

Working to Avoid Incarceration: Jail Threat and Labor Market Outcomes for Noncustodial Fathers Facing Child Support Enforcement

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Child support enforcement is among several contexts in which work requirements are enforced by incarceration for noncompliance. Rather than creating barriers to employment, such incarceration threats may pressure subjects to work more, under worse conditions. We test for this using Fragile Families and Child Wellbeing Study data on respondents' child support, labor market, and criminal justice experiences in twenty cities. We exploit intercity variation in absolute and relative reliance on different child support enforcement techniques, especially punitive ones, such as incarceration, versus financially extractive ones, such as wage garnishment. As predicted, heavier reliance on incarceration sanctions is associated with more hours of work and lower wages among noncustodial fathers most vulnerable to incarceration.

Keywords: incarceration, child support, labor markets, work requirements, legal financial obligations

This study examines the labor market effects of work requirements enforced by threats of future incarceration. It does so in the context of child support enforcement, long linked to the criminal legal system as an important labor market institution, especially for disadvantaged younger men (Holzer, Offner, and Sorensen 2005). Analysis of that linkage has focused on how child support enforcement and incarceration both impede employment. Child support is modeled as a tax that disincentivizes earnings (Miller and Mincy 2012; Cancian, Heinrich, and Chung 2013), and prior incarceration is a "negative credential" that deters hiring (Pager 2008; Stoll and Bushway 2008). Another linkage, however, implies different labor market consequences: both systems impose work requirements on people who are not currently incarcerated but who face incarceration as a sanction for nonwork (Zatz 2019).

Theoretically, a credible threat of incarceration for too little work unambiguously incentiv-

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izes employment by sharply raising the expected cost of nonwork. At the margin, this threat should pressure those facing it to accept working conditions they otherwise would reject, regardless of any past incarceration. All else equal, work should increase and working conditions should deteriorate.

This mechanism is one of compulsion into work, not exclusion from it, and one grounded in potential future incarceration, not current or past incarceration. This shift also implicates the criminal legal system's relationship to the welfare state. The latter's labor disciplining function (Handler and Hasenfeld 1991), intensified by 1990s welfare reforms (Soss, Fording, and Schram 2011), may be migrating back into the carceral state (Rusche and Kirchheimer 1939), alongside increasing attention to un- and underemployment among men (Mead 2007). That cuts against the notion that today's carceral state primarily "warehouses" the economically excluded (Western and Beckett 1999) as neoliberal restructuring produces more precarious labor markets with weaker safety nets (Simon 1993; Wacquant 2009). Work under carceral threat also raises policy concerns about forced labor, workplace vulnerability, and downward pressure on labor standards (Zatz 2019), concerns not captured by measures of employment levels, debt payments, and actual incarceration.

We test for these threat effects by exploiting geographic variation in child support enforcement techniques indicated through parent reports in the Fragile Families and Child Wellbeing Study (Fragile Families) (Geller, Jaeger, and Pace 2018). We hypothesize that heavier reliance on incarceration sanctions for child support nonpayment will increase hours worked and depress wages for noncustodial fathers (NCFs), relative to custodial fathers (CFs). Our data are inadequate to include mothers. This effect should be strongest and clearest among those most exposed to enforcement action and those with relatively low hours and wages. Furthermore, these jail threat effects should be the opposite of those from financial enforcement actions that "tax" earnings. Our results are largely consistent with these hypotheses.

INCARCERATION SANCTIONS AS WORK ENFORCEMENT IN CHILD SUPPORT

Work requirements enforced by incarceration are familiar in probation, parole, and other forms of criminal legal supervision where seeking and maintaining employment is a widespread, explicit condition of supervision (Doherty 2015; Travis and Stacey 2010). Recent research finds both nontrivial aggregate levels of enforcement leading to incarceration or reincarceration (Zatz et al. 2016) and also parole or probation officer-supervisee interactions involving substantial pressure to work (Gurusami 2017; Augustine 2019). Earlier research had reported that such work requirements were vanishing in practical significance (Simon 1993).

Work requirements also arise from child support even though the underlying obligation is to pay. As a practical matter, however, most obligors must pay out of earnings, especially at the margin where compliance and enforcement are at issue. Poverty and un- or underemployment are endemic among obligors with significant arrears (Sorensen, Sousa, and Schaner 2007). Here, the primary issue is having the earnings with which to pay, not refusing to pay from existing income or assets. In the latter scenario, financial enforcement actions- principally wage garnishment-detect and acquire those funds, directly imposing the underlying tax. But they miss the point when obligors have limited income, the modal problem among those with arrears.

Work behavior is not fixed, however, and so enforcement also focuses on increasing earnings. If obligor un- or underemployment is considered "voluntary" (Mead 2007), penalizing nonwork should cause obligors to substitute work, generating earnings for capture by the tax and its financial enforcement.

Reflecting this analysis, child support law treats support obligations as creating a duty to earn enough to pay, not just to pay enough of what one earns. For instance, the California Supreme Court upheld incarceration as a contempt sanction for an obligor who "fails or refuses to seek and accept available employment for which the parent is suited by virtue of education, experience, and physical ability."¹There,

1. Moss v. Superior Court, 950 P.2d 59, 76 (Cal. 1998).

the persistently unemployed obligor had been jailed because the trial judge concluded that he surely "could get a job flipping hamburgers at McDonald's."² The 1996 federal welfare reform law also required state child support enforcement systems to authorize ordering a noncustodial parent into the same range of workrelated activities the law required of their custodial counterpart, when their child received cash assistance.³ Most states already had some form of work requirements in their child support laws, often without any connection to cash assistance. A typical statute in Illinois provides that when any obligor is unemployed, "the court may order the person to seek employment and report periodically to the court with a diary, listing or other memorandum of his or her efforts in accordance with such order."4 Further institutionalizing such requirements has been widely discussed (Sorensen 2010), and it was a priority of the Obama administration (Turetsky 2012).

Additionally, NCF work requirements emerge from the legal regime that permits incarceration as a general child support enforcement technique. Nonpayment may expose obligors to incarceration through civil or criminal contempt proceedings or through criminal prosecution for nonsupport (Patterson 2008; Cook and Noyes 2011; Brito 2012). However, constitutional law limits incarceration of someone lacking the "ability to pay," both for child support and for criminal fines and fees (Colgan 2018; Hampson 2016). In contrast, "willful" or "voluntary" nonpayment, despite the ability to pay, may be punished.

Willful nonpayment, and therefore exposure to incarceration, may be established through voluntary failure to *acquire* the ability to pay through earnings; that was the issue in the *Moss* case. Vice versa, adequate efforts to seek and maintain employment establish a defense to incarceration, even if one remains unable to pay.⁵ The incarceration sanction regime thus imposes work requirements functionally, even when the formal demand is simply to pay.

Unemployment's "voluntariness" implicates the scope and intensity of job search and the working conditions that must be accepted, as is well known for analogous inquiries for public benefits eligibility (Williams 1999). These judgments are obscured by competing portrayals of nonpaying obligors as "deadbeats" versus "deadbrokes" (Cammett 2011). Someone who refuses a job requiring sixty hours per week of dangerous work could be classified either way, depending on whether that refusal is deemed justified. Incarceration for nonwork reflects a judgment that the obligor drew this line in the wrong place. Prospectively, threatening incarceration should move obligors toward accepting and maintaining employment, under worse conditions; that is the premise of affirmative arguments for mandatory work programs (Mead 2007). But such pressures should operate even outside formal work programs. Lynne Haney recently found a typical judge holding child support obligors to the standard of a father who worked four jobs and moonlighted mowing lawns (2018). Similarly, Alexes Harris's research on criminal legal debt found judges insisting that those unable to pay "just needed to work harder" (2016, 138).

Of course, an empty threat cannot drive work behavior, but the jail threat appears real. Systematic prevalence estimates have been lacking, but scattered reports suggest substantial use (Brito 2012; Robles and Dewan 2015). Recent research using Fragile Families data has found that, in large U.S. cities, 14 percent of NCFs with arrears have been jailed by the time their child turns nine (Cozzolino 2018), or 5 percent of all fathers, including 15 percent of all African American fathers (Zatz et al. 2016); the last statistic underlines the racial disparities at this nexus of child support, incarceration, and low-wage work.

- 2. Ibid., 80n16.
- 3. 42 U.S.C. § 666(a)(15).
- 4. 50 III. Comp. Stat. Ch. 5/505.1.
- 5. Turner v. Rogers, 131 S.Ct. 2507 (2011).

JAIL THREAT AS A LABOR MARKET INFLUENCE

Any labor market effects of these jail threats would be significant in several ways. Within the literature on incarceration and work, it would highlight how systems with the power to punish can function as labor market institutions that set the terms on which individuals must work or face jail. Such enforced insertion into work to avoid incarceration would complement the now well-known phenomena of workers' removal from the labor force during incarceration (Western and Beckett 1999) and reduced access to employment after release (Pager 2008; Holzer, Raphael, and Stoll 2004). This emphasis on labor market exclusion also has been extended to criminal legal debt (Cadigan and Kirk 2020).

Although the notion of prior incarceration or conviction as a barrier to employment dominates the field, some findings indicate the complementary mechanism explored here. Some research finds temporarily increased employment immediately following release from incarceration, followed by subsequent declines (Pettit and Lyons 2007; Sabol 2007; Seim and Harding 2020). Beneficial assistance via parole supervision has been suggested as an explanation, but that interpretation is hard to square with the accompanying finding that wages decreased during this period (Pettit and Lyons 2007). Enhanced employment prospects should raise wages. A competing explanation would be that supervision applies pressure that lowers the reservation wage (Seim and Harding 2020). Another recent study found that higher rates of immediate post-release work were associated with greater willingness "to take on poorer quality work" (Sugie 2018), precisely what the threat of incarceration is designed to encourage.

Jail threat is particularly significant amid policy interest in "alternatives to incarceration." These often include expanding criminal legal supervision outside prison walls (Phelps 2016) but backed by the threat of incarceration for violating behavioral conditions, including work (Doherty 2015). Work-related conditions and programs, moreover, often are understood as efforts to obtain the benefits of employment despite the barriers associated with reentry. The question raised, however, is the balance between expanding access to better forms of work versus leaving barriers intact while pressuring people into whatever "bad jobs" (Bumiller 2015) are available to, or created for, people with convictions.

This nexus between work mandates and alternatives to incarceration also arises with criminal fines and fees (Harris 2016). Monetary sanctions may constitute an alternative to incarceration at sentencing, but the resulting payment obligation may later be enforced through incarceration (Colgan 2018). Here, too, mandatory work is put forward as an alternative to incarceration for nonpayment, especially through unpaid "community service" that operates outside conventional employment law and below basic labor standards (Stuart 2011; Zatz 2019).

This active use of state power to enforce and regulate work also contrasts with prevailing accounts of contemporary low-wage and precarious work (Bernhardt et al. 2008; Kalleberg 2011). This literature typically portrays declining government regulation of employers unleashing private market forces. In contrast, we explore here the regulation of workers with punitive techniques characteristic of criminal law. Although child support incarceration often arises through civil contempt (Patterson 2008), this civil application of punitive techniques, often complemented by criminal prosecutions, is itself characteristic of a broader pattern of criminalization seen in welfare (Gustafson 2009) and immigration (Eagly 2010). Likewise, scholarship on the contemporary carceral state cautions against overreliance on formal legal categories of criminal, civil, and the like (Beckett and Murakawa 2012).

Labor market regulation also has been absent from the modest child support literature on incarceration sanctions (Patterson 2008; Brito 2012). The topic rose to prominence with the Supreme Court's 2011 *Turner v. Rogers*⁶ decision about the right to counsel in contempt proceedings, and with a police officer's murder of Walter Scott as he fled an arrest warrant for

6. 131 S.Ct. 2507.

child support nonpayment (Robles and Dewan 2015). This literature, however, largely has focused on procedural rights and on child support's relationship to income support for custodial parents and children, not on its operation as an institution structuring the bottom of the labor market (but see Chung 2011).

In contrast, a significant empirical policy literature addresses how child support enforcement may affect the labor supply of young, disadvantaged men (Holzer, Offner, and Sorensen 2005; Cancian, Heinrich, and Chung 2013). This literature, however, models child support exclusively as a tax on earnings. The resulting substitution effect should reduce work or shift it off the books (Rich, Garfinkel, and Gao 2007; Miller and Mincy 2012). Any potential to increase work is attributed to an income effect, still within the tax framework. In that framework, more enforcement simply intensifies any substitution or income effects. Incarceration figures only in the rear view, as a driver of arrears accrual while an obligor is removed from the labor market (Pirog and Ziol-Guest 2006; Cammett 2011; but see Haney 2018).

Studying these incarceration-backed work requirements bears directly on policy interest in using criminal justice and child support institutions to target work programs toward men (Mead 2007; Sorensen 2010), complementing welfare-to-work programs for custodial parents (primarily women). This interest overlaps with using work programs as an alternative to incarceration for child support nonpayment (Turetsky 2012), as with criminal legal debt. For instance, the Obama administration contrasted its proposed restrictions on states' ability to incarcerate for failure to pay with its endorsement of states' continued authority to incarcerate for failure to work (Office of Child Support Enforcement 2014, 68557).

Analysis of child support work programs has emphasized how services can enhance job finding and retention (Sorensen 2010), not whether the enforcement threat changes responses to constant work opportunities. The literature on welfare-to-work, however, suggests that its employment effects arose in part from making welfare receipt less attractive, not increasing the returns to work, leading participants to exit welfare sooner and for lower earnings (Cancian et al. 2002). In the classic pairing of "help and hassle" (Mead 2007, 54), incarceration takes "hassle" to the extreme. Some suggestive evidence comes from Texas' Non-Custodial Parent Choices program, which used jail threats to mandate participation in workforce development programs (Schroeder and Doughty 2009). One evaluation found negative effects on earnings levels, despite increased employment, and attributed this combination to job gains concentrated in lower-paying positions. Notably, these earnings and potential wage effects generally go unmentioned in reviews that tout the program's positive employment effects (Turetsky 2012).

RESEARCH QUESTIONS AND HYPOTHESES

Our research question is whether the threat of incarceration for child support nonpayment alters the labor market behavior of NCFs. We hypothesize that this threat should induce increased work effort—measurable by increased annual hours worked—and on terms less attractive to workers—measurable by depressed wage rates. Although we expect NCFs and CFs to differ systematically in their labor market characteristics, these differences should widen as enforcement intensifies because CFs should face de minimis threat from potential child support enforcement.

The threat effect should be strongest among NCFs at greatest risk of enforcement action, especially those in arrears (Miller and Mincy 2012). NCFs without arrears, because either they are current on payments or no child support order yet exists, also should exhibit some threat response, but we expect this to fall in between those with arrears and CFs. The predicted relative ranking of these intermediate groups is ambiguous because those with orders but not arrears have the legal basis for enforcement in place but are selected for labor market behavior that insulates them from enforcement.

We hypothesize that, because those with few or no hours likely have weaker employment prospects, transitions from nonwork to lowhours work could depress median hours among those working even while raising hours overall. We expect hours increases to be concentrated in the bottom half of the overall hours distribution because the jail threat focuses on those deemed voluntarily un- or underemployed. Similarly, wage effects may concentrate at lower wages (among those working), where those with the least to gain financially from working should be most responsive to the nonfinancial jail threat.

These hypotheses are grounded in the theory that jail threat increases the relative attractiveness of work by penalizing nonwork. However, that nonfinancial penalty operates in tandem with the more familiar tax on earnings. The net effect is ambiguous in theory, even assuming the tax's substitution effect dominates its income effect. Accordingly, we predict that any threat effect from jail sanctioning will be most pronounced after the extent of financial sanctions are controlled for, the two sanction types having opposite effects. Furthermore, because the work requirement is linked to a payment requirement, any jail threat effect should interact with the rigor of financial enforcement. Increased hours and decreased wages driven by the jail threat should be more pronounced at lower levels of financial sanctioning.

DATA AND EMPIRICAL STRATEGY

We use Fragile Families longitudinal survey data from the parents of 4,898 infants, including a systematic oversample of nonmarital births, born in twenty U.S. cities. The weighted sample represents the families of children with hospital births in each of the cities between 1998 and 2000. Response rates decline over the five survey waves (baseline, then years one, three, five, and nine) but remain high, even for the hardest-to-reach group of unmarried fathers in wave 5 (58 percent) (Geller, Jaeger, and Pace 2018).

Fragile Families contains uniquely detailed data about child support and its enforcement, criminal justice contact, and labor market experience, as well as individual characteristics that may influence labor market outcomes. Under the requisite confidentiality agreement and IRB approval, we use restricted data that contain geographic identifiers, city weights, and city-level data that allow for various controls. This allows us, city by city, to construct policy treatment variables measuring a population's exposure to specific types of child support enforcement, to construct dependent variables measuring labor market outcomes within that population,⁷ and to control for other relevant influences.

Our analysis relies on labor market, incarceration, and child support enforcement data from waves 2 through 5 because of compatibility issues with wave 1 regarding the longitudinal questions (Miller and Mincy 2012; Rich, Garfinkel, and Gao 2007), and because any threat effect of child support enforcement with respect to the focal child should only become relevant in the years after birth.

Policy Treatment and Father Status Variables

We create policy treatment variables that measure local jurisdictions' child support enforcement practices. We rely on data (from fathers or mothers) on 3,218 fathers, after excluding those missing weights. The main policy treatment variable is the percentage of NCFs (N = 2,151) ever jailed as a result of a child support enforcement action.

To identify and categorize NCFs, we use wave-specific data from both mothers and fathers on coresidence among the focal child's parents and the focal child, child support orders, and arrears on those support obligations. We divide fathers into four mutually exclusive categories: fathers with arrears (NCF-arrears); fathers with orders but no arrears (NCF-orders); and fathers without orders or arrears who are defined as NCFs (NCF-only) because they live with the focal child less than 100 percent of the time.8 The remaining fathers are defined as custodial (CF). Because Fragile Families lacks rich, consistent data on parental relationships to nonfocal children, these definitions will misclassify some fathers who are noncustodial (including with orders or arrears) only with re-

7. This approach relies on NCFs remaining subject to the child support enforcement practices and labor markets of the focal child's birth city. Tests of respondents' mobility support this assumption's validity.

 We rejected a 50 percent threshold because of data limitations and our expectation that coresidence between 50 and 100 percent would be more like the former than the latter. spect to a nonfocal child, especially one with a different mother. This increases imprecision but should not generate spurious positive results.⁹

We include in the policy variable denominators anyone who is an NCF in any wave. This allows us to assess potential variation in NCF subtype responses to enforcement threat and to avoid sensitivity to how the enforcement environment might affect which NCFs receive orders and end up in arrears. Sensitivity checks indicate that alternate definitions (including CFs in or including NCF-onlys from the denominator) do not fundamentally change the results.

The policy variable numerators rely on the survey questions asking, beginning with wave 3, whether any child support enforcement has been taken to collect arrears, and further distinguishing among types of action. We divide these into tax-like financial sanctions (garnishing wages, seizing tax refunds or other assets, or placing liens on property), jail sanctions (incarceration or probation), driver's license suspensions, and other actions (unspecified, specified as Other, or business license suspensions).

This study's methodological advance is to measure the mix of enforcement methods localities employ. Prior studies have measured child support enforcement intensity as onedimensional, using variables based mostly on state-level policies and expenditures, actual payments (including at the local level), and rates of establishing child support orders within eligible populations (Holzer, Offner, and Sorensen 2005; Nepomnyaschy and Garfinkel 2010; Miller and Mincy 2012). mother and father reports of whether a given father has experienced any enforcement action and what type or types.¹⁰ Pooling across waves generates more reliable measures by increasing the number of observations, but it assumes a constant policy environment over time. Given the noted substantial sample attrition, this approach could introduce bias, but, on examination, the demographic composition of attrition does not vary systematically across cities.

Similarly, pooling data across father selfreports and mother reports increases the number of observations and mitigates attrition bias among fathers. However, bias could be introduced by differential attrition rates across cities in conjunction with systematic mother-father differences in child support enforcement reports.¹¹ Daniel Miller and Ronald Mincy (2012) performed validity checks on mothers' reports of fathers' child support arrears and found evidence of their reliability. Moreover, the Current Population Survey uses mothers' reports from the biannual Child Support Supplement as the basis for national estimates of child support compliance.

Our measure of jail sanctions also incorporates Fragile Families' criminal justice questions. Beginning with wave 2, this series asks about incarceration or conviction, with specification of the underlying charges that include child support nonpayment as one option.¹² These questions capture some fathers who faced enforcement with respect to a nonfocal child,¹³ and they provide some redundancy with the direct child support questions, mitigating underreporting. They also increase the number of father self-reports because, due to a survey administration error, NCFs mostly were not

We pool within cities and across waves

9. Intercity variation in fathers' rate of multipartner fertility could create a risk of bias.

10. One parent's report of an action dominates negative or missing reports from the other.

11. Mother and father reports show incarceration sanctions at similar rates, but mother reports show much lower rates for other actions, which presumably are much less visible than incarceration.

12. This method aggregates incarceration arising from civil contempt and criminal prosecution. We doubt the procedural differences much affect NCFs' experience of incarceration threat. Indeed, survey answers may not distinguish them, given child support questions about probation and mother questions about father incarceration without a conviction predicate (Geller, Jaeger, and Pace 2016). For similar reasons, we include reports of criminal convictions for nonsupport that may not have led to incarceration.

13. Two fathers with reported incarceration for child support nonpayment were otherwise coded as custodial in relation to the focal child in all waves; these were recoded as NCF for policy variable purposes.

		Any			Driver's			
	Raw	Action	Jai	I (SE)	Finan	cial (SE)	License	Other
	Ν	(1)		(2)		(3)	(4)	(5)
Chicago	53	3%	0%		0%	(0%)	0%	3%
Boston	59	10	4	(0%)	3	(3)	0	3
Corpus Christi	174	13	3	(1)	7	(2)	0	3
Austin	137	13	6	(4)	4	(2)	0	3
New York	104	15	0		10	(6)	5	2
Indianapolis	167	16	8	(2)	5	(0)	0	4
San Antonio	47	16	2	(2)	1	(1)	0	14
Oakland	139	16	1	(0)	5	(1)	4	8
San Jose	110	20	8	(7)	8	(8)	2	3
Newark	162	20	10	(2)	6	(2)	2	6
Detroit	150	21	6	(1)	7	(2)	1	8
Philadelphia	146	23	4	(1)	10	(2)	2	8
Milwaukee	184	27	11	(2)	10	(2)	4	7
Baltimore	166	27	6	(2)	5	(1)	7	11
Nashville	54	28	17	(8)	7	(3)	0	8
Norfolk	49	29	17	(3)	10	(0)	9	1
Jacksonville	61	30	5	(1)	19	(11)	3	6
Toledo	59	31	9	(2)	11	(3)	6	13
Richmond	173	33	16	(6)	12	(3)	2	10
Pittsburgh	51	58	41	(7)	15	(4)	9	5

Table 1. Child-Support Enforcement Action Rates by Type and City, Among Noncustodial Fathers

Source: Authors' calculations from the Fragile Families and Child Wellbeing Survey. *Note:* Wave 2 weights.

asked the child support questions in waves 3 and 4. This provides another reason to include mother reports of incarceration sanctions through both the criminal justice and child support series, which also mitigates the father underreporting known to occur in the former (Dwyer Emory et al. 2020; Geller, Jaeger, and Pace 2016).¹⁴

Table 1 presents intercity variation in child support enforcement actions taken as a percentage of all NCFs and using Fragile Families wave 2 weights. Throughout, cities vary within the same state. Column 1 represents the extensive margin of all actions taken within a city. Our analysis relies on the significant variation by city in column 2, showing the rate of incarceration as an enforcement action. Localities also make extensive use of financial sanctions and driver's license suspensions, shown in columns 3 and 4. The data show variation across locales in these enforcement actions as well. Column 5 presents residual reports of some action other than jail, financial, or driver's license sanctions.

Our measures of jail, financial, and driver's license sanctions are not mutually exclusive, either within a city or even for one individual, who may report more than one action. Although jail and financial sanctions correlate positively (as driver's license suspensions do, too), they do not operate in lockstep. Correlation coefficients between these policy variables are about 48 percent and weakly statistically significant, indicating significant heterogeneity. We use this heterogeneity to isolate the impact of jail enforcement from fi-

14. When nonsupport involves a child in common, mothers may have better information about resulting incarceration, and less incentive to suppress it, than they do for most other offenses, though this may be less true where support is assigned to and retained by the state (Dwyer Emory et al. 2020). nancial enforcement in the regression analysis that follows.

We acknowledge and have attempted to mitigate potential bias relating to self-reports, counterpart reports, and differential attrition, as well as sample size limitations. Ultimately, these are endemic to the large research literature relying on Fragile Families and other surveys, including their use to specify child support-related independent variables (Miller and Mincy 2012). We report standard errors for our main jail and financial sanction policy variables in columns 2 and 3,¹⁵ suppressing them in the other columns to conserve space. They indicate considerable uncertainty about the rank ordering of cities, especially in the middle of the distribution, but also clear differentiation between the low and high ends.16 In a further test of robustness, we ran twenty variations on our main models, each one dropping one city from the analysis. In no case was the change in main results substantial.

With respect to external validity, respondent reports of enforcement actions taken against themselves or their counterparts are an imperfect proxy for the actual rate of enforcement actions representing the policy environment. Nonetheless, the two should be systematically related, and there is no reason to expect intercity variation in the relationship that would introduce bias. These remain the best available sources. Rich. reliable administrative data have proven difficult to obtain (Cook and Noyes 2011). Federal child support performance indicators are reported by state and do not measure sanctions against individual obligors, let alone their type. However, our measures are broadly consistent with the available data. Pittsburgh, our highest enforcement city, sits within Pennsylvania, which consistently ranked at the top of national child support enforcement intensity during the relevant period (Solomon-Fears 2007). Pittsburgh's county (Allegheny) claims to be a top enforcer among urban areas (Family Division 2017). It regularly conducts highly publicized arrests of obligors in arrears, pursuant to a local policy change in 1999 (at the beginning of Fragile Families data collection) emphasizing jail sanctions (*Pittsburgh Post-Gazette* 1999; Roebuck 2007). Vice versa, Chicago, our lowest enforcement city, sits within Illinois, which consistently ranked toward the bottom of federal measures (Solomon-Fears 2007), and its county (Cook) was cited for lax enforcement during the relevant period (Smith 2002).

In general, relying on Fragile Families reports for our policy treatment variables likely distorts our absolute estimates of individual cities' sanction rates. We have no reason, however, to expect these limitations to substantially distort what our analysis relies on, which is cities' relative reliance on particular sanctions, especially incarceration. In most scenarios, these limitations should simply introduce noise that biases us toward null results. Furthermore, because Fragile Families data do not capture the entire population potentially subject to child support enforcement in each cityprimarily by sampling based on one birth cohort-even accurate rates for the surveyed population likely differ from what a more inclusive measure would capture. Nonetheless, there is no apparent reason why this limitation would produce spurious positive results, and it represents the enforcement environment for those most similar to the fathers whose labor market behavior we measure. We also test multiple outcomes to mitigate the influence of spurious results in any one domain.

Of course, our policy treatment variables measure an enforcement environment that is not randomly assigned. Although the procedures and standards governing the establishment and enforcement of child support orders are set at the state level with substantial federal influence, implementation occurs at the local level. Accordingly, even within states, localities can and do exercise discretion to use different intensities and different methods of enforcement (Cook and Noyes 2011), and so there are

15. Because the policy variables use Fragile Families weights, the standard errors do not strictly track the raw N also displayed in table 1.

16. For instance, there is no overlap between the upper bound of the 90 percent confidence interval (not shown) of the eight cities with the lowest jail sanction rate and the lower bound of the 90 percent confidence interval of the six cities with the highest jail sanction rate.

sound reasons to believe these actions are independent across enforcement areas.

Nonetheless, the rate of incarceration sanctions could be endogenous to the labor market characteristics of those facing sanctions. For example, obligors' labor market characteristics should influence the presence, level, and duration of arrears, which may in turn trigger an incarceration sanction.17 We tested for such endogeneity by running a regression predicting individual child support incarceration sanctions as a function of any arrears amounts and indicator variables for cities. The city coefficients were large relative to those for arrears and highly correlated (0.78) with our jail threat policy variable (detailed results available on request). This is inconsistent with variations in city incarceration rates being an artifact of varying arrears levels and instead supports the assumption of policy independence. Furthermore, for wages, any endogeneity should dampen, not exaggerate, any positive results. As among those with similar arrears, we would expect enforcement to target those with a higher earnings capacity, leading incarceration rates to correlate with higher wages, the opposite of our predicted threat effect.

Dependent Variables

To construct our dependent variables, we pool observations on fathers' labor market outcomes and other relevant variables across waves 2 through 5 to increase sample size and statistical power, resulting in 8,930 father-wave observations.¹⁸ Our final sample consists of 8,362 father-wave observations. We eliminate as outliers 230 father-wave observations reporting hourly wage rates equal to or less than \$1 or greater than \$50, or reporting annual hours worked equal to or greater than three thousand. We also eliminate 338 father-wave observations where the father has been identified as experiencing a child support jail sanction in that or any prior wave. This ensures that we measure only the general deterrence effect of the general risk of future sanction, not the specific deterrence effect of having previously experienced the sanction; the latter is of related interest but should not vary with city threat level. This makes ours a conservative test for a jail threat effect because the behavior of those previously sanctioned (but removed from our sample) could reflect both mechanisms (Stafford and Warr 1993); having been previously sanctioned may well select for those most likely to be responsive to the prospective threat even absent prior sanction.¹⁹

The labor market outcome variables are annual hours worked and wage rates. Annual hours worked is measured by taking the average hours per week the respondents report working in most recent formal employment and multiplying by the number (including zero) of weeks worked in formal employment over the past year. This continuous measure allows more precise measurement of work levels, capturing transitions from nonwork to work and increased hours among those already working.

We use wage rates to measure how jail threat may induce NCFs to accept lower job quality. This captures both entry into employment at a wage that, absent the threat, would have been below the reservation wage, and also deterioration of wages within employment due to reduced bargaining power when job loss may trigger incarceration. Earnings cannot capture this because we predict its components of wages and hours to move in opposite directions. Fragile Families did not systematically ask about wage rates but instead elicited earnings over one of several periods; a substantial minority answered with hourly earnings (wages), but other units included days, weeks, or year. Thus, where necessary, we measured wage rates by

17. We found no statistically significant relationship between the incarceration sanction rate and possible enforcement triggers such as the percentage of fathers (or specifically those in arrears) who were African American, high school nongraduates, or who had incomes below the poverty line at the focal child's birth.

18. We focus exclusively on fathers because of the small population and lack of analogous data on noncustodial mothers.

19. We also analyzed an alternative sample including these 338 observations, but this produced no material difference in the results. This also mitigates the concern that underreporting of jail sanctions would lead to inclusion in our main sample of some observations that would have been excluded with full information.

Father Category	Low Threat	Medium Threat	High Threat	High-Low
Annual hours worked				
NCF-only versus CF	-486	-492	-521	-35
NCF-orders versus CF	-191	-307	-213	-22
NCF-arrears versus CF	-531	-567	-614	-84
Wage rates				
NCF-only versus CF	-4.11	-6.22	-6.57	-2.47***
NCF-orders versus CF	-2.78	-4.32	-5.51	-2.73***
NCF-arrears versus CF	-4.12	-6.48	-8.17	-4.05***

Table 2. Differences in Hours and Wages Between Father Categories, by Jail Threat Level

Source: Authors' calculations from the Fragile Families and Child Wellbeing Survey. *Note:* Threat levels are defined by jail action rates of <= 7 percent (low); > 7 percent, <= 11 percent (medium); > 11 percent (high). All father category differences within threat level are statistically significant at 1 percent level. NCF = noncustodial fathers; CF = custodial fathers. *p < .1; **p < .05; ***p < .01

constructing weekly earnings and dividing by weekly hours.²⁰

Initial Results (Unadjusted)

We first explore at the unadjusted mean level initial evidence with respect to the expected jail threat's relationship to wage rates and annual hours worked. Table 2 provides differences in hours worked and in wages between NCFs and CFs within cities grouped by low, medium, or high jail threat, as specified in the table. Not surprisingly, all of these differences are statistically significant, as would be expected if CF versus NCF status selected for labor market characteristics.

Of greater interest is how the difference between CFs and NCFs itself varies with the level of jail threat. In regard to hours worked, we do not find significant differences between high and low threat cities in NCF-CF differences in hours worked. This is unsurprising because these unadjusted results take no account of the concurrent operation of other sanctions, as well as a host of other individual- and city-level factors. To address these considerations, regression analysis is needed.

Even without regression analysis, however, the wage rate results are consistent with our hypotheses. The NCF-CF gap in wage rates is largest in high jail threat cities, significantly higher than in low threat cities across each subtype of NCF. Moreover, this high-low difference is largest for the NCFs in arrears who are most vulnerable to jail threat.

Analytic Strategy

Based on equation (1), we regress individual wages and hours on our policy variables measuring city-level enforcement methods:

$$LF_{i} = \alpha + Incar_{ci}\beta_{1} + \beta'_{2}CEI_{ci} + \beta'_{3}FS_{i} + Incar_{ci} * \beta'_{4}FS_{i} + \beta'_{5}X_{i} + \beta_{6}Wave_{i} + \beta'_{7}Z_{ci} + e_{ci}.$$
(1)

LF refers to annual hours worked and wage rates. *Incar* indicates the citywide jail sanction rate; *CEI* refers to the vector of the other policy variables measuring financial, driver's license, and other sanctions; and *FS* indicates categorical variables for the father's custodial status, CFs being the reference category. *X* refers to the vector of individual-level control variables that includes father's race, age, nativity, educational attainment, prior conviction or incarceration (regardless of offense type), as well as current absence from the labor market due to current school, disability, or incarceration. *Z* indicates citywide control variables that include racial-

20. Because the survey contains data on neither days per week nor hours per day, when daily earnings were reported, we assumed five days of work per week, which helped motivate our relatively aggressive outlier cutoff of \$50 per hour. We found little sensitivity to an alternate measure assuming eight hours per day.

		Jail Action Rate	e	Fin	Financial Action Rate			
		Quantile R	egressions		Quantile Regressions			
Father Category	OLS	25th	50th	OLS	25th	50th		
	(1)	(2)	(3)	(4)	(5)	(6)		
NCF-only versus CF	116.58	1,244.52	633.55*	758.29	-318.74	235.03		
	(0.46)	(1.50)	(1.90)	(1.26)	(-0.19)	(0.36)		
NCF-orders versus CF	10.73	254.57	308.37	106.21	584.71	-650.48		
	(0.05)	(0.34)	(1.02)	(0.20)	(0.39)	(-1.07)		
NCF-arrears versus CF	19.50	880.00	378.27	200.80	-1,060.39	-1,171.45		
	(0.03)	(0.90)	(0.96)	(0.25)	(-0.57)	(-1.56)		

ſab	le	3./	Annual	Hours	Worked	Regressions,	Jail and	Financial	Action	Rate I	Resul	ts
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Source: Authors' calculations from the Fragile Families and Child Wellbeing Survey. *Note:* Based on table A2. T-stats in parentheses. NCF = noncustodial fathers; CF = custodial fathers.

p* < .1; *p* < .05; ****p* < .01

ethnic representation, unemployment and poverty rate, region, and the mean household income. Because the data are clustered by survey wave, we controlled for wave fixed effects as indicated by *Wave*. Variable means (standard deviation) are presented in table A1, both for the overall sample and by father's custodial status. Unless otherwise noted, OLS methods are used to estimate models for both wages rates and annual hours worked. Because the data are clustered by city, we use standard errors clustered on cities.²¹

The policy treatment variables are interacted with father's custodial status to allow differential slope estimates of the influences of enforcement actions by vulnerability to child support enforcement. CFs, who theoretically should be unresponsive to the child support enforcement policy environment, serve as the reference category. Measuring the change in labor market outcomes relative to CFs (Miller and Mincy 2012) also mitigates the risk that observed correlations between policy variables and NCF labor market outcomes are driven by unobserved differences in cities' general labor market characteristics rather than by the influence of policy variation.

MAIN (ADJUSTED) RESULTS

We present results for annual hours worked followed by those for wages.

Annual Hours Worked

Table 3 presents summary regression results for annual hours worked, highlighting the interactions between custodial status and jail or financial sanction rates. Full results for all policy and father status variables are reported in table A2. All regression models also include the complete set of individual and citywide controls described, but their coefficient estimates are suppressed to conserve space; results are consistent with conventional expectations.

Column 1 presents coefficient estimates for the sample as a whole. Recall, the key identifying coefficient estimate of the threat of jail is the interaction between father custodial status, in particular NCFs with arrears, and the percentage of all NCFs who faced a jail enforcement action. Columns 2 and 3 examine potential heterogeneity in the threat effect of jail by presenting quantile regression estimates at the 25th and 50th percentiles of the hours distribution, respectively. Analysis of the data not shown here indicates that annual hours for NCFs in arrears

21. Including city fixed effects was not possible due to multicollinearity, including, unavoidably, with the policy variables. Models that included those city indicator variables not excluded by collinearity produced results comparable to the main model. We also tested a hierarchical linear modeling approach using city levels and robust standard errors. Results (available on request) were similar to our main model with clustered standard errors; we report the latter for ease of interpretation of the magnitude of effects.

	J	Jail Action Rate			Financial Action Rate			
		Quantile Re	gressions		Quantile Regressions			
Father Category	OLS (1)	25th (2)	50th (3)	OLS (4)	25th (5)	50th (6)		
NCF-only versus CF	127.94 (0.24)	2,262.04* (1.91)	719.93	744.30 (1.12)	-828.54 (0.47)	241.56		
NCF-orders versus CF	399.03 (0.96)	1,108.53 (1.00)	271.94 (0.58)	-163.21 (0.32)	-321.49 (0.20)	-614.91 (0.91)		
NCF-arrears versus CF	1,291.78** (2.36)	3,497.67** (2.30)	886.44 (1.38)	-534.01 (0.62)	-2,507.32 (1.27)	-1,714.65* (2.06)		

Table 4	 Annual 	Hours	Worked	Rearessions	s, Marginal	Effects v	with Jail-	Financial	Interactions

Source: Authors' calculations from the Fragile Families and Child Wellbeing Survey.

Note: Based on table A3. Jail coefficients calculated at mean level of financial sanctions, and financial coefficients calculated at mean level of jail sanctions. T-stats in parentheses. NCF = noncustodial fathers; CF = custodial fathers.

*p < .1; **p < .05; ***p < .01

(and also NCF-onlys) are disproportionately located in the bottom quartile and bottom half of the overall hours distribution. Thus, these regressions examine the part of the hours distribution where the threat may be greatest for those most vulnerable to that threat and where responsiveness to threat may be greatest as well, a point to which we will return.

The expected positive effect of jail on hours (relative to CFs) is nontrivial only at the 25th and 50th percentiles, and there it is marginally statistically significant (10 percent) only for NCF-onlys in column 3's quantile regression at median hours. Similarly, the expected effect of financial sanctions in the opposite direction, that is, a tax effect reducing hours worked, is present at the 25th and 50th percentiles for NCFs in arrears (columns 5 and 6); however, it is neither statistically significant there nor consistently present across other measures.

These models assume independence among the policy variables. However, localities can and do use multiple enforcement methods concurrently. Moreover, even the jail sanction alone aims both to increase work effort and to tax back the resulting earnings. Thus, the jail threat coefficient is likely to include competing and countervailing influences of other actions such as financial sanctions, even while controlling for these actions. To mitigate this problem, we further interact the main jail and financial enforcement action variables with one another, as well as with fathers' custodial status, as before.²² This triple interaction increases independence in the estimates of the jail enforcement variable by netting out the influence of financial sanctions. However, this comes at the cost of losing statistical power.

Table 4 presents summary results with jail and financial sanction rates interacted.23 Summing the main and interacted effects is particularly informative (Brambor, Clark, and Golder 2006). Accordingly, we report here the marginal effects of an increase in the jail sanction rate at the mean level of financial sanctioning and, vice versa, the marginal effect coefficients for financial sanction rates (jail sanctioning at mean level). These are generated from postestimation simulations of hours worked, holding the means and marginal effects of all control variables constant while allowing the values of the jail (or financial) enforcement variable to vary by its distribution at its estimated effect (coefficient).

22. To avoid excess complexity, we do not interact either jail or financial sanctions with driver's license or other sanctions.

23. Full results for all policy and father status variables are reported in table A3.

In this interacted model, we find coefficients of the marginal effect of jail sanctioning on annual hours of NCFs with arrears (relative to CFs, the reference group) to increase across the board, relative to the model that is not interacted. In the OLS model (column 1), these marginal effects coefficients are of substantial magnitude (1,291.78 hours) in the predicted positive direction and statistically significant (5 percent). Given that the policy variable scale is between 0 and 1, we can interpret the magnitude of this influence as a 10 percentage point increase in jail threat (at mean financial sanctions) predicting an increase in annual hours worked of 129 hours (several weeks' worth of full-time work) for NCFs in arrears.

The effect is dramatically larger (3,497.67 hours) and similarly significant in the 25th percentile regression shown in column 2. Substantively, this implies that a 10 percent increase in the jail sanction rate corresponds (at mean financial sanctions) to increased annual hours worked of 46 percent (350/768) at the 25th percentile versus 5 percent (89/1,748) at median hours (column 3), and 9 percent (129/1,498) at the mean, relative to hours worked at 0 percent jail sanctioning. Table A3 also shows that, across all three interacted regression models, the main jail coefficient is large and significant for NCFs in arrears, the most dramatic results being in the 25th percentile regression.

The concentration of positive effects on annual hours worked at the bottom of the hours distribution appears to reflect a combination of increased hours among those working and nonwork-work transitions.²⁴ Because fewer than 10 percent of all observations reflect zero hours, peaking at 15 percent for both NCF-only and NCF-arrears, even substantial nonwork-work transitions would not necessarily affect the 25th percentile. To the extent that nonwork-work transitions do drive increased annual hours worked, they do so primarily through work at relatively low annual hours, driving up the 25th

percentile much more than the median; a transition from zero hours to below-median hours will not affect the median. Indeed, we would expect nonwork-work transitions to result in work at the low end of the hours distribution among those working, which should drag down 25th percentile and median hours within this population. To probe this, and in contrast to our main results that include zero-hour reports, we also ran the same regression models on annual hours worked among those reporting nonzero hours. These results (not shown but available on request) show marginal effects of jail sanctioning on hours worked (among those working at all) that are small in magnitude, inconsistent in sign, and not even marginally statistically significant; this holds across all NCF categories, with and without jail-financial interactions, and across mean, 25th percentile, and median hours. As we will discuss further, this suggests, but does not establish, that our main results reflect both nonwork-work transitions and within-work hours increases.

The interacted model can be used to further disentangle the influence of the jail and financial sanctions by conducting simulations at different levels of financial sanctions. The large, negative, statistically significant jail-financial interaction coefficients (table A3, columns 1 through 3) indicate that the positive effect of jail sanctioning upon annual hours worked declines with increased financial sanctioning, as expected. Figure 1 illustrates this with the postestimation simulation of the marginal effects of jail sanctioning for all father types at low (25th percentile), medium (50th percentile), and high (75th percentile) levels of financial sanctioning, showing 95 percent confidence intervals. The simulations support the expectation that jail threat influence on hours, especially for NCFs in arrears, is more pronounced at lower levels of financial sanctioning. The coefficients (not shown separately) for the marginal effect of the jail sanction rates on hours

24. We also ran regressions with an indicator variable for any hours worked substituting for annual hours worked. The results (not shown) indicate substantial, statistically significant increases in employment for NCF-only and NCF-arrears, but not for NCF-orders, associated with increased jail sanctioning. This implies that some of the observed increase in hours among NCF-arrears is attributable to nonwork-work transitions, though we interpret these results with caution because the employment results do not track the hours results for NCF-only, which were small and insignificant in the interacted OLS model.



Figure 1. Response of Hours Worked to Jail Sanctions, by Financial Sanction Level

Source: Authors' calculations from the Fragile Families and Child Wellbeing Survey. *Note:* Based on jail-financial interacted OLS regressions, table A3. NCF = noncustodial fathers.

for the arrears group are 1,890 (low), 1,412 (medium), and 630 (high), the first two statistically significant at the 5 percent level.

More generally, figure 1 shows graphically how the jail threat influence on hours worked operates across the father type categories in the interacted model. Across all levels of financial sanctions, we see slopes of the jail threat influence on hours for NCFs-orders and NCF-onlys that are between those for NCFs-arrears and for CFs. This pattern follows the hypothesis that the jail threat influence on NCFs should be ordered by the vulnerability to that threat.

Not only are increasing rates of jail sanctioning associated with increasing hours worked for NCFs, especially those in arrears, but also the opposite is true for financial sanctioning. Figure 2 illustrates graphically (95 percent confidence intervals) the predicted margins of the jail versus financial sanction influences on hours worked for NCFs in arrears based on interacted regression results in table 4, columns 1 and 4. The figure demonstrates clearly that the marginal influences of jail or financial sanctions on hours worked move in opposite directions, consistent with expectations.

Wage Rates

The presentation of results for wage rates follows closely that for annual hours worked. Table 5 first presents summary regression results for wage rates, without jail-financial interactions, for mean, 25th percentile, and median hours in columns 1 through 3, respectively. Column 1 shows the coefficient estimate for NCFs in arrears is negative as expected and highly statistically significant (1 percent level). Furthermore, the magnitude of the influence is substantial. A 10 percentage point increase in jail enforcement threat predicts a wage rate decline of \$1.27 for NCFs in arrears (relative to CFs). We also find evidence of a substantial and statistically significant (5 percent level) tax impact of financial sanctions operating in the opposite direction for NCFs in arrears (column 4).

As with hours worked, analysis of data not shown here indicates that NCFs in arrears are concentrated in the bottom quartile and bottom half of the overall wage rate distribution. Columns 2 and 3 examine potential heterogeneity in the threat effect of jail with quantile regression estimates at the 25th and 50th percentiles of the wage distribution, respectively.



Figure 2. Response of Hours Worked to Jail Versus Financial Sanctions, for NCFs with Arrears



Source: Authors' calculations from the Fragile Families and Child Wellbeing Survey. *Note:* Based on jail-financial interacted OLS regressions, table A3. Jail coefficients calculated at mean level of financial sanctions, and financial coefficients calculated at mean level of jail sanctions. NCF = noncustodial fathers.

	Ja	il Action Rat	te	Financial Action Rate			
		Quantile F	Regressions		Quantile Regressions		
Father Category	OLS (1)	25th (2)	50th (3)	OLS (4)	25th (5)	50th (6)	
NCF-only versus CF	-6.38**	-5.86*	-10.30***	10.35**	1.23	1.97	
NCF-orders versus CF	-5.98	-1.30	-4.21	5.01	-4.14	-0.43	
NCF-arrears versus CF	(-1.66) -12.68*** (-4.41)	(-0.47) -6.47* (-1.77)	(-1.18) -8.54* (-1.83)	(0.69) 16.92** (2.87)	(-0.73) 9.75 (1.40)	(-0.06) 9.25 (1.04)	

Table 5. Wage Rate Regressions, Jail and Financial Action Rate Results

Source: Authors' calculations from the Fragile Families and Child Wellbeing Survey.

Note: Based on table A2. T-stats in parentheses. NCF = noncustodial fathers; CF = custodial fathers. *p < .1; **p < .05; ***p < .01

These results are again negative, statistically significant (10 percent levels), and substantial for NCFs in arrears. Although, in absolute terms, the magnitude of the influence is much smaller than that for the sample as a whole, the wage distribution is highly asymmetrical above and below median. For those at the 25th percentile, for instance, the roughly \$0.65 wage decrease for NCFs with arrears associated with a 10 percent point increase in jail sanctioning corresponds to a 6.8 percent decline in wage rates (from \$9.53 at 0 percent jail), relative to (again) a 6.8 percent (\$0.85/\$12.59) decrease at median wages and a 9.1 percent decrease at mean wages (\$1.27/\$14.00).

Table 6 presents summary results from re-

	J	ail Action Rat	te	Financial Action Rate			
		Quantile F	Regressions		Quantile Regressions		
Father Category	OLS	25th	50th	OLS	25th	50th	
	(1)	(2)	(3)	(4)	(5)	(6)	
NCF-only versus CF	-0.81	-2.08	-4.47	6.17	-1.50	-1.36	
NCF-orders versus CF	(0.23)	(0.47)	(0.79)	(1.62)	(0.23)	(0.16)	
	-6.62	-1.22	-4.53	5.34	-4.45	-0.57	
NCF-arrears versus CF	(0.76)	(0.29)	(0.86)	(0.57)	(0.74)	(0.07)	
	-12.25**	-5.71	-9.67	16.77**	9.74	9.39	
	(2.16)	(1.00)	(1.33)	(2.49)	(1.31)	(1.00)	

Table 6. Wage	Rate Regressions	, Marginal Effects	with Jail-Financial	Interactions

Source: Authors' calculations from the Fragile Families and Child Wellbeing Survey. *Note:* Based on table A3. Jail coefficients calculated at mean level of financial sanctions, and financial coefficients calculated at mean level of jail sanctions. T-stats in parentheses. NCF = noncustodial fathers; CF = custodial fathers.

*p < .1; **p < .05; ***p < .01

gression models that interact jail and financial sanctioning rates, showing the marginal effect coefficients for jail sanction rates (with financial sanctioning at mean level) and for financial sanction rates (with jail sanctioning at mean level). For NCFs in arrears, magnitudes change little but statistical significance declines somewhat across the board, as expected with the enlarged standard errors associated with the interacted model. Nonetheless, the similarly substantial in magnitude but opposite in sign effects for jail versus financial actions both remain significant at the 5 percent level in the OLS interacted model (columns 1 and 4). In contrast, all the significant results for NCFonlys disappear as we move from the noninteracted to interacted models.

Figure 3 again shows post-estimation simulations of the jail threat influence on mean wages for all father types at the 25th (low), 50th (medium), and 75th (high) percentiles of financial sanctions. Here, variation across levels of financial sanctioning for NCFs in arrears is not substantial. Again, the slopes for NCFs with orders and NCF-onlys largely fall between those for NCFs with arrears and CFs, except for NCFonlys at low levels of financial sanctioning.

Again comparing jail and financial sanctioning, figure 4 shows the predicted margins of their influences on wages for NCFs in arrears based on interacted regression results in columns 1 and 4 of table 6. These are nearly mirror images, moving in opposite directions, as expected.

DISCUSSION

Overall, the reported results are largely consistent with our hypothesized labor market effects of jail threat for failure to work to pay child support. We observe the distinctive combination of declining wages and increasing hours as jail threat rises. This differentiates the posited threat effect from a barrier to employment, such as past incarceration, which should depress both wages and hours. Indeed, though we have treated it mainly as a control rather than a primary object of analysis, a city's rate of suspending driver's licenses to enforce child support obligations does produce this contrasting barriers pattern (Cadigan and Kirk 2020), with consistently large, statistically significant negative effects on both hours worked and wages (tables A2, A3).

Furthermore, this pattern of more work at lower wages appears specifically in the differential response to jail rates of NCFs in arrears (relative to CFs), consistent with their greater vulnerability to jail threat relative not only to CFs but also to other NCFs. The response is particularly strong among those with lower hours, consistent both with where the threat would be concentrated and where it would most likely produce the strongest response. This jail threat response also interacts with the intensity of fi-



Figure 3. Response of Wages to Jail Sanctions, by Financial Sanction Level

Source: Authors' calculations from the Fragile Families and Child Wellbeing Survey. *Note:* Based on jail-financial interacted OLS regressions, table A3. NCF = noncustodial fathers.



Figure 4. Response of Wages to Jail Versus Financial Sanctions, for NCFs with Arrears

Source: Authors' calculations from the Fragile Families and Child Wellbeing Survey. *Note:* Based on jail-financial interacted OLS regressions, table A3. Jail coefficients calculated at mean level of financial sanctions, and financial coefficients calculated at mean level of jail sanctions. NCF = noncustodial fathers. nancial sanctioning, operating most strongly where payment collection efforts are weakest.

Relatedly, jail and financial sanctioning consistently exhibit effects that operate in opposite directions, for both hours and wages. Again, this is theoretically consistent with competing threat and tax mechanisms of influence. Indeed, this may help explain the heretofore surprisingly weak evidence (Miller and Mincy 2012) of the expected work disincentives in analyses that model child support enforcement exclusively as a tax. Such offsetting effects are consistent with how work requirements are sometimes thought to enable policymakers to counteract work disincentives associated with a tax, but variation in the relative intensity of jail versus financial sanctioning highlights the difficulty of calibrating such offsetting, including the risk that jail threats might substantially overcorrect.

This confluence of several distinct, albeit related, theoretically predicted results-the effects of jail threat on both hours and wages, their concentration among NCFs in arrears and particularly those with the lowest hours, and the interaction with levels of financial sanctioning that operate in the opposite direction-enhances our confidence in their validity. That said, not all our results clearly support the predicted effects of jail threat. None of our measures show any statistically significant effect on NCFs with orders but not arrears. The NCF-only group responds to jail threat in ways broadly similar to NCF-arrears in the models without jail-financial interactions, but with that interaction added, this similarity persists only for the hours response among those with relatively low hours. Although they suggest some jail threat effect, it is unclear how to interpret the results for NCFs without arrears. NCF-only may be a particularly heterogeneous group. Some could be quite unresponsive to the sanction regime because they have stable earnings and pay regularly (weakening incentives to establish an order through the formal child support system). Others with marginal employment prospects may lack orders because of disconnection from custodial mothers and state systems or the limited prospect of collecting support, and these fathers might be more responsive to intensified enforcement.

Some ambiguity in how to interpret our re-

sults arises from our measurement of hours and wages specifically in formal employment. The National Research Council has suggested that findings of increased hours of work under parole supervision (Pettit and Lyons 2007; Sabol 2007; Seim and Harding 2020) might reflect work requirements pushing parolees to substitute formal (measured) for informal (unmeasured) work (Travis, Western, and Redburn 2014). Such informal-formal transitions do not intrinsically explain the additional association between increased formal hours and decreased wages that Becky Pettit and Christopher Lyons found for parolees (2007) and that we find here for NCFs with arrears. The theory could, however, be extended to posit that such transitions also sacrifice a wage premium for informality.

We doubt that informal-formal shifts explain our results. This mechanism requires pressure toward formality to a degree that outweighs the hypothesized diminished ability to pay support associated with a lost wage premium. This is less plausible in the child support context where payments provide an independent way to satisfy work obligations, whereas documentation of work may be more independently important in the parole context. Addressing the issue empirically is challenging because Fragile Families data does not include a single, consistent measure of total annual informal hours, and its data on hours in subtypes of informal work cannot reliably be aggregated. Nonetheless, we did run our regressions on individuals' largest annual hours worked in any one subtype of informal work, which provides a reasonable proxy for overall informal hours. Differences in informal hours between CF and NCF-arrears as a function of jail rate were trivial in size and not remotely statistically significant. This is difficult to square with substitution away from informality driving the observed effects on formal hours.

Our results also leave open some questions about the mechanisms by which jail threat influences hours and wages, with potentially important implications both empirically and in policy significance. As noted, transitions from nonwork to low-hours work clearly play a significant role. In theory, these transitions could explain the entire increase in annual hours worked. If those entries are into lower-wage work, they could explain the wage effects as well. However, we doubt that is the case for two reasons.

First, as noted, jail threat's association with increased annual hours worked among the entire population coexists with having no effect on the distribution of hours among those reporting at least some work. The latter is inconsistent with the hypothesis that the former is driven exclusively by nonwork-work transitions. That is because such transitions concentrated in the bottom of the entire hours distribution should cause the 25th and 50th percentiles of hours to fall among the subset of those working, even though hours increase among the entire population. We anticipate that effect empirically because the NCF-only and NCF-arrears groups are overrepresented in the bottom of the hours distribution among those working; so, too, are those with relatively low wages. In addition, we would expect un- and underemployment to correlate with weaker employment prospects. Thus, those transitioning into work in response to jail threat would be more likely to move into less steady jobs with relatively low hours. But no such downward shift in hours among the working population is observed in association with the jail threat that drives these NCFs into work.

Instead, stasis in the hours distribution among those working is more consistent with a combination of nonwork-work transitions and within-work hours increases. Both would drive overall increases in hours worked. The latter, however, necessarily would also shift the hours distribution upward among those working. This, in turn, would offset the former's tendency to shift that distribution downward. Such a combination, then, would reconcile our findings with regard to hours worked among both the entire population and the working population.

Second, a mix of nonwork-work transitions and within-work hours increases is more plausible in theory. Recall that our hours indicator is measured over a year. It would be surprising if people with zero annual weeks of work responded strongly to jail threat by adding hundreds of additional work hours, but those with, say, five or six annual weeks of work were unmoved.

Ultimately, with Fragile Families data we

cannot decompose those working into those who would not have worked absent jail threat and those who would have worked regardless. Accordingly, we cannot directly measure the relative contributions of these two mechanisms to increasing hours.

Similarly, we cannot determine the extent to which wage declines are driven by hours increases. Lower wages could be associated with added hours, if workers are driven by the jail threat to accept less attractive jobs they otherwise would have declined. But wages also could decline within hours that would have been worked even absent the jail threat, because workers' ability to bargain for or switch jobs for higher wages is undermined by their increased vulnerability to sanctions for periods of un- or underemployment.

Future research addressing these empirical questions about nonwork-work transitions versus within-work changes could affect our findings' policy implications. Entry into employment, or increased hours up to a point, might be considered an unalloyed good. In contrast, within-individual wage losses at constant hours are more unambiguously troubling. In the context of child support enforcement specifically, such losses would suggest that efforts to increase some noncustodial parents' hours worked as a way to increase their ability to pay also could, through downward wage pressure, have the perverse effect of undermining other noncustodial parents' ability to earn and pay. Indeed, the availability of new entrants at low wages would be expected to put downward wage pressure on those who would be working regardless.

More generally, the "right to quit" provides an important self-help mechanism, aside from direct legal regulation, in combating abusive or exploitative working conditions (Pope 2010). Accordingly, forced entry into employment at rock bottom wages might be viewed as independently concerning, even though increased work and earnings have their upsides.

Insofar as increased hours are seen as a policy gain, all else equal, different policy tools might achieve that gain. Using the threat of jail to counteract un- and underemployment by lowering reservation wages (and other dimensions of job quality) brings potential costs in downward pressure on labor standards at the bottom of the labor market, exposure to the risk of incarceration, and diminished autonomy at work (Zatz 2019). Employment increases might also be achieved by raising the returns to work rather than intensifying the costs of nonwork (Sorensen 2010).

For these reasons, the potential for increased work to enable greater support payments (Dwyer Emory et al. 2020) does not settle the question, especially when increased work is linked to lower wages rather than enhanced earnings capacity. Furthermore, although increased support payments can enhance the well-being of low-income households with children of noncustodial parents, private intrafamily transfers are not the only way to do so (Brito 2012). Nor do increased payments necessarily deliver such benefits, given that they often are captured by the state to offset expenditures on public assistance.

LIMITATIONS

One may worry that our results could be an artifact of the mechanical problem that enforcement actions can only be taken against NCFs in arrears. This concern is tempered by further analysis of the data regarding enforcement actions by type cross-tabbed by fathers' custodial type categories (table A1). City-based threat levels are uniformly experienced across father category. We also observe some statistically significant results for NCF-only, in the same direction as NCF-arrears, which suggests the operation of some threat effect against those who have not directly experienced any formal enforcement action.

The NCF-only group, with no orders established, also raises the possibility that NCFs could respond to jail threat with intensified efforts to avoid enforcement, not with increased compliance. This could manifest in differential survey attrition. If such attrition correlated with difficulty complying, it could inflate hours of work among those responding. However, this mechanism would also predict an association between greater jail threat and increased reported wages, the opposite of what we find.²⁵ More generally, Fragile Families data are limited by substantial survey attrition. Any bias this introduces, however, should be toward null results. That bias would result from attrition concentrated among those with weaker labor market prospects, where we expect stronger response to jail threat, and among later waves, where enforcement threat likely increases with focal child age. This is in addition to other potential sources of statistical noise already noted. The attrition limitations of Fragile Families also mean our results are most persuasive in regard to the existence of the jail threat influence on hours and wages, and less so in regard to its precise magnitude.

Finally, as a study of work requirements enforced by jail threat, this study is limited by contextual features of child support enforcement. Because the underlying obligation is to pay, the work requirements are sometimes less direct and are intertwined with the financial disincentives of a tax. If anything, this should dampen our results, as our jail-financial interactions suggest, relative to a pure work requirement. This conjunction of work and payment obligations, however, is itself an important policy phenomenon, not only in child support but also in criminal fines and fees, as well as in probation and parole requirements to pay both child support and criminal legal debt (Zatz 2019).

CONCLUSION

Our findings suggest how criminal legal institutions can drive people (deeper) into the labor market under deteriorating conditions. A similar mechanism may also operate outside the child support enforcement context, in other domains with work requirements enforced by potential incarceration, including criminal legal supervision generally and court debt specifically. This phenomenon implies that empirical and policy focus on how incarceration erects barriers to employment is importantly incomplete, and it highlights the value of analyzing the quality of work, not only its quantity.

25. As noted separately, we also find little evidence of such differential attrition. Enforcement avoidance also might lead to differential out-migration among those still responding. This would seem only to introduce noise, however; such relocations also are relatively rare.

Table A1. Summary Statistics

	Overall	CF	NCF-Only	NCF-Orders	NCF-Arrears
	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)	Mean (SD)
Proportion of sample	100%	44.01%	17.48%	23.75%	12.76%
Individual-level variables					
Wage rate	14.7	17.1	12.0	13.5	11.7
	(8.9)	(10.1)	(6.9)	(7.3)	(6.7)
Annual hours	1,728	1,936	1,445	1,709	1,397
	(846)	(713)	(918)	(867)	(921)
African American	46%	28%	60%	61%	65%
	(50%)	(45%)	(49%)	(49%)	(48%)
Hispanic	28%	36%	23%	22%	20%
	(45%)	(48%)	(42%)	(42%)	(40%)
Other	4%	5%	4%	4%	4%
	(20%)	(21%)	(20%)	(20%)	(19%)
Father age	28	29	26	29	26
	(7)	(7)	(7)	(7)	(7)
U.S. born	83%	74%	87%	90%	96%
	(38%)	(44%)	(33%)	(30%)	(19%)
High school grad	35%	26%	41%	44%	42%
	(48%)	(44%)	(49%)	(50%)	(49%)
Some college	23%	26%	17%	23%	19%
	(42%)	(44%)	(38%)	(42%)	(40%)
College grad	11%	19%	4%	4%	2%
	(31%)	(39%)	(20%)	(19%)	(13%)
Not working: jail	2%	0%	/%	2%	5%
	(15%)	(2%)	(25%)	(14%)	(21%)
Not working: school	(6%)	(6%)	(7%)	(6%)	(6%)
Ever convicted	(15%) 26%	(13%)	(16%)	(18%)	(16%)
Ever incarcerated	(44%)	(36%)	(49%) 55%	(45%)	(49%)
	(49%)	(43%)	(50%)	(50%)	(46%)
City-level variables					
Jail action rate	7%	7%	7%	8%	8%
	(7%)	(7%)	(7%)	(7%)	(7%)
Financial action rate	8%	7%	7%	8%	8%
	(4%)	(4%)	(4%)	(4%)	(4%)
Driver's license action rate	3%	3%	2%	3%	3%
	(2%)	(2%)	(2%)	(3%)	(2%)
Other actions rate	6%	6%	6%	7%	7%
	(3%)	(3%)	(3%)	(3%)	(3%)
Foreign-born	13%	15%	13%	13%	11%
	(11%)	(12%)	(10%)	(10%)	(9%)
African American	16%	15%	16%	17%	17%
	(9%)	(8%)	(8%)	(9%)	(9%)
Hispanic	16%	18%	15%	16%	14%
	(18%)	(19%)	(17%)	(19%)	(18%)
Mean income	75,513	75,476	75,621	75,765	75,033
	(14,475)	(15,038)	(13,603)	(14,471)	(13,555)
Metro population	2,862,211	2,883,260	3,052,477	3,006,180	2,257,652
	(3,519,790)	(3,575,541)	(3,510,689)	(3,683,314)	(2,904,356)
Unemployment rate	5.6%	5.5%	5.2%	5.8%	6.0%
	(1.9%)	(1.9%)	(1.6%)	(1.9%)	(1.9%)

Source: Authors' calculations from the Fragile Families and Child Wellbeing Survey and the Current Population Survey. *Note:* City level controls based on CPS metropolitan area data, 1998 through 2010, averaged across individual wave years, except metro population ages eighteen through sixty-four and unemployment rate based on Fragile Families data by sample city MSA, averaged across month of interview. CF = custodial fathers; NCF = noncustodial fathers.

	Annual Hours Worked			Wages			
Enforcement Action		Quantile I	Regressions		Quantile F	Regressions	
Rates and Father	OLS	25th	50th	OLS	25th	50th	
Categories	(1)	(2)	(3)	(4)	(5)	(6)	
NCF-only	-250.64***	-417.49***	-191.16***	-2.41***	-0.50	-0.99	
	(-5.61)	(-2.95)	(-3.35)	(-6.02)	(-0.95)	(-1.47)	
NCF-orders	-49.76	-122.47	-3.41	-1.36*	0.16	-0.47	
	(-0.76)	(-0.90)	(-0.06)	(-2.00)	(0.32)	(-0.72)	
NCF-arrears	-185.36*	-478.71**	-130.95*	-1.98***	-1.02	-1.39	
	(-1.91)	(-2.39)	(-1.78)	(-3.18)	(-1.50)	(-1.60)	
Jail	32.93	201.54	-276.86	4.71	0.60	3.66	
	(.17)	(0.42)	(-1.44)	(1.62)	(0.34)	(1.61)	
× NCF-only	116.58	1,244.52	633.55*	-6.38**	-5.86*	-10.30***	
	(0.46)	(1.50)	(1.90)	(-2.32)	(-1.89)	(-2.61)	
× NCF-orders	10.73	254.57	308.37	-5.98	-1.30	-4.21	
	(0.05)	(0.34)	(1.02)	(-1.66)	(-0.47)	(-1.18)	
× NCF-arrears	19.50	880.00	378.27	-12.68***	-6.47*	-8.54*	
	(0.03)	(0.90)	(0.96)	(-4.41)	(-1.77)	(-1.83)	
Financial	113.58	434.46	901.91*	-9.99	-1.26	-2.89	
	(.20)	(0.36)	(1.86)	(-1.24)	(-0.28)	(-0.50)	
× NCF-only	758.29	-318.74	235.03	10.35**	1.23	1.97	
	(1.26)	(-0.19)	(0.36)	(2.65)	(0.20)	(0.25)	
× NCF-orders	106.21	584.71	-650.48	5.01	-4.14	-0.43	
	(0.20)	(0.39)	(-1.07)	(0.69)	(-0.73)	(-0.06)	
× NCF-arrears	200.80	-1,060.39	-1,171.45	16.92**	9.75	9.25	
	(0.25)	(-0.57)	(-1.56)	(2.87)	(1.40)	(1.04)	
Driver's license	-1,137.90*	-1,561.82	-1,195.62**	8.12	-6.15	2.53	
	(-2.10)	(-1.19)	(-2.25)	(0.86)	(-1.25)	(0.40)	
× NCF-only	750.92	622.18	1,165.48	-6.14	4.95	1.88	
	(1.04)	(0.28)	(1.30)	(-0.88)	(0.59)	(0.18)	
× NCF-orders	705.53	920.88	1,100.54	-7.65	2.99	-4.77	
	(0.94)	(0.46)	(1.37)	(-0.81)	(0.40)	(-0.50)	
× NCF-arrears	-3,780.82**	-5,951.37**	-885.43	-23.88**	-7.52	-25.82**	
	(-2.30)	(-2.33)	(-0.86)	(-3.28)	(-0.79)	(-2.12)	
Other actions	226.87	742.80	121.59	-6.20	4.89	-1.37	
	(0.60)	(0.81)	(0.33)	(-1.07)	(1.43)	(-0.32)	
× NCF-only	-718.65	-515.64	-673.49	8.93*	-4.41	3.32	
	(-1.13)	(-0.32)	(-1.04)	(2.07)	(-0.73)	(0.43)	
× NCF-orders	-96.43	-649.71	-19.58	2.95	-7.35	-1.28	
	(-0.15)	(-0.46)	(-0.03)	(0.41)	(-1.39)	(-0.19)	
× NCF-arrears	423.58	1,599.41	-586.49	1.05	-7.88	1.94	
	(0.43)	(0.87)	(0.79)	(0.20)	(-1.15)	(0.22)	
R ² /pseudo-R ²	0.256	0.230	0.132	0.356	0.144	0.211	

Table A2. Annual Hours Worked and Wage Rate Regressions, Without Jail-Financial Interactions

Source: Authors' calculations from the Fragile Families and Child Wellbeing Survey.

Note: N = 8,362. T-stats in parentheses. NCF = noncustodial fathers. Reference group is custodial fathers. *p < .1; **p < .05; ***p < .01

	Annual Hours Worked			Wages			
Enforcement Action		Quantile R	egressions		Quantile I	Regressions	
Rates and Father Categories	OLS (1)	25th (2)	50th (3)	OLS (4)	25th (5)	50th (6)	
NCF-only	-250.13***	-564.69***	-214.55***	-3.10***	-0.86	-1.81**	
NCF-orders	(-4.20) -97.25 (-1.25)	(-3.22) -225.95 (-1.24)	(-2.89) -3.74 (-0.05)	(-6.00) -1.30* (-1.75)	(-1.31) -0.19 (0.20)	(-2.16) -0.39	
NCF-arrears	-349.15*** (-3.91)	-754.23*** (-3.19)	-334.20*** (-3.35)	(-2.63) (-2.63)	-1.19 (-1.34)	-1.17 (-1.04)	
Jail	-495.15	-383.57	-179.42	5.97	-0.16	5.18	
× NCF-only	(-1.19) 100.82 (0,10)	(-0.25) 4,195.34* (1.72)	(-0.28) 851.86 (0.83)	(0.51) 8.86 (1.22)	(-0.03) 3.64 (0.40)	(0.71) 5.83 (0.50)	
× NCF-orders	1,008.04 (1.20)	2,669.37 (1.19)	252.60 (0.27)	-7.61 (-0.44)	-1.79 (-0.21)	-5.05 (-0.47)	
× NCF-arrears	3,200.70*** (2.90)	6,949.58** (2.25)	3,086.97** (2.37)	-11.57 (-1.02)	-4.59 (-0.40)	-10.89 (-0.74)	
Financial	-29.06	-36.78	912.03	-9.17	-1.49	-2.23	
× NCF-only	(-0.05) 717.71 (1.10)	(-0.03) 1,067.06	(1.63) 370.91 (0.49)	(-1.08) 15.64*** (4.28)	(-0.30) 4.11 (0.61)	(-0.35) 8.73	
× NCF-orders	(1.10) 433.93 (0.75)	(0.39) 1,208.91 (0.73)	-633.87 (0.49)	(4.20) 4.37 (0.60)	(0.01) -5.01 (-0.80)	(1.02) -1.08 (-0.14)	
× NCF-arrears	1,337.69 (1.60)	(0.7.2) 877.27 (0.41)	442.96 (0.49)	17.43** (2.69)	10.84 (1.36)	8.19 (0.81)	
Jail × financial	4,163.15	4,947.45	-715.84	-9.30	4.48	-12.76	
× NCF-only	(1.30) 357.97 (0.05)	-25,519.38 (-1.34)	(-0.13) -1,741.37 (-0.22)	(-0.12) -127.59** (-2.57)	-75.54	-135.85 (-1.50)	
× NCF-orders	-8,038.92	-20,602.95	255.21	13.09	7.58	6.84	
× NCF-arrears	-25,197.61** (-3.11)	-45,564.80* (-1.96)	-29,046.71** (-2.96)	-8.98 (-0.11)	-14.74 (-0.17)	16.10 (0.15)	
Driver's license	-1,248.65**	-1,303.82	-1,116.60*	8.30	-6.36	2.21	
× NCF-only	(-2.13) 757.19 (1.06)	(-0.96) 1,678.45 (0.74)	(-1.95) 1,222.81 (1.27)	(0.89) -2.67 (-0.54)	(-1.25) 7.84	(0.34) 4.10 (0.28)	
× NCF-orders	900.34	(0.74) 1,426.99 (0.71)	(1.27) 1,103.86 (1.30)	-7.51 (-0.87)	(0.32) 3.19 (0.42)	-4.07 (-0.42)	
× NCF-arrears	-3,345.50* (-2.05)	-4,373.76* (-1.70)	-649.93 (-0.60)	-23.43*** (-3.20)	-7.18 (-0.74)	-25.79** (-2.10)	
Other actions	243.23 (0.65)	706.91 (0.78)	100.99 (0.26)	-6.33 (-1.09)	4.65 (1.37)	-1.04 (-0.24)	
× NCF-only	-691.77	-1,347.95	-529.79	8.06**	-5.99	2.34	
× NCF-orders	(-1.08) -129.39 (-0.20)	(-0.84) -724.89 (-0.52)	(-0.78) 25.79 (0.04)	(2.23) 3.27 (0.46)	(-0.99) -6.71 (-1.27)	(0.31) -1.96 (-0.29)	
× NCF-arrears	359.89	1,293.52	1,422.71*	1.33	-7.21	1.39	
R²/pseudo-R²	0.256	0.231	0.133	0.356	0.144	0.211	

Table A3. Annual Hours	Worked and Wag	e Rate Regressions	, with Jail-Financial	Interactions
		e e		

Source: Authors' calculations from the Fragile Families and Child Wellbeing Survey.

Note: N = 8,362. T-stats in parentheses. NCF = noncustodial fathers. Reference group is custodial fathers. *p < .1; **p < .05; ***p < .01

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