

# Deciding to Discipline: Race, Choice, and Punishment at the Frontlines of Welfare Reform

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*Welfare sanctions are financial penalties applied to individuals who fail to comply with welfare program rules. Their widespread use reflects a turn toward disciplinary approaches to poverty management. In this article, we investigate how implicit racial biases and discrediting social markers interact to shape officials' decisions to impose sanctions. We present experimental evidence based on hypothetical vignettes that case managers are more likely to recommend sanctions for Latina and black clients—but not white clients—when discrediting markers are present. We triangulate these findings with analyses of state administrative data. Our results for Latinas are mixed, but we find consistent evidence that the probability of a sanction rises significantly when a discrediting marker (i.e., a prior sanction for noncompliance) is attached to a black rather than a white welfare client. Overall, our study clarifies how racial minorities, especially African Americans, are more likely to be punished for deviant behavior in the new world of disciplinary welfare provision.*

Disciplinary approaches to poverty management are ascendant in the United States today. They are perhaps most visible in the area of criminal justice, where tough new policies have driven incarceration rates to levels that are unprecedented in U.S. history and unrivaled by other nations (Western 2006). Yet mass incarceration is far from an isolated development (Starobin 1998). Welfare policies for the poor have been redesigned in recent years to reflect the idea that the state has a legitimate interest in ensuring that socially marginal groups prac-

tice appropriate behaviors (Schram 2006). Today, public aid programs are more directive in setting behavioral expectations, more supervisory in monitoring compliance, and more punitive in responding to infractions (Mead 1997). “New paternalist” welfare programs use a variety of incentives, surveillance mechanisms, and restrictive rules to modify client behaviors. A system for dispensing punishments when incentives and rules prove insufficient serves as a failsafe in this process. Sanctions—penalties that reduce or terminate benefits in

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response to client noncompliance—are the ultimate expression of welfare’s disciplinary turn and deserve the greatest credit for the exodus of clients from welfare since the mid-1990s (Rector and Youssef 1999).

This article investigates welfare administrators’ decisions to sanction clients, focusing on how the interplay of racial status and character markers shapes such decisions. Our analysis falls into a long tradition of sociological research on the ways that major societal institutions produce racial disparities. Using observational and audit designs, researchers in recent years have revealed stark disparities in the ways black and white Americans are treated in retail transactions (Ayres and Siegelman 1995), the mortgage loan industry (Munnell et al. 1996), the insurance industry (Wissoker, Zimmermann, and Galster 1998), the healthcare sector (Schulman et al. 1999), housing markets (Yinger 1995), and labor markets (Bertrand and Mullainathan 2003; Pager 2007). Despite the broad scope of this research, however, there are few systematic studies of how race matters when government agents use their distinctive authority to punish. The major exception, of course, is the literature exploring the origins and consequences of racial disparities in arrest, sentencing, and incarceration patterns (Pager 2007; Western 2006). It is unclear, however, how findings in the criminal justice domain might generalize to the welfare domain or to other policy areas.

We shift the analytic focus on punishment, in Bourdieu’s (1998) terms, from the “right hand of the state” to the “left hand of the state”—or more precisely, from criminal justice to social welfare provision. It is well established that racial attitudes influence public preferences regarding welfare policy (Gilens 1999) and that state-level welfare policy choices closely track the racial composition of welfare caseloads (Soss et al. 2001). But welfare case managers work directly with clients in concrete organizational settings; as a population, they are quite unlike the “average” citizen or state legislator. Their decisions to punish specific individuals and families are a far cry from expressions of general policy preferences or choices among state policy alternatives.

Although there is a significant ethnographic literature on race and welfare case managers (Bonds 2006; Watkins-Hayes 2009), its insights have had little impact on econometric studies of

sanctioning, where the actions of frontline workers are bracketed in favor of attention to client and community characteristics (Pavetti, Derr, and Hesketh 2003; Wu et al. 2006). Our study bridges these two streams by asking how client race influences caseworker decisions to apply penalties. We break new ground, first, by presenting rigorous empirical tests of racial bias in welfare sanction decisions and, second, by presenting an explicit cognitive model that explains how client race exerts an influence on officials’ decisions to punish.

To theorize the influence of race, we draw on the Racial Classification Model (RCM) of policy choice, which we developed and tested on state-level policy choices elsewhere (Soss, Fording, and Schram 2008). Drawing on models of implicit racism (Quillian 2008), the RCM specifies conditions that can produce policy-based racial disparities even in the absence of racial animus or discriminatory intent. In this article, we test hypotheses derived from the RCM’s assertion that racial disparities become more likely when policy targets possess discrediting traits consistent with negative group reputations. Extending the work of Pager (2007), we show how stereotype-consistent markers can provide “expectancy confirmation” (Darley and Gross 1983) and, hence, strengthen the link between racial status and policy treatment (Correll, Benard, and Paik 2007). As Duster (2008) notes, much of the evidence for implicit racism comes from artificial research settings; there is a pressing need for empirical tests in real-world settings and for models that explain how implicit cognitive biases translate into concrete policy outcomes.

To meet these goals, we test propositions from the RCM using both experimental and administrative data. Our experimental data come from a Web-based survey of Florida Welfare Transition (WT) case managers. Respondents were presented with realistic rule-violation vignettes in which key client characteristics were randomly assigned. Case managers were then asked whether they would impose a sanction in response. To our knowledge, no prior sanctioning study has employed this approach, which offers the advantages of causal inference associated with experiments (Kinder and Palfrey 1993) while remaining close to the phenomenon of study: decisions made by actual case managers.

Despite the advantages of this survey-experimental design, hypothetical vignettes and the limitations of our sample both counsel some humility regarding these data. Accordingly, we triangulate our findings with administrative data from the Florida WT program. While these data offer a weaker basis for causal inference, they have the benefit of reflecting decisions made on actual cases under normal working conditions.

## SANCTIONS AND WELFARE REFORM

The use of sanctions to enforce client compliance predates welfare reform. Prior to 1996, however, sanctions were used infrequently and were applied only to the benefits of the adult head of a household, not the entire family (Bloom and Winstead 2002). Sanctions became far more important under the Temporary Assistance for Needy Families (TANF) program because national welfare reform legislation in 1996 specified stricter work requirements, set narrower exemption criteria, made a broader scope of behaviors subject to sanction, and gave states more options in designing penalties (Hasenfeld, Ghose, and Larson 2004). Perhaps most important, state TANF programs now have to meet specific quotas for the percentage of recipients participating in work-related activities. Sanctions are the primary disciplinary tool available to administrators as they seek to motivate client behaviors to meet these quotas. Sanctions are most often applied when recipients fail to complete required hours of participation in countable work activities such as job search, job-readiness classes, vocational education, training, community work, and paid employment.

State sanction policies vary considerably (Bloom and Winstead 2002). To capture this variation, Pavetti and colleagues (2003) offer a simplifying typology. Seventeen states rely on the strictest combination of choices, enforcing an "immediate full family sanction." In these states, the entire TANF family is removed from the rolls at the first instance of noncompliance. Eighteen states use a "gradual full-family sanction," which potentially has the same effect, but only after continued noncompliance. The remaining states enforce a "partial sanction" of benefits (usually reducing only the adult portion of the grant).

There is extensive evidence that sanctions have played a key role in transforming welfare from a system focused on cash benefits to one focused on enforcing work (see Pavetti et al. 2004). Between 1997 and 1999, nearly 500,000 families lost benefits due to sanctions—approximately one quarter of the caseload reduction for that period (Goldberg and Schott 2000). Indeed, the states with the strictest sanctions experienced caseload declines as much as 25 percent greater than those reported by states with the least stringent sanctions (Rector and Youssef 1999). Some suggest that the threat of sanctions may be responsible for even greater numbers of recipients leaving the rolls (Lindhorst, Mancoske, and Kemp 2000). Moreover, studies suggest that being sanctioned significantly increases hardship among recipients (Reichman, Teitler, and Curtis 2005; Stahl 2008).

Together, these studies suggest the vital importance of understanding how sanction decisions actually get made at the frontlines of welfare reform. Direct responsibility for these decisions falls to individual caseworkers who must respond to individual cases. Such workers have long held discretion in dealing with their clients (Lipsky 1980; Maynard-Moody and Musheno 2003), but welfare reform has given them a variety of new powers and responsibilities (Watkins-Hayes 2009). To understand how race matters for punishment in the new world of welfare, one must theorize and investigate case-manager decision making.

## THE RACIAL CLASSIFICATION MODEL AND DECISIONS TO PUNISH

Our subject stands in the shadow of a long historical relationship between race and welfare provision in the United States (Lieberman 1998; Quadagno 1994). Many studies cast doubt on the idea that contemporary welfare reform represents a break with this troubled history (Neubeck and Cazenave 2001; Schram, Soss, and Fording 2003); race-coded appeals and racialized public responses played a key role in the national debates leading up to reform (Hancock 2004; Reese 2005). Under devolution, the racial composition of welfare caseloads has been a strong predictor of state choices regarding welfare rules and governance arrangements (Soss et al. 2001; Soss et al. 2008). Implementation studies suggest that client race

may affect caseworker decisions in sanctioning and other areas (Goldrick-Rab and Shaw 2005; Gooden 2003; Kalil, Seefeldt, and Wang 2002).

But how and why should client racial characteristics influence decisions to sanction welfare recipients? To answer this question, we draw on the RCM. The RCM offers an explanation for how race and ethnicity influence individuals' policy choices in an era in which *de jure* discrimination is outlawed and egalitarian norms are widely endorsed. Drawing on theories of implicit racism (Quillian 2008), the RCM asserts that racial disparities in policy domains can emerge from cognitive biases in decision making even in the absence of conscious racial animus, out-group threat, or in-group favoritism (cf. Blalock 1967; Key 1949).

An extension of the theory of target populations (Schneider and Ingram 1997), the RCM focuses on how social classifications and group reputations guide decisions about how to design and implement policy tools. The model consists of three basic propositions, which we explain in detail elsewhere (Soss et al. 2008). We briefly review them here to clarify the basis of our hypotheses.

1. To be effective in designing policies and applying policy tools to specific target groups, policy actors must rely on salient social classifications and group reputations; without such classifications, actors could not bring coherence to a complex social world or determine appropriate action.

Policy approaches that work for one group might fail for another. Thus, attempts to choose effective policy actions inevitably depend on beliefs about "what kind" of group or individual one is seeking to influence. The first premise of the RCM holds that policy actors rely on social categories to make complex target groups more interpretable and, when making policy choices, draw on social-group reputations as proxies for more detailed information about these targets.

2. When racial minorities are salient in a policy context, race will be more likely to provide a salient basis for social classification of targets and, hence, to signify target differences perceived as relevant to the accomplishment of policy goals.

The salience of race varies across policy domains, time periods, and political jurisdictions. All else being equal, we expect race to

become more salient in a policy context as racial minorities figure more prominently in policy-relevant political events, media discourses, and target-group images. Under such conditions, the social classifications that guide policy actions—whether aimed at groups or individuals—are more likely to be based on racial categories. Welfare policy offers a case in point (Gilens 1999; Schram et al. 2003).

3. The likelihood of racially patterned policy outcomes will be positively associated with the degree of policy-relevant contrast in policy actors' perceptions of racial groups. The degree of contrast, in turn, will be a function of (1) the prevailing cultural stereotypes of racial groups, (2) the extent to which policy actors hold relevant group stereotypes, and (3) the presence or absence of stereotype-consistent cues.

It is the contrast between group reputations that allows racial markers to underwrite broad assumptions about individual clients and target groups. The third premise of the RCM suggests that such contrasts depend on differences in racial groups' cultural images, differences in how policy actors internalize these images, and proximate cues that can evoke racial stereotypes and suggest whether a particular policy target fits the profile of a racial-group's reputation.

The RCM offers a clear basis for expectations regarding race and sanction decisions. We conceptualize sanctions as tools for motivating clients, stimulating work efforts, and enforcing responsible behavior. Accordingly, case managers should be more likely to apply sanctions when clients are perceived as less motivated and responsible—that is, when clients are perceived as needing a stronger push to follow rules and to achieve welfare-to-work goals. Client race should thus affect sanction decisions to the extent that group reputations suggest differences in motivation, work effort, and responsibility. Race-based sanction disparities should increase when the contrast between reputations is larger; disparities should be weaker and less consistent when the contrast is smaller. Finally, race-based disparities should be more likely to emerge when proximate cues link clients to group reputations in ways that highlight policy-relevant contrasts.

Like other students of implicit racism, we are particularly interested in the potential for discrediting markers to cue group reputations in ways that produce racial disparities (Quillian 2008). Two individuals seen as belonging to a

single racial group may nonetheless be associated with quite different group images. Research on intersectionality, for example, emphasizes that the meaning of any category of social identity will likely change when combined with another (e.g., when woman is combined with black as opposed to white [see Crenshaw 1991; Hancock 2007]). Likewise, social cognition research shows that perceivers tend to distinguish “subtypes” of racial groups based on additional characteristics (e.g., “ghetto blacks” versus “black businessmen”) and to attribute global group traits to these subtypes to very different degrees (Devine and Baker 1991; Richards and Hewstone 2001). As a result, race-of-target effects are often contingent on additional characteristics that strengthen or weaken an individual’s connection to a racial group’s prevailing reputation.

Eberhardt and colleagues (2006), for example, find that black defendants convicted of killing white victims are more likely to receive the death penalty if they are perceived as having a “stereotypically black appearance.” Likewise, Pager (2007) finds that black job applicants are significantly more disadvantaged than their white counterparts by having a felony conviction on their records. Conversely, in their study of black political candidates, Valentino, Hutchings, and White (2002:86) find that “when the black racial cues are stereotype-inconsistent, the relationship between racial attitudes and the vote disappears. . . . The effect emerges only when the pairing of the visuals with the narrative subtly reinforces negative stereotypes in the mind of the viewer.”

The RCM predicts that when minority clients possess discrediting traits consistent with minority stereotypes, they will be sanctioned significantly more often than (1) clients from all racial groups who lack the discrediting trait and (2) white clients who share the discrediting trait. The RCM also predicts that this effect will be greater for blacks than for Latinos because group stereotypes regarding work effort and personal responsibility are more negative in the case of blacks (Fox 2004; Gilens 1999). To test these hypotheses, we make use of two client traits: one evokes the image of single mothers having children while engaging in long-term welfare dependency (see Hancock 2004); the other evokes images of a preference for living off welfare rather than pursuing the hard work of paid employment (see Gilens 1999).

## SURVEY EXPERIMENTAL DESIGN

Our experimental data come from a Web-based survey of Florida Welfare Transition (WT) case managers with sanctioning authority. The Florida WT program relies extensively on sanctions to enforce client compliance, producing one of the highest sanction rates for any state in the nation (Botsko, Synder, and Leos-Urbel 2001). Indeed, a recent analysis of sanctioning in Florida finds that the sanction rate for an entering cohort averages nearly 50 percent after 18 months (Fording, Soss, and Schram 2007). Florida is also one of the most racially diverse states in the country, with sizeable black and Hispanic populations, and the state’s TANF population displays even more diversity. Between January 2000 and March 2004, 36.2 percent of TANF adults were black, 33.7 percent were white (non-Hispanic), and 28.5 percent were Hispanic. Given its reliance on sanctions and its diverse client population, Florida is an ideal state for examining the role of race in the sanctioning process.<sup>1</sup>

To ensure anonymity for respondents, the Agency for Workforce Innovation (AWI) distributed the survey link through e-mail to its 24 Regional Workforce Boards (RWBs) for subsequent distribution to caseworkers via e-mail.<sup>2</sup> Case managers completed the surveys during a two-week period at the end of 2006.<sup>3</sup> Prioritizing

<sup>1</sup> All states must establish formal procedures to ensure fairness when sanctioning clients, and Florida’s process is quite similar to that of other states. For a detailed description of Florida’s sanction policy, see Appendix; for background on Florida’s sanctioning practices, see Fording and colleagues (2007).

<sup>2</sup> AWI requested that the survey be distributed via an official electronic memorandum to RWBs. To meet this request, we used a Web-based survey rather than a mailed, paper survey. RWBs were asked to forward the survey request to their contract agencies so they could encourage case managers to participate. This procedure was designed to motivate participation without case managers feeling any direct pressure from the state government. The authors provided no additional incentives to promote participation in the survey.

<sup>3</sup> All caseworkers shared a survey password to ensure anonymous completion and return. They were sent two follow-up requests for their participation, which extended the time period. We garnered no additional responses after the last follow-up request.

**Table 1.** Distribution of Responses by Region

Region Name (Number)	Frequency	Percent
Workforce Central Florida (12)	27	26.0
First Coast Workforce Development, Inc. (8)	22	21.2
Hillsborough County Workforce Board (15)	16	15.4
Pinellas Workforce Development Board (14)	11	10.6
Pasco-Hernando Jobs & Education Partnership Regional Board, Inc. (16)	6	5.8
Brevard Workforce Development Board, Inc. (13)	5	4.8
Southwest Florida Workforce Development Board (24)	5	4.8
Big Bend Jobs and Education Council, Inc. (5)	3	2.9
Citrus Levy Marion Workforce Development Board (10)	2	1.9
Polk County Workforce Development Board, Inc. (17)	2	1.9
North Florida Workforce Development Board (6)	2	1.9
Gulf Coast Workforce Development Board (4)	1	.96
Workforce Development Board of the Treasure Coast (20)	1	.96
Palm Beach County Workforce Development Board (21)	1	.96
Total	104	100.0

respondent anonymity limits our ability to determine whether all WT program officers passed along the survey as requested, as well as the extent to which various regions participated in the survey. We do, however, have regional response data for the subset of case managers who elected to identify themselves by region, as presented in Table 1. This table suggests that responses were spread across a number of regions but clustered in several of the state's larger regions.

We received survey responses from 144 TANF case managers, the vast majority of whom responded to our vignettes.<sup>4</sup> Although Florida officials were unable to provide a precise number for the overall population of WT case managers, estimates ran from 200 to 250. This suggests a rough estimate of between 58 and 72 percent for our response rate. Certain items in the questionnaire, however, including race ( $N = 98$ ), political party affiliation ( $N = 103$ ), and recent sanctioning behavior ( $N = 108$ ), yielded a larger number of nonresponses than most. Table 2 provides a demographic profile of our survey respondents. Although response patterns suggest that some caution should be used in generalizing from this sample, the respondents are quite diverse and offer

a sample well suited to the needs of our experimental design.<sup>5</sup> Moreover, by triangulating our experimental results with an analysis of statewide administrative data, we offer an independent safeguard against any biases arising from nonparticipation in the survey.

Our analysis is based on two  $2 \times 2$  experiments embedded in the survey, each of which presented case managers with a vignette and asked them to decide whether to impose a sanction. The  $2 \times 2$  design includes variation on race and a discrediting social marker. Each vignette portrays a hypothetical TANF participant who has arguably fallen out of compliance with program requirements. (For a description of relevant rules and procedures in the WT program, see Appendix.) For the racial dimension of the  $2 \times 2$  design, each vignette makes use of a procedure developed by Bertrand and Mullainathan (2003), who show that by randomly assigning "black-sounding" names and "white-sounding" names to a set of identical resumes, they can significantly influence the rate at which employers contact a fictitious job-seeker. Adapting this procedure, we randomly assigned the client

<sup>4</sup> A total of 137 caseworkers responded to Vignette 1 and 131 to Vignette 2.

<sup>5</sup> Tests on the full sample, including respondents who did not indicate their race and other personal characteristics, provided baseline responses for our experimental results that are consistent with our multivariate models.

**Table 2.** Respondent Characteristics

Respondent Characteristics	Percent of Sample
Sex (N = 114)	
Male	21.1
Female	78.9
Race/Ethnicity (N = 98)	
African American / Black	34.7
European American / White	44.9
Hispanic American / Latino	18.4
Other	2.0
Educational Level (N = 115)	
High school diploma	7.8
Some college or trade school	33.0
4-year college degree	37.4
Some graduate school	13.0
Graduate degree	8.7
Marital Status (N = 115)	
Married	57.4
Divorced/separated/widowed	20.9
Single, never married	15.7
Unmarried couple living together	6.1
Political Party Affiliation (N = 103)	
Democrat or Independent Democrat	60.2
Independent	11.7
Republican or Independent Republican	24.3
Other	3.9
Religious Attendance (N = 113)	
Weekly	33.6
At least once a month	23.9
A few times a year	31.0
Never	11.5
Mean years of welfare services experience (N = 143)	7.0

Note: N = number of caseworkers who responded to survey item.

described in Vignette 1 either a Hispanic-sounding name or a white-sounding name. Similarly, we assigned the client described in Vignette 2 either a black-sounding name or a white-sounding name.<sup>6</sup>

The second dimension of the  $2 \times 2$  experiments manipulates client markers that are commonly associated with (1) images of undeserving welfare clients and (2) negative stereotypes of minority racial/ethnic groups.

<sup>6</sup> To guard against confounding effects that might arise from the use of a specific name, we randomly assigned one of three names for each group in each vignette. White-sounding names include Sarah Walsh, Emily O'Brien, and Meredith McCarthy; African American-sounding names include Lakisha Williams, Aisha Jackson, and Tamika Jones; and Hispanic-

We based our selection of these client traits on our field interviews, which revealed substantial caseworker attention to these two client "types": the young mother of multiple children and the repeat recipient who has been sanctioned in the past. We present the vignettes below with our experimental manipulations bracketed.<sup>7</sup>

sounding names include Sonya Perez, Maria Rodriguez, and Luisa Alvarez. To test for name-specific effects, we analyzed responses within each "race condition" to search for significant differences associated with each name. We found no significant differences and thus treat all racial name cues as equivalent.

<sup>7</sup> To minimize the amount of bias potentially introduced by subsequent questions on sanctioning practices, the two vignettes were presented near the beginning of the survey. We tested for order effects,

**VIGNETTE 1**

[Emily O'Brien/Sonya Perez] is a 28-year-old single mother with [one child age 7 / four children ages 7 to 11, and who is currently in her fourth month of pregnancy]. She entered the Welfare Transition program six months ago, after leaving her job as a cashier at a neighborhood grocery store where she had worked for nine months. Emily was recently reported for being absent for a week from her assignment for community service work experience. Immediately after hearing that Emily had not shown up for a week of work, Emily's caseworker mailed a Notice of Failure to Participate (Form 2290) and phoned her to ask why she had missed her assignment. Emily was not home when the caseworker called. However, when she responded to the 2290 three days later, she said she no longer trusted the person who was looking after her [child/children], and she did not want to go back to work until she found a new childcare provider. Emily returned to work the next day.

**VIGNETTE 2**

[Emily O'Brien/Lakisha Williams] is a 26-year-old single mother with two children. She has been in the Welfare Transition program for five months. Lakisha was recently reported for failing to show up for a job interview that had been scheduled for her with a local housecleaning service. Immediately after hearing about the missed interview, Lakisha's caseworker mailed a Notice of Failure to Participate (2290) and phoned her to ask why she had not shown up. Lakisha said she had skipped the interview because she had heard that a better position might open up next month with a home health agency. [She had been sanctioned two months earlier for failure to complete her hours for digital divide.]

The two experiments offer a more likely and a less likely case for finding effects on sanction decision-making.<sup>8</sup> Vignette 1 iden-

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specifically, whether sanctioning decisions in Vignette 1 predict sanctioning decisions in Vignette 2, and find no evidence of such effects.

<sup>8</sup> We developed the questionnaire, including the vignettes, after extensive study of the sanctions process in Florida, including participation on multi-

tifies the client as either white or Hispanic, a contrast associated with smaller stereotype differences than the black-white client contrast used in Vignette 2. The client traits used in the two vignettes reinforce this difference. Vignette 1 focuses on childcare instability, which is a "normal" rather than "deviant" problem for women moving into employment (Loprest 2002). We expect the attribution of numerous children here to cue negative stereotypes related to sexuality and reproduction, but this feature of the vignette also indicates that a comparatively sympathetic group (i.e., children) may suffer hardship as a result of the sanction. As a result, one might expect this cue to produce ambivalence about sanctioning the family. The marker in Vignette 2 is far less equivocal. By stating that the client has previously drawn a sanction, we simultaneously provide case managers with two pieces of information that might cue perceptions of welfare dependency and resistance to achieving self-sufficiency: the client is a repeat recipient who has returned to the program and has a record of at least one previous failure to comply with welfare-to-work rules.<sup>9</sup>

Finally, the two narratives differ in relation to sanction procedures. In Vignette 1, the client was reported for a week's absence from her work experience assignment. Although she was not home when the caseworker called, she responded to the mailed 2290 form well within the 10-day period allotted and reestablished compliance by returning to work the next day. According to both the sanctioning rules and the WT Sanction

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ple occasions in sanctions training for case managers. We developed the vignettes in consultation with AWI staff responsible for this training. The survey/experiment was pilot tested with other AWI staff who had prior experience as frontline case managers.

<sup>9</sup> Under Florida's WT program, a record of a sanction two months prior to the current failure to comply should not itself increase a client's jeopardy of receiving a sanction for the present offense (see Appendix). The client in the second vignette, however, has a prior sanction and therefore faces a lengthier penalty (one month) for noncompliance than would a client without a prior sanction (10 days). It is possible that a case manager might be less willing to impose a sanction knowing that a client faces a longer penalty and therefore a greater amount of time without cash assistance.

Flow Chart (see Appendix), the pre-penalty phase for this client should end with compliance following her return to work. In other words, based solely on the vignette, one cannot say that a sanction is the appropriate response. By contrast, although the client in Vignette 2 responded to the case manager's phone call, her reason for not complying with her welfare contract clearly fails to meet requirements for a "good cause" exemption.<sup>10</sup>

The two vignettes present us with an opportunity to test the RCM in strikingly different cases. Each vignette was followed by a question asking respondents to indicate on a four-point scale whether they strongly favor, somewhat favor, somewhat oppose, or strongly oppose requesting a sanction for the client and situation described.<sup>11</sup>

### SURVEY EXPERIMENTAL ANALYSIS: RESULTS

As anticipated, fewer caseworkers recommended sanctioning the client in Vignette 1 (34 percent) than in Vignette 2 (79 percent). At the same time, these overall results strongly confirm our expectation of variation in caseworker judgment: roughly a third of caseworkers decided that a sanction was warranted in Vignette 1, despite the fact that WT program rules seem to suggest otherwise, and 21 percent opted not to sanction the client in Vignette 2, despite a clear violation of the rules.

Our baseline results indicate differences in how different clients are treated. The Hispanic-named clients were more likely to be recommended for sanctioning than were the white-named clients in Vignette 1 (40 versus 28.6 percent,  $p < .10$ ); in Vignette 2, the

black-named clients were slightly more likely than the white-named clients to be recommended for sanction (82.3 versus 76.8 percent,  $p = .22$ ). Consistent with our hypothesis, the racial disparities widen with the presence of a discrediting marker in each experiment. For Vignette 1, the Hispanic-named clients who are pregnant and have four children were likely to be sanctioned 40 percent of the time, while the non-Hispanic white-named clients were slightly less likely to be sanctioned at 27.3 percent of the time ( $p = .11$ ). For Vignette 2, the black-named clients with a prior sanction were likely to be sanctioned 93.9 percent of the time, compared with 77.4 percent of the time for the previously sanctioned white-named clients ( $p < .05$ ). These simple descriptive results, however, fail to take account of possible differences between managers assigned to each condition of the experiment. To do so, we include relevant covariates in a larger multivariate analysis.

Our multivariate models include dichotomous variables representing three of the four experimental conditions (the white client with a "more deserving" trait serves as the baseline). The models also include measures of selected case manager characteristics that, based on literature on welfare casework, we have reason to believe affect a willingness to impose sanctions: work experience, religiosity, education level, partisan identification, marital status, and racial identity (Dias and Maynard-Moody 2007; Gooden 2003; Watkins-Hayes 2009). We hypothesize that case managers with greater experience in the social welfare field are likely to have either worked under the earlier, more permissive system of welfare or to have witnessed the negative effects of sanctioning; they are therefore less likely to impose sanctions. Case managers with more experience may also be more committed to supporting clients and thus less likely to support sanctioning. Highly religious case managers are likely to have greater commitment to enforcing basic work and family values and thus are possibly more likely to impose sanctions. More educated case managers might be less likely to impose sanctions because they are apt to be more informed about their effects. Democrats are probably less likely to adhere to the new wel-

<sup>10</sup> Although failure to provide evidence of the childcare problem, or the children being too old under Florida's childcare exemption policy, may preclude a "good cause" exemption, Vignette 1 introduces the idea that "good cause" might apply here because the mother provided some evidence of her childcare problem and came back into compliance within the time allowed.

<sup>11</sup> In Florida, sanctions are all of one type—financial penalties for failure to comply with program rules. Penalties increase with each infraction (see Appendix).

**Table 3.** Analysis of Vignette Experiments with Caseworker Characteristics as Covariates

Independent Variables	Vignette 1	Vignette 2
Vignette Condition		
White client, marked	.501 (.60)	.367 (.51)
Minority client, unmarked	1.383† (1.50)	-.075 (-.11)
Minority client, marked	1.693* (1.96)	2.599** (2.28)
Caseworker Characteristics		
Experience	-1.573** (-2.73)	-1.632* (-1.98)
Religiosity	-.314 (-.56)	.018 (.03)
College education	.134 (.24)	.424 (.73)
Democrat	.567 (.99)	-.127 (-.22)
Married	.903 (1.48)	-.152 (-.25)
Black or Hispanic	.301 (.48)	.405 (.63)
LR $\chi^2$	16.90*	16.90*
Log likelihood	-46.304	-41.482
Pseudo R <sup>2</sup>	.154	.169
N	95	94

*Note:* Entries are coefficients followed by z-scores in parentheses. For Vignette 1, the racial minority is Hispanic and the marked condition is “four children and pregnant,” as opposed to one child. For Vignette 2, the racial minority is black and the marked condition is possession of a prior WT sanction, as opposed to no mention of a prior participation spell. The number of cases for each model is lower than the overall sample due to missing data for selected covariates.

†  $p \leq .10$ ; \*  $p \leq .05$ ; \*\*  $p \leq .01$  (one-tailed tests).

fare regime’s use of sanctions to enforce client compliance. Married case managers might be more likely to support sanctions, perhaps as an expression of a commitment to upholding traditional values. We include racial identity based on the hypothesis that white case managers might be more willing to sanction nonwhites. Furthermore, nonwhite case managers might feel they lack the “race privilege” to sanction whites, while also perceiving special obligations to sanction black clients out of grounds of loyalty to the race or a desire to teach clients of the same race the need to be compliant with established norms.

Table 3 presents the results of logit models for both vignettes. Only one of the covariates emerges as statistically significant, and it does so consistently across experiments. Case managers with more experience (defined as those with more than two years of welfare services experience) emerge here as less likely to impose

sanctions. No other covariate achieves statistical significance, and we obtained no significant results in further specification tests of caseworker characteristics not included here.

Caseworkers with more than two years experience are significantly less likely than their less experienced counterparts to choose a sanction in both Vignette 1 ( $p < .01$ ) and Vignette 2 ( $p < .05$ ). Several factors may explain why more experienced case managers are less likely to impose a sanction. For example, case managers with more experience are more likely to have been trained in an earlier era of welfare provision that placed less emphasis on sanctioning (Aid to Families with Dependent Children prior to 1996, or Florida’s initial reform program Work and Gain Economic Self-Sufficiency [WAGES] 1996 to 2000). Alternatively, if case managers learn over time that sanctions have negative consequences for clients or for performance ratings, then more experienced case

managers might become more reluctant to impose sanctions. Another possibility is that experienced case managers develop greater knowledge of how to work with clients and, hence, resort to sanctions less quickly than do novice case managers. Our data do not allow us to arbitrate among these alternatives.<sup>12</sup>

The key findings in Table 3 are indicated by the coefficients for our experimental conditions. The results are striking for their consistency, especially given the considerable differences between our two vignettes. In the first, we find that case managers are no more likely to sanction the white client with multiple children than to sanction the white client with one child. We find borderline results suggesting that case managers may be more likely to sanction the Hispanic client with one child than to sanction the white client with one child (one-tailed test,  $p = .067$ ). By contrast, we find that a Hispanic woman with multiple children is significantly more likely to be sanctioned compared with a white woman with one child.

The results for Vignette 2 tell a similar story, only in stronger form. Here, we find no discernible differences in the odds that a case manager will sanction a white woman with no prior sanction, a black woman with no prior sanction, or a white woman with a prior sanction. Sanction decisions are, from a statistical perspective, invariant across these conditions. When a prior sanction is attributed to a black woman, however, we find a large and statistically significant increase in the possibility of being sanctioned, compared with a white woman without a prior sanction.<sup>13</sup> Indeed, under every other combination of race and sanction history in our experiment, the probability of a new sanction being

applied is significantly lower than what we observe when the client is identified as a black woman with a prior sanction on her record.

To put these results into perspective, we can calculate the predicted probability of being sanctioned for each of the four client conditions we examine in each vignette by holding all the other variables in the model at their median and allowing the variable in question to vary. Figure 1 provides the predicted probabilities for each of the four types of clients for both vignettes. Based on our model for the first vignette, we find that a Latina mother who is pregnant and already has four children has a .47 predicted probability of being sanctioned, while a white mother in the same condition has only a .21 predicted probability of being sanctioned, and a white mother with only one child has only a .14 probability of being sanctioned. For Vignette 2, a black client with a prior sanction has a .97 predicted probability of being sanctioned, compared with .75 for a white client with a prior sanction and .67 for a white client without a prior sanction.

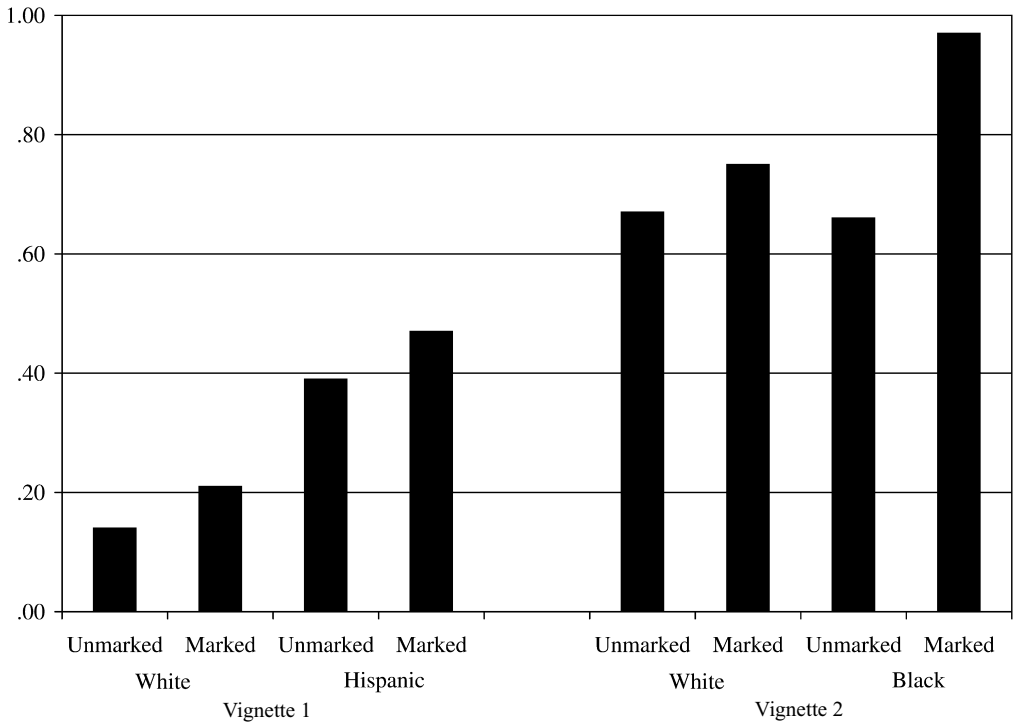
The consistency of findings across two random-assignment experiments, each with very different conditions, is noteworthy. The key limitation of the evidence, however, is that these findings, like most studies of implicit racism, are based on hypothetical scenarios (Duster 2008). When case managers responded to these vignettes, they were not confronted with a real person: they did not have a detailed case file in hand, they did not have to worry about effects on their performance numbers, and they did not have to contemplate real material hardships that might result. To bring our empirical analysis into line with real-world conditions, we must turn to administrative data generated by the Florida WT program itself. In doing so, we lose certainty about whether clients with different characteristics have equivalent cases and must rely on an imperfect process of specifying control variables. In return, however, we gain the ability to triangulate our survey-experimental findings with data that bear a closer relationship to the real world of administrative practice.

## ADMINISTRATIVE DATA: EVENT HISTORY ANALYSIS

The Florida Department of Children and Families (DCF) provided the data for these analyses, which consist of individual-level records for all adults

<sup>12</sup> In alternate specifications of our models, we find no significant interactions between case manager experience and client race. The effects of case manager experience should thus be seen as independent of the racial patterns presented below.

<sup>13</sup> We tested our models including interactions that combine the race of the caseworker with the race of the client to see if the results could be better explained by intergroup hostility between caseworkers and clients, rather than the logic of the RCM. The interaction terms proved insignificant, suggesting the results are not reflective of intergroup differences between the caseworkers and the clients and that the RCM offers a better explanation.



**Figure 1.** Predicted Probabilities by Race and Condition

who received TANF in Florida between January 2000 and April 2004. Our data set consists of monthly records for reporting case outcomes and the characteristics of TANF clients and their families. Our analytic strategy is to replicate the experimental vignettes as closely as possible by estimating an event history model of sanction initiation, focusing on the effect of a client's race or ethnicity and its interaction with the stigmatizing marker used in each experimental vignette. We begin by examining the joint effects of ethnic identity and family size tested in Vignette 1. We then turn to the joint effects of racial status and sanction history tested in Vignette 2.

#### **TRIANGULATING VIGNETTE 1: ETHNICITY AND FAMILY SIZE**

For our first analysis, we construct a sample of all new adult clients who entered TANF during the 24-month period from January 2001 through December 2002.<sup>14</sup> In an effort to match Vignette

1 as closely as possible, we then restrict our sample to unmarried, female clients who are either white (non-Hispanic) or Hispanic and who have either one or four children. The dependent variable for our analysis is a dichotomous variable that takes on a value of 1 in the month that a client is sanctioned. We follow each of the 24 cohorts in our sample for up to a maximum of 12 consecutive months, ending our observation at the spell's termination or the 12-month mark, whichever comes first. Clients who exit for reasons other than a sanction, or who are not sanctioned by the 12th month of the spell, are treated as right-censored. For our first analysis, we restrict our attention to the first TANF spell for each individual during this period; we define a spell as continuous months of

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who have spent at least 12 continuous months without TANF benefits. This precludes the inclusion of clients entering TANF during 2000. In addition, we wish to observe each client cohort for up to 12 months after entering TANF. This forces us to exclude clients who entered TANF during the last year of our observation period.

<sup>14</sup> Our selection of cohorts is limited by two factors. First, we define "new" TANF clients as those

TANF receipt.<sup>15</sup> As defined, and accounting for a small percentage of cases for which values of some variables are missing, our total sample size exceeds 6,000 women and nearly 20,000 person-month observations.

We estimate our model using a Cox proportional hazards approach. The advantage of the Cox model is that it allows for flexible, non-parametric estimation of the baseline hazard, or what we might think of as the effect of spell duration on the probability of sanction (Box-Steffensmeier and Jones 2004).<sup>16</sup> Our primary interest lies in the effect of a client's ethnicity and changes in this effect as we move from clients with only one child to clients with four children. To test this interactive hypothesis, we divide our sample into four groups that parallel the experimental groups in Vignette 1.

Based on past research on sanctions and welfare implementation (Hasenfeld et al. 2004; Kalil et al. 2002; Wu et al. 2006), we include a number of independent variables to control for variation in clients' individual and community characteristics.<sup>17</sup> At the individual level, we include variables measuring a client's citizenship status, her age, the age of the youngest child in the TANF family, and two indicators of human capital (income and education). We also control for a variety of community conditions, including local conservatism,<sup>18</sup> percent black, and

percent Hispanic,<sup>19</sup> and several measures of employment conditions: the county unemployment rate, the county poverty rate, the level of urbanization as measured by county population, and the annual local wage in food service/drinking establishments.<sup>20</sup> Finally, we include a measure of the county TANF caseload, expressed as a proportion of the county population.<sup>21</sup>

Table 4 shows the results for our event history model. Column I presents the results for a model that estimates the additive effect of ethnicity through the inclusion of a single dichotomous variable (Hispanic) measuring whether a client is white or Hispanic. Column II presents results for a model that tests for an interactive effect between ethnicity and the number of children using the indicator variables described above. For each variable in

<sup>15</sup> Based on findings from our field research, we do not include the first two months of the first TANF spell in our analysis. In interviews at all levels of the WT program, officials report that sanctions recorded in the first months of a spell often represent a form of "self-sanctioning" that is distinguishable from "true sanctioning" decisions made by case managers. In this scenario, an applicant with some alternative income options enters the official rolls, begins to receive assistance, but then does not return to the local provider after learning what will be required of her and how much cash aid she will receive in return.

<sup>16</sup> We have replicated our findings using other estimation methods as well, including parametric methods (Weibull) and a discrete-time (logit) model.

<sup>17</sup> See Table A1 in the appendix for variable definitions and sources.

<sup>18</sup> Several studies find that local policy implementation is influenced by the local political environment (Cho et al. 2005; Weissert 2000). Based on these studies, we include a measure of local political ideology, which we expect will be positively related to sanction initiation.

<sup>19</sup> Previous studies often find that racial context has a significant effect on racially relevant policy outcomes. The "threat" hypothesis (Blalock 1967; Key 1949) suggests that the higher the percentage of nonwhites, the greater the support for more punitive policies. The "contact" hypothesis (Alport 1954; Fox 2004) indicates that the higher the percentage of nonwhites, the lower the support for punitive policies. There are also possible effects from an increased proportion of the population nonwhite enhancing minority political power (Keech [1968] 1981). Because there is reason to suspect that any of these effects might exist (see Keiser, Mueser, and Choi 2004), we test for effects of community racial composition by including the percentage of the county population that is Hispanic and black, respectively.

<sup>20</sup> Where employment opportunities are relatively numerous and attractive, TANF clients may be more likely to work enough hours to avoid falling out of compliance with TANF rules. Alternatively, local labor market conditions may influence the sanction decisions of case managers, who may be less inclined to sanction clients when job opportunities are less numerous or less attractive.

<sup>21</sup> As the caseload size increases, administrative pressures to reduce caseloads should result in an increase in sanctioning. Additionally, the heightened burden of more cases may increase the chances that caseworkers will rely on race-relevant heuristics. Alternatively, as caseload size increases, if the number of case managers remains fixed, individual case managers may have less time to closely monitor TANF clients for violations of rules, thus resulting in a lower rate of sanctioning.

**Table 4.** Cox Proportional Hazard Models of Effects of Minority Status and Number of Children on Sanction Initiation

Independent Variables	I Additive Model	II Interactive Model
<b>Individual Characteristics</b>		
Hispanic client	.960	
White client and one child		[Reference]
White client and four children		.981
Hispanic client and one child		.955
Hispanic client and four children		.982
Age of client	.980**	.980**
Age of youngest child	1.013	1.013
Citizenship status (1 = citizen)	1.276	1.274
Education (reference = > high school)		
Less than high school education	1.371**	1.370**
High school education	1.068	1.068
Income (in 1000s)	1.067**	1.067**
<b>Community Characteristics</b>		
Local conservatism	1.085	1.085
Percent black	.997	.997
Percent Hispanic	.987**	.987**
Annual wage – food service/drinking places	.976	.976
Unemployment rate $t-1$	1.029	1.029
Poverty rate	1.017	1.017
Population (in millions)	1.281**	1.281**
TANF caseload $t-1$	.911	.911
Number of subjects	6,214	6,214
Number of failures	1,792	1,792
Time at risk (person-months)	19,798	19,798

*Note:* The sample for this analysis consists of all new TANF clients (single-parent, female, Hispanic or white) who entered TANF from January 2001 through December 2002. All clients are observed for a maximum of 12 months (clients who exit without being sanctioned, or who were sanctioned after 12 months, are treated as censored). Cell entries are hazard ratios, with *p*-values based on robust standard errors (adjusted for error clustering at the county level).

\*  $p < .05$ ; \*\*  $p < .01$  (two-tailed tests).

our models, we report the estimated hazard ratio, which reflects the proportional change in the risk of sanction given a one-unit increase in the independent variable of interest. Most of the control variables in the model perform as expected. The risk of being sanctioned is lower among older and more educated clients and among those who reside in a community with a large Hispanic population. Clients are more likely to be sanctioned if they have older children or reside in heavily populated counties. These results are largely consistent with the findings of past studies of sanctioning (Born, Caudill, and Cordero 1999; Fording et al. 2007; Hasenfeld et al. 2004; Kalil et al. 2002; Keiser et al. 2004;

Koraleck 2000; Mancuso and Lindler 2001; Wu et al. 2006).<sup>22</sup>

Moving to our main hypotheses of interest, we find no evidence in the administrative data to suggest that Hispanic clients are sanctioned at a greater rate than white clients. This is true regardless of whether we examine the additive effect of ethnicity displayed in column I or its conditional effect in column II. Our first analysis of administrative data thus proves to be

<sup>22</sup> We also tested for whether black clients with four children were more likely to be sanctioned than whites with one child and the results are not significant.

inconsistent with the findings for Vignette 1 from our survey of case managers.

### **TRIANGULATING VIGNETTE 2: RACE AND PRIOR SANCTIONS**

We now move to an analysis of administrative data based on Vignette 2, which examined the joint effects of client race (black/white) and sanction history. Once again, we limit our sample to clients with the characteristics described in the vignette—unmarried female clients who are either black or white (non-Hispanic). We also rely on the same general research design by estimating an event history model of sanction initiation. The narrative in Vignette 2, however, presents us with two additional complications for an analysis of administrative data. First, as the stigmatizing condition in Vignette 2 is a prior sanction, we must go beyond the first spell to examine sanctioning outcomes for clients with a prior history of TANF participation. We therefore rely on a sample consisting solely of TANF clients participating in their second spell.<sup>23</sup> Specifically, the sample consists of all TANF clients (unmarried, female, black or white) who entered TANF for the first time between January 2001 and December 2002, and who returned to TANF for a second spell during this same period. As defined, and accounting for some missing data, our sample consists of approximately 19,000 second spell clients, approximately 37 percent of whom experienced a sanction during their second spell.

A second complicating factor arises due to the possibility of sample selection bias. That is, the factors that cause TANF recipients to return to the program for a second spell (and thus enter our sample) may also be related to the outcome

we are trying to explain (sanctioning). Because this nonrandom selection may introduce statistical bias, we must control for the selection process that brings some (but not all) first-spell clients into a second spell. To do so, we rely on an estimator introduced by Boehmke, Morey, and Shannon (2006) for continuous-time event-history models with sample selection. The procedure relies on full information maximum likelihood to simultaneously model the selection and event history processes. Like the Heckman model for continuous outcomes, we first estimate a binary model of the selection process—a client's return to TANF after having exited a first spell.<sup>24</sup> We then use the information garnered from the selection model to correct for selection bias in the event history model. Because the estimation procedure is limited to parametric event history models, we use the Weibull distribution to model duration dependence (i.e., the effect of time on sanction initiation).<sup>25</sup>

Once again, we divide our sample into groups analogous to the four experimental groups featured in Vignette 2. We include indicators for three of the groups in the model, excluding white and no prior sanction as the reference group. Because our theory suggests that whites with no prior sanction should be subject to the lowest rate of sanction, we expect that the coefficients representing each of the three indicator variables included in the model will be positive (and the associated hazard ratios > 1.0). In addition, we expect that the variable representing blacks with a prior sanction (black and prior sanction) will display the largest coefficient. Finally, we also include several individual and community-level controls: age of client, age of youngest child, education, income, local conservatism, percent black, annual wage, poverty rate, and TANF caseload.<sup>26</sup>

<sup>23</sup> While our data allow us to examine sanctioning outcomes beyond the second spell, we limit our analysis to the second spell for at least two reasons. First, because our observation period is fixed, each successive spell necessarily increases the number of right-censored observations. Second, the receipt of a third sanction can result in a TANF client being barred from TANF participation for three months, even if the client comes into compliance with TANF rules. We suspect that this may alter case managers' decision-making processes in ways that render the sanction decision less comparable to the decision prompted by Vignette 2.

<sup>24</sup> We do not present this model in the text, but see Table A2 in the Appendix.

<sup>25</sup> The Weibull distribution allows for a flexible specification of the baseline hazard rate and is appropriate when the baseline hazard rate is monotonically increasing, monotonically decreasing, or flat over time. Based on various diagnostics, we are satisfied that the Weibull model is appropriate for our data.

<sup>26</sup> We do not include *citizenship status*, *number of children*, *percent Hispanic*, or *population*, because these variables proved to be highly insignificant in

**Table 5.** Weibull Selection Model of Effects of Minority Status and Sanction History on Sanction Initiation during Second TANF Spell

Independent Variables	I Additive Model	II Interactive Model
Individual Characteristics		
Black client	1.190**	
Prior sanction	1.091*	
White client and no prior sanction		[Reference]
White client and prior sanction		1.032
Black client and no prior sanction		1.145*
Black client and prior sanction		1.288**
Age of client	.968**	.968**
Age of youngest child	1.012	1.012
Education (in years)	.979**	.979**
Income (in 1000s)	.975	.976
Community Characteristics		
Local conservatism	1.029	1.028
Percent black	.992*	.992*
Annual wage – food service/drinking places	.973	.973
Poverty rate	1.002	1.002
TANF caseload	.887*	.887*
Rho (error correlation)	-.223**	-.223**
Total N	40,891	40,891
Uncensored N (returning for 2nd spell)	18,827	18,827

*Note:* The sample for this model consists of all TANF clients (single-parent, female, black or white) who entered TANF from January 2001 through December 2002 and returned for a second TANF spell during this same observation period. All clients are observed during the 2nd spell until they are sanctioned or they exit TANF for other reasons. Clients who exited TANF without being sanctioned, or whose second spell continued beyond the close of our observation window (April 2004), were treated as right-censored observations in the Weibull model. The model was estimated in Stata 10.0 using the DURSEL procedure (Boehmke 2005). Cell entries are hazard ratios, with *p*-values based on robust standard errors (adjusted for error clustering at the county level).

\* *p* < .05; \*\* *p* < .01 (two-tailed tests).

Table 5 presents the results for our event-history model. The effects of the control variables largely conform to our expectations and look similar to the first-spell results reported in Table 4. The risk of sanction is highest for younger clients who are less educated, and the risk is lower among clients who live in counties with relatively large black populations and high TANF caseloads. The negative effect of percent black is especially interesting given the fact that we found a similar effect for percent Hispanic for the sample that included Hispanic clients. Whether through increased presence within the local welfare bureaucracy, outside pressure on local welfare policymakers, or

increased contact with the nonwhite population, it appears that a large minority presence within a community may offset any racial or ethnic biases in sanctioning that could occur within the implementation process.

Moving to the results for our primary hypotheses, we find strong corroboration of our experimental findings. The additive specification in column I tests for the independent effect of race. The hazard ratios reported here indicate that black clients are indeed significantly more likely than white clients to be sanctioned, regardless of their sanction history. The results in column II allow for a test of our interactive hypotheses and suggest that these two characteristics do interact as expected. Indeed, if we begin with the baseline risk of sanction for a white client who has no prior sanction, we find that the positive effect of making this client black is *significantly greater than* the effect of

preliminary models. Their inclusion in the model does not affect the results reported in Table 5.

giving the white client a prior sanction. Further, having a prior sanction does not seem to matter much for white clients, but it matters a great deal for black clients. The hazard ratio representing the difference in risk of sanction between whites with and without a sanction is close to 1.0 and is far from being statistically significant ( $p = .63$ ). By contrast, the risk of sanction for black clients with no prior sanction is approximately 14 percent higher than that for white clients without a first-spell sanction ( $p < .05$ ). Among black clients with a prior sanction, this risk is doubled; the risk of sanction for black clients with a prior sanction is 28 percent higher during the second spell than the risk for white clients with no prior sanction ( $p < .01$ ). In summary, even though black clients with no prior sanction are already at a higher risk than white clients of being sanctioned in the second spell, the addition of a prior sanction increases black clients' risk of sanction to a significantly greater degree than for white clients.<sup>27</sup>

## DISCUSSION AND CONCLUSIONS

This article confronts, more directly than past research, the question of how and why race influences sanctioning under welfare reform. To do so, we apply a general model developed to explain how racial classifications affect policy choices in diverse domains. The RCM provides clear, testable predictions about where and when racial disparities are likely to emerge in the administration of welfare programs. Our experimental results support these predictions and are corroborated by administrative data indicating how real clients under welfare reform have actually been treated. The results converge to provide striking evidence for both the utility of the model and the enduring power of race in U.S. poverty governance.

Our experiments randomly assigned case managers to vignettes in which clients differed in their race/ethnicity and in the possession of stereotype-consistent discrediting traits. Only one caseworker characteristic emerged as a significant predictor of sanction decisions: case

managers with more than two years experience were significantly less likely to impose sanctions in both experiments. Although we discussed several possible explanations for this pattern, our data do not allow us to distinguish among them, thus raising an important puzzle for future research.

In both law and principle, welfare sanctions should be imposed as responses to client behavior. In practice, however, we find that sanctions are also used in response to client characteristics. Despite having identical case narratives, our first vignette finds that a pregnant Latina client with four children is significantly more likely to be sanctioned than a white client with only one child. In the second vignette, we find that a black client with a prior sanction is significantly more likely to be sanctioned than a white client with no prior sanction. The two vignettes are quite different—in the racial/ethnic contrast involved, the nature of the stereotypical trait, and their relation to program rules regarding sanctioning—but the results are largely the same. White clients in these experiments suffer no statistically discernible negative effects when linked to characteristics that hold negative meanings in the welfare-to-work context. As advocates of administrative consistency might hope, case narratives elicit a stable pattern of responses from case managers, regardless of discrediting attributes, *when clients are white*. Minority clients enjoy no such immunity: their odds of being sanctioned rise in the presence of discrediting markers, even when the details of their case do not change a bit.

Our random-assignment vignettes offer a crisp test of causal effects, but this power is purchased at some cost. In the survey setting, case managers are pulled out of their normal organizational environment, client narratives are reduced to a handful of details, and hypothetical sanction decisions involve no cost to case manager time or recipient-family well being. To close this gap, we triangulated our experimental findings with an analysis of administrative data, a source of evidence that provides a weaker basis for causal inference but more faithfully reflects actual practices on the ground. In so doing, we test experimental evidence of implicit racial bias against evidence arising from the actual exercise of government authority (Duster 2008). The results of this analysis do not support our experimental find-

<sup>27</sup> We tested in the administrative data whether Hispanic clients with a prior sanction were more likely than white clients to be sanctioned again and found this not to be the case.

ings regarding the attribution of multiple children to a Hispanic woman. In the administrative data, Hispanic clients are no more likely than white clients to be sanctioned, and this null finding holds regardless of number of children. By contrast, we find strong support in the administrative data for our experimental results in Vignette 2. Among second-spell participants, black clients with a prior sanction are more likely than their white counterparts to be sanctioned. White clients suffer no discernible increase in their risk of sanction when they have a prior sanction, while black clients—who are already more likely to be sanctioned than whites—become significantly more likely to be penalized when this discrediting marker appears on their record.

Together, these findings offer powerful evidence that racial status and stereotype-consistent traits interact to shape the allocation of punishment at the frontlines of welfare reform. The lone instance in which our findings do not converge (the treatment of Hispanics in the experiment versus the administrative data) might suggest some inconsistency. Viewed in the context of the RCM, however, it actually conforms to a predictable and repeated pattern. Prior research indicates that the gap between Hispanic and white stereotypes is smaller than the gap between black and white stereotypes (Fox 2004; Gilens 1999). Accordingly, the RCM predicts that disparities will emerge more strongly and consistently in the latter case.

Indeed, we have found this precise pattern of race-based disadvantage in studies of other aspects of welfare reform (Fording et al. 2007; Schram, Fording, and Soss 2008; Soss et al. 2008). This article extends this body of evidence by showing that African Americans are distinctively vulnerable to the presence of discrediting social markers. In the present study, the stigma of deviant behavior attaches most strongly to black clients, more weakly and less consistently to Hispanic clients, and not at all to white clients. This pattern is further supported by our analysis of administrative data showing that Hispanic clients—unlike black clients—are no more likely to be sanctioned when they possess a prior sanction on their record. This pattern closely resembles the results Pager (2007) reports in her landmark study of how race and markers of criminality interact to limit African American men's prospects in the labor market.

While Latinas might face other forms of discrimination in the welfare system, including language barriers, our evidence suggests that racial classification leads to greater disparities in sanctioning for blacks than it does for Hispanics.

Our analysis does not directly test whether sanctioning is influenced by racial animus, threat, or loyalty—as opposed to the more cognitive dynamics emphasized by theories of implicit racism (Quillian 2008) and the RCM (Soss et al. 2008). It is worth noting, however, that we do not find evidence in our experiments that white case managers differ from nonwhite case managers in their sanction decisions. White case managers were no more likely to sanction clients overall and no more likely to be influenced by our experimental manipulations of race/ethnicity and client traits. These findings are hard to square with an account that emphasizes white ingroup loyalty or white animus toward blacks. They are far more consistent with models emphasizing how racial classifications operate in implicit ways—without conscious racism—to generate racial disparities (Quillian 2008). Race matters in more subtle ways than overt hostility or loyalty; race is built into the cognitive processes that provide the foundation for decisions about how target groups should be treated in welfare policy settings (Schram 2005).

Sanctioning practices under welfare reform are part of a larger turn toward disciplinary poverty governance in the United States (Wacquant 2001). In this context, it is imperative that social scientists begin to provide some insight into how disciplinary practices operate and how the state's authority to punish may be used in ways that deepen or ameliorate social inequalities. TANF is ostensibly a race-neutral public policy, but it is carried out today in ways that allow preexisting racial stereotypes and race-based disadvantages to produce large cumulative disadvantages (Schram 2005, 2006). Our prior research finds that black TANF recipients, relative to their white counterparts, are more likely to participate in the toughest policy regimes controlled at the most local levels (Soss et al. 2008). Within one such regime, Florida, we find that they are more likely to be sanctioned and their odds of being sanctioned are more likely to rise when they are associated with longer participation spells or participate

in conservative regions (Fording et al. 2007). While Florida may be distinctive in many ways, studies comparing Florida with other states suggest good reasons to think that the racial dynamics we have uncovered are not unique to welfare implementation in this locale (Shaw et al. 2006). The results presented in this article suggest that welfare sanctions, once imposed, become discrediting markers that make black clients even more vulnerable to a future sanction. Our findings reinforce the conclusion that policy choices not only reflect but also *create* the elements that underpin racial inequality in the U.S. welfare system. Under cover of a policy that is officially race-neutral, welfare systems operate in ways that reflect racial classifications, reproduce racial inequities, and call out for attention from both scholars and reformers.

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## APPENDIX: SANCTIONING PROCEDURES IN FLORIDA

Florida's Welfare Transition program is administered by public, nonprofit, and for-profit provider agencies under contract with the State's 24 Regional Workforce Boards (RWBs). In a series of program guidelines issued in February of 2004, Florida's Agency for Workforce Innovation (AWI) and the Department of Children and Families (DCF) clarified the Florida statutes relating to work penalties and pre-penalty counseling, with one goal being to "develop integrated and consistent procedures to implement sanctions" (AWI FG 03-037, 1). According to these guidelines, a first occasion of noncompliance with a work contract results in a full-family termination of temporary cash assistance for a minimum of 10 days or until the individual reestablishes compliance. Second and third instances of noncompliance are attached to periods of termination of one month and three months, respectively. Sanctions that remain in place for more than a 30-day period can be resolved only when the return to compliance has been documented and is accompanied by a new Request for Assistance (RFA) and face-to-face interview (F.S. 445.024). Previous sanctions may be "forgiven" for participants who are compliant with their welfare contracts for a period of six months. Following a six-month compliant period, any sanction levied against a participant is treated as a first occasion (AWI FG 03-037; F.S. 414.065).

The Florida Statutes allow for "good cause" exceptions to the sanctioning policies outlined above. This may represent the point at which the preferences and understandings of individual local actors most clearly come into play (Pavetti et al. 2003). Such exemptions may be granted for instances of noncompliance related to childcare,<sup>28</sup> the current or past effects of domestic

<sup>28</sup> A childcare exemption is reserved for participants who are single custodial parents "caring for a child under six years of age who can prove they are unable to obtain needed child care within a reason-

violence, medical incapacity, outpatient mental health or substance abuse treatment, and “circumstances beyond [the participant’s] control” (AWI FG 03-037, 5). In each of these instances, with the exception of current or past domestic violence, the individual must “prove to the RWB provider” that she has indeed been rendered unable to work. In the case of a physical or mental health or substance abuse exemption, such proof is, in principle, limited to documentation from a licensed physician or recognized mental health or substance abuse professional. However, the ways in which local actors interpret what constitutes adequate proof may be mediated by their relationships with clients, understandings of their role, and local political and economic contexts. In all instances for which a good cause exemption is thought to be ongoing, participants are required to submit to an Alternative Requirement Plan, with the penalties for failed compliance mirroring those applied to nonexempt program participants. Exactly what these alternative requirements should be is left largely to the discretion of the individual caseworker.

Once a participant has been reported for failure to comply with a work contract requirement, the state requires RWBs to provide notice, both in writing and orally, of the penalties attached to noncompliance prior to actually imposing such penalties. During this phase of contact, the *pre-penalty phase*, participants may still avoid a sanction by either establishing good cause for the noncompliance or returning immediately to satisfactory compliance. Throughout this pre-penalty phase, particular emphasis is placed on caseworkers providing counseling, a service that includes both reminding or warning the participant of the consequences of non-compliance and offering services or supports intended to remedy its causes.

While TANF participants are clearly responsible for establishing proof of the existence of grounds for a good cause exemption, program guidelines emphasize an ongoing relationship between the participant and the RWB representative, most often the individual casework-

er. As suggested by interview and narrative response data from our current project, caseworkers interpret the requirements and responsibilities of this relationship in a variety of ways. Moreover, what constitutes pre-penalty counseling may include a range of interventions, from a brief warning or reminder to extensive referral or direct clinical intervention. In both the interpretation and application of sanctioning rules, caseworkers exercise considerable discretion.

Such discretion persists even within the context of extensive provisions for the training of caseworkers in the application of sanctioning rules. Throughout Florida’s 24 regions, caseworkers are trained to apply sanctions according to the Welfare Transition (WT) Sanction Flow Chart developed by AWI (see Figure A1). Tracing the various paths that lead to a sanctioning decision suggests some of the ways individual actors might deviate from the planned course.

Should an individual be found noncompliant with the welfare contract, the caseworker is to mail a Notice of Failure to Participate and Possible Sanction (form AWI-WTP 2290), commonly referred to as the “2290,” within two days of the first failure. As noted above, an attempt to contact the participant orally is also required, and, even if this oral attempt proves unsuccessful, the participant is allowed 10 days from the date of the 2290 mailing to establish good cause for the noncompliance. A sanction is to be requested only if both attempts at contact fail over a period of 10 days following the written notification. If, however, the participant responds to either form of contact within the allotted time period, the pre-penalty phase can be ended with compliance, provided that one of two conditions is met: (1) the participant establishes good cause based on any of the criteria outlined above; or (2) the participant “agrees to demonstrate satisfactory compliance” (AWI FG 03-037, 7) and remains compliant with her welfare contract for a minimum of 30 days following the first failure.

Immediate, full-family sanctions are considered appropriate when a participant has either (1) failed to respond to the 2290 and oral attempts at contact, (2) failed to establish good cause and refused to demonstrate satisfactory compliance, or (3) failed to follow her welfare contract without good cause for a second time

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able distance from their home or worksite, child care by a relative or others is unavailable or unsuitable, or there is no affordable formal child care” (AWI FG 03-037, 7).

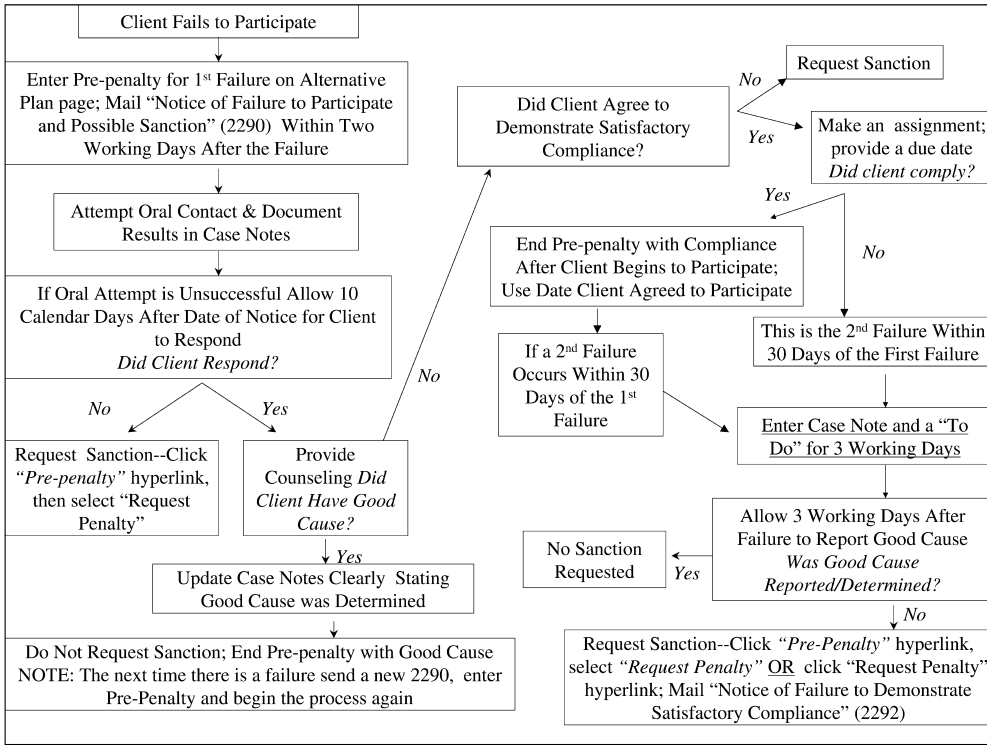


Figure A1. Florida Sanction Flow Chart

Source: Florida Agency for Workforce Innovation ([http://www.floridajobs.org/PDG/TrainingPresentations/WT\\_SancFlowChart061406\\_070306.ppt](http://www.floridajobs.org/PDG/TrainingPresentations/WT_SancFlowChart061406_070306.ppt)).

within 30 days of her first offense. All pre-penalty and sanctioning activities are to be recorded, in accordance with the WT flow chart, in the computerized One Stop Service Tracking (OSST) system. With its system-generated prompts and reminders of what actions are called for and when, the OSST system functions, if not as a check on discretion, then certainly as

a guide to when discretion may be most easily and least riskily applied.

While the flow chart provides a basis for suggesting that sanctioning in Florida is a highly structured process, a close reading of the chart shows that substantial opportunities for case-worker discretion remain. How that discretion gets used is the focus of our analysis.

**Table A1.** Variable Definitions and Data Sources for Analyses Presented in Tables 4 and 5

Independent Variables	Definition	Minimum- Maximum
<b>Individual Characteristics</b>		
Age of client	Client age (in years)	18–72
Number of children	Number of children in TANF family	0–12
Age of youngest child	Age of youngest child in TANF family	0–17
Income	Earned income in 1,000s	0–200
Education (reference category = more than 12 years):		
Less than high school	1 = more than 12 years, 0 = otherwise	0–1
High school	1 = 12 years, 0 = otherwise	0–1
Black client	1 = black, 0 = otherwise	0–1
Hispanic client	1 = Hispanic, 0 = otherwise	0–1
<b>Political Environment</b>		
County conservatism index	Measure of county political ideology, based on factor analysis of local election results for 18 ideologically-relevant constitutional amendments (see Fording et al. 2007).	–2.5–2.2
Percent black	Percentage of blacks in county of client in 2000 (County and City Data Books 2003).	2.1–57.1
Percent Hispanic	Percentage of Hispanics in county of client in 2000 (County and City Data Books 2003).	1.5–57.3
<b>Socioeconomic Environment</b>		
Annual wage	Average annual income in 1997 for employees in NAICS subsector 722, in 1,000s (County and City Data Books 2003).	7.795–16.674
Unemployment rate	Unemployment rate in county of client, measured each month (Florida Research and Economic Database).	1.7–19.7
Poverty rate	County poverty rate for all persons in 2000 (U.S. Census Bureau Small Area Income and Poverty Estimates).	6.9–24.2
TANF caseload	Number of TANF recipients per 100,000 county residents (calculated by authors).	.142–6.907
Population	Total county population in 2000, in millions (County and City Data Books 2003).	.007–2.253

*Source:* Data on client characteristics provided by the Florida Department of Children and Families.

*Note:* TANF = Temporary Assistance for Needy Families; NAICS = North American Industry Classification System. Descriptive statistics are provided for the combined sample and include data for white, black, and Latino welfare clients.

**Table A2.** Regression Results for First-Stage Selection Equation

Independent Variables	$\beta$
Individual Characteristics	
Age of client	-.0212**
Age of youngest child	-.0212
Black client	.3411**
Citizenship status (1 = citizen)	.2052**
Education	.0358**
Income (in 1000s)	-.0394**
Number of children	.0387**
Prior sanction (1st spell)	.2632**
Community Characteristics	
Annual wage – food service/drinking places	-.0159
Local conservatism	-.0466**
Percent black	-.0038**
Poverty rate	-.0399**
TANF caseload $t-1$	.2085**
Sample size (first spell clients)	40,891
Number of clients returning for 2nd spell	18,827

*Note:* The sample for this analysis consists of all new TANF clients (single-parent, female, black or white) who entered TANF from January 2001 through December 2002. The dependent variable is a dichotomous variable equal to 1 for clients who returned to TANF for a second spell during our observation period (and 0 otherwise). Cell entries are coefficients generated from an exponential discrete choice model (see Boehmke et al. 2006), estimated using the DURSEL procedure in Stata 10.0 (Boehmke 2005).

\*  $p < .05$ ; \*\*  $p < .01$ .

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