

## ABSTRACT

Title of Document:                   EMPIRICAL ESSAYS ON DISABILITY  
INSURANCE, EMPLOYMENT, INCOME  
AND HEALTH

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The optimal design of tax and transfer policies involves understanding how income payments affect the behavior of recipients. This dissertation contributes to the public economics literature by examining how various income payments affect employment and health. The first chapter is focused on the relationship between disability payments and employment. The other two chapters explore short-term patterns in mortality and the role of income payments, which advances our understanding of the broader relationship between income and health.

Chapter 1: The Employment Effects of Terminating Disability Benefits: Insights from  
Removing Drug and Alcohol Addictions as Disabling Conditions

A challenge in designing return-to-work policies for Social Security Disability Insurance or Supplemental Security Income disability beneficiaries is identifying who is able to work. Using administrative data, I estimate the employment effects resulting from the 1996 removal of drug and alcohol addictions as disabling conditions, which

eliminated the benefits of approximately 100,000 individuals. Terminated beneficiaries' employment increased by 20-30 percentage points, which is large relative to their work histories. The heterogeneity in employment is consistent with program participation initially increasing employment potential, before being outweighed by the negative consequences of being out of the labor force.

### Chapter 2: Liquidity, Economic Activity, and Mortality (with William N. Evans)

We document a within-month mortality cycle where deaths decline before the first day of the month and spike after the first. This cycle is present across a wide variety of causes and demographic groups. A similar cycle exists for a range of economic activities, suggesting the mortality cycle may be due to short-term variation in levels of economic activity. Our results suggest a causal pathway whereby liquidity problems reduce activity, which in turn reduces mortality. These relationships may help explain the pro-cyclical nature of mortality.

### Chapter 3: The Short-term Mortality Consequences of Income Receipt (with William N. Evans)

Researchers and retailers have documented that consumption declines before the receipt of income, and then rises afterwards. We identify a related phenomenon, where mortality rises immediately after income receipt. We find that mortality increases following the arrival of monthly Social Security payments, regular wage payments for military personnel, the 2001 tax rebates, and Alaska Permanent Fund dividend payments. The increase in short-run mortality is large, and occurs for a large number of causes of death.

EMPIRICAL ESSAYS ON DISABILITY INSURANCE, EMPLOYMENT,  
INCOME AND HEALTH

By

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Dissertation submitted to the Faculty of the Graduate School of the  
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## Dedication

To my wife, Belen, for all of her love and companionship. And to my parents, John and Jenny, for providing support and encouragement throughout my life and emphasizing the value of education.

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## Introduction

The optimal design of tax and transfer policies involves understanding how income payments affect the behavior of recipients. This dissertation contributes to the public economics literature by examining how various income payments affect employment and health. The first chapter is focused on the relationship between disability payments and employment, while the other two chapters explore short-term patterns in mortality and the role of income payments.

The federal government operates two large disability programs. The larger of these programs is Social Security Disability Insurance, which is provided to disabled workers with sufficient time in Social Security-covered employment. The other program is Supplementary Security Income, which is paid to low-income elderly, blind and disabled persons. Approximately 11 million people aged 18 to 64 years currently receive benefits from one or both programs on the basis of a disability, which is six percent of the working-age population.<sup>1</sup> Goodman and Stapleton (2007) estimate that the federal government spends approximately 12 percent of its budget on programs for working-age people with disabilities, the majority of which goes to paying the cash, medical and in-kind benefits associated with these two programs.

The relationship between these disability programs and employment is of considerable interest to both policy-makers and researchers. The number of disability beneficiaries has more than doubled over the past 25 years. Figure 0.1 shows this growth for the Social Security Disability Insurance Program, which grew from 1.8 percent of the working-age population in 1985 to 4.0 percent of the working-age population in 2009. As

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<sup>1</sup> Of these, around seven million were receiving only DI benefits, three million were receiving only SSI benefits, and one million were receiving benefits from both programs (SSA, 2011a).

can be seen in the figure, most of this growth has been among those with musculoskeletal conditions and mental impairments. These two primary impairment categories now constitute the majority of disability beneficiaries in the two programs.

Over the same period, there has been a significant decline in the labor force participation of low-skilled and older males, leading to concerns that much of the growth in disability benefits reflects their work disincentive effects. Such concerns have been reinforced by studies showing that application behavior is a function of economic factors, such as the unemployment rate and the rate at which wages are replaced by benefits (e.g. Gruber, 1999; Black, Daniel and Sanders, 2002; Autor and Duggan, 2003).

Declining exit rates out of these programs have contributed to the growth of these programs. Figure 0.2 shows exit rates from the Social Security Disability Insurance program because of retirement (reaching full retirement age and moving to Retirement and Survivors Insurance), death, and medical recovery/ disqualification. Exits due to retirement and death have declined as beneficiaries have become younger and more likely to have low-mortality conditions. Exits due to medical recovery/ disqualification have remained at around one percent per annum, except for 1997 where the exit rates more than doubled.

The first chapter examines the employment that resulted from the policy that generated this spike. A legislative change in 1996 created a situation where a large number of beneficiaries lost their benefits and had to find alternative means of support. From the early 1970s, an applicant's drug or alcohol addiction could be taken into account in determining whether or not they were entitled to disability benefits. The Congress passed legislation (P.L. 104-121) that changed this: a person with drug or

alcohol problems could still receive disability benefits, but their addiction could no longer be counted among the health problems they had. The law affected 209,000 beneficiaries, of which about half lost their benefits at the beginning of 1997.

I use tax records and other administrative data held by the Social Security Administration to investigate the employment outcomes of the majority of disability beneficiaries affected by this policy. The effects of various program and institutional characteristics are considered, including the time a person had been receiving disability benefits; whether disability status had been determined through an initial determination or the appeal process, and the economic incentives an individual faced when deciding to apply for disability benefits.

I find that the increase in terminated beneficiaries' employment is large relative to their work histories, and much higher than their exit rate prior to the terminations suggested. I also find considerable heterogeneity in the employment effects that is consistent with health improvements initially increasing beneficiaries' employment potential before being outweighed by the negative consequences of an extended period out of the labor force. The results suggest that some beneficiaries are more able to work over time, and that return-to-work policies are an important element of maximizing employment among disabled individuals.

This extends our understanding of the relationship between public income assistance and addiction, which may directly inform policies related to the sizeable minority of current beneficiaries who seem to have substance abuse problems. It also provides a rare opportunity to observe the employment of people who have been receiving disability benefits for some time before losing them.

A large literature has established that individuals from higher income groups tend to have better health, although it has been difficult to establish that this is a causal relationship where income affects health (Kitiwaga and Hauser, 1973; Backlund et al., 1999; Deaton, 2003). At the same time, there are patterns in mortality data that run counter to a positive relationship between income and health. The most prominent one is the pro-cyclicality of mortality, in which deaths increase in booms and decline in recessions (Ruhm, 2000). Understanding these contrasting relationships motivates the analysis undertaken in the second and third chapters.

In Chapter 2, we re-examine a within-month pattern in mortality, in which more people die in the first few days of the calendar month than in the last few days of the month (Christenfeld et al., 2000). Researchers have attributed the pattern to substance abuse (e.g., Rosenheck et al., 2000; Riddell and Riddell, 2006; Dobkin and Puller, 2007). We find a similar pattern in many other causes of death, and argue that this within-month mortality cycle is due short-term variation in economic activity that is due to people getting paid near the start of the month and failing to smooth their consumption between paychecks. Interestingly, the death categories that have the greatest peak-to-trough within the month are the same categories that are the most responsive to changes in the business cycle, suggesting that the mechanisms are similar for both phenomena.

Chapter 3 extends this analysis by establishing a causal connection between the arrival of income and mortality. We find that mortality increases following the arrival of monthly Social Security payments, regular wage payments for military personnel, the 2001 tax rebates, and Alaska Permanent Fund dividend payments. These increases occur for external causes like traffic accidents, as well as for causes of death known to have



activity-related triggers, like heart attacks. Increases are partly offset by declines in mortality in subsequent weeks. These findings provide a possible explanation for the patterns in mortality within the month and across the business cycle, and may explain why it is difficult to estimate the long-term relationship between income and health.

In combination, these essays focus on health and employment-related outcomes and behaviors, and how they are affected by social insurance programs and the economic activity in which individuals engage. They contribute to our current understanding of issues that can assist in the design of public transfer programs.

Figure 0.1: Social Security Disability Insurance Beneficiaries as a Fraction of the Working-age Population, 1985-2009

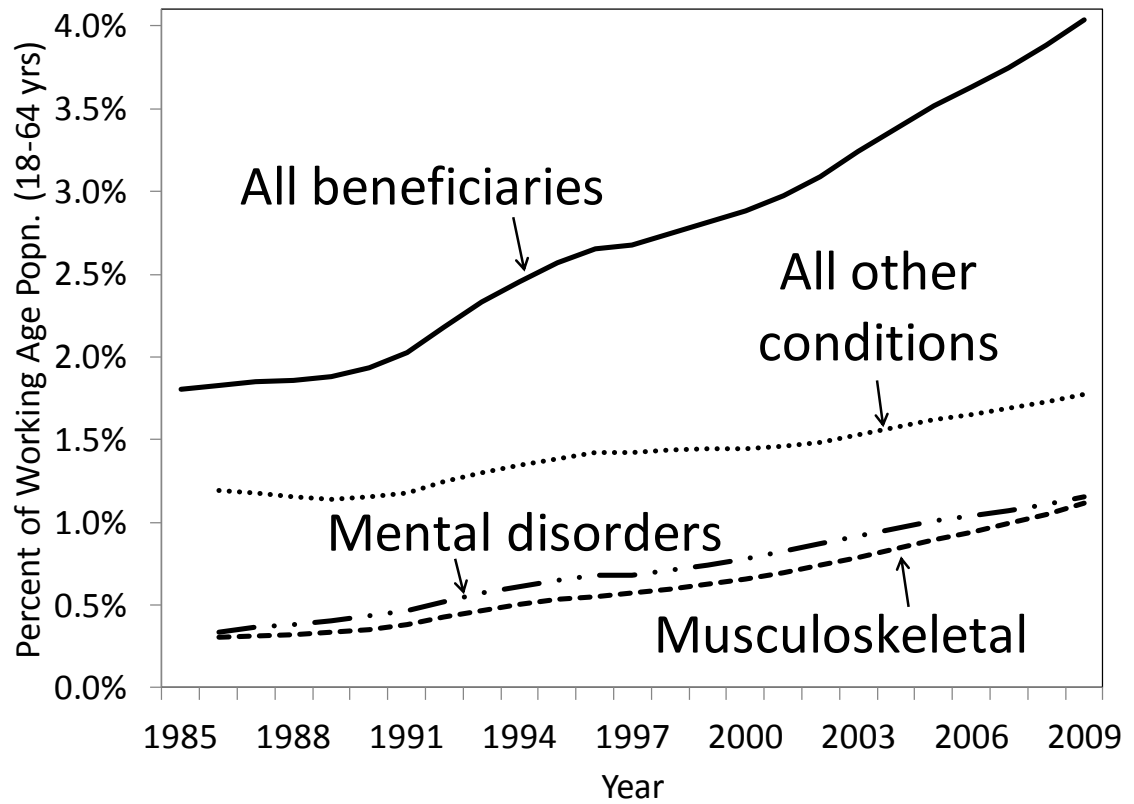
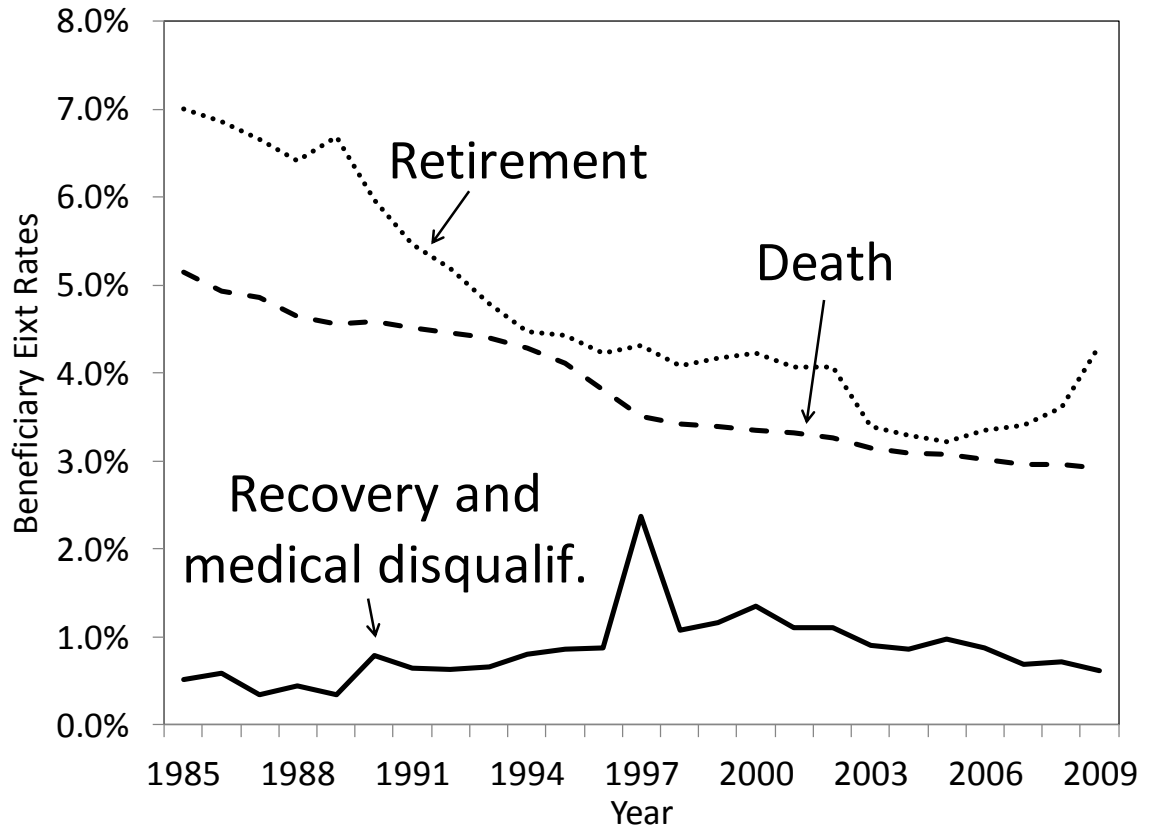


Figure 0.2: Social Security Disability Insurance Exit Rates as a Fraction of Current Beneficiaries, 1985-2009



# Chapter 1 – The Employment Effects of Terminating Disability

## Benefits: Insights from Removing Drug and Alcohol Addictions as Disabling Conditions

### 1.1 Introduction

The Social Security Administration (SSA) operates two large disability programs. Social Security Disability Insurance (DI) is provided to disabled workers with sufficient time in Social Security-covered employment, while Supplementary Security Income (SSI) is paid to low-income elderly, blind and disabled persons. Approximately six percent of people aged 18 to 64 years receive benefits from one or both programs on the basis of a disability (SSA, 2011a). This fraction has more than doubled over the past 25 years, leading to calls for a greater focus on policies that limit the use of disability benefits and increase the labor force participation of individuals with work limitations (e.g., Autor and Duggan, 2006; Drake et. al., 2009).

A growing literature has estimated how many disability beneficiaries would work if they were not eligible for these programs. Starting with Bound (1989), most of these studies have used the employment of denied disability applicants to estimate the likely employment of accepted applicants. Recent studies have looked at wider groups of beneficiaries and used quasi-experimental variation in the disability determination process to better estimate these employment effects (Chen and van der Klaauw, 2008; von Wachter, Manchester and Song, 2010; French and Song, 2011; Maestas, Mullen and Strand, 2011). The relationship between disability benefits and labor force participation

has also been estimated using variation in benefit generosity in the United States (Autor and Duggan, 2003) and Canada (Gruber, 2000), differences in disability insurance rejection rates in the United States (Gruber and Kubik, 1997), and changes in disability eligibility criteria in Austria (Staubli, 2011). All of these studies focus on employment at the time of application, and as a result they provide good estimates of how employment might change as a result of limiting entry into these disability programs.

Another way to decrease beneficiary numbers is to increase the rate at which disability beneficiaries return to work; currently, less than one percent return to the labor force each year (SSA, 2011a). In contrast to the sizeable literature focused on entry into disability programs, studies relevant to understanding exit from these programs are limited to papers that document the number and characteristics of individuals who currently give up their benefits to return to work (e.g., Hennessey, 1996; Schimmel and Stapleton, 2011). Return-to-work policies, like medical reassessments of beneficiaries through Continuing Disability Reviews and trial work periods via the Ticket to Work program, have had limited success in returning beneficiaries to the labor force partly because there is little evidence on which beneficiaries should be targeted (Autor and Duggan, 2006; SSA, 2008; SSA, 2011b).

In this paper, I examine the employment effects of a policy change that terminated the benefits of a large group of SSA disability beneficiaries. In 1996, Congress passed legislation that removed drug and alcohol addictions as disability impairments. Most of the 209,000 beneficiaries affected by this change applied to retain their benefits on the basis of other disabilities, which were most commonly mental disorders and musculoskeletal conditions. Approximately half were successful, while the rest had their

benefits terminated at the beginning of 1997. This is the only time an eligible impairment has been removed from the disability determination process.

This policy change provides a rare opportunity to observe the employment of former disability beneficiaries. Using SSA administrative data on around 85 percent of the affected beneficiaries and tax earnings records through 2008, I compare the wage earnings and employment of those who had their benefits terminated with those reclassified under a different disability, as I show that both groups have similar characteristics and pre-1996 earnings histories. Using linear probability models that include individual fixed effects and sex-specific age effects, I separately estimate the employment responses of terminated beneficiaries in two groups: those who had been receiving DI benefits and those who had been receiving only SSI benefits.

After they lost their benefits, the fraction of terminated beneficiaries with any annual earnings increased by 29 percentage points in the DI sample and 22 percentage points in the SSI sample (relative to reclassified beneficiaries). The employment effects are also assessed in terms of the 1996 “Substantial Gainful Activity” earnings threshold (\$8,339 per annum in 2010 dollars),<sup>2</sup> which is the level at which SSA would have assessed capacity for work. The fraction of terminated DI and SSI beneficiaries reporting earnings above this threshold increased by 22 and 13 percentage points, respectively. The employment effects are large relative to these individuals’ work histories, and statistically significant at the one percent level. Employment declines after four years, primarily because some individuals regain eligibility for disability benefits.

There is considerable heterogeneity in the employment response. Given the lack of previous research into the post-termination employment of disability beneficiaries, a

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<sup>2</sup> All dollars are in 2010 values, unless otherwise noted. Conversions are based on the CPI-U.

conceptual framework is developed to explore possible employment differences. Intuitively, two competing forces can change a beneficiary's ability to work over time. First, an individual's work skills generally decline with time out of the labor force, which should decrease a beneficiary's ability to work over time. On the other hand, the health of some beneficiaries may improve with time, increasing their ability to work. Medicare or Medicaid eligibility would have provided access to treatment for addiction and their other health problems, and there is evidence that both programs improve health (Card, Dobkin and Maestas, 2009; Finkelstein et al., 2011). Regular payments could have also improved health, as they were managed by third parties and could have gone to housing and other basic needs. Importantly, the size of these changes may depend on characteristics that affect the relative benefits and costs of applying for disability benefits in the first place.

In both samples, there is an inverted-U relationship between the employment response and individuals' time on disability benefits. The employment response is highest among those who received benefits for 2.5–3 years prior to termination, when it is around 40 percent larger than the employment of individuals who received benefits for nine months (the shortest period of receipt for anyone in the sample) and of individuals who received benefits for six years. These results suggest that the health of some individuals initially improved with time on benefits.

Among those who received benefits for two or more years prior to termination, the employment response is largest among the young and those who had good earnings histories before applying for disability benefits. These results are consistent with the health improvements being largest among those for whom disability payments are a poor substitute for wage earnings, which in turn suggests that forward-looking individuals took

potential health improvements into account in their decision to apply for disability benefits in the first place.

The role of health changes is further considered by comparing the employment response of those immediately awarded benefits to those initially denied benefits (before gaining eligibility on appeal). At the time they applied, those in the latter group should have been in better health and more able to work. Results for those who spent less than 1.5 years on benefits are in line with this: the employment response of individuals first denied benefits is higher than that of other beneficiaries. However, the response of those immediately awarded benefits increases sharply with time on benefits, so much so that the response among those who received benefits for two to four years is higher in the immediately awarded group than in the initially denied group. These patterns suggest that employment increases, presumably due to health improvements, were largest among those who had the poorest health when they entered these programs.

The paper makes several contributions to the literature on the relationship between disability benefits and employment. First, the employment effects estimated here complement existing studies of the labor disincentive effects of disability benefits. The similarity of the results across the DI and SSI samples is informative because most studies have focused on the DI program. Second, the findings can improve the targeting efficiency of return-to-work efforts. Third, the findings highlight the importance of considering dynamic effects when evaluating the likely employment of current beneficiaries. Judgments about the severity of disabilities may not hold over time, particularly if applicants with the worst health improve the most while receiving disability benefits. These dynamic effects, and the relatively high employment among



those receiving benefits for two to three years, also raise questions about whether temporary benefits are appropriate for some individuals. In efforts to stem the growth of these programs, temporary awards may lead to better employment outcomes than more restrictive eligibility criteria.

The findings are most relevant for understanding the employment potential of the approximately 20 percent of current DI and SSI beneficiaries who have a history of substance abuse problems.<sup>3</sup> They are also especially relevant for the approximately half of current beneficiaries whose primary impairment is a mental disorder or musculoskeletal condition, as they were the main types of impairments that individuals in my sample asked to be reclassified under.

The next section provides background on SSA disability programs and the policies that led to the termination of these beneficiaries. I then describe the data and the sample used in Section 1.2, present estimates of the employment response in Section 1.3, and examine heterogeneity in the employment response in Section 1.4. In Section 1.5, I discuss the implications of this work.

## 1.2 Program Background

Social Security Disability Insurance (DI) provides disability coverage to workers who have been in Social Security-covered employment for at least 20 of the previous 40

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<sup>3</sup> Respondents to the NDSUH are asked about SSI receipt and Medicare eligibility, which is a reasonable proxy for having received DI for two or more years when the respondent is under 65 years. Among 22-64 year old respondents to the 2007 NSDUH, 21 percent of SSI and 19 percent of Medicare beneficiaries had substance abuse problems in the previous 12 months and/or had ever received substance abuse treatment. Author's population-weighted tabulations of the public-use data file (Substance Abuse and Mental Health Services Administration, 2009).

quarters.<sup>4</sup> DI payments are based on beneficiaries' past earnings and a progressive formula that replaces a larger share of the earnings of low wage workers. The average monthly benefit in 2010 was \$1,068. After a two-year waiting period, DI beneficiaries are entitled to Medicare benefits.

Supplemental Security Income (SSI) is a program for people with limited income and resources who are disabled, blind, or 65 years and older. SSI is not based on a person's work history; instead, there are asset requirements that limit eligibility to people who, apart from a home and a car, have no more than a few thousand dollars in assets. The standard monthly federal benefit rate was \$674 in 2010, and most states supplement this with additional cash payments. SSI beneficiaries receive Medicaid coverage and are entitled to receive food stamps.

Most people who are eligible for both programs receive only DI, with two exceptions. First, some newly awarded DI beneficiaries claim SSI during a waiting period, which is at most five months (SSI has no waiting period). Second, some workers have a sufficient earnings history to qualify for DI but have calculated payments that are lower than the SSI rate. These people receive a combination of DI and SSI payments that are equivalent to the SSI benefit rate.

The application process and medical criteria are the same for both programs. Disability applications are first checked to make sure they meet the assets and earnings requirements for one or both programs. An applicant must also have earnings less than the amount that constitutes "Substantial Gainful Activity" (SGA), which was \$1,000 per month in 2010. If these criteria are met, the application is forwarded to the state's

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<sup>4</sup> There are exceptions and additional conditions. More information about these and other program details are available at [www.ssa.gov](http://www.ssa.gov) in the *Annual Statistical Supplement to the Social Security Bulletin, 2010* (SSA, 2011a).

Disability Determination Service to assess the medical severity of the applicant's disability. If it meets or exceeds the criteria for a condition on the official "Listing of Impairments," then a disability award is made. If not, disability examiners base their decision on an applicant's ability to work and an award is made if they determine the applicant cannot work at SGA levels. Denied applicants have several levels of appeal.

Beneficiaries in either program cannot earn more than SGA unless they are participating in a trial work period. SSI beneficiaries also have their benefits reduced by one dollar for every two dollars of earnings greater than \$65 per month. In practice, few beneficiaries have earnings that approach these limits. In December 2009, 0.5 percent of DI beneficiaries had benefits withheld because of substantial work and 2.6 percent of disabled SSI recipients had benefits withheld because of earnings above \$65 per month (SSA, 2011a).

### 1.2.1 The Drug Addiction and Alcoholism Category

The legislation passed in 1972 to create the SSI program allowed an applicant's drug or alcohol addiction to be considered when assessing whether a person was disabled. Those with severe addictions could potentially obtain benefits on that basis alone, while addictions could also be included as a contributing factor for applicants with other disabilities. The legislation mandated that these "Drug Addict and Alcoholic" (DA&A) beneficiaries participate in treatment (if appropriate and available) and be paid through

responsible agents who could manage their money for them (called “representative payees”). The same rules applied to DI applicants.<sup>5</sup>

There were initially 11,200 DA&A beneficiaries moved from the state-based “Aid to the Disabled” programs, which SSI replaced in 1974. Numbers were stable until the late 1980s, then increased sharply. For example, SSI DA&A numbers grew from 17,000 in 1989 to 100,000 in 1994 (Stapleton et al., 1998). Exit rates were low: of 20,000 DA&A individuals who entered SSI in 1990, less than one percent had exited because of medical improvement by 1994 (Department of Health and Human Services, 1994). In response to this, Congress passed changes to the program in August 1994 that included a three-year limit on receiving DA&A benefits and state-level contracts with agencies to manage drug treatment. Numbers continued to grow and, before most of these changes had been implemented, the *Contract with America Advancement Act* (P.L. 104-121) was passed on March 29, 1996 that terminated the DA&A eligibility criteria. No more awards could be made using DA&A criteria, and the payment of DA&A-related benefits would cease on January 1, 1997. Current beneficiaries could apply to be reclassified under co-occurring disabilities and, if successful, would continue to receive benefits (Hunt and Baumohl, 2003).

Key dates associated with this policy are shown in Table 1.1. In May and June 1996, SSA sent termination notices to 209,000 beneficiaries, or 2.6 percent of the adult disability beneficiary population at the time. Beneficiaries were asked to request a reclassification by July 29, 1996; those who did so generally had their case decided by the end of 1996 (Stapleton et al., 1998).

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<sup>5</sup> There was initially some confusion about the medical criteria. A 1982 SSA policy (*SSR 82-60: Titles II and XVI: Evaluation of Drug Addiction and Alcoholism*) clarified that the standard was the same across SSI and DI.

According to Stapleton et al. (1998), a report based on SSA data for all 209,000 beneficiaries, 57 percent were receiving only SSI, 22 percent were receiving both SSI and DI, and 21 percent were receiving only DI.<sup>6</sup> They were predominantly male (73 percent); 45 percent were white and 37 percent were black; and their average age was 43 years. Half had an alcohol addiction; 16 percent had a drug addiction (mostly for heroin or cocaine);<sup>7</sup> 27 percent had both alcohol and drug addictions; and addiction information was missing in six percent of cases.

Stapleton et al. (1998) also had access to reclassification and termination information for all affected beneficiaries. Around 151,000 beneficiaries applied to be reclassified on the basis of their other disabilities, of which 71,000 were successful. In addition to the approximately 80,000 individuals who were unsuccessful in their quest for reclassification, Stapleton et al. estimated that another 23,000 individuals lost eligibility directly as a result of the policy.<sup>8</sup>

There have been several studies of the employment effects of these terminations, although none have used SSA administrative data. Orwin et al. (2004) used employment records of DA&A beneficiaries in Washington State and found employment increased by 10 percentage points after these terminations; they could not distinguish between terminated and reclassified beneficiaries. Campbell, Baumohl and Hunt (2003) analyzed the formal and informal employment of 661 participants in a study that interviewed

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<sup>6</sup> About half of the DI-only beneficiaries (10 of 21 percent) had received SSI benefits during the DI waiting period.

<sup>7</sup> While the specific drug addiction was not in their SSA administrative file, Stapleton et al. (1998) had access to this information from agencies managing the drug treatment of beneficiaries. The authors also noted from this information that about one third had technical training and 84 percent had been charged with a criminal offense.

<sup>8</sup> The other 35,000 notices went to individuals who were misclassified or who exited for other reasons. This high number was because notices were sent to all individuals with active records, and a beneficiary's record can remain active for a year or more when a person is exiting a program or in a non-eligible status.

former DA&A SSI beneficiaries across nine cities. Around half were employed two years after the terminations, and 12 percent were earning more than the cash benefits they lost. Finally, Chatterji and Meara (2010) use pooled cross-sections of the 1994-2002 National Survey of Drug Use and Health – formerly called the National Household Survey on Drug Abuse – and a triple-difference interaction between the probability of SSI usage, likely substance abuse and an indicator for the post-policy change to estimate the effects of the terminations. They found increases in labor force participation and employment that persisted over time in a group with a broad definition of substance abuse, but not among a more narrowly defined group.<sup>9</sup> All of these studies find some employment response, although data limitations made it difficult for them to examine what determined the size of the response.

### 1.3 Data and Sample

To study the employment effects of this policy change, former DA&A beneficiaries were identified from 1996 SSA administrative data. DA&A records were periodically extracted from the Supplemental Security Record, the system used to manage SSI, and the March and June 1996 extracts were located for this project. Stapleton et al. found 84 percent of all DA&A beneficiaries on the March 1996 extract, including many DI beneficiaries who were also processed for SSI eligibility. There are

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<sup>9</sup> The NDSUH and its antecedent has inherent limitations that make it difficult for Chatterji and Meara to explore the interaction between disability benefits and employment. Respondents are not asked about their past use of disability benefits, so the authors use characteristics correlated with SSI receipt and substance abuse problems in the 1994-1996 period to identify those in later waves who may have been affected by the terminations. However, their 1994-1996 sample includes individuals with substance abuse problems who were receiving benefits only on the basis of other disabilities, a group unaffected by the policy change. This may explain why the characteristics of their 1994-1996 sample of likely DA&A beneficiaries is different from administrative data reported by Stapleton et al. (1998).

8,079 individuals on the June extract who are not on the March extract, which means that up to 90 percent of all DA&A beneficiaries can be tracked using these data.

SSA staff used Social Security numbers in the June 1996 extract to produce up-to-date extracts of the Supplemental Security Record, Master Beneficiary Record, 831 File and Master Earnings File. In combination, these provide a complete history of an individual's SSI, DI and Social Security usage; taxable wage earnings; impairments; and various demographic characteristics, including sex, age and education. Information from the 831 File is available from 1989, while the other data is available from 1981 or earlier. All of the datasets finish in 2008.<sup>10</sup>

A sample was created of individuals aged 30 to 64 years on January 1<sup>st</sup> 1997. The lower age limit restricts the sample to those aged at least 22 years in 1989, when education and other time-varying information was first recorded, while most of those 65 or older qualified for other SSA programs. The sample was also limited to those who first received benefits between January 1<sup>st</sup> 1989 and April 1<sup>st</sup> 1996, and those receiving SSA payments in the second quarter of 1996.<sup>11</sup>

The characteristics of the 139,170 people who met these criteria are provided in Column 1 of Table 1.2. They are similar to those of the overall DA&A group described by Stapleton et al. (1998). The sample is predominantly male (71 percent). Almost half are white (48 percent) and most others are black (42 percent). More than half had an alcohol addiction (55 percent), 16 percent had a drug addiction, and 29 percent had both drug and alcohol addictions.

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<sup>10</sup> Extensive documentation on most of these datasets is available from a SSA data linkage projects (see: [http://www.cdc.gov/nchs/data\\_access/data\\_linkage/ssa.htm](http://www.cdc.gov/nchs/data_access/data_linkage/ssa.htm)). Data preparation details are provided in Appendix A1.

<sup>11</sup> To remove people in Medicaid facilities, individuals were omitted if they were due low payments (see Appendix A1).

The sample is divided into DI (56,461 individuals) and SSI-only beneficiaries (82,709 individuals), based on program activity in 1996. The groups are different, as DI beneficiaries worked at least half of the decade before applying while SSI beneficiaries have little to no work history. Of those receiving DI, 41 percent also received top-up SSI payments; this sample is not further split into DI only and DI/SSI beneficiaries because the results are similar for both groups.

The characteristics of the DI and SSI samples are shown in Columns 2 and 5 of Table 1.2, respectively. Relative to the SSI sample, the DI sample has a higher fraction of males (DI: 80 percent; SSI: 65 percent), a lower fraction of blacks (DI: 33 percent; SSI: 48 percent), and a higher fraction with alcohol-only addictions (DI: 59 percent; SSI: 52 percent). On average, the DI sample is relatively more educated (by 0.7 years). The DI sample received average federal benefits in 1996 of \$9,946, 36 percent more than the SSI group. There is little difference in their average age and time on benefits. Relative to the overall SSA disability beneficiary population in 1996, both sets of DA&A beneficiaries were younger, and disproportionately male and black.<sup>12</sup>

The samples are divided into those terminated as a result of the policy and those reclassified based on other disabilities. Memos to Social Security offices in California indicated that beneficiaries terminated as a result of this policy should be assigned a disability cessation code.<sup>13</sup> Tabulations confirm that these codes, which are rarely used, are used extensively in January 1997. A person is considered terminated as a result of the policy if, in January 1997, they had a newly-assigned cessation code and received no

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<sup>12</sup> In 1996, males made up 60 percent of DI beneficiaries and 45 percent of SSI beneficiaries. Blacks comprised 18 percent of DI beneficiaries and 31 percent of SSI beneficiaries. DI beneficiaries had an average age of 49 years and SSI beneficiaries had an average age of 47 years (SSA, 1997).

<sup>13</sup> Memos are available at: <http://www.dhcs.ca.gov/services/medi-cal/eligibility/Pages/1996ACWDLs.aspx>.



payments. A person is considered to have been successfully reclassified if they were in current payment status and paid in January 1997. There are 12 percent of the DI sample and 28 percent of the SSI sample neither paid in January 1997 nor clearly terminated as a result of the policy; they are not included in either group.<sup>14</sup>

The characteristics of terminated and reclassified DI beneficiaries are shown in Columns 3 and 4 of Table 1.2, respectively. Compared to the reclassified group, terminated beneficiaries are younger (by 3.1 years); more educated (by 0.4 years); and had less time receiving benefits (by 0.46 years). A higher fraction of terminated beneficiaries are black and a higher fraction had a drug addiction. Their average 1996 payments are five percent lower than reclassified beneficiaries. Females comprise a slightly smaller fraction of terminated (18 percent) than of reclassified (21 percent) beneficiaries. All of these differences are statistically significant at the five percent level, which is not surprising given the sample sizes.

The characteristics of terminated and reclassified SSI beneficiaries are shown in Columns 6 and 7 of Table 1.2, respectively. Similar patterns emerge: compared to reclassified beneficiaries, terminated beneficiaries are younger (by 3.5 years); more educated (by 0.3 years); and had less time on benefits (by 0.11 years). Their average 1996 payments are four percent lower than reclassified beneficiaries. Terminated beneficiaries are also disproportionately male, black and addicted to drugs. Again, these differences are statistically significant at the five percent level.

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<sup>14</sup> These are probably a mix of people who: exited for other reasons, had an unusual program status in January 1997, or were terminated as a result of the policy but were assigned a rare termination code instead of the right code. Assuming that those assigned rare codes in January 1997 are terminated beneficiaries leads to similar results.

Despite the statistical significance of these differences, no characteristics sharply separate terminated from reclassified beneficiaries. For example, there are both individuals aged in their 30s who were reclassified and individuals aged in their 60s who were terminated; plotting the reclassification rate as function of age shows it to be steadily increasing in age. This lack of sharp differences may be due to the nature of the reclassification process. Reclassification decisions required judgments about how severe other ailments would be in the absence of a drug or alcohol addiction. Yet there is a lot of uncertainty about how substance abuse affects both mental disorders (Grant et al., 2004) and physical conditions, including respiratory (Joshi and Guidot, 2007) and musculoskeletal conditions (Diamond et al., 1989).<sup>15</sup> The difficulties in deciding if a person would be disabled without their addiction may have led to a situation where small differences were important and outcomes partly depended on who reviewed the case.<sup>16</sup>

The similarity of the terminated and reclassified groups is also apparent in their earnings histories; their mean annual earnings for 1981-2008 are shown in Figure 1.1, with the DI sample in Panel A and the SSI sample in Panel B. These are W-2 earnings, so include wages but not self-employment. Vertical lines are drawn at 1995, the last year before the policy was announced.

Terminated and reclassified beneficiaries have similar pre-termination earnings trends, even though there are large changes in earnings over this period. The gap between the average earnings of terminated and reclassified beneficiaries in the DI sample is \$524 in 1989, which is around five percent of their average earnings for that year. This gap

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<sup>15</sup> Alcohol consumption can interfere with bone growth and bone tissue replacement, leading to lower bone density.

<sup>16</sup> Recent studies have found varying allowance rates for similar cases among disability examiners (Maestas et al., 2011) and Administrative Law Judges (French and Song, 2011).

increased by \$205 between 1989 and 1995, while the average earnings of both groups declined by more than \$9,000 over the same period. Terminated and reclassified SSI beneficiaries also have similar pre-1996 earnings trends, with annual differences that are never greater than \$350. As shown in the next section, these trends are even more similar once controlling for age differences.

A second striking feature in these figures are the large increases in the earnings of terminated beneficiaries from 1996, while there is little change in reclassified beneficiaries' earnings. The difference in the mean earnings of terminated and reclassified DI beneficiaries is \$4,293 in 1997, peaks at \$6,046 in 2000, and declines to \$3,270 by 2008. The same pattern is observed in the SSI sample, where these same differences are \$2,172 in 1997, \$3,176 in 2000 (also the peak), and \$1,808 in 2008. These magnitudes are large relative to beneficiaries' earnings histories.

The continued interaction between earnings and the disability programs helps to explain the decline in terminated beneficiaries' average earnings after 2000. Entry into SSA programs before and after the end of the DA&A program is shown for the DI and SSI samples in Panels C and D of Figure 1.1, respectively. Vertical lines are drawn at the end of 1996, when the last DA&A payments were made. Terminated beneficiaries steadily re-enter throughout the 1997 to 2008 period, and 52 percent of DI and 55 percent of SSI terminated beneficiaries receive post-1996 disability payments by 2008.<sup>17</sup> The decline in terminated beneficiaries' earnings after 2000 is mainly due to this re-entry, as individuals are again subject to DI/SSI earnings limits. There is little decline in earnings after 2000 among those who do not again receive disability benefits. It is difficult to

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<sup>17</sup> About two percent of terminated beneficiaries first reappear as a recipient of retirement insurance or old-aged SSI.

interpret these patterns, as the re-eligibility of terminated beneficiaries may be due to poor health, limited employment prospects, or a combination of both. In any case, it does help to explain why the earning effects start to dissipate four years after the terminations occurred.

### 1.3 Empirical Approach and Basic Results

Given the pre-treatment similarities between those who lost and kept disability benefits, the primary approach to estimating the effects of losing benefits is a differences-in-differences analysis where the employment outcomes of those who lost their disability benefits is judged relative to those who retained them. A flexible linear probability regression is initially used to explore the pre-treatment trends and the nature of the employment responses; in subsequent sections, these responses are parameterized to examine heterogeneity in the effects.

For this analysis, data from 1989 to 2008 are used, which includes seven years of data before the terminations were announced (1989-1995), the year that the policy was announced (1996), and twelve years after the terminations occurred (1997-2008). Letting  $y_{it}$  denote the employment outcome for the  $i^{th}$  person in the  $t^{th}$  year, the first equation to be estimated is:

$$y_{it} = \alpha_i + \theta_t + X_{it}\lambda + \sum_{\substack{t=1989 \\ t \neq 1995}}^{2008} D_t * TERMINATED_i \beta_t + u_{it} \quad (1.1)$$

where  $\alpha_i$  represents individual fixed effects that control for time-invariant individual differences in unobserved employment propensities;  $\theta_t$  is a complete set of time fixed effects that capture common time shocks in employment; and  $X_{it}$  represents two sex-specific cubics in age, which are used to control for age-related changes in earnings. The

variable  $TERMINATED_i$  is a dummy variable equal to one if the person lost their benefits, and zero otherwise. The individual fixed effects absorb permanent differences between terminated and reclassified beneficiaries, while the time-varying differences between terminated and reclassified beneficiaries are identified by the interaction of  $TERMINATED_i$  with time dummy variables  $D_t$ , which are equal to one in year  $t$  and zero otherwise. The reference year is 1995, because terminated beneficiaries could have responded to the policy change by increasing their wage earnings during the latter part of 1996 if they decided against appealing or found out that their appeal had been unsuccessful. The coefficients of interest  $\beta_t$  measure the annual differences in the probability of employment of terminated and reclassified beneficiaries, relative to 1995. I estimate standard errors allowing for heteroskedasticity and an arbitrary correlation in errors for each individual.

Binary measures of employment are used rather than earnings directly, because there are many observations with zero earnings.<sup>18</sup> First, an individual is defined as employed if they have any annual earnings. For the DI sample, the 19  $\beta_t$  coefficients (and 95 percent confidence intervals) resulting from estimating equation (1.1) using this definition are plotted in a bold grey line in Panel A of Figure 1.2. Terminated beneficiaries are 6.3 percentage points less likely to report any earnings in 1989 than reclassified beneficiaries, relative to 1995. This difference shrinks during the rest of the pre-termination period, and is less than one percentage point from 1992 to 1994. In 1996, the relative fraction of terminated beneficiaries with any earnings rises to 6.5 percentage

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<sup>18</sup> Results with earnings as the dependent variable tell a similar story, with small earnings differences prior to 1995 followed by a large relative increase in the earnings of terminated beneficiaries. For example, earnings differences (standard errors) between terminated and reclassified DI beneficiaries are under \$301 (102) in 1989-1994; differences are then \$3,510 (61) in 1997, \$5,201 (90) in 2000 (the peak), and \$2,358 (89) in 2008. See Appendix A1 for details.

points; most affected individuals are still receiving disability payments at this time. The relative fraction with any earnings then increases to 26.2 percentage points in 1997 and peaks at 28.5 percentage points in 1998, before steadily declining to 5.0 percentage points in 2008. The coefficients are precisely estimated, with standard errors never larger than 0.6 percentage points. Results from a specification without individual fixed effects are plotted in a dashed grey line in the same panel: the coefficients are almost identical. The results are also similar when a logit specification is used and when the age polynomials are replaced by dummy variables.<sup>19</sup>

A problem with using any earnings to define employment is that an individual can both receive benefits and be regarded as employed (recall that DI and SSI beneficiaries can have some earnings without penalty). A more intensive threshold for regarding someone as employed is one based on Substantial Gainful Activity (SGA). Any beneficiary earning above SGA for a sustained period will lose their benefits. The annualized 1996 SGA level is equal to \$8,339. An added benefit of using this level is that it is close to the average SSA payments made in 1996, and so provides some idea of how many individuals “replaced” their benefits via wage earnings.

Equation (1.1) is estimated for the DI sample with the dependent variable defined in terms of earnings over 1996 SGA (\$8,339). The coefficients of interest and their 95 percent confidence intervals are plotted in bold black lines in Panel A of Figure 1.2.

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<sup>19</sup> The results of these robustness tests are available in Appendix A1 for both samples. The logit specification used is:  $P[y_{it} = 1] = \exp(W_{it}\gamma) / [1 + \exp(W_{it}\gamma)]$ , with  $W_{it}\gamma = \alpha + \theta_t + X_{it}\lambda + \sum_{t=1989}^{2008} D_t * TERMINATED_i \beta_t$

The variables are the same as outlined in equation (1), except that  $\alpha$  is a constant and  $TERMINATED_i$  is included separately (this is also the case for the specification without individual fixed effects). For the interaction terms, marginal treatment effects are calculated as the double differences in the estimated probabilities when each dummy variable equals one as compared to when it is zero. Marginal effects are estimated for each treated individual, and the presented coefficients are the mean values of these effects. Standard errors are calculated using the delta method.

During the pre-termination period, the absolute values of these coefficients are less than 1.2 percentage points. In 1996, the relative fraction of terminated beneficiaries with SGA earnings rises by 3.2 percentage points. This difference increases to 22.2 percentage points in 1999, before steadily declining to 8.4 percentage points in 2008. These coefficients are precisely estimated, with the standard errors never larger than 0.5 percentage points. Again, the coefficients are similar from regressions without individual fixed effects (plotted in a dashed black line in the same panel), a logit regression, and when sex-specific age polynomials are replaced by sex-specific age dummies.

Equation (1.1) is estimated for the SSI sample using the same two employment measures. The results are presented in Panel B of Figure 1.2, with estimates based on the “any earnings” definition plotted in grey and estimates based on 1996 SGA (\$8,339) plotted in black (dashed lines again show the results from regressions without individual fixed effects). Using the “any earnings” definition, the employment differences in the pre-termination period are less than 1.8 percentage points. The relative probability of a terminated SSI beneficiary having any earnings increases to 21.9 percentage points by 1998, stays at a similar level in 1999 and 2000, and then declines to 4.8 percentage points by 2008. Using the SGA threshold, the differences are less than 0.4 percentage points in the pre-termination period, before rising from 1996, peaking at 12.6 percentage points in 2000 and then declining to 5.4 percentage points by 2008.

Most of the response in “any earnings” is present when 1996 SGA is used to define the dependent variable, particularly for the DI sample. This suggests that most terminated beneficiaries who began working did so with some intensity. It is also worth noting that the effects are more persistent in the 1996 SGA regressions: the SGA

coefficients are actually larger than the coefficients using “any earnings” from 2004 to 2008 for the DI sample, and in 2006 and 2007 for the SSI sample. This suggests that the occasional earnings of reclassified beneficiaries obscure some of the persistence in employment among terminated beneficiaries. Given this, the 1996 SGA earnings threshold is used as the main measure in the rest of the paper. Unless otherwise noted, using “any earnings” to define the dependent variable produces similar results.

### 1.3.1 Parameterizing the Employment Response

It is difficult to examine differences across groups using equation (1.1) because it generates 19 coefficients of interest, 12 of which represent the employment response. After inspecting these coefficients for a range of demographic subgroups, it became clear that the employment response follows a similar pattern in these subgroups: employment rose in 1996 and 1997, was highest from 1998 to 2000, and declined from 2000 to 2008. Given this, the interactions between  $TERMINATED_i$  and the year dummy variables after 1997 are replaced by two parameters:<sup>20</sup>

- The rise in employment is captured by  $RESPONSE_{it}$ , which is equal to one if  $t \geq 1998$  and the individual is a terminated beneficiary, and zero otherwise; and
- The subsequent decline in employment is captured by  $POSTTREND_{it} = t - 1999$  if  $t \geq 2000$  and the individual is a terminated beneficiary, and zero otherwise.

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<sup>20</sup> Jacobson, Lalonde and Sullivan (1993) imposed a functional form on the post-policy changes in earnings of displaced workers to get a better idea of the evolution of the differences across demographic groups. Charles (2003) and von Wachter et al. (2011) use similar approaches. I tested plausible alternative specifications, such as estimating  $POSTTREND_{it}$  starting from 1999 or 2001. The differences across groups are similar in these alternate regressions.



The dummy variables for the years 1996 and 1997 are retained, as are the dummy variables on the years 1989 to 1994. The regression specification now becomes:

$$y_{it} = \alpha_i + \theta_t + X_{it}\lambda + \sum_{\substack{t=1989 \\ t \neq 1995}}^{1997} D_t * TERMINATED_i \beta_t + RESPONSE_{it} \delta_1 + POSTTREND_t \delta_2 + u_{it} \quad (1.2)$$

The coefficients of interest are now  $\delta_1$ , which measures terminated beneficiaries' relative shift in employment after 1997, and  $\delta_2$ , which measures terminated beneficiaries' relative employment trends from 2000 to 2008. Differences across groups in the 1996 and 1997 coefficients are hard to interpret, as they may reflect timing differences of the reclassification process rather than just differences in the timing of the employment response. In general, the declines in employment measured by  $POSTTREND_{it}$  are similar across groups or reinforce the initial employment increases (e.g., a large response is accompanied by a small decline). This means that differences across groups can be understood by focusing on the  $RESPONSE_{it}$  coefficients.

Using 1996 SGA to define employment, the coefficients and standard errors for the  $RESPONSE_{it}$  and  $POSTTREND_{it}$  variables for the DI sample are shown in the top four rows of Column 1 in Table 1.3. The  $RESPONSE_{it}$  coefficient is 21.7 percentage points, close to the peak employment response using equation (1.1). The  $POSTTREND_{it}$  coefficient is -1.6 percentage points, reflecting the decline in employment from 2000 to 2008. Both coefficients are statistically different from zero at the one percent level; this is the case for all of coefficients in Table 1.3.

The next three columns of Table 1.3 show the results for DI subsamples separated by addiction type. The  $RESPONSE_{it}$  coefficient is 20.3 percentage points for alcohol only, 21.5 percentage points for drugs only, and 23.8 percentage points for alcohol and

drugs beneficiaries. The higher response of the group with both alcohol and drug addictions can be explained by their relatively high pre-application earnings, which is shown in the next section to increase the employment response. The later declines in employment are similar across the groups. The top four rows of the final three columns of Table 1.3 contain results for groups based on what disabilities individuals applied to be reclassified under in 1996. Reclassification applications sometimes generated entries in the 831 File that listed the basis on which they were reapplying.<sup>21</sup> Results for those reapplying on the basis of mental disorders, musculoskeletal conditions and all other conditions are presented. The employment patterns are broadly similar for the three groups, with an estimated employment response of 21.0 percentage points among those with mental disorders and 20.6 percentage points among those with musculoskeletal conditions.

The equivalent results for the SSI sample are shown in the bottom four rows of Table 1.3. The employment response is 11.9 percentage points, close to the peak response in the dummy variable analysis. The annual decline in employment between 2000 and 2008 is 0.76 percentage points. As for the DI sample, the SSI response is present and broadly similar in subsamples based on individuals' type of addiction and the basis on which they reapplied for benefits.

Given the similarity of the employment response across addiction types and other impairments, heterogeneity across time and across demographic groups is primarily analyzed using the whole DI and SSI samples. Analyses using subsamples based on addiction and impairment information produce similar results, unless otherwise noted.

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<sup>21</sup> Around 62 percent had an 831 record created between May and December 1996. Among those without an 831 record are some reclassified beneficiaries. Clearly, some applications were not processed in a way that generated a record, making it impossible to identify which individuals did not apply to be reclassified.

These subsamples are considered again later in the paper when discussing the external validity of the results.

## 1.4 Examining Heterogeneity in the Employment Response

### 1.4.1 The Relationship Between Benefits, Health and Employment

Which terminated beneficiaries are most able to work? Two types of heterogeneity could create differences in the employment response. First, there are dynamic effects related to changes in health and time out of the labor force that may change a beneficiary's ability to work. Second, the costs of exiting the labor force and applying for disability benefits partly depends on individuals' non-health characteristics. This may lead to initial differences in beneficiaries' ability to work, and also affect the size and nature of the dynamic effects. These issues are discussed here in order to guide and inform the subsequent analysis in this section.

*The Changing Work Potential of Disability Beneficiaries.* The employment prospect of individuals who stop working generally declines over time, as their human capital depreciates and stigma effects increase (Gibbons and Katz, 1991; Edin and Gustavsson, 2008). For some disability beneficiaries, these negative effects may be offset by health improvements, which could come in two forms. First, individuals may have applied for benefits when their health was particularly poor, and it may naturally improve over time. Second, individuals eligible for disability benefits from the DI and SSI programs receive regular payments and gain access to medical care, either of which may improve their health. Card et al. (2009) find that Medicare eligibility at age 65 leads to more intensive use of medical service and a reduction in mortality. Finkelstein et al.

(2011) find that low income individuals who became eligible for Medicaid via a lottery in Oregon increased their healthcare utilization and report better physical and mental health. Such effects are likely to translate to disability beneficiaries, particularly as many individuals do not have health insurance when they first gain eligibility for benefits.<sup>22</sup>

If some beneficiaries' health improves while on DI/SSI, the employment effects related to this improvement may outweigh the loss of skills and increasing stigma due to being out of the labor force. The net effects are ambiguous, so whether beneficiaries become more or less able to work over time is fundamentally an empirical question. It is examined in the next subsection.

*Individual Differences and Applying for Benefits.* It is also important to consider how employment potential might vary across disability beneficiaries, both when they first apply for benefits and over time. Health plays a central role in determining who applies for, and who receives, disability benefits. Not all individuals with the same health will apply, however, as the benefits and costs of doing so will differ on the basis of other characteristics. Studies of the determinants of application behavior have generally focused on three factors: (1) the value of individuals' disability program payments relative to their potential wage earnings; (2) the probability of receiving disability benefits; and (3) the probability of keeping or finding a job.

With respect to the first one, the relationship between potential wage earnings and disability payments depends on individuals' earnings histories and how representative they are of future earnings. A progressive formula determines the relationship between past earnings and disability payments, so that those with low earnings have a greater

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<sup>22</sup> Livermore, Stapleton and Claypool (2010) find that one fifth of DI entrants do not have health insurance. The fraction of SSI entrants who are uninsured is likely to be higher, given their limited work histories.

fraction of their earnings “replaced” than those with higher earnings. For example, someone with average annual past earnings of \$10,000 received annual DI payments of \$8,497 in 2010, while someone with average annual past earnings of \$40,000 received annual payments of \$18,097. SSI payments do not depend on past earnings. To the extent that future earnings are related to past earnings, disability payments are relatively less attractive to higher earners than low ones; Autor and Duggan (2003) show that benefit generosity does affect DI application behavior.

Where individuals are in their working life also changes the relationship between past and future earnings, and therefore the relative generosity of disability benefits. Disability payments are indexed to the CPI, so disability applications are more costly for someone who expects their real income to rise in the future than someone who expects it to remain flat. The opportunity costs of stopping work are therefore generally greater for the young, as earnings tend to increase with age and experience. This is true even for disabled individuals, as the young have the most to gain from developing “disability-specific human capital” (Charles, 2003).

The relationships between the value of disability benefits and the second and third factors are conceptually straightforward: a high probability of being granted benefits will make applying more attractive, while better employment prospects will generally make applying less attractive. Autor and Duggan (2003) find empirical support for both of these relationships using state-level application and award information for the DI program. They estimate that the 1984 liberalization of disability medical standards increased DI applications and induced the labor force exit of low-skill unemployed individuals, and that labor force exits to DI are higher when unemployment rates are

high. Similarly, Gruber and Kubik (1997) find a relationship between state-level disability denial rates and labor supply, while Black, Daniel and Sanders (2002) and Lahiri, Song and Wixon (2008) find disability program entry is higher in bad labor markets than in good ones.

The preceding discussion suggests that applicants who are younger, who have higher earnings, or who are in strong labor markets give up more in future earnings by applying for disability benefits than other applicants. (There are not good sources of variation in the probability of being granted benefits for this sample.) If health is constant over time, then we would expect that applicants with these characteristics would be in worse health than other applicants. But, as discussed at the start of this section, health is unlikely to be constant and may sometimes improve with benefit receipt. Given that individuals should value their health status in the future, beneficiaries with apparently high opportunity costs of applying for benefits may have applied because they expected their health to improve more than other applicants.<sup>23</sup> This possibility is important to consider here, because it suggests that the young, those with higher earnings, and those applying in good labor markets may have the greatest health improvements once they are receiving disability benefits. If true, any increases over time in these beneficiaries' ability to work should be greater than the increases of other beneficiaries.

There is evidence that individuals take the pecuniary value of medical services into account in their application decisions: DI applications are higher among individuals with high-cost health conditions (Lahiri et al., 2008) and SSI applications are higher in

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<sup>23</sup> Such health improvements could be incorporated into models of application behavior like the one in Autor and Duggan (2003) by (1) allowing the health of some individuals to improve more (or deteriorate less) from benefit receipt than working; and (2) letting health enter the utility function directly, rather than as just an effort or work disutility parameter.

states with more generous Medicaid programs (Yelowitz, 1998). While health costs do not necessarily translate into health improvements, there is actuarial evidence that younger DI beneficiaries experience disproportionate reductions in mortality over time that cannot be explained by compositional changes resulting from death and medical recovery (Zayatz, 1999). This is the direction in which one would expect the heterogeneity to go if people's decision to apply for disability benefits partly depends on the degree to which health improves through benefit receipt, as young entrants sacrifice more in future earnings than otherwise similar older individuals.

This discussion guides the analysis of heterogeneity in Section 1.4.3, where the empirical examination proceeds in two stages. The first step is to estimate the employment response within subgroups defined on the basis of characteristics clearly related to the value of disability payments: (1) the age of beneficiaries; (2) their pre-application earnings; and (3) the state-level unemployment rates when they applied for benefits. This establishes whether there are cross-sectional differences in the employment response that are related to these characteristics. The second stage is to estimate the relationship between these responses and the time on disability benefits in order to identify the source of these differences. Disproportionate employment increases over time among young beneficiaries, beneficiaries with relatively high pre-application earnings, and among those who entered the program when unemployment rates are low is consistent with them experiencing disproportionate health improvements that may explain their decision to apply for disability benefits in the first place.

A final subsection is added to the heterogeneity analysis. A data limitation is that there are not good measures of individuals' health. However, there is information about

how easily individuals gained eligibility for disability benefits, which is largely a function of the severity of their disabilities (Hu et al., 2001). Examining whether the response varies by how benefits were awarded is used to help understand how relative levels of health at the time of application affected the later employment response. Given the nature of the results in that section, this analysis further establishes that the response is affected by underlying changes in health.

#### 1.4.2 Differences by Time Receiving Benefits

Beneficiaries' ability to work may change over time as a result of health changes or because they lose work skills and connections the longer they spend out of the labor force. Given these are possibly competing forces, the exact relationship between time on disability benefits and the employment response is an empirical question. From the outset, it is important to recognize that time on benefits is correlated with when individuals applied for disability benefits, because all of the terminations occurred in early 1997. This means it is difficult to separate effects due to time on the program from the changing characteristics of new disability beneficiaries. While several additional analyses indicate that the results are unlikely to be strongly affected by beneficiary cohort effects, their possible role remains a caveat to this analysis.

The effect of time on benefits is estimated using regressions based on equation (2), the parameterization of the employment response. Time on disability benefits,  $DISTIME_i$ , is the length of time between the month when an individual entered DI/SSI and January 1997. To control for employment differences prior to 1996 and to estimate differences in the employment response, the three cubic terms of  $DISTIME_i$  are separately



interacted with the variables identifying employment differences between terminated and reclassified beneficiaries throughout the sample period.<sup>24</sup> Employment is defined as annual earnings over the 1996 SGA threshold.

Results for the DI sample are presented in Column 1 of Table 1.4. The coefficient (standard error) on  $RESPONSE_{it}$  is 0.0810 (0.0208), which means that the estimated increase in employment is 8.1 percentage points before any time receiving benefits. The other main coefficients of interest are those resulting from the interactions between  $RESPONSE_{it}$  and the three cubic terms of  $DISTIME_i$ . All three coefficients are statistically significant at the one percent level, suggesting the increase in post-termination employment varies nonlinearly with time on benefits. The three coefficients resulting from the interactions between  $POSTTREND_{it}$  and the  $DISTIME_i$  cubic terms are not statistically significant, even at the five percent level.<sup>25</sup> Together, these results suggest that the time on disability benefits affected the number of individuals returning to work but not the decline in employment in the latter part of the sample period.

Figure 1.3 shows how the total employment response in the DI sample varies as a function of time on benefits. This is calculated using the nonlinear combination of the coefficients related to  $RESPONSE_{it}$  (in the first four rows of Table 1.4) at different values of  $DISTIME_i$ . It is estimated for values between zero and six years of benefit receipt, beyond which the confidence intervals become wide and uninformative. The 95 percent confidence intervals are shown in dashed lines.

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<sup>24</sup> That is,  $y_{it} = \alpha_i + \theta_t + X_{it}\lambda + Z_{it}\varphi_t + DISTIME_i * Z_{it}\varphi_t + DISTIME_i^2 * Z_{it}\varphi_t + DISTIME_i^3 * Z_{it}\varphi_t + u_{it}$  where  $Z_{it}\varphi_t = \sum_{t=1989}^{1997} D_t * TERMINATED_i\beta_t + RESPONSE_{it}\delta_1 + POSTTREND_t\delta_2$ .

<sup>25</sup> The coefficients (standard errors) on  $POSTTREND_{it} \times DISTIME_i$  is -0.0010 (0.0025), on  $POSTTREND_{it} \times DISTIME_i^2$  is 0.0005 (0.0025), and on  $POSTTREND_{it} \times DISTIME_i^3$  is 0.00004 (0.00006).

There is an inverted-U relationship between the employment response and benefit receipt. The response increases from 8.1 percentage points at zero years of benefit receipt to a peak of 24.2 percentage points at 2.5 years of receipt, and then steadily declines to 17.0 percentage points at six years. The 95 percent confidence intervals show these differences to be statistically significant. As shown in Column 1 of Table 1.4, the peak employment response occurs at 2.54 years of disability benefit receipt, when is 42 percent higher than the employment response of those who received disability benefits for nine months (the shortest period of receipt for anyone in the sample) and 42 percent higher than individuals who received benefits for six years.

Results for the SSI sample are presented in Column 2 of Table 1.4, and are qualitatively similar to those for the DI sample. All three coefficients from the interaction between  $RESPONSE_{it}$  and the cubic terms of  $DISTIME_i$  are statistically significant at the one percent level. In combination with the  $RESPONSE_{it}$  coefficient, they generate an inverted-U shaped pattern. The SSI employment response reaches a peak of 12.8 percentage points at 2.95 years of benefit receipt, where it is 38 percent higher than the employment response of those who received disability benefits for nine months and 21 percent higher than the employment response of those who received benefits for six years.

A number of analyses are conducted to gauge whether the inverted-U relationship is caused by differences between beneficiary cohorts. First, given some of the observable characteristics of DA&A beneficiaries changed as the program grew, the same regression is estimated for subsamples based on those changing characteristics (which are sex, race and addiction type). Second, given the program grew rapidly over time, the regression is

estimated on individuals in the states with the lowest growth in DA&A numbers. Third, given changes to the DA&A program were passed in August 1994 and implemented in March 1995, the regression is estimated without individuals applying in August 1994 and later. Fourth, given that unemployment rates vary over time and may change the work potential of individuals applying for disability benefits, the regression is estimated with controls for state-level unemployment rates at the time of application. The relationship shown in Figure 1.3 is present in each of these analyses, suggesting that cohort effects are unlikely to explain the observed relationship between benefits and the employment response.<sup>26</sup>

What do the results in Table 1.4 and Figure 1.3 suggest about how the employment potential of disability beneficiaries changes over time? Increasing employment over the first 2.5–3 years of benefit receipt suggests that health improves over this period and dominates any negative effects of being out of the labor force. It is difficult to identify the exact source of this improvement. All beneficiaries received SSA payments (handled by third parties), and nearly all had access to Medicare or Medicaid prior to the end of the DA&A program.<sup>27</sup> Mean reversion after a period of particularly poor health could also have had a role in this apparent health improvement.

The decline in the employment response beyond three years of disability benefit receipt suggests that the negative effects of being out of labor force dominate any other effects at that stage. The larger relative decline in the response in the DI sample than in the SSI sample is consistent with the atrophying of work skills and connections, because

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<sup>26</sup> More details are provided in Appendix A1.

<sup>27</sup> While DI beneficiaries face a two year waiting period for access to Medicare, it is backdated to when the onset of the disability can be established. In the DI sample, the average gap between starting to receive disability benefits and Medicare eligibility is around one year.

DI beneficiaries generally had more work skills and experience to lose over time than did SSI beneficiaries.

### 1.4.3 Differences by Age, Prior Earnings and Unemployment Rates when Applying for Benefits

I now examine potentially important individual differences that may have affected the employment response. As discussed at the start of this section, there are three factors that have obvious impacts on the value of disability payments relative to maintaining or seeking employment, and which subsequently may have affected the employment potential of terminated DA&A beneficiaries. An individual's age affects the extent to which disability payments replace future earnings, as does their past earnings. In addition, local unemployment rates affect the chance of finding work and change the opportunity cost of applying for benefits.

For each of these three factors, it is first established whether there are noticeable differences in the employment effects using regressions based on equation (2). Where they are present, the relationship between the employment response and time on disability benefits is examined within subsamples in order to determine whether dynamic effects seem to generate these differences. Results are reported only for the DI sample; they are similar for the SSI sample, unless otherwise noted. Employment is still measured by whether an individual had earnings above the annualized 1996 SGA level (\$8,339).

The role of age is examined first, by adding dummy variables to equation (2) that identify individuals' ages at the start of 1997. Each spans a five-year range, so the youngest group is aged 30-34 years and the oldest is aged 60-64 years. These dummy

variables are separately interacted with the variables identifying employment differences between terminated and reclassified beneficiaries over the sample period: the interactions between the relevant time dummy variables (1989-1994, 1996-1997) and  $TERMINATED_i$ , as well as  $RESPONSE_{it}$ , and  $POSTTREND_{it}$ .

The main coefficients of interest are those that result from the interaction between these age group identifiers and  $RESPONSE_{it}$ . These are plotted in Panel A of Figure 1.4, together with their 95 percent confidence intervals. The employment response is similar among the 30-34 and the 35-39 year old groups, and then monotonically declines with age. For example, it is 23.7 percentage points among 30-34 year olds, 16.3 percentage points among 50-54 year olds, and 1.0 percentage point among 60-64 year olds. The confidence intervals for the younger and older age groups do not overlap. The declines in the employment response, measured by the  $POSTTREND_i$  coefficients, are generally similar across the age groups.<sup>28</sup>

To understand the source of these age-related differences, the same regression used to explore the relationship between the employment response and the time receiving disability benefits is estimated for a sample aged 30-39 years, a sample aged 40-49 years, and a sample aged 50-64 years at the start of 1997. The calculations used to produce Figure 1.3 are done using the regression results for these three samples and presented in Panel B of Figure 1.4.

Among those who spent a short time on benefits, the estimated employment effects for the three subsamples are similar and their 95 percent confidence intervals overlap. With zero time on benefits, for example, the respective coefficients for the total

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<sup>28</sup> Except for the group aged 60-64, the estimated  $POSTTREND_{it}$  coefficients are between -1.74 and -1.45 percentage points. It is -0.60 percentage points for 60-64 year olds, reinforcing the differences in the  $RESPONSE_{it}$  coefficients.

estimated employment response in the 30-39, 40-49 and 50-64 year old samples are 6.6, 7.8 and 6.6 percentage points. The employment response of 30-39 year olds increases rapidly with time on benefits, and peaks at 27.4 percentage points at 2.5 years of benefit receipt. The increase is smaller in other two samples, with an estimated response at 2.5 years of 23.4 percentage points among 40-49 year olds and 16.0 percentage points among 50-64 year olds. The 95 percent confidence intervals for these three point estimates do not overlap at this value. There is a decline in the response of the 30-39 year old group beyond 2.5 years and of the 40-49 year olds beyond three years, while the employment response in the oldest sample remains reasonably constant. The confidence intervals of the estimated employment response in the three samples again overlap beyond around 4.2 years of disability benefit receipt. The respective coefficients of the 30-39, 40-49 and 50-64 year old samples at six years of benefit receipt are 17.1, 17.6 and 16.5 percentage points.<sup>29</sup>

These results suggest that younger beneficiaries do not start out being able to work at higher rates than older beneficiaries. Rather, their relatively large post-termination employment response comes from those who have received disability benefits for two to four years prior to termination. As increasing employment responses are most plausibly explained by health improvements dominating the negative effects of being out of the labor force, these results are consistent with disproportionate health improvements among the young and fit with health improvements being factored into the decision to apply for benefits. The rates at which individuals were reclassified on the

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<sup>29</sup> In the SSI sample, the employment responses of 30-39 year olds vary less with benefit receipt than for 40-49 year olds, although these responses across the groups are only statistically significant for values beyond five years. The SSI results are similar to those shown in Panel B of Figure 4 when “any earnings” is used to define employment, although the differences between the 30-39 and 40-49 year old groups are not statistically significant. These results are provided in Appendix A1.

basis of other disability categories provide support for there being better health among the young when the DA&A program came to an end. Reclassification rates monotonically increase with age, from 51 percent for 30-34 year olds to 85 percent for 60-64 year olds. As the reclassification decision was primarily based on medical factors, the lower reclassification rates (higher termination rates) among the young indicate that they generally had better health than other beneficiaries.

A second characteristic that differentiates the relative value of disability payments is individuals' earnings prior to applying for benefits, as those with higher past earnings have a smaller fraction of them "replaced" by DI benefits than other beneficiaries. When they are applying for disability benefits, individuals may be working less than they could in order to show they have a medical impairment that prevents them from working. To avoid this period, pre-application earnings are measured as the average earnings during the three to five years before an individual applied for disability benefits (the same period used by Maestas et al., 2011).

The effect of prior earnings is examined using a variant of equation (2). Non-overlapping dummy variables are used to identify eight groups based on their average earnings 3-5 years before applying for benefits. The ranges used are a function of the 1996 SGA threshold: a dummy variable identifies those with no prior earnings, six dummy variables each cover a range of average earnings equal to one half of the 1996 SGA level (\$4,170), and a final dummy identifies those with average earnings greater than three times SGA (\$25,017).<sup>30</sup> As with the age-based analysis, these dummy

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<sup>30</sup> That is, the ranges are: (1) \$0; (2) \$1-\$4,170 ( $0 < SGA \leq 0.5$ ); (3) \$4,171-\$8,339 ( $0.5 < SGA \leq 1$ ); (4) \$8,340-\$12,509 ( $1 < SGA \leq 1.5$ ); (5) \$12,510-\$16,678 ( $1.5 < SGA \leq 2$ ); (6) \$16,679-\$20,848 ( $2 < SGA \leq 2.5$ ); (7) \$20,849-\$25,017 ( $2.5 < SGA \leq 3$ ); and (8) over \$25,017 ( $SGA > 3$ ).

variables are separately interacted with the variables identifying employment differences between terminated and reclassified beneficiaries throughout the sample period.

The main coefficients of interest are those that result from the interactions between these group identifiers and  $RESPONSE_{it}$ . They are shown in Panel C of Figure 1.4, together with their 95 percent confidence intervals. The employment response increases with the amount of earnings individuals had in the three to five years before applying for disability benefits. For example, it is around 13 percentage points among those with zero average prior earnings, 24.0 percentage points among those with average prior earnings that were 1–1.5 of SGA, and 26.1 percentage points among those with average prior earnings over three times SGA. The 95 percent confidence intervals around the coefficients for groups with low pre-application earnings do not overlap with the confidence intervals of groups with higher prior earnings.<sup>31</sup>

To understand the source of these differences, the role of time on disability benefits is examined for a sample of individuals with average prior earnings above the 1996 SGA threshold and a sample with average prior earning below that level. The results are shown in Panel D of Figure 1.4. A similar pattern to the age-related analysis emerges. At zero time on benefits, the estimated employment effects for those with average prior earnings below the 1996 SGA level is 15.0 percentage points and for those above the threshold it is 3.3 percentage points. With time on disability benefits, employment in the sample with high pre-application earnings increases markedly, while the employment response in the other group does not change much. At 2.5 years of

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<sup>31</sup> The definition of employment that is used does matter here, as differences related to prior earnings are flatter if “any earnings” is used to define the dependent variable. This is because relatively more reclassified beneficiaries have small amounts of earnings if they had high pre-application earnings than if they had low pre-application earnings. Therefore this flatter relationship is due to changing employment in the control group.



benefit receipt, the employment response in the high prior earnings group is 29.4 percentage points, compared to 19.4 percentage points in the low prior earnings group. Beyond three years, the employment response of the high prior earnings group declines more sharply than the low earnings group, and is 18.7 percentage points at six years of receipt. The 95 percent confidence intervals of the two sets of estimates do not overlap from one to five years of benefit receipt.

The results are again consistent with the discussion at the start of this section. To the extent that the employment response increased with time on benefits, we expect it to come from health improvements that were larger for those who found applying for disability benefits more costly. Those with better earnings histories before applying for benefits have a relatively large increase in their employment response, which fits with their health improving disproportionately while on the program. DA&A reclassification rates are broadly consistent with this.<sup>32</sup>

The third characteristic affecting the value of applying for disability benefits is the unemployment rate at the time of application. Using equation (2), the state-level unemployment rate in the year an individual applied for benefits,  $UNEMP_i$ , is interacted with  $RESPONSE_{it}$ ,  $POSTTREND_{it}$  and the other variables accounting for differences between terminated and reclassified beneficiaries throughout the sample period. The square and cubic terms of  $UNEMP_i$  are separately interacted with the same variables. As unemployment rates are correlated over time, the cubic terms of state-level

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<sup>32</sup> Reclassification rates are highest among those who had low earnings in the three to five years before applying for benefits, although they do not differ much for those with average prior earnings near the SGA level or higher.

unemployment rates in 1997 are separately interacted with the same variables to control for labor market conditions at the time benefits were terminated.<sup>33</sup>

This regression is estimated for the entire sample. The main coefficients of interest are result from the interaction between  $RESPONSE_{it}$  and the cubic terms of  $UNEMP_i$ . All three coefficients are small and statistically insignificant.<sup>34</sup> (The relationship between 1997 unemployment rates and the employment response is also imprecisely estimated.) The relationship between the total employment response and unemployment rates of between 4.5 and 9.0 percent is shown in Panel E of Figure 1.4.<sup>35</sup> There is little change in the employment response over this range, and the 95 percent confidence intervals are wide. The relationship between the unemployment rate at application and the post-termination employment response is too imprecisely estimated to explore dynamic effects. It may be that better data on individuals' employment opportunities is needed to properly understand how labor market conditions affect the later employment potential of individuals receiving disability benefits.

In summary, two of the three characteristics identified as affecting the value of applying for benefits are found to be strong predictors of the post-termination employment response. Importantly, the size and nature of employment effects by age and pre-application earnings are consistent with the employment differences coming from disproportionate health improvements that occurred while individuals were on these disability programs. Reclassification rates, to the extent that they measure health at the

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<sup>33</sup> That is,  $y_{it} = \alpha_i + \theta_t + X_{it}\lambda + Z_{it}\varphi_t + UNEMP_i * Z_{it}\varphi_t + UNEMP_i^2 * Z_{it}\varphi_t + UNEMP_i^3 * Z_{it}\varphi_t + UNEMP_{1997_i} * Z_{it}\varphi_t + UNEMP_{1997_i}^2 * Z_{it}\varphi_t + UNEMP_{1997_i}^3 * Z_{it}\varphi_t + u_{it}$ , where  $UNEMP_{1997_i}$  is state-level unemployment rate in 1997 and  $Z_{it}\varphi_t = \sum_{t=1989}^{1997} D_t * TERMINATED_i\beta_t + RESPONSE_{it}\delta_1 + POSTTREND_t\delta_2$ .

<sup>34</sup> The estimated coefficients (standard errors) on the interaction between  $RESPONSE_{it}$  and  $UNEMP_i$ ,  $UNEMP_i^2$  and  $UNEMP_i^3$  are 0.0049 (0.0755), 0.0037 (0.0109) and -0.0003 (0.0005), respectively.

<sup>35</sup> This range covers the unemployment rates at application for 90 percent of the sample.

time the terminations occurred, provide further support for these “high opportunity cost” beneficiaries experiencing disproportionate health improvements.

#### 1.4.4 Is Employment Higher Among Those Initially Denied Benefits?

Disabilities are more severe among those awarded benefits at earlier stages of the determination process (Hu et al., 2001). While the conceptual framework at the start of this section does not generate specific predictions about how employment potential depends on the adjudication level at which benefits were awarded, examining how the response varies by this characteristic can help to understand how health at the time of application affected later employment. It is also interesting in itself, as von Wachter et al. (2010) find that initially denied beneficiaries are substantially more able to work than other beneficiaries.

Disability applications are first assessed by medical examiners in state-level Disability Determination Services (DDS) offices. A denied applicant can ask for their application to be reconsidered by different DDS examiners. If they remain denied, they can request a hearing with an Administrative Law Judge, then appeal to the Social Security Appeals Council, to the U.S. District Court and finally to the U.S. Circuit Court of Appeals. Around one third of disability awards are made by Administrative Law Judges or by courts (SSA, 2011a).

Information from the 831 File can be used to identify those awarded benefits after their initial determination (“Initial Award”), those awarded after reconsideration by different DDS examiners (“Reconsideration Award”), and those awarded benefits at a

higher level (“Hearings Award”).<sup>36</sup> There are 44 percent in the Initial Award group, 11 percent in the Reconsideration Award group, and 45 percent in the Hearings Award group. As noted previously, a relatively high fraction of DA&A beneficiaries were awarded benefits upon appeal (Stapleton et al., 1998).

To investigate overall differences between these groups, non-overlapping dummy variables identifying the three groups are interacted with  $RESPONSE_{it}$ ,  $POSTTREND_{it}$  and the other variables accounting for employment differences between terminated and reclassified beneficiaries throughout the sample period. This regression is separately estimated for the DI and SSI samples, and the respective  $RESPONSE_{it}$  and  $POSTTREND_{it}$  coefficients are presented in Table 1.5. In the DI sample, the  $RESPONSE_{it}$  coefficients for the Initial, Reconsideration and Hearings Award groups are 21.7, 22.0 and 21.5 percentage points, respectively. The differences between these coefficients are not statistically significant at the five percent level. There are statistical significant differences in the  $POSTTREND_{it}$  coefficients for the Initial Award group (-1.68 percentage points) as compared to the Hearings Award group (-1.47 percentage points), although these coefficients (in combination with the  $RESPONSE_{it}$  coefficients) do not suggest large employment differences.<sup>37</sup> The response is also similar across the three equivalent groups in the SSI sample, with  $RESPONSE_{it}$  coefficients for the Initial, Reconsideration and Hearings Award groups of 11.8, 12.2 and 12.0 percentage points,

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<sup>36</sup> The 831 File includes information recorded at the DDS level, so lists initial determinations and reconsiderations. Those who are not awarded benefits at the DDS-level but who are later receiving benefits must have been awarded benefits through the appeals process. The 831 records with a filing date before April 1996 were used, as these related to initial applications rather than reclassifications associated with the termination of the DA&A program. Two percent of the sample did not have an 831 file over this period; most of these people filed for benefits before 1989, when 831 records are not available. They are omitted from the analysis.

<sup>37</sup> For example, using the point estimates for these coefficients, the estimated employment effects in 2005 are 13.3 percentage points for the Initial Award group and 14.2 percentage points in the Later Award group.

respectively. There are no statistically significant differences between these coefficients or between the  $POSTTREND_{it}$  coefficients estimated for this sample.

The relationship between the employment response and benefit receipt is examined using the same regression used to produce Figure 1.3. The coefficients and 95 percent confidence intervals from the interactions between the  $DISTIME_{it}$  and  $RESPONSE_{it}$  for the Initial and Hearings Award groups are presented for the DI sample in Panel F of Figure 1.4.<sup>38</sup>

There is some interesting heterogeneity in the results. The response in the Hearings Award group starts out higher than the Initial Award group. The  $RESPONSE_{it}$  coefficients in the Initial and Hearings Award groups at nine months are, respectively, 13.5 and 19.7 percentage points. The 95 percent confidence intervals of these estimates do not overlap. Hearings Award beneficiaries were more able to work than Initial Award beneficiaries when they were first receiving disability benefits, which is consistent with the findings of von Wachter et al. (2010).

A second feature is more striking. While the employment responses of both the Initial Award and the Hearings Award groups increase with benefit receipt, the response for the Initial Award group increases by more so that their employment response over two to four years of benefit receipt is higher than for the Later Award group over the same period. The Initial Award employment response reaches a peak of 25.8 percentage points at 2.5 years, when the Later Award response is a 22.4 percentage points. The response of the Initial Award group is in fact higher than the Later Award group at statistically significant levels between 2.25 and 4.5 years of benefit receipt. A similar

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<sup>38</sup> The Reconsideration group is omitted for clarity. The results for this group generally lie between the results for the other two groups. They are available in Appendix A1, as are the results for the SSI sample.

pattern is present in the SSI sample, although the peak for Initial Awardees occurs at 3.5 years and the confidence intervals overlap through most of the period.

These results suggest that the most clearly disabled individuals at the time of application improved the most, so much so that their employment is the higher after two years of benefit receipt than those initially denied benefits. DA&A reclassification rates support health improvement being behind this; in the DI sample they are lower for Initial (48 percent) than for Reconsideration (54 percent) and Later Awardees (57 percent). Those most readily defined as disabled when they applied were least likely to be defined as disabled when the DA&A program ended.

## 1.5 Conclusion

The significant growth in the DI and SSI programs over the past 25 years shows no signs of slowing. Meanwhile, the rate at which disability beneficiaries return to the labor force remains low and largely unchanged in recent years, despite an increasing fraction having low-mortality conditions and a greater focus on initiatives aimed at returning beneficiaries to the workforce.

The estimates provided here are more encouraging about the potential for disability beneficiaries to return to work than these patterns suggest. Many terminated DA&A beneficiaries did start working after they lost their disability benefits, especially relative to their poor work histories. The individuals in the SSI sample reached employment levels that were as high as at any time before they began receiving disability benefits, as did younger DI beneficiaries. This level of labor force re-attachment is

especially surprising given that there was little vocational support provided to help terminated beneficiaries re-enter the labor force (Stapleton et al., 1998).

It is not clear how the magnitudes of the employment response documented in this paper would translate to other types of beneficiaries. One way the beneficiaries studied here are atypical, at least for DI beneficiaries, is in terms of their poor earnings histories. Given that many individuals did not have earnings in the years before their disability application, it is likely that terminated beneficiaries returned to those previous sources of support, which may have included informal earnings and affected the size and nature of the employment response.

Furthermore, the possible interaction between cash payments and addiction adds a dimension to the relationship between benefits and an individual's condition that is normally not present for other medical conditions. The fact that the estimates are similar across individuals addicted to alcohol and drugs suggests that the response did not result from a strong interaction between substance abuse and disability payments, as the cash required to sustain a heavy alcohol addiction is very different to heavy heroin or cocaine addictions (Rhodes et al., 2000). In addition, the similarity of the response among those with either co-occurring mental disorders or musculoskeletal conditions means there are large and identifiable groups of current beneficiaries for whom the estimates are likely to be directly relevant.

Insights related to the heterogeneity in the employment response perhaps have broader implications. A consistent pattern of results across the DI and SSI samples show: (1) employment initially improves with time on benefits; (2) this improvement is greatest among those for whom disability payments are a poor substitute for wage earnings; (3)

those immediately awarded disability benefits experience greater improvement than other terminated beneficiaries. These results hold for subsamples of individuals who applied to be reclassified on the basis of mental disorders and musculoskeletal conditions. The results suggest that the health of some of the beneficiaries improved over time, and that forward-looking individuals took potential health improvements into account in their decision to apply for disability benefits in the first place.

This heterogeneity can provide guidance that may help to increase the targeting efficiency of medical reassessments of current beneficiaries. Disability beneficiaries are currently scheduled to have a Continuing Disability Reviews (CDR) every one, three or seven years, depending on the extent to which medical improvement is expected. In order to deal with resource constraints and backlogs, many of these reviews are either waived or in the form of a mailer that contains six questions about recent health work and training. Responses to this mailer generate a full CDR in 2.5 percent of cases, while CDR themselves generate terminations in 3-4 percent of cases (SSA, 2011b). While there is some profiling in terms of who is sent a mailer and who is subject to a full CDR, the findings here suggest a more focused role for comprehensive medical reassessments and a broader role for non-medical characteristics. Comprehensive reassessments after two or three years of benefit receipt may have better chances of terminations than earlier and later reviews. Focusing on the young and those with good work histories may also be a sensible way to allocate scarce resources.

In a similar way, the observed heterogeneity suggests ways in which current return-to-work initiatives may be made more effective. Reviews of the Ticket to Work program suggest that better outreach to interested beneficiaries would increase uptake,



and that the incentive structure for vocational service providers is such that those who target their services have the best chance of making a profit (SSA, 2008). Focusing outreach efforts on beneficiaries two or three years after they have begun to receive benefits, particularly if they are young or with good earnings histories, may increase voluntary exits through such initiatives.

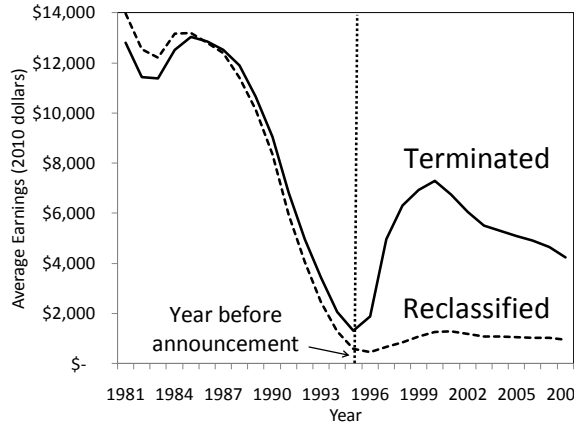
A relationship between the employment response and the time receiving disability benefits has important implications for interpreting studies that use the earnings histories of rejected applicants to estimate the likely employment of those who successfully become beneficiaries (e.g., Bound, 1989; Maestas et al., 2011; von Wachter et al., 2010). While these studies provide precise estimates of the employment potential of accepted applicants at the point they are applying for disability benefits, the dynamic effects identified here suggest we should be cautious about using these results to identify likely employment and employment differences within the disability beneficiary population.

The findings also speak to more fundamental questions about how the DI and SSI programs might be reformed. The relatively high employment of individuals awarded disability benefits early in the disability determination process suggests that it might not be optimal to only focus on tightening application criteria as a way of limiting program growth. In this sample, disability examiners' decisions favored those who were most able to work after two or three years of receiving benefits. Devoting more resources to CDRs or considering providing temporary disability for some conditions may increase long-term employment outcomes more than limiting access to benefits. The fraction of beneficiaries receiving CDRs has declined since 2000, despite SSA actuarial calculations that every dollar spent on CDRs generates about ten dollars in program savings (SSA,

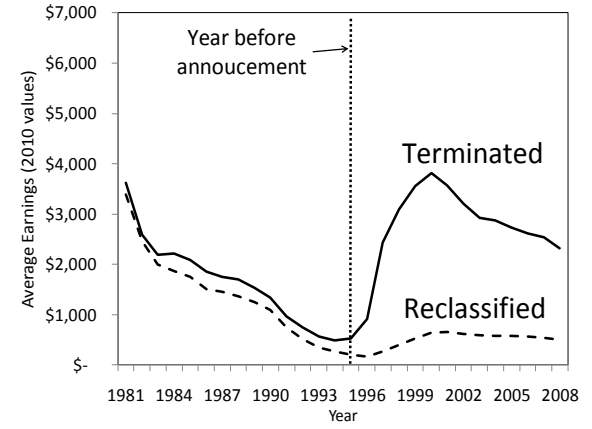
2011b). Recent reforms in the Netherlands, which included medical redeterminations of young disability beneficiaries and a limited period of sickness benefits before individuals could apply for permanent disability benefits, have led to significant reductions in the number of disability beneficiaries (Burkhauser, Daly and de Jong, 2008). The findings here suggest that similar reforms to disability programs in the United States are at least worthy of further consideration.

Figure 1.1: Mean Earnings and First SSA Payments, Terminated vs. Reclassified Beneficiaries, 1981-2008

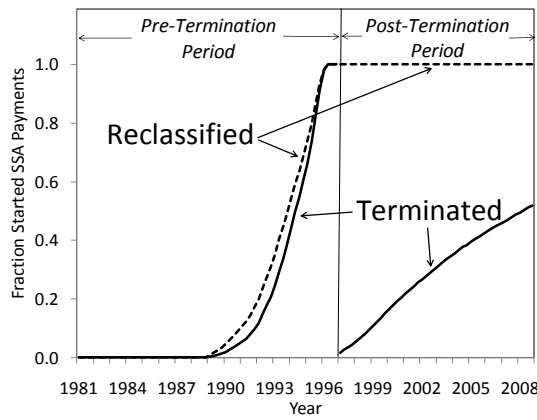
Panel A: Mean Annual Earnings of DI Sample



Panel B: Mean Annual Earnings of SSI Sample



Panel C: DI Sample's First SSA Payments Before and After End of DA&A Program



Panel D: SSI Sample's First SSA Payments Before and After End of DA&A Program

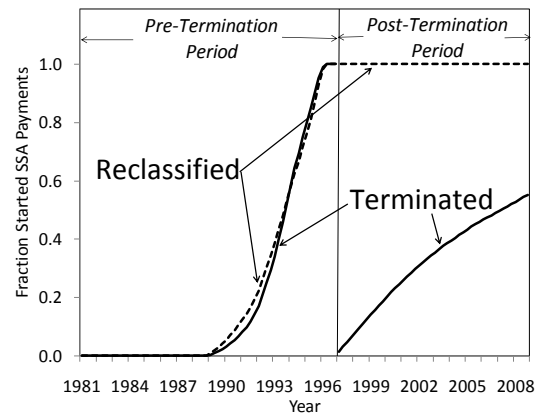
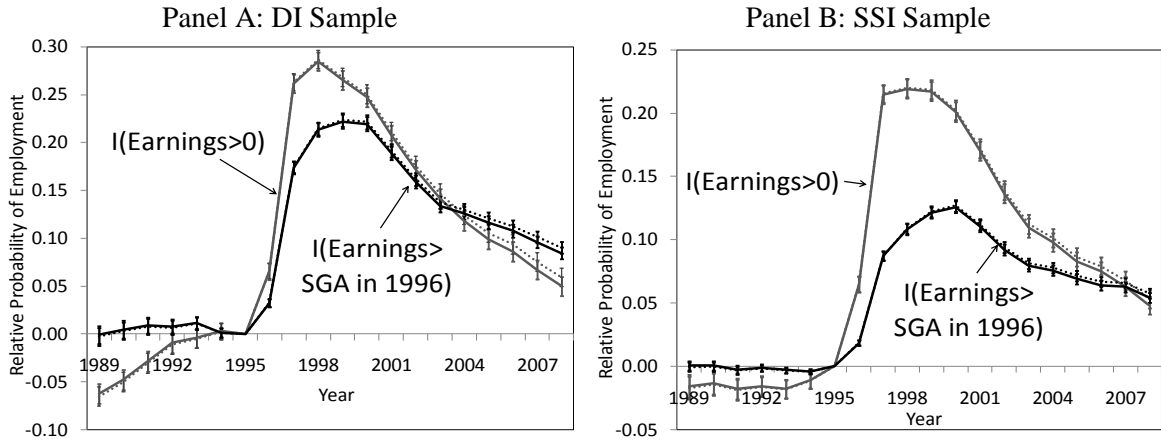
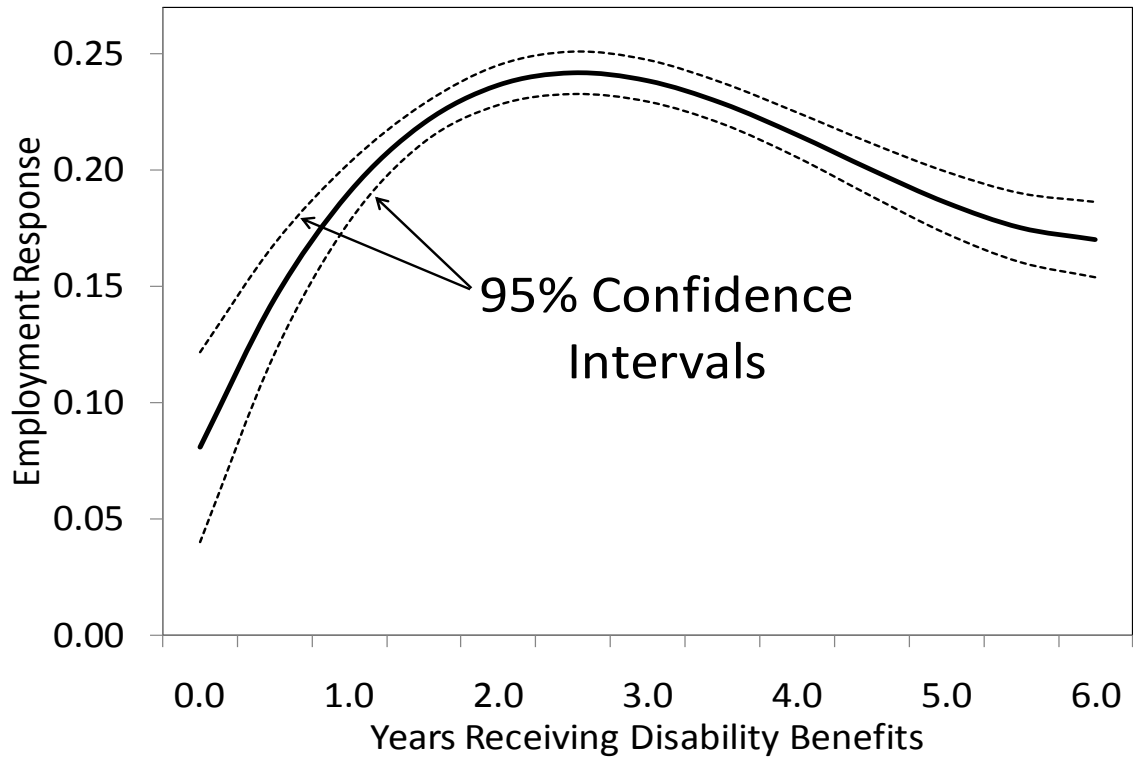


Figure 1.2: Estimates of Terminated Beneficiaries' Relative Probabilities of Employment, for Any Annual Earnings and Earnings Over 1996 Substantial Gainful Activity (\$8,339)



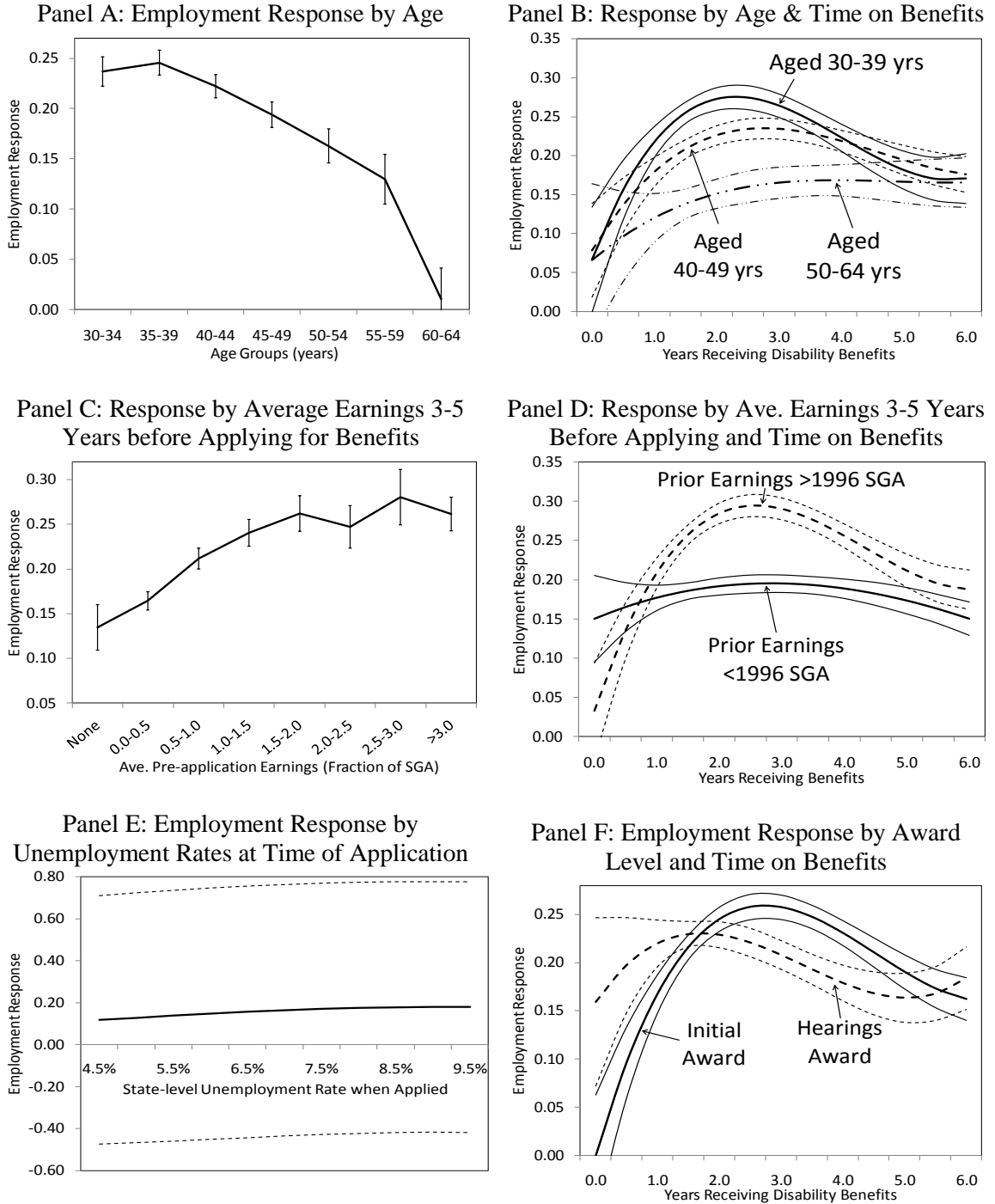
Notes: Regressions are based on equation (1) in text. Coefficients from regressions with individual fixed effects are in bold lines; coefficients from regressions without are in dotted lines. Vertical bars are 95 percent confidence intervals. Panel A and B regressions use 990,340 and 1,190,200 observations, respectively.

Figure 1.3: DI Employment Response by Time Receiving Disability Benefits



Notes: Employment is based on earning more than the 1996 SGA level. The figure shows the full effect of the post-termination increase in employment for different values of time on disability benefits. These are the nonlinear combination of the four coefficients (and standard errors) in Column (1) of Table 1.4. See the text and Table 1.4 notes for more details.

Figure 1.4: Heterogeneity in DI Employment Effects  
Using Annual Earnings Over 1996 SGA (\$8,339)



**Notes:** See the text and the notes in Table 1.3 and 1.4 for regression details. Estimates in Panels A, C and E use 990,340 observations. Panel B uses 329,980 (Aged 30-39), 420,960 (Aged 40-49) and 239,400 (Aged 50-64); Panel D uses 679,480 (Prior <SGA) and 310,860 (Prior ≥SGA); and Panel F uses 425,800 (Initial Award) and 435,040 (Hearings Award) observations.

Table 1.1: Timeline of the Termination of the DA&A Program During 1996

Key Dates	Action
March 29	US Congress passes legislation barring receipt of SSI and DI for beneficiaries who had an alcohol or drug addiction material to their eligibility.
April/May	SSA halts applications on this basis and identifies affected beneficiaries.
Late May/ Early June	These beneficiaries are sent letters informing them of the change and giving details about how they can have a new disability determination made or appeal on the basis they were misclassified under the DA&A category.
July 29	Last day to file a timely request or appeal. Filings by this date would be decided by the end of 1996. SSI benefits would continue if timely appeals were ongoing after this date (until decided).
December 31	Individuals unsuccessful in their disability determination or appeal are terminated from the DI and/or SSI program.

Source: Stapleton et al. (1998).

Table 1.2: Characteristics of DA&A DI Beneficiaries at Time of Program Termination

		Whole	DI Sample			SSI Sample		
		Sample	All	Termin.	Reclass.	All	Termin.	Reclass.
		(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sex	Male	71%	80%	82%	79%	65%	66%	63%
	Female	29%	20%	18%	21%	35%	34%	37%
Race	White	48%	58%	52%	62%	41%	37%	46%
	Black	42%	33%	38%	29%	48%	52%	43%
	Other	8.9%	8.0%	8.2%	7.7%	9.5%	9.6%	9.1%
Age in Jan 1997 (yrs.)	Mean	43.7	43.6	42.0	45.1	43.7	42.0	45.5
	(Std. dev.)	(7.85)	(7.96)	(7.03)	(8.34)	(7.78)	(7.09)	(8.23)
Education (yrs.)	Mean	10.4	10.8	11.0	10.6	10.1	10.3	10.0
	(Std. dev.)	(2.52)	(2.53)	(2.36)	(2.65)	(2.47)	(2.32)	(2.59)
Type of Addiction	Alcohol	55%	59%	54%	63%	52%	49%	56%
	Drugs	16%	15%	17%	14%	16%	18%	16%
	Both	29%	26%	29%	23%	31%	34%	29%
Time on benefits (yrs.)	Mean	3.19	3.03	2.81	3.27	3.29	3.29	3.40
	(Std. dev.)	(1.73)	(1.71)	(1.57)	(1.77)	(1.73)	(1.60)	(1.82)
1996 federal benefits (\$)	Mean	\$8,369	\$9,946	\$9,873	\$10,343	\$7,292	\$7,238	\$7,523
	(Std. dev.)	(2,483)	(3,040)	(2,856)	(3,024)	(1,097)	(1,147)	(807)
Obs.		139,170	56,461	20,229	29,288	82,709	30,077	29,433

Notes: There are 6,944 in the DI sample and 23,199 in the SSI sample who could not be classified as having kept or lost benefits as a result of the policy. For the main sample, race is missing or inconsistent for 1.6 percent and education is missing for 6.1 percent; these fractions are similar in the subsamples. Payments in 1996 are converted to 2010 dollars using the CPI-U.



Table 1.3: Parameterized Regression Estimates of the Employment Responses  
Based on Earning More Than 1996 Substantial Gainful Activity (\$8,339)

	All (1)	Type of Addiction			Disability When Reapplied		
		Alcohol (2)	Drugs (3)	Alcohol & Drugs (4)	Mental Disorders (5)	Musculo- skeletal (6)	Other Reapplic. (7)
<i>DI Sample</i>							
<i>RESPONSE<sub>it</sub></i>	0.2166 (0.0032)	0.2032 (0.0041)	0.2145 (0.0081)	0.2382 (0.0063)	0.2099 (0.0057)	0.2062 (0.0086)	0.2021 (0.0078)
<i>POSTTREND<sub>it</sub></i>	-0.0159 (0.0004)	-0.0162 (0.0005)	-0.0131 (0.0010)	-0.0164 (0.0008)	-0.0148 (0.0007)	-0.0144 (0.0011)	-0.0138 (0.0010)
R-squared	0.347	0.346	0.337	0.351	0.343	0.337	0.345
Observations	990,340	586,880	147,820	255,640	370,320	101,740	144,860
<i>SSI Sample</i>							
<i>RESPONSE<sub>it</sub></i>	0.1188 (0.0020)	0.1050 (0.0026)	0.1212 (0.0050)	0.1362 (0.0037)	0.1131 (0.0037)	0.1111 (0.0060)	0.1064 (0.0047)
<i>POSTTREND<sub>it</sub></i>	-0.0076 (0.0003)	-0.0074 (0.0003)	-0.0076 (0.0007)	-0.0078 (0.0005)	-0.0068 (0.0005)	-0.0084 (0.0008)	-0.0059 (0.0006)
R-squared	0.331	0.316	0.335	0.342	0.307	0.305	0.327
Observations	1,190,200	620,140	198,900	371,160	486,620	121,440	204,120

Notes: The dummy variable *RESPONSE<sub>it</sub>* equals one for years  $t \geq 1998$ , and zero otherwise. The variable *POSTTREND<sub>it</sub>* equals  $t - 1999$  for years  $t \geq 2000$ , and zero otherwise. Regressions also include a full set year dummy variables and sex-specific age cubics. The dummy variable for terminated beneficiaries (*TERMINATED<sub>i</sub>*) is interacted with the year dummy variables for 1989 to 1994 and 1996 to 1997. Errors are clustered on the individual. Standard errors are in parentheses and allow for within-person correlation in errors.

Table 1.4: Employment Response by Time on Benefits  
Using Annual Earnings Over 1996 SGA (\$8,339)

	DI Sample (1)	SSI Sample (5)
$RESPONSE_{it}$	0.0810 (0.0208)	0.0588 (0.0137)
$RESPONSE_{it} \times DISTIME_i$	0.1477 (0.0215)	0.0535 (0.0132)
$RESPONSE_{it} \times DISTIME_i^2$	-0.0413 (0.0063)	-0.0126 (0.0037)
$RESPONSE_{it} \times DISTIME_i^3$	0.0032 (0.0005)	0.0008 (0.0003)
Max. value of combined $RESPONSE_{it}$ coeff.	0.2421	0.1275
Value of $DISTIME_i$ where maximum occurs	2.54 yrs	2.95 yrs
Increase at maximum cf. total effect at 9 months	42%	38%
Increase at maximum cf. total effect at 6 years	42%	21%
R-squared	0.358	0.331
Observations	990,340	1,190,200

Notes:  $DISTIME_i$  measures the years on disability benefits before the terminations occurred in January 1997.  $RESPONSE_{it}$  equals one for years  $t \geq 1998$ , and zero otherwise.  $POSTTREND_{it}$  equals  $t - 1999$  for years  $t \geq 2000$ , and zero otherwise. The three cubic terms of  $DISTIME_i$  are also interacted with both the dummy identifying terminated beneficiaries and year dummy variables (1989-94, 1996-97). Regressions include also include a full set year dummy variables and sex-specific age cubic terms. Standard errors are in parentheses and allow for within-person correlation in errors.

Table 1.5: Employment Responses by Award Level  
Based on Annual Earnings Over 1996 SGA (\$8,339)

	DI Sample			SSI Sample		
	Initial Award (1)	Reconsid. Award (2)	Hearings Award (3)	Initial Award (4)	Reconsid. Award (5)	Hearings Award (6)
<i>RESPONSE<sub>it</sub></i>	0.2168 (0.0044)	0.2204 (0.0088)	0.2154 (0.0048)	0.1177 (0.0025)	0.1219 (0.0054)	0.1196 (0.0035)
p-values of Initial=Recons.	0.71			0.47		
Initial=Hearings	0.83			0.65		
<i>POSTTREND<sub>it</sub></i>	-0.0168 (0.0005)	-0.0157 (0.0011)	-0.0147 (0.0006)	-0.0077 (0.0003)	-0.0083 (0.0007)	-0.0073 (0.0004)
p-values of Initial=Recons.	0.32			0.41		
Initial=Hearings	0.01			0.41		
R-squared	0.349			0.330		
Observations	970,860			1,162,460		

Notes: Dummies identifying Initial, Reconsideration and Hearings Awardees are interacted with *RESPONSE<sub>it</sub>*, *POSTTREND<sub>it</sub>* and the *TERMINATED<sub>i</sub>* interactions with year dummies (1989-94, 1996-97). Standard errors are in parentheses and allow for within-person correlation in errors.

Chapter 2 – Liquidity, Economic Activity, and Mortality (with William N. Evans). To be published in *The Review of Economics and Statistics*, 94:2 (May 2012).

## 2.1 Introduction

Daily mortality counts fluctuate over the course of a calendar month, decreasing by about one percent below the average in the week prior to the 1<sup>st</sup> day of the month, and then increasing to almost one percent above the average in the first few days of the month (Phillips et al., 1999). This within-month mortality cycle is particularly pronounced for suicides, homicides, and accidents. Phillips et al. (p.97) speculate that this cycle may be driven in part by substance abuse, since “money for purchasing drugs or alcohol tends to be available at the beginning of the month and is relatively less available (for people with low incomes) at the end of the month.” Subsequent work has focused almost exclusively on the role that substance abuse plays in explaining this within-month pattern (Verhuel et al., 1997; Maynard and Cox, 2000; Halpern and Mechem, 2001; Swartz et al., 2003; Riddell and Riddell, 2006; and Li et al., 2007). In the most detailed study to date, Dobkin and Puller (2007) use administrative records from California to show there is a within-month cycle for hospital admissions of Supplemental Security Income recipients, with the cycle particularly pronounced for substance abuse admissions.<sup>39</sup>

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<sup>39</sup> In related work, Foley (forthcoming) finds a different monthly cycle for crimes motivated by financial gain, such as burglary, robbery and motor vehicle theft. In cities where transfers administered by the state government are paid at the start of the month, these crimes increase in the last few days prior to the 1<sup>st</sup> of

Although Phillips et al. (1999) document a within-month cycle for deaths not classified as due to substance abuse, none of the existing studies have considered an explanation outside the transfer payment/substance abuse nexus. In this paper, we show that the within-month mortality cycle is a more general phenomenon than is currently understood. Although the peak-to-trough of the within-month cycle is large in percentage terms for substance abuse deaths, these deaths account for a minority of the overall pattern. Updating and extending the earlier work of Phillips et al., we document within-month mortality cycles for many causes of death, including external causes, heart disease, heart attack, and stroke, but not cancer. The within-month cycle is also evident for both sexes and for all age groups, races, marital status groups, and education groups.

The broad-based nature of the within-month mortality cycle leads us to examine whether these cyclic patterns are present for various types of economic activity. To that end, we obtained daily data on a number of different activities and purchases, including going to the mall, visiting retail establishments, purchasing lottery tickets, going to the movies, and the amounts spent on food and non-food retail purchases. These data all show the same pattern, namely, that economic activity declines toward the end of the month and rebounds after the 1<sup>st</sup> of the month.

The concordance between the mortality and activity cycles leads us to conclude that an increase in economic activity after the 1<sup>st</sup> of the month leads to the increase in mortality. For some causes of death, this link is obvious: one cannot die in a traffic accident unless one is in traffic. While it is not so obvious for other causes of death, it is well-documented in the medical literature that certain types of consumption (e.g., eating

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the month and then decline after the 1<sup>st</sup>, a pattern he attributes to the same lack of liquidity towards the end of the month.

heavy meals) and activity (e.g., shoveling snow and exercising) are triggers for heart attacks and strokes.

We provide suggestive evidence that the within-month mortality and economic activity cycles are linked to changing liquidity over the month. First, we document that the peak-to-trough in mortality and consumption is largest for people expected to have the greatest liquidity issues, such as those with low levels of education and income, and those on federal transfer programs. Second, of all the goods and activities we examine, the largest swing in consumption is for lottery tickets: a good that can only be purchased with cash in many states. Finally, we provide direct evidence of a short-term increase in mortality after the receipt of income.

Much of the direct evidence for this last result is provided in Chapter 3, where we consider five different situations in which we can identify when a group of people received an income payment. In each case we find that mortality increases immediately after income receipt. One of these situations is the 2001 tax rebate checks, where mortality increased among 25-64 year olds by 2.7 percent in the week after the checks arrived. In this paper, we extend the analysis to show that this mortality effect was 5.2 percent on the three occasions when these checks arrived at the end of the month – when we believe that liquidity issues are most acute – and 1.6 percent otherwise.

With wages and transfers frequently paid around the 1<sup>st</sup> of each month, the apparent link between liquidity, economic activity and mortality seems to be a consequence of people not smoothing their consumption in accordance with the life-cycle/permanent income hypothesis. Many authors have demonstrated that consumption displays “excess sensitivity” to the arrival of predictable income payments (e.g. Wilcox,

1989; Shea, 1995; Parker, 1999; Souleles, 1999; Johnson et al., 2006). Our work is most similar to Stephens (2003), who found seniors consume more after receiving Social Security checks, and Stephens (2006), who demonstrates that UK workers consume more after payday.

It is not clear how much of this within-month variation is mortality displacement (i.e. the timing of deaths is altered by a few weeks) or additional deaths. The fall in deaths in the last few days of the month and the analysis of one-off payments in Chapter 3 suggests that many of the deaths are being shifted from nearby periods. In any case, there are implications for researchers trying to understand the relationship between activity and mortality, and also for researchers whose phenomena of interest may be obscured by this pattern.

Our work also has implications for a growing literature on mortality over the business cycle. In contrast to a large literature suggesting that higher incomes are protective of health, work by Ruhm (2000) and others suggests that mortality is procyclical, although the reason for this result remains uncertain. In the final section of the paper we show that the death categories with the greatest peak-to-trough in the within-month mortality cycle are also those categories most strongly tied to the business cycle. This suggests that rising mortality in a boom is produced by the increased economic activity generated by a robust economy.

## 2.2 Replicating and Expanding the Basic Findings

### 2.2.1 Pooling Samples from 1973-2005.

The primary data for this analysis are the Multiple Cause of Death (MCOB) data files compiled by the National Center for Health Statistics (NCHS). They contain a unique record of each death occurring in the United States, which includes information about the decedent's age, race, gender, place of residence, and cause of death.<sup>40</sup> Exact dates of death were reported on public use data files starting in 1973, but with the redesign of the public use layout in 1989, this information is now only available on restricted-use versions of the data.<sup>41</sup> Permission to use the restricted data was obtained from the NCHS. Combining the 1973-1988 public use files with the 1989-2005 restricted-use data provides us with information on over 71.5 million deaths.

In Figure 2.1, we graph of the within-month mortality cycle using deaths for the entire 1973-2005 period. The horizontal axis shows days in relation to the 1<sup>st</sup> of the month: *Day 1* is the 1<sup>st</sup>.<sup>42</sup> To provide symmetry, we report the 14 days prior to the 1<sup>st</sup> and the first 14 days of the month, a total of 336 (12\*28) days per year. The height of the graph represents the relative risk of death on a particular day, computed as the average deaths on a given day divided by the average deaths across all days. Thus, a value of 1.1 represents a 10 percent increase in the daily risk of death. The relative risk is represented

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<sup>40</sup> Detailed information about the Multiple Cause of Death data files is available at the NCHS web site, [http://www.cdc.gov/nchs/products/elec\\_prods/subject/mortmcd.htm](http://www.cdc.gov/nchs/products/elec_prods/subject/mortmcd.htm).

<sup>41</sup> Available at the NCHS Research Data Center (NCHS/RDC), <http://www.cdc.gov/nchs/r&d/rdc.htm>.

<sup>42</sup> As in Phillips et al. (1999), the labeling is ..., *Day -2, Day -1, Day 1, Day 2, ...*. Not using a zero allows us to matching the *Day 1* to *Day 14* dummy variables with the first 14 days of the calendar month.



by the hollow circles, while the vertical lines from the circles are 95 percent confidence intervals.<sup>43</sup>

The shape of the graph is similar to that in Phillips et al.<sup>44</sup> Starting about 12 days before the 1<sup>st</sup> of the month, daily deaths decline slowly and fall to 0.8 percent below the average on the day before the 1<sup>st</sup>. Deaths then increase on the 1<sup>st</sup> of the month to 0.6 percent above average. The peak-to-trough represents about a 1.4 percent difference in daily mortality rates. With an average of 5,938 deaths per day in our sample, the increase in deaths from the last day of the month to the 1<sup>st</sup> represents 81 deaths per month, or about 970 deaths per year.

This within-month mortality cycle remains once we control for a set of covariates in a regression similar in structure to that in Stephens (2003). Let  $Y_{dmy}$  be counts of deaths for day  $d$  in month  $m$  and year  $y$ . Days are organized in relation to the 1<sup>st</sup> of the month, so  $d$  goes from -14 to 14. Months do not follow the calendar; instead, they are the 28 days surrounding the 1<sup>st</sup> of the month. *Month 1* contains data from December 18 through January 14 of the next year, *Month 2* from January 18 through February 14, and so on. Synthetic years begin fourteen days before the 1<sup>st</sup> of January. Given this structure for the data, the econometric model we estimate is:

$$(1) \quad \ln(Y_{dmy}) = \alpha + \sum_{\substack{d=-14 \\ d \neq -1}}^{14} \text{Day}(d)\beta_d + \sum_{j=1}^6 \text{Weekday}(j)_{dmy}\gamma_j + \sum_{j=1}^M \text{Special}(j)_{dmy}\varphi_j + \mu_m + v_y + \varepsilon_{dmy} \quad (2.1)$$

Where  $\text{Day}(d)$  is a dummy variable equal to one if it is day  $d$  and zero otherwise,  $\text{Weekday}(j)$  is one of six dummy variables for the different weekdays, and  $\text{Special}(j)$  is

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<sup>43</sup> We use the delta method to construct the variance of the risk ratio. The variance of daily deaths is calculated as follows. Let  $N_t$  be the number of people alive at the start of day  $t$ , and the probability of death that day equal  $p_t$ . Since this is a set of Bernoulli trials, expected deaths ( $d_t$ ) is  $E[d_t] = N_t p_t$ , and the variance of deaths is  $V[d_t] = N_t p_t (1 - p_t) = \sigma_t^2$ . A consistent estimate of  $p_t$  is  $d_t / N_t$ .

<sup>44</sup> Using data from 1973-1988 only, we are able to replicate the basic results in Phillips et al. (1999).

one of  $J$  dummy variables for special days throughout the year.<sup>45</sup> The variables  $\mu_m$  and  $\nu_y$  capture synthetic month and year effects, and  $\varepsilon_{dmy}$  is an idiosyncratic error term.<sup>46</sup> The reference day is the day prior to the start of the month (i.e.  $Day(-1)$ ), and the reference weekday is Saturday. We estimate standard errors allowing for arbitrary correlation in errors within each unique 28-day synthetic month.

In Table 2.1, we report estimates for the 27  $Day(d)$  coefficients from equation (2.1) when controlling for all the other covariates listed above. Even with the regression adjustment, we find a large within-month mortality cycle with daily mortality counts about one percent higher after the start of the month and the estimate has a z-score of 8.9.

To better understand the magnitude of the results in Table 2.1, we alter the model in equation (2.1) and replace daily dummy variables with dummy variables for weeks in relation to the 1<sup>st</sup> of the month. We include three dummy variables:  $Week(-2)$  includes  $Day(-14)$  to  $Day(-8)$ ,  $Week(1)$  includes  $Day(1)$  to  $Day(7)$ , and  $Week(2)$  includes  $Day(8)$  to  $Day(14)$ . The reference period is the week before the 1<sup>st</sup> of the month ( $Week(-1)$ ).

Results for this model are listed in the top row of Table 2.2. Mortality is 0.9 percent higher in the first week of the month than in the preceding week, and this result has a z-score of about 5. On average, the first week of the month has about 4,324 more deaths than the previous week.

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<sup>45</sup> We include unique dummies for a list of reoccurring special days, including January 1<sup>st</sup> and 2<sup>nd</sup>, the Friday through Monday associated with all federal holidays occurring on Mondays (Presidents' Day, Martin Luther King Jr. Day since 1986, Memorial Day, Labor Day, Columbus Day), Super Bowl Sunday and the Monday afterwards, Holy Thursday through Easter Sunday, July 4<sup>th</sup>, Veteran's Day, the Monday to Sunday of the week of Thanksgiving, a dummy for the days from the day after Thanksgiving to New Year's Eve, plus single day dummies for December 24<sup>th</sup> through December 31<sup>st</sup>. We also reduce the number of homicides on September 11, 2001 by 2,902 deaths, which according to a Center for Disease Control report was the number of deaths on that date due to the terrorist attacks <http://www.cdc.gov/mmwr/preview/mmwrhtml/mm51SPa6.htm>. In models of fatality counts for specific demographic groups, such adjustments are not possible so we add a dummy variable for September 11, 2001.

<sup>46</sup> The results throughout the paper are similar when we interact the month and year dummy variables.

## 2.2.2 Does the Within-Month Cycle Extend Past Substance-Abuse Related Deaths?

We now examine how much of the within-month cycle is due to substance abuse. Each observation in the MCODE data has up to 20 causes of death, coded according to the International Classification of Disease (ICD) codes. During our period of analysis, the MCODE used three different versions of the ICD codes: ICD-8 (1973-78), ICD-9 (1979-98), and ICD-10 (1999-2005). In this section, we focus on when the ICD-9 coding system was used, as the specificity of the codes used to identify substance abuse varies substantially across the three versions.

Given that our primary concern is to examine the mortality cycle for deaths unrelated to substance abuse, we err on the side of including too many deaths in the substance abuse category rather than too few. Phillips et al. (1999) define a death as substance abuse-related if it has a primary or secondary cause related to alcohol or drug use.<sup>47</sup> We expand this definition in two ways. First, we use a broader set of ICD-9 codes to identify substance abuse by adding conditions attributable to alcohol or drugs contained in studies on the economic costs of substance abuse in the United States (Harwood et al., 1998), Australia (Collins and Lapsley, 2002), and Canada (Single et al., 1999).<sup>48</sup> Second, a death is classified as a substance abuse death if these codes are listed as any of the 20 causes, rather than just the first two. As a result of our broader definition

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<sup>47</sup> They use the following ICD-9 codes: 291 (drug psychoses), 292 (alcohol psychoses), 303 (drug dependence), 304 (alcohol dependence), 305.0 and 305.2-305.9 (non-dependent abuse of drugs except tobacco), 357.5 (alcoholic polyneuropathy), 425.5 (alcoholic cardiomyopathy), 535.3 (alcoholic gastritis) 571.0-571.3, (chronic liver disease and cirrhosis with mention of alcohol), 790.3 (excessive blood alcohol level), E860 (accidental poisoning by alcohol), 947.3 and E977.3 (alcohol-use deterrents), and 980 (toxic effect of alcohol).

<sup>48</sup> A complete list of these codes is provided in Appendix A2 that is available from the authors.

of substance abuse, we define a far higher proportion of deaths as related to substance abuse (4.4 percent) compared to Phillips et al. (1.7 percent).

Figure 2.2 contains the relative daily mortality rates for deaths related to substance abuse (in Panel A) and deaths not related to substance abuse (Panel B). There is a large peak-to-trough for substance abuse deaths. For the four days prior to the 1<sup>st</sup> of the month, deaths are about two percent below the daily average, before spiking on *Day(1)* to four percent above the daily average. Panel B contains the results for deaths not related to substance abuse. The magnitude of the within-month cycle for this sample is nearly identical to the graph for all deaths in Figure 2.1. The trough occurs on *Day(-1)* and the peak occurs on *Day(1)*, with a difference of more than one percent. The cycle present in Figure 2.1 is not caused solely by substance abuse.

These patterns remain once we estimate the model using the natural log of fatality counts regressed on weekly dummies and the various controls contained in equation (1). The second row of Table 2.2 contains the coefficients on the weekly dummies for all deaths occurring between 1979 and 1998, with the reference period being *Week(-1)*. The results for this limited sample are virtually identical to those for the full sample reported in the first row of the table.

The results for substance abuse and non-substance abuse related deaths appear in the third and fourth rows of Table 2.2. Substance abuse deaths are 3.0 percent higher in the first week of the month compared to the previous week, while for non-substance abuse related deaths this number is 0.77 percent. Notice, however, that there is an average of only 257 substance abuse deaths per day, so a three percent increase means 647 more deaths per year in the first week of the month compared to the previous week.

By comparison, deaths not related to substance abuse average 5,622 per day, so there are 3,636 more of these deaths per year in the first week of the month compared to the last. Therefore, although substance abuse deaths are more cyclic than other causes, they account for only 15 percent of the within-month mortality cycle.

### 2.2.3 Heterogeneity across Demographic Groups

Exploiting the information about decedents in the MCOD data, we can show that the within-month mortality cycle is present for a wide variety of demographic subgroups. In the first row of Table 2.3, we report the *Week(-2)*, *Week(1)* and *Week(2)* coefficients for the full sample from Table 2.2. In the remaining rows of the table, we estimate separate models for subgroups based on sex (male, female), race (white, black, other race), marital status (single, married, widowed, divorced), and age (under 18 years, 18 to 39 years, 40 to 64 years, over 65 years).<sup>49</sup>

The results indicate the breadth of the phenomenon: in all groups, deaths are at least 0.5 percent higher in the first week of the month compared to the previous week and these coefficients are statistically significant at conventional levels. The size of the cycle is large for some groups. The coefficient on *Week(1)* for males is 37 percent larger than for females (although we cannot reject the null the coefficients are the same). Compared to whites, the *Week(1)* coefficients for blacks is four times larger and for Hispanics it is three times larger. The effect for divorced people is 3.5 times than the effect for singles, while for younger people aged 18-39 it is nearly four times larger than for people over 65 years old.

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<sup>49</sup> In a later section of the paper, we generate results by education level.

The results suggest a few things about the within-month mortality cycle. First, the persistence of the effect across all demographic groups suggests that the explanation for the within-month cycle must extend past those on transfer programs, as suggested by Phillips et al. (1999). Second, groups that generally have lower incomes and a greater propensity for liquidity issues have larger within-month cycles, with the larger cycle for males than females the only anomaly in this pattern. We show in the next section, however, that the within-month cycle is particularly pronounced for external causes and heart attacks, and it may be that the differences in results across genders result from these causes having a higher incidence rate among males.

#### 2.2.4 Disaggregating Deaths into Detailed Causes

The breadth of this phenomenon can also be seen in the within-month mortality patterns for different causes of death. We create 15 subgroups based on primary cause of death that are consistently defined across ICD-8, ICD-9 and ICD-10.<sup>50</sup> Four groups are based on external causes (motor vehicle accidents, suicide, homicide, and other external causes) and four are cancer-related groups (breast cancer, leukemia, lung cancer, and other cancers). The remaining categories are heart attacks; heart diseases other than heart attack; chronic pulmonary obstructive disease (COPD); stroke; alcohol-related cirrhosis; cirrhosis not related to alcohol; and a category composed of deaths not included in the previous groups.

The monthly patterns for all of these categories are shown in Figure 2.3. Panel A to Panel D includes the relative daily mortality rates for the four external cause

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<sup>50</sup> Each ICD version has several thousand individual codes, but the changes from version to version mean only large death categories can be consistently defined throughout the sample. The exact mapping of deaths is provided in Appendix A2.

categories: motor vehicle accidents, suicides, murders, and other external causes (such as accidents and drowning). All have a dip before the 1<sup>st</sup> of the month and a spike on the 1<sup>st</sup>. Deaths increase on the 1<sup>st</sup> by 6 percentage points for motor vehicle accidents and suicide, 9 percentage points for murder, and 4 percentage points for other external causes.

External cause-of-death categories are clearly connected to the role of substance abuse. More interesting is that the within-month mortality cycle is present in a number of the other cause-of-death categories. Panel E shows the pattern for deaths in which the primary cause was a heart attack. These deaths increase by more than two percent from the last day of the month to the 1<sup>st</sup>. Other heart diseases, shown in Panel F, display a similar pattern, although the peak-to-trough is of a slightly smaller magnitude (around one percent). The same pattern is observed for COPD (Panel G) and stroke (Panel H), with average differences between deaths on the last day of the month and the 1<sup>st</sup> of 1.8 percent for COPD and 1.0 percent for stroke. In all cases, the 95 percent confidence intervals are below the daily average in the last few days of the month and above the average in the first few days of the month.

The pattern is slightly different for cirrhosis. Alcohol cirrhosis deaths (Panel I) are above the average daily rate between the 4<sup>th</sup> and the 14<sup>th</sup> of the month, peaking at four percent above the average on the 9<sup>th</sup> of the month. Non-alcohol cirrhosis deaths (Panel J) exhibit a similar pattern, increasing above the average on the 4<sup>th</sup> of the month and then peak about three percent above the average on the 8<sup>th</sup> of the month. As short-term changes in cirrhosis are influenced by changes in liver toxicity which occurs with a lag (Cook and Tauchen, 1982), the results are consistent with higher consumption early in the month.

Finally, Panels K to N contain deaths for different types of cancers. Breast cancer (Panel K) and leukemia (Panel L) deaths exhibit no discernible pattern. There is a slight dip below the average prior to the 1<sup>st</sup> for lung cancer deaths (Panel M), but there is an equivalent dip in the first few days of the month, which differs from the general pattern. A similar pattern occurs for other cancers (Panel N). Unclassified deaths (Panel O) show the same pattern as aggregate mortality.

The regression-adjusted pattern for these specific causes of death is investigated using equation (1.2). The week-of-month coefficients are shown in Table 2.4. Focusing on the *Week(1)* dummy, there are statistically significant increases in mortality during the first week for all causes of death except lung cancer, breast cancer, and leukemia. We find a small within-month cycle for other cancers. The largest within-month cycles are (in descending order): suicides, homicides, COPD, alcohol cirrhosis, non-alcohol cirrhosis, and motor vehicle accidents. The percentages of deaths in each category that are defined as related to substance abuse are shown in Table 2.4: heart attacks, heart disease, stroke, COPD, and non-alcohol cirrhosis display within-month cycles yet few deaths in these categories are connected to substance abuse.

The existence of a within-month cycle across many conditions provides further evidence of a phenomenon that requires a more general reason than alcohol and drugs use. The absence of the relationship in leukemia and breast and lung cancer deaths also limits the possibility that the cycle is due to the way in which death records are kept. Given that many types of cancer are generally found to be unrelated to socioeconomic status (Phelan et al., 2004; Espinosa and Evans, 2008), this also increases the possibility that income and economic activity play some role in the phenomenon.



## 2.3 Linking Mortality to Economic Activity

We require a more general explanation of the within-month mortality cycle than changing levels of substance abuse. The causes of death that demonstrate the most cyclical suggest that economic activity spurs on mortality, which means a drop in activity before the 1<sup>st</sup> of the month and the rise in activity after the 1<sup>st</sup> can explain the basic pattern of results.

While the link between economic activity and mortality is obvious for traffic accidents and other external causes that occur outside of the home, extensive empirical evidence suggests that an increase in activity temporarily raises the risks of other causes of death. Nowhere is this more evident than in the literature on the triggers for heart attacks. Strenuous exercise (Mittleman et al., 1993), sexual activity (Moller et al., 2001), eating a heavy meal (Lipovetsky et al., 2004), the Christmas season (Phillips et al., 2004), and shoveling snow (Heppell et al., 1991) are all found to increase the incidence of heart attacks and/or deaths from heart attacks.

Given the structure of the MCOB data, we are unable to directly link increased economic activity to mortality. We can show, however, that there is a within-month consumption cycle for some specific activities and purchases. In each case, we have data aggregated to the daily level and, as a result, we use models similar to those estimated for equation (2.1).

The first product we consider is the purchase of lottery tickets. Most states run lotteries with “daily number” games, where contestants pay \$1 to pick a three or four digit number and win \$500 or \$5000, respectively, if their number is selected. We were

able to obtain data on the daily tickets purchased for Pick 3 and Pick 4 games in two states: Maryland and Ohio. Lottery ticket purchases are an interesting product line to consider because many credit card issuers prohibit the purchase of tickets by credit cards. In some states, including Maryland, retailers are prohibited from accepting credit card payments for lottery ticket purchases. Therefore, for most lottery transactions, consumers must use cash. If liquidity is an issue for consumers near the 1<sup>st</sup> of the month, then the within-month cycle for lottery tickets should be particularly large.

Maryland and Ohio have twice-daily Pick 3 and Pick 4 games, although Ohio has no drawings on Sunday and Maryland only had a single Sunday drawing prior to May 23, 2004. We obtained daily ticket sales for the Pick 3 and Pick 4 games in Maryland from January 1, 2003 to the end of 2006, and for Ohio from June 20, 2005 through June 16, 2007.

The dependent variable is the natural log of daily sales, and we control for the same covariates as those in equation (2.1). In models with the Maryland data, we include a dummy that equals one for Sundays starting on May 23, 2004, to account for the extra draw on that day. We allow for arbitrary correlation in the errors within each unique 28-day synthetic month.

The results from these models are reported in the first two rows of Table 2.5. The Maryland and Ohio lotteries both have a pronounced within-month purchase cycle: ticket purchases in the first week of the month are 7.1 percent and 8.8 percent higher compared to the previous week, respectively. Both of these results are statistically significant.

A nationwide consulting firm for the retail trade sector that conducts a large daily survey of retail establishments and malls<sup>51</sup> provided us with data on average daily foot traffic through malls (from 1/1/2000 to 12/22/2007), all retail establishments (from 1/4/2004 to 12/22/2007) and apparel establishments (1/4/2004 to 12/22/2007). The outcome of interest is the natural log of foot traffic through the establishments. The model for these outcomes is the same as above, except that we omit Christmas Day as traffic on that day is substantially smaller than during the rest of the year. The results are also reported in Table 2.5. For malls, all retail outlets and apparel stores, foot traffic is estimated to be 2.1, 3.4 and 3.3 percent higher during the first week of the month compared to the previous week. These data show a pronounced within-month cycle.

We obtained data on daily box office receipts for the top ten grossing movies from [www.boxofficemojo.com](http://www.boxofficemojo.com) for January 1, 1998 to June 7, 2007. With this data, we use the natural log of the box office receipts as the outcome of interest and use the same covariates as in the previous model, with one exception. New movies are usually released on Fridays and the top movies can change dramatically from week to week, so we define a week as a Friday to a Thursday and add a dummy variable for each unique week in the data.<sup>52</sup> The results for movies are reported in the sixth row of Table 2.5 and we see that the first week of the month generates 5.6 percent more in revenues than the previous week.<sup>53</sup>

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<sup>51</sup> As per our user agreement, we cannot identify the producers of the data.

<sup>52</sup> Movie release dates are based on holidays and seasons; they do not seem to consistently occur at the start or end of the month (Einav, 2007).

<sup>53</sup> The difference between unadjusted (i.e. raw data) and regression-adjusted results is largest for this outcome. The single biggest movie-going week of the year is Christmas Eve to New Year's Eve. Over this period, average daily gross of the top 10 movies is more than twice the average during the rest of the year. Therefore, a plot of average daily gross by days in relation to the 1<sup>st</sup> of the month would show a tremendous spike in attendance before the 1<sup>st</sup> of the month. However, adding the list of special days to the regression controls for the Christmas effect on movies.

We did not find a within-month cycle for two activities for which we obtained daily data. First, we used data on daily attendance at major league baseball games for the 1973-98 and 2000-04 seasons<sup>54</sup> from [www.retrosheet.org/schedule/index.html](http://www.retrosheet.org/schedule/index.html). The unit of observation is a game at a particular stadium and the dependent variable is log attendance. We control for standard covariates including dummies for opening and closing day of the season, a dummy for whether it was before Memorial Day or after Labor Day, indicators for double headers, dummies for whether it was a day or night game interacted with weekday dummies, plus dummies for the team pair at a given stadium in a year.<sup>55</sup> We find no within-month cycle in baseball attendance.

Second, we obtained Washington DC Metro subway ridership figures from January 1, 1997 to September 19, 2007. The outcome of interest is log ridership and the extra controls are dummies for Redskin home games, days during the Cherry Blossom festival, and five dummies for exceptionally large crowds on the mall such as for the Million Man March. The results for this model, presented in the last row of Table 2.5, show no within-month mortality cycle.

These results above are consistent with tests of the life cycle/permanent income hypothesis in which authors have found that predictable changes in income *do* affect consumption. Stephens (2003) found an increase in the consumption of time-sensitive purchases, like perishable food and eating at restaurants, among seniors after the receipt of Social Security checks. Using data for the United Kingdom, Stephens (2006) found an increase in consumption after the receipt of paychecks. Among Food Stamp recipients, Shapiro (2005) found a drop in daily caloric consumption of 10-15 percent over the food

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<sup>54</sup> There was no attendance data for the 1999 season on the web site.

<sup>55</sup> For example, there were separate season dummies for each Red Sox/Yankees game at Fenway.

stamp month, a result he finds consistent with hyperbolic discounting. Likewise, Mastrobuoni and Weinberg (2009) found food consumption declines between Social Security payments among seniors with a high fraction of income coming from Social Security, while Hastings and Washington (forthcoming) use store scanner data and found grocery purchases increase at the start of the month even though prices are slightly higher then.

## 2.4 Is Liquidity Responsible for these Within-Month Cycles?

The previous two sections show there are within-month mortality and economic activity cycles that are similar in nature. There is suggestive evidence that these cycles may be due to liquidity, such as the fact that the mortality cycle is greatest for those we would expect to have more liquidity issues (younger people, females, minorities, divorcees). The most striking evidence is that the one good that must be purchased with cash, lottery tickets, shows the largest peak to trough at the 1<sup>st</sup> of the month. In this section, we provide three pieces of further evidence that liquidity problems at the end of the month are responsible for the within-month cycles.

First, we use data from the Consumer Expenditure Survey to show there is a within-month cycle in individual purchasing behavior, and that this cycle is more pronounced for groups we anticipate have greater liquidity issues at the end of the month. Next, we demonstrate the within-month mortality cycle is largest for those with the lowest education levels. Finally, we provide evidence that the receipt of income leads to a short-run increase in mortality.

### 2.4.1 Heterogeneity in the Within-Month Consumption Cycle: Evidence from the Consumer Expenditure Survey

We further examine consumption activity using data from the Diary Survey component of the Consumer Expenditure Survey (CEX), in which purchases of frequently purchased items (e.g. food, personal care items, and gasoline) are recorded. The CEX is produced by the Bureau of Labor Statistics. The sampled unit for the Diary Survey is a consumer unit (CU), which is a household containing related family members. Beginning at different points in the month, each CU provides detailed information about purchases for a 14-day period.

We use three CEX data files containing information on people who began their two-week diaries from 1996 to 2004. The first is the Consumer Unit Characteristics and Income File, which contains data about the household and its head. The second is the Member Characteristics Income File, which records the income of each CU member. The third is the Detailed Expenditure File. This lists each item's purchase date, price, and Universal Classification Code, which enables items to be grouped into detailed product categories. We have data from 57,972 CUs and roughly 715,000 daily observations, or about 12 daily observations per CU.

We create three daily expenditure categories for each household. The first is all food purchases, including fast food and restaurant purchases. The second is called non-food items, and consists of alcohol, cigarettes, apparel, gasoline, entertainment, personal products, personal services, and over-the-counter drugs. The third is the sum of these two categories. We create the same synthetic month categories as before (December 18<sup>th</sup>

through January 14<sup>th</sup> is *Month 1*, etc.), and convert all expenditures into real December 2008 values.<sup>56</sup>

The dependent variable is real daily expenditure in dollars for the household, and the regressions are similar to those using equation (2.1). Additional covariates include complete sets of dummies for each household head's age, sex, race, marital status, and education. We also include a complete set of controls for the region of residence, size of the urban area, family size, and reported income. The key explanatory variables are *Week(-2)*, *Week(1)*, and *Week(2)*, with the week prior to the 1<sup>st</sup> of the month serving as the reference period.

In the first panel of Table 2.6, we report regression estimates for all the CUs in our sample. All three purchase categories have the familiar within-month cycle. Food purchases during the first week of the month are 27 cents higher than the preceding week, an amount that is 1.8 percent of the sample mean. Non-food items show a statistically insignificant increase of 16 cents a month. The purchase of all items is 42 cents higher (1.5 percent of the sample mean) in the first week of the month than in the previous week. The magnitudes of these results are similar to the size of the peak-to-trough in the within-month mortality cycle.

The start of the month is a focal point of economic activities for many households. In the 1996-2004 CEX sample, about ten percent of respondents who receive a paycheck do so monthly, and we suspect a large fraction are paid on or near the 1<sup>st</sup> of the month. Furthermore, most federal transfer programs distributed checks on or near the 1<sup>st</sup> of the month. Social Security recipients claiming benefits prior to April of

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<sup>56</sup> For synthetic *Month 1*, we use the January CPI, for synthetic *Month 2* (January 18<sup>th</sup> through February 14<sup>th</sup>) we use the February CPI, etc. This approach avoids creating CPI-induced “jumps” on the 1<sup>st</sup> of the calendar month.

1997 receive checks on the 3<sup>rd</sup> of each month, while Supplemental Security Income benefits are paid on the 1<sup>st</sup> of the month.<sup>57</sup> In an email survey of state Temporary Assistance for Needy Family programs, we found that 30 of 41 states that responded distribute checks during the first week of the month.

Likewise, many families have periodic bills that are due on or near the 1<sup>st</sup> of the month. In our CEX samples, half of all households who made a mortgage or rent payment during their 14-day survey period did so between the day before the 1<sup>st</sup> of the month and the first week of the month, with 14 percent paying on the 1<sup>st</sup> of the month. Since most rent and mortgage payments must be paid by check or cash, uncertainty about whether there will be enough in the bank at the start of the month may force some to limit their spending until these bills are paid.

In the rest of the panels in Table 2.6, we provide more evidence that liquidity issues affect these within-month cycles by showing that the groups we would expect to have liquidity issues are precisely those groups with the greatest within-month cycle in the purchases they make.

First, we create sub-samples based on household income by dividing the CEX sample into households with annual incomes of less than \$30,000 and households with incomes of \$30,000 and more.<sup>58</sup> Results for these two groups are reported in the second and third panels in the first row of Table 2.6. Among low income households, we find a statistically significant coefficient on the *Week(1)* dummy for the food and total spending categories. In the total purchases model, for example, the coefficient of 78 cents is about

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<sup>57</sup> Or on the closest prior business day if the normal payment date is a Saturday, Sunday, or public holiday.

<sup>58</sup> There is a third income group: those not reporting income. We have 194,060 observations for this group. Their results look similar to the results for low income families, which is not surprising as the average education of the reference person in these households is close to the education of the reference person in the low income group.



four percent of the sample mean. Among families with an income of \$30,000 or more, we actually find a negative and statistically significant coefficient on the *Week(1)* dummy variable for food purchases.

Next, we divided the sample into three groups based on the household heads' education: 1) those with less than a high school education; 2) those with a high school education or some college; and 3) those with a college degree or more. The results are presented in the second row of Table 2.6. In the least-educated households, food expenditure increases considerably after the 1<sup>st</sup> of the month: the *Week(1)* coefficient is a statistically significant 98 cents, or 8 percent of the sample mean. These households' expenditure on all items in *Week(1)* is also positive and statistically significant. Among CUs with a high school educated head, there are statistically significant within-month purchase cycles in the food and all items categories. In the all items category, the coefficient on the *Week(1)* dummy is \$0.73, or about 2.8 percent of the sample mean for daily spending. Finally, for the most educated group, we find no evidence of a within-month cycle for any spending category and, like the highest income group, statistically insignificant negative *Week(1)* coefficients for food purchases and all purchases.

In the final group of results, presented in the final row of Table 2.6, we group households based on their receipt of government income. The first group consists of households with any federal or state income assistance other than Social Security. Most of these families received income from either the Temporary Assistance for Needy Families (TANF) or the Supplemental Security Income (SSI) programs. There is a large within-month cycle for this group, with food purchases \$2.87 higher (21 percent of the sample mean) and total purchases \$3.48 (15 percent of the sample mean) during the first

week of the month compared to the previous week. The *Week(1)* coefficient on non-food consumption is also positive, but not statistically significant.

The second group consists of households receiving Social Security but no other government income. This group is similar to the sample used in Stephens (2003), although his 1986-96 sample are all paid on the 3<sup>rd</sup> of the month, while our 1996-2004 sample contains some Social Security recipients being paid at other times of the month.<sup>59</sup> As the results in Table 2.6 indicate, we find positive and statistically significant *Week(1)* coefficients for these households' purchases of food items (73 cents), non-food items (54 cents) and all items (123 cents), which represent about five percent of the daily mean in each category.

The third group in this block of results is a sample of households with neither Social Security income nor income from other federal or state transfer programs. This set of estimates provides no evidence of a within-month purchase cycle.

These results suggest liquidity drives the consumption cycle. Households receiving government transfers or with low income or education display such a cycle, while high income and educated households do not. The results may be consistent with a hyperbolic discounting, as suggested by Shapiro (2005) and Mastrobuoni and Weinberg (2009).

#### 2.4.2 Mortality Results by Education Levels

In this section, we examine the heterogeneity in the within-month mortality cycle based on the education of the deceased. Since 1989, the MCOB file has included the

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<sup>59</sup>Those claiming Social Security pre-May 1997 are paid on the 3<sup>rd</sup> of the month, while newer beneficiaries are paid on the second, third or fourth Wednesday of the month depending, respectively, on whether the birth date is on the 1<sup>st</sup>-10<sup>th</sup>, 11<sup>th</sup>-20<sup>th</sup>, or 21<sup>st</sup>-31<sup>st</sup>. <http://www.socialsecurity.gov/pubs/calendar.htm>.

decedent's education, which is usually provided by the next of kin.<sup>60</sup> Educational attainment is strongly and positively correlated with households' wealth and financial savings (Juster et al., 1999), so education should provide a proxy for those with and without liquidity constraints.

We group decedents into three categories: those whose highest education is less than high school completion, those who completed high school but not college, and those who completed college.<sup>61</sup> The results from regressions with week-of-month dummies for these three education-based groups are shown in Table 2.7. The within-month cycle is present for all three education groups. With *Week(-1)* again the reference week, the largest coefficient on *Week(1)* is for those who did not complete high school (1.0 percent), followed by high school completers (0.93 percent) and those with a college education (0.45 percent). The *Week(2)* coefficients display the same pattern; they are higher for high school non-completers (0.93 percent) than high school completers (0.72 percent) and college-educated decedents (0.23 percent). This last coefficient is the only *Week(1)* or *Week(2)* coefficient that is not statistically significant at conventional levels. These mortality patterns are consistent with changing liquidity over the month, as those with less education are most likely to have liquidity problems.

The mortality results show the same general pattern as in consumer spending, namely, that the within-month peak-to-trough decreases as educational attainment

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<sup>60</sup> In 1989, 21 states reported an education for at least 90 percent of decedents. This number rises to 42 states by 1995 and 48 states by 2005. Sorlie and Johnson (1996) assessed the accuracy of education listed on death certificates, and found that certificates match survey data obtained prior to death in about 70 percent of cases. When they differ, the death certificate generally overstates reported education.

<sup>61</sup> Between 1989 and 2002, the number of years of schooling rather than education outcomes is recorded in the MCODE file. Decedents were classed as having less than a high school education if they reported three or fewer years of high school; having a high school education if they completed four years of high school but fewer than four years of college; and having completed college if they had four or more years of college education.

increases. A difference, however, is that we find a statistically significant first-week effect for mortality for the most educated group, while there is no discernible first-week effect in consumer spending for this group. There are large day-to-day differences in spending, both within and across households, which make Type II errors more likely in that analysis than in the mortality models, where we have large samples and more predictable within-month differences.

### 2.4.3 Income Receipt and Mortality: The 2001 Tax Stimulus Checks

The evidence in the first two parts of this section is circumstantial with regard to our liquidity/economic activity/mortality hypothesis. We now exploit the unique characteristics of the 2001 Tax Stimulus Checks to provide direct evidence that income receipt results in a short-term increase in mortality. We also show that this effect is primarily driven by the relaxation of liquidity, and that the results are consistent with liquidity problems being most acute at the end of the month. Some of the results in this section are also reported in Chapter 3.

The *Economic Growth and Tax Relief Reconciliation Act* (PL107-16), signed into law on June 7, 2001, was a sweeping tax bill that lowered individual and capital gains tax rates, increased the child tax credit, and made changes to estate and gift taxes. The portion of the Act we consider is the reduction in the tax rate in the lowest income bracket from 15 percent to 10 percent. This tax change was applied retroactively to all income earned in 2001 and, as an advance payment on the tax cuts, households with taxable income in 2000 were sent rebate checks between June and September of 2001. The maximum rebates for single and married taxpayers were \$300 and \$600,

respectively. Johnson et al. (2006) estimates that households received about \$500 on average, or about one percent of median annual family income. Approximately two-thirds of all households received a rebate check.

Rebate checks were mailed on ten successive Mondays, and check distribution dates were based on the second-to-last digit of the Social Security number (SSN) of the person filing taxes.<sup>62</sup> The first checks were sent to taxpayers whose second-to-last SSN digit was a zero on Monday, July 23, and the last checks were sent to taxpayers whose second-to-last digit was a nine on Monday, September 24.<sup>63</sup> The last three digits of the SSN are effectively randomly assigned. Johnson et al. (2006) exploit this fact using data from a special module in the CEX to show that consumption of nondurable goods increased in the months after the rebate was paid. Agarwal et al. (2007) perform similar tests using administrative data on credit card charges.

We use a similar approach to examine the short-run consequences of the rebates on mortality. This is possible because the NCHS merged the second-to-last digit of a decedent's SSN from the National Death Index<sup>64</sup> to the 2001 MCODE data files at our request. We initially report the basic findings, before showing that these rebates affect mortality in a manner consistent with the resolution of liquidity as the precipitating event.

Given that we have variation across groups in the timing of income payments from the 2001 rebates, the econometric model we use is a difference-in-differences specification. The outcome of interest is the natural log of mortality counts  $Y_{it}$ , where  $i$

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<sup>62</sup> For married taxpayers filing jointly, the first Social Security number on the return determined the mailing date.

<sup>63</sup> The other checks were sent on the following dates (second-to-last digit of SSN): July 30 (1), August 6 (2), August 13 (3), August 20 (4), August 27 (5), September 3 (6), September 10 (7), September 17 (8).

<sup>64</sup> The NDI is designed to assist researchers who want to ascertain whether subjects in their studies have died, and includes each decedent's SSN. More information about the NDI can be found at [www.cdc.gov/nchs/ndi.htm](http://www.cdc.gov/nchs/ndi.htm).

indexes groups of people based on the second-to-last digit of their SSN ( $i=1$  to 9), and  $t$  indexes one of 30 seven-day periods that begin ten weeks prior to the first check being distributed and end ten weeks after the last check was sent. The estimating equation is of the form:

$$(2) \quad \ln(Y_{it}) = \alpha + REBATE_{it}\beta_1 + \eta_i + v_t + \varepsilon_{it} \quad (2.2)$$

where  $REBATE_{it}$  is a dummy variable that equals one in the week that group  $i$ 's rebate checks arrive. The parameter  $\beta_1$  therefore measures the percentage change in weekly mortality associated with rebate check receipt. The fixed effect  $\eta_i$  captures persistent differences in mortality across groups; however, no such differences are expected because of the random assignment of the second-to-last digit of a SSN. The fixed effect  $v_t$  captures differences in weekly mortality counts that are common to all groups but vary across weeks. The September 11 terrorist attacks occurred during Week 18 in our analysis, and the deaths for that week are about twenty percent above the average.<sup>65</sup> The week effects will capture these changes so long as the deaths associated with September 11 are equally distributed across the 10 SSN groups. The remaining variable in the model is  $\varepsilon_{it}$ , which is a random error term.

A key to the analysis is to reduce the sample to people with taxable income in 2000, as they were the only ones to receive a tax rebate. Estimates of taxable income are reported in the Annual Demographic file for the March Current Population Survey (CPS) data (King et al., 2004) and data from the 2001 survey (2000 tax year) suggest that 52 percent of people aged 25 to 64 were in households that paid federal income taxes, while

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<sup>65</sup> <http://www.cdc.gov/mmwr/preview/mmwrhtml/mm51SPa6.htm>.

the comparable number for people aged 65 and older was 26 percent. Therefore, we restrict our attention to people aged 25 to 64.

Even with this restriction, the sample includes many non-taxpayers. It also includes couples who filed their taxes jointly but who were not listed first on the IRS 1040 form, as their household's check was mailed according to their spouse's SSN rather than their own. The IRS 1040 form does not record the sex of the taxpayers, so we cannot ascertain whether husbands or wives are more likely to be listed as the first taxpayer. As both non-taxpayers and the second person listed on joint tax returns should be randomly distributed across the different groups, our results should be systematically biased towards zero. The parameter  $\beta_I$  does not measure the impact of check receipt, but rather the intention to treat with a check.

The results for equation (2.2) are reported in the first column of Table 2.8. There is a statistically significant 2.7 percent increase in mortality for adults aged 25-64 the week rebate checks arrive. We cannot reject the null hypothesis that the group fixed effects are all zero, which provides support for the conjecture that the latter digits of the SSN are randomly assigned. The results suggest a large short-term increase in mortality immediately after income receipt.

We use information from March CPS data to identify individuals likely to have been 'treated' by a tax rebate. It is not clear *a priori* how the estimates should change. A higher fraction of taxpayers means more treated people, but it also means a larger fraction of people with higher incomes, who would be expected to have fewer liquidity problems. Single males aged 25 to 64 is a sample likely to have filed taxes in their own name, and it contains a high fraction of people who paid taxes in the previous year (in excess of 75

percent). Results for this ‘high income, high treatment’ group are presented in column (2). There is a large and statistically significant short-run mortality effect of 4.7 percent. At the opposite end of the spectrum, we estimate the model using a sample of seniors aged 65 and older, a group with a low fraction of people who received a tax rebate (about one quarter). Results for this group are reported in column (3); we find no impact of the rebate on mortality among seniors.

We postulate that a lack of liquidity at the end of the month leads to a decline in mortality, before liquidity and mortality increase on the 1<sup>st</sup> of the month. If so, rebate checks arriving towards the end of the month will relieve liquidity to a much greater degree than those arriving at other times, and should have a commensurately greater effect on mortality.

To see if this is the case, we compare how mortality changed on the three occasions that checks arrived in the last week of the calendar month to the other seven weeks in the rebate payment period.<sup>66</sup> In column (4) of Table 2.8 we estimate the same model as in column (1), except that we allow the coefficient on  $REBATE_{it}$  to vary based on whether the check was received during the last week of the month or at some other time. The effect of receiving a check at the end of the month is large, with mortality increasing by a statistically significant 5.2 percent. This is in contrast to a 1.6 percent increase (t-statistic of 1.37) at other times of the month. There is a p-value of 0.11 on the null hypothesis that both coefficients are equal. The results fit with our prediction that households are liquidity-constrained towards the end of the month, and that this constraint affects their short-term mortality risks.

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<sup>66</sup> These weeks begin on the following Mondays: July 23, August 27, and September 24, 2001.



The results from the 2001 tax rebate shows that the receipt of income leads to a short-term increase in mortality. In Chapter 3, we test for this phenomenon in four other settings. The first two tests exploit the pay structure of Social Security. First, we follow Stephens (2003) by examining seniors who enrolled in Social Security prior to May 1997. These recipients typically received their Social Security checks on the 3<sup>rd</sup> of the month. For this group, deaths decline just before Social Security receipt and are highest the day after payment. Second, seniors enrolling after April 1997 are paid on the second through fourth Wednesday of the month, depending on their birth date. In these younger cohorts, mortality is highest on the days checks arrive.

The third test in Chapter 3 follows Hsieh's (2003) use of Alaska Permanent Fund dividend payments. They find that in the week that direct deposits of Permanent Fund dividends are made, mortality among urban Alaskans increases by 13 percent. Finally, we consider active duty military wage payments made on the 1<sup>st</sup> and 15<sup>th</sup> of the month. Among 17 to 64 year olds in counties with a large military presence, they find that mortality increases by nearly 12 percent the day after mid-month paychecks arrive, while over the same period, there is no change in mortality in counties with little military presence.

These five cases link short-term increases in mortality directly to the receipt of income, providing strong evidence of a connection between liquidity and mortality.

## 2.5 Explaining Mortality over the Business Cycle

A large literature has established that health outcomes are better among individuals with higher socioeconomic status (Kitigawa and Hauser, 1973). This has

been documented for nearly all measures of health and health habits, including mortality (Backlund et al., 1999), self-reported health status (House et al., 1990), child health (Case et al., 2002), smoking (Chaloupka and Werner, 2000), and biomarkers (Seeman et al., 2008).

In contrast to this work is a group of papers that show mortality is pro-cyclical. The basic statistical relationship has been documented for the United States (Ruhm, 2000) and several OECD countries (Gerdtham and Johannesson, 2005; Neumayer, 2004; Tapia Granados, 2005), and for many outcomes including deaths from heart disease (Ruhm, 2000), traffic fatalities (Evans and Graham, 1988), infant health (Dehejia and Lleras-Muney, 2004), and self reported health status (Ruhm, 2003). The one death category that shows a decidedly counter-cyclical pattern is suicides (Ruhm, 2000; Tapia Granados, 2008).<sup>67</sup>

There is no definitive explanation for why mortality is pro-cyclical. Some patterns of behavior are consistent with the opportunity cost of time increasing when an economy strengthens. For example, Ruhm (2005) finds that physical fitness declines and obesity rises in good times, while Ruhm (2007) finds there are fewer medical interventions for heart disease during booms, despite more heart disease deaths occurring during these periods. Mortality is pro-cyclic among retirees and others outside of the labor force, however, casting doubts on the extent to which this mechanism explains the phenomenon (Edwards, 2008; Miller et al., 2009).

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<sup>67</sup> From an econometric standpoint, the socioeconomic status/health literature and the literature on pro-cyclic mortality are measuring different movements in income. Typical measures of socioeconomic status include variables such as education, wealth, income, or occupational status, which can all be considered measures of permanent income. In contrast, the econometric models used to test the cyclicity of mortality all use within-group estimators that hold state characteristics constant and ask whether year to year fluctuations in the unemployment rate alter mortality. These latter models are therefore measuring the impact of transitory changes in economic activities on mortality.

Another possible explanation is that some consumption and economic activity, which increases over the business cycle, has harmful effects (Ruhm, 2000; Tapia Granados, 2008). This explanation involves similar linkages to the ones we have explored in this paper. If similar forces do create pro-cyclical mortality, then the causes of death with the greatest within-month cycles should also be those most strongly tied to the business cycle.

To see if this is the case, we compare the pro-cyclicity of mortality to the within-month cycle for the 15 cause of death categories presented in Table 2.4, using MCOD data for the 1976-2004 period. The methodology for analyzing the pro-cyclicity of mortality dates to Evans and Graham (1988), and is typified in Ruhm (2000). Using pooled time-series/cross-sectional data at the state level, mortality rates are regressed on state and year effects, demographic covariates, and a measure of the business cycle, which is typically the unemployment rate.

Let  $M_{it}$  be the mortality rate for state  $i$  in year  $t$ , defined as deaths per 100,000 people. The model we estimate is of the form:

$$(4) \quad \ln(M_{it}) = X_{it}\beta + UNEMP_{it}\alpha + u_i + v_t + \varepsilon_{it} \quad (2.3)$$

Where  $X_{it}$  is a vector of demographic characteristics,  $u_i$  and  $v_t$  are state and year effects and  $\varepsilon_{it}$  is an idiosyncratic error term. The key covariate is the state  $i$ 's unemployment rate in year  $t$  ( $UNEMP_{it}$ ). In the model, we include in  $X_{it}$  the fraction of people who are under 18, the fraction who are 65 and over, and the fraction that are black. We allow for arbitrary correlation in the errors within a state, and weight observations by population size.

Results from this regression are reported in Table 2.9. In the first row, we report estimates for all-cause mortality. Similar to Ruhm (2000), we find a large, negative and statistically significant impact of the unemployment rate on mortality. A one percentage point drop in the unemployment rate will increase mortality by about 0.4 percent.

In the next 15 rows, we show estimates of the pro-cyclicality of mortality for specific causes that are consistent with previous estimates. Traffic accidents, murders, other external causes, heart attacks, COPD, and the ‘all other causes’ category have pro-cyclical relationships and p-values of at least 0.1. There are statistically significant counter-cyclical results for suicides, lung cancers and other cancers, while diseases like breast cancers, leukemia, heart disease, and non-alcohol cirrhosis have weak relationships with the business cycle.

This pattern of results is similar to the within-month pattern. To demonstrate this point, in Figure 2.4 we plot the coefficients on the unemployment rate from Table 2.9 along the x-axis and the within-month peak-to-trough estimates (the coefficient on the *Week(1)* dummy variable) from Table 2.4 on the y-axis. The graph shows a pronounced negative relationship, and the correlation coefficient between the two series is -0.4. There is one obvious outlier: suicides, which have a large within-month cycle but are decidedly counter-cyclical.<sup>68</sup> When we exclude suicides from the calculation, the correlation between the coefficients on the remaining 14 causes of death rises to -0.8. It is important to stress that we are not testing a particular hypothesis, and the results in Figure 2.4 do not indicate a causal relationship. Rather, the strong negative correlation between the two sets of coefficients in Figure 2.4 is meant to indicate that the most pro-

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<sup>68</sup> The counter-cyclical pattern in suicides is concentrated among males in the working-age population (Wu and Cheng, 2010). It may be that unemployment directly heightens the risks of suicide in a way that swamps any consumption or related effects.

cyclical death categories are in general the same categories that exhibit the greatest within-month mortality cycle, suggesting that similar processes are driving both results.

If the within-month mortality cycle is indeed due to changes in economic activity, then the similarity in the results across death categories between this cycle and the pro-cyclicality of mortality provides suggestive evidence that liquidity-related economic activity is the underlying cause for both. This also helps in reconciling pro-cyclical mortality with the literature on socioeconomic status and health. Typical measures of socioeconomic status include education, wealth, income, and occupational status, which can be considered measures of permanent income. While within-month fluctuations are clearly transitory, the similarity of within-month and pro-cyclical mortality suggests business cycle changes in employment and income should also be thought of as transitory at the aggregate level, despite some long-term effects at the individual level.

## 2.6 Conclusion

When daily counts of deaths in the United States are arranged around the 1<sup>st</sup> day of the calendar month, what emerges is a clear pattern of deaths decreasing during the final days of the month, and then spiking on the 1<sup>st</sup>. We show that this within-month mortality cycle is a broad-based phenomenon that is common to most subgroups and many causes of death. It cannot be satisfactorily explained by changes in drug and alcohol consumption alone.

We find that consumer purchases, mall visits and cinema attendance exhibit similar within-month cycles. While we do not have economic activity and mortality data in a single dataset, medical knowledge of the triggers for specific health conditions,

combined with the similarity of the demonstrated mortality and activity patterns, suggests that short-term changes in economic activity may be the missing explanation for the within-month mortality cycle. Furthermore, these patterns are consistent with liquidity changing over the month and affecting levels of economic activity and, in turn, the number of deaths on a given day.

These results link medical literature on the within-month mortality cycle to the literature on consumption smoothing, with implications for both. For the medical literature, understanding that substance abuse is only part of the within-month mortality cycle means liquidity and payments have broader medical effects than is commonly thought. For consumption smoothing, this pattern points to the potential breadth of the excess sensitivity of consumption to the timing of payments. We use over 70 million deaths in our analysis. If the within-month cycle is mainly due to liquidity changes affecting individuals' economic activity, then excess sensitivity and its explanations – such as hyperbolic discounting – must not be limited to narrow subpopulations.

The magnitudes of the mortality patterns we describe are not small relative to other movements in aggregate mortality rates. In Table 2.2, we estimate that mortality is 0.86 percent higher in the first week of the month compared to the last week. Throughout the sample period, this would have resulted in 4,324 more deaths in the first week of the month than in the last. On the basis of our business cycle calculations, this is equivalent to the additional deaths generated by a half percentage point decline in the unemployment rate.

In order to understand whether there are potential gains to smoothing liquidity we need to know whether short-term variation in liquidity and activity is actually changing

the *total* number of deaths, or merely changing the *timing* of deaths of susceptible people by several days (what epidemiologists refer to as “harvesting”). For some causes, such as motor vehicle accidents and other external causes, it is logical that more activity leads to an increase in deaths; for conditions like heart attacks, the answer is not so clear. Analysis of one-off payments in Chapter 3 suggests for some cases such as heart attacks, much of the variation in mortality may be harvesting, although more work needs to be done to understand this issue properly.

There are some potential policy implications suggested by our results. For example, the within-month mortality cycle and the heightened mortality associated with income receipt might suggest that emergency rooms, hospitals, police, and fire departments should adjust staffing levels in accordance with predictable high- and low-mortality days. Our search of the Internet has so far not provided any anecdotal evidence that such adjustments already exist.

Our results also suggest a complex relationship between income and mortality that may have implications for how and when people are paid. If the resolution of liquidity drives the within-month mortality cycle, then more frequent paychecks may reduce mortality. In contrast, it could be the case that having money in your pocket leads people to engage in activities that are hazardous. If this is the case, the increasing the frequency of payments may make things worse. Chapter 3 provide some evidence to this point when they note that the second paycheck of the month for the military generates particularly pronounced mortality. The recent movement by some states to distribute

welfare payments multiple times each month may provide a potential test for these competing hypotheses.<sup>69</sup>

Finally, the results have implications for our understanding of the pro-cyclicality of mortality. The causes of death with the largest within-month mortality cycle also exhibit the most pro-cyclical mortality, suggesting that whatever drives the within-month mortality cycle also causes mortality to be pro-cyclical. Short-term changes in liquidity are more easily separated from permanent levels of income over the course of a month than over a business cycle. The similarity of the two mortality phenomena suggests that the apparent contradiction between the protective effect of income and the pro-cyclicality of mortality can be resolved by viewing business cycle movements as events that lead to medium-term changes in liquidity, which then affect economic activity and the mortality risks people face.

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<sup>69</sup> Any effort to smooth mortality by increasing paycheck frequency must be weighed against the costs. Previous work on pro-cyclical mortality suggests that the welfare benefits of such smoothing may be small (Edwards, 2009).



Figure 2.1: Relative Daily Mortality Risk (95% Confidence Intervals) by Day in Relation to the 1st of the Month, 1973-2005 MCOB, All Deaths, All Ages

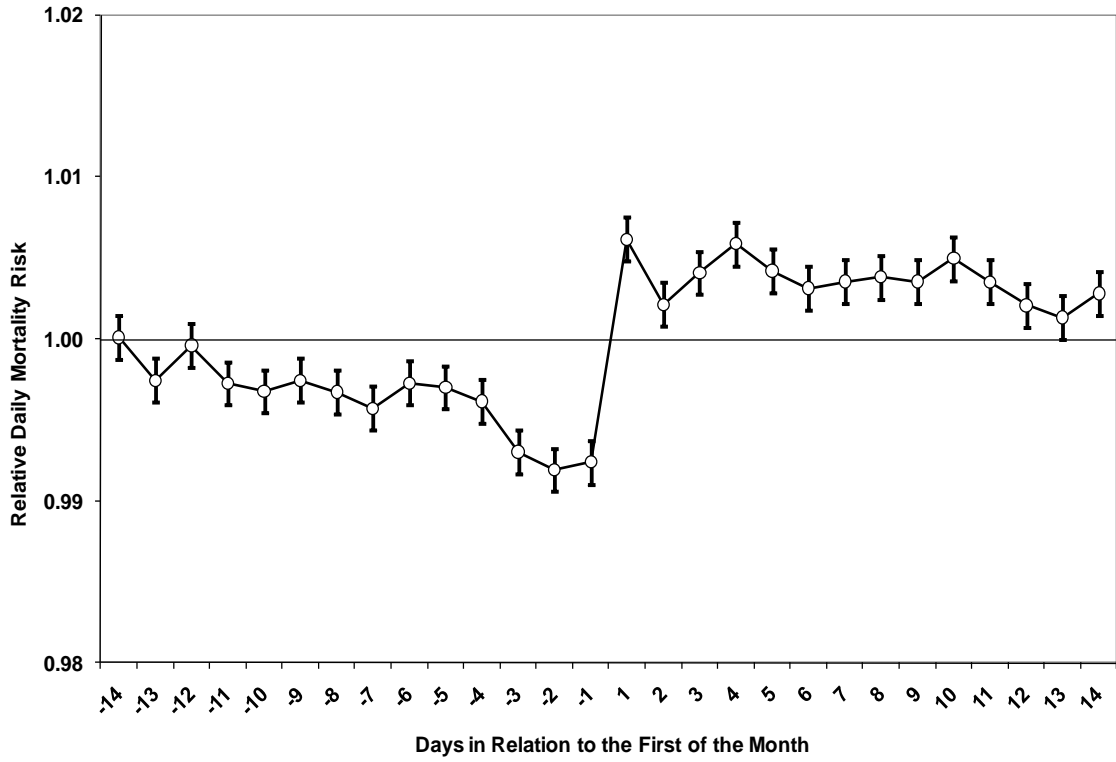
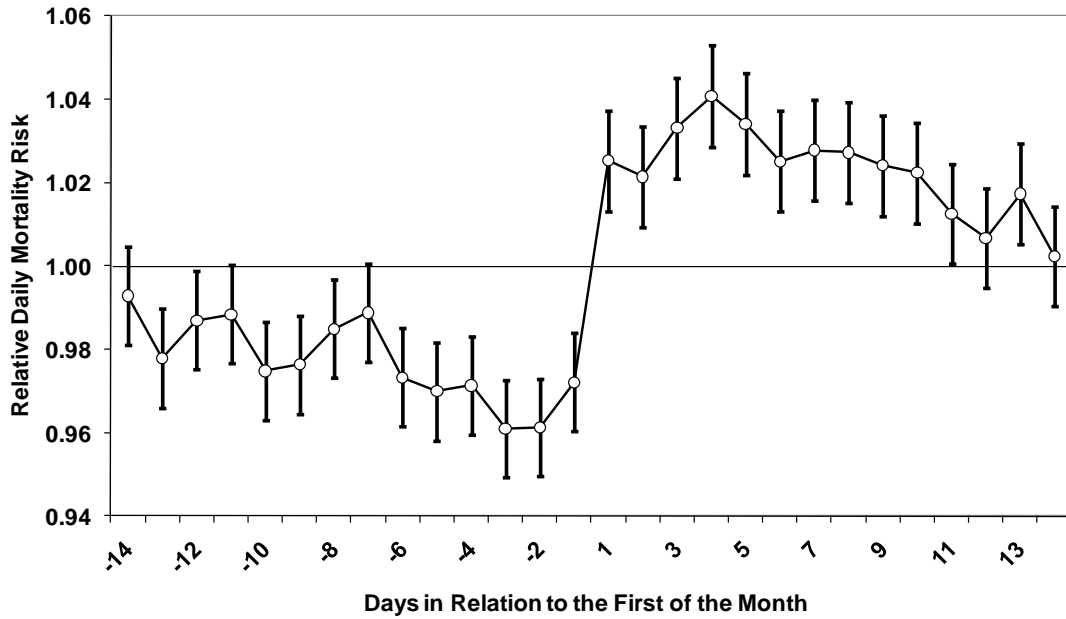


Figure 2.2: Relative Daily Mortality Rates (95% Confidence Intervals), With and Without Mention of Substance Abuse, MCOB Data 1978-1988, All Ages

**A: Substance Abuse Related**



**B: Non-Substance Abuse Related**

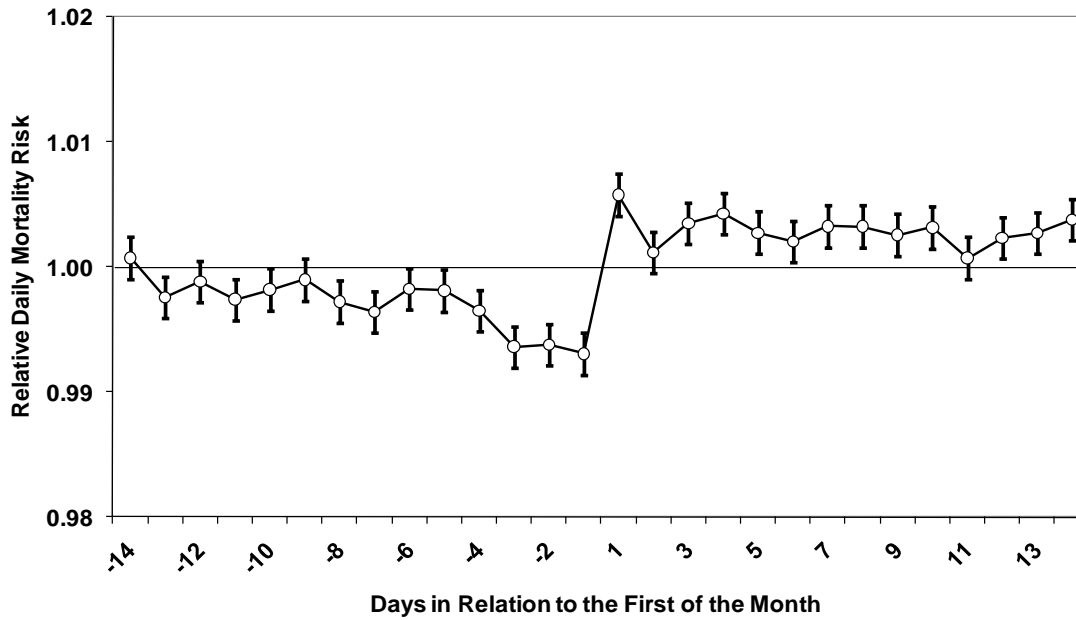
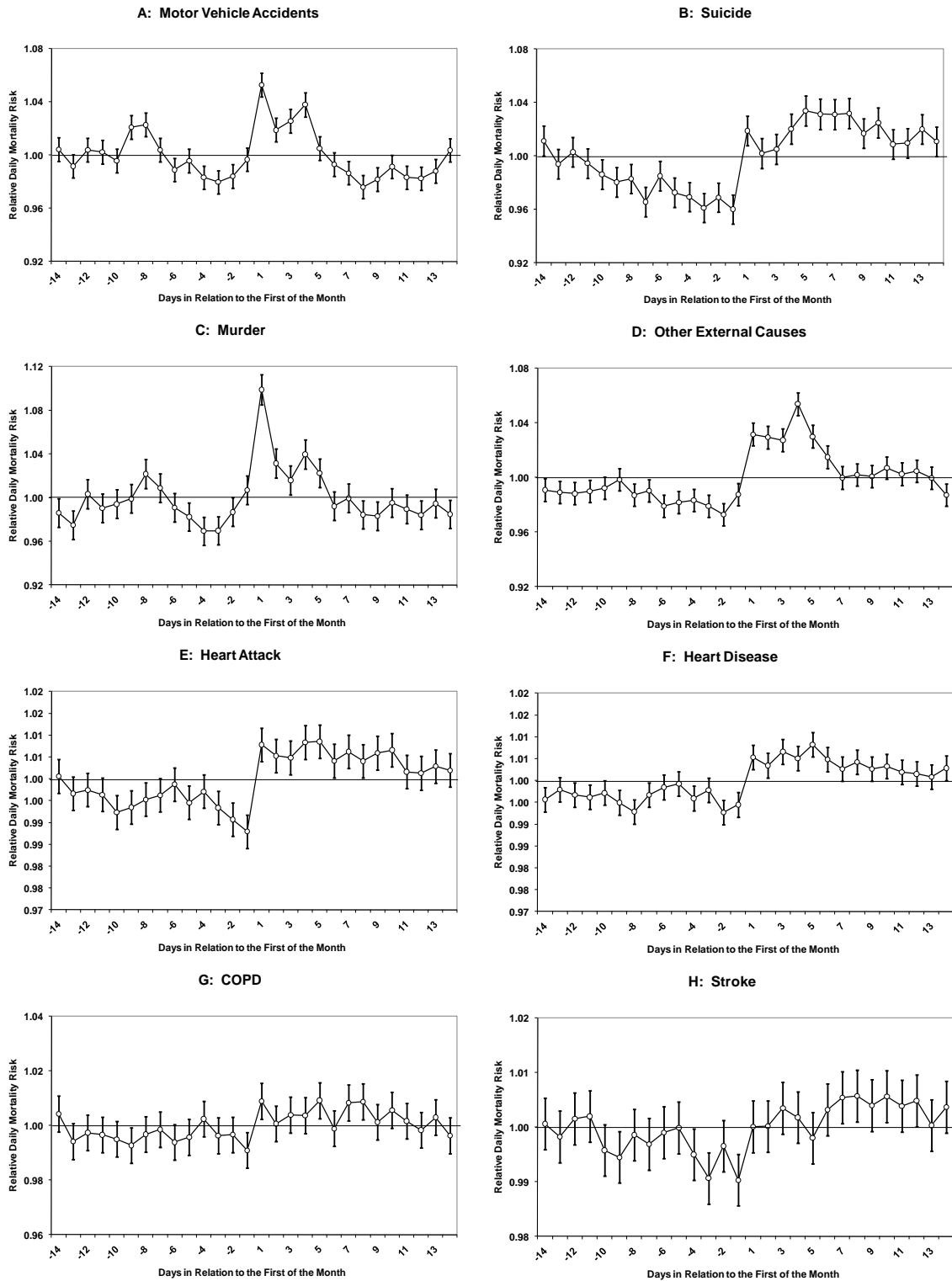
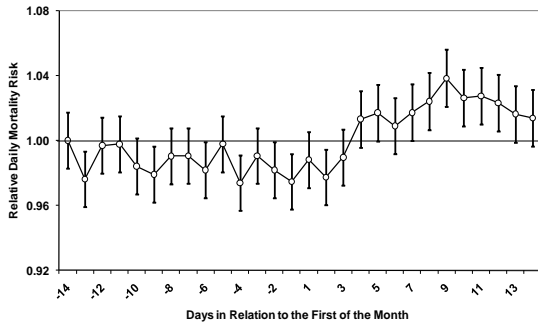


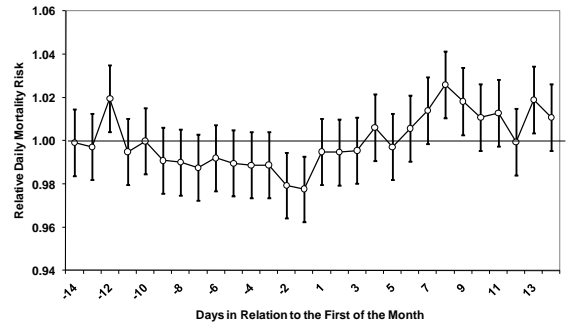
Figure 2.3: Relative Daily Mortality Rates (95% Confidence Intervals), By Specific Causes, 1973-2005 MCOB



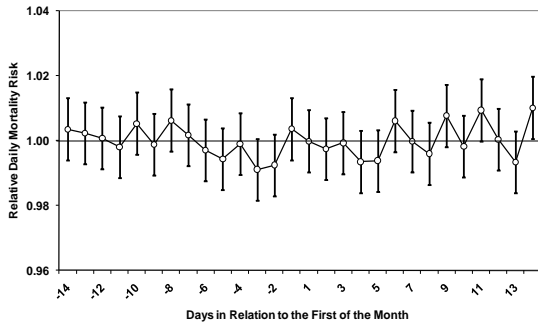
**I: Alcohol Cirrhosis**



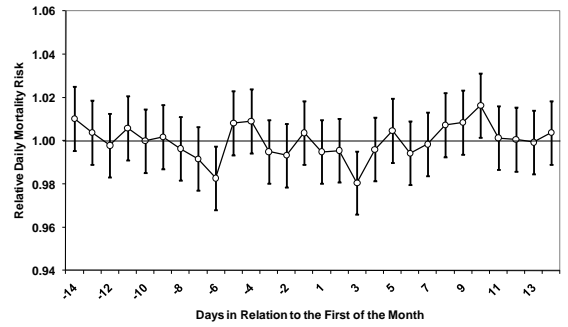
**J: Non-Alcohol Cirrhosis**



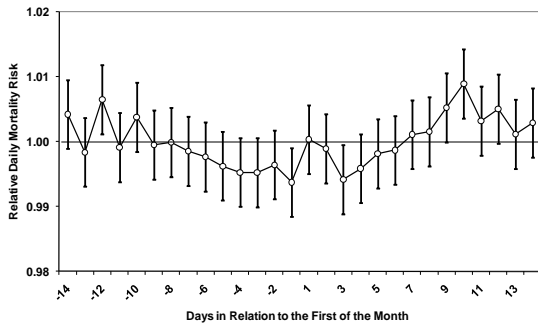
**K: Breast Cancer**



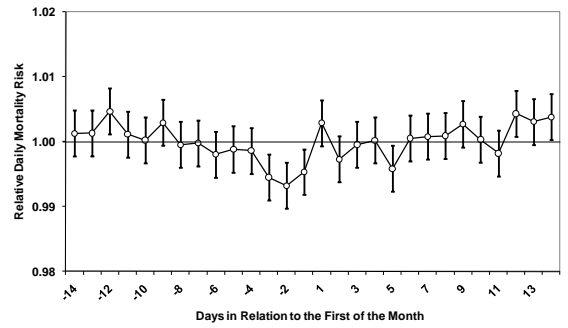
**L: Leukemia**



**M: Lung Cancer**



**N: Other Cancers**



**O: Other causes**

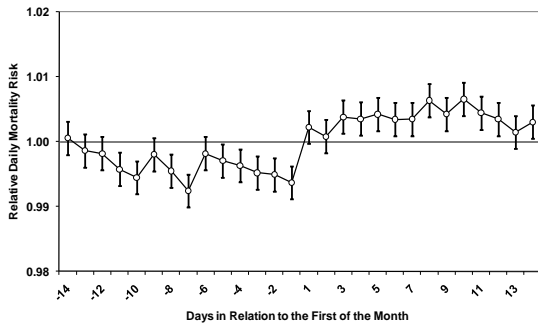


Figure 2.4: Scatter Plot, Mortality and the Business Cycle versus the Size of the Within-Month Mortality Cycle, By Cause of Death

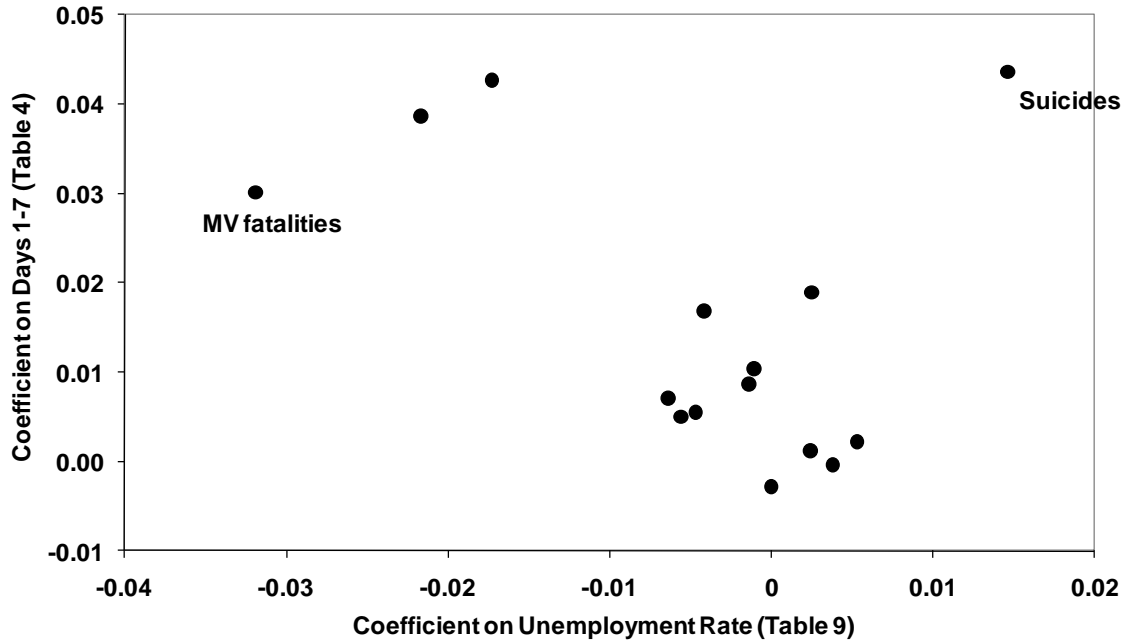


Table 2.1: OLS Estimates of ln(Daily Mortality Counts) Model, MCODE Data 1973-2005

Coefficient (Standard Error) on the <i>Day(j)</i> variable							
<i>Day(-14)</i>	0.0079 (0.0020)	<i>Day(-7)</i>	0.0069 (0.0016)	<i>Day(1)</i>	0.0107 (0.0012)	<i>Day(8)</i>	0.0120 (0.0016)
<i>Day(-13)</i>	0.0057 (0.0019)	<i>Day(-6)</i>	0.0061 (0.0015)	<i>Day(2)</i>	0.0096 (0.0014)	<i>Day(9)</i>	0.0116 (0.0016)
<i>Day(-12)</i>	0.0081 (0.0019)	<i>Day(-5)</i>	0.0053 (0.0015)	<i>Day(3)</i>	0.0127 (0.0016)	<i>Day(10)</i>	0.0129 (0.0017)
<i>Day(-11)</i>	0.0060 (0.0017)	<i>Day(-4)</i>	0.0040 (0.0014)	<i>Day(4)</i>	0.0143 (0.0015)	<i>Day(11)</i>	0.0107 (0.0020)
<i>Day(-10)</i>	0.0079 (0.0017)	<i>Day(-3)</i>	0.0015 (0.0013)	<i>Day(5)</i>	0.0132 (0.0015)	<i>Day(12)</i>	0.0103 (0.0017)
<i>Day(-9)</i>	0.0073 (0.0016)	<i>Day(-2)</i>	0.0005 (0.0011)	<i>Day(6)</i>	0.0116 (0.0016)	<i>Day(13)</i>	0.0097 (0.0017)
<i>Day(-8)</i>	0.0061 (0.0015)			<i>Day(7)</i>	0.0119 (0.0016)	<i>Day(14)</i>	0.0107 (0.0017)

Notes: The  $R^2$  for this model is 0.9083. The reference period is *Day(-1)*. There are 11,088 observations (336 observations per year for 33 years) and there is an average of 5,931 deaths per day. Numbers in parentheses are standard errors that allow for arbitrary correlation in errors within each unique synthetic 28-day month. Other covariates include day of the week effects, synthetic month and year effects, plus dummies for special days of the year (New Year's Day, Christmas, etc.). A complete list of days is included in footnote 45.

Table 2.2: OLS Estimates of ln(Daily Mortality Counts) Model by Cause of Death, MCOD Data 1979-1998

Cause of death	Years	Mean daily deaths	<i>Week(-2)</i>	<i>Week(1)</i>	<i>Week(2)</i>	R <sup>2</sup>
All deaths	1973-2005	5,938	0.0035 (0.0011)	0.0086 (0.0008)	0.0077 (0.0013)	0.908
All deaths	1979-1998	5,879	0.0037 (0.0013)	0.0087 (0.0012)	0.0078 (0.0015)	0.876
Deaths with a substance abuse multiple cause	1979-1998	257	0.0108 (0.0028)	0.0295 (0.0026)	0.0141 (0.0029)	0.599
Deaths without a substance abuse multiple cause	1979-1998	5,622	0.0034 (0.0014)	0.0077 (0.0012)	0.0076 (0.0016)	0.882

Notes: The reference period is *Week(-1)*. All models have 6,720 observations. Numbers in parentheses are standard errors that allow for arbitrary correlation in errors within each unique synthetic 28-day month. Other covariates include synthetic month and year effects plus dummies for special days of the year (New Year's Day, Christmas, etc.). A complete list of days is included in footnote 45.

Table 2.3: OLS Estimates of ln(Daily Mortality Counts) Model by Demographic Subgroups, MCOB Data 1973-2005

Demographic subgroup	Mean daily deaths	<i>Week(-2)</i> [Day -14 to -7]	<i>Week(1)</i> [Day 1 to 7]	<i>Week(2)</i> [Day 8 to 14]	R <sup>2</sup>
All deaths	5,938	0.0035 (0.0011)	0.0086 (0.0008)	0.0077 (0.0013)	0.9083
Male	3,073	0.0048 (0.0009)	0.0114 (0.0009)	0.0091 (0.0010)	0.8217
Female	2,868	0.0030 (0.0010)	0.0083 (0.0010)	0.0069 (0.0010)	0.9340
White	5,137	0.0031 (0.0010)	0.0064 (0.0010)	0.0060 (0.0010)	0.8954
Black	706	0.0062 (0.0014)	0.0235 (0.0015)	0.0176 (0.0015)	0.8433
Other race	85	0.0025 (0.0037)	0.0172 (0.0037)	0.0150 (0.0037)	0.9245
Under 18 years	170	0.0048 (0.0027)	0.0077 (0.0024)	0.0028 (0.0028)	0.8597
18 to 39 years	310	0.0097 (0.0021)	0.0204 (0.0021)	0.0108 (0.0021)	0.8003
40 to 64 years	1,234	0.0062 (0.0010)	0.0161 (0.0010)	0.0141 (0.0010)	0.7862
Over 65 years	4,185	0.0028 (0.0013)	0.0056 (0.0011)	0.0057 (0.0015)	0.9319
Single, 1979-2005	753	0.0043 (0.0015)	0.0150 (0.0015)	0.0087 (0.0015)	0.6748
Married, 1979-2005	2,540	0.0041 (0.0010)	0.0063 (0.0010)	0.0067 (0.0010)	0.7555
Widowed, 1979-2005	2,214	0.0012 (0.0014)	0.0063 (0.0014)	0.0059 (0.0014)	0.9055
Divorced, 1979-2005	540	0.0069 (0.0017)	0.0214 (0.0017)	0.0173 (0.0017)	0.9672
Metropolitan county	4,311	0.0034 (0.0010)	0.0085 (0.0010)	0.0073 (0.0010)	0.9508
Non-metropolitan county	1,609	0.0037 (0.0012)	0.0088 (0.0012)	0.0083 (0.0012)	0.8402

Notes: The reference period is *Week(-1)*. All have 11,088 observations, except for the groups defined by marital status. This information was not included in MCOB data before 1979; these models have 9,408 observations. Numbers in parentheses are standard errors that allow for arbitrary correlation in errors within each unique synthetic 28-day month. Other covariates include synthetic month and year effects plus dummies for special days of the year (New Year's Day, Christmas, etc.). A complete list of days is included in footnote 45.



Table 2.4: OLS Estimates of Ln(Daily Mortality Counts) Model, MCODE Data 1973-2005

Cause of death	Mean daily deaths	Percent substance abuse	<i>Week(-2)</i>	<i>Week(1)</i>	<i>Week(2)</i>	R <sup>2</sup>
All deaths	5,938	4.37%	0.0035 (0.0011)	0.0086 (0.0008)	0.0077 (0.0013)	0.908
By Cause of Death						
Motor vehicle	127.6	43.02%	0.0152 (0.0037)	0.0301 (0.0023)	0.0106 (0.0039)	0.753
Suicides	81.1	14.44%	0.0205 (0.0035)	0.0436 (0.0038)	0.0397 (0.0037)	0.381
Murders	58.0	79.80%	0.0105 (0.0046)	0.0387 (0.0047)	0.0107 (0.0049)	0.591
Other external causes	147.0	22.26%	0.0125 (0.0035)	0.0427 (0.0036)	0.0238 (0.0041)	0.655
Heart attack	678.0	0.19%	0.0031 (0.0016)	0.0104 (0.0016)	0.0067 (0.0018)	0.956
Heart disease	1268.6	0.52%	0.0013 (0.0016)	0.0087 (0.0014)	0.0060 (0.0017)	0.866
COPD	231.8	0.44%	0.0020 (0.0028)	0.0055 (0.0026)	0.0033 (0.0032)	0.937
Stroke	445.0	0.37%	0.0039 (0.0017)	0.0050 (0.0017)	0.0062 (0.0020)	0.832
Cirrhosis, alcohol related	33.3	100%	0.0076 (0.0051)	0.0189 (0.0052)	0.0387 (0.0052)	0.128
Cirrhosis, non- alcohol related	42.3	0.42%	0.0135 (0.0048)	0.0168 (0.0049)	0.0269 (0.0046)	0.418
Breast cancer	109.4	0.06%	0.0034 (0.0028)	-0.0004 (0.0030)	0.0019 (0.0028)	0.521
Leukemia	50.3	0.14%	0.0032 (0.0045)	-0.0028 (0.0043)	-0.0061 (0.0042)	0.446
Lung cancer	353.9	0.12%	0.0036 (0.0019)	0.0022 (0.0018)	0.0075 (0.0018)	0.938
Other cancers	794.5	0.19%	0.0033 (0.0012)	0.0012 (0.0013)	0.0042 (0.0012)	0.913
Other conditions	1517.5	4.49%	0.0025 (0.0016)	0.0071 (0.0014)	0.0078 (0.0019)	0.953

Notes: The reference period is *Week(-1)*. All models have 11,088 observations. Numbers in parentheses are standard errors that allow for arbitrary correlation errors within each unique synthetic 28-day month. Other covariates include synthetic month and year effects plus dummies for special days of the year (New Year's Day, Christmas, etc.). A complete list of days is included in footnote 45. The percentage of substance abuse deaths is calculated using deaths between 1979 and 1998.

Table 2.5: OLS Estimates of the Within-Month Purchase Cycle, Various Sources

Outcome	Time Period	Obs.	Mean daily counts	<i>Week(-2)</i>	<i>Week(1)</i>	<i>Week(2)</i>	R <sup>2</sup>
Ticket sales, MD pick 3 and pick 4	1/1/2003-12/31/2006	1,344	0.81 million	0.0065 (0.0055)	0.0705 (0.0047)	0.0319 (0.0041)	0.924
Ticket sales, OH daily number + pick 4	6/20/2005-6/16/2007	573	1.76 million	0.0121 (0.0071)	0.0875 (0.0061)	0.0388 (0.0061)	0.840
Visits to malls	1/1/2000-12/22/2007	2,657	25.4 million	0.0375 (0.0087)	0.0207 (0.0079)	0.0314 (0.0079)	0.895
Visits to retail establishments	1/4/2004-12/22/2007	1,328	94.1million	0.0549 (0.0175)	0.0341 (0.0140)	0.0198 (0.0145)	0.851
Visits to apparel retailers	1/4/2004-12/22/2007	1,325	60.4 million	0.0578 (0.0175)	0.0328 (0.0148)	0.0225 (0.0152)	0.850
Ticket sales top 10 grossing movies	1/1/1998-6/7/2007	3,171	19.3 million	-0.0100 (0.0191)	0.0558 (0.0192)	-0.0057 (0.0237)	0.928
Attendance at baseball games	1973-1998, 2000-2004	54,939	24,238	0.0036 (0.0049)	0.0013 (0.0052)	0.0337 (0.0059)	0.872
DC Metro ridership	1/1/1997-9/19/2007	3,573	494,011	0.0015 (0.0070)	0.0035 (0.0062)	0.0078 (0.0056)	0.945

Notes: Numbers in parentheses are standard errors that allow for arbitrary correlation in errors within each unique synthetic 28-day month. All dependent variables are natural logs. Other covariates include synthetic month and year effects plus dummies for special days of the year (New Year's Day, Christmas, etc.). A complete list of days is included in footnote 45. Please see the text for any other characteristics of specific models.

Table 2.6: OLS Estimates of Daily Consumption Equations, 1996-2004 Consumer Expenditure Survey Diary Data File

	<i>Week</i>				<i>Week</i>				<i>Week</i>			
	(-2)	(1)	(2)	Mean (\$)	(-2)	(1)	(2)	Mean (\$)	(-2)	(1)	(2)	Mean (\$)
	All families (N=715,213)				Family income <\$30,000 (N=338,890)				Family income ≥\$30,000 (N=182,263)			
Food	-0.059 (0.107)	0.272 (0.108)	0.183 (0.119)	15.38	0.020 (0.130)	0.561 (0.135)	0.172 (0.145)	12.65	-0.572 (0.263)	-0.508 (0.255)	0.174 (0.286)	22.46
Non-food	0.017 (0.134)	0.159 (0.136)	0.213 (0.147)	12.58	0.036 (0.161)	0.229 (0.159)	0.128 (0.172)	10.00	-0.493 (0.348)	0.032 (0.360)	0.100 (0.383)	20.01
Total	-0.062 (0.193)	0.421 (0.197)	0.383 (0.220)	27.86	0.023 (0.238)	0.780 (0.237)	0.271 (0.261)	22.61	-1.086 (0.492)	-0.480 (0.494)	-0.031 (0.552)	42.30
	Head has < high school education (N=109,069)				Head completed high school but not college (N=349,915)				Head completed college (N=256,229)			
Food	-0.119 (0.253)	0.975 (0.253)	0.268 (0.278)	12.37	0.131 (0.145)	0.470 (0.148)	0.274 (0.161)	14.47	-0.273 (0.196)	-0.278 (0.197)	0.025 (0.218)	17.91
Non-food	0.040 (0.278)	0.018 (0.262)	0.018 (0.297)	8.39	0.003 (0.177)	0.252 (0.182)	0.231 (0.196)	11.76	0.009 (0.259)	0.095 (0.262)	0.237 (0.281)	15.59
Total	-0.119 (0.446)	0.957 (0.419)	0.237 (0.482)	20.67	0.107 (0.260)	0.725 (0.266)	0.487 (0.294)	26.16	-0.266 (0.371)	-0.202 (0.370)	0.276 (0.414)	33.26
	Household has government income assistance other than Social Security (N=34,372)				Household has Social Security but no other government income assistance (N=130,239)				Household has no government income assistance (N=550,602)			
Food	-0.227 (0.454)	2.868 (0.497)	1.173 (0.518)	13.49	0.206 (0.208)	0.732 (0.219)	0.259 (0.237)	13.14	-0.102 (0.126)	0.005 (0.126)	0.110 (0.140)	16.03
Non-food	-0.082 (0.528)	0.600 (0.539)	-0.564 (0.562)	9.29	-0.055 (0.247)	0.539 (0.251)	0.330 (0.278)	9.44	0.048 (0.160)	0.047 (0.162)	0.244 (0.174)	13.54
Total	-0.326 (0.819)	3.479 (0.850)	0.570 (0.910)	22.75	0.160 (0.364)	1.228 (0.377)	0.601 (0.424)	22.54	-0.083 (0.233)	0.037 (0.233)	0.338 (0.260)	29.45

Notes: The reference period is *Week(-1)*. Standard errors are in parenthesis and allow for within-person correlation in errors. Covariates include a complete set of dummy variables for age, sex, race and education of reference person; region; urban area; family income; weekday; month; year; and special days during the year, which are listed in footnote 45. Numbers are in real December 2008 dollars.

Table 2.7: OLS Estimates of Ln(Daily Mortality Counts) Model,  
MCOD Data, 1989-2005

Group	Mean daily deaths	<i>Week(-2)</i>	<i>Week(1)</i>	<i>Week(2)</i>	R <sup>2</sup>
All deaths	6,360	0.0015 (0.0015)	0.0091 (0.0015)	0.0074 (0.0015)	0.934
By level of education					
< High school	1,916	0.0021 (0.0018)	0.0102 (0.0018)	0.0093 (0.0018)	0.798
High school	2,908	0.0008 (0.0015)	0.0093 (0.0019)	0.0072 (0.0015)	0.961
College degree	664	0.0031 (0.0020)	0.0045 (0.0020)	0.0023 (0.0021)	0.942

Notes: The reference period is *Week(-1)*. All models have 5,712 observations. Numbers in parenthesis are standard errors that allow for arbitrary correlation in errors within each unique synthetic 28-day month. Other covariates include a complete set of day of the week, monthly and annual dummy variables, plus a complete set of dummies for special days specified in footnote 45.

Table 2.8: Estimates of Ln(Weekly Mortality Counts) Model, 30-Week Period in the Summer and Fall of 2001, MCOD Data

Sample	Unmarried			
	Ages 25-64 (1)	Males, 25-64 (2)	Ages 65+ (3)	Ages 25-64 (4)
<i>Rebate</i>	0.0269 (0.0097)	0.0469 (0.0197)	-0.0009 (0.0056)	
<i>Rebate x LastWeekInMonth</i>				0.0515 (0.0183)
<i>Rebate x NotLastWeekInMonth</i>				0.0163 (0.0119)
Percent paying Federal Taxes	51.5%	75.2%	25.2%	51.5%
p-value: Group effects=0	0.813	0.334	0.127	0.851
p-value: rows (2)=(3)				0.113
R <sup>2</sup>	0.715	0.340	0.8411	0.718
Mean deaths per obs.	1,014	304	3,285	1,014

Notes: Standard errors are in parentheses. Other covariates in the model include week fixed effects and Social Security number group fixed effects. The percent in sample that paid federal taxes in 2000 is estimated from the IPUMS-CPS for March 2001.

Table 2.9: OLS Estimates of State-Level Ln(Cause-Specific Death Rate) Model, 50 States and the District of Columbia, 1976-2004

Cause of death	Deaths per 100,000 people	Coefficient (Standard error) on state-level unemployment	R2
All deaths	869.1	-0.0039 (0.0013)	0.968
		By Causes of Death	
Motor vehicle accidents	21.3	-0.0319 (0.0043)	0.930
Suicides	12.9	0.0146 (0.0059)	0.886
Murders	7.9	-0.0217 (0.0080)	0.907
Other external causes	23.9	-0.0175 (0.0049)	0.803
Heart attacks	102.9	-0.0113 (0.0052)	0.963
Heart disease	177.3	-0.0014 (0.0026)	0.919
COPD	33.8	-0.0046 (0.0024)	0.963
Stroke	66.7	-0.0056 (0.0032)	0.948
Cirrhosis, alcohol related	4.9	0.0026 (0.0092)	0.826
Cirrhosis, non-alcohol related	5.9	-0.0042 (0.0079)	0.819
Breast cancer	15.6	0.0039 (0.0018)	0.910
Leukemia	7.3	-0.0000 (0.0018)	0.845
Lung cancer	50.3	0.0054 (0.0019)	0.959
Other cancers	115.4	0.0024 (0.0012)	0.968
All other causes	223.0	-0.0064 (0.0020)	0.941

Notes: All models have data from 50 states and the District of Columbia over the 29 year period 1976-2004. The dependent variable is the log death rate (deaths per 100,000 people). All models control for state and year effects, plus the fraction black, fraction under five years of age, and the fraction over 64 years of age. Observations are weighted by population. The standard errors are calculated allowing for arbitrary correlation in errors within a state.

## Chapter 3 – The Short-term Mortality Consequences of Income

Receipt (with William N. Evans). Published in *The Journal of Public Economics*, 95:11-12 (December 2011), 1410–24.

### 3.1 Introduction

A large literature spanning many disciplines has established that individuals from higher income groups tend to have lower mortality and morbidity rates, and better health habits (Kitiwaga and Hauser, 1973; Backlund et al., 1999). Although there is some question as to whether these observed correlations represent a causal relationship (Smith, 1999; Deaton, 2003), the evidence is at least suggestive that higher income is protective of health.

In contrast to this work, there are some persistent patterns in mortality data that run counter to the standard income/health gradient. Two examples are the within-month mortality cycle and the pro-cyclic nature of mortality. Mortality steadily declines as the end of the calendar month approaches, then increases by almost one percent on the first day of the month, and remains above the daily average in the first few days of the month (Phillips et al., 1999). A large fraction of the population receives cash infusions at the beginning of the month, either from transfer programs or employment, and there is evidence that these payments increase economic activity and raise mortality rates. Similarly, mortality tends to move negatively with the business cycle, increasing during booms and declining during recessions (Ruhm, 2000). Interestingly, as shown in Chapter 2, the death categories that have the greatest peak-to-trough within the month are the

same categories that are the most responsive to changes in the business cycle.

Both the within-month mortality cycle and the pro-cyclic nature of mortality indicate the possibility of a short-term increase in mortality following income receipt. Such a relationship has been investigated among recipients of transfer payments, whose morbidity and mortality increases following income payments as a result of elevated substance abuse (e.g., Dobkin and Puller, 2007).<sup>70</sup> The within-month mortality cycle and the pro-cyclicality of mortality is present for many demographic groups and causes of death, however, which suggest that this phenomenon may be more general than previously considered.

In this paper, we use various versions of the Multiple Cause of Death (MCO) data, a census of all deaths in the United States, to examine the short-run mortality consequences of income receipt. Taking our cue from research that tests predictions about the life-cycle/permanent income hypothesis (LC/PI) using known dates of income receipt, we examine three cases of income receipt from that literature, as well as two new tests.<sup>71</sup> We examine the mortality consequences of (1) the receipt of Social Security payments on the 3<sup>rd</sup> of each month, (2) changes in the Social Security payment schedule to one based on beneficiaries' dates of birth, (3) the receipt of military wages on the 1<sup>st</sup> and 15<sup>th</sup> day of each month, (4) the 2001 federal tax rebates, and (5) the annual Alaska Permanent Fund dividend payments.

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<sup>70</sup> Papers by Verhuel et al. (1997), Rosenheck et al. 2000, Maynard and Cox (2000), Halpern and Mechem (2001), Riddell and Riddell (2006), and Li et al. (2007) have also found such a relationship.

<sup>71</sup> The LC/PI hypothesis is the standard model for inter-temporal choice in modern macroeconomics. A key implication of the model is that predictable and certain changes in income should have no effect on consumption once they occur. Over the past 15 years, authors have used high-frequency survey data on consumption and exact dates of income receipt to test this prediction. Three of our tests have been used in this way: Stephens (2003) examined the receipt of Social Security checks in the pre-1997 period; Johnson et al. (2006) examined the 2001 tax rebates; and Hsieh (2003) considered consumption after the receipt of Alaska Permanent Fund dividend payments.



In all cases, we find that mortality increases after the receipt of income. Seniors who enrolled in Social Security prior to May 1997 typically received their Social Security checks on the 3<sup>rd</sup> of the month. For this group, daily mortality is a half a percentage point higher the week after checks arrive compared to the week before. For those who enrolled in Social Security after April 1997, benefits are paid on either the second, third or fourth Wednesday of the month, depending on beneficiaries' birth dates. Among this group, daily mortality is one percent higher the week after checks arrive compared to the previous week. In counties with a large military presence, daily mortality among 17-29 year olds increases by around 10 percent the week after mid-month paychecks arrive, while over the same period there is little change in mortality in counties with a small military presence. During the week the 2001 tax rebate checks arrived, mortality among 25-64 year olds increased by 2.5 percent. During the week that direct deposits of Permanent Fund dividends are made, mortality among urban Alaskans increases by 13 percent.

Previous work suggests consumers tend to reduce spending before income receipt and increase purchases immediately afterwards. Stephens (2003) found that seniors increase their consumption of time-sensitive purchases, like perishable food and eating at restaurants, after the receipt of Social Security checks. Stephens (2006) found a similar increase in consumption after the receipt of paychecks in the United Kingdom. This bunching effect is particularly pronounced for those on federal income transfer programs and those with lower incomes. Among Food Stamp recipients, Shapiro (2005) found a drop in daily caloric consumption of 10-15 percent from when food stamps are paid to just before they are next due. Likewise, Mastrobuoni and Weinberg (2009) found food

consumption declined between Social Security payments for seniors with a high fraction of income coming from Social Security.

Results from existing medical literature suggest that short-term health risks may be heightened by increases in consumption or activity. While the link is most obvious in cases like traffic fatalities – since increased travel increases the likelihood of an accident – other causes of death also have well-documented links to consumption and economic activity. For example, many triggers for heart attacks are activity-related.<sup>72</sup> If income payments increase economic activity, one may expect a higher incidence of heart attacks to occur after the receipt of income. This is consistent with the cause-of-death patterns we find in the Social Security analysis. We also find larger mortality responses to income payments among younger groups, which may reflect their having more variable levels of consumption and activity (and a higher fraction of deaths resulting from external cause injuries and acute health problems).

Our work broadens the literature on the short-term relationship between income and mortality that has been largely limited to a single group (those receiving transfer payments) and a narrow group of causes of death (substance abuse).<sup>73</sup> It also provides a possible explanation for the patterns in mortality within the month and across the business cycle, and may explain why it is difficult to estimate the long-term relationship

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<sup>72</sup> The activities that increase the short-term risk of a heart attack include exercise (Mittleman et al., 1993; Albert et al., 2000), sexual activity (Moller et al., 2001), eating a heavy meal (Lipovetsky et al., 2004), the busy Christmas holiday season (Phillips et al., 2004), returning to work on Mondays (Willich et al., 1994; Witte et al., 2005), and shoveling snow (Heppell et al., 1991; Franklin et al., 1996).

<sup>73</sup> Dobkin and Puller (2007), using administrative records from California, find elevated drug-related hospital admissions and within-hospital mortality in the first few days of the month for recipients on federal disability insurance programs paid on the first of the month. They do not find such a similar pattern for people not enrolled in transfer programs. It is likely that we find a broader income-mortality relationship because we exploit exact dates for the arrival of non-transfer income payments, and our sample includes non-hospital mortality. For all age groups, a minority of deaths occur in hospital. Data from the 1986 MCOD indicate that the fractions of deaths occurring in hospitals by age group are: 24 percent (ages 19-39), 37 percent (ages 40-54), 42 percent (ages 55-64), 43 percent (ages 65-74) and 37 percent (ages 75 and over).

between income and health.

The welfare and policy implications of these short-term increases in mortality are uncertain. They depend on how much of the increase in deaths immediately following payments is mortality displacement, and whether alternative disbursement schemes would lessen the change in mortality. On the first issue, increases in aggregate mortality in the first week following the payment of 2001 tax rebates and the Alaska Permanent Fund dividends are offset by declines in mortality in subsequent weeks.<sup>74</sup> In some of the subgroups, however, an initial increase in mortality is not offset by subsequent declines. Age and cause of death are probably important for understanding this issue. We suspect external cause deaths and deaths among younger people are unlikely to be displacement, but our estimates are not precise enough to make any definitive claims on this point.

The second issue depends on how the size of the mortality effect varies with payment size and frequency. It is not clear from our results that greater pay frequency would decrease the size of the mortality response, as evidenced by large mortality effects from the second military wage payment each month. We do not have enough variation in payment size within particular groups to know whether this variable affects short-term mortality.

The results in this paper complement our work on the within-month mortality cycle in Chapter 2. In that paper, the within-month mortality cycle is documented for many causes of death, including external causes, heart disease, heart attack, and stroke, but not cancer. The within-month cycle is also evident for both sexes and for all age groups, races, marital status groups, and education groups. Similar within-month cycles are shown to present in a number of different activities and purchases, including going to

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<sup>74</sup> Recurring payments like the Social Security and military wage payments do not shed light on this issue.

the mall, visiting retail establishments, purchasing lottery tickets, going to the movies, and the amounts spent on retail purchases. Suggestive evidence that the rises in mortality and activity are linked to changing liquidity over the month comes from the peak-to-trough in mortality and consumption being largest for people expected to have the greatest liquidity issues, such as those with low levels of education and income, and those on federal transfer programs. In this current paper, we try to establish a definitive causal link between income payments and mortality in the short-run, which is not done in the other paper.

In Section 3.2, we demonstrate that mortality is higher immediately after the receipt of Social Security checks and military paydays. To examine whether mortality also increases following less regular income payments, in Section 3.3 we consider the mortality effects of the one-time receipt of 2001 tax stimulus checks and the annual receipt of Alaska Permanent Fund dividends. The populations in these examples broaden the phenomenon beyond the elderly and military personnel. In both cases, there is a short-term increase in mortality that is partially offset by a subsequent decrease in deaths, suggesting that some of the effect reflects short-term mortality displacement. In Section 3.4, we discuss the implications of our work.

## 3.2 The Short-Term Mortality Consequences of Regular Income Payments

### 3.2.1 Monthly Social Security Payments

Prior to May 1997, all Social Security recipients received checks on the 3<sup>rd</sup> of each month, or the previous work day when the 3<sup>rd</sup> fell on a weekend or on Labor Day. Stephens (2003) used the structure of these payments and data from the Consumer

Expenditure Survey to demonstrate that Social Security recipients spend more on a variety of goods immediately after their check arrived, including on food at home and “instantaneous consumption,” such as food away from home and admissions to entertainment and sporting events.

Given the connection between these types of spending and mortality risks, it is possible that the mortality of Social Security recipients is higher immediately after they are paid than beforehand. We initially use the “3<sup>rd</sup> of the month” schedule and mortality data from prior to 1997 to investigate this possibility.

The mortality data we use are various versions of the National Center for Health Statistics’ (NCHS) Multiple Cause of Death (MCOB) data file.<sup>75</sup> The MCOB contains a unique record for each death in the United States. Records have information about a decedent’s age, gender, race, place of residence, place of death, and cause of death. Exact date of death is reported on public-use files from 1973 to 1988, but is removed from later public-use files. We obtained permission from the NCHS to use restricted-use MCOB files containing exact dates of death from 1989 to 2006 at their Research Data Center.

We used the information on decedents’ age and exact date of death in the 1973 to 1996 MCOB files to construct daily counts of decedents aged 65 and over, a group consisting almost entirely of Social Security recipients.<sup>76</sup> The Social Security Administration reports that benefits were paid to 32.7 million adults aged 65 and older in

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<sup>75</sup> Information about the MCOB is at [http://www.cdc.gov/nchs/products/elec\\_prods/subject/mortmcd.htm](http://www.cdc.gov/nchs/products/elec_prods/subject/mortmcd.htm).

<sup>76</sup> Workers can claim reduced retirement benefits at 62 and receive full benefits at between 65 and 66 years of age, depending on their cohort. Song and Manchester (2007) report that from 1998 to 2005, half of Social Security beneficiaries enrolled at age 62 and almost all enrolled by age 65. Therefore, we restrict our attention to decedents aged 65 years or more.

2000,<sup>77</sup> which is 93.5 percent of the population in this age group in the 2000 Census.

The basic relationship between mortality and social security payments can be seen in the residuals plotted in Figure 3.1, which come from a regression of the natural log of daily mortality counts on weekday, month and year effects, plus dummies for special days (e.g., Christmas, Thanksgiving, etc.). The solid line is a plot of the averaged residuals over the 14 days prior and the 14 days after checks arrive. From five days before checks arrive, the average daily residuals steadily decrease and mortality is 0.8 percent below the daily average the day before checks arrive. Mortality increases sharply on the day checks arrive, and then the average residuals are generally positive in the days following paycheck receipt. This pattern is very similar to the pattern of results in Figures 1a-1d in Stephens (2003).

Chapter 2 highlights how the concentration of economic activity and other income payments at the start of the month affect mortality. It is important to take that into account here, as Social Security is only one source of income for seniors.<sup>78</sup> To get some idea of whether there is a separate within-month cycle in mortality among those 65 and older, the residuals from the regression described in the previous paragraph are also arranged in relation to the 1<sup>st</sup> of the calendar month and plotted as the dashed line in Figure 3.1. There is a reduction in mortality leading into the 1<sup>st</sup> of the month, and then an increase in the first couple of days of the calendar month.

To further analyze the relationship between Social Security payments and daily mortality, we follow Stephens (2003) and construct ‘synthetic’ months that begin 14 days

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<sup>77</sup> Social Security Administration Office of Research, Evaluation and Statistics, *Annual Statistical Supplement of the Social Security Bulletin, 2001*, Washington, DC: SSA, December 2001.

<sup>78</sup> For families with someone age 65 years and over, 32 percent of income comes from Social Security. Authors’ calculations based on data from the 1974-1997 March Current Population Survey.

prior to the day of Social Security payment and last until 14 days before the next payment.<sup>79</sup> Synthetic months are anywhere from 28 to 34 days in length, as they depend on the day when the checks are distributed and the number of days in the month. We divide each month into five periods: *Payweek(-2)* is the seven days from 14 days before payday to the eighth day before payday; *Payweek(-1)* is the seven days prior to payday; *Payweek(1)* is the seven days after payday (including payday); *Payweek(2)* is the period from eight to 14 days after payday; and *Payweek(3)* is the extraneous days before the next synthetic month starts.

We control for the within-month mortality cycle by creating weekly dummy variables in reference to the 1<sup>st</sup> of the calendar month, where *Week(-2)* equals one if the day is eight to 14 days before the start of the calendar month; *Week(-1)* equals one if the day is one to seven days before the start of the month; *Week(1)* and *Week(2)* equal one for the 1<sup>st</sup> to 7<sup>th</sup> and 8<sup>th</sup> to 14<sup>th</sup> days in the calendar month, respectively; and *Week(5)* is all the extra days before the 14<sup>th</sup> day prior to the start of the next calendar month. As checks not paid on the 3<sup>rd</sup> are almost always paid on Fridays,<sup>80</sup> we also need to control for day-of-the-week effects.

To isolate the short-term mortality impact of receiving a Social Security check from other factors, we estimate the following econometric model. Let  $Y_{dmy}$  be counts of deaths for day  $d$  in synthetic month  $m$  and synthetic year  $y$ .<sup>81</sup> Days are organized in relation to Social Security payments, so  $d = -1$  is the day before payday,  $d = 1$  is payday,

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<sup>79</sup> For example, January 3, 1995 is a Tuesday, so the first synthetic month of the year is December 20<sup>th</sup> of the previous year through to January 19, 1995; month two is then January 20<sup>th</sup> through February 20<sup>th</sup>, and so on.

<sup>80</sup> The lone exception is that when January 3<sup>rd</sup> is a Sunday, checks are distributed on Thursday, December 31.

<sup>81</sup> Synthetic years follow a similar structure, so when both the January and December payments are made on the 3<sup>rd</sup> of the month, the year will begin on December 20<sup>th</sup> and go through to December 16<sup>th</sup> of the following year.

and so on;  $d$  ranges from -14 to 20. The econometric model is of the form:

$$(1) \quad \ln(Y_{dmy}) = \alpha + \sum_{\substack{w=-2 \\ w \neq -1}}^3 Week(w)_{dmy} \delta_w + \sum_{\substack{w=-2 \\ w \neq -1}}^3 Payweek(w)_{dmy} \beta_w + \sum_{j=1}^6 Weekday(j)_{dmy} \gamma_j \\ + \sum_{j=1}^M Special(j)_{dmy} \varphi_j + \mu_m + v_y + \varepsilon_{dmy} \quad (3.1)$$

where  $Payweek(w)$  and  $Week(w)$  are the dummy variables defined as above,  $Weekday(j)$  is one of six dummy variables for the different days of the week, and  $Special(j)$  is one of  $J$  dummy variables that capture special days throughout the year.<sup>82</sup> The variables  $\mu_m$  and  $v_y$  capture synthetic month and year effects and  $\varepsilon_{dmy}$  is an idiosyncratic error term. In this equation, the reference period for the  $Payweek$  dummies is  $PayWeek(-1)$  and the reference period for  $Week$  dummies is  $Week(-1)$ . The reference weekday is Saturday. We estimate standard errors allowing for arbitrary correlation within each unique synthetic month, e.g., we allow for correlation in errors for month 1 of 1995, month 2 of 1995, etc.

The results for equation (3.1) for decedents 65 and older from 1973 to 1996 are reported in the first column of Table 3.1. In the first four rows of the table, we report results which show that deaths are about one half of a percent higher in the seven days after check receipt compared to the preceding seven days.<sup>83</sup> Relative to the week before payment, deaths are one half a percent higher two weeks before payment ( $Payweek(-2)$ )

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<sup>82</sup> We include unique dummies for a long list of reoccurring special days, including for January 1<sup>st</sup> and 2<sup>nd</sup>, the Friday through Monday associated with the all federal holidays occurring on Mondays (Presidents' Day, Martin Luther King Jr Day since 1986, Memorial Day, Labor Day, Columbus Day), Super Bowl Sunday and the Monday afterwards, Holy Thursday through Easter Sunday, July 4<sup>th</sup>, Veteran's Day, the Monday through Sunday of Thanksgiving, a dummy for all days from the day after Thanksgiving though New Year's Eve, plus single day dummies for December 24<sup>th</sup> through December 31<sup>st</sup>.

<sup>83</sup> To provide a frame of reference, Stephens (2003) shows that the probability of any spending among all seniors is 1.6 percent higher in the first week after checks arrive compared to the previous seven days.



and two weeks after payment (*Payweek(2)*).<sup>84</sup> The results suggest a fall in mortality in the last few days before seniors are paid; the increase when they are paid is a return to ‘normal’ mortality. This suggests seniors decrease their level of activity as they run out money, rather than ‘splurge’ when they get paid. It is consistent with the consumption behavior among seniors reported in Stephens (2003), and the food intake patterns found in Mastrobuoni and Weinberg (2009).

In the next four rows, we present results for the calendar weeks in relation to the 1<sup>st</sup> of the month. There is a within-month mortality cycle, with deaths declining the week before the 1<sup>st</sup> and then rising afterwards. Daily death rates are about three-tenths of a percent higher in the first week of the month compared to the previous seven days, with a p-value for the test that the null hypothesis is zero of less than 0.05.

In columns (2) to (4) of Table 3.1, we consider results for age-based subgroups because it is documented in Chapter 2 that the within-month mortality cycle is less pronounced for older groups. Similar mortality patterns are present across the 65-74 years, 75-84 years and 85 years and over groups. The *Payweek* coefficients are generally smaller for the group aged 85 years and over than the other two groups, although the differences between coefficients are not statistically significant.

In column (5), we consider a set of decedents who should NOT be impacted by the “3<sup>rd</sup> of the month” schedule, which allows us to see whether our results are driven by some other effect at the 3<sup>rd</sup> of the month. Starting in May of 1997, the timing of monthly payments for new recipients depended on their birth dates. Those with a birth date from the 1<sup>st</sup> to the 10<sup>th</sup> are now paid on the second Wednesday of each month; those with a

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<sup>84</sup> It is difficult to interpret the *Week(3)* and *Payweek(3)* coefficients in any regressions, because the length of these dummy variables varies across months and creates strong seasonal components that are not necessarily controlled for with other covariates.

birth date from the 11<sup>th</sup> to the 20<sup>th</sup> are paid on the third Wednesday; and those with a birth date from the 21<sup>st</sup> to the 31<sup>st</sup> are paid on the fourth Wednesday. Those already receiving payments on the 3<sup>rd</sup> of the month continued to receive checks as they had before.<sup>85</sup> As a falsification test, we estimate the “3<sup>rd</sup> of the month” model on decedents who should be enrolled via the new payment schedule. The sample we construct for this test uses deaths among 65 to 69 year olds as recorded in the MCODE files for 2005 and 2006, the most recent year data is available. The only cohorts that we can be sure enrolled in Social Security after the change-over in rules are beneficiaries who turned 62 after May of 1997. As before, we restrict our attention to people over 65 because nearly all beneficiaries claim Social Security by age 65. Someone 62 years of age in 1998 is 69 years old in 2005, and therefore anyone aged 65 to 69 years in 2005 and 2006 receiving Social Security benefits would have enrolled on the new schedule.<sup>86</sup>

In column (5) of Table 3.1 we show the results for this group. The coefficient on *Payweek(1)* is statistically insignificant and negative. The lack of precision for this result is not due to small sample sizes. In column (4) we report results for the old payment system using only two years of data (1995-1996) for the same 65 to 69 age range and find a statistically significant two percent increase in daily mortality during *Payweek(1)*.

It is no surprise that the payweek and week effects are somewhat muted in this sample, given that the *Payweek* and *Week* variables overlap in similar ways each month. *Payweek(1)* most commonly covers the 3<sup>rd</sup> to the 9<sup>th</sup> of the month, and the *Week(1)*

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<sup>85</sup> <http://www.ssa.gov/pubs/2007calendar.htm>.

<sup>86</sup> The exceptions are seniors also receiving Supplemental Security Income (SSI) and former Social Security Disability Insurance (SSDI) recipients in that age range. According to the Social Security Administration’s *Annual Statistical Supplement, 2010*, 3.3 percent of recipients of Retirement and Survivors Insurance also received SSI in 2005 and 2006. Tables in the same publication suggest that there should be no more than 500,000 former SSDI beneficiaries per year who are aged 65-69 years, which is about 1.5 percent of Retirement and Survivors Insurance recipients.

variable always covers the 1<sup>st</sup> to the 7<sup>th</sup> of the month, so the *Payweek(1)* coefficient is strongly influenced by differences between the 1<sup>st</sup> and 2<sup>nd</sup> compared to the 8<sup>th</sup> and 9<sup>th</sup> of the month. We are better able to isolate the within-month effect from the payweek effect for Social Security recipients on the new schedule, a group we consider next.

We examine the payday/mortality relationship in the post-May 1997 system using data on 65 to 69 year olds in 2005 and 2006. The restricted-use MCODE data identifies the decedent's exact date of birth, which allows us to place them into three groups: those with birth dates from the 1<sup>st</sup> to the 10<sup>th</sup> of the month (paid on the second Wednesday of the month); those with birth dates from the 11<sup>th</sup> to the 20<sup>th</sup> (paid on the third Wednesday); and those with birth dates from the 21<sup>st</sup> to the 31<sup>st</sup> (paid on the fourth Wednesday). For this sample, we allow the dependent variable to vary across days, months, years and birthday groups ( $k$ ), and estimate an equation of the form:

$$(2) \quad \ln(Y_{kdm}) = \alpha + \sum_{\substack{w=-2 \\ w \neq -1}}^3 Week(w)_{kdm} \delta_w + \sum_{\substack{w=-2 \\ w \neq -1}}^3 Payweek(w)_{kdm} \beta_w + \sum_{j=1}^6 Weekday(j)_{kdm} \gamma_j + \sum_{j=1}^M Special(j)_{kdm} \varphi_j + \lambda_k + \mu_m + \nu_y + \varepsilon_{dm} \quad (3.2)$$

The variables *Week(w)*, *Special(j)*, *Weekday*,  $\mu$ ,  $\nu$ , and  $\varepsilon$  are defined as before. In this model, we add effects for the birthday-based groups ( $\lambda$ ), and *Payweek(w)* variables are now centered on the second, third, or fourth Wednesday of the month, depending on the group. Synthetic months are uniquely defined for each birth date group ( $k$ ). Because pay dates are now fixed on Wednesdays, there are either 28 or 35 days in each synthetic month. If the receipt of income alters short-term mortality, then the payday/mortality cycle should have shifted to different parts of the month for Social Security beneficiaries enrolling after May 1997.

Results from equation (3.2) for 65 to 69 year olds in 2005 and 2006 are reported in the first column of Table 3.2. There is a pronounced within-month mortality cycle, with a statistically significant 1.4 percent value on the *Week(1)* variable. There is also a large pay effect: the coefficient on *Payweek(1)* is a statistically significant 1.1 percent. We also report results for the group effects; the reference group is those born from the 21<sup>st</sup> to the 31<sup>st</sup>.<sup>87</sup> A shortcoming of this test is that not all recipients are paid based on their own birth date. A person who claims Social Security benefits under their spouse's earnings would actually receive the check based on their spouse's birth date. Consequently, there is some measurement error across the three birth date groups. People who never married should be claiming benefits under their own birth date, so in column (2) of Table 3.2 we report results for never-married seniors aged 65 to 69 in the 2005 and 2006 MCOB files. There is a much larger increase in the payday effect on mortality. The coefficient on *Payweek(1)* is now 2.75 percent, although it is a smaller group and so the t-statistic is only 1.56, meaning the results are statistically significant at a p-value of about 0.12.

The final two columns of the table contain the results of two placebo tests. First, we re-estimate the model from equation (3.2) by imposing the new payment schedule on decedents aged 65 to 69 in 1995 and 1996, who would have been on the old payment system. The *Payweek(1)* variable should be small and statistically insignificant in this case, and it is. Second, we estimate the same model for decedents aged 50 to 59 in 2005 and 2006, a group not enrolled in Social Security. As expected, we find no impact on *Payweek(1)*. In both columns (3) and (4), we document large and statistically significant

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<sup>87</sup> In a non-leap year, there are 125 birth days that would put a person into this group, while there are only 120 such days for the other two groups. This is why the dummy variables for these coefficients are negative.

within-month cycles.

The work linking mortality to income payments has to date primarily focused on the impact on deaths related to substance abuse. In this section, we estimate models for causes both related and unrelated to substance abuse. Causes of death in the MCODE files are defined using the International Classification of Disease (ICD) codes. Three different ICD versions are used during the period we consider: ICD-8 (1973-78), ICD-9 (1979-98), and ICD-10 (1999-2006). The codes used to identify substance abuse vary across versions, so for the “3<sup>rd</sup> of the month” analysis we use ICD-9 data from 1979 to 1996. The aim of this analysis is to see whether paycheck/mortality relationship can be explained solely by substance abuse, so we err on the side of defining too many deaths as substance abuse-related, rather than too few. Each death has an underlying cause as well as up to 19 other causes, and we define a substance abuse death as one in which any of the causes has an ICD-9 code associated with substance abuse. The list of causes defined as substance abuse come from Phillips et al. (1999) and studies of the economic costs of substance abuse in the United States (Harwood et al., 1998), Australia (Collins and Lapsley, 2002), and Canada (Single et al., 1999).<sup>88</sup> We classify approximately one percent of deaths among seniors in 1979 to 1996 as substance abuse deaths.

Column (1) of Table 3.3 contains estimates for equation (1) for all causes of death among seniors during the ICD-9 reporting period of 1979-1996. These results are similar to those in Table 3.1. We report results for substance abuse in column (2), and find a large coefficient (standard error) on the *Payweek(1)* variable of 0.0367 (0.0112). There is also pronounced within-month mortality cycle – the *Week(1)* coefficient is 1.90 percent, with a p-value of 0.11. In column (3) we re-estimate the model using non-substance

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<sup>88</sup> A complete list of these codes is provided in Appendix A2.

abuse deaths. These deaths represent 99 percent of all deaths from column (1), so it is no surprise that the results in columns (1) and (3) are virtually identical. The results in columns (2) and (3), together with the mean daily deaths in each category, indicate that substance abuse deaths account for around eight percent of the rise in mortality in the week after checks arrive. Even with some under-reporting of substance abuse deaths, these results suggest that the effect of income on mortality extends well beyond substance abuse.

In the final four columns of Table 3.3, we use causes of death codes in the ICD-8 and ICD-9 to create a few broad underlying cause-of-death categories. For each cause, we estimate equation (1) for decedents 65 and older for the entire 1973-1996 period.<sup>89</sup> In column (4), we present results for external causes of death (e.g., accidents, murders, suicides, motor vehicle crashes), and find both a large pay week effect (coefficient and standard error on *Payweek(1)* is 0.0410 (0.0057)) and a large within-month effect (coefficient and standard error on *Week(1)* is 0.0257 (0.0059)). In column (5), we present results for heart attacks, a cause often associated with a short time from onset to death. The *Payweek* coefficients are slightly larger for heart attacks than for all deaths (as reported in column (1) of Table 3.1). In column (6), we report results for cancer – a cause of death we can view as something of a placebo test, because we suspect cancer deaths are less affected by activity than most other causes. We do not find either a pay week or within-month cycle for cancer, as the results for *Payweek(1)* and *Week(1)*

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<sup>89</sup> The NCHS recoded ICD-8 and ICD-9 deaths into 34 underlying causes. Our external causes group consists of deaths with codes 33 to 36. Heart attacks (acute myocardial infarctions) have an underlying cause of death code of 410 in both ICD-8 and ICD-9. The cancer category was created using a cause of death recode produced by the National Cancer Institute (available at [http://seer.cancer.gov/codrecode/1969+\\_d09172004/index.html](http://seer.cancer.gov/codrecode/1969+_d09172004/index.html)).

demonstrate. Finally, the results for all other causes are presented in column (7), and are similar to the aggregate patterns. Heart attacks account for 26 percent of the size of the *Payweek(1)* coefficient presented in column (1) of Table 3.1. Even though the size of the *Payweek(1)* coefficient for external causes is much larger than for heart attacks, external causes explains less of the aggregate pattern (around 20 percent).

### 3.2.2 The Military Payment Schedule

Military personnel are paid on the 1<sup>st</sup> and the 15<sup>th</sup> of each month, or on the previous business day when these dates fall on a weekend or a public holiday.<sup>90</sup> In this section, we examine whether mortality spikes after these pay dates. Active duty military are predominantly male (currently 85 percent), young (approximately one half are under 25 years of age) and healthy (Segal and Segal, 2004). Newspaper accounts suggest that many military personnel spend more than average on and immediately after payday. The phenomenon appears to be widespread, with payday-generated spending increases reported at bars, restaurants, cinemas, malls and hairdressers near bases in Connecticut,<sup>91</sup> Hawaii,<sup>92</sup> North Carolina<sup>93</sup> and Virginia.<sup>94</sup>

In this section, we compare mortality patterns in counties with and without a high fraction of their population on active military duty. Soldiers normally reside on or near

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<sup>90</sup> We can date this policy as early as 1971, <https://www.usna.com/SSLPage.aspx?pid=6121> but no older veteran or military expert we spoke with could remember a time when wages were not paid on these two dates.

<sup>91</sup> Carbone, Gerald M. "Dive! Dive! Groton Fears Loss of Base," *Providence Journal*, Providence RI, Feb 28, 1995, p.A01.

<sup>92</sup> Song, Jaymes. "Many Businesses that Rely on Sales to the Military Struggle to Survive with Recent Mass Deployments," *Honolulu Star-Bulletin*, Honolulu HI, June 21, 2004.

<sup>93</sup> Foster, J. Kyle. "Retail Boost," *Fayetteville Observer*, Fayetteville NC, May 7, 2001, Local & State Section. Mullen, Rodger. "It Must be Payday," *Fayetteville Observer*, Fayetteville NC, Mar 11, 1990, Lifestyle Section.

<sup>94</sup> Snead Fulk, Sande. "Lifeblood of a Local Economy; Advisory Committee Studies Base's Impact," *The Richmond Times-Dispatch*, Richmond VA, May 22, 2002, p.4.

the base to which they are attached, and these bases are unevenly distributed throughout the country. Since both the size of the military and base locations were fairly uniform over the 1973 to 1988 period, and since the public-use MCOB files contain exact dates of death during this time, we focus on these years.<sup>95</sup> The size of the military changed considerably in the early 1990s following military downsizing and a number of base closings.

We identified counties with more than 15 percent of their population aged 17 to 64 who were military personnel<sup>96</sup> in the 1970, 1980 and 1990 Censuses using Census Summary File 3 data sets.<sup>97</sup> There are 21 counties that meet this criterion.<sup>98</sup> In 1990 there were roughly 326,000 people aged 17 to 64 in these “military” counties of which about one quarter were in the military. The proportion of the population affected by the military payment schedule is higher than this fraction because civilian employees on military bases are paid on the same schedule<sup>99</sup> and both they and military personnel have dependents.

We compare mortality in these counties with deaths in 2,772 “non-military” counties that have less than one percent military among adults aged 17-64 in the 1970, 1980 and 1990 Censuses. We present results for two groups: those aged 17-29 years and 17-39 years. We choose these age breakdowns because, during this period, 69 percent of

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<sup>95</sup> Various issues of the *Statistical Abstract of the United States* indicate that the active duty military was anywhere from 2.04 to 2.25 million from 1973 to 1988, dropping to 1.38 million in 2001 as a number of bases closed across the country.

<sup>96</sup> Enlistment in the military can occur at age 17 years with parental consent, and at age 18 years without.

<sup>97</sup> These data are taken from the National Historical Geographic Information System (Minnesota Population Center, 2004).

<sup>98</sup> The States (Counties) in our sample are: AL (Dale), GA (Chattahoochee, Liberty), ID (Elmore), KS (Geary, Riley), KY (Christian, Hardin), LA (Vernon), MO (Pulaski), NE (Sarpy), NC (Cumberland, Onslow), OK (Comanche, Jackson), SC (Beaufort), TN (Montgomery), TX (Bell, Coryell, VA (Norfolk City), WA (Island).

<sup>99</sup> Data from various issues of the *Statistical Abstract of the United States* indicate that during our analysis period, about one million civilians were employed annually by the military.



all active duty military were aged 17-29 years and 91 percent were aged 17-39 years.<sup>100</sup>

While the widespread nature of the within-month mortality cycle may mean military and non-military counties exhibit a similar time series in mortality counts around the 1<sup>st</sup> of the month, we expect a much greater frequency of paycheck distributions around the 15<sup>th</sup> in military counties compared to non-military counties because the predominant payment frequency outside the military is weekly or biweekly.<sup>101</sup>

In Figure 3.2, we use data from the 1973-1988 MCOB to construct relative daily mortality rates for those aged 17-29 years in military and non-military counties. We construct rates for a 28 day period that represents the seven days before and after the two military paychecks are distributed each month – the first check being near the 1<sup>st</sup> of the month and the second being near the 15<sup>th</sup> of the month. *Day(1)* is the day checks are distributed and *Day(-1)* is the day before checks arrive. The solid line in the graph represents the daily mortality risk for military counties, the dotted line is for non-military counties and the vertical lines are 95 percent confidence intervals for the daily mortality risk.

The two groups show similar patterns around the first payday of the month. There is a within-month mortality cycle for both military and nonmilitary counties, with deaths declining before checks arrive and rebounding afterwards. The spike in deaths around the 1<sup>st</sup> of the month may be due to within-month mortality cycle, and also the fact that three-sevenths of all payments are distributed on a Friday and there is a spike in

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<sup>100</sup> Authors' calculations using data from the 1980 Census 5% Public Use Micro Samples (Ruggles et al., 2010).

<sup>101</sup> Data from the 1996-2004 Diary Survey record of the CEX indicate that 9.6 percent of workers report their last pay check as being paid monthly, while 5.5 percent report being paid twice-monthly. Most respondents are paid weekly (31.4 percent) or every two weeks (50.6 percent), with 2.9 percent paid some other frequency.

deaths for all demographic groups on the weekend. Both groups show increases in mortality right after the second checks arrive, which may again be due by the fact that three-sevenths of these checks are paid on Fridays. A key difference, however, is that the pattern is more pronounced for military counties. Daily mortality rates on *Day(1)* to *Day(4)* are 10 to 18 percent higher than average, a noticeable increase over non-military counties. These raw numbers suggest mortality is higher in military counties right after the second check arrives.

To formally test whether military and non-military counties exhibit different mortality patterns around the 1<sup>st</sup> and 15<sup>th</sup> of the month, we estimate a model similar to equation (1). A key difference is that, because daily mortality counts in the military counties are small and occasionally zero, we use a negative binomial model that allows for integer values and estimate it by maximum likelihood (Hausman et al., 1984). Let  $Y_{idmy}$  be daily mortality counts for group  $i$  (for military and nonmilitary counties) on day  $d$ , month  $m$  and year  $y$ . Let  $X_{idmy}$  be vector that captures the exogenous variables in equation (1). Within the negative binomial model,  $E[Y_{idmy} | X_{idmy}] = \delta \exp(X_{idmy} \beta)$ , where  $\delta$  is a parameter that captures whether the data exhibits over-dispersion.<sup>102</sup> By definition,  $\partial \ln E[Y_{idmy} | X_{idmy}] / \partial X_{idmy} = \beta$  so the parameters in this model are interpreted similarly to those in equation (1).

In constructing the dataset, the “synthetic” months are 28-day periods that begin seven days before the first payment each month and end seven days after the second

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<sup>102</sup> It can be demonstrated that the variance of counts in the negative binomial model is  $\text{Var}[Y_{idmy} | X_{idmy}] = \delta^2 [1+(1/\delta)] \exp(X_{idmy} \beta)$ , so the variance to mean ratio in this model is  $\delta + 1$ . When  $\delta=0$  the negative binomial collapses to a Poisson model which, by construction, restricts the variance to equal the mean.

payment each month.<sup>103</sup> When the 1<sup>st</sup> or the 15<sup>th</sup> of the month are on a weekend or a public holiday, wages are paid on the closest prior working day.<sup>104</sup>

The exact specification for equation  $X_{idmy}\beta$  is of the form:

$$(3) \quad X_{idmy}\beta = \beta_0 + \sum_{j=1}^6 \text{Weekday}(j)_{dmy} \gamma_j + \sum_{j=1}^M \text{Special}(j)_{dmy} \varphi_j + \text{Period1}_{dmy} \beta_p + \text{Military}_i \beta_m + (\text{Period1}_{dmy})(\text{Military}_i) \beta_{pm} + \text{Military}_i \text{Period1}_{dmy} \text{Week}(1)_{dmy} \alpha_{1m} + \text{NonMilitary}_i \text{Period1}_{dmy} \text{Week}(1)_{dmy} \alpha_{1n} + \text{Military}_i \text{Period2}_{dmy} \text{Week}(1)_{dmy} \alpha_{2m} + \text{NonMilitary}_i \text{Period2}_{dmy} \text{Week}(1)_{dmy} \alpha_{2n} + \mu_m + v_y \quad (3.3)$$

where *Weekday*, *Special*, and the synthetic month and year effects are defined as before and we capture the month and year effects through a series of dummy variables. We control for differences across groups with a dummy for counts in military areas (*Military*), across pay periods (*Period1*), and their interaction. Around each payday are two weekly periods, the week before (*Week(-1)*) and the week after checks arrive (*Week(1)*). The key covariates are interactions that measure whether military and non-military counties experience a spike in deaths the week after checks arrive compared to the week before. We examine whether the daily mortality patterns differ across military and non-military counties by testing the null hypothesis  $H_o : \alpha_{jm} = \alpha_{jn}$  for the two pay periods,  $j = 1$  and  $2$ .

The maximum likelihood results for the negative binomial model are reported in Table 3.4. In column (1), we report the results for those aged 17-29 years. The first two rows contain the coefficients on *Week(1)* for non-military and military counties for the

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<sup>103</sup> Days outside of the 28-day pay periods are dropped from the analysis. The two pay periods in each month do not overlap, except when Presidents Day falls on the 15<sup>th</sup> of February and the seven days after the previous wage payment overlaps with the seven days before this payment. The 28 days around these two payments (25<sup>th</sup> January–18<sup>th</sup> February) is removed when this happened in 1982 and 1988.

<sup>104</sup> The relevant public holidays that alter payments in this section are New Year's Day, Presidents Day, Labor Day and Martin Luther King Day (since 1986).

first pay period of the month. The next two rows contain the same set of coefficients for the second pay period. For each group of coefficients, we also report the p-value on the null hypothesis that the military and non-military coefficients are equal. Standard errors allow for arbitrary correlation across observations within the same 28-day synthetic month.

The results in Table 3.4 correspond with the visual evidence in Figure 3.2. Among 17-29 year olds, in the week after the first pay check arrived, there is a spike up in mortality of about 1.8 percent for both county types and the p-value on the test that the coefficients are equal is very high. For this model, during the first pay period, the coefficient for the non-military counties is statistically significant but the coefficient for military counties is not. In the week after the second paycheck of the month arrives, we find a statistically significant one percent increase in mortality in non-military counties and a statistically significant coefficient that is ten times larger in military counties. The p-value of 0.002 means we can reject the null that these results are the same.

In the next column, we include deaths for people aged 17-39 years. Focusing on the second paycheck of the month, we find that in military counties, mortality is a statistically significant 4.6 percent higher the week after the second paycheck arrives, an effect that is 10 times larger than the first-week effect in non-military counties. The p-value on the test of equality of the coefficients means we can reject the null.

In the third and fourth columns of the table, we re-estimate the basic models by restricting the definition of military countries to those with 20 percent or greater adults aged 17-64 years on active duty military. The number of counties falls to 15 and average deaths in the treatment group fall considerably as well, meaning we should witness a

decline in the precision of the military coefficients. However, the fraction of treated people in a county should increase meaning the coefficient on *Military\*Period2\*Week(1)* should rise. Both of these conjectures are borne out in the data. Among 17-29 year olds in military counties, mortality is 13 percent higher the first week after the second check arrives, a number that is 30 percent larger than the effect in column (1) but with a standard error that is 21 percent larger as well. Among 17-39 year olds in this more restrictive sample, the coefficient on *Military\*Period2\*Week(1)* is now almost seven percent, which is statistically significant and 16 times larger than the similar coefficient for the non-military counties.

All of the results for the second pay period indicate that, in military counties, daily mortality rates are substantially higher the week after military checks are normally distributed. There is no comparable effect in non-military counties. More interestingly, the result is much more pronounced than after the first check of the month is received. We suspect the large difference in results between the first and second payday of the month for military personnel to be due to a combination of factors. As discussed in Chapter 2, many households have large re-occurring bills near the 1<sup>st</sup> of the month. We suspect a large portion of the paycheck paid near the 1<sup>st</sup> of the month will go towards these items. This means the second paycheck of the month might have a larger discretionary component. Non-military counties will not display this pattern around the 15<sup>th</sup> of the month since so few outside the military are paid on a twice-monthly basis.

### 3.2.3 Providing a Metric to Scale the Estimates

It is helpful to have a common metric in order to be able to compare the mortality

responses across the different payment amounts used in these tests. We do this by constructing an elasticity that measures the percentage change in aggregate annual mortality given a change in annual income generated by one additional paycheck.

This calculation is most easily made for the military example. Among those aged 17-29 years in military counties, there are 2.62 deaths per day. The second paycheck raises mortality by about 10 percent for one week, so one of these paydays increases annual mortality by about 0.2 percent (10 percent divided by 52 weeks). Data from the 1980 Census 5% PUMS (Ruggles et al., 2010) indicates that, for families in the “military” counties with someone aged 17-29 years, military pay represents roughly 24 percent of family income for the year. Therefore, each payday represents about one percent of aggregate annual income. Dividing 0.002 by 0.01, the elasticity of mortality with respect to the change in income is 0.20. This is a steep gradient in mortality with respect to income (Snyder and Evans, 2006).

For both Social Security pay schedules, each payment is a larger share of annual income because checks arrive once a month. The mortality responses are also smaller than in the military case. Using results from both the pre- and post-1997 Social Security payments schedules, we estimate annual mortality elasticities with respect to income of about 0.01 in the Social Security beneficiary population, which is substantially smaller than in the military wage analysis.

### 3.3 The Short-Term Mortality Consequences of One-time and Infrequent Income Receipt

In this section, we consider the short-term mortality impact of one-time and

infrequent income receipt. Specifically, we consider two cases: the 2001 Tax Rebates and the annual Alaska Permanent Fund payments. Both of these cases have been considered by authors in the literature on excess sensitivity. These two situations broaden the empirical work in this paper along three dimensions. First, these income changes can be considered exogenous increases in income (wealth), unlike the two cases in the previous section. The mortality impact of these payments could generate very different patterns. Second, these groups extend the phenomenon beyond the elderly and military personnel. Third, the infrequent nature of the payments will allow us to determine whether increases represent “short-term mortality displacement” where the deaths of the frail were hastened by a few days, a phenomenon routinely referred to as “harvesting” (Zeger et al., 1999).

### 3.3.1 The 2001 Tax Rebates

The *Economic Growth and Tax Relief Reconciliation Act*<sup>105</sup> was signed into law on June 7, 2001 and included a reduction in the tax rate on the lowest income bracket from 15 to 10 percent. This tax change was applied retroactively for income earned in 2001 and, as an advance payment on the tax cut, households were sent rebates based on their 2000 tax returns in the summer and fall of 2001. The maximum rebates for single and married taxpayers were \$300 and \$600, respectively, and approximately two-thirds of all people lived in households that received a rebate check. Johnson et al. (2006) estimate households received about \$500 on average, or about one percent of median annual family income.

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<sup>105</sup> <http://www.gpo.gov/fdsys/pkg/PLAW-107publ16/pdf/PLAW-107publ16.pdf>.

Rebate checks were mailed over a ten-week period and check distribution dates were based on the second-to-last digit of the Social Security number (SSN) of the person filing the taxes.<sup>106</sup> The first checks were sent on Monday, July 23, to taxpayers whose second-to-last SSN digit was a zero.<sup>107</sup> Table 3.5 shows the exact distribution dates of checks by SSN. The Treasury Department sent letters to taxpayers a few weeks before checks arrived to inform them of the size and date of their check (Johnson et al. 2006).

This tax rebate is a useful setting for testing the mortality consequences of income receipt, as the second-to-last digit of the SSN is effectively randomly assigned.<sup>108</sup> Johnson et al. (2006) use this fact and data from a special module in the Consumer Expenditure Survey to show that consumption of nondurable goods increased in the months after the arrival of checks, with food away from home being the main component that was affected.

We use the check distribution schedule to examine the short-run consequences of the rebates on mortality. For this project, the NCHS merged the second-to-last digit of a decedent's SSN from the National Death Index (NDI)<sup>109</sup> to the 2000-2002 MCOB data files.

The econometric model for this event is straightforward. Let  $i = 0$  to 9 index groups of people based on the second-to-last digit of their SSN. Let  $t$  index one of 30 7-

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<sup>106</sup> For married taxpayers filing jointly, the first Social Security number on the return determined mailing date.

<sup>107</sup> Households who filed their year 2000 tax return late may have been sent their rebates after the ten-week period shown in Table 5. According to Slemrod et al. (1997) 92 percent of taxpayers typically file on or before the normal April 15 deadline, so the vast majority of households would have received their checks according to the schedule outlined in Table 5.

<sup>108</sup> The last four digits of the SSN are assigned sequentially within a geographic area, so are effectively random. The second-to-last digit mailing system was in fact chosen because it was felt the random assignment made it a fair way to allocate the checks (Johnson et al., 2006).

<sup>109</sup> The NDI is an index of death record information designed to assist medical and health researchers who want to ascertain whether subjects in their studies have died, and includes each decedent's SSN. More information about the NDI can be found at [www.cdc.gov/nchs/ndi.htm](http://www.cdc.gov/nchs/ndi.htm).



day periods during 2001, with the first period beginning on Monday May 14<sup>th</sup> and the last beginning on Monday December 3<sup>rd</sup>. This 30-week period starts ten weeks prior to the first check being distributed and ends ten weeks after the last check was sent. Let  $y_{it}$  be the deaths for group  $i$  in week  $t$  and let  $REBATE1_{it}$  be a dummy variable that equals one for the week group  $i$  received a check. The estimating equation is then

$$(4) \quad \ln(y_{it}) = \alpha + REBATE1_{it} \beta_1 + \eta_i + v_t + \varepsilon_{it} \quad (3.4)$$

where  $v_t$  are fixed week effects,  $\eta_i$  are fixed group effects, and  $\varepsilon_{ij}$  is a random error term. The group effects identify persistent differences in weekly mortality counts that vary across groups, but since the second-to-last digit of a SSN is randomly assigned there should be little difference in mortality rates across groups. The week effects capture the differences that are common to all groups but vary across weeks. For example, the September 11 terrorist attacks occurred during Week 18 in our analysis. The Centers for Disease Control estimates that there were 2,902 deaths associated with September 11<sup>th</sup>, which is roughly twenty percent of weekly deaths during this period.<sup>110</sup> There also appears to be a drop in mortality in the weeks just after September 11<sup>th</sup> as individuals stayed home and reduced their travel. The week effects will capture these cyclic changes in mortality so long as the deaths associated with September 11 are equally distributed across the ten SSN groups. The coefficient on  $\beta_1$  is the key variable of interest and it identifies the short-run impact of the rebates on mortality.

There are two caveats to equation (4). First, only taxpaying units with taxable income in 2000 received a tax rebate in 2001. The coefficient on  $\beta_1$  represents a reduced-form effect and not the impact of actually receiving a check. Therefore, a key to the

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<sup>110</sup> <http://www.cdc.gov/mmwr/preview/mmwrhtml/mm51SPa6.htm>.

analysis is to reduce the sample to people likely to have received a tax rebate. We do this by restricting the sample to those aged 25 to 64, who are much more likely to have paid taxes than other age groups.<sup>111</sup> Second, for married couples filing jointly, the rebate check was sent according to the SSN of the first name on the IRS 1040 form. This form does not record the sex of the taxpayers so we have no idea whether husband or wives are more likely to be listed as the first taxpayer. Although both partners in a marriage are presumably treated by the additional income, the mailing of the check was based on the SSN of only one of them. Because people not sent a check but treated with a rebate through their spouse should be randomly distributed across the different groups, this should systematically bias our results towards zero.

The results for equation (4) are reported in Table 3.6. The SSN groups experience a statistically significant 2.7 percent increase in mortality in the week the checks arrive. There is a large p-value on the test that all the group fixed effects are zero, adding empirical support to the assumption that the second-to-last digit of the SSN is randomly assigned. Overall, the results suggest a large short-term increase in mortality immediately after income receipt. This effect is also present amongst the non-married and when we use only using the period prior to the September 11 terrorist attacks.<sup>112</sup>

Although we would prefer to estimate standard errors from equation (4) that allow for correlation in residuals within each group, Monte Carlo estimates suggest that these Huber/White-type procedures perform poorly when the number of groups is small

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<sup>111</sup> The IPUMS-CPS project (King et al., 2004) has attached estimates of taxable income to March Current Population Survey (CPS) data. Using data from the 2001 March CPS (2000 tax year), their estimates suggest that 52 percent of people aged 25-64 were in households that paid federal income taxes but this same number for people aged 65 and older was 26 percent.

<sup>112</sup> Restricting the sample to the unmarried produces a coefficient (standard error) on *REBATE1* of 0.0280 (0.0134). When we re-estimate the original model eliminating all data after week 17, which are observations after the September 11<sup>th</sup> attacks, the coefficient (standard error) on *REBATE1* is 0.0241 (0.0111)

(Wooldridge, 2003). The residuals from column (1) of Table 3.6 regressed on a one-period lag generate an estimate of the AR(1) coefficient (standard error) of 0.0085 (0.0584), suggesting that autocorrelation is not a problem in this case.

In column (2) of Table 3.6, we add *REBATE2*, *REBATE3*, and *REBATE4*, which are dummies for the second, third and fourth week after the checks arrive, respectively, to examine whether the increase in mortality in the first week represents mortality displacement. In the third week after the checks arrive there is a large drop in mortality that is similar in magnitude to the coefficient on *REBATE1*. Adding the *REBATE1* through *REBATE4* coefficients in column (2) produces an estimated change (standard error) in mortality of -0.0237 (0.0233). We cannot reject the null of no aggregate change in mortality over the first four weeks after checks arrive.

We define substance abuse-related deaths using the ICD-10 codes in a similar way as in the previous two sections. We estimate that eight percent of deaths in this sample are due to substance abuse, or 85 deaths per group per week. Column (3) of Table 3.6 contains the results for substance abuse deaths, and only the negative coefficient on *REBATE4* approaches statistical significance. Column (4) contains results for deaths not related to substance abuse, and the results are nearly identical to the results for all deaths in column (2), showing once again a relatively minor role for substance abuse in the aggregate relationship.

We also show the results for three age-based subgroups in Table 3.6: deaths among those aged 25-44 years in column (5), 45-54 years in column (6); and 55-64 years in column (7). For the youngest sample, none of *REBATE1* to *REBATE4* coefficients are statistically significant. The p-value on the test that the group effects are zero is 0.02;

given the persistently high values in the other regression, this may be chance. For 45-54 year olds, deaths increase by a statistically significant 5.3 percent in the first week after the checks arrive. The coefficients on *REBATE2* to *REBATE4* are less than one percent and statistically insignificant. Among 55-64 year olds, the coefficient on *REBATE1* is 1.5 percent and the coefficient on *REBATE2* is -1.5 percent, with neither statistically significant. There is a statistically significant negative coefficient on *REBATE3* of -4.1 percent, while the *REBATE4* coefficient is a statistically insignificant -1.0 percent. The total effects in the three age groups are all statistically insignificant.

Reducing the sample to specific causes of death produces few statistically significant coefficients due to the increased variance associated with disaggregated causes of death. We also estimate two placebo regressions using the same periods and group definitions as 2001, but re-estimated using 2000 and 2002 MCOD data. The coefficients (standard error) on *REBATE1* in these two models are 0.0094 (0.0107) and -0.0174 (0.0107), respectively.

### 3.3.2 Dividend Payments from the Alaska Permanent Fund

The Alaska Permanent Fund was established in 1976 to invest income received by the State of Alaska from the sale of oil, gas, and other minerals for the long-term benefit of current and future Alaskans. The fund has grown significantly over time, and had assets worth approximately \$35.9 billion at the end of the 2008 financial year.<sup>113</sup> Since 1982, an annual dividend has been paid to Alaskans from the income generated by fund investments during the previous five years. The amount paid has been between \$331 in

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<sup>113</sup> From the 2008 Annual Report of the Alaska Permanent Fund Corporation. Available at: <http://www.apfc.org/home/Content/reportspublications/reportArchive.cfm>.

1984 and \$2,069 in 2008 (when a one-off additional payment of \$1,200 was also made).

Alaska residents who have lived in the state for at least one year are eligible for the dividend, and the same amount is paid to everyone, regardless of their length of residency, age, or income.<sup>114</sup> Individuals must apply each year to receive the dividend, and at least 88 percent of Alaskans have received the dividend each year. Table 3.7 contains the dividend amounts and the percentage of the population receiving them in recent years.

Hsieh (2003) uses variation in the size of dividends by family size and over time to test whether nondurable consumption changes in response to dividend payments. Using the CEX from the 1984 to 2001, he finds no evidence households react to these payments – even though household consumption is sensitive to income tax refunds – which leads him to conclude that households adhere to the LC/PIH for large and predictable payments (like the Alaska dividend), but not for small and less predictable payments (like income tax refunds). In recent years, however, the dividend payments have been concentrated in early October and anecdotal evidence of increased spending after dividends arrive suggests activity-induced changes in mortality are possible as a result of the dividend.<sup>115</sup>

We explore the short-term relationship between income payments and mortality for recent years. Payments were initially made entirely by check, mailed at a rate of 50,000 per week. Payment by direct deposit was introduced in 1993. Approximately 30

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<sup>114</sup> Residency requirements have been the same since 1990. Minor changes occurred in earlier years. Historical information is available at: <https://www.pfd.state.ak.us/historical/index.aspx>

<sup>115</sup> See for example: Chambers, Mike. “Alaska Permanent Fund dividend is \$1,850.28,” *The Associated Press*, State & Local News, September 19, 2001; Egan, Timothy. “Fringe Benefits from Oil Give Alaska a Big Payday,” *The New York Times*, October 9, 1996, p. A1; Pemberton, Mary. “Alaskans prepare to spend annual windfall,” *The Associated Press*, State & Regional News, October 3, 2003.

percent of recipients initially received their dividend this way, which grew to two-thirds of recipients by 2001 and three-quarters by 2006. Direct deposits are made on only one or two dates, and since at least 2000, over 90 percent of paper checks were processed and mailed in a single batch shortly after the payment of direct deposits. The exact dates that direct deposits were paid, as well as the dates checks were issued, are shown in Table 3.7 for the years 2000 to 2006. We use the timing of direct deposits from 2000 through 2006 to investigate whether dividend payments change mortality patterns among Alaskans. We focus on this period because of the popularity of direct deposit and the close proximity between the receipt of direct deposits and paper checks.

The primary data for this analysis are from the MCOD restricted-use files from 2000 through 2006, which include decedents' state of residence. We create separate weekly counts of deaths for Alaskans and residents of the rest of the United States for periods that include the direct dividend payments and several weeks afterwards. The econometric model here is a simple difference-in-difference specification, with the data for the rest of the U.S. providing an estimate of the time path that would occur in the absence of the dividend intervention. Let  $w$  denote 12 seven-day periods that begin on Tuesdays,<sup>116</sup> with the first period each year beginning fifteen days after Labor Day (the first Monday in September).<sup>117</sup> Let  $\ln(y_{swy})$  be the natural log of the deaths for state  $s$  (with  $s=1$  for Alaska or  $s=0$  for all other states) in week  $w$  and year  $y$ .  $Dividend(1)$  is a dummy that equals one the first week after dividend payments are made and zero otherwise, and  $Alaska$  is a dummy variable for the state of interest. The model we

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<sup>116</sup> All direct deposits during 2000 to 2006 were made on Tuesdays, Wednesdays or Thursdays.

<sup>117</sup> We select the post-Labor day period for this analysis because daily mortality counts in the end of August and the first two weeks of September were incredibly volatile and did not match the trends in mortality counts for residents from other states.

estimate is:

$$(5) \quad \ln(Y_{swy}) = \alpha + Dividend(1)_{wy} Alaska_s \beta_1 + Alaska_s \beta_3 + v_{wy} + \varepsilon_{swy} \quad (3.5)$$

where  $v_{wy}$  is a fixed effect that varies by week  $w$  and year  $y$ , and  $\varepsilon_{swy}$  is a random error. The *Alaska* dummy variable controls for persistent differences in mortality counts between Alaska and the rest of the United States. The fixed week/year effects capture differences common to both groups, but which vary over time. The parameter  $\beta_1$  captures the short-run impact of the dividend payments on mortality. As in the previous section, we examine whether estimated mortality effects for the week after payments are made are the result of harvesting by including *Alaska\*Dividend(2)* to *Alaska\*Dividend(4)* in subsequent models.

The results for equation (5) are reported in Table 3.8. In the first two columns, we report results for models using all Alaskan deaths. In column (1), we only include *Alaska\*Dividend(1)*; in column (2), we include *Alaska\*Dividend(2)* to *Alaska\*Dividend(4)* as well. The results for the Alaska Permanent Fund tell a story similar to the one told by the results for the 2001 tax rebate. In column (1), we see an increase in deaths of 9.1 percent for the week checks are received, and a p-value of 0.12. The results in column (2) suggest substantial harvesting, with the coefficients on *Alaska\*Dividend(2)* and (3) being -3.7 percent and -9.8 percent, respectively. This final number has a t-statistic of 1.73, which is statistically significant at the 10 percent level.

With about one-fifth of the land mass as the continental United States but only 670,000 residents, Alaska is the most sparsely populated state. A large fraction of residents live in remote areas and have limited access to the Internet, banking services, the postal service, etc. In conversations with representatives of the Alaska Permanent

Fund, they indicated that a much larger fraction of the direct deposit recipients live in the urban areas of Alaska. In column (3) of Table 3.8, we restrict our attention to residents in the boroughs that contain Anchorage (260,283 residents in 2000 Census), Fairbanks (30,224) and Juneau (30,711), the only cities in Alaska with more than 10,000 residents.<sup>118</sup> In this model, we keep the same comparison group of non-Alaskan residents, as nearly everyone in the United States lives in a county with a town of more than 10,000 people.

In this urban sample, there is a 13 percent increase in mortality – an extra four deaths – the week direct deposit occurs. The p-value on this statistic is less than 0.10. As in both column (2) and the case of the 2001 tax rebates, we see a drop in mortality the third week after dividends are paid. The sum of the coefficients over the first four weeks after checks arrive is 0.148, although it is not statistically significant. As with the previous tests, the results are not entirely due to substance abuse. Using the same ICD-10 coding as in the tax rebate section, we attribute 8 percent of deaths among Alaskans to substance abuse. The impact of the Permanent Fund payments on non-substance abuse deaths, reported in column (4), is similar to the corresponding values for deaths in columns (3).<sup>119</sup> The coefficient on *Dividend(1)* is 0.1414 and it is statistically significant at the 10 percent level.

To check the robustness of these results, the rest-of-USA counts are replaced with state-level weekly mortality counts for states that have similarities to Alaska. One comparison uses the ten states in the continental United States with the closest mean

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<sup>118</sup> Alaska is organized into boroughs, which are equivalent to counties and form the basis for the Federal Information Processing System (FIPS) codes in the state. The restricted-use MCODE data identifies the FIPS code of residence for all decedents over this time period.

<sup>119</sup> There are too few substance abuse-related deaths in Alaska to separately estimate the effect for these deaths.



annual temperature to Juneau, Alaska, of which three have a lower average temperature and seven have higher.<sup>120</sup> Another uses ten states with similar per-capita income in 2007. Alaska is ranked 15<sup>th</sup>, and we use the five states ranked just lower and the five states ranked just higher than this level.<sup>121</sup> In both cases, the estimated model remains the same, except that there is a dummy variable for each state to capture underlying differences in mortality counts.

All Alaskan deaths are compared to similar temperature states in column (5) and similar income states in column (6) of Table 3.8. The coefficients are similar in direction and size to the results already discussed; the standard errors shrink, and in both regressions the positive coefficient on *Dividend(1)* and the negative coefficient on *Dividend(3)* are now statistically significant at conventional levels. The urban Alaskan results are re-run using these comparison states and presented in columns (7) and (8) of Table 3.8. In both cases, the coefficients remain qualitatively the same while the standard errors shrink. In the income-based sample, the net effect of the four coefficients is 15.1 percent, which is statistically significant at the 10 percent level.

### 3.3.3 Providing a Metric to Scale the Estimates

For the 2001 tax cuts and the Alaska Permanent fund examples, we can create a metric to compare these estimates to each other and other estimates in the literature. The

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<sup>120</sup> The National Oceanic and Atmospheric Administration have average temperature from 1971-2000 for 48 states here: <http://www.esrl.noaa.gov/psd/data/usclimate/tmp.state.19712000.climo>. They do not provide a figure for Alaska, although similar data is available for Juneau, Alaska for the same period, here: [http://www.census.gov/compendia/statab/cats/geography\\_environment/weather\\_events\\_and\\_climate.html](http://www.census.gov/compendia/statab/cats/geography_environment/weather_events_and_climate.html). The mean temperature in Juneau is 41.5 degrees. There are three states with colder average temperatures than Juneau (ND=40.43, ME=40.97, MN=41.16) and seven states with annual temperatures under 45 degrees (WY, MT, VT, WI, NH, ID, MI).

<sup>121</sup> Per-capita income in 2007 is from: <http://www.census.gov/statab/ranks/rank29.html>. The five ranked lower are IL, RI, HI, PA and FL, and the five higher are CO, MN, DE, NV, WA.

metric in this case is an elasticity that represents the percent increase in excess deaths associated with the percent change in income generated by the one-off payment.

Using data from the March Current Population Survey from 2001 to 2007, we estimate that the Alaska Permanent Fund payments increased annual per capita income over the 2000-2006 period by about 5.5 percent. Data from Column (8) in Table 3.8 suggests that this transfer increases mortality by 15.1% over the first four weeks after checks arrive, which is an increase in annual mortality of 0.29 percent. The mortality/income elasticity is therefore 0.053 (0.0029/0.055).

For the 2001 tax cut, we illustrate how the calculation is made for the 45-54 age group and simply report the results for the other age groups. Using estimates from the 2001 March Current Population Survey of who paid taxes in 2000, and assuming that married couples receive a \$600 rebate and individuals receive a \$300 rebate, we estimate that the tax rebate increased the annual family income of 45-54 year olds by 0.35%. The results from Table 3.6 suggest this increased mortality by 3.4 percent for a four week period, which is an increase in annual mortality of 0.065%. The mortality/income elasticity is therefore  $0.00065/0.0035=0.19$ . For the other two age groups, the sum of the Rebate (1)-(4) coefficients is negative and as a result, we estimate a mortality/income elasticity of -0.28 for adults aged 25-44 and a value of -0.29 for adults aged 55-64 years, which are similar to elasticities in Evans and Snyder (2006) for similar age groups.

### 3.4 Conclusion

As we outline above, a number of authors have documented a paycheck cycle where consumption increases after the receipt of income. These results have been

interpreted as being consistent with liquidity problems and hyperbolic discounting, and at odds with Lifecycle/Permanent Income hypothesis. In this paper, we document a similar phenomenon in health: mortality increases immediately after the receipt of income. The effect is broad-based, occurring for a wide variety of payments methods (transfer payments, paychecks, one-time cash bonuses, and annual residency-based dividends), a range of causes of death (substance abuse and non-substance abuse deaths, external causes, and heart attacks), and a range of populations (the elderly, tax payers, residents of Alaska, and people living near military bases).

The age variation across the Social Security and 2001 tax rebate analyses suggest that mortality in younger populations is more responsive to income receipt than in older groups. If the Social Security and military results are compared by looking at how much mortality increases relative to the percentage of annual income being received, then the effects are much larger in the military context.

Changing levels of consumption/activity is the most plausible mechanism through which income receipt affects mortality. The findings for particular causes of death in the Social Security analysis are consistent with this: we observe such relationships for causes of death connected to short-term consumption – like heart attacks and traffic accidents – but not for cancer deaths, where no such connection exists.

Three alternative reasons for such a relationship are improbable. First, the change to the Social Security payment schedule and the structure of the 2001 tax rebates allow us to rule out within-month or seasonal factors that coincide with income receipt. Second, the criteria for receiving these payments should not encourage people to improperly record dates of death for financial gain. For example, military paychecks are paid for

income that has already been earned, so misreporting death dates cannot change that value. Likewise, a deceased applicant's Permanent Fund dividends go to their estate, and the tax rebates were based on tax returns from the previous year.<sup>122</sup> Third, there is a literature suggesting that some patients tend to die right after milestone dates are reached (e.g., birthdates, anniversaries, holidays, etc.). While it is possible that income recipients wanted to hang on for one more check, the large spike in mortality for external causes and heart attacks and the lack of any effect for cancers runs counter to this argument.

It is important to stress that we cannot say anything about whether people are maximizing their own welfare. Non-smoothing consumption behavior is consistent with a number of utility maximization models, including hyperbolic discounting (Shapiro, 2005). Moreover, increased mortality does not necessarily reflect contemporaneous poor health: those whose deaths have been hastened by a few days may have been in poor health, and external causes of death are largely unconnected to short-term variation in a person's health.

When it comes to understanding the implications of these findings, the most important question is how much of the increased fatality is mortality displacement. While the 2001 tax rebates and the Alaska Permanent Fund payments have the potential to shed light on this, the results are not definitive on this point. In the tax rebate analysis using 25 to 64 year olds, a 2.3 percent increase in mortality in the first week after income receipt is offset by a 2.2 percent decrease in mortality in the third week. Among 45 to 54 year olds, however, there is a 5.3 percent increase in the first week that is not offset by decreases in the next three weeks. Similarly, while the analysis using all deaths in Alaska

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<sup>122</sup> Payments to Social Security beneficiaries cease the calendar month after death. Funeral homes and government agencies report deaths so there are limited opportunities for delaying reporting.

suggests there is not a net increase in deaths in the four weeks after income receipt, in urban areas there is a large increase deaths in the first week that is not fully offset in later weeks.

Age and cause of death are probably important for understanding this displacement issue. It is fairly easy to see how heart attack deaths are displacing mortality by a few days, as someone prone to a heart attack today is probably prone to one in a few days as well. In contrast, it is less likely that accidental deaths today would have occurred in the future. This is particularly true for younger people, who face few competing mortality risks. The Social Security analysis suggests both heart attacks and external causes are responsive to income, which may mean that some deaths are displacement while others are not.<sup>123</sup> Identifying the amount of mortality displacement will clarify the impact of income receipt on life expectancy.

Another interesting question is whether greater pay frequency mitigates some of the damage associated with payday mortality. It is not clear from our results that this is the case. The experience in the military, in particular, gives us pause as to the effectiveness of higher frequency payments. In that case, we found a large increase in mortality associated with the paycheck distributed near the middle of the month. Our conjecture is that since large bills such as rent/mortgage and car payments are bunched near the first of the month, less money from that paycheck is left over for discretionary items. In contrast, the midmonth check has less competition for resources and hence the larger mortality effect. If mortality is linked to having a full wallet, then increasing the number of days with money in the pocket may increase aggregate mortality. This is a

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<sup>123</sup> We tried to estimate the 2001 tax rebate and Alaska results by cause of death, but the sample sizes are too small to generate precise estimates.

subject for further research. The variation in the size of the mortality effect in response to payment size is also a subject for future research, as the structure of the data and the nature of our quasi-experiments do not allow us to examine this.

In recent years, authors have tested whether socioeconomic status causally affects health by using exogenous variation in education<sup>124</sup> and income.<sup>125</sup> There are conflicting results among studies examining the role of income, and our results below may be instructive for this literature. First, authors must measure the impact of income from the time of receipt, because there are immediate consequences which may be different from those in the long-term. Second, the short-term mortality effect of income receipt makes it more difficult to use exogenous variation in income to identify a causal link between income and health. This increases the size of the sample or of the income shock required to find a statistically precise income/health relationship. Third, these short-run effects may impact the efficacy of cash transfers, although more research is required to determine whether the negative mortality effect is a fixed cost of income receipt or changing in the amount of income received.

The results also suggest a potential mechanism for the pro-cyclic nature of mortality outlined in Ruhm (2000). The estimates in Ruhm and subsequent papers isolate a contemporaneous correlation between mortality and measures of the business cycle; yet to date, little has been offered to explain the pathways producing this result. However, if activity rises over the business cycle, then the short-term mortality effects of income

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<sup>124</sup> For example, authors have examined whether health outcomes are altered by increases in education generated by policies such as compulsory schooling (Lleras-Muney, 2005), an increase in access to colleges (Currie and Moretti, 2003) and the Vietnam Draft (de Walque, 2007; Grimand and Parent, 2007).

<sup>125</sup> Such work exploits variation in income produced by such factors as winning the lottery (Lindahl, 2005), German reunification (Fritjers et al., 2005), receiving an inheritance (Meer et al., 2003), South African pensions (Case, 2004) and changes in Social Security (Snyder and Evans, 2006).

receipt may provide just such an explanation. It may also account for much of the within-month mortality cycle described in Chapter 2.

One potential policy consequence flowing from these results is that the heightened mortality associated with income receipt might suggest that emergency rooms, hospitals, police, and fire departments should adjust staffing levels in accordance with predictable high- and low-mortality days. Our search of the Internet has so far not provided any anecdotal evidence that such adjustments already exist.

Figure 3.1: Mean Residuals from Ln(Daily Counts) Regression by “3rd of the Month” Social Security Payment Schedule and the 1st of Calendar Month, 1973-96, Ages 65+

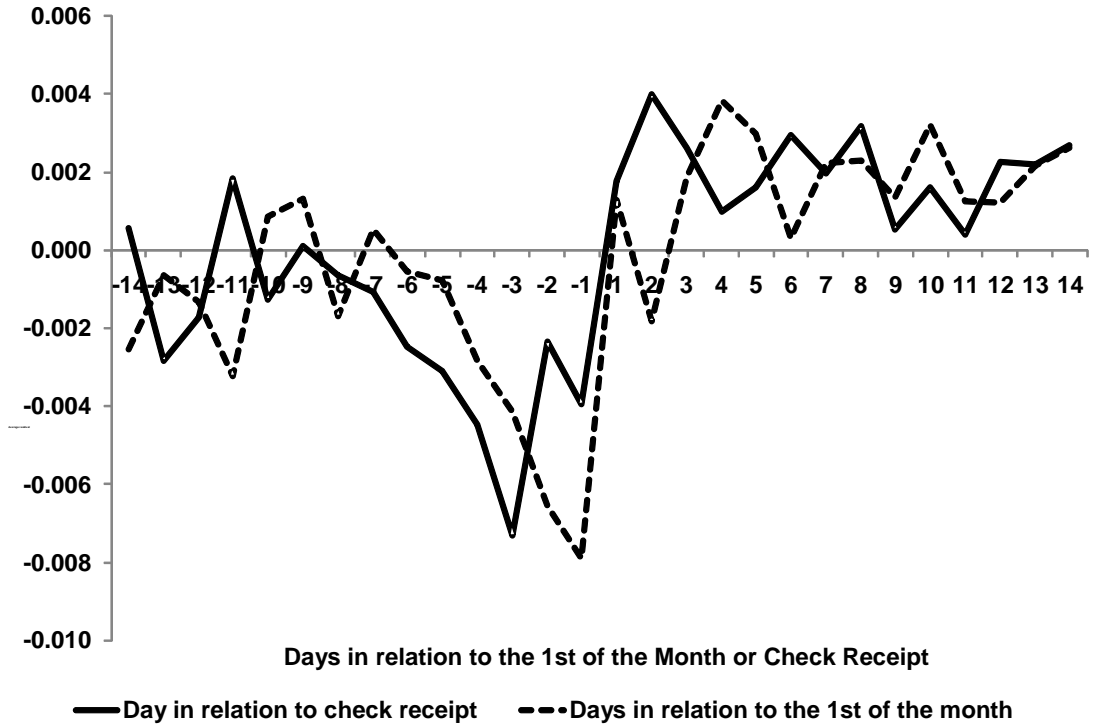




Figure 3.2: Relative Daily Mortality Rates, Military and Non-military Counties  
 Ages 17-29 years, MCOB, 1973-1988

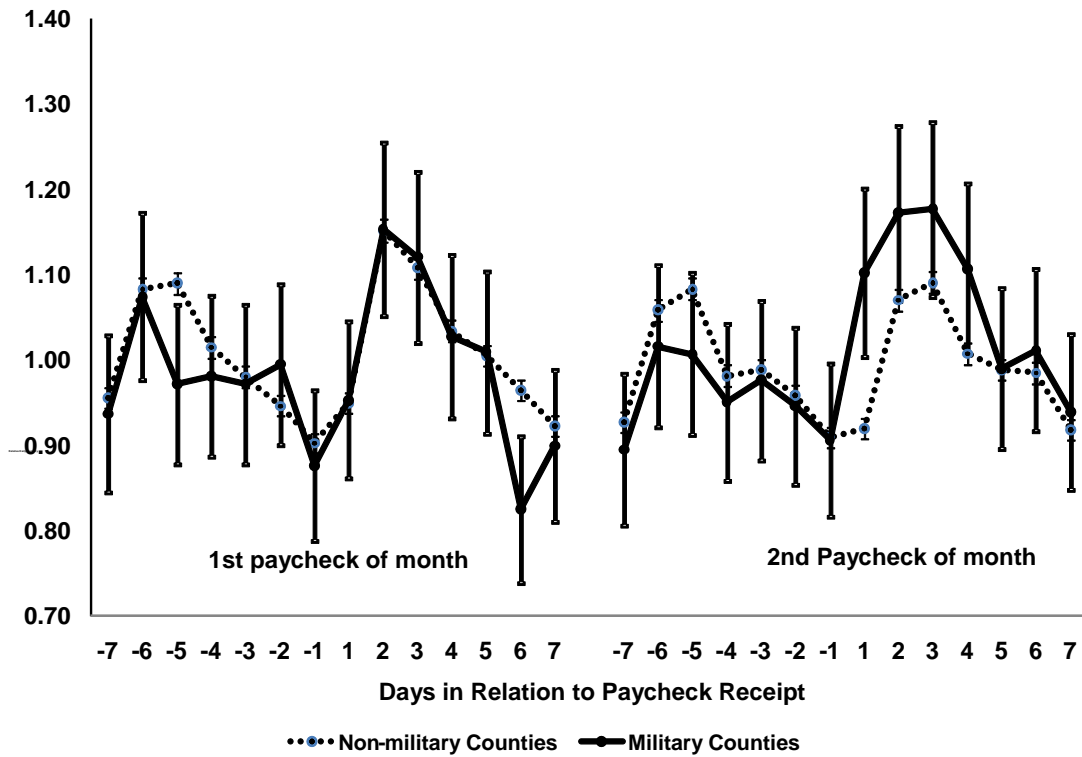


Table 3.1: Estimates of Log of Daily Mortality Counts Equation in Relation to “3<sup>rd</sup> of the Month” Social Security Payment Schedule and the 1<sup>st</sup> of the Calendar Month

	Age 65+	Age-based Subgroups, 1973-1996			Age 65-69	Age 65-69
	1973-96	Age 65-74	Age 75-84	Age 85+	2005-06	1995-96
	(1)	(2)	(3)	(1)	(3)	(4)
Payweek (-2)	0.0041 (0.0016)	0.0036 (0.0017)	0.0049 (0.0019)	0.0039 (0.0025)	-0.0122 (0.0083)	0.0105 (0.0078)
Payweek (1)	0.0046 (0.0015)	0.0063 (0.0017)	0.0050 (0.0018)	0.0022 (0.0021)	-0.0109 (0.0091)	0.0207 (0.0071)
Payweek (2)	0.0051 (0.0020)	0.0056 (0.0021)	0.0057 (0.0024)	0.0042 (0.0029)	-0.0209 (0.0127)	0.0041 (0.0092)
Payweek (3)	0.0050 (0.0029)	0.0050 (0.0027)	0.0064 (0.0032)	0.0037 (0.0043)	-0.0109 (0.0115)	-0.0002 (0.0083)
Week (-2)	-0.0003 (0.0017)	0.0008 (0.0018)	-0.0006 (0.0020)	-0.0011 (0.0027)	0.0154 (0.0070)	0.0028 (0.0068)
Week(1)	0.0027 (0.0014)	0.0045 (0.0016)	0.0015 (0.0018)	0.0020 (0.0020)	0.0155 (0.0085)	0.0044 (0.0055)
Week (2)	0.0020 (0.0018)	0.0027 (0.0020)	0.0013 (0.0023)	0.0018 (0.0026)	0.0219 (0.0095)	0.0134 (0.0103)
Week (3)	0.0005 (0.0021)	0.0011 (0.0022)	-0.0006 (0.0025)	0.0012 (0.0033)	0.0262 (0.0093)	0.0094 (0.0091)
R <sup>2</sup>	0.921	0.731	0.890	0.947	0.577	0.664
Mean Daily Deaths	3,946	1,288	1,538	1,122	472	553
Observations	8,766	8,766	8,766	8,766	730	731

Notes: The reference periods are *Week(-1)* and *Payweek(-1)*. *Week(3)* and *Payweek(3)* are not complete seven-day weeks, as they represent the days outside the 28-day periods centered, respectively, on the 1<sup>st</sup> of the calendar month and each day Social Security is paid. The numbers in parentheses are standard errors that allow for an arbitrary correlation in the errors within a particular synthetic month/year group based on the Social Security payment schedule. Other covariates in the model include a complete set of synthetic month and year effects based on the Social Security payment schedule, weekday effects, and a complete set of dummies for special days throughout the year described in footnote 13.

Table 3.2: Estimates of Log of Daily Mortality Counts Equation in Relation to the Post-1997 Social Security Payment Schedule and the 1<sup>st</sup> of the Calendar Month

	Aged 65-69 All Decedents 2005-06 (1)	Aged 65-69 Singles 2005-06 (2)	Aged 65-69 All Decedents 1995-96 (3)	Aged 50-59 All Decedents 2005-2006 (4)
Payweek(-2)	0.0071 (0.0041)	-0.0013 (0.0231)	0.0010 (0.0054)	-0.0056 (0.0042)
Payweek (1)	0.0111 (0.0035)	0.0275 (0.0176)	0.0001 (0.0042)	-0.0033 (0.0028)
Payweek (2)	0.0023 (0.0057)	0.0033 (0.0232)	-0.0043 (0.0050)	-0.0053 (0.0065)
Payweek (3)	-0.0188 (0.0110)	-0.0605 (0.0296)	-0.0147 (0.0100)	-0.0029 (0.0060)
Week(-2)	0.0052 (0.0061)	-0.0130 (0.0219)	0.0077 (0.0055)	-0.0058 (0.0058)
Week (1)	0.0138 (0.0061)	0.0187 (0.0190)	0.0201 (0.0047)	0.0172 (0.0048)
Week (2)	0.0086 (0.0057)	0.0241 (0.0180)	0.0194 (0.0068)	0.0081 (0.0058)
Week (3)	0.0149 (0.0066)	0.0233 (0.0286)	0.0088 (0.0082)	-0.0097 (0.0057)
Born 1 <sup>st</sup> to 10 <sup>th</sup>	-0.0239 (0.0058)	-0.0190 (0.0116)	-0.0220 (0.0056)	-0.0254 (0.0039)
Born 11 <sup>th</sup> to 20 <sup>th</sup>	-0.0308 (0.0049)	-0.0480 (0.0148)	-0.0356 (0.0048)	-0.0271 (0.0031)
R <sup>2</sup>	0.303	0.080	0.394	0.242
Mean Daily Deaths	157	12.0	185	215
Observations	2,190	2,190	2,193	2,190

Notes: The reference periods are *Week(-1)* and *Payweek(-1)*. *Week(3)* and *Payweek(3)* are not complete seven-day weeks as they represent the days outside the 28-day periods centered, respectively, on the 1<sup>st</sup> of the calendar month and each day Social Security is paid. Decedents are divided into three groups: those born on the 1<sup>st</sup> to 10<sup>th</sup>, 11<sup>th</sup> to 20<sup>th</sup>, and 21<sup>st</sup> to 31<sup>st</sup> of the month. The numbers in parentheses are standard errors that allow for an arbitrary correlation in the errors within a particular synthetic month/year group based on the Social Security payment schedule. Other covariates in the model include a complete set of synthetic month and year effects based on the Social Security payment schedule, weekday effects, a complete set of dummies for special days throughout the year described in footnote 13, and dummies for observations for decedents born in the first two periods in the month.

Table 3.3: Estimates of Log of Daily Mortality Counts Equation in Relation to “3<sup>rd</sup> of the Month” Social Security Payments and the 1<sup>st</sup> of the Calendar Month, By Involvement of Substance Abuse and Cause of Death, Aged 65 Years and Over

	All Deaths 1979-96 (1)	Substance Abuse 1979-96 (2)	Non-Substance Abuse 1979-96 (3)	External Causes 1973-96 (4)	Heart Attacks 1973-96 (5)	All Cancers 1973-96 (6)	All Other Causes 1973-96 (7)
Payweek(-2)	0.0039 (0.0018)	0.0086 (0.0109)	0.0039 (0.0018)	0.0268 (0.0061)	0.0042 (0.0023)	0.0026 (0.0023)	0.0035 (0.0020)
Payweek (1)	0.0038 (0.0016)	0.0367 (0.0112)	0.0036 (0.0016)	0.0410 (0.0057)	0.0048 (0.0023)	0.0009 (0.0022)	0.0043 (0.0018)
Payweek (2)	0.0045 (0.0022)	0.0099 (0.0137)	0.0044 (0.0022)	0.0322 (0.0070)	0.0063 (0.0028)	0.0004 (0.0028)	0.0051 (0.0025)
Payweek (3)	0.0038 (0.0034)	0.0119 (0.0131)	0.0037 (0.0034)	0.0275 (0.0074)	0.0052 (0.0038)	0.0044 (0.0030)	0.0041 (0.0035)
Week(-2)	0.0001 (0.0018)	0.0111 (0.0111)	-0.0002 (0.0019)	0.0077 (0.0061)	-0.0020 (0.0024)	0.0015 (0.0024)	-0.0003 (0.0020)
Week (1)	0.0043 (0.0015)	0.0190 (0.0111)	0.0041 (0.0015)	0.0257 (0.0059)	0.0030 (0.0022)	0.0006 (0.0023)	0.0023 (0.0015)
Week (2)	0.0034 (0.0018)	0.0164 (0.0129)	0.0033 (0.0018)	0.0128 (0.0072)	0.0002 (0.0026)	0.0052 (0.0027)	0.0013 (0.0021)
Week (3)	0.0016 (0.0023)	0.0068 (0.0143)	0.0016 (0.0023)	0.0041 (0.0077)	-0.0017 (0.0031)	0.0051 (0.0030)	-0.0002 (0.0024)
R <sup>2</sup>	0.901	0.370	0.900	0.395	0.847	0.961	0.883
Mean Daily Deaths	4,124	36	4,088	89	1,008	802	2,047
Observations	6,575	6,575	6,575	8,766	8,766	8,766	8,766

Notes: The reference periods are *Week(-1)* and *Payweek(-1)*. *Week(3)* and *Payweek(3)* are not complete seven-day weeks as they represent the days outside the 28-day periods centered, respectively, on the 1<sup>st</sup> of the calendar month and each day Social Security is paid. Decedents are divided into three groups: those born on the 1<sup>st</sup> to 10<sup>th</sup>, 11<sup>th</sup> to 20<sup>th</sup>, and 21<sup>st</sup> to 31<sup>st</sup> of the month. The numbers in parentheses are standard errors that allow for an arbitrary correlation in the errors within a particular synthetic month/year group based on the Social Security payment schedule. Other covariates in the model include a complete set of synthetic month and year effects based on the Social Security payment schedule, weekday effects, a complete set of dummies for special days throughout the year described in footnote 13, and dummies for observations for decedents born in the first two periods in the month.

Table 3.4: Maximum Likelihood Estimates of Daily Mortality Negative Binomial Equation, Counties With and Without a High Military Presence, 1973-1988

Covariates	Treatment counties have 15% military presence		Treatment counties have 20% military presence	
	Deaths 17-29 year olds	Deaths 17-39 year olds	Deaths 17-29 year olds	Deaths 17-39 year olds
(1) Non-military x Pay period 1 x Week 1	0.0177 (0.0045)	0.0191 (0.0039)	0.0175 (0.0051)	0.0191 (0.0039)
(2) Military x Pay period 1 x Week 1	0.0188 (0.0309)	0.0049 (0.0233)	0.0013 (0.0390)	0.0154 (0.0309)
p-value: Test on test: (1)=(2)	0.972	0.661	0.667	0.901
(3) Non-military x Pay period 2 x Week 1	0.0097 (0.0043)	0.0041 (0.0033)	0.0098 (0.0042)	0.0042 (0.0033)
(4) Military x Pay period 2 x Week 2	0.1028 (0.0305)	0.0462 (0.0252)	0.1305 (0.0368)	0.0680 (0.0311)
p-value: Test on test: (3)=(4)	0.002	0.033	0.001	0.028
Mean of dependent variable:				
Non-Military counties	132.3	242.8	132.3	242.8
Military counties	1.62	2.62	1.09	1.78

Notes: There are 10,584 observations in each model. Military counties have over 15 or 20 percent of 17 to 64 year old residents who were active military personnel in the 1970, 1980, and 1990 Censuses while non-military counties had less than one percent of the 17 to 64 year old residents in the military in 1970, 1980 and 1990. Numbers in parentheses are standard errors that allow for an arbitrary correlation across observations within a synthetic month/year group based on military payments. Other covariates include a complete set of synthetic month and year effects, weekday effects, dummies for special days described in footnote 13, a dummy for observations from counties with a high military presence, an indicator for the first pay period, and an interaction between the military county and pay period indicators.

Table 3.5: When 2001 Tax Rebates Were Distributed

Last 2 digits of SS #	Checks distributed during the week of	Last 2 digits of SS #	Checks distributed during the week of
00-09	July 23	50-59	August 27
10-19	July 30	60-69	September 3
20-29	August 6	70-79	September 10
30-39	August 13	80-89	September 17
40-49	August 20	90-99	September 24

Table 3.6: Estimates of Log of Weekly Mortality Counts Equation  
Decedents Aged 25 to 64 Years, 30-Week Period, Summer and Fall 2001

Independent Variable	All Deaths (1)	All deaths (2)	Substance abuse (3)	Non-substance abuse (4)	Aged 25-44 yrs (5)	Aged 45-54 yrs (6)	Aged 55-64 yrs (7)
Rebate1	0.0269 (0.0100)	0.0227 (0.0100)	0.0075 (0.0370)	0.0243 (0.0109)	-0.0089 (0.0198)	0.0530 (0.0179)	0.0151 (0.0150)
Rebate2		-0.0157 (0.0101)	-0.0134 (0.0371)	-0.0161 (0.0109)	-0.0222 (0.0199)	-0.0101 (0.0179)	-0.0160 (0.0150)
Rebate3		-0.0221 (0.0101)	-0.0182 (0.0371)	-0.0233 (0.0109)	-0.0119 (0.0199)	-0.0043 (0.0179)	-0.0414 (0.0150)
Rebate4		-0.0085 (0.0100)	-0.0693 (0.0370)	-0.0029 (0.0109)	-0.0081 (0.0198)	-0.0048 (0.0179)	-0.0100 (0.0150)
Total effect (Rebate1-4)		-0.0237 (0.0233)	-0.0934 (0.0859)	-0.0183 (0.0252)	-0.0511 (0.0460)	0.0338 (0.0415)	-0.0523 (0.0347)
P-value on Test, Group Effects=0	0.813	0.806	0.937	0.829	0.024	0.459	0.581
R <sup>2</sup>	0.715	0.723	0.157	0.724	0.791	0.410	0.256
Mean Weekly Deaths per Group	1,014	1,014	85	929	249	314	451

Notes: Standard errors are in parentheses. The other covariates in the model are week fixed effects and Social Security number group fixed effects. Each regression has 300 observations.

Table 3.7: Timing and Size of Alaska Permanent Fund Dividend Payments

Year	Pop. of Alaska	% Pop. Receiving Payment	Amount of Payment	% Paid by Direct Deposit	Date/Day of Direct Deposit	Date/Day 1 <sup>st</sup> Checks Issued	% Checks Issued in 1st Batch
2000	627,533	93%	\$1,963.86	64%	10/4,W	10/5,Th	92.2%
2001	632,241	93%	\$1,850.28	66%	10/10,W	10/17,W	93.6%
2002	640,544	92%	\$1,540.76	70%	10/9,W	10/16,W	93.3%
2003	647,747	92%	\$1,107.56	72%	10/8,W	10/15,W	93.5%
2004	656,834	91%	\$919.84	72%	10/12,Tu	10/19,Tu	92.1%
2005	663,253	90%	\$845.76	73%	10/12,W	10/21,F	90.9%
2006	670,053	88%	\$1,106.96	76%	10/4,W & 10/19,Th	11/14,Tu	97.8%

Source: Annual Reports of the Alaska Permanent Fund Dividend Division, 2000 to 2008.



Table 3.8: Estimates of Log of Weekly Mortality Counts Equation  
Alaskans Compared to Residents in the Rest of USA, 2000 to 2006

Independent Variable	Compared to Rest-of-USA				Comparison to Similar States			
	All Deaths		Urban Areas		All Deaths		Urban Areas	
	All	All	All	W/o sub. abuse	Temp. based	Income based	Temp. based	Income based
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Alaska	0.0907	0.0799	0.1329	0.1414	0.0836	0.0771	0.1366	0.1301
*Dividend(1)	(0.0551)	(0.0562)	(0.0742)	(0.0817)	(0.0370)	(0.0313)	(0.0398)	(0.0344)
Alaska		-0.0368	0.0272	0.0518	-0.0479	-0.0325	0.0162	0.0315
*Dividend(2)		(0.0562)	(0.0742)	(0.0817)	(0.0370)	(0.0313)	(0.0398)	(0.0344)
Alaska		-0.0975	-0.0809	-0.0507	-0.1033	-0.1055	-0.0867	-0.0889
*Dividend(3)		(0.0562)	(0.0742)	(0.0817)	(0.0370)	(0.0313)	(0.0398)	(0.0344)
Alaska		0.0132	0.0790	0.0946	0.0059	0.0125	0.0717	0.0783
*Dividend(4)		(0.0562)	(0.0742)	(0.0817)	(0.0370)	(0.0313)	(0.0398)	(0.0344)
Total Effect [Alaska* Dividend(1)-(4)]		-0.0412 (0.1333)	0.1582 (0.1761)	0.2370 (0.1938)	-0.0617 (0.0878)	-0.0484 (0.0743)	0.1378 (0.0944)	0.1510 (0.0816)
R <sup>2</sup>	0.9996	0.9996	0.9994	0.9994	0.9941	0.9942	0.9971	0.9970
Mean Weekly Deaths in Alaska	60.4	60.4	33.0	30.4	60.4	60.4	33.0	33.0
Observations	168	168	168	168	770	770	770	770

Notes: Standard errors are in parenthesis. There are 168 observations in each regression. The average deaths per week in the rest of the United States is 45,866. The average number of non-substance abuse deaths per week in the rest of the United States is 44,606. The other covariates in the model are fixed week-year effects and a dummy variable for weekly mortality counts in Alaska.

## Appendices

### A1. Appendices for Chapter 1

This appendix provides additional information about the data sources used for this project and the main data preparation issues. It also contains additional results mentioned in footnotes: that the main results in Section 1.3 are robust to alternative specifications; the results when annual earnings is used as the dependent variable; and that beneficiary cohort effects do not seem to explain the results in Section 1.4.2 for the role of time on disability benefits.

#### A1.1 Data Sources

Five extracts of SSA administrative datasets are used for this project: (1) Supplemental Security Record – DA&A Extract; (2) Supplemental Security Record – Longitudinal File; (3) Master Beneficiary Record – 810 File; (4) Disability Master File (831 File); and (5) Master Earnings File. More details about each of these datasets are provided in this appendix.

*Supplemental Security Record – DA&A Extracts* are extracts of the Supplemental Security Record, the system used to manage the SSI program, which include applicants and recipients with alcohol or drug addictions. These extracts were being produced every three months in early 1996, and the March and June 1996 extracts were obtained for this project. They provide snapshots of recent program activity, and have been used by Barber (1996), Stapleton et al. (1998) and Waid and Barber (2001) to report the number and characteristics of DA&A beneficiaries.

The *Supplemental Security Record – Longitudinal File* (SSR) and *Master Beneficiary Record – 810 File* (MBR) provide details of individuals' program history for, respectively, SSI and DI. The MBR also provides information on an individual's usage of Retirement and Survivor's Insurance. Both files include information on each individual's monthly program status and the federal payments due. A description of the SSR is provided by Pickett and Scott (1996), and documentation on both datasets is provided for the data linkage projects of SSA and the National Center for Health Statistics (see link in footnote 9 of the paper).

The *Disability Master File / 831 File* includes details about medical disability determinations; the "831" name refers to the form from which much of the information comes. A record is generated whenever an initial determination is made by state-level Disability Determination Services (DDS), and additional records are generated for subsequent decisions, corrections and reviews conducted by DDS offices. Higher-level decisions, such as those made by Administrative Law Judges, are handled by a different part of SSA and are normally missing from the 831 File. Chen and van der Klaauw (2006) provide some details about the variables listed on the 831 File.

The primary and secondary impairments are listed on each 831 File record, as is an individual's education. Consistent extracts of the 831 File are available from 1989; education information is reliable from 1992. Given most DA&A beneficiaries applied after 1991 and most applied to be re-classified in 1996, education is available for nearly the whole sample.

*Master Earnings File* contains earnings data used to calculate benefit amounts for SSA benefit payments, and comes from employers and the Internal Revenue Service. The

extract used for this project lists annual wage (W-2) and self employment earnings for individuals and includes from 1978 to 2008. Olsen and Hudson (2009) provide an excellent overview of the Master Earnings File, while Kopscuk, Saez and Song (2009) provide additional information about the quality of these data. There is a Social Security earnings cap above which earnings do not affect Social Security calculations, and the key issue with these data is the quality of earnings data above this cap. SSA retained information on uncapped W-2 earnings for the first time in 1978, and Kopczuk et al. (2009) find these data to be reliable from 1981. Self-employment earnings are not used, as they are less reliable and were effectively top-coded at the taxable maximum until 1993 (when the cap on the Medicare tax was eliminated) (Olsen and Hudson, 2009).

## A1.2 Main Data Issues

The key issues related to preparing the data are outlined below.

*Data Cleaning.* Most demographic information is taken from the DA&A extract; education is taken from the 831 File. Records with missing sex, date of birth and state of residence information are excluded. Particular attention is paid to the quality of date of birth data, as age is important for controlling employment differences in the regressions. Date of birth was taken from the DA&A Extract, SSR and MBR (where available), and an individual was excluded if they were inconsistent (which occurred in around 1.5 percent of cases).

Addiction information was missing in around eight percent of cases; these were omitted, as it was not completely clear whether this group included some beneficiaries whose drug and alcohol addiction was not material in their original application for

disability benefits. A small number of values in the Master Earnings File were unusually large and inconsistent with SSA program usage, and were obviously reporting errors. To remove these errors, 65 individuals who had W-2 earnings that would have put them in the top one percent of households in terms of income were removed; these earnings levels are taken from Piketty and Saez (2003) and updates that Saez provides on his website (available at: <http://elsa.berkeley.edu/~saez/>).

*Sample Restrictions.* The key sample restrictions are mentioned in the text: (1) individuals aged between 30 and 64 years of age at the beginning of 1997; (2) who started to receive payments between 1<sup>st</sup> January 1989 and 1<sup>st</sup> April 1996; (3) who were in current payment status in the second quarter of 1996 (to remove individuals who had died or left the program before the end of the DA&A program was announced); (4) and who were due at least one-third of the standard SSI payment in the second quarter of 1996 (i.e., less than \$200 a month, to remove individuals in Medicaid facilities and where they were unlikely to be dependent on these payments).

Note that these restrictions do not exclude individuals who responded to the policy change prior to termination of benefits in January 1997. Individuals earning at levels that reduced their disability benefits or who no longer adhered to DI or SSI program conditions in the second half of 1996 were still included in the sample. Around three percent of the sample had program status codes in the second half of 1996 that indicated they were earning at levels that limited the disability benefits they received. These individuals were generally assigned program codes in January 1997 that indicated they had been terminated as a result of the policy change.

*Identifying Terminated and Reclassified Beneficiaries.* While I do not directly observe who was reclassified under a different disability and who was terminated as a result of the policy change, I do observe the program classifications and payments in January 1997 and I use that to infer an individual's outcome. The program status variables (PSTAT in the SSR and the Ledger Account File in the MBR) are backdated, so an individual's January 1997 variables should have been updated once the case was decided if it occurred later than then.

A person is considered to have been reclassified if they were in current payment status in either DI or SSI in January 1997. A person is considered to have been terminated as a result of the policy if they were due no payments in January 1997 and had a "disability cessation" program status code (N07 on the SSR and T8 on the MBR). These codes are rarely assigned. For example, tabulations of the raw Master Beneficiary Record file show that there were 23,295 individuals assigned the disability cessation code in January 1997, compared to a monthly average of 53 people throughout 1996. A similar spike in N07 codes in the SSR occurs in January 1997. Therefore the terminated groups should include very few individuals who would have been assigned these codes because of disability cessation unrelated to the policy change.

There are 12 percent of the DI and 28 percent of the SSI sample are neither clearly reclassified nor terminated as a result of the policy. This group is probably a mix of reclassified beneficiaries with an unusual payment status in Jan 1997, individuals losing benefits for reasons unrelated to the policy change, or terminated beneficiaries who were assigned a rare termination code instead of the N07 or T8 codes. The use of rare codes does increase in January 1997, suggesting some staff may have been unclear

on the correct administrative procedures for this one-off policy change. If individuals assigned these rare codes are counted as terminated, then an additional 4,500 people are added to the terminated groups (mostly in the SSI sample). Counting these individuals in the terminated groups leads to similar estimates of the employment effects.

### A1.3 Robustness of Main Results in Section 1.3

The main results in Section 1.3 of the paper are generated using linear probability models with individual fixed effects and sex-specific cubic terms in age, and presented in Figure 2. I note in footnote 18 that the results are similar using three alternative specifications, which are presented here. This is done for both samples and for both employment measures.

Equation (1) in the paper, denoted as (A1) here, defines the main approach:

$$y_{it} = \alpha_i + \theta_t + X_{it}\lambda + \sum_{\substack{t=1989 \\ t \neq 1995}}^{2008} D_t * TERMINATED_i \beta_t + u_{it} \quad (\text{A1})$$

Where  $y_{it}$  denotes binary employment outcome for the  $i^{th}$  person in the  $t^{th}$  year,  $\alpha_i$  are individual fixed effects,  $\theta_t$  are time fixed effects,  $X_{it}$  represents two sex-specific cubic terms in age, and  $TERMINATED_i$  is a dummy variable equal to one if the person lost their benefits (and zero otherwise). The time-varying differences between terminated and reclassified beneficiaries are identified by the interaction of  $TERMINATED_i$  with time dummy variables  $D_t$ , which are equal to one in year  $t$  and zero otherwise. The reference year is 1995, and standard errors are calculated allowing for heteroskedasticity and an arbitrary correlation in errors for each individual.

The second set of results is estimated using the same equation, except that the individual fixed effects are replaced by a single constant and  $TERMINATED_i$  is included

directly to control for permanent differences between terminated and reclassified beneficiaries. That is:

$$y_{it} = \alpha + \theta_t + X_{it}\lambda + TERMINATED_i + \sum_{\substack{t=1989 \\ t \neq 1995}}^{2008} D_t * TERMINATED_i\beta_t + u_{it} \quad (A1')$$

The third set of results is generated using the same regression as this one, except that the sex-specific cubic terms are replaced by sex-specific age dummy variables.

The fourth set of results is produced using the following logit specification:

$$P[y_{it} = 1] = \exp(W_{it}\gamma) / [1 + \exp(W_{it}\gamma)] \quad (A2)$$

Where  $W_{it}\gamma = \alpha + \theta_t + X_{it}\lambda + TERMINATED_i\beta_0 + \sum_{\substack{t=1989 \\ t \neq 1995}}^{2008} D_t * TERMINATED_i\beta_t$ .

For the interaction terms, marginal treatment effects are calculated as the double differences in the estimated probabilities when each dummy variable equals one as compared to when it is zero; see Ai and Norton (2003) for more details. Marginal effects are estimated for each treated individual, and the presented coefficients are the mean values of these effects. Standard errors are calculated using the delta method.

Table A1 contain these results for the DI sample. Columns (1) to (4) contain the results using the “any earnings” definition of employment. The results are similar across the regressions. As shown in the paper, the removal of individual fixed effects produces almost identical results. This is also the case with the use of sex-specific age dummies. While the logit coefficients are the most different, the average differences between the linear probability results without individual fixed effects are 1.8 percentage points and the analysis suggests similar employment patterns and levels of statistical significance. Columns (5) to (8) contain the results when employment is defined in terms of earning more than the 1996 Substantial Gainful Activity threshold (\$8,339). The four sets of



results are again similar; in this analysis, the average difference between the linear probability and the logit coefficients is 0.5 percentage points.

The SSI results are presented in Table A1.2, with results for the “any earnings” measure in columns (1) to (4) and the results for the 1996 SGA earnings threshold in columns (5) to (8). The four sets of results are similar for both employment outcomes; the average differences between the logit and linear probability coefficients are around 1.5 percentage points with both definitions of employment.

#### A1.4 Results using Earnings using Earnings as the Dependent Variable

Binary employment measures are used as the dependent variables in regressions throughout the paper. In footnote 17, I note the results are similar if earnings is used as the dependent variable. Below are the full sets of coefficients generated from regressions using earnings as the dependent variable for the 1989 to 2008 period. DI sample results are presented in Table A1.1 and SSI sample results are presented in Table A1.2.

Two sets of results are shown in each table. The first is estimated using equation (1) in the paper, denoted as equation (A1) here:

$$y_{it} = \alpha_i + \theta_t + X_{it}\lambda + \sum_{\substack{t=1989 \\ t \neq 1995}}^{2008} D_t * TERMINATED_i \beta_t + u_{it} \quad (A1)$$

Where  $y_{it}$  denotes the wage earnings for the  $i^{th}$  person in the  $t^{th}$  year,  $\alpha_i$  are individual fixed effects,  $\theta_t$  are time fixed effects,  $X_{it}$  represents two sex-specific cubic terms in age, and  $TERMINATED_i$  is a dummy variable equal to one if the person lost their benefits (and zero otherwise). The time-varying differences between terminated and reclassified beneficiaries are identified by the interaction of  $TERMINATED_i$  with time

dummy variables  $D_t$ , which are equal to one in year  $t$  and zero otherwise. The reference year is 1995, and standard errors are calculated allowing for heteroskedasticity and an arbitrary correlation in errors for each individual.

The second set of results is estimated using the same equation (1), except that the individual fixed effects are replaced by a single constant and  $TERMINATED_i$  is included directly to control for permanent differences between terminated and reclassified beneficiaries. That is:

$$y_{it} = \alpha + \theta_t + X_{it}\lambda + TERMINATED_i + \sum_{\substack{t=1989 \\ t \neq 1995}}^{2008} D_t * TERMINATED_i \beta_t + u_{it} \quad (A1')$$

Consider the DI sample results in Table A1.1. The coefficients of interest are shown in columns (1) and (2). Both sets of pre-1995 coefficients are smaller than \$301. In 1996, terminated beneficiaries' relative earnings increases by around \$650. Coefficients rise from there, and peak in 1998 at around \$5,200. They decline thereafter, and are around \$2,400 in 2008. The standard errors are never larger than \$126, meaning the coefficients are precisely estimated and the post-termination earnings differences are highly statistically significant. These patterns are similar to those observed when employment thresholds are used to define the dependent variable.

The full sets of covariates are also presented in Table A1.1. All of the sex-specific age variables are large and statistically significant at the one percent level. The time dummy variables are also generally statistically significant at conventional levels in both regressions. These covariates have similarly important roles in the regression results presented throughout the paper.

The SSI sample results are presented in Table A1.2. A similar pattern of results is generated in both regressions: coefficients in the pre-1995 period are less than \$100; there is an increase in terminated beneficiaries' relative earnings of around \$400 in 1996, which then rises to a peak of around \$2,800 in 2000; their earnings steadily decline throughout the rest of the sample period. The covariates perform a similar role in this analysis to that described for the DI sample.

### A1.5 Do Beneficiary Cohort Effects Explain the Section 1.4.2 Results?

All of the terminations occur in January 1997, making it difficult to separate effects related to time on the program from effects related to differences across beneficiary cohorts. As discussed in Section 1.4.2 in the paper, several exercises are undertaken to determine when the inverted U-shaped pattern presented in Table 4 and Figure 3 seems to be due to cohort effects. More detail about these exercises is provided here; the coefficients generated by these results for the DI sample are presented in Table A1.5 and A1.6.

The main results for the DI sample are presented in column (1) of Table A5. These come from the same analysis presented in column (1) of Table 1.4 and in Figure 1.3 in the paper, and generated using the following equation:

$$y_{it} = \alpha_i + \theta_t + X_{it}\lambda + Z_{it}\varphi_t + DISTIME_i * Z_{it}\varphi_t + DISTIME_i^2 * Z_{it}\varphi_t + DISTIME_i^3 * Z_{it}\varphi_t + u_{it} \quad (A3)$$

Where  $Z_{it}\varphi_t = \sum_{\substack{t=1989 \\ t \neq 1995}}^{1997} D_t * TERMINATED_i\beta_t + RESPONSE_{it}\delta_1 +$

$POSTTREND_t\delta_2$ .

Equation (A3) is the same one presented in footnote 23 in the paper. Time on disability benefits,  $DISTIME_i$ , is the length of time between the month when an individual began receiving disability payments and January 1997. All of the other variables are as already described. Column (1) shows all of the same information presented in column (1) of Table 4: the coefficient for  $RESPONSE_{it}$ ; the three coefficients from the interactions between  $RESPONSE_{it}$  and the cubic terms of  $DISTIME_i$ ; the maximum employment response resulting from combining these four coefficients and the value of  $DISTIME_i$  where this maximum occurs; and how much higher this response is than the total employment response at nine months of benefit receipt and at six years of benefit receipt. As discussed in the paper, the four coefficients of interest are each statistically significant at the one percent level and combine to create an inverted-U relationship where the peak employment response at around 2.5 years is approximately 42 percent higher than both the response at nine months and at six years. In addition to the information presented in column (1) of Table 1.4, column (1) of Table A1.5 also contains the total employment effects at yearly intervals between zero and six years of disability benefit receipt. This provides similar information to that presented in Figure 3 in the paper.

The next four columns of Table A1.5 contain results from similar regressions where controls are added or the sample is varied in order to see whether the observed pattern disappears. The first variation is to control for *unemployment rates at time of application*. As discussed in the paper, labor market opportunities can potentially affect the decision to apply for disability benefits. To see whether changes in unemployment rates over time can account for the U-shaped pattern, I also separately interact  $UNEMP_i$ ,

the state-level unemployment rates in the year individuals applied for disability benefits, with the variables identifying employment differences between terminated and reclassified beneficiaries throughout the sample period. That is:

$$y_{it} = \alpha_i + \theta_t + X_{it}\lambda + Z_{it}\varphi_t + DISTIME_i * Z_{it}\varphi_t + DISTIME_i^2 * Z_{it}\varphi_t + DISTIME_i^3 * Z_{it}\varphi_t + UNEMP_i * Z_{it}\varphi_t + UNEMP_i^2 * Z_{it}\varphi_t + UNEMP_i^3 * Z_{it}\varphi_t + u_{it} \quad (A4)$$

Where  $Z_{it}\varphi_t = \sum_{\substack{t=1989 \\ t \neq 1995}}^{1997} D_t * TERMINATED_i\beta_t + RESPONSE_{it}\delta_1 +$

$POSTTREND_t\delta_2$ .

The results from this regression are presented in column (2) of Table A1.5. The three coefficients resulting from the interaction between  $RESPONSE_{it}$  and the cubic terms of  $DISTIME_i$  are statistically at the one percent level. The standard error on the  $RESPONSE_{it}$  coefficient is larger and it is now no longer statistically significant at conventional levels; as a result, the combined employment responses for different values of  $DISTIME_i$  are not statistically significant at conventional levels. The U-shaped relationship is present in the point estimates, however, and of similar magnitudes to the main results presented in column (1). The introduction of unemployment conditions at entry does not seem to explain the 4.B results, which is not surprising given that the analysis in Section 1.4.3 showed that the employment response did not vary much with labor market conditions at the time individuals applied for disability benefits.

The second variation is to restrict the sample to individuals in states with program growth between 1989 and 1995 that was below the growth in the median state. As discussed in the paper, the DA&A program grew rapidly during the late 1980s and early

1990s. Compositional changes should have played less of a role in the states with the lowest program growth.

Equation (A3) is estimated using individuals in the 25 states with the lowest program growth over the period which individuals entered these disability programs. The results are presented in column (3) of Table A1.5. The four coefficients of interest are statistically significant at the one percent level. They combine to create a similar pattern to that shown for the overall sample in column (1), with a peak employment response at 2.45 years that is 27 percent higher than the estimated employment response at nine months and 44 percent higher than the estimated employment response at six years of disability benefit receipt.

The third exercise is to see whether the 1994 legislative changes that affected the DA&A program can account for the U-shaped relationship. The Social Security Independence and Program Improvements Act (P.L. 103-296) was signed into law on August 15, 1994. The legislation introduced a three year time limit for benefits and more sanctions for not complying with drug treatment (Hunt and Baumohl, 2003). New rules related to time limits and back pay were introduced in March 1995. The program compliance aspects of the legislation took longer, as they were handled through state-level Referral and Monitoring Agency contracts. Most new contracts were issued in September 1995; contracts for Michigan, New York and Oregon were issued in early 1996 (Hunt and Baumohl, 2003).

There is not an identifiable change in the type of individuals applying for DA&A disability benefits after August 1994 or after the primary implementation dates (March and September 1995). However, to see whether the inverted-U relationship is present

without those who applied for disability benefits after the 1994 legislation, equation (A3) is estimated using individuals who applied for disability benefits prior to August 1994. These results are presented in column (4) of Table A1.5. The four primary coefficients of interest remain statistically significant at the one percent level. The total employment response displays a similar relationship to time on disability benefits that was produced for the whole sample, with a peak employment response at around 2.3 years of disability that is 17 percent higher than the employment response at nine months and 30 percent higher than the employment response at six years of disability benefit receipt.

Finally, given some of the observable characteristics of DA&A beneficiaries changed as the program grew, equation (A3) is estimated for subsamples based on those changing characteristics. Entrants to the DA&A disability programs were increasingly female and black. They were also more likely to report having both alcohol and drug addictions, and less likely to report having only an alcohol addiction. Table A1.6 presents the results for DI subsamples by sex (males, females), race (white, black, other race) and type of addiction (alcohol only, drugs only, alcohol and drugs). Each displays an inverted-U relationship that is qualitatively similar to the one shown in Figure 3 in the paper; there is a peak employment response that occurs between 2.19 and 2.84 years of benefit receipt and which is generally 30 to 50 percent larger than the employment responses at nine months and at six years of disability benefit receipt. The U-shaped relationship does not seem to be due to compositional changes affecting the levels of the employment response for different groups of disability program entrants.

## A1.6 Heterogeneity Results for DI Subsamples with Mental Disorders and Musculoskeletal Conditions and for the SSI Sample

In the paper, I mention that the employment responses are similar within subsamples of individuals who applied to be reclassified on the basis of mental disorders and musculoskeletal conditions. The magnitudes of these subgroups' employment responses are presented for both the DI and SSI samples in Table 3 in the paper. Importantly, the results describing the heterogeneity in the employment response in Sections 4.C and 4.D in the paper are also similar within these groups.

Results for individuals in the DI sample who reapplied on the basis of mental disorders and musculoskeletal conditions are shown in Figure A1 and A2, respectively. Each figure consists of five panels that show how the employment response differs by age at the time of termination, earnings prior to applying for disability benefits, and the level at which disability benefits were awarded. Panels A to D are estimated in the same way as Panels A to D of Figure 4; see the text in the paper for details. An analog of Panel E of Figure 4, showing that the employment response does not vary much by state-level unemployment rates at the time individuals applied for disability benefits, are not presented for these subsamples as in both cases the relationship is weak and the confidence intervals are wide. Panel E is estimated in exactly the same way as the results in Panel F of Figure 4. As for the regressions used to generate Figure 4, employment is defined in terms of the 1996 Substantial Gainful Activity threshold (\$8,339).

The heterogeneity in both subsamples is qualitatively similar to the results in the paper. The magnitude of the employment response decreases with age at the time of termination, and these differences come from those who have received disability benefits



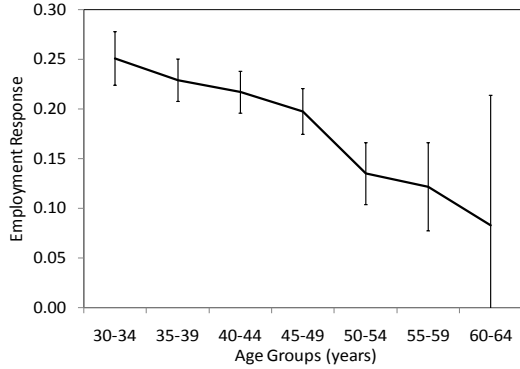
for two to four years prior to termination. The employment response is higher among those with good pre-application earnings, and again the differences are largest across individuals who had received benefits for two to four years prior to termination. Many of these differences remain statistically significant in terms of non-overlapping 95 percent confidence intervals, although the intervals are wider as a result of the smaller sample sizes.

In the comparison of Initial Award and Hearings Award groups the results for the mental disorders sample are similar to the results in the paper, where Hearings Award beneficiaries have a higher response than Initial Award beneficiaries among those receiving disability benefits for a short period of time but a lower response among those receiving benefits for more than two years. In the equivalent results for the musculoskeletal sample, in Panel E of Figure A2, with time on disability benefits the employment response of the Initial Award group converges to the response of the Hearings Award group rather than being distinctly higher over time.

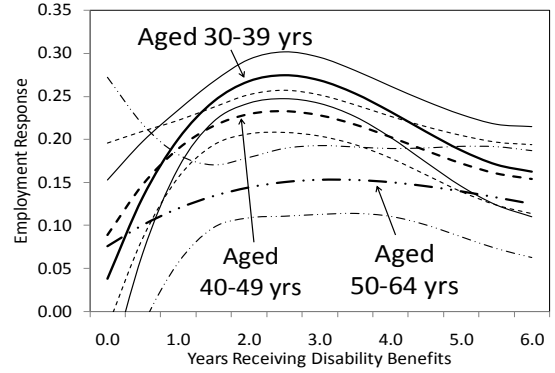
A similar set of results is shown for the SSI sample in Figure A3. The results are qualitatively similar to those for the DI sample, except that the employment responses of 30-39 year olds vary less with benefit receipt than for 40-49 year olds, as shown in Panel B. Panel F shows the same estimates when “any earnings” is used to define employment. The employment responses of the 30-39 and 40-49 year old groups in this analysis are much more similar to the DI sample results, although in both panels the employment responses of the two groups overlap.

Figure A1.1: Heterogeneity in the DI Employment Effects for Mental Disorder Re-applicants

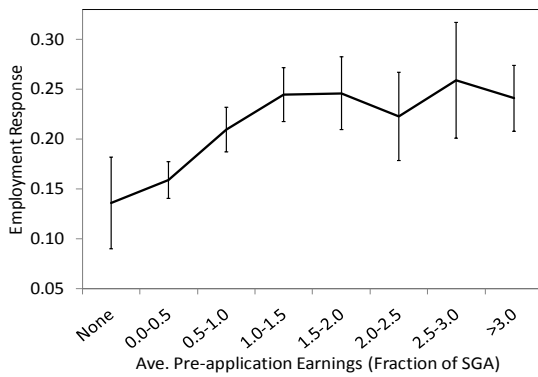
Panel A: Employment Response by Age



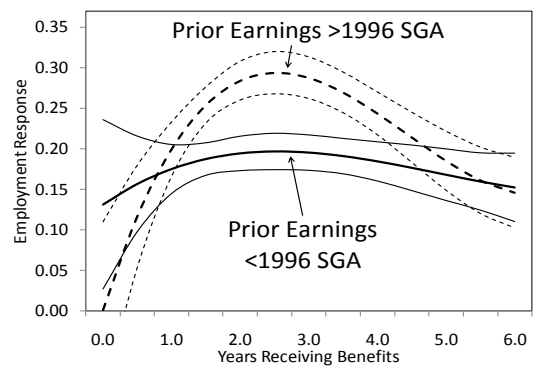
Panel B: Employment Response b By Age & Time on Benefits



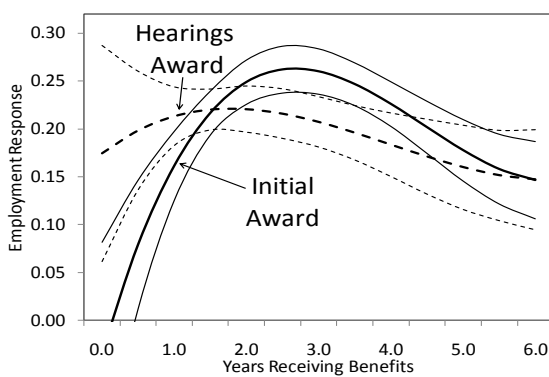
Panel C: Response by Average Earnings 3-5 Years before Applying for Benefits



Panel D: Response by Earnings 3-5 Years Before Applying and Time on Benefits

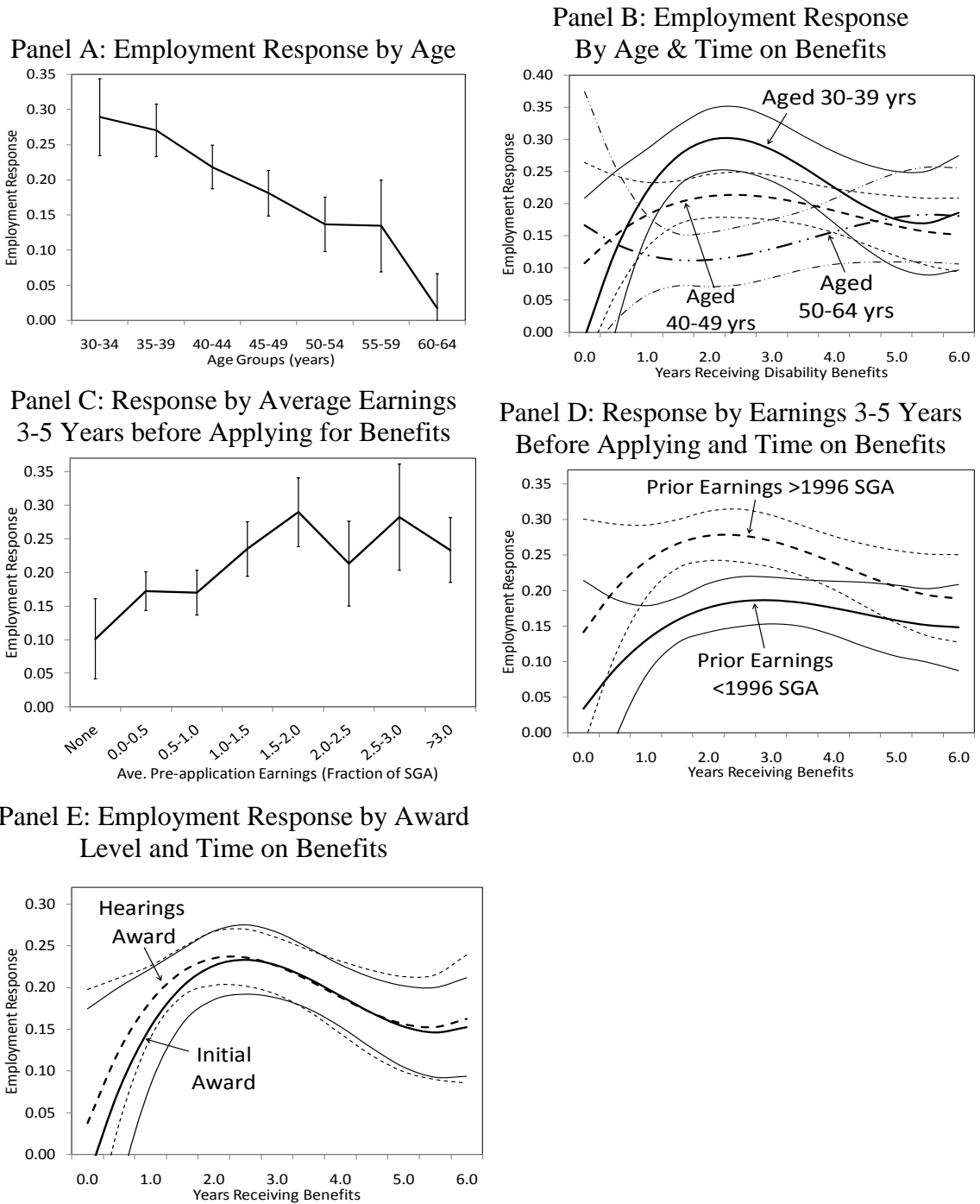


Panel E: Employment Response by Award Level and Time on Benefits



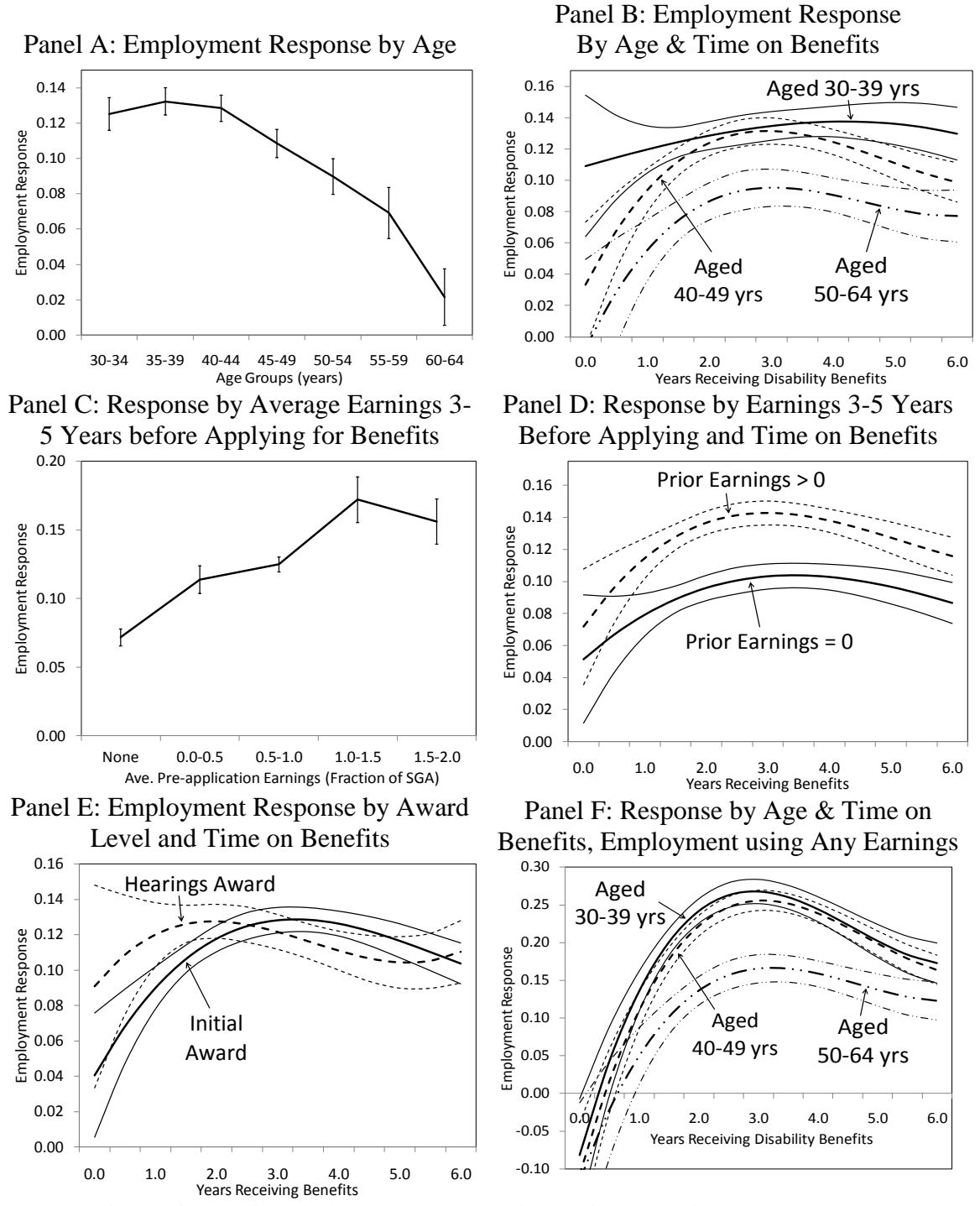
**Notes:** Estimates in Panels A and C use 370,320 observations. Panel B uses 144,440 (Aged 30-39), 162,260 (Aged 40-49) and 63,620 (Aged 50-64); Panel D uses 178,640 (Prior <SGA) and 175,640 (Prior  $\geq$ SGA); and Panel E uses 171,440 (Initial Award) and 149,320 (Hearings Award) observations. An individual is regarded as employed when they have annual earnings above the 1996 Substantial Gainful Activity threshold (\$8,339).

Figure A1.2: Heterogeneity in DI Employment Effects for Musculoskeletal Re-applicants



**Notes:** Estimates in Panels A and C are based on 101,740 observations. Panel B uses 22,360 (Aged 30-39), 43,240 (Aged 40-49) and 36,140 (Aged 50-64); Panel D uses 46,280 (Prior <SGA) and 50,620 (Prior  $\geq$ SGA); and Panel E uses 32,980 (Initial) and 55,880 (Hearings) observations. An individual is employed when they have annual earnings above 1996 SGA (\$8,339).

Figure A1.3: Heterogeneity in the SSI Employment Effects



**Notes:** Estimates in Panels A and C use 1,190,200 observations. Panels B and F use 395,840 (Aged 30-39), 506,560 (Aged 40-49) and 287,800 (Aged 50-64); Panel D uses 495,720 (Prior =0) and 694,480 (Prior >0); and Panel E uses 612,880 (Initial Award) and 419,580 (Hearings Award) observations. Employment is based on earning above the 1996 Substantial Gainful Activity threshold (\$8,339), except in Panel F, where employment is based on any annual earnings.

Table A1.1: Robustness of DI Results in Panel A of Figure 1.2

	I(Earnings > 0)				I(Earnings > SGA)			
	Main Results (1)	Without Ind. FE (2)	Using Age Dummies (3)	Logit Analysis (4)	Main Results (5)	Without Ind. FE (6)	Using Age Dummies (7)	Logit Analysis (8)
1989	-0.0628 (0.0052)	-0.0655 (0.0051)	-0.0657 (0.0051)	-0.0845 (0.007)	-0.0010 (0.0049)	-0.0028 (0.0047)	-0.0028 (0.0047)	-0.0179 (0.0047)
1990	-0.0479 (0.0054)	-0.0502 (0.0052)	-0.0503 (0.0052)	-0.072 (0.0072)	0.0047 (0.0047)	0.0032 (0.0046)	0.0030 (0.0046)	-0.0117 (0.0045)
1991	-0.0287 (0.0055)	-0.0306 (0.0054)	-0.0307 (0.0054)	-0.0547 (0.0075)	0.0090 (0.0043)	0.0077 (0.0042)	0.0075 (0.0042)	-0.0052 (0.004)
1992	-0.0092 (0.0054)	-0.0107 (0.0053)	-0.0108 (0.0053)	-0.0324 (0.0075)	0.0076 (0.0039)	0.0067 (0.0038)	0.0065 (0.0038)	-0.0034 (0.0034)
1993	-0.0043 (0.0052)	-0.0053 (0.0050)	-0.0053 (0.0050)	-0.0212 (0.0074)	0.0116 (0.0033)	0.0110 (0.0032)	0.0109 (0.0032)	0.0045 (0.0029)
1994	0.0029 (0.0044)	0.0024 (0.0043)	0.0023 (0.0043)	-0.0052 (0.0072)	0.0014 (0.0025)	0.0010 (0.0024)	0.0010 (0.0024)	-0.0019 (0.0024)
	--	--	--	--	--	--	--	--
1996	0.0652 (0.0043)	0.0658 (0.0042)	0.0657 (0.0042)	0.0706 (0.0067)	0.0321 (0.0021)	0.0325 (0.0021)	0.0325 (0.0021)	0.0337 (0.0019)
1997	0.2619 (0.0048)	0.2630 (0.0047)	0.2630 (0.0047)	0.2682 (0.0067)	0.1737 (0.0033)	0.1744 (0.0032)	0.1744 (0.0032)	0.1753 (0.002)
1998	0.2851 (0.0050)	0.2869 (0.0048)	0.2869 (0.0048)	0.2948 (0.0067)	0.2134 (0.0036)	0.2145 (0.0035)	0.2144 (0.0035)	0.2152 (0.0021)
1999	0.2655 (0.0050)	0.2679 (0.0049)	0.2680 (0.0049)	0.2769 (0.0068)	0.2221 (0.0037)	0.2237 (0.0036)	0.2236 (0.0036)	0.224 (0.0022)
2000	0.2474 (0.0051)	0.2505 (0.0050)	0.2506 (0.0050)	0.2613 (0.0068)	0.2196 (0.0037)	0.2216 (0.0036)	0.2214 (0.0036)	0.2216 (0.0024)
2001	0.2079 (0.0052)	0.2116 (0.0051)	0.2117 (0.0051)	0.2262 (0.0067)	0.1890 (0.0037)	0.1914 (0.0036)	0.1913 (0.0036)	0.1917 (0.0024)
2002	0.1714 (0.0052)	0.1758 (0.0051)	0.1760 (0.0051)	0.195 (0.0066)	0.1590 (0.0035)	0.1619 (0.0034)	0.1617 (0.0034)	0.1633 (0.0024)
2003	0.1415 (0.0052)	0.1467 (0.0050)	0.1468 (0.0051)	0.1694 (0.0066)	0.1340 (0.0034)	0.1374 (0.0033)	0.1371 (0.0033)	0.1397 (0.0024)
2004	0.1183 (0.0052)	0.1242 (0.0050)	0.1243 (0.0050)	0.1497 (0.0065)	0.1263 (0.0034)	0.1301 (0.0033)	0.1298 (0.0033)	0.133 (0.0024)
2005	0.0990 (0.0051)	0.1057 (0.0050)	0.1057 (0.0050)	0.1335 (0.0066)	0.1166 (0.0033)	0.1209 (0.0032)	0.1206 (0.0032)	0.1244 (0.0025)
2006	0.0862 (0.0052)	0.0936 (0.0050)	0.0937 (0.0050)	0.1234 (0.0066)	0.1077 (0.0033)	0.1125 (0.0032)	0.1123 (0.0032)	0.1165 (0.0025)
2007	0.0670 (0.0051)	0.0753 (0.0050)	0.0753 (0.0050)	0.1076 (0.0066)	0.0960 (0.0032)	0.1014 (0.0032)	0.1013 (0.0032)	0.1058 (0.0026)
2008	0.0499 (0.0051)	0.0590 (0.0049)	0.0589 (0.0049)	0.0944 (0.0066)	0.0843 (0.0032)	0.0902 (0.0031)	0.0902 (0.0031)	0.0953 (0.0026)
R-sq.	0.4564	0.2348	0.2349	--	0.3476	0.1249	0.1250	--

Notes: All regressions have 990,340 observations. The reference year is 1995.

Table A1.2: Robustness of SSI Results in Panel B of Figure 1.2

	I(Earnings > 0)				I(Earnings > SGA)			
	Main Results (1)	Without Ind. FE (2)	Using Age Dummies (3)	Logit Analysis (4)	Main Results (5)	Without Ind. FE (6)	Using Age Dummies (7)	Logit Analysis (8)
1989	-0.0157 (0.0047)	-0.0170 (0.0046)	-0.0174 (0.0046)	-0.0465 (0.0046)	0.0005 (0.0019)	-0.0007 (0.0019)	-0.0007 (0.0019)	-0.0052 (0.0014)
1990	-0.0133 (0.0046)	-0.0145 (0.0044)	-0.0146 (0.0044)	-0.0404 (0.0044)	0.0005 (0.0018)	-0.0005 (0.0018)	-0.0005 (0.0018)	-0.0047 (0.0013)
1991	-0.0178 (0.0043)	-0.0188 (0.0042)	-0.0187 (0.0042)	-0.0381 (0.0042)	-0.0026 (0.0016)	-0.0034 (0.0015)	-0.0033 (0.0015)	-0.0068 (0.0011)
1992	-0.0156 (0.0040)	-0.0164 (0.0039)	-0.0163 (0.0039)	-0.0298 (0.004)	-0.0012 (0.0014)	-0.0017 (0.0013)	-0.0017 (0.0013)	-0.0042 (0.001)
1993	-0.0177 (0.0037)	-0.0182 (0.0036)	-0.0182 (0.0036)	-0.0266 (0.0039)	-0.0029 (0.0012)	-0.0033 (0.0011)	-0.0033 (0.0011)	-0.0049 (0.0009)
1994	-0.0112 (0.0032)	-0.0115 (0.0031)	-0.0115 (0.0031)	-0.0157 (0.004)	-0.0040 (0.0010)	-0.0042 (0.0009)	-0.0042 (0.0009)	-0.005 (0.0009)
	--	--	--	--	--	--	--	--
1996	0.0646 (0.0032)	0.0649 (0.0031)	0.0649 (0.0031)	0.0686 (0.0038)	0.0184 (0.0011)	0.0187 (0.0011)	0.0187 (0.0011)	0.0194 (0.0008)
1997	0.2148 (0.0037)	0.2154 (0.0036)	0.2155 (0.0036)	0.22 (0.0037)	0.0871 (0.0019)	0.0875 (0.0019)	0.0875 (0.0019)	0.0881 (0.0009)
1998	0.2191 (0.0038)	0.2200 (0.0037)	0.2200 (0.0037)	0.2251 (0.0038)	0.1082 (0.0022)	0.1088 (0.0021)	0.1088 (0.0021)	0.1093 (0.001)
1999	0.2173 (0.0039)	0.2186 (0.0038)	0.2185 (0.0038)	0.224 (0.0039)	0.1213 (0.0023)	0.1222 (0.0023)	0.1221 (0.0023)	0.1223 (0.0011)
2000	0.2008 (0.0040)	0.2025 (0.0039)	0.2024 (0.0039)	0.2086 (0.0039)	0.1259 (0.0024)	0.1270 (0.0024)	0.1268 (0.0024)	0.1268 (0.0012)
2001	0.1702 (0.0039)	0.1722 (0.0038)	0.1721 (0.0038)	0.1828 (0.0038)	0.1107 (0.0024)	0.1121 (0.0023)	0.1119 (0.0023)	0.1121 (0.0013)
2002	0.1366 (0.0039)	0.1390 (0.0038)	0.1388 (0.0038)	0.1533 (0.0038)	0.0922 (0.0023)	0.0939 (0.0022)	0.0936 (0.0022)	0.0944 (0.0013)
2003	0.1095 (0.0038)	0.1123 (0.0037)	0.1121 (0.0037)	0.1296 (0.0037)	0.0793 (0.0022)	0.0812 (0.0021)	0.0809 (0.0021)	0.0822 (0.0013)
2004	0.0979 (0.0038)	0.1012 (0.0037)	0.1010 (0.0037)	0.1205 (0.0037)	0.0758 (0.0022)	0.0780 (0.0021)	0.0777 (0.0021)	0.0794 (0.0013)
2005	0.0828 (0.0038)	0.0865 (0.0037)	0.0863 (0.0037)	0.1072 (0.0038)	0.0691 (0.0021)	0.0716 (0.0021)	0.0714 (0.0021)	0.0732 (0.0014)
2006	0.0751 (0.0038)	0.0792 (0.0037)	0.0790 (0.0037)	0.102 (0.0038)	0.0640 (0.0021)	0.0667 (0.0020)	0.0666 (0.0020)	0.0687 (0.0014)
2007	0.0632 (0.0037)	0.0677 (0.0036)	0.0674 (0.0036)	0.0926 (0.0038)	0.0628 (0.0021)	0.0659 (0.0020)	0.0658 (0.0020)	0.0683 (0.0014)
2008	0.0481 (0.0037)	0.0531 (0.0036)	0.0526 (0.0036)	0.0801 (0.0038)	0.0541 (0.0020)	0.0575 (0.0020)	0.0576 (0.0020)	0.0602 (0.0015)
R-sq.	0.3579	0.1013	0.1015	--	0.3309	0.0532	0.0533	--

Notes: All regressions have 1,190,200 observations. The reference year is 1995.

## A2. Appendices for Chapters 2 and 3

### A2.1 Identifying Deaths Related to Substance Abuse

Given the prominent role of substance abuse deaths in current explanations of the within-month mortality cycle, deaths related to use of alcohol and drugs (except tobacco) were separated from other deaths to understand the extent to which they drive aggregate patterns. This is done for deaths coded using the Ninth Version of the International Classification of Disease (ICD-9), which applies to Multiple Cause of Death (MCOB) data from 1979 to 1998.

In their original study into the within-month mortality cycle, Phillips, Christenfeld and Ryan (1999) analyzed substance abuse deaths. We were concerned their approach understated the actual number of substance abuse deaths so, in addition to their ICD codes, we added conditions from other studies that seek to identify substance abuse deaths. The National Institute of Drug Abuse funded a study to estimate the economic costs of drug and alcohol use in the United States in 1992 (Harwood, Fountain, and Livermore, 1998). We include the ICD codes for conditions they wholly attributed to drug abuse, as well as more those of economic costs studies in Australia (Collins and Lapsley, 2002) and Canada (Single et al., 1999). These studies draw on local epidemiological studies as well as studies in other countries, and together these studies provide a broad list of substance abuse conditions. They are listed in Table A2.1.

Up to 20 causes of death can be listed on a death certificate and included in the MCOB files; a death was classified as related to substance abuse if any of these causes of death were one of those listed in Table A2.1.

## A2.2 Creating Consistent Cause-of-Death Categories

The International Classification of Disease (ICD) system applies standard diagnostic definitions to different medical conditions recorded in health records and vital statistics. The Multiple Cause of Death files (MCOB) used in our analysis span three ICD versions: the Eighth (ICD-8) is used for deaths occurring between 1973 and 1978, the Ninth (ICD-9) is used for deaths occurring between 1979 and 1998, and the Tenth (ICD-10) is used for deaths occurring between 1999 and 2005. In this appendix we outline the approach taken to grouping causes of death consistently across these three versions, as the codes and some of the rules governing determining causes of death change with each version.

The deaths in our sample were divided into fifteen subgroups based on the underlying cause of death. There were four cancer-related groups (lung cancer, breast cancer, leukemia, other cancers) and four groups based on external causes (motor vehicle accidents, homicide, suicide and all other external causes). The remaining categories were heart attacks; heart diseases other than heart attack; alcohol-related cirrhosis; cirrhosis not related to alcohol; chronic pulmonary obstructive disease (COPD); stroke; and then a category to cover any causes not covered by the previous fourteen conditions.

Four types of resources were used to create these categories. The first is the Underlying Cause of Death Recodes. When the MCOB files are created, deaths are grouped into broad categories. We used the 34 cause-of-death recode applied to ICD-8 and ICD-9 (UCR34) and the 39 cause-of-death recode applied to ICD-10 (UCR39) to define some of the categories. The second is bridge-coding studies, which accompany each change in ICD versions and report the overlap in categories for a sample of deaths



coded according to the rules and categories of both the old and the new ICD versions. Klebba and Scott (1980) did this when ICD-9 was introduced, and Anderson et al. (2001) did it for the transition to ICD-10. The third is the National Cancer Institute Surveillance, Epidemiology and End Results Program (SEER, 2004), who provide codes to identify types of cancer across ICD versions. The fourth is Jemal et al. (2005), an epidemiological study that report deaths from 1970 to 2002.

These resources were used to identify what deaths to include and exclude in order to develop smooth cause of death categories. Details are now provided for the specific cause-of-death categories.

*Cancer categories.* Together with the UCR34/UCR39 recode, the cancer categories were created using the National Cancer Institute recode (SEER, 2004). Deaths were allocated to the lung cancer and breast cancer categories using these SEER codes. For leukemia, the UCR34 codes were used to create counts during the ICD-8 and ICD-9 years, and then the SEER leukemia codes were used during the ICD-10 years. Deaths were allocated to the “other cancer” category using the UCR34 and UCR39 recodes. All of the coding rules are listed in Table A2.3, and the annual log counts for 1973 to 2004 are shown in Figure A-1 (log counts are used because there is large variation in subcategory counts). The trends are reasonably smooth across versions; the largest change across years where the ICD version changes is within one percent of cancer growth rates in nearby years.

*External causes of death.* Codes for external causes-of-death categories change significantly across the ICD versions, but fortunately using the UCD34 and UCR39 recodes produced smooth series. See Table A2.3 for the specific UCR codes and Figure

A-2 for the natural log of the annual counts of deaths in the four external cause of death categories. There was a brief change from ICD9 to ICD10 that removed some deaths from the motor vehicle accident category. Anderson et al. (2001) suggested this could distort counts in some states; however, no jumps were evident in state-level counts. Percentage changes in transition years were of similar magnitude to other years. The noticeable spike in homicides in 2001 is due to the 9/11 attacks.

*COPD and stroke.* Chronic obstructive pulmonary disease (COPD) and stroke were identified using ICD codes identified by Jemal et al. (2005). For COPD, Anderson et al. (2001) found many deaths coded to bronchitis, emphysema, and asthma in ICD-9 are coded in ICD-10 to J44.8 (Other specified chronic obstructive pulmonary disease). The J44.8 code was not used during the ICD-10 years as it produced a smoother series; no such changes were needed for stroke. Codes for both conditions are in Table A2.3, and the log counts in Figure A-3 show both conditions to be consistently identified.

*Cirrhosis.* Cirrhosis was identified using conditions identified by Jemal et al. (2005), listed in Table A2.3. Given substance abuse is potentially important in explaining within-month variation, alcohol-related cirrhosis conditions were identified as a separate category. The log counts for both categories are show in Figure A-3. In ICD-10 revision rules were changed to create a new category called Alcoholic liver failure (K70.4) (Anderson et al. 2001). A comparison of counts with and without this code suggested it counted more deaths within the cirrhosis subcategories than outside of them, so it was included. The cirrhosis counts are less smooth across transition years 1998 and 1999 than other subgroups, as shown in Figure A-3. Cirrhosis (other than alcohol-related) increased by 6.7 percent, which is much more than the next largest increase in

other years (2.7 percent in 2001). While alcohol-related cirrhosis increased by 13 percent in 1999, this may not be due to the code change as it is a highly variable cause of death (it also increased 13 percent in 2000 and 16 percent in 2004).

*Heart disease categories.* The ICD codes used in Jemal et al. (2005) were used for heart disease. Anderson et al. (2005) found that, when they coded heart-related deaths according to ICD-9 and ICD-10, differences could primarily be explained by most deaths assigned to cardiac arrest code 427.5 in ICD-9 being allocated to other conditions in ICD-10. The removal of deaths with this code produced a smoother series, so it was not included. In order to create a subcategory of heart disease potentially more closely linked to activity, heart attacks (acute myocardial infarctions) were separately coded. The combination of 410 in ICD9 and I21 in ICD10 was used, as they cover deaths generally occurring within 30 days of onset of event. Both heart disease categories produce consistent annual counts, as shown in Figure A-4.

The log counts of deaths not elsewhere classified and total deaths are also shown in Figure A-4. They are also reasonably smooth, which is not surprising given the small changes in the other categories.

Figure A2.1: Log Counts of Deaths 1973-2004: Cancer Categories

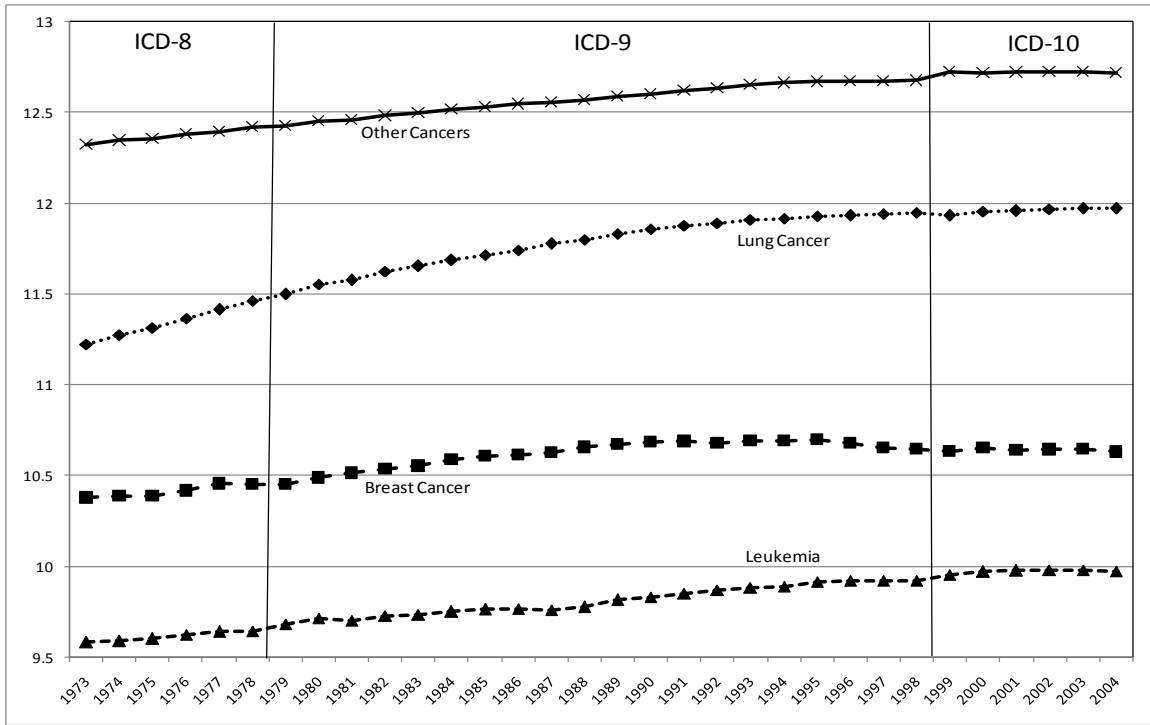


Figure A2.2: Log Counts of Deaths 1973-2004: External Causes of Death

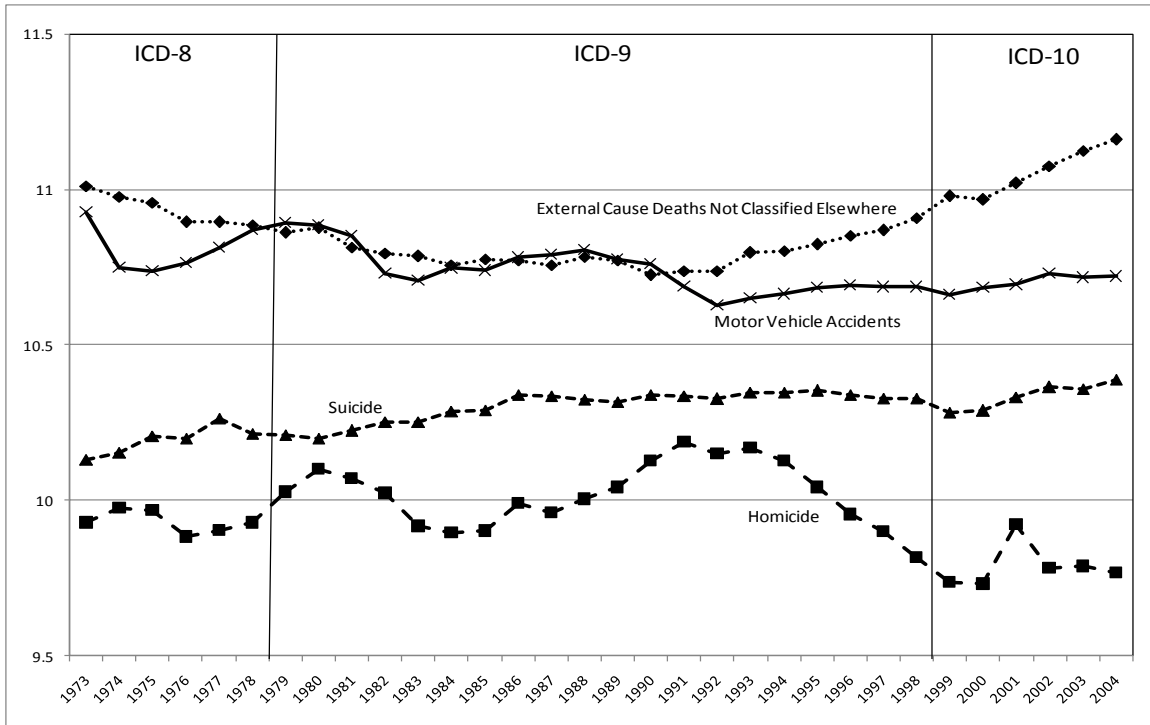


Figure A2.3: Log Counts of Deaths 1973-2004: Stroke, COPD and Cirrhosis Categories

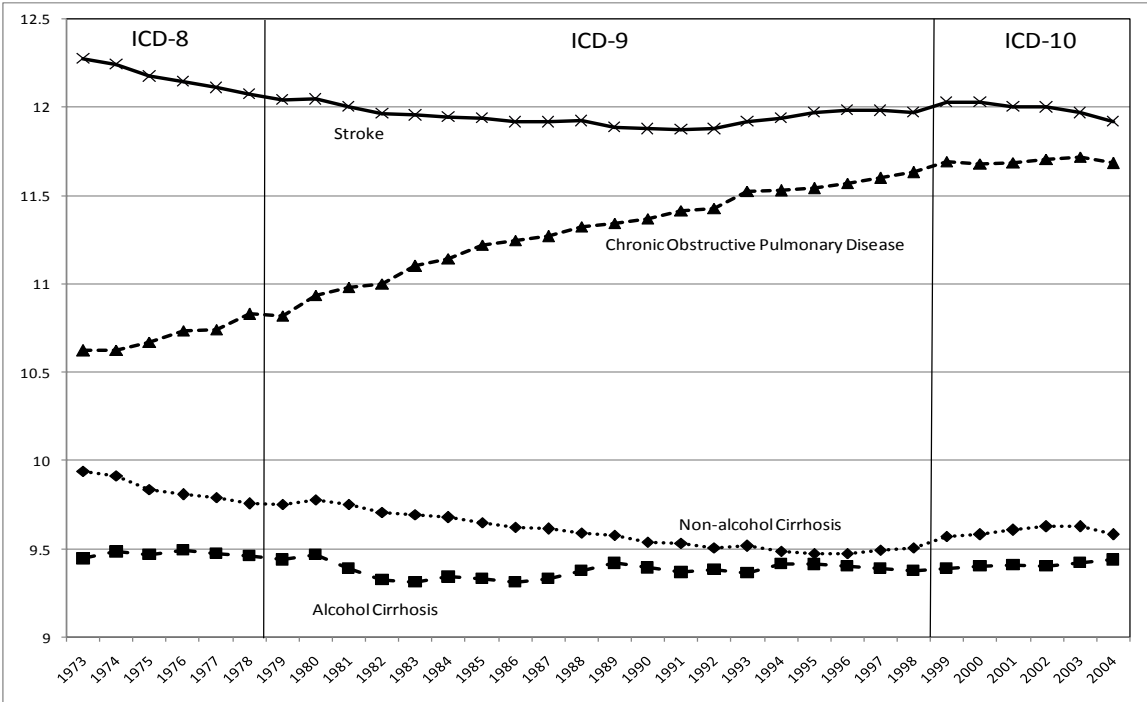


Figure A2.4: Log Counts of Deaths 1973-2004: All Deaths, Heart Disease and Deaths Not Classified Elsewhere

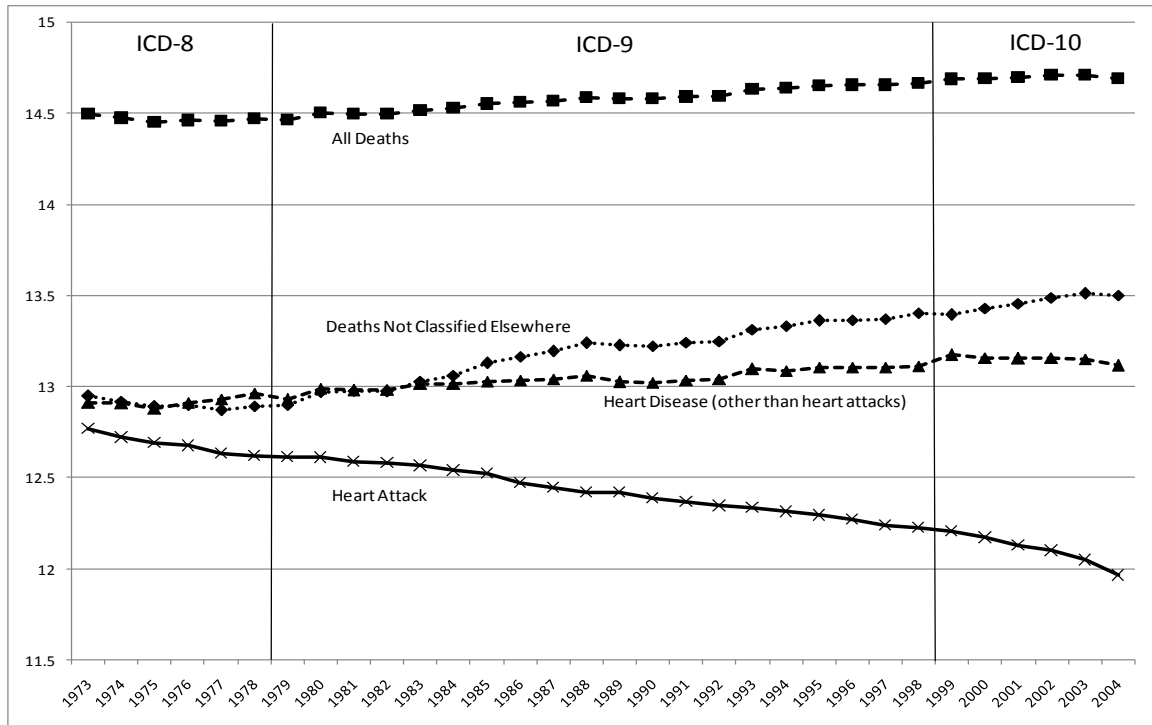


Table A2.1: Substance Abuse Codes for ICD-9

ICD-9 Codes	Conditions
<i>Conditions from Phillips, Christenfeld and Ryan (1999)</i>	
291	Drug psychoses
292	Alcohol psychoses
303	Alcohol dependence syndrome
304	Drug dependence
305.0, 305.2-305.9	Nondependent abuse of alcohol and drugs (except tobacco)
357.5	Alcoholic polyneuropathy
425.5	Alcoholic cardiomyopathy
535.3	Alcoholic gastritis
571.0	Alcoholic fatty liver
571.1	Acute alcoholic hepatitis
571.2	Alcoholic cirrhosis of liver
571.3	Alcoholic liver damage, unspecified
790.3	Excessive blood level of alcohol
947.3	Alcohol use deterrents
977.3	Alcohol use deterrents
980	Toxic effects of ethyl alcohol
E860	Accidental poisoning by alcohol not elsewhere classified
<i>Additional Conditions from Harwood, Fountain and Livermore (1998)</i>	
357.6	Polyneuropathy due to drugs
760.7	Alcohol and drugs affecting fetus or newborn
779.5	Drug withdrawal syndrome in newborns
965	Poisoning by analgesics, antipyretics, and antirheumatics #
967	Poisoning by sedatives and hypnotics
968	Poisoning by CNS muscle tone depressants
969	Poisoning by psychotropic agents
970	Poisoning by CNS stimulants
E850-E858	Accidental poisoning by drugs, medicaments, and biological
E863	Accidental poisoning by agricultural and horticultural chemical and pharmaceutical preparations other than plant foods and fertilizers
E935.0-E935.2, E937-E940	Opiates and other drugs causing adverse effects in therapeutic use
E980	Poisoning by solid or liquid substances where cause is undetermined
<i>Additional Conditions from Australian and Canadian Economic Cost Studies</i>	
640, 641, 648.3, 656.5	Pregnancy complications due to alcohol and drugs (C&L, S)
762.0-762.1, 764-765	Neonatal conditions due to alcohol and drugs (C&L, S)
962.1	Anabolic steroid poisoning (C&L)
E950.0-E950.5	Suicide, self-inflicted poisoning by drugs or medicinal substances (C&L, S)
E962.0	Assault by drugs and medicinal substances (C&L, S)

Notes: # Denotes category broadened from original. C&L: Collins and Lapsley (2002) [Australia]; S: Single et al. (1999) [Canada].



Table A2.2: Substance Abuse Codes for ICD-10

ICD-10 Codes	Conditions
<i>Drug-related conditions</i>	
<i>Office of National Drug Control Policy (2004)</i>	
F11-F19	Mental and behavioral disorders due to psychoactive substance abuse
X40-X44, X46	Accidental poisoning by and exposure to noxious substances
Y10-Y14, Y16	Injury, undetermined whether accidental or purposely inflicted (drug categories)
<i>Added from Collins and Lapsley (2002)</i>	
O35.5	Maternal drug dependence
P04.4, P96.1	Newborn drug toxicity
T38.7	Anabolic steroid poisoning
T40.0-T40.3	Opiate poisoning
T40.4	Poisoning by synthetic narcotics
T40.5	Poisoning by cocaine
T40.7-T40.9	Hallucinogenic poisoning
T43.6	Psychostimulant poisoning
<i>Alcohol-related conditions</i>	
<i>ARDI Alcohol-Related ICD Codes developed by the Centers for Disease Control</i>	
F10.0-F10.2	Alcohol dependence/abuse
F10.3-F10.9	Alcoholic psychosis
G31.2	Degeneration of nervous system due to alcohol
G62.1	Alcoholic poly-neuropathy
G72.1	Alcoholic myopathy
I42.6	Alcoholic cardiomyopathy
K29.2	Alcoholic gastritis
Q86.0	Fetal alcohol syndrome
O35.4	Maternal alcohol dependence
P04.3	Low birthweight
K86.0	Alcohol-induced chronic pancreatitis
K70	Alcoholic liver cirrhosis
K73.0-K74.6	Unspecified liver cirrhosis
<i>Added from Collins and Lapsley (2002)</i>	
T51.0, T51.1, T51.9, X45, Y15	Alcoholic beverage poisoning

Notes: Office of National Drug Control Policy (2004) is an update of Harwood et al. (1998) and is available at: [http://www.ncjrs.gov/ondcppubs/publications/pdf/economic\\_costs.pdf](http://www.ncjrs.gov/ondcppubs/publications/pdf/economic_costs.pdf). The ARDI Alcohol-Related ICD Codes are at: <https://apps.nccd.cdc.gov/ardi/AboutARDICrosswalk.htm>.

Table A2.3: Cause of Death Categories and ICD-8, ICD-9 and ICD-10 Codes

Categories	ICD-8	ICD-9	ICD-10
Lung Cancer	UCOD: 162.2-162.5, 162.8-162.9	UCOD: 162.2-162.5, 162.8-162.9	UCOD: C34
Breast Cancer	UCOD: 174-175	UCOD: 174-175	UCOD: C50
Leukemia	UCR34: Category 10	UCR34: Category 10	UCOD: C90.1, C91-C95
Other Cancer	UCR34: Categories 04, 05, 08, 09 and 11, and not assigned to an above cancer category	UCR34: Categories 04, 05, 08, 09 and 11, and not assigned to an above cancer category	UCR39: Categories 04-07, 10-13 and 15, and not assigned to an above cancer category
Motor Vehicle Accidents	UCR34: Category 33	UCR34: Category 33	UCR39: Category 38
Suicide	UCR34: Category 35	UCR34: Category 35	UCR39: Category 40
Homicide	UCR34: Category 36	UCR34: Category 36	UCR39: Category 41
Other External Causes of Death	UCR34: Category 34	UCR34: Category 34	UCR39: Category 39
Heart Attacks	UCOD: 410	UCOD: 410	UCOD: I21
Heart Disease (Other Than Heart Attack)	UCOD: 390-398, 402, 404, 411-429	UCOD: 390-398, 402, 404, 411-429	UCOD: I00-I09, I11, I13, I20, I22-I51
Alcohol-related Cirrhosis	UCOD: 571.0-571.3	UCOD: 571.0-571.3	UCOD: K70
Cirrhosis Not Related to Alcohol	UCOD: 571.4-571.9	UCOD: 571.4-571.9	UCOD: K73, K74
Chronic Pulmonary Obstructive Disease	UCOD: 490-493, 519.3	UCOD: 490-496	UCOD: J40-J43, J44.0-J44.7, J44.9, J45-J48
Stroke	UCOD: 430-439	UCOD: 430-439	UCOD: I60-I69

Notes: UCOD = underlying cause of death and UCR = underlying cause of death recode, which are the 34 Cause of Death Recodes for the ICD-8 and ICD-9, and the 39 Cause of Death Recode for the ICD-10.

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