

Essays on Child Support Enforcement and Tax Evasion

by

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Chapter 1

Introduction

The three essays in this dissertation explore the impact of state child support enforcement laws and state earned income tax credits on fathers' contact with their children; their participation in regular work and work in the informal, untaxed sector of the economy; and whether they substitute between informal and formal child support in response to "taxes" on formal support. They share common themes in that they each use state and time variation in child support and tax policies to examine men's behavioral responses.

Chapter 2 argues that child support enforcement laws that require employers to withhold child support from paychecks provide an exogenous source of variation in child support payment methods. If payment method (withholding vs. father-initiated payment) does not affect behavior, child support withholding should change neither the amount of in-kind support paid nor the amount of parent-child contact for parents who pay full support in the absence of withholding. In contrast, salience models predict that both additional support and contact will change in response to changes in payment mechanism, and household bargaining models suggest that child support withholding transfers bargaining power from fathers to mothers and will decrease fathers' access to their children. I examine how father-child contact and payment of in-kind support vary by payment method by instrumenting for child support withholding status using variation in the timing of state withholding laws. I find that, for fathers who do not owe back support, withholding reduces the amount of time fathers spend with their children and the amount of in-kind support they provide. These results are consistent with a salience model in which changing the payment method changes the salience of the child's consumption.

In Chapter 3, I examine the effects of state policies that granted taxfilers an additional percentage of the federal Earned Income Tax Credit (EITC). State and federal

EITCs subsidize regular employment and make it relatively more attractive compared to off-the-books work. I use variation in state EITCs between tax years 1997-2005 to identify regular and informal-sector labor supply responses for single men in the Fragile Families longitudinal data. In contrast to previous research on single mothers' labor supply, which finds large participation effects and no hours effects in the regular sector, I find that among unmarried urban men with at least one child born out-of-wedlock an increase in a state EITC of an additional one percent of the federal EITC has large and significant negative effects on participation in informal work but no significant effect on participation in the regular sector. Usual hours worked per week increase in the regular sector and decrease in the underground sector in response to increases in the state EITC. The large magnitudes of the effects and the presence of the effects for men under and over age 25 suggest that many unmarried male taxpayers may claim EITC qualifying children.

Chapter 4 examines whether states' elimination of welfare disregard rules for child support payments, which resulted in higher effective taxes on non-custodial parents' child support payments, caused parents to substitute between formal and informal forms of child support payment. Using state-level policy variation in child support disregard policies after the 1996 welfare reform, I find that a \$100 decrease in the disregard corresponds to a 4.1 percent increase in the probability a child will receive in-kind child support. In addition, I use the only large-scale panel dataset that spans welfare reform to examine characteristics of the child support survey population and find turnover of about one third of the child support sample between waves.

Together, these three essays expand our understanding of how men (particularly low-income and minority men) respond to enforcement policies intended to increase their rates of formal child support payment and regular labor supply. These policies have numerous additional, sometimes unintended, consequences. Child support withholding policies are heralded as an extraordinarily effective way to enforce child support orders but they also appear to decrease father-child contact among men who appear to be responsible fathers. State earned income tax credits that are a function of the federal credit affect the relative attractiveness of claiming zero, one, or two qualifying children when filing for the EITC and they also affect the relative value of working in the regular sector relative to the underground sector. State policies regarding how child support income is disregarded when welfare benefits are calculated affect the decision to pay support formally or informally. Policy analysis should include both intended and unintended effects. This dissertation is an effort in that direction.

Chapter 2

Salience and Payment Methods: The Effect of Child Support Withholding on Father-Child Contact

2.1 Introduction

In December 2007, the Federal Reserve announced that more than two-thirds of non-cash payments are now made electronically. Check-writing declined by an average of 6.4 percent per year between 2003 and 2006, while automated clearinghouse and debit card payments grew at more than 17 percent annually. In a Mastercard Worldwide (2006) survey, consumers cited convenience, stress relief from knowing bills will be paid on time, and rewards incentives as factors that make automatic payment methods like debit and credit card use and online bill pay attractive. Yet, more than a quarter of respondents also listed reluctance to lose control as a reason to resist debit card use, which suggests that electronic payments not only bring convenience but may also affect behavior or understanding of financial matters. With rapid changes in the use of different payment types, it would be helpful to understand whether consumers do, in fact, “lose control” and change their spending habits when they switch to new payment methods.

This paper exploits an exogenous source of variation in payment method: the government’s use of income withholding to collect child support. While most child support payments were once made directly from the noncustodial parent to the child’s guardian, now the majority of child support payments are withheld from noncusto-

dial parents' pay. This change in payment method is the result of state child support enforcement laws that were implemented during the 1980s and 1990s and required automatic child support withholding in all child support cases. Child support withholding is an interesting source of variation in payment method for several reasons. Income withholding payment methods are likely to be similar to automatic bill pay in the degree to which individuals are conscious of possessing, and losing, the money. Since (for fathers who paid full child support in the absence of enforcement) the law changes affect only the method of payment, not the amount, the results can be interpreted as responses to payment method. Additionally, child support withholding allows us to look at spillover effects of payment method onto unconstrained behaviors: payment method should not change the amount of leisure time available to the father, but if payment method has spillover effects, it may affect the amount of contact between parents and children.

I examine the effects of child support payment methods on in-kind support and parent-child contact. Although standard economic theory assumes that payment method is neutral so long as it does not affect the amount or timing of payment, research in psychology and economics offers several reasons that payment method may not be neutral. Payment method may affect the "mental account" from which agents draw their consumption (Prelec and Loewenstein 1998, Thaler 1999). It may affect the salience, or visibility, of payment, or consumers' awareness of their account balances. Empirically estimating the effect of payment method on behavior is difficult because payment method is generally endogenous; more educated, higher-income consumers with greater financial knowledge are more likely to use electronic payments and choice of payment type may depend in part on liquidity constraints or on factors that affect prices such as reward incentives offered by credit card companies. Recent experimental work on payment method by Prelec and Simester (2001) and Soman (2003) indicates that willingness to pay increases when credit cards replace cash, but there is very little work on the effect of payment method outside of experimental work or results from surveys asking about hypothetical scenarios.

I first develop a standard model of child support in which fathers decide how much support to pay and how much time to spend with their children: in this model, payment method does not affect parents' optimal choice of support amount or child contact holding the size of the child support obligation constant. I then contribute to the literature on modeling salience by developing two variations on salience models in which different payment types may affect either the salience (or visibility) of payment or the salience of the child's consumption. In these models, payment method affects

salience and in turn affects optimal choices of in-kind support and contact.

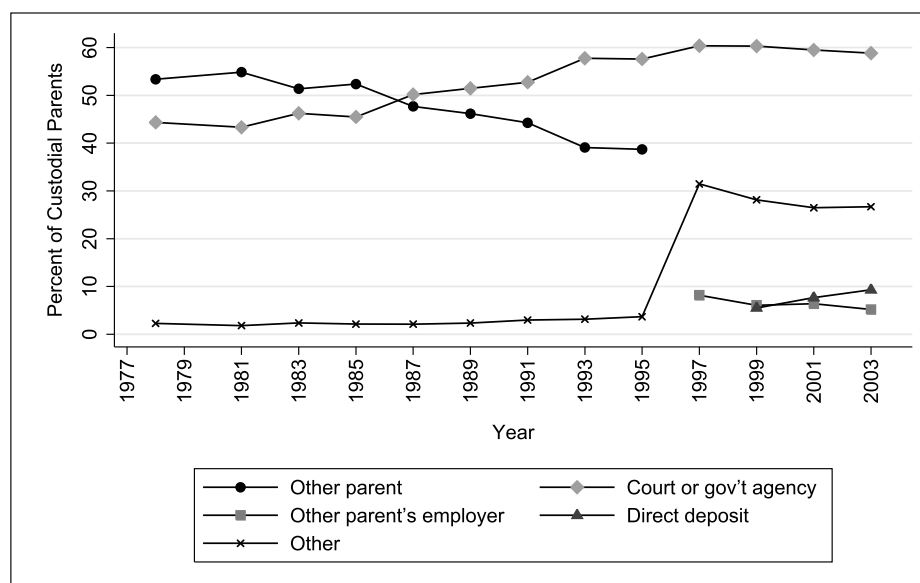
To test whether payment method affects behavior, I use six years of data from the Current Population Survey Child Support Supplement to examine the relationship between child support payment method and two outcomes: parent-child contact and parents' contributions to five categories of in-kind or additional support. I instrument for withholding using variation in state laws mandating income withholding, and I explore which cases contribute most heavily to the IV estimates. The instrumental variables results show that, for parents who are not in arrears, parents subject to withholding have significantly less contact with their children and are less likely to provide additional support. These results are inconsistent with a standard model of child support in which payment method is neutral. The results are consistent with a salience model in which child support payment method affects not only the salience of payment but the salience of children's consumption.

The paper proceeds as follows. Section 2.2 describes the policy environment surrounding introduction of income withholding for child support enforcement. Section 2.3 presents a standard model of parents' choice of support levels and child contact under child support enforcement and develops two salience models in which either salience of payment or salience of child's consumption impact choice. Section 2.4 discusses the implications of the models for empirical work and Section 2.5 presents the data and methodology used to evaluate the effects of withholding on parent-child contact and in-kind support. Section 2.6 discusses the results and sensitivity tests. Section 2.7 concludes.

2.2 A History of Child Support Withholding Policies

Prior to implementation of wage withholding, noncustodial parents made child support payments by check or similar payment either to the custodial parent or to a state agency such as a court or welfare office. The Current Population Survey Child Support Supplement collects data about the route by which custodial parents receive child support payments, as shown in Figure 2.1. Until 1985, more than half of child support recipients reported receiving child support payments directly from the non-custodial parent, while around 45 percent of parents received child support through a child support or welfare agency or through the court. Child support payments passed through welfare agencies when the custodial parent received welfare because states

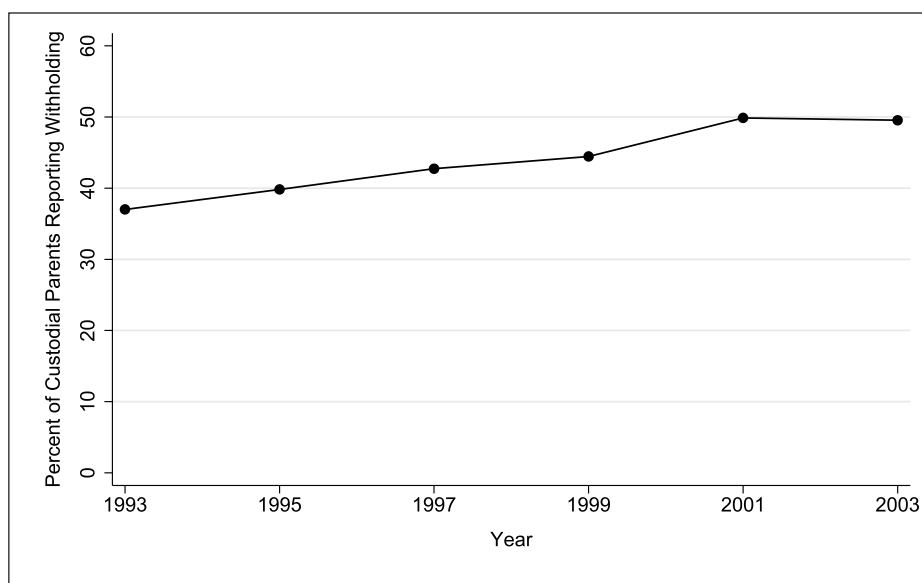
Figure 2.1: Methods by Which Custodial Parents Report Receiving Child Support Payments, 1979-2004 CPS Data



retained the majority of child support payments in welfare cases to offset welfare expenditures. Also, noncustodial parents often paid through courts or child support agencies in order to document payment history.

In the late 1980s the proportion of custodial parents who receive child support directly from the noncustodial parent began to decline. This was largely due to enactment of federal and state laws requiring income withholding of child support. Withholding laws had two main purposes. First, they were intended to improve child welfare by increasing child support collections and raising children's standard of living. Second, states hoped that withholding would offset welfare expenditures by improving child support collections from obligors in cases where the custodial parent received Aid to Families with Dependent Children. The federal Child Support Enforcement Amendments of 1984 required states to withhold the wages of noncustodial parents who fell more than one month behind in their child support payments. By 1987, all states had passed wage withholding laws for delinquent parents (Huang, Kunz and Garfinkel 2002). The federal Family Support Act of 1988 required states to implement withholding for all welfare cases by 1990 and for all child support orders, regardless of payment history or welfare status, by 1994. By 1990, 48 states and the District of Columbia had passed laws requiring withholding in welfare cases. Forty-five states and the District of Columbia had laws requiring withholding in all cases by the 1994 deadline: these laws are referred to in the literature as universal withholding laws.

Figure 2.2: Percent of Custodial Parents Who Report That Child Support Payments Are “Supposed to Be” Deducted from Paycheck, 1994-2004 CPS Data



The only legal exceptions to withholding are for good cause (especially if the custodial parent fears violence if a withholding order is issued) or (in some states) when parents mutually agree on alternative arrangements.¹ These universal withholding laws changed the default payment method for fathers who paid full child support and were not subject to other forms of enforcement. Because withholding laws were not retroactive and applied only to child support orders issued or modified after the passage of the law, they did not change the payment method in individual cases, but they do provide exogenous variation in the distribution of payment method.

Figures 2.1 and 2.2 provide evidence that implementation of these laws was effective and did alter payment methods. The fraction of child support payments received directly from the noncustodial parent declined to 38.7 percent in 1995, after which the survey question was revised and this answer option was eliminated. The trends shown in Figure 2.1 suggests that the majority of responses in the “Other” category after 1995 correspond to payments received directly from the noncustodial parent, and these responses decline steadily as the fraction of child support cases subject to withholding laws increased with time. Beginning with the 1994 CPS, custodial parents were also asked whether child support payments were deducted from the noncustodial parent’s paycheck. Figure 2.2 shows that the percentage of parents with

¹See Appendix Table 2.13 for the dates that wage, immediate, and universal withholding laws were passed by state.

child support cases with withholding increased steadily from 37 percent in 1993 until leveling off at 50 percent in 2001.²

Implementation of the withholding laws appears to vary greatly across states. The first column of Table 2.1 indicates the percentage of cases in the CPS to which withholding should apply, either because of back support due, custodial parent's welfare status, or universal withholding law. The second and third columns show the percent of cases with withholding orders by whether the law and observed case characteristics (welfare and arrears status) indicate withholding should apply. In all states, withholding rates are higher for cases in which the data clearly indicate withholding laws apply, but many cases for which withholding is indicated do not appear to have withholding.³ We cannot tell from the data why some parents do not have withholding orders. They may be self-employed, unemployed, or actively evading child support enforcement; a judge may have chosen to make an exception for good cause; or the custodial parent may simply have poor information about how child support is paid.

Withholding orders are issued by courts or child support agencies and sent to non-custodial parents' employers. All employers are required to comply with the order and withhold the full required amount. Withholding may be less than the the full child support obligation if income is low; state rules limit the percentage of income that may be garnished from pay. Withholding may also be higher than expected based on the monthly child support obligation if the father is in arrears. Child support receives first priority among wage garnishments with the exception of federal IRS tax levies. In some states, employers may charge employees a fee to process child support withholding on their behalf. Employers may not refuse to hire or terminate employees because of child support withholding requirements.

²In fact, the survey asks whether payments were "supposed to be" deducted from the noncustodial parent's paycheck. The "supposed to" language is used throughout the survey to ask custodial parents about the legal requirements of the support order regardless of whether any support was actually received. Consequently, I believe the correct interpretation of the question is whether the custodial parent believes that a court or government agency issued a current withholding order to the noncustodial parent's employer, and I use this measure throughout the paper as my measure of payment method.

³Withholding laws may apply to cases in the third column if the noncustodial parent owed back support or the custodial parent was on welfare in the past. The CPS reports only current arrears and welfare status.

Table 2.1: States' Implementation of Universal, Immediate, and Wage Withholding Laws as Reported By Custodial Parents

	Percent of Cases				Percent of Cases			
	Law Applies	If Law Applies: Have Order	If Law Does		Law Applies	If Law Applies: Have Order	If Law Does	
			Not Apply: Have Order	Not Apply: Not Have Order			Not Apply: Have Order	Not Apply: Not Have Order
Alabama	59.2	53.5	40.7	Montana	72.5	48.6	26.2	
Alaska	51.1	44.9	32.5	Nebraska	61.1	49.6	35.3	
Arizona	85.3	62.5	37.0	Nevada	58.9	35.5	26.7	
Arkansas	72.3	48.6	31.0	New Hampshire	68.8	38.9	32.2	
California	79.0	42.9	27.5	New Jersey	75.3	42.1	31.3	
Colorado	58.1	37.0	30.3	New Mexico	54.8	44.4	24.8	
Connecticut	97.3	33.0	25.0	New York	52.0	45.5	43.5	
Delaware	57.1	56.8	58.2	North Carolina	57.7	45.5	41.3	
District of Columbia	42.7	50.0	44.9	North Dakota	79.8	53.1	22.6	
Florida	91.6	39.3	29.8	Ohio	58.3	65.8	57.9	
Georgia	56.9	34.7	30.7	Oklahoma	44.4	38.0	26.3	
Hawaii	84.9	50.4	18.2	Oregon	64.3	52.6	29.9	
Idaho	62.1	48.1	38.6	Pennsylvania	84.3	61.7	41.7	
Illinois	82.9	45.6	27.2	Rhode Island	56.6	43.7	32.4	
Indiana	38.2	44.2	31.9	South Carolina	49.7	38.1	38.6	
Iowa	59.4	66.4	46.7	South Dakota	75.6	44.3	34.6	
Kansas	71.3	53.4	27.9	Tennessee	51.6	41.8	43.6	
Kentucky	81.9	42.2	17.1	Texas	31.0	61.6	46.5	
Louisiana	56.7	38.8	27.5	Utah	57.8	38.3	34.0	
Maine	74.2	45.0	40.0	Vermont	87.4	60.0	57.1	
Maryland	53.7	42.6	38.8	Virginia	44.6	37.0	34.7	
Massachusetts	17.9	53.5	41.5	Washington	60.2	47.8	34.0	
Michigan	76.2	62.3	52.5	West Virginia	61.8	54.0	38.5	
Minnesota	69.9	68.7	35.2	Wisconsin	87.5	69.7	55.9	
Mississippi	59.4	46.6	36.1	Wyoming	50.9	51.4	33.0	
Missouri	64.7	42.3	34.2	Total	64.2	49.3	38.2	

2.2.1 The Effectiveness of Withholding as an Enforcement Tool

Existing research on the the effects of child support withholding focuses on its enforcement aspects. Withholding is generally considered to be one of the most effective child support enforcement tools available to state governments. Sorensen and Hill (2004) find that previously married mothers were 4.2 percent more likely to receive support after universal wage withholding was implemented in their state, although they find no effect for never-married mothers. Case, Lin and McLanahan (2003) find that universal withholding is associated with average annual child support payments that are higher by \$187 for ever married mothers.

In spite of their effectiveness, the implementation of withholding laws remains problematic. Especially before widespread automation, state agencies had difficulty tracking down noncustodial parents and issuing withholding orders in a timely manner: delinquent parents often left jobs before the authorities could catch up with them. Large caseloads also slowed implementation. Gordon (1994) used administrative data from 1990 on child support cases and survey responses from child support enforcement offices to study the implementation of wage and immediate withholding and found that in cases with arrears due (and therefore subject to wage withholding) enforcement offices attempted withholding in 71 percent of AFDC cases overall and 81 percent of AFDC cases in the past year. Of AFDC cases, 45 percent actually had withholding at the time of the survey and 63 percent of non-AFDC cases had withholding. However, for cases in which the case file did not have information about withholding, the office attempted withholding in only 33 percent of AFDC cases and 26 percent of non-AFDC cases. Gordon concluded that although a large part of the difficulty in establishing withholding orders is unemployment spells or inability to locate parents, poor administrative organization is also to blame. In 1990, only a small minority of child support enforcement offices had automated systems and many offices required caseworkers to check obligors' employment status manually on a monthly basis. Orders were also initiated and enforced by different agencies in many cases, so poor interagency communication contributed to poor implementation. Studies by Case et al. (2003) and Sorensen and Hill (2004) indicate that enforcement and withholding automation have improved significantly, although these studies do not use administrative data.

2.3 Theoretical Models of Payment Methods

Although the universal withholding laws changed only the default payment method and did not change withholding in individual cases, modeling the effect of a change in payment method on individual behavior provides guidance for how we would expect parents' behavior to differ by payment type. First, I develop a standard model of child support enforcement and parents' decisions about the amount of support to pay and the amount of time to spend with their child.

2.3.1 A Standard Model

Consider a simple model of child support payments in which fathers decide how much time and money to spend on their child. Fathers care about their own consumption (Y_P) and that of their child (Y_C), own leisure (L_P), and time spent with their child (L_C). I assume that $U(Y_P, Y_C, L_P, L_C)$ is twice continuously differentiable, concave, and increasing in each parameter.

In order to evade child support withholding, fathers must choose an employer who is willing to disobey state law and ignore a court order or agency order requiring income withholding. In the model, this is represented as a choice between two sectors of the labor market: fathers choose whether to work in the formal sector or informal sector. For simplicity, I assume labor is supplied inelastically conditional on sectoral choice, so that fathers choose between a bundle (W_F, H_F) or (W_I, H_I) , where W_F and W_I are after-tax incomes in each sector and H_F and H_I are total hours of leisure available in those sectors.⁴ In addition to choosing their sector, fathers choose how much child support to pay and how much time L_C to spend with their child. Since H_F and H_I need not be the same, the total amount of time that fathers may divide between their own leisure L_P and time with their child L_C varies with sectoral choice.

The government requires fathers to pay a minimum level of child support \bar{S} . If a father pays support S such that $S < \bar{S}$, he faces a penalty if his evasion is detected. I use a certainty-equivalent cost function $C(\bar{S} - S)$ that is the certainty value of the increased risk borne by the father, where $C' > 0$, $C'' > 0$ and $C(S) = 0$ if $S \geq \bar{S}$. The assumption that $C''' > 0$ reflects the increasing penalties and enforcement mechanisms fathers face as child support evasion increases. Fathers may also pay support above \bar{S} .

⁴In the US, child support payments are not tax-deductable and child support receipts are not taxed as income.

Children's consumption Y_C is a function of the total amount of support S that fathers pay and mother's income Y_M , where $Y_C = Y_C(S + Y_M)$ and $Y_C' > 0$. The second derivative, Y_C'' , depends on the mother's propensity to spend money on the child as income increases; I assume $Y_C'' \leq 0$. In this standard model, payment method has no direct effect on behavior. Fathers whose optimal child support payment amount is not affected by the introduction of withholding do not alter the amount of time they spend with their children.

Prior to Implementation of Withholding

Before implementation of withholding, fathers simply choose the sector in which their utility is highest: if the costs of evasion are the same in both sectors and both after-tax income and total leisure is higher in sector j , fathers will always choose sector j . If there is a trade-off between income and hours between the formal sector (F) and the informal sector (I), sectoral choice will depend on preferences. More likely, the costs of evasion will differ across sectors due to different probabilities of detection or different penalties if caught, since detection of informal work may also trigger tax consequences. Fathers choose their sector in two steps: they optimize in each sector and then choose the sector that gives highest utility $U = \operatorname{argmax}_{j \in F, I} U(S_j^*, L_C^*)$ subject to the budget constraint and time constraint in each sector. Fathers who work in sector j solve

$$\begin{aligned} \max_{S, L_C} U(Y_P, Y_C, L_P, L_C) \text{ subject to} \\ Y_P = W_j - S - C(\bar{S} - S) \\ Y_C = Y_C(S + \bar{Y}_M) \\ H_j = L_P + L_C \\ S, L_C \geq 0 \end{aligned} \tag{2.1}$$

where W_j is after-tax income in sector j .

This yields the following first-order conditions:

$$-U_1(1 - C') + U_2 Y_C' = 0 \tag{2.2}$$

$$-U_3 + U_4 = 0 \tag{2.3}$$

Equation (2.2) requires that at the optimum the marginal benefit from another dollar spent on the father's own consumption should equal the marginal benefit from

another dollar spent on child support. If the father pays less than full support and incurs costs, the marginal rate of substitution between father's consumption and time spent with the child equal $Y'_C/(1 - C')$, the relative cost of another unit of child's consumption in units of own consumption. If the father pays $S \geq \bar{S}$, he sets the marginal rate of substitution equal to the child's marginal consumption Y'_C . Equation (2.3) simply states that the marginal utility of leisure should equal the marginal utility of time the father spends with his child.

The case in which $S^* > \bar{S}$ represents fathers who willingly pay their full mandated child support. For these fathers, an increase in the mandatory level of support \bar{S} has no effect on S^* and L_C^* . For fathers who pay less than the required amount of support ($S^* < \bar{S}$), an increase in the mandated amount of support \bar{S} increases the cost of evasion C and effectively lowers income. Formally,

$$\frac{\partial S^*}{\partial \bar{S}} = -C' \frac{\partial S^*}{\partial W_j} - \frac{U_1 C'' \frac{\partial^2 U}{\partial L_C^2}}{D_{Standard, S^* < \bar{S}}}. \quad (2.4)$$

The first term of this expression illustrates the negative income effect of an increase in mandatory support. The increase in \bar{S} raises the cost of not fully complying with the child support order and has a negative effect on the father's income. If Y_C is a normal good, so that $\frac{\partial S}{\partial W_j} > 0$, this causes them to decrease support S . The magnitude of this effect depends on the steepness of the cost function. However, increasing \bar{S} also makes own consumption more costly relative to child's consumption: the second term illustrates that an increase in \bar{S} also increases the costs of evasion when the cost function is convex.⁵ When $C'' > 0$, the second term of the comparative static expression is positive and, depending on the relative magnitudes of the two terms, an increase in mandatory support \bar{S} can increase support paid.

The comparative static result for the effect of an increase in mandatory withholding on parent-child contact L_C ,

$$\frac{\partial L_C^*}{\partial \bar{S}} = -\frac{\partial L_C^*}{\partial W_j} + \frac{U_1 C'' \frac{\partial^2 U}{\partial S \partial L_C}}{D_{Standard, S^* < \bar{S}}}, \quad (2.5)$$

cannot be signed without additional assumptions about the signs of $\frac{\partial L_C}{\partial W_j}$ and $\frac{\partial^2 U}{\partial S \partial L_C}$. The effect of an increase in mandatory support on the time fathers spend with their children is more likely to be positive if S and L_C are complements so $\frac{\partial^2 U}{\partial S \partial L_C} > 0$.

⁵The concavity assumption ensures that $\left(\frac{\partial^2 U}{\partial L_C^2} / D_{Standard, S^* < \bar{S}}\right) < 0$.

After Implementation of Withholding

If the government implements child support withholding, it can collect full support \bar{S} from all fathers working in the formal sector: in the model, this can be represented by adding an additional constraint $S \geq \bar{S}$. Because these fathers are forced to pay full support, there are no costs associated with evasion, so $C(S) = 0$. For fathers who paid full support prior to implementation of withholding, the policy change has no effect: they face the same constraints as before the policy change.

Fathers who worked in the formal sector but paid less than full support are faced with an income shock. If $(U_F^*|S = \bar{S}) < U_I^*(S^*, L_C^*)$, fathers are now better off switching from the formal to the informal sector; for these fathers, we cannot identify how the time L_C^* they spend with their child will change. For fathers who remain in the formal sector, S^* increases automatically. There is no change in the time constraint, but L_C^* is likely to change as well depending in part on complementarity between Y_P , Y_C , L_P , and L_C . The direction of change cannot be signed without further assumptions.

This standard model of child support payment generates the usual results: implementing withholding has no effect on behavior for individuals whose payment amount does not change.

2.3.2 Salience and Payment Method

The model developed above presumes that child support payments affect father's decisions about how much support to provide identically regardless of how those payments are made. However, a growing body of research suggests that payment method affects individuals' consumption decisions. Much of the existing work focuses on credit cards. Hirschman (1979) finds that individuals with bank credit cards or store cards are more likely to make purchases and to spend more than individuals without credit cards. More recently, Prelec and Simester (2001) find that individuals have higher willingness to pay for sports tickets when they are instructed to pay with credit cards than when told to pay in cash in an experimental setting, and present evidence that liquidity constraints do not fully explain the results. Soman (2003) reports that college students spend more on photocopying when given a prepaid copy card than when given cash. Several papers examine the impact on behavior of automatic payments for obligations to government. Finkelstein (2007) examines the effect of electronic toll collection systems and finds suggestive evidence that introduction of electronic toll

collection decreases the short-run elasticity of driving by 10 percent. Feldman (2004) finds that decreases in tax refunds due to lower income tax withholding decreased the probability that households contributed to an IRA even though tax liability did not change.

Salience, or visibility, is one mechanism through which payment method may affect behavior. Payment method may determine the salience of the payment and affect the way in which individuals weigh costs against benefits when making consumption decisions. Prelec and Loewenstein (1998) and Thaler (1999) argue that payment methods affect consumption by manipulating the strength of the link between payment and specific consumption acts. For example, credit cards decouple payment and consumption by separating the timing of paying a credit card bill from the timing of acquisition and by aggregating payments for multiple consumption decisions on one bill. Payment methods that link payment more closely to consumption reduce the pleasure associated with consumption and lead to fewer purchases relative to payment methods in which payment and consumption are tied less closely

Recent tests of the effect of salience on consumer behavior indicate that individuals are more responsive to relatively salient prices. Chetty, Looney and Kroft (2007) test the effect of salience of purchase costs on consumption decisions using an experiment in which tax-inclusive prices were posted in a supermarket for some goods. Purchases of the goods for which tax-inclusive prices were posted decreased relative to purchases for goods without tax-inclusive price labels by approximately 8 percent. Liebman and Zeckhauser (2004) argue that because in most cases average tax rates are more salient, or better understood by and more visible to, taxpayers than marginal tax rates, we should expect behavior to change in response to average rather than marginal rates. Feldman and Katuščák (2006) use changes in the child tax credit, which changed average but not marginal tax rates for some taxpayers, to test this hypothesis. They find that labor supply decreases for individuals who lose eligibility for the child tax credit, consistent with models in which individuals respond to the average rather than marginal tax rate.

This research suggests that shifting to automatic withholding of child support payments may alter noncustodial parents' behavior. Automatic withholding shifts responsibility for payment from the noncustodial parent to the parent's employer and separates payment from contact with the child or the custodial parent. When support payments occur automatically, the payments are less salient and noncustodial fathers receive fewer reminders of their obligations toward their children.

In modeling salience of child support, it is unclear whether the change in pay-

ment mechanism affects only the salience of payment or whether, in the case of child support, it also affects the salience of child's consumption. With weaker reminders about child support obligations, fathers may receive less utility from meeting those obligations and contributing to their children's wellbeing. If so, withholding affects the degree to which thoughts of the child affect the father's utility by changing the salience of the child's consumption.

A simple way to incorporate salience into the model presented above is to add a multiplicative salience parameter σ that affects the impact of support S , where $\sigma = 1$ corresponds to full awareness of payment, as with direct cash payment, and $0 < \sigma \leq 1$ indicates less-than-complete salience. If the change to withholding affects the salience of payment only, I consider $U(Y_P, \hat{Y}_C(\sigma S + \bar{Y}_M), L_P, L_C)$. In the case in which payment method affects the salience of child's consumption, I am interested in $U(Y_P, \sigma Y_C, L_P, L_C)$. If we assume all of child support payments are withheld (i.e., if the father wants to pay $S > \bar{S}$ he requests larger withholding), then these two options result in identical comparative static results. If any additional support the father provides is paid directly by the father, the implications are slightly different. Consequently, I will treat $S = \bar{S}$ as formal child support paid via withholding and A as additional support paid directly to the custodial mother. If withholding affects only the salience of payment (and not the child's consumption), I examine $U(Y_P, \hat{Y}_C(\sigma \bar{S} + A + \bar{Y}_M), L_P, L_C)$

This method of modeling salience differs from that used in recent work by Chetty et al. (2007). They examine purchasing decisions among consumers for whom salience is effectively optional: agents know sales tax rates but must incur a computational cost to calculate after-tax prices. Agents choose whether to compute true prices and incur the costs or to maximize with respect to inaccurate prices. Their salience costs enter through the budget constraint rather than the utility function. Since they assume computation costs are constant, agents are more likely to incur the costs for larger purchases. In my model in which the salience parameter enters multiplicatively, salience produces larger distortions at higher levels of child support payment since agents receive less utility from high child support payments when payments are less salient.

Salience Model I: Salience of Mandatory Payment \bar{S}

If only mandatory child support payments are withheld and withholding changes only the salience of the withheld child support payments \bar{S} (and not the child's consump-

tion), then fathers in the formal sector who pay at least full support ($S + A > \bar{S}$) face the following problem:

$$\max_{A, L_C} U(W_F - \bar{S} - A, Y_C(\sigma\bar{S} + A + \bar{Y}_M), H_F - L_C, L_C). \quad (2.6)$$

I consider only parents who work in the formal sector before and after withholding. I assume that, for these parents, switching to child support withholding reduces σ ; that is, I assume that withholding makes the child support payment less visible. I focus on fathers who paid full support prior to withholding because withholding changes both payment method and amount (and possibly sectoral choice) for fathers who do not pay full support without withholding enforcement.⁶

Changes in the level of mandated support no longer correspond to a one-for-one change in the level of total support ($\bar{S} + A$) because fathers are fully cognizant of the change in mandatory support on their own consumption but not fully aware of the effect of the change on their child's consumption.⁷ Because of this,

$$\frac{\partial A^*}{\partial \bar{S}} = -\frac{\partial A^*}{\partial W_F} + \sigma \frac{\partial A^*}{\partial Y_M}.$$

When $\sigma = 1$, the model is identical to the standard model and $\frac{\partial A^*}{\partial \bar{S}} = -1$ (where a change in the amount of required support \bar{S} is exactly offset by a change in the amount of additional support, and total support S remains unchanged). This effect has two components: a one-unit increase in required support reduces the father's discretionary income by one unit, so there is a negative income effect $\frac{\partial A}{\partial W_j}$. It also automatically increases the amount of support the child receives, which has the same negative income effect as an additional dollar of mother's income, $\frac{\partial A}{\partial Y_M}$.⁸ If we assume that Y_C is normal but not superior, $0 < \frac{\partial A}{\partial W_F} < 1$ and $-1 < \frac{\partial A}{\partial Y_M} < 0$.⁹ Then

$$-1 < \frac{\partial A}{\partial \bar{S}} < 0. \quad (2.7)$$

⁶If fathers who paid less than full support resent the child support system, additional enforcement such as withholding could increase salience if it also increases their resentment.

⁷Modeling fathers as fully aware of the effect of child support on their consumption, rather than using $Y_P = W_F - \sigma\bar{S} - A$, reflects the fact that fathers' take-home pay is net of child support payments under income withholding.

⁸If mothers are more likely to spend additional child support dollars to benefit the child than additional dollars of their own income, this will not hold.

⁹The assumption that Y_C is normal but not superior simply implies that another dollar of income will increase expenditures on child's consumption by a positive amount less than \$1. This allows both Y_P and Y_C to be normal.

Unlike the standard model, A and \bar{S} are not one-for-one substitutes because the father perceives the two payments differently. Both decrease his own consumption Y_P , but A has a stronger marginal effect on his perception of his child's consumption, and a smaller increase in A is needed to offset a decrease in \bar{S} . As for time spent with the child,

$$\frac{\partial L_C^*}{\partial \bar{S}} = -\frac{\partial L_C^*}{\partial W_F} + \sigma \frac{\partial L_C^*}{\partial Y_M}, \quad (2.8)$$

which is negative when $\frac{\partial L_C^*}{\partial W_F} > 0$ and positive otherwise. Since an increase in W_F does not relax the time constraint, standard assumptions about normality do not allow us to sign $\frac{\partial L_C^*}{\partial W_F}$.

Our main area of interest, however, is the change in A^* and L_C^* when σ changes. The comparative static results are

$$\frac{\partial A^*}{\partial \sigma} = \bar{S} \frac{\partial A^*}{\partial Y_M} \quad (2.9)$$

and

$$\frac{\partial L_C^*}{\partial \sigma} = \bar{S} \frac{\partial L_C^*}{\partial Y_M}, \quad (2.10)$$

where $\frac{\partial A^*}{\partial \sigma} < 0$ if the child's consumption is a normal good for the father. An increase in salience has a negative income effect on additional support A because it makes fathers more aware of the support they are paying formally and increases their perceived value of the child's consumption.¹⁰ Consequently, fathers will substitute toward own consumption. The sign of $\frac{\partial L_C^*}{\partial \sigma}$ depends on the sign of $\frac{\partial L_C^*}{\partial W_F}$. Since an increase in the father's income does not affect the time constraint and salience acts like an increase in the father's income, and increase in salience has ambiguous effects on how much time the father will choose to spend with the child.

The implications of these comparative static results for the empirical estimates are that, among fathers who would pay full mandatory support with or without enforcement, we should expect more support above the mandatory amount from fathers with withholding than those without and that we should expect a nonzero response in the number of visits. If we believe that, even if the time constraint is not relaxed, fathers spend more time with their children when their income is higher, i.e., that $\frac{\partial L_C^*}{\partial W_F} > 0$, this salience model also predicts that fathers whose payments are less salient will spend less time with their children.

¹⁰ $\frac{\partial A^*}{\partial Y_M} < 0$ is the income effect on father's support, due to the mother contributing additional support.

Saliency Model II: Saliency of Child's Consumption Y_C

If withholding changes the saliency of the child's consumption instead of only the saliency of the formal payment, then fathers in the formal sector who pay full support solve

$$\max_{A, L_C} U(W_F - \bar{S} - A, \sigma Y_C(\bar{S} + A + \bar{Y}_M), H_F - L_C, L_C).$$

For fathers who pay full mandatory support \bar{S} and choose $A^* > 0$ the first order condition with respect to A becomes

$$-U_1 + \sigma Y'_C U_2 = 0. \quad (2.11)$$

As in equation (2.2), this states that the marginal benefit from another dollar spent on own consumption should equal the marginal benefit from another dollar spent on the child's consumption at the optimum. When saliency σ is low, the marginal benefit from additional spending on child's consumption is low, and we would expect an increase in saliency to lead to an increase in A : we might expect $\frac{\partial A^*}{\partial \sigma} > 0$. However, the comparative static results illustrate that the effect of a change in saliency is more complicated:

$$\frac{\partial A^*}{\partial \sigma} = \frac{\partial A^*}{\partial Y_M} \frac{Y_C}{\sigma Y'_C} + \left(\frac{Y''_C Y_C}{Y'_C} - Y'_C \right) \frac{U_2 \frac{\partial^2 U}{\partial L_C^2}}{D_{Saliency II, A^* > 0}} \quad (2.12)$$

and

$$\frac{\partial L_C^*}{\partial \sigma} = \frac{\partial L_C^*}{\partial W_F} \frac{Y_C}{\sigma Y'_C} + \left(Y'_C - \frac{Y''_C Y_C}{Y'_C} \right) \frac{U_2 \frac{\partial^2 U}{\partial A \partial L_C^*}}{D_{Saliency II, A^* > 0}}. \quad (2.13)$$

The first term of Equation (2.12) is negative under the assumption that Y_C is normal but not superior and large in magnitude when σ is low. Intuitively, an increase in saliency acts like an exogenous increase in other resources available to the child and allows fathers to substitute away from providing support A . The second term is positive and corresponds to intuitive expectations that an increase in saliency will increase additional support because that support is now more valuable at the margin: the concavity assumptions on utility ensure that $(\frac{\partial^2 U}{\partial L_C^2} / D_{Saliency II, A^* > 0}) < 0$. Consequently, the sign of $\frac{\partial A^*}{\partial \sigma}$ depends on the comparative magnitude of these two effects, but unlike the standard model and Saliency Model I, it is consistent with the intuitive result that when the child's consumption is less salient, fathers contribute less to their child's support.

As with other comparative statics involving L_C , the effect of an increase in saliency

on father-child contact, $\frac{\partial L_C^*}{\partial \sigma}$, depends partly on the sign of $\frac{\partial L_C^*}{\partial W_F}$. Since an increase in a father's after-tax wage earnings, W_F , does not relax the time constraint, the sign of $\frac{\partial L_C^*}{\partial \sigma}$ is indeterminant. However, if we assume $\frac{\partial L_C^*}{\partial W_F} > 0$, then when A and L_C^* are complements, $\frac{\partial L_C^*}{\partial \sigma} > 0$.¹¹ As with the standard model, changes in mandated levels of support do not affect optimal choices of total support or time spent with the child ($\frac{\partial A}{\partial S} = -1$ and $\frac{\partial L_C}{\partial S} = 0$). Because the salience parameter affects the salience of both \bar{S} and A , fathers treat these types of support as perfect substitutes.

Like Salience Model I, this model of the salience of child's consumption predicts that changes in child support payment method will alter the amount of time fathers spend with their noncustodial children and the amount of additional support they provide even when the amount of regular child support paid is held constant. Unlike the first model that examine salience of payment, Salience Model II is consistent with a decline in both visits and in-kind support when fathers switch to less salient payment methods.

2.3.3 Alternative Mechanisms for Payment Method Non-Neutrality: Intrinsic Motivation

The models above assume that salience is the mechanism through which payment method affects behavior. Another possible means by which withholding may affect behavior is by altering fathers' intrinsic motivation to spend time with their children. Frey (1997) argues that external rewards or enforcement mechanisms can crowd out intrinsic motivation to engage in altruistic or socially desirable behavior.¹² The intrinsic motivation argument is based on evidence from psychological experiments that indicate that rewards may decrease motivation under certain conditions. In a classic study, Lepper, Greene and Nisbett (1973) found that by rewarding students with a certificate for drawing, students were less inclined to draw in future without promise of a certificate. Psychologists Deci, Koestner and Ryan (1999) conduct a meta-analysis and review nearly thirty years of similar studies on rewards and intrinsic motivation.

¹¹ $\frac{\partial^2 U}{\partial A \partial L_C} > 0$ if A and L_C are Edgeworth complements.

¹² Economists have debated the plausibility of motivational crowd-out in the context of whether opening a market for blood would decrease altruistic blood donations (Titmuss 1971, Solow 1971, Arrow 1972). More recently, Bénabou and Tirole (2003) incorporate the idea of intrinsic motivation in a principal-agent model in which performance incentives can decrease an agent's effort. In empirical work, Frey and Oberholzer-Gee (1997) examine Swiss citizens' willingness to accept a nuclear waste site in their region when they are and are not offered monetary compensation: contrary to the predictions of standard economic theory, acceptance dropped significantly when monetary incentives were offered. Frey and Oberholzer-Gee interpret this as evidence that government payments reduced intrinsic motivation to contribute to the social good.

They conclude that when a task is intrinsically interesting, external rewards reduce the degree to which individuals take responsibility for their own actions and lead to less of the rewarded behavior. In accord with this research, Frey (1997) claims crowd-out of intrinsic motivation is most likely if: (1) the relationship is more personal; (2) the task is intrinsically interesting; (3) the agent has more room for participation and decision-making; and (4) when the external intervention is uniform across agents. These guidelines suggest that child support enforcement is a likely case for crowd-out. Child support and family relationships are personal and generally self directed, and child support courts and enforcement agencies strive to apply laws uniformly.

There is evidence that both payers and recipients of child support resent government intervention even when they agree that fathers have an obligation to support their children, and this may support the intrinsic motivation story. Waller (2002) conducted interviews with a sample of low-income fathers and mothers in Trenton, New Jersey in the mid-1990s and reports that frequently both parents resent income withholding laws. Fathers in Waller's sample resent income withholding not only because they object to state requirements to pay support but because they generally prefer to make in-kind transfers rather than cash payments because their children understand tangible gifts more easily than paid child support. Furstenberg (1992) reports that fathers think that the state cares only about their monetary contributions and ignores other aspects of being a good father. Waller and Plotnick (2001) summarize other research with similar findings. Child support enforcement restricts the types of support fathers can provide and may lead to resentment and reduce fathers' inclinations to provide for their children.

One way to incorporate intrinsic motivation in a mathematical model is to reinterpret Salience Model II above. Rather than representing salience, σ could represent the degree of intrinsic motivation, or the extent to which fathers receive utility from increases in child's consumption. Since a switch from direct payment to withholding could decrease either salience or intrinsic motivation, the empirical tests presented below cannot distinguish between effects due to salience of the child's consumption or effects on intrinsic motivation. Additionally, the models are not mutually exclusive: both could operate simultaneously. Nevertheless, the empirical results below allow a comparison between the standard model and alternative behavioral explanations and allow us to distinguish between possible types of salience effects.

2.3.4 Alternative Mechanisms for Payment Method Non-Neutrality: Household Bargaining

Bargaining between parents over children's time may provide another explanation for responses to changes in the payment method. Withholding makes child support orders almost perfectly enforceable for men who work in regular jobs and who do not wish to change their work status. If men pay child support in exchange for access to their children, then withholding laws remove the father's power to exchange support for time. If mothers restrict access to their children to gain additional support from fathers, average visits would decrease after implementation of withholding. There is ethnographic evidence that mothers restrict fathers' access to children when fathers do not pay support but this is associated with other trust or conflict issues between parents (Furstenberg 1992, Johnson and Doolittle 1998, Waller and Plotnick 2001). Del Boca and Ribero (2001, 2003) develop this intuition in a model in which mothers and fathers are endowed with income $y_m < y_f$ and mothers are endowed with all rights to a child's time. Each parent i 's time spent with the child h_i is perfectly excludable: $h_m + h_f = 1$. The model predicts that fathers with higher income will pay more support and spend more time with their children and mothers with higher incomes will allow fewer visitations. In this framework, perfectly enforceable child support orders effectively transfer income endowments from the father to the mother and as a result the father will spend less time with the child.

Whether it makes sense to expect withholding to have large effects on visitation for household bargaining reasons depends to a large extent on how mothers feel about fathers' involvement. Del Boca and Ribero's model assumes that mothers (and children) do not benefit directly from father-child contact. If mothers believe that the child's involvement with the father is beneficial to the child or increases leisure time for the mother, the decline in visits due to a transfer of parental endowments from the father to the mother will be smaller than if the mother sees no benefit to father-child contact. This should be especially true for fathers who have high-quality involvement with their children. If fathers who pay full support also provide additional payments or transfers beyond the required amount, bargaining over time with the child may take place over a different support level than that enforced by the child support order. Generally, bargaining models suggest that withholding should have a larger negative effect on father-child contact for lower-income (constrained) fathers and for fathers with lower-quality child involvement.

Table 2.2: Summary of Model Predictions for Parents Who Pay Full Support

	Model		
	Standard	Saliency I	Saliency II
$\frac{\partial A^*}{\partial Y_M}$	$[-1, 0]$	$[-1, 0]$	$[-1, 0]$
$\frac{\partial A^*}{\partial \bar{S}}$	-1	$[-1, 0]$	-1
$\frac{\partial A^*}{\partial \sigma}$	NA	< 0	Cannot be signed
$\frac{\partial L_C^*}{\partial Y_M}$	> 0	> 0	> 0
$\frac{\partial L_C^*}{\partial \bar{S}}$	0	< 0	0
$\frac{\partial L_C^*}{\partial \sigma}$	NA	> 0	> 0

Assumptions: (1) fathers work in the formal sector and pay full support $S^* = \bar{S}$ regardless of withholding status; (2) Y_P and Y_C are both normal (which implies that $0 < \frac{\partial A}{\partial W_F} < 1$); (3) $\frac{\partial L_C}{\partial W_F} > 0$; and (4) A and L_C are complements: $\frac{\partial^2 U}{\partial A \partial L_C} > 0$.

2.4 Implications for Empirical Work

Table 2.2 summarizes the comparative static results derived for the standard model and saliency models under the following assumptions: (1) fathers work in the formal sector; (2) the withholding constraint is not binding, so $S^* = (\bar{S} + A^*) > \bar{S}$ (3) own consumption Y_P and child's consumption Y_C are both normal (which implies that $0 < \frac{\partial A}{\partial W_F} < 1$); (4) the income effect on child's consumption is positive, so $\frac{\partial L_C}{\partial W_F} > 0$; and (5) child support $S = \bar{S} + A$ and time with the child L_C are complements: $\frac{\partial^2 U}{\partial A \partial L_C} > 0$. The assumption that $\frac{\partial L_C}{\partial W_F} > 0$ is reasonable if we relax the assumption that labor is supplied inelastically and consider an ordinary income effect: this assumes time spent with the child is a normal good.¹³ It is more difficult to decide

¹³In this model, $\frac{\partial L_C}{\partial W_F}$ is a pure income effect. In a model with endogenous labor supply, the relevant comparison is a pure income effect in that model, not the wage effect (which will naturally have both an income and a substitution component).

whether A and L_C should be treated as complements or substitutes: some fathers may substitute presents and cash for time spent with the child. However, the child support literature generally indicates that support and time are complementary and visits increase with the amount of support paid. Explanations for the complementarity include that noncustodial parents may wish to monitor how the custodial parent spends their money or they may wish to monitor their child. For example, Aughinbaugh (2001) examines whether noncustodial parents use their children's school performance as a measure of how well the custodial parent cares for the child and looks at whether they adjust their support payments in response to children's performance. Alternatively, money and time spent with the child may be complements because custodial parents may restrict access to children unless child support is paid (Johnson and Doolittle 1998).¹⁴ The simplest explanation is that fathers who care more about their children care about both the child's physical wellbeing and their relationship with the child.

Under these assumptions, the models above predict different effects of withholding on parent-child contact L_C and additional support A for parents whose amount of regular support payments is not affected by withholding. Further, among models that suggest an effect, they have predictions for which types of fathers should be most affected. I examine the effect of withholding on two types of outcomes: the number of days on which fathers and children had contact (my measure of L_C) and whether fathers paid for or provided food, clothing, medical care, child care, or gifts, which I treat as an indirect measure of additional support A . To reflect the fact that a change in payment method can also change payment amount for some fathers, I consider separate effects for fathers with different payment histories. Although income withholding was first used to enforce child support orders, the introduction of universal withholding laws means that withholding applies even for cases in which fathers willingly pay the full amount of mandatory child support. For these fathers, withholding changes only the payment method, not the fraction of required support that is paid. The comparative static results above primarily address this change. For other fathers, withholding changes both payment method and amount paid (and potentially the choice of regular or informal-sector work) as the enforcement aspect becomes important. This means that empirically we should look for separate effects depending on whether we believe fathers would have paid in the absence of withholding. This is a counterfactual we cannot observe; however, to proxy for it I use

¹⁴Although many child support orders establish visitation rights for noncustodial parents, custodial parents can often ignore these rules without penalty.

data on whether or not the father owes back support. I treat fathers who are not in arrears and do not owe any back support as fathers who would pay full support regardless of withholding status, and treat fathers who are in arrears as fathers for whom withholding presents a binding constraint.¹⁵

2.5 Data and Methodology

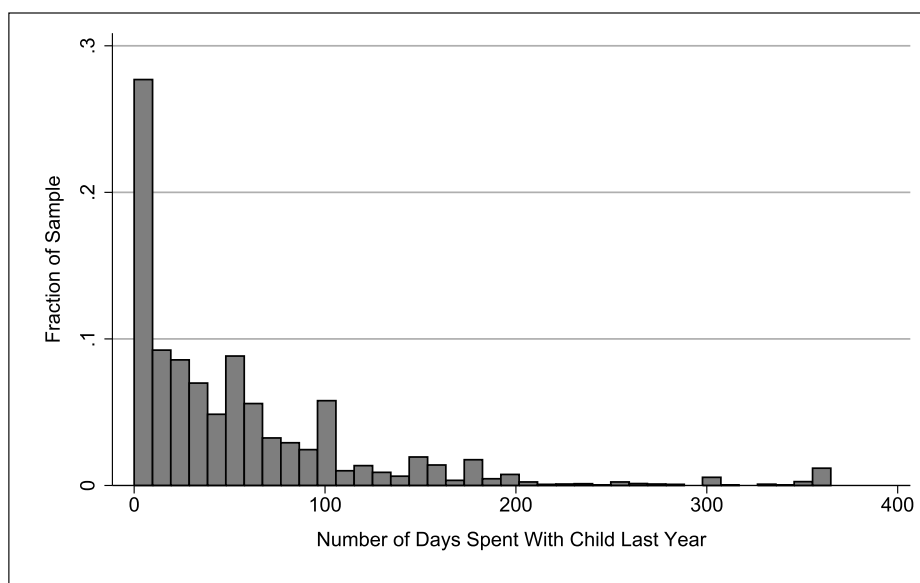
I use pooled cross-sectional data from the Current Population Survey's Child Support Supplement, which collected child support data beginning in 1979 and biannually since 1982 and asked whether child support payments are withheld from the noncustodial parent's paycheck in each survey from 1994 onward. I use data from the 1994, 1996, 1998, 2000, 2002, and 2004 surveys and I restrict the sample to legalized child support cases in which the mother is the custodial parent, which yields a base sample of over 6,500 observations for which mothers report whether or not child support payments were made via withholding and the number of child-parent contact days the previous year.¹⁶ The survey interviews custodial parents about their child support agreements, the amount and method of child support payments, and children's interactions with their noncustodial parent. Unfortunately, as with most child support surveys, there is very little information about the noncustodial parent. The complete supplement includes approximately 90 questions on child support and can be viewed online at the Census website.¹⁷ Mothers report the amount of formal child support paid each year, and overall approximately 60 percent of fathers pay the full amount of formal support due. Among fathers without withholding, about 65 percent of fathers pay full support.

¹⁵If the withholding began immediately when the child support order was issued, this way of observing the counterfactual may be problematic. However, child support orders can and often do require fathers to pay retroactive support for periods before the order was issued unless he can prove he was paying informal support. Fathers who attempt to avoid paying formal child support by delaying getting a formal child support order will owe retroactive back support even if they face immediate withholding, and in most states they will also owe interest on the unpaid support. Unemployment spells, job changes, and self-employment can also allow back support to accrue for fathers who do not voluntarily pay full support. In robustness checks I have also restricted the sample to fathers who pay full support each month and to fathers whose payments are within five percent of the amount of child support due. These sample restrictions do not affect the results.

¹⁶The father is the custodial parent in about 10 percent of cases reported in the CPS. I exclude these cases because it is relatively unusual for fathers to have primary custody of their children; mothers who do not have custody may have different reasons for not spending time with their children than fathers who do not have custody. However, including cases in which the father has custody in the estimation sample does not change the results.

¹⁷See the CPS technical documentation at <http://www.census.gov/cps/>.

Figure 2.3: Histogram of Number of Days Per Year Noncustodial Fathers Spent with Children, Pooled 1994-2004 CPS Data



The CPS asks custodial mothers to report two main measures of children’s interactions with noncustodial fathers. First, they report the number of days on which their child saw the father in the previous year. Figure 2.3 shows a histogram of contact days: about 10.2 percent of the base estimation sample report zero contact days, and about 1.0 percent report 365 contact days. The mean and standard deviation of the contact days variable are 54.9 days and 67.2 days respectively. Mothers also report whether fathers provided in-kind support (instead of or in addition to formal child support) by providing or helping to pay for food, clothing, health care, gifts, or child care or summer camp (the amount of support provided is not available). Means of these variables are provided in Table 2.3. Approximately 79 percent of mothers report that fathers provided gifts. Clothing was the second-most common form of in-kind support, with about 43 percent of fathers providing clothing. Food was provided by 28 percent of fathers and one quarter of fathers helped pay for medical care. Only 9 percent of fathers helped pay for child care or summer camp.

2.5.1 Empirical Strategies

In order to understand how withholding affects fathers' interactions with their children, a reasonable first step is to estimate the linear equation

$$y_i = \alpha + \gamma W_i + X_i\beta + \epsilon_i, \quad (2.14)$$

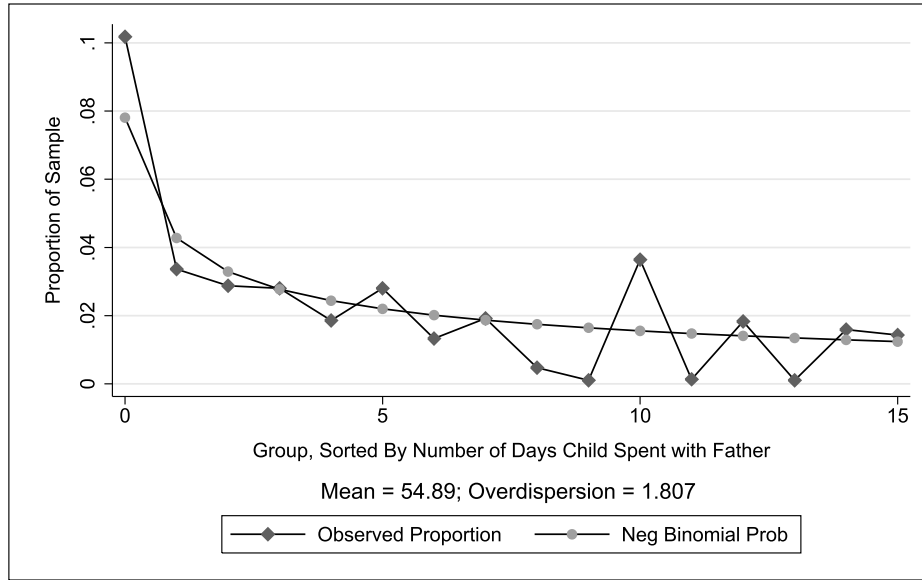
where y_i is the outcome of interest (either the number of contact days between parents and children or an indicator for whether the parent provided in-kind support), W_i is an indicator variable equal to 1 if the father's child support payments are withheld from his paycheck, X_i is a vector of control variables, and ϵ_i is the error term. The theoretical models outlined in Section 2 imply that the effect of withholding will vary depending on whether fathers would pay full support in the absence of enforcement. To address this, I estimate separate effects for the two types of fathers by including an interaction term:

$$y_i = \alpha + \gamma W_i + \eta B_i + \delta(W_i \times B_i) + X_i\beta + \epsilon_i, \quad (2.15)$$

where B_i is an indicator variable equal to 1 if the father owes back support. The effect of withholding for fathers who are not in arrears is captured by γ , while the effect for fathers who are in arrears is measured by $\gamma + \delta$. In addition, I estimate equation (2.14) separately for fathers who do and do not owe back support. Fathers who willingly pay child support are likely to differ from fathers for whom enforcement is binding on numerous unobserved dimensions. In the model in equation (2.15), the effects of all control variables are constrained to be the same for all fathers. Running the regressions separately on the two subsamples recognizes the differences inherent in these samples.

One immediate challenge is that the range of the dependent variable is limited for all the outcomes of interest. For the indicators of in-kind support, the dependent variable takes on only a zero or 1. The father-child days of contact variable is a count variable with a range between 0 and 365. In both cases, using linear regression means that the predicted values may fall outside the feasible range of the dependent variable for some observations. As an alternative to linear regression, I use probit regressions for the in-kind support outcomes and for the contact days outcome I use maximum-likelihood regression in which the underlying distributional assumption is Cameron and Trivedi's (1986) NB2 parameterization of the negative binomial distribution. This is a common generalization of the Poisson distribution

Figure 2.4: Distribution of Father-Child Contact Days: Observed Proportion and Negative Binomial Probability



that allows for overdispersion (the case where the conditional variance is greater than the conditional mean). The NB2 parameterization is derived from a Poisson model with unobserved heterogeneity. The maximum likelihood regression maximizes the log likelihood function for $\exp(\gamma W_i + X_i' \beta)$,¹⁸

$$\ln L(\alpha, \gamma, \beta) = \sum_{i=1}^n \left\{ \left(\sum_{j=0}^{y_i-1} \ln(j + \alpha^{-1}) \right) - \ln y_i! \right. \\ \left. - (y_i + \alpha^{-1}) \ln(1 + \alpha \exp(\gamma W_i + X_i' \beta)) + y_i \ln \alpha + y_i(\gamma W_i + X_i' \beta) \right\}.$$

Figure 2.4 illustrates the degree to which the raw father-child contact data fits a negative binomial distribution. I divide the range of possible father-child contact days into 15 intervals and show the observed proportion of the sample in each of the intervals and the corresponding proportion in that region of a negative binomial distribution with mean and overdispersion equal to those observed in the data. The visual “goodness-of-fit” depends on the number of intervals into which the data is divided: mothers’ reports of contact days cluster around values such as 100, 150, and 200. Consequently, the correspondence between observed proportions and the negative binomial distribution probability is weakest near these values. For wider intervals that smooth over these clusters, the data fit the negative binomial distribution well.

¹⁸See Cameron and Trivedi (1998, p. 71) for derivation.

As discussed below, the results from maximum likelihood estimation are very similar to the results from linear regression.

A more serious challenge to the validity of equation (2.14) is that, since we know that cases in which fathers have been in arrears or mothers were on welfare have historically been selected for withholding, the assumption that $\text{Cov}(W, \epsilon) = 0$ is unlikely to hold. Although we observe current welfare status and whether parents owe back support and can control for them directly, we do not observe past welfare or arrears status and we cannot fully control for these factors. We also observe very little about father characteristics that could be correlated with both withholding and parent-child contact: for example, we do not know whether fathers work in the regular sector or actively avoid withholding by working underground. Consequently, we expect OLS results to be biased, but the expected sign of the bias is unclear. Parents who take less pleasure in spending time with their children are probably more likely to miss child support payments and be forced to go on withholding, so we would expect selection on payment history to downwardly bias the OLS results. (Alternatively, parents who owe back support may spend less time with their children; they may feel guilty or the custodial parent may restrict access to the children as a punishment.) But noncustodial parents who actively avoid child support enforcement and exit the formal labor force or become self-employed are much less likely to have withholding orders. This factor biases OLS results upward because parents whose child support payments are withheld are, to a certain extent, voluntarily complying. Additionally, especially prior to passage of universal withholding laws, judges often had discretion to order withholding in individual cases. Judges may have based their decisions in part on other characteristics we cannot observe but that are correlated with parent-child contact or the likelihood of providing in-kind support.

To address endogeneity of withholding, I instrument for withholding status using the universal withholding laws discussed in Section 2.2, where the instrument is the difference between the year of the current support order and the year of universal withholding implementation.¹⁹ To be a valid instrument, the laws must be correlated with actual withholding status, but should have no direct effect on father-child contact. As can be seen by comparing the second and third columns for each state

¹⁹Appendix Table 2.13 reports the effective date of state withholding laws. All state withholding policy implementation dates are from Huang et al. (2002) with the exception of the date of universal withholding in Texas: Huang et al. do not report a law date for Texas. Texas' final version of the child support withholding law explicitly allows judges to use discretion when ordering withholding. Texas did strengthen the withholding language in its legal code in 1997; I have used 1997 as Texas' date of universal withholding. The results are not sensitive to excluding Texas cases from the analysis.

in Table 2.1, child support cases to which withholding laws apply are more likely to have withholding than cases to which the laws do not apply. Withholding laws do appear to increase withholding, and it is reasonable to believe that the passage of a law regarding payment method has no direct effect on how much time fathers spend with their children.

Furstenberg (1992) lists the following reasons for fathers' decisions not to participate in their children's lives: (1) denial of paternity; (2) another man (such as the mother's new boyfriend or husband) fills the role of "father" in their child's life; (3) the mother spends child support money on herself; (4) the mother denies the father access to the child; and (5) there is conflict between the man and the child's mother.²⁰ Interestingly, there is some evidence that African-American nonresident fathers (who pay less child support on average) are considerably more likely to see their children regularly (Danziger and Radin 1990, Argys and Peters 2001). To address some of these factors, I control for whether there is an adult male living in the household (who may serve as a new father figure) and whether the father has legal visitation privileges.

2.6 Results

2.6.1 Summary Statistics

Table 2.3 presents summary statistics for the full sample and by withholding treatment status. Columns 1 and 2 report means and standard deviations for all cases. Columns 3 and 4 report means and standard deviations in cases for which mothers report that child support is withheld, and columns 5 and 6 report summary statistics for cases in which support is not withheld. The last two columns summarize the difference between the subsamples: they report the mean and (unadjusted) standard error for regressions of each variable on withholding status. Overall, 46 percent of custodial mothers with legal child support agreements report that their child support case has income withholding. Cases with withholding are significantly different from cases without withholding on numerous dimensions: fathers with withholding spend 6.3 fewer days per year with their children than fathers without withholding (a differ-

²⁰See Argys and Peters (2001) for the effects of paternity establishment on child support payments. Danziger and Radin (1990) examine father-child contact in the presence of the child's grandfather and finds no effect on fathers' involvement. Aughinbaugh (2001) develops a model in which fathers monitor mothers' spending on the child.

Table 2.3: Sample Means, by Reported Withholding Status

	All Cases		Cases with Withholding		Cases without Withholding		Difference	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Error
Child support payments withheld from paycheck	0.46	0.50	51.5	68.4	57.8	66.1	-6.33***	(1.64)
Number of days child spent with father last year	54.9	67.2	0.25	0.43	0.31	0.46	-0.05***	(0.01)
Father provided food/groceries	0.28	0.45	0.41	0.49	0.45	0.50	-0.04***	(0.01)
Father provided clothing/diapers	0.43	0.50	0.20	0.40	0.29	0.45	-0.08***	(0.01)
Father provided money for medical care	0.25	0.43	0.76	0.43	0.83	0.38	-0.07***	(0.01)
Father gave child birthday, holiday, or other gifts	0.79	0.40	0.07	0.25	0.11	0.32	-0.05***	(0.01)
Father paid for child care or summer camp	0.09	0.29	0.39	0.49	0.19	0.39	0.20***	(0.01)
Father owes back support	0.28	0.45	34.7	7.5	37.6	7.5	-2.90***	(0.18)
Mother's age	36.2	7.6	0.16	0.37	0.09	0.29	0.07***	(0.01)
Mother is black	0.13	0.33	0.07	0.25	0.06	0.24	0.01	(0.01)
Mother is hispanic	0.06	0.25	0.27	0.44	0.31	0.46	-0.04***	(0.01)
Mother is currently married	0.29	0.46	0.21	0.41	0.10	0.30	0.11***	(0.01)
Mother never married	0.15	0.36	0.27	0.44	0.20	0.40	0.07***	(0.01)
Mother unemployed or not in labor force	0.23	0.42	7.30	5.10	8.94	5.51	-1.75***	(0.14)
Family income percentile, national ranking	8.20	5.39	0.77	0.42	0.71	0.46	0.06***	(0.01)
Mother has HS degree but not 4-year college	0.74	0.44	0.12	0.33	0.21	0.41	-0.09***	(0.01)
Mother has 4-year college degree or more	0.17	0.38	0.63	0.48	0.67	0.47	-0.05***	(0.01)
Father has legal visit privileges	0.65	0.48	0.80	0.40	0.78	0.42	0.02**	(0.01)
Father lives in same state	0.79	0.41	0.48	0.50	0.49	0.50	-0.01	(0.01)
Youngest child on support order is male	0.48	0.50	9.35	4.76	10.82	4.75	-1.47***	(0.12)
Age of youngest child on child support order	10.15	4.81	6.18	4.09	6.74	4.54	-0.55***	(0.11)
Years between survey and original support order	6.48	4.35	0.48	0.50	0.49	0.50	-0.01	(0.01)
Father figure lives in household	0.49	0.50	0.26	0.44	0.16	0.37	0.10***	(0.01)
Mother receives food stamps	0.21	0.41	0.16	0.36	0.09	0.28	0.07***	(0.01)
Mother receives AFDC/ADC/TANF payments	0.12	0.32	1.75	0.89	1.66	0.82	0.09***	(0.02)
Number of children covered by support agreement	1.70	0.85	\$3,713	\$3,823	\$4,389	\$4,841	-\$675***	(117)
Amount of child support received last year	\$4,061	\$4,389	\$4,529	\$3,841	\$4,556	\$4,224	-\$27	(99)
Amount of child support due last year	\$4,543	\$4,051	2.72	5.92	0.68	6.38	2.04***	(0.15)
Difference between year of support order and withholding law (instrument)	1.62	6.25						

ence of 12.2 percent) and are less likely to provide all types of in-kind support. They are also more than twice as likely to owe back support, and mothers report receiving \$675 (18 percent) less in support each year although there is no significant difference in support due. In cases with withholding, mothers are younger, more likely to be black, more likely to never have been married, less educated, and almost twice as likely to be on welfare. These differences across withholding status necessitate conditioning on these observed variables, but also suggest the possibility of similar differences on unobserved variables, which I will address using an instrumental variables approach. In cases with withholding, the difference between the year of the support order and the year of state universal withholding law is significantly more likely to be positive, indicating that the instrument affects treatment status.

2.6.2 OLS, Negative Binomial, and Probit Results

Results from using OLS to estimate equations (2.14) and (2.15), where the dependent variable is the number of father-child contact days, are shown in Table 2.4; they indicate that fathers whose child support payments are withheld have contact with their children on 6.6 fewer days per year than fathers whose child support payments are not withheld, a difference of approximately 13 percent. The third column of Table 2.4 presents results from the model in equation 2.15: the interaction term on withholding and back support is positive, offsetting the overall negative effect of withholding on contact. Columns 4 and 5 show the results of the regressions run separately on the samples in which the father does and does not owe back support. For fathers who owe back support, the net effect of withholding on father-child contact is near zero and not significant; for fathers who do not owe back support, withholding has a significant negative effect on parent-child contact. These effects are consistent with the predictions of a salience model; for fathers whose support payments are the same with or without withholding, automatic withholding may make fathers' responsibilities toward their children less salient and they may decrease contact with their children. For fathers who pay more support as a consequence of withholding, the salience of their responsibilities toward their children may have increased.

Table 2.5 shows the results of negative binomial regressions of the number of father-child contact days on withholding status and a vector of controls. The results indicate that overall, fathers whose child support payments are withheld from their paychecks have 13 percent fewer contact days annually than fathers whose child support payments are not withheld. The effects are significant for the subsample of

Table 2.4: OLS Regressions: Effect of Withholding on Father-Child Contact

	All Cases	All Cases	All Cases	Arrears	No Arrears
Child support payments withheld from pay	-8.69** (1.90)	-6.58** (1.89)	-8.51** (2.23)	-0.23 (2.77)	-9.45** (2.25)
Father owes back support		-19.4** (1.92)	-23.5** (2.39)		
Child support withheld * owes back support			6.95* (3.34)		
Father has legal visit privileges		2.71 (1.70)	2.74 (1.70)	5.19 (2.89)	1.37 (2.18)
Father lives in same state		32.2** (1.46)	32.2** (1.46)	23.4** (2.41)	36.0** (1.82)
Youngest child on support order is male		4.68** (1.65)	4.68** (1.65)	3.69 (2.71)	5.09* (1.98)
Age of youngest child on support order		-1.23** (0.32)	-1.23** (0.32)	-1.47** (0.52)	-1.16** (0.37)
Years between survey and original order		-0.83** (0.23)	-0.82** (0.23)	-0.03 (0.45)	-1.05** (0.27)
Father figure lives in household		-2.77 (2.64)	-2.75 (2.64)	-1.32 (4.82)	-2.74 (3.05)
Mother receives food stamps		-7.03* (3.01)	-7.06* (3.01)	-1.67 (4.75)	-9.00* (4.19)
Mother receives AFDC/TANF payments		1.08 (4.37)	1.34 (4.38)	-2.41 (6.62)	2.61 (5.95)
Mother's age		0.08 (0.17)	0.08 (0.17)	0.23 (0.26)	0.01 (0.21)
Mother is black		-3.21 (3.22)	-3.06 (3.23)	4.17 (4.97)	-7.51 (4.55)
Mother is hispanic		-5.35 (3.95)	-5.31 (3.95)	1.05 (6.30)	-6.98 (4.63)
Mother has HS degree		-4.37 (3.64)	-4.47 (3.62)	-0.92 (5.00)	-5.47 (4.52)
Mother has 4-year college degree or more		-1.75 (4.11)	-1.99 (4.09)	2.51 (5.68)	-3.52 (5.11)
Mother is currently married		-2.96 (3.00)	-2.90 (3.00)	3.69 (4.97)	-6.04 (3.61)
Mother never married		4.58 (2.93)	4.47 (2.93)	3.76 (4.11)	7.11 (3.94)
Mother unemployed or not in labor force		4.87* (2.23)	4.86* (2.23)	6.28 (3.52)	4.80 (2.67)
Household income percentile		0.40* (0.20)	0.40* (0.20)	-0.08 (0.36)	0.61* (0.25)
Constant	57.5** (4.13)	51.2** (7.91)	52.0** (7.83)	23.0 (12.26)	52.6** (9.07)
R-Square	0.017	0.099	0.099	0.080	0.094
N	6779	6668	6668	1852	4816

Regressions include state and year effects. Standard errors clustered by state and year in parentheses. * significant at 5%; ** significant at 1%

Table 2.5: Negative Binomial Regressions: Effect of Withholding on Father-Child Contact

	All Cases	All Cases	All Cases	Arrears	No Arrears
Child support payments withheld from pay	-0.17** (0.04)	-0.13** (0.04)	-0.16** (0.04)	-0.04 (0.08)	-0.17** (0.04)
Father owes back support		-0.39** (0.04)	-0.46** (0.06)		
Child support withheld * owes back support			0.11 (0.08)		
Father has legal visit privileges		0.08* (0.03)	0.08* (0.03)	0.16* (0.08)	0.05 (0.04)
Father lives in same state		0.78** (0.04)	0.78** (0.04)	0.84** (0.09)	0.79** (0.05)
Youngest child on support order is male		0.08** (0.03)	0.08** (0.03)	0.07 (0.07)	0.08* (0.03)
Age of youngest child on child support order		-0.02** (0.01)	-0.02** (0.01)	-0.03** (0.01)	-0.02** (0.01)
Years between survey and original order		-0.02** (0.00)	-0.02** (0.00)	-0.01 (0.01)	-0.02** (0.01)
Father figure lives in household		-0.05 (0.05)	-0.05 (0.05)	-0.01 (0.10)	-0.04 (0.05)
Mother receives food stamps		-0.14* (0.06)	-0.14* (0.06)	-0.08 (0.12)	-0.17* (0.07)
Mother receives AFDC/TANF payments		0.03 (0.08)	0.03 (0.08)	0.02 (0.15)	0.02 (0.09)
Constant	4.04** (0.07)	3.76** (0.15)	3.78** (0.15)	3.12** (0.29)	3.84** (0.16)
α	1.78	1.64	1.64	2.24	1.41
N	6779	6668	6668	1852	4816
Pseudo log-likelihood	-33177	-32356	-32355	-8115	-24117

Regressions include state and year effects and (except for the first column) controls for mother's age, race, marital status, employment, education, and household income. Standard errors clustered by state and year in parentheses. * significant at 5%; ** significant at 1%

fathers who do not owe back support; for fathers who owe back support, the coefficient is much smaller in magnitude and not significantly different from zero. These results are similar in magnitude to the OLS results and provide preliminary evidence that payment via withholding has a non-neutral effect on parent-child contact. Since the OLS results and negative binomial results are very similar, I use simple 2SLS methods to correct for endogeneity below.

Other results from the OLS and negative binomial regressions coincide with most previous research about determinants of visitation and contact. Not surprisingly, fathers who owe back support are significantly less likely to spend time with their

children and visit an average of 39 percent less than fathers who do not owe back support. Noncustodial fathers are much more likely to have frequent contact when they live near their children: fathers who live in the same state visit their children 78 percent more than parents who live out of state. Parents are also more likely to visit when their children are younger. Older children require less child care and have their own interests and time commitments. Also, contact might decline as the time since separation increases. Fathers are 8 percent more likely to visit when the youngest child on the support order is male; fathers may feel that their presence is more important for male children. Although the CPS data does not include the parents' date of separation, this can be proxied using the date of the original child support order. An additional year between the survey date and the initial support order decreases the average days of contact by 2 percent.

Interestingly, the custodial parent's marital status and the presence of another adult male in the household do not have a significant effect on the noncustodial parent's visits. This is consistent with Danziger and Radin (1990), who found no significant effect for the presence of father figures in the household on visits by the noncustodial parent.

Table 2.6 shows mean marginal effects from probit regressions estimating the impact of withholding on the likelihood that noncustodial parents provide in-kind support or money for food, clothing, health care, gifts, and child care or summer camp in addition to their regular child support. Control variables are identical to those in Tables 2.4 and 2.5. For fathers who owe back support, none of the effects are statistically different from zero. For fathers who do not owe back support, effects are statistically different from zero in all categories: fathers with withholding are 2.7 percent less likely to buy gifts and 4.7 percent less likely to provide clothes or diapers.²¹ Salience Model II above, in which withholding alters the salience of child's consumption (rather than salience of payment only) is consistent with these results; however, if these results are biased downward due to selection of disinterested fathers into withholding, these results do not necessarily reject the standard model.

2.6.3 IV 2SLS Results

First-stage regressions, presented in Table 2.7, show that a one-year increase in the amount of time since withholding was implemented corresponds to a 1.4 percent increase in the likelihood of having a withholding order. Including controls does not

²¹Linear probability models generate very similar derivatives.

Table 2.6: Marginal Effects from Probit Regressions: Effect of Withholding on In-Kind and Additional Support

		Father Provided the Following Kinds of Support					
		Food or groceries	Clothes, diapers or shoes	Medicine, medical expenses	Child care/ summer camp	Birthday, holiday, other gifts	
Arrears	Child support payments withheld from paycheck	-0.017 (0.017)	0.018 (0.022)	0.000 (0.016)	-0.007 (0.011)	-0.002 (0.021)	
	N	1949	1948	1833	1548	1944	
	Pseudo R-square Prediction rate ^a	0.075 0.629	0.052 0.593	0.092 0.639	0.152 0.689	0.072 0.629	
No Arrears	Child support payments withheld from paycheck	-0.029* (0.014)	-0.047** (0.017)	-0.036* (0.014)	-0.044** (0.01)	-0.027** (0.01)	
	N	5172	5164	5169	5170	5159	
	Pseudo R-square Prediction rate	0.047 0.599	0.025 0.579	0.065 0.603	0.091 0.630	0.076 0.650	

Regressions include state and year effects. Other control variables are identical to those included in the regressions in columns 4 and 5 of Table 2.4. Standard errors clustered by state and year in parentheses. * significant at 5%; ** significant at 1%

^aI construct the prediction rate following Heckman and Smith (1999) by comparing the predicted probabilities to the population proportion p . If the predicted probability is larger than p , the observation is predicted to be 1 and is predicted to be 0 otherwise. The prediction rate is the rate of correct predictions. Random prediction corresponds to a prediction rate of 0.5.

Table 2.7: First-Stage Regressions: Difference Between Order Year and Law Year on Withholding Status

	All Cases	All Cases	Arrears	No Arrears
Difference, year of support order and year of law	0.014** (0.002)	0.015** (0.002)	0.011** (0.004)	0.016** (0.003)
Father owes back support		0.180** (0.014)		
Father has legal visit privileges		0.016 (0.014)	0.006 (0.027)	0.019 (0.016)
Father lives in same state		-0.006 (0.016)	-0.002 (0.027)	-0.003 (0.019)
Youngest child on support order is male		-0.005 (0.011)	0.001 (0.022)	-0.004 (0.013)
Age of youngest child on child support order		-0.005** (0.002)	-0.003 (0.003)	-0.005* (0.002)
Years between survey and original support order		0.012** (0.003)	0.007* (0.004)	0.014** (0.003)
Father figure lives in household		0.030 (0.017)	0.029 (0.033)	0.024 (0.021)
Mother receives food stamps		0.006 (0.020)	-0.015 (0.034)	0.007 (0.026)
Mother receives AFDC/ADC/TANF payments		0.036 (0.024)	-0.055 (0.039)	0.101** (0.033)
Constant	0.38** (0.02)	0.56** (0.06)	0.90** (0.10)	0.48** (0.07)
R-Square	0.086	0.169	0.110	0.152
N	6779	6668	1852	4816

Regressions include state and year effects and controls for mother's age, race, marital status, employment, education, and household income. Standard errors clustered by state and year in parentheses. * significant at 5%; ** significant at 1%

change the magnitude of the estimates greatly but explains much more of the variation in withholding status. When the sample is restricted to cases in which the father does not owe back support, the coefficient is larger: this is not surprising since fathers who owe back support were subject to withholding regulations prior to introduction of universal withholding laws and should be influenced less by their introduction. Owing back support increases the likelihood of withholding by 18 percentage points and, for fathers who do not owe back support, mothers' AFDC or TANF receipt increases the likelihood of withholding by 10 percentage points. Black mothers are more likely to have withholding while more highly educated mothers are less likely to have withholding: this may reflect differences in unobserved past payment history or welfare status by race and education. Alternately, judges or custodial mothers may pursue withholding more intensively depending on family demographics. The two-stage least squares instrumental variable regression results for parent-child contact are shown in

Table 2.8.²² When covariates are not included in the regressions, the IV estimates of withholding are very large and positive; however, when controls are included the IV estimates are negative (columns 2 and 4) or indistinguishable from zero for fathers who owe back support (column 3). The large positive coefficient in column 1 may be because newer support orders are more likely to be subject to withholding and the instrument takes on larger values for newer support orders. Newer support orders generally mean children are younger and the time since separation is shorter. Since the OLS results indicate that parents are more likely to spend time with younger children, the instrumented withholding dummies may proxy for the age of the children. A quick test suggests this is the case: when I add a control for the years between the survey and the original support order to the regression in column 1, the coefficient falls to -26.75. When all covariates are included in the regressions, withholding appears to have a large negative effect on the frequency of parent-child contact for fathers who do not owe back support; for fathers who owe support the coefficient is positive but not significant. The negative results for fathers who do not owe back support are sensitive to inclusion of the “Years between survey and original support order” variable, which proxies for the length of time since the parents’ relationship ended. This sensitivity may be because fathers are closer to their children at the time the relationship ends than after more time passes. More recent orders are correlated with both closer father-child relationships and higher likelihood of withholding. The results are not sensitive to including additional controls for the amount of child support due annually, the fraction of child support due that is paid, or indicators for whether the mother contacted the state Office of Child Support Enforcement for help locating the father or enforcing a child support order.

The results for in-kind support (shown in Table 2.9) are similar. For fathers who do not owe back support, the coefficient on withholding is negative for all types of in-kind support and is significant for clothing and child care/camp. For fathers who owe back support, the results are not consistent in sign and are not significant with the exception of a positive coefficient on withholding for the “father provided money for medical care” outcome.

These results are not consistent with either the standard model of payment, in which payment method is neutral, or Salience Model I, in which withholding causes fathers to increase child support payments in response to a decline in salience. The

²²Since whether or not fathers owe back support is also endogenous and I do not have an instrument for back support, running the regression in equation (2.15) on the full sample with an interaction term is problematic. Instrumenting for the interaction term $W_i \times B_i$ using $([\text{year of support order} - \text{year of law}] \times B_i)$ does not generate significant results.

Table 2.8: IV 2SLS Results: Effect of Withholding on Number of Days Father Spends with Child

	All Cases	All Cases	Arrears	No Arrears
Child support payments withheld from paycheck	101.23** (17.64)	-35.09* (17.78)	11.86 (31.38)	-58.83** (22.73)
Father owes back support		-14.09** (3.74)		
Father has legal visit privileges		3.25 (1.71)	5.07* (2.83)	2.47 (2.28)
Father lives in same state		32.21** (1.52)	23.32** (2.40)	36.07** (2.05)
Youngest child on support order is male		4.56** (1.67)	3.66 (2.65)	4.89* (2.05)
Age of youngest child on child support order		-1.37** (0.36)	-1.44** (0.52)	-1.42** (0.43)
Years between survey and original support order		-0.81** (0.23)	-0.02 (0.44)	-1.01** (0.28)
Father figure lives in household		-1.93 (2.62)	-1.72 (4.97)	-1.63 (3.13)
Mother receives food stamps		-6.83* (3.03)	-1.54 (4.71)	-8.58* (4.26)
Mother receives AFDC/ADC/TANF payments		2.08 (4.37)	-1.76 (6.78)	7.50 (6.49)
Constant	2.62 (10.03)	72.50** (15.73)	10.73 (34.71)	87.90** (18.50)
Pseudo R-Square	-0.599	0.061	0.071	-0.009
N	6779	6668	1852	4816
First stage F-statistic	77.1	45.5	12.3	32.6

Regressions include state and year effects and controls for mother's age, race, marital status, employment, education, and household income. Standard errors clustered by state and year in parentheses. * significant at 5%; ** significant at 1%

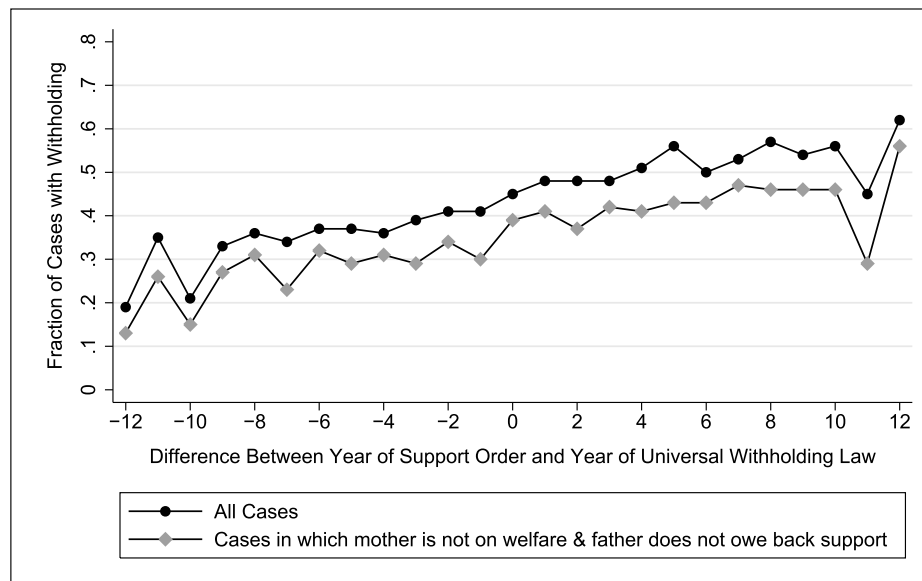
results are most consistent with Saliency Model II, in which withholding decreases the salience of child's consumption and, in turn, can reduce the father's preferred levels of support and time spent with the child for fathers who would choose to pay full support regardless of the withholding constraint. Since the effects are largest for "good" fathers (who should have higher-quality interactions with children) a parental bargaining story in which withholding weakens fathers' ability to pay for access to children seems problematic.²³

Table 2.9: IV 2SLS Results: Effect of Withholding on Parent-Child Contact and Additional Support

	Outcome						
	Days of Contact	Days of Contact	Food	Clothing	Medicine	Child Care/Camp	Gifts
Child support payments withheld	75.07* (29.84)	11.86 (31.38)	-0.10 (0.22)	-0.27 (0.29)	0.60* (0.25)	0.13 (0.10)	-0.10 (0.32)
Covariates	No	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R-Square	-0.307	0.071	0.064	-0.017	-0.686	-0.043	0.080
N	1874	1852	1949	1948	1948	1948	1944
First stage F-statistic	24.4	12.3	12.6	12.6	12.4	12.4	12.1
Child support payments withheld	131.13** (26.74)	-58.83** (22.73)	-0.21 (0.16)	-0.59** (0.18)	-0.07 (0.15)	-0.25** (0.09)	-0.03 (0.13)
Covariates	No	Yes	Yes	Yes	Yes	Yes	Yes
Pseudo R-Square	-0.846	-0.009	0.027	-0.207	0.073	-0.034	0.067
N	4849	4816	5172	5164	5169	5170	5159
First stage F-statistic	46.8	32.6	36.7	36.8	36.6	36.5	37.1

Covariates are identical to those in columns 4 and 5 of Table 2.4 if included. Regressions include state and year effects. Standard errors clustered by state and year in parentheses. * significant at 5%; ** significant at 1%

Figure 2.5: Implementation of Withholding Before and After State Universal Withholding Laws



Choice of Specification for Instrument

The instrument (year of support order - year of universal withholding law) takes on negative values when the support order pre-dates the law and positive values otherwise. Does including these negative values, i.e., including variation in the length of time prior to the withholding law, provide useful information? Figure 2.5 shows that the fraction of cases in which withholding applies increases as the difference between date of the support order and the date of the universal withholding law increases.^{24,25} This suggests that the timing of the support order relative to the timing of the law is important for orders issued both before and after the law. Most likely this is due to how withholding orders were issued for individual cases: judges and child support enforcement officers had discretion to require withholding in individual cases prior to passage of universal withholding laws, and in some states judges had discretion to forego withholding even after the laws were passed. It is reasonable to believe that judges' use of withholding changed with changes in the legal climate and that the legal

²³As noted above, intrinsic motivation arguments are also consistent with these results. However, if intrinsic motivation is the best explanation, we might expect to see negative effects of withholding for parents who owe back support as well.

²⁴The decline in fraction of cases with withholding for very high values of the instrument is due to very small numbers of cases in these categories.

²⁵Figure 2.5 shows the fraction of cases with withholding for values of the instrument between -12 and 12. The full range of values for the instrument for the sample used in Tables 2.7, 2.8, and 2.10 is -28 to 21; results are not sensitive to restricting the sample to the -12 to 12 range.

climate would increasingly favor withholding as the law date drew nearer. This bidirectional instrument consequently captures more information than a unidirectional instrument which only uses variation in time after the law passed.

Table 2.10 considers alternative instruments: the first panel of the table restates the main IV results shown in Table 2.8. In the second panel, the instrument is an indicator variable equal to 1 if a universal withholding law applies to the case and 0 otherwise. The results are qualitatively similar to those in the main IV specification, although the effect of withholding on visits is smaller in magnitude and is not significant. The first-stage F-statistics for this version of the instrument are lower than for the main specification. The third panel of Table 2.10 presents results from a unidirectional version of the instrument used in the main IV results: for cases in which the support order was issued after the universal withholding law passed, the instrument's value is the number of years between the order date and the law. For cases in which the support order predates the law, the variable takes on a value of zero. The first-stage F-statistics indicate that this version of the instrument is much weaker; the results show the same pattern as in the main specification of the instrument.

Magnitude of the IV Results: IV as Local Average Treatment Effect

The IV estimates for the relationship between withholding and parent child contact shown in Tables 2.8-2.10 are very large in magnitude. In fact, the point estimates are similar in magnitude to the unconditional mean of the visits variable. The standard errors on these estimates are also large, so relatively little importance should be placed on the value of the point estimate. However, one explanation for the size of the IV estimates may come from who is affected by the withholding law. IV estimates apply to marginal cases for which the the law affects withholding status. The 2SLS estimate can be thought of as a weighted average of the effect of withholding on different types of people who are affected by the instrument. If the subgroup of child support cases that are affected by withholding laws have a larger effect of withholding on visits, this would account for the large magnitude of the IV estimates.

The instrumental withholding laws are most likely to affect fathers who were not already subject to withholding under other laws. As discussed above, universal withholding laws should be less likely to change withholding status for fathers who owe back support or cases in which the mother receives welfare. In addition, withholding laws are more likely to affect withholding status for fathers who live in the same state as the custodial mother; states have difficulty enforcing withholding orders across

Table 2.10: IV 2SLS Results with Different Instruments: Father Child Contact Days

	Order year - Withholding law year		
	All Cases	Arrears	No Arrears
Child support payments withheld	-35.09*	11.86	-58.83**
	(17.78)	(31.38)	(22.73)
Constant	72.50**	10.73	87.90**
	(15.73)	(34.71)	(18.50)
Pseudo R-Square	0.061	0.071	-0.009
N	6668	1852	4816
First stage F-statistic	45.5	12.3	32.6

	Binary Instrument Equals 1 if (Order Year - Withholding Law Year) > 0, 0 Otherwise		
	All Cases	Arrears	No Arrears
Child support payments withheld	-15.02	16.82	-29.46
	(33.18)	(77.80)	(38.23)
Constant	57.46*	5.72	66.88*
	(25.92)	(80.01)	(28.26)
Pseudo R-Square	0.095	0.062	0.077
N	6668	1852	4816
First-stage F-statistic	16.4	2.4	14.1

	(Order Year - Withholding Law Year) if > 0, 0 Otherwise		
	All Cases	Arrears	No Arrears
Child support payments withheld	-83.87	-7.79	-98.43*
	(44.46)	(94.29)	(48.65)
Constant	109.03**	30.60	116.22**
	(34.59)	(96.58)	(36.62)
Pseudo R-Square	-0.176	0.077	-0.240
N	6668	1852	4816
First stage F-statistic	8.7	1.5	7.9

Regressions include state and year effects and all covariates included in the main IV specification. Standard errors clustered by state and year in parentheses. * significant at 5%; ** significant at 1%

state lines. Judges and child support enforcement officers had discretion to pursue withholding prior to universal withholding laws. When the laws removed that discretion, some fathers were more affected than others. If judges and enforcement officers considered income, education, age, or race when deciding whether to pursue withholding, we might expect high-income, highly educated white fathers to be most affected by universal withholding laws. If mothers had contacted their state Office of Child Support Enforcement for help obtaining child support, withholding laws would also be less likely to affect real withholding status because the enforcement office might use their discretion to initiate withholding in response to the mother’s complaint. Generally, universal withholding laws are blind to father characteristics and will consequently affect “good” fathers who were not subject to withholding for other reasons but against whom withholding can be implemented successfully.

Card (1995) suggested that if instruments affect groups differently, we can use an index to measure the extent to which an observation is likely to be affected by the instrument. Kling (2001) describes a procedure that compares groups with similar exposure to the instrument and treats the IV 2SLS estimate as a weighted average of effects within each subgroup; groups affected more by the instrument receive a higher weight. I follow these procedures to examine which child support cases are affected by the binary version of the law instrument and consequently contribute most to the large IV estimates.

First, I construct an index of parent quality: predicted withholding status based on the factors (other than universal withholding laws) that determine withholding, such as demographic characteristics, past payment behavior, whether the father lives in the state, mothers’ contact with state welfare agencies, and state and year effects to account for non-withholding features of state child support enforcement. To construct the index, I estimate a linear probability regression of withholding on the independent variables using only the observations unaffected by a universal withholding law (for which the support order was issued prior to universal withholding).²⁶ I then use the coefficients from this regression to predict the probability of withholding in the absence of universal withholding laws for the full sample.

I divide this sample into four quartiles based on the 25th, 50th, and 75th percentiles of the estimation sample. The lowest quartile includes individuals least likely to have withholding in the absence of withholding laws. Table 2.11 shows summary statistics for each quartile. Mothers in cases in the lowest “probability of withhold-

²⁶Although the outcome variable is binary, I use a linear probability model because it is simpler to calculate the variance of the probability of withholding in the linear framework.

Table 2.11: Characteristics of “Probability of Withholding” Quartiles

	Lowest	2nd	3rd	Highest
Child support payments withheld from paycheck	0.18	0.32	0.50	0.73
Number of days child spent with father last year	61.7	60.5	57.3	54.1
Father owes back support	0.02	0.06	0.22	0.52
Mother’s age	41.1	38.6	36.1	33.2
Mother is black	0.01	0.05	0.10	0.20
Mother is hispanic	0.03	0.04	0.07	0.09
Mother is currently married	0.38	0.33	0.32	0.24
Mother never married	0.01	0.04	0.11	0.27
Mother unemployed or not in labor force	0.10	0.15	0.22	0.33
Family income percentile, national ranking	11.0	9.0	7.3	5.4
Mother has HS degree but not 4-year college	0.61	0.73	0.77	0.78
Mother has 4-year college degree or more	0.37	0.23	0.14	0.08
Father has legal visit privileges	0.74	0.71	0.67	0.62
Father lives in same state	0.83	0.79	0.78	0.80
Father figure lives in household	0.50	0.50	0.50	0.47
Age of youngest child on child support order	12.3	11.3	10.2	8.6
Years between survey and original support order	7.45	6.88	6.40	5.78
Mother receives food stamps	0.02	0.07	0.15	0.36
Mother receives AFDC/ADC/TANF payments	0.00	0.01	0.07	0.22
Help from OCSE: locate other parent	0.00	0.01	0.04	0.13
Help from OCSE: establish support obligation	0.04	0.09	0.19	0.38
Help from OCSE: obtain collection	0.05	0.16	0.32	0.56
Fraction of due child support paid last year	1.02	1.09	1.07	0.94
N	997	1210	1403	1934

ing” quartile are older on average, are much less likely to be black or hispanic, are more likely to have remarried, have higher income and are highly educated, and their children are older on average. In addition, they are much less likely to have had help from their state Office of Child Support Enforcement and on average fathers in the highest quartile paid a lower fraction of support due. In summary, before universal withholding laws, “bad” parents were more likely to have withholding. This comparison confirms that the withholding prediction regression divides cases along the “parent quality” dimension.

Next I estimate quartile weights for the IV estimates to measure how much results from each quartile impact the overall IV coefficients. In essence, the IV estimate weights the average treatment effects of withholding within each subgroup. I construct the weights following procedures laid out in Kling (2001) using the binary version of the law instrument. First, weights depend positively on the fraction of the

Table 2.12: IV Weights for Each “Probability of Withholding” Quartile

All Cases				
	Sample Fraction in Quartile	Variance of Binary Instrument by Quartile	$\Delta(\text{Withholding}) q$	IV Weight
Lowest quartile	0.180	0.130	0.101 (0.032)	0.454
2nd quartile	0.218	0.121	0.072 (0.030)	0.363
3rd quartile	0.253	0.114	-0.004 (0.028)	-0.020
Highest quartile	0.349	0.089	0.034 (0.027)	0.204

Does Not Owe Back Support				
	Sample Fraction in Quartile	Variance of Binary Instrument by Quartile	$\Delta(\text{Withholding}) q$	IV Weight
Lowest quartile	0.188	0.124	0.130 (0.037)	0.557
2nd quartile	0.219	0.118	0.084 (0.034)	0.396
3rd quartile	0.276	0.106	-0.009 (0.032)	-0.048
Highest quartile	0.317	0.097	0.017 (0.032)	0.096

Quartiles q were constructed by running a linear probability regression of withholding on the determinants of withholding listed in Table 2.11 and state and year effects for cases not subject to universal withholding laws, then generating predicted withholding for the full sample. Quartiles were constructed based on the 25th, 50th, and 75th percentiles of the estimation sample.

Column (1) reports the fraction of the overall sample in each quartile.

Column (2) is the estimated conditional variance, within each quartile, of the dummy variable indicating a universal withholding law applies to the case: $\hat{E}[P(Z|x, q)(1 - P(Z|x, q))]$.

Column (3) is the coefficient on the interaction term for the instrument dummy \times the quartile dummy in a regression of withholding on the quartile dummies, interaction terms, and x .

Column (4) is the IV weight: multiply columns (1), (2), and (3) and sum for all quartiles. The weight for each quartile is the row product over the sum.

sample within each quartile, which need not be equal because quartile cutoffs were estimated using only the subsample for which universal withholding laws did not apply: fractions are $f_q = P(Q)$ where Q identifies the population subgroup. Second, IV estimates with controls X and quartiles q are weighted by the within-quartile variance of the instrument Z conditional on X , $\lambda_{q|x} = P(Z|X, Q)(1 - P(Z|X, Q))$ (Angrist 1998). Quartiles in which there is more variance in the instrument across observations similar on X s receive more weight. Last, weights depend on the within-quartile impact of the binary withholding-law instrument Z on actual withholding: $\Delta W_{q|x} = E[E(W|Z = 1, X, Q) - E(W|Z = 0, X, Q)|Q]$. Naturally, quartiles in which the instrument has a larger effect on actual withholding receive more weight in the IV. The IV weights are

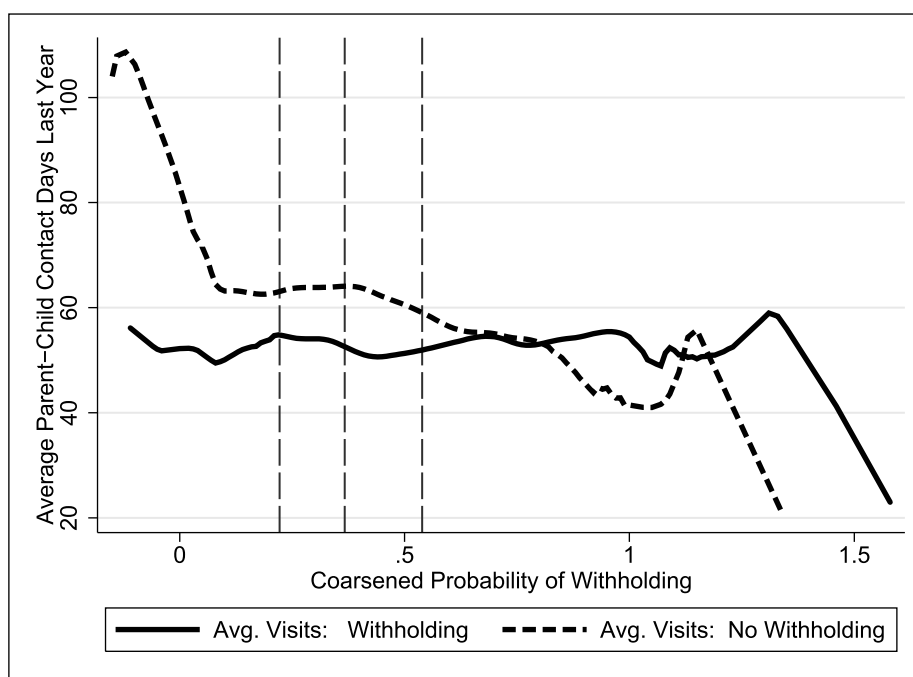
$$\omega_q = \frac{f_q \lambda_{q|x} \Delta W_{q|x}}{\sum_q f_q \lambda_{q|x} \Delta W_{q|x}}.$$

Components of the weights for each quartile are shown in Table 2.12. Consistent with the expectation that universal withholding laws affected parents who were not otherwise candidates for withholding, the IV estimates place weight on the lower quartiles, that is, on cases with low probability of withholding in the absence universal withholding laws. The IV coefficients estimate the effect of withholding for relatively “good” fathers who have strong payment histories and have not caused mothers to contact child support enforcement agencies. These IV estimates are weighted heavily towards child support cases in which the custodial parent is more highly educated and has higher income and the children are older.

The IV estimates heavily weight the observations for which selection bias seems least likely to be of concern (either before or after instrumenting). The observations that contribute most to the IV results are cases for which payment amount and timing are least likely to change if withholding is imposed.²⁷ If salience is the mechanism by which payment method affects behavior, larger effects for fathers whose payment method was determined by default (due to the withholding laws) are reasonable. Figure 2.6 is a lowess-smoothed graph that shows how the difference between average visits for fathers with and without withholding varies as the probability of withholding (as estimated above) varies: for “good” fathers with low probabilities, withholding is associated with lower visits. The dashed vertical lines divide the region into the four quartiles: outliers result in wider ranges for the highest and lowest quartiles.

²⁷The negative weights for cases in the third quartile are because the average difference between withholding status for cases subject to and not subject to withholding laws ($E[E[\text{withholding}|\text{law applies}, X] - E[\text{withholding}|\text{law does not apply}]]$) is negative for this quartile. The estimate comes from the interaction term of the regression described in the table footnotes; it is negative but insignificant.

Figure 2.6: Heterogeneous Treatment Effects: Difference Between Father-Child Contact by Withholding Status



The graph provides an explanation for the large IV estimates: the lowest quartile, which receives the highest weight, has a group of fathers not subject to withholding with particularly high average visit levels, and the difference for this small group of fathers appears to drive the magnitude of the result. This group’s predicted values of withholding are below zero; only 3 percent of the sample has predicted probabilities out of the 0-1 range. However, restricting the sample to exclude the observations that generate out-of-sample predictions and re-estimating the instrumental variable regressions on this subsample does not greatly reduce the size of the IV coefficients. Generally, withholding appears to decrease parent-child contact among “good” fathers with the lowest probabilities of withholding prior to universal withholding laws. This suggests that the change in payment method has non-neutral effects on parental behavior and that bargaining models may not explain the effects.

2.7 Conclusions

Automatic payment methods such as automatic bill pay or income withholding may alter the salience of consumer expenditures, but estimating the effect of payment method on behavior is generally difficult because choice of payment method is en-

ogenous. I use an unusual source of exogenous variation in payment method, state laws governing default payment methods for child support, to estimate the effect of automating child support payments on parents' behavior. I develop formal models of the effect of child support payment method when (1) payment method is neutral; (2) payment method alters the salience of support paid; and (3) payment method alters the salience of child's consumption. Using the difference between the year of the child support order and the year of the state withholding law to instrument for whether child support was paid via withholding, I find evidence that, for the sample of fathers whose payment amounts are unaffected by the law, fathers who pay child support via withholding spend less time with their children and provide less in-kind child support than fathers who make direct child support payments. For fathers whose payment amounts change in response to increased enforcement, I detect no significant effect. These results are consistent with a model of child support in which withholding lowers the salience of child's consumption and reject standard models in which behavior does not depend on payment method.

Payment method may affect behavior through mechanisms other than salience. One possibility is that, rather than reducing salience, child support withholding reduces fathers' sense of responsibility toward their children and reduces their intrinsic motivation to spend time with them. Another possibility is that mothers and fathers bargain over child support and mothers restrict access to children when fathers do not pay. If withholding increases mothers' bargaining power, fathers may be less able to "pay" for access to their children. Although the results above cannot confirm whether payment effects are due to salience, it is difficult to reconcile the results with a standard model in which payment method does not affect choice. The results above strongly indicate that, as payments in many arenas become increasingly automated, we can expect behavioral responses. Automatic payments offer many efficiency gains: they are an extremely efficient means of child support enforcement. However, as payments become increasingly automated we may expect more behavioral changes; new sources of variation in payment method as automation becomes more available may help determine the size of those effects and the mechanisms through which they work.

Appendix Table 2.13: Year of Implementation of Child Support Income Withholding Laws, by State

	Universal	Immediate	Wage	Universal	Immediate	Wage
Alabama	1993	1989	1984	1991	1989	1985
Alaska	1994	1988	1981	1994	1991	1985
Arizona	1987	1987	1977	1993	1989	1985
Arkansas	1991	1989	1983	1993	1993	1985
California	1989	1989	1980	1990	1990	1981
Colorado	1993	1989	1981	1993	1990	1985
Connecticut	1983	1983	1955	1994	1990	1977
Delaware	1994	1990	1974	1993	1989	1975
District of Columbia	1995	1990	1987	1989	1989	1979
Florida	1986	1986	1978	1993	1993	1986
Georgia	1993	1989	1981	1994	1989	1978
Hawaii	1988	1988	1984	1993	1993	1985
Illinois	1988	1988	1984	1989	1989	1985
Indiana	1993	1990	1986	1994	1990	1980
Indiana	1997	1985	1982	1994	1989	1985
Iowa	1993	1990	1984	1990	1990	1986
Kansas	1992	1990	1985	1994	1990	1985
Kentucky	1988	1988	1984	1997	1985	1985
Louisiana	1993	1989	1982	1993	1990	1977
Maine	1991	1990	1985	1989	1989	1983
Maryland	1993	1991	1976	1995	1988	1982
Massachusetts	1998	1986	1986	1994	1989	1984
Michigan	1990	1990	1982	1993	1990	1986
Minnesota	1993	1990	1978	1989	1985	1977
Mississippi	1993	1989	1985	1994	1989	1986
Missouri	1993	1990	1973			

*Source: Huang et al. (2002). The authors do not report a date for universal withholding in Texas. Texas' legal code (Family Law Chapter 158 subchapter A section 158.001-002) states that the court may suspend income withholding for good cause or on agreement of the parties. However, Texas' law was strengthened in 1995 and 1997 to make withholding the default.

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Chapter 3

State Earned Income Tax Credits and Participation in Regular and Informal Work

3.1 Introduction

The federal earned income tax credit (EITC) was designed to incentivize work and to lift working families out of poverty. As of tax year 2007, 20 states offered state earned income tax credits in the form of an additional percentage of the federal credit. State EITCs share the goals of the federal credit, which has been shown to increase labor force participation for single women and to help recipients pay off debt, purchase durables, pay for education, or otherwise invest.¹ In addition, many state EITCs were intended to reinforce work incentives associated with welfare reform and to offset state taxes on families below the poverty line.

Low-income individuals face strong incentives to evade taxes by underreporting income or not filing taxes. Earned income tax credits make work in the regular sector more attractive relative to work in the informal sector by increasing the relative after-tax wage in the regular sector. In addition, state credits result in combined state and federal EITCs that more than offset the employee's share of payroll taxes, even for many childless workers whose federal EITC just offsets the employee's share of payroll taxes, and may change whether working on-the-books has a net positive or net negative effect on after-tax income. Consequently, we might expect combined state and

¹For labor supply responses to the EITC, see (for example) Eissa and Liebman (1996), Ellwood (2000), Grogger (2003), and Hotz, Mullin and Scholz (2006). See Smeeding, Ross and O'Connor (2000) and Romich and Weisner (2000) for research on how EITC recipients use their credits.

federal EITCs to encourage taxpayers to choose regular-sector over informal-sector employment or to report off-the-books work. Additionally, because state EITCs function as a percentage of the federal credit, which has different value depending on the number of qualifying children claimed, state EITCs increase the value of claiming an EITC-qualifying child and may induce some men to claim more children for tax purposes. By claiming more children, men receive credits with higher phase-in rates that further reduce their regular-sector marginal tax rates, making regular-sector work even more attractive relative to informal-sector work.

In this paper I use state-level variation in earned income tax credits to estimate labor supply responses to the tax credits for both regular-sector work and informal work for unmarried, low-income urban men. Four waves of data spanning tax years 1997-2005 from the Fragile Families and Child Wellbeing Survey provide detailed information about types and amounts of informal work. Fragile Families data indicates that rates of participation in the underground sector are high for unmarried men. Approximately 30% of unmarried men report working in the informal sector during each wave of the survey. The high reported rates of informal work among these men make them good candidates for evaluating the impact of taxes on informal-sector labor supply.

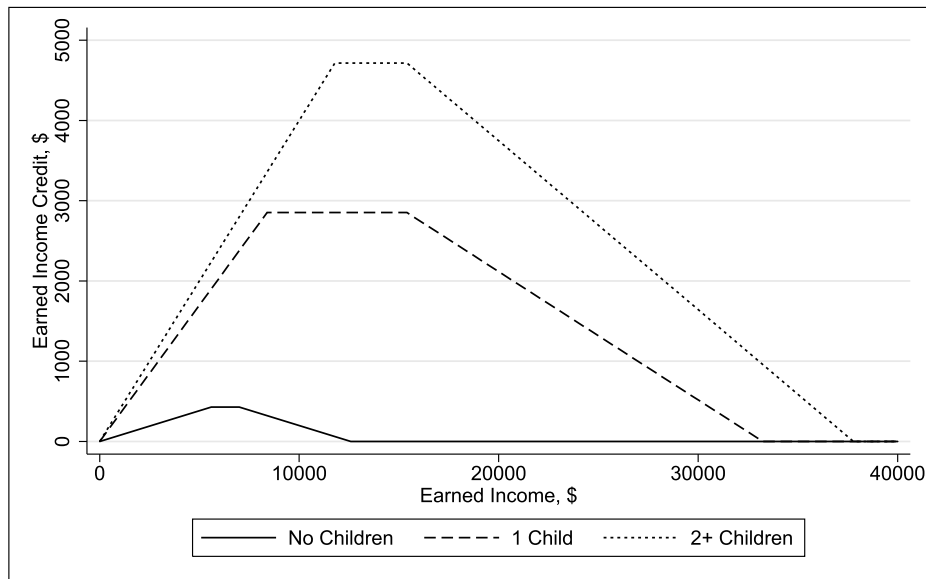
Previous studies, which focus primarily on the regular-sector labor supply of single women with children, have found large employment effects of the EITC but generally no effects on hours. In contrast, I find that among unmarried urban men with at least one child born out-of-wedlock an increase in the state EITC (through an increase in the percentage of the federal credit) has large and significant negative effects on participation in informal work but no significant effect on participation in the regular sector. Usual hours worked per week increase in the regular sector and decrease in the underground sector in response to increases in the EITC, although there is no significant difference in total hours.

I proceed with a brief description of the earned income tax credit and the literature analyzing its effects in Section 3.2. Next, in Section 3.3 I present a simple model that illustrates the lack of clear theoretical predictions for changes in marginal tax rates and refundable credit phase-in rates on participation in the informal sector. Section 3.4 describes the Fragile Families data and my measures of informal work. In Section 3.5 I present results for both extensive-margin and intensive margin responses and discuss the magnitudes of the effects and their sensitivity. Section 3.6 concludes.

3.2 The Earned Income Credit

The Earned Income Credit was designed to reward work and is currently the largest cash transfer program for low-income families. It is characterized by (1) a phase-in region in which earned income is low and only part of the credit may be claimed; (2) a plateau region of earned income in which taxpayers can claim the maximum credit; and (3) the phase-out region in which the amount of the credit that may be claimed decreases with income. Because the credit is refundable, the amount of the credit claimed may be greater than the tax liability, resulting in a refund. The EITC was introduced in 1975 and was expanded over time with the most recent major expansion introduced by the 1993 act, which increased maximum credit levels and extended the EITC to childless workers in 1994. From tax years 1996-2007, the federal EITC phased in at rates of 7.65, 34, and 40 percent for taxpayers with no children, one child, and two or more children respectively. Phase-out rates also stayed constant at 7.65, 15.98, and 21.06 percent. Prior to 2002, the income cutoffs for the phase-in, plateau, and phase-out regions did not depend on taxpayers' filing status. In 2002, law changes extended the length of the plateau region for "married filing jointly" taxpayers by \$1000 of earned income in 2002 and by \$2000 beginning in 2005 but did not alter phase-in or phase-out rates. Figure 3.1 shows the structure of the credit for individuals who file as single, head-of-household, or qualified widow(er) in tax year 2007. The only variation in EITC rates at the federal level during the time

Figure 3.1: Federal Earned Income Credit Amounts in Tax Year 2007, Single, Head-of-Household, or Qualified Widow(er) Filing Status



period covered in this study comes from the change in the lengths of the plateau and phase-out regions, which affects only a small percentage of taxpayers. However, there is considerable variation in state earned income credits during this period. Between 1997-2005 (the tax years spanned by the Fragile Families survey data), 16 states introduced or changed earned income credits based on the federal credit, effectively altering the maximum credit and phase-in and phase-out rates over time and providing exogenous variation in EITC parameters. Table 3.1 shows the states with earned income credits based on the federal credit between 1996 and 2007: 14 states introduced new credits after expansion of the federal credit, and states with pre-existing credits modified them. Michigan and Washington will both add credits in upcoming tax years. Due to the selection process for the Fragile Families data, discussed below, not all states are represented in the sample; states in bold in the table below contain cities in which baseline Fragile Families interviews were conducted.

Table 3.1: State Earned Income Tax Credits Based on Federal Credit, 1996-2007

State	Year(s)	% of Federal Credit	Refundable?	Notes
Colorado	1999	8.5	Yes	Credit is available only when state ends year with Taxpayer Bill of Rights (TABOR) surplus. Not available 2002-2007 due to lack of budget surplus.
	2000-2007	10	Yes	
Delaware	2006-2007	20	No	
District of Columbia	2000	10	Yes	
	2001-2004	25	Yes	
	2005-2007	35	Yes	
Illinois	2000-2007	5	As of 2003	Coverage for parents without qualifying children expanded in 2007.
Indiana	2003-2007	6	Yes	Between 1999-2002, Indiana had an earned income credit that was not based on the federal credit.
Iowa	1996-2006	6.5	No	
	2007	7	Yes	
Kansas	1998-2001	10	Yes	
	2002-2006	15	Yes	
	2007	17	Yes	

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... Table 3.1 continued

State	Year(s)	% of Federal Credit	Refundable?	Notes
Maine	2000-2002	5	No	
	2003-2005	4.92	No	
	2006-2007	5	No	
Maryland	1996-2007	50	Partially	Made 10% of the federal credit refundable for 1998-1999, 15 percent for 2000, 16 percent for 2001-2002, 18 percent for 2003, 20 percent for 2004-2007.
Massachusetts	1997-2001	10	Yes	
	2001-2007	15	Yes	
Minnesota	1996-1997	15	Yes	
	1998-2007	averages 33%	Yes	Eligibility for the Minnesota credit is determined by eligibility for the federal credit, but its structure is different. The credit varies between 25 and 45 percent of the federal credit depending on income.
Nebraska	2006	8	Yes	
	2007	10	Yes	
New Jersey	2000	10	Yes	Prior to 2007, only taxpayers with an eligible child and gross income < \$20,000 could claim the credit.
	2001	15	Yes	
	2002	17.5	Yes	
	2003-2007	20	Yes	
New Mexico	2007	8	Yes	
New York	1996-1999	20	Yes	
	2000	22.5	Yes	
	2001	25	Yes	
	2002	27.5	Yes	
	2003-2007	30	Yes	
Oklahoma	2002-2007	5	Yes	
Oregon	1997-2007	5	As of 2006	
Rhode Island	1996-1997	27.5	No	In 2003, 10% of the state credit became refundable (or 2.5% of federal credit). In 2006, 15% of the state credit
	1998	27	No	
	1999	26.5	No	
	2000	26	No	

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... Table 3.1 continued

State	Year(s)	% of Federal Credit	Refundable?	Notes
	2001	25.5	No	became refundable (3.75% of federal credit).
	2002-2007	25	Partially	
Vermont	1996-1999	25	Yes	
	2000-2007	32	Yes	
Virginia	2006-2007	20	No	
Wisconsin	1995-2007	4, 1 kid	Yes	
		14, 2 kids	Yes	
		43, \geq 3 kids	Yes	

States in bold contain cities that were part of the Fragile Families baseline sample. Some states had Earned Income Credits before 1996 (not shown).

EITC benefits depend on whether filers claim zero, one, or two qualifying children under age 19. Eligible qualifying children must have valid taxpayer identification numbers, live with the tax filer at least half the year, and not be claimed by another filer. Children, step-children, adopted children, and their descendants, and in some cases siblings or step-siblings and their descendants, can count as children. Tax filers who claim the child for EITC purposes need not claim the child as a dependent. The residency requirement is the key test for eligibility. Prior to 1991, EITC-qualifying children had to pass both the residency test and a support test: the filer had to provide at least half the cost of supporting the child. The support test was eliminated because it was complex and difficult to enforce. IRS rules for who can claim the child if two tax filers both meet the requirements have varied over time. Prior to 2002, only the higher-income individual was eligible to claim the child if two taxpayers met the residency test.² In 2002, this “AGI tiebreaker rule” was simplified. Instead of determining who is eligible to claim the child, it is now used only to allocate the child in cases where two taxpayers fail to coordinate and both claim the child.

EITC Noncompliance

The EITC has been criticized for high rates of noncompliance: an Internal Revenue Service (2002) study of 1999 tax returns found that up to \$9.9 billion (31.7 percent) in EITC dollars claimed exceeded the amount to which taxpayers were eligible. These

²Consequently, if unmarried parents cohabited and men had higher income, fathers rather than mothers were eligible to claim the EITC with qualifying children.

IRS figures do not account for underpayments to eligible claimants who did not file taxes or filed but did not claim the EITC, or for offsetting cases in which an EITC child was claimed by an ineligible taxpayer but not claimed by an eligible one, and consequently they are likely to overstate excess EITC payments. IRS studies indicate that EITC noncompliance takes three main forms: claiming non-qualifying children (49% of value of overclaims), misstating income (21%), and choosing an incorrect filing status (11%) (Internal Revenue Service 2002).³ As of 1994, over half the value of EITC overclaims from non-qualifying children errors was due to failure to meet the residency test. Less than 2 percent of the qualifying child error arose when taxpayers claim non-existent children (McCubbin 2000). Elimination of the AGI tiebreaker rule (which accounted for a quarter of the error) means that failures of the residency test are likely to account for an even larger percentage of EITC qualifying-child error today.

Although a large proportion of EITC noncompliance is due to inappropriately claiming qualifying children, we know little about the extent to which these errors are intentional. Complexity in EITC rules, particularly because definitions of EITC-qualifying children differ from definitions of children eligible for the dependent exemption, child tax credit, child and dependent care tax credit, and head-of-household filing status, makes credit eligibility difficult to understand (Holtzblatt and McCubbin 2003). Prior to 2002, the “AGI tiebreaker rule” produced complexity because in some cases the IRS ruled that biological parents were ineligible for the credit when higher-income boyfriends, girlfriends, or relatives of the child’s parent lived in the household (McCubbin 2000, Hotz and Scholz 2001).

Liebman (1998) argues that inadvertent error should not respond to the value of the credit and uses variation in the value of claiming children caused by the Tax Reform Act of 1996 to estimate the percent of EITC error that is due to intentionally claiming ineligible children. Using data from the 1985 and 1988 Taxpayer Compliance Measurement Program, which increased the return to claiming children by different amounts for different taxpayers, he estimates that about 46 percent of EITC overpayments due to errors in claiming qualifying children went to filers who responded to the EITC incentives for noncompliance, while other child-related EITC overpayments went to taxpayers who would wrongly claim children even in the absence of the EITC.

³Errors due to claiming ineligible children were due to both claiming ineligible children (25% of total errors) and claiming children who were otherwise eligible except that a higher-income taxpayer lived in the household (17%). About 6.7% of errors arose because filers both claimed a non-qualifying child and did not file as married filing separately. These three sub-types of error are all included in the “claiming non-qualifying children” category.

Liebman concludes that a 45-percent increase in the maximum EITC increased the noncompliance rate (in terms of whether children were claimed) by 14 percent. Using more recent data from the 1994 IRS Criminal Investigations Division and the IRS Statistics of Income, McCubbin (2000) also finds that the probability of noncompliance is associated with the size of the credit. Her estimates indicate that increasing the EITC by 10 percent would lead to a 4-percent increase in the probability of claiming a child and a 14-percent increase in the dollar amount of EITC overclaims. Some evidence suggests that men are more likely to claim ineligible children for EITC purposes than women (Liebman 2000, Government Accountability Office 2004).

Law changes and IRS enforcement efforts introduced after these studies are likely to have decreased EITC overclaims. The 1994 introduction of a small EITC for filers without qualifying children may have reduced incentives to claim ineligible children for the EITC. Elimination of the support rule and elimination of the AGI tiebreaker rule reduced the complexity of determining which taxpayers could claim children for EITC purposes. Additionally, now the IRS automatically matches tax returns to several databases to screen qualifying child claims: the Federal Case Registry directory of child support cases with information on custodial and non-custodial parents; KidLink, a Treasury Department database that matches children and parents based on Social Security numbers; DM-1, a database of taxpayer identification numbers and associated names; and Numident, a Social Security database of birth certificate information with parent names. Last, the IRS has required qualifying child certification for a small number of filers.

The second-largest identified source of EITC noncompliance is income misstatement. Incentives for income misstatement vary depending on the region of the credit. Taxpayers in the phase-in region of the credit have incentives to overstate income to receive a larger credit, while taxpayers in the phase-out region can increase credit amounts by understating income. Generally, most error appears to be due to underreporting rather than overreporting (McCubbin 2000, Internal Revenue Service 2002). Taxpayers may not understand the structure of the credit well enough to engage in strategic overreporting, and it may be easier to underreport by not reporting self-employment income or leaving out W-2s than to overreport by generating false W-2s or filing false schedules for self-employment income. Although it would be relatively simple to overreport self-employment income, payroll taxes triggered by self-employment may discourage this even though the EITC would more than offset payroll taxes for filers with qualifying children. Additionally, relatively few EITC filers claim self-employment income: only 17.6 percent of filers claim self-employment

income and 54.3 percent of those that do are in the phase-out region of the credit and could receive larger credits with (larger) underreporting (Hotz and Scholz 2001).

3.2.1 Previous Empirical Research

Previous work on the effects of EITC expansions on single women's labor supply find large increases in labor force participation and no change in hours worked among existing participants. Several papers use natural experiments to identify the impact of the EITC on participation in the regular labor market. Eissa and Liebman (1996) examine the effect of the Tax Reform Act of 1986's expansion of the EITC on the labor supply of single women with children compared to the labor supply of single women without children. They find that single women with children (who are most likely to be affected by the EITC) increased labor force participation by 2.8 points relative to single women without children, and find no effect on hours; see also Meyer and Rosenbaum (2000) and Ellwood (2000) for analyses of female labor supply effects for single mothers using cross-sectional data and variation in marital status and number of children. Cancian and Levinson (2005) use 2000 Census data and compare labor market participation of Wisconsin parents (which supplements the federal credit at rates of 4, 14, and 40% for taxpayers with one, two, or three qualifying children) to that of parents in states that don't supplement the EITC. They find large increases in labor force participation and no effect on hours worked. Grogger's (2003) paper on the effects of welfare and EITC features on female labor supply finds that the credit reduces welfare take-up rates and a \$1000 increase in the EITC increases employment of female family heads by about 3.6 percentage points. Hotz et al. (2006) were the first to use longitudinal data to analyze the EITC, which allowed them to control for time-invariant unobservables and avoid bias due to changes in group composition over time. They use California administrative data and find large positive effects of the EITC on employment: they compare employment rates for two-child single-parent families to rates for one-child families and find higher rates of labor force participation among two parent families (who receive larger credits). Eissa and Hoynes (2004) point out that since the EITC is based on family income, it has labor supply disincentive effects for married workers as taxes do for most secondary earners. They examine EITC expansions between 1984-1996 and find that EITC reduced labor supply of secondary earners in married couples.

With the exception of Hotz et al. (2006), most studies of the EITC identify the labor supply effects by comparing labor supply changes of groups eligible for the credit,

such as single mothers, with the labor supply changes of ineligible groups such as women without children. Using longitudinal data like the Fragile Families data has two main benefits: first, it allows me to control for unobserved time-invariant individual characteristics, such as taste for risk. Second, by following the same individuals over time, longitudinal data is less affected by changes in group composition than pooled cross-sectional data.

No studies have examined labor supply responses to the EITC for single men. While the credits for which they are eligible are smaller on average if we assume they cannot claim qualifying children, low-income men may be more likely to participate in the underground economy than women and consequently may be more able to shift between taxed and untaxed sectors in response to changes in the tax rate. Men may be more likely to work in sectors with high rates of off-the-books work. Low-income men are not eligible for many of the means-tested benefits available to single mothers and might be more responsive to tax changes because taxes interact with fewer program rules. Understanding whether and how much men's informal and regular labor supply responds to tax changes will help us understand the effects of tax laws on real labor supply rather than only formal work or taxed income. In addition, understanding who claims the EITC with a qualifying child will help us evaluate EITC incidence and better understand EITC noncompliance.

3.3 Participation in the Informal/Untaxed Sector

3.3.1 Previous Empirical Work

Due to the lack of data on off-the-books work, there are few empirical studies of the relationship between tax rates and informal labor supply. Notable exceptions to this are two studies from the early 1990s that use a survey of workers in Quebec in which jobs were classified based on whether they were reported to tax authorities. Participation rates in the underground sector were 8.5% overall and 23.3% for men aged 18-24 (the group most similar to the Fragile Families sample at baseline). Lemieux, Fortin and Frechette (1994) estimate the responsiveness of underground earnings and hours to marginal tax rates. They adjust for selection by including the inverse Mills ratio from probit participation estimation and instrument for the regular-sector wage using union status, predicted tax rates, and occupation dummies, and they also jointly estimate earning and hours equations. They find a positive and

significant relationship between marginal tax rates and participation in the informal economy before instrumenting for tax rates and wages and a positive relationship after instrumenting, but generally conclude that the tax system does not significantly alter sectoral choice decisions. Lacroix and Fortin (1992) use a framework that allows labor supply in the taxed and untaxed sectors to be imperfect substitutes. They estimate a structural model of participation in the underground sector and find that an increase in the likelihood of audit and the penalty rate on detected evasion are both associated with a small increase in hours in the regular sector and larger decrease in hours in the untaxed sector. The elasticity of hours with respect to the tax rate suggests that a higher tax rate increases hours worked in the untaxed sector. Their estimates strongly imply that regular and informal work are not perfect substitutes, which is not surprising since the two types of work not only have different tax risks but also have different legal protections, wages, and levels of security and individuals may prefer regular (or informal) work for social reasons.

The majority of empirical papers on tax evasion use administrative tax data and look at reported income rather than labor supply in off-the-books work. Clotfelter (1983) uses data from the 1969 IRS Taxpayer Compliance Measurement Program and finds that tax evasion increases as marginal tax rates increase. He finds elasticities of underreported income with respect to the marginal tax rate between 0.5 and 3.0 depending on the type of return, the measure of income, and the income level. Farm and business returns had higher elasticities than non-business returns, probably because business income is easier to hide from authorities than wage income. Slemrod (1985) uses 1977 IRS microdata and looks at the distribution of taxpayers within \$50 brackets of the tax code. He finds that evasion is associated with higher marginal rates, but when adjusted gross income is included as a regressor, tax rates now have a negative effect and income has positive effect. LaLumia (2006) examines the effects of EITC phase-in and phase-out rates on whether individuals report self-employment income. Using pooled cross sections of IRS data, she finds that increasing the EITC phase-in rate increases the share of returns with Schedule C self-employment income.

Rich and Kim (2001) use the first wave of the Fragile Families data to look at participation rates and hours worked in the underground economy and find negative but insignificant effects of federal, state, and local tax rates on participation in the underground. They use variation in state and local tax rates to identify the effect of taxes on labor supply but do not instrument for tax rates, and their estimates are likely to suffer from endogeneity problems. Rich and Kim (2001) do find that alcohol and drug use are strongly associated with informal economy participation rates, sug-

gesting that barriers to regular-sector employment may be important determinants in informal-sector participation.

Some recent studies have used the Fragile Families data to examine informal work in response to non-tax considerations. Rich, Garfinkel and Gao (2007) examine whether strict child support regimes cause fathers to shift to underground employment. Since regular-sector employers are required to report new employees to state new hire directories and then to withhold child support from paychecks, strict child support enforcement might dissuade fathers from regular work. However, Rich et al. (2007) find evidence that in cities with stronger enforcement, fathers work fewer hours in the underground sector and there is no significant difference in regular-sector employment.⁴

3.3.2 Model

Popular wisdom and some past empirical research suggest that off-the-books work increases when tax rates rise, but economic theory offers no such clean-cut prediction. Allingham and Sandmo's (1972) seminal model of tax evasion demonstrated that when the penalty associated with evading taxes is a fraction of the evaded income, an increase in the tax rate generates substitution and income effects that work in opposite directions, making it impossible to sign the effect of an increase in marginal rates on tax evasion. Yitzhaki (1974) noted that if the penalty is a fraction of the evaded tax rather than the evaded income (as it is in the US), there is no substitution effect and optimal tax evasion decreases as the tax rate rises.⁵

Sandmo (1981) and Cowell (1985) demonstrate that when the Allingham-Sandmo-Yitzhaki model is extended to include endogenous labor supply, the problem becomes increasingly difficult because individuals can substitute either on the labor/leisure margin or the sectoral margin. Without restricting the utility function by specifying the nature of risk aversion or the separability of the utility or labor supply functions, it is impossible to sign the effects. Cowell (1985) notes that if income is endogenous in a two-sector model, an increase in the penalty rate or probability of getting caught reduces returns to labor and increases in the penalty rate increase the variance in

⁴Rich et al. (2007) use the first two waves of the Fragile Families data: relatively few men have legal child support orders established so soon after the birth unless they have an order for another child. In addition, avoiding child support enforcement requires fully exiting the regular sector. The majority of men in the sample who do informal work also work in the regular sector.

⁵In the US, penalties for tax evasion are 20% of the evaded tax except in cases of fraud, which have higher penalty rates. For the EITC, additional penalties exist: taxpayers may not claim the credit for two years and must be recertified before claiming the credit after that time.

return to work.

Because tax credits' phase-in and phase-out rates are simply adjustments to the marginal tax rate in a particular income region, it is impossible to sign the effects of the phase-in and phase-out rates in any model where it is impossible to sign the effects of marginal tax rates.

To illustrate these issues, I first present a model that illustrates the effects of marginal tax rates and tax credit phase-in and phase-out rates on regular-labor supply assuming that is the only sector. Then I extend the model to include a second, untaxed sector. Individuals maximize utility, which depends on leisure and income (or consumption of goods), subject to a budget constraint. Utility is increasing in both leisure and income and is strictly concave.

$$\max_{H_0} U(L, Y_0) \quad (3.1)$$

subject to

$$Y_0 = M + (1 - \tau)w_0H_0 + \begin{cases} t_1w_0H_0 & \text{if } w_0H_0 \leq C/t_1 \\ C & \text{if } C/t_1 < w_0H_0 \leq p \\ C - t_2(w_0H_0 - p) & \text{if } p < w_0H_0 \leq p + C/t_2 \end{cases} \quad (3.2)$$

$$H_0 + L = 1 \quad (3.3)$$

$$H_0, L \geq 0 \quad (3.4)$$

where L is hours of leisure, w_0 is the hourly wage rate in the regular sector, H_0 represents hours worked in the regular sector, Y_0 is income in the regular sector, M is non-labor income, τ is the marginal tax rate, t_1 and t_2 are the tax credit phase-in and phase-out rates, C is the maximum credit amount, and p is the income level at the end of the plateau region. If we assume an interior solution, then when the first-order maximization condition

$$\frac{\partial U}{\partial H_0} = 0 = -U_1 + U_2 \left[(1 - \tau)w_0 + \begin{cases} t_1w_0 & \text{if } w_0H_0 \leq C/t_1 \\ 0 & \text{if } C/t_1 < w_0H_0 \leq p \\ -t_2w_0 & \text{if } p < w_0H_0 \leq p + C/t_2 \end{cases} \right] \quad (3.5)$$

is totally differentiated, the comparative static result for the marginal tax rate is

$$\frac{\partial H_0^*}{\partial \tau} = \frac{U_2 w_0}{S_{Region_i}} - w_0 H_0 \frac{\partial L}{\partial M} \Big|_{Region_i} \quad (3.6)$$

This is a standard comparative static result: the first term is the substitution effect, and is negative.⁶ If leisure is a normal good, $\frac{\partial H_0}{\partial M} < 0$ and the income and substitution effects for tax changes act in opposite directions and the net effect of a change in the marginal tax rate on hours worked is ambiguous. The comparative statics for the wage w_0 and the marginal tax rate τ are isomorphic within each region of the credit, and the effects for the phase-in and phase-out rates t_1 and t_2 are identical to those for τ except for the sign in the case of t_1 .

Although the theoretical effect of a change in the credit phase-in rate on hours worked cannot be signed due to the offsetting income and substitution effects, the effect of the credit on the decision whether or not to work is unambiguously positive. Because the credit phase-in increases the marginal return to work by $w_0 t_1$ relative to the no-credit case, any individual who worked when the phase-in rate is low should also choose to work when the phase-in rate increases.⁷ In addition, the credit will increase the value of work above the reservation level for additional workers, so credit introduction or phase-in rate increases will increase regular-sector employment.⁸

Now suppose there is a second, informal, sector and by working in that sector agents can shield income from taxation. Earnings in that sector are $w_1 H_1$. There is a risk that the tax agency will detect informal work and penalize the taxpayer for hiding income: let $V(w_0 H_0, w_1 H_1)$ be the certainty-equivalent utility cost associated with those risks and penalties.⁹ Assume $V_2 > 0$ and $V_{22} > 0$, so that penalties increase with the amount of tax not reported. The individual's problem is now

$$\max_{H_0, H_1} U(L, Y) \quad (3.7)$$

⁶The expression for S_{Region_i} depends on the region of the credit at which the comparative static is evaluated. The concavity assumptions ensures that $S_{Region_i} < 0 \forall i$. $\frac{\partial L}{\partial M}$ also depends on S_{Region_i} .

⁷This need not hold for secondary earners in a married couple; see Ellwood (2000) and Eissa and Hoynes (2004) who find negative effects of the EITC on labor market participation of secondary earners.

⁸However, see Rothstein (2008) for analysis of general equilibrium effects of the increase in labor supply.

⁹Cowell (1990) shows that the certainty-equivalent model is consistent with the Allingham-Sandmo expected utility framework. Because the discontinuities in the credit make the problem complex, the certainty-equivalent utility cost is a useful simplification of the expected utility framework.

subject to:

$$Y = M + w_0 H_0 (1 - \tau) + w_1 H_1 - V(w_0 H_0, w_1 H_1) + \begin{cases} t_1 w_0 H_0 & \text{if } w_0 H_0 \leq C/t_1 \\ C & \text{if } C/t_1 < w_0 H_0 \leq p \\ C - t_2(w_0 H_0 - p) & \text{if } p < w_0 H_0 \leq p + C/t_2 \end{cases} \quad (3.8)$$

$$H_0 + H_1 + L = 1 \quad (3.9)$$

$$H_0, H_1, L \geq 0. \quad (3.10)$$

The corresponding first-order maximization conditions are:

$$\frac{\partial U}{\partial H_0} = 0 = -U_1 + U_2 \left[w_0(1 - \tau) - V_1 w_0 + \begin{cases} t_1 w_0 & \text{if } w_0 H_0 \leq C/t_1 \\ 0 & \text{if } C/t_1 < w_0 H_0 \leq p \\ -t_2 w_0 & \text{if } p < w_0 H_0 \leq p + C/t_2 \end{cases} \right] \quad (3.11)$$

$$\frac{\partial U}{\partial H_1} = 0 = -U_1 + U_2 [w_1 - V_2 w_1] \quad (3.12)$$

The meaning of the first-order conditions is standard: at the optimum, the marginal rate of substitution between leisure and consumption (U_1/U_2) should be equal to the after-tax wage in each sector. Combining the two first-order conditions also implies that the marginal value of an additional hour of work should be equal across the two sectors at the optimum.

The comparative static result for the effect of the marginal tax rate on regular-sector hours worked, shown in Equation 3.13, appears very similar to the effects in the one-sector model.

$$\frac{\partial H_0^*}{\partial \tau} = \frac{U_2 w_0 \left(\frac{\partial^2 U}{\partial H_1^2} \right)}{D_{Region_i}} - w_0 H_0 \frac{\partial H_0}{\partial M} \quad (3.13)$$

Once again, the substitution effect is negative, illustrating that higher marginal tax rates lower the wage in the regular sector relative to the informal sector.¹⁰ Now, however, the sign of the income effect is ambiguous: while before $-\frac{dL}{dM} = \frac{dH_0}{dM}$, now $-\frac{dL}{dM} = \frac{dH_0}{dM} + \frac{dH_1}{dM}$. This means that the assumption that leisure is normal no longer

¹⁰Second-order conditions ensure that $\frac{\partial^2 U}{\partial H_1^2} / D_{Region_i} < 0$.

requires $\frac{dH_0}{dM} < 0$. As in the one-sector model, the effect of an increase in the marginal tax rate on regular-sector labor supply is ambiguous. For similar reasons, the comparative static for informal-sector hours is ambiguous as well:

$$\frac{\partial H_1^*}{\partial \tau} = -\frac{U_2 w_0 \left(\frac{\partial^2 U}{\partial H_0 \partial H_1} \right)}{D_{Region_i}} - w_0 H_0 \frac{\partial H_1}{\partial M}. \quad (3.14)$$

The income effects in the two sectors ($\frac{\partial H_0}{\partial M}$ and $\frac{\partial H_1}{\partial M}$) differ and each depends on the first and second derivatives of the tax evasion cost function $V(w_0 H_0, w_1 H_1)$. Neither can be signed without imposing restrictions on the form of the utility function or the labor supply functions: agents can now substitute either between sectors or between labor and leisure.

The effects for the credit phase-in and phase-out rates t_1 and t_2 are identical (in their respective regions) to the effects for the marginal tax rate except that the effect of t_1 takes the opposite sign because the credit phase-in rate reduces the effective marginal tax rate. As in Slemrod's (2001) model of tax avoidance, the effects of wages and tax rates on labor supply are no longer isomorphic.

As the model illustrates, sectoral labor supply responses to tax rates depend not only on relative wages but on the risks and penalties associated with tax evasion and individuals' attitudes toward risk. Because theoretical predictions of the effect of tax parameters on participation are ambiguous, and because tax evasion constrains the effects of tax policy, empirical estimates of how taxpayers substitute between regular and informal work in response to tax changes are very useful. Not surprisingly, though, data constraints have limited the empirical literature. Until recently, no large-scale survey data included information on off-the-books or illegal work.

3.4 Data and Methods

I take advantage of the relatively new Fragile Families survey data which includes information about types and amounts of underground work. This longitudinal survey followed fathers of babies born in large cities for five years after the child's birth and includes information on regular work, work off the books, in respondents' own businesses, and illegal activities at four time periods. No other large-scale survey includes such detailed questions about informal work; the information on informal economy participation in the Fragile Families data allows me to conduct new evaluations of the effects of taxes on the choice of work type. The high concentration of low-income

individuals in the Fragile Families data also makes it a useful study for examining the effects of the earned income tax credit.

Due to the nature of the Fragile Families sample, fathers' data is not nationally representative, so although the results provide useful information about responses to the EITC among a relevant population they cannot be interpreted as indicative of the average response to state EITCs. The sampling scheme, which was designed to be representative of non-marital births in cities with populations over 200,000, yields a sample of men with at least one child. Since the men interviewed are parents, their connections to the underground economy may be different than among men who are not fathers: they may have child support responsibilities (formal or informal) that result in penalties or costs if income is low, their sectoral choice might be affected by child support enforcement, or they may want to set an example for their child. The men are relatively young and are disproportionately minority and low-income. Another determinant of sample selection is that although births were sampled randomly within hospitals, whether a father interview was obtained for the birth depended in part on the strength of his relationship with the mother: fathers who had stronger relationships with the birth mother were more likely to visit the hospital during or after the birth and to participate in an interview. Consequently, the men in the sample have stronger family attachments than men who did not participate.

The Fragile Families data is constructed as follows: a total of 4898 hospital births occurring between 1998-2000 in twenty US cities with populations of over 200,000 were sampled and separate mother and father interviews were obtained at birth and at the child's first, third, and fifth birthdays.¹¹ Interviews took place in different years in different cities, as shown in Table 3.2, and the resulting data spans 1998-2006. Non-marital births were oversampled relative to marital births: approximately 75% of births in the sample were non-marital.

I restrict my sample to men who were unmarried at baseline and remain unmarried for all subsequent waves. This allows me to examine a consistent population: if men were included in the sample in any year in which they were single, then changes in marriage rates (especially state-specific changes influenced by tax policy) would change the sample composition across years and observed changes could be due either to changes in tax policy or changes in sample composition.¹² If these men file taxes, they should file as single or head-of-household, and (legally) may claim qualifying

¹¹The sampling scheme involved sampling cities, hospitals within cities, and births at hospitals. The Data Appendix describes sample construction and response rates in detail.

¹²That said, the results are very similar if I extend the sample to include men in each wave in which they are unmarried instead of requiring them to be unmarried in all waves.

Table 3.2: Fragile Families Interviews by Wave and Year

Mothers	1998	1999	2000	2001	2002	2003	2004	2005	2006	Not in wave
Baseline	656	1671	2571							0
1-Year Followup		577	639	3149						533
3-Year Followup				557	1552	2122				667
5-Year Followup						486	1550	2063	40	759

Fathers	1998	1999	2000	2001	2002	2003	2004	2005	2006	Not in wave
Baseline	524	1233	2073							1068
1-Year Followup		427	330	2589	21					1531
3-Year Followup				385	1160	1754				1599
5-Year Followup						324	1118	1655	57	1739

Rows sum to 4898, the number of births sampled in the baseline wave. In the fathers' five-year followup, interview dates are missing for 5 cases.

Baseline Interviews 1998	Austin, TX; Oakland, CA
Baseline Interviews 1999	Baltimore, MD; Detroit, MI; Newark, NJ; Philadelphia, PA; Richmond, VA
Baseline Interviews 2000	Boston, MA; Chicago, IL; Corpus Christi, TX; Indianapolis, IN; Jacksonville, FL; Milwaukee, WI; Nashville, TN; New York, NY; Pittsburgh, PA; San Antonio, TX; San Jose, CA; Toledo, OH; Virginia Beach/Norfolk, VA

children for the EITC only if they lived with the children at least half the year and the children are not claimed as EITC-qualifying children by another taxpayer. Another advantage of restricting the sample is that single men are not eligible for most other welfare programs so that changes due to welfare reform should have little effect on the results.¹³

3.4.1 Defining Informal Work

In each of the four waves of the survey, fathers report whether they worked in a job for which they received a regular paycheck and report their earnings, weeks worked per year, and average hours worked per week for regular work. Then fathers are asked whether they participated in off-the-books work; worked in their own business; engaged in drug sales, prostitution, or other “hustles”; or did other work for

¹³Female labor supply does increase dramatically during the late 1990s; see, for example, Ellwood (2000) and Grogger (2003). If low-income women work in the same jobs as low-income men, employment opportunities for men might vary by state depending on welfare and EITC policy. Changes in women's labor supply and earnings might also change expectations for men to help support their partners and children. I ignore these possibilities.

cash.¹⁴ Table 3.3 shows men’s participation rates in the different types of underground work. In the last wave, men were not asked about off-the-books work: of men who reported off-the-books work in wave 3, 19 percent report work in own business, 2 percent hustled, and 16 percent did other informal work in the fourth wave. Fewer people overall report informal work in this wave, most likely because the survey did not ask about off-the-books work. However, as long as how respondents interpreted the survey question did not vary systematically across states with different EITCs, this should not bias the results. I include survey wave indicators in all estimates to account for changes in survey methodology across waves.¹⁵

I treat an individual as working in the informal sector if they report working in any of the four categories of informal work: off-the-books work, work in their own business for which they do not receive a regular paycheck, hustles and other illegal activities, and other activities. Off-the-books work is closest to the conceptual measure in which I am interested because it explicitly implies untaxed status but is likely to be similar to regular work in other ways. Work in the respondent’s own business may be problematic because the survey requires that respondents separately report work in their own business for which they receive a regular paycheck and other work in their own business. Inclusion of hustles, drug sales, and prostitution in informal work captures the idea that taxes alter the relative costs of work but since legal and illegal work are less similar than reported and unreported legal activity we should expect less substitution between them. A chief advantage of the combined measure of informal work is that it is less sensitive to whether individuals change how they define their work from one wave to another. For example, if a respondent paints houses in the summer for a fee, he may report this as off-the-books work in one wave but as work in his own business in the next wave. Using only the off-the books measure would record this as exiting the informal sector. Over 40% of respondents who report informal work in two adjacent waves report different types of underground work in different waves.¹⁶ As illustrated in Table 3.4, many men also transition in and out of informal work between waves. Less than half of men who report working in the

¹⁴Survey professionals instructed respondents to include regular income from self-employment or their own business as regular-sector work. The questions about the four types of informal work were prefaced with this statement: “We are interested in finding out about some ways, other than regular work, in which people make money. This kind of activity may be paid for in cash, or done in exchange for meals, or clothing, a place to live, or something else.”

¹⁵Year indicators do not correct for this potential bias because some years include data for multiple waves. The timing of interview waves varies by city.

¹⁶In spite of the changes in how informal-sector work is reported, the results shown below are very similar to the results when only the off-the-books measures is used.

Table 3.3: Single Men’s Participation in the Underground Economy, Fragile Families Data

	Wave 1 (n=2019)	Wave 2 (n=1680)	Wave 3 (n=1617)	Wave 4 (n=1543)
Participation Rates in Informal Sector				
Any informal work	34.6	36.8	31.0	20.2
Off-the-books	25.6	24.9	22.0	
Own business	7.8	10.2	6.1	9.6
Hustles	4.4	4.4	3.4	2.9
Other informal work	4.3	9.0	6.6	9.2
Conditional on Participation				
Usual hours/week in informal sector	24.9	33.2	26.4	24.4
Usual hours/week off the books	19.8	26.0	22.2	
Usual hours/week in own business	24.5	30.9	26.1	24.0
Usual hours/week hustling	25.7	30.4	30.0	37.0
Usual hours/week in other informal	15.0	18.4	13.0	17.4
Weeks/year off the books	13.2	16.0	14.4	
Weeks/year in own business	25.1	29.1	25.6	25.9
Weeks/year hustling	19.0	22.3	22.8	21.3
Weeks/year in other informal	13.7	15.5	13.7	15.9
Annual earnings in informal sector (\$)		8,121	5,929	6,070
Annual earnings off the books (\$)		4,280	3,595	
Annual earnings in own business (\$)		11,626	13,082	7,764
Annual earnings hustling (\$)		18,274	12,384	13,024
Annual earnings in other informal (\$)		2,406	1,922	1,898

Sample restricted to men unmarried at baseline and in all subsequent waves. In wave 4, the survey asked about only three types of informal work. In wave 1, the survey only collected categorical information about annual earnings in informal work.

informal sector in a given wave also report working informally in the next wave.

A difficulty with asking about participation in illegal or informal work is that respondents may not be truthful. They may fear that their illegal activities will be reported or that the interviewer will disapprove. There is no way to know the extent of underreporting, but a large percentage of respondents do report work in the underground sector, as shown in Table 3.3. Since the primary focus of the study is family dynamics the majority of questions in the survey focus on relationship histories and beliefs about child-rearing. By de-emphasizing the importance of income and employment, the survey may encourage truthful responses.

Table 3.4: Transitions In and Out of the Informal Sector

Of current informal-... sector participants...	% who participate in future waves:		
	Wave 2	Wave 3	Wave 4
Wave 1	49.4	40.8	28.2
Wave 2		47.6	31.3
Wave 3			32.1

Level of Participation in the Regular and Informal Sectors: Hours and Weeks Worked

I use reports of usual hours worked per week in each sector as the measure of hours worked in each sector. Men report this directly for both sectors in the survey. An alternative would be to use annual hours worked calculated from usual hours worked each week and weeks worked per year. This increases measurement error because both hours worked and weeks work will likely be measured with error. Bound, Brown, Duncan and Rodgers (1994) show that survey respondents tend to underreport transitory fluctuations in employment and earnings, generating mean-reverting measurement error. When using fixed effects methods for panel data, which identify changes in hours, nonclassical measurement error biases the results. In fact, using annual hours as the outcome variable resulted in similar signs but unreasonably large magnitudes for the effects of the EITC on hours in either sector.

In addition to usual hours worked per week in each sector, I examine weeks worked per year in the regular sector. Low-income men may take short-term or seasonal jobs and may have more flexibility to adjust the weeks worked per year than hours per week, which may be determined by the employer. I use this measure only for the regular sector. Since many informal-sector participants report working in multiple categories, summing the weeks worked in different categories may not indicate total weeks with informal work.

3.4.2 Additional Variables

Factors other than taxes affect participation in the informal sector; I control for demographic characteristics including age and age squared, education, cohabitation status, and number of children in the household. To control for regular-sector job charac-

teristics that affect access to and attractiveness of the regular economy, I include occupation codes for the most recent regular-sector job and regular-sector hourly wages.¹⁷ I also include two measures of barriers to participation in the regular labor market: an indicator for whether the respondent has served jail time and an indicator for whether the father is subject to a legal child support order. Since child support enforcement agencies withhold child support from fathers' paychecks, having a legal support order makes participation in the regular sector costly. To account for time-variant state characteristics, I include the state unemployment rate and the state marginal tax bracket rate for a single tax filer whose income lies just outside the EITC credit region as a proxy for non-EITC features of the state income tax. Table 3.5 presents summary statistics. The sample is disproportionately minority and over one third of the sample does not have a high-school education. Over half the men are cohabiting with a partner. Over one third have spent time in jail. Only 71% of single men in the last wave worked in the regular sector in the past 12 months. Conditional on working, they worked an average of 45.9 hours a week and 47.7 weeks per year. Production/repair jobs and "Handler/equipment cleaner/laborer" jobs are most common, accounting for 38 percent of workers. Machine operators account for 7 percent and transportation industries for 11 percent. Sample respondents are heavily concentrated in industries in which we might expect high rates of off-the-books work and tax noncompliance.

3.4.3 Identification Strategy

A well-known difficulty in estimating labor supply responses to tax changes is the endogeneity of the tax rate. Tax rates depend on income and income depends in turn on labor force participation. One possible solution is to calculate tax rates based on earnings and then instrument for the tax rate. Success with this method requires both a good instrument and a good measure of taxable income, and good income measures are not easily available in the Fragile Families data. The survey asks about earnings in the respondent's regular job, but respondents can choose the time units for which to report their earnings. Calculating earnings also depends on the number of jobs respondents had, the number of weeks worked per year, and the hours worked

¹⁷Respondents reported how much they earned in their most recent regular job, but could report their earnings by hour, day, week, month, year, or other measure. I calculate hourly wages for all respondents and trim the top and bottom 2% of the wage distribution to eliminate obvious coding errors. Respondents who had not held a regular job were asked what the hourly wage would have to be for them to take a job; I use this as their regular-sector wage.

Table 3.5: Summary Statistics at Five-Year Followup

Variable	Mean	Std. Dev.	Min	Max
State credit percent of federal credit	7.03	14.53	0	50
Has filled out/will file tax form for previous year	0.61	0.49	0	1
Claimed EIC on previous year's taxes	0.22	0.41	0	1
Does not know if claimed EIC on last year's taxes	0.04	0.20	0	1
Black, non-Hispanic	0.64	0.48	0	1
Hispanic	0.23	0.42	0	1
Less than high-school education	0.36	0.48	0	1
High-school degree, GED, or ABE	0.32	0.49	0	1
Some post-high-school education	0.28	0.45	0	1
4-year college degree	0.04	0.19	0	1
Age	29.1	5.8	20	53
Father currently cohabiting with partner	0.53	0.50	0	1
Number of children living in household	0.78	1.16	0	7
Ever spent time in jail	0.37	0.48	0	1
Has legal child support order	0.45	0.50	0	1
Worked in regular job in past 12 months	0.71	0.46	0	1
Hourly wage in current/most recent regular job	12.1	6.9	3.33	53.85
Weeks worked in regular job, past 12 months	31.9	23.5	0	52
Conditional on working in regular sector	45.9	12.5	1	52
Usual hours/week worked in regular sector	33.6	25.6	0	120
Conditional on working in regular sector	47.7	16.1	1	120
Annual hours worked in regular job	1545	1297	0	6240
Conditional on working in regular sector	2223	954	8	6240
Household income	34646	36458	0	600000
Professional/technical	0.04	0.19	0	1
Executive/admin/managerial	0.07	0.25	0	1
Sales	0.06	0.24	0	1
Administrative support	0.05	0.21	0	1
Precision production/craft/repair	0.20	0.40	0	1
Machine operator/assembly/inspection	0.07	0.26	0	1
Transportation/material moving	0.11	0.31	0	1
Handler/equip cleaner/laborer	0.18	0.3	0	1
Service	0.22	0.41	0	1
Other/Unspecified	0.01	0.09	0	1

Sample restricted to men unmarried at baseline and in all subsequent waves.

per week. The survey asks about household income but since nearly half the men in my sample cohabit with a partner, household income is a poor measure of taxable earnings for unmarried men. Taxable earnings calculations based on this data are highly suspect.

Rather than instrumenting for the tax rate, I follow the approach used by several recent papers and use EITC schedule parameters in a differences-and-differences approach. I use the percent of the federal credit the state allows an individual filer to claim as a state EITC as my measure of the state policy.¹⁸ This measure accurately reflects the actual policy changes taking place during tax years 1997-2005. It is important to understand the impact of this policy change. A taxpayer who claims the EITC and files as a single taxpayer with no qualifying children would see his maximum combined state and federal EITC increase by about \$86 if his state introduced an EITC that was 20% of the federal credit. The value of that same policy change is higher for taxpayers who can claim qualifying children: introducing a 20% credit would result in a state EITC of \$571 for filers with one qualifying child and \$944 for filers with 2 qualifying children. Since the value of the state policy for a filer with one child is 6.67 times larger than the value of the state policy for a filer with no child, and the value for a filer with two qualifying children is 11 times larger than the value for a filer with one child, the state EITC policies increase the incentives for fathers to claim children when filing for the EITC. The policies will affect fathers differently depending on their ability to claim qualifying children, and the state EITC will affect the relative attractiveness of the regular and informal sectors both by directly changing the tax rate associated with the regular sector and by further changing the associated tax rate if the men also change the number of children they claim as EITC qualifying children. This could result in large effects of the state EITC, because if fathers change the number of children they claim, they “choose” a new marginal tax rate because the federal credit phase-in rates differ by the number of children claimed. Measuring the state EITC as the percentage of the federal credit captures the effect of both these changes and consequently measures the full impact of the state EITC change on behavior.

The differences-in-differences approach compares average changes in underground economy participation in states with varying levels of earned income tax credits. Con-

¹⁸State codes for the fourth wave of data (the five-year followup) have not yet been released; I assume fathers in the fourth wave have not moved across state lines since wave 3. Although approximately 45 percent of men move between waves, only 3.25 percent of men moved to a different state between waves 1 and 2 and only 3.75 percent moved to a new state between waves 2 and 3. Low rates of inter-state moves also suggest men are not moving to states with more lenient tax policy.

sider a differences-in-differences regression on individual panel data with state, year, and individual fixed effects S_{is} , T_{it} , and D_i :

$$L_{ist} = \beta(EITCrate_{st}) + X_{ist}\gamma + Z_{st}\delta + \sum_{j=1}^s S_{is}\mu_s + \sum_{j=1}^t T_{it}\eta_t + \sum_{j=1}^i D_i + \epsilon_{ist},$$

where L_{ist} is the labor supply variable of interest, X_{ist} is a vector of time-variant individual characteristics and Z_{st} is a vector of time-variant state characteristics.¹⁹ Demeaning the data eliminates the individual fixed effect and yields

$$\begin{aligned} (L_{it} - \bar{L}_i) &= \beta(EITCrate_{st} - \overline{EITCrate_{st}}) + (X_{ist} - \bar{X}_{ist})\gamma + (Z_{st} - \bar{Z}_{st})\delta \\ &+ \sum_{j=1}^s (S_{is} - \bar{S}_{is})\mu_s + \sum_{j=1}^t (T_{it} - \bar{T}_{it})\eta_t + (\epsilon_{ist} - \bar{\epsilon}_{ist}), \end{aligned}$$

This removes the effect of unobserved time-invariant factors such as aspects of taste for risk or access to the regular sector or informal sector from the model.

Although estimating fixed-effects regressions allows me to control for individual characteristics, a disadvantage is they do not accommodate limited dependent variables well. I also estimate pooled probit regressions for the participation outcomes and Tobit regressions for the hours and weeks worked outcomes. Since nearly one-third of the sample does not work in the regular sector and two-thirds do not work in the informal sector, corner solutions are a serious matter in both sectors. The Tobit model accounts for both the probability of choosing positive work hours in a sector and for the number of hours chosen.

The earned income credit has different incentives depending on whether an individual's regular-sector taxable income places them in the phase-in, plateau, or phase-out portion of the credit. My estimation strategy does not account for these different incentives: I ignore them for three reasons. First, examining different effects in different regions of the credit requires that I know which region of the credit is relevant. As I discuss above, I am not confident that calculations of taxable income in Fragile Families are accurate. Furthermore, I do not know how many qualifying children they claim. The endogeneity of earnings further complicates separately estimating effects in different regions of the credit. Second, it is unlikely that most taxpayers fully understand the structure of the credit. Ethnographic research by Romich and Weisner (2000) suggests that low-income taxpayers are aware of the credit but do not fully understand the phaseout features. They interpret the EITC as linearly related

¹⁹State identifiers are available only in the restricted-access version of the Fragile Families file.

to income, which is accurate for workers with regular income in the phase-in region. Work by Feldman and Katuščák (2006) and Liebman and Zeckhauser (2004) suggests taxpayers respond to average rather than marginal tax rates. For similar reasons, I ignore incentives to clump at kink points.

3.5 Results

3.5.1 Participation in the Informal and Regular Sectors: Extensive Margin Results

Table 3.6 presents results from fixed effects regressions of whether an individual participates in the informal sector on the percent of the federal EITC available as an additional state tax credit. After controlling for state taxes and economic conditions, demographics, and jail history and child support, an increase in the state credit of 1 percent of the federal credit reduces the likelihood of participating in the informal sector by half a percentage point. To translate these to dollar terms, if the state credit increased from 0 to 10 percent of the federal credit in 2003, the maximum credit for filers with no children would increase by \$38, for one qualifying child by \$255, and for two qualifying children by \$420, and this policy would decrease participation in the underground sector by 5 percentage points. As we might expect, individuals are less likely to work in the informal sector when its returns are lower relative to the regular sector. These effects are large and the magnitudes are even larger if we restrict the sample to individuals currently working in the regular sector (not shown). Individuals who only work in the informal sector may be more likely to engage in illegal rather than just informal work or they may prefer the informal sector for non-tax reasons, so it is unsurprising to see larger responses in the restricted sample.

Not surprisingly, demographic characteristics play little role in determining underground economy participation in the fixed-effects model; most of the demographic measures change little over time. Cohabiting with a partner increases the likelihood of informal-sector participation by about 2 percent; cohabitation may encourage underground work because complicated rules for program eligibility restrict household income or cohabiting men may have more responsibilities and need more extra income. Alternately, men with connections to the informal economy may need help to support themselves and cohabit for lack of an alternative. As expected, regular-sector activity affects work in the informal sector. Higher wages in the regular sector have

Table 3.6: Fixed Effects Regressions: Participation in the Informal Economy

State percent of federal credit	-0.003 (0.002)	-0.004 (0.003)	-0.004* (0.003)	-0.005** (0.002)	-0.005** (0.002)
State tax bracket rate		0.022 (0.038)	0.021 (0.039)	0.026 (0.039)	0.029 (0.037)
Tax year after 2001 tax cut		0.078* (0.041)	0.084** (0.042)	0.070 (0.046)	0.064 (0.047)
State annual unemployment rate		-0.035** (0.014)	-0.034** (0.014)	-0.037*** (0.014)	-0.032** (0.015)
Age			-0.008 (0.014)	0.001 (0.014)	0.001 (0.014)
Age squared			0.008 (0.019)	-0.004 (0.018)	-0.005 (0.018)
Less than high-school education			-0.009 (0.041)	0.021 (0.043)	0.028 (0.043)
Some post-high-school education			0.035 (0.039)	0.068 (0.042)	0.087** (0.040)
4-year college degree			0.015 (0.100)	-0.049 (0.128)	-0.025 (0.128)
Father currently cohabiting with partner			0.029** (0.013)	0.014 (0.014)	0.018 (0.013)
Number of children living in household			0.004 (0.005)	0.002 (0.006)	0.004 (0.006)
Hourly wage in most recent regular job				-0.003*** (0.001)	-0.003** (0.001)
Ever spent time in jail					0.078* (0.040)
Has legal child support order					0.039** (0.017)
Constant	-0.402 (0.464)	0.712** (0.328)	0.819** (0.405)	0.255 (0.276)	0.674* (0.405)
R-squared	0.047	0.048	0.050	0.053	0.055
N	6301	6301	6233	5546	5420

All regressions include state and year indicators. Standard errors clustered at the state-year level are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

a small but significant negative effect on participation in the informal sector: as the returns to regular sector work increase, the informal sector is less attractive.

The large positive effect of incarceration history on participation in the underground could be due to barriers men with criminal records face when reentering the workforce, or possibly due to new access to the underground economy formed during prison connections. Since the fixed effects regressions examine changes in incarceration status, the coefficient indicates the effect on underground work is not because men who become incarcerated are simply more likely to have connections to the underground, but that becoming incarcerated itself changes their work. As expected, having a legal child support order increases underground economy participation and reduces the likelihood of working in the regular sector.

Pooled probit and fixed-effects versions of my preferred specification in Table 3.6 column (5) for participation in the informal sector and regular sector are shown in Table 3.7. Mean marginal effects from the pooled probit regressions are very similar to coefficients in pooled OLS regressions (not shown). There is no significant effect of an increase in the state EITC on whether or not men are employed in the regular sector. The magnitude and significance of the effect of the earned income credit is generally consistent across specifications for participation in the underground, although effects from the pooled regression appear slightly larger. Spending time in jail decreases the likelihood of subsequent regular-sector employment by 13.8 to 17.3 percentage points; much of this may be because jail time cuts into regular-sector work. Restricting the sample to men who were not in jail at the time of the interview reduces the effect of having spent time in jail on regular participation to -2.8 percentage points (statistically significant). The pooled regressions reveal demographic differences: black men are 10 percent less likely to work in the regular sector than white men, while Hispanics are 6.5 percentage points less likely to report working in the informal sector. Undocumented immigrants may be more reluctant than other respondents to report underground work.

Reasons for Large Informal-Sector Participation Responses

The effects of the state earned income credits on whether men work in the informal sector are large in magnitude, yet there are several reasons why we might expect large effects of the EITC. First, previous research has found large effects on female labor supply. In particular, Hotz et al. (2006) found that a \$439 increase in the EITC for female-headed families with two or more children corresponded to a 3.2 percentage

Table 3.7: Participation in Informal and Regular Sectors, Pooled Probits and Fixed Effects OLS

	Regular Sector		Informal Sector, if Regular Sector > 0	
	Pooled Probit	Fixed Effects	Pooled Probit	Fixed Effects
State percent of federal credit	0.003 (0.002)	0.002 (0.002)	-0.005* (0.003)	-0.005** (0.002)
State tax bracket rate	-0.054** (0.022)	-0.014 (0.028)	0.008 (0.055)	0.029 (0.037)
State annual unemployment rate	0.003 (0.012)	0.009 (0.011)	-0.043*** (0.015)	-0.032** (0.015)
Black, non-Hispanic	-0.101*** (0.019)		-0.006 (0.024)	
Hispanic	0.05** (0.022)		-0.065*** (0.02)	
Age	0.012* (0.007)	0.018 (0.012)	-0.005 (0.008)	0.001 (0.014)
Age squared	0.000 (0.011)	-0.028* (0.016)	0.000 (0.013)	-0.005 (0.018)
Less than high-school education	-0.083*** (0.014)	0.018 (0.040)	0.025* (0.015)	0.028 (0.043)
Some post-high-school education	0.037** (0.015)	0.039 (0.034)	0.025 (0.017)	0.087** (0.040)
4-year college degree	0.044 (0.041)	0.038 (0.090)	-0.029 (0.038)	-0.025 (0.128)
Father currently cohabiting with partner	0.053*** (0.011)	0.019 (0.014)	0.021 (0.014)	0.018 (0.013)
Number of children living in household	-0.003 (0.006)	0.007 (0.006)	0.004 (0.005)	0.004 (0.006)
Hourly wage in most recent regular job	0.007*** (0.001)	0.004*** (0.001)	0.000 (0.001)	-0.003** (0.001)
Ever spent time in jail	-0.173*** (0.013)	-0.138*** (0.030)	0.119*** (0.017)	0.078* (0.040)
Has legal child support order	-0.04*** (0.014)	-0.028** (0.014)	0.024 (0.015)	0.039** (0.017)
Constant		-0.354 (0.458)		0.674* (0.405)
R-squared		0.022		0.055
Pseudo R-squared	0.111		0.053	
Log likelihood	-2844	-835	-3190	-1460
N	5458	5478	5396	5420

All regressions include state and year indicators and occupation codes. Standard errors clustered at the state-year level are in parentheses. Probit results are mean marginal effects for continuous variables and mean effects of discrete changes from 0 to 1 for binary variables. Standard errors for marginal effects were estimated using the delta method and account for state-year clusters. * significant at 10%; ** significant at 5%; *** significant at 1%

point increase in (regular-sector) employment. The magnitude of the effect for participation in the informal sector is in line with this estimate if we assume that men claim qualifying children when filing for the EITC. Between 63 and 70 percent of single men in the Fragile Families data report that the survey focal child lives with them at least half the time, making them eligible for the EITC if the mother (or another filer) does not claim the child for EITC purposes. Men may also claim other children living in their household for EITC purposes. After the baseline, between 40 and 57 percent of men report children in their household at the time of the interview, and by the last wave men report an average of 2.8 biological children. Consequently, it is likely that many of the men in the sample who file for the EITC claim qualifying children. While generally men's labor supply is considered to be highly inelastic, which would make large EITC effects surprising, this is probably less true in this sample: there is high unemployment, approximately 17 percent of men work multiple jobs, and men stop and start work throughout the year.

Second, for filers who do not claim qualifying children, state EITCs may offset payroll taxes enough to cause men to work on-the-books or to report more self-employment income at tax time. The federal EITC phase-in rate for childless filers is 7.65 percent, as is the payroll tax rate. As a result, combined state and federal EITCs will more than offset the employee's share of payroll tax contributions and the return to working on the books instead of under the table may be positive.

Third, whether work is off-the-books is partly a reporting decision, so some of the response will be a reporting response rather than "real" change in labor supply. Auerbach and Slemrod (1997) and Slemrod (1992) note that real responses are low on the hierarchy of behavioral responses to taxation relative to timing responses and accounting responses: "reporting responses", in which men simply choose whether or not to declare their income at tax time, could be even larger since they are almost costless.

Last, the large effects are likely due to the dual impact of the EITC: it directly increases the regular sector wage and it also should induce fathers to claim more qualifying children, resulting in a large change in the marginal tax rate in the regular sector and thus reinforcing the change in relative wages. As discussed above, single men appear more likely than other taxpayers to claim ineligible children for EITC purposes and claims of EITC children do appear to increase with the value of the credit. Consequently, the response to the state EITC has several components: (1) real labor supply changes due to the direct change in the credit phase-in rate and resulting marginal tax rate on regular-sector work introduced by the credit; (2)

reporting responses, in which men do not change the type or amount of labor they supply but work on-the-books instead of off-the-books; and (3) claiming responses in which men claim more children when state EITCs are a larger percent of the federal credit and the return to claiming more children is higher, and then adjust their labor supply to the new phase-in rates they have accessed through these fraudulent claims. By claiming one child instead of none, men receive a 34 percent federal credit phase-in rate instead of a 7.65 percent federal phase-in rate. McCubbin's (2000) estimate that a 10 percent increase in the value of the EITC would increase the likelihood of claiming a child by 4 percent provides a baseline estimate of the magnitude of this type of fraud. However, since evidence suggests that men are more likely to claim ineligible children and since McCubbin's estimates do not address the incentives to claim two children rather than one child, I expect that the effect of state EITCs on new fraudulent EITC-qualifying child claims may be higher for this sample.²⁰ Without knowing how many men claim the EITC with qualifying children, it is difficult to decompose the response to state credits into direct effects and changes related to increases in fraudulent claims, but it is likely that changes in claiming behavior constitute a meaningful part of the response.

3.5.2 Weeks and Hours Worked in the Informal and Regular Sectors: Intensive Margin Results

I next examine the impact of state EITCs on usual hours worked per week in each sector. As expected, hours in the informal sector decline (by about 1.4 hours/week if the state increases its credit by 10% of the federal credit) and hours in the regular sector increase (by 3.3 hours/week if the state increases its credit by 10% of the federal credit) and effects are significant except in the case of the fixed effects estimate for the informal sector. Again, the results contrast with the EITC results in the female labor supply literature: for men, I find no significant extensive-margin response but find large increases in hours in response to an increase in the state EITC corresponding to 1 percent of the federal credit. While I found large extensive-margin effects for the informal sector, the effects on hours are smaller. Since about 70% of the men in the sample already participate in the regular sector, it makes sense that we see effects on the hours margin rather than the participation margin for this group. The hours

²⁰McCubbin (2000) finds smaller effects of state EITCs than the federal EITC on the probability of claiming qualifying children. In 1994, however, only a few states had EITCs and most of those were not refundable. The growth of electronic income tax preparation in urban low-income areas could also strengthen the impact of state credits.

Table 3.8: Usual Hours Worked Per Week in Informal and Regular Sectors, Pooled Tobits and Fixed Effects OLS

	Regular Sector		Informal Sector		Total Hours	
	Pooled Tobit	Fixed Effects	Pooled Tobit	Fixed Effects	Pooled Tobit	Fixed Effects
State percent of federal credit	0.33** (0.1)	0.38*** (0.11)	-0.16* (0.1)	-0.14 (0.09)	0.24 (0.1)	0.23 (0.18)
State tax bracket rate	-3.02** (1.3)	-0.78 (1.58)	-0.94 (1.0)	0.05 (1.01)	-4.29** (1.7)	-0.75 (1.57)
State annual unemployment rate	0.30 (0.7)	0.29 (0.56)	-1.70*** (0.5)	-1.48*** (0.52)	-1.77* (1.0)	-1.03 (0.87)
Age	1.29*** (0.4)	1.73** (0.71)	-0.05 (0.3)	0.08 (0.49)	1.09** (0.5)	1.77* (0.95)
Age squared	-2.11*** (0.7)	-2.61*** (0.93)	-0.15 (0.5)	-0.18 (0.64)	-1.99** (0.8)	-2.68** (1.21)
Less than high-school education	-6.11*** (0.9)	-0.88 (2.26)	0.69 (0.5)	1.52 (1.88)	-4.48*** (1.0)	0.91 (2.68)
Some post-high-school education	2.49*** (0.9)	2.82 (1.93)	0.22 (0.6)	0.90 (1.54)	1.87* (1.1)	4.40 (2.88)
4-year college degree	2.57 (2.0)	9.15 (6.29)	-1.11 (1.2)	-2.34 (4.72)	1.25 (2.4)	5.85 (6.45)
Father currently cohabiting with partner	3.25*** (0.7)	0.68 (0.80)	0.70 (0.5)	-0.08 (0.68)	3.33*** (1.0)	0.55 (1.15)
Number of children living in household	-0.22 (0.4)	0.16 (0.36)	0.05 (0.2)	-0.17 (0.29)	-0.00 (0.4)	0.04 (0.41)
Hourly wage in current/most recent regular job	0.31*** (0.1)	0.06 (0.07)	-0.01 (0.0)	-0.09 (0.06)	0.32*** (0.1)	-0.03 (0.10)
Ever spent time in jail	-11.07*** (0.9)	-6.42*** (1.75)	4.62*** (0.6)	0.65 (1.77)	-5.29*** (1.2)	-5.17*** (2.27)
Has legal child support order	-1.47 (0.9)	-0.57 (0.77)	0.64 (0.6)	1.16 (0.77)	-1.16 (1.2)	0.46 (1.24)
R-square or pseudo R-square	0.019	0.027	0.018	0.045	0.010	0.030
N	5430	5430	5262	5262	5216	5216

All regressions include state and year indicators and occupation codes. Tobit results are presented as marginal effects on the observed outcome evaluated at the mean for continuous variables and effects of discrete changes from 0 to 1 for binary variables. Standard errors clustered at the state-year level are in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

effects for the informal economy are larger when I restrict the sample to individuals who work in the regular sector. Interestingly, the effect on total hours is smaller than regular-sector effect and insignificant in both specifications. This suggests that much of the effect is simply a substitution between sectors or a change in reporting rather than a real change in hours worked. If true, the EITC may not “encourage work” among low-income men, but may change the type of work they do and may encourage reporting. Effects on regular-sector weeks worked per year were positive but not significant.

3.5.3 Hurdle Models

The Tobit model used above imposes the restriction that labor supply determinants have the same effect on whether or not a person works and how many hours they work conditional on entering the labor force. This assumption is unreasonable when considering the labor supply of relatively low-skilled individuals. Fathers in the Fragile Families sample are relatively low-skilled, have less labor market experience, and are more likely to have criminal records than single men in the general population. Consequently, these men are likely to face barriers to regular-sector employment. There are fixed costs associated with employment as well: regular employment requires workers to arrive on a predetermined schedule, to dress appropriately, and sometimes to travel to the jobsite. Men must acquire a job before deciding how much to work or whether to report their earnings. Given the fixed costs of working and the barriers associated with landing a job, the effects of tax policy and other factors on hours worked may be quite different from the effects on the decision to work. One simple specification test for the Tobit model involves comparing the coefficients from the probit participation regression with the adjusted coefficients from the Tobit model: if the Tobit model is correctly specified, $\beta_{Probit} = \beta_{Tobit}/\sigma$. An informal comparison suggests that the assumption does not appear to hold for either the informal sector regressions or the regular sector regressions.

Cragg (1971) proposed a simple double hurdle model that relaxes the assumption that the effect of the independent variables is the same on the intensive and extensive margins. I estimate a probit in which the dependent variable is whether an individual participates in work and then estimate a truncated OLS regression of hours worked, for both the regular and underground sectors. Results are shown in Table 3.9. Relaxing the Tobit restriction that factors affect the participation margin and the hours margin in the same way results in slightly lower but still large and

Table 3.9: Hurdle Model: Marginal Effects for Participation and Hours Worked in Each Sector

	Regular Sector		Informal Sector	
	Probit	Truncated	Probit	Truncated
State percent of federal credit	0.003 (0.002)	0.24*** (0.1)	-0.004 (0.003)	-0.28 (2.0)
State tax bracket rate	-0.055** (0.022)	0.05 (1.6)	-0.019 (0.05)	-17.45 (17.5)
State annual unemployment rate	-0.001 (0.012)	0.52 (0.6)	-0.053*** (0.016)	-7.21 (9.1)
Black, non-Hispanic	-0.101*** (0.019)	-2.56*** (0.6)	-0.011 (0.024)	-2.73 (9.8)
Hispanic	0.048** (0.022)	-0.64 (0.7)	-0.065*** (0.02)	-6.83 (14.3)
Age	0.013* (0.007)	0.83*** (0.2)	-0.007 (0.008)	7.57 (5.8)
Age squared	0 (0)	-0.01*** (0.0)	0 (0)	-0.13 (0.1)
Less than high-school education	-0.084*** (0.014)	-1.66*** (0.6)	0.02 (0.014)	4.57 (9.4)
Some post-high-school education	0.037** (0.015)	1.05* (0.6)	0.019 (0.017)	-30.59* (16.7)
4-year college degree	0.044 (0.042)	1.74 (1.4)	-0.019 (0.038)	-17.05 (22.3)
Father currently cohabiting with partner	0.052*** (0.011)	0.58 (0.6)	0.026** (0.013)	-3.49 (9.7)
Number of children living in household	-0.003 (0.006)	-0.18 (0.2)	0.001 (0.005)	1.33 (3.4)
Hourly wage in most recent regular job	0.007*** (0.001)	-0.05 (0.0)	0 (0.001)	0.15 (0.6)
Ever spent time in jail	-0.173*** (0.014)	-0.33 (0.6)	0.11*** (0.017)	33.74*** (10.1)
Has legal child support order	-0.04*** (0.015)	1.24* (0.7)	0.024 (0.016)	-6.15 (11.2)

Marginal effects and corresponding standard errors clustered at the state year level are shown above. All regressions include state and year indicators and occupation codes. * significant at 10%; ** significant at 5%; *** significant at 1%

significant effects on regular-sector hours compared to the results in Table 3.7. Once again, the results indicate that men in the Fragile Families sample respond to the EITC by increasing hours in the regular sector and becoming less likely to participate in the informal sector. Other effects on hours also differ between the Tobit results and hurdle model results in Tables 3.7 and 3.9. Regular-sector hours effects for the state marginal tax bracket rate for a taxpayer just outside the credit region are zero in the truncated regression and the marginal effects and effects for race and age are smaller than in the Tobit regression. Hours effects for cohabitation and jail time are larger in both the regular and informal sectors.

3.5.4 Some Additional Tests

Unlike the EITC for taxpayers with qualifying children, the childless EITC is only available to workers between the ages of 25 and 64. Consequently, single men under age 25 who cannot claim qualifying children should not alter their labor supply in response to the EITC. Rows 1 and 2 of Table 3.10 shows that this not necessarily the case. Although EITC effects are significant only for men over 25 and these effects are consistent with the general results of participation responses in the informal sector and hours responses in the regular sector, effects for men under 25 are similar in magnitude if not significance. Age is not listed on tax forms, so one possibility is that non-eligible men claim the EITC before they turn 25. However, tax forms do require social security numbers that can be used to verify age. Additionally, tax software programs often ask filers to enter dates of birth in order to calculate credits and deductions correctly. Since more than two thirds of EITC filers used paid preparers as of 2003 and nearly as many use electronic filing (Internal Revenue Service 2005, Kopczuk and Pop-Eleles 2007), it seems unlikely that men are misreporting their age in order to claim larger credits. Another possibility, consistent with the large magnitudes of the effects, is that men are claiming qualifying children and receiving larger EITCs. This is consistent with the incentives discussed above: the maximum credit for filers who claim one qualifying child is almost 7 times as large as the maximum credit for a filer without a qualifying child, while the credit for filers claiming two children is 11 times as great.

We should expect to see effects for men who file or plan to file taxes, but not for men who have no plans to file. This appears generally consistent with the data in terms of the regular-sector results. However, the difference between filers and non-filers does not show up in the effects on informal participation. One possibility is

Table 3.10: Fixed Effects Results of Sectoral Participation by Subpopulation

	Participated in informal sector	Participated in regular sector	Informal hours	Regular hours
Age < 25	-0.004 (0.004)	-0.001 (0.003)	0.136 (0.128)	0.139 (0.152)
Age ≥ 25	-0.004 (0.003)	0.000 (0.002)	-0.279* (0.149)	0.334** (0.135)
Did not file taxes	-0.011 (0.010)	-0.001 (0.007)	0.237 (0.412)	0.477 (0.386)
Did file taxes	-0.008 (0.006)	0.008** (0.004)	0.253 (0.180)	1.023*** (0.237)

Cells show coefficients and clustered standard errors from fixed effects regressions of labor supply on the maximum earned income credit available measured in \$100s. * significant at 10%; ** significant at 5%; *** significant at 1%

that men decrease informal-sector participation and then learn that due to payroll tax consequences it is still not in their interest to file taxes. Still, the effects by tax filing status are disturbing.

3.6 Conclusions

I take advantage of states' introductions of state earned income credits that piggy-back on the federal EITC to identify changes in regular and informal sector labor force participation by single men living in large cities who have fathered at least one child out of wedlock. In contrast to the literature on EITC effects for single mothers, I find that these men adjust their regular-sector labor supply on the intensive rather than the extensive margin. Usual regular-sector hours worked per week increase by 3.3 hours if the state increases its credit by 10% of the federal credit. Participation in the informal sector appears to decline by 5 percentage points if a state increases its credit by 10% of the federal credit.

These effects are large in magnitude, but they incorporate several different types of responses: real labor supply changes due to the direct change in tax rates; reporting responses; and tax evasion responses in which men claim more children when state EITCs are a larger percent of the federal credit and then adjust their labor supply to the new phase-in rates for the credit with children. Although EITC noncompliance

may have increased if one response to state earned income tax credits is claiming more qualifying children, the EITC does seem to decrease tax evasion in the form of off-the-books work by low-income single men.

Saez (2002) argues that the optimal transfer program when behavioral responses primarily occur on the intensive margin is a classical negative income tax. If responses are primarily on the extensive margins, an earned income credit program is optimal. Previous research on the effects of the earned income credit on regular-sector labor supply indicates that most responses are on the extensive margin. For single men, this effect appears to be reversed: this may mean that an alternative transfer policy would be preferable.

3.7 Data Appendix

The Fragile Families and Child Wellbeing Study was conducted by jointly by Princeton University's Center for Research on Child Wellbeing and Center for Health and Wellbeing and Columbia University's Population Research Center and National Center for Children and Families. Principal investigators are Sara McLanahan and Christina Paxson at Princeton University and Irwin Garfinkel, Jeanne Brooks-Gunn, Ron Mincy, and Jane Waldfogel at Columbia University. The Fragile Families sample was chosen to provide detailed information about non-marital childbearing and fathers' involvement. Since state welfare and child support policy (and local implementation of those policies) influence parents' incentives to work, to cooperate with each other, and to contribute to the child, the Fragile Families survey was also designed to capture variation in child support and welfare policy while minimizing survey costs. By choosing cities with varying policy environments and sampling hospitals with high rates of non-marital births, the Fragile Families survey collected data on many non-marital births at relatively low cost. See Reichman, Teitler, Garfinkel and McLanahan (2001) for a detailed description of the sampling scheme used to construct the Fragile Families data.

The Fragile Families survey collected data on marital and non-marital births in 75 hospitals in 20 cities with populations of 200,000 or more in 1994. Researchers sampled cities, hospitals, and births. Baseline interviews for mothers were generally collected in the hospital. Restricting the sample to large cities allowed researchers to sample non-marital births from relatively few hospitals.

Sampling Cities

Cities were grouped by policy characteristics and cities were randomly sampled from each policy characteristics group to ensure variation in policy environments in the final sample. Investigators considered three policy variables: welfare generosity, strictness of child support enforcement, and strength of the local labor market. Cities were grouped into strong, moderate, and weak categories on each dimension. Welfare strictness was based on TANF benefits for a family of four and those benefits divided by median city rent. Child support strictness varied across cities within states because it was measured using city paternity establishment rates, percent of welfare cases with support orders, and city percent of welfare cases with child support payments. Labor market strength was primarily determined by the local unemployment rate but also by measures such as job growth.

Dividing cities into three groups based on three dimensions results in eight categories in which a city is “extreme” (either strong or weak) on all dimensions. Within these groups, cities were weighted by population and randomly sampled, yielding one city for each of the eight “extreme” categories. Eight cities were also randomly selected from the population-weighted group of cities that were not extreme on any of the three policy dimensions. Data was also collected from four cities of special interest to the Fragile Families research team: Detroit, Oakland, Milwaukee, and Newark. Appendix Table 3.11 shows the cities and their policy characteristics. Birmingham, Alabama and Santa Ana, California were originally chosen for the sample but researchers did not get permission from enough hospitals to sample there so these cities were replaced with ones drawn from the same group. Baltimore replaced Birmingham and San Jose replaced Santa Ana.

Sampling Hospitals

Austin, Corpus Christi, Newark, Oakland, and Richmond have five or fewer hospitals and researchers sampled births from all hospitals in the city. In most other cities, hospitals in the city were ranked by the number of non-marital births and hospitals were sampled in order beginning with the one with the most non-marital births until at least 75% of non-marital births were covered (with a limit of six hospitals per city for cost reasons).²¹ In New York and Chicago, which have especially large populations and high numbers of hospitals, researchers randomly sampled six hospitals from among the population of hospitals with more than 1,000 non-marital births per annum.

Sampling Births

Researchers set targets for births sampled in each city. Generally, 325 births (250 non-marital, 75 marital) were sampled in the extreme-policy cities and special-interest cities and 100 births (75 non-marital, 25 marital) were sampled in the moderate-policy cities. Within each hospital, researchers randomly sampled marital and non-marital births until they met quotas based on percent of non-marital births at the hospital. Since the hospital selection scheme is based on non-marital births, the sample of marital births is unlikely to be representative of all marital births in the city. The sample excluded mothers who put up babies for adoption, mothers who were too ill or

²¹In Philadelphia, which has many smaller hospitals, only 63% of non-marital births were covered due to the six-hospital restriction.

Appendix Table 3.11: Selection Characteristics of Cities and Hospitals

	Cities in Sample	Welfare ^a	Child Support	Labor Market	Hospitals
Cities with only “extreme” policies	Austin	W	W	S	All 3
	Boston ^b	S	S	S	Top 3, in order
	Corpus Christi	W	W	W	All 4
	Indianapolis	W	S	S	Top 3, in order
	New York	S	W	W	Random sample of 6
	Richmond	W	S	W	Top 2, in order
	San Jose ^{c,d}	S	W	S	Top 4, in order
	Toledo	S	S	W	Top 2, in order
Cities with moderate policies	Baltimore ^b	M	M	M	Top 5, in order
	Chicago	M	W	M	Random sample of 6
	Jacksonville	M	M	S	Top 3, in order
	Nashville	W	M	M	Top 3, in order
	Norfolk	M	S	S	Top 2, in order
	Pittsburgh ^e	S	S	M	Top 3, in order
	Philadelphia	M	S	M	Top 6, in order
	San Antonio	W	W	M	Top 5, in order
Special-interest cities	Detroit	S	S	W	Top 4, in order
	Milwaukee	S	S	S	Top 3, in order
	Newark	M	S	W	All 5
	Oakland	S	W	M	All 2

^aS indicates strong/strict policies, M indicates moderate policies, and W indicates weak or lenient policies.

^b“In order” means in order of nonmarital births beginning with hospitals with the most nonmarital births. Brigham and Women’s Hospital prohibited data collection immediately before the beginning of the sample, so that in fact births were sampled at only two hospitals in Boston. Consequently, the sample of non-marital births in Boston is not representative.

^cBirmingham and Santa Ana were originally chosen as sample cities. Because not enough hospitals agreed to participate in the survey, they were replaced by Baltimore and San Jose.

^dO’Connor Hospital in San Jose refused to participate: the top 4 hospitals in terms of non-marital birth with the exception of O’Connor Hospital were sampled. More than 75% of non-marital births were covered.

^eThe Western Pennsylvania Medical Center in Pittsburgh refused to participate: the top 3 hospitals in terms of non-marital birth with the exception of the Western Pennsylvania Medical Center were sampled. Consequently, only 72% of non-marital births in Pittsburgh were covered.

Appendix Table 3.12: Fragile Families Survey Response Rates, by Wave

	Mothers		Fathers	
	Unmarried	Married	Unmarried	Married
Baseline	87	82	75	89
One-Year	90	91	71	82
Three-Year	88	89	69	82
Five-Year	87	86	67	78
Ever Interviewed	100	100	86	96

Source: Center for Research on Child Wellbeing (2008). Baseline response rates for mothers refer to the percentage of randomly sampled births for which an interview was obtained. All other response rates refer to the percentage of the 4898 baseline mother interviews for which father interviews and follow-up interviews were obtained.

whose babies were too ill to interview, births in which the father was dead at the time of the baby's birth, and births in which a parent did not speak English or Spanish well enough to interview. Researchers from the National Opinion Research Center and Mathematica Policy Research conducted the interviews. In about two thirds of hospitals, researchers were only allowed to interview parents 18 years of age or older.

Response Rates

Appendix Table 3.12 shows response rates at baseline and in each followup wave. Information on interview eligibility was not collected in two cities at baseline, so response rates are imprecise. Based on five cities, 89% of unmarried mothers and 83% of married mothers responded for a net response rate of 85%. Fathers' response rates were 72% for unmarried fathers and 87% for married fathers respectively. Some non-response was due to mothers' refusal or inability to provide the father's name. Among identified fathers, father non-response is not random: fathers' baseline response rates are highly correlated with the fathers' relationship with the mother, as shown in Appendix Table 3.13.

Appendix Figure 3.2: Fragile Families Survey Questions about Participation in the Underground Economy

K22. We are interested in finding out about some ways, other than regular work, in which people make money. This kind of activity may be paid for in cash, or done in exchange for meals, or clothing, a place to live, or something else.

Please tell me if you have done any of the following in the last 12 months.

RECORD "YES" OR "NO" IN ROW A, THEN ASK B TO F FOR EACH ACTIVITY CODED "YES".

	K22 Work off the books or under the table? PROBE: Include under the table work in someone else's business, or work like housecleaning, household repairs, child care, or providing transportation or some other personal service. PROBE: Do not include work you already told me about. What kind of work? SPECIFY: _____	K23 Work in your own business? PROBE: This could include things such as doing other people's hair, either in your home or theirs. PROBE: Do not include work you already told me about. What type of business? SPECIFY: _____	K24 Sell or deliver drugs, engage in prostitution, or do other kinds of hustles? PROBE: Do not include work you already told me about. SPECIFY: _____	K25 Do anything else to earn money?
A. DID ACTIVITY? During the last 12 months, did you ... For all of these questions: ARTISTS/ATHLETES.....101 ADMIN. SUPPORT.....102 SALES.....103 CNSTRCTN/PRECS TRADE 104 GAMBLING.....105 EDUCATION/TRAINING.....106 SERVICES OCCUPATION.....108 PROFESSIONAL.....109 MECHANICS/REPAIRS.....110 REAL ESTATE/FINANCE.....111 LANDSCAPING/ARCH.....112 INFORMATION TECH.....113 TRANSPORT/DELIVERY.....114 ENTERTAINMENT.....115 ILLEGAL ACTIVITY.....116 OTHER.....117	YES.....1 NO.....2 ASK K23A IF K22A=YES, ASK K22B. IF NO, GO TO K23A	YES.....1 NO.....2 ASK K24A IF K23A=YES, ASK K23B. IF NO, GO TO K24A	YES.....1 NO.....2 ASK K25A IF K24A=YES, ASK K24B. IF NO, GO TO K25A	YES.....1 NO.....2 IF K25A=YES, ASK K25B. IF NO, GO TO SECTION L
B. In the last 12 months, about how many weeks did you (ACTIVITY)....123456789101112131415161718192021222324252627282930313233343536373839404142434445464748495051525354555657585960616263646566676869707172737475767778798081828384858687888990919293949596979899100101102103104105106107108109110111112113114115116117118119120121122123124125126127128129130131132133134135136137138139140141142143144145146147148149150151152153154155156157158159160161162163164165166167168169170171172173174175176177178179180181182183184185186187188189190191192193194195196197198199200123456789101112131415161718192021222324252627282930313233343536373839404142434445464748495051525354555657585960616263646566676869707172737475767778798081828384858687888990919293949596979899100101102103104105106107108109110111112113114115116117118119120121122123124125126127128129130131132133134135136137138139140141142143144145146147148149150151152153154155156157158159160161162163164165166167168169170171172173174175176177178179180181182183184185186187188189190191192193194195196197198199200123456789101112131415161718192021222324252627282930313233343536373839404142434445464748495051525354555657585960616263646566676869707172737475767778798081828384858687888990919293949596979899100101102103104105106107108109110111112113114115116117118119120121122123124125126127128129130131132133134135136137138139140141142143144145146147148149150151152153154155156157158159160161162163164165166167168169170171172173174175176177178179180181182183184185186187188189190191192193194195196197198199200123456789101112131415161718192021222324252627282930313233343536373839404142434445464748495051525354555657585960616263646566676869707172737475767778798081828384858687888990919293949596979899100101102103104105106107108109110111112113114115116117118119120121122123124125126127128129130131132133134135136137138139140141142143144145146147148149150151152153154155156157158159160161162163164165166167168169170171172173174175176177178179180181182183184185186187188189190191192193194195196197198199200
C. And, during those (NUMBER FROM B) weeks, about how many hours per-week did you (ACTIVITY)....123456789101112131415161718192021222324252627282930313233343536373839404142434445464748495051525354555657585960616263646566676869707172737475767778798081828384858687888990919293949596979899100101102103104105106107108109110111112113114115116117118119120121122123124125126127128129130131132133134135136137138139140141142143144145146147148149150151152153154155156157158159160161162163164165166167168169170171172173174175176177178179180181182183184185186187188189190191192193194195196197198199200123456789101112131415161718192021222324252627282930313233343536373839404142434445464748495051525354555657585960616263646566676869707172737475767778798081828384858687888990919293949596979899100101102103104105106107108109110111112113114115116117118119120121122123124125126127128129130131132133134135136137138139140141142143144145146147148149150151152153154155156157158159160161162163164165166167168169170171172173174175176177178179180181182183184185186187188189190191192193194195196197198199200123456789101112131415161718192021222324252627282930313233343536373839404142434445464748495051525354555657585960616263646566676869707172737475767778798081828384858687888990919293949596979899100101102103104105106107108109110111112113114115116117118119120121122123124125126127128129130131132133134135136137138139140141142143144145146147148149150151152153154155156157158159160161162163164165166167168169170171172173174175176177178179180181182183184185186187188189190191192193194195196197198199200	

Appendix Table 3.13: Father Response Rates By Strength of Father-Mother Relationship, Baseline

Married	88.5
Romantically involved on steady basis	85.9
On-again, off-again relationship	65.1
Just friends	53.1
Hardly ever talk to each other	40.6
Never talk to each other	15.2

Appendix Table 3.14: Item Non-Response: Participation in the Informal Sector

		Fathers			
		Not in wave	Missing	Don't know	Refuse
Baseline	Off the books	1068	23	1	2
	Own business	1068	39	0	6
	Hustles	1068	32	0	7
	Other	1068	187	0	5
1-Year	Off the books	1531	0	8	29
	Own business	1531	0	3	17
	Hustles	1531	0	1	17
	Other	1531	0	1	17
3-year	Off the books	1599	0	17	16
	Own business	1599	0	16	12
	Hustles	1599	0	15	15
	Other	1599	0	14	15
5-year	Own business	1739	0	4	22
	Hustles	1739	0	4	27
	Other	1739	0	5	24

Responses out of the total sample of 4898 births.

Item Non-Response

Appendix Figure 3.2 shows the page of the Fragile Families questionnaire relevant to the underground economy participation questions in wave 2. Item non-response for the questions about participation in the underground economy is very low, as shown in Appendix Table 3.14. Generally, responses of “Don’t know” or “Refused” constitute 1 percent or less of all responses. Although it is impossible to know the extent to which respondents underreport underground work, any underreporting in this sample seems to operate primarily through the mechanism of denying underground work rather than refusing to answer the question.

Weights

Only cross-sectional weights are included with the survey data. Weights account for the survey design and non-response. Each set of cross-sectional weights makes the wave representative of the original sampling framework. Since the sampling unit was births and mothers' willingness to be interviewed at birth determined whether the birth was included in the sample, mothers' baseline weights form the basis of all weights. Mother baseline weights account for the probability of selection at the city, hospital, and birth level and adjust for mother non-response. Father baseline weights further adjust for father non-response. In order that the weights make the data representative of the population of births in large cities, the weights were further adjusted via raking using marital status, age, education, and race as demographic characteristics. Weights for followup waves begin with baseline weights and are adjusted first for non-locatability, then for non-response, and then are re-raked.

The national sample weights make the data from the 16 randomly selected cities representative of births in large US cities. Weighting with city-level sample weights makes data representative of births in their particular city in year in which the data collection took place in the city.

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Chapter 4

Shifts Between Formal and Informal Child Support After Welfare Reform

4.1 Introduction

Child support payments for children on welfare are heavily taxed from the perspective of noncustodial parents. States retain most or all of the child support payments collected on behalf of children on welfare in order to offset state welfare costs. Welfare reform legislation increased states' power to tax child support payments: prior to passage of the federal Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA), states were required to pass through \$50 in child support to welfare parents each month and to disregard that income when determining welfare eligibility. After PRWORA, states were permitted to retain all child support payments and about half of states did so. Fathers consequently have strong incentives to pay outside the formal child support system. In this paper, I examine shifts between formal monetary child support payments and informal, in-kind support in response to states' pass-through and disregard policies using panel data from the Survey of Program Dynamics.

Previous research has not examined the tradeoff between formal and informal support, but has found evidence of small declines in formal support paid after pass-through and disregard were eliminated.¹ Since the net effect of these policies on child welfare depends on the changes in both formal and informal payments, it is useful

¹See, for example, Sorensen and Hill (2004), Cassetty, Cancian and Meyer (2002), or Cancian, Meyer and Roff (2005).

to understand whether parents substitute toward informal support. In addition, because avoiding formal child support frequently requires noncustodial parents to exit the formal labor market, understanding responses to disregard policies can tell us about the effects of child support enforcement on fathers' labor supply.

I use panel data from the Survey of Program Dynamics (SPD) to evaluate the effects of child support disregard policies on formal and informal support payments. Specifically, I examine the likelihood of having a formal child support agreement and the likelihood of the father providing in-kind support. Additionally, since mothers may alter welfare take-up rates in response to the policy change, I use multinomial logit regressions to examine mothers' combinations of welfare enrollment and types of child support receipt before and after state policy changes. The SPD panel data allows me to compare outcomes for the same families before and after welfare reform while controlling for family characteristics and unobserved father characteristics more effectively than in pooled cross-section data. I find that when the monthly child support disregard is \$100 lower, the likelihood that children in low-income families receive in-kind support is higher by approximately 4.1 percentage points, but I find no significant effects for the combinations of welfare and child support mothers receive.

In addition, the panel aspects of the Survey of Program Dynamics allow me to examine how the set of respondents to the child support survey questions change over time. One contribution of the paper is that it documents the highly transitory nature of the sample of respondents interviewed about child support. Approximately one-third of the sample turns over between waves.

4.2 Legislative Background

Historically, most children without a father were children of widows and state and local governments generally bore responsibility their maintenance. As divorce and out-of-wedlock childbearing became more common and the number of children with absent fathers grew, states' incentives to pursue child support increased. Policy reasons for pursuing child support obligers include both a desire to increase child wellbeing and an interest in minimizing state welfare expenditures. By 1976, many states attempted to intercept child support payments for children on welfare and retain them to offset state welfare expenditures. Intercepted child support payments faced 100 percent effective tax rates: welfare mothers' net income was unaffected by whether fathers paid formal support. Recognizing the disincentives to paying formal

Table 4.1: State Pass-Through and Disregard Policies in 2001

Disregard Amount, 2001	States
No disregard	Alabama, Arizona, Arkansas, Colorado, District of Columbia, Florida, Hawaii, <i>Idaho</i> , Indiana, <i>Iowa</i> , Kansas, Louisiana, Maryland, Minnesota, Mississippi, Missouri, <i>Montana</i> , Nebraska, New Hampshire, North Carolina, <i>North Dakota</i> , Ohio, Oklahoma, Oregon, <i>South Dakota</i> , Utah, Washington, <i>Wyoming</i>
$0 < \text{Disregard} \leq \50	<i>Alaska</i> , California, Delaware, Illinois, Kentucky, <i>Maine</i> , Massachusetts, Michigan, New Jersey, New Mexico, New York, Pennsylvania, Rhode Island, Texas, Virginia, <i>Vermont</i> , West Virginia
Disregard $> \$50$	Connecticut, Georgia, Nevada, South Carolina, Tennessee, Wisconsin

Survey of Program Dynamics respondents for the states listed in italics above are not included in the analysis because their state of residence cannot be identified in the survey.

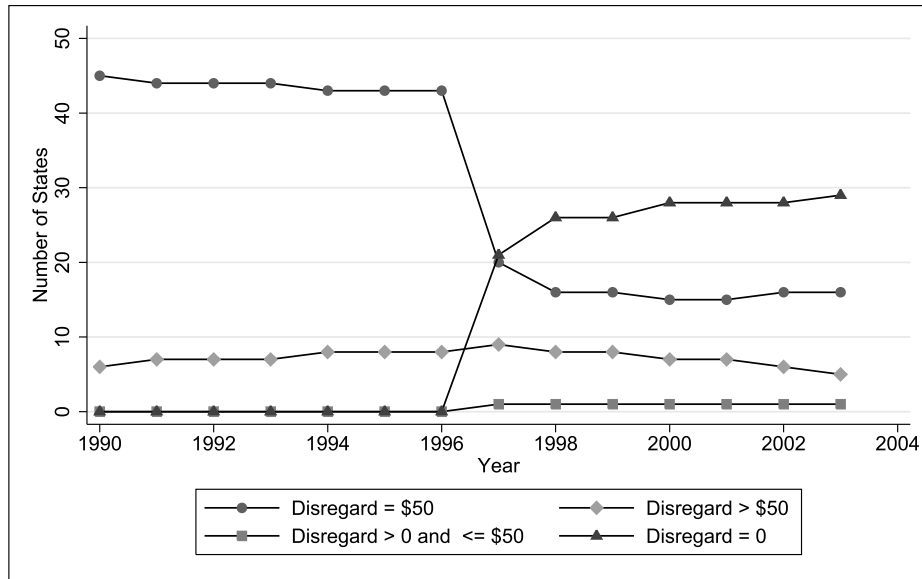
support, in 1976 the federal government passed a set of regulations that required states to pass through \$50 per month of child support for families on welfare and to disregard that income when calculating welfare eligibility. Child support payments in excess of \$50 per month could be retained by state welfare agencies to offset program expenditures. These regulations were modified and clarified in 1984.

In 1996, the federal government eliminated the requirement for \$50 pass-through of child support as part of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA). States were allowed to continue pass-through programs at their own expense; that is, states now had to cover the federal government's share of welfare costs.² About half the states eliminated pass-through and disregard after the change, effectively changing the marginal tax rate on the first \$50 dollars/month of child support from 0 to 100 percent. Table 4.1 shows the status of states' pass-through policies in 2001, the last year for which the Survey of Program Dynamics collected data on child support receipt.³ Both before and after PRWORA, some states passed through and disregarded more than \$50 month. In these states,

²Prior to 1996, the welfare cost of the mandatory \$50 pass-through and disregard of child support was split between the federal and state governments according to the Medicaid funding formula.

³Many states eliminated pass-through and disregard immediately after the passage of welfare reform; however, several states changed their policies after 1997.

Figure 4.1: States' Disregard Amounts, 1990-2003



the welfare payment is below the standard of need and state agencies pass through and disregard child support up to the standard of need. Figure 4.1 shows states' disregard amounts over time.

After October 1, 2008, child support pass-through and disregard may increase in many states due to changes in federal incentives for pass-through and disregard. The federal Deficit Reduction Act of 2005 will allow states to pass through from 0 to \$100 in one-child families and \$200 in families with two or more children without having to reimburse the federal government for its share of TANF costs. The Congressional Budget Office (2006) estimated that this would cost \$161 million over a one-year period, but would generate up to \$21 million in Food Stamp program savings.

States differ in how they account for and deliver child support and welfare payments. I follow Cassetty et al. (2002) and define pass-through as the amount money labeled as child support that is received by a parent and disregard as the amount by which a custodial parent's income increases when full child support is paid.⁴ Typically, these are the same, but they need not be. For example, Minnesota passes all child support on to parents, but disregards none of it for welfare purposes so that the welfare benefit amount was adjusted downward by a dollar for each dollar of support received. In contrast, in 2002 and 2003 Connecticut did not pass through any child support but increased welfare benefit checks by \$50 for parents who received support.

⁴Maria Cancian and Dan Meyer generously shared their pass-through and disregard data with me for this analysis.

I will focus on responses to changes in the disregard because it represents a family's net change in income.⁵

4.3 Should We Expect a \$50 Disregard to Affect Behavior?

Increasing the effective tax on child support makes paying formal support more costly relative to informal support if the penalties associated with avoiding formal support remain the same. Consequently, we should expect parents to shift from formal to informal support agreements. This may mean that fewer parents seek formal child support orders, preferring informal agreements or no agreements instead. It might also mean that instead of paying support through the formal system, fathers may make payments directly to the mother or may provide in-kind support.⁶ In-kind support may be particularly attractive to fathers because it gives them control over how the money is spent. Increased taxes on child support also make welfare less attractive to mothers relative to work (holding the fathers' formal payments constant) because the net value of child support and welfare decreases.

Roff (2008) models fathers' decisions to comply with formal support orders as a Stackelberg game in which mothers move first and choose whether to remain on welfare and whether to cooperate with the child support agency and report the father's paternity (which results in a child support order). The father then decide how much support to pay, and mother chooses her own and the child's consumption. Different child support withholding policies would change the return to welfare and would affect the outcome of the game. With this framework in mind, I examine the effects of state disregard policy on whether children have a legal support agreement and whether they receive in-kind support and what combinations of welfare, formal support, and in-kind support mothers receive.

From the custodial mother's perspective, discontinuation of the disregard has a large effect in percentage terms on combined income from child support and welfare. Welfare benefit levels vary across states: the top panel of Table 4.2 shows the lowest,

⁵If custodial parents treat child support money and welfare money differently, pass-through and disregard might have different effects. Parents may be more likely to earmark child support money for spending on the child while dividing welfare money between child and household expenditures.

⁶Fathers are the custodial parents in about 10% of child support cases. Most of the literature on child support focuses on cases in which the mother is the custodial parent. I restrict my analysis to custodial mothers and will refer to the custodial parent as the mother and the noncustodial parent as the father throughout the paper.

Table 4.2: Average “Tax” Associated with Eliminating a \$50 Child Support Disregard

Post-Reform Tax on Mother’s Welfare Benefit, Relative to Pre-Reform Benefit		
	Statutory Benefit Level	Average Tax Rate
State with lowest benefit level	120	0.29
State with highest benefit level	923	0.05
Median state benefit level	377	0.12
Pre-Reform Tax on Father’s Child Support Payment, If He Paid Ordered Amount in Full		
	Statutory Award	Average Tax Rate
Father’s Monthly Income \$720, Mother \$480		
State with lowest award	0	NA
State with highest award	302	0.83
Median state award	187	0.73
Father \$1500, Mother \$1000		
State with lowest award	240	0.79
State with highest award	585	0.91
Median state award	433	0.88
Father \$2460, Mother \$1760		
State with lowest award	415.4	0.88
State with highest award	821	0.94
Median state award	634	0.92
Father \$6300, Mother \$4200		
State with lowest award	863	0.94
State with highest award	1796	0.97
Median state award	1094	0.95

State TANF benefit levels in 1999 are from Administration for Children and Families (2000). State child support obligations in 1999 are from Pirog, Grieshop and Elliot (2003). State child support obligations amounts are based on the following assumptions. Parents are divorced; neither has remarried. Two children ages 7 and 13 live with the mother. Father files taxes as a single person with one deduction and has \$30 union dues as a condition of employment; he also pays for health insurance for the children at \$25/month. Mother files taxes as head of household with three deductions and pays \$150 in child care per month. The father spends less than 10% of his time with the children.

highest, and median family benefit levels across the fifty states and the District of Columbia for a family of three with no income in 1999. Assuming that at least \$50 in child support was paid on behalf of these families each month, a typical family of three would receive 12 percent less in combined child support and welfare income each month if the disregard was eliminated, and families in some states would receive 29 percent less. For low-income single mothers, a 12 percent decline in monthly income could easily be large enough to either induce her to stop receiving welfare and work instead or to bargain with the father to stop paying formal child support and pay informal support instead.

From the father's perspective, a change in the welfare system's child support disregard policy from \$50/month to no disregard may be less likely to impact behavior. Although the marginal tax rate on the first (monthly) dollar of child support increased from 0 to 100 percent in states that eliminated the disregard, the marginal tax rate on the last dollar of support paid remained the same for many fathers who were cooperating with the formal child system. Fathers who pay formal child support generally pay via income withholding and consequently pay the full amount of obligated support. State laws passed in the 1990s required withholding for all new or modified child support orders and in fiscal year 2001, 60.5 percent of child support monies that passed through state agencies were collected via income withholding and another 9.2 percent were collected by garnishing taxes or unemployment benefits (Office of Child Support Enforcement 2005). Current Population Survey data indicates fewer than one percent of fathers owing support to women on welfare in 1997 had child support obligations under \$50/month.⁷ Since employers withhold the amount required by the child support order and order amounts are almost always more than \$50/month, eliminating disregard does not affect the last-dollar marginal tax rate for fathers who pay via withholding.

We might expect fathers to respond to the increase in the average tax rate rather than the marginal rate if fathers do not understand the details of how pass-through and disregard operate. However, the average tax rate on formal child support payments was already very high before the disregard was eliminated. The top panel of Table 4.2 examines tax rates from the father's perspective. Pirog et al. (2003) report the statutory child support award in each state in 1999 and the bottom panel of Table 4.2 shows the median state's award amount and the amounts in states with the lowest and highest awards for four different parental income scenarios. The last column in the table shows the corresponding average "tax rate" prior to welfare reform, assum-

⁷Author's calculations using 1998 CPS March/April match file data.

ing that the father fully complies with the order and \$50/month is passed through.⁸ For fathers in the lowest-income group, the state median tax on child support was 73% and average tax rates were correspondingly higher for fathers with higher incomes.⁹ Since child support was very highly taxed even before welfare reform, we might not expect many parents to be on the margin of exiting the formal child support system and elimination of the disregard may have no effect.

Eliminating the welfare system's \$50/month disregard and pass-through of child support income had little effect on fathers' incentives to pay formal support in the sense that it did not change marginal tax rates on support for many fathers. But to the extent that fathers care about mothers' and children's support receipt, the effects are larger. Also, the disregards had large effects on mothers' incentives to request informal support from the father or to exit welfare and capture full child support. States' decisions to discontinue pass-through and disregard after welfare reform provide a convenient natural experiment with which to study the effects of these incentives on the resulting support arrangements between mothers and fathers.

4.4 Previous Research on Disregard Policies

The Child Support Demonstration Evaluation was a randomized experiment in which some Wisconsin welfare participants were granted full child support pass-through and others were kept at the maximum of the previous \$50 pass-through or 41% of child support received.¹⁰ The program began in 1997 and was evaluated by the University of Wisconsin-Madison's Institute for Research on Poverty.

Although the experimental Wisconsin program provides a unique and interesting test of the effects of eliminating the disregard, estimates from Wisconsin of the effect of disregarding all child support income may not be generalizable to the other states. The results from Wisconsin apply only to the least employable segment of the welfare population. Wisconsin's welfare program divided participants into four groups based on work experience. Participants in the top two groups did not receive cash benefits, although they did receive employment counseling and caseworker support and had access to other federal and state programs. The tradeoff between welfare and informal child support was consequently different in Wisconsin than in other states: these

⁸Average effective tax rates when \$50 are passed through are similar to those shown for 1999 in 1997 and 1995.

⁹Mothers are unlikely to be on welfare in these scenarios.

¹⁰Wisconsin's share of pass-through costs based on the Medicaid funding formula was 41%.

mothers were least able to exit welfare and it is likely that their partners were more likely to pay child support informally at all disregard levels.

4.4.1 Results from Wisconsin's Pass-Through Experiment

Meyer and Cancian (2001) and Meyer and Cancian (2003) evaluate the effects of the experimental program on the amount of support paid by fathers and on paternity establishment rates. In the first evaluation, Meyer and Cancian (2001) find that in cases in which mothers were eligible for full pass-through, mothers received \$150 to \$200 more child support per year than mothers in the control group, who received an average of \$500 in child support annually. They found a larger impact for those new to welfare than for previous welfare recipients, suggesting that it takes time for current participants to learn new rules. They also surveyed both custodial and noncustodial parents and service providers and found poor understanding of the program. Full pass-through increased paternity establishment rates but the difference was not statistically significant. Fathers in the experimental full pass-through group were about 7 percentage points less likely to have informal income. In the final evaluation, Meyer and Cancian (2003) found that pass-through increased rates of paternity establishment and the likelihood of receiving support, but contrary to their expectations, the effects of the program declined over time: the results in 2003 were smaller in magnitude than in 2001. Surveys of state child support workers indicated that caseworkers continued to have trouble understanding the program and the simultaneous changes in welfare policy may have reduced the impact of information about child support pass-through.

4.4.2 Evidence Using State and Time Variation in the Disregard

Studies using state-time variation in the child support disregard amount use two main sources of data: survey data from the Current Population Survey or state-level administrative data collected by the Office of Child Support Enforcement. Several outcomes have been examined: legal order establishment rates, paternity establishment rates, amount of child support receipt, and receipt of child support and/or welfare. The studies using state-level OCSE data find small statistically significant effects on paternity establishment and no effects on amount of support collected or the likelihood of receiving support, while studies using individual-level survey data

find larger effects on the likelihood of receiving support and the probability of having a formal support order.

Cassetty et al. (2002) and Cancian et al. (2005) use annual state administrative data collected by the federal Office of Child Support Enforcement and supplementary information about the amount of pass-through and disregard in each state to estimate the effect of pass-through and disregard amounts on three outcome variables: annual paternity establishment rates for welfare cases, percent of welfare cases that have payments, and average amount of child support collected per case. Cassetty et al. (2002) find that a \$100 increase in the disregard is associated with a 0.85 percentage point increase in the likelihood of establishing paternity. Using an improved measure of the disregard, Cancian et al. (2005) estimate an effect of 1.6 percentage points. In both studies, the effects of the disregard on percent of cases with collections and on average collections per case are not significant. Sensitivity tests find similar effects for paternities and find significant effects for disregard on percent of cases with collections, but effects are very small. Cancian et al.'s (2005) and Cassetty et al.'s (2002) studies analyze welfare cases only. Because child support policy may influence a mother's decision to request welfare, their results may not capture the full effects of the change in disregard.

Cassetty (2002) uses March CPS data from 1984-1999 to look at the likelihood that custodial parents who were on welfare at some point during the previous year received any child support. She finds that an increase of \$50 in the disregard was associated with a (marginally significant) 7 percentage-point increase in the likelihood of receiving support. Roff (2007) uses pooled cross-sectional data from the 1994-2000 Current Population Survey Child Support Supplement to look at the effect of disregards on types of child support orders. She finds that low-income custodial parents are more likely to have a formal child support order when the disregard is higher. She also examines the probability of having a legal order relative to an informal order or no order and finds that higher disregards reduce the probability of having an informal order or no order relative to a formal order.¹¹ Never-married women drive the results: Roff argues that these women have more choice in establishing a legal support order because paternity is already determined in divorce cases.

Sorensen and Hill (2004) use pooled cross sections of March CPS data from 1976-2001 to look at the effect of a variety of child support laws on mothers' child support and welfare status. Their results indicate that presence of a \$50 pass-through sig-

¹¹In the Survey of Program Dynamics data, very few parents report having informal agreements. Consequently, I do not examine the choice between a formal order, informal order, or no order.

nificantly increases the probability that both previously married and never married mothers will receive both welfare and child support rather than welfare only. Their results imply that, if the pass-through had been rescinded in all the states, the likelihood of receiving child support would have declined by 12.2 percentage points for previously married mothers and 7.2 percentage points for never married mothers. Although they do not use state-level variation in the disregard, Bassi and Lerman (1996) report similar effects by comparing welfare recipients to non-recipients. They cite data from the Child Support Evaluation Program and report that although mothers who receive welfare are no more likely to report that the child's father cannot afford to pay support than mothers who do not receive welfare, child support enforcement agencies are three times more likely to collect in non-AFDC cases.

Meyer and Cancian (2002) use variation in mother's welfare status over time to look at fathers' tendency to pay support. They use monthly Wisconsin Court Record Data merged with monthly administrative AFDC records from 1980-1993 to conduct an event-history analysis of fathers' transitions in and out of payment. They find no difference in the likelihood of starting payment for men whose partners have recently left AFDC, and no difference in the likelihood of stopping payment for men whose partners have recently started AFDC. The authors suggest several possible reasons why they find no effect. Fathers might not understand the system or might not be informed of the mother's shifts on and off of welfare. Alternatively, the child support enforcement system might target enforcement more intensively toward fathers in cases where the mother receives welfare, which would bias the expected effects toward zero.

Another strand of the literature estimates household bargaining models and uses the parameter values to estimate the effects of disregard policy on behavior. Models typically have a Stackelberg structure as in Weiss and Willis (1993). Fathers and mothers each care about own consumption and leisure and about the child's consumption, which is modeled as a public good. Fathers choose their labor supply and how much support to give to the mother, which may involve deciding the sector in which to work. Mothers choose their labor supply or welfare receipt, how much to spend on the child, and (in some models) whether to cooperate with a child support agency. Cobb-Douglas utility is typically assumed for purposes of structural estimation. Estimates of the weight fathers place on their own consumption range from 0.75 to 0.80 (Del Boca and Flinn 1995, Flinn 2000, Roff 2008). Roff (2008), who estimates the model using data from the Teenage Parent Demonstration Project, simulates the effect of moving from a \$50 disregard to an unlimited child support disregard. She finds that the change would lead to a small (< 1 percent) but statistically significant

increase in fathers' compliance with child support orders, an increase in paternity reporting of approximately 3 percent, and higher likelihood of mothers remaining on welfare.

Welfare reform not only allowed states to change how they disregarded child support payments, it also allowed states to change the way they disregarded other types of income. Generally, states increased the earnings disregard and lowered effective tax rates on single mothers' earnings. In many states the effective change in tax rates was quite large. Blank and Matsudaira (2008) examine the effect of changes in welfare earnings disregards on labor supply. Surprisingly, they do not find large or significant of the earnings disregard on single mothers' labor supply on either the intensive or extensive margin. Blank and Matsudaira demonstrate that this was because few women used the earnings disregards. They speculate that working may make welfare participation more difficult because welfare participation is costly in terms of time and psychological commitment; women may leave welfare when they are working in order to preserve future welfare eligibility that is restricted by time limits; or case-workers may have discouraged working women from using the earnings disregards. Since child support disregards are implemented automatically, the take-up rates for child support disregards will be much higher than for welfare earnings disregards.

With the exception of Sorensen and Hill's (2004) study on mothers' child support and welfare receipt, most studies find only small effects of the disregard on behavior. This is not surprising given the high pre-reform tax rates on child support described above. The larger responses in Sorensen and Hill's (2004) study may be because they look at outcomes over which mothers exert more control since the choice to leave welfare is up to the mother.

Previous studies have not examined tradeoffs between formal and informal types of support in response to disregard policies. The ethnographic literature on child support and welfare documents that fathers and mothers are aware of disregard policies, consider them unfair, and prefer informal support. Furstenberg (1992) reports that mothers complained that wage withholding backfired when fathers quit their jobs to avoid paying formal support and some fathers prefer to pay support under the table because the money is more likely to benefit the children. Waller (2002) reports similar findings: in-kind support is more attractive both because it is untaxed by the welfare system and because fathers say that being able to give their children clothes or other tangibles has more impact on children's understanding that their fathers care about them than paying formal, taxed child support. Given the evidence from focus groups and interviews with low-income parents, the role of informal support may be

large. Better understanding of how the current system induces parents to substitute between support types is important for understanding whether current child support policies deliver the most support to children at the lowest cost.

4.5 Survey of Program Dynamics Data and State Policy Variables

I examine changes in in-kind support receipt rates in response to changes in the child support disregard amount. I use the Survey of Program Dynamics (SPD) Third Longitudinal File, which is a follow-up survey to the 1992 and 1993 panels of the Survey of Income and Program Participation (SIPP). Few studies have used SPD data, perhaps because the data is stored in multiple files and different formats. The SPD longitudinal file includes information on demographics, income, and program participation. To this, I merge child support data from the Survey of Program Dynamics cross-section files for 1998-2002 and the child support topical modules for the 1992 and 1993 SIPP.¹² These files contain the data for my outcomes of interest: whether the child has a legal support order, whether the mother received in-kind support on behalf of the child, and the amount of child support and welfare the mother received.

Household panel data have not been used to study pass-through and disregards but they are advantageous for several reasons. First, since child support surveys generally collect data from the custodial parent only, it is usually impossible to control for the noncustodial parent's characteristics. Panel data methods difference out father characteristics that are constant over time. Second, the characteristics of the child support population have changed over time as the percentage of never-married mothers increases. Comparisons of child support and welfare recipients before and after welfare reform may be biased if the composition of the groups changes over time. This is a particularly serious problem in the studies that use Office of Child Support Enforcement administrative data for welfare cases, since whether to receive welfare is one margin on which mothers may have adjusted and consequently the composition of welfare cases could change over time in response to the treatment. Using panel data allows us to examine the effects of a change in the disregard on families eligible for child support before and after welfare reform. Last, the Survey of Program Dynamics panel allows us to track mothers' and children's transitions in and out of the child support sample and it reveals that there is high turnover in the child support sample

¹²These are 1992 topical modules 6 and 9 and 1993 topical modules 3, 6, and 9.

between years. Since the SIPP (on which the Survey of Program Dynamics is based) is constructed similarly to the Current Population Survey, insights about turnover in the Survey of Program Dynamics child support sample are indicative of turnover in the commonly used CPS.

A disadvantage of the Survey of Program Dynamics data is that the sampling scheme changes over time. The 1992 and 1993 Survey of Program Dynamics were designed to be representative of the civilian non-institutionalized US population. The first wave of the Survey of Program Dynamics, conducted in 1998 interviewed a subsample of 19,139 households who participated in the last SIPP wave. The 1998 SPD wave selected all households with household income less than 150 percent of the poverty level and all households with children between 150 and 200 percent of the poverty level, with lower sampling rates for higher-income households, particularly those households without children. Census added 3500 households in 2000 and 5000 households in 2001. Due to budget constraints, Census reduced sample sizes evenly by a third in 2002.

4.5.1 State Policy Variables

I control for a number of state characteristics. To capture state economic conditions that affect parents' access to jobs, I control for annual state median income for a family of four and state unemployment rates. I also include several measures of child support enforcement, including average administrative expenditures per case, the number of years a state has had a new hire directory, and whether the state's automated child support enforcement system is (partially) certified. (Most states received full federal certification after the end of the panel.) Higher administrative expenditures per child support case may indicate states that pursue noncompliant fathers actively. State new hire directory laws required employers to report new hires to state agencies on a monthly rather than quarterly basis, which made it more difficult for fathers employed in the formal sector to evade income withholding. Certification of a state's automated child support enforcement system indicates how quickly states can find fathers and implement support orders.

One concern with using variation in post-welfare reform disregard policies to analyze the change in disregard is that if states that eliminated disregard were also more likely to adopt stricter welfare guidelines, it will be difficult to tell whether changes in child support payment should be attributed to changes in disregard policy or other aspects of welfare reform. However, there does not seem to be a pattern between the

Table 4.3: Relationship Between Child Support Disregard Levels and Welfare Aggressiveness

	Coefficient	Constant	R^2
Aggressive: Ellwood's measure	15.06 (30.38)	41.15 (20.84)	0.005
Aggressive: Ellwood/Meyer & Rosenbaum's measure	5.32 (30.45)	45.42* (22.15)	0.001
Aggressive: Grogger & Karoly's 1st measure	23.06 (30.96)	39.19* (19.39)	0.011
Aggressive: Grogger & Karoly's 2nd measure	8.98 (30.53)	43.30 (22.62)	0.002
Aggressive: Grogger & Karoly's 3rd measure	62.35 (33.73)	32.34 (17.03)	0.065
Est. 3-person family welfare benefit level, \$100s (Ziliak)	-5.89 (13.62)	75.77 (40.22)	0.016
Est. welfare tax on earned income (Ziliak)	-0.48 (1.53)		

See Ziliak (2007) for construction of the welfare aggressiveness measures. * significant at 5%

aggressiveness of states' welfare reform and their decision to eliminate the child support disregard. I test this by regressing state disregard amounts on several measures of welfare aggressiveness provided by Ziliak (2007). The first five measures are binary and are equal to 1 in "aggressive" states.¹³ The last set of measures are Ziliak's (2007) estimates of effective benefit levels and welfare tax rates on earned income in 2001. Results are shown in Table 4.3. There are no statistically significant relationships between any of the aggressiveness measures and disregard levels. Interestingly, the coefficient on all measures is positive, indicating that if anything, states with more aggressive welfare policies were more likely to retain the child support disregard. Although there is little correlation between whether states retained the child support disregard and other measures of welfare reform, I control for the estimated welfare benefit level and welfare tax on earned income in the regressions below.

¹³The first is based on a measure developed by Ellwood (2000) and is a dummy variable equal to 1 if the difference between actual AFDC and predicted AFDC participation with no reform is more than 10 percent. Ellwood constructed the second measure using four characteristics of program aggressiveness suggested by Meyer and Rosenbaum (2001), including the change in benefit levels, imposition of time limits, and sanctions for noncompliance. The third, fourth, and fifth measures are from Grogger (2003) and indicate whether the state had 3 or more waivers in 1992-96, whether the state had any waiver, and whether 4 studies of sanction policy determined policy was stringent.

4.5.2 The Child Support Sample Over Time

One useful feature of the Survey of Program Dynamics data is it allows me to look at whether parents and children who are asked child support questions in one wave remain part of the child support sample in subsequent waves. Surprisingly, although the characteristics of the child support sample remain similar across waves, about one third of children with a parent outside the household who respond to child support questions in one wave do not respond to child support questions in the next wave. This could be either because of sampling and survey issues or because the children are living with both parents in subsequent waves.

The Survey of Program Dynamics is the only large-scale panel dataset that asks child support questions on an almost yearly basis over such a long period of time. It provides new insights into who responds to child support survey questions. Since the Current Population Survey and the Survey of Income and Program Participation (on which the Survey of Program Dynamics is based) identify the child support sample in similar ways, information about transitions in and out of the SIPP/SPD child support sample is likely to apply to the Current Population Survey's March/April match file, which is one of the most frequently used datasets for child support research.

Both the SIPP and CPS base the child support sample on the set of children who have a parent living outside the household. In the SIPP, parents or guardians with children under 21 are asked whether those children have a parent living outside the household. If so, parents/guardians are asked about child support agreements for those children. The CPS asks about child support agreements for children who have a parent outside the household or do not live with both of their biological or adoptive parents. In the Survey of Program Dynamics, child support data is collected at the child level instead of at the mother level.

The extent to which transitions in and out of the SIPP/SPD child support panel indicate similar transitions in the CPS population depends on the extent to which the two surveys capture the same population. I compare data from the first child support topical modules in the 1992 and 1993 SIPP panels to data from the 1994 CPS March/April match file.¹⁴ These correspond to similar time periods: interviews for the SIPP modules were conducted between September and December 1993 and ask about child support during the past 12 months. The CPS interviews were conducted in April 1994 and refer to child support due or collected during 1993. Both surveys were designed to be representative of the US civilian non-institutionalized

¹⁴These are the 1992 SIPP topical module 6 and 1993 SIPP topical module 3.

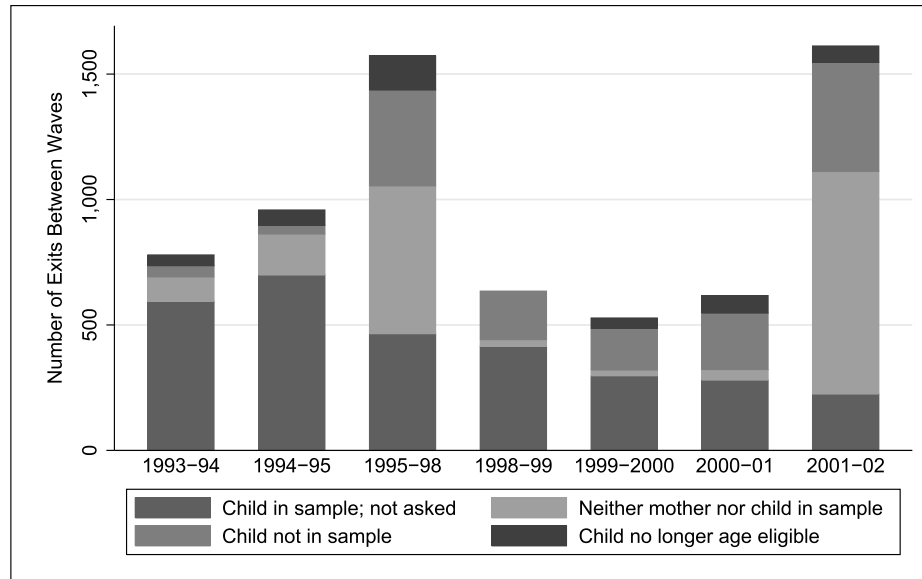
Table 4.4: Comparison of 1994 CPS and 1992/1993 SIPP Child Support Samples

	CPS 1994	SIPP 1992 TM 6	SIPP 1993 TM 3
<hr/>			
Number of Observations			
Full Sample	105,166	51,286	53,935
Age < 21	32,667	16,460	17,495
Children < 21 with parent outside household	8,640	3,622	3,816
Parents with child support data	5,325	2,251	2,302
Weighted Sample Fractions and Means			
Fraction of sample < age 21	0.31	0.32	0.32
Fraction of children < 21 with parent outside household	0.26	0.22	0.22
Fraction of children with any agreement	0.55	0.47	0.49
Fraction of parents with legal agreement	0.56	0.48	0.46
Fraction of parents with informal agreement	0.04	0.06	0.06
Fraction of parents with no agreement	0.40	0.47	0.48
Average year agreement was first reached	1988	1988	1988
Agreements with payment due in 1993/last 12 months	0.94	0.92	0.94
Agreements with back support due in 1993/last 12 months	0.29	0.40	0.42
Fraction of cases in which other parent lives in same state	0.74	0.77	0.74
Average support due to parents with agreements	\$3,581	\$3,313	\$3,722
Average support received by parents with agreements	\$3,085	\$3,205	\$3,578
Custodial parent is male, fraction	0.16	0.13	0.12
Custodial parent is white, fraction	0.70	0.71	0.70
Custodial parent is black, fraction	0.26	0.26	0.27
Custodial parent never married, fraction	0.27	0.24	0.24
Average age of custodial parent	34.7	34.9	34.7

population. Table 4.4 shows summary statistics for the child support populations in the CPS and each SIPP panel.

Overall, the group of custodial parents and children who respond to the child support questions are similar across the surveys. The weighted fraction of the sample under age 21 is very similar, but only 22 percent of children are identified as having a parent outside the household compared to 26 percent in the CPS. This may be partly because the public-use SIPP includes data on only four children per parent or guardian. Another possibility is that because the SIPP asks whether a parent's "own children living here" have a parent outside household, children with neither parent in the household may be under-represented relative to the CPS. Parents of children in the CPS are more likely to report having a legal child support agreement (56% in the CPS as opposed to 46-48% in the SIPP), but among parents with support orders, the amount of child support ordered and paid is very similar, as is the length of time for which parents have had the order. One major difference is that parents in the

Figure 4.2: Reasons for Child Exit from the SPD Child Support Sample



SIPP are much more likely to report that the noncustodial parent owes back support: 29 percent in the CPS compared to 40 to 42 percent in the SIPP.¹⁵ The characteristics of custodial parents are similar across surveys, although the CPS includes more never-married parents.

Given the strong similarities between the SIPP and the CPS, the frequency of transitions in the SIPP sample is likely to be indicative of amount of transitions in the CPS sample, which we do not observe. Although the percentage of people under age 21 with parents outside the household remains fairly constant across waves, nearly 1/3 of the child support sample turns over between interviews. Table 4.5 shows the fraction of children within the SIPP/SPD child support who enter and exit the sample between years. Figure 4.2 shows the reasons for exits from the child support sample. Children who are no longer age-eligible for child support represent a small fraction of the turnover, as do cases in which the mother and child are no longer in the survey.¹⁶ A larger fraction of exits are due to children leaving the household (while the mother remains). This group is small in the 1993-1995 SIPP waves, but much larger in the 1998-2002 Survey of Program Dynamics waves. But in the entire 1993-2002 period, over half of the exits from the child support sample represent children who remain in the survey but are not asked the child support questions.

¹⁵The phrasing of the questions does not provide an obvious explanation for the difference.

¹⁶The exception to this is exits between 1995-1998 and between 2001-2002. In these years the SPD sample was cut.

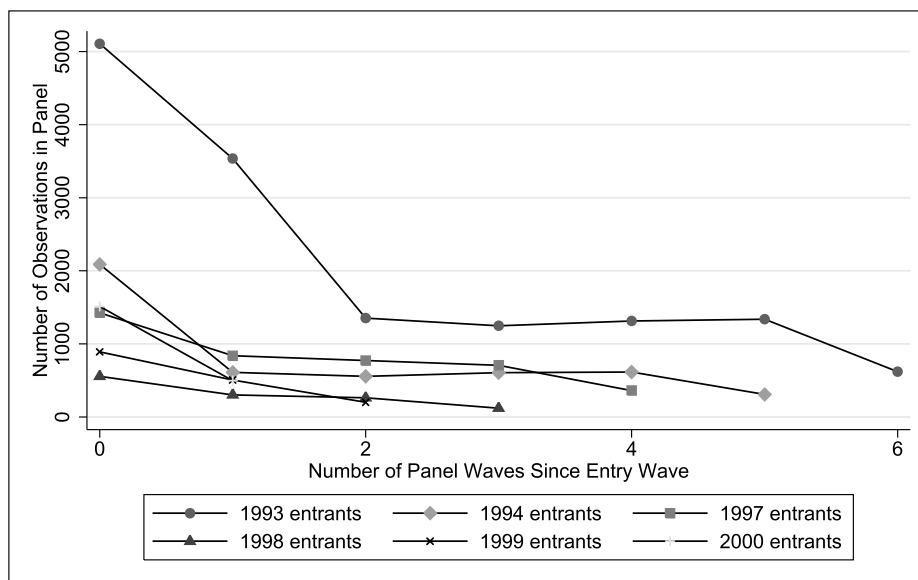
Table 4.5: SIPP and SPD Child Support Sample, By Year

	1993	1994	1995 ^a	1998	1999	2000	2001	2002
(1) Respondents Age < 21	28,000	26,706	14,257	21,490	18,272	20,098	22,903	12,533
(2) Children without father in household	6,324	6,322	2,925	4,385	4,867	5,245	6,144	3,359
(3) Children without father, valid identifiers ^b	5,205	5,846	2,677	3,457	3,233	3,967	5,004	2,674
(4) Children with child support agreement data	5,197	5,835	2,669	3,392	3,199	3,885	4,946	2,646
Fraction of under-21 sample without father (2/1)	0.23	0.24	0.21	0.20	0.27	0.26	0.27	0.27
Fraction of under-21 sample without father, identifiable (3/1)	0.19	0.22	0.19	0.16	0.18	0.20	0.22	0.21
Fraction of children w/out fathers, identifiable (3/2)	0.82	0.92	0.92	0.79	0.66	0.76	0.81	0.80
Fraction of under-21 sample w/ agreement data (4/1)	0.19	0.22	0.19	0.16	0.18	0.19	0.22	0.21
Fraction of children w/out father w/ agreement data (4/2)	0.82	0.92	0.91	0.77	0.66	0.74	0.81	0.79
Fraction who are new entries	1.00	0.37	0.20	0.46	0.16	0.22	0.30	0.20
Fraction who will exit in next wave	0.31	0.49	0.61	0.37	0.31	0.31	0.64	1.00

^aThe SIPP 1992 panel ended before 1995; this column includes only SIPP 1993 panel participants.

^bIn the SIPP, many parents list children as having fathers outside the household but either the children are not in the sample or their personal identifiers are corrupted. In the SPD, some respondents both do not answer the child support questions and do not include a reference child (sibling) whose child support data can be used.

Figure 4.3: Persistence of Child Support Sample Entrance Over Subsequent Panel Waves



Since the child support questions are predicated on the child having a parent who lives outside the household, this may mean that these children now have both parents in the household. However, this is difficult to confirm in the SIPP, which records a designated parent for each child but does not record whether the child has both a father and mother in the household. Most exits from the child support sample occur in the first year after entry into the child support panel, as shown in Figure 4.3. Each line in the graph represents children who entered the child support sample for the first time in a particular year. The decline between the first and second years is quite steep, while in subsequent years the number of participants remaining is relatively stable. (The steep declines in the last year are due to large cuts in the SPD sample size in the final year of the panel.)

Current survey methods for identifying the sample of children eligible for child support result in a child support sample with a large transitory component. One possibility is that these children are not really eligible for child support. Instead, the fathers might be absent from the household for only a short time. If so, survey estimates could overstate the number of children without child support orders. Another possibility is that the child support-eligible population is even larger than past estimates suggest, but we capture only a fraction of it in surveys. Either way, better documentation of how the child support modules in surveys are conducted or more careful design of child support modules would be helpful.

Table 4.6: Legal Agreement Rates and In-Kind Support Receipt Rates by State Disregard, SPD Data

	Disregard	1993	1994	1997	1998	1999	2000	2001
Legal Agreement Rates	Disregard > 0	0.48	0.51	0.52	0.54	0.54	0.54	0.55
	Disregard = 0			0.59	0.64	0.58	0.56	0.60
In-Kind Support Receipt Rates	Disregard > 0	0.13	0.16			0.51	0.51	0.47
	Disregard = 0					0.51	0.52	0.52

4.6 Effects of Disregard Policy on Legal Orders and In-Kind Support

Table 4.6 shows the percentage of children who had legal child support orders and the percentage of children for whom in-kind support was received in each wave of the data, for states with and without positive disregards. States that eliminated the disregard originally had higher rates, and the higher rates of legalized orders persist over time. The gap does not appear to widen over time. The raw data does not indicate an effect of disregard policy on whether parents have a legalized support order. This is not necessarily surprising: states pursued legal child support orders intensively for mothers with welfare claims, so mothers who claim welfare will have limited control over whether a legalized order is issued. Since most non-welfare mothers will not respond to the disregard policy, it is reasonable to see no significant difference in order rates by welfare status. Also, once orders are established, they remain in effect until a child is 18 or 21, which limits parents' ability to adjust to changing laws. A higher percentage of children do receive in-kind support in states with no disregard in 2000 and 2001. It is much easier for parents to adjust how support is paid than whether they have a legal support order. To assess these effects more formally, I use OLS fixed-effects regressions (with individual fixed effects) to estimate the effect of state disregard policies on whether a child is covered by a child support order and whether in-kind support was received on behalf the child. For these regressions, I use the child (rather than the mother) as the unit of observation. This is particularly helpful for tracking whether a child has a legal order. Since mothers may have orders from multiple fathers and the children in the household may change between waves, using the child as the unit of observation ensures that the same orders are compared

over time.¹⁷ I estimate the following model:

$$Y_{ist} = \theta_1 + \theta_2(\text{Disregard}_{st}) + \theta_3 X_{ist} + \theta_4 Z_{st} + \mu_i + \eta_t + \phi_s + \epsilon_{ist}$$

where Y is the outcome of interest for individual i in state s and year t , X is a vector of time-variant individual characteristics, Z are policy variables for a particular state and year, and μ , η , and ϕ capture individual, state, and year fixed effects. Since disregard policy applies only to child support cases for which the custodial parent receives welfare, I restrict the sample to children in households with income less than 200 percent of the poverty level in at least one wave.

Table 4.7 shows results from fixed effects regressions of whether the child has a legal child support agreement on the state disregard and a vector of state policy controls and custodial parents' characteristics.¹⁸ Without controls, the coefficient on the disregard indicator implies that states with disregards have order establishment rates that are lower by 7.7 percentage points (column 1). Including state and year fixed effects is enough to eliminate the negative coefficient on the disregard variable, indicating that the differences in legal order establishment rates in Table 4.6 are due to different states' effectiveness in establishing orders (column 2). Once state effects are accounted for, having a positive disregard has a positive but insignificant effect on having an established child support order. The importance of state enforcement effectiveness is further established by the effect of having a certified child support automation system in place. Certification increases the rate of legal order establishment in a state by 2.8 percentage points (column 3, not significant). Other state policies have no significant effects.

Parent's characteristics combine with state enforcement effectiveness to determine the likelihood of having a legal child support order. In an individual fixed-effects regression, time-invariant demographic characteristics are differenced out, so only time-variant characteristics are included. Age of either the mother or child has a positive effect of having an established order. Time since the parents' separation will generally be longer for older children, so if it takes time to establish an order, we would expect higher order rates for older children. Mothers who are older conditional on the age of their children will generally have better demographic characteristics. The significant negative effects on child's age squared and mother's age squared may indicate that as time since the parents' separation increases, the likelihood of estab-

¹⁷The ideal unit of observation would be the set of fathers with which mothers have children. However, I cannot identify which children have the same father in the data.

¹⁸Attempts to estimate these models in a conditional logit framework did not converge.

Table 4.7: Fixed Effect Regressions: Child Is Covered by Legal Child Support Agreement

	Sample: Children with Family Income \leq 200% of Poverty Level				
	(1)	(2)	(3)	(4)	(5)
Disregard > 0	-0.077** (0.017)	0.022 (0.021)	0.013 (0.022)	0.021 (0.022)	0.012 (0.023)
State has partial certification			0.028 (0.014)		0.029* (0.014)
Years state has had new hire directory			0.001 (0.005)		0.002 (0.005)
State median income for family of 4, \$1000s			0.004 (0.002)		0.004 (0.002)
State unemployment rate			0.001 (0.010)		-0.004 (0.010)
State expenditures per child support case			-0.085 (0.092)		-0.096 (0.093)
Est. welfare benefit (3-person family), \$100s			0.014 (0.019)		0.015 (0.018)
Est. welfare tax rate on earned income			0.057 (0.034)		0.044 (0.034)
Family received public assistance				0.011 (0.018)	0.012 (0.018)
Child's age				0.040** (0.009)	0.039** (0.009)
Child's age squared/100				-0.153** (0.026)	-0.152** (0.026)
Mother's age				0.018 (0.009)	0.018 (0.010)
Mother's age squared/100				-0.025* (0.011)	-0.026* (0.011)
Mother has $<$ high school education				0.005 (0.029)	0.006 (0.029)
Mother has some postsecondary education				0.006 (0.018)	0.007 (0.018)
Mother has 4-year college degree				0.012 (0.041)	0.012 (0.041)
Mother is currently married				-0.087** (0.022)	-0.087** (0.022)
Mother is divorced				0.011 (0.024)	0.010 (0.024)
Number of children < 18 in household				0.001 (0.006)	0.001 (0.006)
State and Year Fixed Effects	No	Yes	Yes	Yes	Yes
R-Square	0.004	0.035	0.037	0.054	0.055
N	20079	20079	20079	19770	19770

Regressions include data from 1993, 1994, and 1997-2001 and are unweighted. Standard errors clustered by state are in parentheses. * significant at 5%; ** significant at 1%

lishing an order eventually decreases. Currently married mothers are less likely to have orders; they may be less likely to pursue support because they receive support from their new husband. Generally, in results including both state characteristics and parental characteristics the effects of the child support disregard variable are small and insignificant, which likely reflects parents' inability to adjust the legality of their order status.

Results for the effects of disregard policy on receipt of in-kind support are shown in Table 4.8. The coefficient from the individual fixed-effects regressions with no controls (column 1) indicate that a \$100 increase in the amount of child support disregarded when calculated welfare benefits corresponds to a 6.2 percentage-point decrease in the likelihood of a child receiving in-kind support (not significant). This result is consistent with fathers substituting between formal and informal support and providing more in-kind support when child support is more heavily taxed. Adding state and year fixed effects to the model reduces the magnitude of the coefficient and it becomes significant at the 5% level. In column 3, a higher welfare benefit level leads to a higher likelihood of in-kind support: higher benefit levels may make mothers more likely to receive welfare and consequently face the child support disregard policy. Other aspects of state child support and welfare policy do not appear to significantly affect the choice of in-kind support. Once we control for other aspects of state enforcement which might determine whether a child has a legal support order, the disregard variable should measure the relative attractiveness of informal and formal support.

Receiving public assistance in a given year increases the likelihood of receiving in-kind support by 7.1 percentage points. Since these individual fixed effect regressions capture how changes in the independent variables affect changes in the outcome, this means that within children, they are more likely to receive in-kind support in years their mother received welfare. This is consistent with the importance of the disregard because the disregard only applies to welfare cases. Interestingly, mothers' receipt of a college degree increases the likelihood of receiving in-kind support by 18 percentage points. Receiving a college education may make mothers more likely to request money for child care or summer camp for children. Overall, a \$100 decrease in the disregard appears to increase the probability of receiving in-kind support by about 4.1 percentage points, and the effects are statistically significant (column 5).

Table 4.8: Fixed Effect Regressions: Child Received In-Kind Support

	Sample: Children with Family Income \leq 200% of Poverty Level				
	(1)	(2)	(3)	(4)	(5)
Disregard, \$100s	-0.062 (0.052)	-0.042* (0.018)	-0.044** (0.015)	-0.039* (0.017)	-0.041** (0.014)
State has partial certification			0.016 (0.031)		0.017 (0.029)
Years state has had new hire directory			-0.001 (0.007)		-0.001 (0.007)
State median income for family of 4, \$1000s			0.006 (0.004)		0.007 (0.004)
State unemployment rate			0.001 (0.014)		0.002 (0.013)
State expenditures per child support case			0.230 (0.126)		0.210 (0.121)
Est. welfare benefit (3-person family), \$100s			0.044* (0.021)		0.035 (0.020)
Est. welfare tax rate on earned income			0.006 (0.058)		0.005 (0.061)
Family received public assistance				0.071** (0.024)	0.070** (0.024)
Child's age				0.034* (0.016)	0.031 (0.017)
Child's age squared/100				-0.024 (0.027)	-0.024 (0.028)
Mother's age				-0.023* (0.010)	-0.024* (0.011)
Mother's age squared/100				0.019 (0.010)	0.020 (0.011)
Mother has < high school education				0.069 (0.044)	0.065 (0.044)
Mother has some postsecondary education				0.002 (0.032)	0.004 (0.032)
Mother has 4-year college degree				0.181* (0.070)	0.180* (0.069)
Mother is currently married				-0.021 (0.039)	-0.018 (0.039)
Mother is divorced				-0.018 (0.031)	-0.016 (0.031)
Number of children < 18 in household				-0.007 (0.008)	-0.007 (0.008)
State and Year Fixed Effects	No	Yes	Yes	Yes	Yes
R-Square	0.004	0.124	0.126	0.134	0.136
N	15239	15239	15239	14936	14936

Regressions include data from 1993, 1994, and 1997-2001 and are unweighted. Standard errors clustered by state are in parentheses. * significant at 5%; ** significant at 1%

4.6.1 Sensitivity Tests

I conduct several sensitivity tests of the robustness of the results for in-kind support receipt. First, I weight the sample using the SPD child longitudinal weights. Since the SPD sampling scheme changes over waves, weighting to adjust for sample selection factors may be important. Weighting may also partially address problems with differential attrition in the child support sample. As discussed above, there are high exit rates within the child support sample between waves. I regress child characteristics on indicators for whether the child exits the child support sample and find that children without a legal support agreement, children of less educated parents, children of never-married mothers, and children in more families are more likely to exit the child support sample. However, the weighted and unweighted regression results are similar for both outcomes; the weighted regression for in-kind support is presented in the first column of Table 4.9.

Another potential issue is that I run the regressions in Tables 4.7 and 4.8 on the sample of all children. Since many mothers have more than one child with an absent father, whether one child in a family has a child support order is related to whether another child has a support order. In addition, even for mothers who have children by multiple absent fathers, these mothers may become more skilled at pursuing orders once they have done it successfully for one child, or the child support agency may pursue orders for all of a mother's former partners. Consequently, the likelihood of having a legal child support order or receiving in-kind support is not independent for children with the same absent father. Clustering the standard errors by the absent father would address this problem; however, the absent father is unobservable. I do not know which children in the data have the same absent father, particularly for children with no support agreements. Simply clustering the standard errors at the household level would also fix the problem, but since the variation in the disregard variable is at the state-year level, I am already clustering the standard errors by state-year.

Instead, I restrict the sample to one child per household. I select the child who appears in the largest number of waves. These results are shown in the second column of Table 4.9 and are similar to the base results in both magnitude and significance, suggesting that including multiple children with the same father is not affecting the results in a meaningful way.

A potential problem with the in-kind support regressions is that in-kind support is measured differently before and after welfare reform. In the Survey of Income and Program Participation, mothers are asked whether the father provided any non-cash

Table 4.9: Sensitivity Tests: Effect of Disregard on Receipt of In-Kind Support

	Weighted	One Kid per HH	No Gifts	Pooled Probit	High Income
Disregard, \$100s	-0.064** (0.017)	-0.043** (0.014)	-0.032** (0.009)	-0.034* (0.014)	-0.016 (0.045)
State has partial certification	0.003 (0.065)	0.010 (0.030)	0.040 (0.025)	-0.006 (0.024)	-0.056 (0.044)
Years state has had new hire directory	0.004 (0.011)	0.002 (0.007)	0.002 (0.006)	0.000 (0.006)	0.008 (0.014)
State median income, \$1000s	0.010 (0.007)	0.008 (0.004)	0.007 (0.004)	0.008** (0.003)	0.009 (0.007)
State unemployment rate	0.010 (0.022)	0.004 (0.014)	-0.002 (0.012)	-0.002 (0.010)	-0.020 (0.023)
State expenditures per CS case	0.197 (0.210)	0.230 (0.129)	0.102 (0.103)	-0.026 (0.137)	-0.076 (0.302)
Est. welfare benefit, \$100s	0.026 (0.041)	0.028 (0.022)	0.037 (0.024)	0.033 (0.019)	0.017 (0.023)
Est. welfare tax on earned income	0.025 (0.099)	-0.010 (0.065)	0.002 (0.055)	-0.051 (0.052)	0.075 (0.123)
Family received public assistance	0.071* (0.029)	0.078** (0.026)	0.068** (0.024)	-0.06** (0.016)	0.014 (0.092)
Child's age	-0.000 (0.025)	0.033 (0.017)	0.020 (0.019)	-0.009** (0.003)	0.009 (0.044)
Child's age squared/100	-0.030 (0.035)	-0.035 (0.031)	-0.023 (0.028)	0.007 (0.015)	0.002 (0.059)
Mother's age	-0.022 (0.018)	-0.022 (0.012)	-0.028* (0.011)	0.009 (0.005)	0.008 (0.027)
Mother's age squared/100	0.020 (0.019)	0.020 (0.013)	0.022 (0.011)	-0.010 (0.006)	-0.015 (0.032)
Mother has < high school education	0.124 (0.071)	0.054 (0.047)	0.065 (0.041)	-0.036* (0.015)	0.319** (0.091)
Mother has some postsecondary ed.	-0.015 (0.038)	0.008 (0.034)	-0.010 (0.024)	0.005 (0.012)	-0.000 (0.066)
Mother has 4-year college degree	0.173 (0.091)	0.181* (0.074)	0.139* (0.063)	0.076** (0.020)	-0.056 (0.115)
Mother is currently married	-0.021 (0.047)	-0.027 (0.036)	-0.016 (0.042)	0.018 (0.014)	-0.047 (0.047)
Mother is divorced	-0.004 (0.032)	-0.032 (0.032)	-0.013 (0.027)	0.039** (0.015)	-0.071 (0.042)
Number of children < 18 in household	-0.002 (0.011)	-0.006 (0.007)	-0.006 (0.007)	-0.011* (0.005)	0.031 (0.022)
Child is male				-0.001 (0.007)	
Child is black				-0.051** (0.014)	
Child is hispanic				-0.079** (0.017)	
R-Square or pseudo R-square	0.111	0.141	0.084	0.138	0.233
N	6479	12013	14938	14936	5471

Regressions include data from 1993, 1994, and 1997-2001 and include state and year effects. Standard errors clustered by state are in parentheses. Probit results are mean marginal effects with standard errors calculated using the delta method. * significant at 5%; ** significant at 1%

items or services for child-support purposes. Approximately 13% and 16% of mothers reported receiving non-cash support in 1993 and 1994 respectively. The Survey of Program Dynamics asked whether the noncustodial parent provided any of the following types of in-kind support: paid for insurance or medicine, paid for housing, provided clothes or food, paid for child care, school, or camp, or gave the child birthday or holiday gifts. These more detailed questions elicited higher reports of in-kind support. The different question format did change the average response, but so long as the new question format did not differentially affect parents in states that eliminated disregard, the fixed-effects regression response will still be valid. Approximately half of children received at least one of these kinds support in 1999, 2000, and 2001. Since mothers may think of birthday or holiday gifts differently from non-cash support, I experiment with excluding them from the definition of in-kind support.¹⁹ Redefining in-kind support so that it does not include gifts does not change the results, as shown in the fourth column of Table 4.9.

I also experiment with the econometric specification by running the regression as a pooled cross-section regression instead of a fixed effects regression and by using a probit model instead of OLS.²⁰ Neither alteration changes the results. The third column of Table 4.9 shows mean marginal effects from a probit regression on pooled cross-section data. These results are also similar to those in Table 4.8, suggesting that the effect of the disregard on the likelihood of providing in-kind support does not differ greatly across children and parents with different unobserved time-invariant characteristics.

Last, since the disregard only applies to welfare families, I can test whether the disregard appears to affect families with income more than 200% of the poverty line in all waves as a false experiment. We should expect no effect for these families because their incomes. Column 5 of Table 4.9 shows this is the case: the magnitude of the coefficient on the disregard measure is much smaller and is not significantly different from zero. This is reassuring: the disregard policy appears to affect in-kind support receipt rates for families who are at risk of being on welfare, but not for families whose income is well above the welfare threshold. Finding no result in the false experiment provides some reassurance that the main results are not spurious.

¹⁹Not including gifts as a type of in-kind support reduces the percentages to between 40 and 42 percent. The fixed effect regression results are not sensitive to whether gifts are included as in-kind support.

²⁰Attempts to estimate the effect of the disregard on in-kind support using a conditional logit model did not converge.

4.7 Multinomial Choice Regressions: Welfare and Support

In addition to whether or not children have legal support orders and receive in-kind support, I examine the mix of welfare, formal child support, and in-kind support custodial mothers receive using a multinomial logit framework. Custodial parents with low incomes must decide whether and how to receive child support and whether to go on welfare. Welfare receipt requires compliance with the child support enforcement agency: welfare benefits will be discontinued if the mother does not cooperate with the child support enforcement agency by reporting who the father is and any information she has about his location or employment status. If the mother receives welfare and the father pays formal support, the mother will receive at most \$50/month in child support, while the father's income will be reduced. The mother receives the most income if she receives welfare and informal support, but both she and the father are at risk of penalty if they are caught. The mother has more control over use of the money if the father pays monetary support rather than in-kind support.

Following Sorensen and Hill (2004), I use a multinomial logit model to estimate the probability of receiving each of four combinations of welfare and child support: welfare only, welfare and child support, child support only, and neither. In addition, I use the multinomial logit model to estimate the types of child support mothers receive: no support, both formal support and in-kind support, formal support only, or in-kind support only. For these regressions, I use mothers (rather than children) as the unit of observation because the mother decides whether or not to pursue welfare.

Table 4.10 shows relative risk ratios for the multinomial logit regressions. In contrast to Sorensen and Hill (2004), I detect no statistically significant difference in child support and welfare receipt by disregard level. One possibility is that sample sizes in the SPD are simply too small to detect a significant effect: Sorensen and Hill's Current Population Survey pooled cross-section yielded samples of nearly 72,000 previously married mothers and 37,000 never-married mothers. It is also possible that, consistent with much of the literature on disregard, any effects are small. This argument is more convincing for why I do not detect significant effects for whether support is paid via in-kind support, formal support, or both, since the incentives have changed relatively little for noncustodial fathers. Since the change in net income for mothers is larger and since Sorensen and Hill find large effects, it is somewhat surprising that the effects on welfare and child support are small.

Since coefficients and relative risk ratios for multinomial logit are difficult to in-

Table 4.10: Multinomial Logit Regressions: Welfare and Child Support and Formal and In-Kind Support

	Base category: welfare only			Base category: formal support only		
	Child support and welfare	Child support only	Neither	Formal and in-kind support	In-kind support only	Neither
Disregard, \$100s	0.120 (0.153)	-0.091 (0.106)	-0.002 (0.095)	-0.182 (0.132)	-0.151 (0.125)	0.082 (0.104)
State has partial certification	0.395** (0.181)	0.317** (0.126)	0.402*** (0.119)	0.481*** (0.176)	0.200 (0.185)	0.290* (0.149)
Years state has had new hire directory	0.005 (0.050)	0.013 (0.035)	-0.004 (0.033)	-0.065 (0.042)	-0.002 (0.043)	-0.041 (0.033)
State median income for family of 4, \$1000s	-0.002 (0.002)	0.012*** (0.001)	0.012*** (0.001)	0.012*** (0.002)	0.008*** (0.002)	0.002 (0.001)
State unemployment rate	-0.044 (0.085)	0.069 (0.061)	0.095 (0.058)	-0.203*** (0.068)	-0.148** (0.072)	-0.007 (0.056)
State expenditures per child support case	-0.222 (1.278)	-0.886 (0.832)	-1.180 (0.786)	-0.415 (1.022)	-1.190 (1.042)	-0.436 (0.757)
Est. welfare benefit (3-person family), \$100s	-0.094 (0.254)	0.428** (0.168)	0.189 (0.158)	0.309 (0.194)	-0.086 (0.206)	-0.180 (0.172)
Est. welfare tax rate on earned income	0.250 (0.575)	0.037 (0.393)	-0.093 (0.373)	-0.850** (0.428)	-0.611 (0.446)	-0.821** (0.347)
Constant	-1.761 (1.690)	-10.670*** (1.157)	-5.160*** (1.052)	-4.394*** (1.330)	1.519 (1.328)	4.350*** (1.083)
Log pseudo-likelihood	-1.18e+04					
N	11919					

Sample restricted to women under 200% of the poverty level at some time during the survey. Welfare/child support regressions include data from 1993, 1994, and 1997-2001. In-kind/formal support regressions include data from 1993, 1994, and 1999-2001. Both sets of regressions are unweighted. Additional demographic controls are included. Standard errors clustered by state and year are in parentheses. * significant at 5%; ** significant at 1%

Table 4.11: Probability of Receiving Welfare and Formal and In-Kind Support

	Probability (\$50 Disregard)	Δ in probability (Increase in disregard from 0 to \$100)
Welfare and child support	0.247	0.028
Welfare only	0.288	-0.002
Child support only	0.233	-0.023
Neither	0.233	-0.002

	Probability (\$50 Disregard)	Δ in probability (Increase in disregard from 0 to \$100)
Formal and in-kind support	0.070	-0.014
Formal support only	0.347	-0.005
In-kind support only	0.086	-0.014
Neither	0.497	0.033

Probability changes calculated for a white, 34-year old never-married mother living in Michigan. Other variables are set to median values.

terpret, I calculate the effect of a change in the disregard from 0 to \$100 on the probability of being in each of the outcome categories for a representative custodial mother. Table 4.11 shows the change in probability of each category for a 34-year-old never-married white mother living in Michigan if the disregard increases from 0 to \$100 per month. (All other variables were set to their median values.) Although the regression coefficients on the disregard measure are not significant and the probability changes are small in magnitude, the effects appear to move in plausible directions. When the disregard is higher, parents are more likely to receive both welfare and child support and less likely to receive only child support. However, the effects are small and not significant: overall, the multinomial logit regressions do not reveal an effect of the disregard on child support payment or welfare receipt.

4.8 Conclusions

Noncustodial parents of child welfare recipients face extremely high effective tax rates on child support payments. Consequently, they must choose whether to cooperate with the formal child support system and not contribute directly to their children, or to evade child support enforcement and pay informal support. Custodial parents must decide whether to pursue formal child support orders or to settle for informal

agreements. Custodial parents must also reevaluate the decision to receive welfare.

I find small effects of the disregard on the probability that fathers provide in-kind support to their children, indicating that a \$100 decrease in the disregard corresponds to a 4.1 percentage point increase in the proportion of children who receive in-kind support. I do not find significant effects on the fraction of children with a legal child support order. Because both the pre- and post-reform average tax rates on child support were so high, the small magnitude of the effects are not surprising: many fathers who are willing to pay informal support were probably doing so before welfare reform. Nevertheless, the results are consistent with fathers substituting toward informal support in response to higher taxes on formal support although it is difficult to measure the degree of substitution without knowing the value of the informal support or in-kind support that fathers provide. Small effects of a \$100 disregard do not mean that changes in disregard policy will not affect behavior. It is possible that larger changes in the disregard, corresponding to larger changes in the fraction of formal support passed on to welfare parents, would have stronger incentive effects.

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Chapter 5

Conclusion

The chapters in this dissertation explore how men with children from prior relationships decide how much contact to have with their children, how to divide their time between leisure and work in the regular and informal sectors in response to earned income tax credit changes, and how to substitute between formal and informal child support payments. My focus on men's behavior is relatively unusual, particularly in regard to responses to the earned income tax credit. The chapters also have methodological methods in common. Each uses state policy variation, either across states or over time, to identify the effects of policies on men's responses.

In Chapter 1, I use state variation in the timing of child support withholding laws to instrument for whether child support payments are withheld from the father's pay. Among fathers who do not owe back support, withholding decreases the frequency with which they spend time with their children. This indicates that payment method (not only payment amount or payment timing) has implications for behavior.

Chapter 2 analyzes the impact of state earned income tax credits (EITCs), which award federal EITC recipients an additional percentage of their federal credit, on men's participation in regular and informal work. If the state credit

I examine the impact of welfare reform changes that ended the practice of disregarding \$50 of child support per month when calculating welfare benefits in Chapter 3. I find that a \$100 decrease in the child support disregard increases the likelihood that children will receive in-kind support by 4.1 percent, indicating that men substitute from formal to informal support when formal child support payments are subject to a higher effective tax rate.

Overall, the results confirm that men respond to state policies partly by changing the extent to which they evade the policies. Many evasion responses are difficult to

observe in data. This dissertation provides insights into the pervasiveness and extent of men's evasion responses to tax and child support enforcement.