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**Deciding to Discipline:
A Multi-Method Study of Race, Choice, and Punishment at the
Frontlines of Welfare Reform**

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Punishment is a core technology of governance, central to the modern state. Across diverse societal domains, penalties and disciplinary systems operate as key features of the policy process, establishing behavioral incentives for target populations (Ehrlich 1996) and shaping the ways individuals understand and regulate their own conduct (Foucault 1979). Indeed, in recent years the state's authority to punish has taken on greater significance in American politics, as governance in the U.S. has come to rely more heavily on disciplinary tools. New criminal justice policies have expanded the use of imprisonment, producing a level of mass incarceration that is unprecedented in American history and unrivaled by other nations (Western 2006). This custodial turn has not been an isolated policy development; it has coincided with a broader shift toward paternalist principles in U.S. social policy (Starobin 1998). Policies for low-income populations, in particular, have been redesigned around the idea that the state has a legitimate interest in ensuring that socially marginal groups practice appropriate behavior. Today, public aid programs are more directive in setting behavioral expectations, supervisory in monitoring compliance, and punitive in responding to infractions (Mead 1997).

As disciplinary systems have come to play a more central role in poverty governance, the study of administrative practice has taken on greater urgency (Mead 2004). Because public benefits are now more contingent on recipient behavior, policy implementers have had to reorganize their operations around the tasks of identifying and penalizing rule violators. Indeed, it is inevitably at the frontlines of policy implementation – in concrete organizations such as courts and schools and prisons and welfare agencies – that the politics of punishment culminates in actual decisions to impose penalties on some but not others. Thus, to understand discipline as an element of governance, political scientists must investigate how such designs are actually put into practice at the organizational level and how disciplinary decisions are actually made by the street-level bureaucrats who work at the frontlines of policy implementation. It is here, in the administrator's use of discretion, that the state's capacity to punish collides with the core question of distributive politics posed by Harold Lasswell (1936): "who gets what, when, how?"

This paper is part of a larger study that analyzes the politics of disciplinary practice in American poverty governance in the contemporary era. The focal point for our study is the Temporary Assistance for Needy Families (TANF) program created in 1996 by the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA). This landmark legislation, widely seen as the centerpiece of “welfare reform,” ushered in a new era of public-aid provision for low-income families. PRWORA abolished the old cash assistance program, Aid to Families with Dependent Children (AFDC), and replaced it with the TANF block grant to states. Under the new program, states quickly reduced the number of families receiving cash assistance and redirected program funds from cash aid to services. By 2000, the national TANF caseload had dropped by over 60 percent; and by 2005, over half the block grant money was being used to provide services rather than income support (Allard 2007).

Under the TANF program, the goal of providing income to poor families with children has been made secondary to the goal of using incentives and penalties to encourage adults in these families to enter paid employment. Under the federal law, families of adult recipients cannot receive assistance for more than five years in a lifetime, and adult recipients must work or participate in “work-related activities” to remain eligible for assistance. State TANF programs must meet quotas regarding the percentage of recipients who participate in work-related activities. If they fail to do so, they suffer penalties in lost federal funding which they must replace with state monies.

“Sanctions” are penalties that reduce or terminate program benefits in response to client non-compliance. In state TANF programs, they function as a primary tool for encouraging the client behaviors that are needed to meet federal quotas. Direct responsibility for this tool falls to the case manager at the frontlines of service provision and, as a result, caseworkers have been given important new responsibilities under welfare reform, including monitoring clients, constructing and enforcing work plans, and, in cases of non-compliance, deciding whether to impose a sanction (Hasenfeld and Powell 2004). Researchers have responded by producing a number of important and well-crafted ethnographic studies of case managers’ activities under welfare reform (see e.g., Morgen 2001; Riccucci 2005; Lurie 2006; Watkins 2007). Yet there remains a curious divide between these field studies of frontline practices

and worldviews, where case managers loom large, and the mostly econometric literature on sanctioning, where decisions made by case managers are bracketed in favor of attention to client and community characteristics (see e.g., Wu et al. 2006; Pavetti, Derr and Hesketh 2003). Lost in the gap between these literatures, the case manager's selection of whom to sanction (arguably the most pivotal decision point in the frontline distribution of punishment) has remained an orphan of political analysis.

This paper presents an analysis of case manager decisions to impose sanctions on welfare clients, paying particular attention to how such decisions are influenced by a client's race and possession of discrediting social markers. To pursue this analysis, we draw on an ongoing study of TANF implementation in Florida, a state that is widely recognized as a leader on four key dimensions of welfare reform: (1) devolution of policy control and the integration of welfare-to-work management into the operations of regional workforce boards; (2) privatization and the contracting out of welfare-to-work services to for-profit and non-profit local providers; (3) performance management and the use of state-of-the-art administrative systems to track the extent to which providers meet performance goals; and (4) the use of sanctions as a method of bringing client behavior into compliance with welfare-to-work goals.

The data used in the larger project come primarily from three related sources: administrative data on all clients participating in the Florida welfare-to-work program from January 2000 through December 2004; field research consisting of observations and interviews in street-level offices known as "one-stop centers" located in purposively selected workforce regions; and a web-based survey/experiment involving welfare-to-work caseworkers across the state. In our earlier work, we have analyzed administrative data and found evidence that: (1) Welfare recipients in conservative counties are more likely to be sanctioned than are recipients in liberal counties. (2) As their participation spells move past the initial months, nonwhites become significantly more likely than whites to be sanctioned. (3) Such racial disparities are significantly greater in conservative counties than in liberal counties. (4) Sanction disparities between liberal and conservative regions also increase when regions receive evidence that they are falling behind in meeting their performance goals. (5) Across the state, sanctions are disproportionately applied to those

clients who are most economically disadvantaged and result in these clients becoming even worse off relative to their non-sanctioned counterparts (for an overview, see Schram, Fording and Soss 2008).

In addition, our analyses of the data we have collected through field research have revealed that: (1) Case manager decisions about sanctioning are deeply entwined with anxieties regarding the satisfaction of performance goals. (2) Caseworkers are often ambivalent about sanctions as a policy tool, doubting that sanctions have beneficial effects on client behavior, family well being, or organizational performance, but also feeling that they lack alternative tools for responding to the gap between performance expectations and the client situations they confront. (3) Sanctions are often perceived as evidence that the caseworker has failed to get the client to participate effectively and, hence, are often applied with reluctance. (4) Nevertheless, caseworkers routinely initiate the sanctioning process as a way to motivate compliance and then, once the first steps in the administrative system have been taken, find that they are carried rapidly toward imposing a sanction. (5) Caseworkers also report imposing sanctions out of frustration with clients whom they perceive as not living up to their responsibilities and, as a result, dragging down their performance numbers. (6) Consequently, judgments about the client's motivation levels and efforts to meet program goals are, along with performance pressures, central to the sanction decision making process (for an examination of this field research, see Fording, Schram and Soss 2006).

The current paper builds on these prior analyses by focusing more directly on case manager decisions and their effects on the distribution of sanctions. The centerpiece of our analysis is a set of random-assignment experiments that we embedded in a web-based survey of Florida WT case managers at the end of 2006. The experiments present case managers with realistic rule-violation vignettes, randomly assigning key characteristics of the clients described and then asking case managers whether they would impose a sanction in response. To the best of our knowledge, no prior research on sanctioning has made use of such an approach, which provides 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. In taking this approach, we hope to shed new light on the ways that client race and stigmatizing traits intersect to influence the allocation of punishment.

Despite the inferential advantages of this survey-experimental design, the use of hypothetical vignettes and the limitations of our sample both suggest that some humility is in order when approaching these data. Accordingly, we adopt an approach in this paper that emphasizes triangulation across data sources. To construct an empirical bridge between our general Racial Classification Model of policy choice (see Soss, Fording, and Schram 2007) and the question of how racial-group reputations relate to TANF sanctions, we present analyses of national survey data. To explain how such reputations fit into caseworker decision-making processes, we draw on findings from in-depth interviews with case managers in the field. And finally, to subject our survey-based findings to a further test, we analyze administrative data from Florida's welfare-to-work program. These data lack the advantages of inference associated with experiments but have the considerable benefit of reflecting actual decisions made about actual cases in the context of real organizational routines and work pressures that are, by definition, held at bay in the context of a survey.

Sanctions and Welfare Reform

Sanctions have long been used by caseworkers to encourage compliance with state welfare rules. Over the past decade, however, these penalties have come to play a more prominent role in welfare implementation. Relative to the AFDC program, the TANF program specified stricter work requirements for participants, narrower exemption criteria, a greater number of behaviors subject to sanction, and stronger penalties for noncompliance (Hasenfeld and Powell 2004). Most analysts agree that sanctions have been a linchpin of the successful effort to transform welfare from a system focused on providing cash benefits (AFDC) to one focused on the promotion of work norms (TANF; see Pavetti et al. 2004).

By federal law, state TANF programs must subject clients to a reduction in benefits if they fail to follow specified program rules, but states have been free to choose among a range of options in deciding exactly how benefits should be reduced. Key elements of sanction design have included: (1) whether to reduce the benefit for the eligible adult, all adults in a family, or the entire family; (2) whether to impose a partial or full reduction of benefits; and (3) whether to impose the state's strongest TANF penalty immediately or follow a progression of increasingly severe sanctions. Seventeen states rely on the strictest

combination of these choices, enforcing what is referred to as an “immediate full family sanction” (Pavetti, Derr, and Hesketh 2003, 2). In these states, the entire TANF family is immediately removed from the TANF 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 (Pavetti, Derr, and Hesketh 2003, 3). The remaining states enforce what is known as a “partial sanction” of benefits (usually reducing only the portion of the grant awarded to adults; Pavetti, Derr, and Hesketh 2003, 2).

Studies using a variety of dependent variables suggest that sanctioning has important effects on the size and characteristics of the TANF caseload. In one of the first relevant studies in the TANF era, Heidi Goldberg and Liz Schott (2000) estimate that between 1997 and 1999, nearly 500,000 families lost benefits due to sanctions. This represents a quarter of the caseload reduction for that period. Other studies that focus on selected states also find the incidence of sanctioning to be quite high (for a review, see Pavetti et al. [2004]). Given these estimates, it is not surprising that many state-level studies find high caseload reductions among states with strict policies. States with the strictest sanctioning policies experienced a caseload reduction that is as much as 25 percent greater than those reported by states with the least stringent policies (Rector and Youssef 1999; Mead 2000).

Case Selection: Why Study Sanctions in Florida?

The Florida Welfare Transition (WT) program provides an important and analytically advantageous case for studying sanctions. We have selected the Florida WT program for our study, not because it is typical of all state TANF programs, but because it provides close to an ideal setting for analyzing how disciplinary practices operate and vary by communities, organization types, and subgroups of the client population. To begin with, Florida has relied heavily on sanctioning as a welfare policy tool. After 1996, its lawmakers adopted “some of the strictest time limits and work requirements in the nation,” broadening the pool of clients subject to sanctions by permitting “few possibilities for exemptions” (Botsko, Snyder, and Leos-Urbel 2001, 4). Sanctions in Florida are both immediate (i.e., actionable on a first offense) and full-family (i.e., applied to every member of a family rather than, for example, exclusively to the working age adults), and are often accompanied by reductions in food stamp benefits

for a whole month with the first infraction. Although cross-state comparisons are complicated by the diverse methods that states use to calculate sanction frequency, our analysis of administrative data suggests that Florida employs sanctions at an extremely high rate. Indeed, sanctions were the most common cause of TANF case closings in fiscal year 2003, accounting for 31 percent of closings as compared to only 21 percent of cases being closed due to increased earnings.

In addition, Florida is 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. These clients participate in a TANF program that is one of the most decentralized and organizationally diverse in the United States. Frontline services in Florida have been contracted out to a diverse array of public, non-profit, and for-profit providers, and primary authority over the "local operating procedures" of the WT program has been shifted down to 24 public/private "Regional Workforce Boards" (RWBs). These RWBs are responsible for strategic planning, policy development, contracting, and oversight of local one-stop delivery systems. They are overseen, not by state agencies, but by a statewide public/private partnership called Workforce Florida, Inc. (WFI). The Florida Department of Children and Families (DCF), a conventional state agency, receives the federal TANF block grant and maintains responsibility for eligibility determination. But otherwise, Florida stands out among American states for its emphasis on local control and privatization within a work-oriented TANF program (Botsko, Snyder, and Leos-Urbel 2001: 7).

Thus, by studying sanctioning in Florida, we are able to observe the interplay of strong disciplinary practices, organizational diversity, devolved policy control, differences in jurisdictional characteristics, and substantial social diversity. Florida offers a crystallization, in strong form, of the intersecting changes that, in various shades of grey, have redefined poverty governance in the U.S.

Caseworker Discretion in the Sanction Process

We focus on caseworker decision making in this study because it is a key distributive mechanism in the allocation of punishment. It is not, of course, the *only* governmental source of variation in the "who

gets what, when, how” of sanctioning. The penalties deployed at the frontlines of welfare agencies are products of choice at numerous levels of governance. As noted earlier, state legislatures have chosen a variety of courses in designing their sanction policies. In most states, additional discretion can be found at the local level, where a variety of officials hold responsibility for interpreting state sanction policy and specifying it through rulemaking. As a result, local TANF officials have substantial discretion over program elements that affect the rate and incidence of sanctioning. Their local policy choices typically establish the process by which clients are informed of TANF rules and penalty procedures; the mode of monitoring participation in required activities; the steps for initiating a sanction, including how and when clients are notified of an impending sanction and the steps needed to avoid it; and the procedures and requirements for “curing” a sanction and returning a client to the TANF rolls. Not surprisingly, given the wide scope of this discretion, field studies find that, even when TANF offices operate under the same state guidelines, sanction philosophies and practices tend to vary dramatically across local communities (Pavetti, Derr, and Hesketh 2003; Born, Caudill and Cordero 1999). In our own study of Florida, we have found considerable evidence that differences in local political climates influence overall rates of sanctioning as well as the extent of racial disparities in sanctioning rates (Fording, Soss, and Schram 2007a, 2007b).

Beyond the effects of state and local policy choices, however, the distribution of sanctions is most directly shaped by the proximate decisions that occur in street-level interactions between case managers and clients. Since the pioneering work of Michael Lipsky (1980), the importance of discretion in street-level bureaucracies has been widely recognized by students of public policy. Of course, several kinds of constraints limit caseworker decision making, but each does so in less than definitive ways. First, supervision may be used to limit or guide caseworker discretion. Such supervision may actually have increased under welfare reform in states such as Florida where state-of-the-art management information systems facilitate monitoring of caseworkers and their clients (Shaw et al. 2006; Fording, Schram, and Soss 2006; Dias Johnson and Maynard-Moody 2007; Ridzi 2004; Lurie 2006). Caseworkers, however, rarely operate under the direct watch of supervisors (Prottas 1979; Lipsky 1980). They confront cases that

are too complex and idiosyncratic to fit neatly under a supervisor's *a priori* directive, and they process cases at rates that make continual consultation impractical (Maynard-Moody and Musheno 2003).

Second, administrative rules place broad limits on caseworkers, but such rules cannot be designed to cover all conceivable situations, nor can a rule "itself step forward to claim its own instance" (Hart 1961: 123). Choices must be made about how and when to apply a given rule, and as the number of rules grows, so too does the scope of frontline decision-making. Rather than eliminating discretion, "a profusion of rules can lead to greater freedom because... the bureaucrat must choose which rules are appropriate in the present [case]" (Feldman 1992: 166; see also Lipsky 1980: 14).

Third, program clients may check caseworker discretion by using formal or informal means to limit decisions contrary to their interests. Here again, however, theory and evidence suggest that client action is a weak constraint. Political economy approaches to social work theory argue that welfare clients occupy a dependent position in a relationship defined by unequal control of power resources (Hasenfeld 1987). To be sure, clients are active participants in welfare interactions, and they do what they can to subtly influence outcomes (Watkins 2007). But field research suggests that welfare clients feel a keen sense of their dependence on case managers; most clients report that they avoid explicitly opposing caseworkers because such opposition is seen as risky and ineffective (Soss 2000). Under the TANF program, moreover, client benefits are no longer an entitlement, and clients have lost some of the formal rights of appeal that they had under AFDC. These changes in policy seem likely to further weaken clients' abilities to impose limits on caseworker discretion (Mink 2002).

Given the limits of these constraints, it has become conventional wisdom that "discretion is inevitable" in street-level work (Maynard-Moody and Musheno 2000, 329). Decision making at the front lines is widely recognized as a political process that elaborates, transforms, and sometimes subverts the policy intentions of lawmakers (Meier 1999). Case manager discretion may serve as an entry point for unjust and unequal treatment or, alternatively, may permit the tailoring of more equitable and humane responses than the rules would lead one to expect (Keiser 1999: 88-89).

Thus, case managers' sanctioning decisions are an important object of study in their own right and a key element of the broader system of discipline that operates under contemporary welfare provision. Yet the *impact* of caseworker discretion on patterns of punishment in welfare programs remains an area where surprisingly little is known. Large-sample econometric studies of sanctioning, based on survey or administrative data, illuminate important questions about who gets sanctioned but do not address the independent role of the caseworker at all (see e.g., Wu et al. 2006; Pavetti, Derr and Hesketh 2003; Hasenfeld, Ghose, and Larson 2002; Kalil, Seefeldt and Wang 2002). Ethnographic studies of street-level welfare offices, by contrast, pay close attention to case managers' practices and worldviews but tell us little about how their decisions actually affect patterns of punishment (see e.g., Lurie 2006; Brodtkin 1997; Watkins 2007). To address this question more directly, we suggest, one must combine field research and statistical analyses of administrative data with the power of survey-experimental designs capable of isolating case manager choices as a distinctive element of a broader and more complex sanctioning process.

Sanction decisions require case managers to make judgments about *clients*, as individuals and group members, and about *narratives* that supply the reasons why a client has deviated from program requirements (Maynard-Moody and Musheno 2000). The focus of our analysis in this paper is on how client racial characteristics figure into and influence such judgments. To develop expectations regarding this process, we rely on a general model of racial classification and policy choice that we have developed and tested in earlier work (Soss, Fording, and Schram 2007; Fording, Soss, and Schram 2007b).

The Racial Classification Model and Street-Level Decisions to Punish

The Racial Classification Model (RCM) offers a general and parsimonious explanation for how race and ethnicity influence policy choice in an era in which de jure discrimination is outlawed and egalitarian norms are widely endorsed (Soss, Fording and Schram 2007). Elaborating on the work of Schneider and Ingram (1993, 1997), the RCM identifies how and when racial classifications should affect target-group constructions and, hence, policy design and implementation patterns. The model does not preclude but, crucially, does not assume the operation of racial animus, racial threat, or racial group

loyalty. It is a minimalist cognitive model of policy decision making that focuses solely on the necessity of social classification and consequences of group reputation. The RCM consists of three basic premises.

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, they would be unable to bring coherence to a complex social world or determine appropriate action.
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.
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 (a) the prevailing cultural stereotypes of racial groups, (b) the extent to which policy actors hold relevant group stereotypes, and (c) the presence or absence of stereotype-consistent cues.

The RCM asserts that policy actors try to choose courses of action that they expect to be effective given what they believe about the specific groups they aim to address. In this process, social group characteristics can serve as proxies for more detailed information about a policy's intended target group. When race is salient to a policy area, as in the case of welfare (Gilens 1999), racial classifications can serve precisely this function, regardless of whether racial animus is present and regardless of the decision makers' own racial identities. In such cases, group reputations can frame interpretations of ambiguous policy-target behaviors and cue broad assumptions about what kinds of policy actions are likely to be effective. The effects of such reputations, however, will depend on their specific policy-relevant content as well as situational factors that may strengthen or weaken their utility as information proxies.

In applying the RCM to TANF case managers' sanction decisions, we conceptualize sanctions as tools for motivating welfare clients, stimulating work effort, and enforcing responsible behavior. Accordingly, case managers should be more likely to implement sanctions in a stringent fashion when TANF clients are perceived as less motivated and responsible in their own right – i.e., when clients are perceived as needing a stronger external stimulus to follow program rules and achieve welfare-to-work goals. In this context, client race should affect sanctioning patterns to the extent that contrasts between racial-group reputations convey information about motivation, work effort, and personal responsibility. A

corollary to this point is that racial disparities should emerge more strongly and consistently when the salient contrast between group reputations is larger; they should emerge more weakly and less consistently when the salient contrast is smaller (see Soss, Fording, and Schram 2007).

Beyond these baseline expectations, however, the RCM points to more situational effects, with the impact of client race depending on factors such as the degree to which policy actors hold relevant group stereotypes and the presence or absence of stereotype-consistent cues. In the present study, we focus particularly on the power of stereotype-consistent client characteristics to cue group reputations in a manner that generates racial disparities in sanctioning.

Two individuals who are perceived as members of a single racial group may nevertheless be associated with quite different group reputations. Research on intersectionality, for example, emphasizes that the meaning of one category of social identity can be altered when combined with another (e.g., when woman is combined with black as opposed to white, see Crenshaw 1991; Hancock 2006). Likewise, social cognition researchers have shown that perceivers tend to distinguish “subtypes” of racial groups based on additional characteristics (such as “ghetto blacks” vs. “black businessmen”) and to attribute global-group traits to these subtypes to very different degrees (Richards and Hewstone 2001; Devine and Baker 1991). As a result, race-of-target effects will often be contingent on additional characteristics that strengthen or weaken the individual’s connection to the racial group’s prevailing reputation. Eberhardt et al. (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.”

Indeed, research suggests that a variety of stereotype-consistent cues can enhance race-based disadvantages. In some instances, this effect functions to widen already-existing racial disparities. In others, it creates disparities where none had otherwise existed. The former scenario is well illustrated by Pager’s (2003, 2007) influential field experiment exploring the effects of race and “the mark of a criminal record” on hiring outcomes. Pager (2003) finds that black job applicants are already disadvantaged relative to whites in the no-felony condition of her experiment, yet the attribution of a felony conviction actually reduces black applicants’ job prospects to *a greater degree* than the prospects of already-

advantaged white applicants. By contrast, the latter scenario is illustrated by a recent study of how racial cues affect preferences for 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.... [Likewise] the presence of black images alone... does not prime negative racial attitudes.... The effect emerges only when the pairing of the visuals with the narrative subtly reinforces negative stereotypes in the mind of the viewer.

Building on this research, the RCM suggests that the presence of a trait that is consistent with minority-group stereotypes should increase the odds of a sanction more for minority clients than for white clients. In the present analysis, we make use of two such client traits: one related to images of long-term welfare dependency and a preference for living off welfare rather than pursuing the hard work of paid employment (see Gilens 1999) and a second related to images of sexual irresponsibility and the licentious welfare queen (see Hancock 2004; Smith 2007). Our central hypothesis is that the presence of such traits will disproportionately penalize clients who are black or Hispanic, resulting in case managers being significantly more likely to sanction these clients relative to (a) white clients with or without the stigmatizing trait and (b) black or Hispanic clients who lack the stigmatizing trait. Specifically, as we detail below, we predict that case managers will be more likely to sanction a pregnant Hispanic client with four children than to sanction a client who is white and/or who has only one child. We also predict that case managers will be more likely to sanction a black client with a prior sanction on her record than to sanction a client who is white and/or has no prior sanction on her record. Moreover, combining the RCM with stereotype data presented below, we expect these patterns to emerge more strongly and consistently in the case of black recipients than in the case of Hispanic recipients.

Empirical Bases for our Expectations

In deriving these predictions from the RCM, we rely on several empirical observations to establish the relevance of the model and specify its application. The first condition for applying the model is that the relevant group reputations must differ significantly with regard to the prevalence of sexual irresponsibility and preferences for welfare over work. Figure 1 presents evidence from two national surveys. Although many surveys ask respondents to assess racial and ethnic group tendencies toward

“laziness” as opposed to “hard work,” the best survey item for our purposes is a more policy-specific question posed by the General Social Survey (GSS) in 1990: “Do people in these groups [e.g., black, Hispanic, white] tend to prefer to be self-supporting or do they tend to prefer to live off welfare?” To assess group stereotypes regarding sexual behavior, we rely on a parallel series of items included in the National Survey on Poverty Policy (NSPP) conducted by one of the authors in 2002 (Soss 2002; for details, see Soss and LeClair 2004): “Where would you rate most [white, Hispanic, black] women on a scale of 1 to 7, where 1 indicates sexually responsible, 7 means sexually irresponsible, and 4 indicates most [white, Hispanic, black] women are not closer to one end or the other?”

[Figure 1]

As Figure 1 shows, group reputations along these two dimensions are consistently defined, with whites being the least closely associated with welfare-relevant stereotypes, blacks being the most closely associated, and Hispanics falling in the middle. Only 4.5 percent of respondents in the GSS agreed that most whites prefer welfare over work, while 45.5 percent held this view of Hispanics and 57.1 percent held this view of African Americans. Similarly, while 19.8 percent of NSPP respondents viewed most white women as sexually irresponsible, 30 percent held this perception of most Hispanic women and 41.9 percent held this view of black women. Accordingly, in applying the RCM to case managers’ sanction decisions, we assume that traits associated with welfare dependence and sexual promiscuity will be more stereotype-consistent for black and Hispanic TANF clients than for white clients, with the associations being greatest of all for black clients.

In addition to establishing that these aspects of reputation vary across groups, it is important to ask whether they have relevance, in the society at large, for the specific policy activity under investigation: the use of sanctions to punish client non-compliance in welfare programs. To answer this question, we can return to the NSPP data from 2002, where respondents were asked to state their support or opposition for a welfare sanction policy along a 7-point scale: “If a welfare recipient misses a job assignment or a meeting with an agency official, the welfare agency should reduce her monthly benefits.” To assess the effects of the two stereotypes on this policy preference, we follow the model specification

used by Martin Gilens (1999) in his influential analysis of racial attitudes and public opposition to welfare spending. Specifically, we rely on sequential ordinal logit models, where sanction policy preference is the dependent variable and the independent variables included as controls include measures of each respondent's sex, age, education, family income, commitments to individualism, non-Hispanic white status, ideological identification, and southern residence. The key independent variables in the two models are (a) the difference between each respondent's placements of "most blacks" and "most whites" on a 1-to-7 scale running from "hard working" to "lazy" and (b) the difference between each respondent's placements of "most Hispanic women" and "most white women" on a 1-to-7 scale running from "sexually responsible" to "sexually irresponsible."

Figure 2 presents the relevant results. Each model as a whole offers significant predictive power (summary statistics are presented in the note to Figure 3; full models are available on request) and, in each case, we find that endorsement of the racial group stereotype has the expected positive effect. The vertical axis in Figure 2 represents the cumulative predicted probability of a respondent supporting welfare sanctions as a policy (i.e., a response greater than 4 on a 1-7 scale). The horizontal axis represents the difference in respondents' perceptions of groups, with higher values indicating that the negative stereotype is more strongly associated with the minority group and a value of 0 indicating equal association. The lines in Figure 2 show, for a hypothetical respondent with mean values on all control variables, how the predicted probability of support for sanctions rises as perceptions of minority group traits move in a more negative direction. The effects are statistically significant, substantial, and quite similar. When we hold all other variables constant at their means, the predicted probability of support starts at .31 in both cases (at the far left of Figure 2). This predicted probability rises to .44 for respondents perceiving blacks and whites as equally likely to be lazy (the zero point) and then rises to .59 for those respondents who perceive the most pronounced difference between black laziness and the white work ethic ($z=2.20$, $p=.014$). This predicted probability rises to .45 for respondents perceiving Hispanic women and white women as equally likely to be sexually irresponsible (the zero point) and then rises to

.60 for respondents who perceive the largest disparity between Hispanic women's promiscuity and white women's tendencies to be responsible in sexual matters ($z=2.08$, $p=.019$).

[Figure 2]

These analyses of national survey data go some distance toward establishing an expectation that the RCM should apply well in the domain of welfare sanctions and using the specific dimensions of group reputation we have chosen. Yet there remains a considerable distance between these findings and our actual object of study. TANF case managers are far from a random subset of the U.S. population; their decisions involve applying sanctions to specific people and situations, not supporting a policy in general; and they make these decisions in the context of particular organizational norms and routines as well as official rules outlined in training sessions and reinforced by supervisors. Are there good empirical reasons to suspect that similar relationships may inform the sanction implementation decisions of case managers?

Field research that makes use of participant observation and in-depth interviews has distinctive advantages for pursuing this empirical question, and we have been engaged in just such a project over the past three years in the Florida WT program. For reasons of space, and because we have not completed a systematic analysis of all our transcripts and field notes, we do not pursue a detailed presentation of field evidence in this paper. Nevertheless, we have considerable confidence in drawing on our field observations to portray some key aspects of case manager decision-making and to suggest how they make the influence of racial classifications plausible.

To date, our field study supports a portrait of caseworkers' sanctioning decisions that blends elements of the "logic of appropriateness" (March and Olsen 1989, 2004) with elements of the "social construction of target populations" (Schneider and Ingram 1997). The case managers we interview strive to make *appropriate* decisions in particular cases "based on mutual, and often tacit, understandings of what is true, reasonable, natural, right, and good" (March and Olsen 2004: 4). They ask, "what kind of a situation is this; what is my role in the program, in the organization, in the community, and in relation to this particular client; and what is a person such as I supposed to do in a situation such as this?" The case

managers we interview strive to do what's "right" in a context in which norms, cues, and incentives frequently point in conflicting directions.

Thus, in deciding what is "appropriate" in the handling of a given case, frontline workers must search for cues that offer some guidance. In this process, few factors play a role as decisive as assessments of clients themselves. As Maynard-Moody and Musheno (2003: 78) conclude in their expansive study of discretion at the frontlines of public service, "street-level workers want to get a sense of who citizen-clients are, get a fix on people, to decide how to handle them. In figuring out who a person is, they may also put a fix on people, assigning them to a social identity or group belonging that carries with it significant meaning and consequence." When sizing up the case and deciding whether to sanction, the case managers we have interviewed report that they are also sizing up the person, asking themselves whom to believe and whether a problem this week is likely to be an aberration or part of a larger pattern.

Perhaps an analogy from the university setting may help to clarify the point. When a student arrives at office hours the day before an assignment is due to ask for an extension and present a story justifying the request, what is a professor to do? One of the most common responses, we suspect, is for professors to assess the plausibility of the student's excuse in light of what she or he knows (or at least *thinks* she or he knows) about the student. Has the student been working hard in the class? Has the student asked for special treatment before? Does the student seem like the "type" of person that this "kind of thing" might happen to? In trying to answer these and related questions, we professors are forced to rely on a species of heuristic reasoning (Gigerenzer 2002), striving to fill in the larger amounts of information needed to make a sound decision based on the smaller bits of information about the person that we actually have. The goal of this reasoning may be to arrive at an appropriate and just decision but, as students of heuristics have long emphasized, this process of arriving at judgments also provides an entry point for cognitive biases (Kahneman, Slovic, and Tversky 1982).

Our field research suggests that case managers engage in just this sort of reasoning when processing new clients and deciding how to handle potentially sanction-able instances of client behavior. They search for cues that will help them "get a fix on people [and] decide how to handle them"

(Maynard-Moody and Musheno 2003: 78). As an illustration, consider this description from a case manager we interviewed in the Florida WT program in 2005.

The first time when I meet with a client, the first thing I'll do is that I'll look on the... screen that we utilize for sanctions. So I'll do research and see how many sanctions they have had in the past or any, and that will give you a feeling if the client is going to participate. That is the first clue there. [...And] clients that are routinely starting [i.e., returning to] the program from the start, you know they will be unwilling to participate and will be someone that will always push the limits of the program.

In this brief excerpt, one can see clearly how the process of sizing up clients begins early and serves as a guide to the disposition of cases. And it is here, in the process of sizing up the client, that racial classifications may enter as unacknowledged and perhaps even unwanted influences on case managers' interpretations of what particular elements of personal and programmatic history suggest. Indeed, as Michael Herzfeld (1992) emphasizes, the categories deployed inside bureaucracies may be stated in distinctive, technical languages, but their meanings and uses depend heavily on the social categories that organize understandings in the broader society. Moreover, at the frontlines of service provision, the categories that matter most concern *types of people*. As Jeffrey Prottas (1979: 123-24) writes in *People Processing*, his classic analysis of street-level bureaucracies:

The critical step in the street-level bureaucrat's job is the categorization of clients. That act defines all activities that follow. Although it is possible, and not uncommon, to take short-cuts in the processing stage, this only represents a small opportunity for the street-level bureaucrat....Client categorization is the official job of street-level bureaucrats. The agency for which the bureaucrat works provides a "menu" of categories; it is for the street-level bureaucrat to distribute clients among them....[T]he sanctioned modes of distinguishing between clients are often inadequate for making the distinctions that the street-level bureaucrat needs to make....For this reason, a system of unsanctioned and informal categories that segments clients by criteria relevant to the immediate needs of street-level bureaucrats has grown up in public service delivery bureaucracies.

In sum, sanctions are nominally penalties tied to *categories of behavior*, but frontline decisions about whether to impose a sanction hinge, to a significant degree, on the ways case managers see and use *categories of people*. The act of sizing up the person is an inseparable part of deciding whether to believe the hard-luck story, whether to see a client's misstep as willful or accidental, whether it will be better to "let this one slide for now" or "send a message that we're not fooling around." In making these sorts of

judgments, case managers draw on “gut feelings” that reflect, not just professional experiences and worksite norms, but also broader societal reputations associated with particular groups.

Accordingly, we hypothesize that when clients exhibit a *combination* of racial minority status and stereotype-consistent case characteristics, they will have a significantly greater likelihood of being sanctioned, relative to other TANF participants. We also hypothesize that such effects will be stronger and more consistent for blacks than for Hispanics, as a result of the more negative stereotypes attached to blacks relative to Hispanics and whites.

Survey-Experimental Analysis: Research Design

To test these expectations, we employed a web-based instrument to survey all front-line case managers who participate in the sanctioning process in Florida’s WT program. In order to ensure anonymity and confidentiality for respondents, Agency for Workforce Innovation (AWI) distributed the survey link through email to its 24 Regional Workforce Boards (RWBs) for subsequent distribution, also via email, to individual caseworkers. Caseworkers completed and returned surveys over a two-week period at the end of 2006.¹ Prioritizing respondent anonymity limits our ability to determine whether all welfare transition program officers passed along the survey as requested, as well as to what extent various regions participated in the survey. We do, however, have regional response data for the subset of caseworkers who elected to identify themselves by region, as presented in Table 1. This table clearly suggests that responses to the survey were clustered in a smaller subset of several of the state’s larger regions, counseling some caution in interpreting the results.

[Table 1]

Nevertheless, we received full or partial survey responses from 144 TANF caseworkers, the vast majority of whom provided responses to our vignettes.² Although the state of Florida was unable to provide a precise number for the size of the overall population of case managers in the WT program,

¹ All caseworkers shared a survey password to ensure anonymous completion and return.

² A total of 137 caseworkers responded to vignette 1 and 131 to vignette 2.

estimates ran from 200 and 250. This would suggest a rough estimate of a response rate somewhere between 58 and 72 percent. Certain items, however, including race (n=98), political party affiliation (n=103) and recent sanctioning behavior (n=108), yielded a larger number of non-responses than most.

Table 2 provides a demographic profile of survey respondents. The majority are women (79 percent), have a minimum of a four-year college degree (59 percent), are married (57 percent), affiliate themselves politically as Democrats or Independent-Democrats (60 percent), and attend religious services at least once a month (58 percent). Of the 98 respondents who identified themselves by a specific race group, 45 percent reported being White or European American, 35 percent reported being Black or African American, and 18 percent described themselves as Latino or Hispanic American.³ They reported an average of 7 years of experience in welfare services, with a range of just beginning (0 completed years) to 26 years. Almost three-quarters of respondents reported having worked in welfare services for more than two years at the time of the survey.

[Table 2]

The analysis presented in this paper is based on two 2x2 random-assignment experiments embedded in the survey, each of which presented case managers with a vignette and asked them to decide whether to impose a sanction in the case as described. 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, some of which are essential for understanding the vignettes below, please see Appendix A.) The experimental design for each vignette makes use of a procedure successfully employed by Bertrand and Mullainathan (2003), who showed that, by randomly assigning African-American-sounding names and white-sounding names to a set of identical resumes, they could significantly influence the rate at which employers in two urban areas contacted a fictitious job-seeker. Adapting this procedure, we randomly assigned the client described in Vignette 1 either a

³ The remaining 2 percent of respondents reported being either Native American (n=1) or Asian American (n=1).

Hispanic-sounding name or a white-sounding name. Similarly, we randomly assigned the client described in Vignette 2 either an African-American-sounding name or a white-sounding name.⁴

The second dimension of our 2x2 experiments manipulates client characteristics that we expect to influence perceptions of client deservingness and that, as we have seen, are associated with different racial/ethnic groups to different degrees. Our selection of these client characteristics is also based on our field interviews, which revealed substantial caseworker attention to these two client “types”: the young mother of multiple children and the repeat welfare recipient who has been sanctioned off the program before. The specific presentations of the two vignettes are presented below, with language related to the experimental manipulations bracketed.⁵

Vignette 1

[Emily O’Brien/Sonya Perez] is a 28 year-old single mother with [one child aged 7 / four children 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, 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 house-cleaning 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

⁴ To guard against confounding effects that might arise from the use of a specific name, we randomly assigned one of three different names for each group in each vignette. White-sounding names included Sarah Walsh, Emily O’Brien, and Meredith McCarthy; African American-sounding names included Lakisha Williams, Aisha Jackson, and Tanisha Johnson; and Hispanic-sounding names included 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, hence, treat all racial name cues as equivalent.

⁵ The two vignettes were presented near the beginning of the survey, in order to minimize the amount of bias potentially introduced by subsequent questions on sanctioning practices. We tested for order effects – specifically, whether sanctioning decisions in Vignette 1 predict sanctioning decisions in Vignette 2, and found no evidence of such effects.

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].

From an analytic standpoint, the two experiments may be seen as representing a more likely and a less likely case for finding effects on sanction decision-making. The group dimension of Vignette 1 identifies the client as white or Hispanic, a group contrast that – as discussed above -- implies a smaller degree of reputational difference than the black-white client contrast used in Vignette 2. By the logic of the RCM, the smaller gap in reputations should be associated with smaller and less consistent racial disparities. The specific client characteristics used in the two vignettes reinforce this difference in relation to the RCM. Vignette 1 focuses on a situation widely reported to be problematic for women moving into employment: child care instability (Loprest 2002; Kalil, Schweingruber, and Seefeldt 2001). In so doing, it cues a strongly negative stereotype of welfare recipients related to sexuality and reproduction, but it 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 information to both cue negative perceptions of the client and produce ambivalence about sanctioning the family. By comparison, the trait manipulated in Vignette 2 provides a more uniformly negative cue. Indeed, by stating that the client has previously drawn a sanction, we simultaneously provide case managers with *two* pieces of information that may cue perceptions of welfare dependency and resistance to achieving self-sufficiency: the client is a repeat recipient who has returned to the program and also has a record of at least one previous failure to comply with welfare-to-work rules.⁶

Third and finally, the two narratives themselves differ in their relation to sanction procedures. The client in Vignette 1 was reported for a week's absence from her community work experience

⁶ Under Florida's welfare transition policy, a record of a sanction two months prior to the current failure should not of itself increase a client's jeopardy of receiving a sanction for the present offense (see Appendix A). However, the previously sanctioned client in the second vignette does face a lengthier penalty for her noncompliance than does her less resistant counterpart (with a minimum of one month for a second sanction as compared to ten days for a first). If caseworkers are concerned about the potentially negative effects of prolonged withdrawals of cash assistance, knowledge of this aspect of policy may well dampen a tendency to sanction based solely on a past record of wrong.

assignment. Although she was not at home when the caseworker attempted phone contact, she responded to the mailed 2290 form well within the ten-day period allotted and reestablished compliance by returning to work the next day. According to both the sanctioning rules and the WT Sanction Flow Chart (see Appendix A), the pre-penalty phase for this client should be ended with compliance following her return to work. In other words, based solely on the description presented in the vignette, one cannot say that the technically appropriate response to Vignette 1 is to sanction the client.⁷ By contrast, even though the client in Vignette 2 responded to the case manager's telephone call, her reason for not complying with her welfare contract clearly fails to meet requirements for a "good cause" exemption (see Appendix A).

Thus, the two vignettes present us with an opportunity to test the RCM in a case where a strongly stereotyped group is associated with a strongly stigmatizing trait and has clearly violated program rules and in a second case where a more moderately stigmatized group is associated with a trait that could arouse stigma or sympathy and has arguably not violated program rules in a way that merits a sanction. Each vignette was followed by a question asking respondents to indicate on a 4-point scale whether they strongly favor, somewhat favor, somewhat oppose, or strongly oppose requesting a sanction for the client and situation described.

Survey-Experimental Analysis: Results

As anticipated, fewer caseworkers recommended sanctioning the client in Vignette 1, 34 percent overall, than recommended sanctioning the client in Vignette 2, 79 percent overall. Even so, it is a striking confirmation of differences in caseworker judgment that a third of caseworkers decided that a sanction was warranted in Vignette 1 *despite* the fact that Florida's welfare transition statutes seem to suggest otherwise, and 22 percent of case managers opted not to sanction the client in Vignette 2 *despite* a clear violation of the rules.

⁷ Although the granting of a "good cause" exemption may be precluded in some cases by a failure to provide evidence of the childcare problem or because the child(ren) are considered too old under Florida's childcare exemption policy, the vignette 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.

Table 3 presents basic summary statistics describing how responses varied across the four conditions of each experiment. These results suggest that the Hispanic client may have been sanctioned more often than the white client in Vignette 1 and that the black client with a prior sanction may have been disproportionately likely to be sanctioned for violating program rules. These simple descriptive results, however, fail to take account of possible differences between groups assigned to each condition of the experiment. To do so, one must include relevant covariates in a larger multivariate analysis.

[Table 3]

Table 4 presents ordered logit models analyzing case managers' sanction decisions in response to each vignette. In addition to dichotomous variables representing three of the four experimental conditions (the white client with a "more deserving" trait serves as the baseline), each model includes measures of the case manager's years of experience, religiosity, education level, partisan identification, marital status, and racial identity. Among these covariates, only one emerges as statistically significant, and it does so consistently across the two models. Case managers with more experience (defined in this case as those with more than two years of welfare services experience) emerge here as less likely to sanction the clients in these vignettes. In Vignette 1, the odds of sanctioning among such caseworkers are estimated to be 79 percent less than the odds for a similar case manager with fewer than two years of experience. In Vignette 2, the odds of sanctioning are estimated to be 80 percent less for more experienced case managers than for less experienced case managers. This finding is intriguing but also difficult to interpret, as it could indicate either the developmental effects of experience on the job or cohort effects associated with case managers who entered earlier versus later in the progression of welfare reform.

[Table 4]

The key findings in Table 4 are indicated by the odds ratios for our experimental conditions. The results here are striking for their consistency, especially given the considerable differences that separate our two vignettes. In the first vignette, 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 (using a one-tailed test for our directional hypothesis, $p=.067$). By contrast, we find strong evidence that a Hispanic woman with multiple children has higher odds of being sanctioned than a white woman with one child. Indeed, the odds ratio for this experimental condition indicates that her odds of being sanctioned are 443 percent greater. Stated in terms of probability, if the white woman with one child had a 50 percent chance of being sanctioned, the probability of the Hispanic woman with multiple children being sanctioned would be 84.5 percent – despite the fact that the two women share an identical case narrative.

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 who has a prior sanction. Caseworker responses to the case narrative 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 odds of a sanction. Relative to a white recipient with no prior sanction, the odds of a black recipient with a prior sanction being sanctioned are 1,245 percent greater. Expressed in the more familiar terms of probability, if the white recipient with no prior sanction had a 50 percent chance of being sanctioned by the case managers in our survey, the black recipient bearing the mark of a prior sanction would have a 93.1 percent chance of being sanctioned.

Because the vignettes hold all factual aspects of the case narrative constant across conditions, and because random assignment and our use of covariates create a presumption against significant respondent differences across the conditions, these results provide important evidence that racial classifications and discrediting markers interact to influence case managers' sanction decisions. Indeed, our faith in these results is bolstered by their consistency across two very different case narratives, two different racial/ethnic contrasts, and two different types of stigmatizing trait attributions. The key limitation of this evidence, however, is that it is based on a hypothetical scenario. When case managers responded to these vignettes, they were not confronted with a real person; they did not have a detailed case file at their disposal; they did not have to worry about implications for their performance numbers; and they did not

have to contemplate any real material hardships that might result from a sanction. To bring our empirical analysis into line with these real-world conditions, we must turn to administrative data generated by case management in the Florida WT program itself. In so doing, 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 as a Basis for Triangulation

The data for our administrative data analyses are provided by the Florida Department of Children and Families (DCF) and consist of individual-level records for all adults who received TANF in Florida between January 2000 and April 2004. Our dataset consists of monthly records for a variety of variables measuring the characteristics of TANF clients and their families. Our basic strategy is to replicate each of the experimental vignettes as closely as possible by estimating an event history model of sanction initiation, where our primary interest lies in estimating the effect of a client's race or ethnicity and its interaction with the stigmatizing factor suggested by each experimental vignette. We begin by examining the joint effects of a client's ethnic identity and family size on sanctioning outcomes in an effort to replicate the hypothesis tested by vignette 1. We then follow this with an analysis inspired by vignette 2, where we estimate the joint effects of a client's race and her sanction history on sanction outcomes for clients who return to TANF for a second spell.

Triangulating Vignette 1: Hispanic-White Disparities in Sanctioning Outcomes

For our first analysis, we constructed a sample by identifying all new adult TANF clients who entered TANF during the 24-month period from January 2001 through December 2002.⁸ In an effort to match Vignette 1 as closely as possible, we then restricted our sample to unmarried, female clients who are either white (non-Hispanic) or Hispanic. The dependent variable for our analysis is a dichotomous

⁸ Our selection of cohorts to observe is limited by two factors. First, we define "new" TANF clients as those clients who have spent at least twelve 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.

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 of the case at the spell's termination or at 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, where we define a spell as continuous months of TANF receipt.⁹ As defined, and accounting for a small percentage of cases for which values of some variables are missing, our total sample size exceeds 11,000 women and nearly 36,000 person-month observations.

We estimate our model using the Cox proportional hazards model. The advantage of the Cox model is that it allows for flexible, nonparametric 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).¹⁰ Our primary interest lies in estimating the effect of a client's ethnicity (measured by the dichotomous variable *Hispanic Client*), and its interaction with a variable measuring the number of children in the TANF family (*Number of Children*). Based on our hypotheses and our results for Vignette 1, we expect the effect of *Hispanic Client* to be positive, indicating that Hispanic clients are more likely to be sanctioned than white clients, and we expect the coefficient for the interaction term (*Hispanic Client* * *Number of Children*) to be positive as well, indicating that disparities between minority and non-minority clients are greatest when minority clients are associated with less deserving characteristics.

Based on past research on sanctions and welfare implementation (Wu et al. 2006; Hasenfeld, Ghose and Larson 2002; Kalil, Seefeldt and Wang 2002; Mancuso and Lindler 2001; Koraleck 2000; Born, Caudill and Cordero 1999), we include a number of independent variables to control for variation in

⁹ 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 have reported 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 them and how much cash aid they will receive in return.

¹⁰ We have replicated our findings using other estimation methods as well, including parametric methods (Weibull), and a discrete-time (logit) model.

clients' individual and community characteristics.¹¹ At the individual level, we include variables measuring a client's citizenship status (*Citizenship Status*), her age (*Age of Client*), the age of the youngest child in the TANF family (*Age of Youngest Child*), and two indicators of human capital (*Income* and *Education*). We also control for a variety of community conditions, including *Local Conservatism*,¹² *Percent Black* and *Percent Hispanic*,¹³ and several measures of employment conditions: the county unemployment rate (*Unemployment Rate*), the county poverty rate (*Poverty Rate*), the level of urbanization as measured by county population (*Population*), and the annual local wage in food service/drinking establishments (*Annual Wage*).¹⁴ Finally, we include a measure of the county *TANF Caseload*, expressed as a proportion of the county population.¹⁵

The results for our event history model are presented in Table 5. Column I in Table 5 presents the results for a model which estimates the additive effects of *Hispanic* and *Number of Children*, while column II presents results for a model that includes an interaction term. For each of the variables in 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 clients who are older and more educated, and who reside in a community with a large Hispanic population. Clients are more likely to be

¹¹ Variable definitions and sources are provided in Appendix Table A1.

¹² Several studies find that local policy implementation is influenced by the local political environment (e.g., Goggin et al. 1990; Weissert 1994; Cho et al. 2005). Based on these studies, we include a measure of local political ideology, which we expect will be positively related to sanction initiation.

¹³ Previous studies have often found that racial context has a significant impact on racially relevant policy outcomes, either through the effects of a "racial threat" felt by the white majority (Blalock 1967; Key 1949), or the effects of increased minority political power (Keech 1968). Because there is reason to suspect that either effect 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 black and Hispanic, respectively.

¹⁴ We expect these variables to affect sanctioning in one of two ways. First, 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 also influence the sanction decisions of case managers, who may be less inclined to sanction clients when job opportunities are less numerous or less attractive.

¹⁵ As the caseload size increases, we might expect that, all else equal, administrative pressures to reduce the caseload would result in an increase in sanctioning. Alternatively, as the 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.

sanctioned if they are U.S. citizens, if they have older children, and if they reside in heavily populated counties. These results are largely consistent with the findings of past studies of sanctioning, including our own (Fording, Soss and Schram 2007; Wu et al. 2006; Keiser, Meuser and Choi 2004; Hasenfeld, Ghose, and Larson 2002; Kalil, Seefedt and Wang 2002; Mancuso and Lindler 2001; Koraleck 2000; Born, Caudill and Cordero 1999).

[Table 5]

Moving to our main hypotheses of interest, we find no evidence in these 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 *Hispanic Client* (column I) or the conditional effect of this variable in association with number of children (column II). Thus, our first analysis of administrative data proves to be inconsistent with the findings for Vignette 1 from our survey of case managers.

Triangulating Vignette 2: Black-White Disparities in Sanctioning Outcomes

We now move to a second 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. However, the narrative presented in Vignette 2 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 who have a prior history of TANF participation. We therefore rely on a sample that consists solely of TANF clients participating in their second spell.¹⁶ Specifically, the sample consists of all TANF clients (unmarried, female, black or white) who entered TANF for the first time between January 2001 and

¹⁶ While our data do 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, with each successive spell we necessarily increase 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 the decision making process of case managers in ways that render the sanction decision less comparable to the decision prompted by vignette 2.

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 a substantial amount of statistical bias, we must control in some way 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 recently introduced by Boehmke, Morey and Shanon (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 outcome model of the selection process – a client’s return to TANF after having exited a first spell.¹⁷ The information garnered from the selection model is then used in the event history model to correct for selection bias. As the estimation procedure is limited to parametric event history models, we therefore choose the Weibull distribution to model duration dependence (i.e., the effect of time on sanction initiation).¹⁸

Our primary hypotheses of interest for this analysis are drawn from our analysis of the responses to Vignette 2. That is, we expect black clients will be sanctioned at a higher rate than white clients. In addition, we expect that the disparity in black-white sanctioning outcomes during the second TANF spell will be greatest among black clients who were sanctioned during their first spell. To test these hypotheses, we divided our sample into four groups which are analogous to the four experimental groups featured in vignette 2. We then created indicator variables for each group as follows:

White & No Prior Sanction: 1= Whites not sanctioned during their first spell, 0=otherwise

¹⁷ We do not present this model in the text, but it is provided as Table A2 in the Appendix for interested readers.

¹⁸ 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.

White & Prior Sanction: 1=Whites sanctioned during their first spell, 0=otherwise.

Black & No Prior Sanction: 1= Blacks not sanctioned during their first spell, 0=otherwise.

Black & Prior Sanction: 1= Blacks sanctioned during their first spell, 0=otherwise.

We include indicators for three of the groups in the model, excluding *White & 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 & Prior Sanction*), will display the largest coefficient. Finally, in addition to these indicator variables, 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*.¹⁹

The results for our event-history model are presented in Table 6. The effects of the control variables largely conform to our expectations and look similar to the first-spell results reported in Table 5. The risk of sanction is highest for younger clients who are less educated, and is lower among clients who reside in counties with relatively large black populations with 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 our sample that included Hispanic clients. Whether it is through increased presence within the local welfare bureaucracy, or through outside pressure on local welfare policymakers, it does appear that a large minority presence within a community may serve to offset any racial/ethnic biases in sanctioning that may occur within the implementation process.

[Table 6]

Moving to the results for our primary hypotheses, we find strong corroboration of our experimental findings. The hazard ratios reported in Table 6 indicate that black clients are significantly more likely to be sanctioned than white clients, regardless of their sanction history. Indeed, if we begin with the baseline risk of sanction for a white client who has no prior sanction, we find that the negative

¹⁹ We do not include *Citizenship Status*, *Number of Children*, *Percent Hispanic* or *Population*, due to the fact that these variables proved to be highly insignificant in preliminary models. Their inclusion in the model does not affect the results reported in Table 6.

effect of making this client black is estimated to be *significantly greater than* the effect of giving the white client a prior sanction. 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 ($p < .05$). And among black clients with a prior sanction, this 14 percent greater risk is doubled. Black clients with a prior sanction are 28 percent more likely to receive a sanction during the second spell than are white clients with no prior sanction ($p < .01$). In sum, even though black clients with no prior sanction are *already* at a higher risk of being sanctioned in the second spell (relative to their white counterparts), the addition of a prior sanction increases the risk of sanction for black clients to a significantly greater degree than for white clients.

Discussion and Conclusion

Surveying contemporary scholarship on race and American politics, one can find progress on many fronts. Yet search as one might, it is hard to find research that explains how and especially *why* race matters for the ways public policies get used to punish or protect social groups in the United States today. Historical studies offer incisive analyses of how race has shaped the development of state institutions and social programs (e.g., Katznelson 2005; Lieberman 1998; 2005), but rarely go beyond the legacies of these developments to examine how race actually operates in the practice of governance today. Political psychologists plumb the micro-foundations that link racial attitudes to policy orientations (e.g., Gilens 1999; Peffley and Hurwitz 1997), but typically do so through laboratory studies and public opinion surveys that leave the direct exercise of state authority untouched. Students of state politics shift the focus onto contemporary governmental action and document racialized patterns of policy action (e.g., Soss et al. 2001; Fording 2003; Fellows and Rowe 2004), but usually sidestep the contentious question of how and why racial factors produce the findings they observe. Econometric studies of welfare sanctioning, which are arguably closest to the focus of our present paper, often find racial differences (e.g., Wu et al. 2006;

Pavetti, Derr and Hesketh 2003), but present these patterns without comment or treat them as uncertain findings possibly generated by unobserved heterogeneity.

In the larger project that provides the backdrop for this paper, we seek to confront more directly the question of how and why race influences the operation of poverty governance and disciplinary practice in the United States today. To do so, we outline a general set of micro-foundations that explains with some precision how race is likely to influence decision making at various points in the policy process. The RCM provides clear, testable predictions about where and when racial disparities in policy design and implementation should emerge in a post-civil-right era in which *de jure* discrimination is banned and racial-egalitarian norms enjoy wide support. In this paper, we have subjected the RCM to a fairly stringent test, triangulating its predictions of welfare-sanctioning patterns from the vantage point of multiple data sources that have different strengths and weaknesses and present different challenges for isolating the predicted relationships. The results converge to provide a striking corroboration of both the RCM and the enduring power of race in poverty governance today.

Analyzing national survey data, we find that mass publics tend to associate the deviant behaviors most central to welfare policy discourse, such as being sexually irresponsible and lazy, with blacks and Hispanics more than with whites. Moving to a multivariate analysis, we find that acceptance of these specific racial stereotypes significantly enhances public support for sanctioning welfare recipients who fall out of compliance with behavioral expectations. Turning to our field research, we find a mode of caseworker decision making that *nominally* is about responding to instances of behavior but *operationally* depends on a process of sizing up types of people to ascertain appropriate courses of action. In this “people processing” mode (Prottas 1979), case managers search for clues about the person that will help them interpret a program misstep, evaluate the story being told, and select the appropriate administrative response. Through heuristic reasoning, little molehills of information – a mode of dress, a social marker, an offhand comment, a past event noted in a case file – are spun into a more mountainous portrait of the person’s motives, intentions, and behavioral profile. Our field research clearly supports the image of frontline decision making sketched by researchers such as Maynard-Moody and Musheno (2003), who

emphasize that street-level workers, in a sense, “profile” the populations they serve, categorize them in relation to standards of deservingness, and use these categories as a basis for applying policy.

This field research component is essential for clarifying how racial classifications may come to inform the allocation of punishment in frontline service provision. Moreover, it helps to shift the spotlight onto case managers, who play a crucial role in sanction decision making but, nevertheless, have typically been studied in ways that provide insights into welfare provision without attending to the operation of discipline (e.g., Riccucci 2005; Lurie 2006). The great limitation of this fieldwork, however, is that it leaves us unable to establish with any precision whether clients from different racial subgroups are actually treated differently in the sanction process. And while our field research allows considerable insight into causal processes (George and Bennett 2005; Lin 1998), it offers little in the way of establishing the existence of causal effects in the relationship between client race/ethnicity and caseworkers’ decisions to impose sanctions (King, Keohane, and Verba 1994). For this purpose, the random-assignment experiments embedded in our survey of case managers offer a superior source of evidence (Kinder and Palfrey 1993).

Using vignettes that varied the race or ethnicity of the client and the attribution of stereotype-consistent traits, we consistently find evidence of the effects predicted by the RCM. In the first vignette, we find that, despite having an identical case narrative, 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 on her record is significantly more likely to be sanctioned for non-compliant behavior than a white client with no prior sanction. The two vignettes are quite different – in the racial/ethnic group involved, in the nature of the stereotype-consistent trait, and in 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. The case narratives elicit a consistent pattern of responses from case managers, regardless of the characteristics attributed, when the client is white. Minority clients, by contrast, enjoy no such immunity. They are vulnerable to the attachment of

discrediting, stereotype-consistent markers: their odds of being sanctioned rise in the presence of such 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, of course, is purchased at some cost. In the survey setting, case manager decision making is pulled out of its 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 somewhat weaker basis for causal inference but reflects actual practices on the ground to a greater degree. The results of this analysis offered no support for our experimental findings regarding the attribution of multiple children to a Hispanic woman. In the administrative data, Hispanic clients do not emerge as 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 again. 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) may appear at first blush to suggest some inconsistency but, viewed in the context of the RCM and our larger project, it actually confirms a repeated pattern. Because the gap between Hispanic and white stereotypes is smaller than the gap between black and white stereotypes, the RCM predicts that disparities will emerge more strongly and consistently in the latter case. Consistent with this prediction, our analyses of state-level policy choices in the TANF program show that the Hispanic percentage of the welfare caseload matters for some disciplinary policies but not others; and the

prevalence and dispersion of Hispanic populations has no effect on decisions to devolve TANF policy control down to local jurisdictions. By contrast, the black percentage of the TANF caseload is strongly and consistently related to the selection of tougher program rules; the distribution of black populations across counties strongly influences the devolution of policy authority; and these choices concatenate to produce dramatic black-white differences in the TANF policy regimes that low-income families confront (Soss, Fording, and Schram 2007). Examining sanction implementation in the state of Florida, we find that participation in a more conservative (as opposed to liberal) county significantly increases the odds of a sanction for both blacks and Hispanics, while having no effect on the odds of a sanction for whites (Fording, Soss, and Schram 2007). By contrast, longer participation spells (a marker consistent with the stereotype of welfare dependency) raises the relative odds of a sanction for blacks to a degree that significantly outstrips the effect for Hispanics (Fording, Soss, and Schram 2007).

Against this backdrop, the findings presented in this paper appear continuous with our previous findings and well-matched to the predictions of the RCM. Here as elsewhere, we find more modest effects for Hispanics than for blacks when both are statistically discernible (as in the case of our survey-experiment) and more variable patterns of relationships for Hispanics than for blacks (as in the contrast between our administrative and experimental findings). Race-based disadvantage, we are finding, attaches most strongly to African Americans in the welfare system today, and it is African Americans who are most vulnerable to the presence of potentially discrediting information. The stigma of deviant behavior attaches most strongly to black clients, somewhat more weakly and less consistently to Hispanic clients, and (in our analyses at least) not at all to white clients. In addition to being consistent with other elements of our larger project, this pattern of race and stigma interacting most strongly for blacks closely resembles the results Devah Pager (2007) reports in her landmark study of how race and the presence of a criminal record interact to undermine African American men's prospects in the labor market.

Sanctioning practices under welfare reform are part of a larger turn toward disciplinary poverty governance in the United States. In this context, it is imperative that students of American politics 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 societal inequalities. TANF, of course, is ostensibly a race-neutral public policy, but it is carried out today in a way that allows pre-existing racial stereotypes and race-based disadvantages to produce large cumulative disadvantages. Relative to their white counterparts, we have found black TANF recipients are more likely to participate in the toughest policy regimes controlled at the most local levels. Within one such regime, Florida, we have found 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. Once such a sanction is imposed, the results in this paper suggest that the prior sanction becomes a discrediting marker that makes black clients even more vulnerable to a future sanction. At each stage, policy choices not only reflect but also *create* the elements that underpin racial inequality in the American welfare system. Under the cover of a policy that is race-neutral on the books, welfare reform is carried out according to a logic of racial classification that reproduces racial inequity and calls out for attention from both scholars and reformers.

Appendix

Sanctioning Procedures in Florida

Within the Florida Welfare Transition program (WT), which many see as a system noteworthy for its emphasis on privatized services and local control (Botsko, Snyder and Leos-Urbel 2001; Fording, Schram and Soss 2006), state-level agencies have made extensive attempts to establish some uniformity in decision-making among local actors. Perhaps nowhere is this attention to consistent implementation so readily apparent as in the relatively “high stakes” practice of delivering sanctions to participants found to be non-compliant with their welfare contracts. 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) set out to clarify those 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 non-compliance with a work contract results in a full-family termination of temporary cash assistance for a minimum of ten days or until the individual reestablishes compliance. Second and third instances of non-compliance 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 do allow for so-called “good cause” exceptions to the sanctioning policies outlined above. This may well represent the point at which the preferences and understandings of individual local actors most clearly come into play (Fording, Schram and Soss 2006; Pavetti, Derr and Hesketh 2003). Such exemptions may be granted for instances of non-compliance related to childcare,²⁰ the current or past effects of domestic 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 in this, as well as in other less clearly defined cases, may be mediated by their relationships with clients, understandings of role, and local political and economic contexts (Fording, Soss and Schram 2007a). 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 non-exempt 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 non-compliance prior to actually imposing such penalties. During this phase of contact, referred to as the *pre-penalty phase*, participants may still avoid a sanction by either establishing good cause for the non-

²⁰ A childcare exemption is reserved for those 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 reasonable 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).

compliance or immediately returning to satisfactory compliance. Particular emphasis is placed throughout this pre-penalty phase on the caseworker's provision of *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 the TANF participant is 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 caseworker. As suggested by interview and narrative response data from the current project, caseworkers interpret the requirements and responsibilities of this relationship in a variety of ways. As we will see in our analysis, what becomes apparent when we examine how caseworkers respond to a set of hypothetical situations is that there is considerable uncertainty as to whose responsibility it is to identify the non-compliance in the first place and whether failure to do so may itself be construed as a sanctionable offense. 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. Thus, in both the interpretation and application of sanctioning rules, caseworkers appear to 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).²¹ Tracing the various paths that lead to a sanctioning decision suggests some of the ways in which individual actors might deviate from the planned course.

Should an individual be found non-compliant 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 simply 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 ten days from the date of the 2290 mailing to establish good cause for the non-compliance. The sanction is to be requested only if both attempts at contact fail over a period of ten 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 either 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 thirty days following the first failure.

Immediate, full-family sanctions are considered appropriate when a participant has either (a) failed to respond to the 2290 and oral attempts at contact, (b) failed to establish good cause and refused to demonstrate satisfactory compliance, or (c) failed to follow her welfare contract without good cause for a second time within thirty 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 the basis for suggesting that sanctioning in Florida is a highly structured process, a close reading of the chart also indicates that substantial opportunities for caseworker discretion remain. How that discretion gets used is the focus of our analysis.

²¹ Source: Agency for Workforce Innovation, Sanction Flow Chart Powerpoint, http://www.floridajobs.org/PDG/training_pres.html, accessed 7/28/07.

Figure A1. Florida Sanction Flow Chart

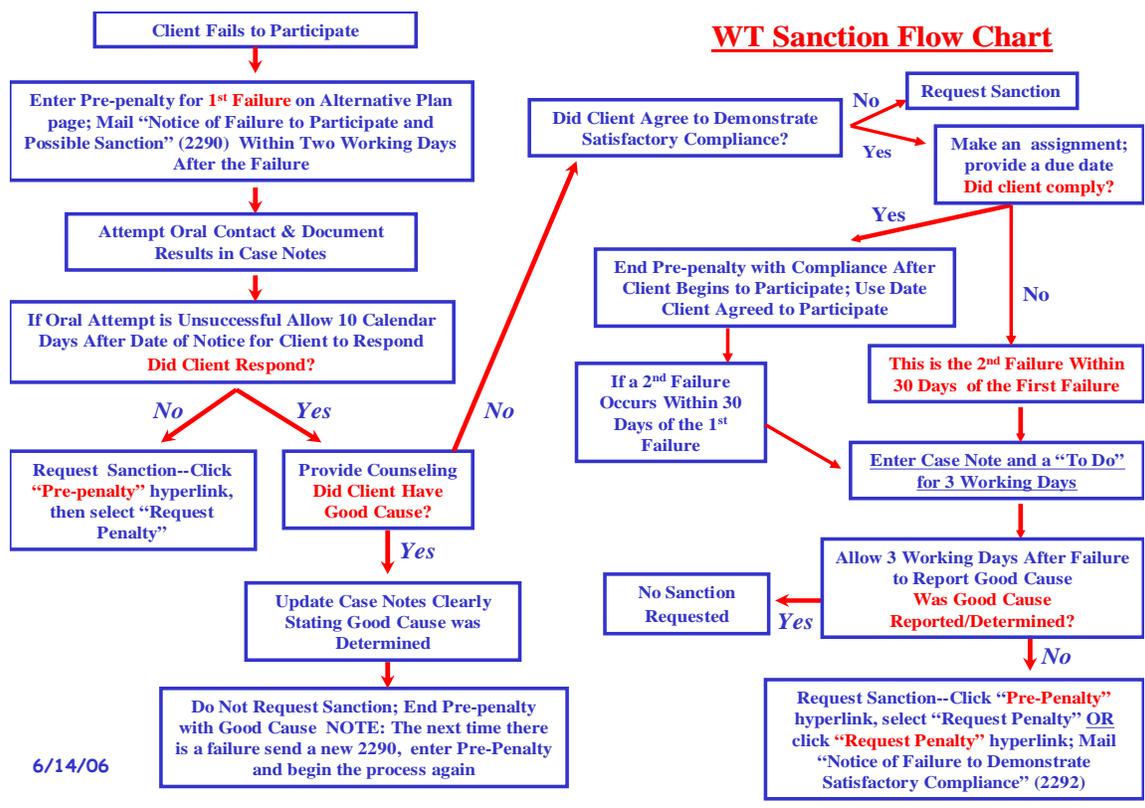


Table A1. Variable Definitions and Sources for Analyses Presented in Tables 5 and 6

Independent Variables	Definition	Minimum- Maximum
<u>Individual characteristics:</u>		
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 (ref. = more than 12 years):		
Less than H.S.	1= more than 12 years, 0 = otherwise	0-1
H.S.	1= 12 years, 0 = otherwise	0-1
Black Client	1 = black, 0 = otherwise	0-1
Hispanic Client	1 = Hispanic, 0 = otherwise	0-1
<u>Political environment:</u>		
County conservatism index	Measure of county political ideology, based on factor analysis of local election results for 18 ideologically-relevant constitutional amendments (see Fording, Soss and Schram 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
<u>Socioeconomic environment:</u>		
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 were provided by the Florida Department of Children and Families. Note. Ref. = reference category; H.S. = high school education; TANF = Temporary Assistance for Needy Families; NAICS = North American Industry Classification System

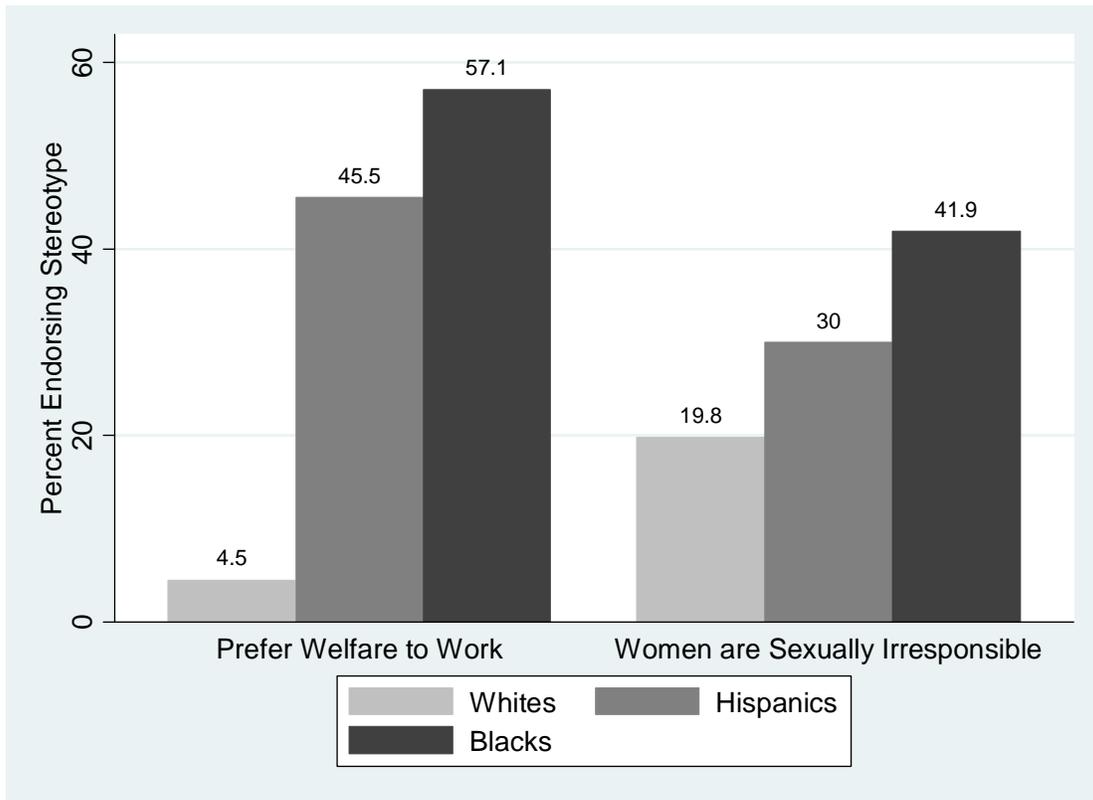
Table A2. Regression Results for First-Stage Selection Equation

Independent Variables	β
<u>Individual Characteristics</u>	
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 (1 st Spell)	.2632**
<u>Community Characteristics</u>	
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 2 nd Spell	18,827

*p<.05, **p<.01

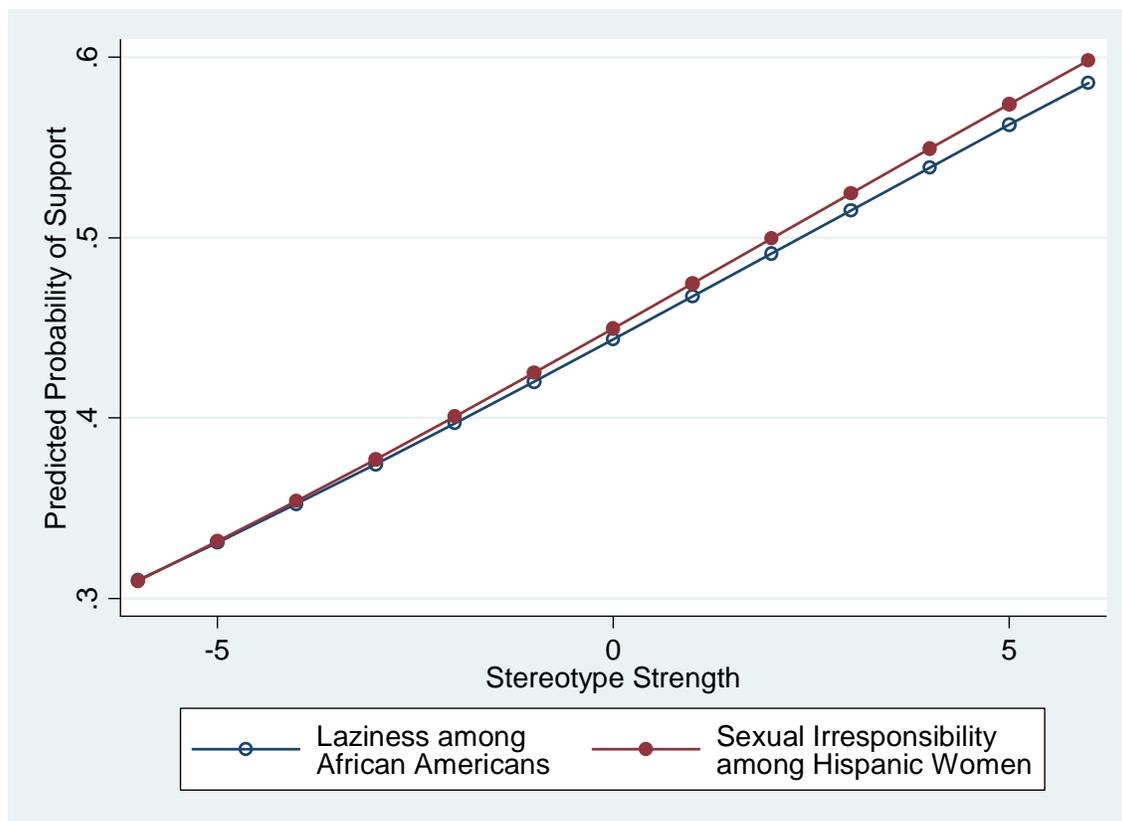
Note: The sample for this analysis consists of all new TANF clients (single-parent, female, Black or white) who entered TANF from January 2001 through December 2002. The dependent variable is a dichotomous variable equal to 1 for clients who returned to TANF for a second spell during our observation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

**Figure 1. National Opinion Data:
Popular Stereotypes of Racial/Ethnic Groups**



Note: Entries represent the percentage of respondents in national surveys who providing responses greater than 4 (neutral) on a 1 to 7 scale for each item. Perceptions of preferences for welfare over work are based on the 1990 General Social Survey, with item-specific sample sizes running from 1,230 to 1,302 respondents. Perceptions of sexual irresponsibility among women are based on the National Survey on Poverty Policy, with item-specific sample sizes running from 968 to 993 respondents (Soss 2002).

Figure 2. Effects of Racial/Ethnic Stereotypes on Popular Support for Welfare Sanctions



Note: Entries represent the predicted probability of a respondent indicating support for the following policy prescription: “If a welfare recipient misses a job assignment or a meeting with an agency official, the welfare agency should reduce her monthly benefits.” Adapting models used by Gilens (1999), the effect of each stereotype was estimated separately with control variables for each respondent’s sex, age, education, family income, commitment to individualism, non-Hispanic white status, ideological identification, and southern. To generate predicted probabilities, all controls were set to their mean values and each stereotype was shifted from its observed minimum value to its observed maximum. For the “laziness model,” $n=764$, $LR \chi^2_{9df}=58.06$, $p<.0001$. For “sexual irresponsibility model,” $n=738$, $LR \chi^2_{9df}=49.74$, $p<.0001$. Data are drawn from the National Survey on Poverty Policy (Soss 2002).

Table 1. Regional Response Rates

Region Name (Number)	Frequency	Percent (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	0.96
Workforce Development Board of the Treasure Coast (20)	1	0.96
Palm Beach County Workforce Development Board (21)	1	0.96
<i>TOTAL</i>	<i>104</i>	<i>100.0</i>

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
More than two years welfare services experience (n=143)	74.1
Mean years of welfare services experience (n=143)	6.97

Table 3. Sanctioning Rates by Caseworker Race and Client Name and Deservingness Condition, Percent (N)

	Client Name and Deservingness Conditions			
	<u>Racial Minority</u>		<u>Non-Hispanic White</u>	
	<u>More Deserving</u>	<u>Less Deserving</u>	<u>More Deserving</u>	<u>Less Deserving</u>
Vignette 1	40.0 (10/25)	40.0 (14/35)	30.3 (10/33)	27.3 (12/44)
Vignette 2	69.0 (20/29)	93.9 (31/33)	76.3 (29/38)	77.4 (24/31)

NOTE: Sanctioning rates include all caseworkers who responded to the sanctioning vignette, regardless of whether or not they answered all items included in our multivariate models. For vignette 1, the racial minority is Hispanic and the less deserving condition is “four children and pregnant” as opposed to one child. For vignette 2, the racial minority is Black and the less deserving condition is possession of a prior WT sanction as opposed to no mention of a prior participation spell at all.

Table 4. Analysis of Vignette Experiments with Caseworker Characteristics as Covariates

<i>Independent Variables</i>	Vignette 1	Vignette 2
Vignette Condition:		
White Client, Less Deserving	1.650 (.60)	1.443 (.51)
Minority Client, More Deserving	3.985* (1.50)	.928 (-.11)
Minority Client, Less Deserving	5.434** (1.96)	13.445*** (2.28)
Caseworker Characteristics:		
Experience	.207*** (-2.73)	.196** (-1.98)
Religiosity	.731 (-.56)	1.018 (.03)
College Education	1.143 (.24)	1.528 (.73)
Democrat	1.763 (.99)	.880 (-.22)
Married	2.467 (1.48)	.859 (-.25)
Black or Hispanic	1.351 (.48)	1.499 (.63)
LR χ^2	16.90**	16.90**
Log likelihood	-46.304	-41.482
Pseudo R ²	.154	.169
N	95	94

* $p \leq .10$; ** $p \leq .05$; and *** $p \leq .01$.

Note: Entries are odds ratios followed by z-scores in parentheses. For vignette 1, the racial minority is Hispanic and the less deserving condition is “four children and pregnant” as opposed to one child. For vignette 2, the racial minority is Black and the less deserving condition is possession of a prior WT sanction as opposed to no mention of a prior participation spell at all.

Table 5. Cox Proportional Hazard Models of Effects of Minority Status and Number of Children on Sanction Initiation

Independent Variables	I	II
<u>Individual Characteristics</u>		
Number of Children	.993	1.001
Hispanic Client	-.923	-.960
Number of Children * Hispanic Client	---	.980
Age of Client	.979**	.979**
Age of Youngest Child	1.018**	1.018**
Citizenship Status (1=citizen)	1.342*	1.345*
Education (reference = >H.S.)		
Less than H.S. Education	1.454**	1.455**
H.S. Education	1.137	1.137*
Income (in 1000s)	1.037*	1.037*
<u>Community Characteristics</u>		
Local Conservatism	1.309	1.308
Percent Black	.994	.994
Percent Hispanic	.924**	.988**
Annual Wage -		
Food Service/Drinking Places	.990	.990
Unemployment Rate _{t-1}	1.014	1.014
Poverty Rate	1.010	1.010
Population (in millions)	1.274**	1.273**
TANF Caseload _{t-1}	.952	.952
Number of Subjects	11,047	11,047
Number of Failures	3,265	3,265
Time at Risk (Person-Months)	35,849	35,849

*p<.05, **p<.01

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 twelve months (clients who exit without being sanctioned, or who were sanctioned after twelve 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).

Table 6. Weibull Selection Model of Effects of Minority Status and Sanction History on Sanction Initiation during Second TANF Spell

Independent Variables	Hazard Ratio
<u>Individual Characteristics</u>	
White Client & No Prior Sanction	[Reference]
White Client & Prior Sanction	1.032
Black Client & No Prior Sanction	1.145*
Black Client & Prior Sanction	1.288**
Age of Client	.968**
Age of Youngest Child	1.012
Education (in years)	.979**
Income (in 1000s)	.976
<u>Community Characteristics</u>	
Local Conservatism	1.028
Percent Black	.992*
Annual Wage - Food Service/Drinking Places	.973
Poverty Rate	1.002
TANF Caseload	.887*
Rho (error correlation)	-.223**
Total N	40,891
Uncensored N (Returning for 2 nd Spell)	18,827

*p<.05, **p<.01

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).

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