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Evidence from Quantile Panel Data Estimation

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Did the Economic Stimulus Payments of 2008 Reduce Labor Supply? Evidence from Quantile Panel Data Estimation*

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Abstract

While the literature has found evidence that tax rebates and economic stimulus payments increase short-term consumer spending, the literature has ignored the possibility that household labor supply may also respond. I exploit the randomized timing of receipt of the 2008 economic stimulus payments and examine changes in household labor earnings by month in the Survey of Income and Program Participation. Because it is unlikely that the effect is uniform throughout the earnings distribution, I estimate quantile treatment effects. My empirical strategy requires conditioning on household fixed effects so I introduce a new instrumental variables quantile regression technique for panel data (QRPD) which maintains the nonseparable disturbance term commonly associated with quantile estimation. This property is crucial to estimating the parameters of interest and distinguishes itself from many of the quantile panel data estimators in the literature which rely on additive fixed effects. I find that tax rebate receipt has significant impacts on labor supply.

Keywords: economic stimulus payments, tax rebates, labor supply, fixed effects, panel data, quantile regression, nonseparable disturbance

JEL classification: C31, C33, D91, E24, E62, H31, J22

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1 Introduction

Recessions often motivate policymakers to use fiscal policy, such as reductions in tax liability, to increase household spending and aggregate demand. While it is typically difficult to isolate the impact of such policies, recent work has used the randomized timing of tax rebate receipt to show that household expenditures respond to receipt of anticipated tax changes. Johnson et al. (2006) exploit the tax rebates of 2001 and find that timing of rebate receipt significantly predicts non-durable good expenditures. Parker et al. (2013) estimate similar effects for the 2008 economic stimulus payments (ESPs).[1] While the literature provides convincing evidence that tax rebates stimulate spending, there is also the economically important possibility that tax rebate receipt could encourage households to work less by "purchasing" additional leisure, substituting the rebate for labor earnings. The literature has highlighted the possible effects of fiscal policy on consumer spending and aggregate demand, but has frequently ignored the possible unintended consequences that such policies may have on labor supply decisions and, by extension, aggregate supply. The usefulness of such fiscal policy depends not only on its effects on consumer spending (and subsequent multiplier effects) but also its possible ramifications on production and household earnings.

This paper studies the effects of the 2008 economic stimulus payments on household labor earnings using monthly data in the Survey of Income and Program Participation (SIPP). The Economic Stimulus Act (ESA) of 2008 was historically large and projected to increase the deficit by $152 billion in 2008 with the majority of this amount due to the stimulus payments.[2] The ESA sent economic stimulus payments to approximately 130 million tax filers in the United States. The payments were primarily issued between April and July with variation resulting from the last 2-digits of the head-of-household's Social Security number. I compare labor earnings of households which received their payments in different months while leveraging the panel nature of the SIPP to condition on household fixed effects. I build on the empirical strategy of Johnson et al. (2006) and Parker et al. (2013) to estimate a causal relationship between payment receipt and household behavior, with a new focus on labor earnings as the outcome of interest. Because it is likely that the stimulus payments had differential effects throughout the earnings distribution, I estimate quantile treatment effects (QTEs). Estimating the mean effect can obscure important distributional

[1] While the 2008 payments were technically advances on tax liability reductions for the 2008 tax year, these payments are frequently referred to as "tax rebates" and I will refer to them in the same manner.

[2] http://www.cbo.gov/sites/default/files/cbofiles/ftpdocs/89xx/doc8973/hr5140pgo.pdf accessed on January 22, 2014.

effects. Furthermore, given that labor earnings are "censored" at 0 and topcoded in the data, mean estimates are potentially biased while quantile estimation can still obtain consistent estimates at uncensored points of the distribution.

Many empirical applications use quantile regression analysis when the variables of interest potentially have varying effects at different points in the conditional distribution of the outcome variable. These heterogenous effects have proven to provide useful information missed by mean regression techniques (Bitler et al. (2006)). Quantile estimation, such as quantile regression (QR) introduced by Koenker and Bassett (1978), allows for the impact of the covariates to vary with a nonseparable disturbance term. With the popularity of both fixed effect and quantile regression models, there has been a growing literature at the intersection of these two methods. Most existing quantile panel data techniques include additive fixed effects. This term alters the interpretation of the parameters of interest by separating the disturbance term into different components and assuming that the parameters do not vary based on the fixed effect. In this paper, I introduce an estimator which uses within-group variation for identification purposes, but maintains the nonseparable disturbance property which typically motivates use of quantile estimation. The resulting estimates can be interpreted in the same manner as cross-sectional quantile estimates. The fixed effects are never estimated and the coefficient estimates are consistent for small T. The estimation technique is straightforward to implement with standard statistical software.[3] I develop the estimator in an instrumental variables (IV) framework which is also necessary given the empirical strategy.

I find that receipt of a stimulus payment reduced labor earnings throughout the earnings distribution with the strongest evidence in the middle of the distribution. For each additional dollar received, household labor earnings were reduced by around 10 to 15 cents in the month of and after receipt for a large part of the distribution. I find evidence of significant but smaller lagged effects in the subsequent two months. These labor supply reductions cannot be explained through extensive margin changes. Households with hourly workers appear to be a main driving force behind the results. Both single and married women are especially responsive while significant responses are also estimated for married men. I find supportive evidence using the SIPP's more detailed information about employment status that rebate receipt is positively associated with unpaid absences from work. The results suggest that the payments had significant intensive labor supply effects as households used

[3]Code is available at http://www.rand.org/economics/resources.html.

the payments to partially substitute for labor earnings.

In the next section, I provide more information about the ESA and discuss the literature on tax rebates and economic stimulus payments. In Section 3, I provide background on quantile panel data estimation and introduce the estimator for this paper. Section 4 describes the data and empirical strategy with results following in Section 5. I conclude in Section 6.

2 Background

2.1 Related Literature

A small literature has noted that the receipt of tax rebates or stimulus payments have profound behavioral effects that are difficult to reconcile with the Permanent Income Hypothesis. Poterba (1988) studies the 1975 tax rebates and finds that an additional dollar in tax rebates increases short-term spending by 20 cents. More recently, Johnson et al. (2006) and Parker et al. (2013) both find increases in consumer spending on non-durable goods during the quarter of rebate receipt using the 2001 tax rebates and 2008 economic stimulus payments, respectively. These papers use spending and rebate receipt data from the Consumer Expenditure Survey. Johnson et al. (2006) concludes that 20-40 percent of rebates were spent on nondurable goods in the quarter of receipt with additional spending in the subsequent quarter. Parker et al. (2013) finds effects on both the purchase of nondurable and durable goods. Using a similar empirical strategy, Agarwal et al. (2007) study credit card accounts and find that consumers, on average, initially saved the 2001 tax rebates but later increased their spending. Liquidity constrained consumers were most responsive to tax rebate receipt.

A related literature has used survey evidence to ask households how they will use their rebates. The options for respondents in these surveys are "mostly spend," "mostly save," and "mostly pay off debt." Shapiro and Slemrod (2003a) and Shapiro and Slemrod (2003b) find that most households plan to pay off debt with a small fraction reporting that they plan to increase spending in response to the 2001 tax rebates. Shapiro and Slemrod (2009) reports similar evidence for the 2008 stimulus payments with about 20% responding that they will "mostly spend" their rebate. This paper concludes that the marginal propensity to consume out of tax rebates is about one-third.

While the interest in understanding the potential for stimulus payments to increase consumer spending is well-motivated, it is surprising that the literature has ignored the possibility that individuals could "purchase" leisure and reduce their household earnings in response to tax rebate receipt. This possible outcome is policy-relevant given that stimulus payments may have the unintended result of reducing household labor supply. Furthermore, the scope for increasing aggregate demand is muted when households reduce their earnings upon receipt of a rebate.

In the same spirit as the above literature, I estimate the effect of differences in rebate timing. This method does not allow me to identify general equilibrium effects of the stimulus payments. On net, the ESA may have increased labor supply even if households work less immediately after rebate receipt. Understanding this partial equilibrium effect still informs the role of tax rebates on labor supply decisions.

2.2 Economic Stimulus Payments of 2008

The Economic Stimulus Act of 2008 was signed by President George W. Bush on February 13, 2008 due to concerns about an impending recession. The bill was projected to increase the deficit by $152 billion with approximately $100 billion marked for economic stimulus payments to be sent directly to approximately 130 million tax filers. Households were eligible for ESPs if they had filed a 2007 tax return and had at least $3,000 in qualifying income. The payment was equal to the taxpayer's tax liability up to $600 for a single person and $1,200 for a married couple filing jointly. Households with low income received a minimum payment of $300 for singles or $600 for couples. An additional $300 payment was made for each qualifying dependent in the household. The total payments were phased out at a rate of 5% for income above $75,000 for single individuals and $150,000 for married couples.

Rebate eligibility and amounts were calculated using 2007 tax returns, though the rebate was technically a reduction in taxes owed for 2008. Households which received larger rebates than they should have using 2008 data were not required to repay the additional amount that they received.

The Treasury Department began making economic stimulus payments to households on April 28. The payments were staggered over time. The Treasury sent payments electronically and by mail, depending on whether the tax filer provided the IRS with a bank routing number in their 2007 tax return. In my data, 45% of respondents report receiving their rebate electronically. Payments were scheduled over a two month time period and varied by the last 2-digits of the tax filer's Social Security number. Households that filed their tax returns late potentially received their ESPs late as well. The rebate schedule is presented in Table 1.

Table 1: Timing of Economic Stimulus Payments

Last 2 Digits of SSN	Electronic Transfer Made By	Check in the Mail By
00-20	May 2	
21-75	May 9	
76-99	May 16	
00-09		May 16
10-18		May 23
19-25		May 30
26-38		June 6
39-51		June 13
52-63		June 20
64-75		June 27
76-87		July 4
88-99		July 11

The literature uses the differential timing of rebate receipt to identify effects of rebate receipt on economic behavior. The randomized nature of the timing differences is especially beneficial for identification, and this paper will adopt an empirical strategy similar to the ones found in Johnson et al. (2006) and Parker et al. (2013). While households received their payments at different times, the range of times is not especially large and requires analyzing outcomes at high frequency. Johnson et al. (2006) and Parker et al. (2013) use quarterly expenditure data and model changes in spending as a function of rebates received at any time in that quarter. I will use monthly labor earnings data, which should allow me to capture behavioral changes resulting from rebate receipt.

3 Quantile Estimation with Panel Data

This paper pursues estimates of quantile treatment effects. The empirical strategy requires conditioning on household fixed effects for identification purposes, but the inclusion of fixed effects poses a special challenge in the context of quantile estimation. A primary motivation of quantile estimation is that it allows the parameters to vary based on a nonseparable disturbance term. Including an additive fixed effect partially undermines this intent as it separates the disturbance term into different components and the parameters are constrained to vary based only on the non-fixed part of the disturbance term. In the next section, I discuss the literature on quantile panel data estimators further, followed by the introduction of an instrumental variables quantile estimator for panel data (QRPD) that preserves the nonseparable disturbance property while conditioning on fixed effects.

3.1 Background

Quantile estimation techniques are typically used to estimate the the distribution of the outcome, represented by Y_{it}, for a given set of treatment variables, represented by D_{it}.[4] In mean regression, panel data allow for the inclusion of fixed effects to identify off of within-group variation. Many quantile panel data estimators use an analogous method and include *additive* fixed effects. However, the additive fixed effects change the underlying model. The existing literature on quantile estimation with fixed effects is primarily concerned with the difficulties in estimating large number of fixed effects in a quantile framework and considering incidental parameters problems when T is small.[5] The QRPD estimator introduced in this paper avoids these concerns since the fixed effects are never estimated and the estimates are consistent even for $T = 2$. The primary motivation for QRPD is conceptual so I discuss the existing quantile panel data estimators in this spirit.

While existing quantile panel data methods frequently focus extensively on estimation of the fixed effects (α_i), let us assume that α_i is instead known. Quantile estimators with additive fixed effects provide estimates of the distribution of $(Y_{it} - \alpha_i)|D_{it}$ instead of estimating the distribution of $Y_{it}|D_{it}$. In many empirical applications, this may be undesirable. Observations at the top of the $(Y_{it} - \alpha_i)$ distribution may be at the bottom of the Y_{it} distribution.

[4] I will use capital letters to designate random variables and lower case letters to denote potential values that those random variables may take.

[5] Graham et al. (2009) show that there is no incidental parameters problem in a quantile model with additive fixed effects when there are no heterogenous effects (i.e., the effect is constant throughout the distribution). It is not clear that their argument could extend generally to the case of heterogenous effects. Ponomareva (2011) introduces an additive effects estimator that is consistent for small T.

I model outcomes as

$$Y_{it} = D_{it}'\beta(U_{it}^*), \quad U_{it}^* \sim U(0,1), \tag{1}$$

where $D_{it}'\beta(\tau)$ is strictly increasing in τ. I use a linear-in-parameters framework due to its popularity in applied work and relative ease in implementing. U_{it}^* represents labor market ability or proneness for the outcome (Doksum (1974)). It is a rank variable and the assumption of a uniform distribution is a normalization. For comparisons with other quantile estimators, let $U_{it}^* = f(\alpha_i, U_{it})$ where $U_{it} \sim U(0,1)$. In words, proneness for the outcome is an unknown function of both an individual fixed effect[6] and an observation-specific disturbance term. I will place no structure on the function $f(\cdot)$ or the relationship between α_i and U_{it}.

The quantile treatments effects (QTEs) represent the causal effect of a change of the treatment variables from d_1 to d_2 on Y_{it}, holding τ fixed:

$$d_2'\beta(\tau) - d_1'\beta(\tau). \tag{2}$$

I introduce a quantile regression for panel data (QRPD) estimator which estimates QTEs for the outcome variables Y_{it}. To adopt similar terminology as Chernozhukov and Hansen (2008), the structural quantile function (SQF) of interest for equation (1) is

$$S_Y(\tau|d) = d'\beta(\tau), \quad \tau \in (0,1). \tag{3}$$

The SQF defines the quantile of the latent outcome variable $Y_d = d'\beta(U^*)$ for a fixed d and a randomly-selected $U^* \sim U(0,1)$. In other words, it describes the τ^{th} quantile of Y for a given d. Estimation of the SQF is possible using QR when U^* and D are, in fact, independent. QRPD will relax the independence assumption necessary to estimate the SQF. QRPD is useful when U^* and D are not independent, but we are still interested in the outcome distribution if policy imposed d on the population (i.e., the outcome distribution for a given d that we would observe if d did not provide information about U^*). QR relies on the conditional restriction

$$P\left(Y_{it} \leq D_{it}'\beta(\tau)|D_{it}\right) = \tau. \tag{4}$$

[6] I refer to the fixed effects as "individual fixed effects" and assume that the data have multiple observations for each individual. The estimator is also applicable in other contexts, such as repeated cross-sections where fixed effects are based on cells.

This condition states that the probability the outcome variable is smaller than the quantile function is the same for all D_{it} and equal to τ. In this paper, I introduce QRPD which allows this probability to vary by individual and even within-individual as long as such variation is orthogonal to the instruments. Since we observe the same person multiple times in panel data, we can use this additional information to learn that the probability that a person has a low value of the outcome variable given their treatment variables may not be τ. Instead, QRPD relies on a conditional restriction and an unconditional restriction (letting $D_i = (D_{i1}, \cdots, D_{iT})$):

$$P\left(Y_{it} \leq D'_{it}\beta(\tau)|D_i\right) = P\left(Y_{is} \leq D'_{is}\beta(\tau)|D_i\right), \tag{5}$$

$$P\left(Y_{it} \leq D'_{it}\beta(\tau)\right) = \tau. \tag{6}$$

Instead of assuming that $P\left(Y_{it} \leq D'_{it}\beta(\tau)|D_i\right) = \tau$ for all D_i, the QRPD estimator allows this probability to vary by person and the estimator only use within-person comparisons of this probability. Some people may be very "prone" to high monthly earnings while others are less prone. Equation (6) ensures that, on average, the probability that the outcome is less than the quantile function is equal to τ though, again, the estimator allows for heterogeneity across individuals. This framework relaxes the assumptions of QR while still estimating the SQF of interest (equation (3)), maintaining the nonseparable disturbance term, and allowing the parameters of interest to vary based on U^*_{it}.

A growing literature has developed quantile panel data estimators with additive fixed effects, including Koenker (2004), Harding and Lamarche (2009), Canay (2011), Galvao Jr. (2011), Ponomareva (2011), Rosen (2012), and Lamarche (2010). The QTEs in an additive fixed effects framework refer to changes in the distribution of $Y_{it} - \alpha_i$ for a fixed and estimated α_i. Note that observations with a large value of $Y_{it} - \alpha_i$ are potentially at the bottom of the Y_{it} distribution so these additive fixed effect models cannot be interpreted in same manner as cross-sectional quantile estimates. The disturbance term has been separated. In many applications, the motivation for using quantile regression is to allow the parameters of interest to vary based on the nonseparable disturbance term U^*_{it}. Separating α_i in these cases partially undermines this original motivation and there is frequently[7] little economic justification to only allow the parameters to vary based on part of the disturbance term and exclude the other part simply because it is fixed over time.

[7]It is possible that one might be interested in the distribution of the outcome variable given a fixed α_i and this may support using an additive fixed effect model. The framework used in this paper is not intended to nest additive fixed effect quantile frameworks.

The motivation for QRPD is that there are many cases when researchers are interested in estimating QTEs for the outcome variable Y_{it}, but they do not believe that they are identified cross-sectionally. Conditioning on individual fixed effects should be helpful in relaxing the exogeneity assumption but using an additive fixed effect quantile model changes the interpretation of the QTEs. QRPD allows the parameters to be interpreted in the same manner as cross-sectional quantile estimates.

Additive fixed effect models constrain one to alter the SQF: $S_Y(\tau|d,\alpha_i) = \alpha_i + d'\tilde{\beta}(\tilde{\tau})$, where $\tilde{\beta}(\tilde{\tau})$ is used to highlight that these parameters are different than those in equation (3). In this case, α_i is fixed and $U \sim U(0,1)$ is sampled independently of d. Put differently, even when the conditions for QR are met, the estimates resulting from QR and additive fixed effect quantile models are not comparable.

Table 2 should clarify the differences between the three types of available quantile estimators with panel data. First, I include pooled QR, which is simply the Koenker and Bassett (1978) estimator and does not include individual fixed effects. Second, the table models a quantile panel data estimator with additive fixed effects. Third, I include the QRPD estimator introduced in this paper. Note that the additive fixed effects change the SQF and the conditional outcome distribution that is being studied. QRPD relaxes the assumptions of QR. Instead of assuming that $U_{it}^*|D_{it} \sim U(0,1)$, $U_{it}^*|D_{it}$ is allowed to have an unknown distribution.

Table 2: Comparison of Estimators

	Pooled QR	Additive Fixed Effects	QRPD
Assumption	$U_{it}^*\|D_{it} \sim U(0,1)$	$U_{it}\|D_{it}, \alpha_i \sim U(0,1)$	$U_{it}^*\|D_i \sim U_{is}^*\|D_i$
Outcome Distribution	Y_{it}	$Y_{it} - \alpha_i$	Y_{it}
Structural Quantile Function	$d'\beta(\tau)$	$\alpha_i + d'\tilde{\beta}(\tilde{\tau})$	$d'\beta(\tau)$
Interpretation for τ^{th} quantile	τ^{th} quantile of U^*	τ^{th} quantile of U	τ^{th} quantile of U^*

Related to the motivation for QRPD, Chernozhukov et al. (2013) discuss identification of bounds on quantile effects in nonseparable panel models with exogenous variables. They show that these bounds tighten as T increases. Note that this paper maintains the nonseparable disturbance property when discussing quantile effects. The interpretation of QRPD parallels the interpretation of the bounds using the Chernozhukov et al. (2013) framework.

The QRPD estimator is, to my knowledge, the first quantile panel data estimator to provide point estimates which be interpreted in the same manner as cross-sectional regression results while allowing an arbitrary correlation between the fixed effects and the policy variables (or instruments). The main contribution is that the estimator allows researchers to condition on fixed effects for identification purposes but still maintain the nonseparable disturbance property of quantile regression models. In addition, the estimator has some other noteworthy features in the quantile panel data literature. It is one of the few quantile fixed effects estimator to provide consistent estimates for small T. It is also one of the few instrumental variables (IV) quantile panel data estimators.

A further advantage of this paper's estimator is that the moment conditions are simple to interpret and implement. Because the individual fixed effects are never estimated or even specified, the number of parameters that need to be estimated is small relative to most quantile panel data estimators and implementation of this estimator is simple compared to those found in the literature. I also use the properties of the moment conditions to reduce the number of parameters that need to be estimated even further, as any set of exogenous fixed effects which saturate the model (eg., year fixed effects) can be solved for given estimates for the other parameters. This simplification makes the estimator straightforward to implement using standard statistical software.

3.2 QRPD

I develop the estimator in an instrumental variables context given instruments $Z_i = (Z_{i1}, \cdots, Z_{iT})$. If the treatment variables are exogenous then $Z_i = D_i$ and many of the identification conditions (discussed later) are met trivially. The motivation of this estimator is that for situations where $U^*_{it}|Z_{it} \nsim U(0,1)$, instrumental variables quantile regression (IVQR, Chernozhukov and Hansen (2006), Chernozhukov and Hansen (2008)) cannot be used to estimate the SQF. The exogeneity assumption for QRPD is that the instruments do not provide information about within-individual *changes* in the disturbance term, U^*. This suggests using pairwise comparisons between observations for the same individual.

All conditions are assumed to hold jointly with probability one.

A1 *Potential Outcomes and Monotonicity:* $Y_{it} = D'_{it}\beta(U^*_{it})$ *where* $D'_{it}\beta(U^*_{it})$ *is increasing in* $U^*_{it} \sim U(0,1)$.

The first assumption (**A1**) is a standard monotonicity condition for quantile estimators (see Chernozhukov and Hansen (2006) for one example). $U^*_{it} \sim U(0,1)$ is simply a normalization. U^*_{it} may be a function of several unobserved disturbance terms and summarizes these terms into one rank variable. Alternatively, one can imagine using an unrestricted disturbance term ϵ^* in the equation of interest $Y_{it} = D'_{it}\beta(\epsilon^*_{it})$.[8] There exists a mapping of ϵ^* to U^*.

A2 *Conditional Independence:* $E\Big[\mathbf{1}(U^*_{it} \leq \tau) - \mathbf{1}(U^*_{is} \leq \tau)|Z_i\Big] = 0$ *for all* s, t.

A2 is a conditional independence assumption. **A2** can be replaced by an assumption of stationarity so that the distributions of $U^*_{it}|Z_i$ and $U^*_{is}|Z_i$ are the same. Instead, I use a slightly weaker assumption. The distribution of $U^*_{it}|Z_i$ can change over time, but this change must be uncorrelated with Z_i.

To aid intuition, let us return to using ϵ^*, which has an unspecified distribution, in the equation $Y_{it} = D'_{it}\beta(\epsilon^*_{it})$. Assumption **A2** puts no structure on the overall mean or variance of ϵ^*_{it} for any t. The assumptions allows $\epsilon^*_{it} = a_t + c_t \times \epsilon^*_{i1}$ (Chernozhukov et al. (2013) includes a similar assumption) for some time-varying constants a_t, c_t. **A2** simply requires Z_i to not be systematically related to changes in the distribution of ϵ^*_{it}.

Note that no restrictions have been placed on the relationship between U^*_{it} and α_i ($U^*_{it} = f(\alpha_i, U_{it})$). There are also no explicit restrictions on U_{it}, which distinguishes this paper's estimator from most of the quantile panel data literature. Furthermore, there are no assumptions on the relationship between α_i and Z_i, paralleling fixed effect mean regression models.

These assumptions lead to two separate moment conditions. Both conditions will be important for identification.

Theorem 3.1 (Moment Conditions)**.** *Suppose* ***A1*** *and* ***A2*** *hold. Then for each* $\tau \in (0, 1)$*,*

$$E\left\{\frac{1}{2T^2}\sum_t\sum_s (Z_{it} - Z_{is})\left[\mathbf{1}(Y_{it} \leq D'_{it}\beta(\tau)) - \mathbf{1}(Y_{is} \leq D'_{is}\beta(\tau))\right]\right\} = 0 \quad \textit{for all } s, t, \tag{7}$$

$$E[\mathbf{1}(Y_{it} \leq D'_{it}\beta(\tau)) - \tau] = 0. \tag{8}$$

Proofs are included in the Appendix. Equation (7) is a useful formulation since it shows that the estimator is simply a series of within-individual comparisons. However, it is also useful to consider equivalent conditions. Specifically, we can rearrange equation (7) and

[8] Where $\epsilon^*_{it} = \tilde{f}(\alpha_i, \epsilon_{it})$.

arrive at an equivalent condition:

$$E\left\{\frac{1}{T}\sum_{t=1}^{T} Z_{it}\left[\mathbf{1}(Y_{it} \leq D'_{it}\beta(\tau)) - \frac{1}{T}\sum_{s=1}^{T}\mathbf{1}(Y_{is} \leq D'_{is}\beta(\tau))\right]\right\} = 0.$$

Estimation details will be discussed below, but I include the corresponding sample moments here. There are two sets of sample moment conditions which I label Sample Moment 1 and Sample Moment 2.[9]

Sample Moment 1

$$g_i(b) = \frac{1}{T}\sum_{t=1}^{T} Z_{it}\left[\mathbf{1}(Y_{it} \leq D'_{it}b) - \frac{1}{T}\sum_{s=1}^{T}\mathbf{1}(Y_{is} \leq D'_{is}b)\right],$$

Sample Moment 2

$$h(b) = \frac{1}{NT}\sum_{i=1}^{N}\sum_{t=1}^{T}\mathbf{1}(Y_{it} \leq D'_{it}b) - \tau.$$

Sample Moment 1 offers some helpful insights. Define $\tau_i(b) \equiv \frac{1}{T}\sum_{s=1}^{T}\mathbf{1}(Y_{is} \leq D'_{is}b)$. Note that the moment condition is similar to the cross-sectional instrumental variable quantile moment condition where τ is replaced by τ_i:[10]

$$g_i(b) = \frac{1}{T}\sum_{t=1}^{T} Z_{it}\Big[\mathbf{1}(Y_{it} \leq D'_{it}b) - \tau_i(b)\Big].$$

This makes intuitive sense. The individual fixed effect provides information about the distribution of the disturbance. Thus, instead of assuming that $\tau_i = \tau$ for each individual, panel data allow us to relax this assumption. τ_i varies by individual with $E[\tau_i] = \tau$.

When discussing identification and other properties, it is easiest to use the following

[9] Sample Moment 1, in fact, includes L sample moments where L is the number of instruments.

[10] Note that there is no incidental parameters problem since consistent estimation of τ_i is not necessary. This is clear from the pairwise comparison version in equation (8).

equivalent formulation[11]

$$g_i(b) = \frac{1}{T}\left\{\sum_{t=1}^{T}\left(Z_{it} - \overline{Z}_i\right)\left[\mathbf{1}(Y_{it} \leq D'_{it}b)\right]\right\}, \tag{9}$$

where $\overline{Z}_i = \frac{1}{T}\sum_{t=1}^{T} Z_{it}$. Equation (9) should provide some useful insights too because it shows that though the quantile function itself is the same as one that would be estimated by IV-QR (if the IV-QR assumptions were met), identification is originating from *changes* in the treatment variables.

Sample Moment 2 relies on the fact that the unconditional distribution of U^* is $U(0,1)$. Notice that this sample moment also holds with traditional quantile estimators such as QR and IV-QR. With IV-QR, one assumes both that $U^* \sim U(0,1)$ *and* $U^*|Z \sim U(0,1)$. The QRPD estimator replaces the latter assumption with a weaker one. This is the gain from employing panel data.

3.3 Estimation

Estimation uses Generalized Method of Moments (GMM). Sample moments are defined by

$$\hat{g}(b) = \frac{1}{N}\sum_{i=1}^{N} g_i(b), \tag{10}$$

using equation (9). Sample Moment 2 is also necessary for identification. I use Sample Moment 2 to define the parameter set. This approach simplifies estimation. Define

$$\mathcal{B} \equiv \left\{ b \quad | \quad \frac{1}{NT}\sum_{i=1}^{N}\sum_{t=1}^{T}\mathbf{1}(Y_{it} \leq D'_{it}b) = \tau \right\}.$$

Then,

$$\widehat{\beta(\tau)} = \arg\min_{b\in\mathcal{B}} \hat{g}(b)'\hat{A}\hat{g}(b) \tag{11}$$

for some weighting matrix $\hat{A}$. $\hat{A}$ can simply be the identity matrix and two-step GMM estimation can be used.

[11]This condition is equivalent to Sample Moment 1 above through a straightforward rearrangement of terms.

There is a straightforward way to confine all guesses b to the set $\mathcal{B}$ when a constant is included in the SQF. In fact, I assume the inclusion of time fixed effects (or any set of dummy variables which saturate the space) since this is common with panel data estimation. Time fixed effects in the context of this estimator allow the entire distribution of the outcome variable to shift in each time period. For example, if this paper were studying the effects of stimulus payments on the 95^{th} percentile of labor earnings over the course of several years in which labor earnings rapidly grew, then time fixed effects would allow the estimates on the treatment variables to be interpreted as the effects on the 95^{th} percentile of labor earnings within a time period. Excluding time effects would imply that the 95^{th} percentile estimates primarily correspond to the outcome in the later part of the sample since that is when labor earnings are highest. This is undesirable in many applications. The time fixed effects allow the outcome distribution to shift over time.

The approach suggested in this paper reduces the number of parameters that need to be estimated. While the empirical strategy will be described in detail later, this insight reduces the number of parameters that need to be independently estimated from 112 to 2 in the main specification of this paper, making estimation possible through simple grid-searching techniques. A contribution of this paper is the introduction of a quantile panel data estimator that is straightforward to use and this reduction in the number of free parameters makes the estimator more practical.

The inclusion of time fixed effects implies

$$P(Y_{it} \leq D_{it}'\beta(\tau)) = P(Y_{is} \leq D_{is}'\beta(\tau)) \quad \text{for all } s, t.$$

Combined with equation (8), the implication is

$$P(Y_{it} \leq D_{it}'\beta(\tau)) = \tau \quad \text{for all } t. \tag{12}$$

This inclusion of separable time fixed effects is simply equivalent to assuming equation (12).[12] By assuming the inclusion of year fixed effects, I can use equation (12) for the sample moments. Let $D \equiv (X, \mathbf{1}(t = 1), \ldots, \mathbf{1}(t = T))$ where X are the policy variables of interest and $\mathbf{1}(t = s)$ is an indicator variable equal to 1 for time s. Let $\tilde{b}$ represent coefficients on X such that $D_{it}'b = \tilde{\gamma}_t + X_{it}'\tilde{b}$. Similarly, define $\tilde{Z}_{it}$ as all instruments excluding the year fixed effects. The sample moments can be replaced by

[12] Nonseparable time fixed effects may be useful in other applications.

Sample Moment 1'

$$g_i(b) = \frac{1}{T}\sum_{t=1}^{T} \tilde{Z}_{it}\left[\mathbf{1}(Y_{it} \le D'_{it}b) - \frac{1}{T}\sum_{s=1}^{T}\mathbf{1}(Y_{is} \le D'_{is}b)\right], \tag{13}$$

Sample Moment 2'

$$h_t(b) = \frac{1}{N}\sum_{i=1}^{N}\mathbf{1}(Y_{it} \le D'_{it}b) - \tau \qquad \text{for all } t. \tag{14}$$

Sample Moment 2' defines the time fixed effects. The value of these fixed effects forces $Y_{it} \le D'_{it}b$ to hold for $100\tau\%$ of the observations in each time period. The benefit of this approach is that it reduces the number of parameters that need to be estimated and offers a simple way to enforce the second sample moment by defining

$$\mathcal{B} \equiv \left\{ b \quad | \quad \frac{1}{N}\sum_{i=1}^{N}\mathbf{1}(Y_{it} \le D'_{it}b) = \tau \quad \text{for all } t \right\}. \tag{15}$$

Define $\gamma_t(\tau, \tilde{b})$ as the τ^{th} quantile of the distribution of $Y_{it} - X'_{it}\tilde{b}$ in year t:

$$\hat{\gamma}_t(\tau, \tilde{b}) \quad \text{solves} \quad \frac{1}{N}\sum_{i}^{N}\mathbf{1}(Y_{it} - X'_{it}\tilde{b} \le \hat{\gamma}_t(\tau, \tilde{b})) = \tau. \tag{16}$$

This equation forces $h_t(b) = 0$ to hold for all t, confining all guesses to $\mathcal{B}$.[13] In words, for any value $\tilde{b}$, the optimal values $\hat{\gamma}_t(\tau, \tilde{b})$ are known and easy to calculate. The time fixed effects are a function of the coefficients on X. This simplifies the estimation process. Alternatively, one could implement GMM with Sample Moments 1 and 2, simultaneously minimizing all sample moments. The proposed estimation strategy is a simplification but not required. The proposed estimation steps are the following. Define a grid of values for the parameters associated with the variables in X. For each value $\tilde{b}$:

1. Calculate the year fixed effects using equation (16).
2. Evaluate $\hat{g}(b)'\hat{A}\hat{g}(b)$ where $g_i(b)$ is defined by equation (13).

The b that minimizes this condition is $\widehat{\beta(\tau)}$. In many economic applications, it is typical to

[13]For any b, the time fixed effects are defined by equation (16) and, consequently, $\mathcal{B}$ is not empty.

have only one or two treatment variables (not counting the time fixed effects). In these cases, grid-searching is appropriate. Chernozhukov and Hansen (2006) make this same argument and recommend grid-searching. With more treatment variables, other optimization methods are necessary, such as Markov Chain Monte Carlo (see Chernozhukov and Hong (2003)). This reduction in the number of parameters that need to be estimated should be helpful regardless of the employed optimization procedure. Note that the discussion of inference below will account for this relationship between the time fixed effect estimates and $\tilde{b}$.

3.4 Properties

This section briefly discusses identification, consistency, asymptotic normality, and inference. These properties are discussed for small T as $N \to \infty$. This discussion will assume that $\mathcal{B}$ is defined by equation (15). I account for the proposed estimation strategy which considers the time fixed effects as functions of the coefficients on X. Let $||\cdot||$ be the Euclidean norm and $f_Y(\cdot)$ represent the pdf of Y_{it} conditional on Z_i.

3.4.1 Identification

This section discusses the uniqueness of $\beta(\tau)$. I continue to discuss the estimator in an IV context, though it should be noted here that some of the assumptions below may be met trivially for the $Z = D$ case.

I designate the number of policy variables by k and define Ψ as a matrix of possible values for D_{it},

$$\Psi \equiv \begin{bmatrix} d^{(1)\prime} \\ \vdots \\ d^{(M)\prime} \end{bmatrix}.$$

Note that Ψ may include all possible values of D_{it} such as when the treatment variables are discrete or may simply include a subset of possible values.[14] I also define a $T \times M$ matrix of the relationship between Z_i and the same $(d^{(1)}, \cdots, d^{(M)})$:

[14] In the application of this paper, many households received rebates that were multiples of \$300 given the rebate formula so there are discrete values with positive probabilities.

$$\Pi(Z_i) \equiv \begin{bmatrix} P(D_{i1} = d^{(1)}|Z_i) & \cdots & P(D_{i1} = d^{(M)}|Z_i) \\ \vdots & \ddots & \vdots \\ P(D_{iT} = d^{(1)}|Z_i) & \cdots & P(D_{iT} = d^{(M)}|Z_i) \end{bmatrix}$$

I add the following assumptions necessary for identification.

IV-A3 Full Rank and First Stage: There exists $(d^{(1)}, \cdots, d^{(M)})$ such that $E[Z_i\Pi(Z_i)]$ is rank M and Ψ is rank k.

IV-A4 Continuity: Y_{it} continuously distributed conditional on Z_i.

IV-A3 imbeds several assumptions. $E[Z_i\Pi(Z_i)]$ is an $L \times M$ matrix,[15] and the requirement that it be rank M assumes that the instruments have a rich relationship with the policy variables. However, it only specifies that this relationship need exist for a subset of values, not all possible values of the policy variables. This helps the identification discussion in the Appendix. A necessary condition is that the number of instruments be at least M, i.e. $L \geq M$. **IV-A3** also includes an assumption that the policy variables are not collinear to allow for separate identification of the parameters. **IV-A4** is an assumption commonly used with quantile estimators and will be necessary for identification to hold.

Theorem 3.2 (Identification)**.** *If (i)* ***IV-A1 - IV-A4*** *hold;*
(ii) $E\left\{\frac{1}{T}\sum_{t=1}^{T}\left(Z_{it} - \overline{Z}_i\right)\left[\mathbf{1}(Y_{it} \leq D_{it}'\breve{\beta})\right]\right\} = 0$*; (iii)* $E\left[\mathbf{1}(Y_{it} \leq D_{it}'\breve{\beta})\right] = \tau$*, then* $\breve{\beta} = \beta(\tau)$.

A discussion is included in the Appendix.

3.5 Consistency

The following assumptions will be needed for consistency and asymptotic normality. Let ϕ represent the true coefficients on the X variables:

IV-A5 (Y_i, D_i, Z_i) i.i.d.

IV-A6 $\mathcal{B}$ is compact.

IV-A7 $\sup_t E\left|\left|Z_{it} - \overline{Z}_i\right|\right| < \infty$, $\sup_t E\left|\left|Z_{it} - \overline{Z}_i\right|\right|^{2+\delta} < \infty$ for some $\delta > 0$.

[15] Z_i is $L \times T$.

IV-A8 $G \equiv E\left[\frac{1}{T}\sum_{t=1}^{T}(Z_{it}-\overline{Z}_i)\left(X'_{it}+\frac{\partial\gamma_t(\tau,\phi)}{\partial\phi'}\right)f_Y(D'_{it}\beta(\tau)|Z_i)\right]$ exists such that $G'AG$ nonsingular.

The formula for G accounts for the recommend estimation procedure which links the coefficients on X to the time fixed effects. The $\frac{\partial\gamma_t(\tau,\phi)}{\partial\phi'}$ term can be excluded if this procedure is not used. The other regularity conditions are standard.

Theorem 3.3 (Consistency)**.** *If* ***IV-A1*** *-* ***IV-A7*** *hold and* $\hat{A}\xrightarrow{p}A$ *positive definite, then* $\widehat{\beta(\tau)}\xrightarrow{p}\beta(\tau)$.

A discussion is included in the Appendix.

3.6 Asymptotic Normality

Newey and McFadden (1994) discuss asymptotic normality results for discontinuous moment conditions. Stochastic equicontinuity is an important condition for these results and follows here from the fact that the functional class $\left\{\mathbf{1}(Y_{it}\leq D'_{it}b), b\in\mathbb{R}^k\right\}$ is Donsker and the Donsker property is preserved when the class is multiplied by a bounded random variable. Thus,

$$\left\{\frac{1}{T}\sum_{t=1}^{T}\left(Z_{it}-\overline{Z}_i\right)\left[\mathbf{1}(Y_{it}\leq D'_{it}b)\right], b\in\mathbb{R}^k\right\}$$

is Donsker with square integrable envelope $2\max_{(i,t)}|Z_{it}-\overline{Z}_i|$. Stochastic equicontinuity follows from Theorem 1 in Andrews (1994). Define $\Sigma\equiv E[g_i(\beta(\tau))g_i(\beta(\tau))']$.

Theorem 3.4 (Asymptotic Normality)**.** *If* ***IV-A1*** *-* ***IV-A8*** *hold and* $\hat{A}\xrightarrow{p}A$ *positive definite, then* $\sqrt{N}(\widehat{\beta(\tau)}-\beta(\tau))\xrightarrow{d}N\left[0,(G'AG)^{-1}G'A\Sigma AG(G'AG)^{-1}\right]$.

The Appendix includes a more extensive discussion.

3.7 Inference

For standard error estimates, I adopt an approach similar to the histogram estimation technique suggesting in Powell (1986) to obtain consistent estimates of G:

$$\hat{G}=\frac{1}{2Nh}\sum_{i=1}^{N}\left[\frac{1}{T}\sum_{t=1}^{T}(Z_{it}-\overline{Z}_i)\left(X'_{it}+\frac{\partial\widehat{\gamma_t(\tau,\phi)}}{\partial\phi'}\right)\mathbf{1}\left(\left|Y_{it}-D'_{it}\widehat{\beta(\tau)}\right|\leq h\right)\right]$$

for small h. Proper inference using the proposed estimation strategy requires accounting for the mechanical relationship between the year fixed effect estimates and the coefficients on the X variables. Let $\phi^{(l+)} = \phi + (\underbrace{0,\ldots,0}_{l-1}, h_I^{(l)}, 0, \ldots, 0)'$ and $\phi^{(l-)} = \phi - (\underbrace{0,\ldots,0}_{l-1}, h_I^{(l)}, 0, \ldots, 0)'$. $h_I^{(l)}$ is the change in the estimate of ϕ for variable l. The estimate of $\frac{\partial \hat{\gamma}_t}{\partial \phi'}$ is determined by

$$\frac{\partial \widehat{\gamma_t(\tau,\phi)}}{\partial \phi_l} = \frac{1}{2h_I^{(l)}}\left[\hat{\gamma}_t(\phi^{(l+)}) - \hat{\gamma}_t(\phi^{(l-)})\right].$$

Consistent estimation of Σ is possible by plugging in $\widehat{\beta(\tau)}$:

$$\widehat{\Sigma} = \frac{1}{N}\sum_i g_i(\widehat{\beta(\tau)})g_i(\widehat{\beta(\tau)})'$$

3.8 Simulations

To illustrate the usefulness of the IVQRPD estimator, I generate the following data (where $F(\cdot)$ represents the CDF of $\alpha_i + U_{it}$):

$$\begin{aligned}
& t \in \{0,1\} \\
\text{Fixed Effect:}\quad & \alpha_i \sim U(0,1) \\
& U_{it} \sim U(0,1) \\
\text{Total Disturbance:}\quad & U_{it}^* \equiv F(\alpha_i + U_{it}) \Rightarrow U_{it}^* \sim U(0,1) \\
\text{Year Effect:}\quad & \delta_0 = 1, \delta_1 = 2 \\
& \psi_{it} \sim U(0,1) \\
\text{Instrument:}\quad & Z_{it} = \alpha_i + \psi_{it} \\
\text{Policy Variable:}\quad & D_{it} = Z_{it} + U_{it} \\
\text{Outcome:}\quad & Y_{it} = U_{it}^*(\delta_t + D_{it})
\end{aligned}$$

The resulting SQF is $\tau(\delta_t + d_{it})$ for a given d_{it}. Note that D is a function of U so IV is necessary. Z is exogenous *conditional on* α_i so IVQR estimates would be inconsistent and conditioning on fixed effects is necessary. The impact of D is a nonlinear function of $\alpha + U$ and varies by observation. Consequently, the coefficients of interest vary by quantile and the coefficient on D in the SQF is equal to τ. Additive fixed effect quantile models assume that the coefficient only depends on U and should also produce inconsistent estimates. Year fixed effects are also crucial as the distribution changes (differentially) across years. I generate these data for $N = 500, T = 2$. Grid-searching is used to minimize the GMM objective function. Table 3 presents the results of the simulation for the coefficient of interest. I show results for IVQR (using Chernozhukov and Hansen (2006)), IVQRFE (using Harding and Lamarche (2009)), and IVQRPD.

Table 3: IVQRPD Simulation (N=500, T=2)

	IVQR			IVQRFE			IVQRPD		
Quantile	Mean Bias	MAD	RMSE	Mean Bias	MAD	RMSE	Mean Bias	MAD	RMSE
5	0.56057	0.55	0.56753	0.39750	0.41	0.42170	-0.00544	0.05	0.07027
10	0.70229	0.70	0.70723	0.34740	0.36	0.37478	-0.01025	0.06	0.09861
15	0.80304	0.80	0.80664	0.29736	0.31	0.32898	-0.00941	0.08	0.11788
20	0.87783	0.88	0.88058	0.24750	0.26	0.28468	-0.01046	0.09	0.13316
25	0.93577	0.93	0.93802	0.19762	0.21	0.24270	0.00099	0.11	0.14822
30	0.98169	0.98	0.98365	0.14765	0.16	0.20403	0.00181	0.11	0.16042
35	1.01647	1.02	1.01806	0.09748	0.13	0.17123	0.00337	0.12	0.16867
40	1.04178	1.04	1.04303	0.04731	0.10	0.14851	0.00291	0.12	0.17832
45	1.06114	1.06	1.06216	-0.00259	0.09	0.14093	0.00773	0.13	0.18106
50	1.06906	1.07	1.06987	-0.05266	0.10	0.15030	0.00852	0.13	0.18329
55	1.06489	1.07	1.06563	-0.10259	0.11	0.17430	0.00442	0.13	0.18429
60	1.04540	1.05	1.04602	-0.15269	0.15	0.20768	0.00167	0.13	0.18474
65	1.00899	1.01	1.00952	-0.20252	0.19	0.24663	-0.00151	0.12	0.18685
70	0.96410	0.96	0.96461	-0.25235	0.24	0.28898	-0.00279	0.12	0.18217
75	0.91812	0.92	0.91867	-0.30238	0.29	0.33360	-0.00361	0.12	0.18069
80	0.86625	0.87	0.86687	-0.35251	0.34	0.37954	-0.00390	0.12	0.17601
85	0.79638	0.80	0.79722	-0.40264	0.39	0.42653	-0.00539	0.12	0.16687
90	0.70683	0.71	0.70813	-0.45260	0.44	0.47395	-0.00672	0.10	0.15145
95	0.58787	0.59	0.59085	-0.50250	0.49	0.52185	-0.01127	0.09	0.12454

MAD=Median Absolute Deviation, RMSE=Root Mean Squared Error
IVQR refers to the estimator introduced in Chernozhukov and Hansen (2006). IVQRFE uses Harding and Lamarche (2009).

IVQR, as expected, performs poorly given that the assumptions for this estimator (eg. $U^*_{it}|Z_{it} \sim U(0,1)$) are not met. Furthermore, IVQRFE is biased for most quantiles. Note that IVQRFE fares well near the median because, for this data generating process, the median of U_{it} and the median of U^*_{it} are equal. The IVQRPD estimator performs well throughout the distribution.

4 Data and Empirical Strategy

4.1 Data

I use the 2008 panel of the Survey of Income and Program Participation (SIPP). The SIPP is a rich panel data set including detailed information about household income and participation in various benefit programs. Each household is interviewed every 4 months and labor earnings information is collected by month for each of the preceding 4 months, resulting in monthly earnings data for each household. A 4 month period is considered a "wave" and each wave has its own topical module. The first two waves of the 2008 SIPP asked questions about the timing and amount of the tax rebates received due to the economic stimulus package. I use the tax rebate information along with the monthly household labor earnings information as my key variables. To my knowledge, the 2008 SIPP is the only data set which includes tax rebate information and frequent reports of labor earnings. Both of these sets of variables are necessary for this paper.

The earliest reported monthly earnings in the data are for May 2008. I use earnings information from the first two waves only, leveraging the longitudinal nature of the data by linking earnings across time for each household. Households report their earnings for the previous four months, but households are interviewed at different points within the wave. While I have eight months of earnings data for each household, the sample itself spans more than eight months because the interviews are staggered. In the end, the first two waves provide earnings data beginning in May 2008 and ending in March 2009. Monthly labor earnings are topcoded, with some exceptions, at \$12,500. This topcoding also motivates the use of a quantile framework since the quantile treatment effects are robust to this censoring.

I select on households with non-missing labor earnings data for all eight months in the first two waves. I also exclude cohabitating non-married couples due to the possible receipt of multiple stimulus payments. I require the head of the household to be between 25 and 60. My specification will include interactions based on month, family size, and number of potential earners in the household. These interactions are important because a 1-person household earning \$3,000 per month is different from a 6-person household earning \$3,000 per month. I define "number of potential earners" as one if the head of the household is unmarried and two if the head of the household is married. The interactions also account for secular trends in the economy during this time period.

Using the weights provided by the SIPP, the total stimulus payments reported in the SIPP account for 96% of the total stimulus payments reported by the Department of the Treasury. Summary statistics are provided in Table 4. I report all summary statistics separately for single heads of the households and married couples. My sample includes 9,725 households with a single head of the household and 13,276 households with a married couple. Each household has 8 observations. The sample receiving a rebate has higher income at the bottom of the distribution, likely due to minimum income requirements to receive a rebate. I also report summary statistics for the sample receiving a rebate in April or May since this group predominantly received their rebates electronically. This group also appears to have higher labor earnings than the rest of the sample. These differences, though relatively small, reinforce the benefits of conditioning on household fixed effects and identifying off of changes in receipt.

77% of single households and 85% of married couples in the sample received a rebate. Though the rebate was phased out based on 2007 income, there were still high levels of receipt throughout the 2008 income distribution. Figure 1 shows the fraction of single households receiving a rebate as a function of their December 2008 income. December 2008 was chosen simply because this month is likely to be relatively "untreated" by rebate receipt. Figure 2 shows the same relationship for married couples. Rebate receipt rates are high even at relatively high levels of income. These high rates even at the top of the earnings distribution are primarily due to three reasons. First, households were given tax credits per child so even with the 5% phase out, households with high incomes would still receive a rebate. Second, rebates were calculated based on 2007 income (and did not have to be repaid if the household earned much more in 2008) and high-earnings households in 2008 may have received generous rebates. Finally, the rebates depended on income, not earnings so the relationship between labor earnings and rebate amounts is not perfect.

Finally, the SIPP asked respondents how they used their rebates. The options were the same as those found in Shapiro and Slemrod (2009). In the SIPP, 28.9% report mostly spending their rebate, 17.4% report mostly saving, and 53.7% report using their rebate to pay down debt. A higher percentage of households respond that they spent their rebate than the results reported in Shapiro and Slemrod (2009). As with the survey in Shapiro and Slemrod (2009), reducing labor supply is not asked and it seems unlikely that individuals equated "spending" with purchasing leisure.

4.2 Empirical Strategy

The primary specification in both Johnson et al. (2006) and Parker et al. (2013) is

$$C_{i,t+1} - C_{it} = \alpha_t + \beta \text{Rebate}_{i,t+1} + X_{it}'\delta + \epsilon_{it}$$

where C represents a consumption measure. To rely solely on rebate timing differences, the papers use instrumental variables with excluded instrument $\mathbf{1}(\text{Rebate}_{i,t+1} > 0)$. The authors argue that receipt of the rebate is conditionally exogenous while the rebate amount may provide additional information that could lead to inconsistent estimates. I adopt a similar empirical strategy. I estimate the SQF:

$$L_{it} = \alpha_{ht}(\tau) + \beta_1(\tau)R_{it} + \beta_2(\tau)LR_{it} \tag{17}$$

where L_{it} represents total labor earnings for household i in month t. α_{ht} is a fixed effect based on household size,[16] number of possible earners,[17] and month. These interactions allow the earnings distribution to shift based on the number of potential earners in the household, the month, and the total household size. Without adjusting for, say, number of earners, the high quantile estimates would primarily refer to married couples.

R represents rebate amount in month t and the previous month $t-1$. LR represents "lagged rebate" - the rebate amount in months $t-2$ and $t-3$. This specification allows the rebate to have an effect beyond the month of receipt. Note that the expenditure literature looks for an effect during the *quarter* of rebate receipt, while I am using monthly data so a lagged effect makes sense in this context.[18]

Following the instrumental variable strategy of Johnson et al. (2006) and Parker et al. (2013), I use instruments $\mathbf{1}(R_{it} > 0)$ and $\mathbf{1}(LR_{it} > 0)$ so that identification originates only from rebate timing. I condition on household fixed effects using QRPD to account for possible differences between those receiving and not receiving rebates in a specific month.

Because a nontrivial fraction of households have no monthly labor earnings and this fraction is larger for single earner households, I select on married couples for estimation at some lower quantiles. This selection ensures that the SQF is not itself censored and is not problematic on its own since I am selecting on a variable that is exogenous in my specification. This selection parallels censored quantile regression techniques which select on observations with a non-censored quantile function.

While my primary analysis will use the entire sample, I will also show that the results are robust to selecting only on households that receive a rebate at some point in

[16] Household size categories are 1, 2, 3, 4, 5, or 6+.

[17] Number of possible earners is equal to 1 if the head of the household is not married and 2 if the head of the household is married. Cohabiting couples were not used, as described in the Data section.

[18] Johnson et al. (2006) and Parker et al. (2013) also look at lagged effects.

2008. I will also replicate the analysis excluding households based on early or late receipt of the rebate. These alternative strategies mirror those also used in Johnson et al. (2006) and Parker et al. (2013).

Finally, labor earnings are censored at 0 for households with no labor earnings and topcoded (with some exceptions) in the data. The reported quantile treatment effects are unaffected by censoring on both sides. In my sample, I observe labor earnings for 184,008 household-months. Topcoding potentially[19] affects 7,510 household-months (6,067 for men and 1,443 for women) in the main analysis sample or, at most, 4% of the sample. While mean estimates may be significantly impacted by such censoring, quantile treatment effects up to quantile 95 are unlikely to be affected. The QTEs estimates should be robust to all censoring concerns.

5 Results

5.1 Graphical Evidence

We can observe suggestive evidence of labor supply effects graphically. I select on married couples for this analysis. I calculate the CDF for household monthly labor earnings, calculating the fraction of households with earnings below $100, $200, etc. I graph the difference in this CDF, showing the CDF for July relative to the CDF for December. We can think about December as an "untreated" or control month since few people are impacted by rebates at that time. I calculate these CDF changes for the households receiving rebates in May, June, or July and, separately, for all other households. These CDF changes are shown in Figure 3. We can see that the rebate group's differenced CDF is, generally, above the non-rebate group's. This graph shows that the rebate group was more likely to have lower monthly earnings throughout most of the distribution, relative to the non-rebate group and accounting for underlying differences between the two groups.

I also present similar graphical analysis for households receiving a rebate in May and households receiving a rebate in July. I graph the difference in the CDF in July relative to June. The July sample has not received a rebate in June. In July, upon receipt of the

[19]The SIPP topcodes individual-level earnings at $12,500, but there are exceptions in which the topcoding occurs at higher levels. Here, I report the number of observations with individual-level earnings at or above $12,500. Some of these may not, in fact, be topcoded and these are overestimates.

rebate, they experience a relative jump in their CDF for monthly earnings above \$3,000 (and under \$7,000). Again, this graph suggests that rebate receipt causes households to reduce monthly earnings.

The graphical evidence is suggestive of important labor supply effects. However, they do not account for differences in family size or include all possible variation in rebate receipt timing. I turn now to regression analysis for the main results of this paper. I will initially use mean regression techniques before estimating QTEs using the QRPD estimator introduced in this paper.

5.2 Mean Estimates

Before presenting QTE estimates, I will show some results from mean estimation. Table 5 presents the first stage relationship between the instruments and the rebate variables. It should not be surprising that there is a strong relationship given that the instruments are a function of the rebates themselves.

Results from OLS and mean IV estimation are reported in Table 6. There is no statistical relationship at the mean. Mean estimates are potentially biased due to topcoding and censoring at 0. Even without these concerns, though, mean estimates may not provide information about the response at any part of the distribution. For these reasons, this paper estimates QTEs.

5.3 Quantile Treatment Effect Estimates

When reporting QTE results, many of the tables will - when relevant - include “Monthly Earnings, Single” and/or “Monthly Earnings, Couple” to provide a general map between the quantiles and the earnings distribution. These statistics are the τ^{th} quantile of the distribution of the data setting $R_{it} = LR_{it} = 0$. In other words, I report the quantile of the counterfactual (or “untreated”) earnings distribution if there were no rebates for singles and married couples. These statistics will also be useful when I select the sample in a way that affects the earnings distribution of the analysis sample such that the quantiles do not refer to the same part of the distribution as the estimates from the full analysis sample.

The main results of this paper are shown in Figures 5 and 6. The estimates on the rebate variable are shown graphically in Figure 5 and the estimates on the lagged rebate variable are shown in Figure 6. I graph the estimates for quantiles 6 to 90. The confidence intervals for the estimates above quantile 90 are large so they are excluded from the graph. These same results are also presented in Table 7 in increments of 5. Subsequent results will be presented in similar tables.

I find consistent evidence of labor supply effects. In Table 7, the estimates are negative throughout the entire distribution for both variables (with the exception of the lagged rebate variable for quantile 95), implying that rebate receipt reduces labor earnings. At quantile 20 - corresponding to $308 in monthly labor earnings for singles and $2,667 for couples - the estimate on the rebate variable is significant at the 5% level and the estimate for the lagged rebate is significant at the 10% level. The estimates imply that for each $1 of the rebate, households reduced labor earnings by 9 cents in both the month of receipt and the subsequent month. In the following two months, earnings were reduced by 6 cents per rebate dollar. The estimates are slightly larger (in magnitude) above quantile 20 until quantile 80 ($4,975 in monthly labor earnings for singles, $10,152 for couples). The estimates for quantiles 25 to 75 imply that an additional $1 in rebate decreases household labor earnings by around 10 to 15 cents. In subsequent months, labor earnings are reduced by around 5 to 10 cents. These results are large and generally significant from 0, suggesting that the stimulus payments had economically important impacts on labor supply. I also find large effects near the top of the earnings distribution (quantile 90).

We might worry that not all of the timing differences are truly random, even conditional on household fixed effects. I address this concern in several ways. First, I replicate the above analysis but using only rebates received in May (Table A.1), June (Table A.2), or July (Table A.3) for identification. The instruments are only equal to 1 for households receiving a rebate in May or June or July, respectively. These tables are included in the Appendix. Given that there is less variation for identification, the estimates are noisier. However, the results are relatively robust regardless of which rebate month is used for identification.

Next, I can select on households that received rebates at some point in 2008. This selection eliminates households that never received a rebate and identification originates solely from differences in timing. The results are presented in Table 8. I find estimates that are similar to the main estimates of this paper. Again, I estimate significant effects for quantiles 25 to 75 with similar magnitudes. The effect at the top of the distribution disappears, though the standard errors of these estimates are relatively large.

Some variation is originating from households that are receiving rebates late. These households could be different on some dimensions. While conditioning on household fixed effects should account for many of these concerns, Table 9 presents results where these households are excluded from the sample. I find similar results as before.

Similarly, there may be concern that those receiving rebates in April or May are different since the electronic payments were sent out during those months and households receiving electronic payments may be different than those that do not. I eliminate these early receivers and present the results in Table 10. Again, the results appear robust to this selection, suggesting that conditioning on household fixed effects accounts for any such differences.

Finally, we may be concerned that rebate receipt is somehow correlated with receipt of other income (eg., EITC payments) or economic conditions. The quasi-randomized nature of the rebates makes this possibility especially unlikely, but I can perform a placebo test using later earnings data in the SIPP. For each household, I assign rebates to the exact same month in 2009 and replicate the analysis using the 2009 labor earnings data (April - December).[20] The results are reported in Table 11. I observe no relationship between these counterfactual rebates and labor supply, suggesting that there is no unobservable variable (such as timing of other payments) correlated with rebate timing that affect labor supply.

5.4 Mechanisms

While the above analysis estimates heterogenous impacts throughout the earnings distribution, this section explores what is driving the resulting patterns by studying heterogeneity across other characteristics. I first explore variation based on demographics, followed by analysis on the possible dimensions of labor supply responses.

5.4.1 Demographic Differences

Table 12 selects on single heads of households. I observe a similar pattern as before, though with more evidence that the top of distribution is affected by the rebates. Table 13 presents the corresponding estimates for married couples. Again, the pattern is similar, though I find evidence of stronger effects. This is potentially due to the presence of secondary earners,

[20] I use data from waves 3, 4, and 5 for this analysis.

who tend to have more elastic labor supply than primary earners. However, Tables 12 and 13 suggest that the results cannot be entirely explained by one type of household.

I study gender differences by type of household as well. Table 14 presents estimates for household heads who are single females. The estimates are large, especially for monthly incomes above $1,600. Table 15 presents the equivalent estimates for single men. These results are noisier. The point estimates still suggest the potential for large effects, but the evidence is not as strong as for single women.

Next, I evaluate married couples. Note, however, that this analysis cannot be done separately by gender at the individual-level since the outcome is household earnings. A low-earning man may be part of a high-earning household. Using the earnings of married men only would imply that the quantile estimates do not necessarily correspond to household earnings. Instead, I hold the monthly labor earnings of the spouse constant throughout the entire time period. For example, when evaluating married men, I use the labor earnings of his wife in the first observed period and assume that this does not change over the time period. Household labor earnings, consequently, only change due to behavioral responses by the man. The results for married women are presented in Table 16. They suggest large effects throughout the earnings distribution.

I evaluate the earnings of married men in Table 17, holding constant spousal earnings. The estimates are also large, though there is some suggestive evidence that the effects may be slightly smaller. Overall, it appears that the estimated effects for households headed by married couples cannot be attributed solely to men or women.

5.4.2 Sources of Responses

The entire earnings distribution can be affected if rebates affect extensive margin decisions. One possibility, given the economic atmosphere during the sample period, is that unemployed people delayed returning to work upon receipt of a rebate. I present evidence here that this possibility does not appear to be driving the results. I also estimate the extensive margin more directly later and find no effects on this margin. Here, I replicate the main analysis of this paper, selecting on households that have at least one worker (making positive labor earnings) and experience no change in the number of individuals making positive earnings in a month (i.e., a household with 2 people with positive labor earnings in the first month but only 1 person with positive labor earnings in the last month would not be used in this analysis).

This selection eliminates extensive margin responses and focuses only on households with labor force participation. We should expect the effects to be attenuated or even eliminated by this selection. Table 18 presents the estimates. Note that the elimination of households with no labor earnings implies that the quantiles refer to high-earnings households than previous results. The results in Table 18 are of similar magnitude as the main results of this paper (Table 7). There is some evidence that the estimates return to 0 faster at higher incomes when the extensive margin is excluded. Overall, though, the results are not being driven primarily be extensive margin behavioral choices.

I can select this sample even further by selecting on households which reported being paid hourly. I select on households with at least one worker that is paid by the hour and also require that the household not experience any changes in the number of hourly workers. As before, this selection potentially eliminates some of the sources driving the main estimates. In Table 19, I find strong evidence of effects for this group.

I can replicate this analysis but selecting on households with at least one individual making positive earnings but no workers that are paid by the hour. As before, I also select on households that experience no changes in this status. Table 20 presents these results. There is less evidence of an effect for this group. These results suggest that hourly workers may be driving the effect. While workers not paid by the hour are not necessarily salaried, it would make sense that this group is less likely to experience earnings differences due to short-term labor supply decisions.

I can also analyze the labor supply effects based on how households reported that they actually used their rebates. Table 21 reports estimates for households reporting that they mostly spent their rebates. Samples are small and the resulting estimates are noisy, but the estimates are similar to the main findings. Households that spend their rebates may work less because they value leisure, but we may also think that consumption-leisure complementarities are important. While isolating the importance of this complementarity is beyond the scope of this paper, I cannot rule out that such complementarities exist and partially explain the results of this paper.

Table 22 reports the same estimates for those reporting that they mostly saved their rebate. I find less evidence of labor supply effects though, again, standard errors are large and I cannot rule out large effects. Table 23 provides the estimates for households reporting that they used their rebates mainly to reduce debt. Here, I find statistically significant and large effects. This is suggestive evidence of the importance of liquidity constraints. Households may want to work less but are unable to borrow against future wealth. The rebates reduced these constraints and allowed households to reduce labor supply.

Finally, the SIPP provides more detailed variables on employment status and reason for absences. I use a detailed monthly employment status variable and study the relationship between rebates and reported employment status. All subsequent analysis is performed at the individual-level, and I employ 2SLS for this analysis. The first row of Table 24 reports these results. I report results for a $1,000 rebate by multiplying the resulting estimates by 1,000. In the first column, I study whether rebates are associated with positive earnings and find no evidence that rebates impact this dimension of labor supply, supporting the earlier conclusion of small extensive margin effects. In the remaining columns, I look at more detailed levels of employment status. The only marginally significant result is on the probability of having a job but missing at least one week of work without pay. This result holds only for absences not due to layoffs, while there is no effect on absences due to layoffs. This evidence supports the earlier findings that rebates result in labor supply reductions on the intensive margin.

Next, the SIPP also reports detailed data on unpaid absences and reasons for those absences. These data are reported by wave, not month. I aggregate all variables by wave. This analysis studies whether an unpaid absence occurred during the same 4-month period as the rebate. I find significant effects on unpaid absences. Furthermore, these absences are not due to illness or injury, layoffs, slack work, or care for children. Instead, there is a positive and significant relationship with "personal days," which supports the hypothesis that individuals use the rebates to "purchase" leisure by working less in the short-term.

6 Discussion and Conclusion

This paper studies the labor supply consequences of issuing tax rebates to households. Exploiting the quasi-randomized timing of disbursement of the 2008 Economic Stimulus Payments, I find robust evidence that households responded to payment receipt by reducing their labor supply. This effect occurs throughout most of the earnings distribution, with especially strong evidence of an effect in the middle of the distribution. To estimate these effects, I introduce a new quantile regression estimator for panel data. Importantly, this estimator allows for one to non-parametrically condition on household fixed effects while maintaining the nonseparable disturbance property that typically motivates use of quantile methods. This estimator should be useful in applications more broadly.

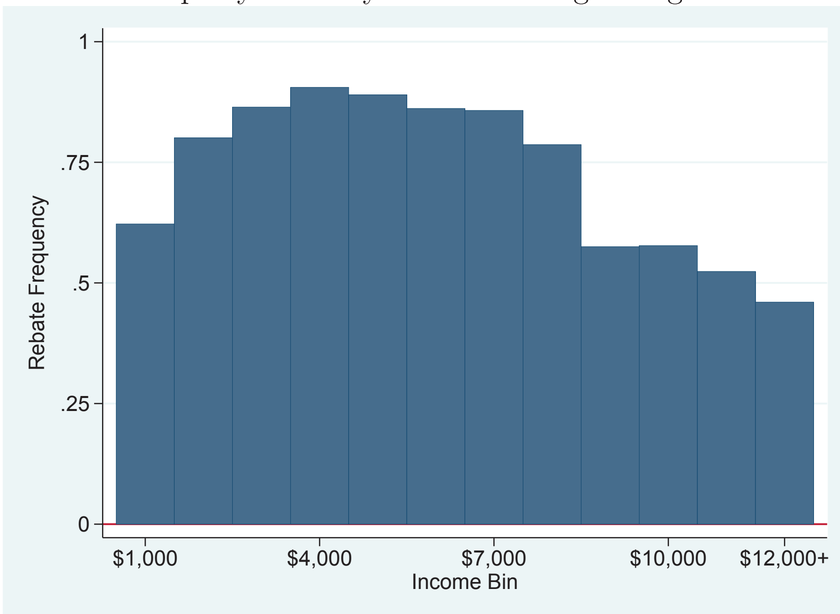
The results suggest labor supply effects even two to three months after rebate receipt. This labor supply effect cannot be explained by extensive labor supply responses and, in fact, there is little evidence of any extensive margin effects. Single women and married couples respond to receipt of stimulus payments with less evidence for single men. The effect appears to be driven by households with hourly workers. Furthermore, the evidence is strongest for households that used the rebate to reduce their debt, suggesting that liquidity constraints are an important source of the overall effect.

Finally, I present complementary evidence that rebates are associated with temporary reductions in labor supply and that these reductions are voluntary. The results of this paper are consistent with households using the stimulus payments as substitutes for labor earnings, allowing them to consume more leisure.

The findings of this paper have important implications for fiscal policy. The literature has provided convincing evidence that tax rebates and economic stimulus payments increase consume spending, implying that they have the potential for economically important aggregate demand effects. However, the literature has ignored the possibility of a simultaneous labor supply shock as households use their rebates as substitutes for labor earnings. This paper presents some of the first evidence of a concurrent labor supply shift. The results imply rather large labor supply reductions and, by extension, the possibility that the economic stimulus payments had effects on aggregate supply.

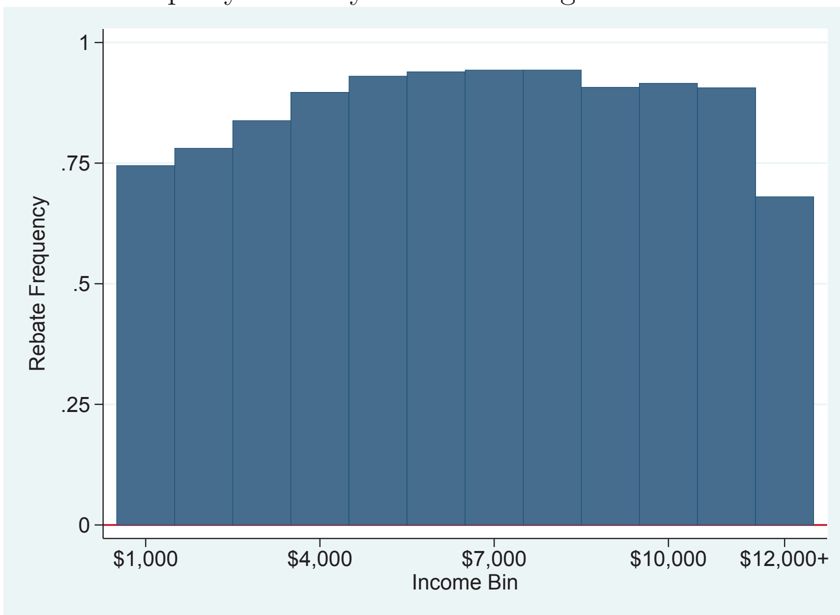
Figures

Figure 1: Rebate Receipt by Monthly Labor Earnings: Single Heads of Households



Income bin based on December 2008 labor earnings.

Figure 2: Rebate Receipt by Monthly Labor Earnings: Married Heads of Households



Income bin based on December 2008 labor earnings.

Figure 3: Changes in Earnings Distribution: Rebate vs. Non-Rebate Households

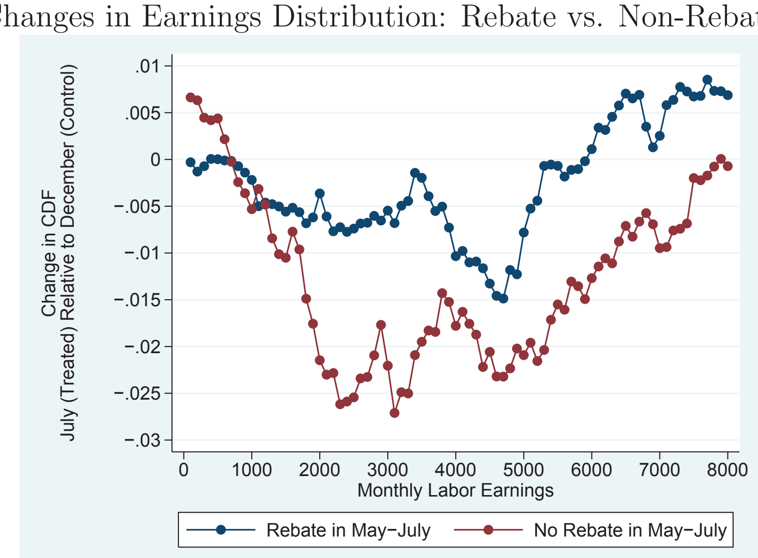


Figure 4: Changes in Earnings Distribution: May Rebate vs. July Rebate Households

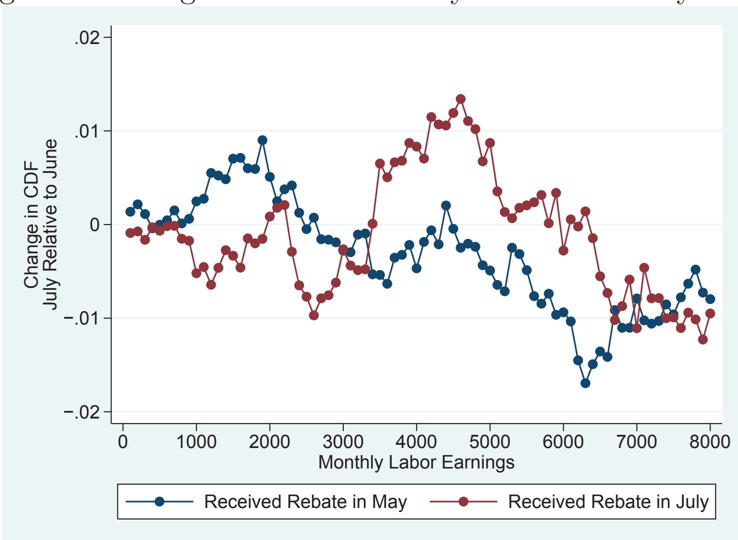


Figure 5: QTE Estimates for Rebate

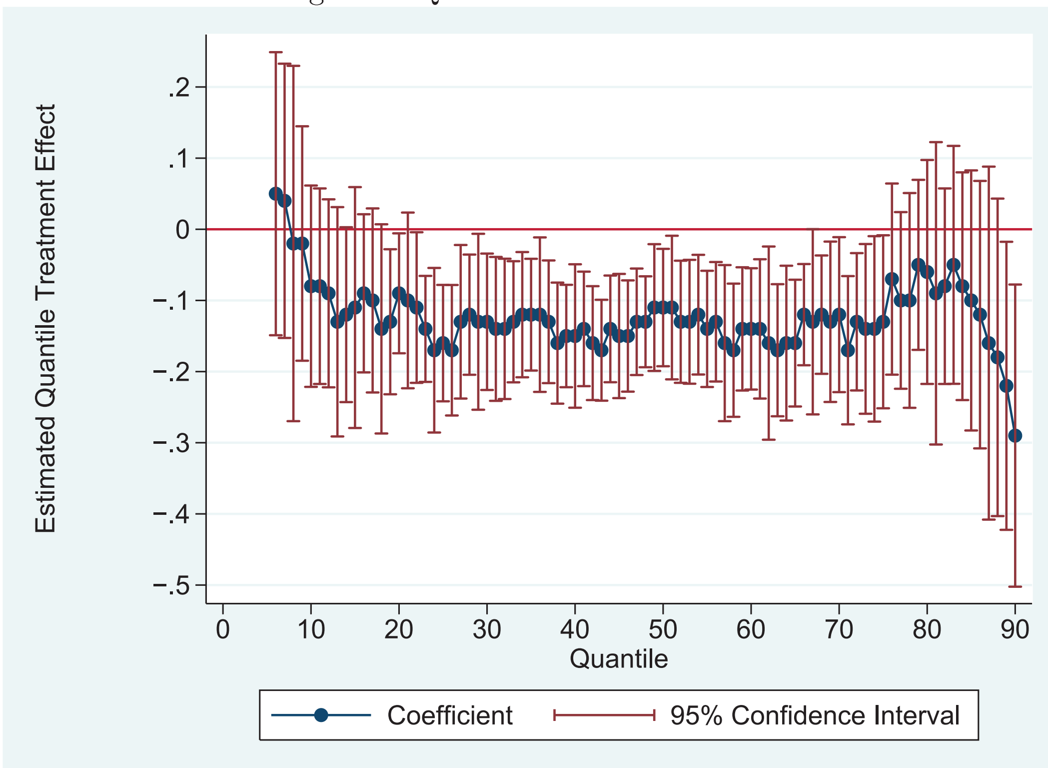
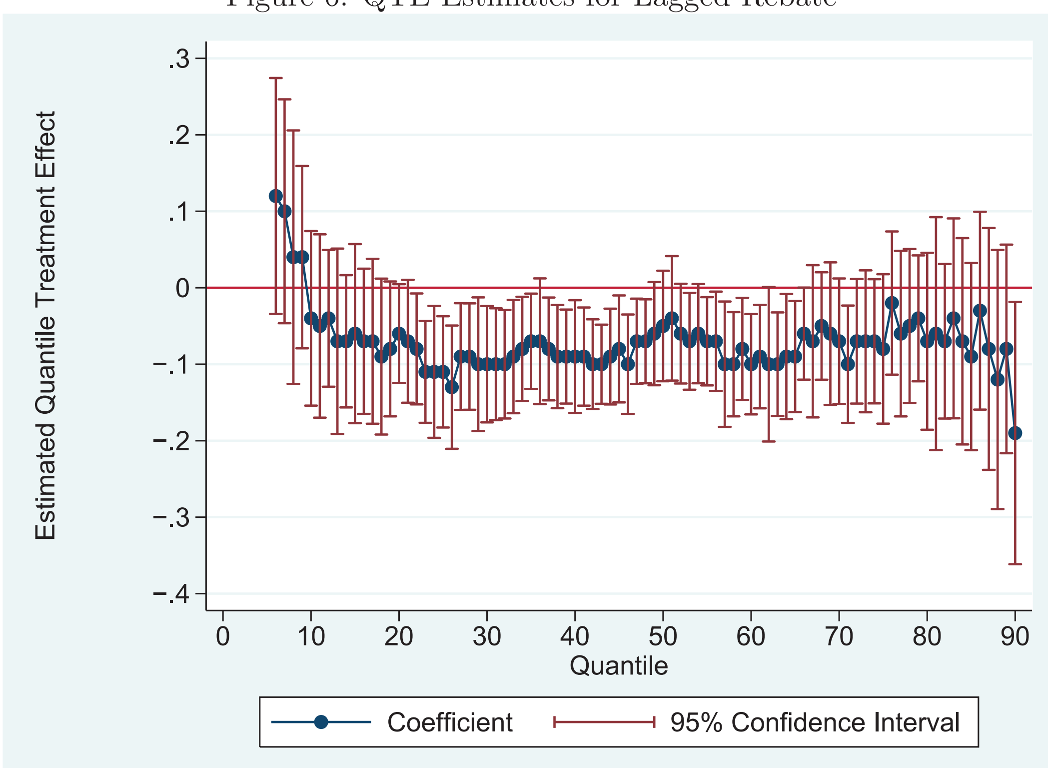


Figure 6: QTE Estimates for Lagged Rebate



Tables

Table 4: SIPP Summary Statistics

	Full Sample		Rebate Receiving Sample		Received Rebate in April/May	
	Single	Married	Single	Married	Single	Married
Received Rebate	76.7	85.3	100	100	100	100
Average Rebate Size in $ (conditional on receipt)	576	1095	576	1095	591	1144
Family Size	1.84	3.39	1.82	3.38	1.80	3.37
No Monthly Labor Earnings (%)	22.07	5.25	17.49	4.58	16.27	3.90
25th Percentile Monthly Labor Earnings	491	3224	1072	3385	1200	3603
Median Monthly Labor Earnings	2425	5881	2634	5833	2815	6000
75th Percentile Monthly Labor Earnings	4333	9155	4333	8711	4535	8778
Number of Households	9,725	13,276	7,433	11,359	2,651	4,284

Table 5: First Stage

	Rebate	Lagged Rebate
$\mathbf{1}(\text{Rebate} > 0)$	879.98***	-10.27***
	(4.72)	(1.33)
$\mathbf{1}(\text{Lagged Rebate} > 0)$	-0.39	874.98***
	(1.09)	(3.90)
F-Stat	34,769.46	50,237.14

***Significance 1%, ** Significance 5%, * Significance 10%. Standard errors in parentheses adjusted for household clustering.

Table 6: Mean Estimates

	Monthly Earnings	Monthly Earnings
Rebate	-0.00	-0.02
	(0.03)	(0.04)
Lagged Rebate	-0.00	-0.02
	(0.03)	(0.03)
OLS/IV	OLS	IV

***Significance 1%, ** Significance 5%, * Significance 10%. Standard errors in parentheses adjusted for household clustering.

Table 7: QTE Estimates of Impact of Rebates on Labor Earnings

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.08	-0.11	-0.09**	-0.16***	-0.13***	-0.12***	-0.15***	-0.15***	-0.11**
	(0.07)	(0.09)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)
Lagged Rebate	-0.04	-0.06	-0.06*	-0.11***	-0.10***	-0.07**	-0.09**	-0.08**	-0.05
	(0.06)	(0.06)	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)
Monthly Earnings, Single	n/a	n/a	308	786	1049	1458	1798	2100	2434
Monthly Earnings, Couple	1382	2083	2667	3291	3803	4333	4883	5400	5914
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.14***	-0.14***	-0.16***	-0.12**	-0.13**	-0.06	-0.10	-0.29***	-0.26
	(0.04)	(0.04)	(0.05)	(0.06)	(0.06)	(0.08)	(0.09)	(0.11)	(0.33)
Lagged Rebate	-0.07***	-0.10***	-0.09**	-0.07*	-0.08	-0.07	-0.09	-0.19**	0.03
	(0.03)	(0.03)	(0.04)	(0.04)	(0.05)	(0.06)	(0.06)	(0.09)	(0.22)
Monthly Earnings, Single	2771	3122	3500	3944	4375	4975	5676	6810	8805
Monthly Earnings, Couple	6456	7062	7700	8375	9167	10152	11479	13400	17133

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for sample period, setting the rebate variables to 0. "n/a" implies that single head of households were dropped due to censoring concerns.

Table 8: QTE Estimates: Sample Limited to Rebate-Receiving Households

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.05	-0.01	0.00	-0.09*	-0.12***	-0.09**	-0.14**	-0.10*	-0.10**
	(0.07)	(0.07)	(0.06)	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)
Lagged Rebate	-0.04	-0.04	-0.01	-0.07**	-0.08**	-0.06**	-0.09**	-0.05	-0.04
	(0.06)	(0.05)	(0.05)	(0.03)	(0.04)	(0.03)	(0.04)	(0.04)	(0.03)
Monthly Earnings, Single	n/a	400	800	1,083	1,483	1,796	2,083	2,397	2,658
Monthly Earnings, Couple	1593	2,252	2,840	3,417	3,936	4,416	4,934	5,393	5,857
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.13**	-0.15**	-0.17***	-0.14**	-0.15**	-0.04	-0.06	-0.12	-0.20
	(0.05)	(0.06)	(0.05)	(0.06)	(0.07)	(0.06)	(0.08)	(0.13)	(0.17)
Lagged Rebate	-0.06	-0.09**	-0.11**	-0.09**	-0.06	-0.01	-0.07	-0.01	-0.06
	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)	(0.05)	(0.05)	(0.10)	(0.15)
Monthly Earnings, Single	2,988	3,300	3,616	4,000	4,369	4,846	5,438	6,333	7,833
Monthly Earnings, Couple	6,337	6,882	7,474	8,083	8,750	9,552	10,543	11,916	14,417

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for sample period, setting the rebate variables to 0. "n/a" implies that single head of households were dropped due to censoring concerns.

Table 9: QTE Estimates: Exclude Late Rebates

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.13	-0.15**	-0.13	-0.17***	-0.12	-0.12**	-0.14***	-0.15***	-0.12**
	(0.10)	(0.07)	(0.10)	(0.05)	(0.08)	(0.05)	(0.05)	(0.06)	(0.05)
Lagged Rebate	-0.09	-0.09*	-0.08	-0.12**	-0.08	-0.06	-0.08**	-0.08**	-0.07
	(0.07)	(0.05)	(0.07)	(0.05)	(0.06)	(0.04)	(0.04)	(0.04)	(0.04)
Monthly Earnings, Single	n/a	n/a	330	786	1053	1472	1800	2110	2459
Monthly Earnings, Couple	1395	2100	2688	3298	3800	4330	4883	5407	5923
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.13**	-0.15**	-0.19***	-0.14***	-0.14	-0.04	-0.14	-0.39***	-0.33
	(0.06)	(0.06)	(0.06)	(0.05)	(0.09)	(0.10)	(0.10)	(0.15)	(0.38)
Lagged Rebate	-0.06*	-0.10**	-0.10**	-0.08	-0.08	-0.05	-0.11	-0.16*	0.00
	(0.03)	(0.05)	(0.05)	(0.05)	(0.07)	(0.08)	(0.08)	(0.09)	(0.29)
Monthly Earnings, Single	2771	3125	3500	3954	4383	5000	5729	6871	8889
Monthly Earnings, Couple	6454	7060	7716	8383	9181	10162	11502	13442	17368

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for sample period, setting the rebate variables to 0. "n/a" implies that single head of households were dropped due to censoring concerns.

Table 10: QTE Estimates: Exclude April and May Rebates

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.08	-0.11	-0.17**	-0.18***	-0.15**	-0.13**	-0.12	-0.19***	-0.16***
	(0.08)	(0.07)	(0.07)	(0.07)	(0.07)	(0.05)	(0.07)	(0.05)	(0.05)
Lagged Rebate	0.03	-0.01	-0.10	-0.10	-0.07	-0.08**	-0.07	-0.12***	-0.10**
	(0.05)	(0.05)	(0.08)	(0.07)	(0.06)	(0.04)	(0.06)	(0.04)	(0.05)
Monthly Earnings, Single	n/a	n/a	n/a	529	825	1,250	1,600	1,949	2,270
Monthly Earnings, Couple	1,176	1,900	2,500	3,072	3,604	4,171	4,706	5,303	5,870
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.12***	-0.16***	-0.11**	-0.15**	-0.1	-0.04	-0.08	-0.13	-0.01
	(0.05)	(0.06)	(0.05)	(0.06)	(0.09)	(0.09)	(0.14)	(0.16)	(0.47)
Lagged Rebate	-0.08*	-0.11*	-0.07	-0.10**	-0.06	-0.02	-0.01	-0.08	-0.05
	(0.04)	(0.06)	(0.07)	(0.05)	(0.07)	(0.08)	(0.10)	(0.17)	(0.32)
Monthly Earnings, Single	2,598	2,992	3,350	3,802	4,280	4,900	5,673	6,925	9,167
Monthly Earnings, Couple	6,440	7,085	7,762	8,500	9,389	10,453	11,969	14,353	19,166

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for sample period, setting the rebate variables to 0. "n/a" implies that single head of households were dropped due to censoring concerns.

Table 11: Counterfactual QTE Estimates: Assigning Rebates to Same Months in 2009

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	0.00	0.03	0.03	0.01	0.03	0.10	0.02	0.01	0.00
	(0.06)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
Lagged Rebate	-0.05	-0.01	0.01	-0.01	-0.01	-0.01	0.00	0.00	0.00
	(0.05)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	0.00	0.00	-0.03	-0.01	-0.01	0.00	-0.04	-0.02	0.11
	(0.03)	(0.03)	(0.03)	(0.03)	(0.05)	(0.05)	(0.06)	(0.08)	(0.15)
Lagged Rebate	0.01	0.02	0.01	0.02	0.03	0.03	0.01	-0.01	0.11
	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.05)	(0.07)	(0.17)

***Significance 1%, ** Significance 5%, * Significance 10%. SIPP waves 3, 4, and 5 used for this analysis.

Table 12: QTE Estimates: Single Heads of Households Only

Quantile	25	30	35	40	45	50
Rebate	0.08	-0.06	-0.11	-0.14	-0.17**	-0.10
	(0.19)	(0.10)	(0.07)	(0.09)	(0.09)	(0.06)
Lagged Rebate	0.03	-0.05	-0.05	-0.09	-0.13**	-0.08
	(0.13)	(0.07)	(0.05)	(0.07)	(0.06)	(0.05)
Monthly Earnings, Single	758	1037	1452	1793	2106	2439

Quantile	55	60	65	70	75	80	85	90	95
Rebate	-0.13**	-0.12	-0.18**	-0.12*	-0.18*	-0.02	-0.29**	-0.40**	-0.40
	(0.07)	(0.08)	(0.08)	(0.06)	(0.10)	(0.11)	(0.14)	(0.17)	(0.68)
Lagged Rebate	-0.06	-0.1	-0.11*	-0.09*	-0.08	-0.01	-0.18*	-0.23*	-0.03
	(0.05)	(0.08)	(0.06)	(0.05)	(0.08)	(0.10)	(0.10)	(0.13)	(0.87)
Monthly Earnings, Single	2768	3118	3500	3944	4375	4966	5708	6808	8800

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for sample period, setting the rebate variables to 0.

Table 13: QTE Estimates: Married Couples Only

Quantile	10	15	20	25	30	35	40	45	50
Rebate	-0.09	-0.10	-0.12**	-0.23***	-0.17***	-0.15**	-0.17***	-0.13***	-0.14**
	(0.07)	(0.06)	(0.06)	(0.05)	(0.06)	(0.06)	(0.05)	(0.05)	(0.07)
Lagged Rebate	-0.04	-0.05	-0.07	-0.15***	-0.13**	-0.10**	-0.10**	-0.06	-0.06
	(0.06)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)	(0.04)	(0.05)	(0.05)
Monthly Earnings, Couple	1,386	2,088	2,684	3,320	3,828	4,350	4,900	5,405	5,993

Quantile	55	60	65	70	75	80	85	90	95
Rebate	-0.15**	-0.14***	-0.14**	-0.12*	-0.08	-0.03	0.00	-0.01	0.16
	(0.07)	(0.05)	(0.06)	(0.06)	(0.07)	(0.09)	(0.14)	(0.13)	(0.47)
Lagged Rebate	-0.08	-0.09**	-0.07	-0.07	-0.07	-0.07	-0.03	-0.02	0.19
	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)	(0.07)	(0.11)	(0.13)	(0.41)
Monthly Earnings, Couple	6,480	7,079	7,704	8,384	9,167	10,158	11,467	13,334	17,087

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for sample period, setting the rebate variables to 0.

Table 14: QTE Estimates: Single Female Head of Household

Quantile	20	25	30	35	40	45	50
Rebate	-0.02	-0.04	-0.12	-0.13	-0.19**	-0.24***	-0.17*
	(0.13)	(0.11)	(0.11)	(0.10)	(0.09)	(0.06)	(0.10)
Lagged Rebate	-0.17*	-0.05	-0.09	-0.08	-0.14**	-0.15**	-0.08
	(0.09)	(0.10)	(0.09)	(0.08)	(0.07)	(0.06)	(0.08)
Monthly Earnings, Single	360	625	950	1,280	1,600	1,940	2,210

Quantile	55	60	65	70	75	80	85	90	95
Rebate	-0.12	-0.22**	-0.15	-0.22***	-0.25**	-0.29***	-0.29*	-0.29	-0.36
	(0.08)	(0.11)	(0.12)	(0.07)	(0.11)	(0.12)	(0.15)	(0.27)	(0.44)
Lagged Rebate	-0.08	-0.14*	-0.13	-0.19***	-0.13	-0.1	-0.14	-0.23	-0.09
	(0.06)	(0.08)	(0.09)	(0.06)	(0.10)	(0.09)	(0.10)	(0.20)	(0.39)
Monthly Earnings, Couple	2,533	2,884	3,240	3,646	4,082	4,583	5,254	6,269	8,333

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for sample period, setting the rebate variables to 0.

Table 15: QTE Estimates: Single Male Head of Household

Quantile			**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate			0.08	0.19	0.11	0.06	-0.01	-0.16	-0.09
			(0.29)	(0.42)	(0.17)	(0.14)	(0.14)	(0.12)	(0.13)
Lagged Rebate			0.19	0.09	0.11	0.06	-0.01	-0.13	-0.04
			(0.20)	(0.31)	(0.14)	(0.10)	(0.10)	(0.08)	(0.11)
Monthly Earnings, Single			656	1,250	1,299	1,700	2,078	2,419	2,750
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.13	-0.14	-0.18	-0.18	-0.04	-0.18	-0.39**	-0.39	-0.2
	(0.12)	(0.11)	(0.13)	(0.16)	(0.25)	(0.13)	(0.19)	(0.33)	(0.78)
Lagged Rebate	-0.01	-0.01	-0.04	-0.03	0.01	-0.11	-0.11	-0.13	0.19
	(0.08)	(0.09)	(0.11)	(0.10)	(0.17)	(0.12)	(0.17)	(0.20)	(0.51)
Monthly Earnings, Couple	3,102	3,456	3,888	4,320	4,800	5,432	6,274	7,500	9,483

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for sample period, setting the rebate variables to 0.

Table 16: QTE Estimates: Married Women (Men's Earnings Held Constant)

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.08*	-0.08**	-0.16***	-0.10***	-0.10***	-0.15***	-0.13***	-0.20***	-0.19***
	(0.04)	(0.04)	(0.05)	(0.03)	(0.04)	(0.05)	(0.03)	(0.05)	(0.05)
Lagged Rebate	-0.01	-0.03	-0.08**	-0.06**	-0.05*	-0.08**	-0.05**	-0.08**	-0.10**
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)	(0.02)	(0.04)	(0.04)
Monthly Earnings, Single	2,124	2,846	3,481	4,060	4,590	5,140	5,667	6,250	6,840
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.18***	-0.13***	-0.16***	-0.19***	-0.15**	-0.13*	-0.23***	-0.15	0.02
	(0.04)	(0.05)	(0.04)	(0.05)	(0.07)	(0.07)	(0.09)	(0.22)	(0.21)
Lagged Rebate	-0.09***	-0.08**	-0.09***	-0.12***	-0.09*	-0.05	-0.12*	-0.07	0.14
	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)	(0.04)	(0.06)	(0.18)	(0.24)
Monthly Earnings, Couple	7,463	8,107	8,788	9,660	10,663	11,917	13,776	16,800	26,618

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for sample period, setting the rebate variables to 0.

Table 17: QTE Estimates: Married Men (Women's Earnings Held Constant)

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.05	-0.12**	-0.14**	-0.12***	-0.12**	-0.14***	-0.14***	-0.20***	-0.13**
	(0.06)	(0.05)	(0.06)	(0.04)	(0.05)	(0.06)	(0.04)	(0.06)	(0.05)
Lagged Rebate	-0.02	-0.07*	-0.07	-0.06**	-0.05	-0.07*	-0.08***	-0.10***	-0.09**
	(0.04)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
Monthly Earnings, Single	1,687	2,400	3,006	3,600	4,167	4,676	5,220	5,800	6,338
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.14***	-0.11**	-0.06	-0.09*	-0.12**	-0.02	-0.04	0.02	0.18
	(0.05)	(0.05)	(0.05)	(0.05)	(0.06)	(0.10)	(0.08)	(0.15)	(0.39)
Lagged Rebate	-0.11***	-0.08**	-0.04	-0.06	-0.08**	-0.06	-0.04	0.08	0.19
	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.07)	(0.07)	(0.14)	(0.30)
Monthly Earnings, Couple	6,928	7,559	8,214	8,941	9,896	10,917	12,447	14,658	20,372

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for sample period, setting the rebate variables to 0.

Table 18: QTE Estimates: Select on Households with No Extensive Margin Labor Supply Changes

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.07	-0.14***	-0.12**	-0.17***	-0.10*	-0.13***	-0.12***	-0.08*	-0.12**
	(0.05)	(0.05)	(0.05)	(0.04)	(0.06)	(0.05)	(0.05)	(0.04)	(0.06)
Lagged Rebate	-0.04	-0.11***	-0.09**	-0.11***	-0.07*	-0.09**	-0.07*	-0.05*	-0.07*
	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)
Monthly Earnings, Single	1,366	1,700	2,000	2,246	2,500	2,735	3,000	3,248	3,507
Monthly Earnings, Couple	2,600	3,331	3,897	4,432	4,946	5,417	5,918	6,392	6,919
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.06	-0.04	-0.07	0.01	-0.07	-0.06	-0.25*	-0.35*	0.08
	(0.05)	(0.06)	(0.06)	(0.09)	(0.08)	(0.09)	(0.14)	(0.19)	(0.38)
Lagged Rebate	-0.05	-0.02	-0.02	0.00	-0.06	-0.06	-0.05	-0.06	0.19
	(0.04)	(0.04)	(0.05)	(0.07)	(0.06)	(0.07)	(0.11)	(0.17)	(0.36)
Monthly Earnings, Single	3,810	4,113	4,443	4,833	5,332	5,911	6,694	7,917	9,851
Monthly Earnings, Couple	7,475	8,004	8,583	9,291	10,124	11,093	12,423	14,500	18,739

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for sample period, setting the rebate variables to 0.

Table 19: QTE Estimates: Households with Hourly Wages

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.05	-0.08	-0.14***	-0.15***	-0.12	-0.15***	-0.13**	-0.11**	-0.12
	(0.08)	(0.07)	(0.05)	(0.06)	(0.08)	(0.05)	(0.06)	(0.05)	(0.09)
Lagged Rebate	-0.01	-0.02	-0.05	-0.11**	-0.1	-0.11**	-0.09**	-0.09*	-0.1
	(0.05)	(0.05)	(0.04)	(0.04)	(0.07)	(0.05)	(0.04)	(0.05)	(0.07)
Monthly Earnings, Single	1,176	1,400	1,596	1,779	1,965	2,124	2,316	2,488	2,652
Monthly Earnings, Couple	2,169	2,680	3,168	3,654	4,140	4,510	4,886	5,274	5,687
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.12*	-0.14*	-0.11	-0.11	-0.20*	-0.19**	-0.18	-0.19	-0.39
	(0.06)	(0.08)	(0.08)	(0.08)	(0.11)	(0.08)	(0.13)	(0.15)	(0.31)
Lagged Rebate	-0.09*	-0.11	-0.07	-0.05	-0.11	-0.05	-0.05	-0.17	-0.07
	(0.05)	(0.07)	(0.06)	(0.06)	(0.07)	(0.06)	(0.09)	(0.12)	(0.30)
Monthly Earnings, Single	2,838	3,094	3,325	3,566	3,962	4,320	4,815	5,583	7,004
Monthly Earnings, Couple	6,094	6,550	7,042	7,566	8,237	8,935	9,793	10,952	13,089

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for sample period, setting the rebate variables to 0. Sample is first selected on households with no extensive margin labor supply changes. The sample is next selected on no changes in the number of workers that are paid an hourly wage. Finally, the sample is selected on households with at least one worker that is paid hourly.

Table 20: QTE Estimates: Households with Non-Hourly Compensation

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.12	-0.05	-0.10	-0.07	-0.10	0.01	0.02	0.04	-0.08
	(0.10)	(0.09)	(0.12)	(0.08)	(0.07)	(0.08)	(0.09)	(0.09)	(0.09)
Lagged Rebate	-0.12*	-0.11	-0.08	-0.07	-0.01	0.01	0.03	0.04	-0.03
	(0.07)	(0.07)	(0.08)	(0.06)	(0.06)	(0.07)	(0.07)	(0.07)	(0.08)
Monthly Earnings, Single	2,083	2,500	2,900	3,171	3,470	3,750	4,000	4,297	4,600
Monthly Earnings, Couple	3,400	4,167	4,925	5,642	6,208	6,675	7,361	7,950	8,403

Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	0.01	0.01	0.03	-0.08	-0.11	-0.10	-0.19	-0.26	-0.01
	(0.10)	(0.11)	(0.08)	(0.13)	(0.11)	(0.15)	(0.23)	(0.47)	(0.78)
Lagged Rebate	0.01	0.00	0.00	-0.01	0.00	0.00	0.08	0.19	0.19
	(0.08)	(0.07)	(0.06)	(0.11)	(0.09)	(0.12)	(0.17)	(0.29)	(0.56)
Monthly Earnings, Single	4,994	5,282	5,667	6,206	6,667	7,425	8,317	9,298	11,833
Monthly Earnings, Couple	9,094	9,950	10,583	11,488	12,500	13,750	15,417	18,333	30,200

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for sample period, setting the rebate variables to 0. Sample is first selected on households with no extensive margin labor supply changes. The sample is next selected on no changes in the number of workers that are paid an hourly wage. Finally, the sample is selected on households with no workers that are paid hourly.

Table 21: QTE Estimates: Households Reporting that They Spent Rebate

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	0.03	-0.12	-0.12	-0.10	-0.16	-0.11	-0.04	-0.02	-0.14
	(0.16)	(0.16)	(0.21)	(0.13)	(0.13)	(0.10)	(0.09)	(0.08)	(0.11)
Lagged Rebate	0.07	-0.05	-0.11	-0.04	-0.1	-0.05	-0.01	-0.01	-0.02
	(0.14)	(0.14)	(0.13)	(0.09)	(0.09)	(0.08)	(0.05)	(0.07)	(0.09)
Monthly Earnings, Single	n/a	358	820	960	1,455	1,800	2,124	2,497	2,808
Monthly Earnings, Couple	1,667	2,484	3,166	3,689	4,320	4,868	5,308	5,813	6,250

Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.11	-0.15	-0.13	-0.16	-0.20*	-0.25	-0.10	-0.02	-0.09
	(0.11)	(0.10)	(0.13)	(0.12)	(0.11)	(0.18)	(0.17)	(0.26)	(1.59)
Lagged Rebate	-0.06	-0.08	-0.06	-0.11	-0.16*	-0.15	-0.08	0.13	0.17
	(0.08)	(0.09)	(0.10)	(0.09)	(0.09)	(0.12)	(0.12)	(0.18)	(1.87)
Monthly Earnings, Single	3,125	3,492	3,911	4,250	4,679	5,167	5,833	6,667	8,360
Monthly Earnings, Couple	6,761	7,358	7,917	8,465	9,160	10,000	10,858	12,250	14,600

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for the first month in the data, setting the rebate variables to 0.

Table 22: QTE Estimates: Households Reporting that They Saved Rebate

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	0.15	0.19	0.14	0.16	0.03	0.05	0.06	0.12	-0.03
	(0.19)	(0.15)	(0.15)	(0.12)	(0.12)	(0.16)	(0.19)	(0.11)	(0.14)
Lagged Rebate	0.12	0.12	0.04	0.14	0.05	0.1	0.08	0.08	0
	(0.12)	(0.12)	(0.13)	(0.09)	(0.11)	(0.13)	(0.15)	(0.08)	(0.13)
Monthly Earnings, Single	623	928	1,600	2,000	2,455	2,729	3,000	3,353	3,750
Monthly Earnings, Couple	2,364	3,172	3,976	4,500	5,167	5,625	6,139	6,617	7,128
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.07	0.02	0.00	-0.01	-0.10	-0.07	-0.17	-0.28	-0.06
	(0.14)	(0.12)	(0.13)	(0.15)	(0.19)	(0.23)	(0.23)	(0.30)	(0.97)
Lagged Rebate	-0.04	0.02	0.01	-0.07	-0.13	-0.16	-0.2	-0.38*	-0.26
	(0.11)	(0.12)	(0.08)	(0.12)	(0.16)	(0.19)	(0.15)	(0.23)	(0.61)
Monthly Earnings, Single	4,020	4,320	4,594	5,000	5,500	6,000	6,667	7,568	9,715
Monthly Earnings, Couple	7,637	8,244	8,722	9,390	10,083	10,929	12,013	13,676	16,035

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for the first month in the data, setting the rebate variables to 0.

Table 23: QTE Estimates: Households Reporting that They Used Rebate to Reduce Debt

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.09	-0.07	-0.05	-0.1	-0.14**	-0.11	-0.14***	-0.16***	-0.15***
	(0.10)	(0.09)	(0.07)	(0.09)	(0.06)	(0.07)	(0.05)	(0.05)	(0.05)
Lagged Rebate	-0.06	-0.04	-0.04	-0.09	-0.10**	-0.09*	-0.10**	-0.11***	-0.08*
	(0.07)	(0.07)	(0.05)	(0.06)	(0.05)	(0.05)	(0.04)	(0.04)	(0.05)
Monthly Earnings, Single	n/a	300	748	1,000	1,350	1,633	1,924	2,167	2,430
Monthly Earnings, Couple	1,400	2,072	2,596	3,088	3,557	4,020	4,460	4,903	5,333
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.14**	-0.17**	-0.20***	-0.22***	-0.15*	-0.13	0.03	-0.18	-0.32
	(0.06)	(0.07)	(0.07)	(0.08)	(0.08)	(0.09)	(0.11)	(0.18)	(0.27)
Lagged Rebate	-0.07	-0.10**	-0.11**	-0.12*	-0.07	0	0.05	-0.014	-0.09
	(0.04)	(0.05)	(0.05)	(0.07)	(0.06)	(0.07)	(0.10)	(0.13)	(0.21)
Monthly Earnings, Single	2,685	3,000	3,325	3,637	4,000	4,417	4,999	5,833	7,210
Monthly Earnings, Couple	5,806	6,300	6,807	7,403	8,050	8,820	9,808	11,142	13,399

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for the first month in the data, setting the rebate variables to 0.

Table 24: Employment Status and Absences

	Monthly Job Status					
	Earnings > 0	With Job All Month Worked All Weeks	With Job All Month Absent 1+ Weeks Without Pay Not Due to Layoffs	With Job All Month Absent 1+ Weeks Without Pay Due to Layoffs	With Job 1+ Weeks No Time on Layoff Not Looking for Work	With Job 1+ Weeks Layoff or Looking for Work
Rebate (/$1,000)	-0.002	0.001	0.002*	0.000	-0.001	-0.001
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Lagged Rebate (/$1,000)	-0.001	0.001	0.001	-0.001	0.000	0.001
	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.080)
	Unpaid Absences					
	Unpaid Absence	Illness or Injury	Personal Days	Layoff	Slack Work	Taking Care of Children
Rebate (/$1,000)	0.011***	0.001	0.004**	-0.001	0.003	0.000
	(0.004)	(0.001)	(0.002)	(0.002)	(0.002)	(0.000)
Lagged Rebate (/$1,000)	-0.002	-0.001	0.001	-0.002*	0.000	-0.000
	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)	(0.000)

***Significance 1%, ** Significance 5%, * Significance 10%. Two-Stage Least Squares estimates reported. Standard errors are adjusted for clustering. Interactions based on month, household size, and number of earners also included in regressions.

A Appendix

A.1 Tables

Table A.1: QTE Estimates: May-Only Instrument

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.13	-0.14	-0.09	-0.15**	-0.08	-0.12*	-0.18***	-0.16***	-0.09
	(0.15)	(0.09)	(0.06)	(0.07)	(0.07)	(0.06)	(0.07)	(0.06)	(0.07)
Lagged Rebate	-0.13	-0.18	-0.14***	-0.19**	-0.08	-0.06	-0.12**	-0.13**	-0.05
	(0.13)	(0.12)	(0.05)	(0.08)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Monthly Earnings, Single	n/a	n/a	318	793	1,040	1,455	1,800	2,107	2,432
Monthly Earnings, Couple	1,388	2,106	2,685	3,300	3,798	4,331	4,897	5,413	5,908
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.11*	-0.09	-0.1	-0.09	-0.14	-0.09	-0.27	-0.40**	-0.37
	(0.06)	(0.06)	(0.08)	(0.07)	(0.11)	(0.11)	(0.17)	(0.18)	(0.58)
Lagged Rebate	-0.08	-0.08	-0.09	-0.06	-0.08	-0.13	-0.26*	-0.34**	0.19
	(0.06)	(0.07)	(0.06)	(0.09)	(0.11)	(0.09)	(0.15)	(0.15)	(0.36)
Monthly Earnings, Single	2,771	3,118	3,494	3,936	4,377	4,994	5,733	6,833	8,800
Monthly Earnings, Couple	6,451	7,048	7,684	8,366	9,167	10,167	11,546	13,452	17,118

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for sample period, setting the rebate variables to 0. "n/a" implies that single head of households were dropped due to censoring concerns.

Table A.2: QTE Estimates: June-Only Instrument

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.19*	-0.19	-0.16	-0.14	-0.22*	-0.17**	-0.20***	-0.20**	-0.19***
	(0.10)	(0.13)	(0.13)	(0.09)	(0.12)	(0.07)	(0.07)	(0.09)	(0.07)
Lagged Rebate	-0.06	-0.06	-0.09	-0.11	-0.19**	-0.14***	-0.15**	-0.12**	-0.15*
	(0.10)	(0.08)	(0.09)	(0.07)	(0.10)	(0.05)	(0.07)	(0.06)	(0.09)
Monthly Earnings, Single	n/a	n/a	320	783	1,065	1,472	1,800	2,117	2,466
Monthly Earnings, Couple	1,386	2,090	2,688	3,287	3,833	4,355	4,909	5,416	5,958
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.26*	-0.23**	-0.25*	-0.33***	-0.28	-0.05	-0.02	-0.35	-0.24
	(0.14)	(0.10)	(0.13)	(0.11)	(0.17)	(0.12)	(0.27)	(0.26)	(0.70)
Lagged Rebate	-0.15*	-0.14	-0.12	-0.19**	-0.18	-0.1	-0.07	-0.13	-0.21
	(0.08)	(0.09)	(0.08)	(0.09)	(0.12)	(0.10)	(0.18)	(0.18)	(0.42)
Monthly Earnings, Single	2,790	3,138	3,500	3,992	4,401	4,983	5,667	6,803	8,831
Monthly Earnings, Couple	6,500	7,083	7,730	8,441	9,219	10,160	11,458	13,400	17,167

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for sample period, setting the rebate variables to 0. "n/a" implies that single head of households were dropped due to censoring concerns.

Table A.3: QTE Estimates: July-Only Instrument

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.1	-0.1	-0.13	-0.14*	-0.15**	-0.12**	-0.13*	-0.11*	-0.09
	(0.11)	(0.10)	(0.10)	(0.08)	(0.07)	(0.06)	(0.08)	(0.07)	(0.07)
Lagged Rebate	-0.12	-0.06	-0.08	-0.05	-0.10*	-0.05	-0.08*	-0.05	-0.03
	(0.10)	(0.05)	(0.07)	(0.06)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)
Monthly Earnings, Single	n/a	n/a	317	779	1,049	1,455	1,793	2,099	2,428
Monthly Earnings, Couple	1,386	2,083	2,678	3,276	3,810	4,330	4,876	5,397	5,900
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.06	-0.1	-0.14**	-0.09	-0.12	-0.13	-0.02	-0.35*	-0.02
	(0.09)	(0.08)	(0.07)	(0.11)	(0.09)	(0.09)	(0.16)	(0.21)	(0.51)
Lagged Rebate	-0.01	-0.08	-0.07	-0.07	-0.10**	-0.12	-0.04	-0.18	0.1
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.08)	(0.11)	(0.13)	(0.48)
Monthly Earnings, Single	2,761	3,118	3,498	3,942	4,380	4,998	5,667	6,811	8,776
Monthly Earnings, Couple	6,421	7,050	7,691	8,370	9,167	10,177	11,456	13,402	17,095

***Significance 1%, ** Significance 5%, * Significance 10%. "Monthly Earnings" refer to τ^{th} quantile of counterfactual distribution for sample period, setting the rebate variables to 0. "n/a" implies that single head of households were dropped due to censoring concerns.

A.2 QRPD Proofs

Theorem 3.1 (Moment Conditions)**.** *Suppose **A1** and **A2** hold. Then for each* $\tau \in (0,1)$,

$$E\left\{ \frac{1}{2T^2} \sum_t \sum_s (Z_{it} - Z_{is}) \left[\mathbf{1}(Y_{it} \leq D'_{it}\beta(\tau)) - \mathbf{1}(Y_{is} \leq D'_{is}\beta(\tau)) \right] \right\} = 0 \quad \textit{for all } s, t, \tag{7}$$

$$E[\mathbf{1}(Y_{it} \leq D'_{it}\beta(\tau)) - \tau] = 0. \tag{8}$$

Proof of (7):

$$E\left\{ (Z_{it} - Z_{is}) \left[\mathbf{1}(Y_{it} \leq D'_{it}\beta(\tau)) - \mathbf{1}(Y_{is} \leq D'_{is}\beta(\tau)) \right] \right\}$$

$$\begin{aligned}
&= E\left[E\left\{ (Z_{it} - Z_{is}) \left[\mathbf{1}(Y_{it} \leq D'_{it}\beta(\tau)) - \mathbf{1}(Y_{is} \leq D'_{is}\beta(\tau)) \right] \Big| Z_i \right\} \right] \\
&= E\left[(Z_{it} - Z_{is}) E\left\{ \mathbf{1}(D'_{it}\beta(U^*_{it}) \leq D'_{it}\beta(\tau)) - \mathbf{1}(D'_{is}\beta(U^*_{is}) \leq D'_{is}\beta(\tau)) \Big| Z_i \right\} \right] \quad \text{by } \mathbf{A1} \\
&= E\left[(Z_{it} - Z_{is}) E\left\{ \mathbf{1}(U^*_{it} \leq \tau) - \mathbf{1}(U^*_{is} \leq \tau) \Big| Z_i \right\} \right] \text{by } \mathbf{A1} \\
&= 0 \qquad \text{by } \mathbf{A2}
\end{aligned}$$

Proof of (8):

$$\begin{aligned}
E[\mathbf{1}(Y_{it} \leq D'_{it}\beta(\tau))] &= E[\mathbf{1}(D'_{it}\beta(U^*_{it}) \leq D'_{it}\beta(\tau))] \qquad \text{by } \mathbf{A1} \\
&= P\left[U^*_{it} \leq \tau\right] \qquad \text{by } \mathbf{A1} \\
&= \tau \qquad \text{by } \mathbf{A1}
\end{aligned}$$

Theorem 3.2 (Identification)**.** *If (i) **IV-A1** - **IV-A4** hold;*
(ii) $E\left\{ \frac{1}{T} \sum_{t=1}^{T} \left(Z_{it} - \overline{Z}_i \right) \left[\mathbf{1}(Y_{it} \leq D'_{it}\breve{\beta}) \right] \right\} = 0$*; (iii)* $E\left[\mathbf{1}(Y_{it} \leq D'_{it}\breve{\beta}) \right] = \tau$*, then* $\breve{\beta} = \beta(\tau)$.

First, some notation:

$$\Gamma(Z_i, \beta) \equiv \begin{bmatrix} P(Y_{it} \leq d^{(1)'}\beta | Z_i) \\ \vdots \\ P(Y_{it} \leq d^{(M)'}\beta | Z_i) \end{bmatrix}.$$

Initially, I assume that the policy variables are discrete such that Ψ includes all possible values. The extension is straightforward and included after the proof.

Proof. Starting with (ii) and the Law of Iterated Expectations, $E[(Z_{it} - \overline{Z}_i)\Pi(Z_i)\Gamma(Z_i, \breve{\beta})] = 0$.

Without loss of generality, assume that $P(Y_{it} \leq d^{(1)'}\breve{\beta} | Z_i) = P(Y_{it} \leq d^{(1)'}\beta(\tilde{\tau}) | Z_i)$ for some $\tilde{\tau} \in (0, 1)$.

By **IV-A3**, we know that $P(Y_{it} \leq d^{(m)'}\breve{\beta} | Z_i) = P(Y_{it} \leq d^{(m)'}\beta(\tilde{\tau}) | Z_i)$ for all m.

IV-A4 implies that $d^{(m)'}\breve{\beta} = d^{(m)'}\beta(\tilde{\tau})$ for all m. By the full rank assumption in **IV-A3** then, $\breve{\beta} = \beta(\tilde{\tau})$.

Because of (iii), we know that $\tilde{\tau} = \tau$, implying that $\breve{\beta} = \beta(\tau)$. $\square$

Extension: The proof is straightforward to extend when Ψ only includes a subset of possible values of the policy variables. This is useful for cases where one or more variables can take on numerous values and, potentially, are continuous at some points. The necessary assumption is that there exists a subset of values that have positive probability.[21] Here, I simply add a term to $\Pi(Z_i)$ for the probability that D_{it} does not equal one of the values in Ψ and a corresponding term to $\Gamma(Z_i, \beta)$:

$$\Pi_F(Z_i) \equiv \begin{bmatrix} \Pi(Z_i) & \begin{matrix} P(D_{i1} \neq d^{(1)}, \cdots, D_{i1} \neq d^{(M)}) | Z_i) \\ \vdots \\ P(D_{iT} \neq d^{(1)}, \cdots, D_{iT} \neq d^{(M)}) | Z_i) \end{matrix} \end{bmatrix}$$

$$\Gamma_F(Z_i, \beta) \equiv \begin{bmatrix} \Gamma(Z_i, \beta) \\ P(Y_{it} \leq D_{it}'\beta | Z_i, D_{it} \neq d^{(1)}, \cdots, D_{it} \neq d^{(M)}) \end{bmatrix}.$$

[21] Importantly, this assumption holds in the application of this paper since many households received rebates that were multiples of \$300.

Note that the above proof does not change when we analyze $E[(Z_{it} - \overline{Z}_i)\Pi_F(Z_i)\Gamma_F(Z_i, \breve{\beta})] = 0$.

Theorem 3.3 (Consistency)**.** *If **IV-A1** - **IV-A7** hold and* $\hat{A} \xrightarrow{p} A$ *positive definite, then* $\widehat{\beta(\tau)} \xrightarrow{p} \beta(\tau)$.

Note that the following conditions hold:

1. Identification holds by Theorem 3.2.
2. Compactness of $\mathcal{B}$ holds by assumption **IV-A6**.
3. $g_i(b)$ is continuous at each b with probability one under **IV-A4**.
4. $E\,||g_i(b)|| \leq \sup_t E\left|\left|Z_{it} - \overline{Z}_i\right|\right| < \infty$ by **IV-A7**.

Under these conditions, consistency follows immediately from Theorem 2.6 of Newey and McFadden (1994).

Theorem 3.4 (Asymptotic Normality)**.** *If **IV-A1** - **IV-A8** hold and* $\hat{A} \xrightarrow{p} A$ *positive definite, then* $\sqrt{N}(\widehat{\beta(\tau)} - \beta(\tau)) \xrightarrow{d} N\left[0, (G'AG)^{-1}G'A\Sigma AG(G'AG)^{-1}\right]$.

Define $\beta_0 \equiv \beta(\tau)$, $\hat{\beta} \equiv \widehat{\beta(\tau)}$.

Also, $\overline{G}(\beta) \equiv E\left[\frac{1}{T}\sum_{t=1}^{T}(Z_{it} - \overline{Z}_i)\left(D'_{it} + \frac{\partial\gamma_t(\tau,\phi)}{\partial\phi'}\right) f_Y(D'_{it}\beta|Z_i)\right]$.

$\overline{g}(\beta) \equiv E\left[\frac{1}{T}\sum_{t=1}^{T}\left(Z_{it} - \overline{Z}_i\right)\left[\mathbf{1}(Y_{it} \leq D'_{it}\beta)\right]\right]$.

Proof:

$\overline{g}(\beta)'A\overline{g}(\beta)$ is minimized at β_0 implying that

$$\overline{G}(\beta_0)'A\overline{g}(\beta_0) = 0.$$

Expanding each element of $\overline{g}(\beta_0)$ around $\hat{\beta}$ and multiplying by $\sqrt{N}$ gives

$$\sqrt{N}\overline{g}(\beta_0) = \sqrt{N}\overline{g}(\hat{\beta}) - \overline{G}(\tilde{\beta})\sqrt{N}(\hat{\beta} - \beta_0) \tag{18}$$

where $\tilde{\beta}$ meets the condition $\left|\left|\tilde{\beta} - \beta_0\right|\right| \leq \left|\left|\hat{\beta} - \beta_0\right|\right|$ and takes on different values for each column of $\overline{G}(\tilde{\beta})$.

By assumption of i.i.d and continuity, $\overline{G}(\tilde{\beta}) \xrightarrow{p} \overline{G}(\beta_0)$.

Focus on the $\sqrt{N}\overline{g}(\hat{\beta})$ term:

$$\begin{aligned} -\sqrt{N}\overline{g}(\hat{\beta}) &= \left[\sqrt{N}g_N(\hat{\beta}) - \sqrt{N}\overline{g}(\hat{\beta})\right] - \sqrt{N}g_N(\hat{\beta}) \\ &= \underbrace{\sqrt{N}\left[g_N(\hat{\beta}) - \overline{g}(\hat{\beta}) - g_N(\beta_0)\right]}_{(1)} + \underbrace{\sqrt{N}g_N(\beta_0)}_{(2)} - \underbrace{\sqrt{N}g_N(\hat{\beta})}_{(3)}. \end{aligned}$$

(1): Define empirical process $v_N(\beta) = \frac{1}{\sqrt{N}}\sum_{i=1}^{N}\left[g_N(\beta) - \overline{g}(\beta)\right]$.

The functional class $\left\{\mathbf{1}(Y_{it} \leq D'_{it}b), b \in \mathbb{R}^k\right\}$ is Donsker and the Donsker property is preserved when the class is multiplied by a bounded random variable (see Theorem 2.10.6

in van der Vaart and Wellner (1996)[22]). Thus,

$$\left\{\frac{1}{T}\sum_{t=1}^{T}\left(Z_{it}-\overline{Z}_i\right)\left[\mathbf{1}(Y_{it}\leq D_{it}'b)\right], b\in\mathbb{R}^k\right\}$$

is Donsker with envelope $2\max_{(i,t)}|Z_{it}-\overline{Z}_i|$. Stochastic equicontinuity of $v_N(\cdot)$ follows from **IV-A7** and Theorem 1 in Andrews (1994). Stochastic equicontinuity and consistency of $\hat{\beta}$ implies that part (1) is $o_p(1)$.

(2): By the Central Limit Theorem, $\sqrt{N}g_N(\beta_0)\xrightarrow{d}N(0,\Sigma)$ where $\Sigma=E[g(\beta_0)g(\beta_0)']$.

(3): By consistency of $\hat{\beta}$, $\sqrt{N}g_N(\hat{\beta})=o_p(1)$.

Plugging into (18) and using the assumption that $G'AG$ nonsingular

$$\sqrt{N}(\hat{\beta}-\beta(\tau))\xrightarrow{d}N\left[0,(G'AG)^{-1}G'A\Sigma AG(G'AG)^{-1}\right]$$

[22]See Example 2.10.10 as well.

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