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Dog Whistles and Work Hours: The Political Activation of Labor Market Discrimination

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Abstract: Many commentators have suggested that Donald Trump's 2016 election emboldened discrimination against racial minorities. We focus on changes in weekly work hours among hourly paid employees during the five months following the 2016 election (relative to 12 months prior). Using two-wave panel data from the Current Population Survey, we find that black workers suffered temporary work hours and earnings losses relative to white workers in areas where Trump received greater electoral support. There were no within-person declines among non-Hispanic whites in high-Trump-support areas or among any groups in lower-Trump-support areas. These patterns are not driven by seasonality, industrial composition, or pre-election trends, suggesting that Trump's victory exacerbated racial disparities where he received strong electoral support. The findings reveal how political events can catalyze surges of discriminatory behavior in labor markets over the short to medium term, and they provide new evidence about the effects of Trump's early presidency on U.S. race relations.

Keywords: work hours; discrimination; racial priming

DONALD Trump's ascendance to the presidency in 2016 was characterized by a distinctly racialized politics, whereby Trump exploited, stoked, and amplified the racial resentments of his white supporters (Flores 2018; McVeigh and Estep 2019). Even as social scientists have debated the causal role of white racial resentment in the 2016 election (Abramowitz and McCoy 2019; Bobo 2017; Bonikowski 2017; Morgan and Lee 2018; Mutz 2018; Newman, Shah, and Collingwood 2018), an equally important set of questions concern the *consequences* of Trump's victory for race relations and stratification. Several studies have suggested that the politics of racial resentment unleashed by Trump's candidacy and election heightened racial animus and activated latent prejudices in the United States (Bobo 2017; Crandall, Miller, and White 2018; Flores 2018; Luttig, Federico, and Lavine 2017). However, most of the extant evidence has focused on the fringes, where Trump's victory motivated a resurgence of white nationalist groups and generated spikes in reported hate crimes (see, e.g., McVeigh and Estep 2019; Potok 2017). Less research has examined whether Trump's victory had a more systematic effect on the incidence of discrimination against minority populations. In this article, we consider the political activation of discrimination by focusing on changes in labor market outcomes of non-Hispanic black, Hispanic, and non-Hispanic white hourly workers in the direct aftermath of the 2016 election. Implicitly, much of the literature on labor market discrimination assumes a constant propensity for employers and managers to engage in discriminatory behavior over the short term (Petersen and Saporta 2004). As a result, studies tend to focus either on cross-sectional detection

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of discrimination or on longer-term shifts in response to organizational policies (Pager, Western, and Pedulla 2009; Quillian et al. 2017).

However, recent research in non–labor market domains has shown that racially salient events can trigger substantial short-term surges of prejudice and discriminatory behavior (Flores 2018; Legewie 2016). Based on these findings, we develop and test the emboldening hypothesis—the idea that the victory of a campaign that amplified racial resentments and promised aggrieved white Americans a restoration of status prompted heightened discrimination against minorities, particularly in geographic areas where Trump's broader messages resonated. We test this hypothesis by analyzing pre- and post-election changes in workers' outcomes in hourly labor markets across core-based statistical areas.

Our analysis focuses on the allocation of weekly work hours among hourly paid employees as a strategic site to gauge short-term changes in discrimination. Relative to other labor market outcomes, such as hiring and firing, the allocation of work shifts remains relatively unconstrained by formal organizational rules and bureaucratic processes (Alexander and Haley-Lock 2015; Lambert 2008). Individual managers can determine which employees receive desired work shifts, which employees are sent home early during slack periods, and which employees receive extra overtime (Halpin 2015; Wood 2018). This discretion means that managers can use hours allocation to punish certain workers and bestow favorable treatment on others (Wood 2018). Furthermore, there is some evidence that racial biases can shape these decisions (Glover, Pallais, and Pariente 2017). Although hours allocation is not the only site where an emboldening effect might appear, it represents a "most likely" case: hourly workers are especially vulnerable to discrimination, and the individualized nature of these negotiations means that politically activated increases in racial bias could result in observable shifts across racial groups even without any changes in organizational practices.

Our empirical approach treats Trump's 2016 electoral victory as a quasi-experiment (Blank et al. 2004:148–54). We apply a triple difference-in-difference design to individual panel data from the Current Population Survey (CPS). Nationally, black and Hispanic hourly workers had similar year-over-year trajectories of weekly work hours as whites in the wake of the 2016 election. We find, however, that weekly work hours trajectories diverged in areas where Trump received stronger electoral support: in core-based statistical areas where Trump won more than 60 percent of the two-party vote share,[1] black hourly workers experienced year-over-year losses of approximately 2.1 weekly work hours relative to whites during the five months following the election (relative to 12 months prior). There were no declines in weekly work hours among whites in high-Trump-support areas or among any groups in areas with lower Trump support. Placebo tests show no evidence of race-stratified changes in high-Trump-support areas during the year preceding the 2016 election, suggesting that Trump's victory served as a triggering event.

Together, the results provide evidence that Trump's racially charged electoral victory emboldened racially discriminatory behaviors in hourly labor markets during the months after the 2016 election. However, the effects of this discrimination were confined to black workers, and they dissipated within four months of the election.

Event-Driven Discrimination and the Trump Effect

Racial Discrimination in the Labor Market

Discrimination is conventionally defined as disparate treatment based on an ascriptive characteristic. Discrimination represents a behavioral phenomenon, which is distinct from attitudinal biases such as prejudices and stereotypes. An extensive literature has documented widespread racial discrimination in the U.S. labor market (Blank et al. 2004; Hirsch and Kornrich 2008; Light, Roscigno, and Kalev 2011; Lucas 2013; Neckerman and Kirschenman 1991; Pager, Bonikowski, and Western 2009; Pager et al. 2009b; Quillian et al. 2017), with much of the research focusing on hiring or wages. In a meta-analysis of historical trends in hiring discrimination, Quillian et al. (2017) found modest declines in discrimination against Hispanics between 1990 and 2010. However, discrimination against blacks has remained remarkably stable over the past three decades.

The current study departs from the existing literature on labor market discrimination in three key respects. First, we analyze disparities in work hours rather than hiring, pay-setting, or promotion. Second, whereas most prior studies have used cross-sectional data, the current analysis uses panel data to capture short-term *change* in the prevalence of discrimination. This follows from recent research in non–labor market contexts showing that salient events can trigger surges of prejudice and racially biased behaviors (Flores 2018; Legewie 2016). Methodologically, focusing on event-driven changes requires a different design than those used in cross-sectional audit studies and residual-based studies. Rather than conceiving race as a treatment, we consider Trump's election as a shock that potentially heightened the salience of racial bias against minority populations in high-Trump-support areas.

Third, our study departs conceptually from most prior research by directing attention to variations in the *impetus* to discriminate rather than variations in the *opportunities* to discriminate. Attitudinal biases play a surprisingly small role in extant accounts of labor market discrimination (Light et al. 2011; Pager and Shepherd 2008; Reskin 2000; Tilly 1998). Because in-group preferences (DiTomaso 2013) and out-group stereotypes are often conceptualized as pervasive and stable over the short to medium term (Tilly 1998), scholars have instead sought explanatory leverage by considering organizational factors that create and constrain opportunities for actors to act on biases (e.g., Goldin and Rouse 2000; Reskin 2000). Indeed, Petersen and Saporta (2004) go as far as to suggest that biases are analytically superfluous; employers will discriminate if they can (P. 856).

A few recent studies have explored the connection between *individual-level* variation in managerial biases and disparate treatment of workers. Using a cross-sectional design, Glover et al. (2017) measured the implicit racial bias of managers in the grocery industry and found that minority workers (but not white workers) worked fewer overtime hours when under the supervision of more racially biased managers. Focusing on managers' political ideologies, Briscoe and Joshi (2017) found that in contexts where managers possess substantial subjective leeway in their evaluations of employees, conservative political ideology is associated with allocating fewer performance rewards to female employees than male employees.

Following a similar logic, we focus on an arena (work hours among hourly paid) where opportunities for discrimination are assumed to be high and stable, but the propensity to discriminate is expected to vary over time and context.

Geographic Variation in the Strength of the Trump Effect

Given the highly polarizing nature of Trump's racialized campaign rhetoric (Schaffner, MacWilliams, and Nteta 2018), we do not expect a uniform behavioral response nationally. Instead, building on racial priming research in political science (Tesler 2017), we argue that Trump's racist and anti-immigrant rhetoric heightened animus toward targeted groups to a greater degree in places where Trump received greater electoral support. It is important to discuss the logic of this argument because it forms the rationale for our use of Trump's local vote share to index minority workers' varied *exposure* to Trump-specific activation of bias.

There are three reasons to think Trump's victory would have emboldened discrimination to a greater degree in areas where he received greater electoral support. First, explicit racial priming tends to elicit greater attitudinal and behavioral responses among those already predisposed to the *message* (Huber and Lapinski 2006; Hutchings, Walton, and Benjamin 2010). In some accounts, the existence of crystallized negative racial attitudes is a precondition for priming effects to occur (Tesler 2017). Expressed racial bias and resentment among whites had become increasingly polarized along partisan lines since the early 2000s, such that by the 2016 election, those whites who openly expressed grievances toward minorities were overwhelmingly likely to vote for Republican candidates (Enders and Scott 2018; Schaffner et al. 2018). Thus, white Trump voters tended to be more responsive to negative racialized cues than white non-Trump voters because they were more likely to hold preexisting racial resentments.

Second, actors are more responsive to racial priming if they are already receptive to the *messenger* (Tesler 2017). In a highly polarized environment, attitudes are malleable and responsive to shifting cues from elites. This is especially true for conservatives and Trump supporters (Barber and Pope 2019). Hence, even absent preexisting racial animus, Trump's efforts to stir racial resentment would likely have gained more resonance among his supporters than among non-supporters. Luttig and colleagues (2017) found that when white Trump voters were exposed to a black actor soliciting support for a social policy, they were more likely to oppose the policy and cast blame on the policy's beneficiaries than either Trump voters solicited by a white actor or white non-Trump voters solicited by a black actor.

Conversely, the same mechanism suggests that Trump's rhetoric was *unlikely* to have activated increased discriminatory behavior among whites who did not support Trump's candidacy. Because explicit race-baiting violates widely held norms of equality, it can alienate audiences and produce a countervailing backlash (Mendelberg 2001). By further rendering race relations into a partisan issue (Enders and Scott 2018), the explicitness of Trump's racist messages may have even galvanized antiracist responses among liberal whites (Hopkins and Washington 2020).

A final set of reasons why Trump's victory would have emboldened racial discrimination in areas where he received strong electoral support follows from social-psychological research on group processes. Perceptions of local norms moderate actors' responses to the same racialized threats and out-group demarcations (Newman et al. 2018; Smith and Postmes 2011; Tankard and Paluck 2016). Being surrounded by like-minded individuals will reduce inhibitions about expressing or acting on biases that others might keep private. Thus, a greater concentration of Trump supporters within a locality tended to amplify the emboldening effect.

These three theorized mechanisms suggest that propensities to engage in prejudicial discrimination ("taste-based" discrimination) are not constant and are amplified by the political environment. Specifically, they imply that minorities in higher-Trump-support areas would have been disproportionately exposed to emboldened biases after the 2016 election, relative to minority workers in lower-Trump-support areas. Importantly, our argument does not hinge on the claim that Trump voters were attracted to Trump *because* of his racial politics. Rather, the argument rests on a less controversial assertion: on average, Trump's appeals to white racial resentment were more likely to have emboldened discriminatory behaviors among his white supporters than among white non-supporters.

Heterogeneity in Discriminatory Treatment

It is an open empirical question regarding which groups would be most vulnerable to Trump-emboldened discrimination in the workplace. During the 2016 campaign, then-candidate Trump made charged (if not outright hostile) comments about several ethnic, racial, and religious minority groups and women in general. Notably, some of his harshest messages were directed against Hispanics, particularly individuals of Mexican origin residing in the United States. The particular prominence of anti-Hispanic immigrant messaging throughout Trump's campaign suggests that emboldened post-election discrimination might have affected Hispanics disproportionately. Prior studies have found that these targeted comments had negative, group-specific attitudinal repercussions. For example, Flores (2018) linked the timing of Trump's campaign kickoff announcement to negative shifts in public opinion toward Hispanics.

There are, however, reasons to be doubtful of a simple one-to-one relationship between the explicitness of Trump's attacks against *specific* groups and the extent of heightened discrimination against those groups. Allport (1954) argues that prejudice against any minority group is a part of a tendency to denigrate out-group members more generally. Given the breadth and frequency of Trump's racialized rhetoric during the campaign, scholars have suggested his statements are better understood as a generalized stimulation of white ethnoracial nationalism (Bonikowski 2017) rather than as independent speech acts targeting specific groups.[2]

Although Trump invoked negative images about blacks in a more oblique manner compared with Hispanics, efforts to stir animus toward out-groups generally might be expected to redound disproportionately against blacks, who remain the quintessential out-group in American society. As noted above, racialized priming by politicians tends to generate negative behavioral responses to the extent that audiences already hold negative attitudes or biases toward a primed out-group. A wide variety of evidence suggests a greater degree of social distance and negative attitudes by whites toward blacks than toward Hispanics (Alba 2020). For instance, intermarriage rates between whites and Hispanics are more than twice as high as and growing faster than intermarriage between whites and blacks (Alba 2020; Livingston and Brown 2017).

Prior research also shows that antiblack prejudice has a more significant impact on the behaviors of whites than anti-Hispanic prejudice (Hopkins 2021). Using data from a panel administered between 2007 and 2016, Hopkins (2021) found that, even after controlling for 2012 vote choice, partisanship, and a range of other social and demographic characteristics, white Americans' 2012 antiblack prejudice was a robust predictor of supporting Donald Trump in 2016, whereas anti-Latino prejudice was not, providing some preliminary evidence that anti-Latino prejudice was a less salient factor in the behaviors of Donald Trump's white supporters.

Work Hours and Stratification

We focus on changes in weekly hours worked among hourly paid workers as a strategic site to examine post-election shifts in labor market discrimination. The majority of the U.S. labor force is paid hourly (non-salaried), and racial minorities are overrepresented within the hourly workforce. In a context of hourly wage stagnation, work hours are highly desired by both part-time workers, who must stitch together sufficient shifts to survive, and middle-wage, full-time workers, who can earn valuable overtime pay for extra hours (Halpin and Smith 2017:350). Underemployment (desiring additional work hours) is a common experience for hourly workers in the United States (Kalleberg 2008; Reynolds 2003), and recent research highlights insufficient hours as a pervasive concern among hourly employees (Carré and Tilly 2012; Halpin and Smith 2017; Lambert and Henly 2010; Sturman and Walsh 2014).

Competition for work hours within establishments has been exacerbated by employers' efforts to increase labor flexibility through the use of "just-in-time" or dynamic scheduling practices. This creates a situation in which limited shifts are rationed among a large group of part-time employees, producing what Carillo and colleagues (2016) term a "reserve army of the underemployed" within establishments. In some retail and service sector industries, hours have arguably supplanted jobs and wages as the key resource on which stratification occurs (Carré and Tilly 2012; Lambert 2008; Schneider and Harknett 2019).

Hours allocation is also an analytically advantageous site for studying short-term changes in the intensity of workplace discrimination. Discretion is a necessary condition for discrimination (e.g., Light et al. 2011; Reskin 2000). There is more individual discretion in allocating work hours than in the determination of other labor market outcomes (Wood 2018), especially in non-unionized workplaces. Establishment managers often face pressure to avoid employing excess workers during slower periods, forcing them to make frequent allocation decisions about which workers to prioritize (Alexander and Haley-Lock 2015; Lambert and Henly 2010). Scheduling thereby functions as a source of power: managers use shift allocation to punish certain workers and privilege others (Wood 2018). As Halpin (2015) notes, the negotiation of the (constantly changing) work schedule is where contemporary employment relations intersect most directly with the micropolitics of managerial control.

Weak bureaucratic oversight makes hours allocation readily subject to favoritism. Surveys of retail managers highlight the ad hoc criteria supervisors use in selecting whose scheduling preferences to prioritize (Lambert and Henly 2010). Because these negotiations tend to occur informally and individually, they are particularly susceptible to ascriptive biases (Glover et al. 2017). More generally, hourly workers are vulnerable to discrimination because of the limited incursion of the employment rights revolution in the industries where hourly workers are concentrated (Dobbin 2009). Given this opportunity structure, it is plausible that politically activated *changes* in the intensity of racial biases or the perceived social acceptability of discriminating could affect minorities' labor market outcomes over the short term, even without any changes in organizational practices. In other words, hours allocation is an outcome for which there are few bureaucratic frictions between managers' biases and disparate outcomes.

Another methodological advantage of analyzing weekly work hours stems from the frequency with which these allocations are made within establishments. Whereas hiring, firing, and promotions reflect infrequent state transitions, regular schedule adjustment means that short-term changes in treatment across racial groups are more likely to register in individual panel data.

Data and Methods

Our main analysis tests the emboldening hypothesis by analyzing within-person changes in weekly work hours. We quantify the emboldening effect of Trump's election on allocative discrimination in terms of a triple difference: to what extent is the magnitude of *racial disparity* in individual workers' year-over-year weekly work hours larger in core-based statistical areas (CBSAs) where Trump received a greater share of the popular vote?

Data and Sample Definition

We use data from the panel component of the Current Population Survey (Flood et al. 2018; Rivera Drew, Flood, and Warren 2014). The CPS is a large monthly household survey conducted by the Bureau of Labor Statistics. It follows a rotating design, such that each household member is interviewed for four consecutive months, dropped for eight months, and then re-interviewed for an additional four months. Although all monthly CPS observations include information on hours worked during the previous reference week, only the fourth and eighth surveys (corresponding to the fourth and 16th calendar months) ask respondents about their earnings and hourly versus salaried status. These latter two-wave panels are known as the merged outgoing rotation groups (MORG). Because the CPS is a residential address survey, CPS panels by design only capture persons who remain in the same residence year-over-year.[3] The fact that individuals are observed 12 months apart effectively controls for work hours seasonality.

Isolating a Trump effect is complicated by the protracted nature of the campaign and the resulting temporal ambiguity of the "treatment." To best approximate a clean pre- and post-Trump design (given the constraints of the 12-month CPS panels), we limit the analysis to the November-to-March waves such that the baseline wave 1 observations precede Trump's emergence as the presumptive Republican nominee in the spring of 2016 (November 2015 to March 2016), whereas the wave 2 observations follow his electoral victory (November 2016 to March 2017).[4]

Although the timing of any Trump effect is ultimately an empirical question, we believe the November election victory represented an inflection point. It gave apparent collective validation to Trump's message of aggrievement and signaled to white supporters that it was time to "take back the country." For instance, Potok (2017) documented sharp spikes in reported hate crimes and bias incidents just after the election. We end with the March panel to minimize potential contamination of the wave 1 baseline measure if Trump's racially charged campaign was already affecting social behaviors during the spring of 2016. To the extent that Trump's rhetoric emboldened racial discrimination before March 2016 (Flores 2018), our estimates will be downwardly biased. We subjected this intuition to empirical tests below.

Our analytic sample consists of prime working age (25 to 55 years) hourly workers who appeared in an outgoing rotation group sample between November 2015 and March 2016 and who remained in the same hourly job 12 months later. We augmented the single-span MORG by backwardly linking additional prior month observations for which a MORG respondent reported being in the same hourly job during the previous month's survey (using the *empsame* indicator). Each hourly worker can thereby be observed across up to four year-to-year spans (e.g., December to December, January to January, February to February, March to March) (mean number of spans, 2.8). This method allows us to make maximal use of the CPS data structure while also effectively confining the panel analysis to observations where the respondent worked in the same job before and after the election.[5] Isolating job mobility from within-job allocational processes is important because our theorized mechanism concerns within-job changes in allocation, although supplementary analyses reveal very similar results when this restriction is loosened (see the online supplement). Figure 1 shows a schematic of the panel data structure.

Sampling on-job immobility necessitates the construction of custom probability weights to render the analytic sample representative of prime-age hourly workers. These weights account for selective attrition due to job mobility or exit from the hourly workforce and for panel attrition from the CPS data due to nonresponse or residential mobility. Here we follow the logic of LaBriola and Schneider (2020) by first reweighting the CPS-provided outgoing rotation group design weights (*EARNWT*) with IPUMS-provided panel weights (Rivera Drew et al. 2014), which account for month-to-month and year-over-year sample attrition. We then used a probit selection model to further reweight our restricted wave 2 analytic sample

Wave 1 (Pre-Election) Analytic Sample: Nov. 2015 – March 2016

Wave 2 (Post-Election) Analytic Sample: Nov. 2016 – March 2017

2015	2015	2015	2015	2015	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016	2016	2017	2017	2017	2017	2017	2017
Aug	Sep	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May	Jun	Jul	Aug	Sep	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May	Jun
A1	A2	A3	**A4**									A5	A6	A7	**A8**							
	B1	B2	B3	**B4**									B5	B6	B7	**B8**						
		C1	C2	C3	**C4**									C5	C6	C7	**C8**					
			D1	D2	D3	**D4**									D5	D6	D7	**D8**				
				E1	E2	E3	**E4**									E5	E6	E7	**E8**			
					F1	F2	F3	**F4**									F5	F6	F7	**F8**		
						G1	G2	G3	**G4**									G5	G6	G7	**G8**	
							H1	H2	H3	**H4**									H5	H6	H7	**H8**

Figure 1: Structure of CPS panel data samples used in analysis of year-over-year change in weekly work hours. *Notes:* The table cells denote distinct CPS survey cohort groups (letter) and month in sample (number). The CPS uses a rotating sample design, whereby respondents in each cohort are observed for four months, dropped for eight months, and then observed again for four months. A new cohort is added to the survey in every calendar month. Workers' hourly paid status is only asked during months 4 and 8 (denoted in bold font), but additional observations can be backwardly linked for those hourly workers who report remaining in the same job as the prior monthly survey. This produces up to four year-over-year spans per respondent (e.g., D1 to D5, D2 to D6, D3 to D7, D4 to D8). Our main analyses are restricted to the range of monthly observations highlighted in orange.

to match the demographic traits of wave 1 hourly workers, which accounts for selective job mobility and exit from hourly status.[6]

Variable Measures

The outcome variable is the total reported hours worked "last week" in hourly paid jobs.[7] Because we are interested in changes in employers' rationing of hours, we exclude observations where respondents reported working zero hours for voluntary noneconomic reasons (vacation, illness, caretaking responsibility). To reduce the effect of outliers and measurement errors, we winsorized the weekly hours measure at the 99th percentile (approximately 80 hours).

We coded respondent race into exclusive non-Hispanic white, black, and Hispanic categories.[8] Hispanic is treated as a superseding category (Massey 2009). All others, including Asian Americans, are omitted because of excessively small sample sizes.

We measure local political and labor market context at the CBSA level. CBSAs approximate the spatial scope of labor markets and are sufficiently granular to capture substate variations in local political contexts. Varying levels of racial prejudice and domination have long been structured by local substate processes (e.g., McVeigh and Estep 2019). We acquired county voting data from Leip (2017), which we aggregate to the CBSA level. We measure local exposure to the Trump effect in terms of Trump's share of the total two-party vote in a CBSA.

One methodological difficulty is that public-use CPS files suppress the location of respondents in micropolitan and rural counties for confidentiality (these cases compose 27.9 percent of the unweighted sample). We adopt two different approaches to deal with non-metropolitan respondents. For the main results presented below, we created a synthetic non-metropolitan "remainder CBSA" for each state and then imputed Trump vote share exposure for respondents in each state–race cell by calculating a weighted average of Trump's vote share across all non-metropolitan (but including micropolitan) counties in a state, weighted separately for each racial group by the county's share of that group's non-metropolitan population in the state.[9] This race-specific, population-weighted approach induces between-race variation in rural respondents' measured exposure to local Trump, but the approach is preferable to assigning all non-metropolitan respondents in a state a single mean value of Trump vote share because it accounts for racial segregation (and associated political heterogeneity) across non-metropolitan counties. This is particularly relevant in the rural South, where racial clustering means that blacks tend to reside disproportionately in counties with relatively lower Trump vote share compared with other rural counties. Our second approach to deal with non-metropolitan respondents is to exclude them from the analytic sample altogether, thereby confining the analysis to metropolitan areas. This results in a smaller and more restricted sample but more straightforward interpretation. In practice, both approaches yield substantively identical results (see Table 2 below).

Table 1 shows unweighted descriptive statistics of the analytic sample by race. The bottom four rows show the distribution of analytic sample observations for each racial group across the distribution of local Trump support.

Model Estimation

We specified a two-period lagged dependent variable (LDV) model (Menard 2002), also known as ANCOVA, conditional change score, or residualized gain score model (Johnson 2005),

$$\begin{aligned}(y_{it2}) = \gamma y_{it1} + \beta_0 + \beta_1 race_i + \tau\,(race_i \times tvote_{locale}) \\ + \beta_3 controls + \delta_{locale} + e,\end{aligned} \tag{1}$$

where the outcome is the post-election weekly hours worked among hourly paid workers, γ represents the parameter for lagged (baseline) hours worked, *tvote* is Trump's share of the two-party vote in the local labor market (CBSA), and δ_{locale} represents CBSA-level fixed effects. The key interaction term τ captures how, conditional on pre-election weekly hours worked, the magnitude of racial differences (relative to the omitted baseline white group) in workers' year-over-year change in hours varies as a function of local Trump support.

It is important to clarify that this approach does not identify an absolute estimate of racial discrimination. Rather, subject to the identifying assumption that the potential outcomes are independent conditional on lagged hours and other covariates,[10] the interaction estimate can be interpreted as the event-induced *effect*

Table 1: Unweighted descriptive statistics of analytic sample, by race

	Non-Hispanic white			Black			Hispanic		
	Mean	SD	Freq.	Mean	SD	Freq.	Mean	SD	Freq.
Hours worked last week	38.29	10.57		39.07	8.82		38.47	8.80	
Change in hours worked from prior year	−0.13	9.16		0.09	8.97		−0.30	9.08	
Trump vote share in CBSA	0.52	0.14		0.47	0.11		0.41	0.12	
Age	41.20	9.09		41.11	9.01		39.62	8.44	
Educational attainment									
Less than high school	0.04		347	0.07		76	0.32		641
High school diploma	0.33		2,601	0.41		446	0.33		660
Some college	0.40		3,168	0.38		411	0.25		503
Bachelor's or higher	0.23		1,777	0.15		159	0.09		184
Industry									
Extraction and utilities	0.03		269	0.01		10	0.04		85
Construction	0.08		648	0.03		36	0.13		264
Manufacturing	0.12		931	0.15		159	0.14		274
Retail	0.13		1,028	0.15		161	0.09		181
Transportation, warehousing	0.06		470	0.07		75	0.05		102
Professional, admin. service	0.13		1,019	0.13		142	0.12		247
Ed., health, public admin.	0.33		2,630	0.35		380	0.19		370
Accommodation, food service	0.11		898	0.12		129	0.23		465
Occupation									
Management and business operations	0.08		592	0.07		72	0.04		72
Engineering, sciences	0.03		249	0.03		30	0.03		50
Education, social, media	0.07		550	0.04		43	0.04		75
Healthcare and protective	0.18		1,403	0.18		194	0.07		148
Food and personal service	0.20		1,559	0.26		289	0.33		648
Sales and office	0.17		1,308	0.17		191	0.11		214
Construction, maintenance, extraction	0.16		1,277	0.11		121	0.24		484
Production	0.05		423	0.07		71	0.07		148
Transit	0.07		532	0.07		81	0.07		149
Trump vote share in CBSA									
Trump vote <40%	0.23		1,796	0.25		272	0.49		981
Trump vote 40%–50%	0.22		1,726	0.31		337	0.27		543
Trump vote 50%–60%	0.24		1,882	0.36		393	0.18		357
Trump vote >60%	0.32		2,489	0.08		90	0.05		107

Notes: The analytic sample includes prime-age, hourly workers in CPS outgoing rotation groups employed in the same hourly job (detailed industry) across pre-election (November 2015 to March 2016) and post-election (November 2016 to March 2017) survey waves.

on discrimination, where the dosage of event exposure is indexed by Trump's share of the local vote.[11]

The model controls for worker characteristics (proxied by the lagged outcome), while CBSA fixed effects capture variations in labor market tightness and other local features that affect all hourly workers, and the group dummies capture unobserved race-specific trends that affect all hourly workers in a given group nationally. We also add industry and occupation fixed effects to ensure that any observed relationship between Trump's vote share and racially disparate changes is not an artifact of geographically differentiated industry shocks or local shifts in labor demand for occupational tasks correlated with race. Because each respondent can be observed across multiple spans, we calculated respondent-clustered standard errors.

Results

Descriptive Patterns of Work Hours Change among Continuously Employed Hourly Workers

We begin by considering aggregate patterns of year-over-year change in weekly hours within the analytic sample by race and electoral geography. Figure 2 plots weighted mean change by group. Despite the tightening labor market (the national unemployment was 0.3 percentage points lower during the follow-up period compared with the baseline period), there is little evidence of significant overall growth in mean weekly hours worked by the continuously employed. The more salient between-group differences are between-gender, with women tending to see a more substantial increase in hours relative to men. This differential is most pronounced among Hispanic/Latino workers, where women experienced a mean growth of 0.75 hours per week, whereas Hispanic men experienced a significant mean loss of 1.0 hours nationally over this period.

The picture of relative stability and invariance in the average national trends by race obscures substantial variation *within* groups. Even after suppressing outliers, the standard deviation of the year-over-year change in the analytic sample is 10 hours for men and 9 hours for women (not shown). The bottom panels of Figure 2 provide some initial suggestive evidence of differential race-specific trajectories across political geography, unadjusted for occupation and industry. White hourly workers tended to see slight positive gains in places with greater Trump support, but not in CBSAs with less than 40 percent Trump support. Meanwhile, black workers tended to gain hours year-over-year in lower-Trump-support CBSAs and lose hours in higher-Trump-support CBSAs. The year-over-year changes for Hispanic workers tended to be more variable and less clearly tied to differences in electoral geography.

CPS Panel Data Analysis

Is local support for Trump is associated with greater *conditional* racial disparities in workers' year-over-year change in hours from late 2015 to late 2016? Table 2 shows the main panel regression results with the linear measure of Trump vote share. The first column shows a simple baseline model. The second and third models add CBSA fixed effects (including a non-metropolitan residual dummy for each state), as well as additional demographic controls and industry and occupation fixed effects. Following the standard convention in labor market studies of estimating separate slopes for men and women, a fourth specification allows the Trump effect to vary by gender by including a triple interaction.

The key coefficient estimates of interest are the interaction terms between race and Trump vote share. These coefficients represent the differential expected change

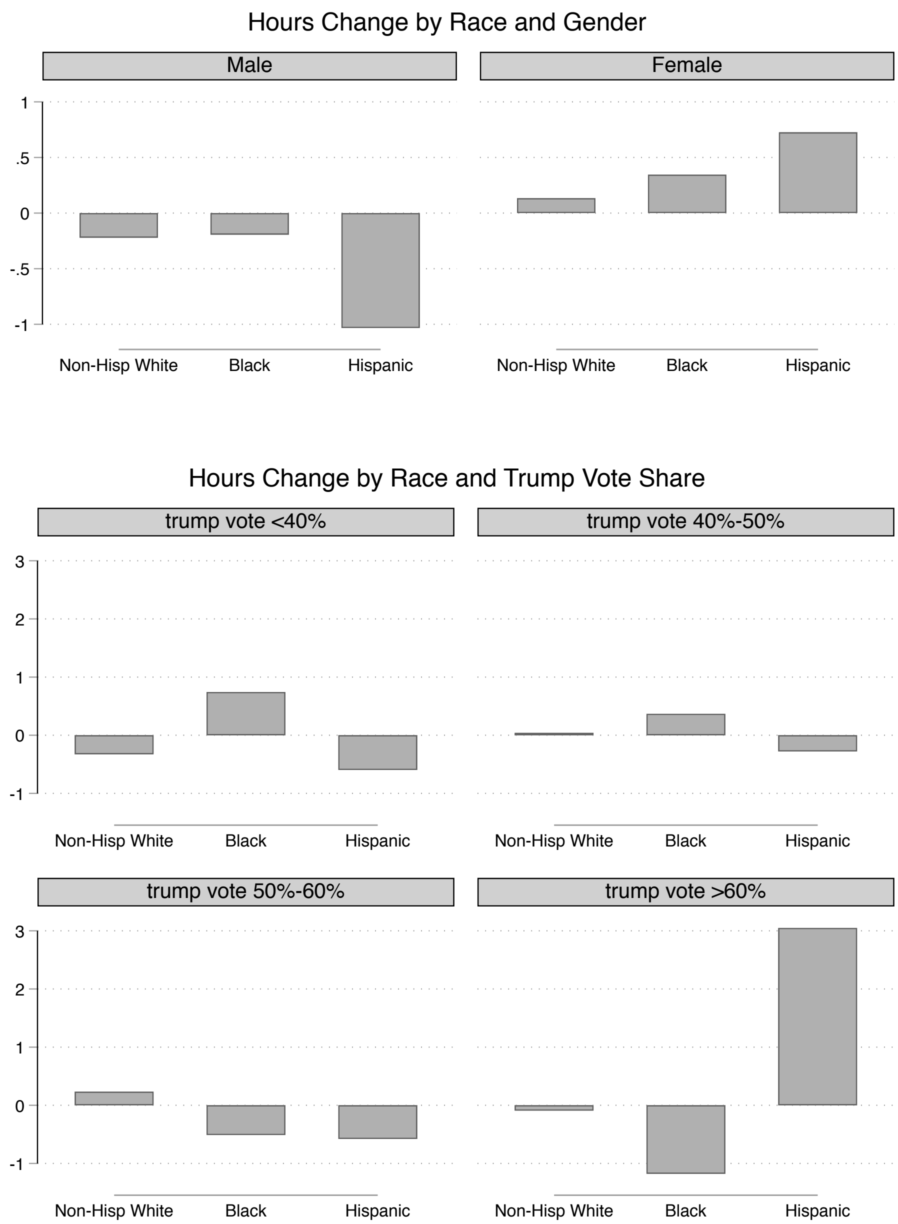

Figure 2: Weighted mean year-over-year change in weekly work hours among hourly workers, November 2015 to March 2017. *Notes:* This figure shows survey-weighted mean changes in year-over-year weekly work hours within analytic sample of prime-age hourly workers employed in the same job from November 2015 to March 2017. $N = 15,962$ person–week observations (7,981 year-over-year spans).

for each racial minority group relative to non-Hispanic whites for a "one unit" (100 percent) increase in Trump's vote share. We refer to this estimate as the "Trump effect."

We turn first to the unadjusted baseline model. The positive Trump vote share coefficient indicates that whites residing in CBSAs with greater Trump support saw greater gains in work hours relative to whites in lower-Trump-support CBSAs. Because the vote share variable is scaled from zero to one, the coefficient estimate of 3.95 indicates that each 10 percent increase in Trump's share of the local vote (i.e., from 45 percent to 55 percent) is associated with a modest pre- to post-election gain of approximately 0.395 hours additional paid work time per week for whites during the November-to-March period. This means that non-Hispanic white hourly workers tended to gain slightly more hours over this period if they resided in higher-Trump-support locales.

We find no evidence of a significant Trump effect on the differential trajectory between non-Hispanic whites and Hispanic/Latino workers. Across electoral geography, Hispanics' mean year-over-year changes tracked closely to those of whites. For instance, in model 1, the Trump $\times$ Hispanic slope is slightly lower than the Trump effect for whites but statistically indistinguishable in relative terms. As shown in models 2 through 4, the Hispanic–white difference-in-difference remains insignificant after conditioning on locality, industry, and occupation fixed effects. Model 4 shows a slight gender differential in the estimated Trump effect among Hispanics, with men exhibiting a negative slope and women exhibiting a positive slope. However, neither is statistically different from zero nor from one another.

In contrast to Hispanics, the results in Table 2 show that year-over-year trajectories for black workers relative to whites depended on the local political context. Consistent with the implications of the emboldening hypothesis, the estimated black–white disparity was significantly greater in higher-Trump-support locales. The coefficient estimate of -10.31 for the black $\times$ Trump vote share term in model 1 can be interpreted to mean that an increase of 10 percent in Trump's vote share (e.g., from 45 percent to 55 percent) is associated with a -1.03 hour differential change for black workers relative to whites, compared with the differential change in a locale with lower Trump support.

As shown in models 2 through 4, the estimated Trump effect on black–white disparities is even greater after conditioning on CBSA, industry, and occupation fixed effects. This implies that the disparate hours losses experienced by black workers in high-Trump-support areas are not simply a reflection of the industry or occupational positions in which blacks tend to be employed in Trump-leaning regions. Model 4 indicates that the Trump effect on post-election black–white gaps is comparable among both men and women.[12]

Finally, column 5 shows a model specification equivalent to model 3 but excluding cases in the non-metropolitan synthetic or remainder "CBSAs." These analyses confirm a significant Trump effect on black–white disparities in the metropolitan-only subsample and confirm that the findings are not driven by biases resulting from the uneven aggregation of metropolitan and non-metropolitan synthetic "CBSAs." Model 5 in Table 2 does not include an absolute estimate of the baseline relationship between Trump vote share and changes in hours among whites because of the fixed

Table 2: Estimates from two-period lagged dependent variable models of weekly hours worked among hourly paid workers, pre- and post-2016 election

	Base model	CBSA fixed effects (incl. rural residual)			Metro only
	(1)	(2)	(3)	(4)	(5)
Lag hours worked	0.560†	0.547†	0.509†	0.508†	0.534†
	(0.019)	(0.019)	(0.019)	(0.019)	(0.022)
Trump vote	3.950†	1.604	0.820	1.507	
	(1.307)	(4.657)	(4.767)	(4.953)	
Black	5.241†	5.730†	5.716†	4.344	5.886†
	(1.657)	(2.076)	(2.083)	(3.183)	(2.253)
Hispanic	0.754	0.384	0.919	0.972	1.936
	(1.128)	(1.591)	(1.629)	(1.933)	(1.751)
Black × Trump vote	−10.31†	−12.71†	−12.54†	−11.31	−12.78†
	(3.211)	(4.209)	(4.227)	(6.395)	(4.774)
Hispanic × Trump vote	−0.438	0.0843	−0.808	−2.526	−3.167
	(2.467)	(3.680)	(3.774)	(4.416)	(4.173)
Age			0.059	0.061	−0.034
			(0.157)	(0.156)	(0.190)
Age squared (/1,000)			−0.724	−0.728	0.597
			(0.192)	(0.191)	(0.233)
Female			−1.500†	−1.912	−1.498†
			(0.334)	(1.465)	(0.383)
Education: high school diploma			1.284†	1.170*	1.512†
			(0.450)	(0.456)	(0.499)
Education: some college			1.394†	1.299*	1.597†
			(0.519)	(0.526)	(0.589)
Education: bachelor's or higher			1.007	0.960	1.508*
			(0.604)	(0.605)	(0.693)
Female × Trump vote				−0.483	
				(2.539)	
Female × black				2.844	
				(3.706)	
Female × Hispanic				−0.162	
				(2.296)	
Female × black × Trump vote				−2.930	
				(7.328)	
Female × Hispanic × Trump vote				4.204	
				(4.974)	
Constant	14.76†	16.49†	17.30†	17.43†	17.79†
	(1.036)	(2.402)	(3.964)	(4.057)	(4.048)
Industry fixed effects	No	No	Yes	Yes	Yes
Occupation fixed effects	No	No	Yes	Yes	Yes
Local area fixed effects	None	CBSA	CBSA	CBSA	CBSA
Observations	8,330	8,330	8,330	8,330	5,857
R-squared	0.330	0.380	0.393	0.395	0.412

Notes: Standard errors in parentheses. Regression is estimated using two-wave year-over-year LDV model with respondent-clustered standard errors. The analytic sample includes prime-age, hourly workers employed in the same hourly job (detailed industry) across pre- and post-election waves. Probability weights are applied to adjust for survey design, MORG sample attrition, and attrition from wave 1 hourly job. † $p < 0.01$; ∗ $p < 0.05$.

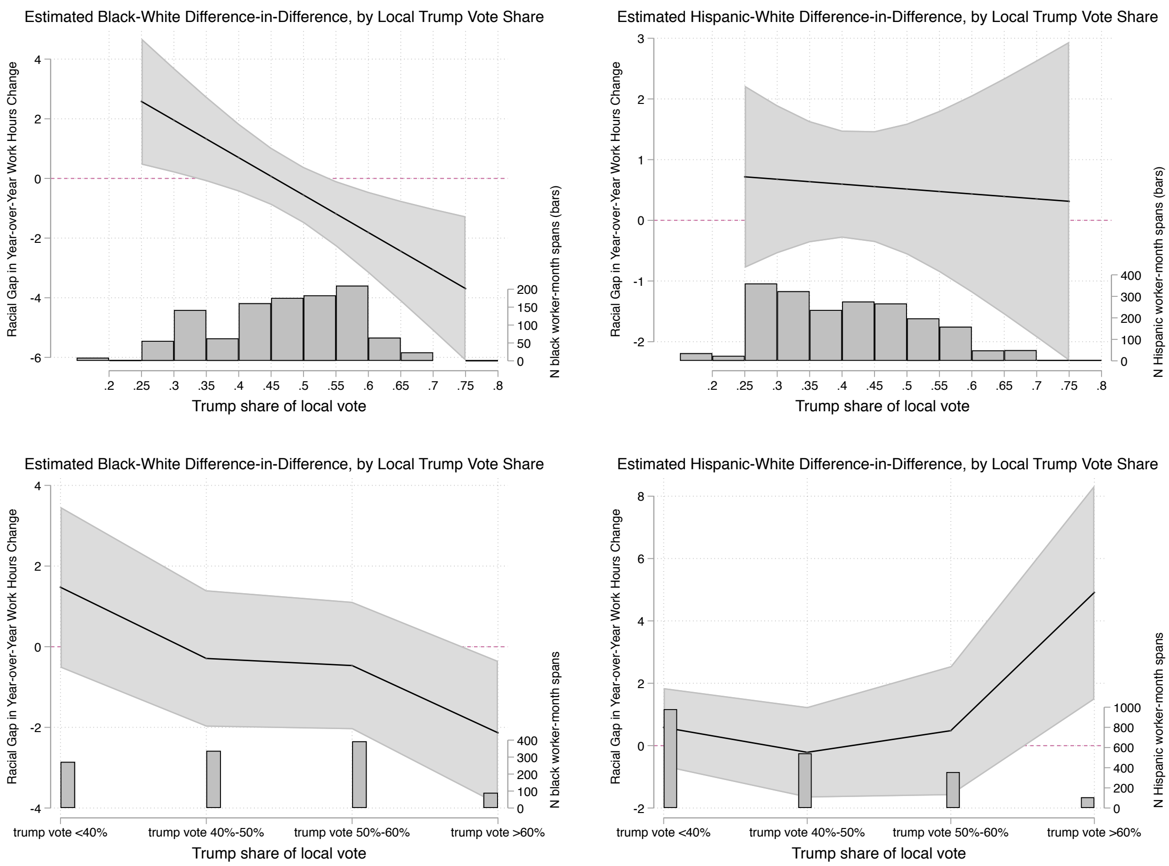

Figure 3: Estimates of racial disparities in workers' year-over-year weekly work hours change, pre- and post-2016 election. *Notes:* Estimates from lagged dependent variable models estimated on sample of prime-age hourly workers employed in the same job year-over-year from November 2015 to March 2017. Models include CBSA, industry, and occupation fixed effects. The top two panels are based on model 3 in Table 2. The bottom two panels use an equivalent specification but substitute a binned measure of Trump vote share. Probability weights are applied to adjust for survey design, sample attrition, and attrition from wave 1 hourly job. Standard errors are clustered by respondent.

effects and the fact that the vote share variable is invariant within metropolitan areas.

The significance of the Trump effect on blacks is most apparent when rendered graphically. Figure 3 plots the estimated difference in the mean year-over-year change in hours between each minority group and non-Hispanic whites (represented by the horizontal x axis). The top two panels show projections from the linear model specification in column 3 of Table 2. (Plots from model 4 with separate race–gender slopes are shown in the online supplement.) The two bottom panels meanwhile show estimates from a separate specification, which uses binned categorical levels of the Trump vote share to ensure that the point estimates are not simply an artifact of the linearity constraint in the main models.

The left panels in Figure 3 highlight the black–white divide in pre- and post-election trajectories across political geography. Conditional on industry and occupation, black workers exhibited parity or even modest year-over-year gains relative to whites in mid- and low-Trump-support CBSAs. However, in places where Trump captured at least 60 percent of the vote, black workers experienced disparate losses of approximately two hours relative to white workers. The relative gains by black workers in low-Trump-support areas are consistent with what we would expect based on the macroeconomic business cycle, given the general tendency for disadvantaged groups to realize more rapid economic gains during periods of overall labor market tightening such as existed in 2016 and 2017 (Aaronson et al. 2019; Cajner et al. 2017). By contrast, the disparate losses for black workers in high-Trump-vote areas represent an anomaly, given that they occur in a context of increasing average overall work hours within these CBSAs (see Figure A1 in the online supplement).

In the case of the black–white gap, the similarity between the linear and binned parameterizations validates the linearity assumption and confirms the negative point estimate in CBSAs with a greater than 60 percent Trump vote share. The binned estimates also confirm that the Trump effect is driven at least as much by blacks' disparate losses relative to whites in high-Trump-support areas as by disparate gains for black workers in low-Trump-support areas.

Although the confidence intervals are relatively wide, the estimated differential trajectories for blacks and whites in high-Trump-support CBSAs represent economically significant disparities in paid work time. To put these figures in perspective, the estimated conditional black–white difference-in-difference in areas where Trump received more than 60 percent of the vote (-2.1 weekly hours) is comparable to the average year-over-year losses experienced by continuously employed male hourly workers during the most rapidly deteriorating periods of the 2008-to-2009 Great Recession.[13] In contrast to that recessionary period, however, black workers' post-2016 election losses occurred in the context of increasing overall work hours in high-Trump-support CBSAs.

Among Hispanics, there is some evidence of nonlinearity across the spectrum of local Trump support. The binned estimates even show a marginally significant gain for Hispanics relative to whites in high-Trump-support CBSAs. Unlike the negative estimates for the black–white difference, however, this surprisingly positive point estimate for Hispanic–white differences is not robust to alternative model specifications.

Overall, the results in Figure 3 lend partial support to the emboldening hypothesis insofar as hourly paid black workers experienced disparate losses of work hours following the 2016 election in high-Trump-support locales. However, there is no evidence of disparate work hours losses among Hispanics in higher-Trump-support areas.

We subjected the above analysis to numerous sensitivity checks. First, we sought to rule out other potential sources of confounding through a series of alternative specifications, which are reported in Tables A2 and A3 in the online supplement. Interpreting the τ parameter as a Trump-induced emboldening effect presumes that the relationship is driven by political context, rather than by some other contemporaneous, racially differentiated labor market shift correlated with the Trump vote share. For instance, if there was a high but stable tendency toward racial discrimination in high-Trump-support areas, exogenous yet targeted declines in local labor demand (e.g., from a plant closure or a severe weather event) could have disproportionately undermined the work hours of black workers even had Trump not won the election.

To check these possibilities, we first specified an alternate model specification with industry-by-state fixed effects (rather than separate additive fixed effects for locales and industries) to capture geographically uneven industry shocks. Second, we applied an alternative two-wave triple difference-in-differences model specification without the lagged dependent variable, which allows for additional time-varying controls for local unemployment. Both yield similar results (see Tables A2 and A3 in the online supplement).[14]

Finally, another possibility is that some portion of the observed Trump effect could be due to heterogeneity in the substitutability of work hours between groups. Opportunities for group-specific discrimination depend on the presence of competing workers who will take those work shifts. Thus, a key contingency that may mediate responses to election-induced emboldening is the availability of other-group workers in the occupations where a focal group is concentrated in a local labor market. Although the main analysis above included broad occupation and industry fixed effects, this could obscure local differences in racial clustering within particular niches (e.g., Rosenfeld and Tienda 1999; Waldinger 1996). For instance, the overrepresentation of Hispanics in jobs such as meat processing might insulate them from some of the effects of activated biases. We examined this possibility by adding additional controls that capture the degree of own-group overrepresentation in every CBSA-detailed occupation cell.[15] Contrary to the niching explanation, minority workers in positions where they composed a greater share of the local workforce fared no better following the election, and inclusion of this variable in the model has no substantive effect on our estimates of the Trump effect for either blacks or Hispanics (see Table A3 in the online supplement). Thus, the lack of a discernable Trump effect on Hispanic–white gaps cannot be attributed to Hispanic workers' high rate of occupational niching.

In addition to the above analyses, we systematically assessed the sensitivity of the results to various coding and modeling decisions (Young and Holsteen 2017). Here, we alternately relax or constrict sample inclusion restrictions, constrict the follow-up period to exclude November 2016,[16] modify variable definitions, and alternate control variables. The results of these alternative specifications confirm a negative Trump effect on changes in black workers' hours relative to whites and a null effect on Hispanic workers' hours (see Table A4 and Figure A3 in the online supplement).

Additional Analyses of the Trump Effect on Work Hours Discrimination

Given that the observed Trump effect is confined to black–white disparities, this section probes the effect in greater detail. These results help clarify the scope and

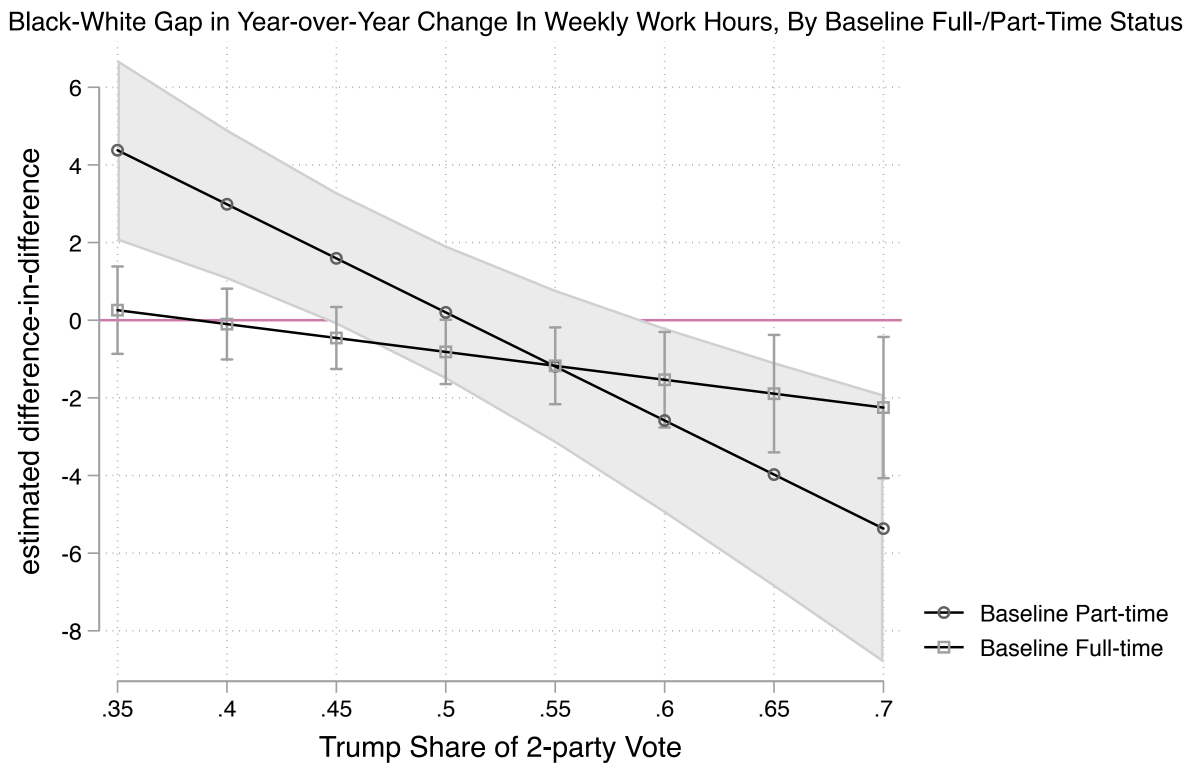

Figure 4: Estimates of year-over-year change in weekly work hours among hourly paid men, by full- and part-time status at baseline. *Notes:* This figure shows estimates from lagged dependent variable models estimated on sample of prime-age hourly workers employed in the same job year-over-year from November 2015 to March 2017. Models include CBSA, industry, and occupation fixed effects, as well as a triple interaction between local Trump vote share, race, and full- and part-time status at wave 1 baseline. Probability weights are applied to adjust for survey design, sample attrition, and attrition from wave 1 hourly job. Standard errors are clustered by respondent.

potential mechanisms underlying the racial differences reported above. The fact that black workers in high-Trump-support areas experienced absolute work hour declines in the context of a tightening labor market is notable, but it also raises further substantive questions:

Where in the work hours distribution do racially disparate losses occur? Are the effects of emboldened discrimination on work hours concentrated among full-time or part-time hourly workers? To answer this question, we incorporated a triple interaction between race, local Trump share, and an indicator variable for whether someone worked 35 hours at an hourly job(s) during the baseline period. These results are shown in Figure 4. The observed racial disparity in the year-over-year change appears in both subgroups, suggesting that the Trump effect was felt broadly among both low- and high-hours workers. However, the political gradient is significantly steeper among part-time positions. This likely reflects the greater degree of allocative discretion among managers of part-time workers.

Are post-election shifts a continuation of pre-election trends? It is possible that black workers in high-Trump-support areas were already experiencing heightened discrimination prior to the election. In that case, the observed association could reflect the continuation of pre-election trends rather than an election-induced emboldening. Unfortunately, the short panel design of the CPS does not permit the examination of individuals' extended pretreatment trajectories. We can, however, examine whether Trump's vote share is associated with changes in racially disparate year-over-year changes during earlier panels.

Figure 5 shows the evolution of the Trump effect by specifying a model in which the interaction coefficient is allowed to vary over bimonthly spans.[17] Prior to the November 2016 election, there is *no* evidence of racially differentiated work hours changes (relative to 12 months prior) due to Trump's vote share. This noneffect over the previous period lends credence to the notion that racially disparate shifts in work hours allocations emerged specifically in the wake of the election outcome rather than during the preceding campaign period.

How long does the Trump effect persist? A closely related issue also addressed in Figure 5 is the span over which the event-driven shock persists after the election. The main results reported above are based on the average year-over-year change during the five-month follow-up window from November 2016 to March 2017, a span selected for methodological reasons given the limitations of the 12-month CPS panels. The results in Figure 5 suggest that the Trump effect was, in fact, slightly shorter-lived. Black workers experienced abnormally disparate changes relative to whites during the four months following the election, after which the triple difference re-converged toward zero. This, combined with the evident lack of a pre-election Trump effect during the spring or summer of 2016, suggests that the dissipation of the post-election shock after February 2017 is real rather than an artifact of baseline contamination.[18]

The fact that the emboldening effect decays within four months of the election is consistent with prior research on racial priming (Tesler 2015) and event-activated discrimination (Legewie 2016), both of which suggest that events that amplify the salience of racial cues spark real but transient effects on social behaviors. We return to this issue in the discussion section below.

Trump effect or Republican effect? The explicitness of Donald Trump's appeals to white racism during the 2016 campaign represented a departure from other recent Republican presidential candidates. This raises the question of whether the emboldening effect of Trump's victory was particularly resonant in areas that swung more heavily Republican in 2016 compared with 2012 or whether mounting racial resentment in longtime Republican strongholds had primed them to be exceptionally responsive to Trump's racial rhetoric (Tesler 2016). To test this, we added a second interaction term between race and Mitt Romney's 2012 local vote share, which effectively conditions the Trump effect estimate on the baseline level of Republican party support. These results reveal that, for a given level of local Trump vote share, the emboldening effect of Trump's 2016 victory on racial discrimination was comparatively *more* pronounced in places that were already more strongly Republican in 2012 (see Figure A4 in the online supplement).

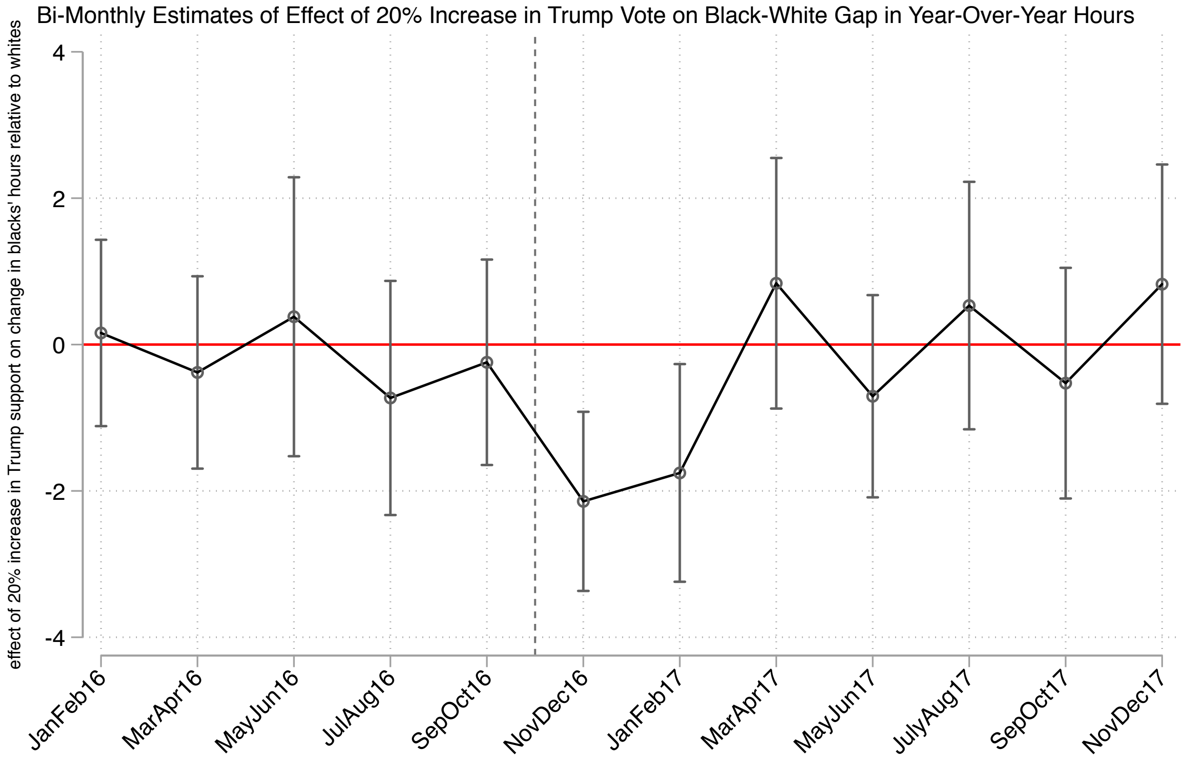

Figure 5: Evolution on the Trump effect: bimonthly estimates of Trump effect on racial disparities in trailing year-over-year weekly hours change. *Notes:* This figure shows bimonthly marginal effects estimates of 2016 Trump vote share variable on the black–white disparity in year-over-year changes in weekly hours worked. Estimates are based on rolling, two-wave LDV models, using sample of prime-age hourly workers employed in the same job year-over-year from January 2015 to December 2017. Models include CBSA, industry, and occupation fixed effects. Probability weights are applied to adjust for survey design, sample attrition, and attrition from wave 1 hourly job. Standard errors are clustered by respondent.

Employer treatment or changing employee behavior? The observed difference-in-differences above are consistent with heightened allocative discrimination in high-Trump-support areas. However, such an interpretation presumes racially differentiated changes are driven by employer behavior rather than by employee behavior. It is possible that some workers in high-Trump-support areas were voluntarily reducing their total hours. Such reductions could reflect avoidant responses to heightened racial hostility in the workplace (Glover et al. 2017). In this case, activation of racial animus suppresses minority hours, but the mechanism operates through workers' responses to generalized hostility rather than allocational discrimination by managers.

We sought to disentangle avoidance and discrimination mechanisms by considering salaried employees as a placebo group. Like hourly employees, salaried employees report hours worked in the CPS. And like hourly workers, salaried workers might reduce or minimize time at work in response to heightened work-

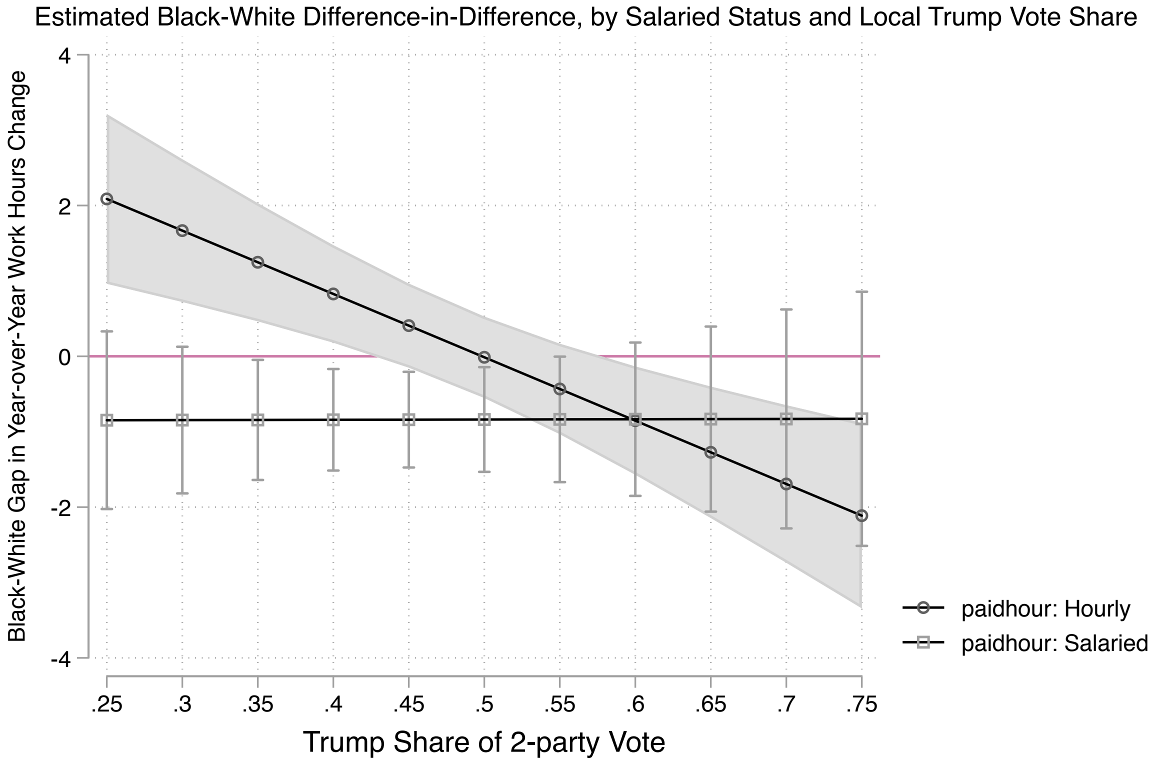

Figure 6: Placebo test: year-over-year change in weekly hours among salaried versus hourly workers. *Notes:* This figure shows estimates from lagged dependent variable models estimated on sample of prime-age hourly workers employed in the same job year-over-year from November 2015 to March 2017. Models include CBSA, industry, and occupation fixed effects, as well as a triple interaction between local Trump vote share, race, and salaried or hourly status. Probability weights are applied to adjust for survey design, sample attrition, and attrition from wave 1 hourly job. Standard errors are clustered by respondent.

place hostility. However, because salaried workers are not subject to allocative hours discrimination, any observed association between local Trump support and declining hours worked would presumably reflect other mechanisms.

Figure 6 shows this placebo test. We expanded the sample to all workers employed in the same job year-over-year and incorporated a triple interaction between salaried status, race, and local Trump share. We find no relationship between Trump's vote share and racially disparate changes in weekly hours among salaried persons. Of course, the absence of any Trump effect on changes in reported work hours among salaried workers does not entirely rule out the possibility that some portion of the observed Trump effect among hourly paid workers was driven by another mechanism, but it does provide additional indirect evidence for our interpretation of the main analysis result as reflecting allocative discrimination.

Weekly Earnings

Finally, we tested the emboldening hypothesis using total weekly earnings among hourly paid workers. If the Trump effect prompted managers to withhold opportunities to black hourly workers, this should be manifested in loss of total earnings in addition to hours. Table 3 shows LDV specifications in which we model the year-over-year change in *log weekly earnings*. These analyses are based on a smaller sample because earnings information is only available in the outgoing rotation group months. Hence, we can only observe a single year-over-year span for each respondent. Earnings trajectories exhibit similar patterns as work hours. Based on a linear specification, each 10 percent increase in Trump's vote share is associated with a -7.6 percent differential year-over-year loss of earnings for hourly paid black workers relative to non-Hispanic whites. There is little evidence of gender-differentiated differences within racial groups. These convergent results imply that the transient decline in blacks' work hours relative to whites in high-Trump-support locales was not a compensating response to increased wages.

Explicit Bias Survey Data

The CPS results show that black workers' hours diminish in high-Trump-support places, but they do not directly connect this to heightened racial animus among whites. To assess the credibility of the putative "activation of bias" mechanism, we conducted a supplementary attitudinal analysis of post-election shifts in expressed antiblack racial bias among whites. Here we used geolocated survey data from the Project Implicit public archive (Xu, Nosek, and Greenwald 2014) to assess whether *explicit* bias increased to a greater extent following the election in high-Trump-support counties compared with low-Trump-support counties. These results (reported in the online supplement) show that patterns of changes in bias align with the changes in minorities' work hours in terms of both timing and distribution across political geography.

Discussion

A growing body of research has shown that racial biases increasingly inflect political identities and beliefs in the post-Obama era (e.g., Enders and Scott 2018; Luttig et al. 2017; Tesler 2016; Wetts and Willer 2018), a trend that raises questions about potential spillover effects on other social behaviors.

We advance this line of inquiry by examining the possibility that Donald Trump's 2016 election sparked heightened discrimination against minority groups in hourly labor markets. This possibility, which we term the emboldening hypothesis, has been advanced by commentators but not rigorously studied. Using two-wave panel data from the CPS, we found that black hourly workers residing in labor markets where Trump received greater electoral support experienced disparate declines in hours worked during the four months following the election, compared with modest increases among hourly white workers in those CBSAs. This pattern occurred among both men and women and held across numerous alternative

Table 3: Earnings: estimates from two-period lagged dependent variable models of weekly (log) earnings among hourly workers

	Base model	CBSA fixed effects (incl. rural residual)			Metro only
	(1)	(2)	(3)	(4)	(5)
Lag (log) earnings	0.600†	0.597†	0.479†	0.478†	0.479†
	(0.032)	(0.033)	(0.032)	(0.032)	(0.032)
Trump vote	0.013	−0.527	−0.458	−0.454	−0.458
	(0.085)	(0.516)	(0.465)	(0.472)	(0.465)
Black	0.148	0.292	0.290	0.294	0.290
	(0.149)	(0.164)	(0.157)	(0.237)	(0.157)
Hispanic	0.031	0.057	0.163	0.166	0.163
	(0.082)	(0.113)	(0.107)	(0.128)	(0.107)
Black × Trump vote	−0.428	−0.799*	−0.761*	−0.820	−0.761*
	(0.298)	(0.335)	(0.320)	(0.473)	(0.320)
Hispanic × Trump vote	−0.116	−0.156	−0.349	−0.408	−0.349
	(0.179)	(0.257)	(0.238)	(0.287)	(0.238)
Age			0.036	0.038	0.036
			(0.105)	(0.105)	(0.105)
Age squared (/1,000)			−0.010	−0.032	−0.010
			(0.131)	(0.131)	(0.131)
Female			−0.141†	−0.183*	−0.141†
			(0.024)	(0.093)	(0.024)
Education: high school diploma			0.121†	0.118†	0.121†
			(0.033)	(0.034)	(0.033)
Education: some college			0.148†	0.146†	0.148†
			(0.037)	(0.037)	(0.037)
Education: bachelors or higher			0.255†	0.254†	0.255†
			(0.044)	(0.044)	(0.044)
Female × Trump vote				0.046	
				(0.162)	
Female × black				−0.004	
				(0.284)	
Female × Hispanic				−0.018	
				(0.158)	
Female × black × Trump vote				0.102	
				(0.563)	
Female × Hispanic × Trump vote				0.166	
				(0.353)	
Constant	2.594†	2.864†	3.607†	3.621†	3.607†
	(0.215)	(0.374)	(0.382)	(0.387)	(0.382)
Industry fixed effects	No	No	Yes	Yes	Yes
Occupation fixed effects	No	No	Yes	Yes	Yes
Local area fixed effects	None	CBSA	CBSA	CBSA	CBSA
Observations	3,728	3,728	3,728	3,728	3,728
R-squared	0.399	0.458	0.516	0.517	0.516

Notes: Standard errors in parentheses. Regression is estimated using two-wave year-over-year LDV model with respondent-clustered standard errors. The analytic sample includes prime-age, hourly workers employed in the same hourly job (detailed industry) across pre- and post-election waves. Probability weights are applied to adjust for survey design, MORG sample attrition, and attrition from wave 1 hourly job. † $p < 0.01$; ∗ $p < 0.05$.

specifications. Black hourly workers also experienced corresponding disparate declines in weekly earnings. Despite Hispanics being a frequent target of Trump's racist and xenophobic rhetoric during the 2016 campaign, we found no effect on Hispanics' trajectories relative to non-Hispanic whites (some specifications even suggest differential increases in work hours for Hispanics in high-Trump-support areas).

Taken together, the results suggest that Trump's election emboldened racial discrimination against black hourly workers in high-Trump-support areas. However, these event-driven effects were relatively short-lived, dissipating within four months.

Next, we explore three specific questions raised by our results: these concern underlying mechanisms, variation in the impact of emboldening across target groups, and timing. We then outline the broader implications for research on racial politics, racial stratification, discrimination, and the sociology of work.

Mechanisms Connecting Trump Support and Adverse Labor Market Outcomes among Blacks

The emboldening hypothesis hinges on the idea that locales where Trump won a greater share of the popular vote represent contexts in which whites were more receptive to Trump's racialized rhetoric and hence were more likely to be emboldened by Trump's victory to act on racial biases. This claim follows from a large body of research in political science and social psychology and is consistent with an emerging literature that has documented Trump's effects on racial attitudes via lab and survey experiments (Crandall et al. 2018; Flores 2018; Luttig et al. 2017). It is also further supported by our supplementary, survey-based analysis of explicit racial bias, which is reported in the online supplement.

Unfortunately, we cannot disentangle at the microlevel how much of the Trump effect on discrimination results from heightened biases or from weakened taboos against acting on preexisting biases. Furthermore, because we cannot observe hours allocations within specific firms, we cannot rule out the possibility that some portion of the effect is due to employees proactively reducing their time in the workplace (e.g., Glover et al. 2017) rather than biased allocative reductions on the part of managers. Future research might examine political spillover effects using administrative data on establishments, which could provide more direct evidence of disparate treatment within organizational contexts (Briscoe and Joshi 2017; Hirsch and Kornrich 2008).

Variation in the Impact of Trump's Messages across Racial Minority Groups

The current results show minimal evidence that Hispanics experienced heightened discrimination in work hours allocations following the 2016 election. Meanwhile, blacks, whom Trump invoked in a more oblique manner during the 2016 campaign, experienced disparate declines in work hours in high-Trump-support areas.

This pattern raises questions about the scope and limits of politically activated discrimination in an era of resurgent white ethnonationalism.

As discussed above, there are theoretical reasons why we should not be surprised by the disproportionately negative effect of Trump's victory on blacks relative to Hispanics. The fact that efforts to stir whites' racial animus toward out-groups redounded disproportionately against blacks is consistent with the expectations of racial priming theories (Tesler 2017), given the greater degree of social distance between whites and blacks in the United States.[19] It is also consistent with the results of other studies of event-driven discrimination. For example, Legewie (2016) examined the effects of incidents in which police officers were killed in the line of duty on police use of force. After the incidents involving black culprits, officers were more likely to use force during stop-and-frisks when stopping blacks. In contrast, there was no increase in the use of force against whites or Hispanics in the aftermath of the shootings involving white or Hispanic culprits.

On the other hand, it is important to acknowledge the possibility of a Type II error. It is conceivable that Trump's election did spur heightened work hours discrimination against Hispanics but that the current study failed to detect it. One methodological limitation of the CPS panels is that they do not follow movers. If Hispanics (especially immigrant Hispanics) are more prone to relocate in the face of diminished opportunities, the CPS panels may disproportionately undercount those who left after experiencing work hour reductions. Such a pattern would downwardly bias the estimated effect.

Duration and Timing

Our results suggest that the negative impact of Trump's election on hourly paid black workers persisted only four or five months after the election. One might have expected these trends to persist longer given Trump's continuing use of racially charged language *after* his inauguration. Although more research is needed on the short- and longer-term effects of Trump-era racial politics on U.S. society and social behavior, the current findings are consistent with an underlying mechanism whereby the collective endorsement of Trump's message in the form of his electoral victory activated a wave of event-driven discrimination as a sort of violent interaction ritual (Collins 2008).[20] That is, by enacting Trump's invocation to "take back the country" through everyday expressions or acts of racial domination, emboldened white supporters could collectively certify his victory and reassert their superordinate position in the racial order. Like most event-driven rituals, however, this too dissipated over time.

Another likely factor militating against continued work hours discrimination was the tightening labor market throughout Trump's early presidency. It is possible that the post-election effect on black workers' hours would have persisted longer if not for a growing worker shortage in 2017 (e.g., Beyer 2017). Organizational demands likely outweighed heightened biases as employers experienced increasing difficulty filling positions.

Implications for the Wider Sociological Literature

Racial priming in politics. We expand research on the effects of racial priming by considering whether it can "spill over" to affect broader socioeconomic behaviors and outcomes. Social scientists have long studied politicians' use of racialized messages (e.g., Gillion 2016; Mendelberg 2001; Tesler 2017). However, this research has been mostly confined to the effects of priming and boundary-making on policy attitudes and electoral behavior (Wetts and Willer 2018). The frequency, explicitness, and visibility of Trump's appeals to racial resentment are unprecedented among modern American national political figures (at least since George Wallace in 1968), raising the question of the degree to which his victory prompted wider shifts in social behaviors and acts of racial domination. Our findings suggest that it did, with economically significant consequences for black workers, at least in the short term.

Race relations and stratification. In recent decades, students of U.S. race relations have grappled with the paradox that, even as explicit expressions of racial prejudice and out-group hostility have waned in surveys, stark disparities between whites and blacks persist across most domains of social and economic life (Bonilla-Silva 2017; DiTomaso 2013; Harris and Lieberman 2013). This pattern has led scholars to focus on less overt mechanisms that perpetuate racial inequality. By showing that events such as Trump's election victory can heighten discrimination, the current results reveal that the role played by overt racism in stratification remains fluid and partly contingent on the prevailing political environment. Here our analysis aligns with other recent research that has linked mesolevel contextual measures of discrimination and prejudice to variations in well-being (Chae et al. 2018; Lee et al. 2015; Lucas 2013).

Studies of discrimination. We contribute to the analysis of discrimination (Lucas 2013; Reskin 2000) by treating it dynamically and considering how its intensity is linked to events in the broader political environment. Although the extant literature on workplace discrimination tends to treat biases as an unmeasured constant (e.g., Petersen and Saporta 2004), the current approach focuses on an exogenous event that is expected to heighten the impetus and social acceptability of discriminatory behaviors (Legewie 2016). In doing so, our analysis answers recent methodological calls to leverage natural experiments (Blank et al. 2004) and variations across social contexts (Lucas 2013). Of course, this approach is limited to cases where the research question concerns dynamic change rather than static levels, but this feature makes it particularly well suited to capturing spillover effects across domains. For example, some education scholars have suggested that the 2016 election similarly sparked increases in school bullying and harassment, with deleterious educational consequences for minority students (e.g., Rogers et al. 2017). Researchers could test this proposition using a design similar to the present one.

More generally, our results suggest that the relationship between racial biases and discriminatory behavior represents an area for renewed research focus. Sociologists have long sought to dispel the popular conflation of prejudice and discrimination (e.g., Blumer 1958; Bonilla-Silva 2017). However, the fact that racist attitudes have never been a necessary condition for systemic discrimination does not make them explanatorily irrelevant (Glover et al. 2017).

Sociology of work. Finally, this study contributes to the sociology of work by examining hours allocation as an understudied site of social stratification (e.g., LaBriola and Schneider 2020; Reynolds and Aletraris 2010; Schneider and Harknett 2019). In the face of anemic wage growth, Americans throughout the occupational structure have relied on increased work hours to maintain consumption needs. At the same time, involuntary underemployment has become more prevalent, a trend that has been amplified by the structural expansion of part-time jobs and the adoption of scheduling practices that render paid work hours increasingly variable and individually negotiated. This partial decoupling of formal employment and paid work time means that getting hired for a job is not necessarily a sufficient condition for generating labor income. Moreover, the fact that hours allocation represents a gap in the workplace rights revolution (Dobbin 2009) renders it one of the few margins over which managers can retain discretion in increasingly bureaucratized and algorithmically managed workplaces.

Students of stratification have only just begun to assess the consequences of these trends, all of which suggest that hours allocations represent an increasingly core feature of workplace inequality (Halpin 2015; Reynolds 2003; Schneider and Harknett 2019). Researchers should continue to examine how the results of these allocations shape broader patterns of socioeconomic stratification in the United States.

Notes

1 Two-party vote share refers to a candidate's share of all votes cast for a Democrat or Republican candidate.

2 For a vivid illustration of such blurring, see Glaude (2018).

3 Because CPS does not follow movers, the MORG data exhibit a relatively high one-year panel attrition rate of approximately 20 percent (Rivera Drew et al. 2014).

4 The 2016 election was held on Tuesday, November 8, during the middle of the reference week for the November CPS survey. In supplementary analysis we exclude the November wave from the follow-up period because that week was not strictly post-election (see the online supplement).

5 The *empsame* variable can be used to link adjacent observations during each respective rotation period (months 1 to 4 and months 5 to 8). Because the *empsame* item is not asked in rotation month 5, we proxy "same employer" across months 4 and 5 by requiring an exact match on detailed (three-digit) industry, in addition to the residential address match that is a function of the CPS design. We tested the reasonableness of this proxy assumption by using the observable four-month spans to calculate the proportion of hourly workers who remained in the same detailed industry and who also remained with the same employer (versus transitioning to a different employer in the same detailed industry). Among hourly CPS workers with full four-month spans of observation in the same industry during 2015 through 2017, 90 percent remained with the same employer.

6 The online supplement also reports specifications using a smaller but more straightforward sample that includes only the MORG observations and relies on only IPUMS-augmented CPS-MORG weights. Those results are very similar.

7 In ancillary analyses, we confine the outcome measure to hours worked at the respondent's "main job" and find very similar results (see the online supplement).

8 The racialization of Hispanics in the United States has been debated (Ortiz and Telles 2012). For the purposes of this article, we treat Hispanic identity as a racial group distinct from non-Hispanic whites and blacks (Massey 2009).

9 In addition to non-metropolitan and non-identified respondents, an additional 1.4 percent of cases (primarily in Alaska and Mississippi) are identified as being in a metropolitan area, but the specific metropolitan locator is suppressed. For this latter group we assign the state mean Trump share. Substantively identical results are obtained if these 1.4 percent of cases are excluded from the analysis altogether.

10 This assumption means that workers with equal baseline hours *and* industry and occupation positions would be expected to exhibit equal hours in wave 2 under the counterfactual condition that Trump had not won the election.

11 An alternative modeling strategy would substitute the lagged dependent variable in model 1 above with a person-fixed effect term and a triple interaction between Trump share, race, and period. This approach, the fixed effect triple difference-in-differences model (DDD), is closely analogous to the LDV insofar as the τ parameter targets the same treatment effect. However, identification using DDD rests on the counterfactual assumption of parallel trends among the groups formed by the interaction, which may be questionable given different economic contexts in high- and low-Trump-support areas. We focus on the LDV model because it permits additional industry and occupation fixed effects and it rests on less stringent assumptions (Gangl 2010). LDV is also more conservatively biased than the DDD when the identifying assumptions are violated (Angrist and Piscke 2009:5.3–5.4; Ding and Li 2019). The online supplement reports results using the DDD specification, which in practice yields similar results.

12 Despite the inclusion of CBSA fixed effects, models 2 through 4 include a coefficient estimate for *Trump vote share* because of the presence of variation in the imputed Trump vote share exposure for respondents within non-metropolitan "remainder CBSAs.'

13 The mean year-over-year work hours loss among hourly paid men in the 10 CPS-MORG panels from January 2008 to October 2009 was −1.8 hours for whites, −2.5 hours for blacks, and −2.7 hours for Hispanics (authors' calculation).

14 To further rule out the possibility of weather-related shocks to work hours, we acquired state-level data on precipitation and temperature abnormalities during the winter of 2017 from the National Oceanic and Atmospheric Agency (NOAA National Centers for Environmental Information 2020). The only state that experienced unusual levels of precipitation during the winter of 2017 was California. And the only region with abnormally cold weather was the Pacific Northwest. Neither of these patterns could plausibly account for the significant yet transient growth of black–white work disparities in high-Trump-support CBSAs during the five months following the election.

15 We calculated racial overrepresentation ratios for each race–occupation–CBSA cell using seven complete years of monthly CPS files. We classify groups with an overrepresentation ratio greater than 1.5 as occupying a niche.

16 Because the reference week for the CPS in November 2016 was the same week as the November 8 election, employee work schedules for this week could have been prenegotiated prior to the event itself. Moreover, some portion of reduced work hours during that week could be attributable to workers taking time off to vote.

17 We used bimonthly spans because small sample sizes result in imprecise month-specific estimates.

18 To the extent that Trump's campaign was already affecting the baseline measures in the spring of 2016, this would negatively bias the year-over-year difference.

19 Lack of an overall Hispanic effect could mask differences in the strength of the Trump effect between immigrants and the native-born. We found no evidence of this, although our sample is too small to test for such differences reliably.

20 We thank an anonymous reviewer for emphasizing this point.

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