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Author(s): Richard C. Fording, Sanford F. Schram, and Joe Soss

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Do Welfare Sanctions Help or Hurt the Poor? Estimating the Causal Effect of Sanctioning on Client Earnings

RICHARD C. FORDING
University of Alabama

SANFORD F. SCHRAM
Hunter College, CUNY

JOE SOSS
University of Minnesota

ABSTRACT This article examines the effect of financial sanctions for noncompliance on the earnings of TANF clients. Current research on TANF sanctioning is descriptive, and few studies estimate the effect of sanctions on client outcomes. To estimate the causal effect of sanctioning, we utilize longitudinal data from Florida and a difference-in-difference propensity-score matching estimator. We compare the growth in earnings of sanctioned clients to a comparable sample of nonsanctioned clients four quarters after exiting TANF and find that sanctioning has a statistically significant negative effect on earnings among TANF clients. The effect is consistent across racial groups, larger among clients with at least 12 years of schooling, and generally increases with the frequency of sanctioning. The finding that sanctioned clients exhibit significantly lower growth in earnings than similar nonsanctioned clients suggests that sanctioning may serve to undermine TANF's goals of reducing welfare use and improving earnings in severely disadvantaged families.

The Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA) fulfilled President Bill Clinton's promise to "end welfare as we know it" (Noble 1998, 127). PRWORA abolished the main cash assistance program for low-income families, Aid to Families with Dependent Children (AFDC), which was used mostly by single mothers and had been in place since the Social Security Act of 1935. In its place, the 1996 law created the Temporary Assistance for Needy Families (TANF) program, a block grant that allows states to use public funds in a variety of ways beyond providing cash assistance. The new TANF program included several key policy mea-

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sures to help achieve the goals of welfare reform, such as time limits, work requirements, and sanctions (i.e., financial penalties) for failure to comply with TANF rules.

Today, there is broad agreement among scholars that welfare reform succeeded in reducing program caseloads. Although studies reach different conclusions regarding the precise effect of TANF compared to other factors such as the economy or the expansion of the Earned Income Tax Credit (EITC), a comprehensive review finds that TANF is responsible for about a 20 percent decline in welfare caseloads since 1996 (Grogger and Karoly 2005). There is little consensus, however, regarding the mechanisms through which TANF reduced welfare caseloads and thus whether TANF has been successful in promoting economically adequate earnings among the poor.

The first wave of studies examining the short-term effects of TANF generally conclude that TANF has a positive effect on the employment and earnings of single mothers, yet the improvement in earnings is much more inconsistent and modest in comparison to the gain in employment (Blank 2002; Grogger and Karoly 2005). More recent studies, which examine the long-term effects of TANF across the income distribution, are less optimistic and suggest that the most disadvantaged poor women are actually worse off since the implementation of TANF, as indicated by poverty rates, average earnings, and other indicators (Ziliak 2009; Shaefer and Edin 2013). Concerns over the possible negative effects of TANF on the well-being of the poor are also heightened by several alarming trends. Between 1994 and 2005, the percentage of TANF-eligible families receiving TANF decreased from 84 percent to only 40 percent (US GAO 2010). This figure is undoubtedly lower in the aftermath of the 2009 recession, due to a combination of increasing poverty rates and lack of growth (and even a continued decline in many states) in the TANF rolls. The number of families living in extreme poverty (less than $2 per person a day) increased sharply from 1.46 million in 1996 to 2.4 million in 2011, and most of the increase was concentrated in families affected by welfare reform (Shaefer and Edin 2013). Finally, there is increasing concern over the growing population of "disconnected women," defined as the percentage of low-income women who report themselves as neither working nor on welfare (Blank and Kovak 2008, 12). According to some estimates, the size of this population doubled between the mid-1990s and mid-2000s and in recent years has stood at 20–25 percent (depending on the precise definition) of all low-income women (Blank and Kovak 2008).

These trends suggest that TANF is doing relatively little to improve the lives of the poor. But could TANF actually be contributing to the worsening condition of the poor? And if so, what specific TANF policies are producing such an effect? Or has TANF helped the poor to become self-sufficient, suggesting that the poor would be even worse off if TANF were not in place? In this article, we seek to address these questions by estimating the effect of financial sanctions for noncompliance on the earnings of TANF clients. Although financial sanctions are one of the most important policies within TANF, most of the research on sanctions is descriptive, and only a handful of studies seek to estimate the effect of sanctions on client outcomes (Meyers et al. 2006). The lack of research on this question is undoubtedly due in large part to the methodological challenges of estimating the causal effect of sanctions. For example, many studies of program effects rely on subjective data (i.e., survey data), yet such data are found to underestimate the degree of sanctioning because many clients do not realize that they are sanctioned (Hasenfeld, Ghose, and Larson 2004). Sanction policies also differ in important ways across states, introducing heterogeneity in national or multistate samples. But perhaps most important, it is now well established that sanctioned clients differ from nonsanctioned clients in ways that are likely correlated with employment outcomes (Meyers et al. 2006). Because there are no random assignment evaluations of sanction policy, researchers must rely on observational data, which are plagued by this inherent selection bias.

Our analysis relies on administrative data from a single state (Florida), which helps to overcome the challenges posed by survey data and policy heterogeneity across states. To estimate the causal effect of sanctioning, we use longitudinal data and a difference-in-difference propensity-score matching (PSM) estimator, which has been successfully used to generate unbiased estimates of the earnings effects of participation in job-training programs (Heckman, Ichimura, and Todd 1997, 1998; Heckman, Ichimura, Smith et al. 1998; Smith and Todd 2005; Mueser, Troske, and Gorislavsky 2007). We treat sanctioned clients as our treatment group and compare their growth in earnings to a comparable sample of nonsanctioned clients four quarters after exiting the first TANF spell.

THE EFFECT OF TANF SANCTIONS: THEORY AND EVIDENCE

Sanctions have long been used by caseworkers to encourage compliance with state welfare rules. Since the implementation of welfare reform, how-

ever, sanctions have come to play a more prominent role in welfare implementation because clients now confront stricter work requirements, narrower exemption criteria, an expanded menu of behaviors subject to sanction, and stronger penalties for noncompliance (Hasenfeld et al. 2004). Indeed, most analysts agree that sanctions are 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 participation in the workforce (TANF).

Federal legislation requires that TANF clients be subject to a reduction in benefits for failure to follow a number of different program rules. The vast majority of sanctions, however, are imposed for work requirements. Under TANF, states have a range of options in determining exactly how benefits should be reduced, the most important of which include whether to reduce the benefit for the adult(s) or the entire family and whether and when to impose a partial or full reduction of benefits. Twenty-one states adopted the strictest combination of these choices, enforcing what are referred to as "immediate full-family sanctions." In these states, the entire TANF family is immediately removed from the TANF rolls at the first instance of noncompliance. An additional 21 states use what are commonly referred to as "gradual full-family sanctions," which remove families from the TANF rolls only after continued noncompliance (usually after a second sanction within a given period of time). The remaining eight states enforce what are known as "partial sanctions," which result in a partial reduction of benefits (usually affecting only the adult portion of the grant; Kassabian et al. 2011).

Welfare experts agree that the implementation of sanctions under TANF has greatly contributed to the historic decline in the welfare caseload since 1997. In one of the first studies to document the full effects of sanctions in the TANF era, Goldberg and Schott (2000) estimate that the number of families who lost benefits from 1997 to 1999 was close to 500,000, or a quarter of the reduction in the TANF caseload during that period. Other studies use state-level panel data and rigorous econometric methods to control for changes in other programs, as well as economic conditions. These studies conclude that states with the strictest sanctioning policies experienced anywhere from 15 to 40 percent greater caseload reduction than states with the least stringent policies (CEA 1999; Rector and Youssef 1999; Mead 2000; MaCurdy, Mancuso, and O'Brien-Strain 2002; Danielson and Klerman 2008).

Several studies also examine the characteristics of sanctioned families, using either surveys of TANF recipients or state administrative data. The findings converge on the conclusion that sanctioned clients tend to exhibit similar demographic characteristics as those of long-term welfare recipients under AFDC (Pavetti, Derr, and Hesketh 2003; Wu et al. 2006). Specifically, these studies find that the probability of being sanctioned is related to a client's race (nonwhite), marital status (single), age (younger), family size (larger), education level (lower) and job experience (less; Born, Caudill, and Cordero 1999; Koralek 2000; Westra and Routely 2000; Mancuso and Lindler 2001; Kalil, Seefeldt, and Wang 2002; Hasenfeld et al. 2004). Together with findings on the overall incidence of sanctioning, these studies suggest that sanctioning practices greatly affect the size and composition of the TANF population.

While it seems clear that sanctions contributed to the decline in welfare caseloads, there is relatively little evidence concerning their effect on employment and earnings. Advocates of what is often called a "get tough approach" to welfare reform argue that sanctions should be expected to improve employment and earnings through their instructional value to the poor. One of the most prominent advocates of this argument is Lawrence Mead (1986), who came out as an early proponent of sanctions in welfare when he noted, "Programs that set no clear standards and possessed no sanctions over their clients could do little to enhance commitment. . . . Clients were free to use the benefits they received wisely or foolishly" (50).

Mead (1998) later makes the argument that the nonworking poor, including single mothers on welfare, are different than other people. When it comes to accepting personal responsibility for their economic circumstances and disciplining themselves to take a job and keep it, Mead argues that government has a primary role in telling the nonworking poor what they need to do. Mead (1997) sees sanctions as central to what he calls the "new paternalism" by teaching the poor that there are economic consequences for failure to comply with obligations associated with work. According to this perspective, if you do not show up, if you fail to complete assigned tasks, or if you do not work a sufficient number of hours, your benefits should be reduced just as would your paycheck, thereby communicating that it is your own personal responsibility to achieve an adequate income. According to this logic, imposing discipline on clients via sanctions educates the client in the lessons of personal responsibility that they evidently had not been able to learn on their own. Given this perspective,

Mead (2005) became an outspoken critic of partial sanctions and a strong proponent of the strictest kind of sanctions—those that reduce the benefits for the entire family at the first instance of an infraction.

Over time, Mead's thinking disseminated throughout the new welfare regime from top to bottom. In 2005, President George W. Bush's Secretary of Health and Human Services Michael Leavitt stated, "The purpose of sanctions is to encourage compliance with work requirements leading to self-sufficiency. A critical benefit of strong work expectations and activities is the ability to acclimate recipients to a working lifestyle—not simply learning how to do a specific job, but to learn through experience what it takes to be employed and remain employed. A weak sanctioning policy could undercut these expectations and do serious damage to a family's prospects to achieve self-sufficiency" (US House of Representatives, Committee on Ways and Means 2005, 33). This perspective on the positive effects of sanctioning can also be seen on the ground. As one welfare-to-work contract agency in Florida explains in its mission statement: "The failure or refusal of individuals to become fully engaged in work activities and alternative plan activity may result in time-limited benefits running out before the family can become economically self-sufficient. As a means of deterring such an outcome, the Welfare Transition Program design calls for strong penalties to be applied to families when customers fail or refuse to participate without good cause" (WorkNet Pinellas 2011, 13). In other words, benefits are sanctioned so that clients can learn to make better decisions faster for their families.

Despite the optimism of sanction advocates, many experts and advocacy groups criticize the tough sanction policies on the grounds that their application ultimately leads to less progress toward adequate earnings among those whose benefits are sanctioned compared to nonsanctioned clients. This possibility is bolstered by a substantial literature in economics, which finds that exogenous, negative income shocks have negative effects on future earnings as well as other measures of family well-being that may indirectly affect parental earnings (Jacobson, LaLonde, and Sullivan 1993; Stevens 1997; Page, Stevens, and Lindo 2009; Lindo 2011). Most of these studies measure negative income shocks by examining job displacement (e.g., layoffs). The logic of this effect arguably extends to the loss of TANF and food stamp benefits, especially in light of evidence that negative income shocks may have more severe negative effects at the lower end of the in-

come distribution (e.g., Oreopoulos, Page, and Stevens 2008; Page et al. 2009).[1]

Research also shows that sanctions are applied to the most disadvantaged clients, those who face major barriers to work that are beyond their control, and that sanctions simply serve to exacerbate their problems. For example, according to Vicki Lens (2006), local county welfare offices and advocacy groups in California cite "illness or a disability (84 percent), followed by a lack of transportation (70 percent) and then child care (42 percent), as the most common reasons why clients were unable to comply with the work rules" (264). Similar responses are given by welfare agencies in other states (e.g., Iowa, New York, Utah; Lens 2006). The problem that this creates is illustrated in the following excerpt from an interview with a sanctioned client (Rainford 2004, 301): "Do you know I would have been off welfare 2 years ago if they weren't so thickheaded about helping me? My son and I wouldn't need their money 'cause I would have . . . a good job. Instead, here we are . . . stuck in poverty. . . . If they lifted the sanction, I could get child care. I could get help with transportation. If they stopped sanctioning me, I could get a job." Do sanctions help clients, as sanctioning advocates would suggest? Or do they impede progress toward adequate earnings, as critics claim?

Unfortunately, there are no random assignment evaluations of sanction policy regarding effects on working and earnings under TANF (Meyers et al. 2006). However, David Greenberg, Andreas Cebulla, and Stacey Bouchet (2005) conducted a meta-analysis of 79 AFDC demonstration projects. They regressed several measures of client outcomes (including earnings) on program characteristics, along with measures of client characteristics and indicators of the local economic context. The authors find that the program sanction rate is positively related to earnings at several points after exit, suggesting that sanctions may actually improve client well-being. Yet, as the authors note, these results are based on AFDC, where sanctions are applied much more selectively and relatively infrequently, and it is therefore unknown if or how they apply to the greater use of sanctions under TANF.

1. The experience of a sanction is also likely to be exogenous for many clients given survey evidence that demonstrates that many sanctioned clients do not understand the rules, and, even when their benefits are sanctioned, they often are unaware that they are in sanction status (Hasenfeld et al. 2004).

Most analyses of the effect of sanctioning rely on research designs that either compare women (or children) in strong-sanction states to women (or children) in weak-sanction states or compare the outcomes of sanctioned clients to those of nonsanctioned clients (either with or without statistical controls). Among the studies that compare across states, two studies find positive effects of sanctioning. Using state panel data, Rebecca Blank and Robert Schoeni (2003) find that the strength of work penalty policies (including both strict sanctions and time limits) is positively related to growth in family income throughout the late 1990s, although the effect seemed to disappear during the post-2000 period. More recently, in their analysis of the anticipated effects of adopting immediate full-family sanctions in California, Caroline Danielson and Debbie Reed (2009) examine pooled cross-sections of Current Population Survey data to estimate the effect of state sanction policies and conclude that "our estimates imply that poverty among children in single-mother families in California would be slightly lower if the state adopted a gradual or immediate grant-elimination sanction policy" (v). One limitation of both studies, of course, is that they do not distinguish welfare users from nonusers, and they do not measure the frequency with which sanctions are applied in the state.

A larger number of studies compare the outcomes of sanctioned clients to nonsanctioned clients to estimate the sanction effect. Many of these studies focus on simple comparisons between sanctioned and nonsanctioned leavers, and nearly all report lower rates of employment, lower earnings, and higher rates of hardship among sanctioned clients (Goldberg and Schott 2000; Westra and Routely 2000; Richardson et al. 2002; Ong and Houston 2003). This is not unexpected, since the probability of being sanctioned is negatively related to several indicators of potential job market success, including education level and job experience (Born et al. 1999; Koralek 2000; Westra and Routely 2000; Mancuso and Lindler 2001; Kalil et al. 2002; Hasenfeld et al. 2004). While this finding is noteworthy in that it underscores that the most disadvantaged are more likely to be sanctioned, it leaves the question of the causal effect of sanctioning on earnings unanswered because of unresolved selection issues.

The most valuable studies of the effects of sanctions are those that control for differences in the characteristics of sanctioned and nonsanctioned clients. Although fewer in number, these studies consistently find that sanctioned clients experience greater hardship and lower earnings upon exit than unsanctioned clients (Cherlin et al. 2002; Kalil et al. 2002;

Lee, Slack, and Lewis 2004; Reichman, Teitler, and Curtis 2005). However, much of this literature can be called into question: since all of these studies are cross-sectional, the sample sizes for the number of sanctioned clients are generally small, and only two studies control for pre-TANF levels of hardship or earnings (Lee et al. 2004; Reichman et al. 2005). The current study provides a more rigorous estimation of the effects of sanctions on earnings by using longitudinal administrative data from the state of Florida.

DATA AND MEASUREMENT

The data for this study come from administrative records of clients in Florida's Welfare Transition (WT) program, supplemented with earnings data from unemployment insurance (UI) records. Although a national sample would be preferable in some ways, relying on administrative data from a single state offers two advantages. First, a single-state sample ensures that all clients in our sample are subject to the same sanction policy, thus minimizing heterogeneity in sanctioning rules and procedures that might confound our results. Second, by using state administrative data to measure the occurrence of a sanction, we can eliminate the measurement error that may be present in (national) survey data due to the fact that many clients do not realize that they are sanctioned (Hasenfeld et al. 2004). The tradeoff, of course, is that we cannot generalize our results to the entire country nor can we estimate the effect of different types of sanction policies (immediate, gradual, full-family, and partial) on client outcomes.

The data set spans the period from January 2000 through September 2003. The choice of this period is ideal in at least two respects. First, it is a period of relative stability in the TANF caseload. By 2000, the massive decline in state caseloads had largely subsided, and although the period was marked by a recession, it was by historical standards relatively brief and mild. Second, the entire analysis period occurred prior to TANF reauthorization, meaning that there were no major policy changes that might confound our results.

While no state can represent all states in a statistical sense, the selection of Florida for our analysis offers many additional advantages. First, Florida's welfare-to-work program (WT) for years had been hailed for its successes and held up as a model to be emulated elsewhere, especially regarding its state-of-the-art information management system, its performance management contracts for enforcing accountability with provider agencies,

and its integration of welfare-to-work into the workforce boards around the state—that is, combining welfare reform from the 1996 law with the employment programming offered under the Workforce Investment Act of 1997 (see Austin 2003; Soss, Fording, and Schram 2011). Second, Florida adopted "some of the strictest time limits and work requirements in the nation" and broadened the pool of clients subject to sanctions by creating "few possibilities for exemptions" (Botsko, Snyder, and Leos-Urbel 2001, 4). The sanctions themselves also fall at the strong end of the spectrum, resulting in an immediate, full-family loss of TANF benefits and a reduction of food stamp benefits to the fullest extent permitted by federal law (Botsko et al. 2001, 6).[2] Moreover, as figure 1 shows, Florida employs sanctions at an extremely high rate compared to other states with full-family sanctions.[3] Between 2000 and 2004, sanction rates in the Florida WT program were fairly stable, with seasonal fluctuations falling around a mean of about 3,200 sanctions per month, more than one-third of all monthly exits in Florida during this period. Thus, given the severity of the sanction penalty, along with the frequency with which it is enforced, if sanctions truly serve to motivate supposedly lazy clients to follow the rules, then there should be evidence of this in Florida. Finally, the selection of Florida also aids our analysis because Florida is one of the most racially diverse states in the country, with sizable black and Latino populations that extend to the TANF population. Between January 2000 and March 2004, 36.2 percent of TANF adults were black, 33.7 percent were white (non-Latino), and 28.5 percent

2. Although it imposes full-family penalties at the first infraction, Florida uses sanction duration as a way to raise penal severity when violations accumulate. First infractions terminate cash aid for at least 10 days; second and third instances result in terminations of at least 1 month and 3 months, respectively. Based on data gathered by the US General Accounting Office, it appears that Florida falls near the middle of the pack for sanction duration: "In 23 states, benefits are restored fully as soon as compliance occurs. In another 21 states, the first sanction continues for 1 month or until families return to compliance, whichever is longer, while the remaining 7 states extend the length of the first sanction for a minimum of 2 or 3 months" (US GAO 2000, 17).

3. Efforts to compare state sanction rates are complicated by differences in the severity of state sanction policies and in the ways that states calculate their sanction rates. The available evidence suggests that Florida's sanction rates fall at the high end of the spectrum. While sanctions account for about one-third of all Florida case closings, federal data indicate that they accounted for only 7 percent of all case closings nationwide in 2002 (US Department of Health and Human Services 2004).

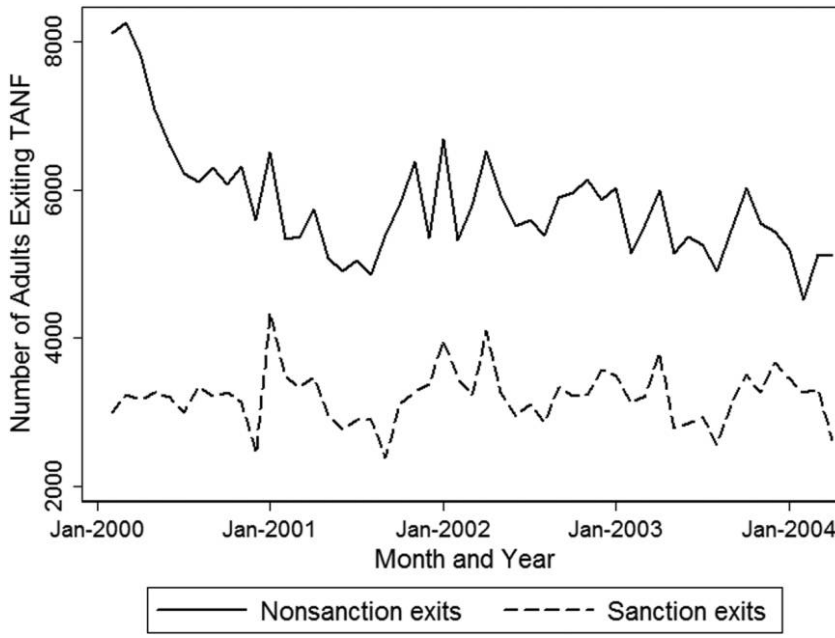

FIGURE 1. Total TANF sanction exits and nonsanction exits in Florida, January 2000–March 2004. Source: WT administrative records, Florida Department of Children and Families.

were Latino. In combination with Florida's large population, we are able to offer the first analysis of differences in the effect of sanctioning across racial and ethnic subgroups.

The data reflect several other advantages over past studies. First, like Joo Lee, Kristen Slack, and Dan Lewis (2004), we use longitudinal data and control for prior earnings. However, unlike their study, we limit our sample to TANF clients who have not used TANF in the preceding four quarters, which arguably improves the validity of past earnings as an indicator of human capital. In addition, we measure earnings at a consistent time lag in relation to the beginning of the TANF spell. This was impossible to achieve in prior studies due to data constraints and resulted in the measurement of past hardship (Reichman et al. 2005) and past earnings (Lee et al. 2004) at time lags that varied by as much as a year or more across clients. Third, we examine the effects of being sanctioned across two groups of clients: those whose benefits were sanctioned just once and those who were sanctioned two times or more. For a variety of reasons, there may be important differences in the effect of sanctioning across these two different groups.

Our sample consists of quarterly data on all unmarried female clients who are less than 50 years of age at the time of their entrance in the WT program and have at least one child less than 19 years of age. We observe all such clients who entered and exited their first TANF spell during the 21-month period between January 2001 and September 2002. Table 1 presents descriptive statistics for each of our three client groups for all variables used in our analysis. Table 1 indicates that of the 36,319 clients who met the criteria for inclusion, 15,768 (43 percent) received one or more sanctions. Of those clients whose benefits were sanctioned, the vast majority (82 percent) were sanctioned only once during the period of analysis. As expected, sanctioned clients exhibited substantially lower earnings than nonsanctioned clients prior to entering TANF. Indeed, nonsanctioned clients earned approximately $600 more than multisanctioned clients and approximately $400 more than once-sanctioned clients, a full year before entering TANF. The table also suggests that there are some additional differences between client groups, but the differences are modest in size for most other variables. Sanctioned clients are slightly less educated, younger, and are slightly more likely to be citizens. The biggest differences across client groups can be seen in their racial and ethnic composition. As many studies confirm, sanctioned clients are more likely to be black or

TABLE 1. Summary Statistics for Sanctioned and Nonsanctioned TANF Clients in Florida

Client Characteristic	Nonsanctioned	Once Sanctioned	Multisanctioned
Avg. earnings (4 quarters prior to entry; $)	1,978.63	1,554.64	1,386.41
Avg. education (years)	10.56	9.72	10.50
Avg. age (years)	30.21	29.43	28.37
Avg. number of children	1.89	1.87	1.99
Avg. age of youngest child	5.23	5.34	4.98
% citizens	90.31	92.37	93.28
% white non-Hispanic	34.68	39.64	30.73
% black	43.18	42.54	54.19
% Hispanic	22.14	17.82	15.07
County poverty rate (2000)	12.65	12.38	12.15
Population (county)	937,154	878,257	929,377
Avg. wage in retail trade sector (annual 2002)	22,618	22,693	22,942
Number of retail trade employees (county 2002, monthly avg.)	53,332	50,416	53,283
Number of retail trade firms (county 2002)	4,119	3,790	4,009
Sample size	20,551	12,895	2,873

Note.—Avg. = average. Our sample includes all unmarried female clients under age 50 with at least one child under age 19 who entered and exited TANF for the first time during the 21-month period between January 2001 and September 2002. All clients are observed during the fourth quarter after exiting the first TANF spell.

Latina. In Florida, we find some evidence of this pattern, but the difference between the sanctioned and nonsanctioned clients appears to be driven by the multisanctioned group, which is disproportionately more likely to consist of black clients.

ESTIMATION AND RESULTS

We begin our analysis of the effects of sanctioning with a graphical analysis of inflation-adjusted client earnings based on a simple difference-in-difference framework. We measure earnings for each client at 10 different points in time, with each observation point defined in relation to the time of TANF entry and exit. Specifically, we measure quarterly earnings for each client during each of the four quarters prior to entering TANF, the quarter of TANF entry, the quarter during the TANF exit (for the nonsanctioned clients) or TANF sanction (for the sanctioned clients), and the four quarters after the TANF exit (or TANF sanction). It is important to note that throughout all of our analyses, clients may be on or off TANF during the post-exit period, including the quarter during which we observe client wages.

The results of our descriptive analyses are presented below in figures 2–4. Each of our graphs defines two different treatment groups. The once-sanctioned group consists of clients who were sanctioned only once, during the first TANF spell. The second treatment group is referred to as multisanctioned and consists of clients whose benefits not only were sanctioned during the first spell but who also were sanctioned at least once more during a subsequent TANF spell during the four-quarter post-exit period. The control group in each case consists of all clients who were never sanctioned at any time between the first TANF spell and the end of the observation period (i.e., four quarters after exit).[4]

Figure 2 reflects results for all clients, regardless of education level or racial or ethnic background. Because these analyses do not control for differences between sanctioned and nonsanctioned clients, the results reveal a clear hierarchy of earnings across the groups during the quarters prior to WT entry. The clients who would never be sanctioned had higher earnings than once-sanctioned clients, who in turn had higher earnings than

4. We omit clients who were not sanctioned during the first spell but who were sanctioned in a subsequent spell.

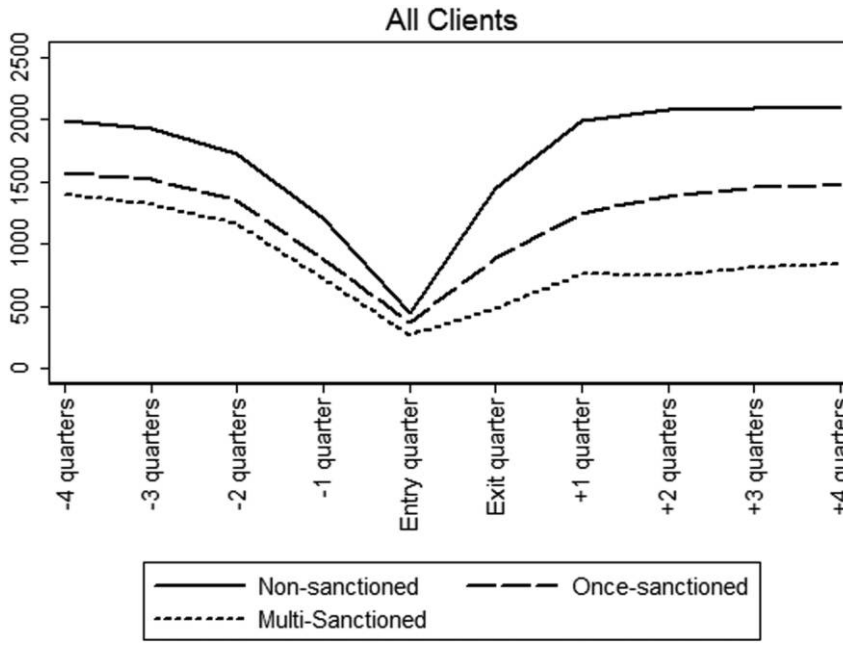

FIGURE 2. Quarterly earnings of TANF clients, by sanction status. Note: this sample includes all unmarried female clients under age 50 with children who entered and exited TANF for the first time during the 21-month period between January 2001 and September 2002.

multisanctioned clients. This same pattern reappears in the post-TANF quarters, but the earnings gaps expand. Prior to WT entry, once-sanctioned clients earned approximately $400 per quarter less than nonsanctioned clients; after exit, this gap rises to over $600 per quarter. And, prior to entry, multisanctioned clients earned approximately $600 per quarter less than clients in the nonsanctioned group; after exit, this gap doubles to more than $1,200 per quarter.

To gauge the substantive meaning of the results in figure 2, we take a closer look at the clients who are most successful in the program, those who are never sanctioned. These clients had very low earnings prior to entering the program, approximately $1,900 per quarter ($633 per month), and experienced an increase of only 5.6 percent in real earnings over a period that spans more than 2 years. By the fourth quarter after exit, the average quarterly earnings for the never-sanctioned group is only $2,104 ($701 per month), a figure that remains far below the poverty line for any family size.

For sanctioned clients, the situation is even more dire, with earnings declining rather than growing between the year prior to entering TANF and the post-exit period. Specifically, once-sanctioned clients experienced a decrease of 6.0 percent, despite the fact that they were never sanctioned again. The erosion of earnings among clients sanctioned more than once is especially dramatic. By the fourth quarter after exit, these clients earned an average of $841 ($280 per month). This represents an average decrease in real earnings of 39 percent when compared to the fourth quarter prior to entering WT.

We replicated this analysis by racial or ethnic group and by education level. Preliminary analysis suggested that sanctioning may affect the earnings of clients with higher levels of education more than others. This seems plausible, since clients with higher levels of human capital, and thus higher earnings potential, might experience income losses if the disruption in household income due to sanctioned benefits impedes their ability to work, for example, by making transportation and child care unaffordable. It also seems plausible that the effect of sanctions on earnings might differ by race. Therefore, we divided the sample into groups, based on race or ethnicity (fig. 3) and on education level (less than high school degree and high school or more; fig. 4). The results reveal remarkably similar patterns when comparing the associations between sanctioning and earnings for each group. In each graph, the gap in earnings between the two sanctioned groups and the nonsanctioned group increases from the pre-TANF period to the post-exit period. The relationships between sanctioning and earnings seem to be somewhat stronger for Latina clients compared to black and white clients, although the differences are not large. A somewhat bigger difference can be seen when comparing the results across education levels, with a stronger negative relationship between sanctioning and earnings among better-educated clients than less educated clients. Specifically, among clients with less than 12 years of education, the earnings growth of nonsanctioned clients (fourth quarter pre-TANF to fourth quarter post exit) exceeded that of once-sanctioned and multisanctioned groups by $43 and $463, respectively. In contrast, among clients with at least 12 years of education, the equivalent figures are $367 and $881, respectively. Given this evidence of heterogeneity in the associations between sanctions and earnings, we conduct separate analyses of sanction effects by combinations of racial/ethnic identity and educational level in our matching analyses below.

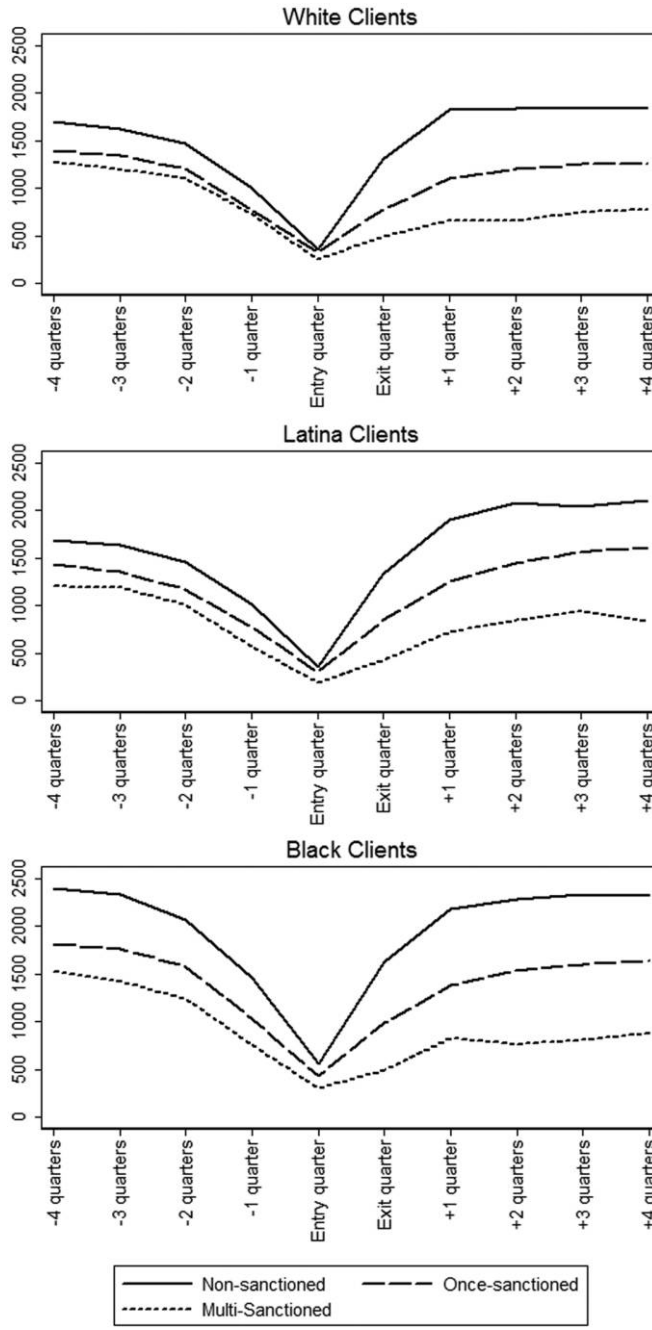

FIGURE 3. Quarterly earnings of TANF clients by sanction status and race. Note: the samples used to produce these graphs include all unmarried female clients under age 50 with children who entered and exited TANF for the first time during the 21-month period between January 2001 and September 2002.

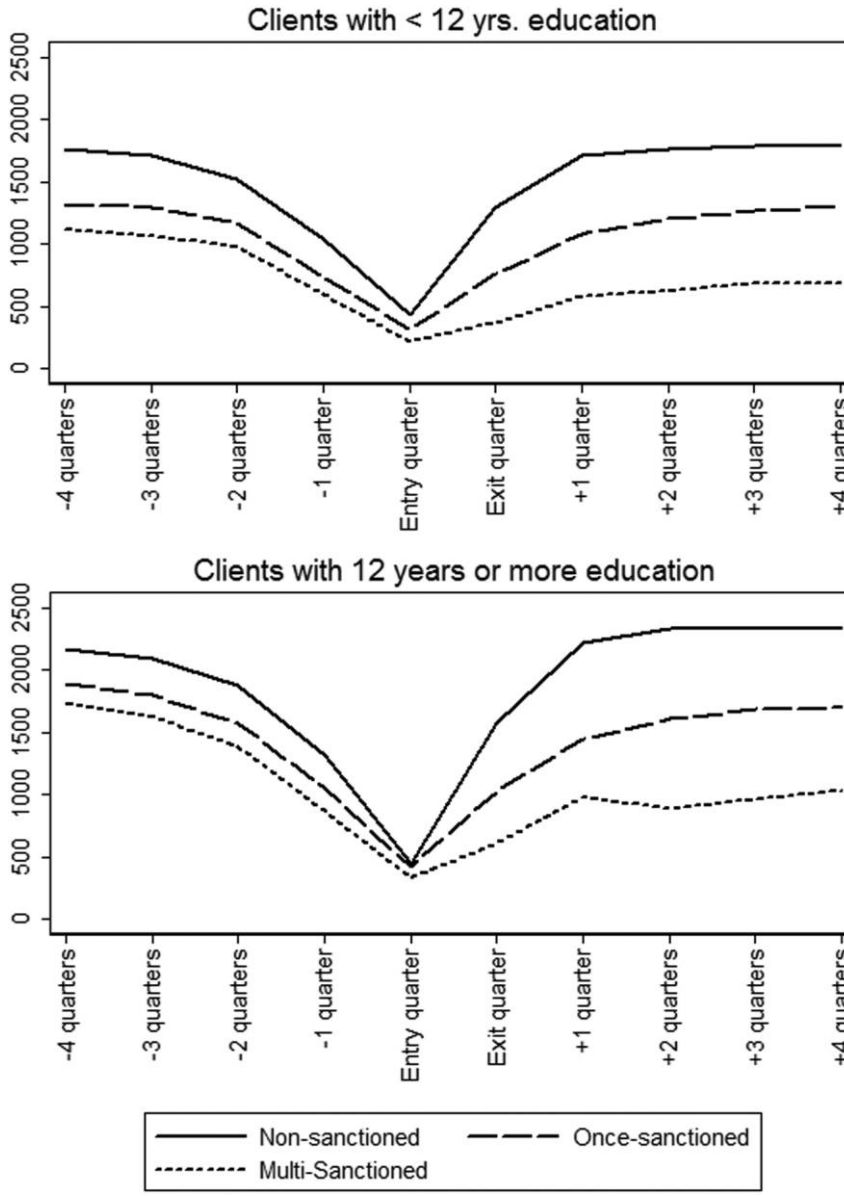

FIGURE 4. Quarterly earnings of TANF clients by sanction status and education level. Note: the samples used to produce these graphs include all unmarried female clients under age 50 with children who entered and exited TANF for the first time during the 21-month period between January 2001 and September 2002.

PROPENSITY-SCORE MATCHING ANALYSIS

The primary weakness of the graphical analyses presented in figures 2–4 is that the assignment of clients to the treatment (sanctioned) and control (nonsanctioned) groups is not random, and therefore the estimation of the sanction effect may be biased due to the existence of confounding factors. Past studies of sanction outcomes either fail to control for such differences or use traditional multivariate methods (i.e., ordinary least square [OLS] or logit/probit) to control for differences between sanctioned and nonsanctioned clients. In contrast to all of these prior studies, our strategy for dealing with the endogenous selection of clients into the sanctioned and nonsanctioned groups is to use a difference-in-difference PSM estimator, which has been shown to produce unbiased estimates of the effect of participation in job-training programs on earnings (Heckman et al. 1997, 1998; Heckman, Ichimura, Smith et al. 1998; Smith and Todd 2005; Mueser et al. 2007).

Propensity-score matching is based on the idea that bias is reduced when the estimation of the treatment effect is made using treated and control subjects who are as similar as possible (Rosenbaum and Rubin 1983). Since matching subjects on more than just a few characteristics is usually not possible due to sample size limitations, this method relies on a single summary measure of pretreatment characteristics of each subject (the propensity score), which makes matching feasible. Although there are several matching strategies to choose from to construct the counterfactual comparison group for treated individuals, we rely on a kernel matching method introduced by James Heckman, Hidehiko Ichimura, and Petra Todd (1997, 1998). In the kernel matching method, rather than matching each treated individual with a single counterfactual case in the control group, each treated individual is matched with a weighted average of all control group subjects, with weights that are inversely proportional to the distance between the propensity scores of treated and control group subjects. This method is often preferred because it relies on more information from the sample.

For each client group, the outcome variable is the change in earnings, measured between the fourth quarter prior to TANF entry and the fourth quarter after either exiting TANF or being sanctioned. Conceptually speaking, our parameter of interest is the difference in earnings growth between the treatment group and the otherwise similar, counterfactual control group—the average treatment effect on the treated (ATT), which is pre-

cisely what PSM is designed to estimate. The analysis proceeds in several steps. First, we estimate a propensity score for treated (sanctioned) and untreated (nonsanctioned) clients by estimating a probit regression of the probability of receiving a sanction (or for the multisanctioned group, multiple sanctions). The independent variables included in this regression are quarterly client earnings for each of the four quarters prior to TANF entry, client age, the number of children in the TANF unit, the age of the youngest child in the TANF unit, client education (in years), and several variables measuring local economic and labor market conditions (county poverty rate, county population, and three characteristics of the county's retail-trade sector—average annual earnings per employee, the number of firms, and the average number of employees per firm).[5] We conducted separate analyses for whites, blacks, and Latinas, and for each racial group we conducted the analysis separately by education level (less than high school, high school, or greater). Separating the sample into these subgroups not only allows us to estimate how the relationship between sanctions and earnings growth varies across race and education level but also creates even greater homogeneity within each sample.

PSM only allows us to reduce, but not to eliminate, the bias generated by unobservable confounding factors. Bias is eliminated only when exposure to treatment is purely random among individuals who have the same value for the propensity score. This is known as the "conditional independence assumption." The extent to which this assumption is met depends on the quality of the control variables used to construct the propensity score. Research on the earnings effects of government-sponsored training programs finds that this assumption is more likely to be met when outcome variables are measured in the same way for both participants and nonparticipants, members of the treatment and comparison groups are drawn from the same local labor markets, and the data include variables measuring an individual's labor force status prior to enrollment (Heckman, LaLonde, and Smith 1999). Our data generally meet these conditions. In particular, an important strength of our analysis is our ability to control for four quarters of pre-TANF earnings. Although we are unable to match sanctioned and nonsanc-

5. We use data from the retail sector as a proxy for the low-wage labor market more generally. Although the low-income population is not exclusively employed in this sector, it is much larger than any other sector that would be a suitable alternative (e.g., accommodation and food services).

tioned clients from the same local labor markets, our propensity-score models include control variables that measure several important characteristics of the local economy.

The results of the probit models used to generate the propensity scores are presented in tables 2 and 3. Table 2 presents results of analyses comparing once-sanctioned clients to nonsanctioned clients, while table 3 presents the results comparing multisanctioned clients to nonsanctioned clients. For each client subgroup analysis, we began with a simple model specification that included no squared terms or interaction terms. If the propensity scores generated from the model did not satisfy the balancing property, which ensures similarity in the mean of all selection variables across treated and untreated clients, then we added squared and interactive terms to the model in an iterative fashion until the balancing property was satisfied. Ultimately, we were able to satisfy the balancing property for each of the 12 models presented in tables 2 and 3.[6]

Table 4 presents our estimates of the sanction effect based on our PSM estimator for each of the 12 client subgroups defined by race/ethnicity, education level, and treatment group status (never sanctioned, once sanctioned, or multisanctioned). The results suggest that being sanctioned has a statistically significant negative effect on earnings for each of the 12 client groups. Variation in the magnitude of the estimates is presented graphically in figure 5. Each point represented in the figure reflects a PSM point estimate, along with a 95 percent confidence interval, for the effect of sanctioning. It is worth reiterating that the treatment and control groups are successfully matched based on pre-TANF earnings (for each of the four

6. In addition to the conditional independence assumption, successful implementation of PSM also requires satisfying the common support assumption, or what is also known as the overlap condition. Basically, this requires that we observe treatment and control subjects that are very similar on all characteristics, discarding observations outside the region of common support (where we are unable to estimate the ATT). If there are too few observations within the region of common support, then PSM analysis is compromised or even impossible. Consequently, an important step prior to PSM estimation is to check the region of common support between the treatment and comparison groups. The most common and straightforward test is a simple visual analysis of the density distribution of the propensity score in both groups. Appendix figures A1 and A2 present these graphs for each of the 12 client subgroups used in our analysis. As can be seen, the propensity-score distributions for sanctioned and nonsanctioned clients are indeed very similar, suggesting that the common support assumption should pose no problems for our analysis.

TABLE 2. Probit Estimates of the Determinants of Sanction Status (Nonsanctioned vs. Once-Sanctioned) of TANF Clients

Independent Variable	White (12 Yrs. or More)	Black (12 Yrs. or More)	Latina (12 Yrs. or More)	White (Less Than 12 Yrs.)	Black (Less Than 12 Yrs.)	Latina (Less Than 12 Yrs.)
Education (Yrs.)	−.036**	1.365	−.035**	−.026**	−.008*	−.013**
Education^2 (Yrs.)	. . .	−.089	. . .	. . .	. . .	. . .
Education^3 (Yrs.)	. . .	.002*	. . .	. . .	. . .	. . .
Age	−.006*	−.013**	−.008	−.014**	−.018**	−.018**
Age^2	. . .	. . .	. . .	. . .	. . .	. . .
Earnings$_{t-4}$	−.012	−.019*	.034*	−.026*	−.037**	−.029
Earnings$^2_{t-4}$	. . .	. . .	. . .	. . .	. . .	. . .
Earnings$_{t-3}$	.011	−.048**	−.018	−.018	−.015	−.006
Earnings$^2_{t-3}$	. . .	.000**	. . .	. . .	. . .	. . .
Earnings$_{t-2}$	−.028*	.003	−.021	.011	−.007	.000
Earnings$_{t-1}$	−.001	−.066**	−.031	−.066**	−.040**	−.029
Earnings$^2_{t-1}$	. . .	.000	. . .	. . .	. . .	. . .
Earnings$^3_{t-1}$	. . .	.000	. . .	. . .	. . .	. . .
Age of youngest child	.024**	.059**	.009	.023**	.025**	.015*
Age of youngest child^2	. . .	−.002**	. . .	. . .	. . .	. . .
Number of children	.080	. . .	.001	.404**	.095	.328
Number of children^2	. . .	. . .	. . .	. . .	. . .	. . .
Poverty rate	.016*	−.001	.012	.010	.003	.011
Population	.003**	.004**	.001	.003	.003**	.002*
Average wage	.000	.000	.083*	.024**	.065**	.014
Employees per firm	.000	−.015**	−.017*	−.015**	−.026**	−.007
Number of firms	−.700**	−.597**	−.122	−.566**	−.350**	−.309**
N	6,495	7,548	3,274	5,744	6,812	3,573
χ^2	166.11**	237.74**	54.29**	202.37**	230.40**	86.85**

Note.—Yrs. = years. Cell entries are coefficients generated from probit analyses of the determinants of receiving a sanction. Earnings, TANF caseload, and county population are measured in 1,000s.

* $p = < .05$.

** $p = < .01$.

quarters preceding entry), years of education, client age, the number of children in the TANF unit, the age of the youngest child in the TANF unit, and measures of local labor market conditions.

As shown in figure 5, the estimated effect of sanctioning is uniformly negative, meaning that sanctioned clients experience statistically significantly lower earnings than never-sanctioned clients as much as a year after receiving the sanction. In addition, each of these effects is highly statistically significant; the upper bound of the 95 percent confidence interval lies far below zero for every effect reflected in the figure. The results also reveal an interesting pattern of effects across education levels and treatment group status. As expected, the estimated negative effect of sanctioning increases as we move from the sample of clients with less than a high school educa-

TABLE 3. Probit Estimates of the Determinants of Sanction Status (Nonsanctioned vs. Multisanctioned) of TANF Clients

Independent Variable	White (12 Yrs. or More)	Black (12 Yrs. or More)	Latina (12 Yrs. or More)	White (Less Than 12 Yrs.)	Black (Less Than 12 Yrs.)	Latina (Less Than 12 Yrs.)
Education (yrs.)	.010	−.023	−.012	.018*	.042**	.025**
Education^2 (yrs.)	. . .	. . .	. . .	. . .	. . .	. . .
Education^3 (yrs.)	. . .	. . .	. . .	. . .	. . .	. . .
Age	−.016**	−.025**	−.017*	−.024**	−.040	−.023**
Age^2	. . .	. . .	. . .	. . .	.000	. . .
Earnings $_{t-4}$	−.004	−.003	−.013	−.031	−.112**	−.054*
Earnings$^2_{t-4}$	. . .	. . .	. . .	. . .	.000*	. . .
Earnings$_{t-3}$	−.006	−.012	−.009	−.033	−.041*	−.003
Earnings$^2_{t-3}$	. . .	. . .	.000	. . .	. . .	. . .
Earnings$_{t-2}$	−.026	−.040*	−.004	.006	.014	.033
Earnings$_{t-1}$	−.036	−.040*	−.073*	−.021	−.105**	−.089*
Earnings$^2_{t-1}$	. . .	. . .	. . .	. . .	. . .	. . .
Earnings$^3_{t-1}$	. . .	. . .	. . .	. . .	. . .	. . .
Age of youngest child	.031**	.028**	.014	.041**	1.028*	.016
Age of youngest child^2	. . .	. . .	. . .	. . .	. . .	. . .
Age of youngest child × education	. . .	. . .	. . .	. . .	−.002	. . .
Age of youngest child × number of children	. . .	. . .	. . .	. . .	−.993*	. . .
Number of children	−.068	−.275*	−.115	−.259	.435	−.196
Number of children^2	. . .	. . .	. . .	. . .	. . .	. . .
Poverty rate	−.008	−.029*	−.041	.015	.004	−.026
Population	.004**	.003**	.001	.005**	−.004	.002
Population^2	. . .	. . .	. . .	. . .	.000**	. . .
Average wage	.040*	.055*	−.084	.031	−1.088	−.001
Average wage^2	. . .	. . .	. . .	. . .	.000	. . .
Average wage^3	. . .	. . .	. . .	. . .	.000	. . .
Employees per firm	−.022*	−.029**	.006	−.030**	.000	−.014
Employees per firm^2	. . .	. . .	. . .	. . .	.000**	. . .
Number of firms	−.631**	−.402**	−.194	−.842**	.891**	−.257
Number of firms^2	. . .	. . .	. . .	. . .	.000**	. . .
N	4,617	5,658	2,536	3,393	4,774	2,446
χ^2	85.49**	176.40**	25.90*	119.41**	394.40**	51.62**

Note.—Yrs. = years. Cell entries are coefficient estimates generated from probit analyses of the determinants of receiving a sanction. Earnings, TANF caseload, and county population are measured in 1,000s.

* $p = < .05$.

** $p = < .01$.

tion to clients with 12 or more years of education. This pattern is reflected for both treatment groups (once sanctioned as well as multisanctioned). Finally, the estimated effect of sanctioning is considerably larger for clients in the multisanctioned group, compared to the once-sanctioned group. Based on these results, the largest effect is therefore experienced by multisanctioned clients with 12 or more years of education. The results suggest that on average, such clients earned approximately $940 dollars less per

TABLE 4. Propensity-Score Matching Estimates of the Effect of Sanctioning on Earnings by Race, Education Level, and Sanctioning Frequency

Client/Yrs. of Schooling	PSM Estimate	Unconditional Difference-in-Difference	OLS Difference-in-Difference	*N* Treatment	*N* Control
Nonsanctioned vs. once sanctioned:					
White, less than 12 yrs.	−232.14**	−31.00	−251.50**	2,797	2,942
White, 12 yrs. or more	−527.63**	−473.71**	−565.89**	2,314	4,180
Black, less than 12 yrs.	−275.94**	−22.93	−299.76**	2,927	3,885
Black, 12 yrs. or more	−481.85**	−194.25*	−560.27**	2,558	4,986
Latina, less than 12 yrs.	−282.75**	−39.87	−292.67**	1,365	2,200
Latina, 12 yrs. or more	−509.92**	−462.87**	−448.97**	933	2,326
Nonsanctioned vs. multisanctioned:					
White, less than 12 yrs.	−507.10**	−315.44**	−598.11**	447	2,892
White, 12 yrs. or more	−955.66**	−868.19**	−1,093.75**	436	4,168
Black, less than 12 yrs.	−783.42**	−401.98**	−825.03**	889	3,796
Black, 12 yrs. or more	−973.66**	−750.76**	−1,170.31**	668	4,952
Latina, less than 12 yrs.	−719.53**	−496.42**	−909.29**	238	2,119
Latina, 12 yrs. or more	−889.41**	−794.39**	−1,055.59**	195	2,308

Note.—The propensity-score matching (PSM) analysis was conducted using kernel density matching in Stata 12.0 (attk command), with the bootstrap and common support options. OLS = ordinary least squares.

* $p = < .05$.

** $p = < .01$.

quarter than otherwise similar clients whose benefits were not sanctioned. Our results generally do not vary substantially across racial/ethnic groups. The one exception to this generalization occurs within the sample of white clients: the effect of receiving more than one sanction is somewhat less among white clients than it is among black and Latina clients.

In table 4 we also present estimates of the sanction effects generated by a simple difference-in-difference analysis that does not control for any preexisting differences between client groups and OLS estimates that include the same control variables as the PSM analysis. The unconditional difference-in-difference estimates generally underestimate the magnitude of the sanction effect (compared to our PSM results). These results are especially misleading for once-sanctioned clients with less than 12 years of education, as they indicate that sanctioning did not have statistically significant effects for any of the three racial subgroups. In contrast, the OLS difference-in-difference estimates (with controls) consistently produce larger effects than our matching estimates. When estimating the effect of being sanctioned once, the OLS estimates are very similar to those produced by PSM; although OLS estimates are larger than those produced through

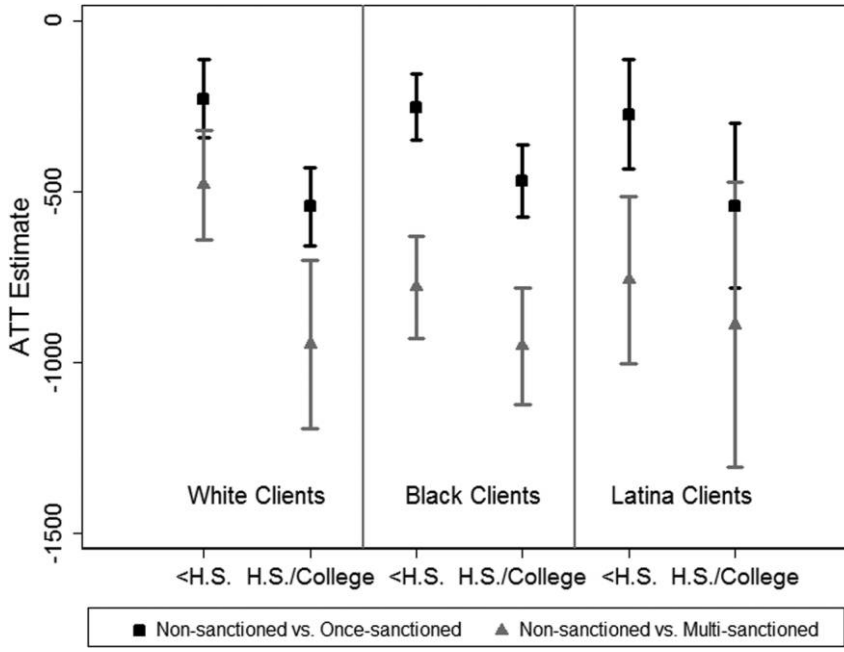

FIGURE 5. Propensity-score matching estimates of the effect of sanctioning on earnings, by race, education level, and sanction frequency. Note: each point plotted in the graph represents the difference in earnings growth (fourth quarter post-exit–fourth quarter pre-entry) between the sanctioned group and the nonsanctioned group, based on the propensity-score matching analysis reported in table 3. The bands surrounding each point estimate represent a 95 percent confidence interval. The samples for these graphs include all unmarried female clients under age 50 with at least one child under age 19 who entered and exited TANF for the first time during the 21-month period between January 2001 and September 2002.

PSM estimates, the differences are not large. However, the difference in OLS and PSM estimates for the multisanctioned analyses are much larger; OLS estimates exceed PSM estimates by an average of 17 percent.

ROBUSTNESS TESTS

The results in table 4 provide strong evidence of the negative effects that sanctions have on earnings, yet our confidence in these results would be enhanced if the results hold across alternative models that reflect different choices regarding sample selection and model specification. We report the results from three such robustness checks below. First, we address the possibility of endogeneity bias caused by the exclusion of a relatively small

percentage of long-term TANF clients. The results in table 4 are based on a sample of clients who entered and exited (either willingly or by sanction) their first TANF spell during the 21-month period between January 2001 and September 2002. Our rationale for this decision was to maximize our sample size, as the last quarter for which we have wage data is September 2003, and we wish to examine wages 1 year after exiting the first TANF spell. However, by defining the sample in this way, we were forced to exclude clients who entered TANF between January 2001 and September 2002 but did not complete their first spell by September 2002. This decision resulted in the exclusion of 19 percent of all clients who entered TANF during the January 2001–September 2002 time period. Therefore, while drawing the sample this way increases its size, it also risks overrepresentation of short-term recipients, making it possible that this biased our results if these long-term recipients differ from our estimation sample of clients in some unobservable way.

To correct for this possibility, we replicated all 12 models of client wage growth by restricting the sample to clients who entered TANF for their first spell no later than the end of the 2001 calendar year but still exited their first spell by the end of the third quarter (September) of 2002. Although this constraint results in a reduction in sample size by approximately one-third, we can be assured that the results are not affected by the overrepresentation of short-term clients, since our rate of excluding clients falls to a mere 2 percent (compared to the 19 percent exclusion rate for the original sample).[7]

The PSM estimates of the effect of sanctioning for this modified sample are presented in column 3 of table 5. Compared to the results for the full sample (see col. 1 of table 5), the estimates based on the modified sample are remarkably similar. The two sets of estimates differ by 10 percent or less in every case, and perhaps more importantly, the estimates based on the modified sample are just as likely to be larger than the original estimates as they are to be smaller. To some degree, the similarity of the results is likely due to the fact that our dependent variable is measured as the change in earnings (rather than the earnings level). But this result also suggests that the variables used to match clients (especially pre-TANF earnings) are effective in controlling for relevant client differences that we cannot directly observe.

7. We define a spell as months of consecutive TANF participation. The mean spell length in our data is 3.26 months.

TABLE 5. Alternative Propensity-Score Matching Estimates of the Effect of Sanctioning on Earnings, by Race, Education Level, and Sanctioning Frequency

Client/Yrs. of Schooling	Full Sample (Table 4)	Entered TANF 1/2001–12/2001	*N* Treatment	*N* Control	Clients with No Earnings Excluded	*N* Treatment	*N* Control	Control for TANF Participation	*N* Treatment	*N* Control
Once-sanctioned vs. nonsanctioned:										
White, less than 12 yrs.	−232.14**	−223.52**	1,988	2,097	−225.08**	1,702	2,049	−235.29**	2,798	2,943
White, 12 yrs. or more	−527.72**	−563.10**	1,484	2,828	−614.10**	1,602	3,141	−529.94**	2,314	4,180
Black, less than 12 yrs.	−275.94**	−250.49**	2,111	2,819	−250.45**	2,090	3,144	−271.03**	2,927	3,885
Black, 12 yrs. or more	−482.95**	−468.64**	1,728	3,380	−508.06**	2,031	4,309	−469.35**	2,558	4,986
Latina, less than 12 yrs.	−282.75**	−251.07**	1,009	1,613	−335.24**	819	1,436	−272.59**	1,365	2,199
Latina, 12 yrs. or more	−509.92**	−465.66**	635	1,598	−611.45**	624	1,670	−487.20**	933	2,322
Multisanctioned vs. nonsanctioned:										
White, less than 12 yrs.	−507.10**	−541.61**	301	2,064	−711.70**	280	2,031	−342.76**	447	2,898
White, 12 yrs. or more	−955.66**	−922.75**	283	2,826	−1,302.13**	295	3,105	−701.26**	436	4,045
Black, less than 12 yrs.	−783.42**	−842.22**	610	2,724	−1,031.04**	609	2,898	−563.64**	889	3,723
Black, 12 yrs. or more	−973.66**	−1,001.00**	445	3,356	−1,197.47**	522	4,258	−752.47**	668	4,949
Latina, less than 12 yrs.	−719.53**	−835.55**	169	1,569	−1,079.79**	130	1,402	−584.88**	238	2,146
Latina, 12 yrs. or more	−889.41**	−828.86**	140	1,557	−1,259.66**	117	1,622	−556.88**	195	2,317

Note.—This analysis replicates the analysis reported in table 4, but the sample is restricted to clients who entered TANF for a first spell during calendar year 2001 and exited by September 2002. The propensity-score matching analysis was conducted using kernel density matching in Stata 12.0 (attk command), with the bootstrap and common support options.

** $p = < .01$.

As a second robustness check, we replicated our PSM analyses after dropping all clients with earnings of zero in both the pre-TANF and post-exit quarters. Such clients comprise approximately one quarter of our original estimation sample, and therefore their inclusion may bias our estimates of the sanction effect.[8] These results are reported in the sixth column of results in table 5. After dropping these clients from our sample, our PSM estimates are uniformly greater in magnitude for all but two client subgroups—both whites and blacks with less than a high school degree. The biggest difference in the effect is seen for the multisanctioned groups, where the sanction effect is found to be approximately $170–$400 larger in every case than the estimates from the sample that included clients with no earnings. Along with the results from the first set of robustness tests, these results provide greater confidence that our original estimates do not overstate the effect of sanctions.

Our final robustness test adds an additional conditioning variable to the analysis by controlling for TANF participation during the fourth quarter after exit (i.e., the post-exit quarter during which we observe wages). As noted earlier, the primary sample is not limited to clients who remained off the TANF rolls; clients may be on or off TANF in our data at any point during the post-exit period. We do not control for TANF participation during the fourth quarter in our initial analyses because we believe that this is one possible mechanism through which sanctioning might affect wage growth. That is, the disruption in clients' lives (such as in child care or transportation) caused by sanctioning may in turn lead sanctioned clients to rely more heavily on TANF for support than nonsanctioned clients. This seems especially plausible for frequently sanctioned clients. By comparing our original estimates to estimates that rely on models that match clients based on TANF participation during the fourth quarter (in addition to the other observed variables listed in tables 2 and 3), we can examine this possibility directly. These results are presented in column 9 of table 5.

Among once-sanctioned clients, the first-stage propensity-score models that include TANF participation (not shown) find that clients in the once-sanctioned group are generally no more likely to be on TANF than clients in the nonsanctioned group during the fourth post-exit quarter. Unsurpris-

8. As our earnings data are based on UI records, the data do not reflect unreported earnings from the informal economy.

ingly, the PSM estimates of the sanction effect for once-sanctioned clients are nearly identical to our original estimates. The exception to this pattern is Latina clients with less than a high school education. Once-sanctioned clients in this subgroup are somewhat more likely to be on TANF during the first quarter, and the PSM estimate of the sanction effect is about 5 percent ($23) lower compared to our original estimate. Thus, we conclude that for the once-sanctioned group, the sanction effect cannot be attributed to greater rates of TANF participation among the sanctioned group.

The results for the multisanctioned group lead us to a very different conclusion. The first-stage propensity-score models find that across all of the client subgroups, multisanctioned clients are statistically significantly more likely to participate in TANF during the fourth post-exit quarter. And in contrast to the results for once-sanctioned clients, the estimated sanction effects for multisanctioned clients are consistently lower for all client subgroups after controlling for TANF participation. On average, the estimated sanction effect decreased by 28 percent ($221) compared to our original estimates. The difference is smallest for Latina clients with more than 12 years of education (18 percent, $135), while the largest difference is seen for Latina clients with less than a high school degree (37 percent, $333). These results suggest that one possible mechanism through which sanctions affect wage growth is through greater reliance on TANF among frequently sanctioned clients. However, even after controlling for TANF participation, the estimates of the sanction effect are much larger for the multisanctioned group. Compared to once-sanctioned clients, the average effect of sanctions (across client subgroups) is approximately $200 (63 percent) greater for multisanctioned clients. The effect of being sanctioned more than once is most pronounced for black and Latina clients with a high school degree or more. For these subgroups, the estimated sanction effect for multisanctioned clients is more than twice as large as the effects estimated for once-sanctioned clients. While greater reliance on TANF may explain some of the sanction effect, clearly a large portion of the effect occurs through other mechanisms.

CONCLUSION

Although there is a large literature on TANF sanctions, there is relatively little research on the effects of sanctions on client outcomes and even less

research that uses appropriate controls for preexisting client differences (Meyers et al. 2006). In this article, we seek to contribute to this literature by estimating the effect of sanctions on one of the most relevant and important outcomes of the TANF program—the earnings of TANF clients. We estimate a consistently negative effect of sanctioning on client earnings, and the effect is both statistically and substantively significant. The effect of sanctioning is generally much greater for clients who receive more than one sanction, but even clients who are only sanctioned once seem to have difficulty recovering as much as a year after receiving the sanction. Perhaps most unexpectedly, the effect of sanctioning is somewhat larger among those with relatively higher levels of education. This is an important finding, as these clients are more likely to have better job opportunities than other clients and are otherwise more likely to successfully use welfare to reenter the labor market and escape poverty (Vartanian and McNamara 2004).

We also compare our matching estimates to difference-in-difference estimates generated by OLS regression (with controls) and simple difference-in-difference estimates that do not control for differences between sanctioned and nonsanctioned clients. Our unconditional estimates generally underestimate the magnitude of the sanction effect (compared to our matching results) and in some cases show no statistically significant effect at all. In contrast, the OLS difference-in-difference estimates (with controls) consistently produce larger effects than our matching estimates, although the magnitude of the difference is relatively small. These comparisons demonstrate the importance of selection bias in the estimation of sanction effects. Perhaps the most surprising finding is that the failure to control for client characteristics serves to underestimate the sanction effect. Comparisons with OLS are more favorable and build confidence in the conclusions drawn by prior studies that control for past client conditions.

Although propensity-score matching is judged to provide accurate estimates of causal effects in evaluations of job-training programs (Heckman et al. 1997, 1998; Heckman, Ichimura, Smith et al. 1998; Smith and Todd 2005; Mueser et al. 2007), we recognize that the treatment we observe in this study (i.e., sanctioning) as well as the population of interest (i.e., TANF clients) are unique in important ways and that we are not able to explicitly control for some variables that previous studies find to be important predictors of both sanctioning and earnings. This includes such variables as

domestic violence, mental health, substance use, and employment stability, among others. Yet, for at least two reasons we believe that our research design might minimize the potential bias caused by the exclusion of these variables. First, while it is certainly likely that sanctioned clients are more likely to experience poor health, domestic violence, and the loss of child care or transportation, to the extent that these experiences are represented in clients' earnings we are able to control for these potentially confounding variables by matching sanctioned and nonsanctioned clients on four quarters of pre-TANF earnings. Second, and perhaps most important, our results hold up under different definitions of the estimation sample. Indeed, the estimated sanction effects actually increase in magnitude when we exclude clients who reported no earnings throughout the entire period of analysis. The results also hold up when controlling for TANF participation during the post-exit period.[9]

Although our data come from the early 2000s, there is no reason to think that the negative effects of sanctioning have diminished. Indeed, in the aftermath of the economic downturn and the changes to federal policy during the 2005 reauthorization of TANF, it is likely that sanctioning may have even stronger effects today. As some critics argue, these changes provide even greater incentives for states to use sanctions to push clients into countable work activities or off the rolls completely. While it is unclear if such incentives are actually operating as claimed, sanction rates have increased since 2005, leading many advocacy groups to call for reform of sanctioning policies (Casey 2010). At the very least, given the persistently grim job market for low-income workers in the wake of the Great Recession, more research is needed on the connections among the eroding safety net, TANF policy, and the well-being of the poor. For now, we can only conclude that welfare sanctioning is a policy that perpetuates poverty rather helps overcome it.

9. One possible alternative explanation for our findings is that the presence of a sanction policy serves to motivate, and thus increase the earnings of clients who are not sanctioned. We find this explanation to be implausible given the relatively flat earnings of nonsanctioned clients across our observation period (see fig. 2). Yet, to provide a stronger test of this possibility, we conducted an analysis of the effect of local sanctioning stringency (measured as the county sanction rate) on the earnings growth of nonsanctioned clients. Despite substantial variation in local sanctioning stringency in Florida, the analyses find no relationship between the degree of stringency and earnings growth. We report this analysis in a supplemental appendix that is available upon request.

APPENDIX

DISTRIBUTION OF PROPENSITY SCORES BY RACE, EDUCATION, AND SANCTION STATUS

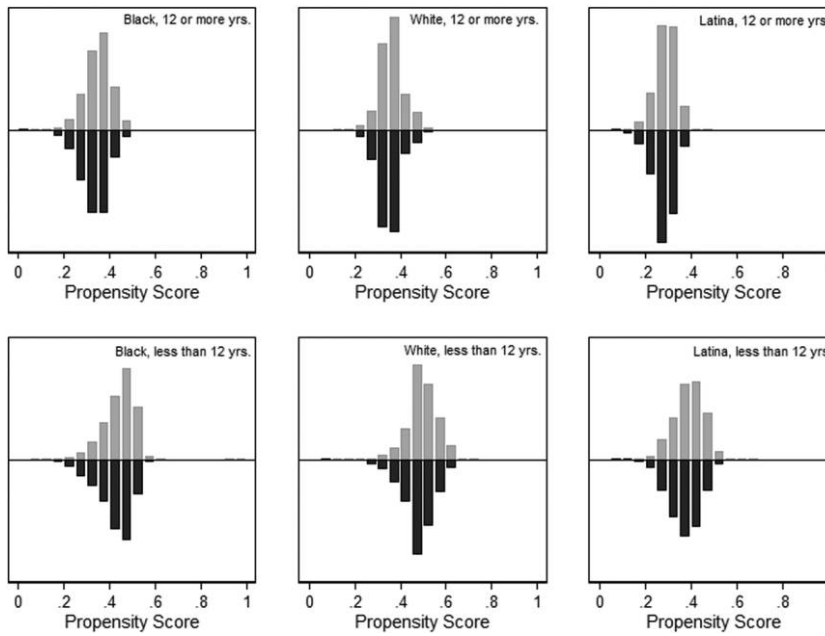

FIGURE A1. Once sanctioned (gray) vs. never sanctioned (black)

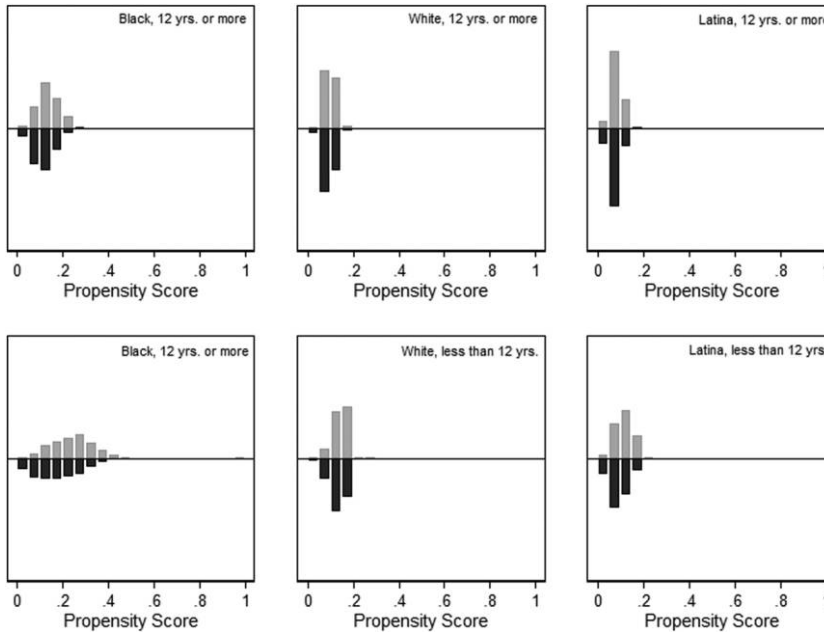

FIGURE A2. Multisanctioned (gray) vs. never sanctioned (black)

NOTE

Richard C. Fording is professor and chair of the Department of Political Science at the University of Alabama. His primary teaching and research interests include poverty and social welfare, race and politics, state politics, and US social movements. rcfording@as.ua.edu.

Sanford Schram is professor of political science at Hunter College (CUNY), where he teaches public policy in Roosevelt House. He is author, coauthor, or coeditor of 12 books and the 2012 recipient of the Charles McCoy Career Achievement Award from the American Political Science Association. He is currently completing a book titled "The Return of Ordinary Capitalism: Neoliberalism, Precarity, Occupy." ss1745@hunter.cuny.edu.

Joe Soss is the inaugural Cowles Chair for the Study of Public Service at the University of Minnesota, where he holds faculty positions in the Hubert H. Humphrey School of Public Affairs, the Department of Political Science, and the Department of Sociology. His research and teaching explore the interplay of democratic politics, socioeconomic inequalities, and public policy. jbsoss@umn.edu.

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