, we report maps for a broader set of years (Figures A1 and A2. For completeness, these maps also include data for all commuting zones). Those figures document that both measures are generally increasing over time, until 2020, when both measures become less intense in much of the country. Notably, there was a drop in overall imports from China in 2019 and 2020 relative to the preceding years, which were largely characterized by year-to-year growth in Chinese imports. In sum, these maps collectively document that there is substantial space and time variation to leverage in our analysis, and that areas with high general import exposure and large white-Black gaps in exposure are not perfectly overlapping.

We further characterize our relative import exposure measure by comparing its distribution to that of the more typically constructed general import exposure measure. The x-axis captures the generic import exposure measure, divided by its standard deviation. The y-axis captures our racial relative import exposure measure, the White - Black I.E. gap, divided by its standard deviation. We plot these measures for both 2008 (hollow black circles) and 2016 (filled gray circles); each point represents a commuting zone. First, again, the two measures are relatively - but not perfectly - correlated. Areas with more general import exposure are, on average, areas where white workers are more exposed to imports than Black workers. That supports the need to control for overall import exposure in some of our specifications to more directly identify the impacts of relative exposure. Yet for any given value of general import exposure (x-axis), there is substantial variation in the difference between white and Black workers' exposure (y-axis).[13] The figure also reveals that the number of commuting zones where Black workers are more impacted than white workers

[13] We also note that if there appears to be a hard maximum on "White-Black I.E." at any given value of "Import Exposure", that is to be expected: white and Black import exposure are both always positive, so the largest the gap can be is a situation where the import exposure is entirely felt by white workers. In that scenario, the gap would be equal to the generic import exposure measure. A careful reader of this figure will note that the maximum numerical value on the y-axis is slightly above the 45 degree line; that is, at import exposure of 2, the maximum White-Black I.E. value is slightly above 2. That can be attributed to dividing by standard deviations, which are different across the two variables.

is very small; these appear on the figure as points in the negative range of the y-axis variable. To be precise, 10 of the 370 commuting zones in our sample fell in this range in 2008.

Figure 2: Plotting the Distributions of Overall Import Exposure and Racial Relative Import Exposure (White-Black I.E.)



The x-axis captures a generic import exposure measure. The y-axis captures our racial relative import exposure measure, the White - Black I.E. gap. We plot these measures for both 2008 (hollow black circles) and 2016 (filled gray squares). We censor the data at the 95th percentile of both measures to allow for easier visualization.

5.2 Outcomes Data

We next outline our various outcome measures. Our outcomes data variables are all either at the county level or the individual level. For variables at the county level, we aggregate them to the commuting zone-by-year level, to match the geographic level of the import exposure that we construct; for the others, we leave data at the individual level, but identify an individual's commuting zone. For most measures, we have a panel running from 2000-2020. We generally use the year 2000 as the base year and then use data from 2008-2020 as "impacted years", omitting 2001-2007. We omit those years because this allows to us to begin measuring the impacts of import exposure on our outcomes (relative to 2000) once that exposure has potentially had enough time to impact first labor outcomes and then the outcomes we study.

5.2.1 Implicit Association Test (IAT)

Our first set of outcomes is drawn from Project Implicit's data repository (Xu et al. 2024), which makes available individual-level data from respondents' completion of the Implicit Association Test (IAT) and associated survey questions. The Implicit Association Test (Greenwald, McGhee, and Schwartz 1998) is a well-known attempt to measure individuals' implicit attitudes about race, ethnicity, and other characteristics. We focus on the race implicit association test, which measures implicit attitudes about Black and white individuals. Data on responses are available from 2002-2020, one of the longest running of the IAT studies. The test asks respondents to rapidly sort a series of words and faces to the left or to the right, where one side might be meant for positive words and Black faces (and the reverse for the other side) for a given series of words and faces and then another series of words and faces where one side is meant for negative words and Black faces (and the reverse for the other side).

We use the "Overall IAT D score" as a main outcome, which captures the overall association between "Black" and "negative" – a more positive association indicates more anti-Black bias. A zero on this score indicates no particular association and no bias captured by the measure. A reversal of sign indicates anti-white bias. Much has been written on the reliability and validity of this measure; a review of that discussion is beyond the scope of this paper. We point readers to Pérez (2013) and Pérez (2016) for thorough discussions on this front, as well as evidence of the usefulness of the IAT measure in linking racial to political attitudes.

We use this measure at the individual level. The data include the year that the respondents took the test and also the county that they live in, allowing us to link them to the localized import exposure they face. The data also report the respondents' race and ethnicity. We restrict our attention to white respondents. Lacking a natural interpretation of the magnitude of the measure described above, we standard-normalize the measure so that results can be interpreted in units of standard deviations.

In addition to the test described above, respondents also complete a survey that includes direct "thermometer"-style questions asking about explicit attitudes about white and Black people, with responses ranging from 0 (negative attitudes) to 10 (positive attitudes). We use the gap between reported white and Black thermometer measures as an outcome also. As with the implicit association measure, positive numbers indicate a preference for white over Black, zero indicates no bias, and negative numbers indicate a preference for Black over white. We then standard-normalize the resulting measure.

We expect a greater gap to generate more anti-black sentiment for white respondents using both the implicit and explicit bias.

5.2.2 Hate Crimes

Our next outcome variable captures the count of anti-Black hate crimes. Specifically, we use data from the Federal Bureau of Investigation's Uniform Crime Reporting (UCR) system. The Hate Crimes portion of the data "covers crimes that are reported to the police and judged by the police to be motivated by hate" (Kaplan 2021). A hate crime, for the purposes of their data collection, is further defined by the FBI as "a committed criminal offense which is motivated, in whole or in part, by the offender's bias(es) against a race, religion, disability, sexual orientation, ethnicity, gender, or gender identity" [14]. Hate Crimes are reported to the FBI by law enforcement agencies. For local agencies, reporting is voluntary but relatively widespread[15]. We use Jacob Kaplan's compilation of these data across years (Kaplan 2020). In those data, each individual hate crime incident is reported. We focus on anti-Black hate crimes and aggregate the count of such crimes to the commuting zone-by-year level for the years 2000 to 2020. In our analyses, our outcome variable is the commuting zone-by-year count of (reported) anti-Black hate crimes per 10,000 people in the population. In the calculation of population-adjusted counts of hate crimes, we

[14]https://ucr.fbi.gov/hate-crime-faqs

[15]The FBI reports that over 14,000 law enforcement agencies (e.g., local police departments) reported hate crimes data to the FBI as of 2022. (https://www.justice.gov/hatecrimes/hate-crime-statistics)

include only counties that report hate crimes more than once during the 20 year span of data that we use.

One potential problem with the hate crimes data is the voluntary nature of reports by agencies. This will lead to substantial underreporting of actual bias-motivated crimes. There are also serious concerns about differences between jurisdictions that choose to report hate crimes to the FBI versus those that do not which would seriously bias cross-sectional analyses of hate crimes. We note however that that concern will be mitigated in our setting. Most of our analyses, including the hate crime analysis, will be estimated in a panel with commuting zone fixed effects. The fixed effects will account for level differences across areas in the propensity to report and shift the focus of analysis to *within*-area changes in the population-normalized count of anti-Black hate crimes. Still, the general underreporting problem remains; our estimated effects will therefore be muted relative to true effects on bias-motivated crimes.

We expect a larger gap to generate more anti-Black hate crimes.

5.2.3 Presidential Vote Shares

As our final outcome, we draw on county-level presidential election partisan vote totals for the 2000, 2008, 2012, 2016, and 2020 elections. We aggregate these vote totals up to the commuting zone-by-election year level and construct a two-party Democratic candidate vote share at that level equal to total Democratic votes divided by total Democratic and Republican votes (MEDSL 2018).

We expect a larger negative gap in import exposure for white workers to lead to a lower Democratic vote share.

In Table A2, we present the mean for each of outcomes in 2008 and 2016, across three terciles of the gap in white and Black import exposure.

6 Results

This section reports results, separately for each outcome outlined above. As described above, each specification in what follows takes the "White - Black Import Exposure" measure as the primary independent variable, controlling for the level of General Import Exposure in some specifications.

In some specifications, we also include White Import Exposure to ensure that our findings are not driven by overall exposure to the shock among white workers. As a reminder, these measures have been divided by the standard errors so that effects can be interpreted in units of standard deviations. Higher values of the "White - Black Import Exposure" measure indicate that white workers are more exposed to Chinese import competition than Black workers in the same labor market.

6.1 Implicit Association Test (IAT)

We begin with results from the Implicit Association Test (IAT) data. Table 4 reports results for the implicit association score. Higher numbers on this score indicate stronger anti-Black associations. All specifications in the table include commuting zone and year-fixed effects.

Per Column 1, we observe a small but strong positive relationship between white workers' relative import exposure and their anti-Black IAT score; a one standard deviation increase in the import exposure measure is associated with a 0.022 increase in the IAT score. In considering the magnitude of the effect, it is worth remembering that, here and elsewhere, we are essentially measuring an "intent-to-treat" effect. Not every white worker is exposed to or impacted by import competition.

Largely to compare our main approach to the approach of measuring import exposure more typical in the literature, Column 2 reports the same type of specification but replaces the relative import exposure measure with the general (not race-specific) import exposure measure. This is also important to consider, as General Import Exposure and the relative import exposure measure are correlated. With that in mind, it is perhaps not surprising to find that there is also a positive relationship between the general measure and anti-Black IAT scores, albeit a weaker relationship. Column 3 includes both on the right hand side and finds that, even controlling for general import exposure, there remains a strong positive impact of the *relative-by-race* import exposure measure. In fact, in this specification, the effect is more clearly positive; a one standard deviation increase in the measure is associated with a 0.038 increase in the IAT score. In Column 4, instead of overall import exposure, we control for white import exposure. There, a one standard deviation increase in the white-Black gap is associated with a 0.056 increase in the IAT score.

Table 4: Anti-Black Implicit Association – White Respondents

VARIABLES	(1) Anti-Black IAT std. norm.	(2) Anti-Black IAT std. norm.	(3) Anti-Black IAT std. norm.	(4) Anti-Black IAT std. norm.
White - Black I.E.	0.022***		0.038***	0.056**
	(0.006)		(0.013)	(0.024)
General I.E.		0.015***	-0.016	
		(0.005)	(0.012)	
White I.E.				-0.032
				(0.022)
Observations	1,171,094	1,171,094	1,171,094	1,171,094
CZ and Yr. FE	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "White - Black I.E." is the difference in localized white Chinese import exposure and Black Chinese import exposure. More positive numbers in the outcome variable indicate more pro-white/anti-Black implicit association.

Table 5 takes the *explicit* expression of anti-Black bias from the post-IAT survey as our outcome. Again, this is the gap between the respondent's white "thermometer" response and their Black "thermometer" response. The findings are generally similar to those using the *implicit* bias measure. The structure of the table is the same as the prior one. Per Column 3, a one standard deviation increase in white workers' relative import exposure is associated with a 0.042 standard deviation increase in pro-white/anti-Black responses to the thermometer measures. The same pattern holds in Column 4.

As will be true for all of our outcomes, the appendix reports additional analyses assessing the robustness of these results – as described in the "Empirical Approach" section. For the preceding two outcomes (IAT implicit bias and the thermometer gap–i.e. explicit bias–measure), see Tables A4 and A5. In Column 1, we re-estimate the models on the full sample – that is, without dropping commuting zones that are below the median in Black population share. Column 2 reports results using the full sample, but weighting the regression by Black population share as an alternative means to reduce the influence of areas with very low Black population share based on a continuous variable. Column 3 adds the set of time-varying controls listed in the prior section. Column 4 adds a set of base-year controls interacted with linear time trends. Across all of these models, the main conclusion is the same as those presented in the main text.

Table 5: IAT Anti-Black Explicit Thermometer Gap

VARIABLES	(1) Wh.-Blk. Therm. Gap	(2) Wh.-Blk. Therm. Gap	(3) Wh.-Blk. Therm. Gap	(4) Wh.-Blk. Therm. Gap
White - Black I.E.	0.019**		0.042***	0.062**
	(0.008)		(0.013)	(0.029)
General I.E.		0.010	-0.025*	
		(0.008)	(0.014)	
White I.E.				-0.042
				(0.029)
Observations	1,072,771	1,072,771	1,072,771	1,072,771
CZ and Yr. FE	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "White - Black I.E." is the difference in localized white Chinese import exposure and Black Chinese import exposure. More positive numbers in the outcome variable indicate more pro-white/anti-Black implicit association.

6.2 Hate Crimes

Table 6 reports results taking anti-Black hate crimes (per 10,000 people in the commuting zone population) as the outcome. Analyses are at commuting zone-by-year level. Note that given the skewed nature of these count data, we use Poisson models to estimate these effects, but otherwise include all of the same fixed effects as our main specifications. The results reported here are marginal effects of Poisson models. Columns 1 and 2 document no impact of relative or general import exposure on hate crime frequency when estimated separately. Column 3 controls for both and documents a significant positive impact of white relative exposure; a one standard-deviation increase in white import exposure (relative to Black) increases anti-Black hate crimes by 0.011 per 10,000 in the population. For reference, the mean of the outcome variable in our sample is 0.071, so the effect we document here is roughly 20 percent of the mean. In Column 4, where we instead control for white-specific import exposure, the effect of the white-Black gap in exposure is even larger.

As a placebo test, Appendix Table A6 estimates our main model using hate crimes tagged as anti-Hispanic or anti-Asian as the outcomes. White import exposure relative to Black import exposure has no impact on these other categories of hate crimes, further suggesting a direct link

Table 6: Anti-Black Hate Crimes

VARIABLES	(1) Anti-Black Crime per 10k in Pop.	(2) Anti-Black Crime per 10k in Pop.	(3) Anti-Black Crime per 10k in Pop.	(4) Anti-Black Crime per 10k in Pop.
White - Black I.E.	0.002		0.011*	0.024**
	(0.004)		(0.006)	(0.010)
Import Exposure		-0.008	-0.015**	
		(0.008)	(0.007)	
White I.E.				-0.026**
				(0.011)
Observations	4,648	4,340	4,340	4,340
CZ and Yr. FE	Yes	Yes	Yes	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "White - Black I.E." is the difference in localized white Chinese import exposure and Black Chinese import exposure. The sample average of "Anti-Black Crime per 10k in Pop." is 0.071. All specifications are Poisson; reported coefficients are marginal effects.

between white-relative-to-Black import exposure and anti-Black racism in particular.

Appendix Table A7 includes the same set of robustness tests as described in prior subsections. Our key result here is robust to expanding to the full sample, adding time-varying controls, or adding the interaction of linear time trends with base-year controls.

Finally, in Table A8, we look at the presence of hate groups as an additional related outcome. We find some evidence that greater relative import exposure for white workers compared to Black workers is associated with a higher probability of having an anti-Black hate group in the commuting zone.

6.3 Election Outcomes

We now turn to our final set of results: the impact of relative import exposure on election outcomes, specifically commuting zone-level Democratic vote share in presidential elections. So far, we have documented that relative import exposure leads to greater racial animus. This is likely to influence voting patterns because of political discourse in the U.S., particularly on the Republican side, evolved to include more emphasis on relative group position.

Table 7 reports results in the same format as prior results. As a reminder, the analysis includes the 2000 presidential election (as a baseline year) and the 2008, 2012, 2016, and 2020 elections. We find that a one-standard deviation increase in white relative to Black import exposure decreases the Democratic candidate's vote share by 2.2 percentage points (Column 1). Column 2 adopts the more typical approach taken by others – using a generic import exposure measure – and documents, like others have (Autor et al. 2017), that the measure is also associated with a decrease in Democratic vote share. However, upon controlling for both, we find that it is primarily our relative import exposure measure that drives changes in presidential vote share (Column 3). In fact, when accounting for both, we no longer detect an effect of the generic import exposure measure, suggesting that the pathway to a shift in political behavior is driven more by relative status concerns than overall economic anxiety. The same is true when controlling instead for the white-specific level of import exposure (Column 4).

We decompose these results by election year and report results graphically in Figure 3. The figure plots coefficient estimates from a single regression that interacts both relative import exposure and generic import exposure with year indicators (see Table A12 for full results). Thus, the model is a richer version of the model we reported in Column 3 of the preceding table. As noted, the year 2000 is included as a baseline year, so all coefficients are relative to that year. Panel (a) reports coefficients from the relative import exposure measure; Panel (b) reports coefficients from the generic import exposure measure.

We find that the relationship between relative and generic import exposure and presidential vote share changed over time. In the 2008 Obama v. McCain election, we do not observe significant impacts of either measure. However, all elections thereafter are characterized by a very different pattern– one that more closely matches the results captured in Table 7. Specifically, from 2012 onwards, there is a significant negative impact of the racial gap in import exposure on Democratic vote share; those effects become increasingly negative over time. From the same models, we observe no statistically significant impact of overall import exposure on Democratic vote share.

One potential explanation for this pattern – especially the growth of the negative effect of our main measure in years 2012 and beyond – was previewed in our theoretical framework. Following group position threat theory, our previous outcomes measuring racial animus may be immediately impacted by a relative shift in import exposure between groups. However, partisan preference is not inherently linked to racial attitudes and perceived group position threat; it can become linked via rhetoric and position-taking of political entrepreneurs as detailed in the theory section. As such, the linking of a racial gap in import exposure to perceived group position threat may be not constant throughout our time period, but may have been increasingly linked to partisan preferences via rhetoric and position-taking of elites during the Obama years.

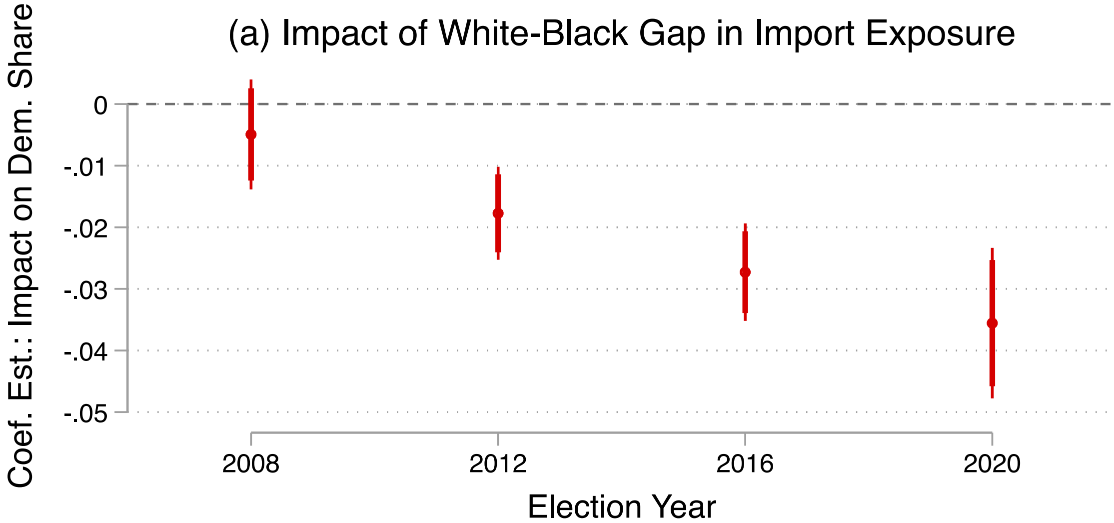
Table 7: Impacts of Overall and Differential-by-Race Import Exposure on Presidential Election Democratic Vote Shares

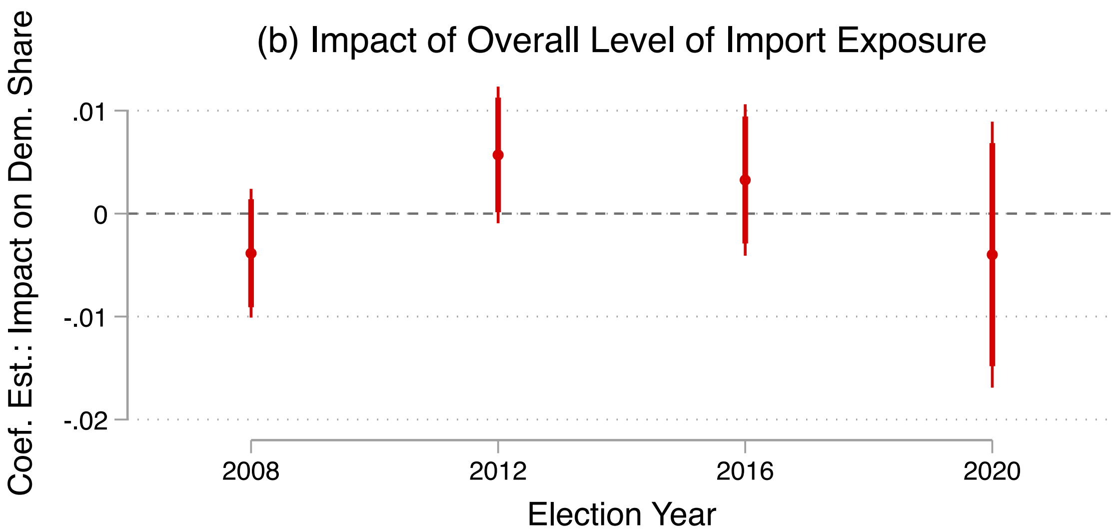
VARIABLES	(1) Two-Party Dem. Share	(2) Two-Party Dem. Share	(3) Two-Party Dem. Share	(4) Two-Party Dem. Share
White - Black I.E.	-0.022***		-0.024***	-0.024***
	(0.003)		(0.004)	(0.006)
Import Exposure		-0.013***	0.003	
		(0.003)	(0.003)	
White I.E.				0.002
				(0.006)
Observations	1,845	1,845	1,845	1,845

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications include commuting zone and year-fixed effects. Sample includes presidential elections in the years 2000, 2008, 2012, 2016, and 2020. Coefficients are reported in table form in Appendix Table A9.

Figure 3: Impacts of Overall and Differential-by-Race Import Exposure on Presidential Election Democratic Vote Shares





Note: All reported coefficients drawn from a single regression interacting year with (panel a) white-Black gap in import exposure and (panel b) overall level of import exposure, with both of these variables represented in units of standard deviations. The specifications also include commuting zone and year-fixed effects. Estimates are relative to the year 2000. The outcome variable is the two-party Democratic presidential vote share at the commuting zone level-by-year level.

These estimates are highly robust to the same set of robustness tests that we have subjected all other outcomes to, as documented in Appendix Table A9. Separately, Appendix Figure A3 replicates the analysis of Figure 3, but using the full sample of commuting zones and weighting the regression by Black population share; there we find similar patterns, but with a significant negative impact of the relative import exposure from 2008 onwards.

Finally, in the presidential elections data, we can address a broader alternate explanation for our findings. It may be that areas that have larger white-Black gaps in import exposure from 2008-2020 are trending towards stronger Republican vote share and would do so in the absence of our "treatment" measure. We speak to this concern in two ways. First, we conduct a placebo test, extending our measure of import exposure from 2008-2020 (relative to 2000) *backwards* in time. Specifically, for each commuting zone, we calculate the average of overall import exposure and also the white-Black gap in import exposure from 2008-2020. Then, we set that value as the import exposure in 1992 and 1996. As in main specifications, 2000 serves as the comparison year; both measures equal zero in that year. If the areas that *would* have larger white-Black gaps were already trending Republican, we would observe a relationship between our treatment measure and 1992 and 1996 elections (relative to 2000). Per Appendix Table A10 we do not.

A second approach to addressing this same issue – that is, that we may simply be identifying areas that are trending more Republican independent of our treatment – leverages the fact that, while both the overall import exposure and also white-Black gap measure steadily increase from 2008-2016, they both decrease in 2020. This is visible in the maps plotted in the appendix (Appendix Figures A1 and A2), with fewer areas in the highest category of white-Black gap or import exposure in 2020 relative to 2016. Areas "trending Republican" should continue to do so from 2016 to 2020. However, there are many areas that were simultaneously "trending" towards larger white-Black gaps in import exposure until 2016 that then move in the opposite direction. If the effect of our measure is causal, and not confounded by or simply reflecting broader trends in partisan voting patterns, areas with a *high* white-Black gap measure in 2016 that then experience a decrease in that measure in 2020 should experience an *increase* in Democratic vote share. We test that notion by conducting a simple difference-in-differences style specification. We restrict our analysis to 2012, 2016, and 2020. We further restrict our sample to areas that were *above median* in the white-Black gap measure in 2012 and 2016 and construct a dummy variable to capture which areas had a *below median* white-Black gap measure in 2020. We then interact the "below-median black-White gap" dummy variable with an indicator variable for the year 2020. We include the same commuting zone and year fixed effects, as well as controls for overall import exposure. Consistent with a causal impact of our measure – and inconsistent with an alterantive explanation wherein our measure is simply identifying areas trending Republican for other reasons – we indeed find that areas that had large white-Black gaps in import exposure but then moved in the opposite direction in 2020 experienced *increases* in Democratic vote share.

6.4 Two-Stage Least Squares Approach

To further support the robustness of our results, we adopt a modification of a two-stage least squares strategy often employed in the literature on the China shock. Autor, Dorn, and Hanson 2013 note that "realized U.S. imports from China ... may be correlated with industry labor demand shocks" (9). To address that potential concern, we follow the instrumental variables approach used by those authors and others. Specifically, recall that our previous measure of import exposure was defined by the following:

$$IE_{ct} = \sum_{k} \Delta M_{tk} \times share_{ck}$$

with ΔM_{tk} capturing growth between the year 2000 and year t in industry k in imports from China to the U.S. Following prior work, we construct a new alternate version of that measure that instead uses growth by industry during the same time period in imports from China received in eight other countries (Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland). As in previous uses of this instrument, we still leverage U.S.-based commuting zone variation in base year industry employment shares. That is, $share_{ck}$ remains the same as before. Our construction of the white- and Black-specific import exposure measures, and the gap between them, proceeds in the same way; they are constructed in the same way as before, simply replacing nationwide industry-level import growth with industry-level import growth across the eight other countries listed above. These alternate measures then serve as instruments for the main measures that we otherwise use in our paper.

Note that we are not able to use the same data to construct this measure, because our main source of data on imports into the U.S. do not also include imports from China to other countries.

It is with that in mind that we turn to the Autor, Dorn, and Hanson 2019 replication files. Doing so restricts our sample period to run only through 2014.

We present a first-stage relationship between our instruments and instrumented variables in Columns 1 and 2 of Appendix Table A14 drawing on data from 2000 and 2008. We include the same commuting zone and year-fixed effects that are in our main specifications. Those columns reveal a strong relationship between the instruments and the instrumented variables.

The remaining columns of that table report the second-stage results of the two-stage least squares, taking IAT, hate crimes, and presidential vote shares as our outcomes. This table shows that our main conclusions from this paper are robust to this alternate approach, with the exception of the hate crimes outcome, which was already found to be less robust than some of our other outcomes.[16]

7 Conclusion

There has been substantial discussion in the past decade, in scholarly work and popular press, around the potential linkages between race and racism, on the one hand, and support for Trump and populist politics more generally, on the other. The discussion has largely centered around whether support for Trump, at least amongst some, was driven by racial animus and resentment *or* "economic anxiety", following a decades-long shift in the nature of the American economy especially impacting the manufacturing sector. However, as noted by some (e.g., Darity Jr 2022), the explanation may not simply be one of the above possibilities *or* the other; instead, they may be linked.

With that in mind, our paper takes a new look at this issue, uniting two distinct literatures. In one literature, research has documented the effects of local labor market shocks on a variety of outcomes, shifting individuals' attitudes and voting behaviors. In another, perceived threat to one's own racial/ethnic group's relative position has been put forth as a potential driver of racial animus (Quillian 1995) and also linked to support for Trump (Jardina 2019; Mutz 2018). Bringing these

[16] We note however though that the difference in sample period seems to play a role here; if we adopt our main approach, but restricting the sample to hate crimes prior to 2014, we observe no effect there.

literatures together, we hypothesize that it is not the overall level of local labor market shocks – and, in our case, rising exposure to competition from Chinese imports – that have an impact on attitudes and behavior, but instead the *relative* impact that shocks have locally on distinct groups. More specifically, we predict that it is when white workers are relatively more exposed to the "China shock" in an area than Black workers in the same area that white racial attitudes and voting behavior change. We anticipate smaller changes in attitudes or behavior in areas that are highly exposed to rising import competition, but in a way that is equally distributed across groups. Prior literature, while alluding to group position threat as a potential channel through which local economic shocks impact attitudes, has not conceptualized and measured the shocks in a way that would identify this effect. Empirically, our paper sets out to do exactly that.

We construct distinct import exposure measures for white and Black workers, with variation in import exposure across areas stemming from differential exposure to rising Chinese imports in particular industries. We then test whether it is the level of import exposure an area faces that shapes attitudes and behaviors or the exposure that white workers face relative to Black workers in the same area. Across a variety of measures, we find stronger evidence of the latter: when white workers are more exposed than Black workers to import exposure, even controlling for the overall level of import exposure, white respondents in the area are more likely to display anti-Black attitudes in measures from surveys. The same pattern appears when we take anti-Black hate crimes as an outcome. We also find that white workers' *relative* exposure to trade shock – and not overall exposure – is associated with heightened votes for Republican presidential candidates within a local area. Interestingly, that pattern is true in 2012, 2016, and 2020, but not in 2008, consistent with a shift in the attachment of racialized themes to political rhetoric and also in the relationship between racial attitudes and views of the presidency (Parker and Barreto 2014; Tesler 2012).

Our work suggests a few key directions for further research into the intersection of economic shocks, relative group position, and political behavior. One important implication is to consider how the position of white workers relative to other historically non-dominant groups shapes attitudes and behaviors related to racial/ethnic groups and ultimately, voting. For example, this includes the impact of relative exposure to negative economic shocks on attitudes toward Latinos. Political elites in the United States, particularly in the Republican Party, have recently used rhetoric to attack groups and policies related to the southern border. Another dynamic relates to anti-Asian sentiment. In other contexts, relative group position may be defined by characteristics other than race or ethnicity (e.g. urban/rural divides as in Green, Hellwig, and Fieldhouse (2022)). In exploring the utility of the theory in other contexts, it is important to identify politically salient groups and local group dynamics.

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See here for full draft with figures A1 and A2. Removed due to file size constraints

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Table A1: Summary of Commuting Zone Characteristics in Base Year (2000), Split by Above/Below Median in White-Black Import Exposure Gap

	Above Median Wht.-Black Gap in I.E.		
	0	1	Total
N	185 (50.0%)	185 (50.0%)	370 (100.0%)
% college-ed. (2000)	47.716 (8.998)	45.234 (8.213)	46.461 (8.688)
% foreign-born (2000)	9.197 (8.865)	4.104 (2.459)	6.623 (6.950)
Manuf. share (2000)	0.173 (0.096)	0.251 (0.098)	0.212 (0.104)
Wallace vote share (1968)	0.316 (0.205)	0.231 (0.184)	0.273 (0.199)
Gini (2000)	0.456 (0.029)	0.439 (0.025)	0.447 (0.028)
% Black (2000)	0.202 (0.155)	0.100 (0.087)	0.150 (0.135)
Reg.: West	0.114 (0.318)	0.011 (0.104)	0.062 (0.242)
Reg.: Midwest	0.049 (0.216)	0.357 (0.480)	0.203 (0.403)
Reg.: South	0.422 (0.495)	0.368 (0.483)	0.395 (0.489)
Reg.: Northeast	0.416 (0.494)	0.265 (0.442)	0.341 (0.475)

Notes: Levels of the gap in import exposure are computed by tercile for the entire sample period (2008 forward).

Table A2: Summary of outcomes by White-Black I.E. gap

	Low Mean	Medium Mean	High Mean
2008 IAT bias			
Anti-black implicit bias	0.062	0.111	0.142
White-Black Therm. gap	0.162	0.262	0.319
N	101396	65792	38916
2016 IAT bias			
Anti-black implicit bias	-0.058	-0.001	0.052
White-Black Therm. gap	-0.142	-0.018	0.080
N	114194	119526	127423
2008 Hate crimes			
Hate crimes	0.072	0.084	0.092
N	126	205	286
2016 Hate crimes			
Hate crimes	0.035	0.029	0.048
Observations	96	145	376
2008 Democratic two party vote share			
Dem. vote share	0.493	0.393	0.428
N	166	247	316
2016 Democratic party vote share			
Dem. vote share	0.489	0.325	0.320
N	119	192	418

Notes: Levels of the gap in import exposure are computed by tercile for the entire sample period (2008 forward).

Labor Outcomes

Table A3: Labor Outcomes, Early and Late Samples

VARIABLES	(1) ln(Earnings)	(2) Working
White I.E.	-0.108***	-0.005***
	(0.025)	(0.001)
Black I.E.	-0.013	0.000
	(0.019)	(0.001)
White I.E. X Black	0.029	-0.000
	(0.018)	(0.001)
Black I.E. X Black	-0.041***	-0.004**
	(0.012)	(0.002)
Observations	4,148,655	4,509,286

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: "[Race] I.E." is race-specific import exposure. "Blk. X [Race] I.E." is interaction with race of Census respondent and captures differential impact of [Race] I.E. on Black respondents. All specifications include state-race FEs, sex-race FEs, and race-age controls. Data draws on the 2008-2012 and 2016-2020 5-year samples of American Community Survey.

IAT

Table A4: Anti-Black Implicit Association – White Respondents, Robustness Tests

VARIABLES	(1) Full Sample	(2) Full Sample Weighted	(3) Time-Vary. Controls	(4) Base Year Controls X Trend
White - Black I.E.	0.037***	0.029*	0.035**	0.028*
	(0.013)	(0.015)	(0.016)	(0.016)
General I.E.	-0.023*	-0.005	-0.019	-0.012
	(0.012)	(0.013)	(0.015)	(0.015)
Yr. X % Coll. Ed. '00				-0.000***
				(0.000)
Yr. X % Foreign-born '00				-0.000
				(0.000)
Yr. X Manuf. Share '00				0.001
				(0.006)
Yr. X Wallace share '68				-0.006
				(0.004)
Yr. X Gini '00				-0.056***
				(0.015)
Yr. X % Black '00				-0.007
				(0.005)
Bartik (non-trade ind.)			-0.030***	
			(0.009)	
% Black population			0.008**	
			(0.004)	
% Foreign Born			0.004	
			(0.004)	
Percent mfg. employment			0.012***	
			(0.004)	
Observations	1,374,572	1,374,557	1,145,155	1,130,156
CZ and Yr. FE	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "White - Black I.E." is the difference in localized white Chinese import exposure and Black Chinese import exposure. More positive numbers in outcome variable indicate more pro-white/anti-Black implicit association.

Table A5: IAT Anti-Black Explicit Thermometer Gap, Robustness Tests

VARIABLES	(1) Full Sample	(2) Full Sample Weighted	(3) Time-Vary. Controls	(4) Base Year Controls X Trend
White - Black I.E.	0.054***	0.023	0.033*	0.015
	(0.012)	(0.014)	(0.018)	(0.017)
General I.E.	-0.044***	-0.002	-0.015	-0.006
	(0.013)	(0.014)	(0.018)	(0.016)
Yr. X % Coll. Ed. ’00				0.000*
				(0.000)
Yr. X % Foreign-born ’00				0.000
				(0.000)
Yr. X Manuf. Share ’00				0.014*
				(0.008)
Yr. X Wallace share ’68				-0.008*
				(0.005)
Yr. X Gini ’00				-0.026
				(0.035)
Yr. X % Black ’00				-0.019***
				(0.007)
Bartik (non-trade ind.)			-0.005	
			(0.017)	
% Black population			0.003	
			(0.005)	
% Foreign Born			0.012*	
			(0.006)	
Percent mfg. employment			0.005	
			(0.005)	
Observations	1,260,795	1,260,782	1,042,714	1,028,288
CZ and Yr. FE	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: “White - Black I.E.” is the difference in localized white Chinese import exposure and Black Chinese import exposure. More positive numbers in outcome variable indicate more pro-white/anti-Black implicit association.

Hate Crimes

Table A6: Placebo: Anti-Asian and Anti-Hisp. Hate Crimes

VARIABLES	(1) Anti-Asian Crime per 10k in Pop.	(2) Anti-Hisp. Crime per 10k in Pop.
White - Black I.E.	0.002	0.006
	(0.002)	(0.004)
Import Exposure	-0.001	-0.020**
	(0.002)	(0.010)
Observations	2,184	3,234
CZ and Yr. FE	Yes	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "White - Black I.E." is the difference in localized white Chinese import exposure and Black Chinese import exposure. Sample average of "Anti-Hisp. Crime per 10k in Pop." is 0.017. Sample average of "Anti-Asian Crime per 10k in Pop." is 0.003. All specifications are Poisson.

Table A7: Anti-Black Hate Crimes, Robustness Tests

VARIABLES	(1) Full Sample	(2) Full Sample Weighted	(3) Time-Vary. Controls	(4) Base Year Controls X Trend
White - Black I.E.	0.014***	0.009	0.013**	0.041***
	(0.005)	(0.009)	(0.006)	(0.011)
Import Exposure	-0.009	-0.031**	-0.016**	-0.032***
	(0.006)	(0.014)	(0.008)	(0.010)
Yr. X % Coll. Ed. '00				0.000
				(0.000)
Yr. X % Foreign-born '00				-0.000
				(0.000)
Yr. X Manuf. Share '00				-0.006
				(0.006)
Yr. X Wallace share '68				0.001
				(0.004)
Yr. X Gini '00				0.000
				(0.000)
Yr. X % Black '00				0.022***
				(0.006)
Bartik (non-trade ind.)			-0.030**	
			(0.015)	
% Black population			0.008	
			(0.009)	
% Foreign Born			0.025*	
			(0.015)	
Percent mfg. employment			-0.005**	
			(0.002)	
Observations	7,630	7,630	4,339	4,284
CZ and Yr. FE	Yes	Yes	Yes	Yes

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: "White - Black I.E." is the difference in localized white Chinese import exposure and Black Chinese import exposure.

Table A8: Presence of Hate Groups

VARIABLES	(1) Any Group	(2) Any Klan	(3) Any Neo-Conf.	(4) Any White Nat.
White - Black I.E.	0.053*	-0.002	0.056**	0.015
	(0.030)	(0.029)	(0.024)	(0.020)
Import Exposure	-0.097***	-0.044	-0.036	-0.031
	(0.033)	(0.032)	(0.025)	(0.020)
Observations	1,442	1,442	1,442	1,442
CZ and Yr. FE	Yes	Yes	Yes	Yes
Full Sample	No	No	No	No

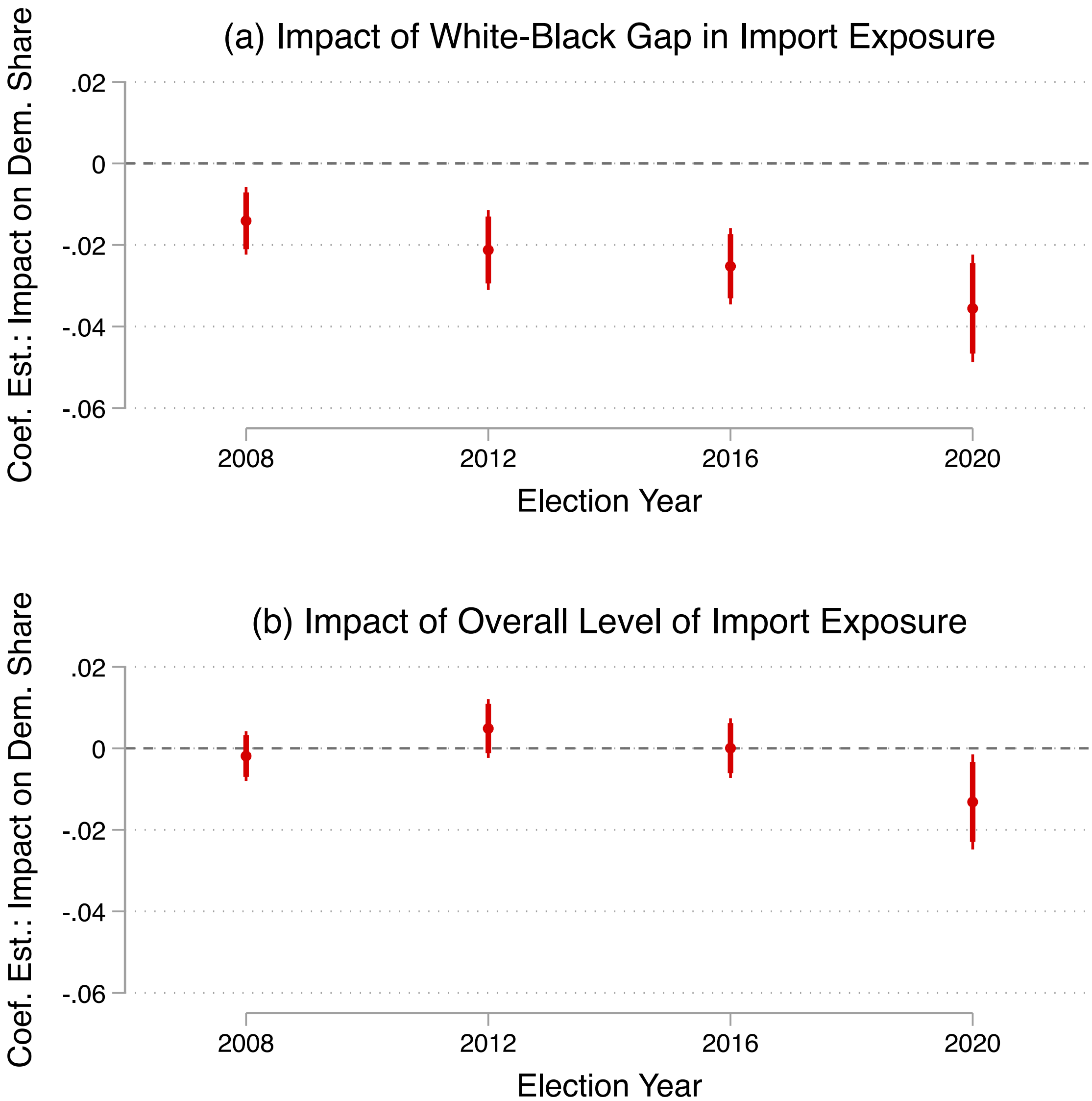
Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: "White - Black I.E." is the difference in localized white Chinese import exposure and Black Chinese import exposure. Outcome data is from Southern Poverty Law Centers mapping of hate groups, from 2000, 2008, 2012, and 2016. All outcomes are dummy variables, equal to one if there is any group (of the type noted in the column header) present in a CZ-year. Group types are as categorized by Southern Poverty Law Center; here, we consider three groupings: Ku Klux Klan (column 2), Neo-Confederate groups (column 3), and White Nationalist groups (column 4). Column 1 captures the presence of any of these three types. 34 percent of CZ-year observations in the sample have at least one group of any of these three types.

Election Outcomes

Figure A3: Impacts of Overall and Differential-by-Race Import Exposure on Presidential Election Democratic Vote Shares, Weighting by CZ Black Pop. Share



Note: All reported coefficients drawn from a single regression interacting year with (panel a) white-Black gap in import exposure and (panel b) overall level of import exposure, with both of these variables represented in units of standard deviations. The specifications also include commuting zone and year-fixed effects. Estimates are relative to the year 2000. The outcome variable is the two-party Democratic presidential vote share at the commuting zone level-by-year level.

Table A9: Impacts of Overall and Differential-by-Race Import Exposure on Presidential Election Democratic Vote Shares, Robustness Tests

VARIABLES	(1) Full Sample	(2) Full Sample Weighted	(3) Time-Vary. Controls	(4) Base Year Controls X Trend
White - Black I.E.	-0.022***	-0.023***	-0.024***	-0.007**
	(0.003)	(0.005)	(0.004)	(0.003)
Import Exposure	0.000	0.002	0.004	0.004*
	(0.003)	(0.004)	(0.003)	(0.002)
Yr. X % Coll. Ed. ’00				0.000***
				(0.000)
Yr. X % Foreign-born ’00				0.000***
				(0.000)
Yr. X Manuf. Share ’00				0.001
				(0.002)
Yr. X Wallace share ’68				-0.003***
				(0.001)
Yr. X Gini ’00				-0.031***
				(0.008)
Yr. X % Black ’00				0.022***
				(0.002)
Bartik (non-trade ind.)			0.006	
			(0.005)	
% Black population			0.004*	
			(0.002)	
% Foreign Born			0.004	
			(0.005)	
Percent mfg. employment			-0.001	
			(0.001)	
Observations	3,645	3,615	1,844	1,820
CZ and Yr. FE	Yes	Yes	Yes	Yes

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications include commuting zone and year fixed effects. Sample includes presidential elections in the years 2000, 2008, 2012, 2016, and 2020.

Table A10: Impacts of Overall and Differential-by-Race Import Exposure on Presidential Election Democratic Vote Shares, Placebo Comparing 2000 to 1992 and 1996

VARIABLES	(1) Two-Party Dem. Share	(2) Two-Party Dem. Share
White - Black I.E.	0.003	0.004
	(0.003)	(0.005)
Import Exposure	0.002	
	(0.003)	
Wht. Import Exposure		0.001
		(0.005)
Observations	1,101	1,101
CZ and Yr. FE	Yes	Yes
Full Sample	No	No
Weighted	No	No

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications include commuting zone and year fixed effects. Sample includes presidential elections in the years 1992, 1996, and 2000. All three import exposure measures measured as averages at commuting zone level from the years 2008-2020. As in main specifications, year 2000 serves as baseline year and therefore all three measures equal zero in that year.

Table A11: Impacts of Overall and Differential-by-Race Import Exposure on Presidential Election Democratic Vote Shares, Late Periods Only - Leveraging Decreases in White-Black Gap in 2020

VARIABLES	(1) Two-Party Dem. Share	(2) Two-Party Dem. Share	(3) Two-Party Dem. Share
Low WB Gap X 2020	0.010*	0.011**	0.010**
	(0.005)	(0.005)	(0.005)
Import Exposure		0.002	
		(0.001)	
Wht. Import Exposure			0.002
			(0.002)
Observations	579	579	579

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications include commuting zone and year fixed effects. Sample includes presidential elections in the years 2012, 2016, and 2020. Sample is further restricted to commuting zones with above-median white-Black gaps in 2012 and 2016. We then define a variable “Low WB Gap” which equals one if the commuting zone is below median – which, by construction is only possible in 2020 – as opposed to remaining above median.

Table A12: Impacts of Overall and Differential-by-Race Import Exposure on Presidential Election Democratic Vote Shares, Tabular Version of Estimates Depicted in Figures 3 and A3

VARIABLES	(1) demshare2P
2008	0.008**
	(0.003)
2008	-0.009***
	(0.003)
2012	0.017***
	(0.003)
2012	-0.023***
	(0.004)
2016	0.002
	(0.002)
2016	0.001
	(0.002)
2020	-0.044***
	(0.007)
2020	0.066***
	(0.008)
Observations	3,632

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Column 1 reports coefficients depicted in Figure 3. Column 2 reports coefficients depicted in Figure A3. All specifications include commuting zone and year fixed effects; standard errors clustered by community zone. Sample includes presidential elections in the years 2000, 2008, 2012, 2016, and 2020.

Table A13: Impacts of White and Differential-by-Race Import Exposure on Presidential Election Democratic Vote Shares

VARIABLES	(1) Two-Party Dem. Share
WB Gap 2008	-0.001
	(0.006)
White IE 2008	-0.007
	(0.006)
WB Gap 2012	-0.021***
	(0.006)
White IE 2012	0.009
	(0.006)
WB Gap 2016	-0.026***
	(0.006)
White IE 2016	0.001
	(0.007)
WB Gap 2020	-0.025**
	(0.010)
White IE 2020	-0.015
	(0.011)
Observations	1,845

Cluster robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: Include commuting zone and year fixed effects; standard errors clustered by community zone. Sample includes presidential elections in the years 2000, 2008, 2012, 2016, and 2020.

Additional Tests

Table A14: Two-Stage Least Squares: First-Stages and Impacts on IAT, Hate Crimes, and Vote Shares

VARIABLES	(1) First-Stage White - Black I.E.	(2) First-Stage Import Exposure	(3) Anti-Black IAT std. norm.	(4) Anti-Black Crime per 10k in Pop.	(5) Two-Party Dem. Share
IV: White - Black I.E.	7.336***	-2.398***			
	(0.317)	(0.456)			
IV: Import Exposure	0.412	10.417***			
	(0.333)	(0.477)			
White - Black I.E.			0.610**	0.020	-0.030***
			(0.241)	(0.098)	(0.009)
Import Exposure			-0.481**	-0.019	0.045***
			(0.232)	(0.088)	(0.017)
Observations	738	738	416,175	1,328	1,107
CZ and Yr. FE	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Notes: All specifications are two-stage least squares models, instrumenting for "White-Black I.E." and "Import Exposure" with alternate measures constructed using industry-level import exposure based on data from Europe.