Essays in Public Finance

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Abstract

My dissertation consists of three chapters on topics in public finance. Chapter 1 studies optimal disability insurance (DI) in two parts. In the first part, I show that the well-established result that DI reduces labor supply is driven largely by making it feasible for the disabled to stop working, rather than by reducing effective wages. Therefore, DI is very valuable because it operates through a non-distortionary (and welfare enhancing) income effect, rather than a price or substitution effect. In the second part of my paper, I show that externalities and internalities create unique challenges for designing DI systems. I study an institutional setting where 80% of the population receives a payment on the same day each month. I find that the probability of an emergency room visit increases for DI beneficiaries, but not others, when monthly income is received, and this response to payments is present even in the years before they were granted DI benefits. The results imply that optimal policy may involve non-traditional policy tools.

Chapter 2 presents evidence on one such alternative policy tool, in-kind transfers, in the context of food stamp benefits in the United States. In November 2013, temporary benefit increases in the American Recovery and Reinvestment Act expired, resulting in lower benefits for all Supplemental Nutrition Assistance Program (SNAP) households. I use scanner data from 400 grocery stores and over 2.5 million SNAP households in Atlanta, Los Angeles, and Columbus, OH to estimate the effect of the benefit cuts on household spending. I find that the impact per household was relatively small, but the aggregate impact was large because 23 million households were affected in November 2013. In contrast, subsequent

legislation passed in February 2014 will impact relatively few households, leading to a much smaller aggregate impact.

Chapter 3 shows that, in addition to ER visits, consumption of hard alcohol, crime, and traffic accidents all increase nationwide in Denmark when 80% of the population receives income each month. Deaths may also increase. This evidence runs counter to standard theories of how households make consumption decisions and sheds new light on potential explanations.

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To Sean, Cheryl, and Michael

Introduction

Chapter 1 presents new evidence on the costs and benefits of disability insurance (DI). I start by estimating the effect of unconditional transfers on the labor force participation of DI beneficiaries in Denmark. I show that a large fraction of the impact of DI benefits on labor supply can be attributed to non-distortionary income effects (i.e., making it feasible for disabled workers to "afford" not to work) rather than distortionary price effects (i.e., reducing effective wages). I then show evidence that DI beneficiaries respond very differently to cash-on-hand than do non-disabled populations. In particular, the probability of an emergency room visit increases for DI beneficiaries, but not other groups, when monthly income is received, and this response to payments is present even in the years before they were granted DI benefits. This "excess sensitivity" creates fiscal externalities and may be caused by behavioral biases. These results imply that the standard approaches to welfare analysis may need to be modified to study DI and that optimal policy may involve setting the frequency at which payments are dispersed in addition to benefit levels.

Chapter 2 studies another alternative policy: food stamp benefits in the United States. In November 2013, all SNAP benefits were reduced for the first time in the program's history when temporary increases in the American Recovery and Reinvestment Act expired. I quantify the impact of these cuts using scanner data from 400 grocery stores and the purchases of over 2.5 million households enrolled in SNAP. I estimate that each $1 of cuts reduced grocery store spending by $0.37. Importantly, the implied marginal propensity to consume food out of food stamps is more precisely estimated than in previous studies, at 0.3 with a 95% confidence interval of [0.154, 0.456]. The revenue impact for the U.S. grocery retailing industry is estimated to be a decline of 0.3% overall. In contrast, I project that the aggregate impact of the 2014 Farm Bill will be an order of magnitude lower.

Chapter 3 returns to Denmark and shows that, in addition to ER visits, consumption of hard alcohol, crime, and traffic accidents all increase nationwide in Denmark when 80% of the population receives income each month. Deaths may also increase. The magnitude of the increases in ER visits, crime, and traffic accidents are positively correlated with one another. There is no systematic relationship with alcohol taxes, weather conditions, or unemployment rates. This evidence runs counter to standard theories of how households make consumption decisions and sheds new light on potential explanations.

Chapter 1

How Do Disability Insurance Beneficiaries Respond to Cash-on-Hand? New Evidence and Policy Implications

1.1 Introduction

Over the last sixty years, modern governments have increasingly devoted resources to providing insurance against adverse events, such as disability, job loss, and illness (Gruber 2007). Of these social insurance programs, disability insurance (DI) is potentially one of the most valuable, but also one of the most costly. It is valuable because the impact of severe disabilities on income and consumption is large and long-lasting (e.g., Meyer and Mok 2013). Further, the private DI market is unlikely to provide adequate insurance coverage because of strong information asymmetry (e.g., Hendren 2013). Disability insurance is costly, because once awarded benefits, exit from the labor force tends to be permanent, so that DI becomes a lifetime annuity. In the United States, DI rolls have expanded as the generosity of the program has increased and screening stringency has been relaxed (Autor and Duggan 2006).

There is now a large empirical literature which has uniformly found causal evidence that disability insurance reduces labor supply (e.g., Maestas, Mullen, and Strand 2013).[1]

However, the normative implications of this relationship are unclear. One interpretation is that these trends and causal evidence reflect that the program is very distortionary, because it reduces effective wages by imposing large implicit tax rates on earnings.[2] Alternatively, the evidence could reflect that the program is very valuable, because it makes it feasible to stop working when one's disability has made remaining in the labor force very costly. In this case, the decline in labor force participation is simply a result of providing resources to those who need it and, in fact, is desirable.[3] The welfare implications of DI and optimal benefit levels depend on quantifying the relative contributions of each of these mechanisms (Chetty 2008).

This paper makes two main contributions. First, I shed new light on the value of disability insurance by measuring the labor supply responses of DI beneficiaries to resources that do not affect marginal incentives to earn (i.e., cash-on-hand). In particular, I measure the labor supply responses of Danish disability insurance beneficiaries to a policy change that affected resources available, but did not preclude them from working (as in Autor and Duggan 2007 and Marie and Castello 2012). I find that labor supply is very responsive to these unconditional transfers. These estimates suggest that the effect of DI on labor force participation is driven in large part by making it feasible for the disabled to stop working, rather than by reducing effective wages. The standard interpretation would be that DI is very valuable from a social welfare perspective.

The second contribution of the paper is to show that this standard approach may need to be modified in order to be applied to disability insurance, because this group's behavioral responses to income generate fiscal externalities and, perhaps, internalities. Using high frequency emergency room (ER) data, I show that the probability of an adverse health

[1] An exception is Campolieti (2004), who reports wide confidence intervals.

[2] For example, Maestas and Song (2010) note that SSDI imposes an implicit marginal tax rate on earnings of up to 100,000 percent.

[3] Indeed, a program designed to maximize labor supply would set benefits to zero at zero hours worked.

event increases for DI beneficiaries in the days after they receive income. Non-disabled groups show no such increases around payment dates. Excess sensitivity in this domain creates fiscal externalities, because ER visits are costly to the government. In a model with externalities or internalities, labor supply responses are no longer sufficient statistics for optimal disability insurance benefit levels. Optimal benefit levels could differ depending on the frequency at which payments are dispersed.

The policy implications of my results are twofold. First, disability insurance appears to be very valuable based on the labor supply evidence. Second, optimal policy may involve non-traditional policy tools such as setting pay frequency. My on-going work evaluates this proposal by testing whether smaller, more frequent payments can smooth rates of adverse health events and reduce rates of adverse health events overall using policy variation in pay frequency of the child benefit in Denmark (Bruich, Nielsen, Simonsen, and Wohlfahrt 2014).[4]

The labor supply evidence builds on a large literature measuring labor supply responses to disability insurance. The estimates I present are for near-retirement age disability insurance beneficiaries, as in Maestas and Yin (2008) and Maestas and Song (2011). In the DI literature on labor supply, only Autor and Duggan (2007, 2008) and Marie and Castello (2012) emphasize the distinction between moral hazard and income effects. The literature on unemployment insurance has been much more active in separately identifying income versus substitution effects (e.g., Card, Chetty, Weber 2007, Chetty 2008, LaLumia 2013, Landais 2014). In this way, this paper presents some of the first evidence on income effects in the disability insurance context and confirms that income effects are large.

To obtain these causal estimates, I use a difference in difference research design around age 65, comparing labor supply across cohorts using a discontinuity in eligibility for a policy change by date of birth. The payments associated with this policy change did not affect

[4]While many studies suggest using pay frequency as a policy tool (e.g., Shapiro 2005, Dobkin and Puller 2007, Mastrobuoni and Weinberg 2009), there is little causal evidence on the impacts of this proposal, in part because changes to pay frequency are relatively rare. One example is Stephens and Unayama (2011) who study the effect of a change in pay frequency of Japanese pensions on consumption smoothing.

marginal incentives to earn. For example, the amount was not taxed, was not reduced with income, and was designed not to affect eligibility for other resources. I focus on a sample of disability insurance beneficiaries over 60, who cannot have their benefits taken away, regardless of how much they work. I argue that these institutional features allow me to interpret these estimates as capturing only an income effect.

The evidence on ER visits has not been shown previously in Denmark, but relates to findings in other countries. This literature has found that expenditures, consumption, mortality, and hospitalizations increase in the short run after both transitory and reoccurring payments are received. As in Dobkin and Puller (2007) and Gross and Tobacman (2014), I find that drug and alcohol related ER visits increase around payment dates.[5] I also find that ER visits for head injuries, which are rarely coded as drug and alcohol related in these Danish data, but may nevertheless be due to drugs and alcohol, increase at these times.

This paper builds on that evidence in three ways. First, I document heterogeneity across groups and characterize *which* disability insurance beneficiaries present at the ER, similar to a recent literature on heterogeneity in consumption smoothing (e.g., Mastrobuoni and Weinberg 2009, Parker 2014). Second, I find that this response to income is exhibited by the disabled even in years before they were awarded DI benefits. This finding suggests that these increases in adverse health events are not a direct result of being on disability insurance rolls per se. Instead, these responses are predictive of receiving DI in the future.

Third, the institutional setting allows me to rule out several proposed explanations. For example, healthcare coverage is universal in Denmark and patients owe no copayments for visits to the ER. In this way, the results documented here are not likely to be driven by the ability to pay for medical care, which has been shown to be important in other contexts, such as in the Oregon Health Insurance Experiment (Taubaum et al. 2014). The results also seem inconsistent with the explanation that the increases are due to heightened activity in

[5]Dobkin and Puller (2007) show that Supplemental Security Income (SSI) beneficiaries' in-patient hospitalizations related to cocaine, heroin, and amphetamines increase when they receive payments. They also find that in-hospital mortality increases. Gross and Tobacman (2014) show that ER visits related to drugs and alcohol increased when the 2008 stimulus payments were received.

the general population (e.g., Evans and Moore 2011, 2012), because I find that it is largely only the disabled who present at the ER, even though over 60% of the population receives income at the same time in Denmark.[6] Instead, the evidence seems to point to models of addiction and imperfect self-control (Gruber and Koszegi 2004, Bernheim and Rangel 2004).

Finally, the paper is related to the behavioral public economics literature (Mullainathan, Schwartzstein, and Congdon 2011). The evidence I present in this paper shows that there is a subset of DI beneficiaries whose behavior and consumption choices may generate fiscal externalities and internalities. A theme in the behavioral public economics literature is that when there is a mixture of behavioral and rational agents, corrective policies are desirable even though they reduce the welfare of rational agents. Intuitively, the welfare gains from bringing behavioral agents closer to the social optimum are first order, while the welfare losses are only second order for the agents who locate at the social optimum on their own. Other policies (nudges) which would have no effect on rational agents, but would change the behavior of the non-rational agents, are even more desirable. Insights from behavioral economics have been applied to the optimal design of taxes and some social insurance programs (e.g., Feldstein 1985, Gruber and Koszegi 2004, O'Donoghue and Rabin 2006). But so far this has not occurred in the disability insurance literature, which is somewhat surprising given the prevalence of mental impairments and substance abuse in this population.

The rest of this chapter is organized as follows. In Section 1.2, I present a static extensive margin labor supply model. I derive the standard result that the optimal disability insurance benefit level can be characterized by the ratio of income effects to moral hazard effects of DI benefits on labor supply. In Section 1.3, I provide institutional details on disability insurance eligibility, benefit levels, the quasi experiment used to identify income effects, and payment dates. I also describe the various datasets used in the paper. In Section 1.4, I

[6] I also find no evidence of an increase in ER visits for heart attacks or strokes, which contrasts with the finding that deaths for these reasons do increase in the United States (Evans and Moore 2011) and Sweden (Andersson, Lundborg, Vikström 2014). One possibility is that the underlying mechanisms that generate deaths and ER visits differ.

estimate the effect of unconditional transfers on the labor supply of disability insurance beneficiaries, which is one of the parameters in the formula presented in Section 1.2. In Section 1.5, I present the results on ER visits in three parts. First, I establish that ER visits increase nationwide in Denmark when a large fraction of the population receives income. Second, I show that, disproportionately, it is those receiving disability insurance benefits and those who will receive it in the future who are coming to the ER on these days. Third, I characterize which disability insurance beneficiaries display this excess sensitivity. Section 1.6 concludes by discussing the welfare and policy implications of these results.

1.2 Moral hazard vs. liquidity and optimal DI

In this section, I review the result that the optimal level of social insurance benefits can be characterized by the ratio of income effects to moral hazard effects of DI benefits on labor supply, as in Chetty (2008). This result provides a framework for interpreting the empirical results.

Consider a static model with a binary labor supply choice.[7] Individuals, indexed by i, all earn the same wage w if they choose to work, but have disutility of working δ_i that is distributed smoothly in the population with cumulative distribution function $F(\delta_i)$. I abstract from the issue of screening for disability because my empirical application focuses on a sample that has already been awarded disability insurance benefits. The government can observe whether i works, but not his disutility δ_i. Disability insurance is a benefit b paid when not working that is financed by a tax t paid by those who are working. Assets A are available both when in the labor force and when not working. Let $c_h = w + A - t$ denote consumption if the agent works and $c_l = A + b$ denote consumption if the agent chooses not to work. Utility is additively separable in consumption and disutility from working. Utility over consumption is concave.

An individual i will choose to work if his net utility from working exceeds his net utility

[7]See Golosov and Tsyvinski (2006), Low and Pistaferri (2010), and Denk and Michau (2012) for dynamic models.

when he does not work:

$$u(c_h) - \delta_i > u(c_l) \tag{1.1}$$

In the population, all i with δ_i above the cutoff $\overline{\delta} = u(c_h) - u(c_l)$ will choose not to work. The cutoff decreases as b and A increase and increases as $w - t$ increases. Let the fraction of workers choosing to work be denoted by $e = \int_{-\infty}^{\overline{\delta}} dF(\delta_i)$.

I show in Appendix A.2 that the effect on welfare of increasing the disability insurance benefit by $1 can be written as:

$$\begin{aligned} \frac{dW}{db} &= \frac{u'(c_l) - u'(c_h)}{u'(c_h)} + \frac{\varepsilon_{e,b}}{1-e} && (1.2) \\ &= \frac{-\frac{\partial e}{\partial A}}{\frac{\partial e}{\partial A} - \frac{\partial e}{\partial b}} + \frac{\varepsilon_{e,b}}{1-e} && (1.3) \end{aligned}$$

where $\varepsilon_{e,b} = \frac{de}{db}\frac{b}{e}$ is the total, uncompensated elasticity of the fraction of the population working with respect to b. The second line re-writes the gap in marginal utilities using comparative statics for the effect of wages, disability insurance benefits, and assets on the fraction of the population working ($\partial e/\partial w$, $\partial e/\partial b$, and $\partial e/\partial A$). Equation 1.3 shows that optimal benefit levels only depend on extensive margin labor supply responses to unconditional transfers (A) and state contingent benefits (b).

The first term in the formula uses revealed preferences to infer the value of b. If many workers exit the labor force in response to an increase in unconditional transfers ($\frac{\partial e}{\partial A} \ll 0$), then this implies that increasing b is very valuable, because an extra $1 of consumption is worth much more when not working, than it is worth when working ($u'(c_l) \gg u'(c_h)$). In contrast, if labor supply does not respond to these resources, then this implies that there is little value in increasing b, because that extra $1 of consumption is worth the same in either case ($u'(c_l) \approx u'(c_h)$). Intuitively, this must mean that agents are able to smooth consumption on their own at the current level of benefits and, therefore, that increasing disability insurance benefits further adds no value.[8,9] The second term reflects that transferring an additional \$1 requires taking more than \$1 away from workers, because fewer people work when the net wage is reduced. The two terms should exactly offset each other at the optimum.

To implement this formula, one needs to estimate two partial elasticities of labor force participation or partial marginal propensities to work ($\frac{\partial e}{\partial A}$ and $\frac{\partial e}{\partial b}$), as well as the total, uncompensated elasticity of labor force participation with respect to benefit levels ($\varepsilon_{e,b}$). In Section 1.4, I estimate the response to unconditional transfers ($\frac{\partial e}{\partial A}$). While this estimate alone is not sufficient to implement the formula, note that the hypothesis that $\frac{\partial e}{\partial A} = 0$ implies that increasing benefits cannot improve welfare ($\frac{dW}{db} \leq 0$). Indeed, knowledge that $\frac{\partial e}{\partial A} = 0$ for all b implies that insurance markets are complete and, therefore, optimal social insurance benefits are zero. In this way, one would logically want to determine whether $\frac{\partial e}{\partial A}$ is non-zero in order to decide whether estimates of $\frac{\partial e}{\partial b}$ and $\varepsilon_{e,b}$ are even needed to assess the welfare effects of increasing b.

The labor supply responses in Equation 1.3 are sufficient statistics for optimal benefit levels because these same parameters would still characterize optimal benefit levels in a more general model with other choice variables and constraints. In a model with externalities or internalities, there will be an additional term added to the formula; I give an example showing this in Appendix A.3. The results in Section 1.5 show that this externality/internality term is non-zero.

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[8]The consumption smoothing interpretation is from the perspective of a representative agent. From the agent's decision rule in Equation 1.1, an increase in A must increase $u(c_l)$ and $u(c_h)$ by the same amount in order for $\frac{\partial e}{\partial A} = 0$.

[9]This connection between the labor supply response to cash on hand and consumption smoothing is used by Card, Chetty, and Weber (2007) to distinguish between theories of intertemporal consumption.

1.3 Institutional background and data

1.3.1 Disability insurance system

I describe the disability insurance system that applies to the sample that I use in the labor supply analysis. Specifically, this system applies to individuals who applied for disability insurance before turning 60, but have since turned 60, and whose benefits were awarded before 2002. The quasi experiment is discussed in the next subsection.

Severity levels. Disability insurance can be awarded at three levels of disability severity, with more severe disabilities corresponding to higher monthly benefits. The level at which disability insurance benefits are awarded is based on an assessment of how an applicant's impairment impacts his ability to work; Appendix A.1 provides a detailed example. In this sense, disability insurance eligibility in Denmark is similar to the assessment of eligibility for SSDI, described in detail, for example, in Chen and van der Klaauw (2008). However, unlike in the U.S., disability can be partial. My main analysis sample (described in more detail below) was awarded benefits at the lowest level of severity, which corresponds to a disability that only partially diminishes one's ability to work.

Benefit amounts. In contrast with the Social Security Disability Insurance (SSDI) program in the U.S., benefit levels do not depend on past earnings. There is a residency requirement, but no past work requirement. Therefore, the replacement rate can be quite large, and even infinite for those who are given benefits at age 18 without having spent time in the labor force. Individuals receive taxable, means tested benefits, as well as non-means tested benefits that are tax free.

Work incentives. Importantly, individuals can and do earn labor income while receiving disability insurance benefits. About 15% of those on disability insurance have positive labor income, both in my data (described below) and in the data studied by Geerdsen (2006). Disability insurance recipients who are over 60 cannot have their benefits taken away, regardless of how much they work.[10] However, the returns to earning labor income are very

[10]Benefits can only be taken away at the request of the beneficiary. In personal communication with the

low due to the taxation of the means tested portion of their benefits as personal income, and even more so, once these benefits begin to be phased out at a 30% rate at 64,300 kr ($12,000) in 2012.[11] Appendix Figure A1 shows the combined income tax and phase out rate for disability insurance beneficiaries on the bottom two levels in 2002. The marginal tax rate on the first $1 of earnings is over 40%.

Transition to social security. Beneficiaries continue to receive disability insurance benefits until they reach retirement age. At that point, beneficiaries are transferred to the social security system. They may also become eligible to receive payouts from their private or public savings plans. Social security benefits are lower than the disability insurance amount, because they are essentially the same as the means tested benefit amounts, but do not include any of the lump sum amounts.[12]

1.3.2 Quasi experiment for labor supply analysis

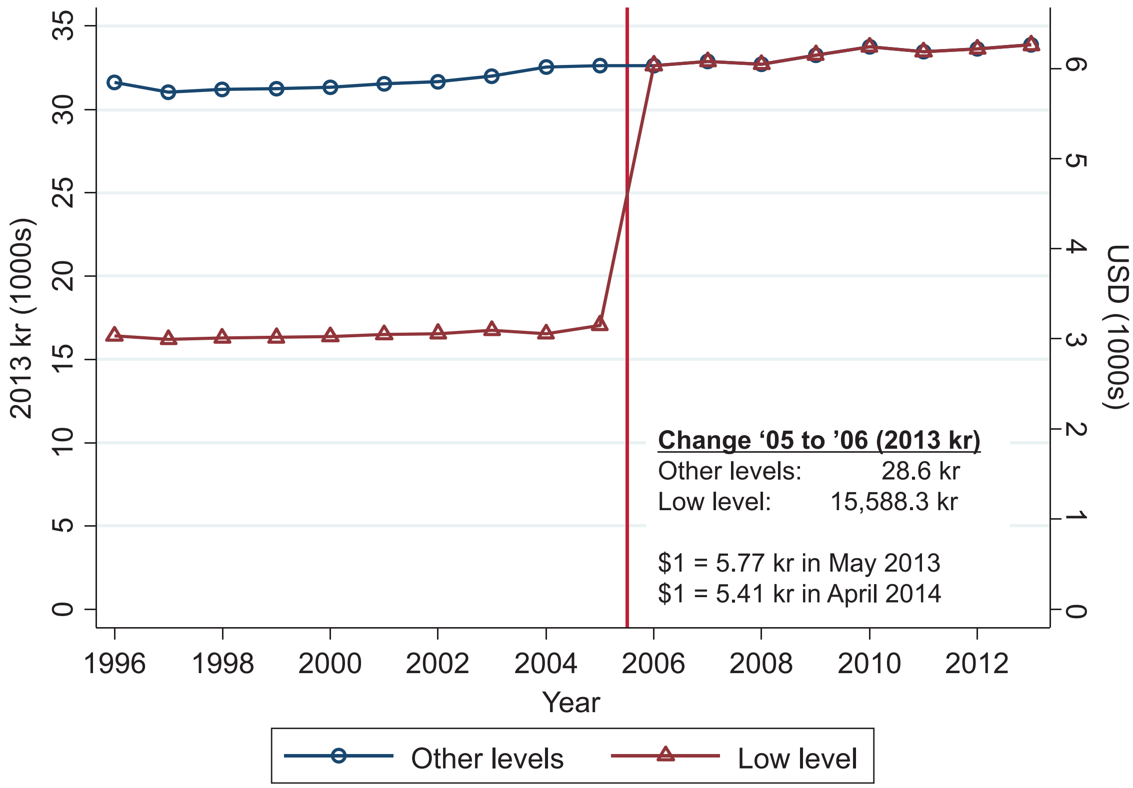
I use policy variation in the non-taxable, non-means tested lump sum benefit amount to identify the impact of unconditional transfers on the labor force participation of disability insurance beneficiaries. As shown in Figure 1.1, the lump sum benefit amount for the low level of disability insurance was increased in 2006 from around $3000 per year to $6000 per

Ministry of Social Affairs and Integration, I have confirmed that these rules have been constant over time. The rules for disability insurance recipients under 60 have changed over time. Currently, this younger group can also work without risk of permanently losing their benefits. However, if they work enough in any given year (earnings above 139,000 kr or around $25,000 in 2013), they may have their disability insurance benefits replaced by a lump sum, tax free amount while they work. However, this is not a sharp cutoff; the threshold may not apply for a number of reasons, described by Geerdsen (2006, footnote 3). Benefits resume at the request of the beneficiary. The amount received while working is approximately the same (228 kr more generous) as the lump sum benefit amount received while on disability insurance benefits, but does not include the means-tested part of their benefits. This contrasts with the situation for those over 60, who are not subject to these limitations. Note though that the main work disincentive in the SSDI program in the United States is the potential for permanent loss of benefits.

[11] The income level cited is for singles. Benefits are phased out at a 60% rate at higher income levels.

[12] If awarded benefits before 2002, the non-means tested benefit amounts continue until turning 67 even for those whose retirement age is 65. The extra two years does not apply to the benefits associated with the quasi experiment described below.

Figure 1.1: *Policy Variation in the Tax Free, Lump Sum Disability Insurance Benefit Amount*



NOTE—This figure plots the yearly lump sum disability insurance benefit amount for beneficiaries on the low (triangles) and higher disability levels (circles). The left y-axis shows the yearly benefit amount in 2013 kroner and the right y-axis shows the amount in dollars using the $\$1 = 5.41$ kroner exchange rate in April 2014.

year.[13]

I argue that changes in labor supply resulting from this quasi experiment can be interpreted as being caused by only an income effect for two reasons. First, the lump sum benefit amount, which was increased in 2006, is not taxable and is not means tested. Therefore, receipt of these additional resources left marginal incentives to earn income unchanged. Note that the fact that these extra payments are untaxed is important: in a progressive income tax system, additional taxable income increases the tax rate paid on

[13]The higher benefit amount was announced in December 2005 and the first extra payments were received in April 2006.

earned income.[14] Second, disability insurance recipients who are over 60 cannot have their benefits taken away, regardless of how much they work. This second point is reminiscent of the arguments made in prior studies of income effects, such as Autor and Duggan (2007) in the U.S. and Marie and Castello (2012) in Spain. For both these reasons, an estimate of the causal effect of this policy change on the fraction of disability insurance beneficiaries working provides an estimate of $\frac{\partial e}{\partial A}$.

The extra payments are only received until beneficiaries reach retirement age. As described in more detail in Section 1.4, my research design uses a difference in difference estimator, comparing cohorts that were affected by the policy change, with those that were ineligible for the increase in benefits because they had already reached retirement age in 2006.

1.3.3 Payment dates

Over 60% of the population receives income on the last business day of the month, which equals the last calendar day of the month unless that day is a weekend or public holiday, in which case the payments are made the day before. Payments received on this day include disability insurance benefits, other transfer and social insurance payments, and salaries for most wage earners.[15] Public benefits and most paychecks are deposited directly into individuals' bank accounts.[16]

1.3.4 Data

Labor supply analysis data. In the labor supply application, I use data on the earnings of disability insurance beneficiaries contained in the registers described in Petersson, Baadsgaard,

[14]Note that VA disability benefits in the U.S. are also not taxed, which is an important but not emphasized feature of Autor and Duggan's (2007, 2008) research design. The basic issue is similar to the high marginal tax rates on spouses of high earners in the United States (e.g., Eissa 1995).

[15]Paydays for wage earners are determined by collective bargaining agreements between unions and employers.

[16]Taxes are withheld from paychecks and public benefits.

and Thygesen (2011) and in Baadsgaard and Quitzau (2011) for years 1990 through 2011. These registers are based on tax records, pension payments, and asset holdings that are third-party reported to the tax authority. All income data are annual measures. Important for my study is that I observe information on the level at which disability insurance benefits have been granted. A limitation in the data is that I do not observe the amount of the lump sum disability insurance payments. However, I believe that compliance with the law as described above is very good.[17] I supplement these data with a data set on chronic diseases that was created to document trends in utilization by people with diabetes, cardiovascular disease, chronic lung disease, musculoskeletal disorders, and mental and behavioral disorders. The mental illnesses include schizophrenia, mood disorders, and dementia. The database is constructed by using ACT active ingredient codes for prescription drugs and ICD-10 diagnosis codes for any type of encounter with the health care system.

I select the main analysis sample as follows. I include anyone whom I observe on the low disability insurance level in the year in which they turn 60 years old. I restrict the sample to only those who were awarded benefits prior to 2002 and prior to turning 60. I make these restrictions because the transition to retirement and benefit amounts differ depending on the year and age when benefits were awarded. Further, I only include birth cohorts whom I observe turning 66 during the period over which I have data, which limits me to individuals born in 1945 and earlier.

Emergency room visit analysis data. To implement the emergency room visit analysis, I use data on all emergency room visits contained in the Danish National Patient Register. Lynge, Sandegaard, and Rebolj (2011) provide a detailed description. The earliest data I use are from 1994, because this is the year in which the register began classifying admissions using ICD-10 diagnosis codes. Note that the reporting of emergency room visits only became mandatory in 1995, but I include visits in 1994 in my analysis because some hospitals did report emergency room visits in this year. My results are not sensitive to including or

[17]Non-compliance would only be due to switching levels prior to the policy change, but switching is rare. In personal correspondence, I have confirmed the date of birth cutoffs with the Ministry of Social Affairs and Integration.

excluding 1994.

I focus on three main groups in the emergency room visit analysis: disability insurance beneficiaries, social security beneficiaries, and wage earners. I define these groups using data on sources of income contained in the registers described for the labor supply analysis.

The hospitalization data are daily, but group affiliations are defined using annual data. For disability insurance beneficiaries and social security beneficiaries, I include individuals who receive that type of income at any point during the year. In addition, the social security sample excludes anyone whom I observe receiving disability insurance income in previous years. To define wage earners, I exclude workers 60 or over, all non-wage earners, wage earners with self-employment income, and wage earners with unemployment benefits. I define wage earners only using information from previous years, because most variables are measured at the end of the current year and wage earner status at year end is endogenous to health events occurring during the year (Cutler, Meara, and Richards-Shubik 2011). I further exclude observations that received DI at any point in the previous two years.

I also use data from Ankestyrelsen that contain up to three reasons that disability insurance benefits were awarded for disability insurance claims in 1999-2012. These data cover approximately 170,000 individuals at the end of the period. I use these data to divide my disability insurance sample into beneficiaries with mental and behavioral disorders and those with all other impairments.

1.4 Effects of Cash Transfers on Labor Supply

In this section, I estimate the effect of unconditional transfers on the labor supply of disability insurance beneficiaries ($\frac{\partial e}{\partial A}$) using the quasi experiment described in Section 1.3. I first lay out my research design in Section 1.4.1 and describe the sample I use to implement my design in section 1.4.2. Section 1.4.3 presents the results.

1.4.1 Research design

In section 1.3, I argued that an estimate of the causal effect of the 2006 policy change on the fraction of disability insurance beneficiaries working provides an estimate of $\frac{\partial e}{\partial A}$. To obtain this causal estimate of $\frac{\partial e}{\partial A}$, I use a difference in difference research design around age 65, comparing labor supply across cohorts using a discontinuity in eligibility for these payments by date of birth. In particular, beneficiaries who turned 65 before 2006 were ineligible for the increase in benefits, because they had already reached retirement age.[18] Beneficiaries turning 65 just before 2006, therefore, provide a control group for beneficiaries who turned 65 just after 2006 (the treatment group). These two groups differ based only on their dates of birth and were both initially assessed at the same level of disability severity (the lowest level). After age 65, both groups are transferred to the social security system, but the treatment group's income declines by \$3000 more than the control group's. Therefore, this research design identifies the effect of a \$3000 reduction in exogenous income, holding all other changes that occur at retirement age fixed. The identification assumption is similar to the common trends needed for a standard difference in difference estimate: in the absence of the 2006 policy change, the change in labor force participation at 65 would have been the same in both groups.

My main point estimates are based on the following estimating equation:

$$y_{itg} = \alpha_i + \alpha_{gt} + \sum_{\substack{a\in[60,69],\\ a\neq 64}} \beta_a 1(age_{it} = a) + \sum_{\substack{a\in[60,69],\\ a\neq 64}} \gamma_a \left[1(age_{it} = a) \times 1(g = \text{treat})\right] + u_{igt} \quad (1.4)$$

where y_{itg} is labor supply for individual i in year t and group g (i.e., treatment group or control group), α_i is an individual fixed effect, and α_{gt} is a year fixed effect interacted with the treatment group indicator to allow time trends to differ in the control and treatment groups. Age a refers to age at year end. The nine β_a coefficients measure the change in labor supply at age a relative to age 64. The nine γ_a coefficients measure how this change

[18] There is also a group of disability insurance recipients on the low level of benefits who turned 67 in 2006 and received 1 to 6 months of the extra benefit, because the retirement age is 67 for those born before July 1, 1939. The retirement age is 65 for those born after this date.

differs for the treated group relative to the control group. The main coefficient of interest is γ_{66}, which measures the change in labor supply at age 66 (the first full year on social security) relative to labor supply at age 64 (the last full year on disability insurance) for the treatment group relative to the control group. I interpret γ_{66} to be the causal effect of the \$3000 reduction in the lump sum benefit amount on labor supply. A static labor supply model would predict $\gamma_{66} > 0$. That is, the treatment group should work more than the control group at age 66 relative to age 64. I cluster standard errors by person.

1.4.2 Summary statistics

Table 1.1 presents summary statistics for the analysis sample. There are 8,476 people in the control group (born in 1939 or 1940) and 16,312 people in the treatment group (born between 1941 and 1945). I follow each group from age 60 to 69, yielding a total of 213,074 person-year observations. In some analyses, I exclude the group born in January to June 1939, because their retirement age is 67 and not 65, leaving 192,156 person-year observations. At age 60, 11% had positive wage earnings. This number drops to 7.3% across ages 60 to 69. A little less than half had positive wage earnings two years before the first year observed on disability insurance.

As shown in Table 1.1, the treatment and control groups are very well balanced along observable characteristics. Four features of this sample should be kept in mind. First, the sample is comprised predominantly (about 70%) of women and the less educated, with only 6% having a college degree; about half is married at age 60 and over 90% is Danish. Second, the sample has received DI for a significant length of time when turning 65 (at least five years). The average first year observed receiving disability insurance is 1994, and this figure understates the length of time spent on DI, because I cannot determine the first year for anyone awarded benefits in 1990 or earlier in these data.[19] Third, chronic diseases are quite common in my sample: 21% has diabetes, 44% has a heart condition, 22% has a lung

[19] Indeed, many other research designs would be possible if I observed the application or award date in these data.

Table 1.1: *Summary Statistics for Labor Supply Analysis Sample*

			Combined (1)	Control group (2)	Treatment group (3)
1.	Year of birth		1939-1945	1939-1940	1941-1945
2.	First year observed on DI		1994	1994	1994
			(3.39)	(2.97)	(3.55)
3.	Male		0.288	0.269	0.298
4.	Married at age 60		0.481	0.506	0.468
5.	Lives in Copenhagen at age 60		0.137	0.135	0.138
6.	College educated		0.0599	0.0582	0.0608
7.	Danish nationality		0.910	0.915	0.907
8.	Characteristics 2 yrs prior to 1st year on DI				
	Fraction with wage income > 0		0.464	0.483	0.455
	White-collar occupation		0.133	0.136	0.131
	Wage income		81,185	89,838	77,053
			(123,310)	(125,975)	(121,806)
9.	Chronic health conditions				
	Diabetes		0.208	0.205	0.210
	Heart conditions		0.436	0.445	0.432
	Lung conditions		0.218	0.214	0.221
	Musculoskeletal disorders		0.100	0.108	0.0959
	Mental and behavioral disorders		0.246	0.239	0.250
10.	Fraction with wage income > 0 at age 60		0.114	0.127	0.108
11.	Fraction with wage income > 0 at ages 60-69		0.0733	0.0735	0.0731
12.	Wage income at ages 60-69		4,174	3,348	4,656
			(26,469)	(21,234)	(29,077)
13.	Wage income at 60-69 conditional on positive		56,970	45,558	63,649
			(80,960)	(64,912)	(88,343)
14.	Non-pension assets at age 60	Mean:	289,815	271,623	299,279
		SD:	(797,459)	(646,674)	(865,441)
		Median:	15,297	15,553	15,172
15.	Transfer payments at ages 60-69		152,708	148,630	155,084
			(58,450)	(56,623)	(59,359)
16.	Number of people at age 60		24,788	8,476	16,312
17.	Person-year observations (ages 60-69)		213,074	78,439	134,635

NOTE -- Table reports means with standard deviations in parentheses unless otherwise noted. All financial data in real 2013 kroner. Pre-DI data excludes observations not observed in the data at least two years before the first year on DI. There are 16,834 observations with non-missing pre-DI data.

condition, 10% has a musculoskeletal disorder, and 25% has a mental or behavioral disorder. Fourth, the sample has very little wealth, with average non-pension assets at age 60 just under 300,000 kr ($60,000) and median wealth around 15,000 kr ($3,000).

1.4.3 Results

I begin with graphical evidence for the full sample. Figure 1.2a plots the change in labor force participation between age 64 and 66 for each birth cohort from 1935 to 1945. Cohorts born between 1941 and 1945 were eligible for the extra payment until reaching age 65, while cohorts born earlier were never eligible for the extra payments.[20] The cohorts born between 1935 and 1938 (shown in gray) have a different retirement age than the rest of the cohorts and are shown for completeness. Labor force participation is defined here as having any positive labor income. To construct this figure, I first use the pooled sample of all birth cohorts shown in the figure to estimate the following model:

$$y_{igt} = \alpha_{gt} + u_{igt} \tag{1.5}$$

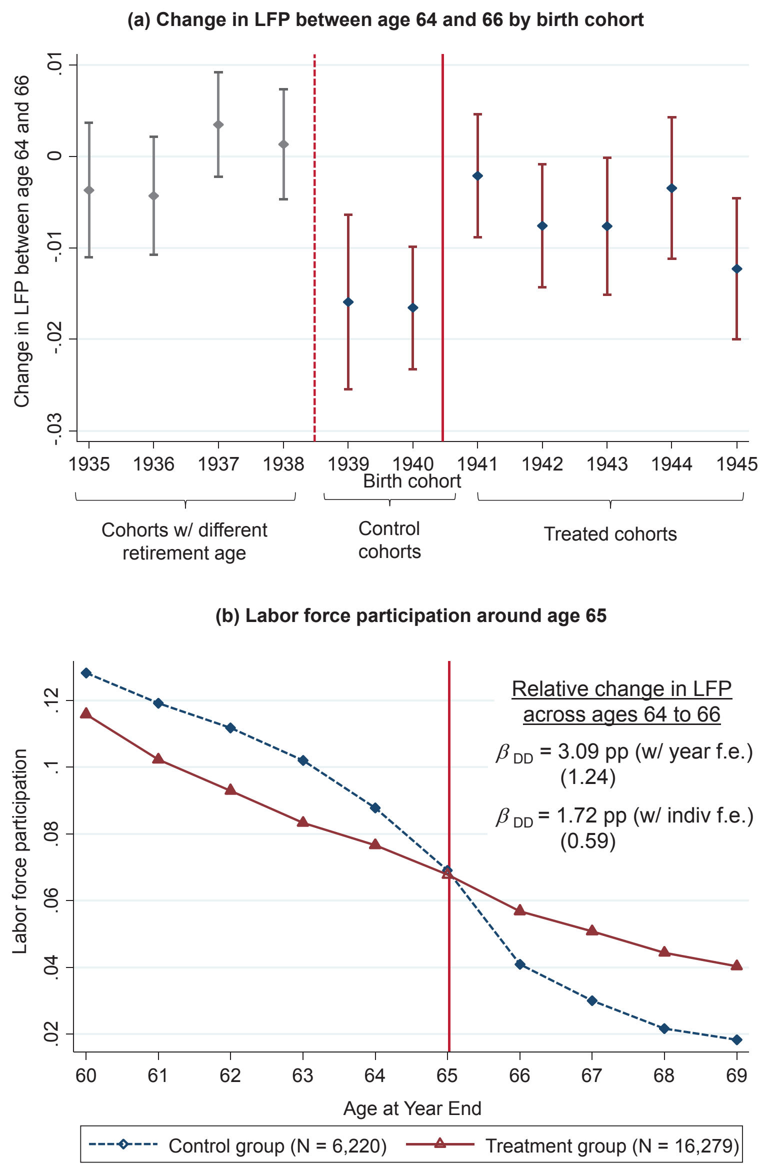
where y_{igt} is an indicator for positive labor income and α_{gt} are year and year $\times$ treatment group fixed effects. I use the residuals $\widehat{u}_{igt}$ as the dependent variable in models of the form:

$$\widehat{u}_{igt} = \alpha_i + \sum_{\substack{a \in [60,69], \\ a \neq 64}} \theta_a 1(age_{it} = a) + v_{igt} \tag{1.6}$$

estimated separately by birth cohort, where α_i is an individual fixed effect. The figure plots the $\widehat{\theta}_{66}$ coefficient for each cohort, which measures the change in labor force participation across age 64 and 66. The graphs show that the change in labor force participation is smaller for cohorts born between 1941 and 1945 than those born between 1940 and 1939. This evidence is consistent with the decline in the labor supply being attenuated by the loss of $3000 between ages 64 and 66 for the treatment group.

[20] As noted earlier, some individuals in the 1939 birth cohort also received amounts, ranging from a total of $1500 to $250. However, this occurred during the year in which they turned age 67, not at age 64 or age 66 which are shown in the figure. The figure and point estimates are similar if one excludes this group.

Figure 1.2: *The Effect of Unconditional Transfers on Labor Force Participation*



NOTE–This figure plots the change in labor force participation between ages 64 and 66 by birth cohort (panel a) and by age for the treatment group and the control group (panel b). To construct panel (a), I first use the pooled sample of all birth cohorts shown in the figure to estimate a model with year fixed effects and year $\times$ treatment group fixed effects. The figure plots the change in labor force participation for each cohort separately using the residuals from this pooled regression as the dependent variable in a model with individual fixed effects. To construct panel (b), I estimate linear probability models separately for the birth cohorts born between 1940 and July 1939 and for the birth cohorts born between 1941 and 1945, where each model includes individual fixed effects and year fixed effects. The figure plots the age coefficients, with y-axis scaled so that the average of the coefficient estimates equals the sample average of the dependent variable for each group.

Figure 1.2b presents the data by age to assess whether labor force participation was trending along the same path prior to age 65 in the treatment group and the control group. To construct this figure, I estimate models of the form:

$$y_{it} = \alpha_i + \alpha_t + \sum_{\substack{a \in [60,69], \\ a \neq 64}} \theta_a 1(age_{it} = a) + u_{it} \tag{1.7}$$

separately for the birth cohorts born between 1940 and July 1939 and for the birth cohorts born between 1941 and 1945, where α_i are individual fixed effects and α_t are year fixed effects. The figure plots the $\widehat{\theta}_a$ coefficients, with y-axis scaled so that the average of the nine $\widehat{\theta}_a$ coefficient estimates and $\theta_{64} = 0$ equals the sample average of the dependent variable for each group. The figure shows that labor supply declines with age for both groups. The two series appear to be parallel until a break at age 65 (perhaps starting at 64) for the control group. There is no break for the treatment group. This figure indicates that there was a relative increase in labor supply in the treatment group when its income decreased at age 65. Under the identifying assumption that the change in labor force participation at 65 would have been the same in both groups in the absence of the 2006 policy change, this relative increase is the effect of losing \$3000 on labor force participation.

To assess the robustness of the results illustrated in Figure 1.2, I present estimates from Equation 1.4 in Table 1.2, defining labor force participation as any positive labor income (columns 1-4) and as labor income above 2,000 real 2013 kr (columns 5-8). The table also reports the mean of the dependent variable to help judge the magnitudes of the regression coefficients. The excluded age is 64 so each coefficient measures the change in labor force participation relative to age 64. The coefficient of interest is $\widehat{\gamma}_{66}$ on the treatment $\times$ age 66 indicator variable. The first column includes year and year $\times$ treatment group fixed effects, but no other controls. The second column adds the following individual level covariates: indicators for marital status, Danish nationality, gender, college education, living in Copenhagen, and white-collar occupation at age 60. The third column adds controls for the chronic diseases that I observe in these data. The fourth column adds individual fixed effects, which additionally control for any other time-invariant omitted variables. The

Table 1.2: *Effect of Unconditional Transfers on Labor Force Participation*

Dependent variable:	1 if labor income > 0				1 if labor income > 2000 kr			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment group x Age 60	**-0.00929**	**-0.0259**	**-0.0265**	**0.0186**	**-0.0139**	**-0.0286**	**-0.0291**	**0.00749**
	(0.0282)	(0.0280)	(0.0280)	(0.0158)	(0.0255)	(0.0253)	(0.0253)	(0.0138)
Treatment group x Age 61	**-0.0121**	**-0.0245**	**-0.0249**	**0.00909**	**-0.0116**	**-0.0226**	**-0.0230**	**0.00473**
	(0.0212)	(0.0210)	(0.0210)	(0.0112)	(0.0192)	(0.0191)	(0.0191)	(0.0100)
Treatment group x Age 62	**-0.0118**	**-0.0201**	**-0.0204**	**0.00226**	**-0.0121**	**-0.0195**	**-0.0197**	**-0.00117**
	(0.0141)	(0.0140)	(0.0140)	(0.00745)	(0.0128)	(0.0128)	(0.0128)	(0.00673)
Treatment group x Age 63	**-0.00960**	**-0.0138***	**-0.0139***	**-0.00263**	**-0.00674**	**-0.0105**	**-0.0106**	**-0.00130**
	(0.00723)	(0.00717)	(0.00717)	(0.00419)	(0.00665)	(0.00662)	(0.00662)	(0.00379)
Treatment group x Age 65	**0.0118***	**0.0160****	**0.0161****	**0.00494**	**0.00851**	**0.0122***	**0.0123***	**0.00312**
	(0.00688)	(0.00685)	(0.00685)	(0.00391)	(0.00633)	(0.00632)	(0.00631)	(0.00352)
Treatment group x Age 66	**0.0309****	**0.0393*****	**0.0395*****	**0.0172*****	**0.0212***	**0.0286****	**0.0289****	**0.0105***
	(0.0124)	(0.0123)	(0.0123)	(0.00586)	(0.0115)	(0.0115)	(0.0115)	(0.00538)
Treatment group x Age 67	**0.0378****	**0.0504*****	**0.0508*****	**0.0171****	**0.0303***	**0.0414*****	**0.0418*****	**0.0141****
	(0.0172)	(0.0172)	(0.0171)	(0.00736)	(0.0160)	(0.0160)	(0.0160)	(0.00658)
Treatment group x Age 68	**0.0416***	**0.0584*****	**0.0589*****	**0.0141**	**0.0288**	**0.0436****	**0.0441****	**0.00726**
	(0.0216)	(0.0216)	(0.0215)	(0.00870)	(0.0202)	(0.0202)	(0.0202)	(0.00779)
Treatment group x Age 69	**0.0426***	**0.0635****	**0.0642****	**0.00845**	**0.0296**	**0.0481****	**0.0487****	**0.00296**
	(0.0257)	(0.0257)	(0.0256)	(0.0106)	(0.0242)	(0.0242)	(0.0242)	(0.00946)
Year x treatment group f.e.'s	x	x	x	x	x	x	x	x
Controls for demographics		x	x			x	x	
Controls for chronic diseases			x				x	
Individual fixed effects				x				x
Mean of dep. var in treat group	0.0731	0.0731	0.0731	0.0731	0.0630	0.0630	0.0630	0.0630
Mean of dep. var in control group	0.0729	0.0729	0.0729	0.0729	0.0591	0.0591	0.0591	0.0591
Mean of dep. Var overall	0.0731	0.0731	0.0731	0.0731	0.0618	0.0618	0.0618	0.0618
Number of people	22,499	22,499	22,499	22,499	22,499	22,499	22,499	22,499
Person-year observations	192,156	192,156	192,156	192,156	192,156	192,156	192,156	192,156

NOTE -- Each column reports results from OLS regressions where the dependent variable is an indicator variable for having positive labor earnings (columns 1-4) or having labor earnings above 2000 kr (columns 5-8). Standard errors are clustered by individual and are reported in parentheses below each coefficient estimate. Age 64 is the excluded age. The sample includes disability insurance beneficiaries observed on the low severity level at age 60 who were born in July 1939 through the end of 1945. Individuals are included from ages 60 to 69. Demographic controls consist of indicator variables for marital status, Danish nationality, gender, college education, living in Copenhagen, and white-collar occupation at age 60. Chronic disease controls consist of indicators for diabetes, heart conditions, lung conditions, musculoskeletal disorders, and mental or behavioral disorders (which include schizophrenia, mood disorders, and dementia). *** p<0.01, ** p<0.05, * p<0.1

estimates in columns 1-3 imply that labor force participation increased in the treatment group relative to the control group between ages 64 and 66 by 3 to 4 percentage points, which is 40% to 60% of the mean in the treatment group across the entire 60-69 age range. Adding fixed effects in column 4 reduces the point estimate to 1.7 percentage points (or 23% relative to the treatment group mean). All these estimates are statistically significant at the 5% or 1% level. Using the 2000 kr cutoff as the dependent variable in columns 5-8 yields slightly smaller point estimates.

Table 1.3 focuses on the subset of disability insurance recipients with recent work experience, which I define as having positive labor income at age 60. There are 2,530 observations that meet this condition. The columns of this table are organized in exactly the same way as in Table 1.2. The regressions in this table only include individuals from ages 61 to 69, since earnings at age 60 are used to define the sample. Average labor force participation for this group is 46% overall between ages 61 and 69, 49% in the treatment group, and 39% in the control group. The point estimates in this table are an order of magnitude larger than those in Table 1.2, but are similar to those for the full sample when expressed as a percentage of the mean (22% for the model with fixed effects to 54% for the other columns). This relative change is shown in Figure 1.3.

Taken together, these estimates reject the hypothesis that $\frac{\partial e}{\partial A} = 0$. Economically, the magnitude is large, both as a percentage of mean labor force participation and relative to the \$3000 used as identifying variation. My estimates of $\frac{\partial e}{\partial A}$ from Table 1.2 are a 1 percentage point to 0.57 percentage point decline in labor force participation per \$1000. As a point of comparison, note that Maestas, Mullen, and Strand (2013) find that SSDI reduces labor force participation by 28 percentage points. The average yearly SSDI benefit amount is \$13,752 in 2014. Scaling Maestas, Mullen, and Strand's (2013) point estimate by the average annual value of benefits yields a ratio of 2 percentage points per \$1000, and this would include both income and work disincentive effects of SSDI benefits. My estimates are of course from a very different setting and my research design uses a very different source of identifying variation. Nevertheless, the evidence shows that this relatively small change in income had a large effect on labor force participation of the elderly disabled.

1.4.4 Estimates by demographics, disease, and asset holdings

This section presents estimates for different subgroups, defined based on demographics, impairments, and assets. The results are summarized in Figure 1.3, which presents the point estimates as a percentage of the mean labor force participation rate in each subgroup, along with 95% confidence interval bands.

Table 1.4 and Figure 1.3 explore heterogeneity by gender and marital status (panel a),

Table 1.3: *Effect of Unconditional Transfers on Labor Force Participation for Recently Working Sample*

Dependent variable:	1 if labor income > 0				1 if labor income > 2000 kr			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment group x Age 61	**-0.286*** **	**-0.287*** **	**-0.280*** **	**-0.0553**	**-0.282*** **	**-0.285*** **	**-0.278*** **	**-0.105**
	(0.101)	(0.0996)	(0.0994)	(0.0848)	(0.102)	(0.101)	(0.101)	(0.0810)
Treatment group x Age 62	**-0.179** **	**-0.180** **	**-0.175** **	**-0.0252**	**-0.180*** **	**-0.183*** **	**-0.178** **	**-0.0613**
	(0.0706)	(0.0699)	(0.0698)	(0.0572)	(0.0698)	(0.0696)	(0.0693)	(0.0544)
Treatment group x Age 63	**-0.103*** **	**-0.104*** **	**-0.101*** **	**-0.0264**	**-0.0850** **	**-0.0863** **	**-0.0838** **	**-0.0256**
	(0.0381)	(0.0378)	(0.0377)	(0.0301)	(0.0371)	(0.0369)	(0.0368)	(0.0283)
Treatment group x Age 65	**0.115*** **	**0.115*** **	**0.113*** **	**0.0378**	**0.0920*** **	**0.0932*** **	**0.0907** **	**0.0325**
	(0.0371)	(0.0369)	(0.0368)	(0.0240)	(0.0355)	(0.0354)	(0.0353)	(0.0220)
Treatment group x Age 66	**0.264*** **	**0.265*** **	**0.260*** **	**0.109*** **	**0.189*** **	**0.191*** **	**0.186*** **	**0.0686** **
	(0.0662)	(0.0656)	(0.0654)	(0.0322)	(0.0643)	(0.0641)	(0.0638)	(0.0301)
Treatment group x Age 67	**0.331*** **	**0.332*** **	**0.325*** **	**0.0971*** **	**0.263*** **	**0.267*** **	**0.259*** **	**0.0825*** **
	(0.0921)	(0.0912)	(0.0906)	(0.0314)	(0.0894)	(0.0891)	(0.0884)	(0.0291)
Treatment group x Age 68	**0.378*** **	**0.379*** **	**0.370*** **	**0.0659*** **	**0.290*** **	**0.295*** **	**0.285** **	**0.0493** **
	(0.115)	(0.114)	(0.113)	(0.0209)	(0.112)	(0.112)	(0.111)	(0.0192)
Treatment group x Age 69	**0.388*** **	**0.389*** **	**0.378*** **		**0.299** **	**0.306** **	**0.293** **	
	(0.138)	(0.137)	(0.136)		(0.135)	(0.135)	(0.134)	
Year x treatment group f.e.'s	x	x	x	x	x	x	x	x
Controls for demographics		x	x			x	x	
Controls for chronic diseases			x				x	
Individual fixed effects				x				x
Mean of dep. var in treat group	0.488	0.488	0.488	0.488	0.433	0.433	0.433	0.433
Mean of dep. var in control group	0.392	0.392	0.392	0.392	0.328	0.328	0.328	0.328
Mean of dep. var overall	0.456	0.456	0.456	0.456	0.398	0.398	0.398	0.398
Number of people	2,530	2,530	2,530	2,530	2,530	2,530	2,530	2,530
Person-year observations	19,909	19,909	19,909	19,909	19,909	19,909	19,909	19,909

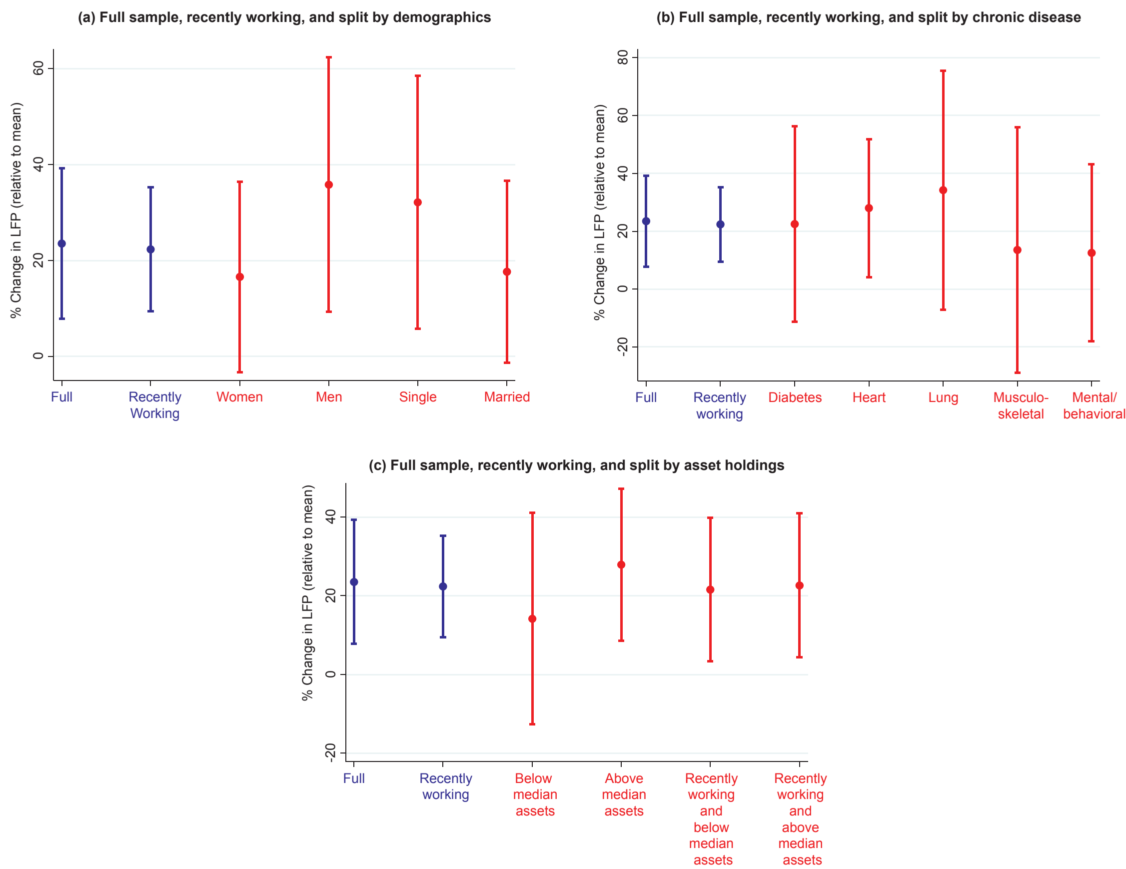
NOTE -- Each column reports results from OLS regressions where the dependent variable is an indicator variable for having positive labor earnings (columns 1-4) or having labor earnings above 2000 kr (columns 5-8). Standard errors are clustered by individual and are reported in parentheses below each coefficient estimate. Age 64 is the excluded age. The sample includes disability insurance beneficiaries observed on the low severity level at age 60 who were born in July 1939 through the end of 1945. The sample is restricted to the subset of observations that had postive labor income at age 60. Individuals are included from ages 61 to 69. Demographic controls consist of indicator variables for marital status, Danish nationality, gender, college education, living in Copenhagen, and white-collar occupation at age 60. Chronic disease controls consist of indicators for diabetes, heart conditions, lung conditions, musculoskeletal disorders, and mental or behavioral disorders (which include schizophrenia, mood disorders, and dementia). *** p<0.01, ** p<0.05, * p<0.1

chronic disease (panel b), and assets (panel c). The table reports the $\widehat{\gamma}_{66}$ estimates of the change in labor force participation between age 64 and 66 for the treatment group relative to the control group. The table also reports average labor force participation for each subgroup. In panel (a), note that labor force participation is twice as high at 10% for men as for women, while the estimated treatment effect is almost four times as large for men as it is for women. Therefore, the larger treatment effect estimates can be partially explained by the higher labor force participation for men. The next two columns present estimates for groups divided by marital status. The point estimate is larger for single disability insurance beneficiaries, while the mean labor force participation is higher for those who are married (6.5% compared to 8%).

Panel (b) reports estimates for each of the six chronic diseases. Mean labor force participation is about 7% for each disease category, except for the mental and behavioral disorder subgroup, which is slightly lower at 6%. The treatment effect estimates suggest that the subgroups with musculoskeletal and mental and behavioral disorders are less sensitive than other groups to loss of income, although the confidence intervals are wide for these groups.

Panel (c) reports estimates by assets measured at age 60. I divide each year of birth cohort into two groups (above and below median assets) to maintain balance of the sample. The group with below median wealth has 6000 kr on average in assets, while the group with above median wealth has 574,000 kr in assets. One may expect that there should be larger responses by those with less liquid wealth, but the treatment effect estimates in columns 1 and 2 appear to run counter to this intuition. However, note that mean labor force participation is much higher for the group with above median assets (9% compared to 5.4%). To make the comparisons as clear as possible, columns 3 and 4 restrict the sample to those with recent work experience, as in Table 1.3. I re-define median wealth for these subgroups. Mean labor force participation is still higher for the group with more wealth, but the difference is small (43% compared to 48%). In this more comparable subgroup, the treatment effect is similar across wealth categories. The apparent unimportance of assets

Figure 1.3: *Heterogeneity in the Effect of Unconditional Cash Transfers on Labor Force Participation*



NOTE–This figure plots the change in labor force participation between ages 64 and 66 in the treatment group relative to the control group for the full sample, the recently working subsample, and by demographics (panel a), chronic disease (panel b), and asset holdings (panel c). The treatment effect is expressed as a percentage of the mean labor force participation in the treatment group.

Table 1.4: *Heterogeneity in Labor Force Participation Responses to Unconditional Transfers*

Panel A: Gender and marital status

Sample:	Women	Men	Single	Married
	(1)	(2)	(3)	(4)
Treatment group x Age 66	**0.00994**	**0.0376*** **	**0.0207** **	**0.0145***
	(0.00608)	(0.0142)	(0.00866)	(0.00798)
Mean of dep. var in treat group	0.0601	0.105	0.0644	0.0823
Mean of dep. var in control group	0.0606	0.107	0.0685	0.0769
Mean of dep. var overall	0.0602	0.106	0.0656	0.0806
Number of people	15,915	6,584	11,714	10,785
Person-year observations	137,702	54,454	96,467	95,689

Panel B: Chronic diseases

Condition:	Diabetes	Heart Conditions	Lung Conditions	Musculo-skeletal	Mental/ behavioral
	(1)	(2)	(3)	(5)	(6)
Treatment group x Age 66	**0.0158**	**0.0202** **	**0.0229**	**0.00891**	**0.00768**
	(0.0121)	(0.00879)	(0.0141)	(0.0142)	(0.00956)
Mean of dep. var in treat group	0.0701	0.0722	0.0669	0.0657	0.0613
Mean of dep. var in control group	0.0638	0.0739	0.0761	0.0863	0.0575
Mean of dep. var overall	0.0682	0.0727	0.0696	0.0723	0.0602
Number of people	4,708	9,801	4,932	2,215	5,550
Person-year observations	43,024	89,887	45,043	20,450	50,462

Panel C: Assets at age 60

			Working at 60	
Assets above or below median:	Below	Above	Below	Above
	(1)	(2)	(3)	(4)
Treatment group x Age 66	**0.00757**	**0.0256*** **	**0.101** **	**0.115** **
	(0.00734)	(0.00906)	(0.0437)	(0.0474)
Mean assets at age 60 (2013 kr)	6,021	573,656	122,734	947,008
Median assets at age 60 (2013 kr)	6,150	187,700	9,749	623,520
Mean of dep. var in treat group	0.0535	0.0919	0.468	0.508
Mean of dep. var in control group	0.0541	0.0912	0.378	0.406
Mean of dep. var overall	0.0537	0.0917	0.437	0.475
Number of people	11,275	11,200	1,273	1,257
Person-year observations	94,128	97,904	9,906	10,003

NOTE -- Each cell reports the treatment group x age 66 coefficient from separate OLS regressions where the dependent variable is an indicator variable for having positive labor earnings. Each regression includes year, year x treatment group, and individual fixed effects (as in Column 4 of Table 2). Standard errors are clustered by individual and are reported in parentheses below each coefficient estimate. Age 64 is the excluded age. The sample includes disability insurance beneficiaries observed on the low severity level at age 60 who were born in July 1939 through the end of 1945. In panel C, each birth cohort is divided separately into two groups (above or below median assets for that birth cohort at age 60). Columns 3 and 4 restrict the sample to those working at age 60; median assets in these columns are re-defined for this recently working subgroup, instead of the pooled sample. *** p<0.01, ** p<0.05, * p<0.1

contrasts with the evidence for unemployment insurance recipients in the U.S. presented by Chetty (2008). One aspect of the research design that could explain this difference is that many pension assets may become accessible at retirement age. Therefore, liquid assets may be less important here simply because of the nature of the identification strategy. Unfortunately, I do not observe pension assets in these data.

The estimates described above show that the difference in labor supply responses across groups defined by demographics, chronic diseases, and asset holdings is small. In contrast, the results in the next section show that there is much heterogeneity in their high frequency responses to receipt of payments.

1.5 Timing of Cash Transfers and ER visits

I now turn to describing the results on the *timing* of payment disbursements and emergency room (ER) visits. The emphasis in this part of the paper is on heterogeneity: even though over 60% of the population receives income on the same day each month, it is, disproportionately, disability insurance beneficiaries and those who will be disability insurance beneficiaries in the future who present at the ER. While the previous section showed large labor supply responses to unconditional transfers, this section shows that other behaviors also display excess sensitivity to income at a much higher frequency. Further, excess sensitivity in this domain creates fiscal externalities because ER visits are costly to the government. It is difficult to reconcile the evidence with a rational model, suggesting that there could additionally be internalities because of self-control problems. In a model with externalities or internalities, labor supply responses are no longer sufficient statistics for optimal disability insurance benefit levels. Instead, there is an additional externality/internality term that would enter the formula in Equation 1.3. The results described below suggest that this extra term is non-zero. In section 1.5.1, I first discuss my empirical strategy for measuring excess sensitivity in ER visits. Section 1.5.2 presents the main results, section 1.5.3 shows robustness checks, and section 1.5.4 provides a discussion.

1.5.1 Empirical framework

The statistic that I use to characterize the increase in ER visits is the mean number of people (per 100,000) who visit the ER on the day after the last business day of the month, relative to the day before the last business day of the month. I adjust these averages for fluctuations in the number of ER visits that occur across day of week, months, years, and on nineteen holidays and other reoccurring special dates listed in Appendix Table A3. In this way, the results reported here can be interpreted as how many more people can be expected to visit the ER beyond normal weekly, seasonal, and yearly patterns.[21]

In particular, let $y_{id} = 1$ if individual i visits the ER on date d and 0 otherwise. Let t index days relative to the last business day of the month, so that $t = 0$ on that date, $t < 0$ on dates before the last business day of the month, and $t > 0$ on dates after the last business day of the month. I restrict my sample to the twenty-eight days spanning $t = -13$ to $t = 14$ so that I have a balanced sample of 215 twenty-eight day months across 1994 to 2011.

I collapse the data to daily averages $\overline{y}_d$ and regress:

$$\overline{y}_d \times 100000 = \alpha + \sum_{s=-13}^{-1} \beta_s 1(t_d = s) + \sum_{s=1}^{14} \beta_s 1(t_d = s) + \gamma X_d + \overline{u}_d \tag{1.8}$$

where I weight each observation by the number of people in my sample on date d.[22] In Equation 1.8, $1()$ is an indicator function that equals 1 if the statement in parentheses is true for date d and 0 otherwise. X includes a vector of indicator variables to allow the mean of the dependent variable to differ by day of the week, month, year, and on holidays and other reoccurring special dates.[23] Throughout, I report standard errors that allow serial correlation across days within a twenty-eight day month.

The twenty-seven $\widehat{\beta}_s$ coefficient estimates measure the difference in the average number

[21] I measure people instead of number of visits for two reasons. First, multiple ER visits by the same person that occur on the same day are likely to be related to the same adverse health event. Second, multiple ER visits listed for the same person on the same day may be due to data entry errors.

[22] Because I weight each observation by the number of people in my sample on date d, this regression is equivalent to a regression using y_{id} instead of $\overline{y}_d$.

[23] I control for twelve "synthetic" months in the terminology of Evans and Moore (2012), instead of calendar months. Each synthetic month is a twenty-eight day period from $t = -13$ to $t = 14$.

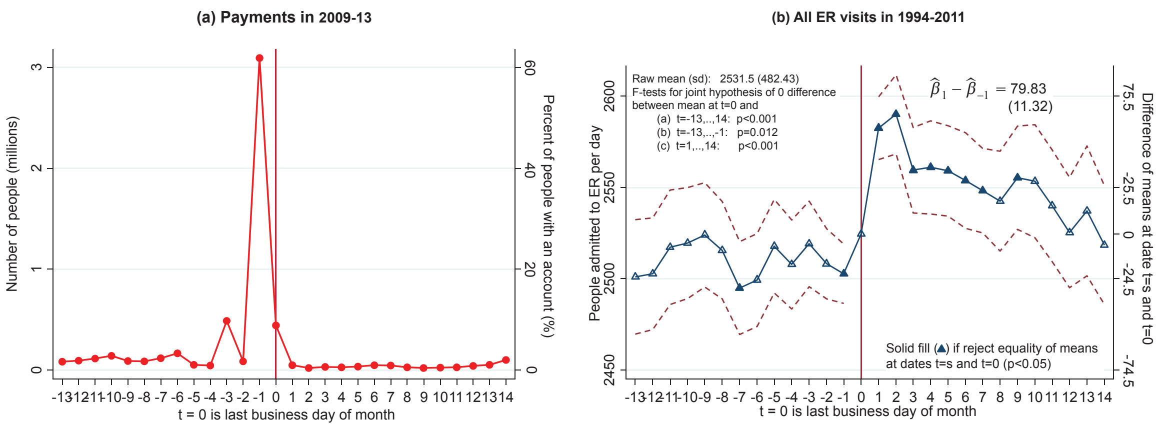
of people (per 100,000) who visit the ER on dates where $t = s$, relative to days where $t = 0$. The coefficients can be interpreted as changes in the probability that an individual will visit the ER at $t = s$ relative to $t = 0$. The standard errors on these coefficients test whether this difference is statistically significant. The difference between $\widehat{\beta}_1$ and $\widehat{\beta}_{-1}$ measures the mean number of people (per 100,000) who visit the ER on the day after the last business day of the month, relative to the day before the last business day of the month. In the figures that follow, I plot the twenty-seven $\widehat{\beta}_s$ coefficients and $\beta_0 = 0$, but I scale the left y-axis so that the average of the twenty-eight points equals the average of the dependent variable.

In robustness checks, I also include indicators for the days before and after each of the holidays and other reoccurring special dates. I consider the impact of excluding the twenty-eight day months containing New Year's, which is a very busy day in the emergency room as shown in column 3 of Appendix Table A3. Because about 40 percent of the paydays occur on a Friday (as seen in Appendix Figure A3), I also consider a robustness check where I exclude these twenty-eight day months from my sample. In addition, all the results shown here have been estimated by including acute in-patient admissions with ER visits. In all these cases, my basic results remain unchanged relative to the more restrictive model and sample criteria. As another robustness check, I find that defining $t = 0$ as the last calendar day of the month, instead of the last business day of the month, changes the results in a predictable way. Because the last business day of the month always precedes the last calendar day of the month in this analysis, an increase in rates of ER visits is visible before $t = 0$ and is smaller than estimates using the last business day of the month.

1.5.2 Results

Figure 1.4 plots the average number of people receiving income (panel a) and visiting the emergency room (panel b) over the two weeks before and two weeks after the last business day of the month. Panel (b) of the figure is constructed by replacing the dependent variable in Equation (1.8) with the sum of y_{id} on date d. Panel (a) shows that 3 million people receive income at the same time, while the point estimate $\widehat{\beta}_1 - \widehat{\beta}_{-1} = 79.83$ in panel (b) shows that

Figure 1.4: *Event Studies of Payments and ER visits around Last Business Day of Month*



NOTE–This figure plots event studies of the number of people receiving payments (panel a) and the number of people visiting the ER (panel b) around the last business day of the month ($t = 0$). Panel A shows the average number of people each day who receive a payment to their NemKonto bank account in 2009-2013 for the 28-day window around $t = 0$. The figure includes payments from any of the 850 government authorities in Denmark, including transfer and social insurance program payments, tax refunds, and wages for government employees. The right y-axis expresses the number of people receiving a payment as a percentage of the total number of people with a NemKonto bank account, which was 4,927,626 on average from 2009 to 2013. Panel B plots the average number of people who visit the ER per day in 1994-2011 for the 28-day window around $t = 0$. The means are adjusted for day of the week, synthetic month, year, and nineteen holidays and other reoccurring special dates by regressing the number of people admitted to the ER on date d on indicators for each date $t = -13, ..., 14$ and indicators for each control variable:

$$\sum_i y_{id} = \alpha + \sum_{s=-13}^{-1} \beta_s I(t_d = s) + \sum_{s=1}^{14} \beta_s I(t_d = s) + \gamma X_d + \widetilde{u}_d$$

where y_{id} is an indicator for whether person i visited the ER on date d. The points in the figure are $\beta_0 = 0$ and the twenty-seven $\widehat{\beta}_s$ coefficients. The dashed lines show a 0.95 confidence interval for $\widehat{\beta}_s$, which equals the difference between means at date $t = 0$ and date $t = s$. Standard errors are clustered by 28-day month. The y-axis along the left-hand side of the figure is scaled so that the mean of the twenty-eight points equals the sample average of the dependent variable. The nineteen reoccurring dates are listed in Appendix Table A3.

about 80 more people visit the ER.[24] About 2,500 people visit the emergency room each day, on average.

Figure 1.5 disaggregates the series in Figure 1.4b using the diagnosis codes associated with each visit. I focus on drug and alcohol related visits and visits for head injuries.[25] While not mutually exclusive by construction, very few head injuries (less than 1%) are coded also as drug and alcohol related.[26] The results for drugs and alcohol are shown in panel (a) and the results for head injuries are shown in panel (c). There are about 9.5 more people who visit the ER for drugs and alcohol on the day after the last business day of the month, relative to the day before the last business day of the month, which is a 30% increase relative to the average daily rate. The number of ER visits for drugs and alcohol gradually declines over the 14 days after the last business day of the month. The pattern for head injuries in panel (c) is somewhat different. Here, there is a spike in people visiting the emergency room that only remains elevated for the three or four days after $t = 0$. The combined increase for drugs and alcohol and head injuries is about 38% relative to the overall increase shown in Figure 4 (and ignoring the small overlap between these categories).

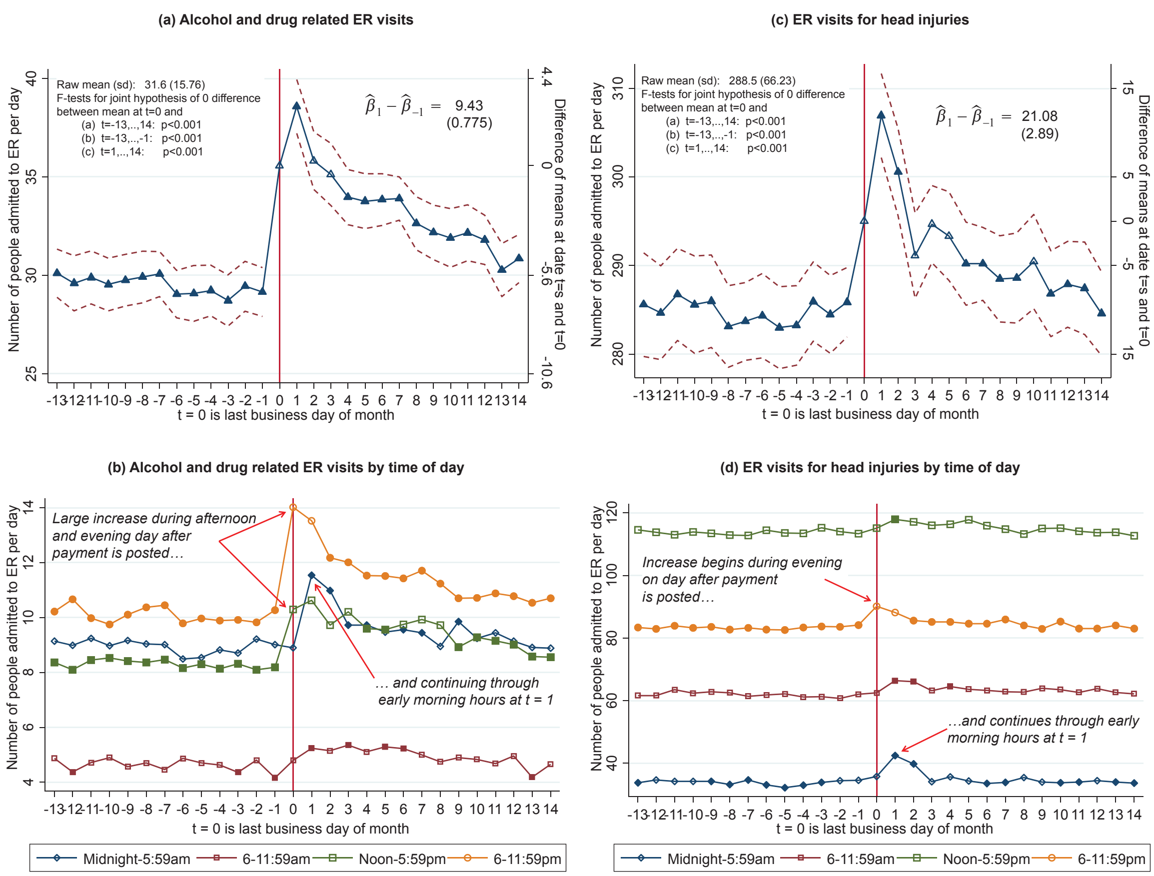
Panels (b) and (d) show that the timing of the increases in drug and alcohol related visits and head injuries are consistent with a behavioral response to receipt of income during the day on the last business day of the month. The figures divide the series in panels (a) and (c) by six hour intervals. The payments are posted at $t = -1$, while the increase in emergency room visits occurs at $t = 0$ in the afternoon and evening, but at $t = 1$ during the after-midnight hours. The intuition here is that simply receiving the income should not, by itself, cause people to go to the emergency room. Consistent with this reasoning, the

[24] Appendix Figure A2 shows the total and average payment amounts.

[25] I define a visit to be drug and alcohol related if any of the diagnosis codes associated with that visit matches an ICD-10 diagnosis code that is drug or alcohol related. My codes are similar to the ICD-9 codes used by Gross and Tobacman (2014) and are listed in the notes to Figure 1.5. I define a visit to be for a head injury if the primary diagnosis code associated with that visit matches the ICD-10 diagnosis codes for head injuries.

[26] I focus on alcohol and drug related visits and head injuries because the literature on hospitalizations and mortality from the United States has focused on external causes of death and hospitalizations for and deaths from drugs and alcohol. An interesting question is what categories of emergency room visits do not increase. Many do not. An example is heart attacks where I find no increase (not reported), which contrasts with the findings of Evans and Moore (2011) and Andersson, Lundborg, Vikström (2014) in mortality data.

Figure 1.5: *Event Studies of ER visits around Last Business Day of Month: Full Population*



NOTE–This figure plots the average number of people who visit the ER per day over the 28-day window around the last business day of the month ($t = 0$) in 1994-2011 and whose diagnosis code is either alcohol and drug related (panels a and b) or for a head injury (panels c and d) . The means are adjusted for day of the week, synthetic month, year, and nineteen reoccurring dates in the same way as in Figure 1.4. A visit is defined as alcohol and drug related if any of the ICD-10 diagnosis codes associated with that visit are for drugs and alcohol (ICD-10 codes T40, T436, T510, and those starting with F10 to F19, but excluding F17). A visit is defined as being for a head injury if the primary diagnosis code is for a head injury (ICD-10 codes starting with S00 to S09). Panels (b) and (d) show the number of people visiting the ER per day divided into four 6-hour intervals: midnight to 5:59 am (diamonds), 6 am to 11:59 am (small squares), noon to 5:59 pm (larger squares), and 6 pm to 11:59 pm (circles). There are $T = 215 \times 28 = 6020$ days in my sample.

increases in emergency room visits occurs with a lag, which would be expected if emergency room visits are due to changes in consumption and behavior.

These aggregate results are for the whole population. I turn now to findings that disaggregate the data. I organize the rest of the results as four key lessons that can be learned from combining the ER visit data with administrative data containing information on sources of income, assets, and information from disability insurance case files.

- Result 1: *It is the disabled who drive the increase in emergency room visits—not wage earners and not social security recipients, even though all three groups receive income on the same day.*

Figure 1.6 present estimates from Equation 1.8 for the disability insurance beneficiary (shown in diamonds), social security beneficiary (shown in squares), and wage earner samples (shown in triangles).

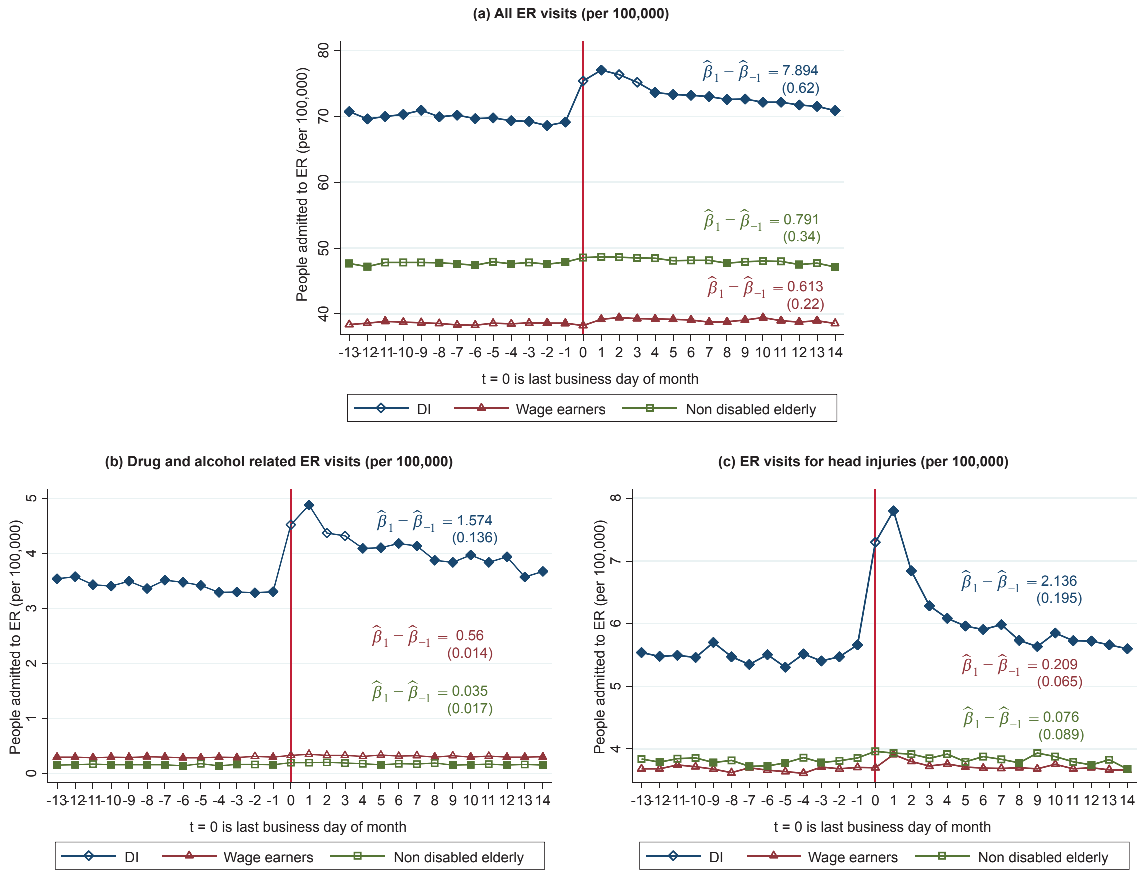
Note first the difference in average daily rates across these groups. The average daily rate of emergency room visits is highest among disability insurance recipients, with about 72 people visiting the ER a day per 100,000 people. For social security beneficiaries and workers, average daily rates are around 50 and 40 people per 100,000 people.[27] Rates for drug and alcohol related visits are an order of magnitude larger for the DI sample compared with the other groups. Daily rates for head injuries in the DI sample are about double the rates for all other groups.

The change in the number of people who visit the ER around $t = 0$ is striking for the disability insurance sample, while no comparable increases are visible for the other groups. Relative to the base rate for the DI sample, the change in probability of visiting the ER is 11% for all ER visits, 41% for drug and alcohol related ER visits, and 37% for ER visits for head injuries.

Expressing the point estimates as a percentage of the aggregate estimates, the increase among the disabled is 25% for ER visits overall, 42% for drug and alcohol related ER visits,

[27] Appendix Figure A4 divides the series for wage earners by lagged earnings quartile, using earnings averaged across the previous two years. Average daily rates for wage earners are progressively lower for each higher quartile of the (lagged) earnings distribution, reflecting a health-wealth gradient.

Figure 1.6: *Event Studies of ER visits around Last Business Day of Month by Group*



NOTE–This figure plots the average number of people per 100,000 who visit the ER per day over the 28-day window around the last business day of the month ($t = 0$) in 1994-2011. The graph shows three samples: those who receive disability insurance during the year in which $t = 0$ falls (blue diamonds), wage earners (red triangles), and non-disabled elderly who receive social security income during the year in which $t = 0$ falls. The figures are drawn for all ER visits (panel a), for ER visits that have at least one diagnosis code that is alcohol and drug related (panel b), and for ER visits whose primary diagnosis code is for a head injury (panel c). The means are adjusted for day of the week, synthetic month, year, and holidays and other reoccurring special dates, by regressing the number of people admitted to the ER per 100,000 on date d on indicators for each date $t = -13, ..., 14$ and indicators for each control variable:

$$\bar{y}_d \times 100000 = \alpha + \sum_{s=-13}^{-1} \beta_s I(t_d = s) + \sum_{s=1}^{14} \beta_s I(t_d = s) + \gamma X_d + \bar{u}_d$$

where each observation is weighted by the number of people in the sample on date d. The twenty-seven β_s coefficients can be interpreted as the difference in the probability of visiting the ER on date $t = s$ relative to date $t = 0$. There are $T = 215 \times 28 = 6020$ days in my sample.

and 25% for ER visits for head injuries. These estimates are precise enough that using the lower bound on the 95% confidence interval only lowers these percentages to 20.9%, 34.9%, and 20.7%, respectively.

These three groups are very different sizes. The disability insurance sample, at around 250,000 people on average over time, is about 40% of the size of the social security sample and 13.1% of the size of the wage earner sample. However, accounting for the difference in sizes of the groups does not change the conclusion that the aggregate increases are driven by the disabled, which is surprising given that the DI sample is only about 4.5% of the full population of Denmark.

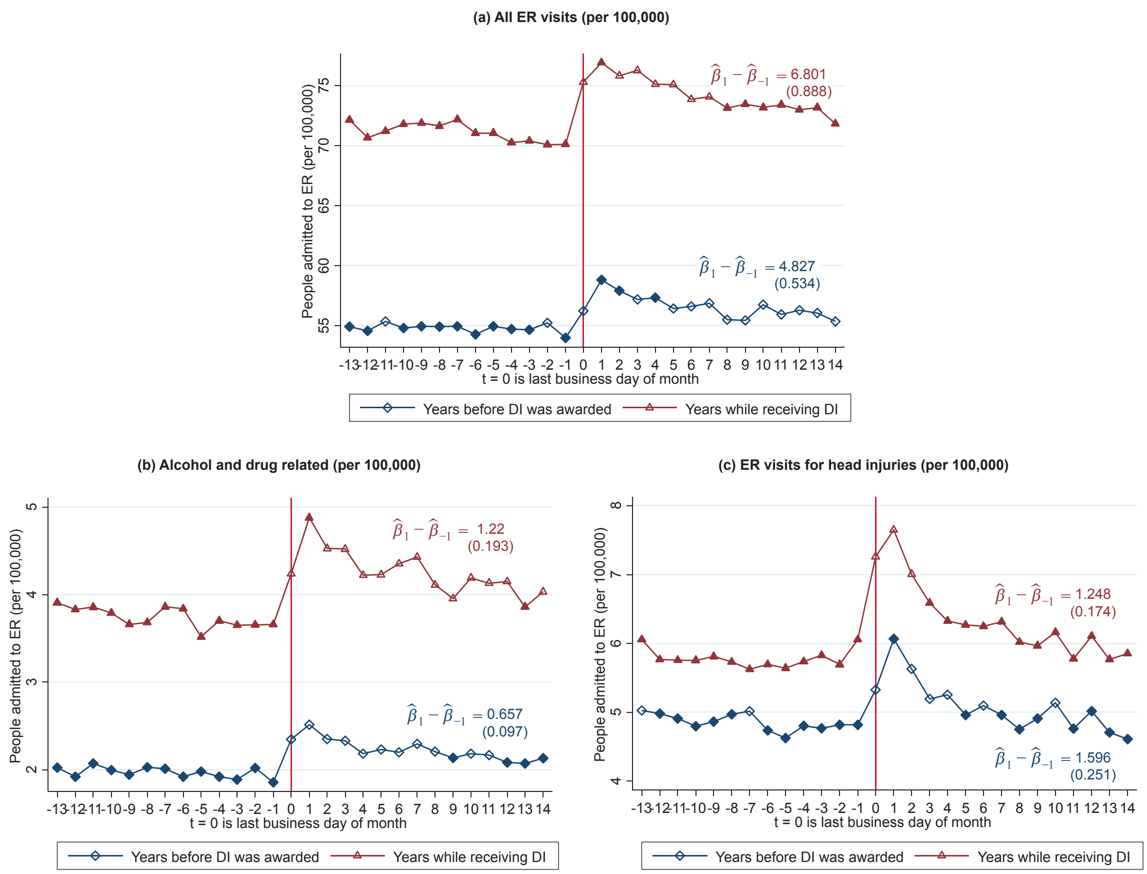
- *Result 2:* *The disabled exhibit increases in emergency room visits on the last business day of the month even in years before receiving benefits.*

This result is shown in Figures 1.7 and 1.8. In Figure 1.7, I present two sets of estimates. The top line (in triangles) is for disability insurance recipients during years in which they receive disability insurance benefits. The bottom line (in diamonds) plots rates for these same people in prior years. I restrict the sample to people for whom I observe at least one year before receiving disability insurance income and one year while receiving the benefits. I use years 1994-2010 for the pre-DI sample and years 1995-2011 for the post-DI sample.

In Figure 1.8, I study the time path of the increase in ER visits around $t = 0$ leading up to the first year that individuals are observed on DI for this same sample. The figure plots the increase around $t = 0$ relative to the base rate in three year bins by time relative to the first year on DI, along with 95% confidence interval bands. Because the last business day of the month is such a common payday, it is likely that these individuals receive income on that day even in years before receiving disability insurance benefits.

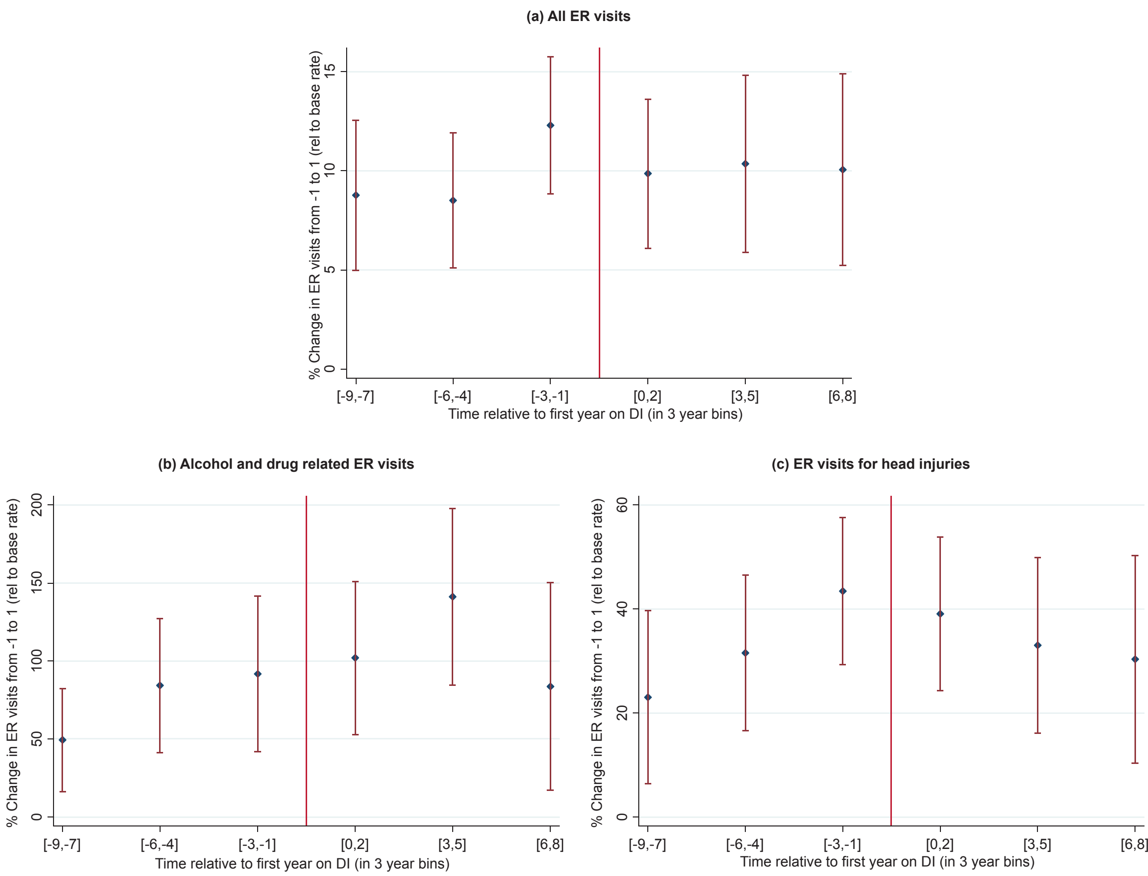
The average daily rate of emergency room visits is higher during years when disability insurance benefits are received, with about 73 people visiting the ER per 100,000 compared with 56.2 people visiting the ER per 100,000 in the earlier years. Similarly, the daily rates for drug and alcohol related ER visits are 4 people per 100,000 in years while receiving the benefits and 2.1 people per 100,000 for years before. For ER visits for head injuries,

Figure 1.7: *Event Studies of ER visits for Years Before and After DI was Awarded*



NOTE—This figure plots the average number of people per 100,000 who visit the ER per day over the 28-day window around the last business day of the month ($t = 0$) in 1994-2011. The sample is restricted to those who receive disability insurance income at some point in the sample. I then follow the same procedure as in Figure 1.6, but I estimate the model twice, first using the years before person i was awarded DI and, second, using the years after. The figures are drawn for all ER visits (panel a), for ER visits that have at least one diagnosis code that is alcohol and drug related (panel b), and for ER visits whose primary diagnosis code is for a head injury (panel c).

Figure 1.8: *Change in ER visits around $t = 0$ by Time Relative to Award of DI*



NOTE—This figure plots estimates of $\widehat{\beta}_1 - \widehat{\beta}_{-1}$, the change in probability of visiting the ER on the day after the last business day of the month relative to the day before, as a percentage of the base rate. The x-axis is time relative to the first year observed on DI (divided into three year bins). The figures are drawn for all ER visits (panel a), for ER visits that have at least one diagnosis code that is alcohol and drug related (panel b), and for ER visits whose primary diagnosis code is for a head injury (panel c).

the corresponding rates are 6.1 people per 100,000 and 5 people per 100,000. That the overall rates of ER visits are higher while on disability insurance is perhaps not surprising if individuals are healthier on average prior to being awarded disability.

While the levels are higher while receiving benefits, Figure 1.7 shows that the probability of visiting the ER increases around the last business day of the month even in the prior years. Figure 1.8 shows that this increase is present even 9 years before the first year observed receiving DI. The change in probability of an ER visit around $t = 0$ relative to the base rate increases slightly over time, peaking right before the first year observed while on DI for all ER visits and ER visits for head injuries, but peaking at three years after for alcohol and drug related ER visits.

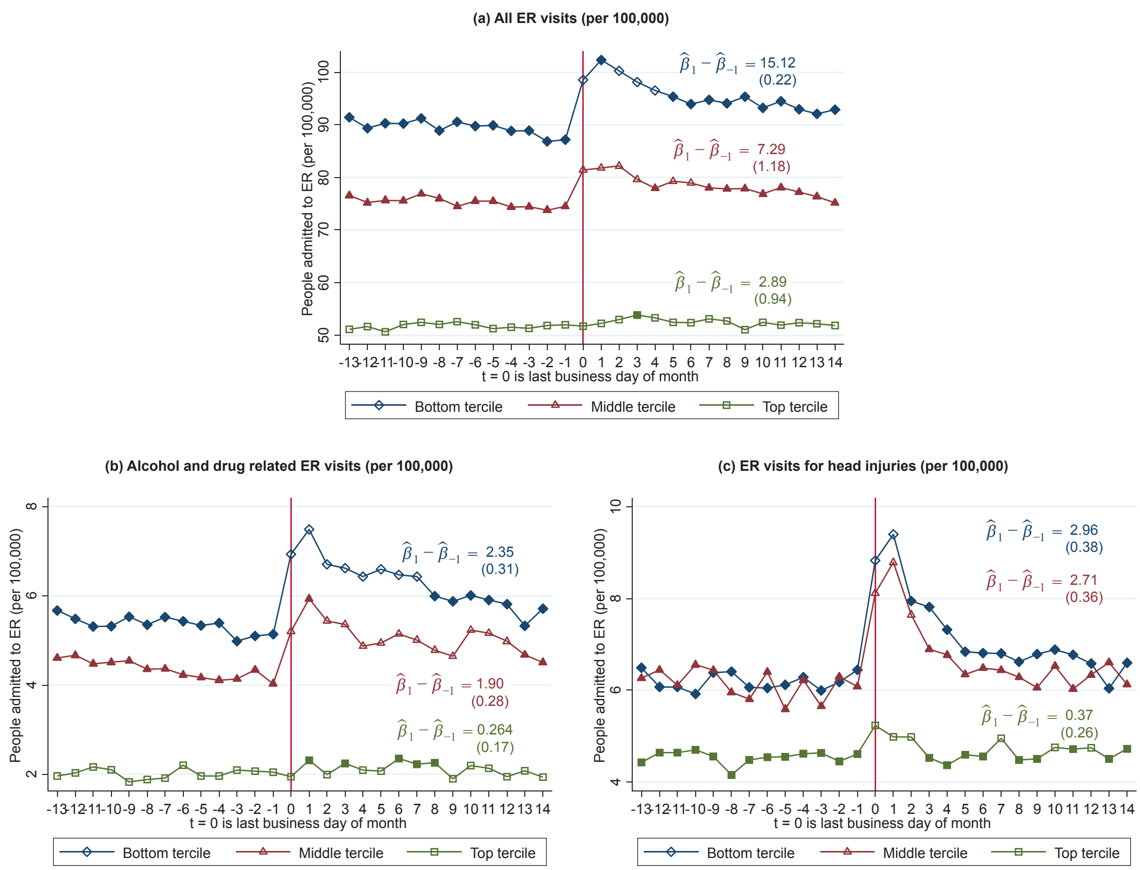
These findings suggest that the increase in the probability of an adverse health event around payment dates is not a direct result of being on disability insurance rolls per se. Instead, these patterns appear to be explained by persistent individual characteristics that pre-date award of DI.

- *Result 3:* *The increase in emergency room visits is driven by the poorest disability insurance recipients.*

Figure 1.9 divides the disability insurance sample into three equal sized groups (terciles) on the basis of their net asset holdings, averaged across the previous two years. In all three panels, the average daily rate of ER visits is higher for the bottom tercile (shown in diamonds) and the middle tercile (shown in triangles), compared to the rate for top tercile (shown in squares). Together, these differences in levels across asset terciles reflect a health-wealth gradient within the disability insurance population.

The figures also clearly show that the probability of visiting the ER increases around $t = 0$ only for individuals in the bottom two terciles of asset holdings. As a percentage of the average daily rate, the magnitude of the increase in all ER visits is decreasing with wealth, at 16.2% in the bottom tercile, 9.5% in the middle tercile, and 5% in the top tercile. For drug and alcohol related visits, the increases for the bottom two asset terciles are essentially equal (39.8% and 40.4%), while the increase for the top asset tercile is significantly smaller

Figure 1.9: *Event Studies by Lagged Net Asset Tercile: DI population*



NOTE–This figure plots the average number of people per 100,000 who visit the ER per day in 1998-2011. The sample is restricted to those receiving disability insurance income during the year in which $t = 0$ falls. I divide the sample by net asset terciles, averaged over the previous two years. The bottom tercile is plotted in diamonds (top line), the middle tercile is plotted in triangles (middle line), and the top tercile is plotted in squares (bottom line). The figures are drawn for all ER visits (panel a), for ER visits that have at least one diagnosis code that is alcohol and drug related (panel b), and for ER visits whose primary diagnosis code is for a head injury (panel c). Please see the notes to Figure 1.6 for more details on the construction of these figures.

at 12.6%. Similarly, the increases for head injuries for the bottom two asset terciles are very close at 44.1% and 41.7%. The increase is again significantly smaller for the top tercile at only 8%.

These results using assets are consistent with the hypothesis that liquidity constraints play a role in the increase in ER visits around dates of payment. However, the results are also consistent with other explanations, including behavioral biases and addiction, either of which could plausibly cause individuals to have low asset holdings and, in addition, to cause them to engage in behaviors that increase the probability of needing to visit the emergency room after receiving income.

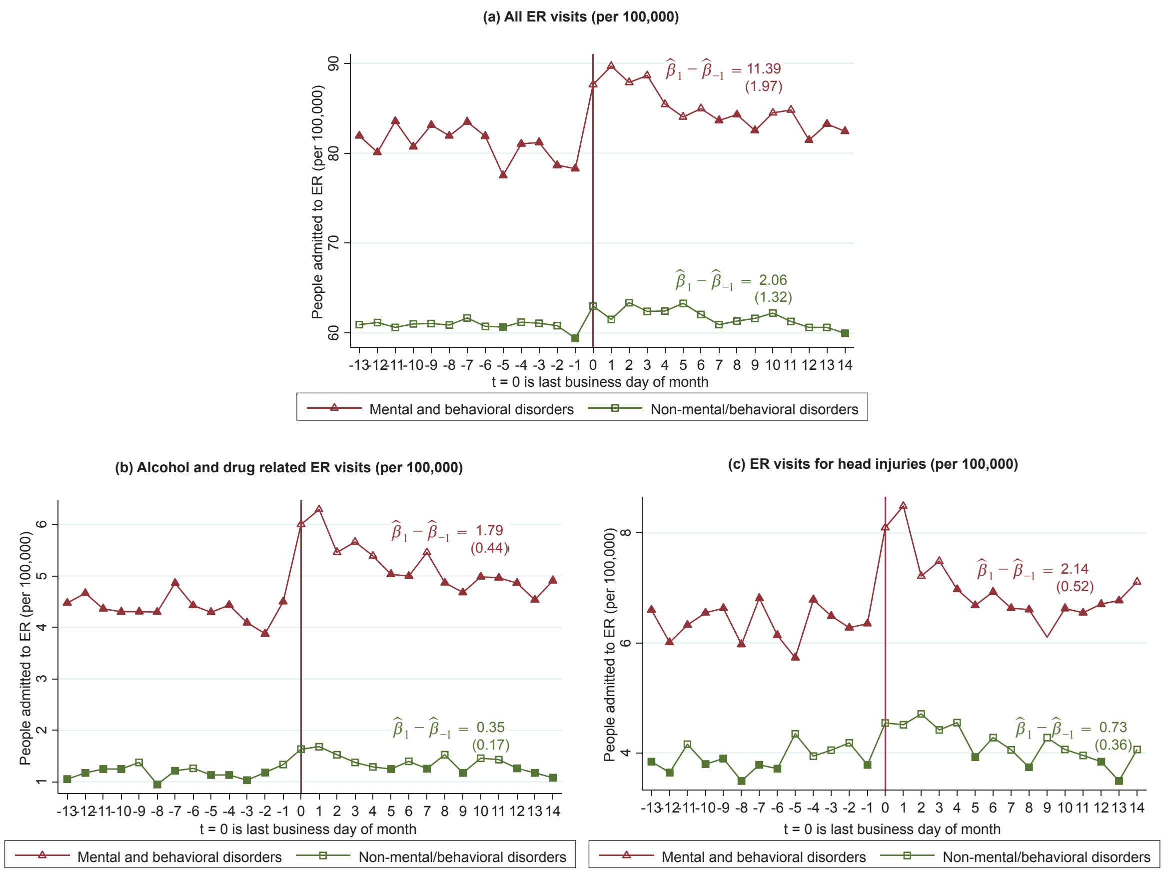
- *Result 4:* *The largest increases in ER visits are for disability insurance beneficiaries with mental and behavioral impairments.*

Figure 1.10 uses the impairments data to separately plot event studies by reason that disability insurance benefits were granted. I divide the sample into disability insurance beneficiaries with mental and behavioral disorders (triangles) and with other disorders (squares).

The mental and behavioral disorders sample is uniformly higher in both average daily rates and in the change in probability of visiting the ER around $t = 0$. The increase as a percentage of the base rate is 13.7% for all ER visits, 37% for drug and alcohol related ER visits, and 31.9% for ER visits for head injuries. No comparable increases are visible for the residual group without mental and behavioral disorders.

In Appendix Figure A5, I restrict attention to those with any mention of drug or alcohol dependence among the reasons that disability was awarded to them. The average daily rates of all kinds of ER visits are about double that of the full DI sample. The daily rates for drug and alcohol related visits are almost four times as large as those for the overall group. The daily rates for head injuries are over three times as large as those for the overall group. The changes in the probability of visiting the ER around $t = 0$ as a percentage of the mean are 27.3% for all ER visits, 49.8% for drug and alcohol related ER visits, and 39.8% for head

Figure 1.10: *Event Studies of ER visits by Reason Awarded Disability Insurance*



NOTE—This figure plots the average number of people per 100,000 who visit the ER per day over the 28-day window around the last business day of the month ($t = 0$) in 1999-2013. The sample is restricted to those who were awarded disability insurance benefits. I divide the sample by reason that disability insurance was awarded: mental and behavioral disorders (in triangles, top line) and other diagnoses that exclude mental and behavioral disorders (in squares, bottom line). The figures are drawn for all ER visits (panel a), for ER visits that have at least one diagnosis code that is alcohol and drug related (panel b), and for ER visits whose primary diagnosis code is for a head injury (panel c).

injuries.[28]

These results show that mental and behavioral disorders and addiction play an important role in explaining why the probability of an adverse health event increases when income is received.

1.5.3 Robustness Checks

This section presents additional results that show that the results described above are robust to alternative samples and controls.

A fully interacted version of Equation 1.8 would be non-parametric in the sense that the conditional expectation function would necessarily be linear. While all the covariates in Equation 1.8 are binary, I do not fully interact them. In this way, I place certain restrictions on the relationship between means on certain days. For example, by not interacting the $t = s$ indicators with the day of the week indicators, I am restricting the change in the mean at $t = 0$ to be the same for every weekday. In Appendix Figure A6, I assess whether the increases in ER visits found around the last business day of the month are different on different days of the week. In each panel of Figure A6, I plot coefficients estimated from Equation 1.8 in the same way as before. However, here, I estimate Equation 1.8 five times, where each time I restrict the sample to exclude the twenty-eight day months where $t = 0$ occurs on a particular day of the week. The figures in the first column are for the full population. The figures in the second column are for the disability insurance sample. Overall, the five lines line up quite closely, with the possible exception being the series plotted in triangles that excludes the twenty-eight day months where $t = 0$ occurs on a Friday. For the full population, this series is lower in levels than the four other series, but for the disability insurance sample, the difference is much smaller. The results are similar no matter which weekday payments are made. The increase around the last business day of the month is not driven by Fridays or any other day of the week.

[28]Note that most head injuries are not coded as drug or alcohol related in these data. In this way, these results illustrate the difficulty in inferring health conditions when one is restricted to diagnosis codes available in hospitalization data.

In Appendix Figure A7, I assess in what ways changes to the vector of controls affect my results. The figures in the first column are for the full population. The figures in the second column are for the disability insurance sample. In each panel, I plot the coefficients from the basic regression model as a reference in diamonds. I then re-estimate the model, adding the days before and after each holiday and interactions of the month and year indicators to the vector of controls. Coefficients from this expanded model are plotted in small squares. I re-estimate the model also where I include these additional controls and exclude the twenty-eight day months during which the New Year's holiday occurs. Coefficients from this third model are plotted in larger squares. While there are small differences in the three series, it is remarkable how closely the lines and points lie on top of each other. The results here are not sensitive to the controls used and are not driven by the New Year's holiday, which always occurs a few days after the final payment of each year.

Lastly, I address the concern that heterogeneity within payment dates could obscure the results for the non-disabled groups. In Appendix Figure A8, I restrict the sample to narrow industries and occupations, where payment dates should be more homogeneous. In panel (a), I plot estimates of Equation 1.8 for teachers (diamonds), the military (triangles), and nurses (squares) using occupation classification codes for the prior year. The results in this figure are similar to those for the wage earner sample overall. I repeat these estimates again in panel (b), but measure time relative to the last calendar day of the month instead of the last business day of the month. Again, these results are very similar to those in the base set of results for wage earners.

1.5.4 Discussion

Prior evidence on adverse health events around payment dates has been in the context and institutional setting of the United States.[29] It is noteworthy then that emergency room visits increase around payment dates even in Denmark, a country with universal healthcare

[29] Andersson, Lundborg, Vikström (2014) have recently presented evidence on mortality in Sweden for government employees around payment dates.

coverage. Patients in the United States are often asked to pay their insurance copayments before leaving the emergency room. In this way, one interpretation of the evidence in the United States is that patients go to the emergency room after receiving income, because that is when they can afford to pay for medical care. Evidence from the Oregon Health Insurance Experiment suggests that this explanation could be very important in the United States among low income households (Taubaum et al. 2014). However, in Denmark, this effect is much less likely to be driving the results. Instead, it seems more plausible that changes in the composition of consumption are the causal channel through which the results are manifested.

While the underlying mechanisms that generate deaths and ER visits could differ, the results documented here do not support the argument made in the literature on mortality (e.g., Evans and Moore 2011, Andersson, Lundborg, Vikström 2014) that increases in rates of adverse health events around payment dates are caused by increased activity in the general population. Instead, I find that the increase in rates of ER visits is largely limited to those who receive disability insurance income or those who will receive it in the future. While activity likely does increase when income is received, my results show clearly that an across the board increase in activity is not what is driving the increase in emergency room visits around the last business day of the month in Denmark. Instead, my results could suggest that either disability insurance beneficiaries engage in high risk behaviors when they receive income, or, that their impairments are so severe that even low risk behaviors are potentially dangerous.

One question is whether the results are driven by supply side responses, which could be especially important here, because more than 60% of the population receives income at once. For example, is there greater availability of drugs and alcohol at these times of the month? It would certainly be interesting to provide additional evidence along these lines. However, one would not expect to find that the increase in ER visits is limited to DI beneficiaries if supply side factors are extremely important.

The results also have implications for what the eligibility criteria should be for disability insurance. For example, the United States eliminated eligibility for disability insurance programs on basis of drug and alcohol abuse in 1996 (Chatterji and Meara 2010). However, Result 2 shows that rates of ER visits increase around the last business day of the month, even in years before receiving disability insurance. Therefore, my results call into question whether eliminating coverage is the right policy to deal with addiction, since other kinds of income, such as wages or other transfer payments, seem to produce the same effect as disability insurance income for a subset of the population.[30] Other policies, such as those tested by Schilbach (2014), may be more effective.[31]

1.6 Conclusions

This paper has presented new evidence from Denmark to quantify the costs and benefits of disability insurance. The first set of results shows that disability insurance is very valuable because the labor supply of disability insurance beneficiaries is sensitive to unconditional transfers. These estimates suggest that the effect of DI on labor force participation is driven in large part by making it feasible for the disabled to stop working, rather than by reducing effective wages. In a standard model, optimal benefits are increasing in the ratio between these two responses. However, the second set of results implies that welfare analysis in this setting may need to be modified because behavioral responses in this population generate fiscal externalities and, perhaps, internalities.

This evidence shows that a subset of disability insurance beneficiaries, who tend to have mental and behavioral disorders and low assets, respond very differently to the timing of payments than do non-disabled populations and other DI beneficiaries. These responses result in increases in the rate of ER visits in the days after payments are received. Although these people respond to income in the same way even before they were awarded DI, the

[30] Autor and Duggan (2006) make a more general critique of the efficacy of eliminating coverage for particular disorders based on the evidence that most of the beneficiaries whose benefits were terminated in 1996 eventually were granted benefits under a different impairment.

[31] Recently, Georgia passed legislation to require drug tests as part of food stamp eligibility. However, Georgia has been prohibited from implementing this law.

externalities (and internalities) generated by their behavioral responses must still be taken into account when setting optimal DI benefit levels. In the formula in Equation 1.3, these results imply that there is an additional term which much be estimated to set benefit levels. In this way, the evidence shows that there are unique challenges to designing a disability insurance system that may not be present for other social insurance programs. For example, the optimal DI benefit amount may differ depending on how disability insurance benefits are dispersed.

An important conclusion from the evidence presented here is that even though the disabled make up a relatively small group, they are the most relevant group to study in order to understand why adverse events increase on the last business day of the month and the group for which alternative program designs should be considered. Many studies suggest using pay frequency as a potential policy tool (e.g., Shapiro 2005, Dobkin and Puller 2007, Mastrobuoni and Weinberg 2009).[32] However, there is little causal evidence on the impacts of proposals of this type. There are two potential research questions with regard to pay frequency. The first question is whether smaller, more frequent payments can smooth rates of adverse events. The second question is whether smaller, more frequent payments can reduce rates of adverse events overall.

My on-going work presents quasi experimental evidence on pay frequency to address these two questions (Bruich, Nielsen, Simonsen, and Wohlfahrt 2014). We use variation in payment frequency of the child benefit in Denmark. Each family with a child receives a cash payment per child until the child turns 18. Benefits are paid out on the 20th of the first month of each quarter for families with children under 14. Benefits for families with children between 15 and 17 were paid out quarterly until 2011, when benefits began to be paid out monthly. Preliminary evidence suggests that the smaller, more frequent payments

[32]One could also change the form of benefits from cash to in-kind or stagger the timing of payments. As an example of the latter, many states have recently staggered Supplemental Nutrition Assistance Program (SNAP) benefits so that different beneficiaries receive income at different times (see e.g., Bruich 2014b for examples). Dobkin and Puller (2007) present quasi-experimental evidence on this type of policy. If one thinks that the evidence on ER visits is driven by social interactions or congestion, this might be another viable alternative payment scheme.

both smoothed and reduced rates of adverse health events overall for families with children in the affected age range, relative to the families with younger children.

The results suggest five directions for future research. First, estimates of the labor supply responses to net wages or state contingent benefits, combined with the estimates in the first part of the paper, would allow one to determine whether increasing benefits further in Denmark would increase welfare using Equation 1.3. Second, implementing a version of this formula that corrects for externalities is also needed based on the evidence in the second part of this paper. Third, experimental or quasi experimental evidence of alternative payment policies affecting the disability insurance population directly would be most valuable, since this group is the sample that displays the most excess sensitivity to the timing of payments. Fourth, it would be useful to develop a theory of optimal pay frequency. Fifth, it would be interesting to provide analogs of the ER visit figures for other outcomes such as crime, traffic accidents, consumption, and expenditures around the last business day of the month. While the overall rate of adverse events is the relevant outcome to guide policy in the case of ER visits, crime, and traffic accidents, in the case of consumption, it is the time path that is most relevant both for policy and for distinguishing between theories of intertemporal behavior.

Chapter 2

The Effect of SNAP Benefits on Household Expenditures and Consumption: New Evidence from Scanner Data and the November 2013 Benefit Cuts

2.1 Introduction

The Supplemental Nutrition Assistance Program (SNAP) spent $80 billion in 2013 to provide monthly income to 47.6 million people in 23 million low-income households. In November 2013, benefits were reduced for all households when temporary benefit increases from the American Recovery and Reinvestment Act (ARRA) expired.[1] These cuts have been widely cited as adversely affecting retail sales and households' ability to feed their families.[2]

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[1] An exception to this is that benefits did not change in Hawaii.

[2] Examples include articles in the *New York Times* (e.g., "As Cuts to Food Stamps Take Effect, More Trims to Benefits Are Expected" by Catherine Rampell on October 28, 2013), *Wall Street Journal* (e.g., "Retailers Brace for

However, no study has been able to quantify the impact of the expiration of these ARRA policies, because most datasets commonly used to study household expenditures are not yet available. This paper provides such estimates.

I use scanner data from 400 grocery stores, aggregated at the store-level, to quantify the impact of the benefit cuts on household expenditures. The stores are located in three large metropolitan areas: Los Angeles, Atlanta, and Columbus, Ohio. The stores have significant market share and make up 8% to 17% of the total number of supermarkets in each area. Household loyalty card identifiers and method of payment are used to measure household spending and establish SNAP participation. The data include the purchases of over 2.5 million households enrolled in SNAP and over 11 million non-participating households.[3]

The "treatment effect" of the expiration of the ARRA varies across stores in my sample depending on the fraction of customers at each store who participate in SNAP.[4] I use a difference in differences research design, where I define the degree of treatment at each store as the pre-period fraction of households shopping at that store who used SNAP to pay for at least one purchase.

Using this approach, I find that each $1 of benefit cuts reduced SNAP household spending by $0.37, with larger impacts in Atlanta than in Los Angeles or Columbus. About 3/4 of the decline was to spending on grocery items and meat, while 16% was from spending on non-food general merchandise and over the counter drugs. I estimate a marginal propensity to consume food out of food stamp benefits of 0.30, with a 95% confidence interval of [0.154, 0.456].

This paper is one of only a handful of studies to present estimates of the effect of SNAP benefits on expenditure using a quasi-experimental research design. The first paper to use a

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Reduction in Food Stamps" by Shelly Banjo and Annie Gasparro on November 1, 2013), and *Slate* (e.g., "Did cuts to Food Stamps Undo Family Dollar?" by Alison Griswold July 28, 2014).

[3]The data only include grocery store spending, but over 80% of all SNAP benefits are redeemed at supermarkets, 96.3% of households redeem at least some of their benefits at these kinds of stores (Castner and Henke 2011, tables A-4 and II.11), and over 70% of low-income household expenditures on food consumed at home occur at grocery stores (Damon, King, and Leibtag 2013, table 3).

[4]The research design is similar to Card (1992).

quasi experimental research design in this literature was Hoynes and Schanzenbach (2009), who study consumption in the Panel Study of Income Dynamics (PSID) during the initial staggered rollout of the Food Stamp Program across counties. Recently, Nord and Prell (2011) and Beatty and Tuttle (2012) measure the effect of the 2009 ARRA benefit increases by comparing changes in consumption and expenditures by SNAP eligible households with those by ineligible households.

Although all the results presented here should be interpreted as applying only to household expenditures at grocery stores, the estimates are in line with those of Hoynes and Schanzenbach (2009, table 6), which is noteworthy given the very different data used and four decades that have passed since the time period studied in that paper. Importantly, my data and research design yield more precise estimates than Hoynes and Schanzenbach, with confidence intervals that are 80% to half as wide. The magnitude of these estimates is about half to 1/3 the size of the largest estimates from the cross-sectional empirical literature from the 1970s and 1980s (surveyed in Fraker 1990 and Burstein et al. 2004).

This paper's broader context is as part of the literature on the impact of the ARRA on the economy (e.g., Chodorow-Reich et al. 2012) and on the optimal design of fiscal policy to stimulate domestic spending during recessions (e.g., Feldstein 2009). This paper also builds on previous studies in public economics that use scanner data to test hypotheses that have important implications for public policy (e.g., Chetty, Looney, and Kroft 2009, Hastings and Washington 2010).

The rest of this paper is organized as follows. The next section summarizes the basic economics of food stamps and provides abbreviated background information on the SNAP program, the November 2013 benefit changes, and how the program distorts labor supply.[5] Section 3 describes the quasi-experimental research design. Section 4 describes the scanner data and presents summary statistics for the stores and households in my sample. I also compare my sample with those from other papers in the literature. Section 5 presents the

[5]See Nord and Prell (2011), Farson Gray and Eslami (2014), and the USDA's *Facts About SNAP* (USDA 2013) for further details on the SNAP program.

estimates of the effect of the SNAP benefit cut on household spending, along with other supporting evidence and policy calculations. Section 6 discusses these results in the context of the 2014 Farm Bill, which further reduced SNAP benefits in 2014. Section 7 is a brief concluding section that ends with directions for additional research.

2.2 Background and the November 2013 benefit cuts

2.2.1 Background and economics of food stamps

SNAP provides income to low-income households each month with the stated goal of helping them buy food.[6] In April 2014, the average SNAP household consisted of just over two people and received $256 in benefits per month. SNAP benefits are restricted in that they can only be used to pay for certain food items purchased at retailers that have applied for and received authorization to participate in the program from the U.S. Department of Agriculture. Excluded items include alcohol, hot foods, and toiletries. Households receive benefits via an Electronic Benefits Transfer (EBT) card. Similar to a debit card, households access their benefits by swiping the card and entering a pin number at the time of purchase.

The "textbook" economic analysis of SNAP benefits contrasts cash benefits, which are unrestricted, with in-kind benefits, like SNAP, which can only be used to purchase certain goods (Schanzenbach 2002 and Hoynes and Schanzenbach 2009). This analysis predicts that there should be no difference between giving households cash or food stamp benefits, as long as they would have spent at least as much on food as the dollar value of their SNAP benefits. These households are infra-marginal and this fungibility result implies that these households should treat a $1 decrease in food stamp benefits in the same way that they would treat losing $1 of other income.[7]

[6]Benefits are federally funded, but states share in the administrative costs.

[7]Put differently, if a paternalistic government were to use in-kind benefits as an instrument to change households' consumption bundles (e.g., less alcohol and more vegetables), then this analysis implies that it would only be successful in influencing what marginal households consume (i.e., households who would not have eaten any vegetables or only very few vegetables if given cash income).

SNAP beneficiaries are exempt from paying local and state sales taxes on any of the products that they purchase with SNAP tender. However, many of the products that are eligible to be paid for with SNAP tender are often already not subject to sales taxes.[8] Whether this sales tax exemption is quantitatively important depends the local sales tax rates and bases in place, as well as the degree to which households pay attention to sales taxes (Chetty, Looney, and Kroft 2009).

SNAP benefits are paid out once per month. Some states pay benefits to all recipients at once, but it is becoming more common for benefits to be staggered so that different households receive benefits on different days. In Los Angeles, benefits are paid out over the first 10 days of each month. In Atlanta, benefits are paid out between 5th and the 23rd of each month. In Columbus, benefits had been paid out across the 1st to 10th of each month, but were recently spread out over the first 20 days (even days only) for SNAP recipients awarded benefits after February 28, 2014.[9,10]

In the next two sections, I discuss how benefit amounts and eligibility are determined and how this changed when the ARRA was enacted and when it expired. Finally, I discuss the labor supply incentives that SNAP households face due to the interaction of SNAP with the personal income tax system.

2.2.2 Benefit amounts

Benefit amounts increase as the number of people in the household increases, and decline at a 30% rate as total household income after deductions increases. Benefits decline at a 24% rate as earned income increases because of a 20% earned income deduction. In particular,

[8]SNAP households with complete information should spend their benefits on taxable items (e.g., carbonated beverages in California, but not in Massachusetts) and buy untaxed items with other resources (e.g., cookies in California and Massachusetts).

[9]The USDA maintains a webpage with information on the benefit issuance schedule for each state at http://www.fns.usda.gov/snap/snap-monthly-benefit-issuance-schedule.

[10]Although Hastings and Washington (2010) find that stores are able to raise prices when benefits are dispersed all at once, in Ohio it was grocery retailers who requested that benefits be staggered more evenly because uneven demand creates inventory and staffing challenges.

benefit amounts are determined by the following formula:

$$B(N, Z, Y, D) = \begin{cases} B_0(N) - 0.3(Y + 0.8Z - D) & \text{if } 0.8Z + Y > D \\ B_0(N) & \text{otherwise} \end{cases}$$

where $\$B_0(N)$ is the maximum that a household of size N can receive, $\$Z$ is earned income, $\$Y$ is unearned income, and $\$D$ are deductions.[11] There are some differences in the allowable deductions across states. The maximum benefit amounts are the same in all states except for Hawaii and Alaska, where they are set higher to reflect higher food costs.

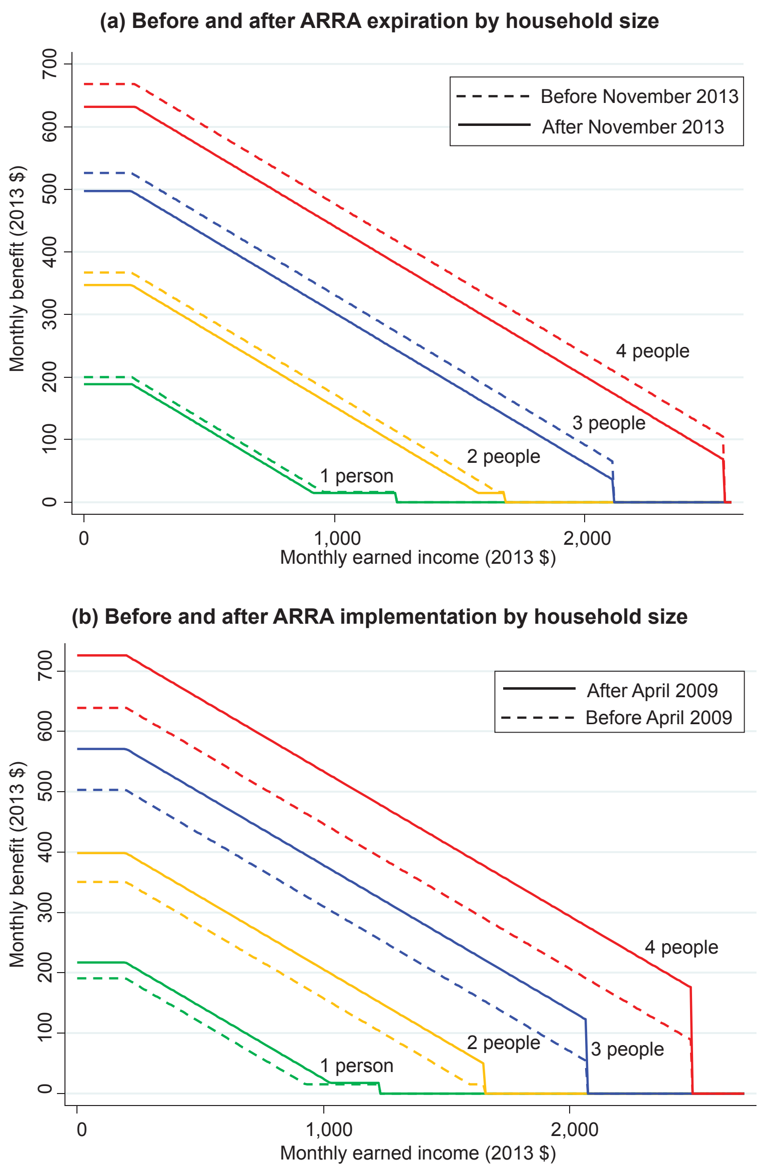
In an effort by policymakers to increase domestic spending during the Great Recession, part of the $800 billion ARRA fiscal package was devoted to raising SNAP benefit amounts. The ARRA modified the SNAP benefit formula by increasing $\$B_0(N)$, the maximum amount each household could receive. The higher benefits started in April 2009 and remained fixed at these levels until November 2013.

Figure 2.1 plots the benefit amount before and after ARRA as a function of earned income for one to four person households. Panel A shows the benefit amounts before (solid line) and after (dashed line) the expiration of the benefit increases in November 2013. Panel B plots the corresponding schedule before (solid line) and after (dashed line) the initial implementation of ARRA in April 2009. Average earned income for SNAP households in 2012 was $326 a month including zeros (Farson Gray and Eslami 2014). At this income level, average benefits per month would have been $168, $335, $493, and $639 in October 2013 for one to four person households, respectively. The decreases in November 2013 for each of these households were $11, $20, $29, and $36. The corresponding increases in April 2009 were larger both in nominal and in real 2013 dollars. As shown in the figure, all households of a given size experienced the same dollar change in benefits, except for some higher-income one and two person households, whose benefits were only increased by $2 in April 2009 and were only reduced $1 a month in November 2013.

Figure 2.2 plots the average benefit amount per household actually paid out for each

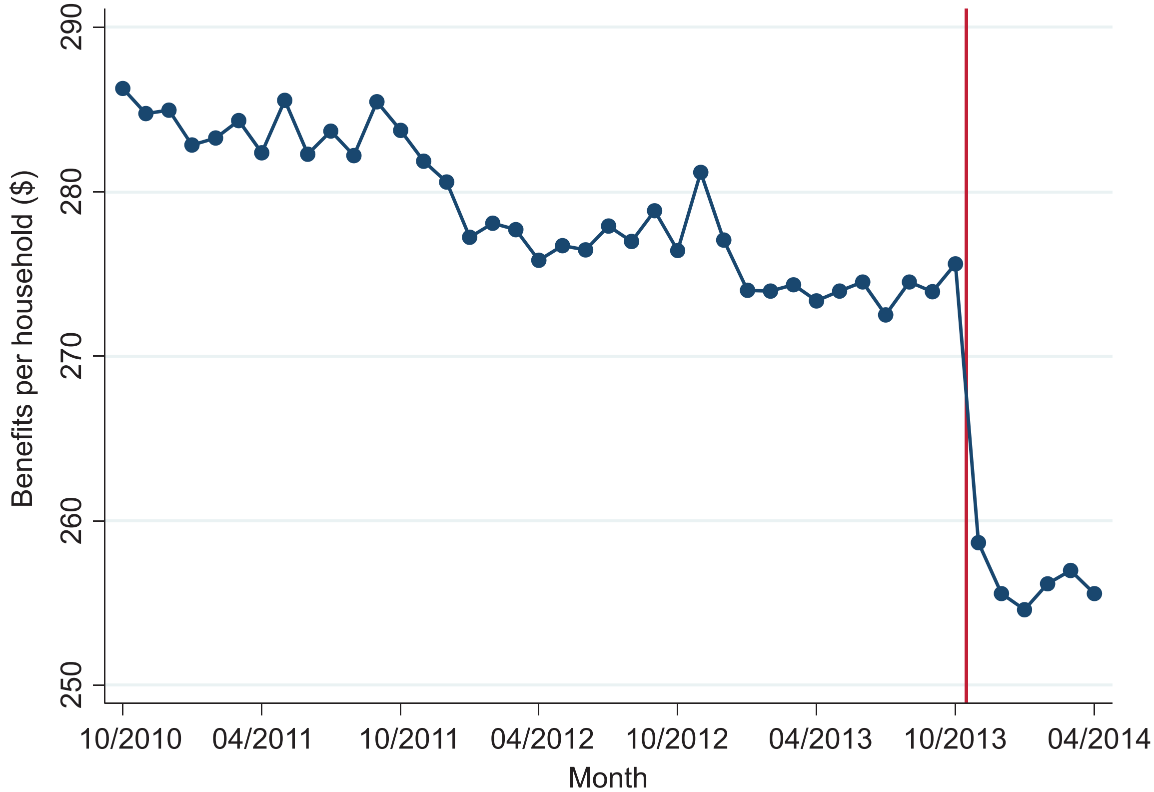
[11] Households receive either $\$B(N, Z, Y, D)$ given by the formula or a minimum amount, currently $15, if the amount given by the formula is smaller than the minimum amount.

Figure 2.1: *SNAP Benefits before and after the ARRA by Household Size and Earnings*



NOTE–This figure plots the SNAP benefit amount as a function of earnings before and after the ARRA expired (panel A) and before and after it was implemented (panel B). Each panel plots the monthly benefit amount along the y-axis with earned income along the x-axis, for one (green), two (yellow), three (blue), and four (red) person households. Panel A shows the decrease in benefits when the ARRA expired in November 2013; the dotted line plots benefit amounts in October 2013 and the solid line shows the benefit amount in November 2013. Panel B shows the increase in benefits when ARRA was implemented in April 2009; the dotted line plots benefit amounts in place in October 2008 to March 2009 and the solid line plots benefit amounts after April 2009. To calculate the benefit amount, I assume that the household has no income other than that shown along the x-axis and that the only deductions taken are the standard deduction and the 20 percent earned income deduction. Benefit amounts and earnings are adjusted for inflation using the CPI-U.

Figure 2.2: *Average Monthly SNAP Benefits per Household, 2010–2014*



NOTE–This figure plots the average monthly SNAP benefit amount per household from October 2010 to April 2014. The red line at November 2013 is when the ARRA benefit increases expired. The change in average benefits from October 2013 to November 2013 is $17. Benefit amounts are nominal (not inflation adjusted). The series plotted is from the United States Department of Agriculture Food and Nutrition Service (2014), Supplemental Nutrition Assistance Program (SNAP) Monthly Data, National level, available at http://www.fns.usda.gov/sites/default/files/pd/34SNAPmonthly.pdf.

month between October 2010 and April 2014. After remaining relatively stable since 2012, mean benefits dropped sharply by $17 in November 2013. It is the impact of this $17 decrease in SNAP benefits that this paper seeks to estimate.

2.2.3 Eligibility

There are two ways for a household to become eligible for SNAP. The first way is if all members of the household receive benefits through either the Supplemental Security

Income[12] program, the Temporary Assistance for Needy Families program, or a county general assistance program. The second way is if household income and assets are below certain thresholds. Income and assets are measured the month prior to applying for benefits and are re-assessed at periodic intervals. In addition, there are minimum work requirements (20 hours per week) for non-disabled adults between 18 and 50 years old without children. SNAP benefits may only be received by adults who do not meet this work requirement for three months out of the previous three years. Beneficiaries can often substitute work training or volunteering for the work requirement.

In addition to changing the maximum benefit levels, the ARRA allowed states to waive this work requirement from April 2009 through September 2010. Nord and Prell (2011, page 13) argue that this eligibility part of the ARRA had a relatively minor effect on participation in SNAP because it impacted only a small fraction of potential recipients. Further, many states already had been granted state-wide or partial waivers based on economic conditions prior to the ARRA. When the ARRA waivers expired at the end of September 2010, most states still remained eligible to waive the work requirement based on these economic criteria, although even some waiver-eligible states have since re-instated the work requirement.

Enforcing the work requirement has two effects on SNAP benefit amounts. First, households who are unable to meet the requirement receive no benefits after three months. Second, households who are induced to work or work more receive lower benefits, because benefits amounts are phased out with income (as shown in Figure 2.1). However, the combined value of SNAP benefits and (now positive) earned income would likely increase.

As of 2014, California and Georgia still have state-wide waivers. Ohio has re-instated the work requirements in most counties, including those that make up the Columbus metro area where the stores in my sample are located. The earliest beneficiaries in Ohio could lose benefits because of the work requirement was January 2014.

[12] An exception is that in California, Supplemental Security Income payments have included an additional cash amount for food stamp benefits since 1974. Therefore, individuals in California who receive Supplemental Security Income cannot also receive SNAP benefits separately.

2.2.4 Work disincentive effects

SNAP distorts labor supply by reducing the financial gains to entering the labor force and by increasing households' marginal tax rates by 24 percentage points over and above the payroll tax and personal income tax rates, resulting in very high rates for households whose income places them in the phase out range of other transfer programs and tax credits. Figure 2.3 illustrates how the personal income tax system interacts with the SNAP phase out rate for households consisting of one adult and either zero, one, two, or three children in 2014. See Appendix B for further discussion. SNAP also reduces labor supply through an income effect. Hoynes and Schanzenbach (2012) find evidence that SNAP does indeed reduce labor supply.[13]

While providing SNAP benefits to households that have suffered temporary or permanent income losses is potentially very valuable, the calculations described above show that beyond the direct costs of the program, SNAP also has efficiency costs due to the ways that it distorts labor supply. These distortionary effects of the program provide a backdrop to results presented in the rest of the paper.[14]

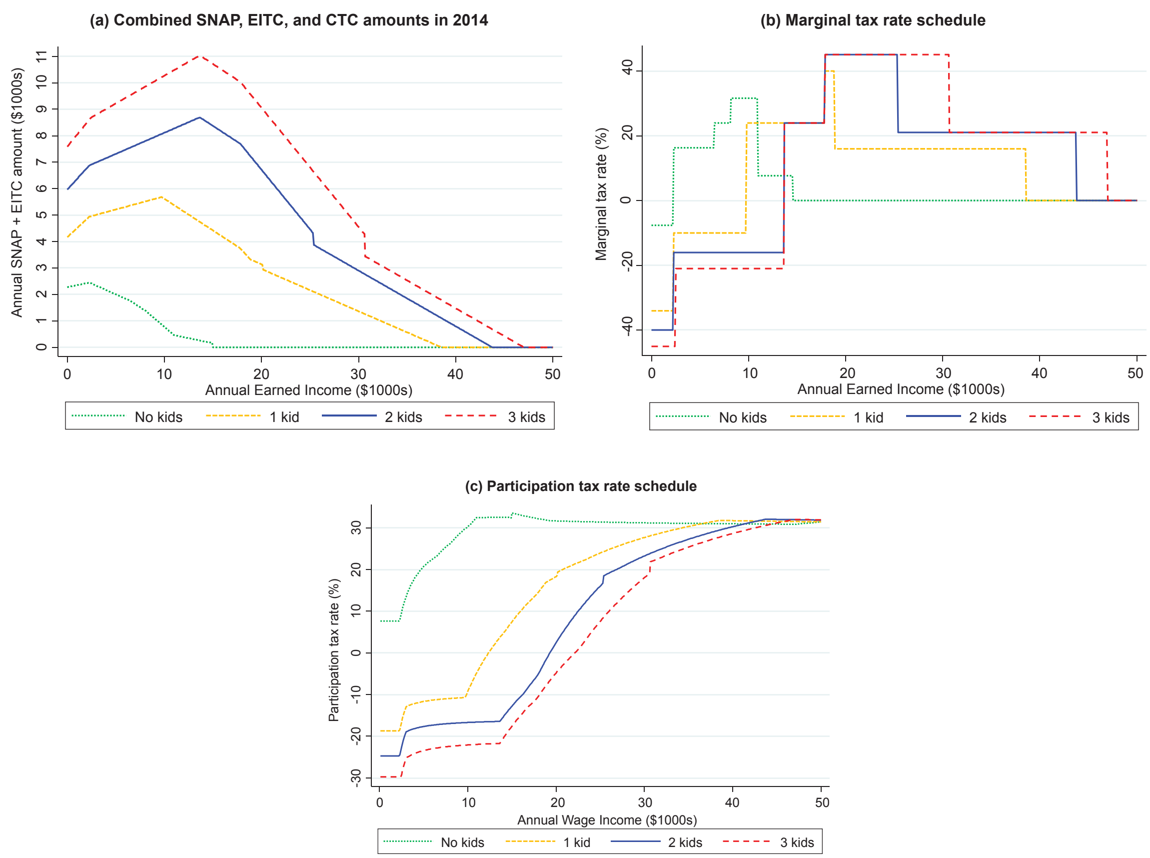
2.3 Empirical strategy

As shown in Figure 2.2, the average monthly SNAP benefit amount fell by $17 when the ARRA benefit levels expired in November 2013. In California, Georgia, and Ohio, the decrease was higher at $20 per household. How did this decrease in benefits affect household spending? The main challenge in answering this question is separating the effect of the SNAP policies from all the other confounding factors that also could have affected household spending.

[13]The reduction to labor supply defined more broadly could be even greater (Feldstein 1999).

[14]A program designed more like the EITC would lessen the distortion to labor force participation, but would not provide as much support to those with the least amount of resources. Saez (2002) formalizes the tradeoffs involved and Brewer, Saez, and Shephard (2010) discuss these results and apply them to the tax and transfer system in the United Kingdom.

Figure 2.3: *Combined SNAP, Income Tax, and Payroll Tax Schedules in 2014*



NOTE–This figure plots the combined SNAP, income tax, and payroll tax schedules in 2014 for a one person household (green dotted line) and households consisting of one adult and one child (yellow dotted line), one adult and two children (blue solid line), and one adult and three children (red dotted line). Panel A plots the combined value of the SNAP benefits, earned income tax credits (EITC), and refundable child tax credits (CTC) for which each household would be eligible. Panel B plots the marginal tax rates faced by each household, including the SNAP benefit reductions, payroll taxes (both employee and employer portions), and federal personal income taxes. Panel C plots the participation tax rate for each of these households. The participation tax rate is calculated as: $1 - (\text{after tax income} + \text{SNAP benefits at this income} - \text{SNAP benefits at zero income})/\text{pre-tax income}$. In all three panels, I calculate SNAP benefits assuming that the household has no income other than that shown along the x-axis and that the only SNAP deductions taken are the standard deduction and the 20 percent earned income deduction. I use the NBER TAXSIM calculator to compute taxes owed and tax credit amounts for these households, assuming that they have no other exemptions or deductions.

To address this identification problem, I use a difference in differences research design that compares changes in sales at stores with many customers whose income fell when the ARRA expired with sales at stores where very few customers were affected. Letting stores be indexed by $k = 1, \ldots, K$, I define the treatment at store k as the pre-period fraction of households shopping at store k who paid for at least one purchase using SNAP during a given month. This approach will recover the effect of the expiration of the ARRA benefit increases on SNAP households if household expenditures at stores with high and low SNAP shares would have changed by the same amount if ARRA did not expire, which is the parallel trends between treated and control units assumption, but here, I use a continuous treatment variable.

In particular, let $SNAP_{it}$ be an indicator variable for whether household i used SNAP to pay for at least one purchase during month t and let $\overline{SNAP}_k$ be the average of $SNAP_{it}$ across households who shopped at store k from January 2012 to April 2013. Conceptually, my estimates are based on equations of the form:

$$\overline{y}_{kt} = \alpha + \beta\overline{SNAP}_k + \delta post_t + \gamma\overline{SNAP}_k \times post_t + \overline{u}_{kt} \tag{2.1}$$

where $\overline{y}_{kt}$ is expenditure per household (or another outcome) at store k during month t, $\overline{SNAP}_k$ is the SNAP intensity measure defined above, and $post_t$ is an indicator variable for whether the month is November 2013 or later.[15] Specifying this estimating equation as linear in $\overline{SNAP}_k$ is motivated by the non-parametric household-level regression that uses y_{itk} and $SNAP_{it}$ defined at the individual-level in place of $\overline{y}_{kt}$ and $\overline{SNAP}_k$ defined at the store-level.

The coefficient of interest is γ on the interaction term, which can be directly interpreted

[15] An alternative estimating equation would group the data into average expenditures for SNAP and non-SNAP households separately at each store, using $2 \times K \times T$ observations, where T is the number of months, to estimate a regression of the following form:

$$\overline{y}_{gkt} = \alpha + \beta SNAP_{gk} + \delta post_t + \gamma SNAP_{gk} \times post_t + \overline{u}_{gkt}$$

where g denotes group (i.e., SNAP or non-SNAP) and $SNAP_{gk}$ is a binary group indicator variable. Although the data are broken down into sales to SNAP and non-SNAP households, the groups are defined at the monthly level so that the composition of each group is changing over time. It would be useful to estimate this alternative estimating equation using data where the groups are defined instead using a stable, pre-treatment definition.

as measuring the effect of the benefit cuts on SNAP households' expenditures. To build intuition for Equation (2.1), note that a store with $\overline{SNAP}_k = 0$ would have no SNAP customers while a store with $\overline{SNAP}_k = 1$ would have all SNAP customers. The coefficient β in Equation (2.1) captures the impact of this one unit change on average household spending before November 2013. The coefficient γ measures the degree to which the effect of this one unit change differed for all the months after November 2013 compared with the earlier months.

Grocery expenditures per household are highly seasonal, with large increases during November and December. If this seasonal trend differs for stores with many SNAP customers and stores with fewer SNAP customers, then estimates based on Equation (2.1) will be biased. Therefore, I replace the dependent variable with the 12-month difference in spending per household at store k as a store-specific way of seasonally adjusting the series. Defining this variable as $\Delta\overline{y}_{kt}$, my preferred estimates are based on the following difference in difference in differences (triple difference) specification:

$$\Delta\overline{y}_{kt} = \alpha + \beta\overline{SNAP}_k + \delta post_t + \gamma\overline{SNAP}_k \times post_t + f_k(t) + \psi X_{kt} + \overline{u}_{kt} \tag{2.2}$$

where the first three independent variables are defined as in Equation (2.1), $f_k(t)$ is a linear time trend interacted with store fixed effects, and X_{kt} is a vector of controls that vary by specification, including indicator variables for each store or region, interaction terms between region and $\overline{SNAP}_k$, and interactions between region and $post_t$ or between region and month fixed effects. The coefficient of interest is again the coefficient γ on the interaction term. I cluster standard errors at the store-level to allow for serial correlation in 12-month differences across months at a particular store. I weight observations by the number of households shopping at store k, averaged over the two months that are differenced.[16]

As a robustness check, I also use a less parametric form of Equation (2.2) where I include

[16]Solon, Haider, and Wooldridge (2013) show that if there is within-group correlation in the error structure, weighting may make my estimates less precise than if I used ordinary least squares. I find slightly larger point estimates from unweighted regressions, but with larger standard errors. The difference is driven entirely by the presence of several smaller stores in Los Angeles. Unweighted and weighted results are virtually identical for Atlanta and Columbus.

indicators for each month (instead of $post_t$) and the interaction of these month indicators with the $\overline{SNAP}_k$ treatment variable. To formally test whether the change in household spending is equal across regions, I also run specifications that include triple interaction terms of region, $post_t$, and $\overline{SNAP}_k$.

Note that in Equations (2.1) and (2.2), I define $\overline{SNAP}_k$ as measured during a base-period rather than concurrently. I follow this approach because the concurrent fraction of households who use SNAP to pay may not be exogenous. In all the regressions presented below, I define the main treatment variable using data ending six months before November 2013.

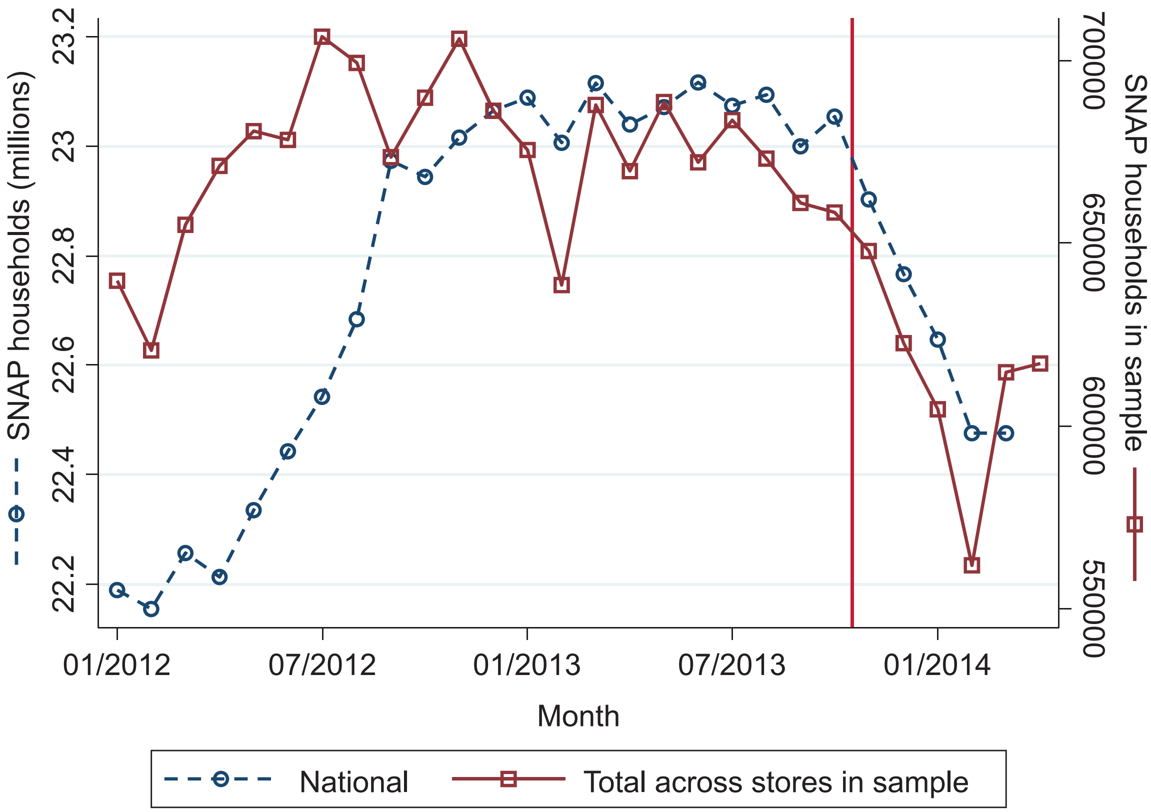
2.4 Data and Summary Statistics

2.4.1 Data

The panel data consist of daily sales at 431 grocery stores from January 1, 2012 to April 30, 2014. The grocery stores are located in Los Angeles, CA, Atlanta, GA, and Columbus, OH. In each market, the retail banner was chosen on the basis of having significant market share. I collapse the data at the month-store level. I restrict the sample to stores with observations in both January 2012 and April 2014, leaving 395 stores in my main analysis sample. On a pure number of establishments basis, the stores make up 8% to 17% of the total number of supermarkets in each area as reported in the U.S. Census Bureau's 2012 County Business Patterns data.

Household loyalty card identifiers and method of payment are used to measure household spending and establish SNAP participation. Each month, the data include the purchases of over half a million households enrolled in SNAP and millions of non-participating households. A household i is defined as a SNAP participating household in month t if it used SNAP benefits to pay for more than half of at least one purchase during month t. Sales for each store are tabulated separately for SNAP and non-SNAP households. The sales data are sales among households that have a loyalty card. Sales to households without loyalty cards

Figure 2.4: *Number of SNAP Households Nationally and in Sample, 2012–2014*



NOTE—This figure plots the total number of people receiving SNAP from 2012 to 2014 nationally (dotted line, left axis) and the number of SNAP households in my sample (solid line, right axis). A household shopping at multiple stores is counted more than once. The national series is from the United States Department of Agriculture Food and Nutrition Service (2014), Supplemental Nutrition Assistance Program (SNAP) Monthly Data, National level, available at http://www.fns.usda.gov/sites/default/files/pd/34SNAPmonthly.pdf.

are excluded.

Households transition in and out of SNAP as their income and family circumstances change (and enter and exit the sample depending on where they shop). Households may also transition out of SNAP because of the requirement that they pay for half of at least one purchase using SNAP tender in order to be counted as a SNAP household in these data. Therefore, it is useful to have a count of the number of unique households who ever used SNAP tender to pay for more than half of one purchase across the entire sample period. These counts are reported in rows 11 and 12 of Panel A in Table 2.1. There are 2.5

million unique households that ever used SNAP to pay for more than half of at least one purchase during the entire 26 month sample period. In the last 12 months in the data, there were 1.5 million such households. The corresponding counts for non-SNAP households are 14 million and 9.9 million, respectively. Therefore, households ever using SNAP make up 18% and 15% of all households in the sample across these two different time periods, respectively.

To assess the representativeness of the sample, Figure 4 plots the number of SNAP households nationally (left axis) and the number of SNAP household-store observations in my sample (right axis) over time. The two series track each other quite closely, first increasing from the beginning of 2012 to the middle of 2013, then declining sharply starting in the Fall of 2013 through 2014. This recent decline is notable because it reverses the trend of increasing SNAP rolls that had been on-going since the early 2000s (Wilde 2013, Ganong and Liebman 2013).

2.4.2 Summary statistics

Table 2.1 presents summary statistics for the households (panel A) and stores (panel B) in my sample. Average household expenditure, visits to the store, and units purchased are almost twice as large among SNAP households as for non-SNAP households. While the difference in my sample is larger, Hastings and Washington (2010) also find a similar pattern at three high poverty Nevada stores. They attribute this difference to larger household sizes, more food consumed at home, and fewer stores visited per month by welfare recipients.

Average spending by SNAP households in my sample at $161 accounts for 80% of monthly grocery store expenditures by low income households reported in Damon, King, and Leibtag (2013).[17] Estimates of total expenditures per month across all types of stores range from $269 for low income households (Damon, King, and Leibtag 2013) to $323.30 for food consumed at home by SNAP eligible households (Hoynes, McGranahan,

[17]Damon, King, and Leibtag (2013) report 7 day average expenditure in 2003 at grocery stores in column 3 of Table 3, which I convert to monthly expenditures in inflation adjusted 2013 dollars.

Table 2.1: *Summary Statistics*

Panel A. Household characteristics

Household type:	All (1)	SNAP (2)	Non-SNAP (3)
1. Total expenditures per month per store	**$96.13**	**$160.71**	**$89.26**
	(25.79)	(40.79)	(27.18)
2. Number of visits to store per month per store	**3.42**	**6.73**	**3.12**
	(0.56)	(1.40)	(0.50)
3. Number of units purchased per month per store	**36.49**	**66.07**	**33.42**
	(8.97)	(14.89)	(9.13)
4. Expenditures in produce department	**$11.24**	**$15.70**	**$10.81**
	(3.32)	(4.19)	(3.56)
5. Expenditures in grocery department	**$52.27**	**$89.54**	**$48.24**
	(13.72)	(22.83)	(14.25)
6. Expenditures in meat department	**$11.59**	**$23.32**	**$10.18**
	(3.82)	(7.38)	(3.37)
7. Expenditures in drug and general merchandise dept.	**$11.01**	**$17.11**	**$10.41**
	(4.83)	(8.12)	(4.74)
8. Expenditures in deli/bakery department	**$6.32**	**$10.48**	**$5.97**
	(2.40)	(3.89)	(2.51)
9. Expenditures in natural foods department	**$3.39**	**$4.04**	**$3.36**
	(3.10)	(3.70)	(1.74)
10. Number of household x store observations per month	**7,075,351**	**633,310**	**6,442,042**
	(170,723)	(29,059)	(158,675)
11. Number of unique households in sample from January 2012 to April 2014	**14,011,395**	**2,501,204**	**11,510,191**
12. Number of unique households in sample from May 2013 to April 2014	**9,885,582**	**1,524,998**	**8,360,584**

Panel B. Store characteristics

Region:	All (1)	Los Angeles (2)	Atlanta (3)	Columbus (4)
1. Households shopping at store per month	**17,919**	**16,579**	**19,691**	**18,912**
	(8,359)	(10,458)	(4,461)	(4,706)
2. Fraction of households that are SNAP	**9.93%**	**8.46%**	**11.60%**	**11.59%**
	(8.60)	(7.89)	(8.75)	(9.88)
3. Number of stores in sample	**395**	**210**	**125**	**60**
4. Census Bureau count of supermarkets in each region in 2012	**3,899**	**2,598**	**950**	**351**
5. SNAP participating retailers in each region in 2014	**14,330**	**8,453**	**4,411**	**1,466**

NOTE -- Table reports means with standard deviations in parentheses. The sample is restricted to 395 stores with observations in both January 2012 and April 2014, except in rows 11 and 12 which use the full 431 store sample. Sample statistics in panel A rows 1-9 are estimated by weighting monthly store-level observations by the number of households shopping at each store each month. SNAP households are households that used SNAP to pay for more than half of at least one purchase that month. The total number of stores in each region is reported in the U.S. Census Bureau's County Business Patterns (CBP) data for 2012. Number of SNAP participating retailers is from the USDA. Columbus is Columbus, Ohio.

Continued on next page

Table 2.1: *Summary Statistics (continued)*

Panel C. Region characteristics	National	Los Angeles	Atlanta	Columbus
	(1)	(2)	(3)	(4)
I. Demographics				
1. Population in 2013	**316,128,839**	**13,131,431**	**5,522,942**	**1,967,066**
2. Percent white in 2010 Census	**72.4%**	**52.8%**	**55.4%**	**77.5%**
3. Percent black or African American in 2010 Census	**12.6%**	**7.1%**	**32.4%**	**14.9%**
4. Percent hispanic or latino (of any race) in 2010 Census	**16.3%**	**44.4%**	**10.4%**	**3.6%**
5. Percent urban in 2010 Census	**80.7%**	**99.5%**	**89.1%**	**85.6%**
II. Labor and housing markets				
6. Median household income (2012 $)	**$53,046**	**$60,583**	**$57,470**	**$54,628**
7. Median home price in 2013	**$197.4K**	**$405.6K**	**$139.5K**	**$142.8K**
8. Percent with income below poverty level	**14.88%**	**15.84%**	**14.49%**	**14.88%**
9. Unemployment rate in October 2013	**7.00%**	**8.50%**	**7.70%**	**6.10%**
10. Unemployment rate in November 2013	**6.60%**	**8.40%**	**7.00%**	**6.10%**
11. Δ Unemployment (Nov. '12 to Nov. '13)	**-0.80%**	**-0.80%**	**-1.10%**	**+0.8%**
III. SNAP enrollment and benefits				
12. Number of SNAP households in 2013, per month on average	**23,027,261**	**658,419**	**444,748**	**131,628**
13. Number of people in 2013, per month on average	**47,486,717**	**1,365,475**	**951,420**	**286,339**
14. Percent of population in 2013	**15.02%**	**10.40%**	**17.23%**	**14.56%**
15. Mean monthly SNAP benefit per household in October 2013	**$276.41**	**$328.44**	**$300.28**	**$295.43**
IV. Changes from October 2013 to November 2013				
16. Δ Mean SNAP benefit per household	**-$16.97**	**-$22.17**	**-$16.76**	**-$20.23**
17. Δ Total benefits/number of HHs in Oct.	**-$18.66**	**-$22.72**	**-$30.42**	**-$21.82**
18. Δ Unemployment rate (% of Oct. rate)	**-5.71%**	**-1.18%**	**-9.09%**	**0.00%**
19. Δ SNAP Households (% of total in Oct.)	**-0.65%**	**-0.18%**	**-4.82%**	**-0.58%**
20. Δ SNAP Households (number)	**-150,851**	**-1,230**	**-20,449**	**-758**

NOTE -- Population estimates are from the Census Bureau. The unemployment rates are unadjusted estimates from the Bureau of Labor Statistics. Median home price is the median sales price for existing single family homes from the National Association of Realtors. Income and poverty are from the 2008-2012 American Community Survey 5-Year Estimates. National SNAP enrollment is from the SNAP Research and Analysis Division of the USDA. Regional SNAP enrollment is from monthly county totals provided by the California Department of Social Services, Georgia Division of Family and Children Services, and the Ohio Department of Job and Family Services. SNAP enrollment is for the 2013 calendar year, except for Atlanta, where enrollment is during the 2013 fiscal year which runs from July 1, 2012 to June 30, 2013. Columbus is Columbus, Ohio.

and Schanzenbach 2014).[18] Therefore, mean expenditures per store for SNAP households in my sample accounts for roughly 50% to 60% of monthly expenditure on food consumed at home by low income households.

Rows 4-9 in Table 2.1 disaggregate spending by store department. The largest department is the grocery department, which includes food such as cereal, snacks, dairy, frozen foods, and drinks as well as some non-food items such as pet food, cleaning products, and toilet paper. The meat department includes meat and fish. The deli and bakery department includes products such as deli meats and cheeses and baked goods. The drug and general merchandise department includes automotive products, cosmetics, health and beauty care, housewares, toys, and candy. The other departments are natural foods and produce. The allocation of expenditures across store departments is nearly identical across household type.

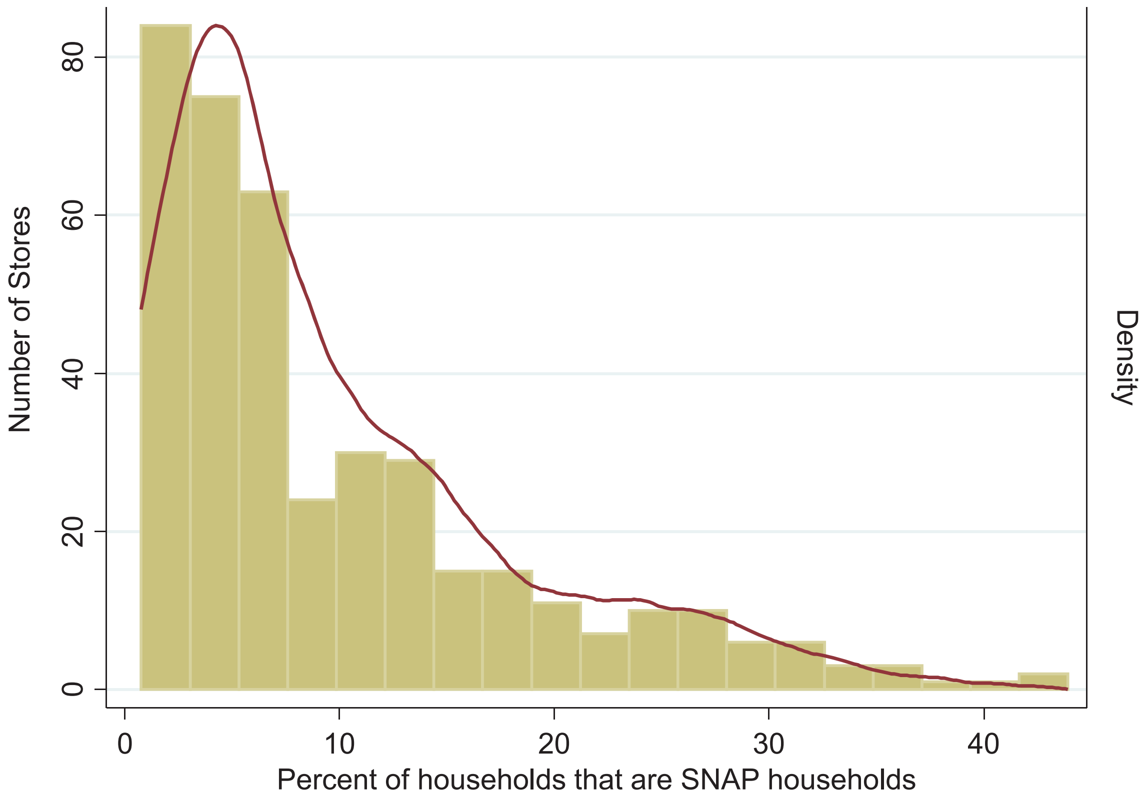
It is sometimes helpful to focus on food spending for comparability with previous studies. To do so, I exclude sales within the drug and general merchandise department from total spending, even though non-food items are also included in some of the other categories. Using this working definition, total expenditures on food by SNAP households are roughly in line with what one would expect from food spending reported in Hastings and Washington (2010, table 1), but are quite a bit lower for non-SNAP households.

Panel B presents summary statistics for the stores in the sample by region. More than half the stores are located in Los Angeles, 32% are in Atlanta, and 15% are in Columbus. About 10% of the households who shop at these stores each month are SNAP households. However, there is substantial variation in the fraction of SNAP households across stores. The stores in Atlanta and Columbus are slightly larger and have higher SNAP shares than the stores in Los Angeles.

Figure 2.5 shows the distribution of $\overline{SNAP}_k$ calculated over the period January 2012 to April 2013. The range is 3% to about 40%, with most of the mass below 15%. My research

[18] All types of stores refers to grocery stores, convenience stores, drug stores, and supercenters/warehouse club stores in Damon, King, and Leibtag (2013). I convert the 7 day average expenditure reported by those authors in column 1 of Table 2 to monthly expenditures measured in 2013 dollars.

Figure 2.5: *Distribution of SNAP Customer Share Across Stores in Sample*



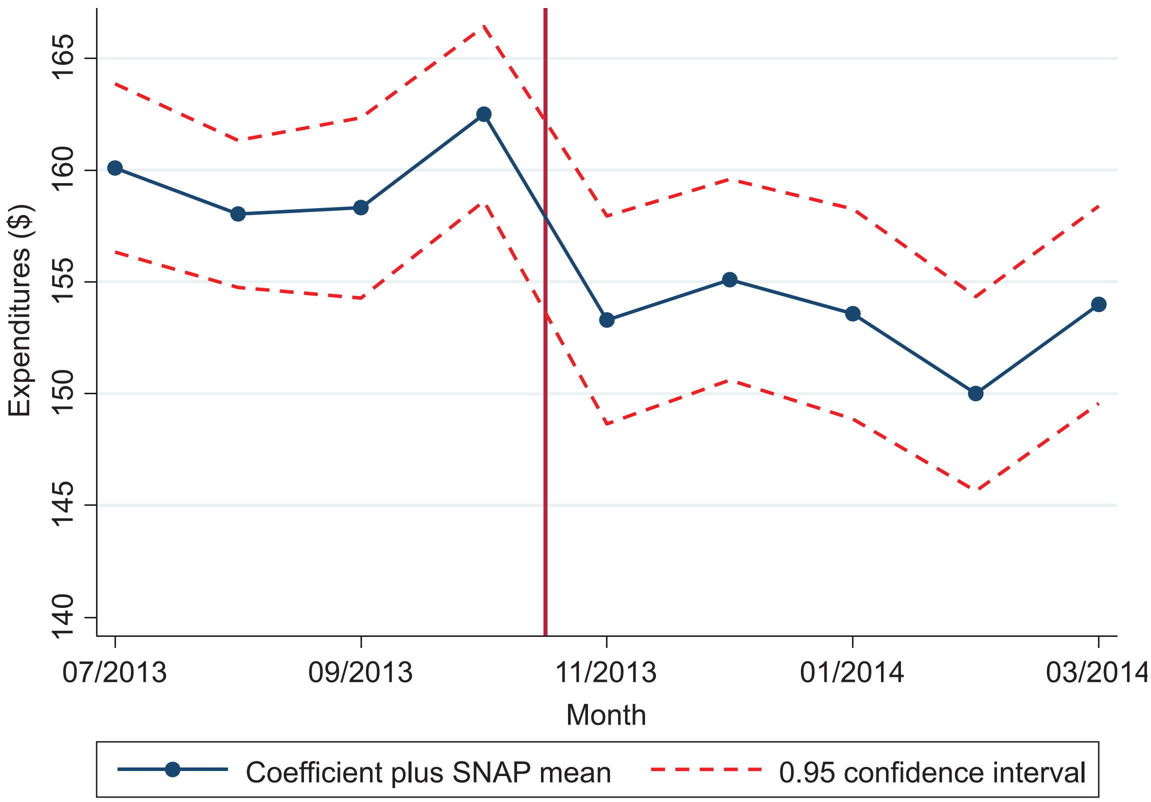
NOTE—This figure shows the distribution of $\overline{SNAP}_k$, the share of customers at stores $k = 1, \ldots, K$ that use SNAP. The variable is constructed as follows. For each month $t = 1, \ldots, T$, I compute the fraction of households who shopped at store k that used SNAP to pay for more than half of at least one purchase. $\overline{SNAP}_k$ is the average of these monthly observations from January 2012 to April 2013 for store k.

design relies upon this variation to identify the impact of the expiration of the ARRA policies.

2.5 Results

In section 5.1, I first discuss my main estimates of the effect of the 2013 SNAP benefit cuts on household expenditures. In section 5.2, I use my point estimates to calculate estimates of the MPC, the incidence of SNAP benefits on grocery retailers, and the aggregate impact of the November 2013 SNAP benefit cuts. Section 5.3 presents supporting evidence. Finally,

Figure 2.6: *Estimated Effect of Expiration of ARRA on SNAP Household Expenditures*



NOTE—This figure plots the coefficients and 95% confidence intervals on the interaction terms between the fraction SNAP and the July 2013 through March 2014 month indicators from column 4 of Table 2.2. I scale the y-axis by adding the mean SNAP household monthly expenditures from Table 2.1 to each coefficient. The red line signifies when the ARRA expired in November 2013.

in section 5.4, I present estimates separately by region and show that the treatment effect was larger in Atlanta than in Los Angeles or Columbus. I discuss possible interpretations.

2.5.1 Effect of ARRA expiration on expenditures

The main results from estimating Equation (2.2) are shown in Table 2.2 and Figure 2.6. The dependent variable is the twelve month change in expenditures per household at each store. The results in Table 2.2 show that the point estimates are robust across specifications.

The model in column 1 of Table 2.2 is a version of Equation (2.2) that includes interaction terms between region and the post November 2013 indicator and between region and the

Table 2.2: *Effect of ARRA Expiration on SNAP Household Expenditures*

Dependent variable:	12 month change in sales per HH, All regions					
	($)	($)	($)	($)	($)	($)
	(1)	(2)	(3)	(4)	(5)	(6)
Fraction SNAP HHs	**-7.873*** **					
	(2.725)					
Post November 2013	**3.370*** **	**3.369*** **	**5.003*** **		**2.819*** **	
	(0.369)	(0.388)	(0.432)		(0.379)	
Fraction SNAP HHs x Post November '13	**-5.915*** **	**-5.905*** **	**-6.299*** **	**-5.960*** **	**-6.138*** **	
	(1.268)	(1.331)	(1.532)	(1.334)	(1.352)	
Fraction SNAP HHs x June '13						**-6.985*** **
						(2.144)
Fraction SNAP HHs x July '13						**-0.615**
						(1.924)
Fraction SNAP HHs x August '13						**-2.678**
						(1.685)
Fraction SNAP HHs x September '13						**-2.396**
						(2.066)
Fraction SNAP HHs x October '13						**1.807**
						(1.997)
Fraction SNAP HHs x November '13						**-7.403*** **
						(2.381)
Fraction SNAP HHs x December '13						**-5.612** **
						(2.302)
Fraction SNAP HHs x January '14						**-7.136*** **
						(2.408)
Fraction SNAP HHs x February '14						**-10.71*** **
						(2.232)
Fraction SNAP HHs x March '14						**-6.731*** **
						(2.265)
Fraction SNAP HHs x April '14						**-9.164*** **
						(2.335)
Store fixed effects		x	x	x	x	x
Month fixed effects				x		x
Controls for region	x	x	x	x	x	x
Linear time trend x store fixed effects			x			
Number of months	12	12	12	12	4	12
Number of stores	395	395	395	395	395	395
Store x month observations	4739	4739	4739	4739	1580	4739

NOTE -- Each column reports results from weighted least squares regressions where the dependent variable is the twelve month change in total monthly sales per household. Standard errors are clustered by store and are reported in parentheses below each coefficient estimate. May 2013 is the excluded month in column 4. Regressions are estimated over the twelve month period from May 2013 to April 2014, except in column 3 which restricts the sample to September 2013 through December 2013. The fraction SNAP is the average from January 2012 to April 2013 of the monthly fraction of all households shopping at each store that used SNAP to pay for more than half of at least one purchase that month. The regressions are weighted by the number of households shopping at each store each month (averaged across the two months that are differenced). Region controls in column 1 consist of indicator variables for region and interactions of region with the fraction SNAP and with the post November 2013 indicator. In other columns, region controls consist of the region indicator variables interacted with post or month indicator variables because these columns include store fixed effects. *** p<0.01, ** p<0.05, * p<0.1

SNAP share variable. This model therefore is flexible in that it does not restrict time trends to be the same in each region and allows the fraction of SNAP households to have different mean impacts in each region. Columns 2, 3, 4, and 5 add store fixed effects (and therefore drop variables included in column 1 that are constants for each store). Columns 1 through 4 are estimated over the twelve month period from May 2013 to April 2014.

The point estimate in Column 1 is that SNAP household monthly spending declined by \$5.92 in the months after the SNAP benefit cuts. Column 2 shows that adding store fixed effects only decreases the point estimate by \$0.01. Adding a linear time trend interacted with the store indicator variables in Column 3 increases the coefficient to \$6.30, while adding month fixed effects interacted with the region indicators in Column 4 increases the point estimate slightly to \$5.96. Column 5 restricts the regression in Column 2 to the two months before and two months after the benefit cuts (September through December). This column addresses the issue of the work requirements being reinstated in Ohio, which only resulted in beneficiaries losing eligibility as early as January 2014.[19] The point estimate is only slightly larger at \$6.14. Coefficients in all five of these columns are statistically significantly different than zero at the 1% level.

Column 6 of Table 2.2 presents a more non-parametric version of Equation (2.2) that includes separate indicator variables for each month and their interactions with $\overline{SNAP}_k$. The excluded month is May so the coefficients on the other indicator variables measure changes relative to May. The coefficients on the interaction terms with the month indicators for November through April are all negative and larger in magnitude than those for the six months before. The one exception is the coefficient on the interaction term with the June indicator variable, which is negative and large, but still smaller than most of the post November 2013 indicators.

Figure 2.6 plots the coefficients for July 2013 through March 2014. To scale the y-axis, I add back mean SNAP household expenditures from Table 2.1 to each coefficient. The

[19]This column also excludes the period after which extended unemployment insurance benefits expired, which happened in January 2014.

series is stable for the four months preceding the benefit cut, drops in November 2013, and remains lower, suggesting that the timing of the decline in spending is consistent with being caused by the expiration of the ARRA and permanently lower SNAP benefit levels.

Table 2.3 explores this decrease in spending in more detail. Each column estimates the same specification as in Column 2 of Table 2.2 but changes the dependent variable to be the 12-month change in spending per household on items within each store department. These estimates can be expressed as a fraction of mean SNAP household spending in each department from Table 1 or as a fraction of the estimated drop in total store spending. Both are shown in the last two rows of the table. The estimates by store department show that about 3/4 of the decline was to spending on grocery items and meat. 16% was from spending on non-food general merchandise and over the counter drugs. One concern that is often voiced is that lower SNAP benefits may adversely impact consumption of healthy foods such as fresh fruit and vegetables, but both ways of expressing the point estimate indicate that spending on produce was not greatly affected by the November 2013 cuts.

2.5.2 Policy calculations

To put these estimates into context, it is helpful to scale the estimated change in household expenditure by the decrease in SNAP benefits. Scaling the estimates in this way produces an estimate of the marginal propensity to consume out of food stamps, where consumption refers to consumption of items purchased at the grocery store. I also inflate the $5.91 point estimate by a factor of 5/4 to reflect that households shop at multiple stores.[20]

Between October and November 2013, average benefits decreased by $20 per household in California, Georgia, and Ohio, implying a marginal propensity to consume out of SNAP benefits of 0.37 with a 95% confidence interval of [0.206, 0.532]. The literature usually focuses on food spending. Excluding over the counter drugs and general merchandise from the change in expenditures yields an estimate of the marginal propensity to consume food

[20] I inflate by 5/4 because mean expenditure in my sample is equal to 80% of total grocery store expenditures (Damon, King, and Leibtag 2013).

Table 2.3: *Effect of ARRA Expiration on SNAP Household Expenditures by Store Department*

Dependent variable:	12 month change in sales per household, All regions					
Department:	Produce	Grocery	Meat	Drug/GM	Deli/Bakery	Natural Foods
	($)	($)	($)	($)	($)	($)
	(1)	(2)	(3)	(4)	(5)	(6)
Post November 2013	**0.0460**	**1.927*** **	**0.847*** **	**0.362*** **	**0.0623**	**0.169*** **
	(0.0727)	(0.186)	(0.0675)	(0.0840)	(0.0526)	(0.0543)
Fraction SNAP HHs x Post Nov. '13	**-0.0652**	**-3.138*** **	**-1.385*** **	**-1.022*** **	**-0.389** **	**-0.283* **
	(0.246)	(0.778)	(0.312)	(0.237)	(0.186)	(0.152)
Store fixed effects	x	x	x	x	x	x
Controls for region	x	x	x	x	x	x
Number of months	12	12	12	12	12	12
Number of stores	395	395	395	395	395	395
Store x month observations	4739	4739	4739	4738	4737	4687
Mean sales per month in department for SNAP households (Table 1)	**$15.70**	**$89.54**	**$23.32**	**$17.11**	**$10.48**	**$4.04**
Point estimate/SNAP mean (Table 1)	**-0.42%**	**-3.50%**	**-5.94%**	**-5.97%**	**-3.71%**	**-7.00%**
Point estimate/Sum of coefficients	**1.04%**	**49.95%**	**22.05%**	**16.27%**	**6.19%**	**4.50%**

NOTE -- Each column reports results from weighted least squares regressions where the dependent variable is the twelve month change in total monthly sales per household. Standard errors are clustered by store and are reported in parentheses below each coefficient estimate. Regressions are estimated over the twelve month period from May 2013 to April 2014. The fraction SNAP is the average from January 2012 to April 2013 of the monthly fraction of all households shopping at each store that used SNAP to pay for more than half of at least one purchase that month. The regressions are weighted by the number of households shopping at each store each month (averaged across the two months that are differenced). Region controls consist of indicator variables for region interacted with the post November 2013 indicator. *** p<0.01, ** p<0.05, * p<0.1

out of SNAP benefits of 0.30. The 95% confidence interval is [0.154, 0.456], which is 80% to half as wide as the confidence intervals reported in Hoynes and Schanzenbach (2009, table 6).

Under some stylized assumptions, the above calculations lead to an estimate of the incidence of the November 2013 benefit cuts (and of a marginal dollar of SNAP benefits more generally) on grocery retailers. The five year average cost of goods sold (excluding depreciation and amortization) for five of the largest publicly traded retailers in the U.S. grocery retailing industry was 75% of revenue. I assume that the marginal cost of these foregone sales is equal to this average COGS/revenue ratio. This implies that if each \$1 in SNAP cuts led to a \$0.37 decline in sales, $\$0.37 \times (1 - 0.75) = \$0.09$ would have been before tax profits. At a marginal corporate tax rate of 35%, after tax profits would have been about 65% of this, or \$0.06. These calculations imply that just 6% of the economic incidence of a marginal dollar of SNAP benefits accrues to grocery retailers. The other 94% accrues to other agents in the economy.

One can also use these estimates to calculate the aggregate effect of the November 2013 SNAP benefit cuts on industry-wide grocery store spending. My estimates imply that total annual grocery store revenue will decline by \$5.91 $\times$ $5/4$ $\times$ 23 million households $\times$ 12 months = \$2.039 billion. To put this number in perspective, the U.S. grocery retailing industry is a \$600 billion a year market (Mintel 2012), so the November 2013 cuts caused a 0.3% reduction in aggregate revenue for this industry as a whole, with a range of 0.19% to 0.49% using the lower and upper endpoints of the 0.95 confidence interval for the point estimate. Using the same COGS/revenue ratio of 0.75 as before, this implies a decrease in profits of \$510 million industry wide.

2.5.3 Method of payment and shopping frequency

This section presents two sets of supporting evidence.

Method of payment. The estimates of the marginal propensity to consume out of food stamps deserve some further comment. If a household lost \$20 in SNAP benefits, then a

MPC of 0.37 implies that it would reallocate resources in order to offset $12.70 of those lost benefits. One possibility would be to use cash to pay for products purchased at the grocery store. In this way, a piece of supporting evidence would be an increase in grocery store spending by SNAP households using methods of payment other than with their SNAP EBT card in the months after SNAP benefits were cut.

While I do not observe spending by SNAP households by method of payment directly, I am able to construct a measure as follows. For each store, I observe two sets of totals of expenditures by SNAP household. The first is SNAP household expenditures when more than half of the purchase was paid for with SNAP tender. The second are all other purchases by these households where this was not the case. I define the cash spending by SNAP households as this second measure plus the part of the first measure that was in the drug and general merchandise category, most of which by rule cannot be paid for with SNAP benefits.[21]

To test the prediction that SNAP households increased cash spending when their SNAP benefits declined, Table 2.4 presents estimates from equations of the following form:

$$\Delta y_{kt} = \alpha + \delta post_t + \psi X_{kt} + u_{kt} \tag{2.3}$$

where $post_t$ is an indicator variable for whether the month is November 2013 or later, X_{kt} is a vector of controls, and the dependent variable is cash expenditures by SNAP households either in dollars (columns 1 through 3) or as a percentage of total expenditures (columns 4 through 5). I take twelve month differences to address seasonality. The coefficient δ simply measures whether my measure of cash spending was higher in months following November 2013. There is no natural control group here since only SNAP households can pay with SNAP benefits.

The results in Table 2.4 suggest that SNAP households did increase their spending using other methods of payment when SNAP benefits were reduced in November 2013. In

[21]Note that the drug and general merchandise department includes candy, which can be paid for with SNAP tender. But most products in this department cannot be paid for with SNAP tender (e.g., automotive products, cosmetics, health and beauty care, housewares, and toys).

Table 2.4: *SNAP Household Expenditures using Cash as Method of Payment*

Dependent variable:	Δ spending w/ cash per SNAP HH			Δ cash share of SNAP HH spending		
	($)	($)	($)	(%)	(%)	(%)
	(1)	(2)	(3)	(4)	(5)	(6)
Mean of cash spending:	*$74.38*			*47.38%*		
[Standard deviation]	*[25.36]*			*[7.54]*		
Post November 2013	**4.156*****	**4.162*****		**2.033*****	**2.038*****	
	(0.241)	(0.251)		(0.0915)	(0.0957)	
June 2013			**-1.090*****			**-0.337*****
			(0.338)			(0.122)
July 2013			**-2.921*****			**-1.166*****
			(0.339)			(0.115)
August 2013			**1.886*****			**0.286*****
			(0.312)			(0.106)
September 2013			**-0.549**			**-0.0745**
			(0.334)			(0.153)
October 2013			**-3.326*****			**-1.344*****
			(0.341)			(0.116)
November 2013			**2.891*****			**1.729*****
			(0.376)			(0.136)
December 2013			**0.844****			**0.904*****
			(0.389)			(0.142)
January 2014			**5.689*****			**2.233*****
			(0.483)			(0.160)
February 2014			**6.409*****			**2.335*****
			(0.475)			(0.157)
March 2014			**-0.634**			**0.195**
			(0.396)			(0.133)
April 2014			**4.206*****			**2.309*****
			(0.435)			(0.128)
Store fixed effects		x	x		x	x
Controls for region	x			x		
Number of months	12	12	12	12	12	12
Number of stores	395	395	395	395	395	395
Store x month observations	4739	4739	4739	4739	4739	4739

NOTE -- Cash refers to all methods of payment other than with SNAP benefits. Each column reports results from weighted least squares regressions where the dependent variable is the twelve month change in total monthly cash sales per SNAP household or the fraction of sales to SNAP households that are paid for with cash. See section 5.3 for details on construction of the dependent variables. Standard errors are clustered by store and are reported in parentheses below each coefficient estimate. May 2013 is the excluded month in column 3 and 6. Regressions are estimated over the twelve month period from May 2013 to April 2014. The regressions are weighted by the number of SNAP households shopping at each store each month (averaged across the two months that are differenced). Region controls consist of indicator variables for region. *** p<0.01, ** p<0.05, * p<0.1

Table 2.5: *Effect of ARRA Expiration on SNAP Household Shopping Frequency*

Dependent variable:	12 month change in visits per HH, All regions			
	(1)	(2)	(3)	(4)
Fraction SNAP HHs	**0.201*** **		**0.172** **	
	(0.0622)		(0.0747)	
Post November 2013	**-0.00578**	**-0.00583**	**0.00952**	**0.00888**
	(0.00905)	(0.00956)	(0.00724)	(0.00834)
Fraction SNAP HHs x Post November '13	**-0.173*** **	**-0.173*** **	**-0.0586* **	**-0.0578**
	(0.0458)	(0.0478)	(0.0317)	(0.0364)
Store fixed effects		x		x
Controls for region	x	x	x	x
Number of months	12	12	4	4
Number of stores	395	395	395	395
Store x month observations	4739	4739	1580	1580

NOTE -- Each column reports results from weighted least squares regressions where the dependent variable is the twelve month change in total visits to the store per household. Standard errors are clustered by store and are reported in parentheses below each coefficient estimate. Regressions in columns 1 and 2 are estimated over the twelve month period from May 2013 to April 2014. Columns 3 and 4 restrict the sample to September 2013 through December 2013. The fraction SNAP is the average from January 2012 to April 2013 of the monthly fraction of all households shopping at each store that used SNAP to pay for more than half of at least one purchase that month. The regressions are weighted by the number of households shopping at each store each month (averaged across the two months that are differenced). Region controls in columns 1 and 3 consist of indicator variables for region and interactions of region with the fraction SNAP and with the post November 2013 indicator. In other columns, region controls consist of the region indicator variables interacted with post indicator variable because these columns include store fixed effects. *** p<0.01, ** p<0.05, * p<0.1

columns 1-2 and columns 4-5, the coefficients on the post-November 2013 indicator are all positive, with and without store fixed effects. Less parametric versions of Equation (2.2) that include indicator variables for each month are presented in Columns 3 and 6. The coefficients are negative and small before November, but turn positive and larger afterwards. Therefore, to the degree that I can measure it, SNAP households did increase their spending using cash to partially offset their lower SNAP benefits. These estimates support the interpretation of the main set of results.

Shopping frequency. Table 2.5 presents estimates of the impact of the reduced benefits on shopping frequency. I view this set of results as a placebo test, because the relatively small change in benefits should not have a large impact on visits to the store. However, I could possibly find large (positive or negative) effects on shopping frequency if the estimator is biased, which would indicate that my estimates for spending are also biased. Reassuringly, this is not what I find.

The dependent variable in Table 2.5 is the twelve month change in number of visits to the store per household. Columns 1 and 2 are estimated over the twelve month period from May 2013 to April 2014 and Columns 3 and 4 are restricted to the four month period from September 2013 to December 2013. The point estimates in all columns are negative, but very small. The key conclusion from these results, and those above, is that any remaining sources of bias would have to be caused by something occurring contemporaneously with the November 2013 benefit cuts, but not something that would cause households to change their shopping frequency.

2.5.4 Effect of ARRA expiration on expenditures by region

Table 2.6 presents estimates by region. Columns 1 and 2 are versions of Equation (2.2) that are fully interacted with the region indicator variables. Column 2 includes store fixed effects. The rest of the columns show results from models that are estimated separately for each of the three regions. The point estimate for Atlanta is about \$9.00, while the point estimates are smaller and imprecisely estimated in Los Angeles at \$2.97 and in Columbus at \$2.34. In columns 1 and 2, coefficients on the triple interaction terms correspond exactly to the difference in these treatment effects across regions. An F-test on these triple interaction terms indicates that the difference in treatment effects across regions is only marginally statistically significant, with a p-value of 4.4% in Column 1 and 5.6% when adding the store fixed effects in Column 2.

One explanation for these differences across regions is statistical. The cross-store estimator in Equation (2.2) is particularly well suited for estimating the treatment effect in Atlanta, and has less power in Los Angeles and Columbus. To see why, note that the standard deviation of $\overline{SNAP}_k$ in Panel B of Table 2.1 is larger in Atlanta than in Los Angeles. The standard deviation is greatest in Columbus, but the sample size in Atlanta is twice as large.

Table 2.6: *Effect of ARRA Expiration on SNAP Household Expenditures by Region*

Dependent variable: 12 month change in sales per household											
Region:	All regions		Los Angeles, CA			Atlanta, GA			Columbus, OH		
	($)	($)	($)	($)	($)	($)	($)	($)	($)	($)	($)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Fraction SNAP HHs	**-9.672*** **		**7.906**			**-10.36*** **			**-9.672*** **		
	(3.231)		(6.188)			(1.778)			(3.257)		
Post November 2013	**2.934*** **	**2.943*** **	**-0.919*** **	**-0.939*** **		**2.037*** **	**2.032*** **		**2.934*** **	**2.943*** **	
	(0.515)	(0.545)	(0.199)	(0.210)		(0.289)	(0.299)		(0.519)	(0.549)	
Fraction SNAP HHs x Post Nov. '13	**-2.288**	**-2.362**	**-3.194**	**-2.970**		**-8.982*** **	**-9.013*** **		**-2.288**	**-2.362**	
	(2.578)	(2.719)	(2.516)	(2.667)		(1.745)	(1.826)		(2.598)	(2.739)	
Fraction SNAP HHs x Post Nov. '13 x Atlanta, GA	**-6.694** **	**-6.651** **									
	(3.110)	(3.273)									
Fraction SNAP HHs x Post Nov. '13 x Los Angeles, CA	**-0.906**	**-0.607**									
	(3.601)	(3.807)									
Fraction SNAP HHs x June '13					**11.78*** **			**-12.35*** **			**-10.93*** **
					(3.246)			(2.810)			(2.516)
Fraction SNAP HHs x July '13					**13.99*** **			**-4.100* **			**-4.826* **
					(3.866)			(2.357)			(2.576)
Fraction SNAP HHs x August '13					**6.489** **			**-6.569** **			**-2.173**
					(3.033)			(2.530)			(2.131)
Fraction SNAP HHs x September '13					**12.75*** **			**-7.933*** **			**-3.333**
					(3.214)			(2.482)			(3.748)
Fraction SNAP HHs x October '13					**12.19*** **			**-3.922* **			**4.785**
					(3.031)			(2.355)			(4.264)
Fraction SNAP HHs x November '13					**9.387** **			**-15.64*** **			**-4.568**
					(3.849)			(2.965)			(4.531)
Fraction SNAP HHs x December '13					**10.89** **			**-14.54*** **			**-1.294**
					(4.415)			(2.716)			(3.774)
Fraction SNAP HHs x January '14					**10.07** **			**-12.80*** **			**-9.342** **
					(4.987)			(3.216)			(3.584)
Fraction SNAP HHs x February '14					**4.885**			**-18.68*** **			**-7.436* **
					(4.862)			(2.675)			(3.976)
Fraction SNAP HHs x March '14					**2.960**			**-11.13*** **			**-5.711**
					(4.284)			(2.709)			(5.272)
Fraction SNAP HHs x April '14					**0.708**			**-16.38*** **			**-3.176**
					(4.172)			(3.070)			(5.142)
Controls for region	x	x									
Store fixed effects		x		x	x		x	x		x	x
Month fixed effects					x			x			x
Number of months	12	12	12	12	12	12	12	12	12	12	12
Number of stores	395	395	210	210	210	125	125	125	60	60	60
Store x month observations	4739	4739	2519	2519	2519	1500	1500	1500	720	720	720
p-value for test of joint significance of triple interaction terms	0.045	0.0557									

NOTE -- Each column reports results from weighted least squares regressions where the dependent variable is the twelve month change in total monthly sales per household. Standard errors are clustered by store and are reported in parentheses below each coefficient estimate. May 2013 is the excluded month in columns 5, 8, and 11. Regressions are estimated over the twelve month period from May 2013 to April 2014. The fraction SNAP is the average from January 2012 to April 2013 of the monthly fraction of all households shopping at each store that used SNAP to pay for more than half of at least one purchase that month. The regressions are weighted by the number of households shopping at each store each month (averaged across the two months that are differenced). *** p<0.01, ** p<0.05, * p<0.1

Because the precision of this estimator is increasing with more variation in $\overline{SNAP}_k$ and with the number of stores in the sample, other research designs may be better suited for estimating the treatment effect in Los Angeles and Columbus.

More interesting but speculative explanations have to do with differences across regions. Panel C of Table 2.1 presents descriptive statistics for the United States and each of the three regions. Atlanta differs from Los Angeles and Columbus along many dimensions, including demographics and cost of living. One could test which factor is most strongly associated with larger observed effects using more detailed information on the locations of the stores within each region or potentially using household level data. In the conclusion, I discuss the importance of extending the results in this direction.

Perhaps the most salient difference across regions in Panel C is that the share of the population receiving SNAP had been much higher in Atlanta and was dropping precipitously at the end of 2013 and beginning of 2014. Between October 2013 and November 2013, the unemployment rate declined in Atlanta by 9% while the number of households receiving SNAP fell by 5%. In Los Angeles and Columbus, both figures were essentially stable. Including the zero SNAP benefits received by these 20,000 households in Atlanta would imply that benefits per household declined by \$30.42 between October and November, but similar calculations imply benefits only fell by around \$22 to \$23 in Columbus and Los Angeles. In this way, one potential explanation for the larger observed effects in Atlanta is that the treatment was larger in Atlanta. However, it is not clear that these changes in participation in Atlanta are what is driving the larger estimated decreases in household expenditures, especially if these households were no longer eligible to receive SNAP benefits because their household income increased, as is suggested by the improved labor market conditions and the absence of changes to eligibility criteria across those months. On the other hand, if food consumed at home is complementary with leisure, while food consumed away from home is complementary with work (e.g., Aguiar and Hurst 2005), then shifting from SNAP rolls to employment could be an explanation for the large observed decreases in spending in the Atlanta sample.

Another possibility is that the decline in participation is due to several recent administrative issues in the processing of SNAP cases in Georgia. For example, in November some households were not notified or received delayed notifications that they were required to submit documents to recertify their benefits.[22] However, households who were affected were allowed extra time (until November 25) to submit the documents and receive full November SNAP benefits. Further, participation continued to decline in December, which is inconsistent with the decline in participation in November being entirely a temporary result of the delayed notifications. Finally, the coefficient on the November 2013 interaction term in Column 8 of Table 2.5 is of a similar size as the coefficient on the December 2013 interaction term, which is inconsistent with the treatment effect in November being caused by any temporary lapses in benefits.

2.6 The 2014 Farm Bill

In this section, I discuss the 2014 Farm Bill and show how one can use my estimates to project what its effect will be on household spending. In the months before and after the ARRA benefit increases expired, Congress debated further cuts to the program. The 2014 Agricultural Act that was passed and became law in February 2014 included some, but not all, of these proposed cuts. A major component of the law involved rules for SNAP beneficiaries who also receive Low-Income Home Energy Assistance Program (LIHEAP) payments. In January 2014, the CBO estimated this part of the law would reduce SNAP costs by $3.73 billion between 2014 and 2018. Other parts of the law affected small subgroups such as lottery winners and convicted felons, which are not likely to have large aggregate impacts.

[22] These issues have been reported on by the Georgia media. An example is the article "Many see delays in benefit renewals from DFACS" published on Nov 18, 2013 by Alyssa Hyman/WTOC-TV.

Recall the formula from section 2.2 that determines SNAP benefit amounts:

$$B(N, Z, Y, D) = \begin{cases} B_0(N) - 0.3(Y + 0.8Z - D) & \text{if } 0.8Z + Y > D \\ B_0(N) & \text{otherwise} \end{cases}$$

where $\$B_0(N)$ is the maximum that a household of size N can receive, $\$Z$ is earned income, $\$Y$ is unearned income, and $\$D$ are deductions. The 2014 Farm Bill reduced SNAP benefits by changing the rules for calculating the deductions that SNAP households can take, but only for a subset of SNAP households who also receive payments from their state's LIHEAP. I describe these changes in the following paragraph.

As a general rule, SNAP households whose shelter related costs are greater than half of income less deductions are allowed to take an additional deduction called the Excess Shelter Expense Deduction.[23] If a household has enough other income, a higher value for this deduction increases the household's monthly SNAP benefits. Prior to the 2014 law, SNAP beneficiaries who received any support from LIHEAP could use a flat amount (a standard utility allowance) for their utility costs, rather than their actual utility costs, in the computation of their total shelter costs. There were 15 states, which are listed in Table 2.7, that awarded very low LIHEAP payments (e.g., \$1 per year in Massachusetts and \$0.10 per year in California) in order to increase their residents' SNAP benefit amounts by making them eligible for the Excess Shelter Expense Deduction (Aussenberg and Perl 2013). The 2014 law set a minimum LIHEAP payment of \$20 per year in order for households to be eligible to use the flat utility amount in this calculation.

The CBO estimated in September 2013 that creating a \$20 minimum LIHEAP payment for using the standard utility allowance would reduce 850,000 SNAP households' benefits by \$90 a month on average each year (CBO 2013). The estimates in this paper imply that this decline in benefits would result in a $\$90 \times 0.3 = \$27$ per household decrease in monthly spending on food and a $\$90 \times 0.37 = \$33.30$ per household decrease in monthly total store

[23] The amount that can be deducted is equal the amount that a household's shelter related costs exceed half of its income less deductions (i.e., its total shelter related costs minus $0.5 \times \{Y + 0.8Z - D\}$), up to a maximum of \$478 in 2014. This deduction could therefore increase monthly SNAP benefits by up to $0.3 \times \$478 = \$143.40$. There is no maximum for elderly and disabled households (Farson Gray and Eslami 2014).

Table 2.7: *Implied Effect of 2014 Agricultural Act on Household Expenditures*

Panel A. By state (total and per household)

State	Decision to Increase LIHEAP in Response to 2014 Agricultural Act?	Total Annual SNAP cuts (millions)	Households affected	Changes per household per month: SNAP benefits	Food spending	Total store spending
(1)	(2)	(3)	(4)	(5)	(6)	(7)
1. California	Yes.	$223	300,000	$62	$19	$23
2. Connecticut	Yes.	$66	50,000	$112	$34	$41
3. Maine		$7	5,500	$106	$32	$39
4. Massachusetts	Yes.	$142	163,000	$73	$22	$27
5. Michigan		$250	235,000	$88	$27	$32
6. Montana	Yes.	$2	2,000	$83	$25	$31
7. New Jersey		$172	157,000	$91	$28	$34
8. New York	Yes.	$457	300,000	$127	$39	$47
9. Oregon	Yes.	$56	78,000	$58	$18	$21
10. Pennsylvania	Yes.	$300	400,000	$65	$20	$24
11. Rhode Island	Yes.	$69	69,000	$83	$25	$31
12. Vermont	Yes.	$14	19,400	$60	$18	$22
13. Washington	Yes.	$70	200,000	$29	$9	$11
14. Washington, DC	Yes.					
15. Wisconsin		$276.20	255,000	$90	$27	$33

Panel B. Totals across states

	SNAP Cuts	Households affected	Δ Food Spending	Δ Store Spending
	(1)	(2)	(3)	(4)
1. Total with no state response (millions per year)	$2,104	2.234	$644	$780
2. Total for ME, MI, NJ, and WI (millions per year)	$705	0.653	$214	$260

NOTE -- States listed in column 1 of Panel A reported in a Congressional Research Services Report (Aussenberg and Perl 2013) as using "heat and eat" policies to increase SNAP benefits; Delaware is excluded because it no longer does this as of 2012. Information in column 2 is from the Food Research and Action Center and the National Conference of State Legislatures. Information in columns 3-5 are from press releases. Relevant numbers for Washington, DC have not been reported. Columns 6-7 use estimates from Section 5.2 to forecast changes in spending. Column 6 uses the MPC for food of 0.3 and Column 7 uses the MPC for total grocery store spending of 0.37.

spending. These calculations imply annual aggregate reductions of $230 million in spending on food and $283 million in total grocery store spending. These numbers represent just a 0.05% reduction total industry revenue.

However, as shown in Column 2 of Table 2.7, all but four of the potentially affected states have since implemented policies to prevent SNAP beneficiaries from being impacted. Columns 3-5 of Table 2.7 present the number of households affected and dollar amounts from press releases for each of these states. Assuming no state response, the implied decreases in food and total store spending are $644 million per year and $780 million per year, respectively. The same estimates are $214 million and $260 million per year once I restrict the estimates to the four states that have not change their policies in response to the 2014 Farm Bill. These numbers are 1/3 of what they would have been if no state changed its LIHEAP payments.

The numbers from these press releases likely over estimate the scope of the bill. It is clear then that the 2014 Farm Bill is not likely to have a large impact on aggregate household food expenditure or retail sales once these state policy responses and modest consumer behavioral responses are taken into account. The reason that these aggregate impacts are so much smaller than the impact of the November 2013 benefit cuts calculated in section 2.5.2 is because the number of people affected by the 2014 Farm Bill is much smaller. Indeed, both the press releases and the CBO forecasts are in agreement that the households who will be affected by the 2014 Farm Bill stand to lose significantly more SNAP benefits than they did in November 2013.

2.7 Conclusion

I estimate that the November 2013 SNAP benefit cuts resulted in a $5.91 decline in monthly SNAP household spending, implying that each $1 of cuts led to $0.37 in less grocery store spending. The aggregate impact is estimated to be 0.3% of total industry revenue. In contrast, the aggregate impact of the SNAP benefit cuts associated with the 2014 Farm Bill is projected to be an order of magnitude lower.

My results imply that the marginal propensity to consume food out of food stamps at 0.3 is about half to 1/3 the size of the largest estimates from the cross-sectional empirical literature from the 1970s and 1980s. In contrast, my estimates are consistent with but more precise than those of Hoynes and Schanzenbach (2009, table 6) based on the initial roll out of the food stamp program in the 1960s and 1970s. The methodological contribution of this paper is to show how scanner data and partnering with industry can facilitate the timely evaluation of policy, while at the same time can provide sample sizes and allow for research designs that generate enough power to identify the impact of even relatively small treatments. Indeed, it is questionable whether a \$5.91 treatment effect would be detectable in data that have a significantly higher degree of measurement error than those used here.

The finding that households increased their spending using non-SNAP dollars while cutting back on both food and non-food items is consistent with standard economic theory. The textbook analysis of SNAP benefits implies that (inframarginal) households should treat a \$1 decrease in SNAP benefits in the same way that they would treat losing \$1 of cash income. My point estimates support this prediction. For example, my estimate of the MPC out of food stamps is entirely consistent with estimates of the MPC out of (cash) unemployment insurance benefits (e.g., 0.21 to 0.27 on page 25 of Gruber 1996). This observation suggests that including both unemployment insurance benefit extensions and SNAP benefit increases as part of future fiscal stimulus packages may be a good strategy in order to increase domestic spending since each dollar of support from both programs lead to similar changes in expenditure.[24] Note though that since my estimates are limited to grocery store spending, SNAP benefits may actually be more effective than unemployment insurance benefits in stimulating total spending.

There are three promising directions for future research. First, it would be valuable to compare the estimates in this paper with those using survey data and to estimate the impact of the SNAP cuts on non-grocery store spending. Second, I have not considered the

[24]However, any definitive statement would additionally require a comparison of the efficiency costs of the two programs during recessions.

welfare implications of these findings. Third, it would be valuable to extend the analysis to study treatment effect heterogeneity and to identify to which households it is most critical to provide SNAP benefits. Evidence from Denmark and recent work on the effects of the 2001 and 2008 stimulus payments show that there are significant differences across households in the degree to which household consumption responds to transitory income (e.g., Bruich 2014a, Kaplan and Violante 2014, Parker 2014). My results for each of the three regions in section 2.5.4 suggest that there is likely also heterogeneity across households in consumption responses to changes in SNAP benefit levels. A key distinction with the evidence from the stimulus payments is that benefit levels were set lower permanently and that all SNAP households have very low income and little liquid wealth, suggesting that the factors that determine consumption responses by SNAP households could be very different than those identified previously. With additional changes to SNAP very much on the table in Washington, such insights would provide very important guidance to policymakers as they debate the future of this program.

Chapter 3

Payday as a Zeitgeber for Consumption, Crime, and Adverse Health Outcomes

3.1 Introduction

Neoclassical consumption models predict that the timing of anticipated income should be irrelevant for households' consumption decisions. Empirical evidence from the U.S. and the U.K. rejects this prediction, with the greatest departures from the standard model by households with little liquid assets who may also suffer from self-control problems (e.g., Gelman et al. 2014, Mastrobuoni and Weinberg 2009). This paper argues that Denmark offers a unique lens through which these results can be understood by documenting seven new facts.

First, I use new data on bank accounts and tax filings by employers to show that 80% of the Danish population receives income on the same day of each month.

Second, I show that on this same day of the month, emergency room visits increase nationwide by 80 visits, compared with 4 million people who receive income on that day. Increases are especially large for ER visits with diagnosis codes that are drug and alcohol related and that are for head injuries, while no increases are found for ER visits for heart attacks.

Third, I use scanner data from most grocery and convenience stores in Denmark to show that aggregate expenditure on hard alcohol also increases in weeks where this large fraction of the population is paid. Spending on staples does not appear to increase.

Fourth, crime increases on these days by 40% relative to the average daily rate, with about 60% of these additional crimes comprised of property crimes. Although all types of crime increase, there are especially large percentage increases in violent crimes, sexual offenses, and narcotics violations. Property crimes decline in the days before and spike in the days after the payments are made.

Fifth, traffic accidents resulting in injuries also increase nationwide by about 2.6 accidents (15.5% relative to the average daily rate). Alcohol-related accidents also increase.

Sixth, I find some evidence that deaths increase as well. I lack sufficient precision to reject that deaths do not increase when this large fraction of the population receives income, but I can rule out that deaths increase by more than 3% relative to the average daily rate.

Seventh, the magnitude of the increases in ER visits, crime, and traffic accidents are positively correlated with one another. That is, a larger increase in one outcome in one month is associated with larger increases in other outcomes during the same month.

Because alcohol consumption appears to be the driving force behind these increases in adverse outcomes, I test whether these patterns are affected by policies which affect alcohol prices. I find no systematic relationship between alcohol excise taxes and the magnitude of the monthly increases in ER visits, crimes, or traffic accidents. I also consider two other correlates, weather conditions and economic conditions as measured by unemployment rates, and similarly find no systematic relationship between the monthly increases and these correlates.

I present the results in the next section. I then conclude with a discussion of these results and directions for future research.

3.2 Results

3.2.1 Result 1: Payment dates

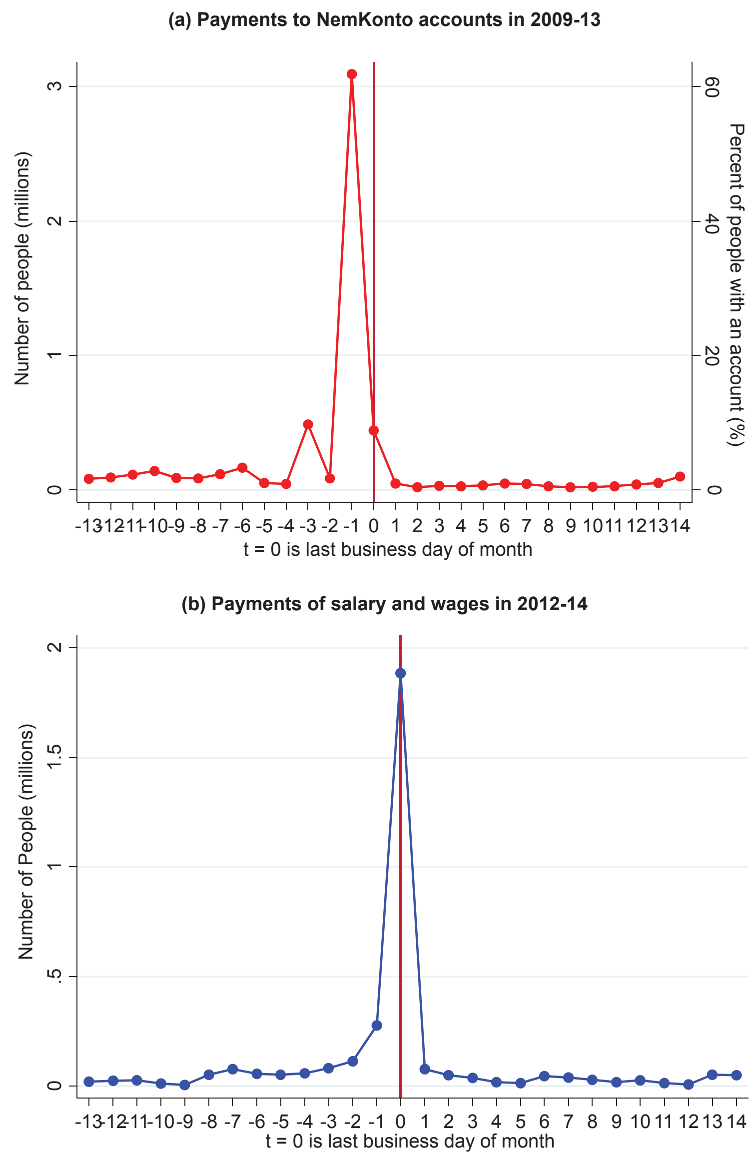
I document payment dates using two databases, neither of which have been used by researchers prior to this study.

The first database is the universe of NemKonto bank accounts in 2009 to 2013. Every person and business in Denmark is required to designate a bank account as their NemKonto bank account, which is then used by the government to make payments via direct deposit. Payments from any the 850 government authorities in Denmark, including transfer and social insurance program payments, tax refunds, and wages for government employees, are included in these data. There were 4,927,626 people with a NemKonto account on average from 2009 to 2013. I am grateful to NemKonto for making these data available to me for this project.

The main source of income not included in the first database is salary payments for the private sector. These private sector payments, in addition to wages for government employees, are included in the second database, which is derived from the eIndkomst (eIncome) register. Employers are required to submit the information that is included on their employees' payslips to the tax authority (known as SKAT), which is then stored in the eIndkomst register. One of the variables collected is the payment date ("Udbetalt dato"). Although the eIndkomst register has been used by researchers previously, the payment date variable has not been used previously and is not included in the version of this database made available to researchers at Statistics Denmark. I am grateful to SKAT for making these data available to me for this project.

Figure 3.1 uses these two databases to plot event studies of the number of people who receive a payment on each day of the twenty-eight day window around the last business day of the month. The last business day of the month equals the last calendar day of the month unless that day is a weekend or public holiday, in which case the payments are made the day before. Panel A plots payments from the NemKonto bank account database, while

Figure 3.1: *Event Study of Payments around Last Business Day of Month*



NOTE–Panel A of this figure plots the average number of people each day who receive a payment to their NemKonto bank account in 2009-2013 for the 28-day window around the last business day of the month ($t = 0$). The figure includes payments from any of the 850 government authorities in Denmark, including transfer and social insurance program payments, tax refunds, and wages for government employees. The right y-axis expresses the number of people receiving a payment as a percentage of the total number of people with a NemKonto bank account, which was 4,927,626 on average from 2009 to 2013. Panel B of this figure plots the average number of people each day who receive a salary or wage payment as reported by their employer to the tax authority (SKAT) for the 28-day window around the last business day of the month ($t = 0$).

Panel B uses the payment information provided by the tax authority for wage earners.

Panel A shows that over 3 million people receive income originating from the government right before the last day of the month. The x-axis in Panel A is the date that the payment is posted to the bank account and shows that at least one payment is posted the day before the last business day of the month for 60% of people with an account. Panel B shows that nearly 2 million people receive their salary on that day. Combining these two figures, I estimate that approximately 4 million people and between 60% and 80% of the population receive some sort of payment on the last business day of the month.

As a point of comparison, Stephens (2002, figure 1) reports 16% of his sample in the U.K. is paid on the last day of the month. In Skiba and Tobacman's (2015) data from a financial services company that offers payday loans in U.S., the most common payday is the last Friday of the month. Between 38.5% and 74.5% of their sample is paid on that day, depending on whether the last calendar day of the month falls on a Friday.

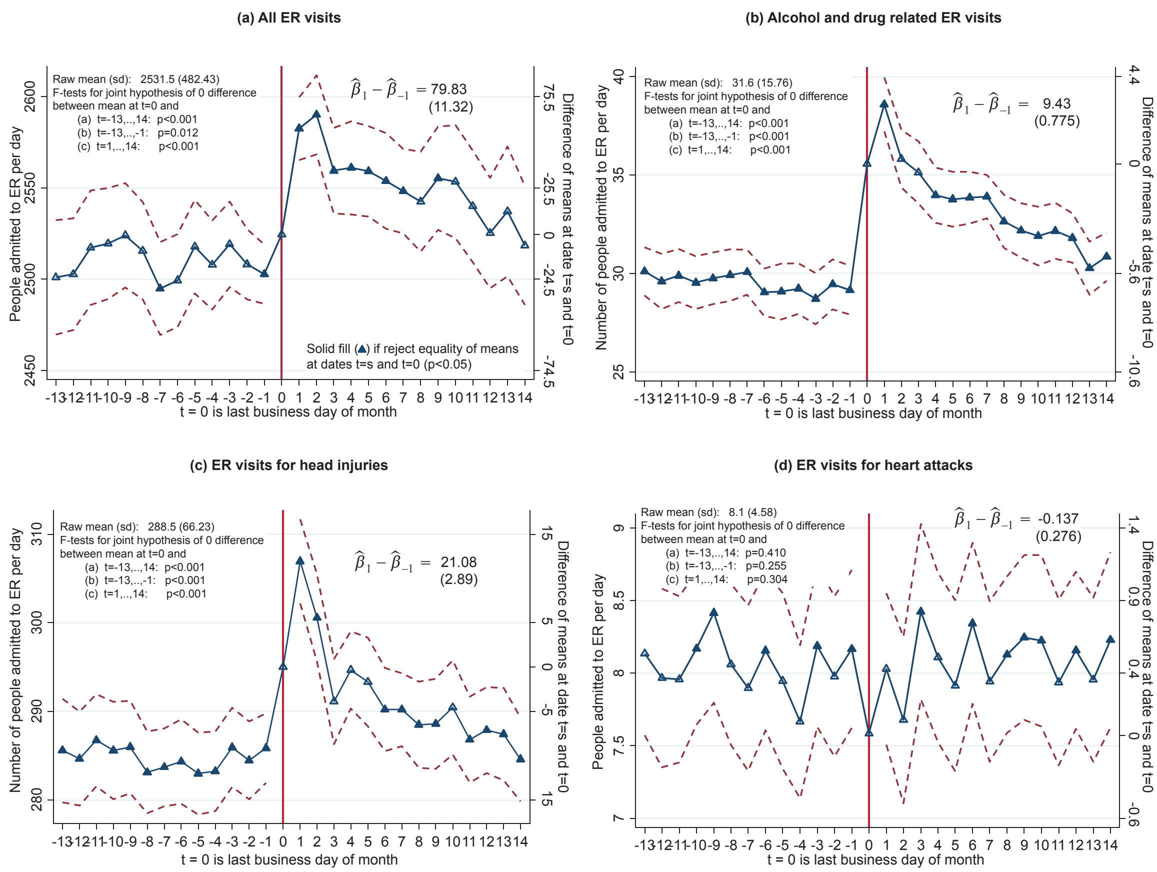
3.2.2 Result 2: ER visits

I use data on all emergency room visits contained in the Danish National Patient Register to document an increase in visits that occurs at the same time that 4 million people receive income.

Lynge, Sandegaard, and Rebolj (2011) provide a detailed description of these data. The earliest data I use are from 1994, because this is the year in which the register began classifying admissions using ICD-10 diagnosis codes. Note that the reporting of emergency room visits only became mandatory in 1995, but I include visits in 1994 in my analysis because some hospitals did report emergency room visits in this year. My results are not sensitive to including or excluding 1994.

Figure 3.2 graphs event studies of the average number of people visiting the ER for the twenty-eight day window around the last business day of the month. The series is adjusted for day of the week, month, year, and holidays as in Bruich (2014a). Panel A shows all ER visits, Panel B plots drug and alcohol related ER visits, Panel C plots ER visits for head

Figure 3.2: *Event Study of ER visits around Last Business Day of Month: 1994-2011*



NOTE—This figure plots the average number of people who visit the ER per day in 1994-2011 for the 28-day window around the last business day of the month ($t = 0$). The figure shows all ER visits (Panel A), alcohol and drug related visits (Panel B), and visits for head injuries (Panel C) and heart attacks (Panel D). The means are adjusted for day of the week, synthetic month, year, and nineteen reoccurring dates by regressing the number of people admitted to the ER on date d on indicators for each date $t = -13, ..., 14$ and indicators for each control variable:

$$\sum_i y_{id} = \alpha + \sum_{s=-13}^{-1} \beta_s I(t_d = s) + \sum_{s=1}^{14} \beta_s I(t_d = s) + \gamma X_d + \widetilde{u}_d$$

where y_{id} is an indicator for whether person i visited the ER on date d. The points in the figure are $\beta_0 = 0$ and the twenty-seven $\widehat{\beta}_s$ coefficients. The dashed lines show a 0.95 confidence interval for the difference between means at date $t = 0$ and date $t = s$. Standard errors are clustered by 28-day month. The y-axis along the left-hand side of the figure is scaled so that the mean of the twenty-eight points equals the sample average of the dependent variable. The nineteen reoccurring dates are listed in Appendix Table A3. There are $T = 215 \times 28 = 6020$ days in my sample. A visit is defined as alcohol and drug related if any of the diagnosis codes associated with that visit are for drugs and alcohol (ICD-10 codes T40, T436, T510, and those starting with F1, but excluding F17). A visit is defined as being for a head injury or heart attack based on the primary diagnosis code (codes starting with S0 for head injuries; I50 and I21 for heart attacks).

injuries, and Panel D plots ER visits for heart attacks. See Bruich (2014a) for more details on the methodology used to construct these figures.

The point estimate $\widehat{\beta}_1 - \widehat{\beta}_{-1} = 79.83$ in Panel A shows that about 80 more people visit the ER when 4 million receive income. Bruich (2014a) disaggregates these data and shows that these 80 people tend to be disability insurance recipients and those who will be disability insurance recipients in the future.

The results for drugs and alcohol are shown in Panel B. There are about 9.5 more people who visit the ER for drugs and alcohol on the day after the last business day of the month, relative to the day before the last business day of the month, which is a 30% increase relative to the average daily rate. The number of ER visits for drugs and alcohol gradually declines over the 14 days after the last business day of the month.

The pattern for head injuries in Panel C is somewhat different. Here, there is a spike in people visiting the emergency room that only remains elevated for the three or four days after $t = 0$. The combined increase for drugs and alcohol and head injuries is about 38% relative to the overall increase shown in Panel A (and ignoring the small overlap between these categories).

Panel D plots ER visits for heart attacks. In contrast with the other three panels of this figure, no clear increase is visually detectable. Statistically, I cannot reject that the series is a flat line, with a p-value of 0.41 for the joint test that average number of ER visits on each of the twenty-eight days are all equal.

Please see Bruich (2014a) for a battery of robustness checks that show that the estimates are not sensitive to on which day of the week the last business day of the month occurs or to the vector of controls used.

3.2.3 Result 3: Household consumption and expenditures

I use weekly scanner data from most major grocery and convenience stores in Denmark to document changes in household buying behavior in weeks where the vast majority of the population receives income. Data were provided by the Nielsen Company for years

2011 to 2014. The data are grouped into six product categories: beer, spirits, non-alcoholic carbonated beverages, coffee, yellow fats (e.g., butter and margarine), and biscuits.

To test whether expenditure increases in weeks where this large fraction of the population receives income, I estimate equations of the form:

$$y_t = \alpha + \beta \text{payment}_t + \gamma X_t + u_t \tag{3.1}$$

where y_t is log total quantity sold in week t, payment_t is an indicator variable that equals one in week t if week t includes the last business day of the month (or any of the three days that follow that day), and X_t is a vector of binary controls that vary by specification. In the most robust specification, X_t includes a linear time trend, year fixed effects, month fixed effects, year $\times$ month fixed effects, and separate indicators for the nineteen holidays and other reoccurring special dates that are listed in Appendix Table A3.

Table 3.1 presents estimates of the coefficient on the payment_t indicator from Equation (3.1). Newey-West standard errors, estimated using four lags, are reported in parentheses below the coefficient estimates. Row 1 reports estimates without any controls. Row 2 adds year, month, and month $\times$ year fixed effects. Row 3 adds the holiday controls. Row 4 also includes a linear time trend.

Focusing on Columns 3-4 of Row 1 of Table 3.1, sales of hard alcohol increase by 15% to 12% in weeks where a large fraction of the population receives income. Adding controls in Rows 2, 3, and 4 reduce the point estimate to a 6% to 8% increase (when the dependent variable is measured in liters), but the estimate remains significant at the 1% level. Sales of beer, non-alcoholic carbonated soft drinks, and coffee all increase by 4% to 6% when payments are received in the specifications without controls. Adding controls reduces the magnitude and statistical significance of the increase in sales for all these categories except for coffee, which increases slightly to 8% to 9% and remains significant at the 1% level.

In contrast, sales of the two staples (yellow fats and biscuits) are small and imprecisely estimated in the specifications without controls. With and without controls, I cannot reject that there is no increase in sales of staples when a large fraction of the population receives

Table 3.1: *Percent Change in Expenditures during Weeks when Payments are Received*

Category:	Alcohol				Non-alcoholic beverages				Staples			
	Beer		Spirits		Carbonated soft drinks		Coffee		Yellow fats (e.g., butter)		Biscuits/cookies	
	Liters (%)	Kroner (%)	Liters (%)	Kroner (%)	Liters (%)	Kroner (%)	Kilograms (%)	Kroner (%)	Kilograms (%)	Kroner (%)	Kilograms (%)	Kroner (%)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
1. No controls	4.54**	4.79**	15.0***	12.0***	5.33***	5.34***	6.84**	5.81**	-0.855	-1.18	4.58	3.79
	(2.16)	(2.08)	(3.00)	(2.61)	(1.70)	(1.47)	(2.90)	(2.28)	(2.94)	(2.95)	(3.56)	(2.77)
2. Month x year fixed effects	2.78	2.93	8.09***	5.22*	3.78*	4.19**	8.41**	7.02***	-1.56	-1.16	3.75	2.66
	(2.54)	(2.39)	(2.57)	(2.80)	(2.13)	(1.73)	(3.26)	(2.53)	(2.85)	(2.88)	(4.29)	(3.37)
3. Month x year f.e. and holidays	2.54	2.31	6.15**	3.57	3.44	3.89*	8.57***	7.18***	-0.777	-0.412	4.86	3.79
	(2.22)	(2.07)	(2.42)	(2.40)	(2.31)	(2.02)	(2.59)	(1.94)	(1.41)	(1.29)	(3.23)	(2.34)
4. Month x year f.e., holidays, and linear time trend	2.05	1.74	7.54***	4.29*	4.57*	3.70*	9.12***	7.66***	0.31	0.927	6.4	4.89
	(2.64)	(2.30)	(2.48)	(2.39)	(2.67)	(1.97)	(3.39)	(2.48)	(2.05)	(1.76)	(4.09)	(3.22)
5. Mean of dependent variable	4,235	68,129	3,207	35,744	5,098	54,884	4,754	41,060	751	33,245	401	20,480
	(1000s L)	(1000s kr)	(100s L)	(1000s kr)	(1000s L)	(1000s kr)	(100s KG)	(1000s kr)	(1000s KG)	(1000s kr)	(1000s KG)	(1000s kr)
6. Number of weeks	157	157	157	157	157	157	157	157	157	157	157	157

NOTE -- Each cell in the table corresponds to a separate OLS regression where the dependent variable is either log weekly quantity sold or log weekly sales (in kroner) in one of six product categories (beer, spirits, non-alcoholic carbonated beverages, coffee, yellow fats, and biscuits). Each cell in the table reports the coefficient on an indicator variable that equals one if the week includes the last business day of the month or the three days that follow that day of the month. Newey-West standard errors with four lags are reported in parentheses. The data are weekly over a three year period from 2011 to 2014. The regressions include either no controls (row 1), month, year, and month x year fixed effects (row 2), these controls plus controls for 19 holidays and other special dates (row 3), and these controls plus a linear time trend (row 4). Row 5 reports the mean of the dependent variable in levels.*** p<0.01, ** p<0.05, * p<0.1

income.

3.2.4 Result 4: Crime

I next document patterns in crimes over the course of the month using nationwide administrative data on crimes reported on each day from 1990 to 2013. There are 1,670.5 crimes reported each day on average across the entire sample period. Of these, 76% are property crimes, 3% are violent and/or sexual offenses, 2.6% are related to narcotics, and 1.2% are related to firearms.

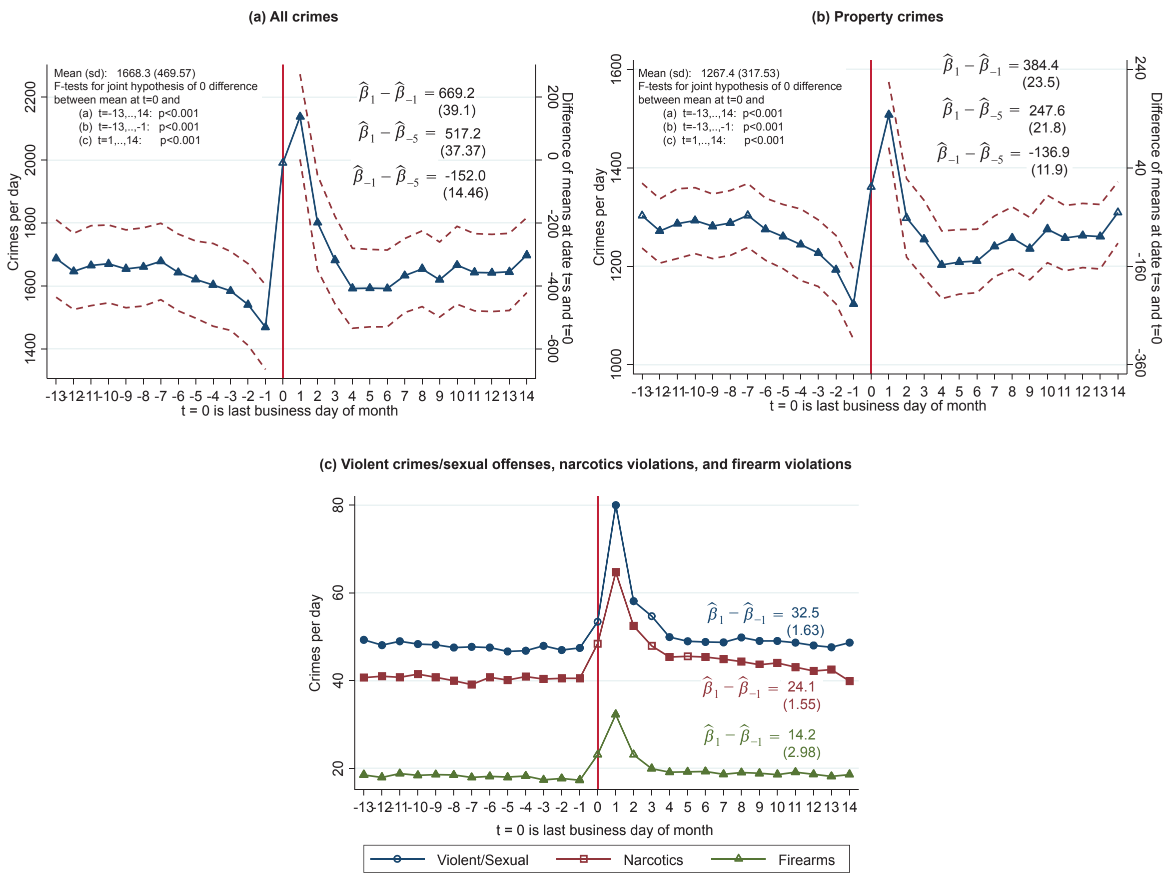
Figure 3.3 graphs event studies of the average number of crimes committed for each day during the twenty-eight day window around the last business day of the month. The series is adjusted for day of the week, month, year, and holidays as in Bruich (2014a).

Panel A plots the series for all crimes. The point estimate $\widehat{\beta}_1 - \widehat{\beta}_{-1} = 669.2$ shows that about 670 more crimes are reported at the same time that 4 million people receive income. This increase is 40% of the average daily rate. Panel B plots the series for property crimes. The point estimate $\widehat{\beta}_1 - \widehat{\beta}_{-1} = 384.4$ shows that about 57% of the total increase in crimes is comprised of property crimes. Property crimes decline in the days before and spike in the days after $t = 0$. Measuring the increase in property crimes after the payments are made and the decrease in property crimes before the payments are made relative to $t = -5$ yields point estimates of $\widehat{\beta}_1 - \widehat{\beta}_{-5} = 247.6$ and $\widehat{\beta}_{-1} - \widehat{\beta}_{-5} = -136.9$, which are 19.5% and -10.8% changes relative to the average daily rate.

Panel C plots the series for violent and sexual crimes in circles, narcotics violations in squares, and firearm violations in triangles. Compared with property crimes, these three categories make up small shares of all crimes committed in Denmark, but the percentage increases on the payment date is especially large, with a 65% increase in violent and sexual crimes, a 55.4% increase in narcotics violations, and a 77% increase in firearm violations.

Appendix Figure C1 confirms that the patterns shown in Figure 3 are driven by payments, and not day of the week effects. I plot five sets of estimates that restrict the sample to exclude the twenty-eight day months where $t = 0$ occurs on a particular day of the week.

Figure 3.3: *Event Study of Crime around Last Business Day of Month: 1990-2013*



NOTE–This figure plots the average number of crimes reported per day over the 28-day window around the last business day of the month ($t = 0$) in 1990-2013. The means are adjusted for day of the week, synthetic month, year, and nineteen reoccurring dates in the same way as in Figure 3.2. Panel A plots all crimes, Panel B plots property crimes, and Panel C plots violent/sexual crimes (in circles), narcotics violations (in squares), and firearms violations (in triangles).

The five lines line up quite closely, indicating that the increase in crimes is similar no matter which day of the week the last business day of the month happens to fall upon.

Appendix Figure C2 assesses in what ways changes to the vector of controls affect my results. I plot the coefficients from the basic regression model as a reference in diamonds. I then re-estimate the model, adding the days before and after each holiday and interactions of the month and year indicators to the vector of controls. Coefficients from this expanded model are plotted in small squares. I re-estimate the model also where I include these additional controls and exclude the twenty-eight day months during which the New Year's holiday occurs. Coefficients from this third model are plotted in larger squares. The three series match closely, indicating that the results are not sensitive to the controls used and are not driven by the New Year's holiday, which always occurs a few days after the final payment of each year.

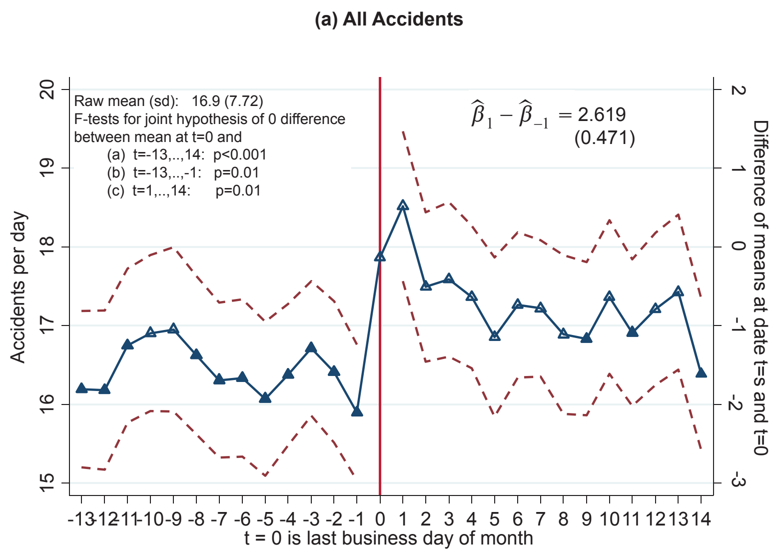
3.2.5 Result 5: Traffic accidents

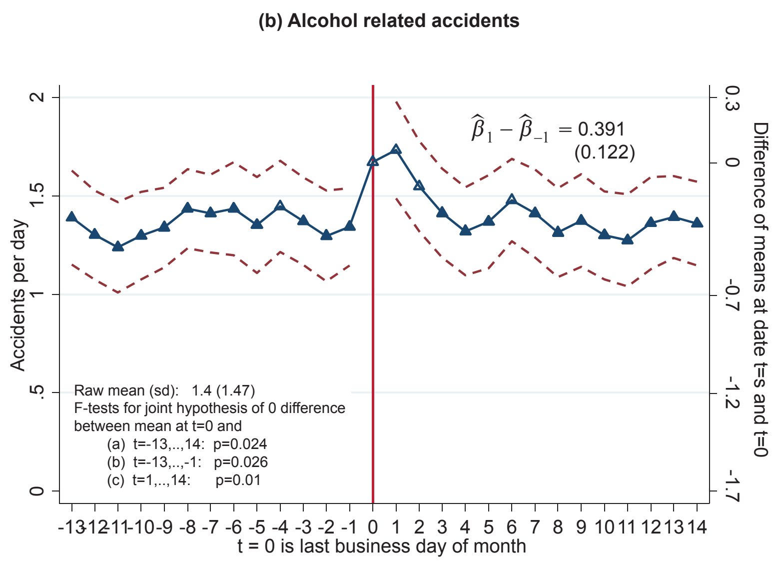
Next, I document similar patterns in traffic accidents using nationwide data on all traffic accidents that resulted in an injury for each day in 1993 to 2013. These data are analogous to the Fatal Accident Reporting System (FARS) data in the United States, but include accidents resulting in injuries in addition to fatalities. The data are police recorded. Greibe (2000) reports that 20% of all accidents, 100% of accidents resulting in fatalities, and 60% of accidents resulting in serious injuries are included in these data.

There are 16.86 accidents per day on average across the entire sample period, of which 1.38 are alcohol related accidents. An accident is classified as alcohol-related if the blood alcohol level of at least one driver involved in the accident was measured to be over 0.050%, which is the legal limit in Denmark as of 1998.

Figure 3.4 plots the average number of accidents that occur per day for the twenty-eight day window around the last business day of the month. The series is adjusted for day of the week, month, year, and holidays as in Bruich (2014a). The point estimate $\widehat{\beta}_1 - \widehat{\beta}_{-1} = 2.62$ shows that about 2.6 more accidents occur at the same time that 4 million people receive

Figure 3.4: *Event Study of Accidents around Last Business Day of Month: 1993-2013*





NOTE–This figure plots the average number of accidents per day over the 28-day window around the last business day of the month ($t = 0$) in 1993-2013. The means are adjusted for day of the week, synthetic month, year, and nineteen reoccurring dates in the same way as in Figure 3.2. Panel A plots all accidents and Panel B plots alcohol-related accidents.

income. This increase is 15.5% relative to the average daily rate.

Panel B restricts attention to alcohol-related accidents. The point estimate for alcohol-related accidents is $\widehat{\beta}_1 - \widehat{\beta}_{-1} = 0.391$, which is a 27.9% increase relative to the average daily rate. The hypothesis that there is no increase in alcohol-related accidents can be rejected with a p-value of less than 0.01.

Appendix Figure C3 confirms that the patterns shown in Panel A are driven by payments, and not day of the week effects. I plot five sets of estimates that restrict the sample to exclude the twenty-eight day months where $t = 0$ occurs on a particular day of the week. The five lines line up quite closely, indicating that the increase in accidents is similar no matter which day of the week the last business day of the month happens to fall upon.

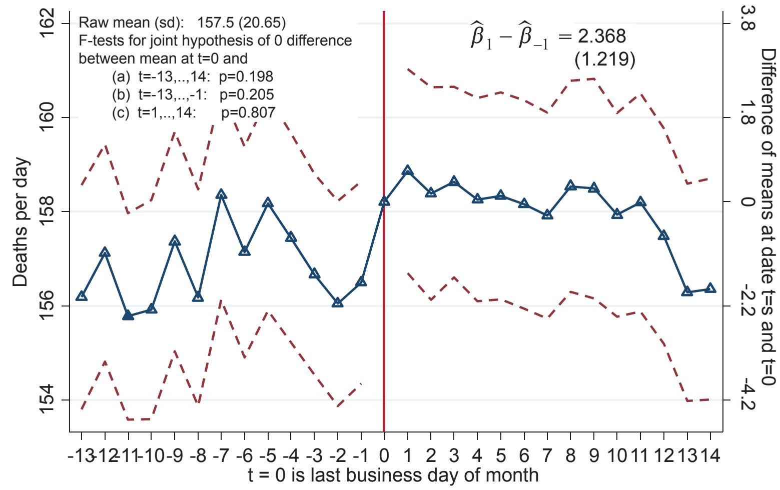
Appendix Figure C4 assesses in what ways changes to the vector of controls affect my results. I plot the coefficients from the basic regression model as a reference in diamonds. I then re-estimate the model, adding the days before and after each holiday and interactions of the month and year indicators to the vector of controls. Coefficients from this expanded model are plotted in small squares. I re-estimate the model also where I include these additional controls and exclude the twenty-eight day months during which the New Year's holiday occurs. Coefficients from this third model are plotted in larger squares. The three series match closely, indicating that the results are not sensitive to the controls used and are not driven by the New Year's holiday, which always occurs a few days after the final payment of each year.

3.2.6 Result 6: Mortality

Finally, I study mortality using the Danish Register of Causes of Death for years 1990-2012. Helweg-Larsen (2011) provides a detailed description of these data. There are 159.2 deaths per day on average over the sample period.

Figure 3.5 plots the average number of deaths per day for the twenty-eight day window around the last business day of the month. The series is adjusted for day of the week, month, year, and holidays as in Bruich (2014a).

Figure 3.5: *Event Study of Deaths around Last Business Day of Month: 1990-2012*



NOTE–This figure plots the average number of deaths per day over the 28-day window around the last business day of the month ($t = 0$) in 1990-2012. The means are adjusted for day of the week, synthetic month, year, and nineteen reoccurring dates in the same way as in Figure 3.2.

While the change in the number of deaths per day is imprecisely estimated, it would imply a 1.5% increase relative to the average daily rate, with a 95% confidence interval of -0.01% to 3%. I cannot reject that the series is a flat line, with a p-value of 0.198 for the joint test that average number of deaths on each of the twenty-eight days are all equal. Extending this analysis using an additional twenty years of data would likely allow me to say definitively if there is an increase in mortality.

3.2.7 Result 7: Correlations between outcomes

Do the monthly increases in these outcomes tend to be large at the same time?

To answer this question, Table 3.2 explores the correlation between the monthly increases in ER visits, crimes, and traffic accidents. For each twenty-eight day window $j = 1, \ldots, J$, let Δy_{kj} be the difference in the number of outcomes of type k occurring at date $t = -1$ and at date $t = +1$. I scale this difference by $\overline{y}_k$, the average daily rate for outcome k across the entire sample period.

Table 3.2 reports coefficient estimates from regressions that use this measure as the

Table 3.2: *Correlations Between Increases Around Last Business Day of Month Across Outcomes, 1994-2011*

Dependent variable:	ER Visits		Crime			Traffic accidents	
	Drug/Alcohol	Head injuries	Property	Violent/Sexual	Narcotics	All	Alcohol
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
1. Drug and alcohol related ER visits	1	0.118***	0.161	0.310***	0.125	0.0481	0.212
		(0.0249)	(0.109)	(0.102)	(0.0988)	(0.0874)	(0.243)
2. ER visits for head injuries	0.890***	1	0.287	0.301	0.274	0.701**	1.761**
	(0.185)		(0.202)	(0.281)	(0.284)	(0.305)	(0.763)
3. Property crimes	0.243*	0.0575	1	0.683***	0.113	0.125	0.809***
	(0.134)	(0.0362)		(0.142)	(0.126)	(0.0983)	(0.273)
4. Violent crimes/Sexual offenses	0.179***	0.0231	0.262***	1	0.284***	0.0906	0.233
	(0.0564)	(0.0219)	(0.0433)		(0.0673)	(0.0656)	(0.225)
5. Narcotics violations	0.0705	0.0206	0.0423	0.278***	1	0.101	-0.277
	(0.0566)	(0.0209)	(0.0455)	(0.0721)		(0.0728)	(0.205)
6. Traffic accidents	0.0324	0.0627***	0.0557	0.106	0.120	1	1.121***
	(0.0587)	(0.0236)	(0.0435)	(0.0817)	(0.0804)		(0.206)
7. Alcohol related traffic accidents	0.0140	0.0155**	0.0356**	0.0267	-0.0325	0.110***	1
	(0.0164)	(0.00640)	(0.0166)	(0.0261)	(0.0233)	(0.0192)	

NOTE -- Each cell reports the slope coefficient from a separate OLS regression that includes day of week, month, and year controls. The dependent variable in each regression is the change in the number of outcomes occuring at t = -1 and at t = +1 relative to the average daily rate for that outcome variable. The independent variable is defined in the same way. The labels in columns correspond to the dependent variable and the labels in the rows correspond to the independent variable. Newey-West standard errors with 4 lags are reported in parentheses below the coefficient estimates. The regressions are restricted to 1994 to 2011. There are 215 observations in each regression. *** p<0.01, ** p<0.05, * p<0.1

dependent and independent variable in regressions of the form:

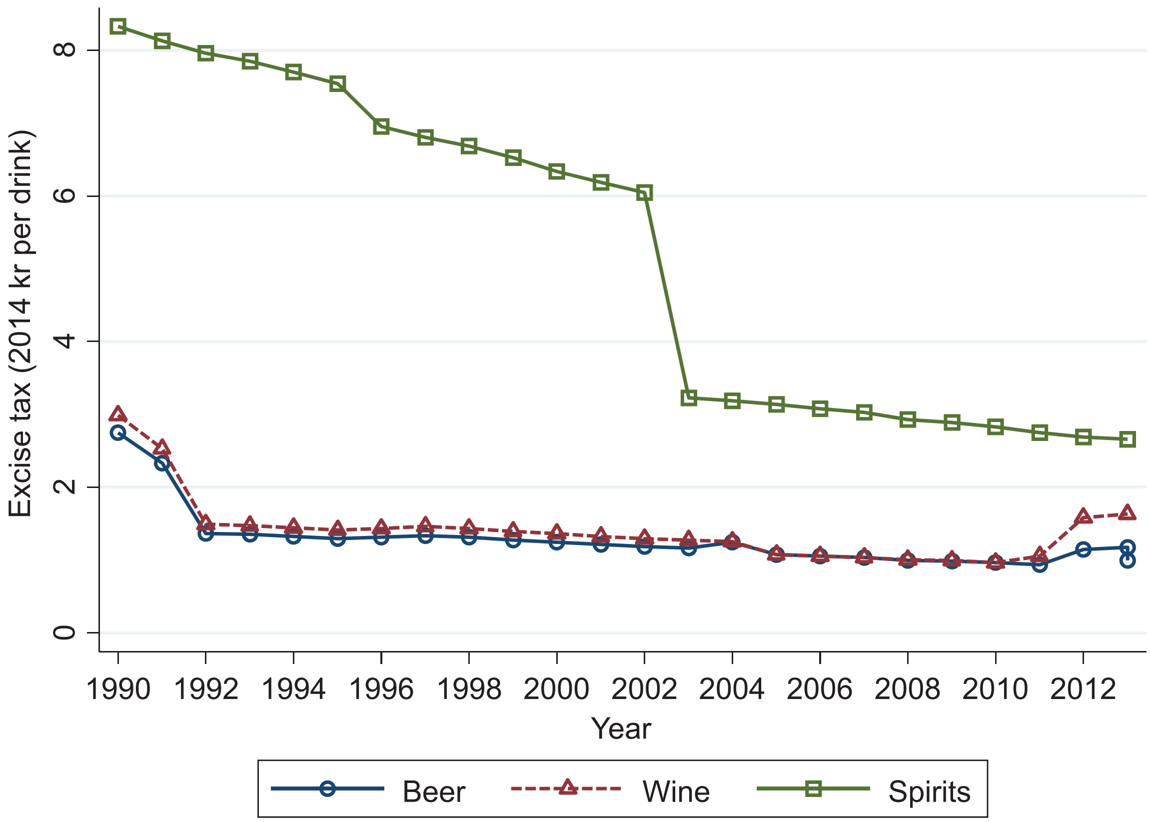
$$\Delta y_{kj}/\overline{y}_k = \alpha + \beta \Delta y_{lj}/\overline{y}_l + \gamma X_j + u_j \tag{3.2}$$

where X_j includes indicator variables for year, month, and day of week. Newey-West standard errors with four lags are reported below the coefficient estimates in parentheses.

In the interest of space, I report correlations among the outcomes that show the most pronounced increases around the last business day of the month: ER visits for drug and alcohol related diagnoses, ER visits for head injuries, property crimes, violent crimes/sexual offenses, narcotics violations, traffic accidents, and traffic accidents that are alcohol related.

As highlighted in gray, the table shows $\beta > 0$ for all outcome pairs, except for the regressions involving the alcohol traffic accident and narcotics crimes pair, where the coefficient is negative but close to zero. The table therefore shows a general finding that greater increases in one outcome around payday are associated with greater increases in other outcomes at the same time.

Figure 3.6: *Alcohol Excise Taxes in 1990-2013*



NOTE–This figure plots the excise tax on beer (circles), wine (triangles), and spirits (squares) in real 2014 kroner per drink.

3.2.8 Correlations with alcohol taxes, weather conditions, and unemployment rates

Finally, I study the correlations of the monthly increases in ER visits, crime, and traffic accidents with alcohol taxes, weather conditions, and economic conditions as measured by unemployment rates.

Because alcohol consumption appears to be the driving force behind these increases in adverse outcomes, I first test whether these patterns are affected by policies which affect alcohol prices. Unlike other Scandinavian countries, alcohol taxes are the main policy tool used to restrict alcohol consumption in Denmark.

I construct a series of alcohol excise taxes on beer, wine, and spirits from 1990 to 2013, drawing upon data from the Danish Grocers Association (De Samvirkende Købmænd 2015) and other sources. I convert the tax in liters of ethanol into the equivalent tax in real 2014 kroner per drink, using the conversions in Cook (2007, page 50).

Figure 3.6 plots these three series. As shown in the figure, there were a number of policy changes over this time period. The largest tax change was a 45% reduction in the excise tax on spirits on October 1, 2003, which reduced prices by 25% (Grittner, Gustafsson, and Bloomfield 2009, Bergman and Hansen 2010). There were more changes in beer and wine excise taxes, but each was smaller in magnitude than the change in spirits tax. Although there were some tax increases, the trend has been towards lower tax rates.

The excise tax is a national tax, so all the variation is across time. While this is a limitation in measuring the impact of alcohol taxes on the monthly increases in ER visits, crime, and traffic accidents, I use the fact that these policy changes typically were effective during mid-year. In the estimates that follow, I include year fixed effects, so the only source of identifying variation is the within-year changes in taxes.

Panel A of Table 3.3 reports coefficients from equations of the form:

$$\Delta y_{kj}/\overline{y}_k = \alpha + \beta \log(\text{alcohol tax}_j) + \gamma X_j + u_j \tag{3.3}$$

where X_j includes indicator variables for year, month, day of week, and holidays. Newey-West standard errors with four to five lags are reported below the coefficient estimates in parentheses. The first row in the table corresponds to the beer excise tax. The second row corresponds to the spirits excise tax. I do not report the wine excise tax separately since it tracks the beer excise tax rate.

The coefficients in row 1 show that a 10% increase in the beer tax is associated with a 0.9 percentage point increase in the monthly spike in drug and alcohol related ER visits relative to its average daily rate, but is associated with decreases in the monthly spikes in ER visits for head injuries and all types of crime. That same 10% increase is associated with a larger spike in traffic accidents, but a 0.7 percentage point decrease in the monthly spike in alcohol related traffic accidents.

Table 3.3: *Correlations between Monthly Increases in ER visits, Crime, and Traffic Accidents with Alcohol Taxes, Weather, and Unemployment Rates*

Dependent variable:	ER Visits				Crime						Traffic accidents			
	Drug/Acohol		Head injuries		Property		Violent/Sexual		Narcotics		All		Alcohol	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
Panel A: Alcohol excise taxes														
Log beer tax (kr per drink)	9.07***		-1.059		-0.688**		-0.0491		-0.756		0.999		-7.383**	
	(3.308)		(1.390)		(0.268)		(0.351)		(0.603)		(0.921)		(3.462)	
Log spirits tax (kr per drink)		-0.115		-0.0285		-0.164		-0.142		0.263		0.377		1.730*
		(0.291)		(0.115)		(0.192)		(0.341)		(0.386)		(0.230)		(1.023)
Panel B: Weather conditions														
Average temperature (°C)	0.012		-0.003		0.012		-0.003		-0.019		0.0156		0.00273	
	(0.0096)		(0.0048)		(0.0079)		(0.011)		(0.015)		(0.0160)		(0.0406)	
Precipiation (mm)		-0.002		-0.006		0.009		-0.004		0.01		-0.001		-0.0291
		(0.0102)		(0.00384)		(0.00990)		(0.0136)		(0.0147)		(0.0127)		(0.0329)
Panel C: Unemployment rates														
Unemployment rate (%)	-0.0127		0.0104		0.0592		0.137*		-0.0516		-0.633		0.0696	
	(0.0713)		(0.0261)		(0.0592)		(0.0698)		(0.101)		(0.699)		(0.0777)	

NOTE -- Each cell reports the slope coefficient from a separate OLS regression that includes day of week, month, year, and holiday controls. The dependent variable in each regression is the change in the number of outcomes occuring at t = -1 and at t = +1 relative to the average daily rate for that outcome variable. Newey-West standard errors are reported in parentheses below the coefficient estimates. Columns 1-4 use four lags because ER visits are only available from 1994 to 2011 and the other columns use 5 lags. *** p<0.01, ** p<0.05, * p<0.1

There is much less identifying variation in the excise tax on spirits, since there was essentially just the one policy change in 2003. However, the coefficients again show both decreases and increases associated with a higher excise tax on spirits depending on the outcome of interest.

I also study weather conditions. Poor weather conditions could possibly attenuate the monthly increases in adverse outcomes if poor weather makes individuals less active and spend more time indoors. Alternatively, poor conditions could combine with alcohol consumption to exacerbate the spikes in adverse outcomes.

Panel B of Table 3.3 reports estimates of equations of the form:

$$\Delta y_{kj}/\overline{y}_k = \alpha + \beta \text{weather}_j + \gamma X_j + u_j \tag{3.4}$$

where weather conditions correspond to the average daily temperature (row 1) or to the average daily precipitation in Denmark (row 2) and X_j includes indicator variables for year, month, day of week, and holidays. I use data from the Danish Meteorological Institute from 1990 to 2010 (Scharling 2012). The coefficients are close to zero and not statistically significantly different than zero.

I also consider the economic conditions, as measured by unemployment rates. Panel C of Table 3.3 reports estimates of equations of the form:

$$\Delta y_{kj}/\overline{y}_k = \alpha + \beta \text{unemployment}_j + \gamma X_j + u_j \tag{3.5}$$

where the unemployment rate is the quarterly seasonally adjusted unemployment rate (Federal Reserve Bank of St. Louis 2015) and X_j includes indicator variables for year, month, day of week, and holidays.

The coefficient in column 4 is marginally statically significant, showing that higher unemployment rates are associated with larger spikes in violent crimes and sexual offenses. The other coefficients are small and not statistically significant.

In future work, it would be straightforward to use regional variation to obtain more credible estimates of the association between the monthly increases in these outcomes and alcohol prices, weather conditions, and economic conditions.

3.3 Discussion

In the epidemiology literature, Denmark has long been recognized as a natural laboratory for research because of its rich population-wide health data that spans decades. This paper documents that this entire country can be viewed as a cohort study in another important respect. In particular, the vast majority of Danish households all receive income on the same day of every month. Surprisingly, this fact had not been documented previously.

Drawing upon several administrative data sets, I show that ER visits, consumption of hard alcohol, crime, and traffic accidents all increase nationwide in Denmark when this large fraction of the population receives income. This evidence runs counter to standard theories of how households make consumption decisions and sheds new light on candidate theories attempting to explain why (e.g., Dobkin and Puller 2007, Mastrobuoni and Weinberg 2009, Evans and Moore 2011, Gross and Tobacman 2014).

One theory is that paydays are associated with a general increase in activity among liquidity constrained households (Evans and Moore 2011). The evidence on crime could be consistent with this theory if, for example, being more active puts one at greater risk of being victimized. However, one would expect to find increases in ER visits related to increased activity (e.g., heart attacks) and increases in expenditures on items other than coffee and hard alcohol if activity was the main driving force behind the increases in adverse health events. Nevertheless, direct evidence on activity would be useful. For example, measures could be defined using GPS location data that are now available from mobile phone carriers.

Another theory is that "full wallets" lead a subset of the population with drug and alcohol problems to engage in risky behavior (Dobkin and Puller 2007, Gross and Tobacman 2014). The evidence presented in this paper suggests that alcohol consumption plays an important part in the observed increases in adverse events around paydays in Denmark. In addition to direct evidence that expenditure on alcohol increases, there is indirect evidence of increased alcohol consumption from observed increases among the kinds of ER visits and traffic accidents that are consistent with excessive consumption of alcohol. Alcohol consumption follows a log normal distribution (Cook 2007). It is therefore plausible that the increase in expenditures on hard alcohol could be generated solely by the upper tail of the alcohol consumption distribution.

The increase in crime is also consistent with the evidence of increased alcohol consumption, because alcohol consumption has a causal impact on crime (Carpenter and Dobkin 2011). The effect of alcohol on aggression, judgement, inhibition, and its sedative effects can increase the both the propensity to commit crimes, as well as the probability of being a victim of a crime (e.g., Carpenter and Dobkin 2011, 2014). The results on reports of crime are similar to those found for arrests by Dobkin and Puller (2006) in California. In contrast, the results do not match evidence from the U.S. that crime substitutes for income (e.g., Foley 2011 and results on arrests for prostitution reported in Dobkin and Puller 2006).

This study has disaggregated the data by qualities of the outcomes themselves (e.g., diagnoses codes or crime classifications) to shed light on underlying causal mechanisms. As demonstrated by Bruich (2014a) using Danish data and Miller et al. (2009) using U.S. data, much can be learned by additionally disaggregating these types of data by characteristics of people instead of the outcomes. For example, what is the time path of alcohol consumption around the last day of the month for each percentile of the alcohol consumption distribution? Are disability insurance beneficiaries, who disproportionally present in the emergency room (as shown by Bruich 2014a), also the victims of crimes at the same time? Extending the work of Hastings and Washington (2010) to study the determinants of firm responses to the increases in demand around this day of the month is also a promising direction for future research.

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Appendix A

Appendix to Chapter 1

A.1 Determination of severity

The level at which disability insurance benefits are awarded is based on an assessment of how an applicant's impairment impacts his ability to work. In this sense, disability insurance eligibility in Denmark is similar to the assessment of eligibility for SSDI, described in detail, for example, in Chen and van der Klaauw (2008). However, unlike in the U.S., disability can be partial. Therefore, it is useful to consider an example to illustrate how these three severity levels are determined. I focus on cardiovascular disease because over 40% of my analysis sample (described in Section 3 and 4) has a heart condition. Municipality caseworkers assess both subjective criteria (e.g., chest pain, shortness of breath) and objective criteria (e.g., cardiogram, stress test). The following vignettes are from the rating guide used by caseworkers who assess disability.[1]

The highest level of disability is quite severe. The manual describes a 28 year old male truck driver with heart disease (cardiomyopathy), right-heart failure, shortness of breath (nocturnal dyspnea), and poor circulation. This individual was assessed at the highest level of disability because of clear medical evidence of cardiac insufficiency (heart failure). The rating guide notes that his occupation is physically demanding.

[1] I am grateful to the Copenhagen municipality for providing me with this rating guide. The information in this section is my translation of the original in Danish.

The middle level of disability is also quite severe. The manual describes a 54 year old male bricklayer with a history of cardiac symptoms requiring hospitalization. It notes evidence of pathological changes in his electrocardiogram (EKG), moderate angina pectoris (1-2 episodes of chest pain per day), but no signs of heart failure. Therefore, the guide reports that this individual was assessed at the medium level of disability because his heart condition is not likely to reduce working capacity, even for physical work like his.

For the lowest level of disability, the manual describes a 49 year old divorced woman who is an office worker. She has a history of blood clots and recently had a heart attack. She has mild angina pectoris (1 episode of chest pain per day) and only mild pathological changes in her EKG. The manual states that she was assessed at the lowest level of disability because her cardiac disorder would not prevent her from working part-time in an office setting. A second example is a 57 year old male artist and church cantor with a recent history of coronary artery occlusion and long history of cardiac symptoms. He has mild angina pectoris (1 episode of chest pain per day), mild pathological changes in his EKG, and no recent deterioration in his condition. The manual states that his disability is assessed at the lowest level because their judgment is that he should still be able to continue his work.

A.2 Liquidity vs. moral hazard and optimal social insurance

This appendix derives the standard liquidity vs. moral hazard representation of the socially optimal social insurance benefit level. Note that the model can be recast as one in which a representative agent chooses the fraction of the population working e to maximize total private surplus:

$$\begin{aligned} V &= \max_e \int_{-\infty}^{\bar{\delta}} \{u(c_h) - d_i\} dF(\delta_i) + \int_{\bar{\delta}}^{\infty} u(c_l) dF(\delta_i) && \text{(A.1)} \\ &= \max_e eu(c_h) + (1-e)u(c_l) - \psi(e) && \text{(A.2)} \end{aligned}$$

where $\psi(e) \equiv \int_{-\infty}^{\overline{\delta}} \delta_i dF(\delta_i)$ integrates the disutilities of labor over all agents with $\delta_i < \overline{\delta}$. The representative agent's first order condition is:

$$u(c_h) - u(c_l) = \psi'(e) \tag{A.3}$$

Intuitively, the representative agent increases the fraction working until the marginal worker is indifferent between exiting the labor force and continuing to work. Written this way, the expected utility and first order condition for the representative agent are exactly the same as in the standard social insurance set up (e.g., in Chetty 2009).

The government's objective is to design a disability insurance system that pays a benefit b to those who do not work. The benefit is financed by a tax t paid by workers:

$$et = (1-e)b \tag{A.4}$$

The government chooses b and t to maximize the representative agent's indirect utility:

$$\max_{b,t} V(t,b) \tag{A.5}$$

subject to the budget constraint for t and the incentive compatibility constraint for e.

At the optimum, a small change db must have no effect on utility:

$$\frac{dV}{db} = \frac{\partial V}{\partial b} + \frac{\partial V}{\partial t}\frac{dt}{db} + \frac{\partial V}{\partial e}\frac{de}{db} = 0 \tag{A.6}$$

There is an envelope condition for e because the representative agent has chosen it to maximize utility: $\frac{\partial V}{\partial e} = 0$. Intuitively, the agent on the margin between working and not working is indifferent ($u(c_h) - u(c_l) = \delta_i$). Therefore, changing b to induce him to exit the labor force has no effect on social welfare. Hence, all the terms that involve $\frac{de}{db}$ in dV/db can be ignored:

$$\frac{dV}{db} = \frac{\partial V}{\partial b} + \frac{\partial V}{\partial t}\frac{dt}{db} = (1-e)u'(c_l) - eu'(c_h)\frac{dt}{db} \tag{A.7}$$

The only behavioral responses that matter here are those in the $\frac{dt}{db}$ term that directly affect the government's budget constraint. $\frac{dt}{db}$ measures how much the tax must be increased to finance a \$1 increase in b because of behavioral responses, which is equal to:

$$\frac{dt}{db} = \frac{1-e}{e}\{1 - \frac{1}{1-e}\varepsilon_{e,b}\} \tag{A.8}$$

where $\varepsilon_{e,b} = \frac{de}{db}\frac{b}{e}$ is the total, uncompensated elasticity of the fraction of the population working with respect to b.

To obtain a money metric, define dW/db to be the change in welfare from increasing b by \$1, $\frac{dV}{db}/(1-e)$, scaled by the change in welfare from increasing the wage by \$1, $\frac{dV}{dw}/e$:

$$\frac{dW}{db} = \frac{\frac{dV}{db}/(1-e)}{\frac{dV}{dw}/e} = \frac{u'(c_l) - u'(c_h)}{u'(c_h)} + \frac{1}{1-e}\varepsilon_{e,b} \tag{A.9}$$

The intuition for this expression is as follows. The first term is the marginal benefit of increasing b by \$1: smoother consumption from the perspective of the representative agent. The second term is the marginal cost of increasing b, which depends on the labor supply response to the benefit level. A larger behavioral response requires a larger increase in t to finance that increase in b. At the optimum $dW/db = 0$ so these two terms must offset each other.[2]

A disability insurance version of the Chetty (2008) liquidity vs. moral hazard formula would then re-write the gap in marginal utilities using comparative statics for the effect of wages, disability insurance benefits, and assets on the fraction of the population working ($\partial e/\partial w$, $\partial e/\partial b$, and $\partial e/\partial A$). From the representative agent's first order condition, we have:

$$\frac{\partial e}{\partial A} = \frac{u'(c_h) - u'(c_l)}{\psi''(e)} \leq 0 \tag{A.10}$$

$$\frac{\partial e}{\partial w} = \frac{u'(c_h)}{\psi''(e)} > 0 \tag{A.11}$$

and combining these yields:

$$\frac{\partial e}{\partial b} = -\frac{u'(c_l)}{\psi''(e)} = \frac{\partial e}{\partial A} - \frac{\partial e}{\partial w} \leq 0 \tag{A.12}$$

[2]Meyer and Mok (2013) implement this formula using the consumption based formula in Chetty (2006a).

The gap in marginal utilities can therefore be written as:

$$\frac{u'(c_l) - u'(c_h)}{u'(c_h)} = \frac{-\frac{\partial e}{\partial A}}{\frac{\partial e}{\partial w}} = \frac{-\frac{\partial e}{\partial A}}{\frac{\partial e}{\partial A} - \frac{\partial e}{\partial b}} \tag{A.13}$$

so that the formula for b^* becomes:

$$\frac{dW}{db} = \frac{-\frac{\partial e}{\partial A}}{\frac{\partial e}{\partial A} - \frac{\partial e}{\partial b}} + \varepsilon_{e,b}\frac{1}{1-e} \tag{A.14}$$

which only depends on the extensive margin labor supply responses to unconditional transfers (A) and state contingent benefits (b).

A.3 Optimal social insurance in a behavioral model

In this appendix, I extend the standard optimal social insurance formula to an example where the agent's decision utility differs from his experienced utility, similar to the models in Feldstein (1985), O'Donoghue and Rabin (2006), or Spinnewijn (2014). This creates an additional term that is akin to a fiscal externality, but here is an "internality" in the terminology of Allcott, Mullainathan, and Taubinsky (2014) and Alcott and Taubinsky (2014).

Suppose that there are two consumption goods, x and y, so that $c_h = (x_h, y_h)$ and $c_l = (x_l, y_l)$, where the price of y is normalized to 1 and the price of x is p (which is fixed). I consider the case where both decision and experienced utility over consumption are additive. Further, I only allow the agent's utility function for decision making and his experienced utility function to diverge with respect to his subutility function for x. Experienced utility from consumption of x and y is

$$u(x,y) = \phi(x) + v(y) \tag{A.15}$$

while decision utility is

$$\widehat{u}(x,y) = \widehat{\phi}(x) + v(y) \tag{A.16}$$

For example, y could be consumption today and x consumption tomorrow. Agents may be impatient, with $\widehat{\phi}(x) = \beta\phi(x)$ and $\beta < 1$, so that the agent puts too little weight on future consumption. Alternatively, x could be a good that has future adverse health consequences that are underweighted, as in O'Donoghue and Rabin (2006).

The representative agent chooses e and consumption to maximize his decision utility:

$$\widehat{V}(b,t) = \max_{e,x_l,x_h} e\{\widehat{\phi}(x_h) + v(A + w - t - px_h)\} + (1-e)\{\widehat{\phi}(x_l) + v(A + b - px_l)\} - \psi(e) \tag{A.17}$$

There are three first order conditions:

$$\widehat{\phi}(x_h) + v(y_l) - \widehat{\phi}(x_l) - v(y_l) - \psi'(e) = 0 \tag{A.18}$$

$$\widehat{\phi}'(x_h) - pv'(y_h) = 0 \tag{A.19}$$

$$\widehat{\phi}'(x_l) - pv'(y_l) = 0 \tag{A.20}$$

The government chooses b and t to maximize the representative agent's experienced utility $V(b,t)$, but is constrained by his choices derived from his decision utility. Its problem is:

$$\max_{b,t} V(b,t) = e\{\phi(x_h) + v(A + w - t - px_h)\} + (1-e)\{\phi(x_l) + v(A + b - px_l)\} - \psi(e)\} \tag{A.21}$$

subject to A.18, A.19, A.20, and the balanced budget constraint

$$t(b) = \frac{e}{1-e}b \tag{A.22}$$

At the optimum, a small change in b and t must leave experienced utility unchanged:

$$\frac{dV}{db} = 0 \tag{A.23}$$

which implies that:

$$\frac{\partial V}{\partial b} + \frac{\partial V}{\partial t}\frac{dt}{db} + \frac{\partial V}{\partial x_h}\frac{dx_h}{dt}\frac{dt}{db} + \frac{\partial V}{\partial x_l}\frac{dx_l}{db} + \frac{\partial V}{\partial e}\frac{de}{db} = 0 \tag{A.24}$$

$$\Rightarrow \frac{\partial V}{\partial b} + \frac{dt}{db}\left[\frac{\partial V}{\partial t} + \frac{\partial V}{\partial x_h}\frac{dx_h}{dt}\right] + \frac{\partial V}{\partial x_l}\frac{dx_l}{db} + \frac{\partial V}{\partial e}\frac{de}{db} = 0 \tag{A.25}$$

where the last three terms in the first line cannot be dropped, since e, x_l, and x_h were chosen to maximize decision utility and not experienced utility $V(b,t)$. Note that the third term in the first line would be zero if the tax were held fixed, because b only affects resources available when not working. But the tax increase needed to finance the higher b will change x_h and this will lead to a first order effect on welfare since x_h is not at the optimum.

In the next section of this appendix, I rewrite each of these terms using the agent's first order conditions from maximizing his decision utility, leading to the following expression for optimal DI benefit levels:

$$\frac{dV}{db}\frac{1}{(1-e)\,v'(y_h)} = \frac{\varepsilon_{e,b}}{1-e}(1+\Omega_2) + \frac{-\frac{\partial e}{\partial A}}{\frac{\partial e}{\partial A}-\frac{\partial e}{\partial b}} + \Omega_1 \tag{A.26}$$

where

$$\Omega_1 = \frac{[\phi'(x_l)-\widehat{\phi}'(x_l)]\frac{dx_l}{dA}\frac{1}{(1-e)}}{v'(y_h)} - \frac{[\phi'(x_h)-\widehat{\phi}'(x_h)]\frac{dx_h}{dA}\frac{1}{e}}{v'(y_h)} \tag{A.27}$$

and

$$\Omega_2 = \frac{[\phi'(x_h)-\widehat{\phi}'(x_h)]\frac{dx_h}{dA}\frac{1}{e}}{v'(y_h)} + \frac{[\{\phi(x_h)-\widehat{\phi}(x_h)\}-\{\phi(x_l)-\widehat{\phi}(x_l)\}]}{v'(y_h)}\frac{e}{b} \tag{A.28}$$

This equation shows that the Chetty (2008) moral hazard versus liquidity formula still holds if we add these correction factors. The $\phi'-\widehat{\phi}'$ terms are the difference between the social and perceived private marginal value of consumption of x, while $\frac{dx}{dA}$ is the marginal propensity to consume x out of income. Note that if $\phi=\widehat{\phi}$, then $\Omega_1=\Omega_2=0$, and the formula collapses to the standard formula.

A.4 Proofs

Note that

$$\frac{\partial V}{\partial x_l}\frac{dx_l}{db} = [\phi'(x_l) - pv'(y_l)]\frac{dx_l}{db} \tag{A.29}$$

$$= [\phi'(x_l) - \widehat{\phi}'(x_l)]\frac{dx_l}{db} \tag{A.30}$$

$$= [\phi'(x_l) - \widehat{\phi}'(x_l)]\frac{dx_l}{dA} \tag{A.31}$$

$$\frac{\partial V}{\partial x_h}\frac{dx_h}{dt}\frac{dt}{db} = [\phi'(x_h) - pv'(y_h)]\frac{dx_h}{dt}\frac{dt}{db} \tag{A.32}$$

$$= [\phi'(x_h) - \widehat{\phi}'(x_h)]\frac{dx_h}{dt}\frac{dt}{db} \tag{A.33}$$

$$= -[\phi'(x_h) - \widehat{\phi}'(x_h)]\frac{dx_h}{dA}\frac{dt}{db} \tag{A.34}$$

where I substitute for $pv'(y_l)$ and $pv'(y_h)$ using A.19 and A.20,. This term is a function of the degree to which the agent's marginal utility from consuming another unit of x differs across the decision utility function and experienced utility function. If these two functions simply differ by a constant (e.g., $\widehat{\phi}(x) = \phi(x) + k$), then this term is zero. Now for the last behavioral term:

$$\frac{\partial V}{\partial e}\frac{de}{db} = [\phi(x_h) + v(y_l) - \phi(x_l) - v(y_l) - \psi'(e)]\frac{de}{db} \tag{A.35}$$

$$= [\{\phi(x_h) - \widehat{\phi}(x_h)\} - \{\phi(x_l) - \widehat{\phi}(x_l)\}]\frac{de}{db} \tag{A.36}$$

substituting in for $\psi'(e)$ from A.18. This condition measures how the level of utility from consuming x differs across the decision utility function and experienced utility function, which drives a wedge between his actual choice of e and the choice of e that he wishes he would have chosen. Again, if two functions simply differ by a constant, then this term is zero (as long as the constant is same whether he works or not).

The two remaining terms are the direct effect of increasing benefits on welfare:

$$\frac{\partial V}{\partial b} = (1 - e)v'(y_l) \tag{A.37}$$

and the direct effect of the required tax increase on welfare:

$$\begin{aligned}
\frac{\partial V}{\partial t}\frac{dt}{db} &= [-ev'(y_h)]\frac{dt}{db} && \text{(A.38)}\\
&= [-ev'(y_h)]\frac{1-e}{e}\{1-\varepsilon_{e,b}\frac{1}{1-e}\} && \text{(A.39)}\\
&= -(1-e)\,v'(y_h)\{1-\varepsilon_{e,b}\frac{1}{1-e}\} && \text{(A.40)}
\end{aligned}$$

using the balanced budget constraint for $\frac{dt}{db}$. These are the standard terms which would combine to form an exact formula for optimal benefit levels. Notice that these two terms exclusively depend on v', the marginal utility of y, and not on the marginal utility of x. This is a special case of Chetty's (2006b) result that any subcomponent of the consumption vector can be used to infer the consumption smoothing benefits of social insurance.

Simply re-writing these terms leads to:

$$\frac{dV}{db} = -(1-e)\,v'(y_h)\{1-\varepsilon_{e,b}\frac{1}{1-e}\} + (1-e)v'(y_l) + (1-e)\,v'(y_h)\Omega \qquad \text{(A.41)}$$

where the behavioral terms are included in $\Omega = (\frac{\partial V}{\partial e}\frac{de}{db} + \frac{\partial V}{\partial x_l}\frac{dx_l}{db} + \frac{\partial V}{\partial x_h}\frac{dx_h}{dt}\frac{dt}{db}) / (1-e)\,v'(y_h)$.

I can show using A.18, A.19, and A.20 that the Chetty (2008) decomposition of the gap in marginal utilities still holds with the subutility function for y replacing $u(c)$. Recall the representative agent's first order condition for e:

$$\widehat{\phi}(x_h) + v(A+w-t-px_h) - \widehat{\phi}(x_l) - v(A+b-px_l) - \psi'(e) = 0 \qquad (IC_e)$$

Consider a change in the wage rate ($\frac{\partial x_l}{\partial w} = 0$ because w only affects the budget constraint if he works):

$$\begin{aligned}
\widehat{\phi}'(x_h)\frac{\partial x_h}{\partial w} + [\frac{\partial w}{\partial w} - p\frac{\partial x_h}{\partial w}]v'(y_h) &= \psi''(e)\frac{\partial e}{\partial w} && \text{(A.42)}\\
\Rightarrow \frac{\partial x_h}{\partial w}\left[\widehat{\phi}'(x_h) - pv'(y_h)\right] + v'(y_h) &= \psi''(e)\frac{\partial e}{\partial w} && \text{(A.43)}
\end{aligned}$$

Consider a change in the benefit (t is fixed and $\frac{\partial x_h}{\partial b} = 0$ because b only affects the budget constraint if he does not work):

$$\widehat{\phi}'(x_l)\frac{\partial x_l}{\partial b} + [\frac{\partial b}{\partial b} - p\frac{\partial x_l}{\partial b}]v'(y_l) = \psi''(e)\frac{\partial e}{\partial b} \tag{A.44}$$

$$\Rightarrow \frac{\partial x_l}{\partial b}\left[\widehat{\phi}'(x_l) - pv'(y_l)\right] + v'(y_l) = \psi''(e)\frac{\partial e}{\partial b} \tag{A.45}$$

Consider a change in assets:

$$\widehat{\phi}'(x_h)\frac{\partial x_h}{\partial A} + [1 - p\frac{\partial x_h}{\partial A}]v'(y_h) - \widehat{\phi}'(x_l)\frac{\partial x_l}{\partial A} - [1 - p\frac{\partial x_l}{\partial A}]v'(y_l) = \psi''(e)\frac{\partial e}{\partial A} \tag{A.46}$$

$$\Rightarrow \frac{\partial x_h}{\partial A}\left[\widehat{\phi}'(x_h) - pv'(y_h)\right] + \frac{\partial x_l}{\partial A}\left[\widehat{\phi}'(x_l) - pv'(y_l)\right] + v'(y_h) - v'(y_l) = \psi''(e)\frac{\partial e}{\partial A} \tag{A.47}$$

Note that the first order conditions for x_h and x_l are:

$$\widehat{\phi}'(x_h) = pv'(y_h) \tag{A.48}$$

$$\widehat{\phi}'(x_l) = pv'(y_l) \tag{A.49}$$

so that the terms in large square brackets are zero, leading to:

$$v'(y_h) = \psi''(e)\frac{\partial e}{\partial w} \Rightarrow \frac{\partial e}{\partial w} = \frac{v'(y_h)}{\psi''(e)} \tag{A.50}$$

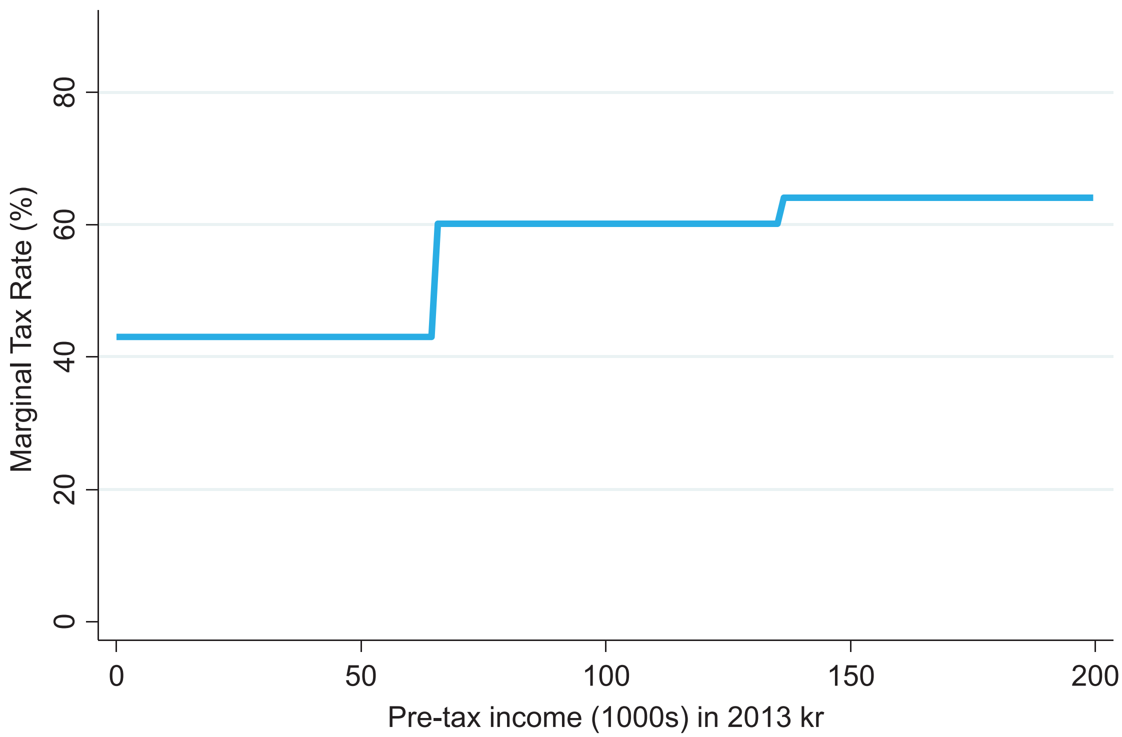
$$v'(y_l) = \psi''(e)\frac{\partial e}{\partial b} \Rightarrow \frac{\partial e}{\partial b} = \frac{v'(y_l)}{\psi''(e)} \tag{A.51}$$

Similarly, for the change in assets term:

$$v'(y_h) - v'(y_l) = \psi''(e)\frac{\partial e}{\partial A} \Rightarrow \frac{\partial e}{\partial A} = \frac{v'(y_h) - v'(y_l)}{\psi''(e)} \tag{A.52}$$

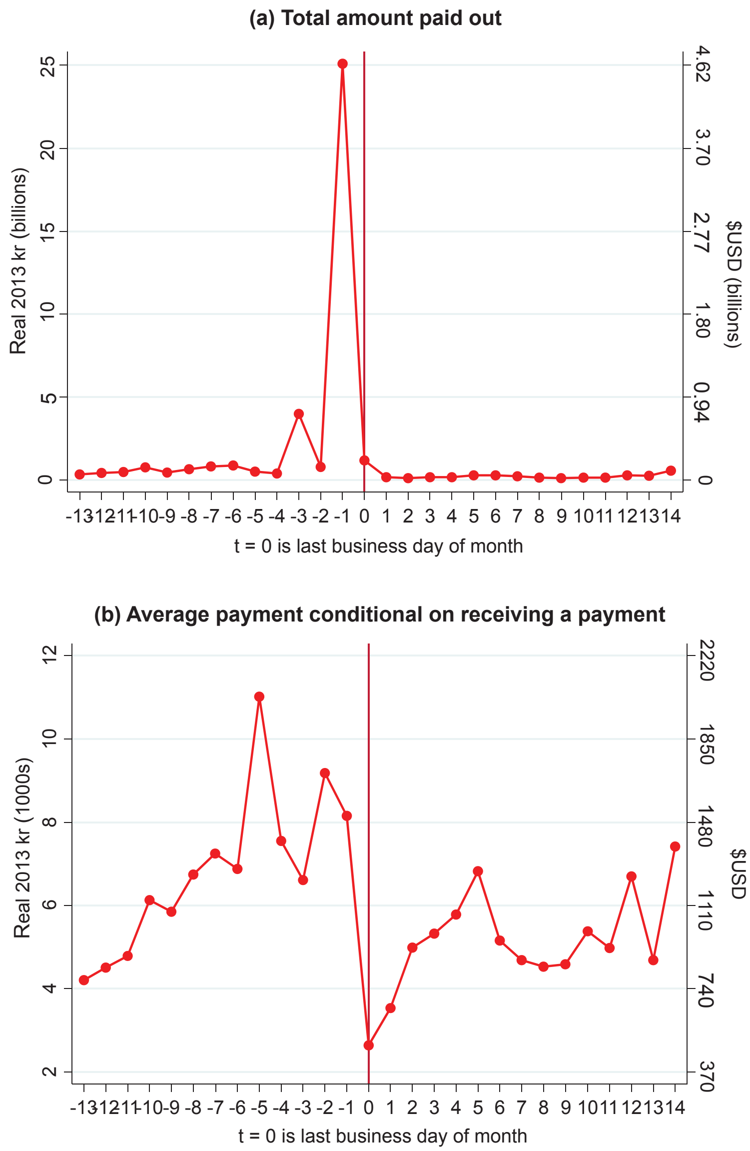
A.5 Supplementary Figures and Tables

Figure A.1: *Implied Marginal Tax Rate on Earnings in 2002*



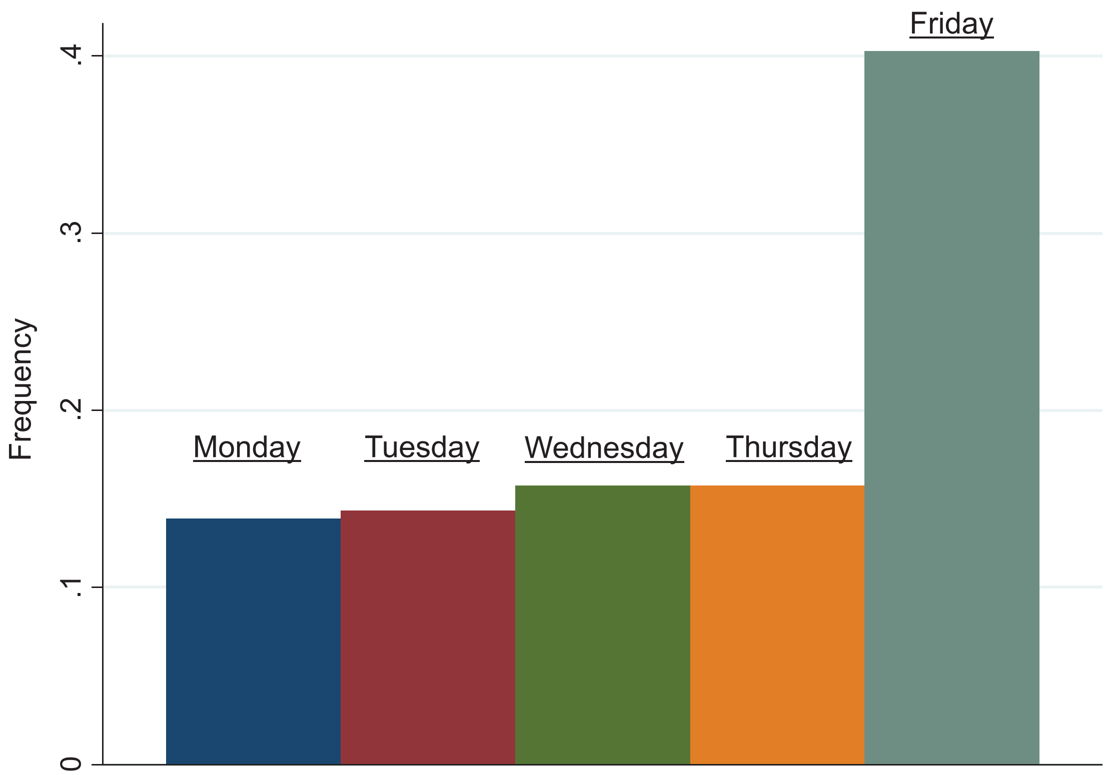
NOTE—This figure plots the marginal tax rate on labor income implied by the income tax system and phase out of disability insurance benefits for a single disability insurance beneficiary who is over 60 and has been awarded benefits at either the middle or low disability insurance level. I use the law in place in 2002, drawing on Hansen (2006, Appendix 1) and http://tax.dk/beregn/skat02.htm, assuming that the disability insurance beneficiary has no income other than that shown along the x-axis.

Figure A.2: *Event Study of Payments around Last Business Day of Month: 2009-2013*



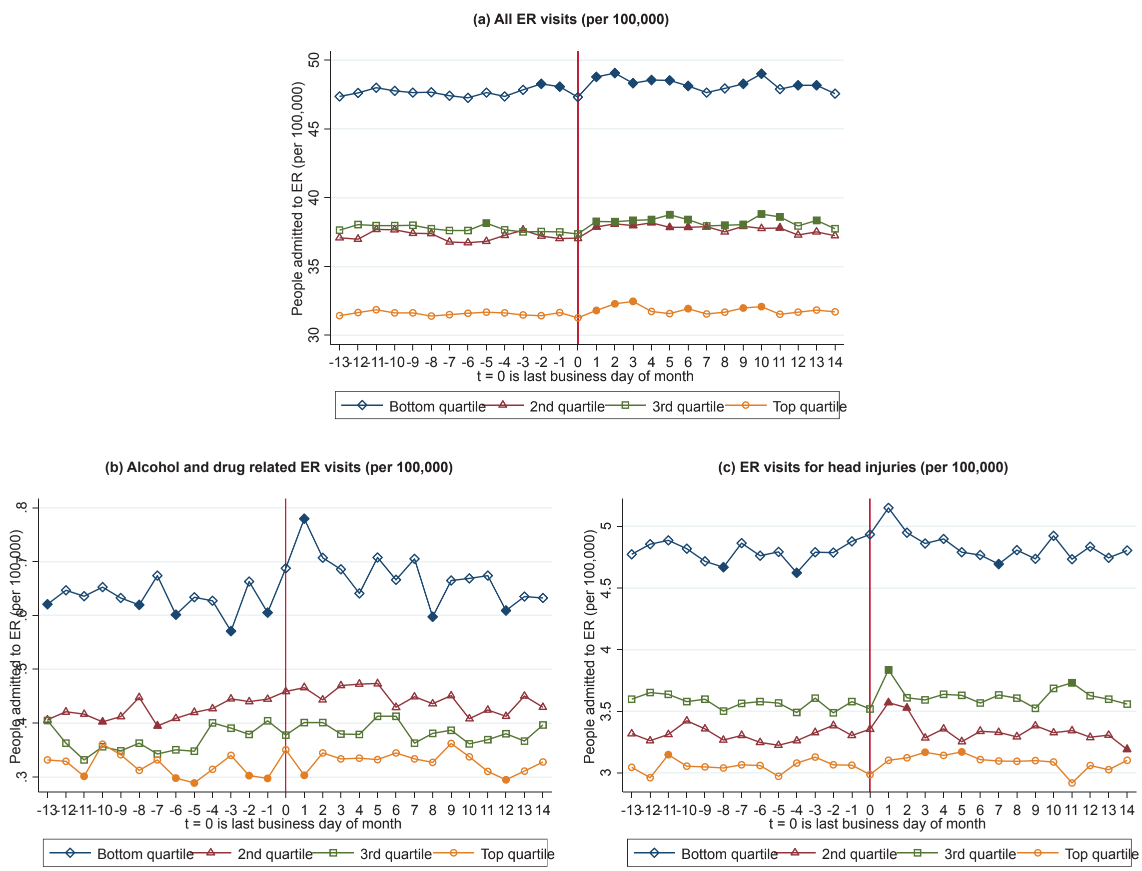
NOTE—This figure plots the dollar amount of payments received in 2009-2013 for the 28-day window around the last business day of the month ($t = 0$). Panel A plots the average daily total across all NemKonto accounts, while Panel B plots the average payment per person conditional on receiving a payment. The left y-axis plots these amounts in real 2013 kroner. The right y-axis converts to USD using the 5.41 kr to $1 exchange rate in April 2014.

Figure A.3: *Frequency of Last Business Day of the Month by Weekday: 1994-2011*



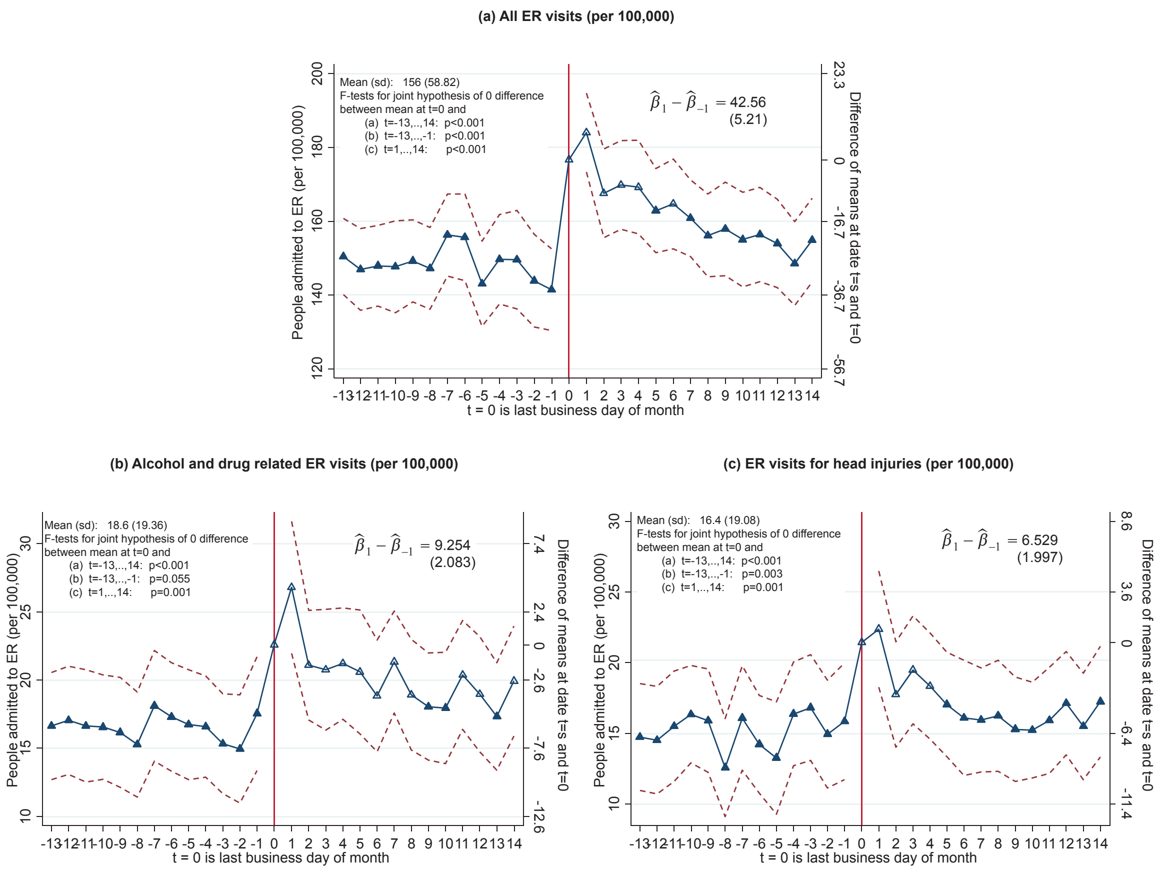
NOTE—This figure shows the days of the week on which the last business day of the month fell in 1994-2011.

Figure A.4: *Event Studies around Last Business Day of Month: Wage Earners*



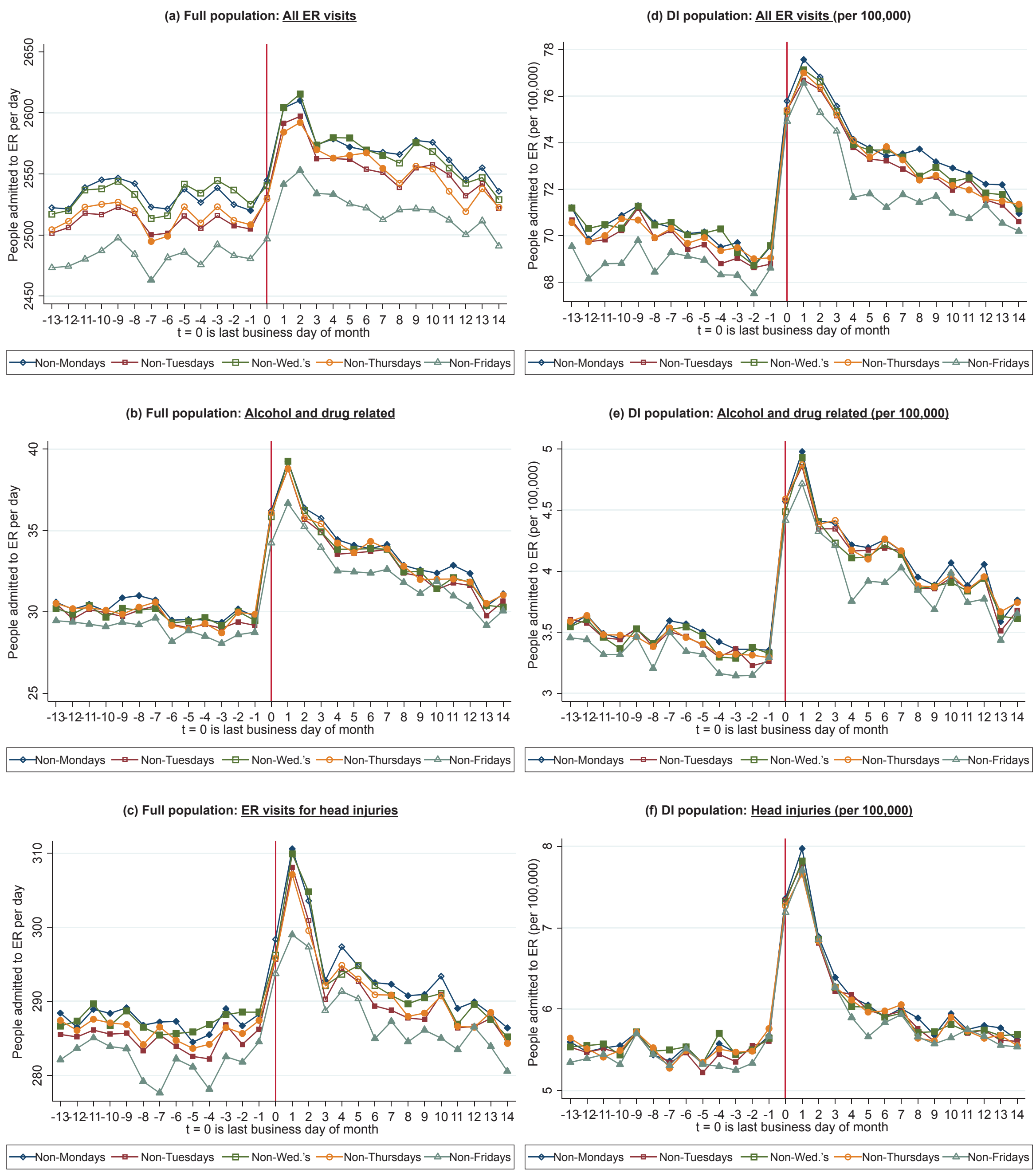
NOTE—This figure plots the average number of people per 100,000 who visit the ER per day over the 28-day window around the last business day of the month ($t = 0$) in 1994-2011. The sample is restricted to wage earners. I divide the sample by lagged earned income quartiles. The bottom quartile is plotted in diamonds, the next highest quartile is plotted in triangles, the third highest quartile is plotted in squares, and the top quartile is plotted in circles. The figures are drawn for all ER visits (panel a), for ER visits that have at least one diagnosis code that is alcohol and drug related (panel b), and for ER visits whose primary diagnosis code is for a head injury (panel c). The average number of people in each quartile is about 477,000. Please see the notes to Figure 1.6 for more details on the construction of these figures

Figure A.5: *Event Studies by Reason Awarded DI: Drug or Alcohol Dependent*



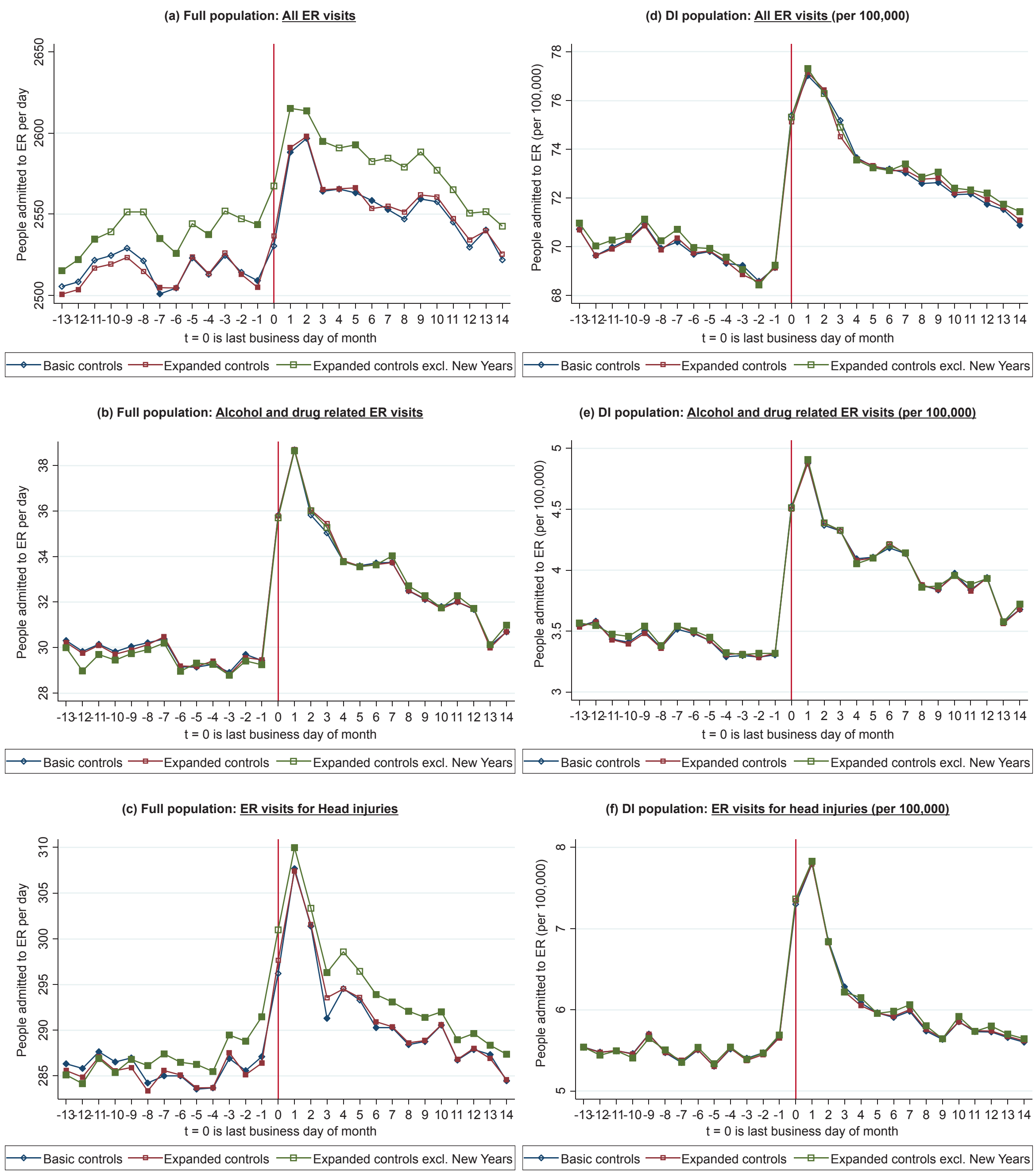
NOTE—This figure plots the average number of people per 100,000 who visit the ER per day in 1999-2013. I restrict the sample to those who were awarded disability insurance benefits and that had mention of drug or alcohol dependence in the reasons that disability was awarded. The figures are drawn for all ER visits (panel a), for ER visits that have at least one diagnosis code that is alcohol and drug related (panel b), and for ER visits whose primary diagnosis code is for a head injury (panel c). Please see the notes to Figure 1.6 for more details on the construction of these figures.

Figure A.6: *Robustness Checks: Payment Dates vs. Day of the Week Effects*



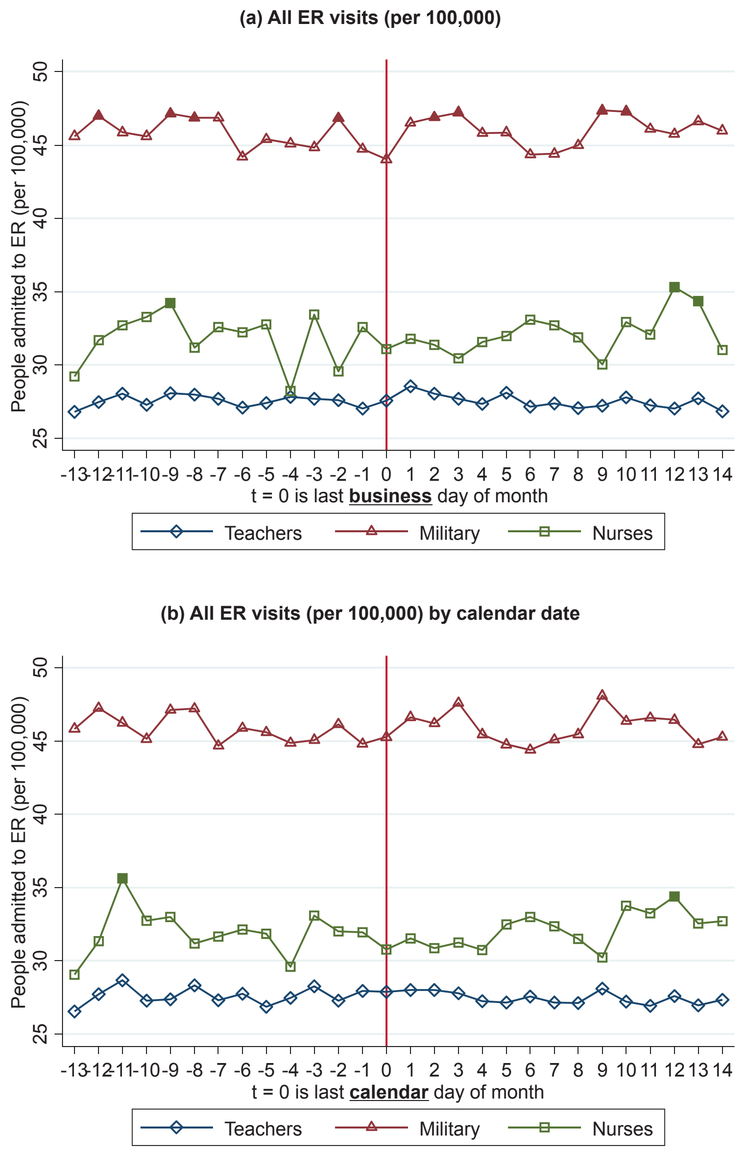
NOTE—This figure reproduces the ER visit event studies for the full population in panel (a), panels (b), and (c), and for the DI sample in panels (d), (e), and (f). In each panel, I exclude the 28-day months where $t = 0$ is a Monday (diamonds), where $t = 0$ is a Tuesday (small squares), where $t = 0$ is a Wednesday (larger squares), where $t = 0$ is a Thursday (circles), and where $t = 0$ is a Friday (triangles).

Figure A.7: *Robustness Checks: Impact of Additional Controls*



NOTE—This figure reproduces the ER visit event studies for the full population in panel (a), panels (b), and (c), and for the DI sample in panels (d), (e), and (f). In each panel, I plot the coefficients from a regression of the basic specifications in those earlier figures (in diamonds), the coefficients from a regression where I add, as additional controls, interactions of the month and year controls as well as indicators for the day before and day after the special days listed in Appendix Table A1 (in small squares), and the coefficients from a regression that includes these additional controls and excludes all the 28-day months where $t = 0$ occurs right before New Years (in larger squares).

Figure A.8: *Event Studies for Wage Earners by Occupation*



NOTE—This figure reproduces the wage earner event studies of ER visits by occupation code in the previous year. Panel (a) defines event time relative to the last business day of the month. Panel (b) defines event time relative to the last calendar day of the month. Teachers include ISCO codes 234100 and 234120 in 2010 and 23300, 23310, 23311, 23312 in 1993-2009. Military include ISCO code 011000 in 1993-2009 and 010000, 011000, 020000, 021000, 030000, 031000 in 2010. Nurses include ISCO codes starting with 2230 in 1993-2009 and 222 in 2010. The average number of teachers in the sample is 57,499, the average number of members of the military is 20,374, and the average number of nurses is 10,626

Table A.1: *Examples from Literature on Effect of Disability Insurance on Labor Supply*

Focus:	Entry into the program	Rejected and accepted applicants	Policies affecting those receiving DI
	(1)	(2)	(3)
	Parsons 1980	Bound 1989	Maestas and Yin 2008
	Gruber and Kubik 1997, 2002	Parsons 1991	Maestas and Song 2011
	Gruber 2000 *	Chen and van der Klaauw 2008	Marie and Castello 2012 *
	Black, Daniel and Sanders 2002	von Wachter, Song, and Manchester 2011	Kostøl and Mogstad 2014 *
	Autor and Duggan 2003, 2007, 2008	Autor, Maestas, Mullen and Strand 2011	Borghans, Gielen, and Luttmer 2014 *
	Campolieti 2004 *	Maestas, Mullen, and Strand 2013	Moore 2014
	Duggan, Singelton and Song 2007	French and Song 2014	
	Duggan and Imberman 2009		
	Mueller, Rothstein, and von Wachter 2014		

NOTE -- Table provides examples of papers on disability insurance and labor supply. There are many other important contributions not listed; see Bound and Burkhauser (1999) for additional references. The papers are divided into three categories. The first category estimates the effect of program parameters and other factors, such as economic conditions, on entry into the program. The second category follows Bound (1989) in using the labor supply of rejected disability insurance applicants to infer the labor supply of accepted disability insurance applicants had their claims not been accepted. Parsons (1991) and Autor, Maestas, Mullen and Strand (2011) can be read as providing evidence that caution must be taken in using this approach. The third category studies the labor supply responses of those receiving disability insurance to changes in program parameters. Papers with asterisks use non-US data.

Table A.2: *Examples of Recent Evidence on Excess Sensitivity to Timing of Payments*

	Expenditures and consumption (1)	Mortality (2)	Hospitalizations (3)	Crime (4)
(a) Reoccurring payments	Stephens 2003 Shapiro 2005 Stephens 2006 * Mastrobuoni and Weinberg 2009 Hastings and Washington 2010 Stephens and Unayama 2011 * Gelman, Kariv, Shapiro, Silverman, and Tadelis 2014	Dobkin and Puller 2007 Evans and Moore 2011 Evans and Moore 2012 Andersson, Lundborg, and Vikström 2014 *	Dobkin and Puller 2007	Dobkin and Puller 2006 Foley 2011
(b) One time payments	Johnson, Parker, and Souleles 2006 Parker 2014	Evans and Moore 2011 Evans and Moore 2012	Gross and Tobacman 2014	

NOTE -- Table provides recent examples of papers showing excess sensitivity of various outcomes to timing of income payments. The table is divided into two categories: reoccurring payments (e.g., monthly SNAP benefits) and one time payments (e.g., stimulus payments). The table excludes many important papers that are related (e.g., consumption responses to predictable changes in income in Shapiro and Slemrod 1995 and Parker 1999). Evans and Moore (2012) also present evidence on various other outcomes as measures of activity (e.g., baseball game attendance). Papers with asterisks use non-US data.

Table A.3: *Control Vector in Main Specificiation*

	Description (1)	Dates (2)	Coef. (3)	S.E. (4)
Panel A. Holidays and other special days				
1.	New Years Day	January 1	657.9	(121.8)
2.	Fastelavn	February/March	-100.9	(101.5)
3.	Palm Sunday	March/April	-28.66	(104.5)
4.	Maundy Thursday	March/April	-123.7	(99.46)
5.	Good Friday	March/April	61.51	(114.3)
6.	Easter Sunday	March/April	-24.61	(100.5)
7.	Easter Monday	March/April	-143.8	(105.3)
8.	Prayer Day	April/May	56.01	(98.62)
9.	May Day	May 1	182.1	(107.8)
10.	Ascension Day	May/June	-26.00	(102.9)
11.	Whit Sunday	May/June	408.9	(116.6)
12.	Whit Monday	May/June	146.9	(111.2)
13.	Constitution Day	June 5	255	(110.0)
14.	Christmas Eve	December 24	-867.9	(75.23)
15.	Christmas	December 25	-518.4	(89.22)
16.	Day after Christmas	December 26	-482.7	(88.16)
17.	New Years Eve	December 31	-762.2	(69.66)
18. and 19.	Midsommerfesten	June 24	221.2	(113.5)
		June 25	367.8	(127.9)
20. and 21.	Longest night of the year	December 13	-109.6	(113.2)
		December 14	-184	(92.28)
	Constant		2,533	(22.25)
	F test: p<0.001			
Panel B. Days of week				
1.	Monday		2,675	(23.71)
2.	Tuesday		2,503	(22.75)
3.	Wednesday		2,482	(22.54)
4.	Thursday		2,480	(22.51)
5.	Friday		2,506	(22.33)
6.	Saturday		2,479	(23.06)
7.	Sunday		2,590	(24.09)
	F test for equality of means: p<0.001			

Notes: Table lists the controls for reoccuring special days and days of week that are included in the main specification. Columns 3 reports coefficients from a regression of the number of people visiting the ER on the indicator variables listed in that panel. The regression in panel B excludes the constant. Column 4 reports Newey-West standard errors with 15 lags. There are T=6,574 observations in each regression.

Appendix B

Appendix to Chapter 2

This appendix describes the calculations in Figure 2.3. In order to be eligible for SNAP, households who also have earned income would typically be eligible to receive the earned income tax credit (EITC) and, if they have dependent children, the refundable Additional Child Tax Credit (CTC). Panel A of the figure plots the annual combined SNAP, EITC, and CTC payments, Panel B plots the combined marginal tax rate schedule, and Panel C plots the participation tax rate for each of these households. Single adults with children and non-disabled, non-elderly single adults without children accounted for 49% of SNAP households in 2012, so the schedules shown in the figure are the relevant ones for a large fraction of SNAP households. In 2012, only 33% of these households had earned income (Farson Gray and Eslami 2014, table 3.2). The Hicksian labor supply elasticity on the intensive margin is of a similar magnitude as on the extensive margin after accounting for optimization frictions (Chetty 2012), indicating that the (long run) effects of SNAP on labor supply through marginal tax rates and participation tax rates are of similar importance.

I use the 2014 SNAP benefit schedule to calculate SNAP benefits and the NBER TAXSIM calculator to calculate personal income tax rates and tax credit amounts. Previous studies have made similar calculations that additionally incorporate state taxes and transfer programs other than SNAP but do so for earlier years (e.g., Dickert, Houser, and Scholz 1995). My calculations assume that each household has no other sources of income other than that shown along the x-axis and that the only SNAP deductions taken are the standard SNAP deduction and the earned income deduction. The figures plot income up to $50,000, but households lose eligibility for SNAP at much lower levels. These annual limits are $14,940 for the household without children and $20,172, $25,392, and $30,624 for the households with one, two, and three children.[1,2]

Panel A shows the sum of the annual SNAP, EITC, and CTC amounts for these households. The combined value of these benefits increases until reaching about $12,630 for the three child household at around $13,700 in earned income. The annual value of these benefits is much more modest for the household without children, reaching a maximum of $2,440 at earnings of $2,300. All but $496 of these benefit amounts come from SNAP for the childless household.

Panel B plots the marginal tax rates for each household, taking into account the SNAP benefit schedule, personal federal income schedule, and the payroll tax (both employer and employee portions).[3] Once earned income exceeds the sum of a household's SNAP deductions, the phase out of SNAP benefits adds 24 percentage points to the marginal tax rates that the household would have otherwise faced. Adding this additional 24 percentage points from SNAP to a household's marginal tax rate reduces its net–of–marginal tax rate by as much as 30% to 50%, depending on income levels and family composition. Chetty, Friedman, and Saez (2013) present strong evidence in the context of the EITC that the labor supply of low-income households with children can be quite responsive to changes to these marginal incentives to earn additional income. Figure 2.3(b) shows that for low incomes, households with children face negative marginal tax rates because of the subsidies provided by the EITC (34%, 40%, and 45% rates for one, two, and three children) and CTC (15% rate), which sum to more than the SNAP phase out rate. However, as incomes increase above

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[1]These limits on gross earned income are binding before the limits on income after deductions.

[2]As can be seen in Figure 1, there are notches in the household budget constraint at these incomes levels, similar to the Medicaid notch studied by Yelowitz (1995). The effect of these notches on marginal tax rates cannot be seen in Figure 2.3(b) because I plot income in $100 increments.

[3]If labor demand is perfectly elastic, then the full incidence of the payroll tax will fall on workers.

\$15,000, marginal tax rates increase up to 70% as households enter the bottom personal income tax bracket, the EITC begins to be phased out, and the CTC amount reaches its maximum of \$1000 per child. Households without children also face high rates, but only up to 57% because the EITC amount is much smaller and is phased out at a lower rate for them. Overall, this figure shows that once household income exceeds \$10,000 to \$15,000, the returns to earning additional income are very low for SNAP households.

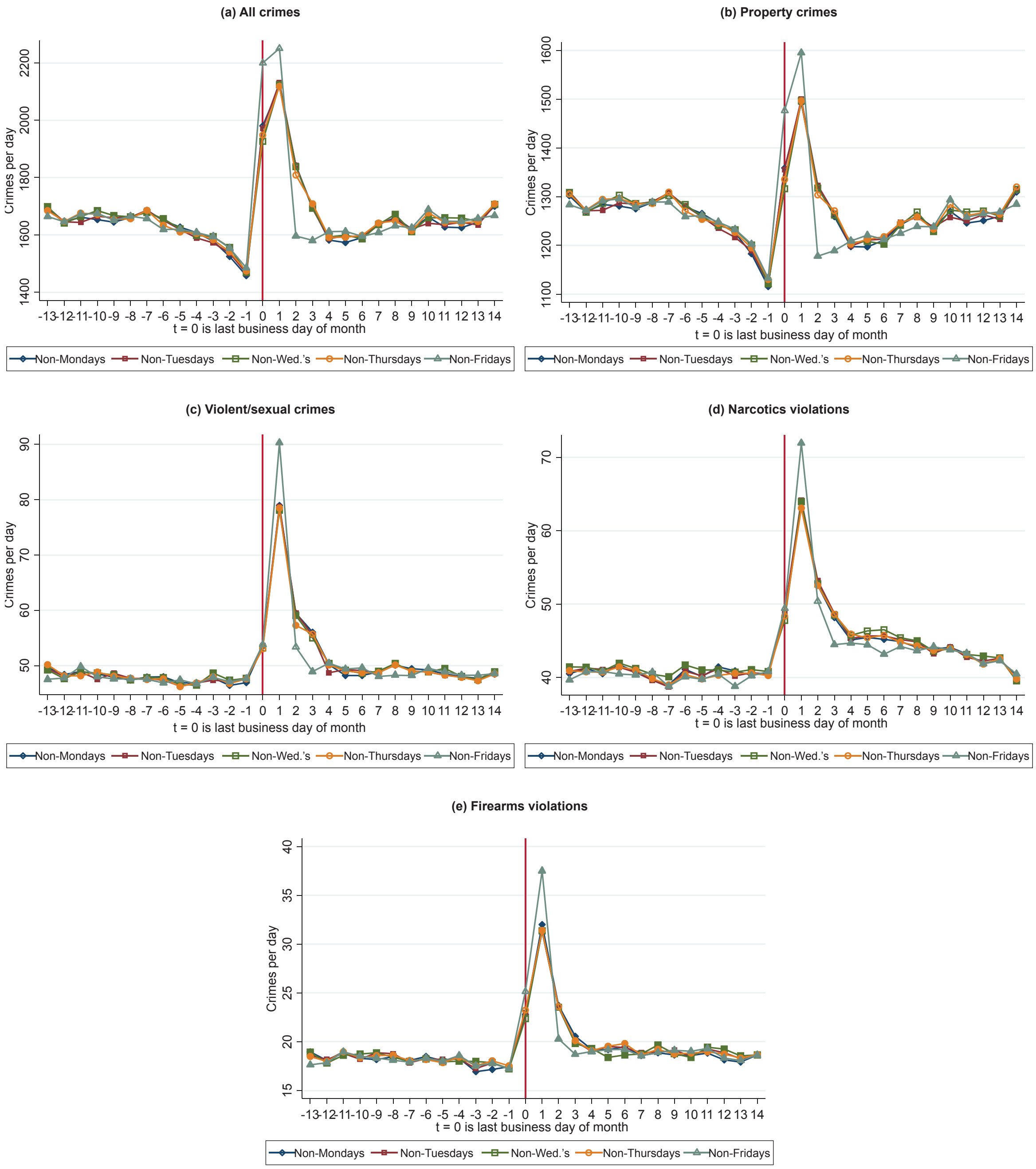
Panel C plots the participation tax rate, which is defined as one minus the financial gains to entering the labor force as a fraction of pre-tax income: $1 - ($after tax income $+$ SNAP benefits at that income level $-$ SNAP benefits with zero income$)/$pre-tax income. While the marginal tax rate is relevant for a household's decision to earn an extra dollar, the participation tax rate is the relevant measure for the decision of whether or not to enter the labor force at all. The figure shows that the participation tax rates for all four households are always less than 100%, so that despite the reduced SNAP benefits each household would still have higher net income by entering the labor force. However, households may lose other benefits such as Medicaid and housing vouchers that would increase the participation tax rates beyond those shown in the figure. Further, in addition to the disutility of working, there are fixed costs to entering the labor force (e.g., child care costs, haircuts) so that any reduction to the financial gains to working will lead some households to optimally choose not to work.

Panel C of the figure shows that these participation effects of SNAP are likely most severe for childless SNAP beneficiaries. The participation tax rates start at negative rates for low levels of earnings for the households with children because the EITC and CTC more than make up for the lower SNAP benefits and taxes that these households would owe. However, the rates are always positive for childless households, increasing from 7.65% to 33% when the household loses eligibility for SNAP at around \$15,000, before leveling off at 30%. Therefore, a job paying \$15,000 would only increase the net income of a SNAP beneficiary without children by \$6,000 because of taxes and the lost SNAP benefits. The same job would increase the net income of the household with three children by \$17,630, which is well over the $15,000 in pretax wages, because of the tax credits the household can receive.

Appendix C

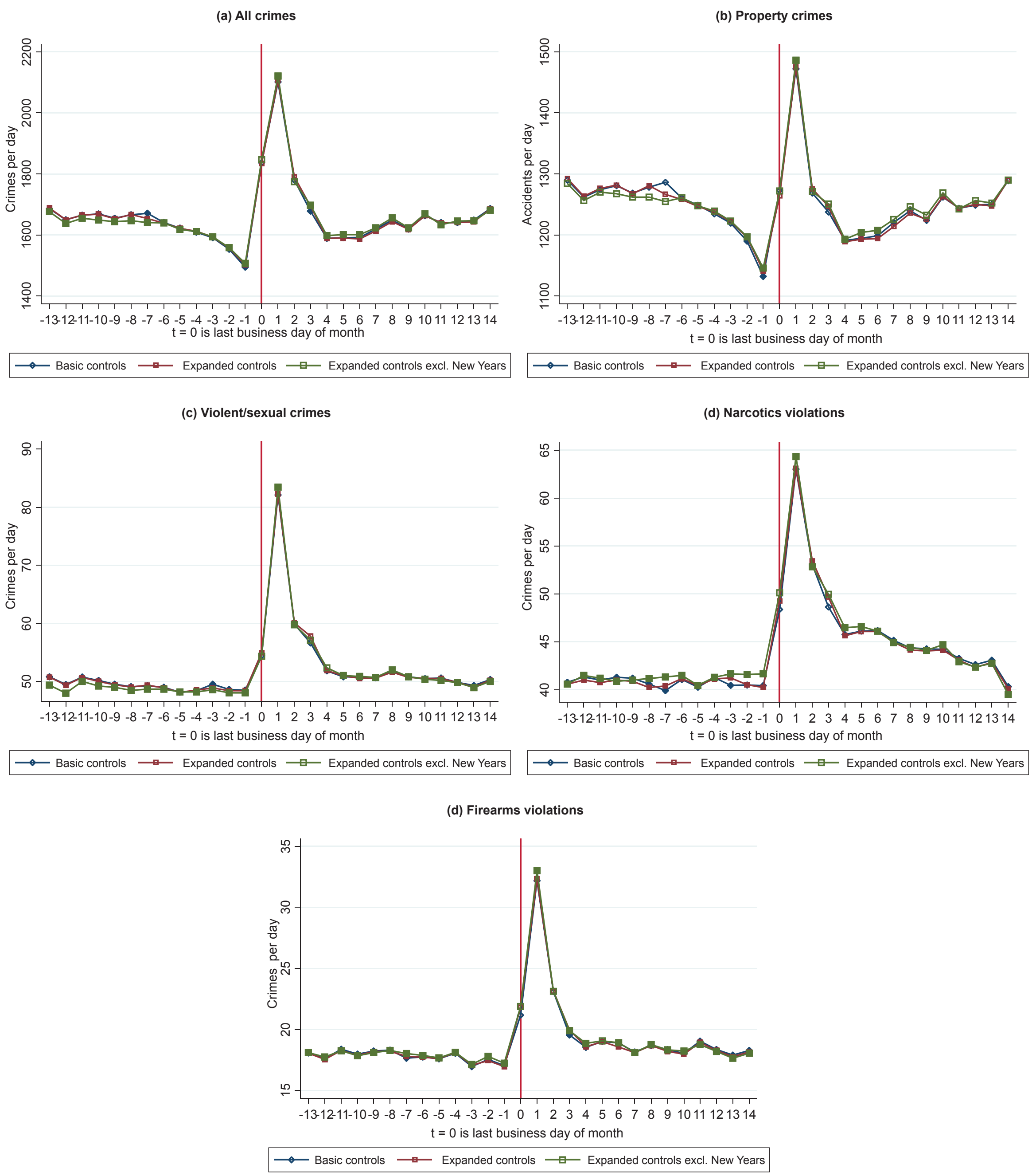
Appendix to Chapter 3

Figure C.1: *Crime Robustness Checks: Payment Dates vs. Day of the Week Effects*



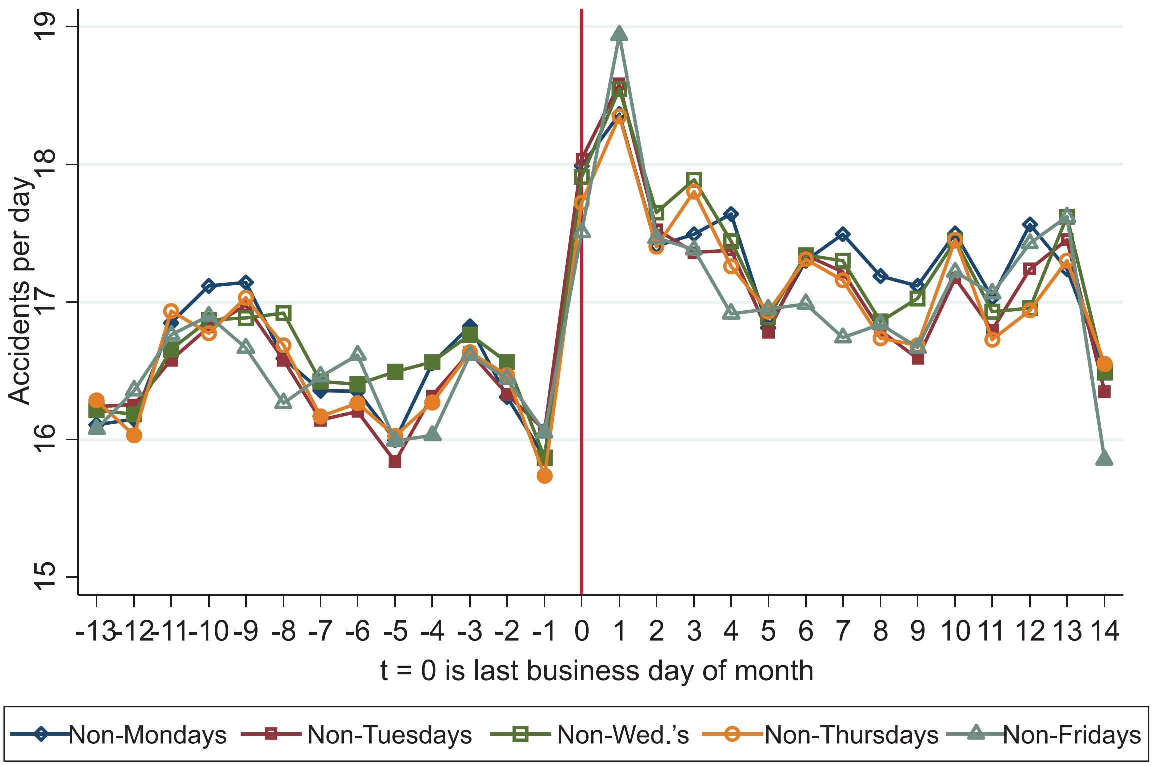
NOTE–This figure reproduces Figure 3.3. In each panel, I exclude the 28-day months where $t = 0$ is a Monday (diamonds), where $t = 0$ is a Tuesday (small squares), where $t = 0$ is a Wednesday (larger squares), where $t = 0$ is a Thursday (circles), and where $t = 0$ is a Friday (triangles).

Figure C.2: *Crime Robustness Checks: Impact of Additional Controls*



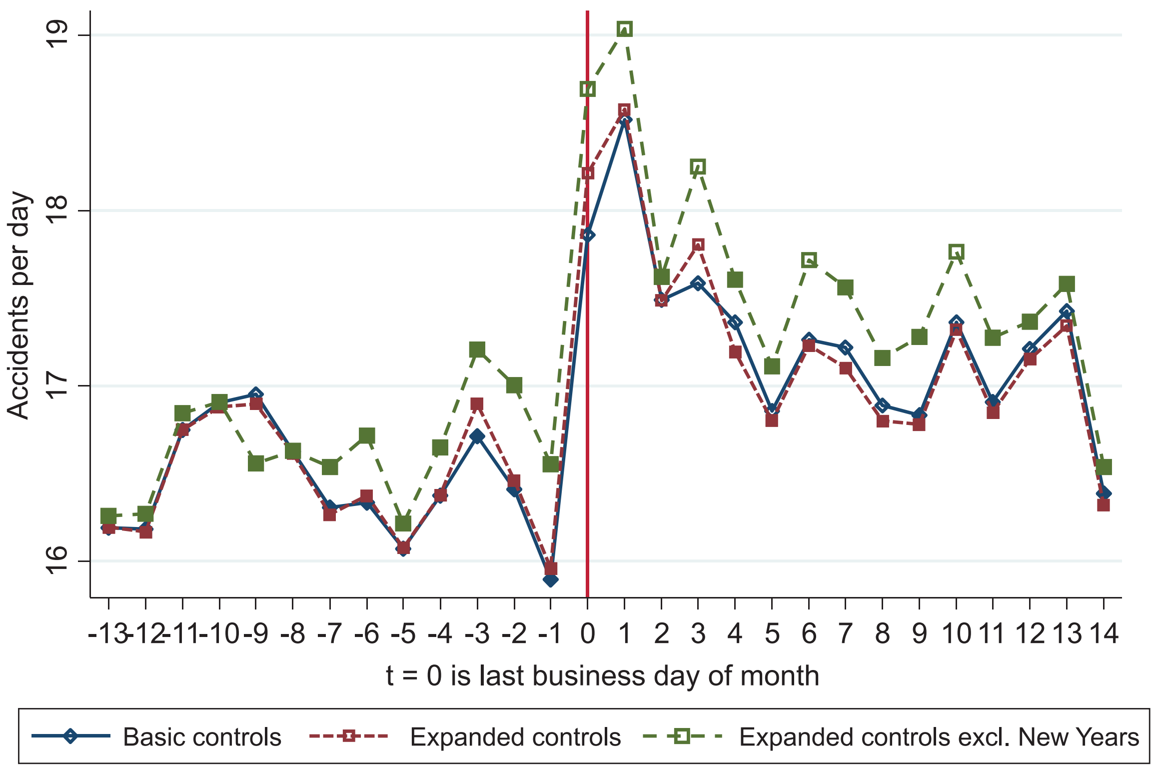
NOTE–This figure reproduces Figure 3.3. In each panel, I plot the coefficients from a regression of the basic specifications in those earlier figures (in diamonds), the coefficients from a regression where I add, as additional controls, interactions of the month and year controls as well as indicators for the day before and day after the special days listed in Appendix Table A1 (in small squares), and the coefficients from a regression that includes these additional controls and excludes all the 28-day months where $t = 0$ occurs right before New Years (in larger squares).

Figure C.3: *Accidents Robustness Checks: Payment Dates vs. Day of the Week Effects*



NOTE–This figure reproduces Figure 3.4. In each panel, I plot the coefficients from a regression of the basic specifications in those earlier figures (in diamonds), the coefficients from a regression where I add, as additional controls, interactions of the month and year controls as well as indicators for the day before and day after the special days listed in Appendix Table A1 (in small squares), and the coefficients from a regression that includes these additional controls and excludes all the 28-day months where $t = 0$ occurs right before New Years (in larger squares).

Figure C.4: *Accidents Robustness Checks: Impact of Additional Controls*



NOTE–This figure reproduces Figure 3.4. In each panel, I plot the coefficients from a regression of the basic specifications in those earlier figures (in diamonds), the coefficients from a regression where I add, as additional controls, interactions of the month and year controls as well as indicators for the day before and day after the special days listed in Appendix Table A1 (in small squares), and the coefficients from a regression that includes these additional controls and excludes all the 28-day months where $t = 0$ occurs right before New Years (in larger squares).