Does Labor Supply Respond to Transitory Income? Evidence from the Economic Stimulus Payments of 2008*

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Abstract

This paper studies household labor supply responses to transitory income, exploiting the differential timing of the 2008 economic stimulus payments. Tax policy is often used to encourage consumer spending in recessions and an influential literature finds evidence that households increase spending in response to receipt of tax rebates. The literature has largely ignored the possible ramifications of rebates on labor supply, though rebates may crowd out labor earnings as households consume additional leisure. The responsiveness of labor supply to transitory income has been underexplored more broadly so it is difficult to predict this labor supply effect. I find that each rebate dollar reduces monthly earnings by 9 cents on average in the short-term with smaller but significant lagged effects up to three months after rebate receipt. Since rebates can be targeted based on earnings, this paper estimates the impact of rebates on the monthly earnings distribution. I find significant heterogeneity with the responsiveness primarily concentrated in the lower half of the distribution. This effect is driven by hourly workers and married couples. The results also suggest that liquidity constraints are an important source of the transitory labor supply responses. I present complementary evidence that rebate receipt increases the probability of unpaid absences from work but not involuntary layoffs or missing work due to illness, consistent with a short-term increased demand for leisure. The results imply that the $96 billion in stimulus payments had a partial equilibrium effect of reducing short-term national labor earnings by over $26 billion.

Keywords: tax rebates, labor supply, transitory income, quantile treatment effects
JEL classification: H31, J22

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1 Introduction

A rich labor supply literature provides estimates of wage elasticities and wealth effects, but there is little evidence of short-term labor supply responses to transitory income. Transitory labor supply responses are potentially important for many policies and especially relevant in the context of stimulus payments distributed in the midst of a recession. Policymakers often use fiscal policy in recessions, such as the distribution of tax rebates, to encourage household spending and increase aggregate demand. Recent work has studied the 2001 and 2008 tax rebates in the United States to show that household expenditures respond in the short-term to rebates (e.g., Johnson, Parker, and Souleles (2006); Agarwal, Liu, and Souleles (2007); Parker, Souleles, Johnson, and McClelland (2013); Misra and Surico (2014); Broda and Parker (2014); Kaplan and Violante (2014)), typically exploiting the quasi-randomized timing of rebate receipt. While the literature provides evidence that tax rebates stimulate spending, this transitory income could also encourage households to consume additional leisure, causing the rebates to crowd out short-term labor earnings. The ramifications on household labor supply and, by extension, production could offset some of the gains associated with the increases in consumer spending. Given the lack of evidence on transitory labor supply responses more generally, it is difficult to predict the potential of rebates to reduce household labor supply.

This paper studies the effects of the 2008 economic stimulus payments (ESPs) on short-term household labor earnings using monthly data from the Survey of Income and Program Participation (SIPP). The Economic Stimulus Act of 2008 included the distribution of stimulus payments totaling almost $100 billion to approximately 130 million tax filers in the United States. The payments were primarily issued between April and July with exogenous variation in the timing based on the last 2-digits of the head-of-household's Social Security number. I compare labor earnings of households that received their payments in different months while leveraging the panel nature of the SIPP to condition on household fixed effects, estimating a causal relationship between rebates[1] and household labor supply behavior. While tests of the permanent income hypothesis (Friedman (1957)) often focus on expenditures (see Jappelli and Pistaferri (2010) for a recent review of methods), this paper examines whether transitory income affects short-term leisure consumption. The economic literature provides little evidence about the relationship between transitory income and labor

[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 use "tax rebates" and "economic stimulus payments" interchangeably.

supply; this paper fills this gap.

Understanding the relationship between household labor supply and transitory income is important for our knowledge of labor supply behavior. Moreover, the timing and frequency of payments through numerous government programs may produce transitory labor supply responses. For example, the Earned Income Tax Credit (EITC) provides a single annual lump sum payment while Social Security and Temporary Assistance for Needy Families (TANF) provide monthly payments. Government payments and, more broadly, earned and unearned income have a transitory component in the sense that there is a specific time in which the payments are received. There is little research which isolates this transitory component from the other incentives and wealth effects associated with these payments. Given the non-uniform timing of government expenditures, such payments potentially represent an important determinant of variation in national labor supply. The permanent income hypothesis posits that short-term household behavior should not respond to small one-time payments beyond their effects on lifetime wealth. The evidence in this paper rejects this hypothesis in the context of labor supply and suggests significant scope for the timing of government payments to affect short-term household labor supply.

To quantify the effects of the 2008 ESPs on household labor supply, I estimate quantile treatment effects (QTEs) using a quantile regression method for panel data which conditions on nonadditive household fixed effects. The labor supply effects of tax rebates may not be constant throughout the earnings distribution and understanding these potentially heterogeneous effects is important given that such policies are often targeted based on earnings. Low-earning households may be more likely to use rebates as substitutes for earnings than high-earning households and estimating a homogenous effect could miss crucial distributional heterogeneity. 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 heterogeneous effects provide useful information missed by mean regression techniques (Bitler, Gelbach, and Hoynes (2006)). Furthermore, monthly labor earnings are subject to topcoding in the data, which biases estimates from traditional mean estimators. Quantile treatment effects are consistent at uncensored points of the distribution and flexibly describe heterogeneous effects.

I estimate that each rebate dollar causes households to reduce monthly labor earnings by 9 cents, on average, in the month of receipt and month after receipt. I also find statistically significant but smaller lagged effects in the subsequent two months. The quantile treatment effect estimates imply significant heterogeneity throughout the earnings distribution. There is little evidence that the top of the distribution responds to rebate receipt but strong evidence of labor supply responsiveness between the 25^{th} and 50^{th} percentiles. These quantiles correspond to $800-$2,400 in monthly earnings for single heads of households and $3,300-$5,900 for married couples. For this part of the distribution, each additional stimulus dollar received reduced household labor earnings by 10 to 15 cents per month in the short-term. The estimates imply that the stimulus payments reduced short-term national labor earnings by over $26 billion.

This paper includes further analysis of the mechanisms underlying the relationship between rebates and labor supply. I find that the overall labor supply reductions are not driven by changes on the extensive margin, defined as having positive household labor earnings in a month. Instead, the results are consistent with workers reducing hours or effort on the intensive margin in response to rebate receipt. Households with hourly workers appear to be the primary driving force behind the results as I find little effect for households with no hourly workers. Furthermore, the results are consistent with a "secondary earner" effect as married couples are more responsive to rebates than single heads of households. I also find evidence consistent with the hypothesis that liquidity constraints are an important determinant of household responsiveness.

The SIPP provides detailed information about work absences and reasons for work absences, and I use this information to provide complementary evidence about the relationship between rebate receipt and voluntary reductions in work effort. I find that rebate receipt increases the probability of voluntary unpaid absences from work, but is not related to involuntary absences and absences due to injury or illness. These results are consistent with the hypothesis that rebates led to short-term increased demand for leisure. I also find little evidence that workers later increased their labor supply, implying that the reduction in labor earnings is not a temporal shift of future work absences.

In the next section, I provide more information about the 2008 ESA and discuss the literature on tax rebates and economic stimulus payments. Section 3 describes the data and empirical strategy. The main results are presented in Section 4. Section 5 includes several robustness checks while Section 6 explores mechanisms driving the overall effect. Section 7 discusses the implications of the results.

2 Background

2.1 Economic Stimulus Act of 2008

President George W. Bush signed the Economic Stimulus Act of 2008 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, an historically large amount. For comparison, the 2001 tax rebates totaled $38 billion. The total amount distributed through the 2008 stimulus payments represented 0.8% of 2008 personal income and 8.7% of federal tax payments (Sahm et al. (2010)).

Households were eligible for ESPs in 2008 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. For all income levels, households received an additional $300 payment 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. While rebate eligibility and amounts were calculated using 2007 tax returns, the rebates were technically reductions in taxes owed for 2008 and refundable. Households that received larger rebates than they should have given their 2008 tax returns were not required to repay the additional amount that they received.

The Treasury Department began issuing rebates to households on April 28, 2008 and the payments were staggered over time. Households received payments electronically or by mail, depending on whether the tax filer provided the IRS with a bank routing number in their 2007 tax return. In the SIPP, 45% of respondents report receiving their rebate electronically. Payments were scheduled between April and July, according to the last 2-digits of the tax filer's Social Security number. Table 1 presents the rebate schedule. Households that filed their 2007 tax returns late may have received their rebates late.

2.2 Related Literature

2.2.1 Responses to Tax Rebates

An influential literature has found that tax rebates have behavioral effects that are difficult to reconcile with the permanent income hypothesis, which suggests that small one-time payments should have little effect on current consumption. 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, Parker, and Souleles (2006) and Parker et al. (2013) use household spending data in the Consumer Expenditure Survey and find increases in consumer spending on non-durable goods during the quarter of rebate receipt for the 2001 tax rebates and 2008 economic stimulus payments, respectively. Johnson, Parker, and Souleles (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. Broda and Parker (2014) studies the spending response of households to receipt of the 2008 tax rebates using high-frequency scanner data and finds results complementary to the conclusions of Parker et al. (2013). This additional household spending is a necessary condition for tax rebates to increase aggregate demand. While a number of papers have examined consumer spending, this is the first paper to study labor supply.

Prior studies have also recognized the policy importance of heterogeneous responses to tax rebates. Agarwal, Liu, and Souleles (2007) studies credit card accounts and finds that consumers, on average, initially saved the 2001 tax rebates but later increased their spending. Liquidity constrained consumers were most responsive. Misra and Surico (2014) adopts a quantile framework to investigate the impacts of both the 2001 tax rebates and the 2008 economic stimulus payments on the distribution of changes in consumption. Kaplan and Violante (2014) studies the 2001 tax rebates by developing a structural model where responsiveness to rebates depends on liquid wealth, finding that the behavior of households with sizable quantities of illiquid assets are an important driver of the magnitude of the response.

I estimate distributional effects of the rebates using quantile estimation, but my approach differs from the previous literature for two primary reasons. First, the tax rebate literature using mean regression techniques accounts for household fixed effects by estimating the specification in first-differences. Misra and Surico (2014) follows this approach by differencing the data and and *then* applying quantile regression. The resulting estimates provide information about the relationship between tax rebates and the distribution of the first-difference in consumption (ΔC), not the distribution of consumption (C). I estimate differential responses throughout the labor earnings distribution (i.e., how low-earning households respond to tax rebates), rather than the first-differenced labor earnings distribution. Studying the distribution of labor earnings provides information relevant to policy that is more likely to target households based on earnings than on changes in earnings.

Second, the tax rebate literature discusses the importance of conditioning on household fixed effects for identification. In a mean estimation framework, first-differencing eliminates the household fixed effect. In a quantile framework, first-differencing does not necessarily account for fixed differences across households, except under restrictive assumptions.[2] The estimator discussed in Section 3.2.3 conditions on household fixed effects in an unrestrictive manner so that the estimates can be interpreted as the effects of rebates on the distribution of earnings.

The literature discussed up to this point estimates consumption changes using expenditure data. A related literature has used survey evidence to understand household responsiveness to rebates. In response to the 2001 tax rebates, Shapiro and Slemrod (2003a) and Shapiro and Slemrod (2003b) find that most households plan to pay off debt (46.2%) with a smaller fraction reporting that they plan to mostly save the rebate (32.0%) or mostly spend the rebate (21.3%). Shapiro and Slemrod (2009) and Sahm et al. (2010) report similar evidence for the 2008 stimulus payments with about 20% of survey respondents saying that they will "mostly spend" their rebate.[3] These surveys do not ask if respondents plan to reduce their labor supply in response to the rebate, except to the extent that respondents equate spending their rebates with purchasing leisure.

The tax rebate literature has generally ignored the possibility that individuals could purchase leisure and reduce earnings in response to tax rebate receipt. This possible outcome is policy-relevant given that stimulus payments may have the unintended result of reducing short-term household labor supply with ramifications for aggregate production. Furthermore, the scope for increasing aggregate demand is muted when tax rebates crowd out labor earnings.

Following the tax rebate literature, I exploit differences in rebate timing to estimate causal effects of the rebates. This identification strategy has also been used to study outcomes other than household spending (e.g., payday loans in Bertrand and Morse (2009); bankruptcy filings in Gross, Notowidigdo, and Wang (2014); health care demand in Gross and Tobacman (2014)). As much of the tax rebate literature points out, comparing individuals receiving tax

[2] Graham et al. (2009) shows that there is no incidental parameters problem in a quantile model and differencing is valid with additive fixed effects when there are no heterogeneous effects (i.e., the effect is constant throughout the distribution).

[3] The SIPP includes a similar survey question, and I find similar responses. 28.9% report mostly spending their rebate, 17.4% report mostly saving, and 53.7% report using their rebate to pay down debt.

rebates at different times can only isolate partial equilibrium effects and ignores multiplier effects which determine the total effect on aggregate demand. In the context of labor supply, a household-level reduction in labor supply is suggestive of an aggregate supply shock in the economy. However, the magnitude of this aggregate supply shift is a function of not only the household labor supply response, but also the ability of firms and the economy to substitute for temporary household-level reductions in labor supply and the relationship between labor supply and output. The partial equilibrium effect is an important component of the total effect and interesting more broadly for understanding whether transitory income affects household labor supply decisions. The value of the approach in this paper is that it allows us to isolate transitory household responses from the broader aggregate demand and supply effects of the stimulus package.[4]

Furthermore, it is difficult in the tax rebate literature to distinguish between additional spending and temporal shifting of spending. The focus of the literature and of this paper is to understand the short-term behavioral responses to rebates. Labor supply reductions in response to tax rebate receipt are inconsistent with the permanent income hypothesis regardless of whether there is compensating behavior in the future. In the context of this paper, the literature's empirical strategy makes it difficult to infer whether households reduce their labor earnings such that this work effort is permanently lost or whether tax rebates temporally shift future work absences. I will study lagged responses which could potentially include "bounce back" labor supply effects, but the primary focus of this paper and the literature is to understand short-term behavioral responses regardless of longer-term compensating behavior.

2.2.2 Labor Supply

This paper also contributes to our understanding of the determinants of labor supply and is related to the literature on the labor supply consequences of unearned income or wealth shocks, which is addressed in contexts such as lottery winners (Imbens et al. (2001); Cesarini et al. (2015)), inheritances (Holtz-Eakin et al. (1993); Joulfaian and Wilhelm (1994); Brown et al. (2010)), Social Security wealth (Krueger and Pischke (1992)), and the negative income tax experiments of the 1970's (see discussion in Pencavel (1986)). Given that these interven-

[4]If rebate receipt increases short-term demand for leisure, an individual may work fewer hours. This reduction in work effort does not necessarily translate to fewer hours at the firm-level (or economy-level) if firms can substitute hours from other workers. Thus, to understand the household-level behavioral response, it is important to isolate the household decision from firm-level compensating behavior. The empirical strategy of this paper isolates the household response.

tions tend to be large or longer-term and the empirical outcomes are often more permanent measures of labor supply behavior, these studies do not address the relationship between transitory income and leisure consumption.

The labor supply literature also estimates the labor supply effects of transitory changes to wage rates (e.g., Camerer et al. (1997); Farber (2005); Fehr and Goette (2007)) while Keane (2011) and Keane and Rogerson (2012) discuss the labor supply implications of transitory wage shocks in lifecycle models. Understanding labor supply responses to transitory wage and income shocks is a critical component of estimating lifecycle models with labor supply (e.g., MaCurdy (1981); Heckman and MaCurdy (1980)). In these models, the transitory components of changes in wages and income must be estimated (and assumed exogenous) by separating permanent income from transitory shocks.

The permanent income hypothesis states that households should not respond in the short-term to small, transitory income shocks beyond their (very small) effect on lifetime wealth. Much of the literature testing the permanent income hypothesis focuses on expenditure smoothing, providing little evidence about the marginal propensity to consume leisure out of transitory income. One exception, in the unemployment insurance context, is Chetty (2008) which finds evidence that labor supply is "excessively sensitive" to transitory income (in the form of severance payments) due to liquidity constraints. This result is specific to the extensive margin (unemployment duration). In contrast, the results in this paper strongly suggest that the intensive margin drives the labor supply response to rebates.

More broadly, all payments have a transitory component to them in the sense that households receive the payments at discrete times. For example, numerous government programs, such as the EITC and Social Security, provide payments to households. These programs influence household behavior through substitution effects (moral hazard) because they encourage specific types of behavior (e.g., the EITC encourages labor force participation) and through wealth effects since the programs increase lifetime wealth. The tax rebates studied in this paper were one-time payments which were generally not dependent on household behavioral decisions (a discussion of possible substitution effects is included in Section 3.3.2) and were small relative to lifetime income, allowing the empirical strategy to isolate the effect of simply receiving the payment. The timing and frequency of payments in government programs are policy choices, but we know little about the ramifications for labor supply of, for example, providing large one-time annual payments to EITC recipients relative to the consequences of switching to higher frequency (and smaller) payments. While many government programs potentially have important effects on the timing of labor supply in the economy, there is little empirical research to inform this issue. The magnitude of the responsiveness of labor supply on a monthly basis estimated in this paper implies meaningful scope for short-term behavioral changes to policy.

3 Data and Empirical Strategy

3.1 Data

I use the 2008 panel of the Survey of Income and Program Participation (SIPP). The SIPP is a rich nationally-representative 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." I use earnings information from the first two waves and leverage the longitudinal nature of the data by linking earnings across time for each household. The interviews are staggered across households within a wave such that the sample itself spans more than eight months; the first two waves provide earnings data beginning in May 2008 and ending in March 2009.

The first two waves of the 2008 SIPP included a special module of questions about the economic stimulus payments. Households reported the size and timing of any rebates received as part of the economic stimulus package. The survey highlighted to respondents that the stimulus payments were different from the tax refunds received in relation to the household's annual income taxes. Using the weights provided in the SIPP to generate national estimates, the total stimulus payments reported in the SIPP account for 96% of the total stimulus payments reported by the Department of the Treasury. I use the tax rebate information along with the monthly household labor earnings information to construct the key variables for this paper's analysis.

I focus on labor earnings as the primary outcome because changes in labor earnings encompass a host of behavioral responses such as hours worked and effort. Furthermore, labor earnings are an important source of household income and policies often target households based on earnings, not hours or effort. I study household earnings rather than individual earnings because spouses can coordinate labor supply reductions and policy is often targeted based on household earnings. Monthly labor earnings are topcoded in the SIPP, with some exceptions, at $12,500.[5] This topcoding further motivates the use of a quantile framework since the quantile treatment effects are robust to this censoring while mean estimates are known to be biased.

The SIPP also provides detailed information about labor force status, work absences, and reasons for work absences. I use this information to study the relationship between rebate receipt and descriptive measures of labor force behavior. The variables related to reasons for work absences are helpful to distinguish between voluntary work reductions and involuntary reasons such as illnesses and temporary layoffs. The hypothesis of this paper is that transitory income affects short-term preferences for leisure such that we should observe voluntary changes in behavior, not involuntary changes in labor force status. Unfortunately, the SIPP includes only limited information about hours worked. While it would be useful to observe the number of hours that each individual worked at high frequency intervals, the SIPP provides the number of hours *usually* worked per week during the four month reference period covered by a wave.[6] It is unclear whether temporary reductions in labor supply would be reflected in this variable.

I select on households with non-missing labor earnings data for all eight months in the first two waves.[7] I exclude cohabiting non-married couples due to the possible receipt of multiple stimulus payments. Finally, I require the head of the household to be between 25 and 60 years old. Table 2 provides summary statistics by initial marital status.[8] My sample includes 9,721 households with a single head of the household and 13,277 households with a married couple. Table 2 shows evidence of small differences in labor earnings by rebate status. 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 appears to have higher labor earnings than the rest of the sample. These differences, though relatively small, reinforce the benefits of

[5]While it is possible to link the SIPP to restricted administrative earnings records, these records only provide information on annual earnings, not monthly earnings.

[6]The SIPP also includes an imputed monthly variable which broadly categorizes monthly labor supply. This variable is imputed primarily using the usual hours worked by wave and likely does not contain precise enough information to identify transitory behavioral responses.

[7]Almost all (87%) of the sample reports labor earnings for all 8 months. A small portion, 9.5%, of the sample is only in one of the waves but reports for all 4 months in that wave. The analysis excludes these households and the remaining households which report for fewer than 8 months.

[8]Marital status changes over the course of the eight month observation period in the data are rare. I observe 22,998 households, and there are 147 marital status changes.

conditioning on household fixed effects and identifying off of within-household changes in receipt of a rebate.

In the sample, 77% of single households and 85% of married couples received a rebate. Among those who received a rebate, the average amount received was $580 for singles and $1,096 for married couples. Though the rebate was phased out based on income, there were still high levels of receipt throughout the 2008 income distribution. Even for households with monthly earnings[9] of at least $12,000, the fraction of households receiving rebates in 2008 was 46% for singles and 68% for married couples. Households were given tax credits per child so even with the 5% phase out, households with high incomes would still receive a rebate. Moreover, rebates were calculated based on 2007 income and did not have to be repaid if the household earned more in 2008. Appendix Section A.1 presents more information about the distribution of rebates based on earnings.

While seam bias in the SIPP has improved greatly beginning with the 2004 panel (Moore et al. (2009)), there is still some concern that reported labor earnings will not respond immediately to actual changes in labor earnings. Any such "seaming" will likely attenuate the estimates since I will observe fewer labor earnings responses to tax rebates than actually occur (and those changes will be reported later, after the household has received the rebate). I control for month-relative-to-the-interview-month fixed effects to reduce concerns of seam bias. I will also test for the importance of seam bias by limiting the sample to interview months in Section 5.4. I find evidence that my primary estimates are indeed conservative.

3.2 Empirical Strategy

I use the differential timing of rebate receipt to estimate the relationship between tax rebates and monthly household labor earnings. I present evidence of this relationship using three complementary estimation strategies.[10] Each of these estimation techniques has advantages and limitations, which I discuss in detail below. First, I estimate the effects using mean regression, which is the approach typically used in tax rebate literature but is less appropriate in the context of this paper given the use of topcoded earnings data. Second, I estimate the cumulative distribution function. This approach should be robust to topcoding concerns. Finally, I estimate quantile treatment effects, which will quantify the effects of the rebates

[9]December 2008 earnings were used to generate these numbers since earnings in this month were likely "untreated" by tax rebates given the timing of the rebates.

[10]I use the SIPP sampling weights in all analyses.

on the distribution of labor earnings. I discuss each of these approaches in more detail in this section. For all estimation methods, standard errors are adjusted for within-household clustering.

3.2.1 Mean Regression

First, I use 2SLS to estimate the mean effect of rebates on labor earnings. This approach is similar to the one used in both Johnson, Parker, and Souleles (2006) and Parker et al. (2013). The SIPP provides higher frequency (monthly) labor earnings data so I include an explanatory variable for the rebate and a lagged rebate variable.[11] I provide 2SLS estimates of the following specification:

$$L_{it} = \alpha_i + \gamma_{h(i)t} + \beta_1 R_{it} + \beta_2 LR_{it} + \epsilon_{it}, \tag{1}$$

where L_{it} represents earnings for household i in month t. R_{it} represents the total rebate amount received in months t and $t-1$, which allows the rebate to have an effect in the month of and month after receipt. LR_{it} represents the lagged rebate variable referring to the total rebate amount received in months $t-2$ and $t-3$, which permits the rebate to have a separate effect two to three months after receipt. The specification includes additive household fixed effects (α_i) to account for fixed differences across households.[12] $\gamma_{h(i)t}$ represents an interaction term based on household size,[13] marital status,[14] month,[15] and month-relative-to-interview-month.[16] To identify solely off of rebate timing differences, I follow the literature and use excluded instruments $\mathbf{1}(R_{it} > 0)$ and $\mathbf{1}(LR_{it} > 0)$.[17] The motivation behind these instruments is that the rebate dollar amount varies based on household characteristics and prior income even among households receiving rebates at the same time. This additional

[11] Johnson, Parker, and Souleles (2006) and Parker et al. (2013) also include lagged effects in some specifications.

[12] The literature typically uses first-differences, but I adopt a fixed effect specification because of the higher-frequency data and because it is more comparable to the quantile estimator discussed in Section 3.2.3 which conditions on household fixed effects.

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

[14] Given the rarity of marital status changes and the unlikelihood that such changes result from the differential timing of tax rebates, the interaction terms use marital status in that month, allowing marital status to change over time for a household. I have estimated specifications where marital status is held fixed and the results do not change in a meaningful manner.

[15] This refers to the calendar month and year.

[16] Since each wave covers 4 months, there are four possible values for month relative to the interview month. Allowing the interactions to vary based on this variable should reduce concerns of seaming bias, as discussed in Section 3.1.

[17] See Johnson, Parker, and Souleles (2006) and Parker et al. (2013) for two examples.

information potentially predicts changes in labor supply behavior over time. This source of variation is eliminated by instrumenting with indicator variables determined solely by rebate receipt timing.

3.2.2 Conditional Cumulative Distribution Function Estimation

Second, I estimate the effect of rebates on the cumulative distribution function of labor earnings. This approach involves estimating a series of relationships linking tax rebates and the probability that the household's labor earnings are below a certain threshold, represented by y. This method follows recent applied work which estimates a linear probability model where the dependent variable is equal to 1 if the outcome is smaller than some value (see Cabral and Mahoney (2014); Hoynes and Patel (2015) for two recent examples). An advantage of this approach is that estimating $P(L_{it} \leq y)$ is robust to topcoding when y is smaller than the censoring point. The specification is:

$$\mathbf{1}(L_{it} \leq y) = \alpha_i(y) + \gamma_{h(i)t}(y) + \beta_1(y)R_{it} + \beta_2(y)LR_{it} + \epsilon_{it}(y), \tag{2}$$

where I index the parameters and error term by y and estimate for income thresholds $y = 100, 200, 300, \ldots, 12000$. The specification includes household fixed effects and the same interaction terms as before. I estimate equation (2) using 2SLS with excluded instruments $\mathbf{1}(R_{it} > 0)$ and $\mathbf{1}(LR_{it} > 0)$.

There are a few disadvantages to this approach that motivate the use of a quantile framework. First, this specification assumes that the probability is a linear function of the explanatory variables. Nonlinear estimators (e.g., probit regression) often require alternative distributional assumptions and may suffer from incidental parameters problems with household fixed effects and small T (the number of time periods). Second, the parameters in specification (2) vary based on y and the specification assumes a uniform effect for all households at y (i.e., the estimates aggregate the effects of small households with one earner and large families with two earners at the same income level).[18] This approach ignores that the observed distribution is itself treated, and we may think that the responsiveness to a rebate of a household with annual earnings around, say, \$30,000 is different for a one person household than a large household with two earners.

Quantile estimation addresses each of these concerns, providing complementary evi-

[18]The interaction terms which vary based on household size and marital status shift the index (probability) additively.

dence to the estimates derived from equation (2). The estimator discused in the next section produces estimates that are consistent for small T and no distributional assumptions are necessary. Furthermore, the distribution of earnings is allowed to shift based on family size, marital status, and time through the inclusion of interaction terms in the quantile function based on these factors. The treatment effects vary based on the household's placement in the distribution given these interaction terms. Finally, the quantile treatment effects are straightforward to interpret as the effect of a rebate dollar on the τ^{th} quantile of the earnings distribution (and not as changes in probabilities of earnings lower than some threshold).

3.2.3 Quantile Treatment Effect Estimates

I use quantile estimation for the main estimates in this paper. Quantile regression methods have proven valuable in estimating the distributional effects of policies, permitting the effects of the policy variables in the quantile function to vary based on a nonseparable disturbance term. For $\tau \in (0, 1)$, I estimate the quantile function:

$$S_{L_{it}}\left(\tau|\gamma_{h(i)t}, R_{it}, LR_{it}\right) = \gamma_{h(i)t}(\tau) + \beta_1(\tau)R_{it} + \beta_2(\tau)LR_{it}. \tag{3}$$

For example, $\gamma_{h(i)t}(0.5)+\beta_1(0.5)R_{it}+\beta_2(0.5)LR_{it}$ represents the median of the earnings distribution given the rebate variables, household size, marital status, month, and month-relative-to-interview. Importantly, this quantile function does not include an additive household fixed effect, though the estimator discussed below will condition on (nonadditive) household fixed effects to estimate equation (3). These nonadditive fixed effects are important for identification and appropriate interpretation of the estimated quantile functions. $\gamma_{h(i)t}$ is the set of interaction terms based on household size, marital status, month, and month relative to the interview month. These interactions permit the earnings distribution to shift based on these factors. For example, if I did not adjust for marital status, the quantile estimates for the top quantiles would primarily refer to married couples given the likely higher earnings for two earner households relative to single earner households.[19] The interactions also account for secular trends in the economy.[20] These are especially important given the major changes

[19]If we believe that low-earning households respond more (or less) than high-earnings households, then we want to shift the distribution based on – empirically – what a "low-earning" household is given family size and marital status. These interaction terms are necessary for this interpretation.

[20]As an additional conceptual example for the importance of allowing the distribution to shift based on these interaction terms, imagine studying the effects of rebates over a time period with hyperinflation. Instead of assuming that the treatment effects vary based on y (as in CDF estimation), which would lump together high earners in the early part of the sample and low earners in the later time period, we would want the effects to vary based on placement in the distribution within each time period.

in national labor earnings in the United States during this time period due to the Great Recession. Appendix Section A.2 provides more discussion of these secular trends to provide context for the economic environment after the rebates were distributed. As before, I use excluded instruments $\mathbf{1}(R_{it} > 0)$ and $\mathbf{1}(LR_{it} > 0)$. The quantile treatment effect estimates of interest are the estimates of $\beta_1(\tau)$ and $\beta_2(\tau)$ in equation (3).

The identification strategy requires conditioning on household fixed effects and identifying off of within-household changes in rebate receipt. Many quantile panel data methods include an additive fixed effect in the quantile function (e.g., Koenker (2004), Harding and Lamarche (2009), Lamarche (2010), Canay (2011), Galvao Jr. (2011), Ponomareva (2011), and Rosen (2012)). However, in a quantile framework, this changes the interpretation of the estimates as the estimated parameters in a model with additive fixed effects (α_i) refer to the effects of the policy variables on the distribution of $L_{it} - \alpha_i$, not the distribution of L_{it}. Observations at the top of the $L_{it} - \alpha_i$ distribution may be at the bottom of the L_{it} distribution. One motivation for using quantile methods is that it permits the treatment effects to vary based on a nonadditive disturbance term. Including *additive* fixed effects 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.

This paper uses quantile regression for panel data (QRPD), introduced in Powell (2016), which conditions on *nonadditive* fixed effects. This estimator has two primary benefits in this context. First, it estimates the function represented in equation (3) and does not require including an additive fixed effect term in the structural quantile function. In other words, it conditions on household fixed effects without altering the quantile function, maintaining the nonadditive disturbance term typically associated with quantile models (the disturbance term is an arbitrary function of unobserved fixed effects and observation-specific disturbance terms).[21] Second, identification originates solely from within-household variation in the instruments over time, the same source of variation used by mean regression, non-parametrically accounting for fixed differences across households. A more technical discussion of QRPD is included in Appendix Section B. QRPD produces consistent estimates of quantile treatment effects for small T, which is important in this context since I only use

[21] If rebates were (unconditionally) randomized (both the receipt and amount), then equation (3) could be estimated using quantile regression. Just because it is necessary to condition on household fixed effects does not imply that we also want to estimate a different quantile function, which would be necessary given other panel data quantile estimators.

eight months of data for each household.

3.3 Additional Considerations

3.3.1 Topcoding

Topcoding in the SIPP can bias estimates generated using mean regression. Even methods that directly account for such censoring by enforcing distributional assumptions on the error term, such as Tobit regression, are problematic because the empirical strategy requires conditioning on household fixed effects for small T. While the motivation for using QRPD instead of a quantile estimator with additive fixed effects is primarily conceptual, applying quantile estimators with additive fixed effects is also difficult within this context given incidental parameters concerns.[22] QRPD produces consistent estimates even in the presence of censoring, nonadditive fixed effects, and small T.

Quantile treatment effect estimates are consistent at uncensored parts of the distribution and my analysis could proceed by estimating quantile functions where the functions are uncensored for the entire sample. Given that family size, marital status, and month predict both topcoding and having no labor earnings,[23] I select on households based on these interactions to estimate quantile treatment effects at quantiles that are potentially censored for some types of households but not for others. For example, married couples are much more likely to have topcoded earnings than single households. At the top quantiles, I can eliminate married couples from the analysis to ensure that the quantile function itself is not censored. This selection is not problematic since I am selecting on a variable that is exogenous in the specification and parallels censored quantile regression techniques which select on observations with non-censored quantile functions (e.g., Chernozhukov and Hong (2011)).

In the context of this paper, this selection is especially straightforward given that I actually observe the untreated distribution for each interaction of family size, marital

[22] To my knowledge, there is no instrumental variable quantile panel data estimator other than QRPD which produces consistent estimates for small T.

[23] I treat observations as censored at zero as well. Households with no earnings cannot reduce their labor earnings further due to rebate receipt so any effect is censored. Alternatively, it is arguable that we want to capture this "zero effect" in our estimates. Modeling labor earnings censored at zero has little effect on the estimates in this paper since I do not estimate significant responsiveness at the very bottom of the labor earnings distribution, which is consistent with the evidence presented later that rebates do not operate through the extensive margin. The conclusions of this paper are the same if I do not treat earnings as censored at zero.

status, month, and month-relative-to-interview since each of these cells includes households not receiving rebates. Thus, I exclude observations based on whether the untreated ($\mathbf{1}(R > 0) = \mathbf{1}(LR > 0) = 0$) distribution for a given family size, marital status, month, and month-relative-to-interview is itself censored or would likely be censored if the rebate variables were not equal to zero. This approach allows analysis closer to the bottom and top of the earnings distribution.[24] Most of the quantile estimates use the full sample.[25]

3.3.2 Substitution Effects of Tax Rebates

As discussed in Section 2.1, the rebates were phased out at high levels of income, generating a small positive tax rate on earnings. I ignore any substitution effects due to this positive tax rate for two primary reasons. First, the tax rate is small – 5% for those affected – and it affects a relatively small fraction of households, primarily at the top of the earnings distribution. Also, many high income households were unaffected because the rebate amounts distributed were based on 2007 tax filings, but households were not required to repay any excess payments when they filed their 2008 taxes, implying a 0% tax rate associated with the rebates. I will estimate statistically significant effects of rebates on earnings in the lower part of the earnings distributions. These households likely did not experience any positive tax rate due to the rebates.

Second, the *timing* of the rebate does not impact the tax rate or whether the household is subject to the phaseout. The identification strategy of this paper relies on variation in the timing of rebate receipt, which should be orthogonal to any positive tax rates generated by the policy.[26]

3.3.3 Interpretation of Estimates

This paper is focused on short-term household labor supply behavioral responses to transitory income shocks. For understanding the broader impacts of tax rebates, a household-level reduction in labor supply is a necessary condition for reductions in labor supply at the

[24] The SIPP topcodes *individual* earnings. Household earnings for married couples may be censored at $12,500 if only one individuals work or at higher values if both work. Since the lowest possible topcoding value for married couples is $12,500, I assume that censoring affects quantile functions at this amount.

[25] A simply way to test whether topcoding is affecting the QTE estimates is to reassign topcoded values to arbitrarily larger values. The estimates in this paper are robust to this test.

[26] If receipt of payment triggered salience of the additional end-of-year tax rate (to those that it applied to) and the treated households responded to variation in the salience of the tax rate and did so at different times, then there may be a confounding substitution effect. For the other reasons provided, this is unlikely to affect the estimates of this paper.

firm- or national-level. The tax rebate literature has generally recognized that quantifying the total effects of tax rebates requires information about the general equilibrium effects of the rebates. Quantifying the magnitude of this general response would require additional knowledge of the ability of firms or the economy to substitute for voluntary household-level labor supply reductions. Furthermore, the rebates impact aggregate demand which should affect labor supply opportunities. The estimates of this paper must be interpreted as partial equilibrium effects. The benefit of this approach is that it isolates the household-level behavioral responses from the general shifts in aggregate demand.

Moreover, consistent with the empirical strategy found in the literature, it is difficult to distinguish between an increase in household spending (leisure) and a temporal shift in spending (leisure). With labor supply, households may plan to save now for a future, temporary reduction in labor earnings. The rebate permits them to take the work absence earlier. Alternatively, the short-term labor supply reductions represent a permanent loss of earned income. In Section 5.3, I try to distinguish between these mechanisms, though both reflect short-term behavioral changes in response to tax rebates.

4 Main Results

4.1 Mean Regression Estimates

The first stage relationship between the instruments and the rebate variables is shown in Appendix Section A.3. Given that the instruments are a function of the rebates themselves, it is not surprising that there is a strong relationship between the excluded instruments and endogenous variables.

I present the mean estimates using OLS and 2SLS in Table 3. Because of topcoding, I expect that mean estimates will be attenuated. In Panel A, I estimate a specification relating household labor earnings to rebate size. I find no statistically significant effects.

I replicate the above results using a log-transformed outcome variable to address the skewness of labor earnings. Since both the labor earning and rebate variables have a nontrivial number of zeroes, I use $\log(\text{labor earnings} + 1)$ as the outcome with the equivalent transformation for the rebate variables, a frequently-used transformation in the presence of zeroes. I report these estimates in Panel B of Table 3. Again, I find no statistically significant relationship between rebates and labor earnings using OLS or 2SLS.

Alternatively, some work recommends transforming the outcome variable by the inverse hyperbolic sine (IHS) when the variable is skewed and includes zeroes. The literature discusses this transformation in detail (Burbidge, Magee, and Robb (1988); MacKinnon and Magee (1990); Pence (2006)) and it is often used in applied work (see Gelber (2011); Carboni (2012); Barcellos and Jacobson (2015) for examples). I use this transformation for the labor earnings and rebate variables and present these estimates in Panel C of Table 3. Again, neither OLS nor 2SLS can reject that rebates do not affect mean labor earnings.

Topcoding potentially biases mean estimates, regardless of the transformation. Even without these concerns, mean estimates do not provide information about the response at any specific part of the distribution, which may be especially important for a policy that is targeted based on earnings. For this reason, I focus on techniques which provide information about distributional impacts for the remainder of this paper.

4.2 CDF Estimates

Since the response to rebates is likely not uniform throughout the earnings distribution, I estimate a series of linear probability models to study how rebate receipt impacts the CDF of earnings. Using $100 intervals, I test the effect of rebates on the probability that household earnings are below some threshold, conditional on household fixed effects. I use 2SLS to identify solely off of rebate timing variation. The estimates refer to how rebates change the probability that a household has monthly earnings lower than a given threshold. Under the hypothesis that households respond to rebate receipt by reducing their labor supply, the estimates should be positive, reflecting an increased probability of lower earnings. I estimate the effects of the rebate and lagged rebate variables simultaneously and report the results graphically in Figures 1.A and 1.B. I scale the coefficients so that the estimates refer to the effect of a $1,000 rebate (about the size of the average rebate for married couples). A positive coefficient implies that rebates increase the probability that a household will have earnings lower than or equal to the specified threshold.

Figure 1.A shows that the rebates have significant effects on the CDF for labor earnings. The relationship between rebates and the earnings distribution is close to zero at low levels of earnings, suggesting that rebate receipt does not cause households to reduce earnings below these low thresholds. The relationship increases in magnitude rather consistently until it hits a peak at $5,400 in monthly household earnings. This estimate implies that a $1,000 rebate increases the probability that the household will have monthly earnings less

than or equal to \$5,400 by 0.009. Above household earnings of \$5,400, the rebate estimates steadily decrease and reach zero at monthly earnings of \$10,800. Figure 1.A includes 120 estimates: 112 are positive with 59 significant at the 10% level and 40 significant at the 5% level.

I also find positive effects for the lagged rebate variable, suggesting that rebates impact household earnings even two to three months after receipt. The evidence here is not as strong, though 110 out of the 120 estimated coefficients are positive. Fifteen of these are statistically significant from zero at the 10% level; four are significant at the 5% level. The estimates are more uniform for the lagged rebate variable, though they also decline to zero near the top of the distribution. In general, the lagged rebate estimates are smaller than the equivalent rebate variable estimates. The estimates peak at 0.005 for the effect of a lagged rebate on the probability that household earnings are less than or equal to \$6,700.

These estimates are somewhat difficult to interpret in terms of how each rebate dollar reduces earnings, but they suggest that household labor supply responds to rebates and that there is significant heterogeneity throughout the earned income distribution. This empirical approach assumes that responsiveness is a function of the household's earnings level, regardless of whether this level of earnings is small or large given marital status, family size, and time. I turn now to quantile estimation which studies how, for example, rebates affect the bottom of the household earnings distribution where placement in the distribution is determined empirically based on household size, marital status, month, and month-relative-to-interview.

4.3 Quantile Treatment Effect Estimates

Next, I use a method to estimate quantile treatment effects in the presence of nonadditive fixed effects. This estimation technique is critical for estimating the effect of tax rebates on labor earnings given that mean estimators are likely biased and miss distributional effects. The results from estimating equation (3) are presented in Figure 2. Figure 2.A shows the estimates for the rebate variable $\beta_1(\tau)$ graphically and Figure 2.B presents the estimates for the lagged rebate variable $\beta_2(\tau)$. The estimates are shown for quantiles 6 through 90.[27] I find consistent evidence of negative labor supply effects. Figure 2.A shows that the estimated coefficient on the rebate variable is negative for almost the entire labor earnings distribution

[27] The estimates above quantile 90 are excluded from the figure because they are noisy. However, the tables will include estimates through the 95^{th} quantile.

and this relationship is statistically significant from zero for a large portion of the lower part of the distribution. Of the 85 quantile estimates shown in Figure 2.A, 46 are negative and statistically significant from zero at the 10% level (32 at the 5% level). In Figure 2.B, the estimated lagged rebate effect reflects a similar pattern, though the magnitudes tend to be smaller and many fewer are statistically significant (31 significant at the 10% level, 22 at the 5% level).

To interpret magnitudes, Table 4 presents the same estimates for every fifth quantile between quantiles 10 and 95 as well as statistics for the τ^{th} quantile of monthly earnings for both singles and couples.[28] These statistics are the τ^{th} quantile of the distribution of the data setting $R_{it} = LR_{it} = 0$ (i.e., the quantiles of the "untreated" earnings distribution for both singles and married couples that would be observed in the absence of rebates)[29] and are provided as a map between the quantiles and the untreated earnings distribution.

At quantile 25 – corresponding to $792 in monthly labor earnings for singles and $3,284 for couples – the estimate on the rebate variable is significant at the 5% level and the estimate for the lagged rebate variable is significant at the 10% level. The rebate estimate implies that for each additional dollar of transitory income, households reduce labor earnings by 14 cents in the month of receipt and the subsequent month. In the following two months, earnings are reduced by 10 cents per rebate dollar.

The estimates for quantiles 25 to 45 imply that an additional rebate dollar decreases household labor earnings by around 10 to 14 cents. In subsequent months, labor earnings decline by around 8 to 10 cents per rebate dollar. These results are large and (generally) statistically significant from zero, suggesting that the stimulus payments had economically important impacts on labor supply. The evidence suggests that the rebate effects may extend even further up the distribution. I estimate a rebate effect of 10 cents at quantile 70, corresponding to $3,935 in household monthly earnings for single households ($8,336 for married couples). This estimate is statistically significant at the 10% level.

Overall, there is strong evidence of negative labor supply effects for a large section of the earnings distribution. In fact, the point estimates are rather consistent (and statistically significant from zero) between the 25^{th} and 50^{th} percentile of the earnings distribution. At higher points in the distribution, the estimates are more likely to be statistically insignificant.

[28] On rare occasions, because I eliminate observations in which the untreated distribution is censored, these earnings metrics will not increase monotonically near the censoring points.

[29] While I include interactions based on marital status, family size, month, and month-relative-to-interview, I present the untreated distributions by marital status only given the large number of interactions otherwise.

There is strong evidence of heterogeneous effects.

4.4 Average and Total Treatment Effects

A further advantage of estimating quantile treatment effects is that the average treatment effects can be estimated by integrating over the quantiles. I estimate average treatment effects using:

$$\widehat{\psi_1} = \int_\tau \widehat{\beta_1(\tau)} d\tau, \quad \widehat{\psi_2} = \int_\tau \widehat{\beta_2(\tau)} d\tau. \tag{4}$$

Only quantile treatment effects for quantiles 6 to 99 were estimated given that the quantile function itself is censored at lower quantiles (i.e., households earn \$0 regardless of rebate receipt). I assume an effect of zero for both variables for the quantiles below 6. I report the estimated average treatment effects for the rebate and lagged rebate variables in Columns (1) and (2) of Table 5. Standard errors for $\widehat{\psi_1}$ and $\widehat{\psi_2}$ are generated using (clustered) subsampling (Politis et al. (1994)). I estimate that each rebate dollar decreased household labor earnings by 9 cents in the month of receipt and the month after receipt. This effect decreases to 5 cents per month in the two subsequent months. Both of these estimates are statistically significantly different from zero.

Next, I estimate the partial equilibrium effect of the stimulus payments on labor earnings in the United States. The estimated coefficients on the rebate variable refer to the effect in the month of receipt and the month after receipt. The estimated coefficients on the lagged rebate variable refer to the effect two and three months after receipt. Since each estimate refers to the per-month effects for two months, it is necessary to multiply both of these coefficients by 2 to calculate the total effect on household labor earnings. I multiply this mean household effect estimate by the total amount of stimulus payments, \$96 billion[30] to estimate the partial equilibrium effect on national labor earnings:

$$\widehat{\psi_T} = \$96 \text{ billion} \times \int_\tau \left[2 \times \widehat{\beta_1(\tau)} + 2 \times \widehat{\beta_2(\tau)} \right] d\tau. \tag{5}$$

Column (3) of Table 5 presents this estimate. I find that economic stimulus payments reduced 2008 national labor earnings by \$26.7 billion. This estimate does not include any aggregate demand shocks that independently affected labor outcomes or compensating behavior by firms. Instead, this reduction is the total short-term household-level labor supply response

[30]Calculation using Daily Treasury Statements included in Parker et al. (2013).

to receiving a rebate.

To benchmark these estimates, it is helpful to discuss consumer spending magnitudes estimated in the literature. Parker et al. (2013) concludes that the rebates caused a partial equilibrium increase in nondurable spending in the quarter of receipt of 12-30 cents per rebate dollar. The labor supply effect estimated in this paper, when limited to the effect over three months (consistent with the Parker et al. (2013) time frame), is 23 cents per rebate dollar. When including durable goods, Parker et al. (2013) finds total effects ranging from 50 to 90 cents per rebate dollar. Thus, the nondurable spending effect and the labor supply effect are approximately equivalent, but the overall consuming spending response dominates the labor supply response.

5 Robustness Tests

5.1 Conditional Exogeneity of Rebate Timing

Conditioning on household fixed effects is crucial to the identification strategy adopted in the literature and applied in this paper. The fixed effects account for earnings differences across households. To test whether the household fixed effects adequately account for differences across households, I limit the sample to households that received rebates at some point in 2008, eliminating households that may be especially different from those receiving rebates. Figure 3 presents the results. The estimates are similar to the full sample estimates. The confidence intervals are slightly larger due to the smaller sample size, but the overall results are consistent with the quantile treatment effect estimates using the full sample.

Some variation in rebate timing is originating from households that received their rebates late, which I define as receiving a rebate after July 2008. These households are potentially different in terms of earnings capacity than the rest of the sample. While conditioning on household fixed effects should account for these differences, I also present estimates in Table 6.A with these households excluded from the sample. The estimates are similar to the quantile treatment effect estimates when using the full sample.

Electronic payments were sent out during April and May and, as observed in the summary statistics, households receiving electronic payments have different earnings distributions than other households. I eliminate these early receivers and present the results in Table 6.B. Again, the results appear robust to this selection, suggesting that conditioning on household fixed effects adequately accounts for any such differences.

5.2 Placebo Test

Next, I perform a placebo test using earnings data in the later waves of the SIPP and assigning "counterfactual" rebates to the exact same month in 2009 as each household received its actual rebate in 2008. I replicate the analysis using the 2009 labor earnings data (April - December) from waves 3, 4, and 5. The purpose of this analysis is to test whether rebate receipt was concurrent with receipt of other income (e.g., EITC payments) or other seasonal factors affecting labor supply. The randomized timing of the rebates makes it unlikely that other factors are correlated with rebate receipt. The estimates are presented in Table 7. I observe no relationship between these counterfactual rebates and labor supply, suggesting that there is no unobservable variable correlated with rebate timing that independently affects labor supply.

5.3 Lagged Effects

The main results of this paper are estimates of a specification which includes a variable pertaining to rebates received in months t and $t-1$ and a lagged rebate variable corresponding with months $t-2$ and $t-3$. In Table 8, I present results that include an additional lag variable for rebates received in months $t-4$ and $t-5$ (and add an instrument with the analogous rebate indicator).[31] I find little evidence of labor earnings responsiveness four to five months after rebate receipt. The lack of an effect four to five months after rebate highlights the transitory nature of the labor supply response. Only one estimate for these months is statistically significant at the 10% level (quantile 40). The estimates on the rebate and lagged ($t-2$ and $t-3$) rebate variables are consistent with the main estimates of this paper, suggesting that excluding additional lags is not biasing the estimates.

Furthermore, one possible explanation for labor supply reductions in response to transitory income is that households substitute immediate labor supply reductions for planned future reductions. For example, households may plan for an unpaid work absence in the future because they are unable financially to take one now. The receipt of the rebate permits these households to reduce their labor supply immediately. Note that this behavior is still evidence of a labor supply response to transitory income and inconsistent with the permanent income hypothesis.

If a temporal shift is occurring, we may observe a "bounce back" effect later. We do

[31] Estimation uses MCMC when the quantile function includes three rebate variables.

not see evidence of this bounce back in Table 8.[32] Similarly, if this substitution occurs over a longer time period, we may expect to see differential responses in 2009, but the placebo test estimates – which test for systematic differences in labor supply behavior in later months – presented previously do not provide evidence that this is occurring. However, this bounce back effect is difficult to observe if, for example, the planned future labor supply reductions would have occurred at similar times across households (unrelated to rebate timing) and, consequently, only those not receiving rebates at all reduce their labor supply at the time originally planned (in the absence of rebates). In this case, the estimated effect would be muted. Given that most of the lagged estimates even four to five months after rebate receipt are primarily negative, there is no evidence of a muted *positive* effect and it does not appear that this temporal shift is occurring. Consequently, I interpret the labor supply reduction as a loss of household labor supply, not a temporal substitution of missed work.

5.4 Seam Bias

One concern with the high-frequency SIPP data is that the reported information may change too rarely within a reference period and too often across reference periods. As discussed in Section 3.1, this seam bias will likely attenuate the estimates. To test this empirically, I adopt the approach used in Ham et al. (2016) and select on the last month observation for each wave, dropping the other months. I include these results in Appendix Table A.2. As expected, the point estimates are larger in magnitude. The standard errors are also larger now because there is much less within-household variation to exploit. However, it appears that seaming may be biasing the estimated effects toward zero.

6 Mechanisms

The above analysis estimates heterogeneous impacts throughout the earnings distribution, providing evidence of mechanisms to explain the overall effects of rebates on household labor supply. This section further explores the underlying behavior driving the reduction in labor earnings as a response to transitory income. I consider demographic differences, labor supply margins, and stated rebate use. I also study labor force status, work absences, and the reasons for work absences.

[32]I have also included additional lags and do not observe labor supply effects on these variables either.

6.1 Demographic Differences

In this section, I study whether the quantile treatment effects vary based on gender and marital status by estimating effects on subsamples of the data. Table 9 presents quantile treatment effect estimates for four groups: single women, single men, married women, and married men. The primary motivation for testing for different treatment effects on these dimensions is to provide evidence for whether the labor supply reduction observed in this paper is potentially a "secondary earner" effect. The labor supply literature often finds that married women are especially responsive to wage incentives (Keane (2011)). I estimate whether there is a differential response in the context of transitory income.

Panel A selects on household heads who are single females. I estimate small effects and cannot reject a null effect for most of the distribution. Panel B presents the equivalent estimates for single men. These results are noisy due to the small sample. The point estimates are larger in magnitude than those for single women, but there is still little statistical evidence of rebate responsiveness.

Next, I evaluate differences by gender for married couples. Since the outcome is household earnings, this analysis cannot be done at the individual-level given that a low-earning individual may be part of a high-earning household. Instead, I hold the monthly labor earnings of the spouse constant throughout the entire time period, using the earnings in the first observed month for the spouse. Household labor earnings, consequently, only change over time due to behavioral responses by the person whose earnings are not held constant.[33] Panel C presents the results for married women. They suggest large effects throughout the earnings distribution. In fact, I estimate large statistically significant results even at quantile 75, corresponding to household monthly earnings of $10,542.

I evaluate the earnings of married men in Panel D, holding spousal earnings constant. The estimates are also large, though I do not observe large effects at such high parts of the distribution as with married women. There is more evidence for men of rebate effects at the lower part of the distribution. Overall, it appears that the estimated effects for households headed by married couples cannot be attributed solely to men or women. The relative size of the effects for married couples compared to single household heads is consistent with a

[33] This approach is not perfect as these initial spousal labor earnings are potentially "treated" by tax rebates for households receiving rebates in this first observed period. However, I hold these earnings constant across time so any bias should be minimal. Other approaches, such as using earnings from later "untreated" periods, produce similar results.

secondary earner response. In Appendix A.5, I include estimates based on marital status only. The results in Appendix A.5 confirm the finding that married couples are more responsive to tax rebates than single heads of household.

6.2 Labor Supply Margins

While I find strong evidence that tax rebates crowd out household earnings, the results do not distinguish between households reducing their hours or effort on the intensive margin or the tax rebates impacting extensive margin labor supply decisions. The entire earnings distribution can be affected if rebates affect the decision to work. I define the extensive margin as having positive household labor earnings in the month.

I replicate the main analysis of this paper while shutting down extensive margin effects. I do this by eliminating households with no monthly labor earnings in the analysis such that estimates refer only to intensive margin decisions. However, eliminating these households drastically changes the mapping between quantiles and household earnings so I shift back the resulting quantiles as if these non-working households had not been eliminated (and extensive margin decisions were random). The technical details of this approach are straightforward and discussed in Appendix Section B.4. By eliminating the extensive margin, we might expect the effects to be attenuated or even eliminated if rebates affect extensive margin decisions. Table 10 presents the estimates. The results in Table 10 are of similar magnitude as the main results of this paper. Overall, I do not find evidence that extensive margin behavioral responses drive the labor supply responses to tax rebates. In Section 6.4, I study extensive labor supply decisions more explicitly and find no relationship with rebate receipt, consistent with these results.

Next, I use the same approach as above (studying intensive margin effects only) and exclude households with only non-hourly workers. Table 11.A presents these results. This group appears especially responsive to rebates as the magnitudes of the estimates are, in general, larger than the main estimates of this paper which include households with only non-hourly workers. These results suggest that the overall response is driven by hourly workers.

I replicate this analysis but include the households with only non-hourly workers and exclude households with any workers who are paid by the hour. Table 11.B presents these results. There is little evidence of an effect for this group. The results in Table 11 suggest that hourly workers are driving the effect. While workers not paid by the hour are not necessarily salaried, it is not surprising that this group is less likely to experience earnings reductions due to short-term labor supply decisions.

6.3 Responsiveness by Stated Rebate Use

I also analyze the labor supply effects based on how households reported that they used their rebates[34] and present these estimates in Table 12. Given smaller samples, the estimates in this table are relatively noisy. In Panel A, I find similar point estimates for those spending their rebates as the main estimates of this paper, though most of the estimates are not statistically significant from zero. Households may work less upon receipt of a tax rebate because they value leisure or because they purchase goods with their rebate and consumption and leisure are complements. This paper is interested in the overall effect,[35] but the evidence is inconsistent with consumption-leisure complementarities fully driving the results. There is less evidence that households that mostly saved their rebates reduced labor supply (Panel B) though this is the smallest group and the standard errors are large.

Table 12, Panel C provides estimates for households reporting that they used their rebates mainly to reduce debt. The sample here is larger so it is not necessarily surprising that I find statistically significant effects for this group. The estimates imply household labor supply responses to tax rebates for this group. 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 relaxed these constraints and allowed households to reduce labor supply in the short-term.

6.4 Labor Force Status and Work Absences

The SIPP provides detailed variables on employment status and reasons for work absences, which I use to study the relationship between rebates and alternative measures of labor supply. I perform this analysis at the individual-level, and I employ 2SLS for this analysis while conditioning on individual fixed effects. The first row of Table 13 reports these results. In the first column, I study whether rebates are associated with positive individual monthly

[34] This reporting is done at the individual-level. Thus, a household may be in (at most) two of these samples if spouses answered the question differently. The vast majority of households are only in one sample.

[35] It is difficult for policy to provide income to households while eliminating consumption-leisure complementarities so we are likely interested only in the total effect. However, it is possible that generalizing these results to other contexts may depend on understanding the relative importance of these complementarities. For example, the effects may be different depending on the availability of consumer goods. Similarly, the transitory labor supply effects of providing in-kinds benefits may differ from the effects of transitory income for many reasons, including the importance of consumption-leisure complementarities.

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 in Panel A, I examine more detailed categories of employment status. These categories (as coded in the SIPP) are based on the number of weeks (0, 1+, or all) that the individual had a job within the month, the number of weeks the individual was looking for work, the number of weeks the individual was absent due to layoff, and the number of weeks the individual was employed but absent without pay. 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 is consistent with the earlier findings that rebates result in voluntary labor supply reductions on the intensive margin.

Next, the SIPP reports detailed data on unpaid absences and reasons for those absences. These data are reported by wave, not month, so I aggregate all rebate variables by wave. While there were benefits to using monthly earnings data in the main analysis, the time interval for this analysis is more similar to the time interval typically studied in the tax rebate literature. I find significant effects on unpaid absences. Furthermore, these absences are not due to illness or injury,[36] 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 voluntarily consume leisure by working less in the short-term. The main results of this paper showed that rebates cause households to decrease their labor earnings in the short-term. The results in this section provide complementary evidence that workers respond to rebates by taking voluntary, unpaid absences from work.

7 Discussion and Conclusion

This paper studies the labor supply consequences of issuing tax rebates. The literature provides little evidence about short-term labor supply responsiveness to transitory income and most tests of consumption smoothing focus on expenditures, not leisure. 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 bottom half of the earnings distribution. To

[36] Gross and Tobacman (2014) finds that tax rebates are associated with increased incidence of emergency room utilization for urgent medical conditions, providing evidence of "dangerous liquidity." I find no evidence that illnesses or injuries are driving the labor supply responses.

estimate these distributional effects, I use a quantile regression estimator for panel data which allows one to non-parametrically condition on household fixed effects while maintaining the nonseparable disturbance property that typically motivates use of quantile methods.

I find that each rebate dollar decreased labor earnings by 9 cents per month on average with larger effects in the lower part of the earnings distribution. I also find smaller but statistically significant lagged effects. Extensive labor supply responses cannot explain this labor supply effect. The effect appears to be driven by households with hourly workers and there is also evidence that this is a "secondary earner" effect. Furthermore, the evidence is strongest for households that used the rebate to reduce their debt, suggesting that liquidity constraints may be an important source of the overall effect.

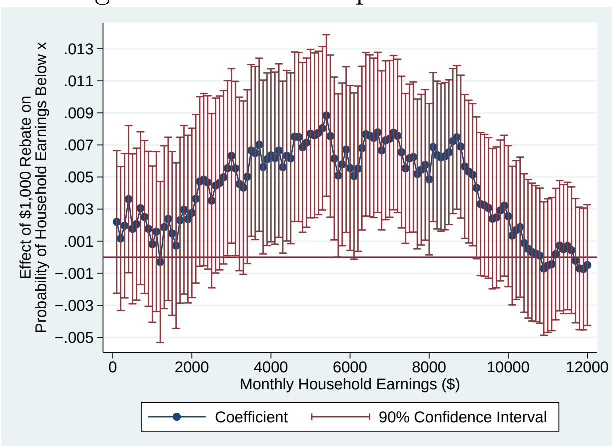
Finally, I find that rebates are associated with temporary reductions in labor supply and that these reductions are voluntary. The results of this paper are consistent with households using the stimulus payments as substitutes for labor earnings, allowing them to consume more leisure. While many empirical tests of the permanent income hypothesis focus on measures of expenditures, this paper presents rare evidence that transitory income impacts short-term consumption of 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 consumer 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 possible concurrent labor supply shift. To understand the aggregate supply shift, we would need more information about the economy's ability to substitute for short-term voluntary labor supply reductions.

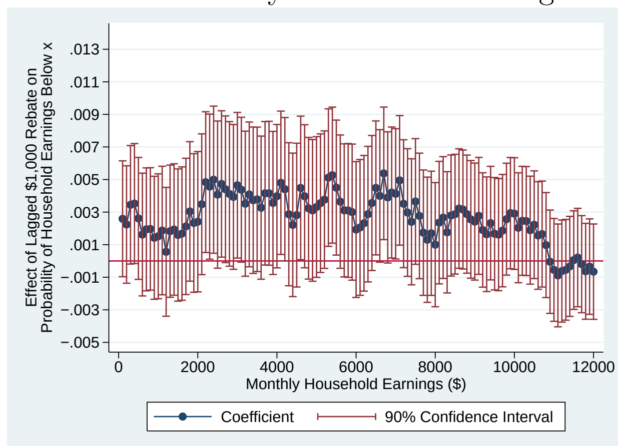
A vast labor supply literature focuses on understanding substitution and wealth effects while providing little guidance about whether household labor supply responds to the receipt of payments. The results of this paper imply that government payment schedules may have important effects on the timing of national labor earnings. Many government programs provide lump sum payments at discrete time intervals and the timing of these payments is in itself a policy lever. The evidence in this paper implies significant scope for policy to affect short-term labor supply decisions.

Figures

Figure 1: Relationship Between Rebate and CDF of Monthly Household Earnings



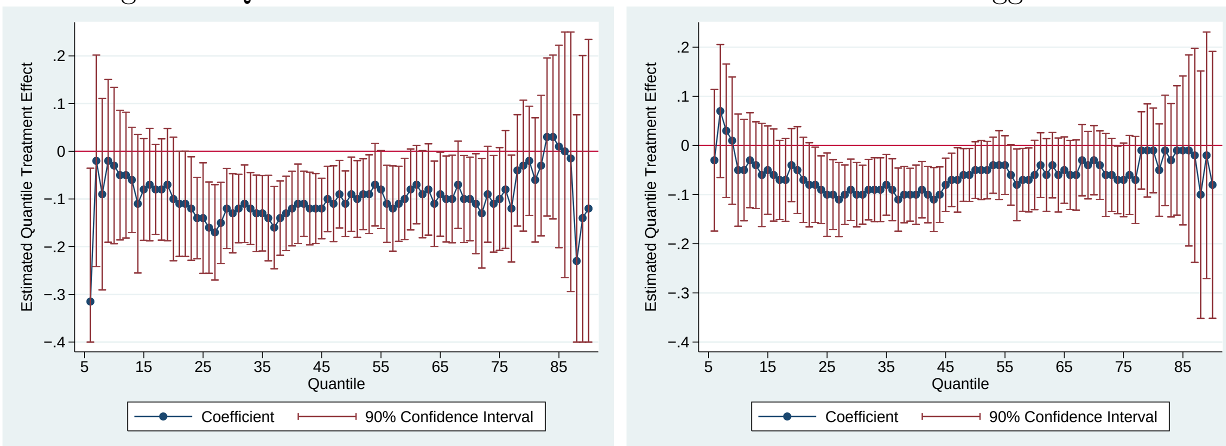
A: Effect of $1,000 Rebate



B: Effect of Lagged $1,000 Rebate

Notes: The figures represent the coefficient estimates ($\times$ 1000) on the rebate variable and lagged rebate variable. A positive coefficient implies that a $1,000 rebate increases the probability that a household has monthly earnings below the value on the x-axis. The effect of the rebate and lagged rebate variable are estimated simultaneously, conditioning also on household fixed effects and interactions based on month, family size, marital status, and month-relative-to-interview-month. The rebate variable refers to the total rebate amount received in that month and the previous month. The lagged rebate variable refers to the total rebate amount received 2-3 months prior.

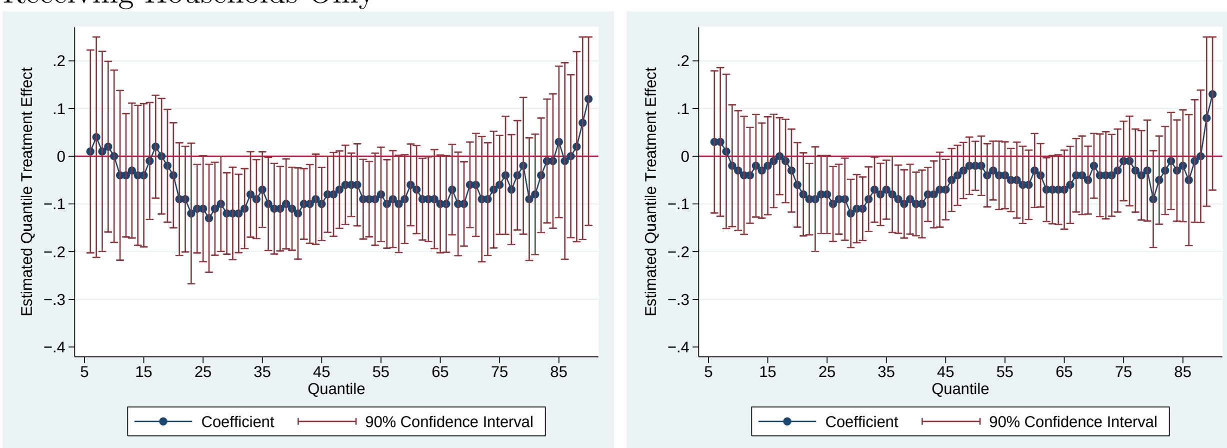
Figure 2: Quantile Treatment Effect Estimates for Rebate and Lagged Rebate



A: QTEs for Rebate B: QTEs for Lagged Rebate

Notes: Each point on Figure A is an estimate of the quantile treatment effect (QTE) of the rebate variable in equation (3) and is jointly estimated with the equivalent point in Figure B, which is the quantile treatment effect of the lagged rebate variable. I truncate the confidence intervals at -0.4 and 0.25. Interactions based on month, household size, marital status, and month-relative-to-interview also included in quantile function. The rebate variable refers to the total rebate amount received in that month and the previous month. The lagged rebate variable refers to the total rebate amount received 2-3 months prior.

Figure 3: Quantile Treatment Effect Estimates for Rebate and Lagged Rebate: Rebate-Receiving Households Only



A: QTEs for Rebate

B: QTEs for Lagged Rebate

Notes: Each point on Figure A is an estimate of the quantile treatment effect (QTE) of the rebate variable in equation (3) and is jointly estimated with the equivalent point in Figure B, which is the quantile treatment effect of the lagged rebate variable. I truncate the confidence intervals at -0.4 and 0.25. Interactions based on month, household size, marital status, and month-relative-to-interview also included in quantile function. The rebate variable refers to the total rebate amount received in that month and the previous month. The lagged rebate variable refers to the total rebate amount received 2-3 months prior.

Tables

Table 1: Timing of Economic Stimulus Payments in 2008

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

Notes: The method of rebate distribution depended on whether the tax filer provided a bank routing number in their 2007 tax return.

Table 2: SIPP Summary Statistics

	Full Sample		Rebate Receiving Sample		Received Rebate in April/May	
	Single	Married	Single	Married	Single	Married
Family Size	1.84	3.39	1.82	3.38	1.80	3.37
Received Rebate (%)	76.7	85.4	100	100	100	100
Average Rebate Size in $ (conditional on receipt)	580	1096	580	1096	590	1143
No Monthly Labor Earnings (%)	22.08	5.26	17.50	4.59	16.28	3.91
25th Percentile Monthly Labor Earnings	489	3240	1065	3382	1200	3606
Median Monthly Labor Earnings	2425	5873	2633	5833	2815	6000
75th Percentile Monthly Labor Earnings	4335	9163	4333	8707	4535	8788
Number of Households	9,721	13,277	7,427	11,361	2,649	4,288

Notes: "Single" and "Married" refer to marital status in the household's first observation in the data. This marital status can change over the 8 months of data, though such changes are rare.

Table 3: Mean Estimates

Panel A:	**Monthly Earnings**	
	(1)	(2)
Rebate	-0.005	-0.022
	(0.035)	(0.038)
Lagged Rebate	-0.007	-0.021
	(0.027)	(0.031)
OLS/IV	OLS	IV
N	183,984	183,984

Panel B:	**log(Monthly Earnings + 1)**	
	(3)	(4)
log(Rebate+1)	-0.002	-0.003
	(0.003)	(0.003)
log(Lagged Rebate+1)	-0.002	-0.003
	(0.002)	(0.002)
OLS/IV	OLS	IV
N	183,984	183,984

Panel C:	**IHS(Monthly Earnings)**	
	(5)	(6)
IHS(Rebate)	-0.002	-0.005
	(0.003)	(0.004)
IHS(Lagged Rebate)	-0.003	-0.005
	(0.003)	(0.003)
OLS/IV	OLS	IV
N	183,984	183,984

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Standard errors in parentheses adjusted for household clustering. Other controls include household fixed effects and interactions based on marital status, household size, month, and month-relative-to-interview month. "IHS" in Panel C refers to the inverse hyperbolic sine transformation.

Table 4: QTE Estimates of Impact of Rebates on Labor Earnings

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.03	-0.08	-0.10	-0.14**	-0.13***	-0.13***	-0.12**	-0.12***	-0.09*
	(0.10)	(0.06)	(0.08)	(0.07)	(0.05)	(0.05)	(0.05)	(0.04)	(0.05)
Lagged Rebate	-0.05	-0.05	-0.05	-0.10*	-0.10**	-0.08**	-0.10***	-0.08**	-0.05
	(0.07)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)
Monthly Earnings, Single	n/a	45	480	792	1,065	1,464	1,800	2,103	2,434
Monthly Earnings, Couple	1,429	2,080	2,667	3,284	3,806	4,333	4,882	5,400	5,908
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.08	-0.08	-0.09*	-0.10*	-0.10	-0.02	0.01	-0.12	0.06
	(0.05)	(0.05)	(0.05)	(0.05)	(0.07)	(0.07)	(0.13)	(0.22)	(0.60)
Lagged Rebate	-0.04	-0.06	-0.05	-0.03	-0.07	-0.01	-0.01	-0.08	-0.18
	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.09)	(0.17)	(0.25)
Monthly Earnings, Single	2,769	3,118	3,486	3,935	4,372	4,973	5,667	6,775	8,710
Monthly Earnings, Couple	6,435	7,042	7,668	8,336	9,165	10,072	11,084	10,992	n/a

Notes: Number of households = 22,998. ***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 these households were dropped due to censoring concerns. Interactions based on month, household size, marital status, and month-relative-to-interview also included in quantile function.

Table 5: Estimates of Mean and Total Effects

	(1)	(2)	(3)
Estimated Effect for:	Rebate	Lagged Rebate	Total Effect (in billions)
	-0.09***	-0.05***	-26.7***
	(0.02)	(0.02)	(7.0)

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Standard errors generated using clustered subsampling. Columns (1) and (2) are the mean estimates generated by integrating over the QTE estimates. The Total Effect is the estimate of the change in labor earnings due to the $96 billion in total stimulus payments.

Table 6: QTE Estimates: Exclude Late or Early Rebates

A. Exclude Late Rebates (N=21,520)

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.06	-0.15*	-0.14	-0.16*	-0.15**	-0.14**	-0.13**	-0.09	-0.10
	(0.10)	(0.08)	(0.12)	(0.08)	(0.07)	(0.06)	(0.05)	(0.06)	(0.07)
Lagged Rebate	-0.06	-0.08	-0.08	-0.09	-0.09	-0.08*	-0.10**	-0.07	-0.05
	(0.07)	(0.07)	(0.08)	(0.06)	(0.06)	(0.05)	(0.04)	(0.04)	(0.05)
Monthly Earnings, Single	n/a	135	500	799	1,065	1,472	1,804	2,117	2,455
Monthly Earnings, Couple	1,450	2,105	2,688	3,291	3,804	4,333	4,888	5,400	5,916

Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.10*	-0.09	-0.12	-0.10	-0.14	-0.10	-0.02	-0.19	0.38
	(0.06)	(0.07)	(0.08)	(0.07)	(0.09)	(0.11)	(0.16)	(0.25)	(0.74)
Lagged Rebate	-0.05	-0.07	-0.04	-0.05	-0.06	-0.03	-0.01	-0.10	-0.21
	(0.04)	(0.05)	(0.05)	(0.05)	(0.06)	(0.07)	(0.12)	(0.17)	(0.35)
Monthly Earnings, Single	2,771	3,121	3,484	3,944	4,383	5,000	5,700	6,833	8,750
Monthly Earnings, Couple	6,443	7,036	7,667	8,334	9,166	10,105	11,111	10,819	n/a

B. Exclude April and May Rebates (N=16,061)

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.06	-0.10	-0.11	-0.18**	-0.16**	-0.11*	-0.12**	-0.12**	-0.11*
	(0.14)	(0.09)	(0.11)	(0.09)	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)
Lagged Rebate	0.01	-0.01	-0.07	-0.08	-0.11*	-0.04	-0.09*	-0.10**	-0.07
	(0.09)	(0.07)	(0.08)	(0.07)	(0.06)	(0.06)	(0.05)	(0.05)	(0.05)
Monthly Earnings, Single	n/a	9	400	577	900	1,255	1,600	1,957	2,266
Monthly Earnings, Couple	1,386	1,905	2,500	3,068	3,632	4,167	4,706	5,275	5,840

Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.13**	-0.15**	-0.11*	-0.17**	-0.10	-0.05	-0.12	-0.23	0.00
	(0.06)	(0.06)	(0.06)	(0.08)	(0.09)	(0.11)	(0.17)	(0.31)	(0.81)
Lagged Rebate	-0.09*	-0.12***	-0.05	-0.09	-0.06	0.02	-0.03	-0.29	0.13
	(0.05)	(0.05)	(0.06)	(0.06)	(0.07)	(0.07)	(0.14)	(0.22)	(0.43)
Monthly Earnings, Single	2,598	2,994	3,348	3,804	4,286	4,901	5,696	6,946	9,000
Monthly Earnings, Couple	6,442	7,083	7,720	8,454	9,316	10,359	11,302	10,547	n/a

Notes: ***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 these households were dropped due to censoring concerns. Panel A sample excludes households that received a rebate after July 2008. Panel B sample excludes households receiving a rebate in April or May 2008. Interactions based on month, household size, marital status, and month-relative-to-interview also included in quantile function.

Table 7: Placebo Rebates: Assigning Rebates to Same Months in 2009

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	0.00	0.06	0.02	-0.01	-0.02	0.00	0.02	0.01	0.01
	(0.07)	(0.05)	(0.05)	(0.04)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)
Lagged Rebate	-0.05	0.00	0.00	-0.02	-0.04	-0.01	0.03	0.01	0.01
	(0.05)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	0.00	0.02	0.00	-0.02	0.00	0.01	0.13	0.15	0.32
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)	(0.08)	(0.14)	(0.26)
Lagged Rebate	0.01	0.03	0.05	0.02	0.04	0.01	0.10	0.13	0.27
	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.07)	(0.13)	(0.23)

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. SIPP waves 3, 4, and 5 used for this analysis. Rebates were reassigned to same month in 2009. Only 2009 data used. This table presents "placebo" effects which are expected to be around 0. Interactions based on month, household size, marital status, and month-relative-to-interview also included in quantile function.

Table 8: QTEs with Additional Lags

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.03	-0.07	-0.12	-0.16*	-0.18***	-0.17***	-0.15**	-0.13**	-0.11*
	(0.12)	(0.07)	(0.09)	(0.09)	(0.06)	(0.06)	(0.06)	(0.05)	(0.06)
Lagged Rebate (2 and 3 months)	-0.02	-0.03	-0.08	-0.13*	-0.14**	-0.13**	-0.16***	-0.10*	-0.07
	(0.11)	(0.08)	(0.08)	(0.08)	(0.06)	(0.06)	(0.06)	(0.06)	(0.05)
Lagged Rebate (4 and 5 months)	-0.01	0.01	-0.01	-0.03	-0.02	-0.05	-0.06*	-0.04	-0.01
	(0.07)	(0.05)	(0.05)	(0.05)	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.12*	-0.09	-0.13**	-0.11	-0.11	-0.09	-0.06	-0.19	0.02
	(0.07)	(0.06)	(0.06)	(0.07)	(0.08)	(0.10)	(0.14)	(0.23)	(0.54)
Lagged Rebate (2 and 3 months)	-0.08	-0.07	-0.08	-0.06	-0.06	-0.05	-0.14	-0.14	-0.13
	(0.06)	(0.06)	(0.06)	(0.07)	(0.07)	(0.09)	(0.14)	(0.21)	(0.36)
Lagged Rebate (4 and 5 months)	-0.05	-0.02	-0.01	-0.01	0.01	-0.01	-0.08	-0.02	0.02
	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.06)	(0.08)	(0.14)	(0.30)

Notes: Number of households = 22,998. ***Significance 1%, ** Significance 5%, * Significance 10%. Interactions based on month, household size, marital status, and month-relative-to-interview also included in quantile function.

Table 9: QTE Estimates By Marital Status and Gender

A. Single Female Head of Household (N=5,842)

Quantile	10	15	20	25	30	35	40	45	50
Rebate			-0.50	-0.10	-0.03	0.00	-0.06	-0.04	0.01
			(0.40)	(0.20)	(0.13)	(0.09)	(0.08)	(0.10)	(0.09)
Lagged Rebate			0.31	-0.03	-0.07	-0.13**	-0.06	-0.08	0.00
			(0.35)	(0.13)	(0.08)	(0.06)	(0.05)	(0.06)	(0.07)
Monthly Earnings, Single			400	690	1,000	1,299	1,600	1,917	2,197
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	0.00	-0.03	-0.06	-0.07	0.01	-0.03	-0.13	-0.26	-0.36
	(0.07)	(0.15)	(0.10)	(0.11)	(0.12)	(0.22)	(0.18)	(0.22)	(0.64)
Lagged Rebate	-0.04	-0.06	-0.01	-0.04	-0.01	-0.01	-0.14	-0.13	-0.27
	(0.06)	(0.09)	(0.08)	(0.08)	(0.08)	(0.15)	(0.16)	(0.18)	(0.44)
Monthly Earnings, Couple	2,508	2,847	3,212	3,607	4,042	4,562	5,250	6,264	8,250

B. Single Male Head of Household (N=3,801)

Quantile	10	15	20	25	30	35	40	45	50
Rebate			0.06	0.32	-0.28	-0.06	-0.08	-0.24*	-0.14
			(0.28)	(0.35)	(0.29)	(0.17)	(0.15)	(0.13)	(0.12)
Lagged Rebate			0.37	0.29	-0.08	-0.07	-0.06	-0.19*	-0.08
			(0.28)	(0.28)	(0.21)	(0.12)	(0.11)	(0.11)	(0.09)
Monthly Earnings, Single			905	1,210	1,351	1,732	2,078	2,425	2,762
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.16	-0.12	-0.26*	-0.14	0.02	0.07	-0.13	-0.29	0.30
	(0.18)	(0.16)	(0.15)	(0.12)	(0.15)	(0.21)	(0.23)	(0.29)	(1.03)
Lagged Rebate	-0.10	-0.08	-0.08	-0.08	0.06	0.05	0.13	-0.01	0.34
	(0.10)	(0.11)	(0.12)	(0.12)	(0.12)	(0.16)	(0.18)	(0.24)	(0.61)
Monthly Earnings, Couple	3,118	3,456	3,897	4,320	4,800	5,417	6,250	7,440	9,000

C. Married Women (Spousal Earnings Held Constant) (N=13,202)

Quantile	10	15	20	25	30	35	40	45	50
Rebate	-0.01	-0.01	-0.07*	-0.03	-0.03	-0.06	-0.08**	-0.12***	-0.12**
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)
Lagged Rebate	0.01	0.00	-0.03	-0.02	-0.01	-0.04	-0.03	-0.04	-0.07*
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Monthly Earnings, Couple	2,111	2,819	3,464	4,033	4,583	5,105	5,651	6,248	6,817
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.10**	-0.10**	-0.13***	-0.12**	-0.21***	-0.19	0.10		
	(0.04)	(0.04)	(0.05)	(0.05)	(0.08)	(0.15)	(0.18)		
Lagged Rebate	-0.05	-0.02	-0.09**	-0.09**	-0.12***	-0.16*	-0.50		
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.09)	(0.35)		
Monthly Earnings, Couple	7,440	8,083	8,750	9,583	10,542	11,413	11,480		

D. Married Men (Spousal Earnings Held Constant (N=13,202)

Quantile	10	15	20	25	30	35	40	45	50
Rebate	-0.05	-0.06	-0.10*	-0.07	-0.11*	-0.11**	-0.11**	-0.09**	-0.08
	(0.07)	(0.06)	(0.06)	(0.05)	(0.06)	(0.05)	(0.06)	(0.04)	(0.06)
Lagged Rebate	-0.03	-0.06	-0.05	-0.05	-0.04	-0.06	-0.06	-0.06*	-0.08*
	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
Monthly Earnings, Couple	1,700	2,400	3,000	3,589	4,167	4,668	5,202	5,767	6,324
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.10*	-0.07	-0.09*	-0.11	-0.09	-0.06	0.25		
	(0.06)	(0.06)	(0.05)	(0.08)	(0.09)	(0.13)	(0.38)		
Lagged Rebate	-0.07	-0.06	-0.05	-0.08	-0.03	-0.08	0.01		
	(0.04)	(0.04)	(0.04)	(0.05)	(0.05)	(0.07)	(0.24)		
Monthly Earnings, Couple	6,913	7,542	8,217	8,888	9,775	10,810	10,535		

Notes: ***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. Spousal earnings are held constant across all time periods in Panels C and D. Interactions based on month, household size, marital status, and month-relative-to-interview also included in quantile function.

Table 10: Intensive Margin Only

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.03	-0.07	-0.10	-0.15***	-0.13***	-0.14***	-0.11***	-0.10***	-0.07
	(0.09)	(0.06)	(0.07)	(0.06)	(0.05)	(0.05)	(0.04)	(0.04)	(0.05)
Lagged Rebate	-0.07	-0.07	-0.08	-0.14***	-0.13***	-0.12***	-0.11***	-0.08**	-0.05
	(0.06)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)
Monthly Earnings, Single	n/a	63	480	797	1,074	1,471	1,799	2,100	2,434
Monthly Earnings, Couple	1,443	2,083	2,675	3,291	3,810	4,336	4,883	5,400	5,903

Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.06	-0.06	-0.07	-0.08	-0.12*	-0.03	0.01	-0.13	-0.30
	(0.04)	(0.05)	(0.05)	(0.05)	(0.06)	(0.07)	(0.12)	(0.21)	(0.35)
Lagged Rebate	-0.04	-0.06	-0.05	-0.02	-0.08*	-0.03	-0.01	-0.09	-0.36
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.09)	(0.16)	(0.23)
Monthly Earnings, Single	2,768	3,118	3,486	3,929	4,378	4,977	5,667	6,778	8,750
Monthly Earnings, Couple	6,432	7,037	7,667	8,334	9,166	10,078	11,084	10,998	n/a

Notes: ***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 these households were dropped due to censoring concerns. These estimates refer to intensive margin responses only by setting the sample equivalents of the moments represented by equation (10) to zero. Interactions based on month, household size, marital status, and month-relative-to-interview also included in quantile function.

Table 11: Hourly and Non-Hourly Workers

A. Hourly Workers (N=15,170)

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	0.01	-0.07	-0.12	-0.14*	-0.18**	-0.22***	-0.18***	-0.21***	-0.21***
	(0.11)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.07)	(0.06)	(0.05)
Lagged Rebate	-0.05	-0.10*	-0.10*	-0.11**	-0.15**	-0.18***	-0.19***	-0.19***	-0.18***
	(0.07)	(0.06)	(0.06)	(0.06)	(0.06)	(0.05)	(0.04)	(0.04)	(0.05)
Monthly Earnings, Single	n/a	n/a	n/a	200	480	756	1,083	1,210	1,502
Monthly Earnings, Couple	1,300	1,733	2,108	2,593	3,065	3,551	4,023	4,492	4,941

Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.14***	-0.11**	-0.08	-0.05	-0.07	-0.07	-0.13	-0.15	0.15
	(0.05)	(0.06)	(0.05)	(0.06)	(0.06)	(0.08)	(0.09)	(0.10)	(0.31)
Lagged Rebate	-0.12***	-0.09**	-0.10**	-0.04	-0.07	-0.04	-0.09	-0.17**	-0.01
	(0.04)	(0.04)	(0.04)	(0.06)	(0.05)	(0.06)	(0.06)	(0.08)	(0.19)
Monthly Earnings, Single	1,772	2,058	2,356	2,623	3,010	3,426	4,000	4,674	6,028
Monthly Earnings, Couple	5,367	5,819	6,324	6,861	7,534	8,279	9,215	10,398	11,533

B. Non-Hourly Workers (N=13,868)

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	0.21	0.17	0.17	0.05	0.06	0.05	0.04	-0.01	0.01
	(0.25)	(0.15)	(0.11)	(0.10)	(0.08)	(0.12)	(0.09)	(0.08)	(0.08)
Lagged Rebate	0.13	-0.01	0.04	-0.06	0.00	0.04	0.04	-0.03	-0.02
	(0.17)	(0.12)	(0.09)	(0.09)	(0.07)	(0.09)	(0.06)	(0.07)	(0.06)
Monthly Earnings, Single	n/a	n/a	n/a	352	800	1,008	1,404	1,760	2,289
Monthly Earnings, Couple	1,000	1,915	2,452	3,326	4,035	4,790	5,483	6,187	6,857

Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.02	-0.01	-0.01	0.02	0.01	0.12	-0.01	0.07	0.27
	(0.07)	(0.08)	(0.09)	(0.08)	(0.10)	(0.15)	(0.24)	(0.30)	(0.54)
Lagged Rebate	-0.06	-0.02	-0.09	-0.01	0.00	0.05	0.00	0.13	0.07
	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.12)	(0.21)	(0.21)	(0.40)
Monthly Earnings, Single	2,828	3,317	3,777	4,222	4,805	5,417	6,274	7,500	9,699
Monthly Earnings, Couple	7,526	8,261	8,903	9,833	10,592	11,325	10,547	n/a	n/a

Notes: ***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 these households were dropped due to censoring concerns. Panel A replicates Table 10 while excluding households with only non-hourly workers. Panel B replicates Table 10 while excluding households with any hourly workers. Interactions based on month, household size, marital status, and month-relative-to-interview also included in quantile function.

Table 12: QTE Estimates Based on Self-Reported Use of Rebate

A. Households Reporting that They Spent Rebate (N=4,765)

Quantile	10	15	20	25	30	35	40	45	50
Rebate	0.00	-0.13	-0.10	-0.13	-0.19	-0.09	-0.06	-0.07	-0.11
	(0.25)	(0.17)	(0.18)	(0.14)	(0.14)	(0.10)	(0.11)	(0.10)	(0.14)
Lagged Rebate	0.13	-0.09	-0.12	-0.13	-0.16	-0.12	-0.08	-0.04	-0.06
	(0.15)	(0.12)	(0.12)	(0.09)	(0.10)	(0.08)	(0.08)	(0.08)	(0.09)
Monthly Earnings, Single	0	303	740	1,057	1,500	1,827	2,157	2,500	2,826
Monthly Earnings, Couple	1,732	2,499	3,167	3,733	4,330	4,900	5,339	5,833	6,263

Quantile	55	60	65	70	75	80	85	90	95
Rebate	-0.05	-0.05	-0.05	-0.06	-0.10	-0.01	0.21	0.37	0.32
	(0.13)	(0.13)	(0.12)	(0.11)	(0.12)	(0.12)	(0.18)	(0.31)	(1.35)
Lagged Rebate	0.00	-0.03	-0.02	-0.07	-0.11	-0.02	0.16	0.39*	0.01
	(0.10)	(0.09)	(0.09)	(0.08)	(0.11)	(0.10)	(0.14)	(0.22)	(0.85)
Monthly Earnings, Single	3,125	3,479	3,902	4,243	4,650	5,156	5,783	6,581	8,333
Monthly Earnings, Couple	6,750	7,333	7,896	8,434	9,133	9,912	10,687	11,083	12,993

B. Households Reporting that They Saved Rebate (N=2,798)

Quantile	10	15	20	25	30	35	40	45	50
Rebate	-0.10	0.05	-0.03	-0.02	0.04	-0.01	0.04	0.02	-0.04
	(0.27)	(0.22)	(0.15)	(0.17)	(0.15)	(0.16)	(0.19)	(0.16)	(0.13)
Lagged Rebate	-0.03	0.07	0.04	-0.03	0.04	0.01	0.01	0.00	-0.03
	(0.23)	(0.14)	(0.11)	(0.12)	(0.11)	(0.12)	(0.12)	(0.13)	(0.11)
Monthly Earnings, Single	1,000	1,083	1,659	2,000	2,464	2,760	3,017	3,366	3,743
Monthly Earnings, Couple	2,400	3,200	4,000	4,595	5,169	5,655	6,164	6,666	7,128

Quantile	55	60	65	70	75	80	85	90	95
Rebate	-0.01	0.01	-0.05	0.01	0.12	0.22	0.18	0.31	0.39
	(0.14)	(0.12)	(0.14)	(0.19)	(0.23)	(0.27)	(0.29)	(0.63)	(1.27)
Lagged Rebate	-0.02	0.03	-0.05	-0.09	-0.09	0.00	-0.02	0.12	0.13
	(0.10)	(0.10)	(0.13)	(0.15)	(0.17)	(0.18)	(0.20)	(0.33)	(1.16)
Monthly Earnings, Single	4,003	4,315	4,600	5,000	5,445	5,917	6,580	7,398	9,079
Monthly Earnings, Couple	7,606	8,233	8,700	9,347	10,000	10,688	11,500	12,671	16,383

C. Households Reporting that They Used Rebate to Reduce Debt (N=11,113)

Quantile	10	15	20	25	30	35	40	45	50
Rebate	0.00	-0.02	-0.02	-0.08	-0.10	-0.11*	-0.10	-0.11*	-0.11*
	(0.10)	(0.09)	(0.09)	(0.07)	(0.06)	(0.06)	(0.07)	(0.05)	(0.06)
Lagged Rebate	-0.03	-0.04	-0.03	-0.09	-0.08	-0.11**	-0.08	-0.09**	-0.06
	(0.09)	(0.06)	(0.07)	(0.05)	(0.06)	(0.05)	(0.05)	(0.04)	(0.05)
Monthly Earnings, Single	27	492	762	1,048	1,348	1,644	1,921	2,165	2,430
Monthly Earnings, Couple	1,440	2,064	2,583	3,076	3,534	4,012	4,435	4,877	5,306

Quantile	55	60	65	70	75	80	85	90	95
Rebate	-0.10	-0.11	-0.13*	-0.12	-0.12	-0.11	-0.05	0.01	0.14
	(0.07)	(0.07)	(0.07)	(0.07)	(0.08)	(0.11)	(0.10)	(0.13)	(0.48)
Lagged Rebate	-0.05	-0.07	-0.09	-0.07	-0.05	-0.02	-0.03	0.00	0.27
	(0.04)	(0.05)	(0.05)	(0.07)	(0.06)	(0.09)	(0.08)	(0.12)	(0.37)
Monthly Earnings, Single	2,685	3,000	3,318	3,625	4,000	4,417	5,000	5,833	7,083
Monthly Earnings, Couple	5,785	6,271	6,780	7,366	8,029	8,819	9,834	10,953	12,338

Notes: ***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. Samples selected on households that received a rebate and with at least one member reporting that rebate was used mostly to increase spending (Panel A), increase savings (Panel B), or reduce debt (Panel C). Interactions based on month, household size, marital status, and month-relative-to-interview also included in quantile function.

Table 13: Employment Status and Absences

	A. Monthly Job Status					
	1(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	-0.002	0.001	0.002*	0.000	-0.001	-0.001
(/$1,000)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Lagged Rebate	-0.001	0.001	0.001	-0.001	0.000	0.000
(/$1,000)	(0.002)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
	B. Unpaid Absences					
	Unpaid Absence	Illness or Injury	Personal Days	Layoff	Slack Work	Taking Care of Children
Rebate	0.010***	0.001	0.004**	-0.001	0.003	0.000
(/$1,000)	(0.004)	(0.001)	(0.002)	(0.002)	(0.002)	(0.000)
Lagged Rebate	-0.002	-0.001	0.001	-0.002	0.000	-0.000
(/$1,000)	(0.003)	(0.001)	(0.002)	(0.001)	(0.002)	(0.000)

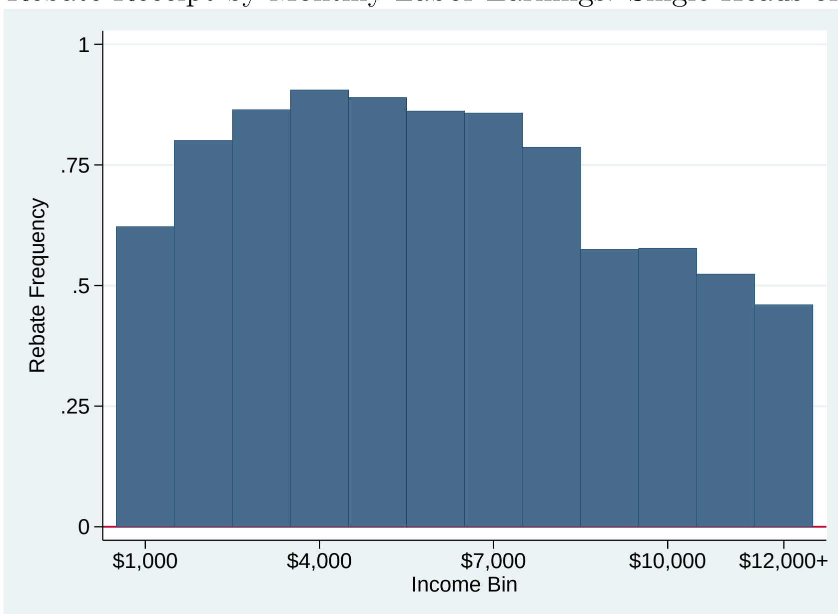
Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Two-Stage Least Squares estimates reported. Standard errors are adjusted for clustering. Individual fixed effects and interactions based on month, household size, marital status, and month-relative-to-interview also included in regressions. All estimates refer to effect of a $1,000 rebate. The SIPP variables include other categories than those listed, but the estimated effects on the rebate variables for these outcomes are small and never statistically significant from zero at the 10% level so they are suppressed.

A Appendix: Additional Results

A.1 Distribution of Rebates by Labor Earnings

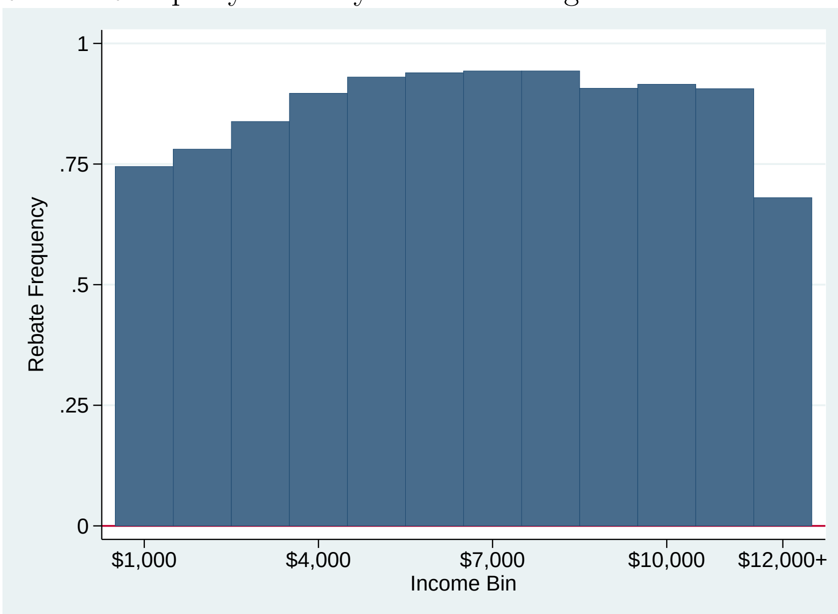
Figures A.1 and A.2 show the frequency of rebate receipt by December 2008 earnings for singles and married couples, respectively. The fraction of households receiving rebates is high even at the top of the observed earnings distribution.

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



Notes: "Income bins" are based on every $1,000 of monthly household earnings. The fraction of households receiving rebates at any point in 2008 were calculated for each income bin. Income bins based on December 2008 labor earnings given that December earnings are less likely to be affected by the rebates due to the schedule for distributing the rebates.

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

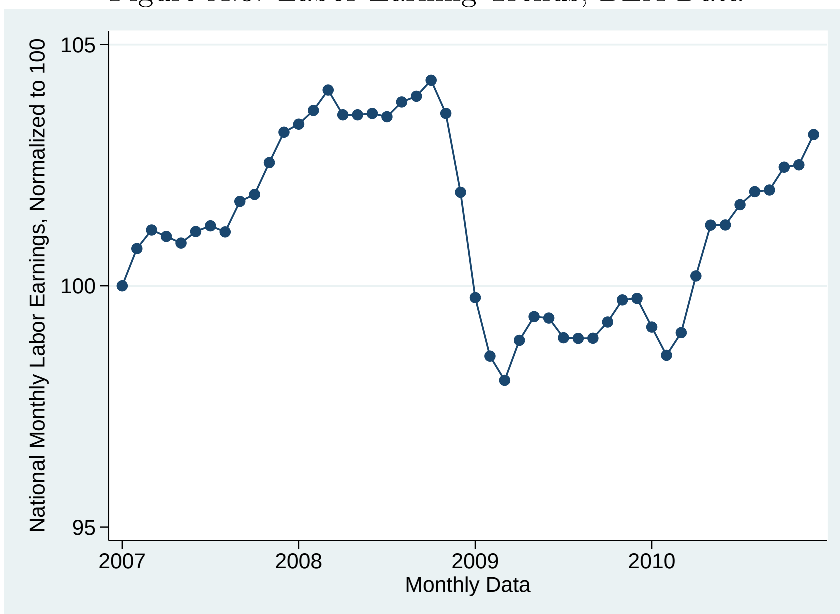


Notes: "Income bins" are based on every $1,000 of monthly household earnings. The fraction of households receiving rebates at any point in 2008 were calculated for each income bin. Income bins based on December 2008 labor earnings given that December earnings are less likely to be affected by the rebates due to the schedule for distributing the rebates.

A.2 National Labor Earning Trends

The sample time period includes large changes in national labor earnings. Using data from the Bureau of Economic Analysis, I construct monthly labor earnings for the United States from January 2007 to December 2011. I normalize the trend to 100 in January 2007 and present it in Figure A.2.

Figure A.3: Labor Earning Trends, BEA Data



Notes: Data are from the United States Bureau of Economic Analysis (BEA). This series is normalized to equal 100 in January 2007.

A.3 First Stage

Table A.1: First Stage

	Rebate	Lagged Rebate
$\mathbf{1}(\text{Rebate} > 0)$	879.72***	-9.82***
	(4.71)	(1.34)
$\mathbf{1}(\text{Lagged Rebate} > 0)$	-0.649	875.04***
	(1.09)	(3.90)
Partial F-Statistic	34,893.63	50,311.74
N	183,984	183,984

Notes: ***Significance 1%, ** Significance 5%, * Significance 10%. Standard errors in parentheses adjusted for household clustering. Other controls include household fixed effects and interactions based on marital status, household size, month, and month relative to interview month.

A.4 Seam Bias

Table A.2: Selecting on Reporting Month Observations

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.05	-0.30	-0.26	-0.17	-0.27*	-0.24*	-0.17	-0.22*	-0.24
	(0.24)	(0.21)	(0.17)	(0.16)	(0.14)	(0.14)	(0.12)	(0.12)	(0.18)
Lagged Rebate	-0.01	-0.11	-0.09	-0.08	-0.17*	-0.17*	-0.21**	-0.16*	-0.18*
	(0.17)	(0.12)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.09)	(0.11)
Monthly Earnings, Single	433	510	450	706	1,000	1,396	1,732	2,078	2,417
Monthly Earnings, Couple	1,426	2,033	2,600	3,200	3,745	4,294	4,800	5,323	5,833
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.24	-0.24*	-0.22	-0.22	-0.16	0.01	-0.29	-0.63	-0.25
	(0.15)	(0.13)	(0.17)	(0.18)	(0.27)	(0.21)	(0.52)	(0.56)	(1.58)
Lagged Rebate	-0.12	-0.16	-0.17	-0.21*	-0.22*	-0.15	-0.21	-0.54	-0.61
	(0.11)	(0.10)	(0.13)	(0.12)	(0.12)	(0.13)	(0.24)	(0.38)	(0.71)
Monthly Earnings, Single	2,739	3,096	3,456	3,897	4,333	4,937	5,606	6,719	8,538
Monthly Earnings, Couple	6,400	7,000	7,639	8,333	9,160	10,033	11,057	11,410	n/a

Notes: ***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 these households were dropped due to censoring concerns. Sample includes only reporting months to eliminate seaming bias. Interactions based on month, household size, marital status, and month-relative-to-interview also included in quantile function.

A.5 Heterogeneity by Marital Status

I examine whether different types of households responded differentially to the economic stimulus payments. Table A.3 selects on single heads of households. There is little evidence of a rebate effect for this group, except at the very bottom of the distribution. Table A.4 presents the corresponding estimates for married couples. I find larger effects for married couples, suggesting that much of the overall effect is a "secondary earner" effect.

Table A.3: QTE Estimates: Single Heads of Households Only

Quantile			**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate			-0.57***	0.06	-0.05	-0.15	-0.03	-0.07	-0.05
			(0.20)	(0.17)	(0.11)	(0.09)	(0.07)	(0.07)	(0.08)
Lagged Rebate			0.17	0.05	-0.05	-0.07	-0.07	-0.09	-0.04
			(0.25)	(0.12)	(0.07)	(0.06)	(0.05)	(0.06)	(0.06)
Monthly Earnings, Single			493	779	1,060	1,458	1,778	2,100	2,425
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.04	-0.04	-0.05	-0.03	-0.04	-0.02	-0.12	-0.19	-0.05
	(0.07)	(0.09)	(0.08)	(0.08)	(0.13)	(0.11)	(0.14)	(0.21)	(0.50)
Lagged Rebate	-0.02	-0.04	-0.05	-0.02	0.00	-0.01	-0.14	-0.08	-0.22
	(0.06)	(0.07)	(0.06)	(0.06)	(0.09)	(0.08)	(0.12)	(0.16)	(0.26)
Monthly Earnings, Single	2,760	3,118	3,479	3,917	4,342	4,966	5,685	6,767	8,697

Notes: Number of households = 9,651. ***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 only includes households with a single head of household in each month. Interactions based on month, household size, marital status, and month-relative-to-interview also included in quantile function.

Table A.4: QTE Estimates: Married Couples Only

Quantile	**10**	**15**	**20**	**25**	**30**	**35**	**40**	**45**	**50**
Rebate	-0.04	-0.07	-0.09	-0.18***	-0.15***	-0.13**	-0.16***	-0.14***	-0.11
	(0.09)	(0.07)	(0.09)	(0.07)	(0.06)	(0.05)	(0.06)	(0.05)	(0.07)
Lagged Rebate	-0.04	-0.04	-0.05	-0.13***	-0.13***	-0.09*	-0.12**	-0.07	-0.05
	(0.06)	(0.05)	(0.07)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)
Monthly Earnings, Couple	1,430	2,083	2,674	3,306	3,824	4,344	4,905	5,409	5,921
Quantile	**55**	**60**	**65**	**70**	**75**	**80**	**85**	**90**	**95**
Rebate	-0.10	-0.11	-0.11	-0.14*	-0.17	-0.04	0.36		
	(0.07)	(0.07)	(0.07)	(0.08)	(0.10)	(0.11)	(0.34)		
Lagged Rebate	-0.05	-0.08	-0.05	-0.04	-0.15**	-0.05	0.22		
	(0.05)	(0.06)	(0.05)	(0.06)	(0.06)	(0.07)	(0.11)		
Monthly Earnings, Couple	6,459	7,064	7,682	8,356	9,167	10,088	11,060		

Notes: Number of households = 13,202. ***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 selected on households with a married head of the household in each month. Interactions based on month, household size, marital status, and month-relative-to-interview also included in quantile function.

B Appendix: QRPD Details

In this section, I discuss QRPD more generally, modeling outcomes (Y_{it}) as a function of treatment variables (D_{it}). For further technical details, consult Powell (2016). Quantile models allow the parameters to vary based on a nonseparable disturbance term. Powell (2016) follows Chernozhukov and Hansen (2006, 2008) to model outcomes as

$$Y_{it} = D_{it}'\beta(U_{it}^*), \quad U_{it}^* \sim U(0,1), \tag{6}$$

where $D_{it}'\beta(\tau)$ is strictly increasing in τ. U_{it}^* represents (unobserved) labor market ability or proneness for the outcome (Doksum (1974)). It is a rank variable (determined by an arbitrary number of unobserved disturbances), normalized to be uniformly-distributed between 0 and 1. The relationship between the treatment variables and the outcome is not constant but, instead, dependent on rank in the underlying ability distribution. To discuss how the model includes fixed effects, let $U_{it}^* = f(\alpha_i, U_{it})$. In words, proneness for the outcome is an unknown function of a household fixed effect and an observation-specific disturbance term. The parameters vary based on this unobserved term, permitting households with high-earning potential to respond differently than households with low-earning potential to the treatment variables. The function $f(\cdot)$ and the fixed effects are never explicitly modeled or estimated, permitting an arbitrary relationship between α_i and U_{it}^*.

The quantile function of interest is

$$S_Y(\tau|D = d) = d'\beta(\tau), \quad \tau \in (0,1). \tag{7}$$

This equation defines the τ^{th} quantile of the outcome variable given treatment variables $D = d$. If the treatment variables were randomized, then estimation of the quantile function would be straightforward using quantile regression (Koenker and Bassett (1978)). Otherwise, alternative methods are needed, but estimation of this quantile function is still possible. Given quantile function (7), the quantile treatment effects represent the causal effect of a change of the treatment variables from d_1 to d_2 on Y_{it}, holding τ fixed:

$$d_2'\beta(\tau) - d_1'\beta(\tau). \tag{8}$$

For the sake of comparison with the moment conditions for QRPD, instrumental variables quantile regression relies on the moment conditions:[37]

$$E\left[Z_{it}\left[\mathbf{1}(Y_{it} \leq D'_{it}\beta(\tau)) - \tau\right]\right] = 0. \tag{9}$$

This condition is derived from the assumption $P\left(Y_{it} \leq D'_{it}\beta(\tau)|Z_{it}\right) = \tau$. This assumption can be relaxed with panel data, allowing the probability to vary by household and even within-household over time. Some households are more likely to have monthly labor earnings below the quantile function; some households are less likely. It is important to permit this heterogeneity. QRPD relaxes the restriction in equation (9) that all observations have the same probability that the outcome is less than the quantile function. Let $\overline{Z}_i \equiv \frac{1}{T}\sum_{t=1}^{T} Z_{it}$ and $Z_i = (Z_{i1}, \ldots, Z_{iT})$. QRPD replaces the moments represented in equation (9) by the following moment conditions:

$$E\left\{\frac{1}{T}\sum_{t=1}^{T}\left[\left(Z_{it} - \overline{Z}_i\right)\left[\mathbf{1}(Y_{it} \leq D'_{it}\beta(\tau))\right]\right]\right\} = 0, \tag{10}$$

$$E\left[\mathbf{1}(Y_{it} \leq D'_{it}\beta(\tau)) - \tau\right] = 0. \tag{11}$$

Equation (10) states that within-household variation in the instruments is uncorrelated with the probability that the outcome is less than (or equal to) the quantile function. A benefit of this approach is that there are no restrictions on $P(Y_{it} \leq D'_{it}\beta(\tau)|Z_i)$. Each household has a household-specific probability of labor earnings smaller than the quantile function and, in fact, this probability can change within-household (over time) as long as the changes are orthogonal to within-household variation in the instruments. On average, the probability is equal to τ (equation (11)), but the household-specific probabilities vary. This variation permits arbitrary correlations between $P(Y_{it} \leq D'_{it}\beta(\tau))$ and the *levels* of the instruments, which parallels the benefits of conditioning on fixed effects in mean regression. This is the gain from using panel data. Put differently, instrumental variables quantile regression restricts $U^*_{it}|Z_{it} \sim U(0,1)$ while QRPD does not place restrictions on the conditional distribution of U^*_{it}. While the unconditional distribution is the same in both case ($U^*_{it} \sim U(0,1)$), QRPD only requires the assumption $U^*_{it}|Z_i \sim U^*_{is}|Z_i$.[38]

[37] Chernozhukov and Hansen (2008) introduces an inverse quantile regression method to simplify estimation which does not use these moment conditions specifically. The above condition is more comparable to the approach taken in this paper.

[38] Powell (2016) discusses less restrictive assumptions as well.

The method still estimates the same quantile function, represented in equation (7), as a cross-sectional quantile regression would estimate, assuming the identification conditions met for cross-sectional quantile regression were met (i.e., $U_{it}^*|Z_{it} \sim U(0,1)$). QRPD relaxes these assumptions, but the resulting estimates can still be interpreted as the effects of the treatment variables on the τ^{th} quantile of the outcome distribution.

B.1 Comparison to Additive Fixed Effects Quantile Estimators

A growing literature has developed quantile panel data estimators with additive fixed effects, including Koenker (2004), Harding and Lamarche (2009), Lamarche (2010), Canay (2011), Galvao Jr. (2011), Ponomareva (2011), and Rosen (2012). 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 the same manner as cross-sectional quantile estimates. In many applications, the motivation for using quantile regression is to allow the parameters of interest to vary based on the nonseparable disturbance term, but additive fixed effect models assume the parameters do not vary based only on the fixed effect. Separating α_i in these cases undermines the typical motivation for quantile estimation. QRPD permits estimation of the distribution of $Y_{it}|D_{it}$ while conditioning on household fixed effects.

Furthermore, much of the additive fixed effects quantile literature is focused on estimating the additive fixed effects, which can be difficult in a quantile model, and requires the number of time periods T to be large. QRPD circumvents these issues since the nonadditive fixed effects are never estimated (or even specified) and the estimates are consistent for small T.

The motivation for QRPD is that researchers often want to study the effect of the treatment variables on the outcome distribution, but it is necessary (or simply desirable) to condition on fixed effects for identification. The additive fixed effect model requires altering the quantile function. QRPD permits estimation of the same quantile function (equation (7)), while conditioning on fixed effects for identification purposes.

QRPD produces consistent and asymptotically normal estimates under standard regularity conditions for small T. Estimation of the variance-covariance matrix modifies the approach introduced in Powell (1986) and accounts for within-household clustering.[39]

[39] Bootstrapping produces similar standard errors for the estimates presented in this paper.

B.2 QRPD Estimation

Powell (2016) introduces an estimation technique which allows for simple grid-searching techniques when there are only one or two treatment variables, regardless of the number of time fixed effects.[40] Estimation uses GMM with the sample moment equivalents of equations (10) and (11). The proposed estimator performs well in simulations. I include simulation results in Appendix Section B.3. The simulation results show that the estimator provides consistent estimates even when instrumental variables quantile regression and additive fixed effect quantile estimators do not.

B.3 Simulations

I generate the following data:

$$\begin{aligned} Z_{it} &= \alpha_i + \psi_{it}, \\ D_{it} &= Z_{it} + U_{it}, \\ Y_{it} &= U^*_{it}(\delta_t + D_{it}), \end{aligned}$$

where $\alpha_i, U_{it}, \psi_{it} \sim U(0,1)$. U^*_{it} is the CDF of $\alpha_i + U_{it}$ and is, consequently, distributed $U(0,1)$. D_{it} is a function of U_{it} so instrumental variables are necessary. Z_{it} is exogenous *conditional on* α_i. The impact of D_{it} is a function of U^*_{it} and varies by observation. The coefficient on D_{it} in the SQF is equal to τ. I generate these data for $N = 500, T = 2$. Table B.1 presents the results of the simulation for the coefficient of interest. I first show results using IVQR (Chernozhukov and Hansen (2006)) to illustrate that the generated data do not meet the assumptions (e.g., $U^*_{it}|Z_{it} \sim U(0,1)$) required to use cross-sectional quantile estimation techniques. I also show results using IVQRFE (Harding and Lamarche (2009)), which assumes an additive fixed effect. Again, the assumptions for this estimator are not met and the estimator, as expected, performs poorly on the generated data. The QRPD estimator performs well throughout the distribution.

B.4 Isolating Intensive Margin Effects

In Section 6.2, I study whether the total rebate effect is driven by intensive margin decisions by eliminating households with no labor earnings. This approach should bias the estimates

[40] "Time fixed effects" are any fixed effects in the quantile function which saturate the space, such as month fixed effects. In this paper, I use interactions based on family size, marital status, month, and month-relative-to-interview.

Table B.1: QRPD Simulation (N=500, T=2)

	IVQR			IVQRFE			QRPD		
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).

if extensive margin decisions are not random. However, simply eliminating households with no labor earnings would drastically alter the relationship between quantiles and household earnings. Instead, I simply set equation (10) to zero for households with no labor earnings. This approach shuts down extensive margin decisions since different values of $\beta(\tau)$ have no effect on (10) for these observations. However, I keep the households in equation (11) to shift the distribution appropriately. This approach works because $\mathbf{1}(Y_{it} \leq D_{it}'\beta(\tau))$ is equal to 1 for all households with no labor earnings, regardless (within reasonable values) of the estimate of $\beta(\tau)$, such that correctly estimating the quantile function for these households is unnecessary for equation (11). Consequently, estimation effectively ignores extensive margin decisions, and the estimates refer only to intensive margin labor supply decisions. The estimates still refer to similar parts of the distribution as the main estimates of the paper because the observations are used in equation (11).

Alternatively, I could select on observations with positive labor earnings but this would dramatically change the map between quantiles and household earnings. The implemented approach is equivalent to excluding the households with zero earnings but not selecting the sample for equation (11), which allows the quantiles to generally refer to the same labor earnings.

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